Applications of High-Frequency Event Studies: Policy Evaluation and Financial Analysis

Universität Hamburg
Fakultät für Wirtschafts- und Sozialwissenschaften

Dissertation

Zur Erlangung der Würde der Doktorin/des Doktors der
Wirtschafts- und Sozialwissenschaften

"Dr. rer. pol."

(gemäß der PromO vom 18. Dezember 2017)

vorgelegt von

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aus Rio de Janeiro, Brasilien

Hamburg, den 7. Juli 2025

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Datum der Disputation:
19. Dezember 2025

Acknowledgements

First and foremost, I would like to express my deepest gratitude to Michael Bauer. Thank you for guiding me on this path toward knowledge and wisdom, and for being an exceptional mentor and role model. I am specially grateful for the freedom and flexibility you granted me to set my own working hours and choose the environments where I felt most productive, while at the same time holding me to the highest standards of excellence in my work. Thank you for your generosity with your time, for consistently offering valuable feedback when I needed it, while also giving me the space to learn and grow from my own mistakes. Your mentorship has been truly exceptional, whether through helping secure funding for key milestones for my doctoral studies, facilitating my visiting stay at UC Berkeley, or supporting my professional career beyond the PhD.

I am deeply grateful to Olaf Posch and Ole Wilms for their guidance and support throughout my doctoral studies. Thank you for offering valuable feedback on my research and career path, for sharing your technical expertise and personal experiences, and for helping me navigate the often challenging journey of pursuing a PhD.

I am also very thankful to all my coauthors who have supported and challenged me throughout the papers we wrote together, making academic life feel like much more than just work. My sincere thanks to Glenn Rudebusch, Daniel Huber, Marlene Renkel, Annalisa Ferrando, and Sara Lamboglia.

A heartfelt thank you to my master's thesis supervisor and Statistics Professor at the University of St. Gallen, Francesco Audrino, for providing the final encouragement to start my PhD. I am also very grateful to Eben Lazarus for his support during my stay at UC Berkeley, offering feedback and motivating me to improve Chapter 2. Finally, I would like to thank Ursel Baumann for her support in the development of Chapter 6 and for helping to make our contribution visible within the ECB.

I can hardly express in words the depth of my gratitude to my parents, Claudia and Marcelo Offner, for always believing in me and supporting me both emotionally and practically. You raised me into the person I am today and generously financed my undergraduate studies, always respecting the decisions I made along the way. I am also profoundly thankful to my sister, Larissa Offner, for her constant emotional support.

Lastly, I want to thank Nina Kruse, my other half and the love of my life, for filling my life with wonderful moments beyond work and for giving me the perfect emotional balance to thrive in my career.

Contents

Chapter 1

Introduction

The field of applied economics relies heavily on third-party data for empirical research. Unlike natural scientists, economists rarely have the opportunity to conduct randomized controlled trials (RCTs) due to ethical and moral constraints. However, since RCTs are a cornerstone for establishing causal relationships in a variety of areas, applied economists must develop alternative methods to identify and quantify causality. Fortunately, as Gürkaynak and Wright (2013) notes, fate occasionally presents economists with a pseudo-natural experiment — events that naturally create conditions resembling a controlled treatment, such as the release of economic news. In general, newly released information can provide a means of identifying the causal effect of its underlying factors on specific variables.

In RCTs, the observed outcome in the control group is typically assumed to represent the counterfactual effect — that is, what would have occurred in the absence of treatment. In economics, however, researchers can often distinguish between units that were affected by news events and those that were not, while controlling for potential pre-treatment differences to identify the treatment effect. When there is no variation in treatment — for example, if all units are exposed to the news or if only a single unit is observed — researchers may assume that, in the absence of the news, the variable of interest would either remain unchanged or follow a predictable path based on economic theory or historical data.

A crucial challenge in making valid identification assumptions lies in defining the optimal time window around the event. The longer this window, the higher the risk that the variable of interest may be influenced by other factors that are not related to the event, such as additional shocks or noise. One way to reduce the effects of confounding variables is to use high-frequency data. This approach to causal identification, which analyzes data at high frequency around specific events, is known as a *high-frequency event study*.

Because this method requires high-frequency data for credible identification, it is primarily applied to the prices of liquid assets, for which data are available at daily, hourly, or even minute intervals. Notably, its popularity has grown over the past few decades, driven by the vast expansion of accessible financial market data, particularly following the "electronification" of trading (Bech et al., 2016). Finally, when analyzing asset prices, an additional and critical assumption is that markets rapidly incorporate the information content of news within the chosen time interval. Evidence from French and Roll (1986) and Fleming and Remolona (1999) supports the view that asset prices adjust very quickly to new information.

Although event studies aimed at assessing the effects of news on financial markets can be valuable in their own right[1], there is an even more fundamental reason why financial markets are of interest: they embed expectations about the future. Understanding the economic forces reflected in asset prices can help reveal the impact of an event on (1) the underlying drivers of those prices, such as expected inflation, discount rates, or expected earnings, and (2) the broader macroeconomy, given that asset prices are linked to the cost of capital and future economic growth. Thus, event studies combined with asset price analysis form a powerful tool, enabling economists to address a wide range of macroeconomic questions, even in the absence of direct macroeconomic data.

This thesis uses the high-frequency event study approach to investigate a range of important and unresolved questions in the fields of macroeconomics and financial economics. While its primary focus is on the consequences of economic policies for firm behavior and financial market participants, it also explores broader topics within applied economics. The analysis relies mainly on a rich panel dataset of firm-level information from the United States and the Euro area, combined with high-frequency event studies as a tool for identifying causal relationships. In certain chapters, theoretical models and alternative econometric methods complement the empirical analysis.

Event studies can take several different forms. The econometric specification varies substantially depending on the specific research question. For example, Chapters 2, 3, and 5 focus on identifying economic shocks, closely following established methodologies in the literature (Kuttner, 2001; Gurkaynak et al., 2005; Bauer and Swanson, 2023a). Chapter 4 investigates how a macroeconomic event influences the cross-sectional variation of the outcome variable, shedding light on how the underlying factors embedded in the news affect outcomes differently based on differences in their characteristics. In addition, Chapter 6 examines unit-specific staggered events to capture the heterogeneous effects of such events on firms, based on methods surveyed by Miller (2023). Furthermore, in Chapters 3 and 6, the high-frequency event study approach is extended to methods suited for analyzing movements in low-frequency variables based on Jordà and Taylor (2025) and Dube et al. (2023).

In general, the thesis aims to apply these methods to address critical questions in two central areas of economic research: monetary economics and climate economics. Monetary policy is arguably one of the most critical and sensitive areas of economic policymaking. Understanding its mechanisms is of paramount importance to ensure a well-functioning economy that promotes prosperity for its citizens. Central banks adjust the policy interest rate to achieve their mandates. Naturally, these adjustments have significant and wide-ranging effects on numerous financial and macroeconomic variables. Moreover, these changes influence economic agents in diverse and complex ways.

The first question I investigate is how monetary policy influences stock prices depending on the valuation-to-fundamental ratios of firms. Stocks with high valuations relative to fundamentals are commonly referred to as growth stocks, whereas those with lower valuations are called value stocks. The role of valuation to fundamental ratios in stock prices is a central issue in macro-finance and asset pricing, with numerous studies continuing to explore this relationship. Many types of stocks can fall into the

[1] For instance, such studies have been used to evaluate the effectiveness of central banks' large-scale asset purchases and other liquidity-provision measures during and after recent financial crises (Gürkaynak and Wright, 2013).

growth category, including technology and green stocks. In this analysis, I use a high-frequency event study to identify exogenous monetary policy surprises in the United States and estimate the causal effects of monetary policy on stock returns following Gurkaynak et al. (2005); Nakamura and Steinsson (2018b); Jarociński and Karadi (2020); Bauer and Swanson (2023a) and many others. I extend the methodology to examine the heterogeneous effects of monetary policy on stock returns, conditional on firms' valuation-to-fundamentals ratios. Using fixed-effects regressions, I demonstrate that growth stocks are more sensitive to monetary policy. Specifically, following an increase in the central bank's policy rate, firms with higher valuation-to-fundamental ratios tend to experience larger declines in stock prices. The main contribution of this chapter is to show that this differential response is driven by differences in cash flow duration, which is correlated with firms' valuation-to-fundamentals ratios. Growth stocks have longer cash flow durations because, by definition, they reinvest a substantial portion of current earnings to fund future growth. As a result, their cash flows are expected further into the future. These future cash flows are discounted to today, leading to higher valuations relative to fundamentals. Cash flow duration has significant implications for how monetary policy transmits to asset prices. Due to the discounting of future cash flows, interest rate changes have a larger effect on assets whose cash flows lie further in the future. This is precisely why growth stocks are more responsive to monetary policy surprises. Finally, I incorporate these insights into an asset pricing model, reconciling my results with the key empirical observations that the difference in unconditional expected returns between value and growth stocks is positive. These findings highlight the critical importance of considering cash flow duration in understanding the transmission of monetary policy to the stock market — a channel that ultimately plays a vital role in central banks' efforts to influence inflation and economic output.

The second question I address concerns the transmission of monetary policy to firms' investment decisions. From an economic perspective, two main factors influence firms' investment choices: investment opportunities, determined by the marginal product of capital, and financial conditions, which reflect firms' ability to access external funding. Empirically, distinguishing between these two components poses a significant challenge. Together with my co-authors, I seek to disentangle these factors using data from the Survey on the Access to Finance of Enterprises in the Euro area. By analyzing the firms' responses to two key survey questions – regarding their need and the availability of external financing — we show that funding needs are primarily associated with investment opportunities, while funding availability reflects financial conditions. Building on these empirical insights, we examine how monetary policy shocks, identified with a high frequency event study following the approach of Altavilla et al. (2019), affect investment. To perform this analysis, we employ the local projection method of Jordà (2005), using monetary policy surprises as proxies for exogenous shocks. We then investigate the respective roles of investment opportunities and financial conditions in shaping the transmission of monetary policy to investment. Our findings indicate that both components are crucial drivers of investment responses. Firms with higher funding needs, reflecting stronger investment opportunities, exhibit greater sensitivity of investment to monetary policy shocks. In contrast, when the availability of external financing is high, indicating more favorable financial conditions, the responsiveness of investment to monetary policy diminishes. These results can be interpreted through the lens of the credit channel. Monetary policy primarily influences investment via firms' balance sheets and the bank lending channel. Consequently, the effectiveness of monetary policy easing may be limited if firms lack the fundamental desire to invest. However, when financial conditions are tight, monetary easing can stimulate investment by improving the access of firms to funding. In general, these insights can help central banks better anticipate the effects of their policy decisions, which often depend on economic conditions beyond their immediate control.

The second overarching theme of this thesis, climate economics, has grown increasingly prominent in response to the urgent need to combat climate change. Alongside climate science, the field of economics plays a crucial role in advancing the green transition, ensuring that the necessary adaptations to mitigate climate change are implemented through effective economic instruments. This involves supporting a smooth transition while maintaining a functioning economic system, whether by creating appropriate incentives for existing sectors to reduce carbon emissions or by supporting the development of new technologies that advance the green transition. Within this context, climate finance is a key component of mitigation efforts, as it examines how to direct private capital toward climate-friendly business models.

Chapter 4 investigates whether the Inflation Reduction Act (IRA) — the largest climate policy action ever undertaken in the United States — achieved one of its central objectives: lifting expected profitability for climate-friendly firms relative to carbon-intensive firms. Signed into law on August 16, 2022, the IRA introduced extensive tax incentives, subsidies, and credits aimed at supporting green technologies. To assess its impact, we focus on two key events that sharply shifted public perceptions about the likelihood of the bill passing. These events created a natural experiment, allowing us to examine whether market expectations reflected the IRA's intended goal of reducing the cost of capital for green firms compared with brown firms. Crucially, these events also represent the realization of transition risk, that is the potential for substantial economic losses arising from regulatory and legislative measures to mitigate climate change, which can lead to stranded assets. Using an event study methodology, we analyze the effects of these developments on the cross-section of stock returns in the United States. Our findings show that green stocks benefited considerably more than brown stocks from the IRA, with effects that are economically and statistically significant. Following these realizations of climate policy transition risk, green and brown stocks experienced pronounced price movements in opposite directions. From a theoretical perspective, favorable news about the IRA's prospects increased expected profitability for green firms, while adversely affecting brown firms through both demand and cost channels. A key insight from this chapter is that although brown firms' stock prices declined in response to the IRA, these declines were neither excessive nor disorderly. This suggests that, despite the unprecedented scale of this climate policy intervention, the risks of financial sector disruptions, bankruptcies, or crises tied to future climate policies may be manageable.

During the period covered by this thesis, the world experienced a significant surge in inflation, initially triggered by both demand and supply shocks arising from the COVID-19 pandemic. In response, central banks around the globe implemented one of the most substantial monetary policy tightening cycles seen in decades. Among the concerns surrounding these policy measures was the possibility that higher interest rates could hinder the green transition, given that green firms typically rely more heavily on upfront capital to finance investments in new technologies. Consequently, there was apprehension that green firms might be disproportionately affected by tighter monetary policy (Schnabel, 2023). To investigate whether this concern is justified, my co-authors and I use the monetary policy surprises developed by Altavilla et al. (2019) to analyze the impact of monetary policy on the equity prices of green and brown firms in Europe. Across a range of analyses, we find that brown firms — identified based on their levels or intensities of carbon emissions — are more negatively affected by tighter monetary policy than green firms. We conduct numerous robustness checks, including the use of alternative monetary policy surprises, different emissions metrics, and various econometric specifications. Moreover, our results indicate that this differential effect cannot be explained by other firm-specific characteristics, suggesting that the degree of "greenness" itself plays a distinct and significant role in determining how sensitive a firm's stock price is to monetary policy. Overall, our findings suggest that green European firms may not have been disproportionately harmed by the ECB's recent interest rate hikes, alleviating concerns about potential negative impacts on the ongoing green transition.

Chapter 6, the final chapter of this thesis, seeks to deepen our understanding of firms' behavior during the green transition. As the transition progresses, firms must adapt their business models to navigate a rapidly changing technological, regulatory, and political landscape. One way to reduce future regulatory impacts — and thereby lower transition risk — is for firms to commit to reducing their carbon emissions. Because financial markets are forward-looking, credible commitments to future decarbonization could immediately enhance firm valuations, for example by lowering risk premiums. However, significant investments in decarbonization can also entail substantial costs and risks that may negatively affect future cash flows and profitability. Whether the market views the benefits of green pledges as outweighing their costs remains an open question. In this chapter, my co-authors and I address this question using a comprehensive dataset of publicly listed U.S. firms combined with a large corpus of news articles mentioning these firms. We leverage the capabilities of the large language model GPT-4 to classify news articles by identifying instances of "corporate green pledges" from specific firms on specific days. We then analyze stock market reactions to these pledges using a high-frequency event study methodology. Our findings indicate that green pledges have a positive and statistically significant effect on stock market valuations, suggesting that investors perceive the anticipated benefits of these commitments as outweighing their costs. This implies that financial markets may provide additional incentives for firms to pursue decarbonization. Moreover, through dynamic event study regressions, we show that green pledges generate immediate and persistent increases in stock valuations, with little evidence of information leakage before the announcements or price reversals afterward. An important additional question is whether such corporate commitments are credible and translate into actual reductions in emissions. Our findings also raise the possibility that firms might announce green pledges purely to achieve short-term market benefits, such as higher stock prices, without genuine plans for implementation, an issue central to current concerns about greenwashing in both public discourse and academic research. To investigate this, we extend our event study framework to examine the impact of identified green pledges on firms' future emissions. Specifically, we estimate difference-in-differences local projections for firm-level carbon emissions, following the methodology of Dube et al. (2023). Our results indicate that green pledges are indeed followed by substantial reductions in both emission levels and emission intensities. These findings help alleviate concerns about greenwashing by demonstrating that green pledges are, on average, reliable predictors of tangible progress toward decarbonization.

All in all, this dissertation brings together a collection of empirical findings in macroeconomics and financial economics that advance the existing literature and offer valuable insights into monetary policy and climate finance. Beyond adding another layer of knowledge, the results presented in this thesis also provide the foundation for a new research agenda. Methodologically, the increasing availability of high-frequency data promises to further expand the scope of high-frequency event studies. I see significant potential in applying these methods to examine the effects of economic policy on variables such as inflation expectations and economic growth, as well as to improve the identification of shocks related to fiscal and climate policies — an area in which progress has already been made by Lengyel (2022), Känzig (2023), and Phillot (2025). While conventional monetary policy has been extensively studied, several questions remain open in the field of unconventional monetary policy. The literature continues to make important advances in this area, including the construction of quantitative easing surprises by Altavilla et al. (2019) and Swanson (2021). Moreover, recent studies have incorporated the information content of central bank press conferences and speeches to enhance the statistical power of monetary policy surprises (Bauer and Swanson, 2023a; Istrefi et al., 2024). Finally, climate economics remains an emerging and dynamic field, with ongoing debates surrounding the existence of a carbon premium (Bolton and Kacperczyk, 2021; Bauer et al., 2022), the estimation of the social cost of carbon (Nordhaus, 2017), and the optimal design of climate policy frameworks (Nordhaus, 1993; Hillebrand and Hillebrand, 2019). High-frequency event studies could prove to be a powerful tool for advancing our understanding of these critical issues.

Chapter 2

Growth vs. Value: The Role of Cash Flow Duration in Monetary Policy Transmission

Abstract

An open question in macro-finance concerns the differing reactions of growth and value stocks to monetary policy. I address this question using a high-frequency event study and find that growth stocks respond significantly more to policy surprises. This finding is consistent across individual stocks, portfolios, and stock indexes and persists for several days post-FOMC announcement. I show that cash flow duration drives these results, contradicting earlier studies that argue that financial constraints are the predominant driver. A decomposition of stock returns indicates that shocks to the risk premium are the predominant channel explaining this difference in sensitivity, aligning with cash flow duration as the primary driver of monetary policy transmission. A model with firm heterogeneity in cash flow duration can simultaneously explain both the stronger sensitivity of growth stocks and the existence of the value premium.

2.1 Introduction

In March 2022, the Federal Reserve initiated the first of what would become the largest series of tightening steps since the Volcker disinflation. Figure 2.1 illustrates the trajectory of the 1-year Treasury yield, superimposed with the Russell 1000 Growth and Value indexes in the first half of 2022. The four Federal Open Market Committee (FOMC) announcements during this period coincide with a negative correlation between the stock market and the 1-year Treasury yield, as well as a significant underperformance of the Russell Growth Index relative to the Value Index by around 20 percentage points. Was this relative underperformance of growth stocks a result of the Federal Reserve's tightening? More broadly, are growth stocks inherently more sensitive to monetary policy? What economic mechanism explains this sensitivity? This paper examines these questions and provides evidence that growth stocks are indeed more sensitive to monetary policy due to their longer cash flow duration, offering new insights into cash flow duration as an important transmission channel through which monetary policy affects financial markets.

The existing literature provides conflicting answers to the question which type of stock, growth or value stocks, is more sensitive to monetary policy. While Ehrmann and Fratzscher (2004) find that growth stocks are more responsive using constituents from the S&P 500, Maio (2014) provides evidence that value stocks exhibit greater sensitivity. In contrast, Ozdagli (2018) does not find statistically significant differences in their responses. The underlying drivers of these sensitivities remain poorly understood, with much of the literature offering no empirical investigation, but rather hypothesizing how financial frictions could explain their findings. Despite the lack of empirical investigation and the limited understanding of these sensitivities, examining the relationship between monetary policy and the performance of growth and value stocks remains crucial. Such an understanding would not only enhance our knowledge of monetary policy transmission channels to financial markets and the real economy, but would also provide valuable insights into the fundamental drivers of growth and value stock prices, shedding light on the underlying reasons for the existence of the value premium.[1]

The first contribution of this paper is to investigate whether growth or value stocks exhibit greater sensitivity to monetary policy using a high-frequency identification approach. I use the monetary policy surprise developed by Nakamura and Steinsson (2018b), which accounts for both target and forward guidance surprises, the latter being especially important for the period of the zero lower bound. I provide new causal evidence that growth stocks are more responsive to monetary policy than value stocks. For instance, a firm-level regression reveals that a change of one standard deviation in market-to-book equity corresponds to an approximate 2.2 percentage points drop in returns following a monetary policy tightening. The heightened sensitivity of growth stocks is evident at the index level, individual stock level, and across portfolio sorts, suggesting that this result does not disappear due to idiosyncratic noise or diversification. Furthermore, I show that after a monetary policy surprise a significantly larger response of growth stocks relative to value stocks will persist on average for more than

[1] Technology and clean-energy companies are prime examples of firms considered to be growth firms. These stocks have attracted the attention of policymakers and investors in recent years. For central banks around the world, understanding the impact of monetary policy on green firms has become a top priority (Patozi, 2024; Döttling and Lam, 2023; Bauer et al., 2024c).

10 days.

Considering that so far the macro-finance literature presented very distinct conclusions about the sensitivity of growth and value stocks to monetary policy, I ensure that my findings remain robust to potential limitations in the empirical analysis. I revisit the studies from Maio (2014), Ehrmann and Fratzscher (2004), and Ozdagli (2018), providing explanations for the differences in the results. For instance, Maio (2014)'s findings rely on monthly changes in federal funds rates as indicators of policy shocks, a choice that introduces endogeneity bias inherent in monetary policy. Additionally, I extend the results of Ehrmann and Fratzscher (2004) to the broader CRSP and Compustat universe and incorporate monetary policy surprises from 1990 to 2018, covering nearly 30 years of data. I show that the effect is stronger for CRSP/Compustat stocks compared to S&P 500 stocks, primarily due to the heightened sensitivity of growth stocks among smaller firms. Additionally, my findings are robust across various datasets, including the Russell Value and Growth Indexes and Fama and French portfolios, indicating that my methodology of sample construction does not play a crucial role. The lack of significant results in Ozdagli (2018) may, therefore, be attributed to differences in sample selection, diverging from both my approach and those used by Fama and French (1992), and the Russell Indexes. My findings also remain robust across various definitions of growth and value stocks, different measures of monetary policy surprises, and during the zero lower bound.

Figure 2.1: Negative Correlation of Yields and Stock Market

The figure shows the 1-year treasury yield on the right y-axis and the performance of the Russel 1000 Value and Growth Index on the left y-axis. The red vertical dotted line represent the FOMC announcements. The sample goes from Dec-2021 to May-2022.

The key finding of my paper is that the increased sensitivity of growth stocks to monetary policy is attributed to their characterization as longer-duration asset. Growth stocks typically exhibit higher cash flow duration because firms with fewer growth opportunities tend to save and invest less, paying a greater share of their resources in the short term, thereby becoming short-duration companies (Gonçalves, 2021). Consequently, if a firm's stock price reflects the present value of its future dividends, changes in discount rates will have a more pronounced impact on the price of growth stocks. This proposition is consistent with previous studies that identify cash flow duration as the fundamental difference between growth and value stocks (Lettau and Wachter, 2007; Gonçalves, 2021; Gormsen and Lazarus, 2023). Indeed, my firm-level regression analysis reveals that, once controlling for different measures of cash flow duration from the literature (Weber, 2018; Gonçalves, 2021; Gormsen and Lazarus, 2023), the sensitivity of growth and value stocks to monetary policy is no longer statistically significant. This finding is further corroborated by a portfolio analysis that double-sorts firms by cash flow duration and market-to-book equity. Firms with different market-to-book ratios but similar cash flow durations exhibit no significant differences in their response to monetary policy shocks. These results remain robust across various methods of estimating cash flow duration, which use alternative datasets, and differ in their underlying assumptions. This finding introduces a novel perspective on the role of cash flow duration as a transmission channel for monetary policy to the cross-section of stock returns, highlighting its relevance for policymakers and stock market investors.

The limited focus on cash flow duration in studies of monetary policy transmission to growth and value stocks can be attributed to a predominant emphasis on the heterogeneous effects of monetary policy through financial constraints. For example, Maio (2014) argues that value stocks should respond stronger to monetary policy, because of the credit channel mechanism through which monetary policy transmissions to investment operate. The balance sheet channel states that after a monetary policy easing, firm's net worth increases due to a higher collateral value. The bank lending channel operates through the fact that banks increase their loan supplies after a monetary policy easing, providing firms with more access to loans. Both channels enable firms to increase investment and ultimately future cash flows. These explanations suggest that a firm's sensitivity to external financing influences how it is affected by monetary policy, which is a point supported by existing research (Ozdagli, 2018; Ottonello and Winberry, 2020; Cloyne et al., 2023).

Despite considerable interest in how financial frictions influence the responses of growth and value stocks to monetary policy, it remains an open question which type of stocks are more financially constrained. Ehrmann and Fratzscher (2004) propose the possibility of two offsetting channels: On the one hand, a variety of investment opportunities might force growth stocks to be more reliant on external funding, and thus more financially constrained. On the other hand, growth stocks might have more favorable conditions in the credit market due to their higher asset valuation. To investigate whether financial constraints could explain the increased sensitivity of growth stocks to monetary policy, I run a fixed effects regression — analogous to the duration analysis — using a set of established financial constraint indexes (Kaplan and Zingales, 1997; Whited and Wu, 2006; Hadlock and Pierce, 2010; Schauer et al., 2019). The results show that, in contrast to cash flow duration, the financial constraint indexes do not explain the sensitivity of growth and value stocks. Although my findings confirm that financial constrained firms are more sensitive to monetary policy, as already shown by Chava and Hsu (2020), financial constraint is not related to growth and value stocks responses. This finding might speak in favor of no clear link between growth and value stocks and financial constraint. Alternatively, it is possible that both mechanisms proposed by Ehrmann and Fratzscher (2004) are correct, with the opposing effects canceling each other out.

My findings highlight that for growth and value firms, fundamental factors appear to play a more critical role than financial conditions in explaining their sensitivity to monetary policy. In particular, the decisive factor is how growth opportunities shape the timing of future cash flow payments, which is ultimately reflected in stock prices. In contrast, investors' evaluations of these firms' ability to access external funding seem secondary and do not account for the observed differences. Disentangling the effects of financial constraints from those of investment opportunities, however, remains a challenge — attempts to address it go back to the last century (see, for example, Gilchrist and Himmelberg, 1998). Because of this complication, financial constraint indexes might also be capturing growth opportunities and vice-versa, which can bias the coefficients. Therefore, I run a regression incorporating both cash flow duration and financial constraints. Specifically, I estimate the first principal component of the three duration measures and the four financial constraint measures and include them in a fixed effects regression. The results remain consistent: financial constraints exhibit a negative coefficient, but it is only when cash flow duration is included that the difference in the sensitivity of growth and value stocks loses its statistical significance.

To better understand how the stock market responds to monetary policy, I follow Bernanke and Kuttner (2005) and log-linearize excess returns to unravel the sensitivity of growth and value stock returns to movements in discount rates and expected cash flows. In the spirit of Campbell and Ammer (1993), I estimate the long-run news on future discount rates and cash flows using a VAR(1). This approach not only helps identify whether the differences in sensitivity in growth and value stocks arise from changes in discount rates or expected cash flows but also acts as a robustness check for the underlying drivers of these responses. Specifically, if cash flow news were the primary driver of these differences, one would expect financial constraints to play a central role. However, my findings indicate that for both growth and value stocks, the risk premium is the dominant channel through which monetary policy is transmitted.[2] Furthermore, this effect is more pronounced in growth stocks, which explains their increased sensitivity to monetary policy shocks.

To understand the source of this result, I analyze the role of each state variable in the VAR and their sensitivity to monetary policy. This analysis reveals that shocks to the risk premium are more pronounced for growth stocks for two main reasons. First, the dividend yields of growth stocks exhibit superior predictive power for their future excess returns, indicating that discount rates more accurately capture the price fluctuations of growth stocks. Golez and Koudijs (2023) show that the predictability of excess returns via dividend yields is closely tied to stocks' cash flow duration. In their model, higher cash flow duration enhances the predictability of excess returns due to the greater persistence of expected returns relative to dividend growth and the higher variance of shocks to expected returns. Second, following a tightening surprise, dividend yields increase more sharply for growth stocks than for value stocks. If financial constraints were the main driver, dividends would likely decrease, potentially leading to a decrease in dividend yields. Together, these two observations provide fur-

[2] The finding that the risk premium is the main transmission channel of monetary policy to financial markets is consistent with previous research, including Bernanke and Kuttner (2005) and Bauer et al. (2023).

ther evidence that cash flow duration is the primary driver of differences in sensitivity between growth and value stocks.

What do these empirical findings reveal about the fundamental relationship between growth and value stocks, particularly regarding the value premium? To explain these new empirical findings in a conceptual framework, I build on the reduced-form asset pricing model from Lettau and Wachter (2011). The model implies that firms' heterogeneity is generated solely by the timing of the cash flow payment, which is a perfect setting to study the duration effects of monetary policy shocks. I follow the modeling strategy of Pflueger and Rinaldi (2022) and include high-frequency monetary policy shocks in a quarterly frequency framework, with policy surprises arising at the end of each quarter. In the model, firms with higher price-dividend ratio have longer cash flow duration and so are more exposed to shocks on discount rates, the main driving channel of monetary policy to stock returns. By simulating the model, I demonstrate that it matches the empirical difference in sensitivity between growth and value stocks well. The difference in response to monetary policy for the growth portfolio decile is as high as 8 percentage points relative to the value decile. The value premium, however, remains largely unchanged compared to the model without monetary policy. This is because monetary policy shocks, by definition, have a mean of zero and a variance that is significantly lower compared to quarterly shocks. As a result, while monetary policy exerts short-term effects on growth and value stocks, it has negligible long-term impact, leaving the value premium unaffected.

This paper contributes mainly to the literature on the effects of monetary policy on stock returns, highlighting the importance of cash flow duration in the transmission of monetary policy to the cross-section of stock returns. Ozdagli (2018) also provides evidence that high duration stocks are more sensitive to monetary policy, but he does not link these results to market-to-book equity. Furthermore, he only provides evidence using the cash flow duration from Weber (2018), which presupposes a constant discount rate in time and firms. This approach implies that any observed variation in duration is driven solely by changes in expected cash flows. Consequently, his empirical analysis possibly underestimates the role of duration in the heterogeneous response of stock returns to monetary policy. The paper of Chen (2022) is closely related to mine. He uses high- frequency monetary policy identification to create a measure of effective equity duration by considering, not only discount rate effects, but also cash flow effects. The main difference, however, is that I am interested in the economic channels of the policy surprises and how these effect the cross-section of stock returns.

My paper also contributes to the vast literature of studies of equity duration (Cornell, 1999; Dechow et al., 2004; Da, 2009; Weber, 2018; Gonçalves, 2021; Chen, 2022; Gormsen and Lazarus, 2023). I use the methods from Gonçalves (2021), who uses equity duration to document new evidence on the short duration premium, Gormsen and Lazarus (2023), who show that cash flow duration is able to explain, among other, the value premium, and from Weber (2018), whose paper also shows that low duration firms have higher returns, with this differences depending on investor sentiment. Golez and Matthies (2022) investigate the effects of monetary policy on the term structure of equity using price of European options on the S&P 500 index. In line with my results, they find that long maturity stocks rise more than short-term stocks after an expansionary monetary policy surprise. However, they also find that short-term stocks increase following a tightening surprise due to central bank information effects. For the conceptual framework I build upon the model from Lettau and Wachter (2011), who developed a model to explain the term structure of equity and interest rates jointly. There is a vast amount of models proposed to explain the equity term structure and equity duration. For a great overview of the literature I refer to Van Binsbergen and Koijen (2017).

This paper is organized as follows: Section 5.1 explains our different data sources and presents summary statistics. Section 2.3 explores the sensitivity of growth and value stocks to monetary policy. Section 2.4 investigates the role of cash flow duration and financial constraints as driving mechanisms on a firm-level analysis. Section 2.5 shows the results of the stock return decomposition. Section 2.6 presents the duration based asset pricing model. Section 6.6 concludes.

2.2 Data and Summary Statistics

2.2.1 High-Frequency Monetary Policy Surprises

My analysis begins with a dataset of FOMC announcements spanning from February 1990 to December 2018, encompassing 255 announcements, including 23 that were unscheduled. To identify causal links between monetary policy and stock returns I follow the high-frequency event study literature (Kuttner, 2001; Gurkaynak et al., 2005; Nakamura and Steinsson, 2018b). In my baseline estimation, I focus on the 30-minute window around FOMC annoucements, capturing the changes in rates implied by the current-month and the three-months funds future contract, and the prices of eurodollars future contracts with maturity of up to a year and extracting their first principal component (Nakamura and Steinsson, 2018b). These discrete changes in rates can be associated with a monetary policy surprise, because the prices will only move provided the FOMC released unexpected information.

The use of interest rates instruments with longer maturities is crucial for the analysis during the zero lower bound. Despite the stagnant target rate in the U.S., monetary policy efficacy was preserved through forward guidance. Thus, my monetary policy surprise accounts for both, target and forward guidance shocks. The methodology for constructing the monetary policy surprise is outlined in the Appendix 2.7.

For additional robustness checks, I use a range of alternative monetary policy surprises. First, I analyze the effects of the target and path surprises proposed by Gurkaynak et al. (2005). The target surprise primarily captures short-term rate changes, while the path surprise reflects intermediate maturities influenced by the Fed's forward guidance on policy rates. A potential concern with monetary policy surprises is the presence of "information effects" — information released during FOMC announcements about the central bank's economic outlook. To address this, I incorporate the surprises from Jarociński and Karadi (2020), who use a Bayesian VAR framework to decompose high-frequency surprises into monetary policy and information shocks. Additionally, I use the shocks from Bauer and Swanson (2023a), who orthogonalize monetary policy surprises with respect to macroeconomic variables that precede FOMC announcements, mitigating concerns about ex ante correlations.

2.2.2 Firm-level Data

I construct an unbalanced panel dataset using quarterly accounting data from Compustat and stock prices from CRSP. The market-to-book equity is generated for each firm in each quarter based on the definition provided by Daniel and Titman (2006). To make sure that market participants include the market-to-book equity in their information set during the FOMC announcements, I lag it by one quarter. In addition to market-to-book equity, my analysis employs several control variables: Size, represented by the logarithm of total assets; book leverage, calculated as the ratio of total debt to total equity plus total debt; profitability, defined as the ratio of operating income before depreciation to total assets; sales growth; and betas, estimated for each firm from 1926 to 2018. For robustness checks, I construct alternative measures for defining growth and value stocks. These include: the price-to-earnings ratio, calculated as the company's closing price for the fiscal year divided by earnings per share excluding extraordinary items; the cash flow-to-price ratio, defined as operating income before depreciation divided by the company's closing price for the fiscal year; and dividend yield, represented by dividends per share divided by the company's closing price for the fiscal year. To mitigate the influence of outliers, I winsorize all variables at the top and bottom 1 percent. Table 2.1 presents the summary statistics for market-to-book equity and the control variables.

The dependent variable is the simple return computed using the closing prices of the FOMC announcement day and the closing price of the preceding day. Consistent with Fama and French (1993) I include stocks exchanged in the NYSE, AMEX, or NASDAQ that have a CRSP share code of 10 or 11. To ensure liquidity, stocks with a price less than $5 or a market capitalization less than $10 million are dropped (Daniel and Titman, 2006; Chava and Hsu, 2020). This yields a total of 512,741 data points with 9,096 different firms.

To investigate the effects of policy surprises on aggregate measures of stock returns, I extract daily prices for the S&P 500 Index, the Russell 1000, and the Russell Growth and Value Index from Bloomberg. The simple return is computed using the closing prices of the FOMC announcement day and the closing price of the preceding day. Finally, the data used for the Fama and French portfolio analysis was downloaded from Ken French's Website.

2.2.3 Cash flow duration

Following the literature, I define equity duration analogously to bond duration as introduced by Macaulay (1938). This definition captures the average maturity of cash flows associated with a given asset i at time t:

$$Dur_{i,t} = \sum_{n=1}^{\infty} \omega_{i,t}^{(n)} \cdot n \tag{2.1}$$

with

$$\omega_{i,t}^{(n)} = \frac{E_t[CF_{i,t+n}]/(1+r_{i,t})^n}{P_{i,t}} \tag{2.2}$$

r is the discount rate, CF the cash flow, and P the price. Calculating bond duration is straightforward because the stream of future cash flows is known and terminates at a predetermined maturity date. For equities, however, the situation is more complex. Equities have risky cash flows and no fixed maturity, making it necessary to make assumptions about the evolution of future cash flows and the behavior of risk-adjusted

Table 2.1: Summary statistics for firm-level data

Panel A: Main variables								
		Cash flow duration			Financial Constraint Index			
	M/B	G	G&L	W	SEB	KZ	WW	HP
Mean	2.76	57.07	15.01	15.13	−1.37	−2,002.32	−0.14	−2.57
SD	2.84	47.21	8.87	17.05	0.70	8,970.56	0.11	1.56
Panel B: Contemporaneous correlations								
M/B		0.24	0.27	0.52	−0.17	−0.08	0.07	0.07
G			0.15	0.19	0.07	0.00	0.01	−0.03
G&L				0.27	−0.03	0.17	0.44	0.42
W					−0.25	−0.11	0.01	0.03
SEB						0.17	0.25	0.15
KZ							0.38	0.33
WW								0.72
Panel C: Control variables								
	Size	Leverage	Profitability	Beta	Sales growth	Price-earnings	Div. yields	Cash flow-price
Mean	2.42	0.40	2.76	1.10	4.52	16.13	0.34	2.37
SD	1.85	0.24	2.97	0.64	22.12	36.43	0.47	3.51

This table presents the mean and standard deviation of firm-level accounting variables for observations spanning January 1990 to December 2018. Panel A reports statistics for market-to-book equity, the three duration measures, and the four financial constraint indexes. Specifically, G represents the cash flow duration measure from Gonçalves (2021), G&L is from Gormsen and Lazarus (2023), and W is from Weber (2018). The financial constraint indexes include the SEB index from Schauer et al. (2019), the KZ index from Kaplan and Zingales (1997), the WW index from Whited and Wu (2006), and the HP index from Hadlock and Pierce (2010). Panel B reports the contemporaneous correlations between these variables, while Panel C provides statistics for additional variables used throughout the study.

discount rates. These assumptions lead to differing methods for estimating equity duration.

In this study, I use three distinct measures of cash flow duration. The first, developed by Gonçalves (2021), provides the most elaborate and complete duration measure, which incorporates both firm-level and time variation in expected cash flows and discount rates. This approach estimates future cash flows using accounting proxies for firm payouts, based on the assumption that book equity, return on equity, and other firm-level characteristics follow a VAR(1) process. Since the current market value of the firm is observed, this method enables the numerical solution of firm-specific discount rates using the estimated cash flows. Weber (2018) proposed a similar approach, assuming that return on equity and book equity growth follow an AR(1) process. Unlike Gonçalves (2021), this method assumes a constant discount rate across time and firms. While this simplifies the estimation process, it sacrifices the ability to capture firm-specific variation in discount rates, leading to a less granular measure of cash flow duration. Finally, Gormsen and Lazarus (2023) use a fundamentally different methodology, relying on analysts' forecasts for long-term earnings growth obtained from the Institutional Bankers' Estimate System (I/B/E/S) database. This method focuses solely on changes in cash flow growth. Its primary advantage is that it is model-free, meaning that it reflects actual market expectations of dividends rather than forecasts derived from a specific model.

Table 2.1 presents the equity duration measures. The measure proposed by Gonçalves

(2021) implies a substantially higher average duration compared to the other two.[3] Additionally, the duration measures are all positively correlated with one another and with market-to-book equity, consistent with the notion that growth stocks exhibit higher duration.

2.2.4 Financial Constraints

Identifying financial frictions is challenging, as accounting variables often reflect both fundamental and financial characteristics. A prominent study in this area is Fazzari et al. (1988), who argue that the sensitivity of investment to cash flow serves as a good proxy for financial constraints. The intuition is that firms with access to external funding can compensate a drop in internal funding with external sources. Consequently, firms whose investment is more sensitive to cash flow are presumed to be more financially constrained. This approach, however, was criticized by Kaplan and Zingales (1997), who argued that while investment-cash flow sensitivity might indicate financial constraints, it is incorrect to assume that greater sensitivity necessarily implies a higher degree of financial constraint. In response, they proposed the Kaplan-Zingales Index, which incorporates cash flow, leverage, dividend payments, and cash holdings to capture financial constraints more comprehensively. Over time, several alternative financial constraint indexes have been developed, addressing limitations in the Kaplan-Zingales Index and others. For instance, Whited and Wu (2006) introduced the Whited-Wu (WW) Index, which adds further accounting variables like size and sales growth. Similarly, Hadlock and Pierce (2010) created the Hadlock-Pierce (HP) Index, which relies solely on firm size and age. More recently, Schauer et al. (2019) proposed the Schauer-Elsa-Breitkopf (SEB) Index, which performs well not only for publicly traded stocks but also for private firms. This index incorporates size, interest coverage, return on assets, and cash holdings. In line with the literature on financial constraints, I employ these four indexes (Ozdagli, 2018; Gürkaynak et al., 2022). In particular, I follow Farre-Mensa and Ljungqvist (2016) and Ozdagli (2018) to construct the KZ, WW, and HP indexes, while the SEB Index is constructed according to the methodology described in Schauer et al. (2019). Table 2.1 presents summary statistics. The values of the financial constraint indexes vary significantly, with the KZ index being notably larger than the others. The correlations with market-to-book equity yield mixed results. The SEB and KZ indexes are negatively correlated with market-to-book equity, suggesting that growth stocks are less financially constrained. In contrast, the WW and HP indexes exhibit positive correlations, indicating the opposite.

In addition to using the financial constraint indexes as continuous variables, I follow the literature by categorizing firms into two groups: constrained and unconstrained. Specifically, for each index, I construct a dummy variable that equals 1 if a firm's financial constraint index value is above the sample median for a given year, and 0 otherwise.

[3] The cash flow duration proposed by Gormsen and Lazarus (2023) represents long-term earnings growth and is therefore not expressed in years.

2.3 Sensitivity of Growth and Value Stocks

2.3.1 Daily Responses

Index-Level Analysis

This section focuses on the effects of policy surprises on the aggregate stock market returns. Table 2.2 shows the regression results of the stock returns on the monetary policy surprise. As the two first columns are proxies for the aggregate market, they revisit the results documented by previous works, such as Gurkaynak et al. (2005), Bernanke and Kuttner (2005), Nakamura and Steinsson (2018b), and Gürkaynak et al. (2022). I document statistically significant negative effects of monetary policy surprises on returns: Stock returns decrease around 9.4 percentage points after a one percentage point tightening surprise. The estimated effect can vary in comparison to other studies, because the sample period and the surprises are not exactly the same. For example, Bernanke and Kuttner (2005) document for the period between 1989 and 2002 a drop of around 4.7 percentage points after a one percentage point tightening surprise. However, their surprise measure does not account for forward guidance.

To analyze the policy surprise effects on aggregate growth and value stocks, I use the Russell Value and Growth 1000 Indexes. The last two columns of Table 2.2 show the results of the one-day returns regression on monetary policy surprises. The estimated coefficients indicate a higher response of growth stocks relative to value stocks. The Russell Growth Index falls by around 12 percentage points after a one percentage point increase in monetary policy surprises, 4 percentage points more than the Russell Value Index. However, as column (5) shows the difference in response of the one-day return is not statistically significant.

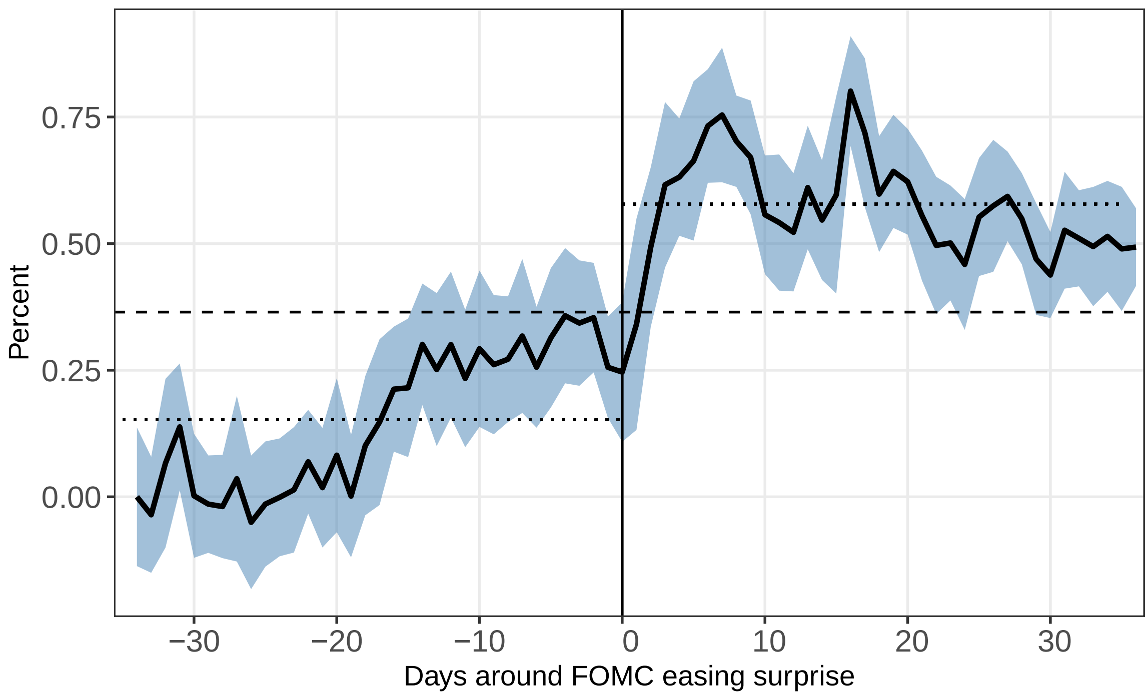
Table 2.2: Reaction of stock indexes to monetary policy surprises

	S&P 500 (Jan-90 - Dec-18)	Russell 1000 (Jan-90 - Dec-18)	Russell Value (Jan-91 - Dec-18)	Russell Growth (Jan-91 - Dec-18)	Growth - Value (Jan-91 - Dec-18)
mps	-9.54^{***}	-9.58^{***}	-7.71^{***}	-11.98^{***}	-4.27
	(2.42)	(2.45)	(2.37)	(3.34)	(2.73)
Constant	0.21^{***}	0.21^{***}	0.22^{***}	0.24^{***}	0.02
	(0.07)	(0.07)	(0.07)	(0.08)	(0.04)
N	259	259	248	248	248
R^2	0.11	0.11	0.08	0.14	0.05

The figure shows the regression of the 1-day stock returns on monetary policy surprises. The sample spans January 1990 to December 2018. ***, ** and * denote significance at the 1%, 5% and 10% level, respectively.

To assess the economic significance of the disparity between growth and value indexes independently of the magnitude of monetary policy surprises, Figure 2.2 presents the average performance of the growth-value spread, as measured by the Russell Indexes, around all FOMC easing surprises. This analysis spans 35 days before and after each easing event. The dotted lines in the figure represent the average cumulative returns during these periods. In the days leading up to the FOMC announcement, the mean spread hovers around zero, showing a slight positive trend two weeks prior to the easing surprise.[4] On the day of the announcement, the average growth-value spread increases by 10 basis points, and this pattern exceeds 50 basis points within six days post-announcement. The cumulative difference in average returns reaches 42 basis points, highlighting a significant post-easing overperformance of growth stocks.

Figure 2.2: Average performance of growth-value spread around expansionary surprises



The figure illustrates the performance of the growth-value spread, defined as the differential performance between the growth and value indexes, over a 35-day period surrounding an expansionary policy surprise. These policy surprises are generated following Nakamura and Steinsson (2018b). The figure features dotted lines to depict the average levels preceding and following the policy announcement, while the dashed line represents the overall average during the entire period.

Panel Regression

In this section I evaluate the response of growth and value firms to monetary policy. To ensure that time- and cross-section varying variables are not a concern, I include time and firms fixed effects in the regression. The estimated model is:

$$r_{t,i} = \beta_0 + \beta_1 \times mps_t + \beta_2 \times M/B_{i,t-1} + \beta_3 \times mps_t \times M/B_{i,t-1} + \gamma_i + \alpha_t + \varepsilon_{i,t}$$

where i denotes the firm, t the day of the FOMC announcement, r the stock return, mps the monetary policy surprise, M/B the market-to-book equity, and γ and α the fixed effects.

[4]The return spread exhibits a modest improvement approximately two weeks prior to the easing surprise. I leave the analysis of the statistical significance of this observation and the underlying reasons for future research.

Table 2.3 shows the results of the panel regressions with different specification designs. In the first column, I analyze the impact of policy surprises on individual stock returns. I find that a one percentage point increase in monetary policy decreases prices, ceteris paribus, on average 7.6%. The interaction effect of market-to-book equity and monetary policy surprise is statistically significant and remains robust when accounting for firm-specific and time-specific fixed effects. For each additional unit of market-to-book equity, the responsiveness of stock returns to policy surprises increases by up to 1.06 percentage points. Consequently, a firm with an average market-to-book equity experiences an average stock price decline of 7.4% following a one percentage point rise in policy surprises. Likewise, a firm with market-to-book equity one standard deviation above the mean witnesses an average stock price decrease of 10.2%. These figures underscore the economic relevance of these findings.

Table 2.3: Reaction of stock returns to monetary policy surprises and market-to-book equity

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
mps	-7.51^{***}	-4.87^{**}	-4.77^{**}				5.30^{***}	0.94
	(2.10)	(2.02)	(1.96)				(1.83)	(1.91)
M/B		0.0000	-0.02^{*}	0.002	-0.02^{*}	-0.01	-0.04^{***}	-0.01
		(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
M/B*mps		-0.98^{***}	-1.07^{***}	-0.77^{***}	-0.87^{***}	-0.73^{***}	-0.51^{**}	-0.63^{***}
		(0.34)	(0.32)	(0.30)	(0.28)	(0.22)	(0.20)	(0.19)
Constant	0.24^{***}	0.25^{***}						
	(0.06)	(0.06)						
N	491,399	491,399	491,399	491,399	491,399	448,156	491,397	448,156
R^2	0.004	0.004	0.15	0.04	0.19	0.45	0.82	0.87
Firms FE			✓		✓	✓	✓	✓
Time FE				✓	✓	✓		
Controls						✓		✓
Abnormal Returns							✓	✓

The table estimates the regression $r_{t,i} = \beta_0 + \beta_1 \times mps_t + \beta_2 \times M/B_{i,t-1} + \beta_3 \times mps_t \times M/B_{i,t-1} + \gamma_i + \alpha_t + \varepsilon_{i,t}$ using observations from January 1990 to December 2018. mps stands for monetary policy surprise and M/B for market-to-book equity. Column (1) regresses returns on monetary policy surprises, columns (2) to (5) estimate the regression model using pooled OLS and different fixed effects specifications. Column (6) and (8) control for size, profitability, book leverage, revenue growth, market beta, and their interaction with monetary policy. Column (7) and (8) use beta-adjusted returns. Two-way clustered standard errors are reported in parentheses. ***, ** and * denote significance at the 1%, 5% and 10% level, respectively.

In column (7), I revisit the regression using CAPM-adjusted stock returns, as suggested by MacKinlay (1997). The interaction term is still negative and significant, yet it decreases in magnitude. The high R^2 indicates that the market-to-book equity explains more than three quarters of the firm-level variation in daily stock returns that is not accounted for in the CAPM.

To ensure the robustness of my findings, I re-ran the analysis incorporating controls for firm size, profitability, market leverage, sales growth, and market betas. The results in columns (6) and (8) confirm that the observed increased sensitivity of growth stocks cannot be attributed to these firm-level characteristics. Although Savor and Wilson (2014) have shown that the CAPM performs well during FOMC announcements, these findings reveal that the increased sensitivity of growth stocks to monetary policy is a distinct phenomenon, not explained by the market exposure.

To ensure the robustness of my findings, I conducted several additional checks. First, to account for specific macroeconomic events, I repeated the regression while including dummy variables for the dotcom period and the zero lower bound, both of which occur within the sample period. As shown in Table 2.12 in the Appendix, the coefficient on the interaction term between market-to-book equity and monetary policy surprises remains statistically significant. Second, I tested the results using alternative definitions of growth and value stocks. Specifically, I categorized stocks based on their cash flow-to-price ratios, earnings-price ratio, and dividend yields rather than market-to-book equity. Table 2.13 demonstrates that the finding — that growth stocks respond more strongly to monetary policy — holds under these alternative definitions. Third, I examined the robustness of the results to different measures of monetary policy surprises. Table 2.14 shows that growth stocks are significantly more affected than value stocks by the target surprise measure introduced by Gurkaynak et al. (2005). Additionally, I considered two extensions of monetary policy surprise measures proposed by Jarociński and Karadi (2020) and Bauer and Swanson (2023a). As shown in Columns (4) and (5) in Table 2.14, growth stocks remain significantly more responsive with respect to both of these refined measures of monetary policy surprises.[5]

Portfolio-Level Results

Running OLS regressions is useful for several reasons; for instance, the interpretation of the results are clear and intuitive, and the inclusion of firm-level controls is straightforward. However, several studies advocate the use portfolio sorts when working with cross-sectional stock returns, because portfolios are less susceptible to idiosyncratic noise (Cochrane, 2009). Also, portfolio sorts enable to discover the presence of nonlinear effects. I group the firm-level sample in 10 equal-size portfolios sorted by their lagged market-to-book equity in each quarter. For each portfolio I estimate the average daily raw return in each FOMC announcement day.

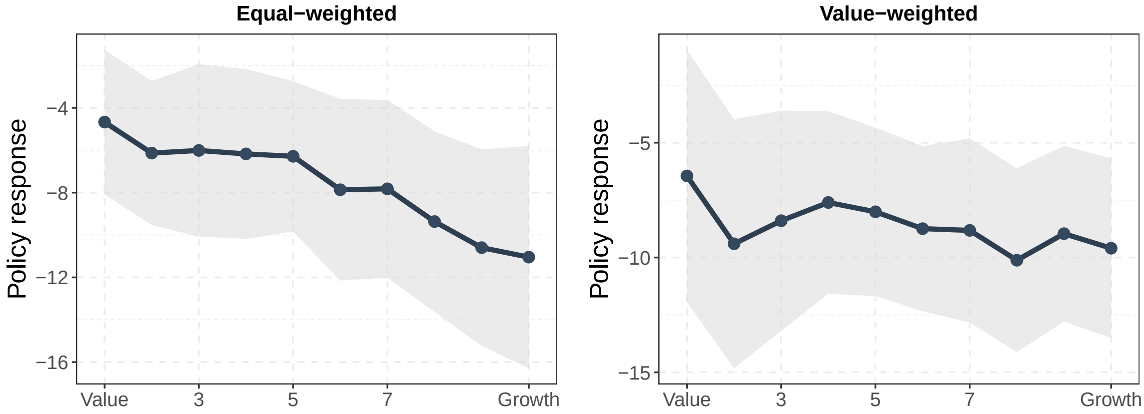
Figure 2.3 shows the estimated responses of each decile portfolio for equal- and value-weighted portfolio returns. Both panels confirm that the surprise response decreases with market-to-book ratio. Specifically, for the equal-weighted portfolio sorts, the decile with the lowest mean market-to-book equity experiences an average loss of 4.3 percentage points, whereas the portfolio with the highest mean market-to-book equity loses, on average, 11.4 percentage points. The 10th value-weighted portfolio decile drops on average 9.6 percentage points, 3.2 percentage points more than the first decile. Both portfolio sorts confirm the higher sensitivity of growth stocks to monetary policy.

To test whether the reactions of portfolios with higher market-to-book equity are significantly larger in magnitude, I estimate the return of spread portfolios from the equal-weighted portfolios and regress them on the policy surprises. Spread returns are constructed by subtracting the returns of the lowest quantile from the highest. For

[5] The interaction between market-to-book equity and the path surprise is not statistically significant. This could be because of the limited effectiveness of forward guidance. Notably, the interaction of market-to-book ratio and monetary policy becomes significant when including a dummy for the zero lower bound.

example, the 30% spread portfolio is the return of a portfolio long on all stocks in the three highest deciles and short on the stocks in the three lowest deciles.

Figure 2.3: Reaction of market-to-book equity sorted portfolios to monetary policy



The figure shows the estimated policy response of the 10 decile portfolios sorted by market-to-book equity. 10% confidence intervals are drawn around the point estimation. The sample spans January 1990 to December 2018.

Table 2.4 shows the regression of the spread portfolios on monetary policy surprises. Columns (1) to (3) show that the 10%, 30% and 50% spread portfolios are in line with the panel regressions: The portfolios with relatively higher average market-to-book equity drop significantly more after a monetary policy tightening surprise. The last two columns demonstrate that the results are not solely driven by a small extreme sample. A portfolio with stocks in the highest 10% spectrum of market-to-book equity (the stocks which are the closest to the growth extremity) react significantly stronger than all others. Likewise, a portfolio with stocks in the lowest 10% spectrum of market-to-book equity (the stocks which are the closest to the value extremity) react significantly less to monetary policy surprises. Figure 2.9 and Table 2.15 in Appendix show the results of the same portfolio analysis using Fama and French portfolios. The results are in line with the panel regression and the portfolio sorts, providing evidence that these findings are independent of the sample construction.

Figure 2.3 highlights the more pronounced sensitivity of growth stocks in equal-weighted portfolios compared to their value-weighted counterparts. The key distinction between equal and value-weighted portfolios lies in their weighting criteria, as value-weighted portfolios allocate greater weights to larger firms. Consequently, it seems reasonable to conjecture that smaller firms are more susceptible to these effects than their larger counterparts. To investigate this hypothesis, I run firm-level regressions of stock returns from both small and large firms on monetary policy surprises. The detailed findings are presented in Table 2.16 in Appendix. Notably, the reaction of smaller firms is nearly double that of larger firms, confirming that the higher sensitivity of growth stocks is more pronounced for small firms. This observation aligns with expectations, considering the previously documented stronger value premium in smaller firm (Fama and French, 1995).

Table 2.16 also shows that the coefficients of the regression including only the S&P 500 constituents is very similar to the one using only big firms. This parallel partially

Table 2.4: Reaction of spread portfolios to monetary policy surprises

	10% − 10%	30% − 30%	50% − 50%	90% − 10%	10% − 90%
mps	-7.11^{***}	-4.66^{**}	-3.29^{**}	-3.70^{***}	-4.20^{**}
	(2.60)	(1.90)	(1.41)	(1.15)	(1.93)
Constant	-0.11^{*}	−0.05	−0.03	-0.11^{**}	−0.01
	(0.06)	(0.04)	(0.03)	(0.04)	(0.04)
N	255	255	255	255	255
R^2	0.08	0.08	0.08	0.05	0.07

The table estimates the regression $r_t^s = \alpha + \beta \times mps_t + \varepsilon_{i,t}$ using the sample from January 1990 to December 2018, where r_t^s is the return of the spread portfolio. The spread portfolios are formed by sorting firms according to the market-to-book ratio and subtracting the 50%, 30% and 10% lowest from the highest companies each period. The last two columns show the spread portfolio of the 90% highest companies and the 10% lowest and vice-versa. White standard errors are reported in parentheses. ***, ** and * denote significance at the 1%, 5% and 10% level, respectively.

explains why Ehrmann and Fratzscher (2004) got substantially weaker results, since they restricted their sample to the S&P 500 constituents. This analysis underscores the importance of extending stock return analysis beyond the realm of large stocks, especially in the context of monetary policy, as policy makers are interested in the effects of monetary policy on all stocks.

2.3.2 Results Based on Multiple Days

Monthly Analysis: Reconciling with Maio (2014)

Research in asset pricing is conducted to a great extent on a monthly basis. This might be required because of the methods used (for example VAR requires a periodic frequency) or because of data availability. Maio (2014) conducted a monthly analysis to investigate whether growth or value stocks react more to monetary policy. The conclusion on the first part of the paper, that growth stocks react stronger to monetary policy than value stocks, contradicts his findings. In order to reconcile my results with his, a study on a monthly frequency is needed. Instead of high-frequency monetary policy identification, Maio (2014) opt to use the monthly changes in federal funds rates as a monetary policy indicator. The main problem with this approach is that there will be confounding variables. Monetary policy is highly endogenous, because the Fed does its best to react to economic conditions. The same conditions that affect stock prices.

To revisit the results obtained by Maio (2014), I regress 10%, 20%, and 30% spread portfolios on the first difference of the federal funds rates.[6] Panel A from Table 2.5 presents the estimated coefficients. Analogous to his study I find a positive significant effect of the change in federal funds rates in the 10% and 20% spread portfolios, which implies that value stocks are more reactive to monetary policy. Panel B re-runs his results starting in 1990, the same period used in this paper and shows that returns responses from value portfolios are no longer significantly higher than the responses from growth portfolios. Thus, Maio (2014)'s results are sensitive to the sample choice.

[6] This analysis differs slightly from Maio (2014) who uses a second monetary policy indicator, but finds no significant coefficients and runs a Wald test instead of spread portfolio regressions.

Panel C shows that his statistically significant findings are present in the whole sample. This could mean that they are driven by the period antecedent the 90s, for example, the great inflation.

Table 2.5: Reaction of monthly spread portfolios to federal funds rates

Spread portfolio	10%	20%	30%
Panel A: Jul-1963 - Jun-2008			
ΔFFR	77.58**	54.07*	31.11
	(37.21)	(28.23)	(23.41)
Constant	−0.60***	−0.47***	−0.38***
	(0.18)	(0.14)	(0.11)
N	540	540	540
R^2	0.01	0.01	0.005
Panel B: Jan-1990 - Dec-2018			
ΔFFR	−62.11	−69.73	−116.91
	(173.05)	(128.15)	(111.61)
Constant	−0.09	−0.07	−0.03
	(0.25)	(0.19)	(0.15)
N	348	348	348
R^2	0.001	0.001	0.005
Panel C: Jul-1963 - Dec-2018			
ΔFFR	72.08**	50.34*	26.21
	(36.25)	(27.76)	(23.32)
Constant	−0.37**	−0.31**	−0.24**
	(0.18)	(0.13)	(0.11)
N	666	666	666
R^2	0.01	0.01	0.002

The table shows the estimated regression of spread portfolio returns on changes in federal funds rates (FFR). Panel A uses the same sample time period as Maio (2014). Panel B uses the same time period as the other results in this paper and Panel C includes all observations available. Columns (1) to (3) are the returns of the 10%, 20% and 30% spread portfolios, respectively. White standard errors are reported in parentheses. ***, ** and * denote significance at the 1%, 5% and 10% level, respectively.

Table 2.6 repeats the regressions from Table 2.5 using the exogenous monetary policy surprises. The policy surprises are aggregated by summing all surprises within a month. In case of no FOMC announcement within a month, the policy surprise is zero. I find negative, but insignificant effects of policy surprises on the spread portfolio returns. This result is not surprising given that decreasing the frequency of returns increases the noise in the regression, which is indicated by the very low R-squared.

In summary, the analysis conducted in this section shows that the conclusion that value stocks respond more to monetary policy is biased by the inherent endogeneity in federal fund rates. By addressing this endogeneity and adjusting the frequency of analysis, my findings reveal a more nuanced reality where growth stocks, in fact, exhibit a heightened sensitivity to changes in monetary policy.

Table 2.6: Reaction of monthly spread portfolios to monetary policy surprises

	10%	20%	30%
mps	−3.71	−3.35	−4.21
	(5.78)	(4.50)	(3.76)
Constant	−0.11	−0.08	−0.04
	(0.26)	(0.19)	(0.16)
N	348	348	348
R^2	0.001	0.001	0.003

The table shows the estimated regression $r_t^s = \alpha + \beta \times mps + \varepsilon_{i,t}$. r_t^s is the return of the spread portfolios which is calculated on a monthly basis using Fama and French portfolios. White standard errors are reported in parentheses. The sample goes from January 1990 to December 2018. Columns (1) to (3) are the returns of the 10%, 20% and 30% spread portfolios, respectively. ***, ** and * denote significance at the 1%, 5% and 10% level, respectively.

Dynamic Response of Stock Returns to Policy Surprises

The analysis using a monthly frequency raises the question how long do the different policy responses of stock returns last for. To answer this question I regress the spread returns k-days ahead of the FOMC announcement on the policy surprise:

$$r^s_{t\to k} = \alpha + \beta \times mps_t + \varepsilon_t$$

Figure 2.4 shows the estimated dynamic response of returns to policy surprises up to 30 days, after the FOMC announcement as well as 95% confidence intervals. According to Panel A, which uses both Russell Indexes, the spread return does not respond to monetary policy in the day of the announcement. The distinct responses of the Russell Growth and Value Indexes becomes significant only after three days. Yet, the response is persistent and its significance lasts more than two weeks. Even after 30 days the response is negative, although due to a higher noise, it is not statistically significant to 5% significance level.

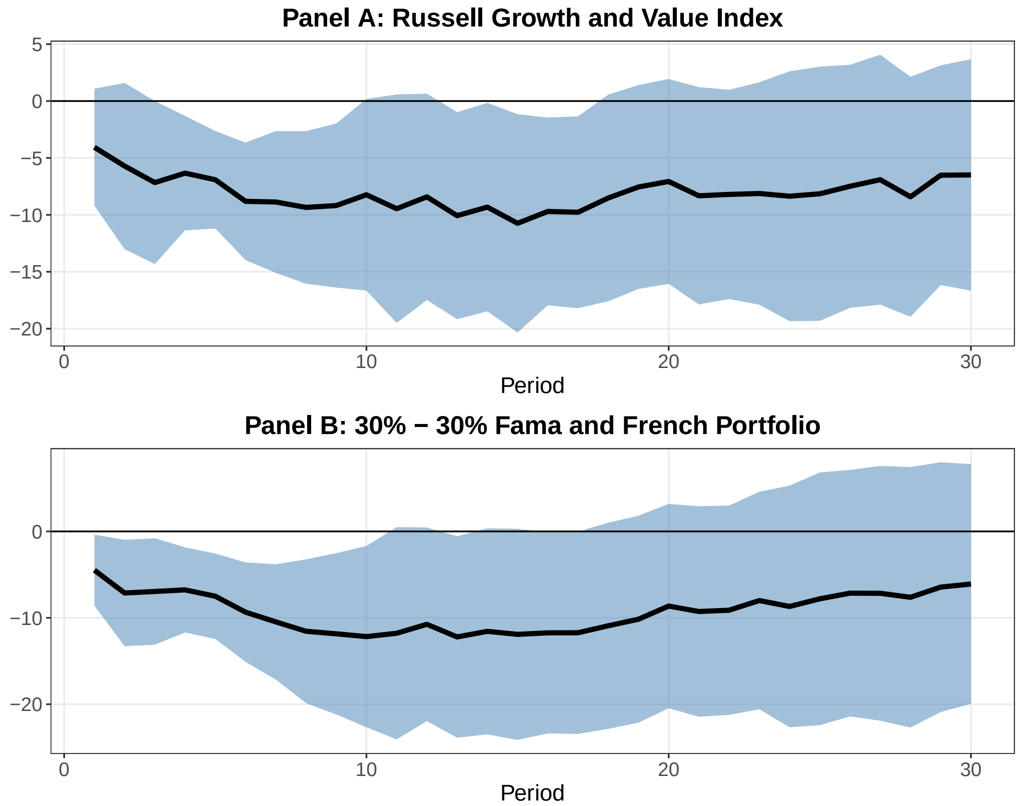
The 30% spread portfolio return is from the start on significant and the effects lasts more than 10 days. The difference in response of growth stocks can reach more than 10% in magnitude. Both panels agree upon the fact that the policy response intensifies at the first, reaching its lowest level (around -10%) 5 to 10 days after the policy surprise. This implies that the market reacts with a lag and that investors might need time to price the policy surprise.

2.4 Explaining the Monetary Policy Sensitivity: Duration and Financial Constraints

2.4.1 Cash Flow Duration

The strong evidence of growth stocks' increased sensitivity to monetary policy raises an important question: what economic mechanism drives this phenomenon? A natural explanation lies in cash flow duration, as growth stocks typically have dividend payments farther in the future, as previously discussed. To investigate this, I estimate

Figure 2.4: Dynamic responses of spread portfolios



Panel A plots the reaction of k-days ahead returns from the Russell Growth Index minus the Value Index to monetary policy surprise, where k goes from 1 to 30. Panel B repeats the analysis for the 30% spread portfolio using Fama and French data. 95% confidence intervals are plotted in blue. The sample goes from January 1990 to December 2018.

a fixed effects regression analogous to the analysis in Section 2.3.1, but with cash flow duration included as an additional control variable:

$$r_{t,i} = \beta_0+\beta_1\times mps_t+\beta_2\times M/B_{i,t-1}+\beta_3\times mps_t\times M/B_{i,t-1}+\beta_3\times mps_t\times Dur_{i,t-1}+\gamma_i+\alpha_t+\varepsilon_{i,t}$$

I use three distinct measures of cash flow duration from Gonçalves (2021), Gormsen and Lazarus (2023), and Weber (2018). Due to the highly skewed distribution of duration, I use the natural logarithm of duration in the regression. Additional control variables include size, leverage, profitability, sales growth, and market beta. Panel A of Table 2.7 presents the regression results of daily stock returns on monetary policy, market-to-book equity, and cash flow duration. Column (1) revisits the effects of monetary policy as a function of market-to-book equity. Since including duration reduces the sample size, Column (2) demonstrates that the coefficient for market-to-book equity remains statistically significant when the sample is restricted to observations with available duration data. The regression results confirm the proposition that stocks with higher cash flow duration respond more strongly to monetary policy shocks due to the discounting effect. Specifically, Column (3) uses the duration measure from Gonçalves (2021) and finds that a 1% higher cash flow duration reduces stock returns by an additional 2.5 percentage points, ceteris paribus, following a 1 percentage point increase in monetary policy surprises. Column (4) uses the long-term earnings forecast from Gormsen and Lazarus (2023) to proxy cash flow duration, revealing an even stronger effect. Firms with 1% higher duration experience a 6.75 percentage point larger decline in stock prices on average after a positive monetary policy shock. Lastly, Column (5) incorporates the duration measure from Weber (2018), finding a smaller but still significant coefficient.

Panel A of Table 2.7 highlights a key result of this study: when cash flow duration is included in the regression, the coefficient on market-to-book equity becomes insignificant. This finding indicates that the heightened sensitivity of growth stocks to monetary policy is primarily driven by their higher cash flow duration. In other words, once differences in the timing of cash flow payments — along with other firm-level characteristics such as size and beta — are accounted for, monetary policy affects growth and value stocks, on average, equally.

These results can be interpreted as a double-sorting approach on market-to-book equity and cash flow duration, accounting for controls and fixed effects (Patozi, 2024). While this interpretation is plausible, directly constructing portfolio sorts provides a more intuitive and relaxes the linearity assumption from the linear regression. To implement this, I first create quintiles based on cash flow duration for each quarter. Within each duration quintile, firms are further sorted into five portfolios based on market-to-book equity. The return spread is then calculated by subtracting the returns of the portfolio with the lowest market-to-book equity from those of the portfolio with the highest within each duration quintile. This process generates five time-series of return spreads, one for each duration quintile, which are subsequently regressed on monetary policy surprises.

Panel B of Table 2.7 presents the results for each duration measure. A significant negative coefficient would suggest that growth stocks are more sensitive to monetary policy within the same duration quintile. However, all coefficients are statistically insignificant, indicating no difference in the sensitivity of growth and value stocks when duration is held constant. This finding further supports the conclusion that cash flow duration is the key driver of the differing sensitivities to monetary policy.

2.4.2 Financial Constraints

Another potential explanation for the sensitivity of growth and value stocks to monetary policy are financial constraints. The relation of financial constraints, monetary policy, and market-to-book equity is not as clear-cut as duration. First, empirical and theoretical studies offer conflicting conclusions on whether financially constrained firms should respond more or less to monetary policy (Ozdagli, 2018; Chava and Hsu, 2020).[7] Second, while growth firms may be more financially constrained due to their greater reliance on external funding, they might also benefit from better conditions in credit markets owing to their higher asset valuations (Ehrmann and Fratzscher, 2004).

To analyze whether financial constraints account for the sensitivity of growth and value stocks to monetary policy, I repeat the fixed effects regression and portfolio sorting with four widely used financial constraint indexes: SEB, KZ, WW, and HP index. For each index, I construct a dummy variable that equals 1 if the financial

[7] Ozdagli (2018) shows that the financial accelerator from Bernanke et al. (1999) implies that constrained firms should be less responsive to monetary policy. Other models, such as models with binding credit constraints like Kiyotaki and Moore (1997), imply that loosening monetary policy might lead the borrowing constraint to unbind. In this case, financially constrained firms should respond more to monetary policy.

Table 2.7: Panel regressions and portfolio sorts of market-book equity and cash flow duration

	(1)	(2)	(3)	(4)	(5)
Panel A					
M/B	−0.00	−0.00	−0.01	0.00	−0.001
	(0.01)	(0.01)	(0.01)	(0.01)	(0.00)
M/B*mps	-0.87^{***}	-0.44^{**}	−0.08	−0.33	-0.23^{*}
	(0.28)	(0.19)	(0.13)	(0.21)	(0.13)
Dur			0.01	-0.08^{***}	-0.03^{**}
			(0.02)	(0.03)	(0.01)
Dur*mps			-2.50^{***}	-6.75^{***}	-0.80^{**}
			(0.87)	(1.92)	(0.36)
N	491,399	307,488	307,488	201,395	353,564
R^2	0.15	0.16	0.16	0.21	0.16
	(1)	(2)	(3)	(4)	(5)
Panel B					
Gonçalves (2021)	−5.47	−2.85	−0.88	−1.68	0.87
	(3.42)	(1.95)	(1.85)	(1.95)	(2.36)
Gormsen and Lazarus (2023)	3.23	3.86	−2.43	−2.43	−4.23
	(3.12)	(3.16)	(2.20)	(2.07)	(3.04)
Weber (2018)	2.42	2.41	−3.87	−3.05	0.30
	(12.56)	(5.00)	(2.89)	(5.61)	(3.70)

This table reports the main results on the effects of monetary policy surprises (mps) on stock returns, conditional on market-to-book equity (M/B) and cash flow duration (Dur). Panel A presents the results of panel regressions where stock returns are regressed on mps, interacted with M/B and the log of cash flow duration, using three different definitions of cash flow duration. Column (1) uses the duration measure from Gonçalves (2021), column (2) from Gormsen and Lazarus (2023), and column (3) from Weber (2018). Control variables include size, profitability, book leverage, sales growth, market beta, and their interactions with monetary policy. All regressions incorporate firm and time fixed effects, with two-way clustered standard errors reported in parentheses. Panel B shows the results of regressions of spread returns on mps. Spread portfolios are constructed by double-sorting returns on M/B and cash flow duration. White standard errors are reported in parentheses. The observations span from January 1990 to December 2018. ***, ** and * denote significance at the 1%, 5% and 10% level, respectively.

constraint index value is above the median for a given year. Panel A of Table 2.8 presents the results, with column (1) using the SEB index, column (2) the KZ index, column (3) the WW index, and column (4) the HP index.

Among the four indexes, only the WW index is statistically significant. Moreover, its negative sign indicates that financially constrained firms are more sensitive to monetary policy — a finding consistent with Chava and Hsu (2020) and Cloyne et al. (2023). The coefficient suggests that the stock prices of financially constrained firms decline, on average, by 3.2 percentage points more than those of unconstrained firms following a monetary policy tightening. In particular, in all cases, the interaction between market-to-book equity and monetary policy surprises remains statistically significant. This finding suggests that, at least when using the financial constraint measures commonly used in the literature, financial constraints do not explain the differential sensitivity of growth and value stocks to monetary policy.

Panel B of Table 2.8 corroborates this result through a double-sorting analysis using financial constraint indexes and market-to-book equity. To be able to construct the quintiles, I use the continuous values of the financial constraint indexes instead of the dummies. The results show that for each quintile, at least one regression of the spread portfolio yields statistically significant negative coefficients. For example, among firms classified as highly financially constrained according to the WW and HP indexes, growth firms respond approximately 7 to 8 percentage points more than value firms to monetary policy. Similarly, growth firms deemed financially unconstrained based on the WW and SEB indexes still exhibit a statistically significant greater response to monetary policy compared to unconstrained value firms. This suggests that even among firms with similar degrees of financial constraint, growth stocks remain more sensitive to monetary policy than value stocks — an outcome that holds across various financial constraint indexes in the literature.

Table 2.8: Panel regressions and portfolio sorts of market-book equity and financial constraint indexes

	(1)	(2)	(3)	(4)
Panel A				
M/B	0.00	−0.00	−0.00	−0.00
	(0.01)	(0.01)	(0.01)	(0.01)
M/B*mps	−0.58**	−0.68***	−0.78***	−0.79***
	(0.24)	(0.23)	(0.25)	(0.26)
FC	0.02	0.03	−0.03	0.02
	(0.02)	(0.02)	(0.02)	(0.03)
FC*mps	0.69	−1.52	−3.20***	−0.22
	(0.63)	(1.04)	(1.10)	(0.57)
N	304,464	372,549	411,314	432,128
R^2	0.15	0.15	0.15	0.15

	(1)	(2)	(3)	(4)	(5)
Panel B					
SEB	−6.82*	−1.30	−0.45	−3.64**	−1.82
	(3.42)	(1.95)	(1.85)	(1.95)	(2.36)
KZ	−0.73	2.13	−8.49*	−2.34	−2.93
	(2.45)	(1.89)	(4.43)	(4.73)	(3.10)
WW	−2.61*	−3.15	−6.12***	−5.33**	−7.39***
	(1.53)	(2.31)	(2.08)	(2.37)	(2.78)
HP	−2.10	−2.27	−6.30**	−8.15**	−8.38***
	(3.07)	(3.26)	(2.85)	(1.74)	(2.04)

This table reports the main results on the effects of monetary policy surprises (mps) on stock returns, conditional on market-to-book equity (M/B) and financial constraint (FC). Panel A presents the results of panel regressions where stock returns are regressed on mps, interacted with M/B and the financial constraints dummies. The dummy equals 1 if a firm's financial constraint index value is above the annual median. Column (1) uses the financial constraint index from Schauer et al. (2019), column (2) from Kaplan and Zingales (1997), column (3) from Whited and Wu (2006), and column (4) from Hadlock and Pierce (2010). Control variables include size, profitability, book leverage, sales growth, market beta, and their interactions with monetary policy. All regressions incorporate firm and time fixed effects, with two-way clustered standard errors reported in parentheses. Panel B shows the results of regressions of spread returns on mps. Spread portfolios are constructed by double-sorting returns on M/B and the financial constraint index. White standard errors are reported in parentheses. The observations span from January 1990 to December 2018. ***, ** and * denote significance at the 1%, 5% and 10% level, respectively.

2.4.3 Robustness results: Principal component analysis

The analysis above provides strong evidence for the key role of cash flow duration in the transmission of monetary policy to the cross-section of stock returns, while clearly rejecting financial constraints as an explanation for the differing sensitivity of growth and value stocks. Concerns regarding measurement issues are mitigated by the use of a variety of duration and financial constraint measures. However, the joint effects of duration and financial constraints have not yet been tested. Specifically, it is possible that the financial constraint indexes capture financial constraints only when duration is accounted for, and vice-versa. In such a case, the coefficients of these variables could shift substantially when controlling for each other. Alternatively, their combined inclusion could reveal that duration is not, in fact, the correct explanatory factor for the observed differences in sensitivity. To address this, I repeat the panel regression, incorporating measures of both duration and financial constraints simultaneously. In particular, I extract the first principal component of the three duration measures and four financial constraint indexes. Table 2.9 presents the fixed-effects regression results of stock returns on monetary policy interacted with market-to-book equity, the duration factor, and the financial constraint factor.

Columns (1) and (2) demonstrate that the previous findings remain robust when using the duration factor and financial constraint factor separately: while duration explains the sensitivity of growth and value stocks to monetary policy, financial constraints do not. Column (3) reports the results when controlling for both duration and financial constraints simultaneously. The coefficients for the duration and financial constraint factors remain largely unchanged, and market-to-book equity is only statistically significant when duration is included in the regression.

2.5 Policy Surprises and Stock Returns Decomposition

This section conducts a Campbell & Shiller decomposition to separate the excess return movements in risk premium, risk-free rate, and cash flow news. I use the news components to run a regression of excess return, cash flow, and real rate news of growth and value stocks on monetary policy surprises. To ensure robustness of the results I proceed with the decomposition analysis on the Russell Indexes and Fama and French portfolios. Due to the limitations of the decomposition to monthly data, I abdicate of a firm-level analysis because of the significant amount of noise.

2.5.1 Decomposing Stock Returns

Following the log-linearization of Campbell and Shiller (1988) and Campbell (1991) current stock price movements can be explained by revisions on future expected dividends, expected excess returns or real rates. Formally, the unexpected component of stock returns is given by following identity:

$$e^y_{t+1} = \tilde{e}^d_{t+1} - \tilde{e}^r_{t+1} - \tilde{e}^y_{t+1} \tag{2.3}$$

Table 2.9: Panel regression using principal components of duration and financial constraint

	(1)	(2)	(3)
M/B	0.00	0.00	0.00
	(0.01)	(0.01)	(0.01)
M/B*mps	-0.24	-0.61^{**}	-0.29
	(0.21)	(0.24)	(0.19)
Dur	-0.03^{*}		-0.02
	(0.01)		(0.02)
Dur*mps	-1.29^{**}		-1.26^{**}
	(0.62)		(0.58)
FC		0.01	0.01
		(0.01)	(0.01)
FC*mps		-1.90^{***}	-1.87^{***}
		(0.65)	(0.60)
N	126,586	298,986	117,956
R^2	0.20	0.15	0.21

This table reports the results on the effects of monetary policy surprises (mps) on stock returns, conditional on market-to-book equity (M/B), cash flow duration factor (Dur), and financial constraint factor (FC). The Dur and FC factors represent the first principal components of the cash flow duration measures and financial constraint indexes, respectively. Controls include size, profitability, book leverage, revenue growth, market beta, and their interaction with monetary policy. All regressions use time and firms fixed effects. Two-way clustered standard errors are reported in parentheses. The observations span from January 1990 to December 2018. ***, ** and * denote significance at the 1%, 5% and 10% level, respectively.

where

$$
\begin{aligned}
e^y_{t+1} &= (E_{t+1} - E_t) y_{t+1} \\
\tilde{e}^d_{t+1} &= (E_{t+1} - E_t) \sum_{j=0}^{\infty} \rho^j \Delta d_{t+1+j} \\
\tilde{e}^r_{t+1} &= (E_{t+1} - E_t) \sum_{j=0}^{\infty} \rho^j r_{t+1+j} \\
\tilde{e}^y_{t+1} &= (E_{t+1} - E_t) \sum_{j=1}^{\infty} \rho^j y_{t+1+j}
\end{aligned}
$$

d is the log-dividend, r the real rate and y the excess return. The log-linearization introduces ρ, which is the steady-state ratio of the equity price to the price plus dividend. Following Campbell and Ammer (1993) I set it to 0.9962. I use a VAR(1) to estimate the future changes in expectations. Let z_t be a vector of state variables, which include the expected returns and the real rates. Then:

$$z_{t+1} = A z_t + \varepsilon_{t+1} \tag{2.4}$$

Equation 2.4 enables to back up the news on expected excess returns, real rates, and current expected returns:

$$\begin{aligned} e^{y}_{t+1} &= s_y \varepsilon_{t+1} \\ \tilde{e}^{y}_{t+1} &= s_y \rho A (1-\rho A)^{-1} \varepsilon_{t+1} \\ \tilde{e}^{r}_{t+1} &= s_r (1-\rho A)^{-1} \varepsilon_{t+1} \end{aligned}$$

where s_y and s_r are selection matrices for y and r, respectively.

The news on future dividends are estimated as residuals of the identity:

$$\tilde{e}^{d}_{t+1} = e^{y}_{t+1} + \tilde{e}^{y}_{t+1} + \tilde{e}^{r}_{t+1}$$

I use a six variable state vector which include the excess equity return, the real interest rate (1-month Treasury bill adjusted by the CPI), the relative bill rate (the 3-month Treasury bill minus its 12-month lagged moving average), the change in the 3-month Treasury bill, the dividend price ratio, and the spread between the 10-year and 1-month Treasury yields (Campbell and Ammer, 1993; Bernanke and Kuttner, 2005; Maio, 2014). The estimation period is January 1990 to December 2018 and covers the whole period, in which the monetary policy surprises are available. Since the VAR specification requires a steady frequency, the model is estimated in a monthly frequency. Thus, I aggregate the monetary policy surprises by the sum of all surprises within each month.

An important caveat, pointed out by Chen and Zhao (2009), is that the decomposition will attribute too much weight to dividends, in case the VAR understates the predictability of expected returns. Consequently, caution is warranted, especially when the influence of the cash flow news is particularly pronounced. This is not the case in my analysis, as the main driver is indeed the discount rate news. Additionally, I incorporate the price-dividend ratio as a state variable, which according to Engsted et al. (2012) is essential for the decomposition to be valid. My main results are robust to different values of ρ and to including dividend growth as a state variable while identifying excess return news as residual.

To understand the effects of monetary policy on the news components, Bernanke and Kuttner (2005) include monetary policy surprises in the VAR:

$$z_{t+1} = A z_t + \Phi mps_{t+1} + \nu_{t+1} \tag{2.5}$$

Since monetary policy surprises and the lagged state variables are orthogonal, Bernanke and Kuttner (2005) estimate the regression above using a two-step estimation method. First, the dynamics of the first-order VAR are estimated without the policy surprise. In the second step, the residuals are regressed on the monetary policy surprises. The effects of monetary policy surprises are given as follows:

$$\begin{aligned} \eta_y &= s_y \Phi \\ \eta_r &= s_r (1-\rho A)^{-1} \Phi \\ \eta_{\tilde{y}} &= s_y \rho A (1-\rho A)^{-1} \Phi \\ \eta_d &= (s_y + s_r)(1-\rho A)^{-1} \Phi \end{aligned} \tag{2.6}$$

The first column of Table 2.10 shows the estimated responses of the S&P 500 Index. The results demonstrate that the risk premium is the main driver of monetary policy: Around three quarters of the response of the S&P 500 to monetary policy is significantly explained by changes in risk premium. Cash flow news and risk-free rate are less important. This outcome resembles the results of Bernanke and Kuttner (2005), even though they use a different policy surprise and a different sample period.

Table 2.10: Breakdown of monetary policy effects on unexpected excess returns

	S&P 500	Russell Value	Russell Growth
Current excess return	-20.78***	-17.48***	-24.62***
	(6.03)	(5.97)	(6.79)
Future excess returns	13.02***	8.35**	24.34***
	(4.74)	(3.76)	(9.03)
Real interest rate	1.99*	1.99*	1.99*
	(1.02)	(1.02)	(1.02)
Dividends	-5.77**	-7.14*	1.71
	(3.35)	(5.97)	(6.79)

The table estimates the impact of monetary policy surprises on the current unexpected excess return and its different components. I estimate a VAR(1) with the excess equity return, the real interest rate, the relative bill rate, the change in the 3-month bill rate, the dividend price ratio, and the spread between the 10-year and 1-month Treasury yields. The VAR is estimated from Jan-1990 to Dec-2018. Coefficients are estimated in two-steps. Standard errors are calculated using bootstrapping and are shown in parentheses. ***, ** and * denote significance at the 1%, 5% and 10% level, respectively.

2.5.2 Effects of Monetary Policy on Growth and Value News

To conduct the Campbell & Shiller decomposition on growth and value stocks I proceed akin to the S&P 500 decomposition. The state variables in the VAR remain the same except for the dividend yields and expected returns which are updated accordingly for the growth and value portfolios. As the dividend yields of the Russell Index go back only to 1995, I extrapolate the data to beginning of 1990 using the fitted value of a regression of the dividend yields of the S&P 500 on the dividend yields of both Russell indexes.[8] Moreover, I do not estimate the risk premium and interest rates separated, because real rates news are equal for all securities and dividend yields predictions from different portfolios should not yield different real rate revisions. To decompose discount rates news in risk premium and real rates I use the estimated real rate news from the S&P 500.

Columns (2) and (3) of Table 2.10 present the estimated reactions of the Russell Value Index and Growth Index. The data indicates that growth stocks respond stronger to policy surprises than the overall market does, whereas the reactions of value stocks are lower, aligning with previous findings. The transmissions of monetary policy to value and growth stocks differ considerably, as the response of growth stocks is explained solely by future excess return news. The Russell Value Index have a higher portion explained by cash flow news, although this portion is only marginally significant. These findings reinforce the notion that the discount rate news pertaining to growth stocks is more profoundly affected by monetary policy.

As the aggregate data might paint a limited picture, Figure 2.5 repeats the stock return decomposition for Fama and French portfolios sorted by market-to-book equity. The upper left figure shows that the response of the forecasting errors to monetary policy decreases with market-to-book equity. This indicates that stocks with higher market-to-book equity ratios experience more substantial price revisions. The magnitude of the response doubles from the first to the last decile. The discount rate news is typically positive and increases on the deciles, implying that the impact of monetary

[8] The results remain robust to starting the sample in 1995.

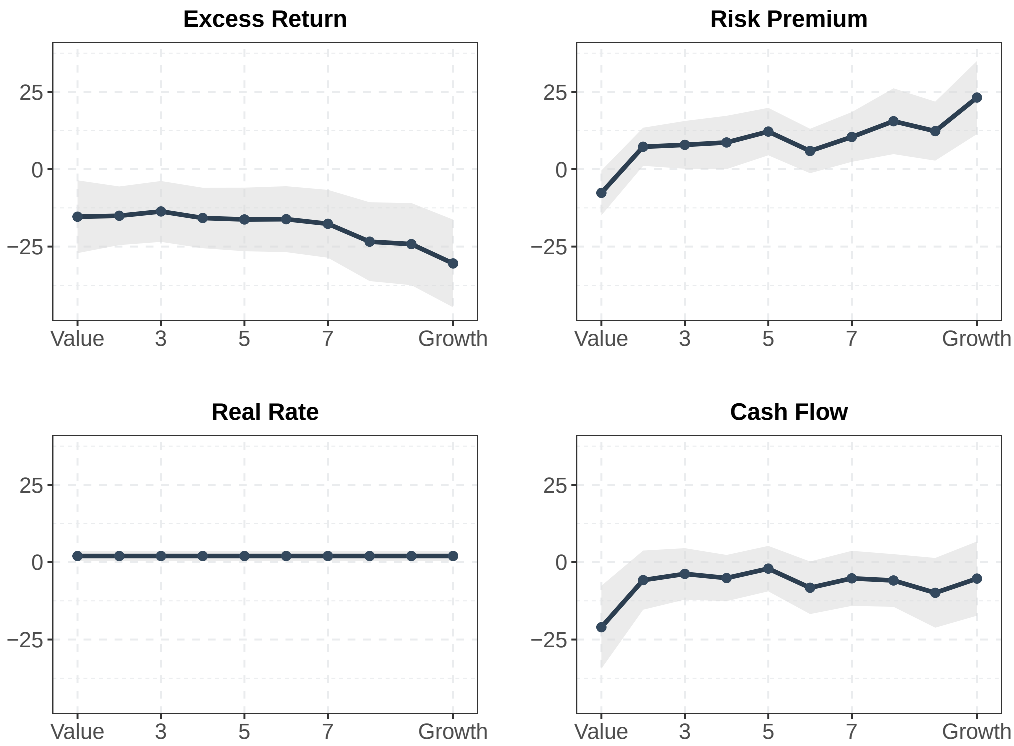
policy on revisions of risk premium is stronger for growth stocks. Cash flow news is slightly increasing in the lower deciles, but is mainly flat throughout the portfolio deciles and is not statistically significant. Overall, the portfolio sorts corroborates the findings from the index results and suggest that the disparate sensitivities observed across decile portfolios are primarily driven by discount rate news.

To provide evidence that my findings are robust, I repeat the Fama and French portfolios decompositions using (1) different values for ρ and (2) dividend growth as a state variable in the VAR while treating discount rate news as the residual component. As ρ is linked to the steady state price-dividend ratio, I estimate each portfolio's ρ using their mean price-dividend ratio. Figure 2.11 in Appendix shows that the results remain almost identical to Figure 2.5 when using portfolio-specific ρ. Because monthly dividend yields are relatively small compared to annual dividend yields, ρ is close to 1 for all portfolios. To estimate the decomposition with dividend growth I include dividend growth as a further state variable in the VAR. The results, depicted in Appendix Figure 2.12, again affirm the consistency of the findings. Because of the poor long-run forecastability of dividend growths, the discount rate news remain the primary driver of monetary policy responses, despite their identification as residuals. Hence, this robustness analysis substantiates the reliability of my findings, confirming that they stand resilient against potential criticisms of the Campbell & Shiller decomposition, such as those raised by Chen and Zhao (2009).

To elucidate the link between the observed risk premium results and the previously documented cash flow duration channel, I analyze the underlying patterns in the risk premium response to monetary policy. Notice that the long-run prediction of the excess return is given by the state variables. Monetary policy will affect the stock return predictions to the extend that it effects the state variables. Hence, it is possible to decompose the effects of monetary policy on future discount rates news into the effects of monetary policy surprises in each state variable. To elucidate this, recall from Equation 2.6 that the influence of monetary policy on risk premium news is quantified by $\eta_{\tilde{y}} = s_y \rho A(1-\rho A)^{-1} \cdot \Phi$. Here, the final asset response, $\eta_{\tilde{y}}$, is the sum of the product of two estimates: the contribution of each state variable to long-run forecastability of excess returns, represented by $s_y \rho A(1-\rho A)^{-1}$, and the impact of monetary policy on each state variable, denoted by Φ.

Figure 2.6 shows the product of the two estimates for each state variable together with the risk premium news across the ten decile portfolio. The figure reveals that the heterogeneous response of growth and value stocks is predominantly explained by dividend yields. This is not surprising considering that with the exception of excess returns and dividend yields, all other state variables remain the same when running the VAR for the growth and value portfolios. In addition, past excess returns are not good predictors of future returns. Consequently, the phenomena that risk premium of growth stocks exhibits greater sensitivity to policy surprises than value stocks can be attributed to two main factors: First, dividend yields of growth stocks demonstrate a superior ability in forecasting future excess returns. Second, the effect of monetary policy on dividend yields increases with market-to-book equity. Using the notation of the Campbell & Shiller decomposition, let $\nu = s_y \rho A(1-\rho A)^{-1}$ represent the vector of coefficients of the state variables on the sum of future expected returns, and let ν_{dp} denote the coefficient associated with dividend yields. Similarly, let ϕ_{dp} denote the effect of monetary policy on dividend yields. Then, $\phi_{dp} \times \nu_{dp}$ increases with market-

Figure 2.5: Response of unexpected excess returns to policy surprises using Fama and French



The figure shows the reaction of the news components of the unexpected excess returns for Fama and French portfolios. The portfolios are sorted from low to high market-to-book equity. I estimate a VAR(1) with the excess equity return, the real interest rate, the relative bill rate, the change in the 3-month bill rate, the dividend price ratio, and the spread between the 10-year and 1-month Treasury yields. The VAR is estimated from Jan-1980 to Dec-2018. The monetary policy surprises are monthly aggregated and go from the Jan-1990 to Dec-2018. Coefficients are estimated in two-steps. Standard errors are calculated using bootstrapping. 95% confidence intervals are drawn around the point estimation.

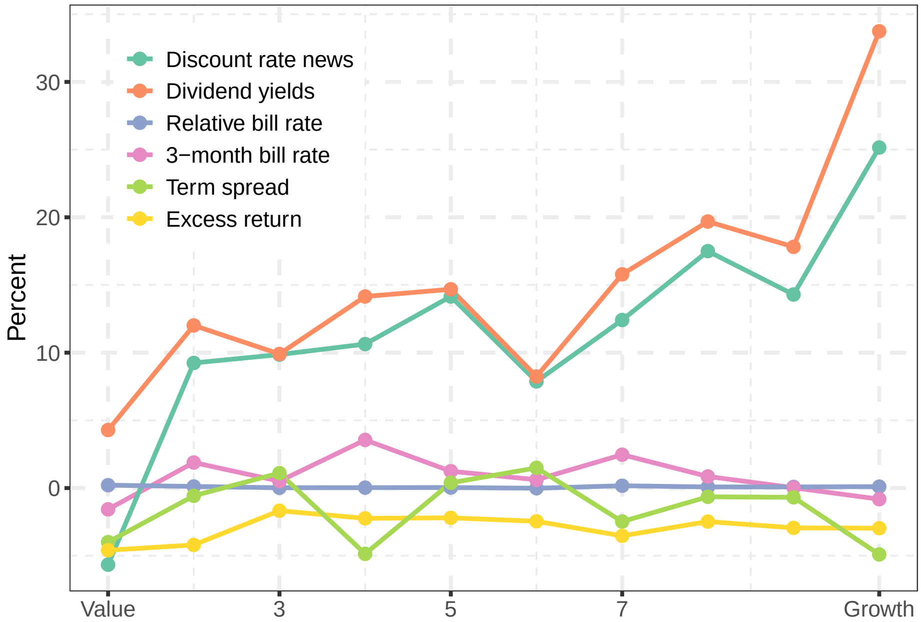
to-book equity, as both ν_{dp} and ϕ_{dp} individually increase with market-to-book equity.[9]

The fact that dividend yields are better predictors of growth stock returns is consistent with the duration effect. According to Golez and Koudijs (2023) discount rates become more important for asset price variation, the higher the duration. This consequently implies that dividend yields can better predict future long-run expected returns. The reason why discount rates increase in importance is two-folded: First, expected returns are more persistent than dividend growth rates. Consequently, when the duration increases, expected returns portions of variation will become larger relative to dividend growth. Second, the variance of future expected returns increases with duration. Maio and Santa-Clara (2015) also find that return predictability is more important for growth stocks and argue in favor of the duration effect. Furthermore, the increase in dividend yields following a tightening surprise indicates that

[9] Figure 2.10 in the Appendix decomposes the contribution of dividend yields to the policy response of future discount rates into (i) the predictability of dividend yields, ν_{dp}, and (ii) the covariance between dividend yields and monetary policy, ϕ_{dp}. Both components are shown to drive the stronger response of growth stocks to monetary policy.

prices are more sensitive than dividends in response to a monetary policy shock. If financial constraints played a significant role, one would expect dividends to decrease, potentially resulting in a decrease of dividend yields.

Figure 2.6: Dividend yields explains discount rate news



The figure shows the effects of monetary policy on the discount rates news on each of the 10 Fama and French decile portfolios. The other lines breaks down these effects on the policy response of each state variable of the VAR. The portfolios are sorted from low to high market-to-book equity. The VAR is estimated from Jan-1990 to Dec-2018.

2.6 Theoretical Framework

In this section I show how to reconcile my findings of value and growth stocks with a theoretical asset pricing model. This exercise serves two purposes: First, it demonstrates that a model that accounts solely for duration heterogeneity across firms can replicate my empirical results. Second, it shows that a model that encapsulates higher sensitivity of growth stocks to monetary policy is still in line with the existence of a value premium. In other words, the additional monetary policy dynamics do not hurt the model's ability to match quarterly stock market moments.

I start with a simplified version of the model from Lettau and Wachter (2011), which is the baseline asset pricing model on the behaviour of growth and value stocks, and models firms as portfolios of zero-coupon equities. Since inflation and nominal interest rates are not relevant to replicate my empirical findings, I do not model them explicitly. I then extend the model to account for monetary policy surprises.

The model starts with the dynamics of four different variables: Aggregate dividend growth, expected dividend growth, risk-free rate, and price of risk.

$$
\begin{aligned}
\Delta d_{t+1} &= z_t + \sigma_d \epsilon_{d,t+1} \\
z_{t+1} &= (1-\phi_z) g + \phi_z z_t + \sigma_z \epsilon_{z,t+1} \\
r^f_{t+1} &= (1-\phi_r)\bar{r}^f + \phi_r r^f_t + \sigma_r \epsilon_{r,t} \\
x_{t+1} &= (1-\phi_x)\bar{x} + \phi_x x_t + \sigma_x \epsilon_{x,t+1}
\end{aligned}
\tag{2.7}
$$

ϵ_t is a iid standard normal shocks.

Following Lettau and Wachter (2011) I assume that only fundamental dividend risk is priced. The stochastic discount factor is given by

$$M_{t+1} = \exp\left(-r^f_{t+1} - \frac{1}{2}x_t^2 - x_t \epsilon_{d,t+1}\right)$$

Let $P_t^{(n)}$ denote the time-t price of the asset that pays the aggregate dividend at time $t+n$ (from here on referred to as zero-coupon equity). Then, the log price-dividend ratio of a zero-coupon equity is affine on the state variables (see Lettau and Wachter (2011) for the complete derivation):

$$\frac{P_t^{(n)}}{D_t} = \exp\left(A^{(n)} + B_z^{(n)}(z_t - g) + B_r^{(n)}(r^f_{t+1} - \bar{r}^f) + B_x^{(n)}(x_t - \bar{x})\right) \tag{2.8}$$

with coefficients

$$B_z^{(n)} = \frac{1-\phi_z^n}{1-\phi_z} \qquad\qquad B_r^{(n)} = -\frac{1-\phi_r^n}{1-\phi_r}$$

and

$$
\begin{aligned}
B_x^{(n)} &= B_x^{(n-1)}\left(\phi_x - \rho_{dx}\sigma_x\right) - \sigma_d - B_z^{(n-1)}\rho_{dz}\sigma_z - B_r^{(n-1)}\rho_{dr}\sigma_r \\
A^{(n)} &= A^{(n-1)} - \bar{r}^f + g - V^{(n-1)}\bar{x} + \frac{1}{2}(V^{(n-1)})^2
\end{aligned}
$$

with boundary conditions $B_x^{(0)} = A^{(0)} = 0$, and

$$V^{(n-1)} = \sigma_d + B_z^{(n-1)}\sigma_z + B_r^{(n-1)}\sigma_r + B_x^{(n-1)}\sigma_x$$

The aggregate market portfolio is the claim to all future dividends. Therefore, under certain parametric conditions the price dividend ratio of the market is

$$\frac{P_t}{D_t} = \sum_{n=1}^{\infty} \frac{P_t^{(n)}}{D_t} = \sum_{n=1}^{\infty} \exp\left(A^{(n)} + B_z^{(n)}(z_t - g) + B_r^{(n)}(r^f_{t+1} - \bar{r}^f) + B_x^{(n)}(x_t - \bar{x})\right) \tag{2.9}$$

The risk premium of a zero coupon equity depends on the loadings of the equity term structure. Because the correlation between dividend growth and price of risk is assumed to be zero, the equity premium is

$$E_t(R^{(n)}_{t+1} - R^f_{t+1}) \approx [\sigma_d + B_z^{(n-1)}\rho_{dz}\sigma_z + B_r^{(n-1)}\rho_{dr}\sigma_r]x_t$$

Where ρ_{dz} and ρ_{dr} denote the correlations of dividend growth with expected dividend growth and risk-free rate, respectively. The expression in parentheses can be interpreted as the quantity of risk, whereas x_t is the price risk. To understand how the model generates a value premium, notice that the condition $\rho_{dx} = 0$ implies that agents are indifferent about holding assets which only differ in their exposure to discount rate risks. This aspect is important, because in consumption-based models, such as the habits model from Campbell and Cochrane (1999) and the long-run risk model from Bansal and Yaron (2004), agents display aversion to discount rate risks. In such models, securities with longer-dated payouts are more vulnerable to discount rate risks, prompting agents to seek a substantial risk premium for holding these securities. Consequently, a negative correlation between the stochastic discount factor and discount rate risk results in a growth premium.

The second important condition in the model is $\rho_{dz} < 0$, implying that shocks to expected dividend growth serve as a hedge against actual dividend growth shocks. In this framework, high duration assets, which load more on dividend growth due to positive growth rates, become less risky and agents require a smaller risk premium to hold them. This condition ultimately gives rise to a value premium.

2.6.1 Identification of Monetary Policy

To include a high-frequency monetary policy shock I follow the modelling approach of Pflueger and Rinaldi (2022) and differentiate between low- and high-frequency variables. Specifically, I assume that at the conclusion of each quarter, there is a FOMC meeting that potentially yields a monetary policy surprise. To analyze the effects of these meetings, I distinguish between shocks before and after FOMC announcements. Variables prior to the FOMC announcement at time t are different from those post-FOMC at time $t-1$, as they encompass information from the shock at the period t excluding the FOMC decisions. The shock is defined as:

$$\epsilon_{i,t} = \epsilon_{i,t}^{pre} + \psi_i \epsilon_t^{MP} \tag{2.10}$$

where $i = d, r, z, x$. The high-frequency returns around monetary policy news are calculated using post- and pre-FOMC prices. I calibrate ψ_i based on a number of empirical studies. I set the effect of monetary policy shock on real rates ($\sigma_r \psi_r$) to 1.06 percent, which is the impact of monetary policy surprise on short-term real rate recorded by Nakamura and Steinsson (2018b). The impact of monetary policy on price of risk is calibrated to 0.6 in line with Bauer et al. (2023), who show that a 10 basis points increase in monetary policy surprise, increases the price of risk by "a little less than half its standard deviation". To pin down the impact of monetary policy on expected dividend growth shocks I generate an empirical proxy of z_t using the consumption-dividend ratio analogous to Lettau and Wachter (2007) and regress it on monetary policy surprises. I find that a one unit increase in monetary policy surprises decreases z_t by 44 basis points. I assume that the dividend payment in the current quarter is not impacted by the FOMC decisions, making the post- and pre-FOMC dividend growth equal: $\Delta d_{t+1}^{pos} = \Delta d_{t+1}^{pre}$.[10] Finally, I match the standard deviation of ϵ_t^{MP} to the empirical standard deviation of monetary policy surprise of 0.04. All other parameters of the model are calibrated according to Lettau and Wachter (2011) and adjusted to quarterly frequency.

To gain intuition about the effects of monetary policy on the stock returns, one can derive the high-frequency log return of a zero-coupon equity analytically by subtracting

[10] Pflueger and Rinaldi (2022) make a similar assumption regarding the consumption and output in their model.

the price dividend ratio before and after the monetary policy shock:

$$r^{(n)}{}_{t+1}^{hf} = [B_z^{(n)}\psi_z + B_r^{(n)}\psi_r + B_x^{(n)}\psi_x]\varepsilon_{t+1}^{MP} \tag{2.11}$$

Equation 2.11 shows that the log return on the zero coupon equity is linear on the monetary policy shocks. This relationship is particularly insightful, revealing that assets with higher maturities are more sensitive to monetary policy shocks, as the product of ψ and the loadings will be negative. This relationship implies that growth stocks are not only more sensitive to monetary policy due to the discount rates, but also because of expected dividend growth.

2.6.2 Growth and value portfolios

The construction of growth and value portfolios follows the methodology of Lettau and Wachter (2011), using a deterministic process for cash flow shares. Specifically, a firm produces a portion s_t^i of the aggregate dividend, increasing at a steady quarterly rate of g_s — which is set to 5% — for the initial 100 quarters and then decreasing at the same rate for the subsequent 100 quarters. The maximum value of a share is $\bar{s} = \underline{s}(1 + g_s)^{N/2}$ with $\underline{s}$ adjusted so the sum of all shares equals one. This modelling approach reflects the varying dividend contributions of firms at different stages of their life cycle. I simulate 200 firms over a 50-year span, which represents a full firm life cycle.

No-arbitrage implies that the price of each firm is its share of the aggregate dividend times its present value:

$$P_t^i = \sum_{n=1}^{\infty} s_{t+n}^i P_t^{(n)} \tag{2.12}$$

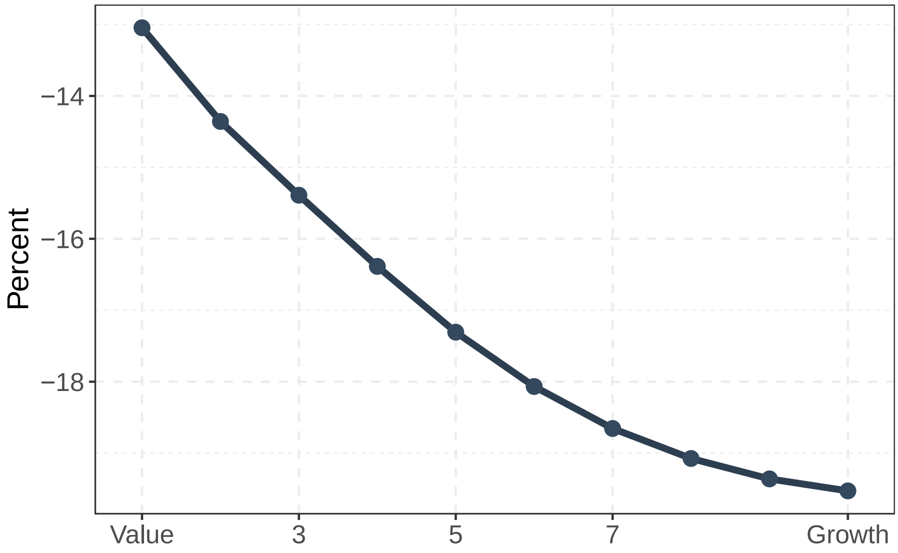
In this model, firms are categorized as value or growth based on their lagged price-to-dividend ratios. Value firms are defined by their low price-to-dividend ratios, indicating a higher proportion of dividends in the short term. Consequently, value firms represent assets with shorter duration. I simulate 50.000 quarters of data and create 10 decile portfolios averaging the firm returns within each portfolio.

2.6.3 Simulated results and discussion

Figure 2.7 illustrates the response of the 10 decile portfolios to monetary policy surprises based on 50,000 quarters of simulated data. The portfolio responses align with the decreasing pattern observed in the empirical data. While the average response of the value portfolio to monetary policy is approximately 13 percentage points, the growth portfolio responds by over 20 percentage points. Although these simulated responses are slightly higher than the empirical findings, the model produces a spread return between the first and tenth deciles of roughly 8 percentage points, closely matching the empirical results shown in Table 2.4.

The underlying mechanism driving this pattern in the model is that a positive monetary policy shock increases both the price of risk and the risk-free rate, while reducing dividend growth. These three effects collectively lead to a stronger response from long-duration assets, and therefore from growth stocks, to monetary policy. This dynamic is further illustrated in Figure 2.8, which depicts the return of zero-coupon equities following a positive monetary policy shock. The prices of all zero-coupon

Figure 2.7: Model-implied portfolio responses to monetary policy



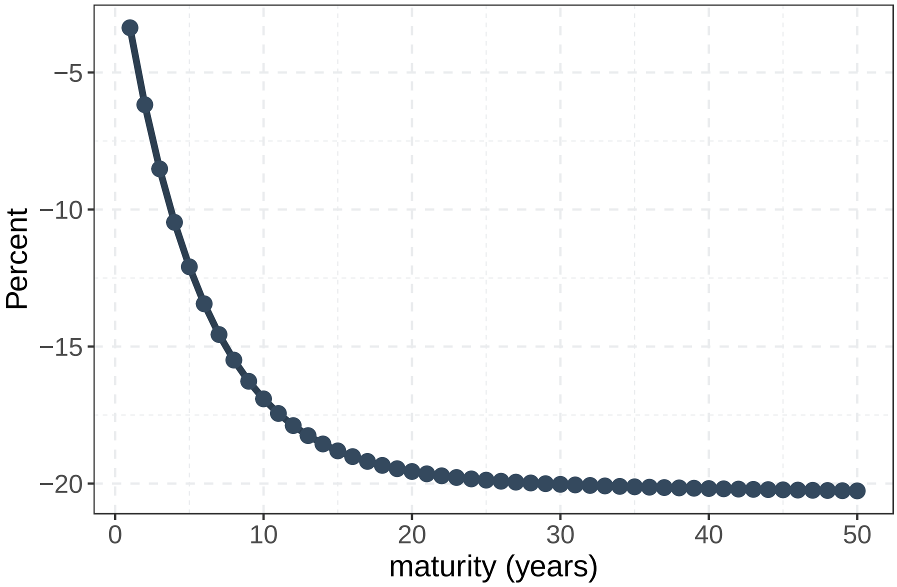
The figure shows the sensitivity of portfolio returns to monetary policy. Portfolios are constructed using 50.000 quarters of simulated data and constructing firms with a determinsitic share process. Firms are then sorted in portfolio deciles and their returns are regressed on high-frequency monetary policy surprises.

equities decline after the shock, with the prices of longer-maturity equities experiencing significantly larger drops. A notable feature of the model is that the marginal impact of monetary policy diminishes with maturity, as evidenced by the curve flattening considerably for higher maturities.

The fact that growth stock prices decline more than value stock prices raises the question of whether monetary policy affects the value premium. In this model, the answer is yes, but the effect is minimal. To understand this, first note that the expectation of monetary policy shocks is zero. Consequently, high-frequency log returns will, on average, also be zero. However, when calculating the equity risk premium, both the expectation and the variance of log returns play a role due to Jensen's inequality. As a result, the expected value of high-frequency returns will be nonzero, albeit small, because the standard deviation of high-frequency monetary policy shocks is very low. In other words, while the inclusion of monetary policy shocks slightly alters the quantity of risk in the model, the change is so minor that the equity premium and the value premium remain virtually unaffected.

To evaluate the performance of the model relative to the data and to examine the impact of monetary policy on the model's main results, Table 2.11 presents the expected returns, standard deviations, and betas for each portfolio decile, as produced by the model and observed in the data. Panel A presents the results from the model simulation without monetary policy shocks, Panel B incorporates monetary policy shocks into the model, and Panel C displays the corresponding statistics from the data, which are estimated using Fama and French portfolios sorted by market-to-book equity from 1952 to 2018. The table confirms that the value premium produced by the model without monetary policy shocks — 4.63, as highlighted in Lettau and Wachter (2011) — is very similar to the value premium generated when monetary policy shocks

Figure 2.8: Term structure of equity response to monetary policy



The figure shows the effects of monetary policy on the term structure of equity implied by the model. Zero coupon equity returns are simulated using 50.000 quarters of data. The zero coupon equity returns is then regressed on the high-frequency monetary policy surprise.

are included, which is 4.97. This is also fairly close to the empirical estimate of 3.87. Finally, the decisive factor for the value premium is not only that growth stocks exhibit lower expected returns, but that their lower expected returns are not fully explained by higher betas. As Table 2.11 shows, this empirical result is also successfully reproduced in the model.

Table 2.11: Risk Premium of decile portfolios

	Value to Growth									
Portfolio	V	2	3	4	5	6	7	8	9	G
Panel A: Model without monetary policy										
$E(R^i - R^f)$	10.35	9.45	8.92	8.33	7.69	7.09	6.57	6.18	5.90	5.72
$\sigma(R^i - R^f)$	17.86	17.85	18.50	19.45	20.30	20.84	21.05	21.03	20.90	20.72
β_i	0.87	0.88	0.92	0.97	1.01	1.04	1.04	1.04	1.03	1.02
Panel B: Model with monetary policy										
$E(R^i - R^f)$	9.92	9.05	8.45	7.76	7.00	6.33	5.79	5.41	5.11	4.95
$\sigma(R^i - R^f)$	16.71	16.70	17.34	18.28	18.99	19.43	19.56	19.51	19.37	19.18
β_i	0.87	0.88	0.92	0.98	1.01	1.03	1.04	1.03	1.02	1.01
Panel B: Data										
$E(R^i - R^f)$	8.79	8.87	8.26	6.29	7.39	6.98	5.80	6.42	6.11	4.92
$\sigma(R^i - R^f)$	20.00	16.54	16.08	15.43	14.83	14.61	15.35	15.40	15.57	16.95
β_i	1.13	0.98	0.95	0.93	0.90	0.91	0.97	0.99	1.01	1.07

The table shows the risk premium, standard deviation and the betas of growth and value stocks implied by the model without monetary policy (Panel A), the model with monetary policy (Panel B), and estimated from the data. The moments in the model are estimated by simulating 50.000 quarters of data. The empirical statistics are estimated using annual Fama and French portfolio returns sorted by market-to-book equity. The data spans from 1952 to 2018.

A limitation of the model is its ability to reproduce the distinct effects of monetary policy on future risk premiums and cash flows, as observed in the Campbell & Shiller decomposition. While the decomposition indicates that discount rates are the predominant drivers of the differential sensitivity between growth and value stocks, the model attributes a significant portion of the stock return response to monetary policy to cash flow news. In fact, even if monetary policy only affected expected cash flows and not the risk premium, long-duration assets would still exhibit greater responsiveness to monetary policy. This feature stems from the assumption that dividend growth follows an AR(1) process, without incorporating any idiosyncratic shocks that could influence it. Under this assumption, B_z increases with cash flow maturity, causing shocks to dividend growth to propagate over time and disproportionately impact long-duration assets. This mechanism provides insight into the effects of cash flow news on the cross-section of stock returns and its relationship with duration. Specifically, not only do aggregate shocks to discount rates affect long-duration assets more, but aggregate shocks to cash flows also have a similar effect due to differences in duration.

2.7 Conclusion

Motivated by the conflicting evidence in the literature, this paper examines the effects of monetary policy on growth and value stocks, as well as the underlying mechanisms driving these effects. The empirical analysis provides substantial and robust evidence that growth firms are more sensitive to monetary policy than value stocks. This finding holds across various levels of aggregation, making it resilient to issues of diversification or idiosyncratic noise. It is also robust to differences in pre-processing methods, alternative definitions of monetary policy surprises, and remains consistent during the zero lower bound period. An analysis of dynamic responses further reveals that the stronger reaction of growth stocks persists for over two weeks. Finally, I reconcile my findings with earlier studies that reported the opposite result, clarifying the differences in methodologies and contexts.

The central empirical finding in this study suggests that the duration of cash flows is mainly responsible for the increased sensitivity of growth stocks, challenging previous beliefs that financial constraints play the dominant role. While financial constraints do explain a significant portion of the cross-sectional variation in stock return responses to monetary policy, they are not jointly related to market-to-book equity and monetary policy. The influence of cash flow duration is further supported by a Campbell & Shiller decomposition, which reveals that growth stocks are more reactive to monetary policy surprises due to larger revisions in the risk premium. This finding arises from the stronger predictability of future excess returns for growth stocks through their dividend yields, consistent with their higher cash flow duration.

My findings are supported by an asset pricing model based on Lettau and Wachter (2011), in which firms differ solely in the timing of their cash flow payments. The model successfully replicates the observed sensitivity of growth and value stocks to monetary policy, while also producing higher expected returns for value stocks compared to growth stocks. In the model, growth stocks are more sensitive to monetary policy due to its effects on the risk-free rate, dividend growth, and, most notably, the price of risk. However, the value premium remains largely unchanged relative to the model without monetary policy, as monetary policy shocks have a mean of zero and exceptionally low variance. Consequently, while monetary policy exerts significant short-term effects on growth and value stocks, its long-term impact is negligible.

Given the significance of the differentiation between growth and value stocks in recent times — for instance, technology and clean-energy firms are commonly categorized as growth stocks — this study underscores the critical role of cash flow duration in the transmission of monetary policy to the stock market. These findings highlight the importance for policymakers and investors to account for cash flow duration when evaluating the effects of monetary policy on the stock market. Moreover, this study contributes to the literature by demonstrating that factors beyond financial conditions play a crucial role in the transmission of monetary policy to the cross-section of firms. In particular, the way growth opportunities translate into heterogeneity in the timing of dividend payments is pivotal in shaping the transmission of monetary policy.

Appendix

Appendix 2.A: Derivation of monetary policy surprises

This exposition closely follows Gurkaynak et al. (2005) Appendix. The Federal funds future contracts have a settlement price which is based on the average federal funds rate over the month specified in the contract.[11]. Let i_0 be the average federal funds rate prevailing before the fed's decision at time $t-\Delta t$ and i_1 the rate after the decision at time t. Finally, denote d as the day of the month of the announcement and D the total number of days in the month. Then, the implied spot rate before the FOMC meeting is

$$ff^1_{t-\Delta t} = \frac{d}{D} i_0 + \frac{D-d}{D} E_{t-\Delta t}(i_1) + \mu^1_{t-\Delta t} \tag{2.13}$$

Where μ^1 is the risk premium. Leading this equation to after the meeting yields:

$$ff^1_t = \frac{d}{D} i_0 + \frac{D-d}{D} i_1 + \mu^1_t \tag{2.14}$$

Kuttner (2001) calculates the surprises by subtracting the spot rate after from the spot rate before the meeting:

$$mp1_t \equiv i_1 - E_{t-\Delta t}(i_1) \approx [ff^1_t - ff^1_{t-\Delta t}] \frac{D}{D-d} \tag{2.15}$$

Two remarks are important here: First, the equation holds only if changes in risk premium μ in this window is small in comparison to the change in expectations itself. An assumption which is backed empirically by Piazzesi and Swanson (2008). Second, the scale $(D-d)/D$ can lead to measurement errors if the FOMC meetings occur very late in the month. Because of that, the unscaled change in the next-month federal funds futures contract is used in the announcements that takes place in the last seven days of the month.

Gurkaynak et al. (2005) extend this analysis to extract two monetary policy surprise factors. They argue that two latent factors can better describe asset prices movements. The Kuttner shock captures current policy surprises, but not changes in the future expectation of these surprises, something which affects asset prices as well. To enhance the analysis, they consider next to the current month federal funds rates future contracts, the three-months funds future contract, and the prices of eurodollars future contracts with maturity 1.5, 2.5 and 3.5 quarters to expiration on average. Formally, let X be a vector of the standardized changes in the future prices. I can decompose X in five principal components F with loadings in Λ.

$$X = F\Lambda \tag{2.16}$$

Nakamura and Steinsson (2018b) take the first factor with the largest R2, call it $F1$ and rescale it so it has a one unit impact on the one year Treasury yield change. Let Δy^1 denote the daily change in the one year Treasury yield. I run the regression:

$$\Delta y^1 = \rho F1 + \epsilon \tag{2.17}$$

In which case the NS surprise is:

$$mps = F1 \cdot \rho \tag{2.18}$$

[11]More precisely, the value at expiration is 100 minus the average federal funds rate.

Appendix 2.B: Further empirical results

Table 2.12: Controlling for different time periods

	(1)	(2)	(3)	(4)
M/B	-0.02^{*}	-0.02	-0.02^{***}	-0.03^{***}
	(0.01)	(0.01)	(0.01)	(0.01)
M/B*mps	-0.87^{***}	-0.87^{***}	-0.86^{***}	-0.93^{***}
	(0.29)	(0.27)	(0.31)	(0.27)
M/B*zlb	0.002			
	(0.01)			
M/B*mps*zlb	-0.05	-0.15		
	(0.47)	(0.43)		
M/B*dotcom			0.02	
			(0.02)	
M/B*mps*dotcom			-0.20	-0.06
			(0.55)	(0.56)
N	491,399	448,156	491,399	448,156
R^2	0.19	0.45	0.19	0.45
Controls		✓		✓

The table shows the regression results of firm-level one-day returns on monetary policy, market-to-book equity, and further controls from January 1990 to December 2018. mps stands for monetary policy surprise, M/B for market-to-book equity, zlb is a dummy for the zero lower bound, and dotcom a dummy for the period commonly referred to as dotcom bubble. Other controls are size, leverage, and profitability, sales growth, market beta, and their interaction with monetary policy. All regressions use time and firms fixed effects. Two-way clustered standard errors are reported in parentheses. ***, ** and * denote significance at the 1%, 5% and 10% level, respectively.

Table 2.13: Alternative definitions of growth and value stocks

	Market-to-book ratio	Cash flow-price ratio	Earnings-Price ratio	Dividend yields
characteristic	-0.02^{*}	-0.03	0.00	0.02
	(0.01)	(0.34)	(0.00)	(0.03)
characteristic*mps	-0.87^{***}	9.35^{*}	0.62^{*}	2.13^{**}
	(0.28)	(5.23)	(0.37)	(0.96)
N	491,399	415,993	287,760	429,012
R^2	0.19	0.15	0.20	0.15

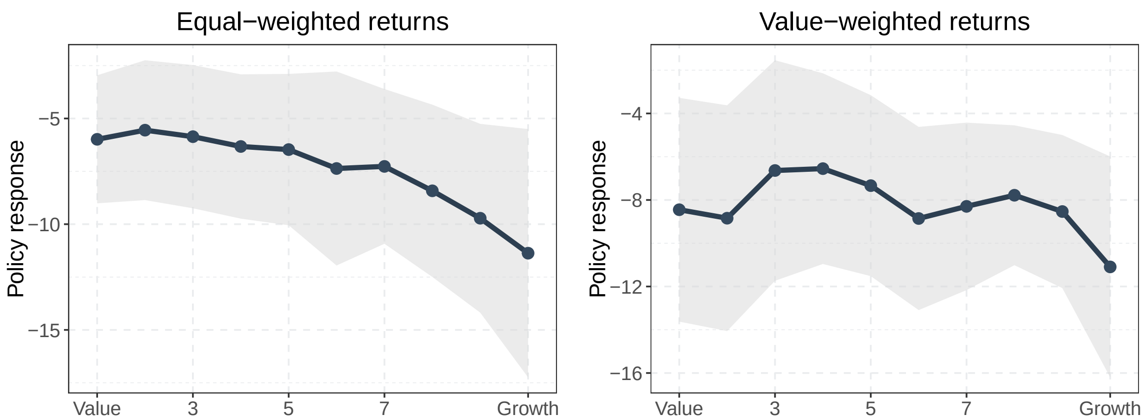
The table shows the regression results of firm-level one-day returns on monetary policy interacted with difference definitions of growth and value stocks from January 1990 to December 2018. Controls include size, profitability, book leverage, revenue growth, market beta, and their interaction with monetary policy. All regressions use time and firms fixed effects. Two-way clustered standard errors are reported in parentheses. ***, ** and * denote significance at the 1%, 5% and 10% level, respectively.

Table 2.14: Alternative monetary policy surprises

	(1)	(2)	(3)	(4)	(5)
M/B	−0.00	0.00	0.00	0.00	−0.00
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
M/B*NS	−0.87***				
	(0.29)				
M/B*Target		−0.59**			
		(0.23)			
M/B*Path			−0.09		
			(0.18)		
M/B*J&K				−0.61***	
				(0.15)	
M/B*B&S					−0.53***
					(0.18)
N	491,399	491,399	491,399	491,399	491,399
R^2	0.19	0.19	0.19	0.19	0.19

The table shows the regression results of firm-level one-day returns on monetary policy interacted with difference definitions of growth and value stocks from January 1990 to December 2018. NS stands for the monetary policy surprises from Nakamura and Steinsson (2018b), my baseline monetary policy surprises. Target and Path are the surprises from Gurkaynak et al. (2005). J&K stands for the surprises from Jarociński and Karadi (2020) and B&S for the surprises from Bauer and Swanson (2023a). Controls include size, profitability, book leverage, revenue growth, market beta, and their interaction with monetary policy. All regressions use time and firms fixed effects. Two-way clustered standard errors are reported in parentheses. ***, ** and * denote significance at the 1%, 5% and 10% level, respectively.

Figure 2.9: Portfolio reaction of market-to-book equity sorted portfolios using Fama and French portfolios



The figure shows the average reaction of the 10 decile portfolios sorted by market-to-book equity using NS surprises against the mean market-to-book equity. 10% confidence intervals are drawn around the point estimation. The samples goes from January 1990 to December 2018.

Table 2.15: Reaction of spread portfolios to monetary policy surprises using Fama and French portfolios

	10% - 10%	30% - 30%	50% - 50%	90% - 10%	10% - 90%
mps	-5.38^{*}	-4.04^{**}	-2.76^{**}	-1.61^{*}	-4.37^{*}
	(2.77)	(1.93)	(1.28)	(0.93)	(2.42)
Constant	-0.06	-0.01	-0.01	-0.03	-0.03
	(0.05)	(0.03)	(0.02)	(0.03)	(0.04)
N	255	255	255	255	255
R^2	0.04	0.07	0.07	0.02	0.06

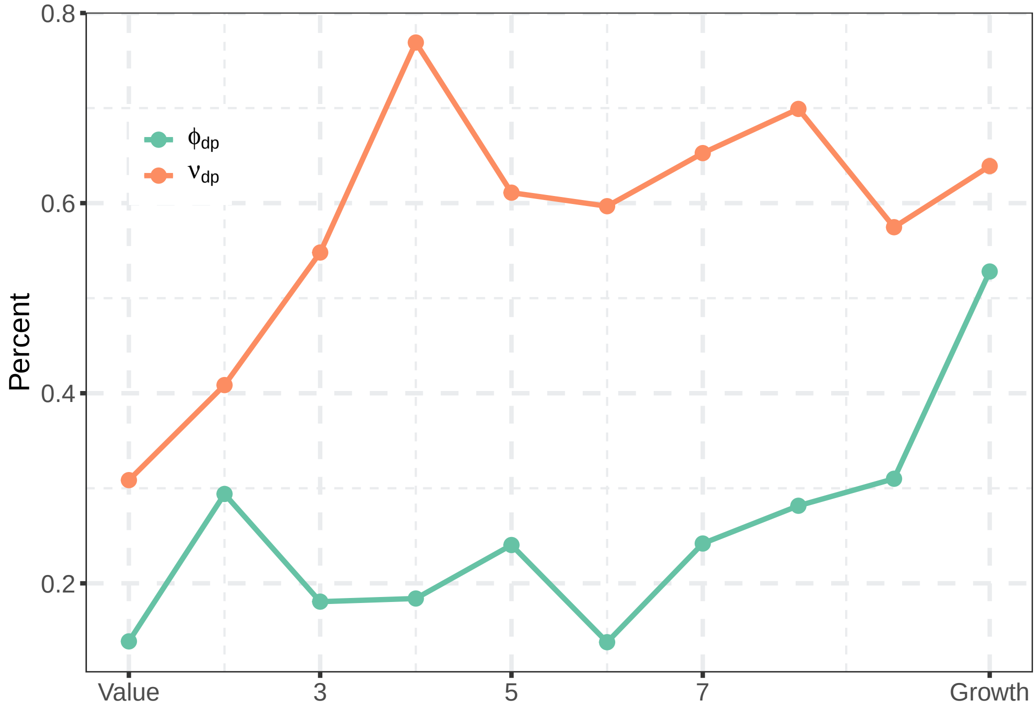
The table estimates the regression $r_t^s = \alpha + \beta \times mps_t + \varepsilon_{i,t}$ using the sample from January 1990 to December 2018, where r_t^s is the return of the spread portfolio. The spread portfolios are formed by sorting firms according to the market-to-book ratio and subtracting the 50%, 30% and 10% highest from the lowest companies each period. The last two columns show the spread portfolio of the 90% highest companies and the 10% lowest and vice-versa. White standard errors are computed. ***, ** and * denote significance at the 1%, 5% and 10% level, respectively.

Table 2.16: Reaction of big and small stocks to monetary policy surprises and market-to-book equity

	Small cap		Big cap		S&P 500	
	(1)	(2)	(3)	(4)	(5)	(6)
mps	-3.45^{**}		-7.31^{**}		-7.51^{***}	
	(1.58)		(2.90)		(2.55)	
mb	0.01	-0.05^{**}	-0.01	-0.002	-0.005	0.001
	(0.01)	(0.02)	(0.01)	(0.01)	(0.01)	(0.01)
mb*mps	-0.99^{***}	-0.96^{***}	-0.57^{*}	-0.45^{*}	-0.54^{*}	-0.42^{*}
	(0.35)	(0.31)	(0.32)	(0.24)	(0.29)	(0.23)
Constant	0.22^{***}		0.26^{***}		0.25^{***}	
	(0.06)		(0.09)		(0.08)	
N	159,112	159,112	159,111	159,111	120,155	120,155
R^2	0.002	0.21	0.02	0.24	0.02	0.19
Fixed effects		✓		✓		✓

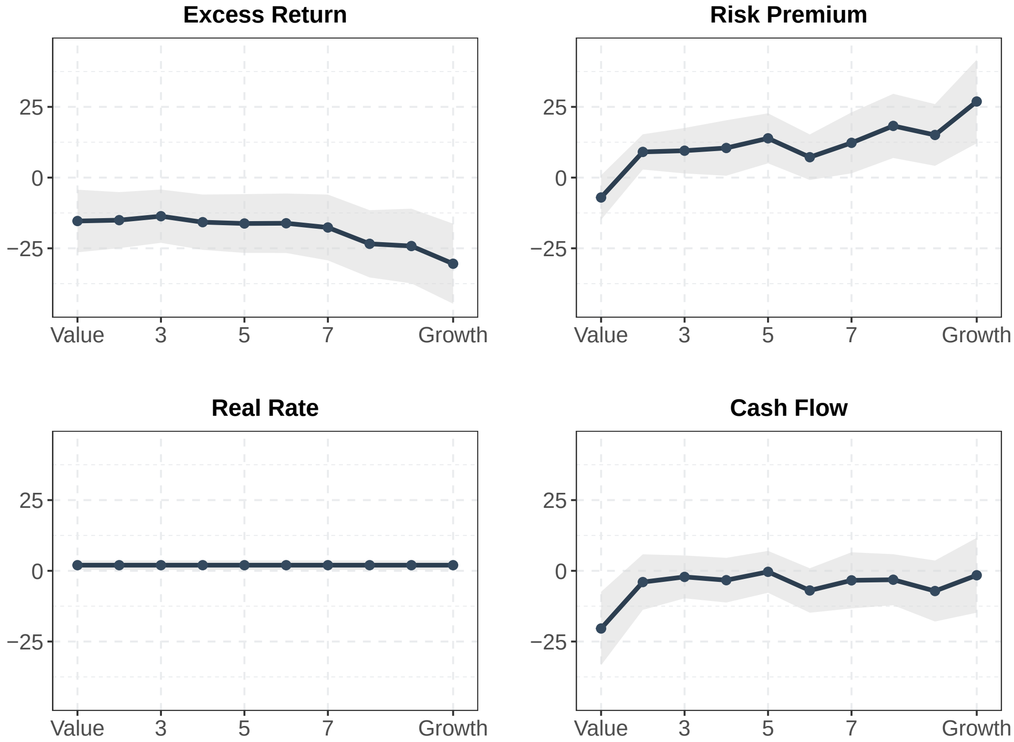
The table shows the regression results of firm-level one-day returns on monetary policy and market-to-book equity from January 1990 to December 2018. mps stands for monetary policy surprise, mb for market-to-book equity. Small firms are defined as firms being in the 30% lowest quantile whereas big firms are in the 30% highest quantile. The last two columns show the results only for all firms which have been part of the the S&P 500 index. Two-way clustered standard errors are reported in parentheses. ***, ** and * denote significance at the 1%, 5% and 10% level, respectively.

Figure 2.10: Decomposition of the contribution of dividend yields to monetary policy response of future discount rates



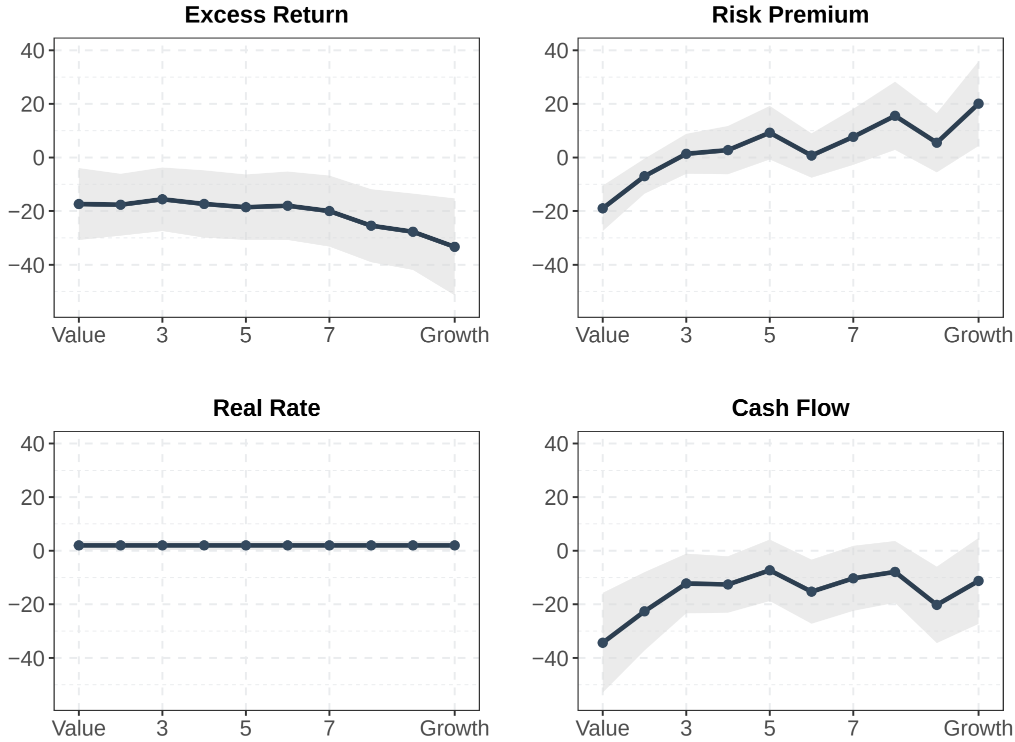
The figure shows the load of dividend yields on the long-run discount rate news, ν_{dp} and the effect of monetary policy on dividend yields, ϕ_{dp}.

Figure 2.11: Response of unexpected excess returns to policy surprises using Fama and French with different ρ



The figure shows the reaction of the news components of the unexpected excess returns for Fama and French portfolios. The portfolios are sorted from low to high market-to-book equity. I estimate a VAR(1) with the excess equity return, the real interest rate, the relative bill rate, the change in the 3-month bill rate, the dividend price ratio, and the spread between the 10-year and 1-month Treasury yields. The VAR is estimated from Jan-1980 to Dec-2018. The monetary policy surprises are monthly aggregated and go from the Jan-1990 to Dec-2018. Coefficients are estimated in two-steps. Standard errors are calculated using bootstrapping. 95% confidence intervals are drawn around the point estimation.

Figure 2.12: Response of unexpected excess returns to policy surprises using Fama and French using Dividend Growth



The figure shows the reaction of the news components of the unexpected excess returns for Fama and French portfolios. The portfolios are sorted from low to high market-to-book equity. I estimate a VAR(1) with the excess equity return, the real interest rate, the relative bill rate, the change in the 3-month bill rate, the dividend price ratio, and the spread between the 10-year and 1-month Treasury yields. The VAR is estimated from Jan-1980 to Dec-2018. The monetary policy surprises are monthly aggregated and go from the Jan-1990 to Dec-2018. Coefficients are estimated in two-steps. Standard errors are calculated using bootstrapping. 95% confidence intervals are drawn around the point estimation.

Chapter 3

Monetary Policy Transmission to Investment: Evidence from a Survey on Enterprise Finance

Abstract

We study how survey-based measures of funding needs and availability impact the transmission of euro area monetary policy to corporate investment. We first provide evidence that firms' funding needs are primarily driven by future investment opportunities, whereas their perceived funding availability is closely related to financial conditions. Using these two measures, we explore how the effectiveness of monetary policy varies depending on fundamentals and financial conditions. Our findings reveal two key insights: first, monetary policy is most effective when fundamentals are strong. Second, firms with favourable financial conditions display a more muted investment response to monetary policy. These results shed new light on the transmission of monetary policy to investment, emphasizing not only the pivotal role of financial conditions, but also the critical importance of structural factors that lie beyond the direct control of central banks.

3.1 Introduction

Private investment is a key driver of GDP growth. Studies have shown that monetary policy can significantly impact aggregate capital investment, thereby shaping the overall response of an economy to policy changes (Cloyne et al., 2023). Recently, policymakers have expressed concerns about the effectiveness of monetary policy in stimulating investment under certain economic conditions. For example, in the ECB's Monetary Policy Accounts from October 2024, the Governing Council noted that investment remained subdued due to structural factors, such as geopolitical tensions, trade uncertainty, and overregulation.[1] As a result, policymakers concluded that monetary easing alone would be insufficient to trigger a significant rebound in investment. In other words, the impact of monetary policy appeared to be constrained by fundamental issues which are beyond the control of a central bank.

Indeed, in complete frictionless markets, investment should be determined solely by economic fundamentals. According to traditional Q-theory, rational firms base their investment decisions on the marginal product of capital, with Tobin's Q serving as a sufficient statistic for these investment opportunities.[2] However, a vast body of research suggests that financial frictions also play a crucial role, meaning that financial variables will impact investment outcomes, and in turn, affect the transmission of monetary policy (Bernanke and Gertler, 1989; Bernanke et al., 1999). With this in mind, the key question we seek to answer is: How do fundamentals and financial conditions shape the transmission of monetary policy to investment?

The main empirical challenge in addressing this question is disentangling the effects of investment opportunities and financial conditions on investment decisions. Early studies, guided by economic models that often assume rationality and complete information, attempted to separately proxy for these two factors by using accounting data (Fazzari et al., 1988; Gilchrist and Himmelberg, 1998; Love and Zicchino, 2006). However, isolating their effects has proved difficult. For example, the marginal Tobin's Q, which Q-theory suggests as a measure of investment opportunities, is not observable, and commonly used proxies, such as the average Tobin's Q, are confounded by the financial health of the firms. Furthermore, a major constraint is that capital market valuations are not available to small and medium-sized enterprises (SMEs). As a result, even if Tobins' Q was a reliable measure, its application would be restricted to larger firms.

To tackle the challenge of distinguishing between investment opportunities and financial conditions, we directly use firms' assessments from the ECB's Survey on Access to Finance and Enterprise (SAFE). Our goal is to examine investment behavior by analyzing firms' responses to questions regarding their funding needs and perceived availability of external financing. A key advantage of this survey data is its broad coverage, encompassing both large firms and SMEs. Since the survey is completed by business owners, CEOs, or CFOs, they offer direct insight into the decision-making process behind investment choices, also reflecting behavioural biases that might not

[1]See the account of the monetary policy meeting of the Governing Council from 16-17 October 2024: `https://www.ecb.europa.eu/press/accounts/2024/html/ecb.mg241114~c0e6f53cf7.en.html`

[2]In this paper, we use the terms "investment opportunities" and "fundamentals" interchangeably, as is common in the literature (Love and Zicchino, 2006). Similarly, we use the term "structural factors" to refer to economic drivers that primarily influence fundamentals, as in the ECB monetary policy meeting account of 16-17 October 2024.

be accounted for when working solely with accounting variables.

We begin by offering an economic interpretation of funding needs and funding availability, as well as their connection to investment. Specifically, we provide evidence that external funding needs are primarily driven by fundamentals, while funding availability is largely influenced by financial conditions. Intuitively, assuming that a firm has a given level of internal funding, the amount of external funding it seeks will depend mainly on its expectations for future growth — a reflection of economic fundamentals. In contrast, a firm's perception of the availability of external financing will be shaped primarily by current financial conditions. Therefore, by examining the role of funding needs and availability in monetary policy transmission, while controlling for key firm-level variables, we can provide deeper insights into how fundamentals and financial conditions shape the impact of monetary policy on investment.

We first examine the contemporaneous correlation between funding needs and availability and accounting variables. Our findings indicate that funding availability is strongly linked to firm-level financial conditions, particularly leverage, which serves as a proxy for asset-based collateral, and internal funding, which can be interpreted as earnings-based collateral. We observe a negative correlation between funding availability and leverage, and a positive correlation between funding availability and internal funding, suggesting that firms with higher leverage or lower internal funding perceive external financing as less accessible. In contrast, funding needs exhibit a positive correlation with leverage and a negative correlation with internal funding and liquidity, implying that an increase in internal funding and liquidity is associated with a lower demand for external funding, in line with the substitutability of external and internal funds.

Second, we demonstrate that funding needs and funding availability influence investment in different ways, with varying effects across different types of firms, although their unconditional impact on investment is similar. Our findings indicate that the effect of funding needs on investment does not differ based on a firm's size, leverage, or debt burden, suggesting that investment decisions are similarly influenced by funding needs across firms. In contrast, funding availability exhibits significant disparities in its impact on investment. Specifically, availability plays a crucial role in determining future investment for smaller firms , as well as for firms with high leverage or greater debt burdens — characteristics commonly associated with financial constraints in the literature. This shows that investment decisions for financially constrained firms are driven significantly more by the perception of the availability of external funding.

Our third piece of evidence supporting our hypothesis about the distinct roles of funding needs and availability is based on a quasi-natural experiment. We examine bank branch changes in subregions in Portugal, drawing on established research that shows that the number of bank branches significantly impact credit lending (Morgan et al., 2016; Nguyen, 2019). More recently, Bonfim et al. (2021) argued that bank branch closures in Portugal during the early 2010s were primarily driven by restructuring efforts unrelated to local profitability. As a result, the number of bank branches varied exogenously with respect to investment opportunities in certain subregions. Building on this insight, we examine how the number of bank branches affects the average reported funding needs and availability among Portuguese firms in our sample. Our findings indicate that an increase in bank branches in a specific subregion significantly enhances the perceived availability of external funding, while funding needs remain unchanged. This supports our hypothesis that shocks to financial conditions primarily influence firms' perceptions of external funding availability, without affecting their fundamental demand for external financing.

Our main empirical contribution aims to understand the transmission of monetary policy with respect to fundamentals and financial conditions. To analyze the effects of monetary policy, we adopt the high-frequency identification approach widely used in the literature, measuring policy surprises through changes in OIS rates within a 30-minute window around ECB Governing Council announcements. We use the monetary policy surprises constructed by Altavilla et al. (2019) from changes in OIS rates up to ten years around the ECB's press conference. We focus on the forward guidance factor because it has a higher impact on longer-maturity yields which are more relevant for firms' long-term borrowing. Additionally, a significant portion of our sample falls within the zero lower bound (ZLB) period, during which the target rate exhibited minimal movement.

Using the funding needs and funding availability, we estimate the impact of monetary policy surprises on investment through local projections. Specifically, we regress future firm-level investment on the monetary policy surprises interacted with funding needs and funding availability, which serve as proxies for fundamentals and financial conditions, while controlling for other firm-level accounting variables. Our results confirm that fundamentals and structural factors driving investment opportunities play a crucial role in the transmission of monetary policy. For example, we find that a one basis point increase in forward guidance surprise leads to a 10 basis points greater reduction in investment for a firm that reported increased external funding needs, compared to a firm whose funding needs remained unchanged over the past six months. This finding suggests that monetary policy effectiveness is significantly weaker when investment is constrained by structural factors, reinforcing the idea that easing monetary policy alone may not be sufficient to stimulate investment under such conditions.

Likewise, our analysis of the effects of monetary policy on investment through the availability of funding suggests that tighter financial conditions amplify the impact of monetary policy on investment, in line with earlier empirical results (Oliner and Rudebusch, 1996a). We find that the difference in investment response to monetary policy can be as large as 15 basis points between firms that experienced an increase in funding availability and those whose availability remained unchanged over the previous six months. We also show that our main results on funding needs and availability are not confounded by balance-sheet characteristics — such as leverage, liquidity, or firm size — which the literature has identified as key factors in the transmission of monetary policy to investment. Our findings remain mostly unchanged when controlling for these variables interacted with monetary policy surprises.

While our results so far shed light on the separate roles of fundamentals and financial conditions in the transmission of monetary policy, they offer limited insight into potential interactions between the two. In practice, firms with high investment opportunities are often smaller and might be subject to tighter financial conditions than their larger peers. To account for this, we extend our analysis to examine the joint influence of fundamentals and financial conditions. Specifically, we construct a dummy variable that takes the value of 1 if a firm increased funding needs and perceived a decrease in funding availability. We interpret this dummy as a survey-based measure of financial constraints. Unlike traditional balance sheet–based proxies — which typically reflect only financial conditions — our measure captures both sides of the constraint: the firm's needs for funding and the availability. According to our definition, a firm is financially constrained only when high funding needs coincide with limited access to external finance. The group-specific local projection estimates confirm that financially constrained firms are significantly more responsive to monetary policy shocks than other firms, with peak effects nearly twice as large. Our results confirm the conclusion of other empirical studies such as Durante et al. (2022) and Cloyne et al. (2023), as well as predictions of theoretical models including Kiyotaki and Moore (1997) and Bernanke et al. (1999).

What do these results reveal about the main transmission channels of monetary policy? Our findings suggest that the credit channel plays a central role in explaining how monetary policy affects firm behavior. Monetary easing is expected to improve the financial conditions of firms by easing borrowing restrictions via the balance sheet channel and facilitating lending through the bank lending channel (Bernanke and Gertler, 1995). On the one hand, if structural weaknesses — such as overregulation and economic uncertainty — keep credit demand low, these channels, which mainly operate through credit supply, will do little to stimulate investment. In such cases, the ability of central banks to boost investment and economic growth through monetary policy remains highly limited. On the other hand, if financial conditions are excessively tight, then, all else equal, easing them would lead to increased investment, particularly for firms, that might have less access to external finance, such as small enterprises and highly leveraged firms. This mechanism is particularly evident at the firm level. When firms have high investment opportunities but limited access to finance, monetary policy easing has a substantial positive effect on their investment.

This paper contributes to the literature on monetary policy transmission channels to investment using firm-level data. This line of research dates back to Gertler and Gilchrist (1994), who showed that the sales and inventories of small firms are more sensitive to monetary policy. More recently, Cloyne et al. (2023) found that the investment of financially constrained firms — defined as young firms that do not pay dividends — reacts more strongly to monetary policy. Similarly, Jeenas (2024) documented that monetary policy shocks lead to larger fluctuations in fixed capital formation, inventories, and sales growth for firms with high leverage and low liquid assets. Ottonello and Winberry (2020) showed that firms with low default risk and low leverage are more responsive to monetary policy, a finding that appears to contrast with the research emphasizing the stronger reactions of financially constrained firms. Gürkaynak et al. (2022) and Jungherr et al. (2024) highlight the importance of debt maturity in the transmission of monetary policy. Our findings align with Durante et al. (2022), who report significant cross-sectional effects of monetary policy on firm investment in the euro area. Beyond confirming the role of financial frictions in monetary policy transmission, our contribution extends the literature by using alternative measures of financial conditions and by providing novel evidence on the transmission of monetary policy conditioned on fundamentals.

Our study is also related to the literature on investment determinants, which dates back to the neoclassical theory of investment, where firms' decisions are based solely on profit maximization, without considering financial factors (Hall and Jorgenson, 1967). Later, empirical studies have challenged this view, showing that financial factors significantly influence investment. For instance, Fazzari et al. (1988) show that cash flow sensitivity is a strong predictor of investment, suggesting that financial conditions matter. However, Kaplan and Zingales (1997) argue that cash flow sensitivity alone cannot be taken as definitive evidence of financial constraints affecting investment. Gilchrist and Himmelberg (1998) address this issue by using a VAR model to estimate a "Fundamental Q", showing that investment remains sensitive to cash flow, even when controlling for future investment opportunities. To disentangle the effects of fundamentals and financial conditions, we use survey-based measures of external funding needs and availability. Our results confirm that both factors influence investment, although fundamentals play a significantly larger role. Similarly, Love and Zicchino (2006) use a VAR approach to examine the effects of fundamentals and financial conditions on investment across countries. They find that financial factors affect investment more in countries with less developed financial systems. Consistent with this, we find that financial conditions play a more critical role in the investment decisions of firms typically identified in the literature as financially constrained — namely, those with lower size, higher leverage, or greater debt burdens.

The remainder of the paper is structured as follows: Section 2 introduces the dataset and presents descriptive statistics. Section 3 investigates the relationship between funding needs, funding availability, and investment, showing that funding needs are primarily linked to firm fundamentals, while funding availability is more closely associated with financial conditions. Section 4 presents our main empirical findings on monetary policy transmission, highlighting the distinct roles of funding needs and availability. Section 5 analyzes the effect of monetary policy on investment by grouping firms based on whether the availability of external funding aligns with their funding needs. Section 6 concludes the paper.

3.2 Data and Summary Statistics

In this section, we outline the data sources used in our analysis. Our primary dataset, SAFE-ORBIS, is proprietary and created by integrating firms from the Survey on the Access to Finance of Enterprises (SAFE) — conducted jointly by the European Central Bank and European Commission — with the ORBIS database supplied by Bureau van Dijk (BvD), a Moody's Analytics subsidiary.
The SAFE is a comprehensive European firm-level survey covering more than 11,000 firms. Launched in 2009, the survey was initially conducted bi-annually and has shifted to a quarterly schedule starting in 2024.[3] Our paper examines all survey rounds from 2010 to 2022.[4] The survey targets a representative sample of non-financial firms across the 20 euro area countries, spanning the four primary sectors: manufacturing, construction, trade, and services. It assesses recent changes in firms' access to finance and their perceptions of the broader economic environment while collecting detailed firm-level information, including size, ownership, age, sector, and financial position.

Our analysis focuses on survey questions related to firms' external financing needs and their perceived availability of various financing sources: bank loans, credit lines, trade credit, equity and debt security issuance. Specifically, each firms is asked the following question: *"For each of the following types of external financing — bank loans, credit lines, trade credit, equity and debt security issuance — please indicate whether your needs (availability) increased, remained unchanged, or decreased during the previous and current quarter"*. The responses to these questions are used to create a

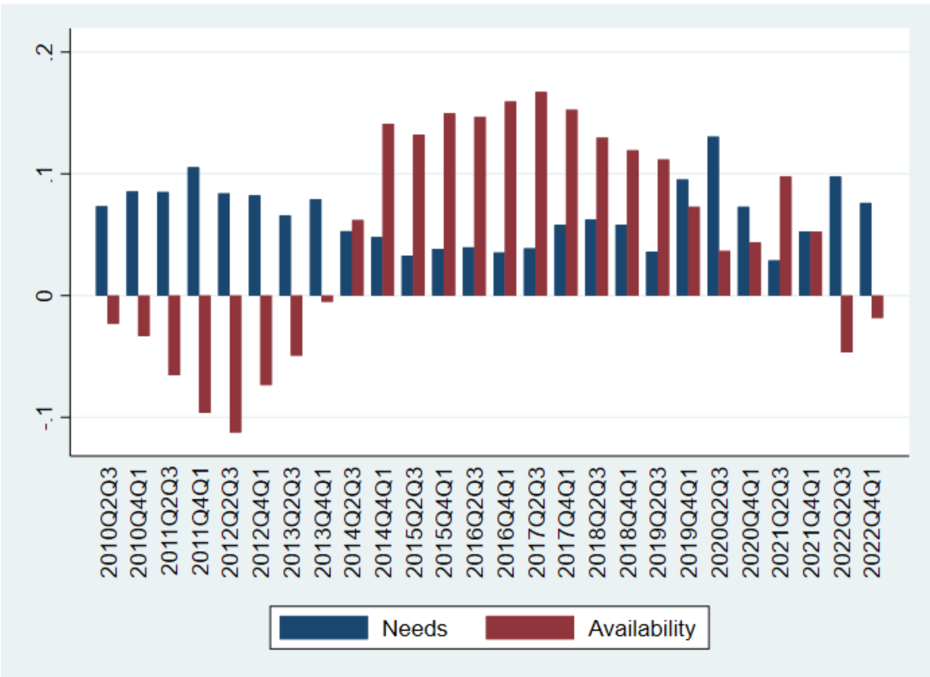
[3]For further details, see `https://www.ecb.europa.eu/stats/ecb_surveys/safe/html/index.en.html`.

[4]Specifically, we analyze survey rounds 3 to 27, covering the period from Q2-Q3 2010 to Q4 2022-Q1 2023.

discrete variable for each type of financing, assigned a value of 1 if the firm reports an increase, −1 for a decrease, and 0 if unchanged. The firm-level average of these values in a given period t generates two key indicators: Needs_t and Avail_t. A positive value for Needs_t or Avail_t indicates that the firm reported an increase in external financing needs or availability for the majority of instruments.[5]

Figure 3.1 presents the average values of Needs_t and Avail_t by survey round. Needs_t is predominantly positive, peaking before the euro area recession triggered by the sovereign debt crisis in 2011 and again in early 2020, just before the COVID-19 outbreak. Between 2010 and early 2014, Avail_t remained mostly negative, primarily due to declining bank loan availability following the sovereign debt crisis. However, from 2014 onward, availability improved, turning positive and peaking in the years leading up to COVID-19, coinciding with the ECB's expansion of its monetary policy toolkit to ease financing conditions and strengthen policy transmission. Toward the end of our sample period, Avail_t declined into negative territory, reflecting tightening lending conditions even before the ECB's first policy rate hike in July 2022.

Figure 3.1: Firms' needs and availability for external finance over time



The figure shows the weighted average of the two indicators Needs_t and Avail_t for each survey round for the complete sample of the survey. The weights, included in the SAFE dataset, restore the proportions of the economic importance of each size class, economic activity and country.

To complement our analysis, we use the ORBIS dataset, which provides annual balance sheet and profit and loss account information for firms. From this dataset, we construct a variety of firm-level financial variables. Our dependent variable, the investment rate, is measured as the annual growth rate of fixed capital. Additionally, we include key financial indicators such as financial leverage, debt burden ratio, liquidity

[5] As an example, a firm that reports increasing need for bank loans, credit lines and trade credit while decreasing equity and debt security issuance will have $\text{Needs}_t = \frac{1+1+1-1-1}{5} = 0.2$. The same value occurs when need for bank loans were increasing while those for all other instruments (credit lines, trade credit, equity and debt security issuance) remained unchanged.

ratio, return on equity (ROE), internal funding, sales growth. We also create two measures of firm size: an SME dummy based on the number of employees and continuous variable based on total assets.[6] To ensure data quality, we follow Kalemli-Özcan et al. (2022) and exclude firms reporting negative total assets, sales, or age (measured as years since incorporation). We also drop observations where fixed assets are negative or missing. Finally, we winsorize the bottom 5% of each variable by country, year and sector, thereby reducing the influence of extreme outliers. Due to potential data quality issues among firms with unusually high investment levels, we trim the investment variable at the 95th percentile. For all our variables of interest the kurtosis remains below 10.

To match the semi-annual SAFE data with the annual ORBIS data, we align each SAFE observation with the most recent ORBIS observation that precedes the SAFE reference period[7]. Our final sample consists of 27,439 firms and 71,301 observations. Table 3.1 presents the descriptive statistics of the variables used in our analysis.

Table 3.1: Summary Statistics

Variable	Mean	SD	p25	p50	p75	Min	Max	N
Needs_t	0.09	0.49	0.00	0.00	0.33	-1.00	1.00	71,301
Avail_t	0.12	0.48	0.00	0.00	0.33	-1.00	1.00	71,301
Investment_{t+1}	0.05	0.28	-0.07	-0.00	0.11	-0.93	1.23	71,301
Leverage_t	0.21	0.21	0.02	0.16	0.33	0.00	1.38	71,301
ROE_t	0.09	0.39	0.01	0.08	0.18	-1.61	1.69	71301
Internal fund_t	0.17	0.17	0.07	0.14	0.23	-0.34	1.09	71,301
Liquidity_t	1.47	1.44	0.68	1.07	1.64	0.03	8.09	71,301
Sales growth_t	0.06	0.29	-0.06	0.03	0.12	-0.84	1.73	71,301
SME	0.57	0.49	0.00	1.00	1.00	0.00	1.00	71,301
Size_t	16.04	2.39	14.18	16.20	17.81	6.78	24.63	71,301
Debt burden_t	0.17	0.17	0.03	0.11	0.29	0.00	0.61	65,019

The table shows the descriptive statistics of the variables used in the analysis. The statistics are weighted according to the SAFE weights that restore the proportions of the economic importance of each size class, economic activity and country. For a detailed description of the variables see 3.8.

Our analysis of monetary policy effects builds on the monetary economics literature that identifies causal effects of monetary policy using high-frequency movements in interest rates around central bank announcements (Kuttner, 2001; Gurkaynak et al., 2005). The key assumption is that within a sufficiently short window, typically 30 minutes, these interest rate movements are driven solely by central bank announcements, provided no other major events occur within the window. We obtain monetary policy surprises from the Euro Area Monetary Policy Event-Study Database (EA-MPD), compiled by Altavilla et al. (2019). They extract monetary policy surprises

[6]Table 3.8 provides a detailed overview of the variable definitions.

[7]For our main dependent variable, future investment, the match differs depending on the reference period of the SAFE rounds. in the survey rounds covering the second and third quarter of the year, we assign to each firm the average investment rate over the two years following the SAFE reference period. In the survey rounds covering the the fourth quarter of the year Y and the first quarter of the year $Y + 1$, we assign the investment rate of the year $Y + 1$. This approach prevents the same investment rate from being matched to survey responses from multiple periods.

from changes in OIS rates at 1, 3, and 6-month maturities, as well as 1, 2, 5, and 10-year maturities, measured around the ECB press release and press conference window.[8] For our analysis, we focus on the forward guidance surprise, which is derived from the press conference window and has been shown to have the strongest effects on yields with maturities between 1 and 5 years which are those more relevant for firms' long-term borrowing. To align these high-frequency monetary policy surprises with our lower-frequency firm-level data, we follow Bauer and Swanson (2023a) and aggregate the surprises by summing them over each six-month SAFE reference period. Figure 3.6 in the appendix illustrates the aggregated monetary policy surprises, while Figure 3.7 presents the original series.

3.3 An Economic Interpretation of Funding Needs and Availability

3.3.1 Factors Related to Needs, Availability and Investment

In this section, we evaluate how the survey-based data on corporate demand for external finance (Needs_t) and credit supply (Avail_t) are related to firms' performance and investment. Specifically, we focus on the selected set of variables presented in the previous section.

Leverage and the debt burden ratio are used to assess balance sheet vulnerabilities, serving as proxies for specific financial frictions related to credit risk (see Ottonello and Winberry (2020) for a detailed discussion of leverage). The liquidity ratio indicates a firm's ability to meet its current debt obligations without relying on external capital and it can also serve as internal funding. Similarly, ROE and our internal funding indicator can reflect a firm's broader capacity to finance its business projects independently. Additionally, we consider past sales growth as an indicator of the firm's potential for expansion. Under information asymmetry, ROE, sales growth, and internal funding measures can also act as signals of financial health to lenders, influencing their assessment of a firm's creditworthiness. Finally, we consider firm's size based on the number of employees and total assets, following Gertler and Gilchrist (1994) who show that smaller firms face greater financial constraint.

Table 3.2 displays the pairwise conditional correlations of all variables used in the econometric analysis with our survey-based funding needs and availability.[9] The correlations are conditioned on sector and time by country fixed effects. Both, funding needs and availability, are positively correlated with future investment, and negatively correlated with each other. This suggests that these two survey-based variables capture distinct types of information that correlate positively with investment. The interpretation of needs and availability becomes clearer, when compared with other financial ratios. Specifically, firms' reported needs for external finance are negatively correlated with profitability, liquidity, and our internal funding indicator — an indication that, when internal funds are abundant, firms tend to turn less at external funds, in line

[8] Altavilla et al. (2019) follow the methodology of Gurkaynak et al. (2005) and Swanson (2021) to identify three latent factors from the OIS rates over their full sample. To provide an economic interpretation, they impose restrictions on how these factors influence the OIS rates using a rotation matrix, which results in three distinct policy surprises: the target factor, the forward guidance factor, and the QE factor.

[9] See Table 3.8 in the appendix for the unconditional correlations.

with the Pecking Order Theory. In contrast, funding needs are positively correlated with leverage. This is plausible in the sense that higher external funding needs should increase leverage. The correlation between reported funding availability and financial ratios indicates that firms perceive greater access to external financing when their performance improves and their financial position strengthens. This is reflected in the positive correlation with ROE and sales growth, as well as the negative correlation with leverage and debt burden, suggesting that firms with stronger financial health report higher funding availability.

This initial piece of evidence highlights that firms' responses to funding needs and funding availability capture distinct information sets, each correlating differently with firms' accounting variables. While these are only conditional contemporaneous correlations, the findings naturally suggest that availability reflects financial conditions, whereas needs are rather tied to investment opportunities. In the following sections, we enhance our empirical analysis by introducing a more structured approach that extends beyond correlations.

Table 3.2: Correlation of needs and availability with accounting variables

	Needs_t	Avail_t
Needs_t		-0.04^{***}
Avail_t	-0.04^{***}	
Leverage_t	0.15^{***}	-0.07^{***}
Investment_{t+1}	0.08^{***}	0.08^{***}
ROE_t	-0.02^{***}	0.06^{***}
Internal fund_t	-0.15^{***}	0.12^{***}
Liquidity_t	-0.02^{***}	0.00
Sales growth_t	0.03^{**}	0.10^{***}
SME-dummy	-0.03^{**}	-0.06^{***}
Size_t	0.01^{***}	0.02^{***}
Debt burden_t	0.22^{***}	-0.38^{***}

The table shows the pairwise correlations between funding needs and availability with leverage, investment, ROE, internal funding, liquidity, sales growth, SME-dummy, size, and debt burden. Correlations are weighted (see notes to Table 3.1) and conditional to sector and country times wave fixed effects. Standard errors are clustered at firm and wave level. ***, ** and * denote significance at the 1%, 5% and 10% level, respectively.

3.3.2 Effects of Funding Needs and Availability on Investment

In this section, we examine how information from the survey affects firms' investment decisions. We begin with a straightforward investment equation that incorporates the set of financial ratios introduced in the previous section, $X_{i,t}$, supplemented by funding needs and availability:

$$\text{Inv}_{i,t+1} = \beta_1 \text{Needs}_{i,t} + \beta_2 \text{Avail}_{i,t} + \gamma X_{i,t} + \text{Inv}_{i,t} + \alpha_{i,s,t} + \epsilon_{i,t} \tag{3.1}$$

Depending on the specification, the set of fixed effects, $\alpha_{i,s,t}$, used to control for potential omitted variable biases are: time and sector, country-by-time fixed effects, and firm fixed effects. However, the use of firm fixed effects significantly reduces the number of observations, as relatively few firms participate in the survey across multiple years. We cluster standard errors at firm and time levels.

Table 3.3 presents the estimated coefficients from the investment function. Focusing on columns 1 and 2, we find that the signs and significance levels of the coefficients remain consistent regardless of whether time and sector fixed effects are included in the specification. Future investment increases on average for firms that have previously invested and experienced high sales growth. Conversely, having high leverage or being a small firm reduces investment. While availability of internal funds does not seem to play a role, the investment rate of firms with higher liquidity is higher. In the last column, we include firm fixed effects to remove firm-specific unobserved effects, such as firms' average investment. In this case, it turns out that firms that have already invested tend to invest less in the subsequent period, in line with the literature that shows that investment is lumpy and firms' do not invest in subsequent periods (DeAngelo et al., 2011; Im et al., 2020). Moreover internal funds, which can be interpreted as earnings-based collateral, become significantly and positively associated with higher investment rate. Our primary interest is to assess the additional role of funding needs and funding availability as predictors of investment. In Table 3.3, both variables exhibit a similar positive impact on investment, even after controlling for firm-level financial variables. This finding suggests that our survey-based measures capture valuable information beyond traditional accounting variables, highlighting their importance in explaining future investment decisions.

To assess the economic relevance of the two variables, we consider how much is the increase in future investment for firms that move from the 25th percentile (no changes in needs/availability) to the 75th percentile of the distribution of Needs_t, respectively of Avail_t (equivalent to a reported moderate increase of needs and availability, equal to 0.3, when the maximum is 1). Focusing on the second column, the increase in future investment would be by 1 percent and 0.8 percent, respectively. In alternative, we can also think in terms of standard deviations. Our estimates imply that a one standard deviation increase in needs and availability (which is similar for both variables at around 0.48) implies an increase in future investment by 1.6 percent and 1.2 percent, respectively.

Table 3.3: Effects of funding needs and availability on future investments

	(1) Inv_{t+1}	(2) Inv_{t+1}	(3) Inv_{t+1}
Needs_t	3.45^{***}	3.45^{***}	1.74^{***}
	(0.47)	(0.45)	(0.39)
Avail_t	2.91^{***}	2.59^{***}	1.59^{***}
	(0.38)	(0.40)	(0.48)
Investment_t	0.06^{***}	0.05^{***}	-0.13^{***}
	(0.01)	(0.01)	(0.01)
Leverage_t	-4.85^{***}	-4.64^{***}	-24.42^{***}
	(0.87)	(0.89)	(3.06)
ROE_t	4.47^{***}	4.47^{***}	3.10^{***}
	(0.56)	(0.57)	(0.66)
Internal Fund_t	-1.83	-1.54	7.21^{**}
	(1.13)	(1.22)	(3.02)
Liquidity_t	0.92^{***}	0.97^{***}	2.08^{***}
	(0.13)	(0.14)	(0.30)
Sales growth_t	5.49^{***}	5.77^{***}	0.84
	(0.94)	(0.96)	(0.86)
SME-dummy	-1.12^{***}	-1.25^{**}	-0.67
	(0.39)	(0.45)	(1.18)
Observations	71,301	71,301	59,275
R-squared	0.02	0.04	0.48
Firm FE	N	N	Y
Time FE	N	N	Y
Sector FE	N	Y	N
Country × FE	N	Y	N

The table reports estimated coefficients of a regression of future investment on needs, availability, and controls. All regressions are weighted using the SAFE weights (see notes to Table 3.1). Standard errors clustered at firm and wave level. ***, ** and * denote significance at the 1%, 5% and 10% level, respectively.

To disentangle potential differences in the channels through which funding needs and availability affect investment, we adopt an alternative specification that exploits firm-level heterogeneity. In Table 3.4, we interact Needs_t and Avail_t with firm characteristics commonly used as proxies for financial constraints. The first two columns examine size-based constraints, following Gertler and Gilchrist (1994) and Perez-Quiros and Timmermann (2000). Column 1 includes an SME dummy, which equals one if the firm has fewer than 250 employees. Column 2 uses an alternative size dummy, that equals one if the firm's log total assets are below the 75th percentile. The third and fourth columns focus on debt-based financial constraints, in line with the financial accelerator literature. Column 3 defines a high-leverage dummy, which equals one if the firm's leverage ratio is above the 75th percentile (equivalent to 32% leverage). Column 4 introduces a high debt burden dummy, which equals one if the debt burden ratio exceeds 27% (the 75th percentile).

Examining the interaction terms, we find no statistically significant differences in the impact of Needs on investment across firm types. This suggests that past external funding needs drive investment similarly for small and large firms, as well as for firms with high and low leverage. However, funding availability exhibits a stronger effect on investment for financially constrained firms, with all estimated availability interaction coefficients being positive and mostly statistically significant. This finding implies that investment in small firms and those with high leverage or debt burden is more sensitive to changes in funding availability. Our results align with Love and Zicchino (2006), who finds that financial factors play a larger role in investment decisions in countries with less developed financial systems.

Our results also suggest that large firms and firms with low leverage, which typically have strong economic and financial performance, are less dependent on the availability of external funding. Instead, their investment decisions are primarily driven by investment opportunities, reinforcing the idea that financially stable firms can fund their investment opportunities regardless of credit market conditions.

Table 3.4: Link of financial constraint and the effects of needs and availability on investment

	Size-based financial constraints		Debt-based financial constraints	
	Inv_{t+1} FC=SME	Inv_{t+1} FC=LowLogTA	Inv_{t+1} FC=High_Lev	Inv_{t+1} FC=High_DebtBurd
Needs_t	3.876^{***}	3.652^{***}	3.216^{***}	3.591^{***}
	(0.736)	(0.664)	(0.515)	(0.475)
Avail_t	1.734^{**}	1.913^{***}	2.287^{***}	1.930^{***}
	(0.817)	(0.508)	(0.414)	(0.443)
FC	-1.923^{***}	1.110^{*}	-2.008^{***}	-3.514^{***}
	(0.622)	(0.614)	(0.439)	(0.541)
FC$\times\text{Needs}_t$	-0.770	-0.318	0.753	-0.305
	(0.762)	(0.635)	(0.811)	(0.810)
FC$\times\text{Avail}_t$	1.440	0.989^{*}	1.086^{*}	1.732^{**}
	(0.848)	(0.573)	(0.602)	(0.680)
Observations	71,301	71,301	71,301	65,019
R-squared	0.03	0.03	0.03	0.04
Sector FE	Y	Y	Y	Y
Country × FE	Y	Y	Y	Y

The table reports the estimated coefficients of a model built on equation 3.1 by adding interactions with the variables signaling financially constrained firms. All regressions are weighted using the SAFE weights (see notes to Table3.1). Standard errors clustered at firm and wave level. ***, ** and * denote significance at the 1%, 5% and 10% level, respectively.

3.3.3 A Quasi-Natural Experiment: Portuguese Bank Branches

In this section, our objective is to further support our assumption that reported funding availability is primarily linked to financial conditions, while reported funding needs are mainly associated with investment opportunities. We use a quasi-natural experiment that we draw from the literature on bank branch closures and their effects on credit lending. Specifically, we build on the insights of Bonfim et al. (2021), who argue that the decline in the number of bank branches in Portugal after the financial crisis was not driven by profitability considerations but was instead "forced upon banks." Especially between 2012 and 2015, the density of branches in Portugal decreased significantly in different regions, in part due to restructuring agreements with the European Commission. This exogenous variation in bank branches provides a natural setting to examine how changes in credit supply conditions influence firms' perceived funding availability while leaving investment opportunities largely unaffected.

To evaluate the impact of bank branch closures on funding needs and availability, we collect data on the annual number of local bank branches at the NUTS3 subregion level in Portugal from the Portuguese Banking Association (APB).[10] We merge these data with our SAFE-ORBIS dataset by averaging the funding needs and availability of Portuguese firms within each subregion. Analogously, we aggregate the usual balance sheet variables within the subregions.

While the variation in bank branch density arguably provides an exogenous source of changes in current financial conditions, it could still be influenced by variables correlated with past investment opportunities in specific regions. To address potential endogeneity concerns, we control for lagged funding needs and availability, along with our control variables, SME, leverage, ROE, sales growth internal funding, and liquidity. We also include subregional and time-fixed effects to account for unobserved regional and time-specific factors.

We then run two-way fixed effects regressions of funding needs and availability controlling for firm-level variables with our sample going from 2010 to 2023. Here, we face a trade-off: Including the whole sample increases the number of observations and helps increase the statistical power of the test. However, because the natural exogeneity coming from the financial crisis took place in the first half of the 2010 to 2020 decade, including the whole sample could potentially relate the closure of bank branches to other factors, not only related to financial conditions. To address this concern, we present results for both the full sample period (2010–2023) and the subperiod most relevant to banking distress (2011–2015). Due to possible stationarity issues we present results on levels using time-fixed effects and using first differences.

Another important consideration is that not all banks were equally affected by the financial crisis. For instance, only three major national banks—Caixa Geral de Depósitos (CGD), Banco BPI (BPI), and Banco Comercial Português (BCP)—were required to close bank branches as part of restructuring agreements with the IMF, the ECB, and the European Commission in exchange for bailout support.[11] Accordingly, in a third robustness exercise, we restrict the sample to branches operated by these three banks.

Table 3.5 presents our main findings. Columns 1 to 2 examine the relationship between the number of bank branches and funding needs with time-fixed effects and first differences, respectively. Panel A presents the results for the full sample. The results show that the number of bank branches in Portugal is largely unrelated to firms' funding needs, suggesting that these are not influenced by changes in financial conditions. Columns 3 and 4 repeat the analysis with funding availability as the dependent variable. In contrast to the results for funding needs, the coefficients here are positive and statistically significant, indicating that changes in financial conditions do

[10] The data is publicly available at `apb.pt/pt/publicacoes/estatisticas`.

[11] See the European Commission Press Release from July 24, 2013: `https://ec.europa.eu/commission/presscorner/detail/en/ip_13_738`.

affect firms' perceived access to external funding. Specifically, a higher number of bank branches is associated with greater reported funding availability by firms in the same regions. These findings reinforce the idea that financial conditions primarily shape firms' perceptions of funding availability, while need for external financing remains unchanged in response to such changes.

Panel B restricts the sample to the core period of the banking crisis (2011–2015). Although the coefficients are somewhat weaker — probably due to the reduced sample size — the effect of bank branches on funding availability remains statistically significant in the first-difference specifications, whereas no significant relationship is found for funding needs. Panel C further narrows the analysis to include only the subset of banks that closed branches as part of restructuring agreements. In this case, an increase in the number of bank branches is again strongly associated with higher reported funding availability, but not with funding needs. Notably, the coefficients on funding availability are substantially larger than those observed in the full-sample regression.

Following forced closure of bank branches unrelated to profitability, Portuguese firms perceived the availability of funding to decrease substantially, without having an effect on their investment opportunities. These results highlight that exogenous variation in financial conditions alone has a large effect on availability of funding reported by firms, while funding needs are not affected by financial conditions.

Overall, our findings indicate that both funding needs and funding availability are important drivers of future investment, even after controlling for accounting variables. In addition, these variables capture fundamentally different information, allowing us to better distinguish between the roles of investment opportunities and financial conditions in shaping firms' investment decisions.

Table 3.5: Impact of bank branches on needs and availability

Panel A: Full sample (2011 - 2023)				
	Needs		Availability	
	(1)	(2)	(3)	(4)
Bank branches	−0.02	−0.09	0.08***	0.07***
	(0.01)	(0.06)	(0.02)	(0.02)
Observations	132	121	132	121
R^2	0.49	0.48	0.81	0.30
Panel B: Short sample (2011 - 2015)				
Bank branches	−0.04	−0.01	0.02	0.03***
	(0.03)	(0.01)	(0.02)	(0.01)
Observations	55	44	55	44
R^2	0.18	0.78	0.86	0.61
Panel C: Full sample with selected banks				
Bank branches	0.03	0.01	0.16***	0.37***
	(0.05)	(0.06)	(0.02)	(0.10)
Observations	130	119	130	119
R^2	0.49	0.46	0.81	0.38

The table presents the estimated coefficients from a linear regression of needs and availability on bank branches and further controls. Columns 1 and 2 show the regression of funding needs on lagged bank branches with region and time fixed effects and first differences regression, respectively. Column 3 and 4,repeat the regressions for funding availability. Panel A shows the results for the full sample period. Panel B uses the short sample, from 2011 to 2015, and panel C restricts the sample to branches of three banks: CGD, BPI, and BCP. ***, ** and * denote significance at the 1%, 5% and 10% level, respectively.

3.4 Monetary Policy Transmission

In this section, we examine how the response of firms' investment to monetary policy depends on their needs and availability of external funding. Addressing this question will help clarify the distinct roles that structural factors and financial conditions play in the transmission of monetary policy.

3.4.1 Unconditional Monetary Policy Transmission

We begin by analyzing the average effect of monetary policy on firm-level investment, which provides a benchmark for assessing the contribution of our cross-sectional results in a subsequent analysis.

To estimate the impulse response functions (IRFs) of monetary policy on investment, we use local projections, following Jordà (2005). Specifically, we regress future firm-level investment on monetary policy surprises while controlling for firm-level accounting variables and macroeconomic conditions. In addition, we incorporate sector-and country-fixed effects to account for possible industry-specific and regional unobserved effects:

$$I_{i,t+h} = \alpha_h + \beta_h \cdot mps_t + \theta_h I_{i,t-1} + \Gamma_h X_{i,t-1} + \gamma_c + \delta_s + \varepsilon_{i,t+h} \tag{3.2}$$

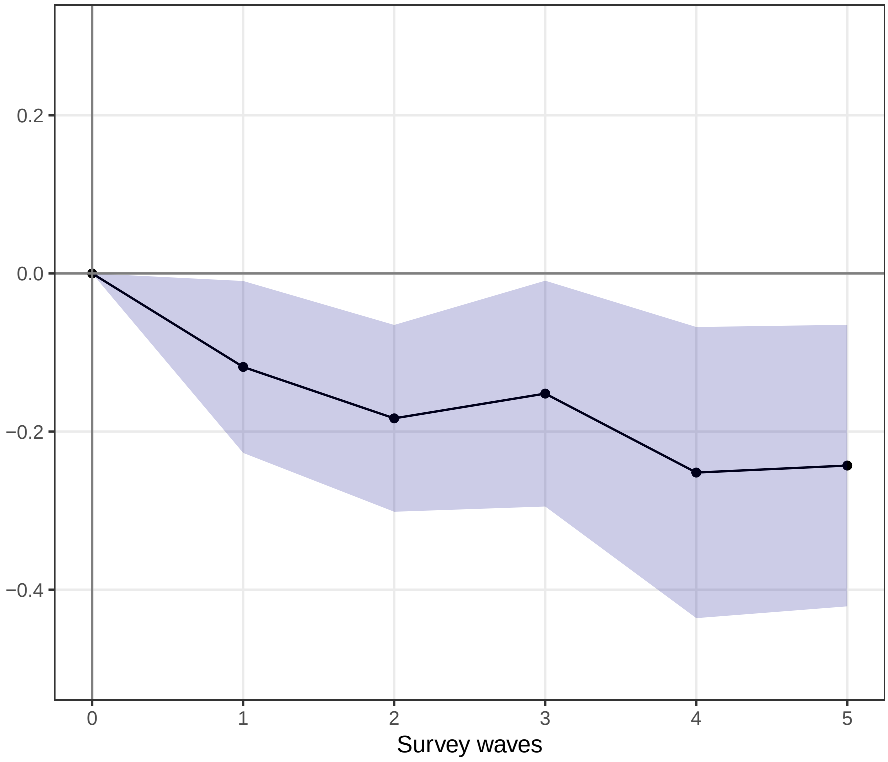
where $I_{i,t+h}$ represents the investment rate of firm i at horizon h, mps_t the monetary policy surprises at time t, $X_{i,t-1}$ firm-level controls measured before the monetary policy surprises, γ_c and δ_s are country and sector fixed effects.[12] $X_{i,t-1}$ contains the variables used in the analysis of Table 3.3 and lagged values of funding needs and availability. Due to the rotating participation of firms in the SAFE survey, relatively few firms remain in the sample in several consecutive survey rounds. As a result, including lagged survey variables leads to a substantial loss of observations in our regressions. To mitigate this issue, we impute missing lagged values of needs and availability by replacing them with the average for firms in the same sector, country, size category that are interviewed in the corresponding period. Due to data limitations, we restrict our analysis to a maximum horizon of five survey rounds — equivalent to two and a half years. In our judgment, this window is sufficient to capture potential lagged effects of monetary policy on real investment, while avoiding a substantial reduction in sample size.[13]

Figure 3.2 presents the results of our local projection analysis. We find that a 1 basis point increase in monetary policy surprises leads to an average decline in investment of 0.2 percentage points after one year and 0.25 after two years. The effect is statistically significant and persists for several periods. Our findings closely align with Durante et al. (2022), who report that a monetary policy surprise reduces investment by 0.3 percentage points after one year in the euro area. This similarity highlights the validity of our results, especially given the differences in the sample coverage and institutional settings. Moreover, our estimated effect is substantially larger than that found by Cloyne et al. (2023) for the U.S. and U.K., likely due to their focus on publicly listed firms, whereas our sample includes a broader set of private companies. Additionally, we observe a U-shaped response of investment to monetary policy, consistent with previous literature, including studies using VAR-based approaches such as Christiano et al. (2005).

[12]Since the data is structured to align with survey rounds, the time periods correspond to the six-month intervals in which the survey takes place.

[13]In our analysis, we use the full sample available at each horizon. However, our results remain robust when restricting the sample only to firms present for five consecutive survey waves.

Figure 3.2: Impact of monetary policy on firm-level investment



The figure presents the estimated IRF of monetary policy surprises on firm-level investment using local projection. The regression includes sector and country fixed effects and controls for past investment, leverage, ROE, sales growth, internal funding, liquidity, and past funding needs and availability. We report 90% confidence intervals, calculated using Driscoll-Kraay standard errors, clustered at the firm and time levels.

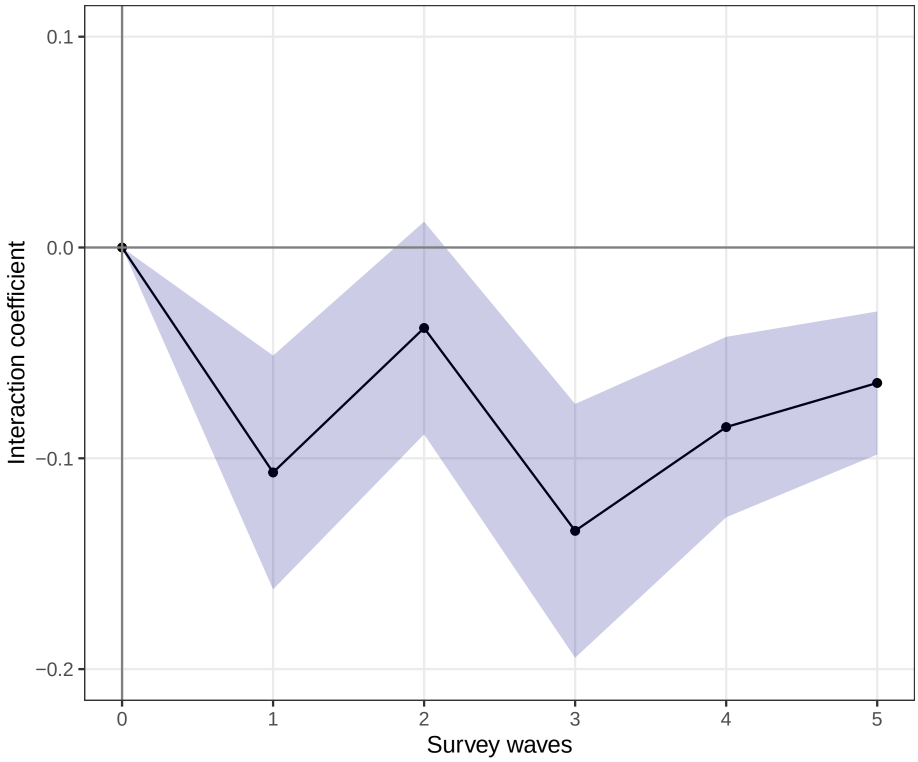
3.4.2 The Role of Funding Needs and Availability in Monetary Policy Transmission

As demonstrated earlier, funding needs are closely tied to fundamental structural factors, while funding availability is strongly associated with financial conditions. We now seek to understand how they influence the effect of monetary policy on investment. To achieve this, we extend Equation 3.2 by incorporating interaction terms with needs and availability. Specifically, we estimate the following extended local projection model:

$$I_{i,t+h} = \alpha_h + \beta_h \cdot mps_t + \phi_h \cdot mps_t \cdot Z_{i,t-1} + \theta_h I_{i,t-1} + \Gamma_h X_{i,t-1} + \gamma_{t,c} + \delta_s + \varepsilon_{i,t+h} \quad (3.3)$$

where Z represents either funding needs or funding availability at the firm-level. Since our primary interest lies in examining the effects of monetary policy conditional on firm-level characteristics, we include country-by-time fixed effects to account for unobserved macroeconomic factors that vary across countries and over time. However, this approach renders the coefficient of monetary policy (β_h) nonidentifiable. This underscores the importance of our unconditional estimates from the previous section, which serve as a benchmark for comparison. The fact that we find a negative effect of monetary policy on investment unconditionally, is crucial for interpreting our key parameter, ϕ_h. Specifically, a positive estimate of ϕ_h implies that the effect of monetary policy on investment is weaker for firms with higher Z, which means that firms with greater funding needs or greater availability experience a more muted response to monetary shocks. In contrast, a negative estimate of ϕ_h suggests a stronger monetary policy effect for firms with higher Z, indicating that these firms are more responsive to policy changes.

Figure 3.3: The role of external funding needs in the monetary policy transmission



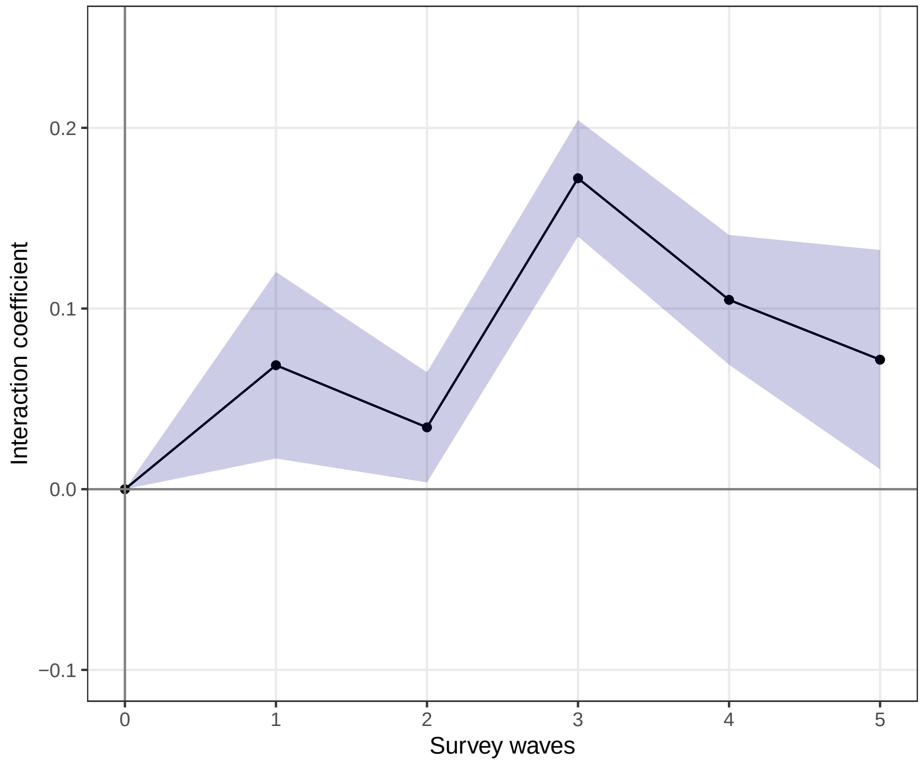
The figure presents the estimated IRF of monetary policy surprises interacted with past external funding needs on firm-level investment using local projection. The regression includes sector and country-by-time fixed effects and controls for past investment, leverage, ROE, sales growth, internal funding, liquidity, and past funding needs and availability. We report 90% confidence intervals, calculated using Driscoll-Kraay standard errors, clustered at the firm and time levels.

Figure 3.3 presents the interaction effect ϕ_h from our local projection analysis. Our findings indicate that firms with higher funding needs respond significantly more to monetary policy shocks. Specifically, a one basis point increase in monetary policy surprises leads to an additional 0.1 percentage point decline in investment after six months, provided that funding needs have increased during the last six months (i.e., when needs = 1). From the opposite perspective, i.e. following a monetary easing surprise, firms that reported an increase in funding needs experience, on average, a 0.1 percentage point larger investment increase compared to firms whose funding needs remained unchanged. This effect is also statistically significant for up to 2 years after the shock.

As we have shown, external funding needs are driven primarily by structural factors. Hence, Figure 3.3 provides evidence that fundamentals play a key role in the transmission of monetary policy. In particular, our findings indicate that monetary easing is most effective for firms with strong fundamentals. By easing monetary policy, central banks can support investment by loosening financial conditions. From a time-series perspective, this also implies that when financial conditions are tight and structural factors remain weak, the effectiveness of monetary policy is highly constrained. Although a monetary easing surprise may provide some relief, it cannot resolve structural barriers, such as regulatory constraints or uncertainty, that fundamentally shape investment incentives.

To assess whether financial conditions also influence the investment response to monetary policy, we repeat our local projection analysis, this time interacting monetary policy surprises with external funding availability. Figure 3.4 presents the interaction coefficient estimated from this specification. Our results indicate that the interaction coefficient between monetary policy surprises and external funding availability is positive, suggesting that firms with greater access to external funding are less affected by monetary policy. Specifically, a one basis point increase in monetary policy surprises leads to a smaller decline in investment for firms that perceived an increase in funding availability, compared to those for which funding availability remained unchanged. This difference in the investment response amounts to 0.15 percentage points after three periods, which is almost the entire effect on investment observed in Figure 3.2.

Figure 3.4: The role of external funding availability in the monetary policy transmission



The figure presents the estimated IRF of monetary policy surprises interacted with past external funding availability on firm-level investment using local projection. The regression includes sector and country-by-time fixed effects and controls for past investment, leverage, ROE, sales growth, internal funding, liquidity, and past funding needs and availability. We report 90% confidence intervals, calculated using Driscoll-Kraay standard errors, clustered at the firm and time levels.

Since availability serves as a proxy for financial conditions, we interpret these results as evidence that firms with better financial conditions — or firms with easier access to external funding — are less responsive to monetary policy. The intuition behind this result is the following: all else equal, monetary easing is most effective when financial conditions are particularly tight and vice versa. Changes in borrowing costs should have little impact on the investment decisions of firms that consistently secure the funding they need thanks to their strong financial positions.

Monetary Policy Effects Conditional on Funding Needs and Availability With Interacted Controls

A growing body of research has documented that the transmission of monetary policy to investment varies with other firm-level characteristics (e.g. Durante et al., 2022; Cloyne et al., 2023; Jungherr et al., 2024). This raises the question of whether the heterogeneity we observe — captured through funding needs and availability — reflects distinct transmission channels or simply proxies for underlying firm characteristics. To address this, we estimate the local projections for funding needs and availability while controlling for firm-level variables interacted with monetary policy surprises. Specifically, alongside the interaction terms between monetary policy surprises and funding needs and availability, we include interactions with past investment, SME, leverage, ROE, liquidity, internal funding, and sales growth.

Table 3.6 presents the estimates from the local projection, covering the the first till the fifth period after a monetary policy shock. The period corresponds to a bi-annual wave so we show results from 6 months to 2.5 years following the shock. The first two rows of the table highlight that higher funding needs amplify the effect of monetary policy on investment, with coefficients statistically significant in all periods except the second. Similarly, monetary policy has a stronger impact on investment when perceived funding availability is low, as shown in the second row. The magnitude of the coefficients is comparable to those obtained without interacted controls. These results therefore confirm that the effects attributed to funding needs and availability are not confounded by other firm-level sources of heterogeneity.

Rows 3 to 9 of Table 3.6 report the coefficients on the interaction terms between firm-level control variables and the monetary policy shock. A few characteristics appear to shape the transmission of monetary policy to investment. Surprisingly, leverage does not show a significant effect — potentially because financial conditions are already captured by the perceived availability of funding.[14] Similarly, SME and internal funding do not yield consistent or significant effects across most horizons.

Three variables show clearer patterns: past investment, ROE, and liquidity. Firms with higher past investment tend to respond less to monetary policy, possibly because monetary policy should play a diminished role, once firms are already committed to an investment path. Both ROE and liquidity are associated with a stronger response to monetary policy, as reflected in their negative interaction coefficients. Interpretation is difficult given the endogeneity of these variables. Although ROE could be highly correlated with Tobins' Q, it is also a proxy for internal funding. Similarly, while one might expect that high liquidity allows firms to self-finance investment, and thus reduces sensitivity to interest rates, empirical evidence suggests otherwise. Cash

[14]The role of leverage in the transmission of monetary policy remains contested in the literature. While some studies find that high leverage amplifies the effect of monetary policy (Durante et al., 2022), others suggest that its influence is more limited (Ottonello and Winberry, 2020).

reserves are often held as precautionary balances to cover unforeseen expenses, rather than being readily available for investment purposes (Acharya et al., 2013).

Table 3.6: Response of investment to monetary policy based on needs, availability, and balance-sheet variables

	I_{t+1} (6 months)	I_{t+2} (1 year)	I_{t+3} (1.5 year)	I_{t+4} (2 years)	I_{t+5} (2.5 years)
FG*Needs	-0.11^{***}	-0.04	-0.14^{***}	-0.10^{***}	-0.08^{***}
	(0.03)	(0.03)	(0.05)	(0.03)	(0.02)
FG*Avail.	0.07^{*}	0.08^{***}	0.14^{***}	0.10^{***}	0.03
	(0.04)	(0.02)	(0.03)	(0.03)	(0.03)
FG*Inv.	0.14^{**}	0.11	0.06^{**}	0.28^{***}	0.23^{***}
	(0.06)	(0.12)	(0.03)	(0.05)	(0.07)
FG*SME	0.01	0.14^{*}	-0.12	-0.08	-0.19^{***}
	(0.06)	(0.07)	(0.08)	(0.09)	(0.02)
FG*Lev.	0.10	-0.003	-0.05	0.23^{***}	0.10
	(0.13)	(0.03)	(0.13)	(0.05)	(0.13)
FG*ROE	-0.06^{**}	-0.10^{***}	-0.04	-0.12^{***}	-0.33^{***}
	(0.02)	(0.02)	(0.03)	(0.04)	(0.03)
FG*Liq.	-0.01	-0.07^{***}	-0.06^{***}	-0.04^{**}	-0.06^{***}
	(0.03)	(0.02)	(0.004)	(0.02)	(0.01)
FG*Internal fund.	-0.04	0.21	-0.15^{**}	-0.02	0.42^{***}
	(0.05)	(0.17)	(0.06)	(0.19)	(0.10)
FG*Sales growth	-0.03^{***}	0.06	0.06	0.08^{***}	0.11^{***}
	(0.01)	(0.04)	(0.05)	(0.01)	(0.01)
Observations	87,693	73,261	62,803	53,233	43,909
R^2	0.02	0.02	0.02	0.02	0.02

This table reports the estimated coefficients from a regression of firm-level investment on monetary policy surprises interacted with lagged values of needs, availability, investment, SME dummy, leverage, return on equity, liquidity, internal funding, and sales growth. Coefficients of uninteracted variables are omitted. The regression includes sector and country-by-time fixed effects. Driscoll-Kraay standard errors, clustered at the firm and time levels, are reported in parentheses. ***, ** and * denote significance at the 1%, 5% and 10% level, respectively.

Overall, we document substantial heterogeneity in the response of euro area investment to monetary policy surprises. Although some firm characteristics contribute meaningfully to this variation, the differential sensitivity of investment based on funding availability and funding needs is robust and not confounded by other firm characteristics.

3.5 Joint Effects of Needs and Availability on Investment Response to Monetary Policy

Building on the previous results from local projections conditioned separately on funding needs and availability, a next question that arises is how these effects evolve when both dimensions are considered jointly at the firm level. Specifically, if firms with either high financing needs or low funding availability are more sensitive to monetary policy, are firms exhibiting both high needs and low availability significantly more responsive than others?

Addressing this question not only enhances our understanding of the heterogeneous responses of individual firms to monetary policy, but also speaks to a broader debate in the literature on whether firms facing financial constraints are more or less responsive to monetary policy. In standard economic models, financial constraints are typically represented as an upper bound on the amount a firm can borrow. The constraint becomes binding when a firm's demand for external finance exceeds its available supply (Bernanke et al., 1999). Within our framework, we define financial constraints as a combination of increasing financing needs and decreasing funding availability.

Figure 3.5: Response of investment to monetary policy based on needs and availability jointly

This figure shows the response of investment of constrained, unconstrained firms, and others to monetary policy. The coefficient of "Others" is β_h, for unconstrained firms is $\beta_h + \phi_h^U$ and for constrained is $\beta_h + \phi_h^C$. The regression includes sector and country-by-time fixed effects and controls for past investment, leverage, ROE, sales growth, internal funding, and liquidity. We report 90% confidence intervals, calculated using Driscoll-Kraay standard errors, clustered at the firm and time levels.

To analyze the investment response conditional on both financing needs and availability, while avoiding too many interaction terms, we classify firms into distinct groups based on two dimensions. Specifically, we construct two dummy variables: the first equals one for firms with *needs* > 0 and *availability* < 0, and the second equals one for firms with *needs* < 0 and *availability* > 0. The first group likely comprises firms with strong investment opportunities that wish to invest but are constrained by limited access to external finance. We refer to these as financially constrained firms. The second group includes firms with limited investment needs, but that perceive an increased availability of external funding. We refer to these as unconstrained firms. Firms not falling into either category are classified as others.

We then estimate local projections interacting monetary policy surprises with these two dummy variables. To allow a full comparison of the responses of constrained and unconstrained firms relative to the "others" group (baseline group), we omit time-fixed effects, allowing us to directly plot the estimated coefficients on the monetary policy surprise without any interaction. Our specification is given below, where the superscript U stands for unconstrained and C for constrained.

$$I_{i,t+h} = \alpha_h + \beta_h mps_t + \phi_h^U mps_t \mathbb{1}_{i,t-1}^U + \phi_h^C mps_t \mathbb{1}_{i,t-1}^C + \theta_h I_{i,t-1} + \Gamma_h X_{i,t-1} + \gamma_c + \delta_s + \varepsilon_{i,t+h} \tag{3.4}$$

Figure 3.5 presents the overall impulse response functions for each firm group. Firms with high financing needs and low availability — those classified as financially constrained — exhibit a significantly larger decline in investment following a positive monetary policy surprise. This effect becomes particularly pronounced after three periods (approximately 1.5 years), with investment falling by 0.42 percentage points — more than twice the magnitude observed for the "others" group. In contrast, firms with low needs and high availability — firms classified as unconstrained — show the opposite pattern. Consistent with previous findings, these firms display a markedly muted response to monetary policy shocks; notably, their investment response in period three is slightly positive.[15]

While Figure 3.5 is useful for visualizing the dynamics of the impulse response functions across groups, it does not provide evidence on whether these differences are statistically significant. To address this, Table 3.7 presents the estimated coefficients from model 3.4. The results confirm that positive monetary policy surprises have an overall negative effect on investment. This effect is significantly stronger for firms with high needs and low availability, with the estimated coefficient being both economically large and statistically significant. In contrast, the impact on firms with low needs and high availability is considerably weaker, as indicated by the positive and statistically significant coefficient. Consistent with the previous section, the largest difference in responses between groups occurs approximately 1.5 years after the shock.

[15] A possible explanation could be the central bank information effect (Nakamura and Steinsson, 2018a). Rather than cutting back, these firms may interpret the policy shift as a signal of future economic strength, leading to an increase in investment despite the contractionary monetary policy surprise.

Table 3.7: Response of investment to monetary policy based on needs and availability jointly

	I_{t+1} (6 months)	I_{t+2} (1 year)	I_{t+3} (1.5 year)	I_{t+4} (2 years)	I_{t+5} (2.5 years)
FG	-0.11^{**}	-0.21^{***}	-0.18^{**}	-0.29^{**}	-0.23^{**}
	(0.05)	(0.07)	(0.08)	(0.11)	(0.10)
FG*$\mathbb{1}^C$	-0.17^{***}	0.09	-0.24^{***}	-0.08^{**}	-0.13
	(0.06)	(0.05)	(0.08)	(0.04)	(0.10)
FG*$\mathbb{1}^U$	-0.06	0.16^{***}	0.43^{***}	0.42^{***}	-0.05
	(0.13)	(0.05)	(0.03)	(0.06)	(0.03)
Observations	87,631	73,212	62,763	53,203	43,883
R^2	0.01	0.01	0.01	0.01	0.01

This table reports the estimated coefficients from a regression of firm-level investment on monetary policy surprises interacted with the unconstrained and constrained group indicators, as specified in model 3.4. The regression includes sector and country-by-time fixed effects and controls for past investment, leverage, ROE, sales growth, internal funding, and liquidity. Driscoll-Kraay standard errors, clustered at the firm and time levels, are reported in parentheses. ***, ** and * denote significance at the 1%, 5% and 10% level, respectively.

These results suggest that financially constrained firms are significantly more sensitive to monetary policy shocks. This finding is consistent with the view that monetary policy can alleviate borrowing constraints. Firms facing increasing investment needs but limited access to external finance are likely to be constrained in their ability to invest. An expansionary monetary policy can relax these constraints, allowing greater investment. This may occur through the balance sheet channel, as improved collateral valuations improve firms' borrowing capacity, or through the bank lending channel through greater loan availability. However, among financially constrained firms, those with greater investment opportunities will benefit the most from easier access to credit.

Our findings contribute to a growing body of empirical research examining how financial constraints influence the transmission of monetary policy. A key advantage of our framework is that it does not rely on traditional balance sheet variables, which are often used as proxies for financial constraints. These variables — such as leverage — are equilibrium outcomes, and thus can be difficult to interpret causally. For example, while leverage is commonly used as a proxy for financial constraints (Ottonello and Winberry, 2020), it presents an identification challenge: a firm may appear more constrained because it has high leverage, or it may have high leverage precisely because it faces fewer constraints. In contrast, our analysis is based on two ex-ante measures — financing needs and credit availability — rather than ex-post balance sheet outcomes, providing a cleaner and more interpretable measure of financial constraint.

The fact that financially constrained firms respond more strongly to monetary policy is also consistent with a range of economic models. For example, Bernanke et al. (1999) extend their financial accelerator model to a two-sector framework, where firms face different costs of external finance, with constrained firms experiencing higher borrowing costs. They show that the investment of firms with limited access to external credit markets increases nearly three times more than the investment of firms with better credit access following a monetary policy shock. Similarly, models with binding credit constraints, such as Kiyotaki and Moore (1997), imply that monetary easing relaxes borrowing constraints, expanding firms' credit capacity and thereby stimulating investment.

3.6 Conclusion

Our study provides novel insights into how monetary policy transmission to investment is influenced by both fundamental and financial conditions, using survey-based measures of firms' funding needs and availability across the euro area. This approach provides a unique perspective on firms' investment behaviour, complementing traditional accounting-based analyses. By leveraging data from the ECB's Survey on Access to Finance and Enterprise (SAFE), we effectively disentangle the distinct roles that economic fundamentals and financial conditions play in shaping investment decisions across firms of different sizes and financial health.

Our analysis shows that monetary policy is most effective on investment when fundamentals are strong, underscoring the critical role of structural factors, such as economic growth prospects and investment opportunities. In particular, our findings indicate that following a one basis point monetary easing surprise, firms that reported an increase in funding needs experience, on average, a 0.15 percentage point larger investment increase compared to firms whose funding needs remained unchanged. This suggests that in environments where fundamentals are weak, efforts by the central banks to stimulate investment through monetary easing may face inherent limitations.

In contrast, we observe that firms with favorable financial conditions, characterized by high funding availability, exhibit a muted response to monetary policy changes. This attenuated reaction points to the fact that for firms already experiencing ease in obtaining external finance, additional monetary accommodation may not significantly influence their investment behaviour. Specifically, a one basis point increase in monetary policy surprise leads to a decline in investment of 0.1 percentage points less, almost half of the total average effect, for firms that perceived an increase in funding availability, compared to those for which funding availability remained unchanged.

By jointly examining funding needs and availability, we find that firms experiencing both rising financing needs and declining funding availability — those we characterize as financially constrained — are the most responsive to monetary policy. Using survey-based data rather than financial data, our finding reinforces existing empirical evidence while offering a clear economic mechanism: credit channel-driven improvements in financial conditions, triggered by monetary easing, can significantly stimulate investment activity, particularly among firms with stronger investment opportunities.

To conclude, our research underscores the complexity of monetary policy transmission to investment, influenced by a combination of structural and financial factors. By highlighting the importance of these factors, we provide additional evidence that can inform more effective policy design, ultimately supporting economic growth and stability. Future research could further explore the dynamic interactions between monetary policy, structural factors, and financial conditions to deepen our understanding of these critical economic relationships.

Appendix

Appendix 3.A

Table 3.8: Description of variables

Variable	Type	Description	Source
Needs	Index(-1,1)	Need for external funding	SAFE
Avail	Index(-1,1)	Availability of external funding	SAFE
Lev	Continuous (percentage)	Financial leverage as ratio of short plus long term debt over total asset.	ORBIS
ROE	Continuous (percentage)	Return on equity	ORBIS
Int fund	Continuous (percentage)	Internal funds as the ratio of capital plus cash flow over total assets	ORBIS
Liq	Continuous (percentage)	Liquidity as current assets minus inventories over current liabilities	ORBIS
Sales gr	Continuous (percentage)	Sales growth	ORBIS
SME	Dummy	Equal to 1 for firms with less than 250 employees	ORBIS
Log TA	Continuous (percentage)	Logarithm of total assets	ORBIS
Debt bur	Continuous (percentage)	Debt burden as interest expenses over earnings before interest and taxes	ORBIS

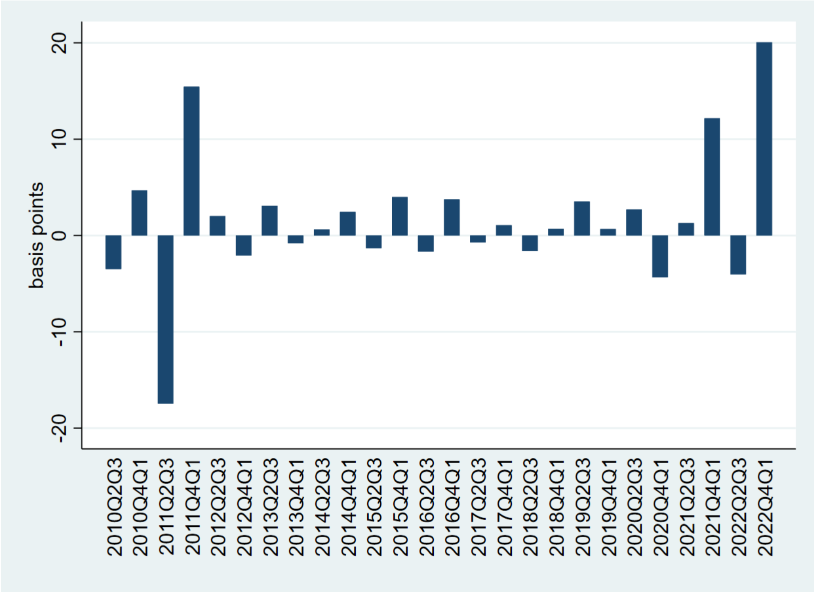
The table gives a detailed description of the variables used in the analysis, their type and the source.

Table 3.9: Correlation of needs and availability with accounting variables

	Inv_{t+1}	Needs_t	Avail_t	Lev_t	ROE_t	Int fund_t	Liq_t	Sales gr_t	SME	Log TA_t
Needs_t	0.05									
Avail_t	0.06	-0.05								
Lev_t	-0.05	0.06	-0.02							
ROE_t	0.07	-0.02	0.06	-0.03						
Int fund_t	0.01	-0.05	0.05	-0.12	0.12					
Liq_t	0.05	-0.07	0.01	-0.16	0.02	0.09				
Sales gr_t	0.08	0.01	0.07	-0.03	0.12	0.08	-0.03			
SME	-0.01	-0.01	-0.04	0.01	-0.01	-0.04	0.03	-0.01		
Log TA_t	0.02	0.02	0.07	0.05	-0.01	0.01	-0.06	0.00	-0.51	
Debt Bur_t	-0.11	0.07	-0.12	0.33	-0.28	-0.23	-0.17	-0.16	0.07	-0.10

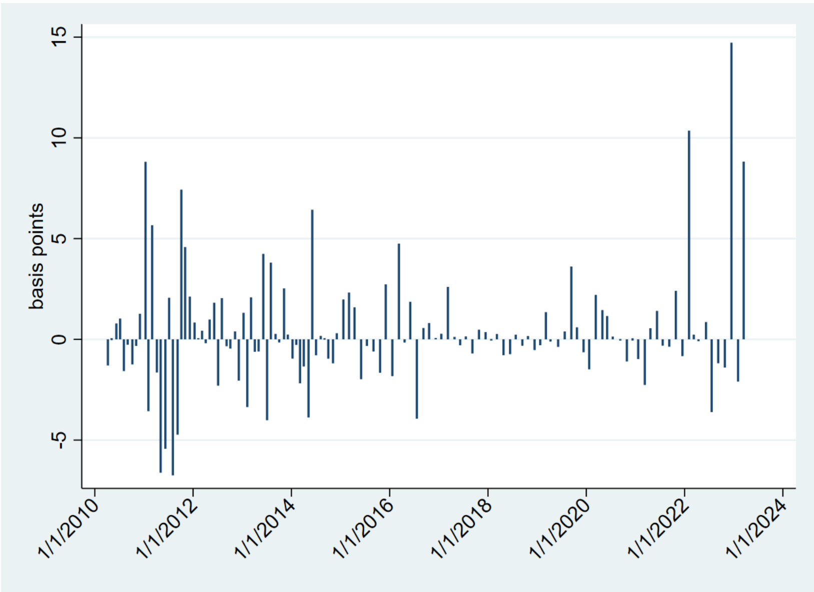
The table shows the weighted (see notes to Table 3.1) correlations between the variables used in the analysis. The correlations are all significant at 1% with the exceptions of those in italics that are not significant. For a detailed description of variables see Table 3.8.

Figure 3.6: Monetary policy surprises



The figure shows the forward guidance surprises aggregated at wave level.

Figure 3.7: Monetary policy surprises



The figure shows the forward guidance surprises monthly series.

Chapter 4

The Effect of U.S. Climate Policy on Financial Markets: An Event Study of the Inflation Reduction Act

Abstract

The Inflation Reduction Act of 2022 (IRA) represents the largest climate policy action ever undertaken in the United States. Its legislative path was marked by two abrupt shifts as the likelihood of climate policy action fell to near zero and then rose to near certainty. We investigate equity price reactions to these two events, which represent major realizations of climate policy transition risk. Our results highlight the heterogeneous nature of climate policy risk exposure. We find sizable reactions that differ by industry as well as across firm-level measures of greenness such as environmental scores and emission intensities. While the financial market response to the IRA was economically significant, it did not lead to instability or financial stress, suggesting that transition risks posed by climate policies even as ambitious as the IRA may be manageable.

The Inflation Reduction Act of 2022 (IRA) is widely considered to be the most ambitious climate policy action in U.S. history. Over the next decade and beyond, a broad array of new tax credits and direct government expenditures will provide substantial financial support for clean technologies and industries. Additionally, the IRA will offer strong direct incentives for U.S. households and firms to invest in the equipment and capital needed to reduce their carbon emissions. Bistline et al. (2023) estimate that the cumulative budgetary effect of the climate-related parts of the IRA could be on the order of $1 trillion over the next 10 years.[1] The economic changes induced by the IRA incentives are also expected to result in significant reductions to U.S. greenhouse gas emissions (Bistline et al., 2023). However, such projections of the economic and climate consequences of the IRA are generally silent about any financial implications. This is true even though the financial sector is integral to supplying the requisite capital for decarbonization and determining climate policy outcomes. The forward-looking responses of financial markets will also be evident much sooner than the economic and emissions effects and can thus provide a useful early reading on policy transmission and success. In this paper, we document the financial market responses to the IRA and provide a new climate finance perspective on this major climate policy action.

One particularly important issue for climate finance is transition risk. The substantial investment required for the transformation to a low-carbon economy will rely heavily on financial markets and institutions (e.g., Battiston et al., 2021).[2] However, the uncertain pace and consequences of a decarbonization have become a major policy concern in recent years (Van der Ploeg and Rezai, 2020). If investor expectations were to adjust precipitously to new climate policies, the resulting adverse revaluations of carbon-dependent assets—potentially resulting in stranded assets—could have severe implications for financial solvency and stability along the lines of what former Bank of England governor Mark Carney termed a "climate Minsky moment" (Carney, 2016). To better quantify the potential risks to banks and other financial institutions from such abrupt shifts in business prospects and asset prices, central banks and financial supervisory authorities are developing climate scenario analyses (Acharya et al., 2023). Clearly, the pricing of transition risks in financial markets has become a first-order policy issue. The passage of the IRA—the culmination of decades of attempts to obtain significant U.S. legislation addressing climate change—represents a major realization of climate policy transition risk.

By investigating the stock market responses to this climate policy realization across firms and industries, we can illuminate the consequences of transition risk. Our study addresses several key questions: Did this legislation materially affect stock prices in a way consistent with the specific climate policy measures? Did stocks of "green" firms—characterized by comparably low CO_2 emissions or low environmental/emissions scores—benefit from the IRA, despite the fact that the legislation did not include any carbon taxation? If there is a differential firm-level stock market

[1]Many of the IRA tax credits are open-ended without fixed budgets, so the fiscal impact depends on usage and the amount claimed. The official IRA budget score by the Joint Committee on Taxation was under $400 billion, but that likely underestimates likely participation and tax credit take-up.

[2]There are three broad roles that financial markets will play in the green transition (Giglio et al., 2021): allocating funds to sustainable investment, informing climate-related economic and policy decisions, and managing climate risks. As an example, information from financial markets can help pin down the long-run social discount rate used in determining the social cost of carbon (Bauer and Rudebusch, 2023).

response, which measures of greenness can capture this heterogeneity?

To answer these questions, we use event-study methods to examine equity price movements following key news events around the introduction of the IRA. Event studies are particularly revealing when relevant new information becomes public via discrete, definitive announcements.[3] The legislative genesis of the IRA in 2022 included two such unambiguous shifts that whipsawed the prospects for climate-related legislation. The first, which we term a "brown event," occurred when news reports surfaced late on July 14 of the withdrawal of any support for new climate spending by Senator Joe Manchin of West Virginia—the pivotal vote required for getting legislation through the Senate. The likelihood of Senate passage of climate legislation plummeted and the probability of any near-term sizable policy action fell to almost zero. The second event, a "green event," took place in the early evening of July 27, when news broke that Senator Manchin had reached a surprise agreement with Democratic leaders on new legislation that unveiled the IRA and made it nearly certain that significant climate policy would ultimately become law. Section 4.1 explains in more detail the provisions of the IRA and the timeline of events preceding its passage.

We carry out several complementary event-study exercises to investigate equity market responses to this climate legislation. In Section 4.2, we examine the returns of green and brown industries around the IRA announcements. We first employ several commonly used energy equity indices such as the S&P Global Clean Energy and S&P 500 Integrated Oil & Gas funds. Clean energy indices had sizable negative abnormal returns after the brown event on July 14 but then rebounded strongly after the July 27 IRA debut, but fossil fuel industry indices showed a reverse pattern. We also use Fama-French industry portfolios to provide insight into what sectoral shifts investors expected. Industries that stand to benefit from the provisions in the IRA for greater green product demand or subsidized production costs exhibited a strong positive response to the green announcement on July 27 while, in contrast, the oil and coal industry lost significant market value after that event.

In Section 4.3, we conduct a more granular analysis at the level of individual firms. The publicly listed firms in our sample are differentiated using measures of greenness based on their actual carbon emissions data as well as emissions scores and broader Environmental scores (or E scores) calculated by a provider of firm-level environmental characteristics. The heterogeneous stock market responses across these measures of greenness are statistically and economically significant and support their use in identifying climate policy exposure. Specifically, the brown event lowered the stock market values of green firms—those with relatively low emission intensities and superior E and emissions scores—and boosted the values of brown firms. By contrast, with the announcement that made this climate legislation a near-certainty, green firms benefited and brown firms did not.

Taken together, these event-study results document substantial and rapid financial asset price reactions to climate policy news. In response to these realizations of climate policy transition risk, green and brown stocks displayed sizable movements in opposite directions. From a theoretical perspective, positive news about the passage of the IRA lifted expected profitability for green firms and disadvantaged brown firms

[3]See MacKinlay (1997) for a review of event-study methods. In macroeconomics, a large literature has employed event studies to examine the financial market effects of monetary policy announcements (e.g., Kuttner, 2001; Gurkaynak et al., 2005; Bauer and Rudebusch, 2014; Bauer and Swanson, 2023a).

through both demand and cost channels.[4] The IRA subsidies for purchases of low-carbon products should lead to a policy-induced strengthening of customer demand for green goods and services, boosting green firm stocks as their business prospects were improved (a demand channel as in Pastor et al., 2021). Other financial incentives in the IRA include clean energy production and investment tax credits and subsidies, lowering production costs and raising profits (a cost channel). Through these channels, news about the IRA appear to have impacted expected future dividends and, ultimately, stock prices.[5]

Section 4.4 shows implications for calibrating climate policy transition risk using industry-level measures of risk exposure. To better understand the risks of climate change and the transition to a low-carbon economy, financial supervisors are developing climate scenario analyses and stress tests to identify potential vulnerabilities in the financial system and assess bank solvency (e.g., Financial Stability Board, 2022; NGFS, 2022a). For many climate-related risk assessments, potential losses have been calculated based on sectoral or industry classifications, in part due to limited firm-level data availability. In Section 4.4, we investigate whether measures of industry-level greenness can account for the cross-industry variation in the equity price responses to the climate policy news. Such metrics have been used, for example, for assessing the exposure of commercial banks to different climate policy scenarios (e.g., Jung et al., 2023). Industry-level greenness appears to be a poor predictor of an industry's responses to the climate policy announcements we study in this paper. This finding suggests that there is a need for a more granular firm- and asset-level accounting of transition risks—much like earlier work has called for with regard to physical risk (Bressan et al., 2022).

A central contribution of our paper is to characterize the asset price responses to a given change in climate policies. We find that declines in brown firm stock prices to the IRA policy action do not appear outsized or disorderly even though this climate policy transition realization was very large by historical comparison. Therefore, the transition risks of financial sector bankruptcies, dislocations, and crises to future climate policies may be manageable. Of course, it may be that other climate policy actions could have more dramatic financial consequences, given that the prevalence of stranded assets may depend on the specific type of climate policy implemented; e.g., taxes versus subsidies (Rozenberg et al., 2020). However, the IRA events that we consider are extraordinary not only in terms of their large fiscal magnitude but also because the policy announcements were made during narrowly circumscribed event windows. It is difficult to envisage other climate policy realizations that could serve as a more definitive event study for assessing climate transition risk. Given that the IRA climate policy action caused manageable financial market responses of brown firms and disadvantaged industries, our evidence suggests limited risk for a climate Minsky moment.

Our paper contributes to a quickly growing literature on the pricing of climate risks in financial markets, and specifically on the pricing of transition risks in green and brown stocks; see Bolton and Kacperczyk (2021), Pastor et al. (2022), and Bauer

[4]As noted by Bistline et al. (2023), the economic incidence of IRA tax credits and other provisions—that is, whether they will be captured by producers or consumers—is relevant for assessing the effects of the IRA.

[5]The passage of the IRA could also have shifted the cost of capital and risk premia, by changing perceptions of future climate risks, but such shifts are much less clear.

et al. (2022), among many others. Most prior work on the effects of climate policy on financial markets has studied events with news about *possible future* climate action and shifts in perceived transition risks, often with mixed results. Ramelli et al. (2021) investigate the stock market reaction to the 2016 and 2020 U.S. Presidential elections, finding better stock market performance of carbon-intensive firms in response to the Trump election but also higher stock returns for firms with higher climate responsibility around *both* the Trump and Biden election wins. Monasterolo and De Angelis (2020) document shifts in the risk characteristics of green and brown stock indices before and after the announcement of the 2015 Paris Agreement, but they find no appreciable penalty on the returns or valuations of high-carbon assets and firms. Other related empirical work considers larger and more heterogeneous sets of climate policy news. Barnett (2023) identifies a number of climate policy events and shows that industries with a larger exposure to changes in oil prices exhibit a more negative stock market response to events that increase the likelihood of future climate policy action. Ardia et al. (2023) show that unexpected increases in a news-based index of climate change concerns benefit green stocks over brown stocks. Cassidy (2023) constructs a dataset of climate policy announcements and documents that brown stocks perform better than green stocks around events with a large amount of climate policy news. Other studies of the effects of climate policies on financial performance more broadly include Kumar and Purnanandam (2022), Bartram et al. (2022), and Jung et al. (2021).

Only few other studies examine clearly identified events with major news about immediate climate policy action, i.e., realizations of transition risk. Ochoa et al. (2022) study the effects of an unexpected carbon tax increase in Germany and find a heterogeneous stock market response based on emission intensity but not based on E scores, in contrast to our findings. Carattini and Sen (2019) document which stocks benefited from news that two carbon tax initiatives in Washington State were rejected by voters. Ivanov et al. (2023) study the passage of California's cap-and-trade legislation and the failed version at the federal level (the Waxman-Markey bil) and document more constrained bank lending to high-emission firms. Hengge et al. (2023) study carbon policy news related to the European Union Emission Trading System (EU-ETS). They examine events with exogenous changes in the price of emission permits following Känzig (2023) and show that a surprise increase in the carbon price leads to negative abnormal returns of brown stocks compared with green stocks, measured using emission intensities. Our paper provides novel evidence on the financial market response to realizations of transition risk, using the stock market response to news about the IRA—the most important climate policy in U.S. history.

4.1 The Inflation Reduction Act

To set the stage for the empirical analysis, we provide a description of the timeline of events leading up to passage of the IRA and then summarize the key climate policy ingredients of this legislation.

Table 4.1 highlights some of the key events in the legislative history of the IRA. The IRA resulted from negotiations in the Senate to rework the Build Back Better Act, which was an expansive package of climate change, health care, tax reform, and social safety net proposals. While the Build Back Better Act passed the House despite unanimous Republican congressional opposition, it faced an evenly divided Senate and would need every Democratic vote for passage. Senator Joe Manchin

Table 4.1: Timeline of key legislative events for Inflation Reduction Act (IRA)

Date	Time	Event
19-Nov-21, Fri.	9:49 am	House passes Build Back Better climate legislation
19-Dec-21, Sun.	9:12 am	Manchin announces decision to vote against Build Back Better
14-Jul-22, Thu.	9:29 pm	Press reports Manchin will not support new climate spending
27-Jul-22, Wed.	5:03 pm	Manchin and Schumer announce new climate legislation: IRA
03-Aug-22, Wed.	3:31 pm	CBO/JCT publish cost estimates of IRA
07-Aug-22, Sun.	2:45 pm	Senate passes IRA
12-Aug-22, Fri.	5:42 pm	House passes IRA
16-Aug-22, Tue.		President Biden signs the IRA into law

A timeline for major legislative events during passage of the IRA, which was a smaller, climate-focused version of the earlier Build Back Better Act. Event times (in ET) reflect initial news accounts according to Dow Jones Newswires, which is a financial news source used by investors worldwide.

became the key holdout, which resulted in months of challenging negotiations and swings of sentiment regarding passage. On the evening of July 14—after U.S. equity markets[6] had closed—press reports surfaced that Senator Manchin had decided to oppose any further attempts to pass the Build Back Better Act and, in particular, had rejected any further climate legislation. One such report, Romm and Stein (2022), noted "Sen. Joe Manchin III (D-W.Va.) told Democratic leaders Thursday he would not support an economic package this month that contains new spending on climate change or new tax increases targeting wealthy individuals and corporations, marking a massive setback for party lawmakers who had hoped to advance a central element of their agenda before the midterm elections this fall." However, two weeks later on July 27, Senator Manchin and Senate Majority Leader Charles Schumer announced that they had reached a new agreement to pass climate legislation, and they unveiled the complete text of the "Inflation Reduction Act of 2022".[7] This announcement was also made after equity markets closed and was generally viewed as essentially guaranteeing passage of climate legislation. Indeed, the IRA sped through the Senate and House within two weeks and was signed into law a few days later.

The dramatic demise and rebirth of climate legislation represented by these July events—with the probability of climate policy action first falling to near zero and then jumping to close to one—are ideal for assessing the impact of the IRA on financial markets.[8] The other events in Table 4.1 are arguably of much less interest for our

[6]The New York Stock Exchange is usually open from Monday through Friday from 9:30 am to 4:00 pm.

[7]As described in Romm et al. (2022): "Sen. Joe Manchin III (D-W.Va.) on Wednesday reached a deal with Democratic leaders on a spending package that aims to lower health-care costs, combat climate change and reduce the federal deficit, [...] Under the deal, Schumer secured Manchin's support for roughly $433 billion in new spending, most of which is focused on climate change and clean energy production. It is the largest such investment in U.S. history, and a marked departure from Manchin's position only days earlier."

[8]Further supporting press accounts are included in Appendix 4.5. It is difficult to augment these narratives with prediction market probabilities. There was a tiny New Zealand prediction market that did record bets on whether the U.S. Senate would pass a budget "reconciliation" bill by September 2, 2022 (see `https://www.predictit.org`). While the reconciliation process was used to pass the IRA, the crucial issue for our event study is whether climate policy would be included in this reconciliation

purposes. The two events that preceded July 2022 pertained to the more expansive Build Back Better legislation and were part of yearlong intermittent negotiations with shifting legislative priorities that included health care, education, immigration, and tax reform. Accordingly, the extent and timing of any climate news content of these earlier events is much less clear. There were also three notable IRA events after July 2022 that included the release of cost estimates and actual IRA passage by the Senate and House. However, once Senators Manchin and Schumer had reached agreement, the August 2022 events were widely anticipated, and, according to contemporaneous press accounts, any residual uncertainty was effectively resolved before the actual votes were recorded in Congress. With climate concerns front and center for the July 2022 events and with the information arrival so clearly delineated, our analysis focuses on these dates to give the cleanest read on climate policy news.

In terms of legislative initiatives to limit climate change, the IRA provides funding for clean energy through a mix of tax incentives, grants, and loan guarantees.[9] It supports investments in clean electricity and transmission, carbon capture and storage, green hydrogen, and electric transportation and energy infrastructure. There are home energy rebates to help make homes more energy efficient and new tax credits to induce consumers to buy new and used electric vehicles. The IRA also introduces a fee on methane emissions from some companies in the oil and gas industry. Relative to past initiatives, the cost of the climate actions in the IRA is enormous—accrued both by expanding existing programs and introducing new ones. The Congressional Budget Office (CBO) and Joint Committee on Taxation (JCT) estimated that U.S. federal budgetary costs through 2031 would be $271 billion in climate-related tax credits and $121 billion for direct expenditures (Bistline et al., 2023). However, as stressed by Bistline et al. (2023), many of the tax credits are uncapped, so their take-up and cost depend on corporate investment decisions and household consumption decisions. Based on a detailed energy systems modeling of the U.S. economy, Bistline et al. (2023) project the budgetary cost of the climate-related provisions to be several times larger than the CBO/JCT estimate—perhaps as high as $1 trillion.

In terms of climate policy effectiveness, the IRA is estimated to significantly reduce carbon emissions, with projected reductions by 2030 of around 37% below 2005 levels (Bistline et al., 2023).[10] This would seem to put the United States within reach of its 50% reduction target by 2030 under the Paris Agreement. To calibrate the magnitude of the IRA policy action, it is possible in theory to provide a rough estimate of the equivalent carbon price that would be needed to achieve the same emissions reduction. That is, a non-carbon price climate policy can be be translated into an emissions-equivalent shadow carbon price, as described in Hänsel et al. (2022). Bistline et al. (2023) estimate that the power sector policies, which account for about 70% of the IRA emissions reductions, may have similar emissions reductions to a U.S. carbon tax of around $12 to $15 per ton of carbon dioxide (CO_2). Scaled up, this suggests that the total IRA would represent an approximate equivalent shadow carbon price of roughly on the order of $20 per ton of CO_2. By this measure, the IRA clearly represents a sizable climate policy initiative with significant effects comparable to

bill, and the prediction market is silent on this key question.

[9]Besides curtailing climate change, the IRA has two other goals: restraining health-care costs and reducing the federal budget deficit. The first of these is notably aided by allowing Medicare to begin negotiating the price of select prescription drugs. Federal deficit reduction is largely achieved via a new 15 percent minimum tax on corporations with earnings of at least $1 billion a year.

[10]Baseline projections without the IRA had reductions by 2030 of about 28% below 2005.

those contemplated in the usual climate scenario analyses associated with central bank climate stress tests (NGFS, 2022a).

By using an emissions-equivalent shadow carbon price that summarizes various climate policy actions such as subsidies, taxes, and regulation (e.g., Hänsel et al., 2022; NGFS, 2022a), it is possible to provide a broad-brush account of the two key climate policy events. With the election of President Biden and Democratic majorities in both houses of Congress in 2020, there were three broad plausible paths for U.S. climate policy: (1) further minimal incremental action, which would continue a more than decade-long trend of little progress on national climate policy; (2) a break from the past in the form of a moderately significant climate policy initiative; or (3) a very ambitious, wide-ranging policy action along the lines of a "Green New Deal" or the Build Back Better climate legislation. These three potential paths could be roughly approximated in terms of the future shadow carbon price path as (1) no change to the path, (2) a level shift upward in the path of about \$20, and (3) a level shift upward of \$50 or more. Before July 2022, there was sizable transition risk associated with the uncertainty around which path would be taken. The July 14 event appeared to adopt the first option with no policy action. However, the IRA announced on July 27 locked in the middle path and represented a substantial climate policy transition realization.

Before turning to the empirical analysis, it is helpful to outline how these climate policy events might be expected to affect the stock market from a conceptual asset pricing perspective. A company's equity price depends on its expected stream of future profits and dividends and on the discount rate used to calculate their (risk-adjusted) present value. The legislative events in July 2022 likely affected stock prices predominantly by changing expected dividends (as in Ochoa et al. (2022)). The shifts in the expected path for the shadow carbon price directly impacted expected profitability of green and brown firms. Higher carbon price paths translate into higher profits for green firms, which will face less competition from carbon-dependent competitors, and lower profits for brown firms. For example, a higher carbon price will raise the production costs and lower sales and revenues of firms that are more dependent on fossil fuels in their operations. Meanwhile, lower carbon price paths benefit brown firms more than green firms. After the July 14 brown event—which established a low expected shadow carbon price path—the expected profits of brown firms would likely rise and those of green firms would fall. Conversely, the jump in the future carbon price trajectory following the July 27 green event would push up expected profits of green firms and depress those of brown firms. Furthermore, these shifts in the expected paths for profits would have corresponding differential effects on the prices of green and brown stocks.

Considering the specifics of the IRA, there are both supply- and demand-side subsidies that favor green firms. On the supply side, the IRA includes production and investment tax credits for clean energy, which can boost green firm profits by lowering costs. The effects on stock prices might be best understood through the lens of a *cost channel*: Firms that benefit from such subsidies see their marginal cost decline and their profits, dividends, and stock prices rise. The IRA also contains demand-side subsidies to business and consumers to increase demand for green products. Via this *demand channel*, profits and dividends of green firms are boosted as well, again raising their stock prices.[11] Brown firms, which are relatively disadvantaged by the

[11] This demand channel is similar to the *customer channel* of Pastor et al. (2021) where firms benefit from additional demand in accordance with their greenness.

production and demand subsidies, would see their profits and dividends—and thus stock prices—decline. Therefore, we anticipate that the two sharp movements in the mean shadow carbon price path will have effects on profitability and equity prices that vary with the overall greenness of a firm, which, following a growing carbon finance literature, can be measured by the firm's CO_2 emissions or environmental score.

Another potential channel through which a change in climate policy could affect stock prices is via the cost of capital. An increase in transition uncertainty would raise the expected returns of firms that are exposed to this type of risk and thus lower their stock prices.[12] The signing of the 2015 Paris Agreement and, more generally, increasing concerns about climate change, raise the likelihood of future climate policy action and therefore transition risk—particularly for carbon-dependent firms. However, unlike many previous analyses of climate-related events and risks (e.g., Ramelli et al., 2021; Jung et al., 2021; Barnett, 2023), we do not interpret our event study results as operating primarily through *changes* in climate policy transition uncertainty and risk. Although some IRA implementation details are still being worked out, we view passage of the IRA as a *realization* of transition risk, because it implements specific new climate policies.

Finally, there are, of course, complications resulting from the specific non-carbon price nature of the U.S. climate legislation. In contrast to a broad, uniform carbon tax, the effects on cost and demand of the IRA subsidies will depend on their particular specification, the relevant market structure and segmentation, and the incidence across firms and industries. For example, subsidies in support of clean electricity may benefit utility firms and energy companies that are in industries with relatively high overall emissions footprints. As a result, the stock market effects of the IRA may depend not only on emissions/greenness of individual firms, but also on whether their industries are specifically favored and subsidized by the IRA—regardless of the greenness of that industry. This is a point that we will return to in Section 4.4.

4.2 Climate policy responses at an industry level

The first part of our analysis focuses on the response of different sectors and industries to the IRA announcements. We consider both stock market indices commonly used to represent the green and brown energy sectors, and then turn to industry portfolios using methods from empirical asset pricing. The goal of this industry-level analysis is to provide some indication of financial market participants' views about whether the IRA represents meaningful climate legislation that will be sustained going forward.

We investigate the returns of about a dozen stock market indices that are classified either as clean or green energy funds or as fossil energy funds. Our selection of indices is informed in part by Monasterolo and De Angelis (2020), who studied the reaction of various equity market indices to the 2015 announcement of the Paris Agreement.[13]

[12]Effects on the cost of capital could also result from a shift in investor preferences for green investments, perhaps from a bandwagon effect in which IRA news increases investor interest in green assets, according to the *investor channel* of Pastor et al. (2021). While it is conceivable that new climate legislation could raise public awareness in a way that drives investors towards green investments—lowering expected returns and raising prices of green stocks—evidence of such a shift is not apparent.

[13]We omit one index that was labeled green in Monasterolo and De Angelis (2020), the STOXX Global ESG Environmental Leaders index. They find that this index is just as well correlated with the brown oil and gas funds. Most tellingly, this index is heavily weighted towards bank stocks, which

Table 4.2 shows event-study results for our 11 selected indices, including six green and five brown indices. We use daily index returns based on end-of-day prices from Bloomberg, and calculate abnormal returns as the differences between raw returns and predicted returns from an estimated market model, following common practice in empirical asset pricing (MacKinlay, 1997). To estimate the market model, we regress daily returns of each index on the return of the CRSP value-weighted stock market index, a proxy for the market portfolio, from January 2016 to May 2022 to avoid overlap with the event days.[14] For each event, we report one-day and three-day returns, with the latter calculated by cumulating daily abnormal returns. As noted above, the events took place in the evenings after trading hours on July 14 and July 27, 2022. The event windows therefore start when the stock market closed on those dates and end at market close on the next day (for one-day returns) or three days later (for three-day returns).[15] In addition to the abnormal returns for each individual index, we also report returns for a green factor and a brown factor, which are weighted averages of the green and brown index returns based on their first principal components. All abnormal event returns and their statistical significance levels are obtained from regressions of the abnormal index returns on dummies for the two IRA events.

Table 4.2 shows these abnormal one-day and three-day returns around the two key IRA events on July 14 and July 27. The two events had substantially different effects on green and brown indices. The direction of the differences is intuitive and their magnitudes are sizable. After the July 14 media reports that lowered the probability that any climate policy action would pass the Senate, green indices performed worse than brown indices, both for one-day and three-day abnormal returns. For one-day returns, the green factor fell by 2.1% while the brown factor was little changed. The relative outperformance of brown indices justifies the “brown event” label. The pattern is reversed and even more stark for the July 27 “green event,” when news of Senator Manchin’s support for the IRA assured its passage. For the one-day window, most green indices had significantly positive abnormal returns, and the green factor was at +4.3%. The brown factor has a slightly negative abnormal return of -0.6%. The differential green-minus-brown return was significantly positive for both the one-day and three day windows around the green event, but the green outperformance was larger and more strongly statistically significant for the one-day event.

To provide further insight on broad market moves around the policy events, Figure 4.1 plots the cumulative returns for eight market indices from market close on July 14 to market close on August 15. These cumulative returns illustrate the persistence of the equity index reactions to climate legislation during the crucial month when climate policy was declared dead and the IRA was announced and later signed by President Biden. The July 14 and July 27 events are indicated with vertical dashed lines.[16] The clean-energy and fossil-fuel-energy stock indices are shown as green and brown lines,

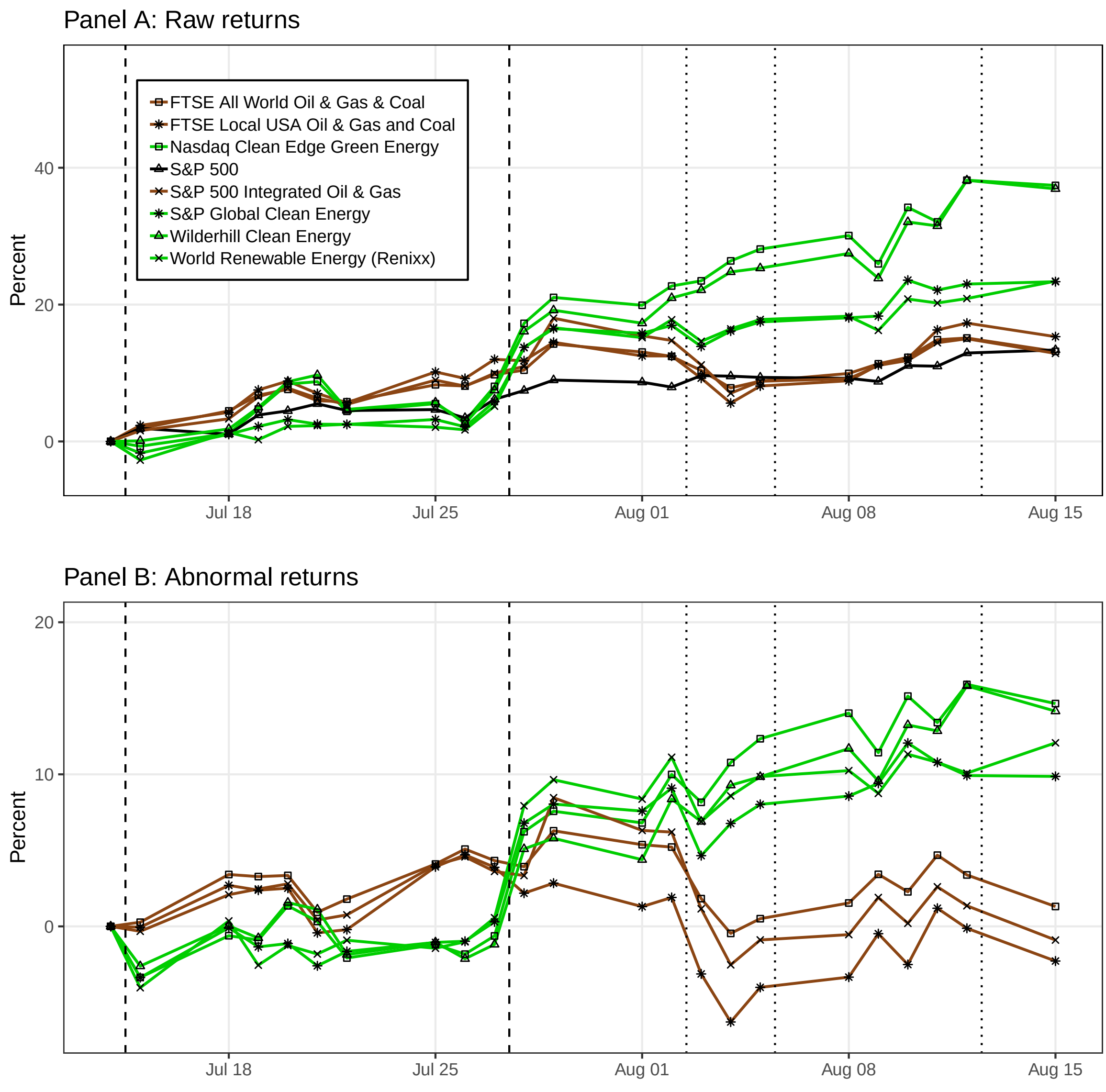
fared poorly with the announcement of the IRA. We also omit a European oil and gas index listed as a brown index in Monasterolo and De Angelis (2020).

[14] For stock market event studies with daily data, the market model captures abnormal movements in stock returns fairly well and adding additional factors is generally found to be unnecessary (e.g., Brown and Warner, 1985; MacKinlay, 1997). Consistent with this prevailing view, we also calculated abnormal returns using the Fama-French three-factor model and found very similar results.

[15] We obtained similar results using just the overnight close-to-open returns but judged that a slightly longer window better captured the market pricing of news during business hours.

[16] Again, the news on these days was released after markets had closed, so these news releases are shown as occurring between the market close quotes on these event days and the subsequent days.

Figure 4.1: Performance of green and brown indices after climate policy events



Daily cumulative returns for green and brown indices from market close on July 14 to August 15. Panel A (B) shows raw (abnormal) returns—see notes for Table 4.2. The brown and green events on July 14 and July 27, respectively, are denoted by vertical dashed lines. Three later events—described in Table 4.1—are denoted by vertical dotted lines. Each vertical line denotes the start of the event window, so the immediate observation to the right of each line shows the market response.

Table 4.2: Abnormal returns of green and brown equity indices

	Brown event (July 14)		Green event (July 27)	
	1 day	3 days	1 day	3 days
Green indices				
Nasdaq Clean Edge Green Energy	-3.4**	-0.9	6.9***	7.5***
Wilderhill Clean Energy	-2.6*	-0.8	6.3***	5.6**
S&P Global Clean Energy	-3.4***	-1.4	6.5***	7.2***
World Renewable Energy (Renixx)	-4.0**	-2.6	7.3***	7.7***
ISE Global Wind Energy	-0.5	0.6	3.3***	3.9**
MAC Global Solar Energy	-4.1**	-2.6	6.2***	6.9**
Brown indices				
S&P 500 Integrated Oil & Gas	-0.3	2.5	-0.3	2.6
FTSE Local USA Oil & Gas & Coal	-0.1	2.4	-1.6	-2.5
FTSE All World Oil & Gas & Coal	0.3	3.3	-0.4	1.0
Dow Jones Select Oil Expl. & Prod.	-0.3	3.1	-1.6	-2.6
Dynamic Energy Expl. & Prod. Intellindex	-0.3	3.7	-1.1	-1.8
Factors				
Green Factor	-2.1**	-0.5	4.3***	2.6***
Brown Factor	-0.1	1.0	-0.6	-0.2
Green-Minus-Brown	-2.1*	-1.5	4.9***	2.8**

Abnormal returns around key IRA events. Expected returns are estimated with a market model using daily value-weighted CRSP market returns from January 2016 to May 2022. Statistical significance levels are obtained from regressions of abnormal returns on event dummies; ***, **, and * denote significance at the 1%, 5% and 10% level, respectively.

respectively.[17] For the cumulative raw returns shown in Panel A, the green indices underperformed the brown ones during the two weeks following the July 14 announcement, ending about 6 percent lower on July 26. Similarly, the outperformance of green indices on the day after the July 27 announcement also persisted. The performance differences in green versus brown stocks are even more pronounced for the abnormal returns shown in Panel B. During the month, the gains in the green indices following the green event significantly outweighed their losses after the brown event, resulting in a cumulative outperformance ranging from 8 to 24 percentage points.

This index-level industry analysis yields a consistent picture of the financial performance of energy-related equity holdings. Indeed, the response of equity markets to major news about the likelihood of the passage of the IRA suggests that markets expected clean/renewable energy companies to benefit and oil/gas/coal firms to be disadvantaged by its policies. Differences between green and brown industry equity indices were far less apparent following the passage of the IRA in the House and Senate later in August 2022. The small impact of these two widely anticipated events is confirmed by event-study results in Appendix Table 4.6, which contains results for the other IRA-related events listed in Table 4.1. Reflecting the smaller, more diffuse

[17]The fossil-fuel stock declines on August 3 and 4 partly reflected a plunge in benchmark crude oil prices, which were down about 6% over those two days on concerns about rising oil supplies and a deteriorating economic outlook. Oil prices posted much smaller movements around the July 14 and 27 events—up 1.8% and down 1.0%, respectively.

releases of releases on these dates described above, the green and brown abnormal event-returns are not dissimilar. Evidently, the two events with the largest effects on stock market indices were the news announcements on July 14 and 27—the focus of our paper.

We now examine the heterogeneous response of equity returns on these two dates using the 17 Fama-French industry portfolios. We use daily, equal-weighted portfolio returns, which are available on Ken French's website and are constructed using four-digit Standard Industrial Classification (SIC) codes.[18] For each of the 17 industry portfolios, we calculate abnormal returns as for the index returns and report the one-day abnormal returns across industries. The resulting abnormal portfolio returns for the 17 Fama-French industries for the brown event on July 14 and the green event on July 27 are shown in Figure 4.2. (Raw event returns are plotted in Appendix Figure 4.5.) The brown event led to industry-level responses that are quite mixed and less clear-cut. But the green event produced industry winners and losers generally in line with what would be expected based on the IRA legislation.

In response to the green event, the best-performing industries were likely to benefit substantially from IRA subsidies and related measures. The utilities industry, which showed the most positive response, contains electric services and natural gas transmission/distribution/services. It stands to gain from the additional demand for electricity, which is given a significant boost by the IRA, as well from tax credits for renewable (biogas) natural gas and construction of renewable electricity plants more generally. The construction industry includes some favored clean energy companies and contractors that focus on solar installations and power generation and will be helped by the tax incentives for energy-efficient home improvements. Tax credits for electric vehicles and charging infrastructure will help the automobile and transportation industries (for an in-depth analysis, see Slowik et al., 2023). The machinery and equipment industry includes electrical equipment/machinery/distribution/components and batteries, which will play an integral part in the green transition.

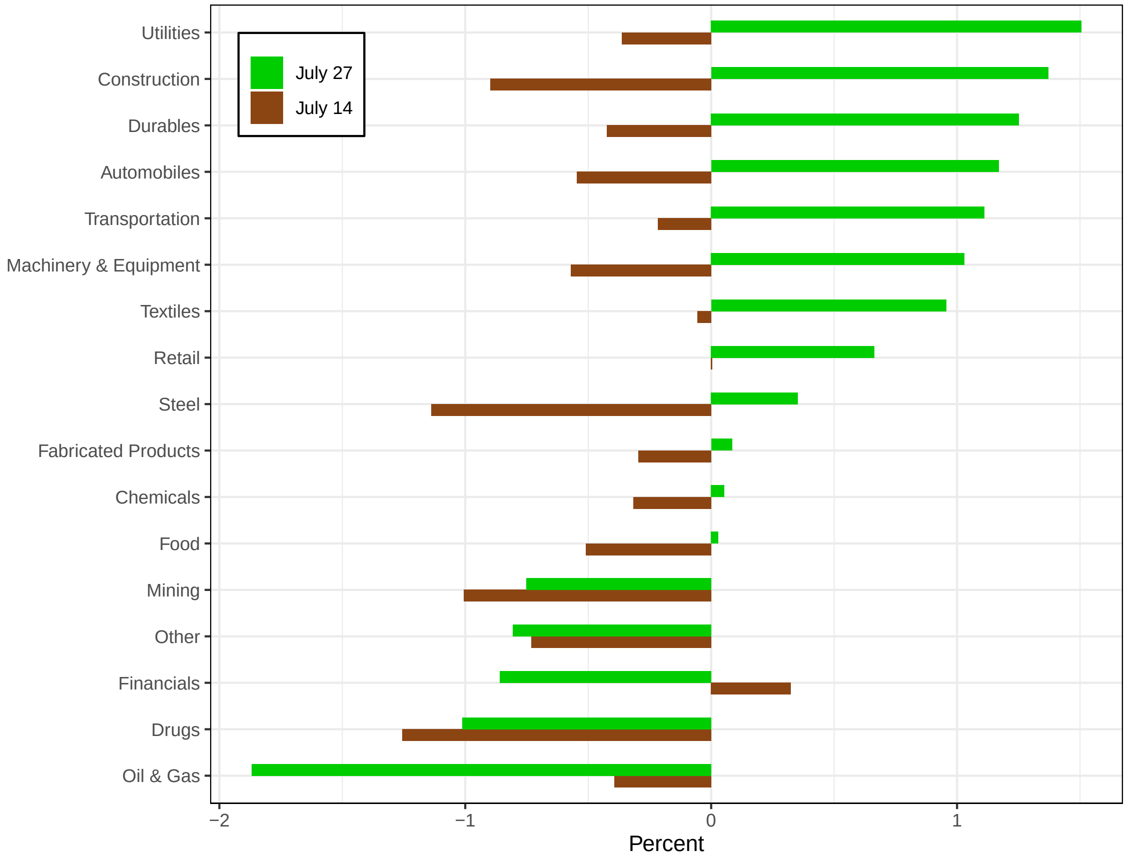
At the other end, the oil & gas and mining industries stand to lose substantially from the IRA legislation and the intended decarbonization of the U.S. economy. Consequently, stocks in these industries exhibited large negative abnormal returns around the green event. Two other industries performed poorly around this event but due to measures in the IRA unrelated to climate change. The drug industry is expected to be adversely impacted by IRA changes to Medicare that try to lower prescription drug prices. And financial institutions are likely to be particularly affected by the 15 percent minimum corporate tax on large corporations, as many large banks and insurance companies pay little or no federal taxes.[19]

The cross-industry heterogeneity in the equity market response conforms quite well with the incidence of the subsidies and credits contained in the IRA. The effects

[18] See `https://mba.tuck.dartmouth.edu/pages/faculty/ken.french/Data_Library/det_17_ind_port.html` (accessed 04/13/2023) for data and details on the industry definitions and return calculations. The use of value-weighted portfolios leads to qualitatively similar results. Our focus for the rest of this paper is on daily returns, which is a common solution to the tradeoff faced in the choice of window length in event studies: longer windows allow for delayed reactions or reversion of the effects, but they also include more noise as other news unrelated to the events affect stock returns.

[19] For example, among the 19 Fortune 100 companies that the Center for American Progress identified in a recent report as paying an effective federal tax rate below 10 percent, four belong to the financial sector (Koronowski et al., 2022).

Figure 4.2: Abnormal industry returns around green and brown events



Daily abnormal returns for 17 Fama-French industry portfolios (equal-weighted). Abnormal returns are calculated using the market model, estimated with daily returns and the CRSP equal-weighted market index return over the period from January 2016 to May 2022. Brown bars show returns using closing prices from July 14 to July 15 (brown event) and green bars show the returns from July 27 to July 28 (green event).

can be understood through the lens of the model of Pastor et al. (2021) as working via the customer channel, as well as the cost channel defined in Section 4.1 whereby production subsidies lower costs and increase profits and stock returns. But it is important to note that the IRA did not necessarily benefit green industries and hurt brown industries, as commonly defined. For example, two industries with generally high levels of emissions—oil & gas and utilities—had entirely opposite responses to the green event, according to Figure 4.2. In Section 4.4, we will revisit the question of whether the observed cross-industry heterogeneity in the equity response is related to emissions or other measures of industry exposure to carbon policy.

Overall, the response of both index returns as well as portfolio returns to IRA announcements show that market participants quickly differentiated between expected winners and losers among various industries. Differences in abnormal returns were sizable, on the order of several percentage points, after news that the IRA passage had become a near certainty.

4.3 Climate policy responses of individual firms

So far, we have established that the two climate policy events had substantial impacts on broad equity valuations—especially for the clean and fossil fuel energy sectors, which appeared particularly sensitive to the news about climate policy. Here, we examine the effects of these events on individual firms. The extent to which individual companies face the prospect of greater or lesser profits resulting from the policy initiatives should be reflected in changes in their equity prices. Specifically, firms will perform better if they are well-positioned to benefit—in relative terms—from clean-economy production and consumer subsidies, or more generally, from a higher (implicit) price of carbon.

Earlier research—such as Hengge et al. (2023), Bauer et al. (2022), Barnett (2023), and many others—identify green and brown firms by using firm-level measures of environmental characteristics such as firm-level CO_2 emissions. Using such firm-level measures of greenness (G_i), including environmental scores and emissions, we investigate the equity responses to climate policy news with the following regression:

$$r_i = \alpha + \beta G_i + \delta X_i + \gamma_s + \varepsilon_i. \tag{4.1}$$

This specification regresses the equity returns of individual firms (r_i) on greenness (G_i), a vector of firm-level controls (X_i), and industry fixed effects (γ_s) for the green and brown event days separately. We focus our attention on estimates of the coefficient β, which captures the role of greenness for the stock market response of individual firms to climate policy news.

The requisite firm-level accounting, equity return, and environmental data are from Refinitiv.[20] We use all available U.S. stocks after imposing some commonly used filters. Initially, our raw data set consists of the 3,601 U.S. firms. We filter out firms without emissions scores for the year 2021, reducing the number of firms to 3,165. Following common practice in the empirical asset pricing literature, we apply a variety of standard filters to avoid unreliable returns data, which reduces our sample

[20] Refinitiv—now rebranded as LSEG Data & Analytics—provides accounting data from the Worldscope database, stock market returns from Datastream, and environmental data from the ESG database.

to 2,537 stocks.[21] Some firms released their earnings data during our event windows, which can lead to large price movements unrelated to the IRA news and thus create noise for our estimates. To mitigate this problem, we exclude firms with an earnings announcement on the event day or the following day. As a result of this additional restriction, our regression samples contain 2,520 firms for the July 14 event and 2,122 firms for the July 27 event. Given the occurrence of both IRA announcements in July 2022, we use firms' 2021 environmental and accounting data in order to best match the information set that investors likely had available when trading in response to the policy news.

For individual firms, our analysis employs several different measures of carbon dependence or greenness. Two of these are proprietary estimated measures.[22] The first is an "emissions score" that aggregates various firm-level metrics of how effective and committed a company is to reducing its emissions. This measure depends heavily on the estimated emissions data but also, for example, on assessments of the quality of a firm's "environmental management systems." The second measure is a broader environmental (E) score, which combines almost 70 metrics in three categories: emissions, innovation, and resource use. Note that the emissions score is one of the three components of the E score. Importantly, in the calculation of both of these scores, each underlying indicator is evaluated relative to peer companies in the same industry. That is, firms are categorized among 60 different industries, and green firms—those with high emissions scores or E scores—perform well within their own industry.

Finally, we also employ a very tangible, narrow measure of greenness: a firm's CO_2 emissions. This metric is widely used in economic research to measure a firm's sensitivity to climate risk and climate policy news. Similiar to Ilhan et al. (2020), Ramelli et al. (2021), and many others, we use emission intensity, defined as the ratio of a firm's emissions to its market capitalization, which accounts for the effect on emission levels of firm size. In calculating emission intensity, it is also useful to differentiate between emissions as reported by firms and estimated emissions (Bauer et al., 2022). Many firms—even those in an ESG database—do not report emissions. For such firms, ESG data vendors provide estimates of emission levels that are largely based on a firm's sales or scale. These estimates have measurement error, and some have argued that using such imputed instead of reported emissions can lead to bias in some empirical analyses (Aswani et al., 2023), although event studies have not been implicated. Accordingly, our analysis using emission intensity is limited to firms with disclosed/reported emissions, which reduces our sample size by about two-thirds but arguably increases the reliability of the results.[23] However, in unreported analysis, we obtained very similar results for the much larger sample of firms available using emission intensity calculated with estimated emissions. In addition, our other two

[21] Specifically, we filter out stocks that are not common equity, primary equity quotations, or listed in NYSE, AMEX, or NASDAQ. We also remove securities with prices lower than \$1 during our estimation and event windows, and securities whose name fields indicate non-common equity affiliation (see, e.g., Ince and Porter, 2006; Griffin et al., 2010; Bauer et al., 2022).

[22] Refinitiv offers a large ESG database that covers about 85% of the global market cap and draws on more than 630 different ESG metrics. For details about the proprietary methodology, see `https://www.refinitiv.com/content/dam/marketing/en_us/documents/methodology/refinitiv-esg-scores-methodology.pdf` (accessed 03/16/2023).

[23] Specifically, we use each firm's reported total 2021 scope 1 and scope 2 emissions (in kilotons of CO_2 equivalents) relative to its market capitalization (in million USD at the end of 2021). We do not include scope 3 emissions because they are hard to monitor and attribute.

Table 4.3: Summary statistics of firm-level data

	Mean	SD	Min	q25	Median	q75	Max	Obs.
Environmental performance								
E score	0.28	0.28	0.00	0.01	0.19	0.50	0.98	2,537
Emissions score	0.31	0.32	0.00	0.00	0.21	0.58	1.00	2,537
Emission intensity	0.20	0.58	0.00	0.00	0.01	0.08	3.70	900
Firm-level controls								
Size	21.70	1.84	17.40	20.44	21.61	22.87	26.37	2,528
Leverage	0.15	0.56	-2.20	0.01	0.09	0.24	3.51	2,133
Rev. growth	0.34	1.13	-1.00	0.04	0.14	0.30	10.39	2,428
Profitability	-0.01	0.18	-1.02	-0.02	0.02	0.07	0.37	2,503
ETR	0.11	0.31	-1.73	0.00	0.15	0.23	1.23	2,537
Daily returns								
Brown event (July 14), raw	1.92	2.60	-44.74	0.86	1.94	3.05	24.35	2,520
Brown event (July 14), abn.	-0.18	2.60	-47.54	-1.06	-0.11	0.91	22.39	2,520
Green event (July 27), raw	1.04	2.99	-26.61	-0.31	0.96	2.33	29.97	2,122
Green event (July 27), abn.	-0.25	2.97	-27.87	-1.60	-0.31	1.07	27.87	2,122

Summary statistics for firm-level environmental measures, controls and accounting variables, and event returns. Environmental measures are described in the text. Size is log of total assets in millions USD, market leverage is EBIT divided by interest expenses, revenue growth is annual growth in total sales, profitability is return on assets, ETR is the cash effective tax rate (total income taxes paid divided by pretax income). Returns are from market close of July 14 (27) to market close of July 15 (28), and abnormal (abn.) returns are the residuals from an estimated market model.

greenness metrics—E scores and emissions scores—incorporate the estimated emission levels, and we use the full sample of available firms for these.

The firm-level characteristics we control for include size (log total assets), market leverage (earnings before interest and taxes divided by interest expenses), revenue growth (annual growth rate in total revenues), and profitability (return on assets). Additionally, we follow Ramelli et al. (2021) and Wagner et al. (2018) and include a measure of cash effective tax rate (ETR), given that the tax burden of a firm can be an important determinant of its exposure to policy changes.[24] For the emission intensity measure of greenness, we also include the standard industry fixed effects using 17 Fama-French industries based on their SIC codes. As noted above, industry effects are already accounted for with E and emissions scores, which are constructed relative to peer companies in the same industry. In any case, omitting industry fixed effects completely or including them everywhere did not significantly change our results reported below.

Table 4.3 reports summary statistics for our firm-level data. The emissions and E scores range from 0 to 1 (as we divide the raw scores by 100). High scores indicate good environmental and emissions performance—i.e., low-carbon firms—but the median firm gets a relatively low score of around 0.2. These scores also display substantial dispersion across firms but little skewness. Emission intensity, which is available for fewer firms, ranges from 0 to 3.7 (kilotons of CO_2 per million USD market cap),

[24]ETR is missing for a fairly large number of firms, and like Ramelli et al. (2021) we replace the missing values with zero and add an indicator variable identifying missing observations.

with higher values indicating higher-carbon firms. As others have noted (Bolton and Kacperczyk, 2021; Bauer et al., 2022), there is a large degree of skewness in emission intensity. A small number of firms have very high emission intensities, so the mean firm has an intensity about 20 times as large as the median firm (0.2 vs. 0.01). This effect is mitigated for E and emissions scores by their within-industry construction. The bottom panel characterizes daily returns around the two IRA events. These returns display little skewness but substantial dispersion across firms.

Table 5.2 provides estimation results for equation (1) for raw event returns, and Appendix Table 4.7 reports results for abnormal returns, which differ minimally from those for raw returns. The results in the first three columns of Table 5.2 pertain to the brown event returns on July 14-15. They show that firms with high E and emissions scores or low emission intensities had a significantly worse daily stock market performance around this event. This is consistent with a deterioration in the outlook for future profits of green firms by the diminished prospects for comprehensive climate policy. By contrast, the differential stock market responses of green and brown firms to the green event on July 27-28 have the opposite sign. The estimated β's for the E and emissions scores are positive, while the coefficient on emission intensity is negative. These responses are also stronger and statistically more significant than for the earlier event. With the astonishing news of near-certain passage of comprehensive climate policy in the form of the IRA, green firms exhibited a substantially better stock market performance than brown ones. Specifically, in terms of E scores, a greener firm at the upper 75 percentile (with an E score of 0.58) had almost a full percentage point higher daily equity return after July 27 than a browner firm at the lower 25 percentile (with an E score of 0.01).

Overall, our results using E and emissions scores are completely consistent with those using emission intensity. All three metrics show that high-carbon firms were expected to have better prospects in the absence of climate policy and that low-carbon firms performed better when the IRA climate policy was announced. This consistency is notable because the environmental scores calculated by ESG data providers are based on relatively subjective collections of indicators using proprietary methodologies. As such, at the firm level, the information in these metrics can differ substantially across providers (Berg et al., 2024; Ehlers et al., 2022). Similarly, we find a modest connection between E and emissions scores and the direct measure of emission intensity: E and emissions scores have a correlation of only -0.11 and -0.08, respectively, with emission intensity. In light of the apparent noise in distinguishing green and brown firms, the consistency of our results points to the strength of the underlying effect of the climate policy news that we identify.[25]

To more finely judge the economic significance of the firm-level results, Figure 4.3 displays the cross section of firm-level returns using portfolio sorts. We first orthogonalize event returns and greenness measures with respect to our regression control variables (size, market leverage, revenue growth, profitability, and effective tax rate—as well as industry fixed effects for emission intensity). Then, a univariate regression of the orthogonalized event return on the orthogonalized greenness measure would recover exactly the coefficient of interest reported in Table 5.2 (according to the Frisch-Waugh-Lovell theorem). We form decile portfolios and plot mean portfolio

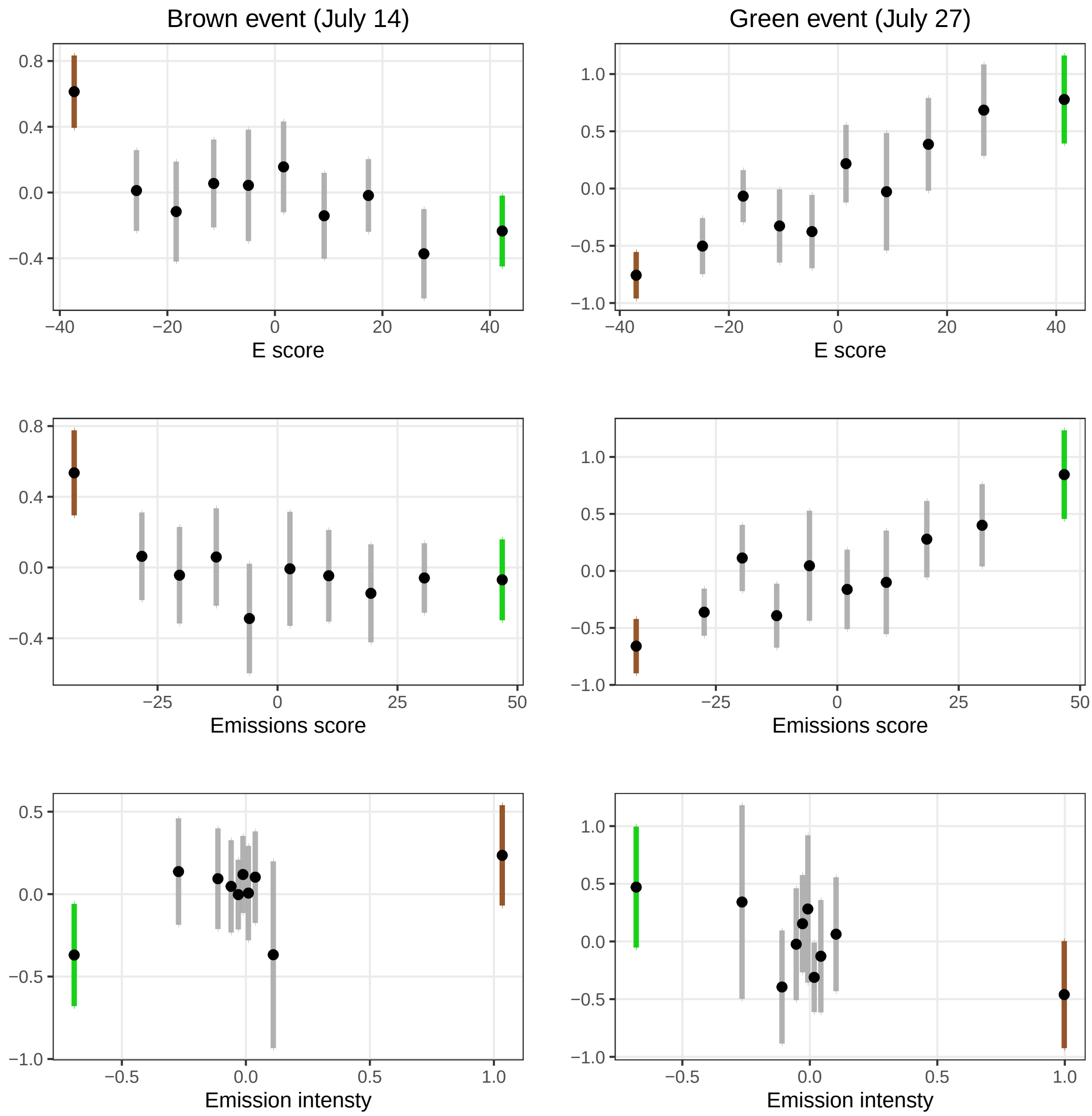
[25] As noted earlier, Ramelli et al. (2021) find conflicting results for the stock market response to the Trump election depending on whether E scores and emission variables were used, but for that event, the timing of any news about the prospects for climate action was less clear-cut.

Table 4.4: Event return regressions

	Brown event (July 14)			Green event (July 27)		
	(1)	(2)	(3)	(4)	(5)	(6)
E score	−0.77***			1.98***		
	(0.19)			(0.27)		
Emissions score		−0.44**			1.47***	
		(0.17)			(0.24)	
Emission intensity			0.31**			−0.43**
			(0.15)			(0.21)
Size	0.16***	0.13***	0.05	−0.15***	−0.13***	−0.21***
	(0.03)	(0.04)	(0.06)	(0.05)	(0.05)	(0.07)
Market leverage	0.30***	0.30***	0.37	0.02	0.03	−0.02
	(0.10)	(0.10)	(0.25)	(0.11)	(0.11)	(0.33)
Revenue growth	0.15*	0.15**	−0.20	−0.04	−0.05	0.28*
	(0.08)	(0.08)	(0.21)	(0.07)	(0.07)	(0.16)
Profitability	0.01	−0.04	0.89	0.45	0.50	−3.94
	(0.58)	(0.58)	(2.24)	(0.87)	(0.87)	(4.20)
ETR	0.12	0.13	−0.01	−0.04	−0.04	−0.38
	(0.19)	(0.18)	(0.16)	(0.17)	(0.17)	(0.31)
ETR missing dummy	−1.21***	−1.20***	−0.32	−0.73	−0.77*	−1.32**
	(0.43)	(0.43)	(0.72)	(0.46)	(0.46)	(0.52)
Constant	−1.23	−0.85		3.94***	3.60***	
	(0.75)	(0.77)		(1.04)	(1.06)	
Observations	2,043	2,043	824	1,693	1,693	669
R^2	0.04	0.03	0.10	0.04	0.03	0.12
Industry fixed effects	No	No	Yes	No	No	Yes

Regression results for event returns. The dependent variable is the one-day raw return from market close on July 14 to July 15 (the brown event) in the first three columns, and the return from July 27 to 28 (the green event) in the last three columns. The key regressors are the environmental pillar score (E score), the emission category score, and emission intensity, calculated as the reported level of scope 1+2 emissions divided by market cap (at the end of 2021). Controls include size, market leverage, revenue growth, profitability, and effective tax rate (ETR), which are described in the text and the notes to Table 4.3. The third and sixth columns include industry fixed effects using the 17 Fama-French industries. Clustered standard errors (by industry) are reported in parentheses. ***, **, and * denote significance at the 1%, 5% and 10% level, respectively.

Figure 4.3: Event returns of decile portfolio



The dots measure event returns of decile portfolios formed using E scores (top row), emissions scores (middle row) or emission intensities (bottom row), for either the brown event on July 14 (left column) or the green event on July 27 (right column). Returns and greenness measures are orthogonalized with respect to the control variables in Table 5.2 (size, market leverage, revenue growth, profitability, and effective tax rate). Vertical bars indicate 90% confidence intervals with greenest (brownest) deciles denoted by green (brown).

returns against greenness (similar to a bin scatter plot of returns on greenness). The resulting Figure 4.3 shows the cross-sectional relationship between event returns and our three measures of greenness. The left-hand column illustrates brown event returns and shows clear negative relationships between returns and E/emissions scores and a positive relationship between returns and emission intensity. These correlations are completely consistent with the associated coefficient estimates in Table 5.2 but allow a finely sliced reading of the firm-level results. For example, much of the response is driven by the greenest and brownest deciles of firms. These extremes are denoted by green and brown colored confidence intervals. For the brown event, the greenest firms had returns that were around 3/4 percentage point lower than brownest firms for *all* of the carbon metrics. For the green event shown in the right-hand panels, the reverse correlations between returns and greenness metrics are evident, with the high-carbon firms performing much worse across all three metrics. The announcement of the new IRA climate policy clearly led to a quantitatively significant green outperformance. Specifically, after the IRA announcement, the returns of green firms were 1.5 percentage points higher than those of brown ones using E and emissions scores and almost 1 percentage point higher using emission intensity. Furthermore, given the considerable lack of overlap of the green and brown confidence intervals in all of the panels of Figure 4.3, the statistical significance of our results across both events is confirmed.

Our analysis provides clear and consistent evidence of the stock market impact of the two climate policy events. When negotiations for further U.S. climate policy action publicly collapsed on July 14, investors bid up shares of brown, carbon-dependent firms while green, low-emissions firms lost value. Conversely, unveiling the IRA climate policy package benefited green stocks and hurt brown stocks. These results hold up whether environmental performance is measured using scores from ESG providers or actual emissions disclosed by the stock market companies. Again, these results are consistent with, for example, the asset pricing model of Pastor et al. (2021) in which green stocks can benefit from a policy-induced greater demand for goods and services of greener providers. In addition, clean-energy investment subsidies and similar policies appeared likely to reduce costs for green firms.

4.4 Industry greenness as a measure of transition risk

So far, we have shown that surprising realizations of U.S. climate policy had substantial effects on equity prices and that these effects differed significantly across firms. Such estimates of policy sensitivities are critical to a rapidly growing literature on the potential adverse implications of climate policy changes for the financial system. Specifically, central banks and financial supervisors are investigating the exposure and resilience of financial institutions to the transition risks posed by imperfectly anticipated efforts to facilitate and force a shift to a low-carbon economy (NGFS, 2022a; Acharya et al., 2023).

Central banks and supervisors are particularly interested in financial transition risk assessments of commercial banks, including how loans and other bank assets are revalued under a range of climate policy scenarios (Jung et al., 2023). The various climate scenarios differ in terms of the scope and pace of the policy-induced economic transformations taken to lower carbon emissions. The associated decarbonization risks include possible declines in asset prices, income, and profitability, and these risks are most material for companies with business models that rely on high carbon emissions, but firm-level emission metrics and data are only available for a subset of firms. Given the inadequate coverage of the available firm-level data, it is difficult to estimate such potential transition-related losses at a granular level. Thus, for many climate-related risk assessments, potential losses have been calculated based on sectoral or industry classifications, which are available for all of a financial institution's loans and assets. For example, Jung et al. (2023) examines the exposure of commercial banks to different climate policy scenarios by employing estimates of the effects of different carbon taxes on the output and profits of various industries as estimated from the general equilibrium models of Jorgenson et al. (2018), Goulder and Hafstead (2017), and NGFS (2022b). In effect, banks' exposures to climate policy shifts depend on the industry composition of their loan portfolio and the estimated industry-level effects of the climate policies that drive decarbonization. Similarly, Choi et al. (2020) identify high-emission firms based on their industry classification.

The climate policy events that we identified can provide a useful case study to assess the appropriateness of using industry classifications to account for climate transition risk. Specifically, we consider whether industry-level greenness metrics can account for the cross-industry variation of the equity price response to climate policy news. This analysis employs three different measures of industry-level greenness. The first two are constructed directly from our firm-level data, which we aggregate up to the 17 Fama-French industries. Industry-level emissions are the sum of all disclosed scope 1 and scope 2 emissions (in tons of CO_2 equivalents) of the firms in each industry. For these industry-level greenness metrics, we use total industry emissions and emission intensity, which divides total emissions by industry market cap (in million USD at the end of 2021).

Our final measure of industry-level greenness is based on the transition risk exposure for each industry as proxied by estimates of the differing loss in output in each sector that would be caused by a carbon tax. Jung et al. (2023) use such carbon tax sensitivities in their study of the transition risk exposures of U.S. banks. Like them, we use the Jorgenson et al. (2018) estimates of carbon tax sensitivities, which are based on an intertemporal general equilibrium model calibrated to U.S. industries.[26] Jorgenson et al. (2018) use the IGEM industry classification. We assign the firms in each of the 60 Refinitiv industries to one of the IGEM industries, which results in 29 IGEM industries for our sample.

To calculate industry-level event returns, we aggregate firm-level abnormal returns—constructed as explained in Section 4.2—into equal-weighted industry portfolios. Table 4.5 relates the industry-level event returns to the three measures of industry greenness. Note that, for all three measures, high values indicate brown industries with high transition risk. Thus, if these measures accurately captured the transition risk of the IRA policies, then we would expect to see positive coefficients in the regressions for the brown event (first three columns), and negative coefficients for the green event (last three columns). Instead, the coefficients on the emissions level variable have the wrong sign in both regressions, although they are not statistically significant. For emission intensity, the coefficients have the expected sign only for the brown event, but that is marginally statistically significant (at the 10-percent level). Finally, the

[26]Specifically, we employ the estimated output sensitivities to an introduction of a \$25 tax per metric ton of CO_2 equivalents, with a 1% tax growth rate.

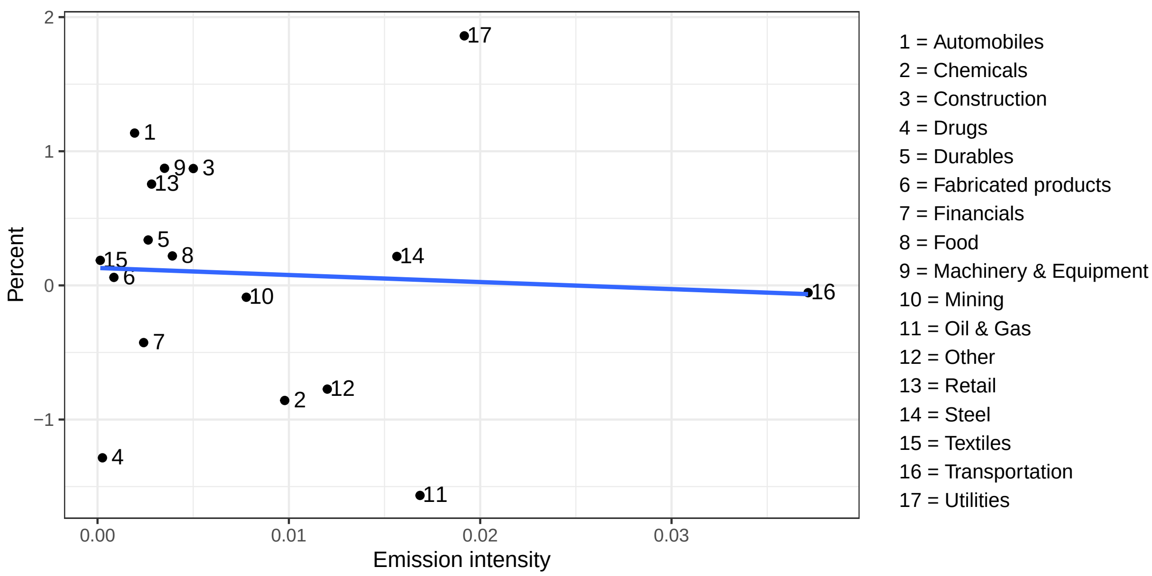
Table 4.5: Event returns and industry-level green metrics

	Brown event (July 14)			Green event (July 27)		
	(1)	(2)	(3)	(4)	(5)	(6)
Emissions	−0.17			1.05		
	(0.31)			(2.56)		
Emission intensity		8.96*			−5.26	
		(4.64)			(19.38)	
Carbon tax sensitivity			−1.31			−7.06
			(3.29)			(8.83)
Constant	−0.38***	−0.49***	−0.05	−0.02	0.13	0.22
	(0.12)	(0.14)	(0.14)	(0.23)	(0.22)	(0.21)
Observations	17	17	29	17	17	29
R^2	0.004	0.067	0.004	0.024	0.003	0.047

Regressions of abnormal returns on emissions, emissions intensity, and a measure of industry sensitivity to carbon taxes. Emissions and emission intensity are aggregated to 17 Fama and French industries from firm-level data. Carbon tax sensitivity is from Jorgenson et al. (2018), the industry output sensitivity to the introduction of a \$25 tax per ton of CO_2 with a 1% growth rate. ***, **, and * denote significance at the 1%, 5% and 10% level, respectively.

coefficients on the carbon tax sensitivity variable only have the expected sign for the green event, but again, neither are statistically significant.

Figure 4.4: IRA announcement: industry abnormal returns and emission intensity



Emission intensity and abnormal returns from July 27 to July 28 (green event) for the 17 Fama and French industries. Firm-level data are aggregated to industry level. Abnormal returns are calculated from raw returns using a market model estimated with daily value-weighted CRSP market returns from January 2016 to May 2022. Emission intensity is constructed as the total emissions (scope 1 and 2) divided by market cap. The best fitting regression is shown as the blue line.

To illustrate the weak relationships between greenness measures and returns, Figure 4.4 provides the underlying scatter plot corresponding to column 5 in Table 4.5. The emission intensities of the 17 industries are measured on the horizontal axis, and the equity responses of firms in those industries are measured on the vertical axis.[27] The regression line is effectively flat indicating very poor fit and weak predictive ability. Two industries illustrate the difficulty of using measures of greenness at an industry level: oil & gas and utilities have similar emission intensities, but they had diametrically opposed IRA equity price responses because clean power subsidies supported the utility sector.

Overall, measured greenness of industries appears to be essentially unrelated to the equity response to the climate policy announcements. By contrast, the within-industry heterogeneity documented in Section 4.3 was significantly correlated with firm-level greenness. One important implication of the poor performance of the industry-level greenness measures is that, based on the climate policy transition realizations that we examined, they are likely a weak foundation for capturing and assessing transition risk. We believe that our event study considers some of the cleanest, most clearly demarcated U.S. climate policy announcements in terms of timing. Of course, the announced policy shifts were much more complicated than the simple carbon tax changes envisioned in many climate stress tests and model simulations. However, policy complexity seems unavoidable and practical, real-world stress testing should take this into account.[28]

4.5 Conclusion

Our event study used the econometric identification of clearly delineated policy news to investigate how financial markets value firms' climate-related prospects. We show that the equity market responses to announcements of climate policy actions were quick, substantial, and distinctly heterogeneous with wide variation across firms and industries. Green stocks—equities of firms with lower carbon emission intensities and better environmental and emissions scores—benefited from news that the IRA would become law, while brown stocks—those of more carbon-intensive and more polluting firms—lost value. This heterogeneity of stock price responses is both statistically and quantitatively significant. We find equity movements in the opposite direction—with brown stocks outperforming green stocks—for the earlier event when the prospects for climate action shifted to negligible. These heterogeneities—particularly the increased investor demand for the stocks of low-carbon firms—are in line with the IRA's goal of fostering a transition away from fossil fuels. These results also appear consistent with several mechanisms that lead to different expected profits for green and brown firms. In particular, the heterogeneities likely reflect the varying effects of IRA tax credits and subsidies on green and brown product demand, revenues, and investment and production costs.

We also provide a cautionary note regarding the use of industry or sectoral measures of greenness for financial risk assessments and climate scenario analyses. Industries

[27] There are small differences in industry returns between Figures 4.2 and 4.4. The former are estimated using all registered firms, and the latter are estimated using only our ESG firm-level dataset. However, the same conclusion is obtained with either measure: on average across all industries, returns and greenness appear uncorrelated.

[28] The Unites States may be especially prone to policy complexity, given the politicization of views on climate change (see, e.g., DiLeo et al., 2023).

likely to benefit from the new policies—in particular, the utilities, construction, and automobile/transportation sectors—saw their stocks appreciate. However, across all industries, there was little correlation between industry-level greenness and stock market response. This finding suggests that a more granular, firm-level level approach may often be necessary to reliably capture exposure to transition risk.

Our examination of the reactions of equity prices to two major climate policy transition realizations can help policymakers, regulators, and investors better understand such transition risks and the likely financial effects of new climate policies. The results of our event study have some reassuring implications for financial transition risk assessments. The highly ambitious IRA climate policy legislation was a significant climate policy transition realization that could have increased the likelihood of stranded assets. Nevertheless, it did not result in any dramatic or disorderly repricing akin to a "climate Minsky moment." That is, the most consequential climate policy action ever in U.S. history did not lead to firm-level equity price responses that were overwhelming or destabilizing. Of course, there are caveats to this conclusion. The impact effects on equity prices may not persist. Financial investors may have naively or sensibly underreacted to the IRA, perhaps downplaying the open-ended tax cuts, or even putting sizable odds on a future policy rollback. Alternatively, other types of climate policies—such as a precipitous and largely unexpected carbon pricing scheme—could have different implications and potentially lead to financial stress and instabilities. But given the significant scope and clear-cut timing of the climate policy news during the passage of the IRA, it is difficult to envisage another set of events that would serve as a more definitive realization for assessing climate transition risk.

Appendix

Appendix 4.A: Supplemental press coverage of climate policy events

Press reports for July 14, 2022 brown event

Joe Manchin Won't Support Climate, Tax Measures in Economic Package
Wall Street Journal, July 14, 2022, 11:48 pm ET

"Sen. Joe Manchin (D., W.Va.), the pivotal vote in Democrats' efforts to pass a bill aimed at fighting climate change, told Senate Majority Leader Chuck Schumer that he wouldn't support an economic package that raises taxes or includes climate provisions, according to people familiar with the matter. Instead, Mr. Manchin told Mr. Schumer on Thursday that he would only support a bill that includes provisions aimed at lowering the price of prescription drugs and a two-year extension of Affordable Care Act subsidies, according to one of the people."

`https://www.wsj.com/articles/joe-manchin-wont-support-bill-that-includes-climate-and-tax-measures-11657848978?page=2&mod=article_inline` (accessed 11/09/2023)

How Joe Manchin Doomed the Democrats' Climate Plan
The New York Times, July 15, 2022

"Senator Joe Manchin III of West Virginia, who took more campaign cash from the oil and gas industry than any other senator, and who became a millionaire from his family coal business, independently blew up the Democratic Party's legislative plans to fight climate change. [...] Privately, Senate Democratic staff members seethed and sobbed on Thursday night, after more than a year of working nights and weekends to scale back, water down, trim and tailor the climate legislation to Mr. Manchin's exact specifications, only to have it *rejected inches from the finish line.* [...] Mr. Manchin's refusal to support the climate legislation, along with steadfast Republican opposition, *effectively dooms the chances* that Congress will pass any new law to tackle global warming for the foreseeable future [...]"

`https://www.nytimes.com/2022/07/15/climate/manchin-climate-change-democrats.html` (accessed 11/09/2023)

Manchin Pulls Plug on Climate and Tax Talks, Shrinking Domestic Plan
The New York Times, July 14, 2022

"Senator Joe Manchin III, Democrat of West Virginia, pulled the plug on Thursday on negotiations to salvage key pieces of President Biden's agenda, informing his party's leaders that he would not support funding for climate or energy programs or raising taxes on wealthy Americans and corporations. [. . .] The decision by Mr. Manchin [. . .] *dealt a devastating blow* to his party's efforts to enact a broad social safety net, climate and tax package."

`https://www.nytimes.com/2022/07/14/us/politics/manchin-climate-taxes.html` (accessed 11/09/2023)

Press reports for July 27, 2022 green event

Joe Manchin Reaches Deal With Chuck Schumer on Energy, Healthcare, Tax Package
Wall Street Journal, July 28, 2022, 10:28 am ET

“Sen. Joe Manchin (D., W.Va.) agreed to back a package aimed at lowering carbon emissions and curbing healthcare costs while raising corporate taxes, marking a *stunning revival* of core pieces of President Biden’s economic and climate agenda that the West Virginia Democrat had *seemingly killed* earlier this month. The deal, negotiated privately between Messrs. Manchin and Senate Majority Leader Chuck Schumer (D., N.Y.) since the start of last week, would raise roughly $739 billion, [...]”

`https://www.wsj.com/articles/joe-manchin-reaches-deal-with-chuck-schumer-on-energy-healthcare-package-11658957299` (accessed 11/09/2023)

Surprise Deal Would Be Most Ambitious Climate Action Undertaken by U.S.
The New York Times, July 28, 2022

“Senate Majority Leader Charles Schumer (D-N.Y.) and centrist Sen. Joe Manchin (D-W.Va.) on Wednesday said they had struck a climate, health and tax package deal [. . .] The new package is a fraction of the more than $3 trillion deal once envisioned by liberal Democrats, but it still could give the party a big win ahead of midterm elections [. . .]”

`https://www.nytimes.com/2022/07/28/climate/climate-change-deal-manchin.html` (accessed 11/09/2023)

Manchin, Schumer announce slimmed-down deal on climate, taxes, health
The Hill, July 27, 2022, 5:38 pm ET

“The $369 billion climate and tax package forged in a surprise deal by Senate Democrats would be the most ambitious action ever taken by the United States to try to stop the planet from catastrophically overheating. The agreement, which Senate Democrats announced late Wednesday and hope to pass as early as next week, shocked even some who had been involved in the sputtering negotiations over climate legislation during the past year. The announcement of a deal, after many activists had given up hope, almost instantly reset the role of the United States in the global effort to fight climate change. And it was delivered by Senator Joe Manchin III of West Virginia, the holdout Democrat who had been reviled by environmentalists and some of his own colleagues after he said this month that he could not support a climate bill due to inflation concerns.”

`https://thehill.com/homenews/senate/3576965-manchin-schumer-announce-slimmed-down-670-billion-deal/` (accessed 11/09/2023)

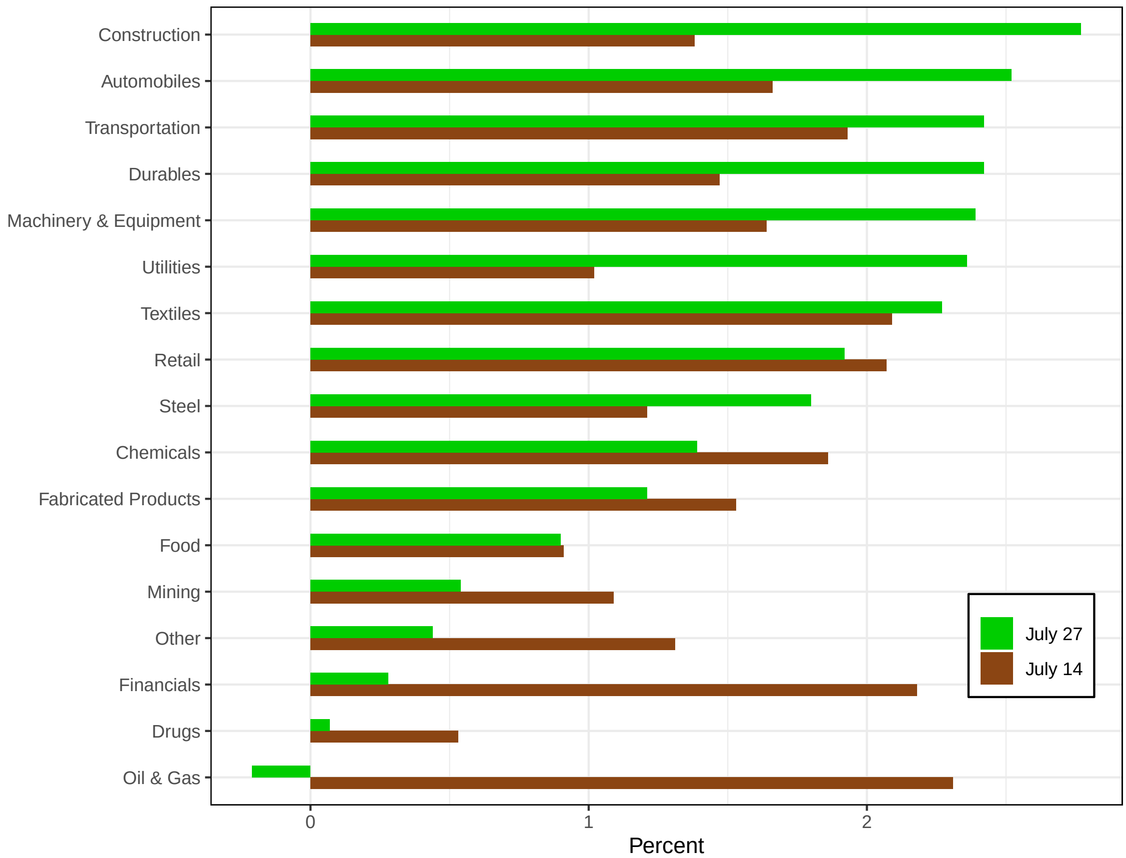
Appendix 4.B: Additional empirical analysis

Table 4.6: Abnormal returns of green and brown equity indices with alternative events

	CBO/JCT (Aug 03)		Senate (Aug 07)		House (Aug 12)	
	1 day	3 days	1 day	3 days	1 day	3 days
Green indices						
Nasdaq Clean Edge Green Energy	-1.7	2.1	1.5	2.5	-1.1	-5.2**
Wilderhill Clean Energy	-1.4	1.3	1.7	3.1	-1.4	-6.3**
S&P Global Clean Energy	-4.1***	-1.0	0.5	3.7	-0.1	-1.2
World Renewable Energy (Renixx)	-3.8**	-1.2	0.4	1.3	1.8	-0.7
ISE Global Wind Energy	-2.0**	-0.3	0.9	2.2	0.1	1.1
MAC Global Solar Energy	-4.6***	-0.8	0.8	3.8	-0.2	-2.2
Brown indices						
S&P 500 Integrated Oil & Gas	-4.8***	-6.7***	0.3	1.1	-2.2	-1.5
FTSE Local USA Oil & Gas & Coal	-4.9***	-5.8*	0.7	1.6	-2.2	0.0
FTSE All World Oil & Gas & Coal	-3.2**	-4.5*	1.0	1.8	-2.0	-1.0
Dow Jones Select Oil Expl. & Prod.	-5.0**	-5.8*	0.4	1.1	-2.6	-0.3
Dynamic Energy Expl. & Prod. Intellindex	-5.5***	-7.0*	-0.1	1.1	-2.9	-0.5
Factors						
Green Factor	-2.1**	0.0	0.7	1.1	-0.1	-0.9
Brown Factor	-2.7***	-2.0**	0.3	0.5	-1.4	-0.3
Green-Minus-Brown	0.6	2.1*	0.4	0.7	1.3	-0.7

Abnormal returns around other IRA-related events. Expected returns are estimated with a market model using daily value-weighted CRSP market returns from January 2016 to May 2022. Statistical significance levels are obtained from regressions of abnormal returns on event dummies; ***, **, and * denote significance at the 1%, 5% and 10% level, respectively.

Figure 4.5: Returns of 17 Fama-French industry portfolios around green and brown events



Daily returns for 17 Fama-French industry portfolios (equal-weighted). Brown bars show returns using closing prices from July 14 to July 15 (brown event), and green bars show the returns from July 27 to July 28 (green event).

Table 4.7: Event-study regressions: abnormal returns

	14 July 2022			27 July 2022		
	(1)	(2)	(3)	(4)	(5)	(6)
E score	−0.86***			1.93***		
	(0.19)			(0.27)		
Emissions score		−0.54***			1.41***	
		(0.17)			(0.24)	
Emission intensity			0.31**			−0.42**
			(0.14)			(0.21)
Size	0.15***	0.13***	0.07*	−0.16***	−0.13***	−0.19**
	(0.03)	(0.03)	(0.04)	(0.05)	(0.05)	(0.09)
Market leverage	0.37***	0.36***	0.43*	0.06	0.06	0.02
	(0.10)	(0.10)	(0.25)	(0.11)	(0.11)	(0.33)
Revenue growth	0.10	0.11	−0.38***	−0.07	−0.08	0.17
	(0.08)	(0.08)	(0.14)	(0.07)	(0.07)	(0.14)
Profitability	0.61	0.56	2.17	0.79	0.84	−3.22
	(0.60)	(0.60)	(2.21)	(0.88)	(0.88)	(4.22)
ETR	0.31*	0.31*	0.12	0.08	0.08	−0.27
	(0.18)	(0.18)	(0.13)	(0.16)	(0.17)	(0.32)
ETR missing dummy	−1.19***	−1.18***	−0.16	−0.72	−0.76*	−1.21**
	(0.44)	(0.44)	(0.70)	(0.45)	(0.45)	(0.52)
Constant	−3.26***	−2.93***		2.77***	2.39**	
	(0.73)	(0.75)		(1.03)	(1.06)	
Observations	2,043	2,043	824	1,693	1,693	669
R^2	0.05	0.04	0.09	0.04	0.03	0.12
Fixed Effects	NO	NO	YES	NO	NO	YES

The figure shows the industry fixed-effects regression of one-day abnormal returns on environmental pillar score (E score), emissions score, and emission intensity. Emission intensity is constructed as total emissions (scope 1 and 2) divided by market cap. Controls include size, market leverage, revenue growth, profitability, and effective tax rate. Fixed effects account for the Fama and French 17 industries. Clustered standard errors are reported in parentheses. ***, **, and * denote significance at the 1%, 5% and 10% level, respectively.

Chapter 5

Green Stocks and Monetary Policy Shocks: Evidence from Europe

Abstract

Policymakers and researchers, notably in Europe, have expressed concerns that the low-carbon transition may be inadvertently delayed by higher global interest rates. To examine whethergreen investment is especially sensitive to interest rate increases, we consider the effect ofunanticipated monetary policy changes on the equity prices of green and brown European firms. We find thatbrown firms, measured in terms of carbon emission levels or intensities, are more negatively affected than green firms by tighter monetary policy. This heterogeneity is robust to different monetary policy surprises, emission measures, econometric methods, and sample periods, and it is not explained by other firm characteristics. This evidence suggests that higher interest rates may not skew investment away from a sustainable transition.

Consumer prices accelerated surprisingly quickly in many countries in 2021 and 2022, and central banks responded by pushing up short-term interest rates at the fastest pace in decades, from levels near zero to around 5% or more. While this global monetary tightening appears to have been successful in fostering a return toward price stability, higher interest rates may also have inadvertently slowed the transition to a low-carbon economy. Such an adverse side effect would raise serious concerns given the necessity of a green transition to forestall further climate change (e.g., IPCC, 2023). Observers have worried that the increase in the cost of capital resulting from tighter monetary policy would significantly and disproportionately reduce investment in new renewable and green projects. For example, Schnabel (2023) highlighted concerns in Europe that the latest monetary policy tightening "may discourage efforts to decarbonize our economies rapidly." She noted the competitive disadvantage of building renewable energy facilities at high interest rates. Intuitively, the substantial initial financing costs of renewable energy infrastructure together with minimal future fuel costs result in long-duration cash flow investments that should be particularly sensitive to interest rates. Indeed, research has highlighted the relevance of financing conditions for the production of renewable energy and identified the level of interest rates an important determinant of the cost of generating clean electricity; see Egli et al. (2018), Schmidt et al. (2019), and the references therein. Thus, the sharp rise in interest rates in recent years may have reduced investments in clean energy production and slowed decarbonization efforts (Monnet and van 't Klooster, 2023; Aguila and Wullweber, 2024).

Of course, a monetary tightening will likely slow business investment of all types. The key open empirical question is whether a monetary tightening has disproportionate effects on green investment, technologies, and businesses. Does monetary policy have heterogeneous effects so that low-carbon "green" companies are more sensitive to interest rates than higher-carbon "brown" firms? In this case, a tighter stance of monetary policy may unduly slow the green transition. In this paper, we investigate this issue and find little supporting evidence in financial markets for such adverse effects from tighter monetary policy. Instead, the equity prices of brown firms appear to be more sensitive to interest rate surprises than those of low-carbon, green firms.

Our empirical approach is an event-study analysis of the firm-level stock market responses to monetary policy actions by the European Central Bank (ECB). As in much recent empirical monetary research, we focus on high-frequency monetary policy surprises, that is, the changes in interest rates over narrow intraday windows around the Governing Council's policy announcements. Because these surprises are exogenous with respect to changes in asset prices around the policy announcements, they can identify the causal effects of monetary policy actions on financial markets and macroeconomic outcomes.[1] We consider several different measures of ECB monetary policy surprises in our analysis. We combine these data with firm-level environmental measures, stock prices, and financial accounting data for euro area firms. Our baseline measure of greenness is emission intensity, calculated as the ratio of carbon emissions to sales, an indicator that is widely used in climate finance to differentiate green and brown stocks (e.g., Bauer et al., 2022; Huij et al., 2024). Alternatively, we measure the greenness of firms using other emission intensity measures, emission levels, and

[1] Early empirical studies using monetary policy surprises include Rudebusch (1998), Kuttner (2001), Bernanke and Kuttner (2005) and Gurkaynak et al. (2005). For recent work and an in-depth discussion of this high-frequency identification, see Bauer and Swanson (2023a).

indicators based on ESG (environmental, social, and governance) data.

The central result of our panel regressions is that ECB policy surprises have greater effects on brown stocks, that is, stocks of firms with high emissions and emission intensities. This result is robust to using a range of different regression specifications and sample choices as well as different measures of monetary policy surprises and different emission-based measures of greenness (although we do not find systematic firm-level differences based on ESG indicators). Regressions that control for sector-by-time fixed effects yield similar estimates as our baseline regressions, which implies that the within-industry green/brown heterogeneity in interest rate sensitivity is similar to the across-industry heterogeneity. In addition to the event-study panel regressions, we also employ tools from empirical asset pricing: We form green and brown portfolios based on lagged emission intensities that investors could have constructed in real-time. The results show that brown portfolios reacted relatively more strongly to monetary policy. For example, after an ECB tightening announcement, brown-minus-green spread portfolios tend to loose substantial value. For the past few years of monetary tightening we document that the largest recent hawkish policy surprises resulted in substantial underperformance of brown stocks, in line with our overall evidence. Overall, our evidence strongly suggests that tighter monetary policy hurts high-carbon companies and brown stocks significantly more than low-emission companies and green stocks.

The firm-level impact of monetary policy has been shown to depend significantly on firm characteristics such as leverage, age, size, and liquidity, among others.[2] Existing studies have focused exclusively on the United States, and there is no established evidence of the heterogeneous effects of ECB monetary policy on the stock prices or investment behavior of European companies, at least to the best of our knowledge. To fill this gap, we consider a range of commonly used firm characteristics, and document how they affect the sensitivity of stock prices to ECB policy surprises. Consistent with the industry-level results of Bernanke and Kuttner (2005) for the U.S., we find that stocks with higher market betas exhibit a stronger sensitivity than low-beta stocks. We also find an important role for the book-to-market ratio, leverage, profitability and age, broadly consistent with U.S. evidence.

Green and brown stocks show certain systematic differences regarding their business characteristics. For example, high-carbon firms tend to have higher leverage, lower profitability, and more tangible capital, which are all attributes typically associated with a greater sensitivity to monetary policy. This firm-level variation naturally raises the question whether the differential response of green and brown stocks that we document is a separate dimension of heterogeneity, or instead is simply a reflection of other differences between brown and green firms. To answer this question, we estimate regressions that include interactions of the ECB policy surprise with both carbon intensity as well as with various other firm characteristics, either individually or jointly. Controlling for other characteristics has essentially no impact on our estimates of the green-brown heterogeneity, which indicates that it is indeed the greenness of a firm that plays a significant independent role for the stock price sensitivity to monetary policy.

Several other recent studies have examined the differential impact of monetary policy interest rate shocks on the equity prices of brown and green firms, including Döttling and Lam (2024), Havrylchyk and Pourabbasvafa (2023), Patozi (2024), and

[2]See, for example, Ozdagli (2018), Ozdagli and Velikov (2020), Ottonello and Winberry (2020), Cloyne et al. (2023), Jeenas (2024), Döttling and Ratnovski (2023), and Jungherr et al. (2024).

Benchora et al. (2023). These all use an event study research strategy to investigate the heterogeneous effects of Federal Reserve monetary policy announcements on the equity prices of U.S. green and brown firms. The four papers broadly arrive at a similar conclusion that higher-carbon brown firms tend to have a stronger equity price responses to monetary policy shocks than lower-carbon green firms. Our results for Europe are qualitatively and quantitatively consistent with these earlier results for the United States. By contrast, despite using similar event study methods, Fornari and Gross (2024) find that green firms are as sensitive to monetary policy as brown firms in the United States and more sensitive in the euro area. The difference between their findings and other work largely stems from their particular classification of brown firms, which includes all firms that do not report emissions data and is binary, ignoring shades of green and brown. Fornaro et al. (2024) classify green U.S. firms using data on green patents, and find that investment by these firms declines more strongly in response to monetary contractions than for other firms, which leads the authors to conclude that "higher interest rates and tighter credit conditions could slow down [...] progress toward climate sustainability goals." Clearly, the empirical findings about differential effects of monetary policy strongly depend on the type of green-brown firm classification, which underscores the importance of further research in this area.

Across all of the studies, there is little agreement as to the relevant causal mechanisms and interpretations for the differential sensitivity of green and brown stocks to monetary policy. We distinguish four broad possible channels. In a "credit channel" explanation, brown firms are more reliant on external financing; therefore, their stock prices are more sensitive to changes in interest rates. However, according to our European results, differences in firm characteristics capturing the importance of external finance, including the share of tangible capital, do not help explain the green/brown policy response heterogeneity, in contrast to the findings of Havrylchyk and Pourabbasvafa (2023) and Döttling and Lam (2024) based on U.S. data. Two other channels relate to the existence of a carbon premium, that is, the higher expected returns for brown stocks. If there is a sizable carbon risk premium in brown stocks, then their stronger response could arise from the effects of monetary policy on the quantity of transition risk (Benchora et al., 2023) or more broadly on risk appetite and prices of risk via a "risk-taking channel of monetary policy" (Bauer et al., 2023).[3] Similarly, if the carbon premium also reflects green investor preferences—a carbon aversion premium—that could also reduce the sensitivity of green stocks to monetary policy shocks (Benchora et al., 2023; Patozi, 2024). While the climate finance has not reached a consensus on the existence of a carbon premium (Bauer et al., 2022; Atilgan et al., 2023; Zhang, 2024), recent evidence by Pastor et al. (2022) and Eskildsen et al. (2024) for higher expected returns of brown stocks provides some indirect support for these two channels. Finally, brown stocks may be more sensitive to monetary policy via a "demand channel" if the high-carbon firms face demand for their products that is more responsive to interest rate changes. It is well known that some industries, including durable goods and housing, are more cyclical and interest rate sensitive than others (e.g. Petersen and Strongin, 1996), and such differences between green and brown industries—together with within-industry differences in interest rate sensitivity

[3] In important related work, Altavilla et al. (2024) examine the effects of ECB monetary policy shocks on climate risk in bank lending and uncover a climate risk-taking channel for monetary policy, so an unexpected monetary tightening raises the climate risk premium charged to high-emission firms.

of demand—may help explain differences in the heterogeneous policy response of green and brown stocks. However, for all of these potential explanation, much more work is needed to sharpen both the evidence and our theoretical understanding of their role in explaining differences in green and brown stock price sensitivities.

Most earlier work in this area focuses solely on the U.S. experience, and our examination of the green/brown heterogeneity of European equity pricing with respect to ECB monetary policy shocks provides an important investigative expansion along several dimensions. First, while it was the monetary tightening of 2022 and 2023 that motivated concerns about the impact of higher interest rates on low-carbon investment, the U.S. studies generally have samples that end before this crucial episode. Our data sample extends to include that tightening and the associated relevant empirical evidence.

Even more importantly, it is in Europe that concerns about the impact of higher interest rates on the green transition have been most salient (Schnabel, 2023). U.S. policymakers have not expressed the same level of urgency or even agreement on this issue as European policymakers.[4] The relative lack of clarity about a U.S. green transition path must affect investors' perceptions and pricing of climate equity risk. For example, a green transition in the United States may be plagued by so much policy uncertainty that investors view changes in the Federal Reserve policy rate as a second-order determinant to green/brown valuations. Our European analysis can better link relevant empirical evidence with the widespread and high-level discourse on the connections between monetary policy and the green transition. In this way, our evidence speaks to the question whether tighter monetary policy and higher interest rates may have slowed the path to carbon neutrality by adversely influencing firms' investment decisions, capital replacement, and sustainability efforts.

There are a variety of other differences between the U.S. and European economies that can inform our understanding of green/brown monetary policy heterogeneity. For example, relative to the United States, the euro area has a different set of climate policies—with a greater skew towards carbon taxes than subsidies–that may interact differently with higher interest rates and monetary policy more generally. In addition, the euro area may also have a greater share of investors with preferences for low-carbon firms given the divisive and politicized views of sustainable investing in the United States, and Patozi (2024) argues that such green investors are less likely to substitute away from green stocks following a contractionary monetary policy shock. Finally, the different sectoral distribution of the European economy provides a useful check on the U.S. results. Havrylchyk and Pourabbasvafa (2023) suggests that a key driver of the U.S. green/brown differential reaction to Fed policy shocks is the outsize reaction of the oil and gas mining and production sectors. This reaction may however partly reflect the sizable interest rate sensitivity of fossil fuel prices and consumer demand, a mechanism that is only tangentially related to the impact of interest rates on the green transition. In Europe, where fossil fuel production is a much smaller share of total output, this confounding effect is greatly reduced. In sum, the European experience provides a new and substantially different empirical sandbox to explore the outstanding issues and discrepancies in earlier research.

In terms of the climate finance literature, our paper makes five empirical contri-

[4] The United States is a notable outlier among developed nations in having weak legal, social, and political commitments to a green transition. For example, the United States is the only country to ever have withdrawn from the Paris Agreement on Climate Change.

butions. First, we document that in the euro area, stocks of high-carbon firms are more sensitive to monetary policy surprises. Second, we provide new results on the heterogeneous effects of the ECB's monetary policy on euro area stocks, which show an important role for firms' leverage, profitability, and book-to-market multiples. Importantly, none of these other firm characteristics account for the heterogeneous impact of monetary policy on green and brown firms. Third, our closer investigation of the recent monetary tightening episode suggests that green European firms were not disproportionately affected by the ECB's rate hikes, alleviating concerns about a negative impact on the ongoing green transition. Fourth, we contribute to the discussion of possible channels for differential brown/green effects of monetary policy. Finally, we show that our results hold even if we narrow our focus to just the energy industry and compare the differential monetary policy effects on renewable and fossil fuel equity indexes.

Our paper is most closely related to the four U.S. studies cited above on the effects of Fed monetary policy surprises on green and brown stocks. Other empirical work in climate finance has used event studies to estimate the differential effects on green and brown stocks of climate and fiscal policies (Bauer et al., 2024b) or election outcomes that shift probabilities of future policies (Ramelli et al., 2021). Separately, a burgeoning macro-climate literature investigates the interactions of monetary policy and climate policies, or the effects of green monetary policies, using general equilibrium models. Ferrari and Nispi Landi (2024, 2023) study green QE—purchases of low-carbon assets by central banks—and find that in the euro area, it likely has a negligible impact on carbon emissions. Both Diluiso et al. (2021) and Dafermos et al. (2018) conclude that green QE for financial stability can reduce climate-induced financial instability. Benmir and Roman (2020) consider interactions of carbon policy and quantitative easing policies. Our paper contributes new empirical evidence on the climate-relevant consequences of monetary policy decisions. Finally, our work is part of the broader climate finance literature on the pricing of green and brown stocks. This literature is large and growing quickly, see Bolton and Kacperczyk (2021), Pastor et al. (2022), Bauer et al. (2022), Aswani et al. (2023), Ardia et al. (2023), Huij et al. (2024), Eskildsen et al. (2024), and Bauer et al. (2024a), among many others.

The paper is structured as follows: Section 5.1 explains our different data sources and presents summary statistics. Section 5.2 shows the main results and various robustness checks. Section 5.3 considers the role of other firm characteristics for explaining the observed heterogeneity in the responsiveness of European firms to monetary policy. Section 5.4 discusses the possible channels and explanations for the documented green-brown heterogeneity. Section 5.5 considers the narrower question about whether interest rates disproportionately affect the renewable energy sector. Section 6.6 concludes.

5.1 Data

5.1.1 Firm-level data

Our firm-level data are from the ESG and Datastream databases of LSEG Data & Analytics. The ESG database is quite comprehensive and currently includes firms that together account for more than 90% of the global stock market capitalization.[5] From this database, we include all euro-area publicly traded firms.[6] While coverage in the ESG database begins in 2002, the availability of emissions data in the early years is quite sparse; therefore, we use emissions data beginning in January 2010. Our sample ends in October 2023.

We consider a range of measures of firm-level greenness. First, we use measures of the carbon emissions of a firm, specifically, the sum of scope 1 (direct) and scope 2 (indirect) total greenhouse gas (GHG) emissions in tons of total CO_2 equivalents. Following other climate finance studies, we leave out scope 3 emissions, because these indirect emissions from upstream and downstream activities of the reporting firm are very large in magnitude and particularly difficult to estimate.[7] We focus on emission intensity as our preferred measure of greenness, calculated as the ratio of emissions and total revenues. This measure is widely used in empirical work in climate finance (e.g., Bauer et al., 2022; Benchora et al., 2023; Havrylchyk and Pourabbasvafa, 2023). As an alternative measure of emission intensity, we scale emissions by a firm's market capitalization. We also consider the (log) level of emissions, which Bolton and Kacperczyk (2021, 2023c) found to be an important determinant of stock returns. For all of these greenness measures, the underlying carbon emissions of some firms are estimates from the data provider that are imputed based on other firm-level data. The inclusion of estimated emissions can be problematic in some empirical applications (Aswani et al., 2023) but not others (Bauer et al., 2022). To ensure that our results are robust to this issue, we also consider alternative samples that are limited to only the observations when firms actually reported emissions, excluding vendor-based estimates.

Second, we consider the environmental ratings that constitute ESG scores. Although these ratings have some disadvantages, including discrepancies across data providers (Billio et al., 2021) and ex post revisions (Berg et al., 2021), they are often used in climate finance research. Indeed, in some applications, certain ESG scores seem to appropriately capture climate transition risk (Pastor et al., 2022; Bauer et al., 2024b). We use two types of scores: "E scores" are the broader environmental scores that incorporate metrics from three different categories: emissions, innovation, and resource use. "Emission scores" are the category score for metrics in the emission category. These scores are between 0 (a poor environmental rating) and 100 (a very green firm), and they are constructed by comparing firms to their industry peers.

We perform a two-sided winsorization of the variables that are based on emissions, as well as all of the accounting variables described below, at the one-percent level. Panel A of Table 5.1 shows summary statistics for the firm-by-year panel of greenness measures.

For financial market data, we extract daily returns and annual accounting data from LSEG Datastream and Worldscope. To create our baseline sample of firms, we filter out stocks that are not common equity or primary equity quotations. We also remove securities with prices lower than $1 and securities whose name fields indicate non-common equity affiliation.[8] Our final sample contains 916 companies. Firm-

[5] For more information on this ESG database, see `https://www.lseg.com/en/data-analytics/financial-data/company-data/esg-data`. It was formerly called the Refinitiv ESG database and, still earlier, the ASSET4 database.

[6] Our results are robust to excluding firms in the financial and utilities industries, as in Ozdagli and Velikov (2020) and Döttling and Lam (2024).

[7] See, for example, Kruse et al. (2020), Bauer et al. (2022) and Huij et al. (2024).

[8] These filters are common practice in the empirical asset pricing literature. For a more detailed

Table 5.1: Summary Statistics

	Mean	SD	Min	q25	Median	q75	Max	Obs.
(A) Environmental performance								
Emissions/sales	0.30	2.14	0.00	0.01	0.04	0.14	107.71	4,435
Emissions/market cap	0.59	3.45	0.00	0.01	0.03	0.18	108.48	4,435
Log emissions	11.63	2.90	0.59	9.60	11.61	13.61	19.09	4,435
E score	55.38	26.32	0.00	36.38	59.41	77.26	98.87	4,434
Emission score	62.59	28.90	0.00	42.13	68.69	87.79	99.85	4,434
(B) Firm-level controls								
Size	21.89	1.67	16.25	20.77	21.87	23.01	28.23	4,435
Book/market	0.87	4.10	0.00	0.30	0.51	0.86	116.73	4,435
Leverage	0.28	0.17	0.00	0.16	0.26	0.38	3.71	4,435
Profitability	0.03	0.57	−35.83	0.00	0.01	0.03	2.93	4,435
Rev. growth	1.33	2.95	−12.55	−0.63	1.06	3.17	15.62	4,435
Investment	0.04	0.04	0.00	0.02	0.04	0.06	0.60	4,435
Log PP&E	20.36	2.21	10.20	18.97	20.46	21.81	26.05	4,435
Liquidity	0.13	0.11	0.00	0.06	0.10	0.17	1.52	4,434
Tangibility	0.77	0.27	0.00	0.63	0.86	0.96	6.83	3,367
Beta	0.98	0.45	−0.23	0.65	0.96	1.28	2.59	4,423
Cash flow	0.07	0.09	−0.89	0.03	0.06	0.09	3.73	4,435
Age	21.96	18.80	−9.00	11.00	19.00	27.00	117.00	4,349
(C) Firm-level stock returns (%)								
Daily return	0.00	2.55	−33.81	−0.96	0.04	1.12	41.69	30,504
(D) High-frequency monetary policy surprises (bps)								
3-months OIS	0.34	2.21	−5.60	−0.17	0.00	0.50	10.35	107
1-Year OIS	0.05	3.37	−14.92	−0.65	−0.12	1.07	10.66	107
PC	0.05	3.61	−12.75	−0.93	−0.15	1.37	12.42	107
Target	0.08	2.07	−6.26	−0.44	−0.30	0.15	11.08	107
FG	0.33	1.92	−4.15	−0.62	0.03	0.71	9.96	107
KTW	0.34	2.21	−5.60	−0.17	0.00	0.50	10.35	67
JK	0.69	4.34	−7.36	−1.37	0.54	2.25	18.72	107

Summary statistics for firm-level variables and monetary policy surprises. Environmental measures are described in the text. Size is log of market capitalization in millions USD, market leverage is total debt divided by by total assets, profitability is return on assets, investment is capital expenditure divided by total assets. Variables based on emissions, and all firm-level controls, are winsorized at the one-percent level. We exclude observations with missing values for any of the variables used in our baseline regression (emission/sales, size, book/market, leverage, profitability, revenue growth, investment, and log PP&E). Monetary policy surprises are the 30-minute changes in the three-month OIS rate around the monetary policy announcement. Returns are daily calculated using close to close prices. Sample for panels A and B is a year-by-firm panel from 2010 to 2021; panels C (announcement-by-firm panel) and D (time series) use 107 ECB announcements from January 2012 to October 2023.

level controls include size (proxied by the log of market capitalization), market-to-book equity, profitability (measured as return on assets), book leverage, sales growth, investment (calculated as capital expenditure divided by total assets), and log of net property, plant, and equipment. The daily stock returns are close-to-close returns

description, see Bauer et al. (2022).

adjusted for buybacks and stock splits. Panels B and C of Table 5.1 show summary statistics of our firm-level controls and the daily stock returns.

Our empirical approach tries to ensure that the differential green/brown equity returns in our event-study analysis could have actually been achieved by investors using publicly available information. Therefore, we account for publication lags of firm-level accounting and ESG data. While accounting data are usually released within three to six months after the end of the reporting year, ESG data—including emissions—are often not made public until up to nine months after the reporting year (Bauer et al., 2022; Ardia et al., 2023). To conservatively account for this information lag, we impose a twelve-month publication lag, which is best illustrated with a specific example: Most accounting data for 2020 would have been published in the second quarter of 2021, and much of the 2020 emissions data were not published until fall 2021. Accordingly, we pair the accounting and emissions data for 2020 with the ECB announcements and stock returns that occurred during 2022.[9]

5.1.2 ECB monetary policy surprises

To estimate the causal effects of the ECB's monetary policy changes on green and brown stocks requires a measure of exogenous policy changes. Following a long tradition in empirical monetary economics, going back to Kuttner (2001) and Gurkaynak et al. (2005), we use high-frequency changes in interest rates around monetary policy announcements, which reflect new information about the current stance and future course of monetary policy.[10]

For the ECB, a detailed high-frequency database of asset price changes around monetary policy announcements is available online: the Euro Area Monetary Policy Event-Study Database, described in Altavilla et al. (2019). To account for the specific way the ECB releases information about its monetary policy actions, which differs from the Fed's monetary policy announcements, this database contains asset price changes over three different intraday windows: the press release window, the press-conference window, and the total monetary event window, which includes both the press release and the President's press conference. Based on changes in different interest rates over the different event windows, a number of policy surprise measures can be constructed.

Our baseline measure is the change in the one-year overnight index swap (OIS) rate over the entire monetary event window, which includes the effects of both the press release and the subsequent press conference on financial markets.[11] The change in the one-year OIS rate captures the revision in the average expected future policy rate over the next year.[12] The one-year maturity corresponds to the horizon of monetary policy expectations typically considered in the literature on high-frequency monetary policy surprises. For example, Gurkaynak et al. (2005), Nakamura and Steinsson (2018a) and Bauer and Swanson (2023a) use money market futures up to the four-quarter-ahead Eurodollar futures contract. This measure also has the advantage of

[9] In additional analysis, we have verified that a six month publication lag, as used in Bauer et al. (2022) and many other empirical asset pricing studies, leads to similar results.

[10] See Bauer and Swanson (2023a) for a recent discussion of the practical considerations and caveats of using high-frequency monetary policy surprises.

[11] Specifically, this is the change in the median OIS quote from around 13:30—before the press release—to the median quote at around 15:45—after the press conference.

[12] Change in risk premia may also contribute to changes in term rates, but their contribution to near-term rate changes at high frequencies is likely to be minor (Piazzesi and Swanson, 2008).

being straightforward to calculate without requiring additional assumptions. We also consider the change in the three-month OIS rate over the monetary event window, following Krusell et al. (2023). A relatively short interest rate maturity is often used in this context, going back to Kuttner (2001) who used the spot-month fed funds futures contract, while Gertler and Karadi (2015) and others use the three-month-ahead fed funds futures contract. However, short-term interest rates in the Euro area were constrained by the effective lower bound on nominal interest rates for a substantial part of our sample period. During this time, monetary policy events mainly affected interest rates and financial markets via forward guidance, which is a strong reason to extend the monetary policy surprise measure to longer maturities.

To consider longer-term rates beyond the one-year OIS rate, we follow the common practice to construct monetary policy surprises by combining changes in different interest rates; see Gurkaynak et al. (2005), Nakamura and Steinsson (2018a), Altavilla et al. (2019), and many others. Specifically, we use the first principal component of changes in the seven OIS rates considered by Altavilla et al. (2019), with maturities of one, three, and six months, and one, two, five and ten years. We again take the change over the monetary event window to capture as much of the new information about the ECB's policy as possible. This surprise is then scaled to have a unit effect (i.e., a principal component loading of one) on the one-year OIS rate. We call this measure "PC" for principal component.

For better comparability with existing results, we also analyze the effects of the "Target" and "Forward Guidance" surprise of Altavilla et al. (2019). Both are based on principal components of the changes in the seven OIS rates mentioned above. The Target surprise uses changes over the press release window, and is constructed in a way to load most strongly on short-term rates changes, in order to capture surprise changes in the target rate similar to the target factor of Gurkaynak et al. (2005) for the Federal Reserve. The Forward Guidance surprise is based on changes in OIS rates around the press conference window, and is closely related to the OIS rates with intermediate maturities that are most strongly affected by ECB guidance about the future path of policy rates. For both of these surprises, Altavilla et al. (2019) have documented significantly negative effects on the European stock market, and we will investigate the heterogeneity of these effects.

One concern about interest rate changes around monetary policy announcements is that they might be driven not only by monetary policy news, but also by signals about the central bank's economic outlook that could directly affect expectations. These non-monetary "information effects" might confound empirical analysis that relies on high-frequency surprises, because they would have opposite effects on asset prices and macroeconomic variables than monetary policy shocks. Information effects therefore received considerable attention in the monetary economics literature; see Campbell et al. (2012) and Nakamura and Steinsson (2018a), among many others.[13] Several studies, including Jarociński and Karadi (2020) and Miranda-Agrippino and Ricco (2021) have proposed new measures of monetary policy surprises that are constructed in ways to mitigate or even eliminate the confounding impact of information effects.

[13] Recent work by Bauer and Swanson (2023b,a) has argued that some of the evidence for information effects may in fact be due to a different channel that arises from incomplete information about the central bank's reaction function. Their evidence supports the view that the Fed's announcements mainly provide new information about the course of monetary policy, and not about the Fed's own economic outlook.

The simplest approach, the "poor man's proxy" of monetary shocks from Jarociński and Karadi (2020), is to exclude from the sample of central bank announcements those observations when the aggregate stock market moved in the same direction as the interest-rate surprise. Krusell, Thürwächter and Weiss (2023) use this approach to identify ECB announcements around which the change in the three-month OIS rate most likely captures a monetary shock, and we consider this "KTW" surprise in our robustness analysis. An alternative approach uses a Bayesian VAR to decompose each high-frequency surprise into monetary policy and information shocks. Jarociński and Karadi (2020) provide the resulting shock series for the ECB, and we include this "JK" series in our analysis.[14]

Summary statistics for all seven monetary policy surprises are presented in panel D of Table 5.1. The firm-level emissions data starts in 2010, and due to the publication lag described above, we begin our sample of stock returns and monetary policy surprises in 2012. In total, our sample contains 107 ECB announcements from January 2012 to October 2023.

5.2 Effects of monetary policy on green and brown stocks

We begin our investigation of the effects of ECB policy surprises on green and brown stocks using the following baseline panel regression:

$$r_{it} = \beta_1\, mps_t + \beta_2\, mps_t\, g_{it} + \beta_3\, g_{it} + \gamma' X_{it} + \alpha_i + \varepsilon_{it}, \tag{5.1}$$

where r_{it} is the stock return of firm i (measured in percent) on ECB announcement day t. The monetary policy surprise, mps_t, is the intraday percentage point change in the one-year OIS rate around the policy event, as described above. The coefficient β_1 is expected to be negative given the conventional evidence that the overall stock market tends to decline in value following a positive monetary policy surprise (e.g., Altavilla et al., 2019; Gurkaynak et al., 2005). Firm-level greenness, g_{it}, is measured using emissions intensity—so that high values of g_{it} correspond to brown stocks—and standardized to have unit standard deviation for ease of interpretation. The coefficient β_2 captures how differing levels of greenness will alter the sensitivity of firm-level returns to mps_t. Therefore, if β_1 is negative, a negative β_2 implies that brown stocks have a stronger negative reaction to policy surprises than green stocks. Following earlier studies, we also include other firm-level controls in X_{it}: size, market-to-book equity, leverage, profitability, sales growth, investment, and the logarithm of "properties, plants and equipment" (PP&E).[15] As explained above in Section 5.1.1, we use a twelve-month publication lag for firm-level controls and the greeneness measure, so X_{it} and g_{it} correspond to the calendar year two years before the ECB announcement t.

[14] Specifically, we use the "MP_median" series from Marek Jarocinski's website, see `https://github.com/marekjarocinski/jkshocks_update_ecb_202310`. This measure results from a decomposition of the surprise that is the first PC of OIS changes with maturities of one, three, six, and twelve months over the monetary event window, based on the Euro Area Monetary Policy Event-Study Database.

[15] These controls are largely the same as those used in Döttling and Lam (2024). Size is the log of the firm's market cap. Leverage is the ratio of total debt to total assets. Profitability is the ratio of gross profits to total assets. Investment is the ratio of capital expenditure to total assets.

Table 5.2: Effects of ECB policy surprises on green and brown stocks

	(1)	(2)	(3)	(4)	(5)
mps	-9.87^{**}	-9.87^{**}			
	(4.94)	(4.94)			
$mps \times g$		-2.02^{***}	-2.07^{***}	-2.22^{***}	-2.13^{***}
		(0.23)	(0.21)	(0.32)	(0.30)
g	0.17^{***}	0.15^{***}	0.14^{***}	0.14^{***}	0.15^{***}
	(0.03)	(0.03)	(0.02)	(0.03)	(0.03)
Firm FE	Y	Y	Y	Y	Y
Time FE	N	N	Y	N	N
Industry-by-time FE	N	N	N	Y	N
Country-by-time FE	N	N	N	N	Y
Observations	30,504	30,504	30,504	30,504	30,504
Adjusted R^2	0.04	0.04	0.35	0.37	0.37

Estimates for panel regression (5.1) and alternative specifications. The dependent variable is the daily stock return of each firm on the day of an ECB announcement. Monetary policy surprises, *mps*, are intraday changes in the one-year OIS rate. The greenness measure, g, is emission intensity, calculated as scope 1+2 GHG emissions divided by total revenue, measured two years before the announcement date t, and standardized to have a unit standard deviation. Additional controls include size, market-to-book equity, leverage, profitability, sales growth, investment, and log PP&E. Industry-by-time fixed effects are constructed using the two-digit SIC industry classification. The sample period from January 2012 to October 2023 includes 107 ECB announcements. Standard errors in parentheses are two-way clustered by firm and announcement, and ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

Table 5.2 reports the estimation results. The first column includes only mps_t but no interaction term in order to establish the average response of stock prices to ECB policy surprises. The estimate of β_1 implies that the announcement of a one percentage point positive (hawkish) monetary policy surprise would cause stock prices to decline on average by 9.87 percent. The standard deviation of the policy surprise is about three basis points (see Table 5.1), so a one-standard-deviation surprise corresponds to a decline of stock prices by about 0.3 percent, a magnitude that is broadly in line with the estimates Altavilla et al. (2019) obtained for European stock market indices.

The second column Table 5.2 shows estimates for the regression with an interaction effect between the policy surprise mps_t and the firm's emission intensity, corresponding to the specification in equation (5.1). The interaction coefficient is negative and strongly statistically significant, meaning that brown stocks with higher emissions intensity g_{it} respond more negatively to positive interest rate surprises than green stocks.

The remaining columns of Table 5.2 show estimates for specifications that add time fixed effects, sector-by-time, or country-by-time fixed effects. Because these fixed effects allow for a different stock market return on each ECB announcement day, mps_t drops out from the regression. The specification

$$r_{it} = \beta_2 \, mps_t \, g_{it} + \beta_3 \, g_{it} + \gamma' X_{it} + \alpha_i + \alpha_{st} + \varepsilon_{it}, \tag{5.2}$$

with firm fixed effects α_i and sector-by-time fixed effects α_{st} is particularly common in the literature on heterogeneous stock market effects of monetary policy (e.g. Patozi, 2024), and these estimates are in column (4). The statistical significance of the estimates of β_2 remains high, and the magnitude of the coefficient largely unchanged, across all fixed effects specifications.

The difference in sensitivities between green and brown firms is quantitatively meaningful. To understand the magnitudes, consider the response of green and brown firms to a one unit contractionary monetary policy surprise, that is, a one percentage point increase in the one-year OIS rate around the ECB monetary policy event. Using the estimates from column (4), a brown firm with emission intensity one standard deviation above average would experience, on average, a decline in its stock price of 12.1 percent. This decline is materially larger than the predicted decline for a green firm with emission intensity one standard deviation below average, which is 7.6 percent.

The heterogeneity of the effects of monetary policy on green and brown stocks could in principle be different within and across industries. For example, it could be the case that stocks in brown, high-carbon industries were more strongly affected by monetary policy than those in green industries, while within industries, green and brown stocks exhibited similar effects. Table 5.2 shows that this is not the case. The estimates in column (4) include industry-by-time fixed effects that capture all of the cross-industry heterogeneity in the effects of mps_t on stock returns, so the interaction coefficient β_2 captures the residual within-industry heterogeneity in this specification. This β_2 estimate is almost identical to the one in column (3), where we do not account for industries. The results therefore suggest that the within-industry and across-industry differences in monetary policy sensitivities are very similar.

To further investigate the heterogeneous response of green and brown firms to monetary policy, we sort our firms into five portfolios based on their emission intensity, and estimate a panel regression separately in each quintile of firms. This grouping approach, which follows Cloyne et al. (2023) and subsequent work, has the advantage that it does not restrict the heterogeneity to be linear, instead allowing each group of stocks to exhibit a different policy response.[16] The top panel of Table 5.3 shows the estimation results for each quintile of firms. The regressions include firm fixed effects as well as all the firm-level controls used in Table 5.2. The sensitivity of brown firms, in the top quintile, is higher than the sensitivity of other firms. The sensitivity increases with quintiles, but not linearly so, which underscores the importance of obtaining group-specific estimates of policy sensitivity.

A related approach uses portfolio returns in time series regressions. We form brown-minus-green (BMG) portfolios using the differences in returns between the top and bottom quintile portfolios—using either equal-weighted or value-weighted returns. The BMG returns on days with ECB announcements are then regressed on the monetary policy surprise. An advantage of this portfolio approach, based on a long tradition in empirical asset pricing, is that investors could have formed these portfolios in real time and obtained exposure to green and brown firms in a similar fashion.[17]

The bottom panel of Table 5.3 shows the estimation results. In the simple time series regression, the coefficient on mps_t is negative but not statistically significant at conventional significance levels for either equal-weighted or value-weighted portfolios. The magnitude of the response is substantially higher for the value-weighted portfolio,

[16]Patozi (2024) and Döttling and Lam (2024) also employ this method to estimate the heterogeneous response of green and brown stocks in the U.S. to Fed policy surprises, although they sort stocks based on E scores and emission levels, respectively.

[17]For recent work in climate finance using the portfolio approach, see Bauer et al. (2022) and the references therein.

Table 5.3: Effects of ECB policy surprises on green and brown portfolios

(A) Quintiles for emission intensity					
	Q1 (green)	Q2	Q3	Q4	Q5 (brown)
mps	-9.98^{**}	-7.60	-10.20^{**}	-9.73^{**}	-11.16^{**}
	(4.97)	(4.88)	(5.15)	(4.94)	(4.94)
Observations	4,292	5,850	6,886	6,979	6,497
Adjusted R^2	0.05	0.04	0.05	0.03	0.04

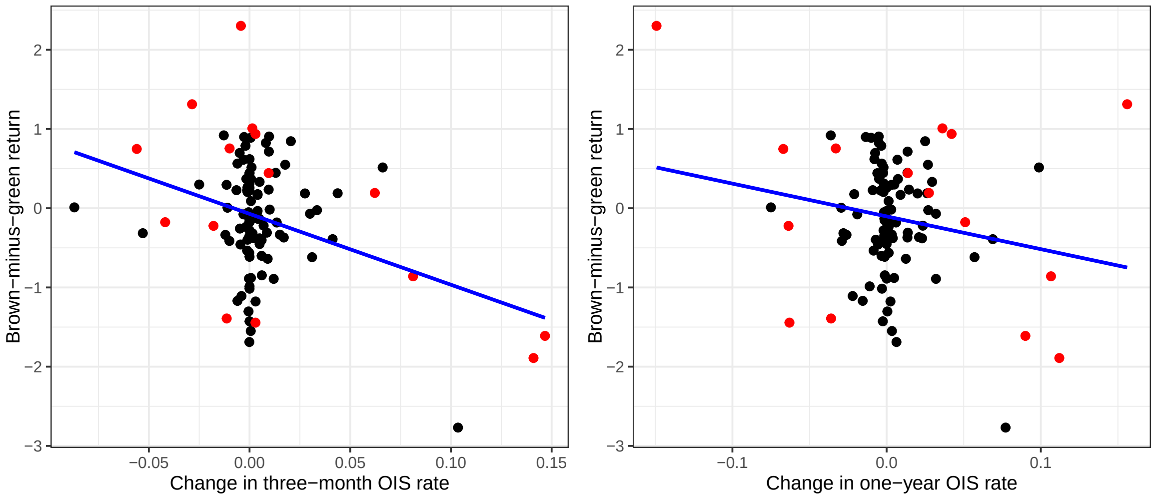
(B) Response of brown-minus-green portfolios						
	Equal-weighted			Value-weighted		
mps	-0.97	-2.98^{*}	-3.32^{**}	-4.14	-6.27	-8.69^{*}
	(1.40)	(1.71)	(1.58)	(3.64)	(5.15)	(4.98)
Observations	107	107	107	107	107	107
R^2	0.01	0.04	0.21	0.04	0.07	0.35
Double-sorted	N	Y	Y	N	Y	Y
Year FE	N	N	Y	N	N	Y
Month FE	N	N	Y	N	N	Y

Event-study regressions for stock returns around ECB announcements, using firm portfolios based on emission intensity. Panel (A) shows the estimated coefficient on the monetary policy surprise, *mps*, in separate panel regressions for each quintile of firms sorted by emission intensity. Controls include size, market-to-book equity, leverage, profitability, sales growth, investment, and log PP&E. We use firm fixed effects and standard errors that are clustered both at the firm and announcement level. Panel (B) reports results for time series regressions of portfolio returns on the monetary policy surprise, with brown-minus-green portfolios formed using the top and bottom quintile of the firm distribution based on emission intensity. We use white standard errors, reported in parentheses. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively. The sample period from January 2012 to October 2023 includes 107 ECB announcements.

suggesting that large firms play an outsized role in explaining the differential response. Following common practice to account for the impact of size in this context, we also double-sort first by size and then by greenness (Döttling and Lam, 2024). In this case, the return for the green portfolio, for example, is the average of the returns for portfolios with large green firms and small green firms. This double-sorting actually increases the magnitude of the estimated response of both equal-weighted and value-weighted BMG portfolio, and in the former case, the coefficient becomes marginally significant. Adding year and month fixed effects further increases the magnitudes of the coefficients, which are now significant at the 5-percent and 10-percent levels, respectively. Overall, the negative response of the BMG return to the policy surprise shows that brown stocks exhibit a stronger response to monetary policy than green stocks.

To better understand the differential response of brown and green portfolios to ECB policy surprises, Figure 5.1 shows a regression of the BMG return (the difference between the top and bottom quintiles of value-weighted portfolio returns) on the monetary policy surprise. The left-hand panel defines the policy shock using high-frequency changes in the three-month OIS rate and the right-hand panel uses the one-year OIS rate, as in our baseline estimates. Both scatter plots show a negative correlation; that is, monetary policy tightening shocks lead to a greater decline for brown stocks compared with green ones. In addition, in Figure 5.1 the ECB an-

Figure 5.1: Reaction of brown-minus-green portfolio to ECB surprises



The figure plots the BMG portfolio return—the difference between the top and bottom quintile value-weighted portfolios sorted by emission intensity—against the monetary policy surprise on ECB announcement days. Surprises are calculated as intraday changes in the three-month or one-year OIS rate around the monetary policy announcements. ECB announcements in 2022 and 2023 are shown in red. The sample period from January 2012 to October 2023 includes 107 ECB announcements.

nouncements for the most recent monetary tightening in 2022 and 2023 are shown in red. These latest observations fit in well with the stock market response over the full sample. Most notably, the three most hawkish surprises in the lower-right area of the left-hand plot—corresponding to the ECB announcements in March 2020, July 2022, and March 2023—led to particularly strong brown underperformance.

Our baseline results use carbon emission intensity as the measure of a firm's greenness, but for robustness, we consider a variety of other measures used in the climate finance literature. For example, there are different ways to scale emissions to calculate emission intensity. Our baseline measure normalizes by firm-level revenue, like many other studies, but emission intensity is also sometimes calculated using market capitalization or other measures of firm size (Ilhan et al., 2020; Bauer et al., 2024b). Furthermore, several empirical studies use the *unscaled* level of CO_2 emissions to characterize greenness.[18] Accordingly, the top panel of Table 5.4 reports estimates of the interaction of ECB policy surprises with greenness, with the latter defined as (1) the ratio of emissions to sales (our baseline result also reported in Table 5.2), (2) the ratio of emissions to market cap, or (3) the (log) level of scope 1+2 emissions. The regression specification is equation (5.2), which includes firm and industry-by-time fixed effects. Across all three emission-based measures, we find that the estimated effect is consistently negative and statistically significant.

A related issue for any greenness measure based on emissions or emissions intensity is that a non-negligible number of firms actually do not report CO_2 emissions. For many of these firms, data providers impute emissions based on other firm-level data.

[18]Bolton and Kacperczyk (2021) estimate a large carbon premium for the level of emissions but not for emission intensity, and argue the former is a better measure, while (Aswani et al., 2023) disagree. Döttling and Lam (2024) use the level of emissions to investigate the heterogeneous response to Fed policy surprises.

Table 5.4: Estimated effects of monetary policy surprises on different greenness measures

(A) All emissions (estimated and reported)			
	Emissions/sales	Emissions/market cap	Log emissions
$mps \times g$	-2.22^{***}	-1.08^{***}	-1.45^{**}
	(0.32)	(0.04)	(0.64)
Observations	30,504	30,504	30,504
Adjusted R^2	0.37	0.37	0.37
(B) Only reported emissions			
	Emissions/sales	Emissions/market cap	Log emissions
$mps \times g$	-0.03	-1.13^{**}	-1.50^{**}
	(0.22)	(0.47)	(0.59)
Observations	23,928	23,928	23,928
Adjusted R^2	0.41	0.38	0.41
(C) Scores			
	Emission score	E score	
$mps \times g$	-0.90	-1.18	
	(0.66)	(0.76)	
Observations	31,720	30,497	
Adjusted R^2	0.38	0.37	

Results of regression (5.2) of daily returns on monetary policy surprises interacted with alternative measures of greenness, g. Controls include size, market-to-book equity, leverage, profitability, sales growth, investment, and log PP&E, and we include firm and time-by-industry fixed effects. The sample period from January 2012 to October 2023 includes 107 ECB announcements. We use two-way clustered standard errors at the firm level and the ECB announcement date level. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

Some researchers have suggested that such imputations may be problematic for certain types of analysis and use samples with only firms that actually report emissions (Aswani et al., 2023; Bauer et al., 2022). In light of this critique, Panel B of Table 5.4 shows estimates for the same three emission-based metrics but only using those observations when firms have actually reported their (scope 1 and 2) emissions. For the case of reported emissions divided by sales, the interaction coefficient is insignificant, but for the other two metrics there is no appreciable difference from limiting the sample in this way.

As a final alternative approach to distinguish green and brown stocks, we consider the environmental scores and emissions scores that underlie the proprietary ESG ratings often used by financial analysts. This approach has been employed in empirical climate finance studies of the stock market; see, for example, Pastor et al. (2022) and Patozi (2024). But these ESG ratings have several shortcomings, including the fact that they are judgmental and can be quite inconsistent across different data providers. In addition, ESG ratings can get revised in subsequent data releases, meaning that the results of empirical studies using revised data may not be based on the same information available to investors at the time.[19] Results for emission scores and E (environmental) scores are shown in panel C of Table 5.4. In contrast to the emission-based measures, we do not find a statistically significantly different response of stocks that are classified as green and brown based on these scores. Overall, the estimated heterogeneity of the effects of monetary policy does not seem to be too sensitive to the exact measure of greenness, as long as it is based on carbon emissions instead of the scores from ESG data providers.

Table 5.5: Estimated effects of different monetary policy surprises on emission intensity

	Simple changes		Principal components			No info. effects	
	OIS 3m	OIS 1y	PC	Target	FG	KTW	JK
(A) Firm fixed effects							
mps	-6.16	-9.87^{**}	-10.3^{*}	-4.56	-11.66^{***}	-39.04	-24.67^{**}
	(9.42)	(4.94)	(6.20)	(9.87)	(4.13)	(26.05)	(10.02)
$mps \times g$	-1.76^{***}	-2.02^{***}	-1.67^{***}	-1.10^{***}	-0.53	-7.36^{***}	-2.94^{***}
	(0.16)	(0.23)	(0.24)	(0.14)	(0.48)	(0.87)	(0.27))
Adjusted R^2	0.01	0.04	0.04	0.01	0.03	0.14	0.15
(B) Industry-by-time fixed effects							
$mps \times g$	-1.76^{***}	-2.22^{***}	-1.82^{***}	-1.13^{***}	-0.88^{**}	-6.81^{***}	-3.14^{***}
	(0.22)	(0.32)	(0.25)	(0.17)	(0.39)	(1.89)	(0.65)
Observations	30,504	30,504	30,504	30,504	30,504	18,977	30,504
Adjusted R^2	0.37	0.37	0.37	0.37	0.37	0.44	0.37

Regressions of daily stock returns on emission intensity interacted with alternative monetary policy surprises (described in the text). Controls include size, market-to-book ratio, leverage, profitability, sales growth, investment, log PP&E, and emission intensity. In the top panel, we include only firm fixed effects, as in equation (5.1) and column (2) of Table 5.2. In the bottom panel, we include firm and industry-by-time fixed effects, as in column (4) of Table 5.2. Industry-by-time fixed effects are constructed using the two-digit SIC industry classification. The sample period from January 2012 to October 2023 includes 107 ECB announcements. Standard errors clustered at firm and announcement level are reported in parentheses. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

We also consider robustness to the different ECB monetary policy surprises described in Section 5.1.2. Table 5.5 shows results for two different regression specifications: The top panel reports estimates of equation (5.1) (corresponding to column (2) of Table 5.2), which include only firm fixed effects. To narrow down to within-industry heterogeneity, the bottom panel shows estimates for regressions that include industry-by-time fixed effects (corresponding to column (4) of Table 5.2).

The first two columns of Table 5.5 show results for simple changes in OIS rates, for the three-month and one-year maturities. The former has less of an impact on the stock market than the one-year rate, our baseline measure, but it also leads to a strongly significant negative interaction coefficient. The next three columns report results for monetary policy surprises constructed as principal components of OIS rate changes. The interaction coefficients are all negative, and generally statistically significant. The

[19] For more discussion of the discussion of the problems with ESG ratings, see Berg et al. (2022), Berg et al. (2021), and Billio et al. (2021).

only exception is the case of the forward guidance (FG) surprise of Altavilla et al. (2019) that uses the event window around press conferences. This surprise has a strong effect on the stock market, but only a marginally significant (bottom panel) or insignificant (top panel) interaction effect. Apparently, the green-brown heterogeneity is much stronger when the stock market response is measured with respect to the ECB surprise over the entire monetary event window. The last two columns use the policy surprise measures of Krusell et al. (2023) (KTW) and Jarociński and Karadi (2020) (JK) that are constructed to avoid contamination with information effects, based on the contemporaneous response of the aggregate stock market as explained in Section 5.1.2. The estimated response of the stock market is by construction much higher than for all other surprises, even though it is estimated quite imprecisely in the case of the KTW surprise. Importantly, the interaction effects are strongly significant, confirming that stocks of high-carbon firms respond more strongly to monetary policy surprises, independent of how this surprise is measured.

5.3 Other sources of heterogeneous effects

Considerable research has explored how interest rate shocks have different effects on companies' investment and operating behavior depending on firm-level characteristics such as leverage, age, size, liquidity, and profitability, among many others.[20] Assuming that investors anticipate these real-side heterogeneous responses, then equity price reactions to monetary policy will also vary with these characteristics. Indeed, in empirical analyses of U.S. data, firms' equity price responses to Fed monetary policy surprises depend on many of these same characteristics (Ozdagli, 2018; Ippolito et al., 2018; Ozdagli and Velikov, 2020; Gürkaynak et al., 2022; Döttling and Ratnovski, 2023). Given the substantial amount of empirical evidence on the heterogeneous transmission of monetary policy, two questions naturally arise in the context of our analysis: First, how does the impact of the ECB's monetary policy on individual stock returns depend on a wider set of characteristics of euro-area firms? Second, does the differential response of green and brown firms that we document represent a separate dimension of heterogeneity, or is it a reflection of other firm characteristics?

In addressing the first question, we note that the analysis of the monetary transmission to individual companies in Europe is of independent interest. Existing studies on the firm-level stock market effects of monetary policy have focused on the United States, and to the best of our knowledge there is no established evidence of the heterogeneous effects of ECB monetary policy on stock prices across European companies. To fill this gap, we consider a range of commonly used firm characteristics, and document how they affect the sensitivity of stock prices to ECB policy surprises. We consider most of the firm-level variables that various U.S. studies have identified for the monetary policy sensitivity of firms' equity returns (e.g., Ozdagli, 2018; Ippolito et al., 2018; Döttling and Ratnovski, 2023). For each of these firm characteristics, z_{it}, we estimate the following regression, where time t denotes ECB announcement events:

$$r_{it} = \delta_1 \, mps_t \, z_{it} + \delta_2 \, z_{it} + \gamma' X_{it} + \alpha_i + \alpha_{st} + \varepsilon_{it}. \tag{5.3}$$

[20] For example, early research suggested that a monetary policy tightening had heterogeneous effects on small and large firms as the former faced greater informational asymmetries in accessing external finance (e.g., Oliner and Rudebusch, 1996a,b). Most recently, differing firm-level responses have been documented in the macro-finance literature (e.g., Cloyne et al., 2023; Ottonello and Winberry, 2020; Jeenas, 2024; Jungherr et al., 2024).

This panel regression for stock returns resembles equation (5.2) except z_{it} takes the place of g_{it}. The significance and size of the δ_1 coefficient will indicate the importance of various firm characteristics for driving heterogeneous effects. The variables z_{it} are standardized, so that δ_1 can be interpreted as the change in the stock price sensitivity to ECB surprises for a one standard deviation increase in the firm characteristic. As in Section 5.2, we include additional firm-level controls, X_{it}, firm fixed effects, α_i, and sector-by-time fixed effects, α_{st}, with annual accounting and environmental variables measured with a two-year lag.

Table 5.6: Firm characteristics and stock price effects of ECB policy surprises

Interactions	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Size	−1.88									−1.82
	(1.33)									(1.15)
B/M		−0.70**								−0.84**
		(0.28)								(0.37)
Leverage			−0.65**							−0.49*
			(0.33)							(0.29)
Profitability				0.99**						0.72***
				(0.50)						(0.26)
Tangibility					0.32					
					(0.59)					
Liquidity						−0.10				−0.32
						(0.59)				(0.64)
Age							0.08			0.61*
							(0.43)			(0.34)
Beta								−2.64***		−2.35**
								(1.00)		(0.94)
Cash flow									0.03	−0.44
									(3.86)	(2.95)
Observations	55,730	55,730	55,730	55,730	39,323	55,710	54,753	55,616	55,730	54,619
Adjusted R^2	0.30	0.30	0.30	0.30	0.32	0.30	0.31	0.30	0.30	0.31

Regressions of daily stock returns on monetary policy surprises interacted with different firm characteristics. Additional controls include size, market-to-book equity, leverage, profitability, sales growth, investment, and log PP&E. The regressions also include firm and time-by-industry fixed effects. The sample period is from January 2012 to October 2023 and includes 107 ECB announcements. The policy surprise is the change in the one-year OIS rate over the ECB monetary event window. Standard errors clustered by firms and announcements are reported in parentheses. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

Table 5.6 shows estimates of regression (5.3) for different firm characteristics across each column, with the right-most column including all characteristics jointly.[21] Several firm characteristics significantly influence stock price sensitivity to ECB surprises. We now briefly consider each firm characteristic in turn.

The size of a firm could affect its sensitivity to monetary policy, because small firms are often younger and more financially constrained; indeed, size is often used as a proxy for the degree of borrowing information asymmetry faced by a firm. If smaller firms rely less on external financing, then they could be less sensitive to monetary policy. Ozdagli (2018) notes that earlier evidence for the U.S. is mixed on the effect of firm size in this context, but he finds that the stocks of large firms respond significantly more strongly

[21]The last column omits tangibility, which is not available for many of the firms in our sample.

to high-frequency Fed policy surprises than those of small firms—consistent with less borrowing constraints. For ECB surprises, we do not find a significant difference in the impact on small and large firms, using log market value as a measure of size following earlier literature. Nevertheless, interaction coefficient in column 1 of Table 5.6 has a negative sign, qualitatively consistent with Ozdagli's findings for the U.S.

A common measure of financial constraints is the leverage of a firm, usually defined as total debt divided by total assets, or book leverage. More financially constrained firms have less access to external financing and lower leverage, so their costs and profits are less sensitive to changes in interest rates. Consistent with this story, Ozdagli (2018) and Havrylchyk and Pourabbasvafa (2023) find that stocks of high-leverage U.S. firms respond more strongly to monetary policy surprises than those of firms with low leverage, and Anderson and Cesa-Bianchi (2024) show that the credit spreads of firms with high leverage respond more strongly to monetary policy. Our estimates in column (3) of Table 5.6 go in this direction as well: European firms with high leverage exhibit a more negative response to ECB policy surprises than the average firm.[22]

Young firms tend to have more constrained access to financing than older, more established firms. Firm age has often been used to proxy for the effects of financial frictions, but the empirical results are often not clear cut. Cloyne et al. (2023) find that the investment of young firms is more sensitive to exogenous changes in interest rates, and Havrylchyk and Pourabbasvafa (2023) find the stock prices of young firms respond more to monetary policy. By contrast, the estimates of Patozi (2024) suggest that the stock prices of old firms are more sensitive to monetary policy surprises. We estimate a positive coefficient on the interaction between policy surprises and age (measured as the time since a firm's IPO date). The coefficient is only marginally significant and only in the multivariate regression with all interaction effects. The positive sign of the coefficient implies that the stock prices of older firms may be somewhat less sensitive to monetary policy than for the average firm, in line with the findings of Havrylchyk and Pourabbasvafa (2023).

The duration of a firm's cash flows should be an important determinant of the impact of monetary policy on firm value, because the present value of longer-duration cash flows are more sensitive to changes in interest rates than shorter durations. Indeed, Ozdagli (2018) finds that U.S. firms with longer cash flow duration are more sensitive to policy surprises. A related dimension of heterogeneity is the difference between value and growth stocks according to valuation multiples like price-earnings or book-to-market ratios, since growth stocks tend to have longer-duration cash flows (Lettau and Wachter, 2007). The evidence of Offner (2025) suggests that U.S. growth firms are more sensitive to monetary policy than value firms. By contrast, Havrylchyk and Pourabbasvafa (2023) and Benchora et al. (2023) find that value stocks, with high book-to-market ratios or low Tobin's q, are more sensitive to monetary policy. The differences between these two sets of results is likely due to the different sample periods. Our estimates show that in the euro area, value stocks are in fact significantly more interest-rate sensitive, as shown in columns 2 and 10 of Table 5.6.

The sensitivity to movements in the overall stock market, as captured by its CAPM-beta, is likely the most important and prominent characteristic of a stock, given its central role in empirical asset pricing. Because the stock market responds strongly to changes in monetary policy, as shown by Bernanke and Kuttner (2005) and many

[22]Ozdagli (2018) notes that book leverage is a slow-moving, noisy measure of financial constraints, so given likely attenuation bias, the true effects of leverage may be even larger than our estimates.

later studies, one would expect the sensitivity of individual stocks to depend positively on their market beta. Bernanke and Kuttner (2005) confirmed this prediction using industry portfolios. While Ozdagli (2018) did not find a significant difference based on CAPM-implied stock sensitivites, Benchora et al. (2023) estimated a higher monetary policy sensitivity for high-beta stocks. Using a five-year rolling beta from Refinitiv for our euro-area sample, we also find that the price response of a stock to monetary policy depends positively on its market beta, as shown in columns 8 and 10 of Table 5.6.

Finally, our estimates show that profitability, which we measure as the return-on-assets (gross profit divided by total assets), significantly affects the stock price response to ECB monetary policy surprises. The sign of the estimated effect is consistent with the results of Ozdagli and Velikov (2020) and Benchora et al. (2023), who also find more profitable firms to be less sensitive to monetary policy.

We also considered several other variables that earlier papers have found to be relevant determinants of firm-level sensitivities to monetary policy: tangibility of the capital stock (Döttling and Ratnovski, 2023; Havrylchyk and Pourabbasvafa, 2023), liquidity (Ozdagli and Velikov, 2020; Jeenas, 2024), and cash flow (Ozdagli, 2018).[23] In our sample of euro-area stocks, none of the interaction effects of these variables with the ECB policy surprise end up as statistically significant. We have also investigated alternative specifications and measured the firm characteristics using categorical variables, and found our estimates to be robust.

Having documented the heterogeneous stock market effects of ECB policy surprises, we now turn to the second question: Is the differential monetary policy sensitivity of green and brown stocks due to systematic differences in the characteristics of low- and high-carbon firms? That is, do the differences in policy sensitivity that we have documented in Section 5.2 simply reflect an underlying firm-level heterogeneity that affects the transmission of monetary policy, or is it an separate, independent dimension of heterogeneity, and a new result in its own right?

Table 5.7 compares the firm-level characteristics of green and brown stocks in 2021 (though we obtain similar results using full sample averages). Brown firms had significantly higher leverage and tangibility and lower profitability and liquidity. Within industries, brown stocks tend to have higher book-to-market value, that is, they tend to be value stocks. Brown firms were also older than green firms. Some of these systematic differences could potentially explain the stronger sensitivity of brown stocks to monetary policy, because we found many of these characteristics to be associated with stronger impact of ECB policy surprises, in particular, leverage, profitability, and book-to-market.

To address this issue, we control for other dimensions of heterogeneity in our event-study regressions with emission intensities. Specifically, we estimate the regression

$$r_{it} = \beta_2\, mps_t\, g_{it} + \beta_3\, g_{it} + \delta_1\, mps_t\, z_{it} + \delta_2\, z_{it} + \gamma' X_{it} + \alpha_i + \alpha_{st} + \varepsilon_{it}, \tag{5.4}$$

where g_{it} is emission intensity (so low values of g_{it} indicate greener firms). The key question is again whether the coefficient on the interaction with greenness, β_2, is also significantly negative, as in our results in Section 5.2, once we control for the various

[23] We calculate tangibility as the ratio of tangible assets (PP&E) to the sum of tangible and intangible assets, following Havrylchyk and Pourabbasvafa (2023). For liquidity, we use the ratio of cash holding and short-term investments to total assets. We measure cash flow as the ratio of operating income to total assets.

Table 5.7: Mean firm characteristics for green and brown stocks

	Overall			Within-industry		
	Brown	Green	t-statistic	Brown	Green	t-statistic
Size	21.50	21.51	-0.10	21.49	21.52	-0.22
B/M	0.77	0.74	0.36	0.88	0.63	2.79
Leverage	0.31	0.23	5.82	0.30	0.24	4.15
Profitability	3.71	13.05	-3.86	5.46	10.6	-2.13
Tangibility	0.83	0.70	6.38	0.83	0.70	6.37
Liquidity	0.13	0.16	-3.40	0.14	0.16	-2.47
Age	22.75	18.65	3.43	21.85	19.56	1.90
Beta	0.96	1.02	-2.11	1.01	0.97	0.96
Cash flow	5.25	5.91	-1.23	4.95	6.20	-2.33

Mean values of firm characteristics for green and brown stocks in 2021. Green stocks are defined as those with emission intensities below the median, while brown stocks have emission intensities above the median. The last three columns report the comparison based on a separate grouping within each industry. We report t-statistics for differences in means between green and brown stocks. The sample is a cross section of 815 firms.

firm-level characteristics in z_{it} either individually or jointly. As before, g_{it} and z_{it} are standardized so that the coefficients β_2 and δ_1 capture the change in policy sensitivity for a one standard deviation increase in each variable.

The estimates in Table 5.8 show that the differential sensitivity of green and brown stocks appears to be largely unrelated to other sources of heterogeneity in the stock market response of European firms to ECB surprises. This is evident from the fact that across all specifications, the estimates of the interaction coefficient with emission intensity, β_2, shown in the first row, remains negative and strongly statistically significant. Even the magnitude of this coefficient is barely affected by the additional interaction terms, suggesting that these other controls for firm-level characteristics bear little relation with the heterogeneity that is the focus of our paper.

Our European result in this regard is broadly consistent with the U.S. studies of the monetary policy responses of green and brown stocks (Döttling and Lam, 2024; Havrylchyk and Pourabbasvafa, 2023; Benchora et al., 2023; Patozi, 2024). These studies also control for a variety of potential determinants of stock price sensitivity, and although there are differences in the controls used and the exact results obtained, across all four U.S. studies the estimates of the heterogeneous effects of monetary policy associated with greenness remains largely robust.[24]

Overall, we document a significant amount of heterogeneity in the responsiveness of euro-area stocks to ECB policy surprises. Our novel evidence is generally consistent with earlier findings for the U.S., although there are also some noticeable differences, as would be expected given our new sample of European firm-level data. Importantly, the differential sensitivity of green and brown stocks to ECB monetary policy is a robust and independent dimension of heterogeneity which is not explained by systematic differences in other firm characteristics.

[24] One exception are the results shown in Table 2 of Havrylchyk and Pourabbasvafa (2023), where the interaction between the monetary policy surprise and emission intensity becomes statistically insignificant once all interacted controls are added to the regression.

Table 5.8: Green-brown heterogeneity and controlling for firm characteristics

Interactions	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Emission intensity	-2.24^{***}	-2.20^{***}	-2.18^{***}	-2.21^{***}	-2.28^{***}	-2.21^{***}	-2.22^{***}	-2.10^{***}	-2.22^{***}	-2.12^{***}
	(0.36)	(0.33)	(0.33)	(0.32)	(0.41)	(0.33)	(0.32)	(0.32)	(0.32)	(0.50)
Size	−0.69									−0.89
	(1.32)									(1.23)
B/M		-0.84^{***}								-0.91^{**}
		(0.29)								(0.46)
Leverage			-0.62^{*}							-0.48^{*}
			(0.34)							(0.28)
Profitability				1.82						−0.89
				(3.11)						(2.32)
Tangibility					0.26					
					(0.75)					
Liquidity						0.70				0.50
						(0.79)				(0.79)
Age							0.42			0.66^{**}
							(0.34)			(0.33)
Beta								-1.61^{**}		-1.52^{**}
								(0.64)		(0.74)
Cash flow									0.09	−2.94
									(5.17)	(4.33)
Observations	30,504	30,504	30,504	30,504	23,029	30,492	29,869	30,481	30,504	29,834
Adjusted R^2	0.37	0.37	0.37	0.37	0.40	0.37	0.38	0.37	0.37	0.38

Regressions of daily stock returns on monetary policy surprises interacted with different firm characteristics. Additional controls include size, market-to-book equity, leverage, profitability, sales growth, investment, and log PP&E. The regressions also include firm and time-by-industry fixed effects. The sample period is from January 2012 to October 2023 and includes 107 ECB announcements. The policy surprise is the change in the one-year OIS rate over the ECB monetary event window. Standard errors clustered by firms and announcements are reported in parentheses. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

5.4 Possible channels for green-brown heterogeneity

Our euro area analysis, along with related U.S. research, shows that tighter monetary policy reduces the equity prices of brown firms more than green ones. Here, we consider a variety of potential channels that could explain this difference in interest rate sensitivities.

A first possible explanation for green/brown differential interest-rate sensitivity is based on a "credit channel" that posits differences in the importance and availability of external financing between green and brown firms. As an example, Havrylchyk and Pourabbasvafa (2023) argue that brown firms have more tangible capital and are therefore more sensitive to changes in interest rates. This rationale is based on earlier evidence showing that firms with a greater share of tangible capital exhibit a stronger stock market reaction to changes in monetary policy (Döttling and Ratnovski, 2023). In theory, the more transparent collateral value of tangible capital can reduce lending informational asymmetries and allow a greater reliance on low-cost but interest-rate sensitive external finance. In their U.S. sample, Havrylchyk and Pourabbasvafa (2023) find that green firms appear to hold a greater share of capital in the form of knowledge and organizational intangible capital, while brown firms own more tangible capital such as property, plant, and equipment. After controlling for firms' capital tangibility, Havrylchyk and Pourabbasvafa (2023) find little difference in green and brown firms' reactions to monetary policy. In our European data sample, we also find that brown firms hold a significantly greater share of tangible capital (Table 5.7), but there is no evidence that capital tangibility even on its own accounts for any heterogeneity in firm-level monetary policy sensitivity (Table 5.6). In contrast, there are other firm characteristics—including leverage and profitability—that do plausibly capture a heterogeneous credit channel reliance on external finance and seem to influence the size of firm-level monetary policy surprise responses (Table 5.6). However, controlling for various firm characteristics that capture the reliance on external finance does not change the result that brown stocks respond more to ECB policy surprises (Table 5.8). In other words, we do not find evidence in support of the credit channel.

Two related channels are based on the role of a carbon premium, that is, higher expected returns for brown stocks. One of them involves the amount of transition risk. Brown firm are exposed to greater financial, reputational, and regulatory risks associated with business models that depend on carbon emissions. Investors in brown firms would therefore be expected to require compensation in the form of higher expected returns for holding additional climate transition risk, resulting in a "carbon risk premium" (e.g., Bolton and Kacperczyk, 2021; Pastor et al., 2022; Bauer et al., 2022; Bolton et al., 2024). A carbon risk premium channel could imply a stronger sensitivity of brown stocks to monetary policy shocks. One mechanism works directly through changes in the amount of carbon risk. Döttling and Lam (2024) provide a theoretical framework in which brown firms are more sensitive to monetary policy exactly because they are more exposed to carbon transition risk: Tighter monetary policy increases the cost of replacing carbon-intensive assets, so brown firms delay transitioning and retain a greater exposure to carbon transition risk, which depresses their stock prices. Alternatively, rather than focus on the quantity of transition risk, another approach considers variation in the price of risk. There is ample theoretical and empirical research for a risk-taking channel of monetary policy, in which tighter policy raises effective risk aversion, the price of risk, and thus risk premia across the board (Bauer et al., 2023). The resulting increase in the carbon risk premium would tend to push down the prices of brown stocks—in order to produce higher expected returns—more than those of green stocks. Benchora et al. (2023) also argue that risk premia are known to depend on the stance of monetary policy as investors search for yield when risk-free rates are low. Such a climate risk-taking channel for monetary policy is given some support by Altavilla et al. (2024), who use granular loan-level data to show that the bank lending risk premium rises more for high emitters relative to firms committed to decarbonization following an unexpected increase in policy rates.

Another channel that is related to the carbon premium and could explain the lower interest-rate sensitivity of green stocks centers around investor preferences. As laid out by the asset-pricing model of Pastor et al. (2021), preferences for green securities by the representative investor would tend to lower brown firm valuations and raise their expected equity returns, resulting in a "carbon aversion premium." The key question is how these green preferences might affect the sensitivity of green and brown assets to shocks. Benchora et al. (2023) and Patozi (2024) provide theoretical models in which the investors who derive a non-pecuniary benefit from holding green assets are reluctant to substitute away from green stocks; thus, their demand for these stocks is relatively less sensitive to changes in interest rates. Under the assumption that this channel is relevant for asset pricing by the marginal investor, the green preferences then lead to a lower sensitivity of green assets to monetary policy shocks. Benchora et al. (2023) and Patozi (2024) provide some supporting evidence for this rationale by finding a correlation between the green/brown monetary policy response heterogeneity and some geographical or temporal dispersion in climate concerns.[25] We don't provide direct evidence on this potential channel, but our use of data from Europe—where surveys find that concerns about climate risk are much less contentious and more widely established—may provide a useful contrast to the United States.[26] In this regard, despite the ostensibly greater acceptance of incorporating climate risk into financial investment decisions in Europe, our results on green/brown monetary policy heterogeneity are somewhat surprisingly not stronger than in the United States. More analysis comparing the behavior of European and American investors may improve our understanding of this potential channel.

While intuitively appealing and potentially promising, explanations based on the carbon premium face the challenge of a lack of empirical consensus on this issue, given the contradictory results about the relative performance of green and brown stocks. Some empirical studies find that green stocks have had lower returns thus confirming a carbon premium, notably, Bolton and Kacperczyk (2021, 2023c); Delmas et al. (2015); Busch et al. (2022); Görgen et al. (2020) and Bansal et al. (2021). In contrast, other studies have documented substantially positive historical returns for portfolios that go long in green stocks and short in brown stocks (Garvey et al., 2018; In et al., 2019; Huij et al., 2024; Pastor et al., 2022; Bauer et al., 2022; Zhang, 2024). This seemingly conflicting evidence could reflect the fact that realized green returns can differ from expected returns for a period of time if risk perceptions or preferences shift unexpectedly over time (Bauer et al., 2022). In fact, Pastor et al. (2022) and Ardia et al. (2023) provide evidence that green stocks may have had higher realized returns than brown stocks because of increased concerns about climate change, but similar or even lower expected returns. Lontzek et al. (2022) show that a sequence of large negative climate shocks can raise perceived climate risks, and during the transitional period growth in demand for sustainable investments could account for the empirical evidence on high green realized returns, meaning that there was a carbon premium in expected returns while at the same time green stocks had temporarily higher realized returns. Eskildsen et al. (2024) use direct estimates of expected returns and provide evidence that they were higher for brown stocks than for green stocks. Bauer et al. (2024a) show that firm announcements of decarbonization commitments raise their stock prices, consistent with a decline in the carbon premium for firms that become greener. Overall, a carbon risk premium channel may be a promising explanation for green/brown monetary policy heterogeneity, but future research is needed to provide further support.

A fourth possible explanation is a "demand channel" based on the differential interest-rate sensitivity of the demand for the products of green and brown firms.

[25] Patozi (2024) finds that the heterogeneous monetary policy response is more pronounced for green firms held by index funds that are located in U.S. counties where climate risk perceptions are stronger and during times of heightened climate concerns. Benchora et al. (2023) find that the green/brown heterogeneity varies over time with the overall level of climate awareness as proxied by newspaper coverage of climate change related events.

[26] An international survey of climate opinions was described in the press as finding that "The US is a hotbed of climate science denial when compared with other countries, with international polling finding a significant number of Americans do not believe human-driven climate change is occurring." (Milman and Harvey, 2019).

Some sectors sell products that have more cyclical demand, with greater sensitivity to changes in interest rates.[27] Such industry differences could matter for cross-industry heterogeneity of green and brown firms in their response to monetary policy. Havrylchyk and Pourabbasvafa (2023) argue that the unique characteristics of fossil fuel industries are part of the explanation for the green/brown policy heterogeneity in the United States. While these high-emitting industries indeed tend to be very interest-rate sensitive, they are much less relevant in the euro area and do not help explain our results.[28] More broadly, however, the stronger monetary policy sensitivity of browner industries may well be related to their greater cyclicality and interest-rate sensitivity. Furthermore, differential firm-level interest rate sensitivity could also be present *within* industries, with products of brown firms being more cyclical and more responsive to interest rates. This could result from a short-term orientation of brown businesses. Consistent with this view, evidence from Table 5.7 and Pastor et al. (2022) suggests that brown stocks tend to be value stocks, which typically have shorter cash-flow duration.[29] By contrast, green stock prices may reflect a longer time horizon that emphasizes positive future growth opportunities as demand for sustainable products likely has an upward trend and sustainable business models may be more stable. Indeed, earlier research shows that sustainable companies tend to be safer and more insulated from economic downturns (Eccles et al., 2014; Jagannathan et al., 2018). Overall, these arguments suggest that brown stocks might be more sensitive to monetary policy tightening because investors expect their business outlook to be more sensitive to changes in interest rates and cyclical fluctuations than for green firms.

We have argued that there is some empirical support for both a carbon premium channel and a demand channel for a differential sensitivity of green and brown stocks to monetary policy. But more research is clearly needed to get a clearer picture of the relative importance of the different channels, and to disentangle carbon premium effects coming from changes in risk premia and the role of green investor preferences.

5.5 Examination of the renewable energy sector

It is useful to return to the narrower debate described in the introduction about whether recent higher interest rates may have disproportionally slowed renewable energy investment. The view that higher interest rates slow down the green transition is typically focused on the renewable energy sector, rather than on the broad, whole-economy heterogeneity across green and brown firms that our paper and earlier U.S. studies explore. The fact that green and brown firms selling a range of consumer and business goods and services are affected differently by monetary policy is of course relevant to the green transition. But a closely related issue is how the transition from fossil-fuel to renewable energy sources is affected by changes in interest rates and monetary policy. The role of higher interest rates in potentially slowing the adoption of

[27] Eijffinger et al. (2017) show that cyclical industries are more responsive to monetary policy. Petersen and Strongin (1996) and Willis and Cao (2015) document changes in interest-rate sensitivity across industries and over time.

[28] We found that removing the 20 firms in our sample that are categorized in the Oil, Gas, Coal, Mining, and Metal industries (SIC 10, 12, 13, 14, and 33) actually strengthens the estimated green/brown heterogeneity. We omit the results for the sake of brevity.

[29] While we control for market-to-book ratios in Table 5.8, it is possible that brown stocks are more short-term oriented and thus more sensitive to near-term demand fluctuations in ways not captured by stock valuation multiples.

renewable energy and thus the green transition has been widely discussed among policy makers, policy analysts, researchers, and market strategists (e.g., Schnabel, 2023; Egli et al., 2018; Schmidt et al., 2019; Kleintop, 2023). Indeed, the common view that higher interest rates will particularly hinder investment in renewables has prompted proposals—especially in Europe—for dual interest rates with a lower green interest rate for sustainable initiatives (Jourdan et al., 2024). For example, one proposal is that the ECB could set up a green targeted lending operation that would provide banks with a lower interest rate that incentivizes green loans.[30]

To better understand this issue, we investigate whether the interest rate effect on solar and wind energy investment and profitability outweighs the effect on oil and gas companies. Renewable energy facilities are the linchpin of any decarbonization pathway, and they require substantial initial construction financing but incur very low future operating costs (e.g., zero fuel costs). For such investments, higher interest rates should have substantial negative effects as future cash flows are discounted more heavily. Indeed, research with sectoral energy models shows that higher discount rates put low-carbon energy sources, with their very long cash flow duration, at a significant cost disadvantage relative to fossil-fuel sources (e.g., Schmidt et al., 2019; International Energy Agency, 2020; Bistline et al., 2023). That said, engineering cost comparisons do not necessarily translate into profitability much less equity pricing.[31]

To focus our investigation on the renewable and fossil fuel energy sectors, we examine the monetary policy response heterogeneity of several prominent stock market indexes representing global energy sectors. The green indexes include Wilderhill Clean Energy, S&P Global Clean Energy, ISE Global Wind Energy, and MAC Global Solar Energy. The brown indexes include the FTSE All World Oil & Gas & Coal, the S&P 500 Integrated Oil & Gas, and the STOXX Europe 600 Oil & Gas Price Index. For each group of indexes, we extract their common factor as the first principal component, which results in a brown factor and a green factor. We also calculate a brown-minus-green factor corresponding to the difference of these two factors, which represents the return on an financial strategy of going long in oil and gas indexes and short in the renewable energy indexes. We use event study regressions to uncover the interest rate sensitivities of these energy sectors using three different measures of ECB policy shocks: the three-month and one-year OIS rate surprises, and the JK surprise from Jarociński and Karadi (2020), which accounts for possible information effects and is generally related more strongly to the stock market, as described in Section 5.1.2.

Table 5.9 shows the event-study regression results. The brown factor appears to be more negatively affected by tighter monetary policy than the green factor. Given the short sample, the estimates are imprecise, and the responses of the brown and green factors tend not to be statistically significant at conventional significance levels. But the responses are significantly different as the negative response of the BMG spread factor is statistically significant at the 1%-level for the three-moth OIS surprise and the JK surprise. These results suggest that for the energy production sector, the fossil fuel firms appear more sensitive to monetary policy than renewable energy firms, providing further confirmation to the broader brown-minus-green findings in Section 5.2.

[30] As French president Emmanuel Macron Macron (2023) advocated: “We must also put private financing and trade at the service of the Paris agreement. The cost of investment must be higher for players in the fossil-fuel sector. We need a green interest rate and a brown interest rate.”

[31] Requirements for storage and dispatchability as well as regulatory and other impediments can insulate the relevant wholesale and retail energy prices from the costs of green or brown power generation (Hirth and Steckel, 2016).

Table 5.9: Effects of monetary policy surprises on energy sector indexes

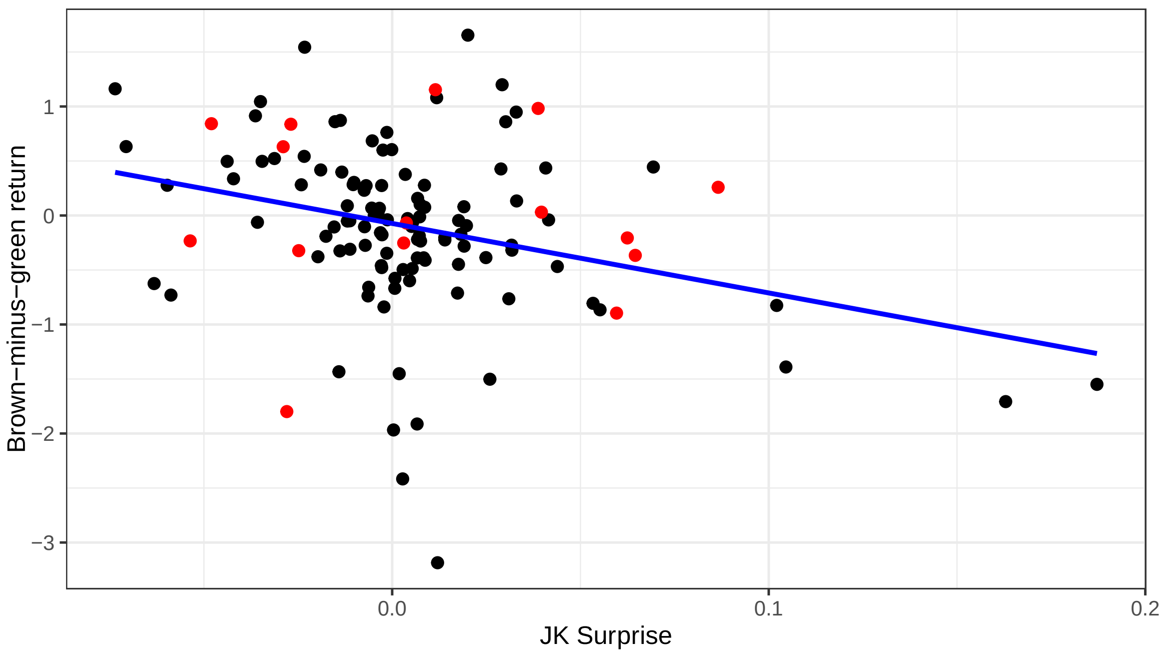
	OIS 3m			OIS 1y			JK		
	Brown	Green	BMG	Brown	Green	BMG	Brown	Green	BMG
mps	−5.47	−0.79	−4.68***	−3.62	−0.88	−2.74	−13.12*	−6.74	−6.38***
	(6.52)	(5.78)	(1.79)	(3.51)	(3.04)	(1.88)	(7.66)	(7.42)	(1.51)
R^2	0.02	−0.01	0.03	0.01	−0.01	0.01	0.23	0.06	0.09

Regressions of first principal components of selected green and brown indexes on alternative monetary policy surprises (described in the text). The sample period from July 2010 to October 2023 includes 124 ECB announcements. Standard errors are reported in parentheses. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

Finally, we should note that the policy concerns of Schnabel (2023) and many others were prompted by worries specifically about the adverse effects of the tightening of monetary policy in 2022 and 2023. That episode also followed a long period during our sample of near-zero interest rates, so it is interesting to examine whether those recent observations are outliers compared to our full sample results. As we noted regarding Figure 5.1, which plots the economy-wide brown-minus-green return against monetary policy surprises, the recent sample of observations does not provide any evidence that the latest monetary policy tightening had a different stock market response. This conclusion also holds for the energy sector. Figure 5.2 shows a scatterplot of the brown-minus-green energy portfolio—narrowly focused on fossil fuel versus renewables equity returns—against the JK monetary policy surprise on ECB announcement days (Jarociński and Karadi, 2020). Again, the ECB announcements in 2022 and 2023 shown in red appear consistent with the rest of the sample.

Our evidence shows that stocks in the renewable energy sector are actually less affected by rising rates from monetary policy compared to fossil fuel firms is at odds with the received wisdom on the adverse effects of interest rates on renewables capital investment cited above. Could these findings be reconciled? Specifically, could a firm curtail capital spending on renewables in response to higher interest rates and yet at the same time not see a decline in stock price? Typically, much macro-finance research assumes a fairly close relationship between equity valuations and real investment opportunities and spending. Indeed, such a connection is formalized in the Q theory of investment, which relates a firm's investment to the ratio of its market value to its replacement cost (Tobin's Q, which is closely related to the firm's market-to-book ratio). However, researchers have generally found little support for this theory in the data, with potential sources for this failure including the existence of financial constraints, decreasing returns to scale, and measurement problems, among other things (e.g., Oliner et al., 1995; Bond and Van Reenen, 2007). Beyond a possible disconnect between equity valuations and investment spending, another way to reconcile our results could be the higher tendency for brown firms to undertake green investments, including renewable energy technologies. As these investments have longer duration and would be disproportionally affected by monetary policy, brown firms would be more policy-sensitive in this case. Some evidence in Döttling and Lam (2024) indeed points in that direction. Relatedly, Fornaro et al. (2024) document that U.S. firms with more green patents reduce their investment more strongly in response to mon-

Figure 5.2: Reaction of brown-minus-green energy portfolio to ECB surprises



The figure plots the returns on a brown-minus-green energy portfolio—narrowly focused on fossil fuel versus renewables—against the JK monetary policy surprise on ECB announcement days. Brown-minus-green returns are constructed as for the regressions in Table 5.9, using the difference between the first principal component of selected brown and green energy indexes. The sample period from July 2010 to October 2023 includes 124 ECB announcements. ECB announcements in 2022 and 2023 are shown in red.

etary contractions than other firms, and it is plausible that these firms are actually high-carbon firms, like energy companies that often play a big role in the development and adoption of new green technologies. The adverse effects of higher interest rates on renewables investment are an important topic for future research.

5.6 Conclusion

The debate in Europe about the effect of higher interest rates on the green transition motivated our investigation about whether equity prices of firms with a lower carbon intensity exhibit a greater response to monetary policy. Our analysis using high-frequency ECB policy surprises uncovered three main results: First, euro-area green, low-carbon stocks appear significantly *less* affected by monetary policy surprises to interest rates than brown,higher-carbon stocks, and this pattern is robust across various greenness metrics, policy surprises, and empirical specifications. Second, these heterogeneous effects are not explained by differences in other firm-level characteristics such as leverage, capital tangibility, or market beta. Third, focusing on narrower energy sector indexes, we find that the interest rate sensitivity of renewable energy stocks is *weaker* than that of oil & gas stocks, consistent with our firm-level results for the entire euro area stock market. These conclusions are in broad agreement with recent research using U.S. data despite notable European differences from the United States in terms of climate and non-climate regulatory and fiscal policies, industry composition, and political, societal, and investor attitudes towards climate change and climate investing.

There are several possible theoretical channels that could explain the differential policy sensitivity of green and brown stocks. However, neither differences in observable firm characteristics (such as the amount of tangible capital) nor industry composition effects appear to account for our euro-area results. Instead, a carbon premium, while not firmly established in the empirical literature, is a promising candidate explanation for the differential green/brown sensitivity. Both possible contributors to the carbon premium—a carbon risk premium driven by differences in transition risk and a carbon aversion premium driven by green investor preferences—may reduce the impact of monetary policy shocks, and possibly other shocks, on green asset prices. Another potential explanation is a demand channel, according to which the product demand for green firms is less cyclical and less interest-sensitive than for brown firms. While both the carbon premium and the demand channels likely contribute to the differential monetary policy sensitivity of green and brown stocks, more work is needed to supply definitive supporting evidence and to sharpen our theoretical understanding of these mechanisms.

The results in our paper and recent U.S. research seem at odds with the view that higher interest rates unduly limit renewable energy production and may endanger the green transition. While a comprehensive reconciliation of earlier work with the evidence based on monetary policy surprises is beyond the scope of this paper, it may be that methodological differences are responsible for the conflicting conclusions. In our view, using exogenous rate changes as measured by high-frequency monetary policy surprises is one of the most reliable approaches for causal inference about the effects of changes in interest rates on economic and financial conditions. While further research is required, the growing body of evidence on the green/brown firm-level effects of monetary policy is starting to call into question some prevailing views about the relationship between interest rates and efforts to decarbonize our economies.

Our study has focused on the differential stock market effects of monetary policy surprises across firms–a methodology with the advantage of sharp econometric identification. Equity valuations reflect investors' forward-looking views of green and brown business prospects, but a crucial companion question is how does monetary policy actually affect future firm-level decisions and outcomes—including investment spending, profits, and GHG emissions. Understanding these real-side consequences would help clarify the heterogeneous effects of monetary policy on corporations and on the green transition. Using U.S. data, Fornaro et al. (2024) find that investment by green firms declines more strongly than by brown firms in response to monetary contractions, while Döttling and Lam (2024) report that brown firms reduce their emissions by more than green firms with tighter monetary policy.

Our evidence contributes to a better understanding of the climate-related issues of monetary transmission. While calling into question the prevailing view that tighter monetary policy necessarily slows the green transition, it certainly does not imply that central bank actions have no climate-related consequences; for example, higher policy rates do reduce green equity prices, albeit less than brown equity prices. But more research is clearly needed on various important outstanding issues—including the theoretical rationale, the reconciliation with the earlier consensus, and the real-side investment implications—for a more complete understanding of the climate-related implications and side effects of monetary policy.

Chapter 6

Corporate Green Pledges

Abstract

We identify announcements of corporate commitments for reductions of greenhouse gas emissions—green pledges—from news articles using a large language model. About 8% of publicly traded U.S. companies have made green pledges, and these companies tend to be larger and browner than those without pledges. Announcements of green pledges significantly and persistently raise stock prices, consistent with reductions in the carbon premium. Firms that make green pledges subsequently reduce their CO_2 emissions. Our evidence suggests that green pledges are credible, have material new information for investors, and can reduce perceived transition risk.

6.1 Introduction

As the world transitions towards a low-carbon economy, firms are challenged with the adaption of their business models to succeed in an evolving technological, regulatory and political environment. They face significant transition risks—including liability and reputational risks—as a result of the shift towards a low-carbon economy that will impact their cash flows, profits, and overall long-term prospects.[1] But companies can manage transition risks: By reducing carbon emissions, they can lessen the future impact of carbon pricing regulations and technological breakthroughs that render carbon-intensive businesses costly and ultimately irrelevant. Indeed, more and more corporations all over the world have announced plans and commitments to lower their carbon emissions.[2] Because financial markets are forward-looking, credible commitments to decarbonize in the future could immediately raise company values: As lenders and investors perceive lower transition risks, the required risk premium and cost of capital decline, with a positive impact on the firm's valuation. On the other hand, decarbonization requires potentially large investments and significant changes to existing business models, which lead to substantial costs and risks affecting future cash flows and profits. It is an open question which of these effects is stronger, and whether the overall effect of such commitments on company values is likely to be positive or negative. This paper addresses this question using a new dataset of time-stamped decarbonization commitments for public U.S. firms, or "corporate green pledges," constructed from a large corpus of news articles with the help of human coding and a large language model. Our event-study evidence shows a positive impact of green pledges on stock market valuations that is statistically and economically significant. Investors appear to view the anticipated benefits of green pledges as outweighing the costs, and the market response may provide additional financial incentives for decarbonization. We also document that firms significantly reduce their emissions following green pledges, indicating that investors have good reasons to view these announcements as credible.

We define a corporate green pledge as a clear, new, actionable commitment by a company to significantly reduce its future greenhouse gas (GHG) emissions. Using textual analysis, we identify the announcements of such green pledges in Dow Jones newswire and newspaper articles from 2005 to 2023. This text data includes corporate press releases, earnings announcements, articles, and other corporate news and announcements. As green pledges are difficult to distinguish from other corporate announcements about GHG emissions, this task would be very challenging for conventional text classification models, requiring a carefully tuned machine-learning model that accounts for semantic information and context, and a very large training sample of labeled articles. We instead classify the articles with a large language model (LLM), which allows us to identify green pledges without a training dataset of manually labeled articles. We use GPT-4 by OpenAI, a model that is well-suited for this complicated classification task because it was trained on vast amounts of textual

[1] See Bolton and Kacperczyk (2021), Pastor et al. (2021), Ilhan et al. (2020) and Krüger et al. (2020).

[2] A 2024 report by S&P global finds that "45% of the leading listed US companies have a net-zero commitment" and that on average companies aim to cut Scope 1+2 emissions by 51%. The "Net Zero Stocktake 2024" reports that "nearly 60% of the 1,977 publicly listed companies we track have set net zero targets" and that the "annual revenue covered by net zero targets has increased from \$13.8 trillion in December 2020 to \$31 trillion in August 2024".

data, has contextual understanding, and can handle linguistic nuances.[3] We classify 44,605 news texts that have topics related to the environment. The successful use of an LLM requires a suitably chosen prompt with accurate and detailed instructions, and a careful evaluation of the results, given the possibility of hallucination, bias, restrictions from content use policies, and other common problems with LLMs. For both, the prompt design and the evaluation, we make extensive use of human coding. In a random subsample of 1000 articles, the classifications from GPT and human coders show a high level of agreement (89%). Using the identified articles and the associated ISINs and time stamps, we derive a dataset of 8,320 unique firm-date combinations. These green pledge events are the basis of our subsequent analysis.

The first contribution of our paper is to document new empirical facts about green pledges of listed U.S. companies. Over our sample period, about 8% of the firms made at least one public commitment to reduce future carbon emissions. Many of those firms made multiple announcements, for example, setting increasingly ambitious goals for future reductions. Towards the end of our sample, there is a clear upward trend in the number of green pledges, consistent with the substantial increase in climate change concerns (Ardia et al., 2023). Furthermore, we find that it is especially large and brown firms (i.e., firms with high emissions or emission intensities) who announce green pledges, both within and across industries. Because large and brown firms are the most relevant for the aggregate transition of the U.S. economy to a low-carbon future, their commitments to reduce emissions are particularly important.

Using the timestamped corporate announcements of green pledges, we can estimate the stock market effects using event studies. Identification of the causal impact is based on the assumption that the corporate announcement of a new decarbonization strategy is predetermined with regard to the company's stock return on the day of the announcement, following a long event-study tradition in empirical asset pricing. Our estimates show that announcements of green pledges have a substantial and persistent positive impact on stock market valuations. The positive effect on the day of the announcement is both statistically and economically significant. On average, a green pledge raises the stock price by 0.14–0.31%, which is a sizable increase when compared to the average daily stock return of 0.015%. Given that our text-based event indicator contains measurement error, these estimates should be viewed as a lower bound for the true effects of green pledges on stock market values. The estimated effects are robust to the choice of firm-level control variables, fixed effects, subsets of green pledge events, and sample period. For example, we find equally strong effects for the periods before and after the 2015 Paris Agreement. Other corporate news, without green pledges, also lead to positive stock returns—in line maybe with a positive effect of attention on stock returns—but the green pledge announcements have significantly larger effects. This placebo test confirms that the identified announcements about future GHG emissions contain important new information for investors. Estimates of dynamic event-study regressions show that green pledges cause immediate and persistent effects on stock market valuations, with little evidence of information leakage before or price reversals after the announcement. The estimated impact of green pledges is heterogeneous across firms, as it is significantly positive only for the largest firms and those with the

[3]Other studies have found that in empirical economic research GPT models often draw similar conclusions as humans, with the advantage of being able to quickly process large amounts of data (Hansen and Kazinnik, 2023; Hansen et al., 2024; Cook et al., 2023; Beckmann et al., 2024; Jha et al., 2024).

highest emissions.

Finally, we investigate whether firms "walk the talk" and actually lower their future CO_2 emissions following a green pledge. There have been ongoing concerns in the public and academic debate about the greenwashing of firms, that is, attempts by firms to convey a false impression about their environmental footprint (Bingler et al., 2022). To address this issue, we estimate difference-in-differences local projections for firm-level CO_2 emissions, using the methodology of Dube et al. (2023). The results show that green pledges are followed by statistically significant reductions in both emission levels and intensities. The reductions are quantitatively meaningful: Emissions of firms that make a green pledge are about 12% lower 5 years after the announcements compared to firms that do not make such a commitment. These estimates partly alleviate concerns about greenwashing, because they confirm that green pledges indeed predict a shift towards decarbonization.

The paper makes three novel empirical contributions to the climate finance literature. First, we provide evidence that decarbonization tends to increase the value of a firm. Positive effects from lower cost of capital and/or improved earnings outlook appear to outweigh the potential negative effects from increased investment requirements and uncertainties. While Hartzmark and Shue (2023) conclude from their analysis that "brown firms face very weak financial incentives to become more green" our results point in a different direction: The possibility of higher valuations provides positive incentives for companies to commit to decarbonization. Second, our results support the carbon premium hypothesis, that is, higher expected returns for brown firms due to investor preferences for green stocks or the higher transition risk of brown stocks (Pastor et al., 2021). If there is a carbon premium, and investors learn that a firm will become greener, its expected return should decline and its stock price increase, consistent with our findings. Our evidence on this issue is particularly relevant because earlier empirical work has so far not established a consensus on the carbon premium (Bauer et al., 2022; Zhang, 2024). Third, we provide evidence that corporate green pledges are generally not cheap talk, given that they predict future greening in the form of lower emissions. Our paper thereby contributes to the discussions on greenwashing and cheap talk in climate commitments (Nemes et al., 2022; Bingler et al., 2022, 2024; Dzieliński et al., 2023; Sastry et al., 2024).

Several recent papers have also investigated climate commitments using data from the Carbon Disclosure Project (CDP) and the Science-Based Target Initiative (SBTi), including Bolton and Kacperczyk (2023b), Aldy et al. (2023, 2024), and Jiang (2024). Our work contributes a news-based identification of green pledges, and the resulting novel database of timestamped announcements of climate commitments allows us to use event studies to estimate the stock market response and the pricing of transition risks.[4] Our results on the prevalence of green pledges differ from the results of Bolton and Kacperczyk (2023b), who find in their annual international dataset that after controlling for industry effects, green firms are more likely to make commitments. They conclude that the aggregate effect of such commitments on global emissions may be rather low, given that large emitters are not yet changing their behavior sufficiently. By contrast, in our sample of U.S. firms, we find that large and brown firms, both within and across industries, are more likely to make commitments. Acharya et al. (2024)

[4]Our database is also broader and more comprehensive, since SBTi recorded commitments only starting in 2015. The Net Zero Tracker (`www.zerotracker.net`) only captures a subset of decarbonization commitments, and does not record the announcement dates.

develop a model showing that firm-level climate commitments, in particular by large firms, can enhance the credibility of climate policies such as carbon taxes or subsidies for green innovations. Using corporate commitments from the SBTi, they provide evidence for their model predictions and, in line with our results, document that large firms are more likely to commit to greening. Sastry et al. (2024) study net zero commitments of banks, documenting that such commitments predict decarbonization of bank loan portfolios, but not reductions in credit supply to brown sectors or an increase in financing for renewable projects.

Our paper contributes to the large literature studying the links between climate transition risk and stock returns. Earlier studies have typically measured transition risk exposure using firm-level emissions and estimated the differential stock returns of green and brown firms, with mixed results. Some papers find evidence for a carbon premium, including Bolton and Kacperczyk (2021, 2023c) and Pastor et al. (2022). Other studies do not find evidence for higher returns of brown stocks, or even document green outperformance; see In et al. (2019), Huij et al. (2024), Aswani et al. (2023), Zhang (2024), and Bauer et al. (2022). Empirical analysis of the carbon premium is generally based on past emissions data as a measure for the exposure to transition risks.[5] But this approach has several shortcomings: Emissions data disclosure has largely been voluntary, resulting in potentially severe selection bias. Estimates of emissions from data vendors can also be quite unreliable, as they are highly correlated with measures of firm size and often revised ex post.[6] In general, emissions data provide only backward-looking and slow-moving, noisy measures of transition risk. By contrast, green pledges are forward-looking and capture new information about future transition risk exposure, making them better suited to study the pricing of these risks in financial markets.

A crucial empirical challenge in climate finance is that average past stock returns are not necessarily good measures of expected returns due to the short sample periods and changes in perceptions about climate risk. This problem is illustrated by Atilgan et al. (2023), who incorporate earnings announcements in their analysis and conclude that the carbon premium in their data sample in fact reflects unexpected returns and mispricing. One potential solution to this problem is to rely on estimates of expected returns for green and brown assets, as in Pastor et al. (2022) and Eskildsen et al. (2024). Alternatively, a number of papers have studied brown and green stock returns around specific events with news about climate risk or climate policies (e.g., Engle et al., 2020; Ardia et al., 2023; Bauer et al., 2024b). We choose a different route by focusing on firm-level news about future greenness, which allows us to cleanly identify the impact on stock market valuations and the pricing of transition risks.

Many studies have used textual analysis for measuring climate risks, usually by constructing broad, aggregate measures of climate risks based on for example newspaper articles. For instance, Engle et al. (2020) use news articles from *The Wall Street Journal* to build a climate news index, Ardia et al. (2023) construct a news-based index of climate change concerns, and Faccini et al. (2023) derive several different

[5] Pastor et al. (2022) instead use environmental scores to distinguish green and brown firms. They also find an ex-post green outperformance, although after a model-based adjustment for negative climate news, brown stocks appear to have higher expected returns.

[6] See the critique by Aswani et al. (2023) as well as the reply by Bolton and Kacperczyk (2023a). In fact, Zhang (2024) argues that earlier evidence of a carbon premium appears to be due to forward-looking firm performance information contained in emissions data and vanishes when accounting for publication lags.

climate risk measures from news articles. Only a few studies have used text methods to investigate the pricing of climate risk at the firm level. Sautner et al. (2023a) and Li et al. (2024) use companies' earnings calls to measure firm-level climate change exposure, and Sautner et al. (2023b) document changes in the risk premium associated with such exposure. Dzieliński et al. (2023) investigate the response of stock prices and future GHG emissions to discusssions of climate-related topics in earnings calls, and find reductions in future emissions as evidence, consistent with our results, that firms "walk the climate talk." Bingler et al. (2022) use the pre-trained ClimateBERT model to construct an index which captures the quality of climate-related annual reports of companies. We instead focus on news about future firm-level emissions and thus *changes* in greenness and transition risk.

Our work is in the long tradition of the empirical asset pricing literature that uses event studies to estimate the stock market effects of firm-level news, going back to seminal contributions by Fama et al. (1969), Ball and Brown (1968), and Sloan (1996); see the surveys of MacKinlay (1997) and Kothari and Warner (2007). Closest to our work is an influential paper by Krüger (2015) who studies how stock prices react to positive and negative news regarding a firm's corporate social responsibility (CSR), based on an identification of CSR events from text data. We build on this literature and the event study methodology by investigating the stock market effects of news about CO_2 emissions and corporate plans for a transition to a low-carbon economy.

The remainder of this paper is organized as follows. In Section 6.2 we describe our text data and our approach to identify decarbonization commitments using a large language model. Section 6.3 documents new facts about green pledges, including their variation over time, across industries, and across firms. In Section 6.4 we analyze the stock market reaction to green pledges using event-study regressions, and in Section 6.5 we estimate the response of future emissions using local projections. Section 6.6 concludes.

6.2 Identification of Green Pledges

Our starting point is to define and identify decarbonization commitments of U.S. firms. Our text corpus is a large news data set from Dow Jones consisting of real-time newswire articles, which are monitored closely by financial market participants, and newspaper articles from The Wall Street Journal, Barrons, and MarketWatch. This dataset contains a wide variety of news, including articles written by journalists, press releases, earnings announcements, and many others. Versions of this text dataset have been used in a number of studies in finance and economics.[7] The text data comes with a variety of attributes, including the geographical regions covered by the news, categories of subjects, and the precise timestamps. Additionally, Dow Jones assigns a list of ISINs to each article, which simplifies the process of linking them to firms. Our sample period ranges from January 2005 to December 2023. We refine our selection

[7] Ke et al. (2020) construct a novel sentiment score from this text data, which they use to predict stock returns. Aprigliano et al. (2023) and Barbaglia et al. (2023) have used data from Dow Jones to analyse the gains of using sentiment measures in macroeconomic forecasting. Furthermore, Ravenpack, the leading provider of financial news sentiment data, constructs its sentiment scores based on Dow Jones Newswires. Ravenpack sentiment data has been used on different finance applications, such as forecasting of bond yields and stock returns (Kim, 2022; Audrino and Offner, 2024), and event studies of stock market reactions to media news (Cepoi, 2020).

of articles by including only those related to companies in the United States and categorized as environmental news.[8] The resulting sample includes 44,605 news articles and announcements.

To accurately classify green pledges, we first need a precise definition of a decarbonization commitment or green pledge. We define it as an *announcement of a new, clear, actionable commitment to significantly reduce future direct greenhouse gas (GHG) emissions.* This definition aims to identify articles with new information for investors, so that we can estimate how stock prices respond to expectations of a more environmentally sustainable business model in the future. In short, green pledges should be news. This criterion also applies to updates of existing commitments, which should contain new information or more stringent emission reductions. For example, if a firm announces that it is advancing a carbon neutrality target by reallocating resources towards green energy initiatives, this would be considered a decarbonization commitment. However, news that simply reaffirm or validate prior commitments should not be classified as such. We focus on official news like articles, announcements, and press releases, and exclude informal news like CEO tweets or interviews. Overall, our goal is to ensure that the announcement contains material news for financial market participants and the broader public about the projected future GHG emissions of a company.

With this definition in hand, our goal is to accurately classify the news articles into those that contain corporate green pledges and those that do not. A wide range of text classification methods could be used for this task.[9] However, it would be very challenging for most commonly used methods to accurately identify green pledges, because on the surface they sound similar to other types of corporate announcements that are related to GHG emissions but do not contain any clear decarbonization commitments. To pick up on these nuances and achieve good classification accuracy, an algorithm would have to be flexible enough to account for language semantics and context, and be carefully tuned in the training process. Furthermore, training this text classifier would require a very large amount of pre-labeled text data—certainly many thousand labeled articles—because a complicated classification task with nuanced differences between categories generally requires a large training data set.

We choose a different route in this paper, and instead use a large language model (LLM) to classify the news articles. Specifically, we use GPT-4, one of the most advanced LLMs publicly available at the time of this writing. GPT and similar LLMs have already been successfully used in various application in empirical economic research.[10] Our application requires a suitably chosen prompt with accurate and detailed instructions that are consistent with our definition of a green pledge. In addition, it is necessary to carefully evaluate the results, given the possibility of hallucination, bias,

[8]Specifically, we use articles tagged with at least one U.S. ISIN, the U.S. indicated as the geographical region, and "environment" as the subject classification, using the metadata provided by Dow Jones.

[9]For excellent treatments of text analysis methods and their applications in economics and other social sciences, see Gentzkow et al. (2019), Grimmer et al. (2022), and Ash and Hansen (2023).

[10]Lopez-Lira and Tang (2024) find that GPT-4 can make accurate stock market predictions based on news headlines. Hansen and Kazinnik (2023) use GPT models to classify the monetary policy stance based on language in FOMC statements, and find logical reasoning similar to human coders. Hansen et al. (2024) show that LLMs come to similar economic predictions as professional forecasters. LLMs have been used to evaluate the information in company earnings calls by Cook et al. (2023) and Beckmann et al. (2024), and to evaluate corporate policies by Jha et al. (2024). See Korinek (2023) and Ash and Hansen (2023) for other applications of LLMs in economics.

restrictions from content use policies, and other related problems of LLMs. For both our prompt design and for evaluation of the GPT classifications we make extensive use of human coding. Our approach is similar to that of Eloundou et al. (2024) who also use human annotations and GPT-4 classifications, and finetune a prompt to yield good agreement between both.

The first step of our text classification was to design a suitable prompt using an iterative process. We started with a simple prompt based on our definition of a green pledge, and then refined the prompt in four successive rounds. In each round, we asked the model to classify a subset of articles that were also given to human coders, and compared the human-based and model-based classifications. We also asked for a concise justification for each classification decision, which offers insights into the model's reasoning process and its comprehension of the assignment. Based on these results, we then modified the prompt in each step, typically making the criteria more stringent to avoid false positives. The result of this process was the following final prompt:

> *Classify the following article as positive or negative depending on whether it contains an announcement that the company will reduce its future emissions of greenhouse gases, such as carbon dioxide. Classify an article as positive only if the company announces a significant reduction of direct emissions, that is, emissions that occur from sources controlled or owned by the company. The announcement should be news and should describe the company's commitments and plans for the future. Do not classify articles as positive that only contain announcements to reduce indirect emissions, that is, emissions that a company causes indirectly from the energy it purchases and uses. Also do not classify articles as positive if they are only about past performance, about a corporate social responsability (CSR) report describing past emission reductions, about other environmental measures such as waste reduction, use of recycled paper, or planting trees, or announcements by the government. If an article is empty, or does not contain enough information, classify it as negative. Answer 'YES' for positive articles and 'NO' otherwise.*

The second step was to classify our entire corpus of news articles. The specific GPT-4 model we used for this purpose was `gpt-4-0613`, based on the availability of OpenAI's LLMs at the time of the analysis. In order to have a model-based classification that is deterministic, given the text input, we set the temperature, a key parameter of any LLM, to zero.[11] Using OpenAI's Python API, we were able to have GPT-4 classify the entire dataset of our 44,605 news articles within just a few hours.

In the following we present three examples of green pledges identified by GPT-4. They demonstrate the successful identification of announcements which contain commitments to reduce GHG emissions:

- *October 14, 2009: Wells Fargo & Company (NYSE: WFC) announced today that it has set a goal to reduce its U.S.–based greenhouse gas emissions by 20 percent*

[11]See Beckmann et al. (2024) for more details on this issue. Note that a zero temperature parameter does not guarantee reproducibility of the results, because the model is not open source and OpenAI could well modify or further finetune it.

below 2008 levels by 2018. The Company is focusing on reducing its carbon footprint as part of its continued environmental commitment to lead by example and to fulfill its pledge as a member of the U.S. Environmental Protection Agency's (EPA's) Climate Leaders program, which Wells Fargo joined last year. [...]

- *January 16, 2020: Microsoft Corp. on Thursday announced an ambitious goal and a new plan to reduce and ultimately remove its carbon footprint. By 2030 Microsoft will be carbon negative, and by 2050 Microsoft will remove from the environment all the carbon the company has emitted either directly or by electrical consumption since it was founded in 1975. [...]*
- *July 21, 2020: Apple today unveiled its plan to become carbon neutral across its entire business, manufacturing supply chain, and product life cycle by 2030. The company is already carbon neutral today for its global corporate operations, and this new commitment means that by 2030, every Apple device sold will have net zero climate impact. [...]*

The third and final step was to validate the model-based results using human coders. To this end, we selected a random subset of 1000 articles and asked five research associates, which were not otherwise affiliated with this project, to decide whether or not they contained green pledges. To provide instructions to our human coders, we followed best practices for human classification as laid out in Grimmer et al. (2022) and created a codebook grounded in our definition of decarbonization commitments.[12] We then compared the two classifications, as shown below. It is important to note that while we treat the human labels as the "truth" for the purpose of evaluating the GPT classification, neither label is necessarily correct. Both reviewers might interpret the task differently, hence a high accuracy only reflects an agreement between the two, but does not necessarily imply that they have accurately identified all green pledges.

Table 6.1: Comparison of classification by GPT-4 and human coders

		Human Coder		
		Negative	Positive	Total
GPT	Negative	842	15	857
	Positive	100	43	143
	Total	942	58	1,000

Confusion matrix for the classifications of 1000 randomly selected news articles by the model GPT-4 and human coders.

Table 6.1 shows a confusion matrix that compares the model-based and human classifications. The accuracy, defined as the fraction of identical classifications, is 89%, suggesting a high level of agreement. However, the high accuracy stems in part from the high number of negatives. The precision, defined as the fraction of GPT-positive articles that were also classified as positive by human coders, is only 30%.[13] The moderate precision reflects the obvious tendency of human coders to be much stricter in the classification: they identified 58 news articles as green pledges, while GPT

[12] The codebook is available in Appendix 6.6.

[13] Using common definitions and considering the human classification as the truth, the sensitivity or recall is 74% and the specificity is 89%. The F2 score is 57%.

found 143 positives. The takeaway is that we can be reasonably confident that most corporate announcements of green pledges were picked up by our text algorithm. At the same time, our model-based classification also appears to contain a fair amount of noise in the form of positives that may not truly represent green pledges. This measurement error problem would tend to cause attentuation bias in regressions that include an indicator variable for green pledges, so that our estimates should be viewed as conservative, i.e., as a lower bound for the true effect of green pledges on stock returns and emissions.

To gain further insights into the problem of identifying corporate green pledges, we also compared the classifications for a random subset of 500 articles from two different human coders. A commonly used measure of intercoder reliability is Cohen's κ, a type of correlation statistic which summarizes the agreement of two different classifications (Grimmer et al., 2022). In our case, $\kappa = 0.43$, a value that indicates "moderate agreement" between the two coders. Apparently, even among human coders, it is difficult to clearly identify and agree on announcements of green pledges. The κ comparing the human and GPT classifications is 0.38, in the range of "fair agreement." These numbers suggest that even human coding of all articles would only slightly increase the precision of the identification of green pledges.

The end result of our classification with GPT is a selection of 862 news articles that likely contain green pledges. Each of these articles can tag multiple firms (ISINs), as sometimes competitors are mentioned or certain decarbonization initiatives involve several companies. In addition, on a given day there may be multiple articles identified as green pledges for the same firm, for example, if a press release is followed by coverage in a newswire or Wall Street Journal article. For our empirical analysis we create unique firm-date events—that is, unique combinations of company ISINs and trading days—to represent corporate green pledges. The result is a sample of 5456 green pledge events.

For many firms, our sample contains more than one and sometimes a significant number of green pledges. A simple approach to reduce the noise in our classification is to only consider the first green pledge for each firm. In total, 1049 firms in our sample have made at least one green pledge, so this is also the number of first-pledge-events. For the stock market analysis in Section 6.4, we will consider both types of green-pledge indicators, either using all events for a firm or using only the first identified green pledge for each firm.[14] For estimating the effects on future emissions in Section 6.5, we use a difference-in-differences approach that considers a firm as either treated or not, based on an indicator variable that turns on when the firm makes its first pledge.

[14]In addition, we also consider two other subsamples of green pledges which impose a more stringent event definition to address two other concerns: First, duplicate or follow-up articles can appear in subsequent days. To address this issue, we require a certain number of days to elapse between two successive events for the same firm. Choosing the required distance as too wide could result in excluding new commitments, while setting it too narrow leads to duplicate events concerning the same green pledge. As a middle ground, we choose a 30 days distances, which we find a reasonable distance to avoid duplicates while not losing new commitments. The second issue arises as articles are sometimes tagging multiple companies. To avoid matching articles to firms that were mentioned in the articles but were not the primary subject of it, we follow Ke et al. (2020) and include only articles tagged with a single company in our analysis. Our "30-days distance" sample contains 4496 events, and our "single-tag" sample contains 4248 events.

6.3 Green Pledges Over Time and Across Firms

This section shows patterns of corporate green pledges over time and across firms. Figure 6.1 plots the annual number of total and positive articles, that is, the full sample of environmental articles (black line), and the sample of articles containing green pledges (green line). We observe a significant number of environmental news articles each year, with a peak in 2010 and a strong increase in recent years. The high number of environmental articles around 2010 can be attributed to the 2009 Copenhagen UN Climate Change Conference and the explosion of Deepwater Horizon in April 2010.[15] The number of positive articles—those deemed by GPT to contain a green pledge—started increasing substantially since 2019, and there have been over 1000 green pledge articles each year since 2021. This pattern may be due to increased public attention to the risks of climate change, as evidenced for example in the text-based measure of climate change concerns of Ardia et al. (2023), as well as increasing climate transition risks which incentivizes more and more companies to take action and commit to emission reductions. By contrast, the years around the Paris Agreement in 2015 did not see a noticeable increase in corporate green pledges, suggesting that this international initiative had little impact on the corporate sector. Even before the recent increase, there was a significant number of green pledges each year: The number of positive articles between 2005 and 2018 was about 150 per year. It is an important advantage of our methodology that it allows us to identify and analyze corporate green pledges for such a long sample period. Other databases of decarbonization commitments, such as SBTi and the CDP which have been used extensively in related research, only start showing a material number of commitments in the mid 2010s.[16]

In order to investigate the distribution of green pledges across firms and industries, we turn from articles to green pledge events, that is, unique firm-date combinations of green pledges. As noted above, our sample contains 5456 green pledge events, corresponding to 1049 different firms, or 8% of the 12701 firms in our CRSP/Compustat sample. The number of green pledges varies significantly across firms. It is most common for firms to make only one pledge, and this is the case for 419 firms. But the median number of pledges per firm is two, and the distribution has a long right tail.[17] The fact that some firms made several pledges, often in double-digits, is an important reason why we also consider first-pledges in our subsequent analysis as well as a sample which requires a 30-day distance between two pledges by the same firm to get rid of duplicate and follow-up articles, as discussed above.

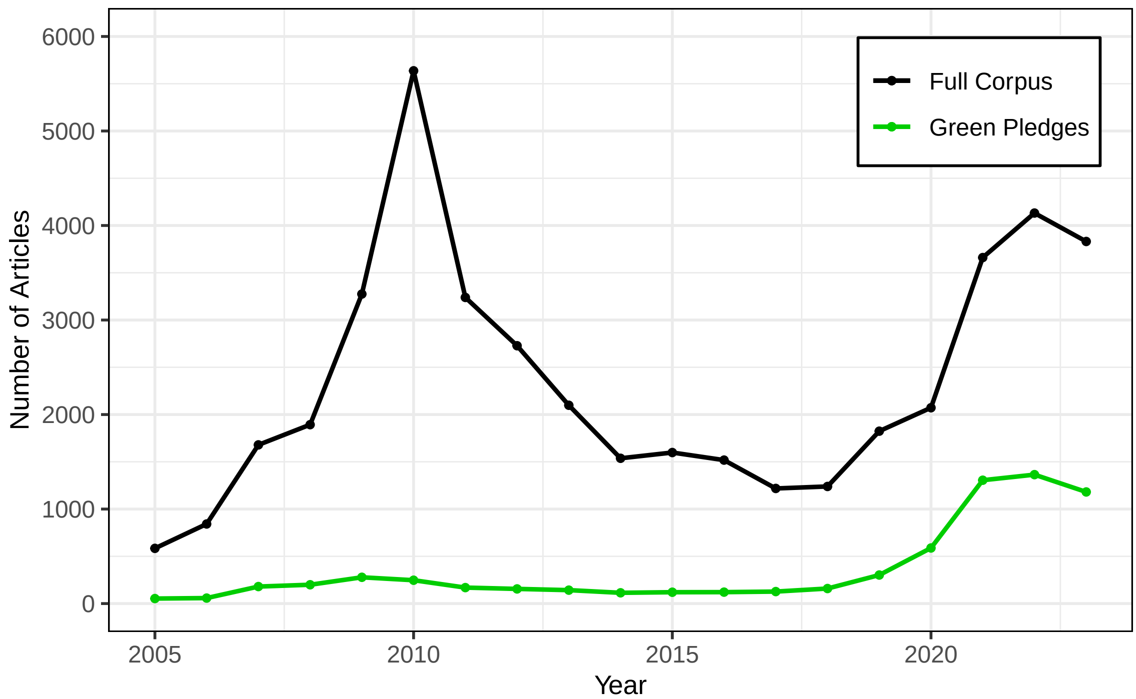
The main focus of this section is to show evidence of the distribution of green pledges across industries, and of the role of firm characteristics. In essence, we are trying to answer the question what kind of firms are most likely to commit to decarbonize their businesses model. For the transition to a low-carbon economy, it is

[15] When computing term-frequency inverse document-frequency (tf-idf) for the year 2010, the most frequent terms are “bp” and “oil”. In contrast, the most frequent terms across the entire sample period include “statements”, “water”, “energy”. This disparity underscores the significant impact of the Deepwater Horizon spill on news coverage in 2010.

[16] In comparing our results to commitments recorded in the SBTi database, we found that for 88% of the firms with SBTi commitments we also identified a green pledge, suggesting reliable coverage of our method for the period when both datasets are available.

[17] Among the firms that made at least one green pledge, the median is 2, the mean is 5.2, the 95% percentile is 21, and the maximum is 83 green pledges per firm. See also Appendix Figure 6.5 for the distribution of the number of pledges across firms.

Figure 6.1: Number of environmental news and green pledges over time



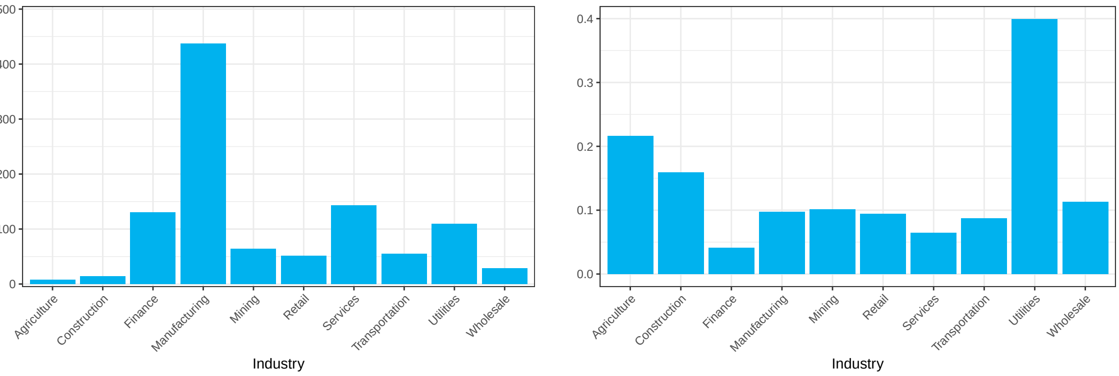
Yearly number of articles in the full text corpus of environmental news articles (black line) and classified as positive by GPT (green line). The total number of articles is 44,605 articles, of which 6,862 are classified as positive, that is, likely containing green pledges. Sample period: 2005 to 2023.

particularly important that large firms with high carbon emissions commit to reduce their carbon footprint. If instead green pledges are announced predominantly by firms that use green technology already, or by firms that have low emissions in the first place and hence a low exposure to transition risks, the aggregate impact on emissions would be small. In this case, green pledges would be less useful for the transition of the U.S. economy to a low-carbon future.

We first study the industry distribution of the firms in our sample. We use SIC codes and apply the industry classification by Bali et al. (2016). Figure 6.2 shows the distribution of pledging firms across industries. The left panel plots absolute frequencies, that is, the number of firms with a green pledge in each industry. The right panel plots relative frequencies, that is, the number of firms with a pledge divided by the total number of firms in the industry.

The industry containing by far the most firms with green pledges is the manufacturing sector, with around 440 pledged firms. This is followed by services, financials, and utilities, each with between 100 and 150 firms with pledges. But the prevalence of firms with pledges in the manufacturing sector stems in part from the fact that it contains a very large number of firms. In relative terms, about ten percent of manufacturing firms have made green pledges, very similar to other sectors. The highest share of firms with green pledges is in the utilities sector, where 40% of firms have made a green pledge. This finding is encouraging because the utilities sector is a particularly brown sector, as shown in Appendix Figure 6.6. We observe a significant share of firms with green pledges also across other brown sectors, such as manufacturing, agriculture, and mining. Overall, Figure 6.2 provides some first cross-industry evidence of the stronger tendency of brown firms to decarbonize.

Figure 6.2: Green pledges per industry



(a) Number of firms with a green pledge (b) Share of firms with a green pledge

Prevalence of U.S. firms with green pledges per industry. Panel (a) plots the total number of firms with a green pledge and panel (b) shows the number of firms with a green pledge divided by the total number of firms in the respective industry. Industry classifications are based on Bali et al. (2016). Sample period: January 2005 to December 2023.

To further explore what kind of firms make decarbonization commitments, we merge the data on green pledges with firm fundamentals and stock market data. Our firm-level analysis uses yearly accounting data from Compustat and emissions from Trucost. We use two emission variables: level of emissions, defined as the sum of scope 1 (direct) and scope 2 (indirect) emissions (in million tons of CO_2 equivalents), and emission intensity defined as the sum of scope 1 and scope 2 emissions divided by revenue (in million USD). Following earlier work in climate finance, we exclude scope 3 emissions because these indirect emissions from upstream and downstream activities of the reporting firm are very large in magnitude and particularly difficult to estimate (Bauer et al., 2022; Huij et al., 2024). Appendix 6.6 provides detailed descriptions and summary statistics for the variables based on accounting financials and emissions.

Table 6.2 summarizes the characteristics of firms with and without green pledges. The sample includes all firms that have CRSP, Compustat, and emissions data, of which there are 12701 in our sample. Among these, 917 firms made at least one green pledge at some point in our sample, and 11784 firms did not. For both groups of firms, Table 6.2 reports sample averages of different firm characteristics—first over time for each firm, and then across firms. The last two columns show p-values and t-statistics for the differences in the cross-sectional mean of the time-averaged characteristics between the two groups. All variables are winsorized at the 1%/99% level.

The comparison in Table 6.2 shows that firms with a green pledge have on average significantly higher emissions, higher emission intensities, and are larger. The differences are both economically and statistically significant. This finding alleviates the concern that commitments to reduce emissions might primarily be made by green firms, with a resulting smaller impact on aggregate emissions. We also find significant differences for book-to-market value, sales growth, and return on equity: Firms with green pledges tend to have higher valuations, lower sales growth, and higher profitability.

Bolton and Kacperczyk (2023b) raise the concern that while unconditionally brown firms are more likely to make decarbonization commitments, *within industries* greener

Table 6.2: Characteristics of firms with and without green pledges

	Green Pledge	No Green Pledge	p-value	t-statistic
Emissions	4.04	0.52	0.0000	8.72
Emission Intensity	0.38	0.13	0.0000	7.50
Size	17.34	1.94	0.0000	45.61
Book-to-market	1.06	1.41	0.0034	-2.94
Leverage	4.22	4.19	0.8038	0.25
Sales growth	0.14	0.21	0.0000	-5.52
Return on equity	0.09	-0.19	0.0000	20.84

Summary statistics for firms with and without green pledges. For both groups, sample averages of the different firm characteristics are reported as well as p-Values and t-statistics for the differences in means between the two groups. Firm characteristics are winsorized at the 1%/99% level.

firms appear to be more likely to make such commitments, according to their analysis. The authors conclude that it is the greener firms, who likely have already adapted their business models to reduce emissions, who make green pledges, and that consequently such pledges may have only modest effects on aggregate emissions and a small role for the green transition.

To address this issue, and to get a more nuanced picture of the type of firm that tend to make green pledges, we estimate panel regressions that control for industry effects and firm characteristics, using a firm-by-year panel dataset. The dependent variable is a binary indicator that takes the value of one in the year a firm makes its first green pledge and remains one in the subsequent years, following the definition of Bolton and Kacperczyk (2023b). The regressions include lagged firm characteristics and year fixed effects. We consider specifications without and with industry fixed effects based on an SIC 2-digit classification

The results in Table 6.3 provide robust evidence that brown firms and large firms are more likely to make green pledges, even after controlling for industry fixed effects and various firm-level characteristics. The effects of log emissions and size on the issuance of a green pledge is positive and statistically significant at the 1% level for all specifications.[18] Our findings in Table 6.3 appear to be at odds with the evidence presented by Bolton and Kacperczyk (2023b), who find that in panel regressions with industry fixed effects green firms are more likely to make decarbonization commitments. There are two fundamental differences between the dataset used in Bolton and Kacperczyk (2023b) and this paper. First, they identify green pledges by using firms who sign up to carbon initiatives such as the CDP and the SBTi, while we use green pledges identified from newspaper articles. Second, Bolton and Kacperczyk use an international sample of firms while we focus on the US stock market. To narrow down the reasons for the discrepancy between the findings, we revisited their analysis for CDP commitments for a sample of U.S. firms only. In this case, we find similar evidence as reported in Table 6.3 with brown and large firms being more likely to reg-

[18]We also estimated regressions using the same set of controls used in Bolton and Kacperczyk (2023b), and found that this leads to the same conclusions as the estimates in Table 6.3. As some of these controls are not available for all firms, we only use the set of controls reported in the table for our main analysis. Our findings also remain essentially unchanged when using GICS 6-digit industry classification for the industry fixed effects as in Bolton and Kacperczyk (2023b).

Table 6.3: Green pledges and within industry variation of firm characteristics

Model	(1)	(2)	(3)	(4)	(5)	(6)
Log Emissions	0.04***	0.04***			0.03***	0.02***
	(0.004)	(0.004)			(0.002)	(0.003)
Log Size			0.03***	0.02***	0.04***	0.04***
			(0.01)	(0.01)	(0.002)	(0.001)
Book-to-market					−5.37***	−5.14***
					(0.54)	(0.55)
Leverage					3.05***	2.09***
					(0.63)	(0.58)
Return on equity					−29.22***	−20.81***
					(4.43)	(4.57)
Sales growth					−17.92***	−18.95***
					(2.09)	(1.45)
Year FE	Y	Y	Y	Y	Y	Y
Industry FE	N	Y	N	Y	N	Y
Observations	30,653	30,653	111,078	111,078	27,595	27,595
R^2	0.14	0.20	0.12	0.16	0.18	0.23

Panel regressions of green pledge indicators on firm characteristics that are lagged by one year. Pledge indicators are equal to one starting in the year of a firm's first green pledge and remain equal to one thereafter. Controls are book-to-market, leverage, return on equity and sales growth. Columns (2), (4) and (6) include 2-digit SIC industry fixed effects. Standard errors which are clustered by firm and time in each model are given in parentheses. ***, **, and * indicate significance at the 1%-, 5%-, and 10%-level, respectively. Values are winsorized at the 1%/99% level. Sample period: January 2005 to December 2023.

ister a commitment with the CDP, in contrast to the results in Bolton and Kacperczyk (2023b) for the international sample.

This section has provided new evidence on the distribution of green pledges over time, across industries, and across firm characteristics. Green pledges have occurred in significant numbers since the beginning of our sample in 2005, they have increased over time, and become particularly widespread since around 2020. We observe green pledges spanning most industries, including traditionally high-emission sectors. Our results show that both within and across industries, brown and large firms are more likely to make green pledges.

6.4 Stock Market Effects

In this section we analyze the stock market reaction to the corporate green pledges. The sign of the effect of green pledges on stock market valuations is *a priori* not clear. On the one hand, valuations may rise because the promised and projected reductions in carbon emissions lower the company's transition risk, including liability, technology and regulatory risks. For example, a firm that unveils plans for a faster decarbonization will be less affected by future carbon taxation or other climate related regulations. In addition to their lower risk, greener stocks can also be more desirable because of inherent green preferences of investors. Pastor et al. (2021) explain both of these channels in a detailed model of the carbon premium. By effectively promising to investors that they will become greener, companies may be able to lower their carbon premium and thus raise their stock price.[19] On the other hand, reductions in carbon emissions will likely require significant up-front investments and potentially far-reaching changes to a company's production processes and business model. Hence, the transition to lower or net-zero emissions may be very costly and risky. The higher costs could weigh significantly on the outlook for earnings and dividends, and the elevated risk could raise the required risk compensation and cost of capital. These forces would tend to push down a company's stock price in response to the announcement of decarbonization plans. The following event-study analysis of stock prices around green pledge announcements will show that the positive effects of corporate green pledges, in the form of lower transition risk exposure and increased investor appeal, appear to be stronger than the negative effects from the costs and risks of decarbonization.

We use event studies to identify the causal impact of the announcements of corporate green pledges on stock prices. The identification assumption is that the corporate announcement is predetermined with regard to the company's stock return on the day of the announcement. By making this assumption, we follow a long tradition in empirical asset pricing that has used event studies to identify the stock market effects of corporate news and announcements, going back to classic work on stock splits (Fama et al., 1969) and earnings announcements (Ball and Brown, 1968; Sloan, 1996). The announcements of green pledges provide new information to investors about a company's trajectory and strategy for GHG emissions, and we estimate the stock price response to this new information.

Our analysis uses daily stock market data for a large cross section of U.S. firms from CRSP, including all common equity listed on either NYSE, AMEX or NASDAQ. To analyze the stock price reactions to corporate green pledges, we need to take account of the exact timing of the announcement and assign each green pledge event to a specific trading day. Announcements made after the New York Stock Exchange closes (at 4pm Eastern time) are assigned to the subsequent trading day. We use several standard control variables in our event study, including size, book-to-market ratio, leverage, sales growth, and return on equity. For those control variables that require annual accounting data, we use a publishing lag of four months, following common practice in empirical asset pricing, to ensure that the information was available to investors at the time of the green pledge. Appendix 6.6 contains a detailed description and summary statistics of the control variables. We also consider emission variables, as described above, in order to investigate the heterogeneity of the effects across green and brown firms.

Using our firm-by-trading-day panel dataset of U.S. firms, we estimate the panel regression

$$R_{it} = \beta d_{it} + \gamma X_{it} + \alpha_s + \delta_t + \epsilon_{it} \tag{6.1}$$

where R_{it} is the stock return of company i on day t, d_{it} is an event dummy that equals one if the firm announced a green pledge on this day, X_{it} includes the firm-specific control variables, α_s are industry fixed effects, δ_t are time fixed effects, and ϵ_{it} is the residual. For industry fixed effects, we use a 2-digit SIC classification. In additional

[19] There are also other possible channels for a positive effect of green pledges on stock prices. For example, the announcements could signal that firms managed to get access to profitable green investment projects.

Table 6.4: Stock market response to green pledges

	All green pledges			First green pledges		
	(1)	(2)	(3)	(4)	(5)	(6)
Green pledge	0.142***	0.156***	0.213***	0.281***	0.285***	0.309***
	(0.04)	(0.04)	(0.04)	(0.11)	(0.11)	(0.11)
Book-to-market	-5.525	-28.971***		-5.509	-28.972***	
	(5.07)	(7.20)		(5.07)	(7.20)	
Leverage	-0.974**	-3.753***		-0.973**	-3.753***	
	(0.39)	(0.63)		(0.39)	(0.63)	
Size	0.004*	-0.110***		0.005*	-0.110***	
	(0.00)	(0.01)		(0.00)	(0.01)	
Sales growth	-13.011***	-0.916		-13.019***	-0.914	
	(4.09)	(3.78)		(4.09)	(3.78)	
Return on equity	54.665***	22.419***		54.658***	22.413***	
	(6.68)	(5.72)		(6.68)	(5.72)	
Number of obs.	14,815,228	14,815,228	17,529,819	14,815,228	14,815,228	17,529,819
R^2	0.18	0.18	0.17	0.18	0.18	0.17
Industry FE	Yes	No	Yes	Yes	No	Yes
Firm FE	No	Yes	No	No	Yes	No
Time FE	Yes	Yes	Yes	Yes	Yes	Yes

Panel regressions of daily stock returns on event dummy for green pledges and firm-specific controls. Columns (1)–(3) show results for an event dummy that equals one on all days that a firm announces a green pledge, and columns (4)–(6) shows results for a dummy that equals one only on the day of the first green pledge of each firm. Industry fixed effects are based on 2-digit SIC codes. Standard errors, clustered by firm and time, are given in parentheses. ***, **, and * indicate significance at the 1%-, 5%-, and 10%-level, respectively. Sample period: January 2005 to December 2023.

specifications, we use firm fixed effects instead of industry fixed effects to assess the robustness of our results. Throughout the paper, we report standard errors that are clustered by firm and time.

Table 6.4 shows the estimation results. The first three columns report the estimates for regressions using the full set of green pledge events, using different fixed effects and either with or without firm-level controls. Across all three specifications, the estimated effect is positive and statistically significant at the 1%-level. The size of the estimated coefficient indicates that the daily stock return increases by between 0.14 and 0.21 percentage points when a firm announces a green pledge. The average daily return in our sample is about 0.015% (see Table 6.9), meaning that on event days returns are about ten times larger than on non-event days. In other words, the estimated effect is both statistically and economically highly significant.

Our green pledge indicators are only a noisy measure of the occurrence of meaningful decarbonization commitments, given the challenges of classifying news articles containing complex and nuanced information about corporate plans for future emissions. As a result, d_{it} likely contains a significant amount of measurement error, as evident from the relatively low precision of the GPT classification compared to human coders in Table 6.1. With attenuation bias in our estimates of β in regression (6.1), the actual effect would be larger in magnitude than the estimates we report in Table 6.4. Our estimates are likely a lower bound for the true stock price impact of corporate green pledges.

The last three columns of Table 6.4 report the estimates for regressions using only the first green pledge for each firm. In these regressions the estimate for β is in the range of 0.28 to 0.31 percentage points, almost twice as large as for the regressions using all pledges. Clearly, the estimated positive effect is particularly strong for the first green pledge issued by a firm, and consecutive pledges by the same firm appear to have much smaller effects, possibly because they contain revisions of previous pledges or even delays and revisions of previously set targets. This simple filter of narrowing down our green pledge events appears to be highly effective in identifying the most meaningful and most influential announcements of decarbonization commitments, reducing the measurement error and the attenuation bias in our estimates.

We establish the robustness of these findings in several ways. First, we consider two other subsets of our green pledge events, either using pledges in news articles that tag only a single firm (ISIN), or requiring at least a 30-day distance between two consecutive pledges by a firm. As Appendix Table 6.10 shows, the results are very similar to those for the sample of all green pledges, indicating that neither duplicate and follow-up articles nor firms that are tagged within green pledges of other firms have a significant effect on our findings.

Another important check for our results is the following placebo test: We estimate the impact of corporate environmental news that were *not* classified as green pledges, by adding to regression (6.1) a separate event dummy that captures whether firm i on trading day t was covered by a negatively-classified news article. The key question is whether the coefficient on the green pledge dummy is significantly larger than the coefficient on this additional "other environmental news" event dummy.

Table 6.5 shows the results for regressions using all green pledges in columns (1) and (2), and using only first green pledges in columns (3) and (4). The coefficient on the dummy for other environmental news is significantly positive in all four regressions. Apparently, any environmental news are "good news" on average and tend to increase a company's stock return, potentially due to an attention effect (Chan, 2003). But the coefficient on the green-pledge event dummy is two to four times larger than the coefficient on the other-news indicator. Tests for the equality of the coefficients on the two indicator variables reject the null at the 5%-level when including all green pledges, and at the 0.1%-level when including only first pledges. These estimates confirm that we are truly picking up the effect of green pledges, which is substantially higher than the "placebo effect" of environmental news coverage. The particularly pronounced differences for the case of first green pledges again support the notion that these event indicators contain the most positive information for investors because they are better measures of substantial decarbonization announcements.

A related question about "other news" is whether our estimates might be confounded by earnings announcements, which have long been known to drive stock prices (Beaver, 1968). As a robustness check, we dropped all observations from our sample where green pledges coincided with earnings announcements on the same days. The estimation results, which we omit for the sake of brevity, were essentially identical to those reported in Table 6.4, indicating that the presence of earnings announcements is not important for our findings.

We also consider the robustness of our results with regard to a sample split for

Table 6.5: Green pledges vs. other environmental news

	All green pledges		First green pledges	
	(1)	(2)	(3)	(4)
Green pledge	0.137***	0.205***	0.278**	0.305***
	(0.04)	(0.04)	(0.11)	(0.11)
Other env. news	0.057***	0.094***	0.066***	0.110***
	(0.02)	(0.02)	(0.02)	(0.02)
Book-to-market	-5.556		-5.552	
	(5.07)		(5.07)	
Sales growth	-13.001***		-13.004***	
	(4.09)		(4.09)	
Leverage	-0.976**		-0.976**	
	(0.39)		(0.39)	
Size	0.004*		0.004*	
	(0.00)		(0.00)	
Return on equity	54.689***		54.688***	
	(6.68)		(6.68)	
Number of Obs.	14,815,228	17,529,819	14,815,228	17,529,819
R^2	0.180	0.168	0.180	0.168
p-value	0.047	0.013	0.000	0.000

Panel regressions of daily stock returns on the green pledges event dummy and a dummy capturing all other environmental news which are not classified as a green pledge. The total number of environmental news not classified as green pledge is 26,821. Column (1) shows results for the sample of all green pledges with industry fixed effects and column (2) shows results without controls. Columns (3)-(4) show the corresponding results for the sample of first green pledges. All regressions include industry fixed effects, based on 2-digit SIC codes, and time fixed effects. Standard errors, clustered by firm and time, are given in parentheses. ***, **, and * indicate significance at the 1%-, 5%-, and 10%-level, respectively. The last row reports p-values for testing the hypothesis that the two dummy coefficients, for green pledge events and other environmental news, are equal. Sample period: January 2005 to December 2023.

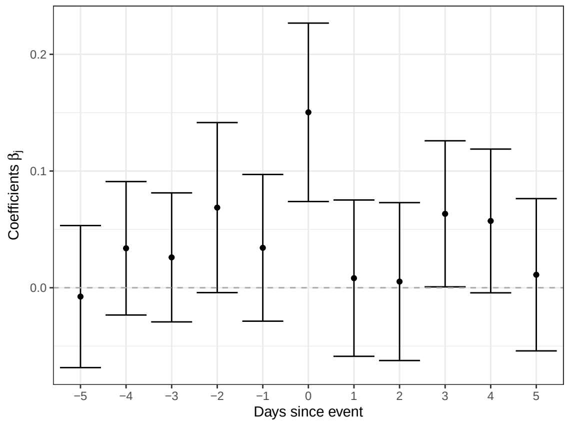
the periods before and after the Paris agreement in December 2015. Increased climate concerns since the Paris agreement might lead to a stronger stock market reaction following a green pledge. The estimates in Appendix Table 6.11 suggest that green pledges had positive effects on stock prices in both sample periods. Overall, we find that our results tend to be robust to different sample splits.

Our analysis so far has focused on the contemporaneous effects of green pledges. We now turn to estimates of the dynamic effects before and after the events, in order to understand (a) whether there might leakage of news prior to the green pledge announcements, and (b) whether there are lagged effects, and potentially a partial reversal, after the announcements. We add five leads and lags of the event dummy and estimate the new panel regression

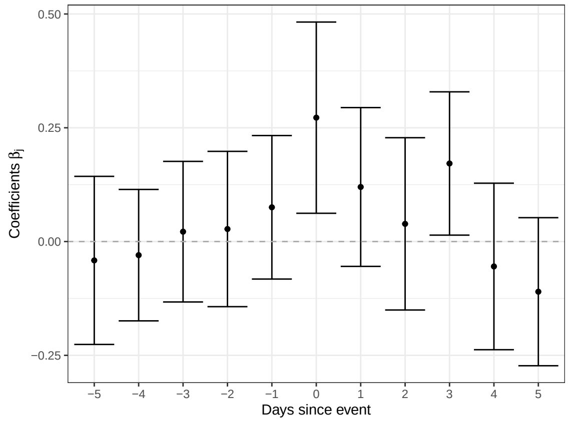
$$R_{it} = \sum_{j=-5}^{5} \beta_j d_{i,t-j} + \gamma X_{it} + \alpha_s + \delta_t + \epsilon_{it}, \tag{6.2}$$

where $d_{i,t-j} = 1$ implies that there was a green pledge event j days before the return observation on day t.

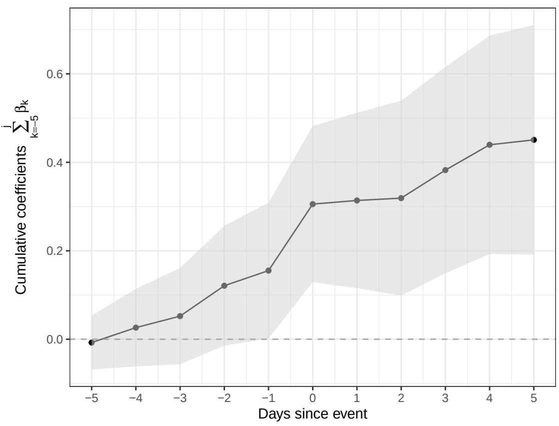
Figure 6.3: Stock return response around green pledges



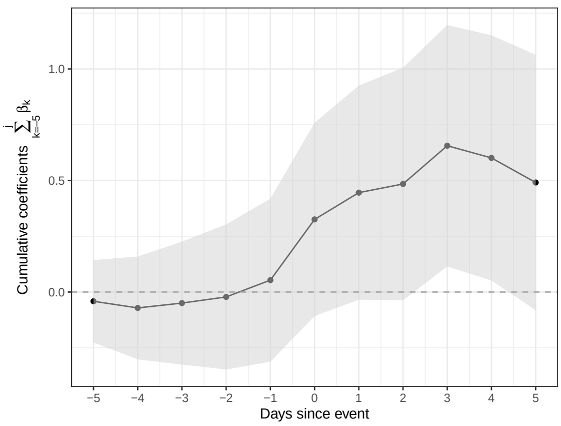
(a) All pledges



(b) First pledges



(c) All pledges – cumulative



(d) First pledges – cumulative

Dynamic event study estimates of the effects of green pledges. The top two panels plot the coefficients β_j of regression (6.2) for leads and lags of the event dummy. Panel (a) shows results for the sample with all green pledges and panel (b) for only first green pledges. Controls are book-to-market, sales growth, leverage, size, and return on equity. The bottom two panels show the cumulative effects, $\sum_{k=-5}^{j} \beta_k$. All plots show 95% confidence intervals based on standard errors clustered by firm and time and are shown around the point estimate. Sample period: December 2005 to January 2023.

Figure 6.3 shows estimates of the coefficients β_j in the top two panels, and the cumulative effects in the bottom two panels, together with 95%-confidence intervals based on clustered standard errors. The left two panels correspond to the regression using all green pledges, and the right two panels correspond to the regression using only first green pledges. Our estimates do not show any evidence for information leakage prior to green pledge events. For the sample with all green pledges, there is a moderate upward drift in the cumulative effects leading up to the event, but none of the coefficients β_j for $j < 0$ is statistically significant at the 5%-level. For the sample with first green pledges, there is no noticeable pre-event drift at all. The positive effect on the announcement days is not reversed in the subsequent days. Instead, the positive valuation effects continue to slightly increase over the days after the announcement; some of the coefficients for the effects several days later are positive and marginally statistically significant. Overall, the estimates in Figure 6.3 suggest that green pledges lead to a persistent increase in the stock market valuation, consistent with a reduction in the carbon premium and the firm's cost of capital.[20]

We now turn to the question of heterogeneity. Do the stock market effects of climate commitments depend on the industry, the firm's size, or its greennes? Firm heterogeneity and the differential effects of climate-conscious investing play a central role for the transition to a low carbon economy. For example, if green pledges really imply a lower exposure to transition risks, we would expect to see an especially strong market reaction for brown firms with high emissions, due to the fact that they have larger transition risk exposures to begin with. If, however, mainly green firms see reductions in the carbon carbon premium and hence, their cost of capital, green investing could be ineffective or even counterproductive as brown firms might response to relatively higher cost of capital by increasing emissions to realize short term profits (Hartzmark and Shue, 2023).

To investigate industry-level heterogeneity, we estimate equation (6.1) separately for each industry. As in Section 6.3, we use the industry classifications by Bali et al. (2016). For each industry, we estimate the panel regression first using all green pledges, and then using only the first green pledges. In both cases the regression includes the usual firm-level controls and time fixed effects. To assess the effects in brown and green industries, we also calculte industry-level emissions and emission intensities.[21] The results are reported in Table 6.6. The estimates for all pledges show that in brown industries with high emission levels and intensities, such as the utilities sector and manufacturing, green pledges significantly increase firm values. For other, less carbon intense industries, the effect appears to be less pronounced. For the sample of first pledges, we obtain higher standard errors due to significantly less pledges per industry, and only the manufacturing sector shows a significantly positive coefficient.

Next, we test directly whether the stock market reaction to green pledges is stronger for brown firms compared to green firms. Our goal is to assess whether brown firms, which have the potential to contribute most to the transition to a low-carbon economy, can increase their valuations by committing to become greener. We measure greenness of a firm by using either the level of carbon emissions or by using emission intensities. Emission levels have been proposed as a direct proxy for firm's exposures to transition risks by Bolton and Kacperczyk (2021) and Bolton and Kacperczyk (2023c). Emission intensities have been used for example by Aswani et al. (2023) and Zhang (2024). In addition to emissions, we examine firm size, which has long been known to be an important determinant of expected return and a firm's risk premium (Banz, 1981; Fama and French, 1995), and also plays a role in firms' differential responses to monetary policy (Ehrmann and Fratzscher, 2004), and fiscal policy (Eskandari and Zamanian, 2023).

To investigate heterogeneous effects of green pledges depending on firm characteristics, we sort firms into five equal-sized groups based on either emission levels,

[20]Figures 6.7 and 6.8 in Appendix 6.6 show the corresponding results the the specifications with either firm fixed effects, no controls, the sample of pledges that only tag a single firm as well as the sample of pledges that requires a 30 day distance between two consecutive pledges. The results are very similar to those shown in Figure 6.3.

[21]We use emissions defined as the sum of scope 1 and scope 2 emissions (in million tons of CO_2), and emission intensity as the ratio of emissions and revenue (in kilotons of CO_2 per USD million of revenue). We first average emission levels or intensities across all firms in an industry on each date, and then calculate the industry-level statistic as the average across time.

Table 6.6: Stock market reaction to green pledges across industries

Industry	Green pledges	First pledges	Number of obs.	Carbon emissions	Emission intensity
Utilities	0.118**	0.148	502,645	21.442	2.658
	(0.05)	(0.26)			
Mining	0.070	0.163	577,368	3.429	0.621
	(0.19)	(0.33)			
Transportation	0.008	0.374	617,985	3.419	0.301
	(0.10)	(0.57)			
Manufacturing	0.171***	0.570***	5,866,646	2.018	0.158
	(0.07)	(0.19)			
Retail	0.470*	0.258	852,831	1.097	0.063
	(0.26)	(0.29)			
Wholesale	0.002	-0.558	430,460	0.627	0.061
	(0.29)	(0.46)			
Agriculture	1.005	1.401	35,756	0.564	0.941
	(1.09)	(1.65)			
Construction	-0.007	0.167	187,999	0.354	0.077
	(0.17)	(0.29)			
Services	0.180*	0.050	2,456,987	0.216	0.045
	(0.11)	(0.24)			
Finance	-0.034	-0.117	3,228,772	0.085	0.010
	(0.10)	(0.19)			

Event-study regression (6.1) estimated separately for each industry. Results are shown for the sample of all pledges as well as for only first pledges. Controls are book-to-market, sales growth, leverage, size, and return on equity. All regressions include time fixed effects. Standard errors clustered by firm and time in each model are given in parentheses. ***, **, and * indicate significance at the 1%-, 5%-, and 10%-level, respectively. The table also reports the average firm level carbon emissions (scope 1+2, measured in million tons of CO_2), and the average emission intensity, per industry (in kilotons of CO_2 per USD million of revenue). Sample period: January 2005 to December 2023.

emission intensities, or size. For emission-based measures, we sort firms annually, while for firm size, which is measured using market value, the sorts are formed anew every trading day. This grouping approach has two advantages over a regression with interaction effects for size or greenness: First, it reduces the impact of measurement errors in the variable measuring heterogeneity. Second, it allows for possible non-linear effects. For each of the quintile groups, we run our main panel regression as specified in equation (6.1) with firm-level controls as well as industry and time fixed effects.

Table 6.7 shows that the stock market response to green pledges is particularly strong for firms with high emission levels. In particular, for emission levels, only stocks within the top quintile (firms with high emissions) exhibit a significant increase in the stock price following a green pledge, with an increase of 9 basis points on average. We find similar effects for large firms with only the stocks in the top quintile showing a significant response to green pledges. As size and log emissions are highly correlated, heavy emitters for which investors require a high carbon premium also tend to be large firms. For emission intensities, we only find a weakly significant stock market

Table 6.7: Cross-sectional response of stock returns to green pledges

	Quintile				
	5	4	3	2	1
(A): Emissions					
Coefficient	0.094**	0.068	0.433	0.145	0.530
	(0.05)	(0.08)	(0.32)	(0.27)	(0.45)
R^2	0.34	0.35	0.33	0.30	0.30
Nr. of obs.	757,179	835,481	889,130	842,668	944,896
Nr. of events	1,929	463	237	90	30
(B): Emission intensity					
Coefficient	0.117	0.217*	0.089	0.089	0.000
	(0.08)	(0.11)	(0.08)	(0.13)	(0.12)
R^2	0.33	0.30	0.27	0.31	0.46
Nr. of obs.	741,486	776,389	953,928	880,731	916,820
Nr. of events	1,435	338	467	285	224
(C): Size					
Coefficient	0.077**	0.027	0.495	1.072	0.788
	(0.03)	(0.13)	(0.31)	(0.88)	(1.13)
R^2	0.37	0.36	0.32	0.17	0.05
Nr. of obs.	2,538,665	2,513,622	2,575,396	2,609,900	2,612,323
Nr. of events	2,442	423	161	108	33

Regressions of daily stock returns on the green pledge event dummies for quintile groups based on log carbon emission, emission intensity, and size. Quintile 5 corresponds to the largest and brownest firms. Controls are book-to-market, sales growth, leverage, size, and return on equity. All regressions include industry and time fixed effects. Standard errors which are clustered by firm and time in each model are given in parentheses. ***, **, and * indicate significance at the 1%-, 5%-, and 10%-level, respectively. Sample period: January 2005 to December 2023.

reaction for the second highest quintile. This might indicate that level of emissions are a better proxy for the exposure towards transitional risks as suggested by Bolton and Kacperczyk (2023c). Table 6.12 in Appendix 6.6 shows the corresponding results for the sample of first pledges which shows similar results.[22] One caveat of this analysis is the loss of power for the groups with green or small firms, due to the small number of green pledges in these groups. But the estimates in Table 6.7 quite clearly shows that the stocks of large and high-carbon firms exhibit strong responses to green pledges.

To summarize, we find evidence that corporate green pledges have a significantly positive impact on firms' stock valuations, suggesting that they reduce the carbon premium. By announcing plans to reduce emissions, companies effectively promise to become greener. The positive impact of this new information on stock prices shows that the value investors attribute to decarbonization—either due to lower transition risks or higher desirability in the presence of green preferences—outweighs the implied costs and risks from the reduction in emissions. The evidence also suggests that the stock market reaction is particularly strong for brown firms with high emission levels.

[22]Note that the power of our tests is rather low in this case as each group only contains a small number of green pledges.

A simple explanation for this pattern is that brown firms can achieve a larger reduction of transition risk exposure and the carbon premium via green pledges, while green firms can at best marginally improve their carbon footprint and change their transition risk.

In principle, the positive impact of green pledges on firm valuations could also be due to changes in dividend expectations. However, there is no theoretical reason to expect green firms to have higher dividends than brown firms, and the climate finance literature has focused on the carbon premium hypothesis to explain differences between green and brown stock returns. Both theoretically and empirically, it appears more plausible that green pledges affect discount rates than dividend expectations.[23]

Of course, the decision to announce a green pledge is not random or exogenous. The firms that choose to make a green pledge are likely those more likely to benefit from such an announcement, with larger reductions in the carbon premium and lower expected costs of decarbonization. Because of this self-selection, our results should be interpreted as capturing the effects of green pledges for those firms that are likely to announce them, and not for the average firm (which might well see smaller effects). Importantly, our evidence in Section 6.3 shows that large and carbon-intensive firms are more likely to make green pledges. These firms have both the highest exposure to transition risks and the greatest potential to make a meaningful contribution to the green transition.

By announcing green pledges, firms can lower their carbon premium and cost of capital. This effect could provide a market-based incentive for decarbonization commitments. The lower cost of capital could also help firms finance their decarbonization strategies and green investments. Our results suggest an overall positive effect of climate-related investing for the transition to a low-carbon economy.

6.5 Future Emissions

Our stock market results suggest that investors tend to view corporate green pledges as credible. But are the announced commitments really followed by reductions in firm-level emissions, or are such announcements empty promises that reflect the desire of firms to polish their environmental image? In the words of Bingler et al. (2022), do firm "walk the climate talk" and follow up their climate commitments with measurable actions? This question is particularly pressing given increasing concerns about greenwashing and cheap talk in climate-related announcements and disclosures (Nemes et al., 2022; Bingler et al., 2022, 2024; Dzieliński et al., 2023). Companies may falsely represent themselves as environmentally friendly by manipulating environmental metrics or re-branding products and marketing strategies touting their clean energy or pollution reduction efforts. And their green pledges might just be cheap talk, without meaningful subsequent reductions in emissions.

To address this question, we carry out difference-in-differences estimation of changes in firm-level emissions after corporate green pledges. The basic idea is to compare changes in emissions before and after the issuance of a green pledge to the changes in emissions for firms that have not made such pledges.

[23] Using a Campbell-Shiller decomposition of stock price effects, Ardia et al. (2023) find that the discount rate channel is the primary channel through which climate transitions risks are priced. In additional results from difference-in-differences estimation, we did not find any evidence for effects of green pledges on future earnings, cash flows, or dividends.

Two aspects of our empirical setting complicate the estimation of the "treatment effects" of green pledges on emissions. Treatments are of course staggered, as different firms announce them at different times. While variation in treatment timing is by itself not necessarily problematic, the combination with heterogeneous treatment effects can render standard event-study estimates unreliable. Since decarbonization commitments may differ widely in terms of their ambition, specificity, and timelines, but are all captured by a simple binary indicator, treatment effects should indeed be expected to be heterogeneous. With staggered treatments and heterogeneous treatment effects, standard two-way fixed effects (TWFE) estimates may yield inconsistent estimates of the average treatment effect (Baker et al., 2022; Roth et al., 2023). The problem are the "forbidden" comparisons between newly treated units and previously treated units, as the latter may still be experiencing delayed treatment effects. Various approaches have been proposed to estimate average treatment effects in a way that addresses the limitations of TWFE (Callaway and Sant'Anna, 2021; Sun and Abraham, 2021; Dube et al., 2023).

We use the local projections difference-in-differences (LP-DiD) approach by Dube et al. (2023) for our baseline estimates of the dynamic response of emissions to green pledges. This method accounts for heterogeneous treatment effects by including only "clean controls," that is, firms that have not yet been treated themselves at the time of each treatment under consideration. In addition to its simplicity and reliability, a further advantage of LP-DiD is that it allows the common trend assumption to hold conditionally, given that the likelihood of making a green pledge might depend on pre-treatment firm characteristics.[24] In a firm-by-year panel, we estimate the LP-DiD specification

$$y_{i,t+h} - y_{i,t-1} = \beta^h \Delta D_{it} + \sum_{p=1}^{P} \gamma_p^h \Delta y_{i,t-p} + \sum_{m=1}^{M} \sum_{p=0}^{P} \gamma_{mp} \Delta x_{m,i,t-p} + \delta_t^h + \epsilon_{it}^h, \qquad (6.3)$$

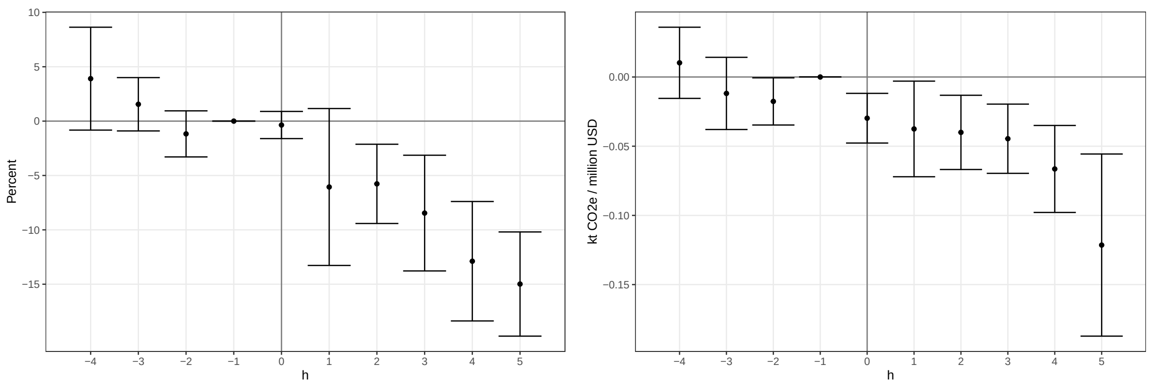
where the indicator D_{it} captures the treatment, i.e., whether firm i has issued a green pledge in year t or before.[25] The sample is restricted to the observations that are either newly treated ($\Delta D_{it} = 1$) or clean controls ($D_{i,t+h} = 0$). The variable y_{it} denotes either log carbon emissions or emission intensity, in both cases based on the sum of scope 1 and scope 2 emissions. The regression estimates the effects of green pledges on log emissions or emission intensity h years after the pledge. Our LP-DiD regression (6.3) also includes time fixed effects, δ_t^h, lagged emissions, and current and lagged values of M different controls. Lagged values of $y_{i,t}$ are included to account for the pattern documented above that brown firms are more likely to commit to decarbonization. We control for firm characteristics that could be correlated with the treatment, including size, book-to-market ratio, leverage, profitability, revenue growth, and log PP&E. By the nature of the LP-DiD estimation, the controls are included as first differences, and we set $P = 2$ for the number of lags.

Figure 6.4 plots the estimated effects on log emissions (left panel) and emission intensities (right panel) from four years before the pledge to five years after. Firms

[24] Given the heterogeneity of treatment effects, an important question is how these effects are aggregated. Dube et al. (2023) show that under the assumption that the covariates have linear and homogeneous effects, the LP-DiD baseline specification implies a variance-weighted average treatment effect.

[25] In other words, for firm i the indicator D_{it} equals one in the year of its first pledge and all subsequent years. Note that the treatment is absorbing—the most common assumption in difference-in-differences estimation—and we estimate the effects of each company's first green pledge.

Figure 6.4: Impact of green pledges on carbon emissions



(a) Log emissions (b) Emission intensity

LP-DiD estimates of the effects of green pledges on the log-level of emissions (left panel) or the emission intensity (right panel), using regression (6.3). Controls include lagged values of size, book-to-market, leverage, profitability, revenue growth, and log PP&E, as well as lagged emissions (log-levels or intensities). Error bars correspond to 95% confidence intervals based on Driscoll and Kraay (1998) standard errors. The number of observations corresponds to the one-year horizon.

who make a decarbonization commitment significantly reduce their carbon emissions after the pledge, compared to firms without such commitments. Green pledges predict six percent lower emissions after one year and 15 percent lower emissions after five years, compared to firms without a pledge. Prior to the announcement there is no significant difference in emissions, which alleviates concerns that firms that issue a green pledge are simply confirming an existing downward trajectory for emissions. For changes in emission intensity, we find broadly similar results, as shown in the right panel of Figure 6.4. Following a green pledge, emission intensity falls significantly, with a decrease of 0.05 (kilotons of CO_2 equivalents per million US dollars revenue) in the first year and 0.12 after five years. Prior to the pledges, changes in emission intensities are not significantly different between firms with and without a pledge.

The LP-DiD approach is one of several methods designed to estimate the average treatment effect (ATE) in the presence of staggered treatment and heterogeneous treatment effects. Sun and Abraham (2021) modify TWFE estimation by including only clean controls and incorporating a cohort-specific dummy variable to capture heterogeneous treatment effects across cohorts, and then estimate the ATE using the appropriate weighted average of cohort-specific effects. These and alternative approaches proposed in the literature ultimately aim to estimate the same object of interest, the ATE, which in our setting is the difference between a firm's emissions after making a green pledge and the emissions it would have produced had it not committed to the pledge. This common objective allows for a meaningful comparison of the estimated effects across different methods. Table 6.8 shows such a comparison, including results for LP-DiD, conventional TWFE estimation, and the Sun and Abraham (2021) (S&A) method. The top panel reports the estimated effects on log emissions, and the bottom panel shows the effects on emission intensities. We obtain estimates for two different time horizons—one year and five years after the pledge—and both for regressions with and without controls. All the different estimation results consistently show a decrease in emissions and intensities following a green pledge. While point estimates differ

Table 6.8: Average treatment effect of green pledges on carbon emissions

	LP-DiD		TWFE		S&A	
	(1)	(2)	(3)	(4)	(5)	(6)
(A) Log emissions						
1 year	-6.06^{*}	-10.63^{***}	-5.24	-6.26	-5.90^{**}	-7.99^{***}
	(3.68)	(0.89)	(3.20)	(4.67)	(2.11)	(2.98)
5 years	-14.98^{***}	-20.00^{***}	-5.02	-12.66^{**}	-4.45^{*}	-12.00^{**}
	(2.44)	(1.95)	(3.53)	(5.81)	(2.71)	(5.71)
Observations	11,198	20,424	17,305	30,640	17,305	30,640
(B) Emission intensity						
1 year	-0.038^{**}	-0.045^{*}	-0.036	-0.065^{**}	-0.048^{**}	-0.056^{**}
	(0.017)	(0.023)	(0.031)	(0.035)	(0.018)	(0.024)
5 years	-0.123^{***}	-0.125^{***}	-0.079^{*}	-0.102^{*}	-0.098^{*}	-0.096^{**}
	(0.035)	(0.033)	(0.047)	(0.057)	(0.054)	(0.049)
Observations	11,197	20,400	17,302	30,608	17,302	30,608
Controls	Y	N	Y	N	Y	N
Time FE	Y	Y	Y	Y	Y	Y
Firm FE	N	N	Y	Y	Y	Y

Alternative estimates of the average treatment effect on firm level emissions one and five years after a firm's first green pledge. Columns 1 and 2 show LP-DiD estimates following Dube et al. (2023), and the first column corresponds to the estimates in Figure 6.4 for $h = 1$ and $h = 5$. Columns 3 and 4 report standard two-way fixed effects (TWFE) estimates. Columns 5 and 6 show estimates using the Sun and Abraham (2021) approach. Estimates for log emissions are in the top panel, and estimates for emission intensities are in the bottom panel. Standard errors are Driscoll-Kraay for LP-DiD, and clustered (by year and firm) for TWFE and S&A. ***, **, and * indicate significance at the 1%-, 5%-, and 10%-level, respectively. Sample period: January 2005 to December 2023.

somewhat across methods, the estimated reduction is generally substantive and in the majority of the cases statistically significant. Table 6.8 also shows that controlling for pre-treatment firm characteristics only slightly reduces the estimated magnitudes. Overall, the estimates in Table 6.8 demonstrate the robustness of our main results across different estimation methods and specifications.

Two points should be kept in mind when interpreting the results in this section. First, these estimates do not capture causal effects, since green pledges are of course neither random nor exogenous and thus not "treatments" in the traditional sense. A firm may well have had long-standing plans for decarbonization, and the public release of these plans is merely the last step before their implementation. Our estimates capture a predictive relationship, akin to Granger causality, but this predictive relationship is key to understanding the actual information content of corporate green pledges. Second, our evidence shows reductions in emissions, but does not directly speak to the question *how* firms decrease their emissions. There are two broadly different strategies, among others: Companies could invest in green technologies and in this way lower emissions in their production processes. Alternatively they might simply divest from certain high-emission business lines, potentially selling these to other companies. Berg et al. (2024) note that divestment is often the main reason for emission reduction of large emitters.

Although corporate green pledges neither cause lower emissions nor tell investors how emissions will be reduced, they contain new information that lowers the expected trajectory for future firm-level emissions. And because emissions are a meaningful proxy measure for a firm's exposure to climate transition risk, green pledges can lower the perceived transition risk exposure and required carbon premium. Alternatively, lower emissions can increase the appeal of a stock for investors that have non-pecuniary green preferences. Through these two channels, our finding that emissions decrease after a green pledge rationalizes the strong and positive stock market response documented in Section 6.4. The estimated decline in emissions gives investors good reasons to view corporate green pledges as credible, and reduces concerns about greenwashing and cheap talk in climate commitments.

6.6 Conclusion

The transition to a net-zero economy poses a major challenge for corporations. Regulations to reduce emissions will impose future costs on firms and might make their business models obsolete. By committing to reduce emissions, companies can lower their exposure towards such transition risks and make their stock more attractive to investors. However, strategies to reduce emissions can be costly and have negative effects on profitability. The outcomes from decarbonization strategies are also uncertain and involve risks. Hence, whether commitments to reduce emissions have positive or negative effects on firm valuations is ambiguous.

Using a new database of corporate climate commitments derived from news articles with a large language model, we study the stock market effects of these commitments. Event-study results clearly show that firms who commit to decarbonize experience a significant increase in their stock price. Our results suggest that a decarbonization commitment reduces the carbon premium, as stocks become greener and thus more desirable due to lower transition risk and/or green preferences, and that investors perceived these positive effects on company valuations to outweigh the costs and risks from decarbonization. This evidence supports the view that investors require significant compensation for transition risks, consistent with the carbon premium hypothesis of Pastor et al. (2021), and that companies are able to reduce their transition risk exposure and carbon premium by issuing green pledges.

Corporate green pledges do not appear to be cheap talk, but are instead followed by significant reductions in firm-level emission levels and emission intensities. This result rationalizes the positive stock market reaction, because investors have good reasons to view green pledges as credible. Furthermore, this result suggests that the financial incentive for climate commitments, in the form of higher stock valuations and lower cost of capital, appear to be justified as the corporate announcements are followed by corporate climate actions. Given that these voluntary commitments are also more prevalent for large and brown firms, they have the potential to meaningfully contribute to the green transition of the U.S. economy.

Our work opens up several avenues for future research. First and foremost, our binary classification method using GPT-4 can be extended in several directions. Using an open-source, local language model would ensure reproducibility and allow for fine-tuning of the model (Cook et al., 2023). To go beyond our binary classification, LLMs can be used to differentiate between different types of commitments according to their stringency and ambition, the amount and time horizon of the planned reductions of emissions (e.g., net-zero versus other commitments), specificity and concreteness of the goals, existence of actionable plans, and other criteria. For promising new work in this direction, see for example Colesanti Senni et al. (2024); their approach could be extended to score the ambition, credibility, and feasibility of decarbonization commitments using common indicators. Announcements that score higher on these dimensions may well lead to an even more positive stock market reaction. Given the global scale of the issue, another natural extension of our work is to incorporate data from other countries, in particular those of the European Union given their ambitious climate goals and the availability of high-quality data on firm emissions. On the methodological side, while our analysis indicates that the carbon premium declines in response to green pledges, it does not speak to the relative importance of transition risk premia in brown stocks vs. investor preferences for green stocks, the two key channels proposed in the literature to explain a carbon premium (Pastor et al., 2021, 2022). Combining our event-study methodology with data on green investor fund flows, as in Patozi (2024), could potentially help researchers make progress on this important issue. Finally, future research should address the question whether on aggregate, corporate climate commitments are sufficient to decarbonize the economy, or quantify the shortfall between corporate commitments and national net-zero goals. New results for the U.S. corporate sector from Pastor et al. (2024) suggest that there is a significant shortfall relative to the goals set forth in the Paris Agreement.

Appendix 6.A: Codebook

In the following we present the codebook provided to the human coders in order to identify green pledges in the newswire articles. In particular, we assigned them the following task:

> In a new research project, we are investigating the financial and environmental effects of corporate announcements of decarbonization commitments. To identify these announcements from corporate news articles, we need your help!
> A *decarbonization commitment* is defined as follows: *A firm makes a clear, actionable commitment to significantly reduce future greenhouse gas emissions.* Greenhouse gases include carbon dioxide (CO_2) and methane.
> In the attached Excel list, you will find a random selection of news articles from Dow Jones (DJ newswires and Wall Street Journal articles). We are asking you to please identify decarbonization commitments in these articles.
>
> *Please read carefully through each article, decide whether it constitutes a decarbonization commitment.* Then label it accordingly with "yes" (positive: the announcement contains a decarbonization commitment) or "no" (negative—no decarbonization commitment).
>
> Please classify an article as positive only if the company announces a significant reduction of direct emissions, that is, emissions that occur from sources controlled or owned by the company. The announcement should be news and should describe the company's commitments and plans for the future. Do not classify articles as positive that only contain announcements to reduce indirect emissions, that is, emissions that a company causes indirectly from the energy it purchases and uses. Also do not classify articles as positive if they are only about past performance, about a corporate social responsibility (CSR) report describing past emission reductions, about other environmental measures such as waste reduction, use of recycled paper, or planting trees, or announcements by the government. If an article is empty, or does not contain enough information, classify it as negative.

Appendix 6.B: Summary Statistics

Table 6.9 provides summary statistics of the firm-level variables employed in our empirical analysis. Returns are from CRSP, using only common equity from NYSE, AMEX or NASDAQ, and the other variables are from Trucost and/or Compustat. The accounting and emission variables are reported annually. Stock returns and firm size (market cap) are measured daily. All variables are winsorized at the 1%/99% level. The average daily return is 0.02%, with a standard deviation of 3.02. Firms emit on average 2.26 million tons of CO_2 annually and 0.24 kilotons of CO_2 for every million dollars earned. We use similar firm-level control variables in our event study as Bolton and Kacperczyk (2021). These variables include: previous day size measured by log of market capitalization, book-to-market as book equity divided by market capitalization at the end of the year, leverage as total assets divided by book equity, sales growth as the 1-year growth of revenue, and return on equity as income divided by book equity of the previous fiscal year. For the controls that include accounting data, we require a publishing lag of four months when matching them with stock-level returns.

Table 6.9: Summary statistics

	Mean	SD	q1	q25	Median	q75	q99	No. Obs
Return (%)	0.02	3.02	-10.26	-1.11	0.00	1.07	11.57	44,277,215
Emissions	2.26	8.59	0.00	0.01	0.08	0.50	63.27	30,436
Log Emissions	11.25	2.92	4.17	9.42	11.32	13.12	17.96	30,436
Emission intensity	0.24	0.70	0.00	0.01	0.04	0.09	4.78	30,412
Size	12.62	2.17	7.80	11.11	12.56	14.08	17.90	44,271,554
Book-to-market	1.35	3.94	0.03	0.30	0.55	0.94	32.57	93,675
Leverage	4.13	5.13	1.04	1.50	2.22	4.08	34.38	94,234
Sales growth	0.15	0.50	-0.84	-0.03	0.07	0.20	3.44	84,244
Return on equity	-0.04	0.55	-3.24	-0.06	0.08	0.16	1.45	83,261

Summary statistics for daily returns, annual environmental measures, and accounting variables for U.S. firms. Returns are shown in percent. Emissions are measured as the sum of scope 1 and scope 2 emissions (million tons of CO_2), emission intensity is defined as the sum of scope 1 and scope 2 emissions divided by revenue, size as market capitalization, book-to-market as book equity at the previous fiscal year end divided by market capitalization at the end of the year, leverage as total assets divided by book equity, sales growth as the 1-year growth of revenue, return on equity as income divided by book equity of the previous fiscal year. Values are winsorized at the 1%/99% level. Sample period: January 2005 to December 2023.

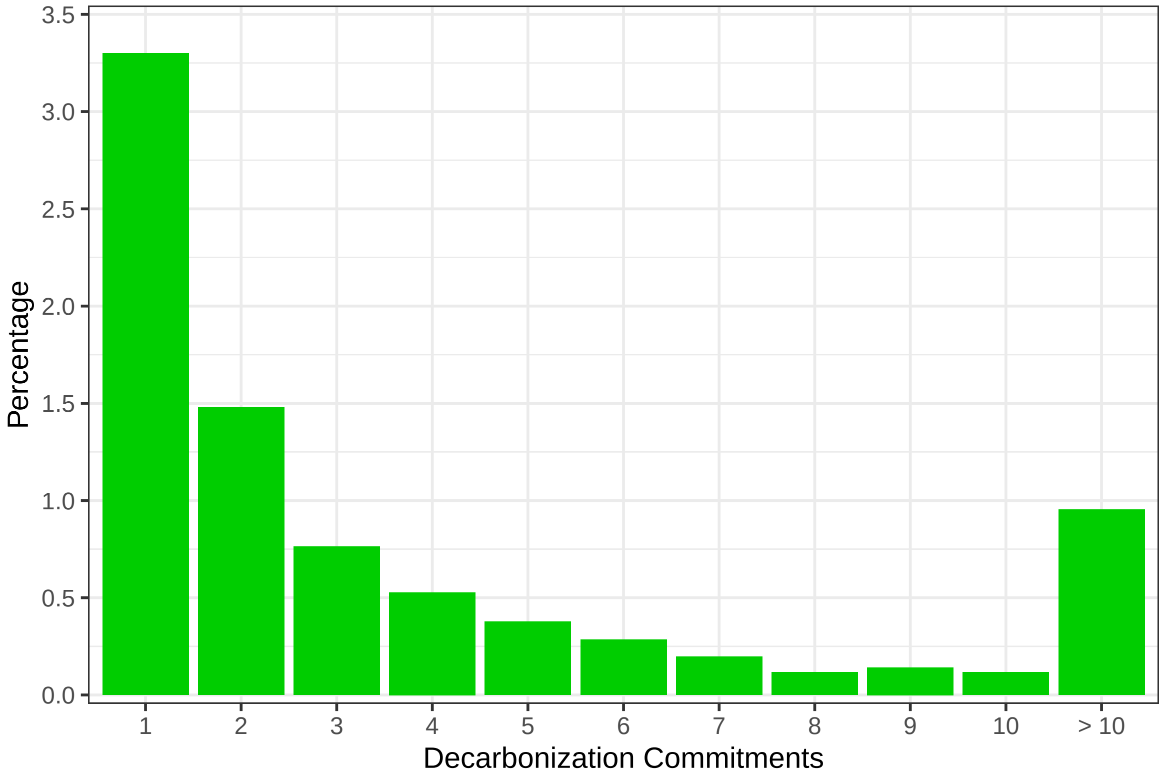
Appendix 6.C: Additional Tables and Figures

Figure 6.5 shows the percentage of firms in our sample with a given number of green pledges. About 3.3% of the firms have made one commitment over the full sample period. Roughly 1.5% of the firms have made two commitments over time and 0.75% have made three commitments. About 1% of the companies have made more than ten green pledges over time. Notably, 11,653 of U.S. firms (91.75%) have not made any green pledge at all.

Figure 6.6, panel (a) reports the average firm level emissions for each industry. For this, we first compute average firm-level emissions over time and then calculate the average across firms within the respective industry. Firms in the utilities sector have the highest emissions (12.9 million tons CO_2 on average), followed by traditional brown industries such as mining, manufacturing and transportation. Panel (b) shows emission intensities by industry, presenting a similar pattern as emission levels. Utilities has the highest emission intensity, with firms emitting on average 1.66 kilotons of CO_2 per million US dollars of revenue, followed by agriculture, mining and transportation.

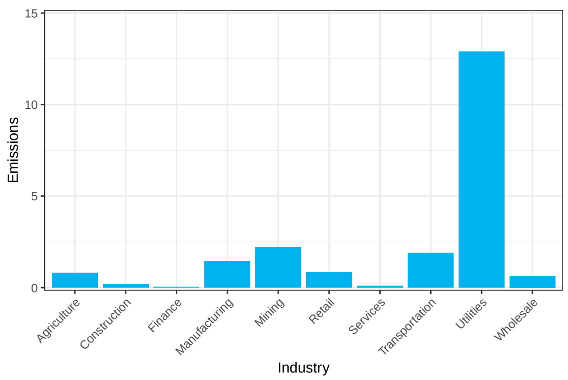
Table 6.10 shows results for the stock market response to green pledges estimated using equation (6.1) for the following two definitions of green pledges: Column (1) shows result for the sample of green pledges that only uses articles that tag a single firm. Column (2) shows results for the sample of green pledges that require at least a 30 day distance between two consecutive announcements by the same firm. The results are highly similar compared to the sample of all green pledges considered in Table 6.4 with daily stock returns being on average about 0.12% higher on the announcement

Figure 6.5: Distribution of green pledges across firms

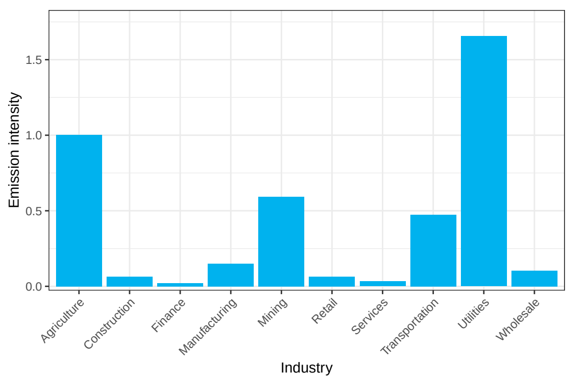


The figure shows the percentage of firms with a given number of decarbonization commitments over our sample. Sample period: January 2005 to December 2023.

Figure 6.6: Average emissions and emission intensities per industry



(a) Emisson levels



(b) Emission intensity

Emissions per industry. Panel (a) plots the average emissions and panel (b) the emission intensities for each industry, using the industry classifications of Bali et al. (2016). Emissions are measured in million tons of CO_2, emission intensity is measured in kilotons of CO_2 per million US dollars. Values are winsorized at the 1%/99% level. Sample period: January 2005 to December 2023.

day of a green pledge compared to non-announcement days and the effect is significant at the 1% level for both specifications.

Table 6.11 reports results for the stock market to green pledges for the sub-periods before and after the Paris agreement. Our findings from Section 6.4 suggest that

Table 6.10: Stock market response to green pledges - robustness

	(1)	(2)
Green pledges	0.124***	0.130***
	(0.04)	(0.04)
Book-to-market	-5.518	-5.519
	(5.07)	(5.07)
Leverage	-0.973**	-0.973**
	(0.39)	(0.39)
Size	0.005*	0.004*
	(0.00)	(0.00)
Sales growth	-13.014***	-13.014***
	(4.09)	(4.09)
Return on equity	54.660***	54.661***
	(6.68)	(6.68)
Number of Obs.	14,815,228	14,815,228
R^2	0.18	0.18
Fixed effects		
Sector	Yes	Yes
Time	Yes	Yes
Firm	No	No

Panel regressions of daily stock returns on the green pledges event dummy. Column (1) shows results for the sample of green pledges that only considers articles that tag a single firm, and column (2) shows results for the sample of green pledges that require at least a 30 day distance between two consecutive announcements by the same firm. Controls are book-to-market, leverage, size, sales growth, and return on equity. Industry fixed effects are based on 2-digit SIC codes. Standard errors, clustered by firm and time, are given in parentheses. ***, **, and * indicate significance at the 1%-, 5%-, and 10%-level, respectively. Sample period: January 2005 to December 2023.

investors require a significant premium for holding stocks that are exposed to transitional risks. The magnitude of this premium depends on two factors: the likelihood investors assign to new climate regulations as well as investors' awareness towards such risks. Both factors have likely increased since the Paris Agreement in 2015 in which most governments around the world have signed an agreement to significantly curb aggregate emission in order to keep the global surface temperature to below 2°C above pre-industrial levels. Bolton and Kacperczyk (2021) and Bolton and Kacperczyk (2023c) provide evidence that the carbon premium has increased since the Paris Agreement. Hence, if the carbon premium hypothesis holds, we should observe a particularly strong stock market reaction to green pledges after the agreement. We test this hypothesis by running our main regression (6.1) for the subsamples before and after the Paris Agreement. We find that for the sample of all green pledges, both before and after the agreement, green pledges lead to a significantly positive stock market reaction providing evidence for a significant carbon premium even in the early sample before the agreement. The coefficient on the green pledge dummy is slightly higher in the post-Paris period with lower standard errors. Hence, we find a slightly stronger stock market reaction after the Paris agreement. Note that for the sample of first pledges we find an insignificant post-Paris stock market reaction which can be explained by the majority of first pledges taking place in the pre-Paris sample.

Table 6.11: Stock market response to first green pledges before and after the Paris agreement

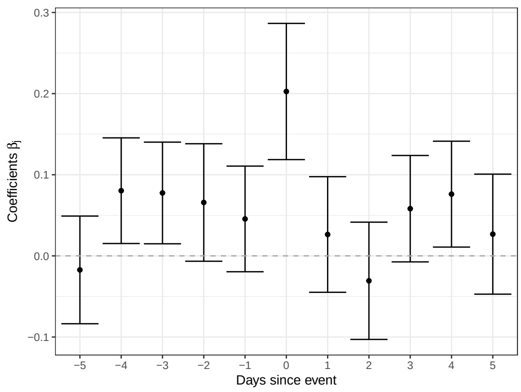
	All green pledges		First green pledges	
Model:	Pre-Paris	Post-Paris	Pre-Paris	Post-Paris
Green pledges	0.1282**	0.1413***	0.3702**	0.2263
	(0.0616)	(0.0481)	(0.1734)	(0.1380)
Book-to-market	-3.052	-11.85	-3.050	-11.81
	(6.224)	(7.578)	(6.224)	(7.579)
Sales growth	-18.40***	-8.909	-18.40***	-8.919
	(4.442)	(5.595)	(4.442)	(5.595)
Leverage	-1.151**	-0.6660	-1.151**	-0.6642
	(0.5354)	(0.5090)	(0.5354)	(0.5089)
log size	0.0015	0.0084***	0.0015	0.0084***
	(0.0035)	(0.0030)	(0.0035)	(0.0030)
Return on equity	59.20***	48.88***	59.20***	48.86***
	(7.060)	(8.576)	(7.060)	(8.575)
Fixed effects				
Industry	Yes	Yes	Yes	Yes
Time	Yes	Yes	Yes	Yes
Number of Obs.	9,278,188	5,537,040	9,278,188	5,537,040
R^2	0.17	0.20	0.17	0.20

Panel regressions of daily stock returns on the green pledges event dummy. Column (1) shows results for all green pledges for the sample period before the Paris agreement from January 1, 2005 to December 11, 2015. Column (2) shows results for all green pledges the sample period after the agreement from December 12, 2015 to December 31, 2023. Columns (3) and (4) show the corresponding results for the sample of first green pledges. Controls are book-to-market, sales growth, leverage, size, and return on equity. Industry fixed effects are based on 2-digit SIC codes. Standard errors, clustered by firm and time, are given in parentheses. ***, **, and * indicate significance at the 1%-, 5%-, and 10%-level, respectively.

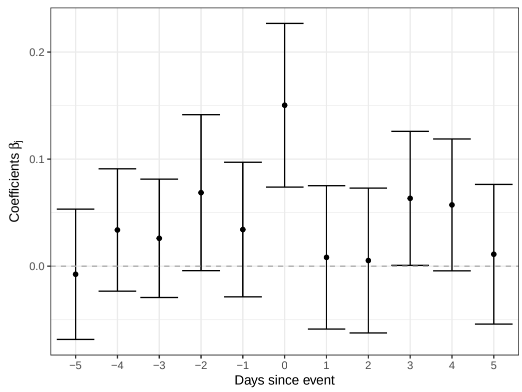
Figures 6.7 and 6.8 show the stock return response and cumulative stock return response for ±5 days around the green pledges respectively. Panel (a) shows results using the sample of all green pledges without controls and panel (b) using firm instead of industry fixed effects. Panel (c) shows results for the sample of green pledges that only tag a single firm and panel (c) for the sample that requires at least a 30 day distance between two consecutive pledges by a firm.

Table 6.12 shows the stock market reaction to first green pledges for different sorting groups. Stocks are assigned to each quintile group based on either emissions levels, emission intensities or size. We find that the positive stock market reaction to first green pledges is primarily driven by firms with high emission levels and intensities.

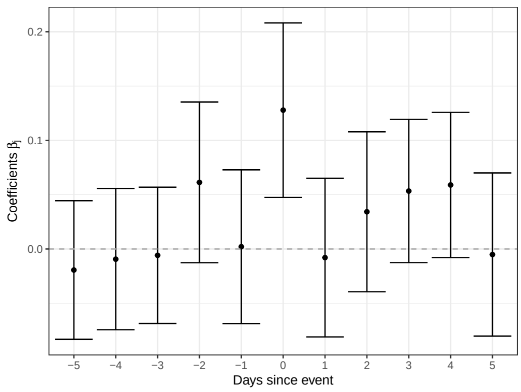
Figure 6.7: Stock return response around green pledges - robustness



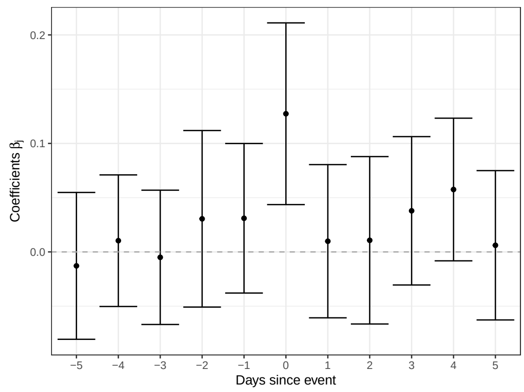
(a) No controls



(b) Firm-time fixed effects



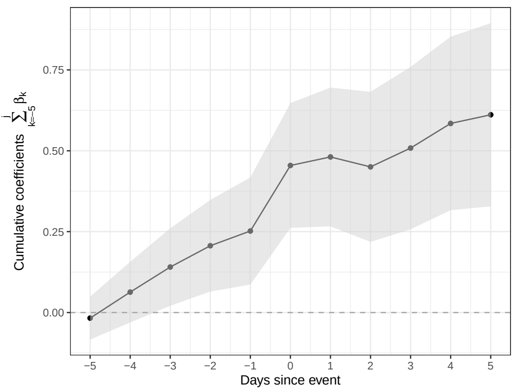
(c) Only pledges that tag a single firm



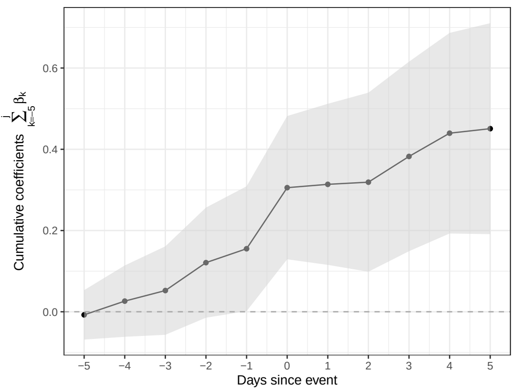
(d) 30 day distance between pledges

The figure plots the coefficients β_j of the equation (6.2) which regresses stock returns on event dummies, and five lags and leads. Panel (a) shows results using the sample of all green pledges without controls and panel (b) using firm instead of industry fixed effects. Panel (c) shows results for the sample of green pledges that only tag a single firm and panel (c) for the sample that requires at least a 30 day distance between two consecutive pledges by a firm. Controls are book-to-market, sales growth, leverage, size, and return on equity. 95% confidence intervals are estimated using standard errors clustered by firm and time and are shown around the point estimate. Sample period: December 2005 to January 2023.

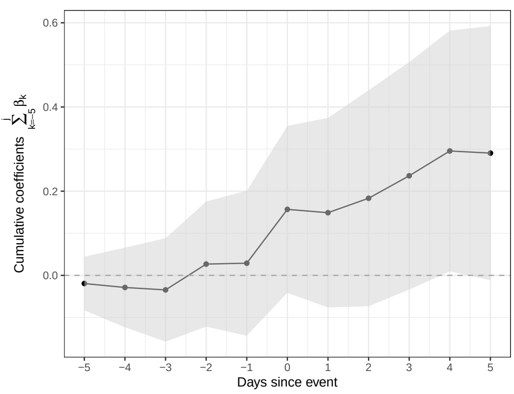
Figure 6.8: Cumulative stock return response around green pledges - robustness



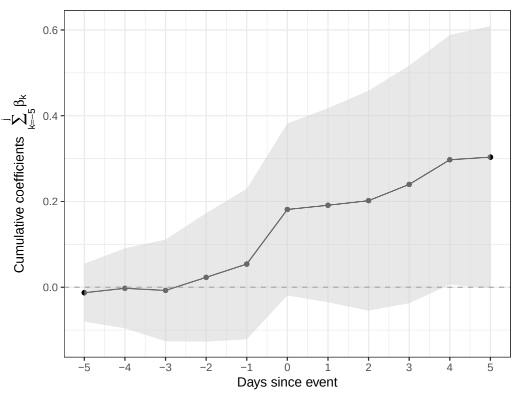
(a) No controls



(b) Firm-time fixed effects



(c) At maximum one mentioned ISIN per article



(d) At least 30 days between firm announcements

The figure plots the cumulative coefficients β_j of the equation (6.2) which regresses stock returns on event dummies, and five lags and leads. Panel (a) shows results using the sample of all green pledges without controls and panel (b) using firm instead of industry fixed effects. Panel (c) shows results for the sample of green pledges that only tag a single firm and panel (c) for the sample that requires at least a 30 day distance between two consecutive pledges by a firm. Controls are book-to-market, sales growth, leverage, size, and return on equity. 95% confidence intervals are estimated using standard errors clustered by firm and time and are shown around the point estimate. Sample period: December 2005 to January 2023.

Table 6.12: Cross-sectional response of stock returns to first green pledges

	Quintile				
	5	4	3	2	1
(A): Emissions					
Coefficient	0.337*	0.158	0.007	0.358	-0.342
Std. err.	(0.20)	(0.20)	(0.28)	(0.42)	(0.52)
R^2	0.34	0.35	0.33	0.30	0.30
Nr. of obs.	757,179	835,481	889,130	842,668	944,896
Nr. of events	162	134	82	42	13
(B): Emission intensity					
Coefficient	0.311	0.271	-0.131	0.228	0.027
Std. err.	(0.24)	(0.29)	(0.25)	(0.25)	(0.20)
R^2	0.33	0.30	0.27	0.31	0.46
Nr. of obs.	741,486	776,389	953,928	880,731	916,820
Nr. of events	139	74	73	88	59
(C): Size					
Coefficient	0.094	0.174	0.882*	0.368	1.156
Std. err.	(0.10)	(0.24)	(0.47)	(1.31)	(1.60)
R^2	0.37	0.36	0.32	0.17	0.05
Nr. of obs.	2,538,665	2,513,622	2,575,396	2,609,900	2,612,323
Nr. of events	335	132	79	20	18

This table reports the results from regressions of daily stock returns on the first green pledge event dummies for quintile group based on log carbon emission, emission intensity, and size. Controls are book-to-market, sales growth, leverage, size, and return on equity. All regressions include industry and time fixed effects. Standard errors which are clustered by firm and time in each model are given in parentheses. ***, **, and * indicate significance at the 1%-, 5%-, and 10%-level, respectively. Sample period: January 2005 to December 2023.

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Summary

High-frequency event studies, which analyze asset price movements within narrow windows surrounding policy announcements or news shocks, provide applied economists with a valuable tool to establish causality in contexts where randomized controlled trials are not feasible. Leveraging the vast growth of real-time financial data and markets' rapid assimilation of new information, this dissertation employs high-frequency event studies in combination with methodologies such as panel regressions, local projections, and difference-in-differences analysis to investigate key questions in monetary economics and climate finance.

Chapter 2 demonstrates that unexpected changes in U.S. policy rates have a stronger impact on growth stocks than on value stocks. This difference arises because growth firms' cash flows are concentrated further in the future. Longer cash-flow durations amplify the sensitivity of these firms' stock prices to changes in discount rates, causing them to decline more sharply following a monetary policy tightening. An asset pricing model reconciles these findings with the existence of the value premium. Chapter 3 focuses on the euro area, employing monetary policy surprises to examine how monetary policy influences firms' investment decisions. We leverage survey data to disentangle investment opportunities, reflected in firms' funding needs, from financial conditions, captured by the availability of external financing. This approach allows for a precise analysis of how each factor shapes the investment response to monetary policy. Our findings reveal that investment is most responsive to interest rate cuts when firms have strong fundamentals but face tight external financing conditions. This highlights the importance of the balance-sheet and bank-lending channels and underscores that the effectiveness of monetary policy ultimately depends on underlying economic fundamentals that lie beyond the direct control of central banks.

The second overarching theme of this thesis is climate economics. Chapter 4 shows that key legislative milestones associated with the Inflation Reduction Act boosted valuations of green firms while lowering those of brown firms, yet without triggering market disruptions or widespread stranded assets. Contrary to concerns that rising interest rates could hinder the green transition, Chapter 5 finds that, in the euro area, brown firms experience larger valuation losses than green firms in response to European Central Bank monetary tightening, even after controlling for factors such as size, leverage, and other firm characteristics. Finally, Chapter 6 leverages a large language model, GPT-4, to identify corporate green pledges from a vast corpus of news articles. Using a high-frequency event study around these pledges, we find that such announcements lead to lasting gains in stock prices as well as subsequent reductions in actual emissions. These results not only imply that investors reward credible climate commitments but also help to mitigate concerns about greenwashing.

Taken together, these findings offer valuable insight into the causal effects of economic policies, helping policymakers and other economic agents develop a clearer un-

derstanding of how such policies influence the economy. Moreover, this thesis demonstrates that high-frequency event studies are a powerful tool for uncovering nuanced and heterogeneous policy effects across firms, while effectively overcoming the identification challenges that often hinder low-frequency analyses.

Zusammenfassung

Hochfrequente Ereignisstudien, welche Kursbewegungen von Vermögenswerten in sehr engen Zeitfenstern um politische Ankündigungen oder Nachrichtenschocks analysieren, bieten angewandten Ökonominnen und Ökonomen ein wertvolles Instrument, um Kausalität nachzuweisen, wo randomisierte Kontrollstudien nicht umsetzbar sind. Indem sie das enorme Wachstum an Echtzeit-Finanzdaten und die rasche Informationsverarbeitung der Märkte nutzen, kombiniert diese Dissertation hochfrequente Ereignisstudien mit Methoden wie Panelregressionen, "Local Projections" und Differenz-von-Differenzen-Analysen, um zentrale Fragestellungen der Geldökonomie und der Klimafinanzforschung zu untersuchen.

Kapitel 2 zeigt, dass unerwartete Änderungen der US-Leitzinsen "Growth-Aktien" stärker beeinflussen als "Value-Aktien". Der Unterschied resultiert daraus, dass die Dividenden von "Growth-Firmen" weiter in der Zukunft liegen. Längere Dividenden-Duration verstärken ihre Sensitivität gegenüber Diskontsatzänderungen, sodass die Kurse von "Growth-Aktien" nach einer geldpolitischen Schock stärker fallen. Ein Asset-Pricing-Modell versöhnt diese Befunde mit der Existenz der Value-Prämie. Kapitel 3 richtet den Blick auf den Euroraum und nutzt geldpolitische Schocks, um zu analysieren, wie Geldpolitik die Investitionsentscheidungen von Unternehmen beeinflusst. Wir verwenden Umfragedaten, um Investitionsmöglichkeiten, abgebildet durch den Finanzierungsbedarf der Firmen, von Finanzierungsbedingungen, erfasst durch die Verfügbarkeit externer Mittel, zu trennen. So lässt sich präzise untersuchen, wie jede dieser Größen die Investitionsreaktion auf geldpolitische Veränderungen prägt. Unsere Ergebnisse zeigen, dass Investitionen am stärksten auf Zinssenkungen reagieren, wenn Unternehmen solide Fundamentaldaten aufweisen, aber unter angespannten externen Finanzierungsbedingungen leiden. Dies unterstreicht die Bedeutung des Bilanz- und Bankkreditkanals und verdeutlicht, dass die Wirksamkeit der Geldpolitik letztlich von grundlegenden wirtschaftlichen Faktoren abhängt, die außerhalb des direkten Einflussbereichs der Zentralbanken liegen.

Das zweite Leitthema dieser Arbeit betrifft die Klimaökonomie. Kapitel 4 belegt, dass zentrale gesetzgeberische Meilensteine im Zusammenhang mit dem "Inflation Reduction Act" die Bewertungen grüner Unternehmen erhöht und jene brauner Unternehmen gesenkt haben, ohne jedoch Marktverwerfungen oder weitreichend gestrandete Vermögenswerte auszulösen. Entgegen der Befürchtung, dass steigende Zinsen die grüne Transformation bremsen könnten, zeigt Kapitel 5, dass im Euroraum braune Unternehmen bei positiven geldpolitischen Shocks der Europäischen Zentralbank größere Kursverluste erleiden als grüne, selbst unter Kontrolle von Größe, Verschuldung und anderen Unternehmensmerkmalen. Kapitel 6 schließlich nutzt ein "Large Language Modell" (GPT-4), um aus einem breiten Korpus von Nachrichtenartikeln unternehmerische Klimaversprechen zu identifizieren. Mithilfe einer hochfrequenten Ereignisstudie um diese Ankündigungen stellen wir fest, dass solche Versprechen zu dauerhaften Kursgewinnen und anschließend sinkenden tatsächlichen Emissionen führen. Dies deutet darauf hin, dass Anleger glaubwürdige Versprechungen von Unternehmen zur Bekämpfung des Klimawandels belohnen und hilft, Greenwashing-Bedenken abzuschwächen.

Diese Ergebnisse liefern wertvolle Einblicke in die kausalen Effekte wirtschaftspolitischer Maßnahmen und unterstützen Entscheidungsträger und andere ökonomische Akteure dabei, besser zu verstehen, wie solche Maßnahmen die Volkswirtschaft beeinflussen. Zudem zeigt die Dissertation, dass hochfrequente Ereignisstudien ein leistungsfähiges Instrument sind, um subtile und heterogene Politikeffekte auf Unternehmensebene aufzudecken und gleichzeitig die Identifikationsprobleme zu überwinden, die Analysen mit niedrigeren Frequenzen häufig behindern.

Liste der aus dieser Dissertation hervorgegangenen Veröffentlichungen

Bauer, Michael D., Eric A. Offner, and Glenn D. Rudebusch (2025) “Green Stocks and Monetary Policy Shocks: Evidence from Europe,“ *European Economic Review*, 105044.

Bauer, Michael D., Eric A. Offner, and Glenn D. Rudebusch (2025) “The effect of U.S. climate policy on financial markets: An event study of the Inflation Reduction Act,“ forthcoming in *Advances in Econometrics*

Erklärung

Hiermit erkläre ich, Eric Athayde Offner, dass ich keine kommerzielle Promotionsberatung in Anspruch genommen habe. Die Arbeit wurde nicht schon einmal in einem früheren Promotionsverfahren angenommen oder als ungenügend beurteilt.

Hamburg, den 7. Juli 2025

Eric Athayde Offner

Selbstdeklaration

Für Kapitel 2 liegt die Eigenleistung bei 100%.

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Eric Athayde Offner

Eidenstaatliche Versicherung

Ich, Eric Athayde Offner, versichere an Eides statt, dass ich die Dissertation mit dem Titel: "Applications of High-Frequency Event Studies: Policy Evaluation and Financial Analysis" selbst und bei einer Zusammenarbeit mit anderen Wissenschaftlerinnen oder Wissenschaftlern gemäß den beigefügten Darlegungen nach § 6 Abs. 3 der Promotionsordnung der Fakultät für Wirtschafts- und Sozialwissenschaften vom 18. Januar 2017 verfasst habe. Andere als die angegebenen Hilfsmittel habe ich nicht benutzt.

Hamburg, den 7. Juli 2025

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