

Essays on Technological Innovation, Capital Investment and Economic Growth

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Abstract

This thesis consists of three chapters on technological innovation, capital investment and economic growth. The first chapter studies the effects of Hurricane Katrina — one of the deadliest and costliest storms in U.S. history — on inventors' innovation outcomes, particularly green innovation which helps combat climate change. Using hazard and patent data for U.S. inventors in a difference-in-differences framework, I find Katrina significantly reduced inventors' green patent production, leaving non-green patents unaffected. Specifically, affected inventors could have produced 24.6% more green patents within a three-year period after 2005, had Katrina not occurred. The negative effects are more pronounced for patent output with greater knowledge diversity and for inventors who had collaborated with more technologically distant peers prior to the disaster. These findings suggest that knowledge diversity may compromise the resilience of technological development in the face of disruption.

The second chapter investigates how checks and balances — an essential democratic feature — impact corporate investment. Exploiting shifts between unified and divided U.S. government resulting from presidential and congressional elections, I show firms reduce investment rates by an average of 13.9% under unified governments relative to divided governments. The effect is stronger for firms reliant on government contracts, operating in less competitive markets, and facing greater investment irreversibility. Results align with the hypothesis that unified governments introduce higher political uncertainty due to their greater legislative power. This suggests divided governments could bolster economic performance by stabilizing investment environments.

Chapter three explores how state leaders' political party affiliations affect economic growth in the U.S., using a regression discontinuity design around close gubernatorial elections. Republican governors significantly boost short-term state real GDP per capita and real GDP growth by 2.0 and 2.6 percentage points annually, respectively. These results underscore that leaders' political affiliations matter significantly for economic outcomes, suggesting election results have tangible, causal impacts on economic growth.

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Declaration

I, Yuan Hu, confirm that the research included within this thesis is my own work. The three chapters of the thesis are all single-authored, and all other material is appropriately referenced.

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Date: 8 August 2025

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Chapter 1

Introduction

Technological innovation, capital investment, and economic growth are central to addressing the pressing economic and environmental challenges of our time. These forces not only shape the prosperity and well-being of societies, but also underpin their ability to respond to complex global developments, such as globalization, environmental degradation, and shifting patterns of production and consumption. In an era defined by rapid change and increasing uncertainty, understanding what drives or impedes these economic activities is crucial for crafting effective policies and building resilient, dynamic economies.

This thesis comprises three distinct yet interconnected chapters that examine how external disruptions, political institutions, and political leadership influence economic activities such as technological innovation and capital investment, thereby affecting overall economic growth. By exploring these interconnected themes, the thesis provides valuable insights into how economic actors respond to changes in their external environments and institutional contexts, as well as how political dynamics at various levels shape the trajectory of economic development.

The first chapter investigates the impact of climate-related disruptions, specifically natural disasters, on technological innovation, with a focus on green technologies. Climate change is increasingly leading to more intense and destructive natural disasters, such as hurricanes, which have significant economic and societal impacts. These events not only disrupt daily life but also have the potential to influence human innovation activities, particularly in the realm of green technology. Green innovation, which focuses on developing technologies to mitigate or adapt to climate change, is crucial for transitioning to a sustainable economy. However, while existing research has explored how disasters affect firms or regional economies, the role of individual inventors — key agents in knowledge creation — remains overlooked.

Individual inventors contribute specialized expertise and creativity to solve complex environmental challenges, often collaborating in diverse teams to integrate cross-disciplinary knowledge. Yet natural disasters, like Hurricane Katrina,

may uniquely disrupt their productivity by destroying infrastructure, displacing talent, or fracturing collaborative networks. For green innovation, which inherently relies on integrating diverse technological knowledge and expertise (Barbieri et al. 2020; Fusillo 2023), such disruptions could be particularly detrimental. This study investigates whether and how Hurricane Katrina, the costliest and one of the deadliest storms in US history, differentially impacted individual inventors' production of green versus non-green patents, addressing a critical gap in understanding how climate-driven disasters shape the trajectory of climate solutions.

Using detailed patent data from the United States Patent and Trademark Office (USPTO) linked with geographic hazard exposure information from the Spatial Hazard Events and Losses Database for the United States (SHELDUS), the chapter employs a rigorous difference-in-differences empirical strategy combined with a one-to-one exact matching method. Inventors located in Katrina-affected areas form the treatment group, while inventors from unaffected regions who were identical to the treated inventors in terms of patenting trajectory before Katrina constitute the control group. The empirical analysis suggests that Hurricane Katrina hindered the growth of green patents. Specifically, if Hurricane Katrina had not happened, a treated inventor could have produced 24.6% more green patents within a three-year period after 2005. Importantly, this effect is specific to green innovation, as non-green patenting remained unaffected. Further analysis reveals that this distinct vulnerability of green innovation seems to be driven by its reliance on knowledge diversity and specialized, technologically distant expertise within inventor teams. Green innovation projects, which typically integrate diverse technological domains, experience greater difficulty in substituting the expertise of inventors who become unavailable due to disruptions. Additional evidence suggests that firms do not play a significant role behind the differential effects observed from Katrina.

These findings underscore a critical paradox: climate-related natural disasters can hinder precisely the innovation needed to address climate challenges, potentially creating a vicious cycle given the anticipated increase in severe climate-related events. This work also strengthens the case for subsidies and public investments in green innovation, which face the dual externality problem of knowledge spillovers and positive environmental externalities. While my findings suggest that green innovation is particularly vulnerable to disruption, further research is needed to fully understand which policy responses are most effective. Policymakers should therefore be aware of this vulnerability and, if they have the knowledge, implement targeted measures to support inventors and innovation teams in the aftermath of natural disasters. For firms and organizations, it is important to balance the benefits and costs of team diversity, as diversity can foster creativity and novelty but may also increase vulnerability to external shocks.

The second chapter turns attention toward institutional influences on corporate investment behavior, specifically focusing on how checks and balances, a critical dimension of democratic governance, impact firms' investment decisions. The

longstanding debate in political economy and development economics centers on whether democratic systems support or hinder economic growth. While some argue that democracy’s ability to constrain political power holders and guarantee property rights fosters an environment conducive to investment, others contend that autocratic governments may be better positioned to enact necessary but politically unpopular reforms. Existing empirical studies typically compare economic outcomes across democracies and autocracies using broad, binary measures, potentially overlooking the institutional mechanisms that operate within democracies themselves.

This chapter addresses this gap by examining how the presence or absence of checks and balances within a democracy affects corporate investment decisions. Focusing on the partisan structure of the U.S. federal government, it analyses how unified versus divided governments — where one or both branches are controlled by a single party — shape firms’ investment behavior. A unified government may be more capable of passing new legislation, creating policy uncertainty that discourages investment, while a divided government, where political power is balanced, can reduce uncertainty by constraining sweeping policy shifts.

To investigate this, I use data on the partisan composition of the U.S. federal government alongside firm-level investment data from 1977 to 2020. Using a classical investment-Q framework, the empirical analysis compares firm investment rates under unified and divided governments, controlling for firm-level growth opportunities, cash flow, and other macroeconomic factors. We also explore heterogeneity by examining whether firms more dependent on government contracts, operating in less competitive markets, or with more irreversible investment structures respond differently to political uncertainty under unified governments.

The findings reveal a strong negative relationship between unified government and corporate investment, with firm investment expenditures declining by an average of 13.9% over a one-quarter period under unified governments. This dampening effect persists even after accounting for presidential elections and is more pronounced at the federal level than at the state level, suggesting that concentrated political power at the federal level introduces significant policy uncertainty. The negative impact is especially strong for firms highly dependent on government contracts, operating in less competitive industries, and with more irreversible investments, underscoring the role of political uncertainty as the key mechanism.

These results highlight an important paradox within democratic systems: while unified governments may have greater capacity to implement policy changes, the absence of checks and balances can create uncertainty that deters private investment. Conversely, divided governments — where political power is shared — appear to foster a more stable environment for investment by reducing policy uncertainty. These findings offer micro-level evidence of how democratic institutions like checks and balances can promote economic growth.

The third chapter examines how political leadership, specifically the partisan

affiliation of state governors, shapes economic growth at the subnational level within the United States. While previous research has demonstrated that individual leaders can have large impacts on growth in autocracies — where institutional constraints are limited — less is known about the extent to which leaders matter in democratic regimes where institutions are typically stronger. However, some evidence suggests that even in democracies, leaders’ political party affiliations can influence economic performance. For example, studies have noted that the U.S. economy has historically grown faster under Democratic presidents, though causality in these findings remains debated given the strong predictive role of macroeconomic conditions in electoral outcomes.

In this chapter, I contribute to this debate by investigating whether the political party affiliation of state governors has a causal impact on state-level economic growth. Governors in the U.S. act as chief executives within their states, wielding considerable autonomy and power over economic and policy decisions. With fifty states and frequent variation in gubernatorial partisan control, the U.S. setting provides a unique opportunity to isolate the economic effects of political leadership within a democratic system.

To identify the causal effect of gubernatorial partisanship, I employ a sharp regression discontinuity design that uses close elections as a natural experiment. This approach allows me to estimate the local average treatment effect of a Republican versus Democratic governor on state economic performance, under the assumption that outcomes of close elections are as good as random. I compile a dataset covering 588 gubernatorial elections, paired with state-level data on real GDP per capita and real GDP growth, to assess whether political party affiliation at the state level translates into meaningful differences in economic outcomes.

The empirical analysis reveals that Republican governors lead to significantly higher state economic growth than Democratic governors. Specifically, I find that average annualized real GDP per capita growth under Republican governors is 2.0 percentage points higher than under Democratic governors. This treatment effect is both statistically and economically significant — equivalent to 143 percent of the mean growth rate in the close-election sample. These findings highlight that leaders, and more precisely their partisan affiliations, do matter for economic growth in democratic settings. Moreover, the results contrast with national-level findings showing higher growth under Democratic presidents, suggesting that the effects of partisan leadership differ across levels of government.

These results underscore the important role that partisan leadership at the subnational level plays in shaping economic growth. They also contribute to broader debates about the economic consequences of election outcomes, suggesting that the effects of political turnover can extend beyond the federal level to state governance. For policymakers and scholars, this work highlights the importance of considering how partisanship and leadership dynamics at the state level shape economic trajec-

tories and performance.

Taken together, the three chapters of this thesis provide complementary perspectives on how climate change and natural disasters, as well as political institutions and leadership, shape innovation, investment, and economic growth. The results highlight that economic outcomes are not only shaped by market forces or individual firm decisions, but are also deeply influenced by external disruptions and the broader institutional and political environments in which firms and inventors operate. By examining these issues from several angles, the thesis offers a more integrated view of the challenges facing modern economies.

Overall, my findings underscore the importance of considering both environmental and political factors when analysing or designing policies to support innovation and growth. Building resilience in innovation systems, ensuring stable and balanced political institutions, and recognising the influence of political leadership all emerge as important for fostering sustainable economic performance. While the thesis makes progress in understanding these dynamics, it also points to the need for further research on the longer-term impacts of political and environmental shocks, and on the effectiveness of policies aimed at mitigating these challenges.

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Chapter 2

Green Technological Change After Natural Disasters: Evidence from Hurricane Katrina

Abstract

This paper studies the effects of Hurricane Katrina — the costliest storm in U.S. history — on inventors’ innovation outcomes, in particular green innovation which helps combat climate change. Using hazard and patent data for U.S. inventors in a difference-in-differences design, I find that Katrina reduced inventors’ production of green patents, while non-green patents were unaffected. This indicates that climate change can disrupt human efforts aimed at addressing it. The negative effects are more pronounced for patent output with greater knowledge diversity and for inventors who had collaborated with more technologically distant peers prior to the disaster. These findings suggest that the diversity of expertise within innovation teams, while beneficial for knowledge creation, may reduce the substitutability of affected inventors and undermine the resilience of technological development in the face of disruption.

2.1 Introduction

Scientific evidence suggests that climate change has led to the increased intensity of natural disasters.¹ Hurricanes, in particular, are becoming stronger and more destructive,² which can significantly disrupt the creation of new technological or scientific knowledge. Innovation is important for productivity growth, and green innovation is especially crucial for the combat against climate change and tran-

¹See, for example, Intergovernmental Panel on Climate Change (IPCC) 2023.

²See, for instance, Emanuel 2005; Nordhaus 2006.

sition to sustainable economy.³ Existing theoretical frameworks (e.g., Acemoglu, Aghion, Bursztyn, and Hemous 2012) usually treat environmental quality as exogenous to both green and non-green innovation supply, i.e., there is no feedback channel from environmental degradation and disasters to technological development. A considerable body of research also focuses on how the price mechanism can direct resources from non-green to green innovation to tackle climate and environmental challenges (e.g., Acemoglu et al. 2012; Aghion, Dechezleprêtre, Hémous, Martin, and Van Reenen 2016; Bøler, Holtmark, and Ulltveit-Moe; Dugoua and Gerarden 2024). However, there is limited understanding of how climate change and extreme weather events, such as hurricanes, affect green and non-green innovation, specifically whether they are impacted in similar or different ways.

In this paper, I investigate the effects of natural disasters on inventors' green and non-green innovation production. Individual inventors play a pivotal role in the production of innovation, contributing their specialized knowledge and skills to create new technologies. While natural disasters may raise inventors' environmental awareness and potentially encourage green technological development, they also pose significant disruptions that could hinder the production of both green and non-green innovation. Previous research has focused on the disruptions to knowledge production caused by the deaths of scientists and inventors, and consistently found that such events negatively impact collaborators' overall innovation productivity and income (Azoulay, Graff Zivin, and Wang 2010; Oettl 2012; Jaravel, Petkova, and Bell 2018; Mohnen 2018). However, these studies have not accounted for the distribution of expertise within teams,⁴ a key factor in determining the resilience of innovation outcomes in the face of disruptions.

Climate-related disasters and extreme weather events may have differential impacts on inventors' production of green and non-green innovations. On one hand, these events can increase inventors' environmental awareness, potentially motivating greater engagement in green innovation while leaving non-green innovation relatively unaffected. On the other hand, the destructive impact of such disasters on inventors' homes and workplaces may significantly constrain their research efforts and disrupt their innovative activities, ultimately reducing overall innovation output.

The negative impact on innovation output may be more severe for green innovation, which inherently differs from its non-green counterpart. According to the World Bank,⁵ green technology tends to be more complex and multifaceted than its non-green counterparts, spanning across all economic sectors. Academic research

³Green innovation generally refers to technologies that reduce environmental burdens and improve environmental performance (Rennings 2000; Popp, Newell, and Jaffe 2010; Popp 2019). In this paper, I specifically focus on technologies and applications for mitigating or adapting to climate change.

⁴Azoulay, Graff Zivin, and Wang (2010) considers the intellectual distance in terms of research topics between superstars and their collaborators but does not examine the distribution of expertise within teams.

⁵See, Pigato, Black, Dussaux, Mao, McKenna, Pan, Toubal, and Zenghelis (2020).

further shows that green innovation combines knowledge from a wider range of technological areas, compared to non-green counterparts,⁶ often requiring teams of individual inventors with more diverse expertise. This greater diversity can increase the technological distance among collaborators.⁷ If a disruption event, like a natural disaster, strikes an individual inventor within a green innovation team, it might be harder to substitute so as to continue the affected inventor’s technological tasks. Therefore, the development of green technology may be more negatively affected than non-green innovation, where the technological expertise among team members tends to be more similar and substitutable.

The natural disaster I use to identify these effects is Hurricane Katrina. The 2005 storm, that resulted in \$200 billion in damages, is the costliest tropical cyclone in the US history.⁸ I use data on individual inventors’ location and patenting activities from the United States Patent and Trademark Office (USPTO), combined with data on county-level property damage related to Katrina from the Spatial Hazard Events and Losses Database for the United States (SHELDUS). Therefore, this merged data from PatentsView and SHELDUS allows me to identify inventors who likely lived and worked in counties (Akcigit, Baslandze and Stantcheva 2016; Moretti and Wilson 2017; Akcigit, Grigsby, Nicholas, and Stantcheva 2022) affected by Katrina at the time of the storm (treatment group), and to track their patent production activities, including green patents.

Using a one-to-one exact matching method, I construct a control group of patent inventors. These inventors did not list Katrina-affected counties as their addresses on patent applications filed before and around the time of the storm. However, they were otherwise identical to the treated inventors in terms of the year of their first patent filing, cumulative patent count, and cumulative green patent count prior to Katrina (Jaravel, Petkova, and Bell 2018). The causal impact of Hurricane Katrina is identified in a difference-in-differences research design, comparing changes in patenting activities at inventor level before and after the storm between the treatment and control group.

I find that Hurricane Katrina hindered the growth of green patents, while non-green patents remained unaffected. Specifically, if Hurricane Katrina had not happened, a treated inventor could have produced 24.6% more green patents within a three-year period after 2005.⁹ I find similar effects of Katrina at extensive and intensive margin for green and non-green patents. The results are robust to: 1. Controlling

⁶See, De Marchi 2012; Barbieri, Marzucchi, and Rizzo 2020; Orsatti, Quatraro, and Pezzoni 2020; Fusillo 2023.

⁷Azoulay et al. (2010) distinguish co-authors of an academic superstar by the intellectual distance that defines how closely or distantly their research topics relate to those of the superstar.

⁸The figures are sourced from NOAA National Centers for Environmental Information (NCEI) U.S. Billion-Dollar Weather and Climate Disasters (2024). Costs are adjusted based on the 2024 Consumer Price Index.

⁹The outcome variable is measured as the number of patents filed over a three-year window. For details, see Section 2.3.3.

for the geographical distance of sample inventors with co-inventors; 2. Using an alternative definition of green patents, based on the OECD ENV-TECH classification (Haščič and Migotto 2015); 3. Not restricting control inventors to the South US; 4. Excluding solo-inventor patents. In addition, I examine whether inventors who migrated after Katrina experienced different effects compared to those who did not migrate. The analysis finds no significant differential effects between migrant and non-migrant inventors. The findings suggests that natural disasters could impede the green transition by adversely affecting inventors' production of green innovation, despite their potential to enhance environmental awareness and accelerate the development of green technology. Furthermore, this raises concerns about a vicious cycle, considering the potential increase in severe climate-related natural disasters and the importance of green innovation in combating climate change.

In the second part of the analysis, I investigate the mechanism behind this observed differential effects on green and non-green innovation. Specifically, I test the hypothesis that there is a larger technological distance among inventors within a green innovation team compared to a non-green innovation team. Despite the limitation of using patent data to measure innovation, it allows me to measure technological distance within a patent team. Following Akcigit et al.(2022), I measure an inventor's technological expertise with the modal CPC subclass based on her patenting history before Katrina. Using the co-occurrence approach (Hidalgo, Klinger, Barabási, and Hausmann 2007; Azoulay et al. 2010), I construct a distance metric in technological expertise. Consistent with the hypothesis, I find that the average technological distance between a sample inventor and her collaborators is larger in a green patent team than in a non-green one. Moreover, the negative impacts of Katrina are more pronounced for patents produced by teams with greater technological distance, and for inventors with more technologically distant collaborators before Katrina. Additionally, I investigate the role of firms behind the differential effects and find that Hurricane Katrina did not have differential effects on firms green and non-green innovation. These findings imply that new technologies, such as green innovation, which rely on knowledge diversity for their development, may be more vulnerable to external shocks, such as natural disasters related to climate change.

This paper contributes to several strands of literature; the first of which is the research on economic impacts of natural disasters. An extensive literature has examined the impact of extreme weather events and disasters on a range of outcomes, including aggregate economic activity at the country level (Dell, Jones, and Olken 2012; Acevedo, Mrkaic, Novta, Pugacheva, and Topalova 2020), local economies (Elliott, Strobl, and Sun 2015; Kocornik-Mina, McDermott, Michaels, and Rauch 2020), and firm-level performance (Somanathan, Somanathan, Sudarshan, and Tewari 2021; Keiller and Van Reenen 2024). More specifically, my work is situated among a number of papers that analyse the impacts of natural disasters, on innovation. Most related to this paper is Ballesteros (2022), which explores the impact of Hurricane Katrina on county-level innovation, focusing on changes in risk

attitudes. In contrast, this paper examines the effect on green and non-green innovation at the individual inventor level, uncovering a negative effect on green innovation but no significant impact on non-green innovation. This study also introduces a novel mechanism based on differences between the two types of innovation.

My study on the role of natural disasters shaping individual inventors’ technological direction complements the large literature on directed technological change and the environment. While existing work has mostly focused on the price mechanism to direct resources from non-green to green innovation (Acemoglu et al. 2012; Aghion et al. 2016; Bøler, Holtmark, and Ulltveit-Moe; Dugoua and Gerarden 2024) to address climate and environmental challenges, my focus is on how climate change and related natural disasters influence technological direction. In this vein, Miao and Popp (2014) find that floods, droughts and earthquakes lead to more innovation of their respective mitigation technologies at the country level; Moscona and Sastry (2022) find that agricultural innovation has shifted towards crops more exposed to extreme temperatures at the county level since the mid-20th century; According to Moscona (2022), agricultural technology development at county level persistently re-directed toward more Dust Bowl-exposed crops in the wake of the American Dust Bowl. Different from these papers, I find that Hurricane Katrina not only failed to encourage green innovation but actually reduced inventors’ green innovation output, while non-green innovation remained unaffected.

Most close to my work is Zhao, Zheng, and Fu (2022), which finds that natural disasters are negatively associated with green innovation in the energy sector at the country level, but shows no correlation with non-green energy innovation. Unlike Zhao et al. (2022), my paper focuses on the causal impact of a specific natural disaster — Hurricane Katrina — on general green innovation at the individual inventor level. Furthermore, Zhao et al. (2022) propose various macro channels through which natural disasters may impact innovation, including economic development level, trade and financial openness, interpersonal globalization, and government stability. This paper introduces a new micro-level channel – team members in green projects are less substitutable due to green innovation’s dependence on broader knowledge diversity and a wider range of team members’ specialized technological expertise. This distinctive dependency suggests that inventors’ green innovation output was more negatively impacted by the disruption caused by Hurricane Katrina.

Second, this paper also contributes to the growing literature on scientists and inventors that sheds light on the supply of innovation (e.g., Goolsbee 1998; Romer 2000; Bell, Chetty, Jaravel, Petkova, and Van Reenen 2019). Previous research has primarily explored micro-level disruptions to knowledge production, such as the death of collaborators, consistently uncovering detrimental effects on individual inventors’ productivity. For instance, Azoulay et al. (2010) find a lasting decline in the quality-adjusted publications of coauthors following the death of a superstar collaborator. Similarly, Oettl (2012) extends the definition of star collaborators to encompass productivity and helpfulness, and finds that the death of such individ-

uals leads to a reduction in the output quality of surviving coauthors compared to the death of non-star collaborators. Furthermore, Jaravel et al. (2018) documents a significant long-term drop in co-inventors' earnings and citation-weighted patents following an inventor's premature death. The predominant explanation in this body of work revolves around knowledge spillovers.

Departing from existing literature, this paper investigates the impact of a macro-level shock — specifically, Hurricane Katrina — and its differential effects on green versus non-green patent production by individual inventors. My findings reveal that Katrina significantly reduced inventors' green patent outputs, while non-green patents remained unaffected. This paper also identifies a novel mechanism of knowledge substitutability – an inventor's technological expertise in green projects are less substitutable due to green innovation's dependence on broader knowledge diversity and a wider range of team members' specialized technological expertise. This highlights a unique aspect of how disruptions to knowledge production can differentially impact innovation, contingent on the diversity of knowledge areas and the inherent technological characteristics of the team involved.

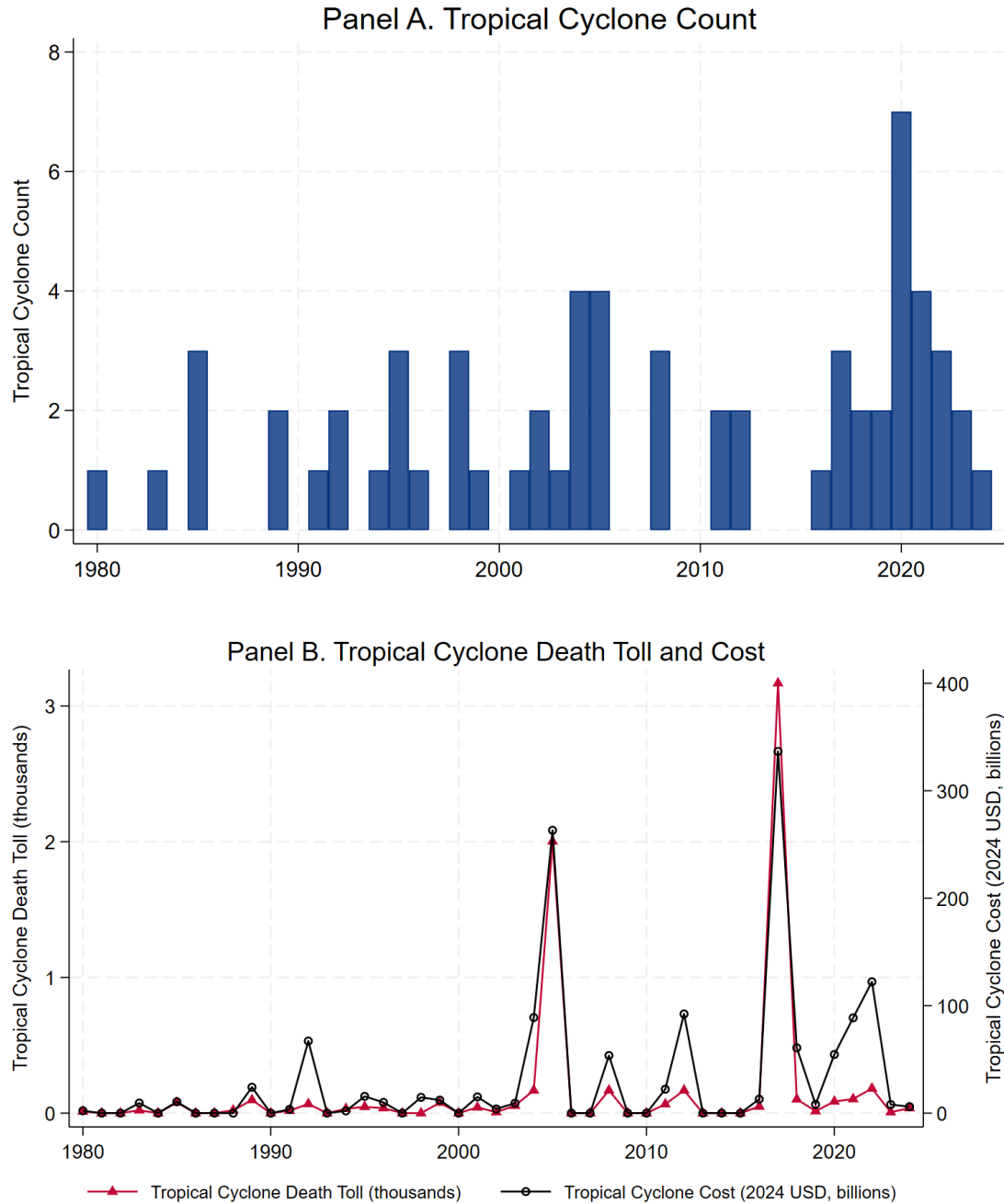
Third, this paper also adds to the literature on labour responses to climate change and weather shocks. The existing research focuses on workers with high exposure to these environmental changes, particularly in the agricultural sector in developing countries, exploring how they respond through geographical migration or sectoral reallocation. For instance, Jayachandran (2006) finds that agricultural workers tend to migrate away from regions affected by negative productivity shocks due to rainfall fluctuations. Colmer (2021) shows that rising temperatures, which reduce agricultural productivity, prompt labour shifts from agriculture to manufacturing and services within the same district. While much of this literature highlights climate inequality, where lower-skilled, high-exposure workers bear the brunt of environmental shocks, my work demonstrates that highly-skilled labor, such as patent inventors, can also be significantly affected.

The paper is structured as follows. Section 2.2 provides background information on Hurricane Katrina. Section 2.3 describes the data, empirical design and some key summary statistics. Section 2.4 presents the empirical results. In section 2.5, I establish that greater knowledge diversity and technological distance within a green innovation team is one important explanatory channel. Section 2.6 discusses and tests the alternative mechanism operating at firm level. Section 2.7 concludes.

2.2 Background on Hurricane Katrina

Climate change is leading to more destructive hurricanes, which can generate great economic and societal impacts. A hurricane is a tropical cyclone with maximum

Figure 2.1 UNITED STATES BILLION-DOLLAR TROPICAL CYCLONE EVENTS 1980-2024



Notes: Panels A and B of this figure show the trends in the number, death count, and cost of billion-dollar tropical cyclones affecting the United States. The data is sourced from the National Oceanic and Atmospheric Administration (NOAA) National Centers for Environmental Information (NCEI) — Billion-Dollar Weather and Climate Disasters (<https://www.ncei.noaa.gov/access/billions/>). This dataset includes only weather and climate disasters with losses exceeding one billion dollars, adjusted for the 2024 Consumer Price Index.

sustained winds of 74 mph (64 knots) or higher.¹⁰ Figure 2.1 shows the increasing frequency rising death tolls, and escalating monetary losses of billion-dollar tropical cyclones in the United States between 1980 and 2024. According to NOAA National Centers for Environmental Information (NCEI) U.S. Billion-Dollar Weather and Climate Disasters (2024), the distribution of damage from U.S. Billion-dollar disaster events from 1980 to 2024 is dominated by tropical cyclone losses. Tropical cyclones have caused the most damage (\$1,418.2 billion, 2024 Consumer Price Index adjusted) and also have the highest average event cost (\$22.5 billion per event, 2024 Consumer Price Index adjusted). Tropical cyclones are also responsible for the highest number of deaths (6,934).

Hurricane Katrina is the costliest tropical cyclone in U.S. history, incurring \$200 billion in damages according to NOAA.¹¹ It is also the second deadliest since 1930, claiming 1,833 lives.¹² It developed from a tropical depression southeast of the Bahamas, making its first landfall on August 25th in southern Florida near the Miami area.¹³ After crossing southern Florida, Katrina intensified into a Category 5 hurricane on August 28th, located approximately 250 miles south-southeast of the Mississippi River’s mouth, with peak winds of 175 miles per hour. By August 29th, when it reached the Louisiana coast, it was a strong Category 3 hurricane with winds exceeding 100 miles per hour, particularly impacting New Orleans. As Katrina moved north-northeast across Mississippi, it gradually weakened but remained at hurricane strength up to 100 miles inland near Laurel, Mississippi. The storm finally downgraded to a tropical depression near Clarksville, Tennessee, on August 30th.

The storm caused extensive storm surge damage, with surges possibly exceeding 30 feet along the coasts of Louisiana, Mississippi, and Alabama. Additionally, it led to significant wind damage and the failure of parts of the New Orleans levee system. Inland effects included high winds and some flooding across Alabama, Mississippi, Florida, Tennessee, Kentucky, Indiana, Ohio, and Georgia. Appendix Figure A2.1 illustrates the geographical distribution of property damage from Katrina, primarily clustered in the Southern United States, with damage extending to northeast areas.¹⁴

The infrastructure damage from Hurricane Katrina was extensive, with over

¹⁰See, <https://www.nhc.noaa.gov/climo/>

¹¹See NOAA National Centers for Environmental Information (NCEI) U.S. Billion-Dollar Weather and Climate Disasters (2024). Dollar amounts are adjusted based on the 2024 Consumer Price Index.

¹²See publications of the National Oceanic and Atmospheric Administration. (Blake, Landsea, and Gibney 2011; NOAA National Centers for Environmental Information (NCEI) U.S. Billion-Dollar Weather and Climate Disasters 2024)

¹³The basic facts about Hurricane Katrina cited throughout are from publications of the National Oceanic and Atmospheric Administration. (Graumann, Houston, Lawrimore, Levinson, Lott, McCown, Stephens, and Wuertz 2005; NOAA National Centers for Environmental Information 2005)

¹⁴For example, the remnants of Hurricane Katrina, combined with an approaching cold front, led to strong winds across Eastern Massachusetts. These damaging wind gusts resulted in fallen tree branches and wires in Hingham, and a tree was also knocked onto power lines in Haverhill. (NOAA National Centers for Environmental Information 2005)

300,000 homes destroyed.¹⁵ Nearly two years later, over 600,000 individuals had yet to return to their homes in the broader affected areas (Deryugina, Kawano and Levitt 2017). Roads and bridges suffered severe structural damage, and critical public infrastructure, including hospitals, schools, and power stations, was significantly affected.

Beyond the devastation to residential and transportation infrastructure, Hurricane Katrina inflicted severe disruptions on business operations and innovation activities. For example, the storm forced the shutdown of offshore oil and gas production, disrupted petrochemical plant operations, and paralyzed rail, truck, and barge transportation across Mississippi, Alabama, and Louisiana. Similarly, the Southern Regional Research Center (SRRC), an agricultural research laboratory operated by the U.S. Department of Agriculture in New Orleans, suffered approximately \$35 million in damages. The facility was closed for a year, necessitating the relocation of about 170 staff members, and only restored full operations two years after the storm. The forced closure disrupted ongoing agricultural research, causing setbacks in the development of new technologies for crop production and pest control.

2.3 Data, Empirical Design and Descriptive Statistics

In this section, I describe the various datasets I use, outline my empirical design, and provide some basic descriptive statistics.

2.3.1 Data Description

Inventors, Patents and Green Patents. — I obtain the disambiguated inventor data from PatentsView,¹⁶ which identifies unique inventors in the USPTO data. This dataset provides comprehensive information on inventors, including their patent number identifiers and geographical locations listed on patent applications. The granular geographical information, encompassing details such as country, state, city, and latitude and longitude coordinates, is likely to reflect the actual location where an inventor lives and works (Akcigit et al. 2016; Moretti and Wilson 2017; Akcigit et al. 2022). To get information on each patent’s characteristics, such as the application year and technological classes, I merge the inventor data with the patent data from PatentsView by patent number identifier.

¹⁵https://www.fema.gov/sites/default/files/2020-08/fema_548_summary_report_building_performance_hurricane_katrina_2005.pdf

¹⁶PatentsView (<https://patentsview.org/home>) is a visualization, data dissemination, and analysis platform that focuses on patent data, supported by the Office of the Chief Economist at USPTO.

I use a classification scheme within the Cooperative Patent Classification (CPC) system to identify green patents aimed at addressing climate change. Specifically, I rely on the Y02 class, which targets technologies and applications for mitigating or adapting to climate change. The Y02 class functions as an additional classification assigned by the patent office, complementing standard technology classifications. Notably, it spans multiple fields and captures innovations in both mitigation and adaptation technologies (Haščič and Migotto 2015). A patent is identified as a green patent if it falls within the Y02 class; otherwise, it is classified as non-green. Appendix Table A2.1 lists the Y02 subclasses and their descriptions. As a robustness check, I also use an alternative definition of green patents based on the OECD ENV-TECH classification (Haščič and Migotto 2015) and find similar results.

I allocate a patent to a specific year according to its application year rather than its year of eventual grant, as this aligns closely with the actual creation of the innovation. An inventor is regarded as active in every year between their first patent and last patent (Akcigit et al. 2022). During the inventor’s active period, if there is no patent application record for them in a particular year, I assign zero to their patent number for that year.

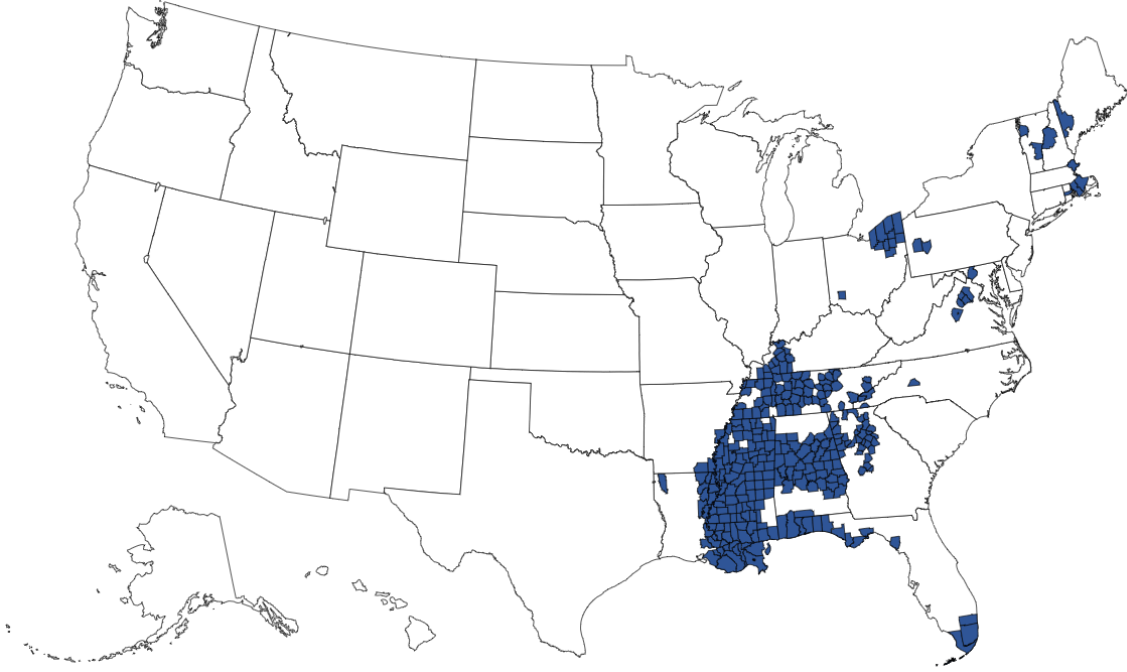
Counties Affected by Hurricane Katrina. — The Spatial Hazard Events and Losses Database for the United States (SHELDUS) offers comprehensive data on county location, property damage, crop losses, injuries, and fatalities associated with various natural hazard events in the US. Property damage refers to damage inflicted to private property as well as public infrastructure and facilities. I utilize relevant SHELDUS data to identify counties affected by Katrina and visualize their geographic distribution in Figure 2.2.

2.3.2 Treatment and Control Group Construction

I use two criteria to identify the treatment group, i.e., inventors who were likely to reside in the area affected by Hurricane Katrina. First, they filed at least one patent in 2004, or in 2005 before 25 August – the date of Katrina’s first landfall – with one of the Southern US counties,¹⁷ which suffered property damage from Katrina, listed as the inventor’s address in the patent application. The majority of counties affected by Katrina were concentrated in the Southern US region (Figure 2.2). Second, their first patent is before the year 2005 when Katrina occurred. Patent data only captures individuals who have already filed patents — it does not account for potential inventors who may have entered the innovation landscape later. Due to this data limitation, this study does not examine new inventor entry, i.e., the extensive margin.

¹⁷As defined by the U.S. Census Bureau (https://www2.census.gov/geo/pdfs/maps-data/maps/reference/us_regdiv.pdf), the Southern US includes the following states: Alabama, Arkansas, Delaware, Florida, Georgia, Kentucky, Louisiana, Maryland, Mississippi, North Carolina, Oklahoma, South Carolina, Tennessee, Texas, Virginia, and West Virginia.

Figure 2.2 GEOGRAPHICAL DISTRIBUTION OF COUNTIES AFFECTED BY KATRINA

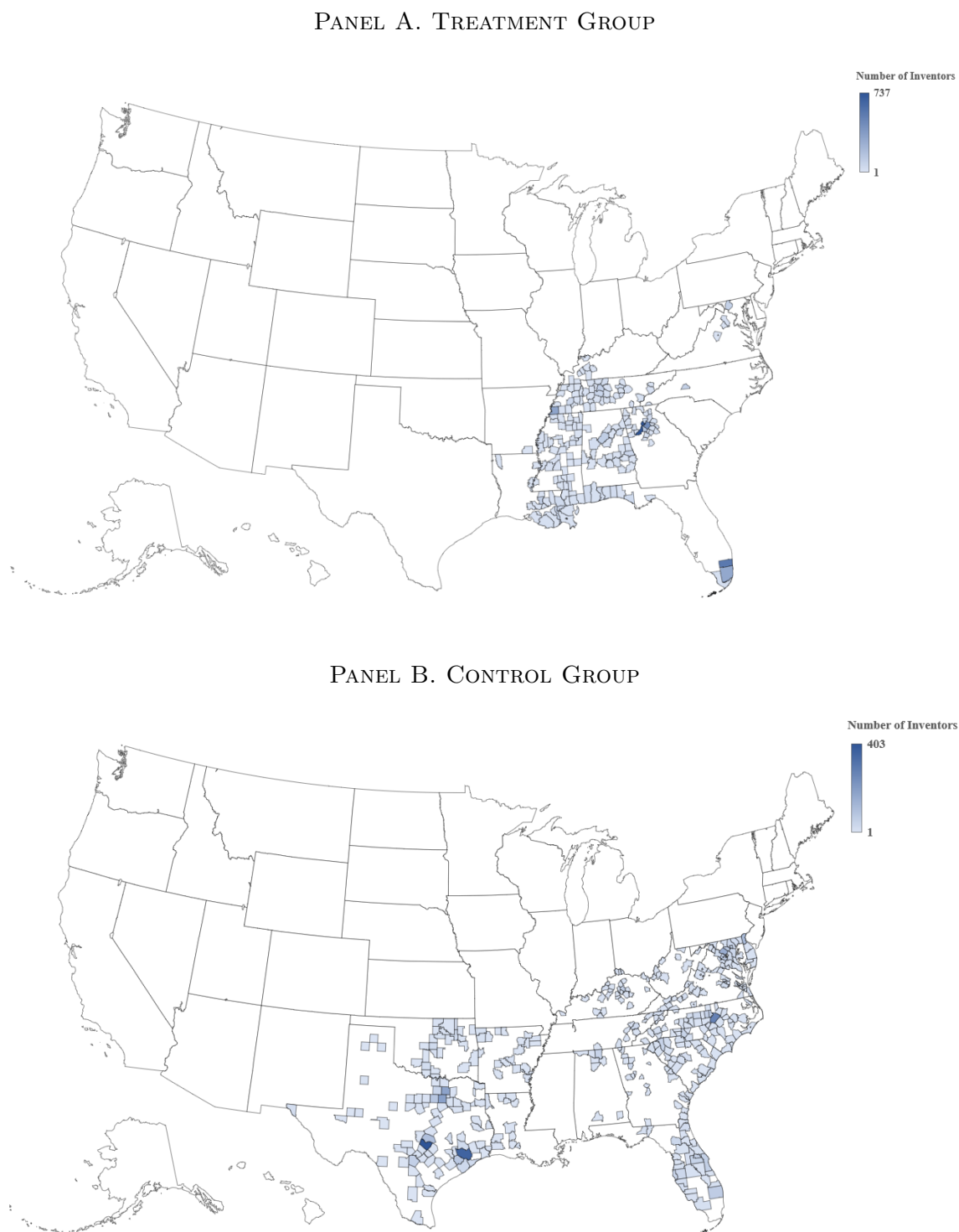


Notes: This figure illustrates the geographical distribution of counties that incurred property damage from Hurricane Katrina, with affected counties highlighted in blue. The majority of the damage is concentrated in the Southern United States, with impacts extending to northeastern regions due to Katrina-related weather events such as strong winds, heavy rain, and flooding. The data is sourced from the Spatial Hazard Events and Losses Database for the United States (SHELDUS), with further details provided in Section 2.3.1.

Next, I construct a control group. I first apply two analogous criteria to decide the pool of potential control inventors. First, they filed at least one patent in 2004, or in 2005 before 25 August with one of the Southern US counties listed as the inventor's address in the patent application. However, none of the inventor locations listed on those patent applications filed during 2004 or in 2005 before August 25, are situated in the Southern US counties affected by Katrina. Second, their first patent predates the year 2005 when Katrina occurred. To find the control inventors who appeared similar to the treatment group before Katrina, I use a one-to-one exact matching method, following Jaravel et al. (2018). The matching is based on three observables: year of first patent filing, cumulative patent applications up to 2004, and cumulative green patent applications up to 2004 – the year before Katrina occurred.¹⁸

¹⁸The matching procedure is conducted year by year without replacement, following the approach in Jaravel et al. (2018). I begin by matching inventors who filed at least one patent in 2005 before August 25, seeking exact matches for each treated inventor. An exact match is defined as a control inventor who filed their first patent in the same year and had the same cumulative number of patent applications up to 2004 and of green patent applications up to 2004 as the treated inventor. The inventors that match are then removed from the sample of potential matches, and the procedure is repeated for inventors who filed at least one patent in 2004. If more than one exact match is found, ties are broken at random.

Figure 2.3 GEOGRAPHICAL DISTRIBUTION OF TREATMENT AND CONTROL GROUPS



Notes: Panels A and B of this figure show the geographical distribution of treated and control inventors at the county level, respectively. Section 2.3.2 provides the definitions of the treatment and control groups.

Additionally, I focus solely on inventors whose first patent is in 1983 and afterwards, as the data for Louisiana are unreliable before 1983 (Akcigit et al. 2022).

Therefore, there are 4773 pairs of treated and control patent inventors. Patent inventors are included in the sample between the years they first apply for a patent and the year of their patent application. To avoid the confounding effects of the COVID-19 pandemic, my sample period ends in 2019. Panels A and B in Figure 2.3 show the geographical distribution of treated and control inventors at the county level, respectively. The treatment group is located in counties affected by Hurricane Katrina, while the control group is in counties spread throughout the southern U.S.

2.3.3 Estimation Strategy

In order to study the effect of Hurricane Katrina on inventors' innovation activities, I run a difference-in-difference regression:

$$Y_{ist} = f(\beta_0 + \beta_1 Treatment_i \times Post_t + \gamma_i + \sigma_s + \tau_t + \sum_j \lambda_j 1_{[tenure_{it}=j]} + \epsilon_{ist}) \quad (2.1)$$

where i indexes inventors, s denotes the states of inventor i , and t is the year of patent applications as this is the date closest to the actual creation of the innovation (Akcigit et al. 2016; Moretti and Wilson 2017; Akcigit et al. 2022). Y_{ist} denotes the innovation outcome, measured separately for green and non-green patents. My main analysis focuses on patent counts — a discrete, non-negative count variable — estimated using a Poisson pseudo-maximum likelihood (PPML) estimator. To complement the main results and capture different margins of the effect, I also construct two alternative measures: (i) a binary indicator for having at least one patent (extensive margin); and (ii) the natural logarithm of patent counts, conditional on having any (intensive margin). Both are estimated using ordinary least squares (OLS). All variables are measured over a three-year window between and including years t and $t + 2$ (Akcigit et al. 2022).

$Treatment_i$ is an indicator variable for treated inventors; $Post_t$ denotes an indicator variable that equals 1 starting in the year 2005 when Hurricane Katrina happened. I measure an inventor's $tenure_{it}$ as the number of years that have passed since the inventor's first successful patent application, and include tenure fixed effects $\sum_j \lambda_j 1_{[tenure_{it}=j]}$ to account for variation in the speed of invention across the life cycle of an inventor. Additionally, I control for inventor, state and year fixed effects, denoted by γ_i , σ_s , and τ_t , respectively. Throughout the analysis, I use two-way clustered standard errors at the state and year level to allow for both serial and spatial correlation.

To further explore the dynamics of the effects, I employ a second specification. This includes interactions between $Treatment_i$ and a series of year indicator variables spanning from 1999 to 2010, and 2004 is the excluded category. I also extend this interaction to include indicators for periods before 1998 and after 2011 to fully

capture the pre- and post-event effects. This specification is as follows:

$$Y_{ist} = \beta_0 + \beta_{1998} 1_{[t \leq 1998]} \times Treatment_i + \sum_{m=1999}^{2010} \beta_m 1_{[t=m]} \times Treatment_i + \beta_{2011} 1_{[t \geq 2011]} \times Treatment_i + \gamma_i + \sigma_s + \tau_t + \sum_j \lambda_j 1_{[tenure_{it}=j]} + \epsilon_{ist} \quad (2.2)$$

Table 2.1: SUMMARY STATISTICS ON INVENTORS

Variable	Sample	Mean	SD
Yearly green patent counts	Full sample	0.073	0.489
	Treatment	0.019	0.145
	Control	0.019	0.148
Yearly non-green patent counts	Full sample	2.024	3.041
	Treatment	0.743	1.179
	Control	0.743	1.177
Cumulative patents	Full sample	9.212	18.472
	Treatment	4.614	5.676
	Control	4.614	5.676
Cumulative green patents	Full sample	0.520	2.644
	Treatment	0.113	0.441
	Control	0.113	0.441
Number of years since first patent application	Full sample	10.214	8.206
	Treatment	5.056	5.689
	Control	5.056	5.689
No. inventors	Full sample	224,891	
	Treatment	4773	
	Control	4773	

Notes: This table presents summary statistics for the full sample, as well as for treatment and control groups of inventors. Statistics include yearly green and non-green patent counts, calculated for the entire panel from 1983 to 2019 for the full sample, and for the pre-Katrina years for treated and control inventors. Cumulative patent counts, cumulative green patent counts, and the number of years since the first patent application are computed specifically in 2004 — the year before Hurricane Katrina occurred — for both treatment and control groups, and across all years for the full sample. For detailed information on data sources and sample construction, refer to Sections 2.3.1 and 2.3.2.

2.3.4 Descriptive Statistics

Table 2.1 presents summary statistics for the variables of interest in the main sample used in the analysis. Full sample is defined as inventors who filed at least one patent in 2004 or 2005 with the address of US listed on the patent application, and filed their first patent after in 1983 and afterwards.¹⁹ Statistics on yearly green and non-green patent counts are computed based on the entire panel for the full sample of inventors, and based on years before Katrina for treated and control inventors. Cumulative patent counts, cumulative green patent counts, and the number of years since the first patent application are computed in the year 2004 before Katrina happened for treatment and control group, and across all years for the full sample. By construction, the means and distributions of cumulative patent counts, cumulative green patent counts, and the number of years since the first patent application for the control group exactly match those of the treatment group. The means and distributions of yearly green and non-green patent counts before Katrina are also very similar in these two groups, although this match algorithm did not match exactly on these variables.

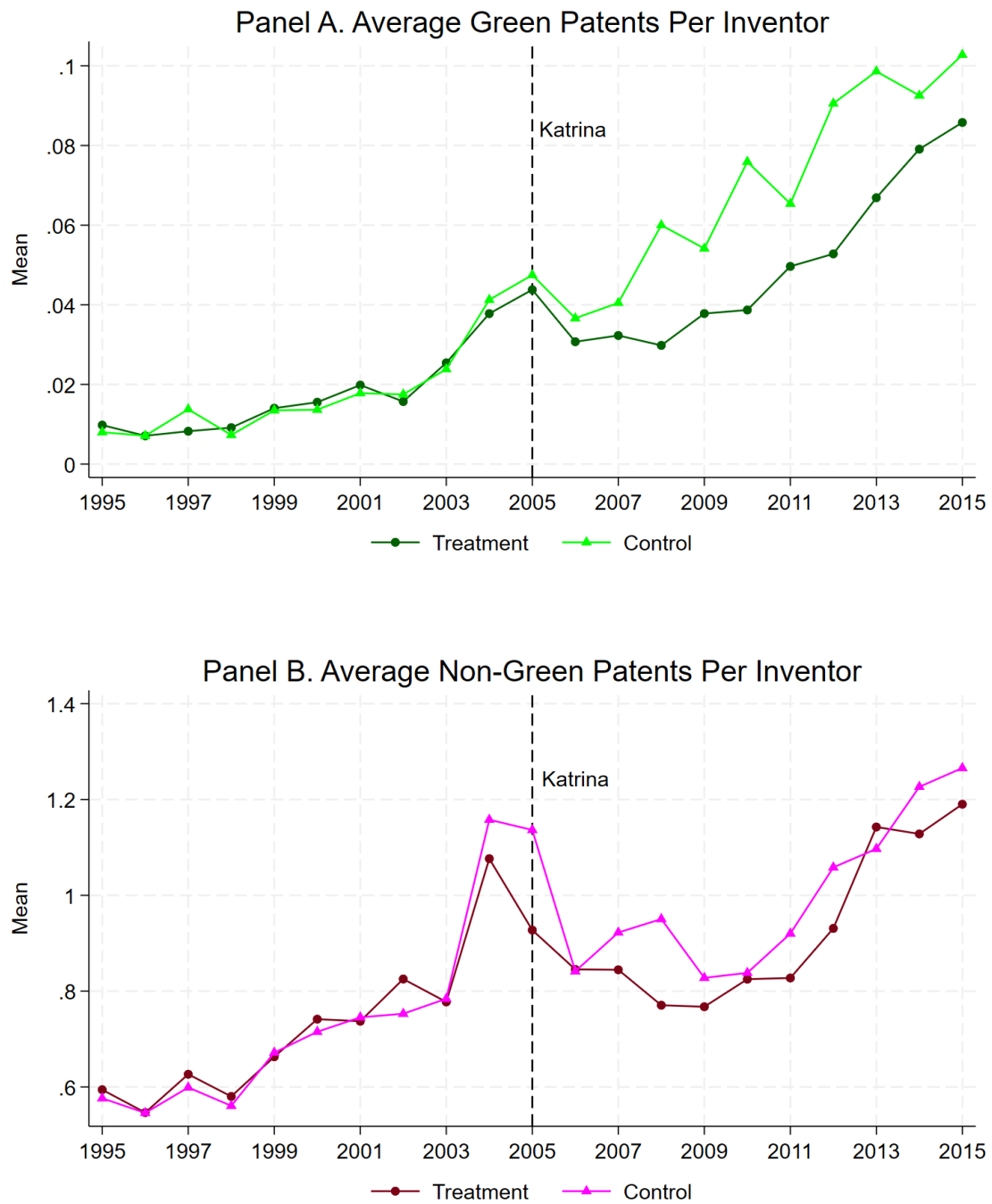
The treatment and control inventors are 5 years younger than inventors in the full sample, in terms of numbers of years since first patent application. Accordingly, they have much lower yearly and cumulative patent and green patent counts compared to full sample inventors. Therefore, it would not be appropriate to use the full population of inventors as a control group for the treatment inventors, as their life-cycle patenting activities are likely to follow different trajectories.

2.4 Results

Panels A and B in Figure 2.4 illustrate the trajectory of annual mean patents, divided into green and non-green categories, for both treated and control inventors around the year 2005 when Hurricane Katrina occurred. They confirm nonparametrically that inventors in both the treatment and control groups followed similar patent trends before 2005, which bolsters the validity of the research design. Before Katrina, treatment and control inventors exhibited similar levels of production in both green and non-green patents. Importantly, the matching algorithm, which only focuses on the stock variables in the year 2004 before Katrina, did not impose that the yearly dynamics before Katrina should be aligned between the treatment and control groups. However, following Katrina, control inventors produced more green patents than treated inventors (Panel A), while no significant divergence was observed in the production of non-green patents by inventors (Panel B).

¹⁹The data for Louisiana are unreliable before 1983 (Akcigit et al. 2022).

Figure 2.4 PATH OF INVENTORS' PATENT OUTCOMES AROUND HURRICANE KATRINA



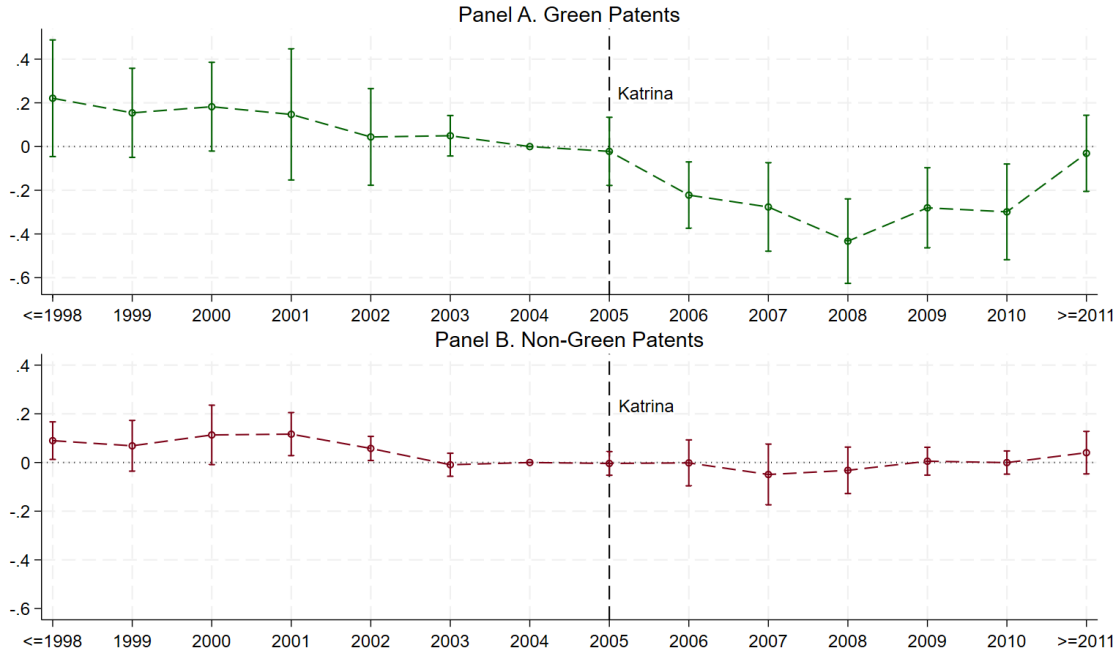
Notes: Panels A and B of this figure display the trends in mean green and non-green patents for treated and control inventors around 2005, the year Hurricane Katrina occurred. The sample inventors were selected based on their patent applications during 2004-2005, which accounts for an observed spike in patent activity during these years. Section 2.3.2 provides the definitions of the treatment and control groups.

Table 2.2: EFFECTS OF KATRINA ON INVENTORS' PATENT PRODUCTION

	Green Patents		Non-Green Patents	
	(1)	(2)	(3)	(4)
Treatment $\times$ Post	-0.212*** (0.0404)	-0.220*** (0.0646)	-0.0296 (0.0394)	-0.0419 (0.0349)
Observations	102,622	102,621	102,378	102,377
Inventor FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
State FE	No	Yes	No	Yes
Tenure FE	No	Yes	No	Yes
Method	PPML	PPML	PPML	PPML

Notes: This table reports the estimated coefficients β_1 from specification (2.1) for green patents (column 1-2) and non-green patents (column 3-4), respectively. The dependent variable is patent counts aggregated over a three-year period, from year t to $t + 2$. The variable *tenure* is measured as the number of years that have passed since the inventor's first successful patent application. Column 2 and 4 include the full set of fixed effects for tenure, inventor, year, and state. Standard errors clustered at the state and year level are reported in parentheses. For more details on the specification, refer to Section 2.3.3. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Figure 2.5 DYNAMIC EFFECTS OF KATRINA ON INVENTORS' PATENT PRODUCTION



Notes: Panels A and B of this figure show the estimated coefficients β_m , β_{1998} and β_{2011} from Poisson pseudo maximum likelihood specification (2.2) for two outcome variables – green and non-green patent counts, which aggregate over three years: between period t and $t + 2$. This specification includes inventor, tenure, state and year fixed effects. Tenure is measured as the number of years that have passed since the inventor's first successful patent application. Standard errors are clustered at the state and year level. For more details on the specification, refer to Section 2.3.3.

Table 2.2 presents the core results. Overall, I find that Hurricane Katrina negatively impacted the production of green patents by inventors, while it had no effect on non-green patents. Specifically, following 2005, treated inventors could have, on average, produced 24.6% more green patents within a three-year period if Hurricane Katrina had not occurred (column 2 in Panel A).²⁰ Columns 3-4 show that Katrina did not affect the production of non-green patents by inventors, as the point estimates are neither economically nor statistically significant.

Table 2.3: EXTENSIVE AND INTENSIVE MARGIN

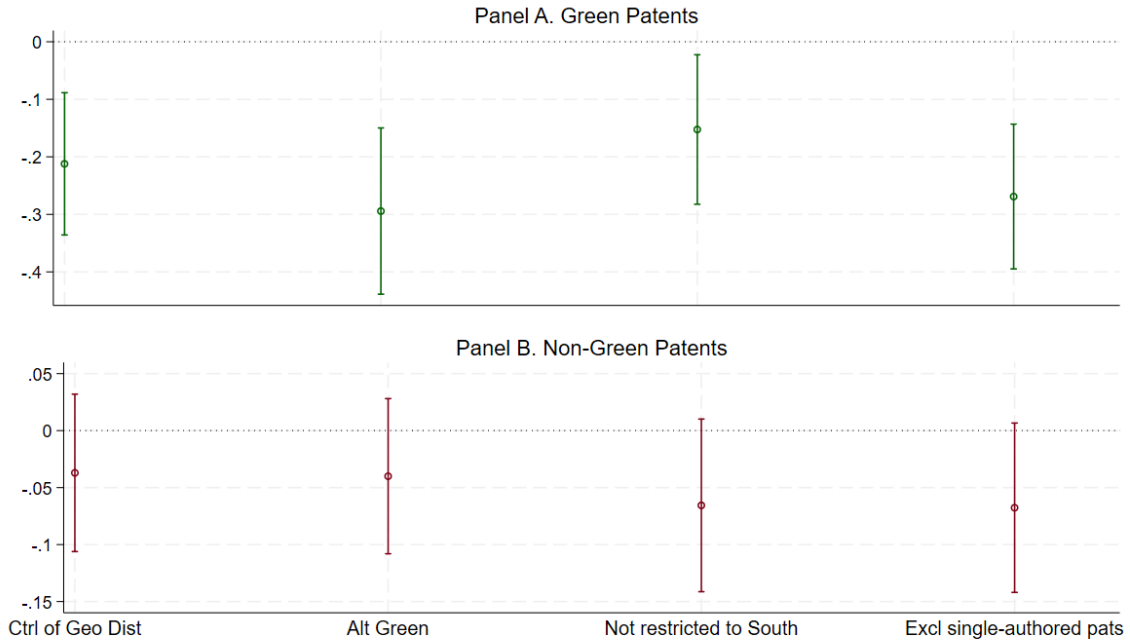
	Green Patents		Non-Green Patents	
	Has Any Patent	Log(Patent Counts	Has Any Patent	Log(Patent Counts
	over Three Years	over Three Years)	over Three Years	over Three Years)
	(1)	(2)	(3)	(4)
Treatment $\times$ Post	-0.0129** (0.00595)	-0.116** (0.0429)	-0.0098 (0.0119)	-0.00317 (0.031)
Observations	102,621	6,749	102,621	75,126
Inventor FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
State FE	No	Yes	No	Yes
Tenure FE	No	Yes	No	Yes
Method	OLS	OLS	OLS	OLS

Notes: This table reports the estimated coefficients β_1 from specification (2.1) for green patents and non-green patents, respectively. To capture intensive- and extensive-margin responses to Hurricane Katrina, I construct two dependent variables, both measured over a three-year window between and including years t and $t+2$ (Akcigit et al. 2022): (i) having any patent or not; (ii) the natural logarithm of patent counts. The variable *tenure* is measured as the number of years that have passed since the inventor's first successful patent application. The estimation method is OLS. Standard errors clustered at the state and year level are reported in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Figure 2.5 reports the point estimates and 95 percent confidence interval for the coefficients β_m , β_{1998} and β_{2011} , obtained from specification (2.2). Consistent with Table 2.2, Figure 2.5 shows a negative effect on green patents but no effect on non-green patents, although the negative effect on green patents appears to be temporary. There is no pre-trending for any of the outcome variables, which lends credibility to the research design. Annual coefficients for green innovation (Panel A in Figure 2.5) exhibits a notable decline in 2006, followed by a further decrease to their lowest point in 2008. Especially, during the years 2006 to 2010, these point estimates are significantly below zero. However, starting from 2011, these annual points began to rebound and approach zero. In contrast, annual coefficients for non-green patents (Panel B in Figure 2.5) exhibits no significant decline after 2005. Most annual coefficients for non-green patents are not significantly different from zero, regardless of the period before or after Katrina.

²⁰Poisson coefficients are transformed as $e^{\beta_1} - 1$ to show percentage changes.

Figure 2.6 ROBUSTNESS



Notes: Panels A and B of this figure plot the estimated coefficient β_1 from Poisson pseudo maximum likelihood specification (2.1) for four robustness exercises: 1. Controlling geographical distance of sample inventors with co-inventors; 2. Using the alternative definition of green patents, based on the OECD ENV-TECH classification ((Haščič and Migotto 2015); 3. Not restricting control inventors to the South US; 4. Excluding solo-inventor patents. The outcome variables are green and non-green patent counts, which aggregate over three years: between period t and $t + 2$. This specification includes inventor, tenure, state and year fixed effects. Tenure is measured as the number of years that have passed since the inventor's first successful patent application. Standard errors are clustered at the state and year level. For more details on the specification, refer to Section 2.3.3.

To gain additional insights, I estimate the effects of Katrina at the extensive and intensive margins for green and non-green patents, as described in Section 2.3.3. Table 2.3 show similar results. Treated inventors could have been more likely to file a green patent (column 1), and conditional on filing any green patent, they could have produced more green patents (column 2). Columns 3-4 show that Katrina affected neither the extensive nor the intensive margin of non-green patent production by inventors. I also do additional robust exercises: 1. Controlling for the geographical distance of sample inventors with co-inventors; 2. Using an alternative definition of green patents, based on the OECD ENV-TECH classification (Haščič and Migotto 2015); 3. Not restricting control inventors to the South US; 4. Excluding solo-inventor patents. Table 2.4 and Figure 2.6 show the robust results that support the main findings.

Table 2.4: ROBUSTNESS

	(1) Controlling geo distance with co-inventors	(2) Alt definition of green	(3) Control group not restricted to South	(4) Excluding solo- inventor patents
<i>Panel A: Green patents</i>				
Treatment $\times$ Post	-0.212*** (0.063)	-0.294*** (0.074)	-0.152** (0.066)	-0.269*** (0.064)
Mean of dep. var before Katrina	0.053	0.053	0.053	0.053
Observations	102,621	102,621	102,621	102,621
Year fixed effects	Yes	Yes	Yes	Yes
State fixed effects	Yes	Yes	Yes	Yes
Inventor fixed effects	Yes	Yes	Yes	Yes
Tenure fixed effects	Yes	Yes	Yes	Yes
Estimation method	PPML	PPML	PPML	PPML
<i>Panel B: Non-green patents</i>				
Treatment $\times$ Post	-0.037 (0.035)	-0.040 (0.035)	-0.065* (0.038)	-.068* (0.038)
Mean of dep. var before Katrina	2.084	2.084	2.084	2.084
Observations	102,621	102,621	102,621	102,621
Year fixed effects	Yes	Yes	Yes	Yes
State fixed effects	Yes	Yes	Yes	Yes
Inventor fixed effects	Yes	Yes	Yes	Yes
Tenure fixed effects	Yes	Yes	Yes	Yes
Estimation method	PPML	PPML	PPML	PPML

Notes: This table reports estimated coefficient β_1 from Poisson pseudo maximum likelihood specification (2.1) for four robustness exercises: 1. Controlling geographical distance of sample inventors with co-inventors; 2. Using the alternative definition of green patents, based on the OECD ENVTECH classification (Hašič and Migotto 2015); 3. Not restricting control inventors to the South US; 4. Excluding solo-inventor patents. The outcome variables are green and non-green patent counts, which aggregate over three years: between period t and $t + 2$. This specification includes inventor, tenure, state and year fixed effects. Tenure is measured as the number of years that have passed since the inventor's first successful patent application. Standard errors clustered at the state and year level are reported in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

I also examine whether inventors who migrated after Katrina experienced similar effects compared to those who did not migrate. Migrant inventors are defined as those whose county address prior to Katrina — specifically in 2004 or in 2005 before August 25 — differs from the address listed on their patent applications in the first year they filed any patents after the hurricane. I estimate a triple difference-in-differences (DDD) specification to assess the heterogeneity in Katrina's impact between migrant and non-migrant inventors.²¹ I also differentiate whether an inven-

²¹Using the notation in specification (2.1), the DDD specification is:

$$\begin{aligned}
 Y_{ist} = & \beta_0 + \beta_1 \text{Treatment}_i \times \text{Post}_t \times \text{Move}_i + \beta_2 \text{Treatment}_i \times \text{Post}_t + \beta_3 \text{Treatment}_i \\
 & \times \text{Move}_i + \beta_4 \text{Post}_t \times \text{Move}_i + \gamma_i + \sigma_s + \tau_t + \sum_j \lambda_j 1_{[\text{tenure}_{it}=j]} + \epsilon_{ist}
 \end{aligned} \tag{2.3}$$

where Move_i is a dummy variable equal to 1 if the county address of inventor i around the time

tor moved and moved to a Katrina county and to a non-Katrina county. Table 2.5 show the results which show that there is no differential effects for migrant inventors and non-migrant inventors, also no differential effects for migrant inventors who moved to Katrina and non-migrant inventors, and for migrant inventors who moved to non-Katrina county and non-migrant inventors.

Table 2.5: EFFECTS OF KATRINA ON MIGRANT V.S. NON-MIGRANT INVENTORS

	Green Patents		Non-Green Patents	
	(1)	(2)	(3)	(4)
Treatment $\times$ Post $\times$ Move	-0.194 (0.189)		-0.0386 (0.0418)	
Treatment $\times$ Post $\times$ Move to Katrina counties		0.412 (0.843)		0.247 (0.198)
Treatment $\times$ Post $\times$ Move to non-Katrina counties		-0.197 (0.160)		-0.0519 (0.0554)
Observations	90,398	90,398	90,398	90,398
Year FE	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes
Inventor FE	Yes	Yes	Yes	Yes
Tenure FE	Yes	Yes	Yes	Yes

Notes: This table reports the estimated coefficients β_1 from the specification (2.3) for green patents and non-green patents, respectively. The dependent variable is patent counts aggregated over a three-year period, from year t to $t + 2$. I estimate the model using a Poisson pseudo maximum likelihood estimator, as the outcome variable is a count of patents. Move is a dummy variable equal to 1 if the county address of inventor i prior to Katrina — specifically in 2004 or in 2005 before August 25 — differs from the county address listed on their first patent application filed after the hurricane. Move to Katrina counties is a dummy variable equal to 1 if Move is equal to 1 and the address listed on their first patent application filed after the hurricane is in a county that experienced property damage from Hurricane Katrina. Move to non-Katrina counties is a dummy variable equal to 1 if Move is equal to 1 and the address listed on their first patent application filed after the hurricane is in a county that did not experience property damage from Katrina. Standard errors clustered at the state and year level are reported in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

before Katrina, i.e., in 2004 or in 2005 before 25 August, differs from the address listed on their patent applications in the first year they filed any patents after the hurricane. For inventors with filings in both years, the modal county address in the year 2005 before 25 August is used. I estimate the model using a Poisson pseudo maximum likelihood estimator, as the outcome variable is a count of patents.

2.5 Do Differences in Technological Distance Matter?

In this section, I seek to understand why Katrina had different effects on green and non-green patents. Specifically, I examine the hypothesis of different technological distance between green and non-green patents. I first present the argument how disruptions to knowledge production affect differently individual inventors' production of green versus non-green innovation. Next, I construct the measurement of technological distance between inventors, and show that technological distance between a sample inventor and collaborators within a patent team is greater for green patents compared to non-green ones. Moreover, I examine whether the negative impacts of Katrina are more pronounced for patents produced by teams with greater technological distance, and for inventors with more technologically distant collaborators before Katrina. In addition to the analysis here, I also examine other potential mechanisms in Section 2.6.

2.5.1 Motivation

Green Innovation with Greater Knowledge Diversity

Green innovation is regarded as an important means of reducing environmental burden and improving environmental performance (Rennings 2000; Popp, Newell and Jaffe 2010; Popp 2019). The development of such green technologies involves complex challenges, often requiring information and skills that are distant from the traditional knowledge base (De Marchi 2012). The inherent complexity of these technologies is evident in their multi-purpose and systemic nature (Ghisetti, Marzucchi and Montresor 2015). Green technologies are expected to satisfy various interconnected objectives, including improved production efficiency and product quality (Florida 1996; Oltra and Saint Jean 2005), while also adhering to stringent regulatory standards (Carrillo-Hermosilla, Del Río and Könnölä 2010). Furthermore, Mealy and Teytelboym (2022) find that green and renewable energy products typically exhibit higher Product Complexity Index values than the industry average. Overall, the development of green technologies is more intricate than that of non-green technologies, requiring more knowledge and information (Horbach, Oltra, and Belin 2013).

The production of new ideas and technologies fundamentally relies on combining existing knowledge (Schumpeter 1934; Weitzman 1998). The breadth of this knowledge search, crucial for technological development, can enhance problem-solving capabilities when it extends beyond current knowledge structures (Winter, 1984; Kaplan and Vakili, 2015). This is especially relevant for green technologies, which are at the forefront of addressing complex environmental and economic challenges simultaneously. The diverse environmental needs — from energy savings and pollu-

tion reduction to CO2 capture and new materials — also benefit significantly from integrating a wide array of knowledge sources.

Empirical evidence indicates that the sources of knowledge for green technologies often span diverse technological fields that do not share significant commonalities, reflecting higher knowledge diversity during the knowledge search phase. For example, Fusillo (2023) uses patent data to show that green technologies exhibit a higher degree of diversity in knowledge sources, measured through co-citations of patent classes in the backward citations of focal patents, weighted by cognitive distance—an indicator of technological proximity between patent classes. Similarly, Barbieri et al. (2020) and Orsatti et al. (2020) find that green technologies draw from a broader knowledge base, as evidenced by the range of patent classes cited and the frequent use of broad collaborations. Moreover, the development of green technologies often involves the collaborative efforts across diverse actors, integrating various competencies and organizational structures. International collaborations, diversified teams, industry-university connections, and public research play crucial roles in advancing green technological development (De Marchi 2012; Quatraro and Scandura 2019; Orsatti et al., 2020; Hötte, Pichler, and Lafond 2021; Fusillo 2023).

The diversity of knowledge effectively recombined also defines the boundaries and scope of invention outcomes. Greater diversity of knowledge sources positively leads to greater knowledge diversity contained in the technological outcomes. Barbieri et al. (2020) analyse patents from 1980 to 2012 and find that green patents exhibit a broader scope, as evidenced by their assigned patent classes. Similarly, Fusillo (2023) identifies greater technological diversity in the knowledge output phase, measured through the co-classification of technology classes in patents, which reflects the amount of knowledge recombined and integrated into protected inventions. Higher diversity of knowledge sources also positively influences the impact of technological outcomes on subsequent inventions. Barbieri et al. (2020) and Dechezleprêtre, Martin, and Mohnen (2022) find that green innovations with greater knowledge diversity generate more knowledge spillovers and have a larger and more pervasive impact on subsequent technological development. Furthermore, the higher diversity of knowledge sources increase the potential applications and the widespread adoption of these technologies. According to the World Bank,²² green technologies, that are fundamentally more complex and multifaceted than their non-green counterparts, have applications that span all economic sectors.

Greater Technological Distance among Inventors within A Green Innovation Team

Inventors rely on their own knowledge stock to create new ideas and technologies. However, they are not born at the cutting edge of knowledge; instead, they must pursue considerable education and complete substantial training. If innova-

²²See, Pigato et al. 2020.

tion builds the stock of knowledge, subsequent generations of innovators may face a greater educational and training load, known as the “knowledge burden” (Jones 2009). This increasing knowledge burden can lead to a trend where innovators choose to specialize in narrower fields, concentrating their expertise to manage the growing complexity of knowledge. Jones (2009) uses patent data to demonstrate a strong and consistent trend among solo inventors to remain within their specialized fields, indicating a move towards greater specialization.

There has been a significant shift towards team-based knowledge production across various fields, including the sciences, social sciences, patenting, and economics (de Solla Price 1963; Adams, Black, Clemmons, and Stephan 2005; Wuchty, Jones, and Uzzi 2007). Analysis of patent data from 1975 to 1999 reveals a rapid increase in team size, with the average rising from 1.73 in 1975 to 2.33 by the end of the period, representing a 35% increase (Jones, 2010). Similarly, in the field of economics, Jones (2019) documents that in 1950, the average number of authors per paper was 1.2. By 2010, it had reached 2.0, and by 2018, the average team size had grown to 2.7. The increase in team size in economics papers over the past decade surpasses the growth observed over the previous fifty years.

Green technologies, which often involve complex and multifaceted problems, typically draw upon a broader range of knowledge from diverse technological fields. Consequently, inventors working on green innovations are often more technologically diverse from each other compared to those working on non-green technologies. This increased technological distance within a green innovation team is a result of the need to integrate knowledge from various disciplines to address both complex environmental and economic challenges effectively. As green technologies require the integration of diverse expertise and knowledge from different fields, the team members may have specialized knowledge that is more distinct from each other, compared to teams working on less complex, non-green innovations. This diversity in knowledge sources can enhance the creative problem-solving capabilities of the team, fostering innovative solutions that span multiple fields. However, it also presents challenges in coordinating and integrating diverse perspective and introduces vulnerabilities to disruptions.

More Negative Impact of Disruptions on Inventors’ Green Innovation Output

The existing literature consistently points to the negative effects on individual inventors from disruptions to knowledge production, with the death of collaborators being a significant disruptive event. Azoulay et al. (2010) document a 5% to 10% lasting decline in the quality-adjusted publications of coauthors following the death of a superstar collaborator. Oettl (2012) reports that the output quality of surviving coauthors falls between 14% and 16% when a star collaborator dies, as opposed to the death of a nonstar collaborator. Furthermore, Jaravel et al. (2018) demonstrate

that an inventor’s premature death leads to a substantial and enduring decrease in their co-inventors’ earnings and citation-weighted patents, with losses of 4 percent in earnings and 15 percent in patent citations after eight years.

Scientific evidence suggests that climate change will lead to increasingly frequent and destructive natural disasters, which can significantly disrupt various production activities, including knowledge production. Such hazards can destroy inventors’ homes and laboratories, both crucial for their productivity. Without stable housing, inventors cannot focus on utilizing their knowledge and skills to generate new ideas. Similarly, the absence of labs and equipment prevents them from conducting experiments essential for innovation. As a result, inventors may see a decline in their output of innovations following a disruptive natural disaster.

The negative effects of disruptive events, such as natural disasters, are likely to be more severe for green innovation outcomes, primarily due to the greater distance in technological expertise among team members involved in green projects. Green innovation often entails a wider array of knowledge (De Marchi 2012; Barbieri et al. 2020; Orsatti et al. 2020; Fusillo 2023). Resulted from this knowledge diversity, inventors within a green innovation team often possess distinct technological expertise, making them less substitutable compared to those in non-green innovation teams. If a disruptive event strikes an individual inventor in a green innovation team, finding a substitute to continue the affected inventor’s technological tasks becomes more challenging. Consequently, the development of green technology may suffer more significantly than non-green innovation.

Meanwhile, the overall innovation performance of firms may remain relatively unaffected by such natural disasters. Firms typically have the advantage of diversifying risk across their innovation portfolios, controlling large groups of inventors and maintaining operations in multiple locations, which helps buffer against the effects of localized disruptions.

2.5.2 Measurement of Technological Distance

To examine whether technological distance is larger within a green patent team than within a non-green one, I first construct the measurement of technological distance between inventors. Following Akcigit et al.(2022), an inventor’s technological expertise is defined as the modal class listed on patents filed before Katrina with her as an inventor. I look at 4-digit CPC subclass level, which is equivalent to the United States Patent Classification 3-digit level in Akcigit et al. (2022), and focus on classes that are invention information.²³

²³There are two types of information regarding technological classifications, i.e., invention information and additional information. Invention information is technical information in the total disclosure of a patent document that represents an addition to the state of the art. Additional information is non-trivial technical information which does not in itself represent an addition to the state of the art but might constitute useful information for the searcher. Y section symbols –

After identifying an inventor's technological expertise, I construct the measurement of technological distance between two inventors. The technological distance d_{ij} between inventors i with modal class a and inventor j with modal class b is defined as:

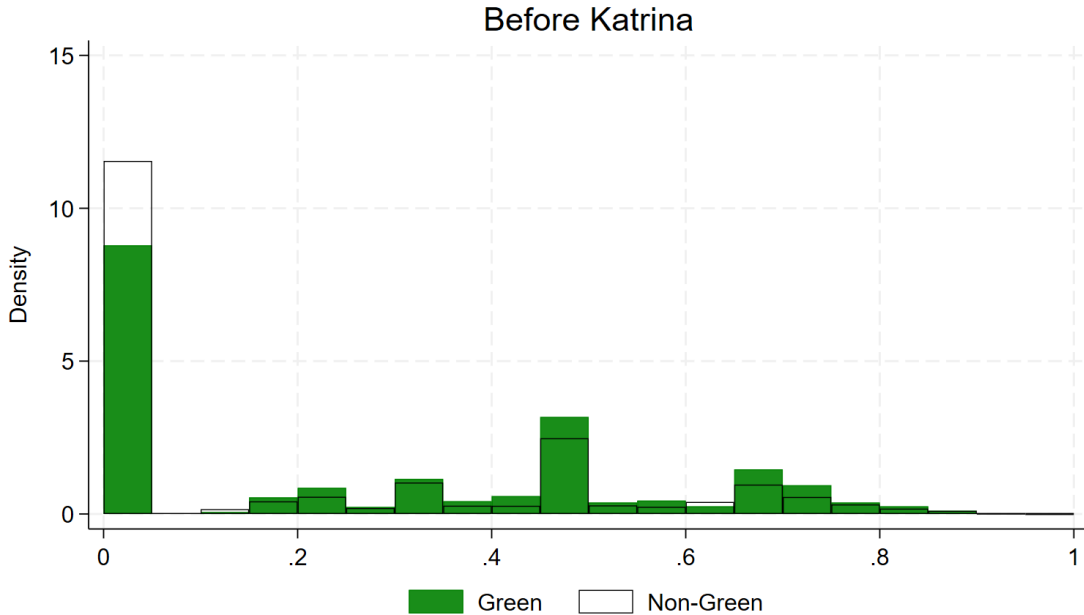
$$d_{ij} = 1 - \phi_{a,b}$$

Where $\phi_{a,b}$ is the technological proximity between modal class a and b . Following Hidalgo et al.(2007) and Azoulay et al.(2010), I adopt the co-occurrence approach to calculate the technological proximity. If two patent classifications are related because they cover similar knowledge and technical subject matter, they are likely to be assigned to one patent simultaneously, whereas dissimilar patent classifications are less likely to be assigned to one patent. This outcomes-based measure does not rely on ex-ante assumptions on which factors potentially affect technological proximity between two patent classifications. Formally, the technological proximity between patent classification a and b is the minimum of the pairwise conditional probabilities of a patent being classified under one given it is classified under the other:

$$\phi_{a,b} = \min\{P(a \in C_p | b \in C_p), P(b \in C_p | a \in C_p)\}$$

Where C_p is the set of patent classifications that are assigned to patent p .

Figure 2.7 DISTRIBUTION OF AVERAGE TECHNOLOGICAL DISTANCE WITHIN A PATENT TEAM FOR GREEN VS NON-GREEN



Notes: This figure shows the distribution of average technological distance within a patent team for green and non-green patents produced by sample inventors before Katrina. Section 2.5.2 details the methodology for measuring technological distance between two inventors. The average technological distance within a patent team is calculated as the mean of all technological distances between a focal inventor, i.e., a sample inventor, and her collaborators.

including Y02 class – are allocated for additional information only.

2.5.3 Difference in Technological Distance between Green and Non-Green Patent Teams

Figure 2.7 presents the distribution of average technological distance within a patent team for green and non-green patents produced by sample inventors before Katrina. The average technological distance within a patent team is calculated as the mean of all technological distances between a focal inventor, i.e., a sample inventor, and her collaborators. The distribution for green patent teams (green bars) is more skewed to the right, indicating a wider technological distance. In contrast, the distribution for non-green patent teams (white bars) is more concentrated on the left, indicating a narrower technological distance. This pattern persists both after Katrina and over the full time period, with green patents consistently showing a wider technological distance. Table 2.6 additionally presents the summary statistics on average team size and average geographical distance between a sample inventor and collaborators within the research team for green and non-green patents. Green patent teams, on average, have larger team sizes and greater geographical distances compared to non-green patent teams.

Table 2.6: PATENT TEAM CHARACTERISTICS: SIZE AND GEOGRAPHICAL DISTANCE

	Green	Non-Green	p-value of equality of means test
Average team size	6.29	4.95	0.00
Standard deviation	11.48	5.36	
Average geographical distance between collaborators and sample inventors (in km)	539.32	444.82	0.00
Standard deviation	1276.61	1130.27	

Notes: This table presents the summary statistics on (i) average team size—measured as the number of collaborators within a patent team—and (ii) average geographical distance between a sample inventor and their collaborators within the same research team, separately for green and non-green patents. A two-sample t-test is conducted to assess whether the differences in means between green and non-green patent teams are statistically significant, and the corresponding p-values are reported.

To empirically test the difference in technological distance between green and non-green invention teams my sample inventors belong to, I estimate the following simple model (Barbieri et al. 2020; Fusillo 2023):

$$dist_p = \alpha + \eta green_p + \theta X_p + state_p + tech_p + year_p + \epsilon_p \quad (2.4)$$

where $dist_p$ is the average technological distance between a sample inventor and collaborators within the research team that produces the patent p . $green_p$ is my focal explanatory variable, and it is a dummy variable taking value 1 if the patent p is classified as green and 0 otherwise. X_p is a set of controls, including the number

of distinct inventors and assignees in the patent p , and the average geographical distance between a sample inventor and collaborators within the research team produce patent p . To account for potential geographical heterogeneity and time-varying effects, I include a set of state dummies based on sample inventors' locations listed on patents, and a set of time dummies corresponding to patent application year. To rule out heterogeneity in technology area, I include technology class dummies at the 3-digit CPC level. Standard errors are clustered at the state-3-digit-CPC level.

Table 2.7: DIFFERENCE IN TECHNOLOGICAL DISTANCE BETWEEN GREEN AND NON-GREEN PATENTS

	(1) Full Period	(2) Before Katrina	(3) After Katrina
Green	0.0510*** (0.0104)	0.0585*** (0.0126)	0.0439*** (0.0117)
Observations	109,450	42,972	66,477
Year fixed effects	Yes	Yes	Yes
State fixed effects	Yes	Yes	Yes
CPC 3-digit fixed effects	Yes	Yes	Yes

Notes: This table reports the estimated coefficients η from specification (2.4), where the dependent variable is the average technological distance between a sample inventor and their collaborators within a patent team. Section 2.5.2 provides a detailed explanation of the methodology used to measure technological distance between two inventors. Columns 1-3 correspond to different sample periods: 1) the full sample period, 2) the period before Katrina, and 3) the period after Katrina. The model controls for several factors, including the number of distinct inventors and assignees associated with each patent, as well as the average geographical distance between a sample inventor and their collaborators within the patent team. To account for potential geographical heterogeneity and time-varying effects, I include state dummies based on the locations of the sample inventors as listed on patents, along with time dummies corresponding to the patent application year. Additionally, to account for variations across different technology areas, I incorporate technology class dummies at the 3-digit CPC level. Standard errors clustered at the state-3-digit-CPC level are reported in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

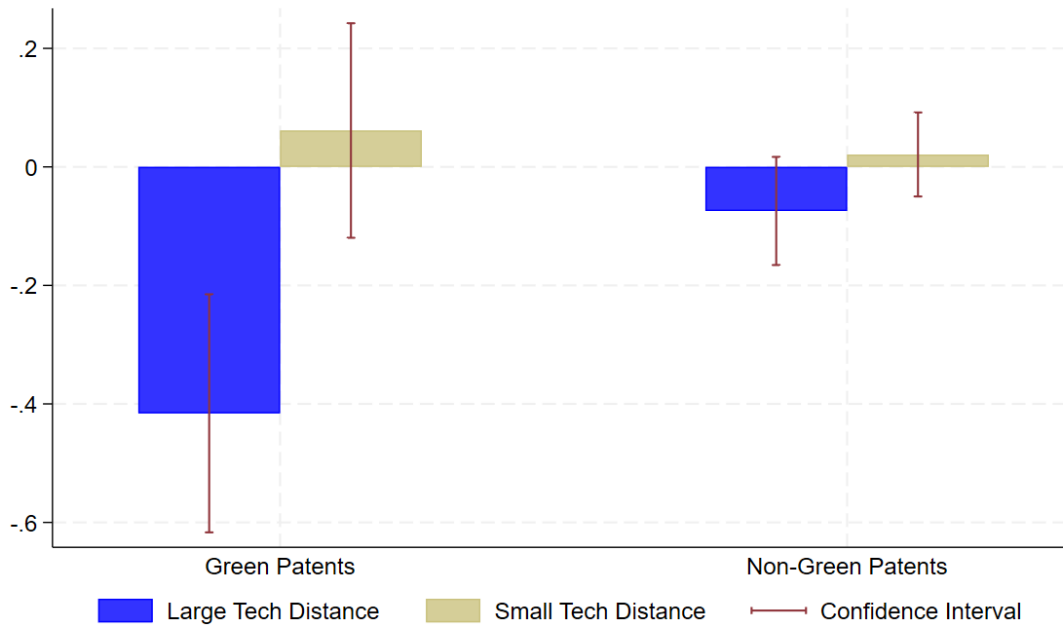
Table 2.7 presents the main results of my empirical analysis. Columns 1-3 correspond to different sample periods: 1) the full sample period, 2) the period before Katrina, and 3) the period after Katrina. The coefficients on the green dummy are all significantly positive, ranging from 0.0439 to 0.0585. Focusing on column 2, the distance between a sample inventor and collaborators within a green patent team is, on average, 0.0585 larger than that within a non-green patent team. This difference accounts for 26.35% of the mean average distance (0.222) before Katrina and 21.67% of the corresponding standard deviation (0.270). These results corroborate my hypothesis, indicating that, on average, the technological distance within a green

patent team is larger than within a non-green patent team.

2.5.4 Differential Effects on Patents of Small vs. Large Technological Distance

Patents produced by inventors within teams characterized by greater technological distance are likely to experience a more severe negative impact due to the disaster, if differences in technological distance within patent teams determine the differential effects of Hurricane Katrina on inventors' green and non-green patent outcomes. To test this hypothesis empirically, I categorize patents into groups with small and large technological distances. This classification is based on the distribution of average technological distances before Katrina, using the median value of 0 as the threshold. Patents with an average technological distance above 0 are categorized as having a large technological distance, whereas those with a distance of 0 are considered to have a small technological distance.

Figure 2.8 DIFFERENTIAL EFFECTS ON PATENTS OF LARGE VS. SMALL TECHNOLOGICAL DISTANCE



Notes: This figure plots the point estimates for the coefficient β_1 from specification (2.1) for two dependent variables: the number of patents with small technological distance and the number of patents with large technological distance. Both aggregate over three years: between period t and $t + 2$. The median value of average technological distance for each patent before Katrina is 0. Patents with an average technological distance above 0 are categorized as having a large technological distance, whereas those with a distance of 0 are considered to have a small technological distance. Section 2.5.2 provides a detailed explanation of the methodology used to measure technological distance between two inventors. The analysis is conducted for both green and non-green patents, using the Poisson pseudo maximum likelihood estimation method. Standard errors are clustered at the state and year levels. For more details on the specification, refer to Section 2.3.3.

I then conduct a separate regression analysis for each group, re-estimating the coefficient of interest, β_1 , using specification (2.1) and the Poisson pseudo maximum likelihood estimation method. The dependent variables for these analyses are the number of patents with small technological distance and the number of patents with large technological distance, respectively. I analyse for green and non-green patents separately. The results, presented in Table 2.8 and Figure 2.8, consistently show that patents with large technological distances suffered significant negative impacts from Hurricane Katrina across green and non-green innovation.²⁴ In contrast, the results for patents with small technological distances show positive but insignificant coefficients across all types of innovation. Additionally, tests confirm that the impacts on patents with small versus large technological distances are statistically different. I also conduct similar analysis for all patents and find similar results (see Appendix columns 1-2 in Table A2.2 and Figure A2.2).

Table 2.8: DIFFERENTIAL EFFECTS ON PATENTS OF SMALL VS. LARGE TECHNOLOGICAL DISTANCE

	Green patents		Non-Green patents	
	Large (1)	Small (2)	Large (3)	Small (4)
Treatment $\times$ Post	-0.416*** (0.103)	0.0614 (0.0924)	-0.0744 (0.0466)	0.0210 (0.0362)
Observations	102,621	102,621	102,621	102,621
Year fixed effects	Yes	Yes	Yes	Yes
State fixed effects	Yes	Yes	Yes	Yes
Inventor fixed effects	Yes	Yes	Yes	Yes
Tenure fixed effects	Yes	Yes	Yes	Yes

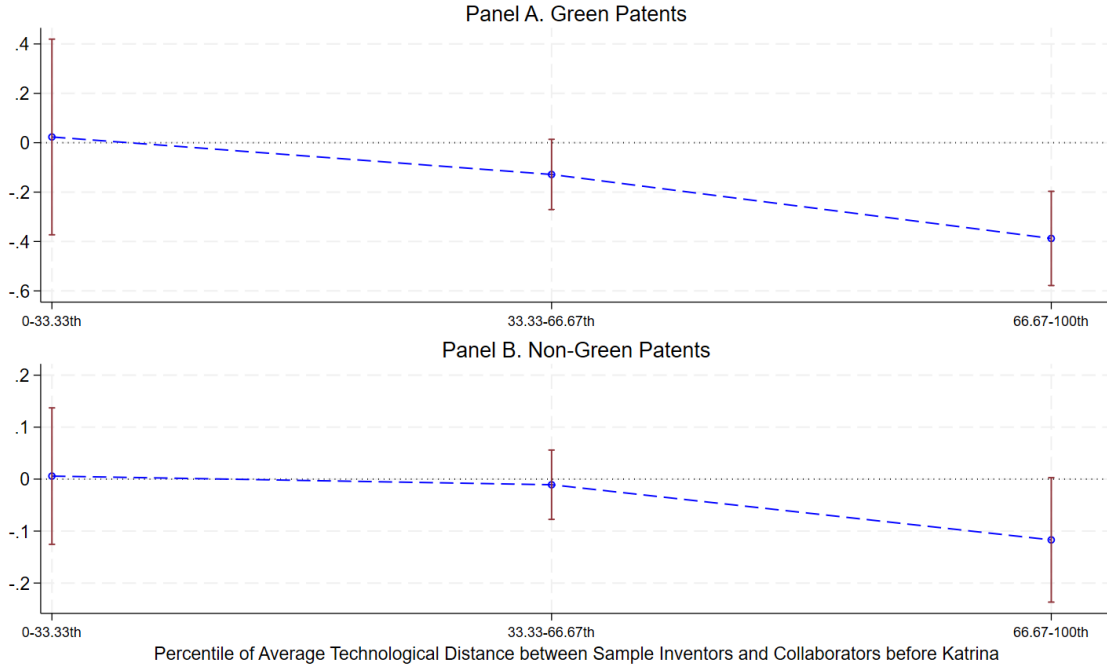
Notes: This table reports the point estimates for the coefficient β_1 from specification (2.1) for two dependent variables: the number of patents with small technological distance and the number of patents with large technological distance. Both aggregate over three years: between period t and $t + 2$. The median value of average technological distance for each patent before Katrina is 0. Patents with an average technological distance above 0 are categorized as having a large technological distance, whereas those with a distance of 0 are considered to have a small technological distance. Section 2.5.2 provides a detailed explanation of the methodology used to measure technological distance between two inventors. The analysis is conducted across green and non-green patents, using the Poisson pseudo maximum likelihood estimation method. The model includes fixed effects for tenure, inventor, year, and state. The variable *tenure* is defined as the number of years since the inventor's first successful patent application. Standard errors clustered at the state and year levels are reported in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

²⁴The effect on non-green patents is marginally significant, with a p-value of 0.11.

2.5.5 Heterogeneity by Technological Distance with Collaborators before Katrina

If the difference in technological distance within a patent team contributes to the differential effect of Katrina on inventors' green and non-green patent outcomes, I would expect that inventors who worked with more technologically distant collaborators before Katrina would experience a more negative impact on their patent outcomes.

Figure 2.9 HETEROGENEITY BY TECHNOLOGICAL DISTANCE WITH COLLABORATORS BEFORE KATRINA



Notes: Panels A and B of this figure plot the estimated coefficients β_m from specification (2.5) for green and non-green patents, respectively. The estimation is performed using the Poisson pseudo maximum likelihood method. The tertiles are computed based on the average technological distance between sample inventors and their collaborators before Katrina. Section 2.5.2 provides a detailed explanation of the methodology used to measure technological distance between two inventors. Standard errors are clustered at the state and year levels. For more details on the specification, refer to Section 2.5.5.

To empirically test this, I estimate the following model:

$$Y_{ist} = \beta_0 + \sum_{m=1}^3 \beta_m X_{im} \times Treatment_i \times Post_t + \sum_{m=1}^2 \beta_m X_{im} \times Post_t + \gamma_i + \sigma_s + \tau_t + \sum_j \lambda_j 1_{[tenure_{it}=j]} + \epsilon_{ist} \quad (2.5)$$

where X_{im} is a dummy variable indicating whether the average technological distance between inventor i and collaborators before Katrina falls between the $(m-1)th$ and

the m th percentile. I conduct this analysis for both green and non-green patents. Table 2.9 presents the regression results, and Figure 2.9 displays the point estimates and 95 percent confidence interval for the coefficients β_m . The analysis reveals that the point estimates for the first and second tertiles hover around zero, while those for the third tertile are significantly below zero, across green and non-green patents. Furthermore, tests confirm that the point estimates for β_m are significantly different between the first and third tertiles for both innovation types. For green innovation, a significant difference also exists between the first and second tertiles. These findings suggest that inventors who collaborated with more technologically distant partners before Hurricane Katrina experienced a greater negative impact on their patent output following the disaster. Similar results also hold for all patents (see Appendix columns 3 in Table A2.2 and Figure A2.3).

Table 2.9: HETEROGENEITY BY TECHNOLOGICAL DISTANCE WITH COLLABORATORS BEFORE KATRINA

	(1) Green patents	(2) Non-Green patents
First tertile $\times$ Treatment $\times$ Post	0.0233 (0.202)	0.00579 (0.0670)
Second tertile $\times$ Treatment $\times$ Post	-0.129* (0.0726)	-0.0108 (0.0340)
Third tertile $\times$ Treatment $\times$ Post	-0.388*** (0.0974)	-0.117* (0.0611)
Observations	102,621	102,621
Year fixed effects	Yes	Yes
State fixed effects	Yes	Yes
Inventor fixed effects	Yes	Yes
Tenure fixed effects	Yes	Yes

Notes: This table reports the estimated coefficients β_m from specification (2.5) for green and non-green patents, respectively. The tertiles are computed based on the average technological distance between sample inventors and their collaborators before Katrina. Section 2.5.2 provides a detailed explanation of the methodology used to measure technological distance between two inventors. The estimation is performed using the Poisson pseudo maximum likelihood method. The model includes fixed effects for tenure, inventor, year, and state. The variable *tenure* is defined as the number of years since the inventor's first successful patent application. Standard errors clustered at the state and year levels are reported in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

2.6 Are Firms behind the Differential Effects of Katrina?

In this section, I seek to understand whether the differential effects of Katrina on inventors' output of green and non-green patents are driven by firm-level decision making. Firms may have been more likely to shy away from green R&D than from non-green R&D if they became financially constrained and more risk-averse following Hurricane Katrina. Langer, Lemoine, Martin, and Verhoeven (2022) find green innovation has higher but more dispersed returns, making them riskier than the non-green alternative. Their quantitative counterfactual analysis suggests that financial constraints following the Great Recession depressed green innovation, as such restrictions increased risk aversion among investors and innovators, particularly when faced with riskier green projects. Similarly, in the aftermath of Katrina, firms may have faced financial pressures and became more risk averse, pushing them to avoid riskier green innovation.

However, firms may also have the incentives to increase investment in green R&D if they expect the increase in the market demand for green product. Consumers affected by natural disasters may also become more environmentally conscious, showing greater concern for their consumption patterns and environmental footprints. This increased awareness often leads to greater demand for green products. In response, firms may develop green technologies and produce green products as a strategic measure to soften price competition (Aghion, Bénabou, Martin, and Roulet 2023). Additionally, Inventors' human capital is 5–10 times more important than firm capabilities for explaining the variance in inventor output (Bhaskarabhatla, Chatterji, Higgins, Lin, and Singh 2021). Therefore, the decline in inventors' green innovation output following Katrina, while non-green innovation remained unaffected, may not be entirely attributable to firms

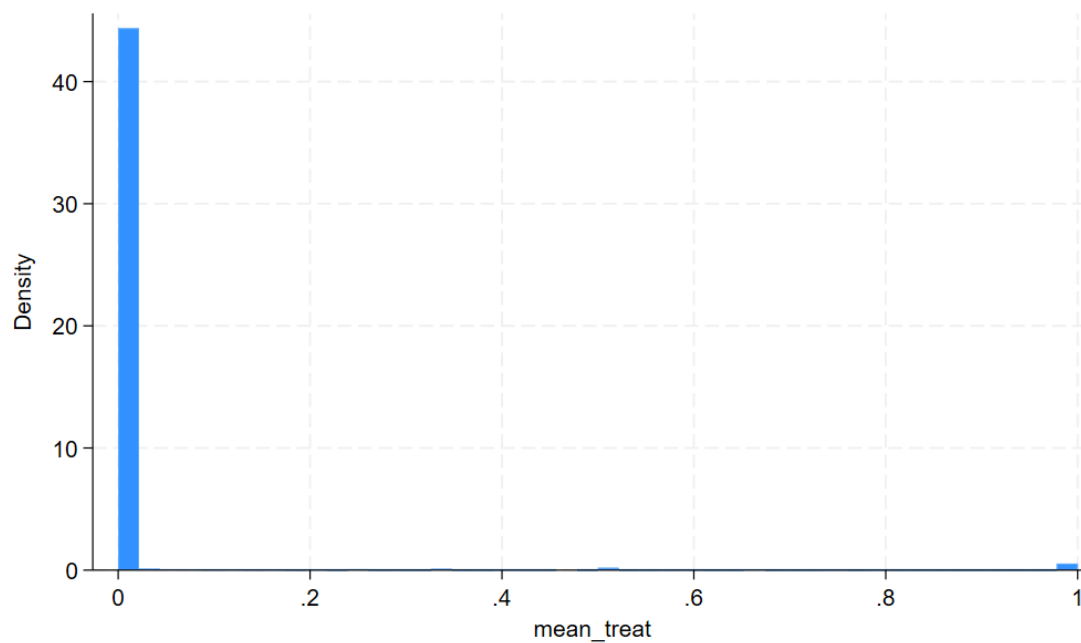
To investigate whether firms contributed to the differential effects of Hurricane Katrina on inventors' green and non-green patent output, I examine the assignee information in patent applications. A patent assignee is an entity or individual that receives the rights to a patent or application from the original owner.²⁵ In my sample, the vast majority of assignees — 96.8% — are firms. To empirically test this, I estimate the following model:

$$Y_{fst} = \beta_0 + \beta_1 \text{Treat}_f \times \text{Post}_t + X_{ft} + \gamma_f + \sigma_s + \tau_t + \sum_j \lambda_j 1_{[\text{age}_{ft}=j]} + \epsilon_{fst} \quad (2.6)$$

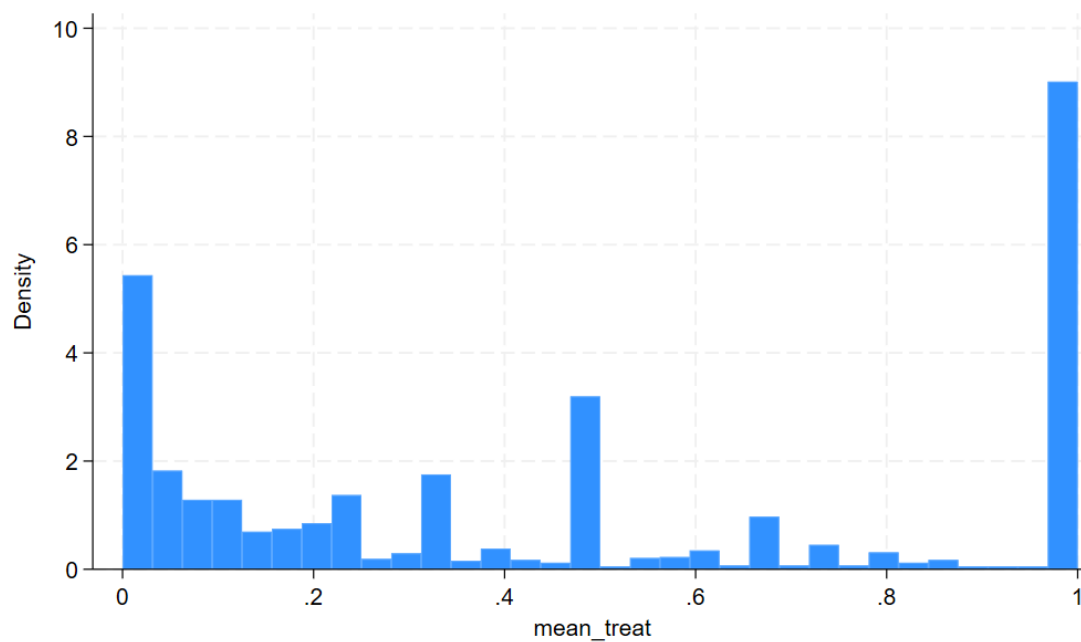
²⁵Patent assignments are a legal way to transfer ownership of a patent from the inventor to a business or other entity.

Figure 2.10 DISTRIBUTION OF FIRM TREATMENT

PANEL A. ALL VALUES OF *mean_treat*



PANEL B. *mean_treat* > 0



Notes: This figure shows the distribution of *mean_treat*. Panel A displays the full distribution, while Panel B focuses on firms with *mean_treat* > 0. *mean_treat* measures the share of a firm's inventors located in Katrina-affected counties, based on patent filings in 2004 and up to August 25, 2005.

where f indexes firms, s denotes the states of inventor i , and t is the year of patent applications. Y_{fst} represents the two innovation outcomes: (i) green patents, (ii) non-green patents. Both outcome variables are measured over a three-year window between and including years t and $t+2$ following Akcigit et al. (2022), and the analysis employs a Poisson pseudo maximum likelihood estimator.

Table 2.10: EFFECTS OF KATRINA ON FIRMS

D.V.	mean-treat		Treatment 1		Treatment 2	
	(1)	(2)	(3)	(4)	(5)	(6)
	Green	Non-Green	Green	Non-Green	Green	Non-Green
Treat X Post	-0.301 (0.202)	-0.175 (0.263)	0.0456 (0.146)	0.0857 (0.129)	-0.359** (0.151)	-0.277*** (0.065)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Firm Age FE	Yes	Yes	Yes	Yes	Yes	Yes
Method	PPML	PPML	PPML	PPML	PPML	PPML
Observations	396,308	396,308	396,308	396,308	396,308	396,308

Notes: This table reports the estimated coefficients β_1 from specification (2.6) for green patents (column 1, 3 and 5) and non-green patents (column 2, 4 and 6), respectively. The dependent variable is patent counts aggregated over a three-year period, from year t to $t + 2$. The variable *age* is measured as the number of years that have passed since the firm f 's first successful patent application. The full set of fixed effects for age, firm, year, and state are included. Standard errors clustered at the state and year level are reported in parentheses. For more details on the specification, refer to Section 2.6. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

The key variable, $Treat_f$, captures the extent to which firm f was affected by Hurricane Katrina, based on inventor locations before August 25, 2005, when Katrina occurred.²⁶ Specifically, I construct a variable, $mean_treat_f$, representing the share of firm f 's inventors located in Katrina-affected counties, using patents filed in 2004 and up to August 25, 2005.

Figure 2.10 Panel A displays the distribution of $mean_treat_f$. There is a pronounced spike near 0, indicating that most firms were not directly affected by Katrina. Panel B of Figure 2.10 shows the distribution among firms with $mean_treat_f > 0$; the median for this subset is 0.4. Consequently, I define three alternative treatment variables for robustness: (1) a continuous measure — $mean_treat_f$;

²⁶In my analysis, I use inventors' locations to define the treatment variable at the firm level. This approach captures firms with multiple operating locations to the extent that these locations are represented in the patent data by inventors linked to the same assignee. However, if a multi-plant firm files patents under different assignee names (e.g., headquarters versus subsidiaries), my method may not fully account for all of the firm's locations, potentially understating exposure for such firms.

(2) a binary indicator — equal to 1 if $mean_treat_f > 0$, and 0 otherwise; and (3) a binary indicator — equal to 1 if $mean_treat_f > 0.4$, and 0 otherwise.

Controls include total accumulated patents and accumulated green patents up to year t . I measure a firm's age_{ft} as the number of years that have passed since the firm f 's first successful patent application, and include age fixed effects $\sum_j \lambda_j 1_{[age_{ft}=j]}$. Additionally, I include firm, state, and year fixed effects, denoted by γ_f , σ_s , and τ_t , respectively. Throughout the analysis, standard errors are two-way clustered at the state and year levels to account for serial and spatial correlation.

Table 2.10 indicates no differential effects on firms' green and non-green innovation across different definitions of $Treat$. Columns 1–4 show no significant impact of Hurricane Katrina on firms' innovation outcomes, regardless of green or non-green. Columns 5 and 6 reveal negative effects on both green and non-green patent outputs for firms with over 40% of their inventors located in Katrina-affected areas.

2.7 Conclusion and Discussion

Climate change will lead to more destructive natural disasters including hurricanes, which can cause significant disruptions in the creation of knowledge and technology. Meanwhile green innovation, which draws on knowledge from a broader scope of the knowledge space, offers a solution to combat climate change. In this paper, I study the effect of disruptions, such as natural disasters, on inventors' innovation production, including green and non-green ones, within the context of the Hurricane Katrina, one of the costliest and deadliest storms in US history. Using hazard data and patent data for US inventors in a difference-in-differences setup, I find that Hurricane Katrina reduced inventors' production of green patents, while non-green patents remained unaffected.

Specifically, had Hurricane Katrina not occurred, a treated inventor could have produced 24.6% more green patents in a three-year period after 2005. This suggests that natural disasters could impede the green transition by negatively impacting inventors' green innovation production, despite the potential for such events to increase environmental consciousness and accelerate the development of green technology. Moreover, this finding sparks worries about a vicious cycle, given the potential rise in severe climate-induced natural disasters and the critical role of green innovation in addressing climate change.

It is important to understand the mechanism behind the observed negative effect on green innovation and no effect on non-green innovation. One possible explanation lies in the difference between green and non-green innovation regarding the distance in technological expertise among inventors within a research team. Green technology tends to be more complex and multifaceted than its non-green counterparts, spanning across all economic sectors (Pigato et al. 2020). Additionally, it

draws from a broader range of knowledge stock (De Marchi 2012; Barbieri et al. 2020; Orsatti et al. 2020; Fusillo 2023). Thus, the development of green technology may require collaboration among inventors with more diverse expertise and technological backgrounds, each fulfilling distinct roles and tasks. Within a green innovation team, there could be more significant technological distance among inventors. If a disruption event, like a natural disaster, strikes an individual inventor within a green innovation team, it will be harder to substitute so as to continue the affected inventor’s technological tasks. Therefore, the development of green technology may be more negatively affected than non-green innovation

Consistent with this conjecture, I find that average technological distance between a sample inventor and collaborators within a green patent team is larger than that in a non-green one. The negative impacts of Katrina are more pronounced for patents produced by teams with greater technological distance, and for inventors with more technologically distant collaborators before Katrina. Additionally, I find that Hurricane Katrina did not have differential effects on firms green and non-green innovation. These results suggest that greater knowledge diversity in inventors’ expertise within a research team make the technological development more fragile and vulnerable, in the face of disruptions to knowledge production, such as natural disasters related to climate change.

Future research could look into whether collaborators of inventors affected by Katrina produced more green innovation. The indirect exposure to a natural disaster, experienced through connections with directly impacted inventors, might increase collaborators’ environmental awareness without disrupting their capacity to innovate. While I document the negative impact on green innovation of direct exposure to a destructive natural disaster, a comprehensive welfare analysis should also consider the potential benefits that indirect exposure might provide. Additionally, since all of the evidence I have presented focuses on individual inventors, it would be interesting to investigate how firm characteristics – such as size, organizational structure, and geographic distribution of R&D activities – influence resilience and innovation outcomes following destructive natural disasters.

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Chapter 3

Checks and Balances and Corporate Investment

Abstract

This paper explores the question of how checks and balances – one important dimension of democracy – affect corporate investment. I exploit the changes in whether partisan controls of the executive and legislative branches are aligned (unified government) or not (divided government), resulted from the U.S. presidential and/or congressional elections. My results show that firms decrease investment rates by an average of 13.9% over a one-quarter period under a unified government relative to under a divided government. I also find that the negative association with unified governments is stronger for firms with greater dependence on government contracts, in less competitive markets, and with a higher degree of investment irreversibility. These results are consistent with the hypothesis that unified governments are more related to political uncertainty because they are more powerful and capable to pass legislation and thus to introduce changes. These findings show that divided governments are associated with some economic benefits compared to unified governments, and suggest that an important element of democratic systems, political balance, may increase GDP by enhancing private investment.

3.1 Introduction

The effect of democracy on economic growth has long been a controversial question in political economy and development economics.¹ Past empirical literature

¹Supporters of democracy argue that a democratic system which constrains political power holders and provides people with more political rights is more likely to set up secure property rights and reduce expropriation risk of governments, and thus enable economic development (Acemoglu Johnson and Robinson 2005). However, Barro (1996b) does not think only democratic governments

finding mixed and inconclusive evidence, mostly employs dichotomous measures of democracy and compares democratic and non-democratic countries.² However, “democracy is too blunt a concept” (Persson and Tabellini 2006). Few works take a close look at the essence of democracy³ and its effects on economic outcomes. My study attempts to fill this void by investigating the link between checks and balances — one important constitutional principle of modern democracies — and corporate investment.

In this paper, I study the relation between checks and balances⁴ and corporate investment, by examining the effects on firm investment of partisan control of both the executive and legislative in the U.S. federal governments. Checks and balances are the principle of government under which separate branches are empowered to prevent actions by other branches and are induced to share power. This institutional structure requires both the executive and the legislative bodies to agree on government actions because it creates a conflict of interest between them (Persson, Roland and Tabellini 1997). Including taxation, industry regulation, fiscal and monetary policy, etc., government actions could exert substantial influences on corporate decisions since they shape the business environment where firms operate. In a two-party system such as the U.S., partisan controls of the executive and the legislature might have important implications for the investment behavior of firms. This is because the capacity of passing new legislation, which decides government future actions, is a function of partisan controls of the executive and legislative branches (Anderson, Box-Steffensmeier, and Sinclair-Chapman 2003; Krutz 2005). A unified government, i.e., both the executive and legislative are controlled by one party, would be more able to implement large policy changes (Edwards III, Barrett, and Peake 1997; Coleman 1999), as powers are concentrated in one party. On the other hand, a divided government is more susceptible to gridlock because powers are distributed between two parties, and collective decisions under a divided government require bipartisan support. Since the possibility of political changes is higher

can maintain economic freedoms and private property rights. Autocratic governments, who can also foster economic rights, can implement painful reforms to achieve long-term goals (Rodrik 1999), while democratic governments with constraints of popularity and re-elections might not be able to do that.

²See, for example, Barro (1996a), Tavares and Wacziarg (2001), Papaioannou and Siourounis (2008), and Acemoglu, Naidu, Restrepo, and Robinson (2019).

³According to Polity5 (2020), “Democracy is conceived as three essential, interdependent elements. One is the presence of institutions and procedures through which citizens can express effective preferences about alternative policies and leaders. Second is the existence of institutionalized constraints on the exercise of power by the executive. Third is the guarantee of civil liberties to all citizens in their daily lives and in acts of political participation. Other aspects of plural democracy, such as the rule of law, systems of checks and balances, freedom of the press, and so on are means to, or specific manifestations of, these general principles.”

⁴In tripartite governments, such as that of the United States, checks and balances work through separating powers among legislative, executive, and judicial branches, and thus creating a balance among them. In this paper, I specifically focus on the sharing power between the executive and legislature.

under unified governments, political uncertainty, i.e., uncertainty concerning future government actions (Pástor and Veronesi 2013), will be greater under concentrated political powers (Bechtel and Füss 2008; Kim, Pantzalis, and Park 2012). Consequently, firm investment decisions might be different between under a unified and a divided government.

Firms might reduce their investment expenditures under a unified government, compared to under a divided government. This is because a unified government is more capable to pass bills and initiate some political changes that could reverse the profitability rankings of a firm's investment projects. Then the option value of waiting to invest would increase and the firm may rationally delay investment. Bernanke (1983), McDonald and Siegel (1986), and Dixit and Pindyck (1994) theoretically model the negative relationship between uncertainty and real investment. In these models, firms reduce investment when uncertainty introduces the benefits of increased information gained by waiting. Hence, greater capability of passing bills under a unified government creates incentives for firms to reduce investment spending.⁵ However, it is also possible that a unified government may reduce uncertainty in some contexts. For example, when the policy platform and priorities of a unified government are clearly communicated and well-understood, firms might face less ambiguity about future regulatory and fiscal environments.

To examine changes in corporate investment between under unified and under divided governments, I use data on the partisan structure of federal government and on the firm-level investment between 1977 and 2020. I then apply the classical investment-Q framework, and compare, econometrically, firm investment under the two types of governments. Consistent with the political uncertainty hypothesis, I document a strong negative relationship between a unified federal government and corporate investment. Under a unified government, corporate investment expenditures decline by an average of 13.9%, controlling for growth opportunities, cash flow rates, sales growth, and political and economic conditions. I find that the negative effect of a unified government dominates that of a presidential election dummy. This implies that political uncertainty from the partisan unity between the executive and legislative branches plays a more important role than the electoral uncertainty (Julio and Yook 2012). I also find the estimate of the unified government dummy is more negative at federal level than at the state level. This implies that federal government actions might have larger effects on corporate investment decisions. Additionally, my results show that a unified government has similar dampening effects on firm investment to the policy uncertainty index developed by Baker, Bloom and

⁵Although firms may know the elected unified government's political agenda, there still could be uncertainty regarding timing, content, and potential impact of potential decisions. For example, when Trump won the election in 2016 and the Republican Party also controlled the Congress, people would have expected substantial corporate tax cut under a Republican unified government. However, details were not clear because Trump and the Republican Congress had different corporate tax plans concerning the exact reduction in the tax rate.

Davis (2016).

To identify whether political uncertainty is the mechanism through which unified governments affect investment, I further investigate whether the negative effect on investment rates of unified governments exhibits heterogeneity in the cross-section. The first source of cross-sectional heterogeneity is government contract dependence. Hassan, Hollander, van Lent, and Tahoun (2019) find that firm-level political risk is highly correlated with government dependence. Baker, Bloom and Davis (2016) show that firms relying more on government contracts for sales reduce investment rates more when there is policy uncertainty. Hence, if uncertainty coming from unified government has a negative effect on corporate investment, then this effect should be stronger for firms with greater exposure to government purchases. I use firm-level government contract data to construct a dependence index. Consistent with the political uncertainty channel, I find that the investments of firms with a higher dependence on government spending are significantly more negatively affected by unified governments.

The second cross-sectional heterogeneity I explore is product market competition. While firms faced with uncertainty delay their investments due to the option value of “wait and see”, competition erodes the option value because firms fear preemption from competitors (Grenadier 2002). Therefore, if unified government depresses investment through uncertainty, then firms in highly competitive industries should reduce investment less. Following industrial organization literature, I use Lerner Index to proxy for product market competition. Consistent with the prediction, I find that the dampening effect of unified governments on investment expenditures is weaker for firms in more competitive markets.

The third test is based on investment irreversibility. One important reason why uncertainty depresses investment is the irreversibility of investment (Bernanke 1983). Costly capital reversibility gives firms an incentive to delay investment when there is uncertainty, because firms would like to wait for more information. The more irreversible the investment is, the more benefits the waiting option provides for firms facing uncertainty. Hence, I would expect that, when faced with the same uncertainty from unified government, more irreversible firms would reduce investments more. I use the ratio of fixed to total assets to proxy for investment irreversibility, and find supporting evidence that firms with a higher degree of investment irreversibility reduce investments more under a unified government.

Political uncertainty might not be the only mechanism through which unified governments affect real outcomes. One potential alternative explanation is political sentiment. The primary concern is that firms facing a unified government may simultaneously also receive bad news associated with political changes (negative political sentiment). If political sentiment drives my results, then I would expect only firms facing negative political news to reduce investment rates under a unified government. However, based on the political uncertainty hypothesis, investment expenditures of

both firms with positive and negative political sentiment would decline. I borrow the measure of political sentiment developed by Hassan et al. (2019) to conduct tests of this alternative hypothesis, and find no evidence that firms with positive and negative political sentiment have different responses to unified governments.

The findings in this paper have important implications for the impact of democracy on economic growth. I provide micro evidence regarding the relation between balance of political powers – an important essence of democracy – and private investment within a democratic country. That is the enhancement in firm investment under a divided government, i.e., balanced political power shared by two parties. I also provide evidence supporting the argument that it could be because the balance of power reduces political uncertainty and instability. This implies that an important element of democratic systems, political balance, could promote economic growth.

The rest of the paper proceeds as follows. Section 3.2 describes how this work relates to existing literature. Section 3.3 describes the data and the empirical strategy. Section 3.4 presents the empirical results, including baseline results, robustness checks, cross-section examination of the political uncertainty channel, and the alternative explanation test. Section 3.5 concludes.

3.2 Related Literature

This paper primarily relates to the literature on the effects of democracy on economic growth. There are opposing theoretical arguments about the impact of democracy on economic growth (e.g., Huntington 1968; North 1990; Acemoglu, Johnson, and Robinson 2005). Both positive and negative arguments focus on the comparison between democracy and autocracy. Accordingly, empirical work uses dichotomous measures of democracy and finds mixed macro evidence (e.g. Barro 1996; Tavares and Wacziarg 2001; Papaioannou and Siourounis 2008; Acemoglu et al. 2019). However, the devil is in the details — “democracy is too blunt a concept” (Persson and Tabellini 2006). I take a close look, within one democratic country, at a device of accountability in modern democracies, i.e., checks and balances. I use a unified government dummy to measure political balance and examine its effects on firm investment. My results show that, when power between the executive and legislature is more balanced between opposing political parties, private investment is enhanced in a democratic system. This is because the balance of power reduces political uncertainty and instability. I provide micro level evidence of this phenomenon. This implies that an important element of democratic systems, political balance, may increase GDP in a democratic economy.

This paper is also tied to the literature stream on the relationship between political uncertainty and investment. Starting from Julio and Yook (2012), there is large empirical work using election dummies to proxy for political uncertainty. However, political uncertainty in a democratic system, i.e., uncertainty about government

future actions (Pástor and Veronesi 2013), does not only rely on the electoral uncertainty concerning the future chief executive. Under checks and balances, government actions depend upon not only the executive but also the composition of the legislature (Alesina and Rosenthal 1996), and so does political uncertainty. Duquerrory (2020) looks at the change in the partisan control of the executive and legislative branches and emphasizes the role of policy uncertainty brought by the switch from a divided to a unified government. Using US firm data and exploiting US gubernatorial elections, she finds that firms reduce investment in the year following a gubernatorial election resulting in a switch from a divided to a unified state government. However, she ignores the mid-term elections concerning the state congress which could also lead to the transitions between divided and unified governments. Furthermore, a large firm with operations in many states may also be affected by policies in those states. Different from Duquerrory (2020), I look at the partisan alignment between the executive and legislature, which could be the results of either the Presidential elections or the Congress elections. Another caveat of this analysis is that it assigns firms to their geographic location based on current headquarter address information. However, firms might strategically change their headquarter locations and/or do investments in more than one state. I look at federal government level and thus have no concern of state-level location assignment. I also find supporting evidence that the effects on corporate investment of a unified federal government might be larger in absolute value than that of a unified state government.

My work is also related to the literature investigating the economic effects of political competition. A vast literature stresses the election motive or accountability, i.e., the public could choose their representatives in the government to enhance their own welfare. Therefore, electoral competition could induce political parties to implement growth-promoting policies (Besley, Persson and Sturm 2010), reduce rent-seeking (Ferraz and Finan 2011), and provide more public goods (Martinez-Bravo, Qian, Yao, and Yao 2012). On the other hand, lack of electoral competition might be harmful to the economy because political power holders are less disciplined, thus excessively seeking rent and providing little government services. Unlike the literature mentioned above, I look at political competition between two parties within the government, which is the result of election outcomes, and its effects on corporate investment, which is an important economic activity of private sectors. Moreover, I focus on the mechanism of political uncertainty, by which lack of competition in government could dampen firm investment, regardless of potential policy directions that might be positive or negative to firms.

3.3 Data and Methodology

3.3.1 Political and Macroeconomic Data

This study considers two dimensions of political unification: (1) at the federal level and (2) at the state level. Starting at the federal level, I consider 11 four-

year political cycles⁶ in the United States, beginning at the start of James Carter’s elected term in 1977 and extending through the end of Donald Trump’s term in 2020. This sample contains three Democratic presidents (Carter-1, Clinton-2, Obama-2) and four Republican presidents (Reagan-2, Bush I-1, Bush II-2, Trump-1), where the suffixes denote the number of terms for each president. A government is unified when the president’s office and the two chambers of the legislature are all controlled by the same party. Table 3.1 shows the federal government structure changes in my sample period. Over the 44 years, there are 14 years under unified governments led by 5 different presidents.⁷ I obtain political data at the federal level from the US government sites. Specifically, president data is from the White House site,⁸ the Senate and House data from their own sites separately.⁹

Table 3.1: TRANSITIONS BETWEEN UNIFIED AND DIVIDED GOVERNMENT

Year	Senate	House	President Party	Unified Government	President
1977–1978	D	D	D	Yes	Carter
1979–1980	D	D	D	Yes	
1981–1982	R	D	R	No	Reagan
1983–1984	R	D	R	No	
1985–1986	R	D	R	No	
1987–1988	D	D	R	No	
1989–1990	D	D	R	No	Bush I
1991–1992	D	D	R	No	
1993–1994	D	D	D	Yes	Clinton
1995–1996	R	R	D	No	
1997–1998	R	R	D	No	
1999–2000	R	R	D	No	
2001–2002	D	R	R	No	Bush II
2003–2004	R	R	R	Yes	
2005–2006	R	R	R	Yes	
2007–2008	D	D	R	No	
2009–2010	D	D	D	Yes	Obama
2011–2012	D	R	D	No	
2013–2014	D	R	D	No	
2015–2016	R	R	D	No	
2017–2018	R	R	R	Yes	Trump
2019–2020	R	D	R	No	

This table presents the transitions between unified and divided governments over the sample period 1977-2020.

Turning to the state level, I obtain state-level political data from Klarner’s

⁶Usually, the presidential election is held every four years while the Senate and House of Representatives elections are held every two years. The presidential election coincides with the elections for the Senates and House of Representatives every four years.

⁷The five different presidents are Carter, Clinton, Bush II, Obama and Trump.

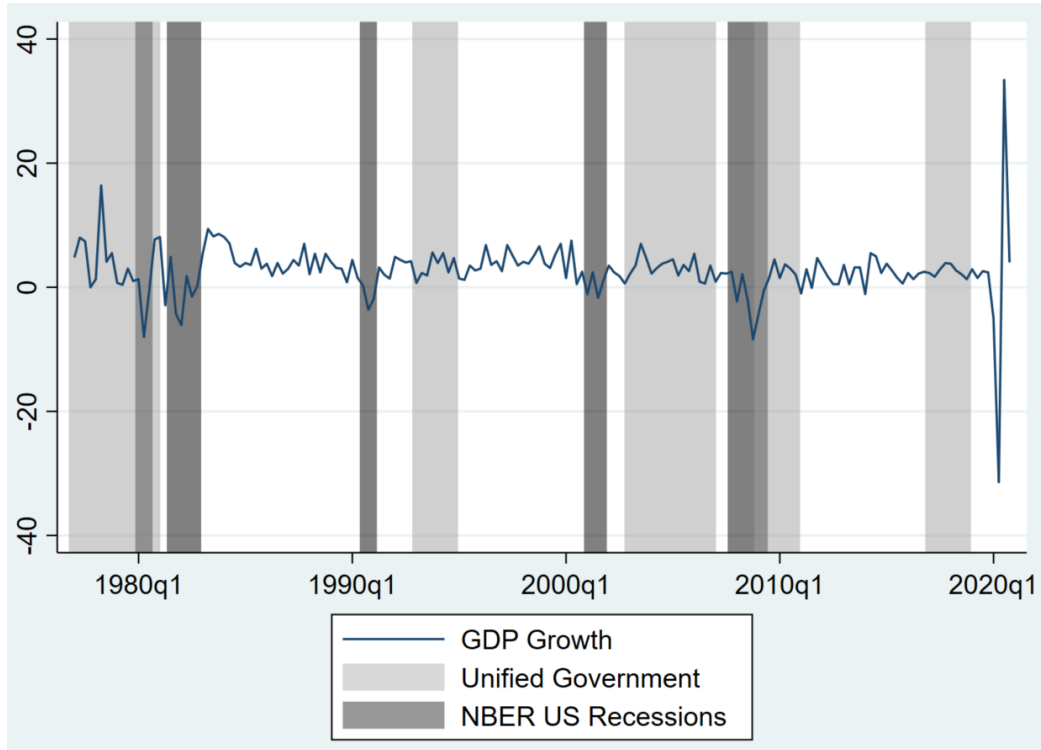
⁸The White House site is <https://www.whitehouse.gov>.

⁹The Senate site is <https://www.senate.gov>; and The House site is <https://clerk.house.gov>.

dataset on State governments (Klarner 2013a, 2013b) and National Conference of State Legislatures. The final political dataset includes information about whether the federal government is unified, whether the president is Democratic, whether the state government is unified, whether the governor is Democratic, and the dates of presidential and gubernatorial elections.

Macroeconomic data include economic policy uncertainty data from Baker, Bloom and Davis (2016)¹⁰ and national GDP growth data from Federal Reserve Economic Data (FRED). Figure 3.1 shows the economic growth trend over time, along with the periods of recessions¹¹ and unified governments, and implies that there is no obvious correlation between unified governments and macroeconomic conditions.

Figure 3.1 UNIFIED GOVERNMENT AND ECONOMIC GROWTH



The blue line plots the quarterly economic growth rate over the sample period of 1977 to 2020. The light grey shaded area represents the periods under a unified government, and the dark grey area is NBER Recession periods.

¹⁰Baker, Bloom and Davis (2016) develop an index of economic policy uncertainty based on newspaper coverage frequency. This index reflects the frequency of articles in 10 leading U.S. newspaper that contain the following trio of terms: “economic” or “economy”; “uncertain” or “uncertainty”; and one or more of “Congress,” “deficit,” “Federal Reserve,” “legislation,” “regulation,” or “White House.”

¹¹I follow the National Bureau of Economic Research (NBER)’s definition of a recession.

3.3.2 Firm-Level Data

The firm-level characteristic data are obtained from the merged CRSP-Compustat Fundamentals Quarterly database. I only include firms whose headquarters are located in the United States. For the regression analysis, I match firm-level data with political data. In order to match with state-level political data, I assign firms to their geographic location based on headquarter address information,¹² following Duquerrory (2020). Observations with missing values for investment, Tobin's Q, cash flow or sales growth are excluded from the sample. Panel A of Table 3.2 provides summary statistics for the firms considered in my analysis. Panel B of Table 3.2 summarizes the mean investment rates under unified and divided governments. The unconditional average investment rate is 3.68 percentage points under divided governments. The rate drops by 0.79 to 2.89 percentage points under unified governments. The reduction, statistically significant at the 1% level, represents a 21.5% decrease in the unconditional average investment rate relative to divided governments. The Appendix for Chapter 3 provides a comprehensive description of variable definitions. Since my data set consists of only publicly listed firms, I cannot conclude anything about the potential effects of unified government on privately held firms within the United States.

Table 3.2: SUMMARY STATISTICS

<i>Panel A: Firm Characteristics</i>				
	N	Mean	Median	Std. Dev.
Investment Rate ($\frac{CAPX_{ist}}{TA_{ist-1}} \times 100$)	630,025	3.46	1.62	5.36
Tobin's Q (TQ_{ist-1})	630,025	2.35	1.41	3.49
Cash Flow Rate ($\frac{CF_{ist}}{TA_{ist-1}} \times 100$)	630,025	0.68	2.31	8.38
Sales Growth ($SG_{ist-1} \times 100$)	630,025	10.19	7.51	53.06
<i>Panel B: Mean Investment Rates under Unified Governments vs. Divided Governments</i>				
Unified Governments		2.89	1.27	4.84
Divided Governments		3.68	1.77	5.54
Difference		-0.79		
Diff(t-stat)		-52.10		

This table reports summary statistics for the firm characteristics used in the analysis. See the Appendix for Chapter 3 for a detailed description of how I calculate each variable.

3.3.3 Specification

I use the investment-Q framework as the baseline specification because it has a solid theoretical foundation. According to Eberly, Rebelo, and Vincent (2009), the investment-Q regression model performs well compared to other empirical models.

¹²One concern here is that the merged CRSP-Compustat data only reports the latest headquarter locations. The majority of firms do not change their headquarter locations (Pirinsky and Wang 2006; Bai, Fairhurst and Serfling 2020). I use state-level political variables as controls, and find that my main result – both economic and statistical magnitude of the estimate of U_t – is robust to state-level political conditions.

Much empirical work studying the relation between political uncertainty and investment, such as Julio and Yook (2012) and Gulen and Ion (2016), employs this specification. Similar to Duquerroy (2020), I augment this investment-Q framework with political unification. However, I employ a dichotomous measure indicating unified federal governments, while Duquerroy (2020) uses a dummy variable set to one when state governments switch from divided to unified.¹³ Specifically, I use the following augmented version of the standard investment-Q specification to evaluate changes in investment rate that cannot be explained by the standard explanatory variables:

$$\begin{aligned} \frac{CAPX_{ist}}{TA_{ist-1}} = & \alpha_i + \beta_1 U_t + \beta_2 PresidtElection_t + \beta_3 DemPresidt_t + \beta_4 StateU_{st} \quad (3.1) \\ & + \beta_5 GubElection_{st} + \beta_6 DemGovernor_{st} + \beta_7 \ln EPU_t + \beta_8 TQ_{ist-1} \\ & + \beta_9 \frac{CF_{ist}}{TA_{ist-1}} + \beta_{10} SG_{ist-1} + \delta_1 \% \Delta GDP_{t-1} + QRT_t + \epsilon_{ist} \end{aligned}$$

where i indexes firms, s indexes states, and t indexes calendar quarters. The dependent variable, investment rate, is defined as capital expenditures (CAPX) normalized by beginning of the period total assets (TA). The independent variable of interest is the unified government dummy, which equals one if the elected federal government is controlled by one party.¹⁴ To better capture the effect on investment of unified governments, I align fiscal quarters with calendar quarters,¹⁵ although the vast majority of firms have fiscal quarters in coincidence with calendar quarters. The coefficient on the unified government dummy, β_1 , captures differences in the conditional investment rate between under a unified government and under a divided government, controlling for firm investment opportunities, economic conditions and other political conditions.

I cannot include year fixed effects in the main specification, because doing so would mechanically absorb all the explanatory power of the unified government dummy variable. In the absence of year fixed effects, I control for possible confounding political and economic factors. At the federal level, I also have $PresidtElection_t$, indicating whether there is a presidential election held in the year that calendar quar-

¹³Duquerroy(2020) includes a post-election dummy, which takes a value of one in the year following a gubernatorial election, and its interaction with the switching dummy without adding the switching dummy itself. Hence, the variable of interest in Duquerroy's work (2020) is the interaction term.

¹⁴Both the presidential and congress elections are held in November. The presidential inauguration day is on January 20, beginning in 1937. The new term of the Congress also starts in January, beginning in 1935. Therefore, if the federal government went from a divided to a unified government because of an election in November 1999, U_t would be set to 0 in the fourth quarter in 1999 and set to 1 in the first quarter in 2000.

¹⁵A firm's fiscal quarter is coded as calendar quarter t , if at least two months in the fiscal quarter belong to the calendar t . For example, if a firm's fiscal year ending in March, then its fiscal quarter one includes April, May and June. I would code this fiscal quarter one as calendar quarter two.

ter t belongs to. I do so because I would like to compare the effects on investment of the uncertainty from unified governments and the electoral uncertainty concerning the national leader (Julio and Yook 2012). To accomplish this, I would compare β_1 and β_2 , in terms of both economic and statistical significance. I additionally include $DemPresidt_t$ which is set to one if it is a Democratic president in calendar quarter t .¹⁶ This is because I am concerned that U_t might capture the economic effect of the partisanship of the president (Blinder and Watson 2016).

At the state level, I have $StateU_{st}$, indicating whether the government in the state s where the firm is headquartered is unified. I do so because I would like to compare β_1 and β_4 , and thus know whether the unified government at the federal or state level (Duquerrory 2020) matters more to corporate investment decisions. Similar to the federal level, I additionally include $GubElection_{st}$ (Jens 2017), which is set to one if there is a gubernatorial election held in the state s in the year that calendar quarter t belongs to, and $DemGovernor_{st}$, which is set to one if it is a Democratic governor in the state s in calendar quarter t .

Gulen and Ion (2016) find a strong negative relationship between firm-level capital investment and the monthly index of economic policy uncertainty constructed by Baker, Bloom and Davis (2016).¹⁷ To ensure that the unified federal government dummy variable is not simply picking up the negative effect of this economic policy uncertainty index, I add $lnEPU_t$ which is the natural logarithm of the arithmetic average of this economic policy uncertainty index in the three months in calendar quarter t (Gulen and Ion 2016). I also include quarterly national GDP growth, measured as the percentage change in real GDP in the quarter previous to the investment decision, to capture the effects of general economic conditions on firm investment.

I control for firm characteristics and investment opportunities to appropriately benchmark the conditional mean investment rate. Tobin's Q (TQ), as a proxy for the incentive to invest, is measured as the ratio of market value of assets to the book value of assets. The cash-flow (CF) variable is normalized by beginning of the period total assets (TA). Sales growth (SG), an additional control for investment opportunities, is calculated as the year-on-year growth in quarterly sales. All firm-level variables are winsorized at 0.5th and 99.5th percentile level, to limit the potential for extreme values to overly influence the regression output.¹⁸ α_i represents firm fixed effects and QRT contains a set of calendar-quarter dummy variables to control for

¹⁶Similar to U_t , $DemPresidt_t$ would be set to 0 in the fourth quarter in 1999 and set to 1 in the first quarter in 2000, if the President changes from Republican to Democratic because of an election in November 1999.

¹⁷Baker, Bloom and Davis (2016) build this index based on newspaper coverage frequency. This index reflects the frequency of articles in 10 leading U.S. newspapers that contain the following trio of term: "economic" or "economy"; "uncertain" or "uncertainty"; and one or more of "Congress," "deficit," "Federal Reserve," "legislation," "regulation," or "White House."

¹⁸My result of negative effect of unified governments on capital investment rate holds, regardless of winsorizing or not.

possible seasonality in capital investment. Standard errors are clustered at the firm, state and calendar-quarter level to correct for potential cross-sectional and serial correlations in the error term ϵ_{ist} .

3.4 Empirical Results

3.4.1 Baseline Results

Table 3.3 presents the main results for my baseline regression specification. The first column reports the regression result of capital investment rate on the unified government, controlling for firm-level covariates — Tobin’s Q, cash flow rates and sales growth, quarterly GDP growth as a proxy for economic condition, as well as firm and quarter fixed effects. Consistent with the hypothesis that political uncertainty associated with unified governments dampens investment rates, the decrease in the conditional mean investment rate under a unified government is economically meaningful and statistically significant.

Table 3.3: BASELINE RESULTS

	<i>Dependent Variable: $CAPX_{ist}/TA_{ist-1} \times 100$</i>							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
U_t	-0.44*** (0.15)	-0.46*** (0.15)	-0.44*** (0.15)	-0.46*** (0.15)	-0.46*** (0.15)	-0.44*** (0.15)	-0.22*** (0.06)	-0.51*** (0.15)
<i>Presidt Election_t</i>		-0.05 (0.15)						0.02 (0.14)
<i>Dem President_t</i>			0.06 (0.15)					0.04 (0.13)
<i>State U_{st}</i>				-0.28*** (0.10)				-0.26*** (0.09)
<i>Gub Election_{st}</i>					0.09 (0.13)			0.10 (0.11)
<i>Dem Governor_{st}</i>						-0.05 (0.14)		0.08 (0.10)
$\ln EPU_t$							-0.21*** (0.06)	-0.63*** (0.18)
Observations	630,025	630,025	630,025	630,025	630,025	630,025	630,025	630,025
R-squared	0.44	0.44	0.44	0.44	0.44	0.44	0.44	0.44
Firm Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Economic Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Qrt FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

This table presents the baseline results. The dependent variable is investment rate, defined as capital expenditures scaled by beginning of the period total assets. The independent variable of interest is U_t , the unified federal government dummy. Additional independent variables include democratic president dummy (*Dem President_t*), presidential election dummy (*Presidt Election_t*), unified state government dummy (*State U_{st}*), democratic governor dummy (*Dem Governor_{st}*), gubernatorial election dummy (*Gub Election_{st}*), the natural logarithm of economic policy uncertainty index ($\ln EPU_t$) from Baker, Bloom and Davis (2016), quarterly gdp growth rate as a control for economic condition, and firm controls — Tobin’s Q, cash flow ratio, sales growth. In column (7), I standardise both U_t and $\ln EPU_t$ to compare their economic magnitudes. See the Appendix for Chapter 3 for a detailed description of how I calculate each variable. Robust standard errors are clustered at the quarter, firm and state level, and reported in brackets. *, ** and *** indicate statistical significance at the 10%, 5% and 1% level.

The following columns in Table 3.3 add federal and state-level political variables. Julio and Yook (2012) use the national election year dummy to capture elec-

toral uncertainty and provide evidence that uncertainty concerning the chief executive of a country reduces investment rates. Accordingly, I add presidential election dummy in column (2) of Table 3.3. The result shows that U_t dominates *Presidential Election_t* in terms of both economic and statistical significance. This is consistent with the notion that political uncertainty related with the alignment between the executive and the legislative is more important than the electoral uncertainty concerning the sole control of presidency. It might be because uncertainty about government actions depends more on both the executive and the legislature in a democratic society with checks and balances, although national leaders do have certain political power. Julio and Yook (2012) also find that the electoral uncertainty dampens investments less under a government with more checks and balances. Since there is empirical evidence showing that stock returns and economic growth are higher under Democratic presidents (Alesina and Sachs 1988; Santa-Clara and Valkanov 2003; Blinder and Watson 2016; Pástor and Veronesi 2020), I also include Democratic president dummy in column (3). The estimation result shows that the negative impact of a unified government remains while Democratic president is not significant.

In column (4), I add unified state government dummy, i.e., $StateU_{st}$, and find that the coefficient on U_t is more negative than that on $StateU_{st}$, not significantly though.¹⁹ This suggests that a unified government at federal level might be more important than at state level. Jens (2017) shows that the gubernatorial election dummy is related with less capital investment. After adding the gubernatorial election dummy ($GubElection_{st}$) in column (5), I find that U_t dominates $GubElection_{st}$ in terms of both economic and statistical significance. I additionally include democratic governor dummy in column (6), and the result show that U_t is robust to $DemGovernor_{st}$ which is statistically insignificant. In column (7) I standardise both U_t and $lnEPU_t$, to compare their economic magnitudes. The result shows that their estimates are similar in terms of both economic and statistical significance, and thus their effects on investment rates of one standard deviation increase are similar.

Depending on the specification, reductions in conditional investment rates range from -0.51 to -0.44, except for the regression result in column (7) where I use standardised U_t . The estimates reported in column (8), which represents the baseline specification throughout the rest of the analysis, show that, compared to a divided government, corporate investment rates decline by an average of 0.51 percentage points over a one-quarter period under a unified government. In terms of economic magnitude, the estimate equals about 0.10 standard deviations, and translates into 13.9% reduction in investment rates relative to the average investment rate under a divided government.

Table 3.4 presents robustness tests for the baseline results. In column (1) and (2), I exclude recessions and financial crisis separately, due to the concern that

¹⁹Based on the estimation result in column (8), I find that the estimate of U_t is significantly more negative than that of $StateU_{st}$ at 5% level.

unified federal government might be correlated with economic contractions that cannot be captured by lagged quarterly GDP growth rate. I also exclude political sensitive firms²⁰ and firms headquartered in states with gubernatorial elections coinciding with presidential elections²¹ in column (3) and (4) respectively, in order to ensure that these special groups of firms do not drive my baseline results. Lagged investment has been found to be correlated with contemporary investment in many samples (Eberly, Rebelo, and Vincent 2011). To address the concern that the autocorrelation in capital investment may contribute to the negative effects of U_t , I include lagged investment rates in column (5). The main result is robust to various tests, consistently showing a significant negative association between unified governments and corporate investment. The point estimates are around -0.5 in columns 1 through 4, while column 5 shows a slightly smaller magnitude of -0.26 .

Table 3.4: ROBUSTNESS TESTS

<i>Dependent Variable: $CAPX_{ist}/TA_{ist-1} \times 100$</i>					
	(1) Exclude Recession	(2) Exclude Financial Crisis	(3) Exclude Sensitive Industries	(4) Exclude Political Presidt Election	(5) Lagged Investment
U_t	-0.49** (0.20)	-0.51** (0.20)	-0.52*** (0.14)	-0.52*** (0.16)	-0.26*** (0.09)
Observations	524,057	557,915	560,119	517,507	611,638
R-squared	0.45	0.44	0.44	0.40	0.55
Firm Controls	Yes	Yes	Yes	Yes	Yes
Economic Controls	Yes	Yes	Yes	Yes	Yes
Political Controls	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes
Qrt FE	Yes	Yes	Yes	Yes	Yes

This table presents robustness tests for the baseline results shown in Table 3.3. Column (1) and (2) exclude recessions and financial crisis respectively. Column (3) excludes political sensitive industries. Column (4) excludes firms in states with elections coinciding with presidential elections. Column (5) includes lagged investment rate. The dependent variable is investment rate, defined as capital expenditures scaled by beginning of the period total assets. The independent variable of interest is U_t , the unified federal government dummy. Additional independent variables include democratic president dummy ($Dem\ President_t$), presidential election dummy ($Presidt\ Election_t$), unified state government dummy ($State\ U_{st}$), democratic governor dummy ($Dem\ Governor_{st}$), gubernatorial election dummy ($Gub\ Election_{st}$), the natural logarithm of economic policy uncertainty index ($lnEPU_t$) from Baker, Bloom and Davis (2016), quarterly gdp growth rate as a control for economic condition, and firm controls — Tobin's Q, cash flow ratio, sales growth. See the Appendix for Chapter 3 for a detailed description of how I calculate each variable. Robust standard errors are clustered at the quarter, firm and state level, and reported in brackets. *, ** and *** indicate statistical significance at the 10%, 5% and 1% level.

²⁰Based on the findings and references of Herron, Hall, and Kerr (1999), I classify firms in tobacco products, pharmaceuticals, health care services, defense, petroleum and natural gas, telecommunications, and transportation industries as politically sensitive.

²¹These states are Delaware, Indiana, Missouri, Montana, North Carolina, North Dakota, Utah, Washington and West Virginia.

3.4.2 Cross-Section Examination

My evidence so far has shown robust and negative effects of unified governments on corporate investment. I now extend the analysis by exploiting cross-section variation and exploring the heterogeneous effects of unified governments, to check whether political uncertainty is the underlying economic mechanism. I add the following regression specifications, to conduct the cross-section examination:

$$\begin{aligned} \frac{CAPX_{ist}}{TA_{ist-1}} = & \alpha_i + \beta_1 U_t + \beta_2 I_{it} + \beta_3 U_t \times I_{it} + \beta_4 PresidtElection_t + \beta_5 DemPresidt_t \\ & + \beta_6 StateU_{st} + \beta_7 GubElection_{st} + \beta_8 DemGovernor_{st} + \beta_9 \ln EPU_t \\ & + \beta_{10} TQ_{ist-1} + \beta_{11} \frac{CF_{ist}}{TA_{ist-1}} + \beta_{12} SG_{ist-1} + \delta_1 \% \Delta GDP_{t-1} + QRT_t + \epsilon_{ist} \end{aligned} \quad (3.2)$$

$$\begin{aligned} \frac{CAPX_{ist}}{TA_{ist-1}} = & \alpha_i + \beta_1 I_{it} + \beta_2 U_t \times I_{it} + \beta_3 PresidtElection_t + \beta_4 DemPresidt_t \quad (3.3) \\ & + \beta_5 StateU_{st} + \beta_6 GubElection_{st} + \beta_7 DemGovernor_{st} + \beta_8 \ln EPU_t \\ & + \beta_9 TQ_{ist-1} + \beta_{10} \frac{CF_{ist}}{TA_{ist-1}} + \beta_{11} SG_{ist-1} + \delta_1 \% \Delta GDP_{t-1} + QRT_t \\ & + Year_t + \epsilon_{ist} \end{aligned}$$

There are two specifications above. Both include two new variables relative to my baseline specification: I_{it} which is a firm-level characteristic and the interaction term between U_t and I_{it} . Equation (3.2) is the same as my baseline specification except the two new variables while Equation (3.3) drops U_t but adds $Year_t$. In this section, I am more interested in the heterogeneous effects of U_t on investment, instead of its average effects. This allows us to replace U_t in the equation (3.1) with a year fixed effect, i.e., $Year_t$ in equation (3.3), which could help control for macroeconomic confounding forces.

Government Contract Dependence

Government procurement could be a channel through which unified government affects firms. Hassan et al. (2019) show that firms which are more dependent on government contracts are more exposed to political uncertainty. Baker, Bloom and Davis (2016) find that firms relying more on government contracts for sales reduce investment rates more when there is policy uncertainty. I seek to test the hypothesis that, if unified governments decrease corporate investment through political uncertainty, it should do so more severely for firms with greater exposure to government.

To test this hypothesis, I use quarterly data on firm-level government contracts from Hassan et al.(2019), and construct an index of dependence on government contract. The index is constructed as follows: I aggregate sales from the federal government for each firm by year, and obtain the yearly ratio of federal purchases to total revenues, and then average these ratios for each firm. Then I define a dependence dummy variable that equals one for firms with a positive index and equals zero otherwise. I first conduct subsample analyses, running the baseline specification for firms dependent and not dependent on government contracts separately. Then I use the full sample to estimate the specifications given by equation (3.3) and (3.4). Since the government contract dependence index is a time-invariant characteristic, it would be absorbed by firm fixed effects.

Table 3.5: GOVERNMENT CONTRACT DEPENDENCE

	<i>Dependent Variable: $CAPX_{ist}/TA_{ist-1} \times 100$</i>			
	(1) Dependent	(2) Not Dependent	(3)	(4)
U_t	-0.23*** (0.05)	-0.15** (0.07)	-0.13* (0.07)	
$U_t \times Dependence_i$			-0.15** (0.05)	-0.14* (0.07)
Observations	101,971	165,367	267,338	267,338
R-squared	0.52	0.54	0.54	0.54
Year Range	2002-2016	2002-2016	2002-2016	2002-2016
Firm Controls	Yes	Yes	Yes	Yes
Economic Controls	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes
Year FE	No	No	No	Yes
Qrt FE	Yes	Yes	Yes	Yes

This table examines whether government contract dependence affects the effect of unified government on firm level capital investment. The government contract dependence proxy is discussed in Section 3.4.2. In the first two columns, I perform subsample analysis by splitting the sample into two subgroups, and then examine the interactive effects between the unified government dummy and the dependence dummy in the last two columns. The dependent variable is investment rate, defined as capital expenditures scaled by beginning of the period total assets. The independent variable of interest is U_t , the unified federal government dummy. Additional independent variables include democratic president dummy ($Dem\ President_t$), presidential election dummy ($Presidt\ Election_t$), unified state government dummy ($State\ U_{st}$), democratic governor dummy ($Dem\ Governor_{st}$), gubernatorial election dummy ($Gub\ Election_{st}$), the natural logarithm of economic policy uncertainty index ($lnEPU_t$) from Baker, Bloom and Davis (2016), quarterly gdp growth rate as a control for economic condition, and firm controls — Tobin's Q, cash flow ratio, sales growth. See the Appendix for Chapter 3 for a detailed description of how I calculate each variable. Due to the data availability of government contract dependence, the sample period is from 2002 to 2016. Robust standard errors are clustered at the quarter, firm and state level, and reported in brackets. *, ** and *** indicate statistical significance at the 10%, 5% and 1% level.

Table 3.5 presents the estimation results which are consistent with my hypothesis.²² I find that the negative effect on investment of unified governments is more pronounced for firms with positive exposure to government purchases. The estimate of U_t is bigger for the dependent group, when comparing results in column (1) and (2). In column (3) and (4), the estimates of the interaction term are both signifi-

²²Due to data availability of government contracts, the sample period is from 2002 to 2016.

cantly negative, regardless of the inclusion of year fixed effects. Specifically looking at column (3), the estimates of U_t (-0.13) and $U_t \times Dependence_i$ (-0.15) imply that the negative effect of U_t is 115% stronger for firms who rely on government contracts.

Product market competition

The effect of unified governments on capital investment might also be moderated by firms' competitive environment. While firms faced with uncertainty delay their investments due to the option value of "wait and see", competition erodes the option value because firms fear pre-emption from competitors (Grenadier 2002). Patnaik (2016) finds that firms in highly competitive industries increase investment in response to higher uncertainty.²³ Therefore, if a unified government depresses investment through uncertainty, then firms in highly competitive industries should reduce investment less in response to this concentrated political power.

To test this hypothesis, I use Lerner Index to proxy for product market competition, following the industrial organization literature. To calculate the price-cost margin, I calculate the firm's operating profit margin (sales minus costs²⁴ divided by sales) by year. Following Peress (2010) who assumes an industry is a market, I subtract the value-weighted average industry (based on 3-digit sic codes) price-cost margin to control for systematic differences in this measure across industries which are unrelated to the level of competition. Higher values indicate stronger market power and weaker competition. Then I define a competitive dummy variable that equals one if a firm's Lerner Index is below the median value and zero otherwise. I first conduct subsample analyses, running the baseline specification for firms in competitive and not competitive markets separately. Then I use the full sample to estimate the specifications given by equation (3.3) and (3.4).

Table 3.6 presents the estimation results which are consistent with my hypothesis. I find that the negative effect on investment of unified government is less pronounced for firms faced with more competition. The estimate of U_t is smaller for the competitive group, when comparing results in column (1) and (2). In column (3) and (4), the estimates of the interaction term are both significantly positive regardless of the inclusion of year fixed effects. Specifically looking at column (3), the estimates of U_t (-0.61) and $U_t \times Competition_{it}$ (0.19) imply that the negative effect of U_t is 31% weaker for firms in more competitive markets. Additionally, I find that the estimates of $Competition_{it}$ are significantly negative in both column (3) and (4), which represents the negative effect on capital investment of competition for firms under divided government.²⁵ This might be because competitive pressure increases

²³Patnaik exploits uncertainty associated with the El Nino Southern Oscillation cycle that affect a variety of industries in the U.S., and uses firm-level data from the Compustat database. He finds that firms in highly competitive industries increase investment in response to higher uncertainty.

²⁴The costs include the cost of goods sold and general and administrative expenses.

²⁵Considering the interaction term $U_t \times Competition_{it}$, the competition effect for firms under

the probability of investment values falling below their initial cost (Grenadier 2002), and thus decreases the expected profitability.²⁶

Table 3.6: PRODUCT MARKET COMPETITION

	Dependent Variable: $CAPX_{ist}/TA_{ist-1} \times 100$			
	(1) Not Competitive	(2) Competitive	(3)	(4)
U_t	-0.56*** (0.14)	-0.42*** (0.12)	-0.61*** (0.16)	
$Competitive_{it}$			-0.60*** (0.10)	-0.57*** (0.10)
$U_t \times Competitive_{it}$			0.19*** (0.06)	0.20*** (0.05)
Observations	308,208	321,312	630,024	630,022
R-squared	0.54	0.39	0.44	0.45
Firm Controls	Yes	Yes	Yes	Yes
Economic Controls	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes
Year FE	No	No	No	Yes
Qrt FE	Yes	Yes	Yes	Yes

This table examines whether product market competition affects the effect of unified government on firm level capital investment. The product market competition proxy is discussed in Section 3.4.2. In the first two columns, I perform subsample analysis by splitting the sample into two subgroups, and then examine the interactive effects between the unified government dummy and the competitive dummy in the last two columns. The dependent variable is investment rate, defined as capital expenditures scaled by beginning of the period total assets. The independent variable of interest is U_t , the unified federal government dummy. Additional independent variables include democratic president dummy ($Dem\ President_t$), presidential election dummy ($Presidt\ Election_t$), unified state government dummy ($State\ U_{st}$), democratic governor dummy ($Dem\ Governor_{st}$), gubernatorial election dummy ($Gub\ Election_{st}$), the natural logarithm of economic policy uncertainty index ($lnEPU_t$) from Baker, Bloom and Davis (2016), quarterly gdp growth rate as a control for economic condition, and firm controls — Tobin’s Q, cash flow ratio, sales growth. See the Appendix for Chapter 3 for a detailed description of how I calculate each variable. Robust standard errors are clustered at the quarter, firm and state level, and reported in brackets. *, ** and *** indicate statistical significance at the 10%, 5% and 1% level.

Investment Irreversibility

One important reason why uncertainty depresses investment is the irreversibility of investment (Bernanke 1983). Costly capital reversibility gives firms an incentive to delay investment when there is uncertainty, because firms would like to wait for more information. Therefore, the effect of uncertainty on investment depends on the level of capital reversibility. The more irreversible the investment is, the more benefits the waiting option provides for firms facing uncertainty. Gulen and Ion (2016) show that the negative relation between policy uncertainty²⁷ and capital unified governments are also significantly negative.

²⁶Theoretically, Tobin’s Q should reflect all relevant expectations of future profitability, including competition environment. In that sense, I should not be able to observe the significance of $Competition_{it}$. However, an additional assumption of perfect competition is required. This is too strong an assumption, which is highly likely to fail in my sample. Therefore, $Competition_{it}$ might capture some additional information about future profitability.

²⁷Gulen and Ion (2016) use the economic policy uncertainty index developed by Baker, Bloom and Davis (2016).

investment is significantly stronger for firms with a higher degree of investment irreversibility. Hence, I would expect that, when faced with the same uncertainty from a unified government, more irreversible firms would reduce investments more.

I use capital intensity ratio to proxy for irreversibility. The more specific a firm's capital (e.g., machinery and plant) is to production (Arrow 1968; Bertola and Caballero 1994), the more irreversible it is. Consequently, the firm might have no other option to acquire the capital for production, such as renting, than investments by itself.²⁸ Then it might have a relatively higher capital intensity ratio, compared with firms with more reversible investments. Under the assumption that the additional capital for expanded production is at the similar level of irreversibility to previous stock, firms with higher capital intensity ratio are more likely to invest in more irreversible projects. I calculate the capital intensity ratio, measured as net property, plant and equipment (PPE) divided by total assets, for each firm by year. I define an irreversible dummy variable that equals one for firms with a capital intensity ratio above the median value of the yearly sample distribution and equals zero otherwise.

Table 3.7: INVESTMENT IRREVERSIBILITY

	<i>Dependent Variable: $CAPX_{ist}/TA_{ist-1} \times 100$</i>			
	(1) Reversible	(2) Irreversible	(3)	(4)
U_t	-0.27*** (0.09)	-0.76*** (0.21)	-0.35*** (0.12)	
$Irreversible_{it}$			1.56*** (0.10)	1.79*** (0.09)
$U_t \times Irreversible_{it}$			-0.38** (0.19)	-0.34* (0.18)
Observations	318,422	311,149	630,025	630,023
R-squared	0.40	0.41	0.45	0.46
Firm Controls	Yes	Yes	Yes	Yes
Economic Controls	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes
Year FE	No	No	No	Yes
Qrt FE	Yes	Yes	Yes	Yes

This table examines whether investment irreversibility affects the effect of unified government on firm level capital investment. The investment irreversibility proxies are discussed in Section 3.4.2. I perform subsample analysis by splitting the sample into two subgroups in the first two columns, and then examine the interactive effects between the unified government dummy and the irreversibility proxies in the last two columns. The dependent variable is investment rate, defined as capital expenditures scaled by beginning of the period total assets. The independent variable of interest is U_t , the unified federal government dummy. Additional independent variables include democratic president dummy ($Dem\ President_t$), presidential election dummy ($Presidt\ Election_t$), unified state government dummy ($State\ U_{st}$), democratic governor dummy ($Dem\ Governor_{st}$), gubernatorial election dummy ($Gub\ Election_{st}$), the natural logarithm of economic policy uncertainty index ($lnEPU_t$) from Baker, Bloom and Davis (2016), quarterly gdp growth rate as a control for economic condition, and firm controls — Tobin's Q, cash flow ratio, sales growth. See the Appendix for Chapter 3 for a detailed description of how I calculate each variable. Robust standard errors are clustered at the quarter, firm and state level, and reported in brackets. *, ** and *** indicate statistical significance at the 10%, 5% and 1% level.

²⁸In Arrow's work(1968), one important result from the assumption of irreversible investment is that investment must be non-negative.

Table 3.7 presents the estimation results. Similar to the previous sections, I perform subsample analyses and then add the irreversible dummy and its interaction term with U_t . I find that the negative effect on investment of unified governments is more pronounced for firms with more irreversible investments. The estimate of U_t is bigger for the irreversible group, when comparing results in column (1) and (2). In column (3) and (4), the estimates of the interaction term are both significantly negative, regardless of the inclusion of year fixed effects. Specifically looking at column (3), the estimates of U_t (-0.35) and $U_t \times Irreversible_{it}$ (-0.38) imply that the negative effect of U_t is 109% stronger for more irreversible firms. This is consistent with my hypothesis that the uncertainty from unified governments would be more pronounced for firms with more irreversible investments. Additionally, I observe that the estimates of $Irreversible_{it}$, i.e. the effect of investment irreversibility for firms under divided government, are both significantly positive in column (3) and (4).²⁹ This might be because the more irreversible its capital is, the more a firm needs to make investments by itself, due to the irreversibility, i.e., the installed capital is highly specialized, and thus it has little or no value unless used in production. Another possible reason could be that firms with a high capital intensity ratio tend to make more capital investment.

3.4.3 Alternative Explanation — Political Sentiment

In this section, I investigate another potential channel through which unified federal governments may lead to reduction in investment rates. The primary concern with my interpretation of the baseline results is that firms facing a unified government may simultaneously receive bad news associated with political events (negative political sentiment). If political sentiment drives my results, then I would expect only firms facing negative political news to reduce investment rates under a unified government. However, based on the political uncertainty hypothesis, investment expenditures of both firms with positive and negative political sentiment would decline.

To perform this test, I use the political sentiment measure constructed by Hassan et al. (2019), which is based on text analysis of firms' earnings conference calls.³⁰ The more positive the measure is, the better political news. To identify firms faced with good political news, I set a positive sentiment dummy to one if the political sentiment measure is above the median value of the yearly sample distribution, and zero otherwise. I split the sample into positive and negative sentiment groups based on the dummy, and perform the baseline regression on each group. I also interact

²⁹Considering the negative estimates of the interaction term $U_t \times Irreversible_{it}$ in column (3) and (4), the overall effect of irreversibility on investment for firms faced with unified governments, i.e., the sum of coefficients on $Irreversible_{it}$ and $U_t \times Irreversible_{it}$, is still significantly positive.

³⁰According to Hassan et al (2019), this political sentiment measure contains firms' significant discussions of positive and negative news about legislation, regulation, and government spending, etc.

the positive sentiment dummy with the unified government dummy. The first two columns in table 3.8 show the results of subsample analysis. I find that there is not much difference in the effect of U_t . The last two columns in table 3.8 show the results of interaction term. Since I am now interested in estimating the interaction term, I add year fixed effects in column (4); this could help control for any macroeconomic, cross-sectionally invariant forces. Regardless of adding year fixed effects or not, estimates in column (3) and (4) tell that there is no significant difference in the effect of unified governments between firms facing positive and negative political news. Overall, these results suggest that political sentiment is not the underlying mechanism through which a unified government depresses firm investment. Additionally, when I look at the estimates on $Political\ Sentiment_{it}$ in column (3) and (4), which represent the effects of sentiment for firms under divided governments, they are both significantly positive.³¹ The better the political news is, the more firms would invest. This implies that the measure of political sentiment contains some additional information concerning investment that is not captured by the measure of Tobin's Q, which is meant to reflect the all relevant expectations of future profitability.

3.5 Conclusion

This paper documents that firms' investment rates tend to be lower under a unified government when compared with under a divided government. Specifically, I find that corporate investment rates decline by an average of 13.9% under a unified government relative to under a divided government. In addition to the main results, I find that the effect is more salient and pronounced for firms with greater dependence on government contracts, in less competitive markets, and with a higher degree of investment irreversibility. These results point to political uncertainty as the most likely explanation for my findings. I also examine a potential alternative explanation, i.e., political sentiment, and find that firms faced with a unified government reduce their investment, regardless of whether the political news are good or bad to firms.

My results support the idea that democracy is conducive to economic growth through the channel of investment (Acemoglu et al. 2019). Specifically, I focus on an important democratic institution – checks and balances. When as a result of an election, power between the executive and legislature is more balanced between opposing political parties, private investment is enhanced. This is because the balance of power reduces political uncertainty and instability. I provide micro level evidence of this phenomenon. This implies that an important element of democratic systems, political balance, may increase GDP.

³¹Considering the negative estimates of the interaction term $U_t \times Political\ Sentiment_{it}$ in column (3) and (4), the overall effect of political sentiment on investment for firms faced with unified governments, i.e., the sum of coefficients on $Political\ Sentiment_{it}$ and $U_t \times Political\ Sentiment_{it}$ is still significantly positive.

Table 3.8: POLITICAL SENTIMENT

	<i>Dependent Variable: $CAPX_{i,t}/TA_{i,t-1} \times 100$</i>			
	(1) Positive	(2) Negative	(3)	(4)
U_t	-0.26*** (0.05)	-0.28*** (0.08)	-0.27*** (0.07)	
$Positive\ Sentiment_{it}$			0.07** (0.03)	0.07* (0.04)
$U_t \times Positive\ Sentiment_{it}$			-0.01 (0.04)	-0.01 (0.04)
Observations	65,987	63,805	130,434	130,434
R-squared	0.61	0.64	0.61	0.61
Year Range	2002-2016	2002-2016	2002-2016	2002-2016
Firm Controls	Yes	Yes	Yes	Yes
Economic Controls	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes
Year FE	No	No	No	Yes
Qrt FE	Yes	Yes	Yes	Yes

This table examines whether firms with positive and negative political sentiment responses differently to a unified government. The political sentiment index is discussed in Section 3.4.3. In the first two columns, I perform subsample analysis by splitting the sample into two subgroups, and then examine the interactive effects between the unified government dummy and the positive sentiment dummy in the last two columns. The dependent variable is investment rate, defined as capital expenditures scaled by beginning of the period total assets. The independent variable of interest is U_t , the unified federal government dummy. Additional independent variables include democratic president dummy ($Dem\ President_t$), presidential election dummy ($Presidt\ Election_t$), unified state government dummy ($State\ U_{st}$), democratic governor dummy ($Dem\ Governor_{st}$), gubernatorial election dummy ($Gub\ Election_{st}$), the natural logarithm of economic policy uncertainty index ($lnEPU_t$) from Baker, Bloom and Davis (2016), quarterly gdp growth rate as a control for economic condition, and firm controls — Tobin’s Q, cash flow ratio, sales growth. See the Appendix for Chapter 3 for a detailed description of how I calculate each variable. Due to the data availability of political sentiment measure, the sample period is from 2002 to 2016. Robust standard errors are clustered at the quarter, firm and state level, and reported in brackets. *, ** and *** indicate statistical significance at the 10%, 5% and 1% level.

The results presented in this paper should be interpreted as evidence of association rather than causation. Although I document robust correlations between partisan controls of the executive and legislative branches and firm investment behavior, the research design does not allow for definitive causal inference. There may be unobserved factors or omitted variables that influence both partisan alignment and firm investment decisions. Future research could seek to identify the causal effects of political institutions on private investment and, by extension, economic growth.

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Chapter 4

Do Political Party Affiliations of Leaders Matter for Economic Growth: Evidence from the US States

Abstract

I study how political party affiliations of state leaders affect state economic growth in the US. Exploiting a sharp regression discontinuity design for close gubernatorial elections, I find that Republican governors cause a significant and substantial short-term increase in real GDP per capita growth and real GDP growth in the states. My baseline results show that, over the four-year term, Republican governors increase at an annual rate real GDP per capita growth and real GDP growth, relative to the average state trend, by 2.0 and 2.6 percentage points, respectively. These findings suggest: 1. leaders, specifically political party affiliations of leaders, matter for short-term economic growth in democratic regimes; 2. election outcomes could have causal impacts on economic outcomes.

4.1 Introduction

Do political leaders matter for growth? Empirical evidence shows that individual leaders in autocracies and particularly those without parties or legislatures could exert large effects on growth (Jones and Olken 2005). On the other hand, leaders might also matter for economic growth in the democratic regimes where institutions are strong. Specifically, political party affiliations of leaders might matter. For example, the US economy has experienced higher economic growth when the president of the US is Democratic rather than Republican (Blinder and Watson 2016, Pástor and Veronesi 2020). However, the party affiliation of a president could be endogenous to macroeconomic performance which is often considered as a strong predictor of US

presidential election outcomes.

In the US, the states are independent entities within the system of federalism. A US governor serves as the chief executive and commander-in-chief in each state, which is similar to the role of a US president to the country. Governors often have considerable power within the state they are selected, such as state executive orders, and have significant influence over the legislature and judiciary, such as being able to veto state bills and to appoint judges. Governors have large autonomy in areas reserved to the states although they are subordinate to the president¹. Moreover, there are fifty states in the US, which provides more variation in political party affiliation of leaders to exploit and thus to establish causality. Therefore, I ask: Does the political party affiliation of a state leader have a causal impact on state economic growth? Is the partisan pattern at state level the same as that at national level?

To estimate the causal effect of political party affiliations of leaders on economic growth, I employ a sharp regression discontinuity (RD) design that uses close elections as an exogenous source of variation in political party affiliations for the governor position. Precisely speaking, this paper estimates the “local average treatment effect” of the political party affiliation of a governor on state economic growth. I use this identification strategy because a comparison of economic growth in states governed by a Democrat to those governed by a Republican may give biased estimates of the impact of political party affiliation. For example, there might be some potential endogenous problem, as it is well-known that macroeconomic performance is a strong predictor of election outcomes which includes the political party affiliation. To identify the causal impact of governor partisanship, I compare economic growth in states where the Democratic party barely wins to outcomes in states where Republican party barely wins. In order for this regression discontinuity design to identify the causal effect of party affiliation, it is essential that variation in governor party affiliations is as good as random in states with close elections. I provide empirical evidence in support of this identification assumption. I also discuss concerns raised in recent literature regarding regression discontinuity estimates of the causal effects of winning politician characteristics (Marshall 2024) in Section 4.4.2 Robustness.

I combine the economic data and election data at state level. My sample includes 588 gubernatorial elections in which the margin victory or loss of Democratic (versus Republican) governor candidates can be computed, and data is available for real GDP per capita and real GDP growth. The context of the US gubernatorial elections has two advantages that allows me to use a sharp regression discontinuity design to study political party affiliations of leaders and economic growth, especially compared with the US presidential and mayoral elections. First, the election rule of US governors provides a good setting for a sharp regression discontinuity design.

¹For example, President Trump declared his intention to open up the U.S. economy in 2020 Easter. As he spoke, 17 governors across the country had placed their states on total lockdown, and another 11 had imposed such orders on the hardest-hit portions of their states.

The direct elections of US governors allow me to construct a running variable for which there is a clear threshold to decide which party wins. On the other hand, US presidential elections use the Electoral College system and permits the election of a winner who does not win the most votes, which makes it hard to apply a sharp RD design. Additionally, the state setting related with gubernatorial elections provides more data variation than the national setting with presidential elections. Second, economic growth data is largely available at state level while it is hard to find economic growth data at a more granular level.

I find that Republican governors elected in close elections cause higher state economic growth over the subsequent gubernatorial term, relative to Democratic governors elected under similar close-election conditions. Specifically, average annualised real GDP per capita growth under a Republican governor term is 2.0 percentage points higher than that under a Democratic governor term. The number is both statistically and economically significant. The mean of the close election sample is 1.4 percentage points and this treatment effect is 143 percent of the mean.² The standard deviation of the close election sample is 2.2 percentage point and this treatment effect is 91 percent of the standard deviation.³ Similar result applies to real GDP growth. These results suggest that leaders, specifically political party affiliations of leaders, matter for economic growth in a democratic society, while people tend to believe leaders matter for economic growth in autocracies. They also suggest that close election outcomes could have causal impacts on short-term macroeconomic performance, while it is more widely believed that macroeconomic performance affects election outcomes.

My paper contributes to the broad literature on leaders' effects.⁴ More specifically, related to my work is the literature of leaders' impact on the economic growth. Jones and Olken (2005) use a cross-country data and find that leaders matter for growth. Their results suggest that the effects of individual leaders are strongest in autocratic settings where there are fewer constraints on a leader's power. On the other hand, leaders could also matter for economic growth in democratic societies where institutions are strong. For example, Blinder and Watson (2016), as well as Pástor and Veronesi (2020), find that political party affiliations of leaders is important to economic growth in the US. Specifically, they find that the US economy has experienced higher economic growth when the president of the United States is a Democrat rather than a Republican. However, this documented relationship between Democratic presidents and economic growth is a correlation. I exploit the state-level variation in the US to study the causal effect from political party affilia-

²The mean of the full sample is 1.3 percentage points, and this treatment effect is 154 percent of the mean.

³The standard deviation of the full sample is also 2.2 percentage point.

⁴Previous studies have examined leaders' effects on fertility-related beliefs and behavior (Bassi and Rasul 2017), the diffusion of radical and innovative ideas (Becker, Hsiao, Pfaff and Rubin 2020), and war (Dippel and Heblich 2021, Cagé, Dagorret, Grosjean, and Jha 2021), and voting behavior (Wang 2021), etc.

tion of leaders to economic growth in a democratic regime. I find that Republican governors cause higher state economic growth, which is different from the partisan pattern at national level, i.e., higher economic growth under Democratic presidents. My results suggests that leaders, specifically political party affiliations of leaders, could have a causal impact on short-term economic growth in a democratic setting.

My paper relates to a second strand of literature on the effect of partisanship on economic outcomes. Using an international setting, Girardi (2020) estimates the effect of partisan electoral victories on stock and bond markets and find that left-wing electoral victories cause significant and substantial short-term decreases in stock market valuations. At national level, previous studies have examined the effect of partisanship on consumer spending (Mian, Sufi, and Khoshkhoh 2021), fertility (Dahl, Lu, and Mullins 2021), and entrepreneurship (Engelberg, Guzman, Lu, and Mullins 2021), etc. Two recent articles study partisanship of presidents and economic growth. Both Blinder and Watson (2016) and Pástor and Veronesi (2020) document a correlation between economic growth and partisanship of presidents, i.e., higher growth under Democratic presidents than under Republican presidents.

Existing work provides evidence that partisanship could influence politicians' policy choices and outcomes at state level.⁵ Besley and Case (2003) find that state spending per capita is higher when there is a higher fraction of Democrat party seats in the state legislature. Leigh (2008) find that Democratic governors tend to implement a higher minimum wage, preside over lower after-tax inequality and lower unemployment rates, and oversee a lower incarceration rate. Beland (2015) estimate the impact of the partisanship of US governors on labor-market outcomes. His results show that Democratic governors cause an increase in the annual hours worked by blacks relative to whites, which leads to a reduction in the racial earnings gap between black and white workers. This paper studies the causal impact of partisanship on economic growth, which has received little attention in the literature.

There is a recent literature that has used regression discontinuity to identify the causal effects of election outcomes. Early empirical work used the approach of global polynomial approximation, to implement the regression discontinuity design (e.g., Ferreira and Gyourko 2009, Belano 2015). They used flexible higher-order polynomials over the entire support of the data. According to Cattaneo, Idrobo, and Titiunik (2019), however, it is widely recognized that a global polynomial approach does not provide good point estimators and inference procedures for the RD treatment effect. Modern empirical work employs the method of local polynomial approximation, which is considered non-parametric, to identify the effect of electoral outcomes. For example, Girardi (2020) uses this non-parametric approach to implement a fuzzy RD design and to study the effect of partisan electoral victories on stock and bond markets at international level. Akhtari, Moreira and Trucco (2022) uses this local

⁵Fernando and Gyourko (2009) study partisanship at city level and find there is no partisan effect on the size of city government, or the allocation of local public spending, or crime rate.

polynomial method to study how political turnover in mayoral elections in Brazil affects public service provision by local government. This paper is, to the best of my knowledge, the first to employ this local polynomial approach to study the causal impact of the election outcome – political party affiliation of leaders, on economic growth – one important index of macroeconomic performance which is usually considered as a strong indicator of election outcomes. I find that Republican governors cause higher economic growth and this suggests that election outcomes could have causal impacts on economic outcomes (e.g., Ferreira and Gyourko 2009, Beland 2015, Girardi 2020).

The paper is structured as follows. Section 4.2 presents the data and Section 4.3 describes the empirical strategy. Section 4.4 presents the empirical results, including baseline results and robustness checks. Section 4.5 concludes.

4.2 Data

I combine gubernatorial election outcomes in the US with state economic data. I first describe the data sources for electoral and economic information and the construction of key variables in the RD design. Then I describe the sample selection and summary statistics.

Electoral Data: The electoral data comes from Voting and Elections Collection by CQ Press Library,⁶ which provides information of the party affiliation of governors and votes for each candidate. This allows me to compute the running variable in my regression discontinuity design: the Democratic candidate’s marginal vote share, defined as the vote share of the Democratic candidate minus the vote share of the Republican candidate.

Economic Data: Real GDP data comes from Klarner’s dataset about State Economic and Government Finance (Klarner 2015) from Harvard Dataverse.⁷ As this dataset only covers from 1963 to 2014, I update it through 2019 using the original sources (the U.S. Bureau of Economic Analysis⁸ and U.S. Bureau of Labor Statistics⁹) and the author’s notes. Data for each state’s population comes from the U.S. Census Bureau.¹⁰ Based on these information, I calculate the dependent variables, i.e., average annualised real GDP per capita and real GDP growth over each governor’s term.

Sample Selection: I take a number of steps to select state-election cycles into my sample, as the unit of my sample is a state-election cycle. A state-election cycle starts from the first year when the newly elected governor comes into power and ends

⁶<https://library.cqpress.com/>

⁷<https://dataverse.harvard.edu/dataset.xhtml?persistentId=doi:10.7910/DVN/CJBTGD>

⁸<https://www.bea.gov/>

⁹<https://www.bls.gov/>

¹⁰<https://www.census.gov/>

in the year when the next gubernatorial election is held. I consider only elections which is followed by a four-year governor term. I drop Vermont and New Hampshire which have a two-year term. Some states used to experience two-year governor terms: Arkansas before 1986, Arizona, New Mexico and Wisconsin before 1970, Florida 1965-1966, Iowa, Kansas, South Dakota and Texas before 1974, Illinois 1977-1978, Massachusetts, Michigan and Nebraska before 1966, Minnesota before 1962, North Dakota before 1964, Rhode Island before 1994, Oregon 2015-2018.¹¹ They are also excluded from my sample. I also drop gubernatorial elections in which the top two candidates are both from one party and in which one of the top two candidates are independent, since I am interested in the partisan control (i.e., Democratic versus Republican) of state leaders. Thus this restriction drops 25 state-election cycles. I also drop election cycles during which the governors were elected through runoff elections, i.e., Arizona 1991-1994 and Louisiana 2016-2019. Overall, I am left with 588 observations of state election cycles between 1963 and 2019.

Summary Statistics: Table 4.1 shows descriptive statistics of the data. The unit of observation in this table is a state-election cycle. Column 1 shows state characteristic for the full sample, and column 2 shows these same characteristics for state-election cycles resulted from close elections. A close election is one when the Democratic governor candidate's vote share margin is between -0.11 and 0.11, which is chosen according to the optimal bandwidth selection of Calonico, Cattaneo, Farrell, and Titiunik (2019). Panel A shows election data and panel shows economic data. The subsample of close elections is similar to the full sample overall with the exception of the running variable, i.e., the Democratic marginal vote share, based on which a close election is defined.

Table 4.1: SUMMARY STATISTICS

	(1) Full Sample			(2) Close Elections		
	N	Mean	SD	N	Mean	SD
Panel A: Election Data						
Democratic Marginal Vote Share	588	0.0077	0.2007	286	0.0002	0.0608
Democratic Governor Dummy	588	0.5136	0.5009	286	0.5000	0.5009
Panel B: Economic Data						
Average Real GDP per capita growth	588	0.0129	0.0218	286	0.0138	0.0218
Average Real GDP growth	588	0.0226	0.0233	286	0.0236	0.0223

Notes: This table shows descriptive statistics for states in the full sample and for states in the close election sample. A close election is one when the Democratic governor candidate's vote share margin is between -0.11 and 0.11, which is chosen according to the optimal bandwidth selection of Calonico et al. (2019)

¹¹Oregon 2016 is a special election between 2014 and 2018, The election determined who would fill the remaining two years of the term of Democratic governor John Kitzhaber, who was re-elected in 2014 and resigned in February 2015.

4.3 Empirical strategy

To estimate the effect of political party affiliation of governors on the economic growth, I rely on a sharp regression discontinuity design (RDD) using close gubernatorial elections in the US. This section describes the details of my RDD identification strategy and provides evidence in support of the identification assumption

4.3.1 Identification Strategy

To identify the effect of party allegiance of governor, I compare outcomes in states where the Democratic governors barely win to outcomes in states where Republican governors barely win; that is, I use a sharp regression discontinuity design for close elections.

My main specification is a quadratic regression for close elections, where “close” is defined according to the optimal bandwidth selection of Calonico et al. (2019), i.e., MSE-optimal bandwidth.¹² I estimate the effect of political party affiliation of governors on economic growth by estimating the following equation for states with close elections:

$$Y_{st} = a + \gamma D_{st} + f(MV_{st} - c) + \delta_s + \epsilon_{st} \quad (4.1)$$

where Y_{st} is the outcome variable of interest (average annualized real GDP per capita growth or average annualized real GDP growth) in state s , measured over the four-year governor term immediately following election t . The running variable of the RD is the Democratic candidate vote margin, MV_{st} , and it is computed as the vote share of the Democratic governor candidate minus the vote share of the Republican candidate. The cutoff point c is 0, as Democratic candidates win elections when $MV_{st} > 0$, while they lose when $MV_{st} < 0$. $f(\cdot)$ is a second order functional form,¹³ allowing for different quadratic forms on each side of the cutoff. The treatment variable is D_{st} , which is an indicator variable equal to one if a Democratic candidate wins the election (i.e., $MV_{st} > 0$) and, hence, the governor in the state s is a Democrat over the term following the election t . Following Beland (2015), I include state fixed effects δ_s to control for time-invariant characteristics of states.¹⁴ ϵ_{st} is the error term.

¹²This is one common mean squared error-optimal bandwidth selector for the RD treatment effect estimator.

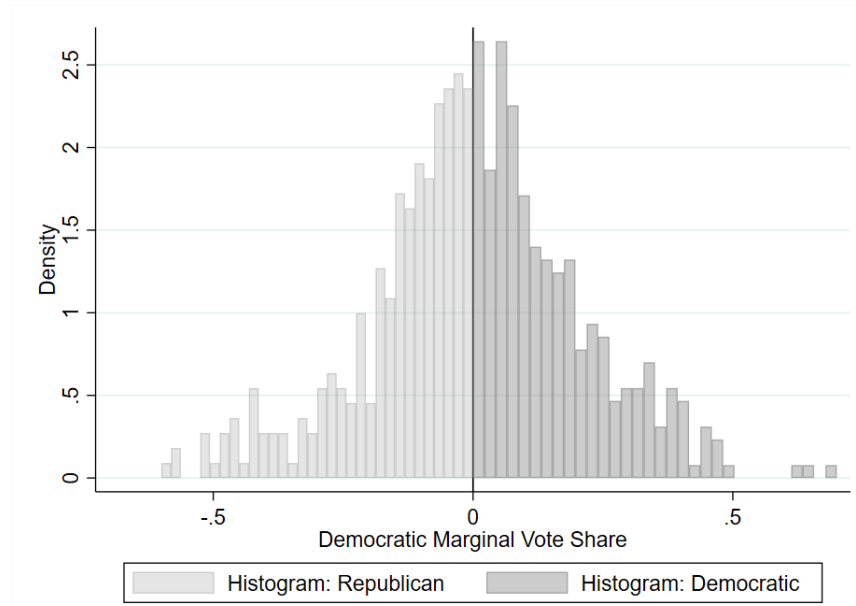
¹³Graphical illustrations, i.e., Figure 4.3 (or 4.4) and Figure A4.1 (or A4.2) in Appendix, show that a polynomial of order 2 fits data better than that of order 1. According to Cattaneo et al. (2019), a local quadratic fit also reduces the approximation error in the estimation of the RD effect. The robustness test later will show my main result also holds for first order functional form.

¹⁴According to Cattaneo et al. (2019), predetermined covariates can be included to improve estimation precision. Following Johnson (2020), I also check for smoothness in baseline covariates by running a regression with D_{st} as the dependent variable and state fixed effect dummies as right-hand side variables, and conducting an F-test that coefficients on all state dummies are equal

4.3.2 Identification Assumption

To estimate the causal effect of Democratic or Republican governors, the key identification assumption is that potential outcomes are continuous around the cut-off $MV_{st} = 0$ and, thus, any discontinuity in outcomes at the cutoff is the result of political party affiliation of governors. Essentially the identification assumption is that in competitive elections, whether a Democratic or a Republican governor candidate wins is “as good as” randomly assigned. To assess the plausibility of this assumption, I examine whether, in a local neighborhood near the cutoff, the number of observations below the cutoff is surprisingly different from the number of observations above it. I find no evidence of sorting in the running variable, MV_{st} , around the zero threshold, which supports the continuity assumption required for the validity of the regression discontinuity design (Cattaneo, Idrobo, and Titiunik 2019).

Figure 4.1 DISTRIBUTION OF DEMOCRATIC MARGINAL VOTE SHARE



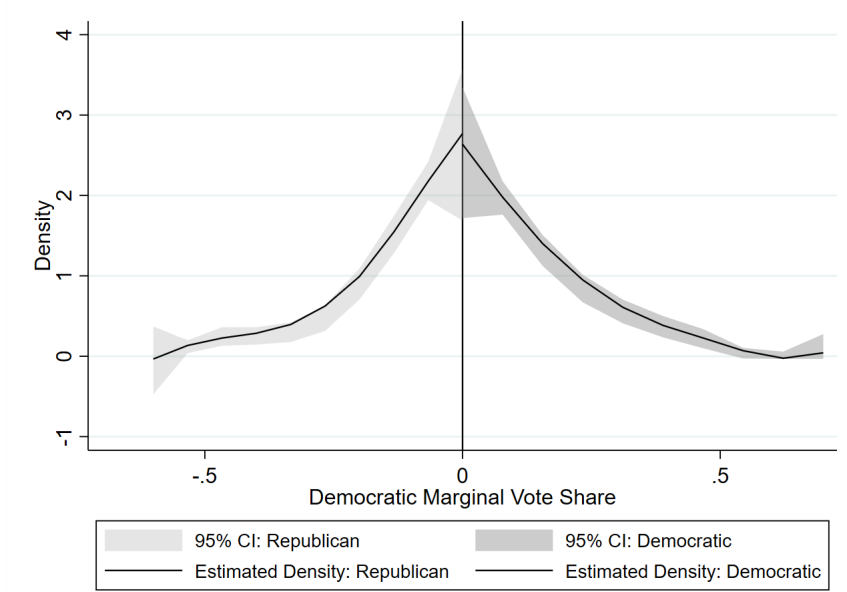
Notes: This histogram shows the distribution of the running variable in the RDD, MV_{st} , in my sample of state-election cycles. MV_{st} is computed as the vote share of the Democratic governor candidate minus the vote share of the Republican governor candidate. Therefore, it is a Democratic governor if $MV_{st} > 0$ while it is a Republican one if $MV_{st} < 0$.

Figure 4.1 shows the distribution of the running variable in my RDD, MV_{st} , for state-election cycles in our sample. State-election cycles with $MV_{st} < 0$ are when Republican governors are in power while state-election cycles with $MV_{st} > 0$ are when Democratic governors are in power. The distribution of MV_{st} seems fairly smooth around the $MV_{st} = 0$ threshold. Figure 4.2 shows the estimated density of

to 0. The results of this specification yield an F-statistic of 1.18 and p-value of 0.20, and thus show no evidence of a discontinuity in any time-invariant state characteristic.

MV_{st} with shaded 95% confidence intervals. The density estimates for Republican and Democratic groups at the zero threshold are very near each other, and the confidence intervals, i.e., shaded areas, overlap. The graphical illustrations, i.e., Figure 4.1 and 4.2, suggest that there is no difference in the density of Republican and Democratic observation at the cutoff. In fact, a formal test for manipulation of the running variable (McCrary 2008; Cattaneo et al. 2019) fails to reject the null hypothesis that MV_{st} is continuous at the zero threshold. The value of the test statistic is -0.1482 and the associated p-value is 0.8822.

Figure 4.2 ESTIMATED DENSITY OF DEMOCRATIC MARGINAL VOTE SHARE



Notes: This figure shows the estimated density of the running variable in the RDD, MV_{st} , in the RDD. The density estimates for Republican and Democratic groups at the zero threshold are very near each other, and the confidence intervals, i.e., shaded areas, overlap.

In the robustness section, I show that the results are robust to several alternative bandwidth selections. This further supports the absence of systematic manipulation of the running variable and, consequently, strengthens the validity of the continuity assumption.

4.4 Empirical Results

4.4.1 Baseline Results

Real GDP Per Capita Growth: Figure 4.3 shows average annualized real GDP per capita growth in states with close elections. Average annualized real GDP per capita growth is higher in states where a Republican governor has barely won compared to states where a Democratic governor has barely won. Table 4.2, columns

1 show the corresponding regression result. A Republican governor, compared with a Democratic one, leads to an increase of 2.0 percentage points in the average annual real GDP per capita growth rate. This is roughly 145 percent of the mean of, and 92 percent of the standard deviation of the average real GDP per capita growth rate in the sample of close elections.¹⁵ This also implies that over a four-year governor term, Republican governors could bring an additional growth of around 8 percentage points, in relative to Democratic ones.

Real GDP Growth: Figure 4.4 shows average real GDP growth in states with close elections. Real GDP growth is higher in states where a Republican governor has barely won compared to states where a Democratic governor has barely won. Table 4.2, column 2 shows the corresponding regression results. Republican governors lead to an increase of 2.6 percentage points in the average annual real GDP growth rate. This is roughly 110 percent of the mean of, and 117 percent of the standard deviation of the average real GDP growth rate in the sample of close elections.¹⁶ This also implies that over a four-year governor term, Republican governors could bring an additional growth of 10.8 percentage points, in relative to Democratic ones.¹⁷

Table 4.2: BASELINE

VARIABLES	(1) Average Real GDP PC Growth	(2) Average Real GDP Growth
RD_Estimate	-0.0204*** (0.0063)	-0.0264*** (0.0076)
Observations	286	277
Optimal Bandwidth	0.1102	0.1050
Polynomial Order	2	2
Kernel	Triangular	Triangular

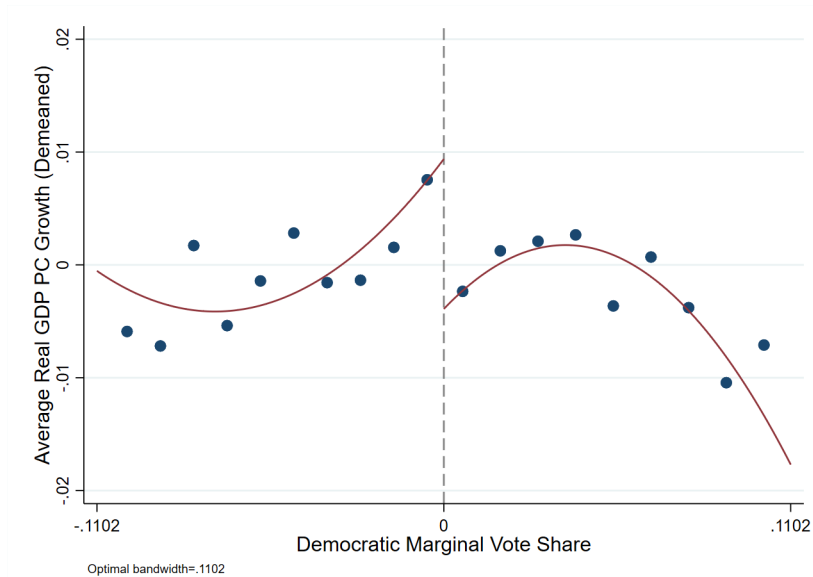
Notes: This table reports the coefficient on Democratic governor dummy estimating equation (1). Column 1 reports the result for outcome variable average real GDP per capita growth while column 2 reports for average real GDP growth.

¹⁵This is also roughly 155 percent of the mean of, and 92 percent of the standard deviation of the average real GDP per capita growth rate in the full sample.

¹⁶This is also roughly 115 percent of the mean of, and 112 percent of the standard deviation of the average real GDP growth rate in the full sample.

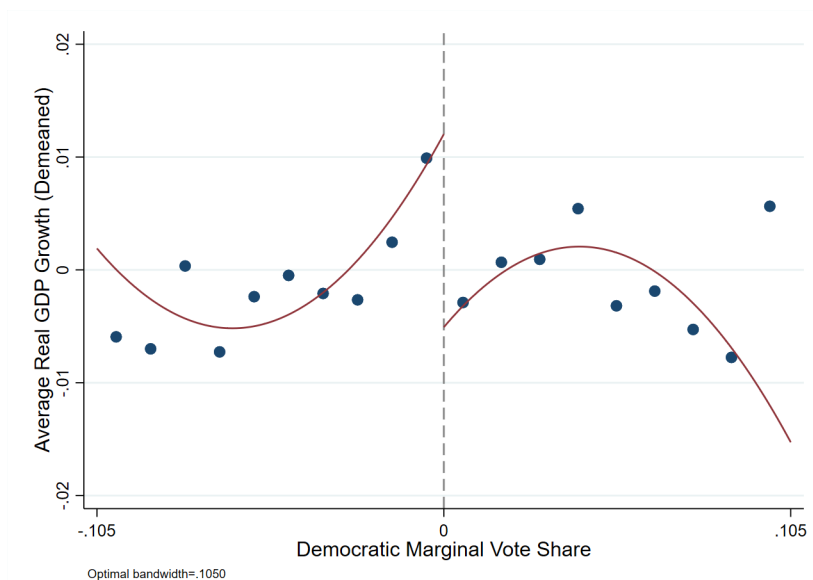
¹⁷The difference in the treatment effects between average real GDP per capita and real GDP growth rates might reflect the difference in population changes, as real GDP per capita is equal to real GDP divided by total population.

Figure 4.3 AVERAGE ANNUALISED REAL GDP PER CAPITA GROWTH



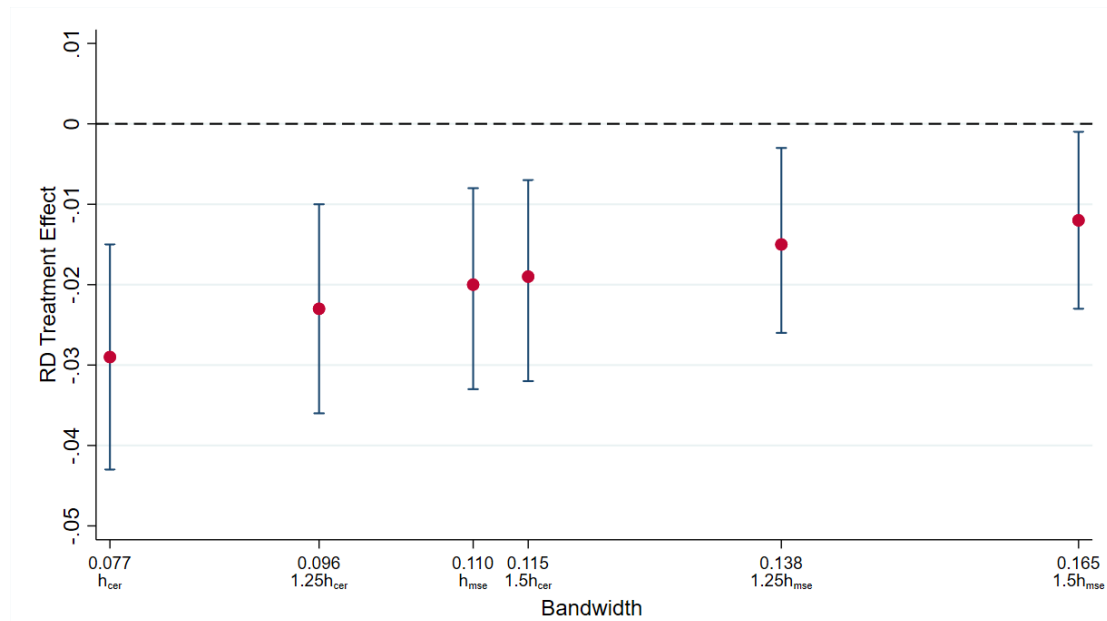
Notes: This figure shows the mean of average annualised real GDP per capita growth rate over a four-year governor term for states from 1963 to 2019 by bins of MV_{st} . States with $MV_{st} < 0$ experienced a Republican governor while states with $MV_{st} > 0$ experienced a Democratic one. The demeaned average real GDP per capita growth is the residual from the regression of average real GDP per capita growth on state fixed effects.

Figure 4.4 AVERAGE ANNUALISED REAL GDP GROWTH



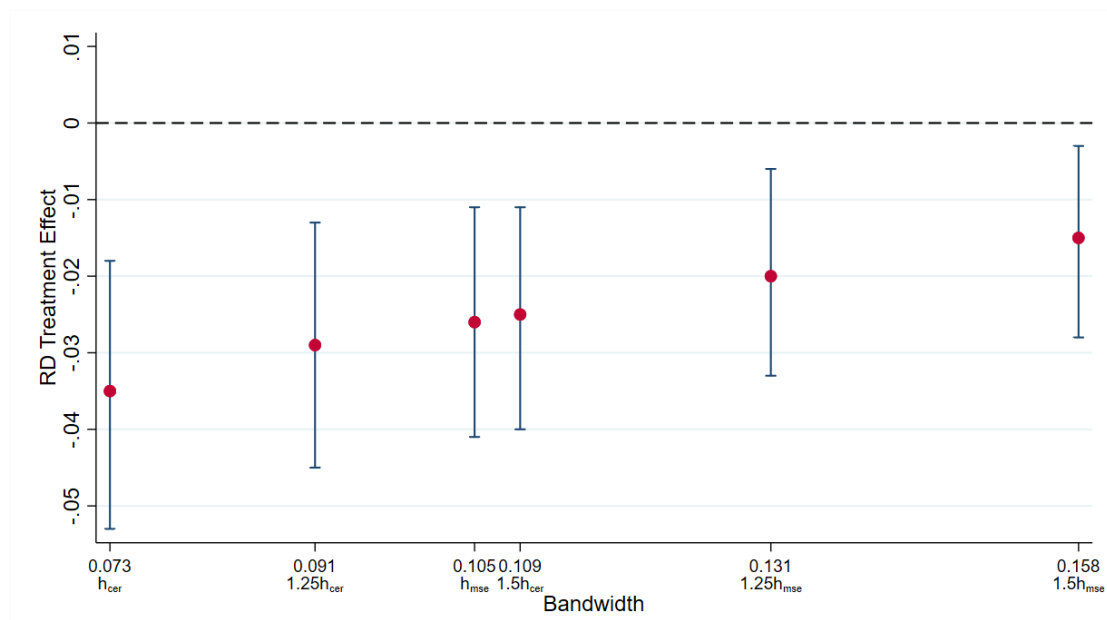
Notes: This figure shows the mean of average annualised real GDP growth rate over a four-year governor term for states from 1963 to 2019 by bins of MV_{st} . States with $MV_{st} < 0$ experienced a Republican governor while states with $MV_{st} > 0$ experienced a Democratic one. The demeaned average real GDP growth is the residual from the regression of average real GDP growth on state fixed effects.

Figure 4.5 BANDWIDTH SENSITIVITY TEST — REAL GDP PER CAPITA GROWTH



Notes: This figure shows coefficients on Democratic governor dummy estimating equation (1) within different bandwidth of Democratic marginal vote share (MV_{st}). The outcome variable is real GDP per capita growth.

Figure 4.6 BANDWIDTH SENSITIVITY TEST — REAL GDP GROWTH



Notes: This figure shows coefficients on Democratic governor dummy estimating equation (1) within different bandwidth of Democratic marginal vote share (MV_{st}). The outcome variable is real GDP growth.

4.4.2 Robustness

Bandwidth Sensitivity: Choosing the bandwidth is one of the most consequential decisions in RD analysis, because the bandwidth may affect the results and conclusions (Cattaneo et al. 2019). Therefore, I also check the sensitivity of my results to the bandwidth choice of Democratic marginal vote share (i.e., MV_{st}). Following Cattaneo et al. (2019), I choose six bandwidth choices close to the MSE-optimal and CER-optimal choices:¹⁸ (i) the MSE-optimal choice h_{mse} , (ii) $1.25 \cdot h_{mse}$, (iii) $1.50 \cdot h_{mse}$, (iv) the CER-optimal choice h_{cer} , (v) $1.25 \cdot h_{cer}$, (vi) $1.50 \cdot h_{cer}$. Figure 4.5 shows the local polynomial RD point estimators for average real GDP per capita growth and robust 95% confidence intervals for each bandwidth. The results based on different bandwidth choices are broadly consistent with the baseline empirical findings obtained with the MSE-optimal choice, i.e., the estimates are all significantly negative. The magnitudes range from -0.029 (at h_{cer}) to -0.012 (at $1.5 \cdot h_{mse}$). Given the mean (0.0138) and standard deviation (0.0218) of the close election sample, -0.012 is still quite economically significant as it is 87 percent of the mean and 55 percent of the standard deviation.¹⁹ The confidence interval decreases as the bandwidth and the number of observations increase. Figure 4.6 shows similar results of the bandwidth sensitivity test for the outcome variable average real GDP growth.

Other Robustness Checks: Table 4.3 reports tests to ensure the estimates are robust to alternative specifications and are not driven by spurious relationships for real GDP per capita growth. Column 1 reproduces the baseline estimate of the causal effect of governors' political party affiliation, as determined by close gubernatorial elections, on real GDP per capita growth. Column 2 uses a linear, rather than quadratic, polynomial in the running variable. The point estimate from the linear specification is about half as large as the baseline estimate. Figure 4.3 and Figure A4.1 in the Appendix show that a quadratic (order 2) polynomial fits the data better than a linear (order 1) polynomial for points with $MV_{st} > 0$, as it captures an increasing trend near the cutoff and a decreasing trend farther from the cutoff. Nevertheless, the linear estimate remains significantly negative and is consistent with the baseline result. Additionally, the estimate -0.0097 is also economically significant, as it is roughly 70 percent of the mean of, and 44 percent of the standard deviation of the average real GDP per capita growth rate in the sample of close elections.²⁰

¹⁸This is one common coverage error rate-optimal (CER-optimal) bandwidth selector for the RD treatment effect estimator.

¹⁹Considering the full sample, -0.012 is 93 percent of the mean (-0.0129) and 55 percent of the standard deviation (0.0218).

²⁰This is also roughly 75 percent of the mean of, and 44 percent of the standard deviation of the average real GDP per capita growth rate in the full sample.

Table 4.3: ROBUSTNESS FOR AVERAGE REAL GDP PC GROWTH

VARIABLES	(1) Baseline	(2) Order 1	(3) Uniform	(4) Epanechnikov	(5) Lagged DV	(6) Excluding Recession
RD.Estimate	-0.0204*** (0.0063)	-0.0097** (0.0042)	-0.0193*** (0.0073)	-0.0188*** (0.0064)	-0.0221*** (0.0062)	-0.0198*** (0.0062)
Observations	286	286	259	283	277	283
Bandwidth	0.1102	0.1085	0.0966	0.1073	0.1163	0.1278
Polynomial Order	2	1	2	2	2	2
Kernel	Triangular	Triangular	Uniform	Epanechnikov	Triangular	Triangular

Notes: This table shows results of robustness checks for outcome variable real GDP per capita growth. Column 1 is the baseline result. Column 2 uses order 1 polynomial. Column 3 and 4 use a uniform and an Epanechnikov kernel, respectively. Column 5 includes lagged dependent variable as an additional control. Column 6 excludes recession periods.

Table 4.4: ROBUSTNESS FOR AVERAGE REAL GDP GROWTH

VARIABLES	(1) Baseline	(2) Order 1	(3) Uniform	(4) Epanechnikov	(5) Lagged DV	(6) Excluding Recession
RD.Estimate	-0.0264*** -0.0076	-0.0121** -0.0049	-0.0222*** -0.0081	-0.0250*** -0.0076	-0.0284*** -0.0066	-0.0313*** -0.0079
Observations	277	285	255	268	264	246
Bandwidth	0.105	0.1076	0.0954	0.1013	0.1072	0.1057
Polynomial Order	2	1	2	2	2	2
Kernel	Triangular	Triangular	Uniform	Epanechnikov	Triangular	Triangular

Notes: This table shows results of robustness checks for outcome variable real GDP growth. Column 1 is the baseline result. Column 2 uses order 1 polynomial. Column 3 and 4 use a uniform and an Epanechnikov kernel, respectively. Column 5 includes lagged dependent variable as an additional control. Column 6 excludes recession periods.

Instead of a triangular kernel, columns 3 and 4 use a uniform and an Epanechnikov kernel (in which an observation's weight decrease parabolically with distance to the cutoff), respectively. The model in column 5 includes lagged dependent variable as a control. Columns 6 excludes recession periods.²¹ In each case the estimate is essentially unchanged from the baseline. Table 4.4 reports the similar results of robustness checks for real GDP growth.

So far, I have provided evidence that Republican governors elected in close elections cause higher state economic growth. However, it is important to note that the estimated causal effect reflects not only the partisanship of the governor but also the specific context of a close election. As Marshall (2024) points out, politician characteristics – such as party affiliation – might themselves influence electoral outcomes in closely contested races. Thus, the regression discontinuity estimates should

²¹According to the definition of recession by NBER, I exclude years of 1990, 1991, 2001, 2007, 2008 and 2009 in my sample.

be interpreted as capturing the causal impact of electing a Republican rather than a Democrat governor in narrowly decided elections, which could potentially include candidate-level characteristics associated with achieving such a close victory.

4.5 Conclusion

Using a sharp regression discontinuity on close gubernatorial elections in the US, I identify the causal impact of political party affiliations of leaders elected in close elections on economic growth in a democratic society. I find that close Republican gubernatorial victories cause average annualized real GDP per capita growth to increase by 2.0 percentage points and average annualised real GDP growth to increase by 2.6 percentage points over the 4-year governor term. Both are economically and statistically significant effects. My results suggest that leaders, specifically political party affiliations of leaders, matter for economic growth in democratic regimes, and that election outcomes could have causal impacts on economic outcomes.

Although this paper improves the understanding of economic growth and leaders in democratic regimes, more work is needed in this area to understand the full extent of the role of political party affiliations of leaders. Subsequent work could examine the channel through which a leader's party affiliation affects economic growth. The state partisan pattern documented in this work is different from the national partisan pattern documented by Blinder and Watson (2016) and Pástor and Veronesi (2020). How to reconcile the difference is outside the scope of this paper and represents a promising avenue for further research.

Additionally, the findings in this paper pertain only to short-run growth effects observed over the gubernatorial term; they do not imply that elected partisan leadership necessarily impacts long-term economic growth, nor do they account for potential trade-offs or the longer-term sustainability of policies implemented by partisan governors. Future research can investigate the long-term impacts and possible unintended consequences of party-affiliated policies that may emerge beyond the initial years following an election.

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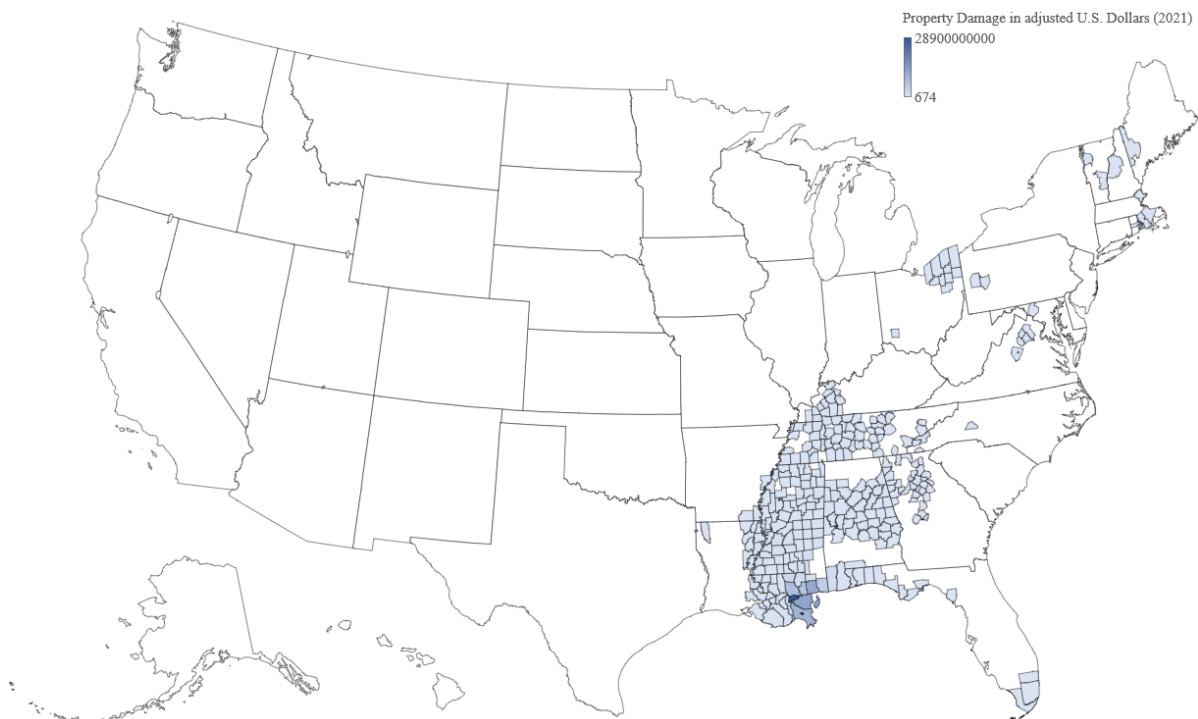
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Appendices

Appendix for Chapter 2

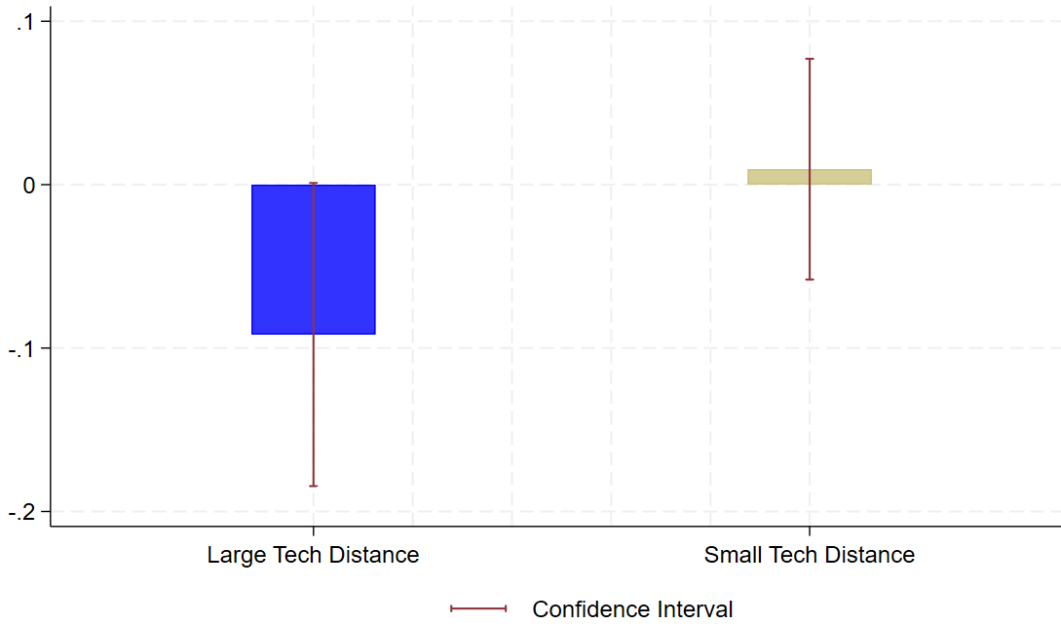
Appendix Figures for Chapter 2

Figure A2.1 GEOGRAPHICAL DISTRIBUTION OF PROPERTY DAMAGE BY KATRINA



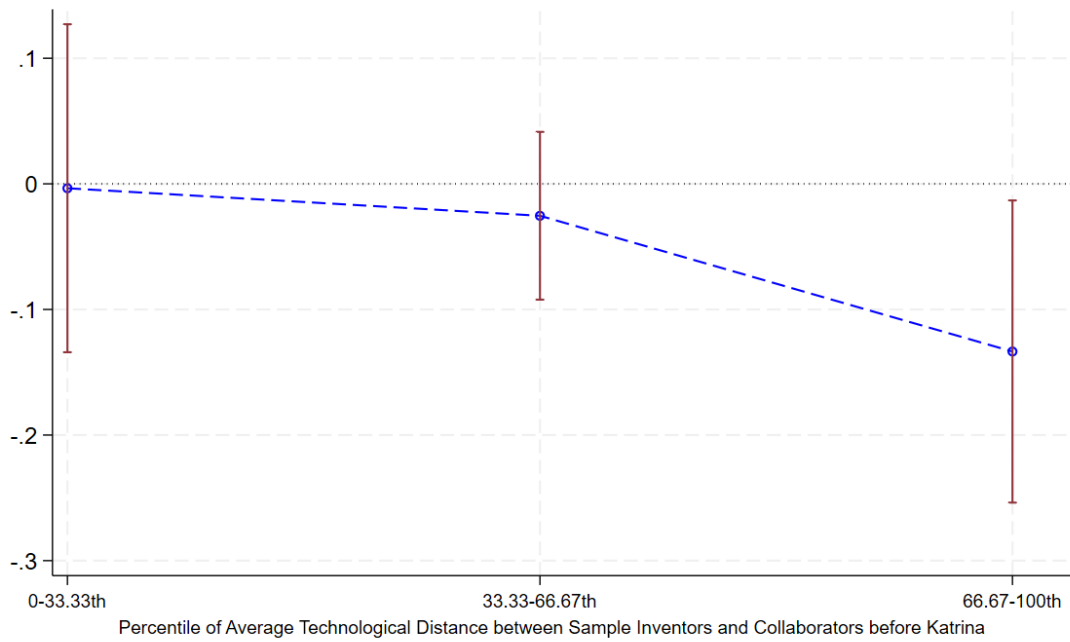
Notes: This figure illustrates the geographical distribution of property damage caused by Hurricane Katrina at the county level, with damage amounts adjusted to 2021 dollars. The shading intensifies from light to dark blue to indicate increasing levels of damage, with the darkest blue representing the most severe impact. New Orleans, Louisiana, is marked as the epicenter of the damage. The data is sourced from the Spatial Hazard Events and Losses Database for the United States (SHELDUS), with further details provided in Section 2.3.1.

Figure A2.2 DIFFERENTIAL EFFECTS ON PATENTS OF SMALL VS. LARGE TECHNOLOGICAL DISTANCE FOR ALL PATENTS



Notes: This table the estimated coefficients β_1 from specification (2.1) for two dependent variables: the number of patents with small technological distance and the number of patents with large technological distance. Both aggregate over three years: between period t and $t + 2$. The median value of average technological distance for each patent before Katrina is 0. Patents with an average technological distance above 0 are categorized as having a large technological distance, whereas those with a distance of 0 are considered to have a small technological distance. Section 2.5.2 provides a detailed explanation of the methodology used to measure technological distance between two inventors. The analysis is performed using the Poisson estimation method. Standard errors are clustered at the state and year levels. For more details on the specification, refer to Section 2.3.3.

Figure A2.3 HETEROGENEITY BY TECHNOLOGICAL DISTANCE WITH COLLABORATORS BEFORE KATRINA FOR ALL PATENTS



Notes: This figure plots the estimated coefficients β_m from specification (2.5) for all patents. The estimation is performed using the Poisson method. The tertiles are computed based on the average technological distance between sample inventors and their collaborators before Katrina. Section 2.5.2 provides a detailed explanation of the methodology used to measure technological distance between two inventors. Standard errors are clustered at the state and year levels. For more details on the specification, refer to Section 2.5.5.

Appendix Table for Chapter 2

Table A2.1: CPC Y02 GREEN TECHNOLOGY SUBCLASSES AND DESCRIPTIONS

Code	Description
Y02A 10/00	At coastal zones; at river basins
Y02A 20/00	Water conservation; Efficient water supply; Efficient water use
Y02A 30/00	Adapting or protecting infrastructure or their operation
Y02A 40/00	Adaptation technologies in agriculture, forestry, livestock or agroalimentary production
Y02A 50/00	In human health protection, e.g. against extreme weather
Y02A 90/00	Technologies having an indirect contribution to adaptation to climate change
Y02B 10/00	Integration of renewable energy sources in buildings
Y02B 20/00	Energy efficient lighting technologies, e.g. halogen lamps or gas discharge lamps
Y02B 30/00	Energy efficient heating, ventilation or air conditioning [HVAC]
Y02B 40/00	Improving efficiency of home appliances (induction cooking, efficient refrigerators, etc.)
Y02B 50/00	Energy efficient technologies in elevators, escalators and moving walkways
Y02B 70/00	Efficient end-user side electric power management and consumption
Y02B 80/00	Architectural/constructional elements improving thermal performance of buildings
Y02B 90/00	Enabling technologies or technologies with a potential or indirect contribution to GHG emissions mitigation
Y02C 20/00	Capture or disposal of greenhouse gases
Y02C 20/10	Of nitrous oxide (N ₂ O)
Y02C 20/20	Of methane
Y02C 20/30	Of PFC, HFC, or SF ₆
Y02C 20/40	Of CO ₂
Y02D 10/00	Energy efficient computing, e.g. low power processors, power/thermal management
Y02D 30/00	Reducing energy consumption in communication networks
Y02E 10/00	Energy generation through renewable energy sources
Y02E 30/00	Energy generation of nuclear origin
Y02E 20/00	Combustion technologies with mitigation potential
Y02E 40/00	Technologies for efficient electrical power generation, transmission or distribution
Y02E 50/00	Production of fuel of non-fossil origin
Y02E 60/00	Enabling technologies with potential/indirect GHG mitigation
Y02E 70/00	Other energy conversion/management systems reducing GHG emissions
Y02P 10/00	Metal processing
Y02P 20/00	Chemical industry
Y02P 30/00	Oil refining/petrochemical industry
Y02P 40/00	Processing of minerals
Y02P 60/00	Agriculture, livestock, agroalimentary industries
Y02P 70/00	Final industrial/consumer products process
Y02P 80/00	Sector-wide applications
Y02P 90/00	Enabling technologies with potential to mitigate GHG emissions
Y02T 10/00	Road transport of goods or passengers
Y02T 30/00	Railways, energy recovery, reducing air resistance
Y02T 50/00	Aeronautics or air transport
Y02T 70/00	Maritime/waterways transport
Y02T 90/00	Enabling technologies or potential for GHG mitigation in transport
Y02W 10/00	Technologies for wastewater treatment
Y02W 30/00	Technologies for solid waste management
Y02W 90/00	Enabling technologies for GHG mitigation in waste management

Table A2.2: HETEROGENEOUS EFFECTS OF KATRINA ON ALL PATENTS

	(1) Patents of large tech distance	(2) Patents of small tech distance	(3) Heterogeneity by tech distance with collaborators before Katrina
Treatment $\times$ Post	-0.0917* (0.0473)	0.00947 (0.0345)	
First tertile $\times$ Treatment $\times$ Post			-0.00350 (0.0666)
Second tertile $\times$ Treatment $\times$ Post			-0.0254 (0.0341)
Third tertile $\times$ Treatment $\times$ Post			-0.134** (0.0614)
Observations	102,621	102,621	102,621
Year fixed effects	Yes	Yes	Yes
State fixed effects	Yes	Yes	Yes
Inventor fixed effects	Yes	Yes	Yes
Tenure fixed effects	Yes	Yes	Yes

Notes: This table reports the heterogeneous effects of Katrina on all patents. All dependent variables aggregate over three years: between period t and $t + 2$. Column 1-2 reports the point estimates for the coefficient β_1 from specification (2.1) for two dependent variables: the number of patents with small technological distance and the number of patents with large technological distance. The median value of average technological distance for each patent before Katrina is 0. Patents with an average technological distance above 0 are categorized as having a large technological distance, whereas those with a distance of 0 are considered to have a small technological distance. Column 3 reports estimated coefficients β_m from specification (2.5). The tertiles are computed based on the average technological distance between sample inventors and their collaborators before Katrina. Section 2.5.2 provides a detailed explanation of the methodology used to measure technological distance between two inventors. The estimation is performed using the Poisson method. The model includes fixed effects for tenure, inventor, year, and state. The variable *tenure* is defined as the number of years since the inventor's first successful patent application. Standard errors clustered at the state and year levels are reported in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Appendix for Chapter 3

Variable Definitions

Firm Characteristics

Investment Rate: capital expenditures divided by beginning-of-period book value of total assets.

Tobin's Q: book value of total assets minus the book value of equity plus the market value of equity scaled by the book value of total assets.

Cash Flow Rate: operating income before depreciation scaled by beginning-of-period book value of total assets.

Sales Growth: the year-on-year growth in quarterly sales.

Political Data

Unified Federal Government: a dummy variable takes a value of one when the presidency and the congress are all controlled by the same party.

Unified State Government: a dummy variable takes a value of one when the governor and the state legislature are all controlled by the same party.

Democratic President: a dummy variable takes a value of one when the president is democratic.

Democratic Governor: a dummy variable takes a value of one when the governor is democratic.

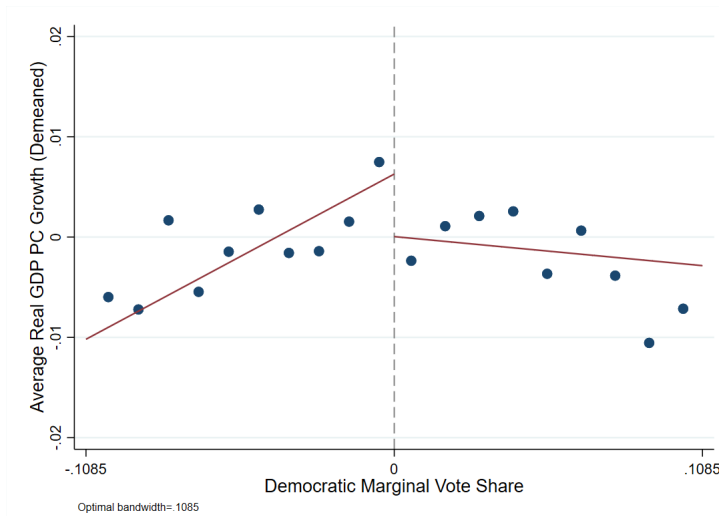
Presidential Election: a dummy variable takes a value of one when the presidential election occurs in the year of t .

Gubernatorial Election: a dummy variable takes a value of one when the gubernatorial election occurs in the year of t .

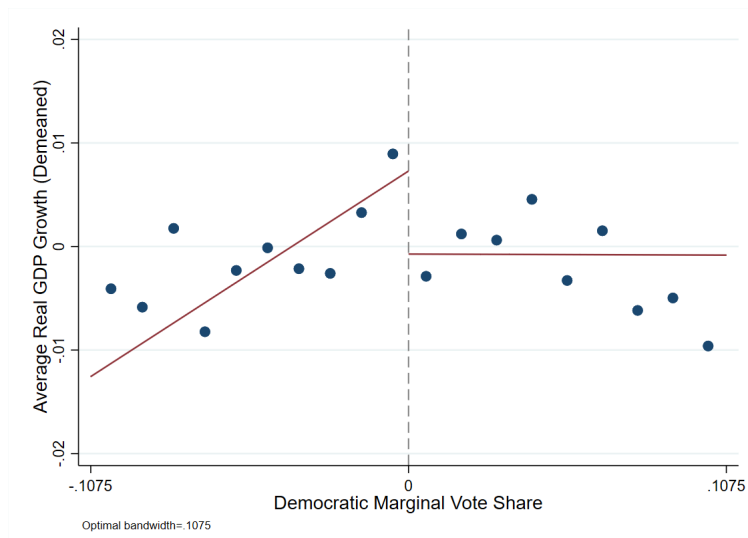
Appendix for Chapter 4

Appendix Figures for Chapter 4

Figure A4.1 AVERAGE ANNUALISED REAL GDP PER CAPITA GROWTH



Notes: This figure shows the mean of average annualised real GDP per capita growth rate over a four-year governor term for states from 1963 to 2019 by bins of MV_{st} . States with $MV_{st} < 0$ experienced a Republican governor while states with $MV_{st} > 0$ experienced a Democratic one. The demeaned average real GDP per capita growth is the residual from the regression of average real GDP per capita growth on state fixed effects.

Figure A4.2 AVERAGE ANNUALISED REAL GDP GROWTH

Notes: This figure shows the mean of average annualised real GDP growth rate over a four-year governor term for states from 1963 to 2019 by bins of MV_{st} . States with $MV_{st} < 0$ experienced a Republican governor while states with $MV_{st} > 0$ experienced a Democratic one. The demeaned average real GDP growth is the residual from the regression of average real GDP growth on state fixed effects.