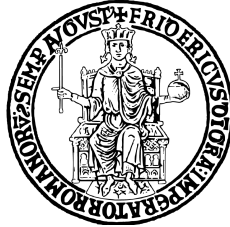


UNIVERSITÀ DEGLI STUDI DI NAPOLI “FEDERICO II”



DOCTORAL THESIS

**Incentives and the Real Effects of Policy and
Exogenous Shocks in Corporate Finance**

Candidate

Kristjan MARKOVIC

Supervisor

Prof. Tommaso OLIVIERO

PhD Director

Prof. Marco PAGNOZZI

*A thesis submitted in fulfillment of the requirements
for the degree of Doctor of Philosophy in Economics and Finance - XXXVII Cycle
at the*

Naples School of Economics
Department of Economics and Statistics

Acknowledgements

The academic journey leading to this thesis has been long and transformative, spanning three continents and four countries, and I am deeply grateful to the many people who have supported and guided me along the way.

First and foremost, I would like to thank my supervisor, Prof. Tommaso Oliviero, for his guidance, encouragement, and unwavering belief in this project. His intellectual rigor, high standards, and constructive feedback profoundly shaped this thesis and helped me grow as a researcher. I could not have asked for a better mentor. I am also grateful to Prof. Marco Pagano for his insightful comments, generous advice, constant support and his belief in my academic potential from the moment I applied to the doctoral program until the completion of this work. My sincere thanks go as well to Prof. Hans Degryse at KU Leuven, for welcoming me during my visiting period and continuing to support my research beyond it. Their expertise and perspective enriched this thesis in countless ways.

This research benefited greatly from discussions with colleagues and friends at the University of Naples Federico II, who created a stimulating and supportive academic environment. I am also thankful to the administrative staff for their professionalism and assistance throughout this process. I gratefully acknowledge the financial support provided by University of Naples Federico II, without which this research would not have been possible.

Beyond the doctoral program, I would like to express my gratitude to Prof. Akira Hibiki whose belief in my abilities and opportunity to study economics laid the foundation without which this thesis would not have been possible. I am also grateful to Prof. Jiro Akita for his dedication, guidance and extracurricular support offered during the master's studies, as well as to Tohoku University and the Ministry of Education, Culture, Sports, Science and Technology of Japan for their financial support, which enabled me to pursue my master's degree and later enter the doctoral program at the University of Naples Federico II.

My academic path would not have begun without Prof. Brian Cudmore, whose introduction to the academic research sparked my initial curiosity, an interest that, years later, culminated in this thesis. I am also indebted to Prof. Keiron Hylton and Prof. Alex Vamosi for their unwavering

support and example of professionalism from our first meeting. Finally, I offer my sincerest and the most heartfelt gratitude to Prof. Michael Slotkin for his continued academic, professional and personal mentorship. His motivation, belief and guidance have shaped my journey and opened doors I never thought possible.

Chi ha un buon maestro, ha un grande tesoro

Beyond academia, I owe immense gratitude to my closest friends, who provided encouragement, distraction, humor, and perspective at exactly the right moments. To the one who served as my constant anchor and my favorite distraction, thank you for reminding me that there is more to life than academic work.

Most importantly, I want to thank my family. To my parents, Marijana Markovc and Primož Markovc, thank you for your unconditional love, patience, and sacrifices that allowed me to pursue my goals from the very beginning to the very end. You believed in me even when I doubted myself. Your belief in me has been my constant source of strength and the reason I could turn what once felt like distant dreams into reality.

Ljubezen staršev je veter v naših jadrih

Abstract

This thesis examines how policy interventions in taxation, banking, and capital markets influence firm behavior, financial stability, and access to funding. Across three empirical chapters, it demonstrates that the effectiveness of corporate finance and macroprudential policies critically depends on their design, heterogeneity in responses, and interaction with market incentives. The first chapter evaluates gross receipts tax regime intended to reduce compliance costs for micro-firms. Average treatment effects appear small, but strong heterogeneity emerges, very small firms benefit through lower administrative burdens and higher growth, while firms near the eligibility limit experience reduced performance and long-run expansion. This reveals a central policy trade-off that may unintentionally discourage firms from growing. The second chapter investigates how public emergencies affect banking stability using the onset of COVID-19 in Hubei Province as a natural experiment. The results show that emergencies increase bank risk, primarily through heightened passive risk-taking driven by operational disruptions. However, banks with more advanced digital transformation are significantly more resilient, maintaining credit operations and mitigating risk escalation. Active, strategic risk-taking remains unaffected. These findings highlight digital infrastructure as a macroprudential instrument. Regulators should therefore promote digital transformation as a core pillar of banking resilience and emergency preparedness. The third chapter analyzes the European Central Bank's Corporate Sector Purchase Program and its impact on bond underwriting costs. Spillover effects increase costs for ineligible firms, indicating that targeted interventions can reshape market pricing more broadly. While this demonstrates the effectiveness of unconventional monetary policy in supporting corporate financing conditions, it also underscores the need to monitor distributional consequences and long-term dependencies on policy support. Together, the chapters reveal that policy tools at different levels of the financial system can effectively promote participation, stability, and financing. Yet their success hinges on accounting for heterogeneous incentives and avoiding rigid designs that impose unintended constraints. The results suggest flexible, well-coordinated policies that align firm-level incentives with macroprudential objectives, enabling growth and a more resilient financial system.

Table of Contents

Acknowledgements	iii
Abstract	v
Table of Contents	vii
List of Figures	xi
List of Tables	xiii
1 Introduction	1
1.1 Thesis Motivation	3
1.2 Unifying Theme	8
1.3 Core Concepts	10
1.3.1 Policy design	11
1.3.2 Incentives and Heterogeneity	12
1.3.3 Passive and Active Risk	13
1.3.4 Digital Transformation	15
1.3.5 Spillovers and Capital Markets	16
1.3.6 Fiscal Policy	17
1.3.7 Unconventional Monetary Policy	18
1.4 Institutional Settings and Roles	19
1.5 Theoretical and Empirical Gaps in the Literature	24
1.5.1 Corporate Finance and Taxation Policies	25
1.5.2 Banking Stability and Macroprudential Policies	27
1.5.3 Capital Market Policies and Monetary Interventions	28
1.6 Research Questions	30
1.7 Methodological Overview	33
1.7.1 Difference-in-Differences and Two-Way Fixed Effects	35
1.7.2 Fuzzy Regression Discontinuity Design	38
1.7.3 Propensity Score Matching	40
1.7.4 Inverse Mills Ratio	41
1.8 Thesis Contributions	42
1.8.1 Academic Contributions	43

1.8.2	Policy Contributions	45
1.8.3	Methodological Contributions	46
1.9	Clarification of Scope and Boundaries	47
1.10	Thesis Structure	51
2	The Impact of Gross Receipt Taxation on Micro-Sized Firms	55
2.1	Introduction	56
2.2	Background and Motivation	60
2.2.1	Gross Receipt Taxation Legislation	61
2.2.2	GRT Tradeoffs	62
2.3	Theoretical Setting	63
2.4	Data	66
2.4.1	Identification Strategy	67
2.4.2	Log Transformation	70
2.5	Empirical Analysis	70
2.5.1	Determinants of Program Participation	70
2.5.2	Fuzzy Regression Discontinuity Design	73
2.5.2.1	Doughnut Fuzzy Regression Discontinuity Design	75
2.5.3	Two-Way Fixed Effects	77
2.5.3.1	Inverse Mills Ratio	81
2.5.3.2	Propensity Score Matching	86
2.5.3.3	Binned TWFE Estimation	89
2.5.4	Placebo Testing	91
2.6	Discussion	95
2.7	Conclusion	97
2.8	Appendix	98
2.8.1	Tables	98
2.8.2	Graphs	107
3	Digital Transformation and Bank Risk in the Public Emergency	111
3.1	Introduction	112
3.2	Hypothesis Development	113
3.2.1	The impact of public emergencies on bank risk	113
3.2.2	The role of digital transformation in public emergencies	115

3.3	Methodology	116
3.3.1	Data and Sample	116
3.3.2	Econometric specification	116
3.4	Empirical Analysis	120
3.4.1	Baseline results	120
3.4.2	Robustness Checks	121
3.5	Further Analysis	125
3.5.1	Active and Passive Risks	126
3.5.2	Mechanism Analysis	127
3.5.3	Heterogeneity Test	128
3.6	Conclusion	130
3.7	Appendix	132
3.7.1	Tables	132
3.7.2	Graphs	134
4	Unconventional Monetary Policy and Corporate Bond Underwriting	135
4.1	Introduction	136
4.2	Background and Institutional Setting	140
4.2.1	Asset Purchase Programs	141
4.2.2	Corporate Sector Purchase Program	143
4.2.3	Theoretical Background	144
4.3	Data	147
4.4	Methodology	152
4.5	Empirical Analysis	153
4.5.1	Main Analysis	154
4.5.2	Robustness	158
4.5.3	Role of Credit Rating	163
4.5.4	Pre-Post Analysis	170
4.5.5	Role of Past Bond Issuance	174
4.6	Discussion	178
4.7	Conclusion	180
4.8	Appendix	182
4.8.1	Additional Tables	182

4.8.2	Additional Graphs	184
5	Conclusions	187
5.1	Main Findings	187
5.2	Thesis Implications	189
5.3	Thesis Limitations	191
5.4	Future Research	192
5.5	Closing Statement	193

List of Figures

2.1	GRT Entrance by Industry	67
2.2	GRT Entrance Heat Maps	73
2.3	FRD Visualization of Results	75
2.4	Doughnut FRD Visualization of Results	77
2.5	Bin Specific Impacts on Financial Performance - Late Adopters	90
2.6	Bin Specific Impacts on Financial Performance - Eligible	91
2.7	Distribution of Financial Margins	107
2.8	Histogram of Entrance Probability	108
2.9	Distribution of Propensity Scores Before and After PSM	109
3.1	Digitalization trends	118
3.2	Impact of Digital Transformation on Bank Risk	122
3.3	Placebo Test Results	124
3.4	Parallel Trends Test	124
3.5	Propensity Score Density	134
4.1	Key Interest Rates	141
4.2	Net ECB Asset Holdings	142
4.3	Historical ECB Holdings	144
4.4	Differences in ECB Holdings and Available Pool	145
4.5	Euro and Non-Euro Denominated Bond Issuance	148
4.6	Visualization of Parallel Trends Coefficients	155
4.7	Debt Security Holdings Held by the ECB	184
4.8	Differences in ECB Holdings and Available Pool by Industry	185
4.9	Number of Bond-Issuing Firms	186

List of Tables

2.1	GRT Timeline	62
2.2	Descriptive Statistics by GRT Entrance	69
2.3	GRT Entrance Determinants	72
2.4	FRD Results	74
2.5	Doughnut FRD Results	76
2.6	Core TWFE Results for Financial Performance	78
2.7	Core TWFE Results for Financial Growth	79
2.8	Core Results for Bankruptcy Probability	80
2.9	IMR Corrected TWFE Results for Financial Performance	82
2.10	IMR Corrected TWFE Results for Financial Growth	84
2.11	IMR Corrected Results for Bankruptcy Probability	85
2.12	PSM Corrected TWFE Results for Financial Performance	87
2.13	PSM Corrected Results for Bankruptcy Probability	88
2.14	PSM Corrected TWFE Results for Placebo Testing	94
2.15	Firm Classification	100
2.16	NACE Rev.2 Classification	101
2.17	List of Variables	102
2.18	Descriptive Statistics Before and After PSM	103
2.19	Bin Specific Impacts on Financial Performance Metrics	106
3.1	Descriptive Statistics of Banks	120
3.2	Baseline Results	121
3.3	Propensity Score Matching Balance	123
3.4	PSM Corrected Results	123
3.5	Sample Selection Results	125
3.6	Active and Passive Risk Analysis	127
3.7	Mechanism Analysis Results	128
3.8	Heterogeneity Test Results	129
3.9	List of Variable Definitions	132
3.10	Correlation Statistics	133

4.1	Descriptive Statistics for Underwritten Bonds	150
4.2	Descriptive Statistics of Bond-Issuing Firms	152
4.3	Parallel Trends Coefficients	155
4.4	Impact of CSPP on Nominal Bond Issuance	156
4.5	Impact of CSPP on Nominal Gross Underwriting Spread	157
4.6	Impact of CSPP on Relative Gross Underwriting Spread	158
4.7	Robust Impact of CSPP on Nominal Bond Issuance	159
4.8	Robust Impact of CSPP on Nominal Gross Underwriting Spread	160
4.9	Robust Impact of CSPP on Relative Gross Underwriting Spread	161
4.10	Robust Placebo Impacts of CSPP	162
4.11	Impact of CSPP on Issuance by Credit Rating without Controls	164
4.12	Impact of CSPP on Issuance by Credit Rating with Controls	166
4.13	Impact of CSPP on Gross Spread by Credit Rating without Controls	168
4.14	Impact of CSPP on Gross Spread by Credit Rating with Controls	170
4.15	Pre-Post Impact of CSPP on Bond Metrics	172
4.16	Pre-Post Impact of CSPP on Bond Metrics by Credit Rating	174
4.17	Prior Issuance Impact of CSPP on Bond Issuance	176
4.18	Prior Euro-Bond Issuance Impact of CSPP on Bond Metrics	177
4.19	Prior Bond Issuance Impact of CSPP on Bond Metrics	178
4.20	Descriptive Statistics of Bond Issuance by Year	183

Chapter 1

Introduction

Policy interventions are fundamental tools through which governments and regulatory institutions shape economic and financial outcomes. By altering incentives, redistributing resources, and managing risks, policies influence how firms operate, how financial institutions allocate credit, and how markets price capital. Taxation regimes affect the cost of doing business and the incentives to formalize, invest, and grow. Banking regulation and macroprudential frameworks determine how risks are monitored, absorbed, or amplified within the financial system. Capital market policies and monetary interventions influence liquidity, borrowing costs, and access to long-term financing. Together, these domains form the core infrastructure through which modern economies function.

Taxation, banking, and capital markets are therefore not isolated components, but interconnected pillars of the financial system. Firms depend on tax policies that shape their profitability and compliance burdens, banks serve as intermediaries that transform savings into credit and maintain financial stability and capital markets provide alternative channels of funding and price signals that influence investment decisions. The design and interaction of policies within these pillars directly affect the behavior of economic agents, the resilience of financial institutions, and the efficiency of capital allocation.

These policies influence firm behavior by altering marginal incentives to invest, hire, or expand. They affect financial stability by shaping the risk-taking behavior and operational capacity of banks, especially during shocks. They shape access to funding by affecting underwriting costs, liquidity conditions, and the distribution of capital across firms. Because firms, banks, and markets do not operate in isolation, policies targeting one level of the financial system often spill over into others, with consequences that may be unintended or underestimated. For all these reasons, economists, policymakers, and society at large must understand

how these policy tools operate in practice. Policy interventions are powerful, but their effects are complex. They have the potential to promote growth and stability, but they can also distort incentives, entrench inequalities, or create long-term dependencies. Understanding when and how policies achieve their objectives, and why they sometimes fail, is central to designing a more resilient and inclusive financial system.

In recent decades, the role of policy intervention in financial systems has expanded dramatically. The Global Financial Crisis of 2008 triggered a fundamental rethinking of regulatory frameworks, leading to the rise of macroprudential policies aimed at safeguarding systemic stability and popularized unconventional monetary policy. The COVID-19 pandemic prompted unprecedented emergency responses by governments, central banks, and supervisory authorities. At the same time, rapid digitalization has transformed the way firms operate, banks manage risks, and markets process information, changing the effectiveness and transmission of traditional policy tools.

Financial systems have also become more complex and interconnected. Shocks transmit across borders and sectors faster than ever. Policies aimed at one segment of the system often produce ripple effects elsewhere. This interconnectedness makes it increasingly difficult to evaluate interventions in isolation and raises the stakes of policy design. Errors or rigidities in one domain can propagate throughout the system. In response, policymakers have shifted from passive or broad-based interventions to targeted and active policy tools. Simplified tax regimes aim to stimulate specific segments of the economy. Macroprudential regulations target particular risk channels or balance sheet exposures. Central banks implement unconventional monetary policies such as corporate bond purchase programs that directly influence market pricing. These targeted tools may be more precise, but they also increase the importance of design, implementation, and interaction with market incentives.

As a result, the success of modern policy interventions increasingly depends on whether they are effective, flexible, and incentive-compatible. Policies must consider the heterogeneity of agents, the operational capacity of institutions, and the dynamic consequences of intervention. They must balance short-term support with long-term efficiency, and stability with innovation and growth. Understanding these trade-offs is not only a theoretical concern but a pressing practical challenge in today's policy environment.

Despite their importance, policy interventions often produce outcomes that diverge from their intentions. Policies are designed to promote growth, stability, or access to finance, yet

they can inadvertently discourage firm expansion, amplify bank risk, or distort market pricing. A recurring challenge is that the intentions of policymakers do not always align with the incentives faced by economic agents. One fundamental reason is that one-size-fits-all policy designs overlook heterogeneity in firm size, business models, technological capacity, or market access. When policies treat all agents as identical, they fail to account for differences in how firms, banks, or investors can respond. As a result, policies may benefit some actors while harming others, creating unequal or even perverse outcomes.

Rigid or overly simplified designs can distort incentives. For example, tax thresholds may encourage firms to remain small rather than grow, regulatory requirements may push banks toward riskier passive exposure when they lack operational capacity and targeted market interventions may shift pricing power toward certain firms or intermediaries. Over time, such distortions can limit long-term growth, reduce competition, or undermine financial stability. At the macro level, policies pursue aggregate stability or broad economic goals. However, the micro-level incentives they create for individual firms, banks, or intermediaries may conflict with these objectives. In other words, macro goals can be undermined by micro behavior, especially when policy design does not align incentives across levels of the financial system.

1.1 Thesis Motivation

With the policymakers increasingly relying on targeted interventions to shape economic and financial outcomes, it became essential to understand not only whether a policy works, but how and why it produces its effects. Traditional evaluations often focus on average treatment effects or headline outcomes, yet these aggregate measures can obscure the underlying mechanisms through which policy operates. Without understanding mechanisms, policymakers risk misinterpreting success or failure, replicating ineffective designs, or overlooking unintended consequences that only emerge in specific segments of the economy.

A major limitation in existing policy evaluations is the lack of attention to heterogeneity. Policies rarely affect all firms, banks, or market participants in the same way. Size, technological capacity, regulatory exposure, market power, or external constraints can lead to divergent responses, meaning that average effects may hide critical distributional consequences. Policies that appear neutral on aggregate may in fact benefit some actors while harming others, reinforcing inequalities or distorting competition.

Moreover, the academic literature often studies the financial system in silos. Corporate finance research focuses on firm behavior and capital structure. Banking research studies risk and regulation. Monetary economics examines asset purchases and liquidity. Each field has developed sophisticated tools and insights, yet they rarely communicate with each other. This fragmentation prevents a system-level understanding of how interventions in one part of the financial system affect others. For example, tax policy affects firm growth and thus credit demand, banking stability shapes the supply of capital to firms and capital market interventions influence both bank balance sheets and firm financing choices. Analyzing policies in isolation overlooks these interdependencies.

Given the increasing complexity and interconnectedness of modern financial systems, there is a growing need to understand policy interventions across the entire financial architecture, from firms to banks to markets. Only by integrating insights across levels can we identify the mechanisms that drive both success and failure, anticipate spillovers, and design policies that reinforce rather than undermine each other. A deeper, more holistic understanding of policy mechanisms is therefore not just an academic exercise, it is essential for effective and responsible policymaking.

A central premise of this thesis is that policy effectiveness depends critically on design. Policies are not neutral tools, but rather their impact is shaped by the specific rules, thresholds, eligibility criteria, and implementation mechanisms through which they operate. Design features determine who is included or excluded, when and how incentives change, and whether agents can adapt or game the system. Thresholds and eligibility rules, while often introduced to simplify administration or target specific groups, can create sharp distortions in behavior. Firms may limit their growth to remain eligible for preferential tax treatment. Banks without the operational capacity to comply with complex regulations may unintentionally increase their exposure to risk. Targeted monetary interventions may shift market power toward certain firms or intermediaries, altering pricing dynamics and competitive balance.

Poorly designed policies can therefore discourage firm expansion, increase systemic vulnerability, raise financing costs for certain firms, or create long-term dependencies on state support. In contrast, well-designed policies align incentives with desired outcomes, enhance operational resilience, and support efficient resource allocation. Design choices regarding flexibility, adaptiveness, and coordination across institutions can allow policies to respond to

heterogeneity in agents and changing economic conditions. The key insight is that policy effectiveness is not binary. Interventions cannot simply be labeled as successful or unsuccessful, their impact depends on the interaction between design features, institutional capacity, agent heterogeneity, and market incentives. Understanding these interactions is essential for crafting policies that deliver their intended goals without imposing unintended constraints. The stakes of policy design are substantial and span multiple levels of the financial system. At the firm level, policies influence growth, productivity, and formalization. Tax regimes that reduce compliance costs can encourage entrepreneurship, but poorly designed thresholds may trap firms at suboptimal scales. Access to credit and capital markets shapes investment, innovation, and long-term expansion.

At the banking level, regulatory frameworks determine financial stability, credit supply, and risk management practices. Well-designed macroprudential policies can mitigate procyclicality and prevent crises, while poorly aligned rules can trigger passive risk-taking or reduce banks' ability to lend in downturns. Digital transformation has emerged as a critical factor in operational resilience, yet many regulatory frameworks still assume traditional models of intermediation. At the capital market level, monetary policy interventions affect the cost of financing, the functioning of pricing mechanisms, and the fairness of market access. Targeted asset purchase programs can lower funding costs for eligible firms, but may raise costs for ineligible competitors, alter underwriting behavior, or concentrate market power in certain intermediaries. These distributional and structural effects are often underappreciated. At the macroeconomic level, the cumulative effects of firm behavior, bank stability, and market functioning shape economic growth, resilience to shocks, and systemic risk. Well-designed policies can support countercyclical stabilization, enhance productivity, and allocate resources efficiently. Poorly designed policies can lead to misallocation, financial instability, and long-term dependency on state support.

The consequences of policy design extend far beyond the immediate targets of intervention. They affect the health, efficiency, and equity of the entire financial system. Recognizing the economic stakes underscores the importance of studying how policies are crafted, how agents respond, and how outcomes propagate across institutional layers. It is precisely this system-wide perspective that motivates the research undertaken in this thesis. These abstract tensions are not merely theoretical as they are clearly visible in real policy experiences across the world. In taxation, numerous countries have implemented simplified or threshold-based

regimes to support micro-firms. While these policies often reduce compliance burdens for the smallest enterprises, empirical evidence shows that firms close to eligibility thresholds may deliberately restrict their revenues or employment to avoid losing preferential treatment. Instead of encouraging growth, poorly designed tax thresholds can create a “small-firm trap,” distorting entrepreneurial incentives and reducing long-term productivity.

The COVID-19 crisis revealed a different but equally important dimension of policy design. During the pandemic, banks faced severe operational disruptions. Many lacked the digital infrastructure to process loans, monitor credit risk, or maintain normal operations under lockdown conditions. As a result, some banks unintentionally increased passive risk exposure, not because of deliberate risk-taking, but due to an inability to adjust portfolios or screen borrowers effectively. In contrast, banks with advanced digitalization maintained credit supply, contained risk escalation, and contributed to financial stability. This divergence illustrates that regulatory frameworks ignoring operational capacity may fail in crisis conditions, even if capital requirements are formally met.

Similarly, unconventional monetary interventions such as the European Central Bank’s Corporate Sector Purchase Program have been effective in lowering funding costs and supporting liquidity for targeted firms. However, these benefits did not extend evenly across the market. Ineligible firms faced higher underwriting costs as financial intermediaries reallocated attention and pricing power toward eligible issuers. This indicates that targeted interventions can produce significant spillovers, altering market dynamics, competitive balance, and cost structures beyond their intended scope.

These real-world examples highlight a common lesson, policy design choices, thresholds, capacity requirements, targeting mechanisms, have powerful and sometimes unintended consequences. Understanding these mechanisms is essential for crafting policies that promote growth and stability without distorting incentives or creating new vulnerabilities. These observations naturally raise deeper intellectual questions. Why do some policy interventions achieve their goals while others fall short or even backfire? Is the key determinant the policy tool itself, or the way in which it is implemented and designed? Can we move beyond evaluating policies in terms of average effects and instead understand the mechanisms that drive success or failure?

A central curiosity that emerges is whether it is possible to design policies that promote

growth and financial stability simultaneously, without sacrificing incentives or creating unintended distortions. Achieving this balance requires recognizing that economic agents are heterogeneous. Small firms, medium-sized enterprises, and firms near policy thresholds do not respond identically. Digitally advanced banks behave differently from traditional banks during crises. Firms eligible for central bank interventions experience different pricing dynamics than ineligible firms. These differences are not noise, they are the mechanisms that determine policy outcomes. This raises a critical question, can micro-level heterogeneity and incentives explain macro-level policy failures? If so, evaluating policies solely by their average effects or aggregate outcomes may be misleading. Instead, understanding policies requires examining how design features interact with heterogeneous agent behavior across multiple layers of the financial system. The recurring insight across these examples is that the success or failure of policy interventions depends not only on the choice of tool, but on how that tool is designed and how diverse agents respond to it. This leads to the central motivating puzzle of the thesis:

How can policy interventions across different levels of the financial system be designed to achieve their objectives without introducing unintended constraints?

Or, more fundamentally:

How do policies interact with incentives and heterogeneity to produce real economic and financial outcomes?

The questions posed above point to a clear need in both academic research and policy practice. To understand and design effective financial policies, we must adopt a comprehensive, multi-layer, and mechanism-based perspective. Isolated evaluations of firms, banks, or markets are no longer sufficient in an interconnected financial system. We require an integrated analysis that spans the entire financial architecture and examines how interventions at one level affect outcomes at others. We also need credible empirical evidence based on causal identification, rather than descriptive correlations. Only robust empirical strategies can reveal how design features, incentives, and heterogeneity shape real-world outcomes. At the same time, we must pay attention to both benefits and unintended consequences, including distortions, spillovers, and long-term dependencies that may undermine policy objectives.

Finally, this calls for a shift in policy evaluation, from asking “Does this policy work?” to asking “Under what conditions, through which mechanisms, and for whom does this policy work or fail?” Such a shift requires moving beyond narrow, tool-specific analyses toward

holistic policy design principles that align micro-level incentives with macro-level goals and account for heterogeneous responses across the financial system.

1.2 Unifying Theme

At the heart of this thesis lies a central research problem of how do policy interventions across the financial system shape real economic and financial outcomes, and why do they sometimes succeed while at other times they produce unintended consequences? Policies in taxation, banking regulation, and capital market intervention are designed to promote growth, stability, and access to funding. However, their effectiveness depends not only on the choice of policy tool but on how the tool is designed, implemented, and interpreted by heterogeneous economic agents. Understanding policy outcomes therefore requires analyzing interventions through the lens of policy design, incentives, and heterogeneity in responses. The core puzzle connecting the thesis is thus, when and how do policy interventions across different levels of the financial system achieve their objectives, and how do their design features and heterogeneous micro-level responses shape their overall effectiveness?

The unifying theme of this thesis can be stated succinctly. It examines how the design of policy interventions interacts with the incentives and heterogeneous characteristics of firms, banks, and market participants to generate real economic and financial outcomes. Although each empirical chapter focuses on a different institutional context, they are bound together by a shared intellectual approach. All chapters investigate policy interventions, analyze how economic agents respond to these interventions and reveal unintended or distributional consequences that arise when policy design does not account for heterogeneity. These are not isolated case studies but complementary lenses on the same fundamental question, applied at different layers of the financial system. By moving from firms to banks to capital markets, the thesis provides a multi-dimensional view of how policy operates in practice.

The conceptual pillars that unify the thesis are explicit and recurring. First, policy design quality is central, thresholds in taxation, eligibility in asset purchases, and digital readiness in banking all shape how policies function. Second, heterogeneity in responses is a key driver of outcomes with small versus large firms, digitally advanced versus traditional banks, eligible versus ineligible firms. Third, the thesis emphasizes incentive alignment or misalignment,

showing how policies can encourage or discourage desirable behavior. Fourth, it analyzes behavioral mechanisms, such as the distinction between passive and active risk. Fifth, it uncovers unintended consequences and spillovers, where policies aimed at one group affect others. Sixth, it reveals trade-offs between rigidity and flexibility, showing that static policy design may fail in dynamic environments. Finally, the thesis demonstrates how micro-level decisions accumulate into macro-level outcomes, influencing financial stability and access to finance across the system. These conceptual pillars form the analytical “glue” that connects all chapters.

To address the research problem holistically, the thesis adopts a multi-level structure aligned with the architecture of the financial system. By systematically analyzing policy interventions at the firm, bank, and market levels, the thesis provides a holistic understanding of how policies propagate through the financial system, how they interact with incentives and heterogeneity, and how their design determines whether they achieve their goals or generate new constraints. Although each chapter addresses a distinct institutional context, the thesis is intentionally structured to build a coherent intellectual progression. This progression reflects a deliberate micro to meso to macro structure. It moves from the narrow behavior of firms, to the intermediary role of banks, to the systemic dynamics of capital markets. In doing so, the thesis advances from local incentive distortions to institutional resilience to market-wide distributional outcomes, creating an implicit narrative arc. This layered design demonstrates that understanding policy effectiveness requires tracing how interventions operate across multiple levels of the financial system, and how mechanisms at one level propagate to others.

By drawing on multiple institutional settings, the thesis enhances external validity and demonstrates that the core mechanisms identified, such as design features, heterogeneous responses, and unintended effects, are not confined to a single domain. The cumulative format also allows for deep empirical analysis within each layer of the financial system while maintaining a coherent and unified research agenda. This approach mirrors the complexity of real-world policy environments, where interventions do not occur in isolation but interact across different institutional levels.

This thesis addresses policies that are not only empirically relevant but also representative of core policy mechanisms used globally. Simplified tax regimes for small firms exist in numerous countries and are a primary tool of entrepreneurship and formalization policy. Banking stability during systemic shocks is a universal concern, and the COVID-19 pandemic provides

a natural experiment to examine operational resilience and macroprudential effectiveness. The ECB's Corporate Sector Purchase Program is a canonical example of unconventional monetary policy and has influenced similar interventions worldwide. SMEs access to finances is heavily dependent on the stability of the banking sector, that is susceptible to exogenous shocks. However, the central banks' interventions and macroprudential policies impact the stability of financial markets, thus causing a trickle down effect back to the SMEs.

These cases were chosen as they illuminate generalizable insights about policy design, incentives, heterogeneity, and unintended consequences. They represent the three fundamental layers of financial intermediation - i.e. firms, banks, and capital markets, and thus provide a comprehensive lens on the financial system and interconnectedness of micro and macro financial environments. The diversity of settings strengthens the robustness and breadth of the conclusions, demonstrating that the mechanisms identified are not context-specific anomalies but systemic features of how policies function in practice.

1.3 Core Concepts

To understand why policy interventions produce such varied outcomes across firms, banks, and markets, this thesis relies on a set of core concepts that form a unified conceptual framework. These concepts appear across all empirical chapters and serve three essential purposes. First, they explain why we observe heterogeneous outcomes from policies that are seemingly uniform in design. Second, they help identify why some policies achieve their goals while others fail or even backfire, despite being well-intentioned. Third, they illuminate the mechanisms through which policy interventions translate into real economic and financial effects, allowing us to move beyond simple average treatment effects toward a deeper understanding of behavior and incentives.

At the heart of this framework is the idea that policy design is never neutral. Policies embed thresholds, eligibility criteria, targeting strategies, and operational requirements that shape the incentives faced by firms, banks, and market participants. These incentives are not experienced equally, agents differ in size, capacity, information, technological readiness, and market power, leading to heterogeneous responses. As a result, two agents exposed to the same policy may behave very differently, generating variation in outcomes that cannot be captured by looking at averages alone.

Moreover, behavior is shaped by mechanisms that are often overlooked in traditional policy analysis. For instance, risk-taking can be active or passive, a distinction that is crucial in understanding how banks respond to crises. Similarly, spillovers and distributional effects mean that policies targeting one group can unintentionally affect others through changes in pricing, competition, or market structure. In many cases, unintended consequences arise not because policies are misaligned with objectives, but because they do not account for the institutional or behavioral realities of the agents they target.

Across all chapters, these core concepts serve as the analytical lens through which policy interventions are evaluated. Together, they provide a consistent framework for analyzing different types of policies across different institutional layers of the financial system. By grounding the empirical work in these concepts, the thesis is able to offer a coherent and generalizable explanation of how policy interventions operate in practice, why their effects vary across contexts, and what principles are needed to improve their design.

1.3.1 Policy design

Policy design refers to the concrete way in which policy instruments are structured, implemented, and operationalized. It is distinct from the mere choice of policy tool. Rather, policy design concerns the rules, parameters, and mechanisms that determine how the tool functions in practice. In the context of this thesis, policy design encompasses elements such as eligibility thresholds, targeting criteria, timing of implementation, administrative simplicity or complexity, digital and operational capacity requirements, and the degree of flexibility or discretion allowed. Whether in taxation, banking regulation, or monetary interventions, these design features shape how agents interact with the policy and, ultimately, determine its effectiveness.

Policy design can be understood along several key dimensions. One dimension is flexibility versus rigidity, some policies allow adaptation to changing conditions or agent characteristics, while others impose fixed rules that may become misaligned with reality over time. Another is simplicity versus accuracy, simplified policies reduce administrative burden but may impose blunt thresholds or uniform rules that fail to capture relevant differences across agents. A third is uniformity versus targeting, policies can apply evenly across all agents or be directed at specific groups, sectors, or conditions, with each approach having benefits and risks. A further dimension is static versus dynamic design, static rules assume stable environments,

whereas dynamic or adaptive policies adjust as conditions evolve. Finally, policies may have direct effects on their target group but also indirect effects or spillovers on others, making it crucial to consider the broader system in which they operate.

Policy design matters as the same policy tool can succeed or fail depending on how it is structured. A tax incentive may encourage formalization and growth if designed with gradual phase-outs, but discourage expansion if it includes rigid eligibility thresholds. A macroprudential regulation may contain risk in theory, but if banks lack the operational or technological capacity to implement it, it may increase passive risk during crises. A central bank asset purchase program may improve liquidity for eligible firms but simultaneously raise financing costs for ineligible firms if market power shifts toward certain intermediaries. In each case, the tool itself is not the issue, the design is.

Poorly designed policies generate distortions, disincentives, and unintended consequences. Threshold-based tax regimes may trap firms at suboptimal sizes. Regulatory frameworks that focus only on capital ratios may overlook operational vulnerabilities, leading to increased risk in times of stress. Monetary interventions that target specific firms may segment markets, undermine competition, or create long-term dependency on central bank support. In contrast, well-designed policies align the incentives of economic agents with policy objectives, support efficient resource allocation, promote resilience, and minimize negative spillovers. Good design is adaptive, incentive-compatible, and sensitive to heterogeneity.

1.3.2 Incentives and Heterogeneity

At the heart of policy effectiveness lies the concept of incentives. Policies alter the relative costs and benefits of different actions, thereby shaping the decisions of firms, banks, and market participants. These incentives may be explicit and intended, such as reducing compliance costs to encourage formalization. or implicit and unintended, such as discouraging firms from growing past an eligibility threshold or encouraging banks to take on risk when supervision is weak or operationally constrained. In all cases, agents respond not to the stated goals of policy, but to the incentive structures embedded in policy design. Understanding these incentives is essential for explaining real-world behavior.

However, incentives alone do not determine outcomes. The same incentive can lead to very different responses across agents due to heterogeneity in characteristics, capabilities, and constraints. Heterogeneity means that economic agents are not identical, firms differ in size,

productivity, growth potential, and administrative capacity, banks differ in digital readiness, business models, and risk appetite, market participants differ in eligibility status, access to intermediaries, and bargaining power. These differences are systematic and consequential, not random noise. As a result, a policy that benefits some agents may disadvantage others, even when applied uniformly.

Heterogeneity matters as average treatment effects can obscure the most important mechanisms. A policy may appear to have little or no effect on average, while producing large positive effects for some agents and large negative effects for others. Such distributional consequences raise both efficiency and equity concerns and often explain why policies fail to achieve their objectives. Moreover, unintended consequences frequently arise because policymakers implicitly assume homogeneous responses, ignoring variation in capacity, incentives, or constraints.

The core insight of this thesis is that incentives and heterogeneity must be analyzed together. The interaction between policy design and agent heterogeneity drives the real effects of intervention. Policies do not act directly on outcomes, they act on behavior, and behavior is filtered through the heterogeneous incentives, capacities, and constraints of economic agents. Predicting policy outcomes therefore requires understanding both what incentives a policy creates and how different types of agents are able or willing to respond. Thus, incentives and heterogeneity together constitute one of the most powerful and unifying mechanisms in the thesis, explaining why similar policies perform differently across contexts, why unintended consequences arise, and how policy design can be improved to better align micro-level behavior with macro-level goals.

1.3.3 Passive and Active Risk

A central conceptual innovation of this thesis is the distinction between active and passive risk-taking. While the banking literature has traditionally focused on active risk-taking, often linked to moral hazard, competition, or search-for-yield behavior, this thesis demonstrates that a significant share of risk in real-world crises emerges not from deliberate choices, but from operational frictions and capacity constraints. Distinguishing between these two forms of risk-taking is essential for understanding how banks actually behave under stress and for designing effective regulation.

Active risk-taking refers to deliberate, strategic decisions by banks to increase their exposure to risk in pursuit of higher returns. It is profit-driven and choice-driven. Banks may relax lending standards, take speculative positions, or allocate capital toward high-yield assets. Active risk-taking is at the core of much of the traditional literature on banking behavior (Anadu, Kruttli, McCabe, & Osambela, 2020; Buston, 2016; Prondzinski & Miller, 2018), which emphasizes moral hazard, competition effects, risk-shifting, and regulatory arbitrage. This body of work assumes that banks have the capacity to adjust their portfolios and make intentional choices about risk exposure.

Passive risk-taking, by contrast, occurs when banks' risk exposure increases unintentionally due to operational disruptions, reduced monitoring capacity, or inability to adjust portfolios. It arises not from strategic decisions but from institutional constraints, breakdowns in risk management, or external shocks that impair normal functioning. For example, during a crisis, banks may face staff shortages, technological failures, or regulatory delays that prevent them from screening borrowers, rolling over loans, or reallocating assets. As a result, risky exposures accumulate by default rather than by design. Passive risk-taking is not a choice, it is a consequence of the bank's inability to act.

This distinction matters as active and passive risk-taking respond differently to policy interventions. Macroprudential tools aimed at discouraging strategic risk-taking may be effective against active risk but have little impact on passive risk if the problem lies in operational capacity rather than incentives. Conversely, policies that enhance digital infrastructure, monitoring capability, or crisis preparedness can mitigate passive risk without necessarily changing the bank's appetite for active risk. In other words, effective regulation must target both incentives and capacity. Focusing on only one dimension leaves the system vulnerable.

Understanding the difference between active and passive risk-taking also requires an appropriate empirical measure of risk. A commonly used and widely accepted measure in the banking and financial stability literature is the Z-score, which captures the probability of bank insolvency by combining profitability, leverage, and earnings volatility into a single indicator. The Z-score is typically defined as:

$$Z_{score} = \frac{(ROA + CAR)}{\sigma(ROA)} \quad (\text{Eq.1})$$

A higher Z-score implies greater distance from default and thus lower overall risk, while a lower Z-score indicates that the bank is closer to insolvency and therefore taking on more risk. The Z-score is particularly valuable as it reflects both income stability and capital buffers, making it a robust indicator of overall bank resilience. Unlike narrow measures such as non-performing loans or capital ratios alone, the Z-score captures dynamic risk by incorporating earnings volatility over time. It is therefore well-suited for analyzing the evolution of risk under policy interventions or external shocks.

However, the Z-score also has a deeper conceptual relevance to this thesis as it helps distinguish between active and passive sources of risk. Active risk-taking may lead banks to intentionally increase leverage or invest in riskier assets, which can lower the Z-score through declining capital ratios or more volatile earnings. Passive risk-taking, on the other hand, may manifest through increased earnings volatility or deteriorating loan portfolios due to operational disruptions, even if banks are not strategically increasing leverage or changing asset allocations. This, too, results in a decline in the Z-score, but for fundamentally different reasons.

Thus, the Z-score serves as a summary outcome that reflects both types of risk-taking but does not itself distinguish between active and passive mechanisms. Identifying the mechanism requires additional analysis. The value of Z-scores has been proven in the literature with abundant reliance (Bouvatier, Lepetit, Rehault, & Strobel, 2018; Elia, Toros, Sawaya, & Balouza, 2021; Khaddafi, Heikal, Nandari, et al., 2017; Lepetit & Strobel, 2013, 2015; X. Li, Tripe, & Malone, 2017).

1.3.4 Digital Transformation

Digital transformation in banking refers to the adoption of advanced technological infrastructure to support credit operations, risk monitoring, data processing, and service delivery. It is not merely an efficiency upgrade, it fundamentally changes how banks function. A digitally advanced bank can process loans remotely, continuously monitor borrower risk, automate compliance tasks, and maintain operational continuity even under extreme disruption. Digital capacity therefore enhances operational resilience, allowing banks to sustain credit supply and risk control during crises. This transforms digitalization from an internal management choice into a macroprudential variable, it becomes a determinant of system-wide stability.

Traditionally, banking regulation has focused on capital ratios and balance sheet constraints, assuming operational capacity as given. Regulators can promote digital readiness proactively, much like capital buffers, as part of macroprudential frameworks. Digital transformation illustrates a broader principle, institutional capacity shapes policy effectiveness. A policy cannot succeed if the institutions implementing it lack the operational tools to carry it out. With the importance of the digitalization and digital transformation, the literature has grown significantly in the recent years with supportive outlook on efficiency (Ayadi, Chiaramonte, Cucinelli, & Migliavacca, 2025; Carbó-Valverde, Cuadros-Solas, Rodríguez-Fernández, & Ey, 2020; Do, Pham, Thalassinou, & Le, 2022; Nguyen, Ho, & Nguyen, 2023; Xie & Wang, 2023). Additionally, the research suggests positive impacts on stakeholders (Tsindeliani et al., 2022), but further argues that digital transformation is no longer an advantage, but rather a necessity (Alonge et al., 2021; Cuesta, Ruesta, Tuesta, Urbiola, et al., 2015). Nonetheless, small subset of literature suggests negative impacts (Khattak, Ali, Azmi, & Rizvi, 2023; Nguyen-Thi-Huong, Nguyen-Viet, Nguyen-Phuong, & Van Nguyen, 2023).

1.3.5 Spillovers and Capital Markets

Spillovers refer to policy effects that extend beyond the directly treated or eligible agents. They can occur horizontally, across firms or intermediaries that compete in the same market, or vertically, across different layers of the financial system, such as firms, banks, and markets. Spillovers arise because policy changes one part of the system, prompting adjustments elsewhere through pricing, competition, or strategic behavior. Spillovers matter as targeted policies rarely stay targeted. Even if a central bank buys bonds from a subset of firms, the intervention can reshape market pricing, underwriting incentives, and access to capital for non-eligible firms. If policymakers evaluate an intervention only by looking at treated units, they risk underestimating unintended costs or distortions. Spillovers therefore raise issues of fairness, distributional impact, and market structure.

Capital markets determine the cost and availability of external finance for firms. Policy interventions influence these pricing mechanisms directly and indirectly. Underwriting costs serve as a measure of financing frictions, reflecting both the bargaining power of intermediaries and the perceived risk of issuers. Policies that lower financing costs for some firms may simultaneously raise costs for others, creating redistributive effects across the market. This introduces equity and efficiency concerns and highlights how policy can unintentionally distort

competition or resource allocation. Moreover, if firms come to rely on policy support for favorable pricing, there is a risk of long-term dependency, weakening private market discipline.

Unintended constraints arise when policies designed to support agents actually limit their choices, suppress growth, or create new vulnerabilities. These constraints are rarely explicit, they emerge from rigid or poorly aligned policy design. In [Chapter 2](#), threshold-based tax regimes reduce compliance burdens but discourage firms from growing, trapping them near the eligibility limit. In [Chapter 3](#), macroprudential frameworks assume operational capacity, when that capacity is weak, regulation fails during emergencies, leading to passive risk-taking. In [Chapter 4](#), targeted monetary interventions improve liquidity for eligible firms but increase financing costs for ineligible firms, restricting their access to capital.

These examples show that well-intentioned policies can undermine their own objectives when design does not account for incentives, capacity, or heterogeneity. Every policy carries trade-offs, simplicity vs. precision, stability vs. flexibility, support vs. distortion. The thesis reveals these trade-offs across institutional settings and argues that making them explicit is essential to better policy design. The empirical findings of this thesis motivate a broader normative conclusion, effective policies must be both flexible and coordinated. Flexible policies are adaptive, context-sensitive, and capable of evolving over time. They adjust to heterogeneity, avoid rigid thresholds, and allow for gradual transitions or differentiated treatment. Flexibility prevents unintended constraints and supports long-term growth and resilience.

Coordinated policies are aligned across layers of the financial system. Tax policies should not discourage firm growth while banking regulations seek to increase lending capacity. Monetary interventions should not undermine market competition while supporting liquidity. System-wide effectiveness requires policy tools to reinforce rather than contradict each other. Flexibility and coordination are not optional, they are the core design principles that emerge from the thesis. They connect the empirical results of all three chapters and define the path toward better financial policy.

1.3.6 Fiscal Policy

Fiscal policy, particularly taxation, is one of the most powerful tools governments use to influence firm behavior, resource allocation, and economic development. In the context of this thesis, fiscal policy is not viewed simply as a source of revenue for the state, but as a micro-level financial policy instrument that directly shapes the incentives and constraints faced by

firms. Through the design of tax systems, especially the structure of tax bases, rates, thresholds, eligibility criteria, compliance procedures, and enforcement mechanisms, governments intentionally or unintentionally alter firms' decisions regarding entry, formalization, investment, employment, and growth.

Traditional analyses of taxation often focus on statutory rates or overall tax burdens. However, this thesis emphasizes that policy design matters as much as, if not more than, the level of taxation itself. Simplified tax regimes, presumptive taxes, or lump-sum systems aim to reduce compliance costs and support small firms, but their actual impact depends crucially on how they are structured. A simplified regime with a rigid revenue threshold, for example, may reduce administrative burden for the smallest firms while creating strong disincentives to grow, as firms choose to remain below the threshold to retain favorable treatment. Thus, taxation is not just a fiscal instrument but a behavioral policy tool, shaping firm trajectories through incentives.

Fiscal policy also reveals the importance of heterogeneity. A uniform tax policy can produce unequal effects across firms depending on size, profitability, growth potential, formality, or industry. The same tax regime may help micro-firms but discourage medium-sized firms from scaling. Some firms benefit from lower compliance costs, while others face binding constraints. This heterogeneity is systematic and predictable, not random. Therefore, average treatment effects can mask critical distributional consequences, making fiscal policy a prime example of why heterogeneity must be central to policy analysis.

Importantly, fiscal policy interacts with other layers of the financial system. Firm growth incentives created by tax policy affect credit demand, influencing banking portfolios. Tax incentives for investment or formalization affect firms' ability to access external finance in both banking and capital markets. Conversely, limited access to finance can reduce the effectiveness of tax incentives. These interactions illustrate that fiscal policy is not isolated, it is deeply connected to financial intermediation and market structure.

1.3.7 Unconventional Monetary Policy

Unconventional monetary policy refers to a set of non-traditional tools used by central banks when standard interest rate policy becomes constrained, most often at the zero lower bound. Rather than influencing the economy solely through short-term interest rates, central banks

use large-scale asset purchases, liquidity facilities, and targeted credit programs to affect long-term rates, market functioning, and financing conditions. These tools, such as quantitative easing (QE), credit easing, and corporate sector purchase programs, have become a defining feature of the post-2008 monetary environment and are now central to financial policy frameworks.

Crucially, unconventional monetary policy does more than provide liquidity, it directly intervenes in specific segments of financial markets, often targeting particular asset classes or types of firms. This makes policy design especially important. The effects of such interventions depend on which assets are purchased, which firms are eligible, when and how purchases occur, and how the program is communicated. Eligibility rules and targeting criteria effectively allocate central bank support and can influence market structure.

Unconventional monetary policy also reveals the importance of heterogeneity. Eligible firms may experience lower yields, cheaper underwriting costs, and better market access, while ineligible firms may face higher financing costs or reduced visibility among intermediaries. Banks and underwriters may shift their attention and resources toward eligible issuers, altering the competitive dynamics of the market. Thus, even when the policy appears aggregate in scope, its distributional consequences are significant.

The impact is not limited to direct participants, spillovers are central. When central banks buy corporate bonds from eligible firms, yields decline not only for those firms but also for others in related markets. At the same time, the reallocation of underwriting or intermediation activity can have vertical spillovers, affecting how banks manage portfolios or how firms structure financing strategies. Ignoring these spillovers leads to incomplete or biased evaluations of policy effectiveness and masks unintended consequences.

Most existing literature on unconventional monetary policy focuses on aggregate effects, credit spreads, yields, liquidity, or macroeconomic outcomes (Bhattarai & Neely, 2022; Neely, 2015). However, these averages often hide distributional asymmetries and market distortions. The thesis argues that understanding unconventional monetary policy requires moving beyond the question of whether it lowers yields on average to asking who benefits, who loses, and how market incentives change.

1.4 Institutional Settings and Roles

The financial system operates across multiple institutional layers, each with distinct functions but deeply interconnected dynamics. At the micro level, firms make investment, employment, and growth decisions within the constraints and incentives created by taxation systems and regulatory environments. Tax policies directly shape profitability, formalization, and expansion, making firms highly sensitive to the design of fiscal interventions. At the meso level, banks serve as the core financial intermediaries that allocate credit, manage risk, and support economic activity. The regulatory and supervisory frameworks governing banks influence their stability, lending decisions, and resilience to shocks. At the macro level, capital markets and monetary authorities determine liquidity conditions, pricing of risk, and access to external financing through interventions such as asset purchase programs or interest rate policies.

These layers are not independent, they form an integrated system. Policies targeting one layer, such as tax incentives for small firms, affect credit demand and bank portfolios. Banking resilience affects capital flows to firms and the stability of capital markets. Monetary interventions change market pricing, which in turn influences both firm financing costs and bank intermediation strategies. Understanding financial policy therefore requires moving beyond isolated analysis and recognizing how interventions at different levels interact and propagate through the system. Studying these layers together reveals the systemic mechanisms through which policy design shapes economic outcomes.

Each empirical chapter examines a specific institutional setting that is both policy-relevant and analytically rich, serving as a natural laboratory for studying how policy design interacts with incentives and heterogeneity. **Chapter 2** focuses on taxation policy for micro-firms. Simplified, threshold-based tax regimes are widely used across the world to reduce compliance burdens and encourage formalization. They represent a common policy tool aimed at supporting entrepreneurship and small business growth. However, these regimes embody a central trade-off, while simplicity reduces administrative costs, fixed thresholds may distort incentives and discourage firms from expanding. Firms are the foundation of the economy, and micro-level policy effects can scale to macroeconomic consequences. This setting is therefore an ideal environment to study how policy design influences firm behavior, growth incentives, and unintended constraints.

Chapter 3 examines banking behavior during public emergencies, using the COVID-19

shock in Hubei Province. Banks are essential intermediaries in the financial system, and their stability is critical for macroeconomic resilience. Emergencies such as pandemics or crises test not only capital adequacy but also operational capacity and regulatory design. The onset of COVID-19 in Hubei provides a rare natural experiment for studying how banks respond under extreme stress and how institutional preparedness affects stability. Importantly, the chapter highlights digital transformation as an emerging macroprudential tool, showing that operational infrastructure can be as important as traditional regulation. This institutional context reveals how policy design and institutional capacity determine risk-taking and resilience.

Chapter 4 analyzes the European Central Bank's Corporate Sector Purchase Program (CSPP). As one of the most influential monetary authorities globally, the ECB has pioneered unconventional monetary policies that have been widely studied and emulated. The CSPP is a major intervention in corporate bond markets, a key financing channel for non-bank firms. This setting illustrates how large-scale, targeted policies can reshape market pricing, alter intermediation incentives, and generate spillover effects beyond directly eligible firms. By examining underwriting costs and distributional outcomes, this chapter shows how policy design affects market structure and competition. Far from being a narrow case, the CSPP exemplifies the broader class of quantitative easing and asset purchase programs implemented in the US, UK, Japan, and elsewhere.

Together, these institutional settings are strategically chosen to represent the most important policy domains in the financial system, fiscal policy at the firm level, macroprudential policy at the banking level, and monetary policy at the market level. They are not arbitrary case studies, but globally relevant policy forms that allow for generalizable insights into policy design and incentives. The empirical contexts in this thesis reflect policy instruments that are widely used across countries and over time. Simplified tax regimes for micro-firms exist in numerous countries, making the mechanisms uncovered in **Chapter 2** broadly applicable. Macroprudential and emergency banking policies have become central to regulatory frameworks worldwide, especially after the 2008 financial crisis and the global impact of COVID-19, making **Chapter 3** highly relevant beyond China. Central bank asset purchases, such as quantitative easing and corporate bond programs, have been implemented by the Federal Reserve, Bank of England, Bank of Japan, ECB, and others, making **Chapter 4** representative of a global policy trend.

Thus, the thesis does not analyze isolated or idiosyncratic cases. Instead, it studies archetypal policy tools that reveal universal mechanisms of policy design, incentive effects, heterogeneity, and unintended consequences. The findings extend beyond the specific geographies and institutions examined and provide insights that are valuable in a wide range of contexts. A key contribution of this thesis is to show that institutions are not neutral backdrops, they actively shape how policies operate and what outcomes they produce. Institutions establish the rules, constraints, and operational capacities under which firms, banks, and market participants make decisions. For example, eligibility criteria and enforcement rules in tax systems shape firm growth behavior. The digital or operational readiness of banks determines whether macroprudential policy succeeds in preventing risk escalation. In capital markets, institutional frameworks influence how monetary policy spillovers are transmitted and who bears the costs.

Institutions determine who can access a policy, how easily it can be implemented, whether compliance is feasible, and how behavior adjusts in response. They also mediate the distributional effects of policy, influencing whether interventions create inequalities or reinforce existing advantages. Understanding policy effectiveness therefore requires understanding institutions, their structure, capacity, flexibility, and alignment with incentives. Ignoring institutional context risks missing the very mechanisms that drive policy outcomes.

The thesis examines three core policy actors, each operating at a different level of the financial system and deploying different tools, specifically (1) Government / Tax Authorities in [Chapter 2](#) use fiscal instruments to influence firm behavior, formalization, and growth. (2) Banking Regulators and Supervisors, together with Banks in [Chapter 3](#) implement macroprudential rules and manage stability under stress. (3) Central Banks and Capital Market Intermediaries [Chapter 4](#) deploy monetary interventions that affect pricing, liquidity, and access to finance.

Each actor operates under different constraints and objectives. Governments balance simplicity and growth incentives. Banking regulators balance stability and credit supply, often without full control over operational capacity. Central banks balance market functioning, distributional neutrality, and long-term dependence risks. Each chapter shows how policy design interacts with these institutional realities, and how misalignment between objectives, tools, and incentives can lead to unintended consequences.

By analyzing these actors across levels, the thesis situates itself within the broader policy ecosystem and demonstrates how firm-level fiscal policy, bank-level regulatory policy, and

market-level monetary policy are interconnected components of a single financial architecture. Although firms, banks, and capital markets operate at different layers of the financial system, they are deeply interdependent. Tax policy affects firm growth, which in turn shapes demand for credit, influencing banks' lending behavior and portfolio composition. Banking stability affects the supply of credit, which determines firms' ability to invest, expand, or access market financing, ultimately shaping market participation and pricing. Capital market policy affects firms' financing conditions, which feeds back into banks' balance sheets and risk exposures, altering systemic stability. Thus, policies implemented at one institutional level inevitably spill over into the others, sometimes reinforcing desired outcomes but often creating unintended distortions or feedback effects. This interconnectedness highlights that financial policy cannot be fully understood in isolation. Achieving system-level resilience and efficiency requires coordination across institutional layers, ensuring that policies at the micro, meso, and macro levels are aligned rather than working at cross-purposes.

The thesis also reveals that institutions face structural trade-offs that shape policy design and outcomes. At the taxation level, there is a tension between simplicity and growth incentives, simplified regimes reduce administrative burdens but risk discouraging firms from expanding if thresholds are rigid. At the banking level, regulators must balance financial stability and operational flexibility, strict rules may improve stability in theory but fail during crises if banks lack the capacity to operate under stress. At the monetary policy level, central banks weigh supporting markets against distorting them, large-scale interventions can improve liquidity and reduce borrowing costs, but may undermine competition or create long-term dependency. More broadly, standardized rules across institutions aim to ensure fairness and predictability, yet they often ignore heterogeneity in agents' capabilities and constraints, leading to uneven outcomes. These trade-offs appear in every empirical chapter and form a unifying thread across the thesis, demonstrating that the success of policy interventions depends not just on their objectives, but on how institutions navigate these inherent tensions.

Institutions do more than administer policy, they create the incentive structures through which policy effects materialize. They determine who is eligible for benefits or subject to regulation, how rules are enforced, and what costs or advantages agents face when responding to policy interventions. For example, tax authorities set thresholds and rates that influence firm size decisions, banking regulators impose capital requirements and operational standards that shape risk-taking behavior, central banks determine which firms are eligible for asset

purchases, influencing market access and competitive positioning. Institutions also create heterogeneity in capacity, digitally advanced banks respond differently to crises than traditional banks, large firms can adapt to policy changes more easily than small firms. In this way, institutions serve as the key mechanism linking policy design to behavioral responses, and therefore to economic and financial outcomes. Understanding institutions is thus essential for understanding how and why policy interventions succeed or fail.

The choice of institutional settings in this thesis is not only conceptually motivated but also timely and policy-relevant. The simplified tax regime analyzed in [Chapter 2](#) reflects widespread global efforts to use fiscal policy as a tool for stimulating entrepreneurship and reducing informality, making it a highly relevant and contemporary policy instrument. The COVID-19 emergency in [Chapter 3](#) provides a rare opportunity to observe banks under extreme stress, serving as a natural stress test of macroprudential frameworks and operational capacity. This context allows the thesis to examine risk-taking and resilience in conditions where policy effectiveness is most critical. The CSPP studied in [Chapter 4](#) represents one of the most significant unconventional monetary policy interventions of the past decade, implemented during a period when central banks expanded their role in corporate financing. Its timing coincides with a broader shift in how central banks influence markets, making it an ideal case to study targeted policy design, spillovers, and distributional effects. By focusing on these moments, the thesis captures policy interventions at the precise times when their mechanisms and consequences are most visible, ensuring both empirical richness and contemporary relevance.

Together, these institutional settings provide rich empirical environments for analyzing how policy design, incentives, and heterogeneity interact across different layers of the financial system. They illustrate the importance of understanding institutions not as passive backdrops but as active determinants of policy outcomes, capable of amplifying or constraining the effects of interventions. These observations set the stage for the next section, which turns to the theoretical and empirical literature to identify what is already known and, crucially, what remains poorly understood about policy design and effectiveness. This literature-based grounding is necessary to position the thesis within existing academic debates and to highlight the research gaps that the empirical chapters address. By linking institutional context to theoretical insight, the thesis prepares the reader for a multi-level empirical research agenda that investigates firms, banks, and markets through a unified conceptual lens.

1.5 Theoretical and Empirical Gaps in the Literature

Although a vast body of research has examined fiscal, banking, and monetary policies, much of the existing literature studies these domains in isolation, focusing either on firm-level taxation, bank-level regulation, or market-level monetary interventions. This siloed approach has generated substantial insights within each field, but it has also left important theoretical blind spots and empirical gaps, especially concerning how policy design, incentives, and heterogeneous responses interact to determine real outcomes.

The purpose of this section is to identify what the literature already knows, what it still does not know, and why these gaps matter, both for academic understanding and for policy effectiveness. Importantly, the aim is not to provide an exhaustive literature review, but to highlight the critical missing elements that motivate the research agenda of this thesis and explain its contributions. Across all three domains, the literature has provided important findings. Corporate finance research has shown that taxes influence firm behavior and investment decisions. Banking literature has demonstrated that regulation affects risk-taking and stability. Monetary policy research has documented that unconventional interventions impact yields, liquidity, and financing conditions. These insights are valuable, but they are incomplete.

1.5.1 Corporate Finance and Taxation Policies

A substantial body of research in corporate finance and public economics has examined how taxation influences firm behavior, investment decisions, and formalization (Audretsch, Belitski, Chowdhury, & Desai, 2021; Block, 2021; Braunerhjelm, Eklund, & Thulin, 2019; Venâncio, Barros, & Raposo, 2020). A central theme in this literature is the widespread use of simplified or preferential tax regimes, such as presumptive taxes, lump-sum systems, or gross receipts taxes, to support micro and small firms (Fernández-Bastidas, 2023; Hansen, Miller, & Weber, 2021; Xing, Bilicka, & Hou, 2022; Xing, Bilicka, Hou, & Raei, 2024). These regimes are designed to reduce compliance costs, lower administrative burdens, and encourage firms to enter the formal economy. Empirical studies generally find that simplified taxation can increase formalization, improve reported revenues, and in some cases support short-term profitability (Block, 2021; Venâncio et al., 2020). However, most of this evidence focuses on average treatment effects, and only a limited number of studies explore the deeper mechanisms through which taxation shapes firm incentives (Brueggemann, 2017; Y. Chen & Ma, 2021; Fernández-Bastidas,

2023; Venâncio et al., 2020). Some work has pointed to the possibility that threshold-based regimes may inadvertently discourage firms from growing, but this insight remains underdeveloped and often anecdotal. Moreover, literature finds negative association between tax code complexity and the entrepreneurship (Block, 2021). Lastly, literature finds positive impact of reduced taxation on entrepreneurship, but only to some extent (Block, 2021; Braunerhjelm et al., 2019).

Despite these contributions, the literature has several theoretical gaps. Much of the existing theory treats taxation as a static cost rather than a dynamic incentive mechanism that influences firm trajectories over time. There is limited formal modeling of how eligibility thresholds or cliff effects embedded in tax design create behavioral distortions that affect firm decisions about scaling, hiring, or investment. The trade-off between administrative simplicity and long-term efficiency is recognized but rarely theorized in a rigorous framework. Moreover, taxation is often discussed in isolation from broader firm dynamics, with little integration into models of business growth, productivity upgrading, or transitions from micro to small and medium-sized firms.

Empirically, several important gaps remain. There is a lack of causal evidence on heterogeneous effects, specifically, which types of firms benefit from simplified regimes and which are harmed or constrained. Very few studies examine long-term or dynamic outcomes, such as whether tax policy affects firm scaling, survival, or expansion over time (Venâncio et al., 2020). The behavioral distortions that occur near eligibility thresholds, where firms may deliberately restrict growth to maintain preferential tax status, are largely underexplored, despite strong anecdotal evidence. Additionally, much of the existing evidence comes from individual country case studies without broader attempts to generalize or extract underlying mechanisms.

These gaps matter as fiscal policies aimed at supporting small firms may unintentionally trap them in smallness, undermining productivity growth and firm development (Ahmad, Audi, & Ahmad, 2025; Coppier, Michetti, & Scaccia, 2023). If thresholds create disincentives to expand, simplified regimes may succeed in reducing compliance costs but fail in fostering long-term economic dynamism. Without understanding heterogeneous responses, policymakers cannot distinguish between firms that are genuinely constrained and those that are strategically optimizing around policy rules. Effective policy design requires knowing who benefits, who is harmed, and why. Focusing on gross receipt taxation (GRT) specifically, the

literature shows that the GRTs are widely used in developing countries due to their ease of administration and their perceived resistance to tax evasion. Moreover, the GRTs are effectively reduce tax evasion (Best, Brockmeyer, Kleven, Spinnewijn, & Waseem, 2013). However, GRTs do cause inefficiencies and financial worsening (Chamberlain & Fleenor, 2007; Hansen et al., 2021; Simbyakula & Hapompwe, 2024; Xing et al., 2022, 2024).

This thesis addresses these gaps directly. In Chapter 2, it provides causal evidence on the effects of a simplified gross receipts tax regime using quasi-experimental methods. It moves beyond average treatment effects to show strong heterogeneity by firm size, demonstrating that very small firms benefit from reduced compliance burdens and exhibit higher growth, while firms near the eligibility threshold experience reduced performance and stagnation. The chapter also examines long-run outcomes, showing how policy design shapes dynamic firm trajectories rather than only short-term behavior. Most importantly, it identifies a core policy trade-off between simplicity and growth incentives, and develops a conceptual framework linking policy design to incentives to heterogeneous responses to dynamic outcomes. In doing so, it contributes both theoretically and empirically to the literature on taxation and firm dynamics, and lays the foundation for the broader argument of the thesis - i.e. policy design and heterogeneity are central to understanding policy effectiveness.

1.5.2 Banking Stability and Macroprudential Policies

A large body of research in banking and financial stability has examined how regulation influences risk-taking and how banks respond to competitive pressures and macroeconomic conditions. The established literature shows that banks engage in risk-taking behavior shaped by capital requirements, reserve ratios, supervisory intensity, and market forces (Banna, Hassan, & Rashid, 2021; Deng, Lv, Liu, & Zhao, 2021; Galletta & Mazzù, 2023; Kandrac & Schlusche, 2021; López-Penabad, Iglesias-Casal, & Neto, 2022; Moudud-Ul-Huq, 2021; Stulz, 2022; R. Wang, Liu, & Luo, 2021). The rise of macroprudential policy following the 2008 financial crisis led to the development of tools such as countercyclical buffers, liquidity coverage ratios, and stress tests, all aimed at reducing systemic risk and preventing crises (Aevoae, Andrieș, Ongena, & Sprincean, 2023; Borri & Di Giorgio, 2022; Danielsson, Macrae, & Uthemann, 2022; Davydov, Vähämaa, & Yasar, 2021; Duan, El Ghouli, Guedhami, Li, & Li, 2021; Duarte & Eisenbach, 2021; Jackson & Pernoud, 2021; Renn et al., 2022). A growing COVID-19 literature recognizes that emergencies place severe strain on banking operations, affecting risk exposure and lending

capacity (Borri & Di Giorgio, 2022; Duan et al., 2021). However, most of this work continues to emphasize active risk-taking, the deliberate pursuit of higher returns through riskier assets, typically explained by moral hazard, regulatory arbitrage, or the search for yield.

Despite these advances, significant theoretical gaps remain. The literature offers limited distinction between active and passive risk-taking, implicitly assuming that all risk arises from strategic choices. There is a lack of frameworks that conceptualize operational disruptions and capacity constraints as independent mechanisms of risk, even though crises frequently impair banks' ability to screen, monitor, and adjust portfolios. Moreover, existing theory rarely considers digital transformation as a component of regulatory design or macroprudential capacity. Digital readiness is typically viewed as a business efficiency choice rather than a stability-enhancing capability or regulatory objective. As a result, current macroprudential theory insufficiently captures how institutional capacity and operational resilience interact with regulation to shape outcomes during systemic stress.

Empirically, the literature also faces substantial gaps. There is limited causal evidence on how emergencies change bank behavior, particularly with respect to risk composition rather than lending volumes (Gramlich, Walker, Zhao, & Bitar, 2023; J. Liu et al., 2023; Skinner, 2021). Most studies examine changes in credit supply or performance but do not differentiate how risk exposure is accumulated or managed. Few studies explicitly measure digital readiness or analyze its role in determining bank resilience. Instead, much of the macroprudential literature implicitly assumes that banks are operationally stable and capable of implementing regulatory policies as intended. This assumption breaks down in real crises, where operational failures, technological limitations, or coordination breakdowns can become primary drivers of risk, regardless of capital adequacy.

These gaps matter for both research and policy. Emergencies are becoming more frequent and complex, pandemics, cyberattacks, geopolitical shocks, climate events, and they stress the operational foundations of banking systems. If operational capacity and technological infrastructure are ignored, regulators may focus on the wrong mechanisms of risk, targeting active risk-taking while overlooking the more immediate and dangerous rise in passive risk caused by an inability to respond. Without understanding how crises reshape incentives and constraints, macroprudential policies may provide a false sense of stability while leaving the system vulnerable in practice. To mitigate the exogenous risk, digital transformation can reduce the impact through various channels (F. Yang & Masron, 2024; Yao & Song, 2023).

This thesis directly addresses these gaps in [Chapter 3](#). Using the onset of COVID-19 in Hubei as a natural experiment, the chapter provides causal evidence that emergencies primarily increase passive rather than active risk-taking. It shows that banks do not strategically seek risk during crises, rather, risk accumulates because they are unable to monitor or adjust their portfolios effectively. Crucially, the chapter demonstrates that digital transformation significantly mitigates passive risk, enabling banks to maintain credit operations and control exposure. This positions digital readiness as a macroprudential instrument, a perspective that expands the scope of regulatory design beyond capital ratios and balance sheet measures. In doing so, the thesis broadens the empirical understanding and evaluation of banking stability, providing a more comprehensive framework for designing macroprudential policies that account for incentives, heterogeneity, and institutional capacity.

1.5.3 Capital Market Policies and Monetary Interventions

The literature on capital market policies and unconventional monetary interventions has demonstrated that central bank asset purchase programs, such as quantitative easing (QE) and credit easing tools like the Corporate Sector Purchase Program (CSPP), can reduce yields on eligible securities, lower financing costs, and support market liquidity ([Bhattarai, Chatterjee, & Park, 2021](#); [Christensen & Gillan, 2022](#); [D'Amico & Kaminska, 2019](#); [Karadi & Nakov, 2021](#)). These interventions have been widely studied in the context of the post-2008 monetary environment, with a strong consensus that such programs are effective in easing credit conditions for targeted firms. Most studies examine bond yields or spreads, demonstrating clear direct effects on eligible assets and supporting the view that unconventional monetary policy can serve as a powerful tool to stimulate corporate financing and stabilize markets ([Christensen & Gillan, 2022](#); [Kedward, Buller, & Ryan-Collins, 2021](#); [Pegoraro & Montagna, 2021b](#)).

However, despite these advances, several gaps remain. First, there is limited understanding of competition spillovers in financial intermediation. Central bank interventions not only affect asset prices but also alter the behavior and incentives of intermediaries such as underwriters and banks ([Pegoraro & Montagna, 2021b](#)). Yet, theory rarely considers how targeted asset purchases may reallocate attention, bargaining power, or liquidity provision across firms. Second, existing frameworks provide little insight into how policy interventions reshape market pricing mechanisms themselves. Rather than simply compressing yields, targeted policies may change how prices are formed, how risk is allocated, and how intermediation occurs ([Dedola,](#)

Georgiadis, Gräb, & Mehl, 2021; Nozawa & Qiu, 2021). Third, the literature lacks robust frameworks to analyze the distributional consequences of monetary policy, who benefits and who is disadvantaged. Most studies assume that effects are broadly symmetric or spillovers are positive, but real-world outcomes often show the opposite (Nozawa & Qiu, 2021; Pegoraro & Montagna, 2021b). Finally, the potential for long-term dependency on central bank support is under-theorized, repeated interventions may erode market discipline, reduce incentives for private risk-pricing, and create expectations of future support, yet these dynamics rarely feature in existing models (Pegoraro & Montagna, 2021b).

Most studies focus on secondary market bond yields, overlooking the primary market and the role of underwriting fees, issuance costs, or intermediary behavior (Duca, Nicoletti, & Martinez, 2016; Krishnamurthy & Vissing-Jorgensen, 2011; Rischen & Theissen, 2021; Siani, 2019a, 2019b; Todorov, 2017). Yet underwriting costs are a critical component of access to finance and reflect how banks allocate attention and negotiate with issuers. Very few studies examine micro-level data on market participants such as underwriters, who act as key transmission channels for policy (Duca et al., 2016; Siani, 2019a, 2019b). Moreover, although spillovers are often mentioned conceptually, they are rarely measured or quantified, leaving policymakers with limited understanding of the unintended consequences of targeted interventions.

These gaps matter because capital markets are complex, interconnected systems, where interventions at one point can produce distortions elsewhere. Targeted monetary policies may reinforce existing market power, privilege certain firms, and disadvantage others, not through direct exclusion, but through altered pricing dynamics and bargaining conditions. By changing the role of underwriters, such policies can reshape the structure of competition itself. Over time, repeated intervention may lead to long-term dependence on central bank support, undermining private market discipline and potentially creating systemic fragility. Without understanding these mechanisms, policymakers risk relying on tools that appear effective in the short term but generate hidden costs or inequalities.

Chapter 4 of this thesis directly addresses these gaps. Rather than analyzing yields alone, it focuses on underwriting costs, offering a novel perspective on how monetary interventions affect the cost of accessing capital markets. Using micro-level data on bond issuance and intermediation, the chapter provides causal evidence that the CSPP reduces underwriting costs for eligible firms but increases them for ineligible firms, revealing a clear spillover and distributional effect that the existing literature largely ignores. It shows that unconventional

monetary policy not only affects asset pricing but reshapes market pricing mechanisms and the competitive behavior of intermediaries. Moreover, the chapter highlights the potential for long-term dependency on central bank support, as firms and underwriters adjust behavior in anticipation of continued intervention. In doing so, the thesis expands the understanding of unconventional monetary policy from a macro tool affecting yields to a micro-structural force that influences incentives, market competition, and access to finance across the system.

1.6 Research Questions

This thesis is driven by a central question of how do policy interventions across different levels of the financial system shape firm behavior, financial stability, and access to funding, and how do their design features and heterogeneous micro-level responses determine these outcomes? This question reflects the core premise that policy tools do not operate in isolation, but rather their effects depend on how they interact with the incentives and capacities of firms, banks, and market participants. It also acknowledges that policy success or failure cannot be understood solely by examining headline outcomes. Instead, one must ask when and why do well-intentioned policies achieve their objectives, and when do they produce unintended consequences? What mechanisms, such as incentive alignment, operational capacity, or passive versus active risk, drive these heterogeneous effects? Finally, the thesis considers the normative dimension of how can policy tools be designed more flexibly and coordinated across institutional layers so that micro-level incentives support, rather than undermine, macro-level goals?

These overarching questions break into several analytical dimensions that guide the empirical work. A first dimension concerns policy design, including thresholds, eligibility rules, targeting mechanisms, and operational requirements. Understanding how these features shape incentives is essential to explain variation in policy effectiveness. A second dimension focuses on heterogeneity, recognizing that firms differ by size and growth potential, banks by technological capability and business models, and market participants by eligibility and bargaining power. These differences lead to divergent responses that strongly influence outcomes. A third dimension addresses behavioral mechanisms, especially in contexts where traditional models of strategic choice are insufficient. For example, distinguishing between active and passive risk offers a way to understand how operational disruptions can lead to risk escalation even

without intentional risk-seeking behavior. A fourth dimension examines unintended consequences and spillovers, where policies targeting one group inadvertently disadvantage others or reshape competitive dynamics. Finally, the thesis explores how micro-level responses translate into system-level effects, influencing growth, stability, pricing, and long-term resilience. These dimensions culminate in the broader goal of identifying principles for designing flexible, incentive-compatible, and well-coordinated policies that function effectively across a complex financial architecture.

To address these overarching questions, the thesis is structured into three empirical studies, each focusing on a distinct institutional layer and policy instrument. The first empirical study, presented in [Chapter 2](#), examines the effects of a simplified gross receipts tax regime targeted at micro-firms. Its core research question asks: How does such a tax policy influence firm financial performance and growth? This chapter first considers the average effects of the policy on firm performance, before moving beyond aggregate estimates to analyze how the policy affects different types of firms. In particular, it investigates whether firms of different sizes respond differently to the regime and whether these heterogeneous responses are driven by variations in incentives or constraints. A central focus is on the role of threshold-based design: Do eligibility rules encourage informal growth, or do they create disincentives for firms nearing the threshold, effectively trapping them below the limit? The chapter further explores the long-term, dynamic consequences of the policy, asking whether firms benefiting from lower compliance burdens successfully scale over time, and whether those near the eligibility cutoff experience suppressed expansion. This analysis directly contributes to the central theme of the thesis by revealing how policy design, incentives, and heterogeneity interact to generate both intended benefits and unintended constraints.

[Chapter 3](#) shifts the analysis to the banking sector and investigates how policy effectiveness depends on institutional capacity in times of systemic stress. Its core question is: How do public emergencies impact bank risk-taking and financial stability? Building on the distinction between different forms of risk behavior, the chapter asks whether crises primarily increase passive risk-taking, arising from operational disruptions and constraints, or active risk-taking, which reflects deliberate strategic choices. It examines the mechanisms through which operational shocks alter risk exposure, highlighting the role of internal processes, technological capacity, and resource allocation under stress. A key dimension of this chapter is the role of digital transformation as it investigates whether digitally advanced banks are more

resilient during emergencies, better able to maintain credit supply, and less prone to passive risk escalation. This leads to a broader question of whether digitalization can function as a macroprudential tool, complementing capital-based regulation. The findings have direct implications for the design of regulatory frameworks, suggesting that operational readiness and technological capacity should be integrated into macroprudential policy. This chapter thus deepens the thesis's analytical framework by introducing the passive versus active risk-taking mechanism and demonstrating how design and heterogeneity shape policy outcomes in the banking sector.

The third empirical study, in [Chapter 4](#), turns to capital markets and asks: How does the European Central Bank's Corporate Sector Purchase Program (CSPP) affect bond underwriting costs and financing conditions? This chapter evaluates whether targeted monetary policy reduces underwriting costs for eligible firms and, crucially, whether it generates spillover effects that increase costs for ineligible firms. It investigates how the intervention reshapes market pricing mechanisms, alters the competitive dynamics among intermediaries, and affects the distribution of resources across firms. The chapter also explores the distributional consequences of selective interventions, highlighting which firms benefit and which are disadvantaged by central bank support. Additionally, it examines whether reliance on such policies may foster long-term dependencies, thereby altering market structure and reducing incentives for private intermediation. By analyzing how policy-induced pricing changes at the micro-level translate into shifts in broader market behavior, this chapter contributes to the overarching theme of the thesis by showing how policy design, incentives, and spillovers interact to produce systemic outcomes.

Taken together, the three empirical studies each address a specific component of the broader research agenda. [Chapter 2](#) focuses on how policy design affects firm incentives and growth. [Chapter 3](#) examines how shocks and operational capacity determine the effectiveness of financial regulation and the resilience of intermediaries. [Chapter 4](#) investigates how large-scale monetary policy interventions shape market pricing dynamics and access to funding. Each chapter provides independent empirical contributions, yet they are tightly integrated as they apply the same conceptual lens, policy design, incentives, heterogeneity, behavioral mechanisms, and unintended consequences, to different layers of the financial system. This multi-level structure enables the thesis to develop a comprehensive understanding of how policy interventions operate in practice, how micro-level responses generate macro-level outcomes,

and why policy success depends on both design features and the characteristics of the agents they target. In this way, the cumulative format is not a collection of separate papers, but a coherent and progressive examination of the same fundamental problem across firms, banks, and markets.

1.7 Methodological Overview

This thesis adopts an empirical and data-driven approach to investigate how policy interventions shape economic and financial outcomes across firms, banks, and capital markets. It relies on quasi-experimental methods and natural experiments to identify causal effects, rather than simple correlations. Each empirical chapter exploits a real-world policy change or exogenous shock to isolate the impact of policy interventions on behavior and outcomes. By combining rigorous identification strategies with rich micro-level data from administrative records, banking databases, and bond issuance transactions, the thesis provides credible causal evidence on how policy design, incentives, and heterogeneity interact in practice. Rather than describing statistical associations, the thesis seeks to uncover causal mechanisms that explain why policies succeed or fail, and how their effects vary across agents and contexts.

Establishing causal inference is essential in policy evaluation. Policymakers must distinguish between correlation and causation to make informed decisions, especially when interventions have financial and economic consequences at scale. Simple correlations are inadequate for understanding heterogeneous effects, behavioral responses, or unintended consequences, as they are easily confounded by selection bias, reverse causality, or unobserved factors. Natural experiments provide a powerful methodological tool to overcome these challenges by exploiting policy changes or shocks that affect some agents but not others in ways unrelated to their underlying characteristics. By structuring each empirical chapter around a clearly articulated identification strategy, the thesis places empirical rigor at the center of its contribution, ensuring that findings are both credible and policy-relevant.

A distinctive strength of the thesis is its multi-study design, which enhances both internal and external validity. Each chapter examines a different policy context and uses a different dataset, creating triangulation of evidence. The firm-level, bank-level, and market-level analyses collectively cover multiple layers of the financial system, allowing for broader generalization of core mechanisms. Although each study employs a different identification strategy

adapted to its context they are all grounded in the same conceptual logic of using exogenous variation in policy exposure to uncover behavioral responses. This combination of methodological diversity and conceptual consistency strengthens the credibility of the overall findings.

Despite differences in context and data, the empirical chapters share several common methodological features that provide coherence. They apply quasi-experimental techniques such as difference-in-differences, event studies, or regression discontinuity designs. Each chapter places strong emphasis on heterogeneous treatment effects, examining how responses vary by firm size, digital capacity, eligibility status, or other relevant characteristics. Rather than focusing solely on aggregate outcomes, the thesis consistently investigates underlying mechanisms, such as incentive distortions, passive risk-taking, or spillovers, to explain why policy effects differ across agents or over time. Finally, every study uses micro-level data, allowing for high-resolution analysis of behavior and performance.

Beyond methodological rigor, the thesis also benefits from the strength and richness of its data sources. Each empirical chapter draws on large-scale, high-frequency, or administrative datasets that enable precise measurement of outcomes and credible identification of causal effects. The data allow for granular analysis at the firm, bank, or transaction level, making it possible to observe behavioral responses and to study heterogeneity in detail. In several cases, the thesis utilizes unique or previously underexploited data sources, such as administrative tax records, supervisory banking data, and bond issuance and underwriting datasets. The use of panel data makes it possible to track dynamics over time, assess long-term effects, and explore how behavior evolves in response to policy changes.

Each chapter applies a tailored identification strategy suited to its specific policy context while adhering to a shared causal logic. **Chapter 2** leverages a policy threshold and eligibility rule to examine the effects of a simplified tax regime, employing techniques such as Two-Way Fixed Effects (TWFE), Fuzzy Regression Discontinuity Designs (FRD), Propensity Score Matching (PSM), and Inverse Mills Ratio (IMR) corrections to mitigate selection concerns and isolate the causal impact of policy exposure. **Chapter 3** treats the onset of COVID-19 in Hubei Province as a natural experiment, comparing affected and unaffected banks using Difference-in-Differences (DiD), complemented by Propensity Score Matching (PSM) to ensure comparability. It incorporates a digitalization index to examine heterogeneity in institutional capacity.

Chapter 4 exploits the announcement and implementation of the ECB's Corporate Sector Purchase Program as a policy shock and uses event studies and Two-Way Fixed Effects models to estimate the effect on bond underwriting fees and issuance costs. By comparing eligible and ineligible firms, and by exploiting variation in bond characteristics and bank participation, the chapter isolates the pricing and distributional effects of targeted monetary policy.

The methodological design is closely aligned with the conceptual framework of the thesis. The focus on heterogeneous effects requires interaction terms, subgroup analysis, and distributional assessments. The examination of incentives relies on observing behavioral adjustments in response to policy design. The emphasis on policy design quality is operationalized by exploiting natural discontinuities, targeting rules, and institutional variation. The search for unintended consequences involves testing for spillovers, distortionary effects, and long-run dynamics. Finally, the analysis across firms, banks, and markets reflects an understanding of the multi-layer structure of the financial system, ensuring that empirical methods are tailored to each level while remaining conceptually coherent across chapters.

To ensure the robustness of the findings, each empirical study incorporates a range of validation strategies. These include sensitivity tests, alternative model specifications, placebo tests, and falsification exercises to verify the credibility of the results. Where possible, the chapters draw on multiple data sources or triangulate empirical evidence to rule out spurious interpretations. These robustness procedures signal empirical discipline and strengthen confidence in the conclusions. These methodological choices allow the thesis to make both academic and policy contributions, not only by identifying causal effects but by uncovering the mechanisms through which policy design, incentives, and heterogeneity shape outcomes across the financial system. This sets the stage for the next section, which details the academic, policy, and methodological contributions of the thesis.

1.7.1 Difference-in-Differences and Two-Way Fixed Effects

Difference-in-differences (DiD) is one of the most widely used empirical strategies in applied economics for estimating the causal effects of policy interventions. It exploits variation in the timing or presence of treatment across units - e.g. firms, banks, regions and over time, comparing changes in outcomes for treated units before and after treatment to changes for untreated units over the same period. By taking differences over time and differences across groups, DiD aims to eliminate time-invariant unobserved heterogeneity and common time

shocks, isolating the effect of the intervention.

In its simplest form, DiD compares two groups - i.e. treated vs. control, over two periods - i.e. pre- vs. post-treatment periods. The identifying assumption is the parallel trends assumption, specifically, in the absence of treatment, the treated and control groups would have experienced the same change in outcomes over time. When this holds, the DiD estimator recovers the average treatment effect on the treated.

$$\hat{\beta}_{DiD} = (Y_{T,post} - Y_{T,pre}) - (Y_{C,post} - Y_{C,pre}) \quad (\text{Eq.2})$$

However, modern applications of DiD often involve panel data, multiple time periods, and staggered treatment adoption, which require more sophisticated implementations, such as Two-Way Fixed Effects (TWFE). DiD has become a cornerstone of empirical research for several reasons, (1) it mimics experimental logic using observational data, (2) controls for time-invariant unobserved heterogeneity, (3) allows for policy evaluation in real-world, non-random settings, (4) is easy to estimate and interpret in simple settings, and (5) is flexible and can be combined with fixed effects, covariates, instrumental variables, and matching.

As a result, DiD is widely used in corporate finance, labor economics, public finance, health economics, banking, and macro-financial policy evaluation. The key identifying assumption in DiD is parallel trends. This is fundamentally untestable but can be assessed using pre-treatment trends and placebo tests. Violations of the parallel trends assumption can arise from (1) different underlying growth trends, (2) anticipation effects, (3) simultaneous policy changes, or (4) selection into treatment. Thus, researchers typically complement DiD with event-study plots, pre-trend tests, and robustness checks to support the validity of this assumption.

Two-way fixed effects (TWFE) framework extends the classic DiD model by allowing for unit fixed effects, controlling for time-invariant unobserved heterogeneity across units, such as firms, banks, or regions, and time fixed effects controlling for common shocks affecting all units in a given period. This structure makes TWFE especially attractive in panel data settings, where policy interventions are implemented at specific times for specific groups, and researchers seek to isolate the causal impact of these interventions. Formally, the TWFE regressions take the modification of the following form:

$$Y_{i,t} = \beta D_{i,t} + \eta_i + \mu_t + \varepsilon_{i,t} \quad (\text{Eq.3})$$

where η_i are unit fixed effects and μ_t are time fixed effects. Variable $D_{i,t}$ represents a treatment indicator. Under the parallel trends assumption, TWFE identifies the average treatment effect by comparing changes in outcomes for treated units before and after treatment to the changes observed in untreated units over the same period. In settings with a single treatment time and a single treated group, TWFE is equivalent to the canonical DiD estimator and has a straightforward interpretation. However, Imai and Kim (2020) show that the equivalence does not hold under more general applied settings. Lastly, Wooldridge (2021) shows that an extended TWFE is also equivalent to the pooled OLS.

TWFE became the standard approach in empirical research for several reasons. (1) It controls for unobserved heterogeneity across units and time, (2) is computationally simple and well-integrated into standard statistical software, (3) allows for staggered treatment timing - i.e. different units adopt treatment in different years, and (4) can be combined with covariates, clustered standard errors, and event-study specifications.

For many years, TWFE was applied almost universally in policy evaluations, especially when treatments were rolled out over time or across regions. However, in the recent years, the TWFE estimation has seen the criticism by the scientific community. Literature has extensively documented negative weights and existence of negative coefficients when average treatment effects (ATEs) are positive (Borusyak, Jaravel, & Spiess, 2024; Callaway & Sant'Anna, 2021; de Chaisemartin & D'Haultfoeulle, 2018; De Chaisemartin & d'Haultfoeulle, 2020; De Chaisemartin & D'haultfoeulle, 2023; Goodman-Bacon, 2021; Hull, 2018; Sun & Abraham, 2021; Wooldridge, 2021). Moreover, the TWFE has seen strong opposition in staggered settings due to the use of already treated units as controls for later-treated (Callaway & Sant'Anna, 2021; De Chaisemartin & D'haultfoeulle, 2023; Goodman-Bacon, 2021). However, the empirical analysis in Chapter 2, Chapter 3 and Chapter 4 do not face the issue of staggered implementation.

Literature has further addressed the issue of testing for the fundamental assumption of DiD and TWFE models, the parallel trends. The literature argues that the assumption is strong and may not be testable or sufficient. First, parallel pre-trends do not guarantee post-treatment parallel trends in the absence of the treatment (Roth, Sant'Anna, Bilinski, & Poe, 2022). The example for such phenomenon is provided by Kahn-Lang and Lang (2020), comparing height of girls and boys up until the age of 13 with parallel trends before the height diverges. However, it would be erroneous to conclude that bar mitzvahs, that happens at the age of 13 for boys has an impact on height.

Second, even in presence of the parallel trends, the tests may reject the trends due to low power (Bilinski & Hatfield, 2018; Freyaldenhoven, Hansen, & Shapiro, 2019; Kahn-Lang & Lang, 2020; Roth, 2018). Additionally, Bilinski and Hatfield (2018) further argue that pre-trends test reverse the hypothesis testing by assuming the parallel trends exist and "reject" the assumption of there is strong evidence against it.

1.7.2 Fuzzy Regression Discontinuity Design

Regression Discontinuity Design (RDD) is a quasi-experimental method used to estimate causal effects in situations where treatment assignment is determined by whether an observed variable, known as the running variable or forcing variable, crosses a known threshold. In its sharp form, treatment status changes deterministically at the cutoff with every unit on one side receives treatment, and no unit on the other side does. However, in many real-world policy settings, eligibility does not imply automatic treatment, and some units may not comply with the threshold rule. In such cases, the appropriate framework is the Fuzzy Regression Discontinuity Design (FRD).

In an FRD, crossing the threshold changes the probability of treatment rather than guaranteeing it. The threshold serves as an instrumental variable (IV) that induces quasi-random variation in treatment status around the cutoff. As a result, the FRD identifies the Local Average Treatment Effect (LATE) for compliers - i.e. those whose treatment status is affected by the threshold rule. Formally, the FRD estimator can be written as a Wald estimator:

$$FRD = \frac{\lim_{x \downarrow c} E[Y_i | X_i = x] - \lim_{x \uparrow c} E[Y_i | X_i = x]}{\lim_{x \downarrow c} E[D_i | X_i = x] - \lim_{x \uparrow c} E[D_i | X_i = x]} \quad (\text{Eq.4})$$

where X_i is the running variable, c is the cutoff, D_i is treatment status, and Y_i is the outcome. The numerator captures the jump in outcomes at the threshold, while the denominator captures the jump in treatment probability.

FRD is especially useful in empirical policy evaluation because real-world policies rarely enforce thresholds perfectly. For example (1) eligibility does not guarantee participation, (2) some ineligible units still obtain treatment, and (3) administrative discretion or phased rollouts create imperfect assignment. Rather than discarding the threshold altogether, FRD leverages it as a quasi-random instrument to recover causal effects in the presence of imperfect compliance. This allows researchers to still draw credible conclusions from policy environments

where assignment is not strictly deterministic.

FRD relies on similar core assumptions as Sharp RDD, but with an instrumental variables interpretation:

- Continuity of potential outcomes: potential outcomes must evolve smoothly in the running variable at the cutoff, ensuring that any observed discontinuity in outcomes is attributable to the treatment effect.
- First-stage relevance: the threshold must induce a discrete jump in the probability of treatment. Without this discontinuity, the instrument is weak and the FRD cannot identify the LATE.
- Monotonicity: crossing the threshold should not make any unit less likely to receive treatment. In other words, the threshold must weakly increase treatment probability for all units.
- Local exclusion restriction: crossing the threshold must affect the outcome only through its effect on treatment, at least locally around the cutoff.
- No precise manipulation of the running variable: units should not be able to fine-tune their running variable to land just above or below the threshold.

However, as other methodologies, FRD does not come without limitations. The main limitations of FRD include (1) local in nature - the effect applies only to units near the threshold, (2) requires sufficient variation in treatment probability at the cutoff, (3) sensitive to bandwidth choice and functional form, (4) compliance behavior must be well understood, and (5) requires careful testing of manipulation and continuity assumptions. Recent methodological work has improved inference in FRD by proposing robust bias-corrected estimators, optimal bandwidth selection, and formal hypothesis testing (Calonico, Cattaneo, Farrell, Palomba, & Titiunik, 2025; Calonico, Cattaneo, Farrell, & Titiunik, 2019; Calonico, Cattaneo, & Titiunik, 2014).

FRD is particularly relevant in [Chapter 2](#), where firms' eligibility for a simplified tax regime is determined by crossing a revenue threshold. However, eligibility does not guarantee participation, and some firms may self-select in or out of the regime, making a sharp design inappropriate. Instead, the revenue threshold creates a discrete change in the probability of treatment, making FRD the correct methodological tool.

The thesis uses the threshold as an instrument to estimate the local effect of the tax regime

on firm behavior and growth, controlling for heterogeneous responses and potential self-selection. This aligns with the broader methodology of the thesis, which emphasizes causal inference, behavioral mechanisms, and heterogeneity in policy effects.

1.7.3 Propensity Score Matching

Propensity Score Matching (PSM) is a widely used method in observational studies to estimate causal effects when random assignment to treatment is not feasible. Its central goal is to create a comparison group that closely resembles the treated group in observable characteristics, thereby approximating the conditions of a randomized experiment. By matching treated units to similar untreated units based on their propensity score, the probability of receiving treatment given observed covariates, PSM seeks to remove selection bias due to observable differences and isolate the effect of treatment on outcomes.

Formally, the propensity score $p(X_i) = P(D_i = 1|X_i)$ summarizes all observed covariates that predict treatment. If treatment assignment is conditionally independent of potential outcomes given the propensity score, then differences in outcomes between matched treated and control units can be interpreted as causal. PSM is particularly attractive in policy settings where (1) treatment is not randomly assigned, (2) some groups may be more likely to receive the policy than others, (3) rich data on pre-treatment characteristics are available, and (4) there is concern that simple comparisons between treated and untreated groups are biased.

In many real-world contexts, including this thesis, policy eligibility or exposure may depend on factors such as firm size, bank characteristics, or market conditions. PSM helps balance observable characteristics to construct more credible counterfactuals. The PSM further offers unique advantages of (1) intuitive and transparent construction, (2) reduces dimensionality of covariates to a single scalar score, (3) helps control for selection bias due to observable characteristics, and (4) can improve comparability and interpretability of treatment and control groups. The PSM approach is often used as a pre-processing step before DiD, instrumental variable (IV), RDD, or TWFE approach, as in this thesis.

However the PSM also has notable limitations, including (1) only addresses selection on observables - unobserved heterogeneity remains a threat, (2) matching on the propensity score alone may increase imbalance in covariates, (3) PSM discards data, leading to loss of precision, (4) strong dependence on model specification, and (5) no inherent control for time trends or

dynamics. The limitations of the approach are discussed in depth by [King and Nielsen \(2019\)](#) for the abovementioned limitations.

1.7.4 Inverse Mills Ratio

In many empirical settings, the outcome of interest is observed only for a non-random subset of units. If this selection process is correlated with unobserved determinants of the outcome, then simple regression estimates will be biased and inconsistent. This problem, known as sample selection bias, is common in policy evaluation, labor economics, corporate finance, banking, and other applied fields.

For example, firms may self-select into a tax regime based on unobserved characteristics, such as managerial ability, growth expectations, or compliance preferences, which also affect their performance. If researchers only compare firms that choose to participate in the policy to those that do not, without accounting for this selection, they may incorrectly attribute differences in outcomes to the policy itself rather than to pre-existing differences between firms.

The two-step selection correction model provides a widely-used solution to this problem ([Heckman, 1978, 1979](#)). It explicitly models the selection process and introduces the Inverse Mills Ratio (IMR), a term derived from the selection equation's error structure, to correct for the correlation between the unobserved factors affecting selection and the unobserved factors affecting the outcome.

The [Heckman \(1979\)](#) approach consists of two steps:

Step 1: Selection Equation (Probit model)

A binary model, normally probit, estimates the probability that a unit selects into treatment - e.g. participates in the tax regime:

$$D_i^* = Z_{i\gamma} + u_i, \quad D_i = 1 \text{ if } D_i^* > 0 \quad (\text{Eq.5})$$

where Z_i includes variables that influence selection. From this model, we compute the Inverse Mills Ratio (IMR):

$$\lambda_i = \frac{\phi(Z_{i\hat{\gamma}})}{\Phi(Z_{i\hat{\gamma}})} \quad (\text{Eq.6})$$

where ϕ is the standard normal density and Φ is the cumulative distribution function.

Step 2: Outcome Equation

The outcome equation is estimated only for the selected sample, but includes the IMR as an additional regressor:

$$Y_i = X_i\beta + \rho\sigma_u\lambda_i + \varepsilon_i \quad (\text{Eq.7})$$

If the coefficient on the IMR term $\rho\sigma_u$ is statistically significant, it indicates selection bias was present. Including the IMR corrects for bias by capturing the expected value of the error term conditional on selection. Thus, IMR acts as a control function that adjusts the outcome equation for non-random selection.

Key assumptions of the IMR approach include (1) normality and linearity of errors, (2) exclusion restriction, (3) correct specification of the selection model, and (4) selection on unobservables, not just observables. However, the advantages of the model include (1) corrects for non-random sample selection, (2) provides a direct test for selection bias, (3) intuitive two-step procedure, (4) computationally simple and (5) can be combined with other methods.

Lastly, the limitations of the model include (1) strong parametric assumptions, (2) sensitive to misspecification of the selection equation, (3) requires strong instruments or exclusion variables for credibility, and (4) identifies only local average effects under selection assumptions. However, despite these critiques, IMR remains a standard and defensible selection correction technique, especially when supported by robustness checks and theory-based exclusion restrictions.

1.8 Thesis Contributions

While each of the three empirical chapters is designed to stand alone as an independent scholarly contribution, the cumulative structure of the thesis allows it to generate broader insights that extend beyond the scope of any single study. The thesis does more than add evidence to existing discussions, it reshapes how we think about policy interventions in the financial system by placing policy design, incentives, heterogeneity, and behavioral mechanisms at the center of analysis.

This thesis makes contributions at three interconnected levels. First, at the academic level, it advances empirical understanding of how policies operate across firms, banks, and capital markets, revealing common mechanisms that determine whether interventions succeed or fail. It introduces new conceptual distinctions, such as the difference between active and passive risk-taking, and reframes policy effectiveness as a function of design quality, heterogeneous

responses, and micro-level incentives. Second, at the policy level, the thesis offers actionable insights for governments, regulators, and central banks. It identifies design features that unintentionally discourage firm growth, increase banking risk during crises, or distort market pricing, while also demonstrating how more flexible, incentive-aligned policies can improve outcomes. Third, at the methodological level, the thesis applies rigorous quasi-experimental techniques to underexplored settings, uncovers mechanisms rather than simple average effects, and develops a multi-layer empirical framework that can be applied to future policy evaluations across different institutional contexts.

By integrating insights across micro, meso, and macro layers of the financial system, the thesis provides a holistic account of how policy interventions function in practice and why they often produce heterogeneous or unintended consequences. It shows that policies cannot be adequately evaluated solely by their stated objectives or aggregate outcomes, but must be understood through the behaviors they induce, the capacities they assume, and the structural conditions in which they operate. In doing so, the thesis offers a new way of thinking about financial policy, one that emphasizes design, coordination, and incentive alignment across institutional layers.

1.8.1 Academic Contributions

This thesis advances the understanding of financial policy by reframing policy interventions not as binary tools that either succeed or fail, but as mechanisms whose effects depend on their design features, the incentives they create, and the heterogeneous responses of economic agents. Rather than treating policy tools as uniform levers, the thesis emphasizes that thresholds, eligibility criteria, targeting rules, operational requirements, and flexibility are fundamental determinants of outcomes. By making policy design a central analytical variable, the thesis challenges the implicit assumption in much of the literature that the tool itself is more important than the way it is implemented.

A second core conceptual contribution is the elevation of heterogeneity from a secondary robustness check to a primary explanatory factor. The thesis demonstrates that differences in firm size, digital capacity, market eligibility, and other agent characteristics systematically shape how policies are perceived and acted upon. As a result, average treatment effects often conceal the very mechanisms that determine policy effectiveness. Heterogeneity is therefore not noise to be averaged out, but a vital dimension through which policy outcomes must be

understood.

The thesis also introduces a distinction between active and passive risk-taking in the context of banking behavior. Traditional theories of bank risk-taking focus on deliberate, strategic increases in risk in pursuit of higher returns. However, the thesis shows that operational disruptions and capacity constraints can lead to passive risk-taking, where risk exposure rises unintentionally due to an inability to adjust or monitor portfolios. This distinction provides a new lens for understanding risk dynamics in crises and has broad implications for macroprudential policy design.

Importantly, the thesis focuses on a unifying conceptual framework that links corporate finance, banking stability, and capital market interventions. It demonstrates that across all three domains, policy outcomes are driven by the interaction between design, incentives, and heterogeneity, and that unintended consequences arise when these interactions are ignored. This integrated perspective moves beyond domain-specific theories and contributes a system-level understanding of policy effectiveness.

At the level of individual literatures, each empirical chapter contributes novel insights. In corporate finance, the thesis shows that tax policy affects not only short-term compliance or profitability but also dynamic firm growth and long-term expansion decisions. It uncovers how threshold-based regimes create incentive distortions that can trap firms near eligibility limits, enriching the literature on firm behavior under policy constraints.

In banking and macroprudential stability, the thesis reconceptualizes how emergencies affect risk-taking by highlighting the role of operational disruption and digital resilience. It demonstrates that crises may increase passive rather than active risk-taking, and that banks' technological capacity is a key determinant of resilience, an insight largely absent from existing theoretical and empirical work.

In capital market and monetary policy research, the thesis reveals that targeted interventions such as the ECB's Corporate Sector Purchase Program reshape market bond issuance cost structure, not just yield spreads. By focusing on underwriting costs and competitive dynamics among intermediaries, the thesis provides a new understanding of how monetary policy affects market structure and access to finance.

Finally, the thesis contributes to interdisciplinary research by bridging corporate finance,

banking, and monetary economics through a shared analytical lens. It demonstrates that behavioral responses, institutional design, and implementation mechanisms are central to understanding real-world outcomes in all three domains. By integrating perspectives from public finance, macroprudential regulation, and market microstructure, the thesis promotes a more holistic view of the financial system and encourages cross-disciplinary dialogue.

1.8.2 Policy Contributions

This thesis provides concrete insights into how policy design shapes real-world outcomes and highlights the trade-offs that policymakers must navigate. In the context of tax policy, it shows that simplification and administrative efficiency can come at the cost of distorted incentives, particularly when regimes rely on rigid eligibility thresholds. While simplified tax systems are intended to support micro-firms by lowering compliance costs, the evidence demonstrates that threshold-based designs can unintentionally discourage firms from growing, leading some to limit their scale in order to retain preferential treatment. This finding underscores a broader principle, policy effectiveness depends not only on the choice of instrument but on its specific design features, including thresholds, targeting mechanisms, and operational requirements. The thesis therefore urges policymakers to move beyond viewing policy tools in abstract terms and to pay close attention to the details of implementation and the incentives they create.

In the banking context, the thesis challenges prevailing assumptions about how crises impact financial stability and offers new guidance for macroprudential policy. It shows that public emergencies do not merely encourage deliberate, strategic risk-taking, but can also generate passive risk-taking driven by operational disruptions, capacity constraints, and reduced monitoring ability. A key contribution of the thesis is demonstrating that digital transformation enhances operational resilience, enabling banks to maintain credit supply and contain risk during crises. This suggests that digital readiness should be viewed as a macroprudential instrument, on par with capital buffers or liquidity requirements. By documenting that digital infrastructure preserves stability without sacrificing credit provision, the thesis provides a compelling case for integrating technological capacity into prudential regulation and crisis preparedness frameworks.

The thesis also contributes to the design and assessment of monetary policy by revealing the distributional and structural consequences of targeted interventions. Using the ECB's Corporate Sector Purchase Program as a case study, it shows that while such policies can lower

financing costs for eligible firms, they also generate spillover effects that raise underwriting costs for ineligible firms. This redistributes market advantages and can lead to distortions in competition and pricing mechanisms. By shifting bargaining power toward eligible firms and specific intermediaries, targeted interventions may affect market access, equity, and long-term efficiency. The thesis argues that central banks must therefore monitor distributional consequences, not just aggregate effects. It also raises concerns about long-term dependency, as repeated reliance on monetary support may weaken the incentives for private market development and reduce market discipline. These insights encourage a more nuanced and fairness-oriented approach to monetary policy evaluation.

Finally, the thesis offers broader guidance on how to design policies that are effective at the system level. It demonstrates that tax, banking, and monetary policies do not operate in isolation, instead, they interact through firm behavior, credit intermediation, and market pricing. Ignoring these interactions can lead to contradictory incentives or amplify systemic risk. The thesis argues for greater coordination across policy domains, ensuring that interventions at different layers of the financial system are aligned rather than conflicting. It recommends designing policies that are flexible, adaptive, and incentive-compatible, allowing them to respond to heterogeneity in agent behavior and changes in economic conditions over time. Moreover, it advocates for holistic evaluation frameworks that assess policies based on mechanisms, distributional consequences, and long-term effects, rather than focusing narrowly on tool-specific outcomes.

1.8.3 Methodological Contributions

This thesis further makes a significant methodological contribution by applying rigorous quasi-experimental methods to policy contexts where causal identification has traditionally been difficult. Each empirical chapter exploits a natural experiment or policy-induced source of exogenous variation. These events provide credible identification strategies such as difference-in-differences, event studies, and discontinuity-based approaches. In doing so, the thesis generates causal estimates in settings where much of the prior literature has relied on descriptive correlations or aggregate trends, thereby offering stronger and more policy-relevant evidence.

A further methodological advance of this thesis is its explicit focus on uncovering mechanisms rather than merely estimating average effects. Instead of asking only whether a policy “worked,” each chapter examines how and why policy effects emerge or fail to materialize.

The thesis traces behavioral channels, such as incentives created by threshold-based tax design, the distinction between active and passive risk-taking in banking, and spillover effects in capital markets. It rigorously analyzes heterogeneous treatment effects, recognizing that variation in responses across firms, banks, and market participants provides key insight into underlying mechanisms.

The thesis also contributes methodologically through the use of rich, granular datasets that enable precise identification of behavioral responses. It draws on micro-level data at the firm level, the bank level, and the transaction level. In several cases, the thesis leverages unique administrative or supervisory data, or novel combinations of data sources, allowing for more detailed analysis than is typically possible. Importantly, the empirical strategies track dynamic and long-term outcomes, rather than limiting analysis to short-term effects. This temporal dimension provides deeper insight into policy persistence, adaptation, and unintended consequences over time.

1.9 Clarification of Scope and Boundaries

This thesis focuses on three major types of financial policy interventions, each situated at a different level of the financial system, specifically (1) firm-level taxation policies, using the introduction of a simplified gross receipts tax (GRT) regime in Slovenia as a case of how tax design shapes firm incentives and growth, (2) bank-level macroprudential and operational resilience policies, using the onset of the COVID-19 pandemic in Hubei Province to study how emergencies influence bank risk-taking and the role of digital transformation, and (3) market-level monetary interventions, using the European Central Bank's Corporate Sector Purchase Program (CSPP) to examine how targeted asset purchases affect bond underwriting costs and capital market access.

The empirical focus of the thesis is on real-world policy interventions analyzed through micro-level data, allowing for credible identification of causal effects and detailed examination of heterogeneous responses. The selected geographical contexts are not arbitrary. Each represents a typical and policy-relevant case of widely used tools of simplified tax regimes for small firms exist in many countries, the COVID-19 pandemic provides a natural experiment in macroprudential stress and CSPP is one of the most prominent examples of unconventional monetary policy globally. The time periods studied align with major policy events or shocks,

the introduction of the GRT in 2013, the COVID-19 crisis in 2020, and the launch of CSPP in 2016.

Across these contexts, the thesis applies a consistent analytical lens. This framework enables comparison across institutional layers and highlights common mechanisms. The selection of cases is therefore purposeful rather than incidental as each chapter examines a representative policy tool widely used around the world, allowing the thesis to generate insights that are both contextually grounded and broadly generalizable. To maintain analytical depth and empirical credibility, the thesis clearly defines its boundaries and does not attempt to cover every possible avenue of inquiry. First, it does not develop new formal theoretical or structural macroeconomic models. Instead, it builds conceptual frameworks informed by theory and tests them empirically. This is a deliberate choice for understanding how policy design interacts with real-world heterogeneity requires data-driven analysis rather than abstract modeling.

Second, the thesis does not analyze every possible tax, banking, or monetary policy instrument. Rather than cataloging policies, it focuses on representative interventions that capture core mechanisms of design, incentives, and behavior. The goal is to generalize mechanisms, not to exhaust all policy types. Third, it does not model micro-level psychological or behavioral foundations in detail. Behavioral responses are inferred from observed outcomes in large-scale data, not from surveys or laboratory experiments. This choice reflects both the empirical orientation of the thesis and the policy relevance of revealed preferences. Fourth, the thesis does not build a full general equilibrium model or attempt complete systemic simulation. Instead, it offers layer-specific causal evidence and integrates the findings conceptually across the financial system. This approach prioritizes mechanism identification, external validity, and policy relevance over abstract generality.

Each empirical chapter addresses a specific policy in a specific institutional setting, and the thesis is explicit about the boundaries and generalizability of each analysis. The analysis in [Chapter 2](#) is limited to this specific instrument and does not attempt to evaluate all tax policies or macro-fiscal effects. Furthermore, due to data limitations, the chapter does not analyze human capital investments or R&D decisions, and must acknowledge the absence of true randomization and the possibility of self-selection around eligibility thresholds. Nevertheless, the mechanism it uncovers, how thresholds distort incentives, is generalizable to simplified tax schemes used in many countries.

The analysis in [Chapter 3](#) does not evaluate capital regulation under normal economic conditions, nor does it assess all forms of bank regulation. Instead, it isolates the role of operational capacity and technological readiness during crises. The findings regarding passive risk-taking and digital readiness are highly relevant to macroprudential policy design and are applicable across different crises and jurisdictions. The [Chapter 4](#) does not study all forms of quantitative easing or monetary policy, nor does it cover global markets beyond the Eurozone. It also relies on a limited sample of bond issuances, which constrains the breadth of the analysis. However, the mechanisms it uncovers, spillover effects, distributional consequences, and long-term dependencies on policy support, are fundamental features of targeted interventions and thus generalizable to similar programs implemented by central banks worldwide.

The thesis relies on observational data, as is typical in policy evaluation, but addresses the inherent limitations of non-experimental settings through the use of credible quasi-experimental research designs. Each empirical chapter exploits plausibly exogenous variation generated by real-world policy changes or shocks to strengthen causal identification. While the empirical contexts are geographically specific, the mechanisms uncovered are generalizable to broader settings where similar policy instruments are used. Robustness checks, alternative specifications, and careful consideration of identification assumptions are employed to mitigate concerns about bias or confounding factors. The thesis openly acknowledges that external validity is inherently bounded by context, but argues that mechanism-based generalization is a more meaningful and transferable form of inference than purely context-free estimates. This transparent treatment of data limitations anticipates potential criticism and demonstrates methodological rigor.

The thesis also clearly defines the conceptual scope of its contribution. The analytical framework emphasizes policy design quality, incentive structures, heterogeneity in responses, behavioral mechanisms, and unintended consequences as the primary lenses through which policy outcomes are assessed. While the thesis examines policy interventions at the firm, banking, and market levels, it does not claim a direct causal linkage between micro-, meso-, and macro-level dynamics in a structural sense. Rather, it studies each layer independently while demonstrating that similar mechanisms emerge across institutional contexts. The aim is not to propose a single universal policy model or an optimal design formula. Instead, the thesis argues for flexibility, coordination, and incentive alignment as guiding principles, recognizing that effective policy must adapt to context-specific features and agent heterogeneity. The

thesis does not attempt to reduce financial policy to one unified theory, rather, it synthesizes insights across domains to reveal common patterns and conceptual continuity. This approach balances intellectual ambition with humility and ensures conceptual clarity.

The empirical analyses in this thesis use micro-level units of observation. These micro-level data allow the thesis to examine individual behavioral responses, institutional resilience, and market-level pricing dynamics with precision. The levels of inference correspond to the structure of the financial system. However, the thesis does not claim to produce full macroeconomic forecasts or general equilibrium models. Rather, it argues that understanding how incentives and heterogeneity influence behavior at these different levels provides critical insight into how policy interventions shape systemic outcomes. This approach preserves analytical clarity while avoiding overstated claims.

Finally, the thesis clarifies the boundaries imposed by its cumulative structure. Each empirical chapter is written in the style of an independent journal article, with its own introduction, literature review, methodology, and results. As a result, some degree of repetition across chapters is both expected and necessary. The thesis does not fully rewrite each chapter to create a single continuous narrative, as doing so would compromise the integrity of the standalone empirical contributions. Instead, the introduction and conclusion chapters serve to integrate the individual studies, highlight their conceptual connections, and articulate the overarching contribution. This structure allows for deep, context-specific empirical analysis in each chapter while preserving a coherent and unified research agenda. By being explicit about the cumulative format and its limitations, the thesis demonstrates awareness of genre constraints and reinforces the rationale for its approach.

Although this thesis has strong policy relevance, it is important to clearly delimit the scope of its normative implications. The thesis does not prescribe detailed policy blueprints for specific countries or institutions. Instead, it offers general principles and mechanisms that policymakers should consider when designing and evaluating interventions. These principles emphasize the importance of policy design quality, incentive alignment, heterogeneity in responses, operational capacity, and coordination across institutional layers. The thesis stresses that policy effectiveness depends on contextual adaptation, what works in one country or market may require modification in another due to institutional differences, governance structures, or economic conditions. By focusing on mechanisms rather than prescriptions, the

thesis avoids over-generalization and maintains the credibility and relevance of its policy contributions.

The boundaries outlined in this section are not limitations always imposed by necessity, but choices made to ensure analytical depth, empirical credibility, and conceptual clarity. Rather than attempting to cover all financial policies or model the entire financial system within a single framework, the thesis adopts a focused, multi-level approach that enables rigorous causal identification and detailed exploration of mechanisms. By concentrating on representative interventions and applying a consistent analytical lens across firms, banks, and markets, the thesis remains coherent and theoretically grounded while still producing findings that are generalizable through mechanisms. The cumulative structure supports in-depth empirical analysis within each context and conceptual integration across chapters, balancing specialization with synthesis. These boundaries ensure that the thesis is not only academically rigorous but also methodologically transparent and policy-relevant.

1.10 Thesis Structure

The structure of the this thesis follows from the broader introduction of the core overarching theme, to the introduction of the leading concepts and extended through empirical works presented in subsequent chapters.

Chapter 1 serves as the introduction and establishes the foundation for the entire thesis. It presents the motivation for examining policy interventions in taxation, banking, and capital markets, and articulates the unifying theme that connects the three empirical studies. It defines the core concepts, such as policy design, incentives, heterogeneity, behavioral mechanisms, passive versus active risk-taking, and unintended consequences, and identifies the high-level theoretical and empirical gaps in the literature. **Chapter 1** formulates both the overarching and chapter-specific research questions, outlines the methodological approach, clarifies the contributions of the thesis, and sets the scope and boundaries. In the absence of a separate literature review chapter, **Chapter 1** also synthesizes relevant theoretical and empirical literatures across corporate finance, macroprudential policy, and monetary interventions, providing a conceptual framework that ties together the analyses at the firm, bank, and market levels.

Chapter 2 presents the first empirical study, focusing on corporate finance and taxation

policy. It examines the impact of a simplified gross receipts tax regime on micro-firms in Slovenia, using a quasi-experimental design to estimate causal effects. The chapter analyzes both average treatment effects and heterogeneous responses, with particular attention to how threshold-based policy design shapes long-term growth incentives. It reveals that while the regime reduces compliance burdens for very small firms, it simultaneously creates disincentives for firms near the eligibility threshold to expand. This chapter contributes to the literature on firm dynamics and tax policy design and illustrates how policy design, incentives, and heterogeneity interact to produce both intended and unintended outcomes, directly reinforcing the overarching theme of the thesis.

Chapter 3 contains the second empirical study, which investigates banking stability and macroprudential policy in the context of the COVID-19 crisis in Hubei Province. The chapter introduces a novel conceptual distinction between active and passive risk-taking and asks how public emergencies affect these different forms of risk. It shows that crises primarily increase passive risk-taking driven by operational disruptions rather than deliberate strategic behavior. The analysis further demonstrates that digital transformation enhances bank resilience by preserving credit supply and limiting risk escalation, suggesting that digital readiness should be considered a macroprudential tool. By highlighting the role of operational capacity and heterogeneous institutional responses, this chapter contributes to banking and financial stability research and reinforces the importance of policy design, incentives, and institutional capability within the thesis's broader framework.

Chapter 4 presents the third empirical study, examining capital market policy and monetary intervention through the lens of the European Central Bank's Corporate Sector Purchase Program. Using micro-level bond issuance data and policy-induced variation, it evaluates how targeted monetary interventions affect bond underwriting costs. The chapter shows that while eligible firms benefit from lower financing costs, ineligible firms face higher underwriting fees due to spillover effects. This finding reveals that monetary policy can reshape market pricing mechanisms, alter competitive dynamics, and generate distributional consequences beyond its direct targets. The chapter also raises concerns about long-term dependency on policy support. By highlighting how design, incentives, spillovers, and heterogeneity shape market outcomes, this chapter directly connects to the thesis's central conceptual pillars.

Chapter 5 concludes the thesis by synthesizing the findings across all three empirical chapters. It compares insights from the firm, bank, and market levels, demonstrating that common

mechanisms, such as policy design quality, agent heterogeneity, incentive alignment or misalignment, behavioral responses, and unintended consequences, consistently determine policy effectiveness across institutional contexts. The conclusion derives broader principles for designing flexible, incentive-compatible, and well-coordinated policies that align micro-level behavior with macro-level objectives. It discusses implications for economic growth, financial stability, and equitable access to finance, and proposes directions for future research. In doing so, **Chapter 5** reintegrates the thesis as a whole and demonstrates its contribution to understanding financial policy at both the academic and practical levels. The next chapter begins with the first empirical analysis of taxation policy and firm behavior, initiating the detailed investigation of how policy interventions function across the financial system.

Chapter 2

The Impact of Gross Receipt Taxation on Micro-Sized Firms

Kristjan Markovc

This paper examines the effects of a gross receipt taxation (GRT) regime introduced in 2013 to simplify tax compliance for micro-firms. Using administrative firm-level data and a comprehensive empirical strategy, including interaction models, fuzzy regression discontinuity, two-way fixed effects, Heckman selection correction, propensity score matching, and heterogeneity analysis, the paper evaluates how GRT affected performance and firm growth. The paper finds that firms self-select into GRT based on the joint distribution of gross receipts and profit margins, consistent with a theoretical model of profit-maximizing behavior. Contrary to a mechanical response at the eligibility threshold, no discontinuity in entry or outcomes is detected, indicating that eligibility alone did not drive participation. Average treatment effects are small or misleading due to strong heterogeneity, with very small firms benefiting from GRT through reduced administrative costs and higher growth, while larger firms near the eligibility limit experienced lower performance and long-run growth. The findings imply that simplified tax regimes can successfully support micro-enterprise entry but may unintentionally discourage scaling if rigidly designed.

JEL Classification — G30, G38

Keywords — Gross receipt taxation; tax compliance; entrepreneurship; micro-sized firms; sole proprietors; tax policy reform

2.1 Introduction

What is the impact of gross receipt taxation (GRT) on the firm's financial performance? The presence of the taxation introduces the market inefficiencies and distortions, with the firms facing additional financial burden due to taxation. The modern taxation is founded on the principal of taxing the firms on their bottom-line with the taxation on profits. The approach ensures the internalization of the firm characteristics with a universally applicable taxation across all industries and firm types. Moreover, the profit-based taxation prevents any further losses of the business due to bad financial performance.

With the taxation being a dead-weight loss, firms are incentivized to reduce the tax basis and consequently minimize the tax payments. The reduction in tax basis is often driven by firms' investments in future operations, with the taxes playing a positive behavior-reinforcing role of disincentivizing firms from holding excessive profits and cash. The taxation of micro-sized firms greatly differs in its application and dynamics. While the taxation scheme is identical to the scheme for the larger corporations, the micro-sized firms operate with different relative sizes of fixed expenses and access to various financial tools and knowledge. Additionally, the taxation requires the firms to comply with tax code and all legal obligations, which are expensive and burdensome to SMEs. However, with the introduction of the form of gross receipt taxation (GRT) in Slovenia, the natural persons - i.e. sole proprietors, with GRT no longer need to submit annual financial reports. The removal of the requirements greatly simplified the administrative processes for natural persons and potentially reduced administrative expenses.

The construction of GRT requires the firms to pay taxes on the gross receipts rather than on profits. This raises an issue and questions the validity of the taxation when the firms are operating with financial losses. By construction, the firms are obligated to pay the taxes, even in the economic downturns with the businesses experiencing financial losses. The firms that earn negative profits due to poor financial performance, are still obligated to pay the taxes on gross receipts. In absence of the GRT, firms would declare financial losses and be waved of taxes, however, the GRT nonetheless obliges firms to pay the obligatory taxes. Consequently, the firms face higher risks of doing business and may lead to precautionary behavior.

The SMEs, more specifically micro-sized firms, are most commonly family owned businesses with the entrepreneurs being self-employed or in the case of sufficiently small businesses, with no formal employees. The personal involvement of entrepreneurs implies the firms will exhibit the same behavior that the entrepreneurs themselves exhibit. In accordance with the microeconomic theory assumptions, the consumers are inherently risk averse. The risk aversion has a multitude of impacts from the avoidance of risky investments to the importance of the welfare maximization. In the context of the GRT, the firms must bear the risk of possible economic shocks, whereas profit-based taxation, the risk is partly

borne by the beneficiary of the taxation - i.e. the government. The GRT exhibits the same underlying characteristics to that of the franchises with the fees being based on the gross receipt.

The gross receipt based taxation exposes the firms to increased levels of risk. The firms hold the obligation to pay set rate of gross receipts in form of taxes, implying any positive shocks to demand or cost of doing business translates into increased profitability of the firm. However, the opposite can be said for the negative shocks. During the economic shocks, the firms will continue being obligated to pay set rate of gross receipts in taxes, even though the firms may not operate with operational profits. The presence of the risk suggests that rational entrepreneurs would evaluate the gains during the economic growth to the losses during the downfall and only choose to use the GRT if the gains are larger than the losses.

With the entire risk being borne by the businesses, the firms can earn higher profits and experience improved performance in the times of economic growth. However, in contrast to the periods of economic growth, the firms will also face higher risk of financial losses and consequently should run a higher probability of default due to potentially elevated losses on the account of GRT. The latter therefore introduces high risk, high reward environment and increased risk status to the investors and banks. However, the eligibility criteria set forth, limits the taxation system to only the smallest firms. The dataset obtained, and discussed in later sections, comprises almost exclusively of sole proprietors and very limited number of limited liability corporations (LLCs).

The paper considers sole proprietors as firms, despite their individual ownership structure, as they engage in entrepreneurial activities that mirror the economic functions of LLCs. Sole proprietors organize resources, undertake production, and assume risks in a manner comparable to incorporated entities. Their operations contribute to economic output, generate employment, and influence market dynamics, which makes them integral components of the broader business ecosystem.

The research questions are difficult to answer due to the limited data available with the Slovenian data offering a unique opportunity. The paper utilizes the uniqueness of Slovenian data to empirically evaluate the impact of the GRT and the role of risk in entrepreneurship. The Slovenian data offers a unique insight in the GRT, however faces numerous empirical challenges. The paper attempts to address the empirical challenges by utilizing the fuzzy regression discontinuity design (FRD) as primary causal analysis, extended to the two-way fixed effect (TWFE) analysis, with the latter further extended using inverse Mills ratio (IMR) and propensity score matching (PSM) for endogeneity control.

Literature Review The increased exposure to the risk associated with the GRT would lead to an increased precautionary behavior by the firms due to higher the probability of default in the case of a negative economic shock. The risk averse nature would amplify the behavior and likely lead to a more conservative investments and adaptation of the profitable opportunities. The notion of the inability

and unwillingness of SMEs to adopt profitable opportunities was explored by Gertler, Higgins, Malmendier, and Ojeda (2022), showing the SME's lack of trust in other companies, limited memory and the overconfidence in memory explain the failure. However, such characteristic are also exhibited by the consumers, blurring the distinction between the behavior of the consumers and the SMEs.

The effects of the corporate taxation were explored by Harberger (1962) with a theoretical framework by proposing a general equilibrium model. Harberger (1962) concluded that in the long-run the market moves towards the equilibrium and the after-tax return of the corporations would equalize. The results imply that the taxation of the profits would lower the rate of return and the investments. The model was criticized by Feldstein (2008), by stating the personal income and corporate taxes should be evaluated separately. The issues associated with a corporate taxation were further raised by Fox and Luna (2002) with the evidence that the effective tax rate fell by one third.

The empirical challenges faced in the paper were addressed and tested by Sufi (2009), as well as by Masso, Meriküll, and Vahter (2011). The lack of anticipatory effects by the passing of the law has been used by Masso et al. (2011) studying a tax reform in Estonia. However, the studied reform fundamentally differs with the Estonian reform changing the taxation from retained earnings to distributed earnings. In the economic theory, taxation is seen as distortionary and can lead to a decrease in innovation and investment (Auerbach, Aaron, & Hall, 1983; Hall & Jorgenson, 1967; Hayashi, 1982; Summers, Bosworth, Tobin, & White, 1981).

The literature thus far has provided ample evidence for the distortionary nature of the taxation with a depression in capital accumulation (Cummins, Hassett, & Hubbard, 1996) and firm growth rates (Carroll, Holtz-Eakin, Rider, & Rosen, 2001). The evidence implies the beneficial tax environment created in Slovenia should have positive impacts on the firms. Moreover, the literature has further shown negative impacts on the entrepreneurial activity due to higher taxation (Gentry & Hubbard, 2000) and consequently GDP growth (Romer & Romer, 2010).

The negative and distortionary nature of the taxation can be offset with a government spending aimed at improving the social welfare. This trade-off between positive and negative impacts of the taxation were proposed by Barro (1990) and further developed by Aghion, Akcigit, Cagé, and Kerr (2016). The negative impacts may outweigh the positive impacts depending on the economic characteristics of a country. As found by Funke et al. (2001) and Funke and Strulik (2003), the tax reform in Estonia decreased the short-term welfare due to a decrease in consumption. The reductions in fiscal revenues were anticipated by the government and directly proposed and acknowledged by the government in the proposal for changing the legislation in 2012 (*Act Amending the Personal Income Tax Act, 2012*). Consequently, the government reaction to the fiscal revenue changes lead to no impact on the firms until more recently with no implications to the research.

GRTs are widely used in developing countries due to their ease of administration and their perceived resistance to tax evasion. [Best et al. \(2013\)](#) argues that while GRTs introduce inefficiencies in production, they effectively reduce tax evasion, which policymakers often prioritize over economic efficiency ([Best et al., 2013](#)). However, [Simbyakula and Hapompwe \(2024\)](#) demonstrates that GRTs negatively affect the profitability of small and medium enterprises (SMEs) in the clothing retail industry, suggesting that a more simplified tax administration system would facilitate compliance and reduce financial strain on businesses.

[Xing et al. \(2024\)](#) provides empirical evidence on the impact of moving from GRT to a VAT, showing that treated service firms experienced an 11.8% increase in sales relative to the control group. Furthermore, the study found significant growth in R&D investment (9%) and employment (5.4%) post-reform, likely due to an increase in outsourcing by downstream manufacturing firms. A notable 22% of the employment gains in service firms were attributed to this shift in outsourcing. The research concludes that GRTs cause production distortions due to tax cascading, whereas transitioning to VAT improves economic efficiency and innovation quality ([Xing et al., 2024](#)).

GRTs are often criticized for their compounding effects on business costs. [Chamberlain and Fleenor \(2007\)](#) highlights that GRT results in harmful “tax pyramiding,” where businesses are taxed at every stage of production, leading to inefficiencies and structural distortions in firms. Unlike well-designed sales taxes, which apply only to final consumer purchases, GRT taxes all transactions, including intermediate purchases, amplifying the tax burden ([Chamberlain & Fleenor, 2007](#)).

[Lawson \(2018\)](#) extends this argument, noting that GRT imposes a heavier tax burden on goods that involve multiple production stages, thereby incentivizing firms to minimize inter-firm transactions and reduce vertical disintegration. This effect disproportionately harms businesses with low profit margins, such as grocery stores, where taxation on gross receipts rather than profit significantly increases their tax burden ([Lawson, 2018](#)).

Tax policies also influence firms’ structural decisions, particularly concerning vertical integration. [Hansen et al. \(2021\)](#) finds that the short-run elasticity of vertical integration in response to intermediate good net-of-tax rate changes is -0.15, with the long-run effect being more than twice as large. This indicates that firms respond strongly to tax policies by restructuring their production processes. Notably, the elimination of GRT has been shown to increase production by 23%, suggesting that such taxes suppress output and reduce market efficiency ([Hansen et al., 2021](#)). The study also notes that GRT disproportionately harms smaller firms that lack the capacity for intermediate production, thus reinforcing market dominance by larger vertically integrated firms ([Hansen et al., 2021](#)).

The tax reform in Slovenia was in part motivated to ensure administrative easing to the entrepreneurs

and consequently allow for the entrepreneurs to focus on doing business. As brought forth by Dabla-Norris, Misch, Cleary, and Khwaja (2017), tax compliance can be costly for the firms. The results found by Dabla-Norris et al. (2017) shows significant positive impacts on productivity of SMEs due to administrative easing. The impact must be considered with the tax reform in Slovenia with the firms using GRT facing minimal tax compliance obligations.

The existing literature suggests that GRTs introduce significant inefficiencies in production and negatively impact business growth, particularly for SMEs. While GRTs reduce evasion, they hinder profitability, and their transition to VAT has been shown to enhance investment, employment, and innovation. Similarly, GRT creates distortions by encouraging vertical integration, increasing tax burdens on low-margin businesses, and ultimately suppressing economic output. Policymakers should consider these effects when designing tax policies to promote economic efficiency and equitable business growth.

The paper is structured as follows, in Section 2.2, the paper discusses the background, legal and economic environments relating to the pseudo-natural experiment in Slovenia, and concludes the section with the tax system trade-off discussion. In Section 2.3, the paper provides a sample model to illustrate the theoretical background. In Section 2.4, the paper describes the data, its sources and the identification challenges and strategy. In Section 2.5, the empirical approach the econometric specification are described and paper presents the results, with the discussion and conclusion in Section 2.6 and Section 2.7, respectively.

2.2 Background and Motivation

The GRT system explored in the paper sets a fixed rate of expenses and consequently a fixed profitability of the firms as a tax basis, upon which the government levies the taxes. Consequently, looking at the GRT from the applied side, the firms are faced with a fixed tax rate on all sources of income, also known as “gross receipts”. The early forms of GRT existed in Europe since the 13th century, and played an important role in France and Germany in early 20th century. The GRT taxes saw a decline in popularity in later years with the introduction of the Value-Added Taxation (VAT). In the United States, the GRT saw a similar decline in the 1970s with the courts ruling them unconstitutional. However, since mid-2000s, the GRT taxation saw a resurgence in popularity in select US states due to the attractive characteristic of tax revenue stability. Moreover, the recent introduction of GRT is not limited to the US, with the emergence of the GRT also in Belgium, limited to diamond traders known as the “*Carat Tax*”, Italy, known as “*Regime Forfetario*”, as well as a number of developing countries.

The GRT taxation saw a decline in popularity due to numerous disadvantageous characteristics

that primarily concern the businesses and the consumers. However, the GRT offer unique advantages of simplicity in calculating and collecting taxes, as well as prove as a reliable source of income to the tax authorities. The former two advantages have been cited in support of GRT in Slovenia, however, the optional choice of GRT in Slovenia provides an additional advantage of possible tax reductions for micro-sized firms.

However, the GRT has seen ample arguments against its implementation with the criticisms regarding the compounding effects of taxation known as the tax pyramiding and unfairness in taxation across firms and industries. The former issue of tax pyramiding leads to large price increases in goods with larger number of intermediaries due to taxation on each individual intermediary, incentivizing firms to vertically integrate to achieve lower prices. However, the latter introduces the complexity of implementing the taxation as each sector is characterized by a different set of profitability structure, leading to the need for a different tax rate for each individual sector.

2.2.1 Gross Receipt Taxation Legislation

In 2004, Slovenia joined the European Union and introduced the Personal Income Tax Act (“PIT Act”) with the introduction of standardized expenses based on the gross receipts (*Personal Income Tax Act, 2006*). First major changes to the PIT Act were passed by the parliament on November 29th, 2012 and announced by the President on December 7th, 2012 (*Act Amending the Personal Income Tax Act, 2012*). With the purpose of providing legal security to the firms and providing administrative relief, the corporate tax rate was set at 20%, while setting the eligibility threshold to enter GRT regime to €50,000 in annual gross receipts with 70% of gross receipts being set as expenses, thus introducing 6% tax on gross receipts. The tax regime was opened to all businesses, regardless of the sector. The changes were further amended on June 20th, 2014 and announced by the President on June 30th, 2014 (*Act Amending the Personal Income Tax Act, 2014*) with the introduction of the eligibility threshold of €100,000 in annual gross receipts for firms with at least one full-time employee for at least five consecutive months with paid social contributions. Lastly, the changes to PIT Act in 2014 introduced the increase to standardized expense rate from previous 70% to 80%; therefore, introducing the effective tax rate of 4% on gross receipts¹.

Contrary to the 2014 change, the changes in 2012 were passed abruptly with the effective date being close to the announcement, prevented the firms from engaging in financial manipulation with the intention of benefiting from the newly announced GRT.

The GRT is available to all firms across all sectors with the participation not being obligatory. The

¹The GRT replaces profit-based taxation and does not exempt sole proprietors whose primary economic activity is the sole proprietorship, from the social security payments.

2004	In 2004, the PIT Act was introduced and the GRT was established at 25% expense rate for a limited set of eligible businesses with gross receipts below €25,037
2005	The GRT was expanded for the agricultural and supportive activities with the expense rate increased to 70% to eligible businesses
2009	The GRT was expanded to include small energy and electricity producers at a 70% expense rate and match the conditions available to other eligible businesses
2010	The Chamber of Craft and Small Business of Slovenia conducted a survey of 94,900 businesses and only 50% were eligible for entering the GRT
2013	The GRT was made available to all firms across all sectors at 70% expense rate while also increasing the eligibility criteria to €50,000 in gross receipts
2014	The eligibility threshold of €100,000 was added for firms with employees and the expense rate was increased to 80% of the total gross receipts

Table 2.1: Timeline of the GRT legislation and its development

firms endogenously choose whether to enter the GRT, subject to eligibility criteria for each fiscal year. However, the firms are obligated to inform the Financial Administration of Republic of Slovenia of the changes to the tax regime used no later than with the submission of the tax fillings for the prior year. However, active firms cannot transition between tax regime during the fiscal year. Lastly, the firms that are newly registered can choose the taxation scheme during the registration, implying the firms who wish to change taxation during the fiscal year can only do so by dissolving the old firm and registering a new firm.

The implementation of GRT taxation in Slovenian in 2013 is not the only implementation of GRT taxation around the globe. With numerous countries, such as Belgium and most importantly Italy, also offering similar types of tax regimes, it is important to discuss the generalizability of the environment studied in Slovenia given the uniqueness of each tax scheme across countries. The core theoretical and behavioral trade-offs firm experience remains constant regardless of the wider implementation details. In [Section 2.3](#), the core theoretical behavior and dynamics are formally demonstrated and any deviation in terms of the size of threshold, if one, or effective tax rate merely affects the position or location of trade-offs and does not affect what the decision really are. Specifically, the removal of eligibility threshold simply implies the firms will always act as do the firms who under optimal behavior remain constant in Slovenia. Similarly, an increase in effective tax rate from 4% in Slovenia in 2013, to some higher value, simply shifts the GRT participation towards none, but the core mechanics remain the same. Therefore, while the Slovenian implementation of the tax regime may offer unique characteristics of the implementation, the studied core dynamics and behavior are persistent across all GRT implementations for micro-sized firms and can be widely generalized.

2.2.2 GRT Tradeoffs

The primary aim of GRT was to simplify compliance for micro-sized firms by replacing profit-based taxation with a gross receipt taxation scheme. The reform reduced documentation and accounting requirements, lowering administrative costs and enabling entrepreneurs to focus on business operations. The predetermined expense rate also increased fiscal predictability by fixing profitability for tax purposes, reducing uncertainty and eliminating incentives for expense manipulation. These features particularly benefited small and inexperienced entrepreneurs by lowering entry barriers and the need for professional accounting services. Additionally, the standardized structure created implicit financial incentives. Firms in low-expense, high-profit sectors, especially services, could reduce their effective tax burden, as the fixed profitability rate often fell below their actual profitability. Such firms benefited from greater retained earnings, which could foster reinvestment and business growth.

However, GRT also imposes rigidities and risks. Due to taxation being levied on gross receipts regardless of realized profits, firms are unable to adjust their tax liability in periods of loss or adverse shocks. Unlike the standard profit-based system, GRT offers no relief for losses, effectively transferring entirety of the financial burden from the state to firms. This design stabilizes public revenues but increases firms' exposure to volatility, particularly in cost-sensitive sectors such as manufacturing. Firms facing unexpected expense shocks may need to incur debt to meet tax obligations or, in extreme cases, suspend operations to avoid further losses. Lastly, the lack of progressive taxation under GRT introduces significant barriers to growth upon reaching financial cap. In essence, GRT offers a clear trade-off as it enhances administrative efficiency, transparency, and fiscal stability, but at the expense of financial flexibility and risk resilience for firms. The regime is most advantageous in stable, high-margin environments, while firms subject to volatile costs may find the system disproportionately burdensome.

2.3 Theoretical Setting

To formally analyze the behavioral responses of firms to the introduction of the GRT regime, this section develops a simple theoretical framework². The model illustrates how firms make participation and entry decisions under different taxation schemes, highlighting the role of expected profitability, risk, and policy-induced constraints. By comparing the outcomes under GRT and profit-based taxation, the framework clarifies the trade-offs between administrative simplicity and financial flexibility. The

²The model aims to formally model the behavior under gross receipt taxation regime. However, due to the nature of this paper, the model does not extend the theoretical setup to complete legal compliance. The latter does not alter the behavior and merely influences thresholds and trade-off limits. The model successfully illustrates the behavior with the limitations imposed, assuming firms exhibit natural behavior - i.e. assuming firms do not engage in fraudulent behavior.

theoretical structure provides the basis for understanding the behavior and decision-making process of firms and suggest possible mechanism of seemingly irrational empirical behavior.

Environment: The model assumes a discrete time $t = 0, 1, 2, \dots$ and a discount factor $\beta \in (0, 1)$. Each period the firm observes a demand of $a_t > 0$ that follows the *autoregressive (AR) model*: $\log a_t = \rho \log a_{t-1} + \varepsilon_t, \varepsilon_t \sim \mathcal{N}(0, \sigma^2)$. Additionally, the firm chooses a level of output $y_t \geq 0$. The output and demand give us:

$$R_t = a_t y_t \quad (\text{Eq.8})$$

Each firm has a cost structure composed of fixed costs F and convex variable costs κy_t^ν , where $\kappa > 0$ and $\nu > 1$:

$$C(y_t) = F1\{y > 0\} + \kappa y_t^\nu \quad (\text{Eq.9})$$

The eligibility for GRT in period t is:

$$E_t = 1\{R_{t-1} < \bar{R}\} \quad (\text{Eq.10})$$

where $\bar{R}$ is a statutory threshold for eligibility in the GRT regime (G) with a tax rate τ_G on gross receipts R_t . Contrary, standard profit-based tax regime (S) levies tax τ_P on positive profits only, further assuming no loss offset across period:

$$T_t^S = \tau_P \max\{R_t - C(y_t), 0\} \quad (\text{Eq.11})$$

Single Period Static Choice: Given the regime $r \in G, S$ and a , the firm chooses y to maximize after-tax profit:

(1) GRT:

$$\pi^G(a; y) = \max_{y \geq 0} [(1 - \tau_G)ay - C(y)] \quad (\text{Eq.12})$$

yielding FOC:

$$\begin{aligned} (1 - \tau_G)a &= \kappa \nu y^{\nu-1} \Rightarrow y_G(a) = \left(\frac{(1 - \tau_G)a}{\kappa \nu} \right)^{\frac{1}{\nu-1}} \\ &\Rightarrow R_G(a) = ay_G(a) \end{aligned} \quad (\text{Eq.13})$$

(2) Standard:

$$\pi^S(a; y) = \max_{y \geq 0} [ay - C(y) - \tau_P \max\{R - C(y), 0\}] \quad (\text{Eq.14})$$

yielding FOC:

$$\begin{aligned} a &= \kappa \nu y^{\nu-1} \Rightarrow y_S(a) = \left(\frac{a}{\kappa \nu} \right)^{\frac{1}{\nu-1}} \\ &\Rightarrow R_S(a) = ay_S(a) \end{aligned} \quad (\text{Eq.15})$$

Multi-Period Dynamic Choice: The eligibility for GRT tax regime (G) depends on the gross receipts in period $t - 1$, such that $R_{t-1} < \bar{R}$. If $E_t = 1$, the firm chooses $r_t \in \{G, S\}$, otherwise the firm must choose standard profit-based tax regime (S). Similarly, the firm's gross receipts in period t determine eligibility for period $t + 1$, hence there exists an incentive for firms to bunch around statutory threshold $\bar{R}$ to keep the eligibility.

Assuming that the state $s_t = (E_t, a_t)$ realizes, we obtain the following value function:

$$V(E, a) = \begin{cases} \max\{V^G(a), V^S(a)\}, & E = 1 \\ V^S(a), & E = 0 \end{cases} \quad (\text{Eq.16})$$

where for regime $r \in \{G, S\}$:

$$V^r(a) = \max_{y \geq 0} [\pi^r(a; y) + \beta \mathbb{E}_a[V(1\{ay < \bar{R}\}, a')]] \quad (\text{Eq.17})$$

Equivalently, the regime's current period problem is:

$$\max_{y \geq 0} [\pi^r(a; y) + \beta \Omega 1\{ay < \bar{R}\}] \quad (\text{Eq.18})$$

where Ω is endogenous and intuitively determines how much the firm values being eligible in the next period:

$$\Omega \equiv \mathbb{E}_{a'}[V(1, a') - V(0, a')] \geq 0 \quad (\text{Eq.19})$$

Tax Regime Selection: The firm chooses the tax regime $r \in \{G, S\}$ at the beginning of the period without any cost given its demand a_t and eligibility status $E_t = 1$. Further, define:

(1) Unconstrained current profits:

$$\pi_r^{uc}(a) \equiv \pi^r(a; y_r^{uc}(a)) \quad (\text{Eq.20})$$

(2) Capped current profits:

$$\pi_r^c(a) \equiv \pi^r(a; y^c(a)) \text{ with } ay^c = \bar{R} \quad (\text{Eq.21})$$

(3) Current loss from capping in regime r :

$$\Delta_r(a) \equiv \pi_r^{uc}(a) - \pi_r^c(a) \geq 0 \quad (\text{Eq.22})$$

Let $y_r^{uc}(a)$ be the unconstrained optimizer in regime r and $y^c(a) = \frac{\bar{R}}{a}$ the cap the exactly preserves eligibility. Additionally, let R_r^{uc} denote the unconstrained gross receipts:

(1) if $R_r^{uc}(a) < \bar{R}$, the eligibility is preserved even unconstrained. Therefore, the firm chooses the regime r such that it maximizes current period's profit using a static comparison:

$$r_t = \max_{r \in \{G, S\}} \{\pi^G(a; y), \pi^S(a; y)\} \quad (\text{Eq.23})$$

(2) if $R_r^{uc}(a) > \bar{R}$ for any regime r , the firm must compare profits under GRT regime when capping gross receipts earned and the profits of unconstrained productivity under standard tax regime. However, contrary to the case (1), firms must choose the regime dynamically as the choice for the current period impacts next period's eligibility:

$$\max\{\max_{r \in \{G, S\}} \pi_r^c(a), 0\} + \beta \mathbb{E}[V(1, a')] \text{ vs } \max\{\max_{r \in \{G, S\}} \pi_r^{uc}(a), 0\} + \beta \mathbb{E}[V(0, a')] \quad (\text{Eq.24})$$

The firm will cap iff:

$$\beta \Omega \geq \left(\max_r \{\pi_r^{uc}(a), 0\} - \max_r \{\pi_r^c(a), 0\} \right) \quad (\text{Eq.25})$$

Therefore, the firm must compare the profits when constrained at the statutory threshold $\bar{R}$ and eligibility continuation value to the profits earned in the given period when unconstrained gross revenues are realized and the eligibility discontinuation value. Intuitively, the firm must decide whether to limit gross receipts by comparing the sum of profits earned between current and next period, subject to eligibility dependent on current period's gross receipts.

Comparative Statics: The increases to τ_G leads to stronger distortion, causing (1) fewer entrant into GRT and (2) less bunching close to statutory threshold $\bar{R}$. On the contrary, higher τ_P leads to improved attractiveness of GRT. Similarly, larger $\bar{R}$ leads to eligibility being more valuable and increases Ω , thus increasing bunching. With increased costs, the attractiveness of GRT falls. Additionally, higher β , ρ and σ lead to increases in Ω .

2.4 Data

The section introduces the data collected and establishes the counterfactual comparison. The data was collected from two separate data sources: (1) Financial Administration of Republic of Slovenia and (2) Bureau van Dijk's database Orbis ("BvD Orbis"). The two databases were merged to build a panel data for firms from 2009 to including 2017. The data collected from the Financial Administration of Republic

of Slovenia is not publicly available and provides the identification of the firms based on the *Tax Identification Number* (TIN). Moreover, the data includes the data on all persons (for-profit and non-profit businesses) in Slovenia. However, the publicly available data collected from BvD Orbis is identified using *Business Identification Number* (“BIN”) with TIN information available.

The data obtained from the Financial Administration of Republic of Slovenia includes both natural and legal persons as business forms, with the legal persons representing the majority of all businesses in Slovenia prior to the introduction of GRT. In 2013, natural persons represented 98.05% of all firms active prior to December 7, 2012 that have transitions to the GRT.

The construction of the GRT with a predetermined rate of expenses implies the possibility of industry-specific dynamics with a higher preference for GRT among select industries rather than the others. The preference of industries for participation in GRT, when eligible, can be visually represented in [Figure 2.1](#) with clear preference among Health, Information, Professional Services and Public Administration industries - see [Table 2.16](#) for industry-sector specification.

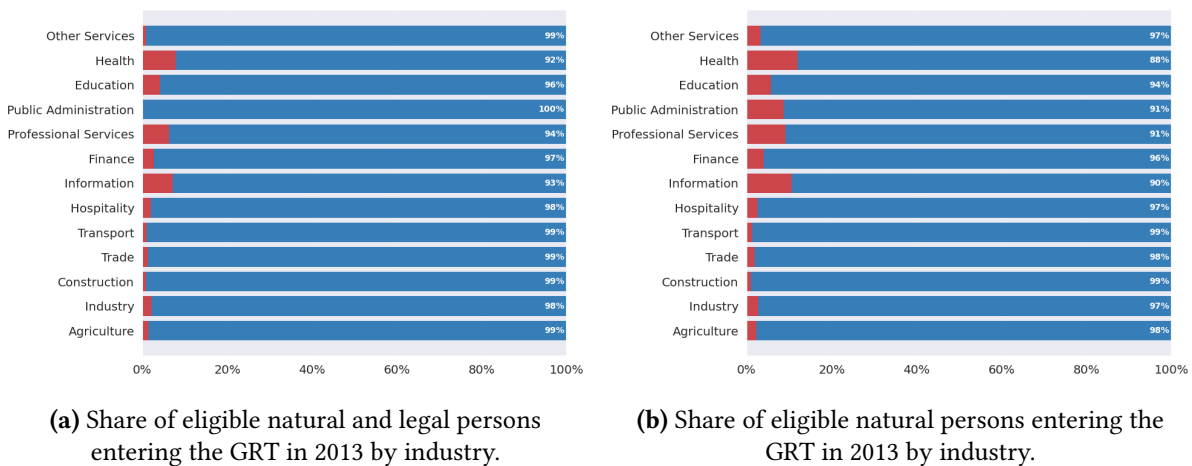


Figure 2.1: Share of firms eligible entering the GRT by industry.

2.4.1 Identification Strategy

The self-selection of the firms imposes the counterfactual issues with the need to establish a valid counterfactual. The choice of firms on entering the GRT imposes issues in treating any firm as a suitable counterfactual. Consequently, the paper uses approach based on [Sufi \(2009\)](#) and focuses on the firms that have entered the GRT in 2013 as the first available year. Due to the endogenous decision of the firms to enter the GRT, subject to eligibility criteria, the firms that were registered after December 7, 2012 are eliminated from the sample. Similarly, the firms active prior to December 7, 2012 that have entered the GRT in the subsequent years cannot be used as a suitable counterfactual due to the

endogeneous decision without further endogeneity controls.

Due to pseudo-randomness, the analysis primarily focuses on the fuzzy regression discontinuity design (FRD), focusing only on the firms in close proximity around the statutory threshold $\bar{R} = \text{€}50,000$ for causal impacts. However, due to limited extend of FRD, the paper extends the analysis by applying inverse mills ratio and propensity score matching to address the self-selection and endogeneity. The paper therefore relies on the pseudo-randomness observed in 2012 due to time constraints, reducing financial manipulation of the firms and learning of the firms.

With the GRT being announced mere 3 weeks prior to the firms entering the GRT, it is reasonable to assume pseudo-randomness occurred among firms that should have entered in 2013. Consequently, it is assumed certain firms that should have entered in 2013, failed to do so and would enter when first possible - i.e. in 2014. The sample therefore eliminates all firms registered from December 7, 2012, as well the firms that have either never entered the GRT or have entered after 2014. The *Early Adopters* group consists of the firms that were active prior to December 7, 2012, were eligible to enter in 2013 - i.e. had gross receipts below €50,000 in 2012, and transitioned in the GRT in 2013, with the *Late Adopters* group being the firms that were active prior to December 7, 2012, were eligible to enter in 2013 and in 2014 and transitioned to the GRT in 2014.

Panel A: Early Adopters vs Late Adopters							
	Early Adopters (EA)			Late Adopters (LA)			Diff
	N	Mean	SD	N	Mean	SD	
Outcome Variable							
Gross Receipts	2,592	9.36	1.26	850	9.27	1.22	0.09*
Other Variables							
Gross Expenses	2,578	8.97	1.12	849	9.02	1.11	-0.05
Profit Margin	2,592	32.38	23.19	850	23.64	20.08	8.74***
Size	1,974	8.58	1.54	692	8.52	1.59	0.07
Liquidity	1,704	0.59	7.10	599	0.30	0.97	0.30*
Return on Assets	1,974	8.77	284.34	691	12.25	225.61	-3.48
Cash	1,729	6.83	2.12	605	6.59	2.10	0.25**
Net Working Capital	1,974	5.78	5.08	692	5.72	4.95	0.06
Collateral	1,013	0.27	0.29	356	0.29	0.29	-0.02
Cash Flow	982	8.64	1.11	324	8.38	1.10	0.26***

Panel B: Early Adopters vs All Eligible for 2013

	Early Adopters (EA)			Eligible (EL)			Diff
	N	Mean	SD	N	Mean	SD	
Outcome Variable							
Gross Receipts	2,592	9.36	1.26	90,585	8.87	1.91	0.49***
Other Variables							
Gross Expenses	2,578	8.97	1.12	79,614	9.10	1.37	-0.13***
Profit Margin	2,592	32.38	23.19	79,764	12.15	16.88	20.23***
Size	1,974	8.58	1.54	56,718	9.00	1.79	-0.41***
Liquidity	1,704	0.59	7.10	44,528	0.85	40.71	-0.25
Return on Assets	1,974	8.77	284.34	56,429	-1.67	550.51	10.44
Cash	1,729	6.83	2.12	47,287	6.43	2.18	0.40***
Net Working Capital	1,974	5.78	5.08	56,709	4.01	6.84	1.77***
Collateral	1,013	0.27	0.29	32,936	0.37	0.34	-0.10***
Cash Flow	982	8.64	1.11	24,330	8.02	1.17	0.61***

Table 2.2: Descriptive statistics and the t-test results the 2010-2012 period. The Early Adopters group consists of firms that were registered prior to December 7, 2012, were eligible for 2013 entry and have transitioned to the GRT in 2013. The Late Adopters group is defined as the firms active prior to December 7, 2012, were eligible for 2013 and 2014 entry and have entered in 2014. The Eligible group refers to all firms registered prior to December 7, 2012 and were eligible to enter in 2013. All continuous variables are in natural log form, with negative profit margin being equal to 0 and winsorized at 0.1% in the right tail.

The [Table 2.2](#) shows the descriptive statistics of early and late adopters in Panel A and early adopters and all eligible firms for 2013 in Panel B. The comparison in Panel A shows a significant difference between two groups in the profit margin³. Additionally, the two groups see a significant difference in cash and cash flows. However, when comparing early adopters to all eligible firms, significant differences emerge across the board, suggesting systematic differences between two groups.

Among firms entering in 2013, 90.42% of all firms had a positive profit margin in 2012, compared to

³Profit margin is defined for positive profits only. Due to the nature of the tax regime, depth of losses does not have an impact on the outcome, hence profit margin for losses is defined as 0. See [Figure 2.7](#).

71.91% among all eligible firms. Additionally, in 2010-2012 period, firms entering in 2013 had a mean profit margin of 31.18% and a median of 29.01%, compared to eligible firms with mean of 46.20% and a median of mere 4.93%.

2.4.2 Log Transformation

The data used in this paper has all continuous variables log-transformed using a natural log. The log transformation used in this analysis is represented in Eq.26:

$$T(X) = \begin{cases} \ln(X + 1) & \text{if } X \geq 0 \\ -\ln(|X| + 1) & \text{if } X < 0 \end{cases} \quad (\text{Eq.26})$$

This approach is favored for its simplicity by compressing larger values and expanding smaller values, the Eq.26 reduces the impact of high-income outliers while maintaining the relative ranking of observations. This makes the log-transformed model highly interpretable and consistent with existing literature, reinforcing the robustness of findings through comparability with prior research. The interpretation of coefficients under the Eq.26 framework yields straightforward interpretation. This interpretative clarity is useful for policy recommendations and economic analyses, as it directly translates into actionable insights regarding income changes.

2.5 Empirical Analysis

This section introduces the results using fuzzy regression discontinuity design (FRD) and two-way fixed effects (TWFE) models. The sample was divided in the *Early Adopters* (EA) if the firm entered the GRT in 2013 and *Late Adopters* (LA) if entered in 2014. Moreover, the firms must have been active prior to announcement of the policy change on December 7, 2012 to eliminate internalization of the GRT when registering the business. The analysis for TWFE is extended to control for endogeneity using Inverse Mills Ratio (IMR) and propensity score matching (PSM). Additionally, to improve the underlying understanding of the driving mechanism, the analysis uses bin-specific TWFE estimation. Lastly, the placebo test are conducted to validate the findings.

2.5.1 Determinants of Program Participation

The empirical analysis first attempts to understand the determinants of program participation in 2013. In accordance with the theoretical model outlined in the Section 2.3, the primary focus of the analysis

are gross receipts and the profit margin. The analysis utilizes the following model:

$$P(\text{GRT}_{i,2013} = 1) = \Phi\left(\beta_0 + \beta_1 \text{Gross Receipts}_{i,2012} + \beta_2 \text{Profit Margin}_{i,2012} + \beta_3 \text{Gross Receipts} * \text{Profit Margin}_{i,2012} + \beta X_{i,2012}\right) \quad (1)$$

where $\text{Gross Receipts}_{i,2012}$ is natural log of gross receipts in 2012 for firm i , $\text{Profit Margin}_{i,2012}$ is a profit margin in percent terms in 2012 for firm i and $X_{i,2012}$ is a vector of firm-specific controls. The latter specifically includes size, cash and cash equivalents, net working capital, liquidity and return on assets.

The empirical analysis examines how firms' gross receipts and profit margins influence the probability of entering the Gross Receipt Taxation (GRT) regime, with the results presented in [Table 2.3](#). When comparing early and late adopters, the estimates show that gross receipts alone have a weakly positive impact on entry, significant at the 10% level, while profit margins exhibit a strong and highly significant positive effect at the 1% level when estimated individually. However, when the two variables are interacted, their individual effects change, specifically gross receipts change direction of the coefficient but keep significance, and the interaction term remains positive and significant at the 1% level, suggesting that the effect of gross receipts depends on the level of profitability. Once firm-level controls are included, the direct impact of gross receipts becomes statistically insignificant, profit margins turn negative and significant at the 5% level, and the interaction term remains positive and highly significant, indicating that firms' entry decisions are primarily driven by the joint relationship between revenue and profitability rather than by either factor independently.

Expanding the comparison to include all eligible firms yields consistent results. Both gross receipts and profit margins are individually positive and significant at the 1% level when estimated separately. When the interaction between the two is introduced, the individual coefficients become negative while the interaction remains positive and significant at the 1% level. The inclusion of additional controls leaves these results largely unchanged, confirming the robustness of the interaction effect and suggesting that GRT participation is influenced by complementarities between firm scale and profitability.

Panel A: Early Adopters vs Late Adopters

	GRT			
	(1)	(2)	(3)	(4)
Revenues (€)	0.033*		-0.043*	0.014
	(0.018)		(0.024)	(0.046)
Profit Margin (%)		0.011***	-0.007	-0.026**

		(0.001)	(0.007)	(0.014)
Revenues * Profit Margin			0.002***	0.004***
			(0.001)	(0.001)
Obs	3,442	3,442	3,442	2,302
Controls	-	-	-	✓

Panel B: Early Adopters vs Eligible for 2013 Entry

	GRT			
	(1)	(2)	(3)	(4)
Revenues (€)	0.080***		-0.022***	-0.134***
	(0.006)		(0.006)	(0.016)
Profit Margin (%)		0.019***	-0.017***	-0.050***
		(0.000)	(0.003)	(0.006)
Revenues * Profit Margin			0.004***	0.008***
			(0.000)	(0.001)
Obs	93,177	82,356	82,356	46,066
Controls	-	-	-	✓

Table 2.3: Probit analysis for the probability of firms entering the GRT in 2013. The sample selection is consistent with prior nomenclature and selection. All continuous variables are in natural log form and the profit margin is windorised at 0.1% in the right tail.

Robust standard errors in parentheses.

*** p<0.01, ** p<0.05, * p<0.1

The graphical evidence supports these regression-based findings. Heat map in [Figure 2.2a](#) shows that firms with low gross receipts but exceptionally high profit margins are the least likely to enter, while the probability of entry increases approximately exponentially in terms of nominal gross receipts from low-revenue, low-profit firms to high-revenue, high-profit firms, with a growing bunching at upper extreme. Histograms in [Figure 2.8](#) further illustrate that the probability of entry rises with higher profit margins, whereas the relationship with gross receipts remains relatively flat, displaying a mild U-shaped pattern. Taken together, the results consistently point to a pattern in which GRT participation is most likely among firms that combine higher profitability with greater gross receipts, underscoring that the incentives to enter the regime arise from the interaction of these two dimensions rather than

their independent effects.

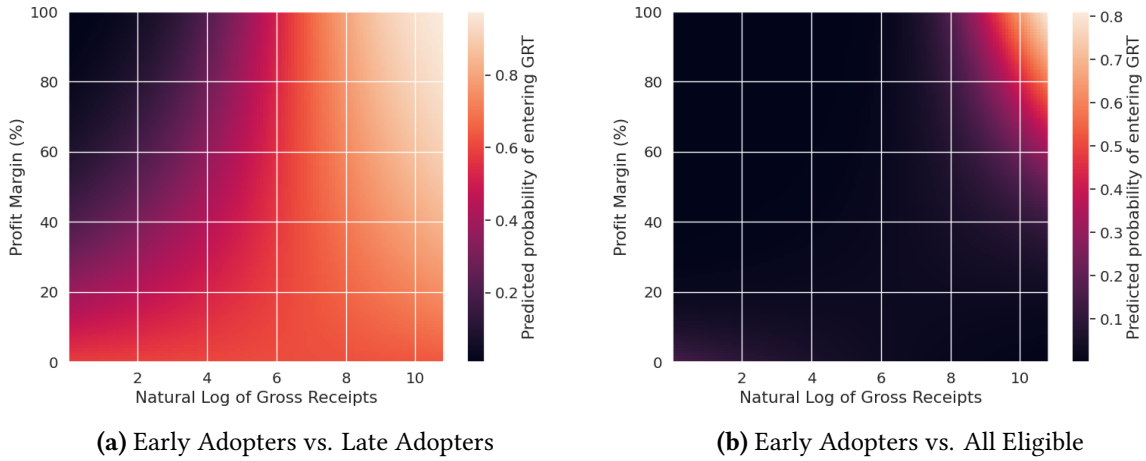


Figure 2.2: The probability of entrance in GRT in 2013 based on 2012 values of profit margin and gross receipts across Early Adopters, Late Adopters and All Eligible for 2013 entrance.

2.5.2 Fuzzy Regression Discontinuity Design

To further identify the causal effect of the GRT regime on firm behavior, a fuzzy regression discontinuity design (FRD) is implemented around the eligibility threshold of €50,000 in gross receipts. The design exploits the discontinuity in GRT eligibility, where firms below the threshold were permitted to enter the regime, while those above were excluded.

Compliance with the eligibility rule was nearly perfect. In 2013, only one non-eligible firm with reported receipts above €50,000 entered the GRT, and in 2014 only four such cases were identified. This near-perfect compliance supports the validity of the discontinuity design and indicates that assignment to treatment can be considered sharp in practice, with only minimal fuzziness. The few exceptions above the threshold are considered random or administrative anomalies rather than evidence of systematic manipulation.

Imperfect compliance below the threshold arises primarily from profit-maximizing behavior rather than from administrative misclassification. Some firms that were eligible to enter the GRT chose to remain under the standard taxation regime, consistent with the theoretical model developed earlier in [Section 2.3](#). Under the model's assumptions, these firms are those for whom expected profits under standard taxation exceed those under GRT - i.e. typically firms with higher expense ratios or greater income volatility. Thus, imperfect compliance below the threshold reflects rational selection rather than random error.

The estimation was conducted using the `rdrobust` package in Python, which implements the bandwidth optimization procedure developed by [Calonico et al. \(2014\)](#). The method selects an optimal local bandwidth by minimizing the mean squared error (MSE) of the bias-corrected local polynomial estimator. In particular, the procedure computes an MSE-optimal bandwidth for both sides of the cutoff, applies bias correction, and adjusts the standard errors using robust inference procedures. Bandwidths were also varied proportionally ($\frac{1}{4}h$, $\frac{1}{2}h$, h , and $2h$) to assess sensitivity to the local window of comparison.

The results, summarized in [Table 2.4](#), indicate no statistically significant discontinuities in entry behavior around the €50,000 threshold. Across all models, the point estimates fluctuate in sign and magnitude without a consistent pattern, reinforcing the conclusion that there is no evidence of a structural discontinuity at the eligibility cutoff.

Panel A: Linear FRD				
	$\frac{1}{4}h$	$\frac{1}{2}h$	h	$2h$
Conventional	17,561.640 (35,721.442)	4,208.876 (35,402.148)	9,130.650 (40,023.112)	30,709.736 (43,946.608)
Robust	44,422.960 (47,708.050)	5,622.312 (46,082.753)	60,737.727 (57,167.631)	-42,455.511 (61,396.212)
Obs	[3290, 2764]	[6986, 5298]	[16423, 9438]	[59552, 15874]
Panel B: Quadratic FRD				
	$\frac{1}{4}h$	$\frac{1}{2}h$	h	$2h$
Conventional	31,839.845 (43,228.264)	-19,859.605 (40,253.180)	66,014.252 (55,658.600)	-10,261.252 (50,200.485)
Robust	59,895.298 (49,786.783)	44,995.285 (49,365.122)	19,026.621 (50,813.703)	-42,825.968 (72,712.334)
Obs	[4007, 3309]	[8539, 6216]	[21661, 10874]	[94423, 17844]

Table 2.4: Fuzzy regression discontinuity design results with estimated optimal bandwidth and corresponding bandwidth modifications for robustness testing.

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

The graphical results in [Figure 2.3](#) support the regression estimates. The FRD plots display the local averages of firm entry probabilities and fitted local polynomial curves on either side of the €50,000

threshold. The visual evidence shows no clear jump in the outcome variable at the cutoff, and the fitted trends appear continuous, further suggesting the absence of a discontinuous treatment effect. The density of firms around the threshold is smooth, indicating no evidence of strategic manipulation or bunching behavior. These graphical patterns, together with the regression estimates, confirm that the discontinuity in eligibility did not translate into a measurable behavioral response at the margin.

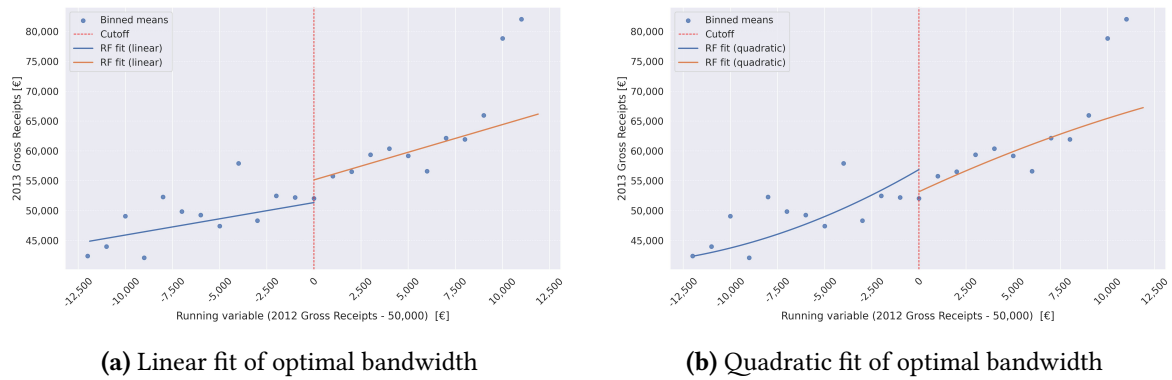


Figure 2.3: Fuzzy regression discontinuity design graphs with estimated optimal bandwidth.

2.5.2.1 Doughnut Fuzzy Regression Discontinuity Design

To assess the robustness of the regression discontinuity estimates, a doughnut-shaped FRD was implemented by progressively trimming observations immediately surrounding the €50,000 eligibility threshold. This approach removes firms located in the immediate neighborhood of the cutoff, where measurement error, administrative noise, or strategic misreporting could distort the discontinuity, and re-estimates the model using progressively wider exclusion zones. The analysis was conducted using the same bandwidths obtained from the MSE-optimal procedure in the `rdrobust` package, with $\frac{1}{4}$, $\frac{1}{3}$, and $\frac{1}{2}$ trims representing the proportion of data symmetrically excluded around the cutoff.

The results, presented in Table 2.5, show that the exclusion of firms closest to the threshold does not materially alter the findings from the baseline FRD specification. In the linear doughnut models (Panel A), the conventional estimates remain statistically insignificant across all trimming levels, while the robust bias-corrected estimates produce weakly significant coefficients at the 10% level in the most restrictive specification ($\frac{1}{4}$ trim). However, both the sign and magnitude of these effects fluctuate across bandwidths, and the statistical significance is not consistent, suggesting that any local variation near the threshold is likely due to noise rather than a true discontinuity.

The quadratic doughnut models (Panel B) display a similar pattern. The conventional estimates are statistically insignificant throughout, and while the robust estimates yield one weakly significant

positive coefficient at the 10% level and one significant negative effect at the 5% level, these results are not stable across trimming levels. The inconsistency in sign and loss of precision as the trimmed window expands further reinforce the absence of a systematic discontinuity in firm entry behavior at the eligibility cutoff.

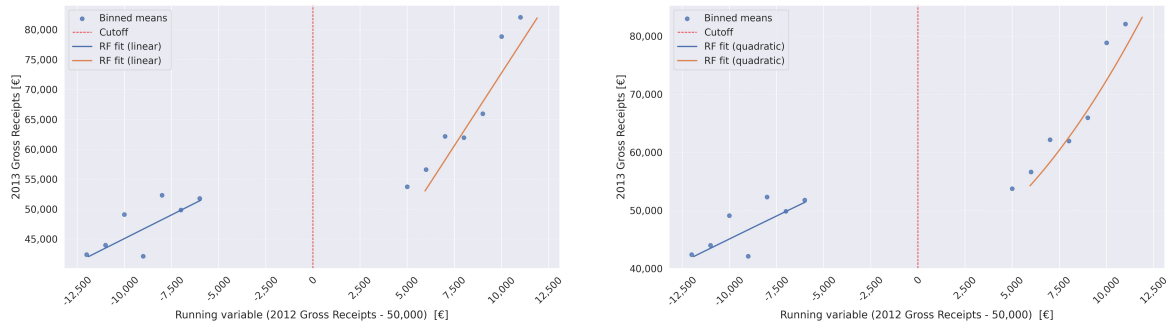
Panel A: Linear Doughnut FRD			
	$\frac{1}{4}$ Trim	$\frac{1}{3}$ Trim	$\frac{1}{2}$ Trim
Conventional	-16,476.814 (124,754.845)	-300,900.161 (247,832.456)	-644,024.296 (662,837.387)
Robust	1,072,302.794* (599,543.609)	1,090,636.491 (1,088,314.089)	-4,171,362.395 (3,165,169.977)
Obs	[13133, 6674]	[11899, 5759]	[9437, 4140]

Panel B: Quadratic Doughnut FRD			
	$\frac{1}{4}$ Trim	$\frac{1}{3}$ Trim	$\frac{1}{2}$ Trim
Conventional	1,762,199.632 (2,422,598.146)	3,080,501.812 (12,615,484.795)	-618,556.108 (980,504.300)
Robust	12,404,361.798* (7,482,993.584)	-99,627,273.230** (47,999,389.825)	-1,903,499.323 (6,953,027.023)
Obs	[17654, 7565]	[16220, 6550]	[13122, 4658]

Table 2.5: Doughnut fuzzy regression discontinuity design results with estimated optimal bandwidth and corresponding bandwidth modifications for robustness testing.

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

The graphical evidence from the doughnut-shaped specifications in [Figure 2.4](#) reinforces these conclusions. The plots display local polynomial fits with 50% trim around the cutoff for linear and quadratic fits. Overall, the doughnut-shaped FRD results confirm the robustness of the main findings. The absence of consistent or statistically meaningful discontinuities after excluding firms immediately surrounding the threshold suggests that the null results from the baseline FRD are not driven by local data irregularities, misclassification, or manipulation.



(a) Linear fit of optimal bandwidth with missing middle 50% (b) Quadratic fit of optimal bandwidth with missing middle 50%

Figure 2.4: Fuzzy regression discontinuity design graphs with estimated optimal bandwidth with missing middle 50%.

2.5.3 Two-Way Fixed Effects

To complement the regression discontinuity analysis, a two-way fixed effects (TWFE) framework is employed to estimate the average association between participation in the GRT regime and gross receipts. The model incorporates both firm and year fixed effects to control for time-invariant firm heterogeneity and aggregate shocks affecting all firms simultaneously. The data is limited to 2013 due to freedom of transition between tax regimes and applies the following model:

$$y_{i,t} = \beta_0 + \beta_1 \text{GRT}_{i,t-1} + \beta X_{i,t-1} + \varepsilon_{i,t-1} \quad (2)$$

where $y_{i,t}$ are gross receipts of firm i at time t . The later extension replaces gross receipts with 3- or 5-year gross receipt growth. $X_{i,2012}$ is a vector of firm-specific controls, specifically size, cash and cash equivalents, net working capital, liquidity and return on assets.

The baseline results are presented in Table 2.6. When comparing early and late adopters of the GRT between 2013 and 2014, the coefficient on GRT participation is negative and weakly significant in the simplest specification, but becomes statistically insignificant once firm-level controls are included. The direction of the coefficient suggests a modest decline in performance among early adopters in the immediate post-adoption period, though the magnitude is economically small and not robust across specifications.

When the comparison is expanded to include all eligible firms, the results change direction: GRT participation is positively and significantly associated with the outcome variable at the 1% level, both with and without controls. This indicates that GRT firms perform better, on average, than the broader pool of eligible firms once entry into the regime has occurred. The inclusion of firm and time fixed effects ensures that these patterns are not driven by systematic firm-level differences or year-specific

shocks.

	Natural Log of Gross Receipts			
	(1)	(2)	(3)	(4)
GRT	-0.076*	-0.039	0.249***	0.189***
	(0.042)	(0.040)	(0.021)	(0.023)
Obs	11,780	5,804	375,512	132,091
R^2	0.000	0.011	0.000	0.011
Controls	-	✓	-	✓
Firm FE	✓	✓	✓	✓
Time FE	✓	✓	✓	✓
Sample	2013 v 2014	2013 v 2014	2013 v Eligible	2013 v Eligible

Table 2.6: TWFE results for 2010-2013 period with 2013 being the only post-GRT period. The period restriction allows for the elimination of later transitions between tax regimes.

Robust standard errors in parentheses.

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table 2.7 extends the analysis to longer horizons by estimating 3-year and 5-year growth effects. For the comparison between early and late adopters (Panel A), GRT participation exhibits no statistically significant impact on growth over either horizon. The coefficients fluctuate in sign and lose significance once controls are added, suggesting that early adoption did not generate measurable medium-term performance differences relative to later entrants. In contrast, when early adopters are compared with all eligible firms (Panel B), GRT participation is negatively associated with both three-year and five-year growth in the specifications without controls, significant at the 1% level. However, the inclusion of additional firm-level controls substantially reduces both the magnitude and statistical significance of these effects.

Panel A: Early Adopters vs Late Adopters				
	3-Year Growth		5-Year Growth	
	(1)	(2)	(3)	(4)
GRT	-0.115	-0.609	1.105	-0.351
	(0.872)	(0.599)	(1.287)	(0.788)

Obs	2,334	1,793	2,334	1,793
R^2	0.000	0.343	0.000	0.406
Controls	-	✓	-	✓
Firm FE	✓	✓	✓	✓
Time FE	✓	✓	✓	✓

Panel B: Early Adopters vs All Eligible

	3-Year Growth		5-Year Growth	
	(1)	(2)	(3)	(4)
GRT	-2.772*** (0.511)	-0.364 (0.381)	-5.957*** (0.915)	-0.890 (0.677)
Obs	62,746	34,656	62,746	34,656
R^2	0.000	0.015	0.000	0.017
Controls	-	✓	-	✓
Firm FE	✓	✓	✓	✓
Time FE	✓	✓	✓	✓

Table 2.7: TWFE estimation of 3- and 5-year growth from 2012 to 2015 and 2012 to 2017 respectively. The results are obtained by estimating a cross-sectional analysis with 2012 data for covariates.

Robust standard errors in parentheses.

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

While the patterns observed in the early-adopter versus eligible-firm comparison provide useful descriptive insights, these estimates should be interpreted as exploratory rather than causal. Endogeneity concerns are likely present, as firms that opted into GRT early may differ systematically from the broader eligible population in ways not fully captured by observable characteristics or fixed effects, such as risk preferences, managerial ability, or expectations about future profitability. Consequently, the results from this broader comparison reflect correlated firm behavior rather than a clean treatment effect of GRT participation. The two-way fixed effects estimates therefore serve primarily as complementary evidence, indicating that while early adoption may correlate with distinct firm trajectories, there is no robust causal effect of GRT participation on firm growth once selection and unobserved heterogeneity are taken into account.

To examine whether the rigidity of the GRT regime translated into higher failure risk, the paper estimates probit models of bankruptcy within three and five years of adoption. The results in Table 2.8 reveal a clear and consistent pattern. When comparing Early Adopters to Late Adopters (Panel A), GRT participation significantly increases the probability of bankruptcy in both the three-year and five-year horizons, even after controlling for firm-level covariates. This finding is highly consistent with the theoretical prediction that GRT imposes greater downside risk by taxing firms on gross receipts regardless of realized profitability. Under adverse shocks or rising expenses, GRT firms cannot adjust their tax burden, making them more vulnerable to financial distress and potential exit. These results also complement the earlier finding that GRT did not improve medium- or long-term growth among Early Adopters relative to Late Adopters. The absence of growth gains, combined with an increase in bankruptcy risk, suggests that GRT helped firms survive in the very short run through administrative simplicity but weakened their resilience over time.

Panel A: Early Adopters vs Late Adopters				
	3-Year Bankruptcy		5-Year Bankruptcy	
	(1)	(2)	(3)	(4)
GRT	0.456*** (0.066)	0.429*** (0.094)	0.137*** (0.052)	0.154** (0.069)
Obs	3,442	2,302	3,442	2,302
Controls	-	✓	-	✓

Panel B: Early Adopters vs All Eligible				
	3-Year Bankruptcy		5-Year Bankruptcy	
	(1)	(2)	(3)	(4)
GRT	-0.017 (0.029)	-0.088** (0.041)	0.039 (0.026)	-0.043 (0.035)
Obs	93,177	46,069	93,177	46,069
Controls	-	✓	-	✓

Table 2.8: Probit estimation of 3- and 5-year bankruptcy probabilities from 2012 to 2015 and 2012 to 2017 respectively.

Robust standard errors in brackets.

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

In contrast, when Early Adopters are compared to all Eligible firms (Panel B), the bankruptcy effects are small and mostly insignificant, with some specifications even showing a negative coefficient. However, this comparison is not causal due to selection, as earlier sections showed that Early Adopters differ systematically from the full eligible population and tend to be stronger firms *ex ante*. Indeed, once controls are added, the GRT coefficient becomes weakly negative in some cases, reflecting the fact that these Early Adopters were positively selected on unobservables such as managerial ability or profitability expectations. This reinforces the importance of using the Early-versus-late comparison as the credible counterfactual.

Taken together, the bankruptcy evidence aligns with the broader set of findings. GRT does not generate long-term growth advantages but instead increases financial fragility for firms close to the eligibility threshold. Thus, the probit results provide a critical link between the lack of growth benefits and the presence of higher exit risk, GRT secures short-run administrative savings but at the cost of long-run resilience.

2.5.3.1 Inverse Mills Ratio

The earlier two-way fixed effects (TWFE) estimates suggest systematic differences between early GRT adopters and other eligible firms, but the results cannot be interpreted as causal due to potential endogeneity in firms' self-selection into the regime. Firms that chose to adopt GRT early may differ from late and non-adopters in unobserved characteristics such as entrepreneurial ability, risk preferences, or expectations about future profitability. These unobserved factors may jointly determine both the decision to enter the GRT regime and subsequent firm performance, leading to biased estimates if not properly accounted for.

To address this issue, a Heckman selection correction was implemented by incorporating the Inverse Mills Ratio (IMR) from a first-stage selection equation estimating the probability of entering the GRT regime. The first stage models GRT participation as a function of observable firm characteristics and financial variables, generating the IMR term that captures the expected value of the selection error. In the second stage, the IMR is included as an additional regressor in the TWFE [Equation 2](#) to correct for potential selection bias. All models retain firm and year fixed effects and are estimated both with and without additional controls to test robustness. The specification for IMR is as follows:

$$P(\text{GRT}_{i,2013} = 1) = \Phi\left(\beta_0 + \beta_1 \text{Gross Receipts}_{i,2012} + \beta_2 \text{Profit Margin}_{i,2012} + \beta_3 \text{Gross Receipts} * \text{Profit Margin}_{i,2012}\right) \quad (3)$$

The results are presented in [Table 2.9](#). After accounting for selection, the GRT coefficient becomes negative and statistically significant in the baseline specifications, ranging from -0.99 to -0.23, depending on the sample and inclusion of controls. This indicates that, conditional on selection, GRT participation is associated with lower short-term performance relative to comparable firms not in the regime. The IMR term is consistently positive and significant across all specifications, confirming the presence of selection bias in the uncorrected models: firms with unobserved characteristics that increase the likelihood of GRT participation also tend to exhibit higher baseline performance, leading to upward bias in naive estimates. Once this bias is corrected, the negative coefficient on GRT implies that the regime's simplifying benefits may come at the cost of lower realized outcomes in the short term.

Natural Log of Gross Receipts				
	(1)	(2)	(3)	(4)
GRT	-0.986*** (0.284)	-0.692** (0.347)	-0.227* (0.137)	-0.255 (0.191)
IMR	0.551*** (0.175)	0.397* (0.218)	0.230*** (0.068)	0.215** (0.094)
Obs	11,780	5,804	375,512	132,091
R^2	0.002	0.012	0.000	0.011
Controls	-	✓	-	✓
Firm FE	✓	✓	✓	✓
Time FE	✓	✓	✓	✓
Sample	2013 v 2014	2013 v 2014	2013 v Eligible	2013 v Eligible

Table 2.9: TWFE with IMR correction of endogeneity results for 2010-2013 period with 2013 being the only post-GRT period. The period restriction allows for the elimination of later transitions between tax regimes.

Robust standard errors in parentheses.

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

[Table 2.10](#) extends this analysis to firm growth over 3- and 5-year horizons. The results reveal a consistent pattern across both comparisons. For early adopters versus late adopters (Panel A), GRT participation is associated with a statistically significant decline in growth over both horizons, with coefficients ranging from -8.06 to -16.27 percentage points, depending on the specification. The IMR term remains positive and significant, indicating persistent selection effects. When comparing early adopters to all eligible firms (Panel B), the estimated effects become larger in magnitude, ranging from

-10.63 to -46.64 percentage points, and remain strongly significant at the 1% level. These effects weaken but remain negative after adding controls, suggesting that the performance gap between early adopters and the broader eligible population persists even after adjusting for observable heterogeneity and selection bias.

Panel A: Early Adopters vs Late Adopters				
	3-Year Growth		5-Year Growth	
	(1)	(2)	(3)	(4)
GRT	-13.809*** (2.580)	-8.287*** (2.148)	-16.274*** (3.610)	-8.183*** (2.605)
IMR	8.288*** (1.574)	4.646*** (1.112)	10.518*** (2.222)	4.739*** (1.371)
Obs	2,334	1,793	2,334	1,793
R^2	0.003	0.345	0.002	0.407
Controls	-	✓	-	✓
Firm FE	✓	✓	✓	✓
Time FE	✓	✓	✓	✓
Sample	2013 v Eligible	2013 v Eligible	2013 v Eligible	2013 v Eligible

Panel B: Early Adopters vs All Eligible				
	3-Year Growth		5-Year Growth	
	(1)	(2)	(3)	(4)
GRT	-26.208*** (1.672)	-10.453*** (1.656)	-46.636*** (2.993)	-15.291*** (2.931)
IMR	11.384*** (0.945)	4.883*** (0.887)	19.759*** (1.691)	6.970*** (1.574)
Obs	62,746	34,656	62,746	34,656
R^2	0.001	0.016	0.001	0.017
Controls	-	✓	-	✓
Firm FE	✓	✓	✓	✓
Time FE	✓	✓	✓	✓

Sample	2013 v Eligible	2013 v Eligible	2013 v Eligible	2013 v Eligible
--------	-----------------	-----------------	-----------------	-----------------

Table 2.10: TWFE estimation with IMR correction of endogeneity of 3- and 5-year growth from 2012 to 2015 and 2012 to 2017 respectively. The results are obtained by estimating a cross-sectional analysis with 2012 data for covariates.

Robust standard errors in parentheses.

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Overall, the Heckman-corrected estimates confirm the presence of endogenous self-selection into the GRT regime and indicate that the unadjusted TWFE results understated the negative association between GRT participation and firm growth. The consistently positive and significant IMR coefficients validate the correction procedure and imply that firms opting into GRT were positively selected on unobserved characteristics. Once this selection is accounted for, GRT participation is found to correlate with lower performance and growth outcomes, consistent with the hypothesis that some firms overestimated the benefits of the simplified taxation regime or artificially restrict growth to preserve the eligibility.

In the comparison between Early and Late Adopters in Panel A of [Table 2.11](#), once selection is accounted for, the effect of GRT on bankruptcy becomes statistically insignificant for both the three-year and five-year horizons. This stands in contrast to the self-selection uncorrected probit results, which showed a significant increase in bankruptcy risk among Early Adopters. The loss of significance after IMR correction indicates that the higher observed bankruptcy rates of Early Adopters were not caused by GRT itself, but rather by unobserved characteristics of the firms that chose to adopt Early. In other words, firms that entered the GRT regime at the outset were already systematically different, perhaps more optimistic, risk-taking, or financially constrained, and these traits, rather than the tax regime, contributed to their higher failure rates.

The story changes when comparing Early Adopters to all eligible firms in Panel B. After IMR correction, we observe a striking pattern that the GRT coefficient alternates from positive in specifications without controls to negative in specifications with controls, while the IMR is consistently significant. This confirms that untreated eligible firms differ substantially from Early Adopters in both observable and unobservable ways. The negative coefficients on IMR in the uncontrolled models suggest that firms more likely to choose GRT are also less prone to fail on unobserved dimensions, which leads naive comparisons to underestimate the risks of GRT participation. Once controls are introduced, the sign of GRT often flips, indicating that failing to address selection biases can not only distort the magnitude of estimated effects but even reverse their direction.

Panel A: Early Adopters vs Late Adopters

	3-Year Bankruptcy		5-Year Bankruptcy	
	(1)	(2)	(3)	(4)
GRT	0.176 (0.345)	0.052 (0.498)	-0.321 (0.294)	-0.453 (0.394)
IMR	0.170 (0.206)	0.229 (0.297)	0.279 (0.176)	0.369 (0.236)
Obs	3,442	2,302	3,442	2,302
Controls	-	✓	-	✓

Panel B: Early Adopters vs All Eligible

	3-Year Bankruptcy		5-Year Bankruptcy	
	(1)	(2)	(3)	(4)
GRT	0.396*** (0.101)	-0.385** (0.164)	0.672*** (0.093)	-0.296** (0.138)
IMR	-0.200*** (0.047)	0.141* (0.075)	-0.306*** (0.043)	0.121* (0.064)
Obs	93,177	46,069	93,177	46,069
Controls	-	✓	-	✓

Table 2.11: Probit estimation of 3- and 5-year bankruptcy probabilities from 2012 to 2015 and 2012 to 2017 respectively when correcting for self-selection bias using IMR.

Robust standard errors in brackets.

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

The IMR-corrected bankruptcy results are consistent with the broader empirical narrative of this paper. GRT did not universally increase or decrease bankruptcy risk in a straightforward manner. Instead, its effects were mediated by firm characteristics and self-selection. The regime did not inherently cause firms to fail, rather, it was disproportionately adopted by firms with specific risk profiles and financial expectations. These findings underscore the importance of properly accounting for both observable and unobservable selection when evaluating tax policy. They also highlight a core policy implication, simplified tax regimes can support some firms but impose financial rigidity on others, and

their ultimate impact on survival depends on which firms choose to participate.

2.5.3.2 Propensity Score Matching

To further address potential differences in observable characteristics between GRT participants and non-participants, a propensity score matching (PSM) approach is employed. This method mitigates selection bias arising from observable heterogeneity by matching firms with similar pre-treatment characteristics but differing treatment status. The matching is performed using a nearest-neighbor algorithm with calipers to restrict matches within a predefined maximum distance on the propensity score, ensuring high-quality pairings and minimizing extrapolation bias. The samples are matched on the identical set of covariates $X_{i,t-1}$ as used for controls in earlier models.

The propensity scores are estimated using firm-level covariates that proxy for size, profitability, and sectoral composition, as reported in [Table 2.18](#). Matching quality is evaluated using the Kolmogorov-Smirnov (KS) test and visual inspection of propensity score distributions ([Figure 2.9](#)). Both diagnostics confirm a substantial improvement in balance after matching.

For the comparison between early and late adopters, the pre-matching KS test indicates a significant difference in distributions (statistic = 0.180, $p < 0.001$), while the post-matching test shows complete balance (statistic = 0.007, $p = 1.000$) using a caliper of 0.00025. Similarly, for the comparison between early adopters and all eligible firms, the KS statistic drops from 0.413 ($p < 0.001$) before matching to 0.002 ($p = 1.000$) after matching, with a caliper of 0.1. These results demonstrate excellent post-matching balance across both samples, indicating that observable characteristics are no longer significantly different between treated and control firms.

[Table 2.12](#) presents the post-matching two-way fixed effects estimates using the matched samples. For early adopters versus late adopters (Panel A), the GRT coefficient remains statistically insignificant across all specifications, suggesting no measurable difference in outcomes once observable characteristics are balanced. The results imply that earlier adoption of the GRT does not lead to discernible short-term performance advantages relative to later adopters when firms are matched on similar pre-treatment attributes.

For early adopters versus all eligible firms (Panel B), the baseline specification (column 1) shows a positive and statistically significant association between GRT participation and firm outcomes (0.240, $p < 0.01$). However, this effect vanishes once the analysis focuses on the 3- and 5-year growth, and the coefficients become negative and statistically insignificant. The change in sign and loss of precision after matching and re-estimation indicate that the previously observed positive association in unmatched samples was largely driven by differences in observable firm characteristics rather than a genuine treatment effect of GRT participation.

Panel A: Early Adopters vs Late Adopters			
	Gross Receipts	3-Year Growth	5-Year Growth
	(1)	(2)	(3)
GRT	-0.051 (0.065)	-0.121 (0.537)	0.038 (0.612)
Obs	5,425	1,201	2,461
R^2	0.000	0.000	0.000
Firm FE	✓	✓	✓
Time FE	✓	✓	✓

Panel B: Early Adopters vs All Eligible			
	Gross Receipts	3-Year Growth	5-Year Growth
	(1)	(2)	(3)
GRT	0.240*** (0.052)	-0.283 (0.912)	-0.419 (1.082)
Obs	11,762	1,201	2,461
R^2	0.005	0.000	0.000
Firm FE	✓	✓	✓
Time FE	✓	✓	✓

Table 2.12: TWFE results after propensity score matching on observable covariates. The analysis does not include covariates as the sample is perfectly matched on the observables such that no statistical difference between two samples in the observables can be identified.

Robust standard errors in parentheses.

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Overall, the PSM analysis confirms that observable heterogeneity plays a key role in explaining the performance differences between GRT participants and non-participants. Once firms are matched on relevant covariates, there is no consistent evidence of a causal effect of GRT participation on short-term performance. The matched-sample results suggest that observable factors drive firms' self-selection into the GRT regime, reinforcing the interpretation that the decision to enter was primarily influenced by firm-specific expectations and constraints rather than by exogenous policy incentives.

In the comparison between Early and Late Adopters in Panel A of Table 2.13, the coefficients on GRT are positive and statistically significant for both three-year and five-year bankruptcy probabilities, even after the inclusion of controls. This confirms that, among observationally similar firms that adopted GRT at different times, those entering earlier were more likely to go bankrupt in subsequent years. These findings align with the theoretical argument that GRT increases downside risk by taxing firms on gross receipts rather than realized profits, thereby leaving them exposed when adverse cost shocks occur. They also reinforce earlier evidence that Early Adopters do not experience higher growth rates and may even demonstrate weaker long-term performance. In contrast, when Early Adopters are compared to all Eligible firms, the GRT coefficients become negative and statistically significant. At first glance, this suggests that GRT participation reduces bankruptcy risk. However, this effect must be interpreted with caution.

Panel A: Early Adopters vs Late Adopters				
	3-Year Bankruptcy		5-Year Bankruptcy	
	(1)	(2)	(3)	(4)
GRT	0.446*** (0.111)	0.493*** (0.114)	0.159* (0.081)	0.184** (0.082)
Obs	1,521	1,521	1,521	1,521
Controls	-	✓	-	✓

Panel B: Early Adopters vs All Eligible				
	3-Year Bankruptcy		5-Year Bankruptcy	
	(1)	(2)	(3)	(4)
GRT	-0.125** (0.055)	-0.115** (0.056)	-0.110** (0.048)	-0.100** (0.048)
Obs	3,282	3,282	3,282	3,282
Controls	-	✓	-	✓

Table 2.13: Probit estimation of 3- and 5-year bankruptcy probabilities from 2012 to 2015 and 2012 to 2017 respectively when correcting for self-selection bias using PSM.

Robust standard errors in brackets.

*** p<0.01, ** p<0.05, * p<0.1

Taken together, the PSM-based bankruptcy results reinforce the broader empirical narrative of this paper. When using a credible counterfactual, GRT participation is associated with a higher probability of failure, supporting the view that the regime's rigidity and lack of loss protection weakened financial resilience. When comparing to all eligible firms, even after matching, the negative coefficients reflect selection rather than treatment. These findings are entirely consistent with the earlier growth results that the GRT did not generate medium- or long-term performance gains.

2.5.3.3 Binned TWFE Estimation

To further investigate whether the effects of the Gross Receipt Taxation (GRT) regime vary across the firm size distribution, the analysis is extended by partitioning the PSM-matched samples into fifteen even bins based on 2012 nominal gross receipts. This approach allows identification of heterogeneous treatment effects along the revenue distribution while maintaining comparability across firms with similar observable characteristics. The estimates are obtained from the two-way fixed effects model without the inverse Mills ratio correction, with separate regressions for each bin. The results are reported in [Table 2.19](#) and visualized in [Figure 2.5](#) and [Figure 2.6](#), each comprising three panels corresponding to gross receipts, 3-year growth, and 5-year growth outcomes.

For the early adopters versus late adopters comparison (Panel A), the estimates reveal distinct heterogeneity across the revenue spectrum. The first two bins, representing the smallest firms, display positive and statistically significant coefficients across all three outcomes. In particular, Bin 1 shows strong and significant effects for gross receipts (0.696, $p < 0.05$), 3-year growth (13.55, $p < 0.01$), and 5-year growth (20.78, $p < 0.05$), indicating that the smallest early adopters benefited most from GRT participation. However, from Bin 4 onward, the coefficients become negative and significant, suggesting that larger firms experienced lower growth rates relative to their late-adopting counterparts. The pattern becomes more pronounced from Bin 6 through Bin 15, where the estimated effects on growth are consistently negative and significant at conventional levels, indicating that for larger firms, early adoption was associated with reduced medium-term performance. This inverted-U pattern is consistent with the theoretical model's prediction that the benefits of simplified taxation diminish with firm size as the standardized expense rate becomes less aligned with actual cost structures.

For the early adopters versus all eligible firms comparison (Panel B), the pattern is broadly similar but more pronounced in magnitude and significance. The smallest firms (Bins 1-3) show large positive effects across all outcomes, with particularly strong gains in three-year and five-year growth for Bin 1 (18.77 and 30.01, both $p < 0.01$). In contrast, from Bin 6 onward, the coefficients for both growth measures turn sharply negative and remain statistically significant through the highest bins. Gross receipts effects are modestly positive up to Bin 11, after which they converge toward zero or become

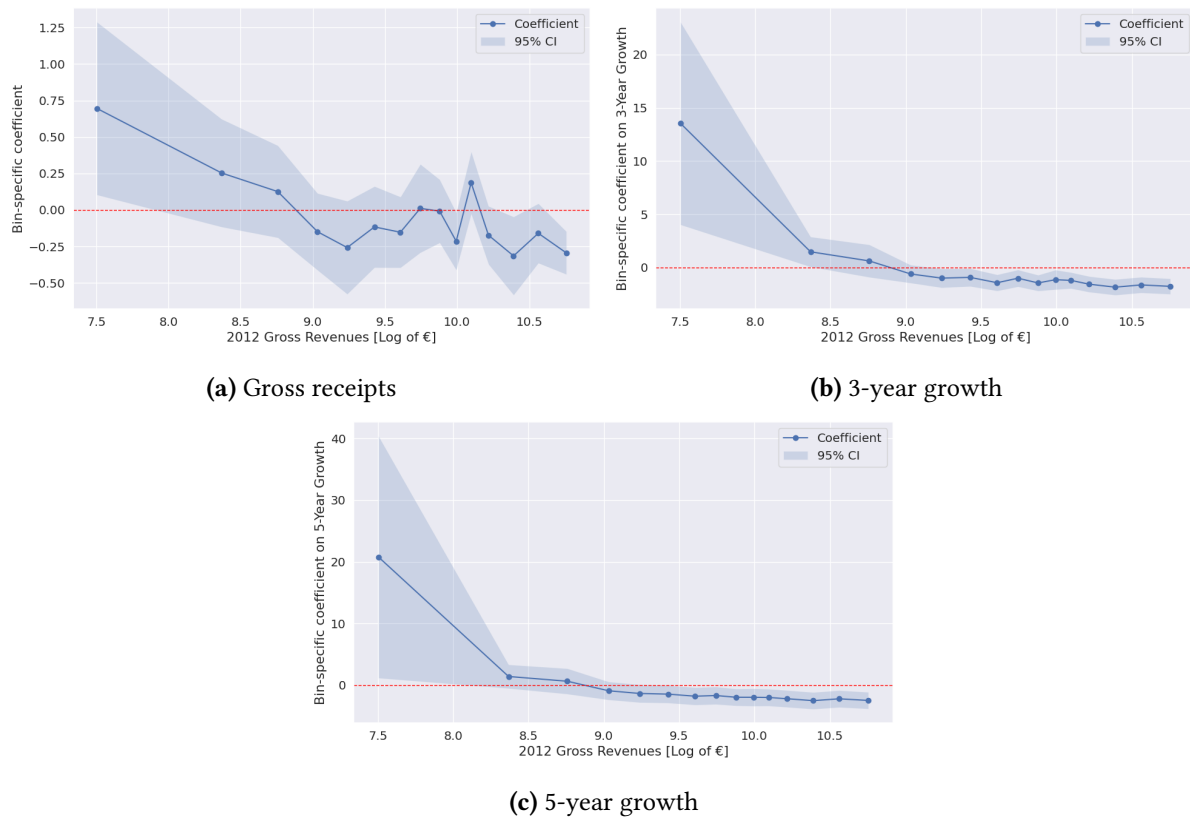


Figure 2.5: Bin-specific impacts after PSM between Early Adopters and Late Adopters. The 2012 gross receipts were split in 15 even bins based on nominal gross receipts.

slightly negative, mirroring the growth results. These patterns indicate that while micro-sized firms benefited from the administrative simplicity and lower compliance costs of GRT, larger firms experienced reduced growth performance, likely due to the inflexibility of the predetermined expense rate and reduced adaptability to changes in actual profitability.

The graphical results corroborate these findings. The bin-wise plots reveal a clear size-dependent relationship, with early adopters showing performance advantages concentrated among the smallest firms and losses emerging progressively with firm size. The positive effects at the lower end of the distribution align with the notion that GRT offered substantial relative gains for firms operating at or near the subsistence margin, where accounting costs constitute a larger share of expenses. Conversely, the consistent decline in coefficients across higher bins visually confirms the diminishing marginal benefit, and eventual cost, of GRT participation as firm size increases. When compared to the earlier TWFE, Heckman-corrected, and PSM analyses, these results clarify the source of the mixed average effects. The lack of overall significance in the earlier models reflects the aggregation of offsetting effects, positive for the smallest firms and negative for larger ones. By disaggregating the analysis across

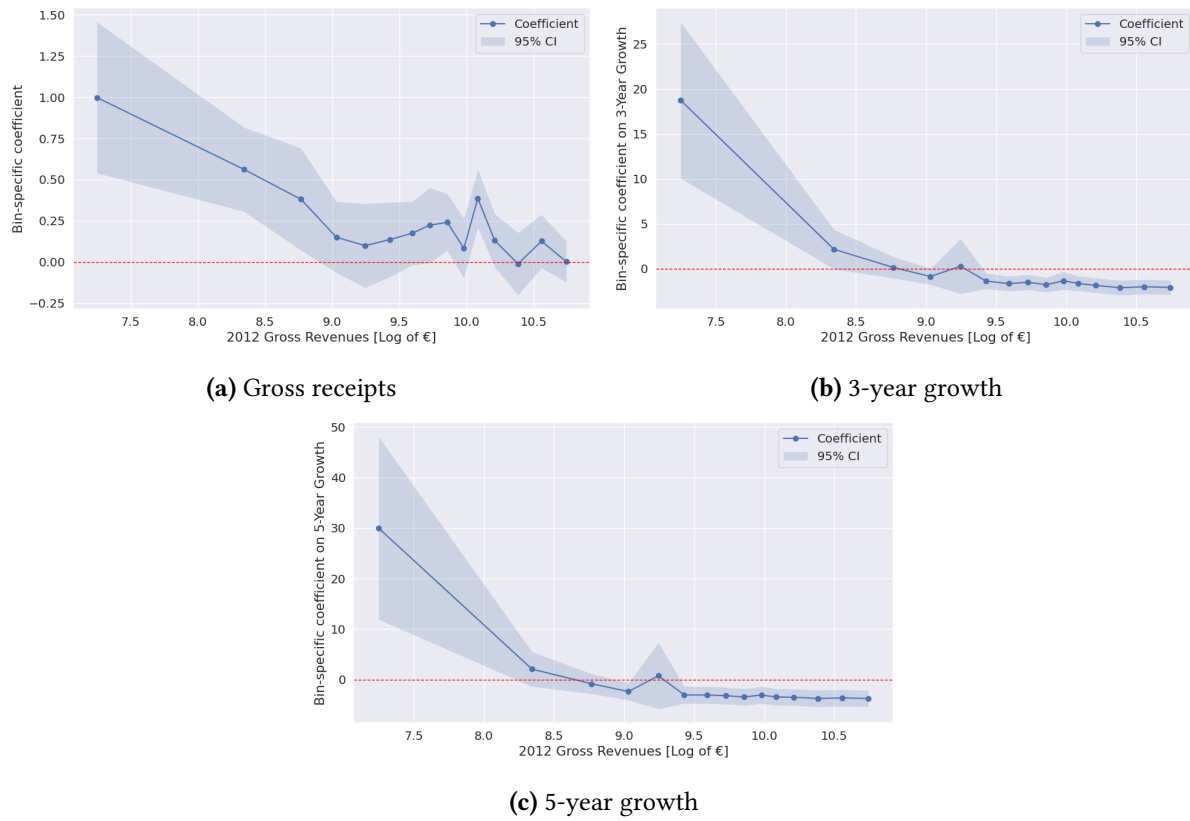


Figure 2.6: Bin-specific impacts after PSM between Early Adopters and Eligible firms. The 2012 gross receipts were split in 15 even bins based on nominal gross receipts.

bins, the heterogeneity becomes evident: the GRT regime benefited very small firms, consistent with its policy intent, but constrained growth among larger firms close to the eligibility threshold. Thus, the bin-based results reconcile the earlier findings by showing that the absence of a uniform treatment effect is not due to estimation noise but to true heterogeneity in the policy's impact across the firm size distribution.

2.5.4 Placebo Testing

To validate whether the estimated effects of GRT participation reflect true post-treatment changes rather than pre-existing trends or spurious correlations, the analysis conducts placebo tests using the pre-policy period 2010-2012. The actual reform occurred in 2013, thus the placebo treatment is artificially assigned to 2011 and 2012 and re-estimate the models exactly as in the baseline analyses. Any significant placebo effect prior to 2013 would indicate violation of the parallel trends assumption or the presence of anticipatory behavior, thus undermining the credibility of the causal interpretation of the main results.

Panel A of Table 2.14 compares Early Adopters to Late Adopters using the core TWFE from Section 2.5.3. Across both placebo years - i.e. 2011 and 2012, the placebo coefficients are small and statistically insignificant. In contrast, Panel B shows statistically significant positive placebo effects in both years, even before GRT existed. This confirms what earlier sections already suggested, comparisons to Eligible are confounded by systematic differences between the two groups. These firms are not comparable prior to treatment, validating the earlier claim that this specification is descriptive, not causal.

Panels C and D add the Inverse Mills Ratio for 2013 entry to correct for selection on unobservables. When comparing early adopters to late adopters in Panel C, the 2011 placebo remains insignificant, but the 2012 placebo turns positive and significant. This suggests that even after controlling for selection, some divergence begins just before the reform. However, this effect appears only in one year and only with IMR correction, indicating that selection dynamics accelerate shortly before adoption - a pattern consistent with firms preparing to enter. For early adopters versus all eligible in Panel D, the placebo effects are consistently large and significant in both years. This again confirms that the comparison between Early Adopters and Eligible reflects deep structural differences between groups rather than policy effects. Importantly, the IMR is significant and negative in all specifications, indicating strong selection bias even before the reform. This reinforces the earlier conclusion that selection correction is necessary but not sufficient if the control group is fundamentally different.

Panels E and F report the placebo tests using PSM-matched samples, which eliminate observable differences prior to estimation. In Panel E for Early Adopters vs Late Adopters, the 2011 placebo is insignificant without controls but becomes weakly significant with controls. However, the 2012 placebo returns to insignificance. These mixed but generally small effects suggest that after matching, early and late adopters are largely comparable in trends, supporting the credibility of the post-2013 results for this comparison. Most importantly, Panel F shows no significant placebo effects in any specification or year. Once observables are balanced through matching, even the previously problematic comparison between Early Adopters and Eligible exhibits parallel pre-trends. This implies that pre-treatment differences were driven primarily by observables rather than unobservables, validating the use of PSM as an effective robustness strategy.

Panel A: Core TWFE Early Adopters vs Late Adopters				
	2011 Placebo		2012 Placebo	
	(1)	(2)	(3)	(4)
Placebo	-0.020	0.398	-0.056	0.005
	(0.048)	(0.384)	(0.045)	(0.040)
Obs	8,338	3,502	8,338	3,502

R^2	0.000	0.033	0.000	0.033
Controls	-	✓	-	✓

Panel B: Core TWFE Early Adopters vs Eligible

	2011 Placebo		2012 Placebo	
	(1)	(2)	(3)	(4)
Placebo	0.257*** (0.024)	0.703** (0.293)	0.338*** (0.022)	0.291*** (0.023)
Controls	-	✓	-	✓
Obs	282,335	86,022	282,335	86,022
R^2	0.000	0.009	0.001	0.011
Controls	-	✓	-	✓

Panel C: IMR Corrected TWFE Early Adopters vs Late Adopters

	2011 Placebo		2012 Placebo	
	(1)	(2)	(3)	(4)
Placebo	0.310 (0.264)	5.817 (5.465)	1.002*** (0.253)	1.332*** (0.208)
IMR	-0.201 (0.158)	-2.863 (3.009)	-0.643*** (0.153)	-0.808*** (0.125)
Obs	8,338	3,502	8,338	3,502
R^2	0.000	0.034	0.004	0.058
Controls	-	✓	-	✓

Panel D: IMR Corrected TWFE Early Adopters vs Eligible

	2011 Placebo		2012 Placebo	
	(1)	(2)	(3)	(4)
Placebo	3.082*** (0.130)	6.793** (3.281)	4.924*** (0.132)	3.982*** (0.156)
IMR	-1.379***	-2.791*	-2.236***	-1.796***

	(0.062)	(1.479)	(0.063)	(0.073)
Obs	282,335	86,022	282,335	86,022
R^2	0.003	0.009	0.010	0.029
Controls	-	✓	-	✓

Panel E: PSM Early Adopters vs Late Adopter

	2011 Placebo		2012 Placebo	
	(1)	(2)	(3)	(4)
Placebo	0.013	0.639***	-0.084	0.016
	(0.064)	(0.208)	(0.060)	(0.039)
Obs	3,730	2,096	3,730	2,096
R^2	0.000	0.015	0.001	0.014
Controls	-	✓	-	✓

Panel F: PSM Early Adopters vs Eligible

	2011 Placebo		2012 Placebo	
	(1)	(2)	(3)	(4)
Placebo	-0.025	-0.515	-0.054	0.007
	(0.044)	(0.483)	(0.038)	(0.027)
Obs	8444	4720	8444	4720
R^2	0.000	0.024	0.000	0.023
Controls	-	✓	-	✓

Table 2.14: Placebo tests using 2011 and 2012 years as treatment introductions for 2010-2012 period. The regressions match core models in [Section 2.5](#) and all use firm and time fixed effects.

Robust standard errors in parentheses.

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

The placebo tests provide four key insights that validate the earlier empirical sections. **(1)** Core TWFE between Early and Late Adopters is likely unbiased. This supports the credibility of the main TWFE results and the bin-based heterogeneity findings within this comparison. **(2)** Early vs. Eligible is not causal without matching. Significant placebo effects in TWFE and IMR-corrected models are

observed, suggesting pre-existing differences drive results. This confirms why the positive or negative coefficients in those specifications are descriptive rather than causal. (3) PSM successfully restores comparability. After matching on observables, placebo effects disappear for both comparisons. This strengthens the reliability of the PSM-TWFE and bin-based results, especially the heterogeneous treatment effects across the distribution. Lastly, (4) Heckman correction detects selection on unobservables but not full comparability. IMR is significant in placebo tests, confirming selection existed before the policy. Thus, Heckman improves internal validity but cannot solve fundamentally incomparable groups without proper matching.

2.6 Discussion

The results paint a coherent and policy-relevant picture of the effects of the gross receipt taxation (GRT) regime. The evidence consistently shows that GRT achieved its intended objective of reducing administrative burdens and facilitating entry for the smallest firms, particularly sole proprietors and micro-enterprises with limited accounting capacity. These firms benefited from lower compliance costs, predictable tax liabilities, and the removal of bureaucratic barriers. Importantly, these gains translated into measurable and sustained improvements in performance and growth for the smallest segment of the firm distribution. This confirms that simplified taxation can serve as an effective tool for promoting entrepreneurship and formalization.

However, the benefits of GRT were not evenly distributed. For firms operating near the eligibility threshold, those with higher gross receipts and more complex cost structures, the regime introduced rigidity and reduced flexibility in responding to shocks. Rather than providing a smooth transition to standard taxation, the regime created a barrier to scaling, firms had incentives to remain artificially small in order to retain favorable tax treatment. In this sense, GRT acted as a short-term entrepreneurship stimulus but a long-term growth deterrent, with elevated rates of bankruptcy both short-term and long-term. It shifted fiscal risk from the government to the firm, stabilizing public revenues, but at the expense of firm resilience and upward mobility.

The credibility of these results is supported by the convergence of multiple empirical strategies. The interaction models show that entry decisions depend on the joint distribution of gross receipts and profit margins, as predicted by the theoretical framework. The regression discontinuity design reveals no discontinuities at the eligibility threshold, indicating that firms were not mechanically constrained by the rule but were making rational, profit-maximizing choices. The lack of FRD effects is not a failure of identification, but rather confirmation that the treatment is determined by firm heterogeneity rather than automatic eligibility. Two-way fixed effects estimates reinforce this view, showing no consistent

average treatment effect when comparing early to late adopters. The endogeneity of the comparison with all eligible firms is revealed by significant differences in pre-trends, and the Heckman selection correction confirms that unobserved characteristics drive selection into GRT.

Crucially, the propensity score matching (PSM) approach removes observable differences and restores comparability between treated and control groups. After matching, placebo tests show no differences in pre-policy periods, validating the parallel trends assumption and confirming that the post-2013 effects can be interpreted as causal for the early versus late comparison. The bin-based heterogeneity analysis further clarifies that the absence of a strong average effect is not the result of noise, but the aggregation of opposite effects, strong positive outcomes for the smallest firms and strong negative outcomes for larger firms near the cutoff. This pattern holds across revenue, short-term growth, and long-term growth, and remains consistent after multiple robustness checks. The placebo tests provide the final confirmation by demonstrating that the credible specifications do not detect treatment effects before the policy was introduced.

When discussing the long-term benefits of GRT, it is essential to focus on the bankruptcy rates. While the results presented show heterogeneous impacts on growth, when introducing bankruptcy rates, the GRT seems to be causing elevated levels of bankruptcy. The results are consistent with the theoretical model presented in [Section 2.3](#) as the firms find it optimal to default than continue the operations. Additionally, the lack of ability to freely switch between tax systems during the fiscal year, forces the firms to default to avoid financial harm, thus reaffirming theoretical discussion and the intuition.

In light of this evidence, the results are not only internally consistent but highly reliable. The combination of theoretical grounding, multiple identification strategies, balance diagnostics, selection correction, matching, heterogeneity analysis, and placebo testing creates a compelling and credible empirical narrative. Each method addresses a different threat to identification, and together they produce convergent findings that strongly support the central conclusion, GRT succeeds where firm size and cost structure align with its design, and fails where they do not.

The policy implications of this analysis are significant. Simplified tax regimes can be powerful tools for encouraging entrepreneurship, reducing informality, and enhancing compliance, but they must be designed with heterogeneity in mind. A uniform expense ratio and hard eligibility cutoff may work well for very small firms but are unsuitable for larger firms with variable expenses or growth ambitions. If policymakers aim to support scaling rather than permanent smallness, tax regimes must incorporate flexibility. Options such as graduated thresholds, sector-specific expense rates, or phased transitions into standard taxation could preserve the administrative benefits of simplicity while reducing the incentive to remain small and the risk of penalizing firms during downturns.

2.7 Conclusion

The paper examined the introduction of gross receipt taxation (GRT) and its effects on firm performance and growth. Using a comprehensive empirical strategy, including fuzzy regression discontinuity, two-way fixed effects, Heckman selection correction, propensity score matching, heterogeneity analysis across revenue bins, and placebo tests, the study provides a clear and internally consistent account of how firms respond to simplified taxation.

The evidence shows that GRT achieved its primary goal of lowering administrative burdens among the smallest firms. These firms experienced meaningful and sustained improvements in performance and growth, confirming that simplified taxation can successfully support micro-entrepreneurship. However, the benefits were not universal. For firms closer to the eligibility threshold, GRT introduced rigidity, increased exposure to downside risk, and discouraged scaling. Instead of providing a bridge to standard taxation, the regime created an incentive to remain small, thereby limiting long-term growth. The results highlight the central role of heterogeneity. Average effects mask a clear distributional pattern, GRT benefits very small, high-margin firms but imposes costs on larger firms with more variable expenses. Importantly, these conclusions are consistent across multiple identification strategies and validated by placebo tests. This confirms that the results are reliable and not driven by model choice or selection bias.

From a policy perspective, the findings suggest that simplified tax regimes can be effective when targeted precisely, but must be designed with flexibility to accommodate firm growth and sectoral differences. A one-size-fits-all structure may promote entry but can unintentionally trap firms at the lower end of the size distribution. Future reforms should therefore consider graduated eligibility thresholds, sector-specific expense assumptions, or smoother transitions into standard taxation to preserve the benefits of simplicity without penalizing expansion.

2.8 Appendix

2.8.1 Tables

Legal persons	
101	General partnership
102	Limited partnership
103	Limited liability company
104	Joint stock company
108	Commercial association of interest
112	Co-operative society (limited liability)
113	Co-operative society
115	Enterprise abroad
118	European economic interest grouping EEIG
141	Principal subsidiary of a foreign business entity
210	Fund
211	Mutual insurance company
213	Public fund
300	Public agency
316	Body, wider local community organizations
319	Local community, other narrower local communities
352	Public economy institution
354	Institution
355	Public institution
357	Community of institutions
358	Chamber
359	Member of university
360	Chamber of commerce and industry
361	Public research institute
362	Community of apartment owners

401	Foundation
402	Agricultural collective
403	Other collective
404	Youth council
405	National community
406	Student organization
407	Non-governmental organization
451	Political party
452	Trade union
453	Association, federation of associations
456	Other collectives
458	Religious community and similar religious organizations
702	Building society
704	Savings and loan service
706	Full liability
708	Agricultural cooperative
713	Privately owned institution
715	Socially owned enterprise
799	Other forms of business subjects
808	LKAU foreign branch

Natural persons

142	Individual private entrepreneurs
143	Registered natural person letting rooms
144	Professional sportsman
145	Free-lance journalist
146	Bailiff
147	Private sports worker
148	Marine fisherman
151	Notary public
152	Registered lawyer

153	Registered doctor, dentist, private medical professional
154	Registered pharmacist
156	Registered free-lance cultural worker
157	Registered private contractor - researcher
158	Registered investigator
160	Other natural persons
162	Person performing supplementary work of other persons
163	Person performing supplementary farm activities
166	Private teacher or educator
167	Unknown
802	LKAU branch office s.p.

Other

999	Unknown
-----	---------

Table 2.15: Legal and Natural persons classification by code set forth by the government of Republic of Slovenia

Industry	2-Digit NACE Rev.2 Code
Agriculture	1 - 5
Industry	5 - 39
Construction	41 - 43
Trade	45 - 47
Transport	49 - 53
Hotels and Restaurants (Accommodation)	55 - 56
Information and Communication	58 - 63
Finance	64 - 66
Professional Services	68 - 82
Public Administration	84
Education	85
Health	86 - 88
Other Services	90 - 99

Table 2.16: Definition of the industries and its corresponding 2-digit NACE Rev.2 codes as presented in [Figure 2.1](#).

Variables	Definitions
Gross Receipts	Sum of all amounts the organization received from all sources during its annual accounting period, without subtracting any costs or expenses(<i>Gross Receipts</i> , n.d.)
Gross Expenses	Sum of all tax recognized expenses of the firm in the fiscal year
Total Assets	The book value of total assets reported at the end of a fiscal year
Size	Natural log of total assets of the firm at the end of the accounting year
Liquidity	Cash over total sales
Return on Assets	Profit (loss) over total assets
Cash	Natural log of cash and cash equivalents
Net Working Capital	Log of (current assets - current liabilities)
Collateral	Total fixed assets over total assets
Loan	Natural log of loans
Capital Expenditure / TA	Capital expenditure over total assets
Profit Margin	$\max \{(\text{Gross Receipts} - \text{Gross Expenses}) / \text{Gross Receipts}, 0\}$

Table 2.17: List of variables used in the regression and their corresponding definitions.

Panel A: Early Adopters vs Late Adopters						
	Before PSM			After PSM		
	Mean (T)	Mean (C)	Diff	Mean (T)	Mean (C)	Diff
Gross Receipts (€)	9.54	9.40	0.15***	9.60	9.57	0.03
Profit Margin (%)	31.86	23.59	8.27***	28.42	28.34	0.08
Size	8.66	8.61	0.05	8.64	8.61	0.02
Liquidity	0.59	0.30	0.30*	0.24	0.28	-0.04
Net Working Capital	6.24	6.18	0.06	6.00	5.80	0.20
Cash	6.84	6.60	0.24**	6.79	6.80	-0.01
ROA	9.22	13.98	-4.75	1.69	10.75	-9.06
Propensity Score	0.75	0.72	0.03***	0.74	0.74	-0.00

Panel B: Early Adopters vs All Eligible						
	Before PSM			After PSM		
	Mean (T)	Mean (C)	Diff	Mean (T)	Mean (C)	Diff
Gross Receipts (€)	9.54	9.54	0.01	9.54	9.54	0.01
Profit Margin (%)	31.86	12.71	19.15***	31.86	31.78	0.08
Size	8.66	9.02	-0.36***	8.66	8.56	0.10*
Liquidity	0.59	0.85	-0.25	0.59	0.80	-0.20
Net Working Capital	6.24	4.54	1.70***	6.24	6.06	0.18
Cash	6.84	6.42	0.42***	6.84	6.74	0.10
ROA	9.22	1.11	8.12	9.22	9.15	0.07
Propensity Score	0.10	0.03	0.06***	0.10	0.10	-0.00

Table 2.18: Descriptive statistics before and after propensity score matching with corresponding t-tests.

*** p<0.01, ** p<0.05, * p<0.1

Panel A: Early Adopters vs Late Adopters			
	Gross Receipts	3-Year Growth	5-Year Growth
	(1)	(2)	(3)
Bin 1	0.696** (0.303)	13.548*** (4.864)	20.781** (10.025)
Bin 2	0.254 (0.189)	1.477** (0.724)	1.397 (0.988)
Bin 3	0.126 (0.161)	0.620 (0.783)	0.626 (1.062)
Bin 4	-0.149 (0.135)	-0.606 (0.435)	-0.924 (0.752)
Bin 5	-0.258 (0.163)	-0.996** (0.459)	-1.357* (0.750)
Bin 6	-0.116 (0.142)	-0.925** (0.432)	-1.465** (0.735)
Bin 7	-0.153 (0.124)	-1.428*** (0.393)	-1.802** (0.727)
Bin 8	0.011 (0.155)	-1.006** (0.408)	-1.694** (0.728)
Bin 9	-0.008 (0.111)	-1.449*** (0.385)	-1.974*** (0.704)
Bin 10	-0.215** (0.101)	-1.144** (0.471)	-1.977*** (0.729)
Bin 11	0.187* (0.110)	-1.208*** (0.389)	-2.010*** (0.705)
Bin 12	-0.173* (0.102)	-1.564*** (0.380)	-2.214*** (0.705)
Bin 13	-0.315** (0.137)	-1.846*** (0.381)	-2.524*** (0.700)
Bin 14	-0.160	-1.634***	-2.227***

	(0.104)	(0.381)	(0.706)
Bin 15	-0.295***	-1.769***	-2.487***
	(0.075)	(0.375)	(0.695)
Obs	5425	1374	1203
R^2	0.022	0.105	0.074
Firm FE	✓	✓	✓
Time FE	✓	✓	✓

Panel B: Early Adopters vs All Eligible

	Gross Receipts	3-Year Growth	5-Year Growth
	(1)	(2)	(3)
Bin 1	0.999***	18.768***	30.011***
	(0.234)	(4.439)	(9.249)
Bin 2	0.562***	2.170*	2.058
	(0.131)	(1.129)	(1.775)
Bin 3	0.381**	0.145	-0.836
	(0.158)	(0.613)	(1.022)
Bin 4	0.150	-0.853*	-2.382***
	(0.110)	(0.460)	(0.873)
Bin 5	0.099	0.289	0.744
	(0.130)	(1.568)	(3.372)
Bin 6	0.136	-1.368***	-3.057***
	(0.116)	(0.446)	(0.865)
Bin 7	0.174*	-1.654***	-3.072***
	(0.099)	(0.427)	(0.866)
Bin 8	0.223*	-1.483***	-3.202***
	(0.116)	(0.434)	(0.861)
Bin 9	0.241***	-1.782***	-3.464***
	(0.088)	(0.422)	(0.851)
Bin 10	0.083	-1.320***	-3.074***

	(0.093)	(0.511)	(0.876)
Bin 11	0.387***	-1.626***	-3.461***
	(0.091)	(0.424)	(0.850)
Bin 12	0.131	-1.847***	-3.515***
	(0.083)	(0.419)	(0.850)
Bin 13	-0.011	-2.103***	-3.742***
	(0.097)	(0.420)	(0.851)
Bin 14	0.127	-1.995***	-3.638***
	(0.083)	(0.419)	(0.850)
Bin 15	0.002	-2.065***	-3.753***
	(0.064)	(0.416)	(0.847)
Obs	11762	2853	2476
R^2	0.015	0.067	0.051
Firm FE	✓	✓	✓
Time FE	✓	✓	✓

Table 2.19: Bin-specific estimations on gross receipts (column 1), 3-year growth (column 2) and 5-year growth (column 3) after propensity score matching.

Robust standard errors in parentheses.

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

2.8.2 Graphs

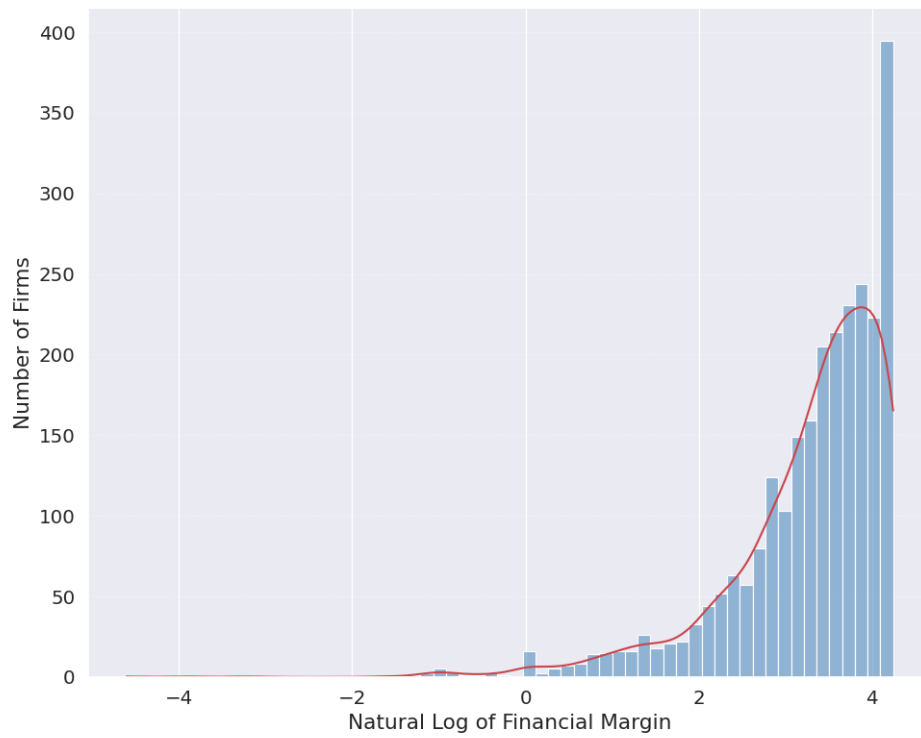


Figure 2.7: The distribution of log value of financial margins where losses as kept in nominal terms - i.e. as opposed to being set to 0. Data is winsorized at 1% in both tails.

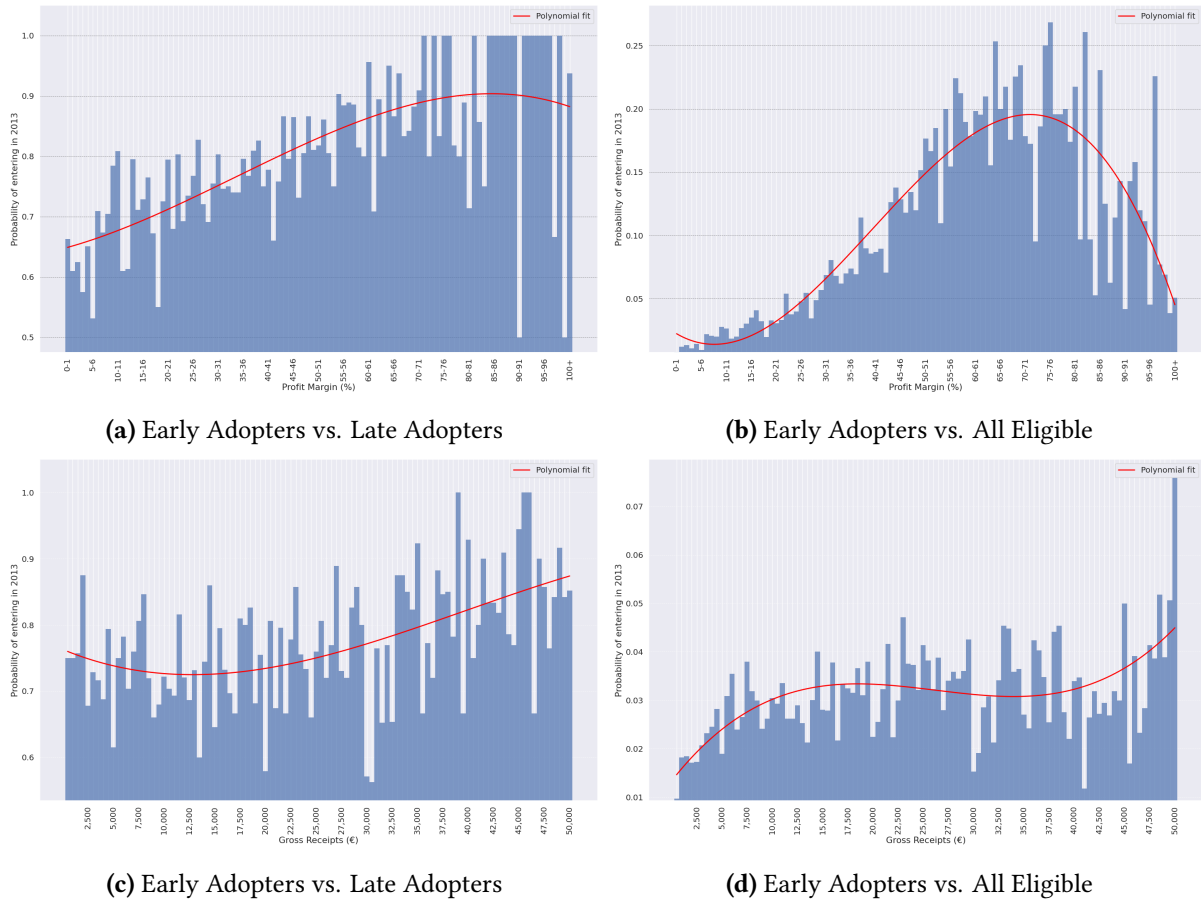
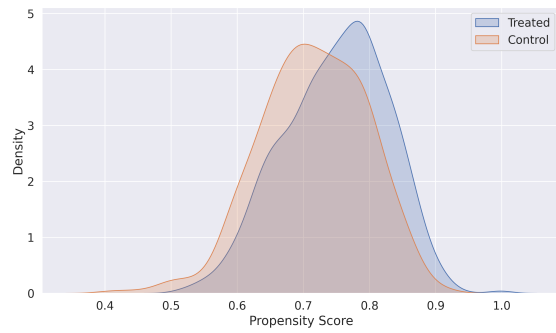
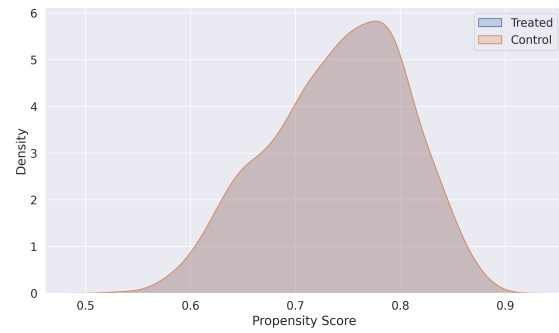


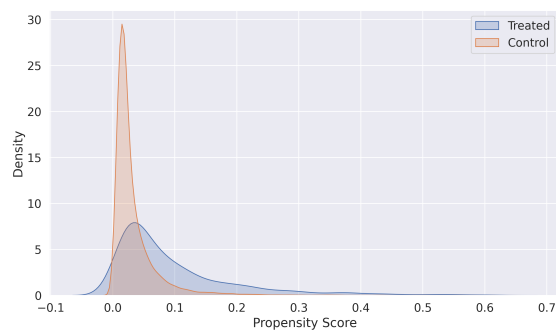
Figure 2.8: The probability of entrance in GRT in 2013 based on 2012 values of profit margin (top row) and gross receipts (bottom row) by bins. The polynomial fit is of a 3rd degree. Groups consist of Early Adopters, Late Adopters and All Eligible, with the latter using all firms eligible for entry in 2013.



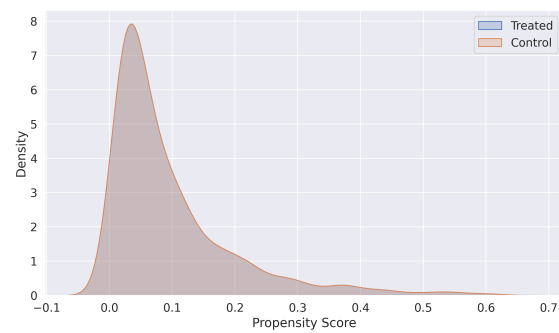
(a) Early Adopters vs Late Adopters - Before PSM



(b) Early Adopters vs Late Adopters - After PSM



(c) Early Adopters vs Eligible - Before PSM



(d) Early Adopters vs Eligible - After PSM

Figure 2.9: Distribution of propensity scores before and after propensity score matching when matching Early Adopters and Late Adopters.

Chapter 3

Digital Transformation and Bank Risk in the Public Emergency

Yuchen Bian¹, Kristjan Markovc, Haifeng Gu², Shuiwen Gao³

Public emergencies significantly impact the stability of the financial system. Leveraging the first COVID-19 outbreak in Hubei Province, China, as a case study, this paper employs a difference-in-difference (DiD) model to investigate these effects. The analysis demonstrates the impact of public emergencies on bank risk and explores the role of digital transformation in mitigating these risks. The findings indicate that public emergencies increase bank risk; however, digital transformation can mitigate challenges to credit operations, thereby reducing this risk. Furthermore, the study reveals that only banks' passive risk-taking is affected by public emergencies, while active risk-taking remains unaffected. This paper advocates for commercial banks to accelerate digital transformation initiatives to control and reduce risks during public emergencies.

JEL Classification — G21, O32, I18

Keywords — Bank risk-taking; public emergency; digital transformation; COVID-19 pandemic

¹Donghua University, China

²Donghua University, China

³Donghua University, China

3.1 Introduction

Since the beginning of the 21st century, numerous public emergencies have occurred worldwide, including the Indonesian tsunami in 2004, the H1N1 pandemic in 2009, the Fukushima nuclear power plant leakage in 2011, the Ebola virus epidemic in 2014, the volcanic eruption in Tonga in 2022, and the ongoing COVID-19 crisis. These events pose significant threats to public safety and social stability, leading to a surge in literature exploring their impacts. Some studies investigate the effects of public emergencies on various aspects of enterprise activities, such as financing, production, transportation, and distribution (Addoum, Ng, & Ortiz-Bobea, 2020; Bartik et al., 2020; Clampit, Lorenz, Gamble, & Lee, 2022; Dessaint & Matray, 2017; De Vito & Gómez, 2020; Fasth, Åkerman, Elliot, & Hilmersson, 2023; Takeda, Truong, & Sonobe, 2022; Z. Wang, Dong, & Liu, 2022; D. Zhang & Sogn-Grundvåg, 2022). These impacts on economic activities can easily transmit to capital markets through supply chains and capital chains (Lang, Hu, Corbet, & Hou, 2024; McTier, Tse, & Wald, 2013; Z. Wang et al., 2022), thereby threatening regional and even global financial security. For instance, in 2020, as COVID-19 spread globally, the US stock market experienced four circuit breakers within two weeks, and stock markets in other countries also suffered significant losses.

The banking sector, being the core of the financial system, is crucial to financial stability, and the insolvency risk of commercial banks is a direct concern. During the COVID-19 pandemic, many bank branches closed or reduced their business hours (Yan & Jia, 2022), significantly affecting their credit operations. Concurrently, numerous enterprises suspended their activities (Dai et al., 2021), increasing default risk (Doran, Bădîrcea, & Manta, 2022; Q. Liu, Shi, Tse, & Zhang, 2023). According to data from the China Banking Regulatory Commission (CBRC), by the end of 2020, the balance of non-performing loans in China's banking system was RMB 3.74 trillion, with the non-performing loan ratio rising to 1.84%, the second-highest value since the 2008 financial crisis.

Public emergencies compel companies to adopt digital transformation to mitigate disruptive effects (Roffia & Mola, 2022). Recent advancements in fintech, such as big data, cloud computing, and artificial intelligence, have significantly accelerated banks' digital transformation. Digital technology-based risk assessment systems enhance the efficiency of default risk evaluations by analyzing customers' digital footprints on technology platforms. Additionally, digital technology applications in finance eliminate geographical restrictions, binding customers to mobile terminals and addressing traditional finance's limitations, such as slow speed, narrow coverage, and high cost.

This paper makes several significant contributions to the existing literature. First, it enhances the understanding of the consequences of public emergencies. Previous research has primarily focused on the impact of public emergencies on enterprise activities, with some studies examining their effects on

commercial banks, mainly in terms of credit supply (B. Chen, Zhang, & Saydaliev, 2022; Silva, de Souza, Guerra, & Tabak, 2023; Y. Zhang, Xing, & Guo, 2023). However, there is limited knowledge about their impact on bank risk. This paper addresses this gap by revealing the effects of public emergencies on bank risk.

Second, it advances the literature on the determinants of bank risk. Due to data limitations, directly assessing the risk effects of public emergencies has been challenging. Unlike existing studies that explore the relationship between public emergencies and bank risk (Duan et al., 2021; Setiyono & Munawaroh, 2024), this paper establishes a quasi-natural experiment using the COVID-19 outbreak in Hubei Province, China. By employing the difference-in-difference (DiD) model for empirical analysis, the study isolates the net effect of public emergencies on bank risk, as Hubei was the initial outbreak site, and no preventive measures were in place beforehand.

Third, it contributes to the literature on the effects of digital transformation. Previous research has demonstrated the positive impact of digital transformation on alleviating financial shortages (Y. Zhang et al., 2023), enhancing innovation capabilities (J. Yang, Zhang, & Pei, 2025), and improving performance (Mishrif & Khan, 2023; Yu & Shao, 2025). Additionally, studies indicate that digital transformation helps mitigate credit risk (Z. Chen, Li, Wang, & Wu, 2023; F. Yang & Masron, 2024). However, its role during public emergencies has not been verified. This paper integrates digital transformation, public emergencies, and bank risk-taking into a single framework, contributing to the growing body of research by showing that digital transformation can mitigate bank risk during public emergencies.

The paper is organized as follows: Section 3.2 develops the hypotheses, Section 3.3 outlines the methodology, Section 3.4 presents the empirical results, and Section 3.6 concludes the paper.

3.2 Hypothesis Development

Public emergencies disrupt the normal functioning of credit markets by heightening uncertainty, stressing borrowers' cash flows, and impairing banks' operations. These shocks can raise both measured portfolio risk - e.g. through deteriorating asset quality and funding fragility, and risk-taking on the extensive margin via precautionary balance-sheet adjustments. At the same time, banks' digital capabilities, such as end-to-end remote service delivery, real-time monitoring, and automated workflows, may cushion these effects by sustaining credit operations and improving the timeliness and precision of risk assessment. Guided by this logic, we formulate two hypotheses that the following subsections develop and test.

3.2.1 The impact of public emergencies on bank risk

Public emergencies are unexpected occurrences that endanger public safety, resulting in substantial casualties, property damage, ecological harm, and profound social disruption. The COVID-19 pandemic exemplifies such an emergency, having disrupted economic progress and threatened financial security, including the stability of commercial banks.

Firstly, the disruption of transportation and communication services caused by public emergencies impairs the normal functioning of the economic system (Tirachini & Cats, 2020). Businesses face widespread shutdowns, production halts, and even permanent closures, leading to increased credit defaults (Bartik et al., 2020). Some bank branches were forced to cease operations (Yan & Jia, 2022), making it difficult for banks to function normally and significantly reducing their business volume. The scarcity of highly creditworthy firms diminishes their demand for credit, leading to increased competition for bank loans. Existing studies link increased competition to lower franchise values, prompting banks to hold riskier assets (Jiménez, Lopez, & Saurina, 2013) and reduce interest payments from firms, which heightens banks' risk-taking (Martinez-Miera & Repullo, 2010). Additionally, competition can increase loan concentration, raising the probability of default (Boyd & De Nicolo, 2005).

Moreover, public emergencies can lead to a reduction in deposit size and an increase in operational costs (Gao, Li, & Wen, 2023; Xie, Mirza, Umar, & Ji, 2024). The traditional banking business model of earning spreads is constrained, increasing liquidity risk by reducing the interest income and cash flow available to service debt. A decrease in banks' ability to attract deposits can expand the scale of interbank business, increasing the complexity of bank operations and complicating financial regulation. More seriously, an increase in interbank business scale magnifies banking leverage. If a liquidity crisis occurs in individual banks, the liquidity risk may spread throughout the entire banking industry, potentially triggering widespread bank failures (Freixas, Parigi, & Rochet, 2000).

Secondly, public emergencies can induce panic in the capital market. Cross-border capital flows, particularly short-term investments, are highly sensitive to market conditions (Bacchetta, Cordonier, & Merrouche, 2023). Such emergencies often trigger capital inflows or outflows. For instance, during the COVID-19 pandemic, there was significant capital flight from China. However, once the epidemic was under control, China became a safe haven for capital. Information spillovers and the "herd effect" among fund management companies (Brown & Wu, 2016) exacerbate this volatility. Homogenized investment strategies lead to herd and stampede behaviors. Consequently, market panic during public emergencies can cause a sharp decline in asset prices, reducing the return on banks' securities investments and destabilizing the returns on their financial products.

Additionally, a downturn in real estate prices increases the probability of default by real estate companies. The decline in the value of fixed asset collateral also heightens credit risk. Market stabilization

often relies on monetary policy regulation (Brzoza-Brzezina, Kolasa, & Makarski, 2021). Governments typically employ quantitative easing to replenish market liquidity during shocks, which can further reduce bank spreads and profitability. Simultaneously, accommodative monetary policies can increase bank leverage and corporate default risk, thereby escalating bank risk (Delis & Kouretas, 2011; Dell’Ar-
iccia, Laeven, & Marquez, 2014). Therefore, we propose the following hypothesis:

Hypothesis 1: Public emergencies can increase bank risk.

3.2.2 The role of digital transformation in public emergencies

The development of digital technologies has led to significant technological spillovers, driving the digital transformation of enterprises (Cappa, Oriani, Peruffo, & McCarthy, 2021; Naimi-Sadigh, Asgari, & Rabiei, 2022). Capital financing, a core function of commercial banks, is being reshaped by the emergence of mobile payments, network credit, and other digital financial services. These innovations offer crucial spatial and temporal advantages in risk prevention during public emergencies.

Firstly, public emergencies disrupt essential services such as transportation and communication, causing inconvenience for bank operations. During these times, internet finance can diminish the role of commercial banks as financial intermediaries, creating a financial disintermediation effect (Z. Chen, Li, & He, 2020). Digital transformation allows banks to connect with users at near-zero cost by leveraging the long-tail effect of digital platforms (Bian & Gu, 2024). By integrating e-commerce, social media, and other services into their ecosystem, banks can increase customer engagement (Levy, 2022). More users now access financial services directly from mobile devices, significantly enhancing service precision (X. Liu, Chau, Liu, & Huang, 2023). This enables banks to extend credit to more small and medium-sized enterprises (SMEs) and offer unsecured loans (Jagtiani & Lemieux, 2019). During the COVID-19 pandemic, visits to bank physical branches decreased significantly, while visits to their websites increased (Kwan, Lin, Pursiainen, & Tai, 2024). Thus, digital transformation addresses the challenge of customer acquisition during public emergencies. Additionally, it enhances banks’ business acquisition capabilities and interest income, thereby improving performance (Doran et al., 2022).

Secondly, digital transformation reduces information asymmetry in credit transactions (Yao & Song, 2023). Traditional credit assessments rely on past financial statements or credit levels, which may not suffice for customers with short credit histories (Bian & Gu, 2024). During public emergencies, traditional credit assessment becomes more challenging, increasing the risk of adverse selection. The “big data + cloud computing” risk assessment model provides more timely insights. Through digital transformation, banks can establish technology platforms for real-time information sharing. Using customer

digital footprints and machine learning algorithms, banks can dynamically assess credit risk (F. Yang & Masron, 2024). Thus, digital transformation mitigates information asymmetry and improves risk control during public emergencies.

Thirdly, digital transformation reduces banks' operational costs (Khattak et al., 2023). Public emergencies can disrupt the regular operations of bank branches, leading to resource wastage. Digital transformation allows services to transition from offline to online, reducing reliance on physical branches and mitigating the impact on credit operations. This shift also shortens loan processing times, lowers operational costs, and extends financial services to remote areas, enhancing refinancing efficiency (Fuster, Plosser, Schnabl, & Vickery, 2019). Moreover, digital transformation promotes digital integration with technology companies (Drasch, Schweizer, & Urbach, 2018; Y. Li, Spigt, & Swinkels, 2017). Establishing information-sharing platforms helps regulate bank competition and mitigate the impact of loan competition on bank risk-taking during public emergencies. Collaborative competition among banks improves market share stability in the banking sector. Thus, we propose the following hypothesis:

Hypothesis 2: Digital transformation mitigates the impact of public emergencies on bank risk-taking.

3.3 Methodology

This section outlines the empirical strategy used to test our hypotheses. We first describe the construction of the dataset, including sample selection, variable definitions, and key transformations. We then present the econometric specification, detailing the identification strategy, estimation framework, and inference procedures. Throughout, we emphasize transparency in measurement, the alignment between theory and variables, and robustness to alternative modeling choices.

3.3.1 Data and Sample

To assess the impact of public emergencies on bank risk, this paper conducts a quasi-natural experiment centered on the COVID-19 outbreak in Hubei Province, China, in 2020. Hubei Province was selected as the study site due to it being the initial location of the COVID-19 outbreak. Prior to 2020, neither the government nor financial institutions had prepared for a pandemic, thereby reducing the potential influence of other policies on the empirical results. Additionally, China was among the earliest countries to promote the development of the digital economy, and its robust digital infrastructure created

favorable conditions for banks to implement digital transformation strategies.

The study period includes the years 2015 to 2019 as the comparison period, during which no public emergencies occurred. The sample consists of 195 commercial banks registered in China, comprising 6 state-owned banks, 12 joint-stock banks, 129 urban commercial banks, and 48 rural commercial banks. The assets of the sampled banks represent over 90% of the total assets in China's banking system, ensuring comprehensive coverage.

Bank-level data were obtained from Wind Information Co., Ltd. The Institute of Digital Finance at Peking University provided the bank digital transformation index. Information on bank branches was sourced from the China Banking Regulatory Commission (CBRC).

3.3.2 Econometric specification

To test our hypotheses, we employ the Difference-in-Differences (DiD) method to conduct the empirical analysis. The DiD method effectively reduces endogeneity interference. Rather than merely comparing the average change in risk, it assesses the pre-existing differences between the treatment and control groups before public emergencies and tracks the changes over time without any intervention. This approach allows for an unbiased estimation of the impact of public emergencies on bank risk. Specifically, we estimate the following regression Eq.27 to analyze this impact.

$$Risk_{i,t} = \alpha_0 + \alpha_1 Treat_i * Post_t + \sum_k \alpha_k X_{k,i,t-1} + \eta_t + \mu_i + \varepsilon_{i,t} \quad (\text{Eq.27})$$

where $Risk_{i,t}$ represents the risk of bank i in year t . Following Laeven and Levine (2009), we measured bank risk by Z-score. This index applies to unlisted financial institutions and has been widely used in empirical analysis. It is calculated as in Eq.28.

$$Z_{score} = \ln \frac{(ROA_{i,t} + CAR_{i,t})}{\sum (ROA_{i,t})} \quad (\text{Eq.28})$$

where $ROA_{i,t}$ is the return on assets, $CAR_{i,t}$ is the capital-to-assets ratio of bank, $\sum (ROA_{i,t})$ is the three-year rolling variance of $ROA_{i,t}$. A greater Z-score reveals lower exposure to insolvency risk.

We set the banks with branches in Hubei Province as the treated group, and the dummy variable $Treat_i$ value is 1; Set the other banks as the control group with $Treat_i$ as 0. $Post_t$ is the dummy variable of public emergency, taking a value of 1 in year 2020 and 0 in otherwise. $Treat_i * Post_t$ represents the primary explanatory variable of public emergency. For possible time effects and unchanging over time individual characteristics that may bias the estimates, year fixed effects η_t and bank fixed effects μ_i are included in the regression model. Lastly, $\varepsilon_{i,t}$ is the error term.

$X_{i,t-1}$ is a vector of bank-level control variables - i.e. profitability is measured by the return on equity; size is measured by the natural logarithm of total assets; cost is measured by the cost-income ratio; profit structure is measured by the percentage of non-interest income; net interest margin is measured by the net interest income on total assets; capital structure is measured by the asset-liability ratio.

Furthermore, to verify the moderating effect of digital transformation on the relationship between public emergency and bank risk, we add the interaction terms for digital transformation and the explanatory variables to the basic model. The extended regression model is shown in Eq.29.

$$Risk_{i,t} = \gamma_0 + \gamma_1 Treat_i * Post_t + \gamma_2 DT_{i,t-1} + \gamma_3 DT_{i,t-1} * Treat_i * Post_t + \sum_k \gamma_k X_{k,i,t-1} + \eta_t + \mu_i + \varepsilon_{i,t} \quad (Eq.29)$$

where $DT_{i,t}$ represents the digital transformation of bank i in year t , and we measure it using the commercial bank digital transformation index (“DT Index”) developed by Peking University. This index utilizes principal component analysis to synthesize three sub-indicators: management digitization, business digitization, and strategy digitization. These sub-indicators are then combined using a weighted average method to create the digital transformation score (Xie & Wang, 2023).

Management digitization is determined by factors such as the bank’s digital structure, digital talent, and digital cooperation, including whether the bank has established a fintech subsidiary, partnered with external tech companies, or the proportion of directors with IT backgrounds.

Business digitization is evaluated based on a bank’s digital business channels, digital products, and investment in digital technology research and development. This includes the launch of mobile banking applications, the number of digital patents, internet credit offerings, and e-commerce initiatives.

Strategy digitization is assessed through text mining analysis. Machine learning methods identify 124 keywords related to digital technologies such as artificial intelligence, blockchain, cloud computing, big data, online services, and mobility in the bank’s annual report. The frequency of these keyword occurrences measures the degree of strategy digitization.

Figure 3.1 illustrate the digital transformation trends in China’s banking sector. Figure 3.1a depicts the rapid pace of digital transformation in recent years. Figure 3.1b compares digital transformation trends among different types of banks in China, indicating that state-owned and joint-stock banks have progressed faster than urban and rural commercial banks.

Table 3.9 provides the definitions of the primary variables. Table 3.1 presents the descriptive statistics of these variables. Specifically, Panel A includes the descriptive statistics for the entire sample, while Panels B and C present the statistics for the treatment and control groups, respectively. Table 3.10 reports the correlation test results for the main variables, indicating a slight correlation coefficient between the explanatory variables. Additionally, the Variance Inflation Factor (VIF) values are

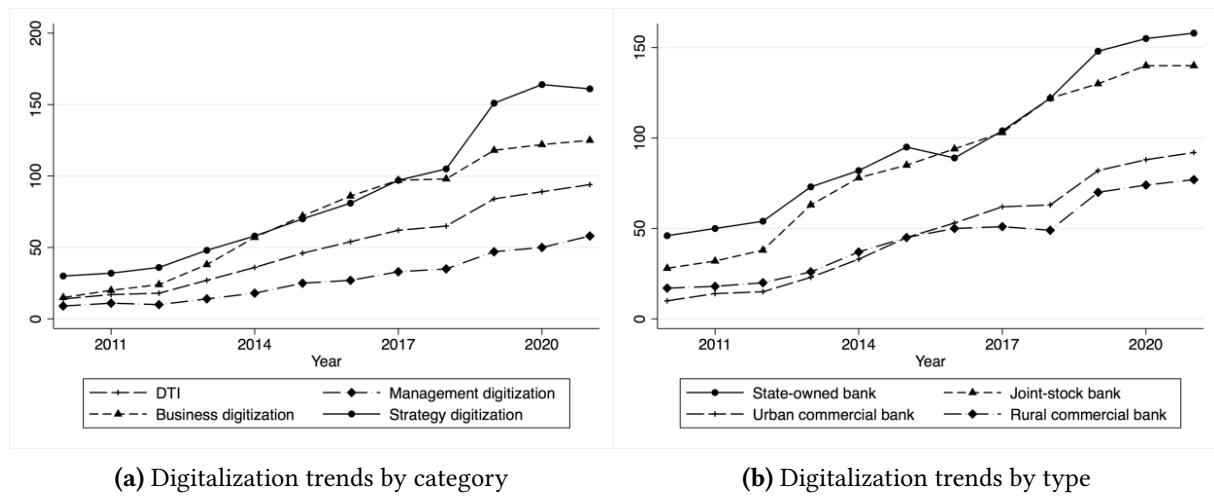


Figure 3.1: Digital transformation trends of commercial banks in China

all less than 5, suggesting no severe multicollinearity among the variables.

Panel A: Full sample

	Mean	SD	Min	25	50	75	Max	Obs.
Bank risk	4.5134	0.8822	2.8977	3.8417	4.4672	5.1120	6.3557	1,006
DT	3.9224	0.7516	1.0986	3.6636	4.0604	4.4308	4.9698	1,006
Profitability	11.6918	5.0858	1.5462	8.7434	11.6486	14.8566	25.2120	1,006
Size	26.0239	1.5367	23.3876	24.9830	25.7607	26.6629	29.8840	1,006
Cost	35.7955	10.1066	22.3000	29.5800	33.4650	38.8100	69.8178	1,006
Profit structure	24.9312	18.1061	0.6782	10.7371	20.7963	35.2171	68.3832	1,006
Net interest margin	2.4101	0.7824	1.0308	1.8500	2.3100	2.8600	4.4700	1,006
Capital structure	92.2108	2.0078	84.1928	91.4069	92.5218	93.5563	95.1245	1,006

Panel B: Treated sample

	Mean	SD	Min	25	50	75	Max	Obs.
Bank risk	4.8342	0.8066	3.1738	4.2566	4.8124	5.4301	6.3557	168
DT	4.3767	0.6160	1.0986	4.1190	4.5539	4.7999	4.9698	168
Profitability	11.9054	4.1701	1.5462	9.3332	11.6537	14.8330	20.5976	168
Size	28.1861	1.6448	24.8440	26.4574	28.7615	29.6169	29.8840	168
Cost	35.0949	11.5902	22.3000	27.6850	31.0650	35.8620	69.8178	168
Profit structure	28.4910	11.5044	0.6782	22.2137	28.6422	34.8696	68.3832	168

Net interest margin	2.1642	0.5071	1.0308	1.8200	2.2000	2.4679	4.1242	168
Capital structure	92.3503	1.6783	86.8651	91.5035	92.4308	93.5323	95.1245	168

Panel C: Control sample

	Mean	SD	Min	25	50	75	Max	Obs.
Bank risk	4.4491	0.8831	2.8977	3.7456	4.4145	5.0261	6.3557	838
DT	3.8313	0.7435	1.0986	3.5835	3.9703	4.3175	4.9698	838
Profitability	11.6490	5.2514	1.5462	8.4754	11.6486	14.8566	25.2120	838
Size	25.5904	1.0812	23.3876	24.8302	25.5591	26.2847	28.6379	838
Cost	35.9359	9.7839	22.3000	30.0400	33.8350	39.1200	69.8178	838
Profit structure	24.2175	19.0832	0.6782	9.5711	17.9024	35.6538	68.3832	838
Net interest margin	2.4594	0.8180	1.0308	1.8618	2.3900	2.9575	4.4700	838
Capital structure	92.1829	2.0673	84.1928	91.3817	92.5749	93.5594	95.1245	838

Table 3.1: Descriptive statistics of the main variables used.

3.4 Empirical Analysis

This section presents the main estimates and probes their credibility. We begin with baseline results that quantify the impact of public emergencies on bank risk and assess how digital transformation moderates these effects. We then examine a battery of robustness checks, to ensure the findings are not artifacts of modeling choices.

3.4.1 Baseline results

Table 3.2 presents the results of the baseline model. Column (1) shows that the estimated coefficient of Treat * Post is -0.3226^{**} , indicating that public emergencies increase bank risk by 0.3226 percentage points. Column (2) demonstrates that the estimated coefficient of DT * Treat * Post is 0.4501^* , suggesting that digital transformation mitigates the impact of public emergencies on bank risk. Specifically, each 1-unit increase in digital transformation reduces the impact of public emergencies on bank risk by 0.4501 percentage points.

	Bank Risk	
	(1)	(2)
Treat * Post	-0.3226** (0.1341)	-2.4573** (0.9701)
DT		-0.1292 (0.0821)
DT * Treat * Post		0.4501** (0.2180)
Obs	1,006	1,006
R^2	0.1993	0.2047
Controls	✓	✓
Bank FE	✓	✓
Time FE	✓	✓

Table 3.2: Baseline difference-in-difference results on bank risk.

Robust standard errors in parentheses.

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Figure 3.2 illustrates the trend of the marginal effect of public emergencies relative to the degree of digital transformation in banks. It shows that as the degree of a bank's digital transformation increases, the impact of public emergencies on bank risk decreases. These findings provide preliminary validation of our hypotheses.

3.4.2 Robustness Checks

We conduct a placebo test to ensure the robustness of the empirical results. This involves re-estimating the model by randomly assigning treatment groups. Specifically, we randomly assign banks affected by the public emergency and the year in which the public emergency occurred, generating an incorrectly estimated coefficient. To further strengthen the reliability, we repeat the random sampling 1,000 times.

Figure 3.3 depicts the distributions of the coefficients and t-values, respectively. These distributions show that the coefficients and t-values are concentrated around zero and approximately follow a normal distribution. This indicates that our estimates are not obtained by chance, thereby confirming the robustness of our empirical results.

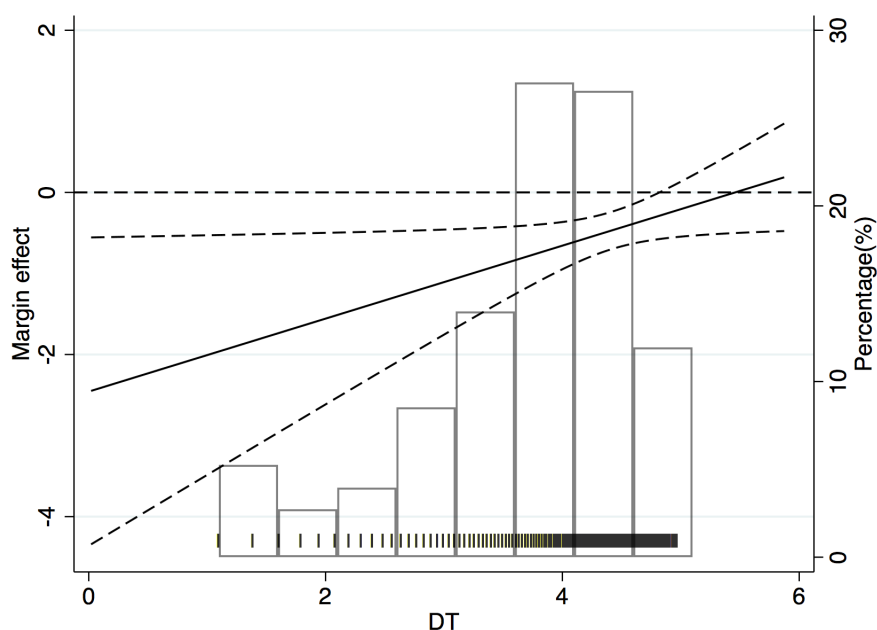
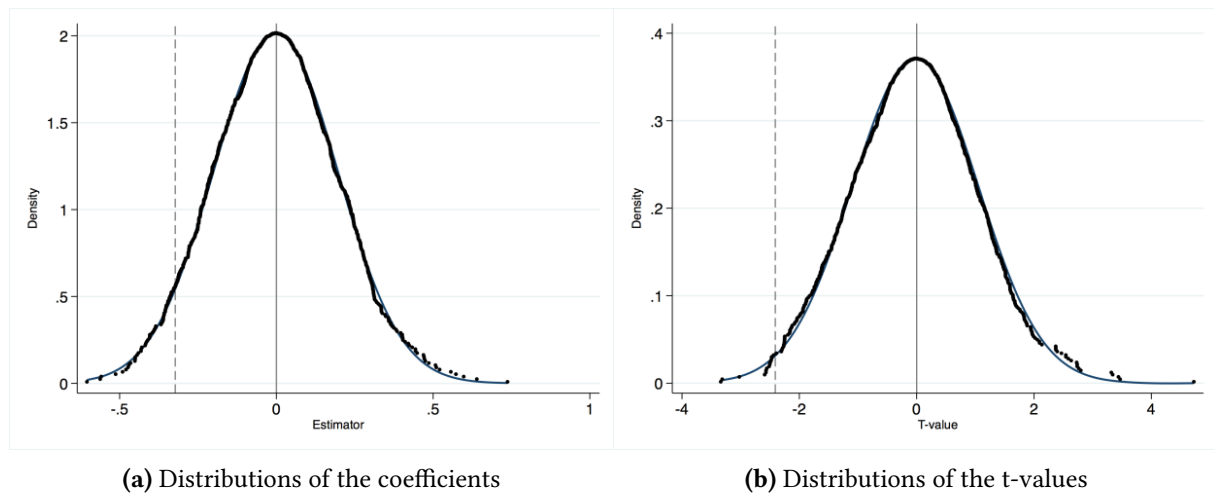
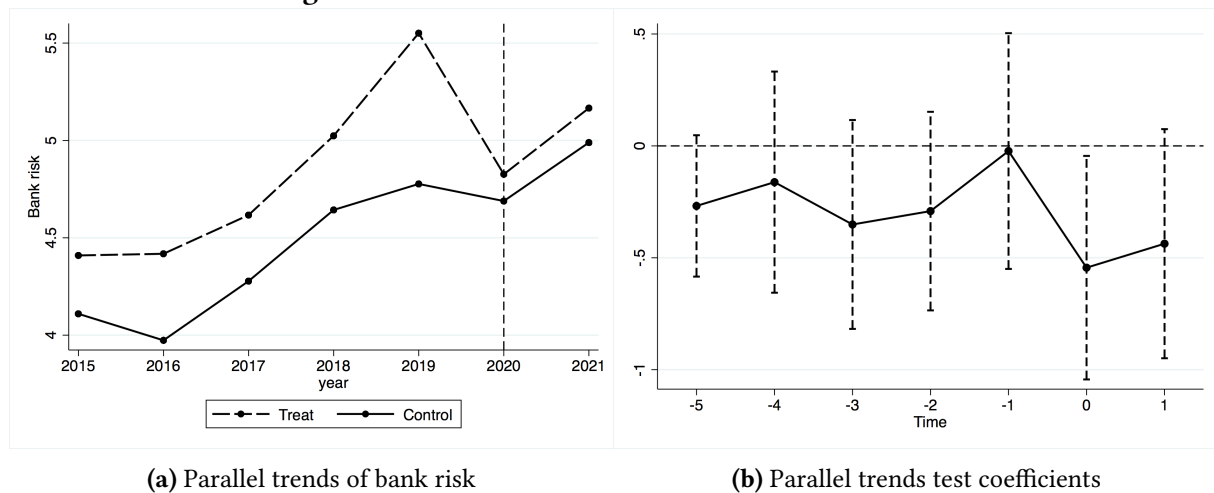


Figure 3.2: The impact of digital transformation on the margin effect of the public emergency

Second, we conducted a parallel trend test to confirm whether bank risk in the treatment and control groups was similar before the public emergency, ensuring the accuracy of the DiD model estimates. **Figure 3.4a** illustrates the trend of bank risk in both groups. Before the COVID-19 outbreak in 2020, the trends in bank risk-taking for the treatment and control groups were relatively similar. However, in 2020, bank risk significantly increased in the treatment group, while the change in the control group was minimal. After 2020, as COVID-19 became a global pandemic, its impact on Hubei Province was no longer significantly different from its impact on other regions in China. Consequently, bank risk in both groups returned to a consistent trend in 2021.

Figure 3.4b depicts the regression results from 2015 to 2019, with 2014 set as the base year, treating the public emergency as occurring at specific points. The results show that only the coefficient for 2020 is significantly different from zero, while the confidence intervals for the regression coefficients in all other years include zero. This indicates that bank risk in the treatment and control groups was significantly different only in 2020, but consistent in other years.

Third, we employed the propensity score matching method (PSM) to re-select the sample and eliminate sample selectivity bias. Specifically, we used 1:2 nearest-neighbor matching with a matching radius of 0.01. The matched characteristic variables included return on assets, capital adequacy ratio, provision ratio, deposit-to-loan ratio, net interest margin, and non-interest income ratio. As a result,

**Figure 3.3:** Distributions of the coefficients and t-values**Figure 3.4:** Visualization of the parallel trends assumption

93% of samples in the treatment group and 100% in the control group fell within the support domain. The matching results in [Table 3.3](#) indicate that the standard deviations of most matched characteristic variables decreased, and none exceeded 5% significantly. This suggests that there is no longer selectivity bias between the treatment and control groups. The matched samples meet the conditional independence and identically distributed assumptions.

		Treat	Control	Bias%	Bias %	T-test
Return on asset	Unmatched	0.82303	0.80931	4.7		0.47
	Matched	0.82751	0.8547	-9.4	-98.2	-0.71
Capital adequacy ratio	Unmatched	13.844	13.937	-3.9		-0.39

	Matched	13.956	14.219	-11.1	-182.8	-0.94
Provision ratio	Unmatched	218.15	238.62	-18.4		-2.05**
	Matched	222.37	223.99	-1.4	92.1	-0.13
Deposit to loan ratio	Unmatched	77.098	66.061	91.8		10.2***
	Matched	75.871	74.485	11.5	87.4	0.92
Net interest margin	Unmatched	1.9859	2.3547	-48.9		-4.56***
	Matched	2.0187	1.9266	12.2	75	1.14
Non-interest income ratio	Unmatched	29.344	25.144	26.2		2.47**
	Matched	28.418	28.397	0.1	99.5	0.01

Table 3.3: Balance test results before and after propensity score matching

*** p<0.01, ** p<0.05, * p<0.1

Figure 3.5 depicts the propensity score kernel density, showing that the probability distributions between the treatment and control groups converge after matching. This confirms that the sample meets the requirements for regression analyses. Table 3.4 reports the basic regression results using the matched sample. The coefficient of Treat * Post in column (1) is -0.4033**, and the coefficient of DT * Treat * Post in column (2) is 0.3945*, consistent with the findings before matching.

	Bank Risk	
	(1)	(2)
Treat * Post	-0.4033** (0.1917)	-2.2751** (1.0943)
DT		-0.2691** (0.1211)
DT * Treat * Post		0.3945* (0.2373)
Obs	522	522
R ²	0.2686	0.2763
Controls	✓	✓
Bank FE	✓	✓

Time FE ✓ ✓

Table 3.4: PSM Corrected difference-in-difference results on bank risk.

Robust standard errors in parentheses.

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Finally, we modified the sample to ensure robustness. First, to address the potential impact of frequent economic activity between neighboring provinces in China and the closure of Hubei Province on our empirical results, we removed banks from Hubei Province's neighboring provinces. The results for this revised sample are provided in Table 3.5. Column (1) shows that the estimated coefficient of Treat * Post is -0.3072**, and Column (2) shows that the estimated coefficient of DT * Treat * Post is 0.4623**.

Second, we removed banks with fewer branches in Hubei Province than the 50th percentile. Column (3) indicates that the estimated coefficient of Treat * Post is -0.3531**, and Column (4) shows that the estimated coefficient of DT * Treat * Post is 0.5522*. Third, we expanded the sample interval to include data starting from 2011. In this expanded sample, Column (5) indicates that the estimated coefficient of Treat * Post is -0.3480**, and Column (6) shows that the estimated coefficient of DT * Treat * Post is 0.5407**. All these tests yield results consistent with our initial findings. Therefore, we can confirm that our empirical results are robust.

	Provinces		Hubei Exposure		Extended Period	
	(1)	(2)	(3)	(4)	(5)	(6)
Treat * Post	-0.3072** (0.1353)	-2.4933** (0.9682)	-0.3531** (0.1697)	-3.0414** (1.3342)	-0.3480** (0.1405)	-2.8963*** (1.0695)
DT		-0.0963 (0.0861)		-0.1081 (0.0874)		-0.0569 (0.0789)
DT * Treat * Post		0.4623** (0.2174)		0.5522* (0.2987)		0.5407** (0.2384)
Obs	912	912	937	937	1,134	1,134
R^2	0.1926	0.1965	0.2070	0.2103	0.1755	0.1781
Controls	✓	✓	✓	✓	✓	✓
Bank FE	✓	✓	✓	✓	✓	✓
Time FE	✓	✓	✓	✓	✓	✓

Table 3.5: Difference-in-difference results when modifying studied sample. Columns 1 and 2 eliminate banks in provinces neighboring Hubei Province, columns 3 and 4 remove banks with number of branches in Hubei Province less than the 50th percentile and columns 5 and 6 extend the period to 2011-2021 inclusive.

Robust standard errors in parentheses.

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

3.5 Further Analysis

We deepen our inquiry along three dimensions. First, we decompose risk into active and passive components to clarify whether emergencies shift deliberate risk-taking or merely elevate background risk. Second, we examine mechanisms through which digital capabilities buffer shocks, operational continuity, information frictions, and credit allocation. Third, we explore heterogeneity across banks and contexts to assess external validity and policy relevance.

3.5.1 Active and Passive Risks

There is a distinction between active and passive risk-taking in the banking sector. Active risk-taking involves the deliberate holding of riskier assets due to increased risk tolerance by banks. In contrast, passive risk-taking occurs when external factors increase credit default risks, thereby raising the bank's risk without any active decision to take on more risk. Both types of risk contribute to overall insolvency risk. Understanding this distinction is crucial for comprehending how public emergencies impact bank risk.

To measure active and passive risk-taking, we use the risk-weighted asset ratio and the non-performing loan ratio, respectively. In [Table 3.6](#), the coefficients of $Treat * Post$ in Column (1) and $DT * Treat * Post$ in Column (2) are insignificant, indicating that public emergencies do not significantly affect the bank's active risk-taking. Additionally, digital transformation does not moderate the relationship between public emergencies and active risk-taking.

Column (3) shows that the coefficient of $Treat * Post$ is 0.1326*, and Column (4) shows that the coefficient of $DT * Treat * Post$ is -0.3588*. These results indicate that public emergencies increase the bank's passive risk-taking, but digital transformation can mitigate this impact. Public emergencies hinder production, leading to higher credit default rates among firms ([Bartik et al., 2020](#)). Additionally, public emergencies disrupt bank operations, causing banks to avoid actively increasing their risky assets to reduce insolvency risk. Digital transformation helps reduce information asymmetry in credit

transactions (Yao & Song, 2023), enabling banks to accurately assess firm creditworthiness and control non-performing loan rates during public emergencies.

	Risky weighted asset ratio		Non-performing loan ratio	
	(1)	(2)	(3)	(4)
Treat * Post	-0.0001 (0.0001)	-0.0004 (0.0007)	0.1326* (0.0791)	1.8286* (1.0471)
DT		0.0001*** (0.0000)		0.0485 (0.0546)
DT * Treat * Post		0.0001 (0.0002)		-0.3588* (0.2149)
Obs	947	947	905	905
R^2	0.1850	0.1984	0.1643	0.1699
Controls	✓	✓	✓	✓
Bank FE	✓	✓	✓	✓
Time FE	✓	✓	✓	✓

Table 3.6: Test results of the impact of public emergencies on different risks.

Robust standard errors in parentheses.

*** p<0.01, ** p<0.05, * p<0.1

3.5.2 Mechanism Analysis

As demonstrated above, public emergencies increase bank risk, while digital transformation can mitigate this impact. Our analyses indicate that public emergencies disrupt economic activity, thereby hampering banks' credit operations. This disruption leads to increased competition, loan concentration, and other factors that can elevate bank risk. Digital transformation enables banks to conduct business online, overcoming geographical constraints and thus mitigating the impact of public emergencies on bank risk. Consequently, credit operations may act as a mediator. To test this hypothesis, we construct the extended model shown in Eq.30 to Eq.33.

$$Credit_{i,t} = \beta_0 + \beta_1 Treat_i * Post_t + \sum_k \beta_k X_{k,i,t-1} + \eta_t + \mu_i + \varepsilon_{i,t} \quad (\text{Eq.30})$$

$$Risk_{i,t} = \varphi_0 + \varphi_1 Credit_{i,t} + \varphi_2 Treated_i * Post_t + \sum_k \varphi_k X_{k,i,t-1} + \eta_t + \mu_i + \varepsilon_{i,t} \quad (\text{Eq.31})$$

$$Credit_{i,t} = \delta_0 + \delta_1 Treat_i * Post_t + \delta_2 DT_{i,t-1} + \delta_3 DT_{i,t-1} * Treat_i * Post_t + \sum_k \delta_k X_{k,i,t-1} + \eta_t + \mu_i + \varepsilon_{i,t} \quad (Eq.32)$$

$$Risk_{i,t} = \tau_0 + \tau_1 Credit_{i,t} + \tau_2 Treat_i * Post_t + \tau_3 DT_{i,t-1} + \tau_4 DT_{i,t-1} * Treat_i * Post_t + \sum_k \tau_k X_{k,i,t-1} + \eta_t + \mu_i + \varepsilon_{i,t} \quad (Eq.33)$$

where “Credit” represents credit operations, measured by the credit balances to total assets. In column (1) of Table 3.7, the coefficient of Treat * Post is -0.0234****, indicating that public emergencies reduce bank credit operations. Column (2) shows that the coefficient of Credit is 2.4257****. Additionally, the absolute value of the Treat * Post coefficient is smaller than in Column (1) of Table 3.2, suggesting that public emergencies increase bank risk by reducing credit operations. This supports the existence of the transmission channel “public emergencies, credit operations, bank risk.”

In column (3), the coefficient of DT * Treat * Post is 0.0603****, indicating that digital transformation mitigates the impact of public emergencies on bank risk. Column (4) shows that the coefficient of Credit is 2.6076****. Furthermore, the absolute value of the DT * Treat * Post coefficient is smaller than in Column (2) of Table 3.2, suggesting that digital transformation reduces the exacerbation of bank risk during public emergencies by mitigating their impact on credit operations.

	Credit		Bank Risk	
	(1)	(2)	(3)	(4)
Treat * Post	-0.0234*** (0.0085)	-0.3037*** (0.0736)	-0.2623* (0.1376)	-1.7125 (1.0739)
DT		0.0131*** (0.0041)		-0.1663** (0.0802)
DT * Treat * Post		0.0603*** (0.0153)		0.3036 (0.2366)
Credit			2.4257*** (0.8600)	2.6076*** (0.8657)
Obs	995	995	995	995
R ²	0.5675	0.5795	0.2148	0.2221
Controls	✓	✓	✓	✓
Bank FE	✓	✓	✓	✓

Time FE	✓	✓	✓	✓
---------	---	---	---	---

Table 3.7: Test results of the mechanism analysis.

Robust standard errors in parentheses.

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

3.5.3 Heterogeneity Test

To further explore the differences in bank risk among banks of different sizes during public emergencies, we grouped banks according to their asset sizes. Specifically, banks with asset sizes greater than the 75th percentile are classified as large-scale banks, while banks with assets less than the 75th percentile are classified as small-scale banks.

Columns (1) and (2) of Table 3.8 show the regression results for large-scale banks. The coefficient of Treat * Post is -0.3753*, indicating that public emergencies have a negative impact on bank risk, although the effect is not significant. Columns (3) and (4) show the regression results for small-scale banks. Here, the coefficient of Treat * Post is -0.4203*, and the coefficient of DT * Treat * Post is 0.9839**, indicating a significant impact of public emergencies on small-scale bank risk, and that digital transformation significantly mitigates this risk.

These results suggest that public emergencies have a stronger impact on the risk of small-scale banks. Digital transformation appears to be particularly beneficial for small-scale banks in mitigating the risks posed by public emergencies. Generally, small-scale banks have fewer assets, weaker capabilities for absorbing deposits, and creating liquidity, leading to higher loan concentrations. Consequently, public emergencies can severely hinder their credit operations, and defaults by major customers can significantly increase their risk. The information acquisition capabilities provided by digital transformation can help small-scale banks diversify their businesses and reduce the impact of public emergencies. In contrast, large-scale banks, with their extensive business reserves and wider distribution of outlets, are less susceptible to local shocks.

	Large-Scale Banks		Small-Scale Banks	
	(1)	(2)	(3)	(4)
Treat * Post	-0.3753*	-3.1955	-0.4203*	-4.5585**
	(0.2033)	(1.9952)	(0.2250)	(1.7827)
DT		-0.1326		-0.1117
		(0.1994)		(0.0899)

DT * Treat * Post		0.5796		0.9830**
		(0.4210)		(0.4432)
Obs	283	283	723	723
R^2	0.3577	0.3607	0.1540	0.1592
Controls	✓	✓	✓	✓
Bank FE	✓	✓	✓	✓
Time FE	✓	✓	✓	✓

Table 3.8: Test results of the heterogeneity test.

Robust standard errors in parentheses.

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

3.6 Conclusion

Over the past few decades, public emergencies have frequently occurred worldwide, significantly jeopardizing financial stability. There is a need to systematically assess their economic consequences and propose measures to mitigate them. This study aims to reveal the impact of public emergencies on bank risk and the moderating role of banks' digital transformation in this process. It contributes to the literature on the impact of public emergencies on bank risk and the role of digital transformation in mitigating these effects. This study employs a Difference-in-Differences (DiD) model to conduct a quasi-natural experiment based on the COVID-19 outbreak in 2020 in Hubei Province, China. As the first outbreak site of COVID-19, Hubei Province provides a unique setting that helps to eliminate interference from other policy factors in the experiment.

Our findings indicate that public emergencies increase bank risk, and that banks' digital transformation can mitigate this impact. Specifically, public emergencies only increase banks' passive risk-taking, not active risk-taking. Digital transformation alleviates the impact of public emergencies on passive risk-taking but does not affect the relationship between public emergencies and active risk-taking. Furthermore, the study identifies the hindrance of credit operations as a transmission channel through which public emergencies increase bank risks. Digital transformation reduces bank risk during public emergencies by mitigating the impact on credit operations. Additionally, the impact of public emergencies on bank risk is heterogeneous, primarily affecting small-scale banks.

This study has valuable practical implications. Firstly, given that public emergencies increase bank risk, governments should coordinate the efforts of various departments to ensure the continuity of financial activities during such events. Regulatory authorities should enhance the financial coordination

supervision mechanism and monitor dynamic changes in bank risk. Commercial banks need to reduce loan concentration and liquidity mismatches to prevent local emergencies from causing widespread credit defaults. Special attention should be given to the bankruptcy risks of small-scale banks, banks with low liquidity creation, and banks with low provision ratios.

Secondly, considering that digital transformation can alleviate the impact of public emergencies on bank risk, commercial banks should accelerate their digital transformation efforts. This can be achieved by increasing R&D investment and partnering with technology companies for joint development. Governments should invest in digital infrastructure construction to provide a supportive environment for banks' digital transformation. Furthermore, data resources are crucial for digital transformation. Therefore, governments should work to break the market segmentation of information elements and encourage enterprises, financial institutions, and other departments to build joint databases. By establishing a digital financial business sharing platform, commercial banks can use digital technology to integrate information, thereby enhancing their business acquisition and credit risk identification capabilities.

3.7 Appendix

3.7.1 Tables

Panel A: Dependent variable	
Name	Definition
Bank risk	Calculated using Eq.27
Panel B: Independent variables	
Name	Definition
Public emergency	Dummy variable representing the first outbreak of COVID-19 in China
Digital transformation	The natural logarithm of the bank digital transformation index developed by Peking University
Panel B: Control variables	
Name	Definition
Profitability	The return on equity
Size	The natural logarithm of total assets
Cost	The cost-income ratio
Profit structure	The percentage of non-interest income
Net interest margin	The net interest income on total assets
Capital structure	The asset-liability ratio

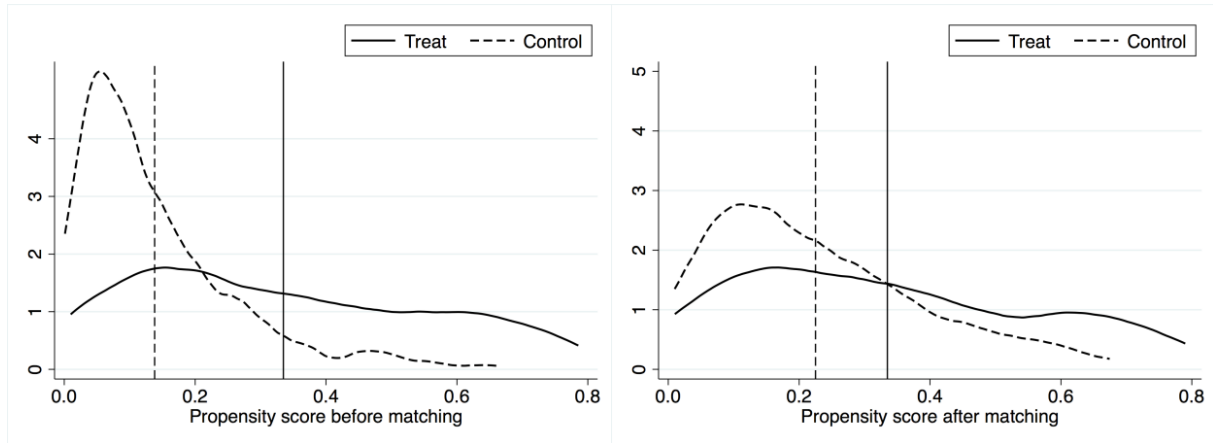
Table 3.9: List of main variables used in the paper and their corresponding definitions.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
(1) Risk								
(2) Treat * Post	0.057*							
(3) DT	0.244*	0.141*						
(4) Profitability	-0.077*	-0.063*	-0.201*					
(5) Size	0.323*	0.207*	0.504*	0.168*				
(6) Cost	-0.095*	-0.012	-0.232*	-0.517*	-0.353*			
(7) Profit structure	-0.0013	0.066*	0.103*	-0.154*	0.012	0.069*		
(8) Net interest margin	-0.163*	-0.080*	-0.307*	0.465*	-0.222*	-0.215*	-0.515*	
(9) Capital structure	0.006	-0.032	0.152*	0.500*	0.371*	-0.412*	-0.078*	0.040

Table 3.10: Correlation statistics of the main variables used.

*** p<0.01, ** p<0.05, * p<0.1

3.7.2 Graphs



(a) Propensity score kernel density before matching

(b) Propensity score kernel density after matching

Figure 3.5: Propensity score kernel density before and after matching

Chapter 4

Unconventional Monetary Policy and Corporate Bond Underwriting

Kristjan Markovc

This paper investigates the impact of the European Central Bank's Corporate Sector Purchase Programme (CSPP) on primary corporate bond market between 2013 and 2019, using a comprehensive dataset of issuance volumes and underwriting spreads. Results indicate a limited direct influence on issuance volumes for eligible firms, with no systematic expansion and some evidence of contraction among higher-rated issuers under a tighter A- or better definition. By contrast, underwriting costs declined for top-rated issuers, with consistent and statistically significant spread compression relative to non-rated peers across multiple specifications. Modest gross spread increases for ineligible firms in the pre-post framework suggest possible spillover effects, potentially via portfolio rebalancing or altered market sentiment. Overall, the CSPP appears to have primarily operated through pricing channels, benefiting already well-positioned issuers while leaving aggregate issuance largely unchanged. The findings highlight the heterogeneous nature of CSPP transmission and underscore the need for policy designs that balance market stabilization with broader access to funding benefits.

JEL Classification — E52, G12, G15, G24

Keywords — ECB; CSPP; Underwriting; Eurozone; Bond Market; Credit Rating

4.1 Introduction

Does the introduction of asset purchase program lead to an increase in competition in the underwriting service market? The global financial crisis of 2008 and its aftermath significantly impacted financial markets and economic growth. In response, central banks around the world adopted various unconventional monetary policies to stimulate economic activity. This paper aims to provide further insight in the impact of the European Central Bank's (ECB) Corporate Sector Purchase Program (CSPP) on a specific market segment of corporate bond underwriting.

Corporate bond issuance plays a crucial role in financing non-financial corporations. However, during economic downturns, access to credit can become constrained. This paper investigates whether the ECB's CSPP, through its unconventional measures, influences the corporate bond underwriting sector. The paper specifically explores how the CSPP impacted the nominal and relative spreads, and size of corporate bond issuance in the euro area. By analyzing these factors, the paper sheds light on the effectiveness of the ECB's program in facilitating corporate financing and potentially boosting economic recovery.

The motivation for this study arises from the observation that existing research has predominantly focuses on the developments and impacts of quantitative easing (QE) on the secondary markets (Abidi & Miquel-Flores, 2018; Bonfim & Capela, 2020; Gnan, 2019; Mäkinen, Mercatanti, Silvestrini, & Li, 2020; Todorov, 2017; Zaghini, 2020), while the primary markets (De Santis & Zaghini, 2021; Kubitza, 2023; Siani, 2019a, 2019b) and firm behavior are largely overlooked. Secondary market analyses provide insights in how QE affects asset prices, yields, and liquidity after securities have been issued. Todorov (2017) finds a significant drop in bonds yields by 30bps (8%) following the CSPP announcement. Moreover, Todorov (2017) finds the positive impact on the issuance of new bonds, The former results are further corroborated by Abidi and Miquel-Flores (2018) with the findings that the CSPP leads to a decline in bond yields by 15bps. Additionally, the paper finds support for further issuance of corporate bonds, results corroborating the findings of Todorov (2017). Lastly, the decline in bonds yields spreads is found by Zaghini (2019a).

Yet, understanding the implications of QE on the initial stages of capital raising in primary markets is equally crucial for a comprehensive assessment of its overall impact on the financial system. However, the literature has a large gap in the focus on the underwriting and the underwriting channel. Important contribution to the literature of underwriters is contributed by Siani (2019b), who finds that the underwriter channel leads to the spillover effects of CSPP on the international markets. While further understanding of the underwriter channel is beyond the scope of this paper, the work sets an important notion of the importance of the underwriting in the impacts of the asset purchase programs

(APP).

The CSPP has a profound impact on the corporate bond market and firm behavior in Europe. Research by [De Santis and Zaghini \(2021\)](#) indicates that CSPP leads to a general increase in overall bond issuance by eligible firms, with a six-month delay. Furthermore, eligible firms increase their capital expenditures and intangible assets but also direct funds towards marketable and equity securities, stock repurchases, cash holdings, and short-term investments. Similarly, [Bats \(2020\)](#) highlight that firms whose bonds are purchased by the ECB rely less on bank borrowing, with the amount of bonds bought correlating with decreased bank dependence. [Betz and De Santis \(2019\)](#) finds that CSPP improved credit access, particularly for firms relying on banks heavily exposed to CSPP-eligible firms, triggering a reallocation of bank loan supply towards firms lacking access to bond-based financing. Meanwhile, [Bekaert and De Santis \(2021\)](#) demonstrate that corporate bond returns in major developed economies are positively associated with lower credit ratings and higher residual maturities, underscoring the dominance of the corporate bond factor in explaining return variations. Furthermore, [Cappiello et al. \(2021\)](#) emphasize the heightened responsiveness of non-bank financial intermediaries to monetary policy measures influencing long-term interest rates, such as asset purchases, thereby strengthening the transmission of long-rate shocks due to the rising importance of debt securities in firm financing structures. Lastly, [Zaghini \(2019b\)](#) report that CSPP significantly impacted yield spreads, both directly for purchased and targeted bonds and indirectly across the broader bond market.

The underwriting plays an important role in debt capital markets (DCM) for firms wanting to raise capital for funding new projects. To reduce the information asymmetry associated with the risk, firms purchase underwriting services from large financial institutions - primarily investment banks, to evaluate the risk associated with the issued bonds. The underwriters act as intermediaries between the firms issuing the bonds - i.e. the issuers, and the investors, while facilitating the issuance and sale of the bonds. The underwriters take upon themselves a risk associated with the issuance of bond by buying the entire bond issue from the issuing firm and reselling the bonds with a markup to the investors. The risk of failure to sell the bonds is solely pushed on the investment banks with profits being the difference between buying and selling prices, known as the gross spread. The spread consists of manager's fees, underwriter fees - i.e. fees for syndicate members, and concession for agents.

The use of underwriting services of the banks provides the issuers of bonds the benefit on removing risk by receiving fund immediately, albeit at prices below market value. However, the price reduction from gross spread of the bonds is smaller than the direct and indirect costs to the firms of issuing the bonds directly. Moreover, the use of large financial institutions allows firms to achieve wider range of investors to sell the bonds, as well as achieve better prices due to expertise of the underwriters. Lastly, successful issuance of underwritten bonds helps the firms with credit score improvements.

The competition in the underwriting sector implies decreased profits to the firms and higher incentives for the firms to issue additional bonds. The implications are found by [Todorov \(2017\)](#) with the findings of a 25% increase in CSPP-eligible bonds after the CSPP announcement. However, the underwriting is neither addressed or discussed. The yields are studied among all bonds and do not focus on the role of underwriting and the impacts on the underwritten bonds.

The objective of this paper can be further expanded to the issues related to the frequency of underwriting services due to CSPP when issuing CSPP-eligible bonds in comparison to ineligible bonds. Moreover, the prior literature established the importance of past firm-bank relationships in DCM and the past relationships have yet to be studied within the scope of CSPP. Lastly, the CSPP was a developing project that was restarted in 2019, with the project running through COVID crisis.

The bond sizes and the issuer size matter in reputable matching with the underwriters ([Carbó-Valverde, Cuadros-Solas, & Rodríguez-Fernández, 2017](#); [McCahery & Schwienbacher, 2010](#)), implying the firms do not have equal access to the underwriters. Moreover, the effects translates to the improvements in the underwriting market by the banks through co-management and lower the costs of issuing ([Narayanan, Rangan, & Rangan, 2004](#)). Additionally, the works of [Yasuda \(2005\)](#), [Iannotta and Navone \(2008\)](#); [Mehmood, Dutordoir, and De Cesari \(2018\)](#); [Yasuda \(2007\)](#) show support for the importance of firm-bank relationships in the underwriting, with the findings of [Dick-Nielsen, Nielsen, and von Rüden \(2021\)](#) supporting positive role of past relationships when rolling over the bonds.

The reputable banks receive higher reputation premia, but the equity issuers working with reputable banks receive higher offer values and lower percentage spreads net of reputational premia ([Fernando, Gatchev, May, & Megginson, 2015](#)). Moreover, the literature finds support for the difference in fees and yields for reputable banks ([Fang, 2005](#); [Mehmood et al., 2018](#); [Santos & Tsatsaronis, 2003](#)). The increased competition in the underwriting market lowers the gross spread and fees ([Iannotta & Navone, 2008](#); [Kollo, 2005](#); [Santos & Tsatsaronis, 2003](#)) and the gross spreads and fees have also declined since the mid-70s ([Kim, Palia, & Saunders, 2008](#)). Lastly, [Song \(2004\)](#) highlights the importance of syndicates, showing that lead investment banks tend to invite commercial banks' participation when clients exhibit higher loyalty in reusing their services.

The importance of the competition in the underwriting services is highlighted by the findings of [Manconi, Neretina, and Renneboog \(2018\)](#) with the findings that powerful underwriters can extract rent from the issuers. Moreover, [Gnan \(2019\)](#) finds a decrease in yield spread following the announcement, with [Bonfim and Capela \(2020\)](#) finding that the effect is larger for eligible bonds than ineligible. Consistent with previously discussed impacts on the cost to firms and bank profits, [Pinto, Kanda, and Silva \(2021\)](#) find substitution effect between eligible bonds and equivalent loans in favor of eligible

bonds. The work of [Santis, Geis, Juskaite, and Cruz \(2018\)](#) shows the CSPP significantly eases financial conditions in Eurozone for non-financial corporations. Interestingly, the work of [Zaghini \(2020\)](#) suggests that CSPP impacts spread out gradually with first the impact being contained to eligible corporation, with ineligible bonds seeing impacts six months into the program.

The aim of CSPP was to impact the market through supply channel, support for which [Kubitza \(2023\)](#) finds in 2023. The work of [Mäkinen et al. \(2020\)](#) used regression discontinuity design (RDD) and suggests that the CSPP has no impact on yield spreads of eligible bonds in comparison to ineligible. The results are refuted by [Bonfim and Capela \(2020\)](#) using difference-in-difference approach by finding that decrease in yields for eligible bonds is larger than for ineligible. However, the CSPP positively impacts the financial conditions of firms. The bonds issued by firms with financial difficulties issue more bonds than CSPP non-targeted firms ([Galema, Lugo, et al., 2017](#)). The work of [Nozawa and Qiu \(2021\)](#) finds evidence that monetary policy announcement and the reduction of default risk due to quantitative easing (QE) explain changing term structure of credit spreads, at least in the short run, results supported by [Pegoraro and Montagna \(2021a\)](#) and [D'Amico and Kaminska \(2019\)](#). However, QE is rarely contained to the issued area, with [Siani \(2019a\)](#) identifying the underwriting channel as the channel through which the CSPP effects spilled over to the US.

Quantitative easing, especially as implemented by the European Central Bank (ECB), introduces a significant demand-side shock to financial markets. The ECB's asset purchase programs, including the Corporate Sector Purchase Program (CSPP), involve large-scale purchases of corporate bonds, which can influence systemic risk within the debt markets. Significant contribution to the literature was achieved by [De Santis and Zaghini \(2021\)](#), [Betz and De Santis \(2019\)](#) and [Zaghini \(2019b\)](#) by studying the bond issuance in post-CSPP period. However, the analysis focuses only on bond issuance without any reference to underwriting and the role of underwriting during bond issuance. The influx of central bank demand can alter the dynamics of bond issuance and underwriting practices, potentially pushing underwriters towards riskier bonds.

The paper positions itself within the literature by providing insights within underwriting service sector of financial markets by analyzing constructed panel data from all issued bonds collected from S&P Capital IQ Pro database. The paper differs from prior literature by focusing on underwritten bonds, contrary to all issued bonds. The paper is an extension of [De Santis and Zaghini \(2021\)](#) with a focus on financial services provided by the market, by signaling bond quality. The empirical analysis finds that the policy has significant spillover effects to non-targeted bonds, with significant effects on 'A'-rated bonds. The results suggest significantly broader implications than initially expected and targeted. Therefore, the results of the paper suggest that financial markets in Eurozone serve as important channels of contagion. The results suggest that quantitative easing policies or asset purchasing programs

by the central banks can be utilized for indirectly improving market conditions of riskier bonds.

Ultimately, these results enhance existing literature by providing vital insight in the spread and contagion of quantitative easing programs through financial markets and associated services, suggesting broader uses of such programs. Additionally, the results suggest towards the relevance and the need for more developed financial markets and services within Eurozone. The remainder of the paper is organized as follows. [Section 4.3](#) describes the data collection and descriptive statistics. [Section 4.4](#) explains the methodology used with [Section 4.5](#) discussing the results. [Section 4.5.1](#) discusses core results of the paper, with [Section 4.5.2](#) providing robustness results by redefining eligibility criteria and [Section 4.5.3](#) focusing on individual credit ratings. Lastly, the [Section 4.5.4](#) focuses on direct pre-post analysis per group. [Section 4.6](#) discusses the results and limitations of the paper and [Section 4.7](#) concludes the paper with final remarks.

4.2 Background and Institutional Setting

Following the 2007-2008 financial crisis, the European Union (EU) economy was characterized by modest economic growth and post-crisis recovery. The economy exhibiting post-crisis recovery at mere modest pace with positive expectations and expected improvements in the upcoming years. The government deficits and debt-to-GDP were expected to show signs of improvements, but faced both internal and external downwards pressures.

The economic slowdown among key European trading partners proved to be detrimental for the economic growth and post-crisis recovery. Moreover, persistent economic challenges among some European countries - e.g. Greece, put additional stress on the European economy with predicted mere 1.6% for Eurozone and 1.8% growth for the entire EU. Additionally, the Eurozone was facing weak credit conditions with banks still recovering from 2008 financial crisis and stricter credit conditions. The lack of strong credit conditions lead to reduced economic growth and post-crisis recovery.

The lack of strong recovery and persistent downwards pressures motivated European Central Bank (ECB) to stimulate the economy. However, the conventional monetary policy (MP) was reaching lower bound with little to no effectiveness, requiring the use of unconventional monetary policy (UMP). In its attempt to stimulate the economy in the post-crisis period, ECB lowered its key interest rates to their lower bounds starting in 2009, with marginal lending facility rate (MLF rate) being set to mere 2% in 2009 and less than 0.5% in 2014. Similarly, the marginal refinancing operations rate (MRO rate) was set to 1% in 2009 and 0% from early 2016. Lastly, the ECB set the deposit facility rate to 0% in 2012 and introduced the negative interest rate in 2014. The changes of key interest rates are visually presented in [Figure 4.1](#). The lack of effectiveness of low interest rates since 2009 in stimulating the economy mo-

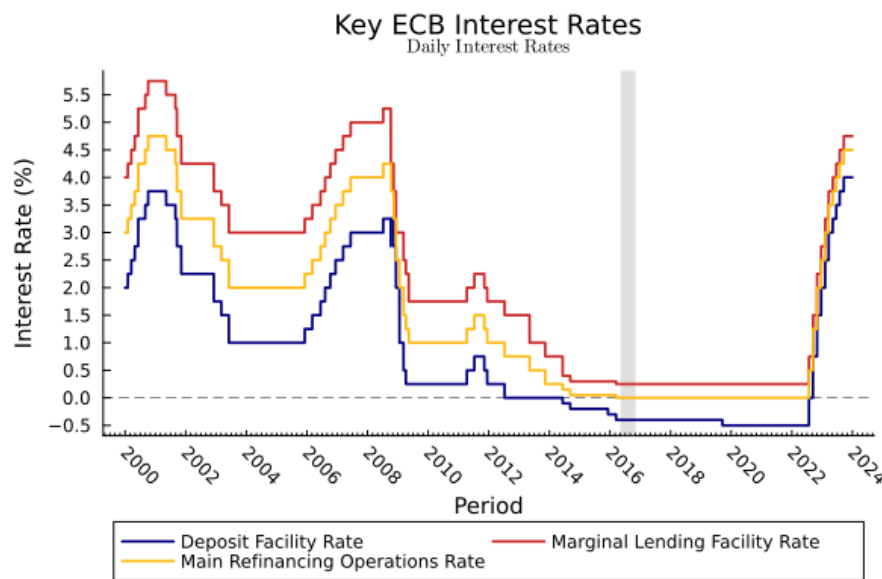


Figure 4.1: Key interest rates set by ECB on a daily frequency from January 1, 2000 to December 31, 2023. The shaded area in 2016 represents the interim period between the CSPP program announcement and the start of asset purchasing under CSPP. Source: ECB Data Portal.

tivated ECB to alter its approach and introduce further economy-stimulating policies. Consequently, in mid-2014, the ECB introduced Asset Purchase Program (APPs) with the start of security purchases in October 2014.

4.2.1 Asset Purchase Programs

The lack of effectiveness of conventional MP and effective lower bound of interest rates, prevented ECB from further easing MP. The effective lower bound of interest rates meant little to no effect from lowering the interest rates any further, while risking negative impacts on both the banks and savers. Consequently, the ECB had to introduce further measures to fight persistent low inflation and slow-growing economy. In mid-2014, ECB announced the Asset Purchase Programs (APPs). The first security purchases began in October 2014 under *third Covered Bonds Purchase Program* (CBPP3), focused on purchasing covered bonds issued by European banks. Covered bonds are a type of debt security backed by a pool of mortgages or other loans.

The second APP was introduced in November 2014 under *Asset-backed Securities Purchase Program* (ABSPP) for buying asset-backed securities (ABS) from European institutions. ABS are financial instruments that pool together various types of loans or receivables and sell them as securities. In March 2015, ECB announced third, and most influential APP, the *Public Sector Purchase Program* (PSPP). The

PSPP aimed to purchase government bonds issued by eurozone governments. The PSPP is considered the most significant part of the ECB's quantitative easing (QE) measures. The PSPP is the most significant part of the APPs introduced in 2014 with the largest impact and represents the largest share of net purchases. The program was further expanded in March 2016 with the announcement of *Corporate Sector Purchase Program* (CSPP), with the start of purchases in June 2016.

The net purchases of ECB under the APP program steadily grew until late 2018 - see Figure 4.2a, after exceeding 2,500 billion euros. The increase in asset holdings by ECB is reflected in debt security holdings ECB held on its balance sheets - see Figure 4.7. Similarly, the net asset purchases of ECB under the APP programs is presented in Figure 4.2b, with 2019 ECB conducting net sale of assets held. The APPs introduced by ECB in 2014 were discontinued in July 2023 with the Governing Council of ECB announcing on June 15, 2023 that by July 2023 they will discontinue reinvestments under the APP.

The aim of ECB when introducing APPs was to provide liquidity to the economy and improve credit

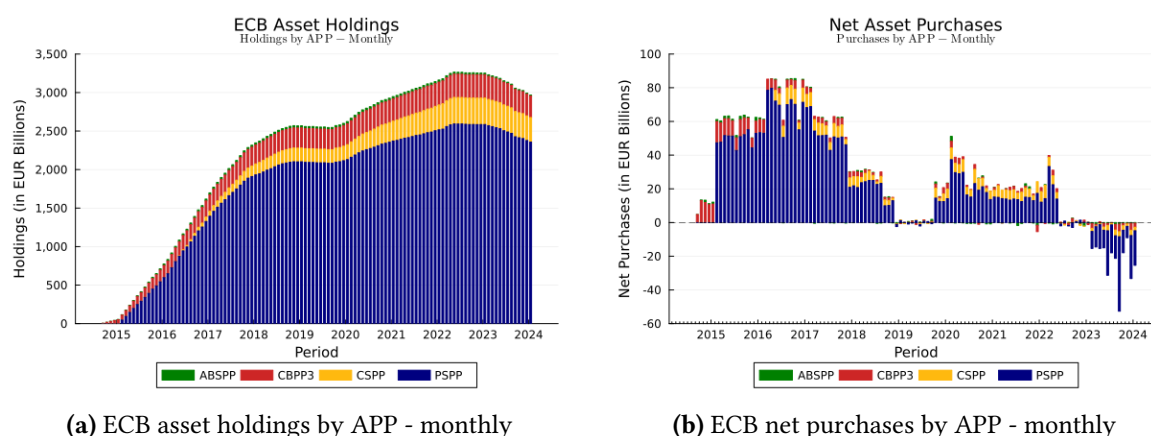


Figure 4.2: Net ECB asset holdings and purchases by asset purchase program from October 2014 to February 2024. The asset holdings are amortized and the statistics are reported at the end of the month. Source: ECB Data Portal

conditions. The APPs provide three (3) core mechanisms through which the ECB was aiming at achieving its goals. The primary channel is *Direct pass-through* by increasing the demand for assets, causing the increase in prices and incentivizing the banks to issue additional loans. Second, the *Portfolio rebalancing* by increasing the demand for assets held by investors, causing excess liquidity for reinvestment by the investors. The mechanisms cause increase in demand and improve broader financial conditions, putting upwards pressure on inflation. Lastly, the APPs aim to act through *signaling effect* by signaling to the economy that the ECB will keep the interest rates low for a foreseeable future, which in turn reduces volatility and uncertainty, facilitating investments and loan-granting (Bank, n.d.).

4.2.2 Corporate Sector Purchase Program

In March 2016, ECB announced a new APP called *Corporate Sector Purchase Program* (CSPP), targeting corporate sector bonds. The asset purchasing under CSPP began on June 8, 2016, creating an interim period between March and June 2016 with CSPP being signaled and announced, but no purchasing happened. The CSPP initially lasted until December 19, 2018 with net purchases, but only reinvestment of the principal between January 2019 and October 2019 - see [Figure 4.2b](#). Starting in November 2019, CSPP was continued until the end of June 2022, with full reinvestment of principals until February 2023. The ECB continued with partial reinvestment from March 2023 to July 2023, after which all reinvestments were discontinued.

The introduction of CSPP was designed to reduce borrowing costs for businesses through increased demand for corporate bonds. Additionally, the CSPP was implemented to boost credit availability by easing the pressure on banks. However, the CSPP did not apply universally to all corporate bonds, but rather to only the bonds meeting ECB designated criteria¹. Eligibility criteria imposed by the ECB served as minimum requirements for a given bond to be considered for a possible purchase either on a primary or a secondary market. The purchases of eligible bonds, as set forth by ECB standards, was conducted through 6 national central banks² (NCBs), each covering assigned geographical areas. Additionally, the eligibility was insufficient for purchase consideration by the relevant NCB³ as the bookkeepers were required to actively participate in the CSPP by submitting the required form to the relevant CSPP, offering the bond for purchase, with ECB, through NCBs, reserving the right whether to purchase the bond. The CSPP, similar to other APPs operates both in primary and secondary markets, with bulk of all purchases being in secondary market. The share of all purchases being from secondary market can be seen in [Figure 4.3b](#). The purpose of participating primarily in the secondary market allows ECB to prevent becoming a price setter and allows market to find and establish a market price for securities. Presented in [Figure 4.3a](#), the holdings of primary and secondary markets is presented. The combined values correspond to CSPP part of [Figure 4.2a](#).

The CSPP program was primarily focused on the secondary market to avoid exhibiting market setting behavior. Consequently, the ECB holdings under CSPP closely resembled the pool of available

¹The ECB designated criteria include, but not limited to: (1) Euro-denominated, (2) issuing entity must be incorporated in Eurozone, (3) issuing entity must not be a non-bank corporation, (4) issuing entity must not have parent subject to banking supervision outside Eurozone, (5) minimum rating of BBB- or equivalent, (6) minimum outstanding issuance amount of €10 million, and (7) initial maturity of 365/366 days or less and a minimum of 28 days of remaining maturity or initial maturity of 367 days with remaining maturity of over 6 months and less than 31 years.

²Nationale Bank van België/Banque Nationale de Belgique, the Deutsche Bundesbank, the Banco de España, the Banque de France, the Banca d'Italia and Suomen Pankki (central bank of Finland)

³Meeting eligibility criteria was necessary but insufficient condition for purchase consideration.

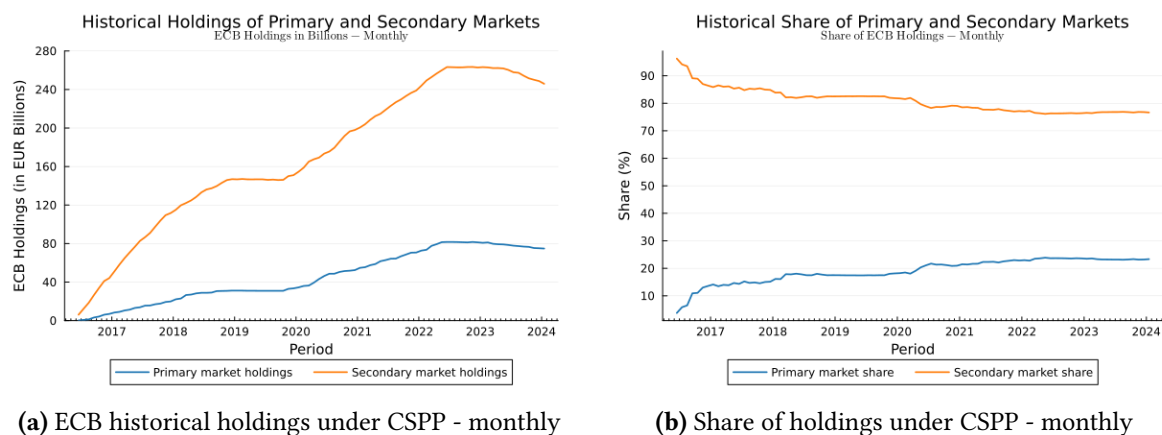


Figure 4.3: Historical holdings and shares of securities bought under CSPP in primary and secondary markets. Source: ECB Data Portal

eligible bonds. The purchases under CSPP were targeted such that, by category - i.e. country, rating and setting, the share of each category of ECB holdings under CSPP closely resembled the shares of the entire pool of available eligible bonds. As presented in Figure 4.4, in Figure 4.4a⁴, ECB initially maintained the share of its holdings within 1% of available bonds - exception being France in the Q3 2017, with the differences and precision decreasing to 3%-4% in 2022-2023 period. Similarly, in Figure 4.4b, ECB initially missed the share of BBB bonds by 4% and improved the share precision to within 2%. The precision of the bond shares by sector are shown in Figure 4.8.

4.2.3 Theoretical Background

The CSPP, by positioning the ECB, through 6 national central banks, as a large and price-insensitive buyer of eligible corporate bonds, had the potential to alter the primary issuance environment in ways that directly influence the gross underwriting spread, a composite measure including the management fee, underwriting fee, and selling concession. While much of the global literature on underwriting cost determinants assumes a firm commitment model, in which underwriters purchase the entire issue from the issuer and bear the associated placement risk, this is less applicable to the European context. In the euro-denominated corporate bond market, underwriting is often conducted on a best efforts basis, meaning the underwriter acts as a placement agent without fully committing its own balance sheet. This institutional feature has implications for how central bank interventions transmit to underwriting costs.

⁴The optimal difference would be 0%, implying the holdings held by ECB perfectly match the shares of the available pool - e.g. eligible bonds of France represent 26% of total available eligible bonds in the market, assuming 27% of all ECB holdings under CSPP are French, the difference would correspond to 0.01 on the y-axis.

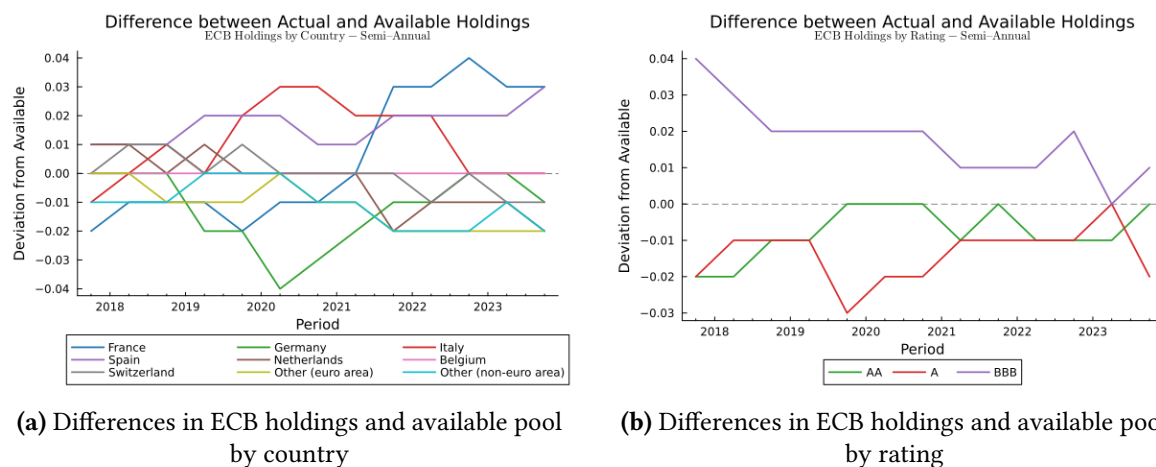


Figure 4.4: Differences between ECB holdings under CSPP and available pools by country (left) and rating (right). The graph represents how closely the ECB mimicked its holdings in comparison to available pool of eligible bonds. The difference between ECB's share of holdings by category and share of the category within the entire pool of available eligible bonds is presented in decimals, implying 0.01 corresponds to 1%. Source: ECB Data Portal

Bond Issuance Stimulus: The presence of ECB as an investor, implies large exogenous shock to the demand for bonds. With the demand seeing sizeable demand increase resulting from ECB's participation in both primary and secondary markets, the saturation of the market suggest the competition for bonds is increased. Therefore, the firms can find bond investors for lower coupon rates, making debt cheaper. The discount on bond-issued debt can lead to a general shift from a loan-driven debt structure to bond-driven debt through larger bond issuances.

Anchored Expectations: ECB's intervention in the market with long-term commitment plays a vital role in signaling and anchoring inflation expectations. With the ECB committing to the asset purchase program, send vital signals to the financial intermediaries and investors future monetary policy stability, directly reducing uncertainty and provides guidance. With the anchoring of the expectations, the firms also find higher incentives to enter the bond-driven debt as the expectation of increased inflation and interest rate would deepen the cost of loans.

Spillover Effects: With the ECB filling a role of a large investor, the market for high quality desirable bonds becomes saturated. The financial depth and information available to the ECB prevents other investors from competing. Consequently, smaller investors are pushed out of the market for top bonds towards riskier bonds. The relative increase in additional earnings from the drop in top bond market, makes riskier bonds more attractive. However, since risky bonds offer equal nominal risk, the underwriting services can help risky firms from differentiating risky bonds among each other. In layman's terms, the presence of the underwriter offers a layer of security and confidence to the investors

newly entering the risky, non-investment grade bond market after being pushed out of the investment grade bond market due to the presence of ECB as an investor. Such security offers new investors an important signal that financial intermediaries are confident in the quality of the bonds and the issuer beyond the signaling offered by publicly available ratings, or lack thereof, thus the role the underwriters already play, is further strengthened and shifts from signaling and convenience, to strong signaling. Lastly, since the risky bond market is often overlooked by larger investors, the underwriters play crucial role in matching investors with the bond issuers. Naturally, the latter role is always present with the underwriters; however, the underwriters serve as catalysts in investors' willingness to commit to investment in riskier bonds.

Demand Conditions and Competitive Pressures: In a best efforts framework, underwriter compensation depends less on capital risk-taking and more on their ability to place securities efficiently and maintain client relationships. By raising investor demand for eligible bonds, both directly through secondary market purchases and indirectly through improved sentiment, the CSPP can reduce the effort and marketing costs associated with placing a new issue. Stronger demand conditions can also intensify competition among underwriters for mandates, particularly for high-grade, CSPP-eligible issuers. This shifts bargaining power toward issuers, enabling them to negotiate narrower spreads, especially in the discretionary components of underwriting fees.

Perceived Credit Quality and Fee Negotiations: Even without committing their balance sheet, underwriters assess the relative ease of placement when setting fees. CSPP eligibility, tied to investment-grade ratings, signals lower credit risk and greater investor interest. For high-grade issuers, this may justify smaller fees as the perceived placement difficulty declines. The program's design may also create a halo effect, benefiting even non-eligible issuers perceived as operating in a stronger market environment.

Selling Concessions and Investor Willingness to Pay: Selling concessions, the discounts offered to investors in the primary market, are also sensitive to demand conditions. Where the CSPP bolsters secondary market liquidity and price support, investors may accept smaller concessions, anticipating favorable post-issuance performance. This reduces the spread component passed on to investors, further compressing total issuance costs.

Limited Impact on Management Fees: Management fees, representing the administrative and coordination costs of organising the issue, tend to be more stable and less sensitive to shifts in market demand or credit perception. While competitive pressure may exert some influence, these cost elements are less likely to be the primary source of observed spread reductions.

Implications for the Primary Market: In the predominantly best efforts underwriting structure

of the European market, CSPP-driven cost compression is therefore more plausibly linked to demand-side effects and heightened competition for mandates than to a reduction in underwriter balance sheet risk. The combination of elevated investor interest, stronger perceived creditworthiness for eligible issuers, and intensified competition among banks creates an environment in which issuers can secure lower total spreads. The most affected components are likely the underwriting fee and the selling concession, with management fees remaining comparatively unaffected.

4.3 Data

The paper relies on the data collected from a single unified data source. Specifically, the data was collected from S&P Capital IQ Pro (CapIQ) database. The CapIQ database collects the data on the issuance of new transactions within DCM with unique transaction identifying information. Moreover, the use of CapIQ allows for the collection of data on the bond issuing information, as well as on bonds acting as underwriters, when available. The cross-sectional data on bond issuance was collected from 2011 to 2019 inclusive, on all new transactions issued and collected by CapIQ for DCM⁵. The individual transactions are identified using unique transaction identification. Moreover, all transactions have the available information on the issuing entity and allow for exact identification of the issuer.

In 2011 to 2019 period, a total of 46 unique firms issued underwritten corporate bonds, with 19 unique firms issuing at least one EUR denominated underwritten bond. Among all firms issuing bonds, 21 firms, or 45.65%, issued underwritten bonds at least once for at least two consecutive years, with 5 firms issuing EUR denominated underwritten bonds at least once in at least two consecutive years. Among 46 firms issuing bonds, only 6 firms issued at least one underwritten bond in every year between 2011 and 2019 inclusive, representing mere 13.04% of all firms, with 1 firm issuing at least one EUR denominated underwritten bond every year.

The bond issuance decreased through the years in both the number of underwritten bonds issued, as well as the cumulative value of issued underwritten bonds. In [Figure 4.5a](#), the left y-axis represents shows the number of underwritten bonds issued, with the right y-axis representing the cumulative value of issued underwritten bonds. The near identical trends of the number and the cumulative value of issued bonds suggests the value per bond stayed constant across the years. However, when focusing on the [Figure 4.5b](#), the story changes. Since the implementation of the CSPP, the EUR underwritten bonds showed some growth in the share of total value of all issued underwritten bonds, with a slight decrease in the share of the number of issuance, suggesting there was a slight decrease in the popularity

⁵The removal of pre-2011 allows for the elimination of 2008-crisis impacts, while removal of post-2019 eliminates any unwanted impacts from COVID crisis.

of issuing EUR underwritten bonds but firms opted foreign currencies. Lastly, in Figure 4.9 the number of firms issuing bonds is visually represented, with Table 4.20 providing exact values. The presence

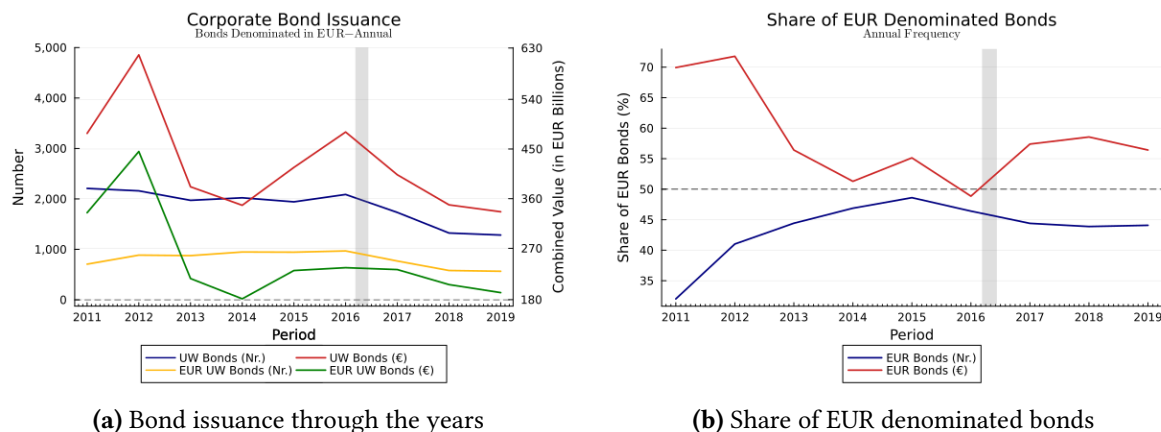


Figure 4.5: The total number and the value of underwritten bond issuance from January 1, 2011 and combined value of issued bonds (left) and the share that EUR denominated represent by respective groups (right). The “UW” is an abbreviation for “Underwritten” with the gray area representing the interim period between the CSPP program announcement and the start of purchases under CSPP. The data is presented on an annual basis.

of interim period between the CSPP program announcement and the start of purchasing under CSPP, implies the possibility of a market reaction and the anticipation effect. Consequently, it is important to evaluate the interim period separately with the bond announcement dates being used to classify the bonds in respective periods. In Table 4.1 the descriptive statistics are presented by period. From the pre-CSPP period, the average eligible underwritten bond size decreased, with the decrease being driven by the reduction in the largest bond size. The fees are insufficiently reported for any estimation or analysis and underlying structure of the spread and fee reporting in European bond market impedes any further conclusions based on the impacts on the spread⁶. The average size of the spread - i.e. both relative and nominal, increased from pre- to post-CSPP period, with the nominal spread seeing a decrease in the largest observed spread. Lastly, the coupon rates saw a large decrease in average size, median and also largest observed spread.

Among non-eligible bonds, sizes seemed to have increased on average but not in the largest observed size. However, contrary to eligible bonds, non-eligible bonds saw no change in nominal spread,

⁶The European bond market heavily rely on drawdowns from European Medium Term Note (EMTN) programs as opposed to standalone transactions. The limitation of reliance on EMTN programs is present in lacking transparency regarding true expenses of transactions due to non-disclosure. Unlike the US bond market, the European bond market discloses spreads as a re-sale spread without the inclusion of the fees, with the latter being typically reported only for standalone transactions and any disclosure of fees from EMTN programs is entirely discretionary and not subject to regulatory obligation.

and a decrease from the pre-CSPP period. Lastly, average and median coupon rates declined, with a large increase to highest observed coupon rate.

Panel A: CSPP Eligible Bonds						
	Period	Mean	Min	Median	Max	Count
Value (mil. €)	CSPP	4.71	2.40	4.47	6.91	48
	Interim	5.74	3.16	6.45	7.31	8
	Pre-CSPP	5.05	2.40	5.30	7.47	100
Spread (th. €)	CSPP	3.97	0.00	6.60	8.52	28
	Interim	6.71	0.00	8.52	9.21	5
	Pre-CSPP	3.52	0.00	1.04	9.21	76
Spread (%)	CSPP	0.57	0.10	0.25	5.00	15
	Interim	0.37	0.10	0.50	0.50	3
	Pre-CSPP	0.49	0.04	0.29	3.50	37
Coupon (%)	CSPP	0.91	0.00	0.88	4.20	40
	Interim	1.09	0.00	1.00	2.88	5
	Pre-CSPP	2.22	0.00	1.62	6.00	37

Panel B: Other Bonds						
	Period	Mean	Min	Median	Max	Count
Value (mil. €)	CSPP	4.00	0.00	3.91	8.52	5,166
	Interim	3.89	0.56	3.80	8.57	557
	Pre-CSPP	3.66	0.01	3.60	10.46	10,307
Spread (th. €)	CSPP	3.06	0.00	2.60	11.23	2,819
	Interim	2.04	0.00	0.00	9.49	349
	Pre-CSPP	3.01	0.00	2.11	12.77	6,973
Spread (%)	CSPP	0.54	0.00	0.25	10.00	1,475
	Interim	0.71	0.02	0.30	5.20	111
	Pre-CSPP	1.11	0.00	1.00	12.50	3,975
Coupon (%)	CSPP	2.19	0.00	1.39	30.00	3,507
	Interim	2.11	0.00	1.30	10.34	319

Pre-CSPP	3.17	0.00	2.83	18.00	5,193
----------	------	------	------	-------	-------

Table 4.1: Descriptive statistics for eligible underwritten bonds in Panel A and other underwritten bonds in Panel B. The Panel A defines the underwritten bonds as bonds that are either marked as underwritten in S&P Capital IQ Pro database, have aggregate spread, gross spread or a known lead book manager. The Panel B uses all other remaining bonds that are not possible to be determined whether they are eligible due to missing data or are not eligible. All nominal values presented are in a natural log form, with percentage values being presented as percent and not decimals.

It is important to acknowledge that firms issuing underwritten bonds, may represent a systematically different subset of the corporate sector compared to those that do not. Underwritten euro-bond issuance in the Euro market is typically associated with larger, more established, and more creditworthy firms, which are better positioned to access international capital markets and meet the due diligence requirements of investment banks. These firms often enjoy broader investor networks, greater name recognition, and more predictable cash flows, making them more attractive clients for underwriters and more acceptable counterparties for large-scale institutional investors. Notably, credit ratings appear to be less prevalent among Euro market issuers compared to their counterparts in the USD market. A further limitation is that credit ratings in the dataset are observed only at the time of data collection, not at issuance or subsequent years, raising the possibility of treatment misclassification if ratings changed over time. Such misclassification, especially if rating transitions are non-random, would likely attenuate estimated CSPP effects, biasing coefficients toward zero by blurring the distinction between treated and untreated issuers. These factors together mean the results primarily reflect conditions within a narrower, relatively high-quality and rating-sparse segment of the market, rather than the full corporate bond universe.

Table 4.2 presents summary statistics for outcome and control variables for treated and untreated firms in the pre- and post-CSPP periods. Before the introduction of the CSPP, treated firms exhibited significantly higher underwriting spreads than untreated firms, both in euro terms and in log or percentage form, with differences significant at the 5% level. Treated firms were also substantially larger, held more cash, and displayed higher return on assets, though they had significantly lower liquidity. Following the CSPP, differences in issuance value and underwriting spreads between treated and untreated firms largely disappeared and became statistically insignificant. However, treated firms remained significantly larger and more cash-rich than their untreated counterparts, with size and cash differences highly significant at the 1% level in both periods.

Panel A: Pre-CSPP period

	Untreated		Treated		Diff
	Mean	Observations	Mean	Observations	
Value (€)	8.102	675	3.704	135	-4.399 [-0.68]
Value (log)	0.1	675	0.046	135	-0.054 [-0.8]
Value (w. ave log)	0.096	675	0.046	135	-0.05 [-0.76]
Value (average log)	0.095	675	0.046	135	-0.049 [-0.75]
Spread (€)	0.0	675	15.741	135	15.741** [2.24]
Spread (log)	0.0	675	0.057	135	0.057** [2.24]
Spread (w. ave log)	0.0	675	0.057	135	0.057** [2.24]
Spread (average log)	0.0	675	0.057	135	0.057** [2.24]
Spread (average %)	0.0	675	0.003	135	0.003** [2.24]
Spread (w. ave %)	0.0	675	0.003	135	0.003** [2.24]
Coupon (average)	0.026	675	0.021	135	-0.005 [-0.18]
Size	14.318	294	17.751	123	3.433*** [12.52]
Cash (log)	9.899	278	13.499	123	3.6*** [9.56]
ROA	0.487	267	1.697	120	1.21** [2.24]
Liquidity	2583.07	276	-11943.784	123	-14526.855* [-1.76]

Panel B: Post-CSPP period

	Untreated		Treated		Diff
	Mean	Observations	Mean	Observations	
Value (€)	6.011	900	6.944	180	0.933 [0.19]
Value (log)	0.076	900	0.1	180	0.024 [0.43]
Value (w. ave log)	0.074	900	0.1	180	0.025 [0.46]
Value (average log)	0.074	900	0.1	180	0.025 [0.46]
Spread (€)	9.514	900	0.0	180	-9.514 [-0.69]
Spread (log)	0.026	900	0.0	180	-0.026 [-0.77]
Spread (w. ave log)	0.025	900	0.0	180	-0.025 [-0.77]
Spread (average log)	0.025	900	0.0	180	-0.025 [-0.77]

Spread (average %)	0.001	900	0.0	180	-0.001 [-0.71]
Spread (w. ave %)	0.001	900	0.0	180	-0.001 [-0.72]
Coupon (average)	0.017	900	0.013	180	-0.003 [-0.21]
Size	14.435	387	17.726	176	3.291*** [14.72]
Cash (log)	10.532	370	14.222	168	3.691*** [13.69]
ROA	0.857	373	1.752	174	0.895 [1.49]
Liquidity	2153.831	363	45.686	168	-2108.144 [-1.03]

Table 4.2: Descriptive statistics and differences in means between treated and untreated firms in the pre- and post-CSPP periods. The table reports sample means, the number of observations, and the difference in means with corresponding t-statistics in brackets.

*** p<0.01, ** p<0.05, * p<0.1

4.4 Methodology

The paper applies a base two-way fixed effect (TWFE) model with slight modification in the variable of interest. The base regression for panel data analysis is as follows:

$$y_{i,t} = \alpha + \beta_1 Treated_i * Post_t + \beta X_{i,t-1} + \mu_i + \xi_t + \varepsilon_{i,t} \quad (4)$$

where $y_{i,t}$ is the dependent variable and includes outcome variables of firm i , issued at time t . The independent variables include the variable of interest $Post_t$ taking a value of 1 if the year is 2016 or later, interacted with $Treated_i$ that takes a value of 1 if the firm can issue CSPP eligible bonds. The variables μ_i and ξ_t are firm and time fixed effects respectively. The $X_{i,t-1}$ is a vector of lagged firm characteristics of the issuer i , including:

- X_1 : Size - defined as natural log of total assets,
- X_2 : Natural log of cash and cash equivalents,
- X_3 : ROA,
- X_4 : Liquidity

The baseline TWFE model serves as the foundation for section-specific modifications, as discussed in subsequent sections. The treated group is defined as firms meeting the following criteria:

- Credit rating of BBB– or higher (investment grade),

- Not classified as a credit-granting institution (NAICS 3-digit code 522 and respective sub-classifications excluded),
- Incorporated in the Eurozone.

The panel dataset excludes all firms lacking information on either the NAICS classification or country of incorporation. Firms without a recorded credit rating are classified as untreated, under the assumption that the absence of such information reflects the firm's never having received a formal rating, thereby rendering it ineligible for bond purchases under the CSPP. The untreated group also includes all firms with a credit rating below BBB-.

The panel data used in the paper was manually created by aggregating cross-sectional data for bond issuance. The cross-sectional data was collected from Capital IQ Pro database and bond data was aggregated on a firm-year level. The data was merged with annual firm-level data using 1-year lag. The aggregated data was converted to logarithmic form when appropriate⁷. Additionally, simple average of bonds on firm-year level was calculated along with a weighted average (WAVG). The WAVG was calculated by calculating combined value of euro-denominated underwritten bonds on a firm-year level, followed by the calculation of what share of total issuance does each euro-denominated underwritten bond represent. Using the obtained share, the nominal gross spread, relative gross spread and coupon rate were multiplied prior to summation of obtained products - i.e. all on individual firm-year levels for euro-denominated underwritten bonds.

Lastly, it is crucial to understand credit ratings due to paper's heavy reliance on the ratings. The CSPP eligibility requires BBB- or better credit rating at the time of purchase for eligibility. However, due to data limitations, only credit rating at the time of data collection can be observed. This in turn prevents understanding what credit rating, if there even was one, the firm has in 2016 when issuing bonds. Therefore, the paper works with the limitation of using the most recent credit rating available with the assumption that the credit ratings did not greatly change since 2016. To limit the possible transitions between credit rating around threshold, [Section 4.5.2](#) re-does the analysis using stricter credit score requirement, assuming firms likely did not go from at best BB+ to at least A- since 2016.

4.5 Empirical Analysis

This section investigates the impact of the European Central Bank's Corporate Sector Purchase Programme (CSPP) on the primary corporate bond market, with a focus on issuance volumes and underwriting spreads. The analysis proceeds in several stages, combining treated-untreated comparisons,

⁷The logarithmic transformation was done when evaluating values presented in thousands of euros and not for shares (%) or ratios.

alternative treatment definitions, credit rating-specific examinations, and pre-post assessments to capture both the direct and indirect channels of policy transmission. The empirical strategy is designed to account for the CSPP's institutional features and the limitations of the available data. Since program eligibility at the time of purchase was credit-rating dependent, classification of treated firms is based on the most recent credit rating available in the dataset. This necessarily introduces some imprecision with historical ratings may differing from current ones, and some issuers lack ratings entirely. The analysis addresses this by not only using the baseline definition of BBB- or better eligibility, but also testing a narrower A- or better threshold in a robustness exercise. Additionally, because the euro-denominated corporate bond market features relatively few underwritten bonds, and even fewer once segmented by credit rating, the sample size in certain specifications is constrained, limiting statistical power.

4.5.1 Main Analysis

The empirical analysis begins with the panel data analysis within the context of the Euro-denominated bonds, examining the dynamic impacts of economic policies and market conditions on firm underwritten bond issuance and bond conditions. **Table 4.3** offers a regression analysis to test the parallel trends assumption, with the year 2015 serving as the baseline. The regression results, covering the period from 2013 to 2019, indicate that the interaction terms between the treatment status and the years before and after 2015 are statistically insignificant. This finding implies that the pre-treatment trends for treated and untreated firms were indeed parallel, validating the use of the TWFE methodology.

	Value (log €)	Coupon (average %)
	(1)	(2)
Treated * 2013	-0.566 (0.567)	-0.014 (0.057)
Treated * 2014	0.587 (0.567)	0.048 (0.057)
Treated * 2016	-0.405 (0.567)	-0.007 (0.057)
Treated * 2017	-0.667 (0.567)	-0.014 (0.057)
Treated * 2018	-0.134 (0.567)	0.017 (0.057)

Treated * 2019	-0.441	0.032
	(0.567)	(0.057)
Obs	10,829	10,829
R^2	0.008	0.001

Table 4.3: Empirical test of parallel trends for Eurozone firms from 2013 to 2019.

The parallel trends test treats all firms whose eligibility cannot be determined as untreated - separated during the analysis.

Robust standard errors in parentheses.

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

The coefficient plot in [Figure 4.6](#) that follows further visualizes these results, reinforcing the lack of significant deviations from the baseline trend.

[Table 4.4](#) presents the evolution of corporate bond issuance by the sample firms between 2013

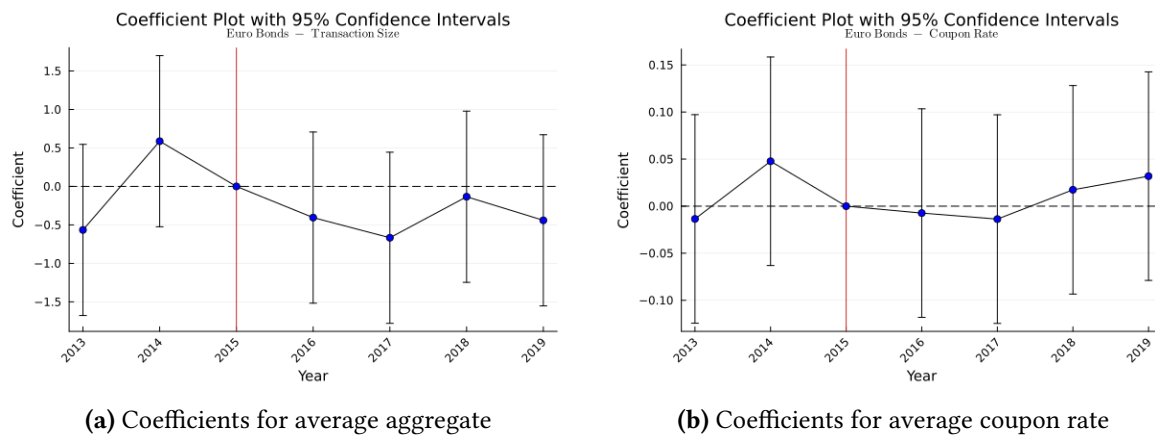


Figure 4.6: Visual representation of the parallel trends regression with 95% confidence intervals and a base year of 2015 - i.e. last pre-treatment year. The parallel trends test treats all firms whose eligibility cannot be determined as untreated - separated during the analysis.

and 2019, with particular attention to the period surrounding the introduction of the Corporate Sector Purchase Programme (CSPP). Across all specifications, the data offer no statistically discernible shift in issuance behavior attributable to the policy's implementation. Whether measured in nominal terms, transformed logarithmically to account for skewness, or averaged across firms with and without weighting, the series remains essentially flat through the intervention period. This absence of a detectable response suggests that, at least in aggregate, the CSPP did not alter firms' propensity to tap

bond markets for funding. If anything, the stability in both mean and weighted-average issuance levels underscores the robustness of pre-existing issuance patterns, pointing toward either limited transmission of the policy to the primary corporate debt market or an offsetting set of market forces that neutralized potential effects.

Panel A				
	Value (€)		Value (log €)	
	(1)	(2)	(3)	(4)
Treated * Post	5.332	-5.179	0.078	-0.052
	(6.345)	(7.784)	(0.080)	(0.099)
Obs	1,890	869	1,890	869
R^2	0.216	0.310	0.221	0.293
Controls	-	✓	-	✓
Panel B				
	Value (w. ave log €)		Value (average log €)	
	(1)	(2)	(3)	(4)
Treated * Post	0.075	-0.050	0.075	-0.050
	(0.080)	(0.099)	(0.080)	(0.099)
Obs	1,890	869	1,890	869
R^2	0.224	0.284	0.224	0.282
Controls	-	✓	-	✓

Table 4.4: Impact of CSPP on nominal bond issuance by firms from 2013 to 2019 inclusive. Treated firms have credit rating BBB- or better, with untreated firms either having credit rating below BBB- or are unrated. All firms have 3-digit NAICS code different than 522 and are incorporated in Eurozone.

Robust standard errors in parentheses.

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Turning from issuance volumes to issuance costs, [Table 4.5](#) reports results for the gross spread across the same set of specifications. Here, too, the overarching picture is one of relative stability. While the magnitudes are small in economic terms, they consistently hint at the possibility that the CSPP may have marginally compressed underwriting costs for the eligible issuers. The consistency

of the sign and size of these coefficients, even if insignificant, lends consistent results to an otherwise muted set of results. While the results fail to meet minimum criteria for statistical significance, the latter could be caused due to low number of observations rather than the true lack of effect.

Panel A				
	Spread (€)		Spread (log €)	
	(1)	(2)	(3)	(4)
Treated * Post	-25.255	-23.412	-0.083	-0.093
	(16.560)	(19.210)	(0.057)	(0.072)
Obs	1,890	869	1,890	869
R^2	0.239	0.151	0.224	0.150
Controls	-	✓	-	✓
Panel B				
	Spread (w. ave log €)		Spread (average log €)	
	(1)	(2)	(3)	(4)
Treated * Post	-0.082	-0.092	-0.081	-0.091
	(0.057)	(0.072)	(0.057)	(0.071)
Obs	1,890	869	1,890	869
R^2	0.223	0.150	0.224	0.150
Controls	-	✓	-	✓

Table 4.5: Impact of CSPP on nominal gross underwriting spread from 2013 to 2019 inclusive. Treated firms have credit rating BBB- or better, with untreated firms either having credit rating below BBB- or are unrated. All firms have 3-digit NAICS code different than 522 and are incorporated in Eurozone.

Robust standard errors in parentheses.

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

When the spread is expressed in percentage terms in [Table 4.6](#) rather than in basis points or nominal values, the evidence mostly aligns closely with the underwriting cost results. Without or with controls, no specification yields significant results at any level. This reinforces the impression that any observed contraction in spreads is fragile and potentially attributable to factors outside the program's influence.

	Spread (average %)		Spread (w. ave %)		Coupon (average %)	
	(1)	(2)	(3)	(4)	(5)	(6)
Treated * Post	-0.004	-0.004	-0.004	-0.004	0.002	-0.043
	(0.003)	(0.004)	(0.003)	(0.004)	(0.026)	(0.035)
Obs	1,890	869	1,890	869	1,890	869
R^2	0.245	0.152	0.244	0.151	0.196	0.205
Controls	-	✓	-	✓	-	✓

Table 4.6: Impact of CSPP on relative gross underwriting spread from 2013 to 2019 inclusive. Treated firms have credit rating BBB- or better, with untreated firms either having credit rating below BBB- or are unrated. All firms have 3-digit NAICS code different than 522 and are incorporated in Eurozone.

Robust standard errors in parentheses.

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

4.5.2 Robustness

To assess the stability of the main findings, this section explores an alternative definition of treated firms under the CSPP. In the baseline analysis, eligibility was assigned to issuers with a credit rating of BBB- or better. In this section, the threshold is tightened to include only firms rated A- or higher, thereby narrowing the treatment group to those with stronger credit profiles. This adjustment is motivated by the desire to isolate data limitations. CSPP eligibility at the time of purchase was credit-rating dependent, yet the ratings observed in the dataset reflect the most recent available assessments. These may not perfectly correspond to the ratings in place at the time of the ECB's purchases.

Table 4.7 reports the impact of this redefined treatment on nominal bond issuance, using the same four specifications as in the baseline analysis. Under this narrower eligibility criterion, the nominal issuance model with controls yields a statistically significant result with a coefficient of -14.462, significant at the 5% level. This negative sign suggests a reduction in nominal issuance among A- or better firms in the post-CSPP period, conditional on the included covariates. Similar results can be seen for nominal issuance without controls and logarithmic transformation, weighted and simple average with controls, all significant at 10%. Specifications imply that this effect is robust across alternative measures of issuance activity. The result therefore points to a systematic shift in issuance behavior under the revised treatment definition.

Panel A				
	Value (€)		Value (log €)	
	(1)	(2)	(3)	(4)
Treated * Post	-1.791	-14.462**	-0.008	-0.164*
	(5.635)	(7.257)	(0.074)	(0.096)
Obs	1,890	869	1,890	869
R^2	0.216	0.312	0.221	0.295
Controls	-	✓	-	✓
Panel B				
	Value (w. ave log €)		Value (average log €)	
	(1)	(2)	(3)	(4)
Treated * Post	-0.011	-0.163*	-0.011	-0.162*
	(0.073)	(0.095)	(0.073)	(0.095)
Obs	1,890	869	1,890	869
R^2	0.223	0.286	0.224	0.285
Controls	-	✓	-	✓

Table 4.7: Impact of CSPP on nominal bond issuance by firms from 2013 to 2019 inclusive. Treated firms have credit rating A- or better, with untreated firms either having credit rating below A- or are unrated. All firms have 3-digit NAICS code different than 522 and are incorporated in Eurozone.

Robust standard errors in parentheses.

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Moving from issuance volumes to issuance costs in [Table 4.8](#), the redefined treatment criterion fails to yield any noteworthy results on gross spread. Regardless of the specification and gross spread transformation, the models fail to find any significant results or impact due to CSPP, suggesting results obtained in the [Section 4.5.1](#) was not due to changes in lowest eligibility meeting credit rating group.

Panel A		
	Spread (€)	Spread (log €)

	(1)	(2)	(3)	(4)
Treated * Post	-27.867	-26.621	-0.092	-0.104
	(19.156)	(22.619)	(0.067)	(0.084)
Obs	1,890	869	1,890	869
R^2	0.239	0.152	0.224	0.151
Controls	-	✓	-	✓

Panel B

	Spread (w. ave log €)		Spread (average log €)	
	(1)	(2)	(3)	(4)
Treated * Post	-0.091	-0.103	-0.091	-0.102
	(0.067)	(0.083)	(0.067)	(0.083)
Obs	1,890	869	1,890	869
R^2	0.224	0.151	0.225	0.151
Controls	-	✓	-	✓

Table 4.8: Impact of CSPP on nominal gross underwriting spread from 2013 to 2019 inclusive. Treated firms have credit rating A- or better, with untreated firms either having credit rating below A- or are unrated. All firms have 3-digit NAICS code different than 522 and are incorporated in Eurozone.

Robust standard errors in parentheses.

*** p<0.01, ** p<0.05, * p<0.1

When expressed in percentage terms in [Table 4.9](#), the gross spread results reinforce the lack of observed pattern. Under the revised treatment definition, the coefficients are robust in direction, but fail to find any impact from CSPP. However, the analysis provides a negative, statistically significant result for a reduction in coupon rate, significant at 10%. This finding indicate that, for higher-rated issuers, the CSPP period may have been associated with a modest yet measurable easing in primary market costs due to coupon rate reduction, even when issuance volumes themselves displayed little systematic change.

	Spread (average %)		Spread (w. ave %)		Coupon (average %)	
	(1)	(2)	(3)	(4)	(5)	(6)
Treated * Post	-0.005	-0.005	-0.005	-0.005	-0.017	-0.065*

	(0.004)	(0.004)	(0.004)	(0.004)	(0.028)	(0.036)
Obs	1,890	869	1,890	869	1,890	869
R^2	0.245	0.153	0.244	0.152	0.196	0.207
Controls	-	✓	-	✓	-	✓

Table 4.9: Impact of CSPP on relative gross underwriting spread from 2013 to 2019 inclusive. Treated firms have credit rating A- or better, with untreated firms either having credit rating below A- or are unrated. All firms have 3-digit NAICS code different than 522 and are incorporated in Eurozone.

Robust standard errors in parentheses.

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

To further verify that the estimated effects are not driven by spurious correlations or pre-trends, the paper re-estimates the TWFE model, replacing the actual post-treatment indicator with placebo post dates set one and two years before the true CSPP start year. For each placebo test as shown in [Table 4.10](#), the sample is restricted to the pre-policy period to ensure that no observations from the actual treatment period are included. While several specifications in the baseline analysis yield statistically significant treatment effects, none of these remain significant under the placebo definitions. This absence of placebo effects suggests that the baseline results are not driven by anticipatory behavior or other pre-existing differences, reinforcing the credibility of the identification strategy.

	2015 Placebo		2014 Placebo	
	(1)	(2)	(3)	(4)
Value (€)	-7.885	-10.521	3.7	0.467
	(9.464)	(10.903)	(9.741)	(13.297)
Value (log)	-0.062	-0.143	0.067	0.056
	(0.111)	(0.133)	(0.115)	(0.159)
Value (w. ave log)	-0.068	-0.147	0.058	0.049
	(0.111)	(0.133)	(0.113)	(0.155)
Value (average log)	-0.069	-0.147	0.056	0.048
	(0.11)	(0.133)	(0.113)	(0.154)
Spread (€)	-27.961	-32.982	27.961	35.472
	(34.086)	(41.24)	(34.086)	(42.937)

Spread (log)	-0.101 (0.123)	-0.119 (0.149)	0.101 (0.123)	0.128 (0.155)
Spread (w. ave log)	-0.101 (0.123)	-0.119 (0.149)	0.101 (0.123)	0.128 (0.155)
Spread (average log)	-0.101 (0.123)	-0.119 (0.149)	0.101 (0.123)	0.128 (0.155)
Spread (average %)	-0.006 (0.007)	-0.007 (0.008)	0.006 (0.007)	0.007 (0.009)
Spread (w. ave %)	-0.006 (0.007)	-0.007 (0.008)	0.006 (0.007)	0.007 (0.009)
Coupon (average %)	-0.013 (0.05)	-0.052 (0.057)	0.027 (0.05)	0.024 (0.062)
Controls	-	✓	-	✓

Table 4.10: Table reports coefficients on the interaction between the robustness treatment indicator and alternative placebo post-CSPP periods, along with the main post-CSPP specification. The placebo post periods are defined as one and two years before the actual policy implementation year, and the regressions are restricted to pre-policy observations to avoid contamination from true post-CSPP effects.

Robust standard errors in parentheses.

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

The robustness exercise, restricting treatment to firms rated A- or higher, yields a more nuanced picture than the baseline results. While the impact on issuance volumes remains generally weak, emerging significantly only in a single nominal with controls specification, the evidence for underwriting spread compression becomes more coherent. Across nominal, log, weighted-average, and percentage measures, the coefficients are uniformly negative, with several achieving conventional significance thresholds. This suggests that the CSPP's influence, if present, may have operated more through the pricing channel than through the quantity of debt issued, and that higher-quality issuers were marginally better positioned to capture these benefits. Nonetheless, the effect sizes remain modest, cautioning against overinterpretation and underscoring the importance of complementary factors in shaping primary market conditions.

4.5.3 Role of Credit Rating

The preceding sections examined the CSPP's effects through a binary lens - i.e. treated versus untreated issuers, either in the baseline BBB- or better definition or in the robustness check using A- or better. While informative, such dichotomies inevitably mask variation within the treated and untreated categories. Issuers across the credit-quality spectrum may respond differently to the same policy intervention, reflecting differences in market access, investor demand, and refinancing needs. To uncover these dynamics, this section disaggregates the analysis by credit rating category⁸, directly comparing issuance behavior across specific rating groups. This approach allows for a more granular view of how market reactions to the CSPP may have varied along the credit-quality dimension, rather than being averaged into broad eligibility buckets.

To examine differences across credit rating categories, the model is re-estimated in a matrix format where each row corresponds to a separate regression using a different credit rating group as the base category. The specification is otherwise identical to the baseline model described earlier, with the same dependent variables, control variables, and fixed effects structure. Changing the base category alters the interpretation of coefficients, allowing pairwise comparisons between each rating group and the selected reference group. Because the regressions are symmetric in design, missing entries on one side of the matrix correspond to the opposite comparison elsewhere in the table. [Table 4.11](#), estimated without controls, reveals no statistical results. For all issuance metrics, the results suggest no correlation between different credit ratings.

Panel A: Bond issuance (€)					
Base	NR	C	B	A	AA
NR		2.149 (3.448)	37.863 (24.059)	-1.324 (6.727)	2.149 (3.427)
C			35.714 (23.835)	-3.472 (5.875)	0.0 (1.004)
B				-39.187 (24.525)	-35.714 (23.832)
A					3.472 (5.863)

⁸Ratings AA- or better classify as AA, A- to A+ as A category, BBB- to BBB+ as B category, BB+ or lower as C category and all unrated firms are categorized as NR category.

Panel B: Bond issuance (log €)

Base	NR	C	B	A	AA
NR		0.025 (0.037)	0.469 (0.299)	0.002 (0.09)	0.025 (0.036)
C			0.444 (0.297)	-0.023 (0.083)	0.0 (0.011)
B				-0.467 (0.308)	-0.444 (0.297)
A					0.023 (0.083)

Panel C: Bond issuance (average log €)

Base	NR	C	B	A	AA
NR		0.022 (0.035)	0.466 (0.299)	-0.001 (0.089)	0.022 (0.035)
C			0.444 (0.297)	-0.023 (0.083)	0.0 (0.011)
B				-0.467 (0.308)	-0.444 (0.297)
A					0.023 (0.083)

Table 4.11: Impact of CSPP on total bond issuance by the firms from 2013 to 2019 inclusive without controls. All firms without a credit rating are treated as "NR" - i.e. not rated. Results matrix is a series of repeated PCFAM regressions with a changing base as shown in column 1. Missing values are merely the opposite values. Coefficients and standard errors reported in the table above correspond to the variable *Post * Credit Rating*, with each row reporting regression results with the base credit rating corresponding to the credit rating in the first column.

Robust standard errors in parentheses.

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

A similar structure emerges when including controls, with C- and AA-rated firms issue significantly

less than NR firms, both significant at 10%. When considering the log and average log of issuance, the results remain consistent, with an improvement in significance for AA rated firms. The results for C-rated firms remains consistent at 10%; however, the results of AA-rated firms, when comparing to NR firms improves to 5% significance. The results suggest non-targeted C-rated firms and highest rated AA-rated firms see statistical change compared to non-rated, but no other dynamics between ratings can be observed.

Panel A: Bond issuance (€)

Base	NR	C	B	A	AA
NR		-8.739*	28.754	-13.173	-8.269*
		(5.297)	(26.438)	(8.197)	(4.768)
C			37.493	-4.434	0.469
			(26.314)	(7.568)	(3.558)
B				-41.927	-37.023
				(26.966)	(26.205)
A					4.903
					(7.099)

Panel B: Bond issuance (log €)

Base	NR	C	B	A	AA
NR		-0.112*	0.354	-0.145	-0.106**
		(0.062)	(0.327)	(0.11)	(0.054)
C			0.466	-0.033	0.005
			(0.326)	(0.104)	(0.042)
B				-0.499	-0.46
				(0.337)	(0.325)
A					0.038
					(0.1)

Panel C: Bond issuance (average log €)

Base	NR	C	B	A	AA
NR		-0.109*	0.356	-0.142	-0.104**

	(0.061)	(0.327)	(0.109)	(0.053)
C		0.466	-0.033	0.005
		(0.326)	(0.104)	(0.041)
B			-0.499	-0.461
			(0.338)	(0.325)
A				0.038
				(0.1)

Table 4.12: Impact of CSPP on total bond issuance by the firms from 2013 to 2019 inclusive with controls. All firms without a credit rating are treated as "NR" - i.e. not rated. Results matrix is a series of repeated PCFAM regressions with a changing base as shown in column 1. Missing values are merely the opposite values. Coefficients and standard errors reported in the table above correspond to the variable *Post * Credit Rating*, with each row reporting regression results with the base credit rating corresponding to the credit rating in the first column.

Robust standard errors in parentheses.

*** p<0.01, ** p<0.05, * p<0.1

Shifting focus to issuance costs in [Table 4.13](#), the credit rating group comparisons reveal several statistically significant differences even without controls. The log and average log specifications show a consistent pattern with AA-rated issuers have lower spreads than non-rated issuers at the 10% level. In the coupon specification, B-rated issuers face higher yields than A-rated firms at the 10% level. Taken together, these results suggest that, in the uncontrolled setting, cost differentials across the rating spectrum are evident and generally benefit higher-rated peers.

Panel A: Gross spread (€)					
Base	NR	C	B	A	AA
NR		-9.775	-9.775	-33.386	-9.775
		(6.686)	(6.63)	(23.913)	(6.588)
C			-0.0	-23.611	-0.0
			(3.106)	(23.185)	(3.015)
B				-23.611	-0.0
				(23.169)	(2.889)
A					23.611
					(23.157)

Panel B: Gross spread (log €)

Base	NR	C	B	A	AA
NR		-0.027 (0.017)	-0.027 (0.016)	-0.112 (0.085)	-0.027* (0.016)
C			0.0 (0.009)	-0.085 (0.083)	0.0 (0.008)
B				-0.085 (0.083)	0.0 (0.008)
A					0.085 (0.083)

Panel C: Gross spread (average log €)

Base	NR	C	B	A	AA
NR		-0.025 (0.016)	-0.025 (0.015)	-0.11 (0.084)	-0.025* (0.015)
C			0.0 (0.008)	-0.085 (0.083)	0.0 (0.008)
B				-0.085 (0.083)	0.0 (0.008)
A					0.085 (0.083)

Panel D: Gross spread (average %)

Base	NR	C	B	A	AA
NR		-0.001 (0.001)	-0.001 (0.001)	-0.006 (0.005)	-0.001 (0.001)
C			0.0 (0.0)	-0.005 (0.005)	0.0 (0.0)
B				-0.005 (0.005)	0.0 (0.0)

A	0.005
	(0.005)

Panel E: Coupon (average %)

Base	NR	C	B	A	AA
NR		0.01	0.086	-0.02	0.01
		(0.015)	(0.053)	(0.034)	(0.014)
C			0.076	-0.03	0.0
			(0.051)	(0.032)	(0.008)
B				-0.106*	-0.076
				(0.06)	(0.051)
A					0.03
					(0.031)

Table 4.13: Impact of CSPP on gross spread from 2013 to 2019 inclusive without controls. All firms without a credit rating are treated as "NR" - i.e. not rated. Results matrix is a series of repeated PCFAM regressions with a changing base as shown in column 1. Missing values are merely the opposite values. Coefficients and standard errors reported in the table above correspond to the variable *Post* * *Credit Rating*, with each row reporting regression results with the base credit rating corresponding to the credit rating in the first column.

Robust standard errors in parentheses.

*** p<0.01, ** p<0.05, * p<0.1

Once controls are introduced in [Table 4.14](#), the results diminish in significance. While the dynamics between B- and A-rated firms remains for coupon rates, the remaining results diminish to no significance. The coefficients remain similar in level and direction for majority of comparisons, but possibly due to limited number of observations per category, the significance is not detected.

Panel A: Gross spread (€)

Base	NR	C	B	A	AA
NR		-4.582	-5.445	-31.955	-5.149
		(6.743)	(5.747)	(27.465)	(5.897)
C			-0.863	-27.373	-0.567
			(4.914)	(27.689)	(4.976)

B	-26.51	0.296
	(26.678)	(3.516)
A		26.807
		(27.013)

Panel B: Gross spread (log €)

Base	NR	C	B	A	AA
NR		-0.026	-0.029	-0.124	-0.028
		(0.033)	(0.03)	(0.101)	(0.031)
C			-0.003	-0.099	-0.002
			(0.02)	(0.1)	(0.02)
B				-0.095	0.001
				(0.096)	(0.014)
A					0.096
					(0.098)

Panel C: Gross spread (average log €)

Base	NR	C	B	A	AA
NR		-0.023	-0.026	-0.122	-0.025
		(0.031)	(0.027)	(0.101)	(0.028)
C			-0.003	-0.099	-0.002
			(0.019)	(0.1)	(0.019)
B				-0.095	0.001
				(0.096)	(0.014)
A					0.096
					(0.097)

Panel D: Gross spread (average %)

Base	NR	C	B	A	AA
NR		-0.0	-0.001	-0.006	-0.001
		(0.001)	(0.001)	(0.005)	(0.001)

C	-0.0 (0.001)	-0.005 (0.006)	-0.0 (0.001)
B		-0.005 (0.005)	0.0 (0.001)
A			0.005 (0.005)

Panel E: Coupon (average %)

Base	NR	C	B	A	AA
NR		-0.035 (0.027)	0.045 (0.06)	-0.069 (0.043)	-0.032 (0.023)
C			0.08 (0.057)	-0.034 (0.039)	0.003 (0.016)
B				-0.114* (0.066)	-0.077 (0.056)
A					0.037 (0.037)

Table 4.14: Impact of CSPP on gross spread from 2013 to 2019 inclusive with controls. All firms with a credit rating missing are treated as "NR" - i.e. not rated. Results matrix is a series of repeated PCFAM regressions with a changing base as shown in column 1. Missing values are merely the opposite values. Coefficients and standard errors reported in the table above correspond to the variable *Post* * *Credit Rating*, with each row reporting regression results with the base credit rating corresponding to the credit rating in the first column.

Robust standard errors in parentheses.

*** p<0.01, ** p<0.05, * p<0.1

4.5.4 Pre-Post Analysis

While the baseline and robustness specifications identify CSPP effects by exploiting cross-sectional variation in eligibility or credit quality, they implicitly assume that the relationship between treated and untreated issuers is stable over time. A complementary perspective is offered by a pre-post analysis, which examines changes in issuance behavior and pricing within each group before and after the program's introduction. This approach focuses on temporal shifts rather than purely cross-sectional

contrasts, allowing us to capture whether the CSPP coincided with structural breaks in market outcomes for different issuer categories.

For the pre-post analysis, the specification remains the same as in the baseline model, but the variable of interest is now a post-program indicator capturing the period after the CSPP's introduction. The analysis is implemented both for the treated-untreated framework and for individual credit rating categories. In the credit rating-level tables, each cell represents a separate regression, enabling direct before-after comparisons within each segment. Any missing entries occur where insufficient observations prevent estimation.

The chapter proceeds in two stages. First, I compare the evolution of key issuance and spread metrics for the treated and untreated groups under the original eligibility definition, highlighting whether program onset was associated with significant within-group changes. Second, I disaggregate further, performing the same before-after comparisons within individual credit rating categories. This dual structure makes it possible to identify whether any observed program effects are broadly shared across eligible issuers or concentrated among specific rating tiers, and whether the magnitude or direction of change differs across the credit-quality spectrum.

In the treated-untreated framework reported in [Table 4.15](#), the pre-post comparison reveals an asymmetric pattern in issuance behaviour. For treated firms, no specification, whether nominal value, log-transformed issuance, or average log issuance, yields a statistically significant change between the pre- and post-CSPP periods. The notable exception are the gross spread and log of gross spread for untreated group without controls with positive coefficient at 10% significance. This absence of measurable movement suggests that, within the eligible group, the CSPP's introduction did not materially alter primary market issuance intensity.

	Untreated		Treated	
	(1)	(2)	(3)	(4)
Value (€)	-2.091 (3.434)	2.191 (4.308)	3.241 (5.559)	2.863 (5.876)
Value (log €)	-0.025 (0.037)	0.031 (0.049)	0.054 (0.074)	0.048 (0.077)
Value (average log €)	-0.021 (0.035)	0.036 (0.047)	0.054 (0.074)	0.048 (0.077)
Spread (€)	9.514 (6.195)	3.076 (3.091)	-15.741 (15.732)	-17.917 (18.021)

Spread (log €)	0.026*	0.017	-0.057	-0.065
	(0.015)	(0.017)	(0.057)	(0.065)
Spread (average log €)	0.025*	0.015	-0.057	-0.065
	(0.014)	(0.015)	(0.057)	(0.065)
Spread (average %)	0.001	0.0	-0.003	-0.004
	(0.001)	(0.0)	(0.003)	(0.004)
Coupon (average %)	-0.01	0.016	-0.008	-0.011
	(0.014)	(0.018)	(0.023)	(0.026)
Controls	-	✓	-	✓

Table 4.15: Impact of CSPP on outcome variables from 2013 to 2019 inclusive without controls. All firms with a credit rating missing are treated as "NR" - i.e. not rated. Results presented are direct pre-post analysis per outcome-group level individually. Table reports coefficients and standard errors for *Post* variable of the regression, with each result representing one regression.

Robust standard errors in parentheses.

*** p<0.01, ** p<0.05, * p<0.1

Extending the pre-post comparison to the individual credit rating categories in Table 4.16 yields little evidence of systematic shifts around the CSPP's introduction. The C-rated and AA-rated groups lack sufficient observations for reliable estimation and are therefore excluded from the analysis. Among the remaining rating categories, no statistically significant changes are detected in issuance volumes nor in any of the spread specifications, including nominal, log, and percentage measures. Exception to the lack of singificance can be observed for logarithmically transformed and average of logarithmically transformed gross spread for unrated group without controls, yielding results significant at 10%.

The absence of detectable effects suggests that, once issuers are segmented into finer rating tiers, the pre-post variation in issuance and pricing largely dissipates. This could indicate that any temporal changes observed in the broader treated-untreated comparison are driven by compositional differences across ratings rather than uniform shifts within specific credit-quality bands. In short, when evaluated in isolation, no rating group displays a clear structural break in market behavior coinciding with the program's launch.

Panel A: Without controls					
	NR	C	B	A	AA
Value (€)	-2.149		35.714	-3.472	

	(3.528)	(24.848)	(5.93)
Value (log €)	-0.025	0.444	-0.023
	(0.038)	(0.309)	(0.083)
Value (average log €)	-0.022	0.444	-0.023
	(0.036)	(0.309)	(0.083)
Spread (€)	9.775		-23.611
	(6.365)		(23.592)
Spread (log €)	0.027*		-0.085
	(0.015)		(0.085)
Spread (average log €)	0.025*		-0.085
	(0.015)		(0.085)
Spread (average %)	0.001		-0.005
	(0.001)		(0.005)
Coupon (average %)	-0.01	0.076	-0.03
	(0.014)	(0.053)	(0.032)

Panel B: With controls

	NR	C	B	A	AA
Value (€)	2.269		23.777	-4.189	
	(4.45)		(17.217)	(5.623)	
Value (log €)	0.033		0.296	-0.04	
	(0.051)		(0.214)	(0.074)	
Value (average log €)	0.037		0.296	-0.04	
	(0.049)		(0.214)	(0.074)	
Spread (€)	3.153			-23.067	
	(3.169)			(23.247)	
Spread (log €)	0.017			-0.083	
	(0.017)			(0.084)	
Spread (average log €)	0.016			-0.083	
	(0.016)			(0.084)	
Spread (average %)	0.0			-0.005	

	(0.0)		(0.005)
Coupon (average %)	0.017	0.05	-0.03
	(0.019)	(0.036)	(0.031)

Table 4.16: Impact of CSPP on outcome variables from 2013 to 2019 inclusive. All firms with a credit rating missing are treated as "NR" - i.e. not rated. Results presented are direct pre-post analysis per outcome-credit rating classification level individually. Table reports coefficients and standard errors for *Post* variable of the regression, with each result representing one regression.

Robust standard errors in parentheses.

*** p<0.01, ** p<0.05, * p<0.1

4.5.5 Role of Past Bond Issuance

A key control variable in the context of bond issuance is past issuance, which refers to the prior issuance of bonds by the firms. This variable is critical as past issuance can influence current contemporary issuance through trust, transparency and networking. By controlling for past issuance, the regression model can more accurately isolate the effects of the primary variables of interest, thereby providing a clearer understanding of the underlying economic dynamics. Including past issuance as a control variable enhances the model's robustness and reliability. It mitigates the risk of omitted variable bias, ensuring that the results are not skewed by the past behaviors.

The analysis of past issuance requires the modification of Equation 4 with the inclusion of *Past Bond_{i,t}* variable in a triple-interaction term. The variable *Past Bond_{i,t}* takes one of three possible values: (1) *Past Euro_{i,t}*, (2) *Past Foreign_{i,t}* or (3) *Past Issue_{i,t}*. The first variable takes a value of 1 if the firm *i* issued at least 1 Euro-denominated bond in the three years period prior to year *t* - i.e. in years *t* - 3 to *t* - 1. The second takes a value of 1 if the firm issued at least 1 foreign currency bond - i.e. non-Euro bond, in the three years period prior to year *t*. Lastly, the *Past Issue_{i,t}* takes a value of 1 if either *Past Euro_{i,t}* or *Past Foreign_{i,t}* takes a value of 1, corresponding to taking a value of 1 if firm *i* issued at least 1 bond irrespective of the currency in the three-year period prior to year *t*. The section relies on the Equation 5:

$$\begin{aligned}
 y_{i,t} = & \alpha + \beta_2 \text{Past Bond}_{i,t} + \beta_2 \text{Treated}_i * \text{Post}_t + \beta_3 \text{Treated}_i * \text{Past Bond}_{i,t} + \\
 & + \beta_4 \text{Post}_t * \text{Past Bond}_{i,t} + \beta_5 \text{Treated}_i * \text{Post}_t * \text{Past Bond}_{i,t} + \\
 & + \beta X_{i,t-1} + \mu_i + \xi_t + \varepsilon_{i,t}
 \end{aligned} \tag{5}$$

The inclusion of past issuance starts in [Table 4.17](#) with the impacts on bond aggregate. In Panel A, the regression results study the impact of past Euro-denominated issuance on the bond aggregate. The coefficients for the variable “Past Euro” across all models are relatively small and not statistically significant, indicating a limited direct effect on the bond aggregates. However, the interaction terms involving treatment and post-treatment periods suggest nuanced dynamics. Specifically, the “Post * Past Euro” interaction shows negative and statistically significant coefficients, suggesting that past issuance has a diminishing effect on bond aggregates in the post-treatment period. This implies that historical Euro-denominated issuance might lead to reduced bond aggregates post-intervention for Euro-denominated bonds. Panel B, addressing the impact of past issues in a general sense, presents mixed results. The direct effect of “Past Issue” is generally insignificant, while the interaction terms show varying significance. Notably, the “Post * Past Issue” interaction is significantly negative without controls, indicating that past issuance negatively impact bond aggregates post-treatment. This pattern is consistent with the notion that past market activities might lead to reduced marginal gains in subsequent periods.

Panel A		
	Value (log €)	
	(1)	(2)
Treated * Post	-0.010 (0.118)	-0.090 (0.159)
Treated * Past Euro	-0.002 (0.134)	-0.015 (0.165)
Post * Past Euro	-0.093*** (0.029)	-0.107* (0.061)
Treated * Post * Past Euro	0.166 (0.152)	0.196 (0.192)
Obs	10,829	3,500
R^2	0.260	0.415
Controls	-	✓
Panel B		
	Value (log €)	
	(1)	(2)

Treated * Post	0.096 (0.123)	0.095 (0.168)
Treated * Past Issue	0.187 (0.139)	0.240 (0.174)
Post * Past Issue	-0.092*** (0.029)	-0.091 (0.062)
Treated * Post * Past Issue	-0.020 (0.154)	-0.076 (0.198)
Obs	10,829	3,500
R^2	0.261	0.415
Controls	-	✓

Table 4.17: Panel A includes past issuance of currency matching bond - i.e. Panel A studies whether past bond-issuance impacted contemporary bond issuance. Past issuance - i.e. currency matching or general issuance, is determined whether the firm issued at least one bond matching currency condition in the 3-year period prior to year t .

Robust standard errors in parentheses.

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

In [Table 4.18](#), the regression results focus on Euro-denominated bonds. Here, the regression results highlight a significant and negative “Post * Past Euro” interaction term on the weighted average bond aggregate and coupon rates. This effect is contrasted by the “Treated * Post * Past Euro” interaction, which shows a positive and significant impact on the spread.

	Value (w. ave log €)		Spread (w. ave log €)		Coupon (average %)	
	(1)	(2)	(3)	(4)	(5)	(6)
Treated * Post	-0.009 (0.114)	-0.093 (0.152)	-0.007 (0.005)	-0.011*** (0.002)	-0.056 (0.051)	-0.065 (0.054)
Treated * Past Euro	0.002 (0.129)	-0.015 (0.157)	-0.008 (0.006)	-0.011*** (0.002)	-0.051 (0.058)	-0.033 (0.056)
Post * Past Euro	-0.082*** (0.028)	-0.099* (0.058)	0.000 (0.001)	0.000 (0.001)	-0.063*** (0.013)	-0.035* (0.021)

Treated * Post *	0.157	0.190	0.008	0.011***	0.100	0.081
Past Euro	(0.147)	(0.183)	(0.007)	(0.003)	(0.066)	(0.065)
Obs	10,829	3,500	10,829	3,500	10,829	3,500
R^2	0.250	0.403	0.151	0.157	0.182	0.419
Controls	-	✓	-	✓	-	✓

Table 4.18: Panels A and B include past issuance of currency matching bond - i.e. Panel A studies whether past bond-issuance impacted contemporary bond issuance. Past issuance - i.e. currency matching or general issuance, is determined whether the firm issued at least one bond matching currency condition in the 3-year period prior to year t .

Robust standard errors in parentheses.

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table 4.19 focuses on analyzing broader past issuance. The results indicate that past issuance have a more pronounced impact. The “Post * Past Issue” interaction term is significantly negative for bond aggregate and coupon rates. Conversely, the “Treated * Post * Past Issue” interaction term shows mixed results, with some negative impacts on the spread.

	Value (w. ave log €)		Spread (w. ave %)		Coupon (average %)	
	(1)	(2)	(3)	(4)	(5)	(6)
Treated * Post	0.097	0.093	0.000	0.000	-0.009	0.008
	(0.119)	(0.160)	(0.006)	(0.002)	(0.053)	(0.057)
Treated * Past	0.192	0.241	0.004	0.004	0.030	0.072
Issue	(0.135)	(0.166)	(0.006)	(0.002)	(0.060)	(0.059)
Post * Past Issue	-0.082***	-0.083	0.000	0.000	-0.062***	-0.040*
	(0.028)	(0.059)	(0.001)	(0.001)	(0.012)	(0.021)
Treated * Post *	-0.029	-0.083	-0.005	-0.006**	0.017	-0.026
Past Issue	(0.149)	(0.189)	(0.007)	(0.003)	(0.066)	(0.067)
Obs	10,829	3,500	10,829	3,500	10,829	3,500
R^2	0.250	0.403	0.151	0.149	0.182	0.419

Controls	-	✓	-	✓	-	✓
----------	---	---	---	---	---	---

Table 4.19: Past issuance is determined whether the firm issued at least one bond matching currency condition in the 3-year period prior to year t .

Robust standard errors in parentheses.

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

The investigation of the impacts of past bond issuance on various metrics consistently showed that past issuance have limited direct effects on bond aggregates but significantly influence borrowing costs, particularly through interactions with treatment conditions. Specifically, past foreign issuance generally enhance borrowing conditions for foreign currency denominated bonds, whereas combined effects with post-treatment periods can lead to increased costs.

4.6 Discussion

The results across all specifications and analytical perspectives paint a complex, and at times counterintuitive, picture of the CSPP's influence on primary corporate bond market activity. Far from delivering a uniform, unambiguous boost to issuance volumes among eligible firms, the evidence suggests that the program's effects, if present, are highly contingent on both the metric under consideration and the analytical lens applied.

The modest issuance increase for untreated firms in the pre-post analysis suggests a degree of positive spillover, or contagion, from CSPP eligibility into the wider market. This could operate through several mechanisms. First, portfolio rebalancing by investors selling CSPP-eligible bonds to the ECB may have redirected demand toward ineligible corporate debt, easing funding conditions indirectly. Second, the program's presence could have improved overall market sentiment and reduced perceived systemic risk, encouraging issuance even by non-beneficiaries. Finally, a more technical channel might involve relative pricing adjustments across the corporate credit spectrum, whereby yield compression for eligible bonds narrows spreads for similar-risk but ineligible securities.

The credit rating-level results temper this interpretation somewhat. The absence of significant pre-post changes within individual rating categories, aside from the aggregate untreated group, suggests that any contagion effects are diffuse and compositional, rather than concentrated within specific credit tiers. In other words, the upward shift in untreated issuance appears to be a market-level phenomenon rather than a targeted improvement in conditions for a given rating segment.

These findings align with strands of the literature that question the breadth and depth of unconventional monetary policy transmission to the corporate primary market. While secondary market yields

often display more immediate and measurable responses to large-scale asset purchases, the translation into new issuance volumes and terms is frequently partial and mediated by firm-specific considerations, capital market conditions, and investor behavior. The CSPP's design, focused on eligible bonds in the secondary market, may naturally have had a stronger direct effect on pricing than on primary issuance decisions, particularly for firms already enjoying favorable market access.

Although the gross spread in the data aggregates the management fee, underwriting fee, and selling concession, the structure of the corporate bond underwriting process suggests that the observed reduction is likely driven by lower underwriting fees. Underwriting fees are closely linked to the underwriter's assessment of placement risk and the level of competition among syndicate members. By reducing the perceived credit risk of CSPP-eligible issuers and increasing investor appetite for their bonds, the program may have intensified competition among underwriters, thereby exerting downward pressure on this component of the spread. Selling concessions could also have narrowed if demand conditions in the primary market improved as a result of the ECB's purchases, limiting the need for underwriters to offer price incentives to investors. In contrast, management fees tend to reflect fixed administrative and coordination costs and are therefore less sensitive to shifts in market risk or demand. Taken together, the evidence and market logic suggest that the bulk of the observed spread compression is likely attributable to the more variable, risk-sensitive elements of the gross spread, particularly the underwriting fee, rather than to fixed-cost components.

In the broader context of unconventional policy, these results suggest that while secondary market purchases can have meaningful pricing effects, especially for the strongest credits, their capacity to uniformly stimulate new corporate bond issuance is limited. Spillovers to ineligible firms exist but are subtle, hinting at contagion that is more atmospheric than structural. Ultimately, the CSPP appears to have worked more as a pricing stabilizer than as a catalyst for a broad-based issuance boom.

The discussion also emphasizes that the weighted average and average bond issuance behaviors remain nearly identical, indicating no fundamental change in how firms issue bonds post-CSPP. This consistency suggests that while the cost of issuance might be affected, the overall strategies and behaviors of firms regarding bond issuance remain stable.

While this study focuses on the euro-denominated corporate bond market, the CSPP's distinctive nature as a large-scale primary market intervention raises broader questions for global bond markets and central banking practice. The implications for other currency areas, potential cross-border spillovers through portfolio rebalancing, and lessons for central banks considering similar measures merit further investigation. Exploring these wider effects in depth lies beyond the scope of the present analysis but will be the subject of a future research.

Overall, the analysis of the CSPP's impact must be interpreted in light of important data constraints.

A substantial share of issuers in the sample lack historical credit rating information, limiting the precision of rating-based classifications and, by extension, the ability to draw firm conclusions about differential program effects across credit tiers. In addition, the relatively low volume of underwritten bonds in the euro-denominated corporate bond market further reduces the number of usable observations, particularly when the data are segmented by rating, issuance type, or specification. These factors not only constrain statistical power, making it more difficult to detect smaller but potentially meaningful effects, but also increase the sensitivity of results to model specification and sample composition. Taken together, the scarcity of complete credit rating histories and the limited depth of the underwritten eurobond market underscore the challenges of empirically isolating the CSPP's impact, and highlight the need for richer datasets to fully capture the program's transmission channels.

4.7 Conclusion

This paper examined the effects of the ECB's Corporate Sector Purchase Programme (CSPP) on primary corporate bond market activity, drawing on an extensive dataset covering issuance volumes and underwriting spreads from 2013 to 2019. By combining treated-untreated comparisons, credit rating-specific analyses, robustness checks with alternative eligibility definitions, and pre-post evaluations, the study provides a multifaceted assessment of the program's reach and limitations.

The evidence points to a limited direct impact of the CSPP on issuance volumes for eligible firms. Across most specifications, treated issuers did not expand their debt issuance in response to the program, and in some cases, particularly among higher-rated firms under the A- or better robustness definition with issuance even contracted slightly. By contrast, the program's influence on issuance costs is more discernible, especially for higher-quality credits. Robustness checks and rating-level analyses reveal consistent and statistically significant reductions in underwriting spreads for AA-rated firms relative to non-rated peers across nominal, log, and percentage measures, even after controlling for observable characteristics. These findings suggest that the CSPP's most tangible impact is to compress primary market pricing for top-rated issuers, while lower-rated and unrated firms saw little measurable benefit.

Taken together, the results underscore the heterogeneity of CSPP transmission. The program's design, focused on secondary market purchases of investment-grade corporate debt, appears to have disproportionately advantaged already well-positioned firms, providing them with marginally lower funding costs but not necessarily inducing additional issuance. Spillover effects to ineligible firms were present but modest, and their strength varied depending on the analytical lens applied.

In sum, the CSPP's legacy in the primary corporate bond market is one of subtlety, a quiet reshaping of pricing conditions in favor of the strongest credits, a diffuse but shallow contagion to the wider market, and only marginal shifts in the quantity of debt issued. Its impacts, though real, were neither uniform nor transformative, offering both lessons and caution for the design of future unconventional monetary policy interventions.

4.8 Appendix

4.8.1 Additional Tables

Panel A: Number of Issued Bonds

Year	Bonds	UW Bonds	EUR Bonds	UW EUR Bonds
2011	8,472	122	4,644	61
2012	7,721	190	4,015	113
2013	6,955	200	3,677	82
2014	7,107	147	4,048	71
2015	6,880	136	3,556	41
2016	7,016	155	3,480	56
2017	7,321	96	3,379	29
2018	7,430	138	3,105	49
2019	7,241	52	3,119	24

Panel B: Value of Issued Bonds (in EUR Billions)

Year	Bonds	UW Bonds	EUR Bonds	UW EUR Bonds
2011	1,484.43	58.93	1,091.55	48.17
2012	1,750.47	84.39	1,208.61	60.26
2013	1,264.87	36.09	798.42	14.34
2014	1,173.49	22.03	798.33	12.81
2015	1,379.69	16.33	852.86	7.40
2016	1,283.08	32.87	789.50	9.08
2017	1,224.47	20.76	736.39	8.24
2018	1,210.45	24.61	754.79	15.45
2019	1,248.41	19.59	770.03	12.76

Panel C: Number of Firms Issuing Bonds

Year	Bonds	UW Bonds	EUR Bonds	UW EUR Bonds
------	-------	----------	-----------	--------------

2011	1,045	81	531	36
2012	1,169	98	539	52
2013	1,321	115	555	48
2014	1,224	89	577	42
2015	1,301	81	589	34
2016	1,371	91	619	40
2017	1,544	64	641	25
2018	1,600	100	633	38
2019	1,623	37	688	17

Table 4.20: The numerical values of bonds, underwritten bonds, EUR bonds, and EUR underwritten bonds respectively by year. The Panel A provides the number of bonds issued, with Panel B issuing the cumulative value of bonds, and Panel C provides the number of firms issuing the bonds. The cumulative values are provided in billions of euro

4.8.2 Additional Graphs

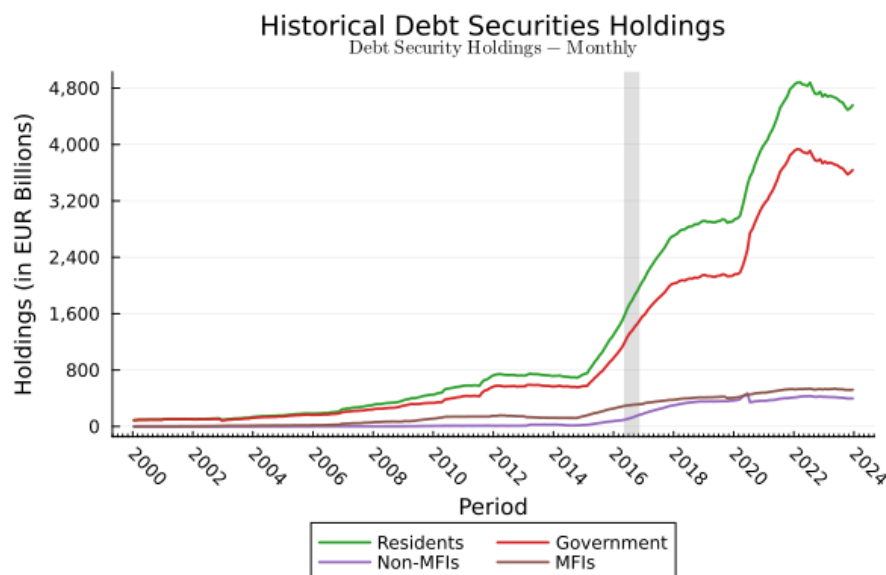


Figure 4.7: Debt security holdings held by the ECB on the balance sheet from January 1, 2000 to December 31, 2023. The data was collected from ECB's Data portal, specifically *BSLM.U2.N.C.A30.A.1.U2.0000.Z01.E* for residents, *BSLM.U2.N.C.A30.A.1.U2.1000.Z01.E* for MFIs, *BSLM.U2.N.C.A30.A.1.U2.2100.Z01.E* for government and *BSLM.U2.N.C.A30.A.1.U2.2200.Z01.E* for non-MFIs. The shaded area in 2016 represents the interim period between the CSPP program announcement and the start of asset purchasing under CSPP Source: ECB Data Portal

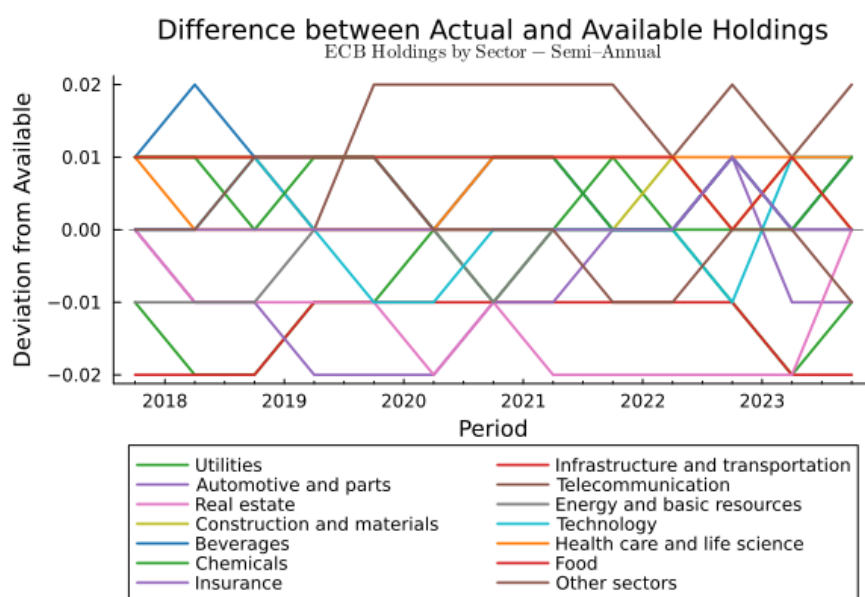


Figure 4.8: Difference between share of all holdings held by ECB under CSPP by sector and the share of the sector in the total market pool. The graph represents how closely the ECB mimicked its holdings in comparison to available pool. The optimal difference would be 0%, implying the holdings held by ECB perfectly match the shares of the available pool - e.g. eligible bonds within “Insurance” sector represent 6% of total available eligible bonds in the market, assuming 7% of all ECB holdings under CSPP are from “Insurance” sector, the difference would correspond to 0.01 on the y-axis. Source: ECB Data Portal

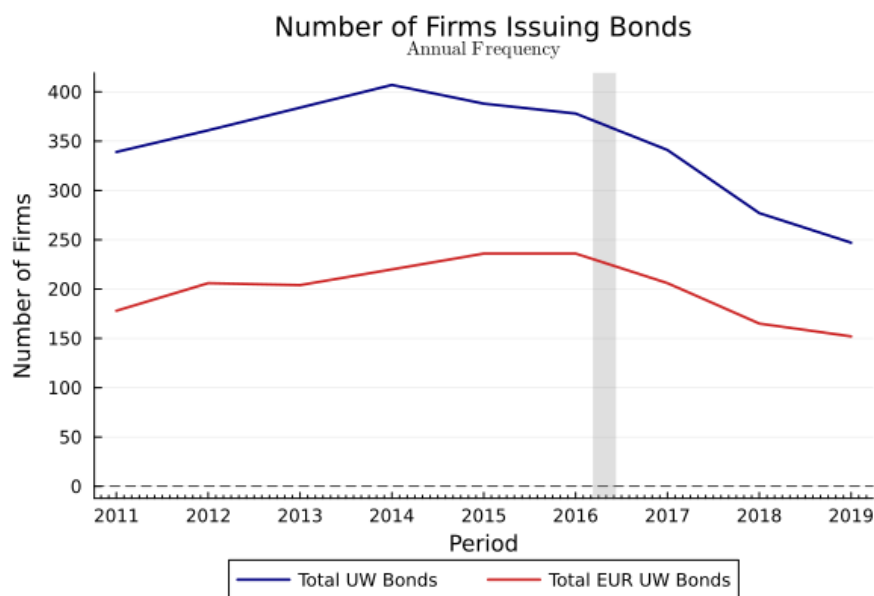


Figure 4.9: The number of firms issuing bonds by respective category. The “UW” is an abbreviation for “Underwritten” with the gray area representing the interim period between the CSPP program announcement and the start of purchases under CSPP. The data is presented on an annual basis for clarity and readability.

Chapter 5

Conclusions

This thesis set out to answer a central question of how do policy interventions across firms, banks, and capital markets shape real economic and financial outcomes, and why do similar policies succeed in some contexts but fail or backfire in others? The analysis demonstrated that policy effectiveness cannot be understood by examining the tool itself in isolation, but rather by examining how policies are designed, which incentives they create, and how heterogeneous agents respond. Across taxation, banking regulation, and monetary interventions, the thesis showed that policy design features, such as thresholds, eligibility rules, operational capacity requirements, and targeting, interact with differences in firm size, institutional capability, and market position to produce highly varied outcomes.

The unifying theme of the thesis is clear, policy tools do not generate outcomes, policy design interacting with incentives and heterogeneity does. When design aligns incentives with objectives and accounts for institutional capacity, policies can promote growth, stability, and access to finance. When design is rigid or insensitive to heterogeneity, policies can create distortions, exacerbate risk, or entrench inequality. By examining policy interventions at the micro level using firm-level data to analyze tax reform, meso level utilizing bank-level data for macroprudential stability analysis, and macro level through capital markets and unconventional monetary policy analysis, the thesis offers a multi-layered perspective on financial policy effectiveness. This system-wide approach reveals that interventions at one layer often spill over into others, and that understanding the financial system requires analyzing how design, incentives, and heterogeneity operate across institutional boundaries. In doing so, the thesis provides a coherent framework for evaluating and improving policy across the entire financial architecture.

5.1 Main Findings

Across the three empirical chapters, the thesis reveals a consistent message that policy outcomes are driven not by the policy tool itself, but by how it is designed and how heterogeneous agents respond

to the incentives it creates. At the micro level, the taxation regime demonstrates that even policies intended to support firm growth can unintentionally suppress expansion when thresholds or eligibility rules distort incentives. At the meso level, banking responses to emergency conditions show that stability is shaped less by formal regulation and more by operational capacity on the passive risk-taking, highlighting that resilience depends on a bank's ability to function and associated mitigation investments, such as digital transformation, not just on its capital ratios. At the macro level, the targeted asset purchase program reveals that unconventional monetary policy can lower financing costs for some firms while raising them for others, thereby reshaping market pricing and access to finance far beyond its intended scope.

Despite operating in different domains, each chapter uncovers the same underlying mechanisms, (1) policy design features such as thresholds, targeting, and operational requirements, (2) incentive effects that shape behavior in often predictable ways, (3) heterogeneity in capacity, size, or eligibility leading to divergent outcomes, and (4) unintended consequences and spillovers that extend beyond the direct targets of policy. Together, the chapters collectively answer the overarching puzzle that the policy effectiveness is not binary, success depends on aligning design with incentives and accounting for heterogeneity. The micro, meso, and macro analyses are not isolated case studies but complementary perspectives on the same phenomenon, demonstrating that the logic of policy design, incentives, and behavioral mechanisms applies across the entire financial system.

The three chapters also reveal that policies at different institutional levels are not independent, but deeply interconnected in a system of feedback loops. At the micro level, tax policies that discourage firm growth can weaken entrepreneurship and reduce the demand for productive credit, ultimately shaping the composition of bank loan portfolios. At the meso level, bank stability and operational capacity determine whether firms can access credit during stress, which affects their investment and financing strategies and, in turn, influences market conditions. At the macro level, central bank interventions reshape market pricing, underwriting incentives, and the allocation of liquidity, which feeds back into both firm-level financing choices and banks' portfolio decisions.

These cross-level dynamics demonstrate that financial policy must be understood as a system, not a set of isolated instruments. A well-designed tax regime can be undermined by banking instability, resilient banks can be undermined by distorted capital markets, and monetary policy can have unintended consequences if it ignores firm and bank incentives. The thesis therefore shows that analyzing policy in silos is insufficient, policies interact across institutional layers, and system-level resilience requires coordination and coherence across fiscal, banking, and monetary tools.

Beyond empirical findings, the thesis advances several novel conceptual contributions that reshape

theoretical debates in corporate finance, banking, and macroeconomic policy. The thesis reframes policy evaluation by emphasizing that design features, such as thresholds, targeting and operational rules, determine how agents respond. This shifts attention from “what policy tool is used” to “how it is designed and implemented.” Policies generate incentives, but agents differ in size, capacity, or eligibility. The interaction between design and heterogeneity explains why average effects mislead and why distributional outcomes matter. This provides a unifying behavioral lens across all policy domains.

The thesis introduces a distinction in banking theory. While existing literature focuses on deliberate, strategic or active risk-taking, this thesis shows that public crises generate passive risk that can be mitigated with proactive measures that can be motivated by the policy-makers. This mechanism has been largely overlooked but is crucial for understanding stability and designing macroprudential tools. The thesis elevates digital capacity from an internal efficiency choice to an essential component of financial stability. It argues that operational resilience and technological readiness are regulatory variables and should be explicitly incorporated into macroprudential frameworks.

The thesis demonstrates that unconventional monetary policy does not simply lower yields in aggregate, it redistributes advantages, alters competition, and reshapes pricing mechanisms. It introduces a structural perspective on monetary policy as a force that changes incentives and market power, not only interest rates. Through these contributions, the thesis acts as a conceptual bridge across corporate finance, banking, and monetary economics. It shows that the same theoretical principles, policy design, incentives, heterogeneity, capacity, and spillovers, apply across institutional layers. In doing so, it offers a unified framework for understanding financial policy effectiveness and lays the groundwork for a more integrated theory of financial systems.

5.2 Thesis Implications

The findings of this thesis carry important lessons for policymakers across taxation, banking regulation, and monetary policy. Collectively, they reveal that policy tools are only as effective as their design, and that aligning incentives with policy objectives is essential for long-term growth, stability, and fairness. Policy design must carefully balance simplification and growth incentives. While simplified or preferential regimes reduce compliance costs and promote formalization, rigid eligibility thresholds can discourage firms from expanding, trapping them at suboptimal size. To avoid these unintended constraints, tax policy should be dynamic and scalable, allowing firms to transition smoothly as they grow rather than facing discrete penalties. Well-designed tax regimes should support productivity and scaling, not just survival.

Regulation must recognize that risk arises not only from deliberate choices but also from operational failures. The thesis demonstrates that passive risk-taking, driven by disruptions, capacity constraints, or inability to monitor portfolios, is a major source of instability during crises. This implies that operational and digital capacity is a core determinant of financial stability, not a secondary efficiency concern. Digital transformation should therefore be treated as a macroprudential instrument, promoted and supported by regulators in the same way as capital buffers or liquidity requirements.

Unconventional monetary policies such as the CSPP do more than lower yields, they restructure market pricing and competitive dynamics. The thesis shows that targeted interventions can reduce financing costs for eligible firms while raising costs for ineligible firms, creating distributional consequences and potential inequalities in access to capital. Central banks must monitor spillovers and long-term dependencies, ensuring that repeated interventions do not entrench market power or erode market discipline. Monetary policy should support market functioning without inadvertently distorting it.

Most importantly, the thesis demonstrates that policy interventions at different levels of the financial system interact and spill over across institutions. Tax policy shapes firms, which influence banks, which feed into markets. Stability at one level can be undermined by distortions at another. Therefore, policy must be flexible, incentive-compatible, and coordinated across institutional layers. A system-level approach, rather than siloed interventions, is essential for achieving growth, fairness, and resilience in modern financial systems.

In addition to its theoretical and policy insights, this thesis makes important methodological contributions to empirical research in finance and public economics. First, it demonstrates the power of causal inference in underexplored real-world policy settings. Across all three chapters, the thesis uses exogenous variation from natural experiments, a tax regime introduction, a pandemic shock, and a central bank intervention, to identify causal effects rather than relying on correlations. Techniques such as difference-in-differences, fuzzy regression discontinuity, propensity score matching, and inverse Mills ratio correction are employed to isolate mechanisms, address selection bias, and examine heterogeneous responses.

Second, the thesis goes beyond average treatment effects to uncover behavioral mechanisms and distributional consequences. Rather than asking “did the policy work?”, it asks how, why, and for whom. This focus on heterogeneous treatment effects and mechanism-based inference moves empirical analysis closer to theory and policy relevance. Third, the thesis uses rich micro-level data across multiple layers of the financial system, firm-level administrative data, bank-level supervisory data, and transaction-level bond issuance data. This multi-level data architecture allows for precise measurement of behavior and risk and enables comparisons across institutional contexts. It is rare for a single thesis

to incorporate micro data from firms, banks, and capital markets, and this breadth enhances both internal and external validity.

Finally, the thesis develops a replicable policy evaluation framework that can be applied across different domains, such as policy design, incentives, heterogeneity, mechanisms, unintended consequences and system-level effects. This framework moves policy evaluation beyond tool-specific analysis toward a generalizable, mechanism-based approach. It provides a blueprint for future research on how policies shape behavior in complex financial systems and sets a methodological standard for integrating micro-level evidence with macro-level implications.

5.3 Thesis Limitations

While this thesis provides new insights into the role of policy design, incentives, and heterogeneity across firms, banks, and capital markets, it is important to acknowledge several limitations. These limitations do not undermine the validity of the results, rather, they clarify the scope of inference and highlight avenues for further research.

Context specificity Each empirical chapter focuses on a specific institutional setting, a simplified tax regime in Slovenia, the COVID-19 emergency in Hubei banks, and the ECB's Corporate Sector Purchase Program in European bond markets. These contexts are not arbitrary, they are analytically rich and represent globally relevant policy instruments. However, institutional environments differ across countries, and policy design features may vary. The findings should therefore be interpreted in light of these contexts. Nonetheless, the mechanisms identified, threshold distortions, passive risk-taking, digital capacity, spillovers, and incentive misalignment, are general and apply to similar policies in other settings. The strength of the thesis lies not in context-specific conclusions, but in mechanism-based generalization.

Data limitations The thesis relies on high-quality micro-level data, yet some constraints are inevitable. In certain cases, sample sizes are limited - e.g., bond issuance data for [Chapter 4](#), or data impose indirect measures - e.g., digital capacity proxies, risk metrics. While robust econometric techniques and multiple validation exercises are used to mitigate these limitations, more granular or broader datasets could deepen future analysis. Importantly, the thesis prioritizes causal identification and internal validity, even when this restricts sample scope. This trade-off favors credibility over breadth.

Conceptual boundaries The thesis does not attempt to construct a full general equilibrium model of the financial system or simulate policy interactions in a structural framework. Nor does it develop formal theoretical models in each chapter. Instead, it adopts a cumulative empirical approach, using quasi-experimental variation to uncover mechanisms. Given the journal-article format of each chapter,

there is some necessary repetition and limited space for full theoretical integration within each paper. However, the introduction and conclusion provide the conceptual synthesis, linking empirical insights across levels. This structure reflects a deliberate choice to prioritize causal evidence and mechanism identification over abstract generality.

Scope of inference The thesis does not claim that there is a single optimal policy design, nor that its findings can be mechanically applied in all contexts. Rather, it identifies principles such flexibility, incentive alignment, attention to heterogeneity, and institutional capacity, that improve policy effectiveness. These principles hold across domains, even if the precise design details must adapt to local conditions.

In acknowledging these limitations, the thesis demonstrates intellectual transparency and rigor. At the same time, by revealing the mechanisms through which policy design shapes behavior, it provides a foundation for generalizable insights that extend beyond the specific cases studied. Indeed, several of these limitations, context specificity, data constraints, and conceptual boundaries, create opportunities for future research to build on and extend the thesis's contributions.

5.4 Future Research

The thesis offers new insights into how policy design, incentives, and heterogeneity shape outcomes across firms, banks, and financial markets. At the same time, it opens several promising avenues for future research that can deepen, extend, and generalize these findings.

Dynamic tax design and firm growth transitions Future research could explore how tax systems can be redesigned to support firm growth across stages rather than create binary eligibility cutoffs. This includes developing graduated or continuous tax regimes, studying transition mechanisms, and analyzing how firms adjust behavior when incentives evolve over time. Structural and dynamic models of firm growth could integrate tax policy design directly into scaling decisions, moving the literature beyond static treatment effects.

Digital transformation and supervision in global banking systems While this thesis shows that digital capacity enhances resilience, more research is needed to determine how regulators can measure, incentivize, and integrate digital readiness into macroprudential frameworks. Comparative studies across countries, banking models, and regulatory regimes could clarify the institutional and technological prerequisites for effective digital supervision. There is also scope for analyzing cost-benefit trade-offs and questions of access and inequality in digital transformation.

Long-term effects of unconventional monetary policy on market structure and competition Most studies examine short-term yield effects of QE and CSPP, but little is known about long-term

consequences. Future work could analyze whether prolonged asset purchases alter underwriting competition, concentration, or market power, and whether they create path dependencies or moral hazard in capital markets. Understanding when interventions support market functioning and when they create dependency is essential for designing exit strategies and ensuring long-term market health.

System-level modeling of coordinated policy across fiscal, banking, and monetary layers

The thesis reveals interdependence across policy domains, but formal models rarely incorporate simultaneous policy interactions. Future research could build general equilibrium or network-based models that simulate how taxation, macroprudential policy, and monetary interventions interact. Such models could identify complementarities, conflicts, and optimal sequencing of policies across institutional layers, advancing both theory and policymaking.

Behavioral responses to policy in different institutional environments Future research could examine how institutional context, legal systems, enforcement quality, financial development, digital infrastructure, shapes the behavioral response to identical policy tools. This would help determine when and where certain policy designs are more or less effective, contributing to a more context-aware and adaptive approach to policy formulation.

Collectively, these research directions extend the thesis's central contribution by moving beyond average effects and isolated tools toward a deeper understanding of how policy design interacts with incentives, heterogeneity, institutional capacity, and system dynamics. By pursuing these questions, future work can build a richer, more integrated theory of financial policy, one that supports growth, stability, and resilience across the entire financial system.

5.5 Closing Statement

This thesis demonstrates that policy tools themselves are not inherently effective or ineffective, what ultimately determines outcomes is how they are designed, how they shape incentives, and how heterogeneous agents respond. Across firms, banks, and capital markets, the evidence shows that policies succeed when they align incentives with policy goals and fail when they impose rigid rules that ignore institutional capacity or behavioral realities. Yet the findings also make clear that this is not a limitation of policy. it is an opportunity. By recognizing the central role of design, incentives, and heterogeneity, policymakers can move beyond one-size-fits-all approaches and toward flexible, scalable, and adaptive policies that evolve with the economy.

Crucially, the thesis shows that micro-level incentives and macro-level objectives do not have to conflict. When policy design respects how firms grow, how banks manage risk, and how markets price capital, it is possible to create interventions that support both economic growth and financial stability.

The key is to view the financial system as an interconnected structure in which fiscal, macroprudential, and monetary policies must be coordinated rather than isolated. Policies that are incentive-compatible, operationally feasible, and sensitive to heterogeneity can foster resilience without suppressing innovation or competition.

Ultimately, this thesis offers both empirical evidence and conceptual principles for designing better financial policies in an increasingly complex world. It provides a unified framework for understanding how policy operates across institutional layers and how mechanisms such as passive risk-taking, digital capacity, spillovers, and unintended constraints shape outcomes. By advancing this multi-level, mechanism-based perspective, the thesis contributes not only to academic debates but also to the development of smarter, more coherent, and more resilient financial policy frameworks. In an era where policy interventions are more frequent, more targeted, and more interconnected than ever, the core message is optimistic, with thoughtful design and coordination, financial policy can promote growth, stability, and fairness, not by chance, but by construction.

Acknowledgements

The Impact of Gross Receipt Taxation on Micro-Sized Firms

I would thank Nello Esposito, Tommaso Oliviero, Andrew Ellul, Marco Pagano, Marco Pagnozzi, Lorenzo Pandolfi, Ettore Panetti, Sebastian Doerr, Florian Hoffmann and the participants of internal seminar on the University of Naples Federico II for valuable comments, suggestions and critiques. I would further extend my gratitude to the Financial Administration of the Republic of Slovenia for granting me the access to the data.

Digital Transformation and Bank Risk in the Public Emergency

We would thank Prof. Tommaso Oliviero (University of Naples Federico II, Italy) and Prof. Haifeng Gu (Donghua University, China) for their support and valuable comments. We would further thank Prof. Xuanli Xie (Peking University, China) for providing the data.

Unconventional Monetary Policy and Corporate Bond Underwriting - Evidence from ECB's Asset Purchase Program

I would thank Nello Esposito, Tommaso Oliviero, Marco Pagano, Marco Pagnozzi, Lorenzo Pandolfi, Ettore Panetti, Hans Degryse, Tong Zhao, Aleksy Leeuwenkamp, Sebastian Doerr and the participants of internal seminar on the University of Naples Federico II for valuable comments, suggestions and support.

Bibliography

- Abidi, N., & Miquel-Flores, I. (2018). Who benefits from the corporate qe? a regression discontinuity design approach. *International Corporate Finance eJournal*.
- Act amending the personal income tax act*. (2012). Official Gazette of the Republic of Slovenia (No. 94/12). (page 9576)
- Act amending the personal income tax act*. (2014). Official Gazette of the Republic of Slovenia (No. 50/14). (page 5555)
- Addoum, J. M., Ng, D. T., & Ortiz-Bobea, A. (2020). Temperature shocks and establishment sales. *The Review of Financial Studies*, 33(3), 1331–1366.
- Aevoae, G. M., Andrieş, A. M., Ongena, S., & Sprincean, N. (2023). Esg and systemic risk. *Applied Economics*, 55(27), 3085–3109.
- Aghion, P., Akcigit, U., Cagé, J., & Kerr, W. R. (2016). Taxation, corruption, and growth. *European Economic Review*, 86, 24–51.
- Ahmad, M., Audi, M., & Ahmad, K. (2025). Tax burden, incentives, and informality: Determinants of sme growth and formalisation in emerging markets. *Contemporary Journal of Social Science Review*, 3(1), 1299–1308.
- Alonge, E. O., Eyo-Udo, N. L., CHIBUNNA, B., UBANADU, A. I. D., BALOGUN, E. D., & OGUN-SOLA, K. O. (2021). Digital transformation in retail banking to enhance customer experience and profitability. *Iconic Research and Engineering Journals*, 4(9).
- Anadu, K., Kruttli, M., McCabe, P., & Osambela, E. (2020). The shift from active to passive investing: Risks to financial stability? *Financial Analysts Journal*, 76(4), 23–39.
- Audretsch, D. B., Belitski, M., Chowdhury, F., & Desai, S. (2021). Necessity or opportunity? government size, tax policy, corruption, and implications for entrepreneurship. *Small Business Economics*, 58, 2025 - 2042.
- Auerbach, A. J., Aaron, H. J., & Hall, R. E. (1983). Corporate taxation in the united states. *Brookings Papers on Economic Activity*, 1983(2), 451–513.
- Ayadi, R., Chiaramonte, L., Cucinelli, D., & Migliavacca, M. (2025). Digitalization and banks'

- efficiency: Evidence from a european analysis. *International Review of Financial Analysis*, 97, 103837.
- Bacchetta, P., Cordonier, R., & Merrouche, O. (2023). The rise in foreign currency bonds: The role of us monetary policy and capital controls. *Journal of International Economics*, 140, 103709.
- Bank, E. C. (n.d.). *How does the ecb's asset purchase programme work?* ECB. Retrieved from <https://www.eurosystem.org/explainers/asset-purchase-programme.html>
- Banna, H., Hassan, M. K., & Rashid, M. (2021). Fintech-based financial inclusion and bank risk-taking: Evidence from oic countries. *Journal of International Financial Markets, Institutions and Money*, 75, 101447.
- Barro, R. J. (1990). Government spending in a simple model of endogenous growth. *Journal of political economy*, 98(5, Part 2), S103–S125.
- Bartik, A. W., Bertrand, M., Cullen, Z. B., Glaeser, E. L., Luca, M., & Stanton, C. T. (2020). *How are small businesses adjusting to covid-19? early evidence from a survey* (Tech. Rep.). National Bureau of Economic Research.
- Bats, J. (2020). Corporates' dependence on banks: The impact of ecb corporate sector purchases.
- Bekaert, G., & De Santis, R. A. (2021). Risk and return in international corporate bond markets. *Journal of International Financial Markets, Institutions and Money*, 72, 101338.
- Best, M., Brockmeyer, A., Kleven, H. J., Spinnewijn, J., & Waseem, M. (2013). Production versus revenue efficiency with limited tax capacity: Theory and evidence from pakistan. *Journal of Political Economy*, 123, 1311 - 1355.
- Betz, F., & De Santis, R. A. (2019). Ecb corporate qe and the loan supply to bank-dependent firms. *Available at SSRN 3453159*.
- Bhattarai, S., Chatterjee, A., & Park, W. Y. (2021). Effects of us quantitative easing on emerging market economies. *Journal of Economic Dynamics and Control*, 122, 104031.
- Bhattarai, S., & Neely, C. J. (2022). An analysis of the literature on international unconventional monetary policy. *Journal of Economic Literature*, 60(2), 527–597.
- Bian, Y., & Gu, H. (2024). Can setting up inside debt accelerate the bank's digital transformation? evidence from china. *Baltic Journal of Management*, 19(2), 270–287.

- Bilinski, A., & Hatfield, L. A. (2018). Seeking evidence of absence: Reconsidering tests of model assumptions. *arXiv preprint arXiv:1805.03273*, 8.
- Block, J. (2021). Corporate income taxes and entrepreneurship. *IZA World of Labor*.
- Bonfim, D., & Capela, A. (2020). The effect of corporate bond purchases by the ecb on firms' borrowing costs. *Banco de Portugal Economic Studies*, 2.
- Borri, N., & Di Giorgio, G. (2022). Systemic risk and the covid challenge in the european banking sector. *Journal of Banking & Finance*, 140, 106073.
- Borusyak, K., Jaravel, X., & Spiess, J. (2024). Revisiting event-study designs: robust and efficient estimation. *Review of Economic Studies*, 91(6), 3253–3285.
- Bouvatier, V., Lepetit, L., Rehault, P.-N., & Strobel, F. (2018). Bank insolvency risk and z-score measures: caveats and best practice.
- Boyd, J. H., & De Nicolo, G. (2005). The theory of bank risk taking and competition revisited. *The Journal of finance*, 60(3), 1329–1343.
- Braunerhjelm, P., Eklund, J., & Thulin, P. (2019). Taxes, the tax administrative burden and the entrepreneurial life cycle. *Small Business Economics*, 56, 681–694.
- Brown, D. P., & Wu, Y. (2016). Mutual fund flows and cross-fund learning within families. *The Journal of Finance*, 71(1), 383–424.
- Brueggemann, B. (2017). Higher taxes at the top: The role of entrepreneurs. *Political Economy - Development: Fiscal & Monetary Policy eJournal*.
- Brzoza-Brzezina, M., Kolasa, M., & Makarski, K. (2021). *Monetary policy and covid-19*. International Monetary Fund.
- Buston, C. S. (2016). Active risk management and banking stability. *Journal of Banking & Finance*, 72, S203–S215.
- Callaway, B., & Sant'Anna, P. H. (2021). Difference-in-differences with multiple time periods. *Journal of econometrics*, 225(2), 200–230.
- Calonico, S., Cattaneo, M. D., Farrell, M. H., Palomba, F., & Titiunik, R. (2025). Treatment effect heterogeneity in regression discontinuity designs. *arXiv preprint arXiv:2503.13696*.
- Calonico, S., Cattaneo, M. D., Farrell, M. H., & Titiunik, R. (2019). Regression discontinuity designs using covariates. *Review of Economics and Statistics*, 101(3), 442–451.
- Calonico, S., Cattaneo, M. D., & Titiunik, R. (2014). Robust nonparametric confidence intervals for regression-discontinuity designs. *Econometrica*, 82(6), 2295–2326.

- Cappa, F., Oriani, R., Peruffo, E., & McCarthy, I. (2021). Big data for creating and capturing value in the digitalized environment: unpacking the effects of volume, variety, and veracity on firm performance. *Journal of Product Innovation Management*, 38(1), 49–67.
- Cappiello, L., Holm-Hadulla, F., Maddaloni, A., Arts, L., Meme, N., Migiakis, P., ... others (2021). Non-bank financial intermediation in the euro area: implications for monetary policy transmission and key vulnerabilities.
- Carbó-Valverde, S., Cuadros-Solas, P. J., & Rodríguez-Fernández, F. (2017). Do banks and industrial companies have equal access to reputable underwriters in debt markets? *Journal of Corporate Finance*, 45, 176–202.
- Carbó-Valverde, S., Cuadros-Solas, P. J., Rodríguez-Fernández, F., & Ey. (2020). The effect of banks' it investments on the digitalization of their customers. *Global Policy*, 11, 9–17.
- Carroll, R., Holtz-Eakin, D., Rider, M., & Rosen, H. S. (2001). Personal income taxes and the growth of small firms. *Tax policy and the economy*, 15, 121–147.
- Chamberlain, A., & Fleenor, P. (2007). Tax pyramiding: The economic consequences of gross receipts taxes..
- Chen, B., Zhang, C., & Saydaliev, H. B. (2022). Does bank complexity during the covid-19 crisis alter the financing mechanism for small and medium-sized enterprises? *Economic Analysis and Policy*, 75, 705–715.
- Chen, Y., & Ma, Y. (2021). Does green investment improve energy firm performance? *Energy Policy*, 153, 112252.
- Chen, Z., Li, H., Wang, T., & Wu, J. (2023). How digital transformation affects bank risk: Evidence from listed chinese banks. *Finance Research Letters*, 58, 104319.
- Chen, Z., Li, K., & He, L.-Y. (2020). Has internet finance decreased the profitability of commercial banks?: evidence from china. *Emerging Markets Finance and Trade*, 56(13), 3015–3032.
- Christensen, J. H., & Gillan, J. M. (2022). Does quantitative easing affect market liquidity? *Journal of Banking & Finance*, 134, 106349.
- Clampit, J. A., Lorenz, M. P., Gamble, J. E., & Lee, J. (2022). Performance stability among small and medium-sized enterprises during covid-19: A test of the efficacy of dynamic capabilities. *International Small Business Journal*, 40(3), 403–419.
- Coppier, R., Michetti, E., & Scaccia, L. (2023). Size-dependent enforcement, tax evasion and dimensional trap. *Computational Economics*.

- Cuesta, C., Ruesta, M., Tuesta, D., Urbiola, P., et al. (2015). The digital transformation of the banking industry. *BBVA research*, 1(4), 1–10.
- Cummins, J. G., Hassett, K. A., & Hubbard, R. G. (1996). Tax reforms and investment: A cross-country comparison. *Journal of public Economics*, 62(1-2), 237–273.
- Dabla-Norris, M. E., Misch, F., Cleary, M. D., & Khwaja, M. (2017). *Tax administration and firm performance: New data and evidence for emerging market and developing economies*. International Monetary Fund.
- Dai, R., Feng, H., Hu, J., Jin, Q., Li, H., Wang, R., ... Zhang, X. (2021). The impact of covid-19 on small and medium-sized enterprises (smes): Evidence from two-wave phone surveys in china. *China Economic Review*, 67, 101607.
- D’Amico, S., & Kaminska, I. (2019). Credit easing versus quantitative easing: Evidence from corporate and government bond purchase programs.
- Danielsson, J., Macrae, R., & Uthemann, A. (2022). Artificial intelligence and systemic risk. *Journal of Banking & Finance*, 140, 106290.
- Davydov, D., Vähämaa, S., & Yasar, S. (2021). Bank liquidity creation and systemic risk. *Journal of Banking & Finance*, 123, 106031.
- de Chaisemartin, C., & D’Haultfoeuille, X. (2018). Two-way fixed effects estimators with heterogeneous treatment effects. *ERN: Estimation (Topic)*.
- De Chaisemartin, C., & d’Haultfoeuille, X. (2020). Two-way fixed effects estimators with heterogeneous treatment effects. *American economic review*, 110(9), 2964–2996.
- De Chaisemartin, C., & D’haultfoeuille, X. (2023). Two-way fixed effects and differences-in-differences estimators with several treatments. *Journal of Econometrics*, 236(2), 105480.
- Dedola, L., Georgiadis, G., Gräb, J., & Mehl, A. (2021). Does a big bazooka matter? quantitative easing policies and exchange rates. *Journal of Monetary Economics*, 117, 489–506.
- Delis, M. D., & Kouretas, G. P. (2011). Interest rates and bank risk-taking. *Journal of Banking & Finance*, 35(4), 840–855.
- Dell’Ariccia, G., Laeven, L., & Marquez, R. (2014). Real interest rates, leverage, and bank risk-taking. *Journal of Economic Theory*, 149, 65–99.
- Deng, L., Lv, Y., Liu, Y., & Zhao, Y. (2021). Impact of fintech on bank risk-taking: Evidence from china. *Risks*, 9(5), 99.
- De Santis, R. A., & Zaghini, A. (2021). Unconventional monetary policy and corporate bond issuance. *European Economic Review*, 135, 103727.

- Dessaint, O., & Matray, A. (2017). Do managers overreact to salient risks? evidence from hurricane strikes. *Journal of Financial Economics*, 126(1), 97–121.
- De Vito, A., & Gómez, J.-P. (2020). Estimating the covid-19 cash crunch: Global evidence and policy. *Journal of Accounting and Public policy*, 39(2), 106741.
- Dick-Nielsen, J., Nielsen, M. S., & von Rüden, S. L. (2021). The value of bond underwriter relationships. *Journal of Corporate Finance*, 68, 101930.
- Do, T. D., Pham, H. A. T., Thalassinou, E. I., & Le, H. A. (2022). The impact of digital transformation on performance: Evidence from vietnamese commercial banks. *Journal of risk and financial management*, 15(1), 21.
- Doran, N. M., Bădîrcea, R. M., & Manta, A. G. (2022). Digitization and financial performance of banking sectors facing covid-19 challenges in central and eastern european countries. *Electronics*, 11(21), 3483.
- Drasch, B. J., Schweizer, A., & Urbach, N. (2018). Integrating the ‘troublemakers’: A taxonomy for cooperation between banks and fintechs. *Journal of economics and business*, 100, 26–42.
- Duan, Y., El Ghoul, S., Guedhami, O., Li, H., & Li, X. (2021). Bank systemic risk around covid-19: A cross-country analysis. *Journal of Banking & Finance*, 133, 106299.
- Duarte, F., & Eisenbach, T. M. (2021). Fire-sale spillovers and systemic risk. *The Journal of Finance*, 76(3), 1251–1294.
- Duca, M. L., Nicoletti, G., & Martinez, A. V. (2016). Global corporate bond issuance: what role for us quantitative easing? *Journal of international money and finance*, 60, 114–150.
- Elia, J., Toros, E., Sawaya, C., & Balouza, M. (2021). Using altman z”-score to predict financial distress: Evidence from lebanese alpha banks. *Management Studies and Economic Systems*, 6(1/2), 47–57.
- Fang, L. H. (2005). Investment bank reputation and the price and quality of underwriting services. *The Journal of Finance*, 60(6), 2729–2761.
- Fasth, J., Åkerman, N., Elliot, V., & Hilmersson, M. (2023). Small and medium-sized enterprises response to covid-19: The effect of external openness and procedural management. *International Small Business Journal*, 41(5), 465–487.
- Feldstein, M. (2008). Effects of taxes on economic behavior. *National tax journal*, 61(1), 131–139.
- Fernández-Bastidas, R. (2023). Entrepreneurship and tax evasion. *Economic Modelling*.

- Fernando, C. S., Gatchev, V. A., May, A. D., & Megginson, W. L. (2015). Prestige without purpose? reputation, differentiation, and pricing in us equity underwriting. *Journal of Corporate Finance*, 32, 41–63.
- Fox, W. F., & Luna, L. (2002). State corporate tax revenue trends: Causes and possible solutions. *National Tax Journal*, 55(3), 491–508.
- Freixas, X., Parigi, B. M., & Rochet, J.-C. (2000). Systemic risk, interbank relations, and liquidity provision by the central bank. *Journal of money, credit and banking*, 611–638.
- Freyaldenhoven, S., Hansen, C., & Shapiro, J. M. (2019). Pre-event trends in the panel event-study design. *American Economic Review*, 109(9), 3307–3338.
- Funke, M., et al. (2001). Determining the taxation and investment impacts of estonia's 2000 income tax reform.
- Funke, M., & Strulik, H. (2003). Taxation, growth and welfare: Dynamic effects of estonia's 2000 income tax act.
- Fuster, A., Plosser, M., Schnabl, P., & Vickery, J. (2019). The role of technology in mortgage lending. *The review of financial studies*, 32(5), 1854–1899.
- Galema, R., Lugo, S., et al. (2017). When central banks buy corporate bonds:: Target selection and impact of the european corporate sector purchase program. *USE Discussion paper series*, 17(16), 1–33.
- Galletta, S., & Mazzù, S. (2023). Esg controversies and bank risk taking. *Business Strategy and the Environment*, 32(1), 274–288.
- Gao, H., Li, J., & Wen, H. (2023). Bank funding costs during the covid-19 pandemic: Evidence from china. *Pacific-Basin Finance Journal*, 79, 102006.
- Gentry, W. M., & Hubbard, R. G. (2000). Tax policy and entrepreneurial entry. *American Economic Review*, 90(2), 283–287.
- Gertler, P., Higgins, S., Malmendier, U., & Ojeda, W. (2022). Why small firms fail to adopt profitable opportunities.
- Gnan, P. (2019). The privilege of eligibility: Announcement effects and spillovers of the ecb's corporate sector purchase program. *Available at SSRN 3519367*.
- Goodman-Bacon, A. (2021). Difference-in-differences with variation in treatment timing. *Journal of econometrics*, 225(2), 254–277.
- Gramlich, D., Walker, T., Zhao, Y., & Bitar, M. (2023). After the storm: Natural disasters and bank solvency. *International Journal of Central Banking*, 19(2), 199–249.

- Gross receipts.* (n.d.). <https://www.irs.gov/charities-non-profits/gross-receipts-defined>. (Accessed: 2021-09-30)
- Hall, R. E., & Jorgenson, D. W. (1967). Tax policy and investment behavior. *The American economic review*, 57(3), 391–414.
- Hansen, B. C., Miller, K. S., & Weber, C. E. (2021). Vertical integration and production inefficiency in the presence of a gross receipts tax. *SSRN Electronic Journal*.
- Harberger, A. C. (1962). The incidence of the corporation income tax. *Journal of Political economy*, 70(3), 215–240.
- Hayashi, F. (1982). Tobin's marginal q and average q: A neoclassical interpretation. *Econometrica: Journal of the Econometric Society*, 213–224.
- Heckman, J. J. (1978). Dummy endogenous variables in a simultaneous equation system. *Econometrica*, 46(4), 931–959.
- Heckman, J. J. (1979). Sample selection bias as a specification error. *Econometrica*, 47(1), 153–161.
- Hull, P. (2018). Estimating treatment effects in mover designs. *arXiv preprint arXiv:1804.06721*.
- Iannotta, G., & Navone, M. (2008). Which factors affect bond underwriting fees? the role of banking relationships. *European Financial Management*, 14(5), 944–961.
- Imai, K., & Kim, I. S. (2020). On the use of two-way fixed effects regression models for causal inference with panel data. *Political Analysis*, 29, 405 - 415.
- Jackson, M. O., & Pernoud, A. (2021). Systemic risk in financial networks: A survey. *Annual Review of Economics*, 13(1), 171–202.
- Jagtiani, J., & Lemieux, C. (2019). The roles of alternative data and machine learning in fintech lending: Evidence from the lendingclub consumer platform. *Financial Management*, 48(4), 1009–1029.
- Jiménez, G., Lopez, J. A., & Saurina, J. (2013). How does competition affect bank risk-taking? *Journal of Financial stability*, 9(2), 185–195.
- Kahn-Lang, A., & Lang, K. (2020). The promise and pitfalls of differences-in-differences: Reflections on 16 and pregnant and other applications. *Journal of Business & Economic Statistics*, 38(3), 613–620.
- Kandrac, J., & Schlusche, B. (2021). Quantitative easing and bank risk taking: evidence from lending. *Journal of Money, Credit and Banking*, 53(4), 635–676.

- Karadi, P., & Nakov, A. (2021). Effectiveness and addictiveness of quantitative easing. *Journal of Monetary Economics*, 117, 1096–1117.
- Kedward, K., Buller, A., & Ryan-Collins, J. (2021). Quantitative easing and nature loss: Exploring nature-related financial risks and impacts in the european central bank's corporate bond portfolio.
- Khaddafi, M., Heikal, M., Nandari, A., et al. (2017). Analysis z-score to predict bankruptcy in banks listed in indonesia stock exchange. *International Journal of Economics and Financial Issues*, 7(3), 326–330.
- Khattak, M. A., Ali, M., Azmi, W., & Rizvi, S. A. R. (2023). Digital transformation, diversification and stability: What do we know about banks? *Economic Analysis and Policy*, 78, 122–132.
- Kim, D., Palia, D., & Saunders, A. (2008). The impact of commercial banks on underwriting spreads: Evidence from three decades. *Journal of Financial and Quantitative Analysis*, 43(4), 975–1000.
- King, G., & Nielsen, R. A. (2019). Why propensity scores should not be used for matching. *Political Analysis*, 27, 435 - 454.
- Kollo, M. G. (2005). Underwriter competition and gross spreads in the eurobond market.
- Krishnamurthy, A., & Vissing-Jorgensen, A. (2011). *The effects of quantitative easing on interest rates: channels and implications for policy* (Tech. Rep.). National Bureau of Economic Research.
- Kubitza, C. (2023). Bonds at a premium: the impact of insurers on corporate bond issuers.
- Kwan, A., Lin, C., Pursiainen, V., & Tai, M. (2024). Stress testing banks' digital capabilities: Evidence from the covid-19 pandemic. *Journal of Financial and Quantitative Analysis*, 59(6), 2618–2646.
- Laeven, L., & Levine, R. (2009). Bank governance, regulation and risk taking. *Journal of financial economics*, 93(2), 259–275.
- Lang, C., Hu, Y., Corbet, S., & Hou, Y. G. (2024). Tail risk connectedness in g7 stock markets: Understanding the impact of covid-19 and related variants. *Journal of Behavioral and Experimental Finance*, 41, 100889.
- Lawson, R. A. (2018). The economics of gross receipts taxes: A case study of ohio. *Political Economy: Taxation*.

- Lepetit, L., & Strobel, F. (2013). Bank insolvency risk and time-varying z-score measures. *Journal of International Financial Markets, Institutions and Money*, 25, 73–87.
- Lepetit, L., & Strobel, F. (2015). Bank insolvency risk and z-score measures: A refinement. *Finance Research Letters*, 13, 214–224.
- Levy, S. (2022). Brand bank attachment to loyalty in digital banking services: mediated by psychological engagement with service platforms and moderated by platform types. *International Journal of Bank Marketing*, 40(4), 679–700.
- Li, X., Tripe, D. W., & Malone, C. B. (2017). Measuring bank risk: An exploration of z-score. *Available at SSRN 2823946*.
- Li, Y., Spigt, R., & Swinkels, L. (2017). The impact of fintech start-ups on incumbent retail banks' share prices. *Financial Innovation*, 3(1), 26.
- Liu, J., Huang, S., Fu, Q., Luo, Y., Qin, S., Cao, Y., ... Yang, S. (2023). The open banking era: An optimal model for the emergency fund. *Expert Syst. Appl.*, 244, 122915.
- Liu, Q., Shi, C., Tse, Y., & Zhang, L. (2023). The impact of public health emergencies on small and medium-sized enterprises: Evidence from china. *Global Finance Journal*, 58, 100892.
- Liu, X., Chau, K. Y., Liu, X., & Huang, F. (2023). The determinants of customer intentions to use fintech services in a commercial chinese bank. *Journal of Business-to-Business Marketing*, 30(3), 257–276.
- López-Penabad, M. C., Iglesias-Casal, A., & Neto, J. F. S. (2022). Effects of a negative interest rate policy in bank profitability and risk taking: Evidence from european banks. *Research in International Business and Finance*, 60, 101597.
- Mäkinen, T., Mercatanti, A., Silvestrini, A., & Li, F. (2020). Effects of eligibility for central bank purchases on corporate bond spreads. *Bank of Italy Temi di Discussione (Working Paper) No, 1300*.
- Manconi, A., Neretina, E., & Renneboog, L. (2018). Underwriter competition and bargaining power in the corporate bond market. *European Corporate Governance Institute (ECGI)-Finance Working Paper(548)*.
- Martinez-Miera, D., & Repullo, R. (2010). Does competition reduce the risk of bank failure? *The Review of Financial Studies*, 23(10), 3638–3664.
- Masso, J., Meriküll, J., & Vahter, P. (2011). Gross profit taxation versus distributed profit taxation and firm performance: effects of estonia's corporate income tax reform. *The*

- University of Tartu Faculty of Economics and Business Administration Working Paper*(81-2011).
- McCahery, J., & Schwienbacher, A. (2010). Bank reputation in the private debt market. *Journal of Corporate Finance*, 16(4), 498–515.
- McTier, B. C., Tse, Y., & Wald, J. K. (2013). Do stock markets catch the flu? *Journal of Financial and Quantitative Analysis*, 48(3), 979–1000.
- Mehmood, Z., Dutordoir, M., & De Cesari, A. (2018). The impact of underwriter reputation on bond fees and yields during the global financial crisis. *Available at SSRN 3270228*.
- Mishrif, A., & Khan, A. (2023). Causal analysis of company performance and technology mediation in small and medium enterprises during covid-19. *Journal of the Knowledge Economy*, 14(4), 4730–4753.
- Moudud-Ul-Huq, S. (2021). Does bank competition matter for performance and risk-taking? empirical evidence from brics countries. *International Journal of Emerging Markets*, 16(3), 409–447.
- Naimi-Sadigh, A., Asgari, T., & Rabiei, M. (2022). Digital transformation in the value chain disruption of banking services. *Journal of the Knowledge Economy*, 13(2), 1212–1242.
- Narayanan, R. P., Rangan, K. P., & Rangan, N. K. (2004). The role of syndicate structure in bank underwriting. *Journal of financial Economics*, 72(3), 555–580.
- Neely, C. J. (2015). Unconventional monetary policy had large international effects. *Journal of Banking & Finance*, 52, 101–111.
- Nguyen, Q. T. T., Ho, L. T. H., & Nguyen, D. T. (2023). Digitalization and bank profitability: evidence from an emerging country. *International Journal of Bank Marketing*, 41(7), 1847–1871.
- Nguyen-Thi-Huong, L., Nguyen-Viet, H., Nguyen-Phuong, A., & Van Nguyen, D. (2023). How does digital transformation impact bank performance? *Cogent Economics & Finance*, 11(1), 2217582.
- Nozawa, Y., & Qiu, Y. (2021). Corporate bond market reactions to quantitative easing during the covid-19 pandemic. *Journal of Banking & Finance*, 133, 106153.
- Pegoraro, S., & Montagna, M. (2021a). Demand-driven bond financing in the euro area. *Available at SSRN 3791730*.
- Pegoraro, S., & Montagna, M. (2021b). *Issuance and valuation of corporate bonds with quantitative easing* (Tech. Rep.). ECB Working Paper.

- Personal income tax act.* (2006). Official Gazette of the Republic of Slovenia (Nos. 13/11 – UPB, 9/12 – odl. US, 24/12, 30/12, 40/12 – ZUJF, 75/12, 94/12, 52/13 – odl. US, 96/13, 29/14 – odl. US, 50/14, 23/15, 55/15, 63/16, 69/17, 21/19, 28/19, 66/19, 39/22, 132/22 – odl. US and 158/22).
- Pinto, J. M., Kanda, J., & Silva, B. A. (2021). The cspp impact on non-financial firms' cost of borrowing and debt choice. *Banking & Insurance eJournal*.
- Prondzinski, D., & Miller, M. (2018). Active versus passive investing: Evidence from the 2009-2017 market. *Journal of Accounting and Finance*, 18(8), 119–143.
- Renn, O., Laubichler, M., Lucas, K., Kröger, W., Schanze, J., Scholz, R. W., & Schweizer, P.-J. (2022). Systemic risks from different perspectives. *Risk analysis*, 42(9), 1902–1920.
- Rischen, T., & Theissen, E. (2021). Underpricing in the euro area bond market: New evidence from post-crisis regulation and quantitative easing. *Journal of Financial Intermediation*, 46, 100871.
- Roffia, P., & Mola, L. (2022). Is covid-19 enough? which underestimated conditions characterise the adoption of complex information infrastructures in small and medium-sized enterprises. *Journal of Business Research*, 144, 1249–1255.
- Romer, C. D., & Romer, D. H. (2010). The macroeconomic effects of tax changes: estimates based on a new measure of fiscal shocks. *American Economic Review*, 100(3), 763–801.
- Roth, J. (2018). Should we adjust for the test for pre-trends in difference-in-difference designs? *arXiv preprint arXiv:1804.01208*.
- Roth, J., Sant'Anna, P. H. C., Bilinski, A. M., & Poe, J. (2022). What's trending in difference-in-differences? a synthesis of the recent econometrics literature. *Journal of Econometrics*.
- Santis, R. A. D., Geis, A., Juskaite, A., & Cruz, L. V. (2018). The impact of the corporate sector purchase programme on corporate bond markets and the financing of euro area non-financial corporations. *ECB Economic Bulletin*.
- Santos, J. A., & Tsatsaronis, K. (2003). The cost of barriers to entry: evidence from the market for corporate euro bond underwriting.
- Setiyono, B., & Munawaroh, U. (2024). Related party lending and rural bank risk: Evidence during the covid-19 period. *Research in International Business and Finance*, 67, 102079.
- Siani, K. (2019a). Global demand spillovers in corporate bond issuance: The effect of underwriter networks. *Available at SSRN*.

- Siani, K. (2019b). Global demand spillovers: When the central bank buys corporate bonds. *Available at SSRN 3508937*.
- Silva, T. C., de Souza, S. R. S., Guerra, S. M., & Tabak, B. M. (2023). Covid-19 and bank branch lending: The moderating effect of digitalization. *Journal of Banking & Finance*, 152, 106869.
- Simbyakula, T. K., & Hapompwe, C. C. (2024). Effect of turnover tax on small and medium enterprises' (smes) growth in the clothing retail industry: A case study of lusaka. *JOURNAL OF ECONOMICS, FINANCE AND MANAGEMENT STUDIES*.
- Skinner, C. P. (2021). Central bank activism. *Duke LJ*, 71, 247.
- Song, W.-L. (2004). Competition and coalition among underwriters: The decision to join a syndicate. *The Journal of Finance*, 59(5), 2421–2444.
- Stulz, R. M. (2022). Risk-taking and risk management by banks. *Journal of Applied Corporate Finance*, 34(1), 95–105.
- Sufi, A. (2009). The real effects of debt certification: Evidence from the introduction of bank loan ratings. *The Review of Financial Studies*, 22(4), 1659–1691.
- Summers, L. H., Bosworth, B. P., Tobin, J., & White, P. M. (1981). Taxation and corporate investment: A q-theory approach. *Brookings Papers on Economic Activity*, 1981(1), 67–140.
- Sun, L., & Abraham, S. (2021). Estimating dynamic treatment effects in event studies with heterogeneous treatment effects. *Journal of econometrics*, 225(2), 175–199.
- Takeda, A., Truong, H. T., & Sonobe, T. (2022). The impacts of the covid-19 pandemic on micro, small, and medium enterprises in asia and their digitalization responses. *Journal of Asian Economics*, 82, 101533.
- Tirachini, A., & Cats, O. (2020). Covid-19 and public transportation: Current assessment, prospects, and research needs. *Journal of public transportation*, 22(1), 1–21.
- Todorov, K. (2017). Quantify the quantitative easing: Impact on bonds and corporate debt issuance. *Corporate Finance: Capital Structure & Payout Policies eJournal*.
- Tsindeliani, I. A., Proshunin, M. M., Sadovskaya, T. D., Popkova, Z. G., Davydova, M. A., & Babayan, O. A. (2022). Digital transformation of the banking system in the context of sustainable development. *Journal of Money Laundering Control*, 25(1), 165–180.
- Venâncio, A., Barros, V., & Raposo, C. C. (2020). Corporate taxes and high-quality entrepreneurship. *Small Business Economics*, 58, 353–382.

- Wang, R., Liu, J., & Luo, H. (2021). Fintech development and bank risk taking in china. *The European Journal of Finance*, 27(4-5), 397–418.
- Wang, Z., Dong, Y., & Liu, A. (2022). How does china's stock market react to supply chain disruptions from covid-19? *International Review of Financial Analysis*, 82, 102168.
- Wooldridge, J. M. (2021). Two-way fixed effects, the two-way mundlak regression, and difference-in-differences estimators. *ERN: Estimation (Topic)*.
- Xie, X., Mirza, N., Umar, M., & Ji, X. (2024). Covid-19 and market discipline: Evidence from the banking sector in emerging markets. *International Review of Economics & Finance*, 89, 612–621.
- Xie, X., & Wang, S. (2023). Digital transformation of commercial banks in china: Measurement, progress and impact. *China Economic Quarterly International*, 3(1), 35–45.
- Xing, J., Bilicka, K., & Hou, X. (2022). How distortive are turnover taxes? evidence from replacing turnover tax with vat. *SSRN Electronic Journal*.
- Xing, J., Bilicka, K., Hou, X., & Raei, S. (2024). How distortive are turnover taxes? evidence from china. *Journal of Development Economics*.
- Yan, J., & Jia, P. (2022). The impact of covid-19 on bank sector traditional business model sustainability in china: Bank branch versus fintech. *Frontiers in Physics*, 10, 820646.
- Yang, F., & Masron, T. A. (2024). Role of financial inclusion and digital transformation on bank credit risk. *Journal of International Financial Markets, Institutions and Money*, 91, 101934.
- Yang, J., Zhang, X., & Pei, Y. (2025). Digital transformation of the business models of chinese sporting goods enterprises in the post-covid-19 era: a knowledge-management perspective. *Journal of Knowledge Management*, 29(3), 723–734.
- Yao, T., & Song, L. (2023). Can digital transformation reduce bank systemic risk? empirical evidence from listed banks in china. *Economic Change and Restructuring*, 56(6), 4445–4463.
- Yasuda, A. (2005). Do bank relationships affect the firm's underwriter choice in the corporate-bond underwriting market? *The Journal of Finance*, 60(3), 1259–1292.
- Yasuda, A. (2007). Bank relationships and underwriter competition: Evidence from japan. *Journal of Financial Economics*, 86(2), 369–404.
- Yu, Y., & Shao, P. (2025). Enterprise digital transformation, covid-19 and innovation persistence: evidence from china. *Applied Economics Letters*, 32(8), 1097–1102.

- Zaghini, A. (2019a). The cspp at work: Yield heterogeneity and the portfolio rebalancing channel. *Journal of Corporate Finance*.
- Zaghini, A. (2019b). The cspp at work: Yield heterogeneity and the portfolio rebalancing channel. *Journal of Corporate Finance*, 56, 282–297.
- Zaghini, A. (2020). How ecb purchases of corporate bonds helped reduce firms' borrowing costs. *Research Bulletin*, 66.
- Zhang, D., & Sogn-Grundvåg, G. (2022). Credit constraints and the severity of covid-19 impact: Empirical evidence from enterprise surveys. *Economic Analysis and Policy*, 74, 337–349.
- Zhang, Y., Xing, C., & Guo, X. (2023). The shielding effect of access to finance on small and medium-sized enterprises during the covid-19 crisis: Comparing fintech and traditional finance. *Emerging Markets Finance and Trade*, 59(8), 2383–2397.