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Heterogeneous Impacts of Macroprudential Policies: Financial Advisors, Regulatory Caps, and Mortgage Risk*

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Abstract

This paper examines the impact of borrower-based macroprudential policy tightening on mortgage lending in Slovakia, focusing in particular on the role of financial advisors in shaping loan characteristics. Using a comprehensive loan-level dataset from Slovak banks, we analyze the effects of key regulatory tools — Loan-to-Value (LTV), Debt-to-Income (DTI), and Debt Service-to-Income (DSTI) limits — on mortgage risk profiles. Our contributions include: (1) showing that restrictive borrower-based measures (BBMs) reduce the riskiest loans but push lower-risk segments toward regulatory thresholds, thus increasing portfolio risk; (2) demonstrating that advisor-mediated loans tend to have higher amounts, LTVs, DTIs, and longer maturities, raising their riskiness; and (3) finding that strict enough DSTI limits not only reduce DSTI but may also indirectly effect other loan characteristics, such as DTI, LTV ratios, and loan volumes, suggesting broader policy impacts. Additionally, we identify significant front-loading behavior following policy tightening announcements, particularly for advisor-mediated loans. These findings highlight the importance of detailed micro-level data in capturing policy effects and informing more effective macroprudential regulation.

JEL code: G21, D18, D12.

Keywords: debt behavior, financial advice, macroprudential policy, policy evaluation, heterogeneous effects, register microdata.

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Non-Technical Summary

This paper investigates the impact of macroprudential policies on mortgage lending, with a focus on borrower-based measures (BBMs) such as Loan-to-Value (LTV), Debt-to-Income (DTI), and Debt Service-to-Income (DSTI) limits. Leveraging detailed data from Slovakia — a context characterized by unique market conditions and recent policy interventions — we analyze how these policies influence mortgage loans, including those mediated by financial advisors.

Mortgage loans represent a significant financial commitment for households and play a crucial role in the broader economy. Consequently, policymakers frequently implement regulatory measures to mitigate systemic risks in housing and mortgage markets. However, the effectiveness of these policies varies significantly depending on their design and the prevailing market conditions. This study contributes to this discussion by comparing advised loans, facilitated by financial advisors, with non-advised loans obtained directly from banks.

Our findings indicate that mortgages mediated by financial advisors typically feature higher loan amounts and elevated risk characteristics, such as increased LTV and DTI ratios. Financial advisors often extend loan maturities to maintain manageable monthly repayments; however, this practice also raises overall loan risk. The study underscores that the effects of regulatory tightening are strongly influenced by the specific measures implemented and their degree of restrictiveness.

For example, the less restrictive measures introduced in 2017 had a limited market impact, allowing riskier loan characteristics to persist. In contrast, the more stringent DSTI limits implemented in 2020 had a pronounced effect, significantly reducing DSTI and indirectly influencing DTI, LTV ratios, and loan sizes. These findings highlight the critical role of policy restrictiveness in shaping market behavior.

The study also reveals that policy announcements can prompt "front-loading" behavior, with borrowers rushing to secure loans before tighter limits take effect. This behavior was particularly pronounced for advisor-facilitated loans, underscoring the importance of market-specific characteristics in policy transmission. Once the policies were fully implemented, advisor-mediated loans — typically characterized by higher initial risk levels — experienced more adverse effects compared to direct loans.

Our research demonstrates that average effects of policy interventions can be misleading, as they often mask substantial variations across different segments of the loan distribution. For instance, while these policies effectively curtailed the riskiest loans, they also induced shifts in loan characteristics just below the imposed thresholds, subtly elevating risk in the broader loan portfolio. These findings underscore the necessity of detailed, micro-level data to craft regulations that minimize unintended consequences.

This study advances the understanding of how macroprudential policies influence market behavior, particularly in mortgage lending environments shaped by financial advisors. By highlighting the differential impacts of these regulations, our findings offer valuable insights for policymakers seeking to enhance financial stability while accounting for the complexities of mortgage markets.

1 Introduction

Financing a home through a mortgage is one of the most significant financial commitments an individual can undertake, with profound implications for personal finances and the broader economy. Housing, as a key sector of the real economy, constitutes a major component of household wealth and bank assets (ESRB, 2022). However, housing markets are highly volatile and capable of triggering widespread economic disruptions. Consequently, the development of real estate and mortgage markets is a central focus for macroprudential policymakers worldwide. To mitigate systemic risks and rising household indebtedness, several macroprudential policy tools are employed, including borrower-based measures (BBMs) such as Loan-to-Value (LTV), Debt-to-Income (DTI), and Debt Service-to-Income (DSTI) limits. The activation and enforcement of these regulatory measures have become increasingly widespread. Nevertheless, comprehensive evidence analyzing their impact across the full spectrum of loans and critical risk measures at the loan level remains limited. This study seeks to address this important gap.

This paper examines the differential impacts of BBMs on mortgage loans, leveraging a unique and comprehensive dataset from Slovakia — a country characterized by recent, targeted policy interventions and a distinctive market structure. The role of financial advisors is a central focus, as advisors significantly influence loan contract terms, particularly in markets where consumers lack financial sophistication. Advisor-mediated loans often involve larger amounts with higher LTV and DTI ratios, potentially amplifying risks despite regulatory caps. Analyzing the role of advisors provides critical insights into how macroprudential policies affect risk profiles differently. This understanding is essential for determining whether advisors' influence undermines or complements macroprudential policy objectives, enabling more effective systemic risk mitigation. Ignoring the advisor dimension would risk overlooking these heterogeneous effects and could misguide policy design intended to promote financial stability.

Our study is related to several strands of literature. First, it builds on the household and personal finance literature, which is well-developed, particularly regarding the asset side of household portfolios (see Gomes et al., 2021, for a literature survey). Since the global financial crisis, research has increasingly focused on understanding the drivers of household (over-)indebtedness and the consequences of risky debt behavior. The literature identifies various factors influencing the demand for mortgage and non-mortgage debt, including individual income, peer income (e.g., Georgarakos et al., 2014), age, risk appetite, education, and financial or debt literacy (e.g., Lusardi and Tufano, 2015; Guiso et al., 2022).

Our study is also closely aligned with the second strand of literature, which examines the role of financial advisors and intermediaries in individual economic and financial decisions. In the absence of financial sophistication, consumers often rely on information from various sources, including friends, relatives, and professional advisors, to make better financial decisions. Households, in particular, depend heavily on advice from financial intermediaries: 80% of households in Germany, 91% in the UK, and 73% in the US seek such guidance (Guiso et al., 2022). A growing body of research highlights both the positive and negative impacts of advisors on clients' financial decisions. For instance, Lin et al. (2017) demonstrate a positive effect of financial advisors on life insurance demand. Similarly, Foerster et al. (2018) report increased stock market participation among clients with financial advisors, noting that a longer advisor-client relationship (indicative of trust) enhances clients' willingness to take financial risks. Liu et al. (2019) estimate a positive influence of professional financial advice on savings behavior, with the effect being more pronounced among individuals with lower self-control.

On the other hand, financial advisors are rewarded based on the size of the assets they advise on, whether it is insurance, portfolio size, or loan amount. This creates a potential conflict of interest between the client and the advisor, subjecting financial advisors to the classic principal-agent problem (see LaCour-Little, 2009). Furthermore, Choi (2022) highlights that popular financial advice[1] might diverge from benchmark academic advice. For example, while popular financial advice typically recommends fixed-rate mortgages, academic consensus suggests adjustable-rate mortgages are preferable unless interest rates are low (see Choi, 2022, Table 1). Another issue with such advice is its one-size-fits-all nature, as demonstrated by Foerster et al. (2017). They show that financial advisors exert substantial influence over their clients' asset allocation but provide limited customization. Calcagno and Monticone (2015) adds to this critique, showing that advisors are less

[1]In this context, popular financial advice refers to popular financial books that ordinary consumers use to make financial decisions. While we see some similarities between popular financial advice and financial advisors, we recognize that the two are not necessarily the same.

informative to investors with lower financial literacy and offer valuable information primarily to more financially informed clients.

Previous studies have highlighted varied outcomes associated with mortgage loans obtained through advisors. For instance, LaCour-Little and Chun (1999) and Alexander et al. (2002) demonstrate that advisor-mediated mortgages can exhibit riskier characteristics, such as higher default probabilities, but may also show higher repayment probabilities. Consequently, steering and biased advice in the mortgage market can result in welfare losses for unsophisticated borrowers (Guiso et al., 2022).

Finally, our research aligns with the third strand of literature, which focuses on the empirical assessment of macroprudential policy efficiency as a financial stability tool (Akinci and Olmstead-Rumsey, 2018). Much of the existing literature relies on aggregate data to assess the impact of borrower-based measures on bank loan provision. An overview of these studies, along with evidence of significant heterogeneity in empirical results, is provided by Malovaná et al. (2024).

Several notable country case studies rely on individual survey or loan-level register data. For instance, Hodula et al. (2023a) employs loan-level regulatory survey microdata from Czechia and finds that tightening borrower-based measures, such as DTI and DSTI, helped reducing the average loan size and prompted more risk-sensitive pricing, thereby increasing the average lending rate. Kinghan et al. (2022) analyze the introduction of macroprudential limits on LTV ratios and their effect on the borrowing behavior of first-time homebuyers in Ireland using a quasi-experimental setting. Similarly, Acharya et al. (2020) combine loan-level data on residential mortgages, county-level house prices, and detailed data on banks' other assets to examine the introduction of LTV and LTI limits in Ireland. They document a reallocation of mortgage credit from low- to high-income households and from hot, mostly urban housing markets to cooler ones. Using administrative household-level data for Dutch households, van Bekkum et al. (2024) demonstrate that the LTV limit reduced mortgage leverage among first-time homeowners, with a notable bunching effect at the LTV limit.

An ex-ante evaluation of borrower-based measures across seven European countries, using survey wealth microdata, is provided by Gross and Población (2017). Their findings demonstrate that

LTV and DSTI caps effectively reduce both the probability of default and the loss given default of mortgages. Applying an adjusted model to Slovakia, Jurca et al. (2020) show that LTV limits primarily reduce loss given default, while DSTI limits lower the probability of default. This aligns with the general observation that stricter LTV ratios primarily affect the volume of mortgages granted and indirectly influence housing prices, whereas probabilities of default are driven largely by other loan characteristics. A cross-country analysis for 19 European countries is presented in Giannoulakis et al. (2023). Additionally, using survey microdata for Luxembourg, Giordana and Ziegelmeyer (2024) demonstrate that combining several LTV ratios can more effectively target households that are at risk of becoming vulnerable after an income shock.

Interestingly, research on the impacts of macroprudential policies, particularly in Ireland and Sweden, has shown that the introduction of LTV limits results in *bunching* at these thresholds and compression at the upper end of the distribution (Keenan et al., 2016; Kinghan et al., 2017; Bäckman et al., 2024). Evidence of similar distributional shifts toward implemented LTV limits has been documented in Slovakia by Cesnak et al. (2021) and in the Netherlands by van Bekkum et al. (2024). These findings highlight the importance of fine-tuning LTV limits to mitigate unintended consequences, as emphasized by Montalvo and Raya (2018) and Gatt (2024), who caution that such regulatory measures may produce complex effects beyond their intended scope.

Our research contributes to a small but significant body of studies (e.g., Acharya et al., 2020; Hodula et al., 2023a; van Bekkum et al., 2024) that utilize unique administrative microdata to analyze the consequences of tighter borrower-based measures. However, our study goes further by examining how macroprudential policy changes in the mortgage market are differentially transmitted to individual mortgage behavior through financial advisors. While several notable studies have explored the role of financial advisors (e.g., Foà et al., 2019; Guiso et al., 2022) in the mortgage market, we are not aware of any that address this complex interplay comprehensively.

Our study offers a comprehensive evaluation of the effects of financial advice on mortgage outcomes, clearly distinguishing between advised and direct loan procurements. It advances the understanding of policy impacts across the full spectrum of loan distributions, specifically investigating effects both above and below the policy-imposed thresholds. One of our key findings is the phenomenon of "bunching from below," where less risky loans shift toward the regulatory caps, thereby increasing the overall risk profile of the loan portfolio. This initially counterintuitive effect suggests that tighter macroprudential policies, while effective in limiting the riskiest loans, may inadvertently compress the distribution toward riskier terms just below the imposed limits. Such effects are likely more pronounced in the early stages of policy implementation, as the distribution of contractual parameters tends to be wider before policies have been in place for a longer period. We hypothesize that as policies mature and markets adapt, this bunching effect may stabilize, reducing the tendency for loans to converge at the regulatory thresholds.

We employ a broad set of control variables and machine learning techniques to mitigate selection bias. Additionally, by analyzing the joint distributions of risk measures — such as LTV and DSTI ratios — our approach accounts for potential misinterpretations that may result from oversimplified linear assumptions.

Our analysis reveals significant disparities between advised and non-advised mortgage loans. In general, mediated loans are granted with higher amounts, LTV, and DTI ratios. Longer maturities allow advisors to maintain monthly instalments of mediated loans at levels comparable to those (of loans) granted directly by banks.

We examine the impact of several macroprudential policy tightenings implemented gradually since 2017 by the National Bank of Slovakia. Our findings reveal diverse effects of these policies, largely depending on their restrictiveness, stage of implementation, and transmission channel (mediated vs. non-mediated loans). The less restrictive DSTI tightening and maturity cap introduced in 2017 facilitated a shift in mortgage market preferences toward riskier loans in terms of repayment burden. In contrast, the more restrictive DSTI tightening in 2020 led to a significant decrease in DSTI, which also indirectly influenced other risk measures and loan volumes. We also find evidence of front-loading during the announcement period of LTV and DTI tightening in 2018, an effect amplified by advisors. However, advisor-mediated loans were more affected after the full implementation of the policies, as the restrictions were more binding given the pre-policy loan distribution of mediated loans.

Furthermore, we argue that the impact of the policy extends beyond simple averages, demonstrating significantly different effects across various quantiles of the distribution of selected loan outcomes. Specifically, during the tightening of LTV and DTI in 2018, we observe a positive impact of the policy in the lower quantiles of the distributions for LTV, DTI, and granted amounts, but a weaker or negative impact in the upper quantiles. This indicates that, while the policy successfully restricted the riskiest loans above the threshold as intended, it also increased risk below the threshold by shifting the lower part of the distribution closer to the limit.

The research highlights the complexity of market reactions to macroprudential policy decisions, which are further exacerbated by the role of financial advisors. Our findings suggest that financial advisors can contribute to both unintended risks and strategic compliance behaviors, underscoring the importance of incorporating the advisor–non-advisor distinction in regulatory design. We emphasize the critical need for detailed microdata to tailor policies that effectively address these diverse behaviors and caution against relying on simplistic models based solely on aggregated data.

The remainder of the paper is organized as follows. Section 2 presents the institutional background and policy details. Section 3 introduces the data and provides descriptive statistics. Section 4 outlines our empirical strategy, followed by the main results in Section 5. We complement the main findings with robustness checks and additional analyses. Finally, Section 6 concludes the paper and discusses policy implications.

2 Institutional background

We describe the specific role of financial advisors in the Slovakian mortgage market in subsection 2.1, along with the implementation of macroprudential regulation and other relevant legislative changes in subsection 2.2. The interplay of these factors enhances our understanding of the complex mechanisms through which macroprudential policy influences mortgage markets.

2.1 Financial advisors in Slovakia

Financial advisors have been active in Slovakia since the dissolution of Czechoslovakia in 1993. Their services have evolved alongside the development of financial products available in the market. Initially focused on building savings, their activities gradually expanded to include insurance, collective investments, pension savings, and loans. Currently, more than 400 companies operate in the field of financial advice and intermediation. However, the market is relatively concentrated, with 69% of all financial agents employed by the 10 companies holding the largest market share.

Financial advisors have focused on the mortgage market since its inception in 2003–2004. The share of housing loans granted through financial advisors has steadily increased over time. While approximately 50% of mortgages were brokered in 2015, this share rose to 65% by 2022. The activity of financial advisors may have been boosted by a legislative change in March 2016, which capped the fee for early mortgage repayment at 1% of the outstanding notional amount. This reform led to a rapid increase in loan refinancing, as it became highly affordable for clients in an environment of declining interest rates (NBS, 2016).

Although the activity of financial advisors can be beneficial for clients, several risks have been highlighted by the Národná banka Slovenska (NBS). Increased refinancing activities in Slovakia have often been associated with higher notional amounts of new loans, leading to greater household indebtedness. Additionally, growing brokerage activity may exert pressure on banks' credit standards. Finally, the activity of brokers can intensify competition between banks, yielding both positive and negative effects. While increased competition can improve terms for consumers, it may also weaken banks' credit standards, potentially elevating systemic risk within the financial system (NBS, 2019).

2.2 Legislative changes and macroprudential policy implementation

A significant legislative change came into force on 1 January 2018, concerning the covered bond issuance framework. Until the end of 2017, only banks with a mortgage license were authorized to provide mortgage loans. Additionally, these banks were required to finance at least 90% of their mortgage loans with mortgage bonds. This framework, introduced in 1996, became particularly prominent following the restructuring of the banking sector in the early 2000s. By 2017, however, most banks were issuing other types of housing loans, and mortgage loans were primarily used due to the state subsidy for young households.[2]

[2]Young debtors aged up to 35 years with earnings below 1.3 times the average wage in the economy could apply for a state subsidy. Under this program, the interest rate on the mortgage was reduced by 3 percentage points: 2 percentage points subsidized by the state and 1 percentage point by the granting bank. This subsidy applied to mortgages with a nominal value up to 50,000 EUR and an LTV ratio of up to 70%.

By 2018, the previous system had become largely outdated. The introduced legislative changes unified mortgage loans and other housing loans, eliminating the requirement for banks to hold a mortgage license. Following this change, banks were no longer obligated to finance mortgages with mortgage bonds. Instead, they were permitted to issue covered bonds, primarily secured by mortgages with an LTV of up to 70%. This reform enabled banks to access the international covered bond market and secure cheaper funding. It also created a housing loan market more comparable to those in other euro area countries (NBS, 2017).

These legislative changes are directly relevant to our borrower-based policy analysis, as they significantly altered the structure and dynamics of mortgage financing in Slovakia. By removing the mortgage license requirement and enabling access to international covered bond markets, banks gained greater flexibility and access to cheaper funding sources. For example, the increased competitiveness in the mortgage market resulting from these changes may have encouraged banks to maximize lending near the new regulatory thresholds, necessitating more stringent macroprudential measures to mitigate systemic risks. Additionally, the cancellation of subsidies for young borrowers likely influenced borrower behavior, potentially shifting the distribution of LTV ratios as households adapted to the new regulatory and financial environment.

The NBS was granted a mandate to address systemic risks in 2014. Until 2016, it implemented borrower-based measures primarily as recommendations. Since 2017, supported by a legal mandate for such measures, these limits have become legally binding. In response to excessive credit growth, rising real estate prices, and mounting household indebtedness, the NBS introduced a comprehensive set of limits, including LTV, DSTI, DTI, and maturity restrictions. The maturity limit of 30 years for mortgage loans and 8 years for consumer loans was effectively applied from 2014 and became legally binding in the first quarter of 2017. An overview and timeline of the implementation of LTV, DTI, and DSTI limits are provided in Table 2.

In 2018, the NBS implemented one of its most comprehensive packages of borrower-based measures. An LTV cap of 90% was introduced, with only 20% of new mortgages per quarter permitted to exceed an 80% LTV. This limit was phased in gradually, starting in July 2018 and concluding in July 2019. A DSTI cap of 80% was also implemented, with a gradual phase-in ending in July 2018.

While this cap is relatively high compared to limits in other countries, its definition differs. In most cases, the debt service is divided by net or gross income; in Slovakia, however, net income is reduced by the minimum subsistence amount (including the minimum subsistence amount for children and a spouse, if applicable).[3] Additionally, a DTI cap of 8 was introduced in July 2018, with a phase-in period extending to July 2019. This means that a borrower's total debt cannot exceed eight times their annual net income.[4]

A comprehensive set of measures was implemented because they complement rather than substitute one another. LTV limits primarily aim to reduce the potential losses banks may incur in the event of borrower default. DSTI and DTI limits, on the other hand, influence the likelihood of borrower default. By design, the DSTI limit in Slovakia is more binding for lower-income borrowers, as their income is closer to the subsistence minimum. In contrast, the DTI limit focuses more on higher-income borrowers and was introduced to constrain the overall growth of household indebtedness. Together, these measures are intended to address the perceived accumulation of systemic risks in Slovakia.[5]

[3] An overview of measures implemented by different EU countries is available on the European Systemic Risk Board website: https://www.esrb.europa.eu/national_policy/other/html/index.en.html.

[4] An overview of the current measures and their definitions is available on the NBS website: https://nbs.sk/en/financial-stability/fs-instruments/.

[5] This is supported by empirical evidence; see, e.g., (Harrison et al., 2018).

Table 1: Dates of policy change announcements and implementations

Document	Announcement	Implementation	Full implementation
Decree 1	16–09–22	17–01–01	17–07–01 for LTV 18–07–01 for DSTI
Decree 2	18–03–06	18–07–01	19–07–01
Decree 3	19–11–20	20–01–01	20–07–01

Table 2: An overview and timeline of the implementation of BBM limits

	Date	**LTV**		**DSTI**		**DTI**	
		Maximum	Restriction	Maximum	Exception	Maximum	Exception
Recommendation	14–11	100%	25% between 90%–100%	100%			
	15–07		20% between 90%–100%				
	16–04		15% between 90%–100%				
	17–01		10% between 90%–100%				
Decree 1	17–01	100%	10% between 90%–100% 50% between 80%–100%	Specification of the definition of DSTI			
	17–03			95%			
	17–07		10% between 90%–100% 40% between 80%–100%	90%			
	18–01			85%			
	18–07			80%			
Decree 2	18–07	90%	35% between 80%–90%			8 years	20% above
	18–10		30% between 80%–90%				15% above
	19–01		25% between 80%–90%				10% above
	19–07		20% between 80%–90%				5% + 5% (younger borrowers) above
Decree 3	20–01			60%	15% between 60%–80%		
	20–04				5% between 60%–80%		
	20–07				5% between 60%–70%		

Notes: Specification of the definition of DSTI includes borrowers' subsistence minimum and 2 p.p. interest rate shock into the calculation of the DSTI. 30 years limit (with 10% above exception) on housing loans maturity was part of the recommendation and has been fully implemented within the Decree 1. 8 years limit on consumer loans maturity as well as the consideration of consumer loan instalments in DSTI calculation (and volumes in DTI calculation later on) has been implemented since the start of 2018.

3 Data, variables, and estimation sample

In our study, we leverage a comprehensive dataset comprising the universe of mortgage loans in Slovakia (loan tapes), including both loans granted directly (non-mediated) and those facilitated by advisors (mediated). The loan register data span quarterly reports from 2018Q2 onwards, encompassing all active loans at the time of each report. However, loans granted before 2018Q2 are included only if they remained active as of that quarter. Our focus is on newly granted mortgage loans[6] between 2013Q1 and 2022Q4, enabling us to explore differences between loans mediated by financial advisors and those that are not.[7] While the dataset includes a time dimension, each loan appears only once in our sample at the time of granting, resulting in repeated cross-sectional data.

In selecting variables for our analysis, we adopt a rigorous approach to incorporate relevant information while mitigating potential biases. This involves constructing new variables, such as indicators for additional housing or consumer loans, to capture key aspects of borrower behavior and financial status. Additionally, we implement meticulous data cleaning procedures, including the recovery of missing information[8] and the removal of observations with missing or unreasonable values. To enhance data quality, we trim extreme values from continuous variables.[9] Despite these efforts, substantial sample size reductions occur, primarily due to missing or unreasonable values in critical variables such as income or collateral value. Through these rigorous procedures, we arrive at a cleaned dataset that serves as the foundation for our empirical analysis. We are confident in the representativeness of our data, as the initial dataset comprises virtually the entire universe of retail loans granted by Slovak banks. We find no evidence to suggest that our data cleaning procedures introduce significant selection bias. For definitions of variables used in our empirical analysis, see Appendix A.

Table 3 presents key characteristics of these loans, shedding light on significant differences between the two groups. Non-mediated loans, totaling 145,710, exhibit a mean granted amount of

[6] We focus exclusively on pure new loans collateralized by real estate. Refinancing loans or renegotiated loans are excluded from our sample.

[7] It is important to note that as we examine earlier periods, the number of loans diminishes due to repayments or refinancing.

[8] We leverage quarterly reports of all active loans and apply a "forward-looking approach" (examining newer reports) to recover missing information for certain covariates. Furthermore, some missing values are calculated based on other related variables (e.g., DTI derived from income and total borrower debt).

[9] We trim 0.1% of values from both ends of the distribution for all continuous variables.

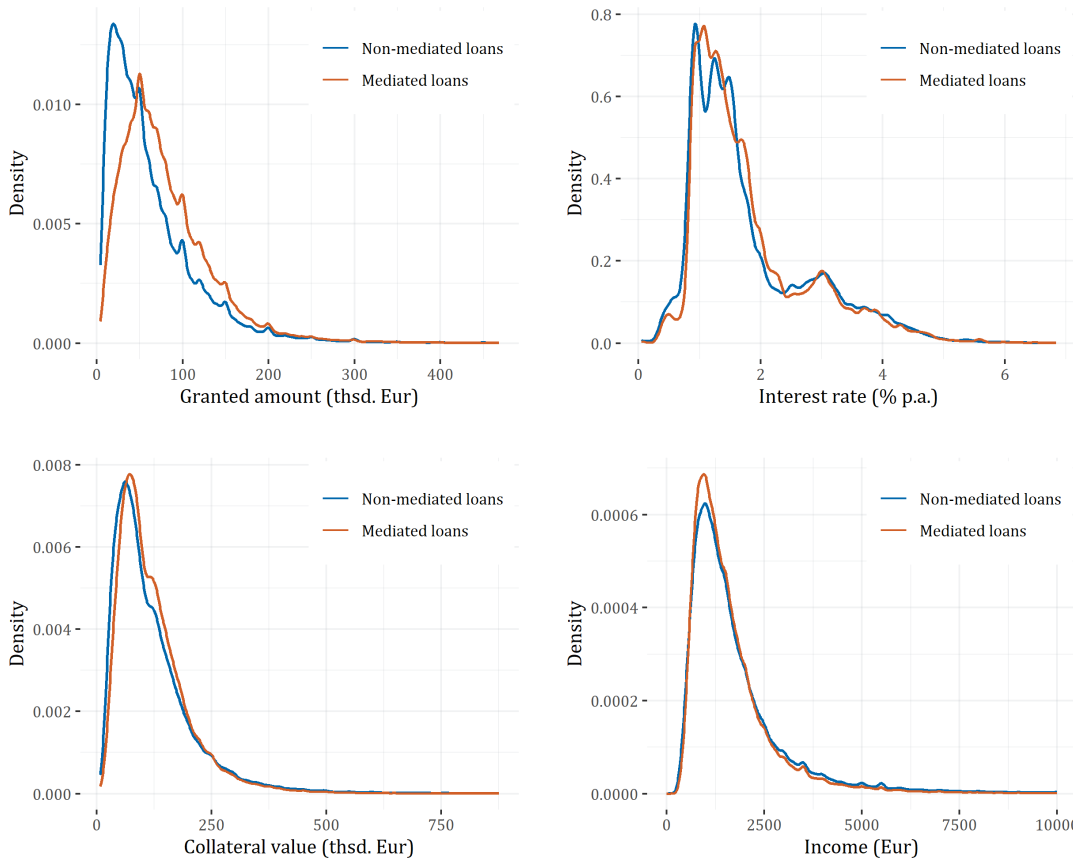
63.1 thsd. EUR with a standard deviation of 52.9 thsd. EUR, while mediated loans, totaling $211,374$, have a higher mean granted amount of 79.9 thsd. EUR and a similar standard deviation of 52.5 thsd. EUR. Additionally, mediated loans have a longer mean granted maturity (26.8 years compared to 22.6 years for non-mediated loans) and higher mean LTV ratios (71% compared to 65%). These differences in loan characteristics provide valuable insights into the heterogeneous nature of mortgage lending practices in Slovakia and serve as a foundation for our subsequent analyses exploring the differential impacts of macroprudential policies on these loans.

Table 3: Descriptives of main variables

Variables	**Non-mediated loans** ($N = 145,710$)		**Mediated loans** ($N = 211,374$)	
	Mean	SD	Mean	SD
Granted amount (thsd.)	63.1	52.9	79.9	52.5
Granted maturity	22.6	7.8	26.8	5.7
LTV	0.65	0.22	0.71	0.19
DTI	4.23	2.26	5.19	2.17
DSTI	0.45	0.20	0.47	0.18
Interest rate	1.80	1.00	1.80	0.96
Collateral (thsd.)	120.0	90.4	123.1	81.0
Income (thsd.)	1.9	1.9	1.7	1.6
Financial assets (thsd.)	12.9	25.5	9.8	22.2
Another HL	0.31		0.22	
Another CL	0.17		0.16	
No child	0.47		0.56	
1 child	0.25		0.23	
2 children	0.21		0.16	
3 children	0.05		0.04	
4 and more children	0.02		0.02	
Co-borrower	0.58		0.49	
Employed	0.85		0.86	
Self-employed	0.12		0.12	
University	0.51		0.47	
Age	37.7	8.8	34.2	7.7
Female	0.31		0.32	

Figure 1 presents the full distributions of selected key variables for non-mediated and mediated loans. It illustrates that the amounts granted are generally larger for loans mediated through financial advisors, though the overall distributions are largely similar.

Figure 1: Distributions of selected variables

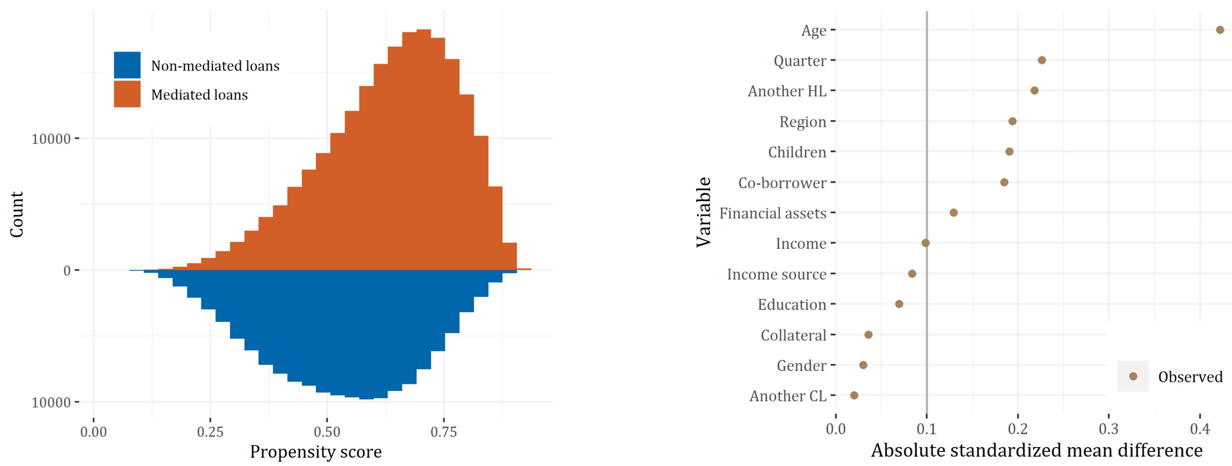


Notes: Due to the high skewness of this variable, the right tail of the income distribution was truncated at 10,000.

Figure 2 illustrates the propensity score derived from a generalized boosted models, predicting whether a loan is mediated through an advisor, alongside standardized mean differences across the predictor variables. While some variation is evident, the propensity score distribution shows substantial overlap, indicating considerable similarity in borrower characteristics between mediated and non-mediated loans. Nevertheless, to address any remaining covariate differences, we employ re-weighting methods as an additional control for potential selection bias in Section 4.

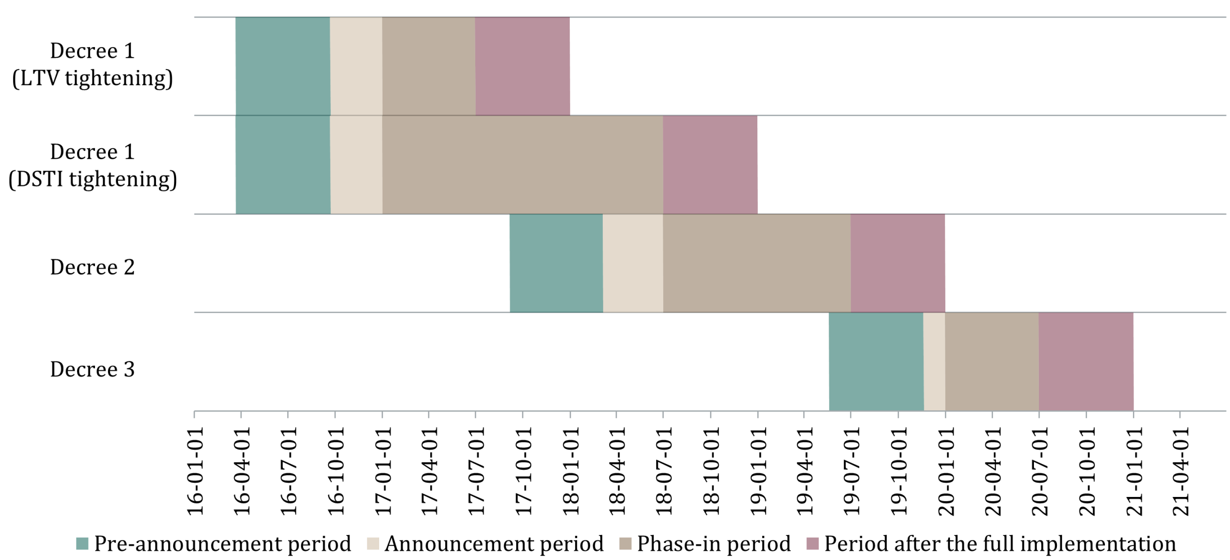
In selecting the estimation sample for the policy effect evaluation, we account for the fact that the macroprudential policy changes were implemented relatively close to each other (see Table 1). Selecting a sample with a wider time window around these policies could lead to potentially biased estimates due to the interference of different policies. For our main analysis, we select the samples starting two quarters before the announcement and ending two quarters after the full implementation of the respective policies (+/− 2Q samples). Figure 3 illustrates the overlaps of these samples. With the exception of the overlap between DSTI tightening of Decree 1 and Decree 2, there is almost no interference between the samples (only approximately 1 month overlap between Decree 2 and Decree 3). Additionally, we run our estimates using also a one year window samples (+/− 1Y), while carefully considering the potential policy interference.

Figure 2: Common support of mediated and non-mediated loans



Notes: The propensity score is estimated with a generalized boosted models – see Section 4.3 for more details. In case of multinomial variables we plot a generalized form of the standardized mean difference metric proposed by Yang and Dalton (2012).

Figure 3: Overlap of policies: +/− 2Q samples



Notes: +/− 2Q indicates subsamples of loans granted from 2 quarters before the announcement to 2 quarters after the full implementation of the respective policy.

4 Econometric specifications

We analyze the differential impacts of BBMs on mortgage loans, with a particular focus on how these effects differ between mediated and non-mediated loans facilitated by financial advisors. In the absence of random assignment in our data, or even a quasi-experimental setting (e.g., Kinghan et al., 2022; Hodula et al., 2023b), our econometric strategy relies on controlling for a wide array of covariates to robustly identify potential policy effects. Specifically, we compare mortgage loans granted before and after the policy announcement or across different stages of the policy rollout, aiming to balance covariate characteristics to address potential selection bias. Similarly, we balance covariate characteristics between mediated and non-mediated loans (see Section 4.3 for details). Our approach is strengthened by a sufficiently large sample size, enabling us to maintain statistical power and precision.

4.1 Regression Analysis

First, we run the following standard regression to estimate the effect of advisors, the policy and their interaction (to cover different policy effects on risk measures for mediated loans):

$$Y_{i,t} = \alpha + \beta Advisor_{i,t} + \gamma BBM_t + \delta \left(Advisor_{i,t} \times BBM_t \right) + \zeta X_{i,t} + \eta Z_{i,t} + \lambda + \varepsilon_{i,t}, \tag{1}$$

where $Y_{i,t}$ captures respective parameters of the i-th individual loan (granted amount, maturity, LTV, DTI, DSTI) granted at time t, $Advisor_{i,t}$ indicates whether the i-th individual loan was mediated through financial advisor in time t. We work with two versions of BBM_t variable. The first version is a dummy variable indicating whether the loan was granted after the policy announcement and thus captures the total effect of the policy, starting immediately after the announcement. The second version is a multinomial variable determining different periods related to the policy implementation: period before the announcement, period between the announcement and implementation, phase-in period and period after the full implementation. This version allows us to explore the different effects of the policy at different stages of the implementation. Besides these covariates of interest, we also include a set of loan parameters ($X_{i,t}$), borrower characteristics ($Z_{i,t}$) as well as bank, region, quarter and month of year fixed effects (λ) to control for borrower preferences and mortgage market characteristics.[10] Standard errors are clustered at the bank level to account for intra-group correlation, enhancing the robustness of our inference.

Our estimation approach is based on the assumption that the loans granted after the policy announcement would have been - without the announcement and/or policy taking place - distributed equally as loans granted in the period before the announcement. In other words, we use the pre-announcement loan distribution as the counterfactual distribution for estimating policy effects in post-announcement periods (similar assumption can be found in, e.g., Bäckman et al., 2024).

4.2 RIF Unconditional Quantile Regressions

Next, we examine the effects of policy and financial advice (as well as their interactions) on different parts of the distribution of the respective outcome variables. To do so, we use unconditional quantile regressions (UQR) developed by Firpo et al. (2009), which are based on the concept of recentered influence functions (RIF) – a widely used tool in robust estimation.[11] This method has several advantages over standard conditional quantile regressions, making it an attractive tool for studying distributional effects.

Once the RIF of the unconditional quantile (τ) of the outcome variable is obtained, the UQR can generally be estimated within a simple OLS framework as follows:

$$RIF(Y, Q_\tau(.), F_Y) = \beta_0(\tau) + \beta_X(\tau)X + \varepsilon, \tag{2}$$

with τ taking quantile values from a range of $0.05 - 0.95$.

The UQR thus marginalizes the effect of the variables of interest (mediated vs non-mediated loans and the policy change) over all other control variables included in the model. In terms of inference, bootstrap standard errors are estimated by drawing 1,000 random samples with replacement from the original sample.

[10] We include income and collateral value in logs, financial assets ihs transformed and squared term of age variable into the model.

[11] For more details on the empirical application of this method, we refer the interested reader to, e.g., Maclean et al. (2014) or Cupak et al. (2022).

4.3 Inverse probability of treatment weighting (IPTW)

We acknowledge that any causal claims are contingent upon the assumption that selection bias can be adequately addressed through observable variables. We have implemented a comprehensive strategy that integrates both, flexible parametric and non-parametric functional forms, to minimize the emergence of such bias where possible. We carry out the two-stage regression procedure, also known as "doubly robust approach". In the first stage, the covariates are balanced between mediated and non-mediated loans and similarly between loans granted before and after the policy announcement or among different stages of the policy implementation using the inverse probability of treatment weighting (IPTW) technique.[12] In the second stage, the OLS regression (1) is estimated using the weighted sample obtained in the first stage.

The intuition behind balancing the populations is to make them as similar as possible in terms of the available covariates. The aim is to reduce the bias arising from possible non-random assignment of observations to treatment or structural differences between observations in the pre- and post-intervention periods. In general, the aim is to design a quasi-experimental framework with the observational data. We measure the similarities among observations using propensity scores[13], i.e. the conditional probabilities of being assigned into the treatment d[14].

The literature on the subject is not entirely consistent as to which variables should be part of the balancing process. However, there are theoretical arguments in favor of using only pre-treatment variables that simultaneously influence the participation decision and the outcome variable (Caliendo and Kopeinig, 2008; Austin, 2011). Therefore, we exclude interest rate and bank fixed effects from the balancing process, as they are measured at the "post-treatment" and only influence the outcome variables.

In addition, there are a few other adjustments that need to be made to successfully balance loans before and after the policy announcement, or among different stages of policy implementation. First,

[12] See, e.g., Austin and Stuart (2015) for practical guidance in application of IPTW.

[13] First introduced in Rosenbaum and Rubin (1983).

[14] In our case the treatment d refers to the possible state of the loan – mediated/non-mediated, if the treatment variable is $Advisor$; granted pre-/post-policy announcement, if the treatment variable is BBM (binomial) or granted during specific stage of the policy implementation (pre-announcement period, announcement period, phase-in period, post-full implementation period) if the treatment variable is BBM (multinomial).

we exclude quarter fixed effects as they would fully or almost fully explain our treatment variable.[15] Second, we add the advisor dummy variable to the list of explanatory variables to account for the potentially different structure of loans granted at different stages of the policy implementation. Finally, we normalize variables that show increasing trends over time (income, collateral value and financial assets) by dividing each value by the mean value of a given quarter.

More formally, propensity scores are estimated using generalized boosted models (GBM):

$$p_d(X_i) = Pr(D_i[d] = 1|X_i), \tag{3}$$

where D_i represents our treatment variable of interest ($Advisor$ or BBM) and X_i contains all the selected covariates that we aim to balance our sample across.[16]

In particular, GBM is a machine learning method used for predicting values of a dependent variable based on the values of independent variables. It is based on sequential fitting of decision trees, where each new tree is fitted to the residuals of the previous one, gradually correcting the predictions and minimizing the residual error[17]. The strong advantage of GBM is that it doesn't require any assumptions about the underlying form of the model. The algorithm automatically conducts selection of variables among covariates as well as captures higher order relationships among them. For details about the tuning of model's parameters see Appendix B.

Finally, we calculate the weight of each observation as an inverse of the probability of the treatment received[18], obtained from equation (3):

$$w_i = \frac{1}{p_d(X_i)}. \tag{4}$$

[15]For the same reason we do not include month of year fixed effects. However, we do not include this variable in balancing between mediated and non-mediated loans either, as the time-related variable is already included in the form of quarter fixed effects.

[16]See McCaffrey et al. (2004) for more details on the empirical application of generalized boosted models in propensity score estimation, and McCaffrey et al. (2013) for their application in a multiple treatment setup.

[17]The residual error in this case is the gradient of the loss function, which is based on the underlying distribution of the dependent variable.

[18]That is, for the i-th individual loan we use the propensity score $p_d(X_i)$, where d refers to the original state of the loan (mediated/non-mediated; granted pre-/post-announcement; granted in a specific stage of the policy implementation).

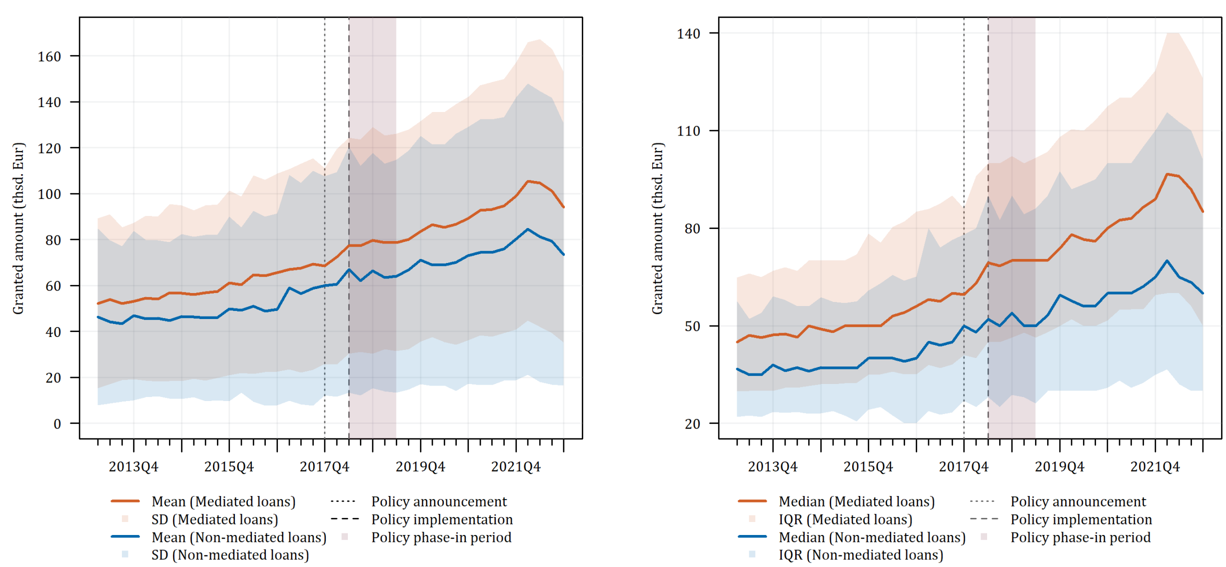
This procedure is designed in a way that attributes more weight to observations that are less likely to be assigned into the treatment that they were actually assigned. These subjects are more rare and more informative as they give us a picture of how treatment would influence the non-treated subjects. IPTW creates a pseudo-sample, in which the treatment is independent of the measured covariates.[19]

5 Estimation results

5.1 Baseline results

Figure 4 presents the trend of granted loan amounts over time. A notable surge in amounts is evident for both mediated and non-mediated loans, with a slightly more pronounced increase observed for mediated loans shortly after the announcement of Decree 2 policy. Both average and median loan amounts appear to stabilize during the phase-in period after the policy implementation. While one might initially attribute the increase in the granted amount to borrowers seeking larger volumes before the policy takes effect, a deeper analysis reveals more intricate shifts in the distributions, indicating changes beyond mere mean values. The trends of other loan outcomes are shown in Appendix C.

Figure 4: Average and median granted amount over time



Notes: The policy shown in the figure represents the announcement, implementation and phase-in of Decree 2.

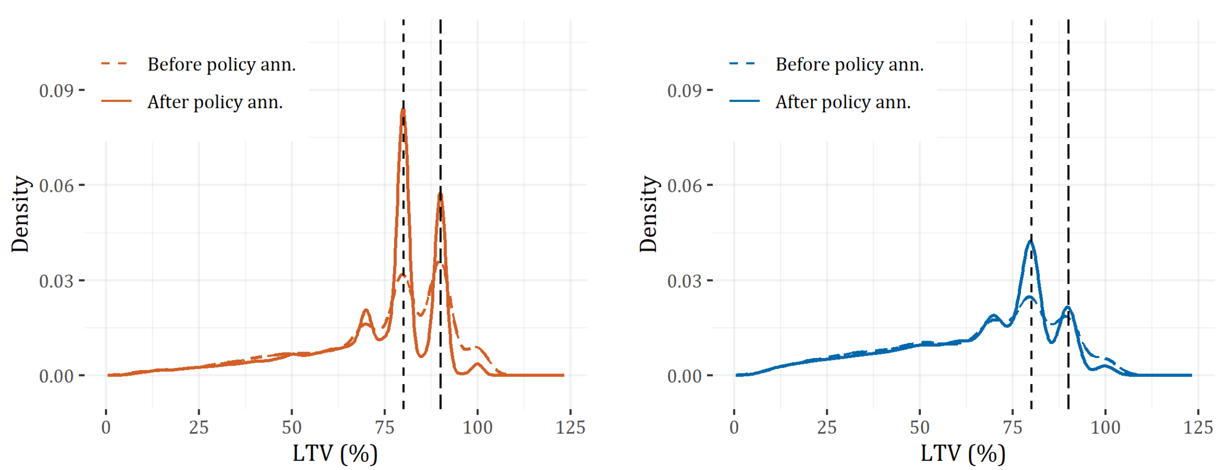
[19] A detailed diagnosis of the balance can be found in Appendix B.

Figure 5 displays the LTV distributions of mediated (left panel) and non-mediated (right panel) loans, comparing loans granted before and after the announcement of Decree 2. The distinctive bunching effect around the policy thresholds (80% and 90% LTV) is evident, indicating a clear impact of the policy. Notably, the policy not only results in new loans falling below the introduced limits but also induces a shift in weight from below towards these newly established thresholds. This shift suggests that loan levels previously considered unnecessary or perceived as too risky by borrowers are now moving closer to the regulatory caps, possibly because these thresholds are perceived as "safe" benchmarks set by the policy. This perception may also be reinforced by financial advisors, who can leverage these policy-imposed caps to convince borrowers that loans close to the thresholds are inherently safe. As a result, while the policy successfully restricts the most excessive risk-taking, it simultaneously encourages clustering near the caps, which could increase the overall risk of the portfolio in the short term. The remaining share of loans granted after the announcement with LTV above the upper limit of 90% indicates possible front-loading before the policy takes effect. We observe similar bunching patterns and weight shifts in other risk measures and for other policies (DTI and DSTI, see Appendix D).

Figure 5: LTV distributions before vs. after the policy announcement (+/− 2Q sample)

(a) Mediated loans

(b) Non-mediated loans



Notes: Policy announcement refers to the announcement of Decree 2. +/− 2Q indicates subsamples of loans granted from 2 quarters before the announcement to 2 quarters after the full implementation of the policy. Vertical lines mark the policy thresholds.

Table 4 provides the estimated coefficients of the regression model examining the impact of the financial advisors (mediated vs. non-mediated loans dummy) on granted amounts, LTV, DTI and DSTI ratios, and loan maturity. These effects are estimated for the full sample (2013Q1 – 2022Q4), controlling for a large number of borrower- and mortgage market-based covariates. Results confirm that the volume of loans granted via financial advisors are significantly different in nominal terms as well as relative to income or collateral. Mediated loans are in general larger by approximately 16 % of volume, 4 p.p. of LTV and almost 40% of the borrowers' annual net income. Interestingly, mediated loans differ only slightly from non-mediated loans in terms of debt service burden. This is because mediated loans are on average granted with a longer maturity of around 1.8 years, which allows borrowers to take on more debt for the same amount of monthly repayments.

Table 4: Effect of financial advisors on selected loan outcomes

	Log of granted amount	**LTV**	**DTI**	**DSTI**	**Loan maturity**
	(1)	(2)	(3)	(4)	(5)
Advisor	0.157***	0.040***	0.386***	0.004*	1.774***
	(0.018)	(0.004)	(0.029)	(0.002)	(0.125)
Loan parameters	Yes	Yes	Yes	Yes	Yes
Borrower characteristics	Yes	Yes	Yes	Yes	Yes
Bank FE	Yes	Yes	Yes	Yes	Yes
Region FE	Yes	Yes	Yes	Yes	Yes
Quarter FE	Yes	Yes	Yes	Yes	Yes
Month of year FE	No	No	No	No	No
Sub-sample	full	full	full	full	full
Observations	356,823	356,706	342,659	356,452	347,551
Adjusted R^2	0.614	0.257	0.521	0.410	0.557

Notes: Full estimation sample period is 2013Q1-2022Q4. Clustered standard errors at the bank level are reported in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 5 extends the results above by including the impact of the policy (before announcement vs. after announcement dummy) and its interaction effect with advisors in the regression model specification. In this case the samples are restricted to shorter periods, starting 2 quarters before the announcement and ending 2 quarters after the full implementation of the policy. We exclude quarter fixed effect from the specification due to the strong collinearity with policy variable and replace it with month of year fixed effect to control for time varying characteristics of the mortgage market. We focus only on the loan outcomes that the policy was directly aimed at. However, we provide additional regression results for each loan outcome and each decree in Appendix E to explore possible indirect effects of the policies.

The impact of advisors on all considered loan outcomes is consistent with the overall effect of advisors estimated for the full sample. However, the impact of the policy is not so straightforward. We observe a strong positive effect of the Decree 1 policy on DSTI and loan maturity, shifting these parameters by almost 11 p.p. and 1 year respectively[20]. The significant lengthening of loan maturities after underscores the adaptive strategies employed by both borrowers and financial advisors in response to new regulatory constraints. This lengthening likely represents an effort to maintain manageable monthly repayments despite the tightening limits on DSTI and LTV. While the extension of maturities can be an effective tool for reducing short-term repayment burdens, it also carries the potential for increased long-term risk, as borrowers remain indebted for longer periods. The evidence of these effects during both the phase-in period and full implementation, as shown in Table 6, highlights a consistent trend of adapting loan terms to align with policy shifts, which may have implications for long-term financial stability. Greater emphasis on loan duration effects is crucial for understanding the full spectrum of borrower-based policy impacts, not just on immediate risk metrics like DSTI, but also on the sustained financial commitments of borrowers.

There is no significant effect of the Decree 2 policy on loan amounts, LTV and DTI. Finally, the effect of Decree 3 policy is significantly negative, decreasing the average DSTI by around 4 p.p. Moreover, we do not find any significant effect of the advisor-policy interaction term, suggesting that advisors do not behave differently when macroprudential policy changes.

[20] One potential explanation is the strong increase of DSTI and loan maturities in general during the implementation of Decree 1 that was not restrictive enough to stop such increase. However, the development of the average DSTI could have been affected also by the fact that the definition of this ratio was not unified until the legal implementation of the decree and therefore there can be reporting issues on the banks' side in case of loans granted before Decree 1.

Table 5: Effect of advisors and policy on selected loan outcomes

	DSTI (Decree 1)	**Loan maturity** (Decree 1)	**Log of granted amount** (Decree 2)	**LTV** (Decree 2)	**DTI** (Decree 2)	**DSTI** (Decree 3)
	(1)	(2)	(3)	(4)	(5)	(6)
Advisor	−0.007	2.128***	0.143***	0.046***	0.535***	0.002
	(0.013)	(0.275)	(0.021)	(0.005)	(0.103)	(0.005)
Policy (total)	0.107**	0.976**	0.011	0.015	0.242*	−0.043***
	(0.045)	(0.397)	(0.044)	(0.013)	(0.137)	(0.006)
Advisor × Policy (total)	0.012	−0.021	0.027	−0.007	−0.161	0.004
	(0.015)	(0.168)	(0.018)	(0.006)	(0.104)	(0.007)
Loan parameters	Yes	Yes	Yes	Yes	Yes	Yes
Borrower characteristics	Yes	Yes	Yes	Yes	Yes	Yes
Bank FE	Yes	Yes	Yes	Yes	Yes	Yes
Region FE	Yes	Yes	Yes	Yes	Yes	Yes
Quarter FE	No	No	No	No	No	No
Month of year FE	Yes	Yes	Yes	Yes	Yes	Yes
Sub-sample	+/− 2Q	+/− 2Q	+/− 2Q	+/− 2Q	+/− 2Q	+/− 2Q
Observations	96,256	93,243	101,697	101,621	96,099	72,894
Adjusted R^2	0.407	0.552	0.594	0.239	0.530	0.431

Notes: +/− 2Q indicates subsamples of loans granted from 2 quarters before the announcement of the policy to 2 quarters after the full implementation of the policy. Pre-announcement period is the reference category for the policy-related variables. Clustered standard errors at the bank level are reported in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

As outlined at the beginning of this section, the effect of the policy might vary across different stages of the policy implementation, e.g., possible front-loading behavior before the policy fully hits. While not differentiating between stages of implementation may give us an overall picture of the policy impact on loan production, it may mask some important distinctive behavior of the mortgage market with the policy being gradually implemented. This could also explain the overall weak impact of the policy, especially for Decree 2 and for advisor-channeled policy effects.

We further extend our regression models and break the policy variable into the separate effects of the policy during different stages of the implementation: the announcement period, the phase-in period, and the period after full implementation. These effects are estimated with relation to the pre-announcement period and thus can not be interpreted as additive. Similarly, we explore the distinctive behavior of advisors during different stages of the policy implementation by adding the interactions of these stages with the advisor dummy variable. We evaluate these effects in Table 6.

The impact of advisors on all considered loan outcomes remains consistent with the previous regression specifications. However, by splitting the policy variable, we discover some very interesting patterns. DSTI and loan maturities increased significantly after the implementation of the Decree 1 policy by almost 12 p.p. and by more than 1 year respectively, and remained elevated even after the policy was fully implemented. This suggests that, prior to the policy, certain lending levels were either unnecessary due to borrower preferences or perceived as too risky. However, the establishment of clear thresholds may have induced a perception that anything up to the regulatory limit is inherently safe. This perception, in turn, could lead to increased risk-taking as both borrowers and lenders - particularly those mediated through advisors-might view everything below the regulatory threshold as a guaranteed safe zone. Financial advisors, leveraging this perception, might have found it easier to convince borrowers to take on larger loans, thus pushing lending levels closer to the regulatory limits and potentially elevating the overall risk profile of the portfolio. Except of the increase in loan maturity by almost half a year during the announcement period, there is no significantly different effect of the policy channeled through advisors.

We observe a significant impact of the Decree 2 policy on loan amounts, LTV and DTI even before the policy was implemented. LTV increased by almost 3 p.p., DTI by more than a third of borrowers' annual net income and loan amounts increased by more than 5% during the announcement period. Interestingly, the amounts increased by an additional 3.5% if mediated by advisors. These results could be explained by agents (borrowers, banks, advisors), realizing the restrictiveness of the policy, taking the advantage of the last possible moments of relaxed conditions before the policy becomes binding. Surprisingly, after the policy was implemented the values of loan outcomes returned to their original levels, represented by insignificant coefficients for phase-in and full implementation. However, this does not mean that the implementation of the policy has no impact on considered loan outcomes. The overall weak impact of the policy can reflect the negative impact on the distribution above the BBM limits and the potentially positive impact on the distribution below the BBM limit. We investigate this potential heterogeneity of effects across the distribution in Section 5.2. As expected, mediated loans were more restricted, in terms of LTV (by almost −2 p.p.) and DTI (by almost −0.25), compared to non-mediated loans, after the policy was fully implemented, as these loans had higher values of LTV and DTI already before the policy.

Decree 3 seems to be the most restrictive out of the three policy packages. We observe a significant decrease of DSTI after the implementation, by almost 7 p.p. during phase-in stage and by 4 p.p. after the full implementation. Mediated loans decreased by additional 1 p.p. even during the announcement period, indicating possible risk perception of banks. Moreover, by tightening the DSTI indicator, Decree 3 indirectly decreased DTI and partially also LTV (see Appendix E), confirming the strong restrictiveness of this policy.

Table 6: Impact of advisors and policy implementation on selected loan outcomes

	DSTI **(Decree 1)**	**Loan maturity** **(Decree 1)**	**Log of granted amount** **(Decree 2)**	**LTV** **(Decree 2)**	**DTI** **(Decree 2)**	**DSTI** **(Decree 3)**
	(1)	(2)	(3)	(4)	(5)	(6)
Advisor	−0.008	2.101***	0.143***	0.046***	0.539***	0.002
	(0.012)	(0.264)	(0.021)	(0.005)	(0.104)	(0.005)
Announcement	0.013	−0.235	0.053***	0.029***	0.358**	0.005*
	(0.011)	(0.145)	(0.015)	(0.005)	(0.173)	(0.003)
Phase-in	0.118**	1.026***	0.002	0.012	0.234*	−0.068***
	(0.051)	(0.381)	(0.039)	(0.012)	(0.141)	(0.018)
Full implementation	0.100**	1.494**	0.022	0.022	0.247*	−0.040***
	(0.048)	(0.688)	(0.062)	(0.020)	(0.126)	(0.004)
Advisor × Announcement	0.008	0.420***	0.035**	0.006	−0.063	−0.008***
	(0.007)	(0.108)	(0.014)	(0.004)	(0.108)	(0.003)
Advisor × Phase-in	0.013	−0.030	0.034*	−0.006	−0.147	0.004
	(0.016)	(0.207)	(0.018)	(0.007)	(0.109)	(0.006)
Advisor × Full implementation	0.010	−0.173	0.009	−0.017**	−0.245**	0.006
	(0.019)	(0.184)	(0.026)	(0.008)	(0.099)	(0.010)
Loan parameters	Yes	Yes	Yes	Yes	Yes	Yes
Borrower characteristics	Yes	Yes	Yes	Yes	Yes	Yes
Bank FE	Yes	Yes	Yes	Yes	Yes	Yes
Region FE	Yes	Yes	Yes	Yes	Yes	Yes
Quarter FE	No	No	No	No	No	No
Month of year FE	Yes	Yes	Yes	Yes	Yes	Yes
Subs-sample	+/− 2Q	+/− 2Q	+/− 2Q	+/− 2Q	+/− 2Q	+/− 2Q
Observations	96,256	93,243	101,697	101,621	96,099	72,894
Adjusted R^2	0.419	0.554	0.595	0.240	0.530	0.435

Notes: +/− 2Q indicates subsamples of loans granted from 2 quarters before the announcement of the policy to 2 quarters after the full implementation of the policy. The pre-announcement period is the reference category for the policy related variables. Clustered standard errors at the bank level are reported in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

The results presented above differ in some cases from results obtained with wider samples (see Appendix E). These differences occur specifically for policy-related effects, reflecting the distortion caused by the interference of different policies when using wider samples.

The regression results provide clear and robust evidence of financial advisors mediating loans with higher amounts, both in absolute terms and relative to the value of collateral or the income of borrowers. While maturities are also higher in case of mediated loans, there is no significant difference in the debt service burden. These results suggest that prolonging the maturity of mortgage loans enabled the advisors to mediate larger loans to their customers, while keeping monthly payments comparable to clients having a mortgage without an advisor.

Further, the results show that the effect of the policy differs across the stages of implementation. This segmentation reveals interesting responses of the mortgage market to policy change, such as front-loading following the announcement of Decree 2, or the shift towards the more risky values of BBMs that are still considered safe, but have been used less before the Decree 1 policy. The latter also relates to another observation. The effect of the policy also depends on how restrictive it is. The less restrictive limits imposed by Decree 1 enabled a further shift of average DSTI and maturity towards riskier levels. On the contrary, the more restrictive policy (Decree 3) not only has a strong negative impact on DSTI, but even spreads the negative impact to other BBMs.

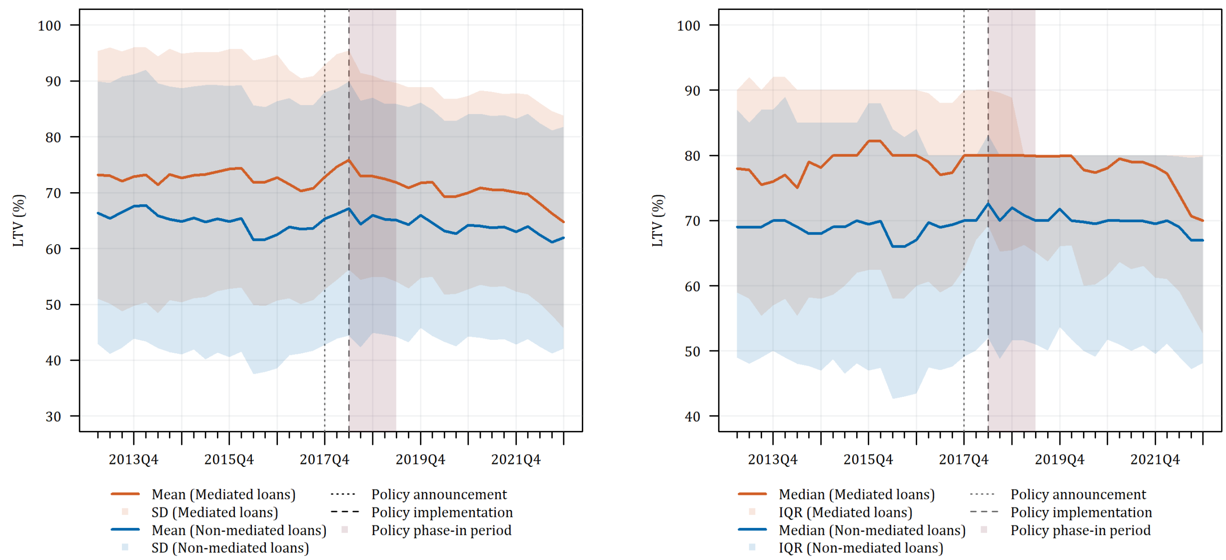
Moreover, in most cases, the impact of the policy on the mediated loans is not significantly different from that on non-mediated loans. However, these average effects may be disrupted by the divergent effects on distribution above and below the imposed limits. In the next subsection we take a closer look at the impact of advisors and macroprudential policy on different parts of the loan distribution.

5.2 Quantile effects

Figure 6 shows the mean and median Loan-to-Value (LTV) ratios of mediated and non-mediated loans over time. While the means of both mediated and non-mediated loans may have been influenced by larger LTVs between the time of announcement and implementation of Decree 2 policy, during which LTVs above the threshold were still permitted, it is evident that the increase in the median LTV since the policy announcement remains stable, particularly for mediated loans, around the lower policy limit of 80%.

Figure 7 shows a large increase in the lower part of the LTV distribution (20th percentile) for mediated loans at the time of policy announcement, which remains at a much higher level than before the policy (at about 60% versus 50% before). For the non-mediated loans this effect is somewhat

Figure 6: LTV mean and median over time



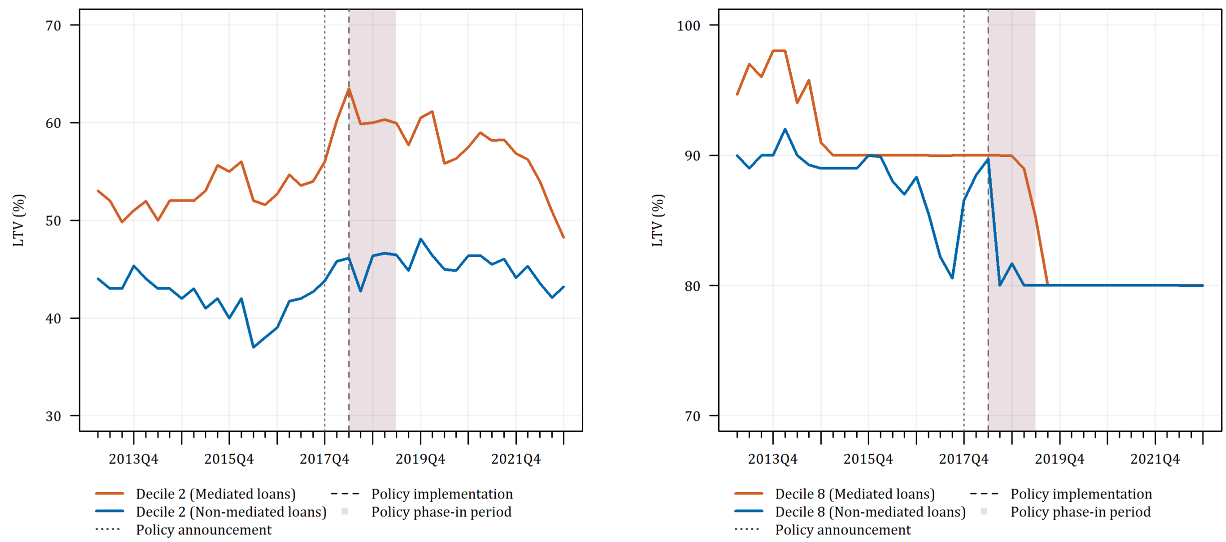
Notes: Policy shown in the figure represents the announcement, implementation and phase-in of Decree 2.

smaller but also persistent. In the upper part of the distribution (80th percentile), mediated loans are practically all the time at the regulatory limit of 90% and 80%, respectively. Non-mediated loans reacted strongly after the policy announcement and the LTV increased rapidly. After the policy implementation, the LTV of both mediated and non-mediated loans stays at the regulatory limit. Overall, these figures clearly demonstrate the positive effect of the policy in the lower and the negative effect in the upper part of the distributions on the LTV of the granted loans.

To analyze these distributional effects in more detail we run RIF regressions as introduced by Firpo et al. (2009), which are also referred to as unconditional quantile regressions as they allow to evaluate distributional effects beyond the mean in a regression framework. Figures 8, 9 and 10 show the impact of the advisors, the impact of the policy (broken down to different stages of implementation) and their combined impact on the different quantiles of the distributions of the selected loan outcomes. We show estimates for the samples starting 2 quarters before the announcement and ending 2 quarters after the full implementation of the respective policies. We provide additional results for wider samples, all considered loan outcomes and each decree in Appendix G.

We observe a strong positive effect of advisors on the lower half of the loan maturity distribution around the time of the Decree 1 policy implementation (Figure 8, panel a). The effect is quite

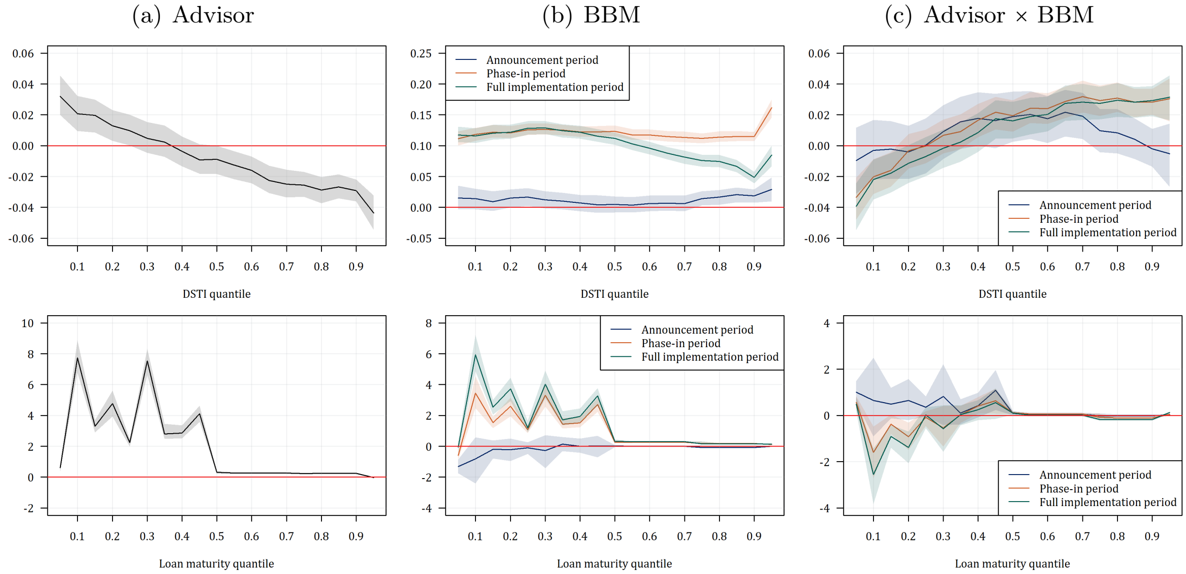
Figure 7: Development of the 20th and the 80th percentile of the LTV distribution



Notes: Policy shown in the figure represents the announcement, implementation and phase-in of Decree 2.

volatile as loans tend to be granted with a few preferred maturities, such as 15, 20, 25 or 30 years. In contrast, we observe only a small effect in quantiles above 50th percentile, as more than half of the loans are granted at or close to the upper limit of 30 years. In line with the results in the previous section, mediated loans are similar to non-mediated loans in terms of the DSTI, only slightly more concentrated around the mean value. The effect of the Decree 1 policy on the DSTI during the phase-in and after full implementation is strongly positive across the whole distribution of DSTI, including the highest quantiles (Figure 8, panel b). This again confirms that there was a significant increase in the DSTI until mid-2017 for both mediated and non-mediated loans. We observe similar positive effect of the policy on loan maturity as well. The advisor-policy effect during the phase-in period and after full implementation of the policy offsets the overall effect of advisors, suggesting that advisors have more or less adjusted the DSTI of loans to market standards (Figure 8, panel c). There is only a small additional negative impact of the policy on the maturity of mediated loans, and only at the lower end of the distribution. This potentially reflects the fact that advisors were continually mediating loans with the largest possible maturities throughout the whole sample.

Figure 8: Quantile effects of Decree 1 policy on selected loan outcomes (+/− 2Q sample)

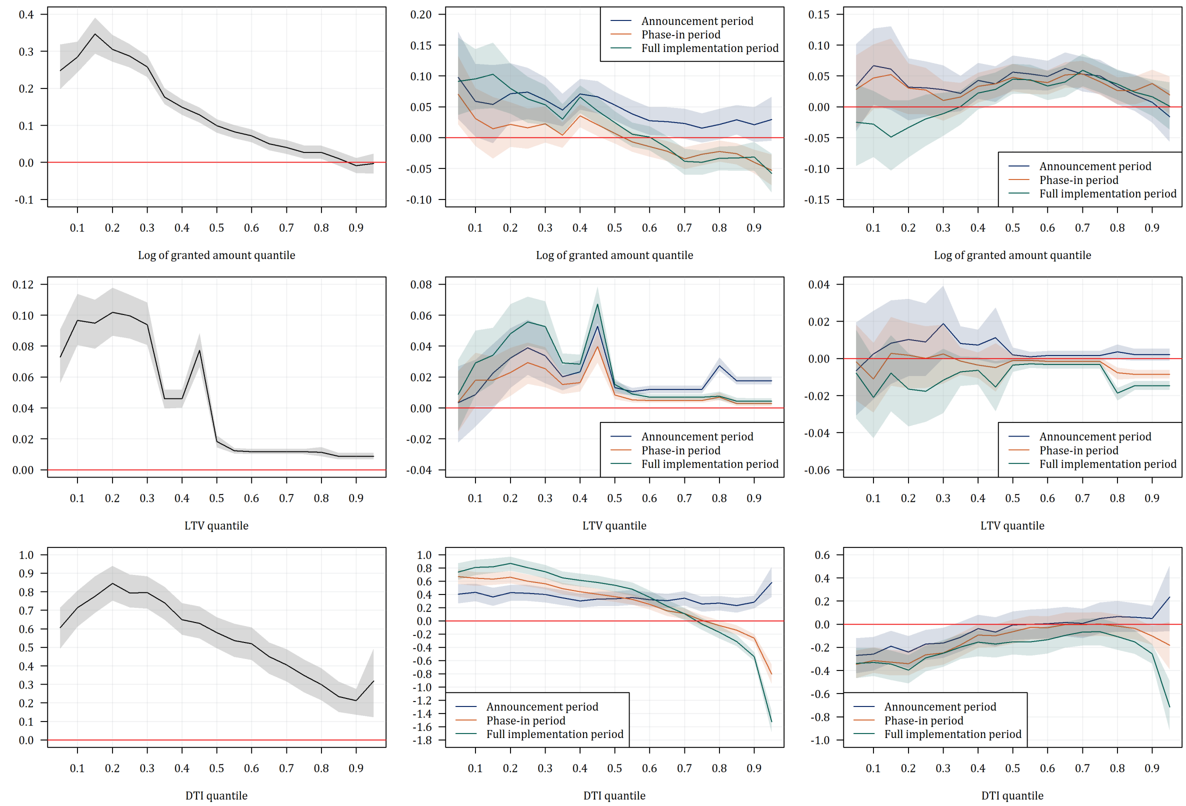


Notes: The figure shows the effect of advisors vis-a-vis the policy on the distribution of selected loan outcomes. The +/− 2Q sample refers to the estimation period including all loans granted from 2 quarters before the announcement of Decree 1 to 2 quarters after the full implementation of DSTI tightening of Decree 1. The pre-announcement period is the reference category for the policy related variables. 95 % confidence bands estimated from 1000 bootstrap replications are represented by the light-colored areas. The red line indicates the zero effect threshold.

During the time of Decree 2 policy implementation, the effect of advisors was significantly positive especially in the lower quantiles of LTV, DTI and granted amounts, demonstrating that advisors are generally targeting larger loans (Figure 9, panel a). As a result of bunching, we observe increased volatility of the effects in the LTV quantiles referring to the regulatory limits. The significant positive effect of the Decree 2 policy during the announcement period, and especially the positive effect at the upper end of the distributions confirm the presence of front-loading behavior (Figure 9, panel b). The impact of the policy implementation (phase-in and full implementation) is in general positive and stronger in case of the lower quantiles, gradually turning weaker and below the average impact when reaching higher quantiles. This means that the higher increase in the volume of loans, LTV and DTI after the policy implementation happened in the part of the distribution below the implemented limits, pushing this part of the distribution to the right, closer to the regulatory burden. Naturally, the part of the distribution being closer to the limits or above was affected less strongly or even negatively. We observe different impact of the policy implementation on mediated loans mostly at the highest quantiles, indicating that the policy was even more restrictive for advisor-mediated loans (Figure 9, panel c).

Figure 9: Quantile effects of Decree 2 policy on selected loan outcomes (+/− 2Q sample)

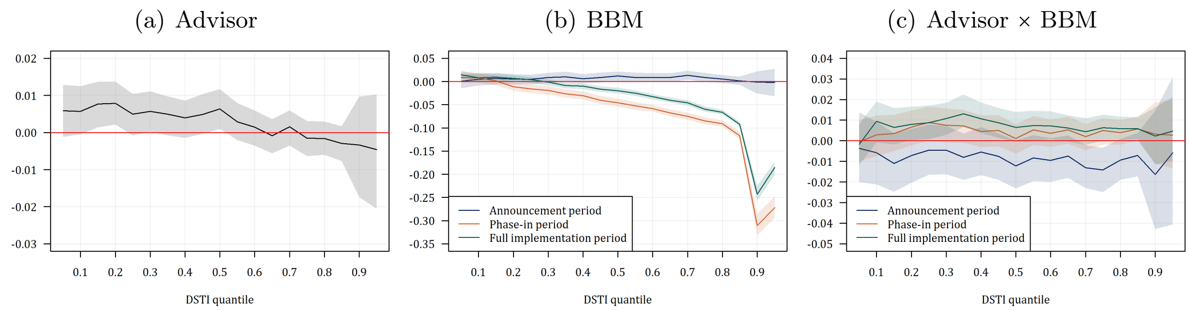
(a) Advisor (b) BBM (c) Advisor × BBM



Notes: The figure shows the effect of advisors vis-a-vis the policy on the distribution of selected loan outcomes. The +/− 2Q sample refers to the estimation period including all loans granted from 2 quarters before the announcement of Decree 2 to 2 quarters after the full implementation of Decree 2. The pre-announcement period is the reference category for the policy related variables. 95 % confidence bands estimated from 1000 bootstrap replications are represented by the light-colored areas. The red line indicates the zero effect threshold.

Regarding Decree 3, we see almost no significant effect of advisors, either before or after the policy announcement. The policy had an equally negative impact on all loans after the implementation, shifting practically the whole distribution of DSTI to the left, below the introduced limit of 60%. Not surprisingly, we observe the strongest effects in the highest quantiles. Additional results from Appendix G show that the policy had an indirect negative impact not only on LTV and DTI, but also on the amount of larger loans (approximately above 40th percentile).

Figure 10: Quantile effects of Decree 3 policy on selected loan outcomes (+/− 2Q sample)



Notes: The figure shows the effect of advisors vis-a-vis the policy on the distribution of DSTI. The +/− 2Q sample refers to the estimation period including all loans granted from 2 quarters before the announcement of Decree 3 to 2 quarters after the full implementation of Decree 3. The pre-announcement period is the reference category for the policy related variables. 95 % confidence bands estimated from 1000 bootstrap replications are represented by the light-colored areas. The red line indicates the zero effect threshold.

The results of the quantile regressions extend the findings of the OLS regressions and explain more complex effect of advisors as well as different effects of policy changes on the distribution of loans, including the bunching at the regulatory thresholds.

Moreover, these results revealed an interesting additional information about the effect of the Decree 2 policy. The LTV and DTI limits were designed to cut off the riskiest loans and thus mitigate the fast growth of loans through the upward pressure on loan volumes. However, it seems that the market compensated this missing mass by producing more loans still below, but closer to the limits. Moreover, the window between the announcement and implementation of the policy was used to front-load with loans exceeding these limits.

5.3 The impact of policy announcement on the joint distribution of the main risk parameters

The two most widely acknowledged parameters affecting the riskiness of the mortgage loans are LTV and DSTI, LTV having more an impact on loss given default and DSTI on the probability of default. Naturally, loans with high DSTI combined with high LTV are deemed most risky from a financial stability perspective. Therefore, in Table 7 we show the joint distribution of these two ratios at the loan level before and after the policy announcement. It is evident that a significant proportion of loans have LTVs ranging between 70% and 90%, distributed across various DSTI levels.

Table 7: Joint distribution (in %) of DSTI and LTV before and after the policy announcement

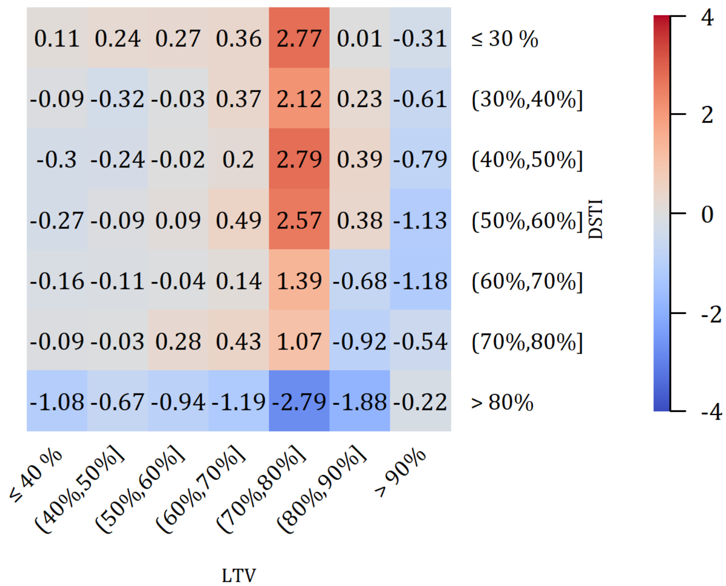
			LTV							
			≤ 40%	(40%, 50%]	(50%, 60%]	(60%, 70%]	(70%, 80%]	(80%, 90%]	> 90%	Sum
DSTI	≤ 30%	Before	3.01	1.26	1.24	1.75	4.31	2.11	0.40	14.07
		After	3.12	1.50	1.51	2.12	7.07	2.11	0.09	17.52
	(30%, 40%]	Before	1.80	1.31	1.14	1.41	3.88	2.69	0.80	13.04
		After	1.71	0.99	1.12	1.78	6.00	2.92	0.19	14.71
	(40%, 50%]	Before	1.88	1.27	1.26	1.74	4.19	3.65	1.10	15.09
		After	1.59	1.04	1.24	1.95	6.98	4.04	0.31	17.14
	(50%, 60%]	Before	1.70	1.06	1.29	1.58	4.54	4.23	1.52	15.91
		After	1.43	0.97	1.37	2.08	7.11	4.61	0.39	17.95
	(60%, 70%]	Before	1.29	0.96	1.12	1.57	3.97	4.49	1.62	15.03
		After	1.13	0.85	1.08	1.71	5.36	3.82	0.44	14.39
	(70%, 80%]	Before	1.38	0.96	0.92	1.31	3.76	4.42	0.87	13.63
		After	1.29	0.94	1.20	1.74	4.83	3.50	0.34	13.83
	> 80%	Before	1.84	1.04	1.38	1.74	4.54	2.39	0.29	13.23
		After	0.76	0.37	0.44	0.55	1.76	0.51	0.07	4.46
	Sum	Before	12.91	7.87	8.34	11.11	29.19	23.98	6.61	
		After	11.04	6.65	7.95	11.91	39.11	21.51	1.83	

Notes: The table shows the joint distribution of loans granted before the announcement of the policy in contrast with the joint distribution of loans granted after the announcement of the policy. The sample includes all loans granted from 2 quarters before the announcement to 2 quarters after the full implementation of the policy. The policy refers to the Decree 2.

Figure 11 presents a heatmap illustrating the changes in the joint distribution, comparing the distributions before and after the policy announcement. There was a noticeable shift of the distribution closer to the LTV limit of 80% across a wider range of DSTI brackets. The largest drop was in the share of loans with DSTI above 80% due to the ongoing DSTI tightening of Decree 1. Naturally, notable drop occurs also in the share of loans exceeding the imposed LTV limit of 90%.

In general, the policy has led to the sizeable mass of loans being collected from both ends of the LTV distribution and concentrated around the new 80% threshold. Importantly, this rearrangement is relatively uniform across the whole DSTI distribution (below the 80% threshold).

Figure 11: Changes in the joint distribution of DSTI and LTV in percentage points after the policy announcement

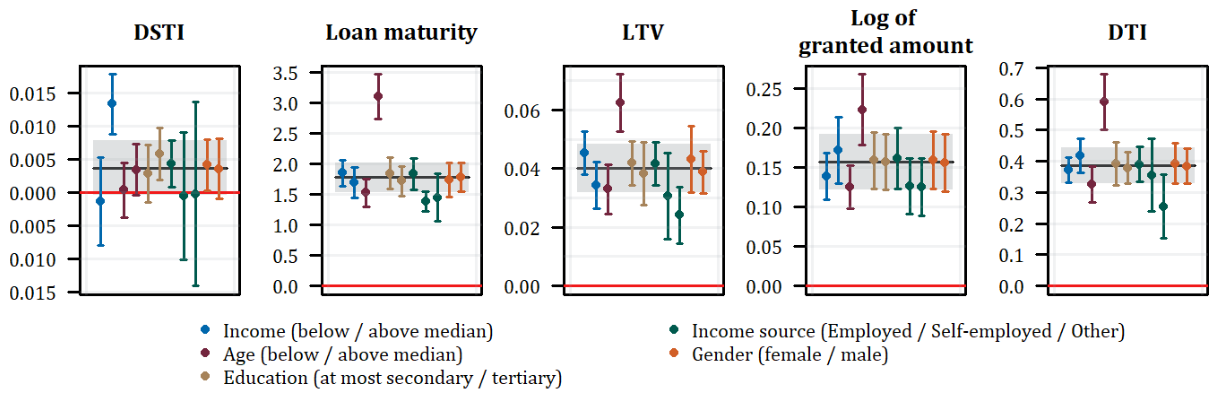


Notes: The figure shows the p.p. changes in joint distribution of loans granted after the announcement of the policy in contrast with the joint distribution of loans granted before the announcement of the policy. The sample includes all loans granted from 2 quarters before the announcement to 2 quarters after the full implementation of the policy. The policy refers to the Decree 2.

5.4 Heterogeneity across borrower characteristics

We conduct subsample estimations to understand how financial advisors and policies affect different borrower groups. Therefore we divide the sample by characteristics such as income, age, education, income source, and gender. Figure 12 reveals that financial advisors positively influence LTV, DTI, granted amounts, and loan maturities across all borrower subsamples. The effect is especially pronounced for older borrowers, indicating higher susceptibility to advisor influence, whereas it is less significant for self-employed borrowers and borrowers with income from other sources. Advisors also notably increase DSTI ratios for higher-income and more-educated borrowers, suggesting targeted negotiation of larger loans for creditworthy clients.

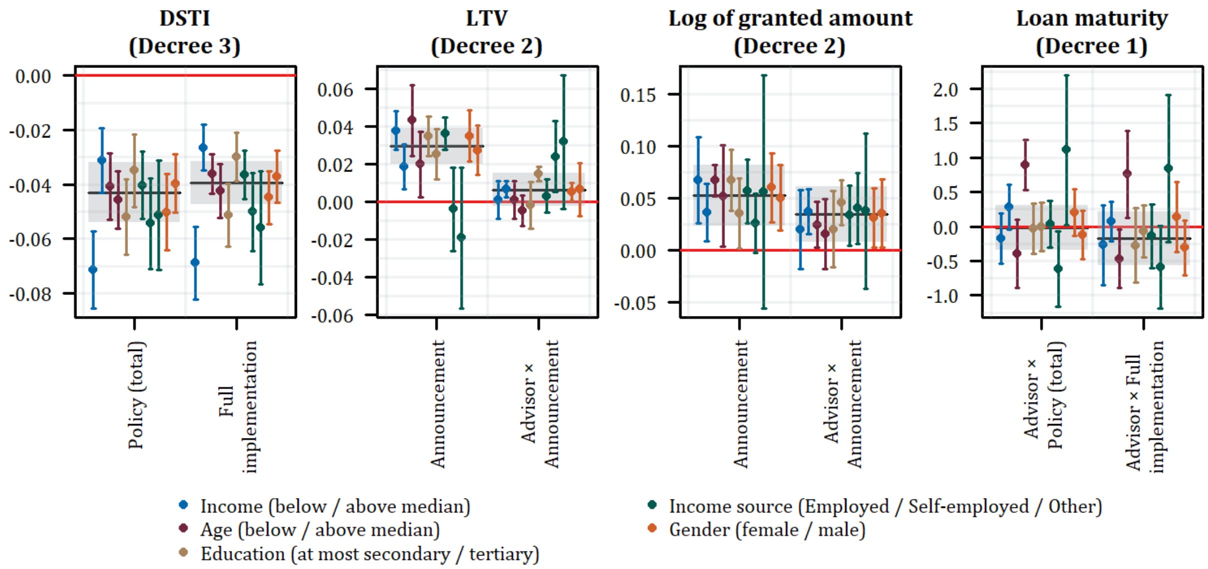
Figure 12: Effect of financial advisors on different borrower groups



Notes: The figure shows the average effect (with 95% confidence interval) of advisors on selected loan outcomes using different subsamples of borrowers based on their characteristics. The effects are estimated using equation (1) with the model specification described in Table 4. The red line indicates the zero effect threshold. The black solid line with gray area represents the average effect with 95 % confidence intervals obtained from the baseline estimates. The results are obtained using the full estimation sample period 2013Q1-2022Q4.

Regarding policy impacts, Figure 13 shows that the DSTI tightening under Decree 3 disproportionately affected lower-income, less-educated, self-employed borrowers and borrowers with other source of income, reflecting a regressive policy impact. Additionally, significant frontloading was observed before Decree 2 implementation, particularly among younger, less-educated, and lower-income borrowers, suggesting they might have tried to secure loans before stricter regulations took effect. Advisors amplified this behavior, mainly among higher-income, more-educated and self-employed borrowers. Furthermore, advisors were also linked to extending loan maturities post-Decree 1, particularly for older borrowers, as a response to regulatory constraints. We provide extended results for all estimated effects in Appendix F.

Figure 13: Selected effects of policy on different borrower groups

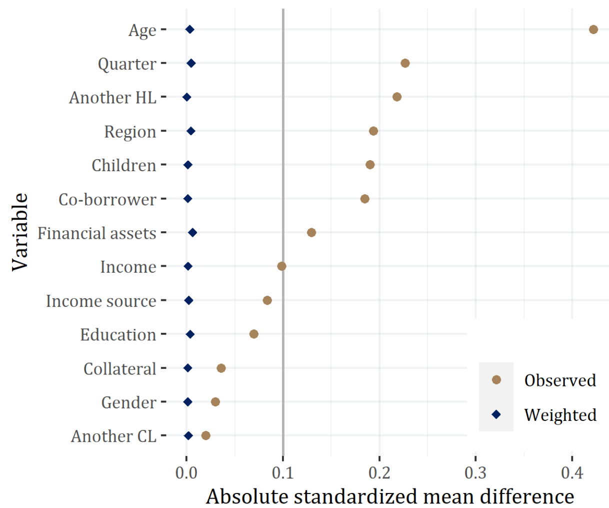


Notes: The figure shows the average effect (with 95% confidence interval) of policies on selected loan outcomes using different subsamples of borrowers based on their characteristics. The effects are estimated using equation (1) with the model specifications described in Table 5 and Table 6. The red line indicates the zero effect threshold. The black solid line with gray area represents the average effect with 95 % confidence intervals obtained from the baseline estimates. In the case of Decree 1, we consider the full implementation of the DSTI tightening. The results are obtained using the sample of loans granted from 2 quarters before the announcement to loans granted 2 quarters after the full implementation of the respective policy.

5.5 Heterogeneity analysis and robustness checks

As a crucial robustness check, we balance the covariates of mediated and non-mediated loans, those before and after the policy announcement, as well as those granted during four different stages of policy implementation separately. Figure 14 demonstrates the effectiveness of our preferred method, the inverse probability of treatment weighting based on propensity scores estimated by a machine learning generalized boosted models approach, in achieving covariate balance between mediated and non-mediated loans for the full estimation sample (2013Q1-2022Q4). A similarly successful balance has been achieved between loans granted before and after the announcement of the respective policies as well as among different stages of the implementation of these policies (see Appendix B for detailed balance assessment). Following the data balancing procedures, we adopt a doubly robust approach by retaining the same regression models, inclusive of all controls as detailed in subsection 4.3.

Figure 14: Effectiveness in achieving covariate balance between mediated and non-mediated loans



Notes: Weights are obtained by inverse probability of treatment weighting using the propensity score estimated with the generalized boosted models. In case of multinomial variables we plot a generalized form of the standardized mean difference metric proposed by Yang and Dalton (2012).

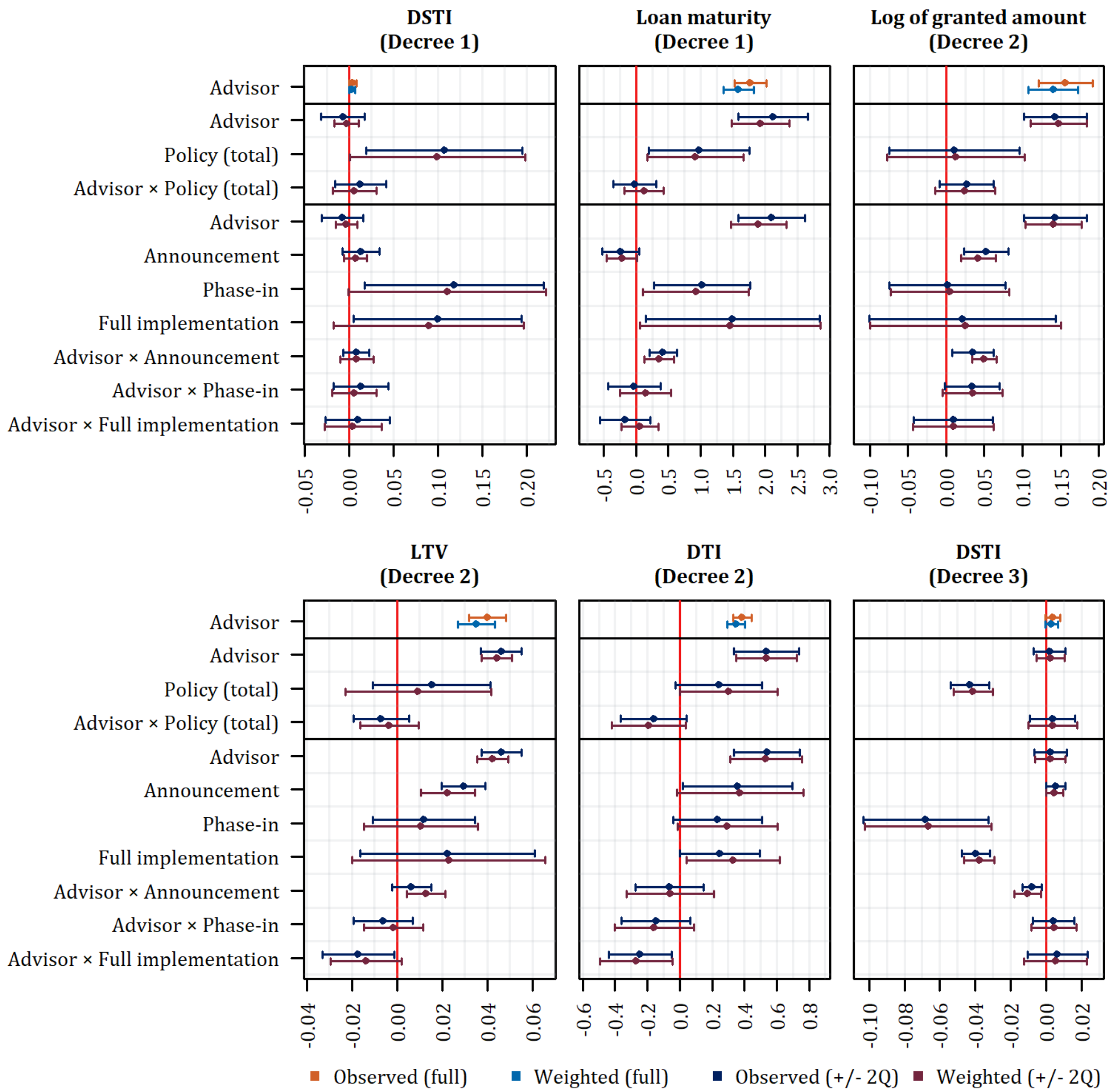
Figure 15 shows the estimated coefficients from section 5.1 side by side with coefficients estimated using doubly robust approaches. Each of the six subfigures includes three specifications of regression model (1), separated by black solid line. The upper specification presents the coefficients of a loan being mediated through a financial advisor, without considering the effect of the policy-relevant variables. In this scenario, the weights are based on balancing covariates between mediated and non-mediated loans. The middle specification includes the overall effect of policy (i.e., the total effect of the policy starting with the announcement) as well as the interaction between financial advisors and the policy. The weights are based on balancing the covariates of borrowers before and after the announcement of respective policies. The bottom specification splits the overall effect of policy into separate effects of the policy during different stages of the implementation (announcement period, phase-in period, full implementation period). In this last specification, the weights are based on balancing the covariates of borrowers among all stages of implementation of respective policies.

The consistency of the estimated coefficients largely confirms our main findings. The effect of advisors exhibit high levels of significance and robustness across nearly all outcomes and specifications. We can thus interpret this consistent evidence, indicating that loans mediated through financial advisors are associated with higher granted amounts, longer maturities, and elevated risk measures (mainly LTV and DTI), as highly robust. This analysis also confirms that the DSTI is comparable between mediated and non-mediated loans.

On average, DSTI and loan maturity continued in mild increase even after the implementation of Decree 1. A significant impact of the Decree 2 policy is observed immediately after the announcement, explained by the front-loading of loans with larger volumes and risk measures (LTV and, to a lesser extent, DTI). The front-loading is even more pronounced in the case of mediated loans, but at the same time, these loans received a stronger negative hit after the policy was fully implemented. Finally, the implementation of the Decree 3 policy had a significant negative impact on the average DSTI of loans, and indirectly on average DTI and LTV as well (see Appendix E), with no significant difference between mediated and non mediated loans. However, it should be emphasized that the quantile regressions reveal diverse distributional effects that are hidden behind these simple average effects.

Appendix E provides doubly robust estimates for wider samples (+/− 1Y), including additional results for all loan outcomes and policies considered. Estimates using wider samples are robust to selection bias as well. In several cases, however, the policy-related coefficients are notably different from the baseline results. Nonetheless, these differences are to a large extent driven by the interference of different policies when using wider samples.

Figure 15: Effects of financial advisors vis-a-vis policy announcement on loan outcomes with doubly robust approach



Notes: The figure shows the average effect (with 95% confidence interval) of advisors and policies on selected loan outcomes using different samples and specifications of equation (1). The specifications are separated by a horizontal black solid line. The top, middle and bottom specifications refer to specifications described in Tables 4, 5 and 6 respectively. Weighted samples are obtained by inverse probability of treatment weighting using the propensity score estimated with the generalized boosted models. In the top specification, the sample is balanced between mediated and non-mediated loans. In the middle specification, the sample is balanced between loans granted before and after the policy announcement. In the bottom specification, the sample is balanced among stages of policy implementation (pre-announcement period, announcement period, phase-in period, full implementation period). In the case of Decree 1, we consider the full implementation of the DSTI tightening for DSTI and loan maturity. The full estimation sample period is 2013Q1-2022Q4. +/− 2Q denotes subsamples of loans granted from 2 quarters before the announcement to 2 quarters after the full implementation of the policy. The pre-announcement period is the reference category for the policy-related variables. The red line indicates the zero effect threshold.

6 Conclusions

Our study offers valuable insights into the complex dynamics of mortgage lending and the effectiveness of macroprudential policies, focusing on a market characterized by active implementation of borrower-based measures and a significant presence of financial advisors.

We find robust evidence that mortgages mediated through financial advisors are granted with higher amounts and elevated risk parameters, such as LTV and DTI. Extending the maturity of these loans enables advisors to facilitate loans with monthly installments comparable to those of non-mediated loans. The impact of borrower-based measure tightening is ambiguous, largely depending on the design, restrictiveness, and structure of the mortgage market.

The tightening of the DSTI, combined with the maturity cap introduced in 2017, occurred during a period of increasing loan maturities and rising riskiness, particularly in terms of higher debt service. The relatively low restrictiveness of the limits (as only a very small proportion of loans had been granted above these thresholds prior to the policy) allowed these parameters to increase further. One possible explanation is that, once informed of the "safe space" defined by the regulator, agents in the mortgage market (borrowers, advisors, and banks) adjusted their preferences towards higher values, which had been less utilized before the policy. In contrast, the more restrictive tightening of the DSTI in 2020, from 80% to 60%, had a strong negative impact not only on DSTI but also indirectly on DTI, and to some extent on LTV and loan amounts granted.

The announcement of LTV and DTI tightening in 2018 led to an immediate increase in the granted amount, LTV, and DTI on average. More interestingly, the significant positive effect persists even at the highest quantiles of the distributions of the considered loan outcomes. This can be explained by the phenomenon of front-loading, i.e., increased demand for loans potentially surpassing the limits once they become binding. Front-loading behavior is even more pronounced for mediated loans, emphasizing the importance of mortgage market specifics in macroprudential policy transmission. At the same time, mediated loans were more negatively affected after the policy was implemented, as they already had higher values of LTV, DTI, and loan volumes prior to the tightening.

Average effects often mask significantly different reactions across various parts of the loan distribution. We document a positive impact of the LTV and DTI tightening implemented in 2018 in the lower quantiles of the distributions for LTV, DTI, and granted amounts, and, in contrast, a negative impact in the upper quantiles. This suggests that while the policy successfully dampened the production of the riskiest loans, as intended, it indirectly led to the production of loans with parameters still below but closer to the established limits.

Effective policy design requires a granular analysis of country-specific loan distributions and processes. Our study emphasizes the importance of leveraging detailed microdata to tailor policies accurately and avoid unintended consequences. Future research may focus on longitudinal studies to explore post-granting dynamics and the longer-term impacts of macroprudential interventions on mortgage markets.

In summary, our findings enhance the understanding of the interplay between policy interventions, borrower behavior, and market dynamics in the mortgage lending sector. By highlighting these complexities, our research provides valuable insights for policymakers and stakeholders in their efforts to promote financial stability and sustainable lending practices.

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Appendix

A Description of variables

Table A.1: Description of variables used in empirical analysis

Outcome variables	Granted amount	The granted amount of the loan (in Eur)
	Maturity	Maturity of the loan (in years)
	LTV	Loan-to-value ratio
	DTI	Debt-to-income ratio
	DSTI	Debt service-to-income ratio
Treatment variables	Advisor	=1 if loan was mediated via advisor
	BBM	The stage of the policy implementation (period before announcement, period between announcement and implementation, phase-in period, period after full implementation)
Loan parameters	Interest rate	Loan interest rate (in % p.a.)
	Collateral	Collateral value (in Eur)
Borrowers' socio-economic characteristics	Income	Total income of all borrowers (in Eur)
	Financial assets	Sum of borrowers' deposits at the granting bank (in Eur)
	Another HL	=1 if at least one of the borrowers has another housing loan
	Another CL	=1 if at least one of the borrowers has another consumer loan
	Children	Number of borrowers' children (0,1,2,3,4 and more)
	Co-borrower	=1 if there is a co-borrower in the loan-contract
	Income source	Income source of the highest earning borrower (employed, self-employed, other)
	Education	Highest achieved education among borrowers (university education, at most secondary education)
	Age	Average age of borrowers (in years)
	Gender	Gender of the highest earning borrower
Market characteristics	Bank	The origin of the loan contract (12 banks)
	Region	Region of the collateral (8 regions)
	Quarter	2013Q1–2022Q4 (40 quarters)
	Month of year	January–December (12 months)

Notes: All values are measured at the time the loan is granted.

B GBM tuning and balance assessment

GBM tuning

The model has several parameters (called hyperparameters), that determine the performance of the model. In order to select the best model we perform grid search across the model's main hyperparameters[21]. Empirical papers indicate, that using balance metrics for model evaluation rather than using fit accuracy yields better results of covariate balance (see, e.g., Griffin et al., 2017). Therefore, we pick the model with the lowest maximum of absolute standardized mean differences across all covariates (and, in case of multiple treatment setup, also across all pairwise group comparisons) achieved in subsequently weighted sample.

Balance assessment

To assess whether the populations are sufficiently balanced without major changes in the sample properties, we use a combination of the distribution overlap plots, various balance metrics and the effective sample size measure.

It is difficult to visually assess the overlap of a joint distribution of covariates in high dimensions. Therefore, we follow the common practice and compare the empirical distributions of our target populations in terms of estimated propensity scores to assess the similarity between these populations before weighting.

The balance achieved after weighting is assessed using various balance metrics. As a baseline, we use the most common metric – (absolute) standardized mean difference (SMD) – which assesses the balance of each covariate in terms of their means.[22] In addition to computing SMDs only on the covariates themselves, we also compute them on their squares (in case of continuous variables) and pairwise interactions to introduce a multivariate dimension into the assessment. As a supplement to SMD we use another commonly used metric – the Kolmogorov-Smirnov statistic (KS) – which represents the maximum difference of covariates' empirical cumulative distribution functions be-

[21] Number of trees, depth of trees, minimum number of observations in each node, learning rate, the fraction of the data used in growing the trees in subsequent iterations.

[22] In case of categorical variable the balance in each level.

tween treatment groups[23] To not to miss any potentially important information about the resutling balance, we compute both mean and maximum SMD and KS across all covariates.[24] The most commonly used thresholds in the literature are 0.1 for SMD and 0.05 for KS. However, these limits are considered as a rule of thumb rather than being supported by any theory. In general, the closer to the zero, the better.

Unfortunately, a balance achieved with IPTW comes at a cost of increased sampling variance. We measure this "cost of balance" using effective sample size (ESS):

$$ESS_d = \frac{\left(\sum_{i=1}^{N} D_i[d]w_i\right)^2}{\sum_{i=1}^{N} D_i[d]w_i^2}, \tag{5}$$

for treatment d, where weights w_i are obtained from equation (4). ESS in relation to number of observations (N) gives an information about the disparity of estimated weights in the treatment group, leading to potential loss in precision and statistical power. Low ESS relative to N indicates that small number of observations received very large weights.

Figure B.1 compares propensity score distributions of mediated and non-mediated loans, Figures B.2 and B.3 compares propensity score distribution of loans granted before and after the policy announcement and Figures B.4 and B.5 compares propensity score distributions of loans granted in different stages of the policy implementation (for each stage separately). All distributions seem to overlap sufficiently, only one case could be the subject of concern – loans granted during the announcement period of the Decree 2 (+/− 2Q sample). It seems that these loans are substantially different from loans granted in other stages of policy implementation (see B.4, panel c).

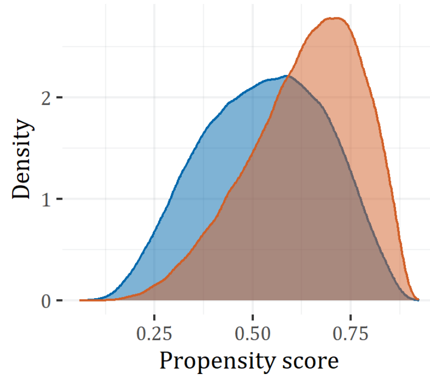
Tables B.1 and B.2 summarize the balance and the cost of balance achieved with IPTW in each combination of sample and treatment variable considered in our empirical analysis. The balance of the covariates between treatment groups improved singnificantly after weighting, both in terms of their mean differences and the distribution distances. The values of all balance metrics fell well below the acceptable thresholds. Generally, GBM captures also higher order relationships among variables, resulting in a very small SMD across squares and interactions as well as very low

[23] For categorical variables, the KS statistic is reported as the difference in proportions for each level of the variable.
[24] We use R software package *cobalt* (Greifer, 2024).

distributional differences of covariates.

As expected from the distribution plots, the fact that loans granted during the announcement period of the Decree 2 (+/− 2Q sample) are very different from loans granted at other stages of the policy implementation (in terms of pre-treatment covariates) led to the ESS being almost halved compared to N (originally N = 14,774, while ESS = 9,715.62). We observe a similar hit to the ESS for loans granted before the announcement of the DSTI tightening of Decree 1 (for both single and multiple treatment setup and for both +/− 2Q and +/− 1Y samples). Apart from this, we do not observe any notable differences between N and ESS that could indicate the possible presence of extreme weights.

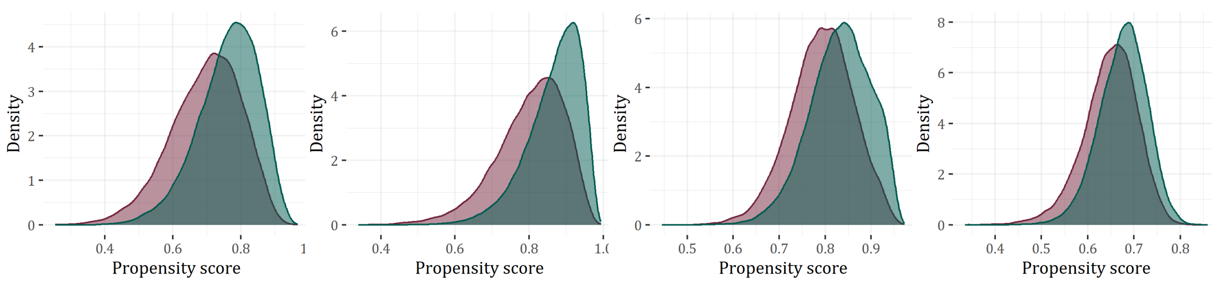
Figure B.1: Empirical propensity score distributions (mediated vs. non-mediated loans)



Notes: The figure shows the distribution of the probability of a loan being mediated. The orange color is assigned to mediated loans, the blue color is assigned to non-mediated loans. The propensity score is estimated using a generalized boosted models.

Figure B.2: Empirical propensity score distributions in single treatment setup (+/− 2Q)

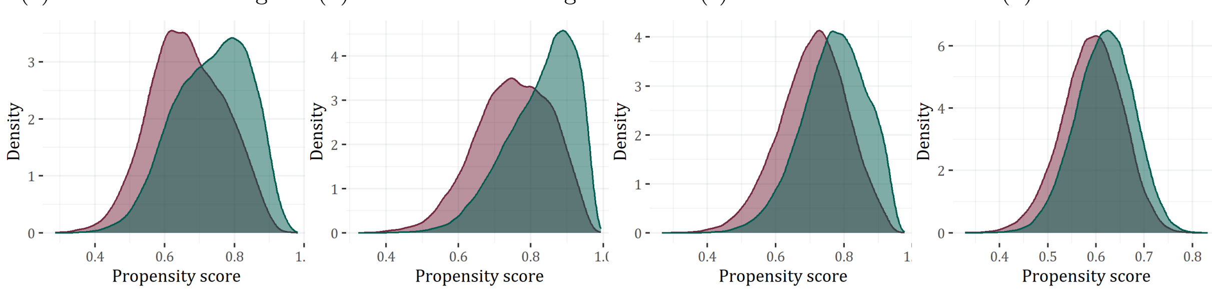
(a) Decree 1 – LTV tight. (b) Decree 1 – DSTI tight. (c) Decree 2 (d) Decree 3



Notes: The figure shows the distribution of the probability of a loan being granted after the policy announcement. The green color is assigned to loans granted after the policy announcemnt, the maroon color is assigned to loans granted before the policy announcement. The propensity score is estimated using a generalized boosted models. +/− 2Q denotes subsample of loans granted from 2 quarters before the announcement to 2 quarters after the full implementation of the policy. Tight. = tightening.

Figure B.3: Empirical propensity score distributions in single treatment setup (+/− 1Y)

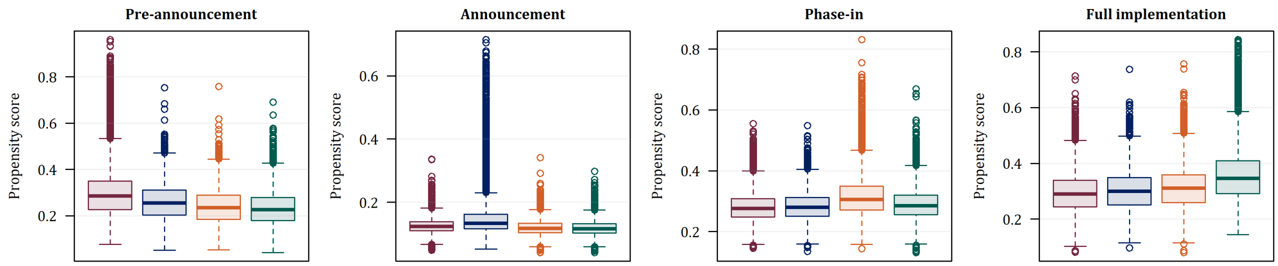
(a) Decree 1 – LTV tight. (b) Decree 1 – DSTI tight. (c) Decree 2 (d) Decree 3



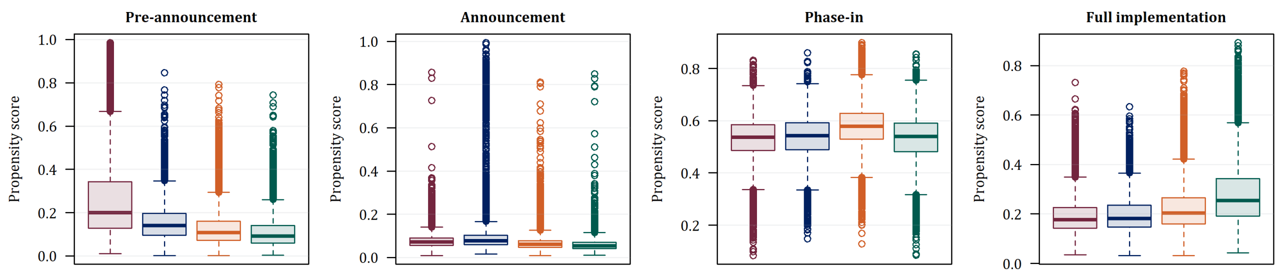
Notes: The figure shows the distribution of the probability of a loan being granted after the policy announcement. The green color is assigned to loans granted after the policy announcemnt, the maroon color is assigned to loans granted before the policy announcement. The propensity score is estimated using a generalized boosted models. +/− 1Y denotes subsample of loans granted from 1 year before the announcement to 1 year after the full implementation of the policy. Tight. = tightening.

Figure B.4: Empirical propensity score distributions in multiple treatment setup (+/− 2Q)

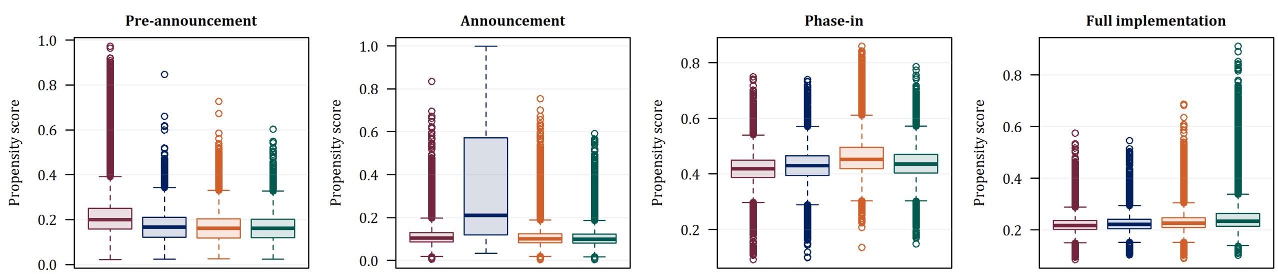
(a) Decree 1 – LTV tightening



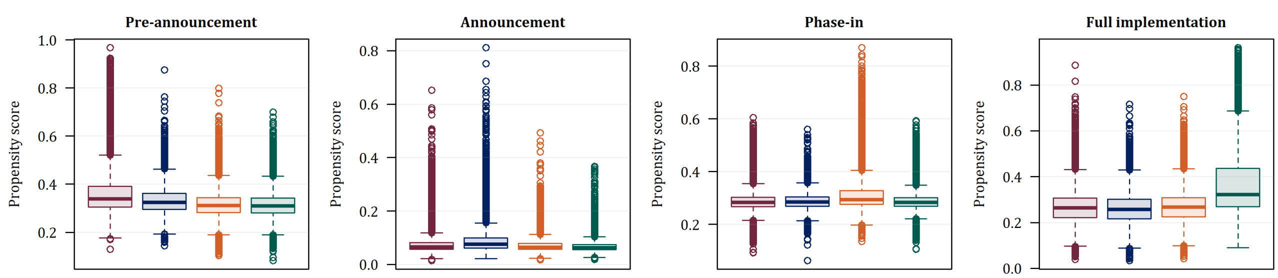
(b) Decree 1 – DSTI tightening



(c) Decree 2



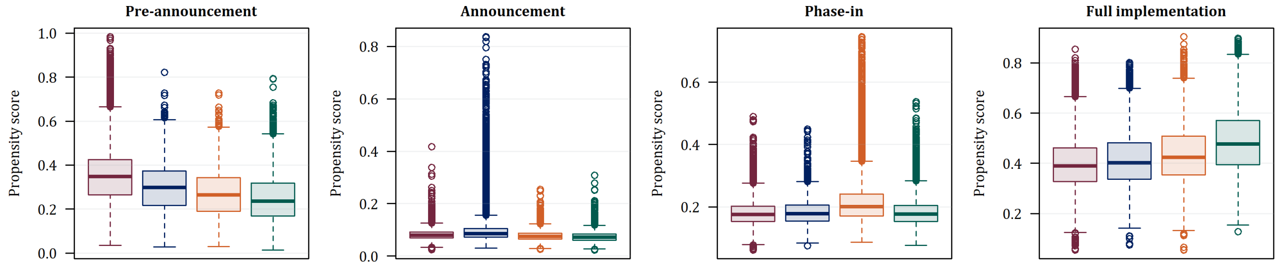
(d) Decree 3



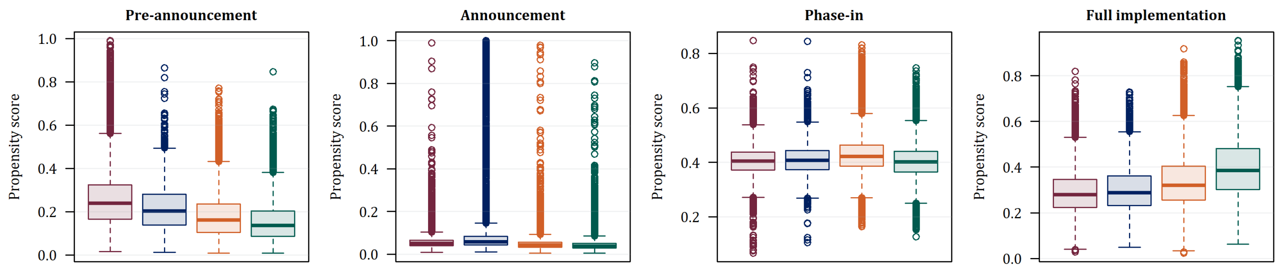
Notes: The figure shows the distribution of the probability of a loan being granted at different stages of policy implementation (panels from left to right refer to the probability of a loan being granted before the policy announcement, between the announcement and implementation of the policy, during the phase-in of the policy and after the full implementation of the policy respectively). The maroon color is assigned to loans granted before the policy announcement, the blue color is assigned to loans granted after the announcement and before the implementation of the policy, the orange color is assigned to loans granted during the phase-in period of the policy and the green color is assigned to loans granted after the full implementation of the policy. The multiple treatment propensity scores are estimated using a generalized boosted models. +/− 2Q denotes subsample of loans granted from 2 quarters before the announcement to 2 quarters after the full implementation of the policy. Tight. = tightening.

Figure B.5: Empirical propensity score distributions in multiple treatment setup (+/− 1Y)

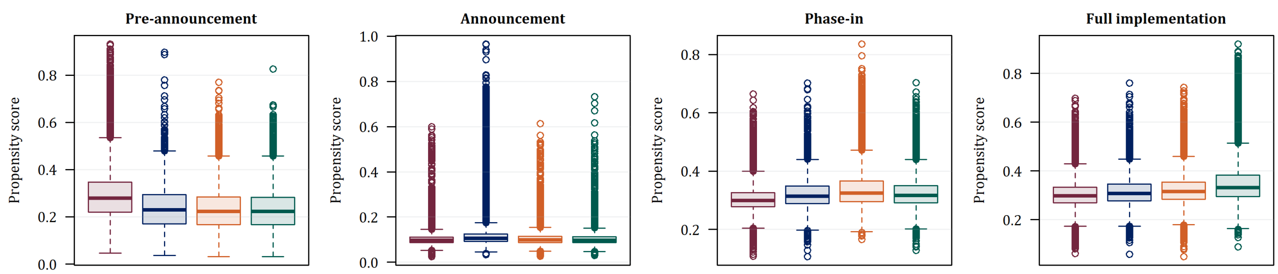
(a) Decree 1 – LTV tightening



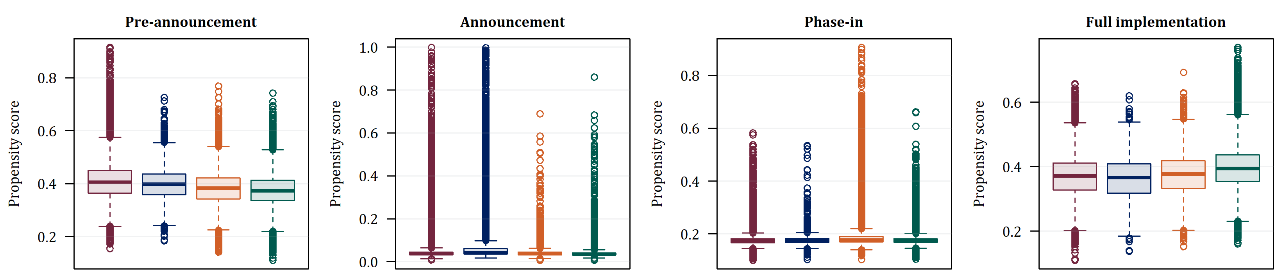
(b) Decree 1 – DSTI tightening



(c) Decree 2



(d) Decree 3



Notes: The figure shows the distribution of the probability of a loan being granted at different stages of policy implementation (panels from left to right refer to the probability of a loan being granted before the policy announcement, between the announcement and implementation of the policy, during the phase-in of the policy and after the full implementation of the policy respectively). The maroon color is assigned to loans granted before the policy announcement, the blue color is assigned to loans granted after the announcement and before the implementation of the policy, the orange color is assigned to loans granted during the phase-in period of the policy and the green color is assigned to loans granted after the full implementation of the policy. The multiple treatment propensity scores are estimated using a generalized boosted models. +/− 1Y denotes subsample of loans granted from 1 year before the announcement to 1 year after the full implementation of the policy. Tight. = tightening.

Table B.1: Balance across all covariates and cost of balance in single treatment setup

Sample	Adjustment	mean SMD	max SMD	max SMD (.^{2}, ×)	mean KS	max KS	N or ESS (Non-mediated)	N or ESS (Mediated)
Full sample	Observed	0.0538	0.4221	0.4221	0.0185	0.1820	145,710	211,374
	Weighted	0.0010	0.0061	0.0292	0.0003	0.0026	123,238.00	192,981.90
							N | ESS (Before ann.)	N | ESS (After ann.)
Decree 1 – LTV tightening (+/− 2Q)	Observed	0.0480	0.2350	0.2650	0.0212	0.0912	13,786	40,210
	Weighted	0.0041	0.0361	0.0616	0.0017	0.0082	11,853.88	39,508.89
Decree 1 – LTV tightening (+/− 1Y)	Observed	0.0521	0.3091	0.3600	0.0230	0.1203	24,280	60,577
	Weighted	0.0040	0.0246	0.0554	0.0018	0.0066	20,244.94	59,034.25
Decree 1 – DSTI tightening (+/− 2Q)	Observed	0.0586	0.2960	0.3663	0.0270	0.1178	13,786	82,721
	Weighted	0.0054	0.0313	0.0630	0.0023	0.0111	9,984.33	81,969.83
Decree 1 – DSTI tightening (+/− 1Y)	Observed	0.0554	0.3466	0.4295	0.0259	0.1367	24,280	105,851
	Weighted	0.0057	0.0317	0.0652	0.0024	0.0101	17,169.91	104,080.80
Decree 2 (+/− 2Q)	Observed	0.0393	0.1156	0.1267	0.0209	0.0897	17,807	83,946
	Weighted	0.0051	0.0166	0.0465	0.0024	0.0110	14,827.75	83,339.38
Decree 2 (+/− 1Y)	Observed	0.0372	0.1664	0.2032	0.0202	0.0892	34,075	105,556
	Weighted	0.0048	0.0150	0.0369	0.0020	0.0054	28,041.40	103,642.10
Decree 3 (+/− 2Q)	Observed	0.0180	0.0849	0.1043	0.0090	0.0410	24,399	48,731
	Weighted	0.0013	0.0045	0.0197	0.0008	0.0042	23,748.57	48,416.35
Decree 3 (+/− 1Y)	Observed	0.0231	0.0989	0.1245	0.0117	0.0494	46,692	72,117
	Weighted	0.0010	0.0056	0.0140	0.0006	0.0031	45,541.79	71,373.77

Notes: SMD = (absolute) standardized mean difference; KS = Kolmogorov-Smirnov statistic; .^{2},× = squares and interactions. Pooled standard deviation is used as a denominator in SMD calculation. Weighted samples are obtained by inverse probability of treatment weighting using the propensity score estimated with the generalized boosted models. N = number of observations in the treatment group; ESS = effective sample size of the weighted treatment group. Ann. = announcement of the policy. The full estimation sample period is 2013Q1-2022Q4. +/− 2Q and +/− 1Y denote subsamples of loans granted from 2 quarters and 1 year (respectively) before the announcement to 2 quarters and 1 year (respectively) after the full implementation of the policy.

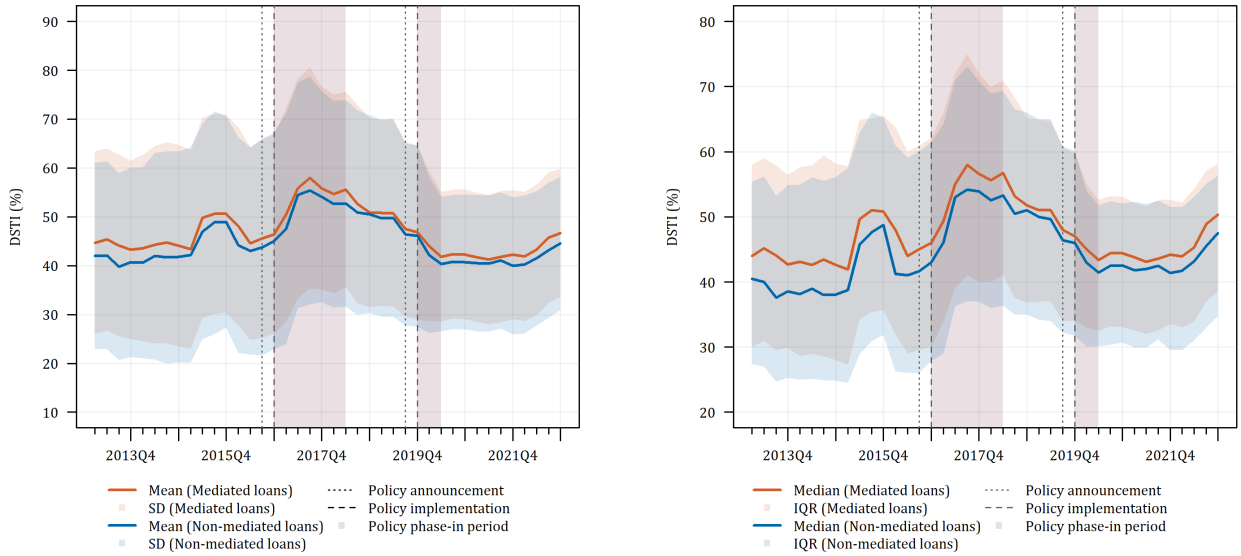
Table B.2: Balance across all covariates and all pairwise group comparisons and cost of balance in multiple treatment setup

Sample	Adjustment	mean SMD	max SMD	max SMD (.^{2}, ×)	mean KS	max KS	N or ESS (Pre-ann.)	N or ESS (Ann.)	N or ESS (Phase-in)	N or ESS (Full impl.)
Decree 1 – LTV tightening (+/− 2Q)	Observed	0.0776	0.2504	0.3174	0.0333	0.1053	13,786	6,767	15,953	17,490
	Weighted	0.0106	0.0201	0.0461	0.0052	0.0158	12,326.89	6,166.34	15,379.51	16,349.38
Decree 1 – LTV tightening (+/− 1Y)	Observed	0.0836	0.3342	0.4363	0.0359	0.1350	24,280	6,767	15,953	37,857
	Weighted	0.0125	0.0276	0.0558	0.0057	0.0144	20,266.63	5,957.53	14,788.04	35,353.10
Decree 1 – DSTI tightening (+/− 2Q)	Observed	0.0862	0.3690	0.4641	0.0412	0.1444	13,786	6,767	53,810	22,144
	Weighted	0.0143	0.0372	0.0725	0.0069	0.0189	8,559.78	5,381.52	52,696.67	18,911.48
Decree 1 – DSTI tightening (+/− 1Y)	Observed	0.0794	0.3866	0.5013	0.0375	0.1585	24,280	6,767	53,810	45,274
	Weighted	0.0108	0.0340	0.0697	0.0057	0.0193	16,983.06	4,993.77	52,607.95	40,126.35
Decree 2 (+/− 2Q)	Observed	0.0621	0.1535	0.1902	0.0294	0.0994	17,807	14,774	45,274	23,898
	Weighted	0.0106	0.0195	0.0549	0.0056	0.0168	14,642.45	9,715.62	44,403.20	22,894.65
Decree 2 (+/− 1Y)	Observed	0.0632	0.2051	0.2220	0.0291	0.0985	34,075	14,774	45,274	45,508
	Weighted	0.0089	0.0206	0.0466	0.0046	0.0118	28,658.16	13,600.98	44,091.89	43,652.05
Decree 3 (+/− 2Q)	Observed	0.0426	0.1470	0.1521	0.0210	0.0832	24,399	5,331	21,610	21,790
	Weighted	0.0080	0.0181	0.0415	0.0044	0.0175	23,316.12	4,682.63	20,942.31	19,533.58
Decree 3 (+/− 1Y)	Observed	0.0461	0.1639	0.1685	0.0225	0.0857	46,692	5,331	21,610	45,176
	Weighted	0.0106	0.0167	0.0428	0.0052	0.0142	45,344.76	4,350.83	20,723.16	43,771.96

Notes: SMD = (absolute) standardized mean difference; KS = Kolmogorov-Smirnov statistic; .^{2},× = squares and interactions. Pooled standard deviation is used as a denominator in SMD calculation. Weighted samples are obtained by inverse probability of treatment weighting using the propensity score estimated with the generalized boosted models. N = number of observations in the treatment group; ESS = effective sample size of the weighted treatment group. Pre-ann. = period before announcement of the policy; Ann. = period between announcement and implementation of the policy; Phase-in = policy phase-in period; Full impl. = period after full implementation of the policy. +/− 2Q and +/− 1Y denote subsamples of loans granted from 2 quarters and 1 year (respectively) before the announcement to 2 quarters and 1 year (respectively) after the full implementation of the policy.

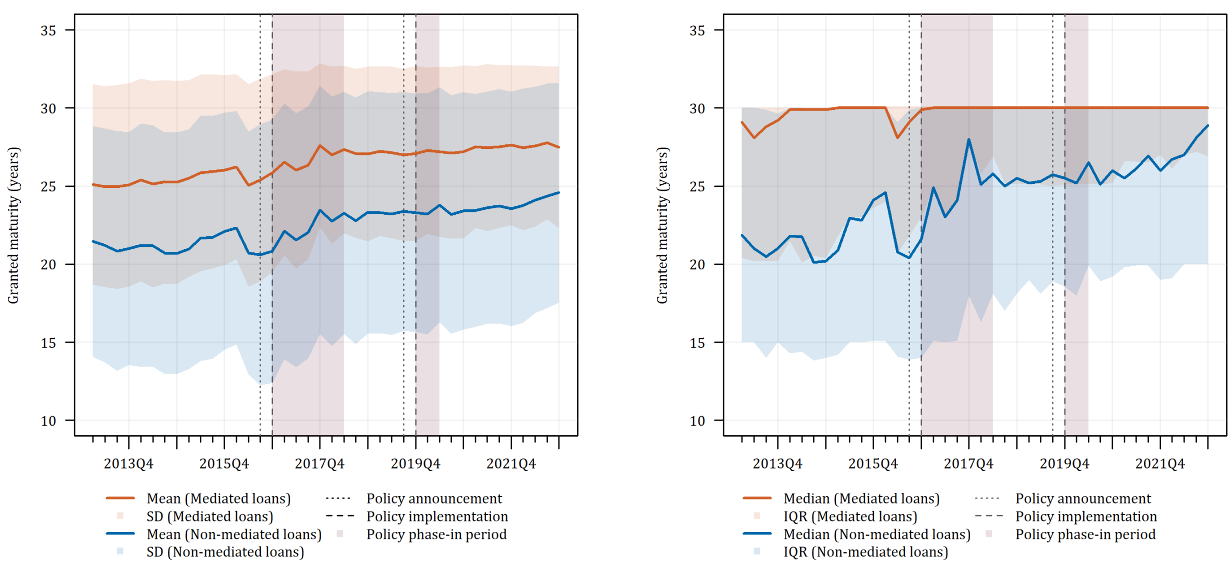
C Averages and medians over time

Figure C.1: Average and median DSTI over time



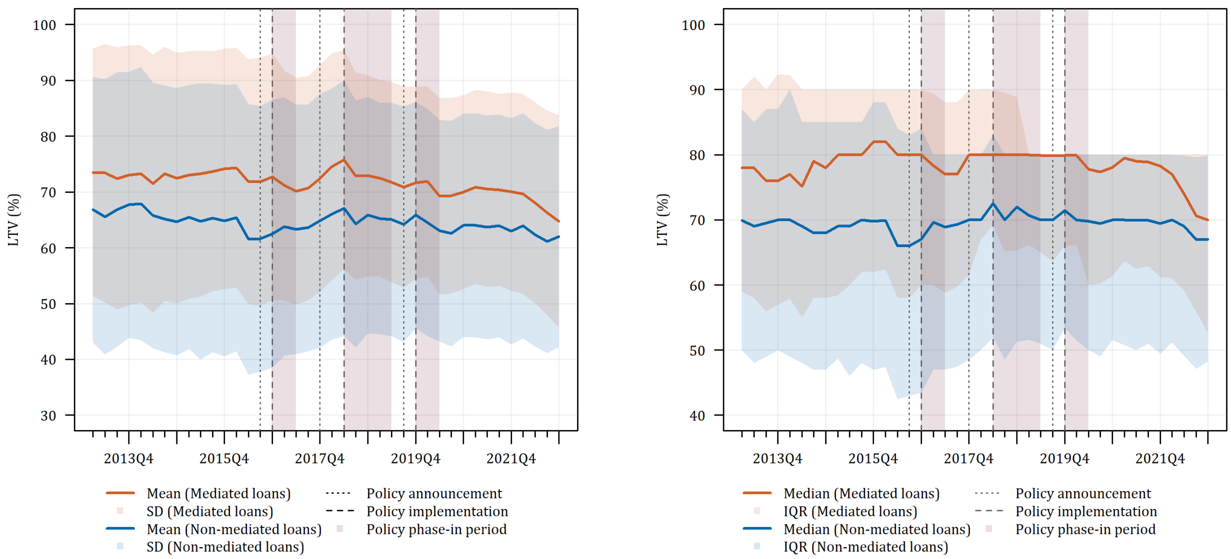
Notes: Policies shown in the figure represents the announcement, implementation and phase-in of DSTI tightening of Decree 1 and Decree 3 respectively.

Figure C.2: Average and median loan maturity over time



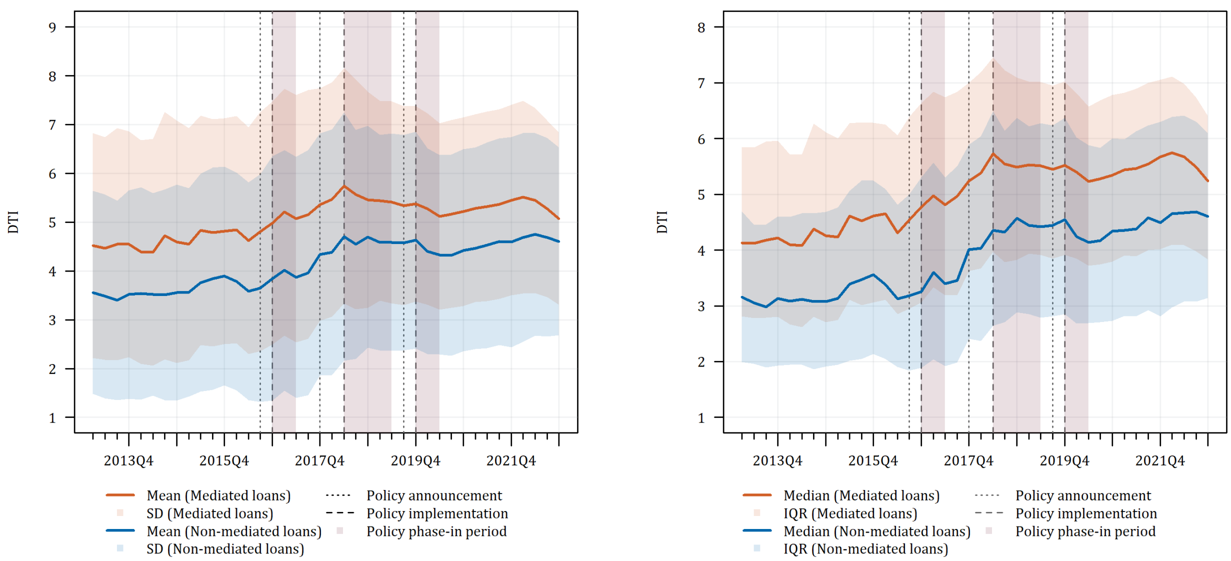
Notes: Policies shown in the figure represents the announcement, implementation and phase-in of DSTI tightening of Decree 1 and Decree 3 respectively.

Figure C.3: Average and median LTV over time



Notes: Policies shown in the figure represents the announcement, implementation and phase-in of Decree 1 (LTV tightening), Decree 2 and Decree 3 respectively.

Figure C.4: Average and median DTI over time



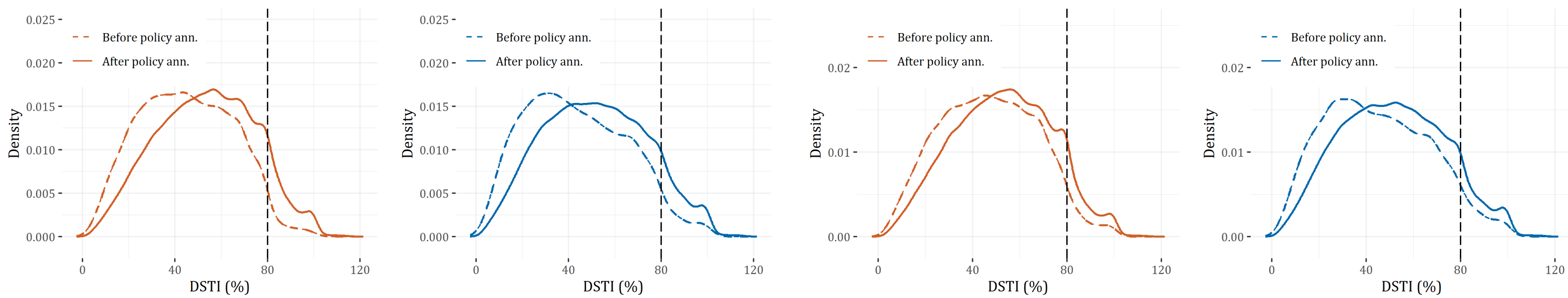
Notes: Policies shown in the figure represents the announcement, implementation and phase-in of Decree 1 (LTV tightening), Decree 2 and Decree 3 respectively.

D Distributions before and after policy announcement

Figure D.1: Distribution of DSTI before vs. after the announcement of Decree 1 policy

(a) +/– 2Q

(b) +/– 1Y

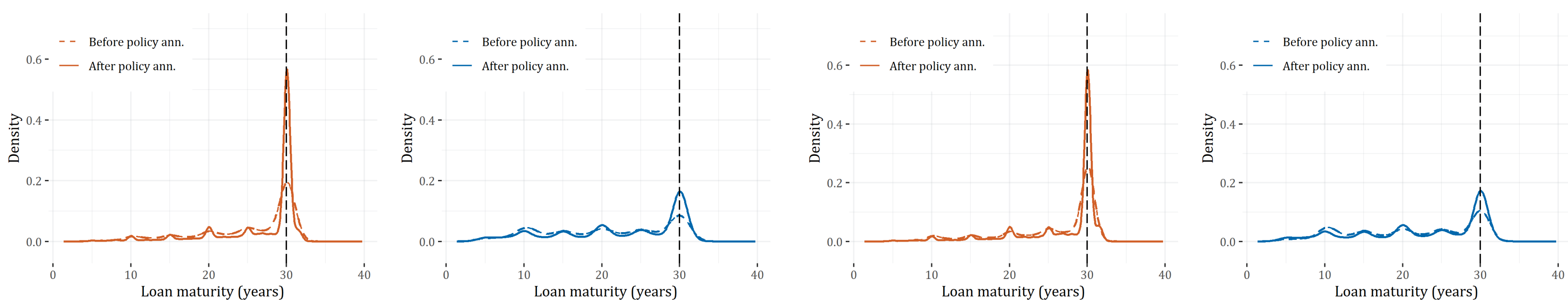


Notes: Red lines represent the distributions of advisor-mediated loans. Blue lines represent the distribution of non-mediated loans. +/– 2Q and +/– 1Y denote subsamples of loans granted from 2 quarters and 1 year (respectively) before the announcement to 2 quarters and 1 year (respectively) after the full implementation of the DSTI tightening. Vertical dashed line marks the policy threshold.

Figure D.2: Distribution of loan maturity before vs. after the announcement of Decree 1 policy

(a) +/– 2Q

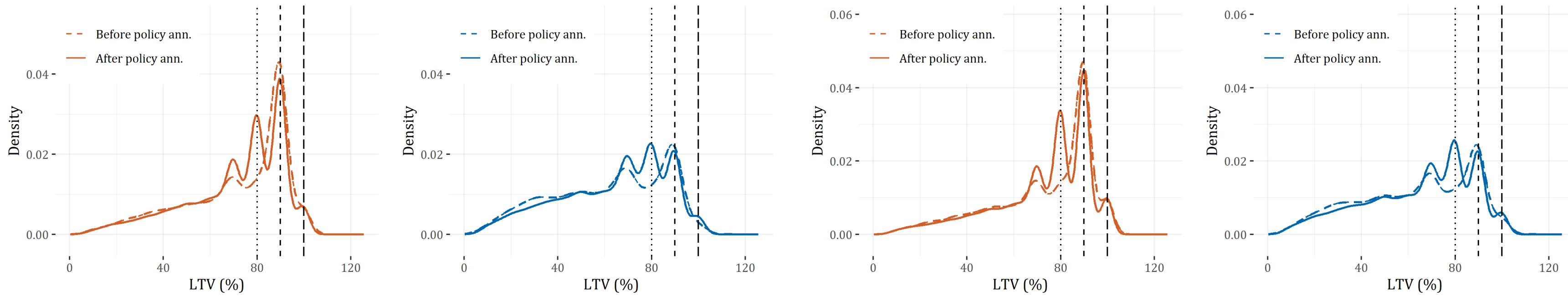
(b) +/– 1Y



Notes: Red lines represent the distributions of advisor-mediated loans. Blue lines represent the distribution of non-mediated loans. +/– 2Q and +/– 1Y denote subsamples of loans granted from 2 quarters and 1 year (respectively) before the announcement to 2 quarters and 1 year (respectively) after the full implementation of the DSTI tightening. Vertical dashed line marks the policy threshold. Vertical dashed line marks the policy threshold.

Figure D.3: Distribution of LTV before vs. after the announcement of Decree 1 policy

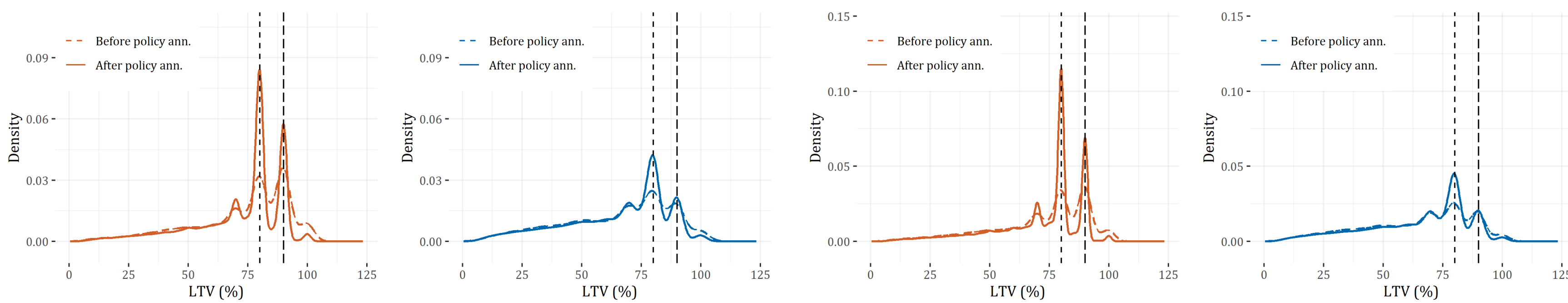
(a) +/− 2Q　　　　(b) +/− 1Y



Notes: Red lines represent the distributions of advisor-mediated loans. Blue lines represent the distribution of non-mediated loans. +/− 2Q and +/− 1Y denote subsamples of loans granted from 2 quarters and 1 year (respectively) before the announcement to 2 quarters and 1 year (respectively) after the full implementation of the LTV tightening. Vertical lines mark the policy thresholds.

Figure D.4: Distribution of LTV before vs. after the announcement of Decree 2 policy

(a) +/− 2Q　　　　(b) +/− 1Y

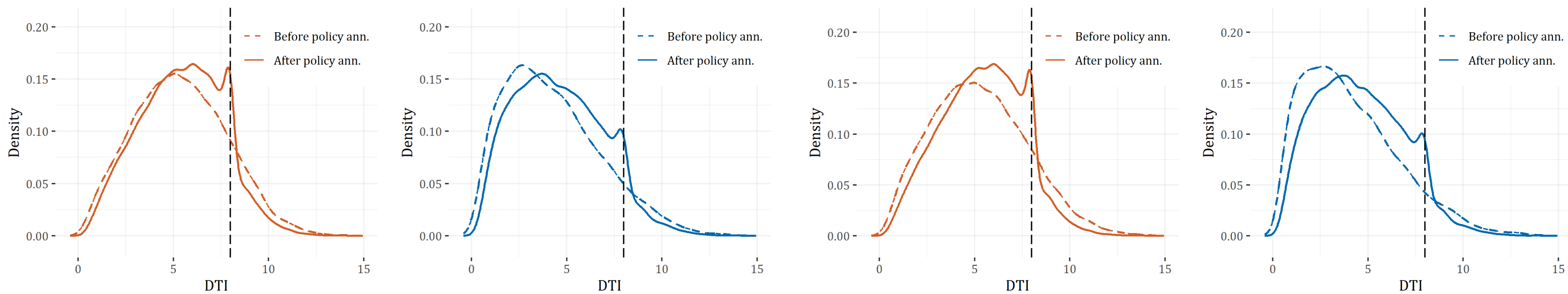


Notes: Red lines represent the distributions of advisor-mediated loans. Blue lines represent the distribution of non-mediated loans. +/− 2Q and +/− 1Y denote subsamples of loans granted from 2 quarters and 1 year (respectively) before the announcement to 2 quarters and 1 year (respectively) after the full implementation of the policy. Vertical lines mark the policy thresholds.

Figure D.5: Distribution of DTI before vs. after the announcement of Decree 2 policy

(a) +/− 2Q

(b) +/− 1Y

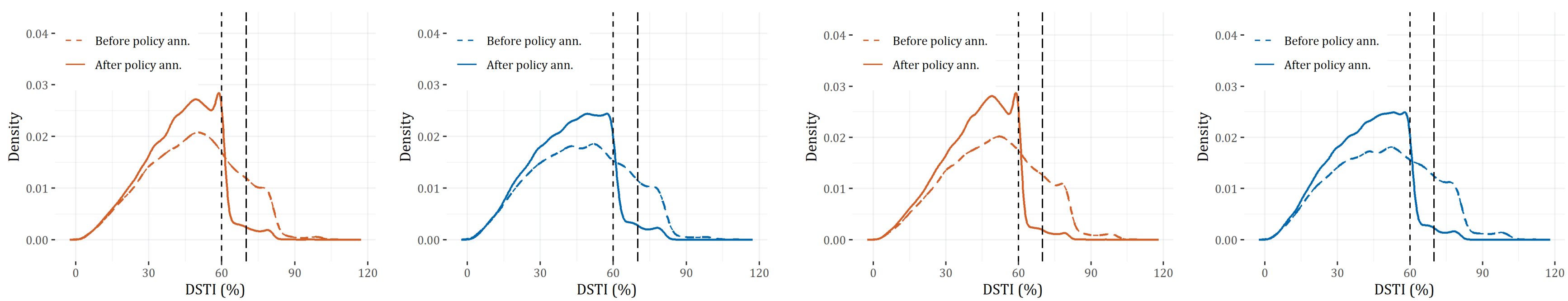


Notes: Red lines represent the distributions of advisor-mediated loans. Blue lines represent the distribution of non-mediated loans. +/− 2Q and +/− 1Y denote subsamples of loans granted from 2 quarters and 1 year (respectively) before the announcement to 2 quarters and 1 year (respectively) after the full implementation of the policy. Vertical dashed line marks the policy threshold.

Figure D.6: Distribution of DSTI before vs. after the announcement of Decree 3 policy

(a) +/− 2Q

(b) +/− 1Y

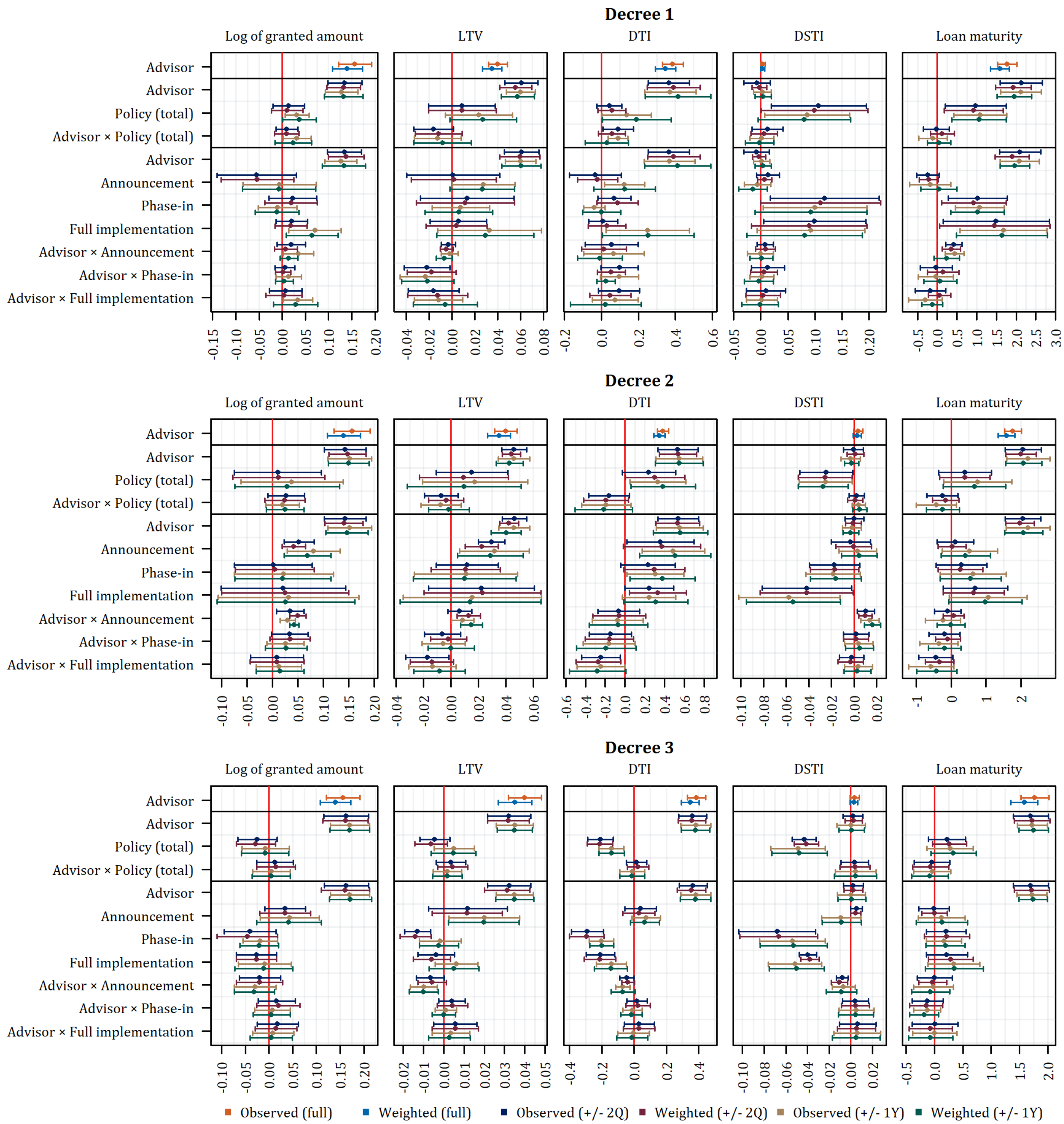


Notes: Red lines represent the distributions of advisor-mediated loans. Blue lines represent the distribution of non-mediated loans. +/− 2Q and +/− 1Y denote subsamples of loans granted from 2 quarters and 1 year (respectively) before the announcement to 2 quarters and 1 year (respectively) after the full implementation of the policy. Vertical lines mark the policy thresholds.

E Additional results of OLS estimates

In this appendix we extend the results from section 5.1 by running regression model using all three specifications, each considered loan outcome and policy. The results are summarized in Figure E.1. Each subfigure include three specifications of regression model (1), separated by black solid line. The upper specification estimates the effect of advisors without considering the effect of the policy-relevant variables. The middle specification considers the overall effect of policy (i.e., the total effect of the policy starting with the announcement). The bottom specification splits the overall effect of policy into separate effects of different stages of the policy implementation (announcement period, phase-in period, full implementation period). Moreover, the figure serves also as a proof of robustness of our results, showing the estimates for weighted samples and also for wider samples (+/− 1 year).

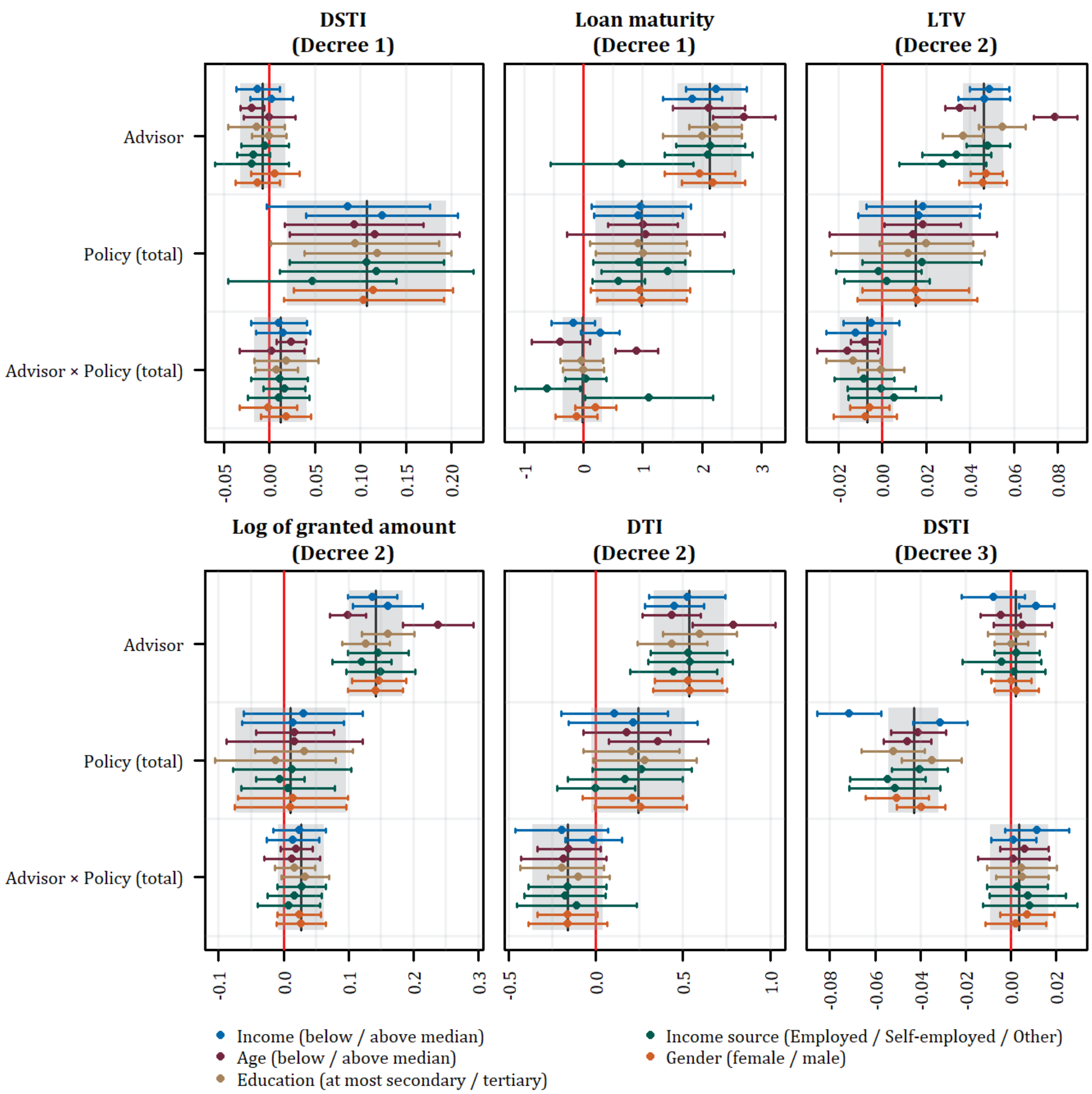
Figure E.1: The effect of advisors and policy on selected loan outcomes – extended results



Notes: The figure shows the average effect (with 95% confidence interval) of advisors and policies on selected loan outcomes using different samples and specifications of equation (1). The specifications are separated by a horizontal black solid line. The top, middle and bottom specifications refer to specifications described in Tables 4, 5 and 6 respectively. Weighted samples are obtained by inverse probability of treatment weighting using the propensity score estimated with the generalized boosted models. In the top specification, the sample is balanced between mediated and non-mediated loans. In the middle specification, the sample is balanced between loans granted before and after the policy announcement. In the bottom specification, the sample is balanced among stages of policy implementation (pre-announcement period, announcement period, phase-in period, full implementation period). In the case of Decree 1, we consider the full implementation of the DSTI tightening for DSTI and loan maturity, while for LTV, granted amount and DTI we consider the full implementation of the LTV tightening. The full estimation sample period is 2013Q1-2022Q4. +/− 2Q and +/− 1Y denote subsamples of loans granted from 2 quarters and 1 year (respectively) before the announcement to 2 quarters and 1 year (respectively) after the full implementation of the policy. The pre-announcement period is the reference category for the policy-related variables.

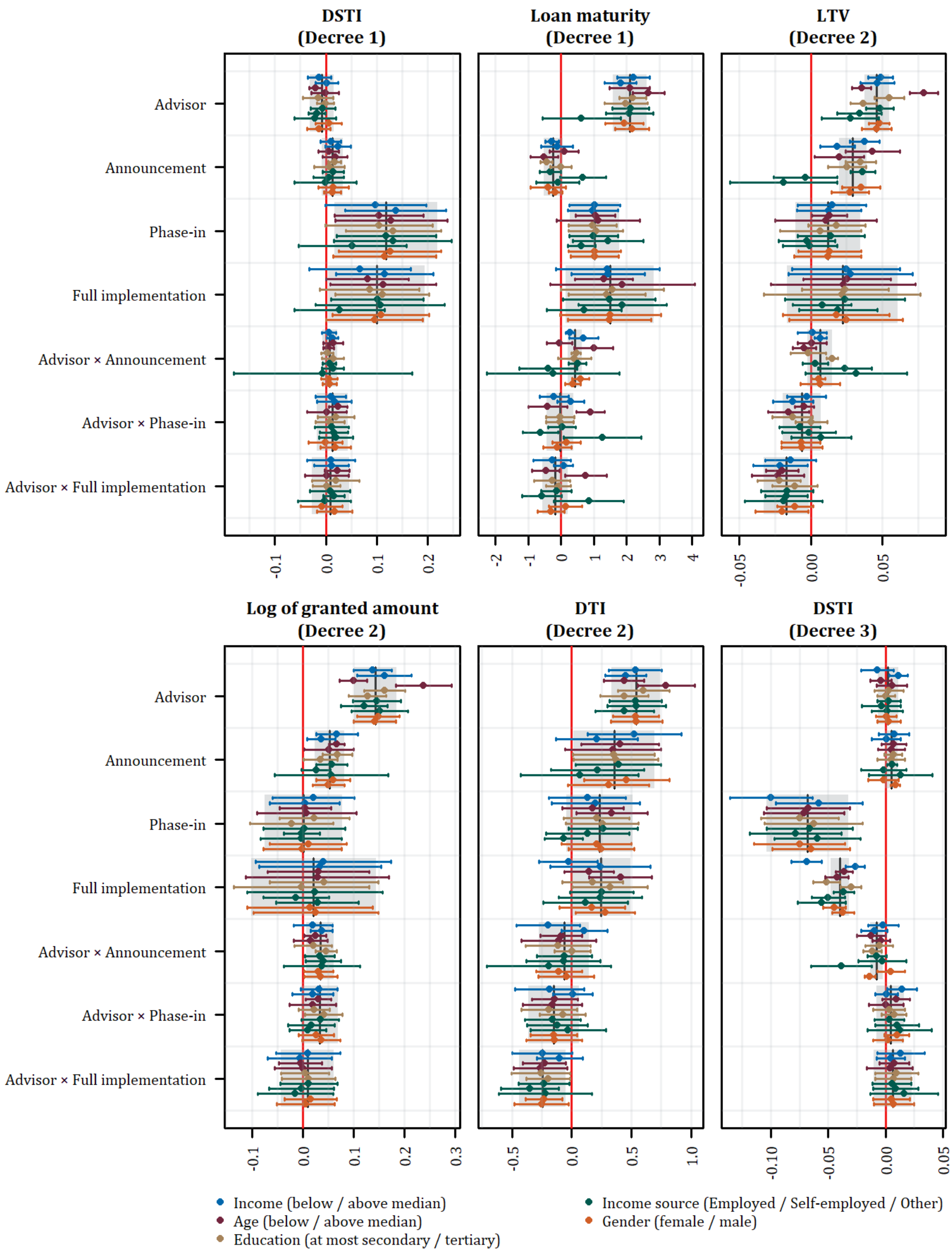
F Extended results of heterogeneity across borrower characteristics

Figure F.1: Effect of financial advisors and policy on different borrower groups



Notes: The figure shows the average effect (with 95% confidence interval) of policies on selected loan outcomes using different subsamples of borrowers based on their characteristics. The effects are estimated using equation (1) with the model specification described in Table 5. The red line indicates the zero effect threshold. The black solid line with gray area represents the average effect with 95 % confidence intervals obtained from the baseline estimates. In the case of Decree 1, we consider the full implementation of the DSTI tightening. The results are obtained using the sample of loans granted from 2 quarters before the announcement to loans granted 2 quarters after the full implementation of the respective policy.

Figure F.2: Effect of financial advisors and policy on different borrower groups

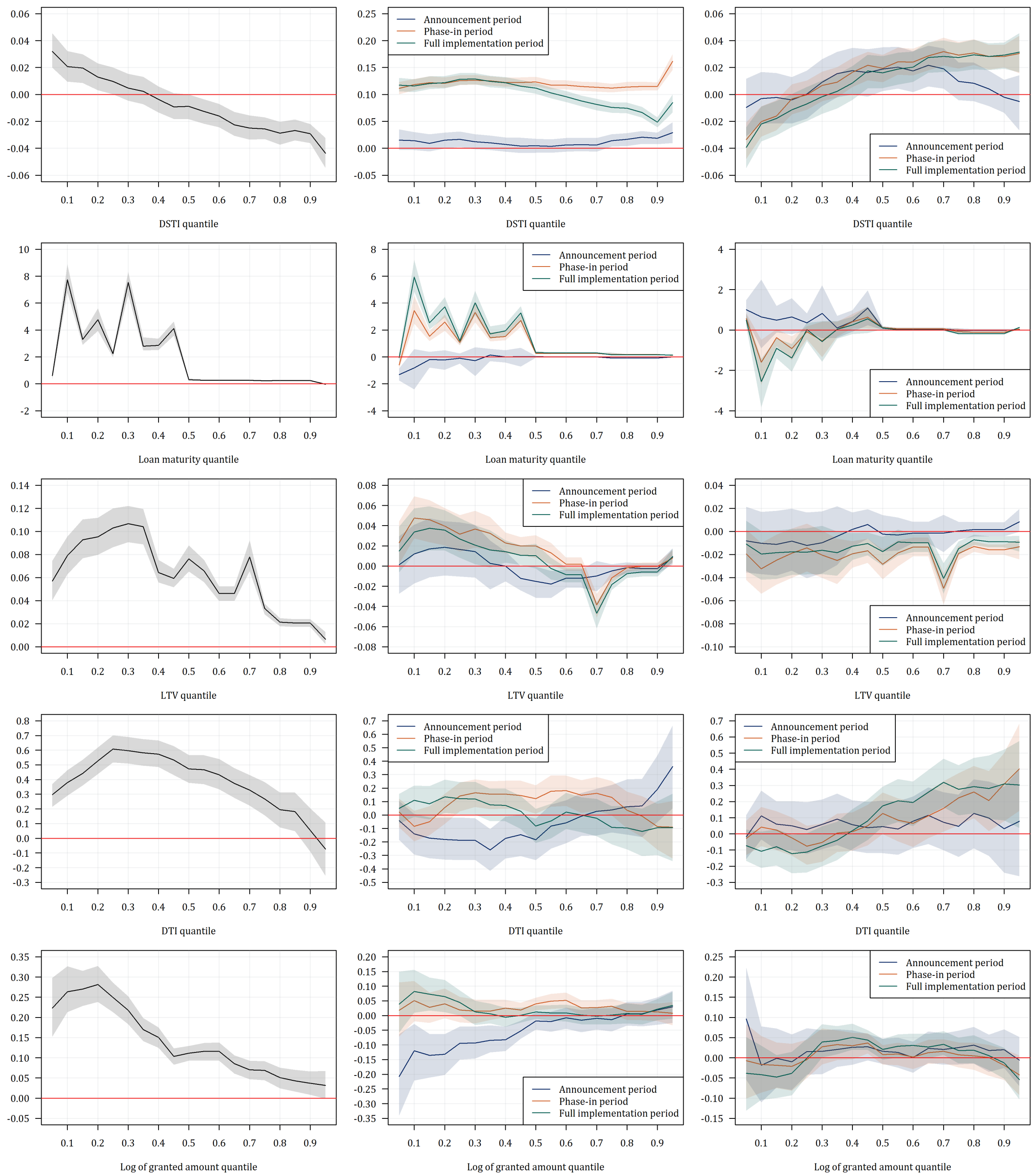


Notes: The figure shows the average effect (with 95% confidence interval) of policies on selected loan outcomes using different subsamples of borrowers based on their characteristics. The effects are estimated using equation (1) with the model specification described in Table 6. The red line indicates the zero effect threshold. The black solid line with gray area represents the average effect with 95 % confidence intervals obtained from the baseline estimates. In the case of Decree 1, we consider the full implementation of the DSTI tightening. The results are obtained using the sample of loans granted from 2 quarters before the announcement to loans granted 2 quarters after the full implementation of the respective policy.

G Additional results from quantile regressions

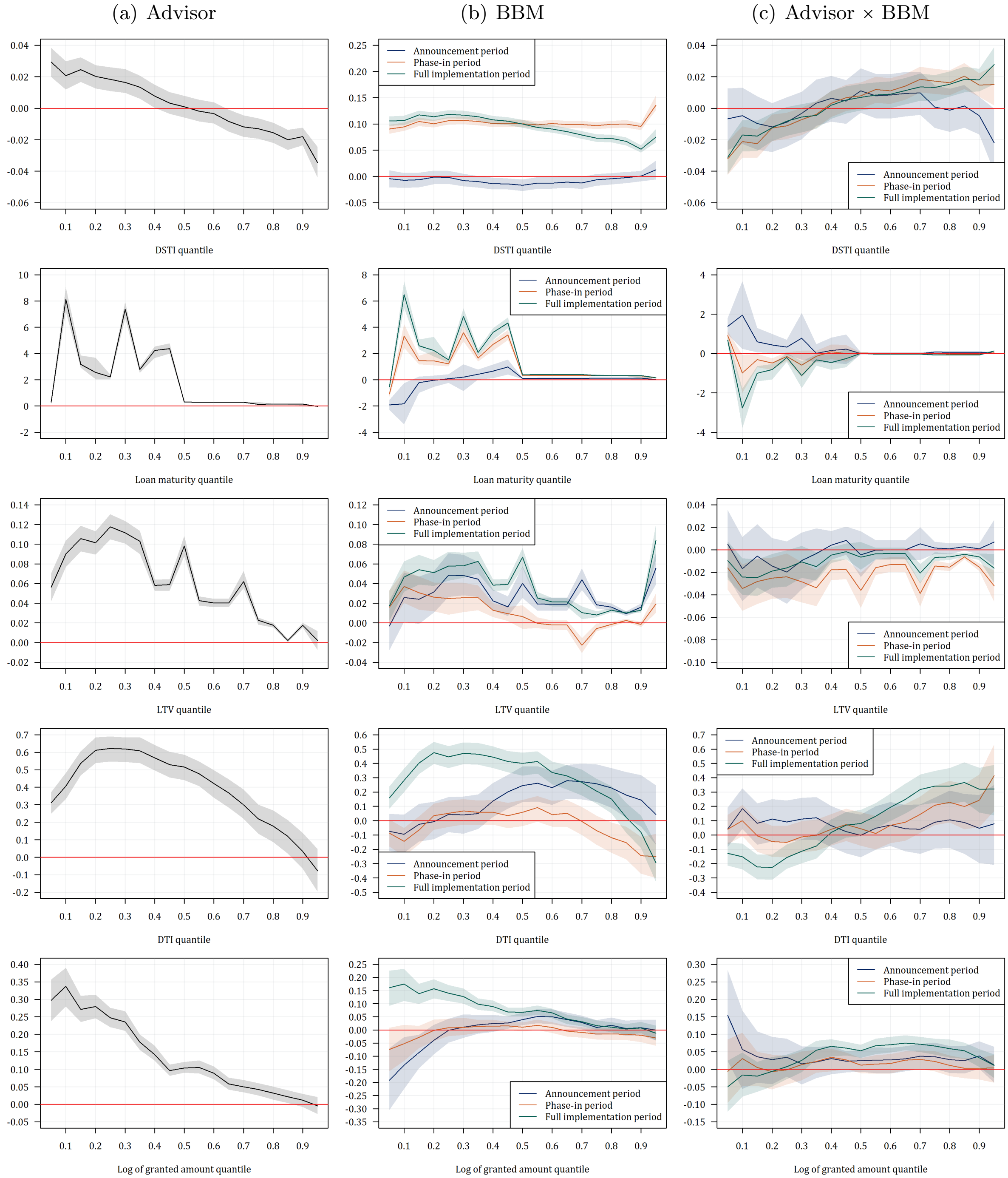
Figure G.1: Quantile effects of Decree 1 policy on selected loan outcomes (+/− 2Q sample)

(a) Advisor (b) BBM (c) Advisor × BBM



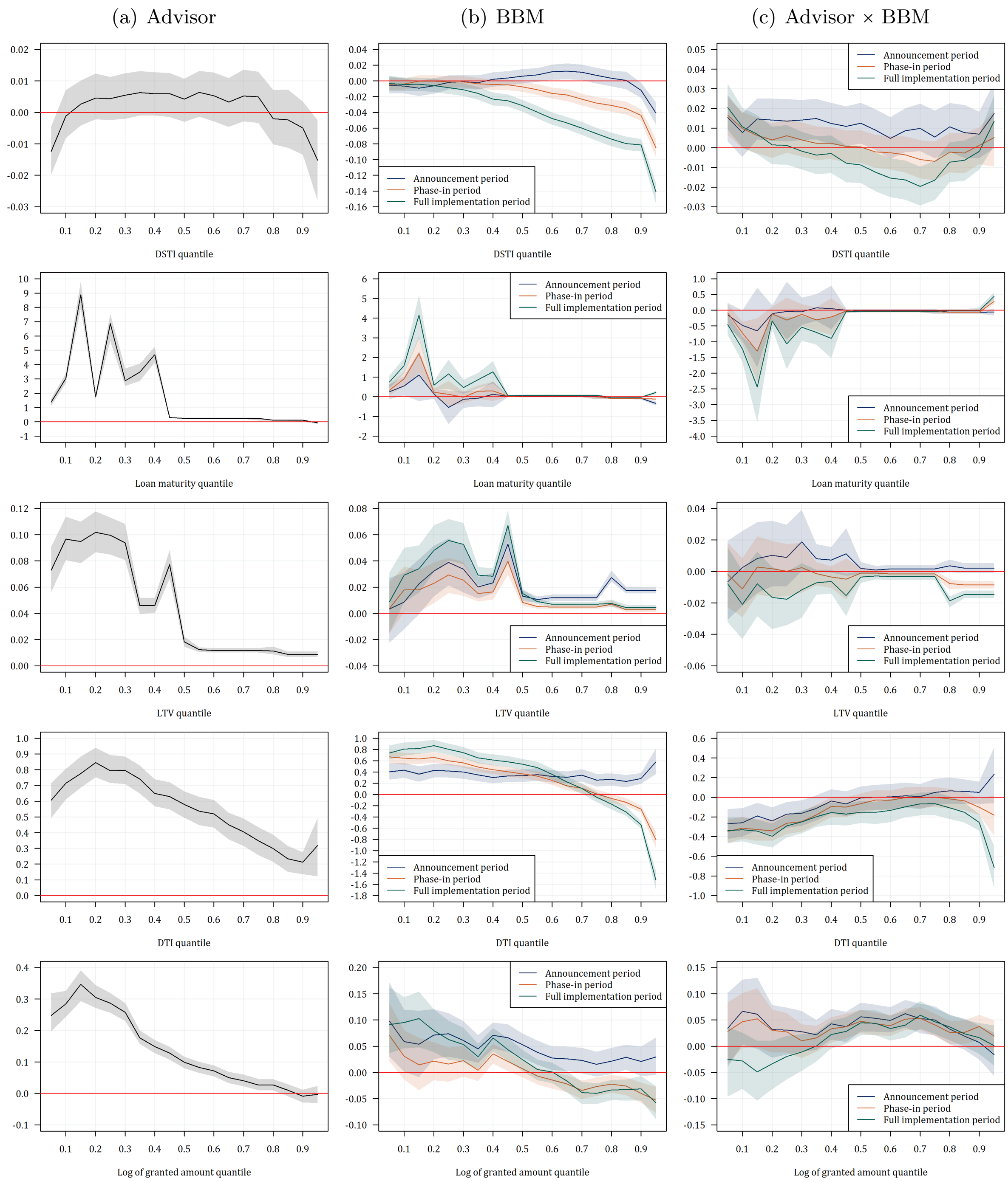
Notes: The figure shows the effect of advisors vis-a-vis the policy on the distribution of selected loan outcomes. The +/− 2Q sample refers to the estimation period including all loans granted from 2 quarters before the announcement of Decree 1 to 2 quarters after the full implementation of Decree 1 (for DSTI and loan maturity we consider the full implementation of DSTI tightening, while for LTV, granted amount and DTI we consider the full implementation of LTV tightening). The pre-announcement period is the reference category for the policy related variables. 95 % confidence bands estimated from 1000 bootstrap replications are represented by the light-colored areas. The red line indicates the zero effect threshold.

Figure G.2: Quantile effects of Decree 1 policy on selected loan outcomes (+/− 1Y sample)



Notes: The figure shows the effect of advisors vis-a-vis the policy on the distribution of selected loan outcomes. The +/− 1Y sample refers to the estimation period including all loans granted from 1 year before the announcement of Decree 1 to 1 year after the full implementation of Decree 1 (for DSTI and loan maturity we consider the full implementation of DSTI tightening, while for LTV, granted amount and DTI we consider the full implementation of LTV tightening). The pre-announcement period is the reference category for the policy related variables. 95 % confidence bands estimated from 1000 bootstrap replications are represented by the light-colored areas. The red line indicates the zero effect threshold.

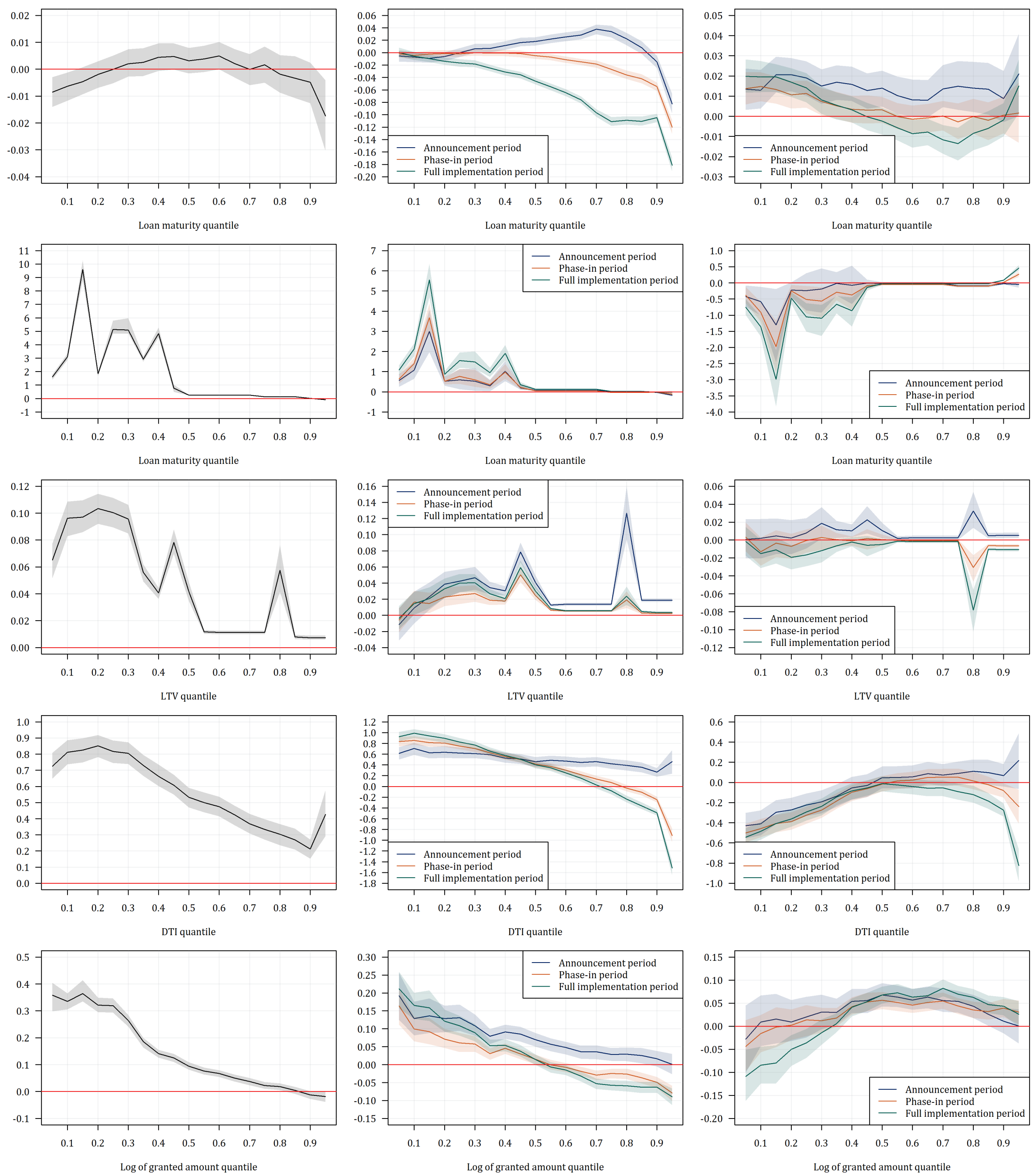
Figure G.3: Quantile effects of Decree 2 policy on selected loan outcomes (+/− 2Q sample)



Notes: The figure shows the effect of advisors vis-a-vis the policy on the distribution of selected loan outcomes. The +/− 2Q sample refers to the estimation period including all loans granted from 2 quarters before the announcement of Decree 2 to 2 quarters after the full implementation of Decree 2. The pre-announcement period is the reference category for the policy related variables. 95 % confidence bands estimated from 1000 bootstrap replications are represented by the light-colored areas. The red line indicates the zero effect threshold.

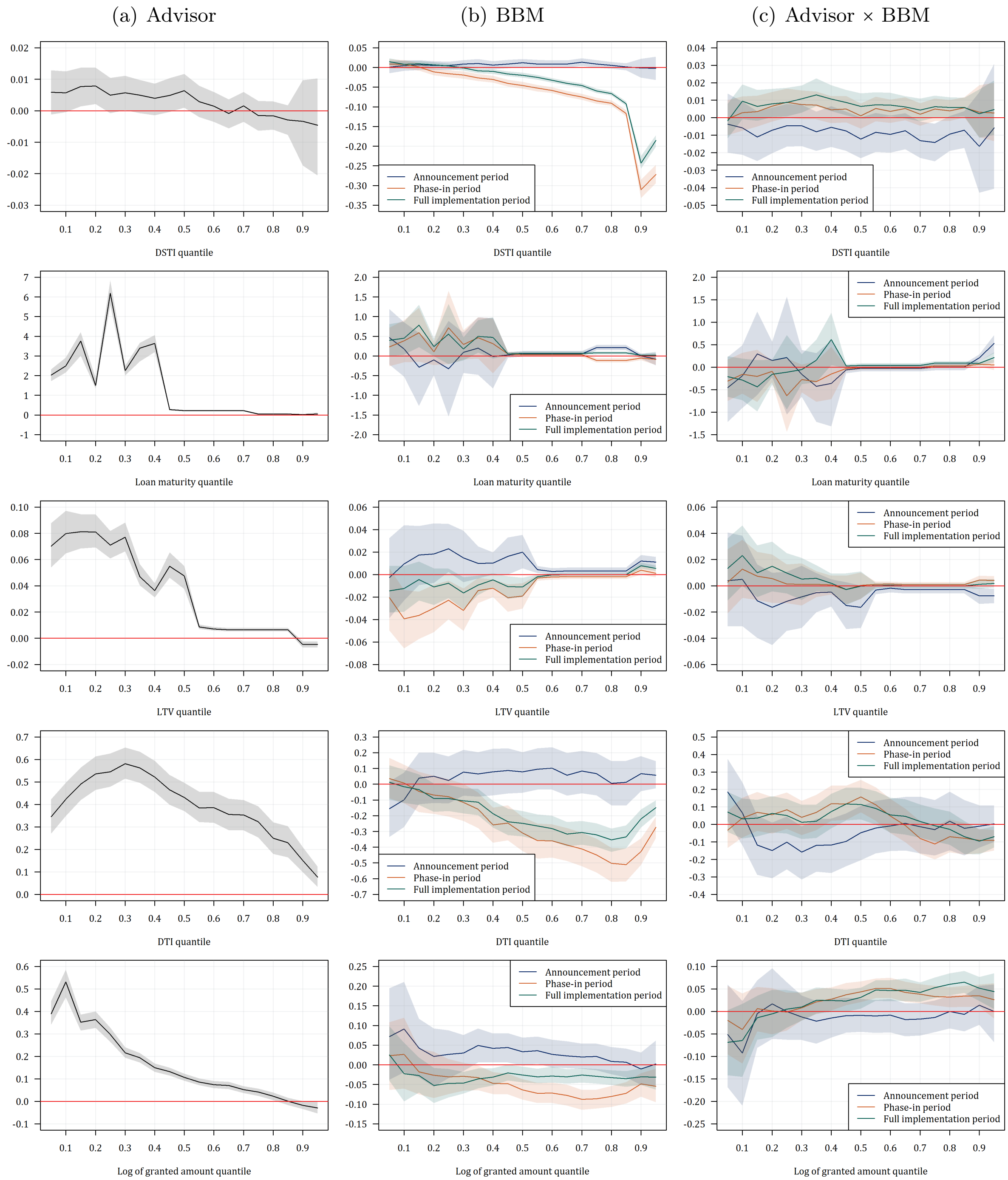
Figure G.4: Quantile effects of Decree 2 policy on selected loan outcomes (+/− 1Y sample)

(a) Advisor (b) BBM (c) Advisor × BBM



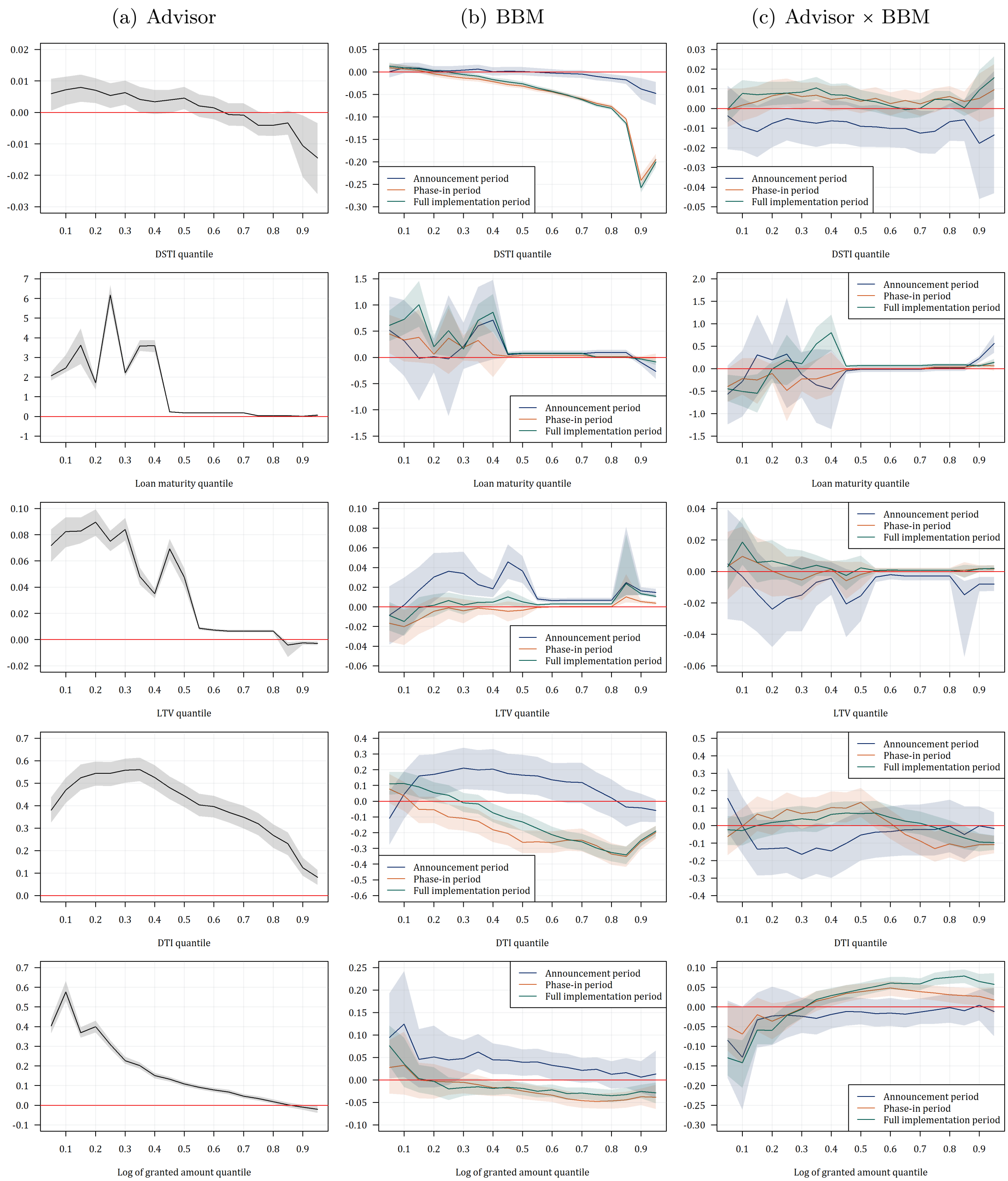
Notes: The figure shows the effect of advisors vis-a-vis the policy on the distribution of selected loan outcomes. The +/− 1Y sample refers to the estimation period including all loans granted from 1 year before the announcement of Decree 2 to 1 year after the full implementation of Decree 2. The pre-announcement period is the reference category for the policy related variables. 95 % confidence bands estimated from 1000 bootstrap replications are represented by the light-colored areas. The red line indicates the zero effect threshold.

Figure G.5: Quantile effects of Decree 3 policy on selected loan outcomes (+/− 2Q sample)



Notes: The figure shows the effect of advisors vis-a-vis the policy on the distribution of selected loan outcomes. The +/− 2Q sample refers to the estimation period including all loans granted from 2 quarters before the announcement of Decree 3 to 2 quarters after the full implementation of Decree 3. The pre-announcement period is the reference category for the policy related variables. 95 % confidence bands estimated from 1000 bootstrap replications are represented by the light-colored areas. The red line indicates the zero effect threshold.

Figure G.6: Quantile effects of Decree 3 policy on selected loan outcomes (+/− 1Y sample)



Notes: The figure shows the effect of advisors vis-a-vis the policy on the distribution of selected loan outcomes. The +/− 1Y sample refers to the estimation period including all loans granted from 1 year before the announcement of Decree 3 to 1 year after the full implementation of Decree 3. The pre-announcement period is the reference category for the policy related variables. 95 % confidence bands estimated from 1000 bootstrap replications are represented by the light-colored areas. The red line indicates the zero effect threshold.

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