

Search Frictions and Policy Interventions in Lending: Evidence from the Hungarian Mortgage Market

by

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All remaining errors are my own.

Abstract

This dissertation consists of three self-contained essays on the Hungarian mortgage loan market, contributing to the empirical and theoretical understanding of household finance, consumer protection, and the real effects of credit market policy.

Chapter 1 — Price Cap, Search Frictions, and Market Access: Evidence from the Hungarian Housing Loan Market studies the unconventional application of an interest rate cap on new housing loans in Hungary from October 2023 to June 2024. The program increased aggregate consumer surplus by lowering average interest rates and expanding total loan volume, but a sizeable share of the riskiest borrowers lost market access, generating an implicit redistribution from high- to low-risk borrowers. A significant ripple effect in declining interest rates is estimated, and market-boosting effects survived the programme’s expiry. These findings are rationalised through a search-friction framework in which the cap served as a transparency tool, signalling the recovery from the European energy crisis to pessimistic borrowers.

Chapter 2 — Birds of a Feather Indebted Together: Solutions to the Information Problem in the Case of Mortgage Loans examines peer effects in mortgage borrowing decisions. Borrowers with financially literate colleagues obtain interest rates roughly 0.2 percentage points lower than similar borrowers whose peers have lower financial literacy — a saving equivalent to four to five monthly instalments over the loan term. Peer effects are stronger where bank competition is weaker and for borrowers with low mathematical skills. The introduction of a standardised loan product is shown to offset the peer effect, suggesting that regulation can substitute for informal information channels.

Chapter 3 — Liquidity Transfer and the Real Economy during a Crisis: Evidence from a Loan Payment Moratorium analyses the consumption impact of Hungary’s broad-based, unconditional COVID-19 loan payment moratorium. Using a regression discontinuity design with individual-level bank account data, the paper finds that approximately 90% of the liquidity transfer was consumed. The programme also reduced the non-performing loan rate by 1.7 percentage points. District-level panel regressions indicate that around 20% of the aggregate extra household liquidity was consumed, with the gap reflecting selection bias absent from the individual-level identification.

Contents

Acknowledgements	ix
Abstract	xi
List of Figures	xix
List of Tables	xxiii
1 Price Cap, Search Frictions, and Market Access: Evidence from the Hungarian Housing Loan Market	1
1.1 Introduction	2
1.2 Program	6
1.2.1 Hungarian mortgage market	6
1.2.2 Implementation of the interest rate cap	7
1.3 Database	9
1.3.1 International dataset	12
1.4 Empirical approach	17
1.4.1 Constructing product-borrower segments	17
1.4.2 Difference-in-differences	19
1.4.3 Synthetic difference-in-differences	22
1.4.4 Regression discontinuity design	28
1.5 Results	32
1.5.1 Difference in differences	33
1.5.2 Synthetic difference-in-differences estimates	33
1.5.3 Regression discontinuity	38

1.5.4	Distributional Consequences: Crowding Out and Redistribution . . .	40
1.5.5	Model setup	43
1.5.6	Model predictions	48
1.5.7	Discussion	52
1.6	Conclusion	55
	Appendix Appendices	60
	Appendix 1.A Product-borrower segment indicator estimation	60
	Appendix 1.B Difference-in-differences estimation with covariates	62
	Appendix 1.C Synthetic difference-in-differences robustness tests	62
	Appendix 1.D Solution of the model	64
1.D.1	Pricing and searching	64
1.D.2	Interest rate cap as a costly signal	68
1.D.3	Credibility of direct communication	74
	Appendix 1.E Proofs	76
	Appendix 1.F Dispersion in interest rates	86
2	Birds of a Feather Indebted Together: Solutions to the Information Problem in the Case of Mortgage Loans	89
2.1	Introduction	90
2.2	Database	97
2.2.1	Hungarian State Treasury Database	97
2.2.2	ONET database	98
2.2.3	Credit Registry Database	98
2.3	Empirical approach	99
2.3.1	Network	99
2.3.2	Baseline model	101
2.3.3	Placebo test	104
2.3.4	Further specifications	105
2.3.5	Bank choices	105
2.4	Results	106

2.4.1	Baseline results	106
2.4.2	Heterogeneity in peer effects	108
2.4.3	Placebo test	110
2.4.4	Alternative outcomes	113
2.4.5	Similar bank choices	115
2.4.6	Certified Consumer Friendly Housing Loan	118
2.4.7	Estimating the impact of the introduction of Certified Consumer Friendly Housing Loans on peer effects	121
2.5	Conclusion	125
	Appendix Appendices	136
	Appendix 2.A Alternative specification: main results using linear weights . . .	136
	Appendix 2.B Alternative specification: controlling on regional level bank supply with additional fixed effects	137
	Appendix 2.C Description of the choice model	138
3	Liquidity Transfer and the Real Economy during a Crisis: Evidence from a Loan Payment Moratorium	141
3.1	Introduction	142
3.2	Policy background	148
3.2.1	Setup of the Hungarian moratorium	148
3.2.2	Sudden stop of consumption during Covid crisis	149
3.3	Data	150
3.3.1	Data used for individual level analysis on spending	150
3.3.2	Data used for individual level analysis on further outcomes	151
3.3.3	Data used for district level analysis	153
3.4	Empirical approach	158
3.4.1	Individual level observations: Regression discontinuity	159
3.4.2	District level estimations: Cross-sectional regressions	166
3.4.3	District level estimations: Panel regressions	169
3.5	Results	171

3.5.1	Regression discontinuity	171
3.5.2	Cross sectional regressions	177
3.5.3	Panel models	180
3.6	Conclusion	181
Appendix 3.A	Appendix	187
3.A.1	The role of a loan repayment moratorium	187
3.A.2	Drivers of participation in the moratorium in Hungary	187
3.A.3	Constructing product-borrower segments	189
3.A.4	Control variables used for cross-sectional regression	190
3.A.5	Individual level observations: Instrumental variable approach . . .	192

List of Figures

1.1	APR distribution of newly disbursed, market-based housing loans (left) and home equity loans (right)	13
1.2	Distribution of loan amounts of new market-based housing loans by APR in September, October 2023 (left), and in January 2024 (right)	13
1.3	Mortgage loan interest rates and quarterly log change of volumes in Hungary and IQR of EU member states	14
1.4	Selected macroeconomic indicators of Hungary and IQR of EU member states	16
1.5	Binscatter of mortgage loan interest rate and the estimated product indicator	20
1.6	APR (top) and logarithm of volumes (bottom) of mortgage and home equity loans by client categories	23
1.7	Main borrower and loan characteristics of newly granted mortgage loans around the implementation of the APR cap	30
1.8	Main borrower and loan characteristics of newly granted home equity loans around the implementation of the APR cap	31
1.9	Estimated impact of APR cap on APR by quintiles of product-borrower segment	34
1.10	Estimated impact of APR cap on loan volumes by quintiles of product-borrower segment	34
1.11	Spaghetti plot of APR with home equity loans as controls	36
1.12	Spaghetti plot of change of APR with international markets as controls . .	38
1.13	Spaghetti plot of Ln(loan volume) with international markets as controls .	39

1.14	Average interest rate and loan volume of mortgage loans around the implementation of the APR cap	40
1.15	Schematic figure of quantifying redistributive impacts	42
1.16	Redistributive effects of the APR cap across borrower segments	43
1.17	Marginal costs under different cost environments	47
1.18	Proxies for the cost environment	48
1.19	Equilibrium distributions of interest rates in the leading example	50
1.20	Effects of the interest rate cap on government's payoff in the leading example	52
1.21	Weights of controls in the SDiD estimations on APR using home equity loan groups (left) and EU member countries (right) as donors	62
1.22	Equilibrium with different beliefs	67
1.23	The narrative structure	72
1.24	Price dispersion (left) and its evolution with bank number and lowest interest rates	88
2.1	Estimated impact of the change in the average mathematics score of loan-taking peers	110
2.2	Estimated impact of the change in the average mathematics score of borrower's peers	111
2.3	Distribution of new housing loan volume by interest rate fixation, and the share of Certified Consumer Friendly Housing Loan products	121
3.1	Evolution of consumption variables, levels, Jan 2015 - Sept 2021	150
3.2	Share of loans in moratorium by value outstanding, April 2020	155
3.3	Extra liquidity provided to households, April 2020	156
3.4	Relationship between consumption change and extra liquidity	157
3.5	Monthly instalments around the cutoff	160
3.6	Wages around the cutoff	160
3.7	Maturity of loans around the cutoff	161
3.8	Age of borrowers around the cutoff	161

3.9	Evolution monthly spending around the disbursement of a mortgage loan .	162
3.10	Ratio of spending to inflow of borrowers' accounts around the cutoff . . .	163
3.11	Daily participation rate around the cutoff	164
3.12	Evolution of income when moratorium was introduced	166
3.13	Volume of existing loans	167
3.14	Age of borrowers	167
3.15	Evolution of OCR turnover by groups of districts	169
3.16	30+ days delinquency of non-eligibles from April 2020 to October 2021 .	175
3.17	90+ days delinquency of non-eligibles from April 2020 to October 2021 .	175
3.18	90+ days delinquency of non-eligibles from July 2022 to November 2024	176
3.19	Early repayment of loans	176
3.20	New loan taking in the mass market segment	177
A1	Binscatter of mortgage loan interest rate and the estimated product-borrower segment indicator	190

List of Tables

- 1.1 Descriptive statistics of Housing loans 11
- 1.2 Descriptive statistics of Home Equity loans 12
- 1.3 Descriptive statistics of quarterly mortgage loan interest rates volumes of
EU member states during 2020 - 2024 14
- 1.4 Descriptive statistics of quarterly macroeconomic indicators of EU mem-
ber states during 2020 - 2024 15
- 1.5 Balance test 29
- 1.6 Difference-in-differences estimation results by quintiles 33
- 1.7 Results of Synthetic Difference-in-Differences estimations APR with home
equity loans as control group 36
- 1.8 Results of Synthetic Difference-in-Differences estimations APR with in-
ternational control group 37
- 1.9 Results of Synthetic Difference-in-Differences estimations of Ln(loan vol-
umes) 39
- 1.10 Results of RD estimations by quintiles of product-borrower segment . . . 40
- 1.11 Searching and equilibrium prices in the leading example 51
- 1.12 Difference-in-differences estimation results by quintiles, including co-
variates 62
- 1.13 Results of Synthetic Difference-in-Differences estimations APR with home
equity loans as control group 63
- 1.14 Results of Synthetic Difference-in-Differences estimations APR with in-
ternational control group 63

1.15	Results of Synthetic Difference-in-Differences estimations of Ln(loan volumes)	63
2.1	Descriptive statistics of the merged dataset	99
2.2	Baseline specification	108
2.3	Heterogeneity based on mathematics and competition	111
2.4	Placebo test	113
2.5	Alternative outcomes	115
2.6	Conditional logit model on bank choice	117
2.7	Spread of CCFL	123
2.8	Spread of CCFL - IV estimation	125
2.9	Baseline results with linear weights	136
2.10	Heterogeneity with linear weights	137
2.11	Main specifications controlling on regional level bank supply with fixed effects	138
3.1	Descriptive statistics of the sample used in estimation of spending	152
3.2	Descriptive statistics of the sample used in estimation of Probability of default, early repayment and additional borrowing	152
3.3	Balance test of key borrower and loan characteristics	162
3.4	Results of RD estimations on 30+ delinquency by product-borrower segments	173
3.5	Results of RD estimations on 90+ delinquency by product-borrower segments	174
3.6	Results of RD estimations on Long run impact on 30+ and 90+ days delinquency	174
3.7	Cross sectional regressions	178
3.8	Regressions on panel data	181
A1	Product-borrower segment indicator: A model of observed interest rates	191
A2	Description of variables	193

A3	Included observations	194
A4	Bank account spending	196
A5	Descriptive statistics	198
A6	Bank account spending	199

Chapter 1

Price Cap, Search Frictions, and Market Access: Evidence from the Hungarian Housing Loan Market

This chapter is co-authored with Gergely Lakos (Central Bank of Hungary). It has been prepared as a chapter of this doctoral dissertation. The author's contribution encompasses the theoretical model, the empirical strategy, and the interpretation of results.

Abstract

We study the unconventional application of an interest rate cap on new housing loans applied from October 2023 to June 2024 in Hungary. The program achieved an increase in aggregate consumer surplus through decreasing average interest rates and increasing total loan amount. A sizeable part of riskiest borrowers lost market access, thereby the program generated an implicit redistribution benefiting borrowers with low risk and harming borrowers with high risk. A significant ripple effect in decreasing interest rates is estimated. Market boosting effects survived the program. We rationalize these empirical findings with search frictions. We argue that the temporary use of the interest rate cap served as a transparency tool that signaled to pessimistic borrowers the significant negative cost shock brought forth by the starting

recovery from the European energy crisis in 2022 and 2023. Alternative governmental transparency tools that can save the cost of market exclusion require investment in reputation or in some commitment device. Keywords: interest rate cap, search friction, mortgage loan

JEL codes: J24, G21, G41

1.1 Introduction

Price control can work well when the price mechanism is performing poorly. Prominent examples include regulating natural monopolies substituting for missing price competition, minimum wage constraining market power of employers, and consumer protection curbing confusing information and fraudulent practices.¹ In any case, the price regulation aims to restore incentives that competitive prices should transmit to market agents. Usually, this is an involved task, as competitive prices reflect a variety of information that is scattered throughout the economy and only partially available for the regulators.

In this paper, we study the unconventional application of an interest rate cap on new housing loans applied temporarily from October 2023 to June 2024 in Hungary. The program was introduced around the beginning of the recovery from the European energy crisis in 2022 and 2023 that worsened economic outlook materially and brought about the highest inflation in the EU coupled with extremely strict monetary conditions. The housing loan market was hit largely during the crisis as the disbursement of housing loans fell sharply together with transactions at the housing market.

We use administrative data from the central bank of Hungary (Magyar Nemzeti Bank, MNB). Our main dataset covers the full universe of mortgage loans disbursed between January 2020 and December 2024, allowing for a comprehensive assessment of during-

¹Details can be found in Laffont and Tirole (1993), Dube and Lindner (2024), and Heidhues and Kőszegi (2018), respectively.

and after-program effects. Our empirical strategy relies on a combination of several quasi-experimental methods. First, we use home equity loans as a control group in a difference-in-differences (DiD) approach, arguing that they share similar risk and cost characteristics with housing loans but were not subject to the cap. This allows us to investigate price effects during and after the policy. Unfortunately, we cannot credibly estimate volume effect as parallel trend assumption may not be valid regarding volumes. Therefore, we exploit that the interest rate cap was a hefty and unusual intervention in the EU that was systematically hit by the energy crises before the program and apply a synthetic difference-in-differences (SDiD) method with country level EU data. Finally, as a robustness test we implement a regression discontinuity (RD) approach. We exploit the unexpected timing of the policy's introduction and compare loans originated immediately before and after the cap's implementation.

We find strong evidence of the APR cap causing indeed a material decrease (1.1 p.p according to DiD results) in the mean APR of housing loans during the 9 months of the program. This effect explains the three quarters of the observed average fall in the mean APR. We also demonstrate that the program also triggered a substantial expansion (46 per cent according to SDiD results) in the volume of housing loans issued. This amounts to one-fourth of the additional disbursement until the end of the program compared to the level before the program. Although the quantity effect compared to previous empirical results from non-crisis times is large, we find that it leads to strong exclusion from the market in the lowest product-borrower segments.

These aggregate effects exhibit pronounced heterogeneity according to product-borrower segments. We proxy product-borrower segments and split the sample into deciles. According to DiD results, borrowers in the lowest two deciles, i.e. mass market borrowers experienced around 2 p.p decrease in mean APR as a consequence of the program. That is significantly higher (both statistically and economically) than the average decrease of 0.6 p.p for borrowers in the top two deciles (i.e. affluent borrowers). Based on SDiD analysis we find a significant decrease – more than 28 per cent – in total volume of housing loans

taken out by these more risky borrowers. At the same time, less risky borrowers enjoyed an increase of around 70 per cent additional new housing loan amount due to the program.

We rationalize our empirical findings with costly consumer search, where banks and the government are able to observe the *common* cost factor of lending, while borrowers are not. Costly search for better market offers generate equilibrium dispersion in interest rate offers. The interest rate cap narrows the range of possible offers, which dampens incentives for searching. Banks react to less intensive search with higher interest rate offers within the admissible set of interest rates. This could even lead to increasing equilibrium average interest rate. However, when the introduction of the cap also signals a low cost environment to pessimistic borrowers (who perceive high cost environment), the *expected* range of interest rate offers also widens from below, which increases search efforts and decreases interest rates. As a result, equilibrium distribution of interest rates moves uniformly downwards (ripple effect). Lifting the cap after a short period of time is optimal, since its temporary benefit of encouraging search disappears as the information asymmetry is resolved. The interest rate reducing effect of the cap survives the program if borrowers' pessimism is persistent enough in the counterfactual. Alternative explanations for the ripple effect of price regulation would not predict such a post program outcome.

Up to this point, our setting does not rely on any specificities on housing loan market. In the context of lending, different product types and borrowers with different creditworthiness involve significant heterogeneity in *idiosyncratic* costs. Therefore, any binding interest rate cap comes at a cost of excluding risky borrowers from the market for the duration of the program. This cost can be avoidable with direct announcements on the cost environment by the government if the government has the reputation of resisting temptation to spread optimism when it is unfounded. The source of this temptation is the generally undersupplied search effort, as borrowers do not internalize that more search effort decreases interest rates for everybody.

This paper contributes to the empirical literature that studies interest rate caps.² Most of

²Maimbo and Henríquez (2014) provides a review of currently used interest rate caps.

this work deals with permanent caps on consumer credit in normal times, whereas we study housing loans during a large negative cost shock also investigating post program effects of a temporary cap. The closest reference to our work is Akgündüz et al. (2023) analyzing decreased mortgage rates by state-owned commercial banks in Turkey from June to August 2020 as a part of the COVID-19 stimulus package. In the given situation, it is likely that market exclusion effect caused by lower interest rates was limited by state-owned banks. However, compared to our findings, a substantially lower aggregate effect in loan expansion (without analyzing its heterogeneity) during the program and no significant effect after the program were estimated. This can be partly attributed to the Turkish program conveying no good news about the macroeconomic environment. Using consumer loan data from Chile, the structural econometric model of Cuesta and Sepúlveda (2021) provides a detailed analysis of welfare effects of an interest rate cap. Benmelech and Moskowitz (2010) investigate anti-usury interest rate caps in the 19th century US. Several papers deal with more recent US data on interest rate caps regarding unsecured consumer loans: Bolen et al. (2023), Fekrazad (2020), Rigbi (2013), Zinman (2010). Employing the introduction of a usury law in Italy in 1997, Alessie et al. (2005) estimate consumer loan demand elasticity. Relaxed interest rate cap on automobile loans in the US is investigated by Melzer and Schroeder (2017). Burga et al. (2024) estimate the financial and real effects of a lending rate cap for micro and small business loans that came into effect in Peru in 2021. Cubillos-Rocha et al. (2021) study lifting of interest rate ceiling for microcredit loans in Colombia in 2007.

We add to the literature of costly consumer search the insight that a price cap can serve as a signal of low cost environment. Papers that investigate the asymmetric information on the common cost factor of the production between the consumers and the firms do not consider the possibility of a price cap. These include Benabou and Gertner (1993), Dana (1994), Janssen et al. (2011), Cabral and Fishman (2012), Janssen et al. (2017) applying sequential search protocol and Yang and Ye (2008), Tappata (2009) applying non-sequential search. Fershtman and Fishman (1994) and Armstrong et al. (2009) analyze the effects of a price cap without uncertainty on production cost. We combine the

approaches of Tappata (2009) and Armstrong et al. (2009) as the former lacks a price limit, while the latter does not consider learning in the searching process. Our main contribution is that eliminating borrowers' pessimism with a price cap counteracts the decline in search efforts that the cap induces by dampening the incentives for searching. We also highlight that this beneficial effect can be only temporary. Based on the results of Duffie et al. (2017) and Atayev (2023) studying the incentives for providing transparency at markets with search costs and asymmetric information, a price cap as a transparency tool may be optimal to use when the government did not invest in reputation or some commitment device that can provide credible information disclosure of large negative cost shocks. This paper also relates to the literature on monetary policy transmission in mortgage markets, as we investigate the spillover of a large negative cost shock into the interest rates of housing loans during a period of large monetary easing.

1.2 Program

1.2.1 Hungarian mortgage market

Hungarian household indebtedness through mortgages is significant, although it can be considered relatively low in European comparison. The outstanding housing loans and home equity loans stood at around HUF 5000 billion (EUR 13,1 billion) and HUF 740 billion (EUR 1,9 billion), respectively at the end of 2023. These amounts gave 41 per cent and 6 per cent of household debt, and 7 per cent and 1 per cent of the GDP in a country where 91 per cent of households live in owner occupied residential properties. The 4.1 million households had a total of 652 thousand housing loan contracts. Housing loans are supplied almost exclusively by banks registered in Hungary, but only 5 bigger banks (out of the 8 systemically important banks) are active in home equity lending.

Hungarian mortgages are full-recourse loans denominated in Hungarian forint (HUF) and amortized according to the annuity rule. Housing loans can only be used for specific pur-

poses (buying, building, or renovating residential properties), while home equity loans do not involve such a restriction. As a result, home equity loans are often used for consumption purposes and are typically more risky than housing loans³. Otherwise, Hungarian mortgage loans can be fully described by and exhibit considerable heterogeneity according to five additional characteristics: loan amount, collateral value of the residential property, maturity, annual percentage rate of charge (APR), and the interest rate rule. APR is the effective interest rate that borrowers face since the present value of the cash flow from the loan contract including all the additional charges and fees is equal to zero using APR as the discount rate. These charges and fees are commonly requested by banks for certain elements of their service like credit risk scoring, valuation of the property, contract conclusion, disbursement, credit insurance, and principal prepayment. Both fixed rate and adjustable rate mortgages are prevalent. In the latter case, there is no initial grace period, the interest rate changes periodically (usually every 5 or 10 years) according to the stipulated reference rate and the fix premium over it.

Since 2015, loan amounts have been constrained by macroprudential regulatory limits as a function of collateral value and borrower income (so-called debt break rules) which are set to 80 per cent and 50 per cent as the main rule for loan-to-value (LTV) and debt service-to-income (DSTI), respectively. A notable share of the housing loans (19 per cent at the end of 2023) involves some governmental subsidy mainly in the form of subsidized interest rate for home buyers with children.

1.2.2 Implementation of the interest rate cap

By the first half of 2023, the volume of new housing loans fell to a third of its previous level one year before, while their average interest rate rose from around 4 per cent to 9 per cent. This was the consequence of the European energy crisis started in 2022 which worsened economic outlook materially and brought about the highest inflation in the EU

³Consequently, the average interest rate of home equity loans is a few percentage points higher than a comparable housing loan's rate.

(25.7 per cent at the top in January 2023) with extraordinarily high central bank policy rate (18 per cent at the top from October 2022 to May 2023).

From October 2023 to June 2024, the APR of newly contracted non-subsidized housing loans was subject to a cap initiated by the government in order to reactivate housing lending. Government officials mentioned publicly first on 22 September 2023 that the government expects the banking sector to restart lending in order to restore economic growth, rather than imposing a new wave of windfall taxes on banks earning high sector level profits. On 29 September it was revealed that the government expects especially new housing loans to become cheaper as the result of consultations with the Banking Association, even by introducing a new interest rate cap. On 3 October, the APR cap on newly contracted non-subsidized housing loans from 9 October was announced. Specifically, the government asked the Banking Association to promote the voluntary implementation of the cap among its members which the Banking Association did publicly the same day. In the next few days, banks declared that they would comply.

The program was open ended without pre-established conditions for changing or abolishing the cap. The requirements were disclosed by the government, but no formal enforcement mechanism associated with them, as they were not part of a regulation imposed by legal means. From 9 October, the APR cap was set to 8.5⁴ per cent which was announced on 13 December to be lowered to 7.3 per cent from 1 January 2024. On 22 May, the minister of national economy asked the banking sector to maintain the APR cap until June 30. Accordingly, at the end of June, OTP Bank the biggest lender by far started to advertise some of its housing loan products for July with APRs higher than the cap. Other banks followed suit only one or two months later. The temporary APR cap was much more stringent than the permanent anti-usury APR cap that exists from 2010 and is equal to the central bank policy rate plus 24 percentage points in the recent years.

⁴average non-subsidized housing loan interest rate was 9.2 percent in September 2023

1.3 Database

In order to analyse the impact of the voluntary annual percentage rate of charge (APR) cap on newly disbursed, market-based housing loans, we primarily used data from the Central Bank's Credit Register (HITREG in Hungarian), supplemented by data from other databases. The credit register contains anonymized contractual data on retail loans on the balance sheets of credit institutions. The HITREG data reporting is a monthly data reporting, with data available from December 2019. Alongside the aforementioned databases, we also used the K23 report, which includes data on individual credit agreements of households and nonprofit institutions serving households.

The analysis covers the period from January 2020 to December 2024, on a monthly frequency. Among the types of retail loans, the analysis includes newly disbursed, market-based forint housing loans affected by the voluntary APR cap, as well as home equity loans. The latter was necessary to estimate how the interest rates of newly disbursed housing loans would have evolved without the APR cap. The database used for the analysis contains 283 641 individual observations, however there are only 198 599 where without missing data of which 167 692 (84 per cent) are housing loans and 30 907 (16 per cent) are home equity loans. The following data cleaning was carried out:

Contracts related to home savings banks were excluded, as the special pricing of these institutions would distort the estimation.

For interest rates, we assumed that if the interest rate is less than 0.4 percent, it can be considered a wrongly reported contract or employee loan that is priced and managed differently than market loans, so these contracts were filtered out of the database.

Extreme values of contracted amounts and household income were also eliminated: observations were kept for contracted amounts between 100 000 and 500 000 000 forints, and household income between 50 000 and 5 000 000 forints.

Data cleaning was also performed according to initial loan-to-value (LTV) ratio: contracts with an initial LTV greater than 90 percent were filtered out as according to the Borrower-based Measures Regulation⁵, introduced 1 January 2015, LTV ratio cannot exceed 80 percent or 90 percent in case of first-time buyers. Missing observations were as well filtered out.

In the following section we separately analyse the characteristics of housing loans (Table 1.1) and home-equity loans (Table 1.2). The variables were grouped according to customer characteristics and transaction characteristics. The customer characteristics include income, average age of the debtors, and number of debtors. For transaction characteristics, we examine the annual percentage rate, the payment-to-income ratio (PTI), the initial loan-to-value ratio, the contracted loan amount, the length/maturity of the loan and the interest rate fixation period. Within the full sample period (January 2020 - December 2024), we also analysed two sub-periods: the first sub-period is from the end of the payment moratorium to the start of the APR cap, i.e., from December 31, 2022, to October 9, 2023, and the second sub-period is the duration of the APR cap from October 9, 2023, to June 30, 2024.

The median average age of housing loan and home-equity loan borrowers was 39 and 44 years in the full sample, respectively, with the number of borrowers being two per loan. Based on the median income, these clients belong to the higher-income segment of the population. More than 70 per cent of borrowers for both housing loans and home equity loans (75 percent and 72 per cent, respectively) live either in Budapest, in county seats, or in other cities. These customer characteristics did not show significant changes in the two sub-periods. The analysed loans are typically taken out with longer (20-year) maturity and with interest period of 10 years or more. The contracted loan amounts are higher for housing loans both in the entire sample and in each sub-period, but the median contracted amounts – similarly to APRs – differ the most during the period of the APR cap. Under the APR cap, the typical housing loan amount is 16 million forints with an

⁵see details here: <https://www.mnb.hu/en/financial-stability/macprudential-policy/the-macprudential-toolkit/borrower-based-measures-ltv-dsti>

APR of 7.3 percent, while home equity loans typically amounted to 10 million forints with an APR of 11.6 per cent. Based on these differences, we can preliminarily conclude that the voluntary APR cap had a substantial impact on the development of housing loan interest rates and may have encouraged larger loan amounts. However, the rise in house prices also influences the development of loan amounts, the effect of which is out of the scope of this study.

Table 1.1: Descriptive statistics of Housing loans

Sample	Statistics	Customer characteristics			Contract characteristics					
		Income (HUF)	Average age of debtors	Number of debtors	APR (%)	PTI (%)	LTV (%)	Loan amount (HUF million)	Maturity (year)	Interest rate fixation period (year)
Full sample	p10	250 895	28	1	3.4	15.9	18.5	4.0	8.1	5.0
	p25	360 299	32	1	4.2	22.1	32.1	6.0	10.2	9.9
	median	518 563	39	2	5.7	30.2	51.6	11.2	20.3	10.0
	p75	740 485	46	2	7.4	39.0	69.2	19.6	20.5	11.0
	p90	1 050 643	52	2	9.2	46.7	78.6	29.6	25.5	20.0
	average	610 736	40	2	6.1	30.8	49.8	14.7	17.3	11.2
	std dev	401 418	9	1	4.5	11.6	21.7	12.8	7.4	5.9
1st sub-period	p10	327 838	28	1	8.2	17.5	16.6	3.5	9.7	10.0
	p25	450 507	32	1	9.0	24.3	30.6	7.0	11.3	10.0
	median	631 256	38	2	9.9	33.0	50.0	13.4	20.3	11.0
	p75	883 333	46	2	11.7	41.9	67.7	21.0	20.5	20.0
	p90	1 241 599	52	2	12.2	48.8	77.3	32.0	25.5	25.0
	average	737 106	39	2	10.1	33.1	48.2	16.3	18.0	14.6
	std dev	452 271	9	1	2.0	11.8	22.0	13.8	7.2	6.7
2nd sub-period	p10	360 358	28	1	6.2	17.1	20.4	5.1	10.2	10.0
	p25	489 106	32	1	6.7	23.6	34.4	10.0	15.2	10.0
	median	692 702	38	2	7.3	31.9	53.7	16.0	20.3	11.0
	p75	980 735	46	2	8.0	40.3	70.8	25.5	22.5	20.0
	p90	1 363 650	52	2	8.4	47.3	79.4	38.0	25.5	25.0
	average	804 077	39	2	7.2	32.1	51.6	20.1	18.9	14.8
	std dev	475 031	9	1	1.3	11.4	21.6	15.8	6.4	6.6

Notes: The table reports descriptive statistics for newly disbursed, market-based forint housing loans from January 2019 to June 2024. The first sub-period covers December 31, 2022 to October 9, 2023 (post-moratorium, pre-cap). The second sub-period covers October 9, 2023 to June 30, 2024 (APR cap period). Income is reported in Hungarian forints (HUF). APR denotes the annual percentage rate of charge. PTI is the payment-to-income ratio. LTV is the initial loan-to-value ratio. Loan amount is in HUF millions. Maturity is in years. Interest rate fixation period is in years. Statistics reported are the 10th, 25th, 50th (median), 75th, and 90th percentiles, mean, and standard deviation.

During the period of the voluntary APR cap, the pricing range of housing loans significantly narrowed. According to the recommendation effective from 9 October 2023, domestic commercial banks were requested to provide housing loans with a maximum APR of 8.5 per cent. The APR cap was reduced to 7.3 per cent from 1 January 2024, and remained in effect until 30 June 2024. The measure had a substantial impact on the domestic housing loan market, as 87 per cent of the market-based housing loan volume was contracted below the cap between October 2023 and June 2024. The APR distribution of newly contracted housing loans significantly narrowed during this period (Figure 3). This

Table 1.2: Descriptive statistics of Home Equity loans

Sample	Statistics	Customer characteristics			Contract characteristics					
		Income (HUF)	Average age of debtors	Number of debtors	APR (%)	PTI (%)	LTV (%)	Loan amount (HUF million)	Maturity (year)	Interest rate fixation period (year)
Full sample	p10	212 521	32	1	4.7	18.8	14.8	3.0	6.2	5.0
	p25	308 000	37	1	6.2	25.9	24.5	5.0	10.2	9.9
	median	469 687	44	2	8.5	34.7	40.0	8.6	15.3	10.0
	p75	696 000	50	2	10.6	43.9	58.0	15.0	20.4	10.0
	p90	1 038 485	56	2	12.5	49.8	69.9	24.9	25.4	20.0
	average	580 116	44	2	8.5	34.5	41.3	12.1	16.1	10.8
	std dev	446 994	9	1	3.0	11.8	20.1	12.5	7.0	4.9
1st sub-period	p10	311 482	31	1	9.5	21.7	15.3	4.0	10.2	10.0
	p25	427 055	37	1	10.3	29.5	24.8	6.5	11.2	10.0
	median	632 098	44	2	11.4	38.3	40.5	11.2	20.3	10.1
	p75	953 348	50	2	13.6	46.9	57.5	20.0	20.4	20.0
	p90	1 447 469	56	2	14.1	50.0	69.7	37.9	25.4	25.0
	average	793 465	44	2	11.8	37.5	41.3	17.0	17.1	13.9
	std dev	585 205	9	1	2.1	11.5	19.6	17.4	6.2	5.7
2nd sub-period	p10	326 025	32	1	7.9	20.7	14.3	4.5	10.2	10.0
	p25	441 487	37	1	10.0	27.5	23.9	6.5	11.3	10.0
	median	631 300	44	2	11.6	36.3	39.4	10.0	20.3	10.1
	p75	897 432	50	2	12.4	45.0	56.2	17.0	20.4	20.0
	p90	1 300 075	56	2	12.7	50.0	69.2	27.9	25.4	25.0
	average	756 238	44	2	10.9	36.0	40.5	14.5	17.5	14.0
	std dev	498 633	9	1	2.3	11.5	19.7	14.6	5.8	6.0

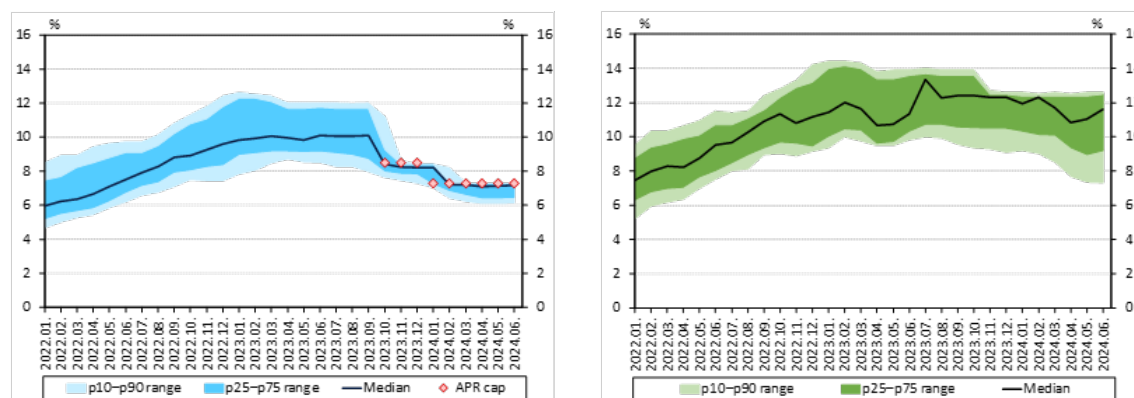
Notes: The table reports descriptive statistics for newly disbursed forint home equity loans from January 2019 to June 2024. The first sub-period covers December 31, 2022 to October 9, 2023 (post-moratorium, pre-cap). The second sub-period covers October 9, 2023 to June 30, 2024 (APR cap period). Home equity loans were not subject to the APR cap and serve as the control group in the difference-in-differences estimations. Income is reported in Hungarian forints (HUF). APR denotes the annual percentage rate of charge. PTI is the payment-to-income ratio. LTV is the initial loan-to-value ratio. Loan amount is in HUF millions. Maturity is in years. Interest rate fixation period is in years. Statistics reported are the 10th, 25th, 50th (median), 75th, and 90th percentiles, mean, and standard deviation.

suggests that the APR cap hindered risk-based pricing, as banks could not necessarily enforce the actual risk assessment of customers in the pricing of housing loans due to the cap. After the release of the APR cap, the upper part of the distribution slightly widened, although there was no significant change in the median APR between July and October. For home equity loans, the median APR did not change significantly during the period of the APR cap and remained around 10 per cent.

1.3.1 International dataset

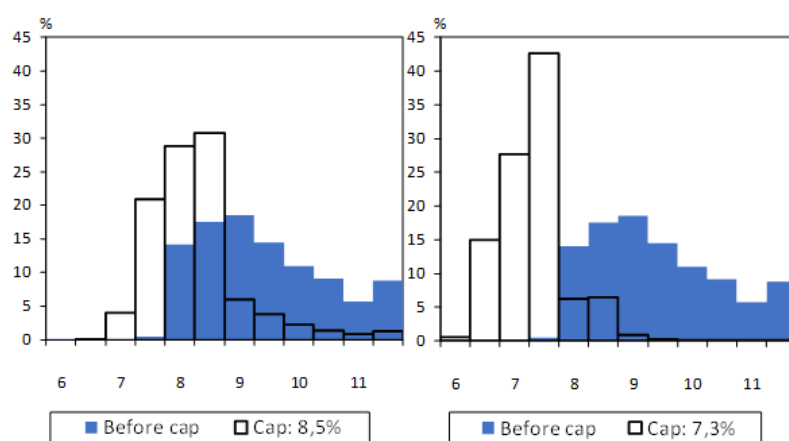
Furthermore, we built an international database that includes data of European Union member states' mortgage loan markets. New business volumes and interest rates are downloaded from the European Central Bank's data portal and also collected by hand

Figure 1.1: APR distribution of newly disbursed, market-based housing loans (left) and home equity loans (right)



Note: The figure shows the monthly distribution of the annual percentage rate (APR, per cent) for newly disbursed market-based forint housing loans (left panel) and home equity loans (right panel) from January 2020 to June 2024. The shaded bands represent the interquartile range (p25–p75, darker) and the p10–p90 range (lighter). The solid black line shows the monthly median APR. Red dots in the left panel indicate the applicable APR cap level (8.5% from October 9, 2023 and 7.3% from January 1, 2024). The cap was in effect from October 9, 2023 to June 30, 2024. Source: Magyar Nemzeti Bank (MNB) Credit Register.

Figure 1.2: Distribution of loan amounts of new market-based housing loans by APR in September, October 2023 (left), and in January 2024 (right)



Note: This table presents the distribution of APR of newly disbursed housing loans in the months when the interest rate cap was introduced (September, October 2023 (left)) and was adjusted (January 2024 (right))

in case of a few countries⁶ from the Quarterly Review series of the European Mortgage Federation. Both newly disbursed mortgage loan volume and mortgage interest rates show similar dynamics as the European member states' corresponding figures, though the Hungarian mortgage loan volume is more volatile and mortgage interest rates are much higher than in most European countries.

Macroeconomic dataset was obtained from Eurostat and contains time series on wages,

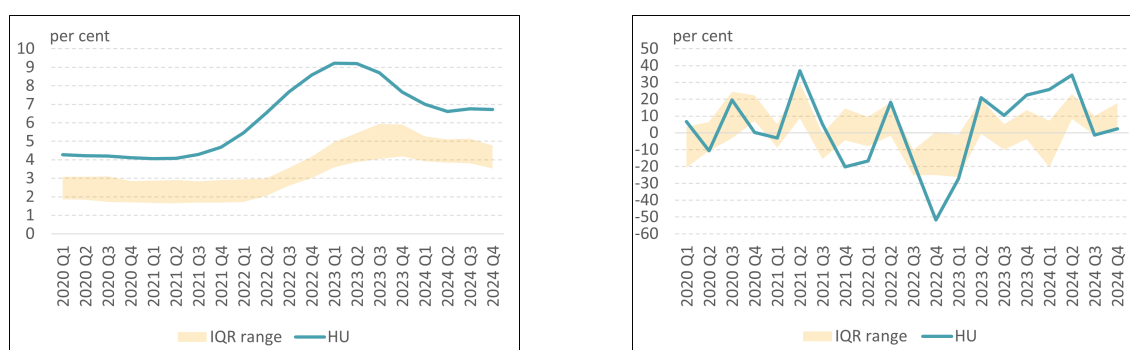
⁶New business volumes of the Czech and Romanian mortgage loan markets were missing from the ECB database. Interest rate of the Czech newly disbursed mortgage loans were missing as well, therefore we downloaded that from the ARAD database of the Czech National Bank mortgage

Table 1.3: Descriptive statistics of quarterly mortgage loan interest rates volumes of EU member states during 2020 - 2024

Country	Volume of newly disbursed mortgage loan (EUR million)			Annual percentage rate (APR) of newly disbursed mortgage		
	Mean	Std	N	Mean	Std	N
AT	7485	2074	24	2.7	1.2	24
BE	10393	2152	24	2.6	0.9	24
BG	N/A	N/A	N/A	3.6	0.4	24
CY	304	104	24	3.5	0.8	24
CZ	3146	1690	24	3.7	1.3	24
DE	59850	13198	24	2.4	1.2	24
EE	672	248	24	3.6	1.4	24
ES	14050	2706	24	2.5	0.9	24
FI	7359	1232	24	2.0	1.4	24
FR	59482	16592	24	2.6	0.9	24
GR	218	119	17	3.8	0.8	24
CR	508	196	24	3.4	0.5	24
HU	202	56	24	6.0	1.8	24
IE	3158	802	24	3.2	0.6	24
IT	17721	2802	24	2.8	1.2	24
LT	549	170	24	3.6	1.5	24
LU	1815	610	20	2.4	1.1	24
LV	254	89	24	3.8	1.5	24
MT	292	123	18	2.8	0.2	24
NL	29124	6205	24	2.7	0.9	24
PL	N/A	N/A	N/A	6.3	4.2	24
PT	4369	1381	24	3.9	1.6	24
RO	872	227	24	5.0	1.7	24
SE	N/A	N/A	N/A	N/A	N/A	N/A
SI	403	130	24	3.0	0.9	24
SK	3478	1198	24	2.4	1.1	24

The table reports descriptive statistics of quarterly newly disbursed mortgage loan volumes (in EUR million) and annual percentage rates (APR, in per cent) for EU member states over 2020 Q1–2024 Q4. Volume data are downloaded from the European Central Bank's data portal; missing series for Czech Republic and Romania are collected from the Quarterly Review series of the European Mortgage Federation. Czech APR data are from the ARAD database of the Czech National Bank. N denotes the number of quarterly observations.

Figure 1.3: Mortgage loan interest rates and quarterly log change of volumes in Hungary and IQR of EU member states



(a) APR of mortgage loans

(b) Quarterly log change of mortgage loan volumes

Note: Panel (a) shows the quarterly average annual percentage rate (APR, per cent) of newly disbursed mortgage loans in Hungary (solid teal line) and the interquartile range (IQR) across EU member states (shaded area) from 2020 Q1 to 2024 Q4. Panel (b) shows the quarterly log change of newly disbursed mortgage loan volumes for Hungary and the IQR of EU member states over the same period. The IQR is computed across EU member states for which data are available; see Table 3 for country coverage. Data sources: European Central Bank Statistical Data Warehouse; European Mortgage Federation Quarterly Review; ARAD database of the Czech National Bank; Eurostat.

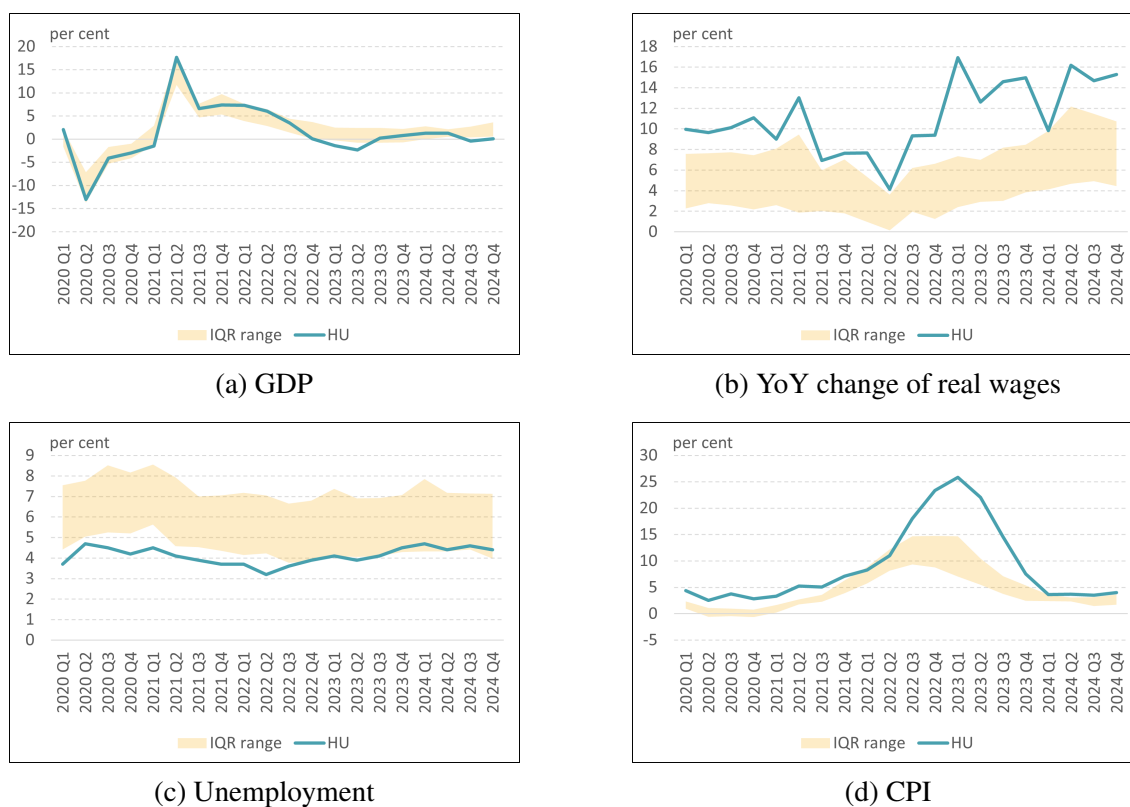
price level, year-on-year change of real GDP and unemployment rate of the 15-64 years old population by country for the time period of 2020 - 2024 on quarterly basis. The evolution of these indicators is similar to EU member states' series, however increase of prices and real wages are much faster while unemployment rate is lower in Hungary during the observed period.

Table 1.4: Descriptive statistics of quarterly macroeconomic indicators of EU member states during 2020 - 2024

Country	YoY change of real GDP			YoY change of total wages and salaries on quarterly			Unemployment rate of puplation, age cohort: 15-			12 month average price level change on quarterly		
	Mean	Std	N	Mean	Std	N	Mean	Std	N	Mean	Std	N
AT	0.8	5.4	24	4.1	3.0	24	5.2	0.8	24	4.2	3.2	24
BE	1.8	4.4	24	3.5	2.7	24	5.7	0.4	24	3.6	3.5	24
BG	2.9	3.5	24	10.4	3.2	24	4.6	0.7	24	5.1	4.6	24
CY	5.0	5.6	24	3.2	3.6	24	6.6	1.2	24	2.7	3.1	24
CZ	1.1	3.8	24	6.3	2.7	24	2.5	0.4	24	6.5	5.4	24
DE	0.2	3.6	24	2.9	1.5	24	3.4	0.3	24	3.7	3.1	24
EE	0.9	4.3	24	7.0	2.9	24	6.4	1.2	24	6.4	7.2	24
ES	1.8	7.4	24	2.6	1.6	24	13.6	1.6	24	3.0	2.9	24
FI	0.3	2.5	24	2.3	1.0	24	7.6	0.9	24	2.7	2.6	24
FR	1.2	5.5	24	2.3	1.3	24	7.8	0.6	24	3.0	2.2	24
GR	2.0	6.5	24	3.4	2.7	24	13.8	2.9	24	2.7	3.7	24
CR	3.8	7.1	24	5.8	4.4	24	6.6	1.2	24	4.4	4.2	24
HU	2.0	5.5	24	11.2	3.1	24	4.0	0.4	24	8.1	7.0	24
IE	6.0	6.7	24	3.5	1.0	24	5.1	1.0	24	2.9	3.1	24
IT	1.2	6.5	24	1.6	1.9	24	8.6	1.4	24	3.0	3.6	24
LT	2.9	2.8	24	15.4	10.7	24	7.3	1.0	24	6.1	6.9	24
LU	1.5	3.5	24	3.3	2.2	24	5.7	0.9	24	3.1	2.7	24
LV	1.0	4.3	24	7.8	2.2	24	7.3	0.7	24	5.7	7.0	24
MT	5.8	6.7	24	3.3	2.1	16	3.9	0.7	24	2.9	2.3	24
NL	1.7	4.2	24	3.3	2.8	24	3.7	0.4	24	4.3	3.7	24
PL	3.0	4.2	24	8.1	2.3	24	3.1	0.3	24	6.5	4.5	24
PT	2.1	6.5	24	3.9	3.7	24	6.7	0.6	24	2.9	3.2	24
RO	2.3	3.8	24	14.3	10.0	24	5.3	0.7	24	6.3	3.7	24
SE	1.3	3.1	24	2.8	0.8	24	8.1	1.0	24	3.5	2.9	24
SI	2.5	4.6	24	5.5	2.9	24	4.3	0.7	24	3.7	3.7	24
SK	1.7	3.4	24	7.4	2.3	24	6.2	0.6	24	5.7	4.5	24

Note: The table reports descriptive statistics of quarterly macroeconomic indicators for EU member states over 2020 Q1–2024 Q4. YoY change of real GDP is the year-on-year growth rate of real gross domestic product (per cent). YoY change of total wages and salaries is measured on a quarterly basis (per cent). Unemployment rate refers to the 15–64 years old population (per cent). The 12-month average price level change is the year-on-year consumer price inflation (per cent). Data source: Eurostat. N denotes the number of quarterly observations.

Figure 1.4: Selected macroeconomic indicators of Hungary and IQR of EU member states



Note: The figure shows quarterly macroeconomic indicators for Hungary (solid teal line) and the interquartile range (IQR) across EU member states (shaded area) from 2020 Q1 to 2024 Q4. Panel (a): year-on-year change of real GDP (per cent). Panel (b): year-on-year change of total wages and salaries (per cent). Panel (c): unemployment rate of the 15–64 years old population (per cent). Panel (d): 12-month average consumer price inflation (per cent). Data source: Eurostat.

1.4 Empirical approach

1.4.1 Constructing product-borrower segments

The interest rate cap is likely to have heterogeneous effects. Borrowers who would face the highest interest rates absent the cap are expected to experience the largest impact, as their rates should decrease the most. The impact on clients whose counterfactual interest rate falls below the cap is ambiguous: banks might increase their rates (e.g., to recoup losses incurred on other clients) or leave them unchanged due to local competition. Similarly, loan volume dynamics are likely to be heterogeneous. Banks may be unwilling to serve the riskiest mass-market clients if the mortgage becomes unprofitable at the capped rate (crowding-out effect), while simultaneously shifting supply toward other segments. To understand the mechanisms driving market outcomes, accounting for this heterogeneity is crucial.

To this end, we create a product-borrower segment indicator to define groups of loans. From the banks' perspective, these groups represent distinct market segments (e.g., the mass market versus affluent clients). It would be a natural solution to define these segments in this analysis based on a standardized credit risk metric (such as the FICO score in the US). Unfortunately, no such widely available credit score exist on the Hungarian market. Therefore, we exploit information concentrated in the interest rate of the loans.

The interest rate of housing and home equity loans depends on several factors: the riskiness of the client, financing costs, banks' operational costs, required profit margins and the local market structure. As we do not have precise information about these variables we must rely on observable proxies: client characteristics, loan characteristics, market concentration and indicator variables of the granting bank.

To construct a measure that robustly proxies the underlying borrower risk, we must deliberately separate the borrower-specific premium from other pricing components. Specifi-

cally, we need to exclude the baseline macroeconomic financing cost (f_i)—which is distinct from APR⁷—to ensure our indicator captures cross-sectional borrower heterogeneity rather than aggregate time-series fluctuations in funding costs. Furthermore, we must abstract away from bank-specific fixed effects (B_i) and local market competition proxies (H_i) to prevent our measure from being contaminated by idiosyncratic bank pricing strategies and localized market power.

Equipped with these observable variables, we first estimate a comprehensive baseline pricing model:

$$Y_i = \alpha + \beta C_i + \delta L_i + \phi_i f_i + \gamma B_i + \mu H_i + \omega B_i \times H_i + \varepsilon_i \quad (1.1)$$

where Y_i is the APR of loan i , C_i is a vector of client related variables, L_i is a vector of loan related variables, f_i is financing cost of the loan, B_i is a vector of indicator variables marking the granting bank, H_i is the Herfindahl Hirschmann index⁸, $B_i \times H_i$ stands for the interaction of these variables and ε_i is a disturbance term. We estimate the regression on a pre-cap⁹ sample from 2020 Q1 to 2023 Q3.

As shown in Appendix 1.A, this full model yields a strong fit¹⁰.

⁷Financing cost is estimated on the bank level using reference rates corresponding to the repricing period of the loan. In Hungary banks are forced to use reference rates (these can be interbank swap rates, various terms of Budapest interbank rate (BUBOR) or various terms of government bond yields.) to price their loans from previous months or the current month. We use the interbank swap rate with the appropriate term and lag structure as a proxy for the reference rate. Unfortunately, we do not have information about which exact lag of the reference rate is used for a given product. According to the legislation these lags vary typically between one and six months or averages of last one to six months. We select the lag that leads to the highest correlation between interest rates of newly disbursed loans and the lagged swap rates. We apply different lags on the bank and term level.

⁸We calculate the index using the observed market share of banks district-by-district in all quarters separately using the following formula:

$$H_i = \sum_{b=1}^{10} bankshare_{d,t,b}^2 \quad (1.2)$$

where d stands for district, t for time, b for bank and bank share is the sum of loan amounts in a given district at a given time for a given bank. These variables are unique for all loan i .

⁹We leave out loans from the sample that were disbursed during or after the APR cap.

¹⁰adjusted R^2 of the two models are around 80%, root mean squared errors are below 1 percentage point that means a relative error of around 15%

Following our conceptual framework, we construct our final product-borrower segment indicator using a reduced-form prediction that explicitly omits the aforementioned bank fixed effects, local competition proxies, and financing costs:

$$\hat{Y}_i = \hat{\alpha} + \hat{\beta}C_i + \hat{\delta}L_i \quad (1.3)$$

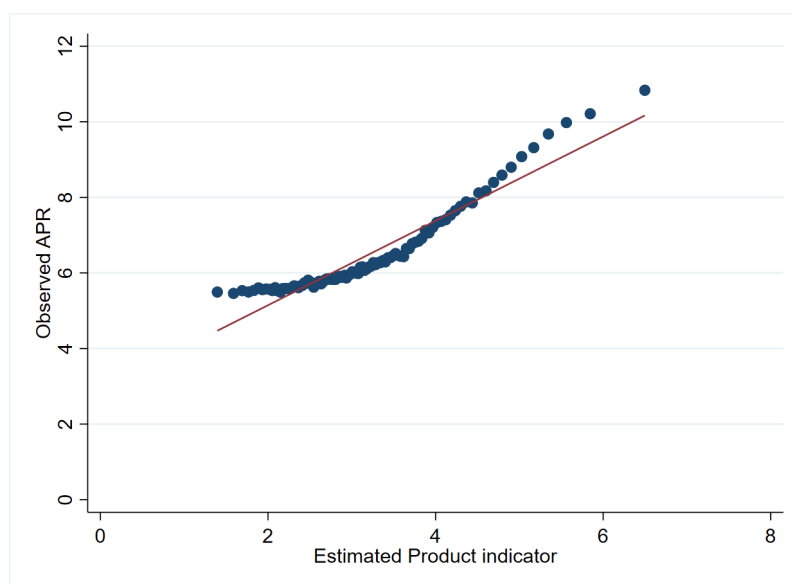
We acknowledge that this predicted rate ($\hat{Y}_i$) does not purely isolate credit risk. Rather, it captures a combination of the underlying risk premium and segment-specific pricing power. For instance, varying profit margins can be driven by search frictions, as different socio-demographic groups might exhibit different search behaviors and face different informational barriers. Nevertheless, omitting bank and local controls successfully captures this broader, segment-level premium tied to observable traits.

This procedure generates a robust ranking (see Figure 1.5). We split the sample into deciles based on the predictions of Equation (1.3). This yields homogeneous segments that pragmatically proxy both the unobserved credit risk and the segment-specific market frictions, allowing us to accurately separate borrowers and evaluate the heterogeneous effects of the interest rate cap.

1.4.2 Difference-in-differences

The first challenge in identifying the effect of the APR cap is finding a control group. As the cap was applied to all housing loans, if we include only housing loans in the analysis we may not be able to answer the question what would have happened to interest rates and loan volumes in the absence of the program. We argue that home equity loans may be suitable as control group – similar economic forces drive the interest rate of home equity loans and housing loans as they are all backed by property. Home equity loans are also long term loans whose interest rate is linked to similar instruments as housing loans. Therefore, macroeconomic processes regarding the housing market have similar impact

Figure 1.5: Binscatter of mortgage loan interest rate and the estimated product indicator



Note: The figure shows a binscatter plot of the observed annual percentage rate (APR, per cent, y-axis) against the estimated product-borrower segment indicator (x-axis) for housing loans disbursed between 2020 Q1 and 2023 Q3 (the estimation sample for the segment indicator model). Each dot represents the mean observed APR within an equal-sized bin of the estimated product indicator. The red line is a linear fit. The product-borrower segment indicator is constructed as the predicted value from Equation (3), which excludes financing cost and bank fixed effects from the full specification in Equation (1), so that the ranking reflects borrower and loan risk characteristics rather than cyclical cost conditions or bank-specific pricing. Source: Magyar Nemzeti Bank (MNB) Credit Register.

on the riskiness of home equity loans as on housing loans. Nevertheless, there are a few differences. First, housing loans can be taken solely for home purchase or renovation while home equity loans can be used freely. The attitude of taking a higher interest rate loan for consumption purposes reveals higher riskiness of the borrower. Second, the term of a typical home equity loan is 15 years while it is 20 years for a typical housing loan. Third, the typical amount of home equity loans is smaller: half of all home equity loans contracted were between 5 and 14 million forints while housing loans were between 8 and 20 million forints. All in all, home equity loans are more risky than housing loans, therefore their interest rates are higher. However, the difference between the interest rates of home equity loans and housing loans are stable through time, therefore home equity loans are an appropriate control group in a difference-in-differences estimation strategy. We estimate a standard difference-in-differences model on repeated cross-sections. The unit of observation is a loan granted to a borrower in a given period. The treatment group consists of housing loans that were granted after 9 October 2023, while the control group consists of home equity loans. We observe loans granted from January 2019 to June 2024.

We estimate the following model:

$$Y_{i,t} = \beta Post_t \times Type_i + \delta Post_t + \gamma Type_i + \omega Controls_{i,t} + \phi_t + \varepsilon_{i,t} \quad (1.4)$$

where $Y_{i,t}$ is the annual percentage rate of loan i if the loan belongs to group g and was granted in period t . $Type_i$ is an indicator variable that takes the value of 1 if the loan is a housing loan. $Post_t$ is also an indicator variable that takes the value of 1 during the periods when the APR cap was applied and 0 otherwise. The key variable of interest in the model is the interaction of these variables that marks the loans that were treated by the program. ϕ_t captures quarterly time fixed effects while $Controls_{i,t}$ represent control variables such as age and squared age, natural logarithm of the total income of all borrowers of a given contract, reference interest rate, natural logarithm of the loan amount, debt service to income ratio, loan to value ratio, maturity, indicator variables of the bank granting the loan and indicator variables of the district where the borrower lives. (i, t) is a disturbance term.

A potential weakness of our estimation strategy is that there can be unobserved characteristics or shocks that are related to the implementation of the program. Moreover, the key assumption behind the difference-in-differences estimation, i.e. parallel trends, may be valid only if we control for special characteristics of the loans and include variables that can absorb shocks. We think that controlling for a large set of fixed effects these problems can be eliminated: district level indicator variables can absorb unobserved time-invariant local shocks, while the set of time indicator variables capture those shocks that are common to loans granted in the same period.

The validity of the Difference-in-Differences (DiD) framework in this study depends on the specific outcome variable being analyzed. For price-based metrics such as the Annual Percentage Rate (APR) of mortgage loans, the critical parallel trends assumption appears well-satisfied. These rates are anchored by systemic financing and risk costs. Consequently, a traditional DiD estimator provides a reliable basis for identifying the impact of

the APR cap on mortgage pricing.

In contrast, the parallel trends assumption is likely violated when estimating policy impacts on newly disbursed loan volumes. Historical event-study charts reveal divergent pre-treatment trajectories between the mortgage market and the home equity loan control group. This divergence is primarily attributable to the structural volatility and smaller scale of the home equity market relative to the mortgage market. Because of this smaller sample size, the control group is more prone to idiosyncratic shocks that do not reflect broader mortgage market conditions, thereby undermining the counterfactual for volume analysis.

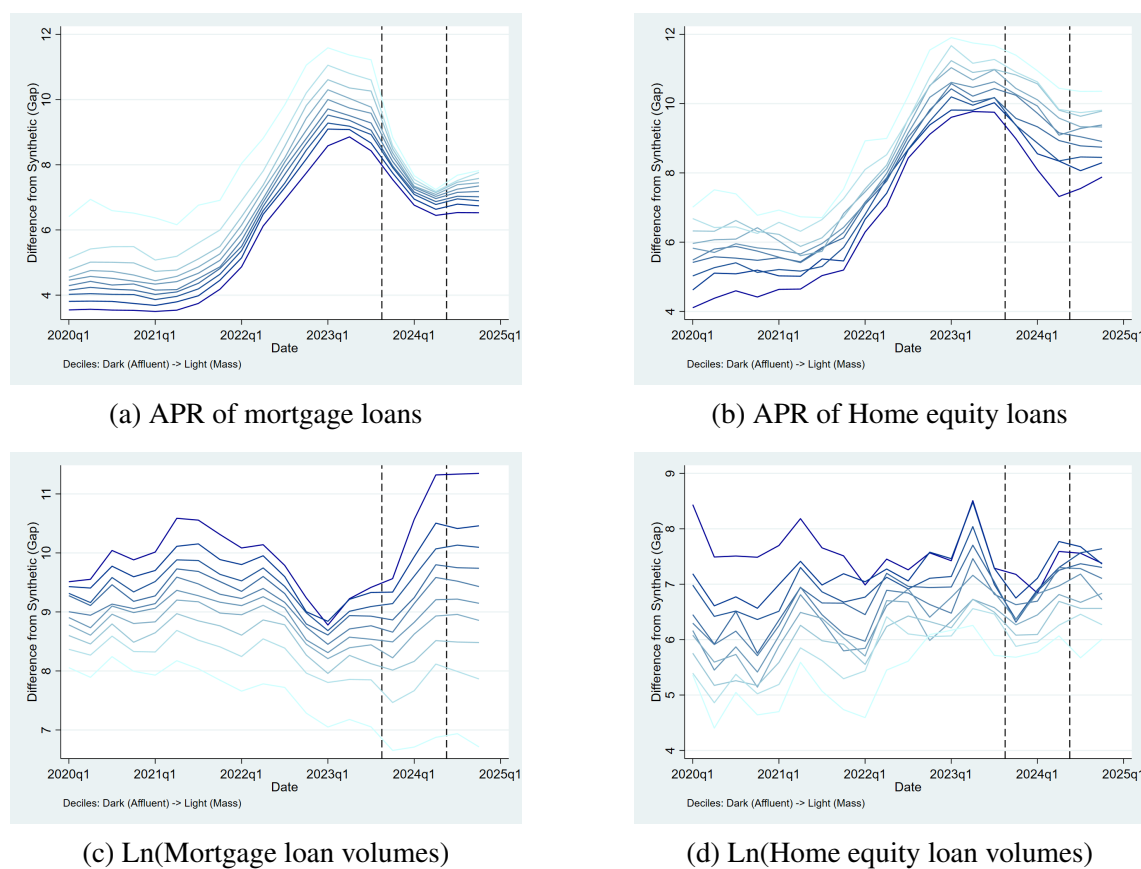
Arbitrage activities within the home equity market further distorted the volume trends required for a valid DiD estimation. Anecdotal evidence suggests that certain client segments utilized home equity loans to exploit the yield spread between high-yielding, inflation-linked government bonds and the lower interest rates of the home equity loans¹¹. While these investment flows significantly inflated loan volumes in several periods, they did not fundamentally alter the underlying APR structures. These episodes indicate that while the home equity market may be a valid control for price analysis, it may lead to biased counterfactual for volume analysis under a standard DiD framework.

1.4.3 Synthetic difference-in-differences

To address the non-parallel volume trends identified in the previous section, we employ a Synthetic Difference-in-Differences (SDiD) model. The key idea is to re-weight both donor units and time periods so that the weighted pre-treatment trajectory of the control group matches that of the treated unit as closely as possible. This produces a synthetic counterfactual that more plausibly satisfies the parallel trends assumption for loan volumes than the standard DiD approach. The method, proposed by Arkhangelsky et al.

¹¹The National Bank of Hungary noted this shift in its September 2023 *Trends in Lending* report, stating that "...home equity lending expanded significantly... driven mainly by investment motivations, based on the composition of borrowers and the characteristics of the loans granted" // <https://www.mnb.hu/en/publications/reports/trends-in-lending/trends-in-lending-september-2023>

Figure 1.6: APR (top) and logarithm of volumes (bottom) of mortgage and home equity loans by client categories



Note: The figure shows quarterly trends in APR (top panels) and the natural logarithm of disbursed loan volumes (bottom panels) for housing loans (left panels) and home equity loans (right panels), separately by quintile of the product-borrower segment indicator. Lines run from darker (quintile 1, least risky, most affluent borrowers) to lighter (quintile 5, riskiest, mass market borrowers). The two vertical dashed lines indicate October 2023 (start of the APR cap) and June 2024 (end of the APR cap). The top panels motivate the parallel trends assumption for APR; the bottom panels illustrate divergent pre-trends for loan volumes, justifying the use of the Synthetic DiD approach for volume estimation. Source: Magyar Nemzeti Bank (MNB) Credit Register.

(2021), is well-suited to settings with a small number of treated units and a larger donor pool, and has the additional advantage of down-weighting pre-treatment periods that are less informative about the counterfactual trend.

The estimation is based on the following optimization:¹²

$$(\hat{\tau}^{sdid}, \hat{\mu}, \hat{\alpha}, \hat{\beta}) = \underset{\tau, \mu, \alpha, \beta}{\operatorname{argmin}} \left\{ \sum_{i=1}^N \sum_{t=1}^T (Y_{i,t} - \mu - \alpha_i - \beta_t - W_{i,t}\tau)^2 \hat{\omega}_i^{sdid} \hat{\lambda}_t^{sdid} \right\} \quad (1.6)$$

where $\hat{\tau}^{sdid}$ is the average causal effect, μ is the difference between treatment and control groups, α_i is group specific fixed effect, $W_{i,t}$ is the treatment indicator, $\hat{\omega}_i^{sdid}$ is a unit specific weight, while $\hat{\lambda}_t^{sdid}$ is a time specific weight.

We use the SDiD estimator for two distinct outcome variables and two distinct donor pools, resulting in four main specifications. For the APR outcome, we first use home equity loan quintiles as the donor pool — closely paralleling the standard DiD of Section 4.2 but allowing for synthetic re-weighting — and then use EU member states' mortgage loan markets as an alternative donor pool. For the loan volume outcome, we rely exclusively on the international donor pool, since the home equity loan volume trends are distorted by arbitrage activity as discussed in Section 4.2. To verify the validity of each specification, we report spaghetti plots of the pre- and post-treatment gaps between treated and synthetic control units (Figures 1.11, 1.12, 1.13), and conduct exact Fisher permutation tests by randomly reassigning the treatment indicator across donor units to construct an empirical null distribution. This permutation-based inference is robust to the small number of donor units in each pool.

¹²Standard panel DiD estimation is similar and consistent with the following minimization problem:

$$(\hat{\tau}^{did}, \hat{\mu}, \hat{\alpha}, \hat{\beta}) = \underset{\tau, \mu, \alpha, \beta}{\operatorname{argmin}} \left\{ \sum_{i=1}^N \sum_{t=1}^T (Y_{i,t} - \mu - \alpha_i - \beta_t - W_{i,t}\tau)^2 \right\} \quad (1.5)$$

Selection of control units

The construction of the donor pool for the SDiD model follows a rigorous selection process to ensure the validity of the synthetic counterfactual. We utilize newly disbursed mortgage loan volumes from European Union member states as potential control units. For both Synthetic Control (SC) and SDiD frameworks, the estimator's reliability depends on selecting donors that shared common pre-treatment shocks with the treated unit but remained insulated from large, idiosyncratic shocks during the treatment period. As noted by Abadie 2021 and Arkhangelsky 2021, inclusion of units with divergent post-treatment shocks can lead to significant bias, as the model may incorrectly attribute the control group's idiosyncratic movements to the treatment effect itself.

To mitigate the risk of spurious trend matching, we restrict the donor pool to countries with strong macroeconomic and structural linkages to Hungary. We specifically focus on the Central and Eastern European (CEE) and Baltic regions, as these markets share similar macroeconomic developments. Furthermore, Germany is included in the donor pool due to its profound trade linkages with Hungary. By selecting countries subject to common regional and macroeconomic factors, the model is better equipped to filter out noise and isolate the true policy impact of the APR cap¹³.

The donor pool includes all EU member states' time series that are neighbouring or close to Hungary. There is no threat of distorting idiosyncratic shocks regarding Austria, Slovenia, Romania and Czech Republic between 2020 and 2024. In case of Slovakia there have been a few policy interventions on the mortgage loan market. Both DSTI and DTI limits were tightened between 2020 and 2024 that could affect demand for mortgage loans. DSTI limits decreased in 2020 from 80 to 60 per cent¹⁴ while DTI limits decreased for borrowers older than forty years old during 2022¹⁵ Bank taxes also changed during the observed period (bank levy repealed in 2021 new bank tax introduced in 2023) which

¹³As a robustness test we also use the product-borrower groups of home equity loans as donor pool to estimate the impact on APR. We do not use this donor pool to estimate volume effect

¹⁴<https://nbs.sk/en/news/statement-from-the-nbs-bank-boards-24th-meeting-of-2019/>

¹⁵<https://nbs.sk/en/news/new-rules-on-lending-to-households/>

may have had an impact on banks' loan supply.

Croatia introduced the euro on January 1 2023. There was a run-up on the mortgage loan market at the end of 2022 as borrowers expected higher interest rates in 2023¹⁶. Moreover, the Croatian government introduced a special scheme of housing loan subsidy in 2017 that is open for applications only in a certain period of the year¹⁷ leading to strong seasonality of the newly disbursed loan volumes. As a result we leave out Croatia from our donor pool as the Euro adoption and the seasonal nature of the housing loan subsidy might have caused divergent lending trends compared to the Hungarian mortgage loan market.

Although Polish mortgage loan market could also serve as a good control due to the similar level of economic development the introduction of 2% Safe Mortgage program in mid 2023 makes it necessary to leave out from the pool. The program let banks to offer state subsidized loans on 2 per cent interest rate (the gap between market rate and 2 per cent was funded by the state). The program considerably boosted mortgage loan outflow in the second half of 2023¹⁸.

The market interventions of the Baltic states not necessarily affected loan outflow. Latvia implemented a loan payment subsidy for 2024 as a one-off to support those borrowers whose mortgage loan was disbursed not later than 31 October 2023¹⁹. The program was in effect only during 2024. The subsidies that were paid by the state were funded by a 0.5 per cent bank levy on credit institutions' total outstanding loan balance as of 31 October 2023.

Lithuania introduced a temporary tax on banks' net interest income in May 2023. Lithuanian government aimed to collect a certain fraction of the extraordinary high profit of

¹⁶<https://www.hnb.hr/documents/20182/4643477/e-fs-24.pdf/b532e4b8-82b0-56f0-f908-634b8e619756>

¹⁷<https://mpgi.gov.hr/naslovna-blokovi-133/about-the-ministry-139/scope-of-the-ministry/housing-8652/subsidisation-of-housing-loans/8653>

¹⁸More details about the program can be found in the Financial Stability Report of Poland May 2024 p22-24: <https://nbp.pl/wp-content/uploads/2024/12/Raport-o-stabilnosci-systemu-finansowego.-Grudzien-2024-EN.pdf>

¹⁹<https://www.saeima.lv/en/news/saeima-news/32947-mortgage-borrowers-will-receive-30-compensation-of-total-interest-payments> // <https://www.elibrary.imf.org/view/journals/001/2024/143/article-A001-en.xml>

banks during the time of very high interest rates to fund national security expenses²⁰. Although the tax is temporary it was extended for 2025 also. The base of the tax is the difference between current and the average net interest income (NII) of 2019-2022 period and the tax rate is 60 per cent. According to the view of Lietuvos Bankas the main source of the extremely high net interest income is the high liquidity of the banking sector held at the central bank and coupled with low interest rates on deposits²¹. It is important to underline that new loans are exempt from the tax base²².

Austria tightened its macroprudential framework in 2022, introducing binding limits of 90 per cent on the loan-to-value ratio and 40 per cent on the debt service-to-income ratio for new mortgage loans. While this regulatory change may affected the level of newly disbursed mortgage volumes in Austria, we argue it does not disqualify Austria from the donor pool for two reasons. First, the tightening took effect before our pre-treatment window and is therefore fully absorbed into the pre-treatment trend that the SDiD estimator uses to construct the synthetic counterfactual. Second, the nature of the Austrian intervention - a demand-side quantity restriction - is fundamentally different from the Hungarian price cap, which operated through the interest rate channel. As a result, the Austrian mortgage market was not subject to a treatment-like shock during the October 2023 – June 2024 program period, and its inclusion in the donor pool does not threaten the validity of the synthetic control.

Most of the interventions regarding Slovakian, Latvian and Lithuanian banking sector can be considered by banks as sunk costs, therefore they potentially not affect the amount of newly disbursed mortgage loans. Nevertheless, as a robustness test we also run our main specification without the time series of these countries.

Finally, we have nine countries in the donor pool: Austria, Slovakia, Slovenia, Romania,

²⁰<https://finmin.lrv.lt/en/news/ministry-of-finance-a-one-year-extension-of-the-bank-solidarity-contribution-will-make-an-input-to-everyones-security/>

²¹Lietuvos Bankas' Financial Stability Review 2023, box 1: https://www.lb.lt/uploads/publications/docs/42236_f7dfb653eb

²²IMF 2023 Article IV consultation staff report, "Statement by Vitas Vasiliauskas, Executive Director for the Republic of Lithuania and Raido Kraavik, Advisor to the Executive Director", p3: <https://www.imf.org/-/media/files/publications/cr/2023/english/1ltuea2023002.pdf>

Czech Republic, Estonia, Latvia, Lithuania and Germany.

1.4.4 Regression discontinuity design

As a second identification strategy, we employ a regression discontinuity (RD) design. We compare borrowers who originated loans immediately prior to the implementation of the cap with those who did so immediately after. We premise this on the assumption that these two groups of borrowers share similar characteristics, differing only in that the latter group originated their loans while the policy was in effect. The identifying assumption of this approach is that, in the absence of the program, outcome variables (e.g., interest rates) would evolve smoothly across the cutoff. Formally, for an outcome Y , we estimate:

$$\tau_y = \lim_{z \rightarrow 0^+} \mathbb{E}[Y|Z_i = z] - \lim_{z \rightarrow 0^-} \mathbb{E}[Y|Z_i = z], \quad (1.7)$$

where z is the difference between the date of origination and October 9, 2023, the cutoff, that is the day of implementation of the APR cap. We estimate (1.7) by local linear regressions. We use triangular kernel with MSE-optimal bandwidth following Imbens and Kalyanaraman (2012) and Calonico, Cattaneo, and Titiunik (2014).

To ensure accurate identification, we refine our sample to exclude a short transition period immediately following the program's introduction. This exclusion is necessary because the loan origination process involves a latency of several weeks. Regulations mandate that loan offers remain valid for at least two weeks; furthermore, administrative procedures such as credit risk screening may add several days to the process. Consequently, a loan originated immediately after the cutoff may still reflect terms agreed upon prior to the cap (e.g., if a loan was already in the pipeline, the bank may honor the pre-agreed rate even if it exceeds the new APR cap). Furthermore, borrowers face contractual deadlines regarding property purchases, preventing them from easily restarting the loan application process to secure better terms.

To address these transitional frictions, we implement a “donut” regression discontinuity design following Barreca, Lindo, and Waddell (2011). This specification excludes observations in the immediate vicinity of the cutoff—specifically, the two-week period following the policy implementation—to ensure that our estimates are not biased by loans that were negotiated prior to the cap but originated shortly after. While our primary specification eliminates this two-week transition window, we also conduct robustness checks excluding intervals of 4, 6, and 8 working days.

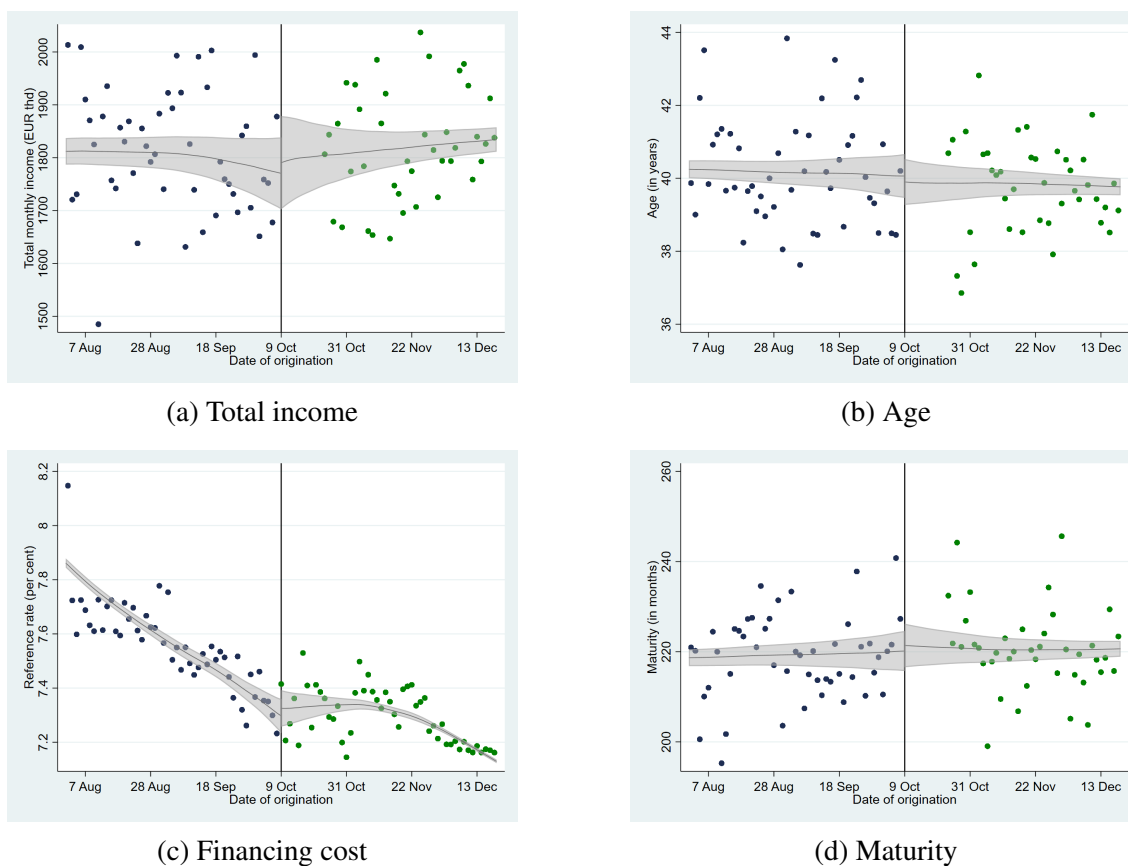
Table 1.5: Balance test

	Without cap	With cap	Standard deviation	Difference	p value
Wage (EUR)	1,979	1,969	1,158	-10	0.79
Age (year)	39.2	39.0	9.2	-0.1	0.65
Loan amount (EUR)	44,200	44,453	33,164	252	0.82
Maturity (year)	19.1	19.1	5.9	0.0	0.93
LTV (per cent)	50.8	51.3	20.7	0.5	0.47
DSTI (per cent)	32.2	32.1	11.0	-0.1	0.71

Note: The table reports a covariate balance test for the regression discontinuity design. The sample is restricted to housing loans originated within the MSE-optimal bandwidth around the October 9, 2023 policy cutoff, excluding a two-week donut window immediately after the cutoff. “Without cap” refers to loans originated before October 9, 2023; “With cap” refers to loans originated after. Wage is the monthly income of borrowers converted to EUR using the average EUR/HUF exchange rate. The difference column reports the raw difference in means between the two groups. The p-value is from a two-sample t-test of the null hypothesis that the group means are equal. The absence of statistically significant differences supports the validity of the RD identification assumption.

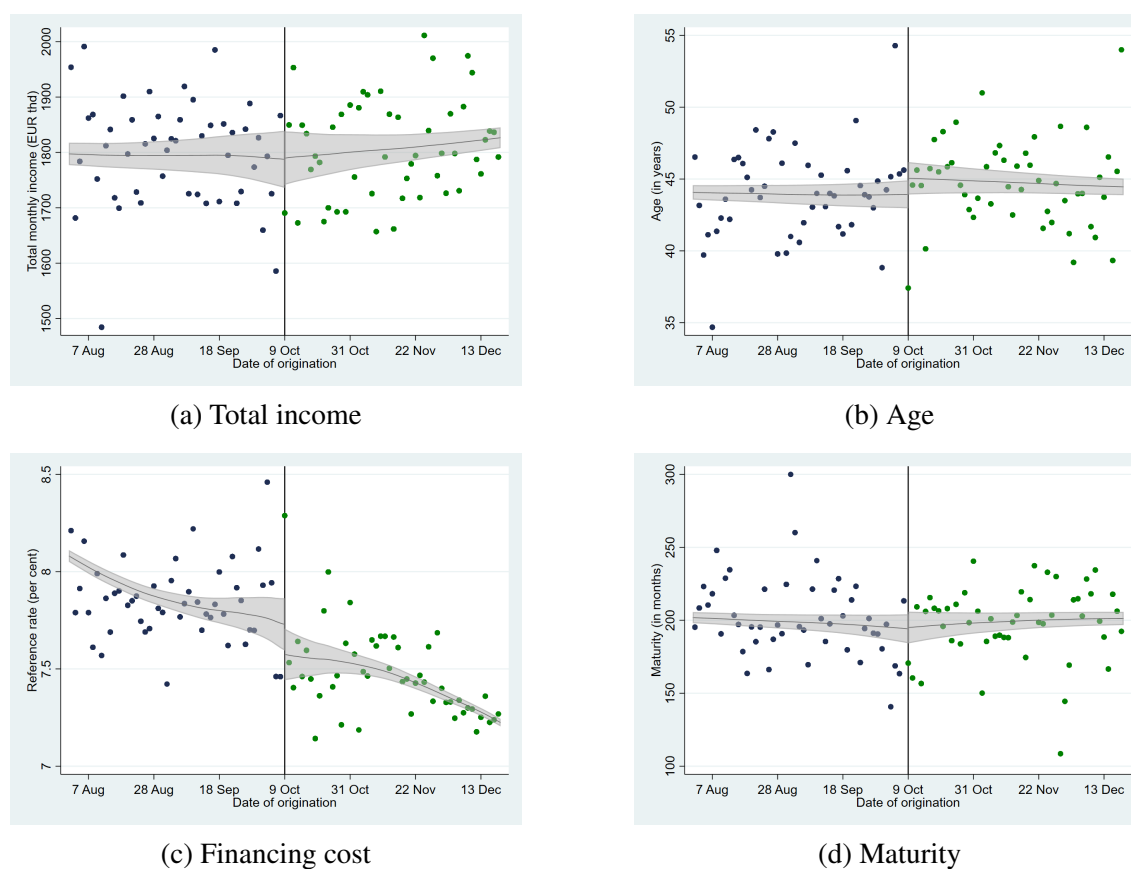
We demonstrate that the main characteristics of loans and borrowers remain balanced within a narrow window around the program’s introduction. This stability supports the validity of the underlying RD assumption: that in the absence of the program, the time series of interest would evolve smoothly, and no discontinuity would be observed at the cutoff.

Figure 1.7: Main borrower and loan characteristics of newly granted mortgage loans around the implementation of the APR cap



Note: The figure shows binscatter plots of key borrower and loan characteristics against the date of origination for newly granted housing loans, within the MSE-optimal bandwidth around the October 9, 2023 APR cap implementation date (vertical line). Panel (a): total monthly income of all borrowers on the contract (EUR). Panel (b): average age of borrowers (years). Panel (c): reference interest rate (financing cost, per cent). Panel (d): maturity (months). Black dots indicate loans originated before the cutoff; green dots indicate loans originated after. Grey shaded areas represent 95% confidence intervals from local linear regression with triangular kernel. The absence of discontinuities supports the covariate balance required for the RD identification assumption. Source: Magyar Nemzeti Bank (MNB) Credit Register.

Figure 1.8: Main borrower and loan characteristics of newly granted home equity loans around the implementation of the APR cap



Note: The figure shows binscatter plots of key borrower and loan characteristics against the date of origination for newly granted home equity loans, within the MSE-optimal bandwidth around the October 9, 2023 APR cap implementation date (vertical line). Panel (a): total monthly income of all borrowers on the contract (EUR). Panel (b): average age of borrowers (years). Panel (c): reference interest rate (financing cost, per cent). Panel (d): maturity (months). Black dots indicate loans originated before the cutoff; green dots indicate loans originated after. Grey shaded areas represent 95

1.5 Results

In the next section we show the results of the various estimations outlined in the previous section. DiD estimation is the most traditional in our problem, therefore we start with that. This estimation also lets us say something about the heterogeneity in the impact. Nevertheless, as the assumption of parallel trends may not hold in case of loan volumes, we let the more flexible Synthetic DiD estimator to construct valid control groups. Luckily, we can repeat the estimation on APR using the same control groups as in the standard DiD exercise and we show that results are consistent. That fact encourages us to turn to a differently selected donor pool that consists of international mortgage loan market data. Results of estimations on international data are consistent with results on APR using only Hungarian home equity loans as control group in case of the standard DiD and the SDiD exercises. Finally, we estimate a SDiD on the logarithm of loan volumes using international donor pool. Results remain significant and show large heterogeneity in the effect of the program on loan volumes: Volumes of highest product-borrower segment increased the most while lowest segment loan volumes shrank considerably, i.e. a sign of crowding out effect. As a robustness test, we also estimate a RDD. The advantage of such an estimation is that it does not rely on a control group that consists of different type of loans. Moreover, we can also test if there was any immediate impact of the program on home equity loans by running RDD on that dataset only. The drawback of this approach is twofold: firstly, it can show only the immediate impact of the program that may not capture the total impact. Secondly, as we expect loan volumes increase with a certain lag after the introduction of the program (due to the fact that taking out housing loans may take several weeks or even months), we cannot test the effect on loan volumes. Having equipped with all these results we turn to a simple model of loan demand and try to give an indication about the welfare effect of the program.

1.5.1 Difference in differences

The DiD estimation shows that the total impact of the program during the 3 months it was in effect is 1.69 percentage points. There is a considerable heterogeneity even on the best product-borrower segment loans, which suggests that even those loans' interest rate decreased that would have been below the cap in the absence the program. Estimated effect of the program on the best product-borrower segment is one percentage point decrease, while impact on the lowest product-borrower segment is 2.3 percentage point decrease. Including a large set of covariates that we used when we created the client-borrower indicator does not change the result significantly see 1.12 in Appendix 1.B. Unfortunately, as discussed earlier the parallel trend assumption may not hold in case of the volumes (see 1.10). Therefore we turn to the synthetic control approach that can improve on this aspect of the estimation.

Table 1.6: Difference-in-differences estimation results by quintiles

	(1)	(2)	(3)	(4)	(5)	(6)
	Total market	1st quintile	2nd quintile	3rd quintile	4th quintile	5th quintile
Post x Mortgage	-1.139*** (0.0829)	-0.577*** (0.0805)	-0.659*** (0.0811)	-0.776*** (0.0837)	-1.240*** (0.0937)	-2.016*** (0.132)
Observations	171389	46843	46392	46018	45580	45219
R^2	0.753	0.872	0.859	0.845	0.796	0.727
Time FE	Yes	Yes	Yes	Yes	Yes	Yes
Bank FE	Yes	Yes	Yes	Yes	Yes	Yes
District FE	Yes	Yes	Yes	Yes	Yes	Yes

Standard errors clustered on county x bank level in parentheses

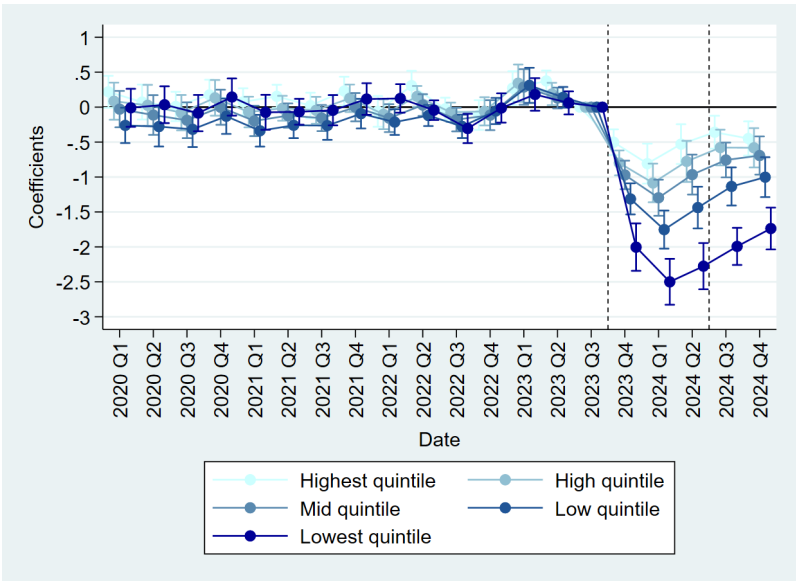
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

The table reports difference-in-differences estimates of the effect of the APR cap on the annual percentage rate (APR, in percentage points) of newly disbursed housing loans. The treatment group consists of housing loans; the control group consists of home equity loans. Post x Mortgage is the interaction term capturing the effect of the cap on housing loan APRs relative to home equity loans. Columns (1)–(6) correspond to the full sample and quintiles 1–5 of the product-borrower segment indicator, respectively, where quintile 1 contains the least risky (most affluent) borrowers and quintile 5 contains the riskiest (mass market) borrowers. All specifications include time, bank, and district fixed effects.

1.5.2 Synthetic difference-in-differences estimates

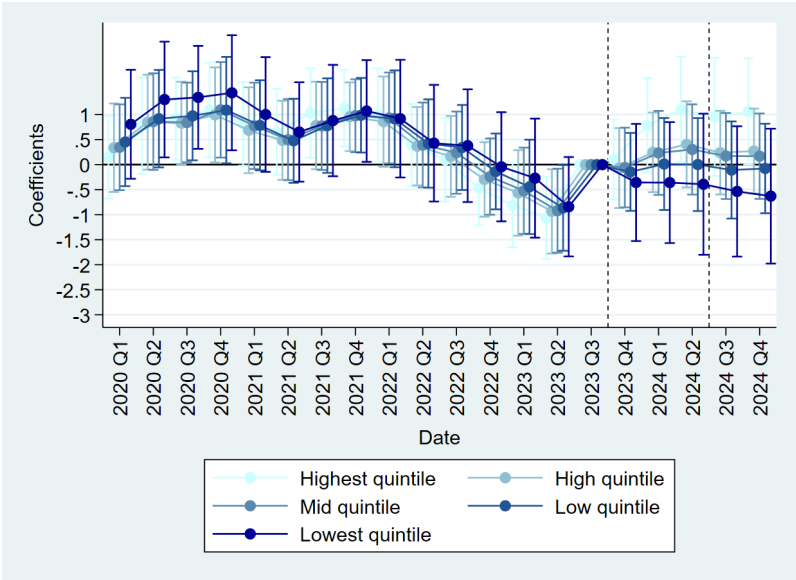
We present the SDiD results in three steps, moving from the APR outcome to the volume outcome, and from the home equity loan donor pool to the international donor pool. This

Figure 1.9: Estimated impact of APR cap on APR by quintiles of product-borrower segment



Note: The figure shows the event-study coefficients from the difference-in-differences estimation of the effect of the APR cap on the annual percentage rate (APR, in percentage points) separately for each quintile of the product-borrower segment indicator. Quintile 1 (Highest quintile) contains the least risky, most affluent borrowers; quintile 5 (Lowest quintile) contains the riskiest, mass market borrowers. Coefficients are estimated quarterly relative to 2023 Q3 (the last pre-treatment period, normalized to zero). The two vertical dashed lines indicate the start (2023 Q4) and end (2024 Q2) of the APR cap. Error bars represent 95

Figure 1.10: Estimated impact of APR cap on loan volumes by quintiles of product-borrower segment



Note: The figure shows the event-study coefficients from the difference-in-differences estimation of the effect of the APR cap on the natural logarithm of newly disbursed housing loan volumes, separately for each quintile of the product-borrower segment indicator. Quintile 1 (Highest quintile) contains the least risky, most affluent borrowers; quintile 5 (Lowest quintile) contains the riskiest, mass market borrowers. Coefficients are estimated quarterly relative to 2023 Q3 (the last pre-treatment period, normalized to zero). The two vertical dashed lines indicate the start (2023 Q4) and end (2024 Q2) of the APR cap. Note that the parallel trends assumption for loan volumes may be violated for home equity loans as control group; the SDiD results in Table 9 provide the preferred volume estimates. Error bars represent 95

progression serves two purposes: it allows us to validate the SDiD approach against the standard DiD results on APR, and it motivates the switch to the international donor pool for the volume estimation.

APR estimates using home equity loans as donor pool

We first estimate the effect of the program on APR using home equity loan quintiles as the donor pool. This specification is directly comparable to the DiD estimates of Section 5.1, but replaces the equal-weighting assumption with synthetic re-weighting. The SDiD estimator assigns higher weight to those home equity loan quintiles whose pre-treatment APR trajectory most closely resembles that of the treated housing loan quintiles; Figure 21a shows the resulting unit weights.

The estimated effects are somewhat smaller than the DiD results: the average treatment effect across all groups is -0.921 percentage points (Table 7), compared to -1.1 percentage points in the DiD (Table 6). The heterogeneity pattern across quintiles is preserved: the estimated effect is smallest for quintile 1 (-0.560 percentage points) and largest for quintile 5 (-1.905 percentage points), consistent with the DiD results. The spaghetti plot in Figure 11 shows that pre-treatment gaps are close to zero across all treated quintiles, supporting the validity of the parallel trends assumption for APR.

APR estimates using EU member states as donor pool

As a further robustness check, we re-estimate the APR effect using EU member states' mortgage loan markets as the donor pool. This specification is entirely independent of the home equity loan control group and therefore provides external validation of the APR results. The donor pool selection follows the criteria described in Section 4.3.1; Figure 21b shows the resulting country weights. We use the two-period lagged value of Hungarian housing loan APR as the treated series. The spaghetti plot in Figure 12 confirms that pre-treatment gaps are close to zero.

Table 1.7: Results of Synthetic Difference-in-Differences estimations APR with home equity loans as control group

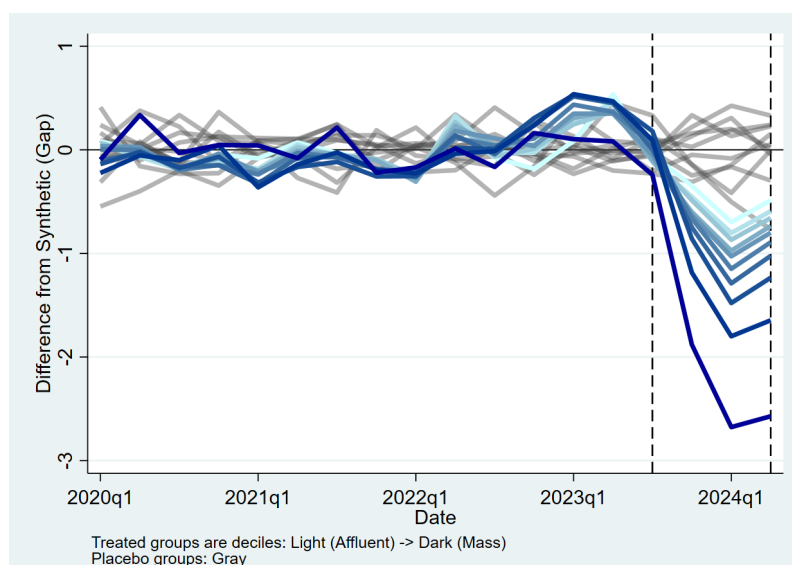
	(1)	(2)	(3)	(4)	(5)	(6)
	All groups	1st quintile	2nd quintile	3rd quintile	4th quintile	5th quintile
Program period	-0.921*** (0.0568)	-0.560*** (0.0823)	-0.712*** (0.0823)	-0.827*** (0.0823)	-0.999*** (0.0823)	-1.905*** (0.0823)
Observations	360	216	216	216	216	216
Fisher perm. test p value	-	0.022	0.022	0.022	0.022	0.022
R^2						
Covariates	No	No	No	No	No	No
Time FE	Yes	Yes	Yes	Yes	Yes	Yes

Standard errors in parentheses are based on placebo permutation inference

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Note: The table reports Synthetic Difference-in-Differences (SDiD) estimates of the effect of the APR cap on the annual percentage rate (APR, in percentage points) of housing loans, using home equity loan quintiles as the donor pool. Program period is the average treatment effect during October 2023 – June 2024. Columns (1)–(6) correspond to all groups pooled and quintiles 1–5 of the product-borrower segment indicator, respectively, where quintile 1 contains the least risky borrowers and quintile 5 the riskiest. The SDiD estimator re-weights control units to construct a synthetic parallel trend. Fisher permutation test based p values show the exact Fisher permutation inference, in which the treatment indicator is randomly reassigned across units to construct an empirical null distribution of the estimator. This approach is robust to the small number of donor units in the home equity loan donor pool.

Figure 1.11: Spaghetti plot of APR with home equity loans as controls



Note: The figure shows the spaghetti plot underlying the Synthetic Difference-in-Differences (SDiD) estimation of the APR effect using home equity loan quintiles as the donor pool (Table 7). Each line represents the gap between a treated housing loan quintile (or a placebo control unit) and its synthetic control. Treated groups are quintiles of the product-borrower segment indicator, running from light blue (quintile 1, least risky, most affluent) to dark blue (quintile 5, riskiest, mass market). Grey lines are placebo gaps for the control units. The two vertical dashed lines mark the start (2023 Q4) and end (2024 Q2) of the APR cap. Pre-treatment gaps close to zero and parallel trends support the validity of the SDiD identification. Source: Magyar Nemzeti Bank (MNB) Credit Register.

The estimated effects remain significant and are broadly consistent with the home equity loan results, though somewhat smaller in magnitude (Table 8). The average treatment effect across all groups is -0.700 percentage points, and the quintile heterogeneity pattern

is preserved. The consistency of APR results across two independent donor pools — domestic home equity loans and international mortgage markets — provides strong support for the validity of the identification strategy.

Table 1.8: Results of Synthetic Difference-in-Differences estimations APR with international control group

	(1)	(2)	(3)	(4)	(5)	(6)
	All groups	1st quintile	2nd quintile	3rd quintile	4th quintile	5th quintile
Program period	-0.700*** (0.266)	-0.560*** (0.182)	-0.595*** (0.175)	-0.677*** (0.175)	-0.787*** (0.175)	-0.896*** (0.175)
Observations	342	162	198	198	198	198
Fisher perm. test p value	-	0.027	0.027	0.027	0.027	0.027
R^2						
Covariates	No	No	No	No	No	No
Time FE	Yes	Yes	Yes	Yes	Yes	Yes

Standard errors in parentheses are based on placebo permutation inference

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

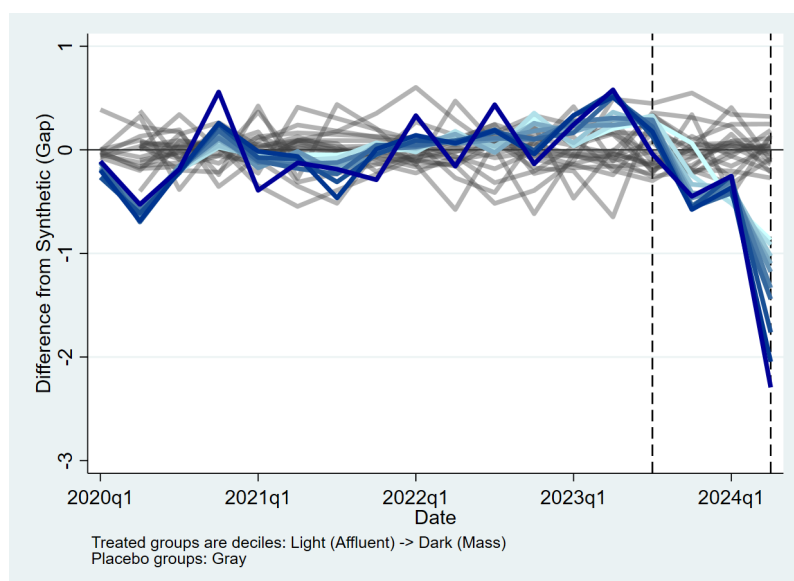
Note: The table reports Synthetic Difference-in-Differences (SDiD) estimates of the effect of the APR cap on the annual percentage rate (APR, in percentage points) of housing loans, using EU member states' mortgage loan markets as the donor pool. The donor pool includes Austria, Slovakia, Slovenia, Romania, Czech Republic, Estonia, Latvia, Lithuania, and Germany. Program period is the average treatment effect during October 2023 – June 2024. Two-period lagged Hungarian housing loan volumes are used to satisfy the parallel trends assumption. Columns (1)–(6) correspond to all groups pooled and quintiles 1–5 of the product-borrower segment indicator, respectively, where quintile 1 contains the least risky borrowers and quintile 5 the riskiest. Standard errors in parentheses are based on placebo permutation inference. Fisher permutation test p values show the exact Fisher permutation inference, in which the treatment indicator is randomly reassigned across units to construct an empirical null distribution of the estimator. This approach is robust to the small number of donor units in the home equity loan donor pool.

Volume estimates using EU member states as donor pool

Finally, we estimate the effect of the program on the natural logarithm of loan volumes using the international donor pool. As discussed in Section 4.2, the home equity loan donor pool is not suitable for volume estimation due to arbitrage-driven distortions in the pre-treatment period. The spaghetti plot in Figure 13 confirms that the international donor pool achieves a good pre-treatment fit for loan volumes.

The results reveal a strong and heterogeneous effect of the program on loan volumes (Table 9). The average treatment effect on the total market is a log increase of 0.462. This aggregate expansion masks a striking distributional pattern: the estimated log volume change is largest and positive for the best product-borrower segment (0.702, quintile 1) and turns negative for the riskiest segment (-0.283, quintile 5). This monotone pattern — whereby the program expanded volumes for low-risk borrowers while contracting them

Figure 1.12: Spaghetti plot of change of APR with international markets as controls



Note: The figure shows the spaghetti plot underlying the Synthetic Difference-in-Differences (SDiD) estimation of the APR effect using EU member states' mortgage loan markets as the donor pool (Table 8). Each line represents the gap between a treated Hungarian housing loan quintile (or a placebo donor country) and its synthetic control. Treated groups run from light blue (quintile 1, least risky) to dark blue (quintile 5, riskiest). Grey lines are placebo gaps for donor countries. The donor pool includes Austria, Slovakia, Slovenia, Romania, Czech Republic, Estonia, Latvia, Lithuania, and Germany. The two vertical dashed lines mark the start (2023 Q4) and end (2024 Q2) of the APR cap. Data sources: European Central Bank Statistical Data Warehouse; European Mortgage Federation Quarterly Review; Magyar Nemzeti Bank Credit Register.

for high-risk borrowers — is the key evidence of the crowding-out effect.

This finding may appear surprising at first: the cap lowered interest rates most for the riskiest borrowers (as shown in Tables 6 and 7), yet their loan volumes fell. The explanation is credit rationing: banks were unwilling to serve the riskiest borrowers at the capped rate because the loan would be loss-making given the high cost of risk in those segments. The monotonicity of the volume estimates across quintiles corroborates this interpretation and suggests that the product-borrower segment indicator successfully ranks borrowers by their effective riskiness from the banks' perspective, even if it remains an imperfect categorization.

1.5.3 Regression discontinuity

Results of the donut RDD estimates reinforce previous findings. Estimated impacts are smaller (around half of DiD and SDiD results) due to the fact that we can only measure

Table 1.9: Results of Synthetic Difference-in-Differences estimations of Ln(loop volumes)

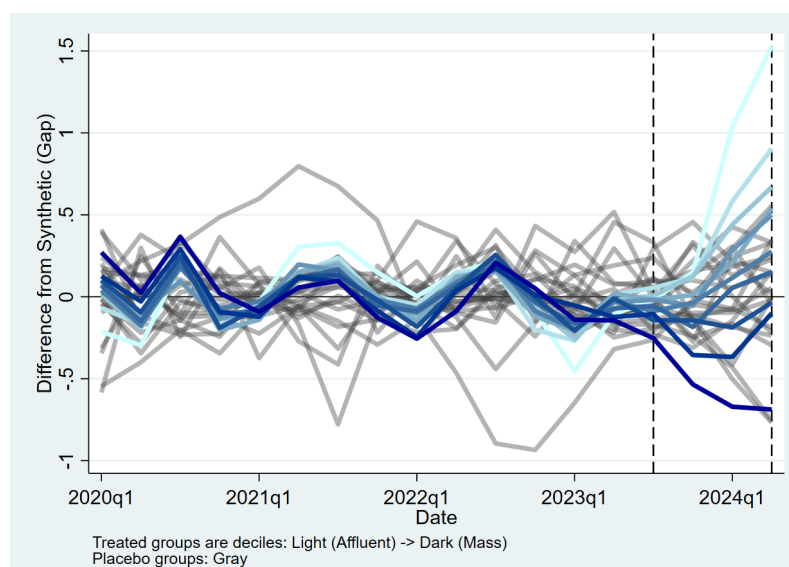
	(1)	(2)	(3)	(4)	(5)	(6)
	Total market	1st quintile	2nd quintile	3rd quintile	4th quintile	5th quintile
Program period	0.462*** (0.154)	0.702*** (0.108)	0.338*** (0.108)	0.230** (0.108)	0.0320 (0.108)	-0.283*** (0.108)
Observations	180	198	198	198	198	198
Fisher perm. test p value	-	0.027	0.054	0.162	0.73	0.027
R^2						
Covariates	No	No	No	No	No	No
Time FE	Yes	Yes	Yes	Yes	Yes	Yes

Standard errors in parentheses are based on placebo permutation inference

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Note: The table reports Synthetic Difference-in-Differences (SDiD) estimates of the effect of the APR cap on the natural logarithm of newly disbursed housing loan volumes, using EU member states' mortgage loan markets as the donor pool. The donor pool includes Austria, Slovakia, Slovenia, Romania, Czech Republic, Estonia, Latvia, Lithuania, and Germany. Program period is the average treatment effect during October 2023 – June 2024. Columns (1)–(6) correspond to the total market and quintiles 1–5 of the product-borrower segment indicator, respectively, where quintile 1 contains the least risky (most affluent) borrowers and quintile 5 contains the riskiest (mass market) borrowers. A positive coefficient indicates an expansion of loan volumes; a negative coefficient indicates contraction (crowding-out). Fisher permutation test p values show the exact Fisher permutation inference, in which the treatment indicator is randomly reassigned across units to construct an empirical null distribution of the estimator. This approach is robust to the small number of donor units in the home equity loan donor pool.

Figure 1.13: Spaghetti plot of Ln(loop volume) with international markets as controls

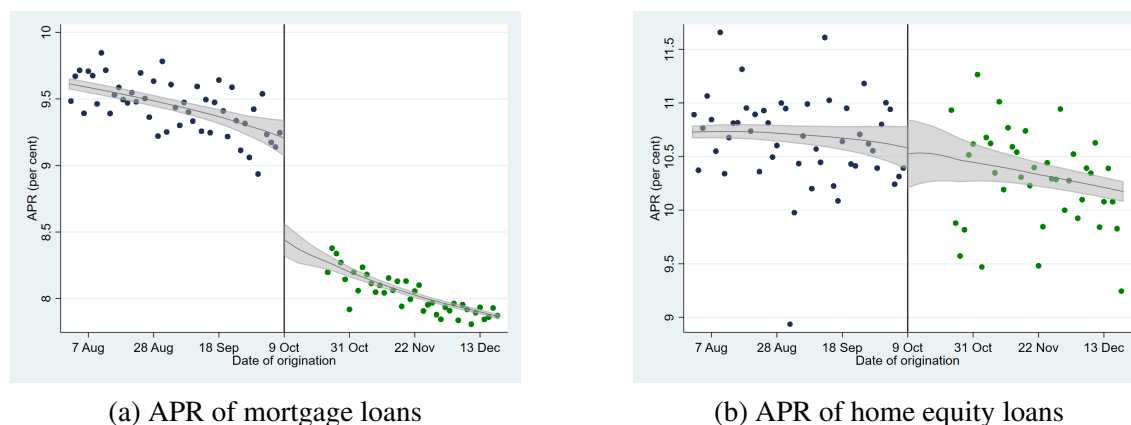


Note: The figure shows the spaghetti plot underlying the Synthetic Difference-in-Differences (SDiD) estimation of the loan volume effect using EU member states' mortgage loan markets as the donor pool (Table 9). Each line represents the gap in the natural logarithm of newly disbursed mortgage loan volumes between a treated Hungarian housing loan quintile (or a placebo donor country) and its synthetic control. Treated groups run from light blue (quintile 1, least risky) to dark blue (quintile 5, riskiest). Grey lines are placebo gaps for donor countries. The donor pool includes Austria, Slovakia, Slovenia, Romania, Czech Republic, Estonia, Latvia, Lithuania, and Germany. The two vertical dashed lines mark the start (2023 Q4) and end (2024 Q2) of the APR cap. Data sources: European Central Bank Statistical Data Warehouse; European Mortgage Federation Quarterly Review; Magyar Nemzeti Bank Credit Register.

the immediate impact of the policy with this approach. We also run some kind of placebo tests by running RDD on the home equity loans. This exercise is very useful as can

validate that home equity loans serve as good controls, because all placebo tests with this group are insignificant (we do not report the regression results but show the RDD chart as evidence 1.14a and 1.14b).

Figure 1.14: Average interest rate and loan volume of mortgage loans around the implementation of the APR cap



Note: The figure shows regression discontinuity plots of the APR (per cent) for housing loans (panel a) and home equity loans (panel b) around the October 9, 2023 APR cap implementation date. Each dot represents the mean APR within a one-week bin. Black dots indicate the pre-cutoff period; green dots the post-cutoff period. The grey shaded areas represent 95

Table 1.10: Results of RD estimations by quintiles of product-borrower segment

VARIABLES	(1) 1st quintile	(2) 2nd quintile	(3) 3rd quintile	(4) 4th quintile	(5) 5th quintile
Treatment effect	-0.0880	-0.418***	-0.694***	-1.051***	-2.040***
Observations	29764	27129	25572	24613	21860
Bandwidth	32.473	38.630	37.541	39.360	44.435

Note: The table reports donut regression discontinuity (RD) estimates of the immediate effect of the APR cap on the annual percentage rate (APR, in percentage points) of newly disbursed housing loans. The running variable is the number of calendar days relative to the October 9, 2023 implementation date. A two-week donut window immediately after the cutoff is excluded to account for pipeline loans negotiated before the cap. Bandwidth is selected using the MSE-optimal procedure of Calonico, Cattaneo, and Titiunik (2014). Columns (1)–(5) correspond to quintiles 1–5 of the product-borrower segment indicator, where quintile 1 contains the least risky borrowers and quintile 5 the riskiest. Local linear regression with triangular kernel is used. Robust bias-corrected standard errors are applied.

1.5.4 Distributional Consequences: Crowding Out and Redistribution

Was this policy a good intervention? Who gained and who lost? Was this policy fair, and how did it affect inequality? We use the empirically identified price and volume effects from our DiD and SDiD estimations to recover the implied demand curve for housing loans across borrower segments, and use it to characterize the redistributive impact

of the program. We are explicit that this is a back-of-the-envelope exercise: it relies on simplifying assumptions (constant elasticity and linear demand within the relevant range), and our goal is to establish the direction of redistribution — which is robust across a wide range of elasticity values — rather than to provide precise magnitudes.

Our DiD and SDiD estimates provide, for each product-borrower segment, a jointly identified price effect (change in APR) and volume effect (change in log loan volume). Under the assumption that these represent movements along a stable demand curve, we recover the implied demand elasticity as the ratio of the log volume change to the log price change for each segment. The implied elasticities are large in absolute value, ranging from approximately -3 to -4 across borrower segments — substantially above estimates typically reported in the mortgage demand literature.²³ We interpret this as reflecting the amplified demand response during the post-crisis recovery period, consistent with our theoretical model in the next section.

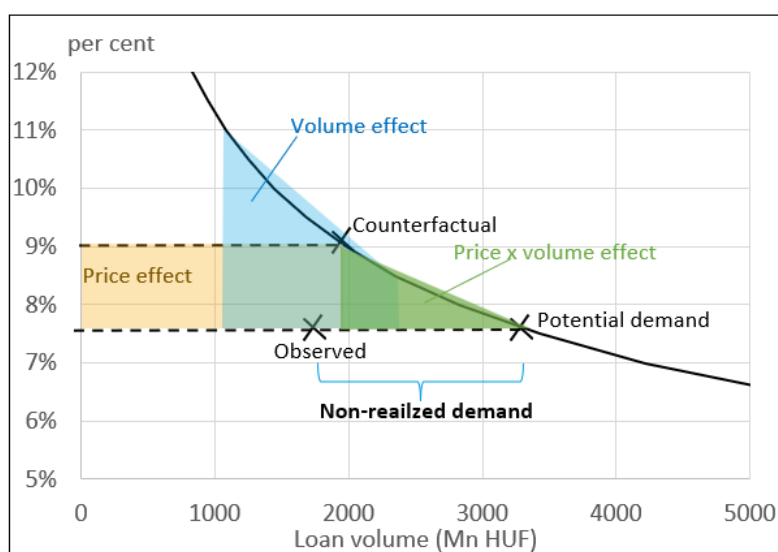
Back-of-the-envelope estimation of redistributive effects

Figure 1.15 illustrates the geometry of the consumer surplus decomposition schematically. For each product-borrower segment, we calculate counterfactual APR and loan volume figures using the DiD and SDiD estimates (Tables 6 and 9) and the observed pre-program values, and recover the demand curve by assuming that the counterfactual price-volume pair lies on it. The change in consumer surplus is decomposed into a price effect (lower rates for borrowers retaining market access), a volume effect (additional surplus from expanded lending), and a crowding-out effect (surplus lost due to market exclusion of high-risk borrowers). The observed change in loan volume — the net of crowding-out and new market entrants — serves as a lower bound for the total crowded-out volume.²⁴

²³For comparison, Cuesta and Sepulveda (2021) and Akgunduz et al. (2023) report elasticities in the range of -0.5 to -1.5 in non-crisis settings.

²⁴Consumer surplus is expressed in net present value terms: we compute the NPV of the cash flow stream of each loan using a discount rate 2 percentage points below the average financing cost (approximately 4.6 per cent), and divide by the loan volume to obtain a per-unit price measure. The crowding-out volumes are taken directly from the SDiD estimates in Table 9 without further modeling assumptions.

Figure 1.15: Schematic figure of quantifying redistributive impacts



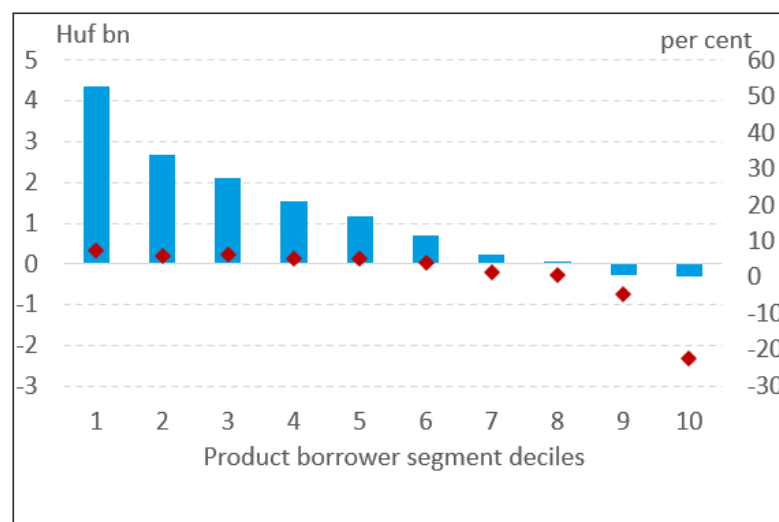
Note: The figure provides a schematic illustration of the consumer surplus decomposition used in the back-of-the-envelope analysis. The black curve represents the demand curve for housing loans. The counterfactual price-volume pair (without the cap) is marked with an “X” at approximately 9% APR. The observed price-volume pair under the cap is marked with an “X” at approximately 7.3% APR. The orange rectangle (“Increase of CS: price effect”) represents the consumer surplus gain from lower interest rates for borrowers who retain market access. The green triangle (“Price x volume effect”) represents the additional surplus from expanded loan volumes. The blue segment (“Non-realized demand”) represents borrowers who would take out loans on the given interest rate but are not served by banks due to their high risk. The analysis assumes constant demand elasticity and linear demand within the relevant range. Loan volume is in HUF million; APR is in per cent.

Figure 1.16 reports the total change in consumer surplus across the ten deciles of the product-borrower segment indicator (group 1 = least risky, most affluent; group 10 = riskiest, mass market borrowers). Blue bars show the absolute change in billion HUF; red diamonds show the change relative to the counterfactual loan value (per cent).

The central finding is robust to the choice of elasticity: the program generated a clear and monotone redistributive pattern in which gains accrued to low-risk, high-income borrowers while losses were concentrated among high-risk, low-income borrowers. The total change in consumer surplus is positive and large for borrowers in the top deciles (approximately 4.4 billion HUF for group 1, around 7 per cent of the counterfactual loan value) and negative for borrowers in the bottom deciles (approximately –0.3 billion HUF for group 10, around 22 per cent of the counterfactual loan value). This conclusion does not rest on the precision of our elasticity estimates: it follows directly from the monotone pattern of APR decreases (larger for riskier borrowers, Table 6) combined with the monotone pattern of crowding-out (larger volume losses for riskier borrowers, Table 9).

These two patterns together imply that the program was regressive in its distributional consequences, regardless of the exact welfare magnitudes.

Figure 1.16: Redistributive effects of the APR cap across borrower segments



Note: The figure reports the results of the back-of-the-envelope analysis across the ten deciles of the product-borrower segment indicator (group 1 = least risky, most affluent; group 10 = riskiest, mass market). Blue bars show the welfare change in billion HUF, red diamonds show the welfare change relative to the original (counterfactual) loan amount. The welfare calculation uses a discount rate 2 percentage points below the average financing cost (approximately 4.6%). Source: Authors' calculations based on Magyar Nemzeti Bank (MNB) Credit Register and DiD and SDiD estimates.

1.5.5 Model setup

We apply a nonsequential consumer search framework originated by Varian (1980) and Burdett and Judd (1983). Specifically, we consider the basic setting in Tappata (2009) extended with (1) price regulation (2) at a selection market (3) by a strategically behaving government. The second refers to markets where customers' characteristics affect the cost of the product, in our case the creditworthiness of borrowers.²⁵ As we use in the analysis only this specificity of the housing loan market, we derive relevant results for a broader set of transactions.

Identical banks, N in number, supply a homogeneous product (loan) to a continuum of borrowers of unit mass indexed by $i \in [0, 1]$. A housing loan contract involves a series of uncertain cost elements during the maturity, which we condense in the constant marginal

²⁵In contrast to the typical approach in this literature summarized recently in Einav et al. (2021), we assume that these characteristics are observable by the sellers, which simplifies the analysis a lot.

cost $c + d$ with zero fixed costs. The cost environment that is common for each bank and borrower can be high or low depending on the cyclical position of the economy: $c \in \{\underline{c}, \bar{c}\}$. We think of c as representing operating costs, financing costs and partly risk costs that are determined by the general macroeconomic stance. There are 3 types of borrowers with different creditworthiness d called safe, moderate, and risky with respective values $a_1 < a_2 < a_3$ and masses m_1, m_2 , and m_3 (where $\sum_{j=1}^3 m_j = 1$ and $0 < m_j$).²⁶ Banks observe costlessly not only c but also d of their borrowers.²⁷ Loan products differ only in their prices that are summarized in an interest rate r . Banks compete through prices by offering an interest rate to any borrowers who contact them. A loan contract between a bank and a borrower with risk type a_j in a cost environment c yields a profit of $r - c - a_j$ for the bank.²⁸

Each borrower has a unit demand with a common reservation price of v . Borrowers are not aware of the cost environment and form a belief that is high ($c = \bar{c}$) with probability α and low ($c = \underline{c}$) with the complementary probability $1 - \alpha$. They obtain information about market prices through all-or-nothing nonsequential search. Borrower i can choose between searching for all the available offers, which involves a fixed cost $s_i \in [0, \bar{s}]$ with $\bar{s} > 0$, and receiving one offer from a random bank for free.²⁹ Search cost s_i is a private information of the borrower. The following is common knowledge. Among each risk type a_j , there is a fraction λ of consumers called *shoppers* with zero search cost. The remaining $1 - \lambda$ *non-shoppers* have nonnegative search costs that are drawn from a differentiable and strictly increasing cumulative distribution function $G(s)$, $s \in [0, \bar{s}]$. A loan contract between a bank and a borrower with risk type a_j and searching decision $e_i \in \{0, 1\} = \{\text{do not search, search}\}$ in a cost environment c yields a utility of $v - r - e_i s_i$ for the borrower. No contract results in zero utility.

²⁶Borrowers are arranged in a monotonically increasing order: $d_i = a_j$ if $\sum_{k=0}^{j-1} m_k \leq i < \sum_{k=1}^j m_k$, where we introduce $m_0 = 0$.

²⁷This is ensured by an applicant assessment process just before signing the loan contract. If it involves some fixed cost, it can be considered as incorporated in c .

²⁸This can also be understood as an interest rate premium over a reference rate capturing all the costs of lending.

²⁹This searching protocol may be a good approximation of an optimal one, as searching at the housing loan market exhibits some economics of scale because of price-comparing websites, brokers, and the lengthy process of preliminary assessment of loan applicants.

Before banks and borrowers interact, the government observes the cost environment and may implement a publicly observable interest rate cap $r^* \in \{c+a_1, c+a_2, c+a_3, v\}$ where $r^* = v$ corresponds to no intervention. She values aggregate expected consumer surplus and the aggregate positive externality of better housing on human capital accumulation that housing loans help to realize. If the externality per contract is denoted by w , the government's objective function becomes

$$V(c, r^*, E(c|r^*)) = \int_0^1 \mathbf{1}_{r^*-c-d_i \geq 0} * E(w + v - r_i - e_i s_i) di - \int_0^1 \mathbf{1}_{r^*-c-d_i < 0 \leq r^*-E(c|r^*)-d_i} * e_i s_i di$$

A series of remarks are in order. First, the expectation in the first term refers to the interest rate to be accepted (r_i) after observing one or more random offers. Second, the complete neglect of producer surplus means in fact only that the government puts a somewhat higher weight on borrowers than on banks in her preferences. This is because the redistributive aspect is very strong in price regulation with a unit demand. Third, the value of borrower's idiosyncratic risk d_i is a "local" information of the borrower and the bank where he takes out a loan. Accordingly, banks can offer different interest rates to different risk types. The interest rate cap, however, has to be uniform across the three markets of different borrowers. Judging precisely the state of the cost environment requires proper expertise that makes c a "global" information of banks and the government. Thereby, the interest rate cap can depend on the cost environment.

Fourth, as we will elaborate later, each borrower can find an acceptable deal exactly when the interest rate cap is at least as high than the respective marginal cost: $r^* \geq c + d_i$. Yet, borrowers may exert wasteful search efforts when they are too optimistic about the marginal cost: $r^* < c + d_i$, but $r^* \geq E(c|r^*) + d_i$. Since information sets of borrowers are always identical at the time when they decide on searching that is when they have already observed r^* , their common expectation is denoted by $E(c|r^*)$. Fifth, we only allow for a discrete set of marginal costs as possible interest rate caps. This restriction is in line with the assumption of discrete risk types. This way, more binding interest rate cap always excludes some borrowers with higher risk from the market, as in reality. Finally,

the externality term may capture not only genuine noninternalized spillover effects, but also some additional political gains stemming from wider access to housing loan market that signals proper quality or aligned preferences of the government.³⁰

The timing of the events is the following. First, nature determines the cost environment, banks and the government observe it. Second, the government decides on the interest rate cap. Third, banks and borrowers decide simultaneously on interest rate offers and searching efforts. As borrowers commit to observe either one or all the market offers, this decision cannot be altered once the first random offer turns out to be favourable or disappointing. Therefore, borrowers using their prior beliefs form rational expectations on banks' pricing strategies to forecast the gains from search. At the same time, firms set their prices anticipating search efforts exerted by the borrowers. Fourth, contracting takes place the following way. Banks send a menu of risk type dependent interest rate offers to any borrower whom he gets in touch with. Each offer is drawn independently from his equilibrium (possibly mixed) pricing strategy. Borrowers decide among offers they possess or decline all of them. The chosen bank verifies the borrower's risk type. Finally, contracts are signed and executed.

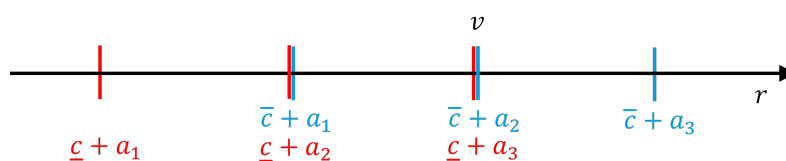
We are interested in the symmetric Perfect Bayesian Equilibrium. The following regularity assumption represented in Figure 1.17 focuses on the most interesting case and also provides tractability.

Assumption 1 *The variability of idiosyncratic costs is twice as large as the variability in the cost environment: $a_3 - a_2 = a_2 - a_1 = \bar{c} - \underline{c}$. Transactions with risky borrowers are just about possible under low cost environment: $v = \underline{c} + a_3$. Consequently, the set of possible interest rate caps simplifies to $r^* \in \{r_1^*, r_2^*, v\}$ with the notation $r_1^* = \underline{c} + a_1$, and $r_2^* = \underline{c} + a_2$.*

A comprehensive narrative of the interest rate cap at the Hungarian housing loan market between 2023 October and 2024 June can be assembled with a series of equilibria of

³⁰It has a further technical role in substituting for the consumer surplus of risk types between a_2 and a_3 that are missing in the discrete setting of the model.

Figure 1.17: Marginal costs under different cost environments

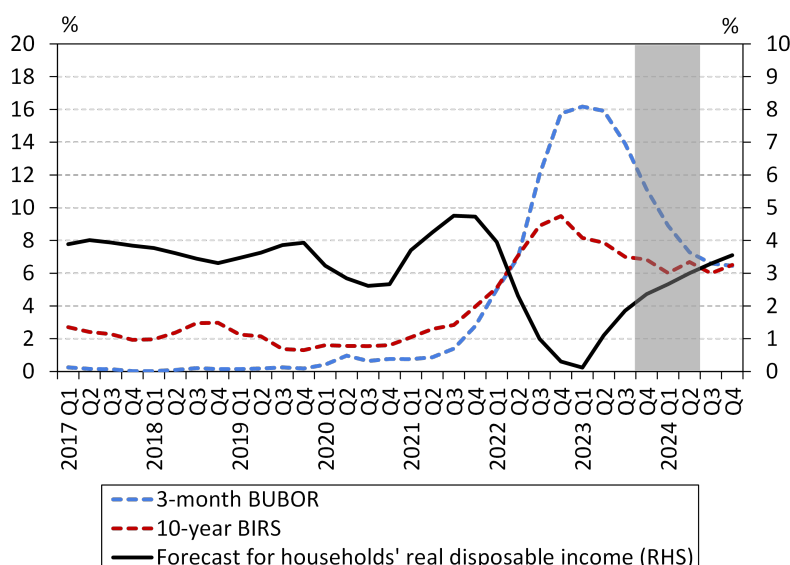


Notes: This figure is valid under Assumption 1.

the model with different cost environment and borrower beliefs. Figure 1.18 shows the sudden and material rise in proxies for financing and aggregate risk costs during 2022 triggered by the European energy crisis. This means that the cost environment of housing lending has become unusually high by the second half of 2022, which also transmitted to the beliefs of borrowers by turbulent developments accompanied the crisis. In line with this observation, the before program situation can be described by assuming a high cost environment that is common knowledge ($c = \bar{c}$, $\alpha = 1$). We show in Appendix 1.D that under general conditions interest rate cap is not optimal to implement in this case, indeed. According to Figure 1.18, general lending costs started to decrease substantially from 2023. Since the early phase of a gradual recovery is less transparent than the outbreak of a severe crisis, we presume that this negative cost shock might have remained unrealized by borrowers for a while. Accordingly, the program period can be described by a version of the model with low cost environment and more or less unchanged borrowers' belief making them pessimistic ($c = \underline{c}$, $\alpha \in (0, 1)$ and high). Our equilibrium analysis in Appendix 1.D reveals that implementing the interest rate cap was beneficial only temporarily, as long as it could signal low cost environment to pessimistic borrowers. After that, a full information equilibrium turns to be the relevant description ($c = \underline{c}$, $\alpha = 0$), where the interest rate cap becomes inferior again (under general conditions).

The counterfactual scenario during and after the program is represented by the equilibrium with low cost environment and pessimistic beliefs, but without the possibility of interest rate regulation ($c = \underline{c}$, $\alpha \in (0, 1)$ and high, $r^* \in \{v\}$). At least two implicit assumptions are applied here. First, the introduction of a relatively tight interest rate cap at the housing loan market was generally unexpected, as no policy framework, no clear

Figure 1.18: Proxies for the cost environment



Notes: The shaded area is the period of the interest rate cap being in effect. BUBOR: Budapest Interbank Offered Rate, BIRS: Budapest Interest Rate Swap, where the basis of the quoted interest rate swap is 6-month BUBOR. The 3-month BUBOR and the 10-year BIRS represent financing cost of lending as they are the main short term and long term interbank lending rates. The forecast for household income captures the aggregate risk cost of lending. This is the 4-quarter rolling average of average forecasted annual growth rates of households' real disposable income for 8 quarters ahead. Forecasts are derived from the quarterly Inflation Reports of the MNB. Source: MNB.

regulatory intention, or no close and applicable benchmark existed beforehand.³¹ Second, we build on the premise that information spreads slowly among potential borrowers without a public intervention, especially over a long period that includes even the post program period. A more realistic counterfactual for the after program effects involves a bit lower α , but still significantly higher than 0. In what follows, we focus on the extreme possibility when pessimism is at its strongest: $\alpha \rightarrow 1$.

1.5.6 Model predictions

Elements of our narrative structure is elaborated in Appendix 1.D. Here, we only summarize in intuitive terms the theoretical predictions that our search friction framework

³¹Partly an exception for the latter is the so called “interest stop” measure which came in effect at the beginning of 2022. Pursuant to Government Decree No. 782/2021 (XII.24) and its extensions, the interest rate of a housing loans that was taken out before 2022 and classified as an adjustable rate housing loan with short fixation period was capped at the interest rate that was applicable to the same contract in October 2021. The aim of the measure was to provide temporary protection for the debtors against the significantly increasing interest rate environment during and in the aftermath of the European energy crisis.

can provide. It should be emphasized again that the counterfactual scenario involves the strongest possible pessimism, which becomes less and less realistic as time passes, especially after the program. Consequently, predicted after program effects should be considered as an upper bound for the absolute values of the respective realistic terms. During program effects compared to the counterfactual:

1. Number of loans remains unchanged except for risky borrowers who lose market access.
2. Share of searchers remains unchanged at active markets.
3. The whole pricing rule shifts "downwards" at active markets.
4. Average interest rate decreases at active markets.

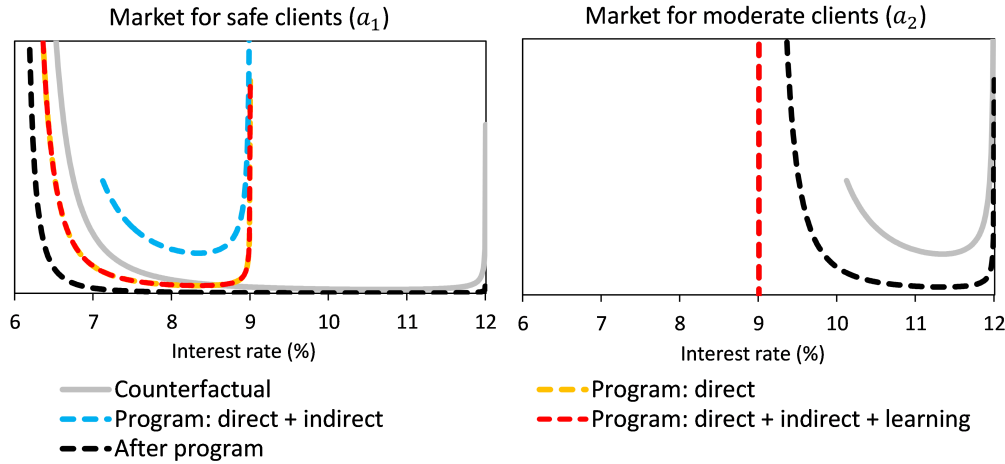
After program effects compared to the counterfactual:

1. Number of loans remains unchanged.
2. Share of searchers increases at markets for safe and moderate borrowers.
3. The whole pricing rule shifts "downwards" at markets for safe and moderate borrowers.
4. Average interest rate decreases at markets for safe and moderate borrowers.

According to the model predictions, the interest rate cap should be used only when (1) a significant negative cost shock occurred, and (2) its pass-through into interest rates is delayed because search frictions are exacerbated with unaware borrowers. The cap shuts down the market for risky borrowers, while decreases the whole distribution of equilibrium interest rates at other markets. The share of searching borrowers does not change compared to the counterfactual. When the negative cost shock becomes common knowledge precisely as a result of the temporary operation of the cap, the cap should be revoked. Compared to pessimistic counterfactual borrowers, the new generation of informed borrowers after the program search more and attain lower interest rates. The market for risky borrowers starts to operate after the program. The share of searchers

increases in time comparing during and after program periods, while the dynamics of average equilibrium interest rate is increasing for moderate borrowers, but ambiguous for safe ones.

Figure 1.19: Equilibrium distributions of interest rates in the leading example



Notes: Probability density functions. The red dashed line corresponds to the during program equilibrium (in the model with asymmetric information and low cost environment $c = \underline{c}$). The yellow and blue dashed lines provide a decomposition of the difference between the interest rate distributions during program and in the counterfactual situation. Yellow line represents the case when only the interest rate cap is introduced in the counterfactual situation, but search efforts are kept unchanged. Blue line represents the case when additionally search efforts are changed but without modifying borrowers' expectations (no learning from the introduced cap). In the example $v = 12\%$, $\underline{c} = 6\%$, $\bar{c} = 9\%$, $a_1 = 0\%$, $a_2 = 3\%$, $a_3 = 6\%$, $m_1 = 0.5$, $m_2 = 0.3$, $m_3 = 0.2$, $\lambda = 0.3$, s follows a uniform distribution, $\bar{s} = 1.5\%$, $n = 4$.

We illustrate these predictions in an example where parameter values were chosen to reflect more or less their observable counterparts in the Hungarian regulatory episode: $v = 12\%$, $\underline{c} = 6\%$, $\bar{c} = 9\%$, $a_1 = 0\%$, $a_2 = 3\%$, $a_3 = 6\%$, $m_1 = 0.5$, $m_2 = 0.3$, $m_3 = 0.2$, $\lambda = 0.3$, s follows a uniform distribution, $\bar{s} = 1.5\%$, $n = 4$. Cap is set to 9% and without its informative effect, it would increase average equilibrium interest rate for safe borrowers from the counterfactual level of 7.5% to 8.1% (gray and blue lines on the left panel of Figure 1.19). The informative effect of the cap amounting to 1.3% reduces it back to 6.8% (blue and red lines on the left panel of Figure 1.19). The market for moderate borrowers reacts to the cap much more simply: costly searching together with price dispersion vanishes because loans are supplied at their marginal cost of 9% (right panel of Figure 1.19). The predicted total effects of the cap on the average interest rates are -0.8% for safe clients and -2.1% for moderate clients, which corresponds to an aggregated effect of -1.9% taking also into account the exclusion of risky borrowers as

well (see Table 1.11). Aggregate price dispersion is measured as the weighted average of standard deviations of equilibrium interest rates across markets, which is compressed by the cap from the counterfactual level of 0.9% to 0.5%. Decreasing interest rates coupled with unchanged search costs result in expanding consumer surpluses visualized by the rectangles with green perimeters on the left panel of Figure 1.20. Their total area is larger than the area of the rectangle with red perimeter representing the loss from excluding risky borrowers from the market, which means that the interest rate cap at 9% is indeed worth to introduce.

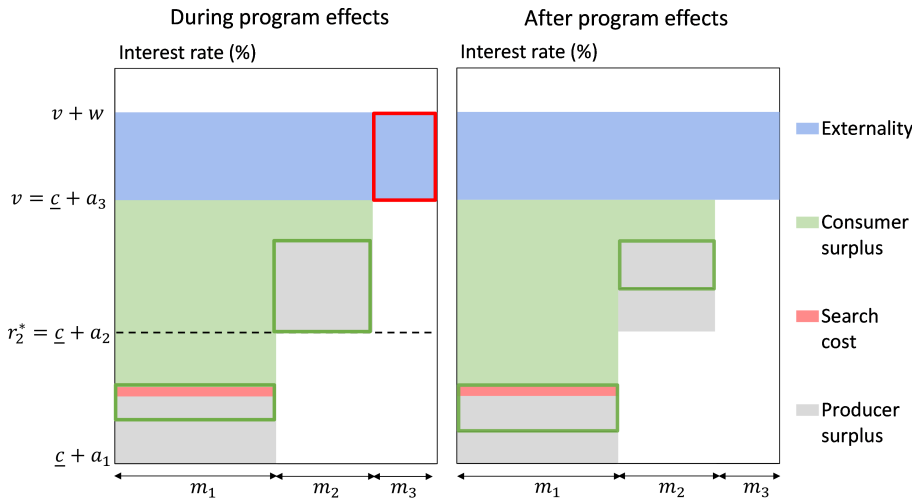
Table 1.11: Searching and equilibrium prices in the leading example

	Before program	Counter- factual	During program	After program	During program effect	After program effect
Share of searchers						
Market for safe clients (a1)	0.74	0.74	0.74	0.96	0.00	0.21
Market for moderate clients (a2)	0.30	0.30	0.30	0.74	0.00	0.44
Market for risky clients (a3)	-	0.30	-	0.30	-	0.00
Total	0.58	0.52	0.58	0.76	0.06	0.24
Average equilibrium interest rate, %						
Market for safe clients (a1)	9.8	7.5	6.8	6.3	-0.8	-1.3
Market for moderate clients (a2)	12.0	11.1	9.0	9.8	-2.1	-1.3
Market for risky clients (a3)	-	12.0	-	12.0	-	0.0
Total	10.6	9.5	7.6	8.5	-1.9	-1.0
Price dispersion (standard deviation of equilibrium interest rates), %						
Market for safe clients (a1)	0.7	1.5	0.7	0.6	-0.7	-0.9
Market for moderate clients (a2)	0.0	0.7	0.0	0.7	-0.7	0.1
Market for risky clients (a3)	-	0.0	-	0.0	-	0.0
Total	0.5	0.9	0.5	0.5	-0.5	-0.4

Notes: Data is missing where the market is not in operation. In the example $v = 12\%$, $\underline{c} = 6\%$, $\bar{c} = 9\%$, $a_1 = 0\%$, $a_2 = 3\%$, $a_3 = 6\%$, $m_1 = 0.5$, $m_2 = 0.3$, $m_3 = 0.2$, $\lambda = 0.3$, s follows a uniform distribution, $\bar{s} = 1.5\%$, $n = 4$.

In the example, average equilibrium interest rate for safe borrowers decreases further by 0.5 percentage points to 6.3% after the cap is revoked. Despite the regained possibility of offering higher interest rates than 9%, price dispersion shrinks even more to 0.6%. After program average interest rate increases for moderate borrowers by 0.8 percentage points up to 9.8%. The aggregated average interest rate also counts with risky borrowers gaining market access. It increases which means that the predicted total after program effect on the average interest rate reduces to -1.0%. The gain in consumer surplus that is caused by the interest rate cap increases for safe borrowers but decreases for moderate borrowers, which can be also seen from the comparison of left and right panels in Figure 1.20.

Figure 1.20: Effects of the interest rate cap on government's payoff in the leading example



Notes: Coloured areas are proportional to the aggregate-level externality, search cost, consumer and producer surplus in the counterfactual situation. Rectangles with coloured perimeter show the increases in consumer surplus (green) and decreases in externalities (red) caused by the interest rate cap. In the example $v = 12\%$, $\underline{c} = 6\%$, $\bar{c} = 9\%$, $a_1 = 0\%$, $a_2 = 3\%$, $a_3 = 6\%$, $m_1 = 0.5$, $m_2 = 0.3$, $m_3 = 0.2$, $\lambda = 0.3$, s follows a uniform distribution, $\bar{s} = 1.5\%$, $n = 4$.

1.5.7 Discussion

Quantity effect

Each borrower has the same unit demand function in the model implying no response in demanded quantities to decreasing interest rates.³² Three possible extensions could generate genuine volume effects.

First, a downward sloping aggregate demand function could be introduced either by keeping unit demand with different gross valuations (preference heterogeneity), or by introducing the intensive margin of loan demand. Second, since purchasing a durable good can be timed, market entry decision also could be incorporated that would generate delayed demand. If refinancing a loan involves more costs than the delay, it may be optimal in a cost environment perceived to be high to wait until the cost environment normalizes. Third, this delayed demand effect can be reinforced by business cycle sensitive demand. Macroeconomic conditions matter through expectations on future income and potential

³²From the point of view of the banks, the market demand is not perfectly inelastic, as higher searching intensity decreases the aggregate loan amount that can be disbursed at high interest rates.

future nonpayment difficulties. During the crisis, grim prospects restrict demand until borrowers are convinced about a more encouraging future. In the last two cases, borrowers' beliefs about macroeconomic prospects influences not only search efforts but also entry decisions.

In each case, eliminating borrowers' pessimism would not only harm banks through rent extraction by lower interest rates but also benefit them through the producer surplus on the additional loan amount realizing. Banks may even become interested collectively and/or individually in revealing the cost environment to the borrowers. If this does not involve large costs, public intervention may become pointless. The interaction between pricing and searching becomes more involved with these extensions, and analytical solution may be hard to find.

Alternative explanations

Material unexplained variation in the interest rates can be found in our data after using a wide range of product, borrower and bank characteristics as predictors (see Appendix 1.F). This suggests that some random element is present in loan pricing, even if banks and clients may have additional information that can influence interest rates. Recent research also documents sizeable interest rate dispersion and search costs at mortgage markets in Canada (Allen et al., 2014, and Allen et al., 2019) and in the US (Bhutta et al., 2020).

The three main observations are hard to reconcile with several possible explanations other than costly consumer search: (1) temporary application of the cap, (2) decreasing interest rate even below the cap, and (3) persistent post program effects.

The positive aggregate volume effect indicates that loans were typically priced above their marginal costs. If (tacit) collusion is the reason, the interest rate cap can confuse this delicate practice, which takes time to reorganize. Thereby the prediction of persistent post program effects. Collusion, however, requires prices that can be easily checked by colluding partners (Ivaldi et al., 2003). This is not a straightforward assumption in the Hungarian

housing loan market where pricing rules are complex and also depend on non-transparent client characteristics. Furthermore, focal points might have blurred already just before the program when the interest rates had been rising rapidly during the crisis. In this situation, a newly introduced interest rate cap might have even helped to coordinate collusion at the limit (see for example Knittel and Stango, 2003). Consequently, even if collusion played an important role, it is not clear why it decreased interest rates substantially below the cap. Additionally, if the price cap is effective in limiting collusion, it should be applied regularly, maybe even permanently.

Other explanations are more plausible in predicting ripple effect of the interest rate cap. First, substitution in demand can happen for example, when some banks are more trusted or attainable than others and therefore, "better" banks are able to price their loans higher in equilibrium. As the cap becomes binding earlier with more expensive loans, more consumer turn to "better" banks as they start to become cheaper. "Worse" banks react with decreasing the interest rates of their loans even when they are not constrained directly by the cap. Second, an example for substitution in supply can be when resources in marketing and client services are reallocated from market segments where the cap has a larger bite to other segments, where the competition intensifies as a result. Third, implementing some screening technic in loan pricing in order to facilitate self selection with respect to non-observable borrower characteristics may also predict changes in interest rates below the cap. This is because maintaining incentive compatibility of the whole menu of contracts may require adjustments even in interest rates not directly affected by the cap. None of these hypotheses predict slow rearrangement once the cap is lifted.

However, we can combine these explanations with the possibility that the government actually maintained the implicit threat of reintroducing the cap or harming banks with other interventions in the case when the banks would increase significantly the interest rates after the program. Avoiding a sector level sanction might have been the common interest of the banking sector, but not necessarily an individual interest of each bank. Furthermore, public information on the consultation between the government and the

Banking Association revealed early that the government accepts the very narrow scope of the new interest rate cap of 5 percent that eventually came into force from April 1, 2025 and lasted for half a year: first time buyers under 35 years, energy-saving, small (less than 60 sq m), not expensive (less than 3000 EUR per sq m) flats.³³

1.6 Conclusion

In this paper, we investigated the impact of the voluntary interest rate cap introduced in the Hungarian mortgage market in October 2023. Our primary identification strategy relied on Difference-in-Differences (DiD) and Synthetic Difference-in-Differences (SDiD) approaches, complemented by Regression Discontinuity (RD) estimates as a robustness check.

Our findings reveal a significant distributional impact of the policy. Contrary to the intended goal of uniform market stimulation, we document a strong crowding-out effect among high-risk borrowers. While the lending volume for safer clients remained stable or increased, banks responded to the regulatory constraint by restricting credit supply to riskier segments, effectively substituting price adjustments with quantity rationing.

We rationalize these empirical results through a theoretical framework based on search frictions and information asymmetry. In our model, the rate cap serves as a credible signal of a low-cost environment to pessimistic borrowers, thereby maintaining search efforts. This signaling mechanism explains why interest rates fell even in segments not directly bounded by the cap and why the positive effects on lending persisted after the policy was suspended. Our results suggest that well-timed price caps can effectively coordinate market expectations and restart lending activity, although cannot avoid market distortions.

Our findings have direct implications for the design of future interest rate cap policies.

³³On September 23, 2024, the Ministry for National Economy asked the Banking Association to elaborate a program with an APR cap on new housing loans with 5%. In a month, the ministry gave the banks the understanding to accept their proposed conditions.

The crowding-out effect documented in this paper is an inherent consequence of a uniform cap in a market with heterogeneous borrower risk: when banks cannot price risk freely, they respond by restricting credit to the riskiest segments. Three design principles follow from our analysis. First, differentiating the cap by observable borrower risk characteristics - such as the loan-to-value or debt service-to-income ratio - would allow banks to continue risk-based pricing within each segment, reducing market exclusion at the cost of greater administrative complexity. Second, keeping the program short in time limits the cumulative crowding-out loss while preserving the signaling benefit, which our model shows dissipates once the information asymmetry is resolved. Third, and most fundamentally, our theoretical framework suggests that investing in credible transparency tools - such as well-established central bank communication or benchmark rate mechanisms - can render the price cap unnecessary altogether, avoiding its distributional costs without sacrificing the informational benefit.

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1.A Product-borrower segment indicator estimation

	(1) Mortgage	(2) Home equity
Real wage (HUF) squared	0.000857*** (0.000123)	0.00151*** (0.000280)
Real wage (HUF) squared	-7.85e-08** (3.88e-08)	-0.000000303*** (7.80e-08)
Age	0.00196 (0.00316)	-0.00875 (0.00772)
Age squared	0.00000270 (0.0000324)	0.000161** (0.0000749)
Payment to income	0.0489*** (0.00418)	0.0621*** (0.00464)
Payment to income: other loans > 50%	-0.00553*** (0.00140)	-0.00972*** (0.00172)
Payment to income: other loans	0.00191*** (0.000560)	0.000991 (0.000841)
High payment to income other	-0.0104*** (0.00176)	-0.00802** (0.00322)
Loan to value	0.00208*** (0.000391)	0.00100 (0.000762)
Maturity (months)	0.00249*** (0.000204)	0.00264*** (0.000336)
Loan amount (HUF)	-1.600*** (0.0864)	-1.947*** (0.117)
Financing cost	0.709*** (0.0161)	0.657*** (0.0311)
HHI	0.266 (0.292)	-0.110 (0.121)
Constant	4.585*** (0.185)	6.401*** (0.308)
Observations	128558	24579
R^2	0.820	0.776
Repricing period FE	Yes	Yes
District FE	Yes	Yes
Loan purpose FE	Yes	Yes
Bank FE	Yes	Yes
HHI x Bank FE	Yes	Yes

Standard errors clustered on county x bank level in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Note: All regressions include bank, district, repricing periods (5 categories: less than 1 year, at least 1 and less than 3 years, at least 3 and less than 5 years, at least 5 and less than 10 years, at least 10 years), loan purpose (buying new property, buying used property, renovation, construction of new property) fixed effects and bank fixed effect x Herindhal Hirschman index variable. Payment to income considers only the monthly instalment of the given mortgage, High payment to income other refers to loans with payment-to-income above 50 percent

1.B Difference-in-differences estimation with covariates

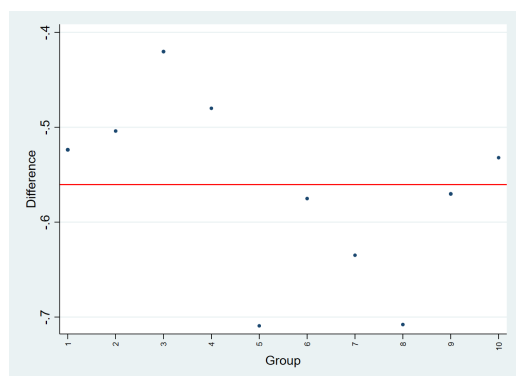
Table 1.12: Difference-in-differences estimation results by quintiles, including covariates

	(1)	(2)	(3)	(4)	(5)	(6)
	Total market	1st quintile	2nd quintile	3rd quintile	4th quintile	5th quintile
Post x Mortgage	-1.296*** (0.204)	-0.952*** (0.189)	-1.107*** (0.186)	-1.281*** (0.197)	-1.614*** (0.215)	-2.426*** (0.319)
Observations	162463	41006	43660	44751	45433	46549
R^2	0.843	0.902	0.889	0.875	0.854	0.794
Time FE	Yes	Yes	Yes	Yes	Yes	Yes
Bank FE	Yes	Yes	Yes	Yes	Yes	Yes
District FE	Yes	Yes	Yes	Yes	Yes	Yes

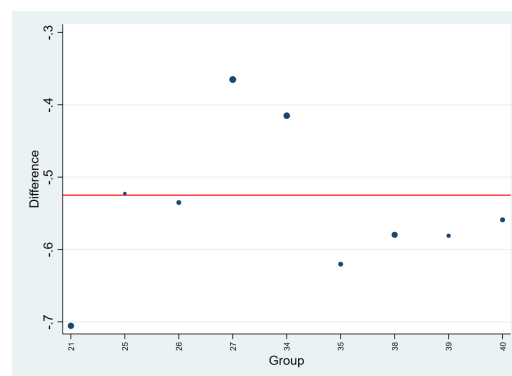
Standard errors clustered on county x bank level in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

1.C Synthetic difference-in-differences robustness tests



(a) APR using home equity loans in donor pool



(b) APR using home EU member states' loans in donor pool

Figure 1.21: Weights of controls in the SDiD estimations on APR using home equity loan groups (left) and EU member countries (right) as donors

Note: Home equity loans are coded the following way: decile of highest quality:1 deciles of lowest quality:10 between these changes monotonically. Countries are coded as: 21: Austria, 25: Czech Republic, 26: Germany, 27: Estonia, 34: Lithuania, 35: Latvia, 38: Romania, 39: Slovenia, 40: Slovakia.

Table 1.13: Results of Synthetic Difference-in-Differences estimations APR with home equity loans as control group

	(1)	(2)	(3)	(4)	(5)	(6)
	All groups	1st quintile	2nd quintile	3rd quintile	4th quintile	5th quintile
Program period	-1.086*** (0.0908)	-0.654*** (0.169)	-0.896*** (0.171)	-1.032*** (0.171)	-1.369*** (0.171)	-2.066*** (0.170)
Observations	360	216	216	216	216	216
R^2						
Covariates	Yes	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes	Yes

Standard errors in parentheses

* p<0.10, ** p<0.05, *** p<0.01

Table 1.14: Results of Synthetic Difference-in-Differences estimations APR with international control group

	(1)	(2)	(3)	(4)	(5)	(6)
	All groups	1st quintile	2nd quintile	3rd quintile	4th quintile	5th quintile
Program period	-0.805*** (0.151)	-0.486*** (0.161)	-0.640*** (0.167)	-0.670*** (0.169)	-0.957*** (0.161)	-1.116*** (0.161)
Observations	342	198	198	198	198	198
R^2						
Covariates	Yes	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes	Yes

Standard errors in parentheses

* p<0.10, ** p<0.05, *** p<0.01

Table 1.15: Results of Synthetic Difference-in-Differences estimations of Ln(loan volumes)

	(1)	(2)	(3)	(4)	(5)	(6)
	Total market	1st quintile	2nd quintile	3rd quintile	4th quintile	5th quintile
Program period	0.356** (0.179)	0.571*** (0.132)	0.249* (0.132)	0.137 (0.132)	-0.0582 (0.132)	-0.365*** (0.132)
Observations	180	198	198	198	198	198
R^2						
Covariates	No	No	No	No	No	No
Time FE	Yes	Yes	Yes	Yes	Yes	Yes

Standard errors in parentheses

* p<0.10, ** p<0.05, *** p<0.01

1.D Solution of the model

1.D.1 Pricing and searching

Given cost environment $c \in \{\underline{c}, \bar{c}\}$, interest rate cap $r^* \in \{r_1^*, r_2^*, v\}$, and borrowers' belief $E(c|r^*) \in [\underline{c}, \bar{c}]$, outcomes at the three markets are determined in this section.

In a symmetric equilibrium, all banks apply the same risk type dependent pricing strategy which is the best reply to searching decisions of borrowers and the same pricing strategy of other banks. If a risk type cannot be supplied profitably ($r^* < c + a_j$), no bank offers any loan contract to these borrowers. At other markets, the standard search-theoretic reasoning found, for example, in Varian (1980), implies that there is a unique mixed strategy in which offers are drawn from a distribution on $[r_L, r^*]$ without gaps and atoms, where $r_L \in [c + a_j, r^*]$. Let $F_j(r)$ be the equilibrium cumulative distribution function of interest rate offers and μ_j the equilibrium share of searchers at the market for risk type a_j . A necessary condition for the equilibrium is that pure strategies r^* and an arbitrary $r \in [r_L, r^*]$ have to yield the same expected profits:

$$\frac{1 - \mu_j}{N} m_j(r^* - c - a_j) = \left(\frac{1 - \mu_j}{N} + \mu_j (1 - F_j(r))^{N-1} \right) m_j(r - c - a_j)$$

Only non-searchers accept an offer with the highest possible interest rate (r^*), while a lower offer is also accepted by searchers whose alternative offers are higher. After rearranging, the equilibrium pricing rule can be obtained.

$$F_j(r, r^*, \mu_j) = \begin{cases} 0 & \text{if } \underline{c} + a_1 < r \leq r_L(r^*, \mu_j) \\ 1 - \left(\frac{1 - \mu_j}{\mu_j} \frac{1}{N} \frac{r^* - r}{r - c - a_j} \right)^{\frac{1}{N-1}} & \text{if } r_L(r^*, \mu_j) < r \leq r^* \\ 1 & \text{if } r^* < r \leq v \end{cases}$$

where

$$r_L(r^*, \mu_j) = (c + a_j) + \frac{1 - \mu_j}{1 - \mu_j + N\mu_j} (r^* - c - a_j)$$

We assume that shoppers play their weakly dominant strategy of searching. Non-shopper i with risk type a_j searches as a best reply to the common pricing rule of the banks and the searching decisions of other borrowers summarized by μ_j when

$$v - E[E(r_j|c, r^*, \mu_j)] \leq v - E[E(r_{j,min}|c, r^*, \mu_j)] - s_i$$

where $r_{j,min}$ denotes the minimum value of a sample with N elements independently drawn from the distribution defined by $F_j(r, r^*, \mu_j)$. After changing variables $z = 1 - F_j(r, r^*, \mu_j)$, we get that both non-searchers and searchers can expect an offer that is a convex combination of the highest possible and the lowest possible offers.

$$E[r_j|c, r^*, \mu_j] = Ar^* + (1 - A)(c + a_j) \quad (1.8)$$

$$E[r_{j,min}|c, r^*, \mu_j] = Br^* + (1 - B)(c + a_j) \quad (1.9)$$

where

$$A = \int_0^1 \frac{1 - \mu_j}{\mu_j N z^{N-1} + 1 - \mu_j} dz \in [0, 1]$$

$$B = \int_0^1 \frac{(1 - \mu_j) N z^{n-1}}{\mu_j N z^{n-1} + 1 - \mu_j} dz \in [0, 1]$$

and $A \geq B$. The optimality condition simplifies to

$$(A - B)(r^* - E(c|r^*) - a_j) \geq s_i \quad (1.10)$$

The expected *private* benefit of searching at the left hand side is zero whenever there is no price dispersion either because nobody searches ($\mu_j = 0$, which never can be an equilibrium as $\mu_j \geq \lambda$) or because everybody searches ($\mu_j = 1$). Consequently, the expected improvement on collected offers is highest in case of intermediate values of μ_j . Furthermore, private benefit of searching is proportional to the perceived total gain from the transaction. Later, we use intensively the notion that this gain is reduced by the more binding interest rate cap “from above” and pessimism about the cost environment “from below”.

For the marginal searcher with $\tilde{s}_j$, condition (1.10) holds with equality, and the share of searchers at the market can be defined by $\mu_j = \lambda + (1 - \lambda)G(\tilde{s}_j)$. As G is strictly increasing and continuous, there exists its inverse function $G^{-1} : [0, 1] \mapsto \mathbb{R}^+$ that is also strictly increasing and continuous. In equilibrium, we have

$$(A(\mu_j) - B(\mu_j))(r^* - E(c|r^*) - a_j) = G^{-1}\left(\frac{\mu_j - \lambda}{1 - \lambda}\right) \quad (1.11)$$

The main insights from this section are summarized in Lemma 1 and represented in Figure 1.22.

Lemma 1 *The following properties hold for the searching behaviour.*

1. *The expected benefit of searching is strictly concave in μ_j : $\frac{\partial(A(\mu_j) - B(\mu_j))}{\partial \mu_j} < 0$.*

2. *The market equilibrium is unique if*

(a) $\operatorname{argmax}_{\mu_j} (A(\mu_j) - B(\mu_j)) \leq \lambda$, or

(b) $\lambda < \operatorname{argmax}_{\mu_j} (A(\mu_j) - B(\mu_j))$ and $\frac{\partial(A(\mu_j) - B(\mu_j))}{\partial \mu_j} (r^* - E(c|r^*) - a_j) < \frac{\partial G^{-1}\left(\frac{\mu_j - \lambda}{1 - \lambda}\right)}{\partial \mu_j}$
for all $\mu_j \in [\lambda, \operatorname{argmax}_{\mu_j} (A(\mu_j) - B(\mu_j))]$

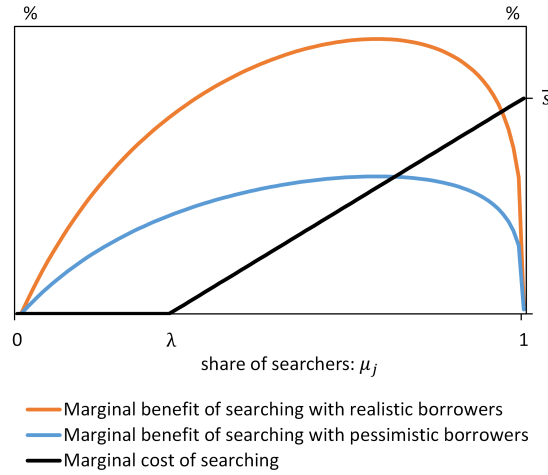
3. *Equilibrium share of searchers $\mu_j(r^*, E(c|r^*))$ increases in the interest rate cap and decreases in expected cost environment: $\frac{\partial \mu_j(r^*, E(c|r^*))}{\partial r^*} > 0$, $\frac{\partial \mu_j(r^*, E(c|r^*))}{\partial E(c|r^*)} < 0$.*

4. *Equilibrium share of searchers is weakly lower at markets for more risky borrowers: $\mu_1(r^*, E(c|r^*)) \geq \mu_2(r^*, E(c|r^*)) \geq \mu_3(r^*, E(c|r^*))$.*

Proof: Tappata (2009) with trivial modifications. $\square$

The sufficient condition for uniqueness is quite general as the left hand side in equation (1.11) is strictly concave, while the right hand side is strictly increasing in μ_j . Using law of large numbers and equations (1.8) and (1.9), the average accepted interest rate in the

Figure 1.22: Equilibrium with different beliefs



Notes: In the example $v = r^* = 12$, $c = \underline{c} = 6$, $\bar{c} = 9$, $a_j = 0$, $\lambda = 0.3$, s follows a uniform distribution, $\bar{s} = 1.5$, $n = 4$. Realism means $E(c) = c = \underline{c}$, pessimism means $E(c) = c = \bar{c}$.

equilibrium turns to be the following.

$$\begin{aligned} \mu_j(r^*, E(c))E[r_{j,min}|c, r^*, \mu_j(r^*, E(c))] + [1 - \mu_j(r^*, E(c))]E[r_j|c, r^*, \mu_j(r^*, E(c))] &= \\ &= [1 - \mu_j(r^*, E(c))]r^* + \mu_j(r^*, E(c))(c + a_j) \end{aligned} \quad (1.12)$$

Government's objective function can be rewritten as

$$\begin{aligned} V(c, r^*, E(c | r^*)) &= \sum_{j=1}^3 \mathbf{1}_{r^* \geq c+a_j} * m_j [w + v - r^* - \mu_j(r^*, E(c|r^*)) (r^* - c - a_j)] - \\ &\quad - \sum_{j=1}^3 \mathbf{1}_{r^* \geq c+a_j} * m_j (1 - \lambda) \int_0^{G^{-1}\left(\frac{\mu_j(r^*, E(c|r^*)) - \lambda}{1 - \lambda}\right)} s dG(s) \\ &\quad - \sum_{j=1}^3 \mathbf{1}_{c+a_j > r^* > E(c|r^*)+a_j} * m_j (1 - \lambda) \int_0^{G^{-1}\left(\frac{\mu_j(r^*, E(c|r^*)) - \lambda}{1 - \lambda}\right)} s dG(s) \end{aligned} \quad (1.13)$$

Division of total transaction value $v - c - a_j$ between banks and borrowers can be understood by the pricing decisions. Banks use r^* in their optimal mixed pricing strategy and as a pure strategy, it would yield a sector level profit of $N \frac{1-\mu_j}{N} (r^* - c - a_j)$ at the market for risk type a_j . All pure strategies in the optimal mixing yield the same expected profit, therefore total sector level equilibrium profit is $(1 - \mu_j(r^*, E(c)))(r^* - c - a_j)$, while

the remaining part of the total transaction value goes to the borrowers (net of searching costs).

Note that with realistic beliefs $E(c) = c$ (full information), the marginal *social* benefit of searching at the market for risk type a_j is $r^* - c - a_j$ in any equilibrium. This is higher than the private one (see equation (1.11)) as borrowers do not internalize that higher search efforts reduce the equilibrium pricing rule. The marginal social and private costs of searching are the same in equilibrium: $\tilde{s}_j = G^{-1}\left(\frac{\mu_j(r^*, c) - \lambda}{1 - \lambda}\right)$. Consequently, the government has an incentive to spread some overoptimism among borrowers in order to raise equilibrium ratio of searchers above $\mu_j(r^*, c)$.

1.D.2 Interest rate cap as a costly signal

This section determines different equilibria of the model describing the before, during and after program situations together with the counterfactual scenario.

First, full information equilibria are investigated which correspond to before and after program situations with $c = \bar{c}$ and $c = \underline{c}$ respectively. With observable cost environment, expectations cannot be influenced by the interest rate cap, and there are no wasteful search efforts either. Government chooses the interest rate cap $r^* \in \{\underline{c} + a_1, \underline{c} + a_2, v\}$ that maximizes the full information version of her payoff function (1.13):

$$V(c, r^*, c) = \sum_{j=1}^3 \mathbf{1}_{r^* \geq c + a_j} * m_j [w + v - r^* - \mu_j(r^*, c)(r^* - c - a_j)] \\ - \sum_{j=1}^3 \mathbf{1}_{r^* \geq c + a_j} * m_j (1 - \lambda) \int_0^{G^{-1}\left(\frac{\mu_j(r^*, c) - \lambda}{1 - \lambda}\right)} sdG(s)$$

The essential trade off is between market access and rent extraction. As the government must rely on a uniform interest rate cap across markets with different marginal costs, any binding cap ($r^* < v$) renders some higher risk types non-profitable to supply. At markets that are still operating, generated changes in pricing and searching can be decomposed

in two effects. The direct price effect of the interest rate cap is the change in consumer surplus that would happen if searching decisions remained unaffected. Indirect price effect is the remaining change in consumer surplus triggered only by the adjustment in search efforts.

$$\frac{\partial V(c, r^*, c)}{\partial r^*} = \sum_{i=1}^3 \mathbf{1}_{r^* \geq c+a_j} m_j \left[\underbrace{\frac{\partial \mu_j(r^*, c)}{\partial r^*} (r^* - c - a_j - \tilde{s}_j)}_{\text{indirect price effect}} - \underbrace{(1 - \mu_j(r^*, c))}_{\text{direct price effect}} \right]$$

The direct price effect is beneficial for the borrowers, especially the higher the average price is (see equation (1.12)). The indirect price effect is harmful because it is driven by effort substitution: the cap decreases potential improvement of additional searching on a random offer. The following relations are implied by Lemma 1 and the equilibrium condition (1.11).

$$\frac{\partial \mu_j(r^*, c)}{\partial r^*} (r^* - c - a_j - \tilde{s}_j) > \frac{\partial \mu_j(r^*, c)}{\partial r^*} [(A(\mu_j(r^*, c)) - B(\mu_j(r^*, c)))(r^* - c - a_j) - \tilde{s}_j] = 0$$

Special cases in similar settings are provided by Fershtman and Fishman (1994) and Armstrong et al. (2009) where indirect effect dominates direct effect in the sense that a tighter price cap increases the average equilibrium price, which shuts down the rent extraction role of the interest rate cap. However, a cap sufficiently close to the marginal cost always benefits borrowers as it transfers almost all the total transaction value to them without relying significantly on costly search efforts.

Introduction of an interest rate cap is not optimal for the government when its market exclusion effect is strong enough, that is when either the number of borrowers losing market access is relatively large or the positive externalities of better housing are significant. Proposition 1 provides exact conditions which also emphasizes that the same interest rate cap threatens different borrower groups in different cost environments. This is reflected in Figure 1.23 by the lower left and upper left corners being the different parameter ranges where the cap r_2^* is preferred to no cap under low and high cost environment, respectively.

Proposition 1 *If there is full information ($E(c) = c$), the following conditions imply no*

interest rate cap in equilibrium ($r^* = v$). The first condition is also necessary when the cost environment is high ($c = \bar{c}$).

$$m_2 w \geq m_1 \left[(1 - \mu_1(v, \bar{c})) (a_2 - a_1) + (1 - \lambda) \int_0^{G^{-1}\left(\frac{\mu_1(v, \bar{c}) - \lambda}{1 - \lambda}\right)} sdG(s) \right] \quad (1.14)$$

$$\begin{aligned} m_3 w \geq & \sum_{j=1}^2 m_j (1 - \mu_j(v, \underline{c})) (a_3 - a_2) - \\ & - m_1 (\mu_1(v, \underline{c}) - \mu_1(r_2^*, \underline{c})) (a_2 - a_1) + \sum_{j=1}^2 m_j (1 - \lambda) \int_{G^{-1}\left(\frac{\mu_j(r_2^*, \underline{c}) - \lambda}{1 - \lambda}\right)}^{G^{-1}\left(\frac{\mu_j(v, \underline{c}) - \lambda}{1 - \lambda}\right)} sdG(s) \end{aligned} \quad (1.15)$$

Proof: See Appendix 1.E. $\square$

The realistic assumption is that housing externalities are significant and both moderate and risky borrowers are numerous. Consequently, Proposition 1 says that our model with informed borrowers typically predicts that it is not worth introducing a tight interest rate cap. This confirms the before and after program policy practice in the Hungarian case study.

Now, we turn to investigate the model with asymmetric information ($\alpha \in (0, 1)$). The description of during program observations is provided by the special case when the cost environment is eventually low ($c = \underline{c}$), which also means that borrowers are pessimistic. We will show that there is an equilibrium where an interest rate cap is optimal to introduce only when the cost environment is low. The counterfactual scenario in our estimates of causal effects corresponds to the same situation ($\alpha \in (0, 1)$, $c = \underline{c}$) with one alteration: the interest rate cap is not available. The counterfactual equilibrium is unique and fully characterised by the previous section. We focus on the extreme possibility when pessimism is at its strongest: $\alpha \rightarrow 1$.³⁴

³⁴Decisions in this counterfactual are the same as in the equilibrium with asymmetric information, available interest rate cap, and high cost environment ($c = \bar{c}$). Put it differently, the counterfactual can also be considered as a situation where the lack of the interest rate cap is the perfect signal of the high cost environment.

Setting the optimal interest rate cap in a separating equilibrium involves a new channel of the indirect price effect compared to the full information case. A tighter cap influences search efforts not only directly, but also through borrowers' beliefs. In the equilibrium, government introduces the cap only when she observes low cost environment. Borrowers know this, therefore observing the regulatory regime also informs them perfectly about the underlying cost environment. Consequently, an interest rate cap not only dampens the incentives for searching as in the full information case but also improves them by making borrowers more optimistic. If we consider an infinitesimal change of the interest rate cap in the government's payoff function (1.13) with pessimistic borrowers ($E(c|r^*) \geq c$) and optimism-promoting interest rate cap ($\frac{\partial E(c|r^*)}{\partial r^*} > 0$), we get the following formula for the indirect price effect for risk type a_j , where the positive sign of the last term is implied by borrowers pessimism and equilibrium condition (1.11) together.

$$\left[\underbrace{\frac{\partial \mu_j(r^*, E(c|r^*))}{\partial r^*}}_{>0} + \underbrace{\frac{\partial \mu_j(r^*, E(c|r^*))}{\partial E(c|r^*)}}_{<0} \underbrace{\frac{\partial E(c|r^*)}{\partial r^*}}_{>0} \right] \underbrace{(r^* - c - a_j - \tilde{s}_j)}_{>0}$$

In our setting, the two opposing parts of the indirect price effect can arise only at the market for safe borrowers and only under low cost environment. The reason is that any binding cap is at most as high as the marginal cost of supplying moderate borrowers in the low cost environment ($\underline{c} + a_2$), which makes searching pointless in any other situation. Furthermore, these two effects cancel each other out in the equilibrium of Proposition 2 in our model, eliminating the adverse impact of the indirect price effect on borrowers. This additional benefit of the interest rate cap is the reason why the cap can arise in equilibrium with asymmetric information even with parameter values which eliminate the cap from the equilibria with full information. As this additional benefit is larger when the cost environment is lower, the equilibrium with asymmetric information takes the form of a selectively applied cap depending on the cost environment.

Proposition 2 *If there is asymmetric information ($\alpha \in (0, 1)$), condition (1.14) together*

with the necessary condition

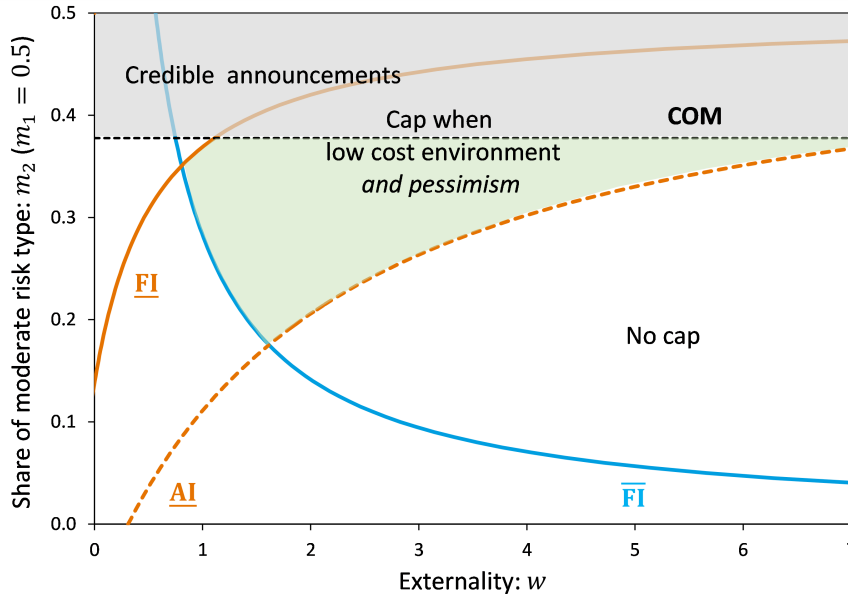
$$\sum_{j=1}^2 m_j (1 - \mu_j(v, \bar{c})) (a_3 - a_2) \geq m_3 w \quad (1.16)$$

imply that the following separating equilibrium exists:

- *Regulation:* $Pr(r_2^*|\underline{c}) = 1$, $Pr(v|\bar{c}) = 1$,
- *Market outcomes:* $\mu_j(r^*, E(c|r^*))$,
- *Equilibrium beliefs:* $Pr(\underline{c}|r_2^*) = 1$, $Pr(\bar{c}|v) = 1$,
- *Out of equilibrium beliefs:* $Pr(\underline{c}|r_1^*) = 1$.

Proof: See Appendix 1.E. $\square$

Figure 1.23: The narrative structure



Notes: The figure shows the example in which $v = 12\%$, $\underline{c} = 6\%$, $\bar{c} = 9\%$, $a_1 = 0\%$, $a_2 = 3\%$, $a_3 = 6\%$, $m_1 = 0.5$, $\lambda = 0.3$, s follows a uniform distribution, $\bar{s} = 1\%$, $n = 4$. The curves labeled with $\overline{FI}$, $\underline{FI}$, $\underline{AI}$ and COM are defined by condition (1.14), (1.15), (1.16, and (1.17) each with equality, respectively.

Condition (1.16) makes it clear that the benefit of the cap under low cost environment is the direct price effect at markets for safe and moderate borrowers, while its cost is the exclusion of risky borrowers. This optimality condition for using the cap is more relaxed than the analogous condition (1.15) under full information, which is shown in Proposition 3 and Figure 1.23.³⁵ The separating equilibrium in Proposition 2 is likely to

³⁵Note that the higher benefit of the cap in low cost environment under asymmetric vs. full information

coexist with full information equilibria in Proposition 1 under realistic assumptions on model parameters: moderate level of housing externalities w , non-extreme distribution of risk types m_1 , m_2 , and m_3 , and moderate levels of parameters capturing search frictions: λ and $G(s)$.

Proposition 3 *There is a parameter range where no interest rate cap $r^* = v$ is optimal under full information and under asymmetric information with high cost environment $c = \bar{c}$, while a moderate level of interest cap $r^* = r_2^*$ is optimal under asymmetric information with low cost environment $c = \underline{c}$. Put it differently, conditions (1.14), (1.15), and (1.16) may coexist.*

Proof: See Appendix 1.E. $\square$

Predictions of the model are collected in the following proposition.

Proposition 4 *Let $\mu_j(r^*, E(c|r^*))$ and $F_j(r, r^*, \mu_j(r^*, E(c|r^*)))$ denote the equilibrium share of searchers and the cumulative distribution function describing the equilibrium pricing rule applied by each bank at market for borrowers with risk type a_j and $j \in \{1, 2, 3\}$. Expected value $E(c|r^*)$ refers to the common belief of the borrowers after observing the interest rate $r^* \in \{r_1^*, r_2^*, v\}$. Under conditions (7), (8), and (9), the model predicts the following causal effects of the interest rate cap r_2^* compared to the counterfactual equilibrium.*

During program effects:

1. *Number of loans:* 0 for $j \in \{1, 2\}$ and $-m_3$
2. *Share of searchers:* $\mu_j(r_2^*, \underline{c}) - \mu_j(v, \bar{c}) = 0$ for $j \in \{1, 2\}$
3. *Pricing rule:* $F_j(r, r_2^*, \mu_j(r_2^*, \underline{c})) - F_j(r, v, \mu_j(v, \bar{c})) \geq 0$ for $r \in [\underline{c} + a_1, v]$ and $j \in \{1, 2\}$
4. *Average interest rate:* $-(1 - \mu_j(v, \bar{c}))(a_3 - a_2) < 0$ for $j \in \{1, 2\}$

consists not only of the informative effect appearing in the indirect price effect but also of stronger direct price effect. This latter stemming from the fact that under asymmetric information with no cap, borrowers are pessimistic as opposed to realistic borrowers under full information with no cap: $\mu_j(v, \bar{c}) < \mu_j(v, \underline{c})$.

After program effects:

1. *Number of loans:* 0 for $j \in \{1, 2, 3\}$
2. *Share of searchers:* $\mu_j(v, \underline{c}) - \mu_j(v, \bar{c}) > 0$ for $j \in \{1, 2\}$
3. *Pricing rule:* $F_j(r, v, \mu_j(v, \underline{c})) - F_j(r, v, \mu_j(v, \bar{c})) \geq 0$ for $r \in [\underline{c} + a_1, v]$ and $j \in \{1, 2\}$
4. *Average interest rate:* $-(\mu_j(v, \underline{c}) - \mu_j(v, \bar{c}))(a_3 - a_j) < 0$ for $j \in \{1, 2\}$

Proof: See Appendix 1.E. $\square$

1.D.3 Credibility of direct communication

The previous sections showed that the informative effect is the reason why it may be beneficial to introduce an interest rate cap temporarily. This intervention, however, involves nonnegligible costs of excluding risky borrowers from the market (for the time the cap is in force). There may be, however, some other and cheaper transparency tool of the government, which is potentially more efficient than the interest rate cap.

This section argues that direct announcements about the cost environment are often not helpful in our model. First, disclosed information about a complex issue like banks' future risk cost is rarely easy to verify (no hard information). Governments often publish even detailed assessments about various macroeconomic issues, which are very useful sources of information, but without expertise or trust (in the government or in experts interpreting this information), many borrowers may be left with doubts.

Second, government incentives do not always support direct information disclosure. Proposition 5 determines the exact condition in our model when the government prefers announcing a low cost environment not only when the cost environment is indeed low, but also when it is high. This temptation stems from positive externality of search efforts that is generally undersupplied in market equilibrium. Since optimistic borrowers (who believe that the cost environment is lower than the real one) exert more search efforts than

realistic borrowers, the government wants to spread optimism at markets where search efforts are too low otherwise. If each market always had this property, she would always want to declare low cost environment, which would make these announcements uninformative. In our model, however, there is also a cost of spreading optimism under high cost environment. It is the wasteful search of moderate borrowers who think that they can find better deals with searching ($E(c|m = \underline{c}) + a_2 < v$), while banks being aware of the high cost environment offer nothing but the best deal possible ($r = \bar{c} + a_2 = v$).

Proposition 5 *Suppose that there is asymmetric information ($\alpha \in (0, 1)$) and instead of introducing an interest rate cap, the government can send a public cheap talk message $m \in \{\underline{c}, \bar{c}\}$. The following inequality*

$$m_1 \left[(\mu_1(v, \underline{c}) - \mu_1(v, \bar{c})) (a_2 - a_1) - (1 - \lambda) \int_{G^{-1}\left(\frac{\mu_1(v, \bar{c}) - \lambda}{1 - \lambda}\right)}^{G^{-1}\left(\frac{\mu_1(v, \underline{c}) - \lambda}{1 - \lambda}\right)} s dG(s) \right] > \\ > m_2 (1 - \lambda) \int_0^{G^{-1}\left(\frac{\mu_2(v, \underline{c}) - \lambda}{1 - \lambda}\right)} s dG(s) \quad (1.17)$$

is the necessary and sufficient condition for fully revealing equilibrium not to exist. A sufficient condition for (1.17) is

$$\frac{m_1}{\max\{m_1, m_2\}} (\mu_1(v, \underline{c}) - \mu_1(v, \bar{c})) (a_2 - a_1) \geq (1 - \lambda) E(s) \quad (1.18)$$

Proof: See Appendix 1.E. $\square$

If direct announcements are credible, the government can costlessly inform borrowers about the cost environment. This would restore the equilibrium with full information and make the interest rate cap unnecessary in the typical parameter range. According to the sufficient condition (1.18), moderate search frictions and a high number of safe borrowers are the crucial conditions under which this is not likely.³⁶ In this case, search decisions are sensitive to beliefs, and overoptimism can trigger large increases in search efforts, while

³⁶Note that for more significant search frictions, condition (1.18) is less useful. The higher the search costs are, the more restrictive the sufficient condition (1.18) compared to condition (1.17) is because representing search costs in equilibrium with the maximum possible level becomes less realistic.

searching costs (wasteful or not) are not too high. Figure 1.23 clearly shows that the externality w does not play a role in determining the credibility of direct announcements.

Without complete credibility of direct announcement, the government may want to opt for other transparency tools. In special cases, there may be a partially informative equilibrium with direct announcements where the government randomizes between messages and borrowers cannot fully learn the real cost environment. Other possibility is applying some commitment device that refrains the government from disclosing unwarranted messages. The following examples may also involve some costs: previously established reputation, an automated procedure like a benchmark, delegating the task to an agent with different incentives. A relatively costly alternative is the interest rate cap which can be helpful in a large part of the relevant parameter range of our model depicted by the green area in Figure 1.23.

1.E Proofs

Proof of Proposition 1: When the cost environment is high, no interest rate is optimal if and only if

$$V(\bar{c}, v, \bar{c}) \geq V(\bar{c}, r_1^*, \bar{c})$$

$$V(\bar{c}, v, \bar{c}) \geq V(\bar{c}, r_2^*, \bar{c})$$

The first inequality always holds as $V(\bar{c}, v, \bar{c}) > 0$ and $V(\bar{c}, r_1^*, \bar{c}) = 0$. Rearranging terms in the second inequality results in (1.14).

When the cost environment is low, no interest rate is optimal if and only if

$$V(\underline{c}, v, \underline{c}) \geq V(\underline{c}, r_1^*, \underline{c})$$

$$V(\underline{c}, v, \underline{c}) \geq V(\underline{c}, r_2^*, \underline{c})$$

Rearranging terms in the second inequality results in (1.15). A sufficient condition for the first inequality is

$$V(\underline{c}, r_2^*, \underline{c}) \geq V(\underline{c}, r_1^*, \underline{c})$$

or equivalently

$$\begin{aligned} & m_1 \left[v + w - \underline{c} - a_2 + \mu_1(r_2^*, \underline{c})(\underline{c} + a_2 - \underline{c} - a_1) - (1 - \lambda) \int_0^{G^{-1}\left(\frac{\mu_1(r_2^*, \underline{c}) - \lambda}{1 - \lambda}\right)} sdG(s) \right] \\ & + m_2 \left[v + w - \underline{c} - a_2 + \mu_2(r_2^*, \underline{c})(\underline{c} + a_2 - \underline{c} - a_2) - (1 - \lambda) \int_0^{G^{-1}\left(\frac{\mu_2(r_2^*, \underline{c}) - \lambda}{1 - \lambda}\right)} sdG(s) \right] \geq \\ & \geq m_1 [v + w - \underline{c} - a_1] \end{aligned}$$

$$m_2 [w + a_3 - a_2] \geq m_1 \left[(1 - \mu_1(r_2^*, \underline{c}))(a_2 - a_1) + (1 - \lambda) \int_0^{G^{-1}\left(\frac{\mu_1(r_2^*, \underline{c}) - \lambda}{1 - \lambda}\right)} sdG(s) \right]$$

This always holds, since condition (1.14) and $\mu_1(r_2^*, \underline{c}) = \mu_1(v, \bar{c})$ imply the following relations

$$\begin{aligned} m_2 [w + a_3 - a_2] & \geq m_2 w \geq m_1 \left[(1 - \mu_1(v, \bar{c}))(a_2 - a_1) + (1 - \lambda) \int_0^{G^{-1}\left(\frac{\mu_1(v, \bar{c}) - \lambda}{1 - \lambda}\right)} sdG(s) \right] = \\ & = m_1 \left[(1 - \mu_1(r_2^*, \underline{c}))(a_2 - a_1) + (1 - \lambda) \int_0^{G^{-1}\left(\frac{\mu_1(r_2^*, \underline{c}) - \lambda}{1 - \lambda}\right)} sdG(s) \right] \end{aligned}$$

□

Proof of Proposition 2: Equilibrium beliefs of the borrowers are indeed derived from the equilibrium strategy of the government by the Bayes rule.

$$Pr(\underline{c}|r^*) = \frac{\alpha Pr(r^*|\underline{c})}{\alpha Pr(r^*|\underline{c}) + (1 - \alpha)Pr(r^*|\bar{c})} = \begin{cases} 1 & \text{if } r^* = v \\ 0 & \text{if } r^* = r_2^* \end{cases}$$

Given the rule for the beliefs, market outcomes (pricing and searching decisions) are indeed determined by $\mu_j(r^*, E(c|r^*))$, as it is summarized in Lemma 1 and the discussion before.

Given the rule for the beliefs, and market outcomes consistent with it, the government wants no interest rate cap in high cost environment ($c = \bar{c}$) if and only if

$$V(\bar{c}, v, \bar{c}) = \max_{r^* \in \{r_1^*, r_2^*, v\}} V(\bar{c}, r^*, E(c|r^*))$$

Equivalently,

$$V(\bar{c}, v, \bar{c}) \geq V(\bar{c}, r_1^*, \underline{c})$$

$$V(\bar{c}, v, \bar{c}) \geq V(\bar{c}, r_2^*, \underline{c})$$

The first inequality always holds as $V(\bar{c}, v, \bar{c}) > 0$ and $V(\bar{c}, r_1^*, \underline{c}) = 0$. Since condition (1.14) is equivalent to

$$V(\bar{c}, v, \bar{c}) \geq V(\bar{c}, r_2^*, \bar{c}),$$

it is sufficient for proving the second inequality to show that

$$V(\bar{c}, r_2^*, \bar{c}) \geq V(\bar{c}, r_2^*, \underline{c}).$$

This always holds as spreading optimism has only an effect at market a_1 generating wasteful search efforts.

$$\begin{aligned} & m_1 \left[v + w - \bar{c} - a_1 + \mu_1(r_2^*, \bar{c}) (\bar{c} + a_1 - \bar{c} - a_1) - (1 - \lambda) \int_0^{G^{-1}\left(\frac{\mu_1(r_2^*, \bar{c}) - \lambda}{1 - \lambda}\right)} sdG(s) \right] \geq \\ & \geq m_1 \left[v + w - \bar{c} - a_1 + \mu_1(r_2^*, \underline{c}) (\bar{c} + a_1 - \bar{c} - a_1) - (1 - \lambda) \int_0^{G^{-1}\left(\frac{\mu_1(r_2^*, \underline{c}) - \lambda}{1 - \lambda}\right)} sdG(s) \right] \end{aligned}$$

$$0 \geq -m_1 (1 - \lambda) \int_0^{G^{-1}\left(\frac{\mu_1(r_2^*, \underline{c}) - \lambda}{1 - \lambda}\right)} sdG(s)$$

Given the rule for the beliefs, and market outcomes consistent with it, the government wants to implement an interest rate cap in low cost environment ($c = \underline{c}$) if and only if

$$V(\underline{c}, r_2^*, \underline{c}) = \max_{r^* \in \{r_1^*, r_2^*, v\}} V(\underline{c}, r^*, E(c|r^*))$$

Equivalently,

$$V(\underline{c}, r_2^*, \underline{c}) \geq V(\underline{c}, r_1^*, \underline{c})$$

$$V(\underline{c}, r_2^*, \underline{c}) \geq V(\underline{c}, v, \bar{c})$$

The expanded version of the first condition is

$$\begin{aligned} & m_1 \left[v + w - \underline{c} - a_2 + \mu_1(r_2^*, \underline{c})(\underline{c} + a_2 - \underline{c} - a_1) - (1 - \lambda) \int_0^{G^{-1}\left(\frac{\mu_1(r_2^*, \underline{c}) - \lambda}{1 - \lambda}\right)} sg(s) ds \right] \\ & + m_2 \left[v + w - \underline{c} - a_2 + \mu_2(r_2^*, \underline{c})(\underline{c} + a_2 - \underline{c} - a_2) - (1 - \lambda) \int_0^{G^{-1}\left(\frac{\mu_2(r_2^*, \underline{c}) - \lambda}{1 - \lambda}\right)} sg(s) ds \right] \geq \\ & \geq m_1 \left[v + w - \underline{c} - a_1 + \mu_1(r_1^*, \underline{c})(\underline{c} + a_1 - \underline{c} - a_1) - (1 - \lambda) \int_0^{G^{-1}\left(\frac{\mu_1(r_1^*, \underline{c}) - \lambda}{1 - \lambda}\right)} sg(s) ds \right] \\ & m_2[w + a_3 - a_2] \geq m_1 \left[(1 - \mu_1(r_2^*, \underline{c}))(a_2 - a_1) + (1 - \lambda) \int_0^{G^{-1}\left(\frac{\mu_1(r_2^*, \underline{c}) - \lambda}{1 - \lambda}\right)} sg(s) ds \right] \end{aligned}$$

This always holds, since condition (1.14) and $\mu_1(r_2^*, \underline{c}) = \mu_1(v, \bar{c})$ imply the following relations

$$\begin{aligned} m_2[w + a_3 - a_2] & \geq m_2 w \geq m_1 \left[(1 - \mu_1(v, \bar{c}))(a_2 - a_1) + (1 - \lambda) \int_0^{G^{-1}\left(\frac{\mu_1(v, \bar{c}) - \lambda}{1 - \lambda}\right)} sg(s) ds \right] = \\ & = m_1 \left[(1 - \mu_1(r_2^*, \underline{c}))(a_2 - a_1) + (1 - \lambda) \int_0^{G^{-1}\left(\frac{\mu_1(r_2^*, \underline{c}) - \lambda}{1 - \lambda}\right)} sg(s) ds \right] \end{aligned}$$

The second condition is equivalent to

$$\begin{aligned}
& m_1 \left[v + w - \underline{c} - a_2 + \mu_1(r_2^*, \underline{c})(\underline{c} + a_2 - \underline{c} - a_1) - (1 - \lambda) \int_0^{G^{-1}\left(\frac{\mu_1(r_2^*, \underline{c}) - \lambda}{1 - \lambda}\right)} sg(s) ds \right] \\
& + m_2 \left[v + w - \underline{c} - a_2 + \mu_2(r_2^*, \underline{c})(\underline{c} + a_2 - \underline{c} - a_2) - (1 - \lambda) \int_0^{G^{-1}\left(\frac{\mu_2(r_2^*, \underline{c}) - \lambda}{1 - \lambda}\right)} sg(s) ds \right] \geq \\
& \geq m_1 \left[v + w - \underline{c} - a_3 + \mu_1(v, \bar{c})(\underline{c} + a_3 - \underline{c} - a_1) - (1 - \lambda) \int_0^{G^{-1}\left(\frac{\mu_1(v, \bar{c}) - \lambda}{1 - \lambda}\right)} sg(s) ds \right] \\
& + m_2 \left[v + w - \underline{c} - a_3 + \mu_2(v, \bar{c})(\underline{c} + a_3 - \underline{c} - a_2) - (1 - \lambda) \int_0^{G^{-1}\left(\frac{\mu_2(v, \bar{c}) - \lambda}{1 - \lambda}\right)} sg(s) ds \right] + m_3 w
\end{aligned}$$

or

$$\begin{aligned}
& m_1 [(1 - \mu_1(v, \bar{c}))(a_3 - a_2) - \mu_1(v, \bar{c})(a_2 - a_1) + \mu_1(r_2^*, \underline{c})(a_2 - a_1)] \\
& + m_1 (1 - \lambda) \left[\int_0^{G^{-1}\left(\frac{\mu_1(v, \bar{c}) - \lambda}{1 - \lambda}\right)} sg(s) ds - \int_0^{G^{-1}\left(\frac{\mu_1(r_2^*, \underline{c}) - \lambda}{1 - \lambda}\right)} sg(s) ds \right] \\
& + m_2 \left[(1 - \mu_2(v, \bar{c}))(a_3 - a_2) + (1 - \lambda) \int_0^{G^{-1}\left(\frac{\mu_2(v, \bar{c}) - \lambda}{1 - \lambda}\right)} sg(s) ds \right] \geq m_3 w
\end{aligned}$$

If we use $\mu_1(v, \bar{c}) = \mu_1(r_2^*, \underline{c})$, we get condition (1.16):

$$\sum_{j=1}^2 m_j (1 - \mu_j(v, \bar{c}))(a_3 - a_2) \geq m_3 w.$$

□

Proof of Proposition 3: We will show that for any values of the parameters other than m_2 , m_3 , and w , there is a high enough $m_2 \in (0, 1 - m_1)$ with $m_3 = 1 - m_1 - m_2$ and a high enough $w \in (0, \infty)$ that all three conditions hold.

We define the following functions on $x \in (0, 1 - m_1)$

$$\begin{aligned}
 k(x) &\equiv \frac{m_1}{x} \left[(1 - \mu_1(v, \bar{c})) (a_2 - a_1) + (1 - \lambda) \int_0^{G^{-1}\left(\frac{\mu_1(v, \bar{c}) - \lambda}{1 - \lambda}\right)} sdG(s) \right] \\
 l(x) &\equiv \frac{m_1}{1 - m_1 - x} (1 - \mu_1(v, \underline{c})) (a_3 - a_2) + \frac{x}{1 - m_1 - x} (1 - \mu_2(v, \underline{c})) (a_3 - a_2) \\
 &\quad - \frac{m_1}{1 - m_1 - x} (\mu_1(v, \underline{c}) - \mu_1(r_2^*, \underline{c})) (a_2 - a_1) + \frac{m_1}{1 - m_1 - x} (1 - \lambda) \int_{G^{-1}\left(\frac{\mu_1(r_2^*, \underline{c}) - \lambda}{1 - \lambda}\right)}^{G^{-1}\left(\frac{\mu_1(v, \underline{c}) - \lambda}{1 - \lambda}\right)} sdG(s) \\
 &\quad + \frac{x}{1 - m_1 - x} (1 - \lambda) \int_{G^{-1}\left(\frac{\mu_2(r_2^*, \underline{c}) - \lambda}{1 - \lambda}\right)}^{G^{-1}\left(\frac{\mu_2(v, \underline{c}) - \lambda}{1 - \lambda}\right)} sdG(s) \\
 m(x) &\equiv \frac{m_1}{1 - m_1 - x} (1 - \mu_1(v, \bar{c})) (a_3 - a_2) + \frac{x}{1 - m_1 - x} (1 - \mu_2(v, \bar{c})) (a_3 - a_2)
 \end{aligned}$$

With these functions, conditions (1.14), (1.15), and (1.16) can be rewritten respectively as

$$k(m_2) \leq w$$

$$l(m_2) \leq w$$

$$w \leq m(m_2)$$

It is sufficient to show the following properties.

1. $\frac{\partial k(x)}{\partial x} < 0$ for any $x \in (0, 1 - m_1)$
2. $\lim_{x \rightarrow 0} k(x) = \infty$
3. $\frac{\partial m(x)}{\partial x} > 0$ for any $x \in (0, 1 - m_1)$
4. $\lim_{x \rightarrow 1 - m_1} m(x) = \infty$
5. $l(x) < m(x)$ for any $x \in (0, 1 - m_1)$

This is because properties 1-4 imply that there is a unique intersection of functions k and m at some $\hat{m}_2 \in (0, 1 - m_1)$. Any $m_2 \in (\hat{m}_2, 1 - m_1)$ with $m_3 = 1 - m_1 - m_2$ and $w = m(m_2)$ suffices with the other parameter values together to satisfy conditions (1.14), (1.15), and (1.16).

Only property 5 is not trivial. Expanding the relation $l(x) < m(x)$ yields

$$\begin{aligned} & \frac{m_1}{1-m_1-x} (1-\mu_1(v, \bar{c})) (a_3 - a_2) + \frac{x}{1-m_1-x} (1-\mu_2(v, \bar{c})) (a_3 - a_2) < \\ & < \frac{m_1}{1-m_1-x} (1-\mu_1(v, \underline{c})) (a_3 - a_2) + \frac{x}{1-m_1-x} (1-\mu_2(v, \underline{c})) (a_3 - a_2) \\ & - \frac{m_1}{1-m_1-x} (\mu_1(v, \underline{c}) - \mu_1(r_2^*, \underline{c})) (a_2 - a_1) + \frac{m_1}{1-m_1-x} (1-\lambda) \int_{G^{-1}\left(\frac{\mu_1(r_2^*, \underline{c})-\lambda}{1-\lambda}\right)}^{G^{-1}\left(\frac{\mu_1(v, \underline{c})-\lambda}{1-\lambda}\right)} sdG(s) \\ & + \frac{x}{1-m_1-x} (1-\lambda) \int_{G^{-1}\left(\frac{\mu_2(r_2^*, \underline{c})-\lambda}{1-\lambda}\right)}^{G^{-1}\left(\frac{\mu_2(v, \underline{c})-\lambda}{1-\lambda}\right)} sdG(s) \end{aligned}$$

or

$$\begin{aligned} & [m_1(\mu_1(v, \underline{c}) - \mu_1(v, \bar{c})) + x(\mu_2(v, \underline{c}) - \mu_2(v, \bar{c}))] (a_3 - a_2) \\ & - [m_1(\mu_1(v, \underline{c}) - \mu_1(r_2^*, \underline{c})) + x(\mu_2(v, \underline{c}) - \mu_2(r_2^*, \underline{c}))] (a_3 - a_2) > \\ & > m_1 \left[-(\mu_1(v, \underline{c}) - \mu_1(r_2^*, \underline{c})) (a_3 - a_1) + (1-\lambda) \int_{G^{-1}\left(\frac{\mu_1(r_2^*, \underline{c})-\lambda}{1-\lambda}\right)}^{G^{-1}\left(\frac{\mu_1(v, \underline{c})-\lambda}{1-\lambda}\right)} sdG(s) \right] \\ & + x \left[-(\mu_2(v, \underline{c}) - \mu_2(r_2^*, \underline{c})) (a_3 - a_2) + (1-\lambda) \int_{G^{-1}\left(\frac{\mu_2(r_2^*, \underline{c})-\lambda}{1-\lambda}\right)}^{G^{-1}\left(\frac{\mu_2(v, \underline{c})-\lambda}{1-\lambda}\right)} sdG(s) \right] \end{aligned}$$

Since $\mu_j(v, \bar{c}) = \mu_j(r_2^*, \underline{c})$ for $j \in \{1, 2\}$ we have to show

$$\begin{aligned} 0 > m_1 & \left[-(\mu_1(v, \underline{c}) - \mu_1(r_2^*, \underline{c})) (a_3 - a_1) + (1-\lambda) \int_{G^{-1}\left(\frac{\mu_1(r_2^*, \underline{c})-\lambda}{1-\lambda}\right)}^{G^{-1}\left(\frac{\mu_1(v, \underline{c})-\lambda}{1-\lambda}\right)} sdG(s) \right] \\ & + x \left[-(\mu_2(v, \underline{c}) - \mu_2(r_2^*, \underline{c})) (a_3 - a_2) + (1-\lambda) \int_{G^{-1}\left(\frac{\mu_2(r_2^*, \underline{c})-\lambda}{1-\lambda}\right)}^{G^{-1}\left(\frac{\mu_2(v, \underline{c})-\lambda}{1-\lambda}\right)} sdG(s) \right] \end{aligned}$$

This is true if for $j \in \{1, 2\}$

$$\begin{aligned} & \mu_j(v, \underline{c})(v - \underline{c} - a_j) - (1-\lambda) \int_0^{G^{-1}\left(\frac{\mu_j(v, \underline{c})-\lambda}{1-\lambda}\right)} sdG(s) > \\ & > \mu_j(r_2^*, \underline{c})(v - \underline{c} - a_j) - (1-\lambda) \int_0^{G^{-1}\left(\frac{\mu_j(r_2^*, \underline{c})-\lambda}{1-\lambda}\right)} sdG(s) \end{aligned}$$

We verify this by showing that the following functions are strictly concave in $\mu \in (\lambda, 1)$

$$S_j(\mu) = \mu(v - \underline{c} - a_j) - (1 - \lambda) \int_0^{G^{-1}\left(\frac{\mu - \lambda}{1 - \lambda}\right)} s dG(s)$$

since

$$\begin{aligned} \frac{\partial S_j(\mu)}{\partial \mu} &= v - \underline{c} - a_j - G^{-1}\left(\frac{\mu - \lambda}{1 - \lambda}\right) \\ \frac{\partial^2 S_j(\mu)}{\partial \mu^2} &= -\frac{1}{G'\left(G^{-1}\left(\frac{\mu - \lambda}{1 - \lambda}\right)\right)(1 - \lambda)} < 0 \end{aligned}$$

where the last inequality follows from the strictly increasing G . Consequently, there exists a unique $\hat{\mu}_j \in [\lambda, 1]$ that maximizes $S_j(\mu)$. Since $0 < \frac{\partial S_j(\lambda)}{\partial \mu}$, we have $\lambda < \hat{\mu}_j$. If $\hat{\mu}_j = 1$, which happens exactly when $v - \underline{c} - a_j - \bar{s} > 0$, then

$$\mu_j(r_2^*, \underline{c}) < \mu_j(v, \underline{c}) < \hat{\mu}_j = 1$$

where the first inequality is implied by Lemma 1. If $\hat{\mu}_j \in (0, 1)$, equilibrium condition (1.11) implies

$$\begin{aligned} \frac{\partial S_j(\hat{\mu}_j)}{\partial \mu} = 0 &= [A(\mu_j(v, \underline{c})) - B(\mu_j(v, \underline{c}))](v - \underline{c} - a_i) - G^{-1}\left(\frac{\mu_j(v, \underline{c}) - \lambda}{1 - \lambda}\right) < \\ &< (v - \underline{c} - a_i) - G^{-1}\left(\frac{\mu_j(v, \underline{c}) - \lambda}{1 - \lambda}\right) = \frac{\partial S_j(\mu_j(v, \underline{c}))}{\partial \mu} \end{aligned}$$

This together with concavity of S_j imply the second inequality below, while the first inequality is a consequence of Lemma 1:

$$\mu_j(r_2^*, \underline{c}) < \mu_j(v, \underline{c}) < \hat{\mu}_j < 1$$

In either case, $0 < \frac{\partial S_j(\mu)}{\partial \mu}$ for each $\mu \in [\mu_j(r_2^*, \underline{c}), \mu_j(v, \underline{c})]$, which implies the relations that we wanted to establish:

$$S_j(\mu_j(r_2^*, \underline{c})) < S_j(\mu_j(v, \underline{c})).$$

□

Proof of Proposition 4: The nontrivial results follow from Lemma 1 and the expression (1.12) for the average equilibrium interest rate. □

Proof of Proposition 5: Without loss of generality, suppose that there is a fully revealing equilibrium with equilibrium messages $m(c) = c$. Bayes rule implies that $E(c|m = c) = c$, which triggers searching decisions aggregated in $\mu_j(v, c)$. Anticipating all of these, the government finds the communication rule $m(c) = c$ optimal if and only if

$$V(\underline{c}, v, E(c|m = \underline{c})) \geq V(\underline{c}, v, E(c|m = \bar{c}))$$

$$V(\bar{c}, v, E(c|m = \bar{c})) \geq V(\bar{c}, v, E(c|m = \underline{c}))$$

The first inequality is the following.

$$\begin{aligned} & \sum_{j=1}^2 m_j \left[v + w - \underline{c} - a_3 + \mu_j(v, \underline{c}) (\underline{c} + a_3 - \underline{c} - a_j) - (1 - \lambda) \int_0^{G^{-1}\left(\frac{\mu_j(v, \underline{c}) - \lambda}{1 - \lambda}\right)} sdG(s) \right] + m_3 w \geq \\ & \geq \sum_{j=1}^2 m_j \left[v + w - \underline{c} - a_3 + \mu_j(v, \bar{c}) (\underline{c} + a_3 - \underline{c} - a_j) - (1 - \lambda) \int_0^{G^{-1}\left(\frac{\mu_j(v, \bar{c}) - \lambda}{1 - \lambda}\right)} sdG(s) \right] + m_3 w \\ & \sum_{j=1}^2 m_j \left[(\mu_j(v, \underline{c}) - \mu_j(v, \bar{c})) (a_3 - a_j) - (1 - \lambda) \int_{G^{-1}\left(\frac{\mu_j(v, \bar{c}) - \lambda}{1 - \lambda}\right)}^{G^{-1}\left(\frac{\mu_j(v, \underline{c}) - \lambda}{1 - \lambda}\right)} sdG(s) \right] \geq 0 \end{aligned}$$

This is true if for $j \in \{1, 2\}$

$$\begin{aligned} & \mu_j(v, \underline{c}) (v - \underline{c} - a_j) - (1 - \lambda) \int_0^{G^{-1}\left(\frac{\mu_j(v, \underline{c}) - \lambda}{1 - \lambda}\right)} sdG(s) > \\ & > \mu_j(v, \bar{c}) (v - \underline{c} - a_j) - (1 - \lambda) \int_0^{G^{-1}\left(\frac{\mu_j(v, \bar{c}) - \lambda}{1 - \lambda}\right)} sdG(s) \end{aligned}$$

This can be shown analogously to the argument used in the proof of Proposition 3. The

following functions are strictly concave in $\mu \in (\lambda, 1)$

$$S_j(\mu) = \mu(v - \underline{c} - a_j) - (1 - \lambda) \int_0^{G^{-1}\left(\frac{\mu - \lambda}{1 - \lambda}\right)} sdG(s)$$

since

$$\begin{aligned} \frac{\partial S_j(\mu)}{\partial \mu} &= v - \underline{c} - a_j - G^{-1}\left(\frac{\mu - \lambda}{1 - \lambda}\right) \\ \frac{\partial^2 S_j(\mu)}{\partial \mu^2} &= -\frac{1}{G'\left(G^{-1}\left(\frac{\mu - \lambda}{1 - \lambda}\right)\right)(1 - \lambda)} < 0 \end{aligned}$$

where the last inequality follows from the strictly increasing G . Consequently, there exists a unique $\hat{\mu}_j \in [\lambda, 1]$ that maximizes $S_j(\mu)$. Since $0 < \frac{\partial S_j(\lambda)}{\partial \mu}$, we have $\lambda < \hat{\mu}_j$. If $\hat{\mu}_j = 1$, which happens exactly when $v - \underline{c} - a_j - \bar{s} > 0$, then

$$\mu_j(v, \bar{c}) < \mu_j(v, \underline{c}) < \hat{\mu}_j = 1$$

where the first inequality is implied by Lemma 1. If $\hat{\mu}_j \in (0, 1)$, equilibrium condition (1.11) implies

$$\begin{aligned} \frac{\partial S_j(\hat{\mu}_j)}{\partial \mu} = 0 &= [A(\mu_j(v, \underline{c})) - B(\mu_j(v, \underline{c}))](v - \underline{c} - a_i) - G^{-1}\left(\frac{\mu_j(v, \underline{c}) - \lambda}{1 - \lambda}\right) < \\ &< (v - \underline{c} - a_i) - G^{-1}\left(\frac{\mu_j(v, \underline{c}) - \lambda}{1 - \lambda}\right) = \frac{\partial S_j(\mu_j(v, \underline{c}))}{\partial \mu} \end{aligned}$$

This together with concavity of S_j imply the second inequality below, while the first inequality is a consequence of Lemma 1:

$$\mu_j(v, \bar{c}) < \mu_j(v, \underline{c}) < \hat{\mu}_j < 1$$

In either case, $0 < \frac{\partial S_j(\mu)}{\partial \mu}$ for each $\mu \in [\mu_j(v, \bar{c}), \mu_j(v, \underline{c})]$, which implies the relations we wanted to establish:

$$S_j(\mu_j(v, \bar{c})) < S_j(\mu_j(v, \underline{c})).$$

The equivalent form of the second incentive constraint is the following.

$$\begin{aligned}
& m_1 \left[v + w - \bar{c} - a_2 + \mu_1(v, \bar{c}) (\bar{c} + a_2 - \bar{c} - a_1) - (1 - \lambda) \int_0^{G^{-1}\left(\frac{\mu_1(v, \bar{c}) - \lambda}{1 - \lambda}\right)} s dG(s) \right] + m_2 w \geq \\
& \geq m_1 \left[v + w - \bar{c} - a_2 + \mu_1(v, \underline{c}) (\bar{c} + a_2 - \bar{c} - a_1) - (1 - \lambda) \int_0^{G^{-1}\left(\frac{\mu_1(v, \underline{c}) - \lambda}{1 - \lambda}\right)} s dG(s) \right] \\
& \quad + m_2 \left[w - (1 - \lambda) \int_0^{G^{-1}\left(\frac{\mu_2(v, \underline{c}) - \lambda}{1 - \lambda}\right)} s g(s) ds \right] \\
& m_1 \left[(\mu_1(v, \underline{c}) - \mu_1(v, \bar{c})) (a_2 - a_1) - (1 - \lambda) \int_{G^{-1}\left(\frac{\mu_1(v, \bar{c}) - \lambda}{1 - \lambda}\right)}^{G^{-1}\left(\frac{\mu_1(v, \underline{c}) - \lambda}{1 - \lambda}\right)} s g(s) ds \right] \leq \\
& \leq m_2 (1 - \lambda) \int_0^{G^{-1}\left(\frac{\mu_2(v, \underline{c}) - \lambda}{1 - \lambda}\right)} s g(s) ds
\end{aligned}$$

The latter is a contradiction with condition (1.17). This proves the first part of the proposition. After rearranging terms and using $\mu_2(v, \underline{c}) = \mu_1(v, \bar{c})$, condition (1.17) becomes

$$\begin{aligned}
& m_1 (\mu_1(v, \underline{c}) - \mu_1(v, \bar{c})) (a_2 - a_1) > \\
& > m_1 (1 - \lambda) \int_{G^{-1}\left(\frac{\mu_1(v, \bar{c}) - \lambda}{1 - \lambda}\right)}^{G^{-1}\left(\frac{\mu_1(v, \underline{c}) - \lambda}{1 - \lambda}\right)} s dG(s) + m_2 (1 - \lambda) \int_0^{G^{-1}\left(\frac{\mu_1(v, \bar{c}) - \lambda}{1 - \lambda}\right)} s dG(s)
\end{aligned}$$

which is implied by

$$m_1 (\mu_1(v, \underline{c}) - \mu_1(v, \bar{c})) (a_2 - a_1) > \max \{m_1, m_2\} (1 - \lambda) E(s)$$

□

1.F Dispersion in interest rates

Here we estimate the dispersion of interest rates within a certain point in time and district.

The aim of this analysis is to bring theoretical considerations closer to empirics, i.e. test

those results of the theoretical model that data allow us to do so. More precisely we want to show suggestive empirical evidence regarding that there is price dispersion in the housing loan market. Moreover, we also want to show how price dispersion is related to the number of banks in a certain market. In our approach all the borrowers who take out their loan in the same district in the same quarter belong to the same market. Ideally we would show that identical borrowers may end up with different interest rates on the same market and this is based on the price setting mechanism and search friction that was discussed in the previous section. However, as we cannot observe all characteristics of the borrowers we can only estimate dispersion of prices conditional on the observed factors of a certain loan and borrower. Consequently, these results may be looked at as a maximum estimate of price dispersion.

We estimate the following equation:

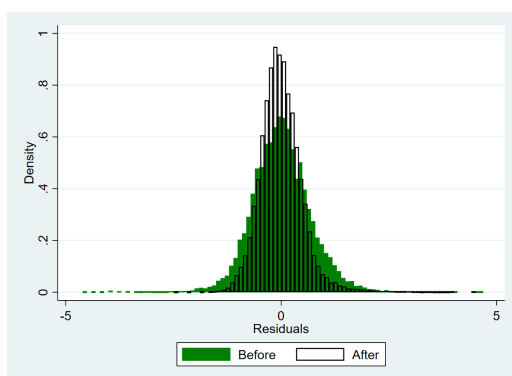
$$Y_{i,d,t} = \alpha + \beta C_{i,d,t} + \delta L_{i,d,t} + \phi_i f_{i,d,t} + \gamma B_{i,d,t} + \mu H_{d,t} + \omega B_{d,t} \times H_{d,t} + \Theta T_t + \varepsilon_i \quad (1.19)$$

where Y_i is the annual percentage rate of loan i , C_i is a vector of client related variables, L_i is a vector of loan related variables, f_i is financing cost of the loan, B_i vector of indicator variables marking the granting bank, H_i is the Herfindahl Hirschmann index. This equation is similar to 1.3, the only difference is that here we include time fixed effects³⁷. We estimate the model before and after three quarters of the program introduction. We take the prediction error of these models and compare them. We also compare how price dispersion comoves with the number of banks. We find that the higher the number of banks, the higher the price dispersion. Moreover, we also show that high number of banks mainly related to how low interest rates comove with number of banks. We calculate the evolution of low interest rates (5th percentile of interest rates) and high interest rates (95th percentile of interest rates) within a given market in time and plot these figures

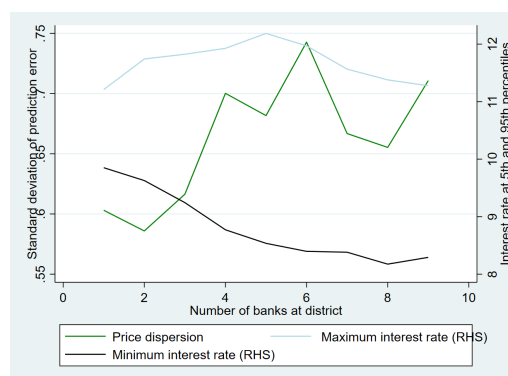
³⁷We use different notation here to emphasize that loans have time and place dimension, however, the unit of observation is the same as in 1.3

against the number of banks in that market. Comparing those markets where the number of banks are low to those where the number of banks are large we see that low interest rates are considerably lower, while high interest rates are on the same level. This finding is consistent with the empirical model where price dispersion is related to the number of banks and driven by pricing closer and closer to marginal costs when the competition due to higher number of banks makes it necessary for banks.

Figure 1.24: Price dispersion (left) and its evolution with bank number and lowest interest rates



(a) Price dispersion



(b) Price dispersion, lowest interest rates and number of banks

Chapter 2

Birds of a Feather Indebted Together: Solutions to the Information Problem in the Case of Mortgage Loans

Abstract

We examine peer effects in mortgage borrowing decisions. We find that having financially literate colleagues improves the borrowing decisions of financially less literate co-workers. Interest rates on the mortgage loans of these co-workers are significantly lower than for similar employees at other companies, where their peers have lower financial literacy. The magnitude of the effect is economically significant, amounting to roughly 4 to 5 monthly instalments until maturity. The results are heterogeneous: advice is more valuable for borrowers with low mathematical skills, and the peer effect is considerably higher in districts, where competition is weaker among banks. We also find that introducing a standardised loan product can offset the impact of the peer effect by making the decision problem of borrowers less complex.

Keywords: peer effects, skills, borrowing decisions, mortgage loan, standardised loan product

JEL codes: J24, G21, G41

2.1 Introduction

Taking out a mortgage loan is one of the most important decisions that people make from a financial perspective. Mortgage loans are complex products, and considerable effort and financial knowledge are required to collect, understand and compare the information on the available bank offers¹ Moreover, professional help does not necessarily serve the borrowers due to conflict of interests of the intermediaries.² As a result consumers often make mistakes as demonstrated for example by Agarwal et al. (2015a) and Agarwal and Mazumder (2013). Little is known about the factors that drive these mistakes.

In this paper we analyse two salient remedies to cope with financial mistakes: informal advice and regulation. In our special setup we are also able to account for the interaction of these two aspects. We show that the informal advice of colleagues is relevant in the decision on a mortgage loan and that the financial literacy of the colleagues matters as well. We proxy financial literacy by mathematical skills and by experience on the mortgage loan market.³ To identify the impact of peers' financial literacy we use the variation in timing: upon taking out a mortgage, some borrowers have highly skilled colleagues who are already experienced with mortgage loans, while other borrowers do not have such colleagues. Therefore, we compare borrowers who are similar in most characteristics but differ in respect of the financial literacy of their peers. We find that borrowers who have low skills, but have the financially most literate colleagues tend to take mortgage loans with interest rates that around 0.2 percentage point lower than similar borrowers who have less literate colleagues, which translates into savings that amount to four to five monthly instalments when considering the total term of the mortgage. Finally, we show that peer effects fade with regulation – for example, with the introduction of a standardised loan product – suggesting that regulation may help alleviate the information problem

¹For example, Lusardi and Mitchell (2014), Gerardi, Goette, and Meier (2010), Campbell (2016), Barr, Mullainathan, and Shafir (2008), Heidhues, Johnen, and Kőszegi (2021)

²e.g. Inderst and Ottaviani (2012), Fan and Chatterjee (2017)

³Based on, for example Gerardi, Goette, and Meier (2010), Lusardi and Mitchell (2007), Lusardi and Mitchell (2014), Gaudecker (2015) and Frijns, Gilbert, and Tourani-Rad (2014)

of households.

To study this issue, we combine three administrative datasets that cover the entire universe of borrowers in Hungary. We merge the Hungarian credit registry database for the period 2015-2019 with the Hungarian administrative linked employer-employee database. In this manner, we can establish a co-worker network and see borrowing decisions as well. From the credit registry, we use key loan characteristics (e.g. loan amount, term of the loan, repricing period, bank of origination etc.) as well as demographic variables such as age and district of the property. Using data from the National Tax and Customs Administration we can observe balance sheet information as well as the four digit NACE code. We also use skill sets for occupational categories from the ONET database⁴.

In our empirical strategy we use the interest rate of the loan as the main dependent variable. We look at that as the price of the loan while we control for most credit risk related demographic variables, including the borrower's own mathematical skill. In some specifications we include a large set of loan characteristics as well. Moreover, we also analyse our main specification with alternative outcomes where we replace the interest rate of the loan with other loan characteristics such as repricing period, maturity and total loan amount. In one specification we also include bank fixed effects to test if the choice of bank may be part of the informal advice. Our key independent variable is the average financial literacy of a borrower's peers. We proxy financial literacy with the level of mathematical skill from ONET and experience in the mortgage loan market, as it is well documented in the literature that these aspects are important in financial decisions (e.g. Agarwal and Mazumder (2013), Lusardi and Mitchell (2007), Lusardi and Mitchell (2014), Folke et al. (2021), Frijns, Gilbert, and Tourani-Rad (2014)).

If studying peer effects, at least two challenges arise that need to be addressed. First, the definition of peer groups may be ambiguous, as researchers rarely have detailed information on the subject's social network. We treat all of the colleagues of an employee as her

⁴ONET reports required level of skills for 873 occupations. We can merge these required skills based on the occupation of borrowers that we observe in the administrative data

peers, but we make a distinction between the relevance of different peers. Based on the homophily literature (e.g. Currarini, Jackson, and Pin (2009)) we assign a high weight to colleagues who share similar characteristics with the borrower and a low weight to colleagues who are less similar. In doing so we follow the approach of Giorgi, Frederiksen, and Pistaferri (2020). Our main results are robust to the variation of applied weighting scheme.

Second, following the terminology of Manski (1993) we face the reflection problem and the problem of correlated effects. We focus on the past decisions of peers, and can thus avoid the problem of simultaneity in peers' decisions (reflection problem). Regarding correlated effects, we pursue two strategies. We include a large set of control variables including demographic controls, company fixed effects and the interaction of industry and time fixed effects. Therefore, we compare borrowers within the same firm and same time period also taking into account industry level and local shocks. Moreover, we include the sales of the company to control for company level time-varying shocks that are related to economic conditions of the company. We argue that after including this large set of controls it is unlikely that plausible endogeneity problems could distort our results. Our identification is based on the change in the composition of borrowing colleagues over time. As a robustness check we also control for regional-level bank supply shocks by adding the interaction of bank, district and time fixed effects.

However, even including such a large set of fixed effects does not fully eliminate the threat of time-varying local shocks at the peer group level. We perform a placebo test: we replace the peer group that contains only borrowers who have taken out a mortgage loan in the *recent past* with a peer group that contains borrowers who take out a loan in the *near future*. We argue that if the peer effect functions according to our narrative then the peers taking out a loan in the near future are assumed to have no impact on current borrowers. However, if the impact that we measure is solely due to time-varying common shocks and not due to the peer effect, then that shock should have an impact on both the borrowers and the peers who take their loans in the future. Consequently, in that case our

placebo test would result in significant coefficients. The fact that the key coefficient in our placebo test is insignificant and the point estimate is small suggests that time-varying common shocks do not distort our results.

We also analyse whether peers are related to the borrower's choice of bank. We model the borrowers' choice by running a conditional logit model where the borrower has not only two but eleven⁵ choices. The key dependent variable in this analysis is the share of colleagues in the borrower's network who have chosen the given bank. We also run a placebo test, similar to the previous placebo test, to ensure that no time-varying common shocks distort our results.

According to our main results a borrower who has financially literate peers tends to take out loans at a lower interest rate compared to one whose peers have low skills. There is considerable heterogeneity in this effect, and the lower the own skills of a borrower the stronger the peer effect is. Similarly, peer effect is stronger in districts where bank competition is low. Comparing a borrower who either has low skills or lives in districts where competition is low, the estimated effect of having financially literate peers is between 0.15 - 0.24 percentage point compared to a similar borrower who does not have such colleagues. These results are consistent with the narrative that peers' advice is most valuable if it is difficult for the borrower to find a good loan. The estimated effect accounts for four to five monthly installments taking into consideration all repayments until maturity.

Among the alternative outcomes, we find a statistically and economically significant impact only in the case of the loan amount. The presence of financially literate peers leads to higher loan amounts, especially in the case of borrowers with low mathematical skills: the measured impact can be up to 14 per cent.⁶ With the interest rate as the dependent

⁵We have ten large banks plus one category for all the remainder small banks adding up to eleven institutions in the sample.

⁶According to the results those borrowers with low mathematical skill whose experienced peers' average mathematical skills is at the 90th percentile of the distribution of mathematical skill tend to take 14 per cent higher amount of loan on average than those, whose experienced peers' average mathematical skill is at the 10th percentile.

variable and including all loan characteristics as controls, we obtain the same result in qualitative terms as the original regression without additional controls. Including bank fixed effects decreases the measured impact of peers by one-third, suggesting that one important channel of peer effect is the choice of bank.

Results on bank choice strengthen these observations. If all of the colleagues in a borrower's network have chosen the same bank, then the borrower's chance of choosing that bank is more than 20 per cent higher⁷ compared to a borrower whose colleagues in his network have not chosen that bank. Placebo tests again show no significant result, suggesting that no time-varying common shocks distort the results.

How could regulation improve mortgage loan market outcomes? How could regulation be related to peer effects? To answer these questions we analyse the impact of introducing a standardised product, the Certified Consumer Friendly Housing Loan (CCFL) on the mortgage loan market at the end of 2017. One of the goals of the Central Bank of Hungary in introducing such a loan was to support borrowers by decreasing the complexity of the problem of finding a suitable loan. We exploit the exogenous variance in the spread of the product among districts over time. We argue that the spread of this product after using a large set of controls in the regression – including a bank competition proxy – is orthogonal to our key variable of interest: the average financial literacy of peers. As a robustness check we also carry out an IV strategy. The validity of the IV is based on the assumption that there was random variation in the spread of CCFL in the first few months after the introduction of this product.⁸ We find that the spread of CCFL is negatively related to the peer effect and also negatively related to interest rates, i.e. peer effect diminishes when

⁷Regarding the baseline probability of choosing that bank. For example, if the baseline probability of that borrower *i* chooses bank *A* is 10 per cent, then it increases to 12 per cent.

⁸We argue that the spread shortly after the introduction of the product was exogenous. Our instrument is based on the share of CCFL among all housing loans at the district level in the first half-year after its introduction of CCFL. The idea behind such an IV is that the introduction of the programme was based on the cooperation of CBH and the product development departments of banks. Therefore, depending on a given bank's product development capacity the bank could either come up with the product early, or spent more months to be able to finalise the exact details of the loan. As a result in some districts CCFL was already offered by most banks in the first few months after its introduction of CCFL, while in some others the product appeared later. The instrumented variable is CCFL share in housing loans per districts observed on a monthly basis.

CCFL appears on the market. IV results reinforce these findings. Therefore, we argue that CCFL has potentially helped borrowers to solve their information problem and also improved the fairness of the mortgage loan market.

Our research contributes to four strands in the literature. First, to the best of our knowledge, there is no research on how peers affect the choices of borrowers in the mortgage loan market. The closest paper is the research of Maturana and Nickerson (“Teachers Teaching Teachers: The Role of Workplace Peer Effects in Financial Decisions”), which analyses the impact of co-workers on the decision of whether to refinance a mortgage. Although the topic may seem similar, their research differs in three relevant aspects. The problem that borrowers face in our research is mainly related to complexity and search costs, while in Maturana and Nickerson (“Teachers Teaching Teachers: The Role of Workplace Peer Effects in Financial Decisions”) it is a well-defined decision that lacks complexity issues. Second, we also show results on the skills of borrowers and peers. Third, we also analyse the impact of regulation on peer effect. There are also other papers that focus on peer effects in financial decisions (e.g. Ouimet and Tate (2020), Duflo and Saez (2002)), but again, the problem of the actor in our case (complexity and search costs) is strikingly different (to participate or not in company pension or investment programmes). Lastly, according to our current understanding it is unique in the finance literature that we have different peer groups for each individual. There are studies (see previous examples) which deal with peer groups in financial decisions, but in these specifications the peer groups do not vary on individual level, while in our setup they do.

Second, our results also add to the literature on financial advice. Campbell (2016) argues that research on *formal* financial advice shows discouraging results (see e.g. Foerster et al. (2017) and Bhattacharya et al. (2024)). There is also rare evidence that *informal* financial advice can improve the decision on complex financial problems. In a recent paper, Ambuehl et al. (2022) argues that there is lack of evidence on whether informal advice improves or deteriorates financial decisions. According to our results, financially literate peers improve the borrowing decision, while peers with lower skills are not relevant in the

decision.

Third, our research also contributes to the findings of the literature on financial regulation. The results of the literature are mixed regarding the optimal regulation. On the empirical side for instance Nicholson, Skelton, and Tarr (2019) show the drawbacks of regulation on the Australian mortgage loan market, while Barr, Mullainathan, and Shafir (2008) argues why the Truth in Lending Act was inefficient to improve the US mortgage loan market and what other measures could help. Agarwal et al. (2015b) show how the 'CARD act' improved credit card market conditions. On the theoretical side, there are also several studies that try to pin down how financial markets work and then suggest adequate regulation. Heidhues, Johnen, and Kőszegi (2021) argue that caps on hidden prices of products may help in achieving perfect competition and also mention that certificates are less likely to make a difference. Both Campbell (2016) and Barr, Mullainathan, and Shafir (2008) call for special disclosure rules. We find that special regulation may be able to improve mortgage borrowers' situation, decrease the peer effect and therefore work against inequality. The regulation that we analyse however, has several features that were implemented at the same time (e.g. cap on margin, issue certification by CBH, control over special terms of loans), and therefore we cannot pin down the impact of those features one-by-one, but only on aggregate.

Lastly, we show that peer effects contribute to inequality. Chetty et al. (2022) argues that social capital may help in social mobility. We find similar results, as high social capital ('having smart colleagues') improves the borrowing decision. Conversely, our results also highlight that social capital may be positively related to inequality. Our research also loosely connects to the herding literature, where agents change their mind in a financial decision and follow the crowd (see Spyrou (2013), for example). The peer effect can cause herding, but during our examined period the economic environment was calm and we did not observe herding behaviour. Furthermore, in herding models agents observe each others' previous financial choices and make their own decision (see e.g. Cipriani and Guarino (2014) or Park and Sabourian (2011)); in our setup, however, borrowers get

informal advice and are not just simply copying others' decisions.

The study is organised as follows. In Section 2.2 we present the databases and descriptive statistics. In Section 2.3 we show the identification strategy. We carry out the estimation and discuss the results in Section 2.4. Conclusions are presented in Section 2.5.

2.2 Database

We use three different data sources to test our research hypothesis. We merge these three databases using individual identifiers and occupational codes.

2.2.1 Hungarian State Treasury Database

The database of the Hungarian State Treasury (HST) contains the entire population, who had at least one day of legal work between 1997-2019. The following variables are used in the study:

- district level place of residence,
- four digit occupational code (FEOR),
- firm identifier.

There are 198 districts in Hungary, with an average population of 55 thousand. One district can be considered as a local labour market, because according to the 2011 Census, except for Budapest and its surroundings, 80% of the residents live and work in the same district. This ratio is 72% for Budapest and its commuting area.

For every debtor, we choose the firm, where he earns the most as an employee. Based on this definition we can identify the co-workers, which is an essential measure in our calculations.

2.2.2 ONET database

We define skills based on the ONET database, which contains detailed information on the level of skills that are needed for specific occupations. Based on the literature mathematical skill is one of the most important skills in financial decisions (e.g. Lusardi and Mitchell (2007), Lusardi and Mitchell (2014), Folke et al. (2021)), and therefore we focus on the mathematics score. It is measured on a scale of 0 to 100 which we normalise to be between 0 and 1 in our estimations. The ONET dataset is based on the international system of occupations categories (ISCO-08). We can merge the international occupational categories with the four-digit Hungarian counterparts (FEOR-08) but there are cases, where we cannot find an exact match for some Hungarian occupational categories. The matching rate is 73%. To increase the number of observations we can use in our analysis, we average the mathematical score for every three-digit occupational category and use this average if the four-digit mathematical score is missing. In this case, we have mathematical scores for 87% of the total observations.

2.2.3 Credit Registry Database

The Credit Registry (CR) database contains all loans taken out by individuals. We use the 2015-2019 time period⁹. We observe the day the loan was taken out, the total loan amount, the interest rate, maturity and repricing period of the loan, the bank that granted the loan, the place of residence of the borrower, and also the age and wage of the borrower.

After merging all datasets we have almost 80 thousand observations¹⁰. In the main estimations due to constraints on the minimum number of employees, we are able to use more than 73 thousand observations in most estimations. Table 2.1 shows the descriptive statistics of the merged dataset.

⁹The reason for this is that the earliest point in time where we have detailed information on loans is 2015 and we wanted to end our analysed period before the Covid crisis started

¹⁰Total number of newly disbursed housing loans were larger than 80 thousand, however we lose around one third of our observations when we merge credit registry data to HST data. The reason is mainly that

Table 2.1: Descriptive statistics of the merged dataset

	Mean	10 th percentile	90 th percentile	Standard deviation
Interest rate	5.1	3.5	6.7	1.3
Maturity (year)	15.3	7.0	25.0	6.5
Repricing period (month)	76.4	12.0	121.0	54.8
Principal (EUR)	24885.0	7142.9	47619	20419.4
Wage (EUR)	1323	519.9	2288.3	816.3
Age	36.6	27.0	48.0	8.3
Bank concentration	0.3	0.2	0.4	0.1
Mathematics	0.4	0.3	0.6	0.1
Mathematics average of colleagues	0.4	0.3	0.5	0.1
Observations	79135			

All mortgage loans are denominated in HUF; we calculated EUR values using an exchange rate of EUR/HUF 314.95, which was the average exchange rate in 2015-2019.

2.3 Empirical approach

2.3.1 Network

We face two types of challenges when we want to identify the peer effect on borrowing decisions. One problem is related to the identification of peer groups, and several endogeneity-related concerns also arise regarding the estimation. We start by describing how we construct peer groups from the available data and then turn to discussing the endogeneity problems.

To define peer groups that may be relevant in helping to choose a good loan the researcher would ideally know the social network of borrowers. However, data on exact social network is rarely available, and therefore researchers often use other alternative data sources. Several studies exploit the fact that colleagues tend to know each other well and potentially share information on their own experiences regarding financial products (Ouimet and Tate (2020)), charity activities (Lieber and Skimmyhorn (2018)), consumption, parental leave (Dahl, Loken, and Mogstad (2014)), etc. We observe all of the colleagues of the borrowers, and therefore we can thus construct peer groups of colleagues.

We do not have precise information on the informal network of colleagues, and therefore

not all loan takers are present in the HST data, e.g. self-employed borrowers are missing.

we build on the homophily literature to define a realistic network structure. We cannot assume that a peer group consists of all of the colleagues of a borrower: for example in the case of large companies it is likely that a borrower knows only a small fraction of all employees. We do not have information regarding these ties. However, following Giorgi, Frederiksen, and Pistaferri (2020) we assume that employees with similar characteristics have a high likelihood of knowing each other. Their approach is consistent with the literature on homophily: McPherson, Smith-Lovin, and Cook (2001) surveys the literature and argues that more similar people tend to be more influential. They argue that the most relevant characteristics regarding homophily are race, religion, gender, age, education and occupation. Currarini, Jackson, and Pin (2009) build a structural model that rationalises that similar people tend to make more friendships than dissimilar ones. Stolper and Walter (2019) also find that homophily plays an important role in financial decisions.

Following Giorgi, Frederiksen, and Pistaferri (2020) we create a distance measure between every pair of employees. This measure is based on education and occupation, however as we do not have data on education we use wage as a proxy for it. We also use occupation and add age as well when calculating the distance metric. We define distance in the following way. First we evenly distribute all employees into three buckets of age (younger than 32 years, between 32-41 years, older than 41 years) and three buckets of wage (less than €953 , between €953 - €1587, more than €1587). We also split borrowers into three buckets based on their occupation (managerial, white collar and blue collar occupations). We assign integers 1,2 and 3 to the three buckets and then define the distance as the sum of the absolute deviations between the buckets of employee i and employee j :

$$d_{i,j} = (|age_i - age_j| + |wage_i - wage_j| + |occupation_i - occupation_j|) \quad (2.1)$$

We use the distance measure $d_{i,j}$ to assign weights to all colleagues of the borrowers.

We continue to follow Giorgi, Frederiksen, and Pistaferri (2020) and use the distance measure as weights when we calculate peers' average characteristics. Similarly to Giorgi, Frederiksen, and Pistaferri (2020) we also use quadratic weighting scheme in order to add higher weights to the more similar colleagues and lower weights to the less similar colleagues. The weights are defined as:

$$\omega_{i,j} = (d_{i,j} + 1)^{-2} \quad (2.2)$$

The weighted average of any characteristic x (for instance mathematics score) of the colleagues of borrower i who works at company c and takes out a loan in period t is then calculated by the formula:

$$\overline{x_{i,c,t}} = \left(\sum_{i,j \neq i} \omega_{i,j} \right)^{-1} \sum_{i,j \neq i} \omega_{i,j} x_{j,c,t} \quad (2.3)$$

As this weighting scheme is somewhat ad-hoc, we also apply linear weights and run the main specifications based on that weighting scheme as well. Changing the weighting scheme does not significantly change our results; the estimation results can be found in Appendix 2.A.

2.3.2 Baseline model

Turning to the estimation of peer effects there are at least three challenges that should be handled to be able to identify peer effects. These are i) simultaneity, ii) those omitted variable biases that are due to the fact that peer groups are formed endogenously; and iii) common shocks. Manski (1993) refers to i) as reflection problem while ii) and iii) are known together as correlated effects. To avoid the problem of simultaneity we focus on peers who made their choice earlier than the borrower: at least three months and at most one year before. We assume that a borrower who is currently trying to find the

appropriate loan has no direct effect in this search process on a peer who took out a loan in the aforementioned time window. This assumption is based on the observation that financial experience is an important building block in financial literacy (Frijns, Gilbert, and Tourani-Rad (2014), Weiner et al. (2005), Mandell (2006)). One could say that good understanding of the mortgage loan market requires not only good skills, but experience in taking out loan as well.

An example for the second problem (sorting) is that if all colleagues have a high level of education, it obviously helps them in making a good choice among mortgage loan offers. It can happen that the researcher does not measure the impact of peers on each other but rather measures the similarity of peers regarding the unobserved factor of education. In this case, it is this omitted variable that makes peers' decisions similar and the estimated peer effect is biased. This problem can be partially solved by including company fixed effects. These can capture the impact of all company level time-invariant unobserved factors (such as a special offer of a bank to a given company). Time-variant company level shocks, such as the impact of sorting due to laying off (hiring) employees whose skills are low (high) during an economic downturn (upturn) are not captured by company fixed effects. We interact month fixed effects with industry fixed effects to control for time varying industry level shocks. These fixed effects also control for the downward trend in interest rates during the sample period. We believe that this control rules out most time-varying unobserved shocks. Nevertheless, company level, (or even peer group level) time-varying shocks could still distort our estimation. To show that such common shocks do not bias our result we carry out a placebo test that is discussed after the main results have been shown.

Following the terminology of Manski what we want to identify is the contextual (exogenous) peer effect: the effect of the characteristics of peers on the choice of the borrower. The equation that we estimate by OLS regression is the following:

$$y_{i,c,t} = \beta X_{i,c,t} + \delta \overline{X_{i,c,t-1}} + \phi S_{c,t} + \Omega_c + \Theta_{g,t} + \varepsilon_{i,c,t} \quad (2.4)$$

where $y_{i,c,t}$ stands for the interest rate of the mortgage loan of borrower i , who works at company c and has taken out the loan in period t . As a mortgage loan is a product that is usually not taken out more than one or two times in a borrower's lifetime, in our narrow time window of five years we observe borrowers taking out loans only once. However, we use t subscript to indicate that our data has a time dimension as well; this is important to be able to estimate company and time fixed effects, as we discuss below. Similarly, we use c subscript to indicate that in our estimation we keep track of the company of the employee as well.

$X_{i,c,t}$ includes all of the borrower level explanatory variables that we include in the regression. These are wage, age and age squared of the borrower and the own mathematics score associated with the borrower's occupation. $\overline{X_{i,c,t-1}}$ includes the colleagues' weighted average of the same variables. The weighted average only includes the colleagues, who have taken out a mortgage loan in the time window of a minimum of three months and a maximum of twelve months before the borrower takes out a loan. $S_{c,t}$ stands for the logarithm of sales of company c in period t . This control variable can capture shocks that are related to the changing economic conditions of the company. Ω_c is a company fixed effect and $\Theta_{g,t}$ is an industry $\times$ time (month) fixed effect, while $\varepsilon_{i,c,t}$ is an individual level shock (error term). The company fixed effect absorbs all of the unobserved time-invariant company-level shocks, while the interacted industry-time fixed effect absorbs all of the industry level time-varying shocks. By including these fixed effects, we compare borrowers who take out their loan within the same firm also accounting for time-varying industry-level shocks.

Our identification comes from the random variation in the composition of colleagues who take out a mortgage loan in the given period of time. In some periods mortgage loans are taken out by people whose mathematics score is high, while in other periods they are taken out by people whose mathematics score is low. In theory the change of colleagues (some employees leave the firm and there can be new hires as well) could also add to the random variation, but the share of borrowers who change company is negligible in our

sample (less than 1.5 per cent of all employees change company during this period, and this figure is lower in the case of borrowers).

One threat to our estimation is that in addition to the impact of colleagues, borrowers' own credit risk has a strong impact on the interest rate of the mortgage loan. If borrowers with high credit risk are systematically surrounded by colleagues with low mathematics scores, for example, then our estimated parameter on colleagues' mathematics score would be biased. By including the age, wage, company and own mathematical skill of the borrower in the regression, we believe that credit risk is captured and that the aforementioned omitted variable bias does not distort our estimation.

2.3.3 Placebo test

We apply a placebo test to show suggestive evidence on the lack of time-varying common shocks. As our identification is based on the change over time in the composition of peer groups, we cannot directly control for time-varying shocks at the peer group level. For instance, by including company $\times$ time fixed effects, we would not only control for common shocks, but would also remove the aforementioned variation in the composition of peers over time (that serves as the source of identification). Consequently, instead of directly controlling for shocks, we implement a placebo test that should result in significant parameters if time-varying common shocks distort our results. Non-significant results mean that we cannot reject the hypothesis of no time-varying common shocks.

In our placebo test we focus on colleagues who take out a loan *after* the borrower. If it is true that experienced peers help current borrowers, then peers who take out their loans later than the borrower should have no impact on the borrower's interest rate. However, if our results are governed by common shocks, then these shocks affect both the current borrower and her peers, who borrow later. Therefore, the estimated coefficient should be significant. If it is not significant then it suggests that common shocks do not distort our results.

We estimate the following specification, where the only difference compared to our baseline model is the change in the timing of peers' borrowing:

$$y_{i,c,t} = \beta X_{i,c,t} + \delta \overline{X_{i,c,t+1}} + \phi S_{c,t} + \Omega_c + \Theta_{g,t} + \varepsilon_{i,c,t} \quad (2.5)$$

2.3.4 Further specifications

We also run specifications where we include several other loan related variables such as the repricing period, maturity and principal of the loan. These variables are determined simultaneously with the interest rate; therefore estimating a regression where all of these are included may lead to a bad control problem. However, we view these regressions more as a robustness check: if the results remain qualitatively the same it suggests that leaving out these variables from the original regression does not distort our results. Finally, we also run regressions with alternative outcomes.

2.3.5 Bank choices

We suspect that bank choice may be a channel of the peer effect. We test if borrowers tend to choose the same bank as their peers. To do so we model the choice of borrower with a conditional logit model, where the borrower can choose from all of the banks that have at least one branch in the district where the borrower lives (see a more detailed description of the choice model in Appendix 2.C).

The dependent variable is the choice of the bank which takes the value of 1 if a borrower has chosen a given bank and 0 otherwise¹¹. The marginal effects of the choice model can be computed based on the relationship between the estimated coefficients and the probability of a borrower choosing a given bank:

¹¹All alternatives that could be chosen by the borrower appear as separate observations.

$$\frac{\partial p_{i,j}}{\partial x_{i,k}} = \begin{cases} p_{i,j}(1 - p_{i,j})\beta & j = k \\ -p_{i,j}p_{i,k}\beta & j \neq k \end{cases} \quad (2.6)$$

where $p_{i,j}$ is the probability of borrower i chooses bank j , $x_{i,k}$ is a vector that contains the variables that may affect the choice of borrower: the share of the borrower's loan taking colleagues have chosen the given bank, the share of all colleagues who have chosen a given bank, bank fixed effects for all alternatives (interact bank fixed effects with borrowers' other potential choices).

A one unit increase in $x_{i,j}$ increases the probability of choosing alternative j by $p_{i,j}(1 - p_{i,j})\beta$ and decreases the probability of choosing any of the other alternatives by $p_{i,j}p_{i,k}\beta$. Therefore, to obtain the marginal effect of an increase in the share of peers choosing a given bank in the borrowers network both own and cross-effects should be considered.

As a final step, we run a placebo test to show that no time-varying common shocks distort our results.

2.4 Results

2.4.1 Baseline results

In Table 2.2 we present 3 different specifications: first without company fixed effect in Column (1), and then with company fixed effects (Column (2) and (3)). In specification (1) the estimated peer effect is -0.63, which means if we compare two similar borrowers with the only difference between them being that one has peers whose average mathematics score is 0.54 (90th percentile) and the other has peers whose average mathematics score is 0.29 (10th percentile), then the first borrower's estimated interest rate is 16 basis points (0.158 percentage point) lower than the second borrower's interest rate. The own mathematics score coefficient is -0.945, meaning that for someone, whose mathematical

knowledge is at the maximum point the interest rate is 95 basis points lower than for someone, whose maths knowledge is at 0. We include company fixed effects in Specification (2), and the peer effect halves and decreases to -0.29. This result shows that even if we control for company specific (time-invariant) shocks there remains a significant unexplained part, which we consider as peer effect. In Column (3) we include company fixed effect and industry $\times$ time fixed effect. In this specification we control for time-invariant company shocks and time dependent industry shocks. The coefficient of the peer effect is -0.309 in Specification (3), which does not differ significantly from the result in Specification (2). The coefficient of own mathematics score is stable across all three specifications. In Appendix 2.B we show the results of a regression where we also control for regional level time-varying bank supply effects. We control for bank supply by including bank fixed effects interacted with district fixed effects and with time fixed effects. The results remain qualitatively the same.

Repeating the calculation of changing from peers with a low mathematics score to peers with a high mathematics score results in an interest rate which is 8 basis point lower according to Specification (3). This is an economically moderate impact: considering all payments until the loan is fully repaid, the total saving amounts to one and a half monthly instalments. Two things should be taken into account regarding this result. Firstly, although the total impact is low compared to the total loan amount, it is not small compared to the cost of obtaining the effect. Unfortunately, we cannot measure how much discussion is needed between the peers in order to realise this gain. However, we imagine that exchange of information about loan taking experience happens during non-working periods such as coffee breaks or for instance when peers have lunch together. Therefore, the costs of obtaining the information is low and the total saving compared to this low costs is relatively high. Secondly, the next set of results show that the impact of peers is heterogeneous with respect to own mathematical skills and bank concentration, and therefore some borrowers gain much more from the peer effect.

Table 2.2: Baseline specification

	(1)	(2)	(3)
	Interest rate	Interest rate	Interest rate
maths avg col	-0.630*** (0.0929)	-0.290*** (0.0937)	-0.309*** (0.0956)
maths own	-0.945*** (0.0475)	-0.856*** (0.0531)	-0.847*** (0.0538)
Observations	75962	73018	73016
R^2	0.094	0.176	0.184
Covariates	Yes	Yes	Yes
Time FE	Yes	Yes	No
Company FE	No	Yes	Yes
Industry x Time FE	No	No	Yes

Dependent variable: mortgage loan interest rate. Other independent variables: age, age², own wage, peers' weighted average wage, log(sales). Standard errors clustered on company level. *** p<0.01, ** p<0.05, * p<0.1

2.4.2 Heterogeneity in peer effects

The results of the specification containing heterogeneous peer effects are shown in Table 2.3. The only difference compared to the previous regression is that we add an interaction term of the average mathematics score of colleagues with own mathematical skill in Specification (1) and with bank concentration in Specification (2). The results suggest that there is a considerable heterogeneity with respect to these factors. Peers' average mathematics score results in a lower interest rate only in the case of borrowers whose own mathematical skill is low: the estimated impact is twice as large (-15 bp) in the case of borrowers with mathematical skill at the 10th percentile of the distribution of mathematical skills as compared to the results in the baseline specification (Column (3) in Table 2.2).

In the second specification we include a measure of bank concentration and we also interact this measure with peers' average mathematics score. This measure is the Herfindahl-Hirschmann index (HHI) and we normalise it to take values between 0 and 1: the higher the HHI, the stronger the bank concentration. The index is calculated taking into account the 10 large banks in Hungary and treating all of the small ones as one institution. Bank

market shares are calculated based on the observed loan disbursements in our sample and calculated at the district-year level.

$$HHI_{d,t} = \sum_{b=1}^{11} bankshare_{d,t,b}^2 \quad (2.7)$$

where

- d district, t time, b bank.
- bank share - sum of loans in a given district at a given time for a given bank.

We use this index as a proxy for competition among banks. Similarly to the previous result, the estimated impact is small in districts where HHI is at its average, and much larger where bank concentration is high. We show the level of heterogeneity with respect to own mathematical skill in Figure 2.1 and to bank concentration in Figure 2.2.

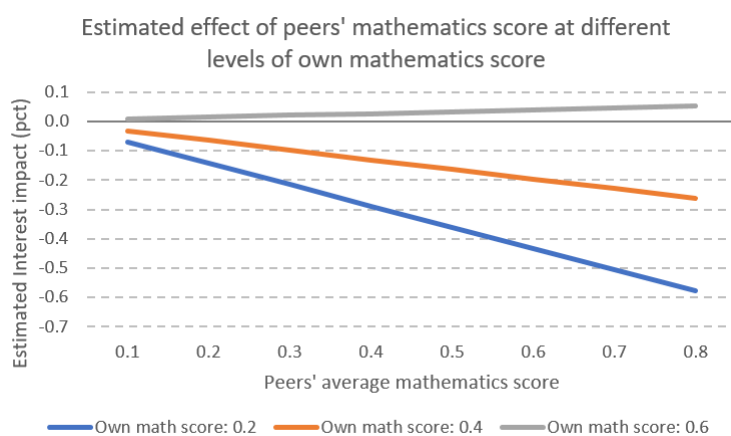
In Figure 2.1 we compare three individuals with different mathematics knowledge scores of: 0.2 (around the 10th percentile), 0.4 (around the median) and 0.6 (around the 90th percentile). The results show that for employees with high mathematics knowledge there are no peer effects, while in the case of employees with low mathematical skill the peer effect can be large. Comparing two borrowers (A and B) with low mathematical skill (10th percentile of the distribution of mathematical skill in our sample) and with the only difference between them being that borrower A has peers with low average mathematics score (10th percentile of the distribution of peers' mathematical skill in our sample) and borrower B has peers with high mathematics score (90th percentile of the distribution of peers' mathematical skill in our sample) borrower B's interest rate is expected to be 15 basis points lower than borrower A's interest rate.

Repeating this back of the envelope calculation with borrowers who live in districts where bank concentration is high (90th percentile of the HHI distribution) and the only difference between borrowers again being the average mathematics score of the peers then the borrower whose peers have the high average mathematics score is expected to have an

interest rate that is 24 basis points lower than the other borrower.

Considering a typical loan with 20-year maturity (the maturity that is taken out most often) and an interest rate of 5.1 per cent (the mean interest rate in our sample) and comparing a borrower whose peers have a high mathematics score and one whose peers have a low mathematics score, the estimated effect of having peers with a higher mathematics score calculated by the baseline regression (Table 2.2 Specification (3)) is 8 basis points which is consistent with a saving amounting to one and a half monthly instalment¹². The similar impact based on the regression that takes into account the heterogeneity with respect to the own mathematics score of the borrower (Table 2.3 Specification (1)) is 15 basis points¹³. Finally, the estimated impact based on the regression that takes into account the heterogeneity with respect to bank concentration (Table 2.3 Specification (2)) is 24 basis points, which is consistent with a saving amounting to four to five monthly instalments.

Figure 2.1: Estimated impact of the change in the average mathematics score of loan-taking peers



The three lines are selected based on the distribution of math scores: 0.2 is around the 10th percentile, 0.4 is around the median and 0.6 is around the 90th percentile. The calculation is based on Table 2.3 Column (1).

2.4.3 Placebo test

The last specification controls for various borrower (demographic variables), company (company fixed effect and sales) and time-varying industry (industry $\times$ time fixed effect)

¹²The calculated impact is the result of discounting the difference between the two loans' monthly instalments over the 20-year period until maturity, using a discount factor rate of 3 per cent.

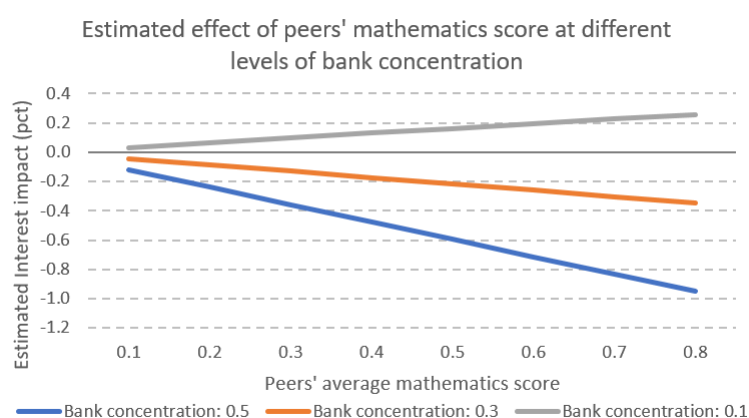
¹³That is consistent with savings amounting to three monthly instalments.

Table 2.3: Heterogeneity based on mathematics and competition

	(1) Mathematics	(2) Competition
maths avg col	-1.113*** (0.212)	0.745*** (0.249)
maths avg col x maths own	1.966*** (0.461)	
maths own	-1.668*** (0.201)	-0.836*** (0.0522)
maths avg col x bank concentration		-3.858*** (0.865)
bank concentration		2.864*** (0.379)
Observations	73016	66991
R^2	0.184	0.202
Covariates	Yes	Yes
Time FE	No	No
Company FE	Yes	Yes
Industry x Time FE	Yes	Yes

Dependent variable: mortgage loan interest rate. Main independent variables: maths avg col: weighted average math score of colleagues, maths own: own maths score, bank concentration: measured as a Herfindahl-Hirschman-index. Other independent variables: age, age², own wage, peers' weighted average wage, log(sales). Standard errors clustered at company level. *** p<0.01, ** p<0.05, * p<0.1

Figure 2.2: Estimated impact of the change in the average mathematics score of borrower's peers



The three lines are selected based on the distribution of bank concentration scores: 0.1 is around the 10th percentile, 0.3 is around the median and 0.5 is around the 90th percentile. The calculation is based on Table 2.3 Column (2).

related shocks. Including this large set of controls considerably decreases potential endogeneity problems, leaving time-varying peer group level common shocks as the main

source of potential biases. As there is some lag between the peers' and the borrower's decision even time-varying common shocks threaten our results only if they are autocorrelated. As we cannot rule out the possibility of autocorrelated common shocks we carry out a placebo test to show suggestive evidence that such shocks do not bias our results.

To conduct this exercise we replace the peers' average mathematics score with the average of the peers who took out a loan after the borrower. Nevertheless, there is one more challenge in this setup. Namely, if there is any peer effect, then the borrower herself may have an impact on colleagues who take out a loan in the following period. In this case, the placebo test may result in significant coefficient not because of the presence of common shocks, but simply due to the peer effect that we are after. To overcome this issue, we restrict our analysis to low skilled borrowers. The reason we use low-skilled borrowers is that, according to our result in Section 2.4.2, low-skilled borrowers have a limited impact on their peers because peer impact increases in conjunction with mathematical skill. Therefore, a low mathematics score points to a low peer effect (as in Table 2.3).

The results of the placebo test are shown in Table 2.4. Here, we restrict our sample to those whose mathematics score is below the median. According to our results, those who have low mathematical skill have a low impact on the peer's interest rate. In this case, the parameter of the peer effect is -0.458 in the original setup.¹⁴ It is significant at all conventional levels. In the placebo case the parameter is -0.154 and it is not significant. These estimations suggests that common shocks do not distort our results¹⁵.

¹⁴This result is somewhat different from the one in Table 2.2 Specification (3) due to the fact the sample is different (narrower). We include only those observations that both have peers in the before and after period and we also restrict for low skilled borrowers.

¹⁵This placebo design addresses the threat of time-varying common shocks and is consistent with an experience-based interpretation of the peer effect. Since the placebo operates on the time dimension of peer group composition, its insignificance provides a general validity check for the entire identification strategy - ruling out time-varying common shocks across all specifications. It does not substitute for - nor invalidate - the heterogeneity results on mathematical skill, which are identified through cross-sectional variation in peers' skill levels

Table 2.4: Placebo test

	(1) Before	(2) After
maths avg	-0.458*** (0.147)	
maths avg after		-0.154 (0.146)
maths	-0.349** (0.155)	-0.362** (0.155)
Observations	33462	33462
R^2	0.168	0.168
Covariates	Yes	Yes
Time FE	No	No
Company FE	Yes	Yes
Industry x Time FE	Yes	Yes

Only those borrowers' interest rate is included in the estimation whose own math score is below the median.

Dependent variable: mortgage loan interest rate. Main independent variables: maths avg col: weighted average math score of colleagues, maths own: own maths score, maths avg col after: weighted average math score of colleagues who take a mortgage after. Other independent variables: age, age², own wage, peers' weighted average wage, log(sales). Standard errors clustered at company level. *** p<0.01, ** p<0.05, * p<0.1

2.4.4 Alternative outcomes

As we discussed earlier, a mortgage loan is a complex financial product and it is not only the interest rate that defines its features. We test alternative outcomes (Table 2.5) to determine whether there are peer effects in respect of such features. First we examine the repricing period.¹⁶, i.e. the period for which the interest rate of a loan is fixed. For instance, if the repricing period is 10 years, this means that the interest rate is fixed for the first 10 years and is then subsequently recalculated based on the current market interest rate. After the new interest rate is calculated it remains fixed for the next 10-year period. We see a positive effect for the repricing period, which means that ceteris paribus if the mathematics score of a borrower's peers is higher then the repricing period of the borrower is shorter. An increase in peers' mathematics score from the 10th to the 90th percentile

¹⁶The repricing period is calculated as fraction of the time the interest rate is fixed to the total maturity of the loan. For instance, it is 1 if the interest rate is fixed for the whole period. It is 0.5 if the interest rate is fixed for the first half of the loan.

implies a decrease in the repricing period by around 1.5%, which is a rather small impact.

Maturity is the period of time during which the consumer should repay the loan. We cannot measure any peer effects in respect of this feature.

The principal is the amount of money, which the consumer borrowed from the bank. According to our results changing the mathematics score of peers from the 10th to the 90th percentile implies an increase in principal of roughly 12% (in the case of low-skilled borrowers). These results may indicate some increase in risk-taking behaviour if peers' mathematics score increases.

If we include the latter three dependent variables into the original estimation as controls we obtain similar results. This result suggests that omitting these variables from the original regression does not distort our results, and we do not face the problem that we leave out key variables that have a strong impact on the pricing of loans. We refer to these estimation as being exposed to the so-called 'bad control' problem, because these control variables may be treated as outcome variables as well (all of these variables together with the interest rate are determined simultaneously). The inclusion of these variables does not affect the main result. If we add the bank dummies the parameter estimates decreases by one third. This suggests that one part of the advice is probably about which bank to choose. The estimated parameters are very similar to what we obtained in the original estimation in Subsection 2.4.2.

Table 2.5: Alternative outcomes

	(1) Repricing Repricing period	(2) Maturity Maturity	(3) Principal Ln(Principal)	(4) Bad controls 1 Interest rate	(5) Bad controls 2 Interest rate
maths avg col	-0.101** (0.0510)	-1.110 (0.981)	0.668*** (0.125)	-0.605*** (0.191)	-0.395** (0.178)
maths avg col x maths own	0.194* (0.113)	1.811 (2.260)	-1.006*** (0.299)	1.134*** (0.414)	0.797** (0.379)
repricing period				1.446*** (0.0243)	1.447*** (0.0255)
maturity				0.0461*** (0.00113)	0.0381*** (0.00110)
ln(principal)				-0.466*** (0.00875)	-0.291*** (0.00860)
maths own	-0.0517 (0.0487)	-2.669*** (0.969)	0.954*** (0.121)	-1.025*** (0.181)	-0.690*** (0.165)
Observations	73016	73016	73016	73016	73016
R^2	0.124	0.156	0.285	0.330	0.426
Covariates	Yes	Yes	Yes	Yes	Yes
Time FE	No	No	No	No	No
Company FE	Yes	Yes	Yes	Yes	Yes
Ind. x Time FE	Yes	Yes	Yes	Yes	Yes
Bank FE	No	No	No	No	Yes

Dependent variables: (1) repricing period, (2) maturity, (3) ln(principal), (4)-(5) mortgage interest rate. Independent variables: maths avg col: weighted average math score of colleagues, maths own: own maths score. Other independent variables: age, age², own wage, peers' weighted average wage, log(sales). Standard errors clustered at company level. *** p<0.01, ** p<0.05, * p<0.1

2.4.5 Similar bank choices

In this section we present additional suggestive evidence that one channel of advice is the choice of bank. We test if borrowers tend to choose the same bank as peers have chosen. We model the borrowers' choice of bank in a random utility model framework estimating a conditional logit model.

The three lines are selected based on the distribution of math scores: 0.2 is around the 10th percentile, 0.4 is around the median and 0.6 is around the 90th percentile. The calculation is based on Table 2.3 Column (1).

Our key independent variable is the weighted number of peers who have chosen the given

bank in the borrower's network¹⁷. Our previous results showed that peers with mathematics scores have an impact on borrowers, therefore, when we calculate the share of peers choosing a given bank we include only those peers whose mathematics score is higher than the median mathematics score. We also include the share of the bank chosen in the borrower's company (again, including only peers with above median mathematics scores) over the entire time frame (time-invariant variable). This variable captures shocks related to the company for instance if a bank has a branch close to the company and therefore most employees use that bank. Finally, we add bank fixed effects for all alternatives¹⁸ which pins down how often a specific bank is chosen in general during the observed period¹⁹.

The main concern with this approach could be that, although we control for company shares and bank fixed effects, time-varying local shocks may distort our results. To overcome this problem we once again run a placebo test where we replace the peers who have already taken out a loan to with the peers who take out a loan later than the borrower (from 3 to 12 months later), including only peers with higher-than-median mathematics scores. The peers who take out their loan later are supposed to have no impact on the borrowers if the mechanism is indeed informal advice. To put it more precisely, if there are time-varying common shocks, then we expect the placebo test to be significant. The insignificance of the placebo test would suggest that there are no time-varying common shocks distorting our results.

As summarised in Table 2.6, the results show a significant impact of peers on the borrower's choice as the variable *Weighted avg share of bank in network* is strongly significant. The economic relevance of this impact can be shown by an example: comparing the chance that a borrower chooses bank A if 0 per cent of his peers has chosen bank A versus 100 per cent having chosen the bank. In the case of that bank A has a 10 per cent share

¹⁷We restrict the choice set of borrowers to those banks that are active in the district of the borrower in the given year. We treat a bank as active if the share of loans granted is at least 5 per cent of all the loans granted in the district.

¹⁸We interact bank fixed effects with fixed effects of alternatives

¹⁹Bank fixed effects capture all bank related, time-invariant characteristics that are not observed.

Table 2.6: Conditional logit model on bank choice

	(1) Baseline	(2) Before	(3) After
Weighted avg share of bank in network	0.129*** (0.0363)	0.158*** (0.0369)	
Weighted avg share of bank in network, after			-0.00685 (0.0383)
Share in company	5.317*** (0.213)	4.871*** (0.205)	5.020*** (0.211)
Observations	469804	212409	212409

Column (1) contains the full sample. Column (2)-(3) contains those observations, where the own math score is below the median. One observation is one potential choice of the borrower. Dependent variable: choice of bank – indicator variable. Main independent variables: Weighted avg share of bank in network: weighted number of peers who have chosen the given bank divided by weighted number of all peers. Bank chosen in network (weighted) after: weighted number of peers who have chosen the given bank during 3 to 12 month after the borrower has taken her loan divided by weighted number of all peers in the given period. Share in company: share of colleagues who have chosen the same bank (time-invariant). Share in total sample (not shown): share of borrowers who have chosen the same bank (time-invariant). Standard errors clustered at company level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

in the sample there is 2.3 per cent higher chance that the borrower chooses this bank²⁰. That is equivalent to a 23 per cent impact compared to the original chance of choosing the bank. Although this figure varies with the share of the banks ($p_{i,j}$) the impact of this variation is limited regarding our analysis: taking into account the shares of the banks in our sample the weighted average impact of the change outlined above in the share of peers choosing a certain bank (and not choosing others) is 20 per cent.

The results suggest that part of the informal advice is the choice of bank. Our key variable is significant even after controlling for the share of borrowers choosing a certain bank at the same company. The placebo test, where only those are included in the sample whose mathematics score is below the median and are therefore unlikely to give advice to borrowers (similar as in the main specification) shows no significant results, suggesting that no time-varying common shocks are distorting the results.

²⁰Estimated impact can be calculated the following way. The own effect of choosing the bank will increase the chance that borrower i chooses bank A by $p_{i,j}(1 - p_{i,j})0.129 = 0.1 \cdot 0.9 \cdot 0.129 = 0.0116$. The cross-effects also increase the chance of choosing bank A by $\sum_k (p_{i,j}p_{i,k}0.129) = \sum_k (0.1p_{i,k}0.129) = 0.0116$, as the share of choosing other banks decreases in the network of the borrower. The total impact is the sum of own and all cross-effects.

2.4.6 Certified Consumer Friendly Housing Loan

The reason for the presence of peer effect is likely to be an information problem of the borrower. There are at least three potential problems that makes the decision of the borrower difficult. First, mortgage loan is a complex product as when a borrower decides about a mortgage loan, she should decide about several things at the same time. These are for instance the loan amount, maturity, acceptable interest rate, repricing period, which bank to choose etc. Second, it is difficult to collect information about all the loans that are available to a borrower. It is not enough to go to all the banks but should collect all offers of the bank as banks have potentially multiple offers. Bank offer comparison websites may help borrowers, however those not necessarily contain all offers, and all details of the offers that are relevant. Finally, even having all the information on the offer level it is difficult to digest all that information and make a good comparison of the offers. Moreover, there is an additional layer of these problems as borrowers may not trust in the information provided by banks.

Based on our results so far we argue that a colleague may help in all of these problems. A skilled peer can share own experience and explain the important aspects of the loan based on own experience. Then need for more information and for comparison of offers may substantially decrease. Moreover, borrowers can also trust more their peers.

What would be then the policy implication of our results? The simplest implication would be to place peers who has taken a loan recently and has high mathematical skill to all borrowers. This is obviously not a feasible solution. The closest feasible solution to this would be to run a free advisory that supports borrowers in this complex problem. One drawback of that solution is that running such an advisory on the country level may be expensive.

Controlling the offers of banks by regulation can also support borrowers. Nevertheless, strong control may also decrease the options that borrowers have and may have negative effects as well. Forcing banks to offer standardised loans (among other loans as well) that

are easy to compare and borrowers can trust them would solve most of the information problems of borrowers. Certified Consumer Friendly Housing Loan that was introduced in Hungary at the end of 2017 was such a product.

At the end of 2017, The Central Bank of Hungary (CBH) introduced the Certified Consumer Friendly Housing Loan (CCFL) to support borrowers in finding a suitable product on the mortgage loan market. Working together, Hungarian banks and the CBH elaborated the details of these products²¹ to ensure that they were standardised and favourable for borrowers. The margin above financing costs was capped at 3.5 percentage point. The disbursement fee was limited at 0.75 percentage point of the current principal while the prepayment fee was also capped at 1 percentage point of the prepaid amount. Repricing periods could be either 5/10/15 years or last to maturity (i.e. fully fixed interest rate, no repricing). Credit assessment could not exceed 15 working days while loan disbursement (if all requirements were met) was maximised at 2 working days. Monthly payments of CCFLs were determined according to annuities. CCFLs were developed by commercial banks and the CBH together. To obtain the certification that enables banks to use the logo of CCFL the offered product had to meet all of the criteria set by the CBH. Introducing CCFL decreased the complexity of choosing a suitable loan product as these loans were better in most characteristics compared other available loans on the market and it was also advertised by the CBH²².

Banks were urged to participate in the programme by the CBH, but were not legally obliged to do so. The terms on these loans were different from bank-to-bank, but the limits shown above lead to similar products regardless of which bank offered them. In line with the capacities of banks' product development departments the introduction of the product differed at the bank level: some banks were able to introduce it early, while some banks had less capacity and were only able to roll out a CCFL product later. In theory, banks could also start to offer CCFLs in a strategic way: by offering it only if

²¹<https://www.mnb.hu/en/supervision/news/certified-consumer-friendly-housing-loans-to-reduce-interest-rates>

²²Financial Stability Report (November 2017) , box 2 and Financial stability Report (May 2018) chapter 8.

the competition they face made it necessary to do so and by continuing to offer more expensive products when low a level of competition allowed that. We consider these aspects in our empirical strategy concerning CCFL.

To understand what impact CCFL had on borrowing decisions, we focus on the first 2 years of the newly introduced product. We argue that the spread of this product is plausibly exogenous after controlling for bank competition, company fixed effects and the interaction of time and industry fixed effects. First, we measure if informal advice incentivised borrowers to choose CCFL. We also include an interaction term of the share of CCFLs and the average mathematical skill of colleagues. This variable helps to understand whether informal advice changes when CCFL becomes widely spread. We expect that high mathematical skill of colleagues is related to higher chance of exploiting the good opportunity of CCFLs, but, when the loan is widely spread this effect may change.

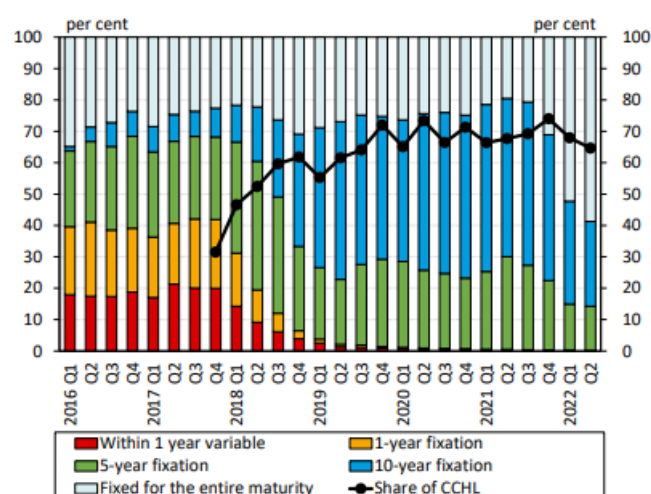
In a separate regression we have the interest rate as independent variable and by including the interaction of share of CCFL in a district and the average mathematical skill of colleagues we test how the spread of CCFL impacted informal advice with respect to the interest rate on the loan. Finally, although we think it is difficult to find any plausible endogeneity story, as a robustness check we carry out an IV strategy that is based on the random variation of the spread of CCFLs by banks in the first few months after official introduction of the product. The validity of the instrument relies on the assumption that how quickly banks were able to develop the new product was related solely to the capacity of their product development departments and is orthogonal to the problem of informal advice - especially after controlling for company and industry $\times$ time fixed effects and bank competition.

2.4.7 Estimating the impact of the introduction of Certified Consumer Friendly Housing Loans on peer effects

Implementation of new products needs time. Some banks with efficient product development could introduce CCFL quickly, while other banks were slower with the introduction. At the end of 2017 around 10-15 per cent of all mortgage loans were CCFL and almost one third of fixed repricing period loans. From 2020 two out of three newly disbursed mortgage loans were CCFL (see Figure 2.3), and this share stabilised.

To understand how the introduction of CCFL affected peer effect we use the spread of CCFL that is the period from the second half of 2017 to end 2019 for analysis. If the introduction of CCFL helps to decrease the information problem, then impact of peer effect should decrease after the introduction. Nevertheless, it is also possible that the peer effect remains relevant but narrows down to suggesting choosing a CCFL product. That would be a sign that product standardisation cannot solve the information problem, as remaining presence of peer effect would indicate remaining presence of the information problem.

Figure 2.3: Distribution of new housing loan volume by interest rate fixation, and the share of Certified Consumer Friendly Housing Loan products



Note: Share of CCHL products compared to new issues with at least 3 years of interest rate fixation (at least 5 years since Q4 2018) excluding disbursements by building societies and without FGS GHP. Source: MNB

Source: Trends in Lending 2022 September
<https://www.mnb.hu/letoltes/hitelezesi-folyamatok-2022-szeptember-en.pdf>

First we estimate the effect of peers in choosing CCFL product after the introduction of it. We run a similar regression as in Table 2.3, however we model whether the borrower has chosen CCFL by changing the dependent variable to an indicator variable that takes the value of one if a borrower has chosen CCFL product. Moreover, we analyse the heterogeneity of the peer effect with respect to the spread of CCFL. We do so by interacting the average mathematics score of peers by CCFL share among newly disbursed loans in the district in the given month. We also include the share of CCFL as a control variable, it varies between zero and one. We argue that the spread of this product was partially random, as it depended on the product development of the bank - how quickly it could introduce a product that meets the CCFL criteria. We keep including company fixed effect and industry $\times$ time fixed effect in the regressions. Moreover, in this regression we also include the bank concentration measure (HHI) to control for the change in bank competition in different districts.

The results of the estimation are in Table 2.7 Specification (1). Average mathematics score of peers is large and significant suggesting that peers advise choosing CCFL. However, the interaction term of peers' mathematics score and CCFL share in the district in the given period is negative and (depending on the share of CCFL) it may fully offset the peer effect. This result supports that the introduction and spread of CCFL decreases peer effect. It is suggestive evidence that it also decreases the information problem.

We run a similar regression, however now we use interest rate as dependent variable. Explanatory variables are the same as previously. Results that are in the second column of Table 2.7 are also similar. peer effect decreases the interest rate of borrowers, however, if CCFL share is large, then peer effect vanishes. Again, we conclude that this result is suggestive evidence that the introduction of a standardised product can decrease the information problem.

We think that the spread of CCFL is driven by such forces that are unrelated to peer effect, i.e. our results are not biased. Although, endogeneity concerns regarding the spread of such a product could arise, we include a wide set of control variables (company

Table 2.7: Spread of CCFL

	(1) Choice	(2) Interest
maths avg col	0.475*** (0.0848)	-0.998*** (0.245)
maths avg col $\times$ CCFL share	-1.044*** (0.183)	1.217** (0.513)
CCFL share	0.963*** (0.0767)	-0.960*** (0.214)
bank concentration	0.0139 (0.0399)	1.615*** (0.126)
maths own	0.144*** (0.0239)	-0.909*** (0.0639)
Observations	30022	30022
R^2	0.477	0.313
Covariates	Yes	Yes
Company FE	Yes	Yes
Industry x Time FE	Yes	Yes

In column (1) the dependent variable is a dummy variable indicating whether the borrower choose the CCFL or not. In column (2) the dependent variable is the mortgage interest rate. Main independent variables: maths avg col: weighted average math score of colleagues, CCFL share: the share of CCFL in a given district at a given month, bank concentration: measured as a Herfindahl-Hirschman-index, maths own: own maths score. Other independent variables: age, age², own wage, peers' weighted average wage, log(sales). Standard errors clustered at company level. *** p<0.01, ** p<0.05, * p<0.1

fixed effect, industry $\times$ time fixed effect, demographic controls, bank concentration) that captures most of the shocks that we could imagine would bias our estimate. Although we think that it is difficult to find a plausible endogeneity story, in theory any company or district level time-varying common shock can distort the results. As a robustness check we implement an IV strategy.

In our IV estimations the endogenous variable is the spread of CCFL. We use the spread of CCFL in the sixth month of introduction as an instrument²³. To also account for the fact that CCFL share is increasing through time we interact CCFL share in the sixth

²³The rationale for using the sixth month is that we see that in the first few months the product was not present in most districts, therefore our IV would not be strong enough. We also do not want to use much later periods as that is not consistent with the exogenous shock that roots in the variation of time needed for banks to implement the new product. Nevertheless, using seventh or eighth month instead of the sixth month does not change our results significantly.

months after introduction with a time trend²⁴. The exogenous variation comes from the noise around the introduction of CCFL. As we mentioned earlier, depending on banks how quickly could develop the new product they granted either in large or in small scale after the first half year of the introduction of CCFL. Whether those banks are present in a district that were successful in implementing the CCFL or not is not related to the information problem that we analyse. Moreover, using our large set of controls in the first stage we cannot find any plausible endogeneity issue that would question the validity of the instrument.

The results of the IV estimation are similar to the OLS results (see in Table 2.8). Although point estimates of both peer effects and the interacted terms are somewhat larger in absolute terms in the IV estimation, they are not significantly different from the OLS results and they also tend to offset each other if CCFL share is large. This robustness check strengthens the results of the OLS estimates.

²⁴It is important to do so as company fixed effects would fully absorb CCFL share if that is not changing through time.

Table 2.8: Spread of CCFL - IV estimation

	(1) Choice	(2) Interest
maths avg col	0.650*** (0.153)	-1.376*** (0.385)
mathematics avg col x CCFL share	-1.485*** (0.386)	2.136** (0.972)
CCFL share	0.787*** (0.201)	-1.370*** (0.523)
bank concentration	-0.138** (0.0612)	1.621*** (0.168)
maths own	0.149*** (0.0237)	-0.906*** (0.0645)
Observations	30022	30022
R^2	0.039	0.064
Covariates	Yes	Yes
Company FE	Yes	Yes
Industry x Time FE	Yes	Yes

In column (1) the dependent variable is a dummy variable indicating whether the borrower choose the CCFL or not. In column (2) the dependent variable is the mortgage interest rate. Main independent variables: maths avg col: weighted average math score of colleagues, CCFL share: the share of CCFL in a given district at a given month, bank concentration: measured as a Herfindahl-Hirschman-index maths own, own maths score. Other independent variables: age, age², own wage, peers' weighted average wage, log(sales). Standard errors clustered at company level. *** p<0.01, ** p<0.05, * p<0.1

2.5 Conclusion

In our paper we used the whole universe of mortgage loans granted between 2015-2019 in Hungary for identifying peer effect of co-workers. As taking a loan is a complex task borrowers often make mistakes. We analyzed two salient strategies to cope with the difficulty of taking a mortgage loan: informal advice and regulation. In our special setup we could also account for the interaction of these two factors.

First, we tested whether informal advice play an important role in the borrowing decision. We find that if a mortgage loan-taker has low mathematical skill then if he has peers, who have high mathematical skill and experience in the mortgage loan market, the interest rate of the borrower tends to be lower compared to similar borrowers with no smart and

experienced peers. We showed that there is heterogeneity in peer effects depending on the strength of competition among banks. If the level of competition is lower, then the peer effect is considerably higher. According to our results, considerable part of the peer effect happens through the choice of the bank.

Second, we find that the introduction of a standardised loan can ease the information problem of the borrower. After the introduction of Certified Consumer Friendly Housing Loans the relevance of informal advice decreased. We show evidence that in the first part of the program peers helped borrowers to find the CCFL products, while later when CCFL was widespread peers become less important in the borrowing decision.

The policy implication of our results connects to the social capital literature (see e.g. Chetty et al. (2022)). We see that banks offer different rates not just based on the risk of individuals (which we measure by several control variables) but also on skills and experience. This can happen due to information problems, namely some people are unable to make comparison between different offers or even not aware of the existence of other offers. There are several ways how to solve this information asymmetry. First, policy makers can introduce general education programmes to increase the average financial literacy of the society. This can be a long and expensive process. Based on our results peers can help to find better bank offers. We argue that peers can considerably decrease the complexity of the problem and serve also as a trustful source of information. Consistent with this narrative, we also found that introducing a standardised product can decrease peer effect. The standardised product takes the place of peers in the information problem. It improves the utility of those borrowers who do not have peers that could help them. The introduction of standardised products can be fast and cheap.

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2.A Alternative specification: main results using linear weights

In this section we run the same regressions as in Section 2.4, except for that we change Equation 2.2 to linear weights as follows:

$$\omega_{i,j} = (d_{i,j} + 1)^{-1} \quad (2.8)$$

This modification does not change the results much (see Table 2.9 and 2.10).

Table 2.9: Baseline results with linear weights

	(1)	(2)	(3)
	Interest rate	Interest rate	Interest rate
maths avg	-0.631*** (0.100)	-0.178* (0.0988)	-0.195* (0.101)
maths	-0.969*** (0.0473)	-0.873*** (0.0542)	-0.865*** (0.0550)
Observations	75962	73018	73016
R^2	0.094	0.176	0.184
Covariates	Yes	Yes	Yes
Time FE	Yes	Yes	No
Company FE	No	Yes	Yes
Industry x Time FE	No	No	Yes

Dependent variable: mortgage loan interest rate. Other independent variables: age, age², own wage, peers' weighted average wage, log(sales). Standard errors clustered at company level. *** p<0.01, ** p<0.05, * p<0.1

Table 2.10: Heterogeneity with linear weights

	(1) Mathematics	(2) Competition
maths avg	-1.034*** (0.228)	0.812*** (0.274)
maths avg x maths	2.061*** (0.499)	
maths	-1.724*** (0.216)	-0.856*** (0.0556)
maths avg x bank concentration		-3.848*** (0.940)
bank concentration		2.895*** (0.409)
Observations	73016	67309
R^2	0.184	0.226
Covariates	Yes	Yes
Time FE	No	No
Company FE	Yes	Yes
Industry x Time FE	Yes	Yes

Dependent variable: mortgage loan interest rate. Main independent variables: maths avg col: weighted average math score of colleagues, maths own: own maths score, bank concentration: measured as a Herfindahl-Hirschman-index. Other independent variables: age, age², own wage, peers' weighted average wage, log(sales). Standard errors clustered at company level. *** p<0.01, ** p<0.05, * p<0.1

2.B Alternative specification: controlling on regional level bank supply with additional fixed effects

In this section we run the same regressions as in Section 2.4, except for that we add district level bank fixed effects that are interacted with time fixed effects. By doing so, we control on district level bank supply changes from month-to-month. Although, this specification rules out several endogeneity problems it may also be prone to overcontrolling as any advice on the choice of bank may be part of the mechanism that we are after. Any impact through this channel is likely to be absorbed by including bank fixed effects. Therefore, true impact may be larger than the estimations below. We do not estimate again the specification with bank concentration as the additional fixed effect included in

these regressions would absorb the variation in bank concentration.

Table 2.11: Main specifications controlling on regional level bank supply with fixed effects

	(1)	(2)	(3)	(4)
	Basic_spec	Before	After	Interaction
maths avg	-0.221** (0.0918)	-0.395** (0.175)		-0.800*** (0.206)
maths avg x maths				1.418*** (0.442)
maths avg after			0.0201 (0.176)	
Observations	66023	27162	27162	66023
R^2	0.487	0.517	0.516	0.487
Covariates	Yes	Yes	Yes	Yes
Time FE	No	No	No	No
Company FE	Yes	Yes	Yes	Yes
Industry x Time FE	Yes	Yes	Yes	Yes
Bank x District x Time FE	Yes	Yes	Yes	Yes

Dependent variable: mortgage loan interest rate. Other independent variables: age, age², own wage, peers' weighted average wage, log(sales). Standard errors clustered at company level. *** p<0.01, ** p<0.05, * p<0.1

2.C Description of the choice model

Following Cameron and Trivedi (2010), we show the main assumptions and the model that we estimate below. The cornerstone of the model is the assumption that borrowers choose the alternative that gives them the highest utility.

$$U_{i,j} > U_{i,k}, \forall j \neq k \quad (2.9)$$

where $U_{i,j}$ stands for the utility of borrower i choosing alternative j .

Assuming additive random utility, the utility of borrower i can be written in the form:

$$U_{i,j} = V_{i,j}(x_{i,j}) + \varepsilon_{i,j} \quad (2.10)$$

where $V_{i,j}()$ is a function of observable characteristics of alternatives and $\varepsilon_{i,j}$ is an idiosyncratic taste shock that is not observable by the researcher and is assumed to follow an iid extreme value distribution.

The probability of choosing bank j related to the observable characteristics can then be expressed as below.

$$p_{i,j} = \frac{e^{V_{i,j}}}{\sum_k e^{V_{i,k}}} = \frac{e^{x_{i,j}'\beta}}{\sum_k e^{x_{i,k}'\beta}} \quad (2.11)$$

where $x_{i,j}$ contains the following explanatory variables: our key independent variable is what share of the borrower's loan taking colleagues have chosen the given bank. We keep the weighting scheme from the original regressions. We also include what share of all colleagues (no weighting) has chosen a given bank - a variable that is similar to a company fixed effect. Lastly, we include bank fixed effects for all alternatives (interact bank fixed effects with borrowers' options (i.e. alternatives)).

Chapter 3

Liquidity Transfer and the Real Economy during a Crisis: Evidence from a Loan Payment Moratorium

This chapter is co-authored with Gergely Lakos and Balázs Spéder (Central Bank of Hungary). It has been prepared as a chapter of this doctoral dissertation.

Abstract

Loan payment moratorium was a key measure to mitigate the economic consequences of the COVID-19 shock in most countries. As this measure has been rarely used in the past little is known about the effects of such a program. We analyze the impact of the loan payment moratorium on consumption in Hungary, where the program was exceptionally widely available. We use granular data of individual level spending in a regression discontinuity design and identify the true impact of the program on spending: the additional spending when the moratorium was introduced accounts for 90% of the liquidity transfer. Due to selection bias, the effect on the aggregate level is potentially underestimated: we find that up to 20% of the liquidity transfer was consumed by borrowers. Moreover, we measure that the program helped to avoid 1.7 percentage point increase in the loan delinquency rate. We do not find any impact of the program on early repayment of loans or on new loan tak-

ing behavior. Therefore, we argue that loan payment moratorium is not only a tool that mitigates creditworthiness problems but can also help in boosting the economy during crisis times.

Keywords: Covid-19, consumption, moratorium, regional disparities

JEL: D12, E21, E62, R12

3.1 Introduction

Many countries have implemented payment deferral programmes also known as payment moratoria during the COVID-19 pandemic in order to support economic activity¹. The scope and the extent of this policy tool was a novelty in the history of economic crisis management. The reason is partly that the COVID-19 pandemic featured important temporary liquidity shocks mainly without previous excessive credit booms. Additionally, the last global crisis of 2007-2009 has highlighted the role of distress in household finances as a source of severe externalities on the rest of the economy (Mian and Sufi (2014), Verner and Gyöngyösi (2020)). Therefore, payment moratoria were also meant to prevent liquidity problems from turning into solvency problems.

Payment moratorium in different countries came in different forms regarding mandatory bank participation, deferral of principal and interest, targeted loan type, targeted borrowers, and duration, which reflects the lack of precise knowledge about its optimal specification. Our paper aims to contribute to a better understanding of how the extra liquidity provided to households by the payment moratorium helps prevent depressing the real economy. We present new evidence on consumption effect and results on the impact of

¹The IMF Policy Tracker summarizes the key economic responses governments in 197 economies have done until July 2021 to limit the human and economic impact of the COVID-19 pandemic: <https://www.imf.org/en/Topics/imf-and-covid19/Policy-Responses-to-COVID-19>. A more detailed taxonomy of economic policy measures for European countries is provided by the ESRB: <https://www.esrb.europa.eu/home/search/coronavirus/countries/html/index.en.html>

the program on delinquency rate, early repayment of loans and new loan taking.

The experience in Hungary is remarkably suitable to study real effects of the payment moratorium, since the program was broad-based, unconditional and protracted in international comparison.² Early on the pandemic, the Hungarian Government decided to suspend any loan repayment obligations until the end of 2020 of all types of loans signed before 18 March 2020 of all debtors in the household sector (and for a large portion of loans in the non-financial corporate sector as well). The program has been eventually extended several times. Even after taking advantage of the "opt-out" option provided to borrowers, 64% of the outstanding household loans remained in moratorium until May 2020, amounting to 9% of banks' balance sheet and 8% of the GDP.³ The moratorium resulted in extra household liquidity equivalent to 2.8% of the consumption expenditure in the previous year of 2019.

We use individual-level and regional-level heterogeneity to identify the consumption effect of extra household liquidity from payment moratorium. Individual-level heterogeneity is explored with monthly individual level bank account data (both balances and transactions) between January 2020 and November 2024 from one anonymous large bank in Hungary. We also use individual level data from credit registry to identify the impact of the program on delinquency rates, early repayments and new loan taking. As a proxy for the monthly aggregate consumption at the regional level, we use transactional-level data of the turnover of Hungarian online cash registers (OCR) aggregated to the 197 districts (subregions) at monthly frequency from January 2019 to December 2020. OCR turnover follows retail sales very closely, since OCRs recording both electronic and cash payments are mandatory to use in several sectors. For the monthly extra household liquidity at district level, we first calculate monthly instalments of each borrower from the credit register of the Hungarian central bank. We also use district level data from the Hungarian statistical office to control for regional differences and to keep track of spillovers to firms.

²For interim reviews of payment moratoria in Europe see EBA (2020) and ESRB (2021).

³These ratios have been only moderately decreased during the year because of the free exit from and entry to the program.

Concentrating only on mortgages with individual-level data we are able to identify causal effects. We exploit that the program was announced unexpectedly and compare loan contracts signed just before the eligibility date (i.e. date of the announcement), to loan contracts signed just after it in a regression discontinuity design (RDD). This methodology effectively addresses the core identification issue: selection bias. We find that being eligible for the program results in an additional monthly spending during all months of the program of 100 thousand HUF (250 EUR). This is equivalent to saying that around 90% of the extra liquidity is spent. Moreover, we measure also with RDD that debt payment moratorium decreased non-performing rate by 1.7 percentage point among eligible debtors. We have weak evidence on that the program had longer run effects on loan performance as well: moratorium increased non-performance among the group of eligible borrowers by 1.5 percentage points during the one and a half year after the program was released.

We find smaller estimated impact of the payment moratorium on consumption on the macro level. We employ a panel regression with fixed effects in the 4 quarters of 2020 to control for time invariant unobserved heterogeneity on the district level. 20% of the extra household liquidity from payment moratorium is estimated to be consumed. This means that without the program, consumption would have dropped around 20% more during the first three quarters of the pandemic in Hungary. In contrast, a simple cross-sectional estimate for the month of April 2020 gives a marginal propensity to consume (MPC) of around 10%. It is important, however, that self-selection into the program is prevalent according to Dancsik and Fellner (2021) and borrowing decisions before the pandemic and consumption decisions during the pandemic may be influenced by the same unobservable confounding factors. We could eliminate such distorting effects only in the estimations on the individual level (RDD) but not on the macro level (panel and cross sectional regressions).

We have additional preliminary results from the district level. We can demonstrate some positive spillover effects to firms with cross-sectional and panel regression: districts with

more household liquidity from payment moratorium exhibit lower shares of sole proprietors suspending their activity.

The difference between the district-level and the individual-level results may be explained with the following factors. First, the latter can be interpreted as causal effects, but the former are not necessarily. Second, we use considerably different proxies for consumption. Third, individual-level observations stem from one large bank, and they are not representative of all the indebted households, while district level observations cover the whole country.

We contribute to a growing body of research on payment moratorium during crises. Our paper is most closely related to Albuquerque and Varadi (2022), who examine the UK mortgage payment holiday introduced in March 2020. However, there are number of important differences between the two studies. First, Albuquerque and Varadi (2022) focus only on mortgages, and cannot precisely determine the exact amount of monthly instalments involved in payment holiday, which hinders the calculation of MPCs. Second, they use a difference-in-differences research design, where the treatment group includes mortgagors with a payment holiday for at least one month during March-November 2020. Households non-eligible for the policy, i.e. renters and outright owners, comprise the control group. Even after applying a number of robustness checks aiming to deal with possible time-varying unobserved household characteristics, they find no statistically significant consumption effect for the average household. Third, while their consumption data from an online budgeting application at transactional-level are more precise than ours, we observe not only flows, but also balances of individual bank accounts. This allows us to pinpoint loan origination dates and remaining maturities in order to employ RDD approaches, which eliminates the problem of time-varying unobserved confounding factors. Finally, Albuquerque and Varadi (2022) do not perform a district-level analysis, and accordingly does not address spillover issues.

Other studies dealing with payment moratorium have focused on the predictors of entry to the program, or on the impact on non-payment, for example in the U.S. (Cherry et

al. (2021), Shi (2021), An et al. (2022), Kim et al. (2022)), in Spain (Jiménez et al. (2021)), or in 10 Central, Eastern and Southeastern countries (Allinger and Beckmann (2021)). Based on Vihriälä (2022), the limited take-up of mortgage forbearance can be partly explained by the commitment to mandatory debt repayment against overconsumption due to self-control problems. Capponi et al. (2021) show that granting forbearance to households facing foreclosures in the U.S. has positive externalities on a broader range of households who intend to refinance.

Our paper is also related to the literature about mortgage market interventions in general. The seminal paper by Ganong and Noel (2020) examines the largest government intervention in the U.S. concerning mortgage debt renegotiation in the aftermath of the global financial crisis, the Home Affordable Modification Program (HAMP). It finds that consumption and default of homeowners respond much more to payment rather than debt reductions. In accordance with that, normative analyses in Eberly and Krishnamurthy (2014), Campbell et al. (2021), and Boar et al. (2022) show that different kinds of mortgage assistance policies aimed at providing temporary liquidity relief to homeowners yield a sizable welfare gain. According to Noel (2021), the payment moratorium design in the U.S. during the COVID-19 pandemic addressed each of the major shortcomings of the prior HAMP intervention including that the forbearance provided almost three times the immediate liquidity at one-sixth the NPV cost.

Our analysis also connects with studies estimating consumption effects of temporary fiscal stimulus payments. These papers document considerable average impacts, especially for low-income households and individuals with low levels of liquid wealth. Shapiro and Slemrod (2003) observe little more than 20% of survey respondents would use the 2001 tax rebates in the U.S. mainly to increase spending. Johnson et al. (2006) estimate causal effects for the same tax rebates and find that households spent 20% to 40% of their rebates on nondurable goods during the three-month period in which their rebates arrived, and roughly two-thirds of their rebates cumulatively during this period and the subsequent three-month period. Using a similar methodology, Parker et al. (2013) estimate 12% to

30% of stimulus payments in 2008 in the U.S. was spent on nondurable consumption during the three-month period in which the payments were received. With a significant effect on the purchase of durable goods the total effect reaches about 50% to 90%. Agarwal and Qian (2014) analyzes an exogenous unanticipated fiscal policy announcement by the Singapore government in 2011. During the 10 subsequent months, for each \$1 received, consumers on average spent \$0.80.

According to the large-scale survey of Coibion et al. (2020), individuals report having spent or planning to spend only around 40% of the one-time stimulus payments in the U.S. at the beginning of the COVID-19 pandemic on average. Estimated causal effects are material as well. Using high-frequency transaction data from the U.S., Baker et al. (2020) demonstrates that households respond rapidly to the receipt of stimulus payments, with spending increasing by \$0.25-\$0.40 per dollar of stimulus during the first weeks, mainly driven by food and non-durables. Highly liquid households show no response. Chetty et al. (2020) estimate that spending levels rose discontinuously right after receiving the stimulus payments by 25 pp for low-income households and 8 pp for high-income households. The program did not channel money back to the businesses that lost the most revenue due to the COVID shock. Karger and Rajan (2021) also documents large and immediate effect of receiving a stimulus check on spending, especially among low-income households. Hattori et al. (2021) provide evidence on lower consumption effects of cash transfers in Japan at the beginning of the pandemic. The estimated average MPC is approximately 10%, which does not differ from levels during non-pandemic periods in Japan.

The remainder of the paper is structured as follows. Section 2 discusses the background and the details of the Hungarian payment moratorium. Section 3 describes the data. Section 4 elaborates on the empirical methodology. Section 5 presents the results, and Section 6 concludes.

3.2 Policy background

3.2.1 Setup of the Hungarian moratorium

A general and unconditional loan repayment moratorium has been introduced in Hungary. Announced on 18 March 2020, the program came into effect on 19 March, initially lasting for 9 months until 31 December 2020. In Phase 1, an opt-out type of general moratorium was introduced, being available automatically to everyone who had a loan already disbursed on 18 March 2020. Initially all customers participated automatically in the program, but continuing repayments under the contract could have been requested. Phase 1 applied to both corporate and retail credit agreements, with the rule that the sum of the instalment due and the total amount of interest and fee arising under the payment moratorium and to be paid in instalments may not exceed the instalment under the original contract. Until May 2020, around 1.7 million retail clients remained in the moratorium with 64% of the eligible outstanding loans, amounting to 4,000 billion HUF. By August 2020, this ratio has only marginally decreased to 60% and 1.6 million debtors with a loan portfolio of 3,700 billion HUF. In December 2020, still 54% of the eligible retail loans amounting to 3,400 billion HUF participated in the moratorium (MNB (2021a)). Once adding up all monthly instalments under moratorium, this results in approximately 600-650 billion HUF additional extra liquidity provided to households by suspending payment of instalments for 2020. The biggest part of it being instalments of housing mortgages of 303 billion HUF, while personal loans being 166 billion HUF (MNB (2020b)). This is approximately 2.8% of the household consumption expenditure of the previous year of 2019.

The moratorium has been extended in an unchanged form in Phase 2 until 31 October 2021. The same conditions of opt-out program design applied for these 10 months, even though automatic entry was not provided. If anyone had an already disbursed loan on 18 March 2020, but did not participate in Phase 1, they were referred in person to the bank

where they could do it by applying in person. Exit from the program was only limited in Phase 2, meaning that in 2021Q3 still around 35% of all loans were participating in the scheme (MNB (2021b)). This extension is estimated to mean a 650-700 billion HUF extra liquidity to the participant households. From 1 November 2021, only the financially vulnerable and key social groups could request an extension of the moratorium in Phase 3. This time, moratorium design has been fundamentally altered, and an opt-in type of participation was introduced. Application for continued participation in the moratorium had to be requested in person at the bank. Only 8% of eligible loans remained in the moratorium, with an estimated portfolio of 470 billion HUF by the end of November 2021.

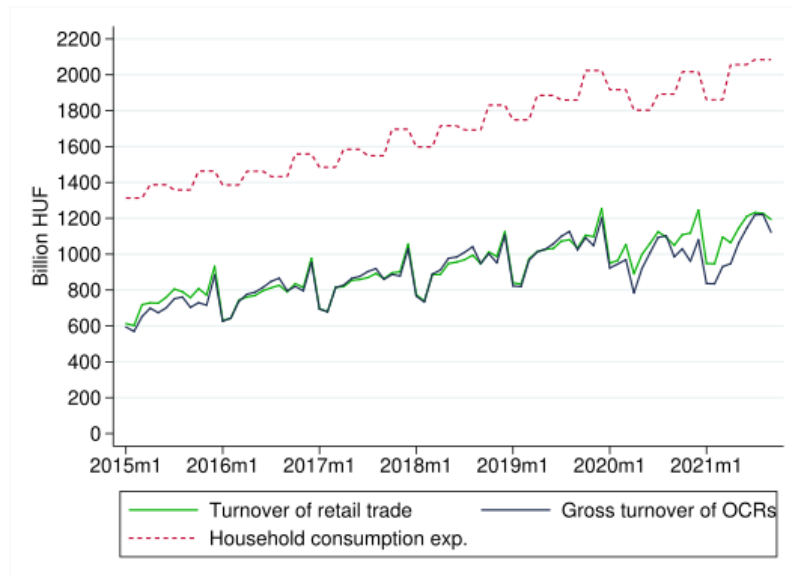
3.2.2 Sudden stop of consumption during Covid crisis

The aim of this research is to understand how debt payment moratorium affected consumption and spending during 2020-2021. The steadily increasing trend of household consumption stopped when Covid crisis hit the economy in early 2020 (see Figure 3.1). Unfortunately, consumption itself is not accessible on a granular level, therefore we use Online Cash Register (henceforth: OCR) turnover⁴ for our analysis to proxy the dynamics of consumption during the pandemic. Payments through OCR accounts for almost two-thirds of total consumption. We also demonstrate here that OCR turnover and turnover of retail trade are strongly related and follow similar trend as consumption. Similarly to consumption, increasing trend of turnover of retail trade stopped and it even started to slightly decrease in the first quarter of 2020. The gross turnover of online cash registers takes almost the same value as retail trade, although during 2020 and 2021 the gap between retail trade and OCR turnover widens. The reason for the increasing difference is that the volume of OCR turnover includes part of the catering, taxi services and accom-

⁴OCR data is collected on the transactional level by the National Tax and Customs Administration and here is aggregated on the country and monthly level. See detailed information about the dataset in Section 3.3.3.

modation services turnover that is not part of retail sales⁵ and decreased considerably due to pandemic induced lockdown measures⁶. Nevertheless, all three variables evolve similarly between 2015 and 2022, therefore we argue that OCR turnover's evolution can serve as a good proxy for the path of consumption.

Figure 3.1: Evolution of consumption variables, levels, Jan 2015 - Sept 2021



Note: Household consumption is from quarterly national accounts and divided by 3 for a monthly average.

Source: Hungarian Central Statistical Office, National Tax and Customs Administration

3.3 Data

3.3.1 Data used for individual level analysis on spending

We have access to individual level data on monthly current account spending of one anonymous bank. We merge this dataset to the credit registry, therefore we see all information from the credit registry and we also see a few additional demographic variables.

These are schooling, marital status and number of children. These help us to achieve more

⁵Magyar Nemzeti Bank: Inflation Report - March 2023. Budapest, Magyar Nemzeti Bank, 2023. p. 35. Available at: <https://www.mnb.hu/letoltes/eng-ir-digitalis-19.pdf> (Accessed: 15 March 2026)

⁶This difference is mainly driven by the inclusion of such services in OCR statistics whose consumption decreased directly due to lock down measures and the decline is not related to current income of households. As this drop is also unrelated to the debt payment moratorium it is unlikely that it would drive our results. However, including items in the dependent variable that are unrelated to the treatment may decrease the estimated effect. If that happens, than our results could be underestimated compared to the true effect.

precise estimations. We focus only on mortgage loans for two reasons: there are only a small number of observations for other types of loans in the dataset. This is binding in case of a regression discontinuity approach, that allows us to include borrowers, whose loans were originated only in a few weeks period.

Secondly, we observe a unique pattern of increased spending typically after taking a mortgage. We suspect that it is related to consumption that typically occurs after buying a property (e.g. buying furnitures, renovation, etc.). It potentially makes our estimations noisy, therefore we should find a way to handle this. We elaborate more on the matter when we describe the regression discontinuity design in the section of empirical approach.

We briefly discuss the descriptive statistics of the data used for the RDD estimations. The dataset is narrowed down to those borrowers whose loan was disbursed within 50 days around the cutoff of 18 March 2020. Only those observations are considered that are part of the estimations, therefore monthly account information (spending and inflow) between July 2020 and 2021 October. 64% of the borrowers were eligible to participate in the moratorium and taking into account all the 16 months of our sample the average monthly participation rate was 18%. On monthly average the 29% of the eligibles participated in the program. Average monthly spending during the 16 months was above 860.000 HUF and the average inflow was close to 985.000 HUF. Compared the average gross wage of around 823.000 HUF these inflows and spending figures are high. The reason is likely that account inflows and outflows related either to the transaction or real estate linked consumption also appear in these figures, therefore these numbers are somewhat inflated. Monthly instalment is around 108.000HUF in our sample. Average maturity of the loans is 192 months (16 years), while the average age of the borrowers is 40.

3.3.2 Data used for individual level analysis on further outcomes

We use individual data from HITREG to estimate RD models on non-performing loans, early repayment of loans and additional loan taking behavior of clients. Our sample con-

Table 3.1: Descriptive statistics of the sample used in estimation of spending

	Average	Standard deviation
Eligible (%)	64	48
In moratorium (%)	18	39
In moratorium among eligible (%)	29	45
Spending	860 774	2 526 155
Inflow	984 730	2 730 282
Monthly instalment	108 431	65 639
Wage	823 110	598 845
Maturity	192	67
Age	40	8
Observations	18879	

Note: Table contains the same data used in estimations. Timeframe: Loans disbursed between 27 January 2020 and 8 May 2020, spending, inflow and participation are averaged through the months from July 2020 to October 2021. Wage is the gross wage reported to the National Tax Authority.

sists the whole universe of housing loans that were granted during the first half of 2020. We observe the loans until November 2024. We see monthly data on the performance of the loan (number of days in delinquency, total unpaid amount) and we also see if the borrower of the loan has taken a new loan or repayed any of her existing loans. We also observe key borrower characteristics such as age and wage of the borrower⁷, district where the borrower lives, Payment-to-Income ratio, Loan-to-value ratio, total amount of the loan, maturity and interest rate.

Table 3.2: Descriptive statistics of the sample used in estimation of Probability of default, early repayment and additional borrowing

	Average	Standard deviation
In Moratorium (per cent)	23.7	42.5
Eligible to Moratorium (per cent)	33.4	47.2
In moratorium among eligible (per cent)	70.9	-
Wage (HUF Thd)	511.7	322.5
Loan amount (HUF Mln)	10.3	9.6
Interest rate (per cent)	4.1	1.5
Monthly instalment (HUF Thd)	82.7	61.7
Maturity	15.0	7.8
Age	39.5	9.7
Observations	45819.0	

Note: Table contains the same data used in estimations. Timeframe: Loans disbursed between 1 January 2020 and 30 June 2020. Wage is the sum of net wages of all borrowers of the loan (reported by banks to ensure compliance with Payment-to-income legislation).

⁷Regarding wage we observe the total income that of all the borrowers of the given contract. This is the income that is used to calculate Payment-to-Income ratio.

3.3.3 Data used for district level analysis

Consumption

We use the gross turnover of Hungarian online cash registers aggregated at monthly frequency and on a regional (district-level) scale to approximate consumption.⁸ OCRs are mandatory to use in several parts of the services sector, namely retail trade, wholesale trade, taxi services, food and accommodation services, rental activities and repair activities. From 2017, OCRs are mandatory in parts of e-commercial activities also.

OCR data is collected by the National Tax and Customs Administration (henceforth: NTCA) on a transaction level. Following strict anonymization rules, we use machine level data on a daily basis. The database includes the net and gross value of the transaction, the type of payment (cash or card), the 4-digit NACE code of the main operator of the machine (which might deviate somewhat from the NACE of the establishment itself), and the district where the machine is located (out of the 197 district of Hungary). A novelty of OCR data is that since credit card and cash payments are both recorded for each payment, this data is free of the changing composition effect between cash or card based payments, which would bias our results during such epidemic induced economic shocks.⁹

Official branch statistics for retail trade published by the Hungarian Central Statistical Office (henceforth: HCSO) utilizes firm level surveys, VAT information for parcel delivery, data of National Tobacco Trading Ltd. and turnover of OCR machines to estimate monthly retail trade statistics.¹⁰ In turn, retail trade statistics are utilized besides input-

⁸During the course of 2013 and 2014, the Hungarian government introduced OCRs in several branches of the services sector to increase the government's tax revenues, and reduce the size of the shadow economy and the extent of tax avoidance. The gradual introduction of OCRs in the first batch finished by 2016, with several new branches were included from 2017. Lovics et al. (2019) inspect extensively the effects of the introduction on VAT returns.

⁹As the result of the pandemic, governments initiated policies to avoid in-person contacts including payments. Several studies find a large and persistent substitution effect from cash to card-based transactions, especially towards contactless POS payments and in some case e-commerce activity. Studies include for example Ardizzi et al. (2020) in case of Italy, Jonker et al. (2020) for the Netherlands, Kraenzlin et al. (2021) for Switzerland, Kotkowski & Polasik (2021) for several European countries.

¹⁰See <https://www.ksh.hu/docs/hun/modszgyors/kismodsz21.html> on the methodological background.

output tables in estimating household consumption expenditure for national accounts.¹¹

There is strong comovement between the OCR turnover and both retail trade and household consumption expenditure series. In 2015Q1, both OCR turnover and retail trade is estimated to be a monthly 600 billion HUF, while the monthly average of household consumption expenditure reaching approximately 1300 billion HUF. This ratio did not change meaningfully in the last 6 years, as OCR turnover averaged around 1200 billion HUF while average monthly consumption around 2100 billion HUF in 2021Q3. As OCRs do not entirely cover household consumption, but a part of consumption is reflected in OCR turnover, we do not aim to precisely estimate the level of consumption, but to approximate its dynamics instead.

Moratorium participation

We use the participation characteristics of each outstanding individual loan contract in April 2020 as a reference for our calculations. Household loan data is provided by the Central Bank Credit Registry ('HITREG'), an anonymous registry on all loan contracts provided by credit institutions on a monthly frequency.¹² Including around 500 variables, this extremely rich dataset enables us to utilize information on all loans both on the contract and the debtor level. From this database, we will use the amount of loan, type of loan, maturity, interest rate, age, district, and outstanding principal to classify the loan. Household data contains the take-up of both new household loans and already disbursed ones.

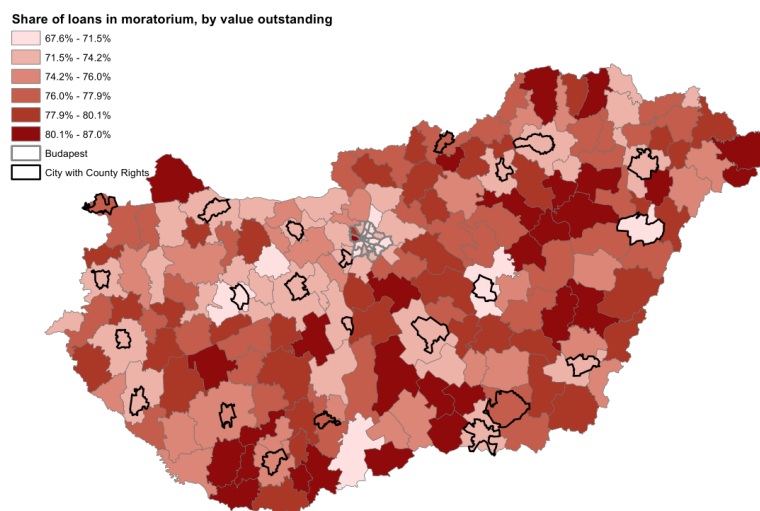
In May 2020 around 1.7 million retail clients were participating in the moratorium with 64% of the eligible outstanding loans, amounting to 4,000 billion HUF. There exists a considerable variation in loan participation. Figure 3.2 presents the regional disparities in

¹¹Value added of retail trade was 12% of Hungarian GDP in 2019. Total Turnover of retail trade consisted around 26% of GDP, while household consumption is a much broader group added up to 44% of the Hungarian GDP in 2019.

¹²The following institutions are included: banks, savings cooperatives, leasing companies, companies specialising in the provision of personal loans, sales finance and housing loans, and other financial enterprises.

the share of loan value in moratorium for each district. Moratorium participation is lower in districts including cities with county rights, higher in the countryside and somewhat less pronounced in the central and northwestern districts of the country.

Figure 3.2: Share of loans in moratorium by value outstanding, April 2020



Source: Own calculation based on Central Bank of Hungary

Extra liquidity

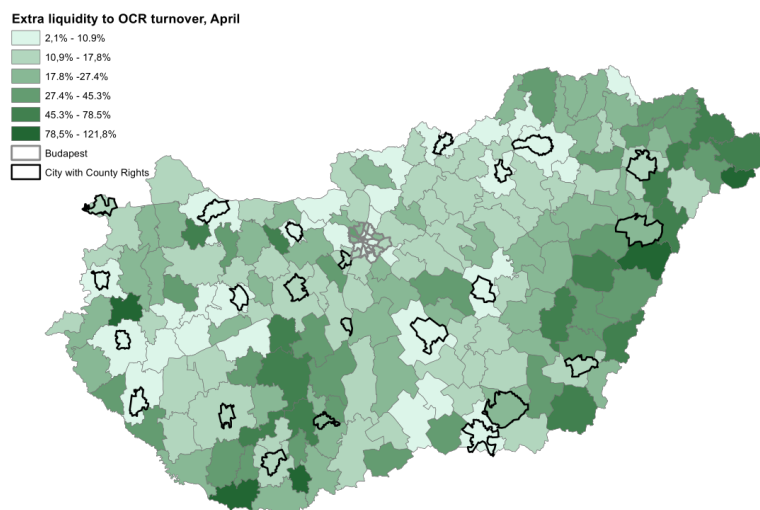
Based on the location of the debtor, we aggregate monthly instalments at the district level to calculate the sum of all instalment payments under repayment moratorium on a monthly frequency.

We define *extra liquidity to OCR turnover* by normalizing the actual loan instalments under moratorium with the consumption level of the district twelve months earlier. By doing so, we rule out seasonality problems and any potential problems that are related to the differences of consumption levels of the districts. Mathematically, we calculate the following, where i is a district, t is the month, and b is the number of loans in moratorium in district i :

$$\text{Extra liquidity}_{i,t} = \frac{\sum_{n=1}^b \text{Monthly loan instalments}_{i,t}}{\text{Gross turnover of online cash registers}_{i,t-12}} \quad (3.1)$$

Figure 3.3 presents the regional distribution of the extra liquidity provided to households on the district level. Extra liquidity is most pronounced in the countryside districts, especially the northeastern, eastern and southwestern parts of the country.

Figure 3.3: Extra liquidity provided to households, April 2020



Notes: This figure displays the extra liquidity left at households (unpaid monthly instalments) as a share of the aggregate district-level online cash register turnover.
Source: Own calculation based on Central Bank of Hungary, National Tax and Customs Administration

Figure 3.4 plots the relation between extra liquidity and consumption change measured by annual dynamics of OCR turnover in April 2020. Most importantly, higher moratorium participation at the district level correlates with lower drop in annual consumption in April 2020. Consumption drop is especially pronounced in more urbanized areas, as contact-intensive services are mostly concentrated in cities. At the same time, consumption drop is moderate in several countryside districts.

In Budapest city center the extra liquidity from moratorium was small. The residential districts of Budapest show a somewhat bigger role for the extra liquidity, and locate almost similarly with districts which include a city with county rights (the regional economic centers of the country). Here, moratorium provided an additional liquidity of around 20% to the local purchasing power. Finally, rather rural-type of countryside districts experienced a huge inflow of additional liquidity compared to OCR turnover twelve months earlier. Exposure of these districts to tourism and services was limited and aggregate consumption was also low in these areas.

Figure 3.4: Relationship between consumption change and extra liquidity



Source: Own calculations based on Central Bank of Hungary, National Tax and Customs Administration

District fixed effects

District fixed effects are defined as dummy variables taking up the value of 1 if a district is part of a given subgroup. The capital of Budapest is split into 2 subgroups: while the city itself had a population of 1.76M in 2019, and a population share of 18% in the country, 26% of OCR turnover is reported there. *Budapest city center* consists of the 5 central districts of Budapest being the hub of services and tourism activities, namely District 1, 5, 6, 7, 8. *Budapest residential* consists of the remaining 18 districts of the capital. In terms of administrative borders, Hungary consists of 19 counties and the capital of Budapest which is classified as a separate county. Altogether 23 cities are endowed with county rights, 19 of them being a county seat also. These cities are the regional centres which are the hub of regional economy, even though their population and hence local market size has a wide variation ranging from 32 to 200 thousand inhabitants. In our analysis, we define a subgroup of those districts that incorporate a settlement with county rights, namely *including city with county rights*. The remaining 151 mainly rural type of districts rarely have a town with a population over 25 thousand, hence they are classified as *countryside*

regions.

3.4 Empirical approach

The main challenge when we want to estimate the impact of the debt payment moratorium on consumption is that participants are selected endogenously (see more details about participation in Appendix 3.A.2). There can be unobserved factors that effect both moratorium participation and consumption. For instance the threat from the negative economic consequences of closures related to COVID-19 may have served as an incentive to accumulate liquidity and cut back consumption. Then by comparing participants' consumption to non-participants' consumption we would underestimate the impact of the program. However, it could be liquidity constrained borrowers (who want to consume more than they can) were more likely to participate in the program. Then the comparison of the consumption of participants and non-participants would be again misleading and would result in overestimation of the impact of the program. Consequently, the estimated impact of the debt payment moratorium on consumption is potentially biased. However, we find exogenous variation of participation on the individual level that allows us to identify the causal effect of debt moratorium on spending. We estimate the impact in a regression discontinuity design. We exploit that the program was introduced unexpectedly and those borrowers who received their loans after the program was announced were not eligible to participate in the program.

We also attempt to show that the program had an economically relevant impact on the macro level as well. We estimate how strongly related the moratorium induced extra liquidity to consumption. Unfortunately, we cannot fully eliminate selection bias in our district level estimations. We explore the co-movement between consumption and debt moratorium participation by running cross-sectional regressions. Although we try to include many explanatory variables on the district level we cannot rule out the possibility

of omitted variable bias. Therefore, we add time dimension to our analysis and estimate similar models in a panel setup. We expect that the panel model can partially address the endogeneity issues.

3.4.1 Individual level observations: Regression discontinuity

Spending

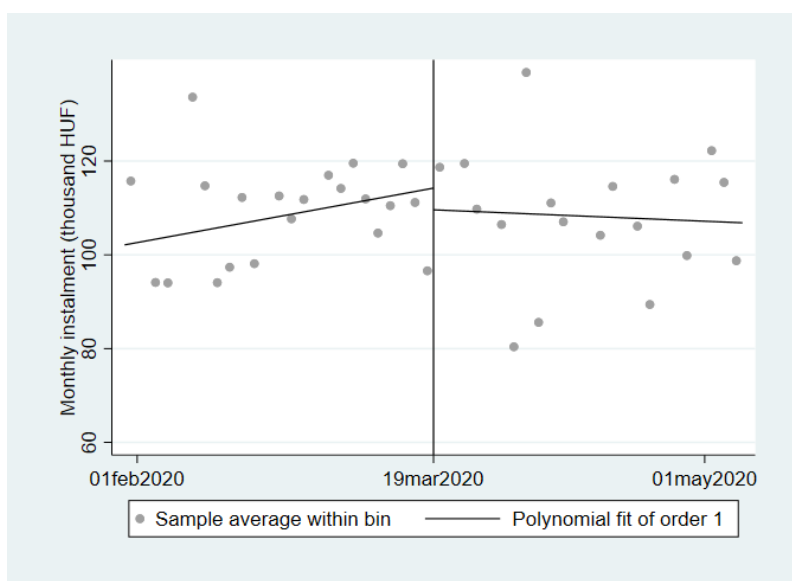
In the next section we discuss our approach to identify the causal effect of the debt payment moratorium program on spending, Non-performing loans, early repayment of loans and on new loan taking. As we have access to individual level data on mortgage loans, we can analyze spending in a regression discontinuity setup.

The debt moratorium was open to all borrowers, who were granted their loans *before* the announcement of the program. Granting a mortgage loan takes 2-3 months on average and there is high variance regarding the length of the process. We argue that among those who received their loans within a small neighborhood of the cutoff date (date of announcement of the program) can be taken as random whether that date was before or after the cutoff. Therefore, we expect that these two groups of people are similar in all their characteristics except that those who received their loans before the cutoff are eligible to participate in the moratorium. This assumption is tested on the observable characteristics of the borrowers. First we show that there is no jump in the observed covariates around the cutoff, therefore we think that getting the loan before or after the treatment can be looked at as random (Figure 6 - Figure 9).

To formally test there is no jump around the cutoff date we run balance tests (t-tests) on the observed covariates.

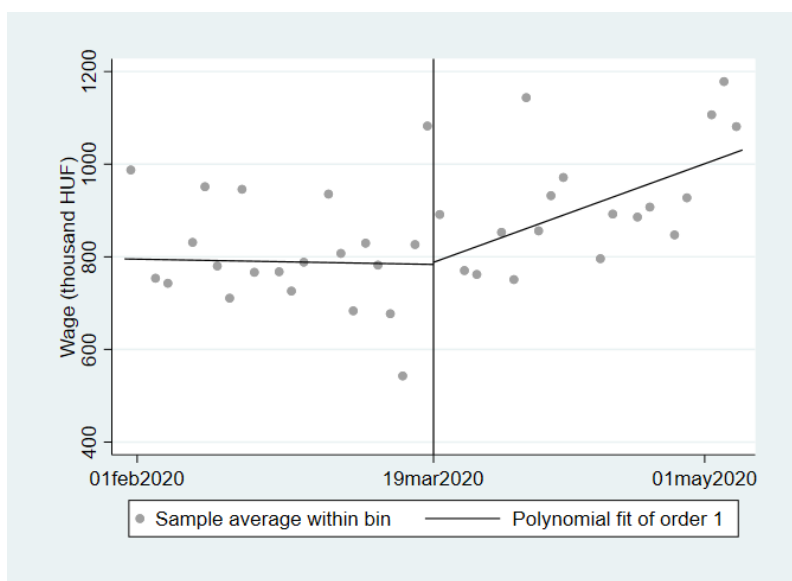
It is an important aspect, however, that all these loans were newly granted mortgages. It can be observed that those borrowers who have just received their mortgage loans tend to spend more than normally. Figure 3.9 shows the 30 day average spending of borrowers

Figure 3.5: Monthly instalments around the cutoff



Note: This figure plots the local averages of monthly instalments of those whose loan was disbursed around 18 March 2020 (+/- 50 days). The solid lines denote local linear fits. The absence of any significant discontinuity at the threshold indicates that the treatment and control groups are balanced, satisfying the fundamental RDD identifying assumption.

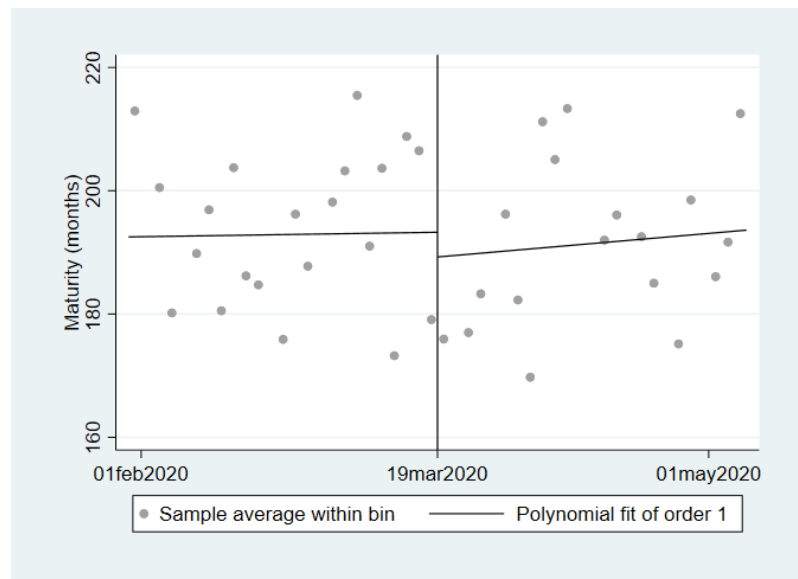
Figure 3.6: Wages around the cutoff



Note: This figure plots the local averages of monthly wages of those whose loan was disbursed around 18 March 2020 (+/- 50 days). The solid lines denote local linear fits. The absence of any significant discontinuity at the threshold indicates that the treatment and control groups are balanced, satisfying the fundamental RDD identifying assumption.

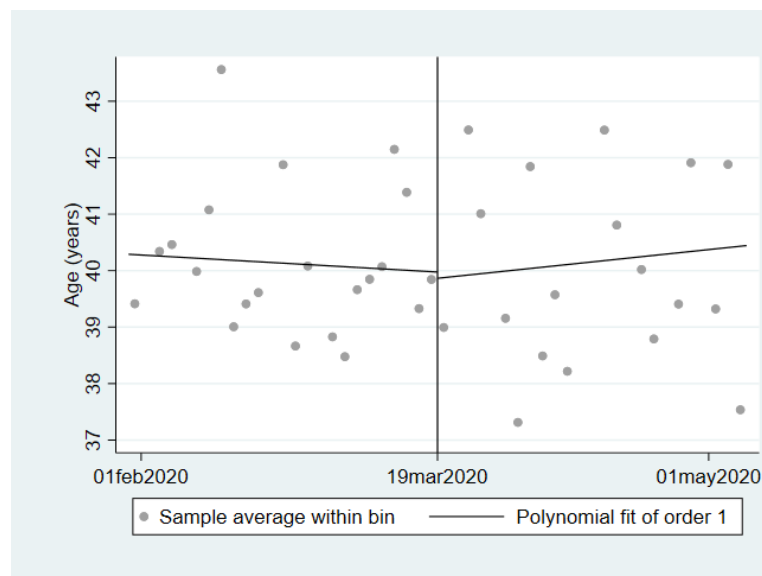
around the day of the mortgage was disbursed. Spending is high both before and after loan origination. High spending may be related to the real estate transaction in the one or two months window around loan origination. The high spending in later months may be driven by high costs related to switching home: buying furnitures, renovation, paying taxes related to buying new properties, etc.

Figure 3.7: Maturity of loans around the cutoff



Note: This figure plots the local averages of loan maturities (in months) of those whose loan was disbursed around 18 March 2020 (+/- 50 days). The solid lines denote local linear fits. The absence of any significant discontinuity at the threshold indicates that the treatment and control groups are balanced, satisfying the fundamental RDD identifying assumption.

Figure 3.8: Age of borrowers around the cutoff



Note: This figure plots the local averages of age of those borrowers whose loan was disbursed around 18 March 2020 (+/- 50 days). The solid lines denote local linear fits. The absence of any significant discontinuity at the threshold indicates that the treatment and control groups are balanced, satisfying the fundamental RDD identifying assumption.

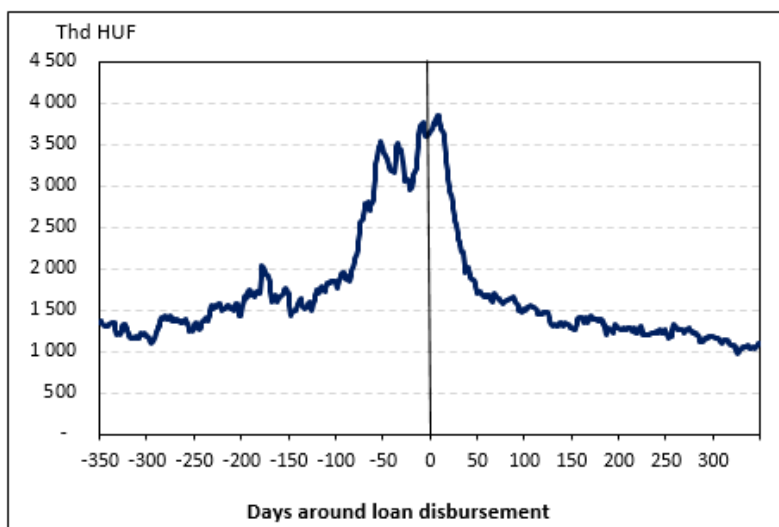
We try to overcome this problem by two ways. First, we use the ratio of the total inflow to total outflow of all the borrower's accounts. This can be looked at as a normalization. It is especially helpful in case of large inflows and outflows that may be related to the real estate transaction. Second, we include observations only between July 2020 and October 2021. By leaving out the first three and a half months of the program (March 2020 - June

Table 3.3: Balance test of key borrower and loan characteristics

	Noneligible	Eligible	Diff	pvalue
Wage (HUF)	858 204	783 678	-74 526	0.1054
Age (year)	40.0	39.8	-0.2	0.7351
Maturity (month)	195	193	-1.8	0.7222
Monthly instalment (HUF)	107 915	111 699	3 784	0.4740

Note: Table contains data of loans that were disbursed between 18th of February 2020 and 19th of April 2020

Figure 3.9: Evolution monthly spending around the disbursement of a mortgage loan



This figure illustrates the dynamic evolution of monthly spending around the date of taking a mortgage loan (denoted as time 0 on the x-axis). Data shown only for customers of one large bank with mortgage loans disbursed between 2019 December and 2020 May.

2020), we focus on observations where spending related to paying the price of the real estate occurs with low probability.

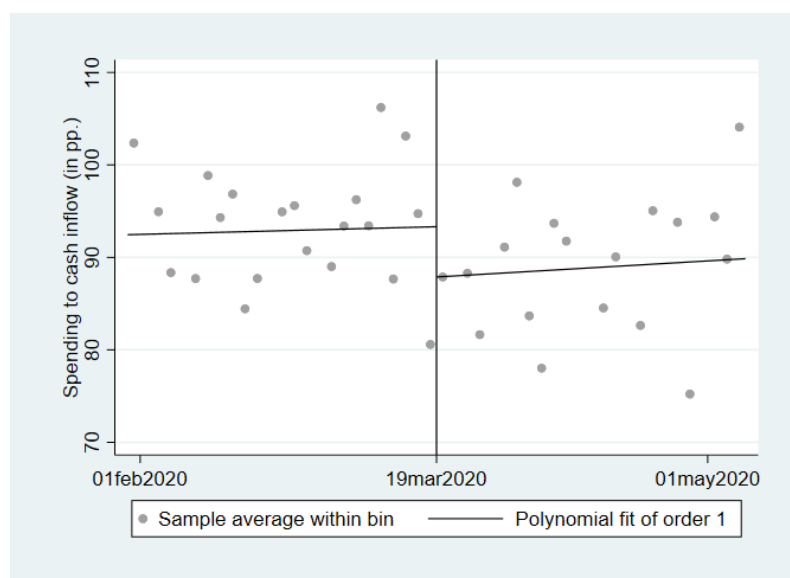
A potential threat to our identification is that as the COVID-19 shock and closures hit the economy some borrowers may have stepped back from taking a loan or the banks may have rejected some applications. The series of the number of loans granted around the cut-off date do not show any suspicious change, therefore we think that this problem does not hurt our identification. Wages of borrowers increase gradually after the cutoff, however it is important to note there is no jump at the cutoff. Therefore, the main identifying assumption of RDD, i.e. absent the debt payment moratorium the dependent variable would evolve smoothly around the cutoff is still plausible. Moreover, the narrow windows of 20 - 30 days around the cutoff that we choose for the estimations ensures that those wages that are in the estimation sample are similar.

Another potential threat is that the composition of borrowers changes considerably during the program. Ideally, the RD approach takes care of this if the assumption that the narrow vicinity of the cutoff borrowers are randomly assigned to eligible or non-eligible groups is valid. Compositional changes further away from the cutoff are captured by the running variable and do not pose any threat to the identification.

We estimate intention-to-treat effect. Intention-to-treat estimates may be relevant from a policy perspective as the policymaker can decide on either introduce debt payment moratorium or not, but cannot force all borrowers to participate.

We use the ratio of spending to inflow of all the borrowers' accounts in a given month. This helps us to partially handle the issue of large spending that may be related to the property transaction (e.g. borrowing from a family member and send the amount further to the seller of the property). First we show how the ratio of spending evolves around the cutoff:

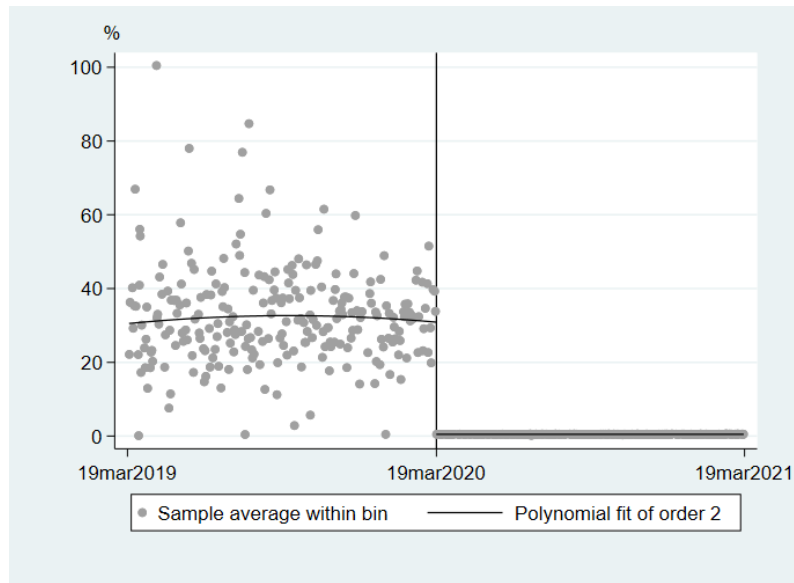
Figure 3.10: Ratio of spending to inflow of borrowers' accounts around the cutoff



Note: This figure displays a regression discontinuity plot of the spending/inflow rate (in percentage points) of those whose loan was disbursed around 18 March 2020 (+/- 50 days). The x-axis represents the date of loan origination, centered around the March 18, 2020 threshold. The solid lines represent fitted values from linear regressions, while the scatter points indicate daily averages. The significant discontinuous drop at the threshold illustrates the causal impact of the moratorium in preventing short-term financial distress among eligible borrowers.

We also check how the eligibility is related to the treatment. Non-eligibles are never treated, while close to 30 percent of the eligibles participates the program.

Figure 3.11: Daily participation rate around the cutoff



Note: Participation rate of those whose loan was disbursed during the one year window around 18 March 2020

We estimate the following equation:

$$Y_{i,t} = \alpha + \beta_1 D_{i,t} + \beta_2 F_{i,t} + \beta_3 D_{i,t} F_{i,t} + \gamma Controls_{i,t} + \epsilon_{i,t} \quad (3.2)$$

where $Y_{i,t}$ is the ratio of total bank account outflow to inflow in a given month, $D_{i,t}$ is an indicator variable that takes the value of 1 if the borrower was eligible for debt payment moratorium, $F_{i,t}$ is the running variable, $D_{i,t} F_{i,t}$ is the interaction of the treatment indicator and the running variable, while $Controls_{i,t}$ is a vector that include control variables (wage, maturity, age, time fixed effect) and $\epsilon_{i,t}$ is the disturbance term.

Loan performance, early repayment and new loan taking

We estimate a similar regression as in the previous section to identify the impact of the program on loan performance, early repayment of loans and new loans taken. We use the following metrics to proxy non-performance: all the loans that were in 30 days (or 90 days) delinquency with at least an amount of HUF 10 thousand marked as non-performing. We use two timeframes: first we follow the loans until the end of the pro-

gram of 2021 October, therefore if there is any month in which a loan was in 30 or 90 days delinquency that is considered as a non-performing loan. We also test if there was any longlasting effect of the program on performance: we mark all loans that were in 30 days or in 90 days delinquency between July 2022 and November 2024.¹³ We use the same timeframes to measure if the borrower has payed back any loans or has taken new loans. Early repayment is calculated as the total sum of repayed loan amount divided by the total loan amount of the borrower in the month when the moratorium related housing loan was taken. Similarly, new loan taking is calculated as the total sum of newly granted loan amount divided by the total loan amount of the borrower in the month when the moratorium related housing loan was taken. We show how other variables evolve through the cutoff to demonstrate that there was no manipulation of the policy (see Figure 3.12, Figure 3.13 and Figure 3.14).

We estimate these effects by local linear regressions:

$$\tau_y = \lim_{z \rightarrow 0^+} \mathbb{E}[Y|Z_i = z] - \lim_{z \rightarrow 0^-} \mathbb{E}[Y|Z_i = z], \quad (3.3)$$

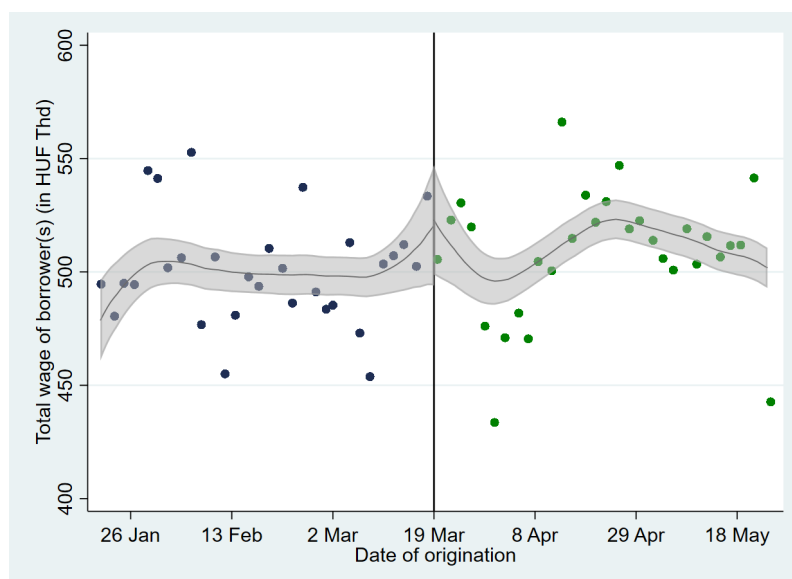
where z is the difference between the date of origination and March 19, 2020, the cutoff, that is the first day when the moratorium was in effect. We estimate (3.3) by local linear regressions. We use triangular kernel with MSE-optimal bandwidth following Imbens and Kalyanaraman (2012) and Calonico et al. (2014).

We also create an indicator that proxies product-borrower segments that helps to detect heterogeneity of the effect of the program. First, we model the interest rate based on observable characteristics of borrowers, loans, local market concentration and also including bank and district fixed effects. We use only a subset of the estimated parameters, namely the coefficients on loan characteristics and borrower characteristics to create a score of the given loan. By this approach we separate the observable borrower and loan related aspects

¹³The starting point of this second timeframe is the first month after the partial moratorium (in which only indigent borrowers were allowed to participate) ended.

of interest rates from bank and market related aspects and also from the non-observable aspects of interest rates. Based on the score we create ten deciles of loans and create three broad categories: the affluent segment that we define as loans in the bottom 3 deciles, the mass segment that we define as loans in the top 3 deciles and mid-market that is loans in the remaining middle 4 deciles¹⁴.

Figure 3.12: Evolution of income when moratorium was introduced



Note: This figure plots the local averages of wages around the March 18, 2020 eligibility threshold. The solid lines denote local linear fits. The absence of any significant discontinuity at the threshold indicates that the treatment and control groups are balanced, satisfying the fundamental RDD identifying assumption.

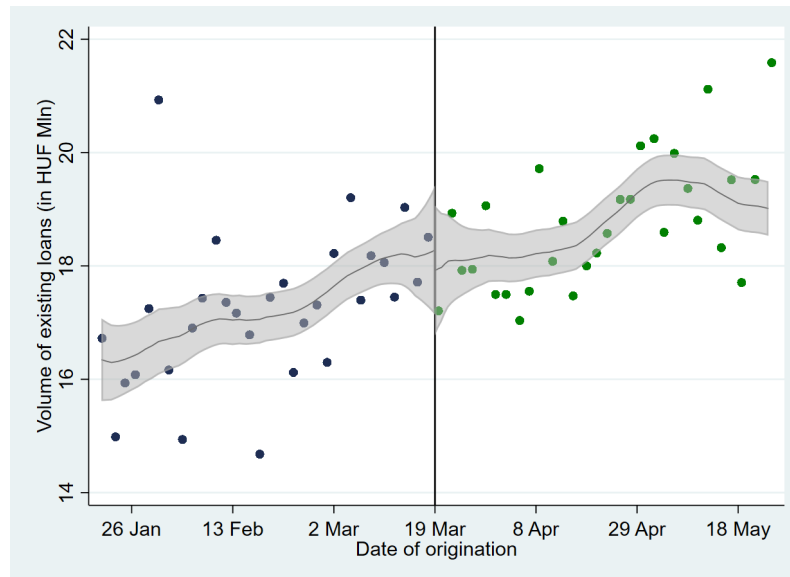
3.4.2 District level estimations: Cross-sectional regressions

We start by describing the relationship between the yearly change of online cash register turnover and the extra liquidity that is left at borrowers by participating in the debt moratorium. Estimations are on the district level and run for one month: 2020 April. We estimate the following specification:

$$Y_i = \alpha + \beta X_i + \gamma Controls_i + \epsilon_i \quad (3.4)$$

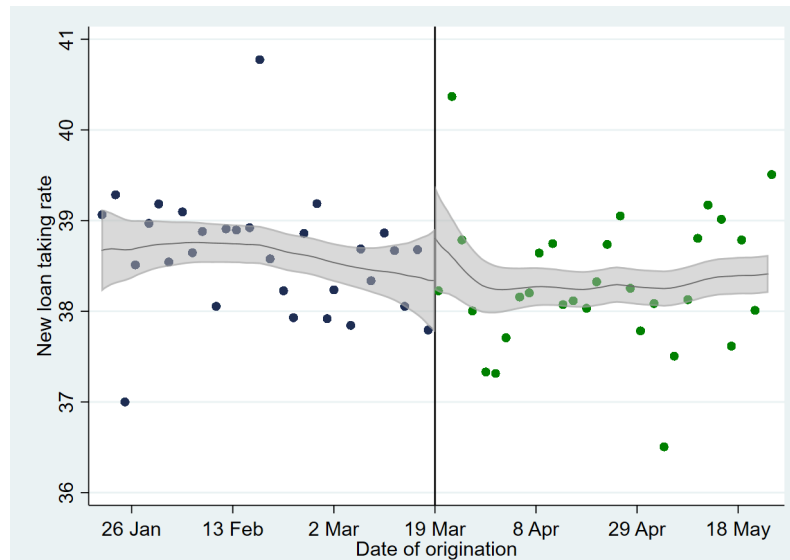
¹⁴Exact details of the indicator and the way how we calculate are in Appendix 3.A.3

Figure 3.13: Volume of existing loans



Note: This figure plots the local averages of total volumes of borrowers' existing loans around the March 18, 2020 eligibility threshold. The solid lines denote local linear fits. The absence of any significant discontinuity at the threshold indicates that the treatment and control groups are balanced, satisfying the fundamental RDD identifying assumption.

Figure 3.14: Age of borrowers



Note: This figure plots the local averages of age of borrowers around the March 18, 2020 eligibility threshold. The solid lines denote local linear fits. The absence of any significant discontinuity at the threshold indicates that the treatment and control groups are balanced, satisfying the fundamental RDD identifying assumption.

$$Y_i = \frac{OCR_{i,2020\text{April}}}{OCR_{i,2019\text{April}}} \quad (3.5)$$

$$X_i = \frac{\text{Extra liquidity}_{i,2020\text{April}}}{OCR_{i,2019\text{April}}} = \frac{\sum_{n=1}^b \text{Extra liquidity}_{n,i,2020\text{April}}}{OCR_{i,2019\text{April}}} \quad (3.6)$$

where i is a district, b is the number of loans in moratorium in district i , ϵ_i is a disturbance term. Although it is not possible to fully rule out all the endogeneity issues, we include a large set of control variables to partially address this problem. We include a proxy for wages, jobseeker rate, several development and population related variables and settlement type dummy variables. We describe the key variables in the Appendix 3.A.4. Although we use a large set of controls, concerns remain that there can be other unobserved factors that affect both participation in the program and consumption (selection bias) and then the coefficient estimates are biased.

We try different functional forms regarding the key explanatory variable. As we suspect that the relationship between extra liquidity and consumption may be not linear, we try a specification where the squared term of extra liquidity is added and in another specification we use the logarithm of extra liquidity.

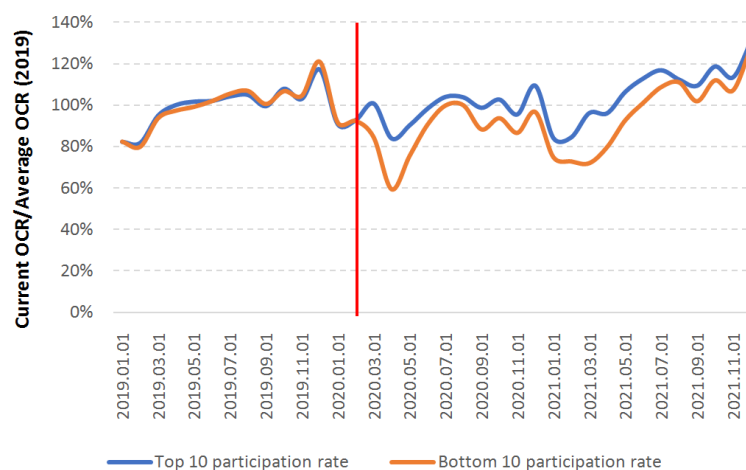
This program was so large scale that it could potentially increase consumption on the aggregate level. Therefore, there can be spillover effects that are observable on the district level. Due to data limitations, now we test only if there is any co-movement between consumption and activity of sole entrepreneurs during the COVID-19 shock. Activity of sole entrepreneurs is proxied by the share of sole entrepreneurs who suspended their businesses. We expect that where higher amount of extra liquidity was left at borrowers, there consumption was boosted on the district level. Therefore, sole entrepreneurs faced higher demand, and had a higher chance of continuing their businesses compared to districts where consumption was not boosted such a way.

In this section we discussed how we can describe the co-movement between consumption and moratorium participation at a certain period. In the next section we add time dimension to our analysis and show similar estimations in a panel model setting.

3.4.3 District level estimations: Panel regressions

Figure 3.15 shows the evolution of consumption among those 10 districts where moratorium participation was the highest and those 10 districts where moratorium participation was the lowest. Consumption in every month is divided by the average consumption of 2019, this consumption rate is plot on the figure below. Before COVID-19 and the corresponding closures hit the economy the consumption rate of the two groups were similar: people who lived in these districts cut back consumption in the beginning of the year (after Christmas), gradually increased and then kept consumption on the average level of 2019, and finally increased consumption around Christmas time. There were no differences among the two groups of districts. However, since March 2020 there is a large gap between the two groups of districts: those districts where moratorium participation was the lowest tended to decrease consumption systematically more than those districts where moratorium participation was the highest. Our intuition is that differences between moratorium participation drove the differences between consumption rates.

Figure 3.15: Evolution of OCR turnover by groups of districts



Note: This figure plots the evolution of the annual percentage change in Online Cash Register (OCR) turnover for two subsets of districts: the top 10 districts with the highest moratorium participation rates (blue line) and the bottom 10 districts with the lowest participation rates (orange line). The vertical line denotes February 2020, the last month before the onset of the COVID-19 shock. The divergence in the post-shock period illustrates the mitigating real-economic effect of the targeted liquidity transfer.

Current OCR: Monthly total OCR volume by group

Average OCR 2019: Average of total OCR volumes in 2019 by group

To investigate our conjecture more formally and more broadly we run panel model estimations on the district level. The main contribution of the panel model setup is that a

large part of endogeneity problems can be eliminated. *Time invariant* unobserved factors that affect both consumption and moratorium participation can be ruled out by panel model techniques (e.g. within transformation). Nevertheless, there can be *time variant* unobserved factors as well. Those cannot be eliminated by the method that we follow, therefore panel model results still cannot be interpreted as causal effects.

We assume that the data generating process can be modeled by the following expression:

$$Y_{i,t} = \alpha + \delta_i + \beta_1 X_{i,t} + \epsilon_{i,t} \quad (3.7)$$

The dependent variable is the yearly change of online cash register turnover in a given district at period t :

$$Y_{i,t} = \frac{OCR_{i,t}}{OCR_{i,t-4}} \quad (3.8)$$

$X_{i,t}$ is the extra liquidity that is left at borrowers aggregated up to the district level at period t , b is the number of loans in moratorium in district i :

$$X_{i,t} = \frac{Extra\ liquidity_{i,t}}{OCR_{i,t-4}} = \frac{\sum_{n=1}^b Extra\ liquidity_{n,i,t}}{OCR_{i,t-4}} \quad (3.9)$$

δ_i stands for district level fixed effect that is to be eliminated by within transformation, $\epsilon_{i,t}$ is the disturbance term of district i at period t . Models are estimated on a quarterly basis.

We also test if the relationship between share of suspenders can be found in the panel setting as well. We calculate the share of suspenders in the same way as in the cross-sectional setup, just now a time dimension is added as well.

As a robustness check we also estimate the coefficient of the key explanatory variable using first differences instead of the within transformation. In this case we lose the first

set of observations therefore our sample size becomes smaller.

3.5 Results

3.5.1 Regression discontinuity

Spending

First, we show the results of estimations of the regression discontinuity design. As granting a loan takes typically 2-3 months we take a one month window around the cutoff. The reason to use such a large window is to keep the number of observations high. As a robustness check, we also test our results on different windows around the cutoff.

	(1)	(2)	(3)	(4)
	Spending rate	Spending rate	Spending rate	Spending rate
Eligible	8.660*** (3.52)	8.893*** (2.79)	10.14** (2.24)	11.08** (2.16)
Running var	0.137*** (2.73)	0.131 (1.55)	0.0289 (0.12)	0.255 (0.78)
Eligible x Running var	-0.108** (-1.97)	-0.101 (-0.93)	0.179 (0.63)	-0.140 (-0.37)
Observations	31965	18106	11200	9742
R^2	0.010	0.012	0.015	0.013
Controls	YES	YES	YES	YES
Time FE	YES	YES	YES	YES
Loan disbursement window	1 year	50 day	30 day	20 day

t statistics in parentheses, standard errors clustered on client id

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Note: This table reports regression discontinuity (RDD) estimates of the moratorium's causal effect on spending using account-level data. The dependent variable is the monthly spending rate (spending relative to total monthly inflow to the bank account). The treatment variable, "Eligible", is an indicator equal to 1 for loans originated before the March 18, 2020 eligibility cutoff date. Controls include borrowers' wage, age and the maturity of the loan.

Column 1 shows a year wide window around the cutoff, therefore we focus on Column 2-4. Eligible dummy variable takes the value around 10 percent that is around 90% of the monthly instalment¹⁵. This variable is the estimated intention-to-treat effect, therefore it

¹⁵The average monthly inflow of the bank accounts in the sample is almost 985 thousand HUF, while the

can be taken as the average effect of introducing the policy on all mortgage borrowers. From a policy perspective it is relevant as it indicates the total additional spending due to the policy. We also ran these estimations without any control variables and the results are almost the same and statistically not different.

High figures suggest that almost the total amount of extra liquidity was spent due to the program. There are a few possible explanations for the high result. First of all, the control group consists of those borrowers who could not participate in the program. The total impact (almost full monthly instalment is spent) is the difference of how much eligible borrowers spent more and non-eligible borrowers spent less. It could have happened that non-eligibles cut back their spending even more than a monthly instalment and then eligibles cut back only a little. And there are also some other potential explanations. Real estate related investments could also inflate estimated figures. Namely, if borrowers decided to bring forward some investments related to the new property (renovation, buying new furnitures, etc.) due to the extra amount of money they have received through the program that could result in higher spending than the extra liquidity. One potential driver of this could be the special home renovations program that was also available during the time of the program.

Loan performance

Although the Covid-19 shock fundamentally eroded underlying loan performance, the introduction of debt payment moratorium shielded eligible loans from deteriorating into non-performing status. We find that debt payment moratorium strongly decreased the share of non-performing loans among newly disbursed housing loans: it decreased 30+ days delinquency rate by 4 percent and 90+ days delinquency rate by 2 percent¹⁶.

There is remarkable heterogeneity behind this result with respect to product-borrower

average monthly instalment is 108 thousand HUF (see Table 3.1). 10 percent of the monthly inflow is 98 thousand HUF that accounts for 91% of the monthly instalment.

¹⁶We take into account the time frame from March 2020 to October 2021 and consider a loan non-performing if it was late at least once by 30+ days or by 90 days during this period

segments: we measure no significant impact on affluent borrowers in case of 30+ days delinquency, 5.2 percentage point impact on the mid-market segment and 4.4 percentage point on the mass market segment. Regarding 90+ days of delinquency estimated figures are: no significant impact on affluent borrowers, 1.1 percentage point on the mid-market segment and 1.3 percentage point on the mass market segment. The mid-market and mass market segments' figures are not significantly different from each other in both cases.

Finally, we find opposite results on the long run. Taking into account the months between July 2022 and November 2024 we measure that debt payment moratorium increased 90+ days delinquency rate of eligibles by almost 1.5 percentage points. Although we do not have further results on the long run, this figure suggests that there could be some perverse effect of the program regarding e.g. payment moral or overspending (that could also lead to non-performance).

Table 3.4: Results of RD estimations on 30+ delinquency by product-borrower segments

VARIABLES	(1) Full sample	(2) Mass market	(3) Mid-market	(4) Affluent
Non-eligible	0.0452*** (0.00797)	0.0439*** (0.0150)	0.0518*** (0.0136)	0.0105 (0.00714)
Observations	42542	10395	13709	10063
Bandwidth	20.000	20.000	20.000	20.000

Note: This table presents regression discontinuity (RDD) estimates of the moratorium's causal effect on the 30-day loan delinquency rate across different product-borrower segments between April 2020 and October 2021. The dependent variable is an indicator for 30+ days delinquency. The treatment variable, "Non-eligible", equals 1 for loans originated after the March 18, 2020 cutoff date. To explore heterogeneity in the effects, the sample is divided into distinct product-borrower segments (mass market, mid-market, affluent). These segments are constructed based on predicted loan interest rates—using pre-origination borrower and loan characteristics—which serve as a comprehensive proxy for market segments. Robust standard errors are reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Early repayment of loans and new loan taking

We also run RDD to identify the impact of debt payment moratorium on early repayment of loans and on new loan taking. We do not find any significant impact on these (see Figure 3.19 and Figure 3.20). In case of early repayments there is some sign of impact (see Figure 3.19), however estimated impact is non-significant. Similarly, in case of new

Table 3.5: Results of RD estimations on 90+ delinquency by product-borrower segments

VARIABLES	(1) Full sample	(2) Mass market	(3) Mid-market	(4) Affluent
Non-eligible	0.0169*** (0.00491)	0.0130** (0.00623)	0.0112* (0.00669)	0.00609 (0.00549)
Observations	42542	10395	13709	10063
Bandwidth	20.000	20.000	20.000	20.000

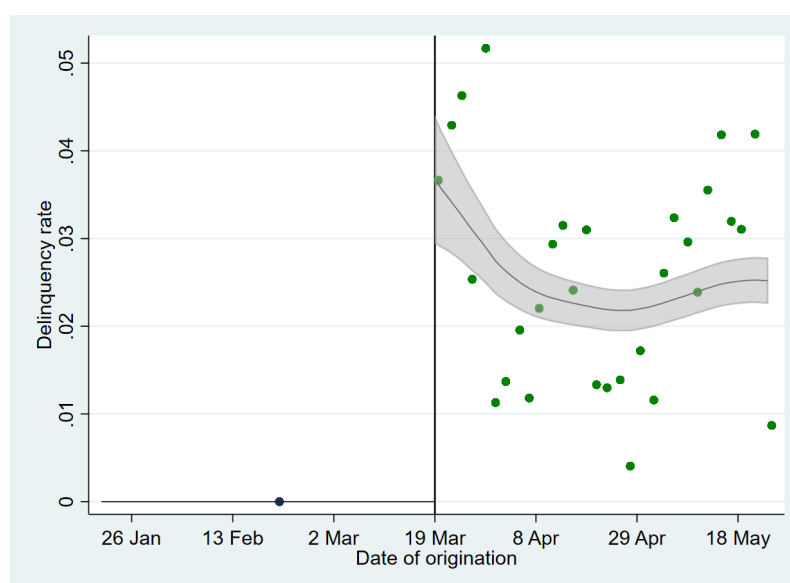
Note: This table presents regression discontinuity (RDD) estimates of the moratorium's causal effect on the 30-day loan delinquency rate across different product-borrower segments between April 2020 and October 2021. The dependent variable is an indicator for 90+ days delinquency. The treatment variable, "Non-eligible", equals 1 for loans originated after the March 18, 2020 cutoff date. To explore heterogeneity in the effects, the sample is divided into distinct product-borrower segments (mass market, mid-market, affluent). These segments are constructed based on predicted loan interest rates—using pre-origination borrower and loan characteristics—which serve as a comprehensive proxy for market segments. Robust standard errors are reported in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 3.6: Results of RD estimations on Long run impact on 30+ and 90+ days delinquency

VARIABLES	(1) 30+	(2) 90+
Non-eligible	0.00247 (0.00977)	-0.0145** (0.00712)
Observations	42542	42542
Bandwidth	17.881	17.211

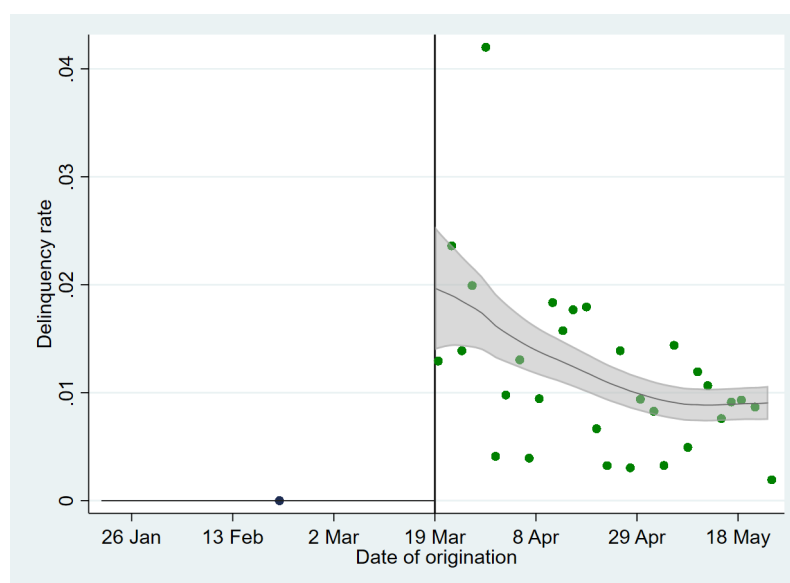
Note: This table presents regression discontinuity (RDD) estimates of the moratorium's causal effect on the 30-day loan delinquency rate between July 2022 and November 2024. The dependent variable is an indicator for 30+ days and 90+ delinquency. The treatment variable, "Non-eligible", equals 1 for loans originated after the March 18, 2020 cutoff date. Robust standard errors are reported in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Figure 3.16: 30+ days delinquency of non-eligibles from April 2020 to October 2021



Note: This figure displays a regression discontinuity plot of the 30-day loan delinquency rate around the eligibility cutoff date, regarding non-performance between April 2020 and October 2021. The x-axis represents the date of loan origination, centered around the March 18, 2020 threshold. The solid lines represent fitted values from local linear regressions, while the scatter points indicate daily averages within the bandwidth. The significant discontinuous jump at the threshold illustrates the causal impact of the moratorium in preventing short-term financial distress among eligible borrowers.

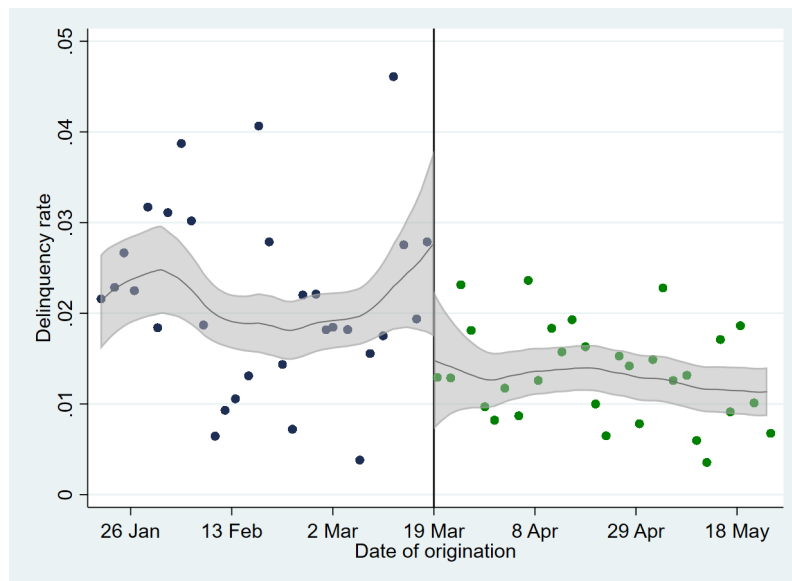
Figure 3.17: 90+ days delinquency of non-eligibles from April 2020 to October 2021



Note: This figure displays a regression discontinuity plot of the 90-day loan delinquency rate around the eligibility cutoff date, regarding non-performance between April 2020 and October 2021. The x-axis represents the date of loan origination, centered around the March 18, 2020 threshold. The solid lines represent fitted values from local linear regressions, while the scatter points indicate daily averages within the bandwidth. The significant discontinuous jump at the threshold illustrates the causal impact of the moratorium in preventing short-term financial distress among eligible borrowers.

loan taking we cannot find significant impact. We show here that in the mass market segment there is a sign of a mild impact of the program on new loan taking (see Figure

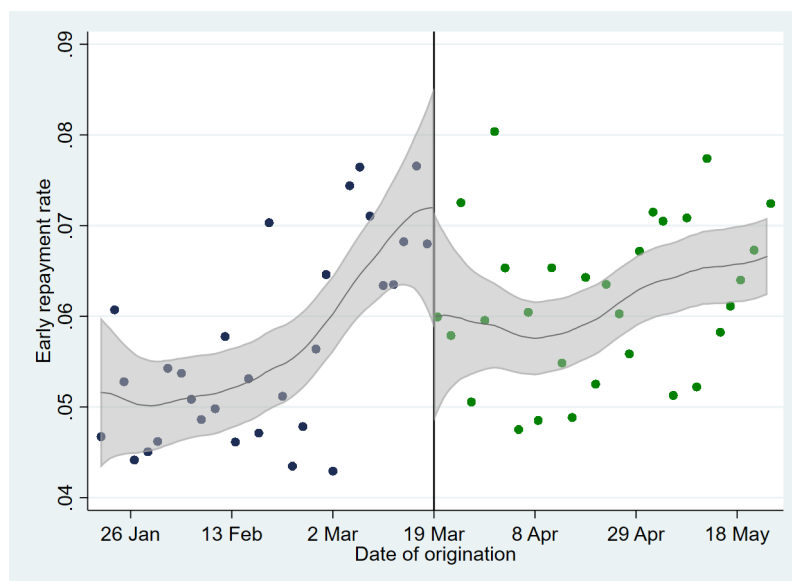
Figure 3.18: 90+ days delinquency of non-eligibles from July 2022 to November 2024



Note: This figure displays a regression discontinuity plot of the 90-day loan delinquency rate around the eligibility cutoff date, regarding non-performance between July 2022 and November 2024. The x-axis represents the date of loan origination, centered around the March 18, 2020 threshold. The solid lines represent fitted values from local linear regressions, while the scatter points indicate daily averages within the bandwidth. The significant discontinuous jump at the threshold illustrates the causal impact of the moratorium in preventing short-term financial distress among eligible borrowers.

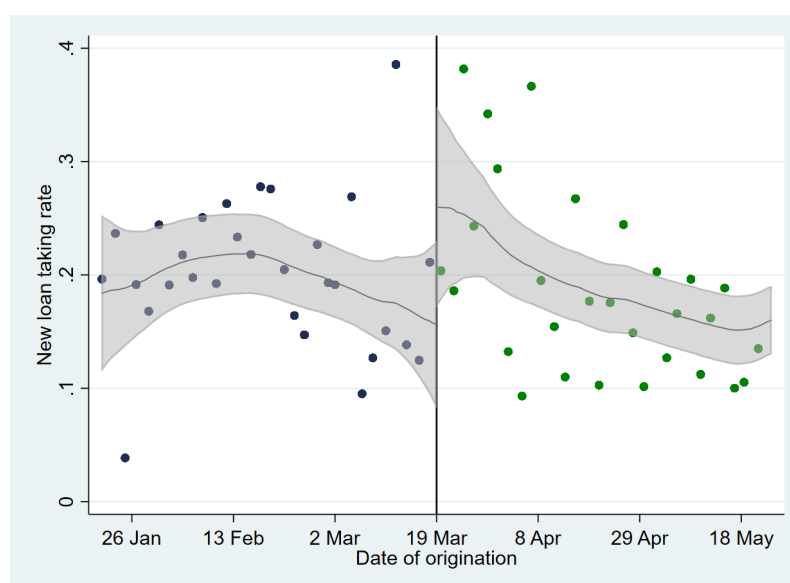
3.20), however it is also non-significant. These results suggests that borrowers did not use the extra funds to repay existing (potentially expensive) loans and the program did not have a strong impact on their loan taking behavior.

Figure 3.19: Early repayment of loans



Note: This figure presents a regression discontinuity (RDD) plot for early loan repayment rates. Early repayment is calculated as the total sum of repayed loan amount divided by the total loan amount of the borrower in the month when the moratorium related housing loan was taken. The x-axis denotes the loan origination date relative to the March 18, 2020 eligibility threshold. The absence of a statistically significant jump at the cutoff suggests that the extra liquidity generated by the moratorium was not systematically used for deleveraging.

Figure 3.20: New loan taking in the mass market segment



Note: This figure presents a regression discontinuity (RDD) plot for new loan origination volumes. New loan taking is calculated as the total sum of newly granted loan amount divided by the total loan amount of the borrower in the month when the moratorium related housing loan was taken. The x-axis denotes the loan origination date relative to the March 18, 2020 cutoff. Similar to the deleveraging results, the lack of a discontinuity at the threshold indicates that the liquidity relief did not induce additional credit-seeking behavior among eligible households.

3.5.2 Cross sectional regressions

In this section we show that the effect of the program is visible even on the macro level. First, we will inspect the correlation between the extra liquidity provided to the households on a district level and the change in regional consumption. OLS estimations are conducted, robust standard errors are estimated to ensure the uniformity of variance. Table 1 presents the results.

Model 1 includes only our main independent variable of extra liquidity. This can be looked at as a naive model. Model 2 includes all our control variables also. We try different functional forms in Model 3 and Model 4. We add quadratic term and use the logarithm of extra liquidity as well. Results suggest that the relationship between extra liquidity and consumption may be nonlinear, as nonlinear terms are also highly significant in Model 3 and Model 4. In all estimations, our variable of main interest, namely extra liquidity and its squared term or the logarithmic form are strongly significant.

In our full models (Model 2 - Model 4), we include several control variables. Although

Table 3.7: Cross sectional regressions

	(1)	(2)	(3)	(4)	(5)
Extra liquidity to OCR turnover	0.377*** (4.08)	0.109** (2.06)	0.448*** (3.05)		-0.0652** (-2.03)
Extra liquidity to OCR turnover ²			-0.00583*** (-2.88)		
Log of extra liquidity to OCR turnover				3.568*** (3.47)	
Log of PIT base, 2019		3.446** (2.31)	3.621** (2.47)	4.280*** (2.91)	
Jobseeker rate, 2019		0.437 (1.16)	0.598 (1.54)	0.587 (1.54)	
Population density in thousand, 2019		0.0333 (0.09)	0.0745 (0.20)	0.108 (0.31)	
Nr of firms in thousand, 2019		-0.485*** (-3.75)	-0.509*** (-4.06)	-0.545*** (-4.37)	0.132*** (3.09)
Share of firms operating in manufacturing, 2017		31.53 (0.71)	31.39 (0.72)	45.10 (1.00)	22.06 (1.46)
Share of firms operating in tourism, 2017		-184.7*** (-3.99)	-183.8*** (-3.96)	-169.7*** (-3.65)	-2.357 (-0.14)
Bank branches per thousand inhabitants, 2019		-13.00*** (-3.41)	-15.49*** (-3.96)	-15.96*** (-4.08)	-2.660 (-0.99)
Education max elementary in %		0.325 (1.58)	0.201 (0.94)	0.196 (0.93)	
Education tertiary ratio in %		-0.300 (-1.25)	-0.358 (-1.50)	-0.358 (-1.53)	
District: Budapest city center		-36.62*** (-4.31)	-35.65*** (-4.31)	-34.07*** (-4.28)	
District: Budapest residential		-11.06** (-2.07)	-10.89** (-2.11)	-9.911** (-2.01)	
District: Includes city with county rights		-7.574*** (-3.78)	-7.246*** (-3.70)	-7.000*** (-3.62)	-0.0606 (-0.09)
Constant	-16.59*** (-10.63)	-90.89** (-2.20)	-92.42** (-2.28)	-97.41** (-2.46)	16.15*** (10.04)
Observations	197	197	197	197	174
R ²	0.070	0.773	0.780	0.786	0.223

t statistics in parentheses

* p<0.10, ** p<0.05, *** p<0.01

Note: This table presents estimation results from district-level cross-sectional regressions. The dependent variable is the annual percentage change in Online Cash Register (OCR) turnover at the district level in 2020 April. The key explanatory variable is the extra liquidity provided by the payment moratorium, measured as a share of aggregate district-level OCR turnover.

some of them are highly correlated (correlation coefficient > 0.4), we leave them in the model as keeping them in or leaving them out does not change the results, but keeping them in may decrease omitted variable bias. In addition to the district specific dummies,

we add population density also. While being insignificant, we leave it in the model to ensure that we are controlling also for the size of the local market. Log of PIT base and jobseeker rate controls for the level of development and the labour market conditions of the given district before the crisis. The more developed a district is, the lower the drop in consumption. At the same time, number of firms controls for business density before the crisis, and suggests that once there are 1000 more firms in a given district, consumption change is lower with 0.5%, all else being equal. The share of firms in tourism controls for exposure to tourism activities, and its coefficient is significantly negative. At the same time, the share of manufacturing firms is positive but insignificant. Bank density (i.e. number of bank branches per thousand inhabitants) is controlled for the probability of loan take-up and also because once banks are present in a district, clients had better chances to collect information on the moratorium and to opt-out from it. Higher bank density implies bigger consumption drop. Finally, education control variables are also included, however, they remain insignificant. The explanatory power of the models remains same once all our controls included and the models explain around 78% of the total variance in the turnover of OCRs. The coefficient of extra liquidity from Model 2 implies that moratorium explains around 2.4 percentage points of the change in turnover of OCRs. Since the drop in consumption measured by OCRs was somewhere around 12.4%, this means, that once moratorium take-up is present, aggregate consumption drops around 20% less than without it.

Finally, we also estimated a similar equation to the above, where the dependent variable is the share of sole proprietors who suspend their businesses in the given month. As mentioned above some of the variables are highly correlated, and in this case leaving them out had an impact on the results. Therefore, we present our result by ruling out those variables that are strongly correlated with each other. This limits the strengths of our findings, presented results are only indicative.

Extra liquidity is strongly significant in this last specification. We think that the program could boost consumption so strongly that resulted in spillover effects from moratorium

to business activity. The magnitude of our variable is large, standard deviation of extra liquidity to OCR explains almost 24% of standard deviation of share of suspenders keeping other things constant. Nevertheless, we look at this result as a weak sign of spillover effects as many endogeneity problems arise regarding this specification. In the next section by estimating a panel model we rule out some part of the endogeneity problems. We believe that it is worth expanding our research into this direction in the future as finding such spillover effects is potentially interesting for both academics and policy makers.

3.5.3 Panel models

Adding time dimension to the analysis above helps to reduce selection bias. Therefore, one can expect that the coefficient of extra liquidity increases. Results are consistent with this expectation. The first column shows results of the fixed effect specification while the second column shows the results of the first difference specification. In case of both models the coefficient of interest is close to 20%, that is the double of the cross-sectional result. In all our models we include time fixed effects, to control for the change in the macroeconomic environment.

The dependent variable at Model 3 and 4 is the share of sole proprietors who suspended their businesses. Similarly to the cross sectional regression, extra liquidity is negatively related to the share of suspenders. However, the magnitude is much smaller, remains less than -2% at both models. We suspect that due to omitted variable bias the coefficient in the cross-sectional regression is overestimated, and panel regression results are closer to the true value. Nevertheless, the size of the coefficient is still high, standard deviation of extra liquidity if the time dimension is considered as well explains almost 11% of the standard deviation of share of suspenders.

Point estimates of OLS and panel regressions are considerably different than point estimates of RDD¹⁷. To understand what drives that difference it is important to underline

¹⁷ As the confidence interval around RDD estimates is large panel and RDD results are not significantly different from each other.

Table 3.8: Regressions on panel data

	(1)	(2)	(3)	(4)
Extra liquidity to OCR	0.183*** (6.38)		-0.0166*** (-4.25)	
D.Extra liquidity to OCR		0.205*** (6.83)		-0.0194*** (-5.25)
Constant	9.965*** (28.89)	-20.16*** (-21.92)	11.69*** (240.95)	5.319*** (65.91)
Time fixed effect	YES	YES	YES	YES
Observations	788	591	696	522
R^2	0.736	0.700	0.924	0.932

t statistics in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Note: This table presents estimation results from district-level panel regressions. The dependent variable is the annual percentage change in Online Cash Register (OCR) turnover at the district level. The key explanatory variable is the extra liquidity provided by the payment moratorium, measured as a share of aggregate district-level OCR turnover. All panel specifications control for time (quarterly) and district fixed effects to absorb unobserved spatial and temporal heterogeneity

that RDD rules out selection bias, while OLS and panel results do not. Moreover, control group of the RDD is different than the control group in the OLS and panel regressions. In case of RDD the consumption of eligibles is compared with the consumption of non-eligibles. In case of OLS and panel regressions as extra liquidity coming from moratorium varies through time in districts, consumption (proxied by OCR turnover) of a given district in a given period compared to both other districts' consumption and to the consumption in the same district in a different time period.

3.6 Conclusion

In this research we analyzed the relationship between debt moratorium program and consumption in Hungary. Having individual level spending data for the group of borrowers of one large bank we identify the impact of moratorium on spending in a regression discontinuity design. We find evidence that debt payment moratorium increased the spending of borrowers by around 90% of the monthly instalment, i.e. borrowers consumed almost the whole extra liquidity that they got. We find that the consumption effect of debt mora-

torium is observable even on the macro level, though our estimations on the macro level are prone to selection bias and likely to be underestimated. Furthermore, we also measure significant effect of the program on loan-performance: our results suggest that almost 2 per cent of loans would have become non-performing until 2021 October without the program. We also measure some long lasting effects: the program led to a 1.5 percentage point higher delinquency rate among eligibles. We test if the program had any impact on early repayments of loans or new loan taking, however we do not find significant results, which suggests that there were no such effects. Our results suggest that this policy has strong aggregate consumption effects and also relevant in mitigating creditworthiness problems during crisis times.

In conclusion, our findings offer a broader perspective on the role of payment moratoria within the crisis management toolkit. While traditional monetary and fiscal interventions may face transmission lags or targeting frictions during sudden macroeconomic shocks, our results indicate that a broad-based debt moratorium can function as an immediate liquidity transfer mechanism. By relaxing binding liquidity constraints at the household level, the policy not only mitigates short-term financial distress and curbs rising loan defaults but also supports aggregate demand. For future crisis management, policymakers might consider such interventions not exclusively as macroprudential tools to shield bank balance sheets, but also as complementary measures to stabilize the real economy and sustain consumption when traditional monetary transmission channels are impaired.

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3.A Appendix

3.A.1 The role of a loan repayment moratorium

Loan repayment moratorium have become a widely used crisis management tool during the COVID-19 crisis Gaffney & Greaney (2020). Payment moratorium is believed to support both the real economy and the financial stability, assuming a shock is only transitory, and do not effect the long-term credit-worthiness of the borrowers. As in a moratorium the repayment obligations are automatically suspended for a fixed period agreed in advance, it is a powerful tool for mitigating the short-term default risk of potentially vulnerable debtors. Borrowers can overcome loan payment problems due to temporary decrease of income until the shock runs out. The maturity of the loan gets extended in relation to the length of participation in the program. Distressed debtors can cover living expenses easier, as the moratorium is practically providing extra liquidity to the participants. The role and effectiveness of a payment moratorium is less straightforward than of a simple fiscal transfer. On the one hand, debtors can basically borrow time and money until uncertainty is decreased and enough liquidity is accumulated for further repayments. If precisely conducted, it can even increase social trust in the financial system. On the contrary, moral hazard among participants can easily lead to increased default risk. Additionally, an opt-out moratorium can bring further free-riders increasing the cost and decreasing the effectiveness of targeting vulnerable debtors. Moreover, financially less educated customers and non-careful customers might simply remain in the moratorium.

3.A.2 Drivers of participation in the moratorium in Hungary

Next, we discuss the key findings of two studies about the participation in the Hungarian debt moratorium. Although our research does not focus on the motives of participation, we think that these are relevant to conceptually understand how the program affected

consumption.

Participation in the Hungarian moratorium is well documented by Drabancz et al. (2021). There is considerable variation in participation by loan type and also in debtor characteristics. Importantly, there is no huge difference in participating loans with regards to distribution by volume and distribution by the number of contracts. In case of housing loans only around 50 % of the volume of loans is under moratorium, while this ratio is averaging around 72-75 % for government subsidized prenatal loans and personal consumer loans. They find, that the moratorium participation was generally higher among more vulnerable clients: younger people under 35, people living in smaller settlements, and people in less developed areas. Interestingly, there is no variation with respect to the number of children. Based on an individual data provision by commercial banks, they establish that people with monthly net wages below 200,000 HUF and people with debt-to-income ratio above 1 year participate at a greater extent. They find that the additional liquidity was beneficial for the economy, and had only moderate costs for the financial institutions. However, they do not link moratorium participation to any other data to verify the link with consumption change or with aggregate demand.

According to an individual survey of the Central Bank of Hungary, Dancsik and Fellner (2021) investigate the drivers of the participation of 36,000 respondents. They underline previous results, and additionally find that people with lower educational levels, jobseekers, part-time workers, credit constrained households, and people with insufficient savings are more likely to participate in the moratorium. They find great variability by the sector of employment activity of the respondent. Consistently with the uneven effect of the shock across occupations, arts, entertainment, leisure, accommodation and other services professionals tend to be more likely to participate in the moratorium. At the same time, construction, IT, administrative, defense, health, social, finance and professional service workers are less likely to participate.

3.A.3 Constructing product-borrower segments

We define loan market segments (e.g., mass vs. affluent) using a product-borrower indicator, pragmatically grouping loans with similar predicted interest rates. Assuming predicted rates act as a sufficient statistic for market identification, we model these rates based on observable proxies for client risk such as income, age, location, and loan characteristics like LTV and maturity.

We model the interest rate of loans based on these observed variables. The model that we estimate is:

$$Y_i = \alpha + \beta C_i + \delta L_i + \gamma B_i + \mu H_i + \omega B_i \times H_i + \varepsilon_i \quad (3.10)$$

where Y_i is the annual percentage rate of loan i , C_i is a vector of client related variables, L_i is a vector of loan related variables, B_i vector of indicator variables marking the granting bank, H_i is the Herfindahl Hirschmann index¹⁸, $B_i \times H_i$ stands for the interaction of these variables and ε_i is a disturbance term. We estimate the regression on loans disbursed between January 1 2020 and June 30 2020.

Results of this regression are below. The model gives an acceptable fit¹⁹, and can be used to try to separate borrowers that are likely to get low interest rate offers from bank based on their observable characteristics from those who would rather contract on high interest rate. We expect the model to give a ranking of predicted interest rates that is similar to the ranking of observed interest rates. Having the predicted interest rates we split the sample into deciles. The product-borrower segment indicator is calculated the following way:

¹⁸We calculate the index using the observed market share of banks district-by-district in all quarters separately and using the following formula:

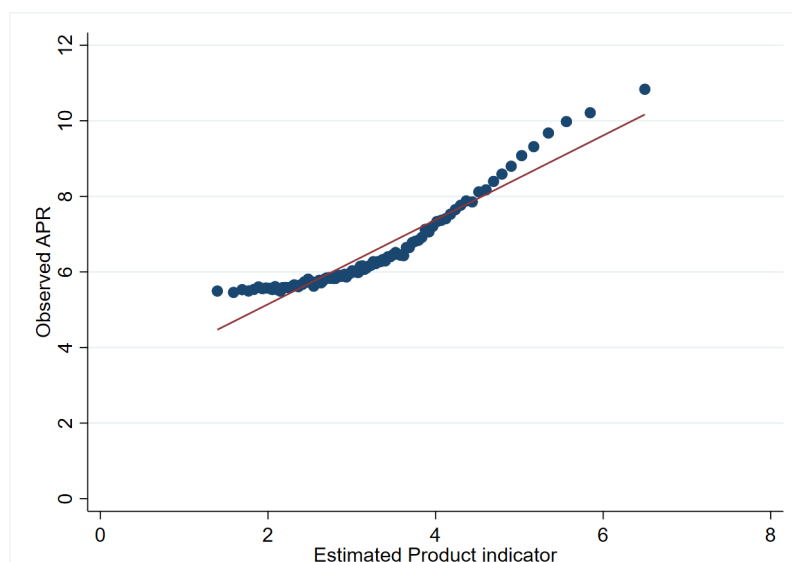
$$H_i = \sum_{b=1}^{10} bankshare_{d,t,b}^2 \quad (3.11)$$

where d stands for district, t for time, b for bank and bank share is the sum of loan amounts in a given district at a given time for a given bank. These variables are unique for all loan i .

¹⁹adjusted R^2 of around 56 %

$$\hat{Y}_i = \hat{\alpha} + \hat{\beta}C_i + \hat{\delta}L_i \quad (3.12)$$

Figure A1: Binscatter of mortgage loan interest rate and the estimated product-borrower segment indicator



Note: This table presents the OLS regression results used to construct the product-borrower segments. The dependent variable is the observed loan interest rate, which is modeled using pre-origination borrower characteristics (e.g., income, age, location) and loan characteristics (e.g. LTV and maturity) and market concentration proxy (Herfindahl-Hirschman index) and product and bank fixed effects. Based on coefficients on borrower and loan characteristics a score created for every loan. These scores are then used as a single indicator to group loans into homogeneous market segments (e.g., mass vs. affluent).

3.A.4 Control variables used for cross-sectional regression

We will use several regional district-level macroeconomic and social variables from the Land Information System (in Hungarian: TeIR - Területi Információs Rendszer) and from the System of Regional Statistics (in Hungarian: T-Star) provided by the Lechner Knowledge Center and the HCSO as control variables. Once a variable is available at a lower regional level, we aggregate accordingly to have district-level control variables. Table A1 below summarizes the variables used.

The log of personal income tax base is the logarithm of the aggregate district level personal income tax base and is deemed to measure the level of economic development in a given district in 2019. The distribution of the taxpayers and tax base within a given dis-

Table A1: Product-borrower segment indicator: A model of observed interest rates

	(1) Interest rate
Wage (HUF thd)	0.000813*** (0.000104)
Wage (HUF thd) squared	-0.000000188*** (4.62e-08)
Age	-0.00922** (0.00414)
Age squared	0.0000914** (0.0000466)
PTI	0.0383*** (0.00161)
Payment to income: current loan > 50%	-0.00798*** (0.00175)
Payment to income: other loans	0.00490*** (0.000704)
Payment to income: other loans > 50%	0.00117 (0.00288)
Loan to value	-0.000625* (0.000357)
Maturity (years)	0.0344*** (0.00183)
Loan amount (HUF mln)	-0.952*** (0.0234)
HHI	0 (.)
Constant	4.863*** (0.118)
Observations	11973
R^2	0.559
Repricing period FE	Yes
District FE	Yes
Loan type FE	Yes
Bank FE	Yes
HHI x Bank FE	Yes

Standard errors clustered on district level in parentheses

* p<0.10, ** p<0.05, *** p<0.01

trict is not used as it is strongly correlated with the level of development itself. Jobseeker rate in 2019 is utilized to control for pre-pandemic labor market situation. The number of firms captures the extension of the business sector. Firm density would make a more natural variable to use, however, it is strongly correlated with the log of PIT base. The share of firms operating in manufacturing and tourism in 2017 are utilized to control for the economic structure of a district, and consequently to its exposure to the COVID-19 shock. To control for the educational status of a district, we utilize data from 2016 to calculate a variable low education rate as the share of population not having completed more than elementary education of maximum 8 classes. Similarly, we calculate high education rate as the ratio of population with at least a university diploma. Additionally, we will use control variables for local financial depth and finance availability by using bank branch data. We use bank density calculated as the number of bank branches for 1000 inhabitants. It is important to control for availability of in-person financial services, as it could be an important driver in acquiring information about the moratorium and potentially opting out from the program. As banks are strongly concentrated in towns and cities, we shall note, that this variable captures partially the urbanization level of the area too. However, we will also use population density to capture the urbanization level of a given district.

3.A.5 Individual level observations: Instrumental variable approach

We also estimate the impact of moratorium participation on consumption in an instrumental variable approach. Here, we rely on the fact that borrowers had different incentives to participate in the program due to terms of their loans. We argue that the *remaining maturity* of a loan is such a characteristic that affects consumption only through debt moratorium participation (assuming that we can control all other channels).

We use a different dataset for this analysis than for the RDD approach. Lastly, there are numerous accounts with extreme high monthly spending. We think that these are business related accounts of the borrowers, therefore the transactions of these are not representative

Table A2: Description of variables

	Mean	Std Dev	Min	Max	N
<i>Dependent variable</i>					
OCR turnover, annual % change, April 2020	-12.39	14.85	-85.98	10.17	197
OCR turnover, annual % change, May 2020	-3.31	13.74	-76.90	19.23	197
OCR turnover, annual % change, June 2020	-2.38	9.50	-56.73	19.16	197
OCR turnover, annual % change, %, Q2 2020	-6.01	12.12	-73.01	13.13	197
OCR turnover, durables, annual % change, April 2020	20.16	70.98	-91.80	398.01	197
OCR turnover, nondurables, annual % change, April 2020	-11.06	13.51	-82.21	7.87	197
OCR turnover, services, annual % change, April 2020	-56.22	26.93	-95.33	104.97	197
<i>Main independent variable</i>					
Extra liquidity to OCR turnover	23.73	19.93	2.06	121.83	197
Extra liquidity to OCR turnover SQ	958.33	1,986.50	4.26	14,842.87	197
Extra liquidity to local PIT base	1.18	0.84	0.41	6.41	197
Extra liquidity to local PIT base SQ	2.09	4.09	0.17	41.15	197
Extra liquidity to mean wage per taxpayer	1.18	0.84	0.41	6.41	197
Extra liquidity to mean wage per taxpayer SQ	2.09	4.09	0.17	41.15	197
Extra liquidity to mean wage per population	1.18	0.84	0.41	6.41	197
Extra liquidity to mean wage per population SQ	2.09	4.09	0.17	41.15	197
% of loans in moratorium by value	0.78	0.05	0.66	0.88	197
% of loans in moratorium by value SQ	0.61	0.07	0.44	0.78	197
% of loans in moratorium by number	0.77	0.05	0.65	0.86	197
% of loans in moratorium by number SQ	0.59	0.07	0.42	0.74	197
<i>Control variables</i>					
Population density in thousand, 2019	0.78	2.64	0.03	24.61	197
Jobseeker rate, 2019	3.06	2.08	0.45	9.20	197
Log of PIT base, 2019	24.73	0.82	23.09	26.67	197
Number of firms in thousand, 2019	9.08	8.16	0.97	41.06	197
Bank density for thousand population, 2019	0.22	0.12	0.06	1.54	197
Education max elementary ratio in %, 2016	34.44	8.45	12.10	52.89	197
Education tertiary ratio in %, 2016	13.84	9.07	4.32	50.90	197
% of firms operating in manufacturing, 2017	0.07	0.02	0.03	0.11	197
% of firms operating in tourism, 2017	0.05	0.02	0.02	0.12	197
<i>District fixed effects</i>					
Budapest city center	0.03	0.16	0.00	1.00	197
Budapest residential	0.09	0.29	0.00	1.00	197
Includes city with county rights	0.12	0.32	0.00	1.00	197
Countryside	0.77	0.42	0.00	1.00	197

to the household sector. We try to eliminate these accounts by applying several filtering rules. The main idea behind the rules that we apply is to keep only those accounts in the sample that are used to cover the monthly expenses. Therefore, we want to rule out those accounts that show too low or too high spending compared to the monthly wage of the borrower. We also drop those borrowers from our sample whose monthly instalment is much larger than their monthly spending. They probably use other accounts (potentially at other banks) to cover their monthly expenses. We also rule out those observations which are exceptionally high compared to the median spending of the household. Therefore, we think that our results are mainly valid considering non-durable consumption. Lastly, we

drop borrowers with exceptionally high monthly instalment or exceptionally high wage. We think that there is high chance that these borrowers have other accounts as well that we cannot observe, therefore their inclusion would distort our analysis. Table A3 summarizes the rules that we follow in our filtering process.²⁰

Table A3: Included observations

	Spending to wage	Instalment to median spending	Spending to median spending	Spending	Wage	Instalment
Min	0.5	0	0	0	0	0
Max	3	3	3	5000,000	2000,000	500,000

We increase the scope of our analysis then by implementing an instrumental variable approach. An advantage of this approach compared to the regression discontinuity is that here we do not rely only on borrowers, who applied for a mortgage loan in early 2020, but we can include all the borrowers with existing loans during 2020. To avoid any problems related to our observation that borrowers tend to increase their spending in the first year after taking a loan we include only those borrowers who took their loan at least one year before debt payment moratorium was announced. Our sample starts at 2016.

To identify the causal impact of moratorium participation on individual spending we instrument moratorium participation by the *remaining maturity* of the loan. Ideally, we want to compare borrowers who are similar in all their characteristics except the remaining maturity of their loan. We argue that the borrower whose remaining maturity is longer is more likely to participate in the moratorium compared to a similar borrower with lower remaining maturity. One reason for this is that the extra liquidity should be payed back later, therefore it can be used for a longer period. It should be noted that from financial perspective the farther away in the future the payback time is the higher the transfer from the bank to the borrower if the discount rates are the same. Considering behavior aspects such as hyperbolic discounting strengthens this argument further.

Our instrument is only valid if it is related to spending only through moratorium participation. To shut down all the channels other than moratorium participation we control on the

²⁰Although we did some sanity checks to these rules and found no strong influence of small changes of the rules on our results, a thorough analysis of robustness checks should be added in the future version of this paper.

terms of the loan (original maturity, type of the loan, monthly instalment) and on several personal characteristics (age, wage, schooling, marital status, number of children).

We estimate the following regression, where $\hat{X}_i$ represents that variable X_i is instrumented.

$$Y_i = \alpha + \beta_1 \hat{X}_i + \gamma Controls_i + \epsilon_i \quad (3.13)$$

$$Y_i = Spending_i \quad (3.14)$$

$$X_i = Moratorium_i \quad (3.15)$$

One threat to the validity of this instrument is that consumers may smooth their consumption, therefore remaining maturity can be directly related to consumption (not only through moratorium participation). We think that this argument only holds for short remaining maturities, in case of long maturities consumption smoothing can hardly affect current spending. For instance, in case of a loan that matures in one or two years, borrowers may start to spend a little more as they get closer to maturity. However, in case of a loan that matures in five or ten years time we do not expect that borrowers would adjust their spending. As in our sample the remaining maturity of loans vary between five and twenty-five years we argue that consumption smoothing has no effect on current spending.²¹

IV RESULTS

As a first step we show that the our proposed instrument is indeed relevant. We estimate two specificaions. In the first model (column 1) the dependent variable is the moratorium

²¹Nevertheless, we think that in our future research a more formal analysis or references to existing studies would improve our argument.

participation dummy. In the second model (column 2) the dependent variable is the extra liquidity divided by the median monthly spending of the borrower during 2019. The first stage results show that remaining maturity is strongly significant in both models and the F statistic of the regression is high as well (around 3500 and 300 respectively). In our estimation we observe one borrower in at most 9 months (from 2020 April to 2020 December). We observe 591 borrowers.

Table A4: Bank account spending

	(1)	(2)
	In moratorium	Extra liquidity to median spending
Remaining maturity	0.00290*** (34.85)	0.000688*** (15.93)
Instalment	0.000000232* (1.95)	0.00000194*** (13.00)
Interest rate	9.745*** (22.92)	1.732*** (7.65)
Maturity	0.0000760 (0.46)	-0.000300*** (-3.67)
Wage	3.89e-08 (1.44)	-0.000000298*** (-14.89)
Age	-0.00625*** (-3.11)	-0.00273*** (-2.86)
Age ²	0.0000708*** (2.61)	0.0000515*** (3.75)
Time FE	YES	YES
Product type FE	YES	YES
Schooling FE	YES	YES
Marital status FE	YES	YES
Nr of children FE	YES	YES
Observations	4441	4441
R ²	0.860	0.562
F	3493.1	302.5

t statistics in parentheses

* p<0.10, ** p<0.05, *** p<0.01

It is also important to understand whether the instrument is large enough to bring considerable amount of exogenous variation into the decisions of clients. Multiplying the coefficient by the standard deviation of remaining maturity ($0.0029 \times 72 = 0.21$) informs about that how large is the variation in moratorium participation due to remaining maturity. This remaining maturity driven variation accounts for 45% of the standard deviation of moratorium participation (0.45). This figure is considerably smaller in case of the second specification - around 10%. Therefore, our results are only valid for a subset of clients. These clients - based on their characteristics other than remaining maturity - are close to be indifferent regarding their moratorium participation decision. Consequently, remaining maturity of their loan affects their participation decision: they participate only if their loan's remaining maturity is long enough, otherwise they opt-out.

Reasoning is very similar in case of the second regression (second column). The only difference is that the dependent variable is the extra liquidity divided by the borrower's median monthly spending of 2019. Obviously, if a borrower does not participate in the program, then his extra liquidity is zero, therefore the dependent variable is zero as well. The magnitude of the explanatory variable is similarly high in this case as well (both the dependent variable and the coefficient of the independent variable are around the fifth of the previous models' corresponding figures).

Control variables are again just help us to increase precision and keep our instrument valid. Coefficient on instalment is positive that means that borrowers with high instalment were more likely to participate than borrowers with smaller instalment. Size of interest rate is significantly positively related to moratorium participation and to extra liquidity to median spending. Original maturity is only significant in the second specification. The magnitude is economically relevant: one standard deviation increase in maturity leads to 2 percentage point smaller rate of extra liquidity to median spending *ceteris paribus* ²². Coefficient of wage is significantly negative: one standard deviation increase of wage coincides with 7.5 percentage point smaller rate of extra liquidity to me-

²²The standard deviation of extra liquidity to median spending is 32% in the sample

dian spending (*ceteris paribus*). Moratorium participation and extra liquidity to median spending is significantly negatively related to age, however this relationship weakens as age increases.

Table A5: Descriptive statistics

Moratorium	Spending	Remaining maturity	Instalment	Interest rate	Wage	Age	Nr of children	Schooling
OUT	486058	94	72928	.041	408840	41.8	.87	2.7
IN	527475	169	72698	.048	385522	40.8	.90	2.7
Total	519184	154	72744	.047	390190	41.0	.89	2.7

Now we turn to the results of the IV estimations.²³ The first two columns show regression results when the dependent variable is the money spent by the borrowers and the key explanatory variable is the moratorium dummy. The first column summarizes the results of a straight OLS estimation (serving as a benchmark), while the second column shows the results of the IV estimations. According to the OLS results those who participated the moratorium tended to spent around HUF 60.000 more than non-participants. This sum accounts for 83% of the monthly instalment. The IV estimation is double large as the OLS one (more than HUF 121.000), which is surprisingly high. We do not expect that borrowers spend more than the transfer itself. An explanation of these high numbers can be that the uncertainty around the results is large (large standard errors), therefore there is high chance that the true impact is not as large as the point estimate.

The third and fourth column contains an OLS and IV regression results, however the dependent variable is the ratio of the current rate and the median monthly spending of the borrower in 2019. The key explanatory variable is the monthly instalment divided by median monthly spending of the borrower in 2019. As the denominator of the dependent and independent variables are the same, the interpretation of the results is straightforward. The estimated coefficient of 0.5 in case of the OLS model can be interpreted as 50% of the extra liquidity was spent by the borrowers. The result of the instrumental variable approach is 1.03 suggesting that the whole extra liquidity was spent by the borrowers.

Similarly to the previous approach we again use a set of control variables including several

²³ Similarly to the regression discontinuity estimations, we clustered standard errors on the borrower level.

dummy variables/fixed effects. We focus now only on the variables that are significantly different from zero. Original maturity is significantly positive in the first two specifications and their magnitude is large: one standard deviation increase in the original maturity is related to HUF 49.000 to HUF 58.000 higher spending keeping other things equal. The coefficient of wage in the first two specifications is somewhat larger but similar in magnitude than in the regression discontinuity design. Age and age squared variables are strongly significant only in the third and fourth specifications: higher age is related to higher spending or spending rate, though this relationship weakens as age increases.

Table A6: Bank account spending

	(OLS 1) Spending	(IV 1) Spending	(OLS 2) Spending rate	(IV 2) Spending rate
In moratorium	60347.9** (2.16)	121173.6* (1.96)		
Extra liquidity to median spending			0.505*** (2.64)	1.028*** (2.87)
Instalment	0.0679 (0.18)	0.0711 (0.19)	8.68e-08 (0.26)	-0.000000893 (-0.94)
Interest rate	-159866.7 (-0.16)	-626607.4 (-0.56)	1.607 (1.23)	0.959 (0.71)
Maturity	833.8** (2.11)	706.8* (1.73)	-0.000397 (-0.98)	-0.000490 (-1.15)
Wage	0.879*** (8.19)	0.879*** (8.20)	-0.000000216*** (-2.79)	-5.61e-08 (-0.38)
Age	5566.5 (1.47)	4097.3 (1.05)	0.0468*** (9.58)	0.0444*** (9.31)
Age ²	-87.38* (-1.66)	-68.39 (-1.25)	-0.000549*** (-7.53)	-0.000528*** (-7.44)
Time FE	YES	YES	YES	YES
Product type FE	YES	YES	YES	YES
Schooling FE	YES	YES	YES	YES
Marital status FE	YES	YES	YES	YES
Nr of children FE	YES	YES	YES	YES
Observations	4441	4441	4441	4441
R ²	0.727		0.850	

t statistics in parentheses

* p<0.10, ** p<0.05, *** p<0.01

There are two takeaways of the results above. First, OLS results are likely to be biased as the IV point estimates are double as large as the OLS ones. We suspect that borrowers who want to accumulate liquidity are more likely to participate in the program and also more likely to cut back spending. If we want to understand what was the causal effect of the moratorium on spending then this endogeneity should be ruled out. Second, although

our estimates are noisy, results suggest that around the whole extra liquidity was spent by borrowers. The causal interpretation is that without the moratorium borrowers would have spent less by their total monthly instalment. Nevertheless, this result holds only for those whose participation decision was potentially affected by the remaining maturity of their loan.