Mortgage Moratoria as Fiscal-free Stimulus: Evidence from Belgium[1]

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Abstract

How can governments stimulate demand and reduce inequality during crises without direct fiscal transfers? We study Belgium's 2020 mortgage moratorium using proprietary account-level transaction and credit data and a discontinuous eligibility rule based on pre-crisis delinquency and liquid-asset thresholds. Eligible households cut mortgage payments and increase non-durable and services consumption, with the strongest gains at the bottom of the distribution. The policy compresses consumption inequality and, in the administrative income records, reduces measured income inequality during the moratorium window. We develop a life-cycle heterogeneous-agent model with long-term, defaultable mortgages. Disciplined by pre-crisis balance-sheet moments and the average spending response, the model delivers the correct sign and conservative magnitude and reproduces, out of sample, the observed heterogeneity and consumption-inequality dynamics. In general equilibrium, moratoria stabilize house prices and bank net worth, amplifying consumption gains without fiscal outlays; however, broad extensions can backfire by weakening bank balance sheets, contracting credit, and ultimately reducing aggregate demand.

Keywords: Mortgage moratoria, debt relief, inequality, heterogeneous agent models
JEL Codes: E62, D31, E21, G51

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1 Introduction

A central challenge of macroeconomic policy is to stimulate aggregate demand during crises while maintaining financial stability, avoiding increases in inequality, and doing so without relying on public spending. Debt moratoria—temporary suspensions of periodic debt payments, often referred to as payment holidays—offer a rare combination of immediacy, simplicity, and fiscal neutrality. By pausing payments without forgiving debt, they provide households with liquidity relief at no direct cost to the public purse. For example, under a typical mortgage moratorium, monthly payments are suspended for six months, and the loan's maturity is extended accordingly, with interest continuing to accrue and the present value of the obligation unchanged.

While the idea of debt moratoria has deep historical roots, their widespread use was rare until the COVID-19 pandemic. One of the earliest known references to debt relief appears in the Quran (2:280), underscoring its longstanding recognition as a means of financial protection. In the modern era, moratoria were implemented only sporadically—for instance, to support farmers during 19th-century droughts (Alston, 1984). The COVID-19 crisis, however, marked a turning point: over 70 countries adopted debt moratoria, primarily to stabilize financial markets and support household balance sheets, particularly among mortgage borrowers. Moratoria are simple to implement, fiscally neutral, and unlike refinancing, do not adversely affect borrowers' credit scores. These features make moratoria a particularly attractive instrument for policymakers seeking to cushion aggregate shocks without deploying public funds. These questions are particularly salient during crises when fiscal space is constrained, administrative capacity is strained, or monetary policy is near the zero lower bound. As governments search for cost-effective stabilization tools, understanding whether liquidity can be reallocated without public transfers becomes critical. We study mortgage moratoria as a policy that delivers such reallocation—by postponing payments rather than forgiving them—and examine their effects on household behavior, inequality, and macroeconomic stability. Our Belgian setting uniquely combines transaction-level consumption measurement with distributional outcomes and a structural model disciplined by aggregate moments and the average spending response to payment suspensions, and validated out of sample on heterogeneity and consumption-inequality dynamics.

We make two main contributions—one empirical, the other theoretical. On the empirical side, we leverage proprietary transaction and credit data from BNP Paribas Fortis, Belgium's largest bank. Our identification strategy exploits a sharp eligibility rule based on delinquency status. The official policy required borrowers to have no overdue payments

as of February 1, 2020. Because our data record delinquency only at month-end, we proxy this rule using February 29, 2020, and classify as *eligible* those with fewer than 30 days past due and as *non-eligible* those already delinquent at the end of January. This definition generates a quasi-natural experiment around a clearly defined threshold: two groups of financially stressed households with similar observable characteristics, distinguished only by their eligibility for the moratorium. Since no debt suspension policies existed prior to March 2020 and assignment is predetermined relative to the March 22 announcement, anticipation effects are unlikely. To estimate the policy's impact, we employ an event-study Difference-in-Differences design that traces its causal effects on spending and inequality among distressed borrowers.

We find that eligibility for the moratorium led to a significant reduction in mortgage payments. During the policy period, eligible households paid on average €232 less per quarter than non-eligible households. This liquidity relief translated into a substantial and persistent increase in consumption: eligible households spent an additional €297 over the second, third, and fourth quarters of 2020, with effects persisting into 2021. We further show that the consumption response was heterogeneous. Young households, those with moderate savings, and with high loan-to-value (LTV) ratios benefited most. Additionally, we provide the first empirical evidence on the distributional effects of debt moratoria. Specifically, we show that consumption and income inequality increased less among eligible households relative to non-eligible households following the policy's implementation. This decline is driven by reduced dispersion within the eligible group, highlighting the potential of moratoria to mitigate rising disparities during economic crises.

While our empirical strategy provides a credible basis for inference, it identifies local average effects for households near the eligibility thresholds and is therefore limited in external validity. Moreover, the estimates are partial equilibrium by construction: they compare households just above and below the cutoff and do not incorporate equilibrium feedback through house prices, wages, or credit supply. These limitations raise natural questions about general equilibrium amplification, welfare, and how the effects differ across the population.

To address these questions, our second contribution is a dynamic general equilibrium model with heterogeneous agents, long-term mortgage contracts, and housing. The economy features households, banks, rental firms, and a government that provides retirement benefits financed by payroll taxes. Banks issue long-term, defaultable mortgages; households face uninsurable income and employment risk, liquidity constraints, and housing

adjustment costs. We calibrate the model to Belgian micro and macro moments and model the COVID-19 recession as a temporary aggregate productivity shock.

A key challenge is connecting the model to the micro evidence. We discipline the model using aggregate moments and the average spending response to payment suspensions (in a partial-equilibrium counterfactual that holds prices fixed, mirroring the empirical design). Importantly, we then validate the model out of sample using untargeted cross-sectional and distributional patterns: the model reproduces the empirical ranking of consumption responses across balance-sheet characteristics (e.g., leverage, age, and liquid wealth) and generates a decline in *consumption* dispersion among eligible households similar to that in the data. Since income risk is exogenous in the model, we interpret the model's distributional implications as pertaining to consumption inequality, while income-inequality dynamics are documented empirically.

We use the model to assess the macroeconomic and distributional consequences of moratoria and to evaluate counterfactual designs. In general equilibrium, a targeted moratorium mitigates the adverse effects of the shock by stabilizing house prices and bank net worth, which amplifies consumption gains relative to partial equilibrium, especially for highly leveraged and low-wealth households. The model also predicts a measurable decline in consumption inequality, with the largest gains concentrated at the bottom of the consumption distribution.

However, the model implies that the effectiveness of moratoria depends critically on their scope and duration. Broad extensions that cover all mortgage holders deliver limited additional relief to unconstrained households but substantially erode bank cash flows and net worth, tightening credit supply and raising lending rates; the resulting decline in house prices and wages can overturn the short-run gains in consumption. Similarly, longer moratoria can provide additional short-run relief but slow the recovery by prolonging stress on intermediary balance sheets. These counterfactuals highlight a central lesson: policies that appear more generous at the household level may backfire once general equilibrium and financial-intermediary channels are taken into account, underscoring the importance of targeting and program design.

2 Literature review

Our paper contributes to four strands of literature on macroeconomic policy and household behavior during crises.

First, we relate to the literature on fiscal stimulus and liquidity provision in downturns. Classic fiscal tools—such as direct transfers, tax rebates, and unemployment insur-

ance—are known to stimulate demand (Johnson et al. (2006); Parker et al. (2013); Fagereng et al. (2021). More recent work evaluates the effectiveness and targeting of such measures during COVID-19 (Chetty et al. (2020); Hagedorn et al. (2023). In contrast to these public spending tools, we study a policy that expands household liquidity through payment deferral rather than transfers, with no direct fiscal cost. This positions our work alongside recent debates about the role of automatic stabilizers (McKay and Reis (2016)) and private-sector coordination mechanisms (Farhi and Werning (2016)).

Second, we contribute to a growing literature on credit relief. Several studies have examined loan forbearance and mortgage modification programs during the Great Recession, such as Home Affordable Modification Program (HAMP) in the U.S. (Agarwal et al., 2017; Ganong and Noel, 2020), which typically involved government subsidies or credit guarantees. Closer to our setting, Dobbie and Song (2015) analyze the macroeconomic effects of bankruptcy protection. Our work differs in two key respects: we study a purely private, opt-in moratorium policy implemented at scale, and we estimate both microeconomic and general equilibrium effects. Moreover, unlike bankruptcy or loan modification programs, mortgage moratoria do not involve any debt forgiveness or write-downs.[1] More recently, as moratoria were widely implemented during the COVID-19 pandemic, a new literature has begun to emerge. Guler et al. (2024) and Önder et al. (2023) examine the effects of mortgage and corporate debt moratoria in Colombia, providing evidence from a developing-country context. Besides, this paper measures consumption directly using transaction-level spending data, documents distributional impacts on consumption inequality, and provides out-of-sample validation of the structural model's cross-sectional predictions. Hatchondo et al. (2025) study payment suspensions for sovereign debt within a standard quantitative default framework. Dinerstein et al. (2023) evaluate the effects of the 2020 U.S. student loan moratorium. In contrast to these settings, our policy involved no debt forgiveness and targeted financially distressed individuals with recent delinquen-

[1] In contrast to existing forbearance measures, mortgage moratoria offer a distinct form of credit relief that combines immediacy, simplicity, and fiscal neutrality. Unlike maturity extensions or interest rate reductions, which often require loan-level renegotiation and administrative complexity, moratoria can be implemented quickly at scale, either automatically or through simple opt-in mechanisms. Compared to grace periods or principal forbearance, moratoria typically provide larger and more front-loaded liquidity relief by pausing the entire payment stream, not just its interest or principal components. Crucially, moratoria do not entail any nominal debt forgiveness and usually generate no direct cost to the public sector. This contrasts with programs like the U.S. HAMP, which relied on significant fiscal transfers funded through the Troubled Asset Relief Program (TARP) to subsidize interest reductions and principal modifications. While HAMP targeted borrowers in distress and provided durable NPV relief, it was administratively burdensome, slower to scale, and fiscally costly. In contrast, moratoria shift the timing of payments without reducing their present value (at least contractually), providing rapid and broad-based liquidity support in times of crisis without distorting public finances. This makes them a particularly attractive policy tool for macroeconomic stabilization when timely demand support is needed, but fiscal space is limited.

cies—highlighting both the design and distributional specificity of the intervention we study.

Third, our paper connects to research on the distributional impact of stabilization policies. A growing body of work explores how macro policies affect inequality through liquidity channels and market incompleteness (Kaplan et al. (2018); Broer et al. (2022)). We provide new evidence that a liquidity intervention targeted at mortgage borrowers—many of whom are liquidity constrained—can reduce both income and consumption inequality, even without transfers.

Finally, we build on structural macroeconomic models of heterogeneous agents with housing and financial frictions. Our framework extends prior work on quantitative models of housing with mortgage default, while introducing a novel moratorium policy experiment calibrated to Belgian data.[2] Importantly, we validate the model against micro-level reduced-form effects, and use it to uncover general equilibrium forces—via credit markets, house prices, and bank balance sheets—that amplify the impact of moratoria and to study the policy's distributional effects.

3 Moratoria in Belgium

3.1 Policy Setup

On March 22, 2020, the National Bank of Belgium (NBB) and the Belgian Financial Sector Federation (Febelfin) announced a moratorium policy to provide liquidity relief to mortgage holders impacted by the COVID-19 pandemic [3]. The policy allowed eligible borrowers to suspend capital and interest payments for up to six months, starting in April 2020.

Eligibility for the moratorium was restricted to mortgage holders who met several criteria: (1) no overdue payments on loans, taxes, or social security contributions as of February 1, 2020; (2) less than €25,000 on the current, saving and investment account; and (3) demonstrable economic hardship due to the pandemic. The participation in the moratorium program was not automatic. Eligible individuals had to apply in their bank. The request for the payment suspension could be submitted via e-mail - by sending the required documents or in person at the bank branch. The information about the policy was spread by banks and local news media.

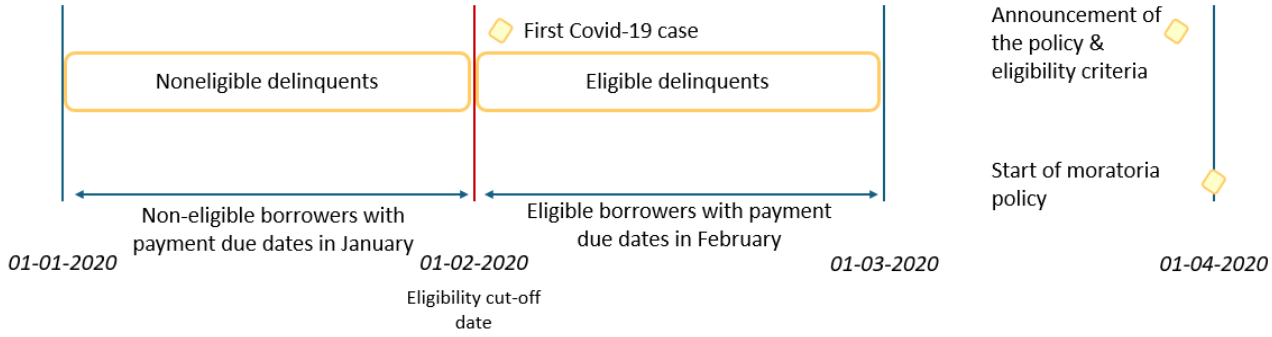
[2] Prominent examples are Jeske et al. (2013), Guler (2015), Arslan et al. (2015), Corbae and Quintin (2015), Hatchondo et al. (2015), Chatterjee and Eyigungor (2015), Guren and McQuade (2021), Arslan et al. (2025)

[3] Official comprehensive information on the policy is provided by the National Bank of Belgium (NBB moratoria).

Following the suspension period, regular payments resumed, and the debt maturity was extended by the duration of the moratorium. To account for the deferred interest payments, monthly repayments were recalculated to ensure that the present discounted value of the mortgage remained unchanged. Additionally, banks were prohibited from imposing any extra fees or administrative charges. After payment suspension, any overdue days acquired later were reset to zero, hence, moratorium policies did not impair borrowers' future borrowing capacity. Eligible households could apply for the policy between April and September 2020.

3.2 Identification

Figure 1: Timeline of Debt Suspension Policy and Identification



Our identification strategy exploits repayment behavior around the policy-defined eligibility cutoff. Figure 1 summarizes the timeline of key events—the policy announcement, the application window, and the start of the moratoria—and illustrates how our eligible and non-eligible groups map into the official criteria.

The policy eligibility rule is defined at a daily date: borrowers must have *no overdue payments as of February 1, 2020*. Our credit records, however, report delinquency only at *month-end*. As a result, we cannot implement a standard regression discontinuity design with a continuously observed running variable around February 1. Instead, we construct a near-cutoff comparison using a *coarsened* measure of days past due at the end of February: households with fewer than 30 days past due as of February 29 are classified as eligible, while those with 30–60 days past due are classified as non-eligible. This mapping is natural in our setting because scheduled mortgage payment orders are due at the beginning of the month, so end-of-month delinquency closely tracks whether arrears were already present at the February 1 cutoff. Consequently, our design compares financially stressed borrowers

who *narrowly qualify* for the moratorium (arrears start in February) to those who *narrowly miss* eligibility (arrears date back to January).[4]

To further ensure comparability, we restrict attention to borrowers who experience missed payments in February and satisfy the liquid-wealth criterion of the policy (movable assets below €25,000). While the official program also required the absence of other arrears (e.g., taxes or social security contributions) and demonstrable pandemic-related hardship, these components are not perfectly observed in our bank records; focusing on distressed borrowers with missed payments and imposing the liquid-wealth threshold yields two groups facing similar repayment challenges, differentiated primarily by the *timing* of delinquency onset relative to the cutoff. Consistent with this interpretation, pre-policy characteristics and pre-trends in spending, income, debt, and demographics are balanced across the two groups (Section Appendix A), supporting the parallel-trends assumption underlying our event-study DiD estimates.

3.3 Data

In this study, we use a highly granular, proprietary dataset comprising anonymized credit, transaction, and account records from BNP Paribas Fortis (BNPPF). BNPPF has around a quarter of the commercial banking market in Belgium. The BNPPF dataset contains daily transactions, demographics and end-of-month balances for more than 4 million Belgian retail clients ranging from 2012 to 2023 and covering all regions of the country.

As in De Schryder et al. (2025) we assign each client to a household using a unique household identifier. Clients are grouped under the same household identifier if they either share a bank account or have reported a familial relationship to the bank. Information about familial relationships is obtained either through client self-reporting or via the annual requirement for clients to confirm their ID information, which includes recorded family relationships. This identifier thus encompasses both traditional households (e.g., parents and children) and economic households (e.g., cohabiting individuals). The household identifier is dynamic and updates when members change their domicile address, allowing for analysis at the household level. The dataset includes all transactions—debit card purchases, cash withdrawals, and wire transfers—along with comprehensive categorization of both income and spending. Income categories are particularly reliable due

[4]The February 1 cutoff is policy-defined and therefore arbitrary. Because February 1, 2020 fell on a Saturday, we considered shifting the cutoff by a few days. Bank records show no payment orders due on different days than the first day of the month; consequently, adjusting the cutoff does not change assignment or sample size.

to legally mandated communication patterns for wage payments in Belgium Boudt et al. (2023). Expenditures are classified into 58 consumption categories following the United Nations' official Classification of Individual Consumption by Purpose (COICOP), allowing us to distinguish between durable, semidurable, non-durable goods and services. The richness of the dataset enables us to track household income and consumption patterns at a monthly frequency, while accounting for their level of liquid wealth and a wide range of potential confounders.

Importantly for our study, the BNPPF dataset also includes loan-specific information such as loan size, interest rates, maturity, loan type, monthly payment amounts, and delinquency days (i.e., past due days on debt payments). Additional information is provided regarding whether the loan was subject to a temporary moratorium, as well as the duration of such a suspension. In the dataset, approximately 40,000 mortgages were subject to this policy during the COVID-19 pandemic in 2020. This represents approximately 5% of the total outstanding mortgages at BNPPF in 2020, a figure that closely aligns with the statistics reported by official authorities. Besides, the dataset contains various non-identifying individual information, including age, gender, region of residence, and household size. A more detailed description of the credit and transaction dataset is provided in the appendix of Boudt et al. (2022). They illustrate the representativeness of the data by showing that the distribution of income in the BNPPF dataset closely aligns with the definition used by the Belgian statistics institute (Statbel 2019).

Several transformations are applied to construct the analysis sample. First, following the identification strategy, the sample is restricted to borrowers with past-due mortgage payments. Since the majority of moratorium recipients were financially sound with no payment arrears in February 2020, only those with outstanding past-due amounts are retained—representing less than 1% of loans outstanding in 2020. Although this substantially reduces the sample, it is necessary to ensure comparability between eligible and ineligible borrowers. Next, individual-level records are then matched using household identifiers based on residential address. We verified compliance with the liquid wealth criterion, which was an additional criterion of the policy. Only households holding less than €25,000 in current, savings, and investment accounts were eligible and are retained in our sample. This transformation ensures that households in both groups had similar financial resources. The dataset is subsequently merged with consumption data. To ensure that it comprises households actively using their current accounts as their principal accounts, only those with average monthly non-durable consumption expenditure of at least €166 (adjusted for inflation) over the period 2019–2021 are retained. The minimum consumption threshold follows the reference budget estimates of Storms and Van den

Bosch (2009), which establish the minimum expenditure required for adequate social participation in Flanders. This threshold is widely adopted in the literature examining household financial behavior in Belgium (see, e.g., De Schryder et al. (2025)). While this constitutes another substantial reduction in sample size as the sample drops by 60%, it is necessary to exclude households that do not use the observed account as their primary financial account. In an online Appendix F, a sensitivity analysis is conducted relaxing the consumption threshold to require at least non-zero expenditure in all quarters.

Finally, to mitigate volatility in the transaction data, expenditures and income are smoothed using a six-month moving average and subsequently aggregated into quarterly observations. The final sample consists of 1,215 households, comprising 830 eligible and 385 ineligible households.

3.4 Econometric Specification

We present the econometric framework used to estimate the intention-to-treat (ITT) effect. The ITT measures the effect of the eligibility for the policy irrespective of whether households received debt payment suspension in practice. We estimate the event-study difference-in-differences design (DiD) between 2019Q1 – 2021Q4 controlling for time and household fixed effects. The dependent variable, $(S_{i,t})$, denotes the set of variables of interest that are used in the analysis: consumption spending (separately for non-durable goods, services) and mortgage payments denominated in euros in quarter t for household i. Specifically, we estimated the following equation:

$$S_{i,t} = \beta^{ITT} \cdot T_{2020Q2\&3\&4} \cdot \text{Eligible}_i + \underbrace{\sum_{n=2019Q1}^{2020Q1} \omega_n \cdot T_{n,i} \cdot \text{Eligible}_i}_{\text{Pre-Policy Period Effects}} + \underbrace{\sum_{m=2021Q1}^{2021Q4} \beta_m T_{m,i} \cdot \text{Eligible}_i}_{\text{Post-Policy Period Effects}} + \eta_i + \lambda_t + \epsilon_{i,t} \tag{1}$$

The indicator $Eligible_i$ equals 1 if a household satisfied the eligibility criteria for moratorium policy. Our primary parameter of interest β^{ITT}, captures the average ITT effect over 2020Q2–2020Q4, the period during which eligible households could apply for debt suspension. [5] The additional interaction terms between the time dummies (T) and the eligibility indicator trace out both pre- and post- policy effects. The pre-policy coefficients

[5] We pool the three quarters of the policy period because consumption opportunities were limited at the beginning of the pandemic due to COVID-19 restrictions, and a single coefficient facilitates comparison with the quantitative model. Quarterly disaggregated results in Table D2 show similar magnitude of the point estimate in 2020Q2 and 2020Q3 but are statistically insignificant.

provide a test of the parallel trends assumption, while the post-policy coefficients capture the persistence and medium-term effects of the intervention.

The specification includes household fixed effects η_i to absorb time-invariant heterogeneity in mortgage payments and consumption, and year-quarter fixed effects λ_t for each quarter from 2019Q1 through 2021Q4. We omit 2019Q4 as the reference period, which predates both the policy announcement and the onset of COVID-19, allowing us to interpret the coefficients as deviations from this pre-pandemic baseline. Standard errors are clustered at the household level. $\epsilon_{i,t}$ denotes the error term.

A natural concern is that contemporaneous shocks associated with the pandemic may confound our estimates. In particular, the onset of COVID-19 could have altered borrower behavior prior to the moratorium's introduction, potentially violating the identifying assumption. Two institutional features help to mitigate this concern. First, eligibility was determined by delinquency status as of end-February 2020, prior to the policy announcement on March 22 (Figure 1). Second, the eligibility criteria were not publicly known before the announcement date. Together, these features imply that households could not have strategically adjusted their repayment behavior to obtain eligibility, ruling out selection into treatment based on anticipation of the policy. This design thus provides a credible source of quasi-experimental variation.

A second concern is that borrowers may have selected into treatment by adjusting their repayment behavior in anticipation of the policy. While self-selection into actual policy take-up may indeed be present, our identification strategy is less susceptible to tis concern. We define treatment based on eligibility, that is, whether a household satisfied the official criteria, rather than on actual policy participation. Because eligibility was determined by pre-announcement delinquency status, borrowers could not manipulate their treatment assignment after learning of the policy. Any self-selection into take-up among eligible households would attenuate our ITT estimates toward zero, implying that our results represent a lower bound on the policy effect for compliers. Moreover, anticipation of the policy itself was unlikely: the COVID-19 pandemic was an unprecedented shock, Belgium had no prior experience with mortgage moratoria, and the policy was announced earlier than in most other countries.[6]

An important assumption of the DiD approach is that eligible and non-eligible borrowers would not exhibit significant differences in the absence of the policy. To test this, we analyze whether the two groups have statistical differences in consumption and

[6]The first lockdown was announced by the National Security Council on March 12, 2020. See https://www.belgium.be/en/news/2020/coronavirus_phase_2_maintained_transition_federal_phase_and_additional_measures. Belgium was among the first of over 70 countries to introduce a mortgage moratorium. For a cross country overview, see Guler et al. (2024).

spending trends before the policy implementation. Appendix B presents the evolution of trends of consumption, labor income and mortgage payments before, during and after the debt suspension policy was implemented. The trends for all outcome variables clearly show parallel behavior before the policy was implemented and notable changes during and after the program. Our quantitative findings also support the lack of statistically significant differences in pre-policy periods, supporting the validity of the assumption of parallel trends (Appendix C). In other words, there were no differences in mortgage payments, expenditures, saving balances and household characteristics between eligible and non-eligible before moratoria policies were implemented.

4 Empirical Results

4.1 Impact on Household Balance Sheets

This section presents intention-to-treat estimates of the effect of eligibility for debt suspension on mortgage payments, consumption expenditure, and labor income.

Table 1 presents our main results. We report coefficients for the pre-policy period (2020Q1), the debt suspension policy period (2020Q2-4), and the post-policy period (2021Q1–2021Q4) following the methodology described in section 3.4. Column 1 reports effects on mortgage payments, columns 2 through 4 report effects on consumption expenditure, and column 5 reports effects on labor income. The sample comprises 1,215 households, with 2019Q4 serving as the reference period.

The estimates reveal three main findings. First, eligible households reduced mortgage payments during the policy period. The coefficient indicates that eligible borrowers paid €232 less than non-eligible households, an effect that is statistically significant at the 5 percent level. In the post-policy period, the coefficients remain negative but are smaller in magnitude and statistically insignificant, indicating that payment patterns converged across groups after the program ended.

Second, eligible households increased consumption expenditure relative to non-eligible households. Spending on non-durable goods and services rose by €297 during the policy period, significant at the 10 percent level. Disaggregating by category, services account for the majority of this increase (€249, significant at the 10 percent level), while the effect on non-durable goods alone is smaller and statistically insignificant. The consumption response persists into the post-policy period, with coefficients on non-durable goods and services remaining elevated and statistically significant through 2021Q1, ranging from €332 to €400.

Third, the estimates show no significant differences between labor income of eligible and non-eligible borrowers. Point estimates are negative across all periods, with magnitudes between €97 and €245, but standard errors are large and none of the coefficients are statistically different from zero.

Finally, the coefficient for the pre-policy period (2020Q1) is small and statistically insignificant across all outcomes, supporting the parallel trends assumption underlying our identification strategy. These reduced form estimates provide a benchmark for evaluating the quantitative model developed in the following section.

Table 1: Estimated Intention-to-Treat Effects to Moratoria Policy

	Mortgage Payments	**Non-durable & Services**	**Non-durable & goods**	**Services**	**Labor Income**
Pre-Policy Period					
ITT_{2020Q1}	103.67	112.84	20.97	143.12	-113.53
	(144.28)	(100.72)	(35.45)	(96.31)	(104.98)
Policy Period					
$ITT_{2020Q2-4}$	-231.58**	296.81*	36.82	249.10*	-245.46
	(115.94)	(162.19)	(61.76)	(150.04)	(188.33)
Post-Policy Period					
ITT_{2021Q1}	-95.91	390.18*	89.93	306.31	-169.66
	(155.72)	(208.77)	(78.02)	(193.65)	(208.68)
ITT_{2021Q2}	-178.70	400.11	98.62	265.07	-96.91
	(140.81)	(221.51)	(82.73)	(200.11)	(231.62)
ITT_{2021Q3}	-236.26	332.67	100.21	259.64	77.89
	(193.70)	(225.70)	(86.43)	(199.82)	(245.49)
ITT_{2021Q4}	-145.21	397.50	113.52	288.52	224.35
	(144.50)	(225.10)	(84.20)	(204.86)	(260.88)
Time FE	Yes	Yes	Yes	Yes	Yes
HHs FE	Yes	Yes	Yes	Yes	Yes
Households	1215	1215	1215	1215	1215

Authors' calculations. The estimated parameters represent the effect of the ITT moratorium policy on household spending, labor income, and mortgage payments between 2019Q1 and 2021Q4. *Pre-policy* refers to the period 2020Q1 as a test for parallel trend. The remaining parameters for pre-policy quarters are insignificant and omitted from the table for the clarity of the results and shown on Figure C2. 2019Q4 is the reference period, unaffected by the policy and the COVID-19 pandemic. *Policy period* refers to the period during which households could participate in the policy. *Post-Policy* refers to the period when all suspensions had ended. All variables are in euros, deflated by the CPI index. Consumption expenditures and labor income are calculated as 6-month moving averages. Mortgage payments refer to quarterly payments on existing loans originated before the policy. All columns control for household and time-fixed effects. Standard errors, clustered at the household level, are in brackets. Asterisks indicate significance levels: *.1, **.05, ***.01.

4.2 Heterogeneity Analysis

In this section, we analyze the heterogeneous impacts of moratorium eligibility across various household dimensions. Figure 2 illustrates the variations in estimated coefficients across different levels of: (1) loan-to-value (LTV) ratios, (2) household age defined as the age of the oldest household member, and (3) pre-policy savings percentiles. These effects are compared across three distinct periods: the pre-policy baseline (2019Q1–2020Q1), the debt suspension phase (2020Q2–2020Q4), and the post-policy period (2021Q1–2021Q4).

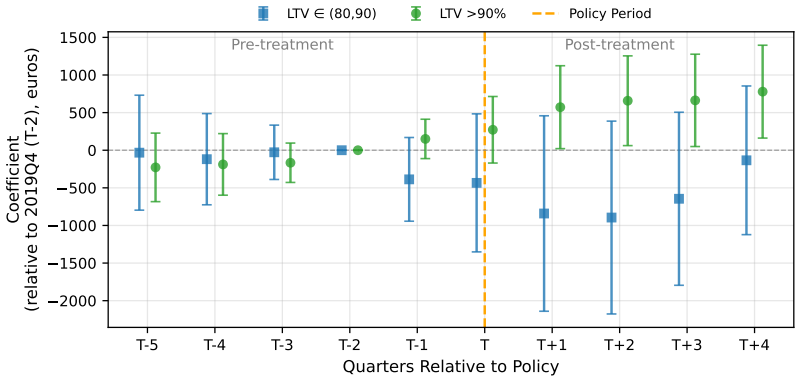
Loan-to-Value Ratio Panel (a) reveals striking differences in consumption responses across LTV groups. Households with high LTV ratios (above 90%) exhibit substantially larger positive consumption effects during the policy implementation compared to those with LTV ratios between 80% and 90%. This pattern is consistent with the hypothesis that more leveraged households—those facing tighter liquidity constraints and greater debt burdens—derive larger benefits. The divergence between groups becomes particularly pronounced in quarters T+2 through T+4, suggesting that high-LTV households utilized the freed-up liquidity for consumption once pandemic-related restrictions eased.

Household Age Panel (b) demonstrates significant heterogeneity by the age of the oldest household member. Young households (defined as under 35 years) display the largest consumption increases following policy eligibility, with effects that persist and grow throughout the post-policy period. Middle-aged households (35–60 years) show more moderate responses, while older households (60+ years) exhibit the smallest effects, with point estimates closer to zero. Younger households typically face binding liquidity constraints and have higher marginal propensities to consume out of transitory income shocks.

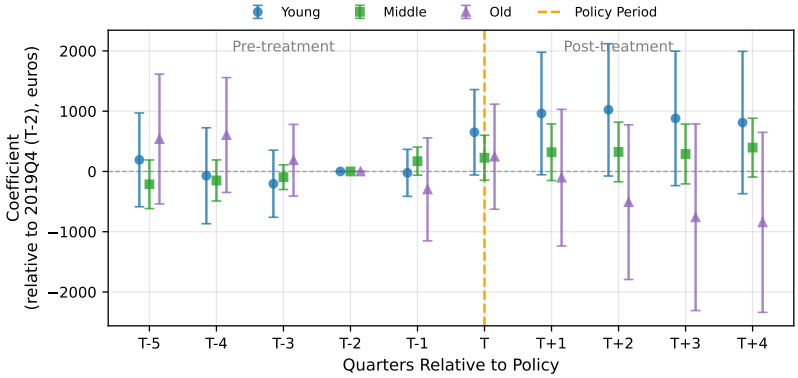
Savings Panel (c) disaggregates the consumption effect by savings levels. Households in the middle savings percentiles (50–90%) experience the most pronounced consumption increases. This nonlinear pattern suggests that the policy was particularly effective for borrowers with moderate liquid asset buffers.

Across all specifications, we observe that consumption effects are muted during the initial quarters of the policy period (T to T+1) before strengthening considerably in later quarters (T+2 to T+4). As mentioned in the previous section, this delayed response likely reflects the severe restrictions on consumption opportunities imposed during the COVID-19 pandemic, which temporarily suppressed households' ability to translate additional liquidity into spending on non-durable goods and services.

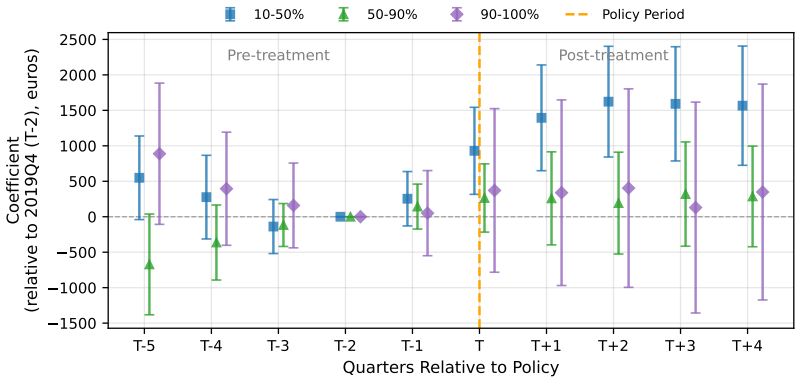
Figure 2: Heterogeneity in ITT effect on Non-durable goods & Services Spending



(a) by LTV ratio



(b) by Households' Age

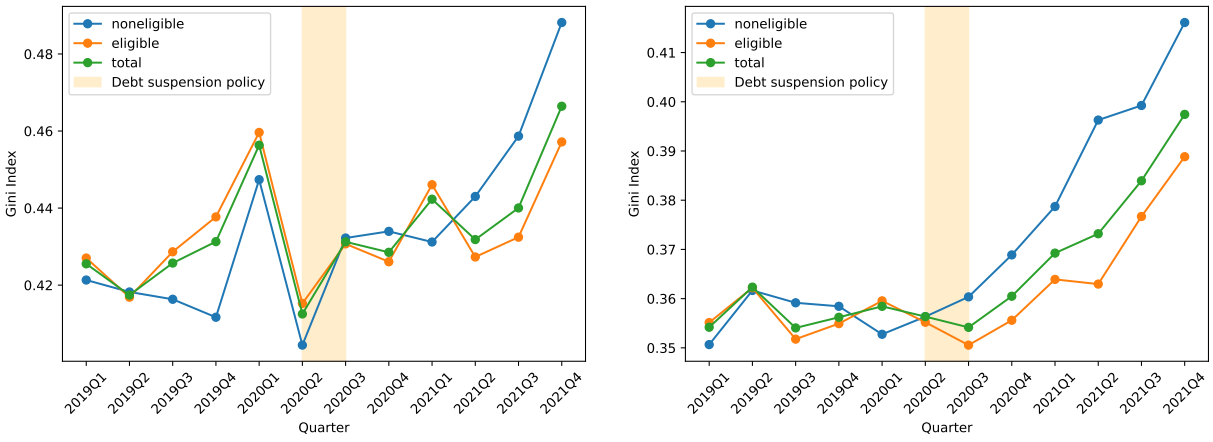


(c) by Savings Percentiles

This figure presents heterogeneous estimates of the consumption effects of eligibility for the moratorium policy. The $T-2$(2019Q4) is a reference period and, therefore, it is omitted from the estimation. T corresponds to the 2nd, 3rd, and 4th quarter of 2020. Vertical bars indicate 95% confidence intervals based on standard errors. The wide confidence intervals result from the small sample size of subgroups and high volatility of the data. All coefficient estimates with standard errors are reported in the Appendix E. Notes: (a) LTV groups are defined as households with LTV ratios in the range [80%, 90%) and LTV > 90%. (b) Age groups are defined based on the age of the oldest household member: young (<35 years), middle-aged (35–60 years), and older (60+ years). Group thresholds were also chosen to ensure adequate sample sizes within each category. (c) Savings denotes total savings on current and saving accounts are grouped into percentiles, where the bottom 10th percentile is omitted due to high volatility in the data and presence of zero balance on saving accounts.

Additionally, we examine the distributional effects of the moratoria policy. Figure 3 illustrates the evolution of the Gini coefficient for income and consumption expenditures, comparing eligible and non-eligible households over time. For income (panel a), prior to the policy implementation, the Gini coefficient fluctuated between 0.42 and 0.45, with no substantial difference between the two groups, indicating moderate income inequality in the sample. During the policy period, the Gini coefficient declined markedly for both groups, suggesting a temporary compression in income inequality due to COVID-19 repercussions. However, in the quarters following the policy's expiration, inequality increased sharply among non-eligible households while remaining relatively stable for eligible households. This divergence suggests that the moratoria may have provided a more lasting equalizing effect for eligible households. For consumption (panel b), a similar pattern emerges. The Gini coefficient for both groups hovered around 0.36–0.38 before the policy, indicating lower inequality in consumption than in income. During the moratoria period, consumption inequality declined for both groups. Following the policy's end, however, inequality rose more steeply among non-eligible households, while the increase was more moderate for eligible households. This pattern reinforces the finding that the policy had differential distributional consequences depending on eligibility status.

Figure 3: Distributional Effects of the Debt Moratorium: Income and Consumption Inequality Over Time



(a) Gini index for income

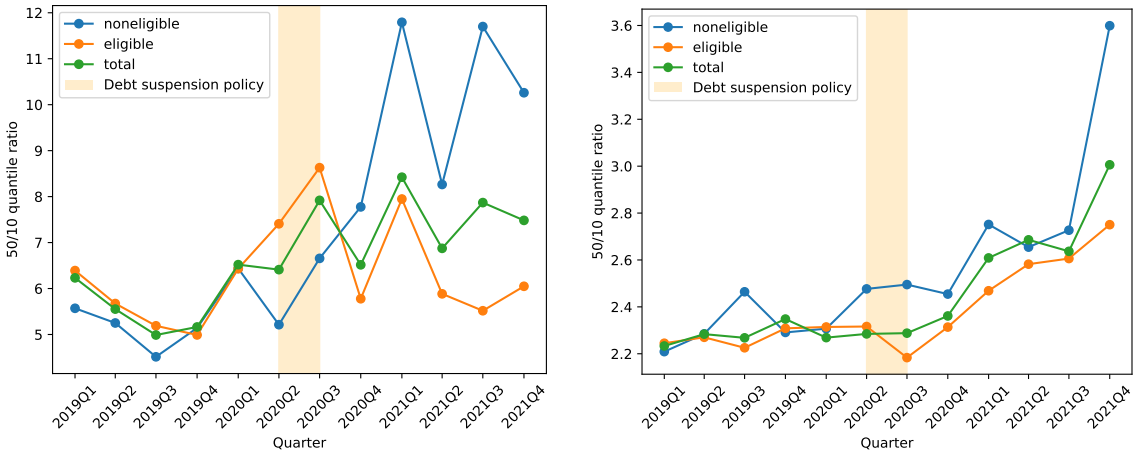
(b) Gini index for consumption

The legend distinguishes between three groups: El – eligible households (treated and non-treated), NE – non-eligible households, and Total – the entire sample, including both eligible and non-eligible households. Income is an aggregated measure of real labour income received on the bank account. Consumption represents the sum of expenditures for non-durable goods and services as describe in section 3.3. A Gini coefficient of 1 represents perfect inequality.

Additionally, Figure 4 presents the evolution of the 50th-to-10th ratio, defined as the ratio between the median and the bottom 10th percentile of the income and consumption distributions. This measure captures changes in inequality at the lower end of the distribution. Prior to the policy, we observe parallel trends in the 50th-to-10th ratio for both income and consumption across eligible and non-eligible households, supporting the comparability of the two groups.

Following the policy period, however, the income 50th-to-10th ratio rises sharply for non-eligible households, indicating that the increase in inequality is driven primarily by a deterioration at the lower end of the income distribution. In contrast, the increase in the consumption 50th-to-10th ratio for non-eligible households is more modest, suggesting a smaller shift in consumption inequality among low-income individuals.

Figure 4: Evolution of 50th-10th percentile ratio by Eligibility Status



(a) 50th-to-10th percentile ratio for income

(b) 50th-to-10th ratio for consumption

The legend distinguishes between three groups: El – eligible households (treated and non-treated), NE – non-eligible households, and Total – the entire sample, including both eligible and non-eligible households. Income is an aggregated measure of real labour income received on the bank account. Consumption represents the sum of expenditures for non-durable and services as describe in section 3.3. Gini coefficient equals 1 represents perfect inequality.

4.3 Robustness Analysis

To ensure the robustness of our results, we conduct several sensitivity analyses.[7] First, our baseline specification restricts the sample to households with monthly non-durable consumption expenditure of at least €166 (inflation-adjusted). We relax this requirement by including all households with non-zero consumption levels between 2019 and 2021. This

[7]The results of these exercises are reported in online Appendices Appendix F and Appendix G.

modification increases the number of observations by approximately 60 percent; however, it may also introduce households that use their current accounts infrequently, potentially attenuating our estimates. Table F5 presents the parameter estimates for this extended sample. The results remain qualitatively similar to those reported in the main analysis, although the coefficients are somewhat smaller in magnitude. We therefore conclude that our main findings are robust to relaxing the minimum consumption expenditure requirement. Second, we estimate the effects using consumption data aggregated to quarterly frequency but without applying moving average smoothing. Although the raw consumption data exhibit considerable volatility, the estimated results remain similar to those from the baseline specification.

When relaxing the minimum consumption threshold, our findings remain virtually unchanged. When using unsmoothed data, Figure G3 confirms that consumption expenditure exhibits greater volatility. The estimated coefficients remain similar in magnitude to those in the baseline specification; however, the effects on services expenditure lose statistical significance. Additionally, we detect a statistically significant effect in the pre-policy period, suggesting that the increased noise in unsmoothed data may introduce spurious variation. Despite these differences, the overall pattern of results supports the conclusions drawn from our main analysis.

4.4 Discussion

Three findings emerge from our empirical analysis that shed light on how allowing for debt relief policies affect household behavior.

First, eligible households substantially reduced their mortgage payments more than non-eligible during the policy period. The eligible group comprises both households that formally enrolled in the moratorium and those that did not. However, only around 5 percent of eligible households formally participated in the program, yet we detect significant payment reductions across all eligible households.[8]

Several factors may explain this behavior. Some eligible households may not have been fully aware of the formal debt suspension option despite widespread dissemination through media and banks. Others may have been hesitant to formally enroll due to uncertainty or mistrust, given that this was the first introduction of such a policy in Belgium. While participation did not negatively affect credit scores, less informed households may have been deterred by perceived risks. The availability of the suspension option may

[8] Appendix I presents robustness exercises estimating the intention-to-treat effect while excluding borrowers who formally participated in the moratorium. The results remain consistent with the main findings, indicating that the observed effects are not only driven by treated households.

also have functioned as implicit insurance, allowing households to informally adjust their payments knowing that formal relief was accessible if needed. Finally, as the pandemic was initially perceived as temporary and did not substantially affect income for many households, some may not have felt the necessity of formal enrollment. The low participation rate is consistent with findings from other countries, and the determinants of take-up merit further investigation.

Second, the payment moratorium generated a significant consumption stimulus. Eligible borrowers redirected suspended payments toward increased spending, with the effect concentrated in services. This pattern suggests that the moratorium functioned not only as financial relief but also as an indirect consumption stimulus, consistent with an insurance channel through which access to potential liquidity support encouraged spending.

The consumption response emerged gradually over time. During the initial quarters of the policy period, pandemic related restrictions in Belgium and elsewhere physically constrained the ability of households to spend on non-durable goods and services. Once these restrictions were eased, eligible households exhibited a sharp increase in consumption, particularly within the services sector.

The findings underscore the importance of examining heterogeneous policy effects. Young households, those with moderate savings, and those with high loan-to-value ratios experienced the largest increases in expenditure. This pattern is consistent with the hypothesis that liquidity constrained households derived the greatest benefit from the policy, as these groups typically face binding financial constraints and exhibit higher marginal propensities to consume. Because the policy primarily benefited the most vulnerable and constrained households, it effectively stabilized inequality among eligible households even as inequality widened substantially among non-eligible ones. Aggregate estimates alone would mask this meaningful variation in household responses.

5 Quantitative Model

Why a quantitative model? While our empirical estimates provide credible evidence on the causal effects of mortgage payment suspensions, they are inherently partial equilibrium in nature. That is, the design compares households narrowly around the eligibility threshold, holding prices, wages, and credit conditions constant. This allows us to isolate local, intensive-margin behavioral responses, but it abstracts from potential general equilibrium feedback effects. For instance, moratoria may influence house prices, bank balance sheets, and credit supply—mechanisms that are muted in our empirical setting but could significantly amplify or dampen the aggregate impact. To capture these economy-wide

interactions and extend our analysis to the full population, we now turn to a state-of-the-art dynamic general equilibrium model with heterogeneous agents and financial frictions.

An important ingredient of our framework is modeling banks explicitly and we assume that banks take this moratorium policy as a one-time non-recurrent policy in line with our event-analysis in Section Appendix H where we find no evidence that banks repriced loans, changed the amount of mortgage granted or altered maturities following the introduction of moratoria within a one-week and two weeks windows. This suggests that banks did not internalize the policy as a recurrent risk, consistent with our modeling of moratoria as a one-time intervention.

The economy consists of five sectors: households, financial intermediaries (banks), rental companies, firms, and government. There is no aggregate uncertainty, but individuals are subject to idiosyncratic income shocks. These shocks lead to heterogeneity in income, wealth, housing tenure and mortgage debt across households. We study the effects of debt policies in response to unexpected and persistent shocks to the economy. Perfect foresight is assumed along the transition.

5.1 Households

There is a continuum of households who are all born identical and with zero wealth. They all have finite and deterministic life J. They work in the initial $J_R < J$ periods and enjoy retirement in their remaining life-time.

Households derive utility from consumption and housing services and can choose between renting or owning a house of their desired size.

$$E_0 \sum_{j=1}^{J} \beta^{j-1} u(c_j, s_j),$$

where β is the discount factor, c_j is consumption, and s_j is the housing services of an age j household.

Households supply labor inelastically during working age and earn the market wage w per their labor efficiency. When they retire, they receive fixed retirement benefits as a function of their labor efficiency at the time of retirement. Retirement benefits are financed through proportional labor income taxes, τ_r, on working age households. Labor efficiency before retirement is subject to idiosyncratic shocks. The log of labor efficiency includes a deterministic component $f(j)$, which only depends on age, and a stochastic component z_j, which is an AR(1) process.

Thus, a household's labor income $y(j, z_j)$ can be summarized by

$$y(j, z_j) = \begin{cases} w\,(1-\tau_r)\exp(f(j)+z_j), & \text{if } j < J_R \\ w y_R(z_R), & \text{if } j \geq J_R \end{cases} \tag{2}$$

$$z_j = \rho z_{j-1} + \varepsilon_j, \quad \varepsilon_j \sim i.i.d. \quad N(0, \sigma_\varepsilon^2),$$

where w is the wage per efficiency units of labor, τ_r is the social security tax rate, and $y_R(z_R)$ is a regressive pension benefit system conditional on the realization of labor efficiency z_R upon retirement.

Housing Choices: Upon experiencing an income shock, households make decisions regarding their housing tenure —whether to rent or own— in addition to their consumption and savings choices. The sole financial investment available to them is mutual funds, which yield a return denoted by r_k.

To obtain housing services, households may choose to rent or purchase a home. Renters pay an endogenous rental rate p_r per unit of housing services s. Alternatively, households can acquire housing services through homeownership, financed via long-term, defaultable mortgages. The terms of these mortgages are endogenously determined by financial intermediaries, who price them based on household-specific default risk.

Homeowners retain the flexibility to remain in their homes or transition back to renting, either by selling their property or defaulting on their mortgage obligations. They may also refinance their mortgage at any time, which is treated equivalently to initiating a new mortgage. Furthermore, households can adjust their housing consumption by upgrading or downsizing—selling their current home and purchasing another.

Homeownership entails several transaction costs. The purchase price of a home is p_h per unit of housing. To finance this, households may obtain a mortgage, which involves two types of origination costs: a fixed fee φ_f (e.g., attorney, appraisal, and title fees), and a variable fee φ_m, calculated as a fraction of the mortgage principal. Selling a home also incurs a cost, with sellers paying a fraction φ_s of the sale price, primarily due to real estate agent commissions.

Houses depreciate over time, requiring homeowners to spend a fraction δ_h of the home's value on maintenance each period. Given the risk of default, lenders incorporate a risk premium into the mortgage origination price.

While mortgage default is permitted, it carries significant consequences. Households that default become inactive renters, temporarily barred from homeownership and re-

quired to rent. These households regain eligibility to purchase a home with probability π in each subsequent period.

Mortgage Payments: Mortgages are modeled as long-term financial contracts. Following Arslan et al. (2025), we assume that mortgage balances amortize by the end of the life-cycle which implies the maturity of the mortgage as the remaining life of the individual. Then, given a mortgage interest rate r_ℓ, the periodic mortgage payment m is computed by the standard formula which equates the present discounted value of the mortgage payments to the mortgage principal at the origination:

$$m = d\frac{r_\ell}{1 + r_\ell - (1 + r_\ell)^{j-J}} \tag{3}$$

where d denotes the current mortgage principal, j is the current age of the individual and J is the terminal age of the individual. This formulation implies that mortgage principal follows as:

$$d' = (d - m)(1 + r_\ell)$$

Although mortgage default risk varies across households due to ex post heterogeneity, we simplify the model by computing the amortization schedule using the risk-free mortgage rate r_ℓ, rather than household-specific interest rates. This approach, consistent with Arslan et al. (2025), Hatchondo et al. (2015), and Kaplan et al. (2020), avoids introducing an additional state variable.

Importantly, household-specific default risk is reflected in the mortgage pricing at origination rather than in the interest rate itself. In effect, all households pay upfront points to reduce their mortgage rate to the common risk-free mortgage rate r_ℓ.

Value Functions: At any given time, households can be in one of three housing statuses: homeowner, active renter, or inactive renter. The distinction between active and inactive renters lies in their access to homeownership. Active renters are eligible to purchase a home, whereas inactive renters are temporarily barred from doing so. A homeowner transitions to inactive renter status upon defaulting on a mortgage. Inactive renters regain eligibility to become active renters with probability π each period.

The household's state is represented by the vector $\theta \equiv \{\omega, z, j, d, h\}$, where: ω is current cash-on-hand, z is labor efficiency, j is age, d is outstanding mortgage debt (if any), h is house size (set to zero for renters).[9]

[9]For renters, both d and h are zero, as they do not own property.

The central decision problem is that of an active renter considering a home purchase. We denote this household's value function as V^{rh}. Upon choosing to buy a home, the household accesses the mortgage market and selects a mortgage amount d', which determines the mortgage price at origination, $q^m(\theta)$, a function of the household's current state θ.

The purchaser's problem is formulated as:

$$V^{rh}(\theta) = \max_{c,d',h',a' \geq 0} \left\{ u(c,s) + \beta \mathbb{E} V^h(\theta') \right\}$$

subject to the constraints:

$$\begin{aligned} c + p_h h' + \delta_h p_h h' + \varphi_f + a' &= \omega + d' \left(q^m(\tilde{\theta}) - \varphi_m \right) \\ \omega' &= w'(1-\tau) y(j+1, z') + a'(1 + r_k') \\ s &= h' \\ d' &\leq p_h h'(1-\chi) \end{aligned}$$

where $\tilde{\theta} \equiv \{d'; a', h', z, j\}$, , p_h is the unit price of housing, δ_h is the maintenance cost as a fraction of house value to cover the depreciation of the house, φ_m is the variable cost of mortgage origination, φ_f is the fixed cost of mortgage origination, χ is the minimum down payment requirement (i.e., households must finance at least a fraction χ of the house value themselves).

5.2 Mutual Funds

Mutual funds serve as the owners of both the goods-producing firms and rental companies. They raise capital by borrowing from households and allocate these funds to operate the firms and rental enterprises. The interest rate at which mutual funds borrow from households is denoted by r_k.

5.2.1 Firms

A perfectly competitive firm produces final output by combining rented capital K and hired labor N. The rental rate of capital is r_k, and the wage per unit of labor efficiency is w. Capital depreciates at a constant rate δ_k.

The firm's optimization problem is given by:

$$\max_{K,N} \mathbb{Z} K^{\alpha} (N)^{1-\alpha} - (r_k + \delta_k) K - wN,$$

where $\mathbb{Z}$ denotes aggregate productivity.

5.2.2 Rental Companies

Rental companies are the owners of rental housing units. They actively participate in the housing market by buying and selling these units—both from households and among themselves. The portion of their housing stock not sold is rented out to households at a per-period rental rate p_r per unit. Each unit incurs a maintenance cost κ, and the housing stock depreciates at a rate δ_h.

The dynamic optimization problem of a representative rental company is given by:

$$(1 + r_k)V^{rc}(H_r) = \max_{H_r'} \left\{ (p_r - \kappa - p_h) H_r' + (1 - \delta_h)H_r + V^{rc}(H_r') \right\},$$

where r_k: return on capital, p_h: price of housing, η: parameter governing the magnitude of adjustment costs, $V^{rc}(H_r)$: value function of the rental company.

In equilibrium, the return on rental housing must equal the return on capital, r_k. This condition yields the rental pricing equation:

$$p_r = \kappa + p_h - \frac{(1 - \delta_h)p_h'}{1 + r_k},$$

where p_h' is the expected future housing price.

5.3 Financial Intermediaries

Following Arslan et al. (2025), we consider an environment populated by infinitely many stand-alone, risk-averse financial intermediaries operating in a perfectly competitive market. These intermediaries seek to maximize the present discounted value of their dividend streams. They borrow from the international capital market at the exogenous interest rate r_t and extend credit to households as long-term mortgages.

The optimization problem faced by a representative intermediary is:

$$\max_{L_{t+1}, B_{t+1}} \sum_{t=0}^{\infty} \beta_L^t \log(d_t^B),$$

subject to the constraints:

$$\begin{aligned} d_t^B + L_{t+1} &= \omega_t + B_{t+1}, \\ \omega_{t+1} &= L_{t+1}(1 + r_{\ell,t+1}) - B_{t+1}(1 + r_{t+1}), \end{aligned}$$

where d_t^B denotes dividends, ω_t is the intermediary's net worth, B_{t+1} represents borrowing from the international market, and L_{t+1} is total lending to households.[10]

Consistent with Gertler and Kiyotaki (2010), we assume that intermediaries may default at the beginning of a period by diverting a fraction ξ of their total assets. In the event of default, they are restricted to investing at the risk-free rate r_t. Departing from Gertler and Kiyotaki (2010), we model a small open economy—reflecting Belgium's financial openness—where banks can access international borrowing. While banks do not default in equilibrium, the threat of default imposes an endogenous leverage constraint:

$$(1-\phi_{t+1})(1+r_{\ell,t+1})L_{t+1} \geq (1+r_{t+1})B_{t+1},$$

with ϕ_t defined recursively as:

$$\phi_t = \xi^{1-\beta_L}\left(\frac{1+r_{t+1}}{1+r_{\ell,t+1}} - (1-\phi_{t+1})\right)^{\beta_L}.$$

If asset diversion is not permitted (i.e., $\xi = 0$), the spread between the lending rate $r_{\ell,t}$ and the borrowing rate r_t collapses, preventing banks from accumulating net worth.

5.4 Government:

The government runs a pay-as-you-go pension system. Social security taxes are collected from working-age employed households and redistributed to retirees. We assume the government pension system is on a balanced-budget.

$$\sum_{j<J_R}\sum_{z}\tau_r y(j,z)\pi_j(z) = \sum_{j\geq J_R}\sum_{z} y_R(j,z)\pi_j(z)$$

where $\tau_r y(j,z)$ denotes the social security tax paid by individuals of age j with income shock z, $y_R(j,z)$ represents the pension income received by retirees with income shock z, $\pi_j^e(z)$ is the measure of employed individuals of age j experiencing income shock z, and $\pi_j(z)$ is the measure of all individuals of age j experiencing income shock z.

[10] Although the lending portfolio is more nuanced in practice—due to household heterogeneity and varying default risks—the assumptions of no aggregate risk and the law of large numbers imply that intermediaries earn identical returns across loans. This justifies the aggregation of the loan portfolio, as shown in Arslan et al. (2025).

5.5 Equilibrium Conditions

Labor Market In equilibrium, total labor demand N_t must equal the effective labor supply from the working-age population. Since the total working-age population is normalized to 1, we have:

$$N_t = 1.$$

Asset Market The total financial assets held by households, A_t, must equal the total demand for assets in the economy. This includes firms' capital demand K_t and the value of assets held by rental companies, denoted by $V_t^{rc}(H_{t-1}^r)$, which represents the present discounted value of their profits:

$$A_t = K_t + V_t^{rc}(H_{t-1}^r).$$

Loan Market Let $\Gamma_t(\theta)$ denote the distribution of all outstanding mortgages at the beginning of period t, and let $\ell_t(\theta)$ represent the mortgage loans held by financial intermediaries at that time. The loan market clearing conditions are:

$$\begin{aligned} \ell_t(\theta) &= \Gamma_t(\theta), \\ L_{t+1} &= \int_\theta p_t(\theta)\Gamma_t(\theta), \end{aligned}$$

The first equation ensures that the representative financial intermediary holds the equilibrium mortgage portfolio. The second equation implies that the intermediary is indifferent between all loans originated to households, and it determines the equilibrium lending rate $r_{\ell,t+1}$.

Housing Market Total housing supply is fixed and must equal total housing demand:

$$\begin{aligned} H &= \int_\theta h_t(\theta)\, d\Gamma^h(\theta) + H_t^r, \\ H_t^r &= \int_\theta h_t(\theta)\, d\Gamma^r(\theta), \end{aligned}$$

where Γ^h is the conditional distribution of homeowners, Γ^r is the conditional distribution of renters.

The first equation determines the equilibrium house price p_t^h, while the second determines the rental price p_t^r.

6 Model Calibration

A model period is a year. The model is calibrated to Belgium targeting the averages of 2010 to 2019. Tables 2 and 3 present externally set and internally calibrated parameters under the columns labeled External and Internal respectively.

Table 2: Externally Set Parameters

Parameter	Explanation	Value
σ	risk aversion	2
α	capital share	1/3
ρ_ε	annual persistence of income	0.96
σ_ε	annual std of innovation to AR(1)	0.17
φ_h	selling cost for a household	7%
φ_e	selling cost for foreclosures	25%
φ_f	fixed cost of mortgage origination	3%
φ_m	variable cost of mortgage origination	0.75
δ_h	annual housing depreciation rate	2.5%
π	annual prob. of being an active renter	14%
$\bar{H}$	housing supply	1
χ	down payment requirement	10%
δ_k	annual capital depreciation rate	10%

Demographics and Preferences: We assume individuals are born into the economy at the age of 25, work until age 64, retire at age 64 and die at age 84. We assume that households receive utility from consumption and housing services captured by the following Cobb-Douglas utility specification:

$$u(c,s) = \frac{\left(c^{1-\gamma}s^{\gamma}\right)^{1-\sigma}}{1-\sigma}.$$

Following the literature, we set $\sigma = 2$, which implies an elasticity of intertemporal substitution of 0.5. We calibrate γ to match the share of housing services in GDP as 9 percent and the discount factor β to match the capital-output ratio of 2.[11]

Income Process: We set the deterministic component of labor efficiency such that labor income growth doubles from age 25 to 55 and drops by 10% between age 55 and 65. We normalize the average efficiency for the working population is 1.

[11] See https://dataexplorer.nbb.be/

We set the annual persistency of the income process as 0.96 and standard deviation of the innovations to the log of the labor efficiency as 0.17. The retirement system is modeled as in Guvenen and Smith (2014), where we set the average retirement replacement rate as 60% as reported in OECD (2025).

Housing Markets: Housing transactions and mortgage issuances are costly. We set the selling cost of the house, φ_h, as 7% to replicate the typical real-estate agent cost and moving costs for a seller. The price discount for foreclosed properties, φ_e is set to 25% following Campbell et al. (2011). The fixed cost of mortgage origination, φ_f, is set to 3% of the annual output. These costs represent the typical title costs, attorney fees, appraisal fees charged at the time of mortgage origination. The variable cost of mortgage origination, φ_m, is set to 0.75%, which represents typical lender's fees. The minimum downpayment to get a mortgage, ϕ, is set to 10%, which allows for a loan-to-value ratio up to 90% at the mortgage origination.

The probability of an inactive renter to become an active renter, π, is set to 14% to replicate the fact the default credit flags stay on the credit history, on average, around 7 years. The housing depreciation rate, δ_h, is set to 2.5%. We normalize the total housing stock, $\bar{H}$, to be 1. We use the minimum house size, $\underline{h}$, to match the homeownership rate of 71%.

Financial Sector: We calibrate the discount factor of the financial intermediaries, β_L, to match an annual lending premium $r_\ell - r$ of 1.5% as reported by ECB, financial intermediaries' asset seizure rate, ξ, to match leverage ratio of 10, borrowing rate of the financial intermediaries, r_t, to match the mortgage debt to annual GDP ratio of 0.5.[12]

Production Sector: The capital share in production, α, is set to 1/3 and the capital depreciation rate, δ_k, is set to 10%.

Rental Companies: We calibrate the rental maintenance cost, κ, to match the house price to rental price ratio of 9 consistent with Belgium data reported in European Commission 2019 report.

[12] See https://data.ecb.europa.eu/

Table 3: Internally Calibrated Parameters

Parameter	Explanation	Value
β	discount factor	0.96
$\underline{h}$	minimum house size	0.85
r	bank borrowing rate	4.5%
γ	weight of housing services in utility	0.13
κ	rental maintenance cost	0.05
ξ	bank seizure rate	0.20
β_L	bank discount factor	0.82

7 Model Results

Table 4 presents the fit of the model. The model matches all the targeted moments. Although all parameters affect each moment, there is a clear relation between each calibrated parameter and one of the calibrated moments which help us match the moments perfectly.

We employ the structural model to quantify the full impact of Belgium's mortgage debt moratorium policy. As discussed in Section 4, our empirical model captures only partial equilibrium effects and therefore does not reflect the full impact of the policy. To address this limitation, we turn to our structural framework, which incorporates both partial and general equilibrium dynamics.

Table 4: Calibrated Moments

Moment	Data	Model
Capital to GDP ratio	2.0	2.0
Housing services share in GDP	0.09	0.09
Homeownership Rate	0.71	0.71
Mortgage Debt to GDP ratio	0.5	0.5
House Price to Rent ratio	9.0	9.0
Mortgage Premium	1.5%	1.5%
Bank Leverage	10	10

To simulate the economic environment in Belgium around 2020, we introduce an unexpected, temporary 5% productivity shock. This shock follows an AR(1) process with an annual persistence parameter of 0.4. We assume perfect foresight after the shock. We then re-solve the model, this time incorporating the debt suspension policy as implemented in Belgium, to assess its broader macroeconomic effects.

7.1 Benchmark Results

We begin by examining how an exogenous aggregate productivity shock propagates through the economy. Figure 5 illustrates the response of key macroeconomic variables to an unexpected 5% decline in aggregate productivity. This initial shock reduces aggregate output on impact.

The decline in productivity reduces labor demand, leading to a 5% drop in wages. Lower labor income diminishes household purchasing power, which in turn reduces non-durable consumption, demand for housing services, and savings. Aggregate non-durable consumption and household assets fall by a little more than 1.5%. The contraction in housing demand due to lower wages causes house prices to fall by 1%.

The change in the value of long-term mortgages owned by banks reduces their net worth slightly. Although this decline in bank net worth tightens credit supply, the contraction in credit demand—driven by weaker housing demand—dominates. As a result, lending rates decrease slightly, by about 10 basis points. However, the increase in mortgage demand, to smooth consumption, increase the lending rate in the following periods.

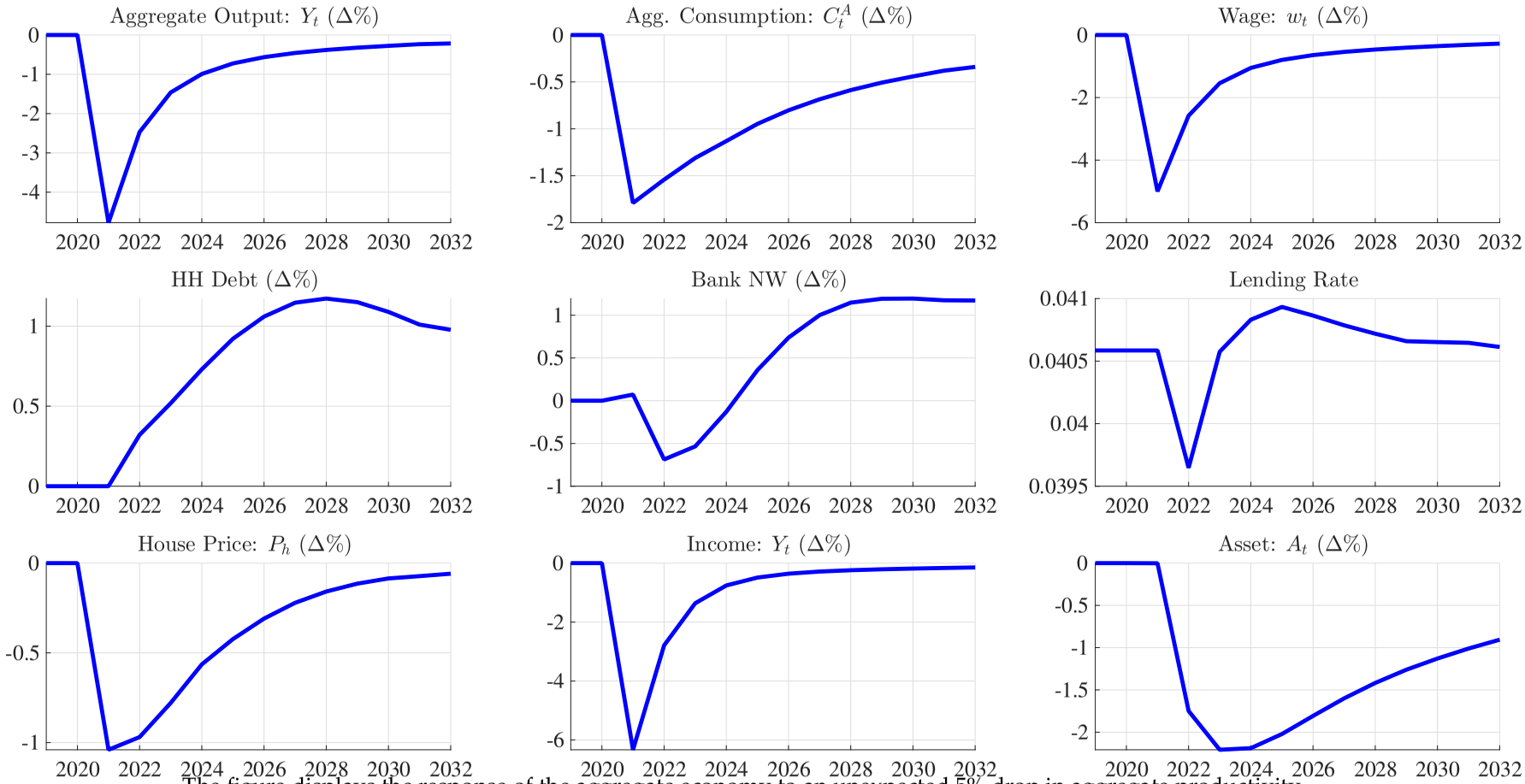
7.2 Debt Suspension Policy

Next, we analyze the implementation of a mortgage debt moratorium policy, modeled after the program introduced in Belgium in 2020. In our framework, the government suspends mortgage payments for one period for borrowers whose financial assets is less than 62% of average household income at the time of the shock. This threshold is chosen to reflect the actual policy, which restricts eligibility for households with the total movable assets in current and savings accounts and in an investment portfolio to be less than EUR 25,000.[13] Consistent with the real-world policy, the moratorium applies only to existing mortgage holders, and interest continues to accrue on the principal during the suspension period. In the data, 12.6 billion Euros of mortgage debt, which corresponds to 6% of outstanding mortgage debt in Belgium in 2020 was covered by the policy. In the model, this ratio is 8%, which closely matches the size of the policy in the data. We assume mortgage payments resume in the following period and the interest accrues on the existing debt to reflect the missed payments.

Figure 6 compares the evolution of key macroeconomic aggregates with and without the policy intervention. The most immediate effects are observed in mortgage debt, bank net worth, and the lending rate. As households pause mortgage payments, outstanding

[13] Average household income in Belgium in 2020 was EUR 40,000.

Figure 5: Benchmark Results



The figure displays the response of the aggregate economy to an unexpected 5% drop in aggregate productivity.

mortgage debt remains higher than in the scenario without the policy. However, missed mortgage payments reduce the cash flows banks are supposed to receive, leading to a slight decline in banks' net worth. As a consequence, credit supply contracts and the lending rate increases.

At the same time, the suspension of payments improves households' balance sheets by enhancing liquidity and alleviating forced house sales. Improved liquidity allows households to smooth consumption more effectively and mitigates the decline in housing demand. As a result, aggregate consumption slightly increases and the decline in house prices is attenuated.

Although the policy slightly weakens banks' balance sheets, its effects on the aggregate economy remain limited, and the policy largely achieves its intended goal of stimulating economic activity, albeit with modest overall effects.

7.3 Model meets Data: Empirical vs. Model-Implied Elasticities

To compare the empirical responses to the quantitative model, we summarize both in a scale-free elasticity of consumption with respect to mortgage payment relief.

Elasticity measure

We define the elasticity of consumption with respect to mortgage payments as

$$\varepsilon_{c,m} \equiv \frac{\Delta c / \bar{c}}{\Delta m / \bar{m}}, \tag{4}$$

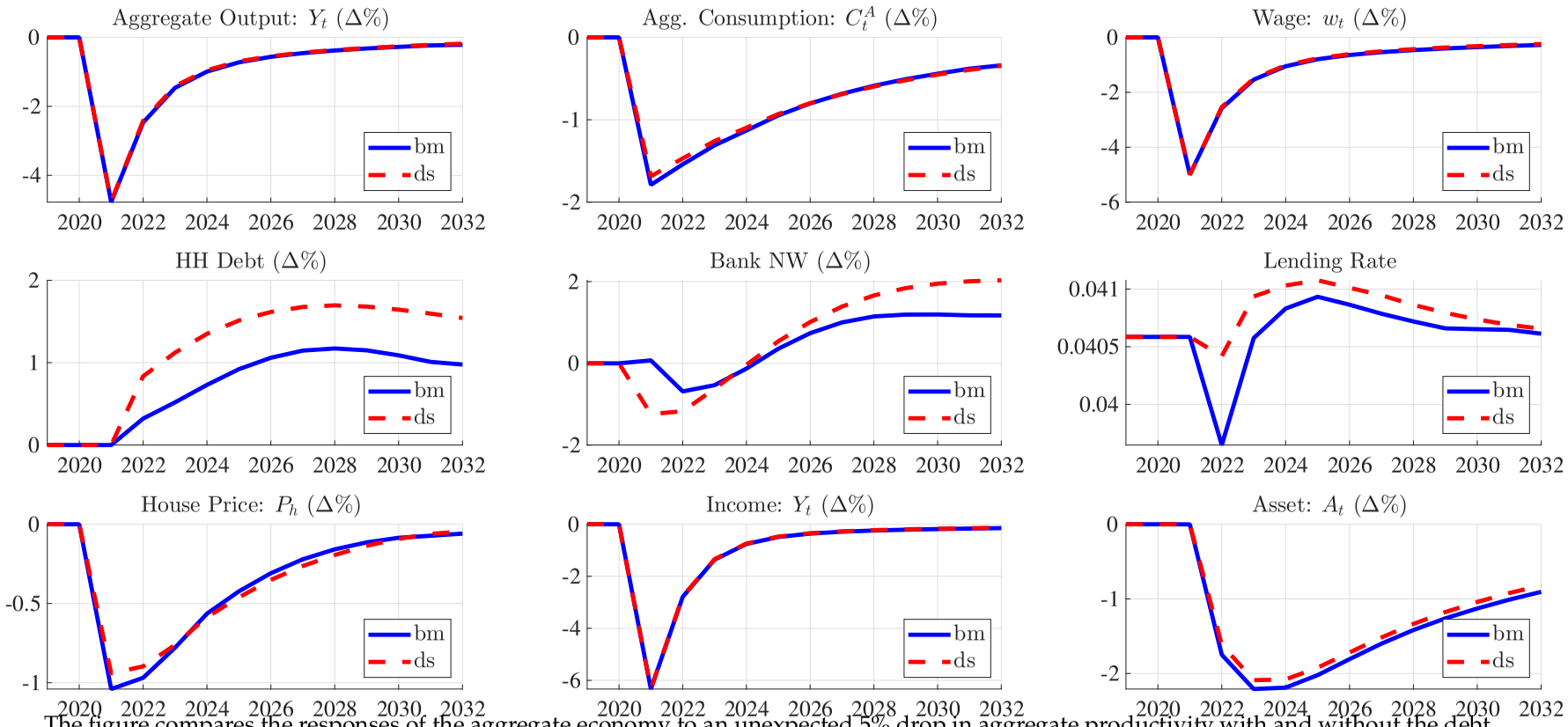
where Δc denotes the average change in non-durable and services spending over 2020Q2–2020Q4 (the policy application window), $\bar{c}$ is baseline consumption (2019), Δm is the corresponding change in mortgage payments induced by eligibility for the policy, and $\bar{m}$ is the average mortgage payment level in 2019. This elasticity captures the percent change in consumption associated with a one-percent change in mortgage payments.

Empirical elasticity (reduced-form, ITT)

Our event-study DiD identifies the *intention-to-treat* (ITT) effect of eligibility on both consumption and mortgage payments. Using the estimated ITT effects during 2020Q2–2020Q4 and baseline levels, we obtain

$$\varepsilon_{c,m}^{\text{data}} = \frac{\frac{298.15}{7066.49}}{\frac{-231.58}{2844}} = -0.52. \tag{5}$$

Figure 6: Debt Suspension



The figure compares the responses of the aggregate economy to an unexpected 5% drop in aggregate productivity with and without the debt suspension policy described in the main text. The solid blue line plots the benchmark economy (labeled as "bm") and the dashed red line plots the economy with debt suspension policy (labeled as "ds").

Thus, in the data, a 1% decline in mortgage payments is associated with a 0.52% increase in non-durable and services consumption during the policy period.

Model elasticity (partial equilibrium)

In the quantitative model, we conduct an analogous *partial-equilibrium* experiment by comparing two simulated economies that differ only in the presence of the moratorium, holding prices (house prices, rents, wages, and lending rates) fixed at their benchmark paths.[14] Letting Δc^M and Δm^M denote the model-implied changes for households eligible for the policy, the model elasticity is

$$\varepsilon_{c,m}^{\text{model}} \equiv \frac{\Delta c^M / \bar{c}^M}{\Delta m^M / \bar{m}^M} = -0.09. \tag{6}$$

Interpretation and comparability

Two features of the Belgian moratorium imply that $\varepsilon_{c,m}^{\text{data}}$ and $\varepsilon_{c,m}^{\text{model}}$ need not coincide mechanically. First, take-up was opt-in and incomplete. Second, even among eligible borrowers, observed payment relief in the data reflects a mix of full suspensions, partial payments, and informal repayment adjustments. By contrast, the model experiment applies a stylized payment suspension to all households that satisfy the model eligibility rule.

For transparency, we therefore interpret $\varepsilon_{c,m}^{\text{data}}$ as the *policy-as-implemented* reduced-form elasticity (eligibility-induced changes in spending relative to eligibility-induced changes in payments). The model elasticity is a *full-compliance, partial-equilibrium* object under the model's stylized implementation rule. The fact that the model delivers the correct sign and a smaller magnitude indicates that the model's liquidity/constraint channel generates a consumption response to payment relief, but that the empirical response is stronger—consistent with selection into take-up by high-MPC households and/or additional margins present in the data but abstracted from in the model. We view this comparison as informative about orders of magnitude, while the distributional validations in Figures 8–9 provide sharper out-of-sample discipline on *who* responds and how consumption dispersion evolves.

[14] To mimic the empirical setup, we run the counterfactual with the debt suspension policy by keeping all prices (house prices, rents, wages, and lending rates) as in the benchmark economy.

7.4 General Equilibrium Effects

Comparison of the benchmark economy and the economy with the debt suspension includes both partial equilibrium and general equilibrium effects. The empirical evidence we provide in Section 4 measures only the partial equilibrium effects as the effect of the policy on the equilibrium prices will be muted due to the empirical design. As we show in Figure 6, debt suspension policy lowers the drop in house prices and wages, and increases the drop in the lending rate. To highlight the significance of the general equilibrium effects, we solve the economy with debt suspension policy by feeding all the prices (house prices, wages, lending rates, rental prices, capital rental rate) and taxes as in the benchmark economy and solve for the transition.

Figure 7 plots the comparison of the changes in consumption, mortgage debt, financial assets, and home-ownership rate across the benchmark economy, the economy with debt suspension policy, and the economy with debt suspension policy and benchmark prices. The figures show that general equilibrium effects amplify the effects of the policy. The policy not only has direct effects on household-level variables, but also mitigates the negative effects of the policy on consumption and homeownership rates by lowering the decline in house prices and wages.

The consumption in the benchmark model drops by 1.79%. The direct effect of the policy lowers the drop in consumption to 1.73%. However, fully accounting the general equilibrium effects lowers the drop in the consumption to 1.68%. The full effects of the policy have substantially different effects than the direct partial equilibrium effects of the policy.

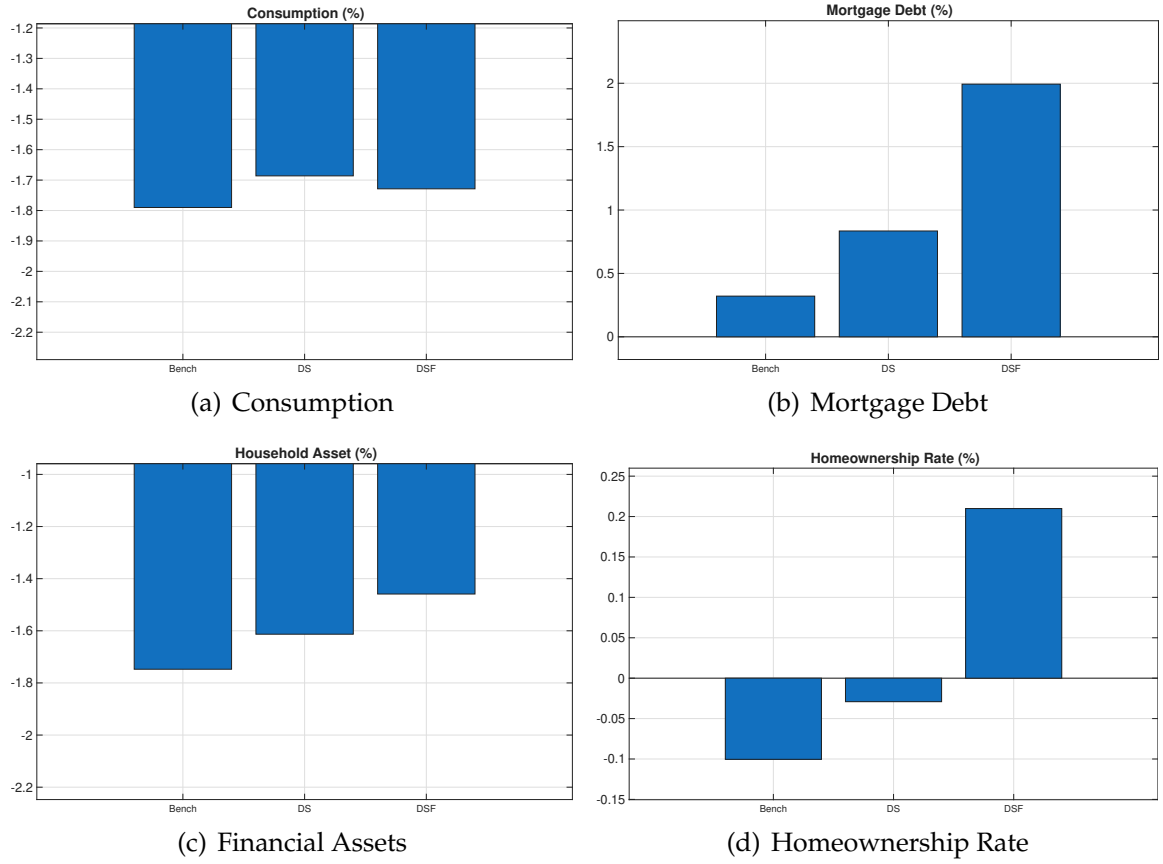
7.5 Out-of-sample validation: heterogeneity and inequality

The quantitative model is disciplined by aggregate moments and by the average consumption response to payment relief. Importantly, we do *not* target the cross-sectional gradients and distributional dynamics documented in the empirical event-study figures. Figures 8–9 therefore provide an out-of-sample assessment of the model: conditional on matching the average response, does the model also reproduce (i) *who* responds more to the policy and (ii) *how* consumption dispersion evolves?

7.5.1 Heterogeneous Effects of the Policy

While the debt suspension policy alleviates liquidity problems and mitigates the adverse effects of the shock, it has heterogeneous effects across the population. Figure 8

Figure 7: General Equilibrium Effects



(a) Consumption (b) Mortgage Debt

(c) Financial Assets (d) Homeownership Rate

The figure plots the comparison of the household responses of consumption, mortgage debt, financial assets, and homeownership rates across different economies. In each figure, the left bar shows the response in the benchmark economy. The middle bar shows the response in the economy with debt suspension policy. The right bar shows the response in the economy with debt suspension policy with benchmark prices.

illustrates these heterogeneous responses for consumption.[15] The results highlight that the aggregate averages mask substantial variation: vulnerable groups such as renters, young households, and low-wealth bear the largest burdens from the shock, and they also benefit most from the policy.

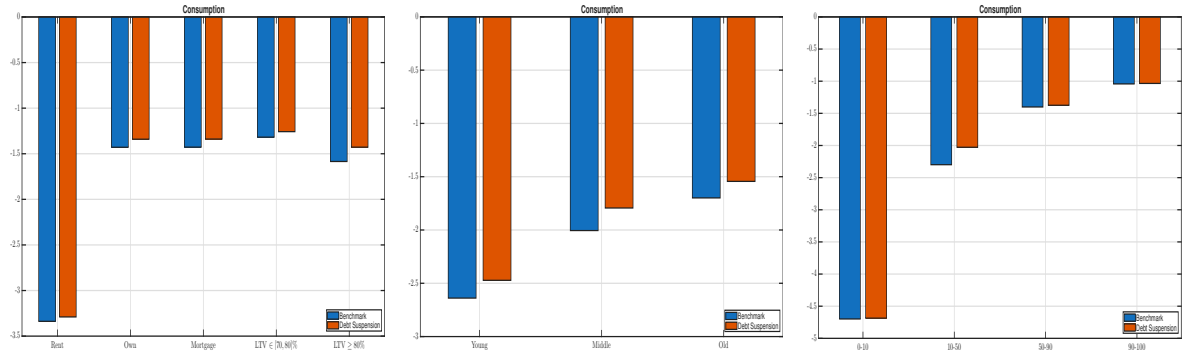
To construct these figures, we classify individuals according to their status right before the shock. Then, to eliminate the life-cycle effects, we compare the changes in their consumption compared to the economy without any shock. That is, each bar shows the percentage change in consumption for a certain group, labeled right before the shock, compared to the economy without any shock.

Figure 8 plots the heterogeneous consumption responses to the shock and the policy. On average, consumption falls by 1.8%, but this masks considerable heterogeneity.

[15] Appendix K.1 reports the heterogeneous responses for further variables.

- *Housing status.* Renters face the sharpest decline (around 3.5%), followed by highly leveraged mortgage holders (1.6%), while outright owners are less affected (about 1.4%). Among mortgage holders, those with loan-to-value ratios above 80% experience declines exceeding 1.6%, reflecting acute liquidity constraints. The moratorium has small effects on outright owners and renters, but it cushions high-LTV borrowers the most.
- *Age.* Young households lose over 2.5% of consumption compared to less than 1.7% among the old. The policy increases consumption of the young and middle by roughly 0.2%, compared to 0.15% for older households.
- *Net worth.* Households in the bottom decile of the wealth distribution experience more than 4.5% drop in consumption, while those in the top decile lose around 1%. The largest gains from the policy accrue to households in the 10–50th percentiles, who are most likely to hold mortgages, whereas benefits for the very poor and the very wealthy are minimal.

Figure 8: Heterogeneity in Consumption Response



(a) Housing Status (b) Age (c) Net Worth

The figures compare the percentage deviation of consumption during the bust period in the economy with debt suspension policy and the benchmark economy across individuals with different characteristics.

Taken together, these figures underscore that the policy's benefits are highly progressive and heterogeneous. Mortgage moratoria do not eliminate risk or fully offset the consumption losses of all households, but they disproportionately protect those most vulnerable: the young, the low-wealth, and households with high-LTV mortgages. By preventing concentrated distress at the bottom of the distribution, the policy enhances both financial stability and aggregate demand. Next, we will study its effects on inequality.

Out-of-sample heterogeneity evidence: (Figure 8 versus Figure 2) Figure 8 mirrors the empirical heterogeneity patterns in Figure 2. In the data, the consumption response is largest for more constrained households (e.g., high-LTV borrowers and younger house-

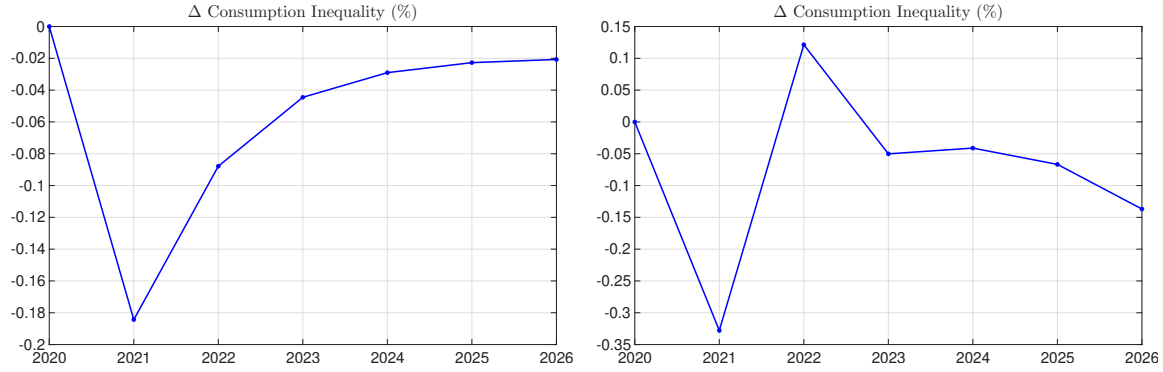
holds). Without being targeted, the model generates the same qualitative ranking: following the aggregate shock, consumption falls most for households with tight balance sheets, and the moratorium attenuates these losses disproportionately for high-MPC borrowers, consistent with a liquidity/constraint mechanism.

7.5.2 Inequality

As discussed in Section 4.2, the policy reduces consumption inequality among eligible individuals. To examine this effect within the model, we classify individuals with mortgages exceeding 80% loan-to-value (LTV) in the steady state as "eligible." We then compare the consumption patterns of this group in both the benchmark economy and the economy with the debt suspension policy.

Figure 9 illustrates the percentage deviation in consumption inequality under the debt suspension policy relative to the benchmark. The left panel shows the variance of consumption, while the right panel presents the 50th-to-10th percentile ratio of the consumption distribution. Both metrics indicate a consistent outcome: the debt suspension policy significantly reduces consumption inequality, particularly at the lower end of the distribution, where its effects are most pronounced.

Figure 9: Consumption Inequality



(a) Variance (b) 50/10 Ratio

The figures compare the percentage deviation of the consumption inequality among the distressed individuals (those who have mortgages with more than 80% LTV) in the economy with debt suspension policy compared to the benchmark economy without the policy. Inequality is measured as the variance of consumption on the left and ratio of the 50th to 10th percentile of consumption distribution on the right.

Out-of-sample inequality evidence: (Figure 9 versus Figures 3 and 4). Because income risk and hence income inequality are exogenous in the model, we do not interpret the

model as matching the empirical *income*-inequality series. Instead, we use the model to speak to the distribution of *consumption*, which is endogenous through households' balance sheets and borrowing constraints. In the data, eligible households exhibit a smaller increase in consumption dispersion after the policy, including in lower-tail measures (Figures 3 b and 4b). The model reproduces this qualitative pattern out of sample: conditioning on distressed/high-LTV households, the moratorium generates a persistent decline in consumption inequality, with pronounced effects on tail-sensitive statistics (Figure 9). These distributional dynamics are not imposed; they arise endogenously from the same mechanisms that govern the heterogeneous consumption responses in Figure 8.

7.6 Counterfactual Policies

Expanding Coverage: The benchmark debt suspension policy is restricted to households with total assets below a specified threshold. Under this criterion, only 6% of households qualify for the policy, and the total amount of mortgage debt covered represents 8% of the outstanding mortgage debt. Importantly, the policy incurs no direct fiscal cost, as mortgage interest continues to accrue during the suspension period. Consequently, it imposes no immediate financial burden on any agents in the economy. However, as demonstrated in previous sections, it serves as an effective demand stabilizer, mitigating the adverse effects of the shock.

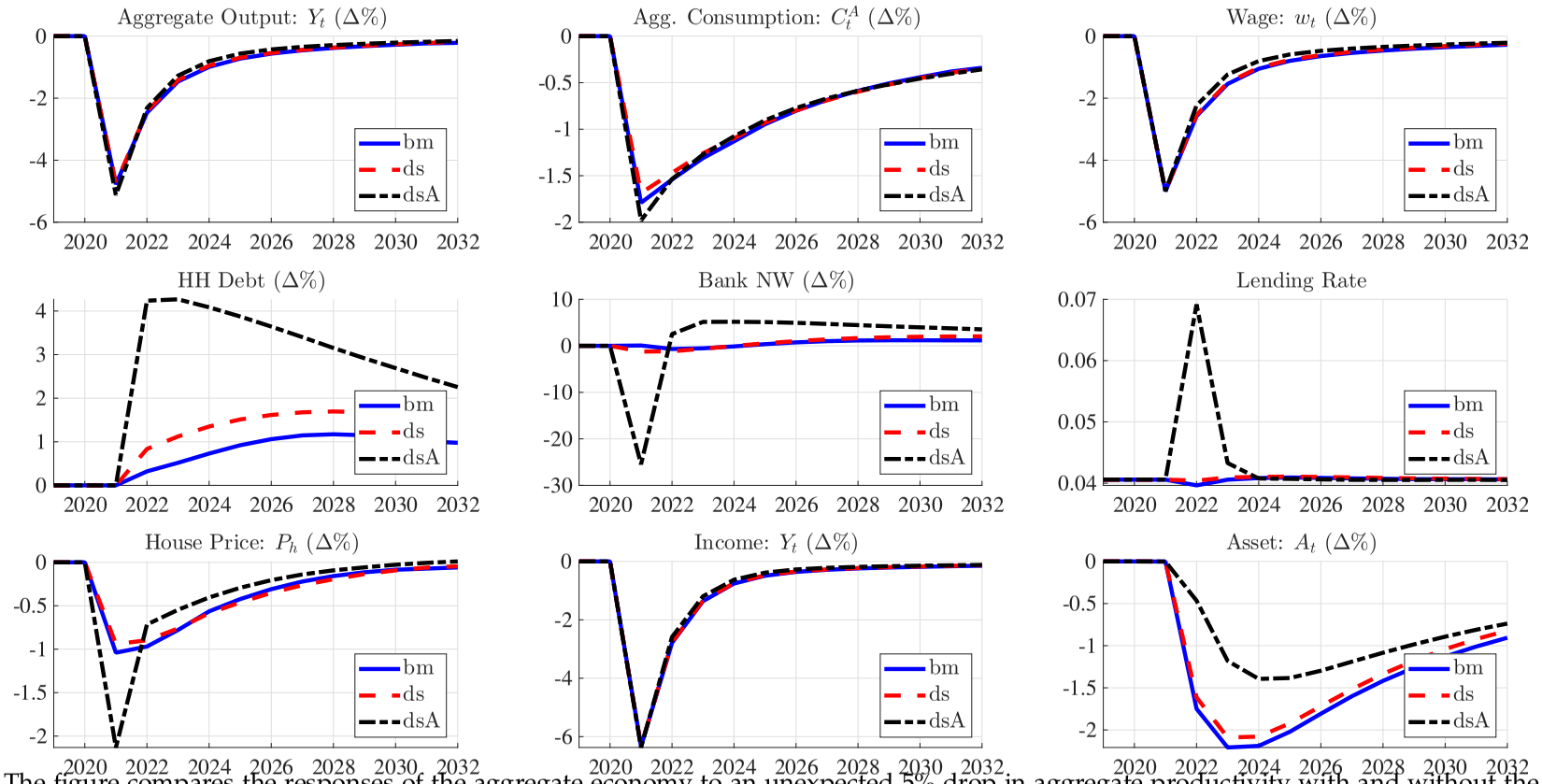
We now examine a counterfactual policy in which the eligibility criteria are removed, allowing all mortgage holders to benefit. Under this scenario, all existing mortgage holders receive support.

As shown in Figure 10, expanding the coverage has adverse effects on the economy. The positive impacts on output and consumption observed under the benchmark policy are reversed. This negative outcome is primarily driven by the deterioration of banks' balance sheets. The decline in bank net worth leads to higher lending rates, which raise borrowing costs for households. These effects suppress consumption and housing demand, ultimately resulting in lower house prices and reduced consumption.[16].

Policy Duration While the benchmark debt suspension policy is short-lived, it still generates significant positive effects on the aggregate economy. A natural question arises: does extending the duration of the policy amplify its benefits?

[16]We arrive to a similiar conclusion when we study two different policy: (1) suspending mortgage payments without interest rate accrual and (2) forgiving part of the debt. Results are presented in Appendix K.2

Figure 10: Policy Comparison



The figure compares the responses of the aggregate economy to an unexpected 5% drop in aggregate productivity with and without the debt suspension policy described in the main text. The solid blue line (labeled as "bm" plots the benchmark economy), the dashed red line (labeled as "ds") plots the economy with benchmark debt suspension policy, long dashed black line (labeled as "dsA") plots the economy with alternative debt suspension policy which allows all mortgage holders to qualify.

To explore this, we consider two extended versions of the debt suspension policy: one lasting two periods and another lasting four periods. Figure 11 presents the comparative results. Extending the policy to two or four periods increases the positive effects of the policy on consumption and house prices. However, they come at the cost of worsened bank balance sheets and longer recovery from the crisis due to higher prolonged borrowing costs for households.

7.7 Discussion of the Quantitative Results

Our findings suggest that mortgage moratoria can be effective tools for stabilizing demand and mitigating the adverse effects of economic shocks. However, this raises an important question: if such policies are beneficial, why are they not more widely implemented?

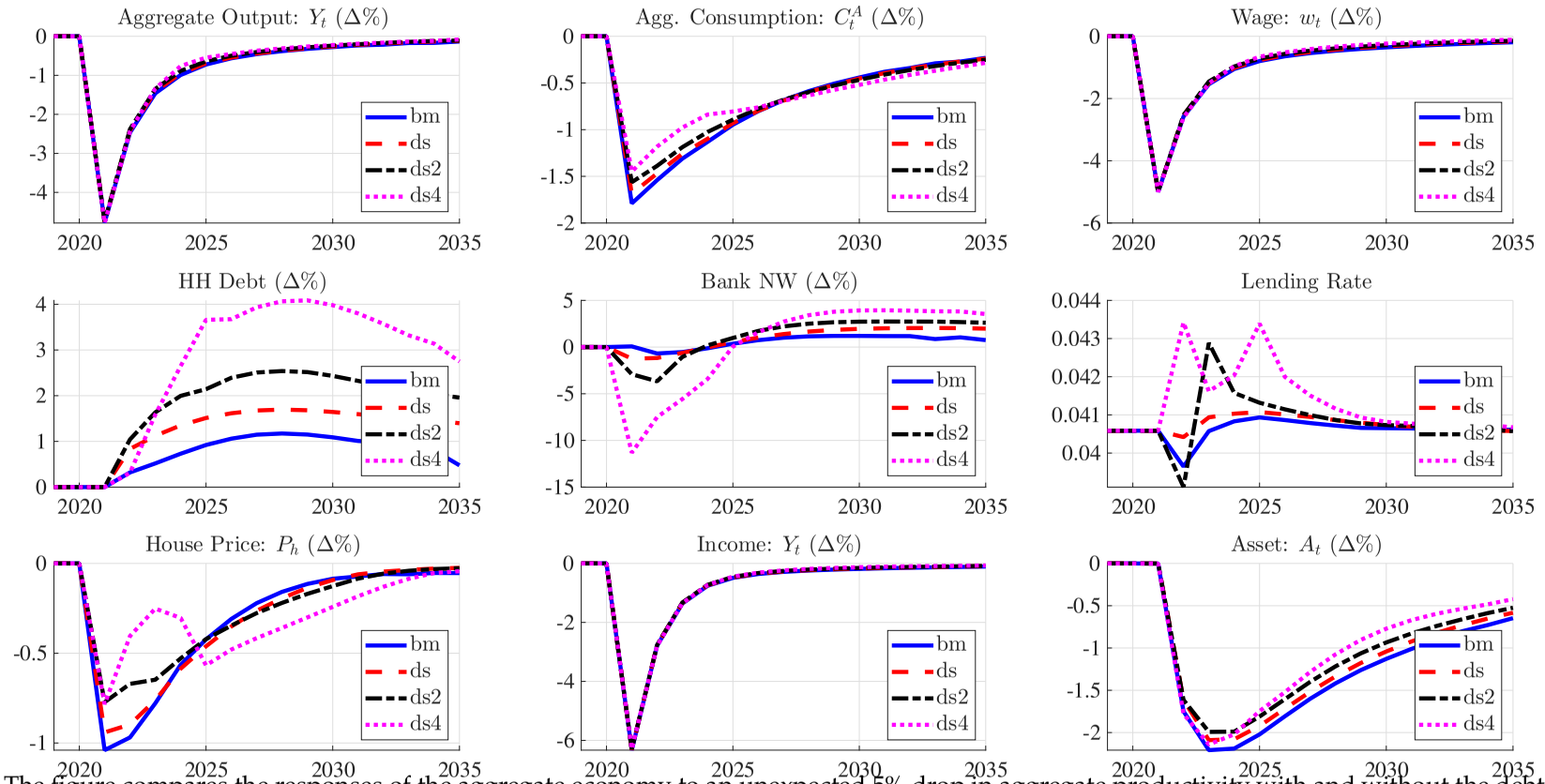
One plausible explanation lies in concerns about moral hazard. A key limitation of our analysis is that the mortgage moratorium is modeled as an unexpected, one-time intervention. Agents in the economy do not anticipate the policy, and therefore do not adjust their behavior in advance. In reality, if households and banks expect such policies to be implemented in future downturns, their behavior could change in ways that our model does not capture—potentially leading to riskier borrowing or lending practices.

Despite this limitation, our analysis highlights important dynamics arising from the interaction between household and bank balance sheets. As demonstrated through the counterfactual scenarios, the positive effects of debt suspension policies on households can be offset—or even reversed—by their negative impact on banks. This underscores the importance of considering the broader financial system when designing and evaluating stabilization policies.

8 Conclusions

We study the macroeconomic and distributional effects of a large-scale mortgage moratorium introduced during the COVID-19 crisis in Belgium. Using high-frequency credit and transaction data from the country's largest bank, and exploiting a policy-defined eligibility discontinuity that we map into month-end delinquency bins, we estimate the effect of payment suspension on household behavior using an event-study DiD design. Eligible borrowers reduced their mortgage payments while exhibiting a delayed but sustained increase in consumption, particularly on services, once pandemic-related restrictions eased.

Figure 11: Debt Suspension Duration



The figure compares the responses of the aggregate economy to an unexpected 5% drop in aggregate productivity with and without the debt suspension policy described in the main text. The solid blue line plots the benchmark economy (labeled as "bm"), the dashed red line plots the economy with one period debt suspension policy (labeled as "ds"), long dashed black line plots the economy with two periods debt suspension policy (labeled as "ds2"), and dotted pink line plots the economy with four periods debt suspension policy (labeled as "ds4").

Our analysis also reveals heterogeneous impacts of the policy. The largest positive effect on consumption is concentrated among households with high LTV ratios, young borrowers, and moderate savings households—those most likely to be financially constrained. Additionally, the moratorium policy decreased both consumption and income inequality, highlighting the potential of debt relief to serve as a redistributive and protective instrument, even in the absence of full policy take-up.

To assess the broader implications of such a policy, we develop a heterogeneous-agent general equilibrium model with long-term, defaultable mortgages and income risk. The model replicates the empirical response of consumption to mortgage relief and reveals that general equilibrium feedback—through housing markets and bank balance sheets—significantly amplifies the effects. Mortgage moratoria not only stabilize household balance sheets but also mitigate declines in house prices, and support aggregate demand, all without direct fiscal transfers. However, broad coverage dilutes these benefits by weakening banks' balance sheets and reducing consumption.

Taken together, our findings show that mortgage moratoria can act as powerful, fiscally neutral macro-stabilizers. By reallocating liquidity across time and households without increasing public debt, they offer a targeted and timely policy instrument for responding to large adverse shocks. These findings suggest that in future crises, policymakers can consider moratoria as a fiscally neutral complement to standard fiscal and monetary tools.

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Appendix A Household Pre-policy Financial and Demographic Characteristics

Table A1: Summary Statistics

	Eligible						Non-eligible						
	Mean	Std. Dev.	p^{25}	p^{50}	p^{75}	No. obs.	Mean	Std. Dev.	p^{25}	p^{50}	p^{75}	No. obs.	t-stats of Δ
Expenditure													
Non-durable & services	7254.56	8314.71	2486.98	5590.11	8603.09	830	6630.20	5205.68	3259.20	5125.90	8368.89	385	1.56
Non-durable	2382.7	3005.02	980.58	1868.80	2945.11	830	2081.10	190116	921.55	1679.75	2754.60	385	1.68
Services	4871.85	7257.38	1772.39	3415.14	5918.00	830	4549.57	4824.85	1601.35	3118.38	5576.76	385	1.65
Cash Withdrawal	1211.85	2306.97	16.18	574.67	1479.59	830	1084. 23	1458.025	107.07	574.27	1351.57	385	1.14
Personal Account													
Savings account balance	3753.91	6228.45	0.31	97.30	1845.18	830	4435.17	6434.39	0.1	54.02	1598	385	-0.26
Total saving balance	5082.05	21386.06	111.27	833.35	3779.39	830	4749.50	186945.06	254.92	886.30	3189.33	385	0.53
Labor Income													
Income	8888.85	7490.20	3602.77	8083.50	12372.60	830	8135.86	6133.27	3580.46	7664.63	11647.69	385	1.8
Demographic													
Household size	2.23	1.13	1	2	3	830	2.28	1.19	1	2	3	385	-0.52
Oldest age	44.53	9.57	38	44	51	830	44.99	9.81	38	45	52	385	-0.69
Wallonia region	0.26	0.44	0	0	1	830	0.25	0.43	0	0	1	385	0.35

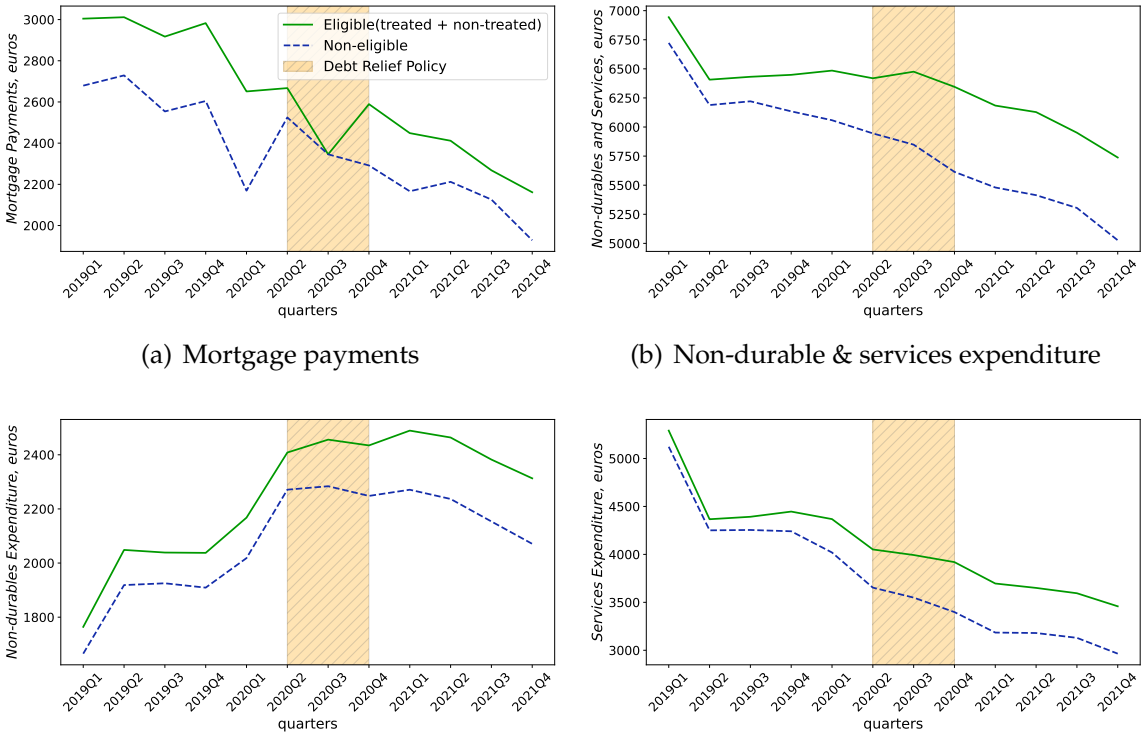
Authors' calculations. Characteristics of households in the eligible and non-eligible group, measured in 2019Q4. All variables are adjusted to CPI inflation. Variable *Oldest age* corresponds to the age of the oldest member in the household. The variable *Wallonia region* represents a fraction of households living in the Wallonia region. The last column presents the results of independent samples t-statistics measuring the statistical significance of means. T-critical value at 5% significance level equals 1.96.

Appendix B Trends in Mortgage Payments, Consumption and Labor Income

Figure B1 presents the evolution of averages of (a) mortgage payments, (b-d) consumption expenditure, and (e) labor income between eligible and non-eligible households. Visually, almost all trends evolve in parallel before the policy implementation, with the exception of services. Panels (b), (c), and (d) display consumption expenditure patterns. Non-durable and services expenditure (panel b), non-durable goods spending (panel c), and services (panel d) all show generally parallel trends before the policy period. Services begin to diverge slightly from 2019Q2, with more pronounced differences emerging after policy implementation, though formal parallel trend tests do not reject the parallel trends assumption.

Panel (e) shows labor income trends, which remain relatively stable and parallel between the two groups throughout the observation period.

Figure B1: Evolution of average mortgage payments and consumption expenditure in euros

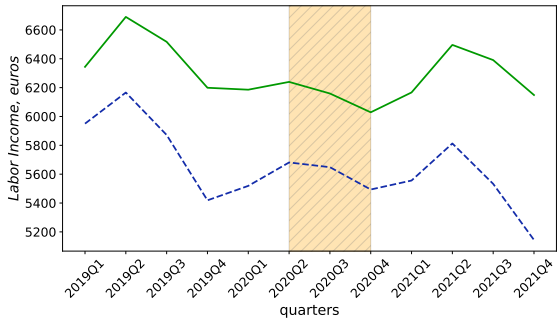


(a) Mortgage payments

(b) Non-durable & services expenditure

(c) Non-durable goods spending

(d) Services



(e) Labor income

Authors' calculations. The charts present comparison of trends of quarterly average mortgage payment in euros and consumption expenditures between eligible and non-eligible borrowers between 2019Q1-2021Q4. All variables are expressed in euros deflated by CPI index. Consumption expenditures and labor income are calculated as the 6-month moving average and aggregated to quarterly levels. Mortgage payments refer to the quarterly sum of monthly payments on mortgages that originated before the policy implementation.

Appendix C Parallel Trend Assumption

The charts display the estimated coefficients from equation 1 across all periods. The last quarter of 2019 is used as the baseline period, instead of 2020Q1, to avoid any confounding effects related to the onset of the COVID-19 pandemic. For all outcome variables, there are no statistically significant coefficients prior to the policy implementation, suggesting no meaningful differences between eligible and non-eligible borrowers before the intervention. This supports the validity of the parallel trends assumption.

Figure C2: The ITT effects of mortgage moratoria



(a) Mortgage payments

(b) Non-durable & services expenditure

(c) Non-durable goods spending

(d) Services

(e) Labor income

Authors' calculations. All variables are expressed in euros deflated by the CPI index. T corresponds to the 2nd, 3rd, and 4th quarter of 2020. Consumption expenditures and labor income are calculated as the 6-month moving average.

Appendix D Quarterly Breakdown of Intention-to-Treat Results

Table D2 presents a breakdown of the estimated intention-to-treat effects from the main analysis. Quantitatively, the results are robust and consistent with those reported in Table 1 in Section 4.

Table D2: Estimated Intention-to-Treat Effects of Moratoria Policy

	Mortgage Payments	**Non-durable& Services**	**Non-durable goods**	**Services**
Pre-Policy Period				
ITT_{2020Q1}	103.67	112.84	20.97	143.12
	(144.28)	(100.72)	(35.45)	(96.31)
Policy Period				
$ITT_{2020Q2\&Q3}$	-307.12**	236.65	26.20	122.54
	(121.42)	(159.79)	(62.50)	(1146.14)
ITT_{2020Q4}	-80.50	417.13**	58.06	193.28*
	(142.62)	(187.83)	(70.42)	(108.24)
Post-Policy Period				
ITT_{2021Q1}	-95.91	390.18*	89.93	186.13
	(155.74)	(208.77)	(78.03)	(115.93)
Time FE	Yes	Yes	Yes	Yes
HHs FE	Yes	Yes	Yes	Yes
Households	1215	1215	1215	1215

Authors' calculations. The estimated parameters represent the effect of the moratorium policy on mortgage payments and household spending between 2019Q1-2021Q4. Authors' calculations. 2019Q4 is the reference period not affected by policy and COVID-19 pandemic. The rest of the parameters for 2021 are omitted for clarity. All variables are in euros deflated by the CPI index. Consumption expenditures and labor income are calculated as a 6-month moving average. Mortgage payments refer to quarterly payments on existing loans originated before the policy. All columns control for households and time-fixed effects. Standard errors, clustered at the household level, are in brackets. Asterisks indicate significance levels: *.1, **.05, ***.01.* ∗ ∗.01.

Appendix E Heterogeneous ITT Effects Across the Wealth Distribution Across Household Characteristics

Table E3: Heterogeneous Intent-to-Treat Effects on Non-durable goods and Services Consumption

	LTV Ratio		**Household Age**		
	80–90	90+	Young	Middle	Old
	(1)	(2)	(3)	(4)	(5)
Pre-Policy Period					
ITT_{2019Q1}	-32.56	-227.84	192.43	-212.30	537.93
	(389.52)	(232.41)	(396.87)	(205.82)	(548.57)
ITT_{2019Q2}	-119.07	-188.41	-70.00	-149.36	604.72
	(309.14)	(208.81)	(406.09)	(173.84)	(485.41)
ITT_{2019Q3}	-27.49	-166.56	-203.75	-94.65	186.61
	(184.16)	(133.48)	(283.84)	(104.65)	(302.86)
ITT_{2020Q1}	-387.19	150.39	-22.73	170.83	-297.45
	(283.63)	(133.50)	(199.04)	(119.11)	(435.54)
Policy Period					
$ITT_{2020Q2\&Q3\&Q4}$	-433.94	271.86	650.12*	227.33	245.12
	(467.74)	(225.64)	(361.66)	(190.14)	(444.13)
Post-Policy Period					
ITT_{2021Q1}	-841.18	572.14**	962.04*	319.37	-103.22
	(662.31)	(281.10)	(518.62)	(239.52)	(577.68)
ITT_{2021Q2}	-894.87	657.81**	1022.7*	323.82	-509.04
	(653.44)	(304.30)	(568.01)	(252.51)	(654.07)
ITT_{2021Q3}	-645.23	662.74**	880.34	289.71	-759.85
	(586.35)	(313.03)	(568.83)	(253.35)	(788.99)
ITT_{2021Q4}	-133.99	778.57**	811.60	395.55	-844.49
	(503.79)	(314.84)	(602.22)	(248.93)	(761.10)
Time FE	Yes	Yes	Yes	Yes	Yes
HHs FE	Yes	Yes	Yes	Yes	Yes
Observations	1,776	8,016	2,352	11,280	936
Households	148	668	196	940	78

This table reports heterogeneous intent-to-treat (ITT) effects on real non-durable services consumption (moving average) across loan-to-value (LTV) ratio groups (columns 1–2) and household age groups (columns 3–5). ge groups are defined based on the age of the oldest household member: young (<35 years), middle-aged (35–60 years), and older (60+ years). Group thresholds were also chosen to ensure adequate sample sizes within each category. The omitted reference period is Q4 2019. All specifications include household and time fixed effects with clustered standard errors at the household level reported in parentheses. Asterisks indicate significance levels: *.1, **.05, ***.01.* ∗ ∗.01.

Table E4: Heterogeneous Intent-to-Treat Effects on Non-Durable Services Consumption by Savings Percentile

	Savings Percentile Group			
	0–10%	10–50%	50–90%	90–100%
	(1)	(2)	(3)	(4)
Pre-Policy Period				
ITT_{2019Q1}	-501.77	549.21	-671.95	888.37
	(289.08)	(300.60)	(362.35)	(507.63)
ITT_{2019Q2}	-352.07	276.25	-363.29	394.93
	(260.25)	(300.85)	(269.90)	(406.45)
ITT_{2019Q3}	-186.35	-138.25	-116.47	159.25
	(179.23)	(194.18)	(154.34)	(304.66)
Policy Period				
$ITT_{2020Q2-Q4}$	-494.92	929.25***	264.52	371.18
	(327.61)	(312.94)	(245.35)	(587.96)
Post-Policy Period				
ITT_{2020Q1}	-65.21	253.88	143.31	50.72
	(204.27)	(195.56)	(161.70)	(305.89)
ITT_{2021Q1}	-578.06	1394.5***	259.19	338.95
	(426.42)	(380.27)	(334.53)	(667.06)
ITT_{2021Q2}	-773.36*	1622.6***	192.32	403.73
	(435.57)	(398.07)	(366.23)	(713.17)
ITT_{2021Q3}	-1122.2***	1591.8***	319.66	129.53
	(431.95)	(410.76)	(374.97)	(757.85)
ITT_{2021Q4}	-822.99*	1565.9***	286.35	348.56
	(602.22)	(428.73)	(361.88)	(775.76)
Time FE	Yes	Yes	Yes	Yes
HHs FE	Yes	Yes	Yes	Yes
Observations	3,168	4,128	5,820	1,464
Households	264	344	485	122

This table reports heterogeneous intent-to-treat (ITT) effects on real non-durable services consumption (smoothed with 6-month moving average) across savings percentile groups. Savings are calculated as a sum of balances on the currant and saving accounts for 2019Q4. The omitted reference period is Q4 2019. All specifications include household and time fixed effects with clustered standard errors at the household level reported in parentheses. Asterisks indicate significance levels: *.1, **.05, ***.01.* * *.01.

Appendix F Robustness to Relaxing the Minimum Consumption Restriction

We examine the sensitivity of our results to relaxing the minimum consumption criterion. In the baseline, households must have monthly non-durable expenditure of at least €166 (inflation-adjusted). Here, we include all households with non-zero consumption between 2019 and 2021, increasing observations by approximately 67 percent. However, this may include households using their accounts infrequently, potentially attenuating estimates. Table F5 shows that results remain qualitatively similar to the main analysis, though coefficients are smaller, confirming robustness to this assumption.

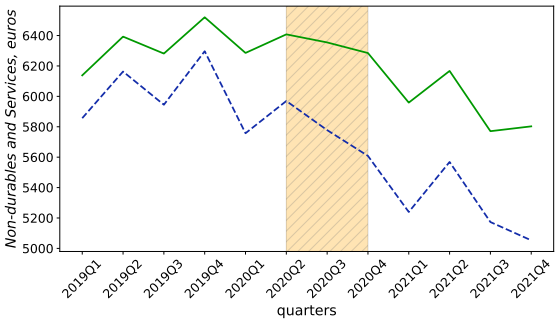
Table F5: Sensitivity Analysis: Relaxing the Minimum Consumption Restriction

	Mortgage Payment	**Non-durable & Services**	**Non-durable goods**	**Services**
Pre-Policy Period				
ITT_{2019Q1}	21.85	-110.47	-4.20	-93.86
	(103.62)	(133.89)	(45.52)	(117.64)
ITT_{2019Q2}	-65.22	-105.46	1.97	-83.10
	(102.64)	(117.63)	(40.47)	(100.82)
ITT_{2019Q3}	74.80	-89.26	-13.86	-70.72
	(120.04)	(70.30)	(24.39)	(61.70)
ITT_{2020Q1}	76.43	118.88	15.08	63.22
	(124.83)	(77.00)	(26.99)	(62.27)
Policy Period				
$ITT_{2020Q2\&3\&4}$	-207.81**	219.93*	61.73	131.30
	(90.45)	(120.47)	(47.56)	(95.99)
Post-Policy Period				
ITT_{2021Q1}	-97.57	351.76**	90.09	246.71**
	(120.29)	(148.28)	(59.78)	(119.45)
ITT_{2021Q2}	-161.29	300.74*	90.21	214.16*
	(118.30)	(159.11)	(62.91)	(124.19)
ITT_{2021Q3}	-196.67	212.23	77.21	137.66
	(139.68)	(162.58)	(66.27)	(124.41)
ITT_{2021Q4}	-123.90	204.58	92.53	130.21
	(117.99)	(166.98)	(64.11)	(128.19)
Time FE	Yes	Yes	Yes	Yes
HHs FE	Yes	Yes	Yes	Yes
Households	2,041	2,041	2,041	2,041

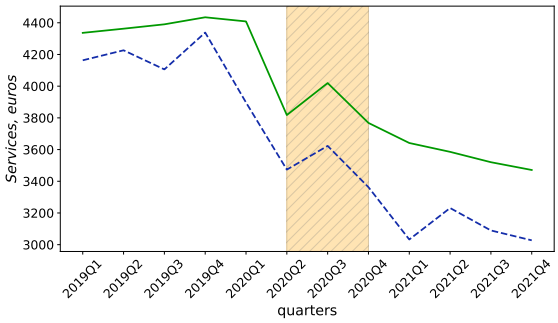
Authors' calculations. The estimated parameters represent the effect of the moratorium policy on household spending between 2019Q1–2021Q4. 2019Q4 is the reference period not affected by policy and the COVID-19 pandemic. All variables are in euros deflated by the CPI index. Consumption expenditures are aggregated to quarterly frequency after smoothing with 6-month moving average. All columns control for household and time fixed effects. Standard errors, clustered at the household level, are in parentheses. Asterisks indicate significance levels: *p<.1, **p<.05, ***p<.01.

Appendix G Sensitivity Analysis: Quarterly Aggregation Without Smoothing

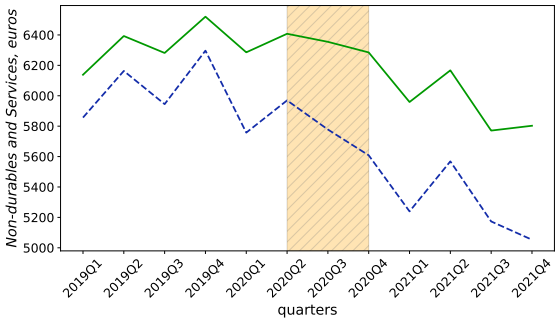
Figure G3: Evolution of average mortgage payments and consumption expenditure in euros (withouth 6-month smoothing)



(a) Non-durable goods & services expenditure



(b) Services



(c) Non-durable goods spending

Table G6: Sensitivity Analysis: Quarterly Aggregation Without Smoothing

	non-durable & Services	Non-durable	Services	Labor Income
Pre-Policy Period				
ITT_{2020Q1}	304.93*	-33.84	415.11**	-447.54**
	(178.37)	(56.04)	(187.41)	(213.87)
Policy Period				
$ITT_{2020Q2\&3\&4}$	340.55*	31.75	285.65	–
	(188.22)	(71.45)	(178.41)	–
Post-Policy Period				
ITT_{2021Q1}	496.07**	34.77	512.53**	-476.76*
	(236.12)	(83.63)	(229.81)	(253.69)
ITT_{2021Q2}	375.42	100.82	257.96	-200.27
	(250.95)	(96.88)	(226.85)	(301.72)
ITT_{2021Q3}	374.28	48.97	334.19	-163.42
	(248.71)	(90.29)	(229.70)	(285.67)
ITT_{2021Q4}	526.32**	148.96	347.38	304.67
	(245.32)	(93.11)	(239.77)	(318.19)
Time FE	Yes	Yes	Yes	Yes
HHs FE	Yes	Yes	Yes	Yes
Households	1,215	1,215	1,215	1,215

Authors' calculations. The estimated parameters represent the effect of the moratorium policy on household spending between 2019Q1–2021Q4. 2019Q4 is the reference period not affected by policy and the COVID-19 pandemic. All variables are in euros deflated by the CPI index. Consumption expenditures are aggregated to quarterly frequency without moving average smoothing. All columns control for household and time fixed effects. Standard errors, clustered at the household level, are in parentheses. Asterisks indicate significance levels: *.1, **.05, ***.01.

Appendix H Event Analysis: Did Banks Take the Policy as One-time

Table H7 presents the results of our analysis investigating whether banks/market perceived the moratoria policy as a one-time intervention or priced in the risk of recurrence. If the policy were anticipated to recur, we would expect the announcement to significantly alter interest rates, maturities, or loan amounts to reflect this new risk. The table shows the movement of these key variables in the one-week and two-week windows surrounding the March 22nd policy announcement. The T-test results provide no statistical evidence that banks repriced loan rates, adjusted mortgage amounts, or altered maturity terms following the moratoria's introduction. We find no repricing around the announcement, consistent with banks treating the policy as a one-off intervention.

Table H7: Summary Statistics and Balance Tests for Event Studies

	1 week window			**2 weeks window**		
Variable	Control	Treated	T-test	Control	Treated	T-test
		Interest Rate				
Mean	1.31	1.48		1.30	1.40	
Std. Dev.	0.38	0.49		0.38	0.38	
N	38	25		85	52	
T-test (p-value)			0.129			0.115
		Maturity				
Mean	11.93	11.95		12.23	11.59	
Std. Dev.	5.14	6.46		5.48	5.80	
N	38	25		85	52	
T-test (p-value)			0.987			0.354
		Loan Amount				
Mean	172,008	120,080		160,604	100,848	
Std. Dev.	271,982	168,997		221,345	128,924	
N	38	25		85	52	
T-test (p-value)			0.398			0.071

Notes: This table presents descriptive statistics and Welch's two-sample t-test results for the variables used in two separate event studies. Control group represents the pre-moratoria, and Treated represents the post-moratoria. The null hypothesis for the t-tests is that the means between the two groups are equal.

Appendix I Estimates Eligible Non-treated Borrowers vs. Non-eligible

This exercise aims to disentangle the effect of the main results. The estimations in the table are re-run, excluding very few households that participated in the debt suspension. We show that the effect from the main results is mostly driven by eligible non-treated households.

Table I8: Estimated Intention-to-Treat Effects of Moratoria Policy

	Mortgage Payments	**Services & Non-durable**	**Non-durable goods**	**Services**
ITT^{T-1}_{2020Q1}	-96.566	105.26	12.27	66.21
	(145.46)	(101.70)	(35.72)	(83.18)
$ITT^{T}_{2020Q2\&3}$	-217.91*	214.46	10.74	152.46
	(121.64)	(161.33)	(63.19)	(133.38)
ITT^{T+1}_{2020Q4}	-68.07	393.01**	49.32	295.15*
	(143.98)	(188.81)	(71.36)	(156.82)
ITT^{T+2}_{2021Q1}	-96.44	374.96*	82.37	289.55*
	(157.11)	(210.51)	(79.21)	(165.67)
ITT^{T+3}_{2021Q2}	-192.54	393.37*	91.47	263.64
	(142.04)	(223.22)	(83.66)	(175.00)
ITT^{T+4}_{2021Q3}	-160.24	329.00	91.59	243.78
	(194.91)	(227.94)	(887.41)	(178.21)
Time FE	Yes	Yes	Yes	Yes
HHs FE	Yes	Yes	Yes	Yes
Households	1173	1173	1173	1173

Authors' calculations. The estimated parameters represent the effect of the moratorium policy on mortgage payments and household spending between 2019Q1–2021Q4. The rest of the coefficients are omitted intentionally for clarity. 2019Q4 is the reference period not affected by policy and COVID-19 pandemic. All variables are in euros deflated by the CPI index. Consumption expenditures and labor income are calculated as a 6-month moving average. Mortgage payments refer to quarterly payments on existing loans originated before the policy. All columns control for households and time-fixed effects. Standard errors, clustered at the household level, are in brackets. Asterisks indicate significance levels: *.1, **.05, ***.01.

Appendix J Household Value Functions

Appendix J.1 Active Renters

An active renter has two choices: to continue to rent or purchase a house, that is, $V^r = \max\left\{V^{rr}, V^{rh}\right\}$ where V^{rr} is the value function if she decides to continue renting and V^{rh} is the value function if she decides to purchase a house. If she decides to continue to rent, she chooses rental unit size s at price p_r per unit, makes her consumption and saving choices, and remains as an active renter in the next period. After purchasing a house, she begins the next period as a homeowner. The value function of an active renter who decides to remain as a renter is given by

$$V_j^{rr}(\omega, z) = \max_{c,s,a' \geq 0} \left\{ u(c,s) + \beta E V_{j+1}^{r}(\omega', z') \right\} \tag{J1}$$

subject to

$$\begin{aligned} c + a' + p_r s &= \omega \\ \omega' &= w' (1-\tau) y(j+1, z') + a' \left(1 + r_k'\right), \end{aligned}$$

where ω is the beginning-of-period cash-on-hand, $p_r s$ is the rental payment, r_k is the return to savings, and w is the wage rate per efficiency unit of labor. The expectation operator is over the income shock z'.

Appendix J.2 Inactive Renters

Inactive renters are not allowed to purchase a house because of their default in previous periods. However, they can become active renters with probability π. Since they cannot buy a house, they only make rental size, consumption, and saving decisions. The value function of an inactive renter is given by

$$V_j^{e}(\omega, z) = \max_{c,s,a' \geq 0} \left\{ u(c,s) + \beta \left[\pi E V_{j+1}^{r}(\omega', z') + (1-\pi) E V_{j+1}^{i}(\omega', z') \right] \right\} \tag{J2}$$

subject to

$$\begin{aligned} c + a' + p_r s &= \omega \\ \omega' &= w' (1-\tau) y(j+1, z') + a' \left(1 + r_k'\right) \end{aligned}$$

Appendix J.3 Homeowners

The options of a homeowner are: 1) stay as a homeowner, 2) refinance, 3) sell the current house (become a renter or buy a new house), or 4) default. The value function of an owner is given as the maximum of these four options, that is, $V^h = \max\left\{V^{hh}, V^{hf}, V^{hr}, V^{he}\right\}$, where V^{hh} is the value of staying as a homeowner, V^{hf} is the value of refinancing, V^{hr} is the value of selling, and V^{he} is the value of defaulting (being excluded from the ownership option). A stayer makes a consumption and saving decision given his income shock, housing, mortgage debt, and assets. Therefore, the problem of the stayer can be formulated as follows:

$$V_j^{hh}(\omega, h, d, z) = \max_{c,\omega' \geq 0} \left\{ u(c,s) + \beta E V_{j+1}^{h}\left(\omega', h, d', z'\right) \right\} \tag{J3}$$

subject to

$$\begin{aligned} c + \delta_h p_h h + a' + m &= \omega \\ \omega' &= w' (1-\tau) y(j+1, z') + a' \left(1 + r_k'\right) \\ d' &= (d-m)(1+r^*) \\ s &= h \end{aligned}$$

where m is the mortgage payment following the amortization schedule determined in equation 3.

The second choice for the homeowner is to refinance, which also includes prepayment. Refinancing requires paying the full balance of any existing debt and getting a new mortgage. We assume that refinancing is subject to the same transaction costs as new mortgage originations. So, we can formulate the problem of a refinancer as

$$V_j^{hf}(\omega, h, d, z) = \max_{c, d', a' \geq 0} \left\{ u(c, s) + \beta_i E V_{j+1}^h(\omega', h, d', z') \right\} \tag{J4}$$

subject to

$$\begin{aligned} c + d + \delta_h p_h h + \varphi_f + a' &= \omega + d' \left(q^m(d'; a', h, z, j) - \varphi_m\right) \\ \omega' &= w' (1-\tau) y(j+1, z') + a' \left(1 + r_k'\right) \\ d' &\leq p_h h (1 - \varrho) \\ s &= h \end{aligned}$$

The third choice for the homeowner is to sell the current house and either stay as a renter or buy a new house. Selling a house is subject to a transaction cost that equals fraction φ_s of the selling price. Moreover, a seller has to pay the outstanding mortgage debt, d, in full to the lender. A seller, upon selling the house, can either rent a house or buy a new one. Her problem is identical to a renter's problem. So, we have

$$V_j^{hr}(\omega, h, d, z) = V_j^r(\omega + p_h h(1 - \varphi_s) - d, z).$$

The fourth possible choice for a homeowner is to default on the mortgage, if she has one. A defaulter has no obligation to the bank. The bank seizes the house, sells it on the market, and returns any positive amount from the sale of the house, net of the outstanding mortgage debt and transaction costs, back to the defaulter. For the lender, the sale price of the house is assumed to be $(1 - \varphi_e) p_h h$. Therefore, the defaulter receives $\max\{(1 - \varphi_e) p_h h - d, 0\}$ from the lender. The defaulter starts the next period as an active renter with probability π. With probability $(1 - \pi)$, she stays as an inactive renter. The problem of a defaulter becomes the following:

$$V_j^h(\omega, d, z) = \max_{c, s, a' \geq 0} \left\{ u(c, s) + \beta_i E \left[\pi V_{j+1}^r(\omega', z') + (1 - \pi) V_{j+1}^i(\omega', z') \right] \right\} \tag{J5}$$

subject to

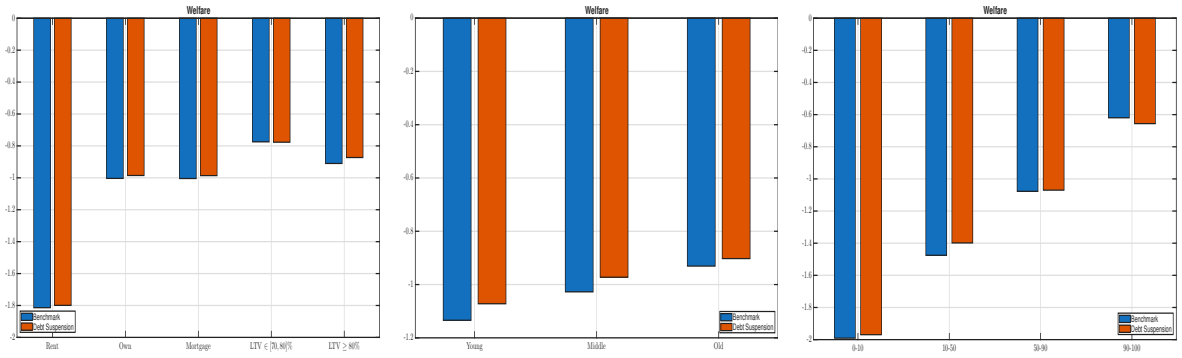
$$\begin{aligned} c + a' + p_r s &= \omega + \max\left\{(1-\varphi_e)\, p_h h - d, 0\right\} \\ \omega' &= w'\,(1-\tau)\, y(j+1, z') + a'\left(1 + r_k'\right) \end{aligned}$$

The problem of a defaulter is different from the problem of a seller in two ways. First, the defaulter receives $\max\left\{(1-\varphi_e)\, p_h h - d, 0\right\}$ from the housing transaction, whereas a seller receives $(1-\varphi_s)\, p_h h - d$. We assume that the default cost is higher than the sale transaction cost, that is, $\varphi_e > \varphi_s$, the defaulter receives less than the seller as long as $(1-\varphi_s)\, p_h h - d \geq 0$ (i.e., the home equity net of the transaction costs for the homeowner is positive). Second, a defaulter does not have access to the mortgage in the next period with some probability. Such an exclusion lowers the continuation utility for a defaulter. In sum, since defaulting is costly, a homeowner will choose to sell the house instead of defaulting as long as $(1-\varphi_s)\, p_h h - d \geq 0$ (i.e., net home equity is positive). Hence, negative equity is a necessary, but not sufficient, condition for default in the model. Therefore, in equilibrium, a defaulter gets nothing from the lender.

Appendix K Additional Model Results

Appendix K.1 Heterogeneous Effects of the Policy

Figure K4: Heterogeneity in Welfare



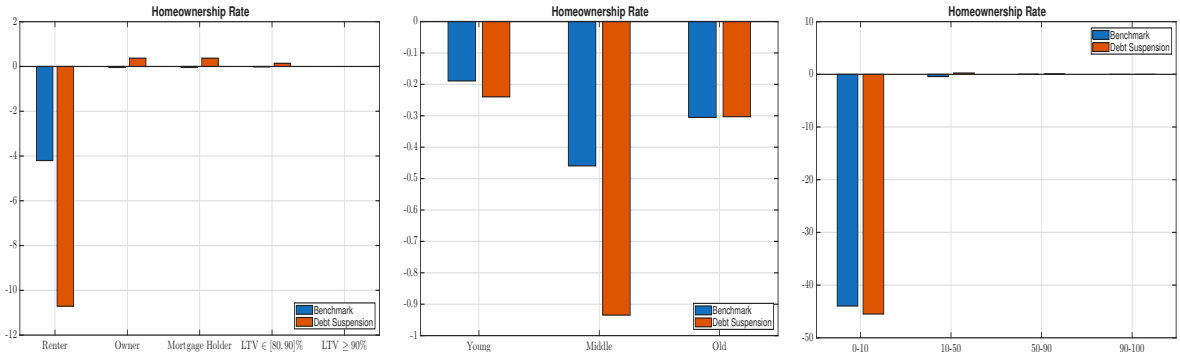
(a) Housing Status (b) Age (c) Net Worth

The figures compare the percentage deviation of consumption-equivalent welfare during the bust period in the economy with debt suspension policy and the benchmark economy across individuals with different characteristics.

Appendix K.2 Further Policy Counterfactuals

Figure K7 plots the comparison of two further policies. In the first policy, the government forces the banks to forgive the excess debt of households who have more than

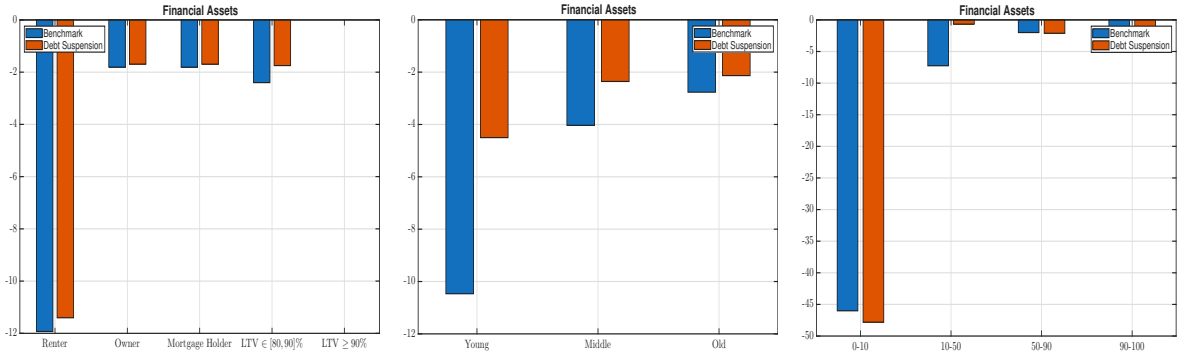
Figure K5: Heterogeneity in the Howeownership Rate



(a) Housing Status (b) Age (c) Net Worth

The figures compare the change in homeownership rate during the bust period in the economy with debt suspension policy and the benchmark economy across individuals with different characteristics.

Figure K6: Heterogeneity in the Response of Financial Assets

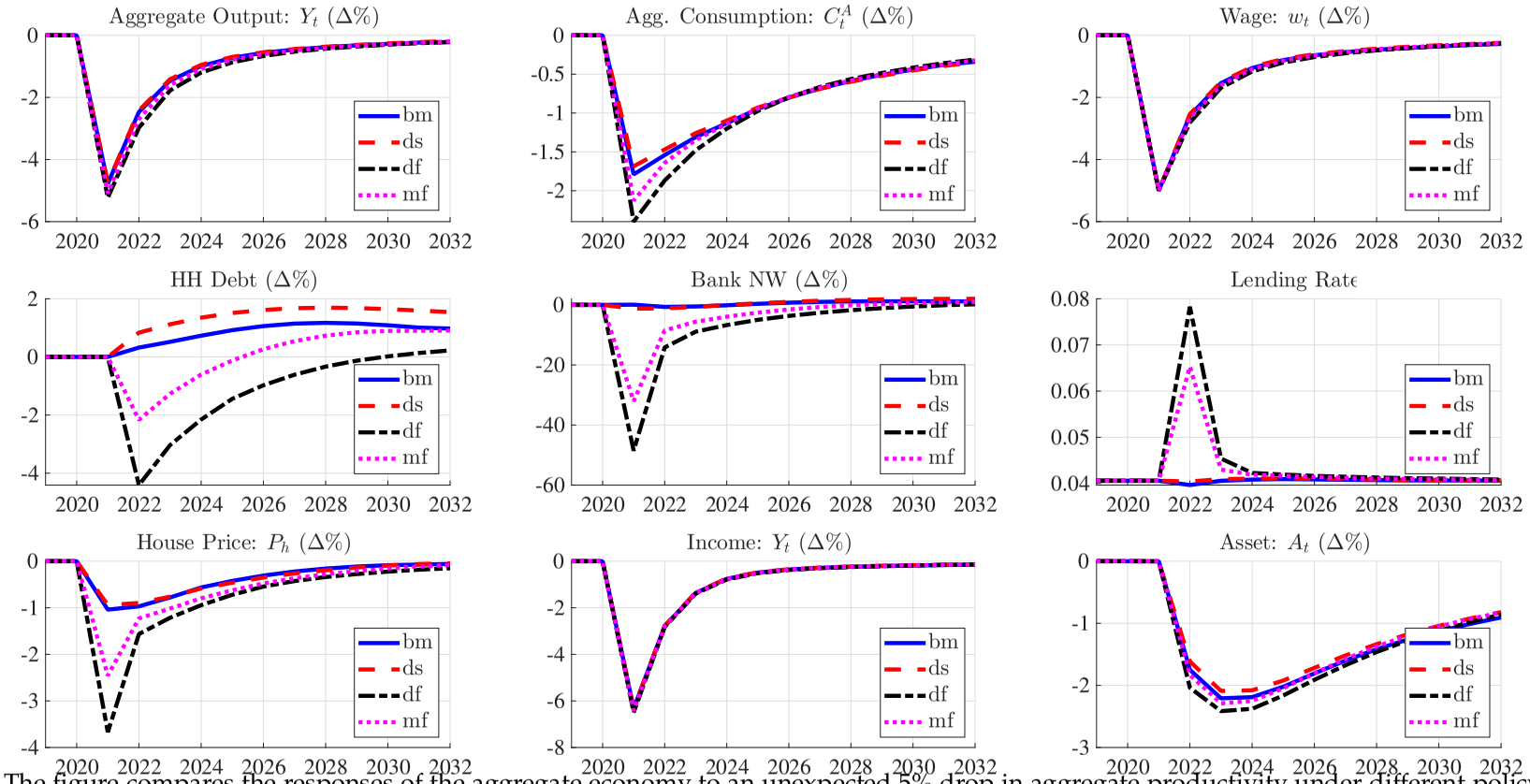


(a) Housing Status (b) Age (c) Net Worth

The figures compare the percentage deviation of financial assets during the bust period in the economy with debt suspension policy and the benchmark economy across individuals with different characteristics.

80% LTV at the time of the shock. The second policy is similar to the one-period debt suspension policy, but assumes that interest does not accrue for the suspended payments.

Figure K7: Alternative Policies



The figure compares the responses of the aggregate economy to an unexpected 5% drop in aggregate productivity under different policy interventions. The solid blue (labeled as "bm") line plots the benchmark economy, the dashed red line (labeled as "ds") plots the economy with one period debt suspension policy, long dashed black line (labeled as "df") plots the economy with debt forgiveness policy which forgives the excess debt of households higher 80% LTV at the time of the shock, and dotted pink line (labeled as "mf") plots the economy with one period debt suspension policy without interest accrual on the suspended payments.