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**Working Paper**

## The heterogeneous effects of household debt relief

ECB Working Paper, No. 3034

**Provided in Cooperation with:**

European Central Bank (ECB)

*Suggested Citation:* Adelino, Manuel; Ferreira, Miguel A.; Oliveira, Miguel (2025) : The heterogeneous effects of household debt relief, ECB Working Paper, No. 3034, ISBN 978-92-899-7116-4, European Central Bank (ECB), Frankfurt a. M., <https://doi.org/10.2866/4885866>

This Version is available at:

<https://hdl.handle.net/10419/322045>

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EUROPEAN CENTRAL BANK

EUROSYSTEM

## Working Paper Series

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### The heterogeneous effects of household debt relief

No 3034

## ABSTRACT

Large-scale debt forbearance is a key policy tool during crises, yet targeting is challenging due to information asymmetries. Using transaction-level data from a Portuguese bank during COVID-19, we find that financially fragile households are more likely to enter forbearance, irrespective of income shocks. Mortgage payment suspension increases consumption and savings, but effects differ across households. Low liquid wealth and income are associated with a higher marginal propensity to consume. Additionally, ineligible households accessing forbearance show a higher propensity to consume than eligible ones. Our results suggest that observable household characteristics can help in the design of effective debt relief policies.

**JEL classification:** E21, E62, G28, G50, H31

**Keywords:** Debt forbearance, Mortgages, Consumption, Income, COVID-19 pandemic

## Non-Technical Summary

The COVID-19 pandemic brought unprecedented financial challenges to households worldwide, prompting governments to implement swift policy interventions to prevent widespread financial distress. One such measure was debt forbearance, which allowed borrowers to temporarily pause mortgage payments. This policy provided immediate liquidity to households, mitigating the risk of defaults and stabilizing financial markets. In general, debt forbearance programs targeted households experiencing significant income reductions or job losses due to the pandemic. However, the enforcement of eligibility criteria was notably lenient, allowing many ineligible households to benefit according to program rules.

This study leverages transaction-level data from a major Portuguese bank to investigate the effects of debt forbearance on household behavior during the pandemic. The analysis focuses on understanding the characteristics of the households that accessed the program and evaluating how the temporary suspension of mortgage payments impacted their consumption behavior. The findings provide crucial insights into the program's effectiveness and the broader implications for policy design.

Households who opted for forbearance were generally more financially fragile than those who did not. They had lower incomes, fewer savings, and higher debt burdens even before the pandemic. Interestingly, while the program was designed to support households experiencing pandemic-related financial shocks, many eligible households chose not to participate, while a substantial number of ineligible households did access the program. This outcome highlights the challenges of implementing broad relief measures, where lenient enforcement can lead to resource allocation beyond the intended targets.

Debt forbearance provided significant short-term relief, allowing households to increase consumption. On average, households spent 15 cents per euro of deferred mortgage payments in the first year, with spending rising to 22 cents in the long term. Consumption responses were particularly pronounced among households with lower liquid wealth and lower income, who spent a greater share of their deferred payments, at approximately 30 cents per euro of postponed payment after entering forbearance. This

heterogeneity highlights the varying financial needs and behaviors across household groups, indicating that a one-size-fits-all approach may not be optimal.

The program also reduced unsecured debt, such as credit card balances, with reductions of 4 cents per euro of deferred mortgage payments. Thus, the liquidity provided by forbearance was primarily for consumption and savings, rather than to pay off debt. Despite these benefits, households struggled to fully adjust their spending behavior after forbearance. Consumption remained elevated even as payments resumed, raising concerns about the possible debt burden for participants in the long term.

A notable finding is the significant consumption response among ineligible households that accessed the program. On average, these households spent 18 cents per euro deferred, compared to just 7 cents among eligible households in forbearance. This difference suggests that the less stringent eligibility criteria allowed financially fragile households, who could otherwise have been excluded, to benefit from the program. However, this also diluted the program's focus on households experiencing acute pandemic-related financial shocks. Eligible households that did not take up forbearance faced greater consumption reductions, highlighting the potential costs of forgoing participation in such relief measures.

The study concludes that while debt forbearance programs provided essential support during the COVID-19 crisis, they also raise important questions for policymakers. Designing effective relief measures requires balancing accessibility with targeting to ensure that resources reach those most in need. Observable characteristics such as household income, wealth, and debt-to-income ratios can play a crucial role in identifying households that are likely to benefit from such interventions. In addition, understanding the heterogeneous responses of households to financial relief can help tailor programs to address diverse needs effectively.

As governments consider future policy responses to crises, this paper underscores the importance of integrating lessons from past interventions. Programs like debt forbearance must be designed to address both immediate financial distress and support long-term economic stability. Ensuring that relief measures are inclusive and targeted can help achieve these objectives, maximizing the benefits for households and the broader economy.

# 1 Introduction

Large-scale debt relief for distressed borrowers is often hampered by information frictions between lenders and borrowers ([Adelino, Gerardi, and Willen \(2013\)](#), [Eberly and Krishnamurthy \(2014\)](#)), institutional frictions such as securitization ([Piskorski, Seru, and Vig \(2010\)](#), [Agarwal, Amromin, Ben-David, Chomsisengphet, and Evanoff \(2011\)](#), [Kruger \(2018\)](#)) and financial and organizational constraints faced by intermediaries ([Aiello \(2022\)](#)). Thus, designing debt relief programs requires making trade-offs between a slower and document-intensive approach that targets only “truly” distressed households (that minimizes type I error) versus a quick-to-implement and catch-all approach that reaches most households (that minimizes type II error). During the Great Recession, the U.S. government chose the former approach. Despite the implementation of the Home Affordable Modification Program (HAMP), which incentivized financial intermediaries to modify delinquent mortgages, up to two-thirds of heavily indebted households received no debt relief ([Noel \(2021\)](#)). The failure to assist more households contributed to employment losses and the slow economic recovery after the crisis ([Dynan, Mian, and Pence \(2012\)](#), [Mian and Sufi \(2014\)](#), [Piskorski and Seru \(2021\)](#)). Two key questions emerge from this literature: first, to what extent the information accessible to lenders effectively characterizes the households that opt into assistance measures, and second, how these households use the additional liquidity they receive.

During the initial months of the COVID-19 pandemic, unlike the 2007-2009 foreclosure crisis, governments worldwide swiftly implemented debt forbearance programs, temporarily suspending debt payments to prevent widespread financial distress and defaults. In Portugal, eligibility for mortgage payment suspensions required households to be employed in sectors directly affected by lockdowns or to have experienced an income reduction of at least 20% compared to pre-pandemic levels. However, the enforcement of these eligibility criteria was typically lax, with limited

verification of households' compliance. The focus on households facing pandemic-related financial hardship and the practice of allowing easy access to the program was also adopted by other European countries and the United States.<sup>1</sup>

This paper uses a transaction-level panel from a leading Portuguese bank to examine the determinants of selection into forbearance and the effects of debt payment suspension implemented between March 2020 and September 2021. The analysis focuses on the impact of forbearance on household consumption, savings, and unsecured debt. We measure the effect of the additional liquidity provided by forbearance by relying on: (1) the high frequency of the data on checking and savings accounts, as well as credit and debit card activity, which allows us to identify sharp changes in behavior around the start of forbearance; (2) the ability to control for both levels and changes in income, the main unobserved variables in similar studies, and performing the comparisons within time-varying income-by-wealth household groups; and (3) the validation that household consumption, saving, and unsecured debt were following parallel trends before the forbearance.

In our analysis, we are interested not only in the changes in household behavior due to the program but also in the selection of households into forbearance (whether they met the formal criteria or not) and potential heterogeneous treatment effects. Thus, our estimated average effects should be interpreted as the effect of forbearance on the set of households who choose to suspend debt payments relative to otherwise similar households who choose not to suspend payments. To the best of our knowledge, this is the first study to use household-level data on income, consumption, and balance sheet to explore who accesses large-scale forbearance programs and to measure the marginal propensity to consume (MPC) and save out of liquidity, two key inputs into the design of such programs.

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<sup>1</sup>The U.S. CARES Act was targeted at borrowers experiencing “financial hardship,” but the program did not require any proof or documentation of hardship ([National Credit Union Administration \(2020\)](#)). The programs were similar across countries in the European Union (see, e.g., [European Banking Authority \(2020\)](#)).

We show that, on average, households who entered forbearance were more financially fragile than those who did not, as they had lower income, lower wealth, and higher debt burden even before the pandemic, consistent with the experience in the United States (Cherry, Jiang, Matvos, Piskorski, and Seru (2021), Gerardi, Lambie-Hanson, and Willen (2022)). They also suffered a larger wage drop in March and April of 2020, although total income was less affected than wages as these households received larger government transfers.

Households in forbearance increased their consumption starting in the summer of 2020 compared to the pre-pandemic period by utilizing a significant fraction of their postponed debt payments. We estimate an MPC out of postponed debt payments of about 15 cents per euro during the first year after payment suspension, rising to about 22 cents in the long term (i.e., more than one year post-suspension). These estimates align closely with those reported in Johnson, Parker, and Souleles (2006) and Parker, Souleles, Johnson, and McClelland (2013) regarding the effects of temporary income shocks, such as federal income tax rebates and the 2008 economic stimulus payments, respectively. This similarity is notable given the distinct nature and duration of the shocks. Unlike the one-time nature of tax rebates, the forbearance program provides households with additional liquidity over an extended period of 18 months. Moreover, it does not constitute a net transfer, as no principal is forgiven, and missed interest payments are capitalized into the loan balance.

We include the change in income, relative to the 2019 average, as a control variable in our regression analyses. Our findings indicate that the MPC from liquidity provided via forbearance is of similar magnitude to the MPC observed in response to income changes for the households in our sample. Specifically, the sensitivity of consumption to income changes is approximately 0.08 euros per euro of income on average, increasing to around 0.13 euros per euro for households receiving forbearance.

Households in forbearance also increased their saving in deposits (checking and savings accounts) by about 35 cents per euro in forbearance shortly after the program's initiation, stabilizing at about 23 cents in the long term. Mortgage payment suspension



also resulted in higher consumption and saving rates (relative to income) among households in forbearance compared to those outside forbearance. Specifically, forbearance is associated with a 6 percentage point increase in the consumption rate and a 5 percentage point increase in the saving rate.

The impact on unsecured debt was modest compared to consumption and saving. Households reduced their unsecured debt (i.e., overdrafts and credit card borrowing) by about 4 cents per euro in forbearance shortly after entering the program, while debt at other banks decreased by 4 cents only more than one year after the start of the program.

These average estimates overlook substantial heterogeneity based on liquid wealth (as proxied by total deposits), income, and indebtedness (as proxied by the debt payment-to-income ratio). Households with below-median wealth increased their consumption by about 30 cents per euro of postponed payment after entering forbearance. In contrast, households with above-median wealth at the bank had a much smaller increase in spending of about 7 cents per euro. While this may seem an intuitive result, previous work has not always found a strong cross-sectional spread in MPC estimates based on the level of assets (e.g., [Parker, Souleles, Johnson, and McClelland \(2013\)](#)). Our finding of substantial heterogeneity in MPCs aligns with recent studies using high-quality data on liquid assets. Notably, [Ganong, Jones, Noel, Greig, Farrell, and Wheat \(2023\)](#), who examine consumption responses to income shocks using data from the JPMorgan Chase Institute, and [Fagereng, Holm, and Natvik \(2021\)](#), who use tax return data from the Norwegian Tax Administration. Although both studies focus on income shocks rather than changes in available liquidity, as is the case in our analysis, the observed heterogeneity in MPCs remains consistent across these different settings. A similar pattern emerges when households are categorized by median income or indebtedness.<sup>2</sup> Additionally, we find that public sector employees who face less income

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<sup>2</sup>Although, once again, we focus on changes in liquidity rather than income, our finding of heterogeneous effects by income is consistent with [Baker, Farrokhnia, Meyer, Pagel, and Yannelis \(2020\)](#) for the effects of the COVID-19 stimulus package by income level, as well as previous work using temporary shocks to household income (e.g., [Hall and Mishkin \(1982\)](#), [Johnson, Parker, and Souleles \(2006\)](#), and [Di Maggio, Kermani, Keys, Piskorski, Ramcharan, Seru, and Yao \(2017\)](#)).

uncertainty exhibit a higher MPC (25 cents) than private sector employees (11 cents).

Saving shows the reverse pattern from consumption in the cross-section of households. While the below-median wealth group only saved about 10 cents per dollar out of postponed repayment, the above-median wealth group saved as much as 40 cents after the start of the forbearance. We find qualitatively similar results when we split households by median income or DTI ratio. Overall, our heterogeneous consumption and saving responses raise questions about the need for a mortgage moratorium for less fragile groups that opted for forbearance.

The design of the forbearance program, with its specific eligibility criteria, combined with the richness of our data, allows us to measure the responses of both targeted households and non-targeted households that nevertheless gained access.<sup>3</sup> We construct individual proxies for eligibility based on wage change and sector of employment, the two primary criteria for the program. Our estimates indicate that around 10% of households in our sample were eligible; however, actual enrollment diverged significantly from these eligibility criteria. Notably, most eligible households opted not to participate in the program (90%), while 80% of households that did suspend payments do not appear to be formally eligible based on our proxies (we refer to this group as “ineligible” below). This confirms that banks employed loose screening criteria, favoring households proactively seeking forbearance. Consequently, the program missed many intended targets but reached a broader range of households not originally designated as recipients.

Interestingly, ineligible households that entered forbearance were, on average, the most financially fragile group before the pandemic, even though their income was not disproportionately affected by the pandemic. This indicates that, although the program was intended for households experiencing significant shocks during the pandemic, the

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<sup>3</sup>Previous studies have considered the characteristics of borrowers accessing forbearance during the COVID pandemic, including differences in credit scores, race, and income levels, as well as the effects on delinquencies and debt usage (see [Yannelis and Amato \(2023\)](#) for a survey). [Ganong and Noel \(2020\)](#) study the consumption response to mortgage modifications following HAMP using end-of-month credit card balances and payments to calculate monthly expenditures. [Albuquerque and Varadi \(2024\)](#) consider the consumption response around the U.K. mortgage moratoria during the pandemic using data from an online personal budgeting application.

actual participants were more likely to be those with pre-existing low income, low wealth, or high debt burden. In fact, we show that the level of deposits at the bank is a stronger predictor of forbearance status than the wage drop due to the pandemic. These ineligible households also exhibit a higher likelihood of job loss one year into the pandemic, suggesting they may have sought forbearance as a precaution against anticipated future economic shocks.

When we break down the average consumption effect by eligibility status, we find that ineligible households who opt for forbearance exhibit significantly higher marginal propensities to consume, averaging about 18 cents per euro of postponed payment, compared to just 7 cents for eligible households in forbearance, even controlling for time-varying income-by-wealth fixed effects. This 11-cent difference between the two groups indicates that the ease of access and the inclusion of ineligible borrowers in the forbearance program substantially influenced the estimated average effect of forbearance. Additionally, eligible households that did not take up forbearance and continued to make their mortgage payments reduced consumption by about 14 cents compared to ineligible households that did not take up the program. This suggests that choosing not to enter the program required significant adjustments for these households. The pattern for saving in bank deposits is reversed. Eligible households in forbearance increased their saving by approximately 50 cents per euro of postponed payment, while ineligible households increased their saving by only 28 cents.

Finally, we examine the outcomes for households exiting the forbearance program and the potential effects of additional debt relief provided by banks in September 2021 at the direction of the regulator. Our findings indicate that households do not fully revert the increased consumption observed during forbearance. Specifically, consumption per euro of postponed payment declines from 23 cents to 13 cents once payments resume. This consumption response is accompanied by a reduction in saving of similar magnitude once payments resume. It is surprising that households do not immediately adjust to the resumption of payments, especially since much of the consumption response during

forbearance occurs in categories that are typically considered “adjustable” ([Chetty and Szeidl \(2007\)](#)).

A small fraction of households that had entered forbearance in the first half of 2020 opted to take up additional relief measures offered by the bank in September 2021. This additional relief involved reduced or suspended payments, including loan maturity extensions, interest rate reductions, or further payment suspensions, targeting households that had been in forbearance over the previous 18 months and might still require support. Like the government moratoria, the bank was explicitly instructed to provide relief to any household that *might* need it, rather than limiting assistance to the most distressed borrowers. This approach involved proactively contacting all borrowers in forbearance to assess their need for further relief. During the initial program period, households that took up these additional measures exhibited a somewhat lower consumption sensitivity to forbearance and allocated more towards saving from postponed payments. This behavior persisted even after the implementation of the additional relief measures.

Overall, our findings provide new insights into the design of large-scale debt relief programs. Optimal program design should account for the behavior of ineligible households that strategically enter the program and how they allocate the additional resources between consumption and saving. Our results indicate that ineligible households substantially increase consumption and may face a heightened debt burden after exiting forbearance due to postponed principal and interest payments without a corresponding increase in savings. To the extent that debt relief policies are also aimed at stimulating demand during times of crisis, understanding the MPC of those who opt into forbearance is also important for policy design. We also show that consumption does not fully adjust downwards after exiting forbearance. This indicates that the program may inadvertently lead to more indebtedness among households that were not the intended targets. Our findings suggest that observable household characteristics, which are typically available to lenders (and not just unobservables, as earlier literature

might have implied), can effectively identify households more likely to opt for forbearance when access criteria are less stringent, as well as predict how they are likely to use the additional liquidity.

Our paper adds to the literature on the effects of government and private debt relief programs. This literature focuses on information and institutional frictions, the impact of loan modifications on delinquency and consumption, and optimal policy design. In research that is directly relevant for understanding the optimality of short-term forbearance programs and the selection into these programs, [Eberly and Krishnamurthy \(2014\)](#) develop a framework for assessing and designing efficient mortgage modification programs. They show that a program with temporary payment reduction during a crisis is a cheaper alternative than principal forgiveness when borrowers are liquidity-constrained.<sup>4</sup> At the same time, lenders may find it optimal to perform principal reductions to reduce the incentive for borrowers to default. The most often cited concern about providing blanket debt relief to households is strategic behavior, i.e., that “too many” households will request help, even though most do not need assistance to remain current on their debts. Recent work shows that borrower default is generally not consistent with pure strategic behavior, i.e., borrowers do not default purely due to negative equity ([Guiso, Sapienza, and Zingales \(2013\)](#), [Gerardi, Herkenhoff, Ohanian, and Willen \(2018\)](#) and [Ganong and Noel \(2023\)](#)).<sup>5</sup>

The consumption and delinquency effects of the policies during the Great Recession in the post-2008 period are already well documented (see, among many others, [Agarwal, Amromin, Ben-David, Chomsisengphet, Piskorski, and Seru \(2017\)](#), [Ganong and Noel \(2020\)](#), [Abel and Fuster \(2021\)](#), [Agarwal, Amromin, Chomsisengphet, Landvoigt, Piskorski, Seru, and Yao \(2023\)](#)). For work on debt relief during the COVID-19

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<sup>4</sup>Using a randomized trial that compares commonly employed debt relief measures, [Aydin \(2021\)](#) finds that forbearance is more effective when applied to constrained households or late-cycle delinquencies.

<sup>5</sup>A notable exception is [Mayer, Morrison, Piskorski, and Gupta \(2014\)](#) who find that borrower delinquency rates increase when Countrywide is forced by court decision to offer more generous modification terms. Recent experimental work looking at forbearance programs in India by [Fiorin, Hall, and Kanz \(2023\)](#) suggests that the effect on moral hazard may be muted, and that borrowers are more likely to interact with the bank in the future if they are offered forbearance.

pandemic, [Cherry, Jiang, Matvos, Piskorski, and Seru \(2021\)](#) and [Gerardi, Lambie-Hanson, and Willen \(2022\)](#) show that public and private forbearance programs contributed to low delinquencies in the United States. [Hong and Lucas \(2023\)](#) show that the credit policies implemented during the pandemic were an important source of incremental resources for households, in addition to governments' fiscal response, and [Lee and Maghzian \(2023\)](#) link forbearance to better macroeconomic outcomes. The reduction in delinquency rates was higher among low-income and minority individuals ([Gerardi, Lambie-Hanson, Willen, et al. \(2021\)](#), [An, Cordell, Geng, and Lee \(2022\)](#), [Shi \(2022\)](#)), but financial intermediary frictions may have prevented some borrowers from receiving forbearance ([Cherry, Jiang, Matvos, Piskorski, and Seru \(2022\)](#), [Kim, Lee, Scharlemann, and Vickery \(2024\)](#)).

Our paper is also related to the literature on the consumption response to the COVID-19 pandemic. This literature focuses on the effects of (one-time or repeated) transfers rather than debt forbearance. [Baker, Farrokhnia, Meyer, Pagel, and Yannelis \(2020\)](#) studies the consumption response of households at different income levels and with different shocks to income around the onset of the pandemic and as a function of shelter-in-place orders. [Ganong, Greig, Noel, Sullivan, and Vavra \(2024\)](#) show that unemployment benefits introduced at the height of the crisis had a large impact on spending but a small impact on employment, and [Cox, Ganong, Noel, Vavra, Wong, Farrell, Greig, and Deadman \(2020\)](#) investigate the heterogeneity across households and consumption categories in the initial phase of the pandemic.<sup>6</sup>

Our paper provides new insights into the dynamics of household consumption, saving, and unsecured debt around large-scale debt relief programs. Understanding the selection patterns for debt forbearance and the heterogeneous effects of policy tools on different

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<sup>6</sup>Intermediary frictions shaped the implementation of the CARES Act-driven debt relief during the pandemic. [Cherry, Jiang, Matvos, Piskorski, and Seru \(2021\)](#) and [Kim, Lee, Scharlemann, and Vickery \(2024\)](#) show that shadow banks provided mortgage forbearance at lower rates than banks, while [Cherry, Jiang, Matvos, Piskorski, and Seru \(2022\)](#) show higher forbearance provision among better-capitalized shadow banks. Research on other pandemic relief programs also finds variation in outcomes across financial intermediaries (e.g., [Granja, Makridis, Yannelis, and Zwick \(2022\)](#)).

household groups is crucial for effectively designing financial stability policies that operate through the household balance sheet channel.

## 2 Institutional Details

At the outbreak of the COVID-19 pandemic, governments and financial institutions worldwide issued legislative and non-legislative moratoria on loan payments, targeting households and non-financial corporations.<sup>7</sup> By the end of March 2020, just a week after the State of Emergency was declared and a national lockdown imposed, the Portuguese government mandated a debt forbearance program, suspending principal and interest payments for certain types of loans upon eligible borrowers' applications.<sup>8</sup> By then, the measure's scope was restrictive, only including mortgage loans for acquiring owner-occupied properties. As the loan maturity date was deferred according to the duration of the forbearance, banks would bear the potential cost of the policy.

Access to the initial government program was limited to individuals: (1) infected with COVID-19 or providing assistance to a relative infected with COVID-19 (which represented a very small fraction of the individuals in the population during the early months of the pandemic);<sup>9</sup> (2) working in companies that reduced work hours due to the pandemic and requested paycheck assistance (the “layoff” regime); (3) unemployed; (4) eligible for financial support for self-employed; or (5) individuals working in industries more affected by the COVID-19 lockdowns as defined in the government legislation. If individuals satisfied one of these criteria, they could request a suspension of loan payments for six months until September 2020. Moreover, the eligibility criteria restricted forbearance to individuals who were not delinquent at the time (defined as those not having payments 90 days past due) nor had outstanding tax or social security

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<sup>7</sup>Figure IA.1 in the Internet Appendix provides an overview of the main events related to the Portuguese government's response to the pandemic, highlighting the debt forbearance program.

<sup>8</sup>Borrowers would restart making higher payments at the end of moratoria due to unpaid interest. The number of payments would not change, so the loan maturity was effectively extended.

<sup>9</sup>About 42,000 cases in a population of about 10 million by the end of June of 2020, <https://coronavirus.jhu.edu/region/portugal>).

liabilities. The program received widespread coverage across television and newspapers, reducing the likelihood that mortgage holders were unaware of the availability of assistance.

By April 2020, an interbank agreement led to a complementary and non-legislative moratorium, expanding the set of loans eligible for forbearance by including other mortgage loans, personal, and auto loans. The government also soon broadened the legislative moratoria, and by mid-June, the measure was extended to all individuals experiencing, or expecting, a 20% reduction in income due to the pandemic (the exact timing or definition of income was not clear in the legislation). At the same time, changes were made to the legislative program in order to include all types of mortgages for residential property and student loans. As a result of these measures, loans in forbearance as a percentage of the total number of loans increased from around 13% in April to 18% in June and then stabilized until 2021. In addition, the suspension of loan payments was extended until the end of March 2021, which would be extended until September 2021, or 18 months after the forbearance was first implemented.

Portugal was among the top three countries in Europe with the highest share of mortgages on repayment moratoria. According to the European Banking Authority (EBA), €365 billion in household loans (€268 billion of which were mortgages) entered moratoria in the Euro area by June 2020, about 7% of household loans ([Nicolaou \(2020\)](#)). In Portugal, €17 billion in mortgages, representing about 18% of all mortgages, were under repayment moratoria by June 2020. The U.S. Government Accountability Office reports that the use of forbearance peaked in the United States in May 2020 at about 7% of single-family mortgages (about 3.4 million) and gradually declined to about 5% percent by February 2021 ([Pendleton \(2021\)](#)).

During 2021, concerns over households' ability to resume payments led to new regulatory guidelines on the prevention and management of arrears, demanding a more proactive role for banks. In addition to closely monitoring borrowers, the local regulator asked banks to offer additional assistance measures after September 2021 to individuals



at risk of defaulting. The exact nature of such measures was left at the banks' discretion and could include loan maturity extensions, interest rate reductions, or additional loan payment suspension. As we will show below, despite the bank's active effort to make the additional assistance measures known to borrowers, only a small fraction of them took advantage of this possibility.

### 3 Data

Our data comprises account-level transactions provided by a leading Portuguese bank. We restrict our analysis to clients who have an outstanding mortgage with the bank. We then group clients with a joint mortgage and who share checking accounts to define a household. In addition, in order to identify households using this particular bank as their primary bank, we focus on households who simultaneously satisfy the following criteria: (1) at least one member of the household has direct deposit of wages, pensions, or social security benefits (e.g., unemployment insurance);<sup>10</sup> and (2) at least one member of the household regularly uses debit and credit cards held at the bank for purchases and payments (an average of at least ten transactions per month).<sup>11</sup> The final sample includes about 137,363 households between January 2018 and June 2022.

Our data include purchases and payments with debit or credit cards, cash withdrawals, and electronic transactions from checking accounts at the transaction level. Given that our sample is composed of households with direct deposit of wages, pensions, and other social security benefits, we are able to estimate monthly household income using checking account transfers. Thus, we can track income even if individuals change jobs or become unemployed. We complement the data with third-party transfers, which include incoming transfers such as within-household transfers from other banks, tax refunds, or rental income. We can also identify the company where wage earners work.<sup>12</sup>

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<sup>10</sup>Households are offered a reduction in the mortgage spread if they choose to have wages and pensions deposited directly at the bank.

<sup>11</sup>On average, households in our sample made 39 monthly transactions, and the median is 35.

<sup>12</sup>Out of the 137,363 households, we find a valid employer match for about 100,000 of them. The

Our measure of consumption includes any purchases or payments using debit or credit cards using data from point-of-sale transactions and cash withdrawals.<sup>13</sup> We complement the data with automatic payments of utilities and other services. Our transaction-level data bypasses the concerns of using annual household wealth snapshots to calculate imputed consumption discussed in [Baker, Kueng, Meyer, and Pagel \(2022\)](#). Although we have an almost complete picture of the activity at the bank, we do not observe outbound transfers to other banks or their intended recipients. This could represent additional saving (if a transfer goes to accounts at other banks owned by the household itself) or additional consumption (purchases of goods and services paid for by bank transfer rather than by card, cash, or automatic withdrawal). We are able to classify purchases by category starting in January 2020. We categorize transactions by relying on point-of-sale terminal information, namely the reported Merchant Category Code (MCC). This classifies merchants into categories based on the type of business and the reported industry code according to *Classificação das Actividades Económicas* (CAE) Revision 3.

The household balance sheet data include end-of-the-month balances for all checking and savings accounts held at the bank, as well as balances for all liabilities, including mortgages, personal loans, auto loans, credit cards, and overdrafts.<sup>14</sup> The data also include additional liabilities information, such as interest rate (as of August 2021), origination date, maturity, and monthly installment before the pandemic.

We merge the internal information of the bank with data from the Credit Register

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remaining households include about 20,000 non-employed households (i.e., unemployed and retired), and about 17,000 employed individuals with an unmatched employer. To achieve this, we consider the name of the entity ordering the Single Euro Payments Area (SEPA) transfer and then use the Levenshtein Distance string metric to match the employer with the universe of firms operating in Portugal. Firm names and industry codes are drawn from SABI (Iberian Balance Sheet Analysis System).

<sup>13</sup>Including cash withdrawals is crucial for measuring consumption accurately, as a significant fraction of retail transactions in Portugal (and across Europe) are still done in cash during this period. According to the 2022 study on the payment attitudes of consumers in the euro area (SPACE) conducted by the European Central Bank, 64% of in-person retail transactions are done in cash in Portugal, as opposed to 31% by card and 5% by other means.

<sup>14</sup>We are unable to compute account balances for financial assets (e.g., individual stocks, bonds, and mutual funds) due to data limitations. However, we see that about 12% of our sample households hold financial assets.

(*Central de Responsabilidades de Crédito*) managed by Bank of Portugal to obtain outstanding loans at other banks for each household. By matching these databases, we can fully track the liability side of the household balance sheet over the sample period and delinquency. While we have daily information on loan-level delinquency for all contracts held with this particular bank, we can only observe end-of-the-month overdue debt in other banks using the Credit Register.

Our data allow us to determine which households applied for and received forbearance and which did not. In addition, we use our data to infer whether a household was eligible to obtain forbearance according to program rules. Specifically, we consider that a household is formally eligible for forbearance if any household member: (1) suffered an income drop of at least 20%, as proxied by the change in wages from the first quarter of 2020 (i.e., the pre-pandemic period) to the second quarter of 2020 (i.e., the start of the pandemic); (2) was working in more affected industries during the first quarter of 2020 as given by the list of industries included in the program rules.<sup>15</sup>

### 3.1 Summary Statistics

Table 1 presents pre-pandemic (as of December 2019) averages of variables for our sample of households, separately for households in and outside forbearance and eligible and ineligible households. This allows us to examine whether selection on observable characteristics plays an important role in applications for the government forbearance program at the beginning of the pandemic. Table IA.1 in the Internet Appendix presents detailed summary statistics for our full sample. Households in our sample comprised 1.7 mortgagors on average, with negligible differences between those who got forbearance and those who did not. The average household monthly total income in our full sample is €2,527, which is significantly higher than the average in the country,

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<sup>15</sup>The list considers a broad industry definition, which we then match to the Portuguese industry classification code list (*CAE*, Revision 3).

which in 2019 amounted to €1,800.<sup>16</sup> The 90th percentile of total income is €4,659 per month.<sup>17</sup>

The average wage is higher for households outside of forbearance, particularly in the case of eligible households. Total income, including pensions (for 40,000 of the 137,363 households in the sample), social security benefits, and other inbound transfers (i.e., rents, business or professional income) follow a similar pattern. For instance, the average total income for eligible households outside forbearance is substantially higher at €2,759 per month than that of eligible households in forbearance at €2,449. In addition, ineligible households in forbearance are the group with the lowest average wage and total income at €1,416 and €2,046, respectively.

Average household consumption is about €1,500 per month (from 39 monthly transactions per household, including cash withdrawals, on average). This compares to an average consumption expenditure per household of about €1,560 in the whole country.<sup>18</sup> Average household consumption follows a similar pattern to income across groups of households, with households outside forbearance exhibiting higher spending levels. In addition, ineligible households in forbearance are the group with the lowest average consumption at €1,320 per month.

Households, on average, maintain checking account balances of €6,700 and savings account balances of €17,300, conditional on having a savings account. However, median balances are substantially lower, at €2,000 for checking accounts and €5,700 for savings accounts, respectively (see Table IA.1). Importantly, households in forbearance exhibit significantly lower average balances in both checking and savings accounts, which aligns with the observation that these households are generally more financially fragile,

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<sup>16</sup>Annual mean net income per household (€) by Deciles of income; INE - *Instituto Nacional de Estatística*, Statistics on Income and Living Conditions (*Inquérito às Condições de Vida e Rendimento*).

<sup>17</sup>The group of homeowners with mortgage comprised around 30% of all Portuguese households in 2021 (INE, Population and Housing Census (*Recenseamento da população e habitação*), 2021), with its median income being substantially higher (at least 25%) than the remaining households, per adult equivalent (Xerez, Pereira, and Cardoso, 2019).

<sup>18</sup>Estimate for 2015, excluding actual or imputed rentals for housing (Peralta, Carvalho, and Esteves, 2021).

irrespective of their eligibility status. Figure IA.2 of the Internet Appendix shows that total deposits are a particularly strong predictor of forbearance access. Indeed, in a penalized regression setting (LASSO), total deposits are more important in predicting forbearance access than changes in wages, even though the government eligibility criteria primarily focus on changes in wages and employer industry.

Table IA.2 of the Internet Appendix examines whether *future* outcomes differ significantly between eligible and ineligible households. The findings indicate that ineligible households in forbearance are the most likely to lose their job within the 12 months following the start of forbearance. Furthermore, among those who remain employed, ineligible households exhibit lower wage growth compared to eligible households. These results reinforce the notion that ineligible households in forbearance represent a particularly fragile group of the population.

Mortgage balances are, on average, €69,000, and households in forbearance have higher average balances. Almost all clients have a credit card or an overdraft, holding an average balance of about €420. In contrast, only about 1% of households in our sample hold student or auto loans, and 7% hold other types of loans, such as personal loans. Finally, most households in our sample have loans with other banks, with a balance of about €7,500 on average.

The average total loan payment (mostly mortgage payments), including principal and interest, is €315 per month, higher than the country's average by the end of 2019 (€248).<sup>19</sup> Moreover, households in forbearance have higher loan payment commitments (€377) than households outside forbearance (€311). By entering forbearance, the average household postpones €345 per month. Considering total income, we estimate an average debt payment-to-income (DTI) ratio of about 19% in 2019, slightly above the country's average in 2022 (17%).<sup>20</sup> As expected, the average DTI is higher for households in forbearance, in particular for ineligible households at 32%. Debt

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<sup>19</sup>Press Release INE, Interest rates implied in housing loans, January 19, 2022.

<sup>20</sup>Banco de Portugal, Relatório de Estabilidade Financeira, November 2022.

delinquency is infrequent in our sample, with just 1% of households having payments more than 30 days past due. We conclude that ineligible households in forbearance are the most fragile group with lower income, consumption, total deposits, and higher DTI.

## 4 Empirical Methodology

We estimate a difference-in-differences regression to compare the marginal propensities to consume, save, and borrow between households in forbearance and households outside forbearance around the start of the debt forbearance program:

$$Y_{i,t} - \bar{Y}_{i,2019} = \beta \text{Forbearance Amount}_i \times \text{Post}_t + \lambda X_{i,t} + \mu_{g,t} + \varepsilon_{i,t}, \quad (1)$$

where the outcome variable is either the change in monthly consumption expenditures ( $\Delta \text{Consumption}$ ); change in saving in total deposits ( $\Delta \text{Saving in Deposits}$ ); change in credit card and overdraft borrowing ( $\Delta \text{Borrowing Credit Card \& Overdraft}$ ); and change in unsecured debt borrowing at other banks ( $\Delta \text{Borrowing Other Bank's Debt}$ ) for household  $i$  at time  $t$ . Changes are calculated relative to each variable's 2019 average. Saving is the change in end-of-month checking and savings account balance ( $\Delta \text{Total Deposits}$ ). Borrowing is the change in end-month credit card and overdraft balance or unsecured debt outstanding at other banks.

*Forbearance Amount<sub>i</sub>* is the amount of postponed debt payments (mostly mortgages, but it may include other loans for some households) for household  $i$ . *Post<sub>t</sub>* is a dummy variable that takes a value of one for the period starting when mortgage payments are suspended and zero otherwise.<sup>21</sup> The coefficient of interest is  $\beta$ , which captures whether forbearance is associated with a differential effect on consumption, saving or borrowing around the program's start. The sample period is between January 2018 and September 2021 (i.e., the end of the government program).

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<sup>21</sup>We drop the household subscript from *Post<sub>t</sub>* for expositional purposes. The *Post<sub>t</sub>* dummy variable is household-specific since households entered forbearance between April and June 2020. However, most households in our sample start forbearance in April 2020 (65%), plus 28% in May and just 7% in June.

We also estimate the effect at different horizons by replacing the *Post* dummy variable with three dummy variables: (1) *Immediate Effect* for months 1 through 3 after a household enters forbearance; (2) *Short-run Effect* for months 4 through 12 after forbearance; and (3) *Long-run Effect* for the period starting 12 months after forbearance and up to the end of the government program.

$X_{i,t}$  is a set of household-level time-varying controls, which includes changes in monthly total income ( $\Delta Income$ ) relative to average monthly income in 2019, and the  $Forbearance_i$  dummy variable that takes the value of one for households receiving forbearance, and zero otherwise. All the regressions include month-year fixed effect ( $\mu_t$ ) to absorb shocks that may affect all households in a given period or group-by-month-year fixed effects ( $\mu_{g,t}$ ) to absorb time-varying shocks for households by quartile of pre-pandemic income and wealth (2019 averages). Thus, the regressions include a total of 16 group indicators (four quartiles of income by four quartiles of total deposits) interacted with dummy variables for each month-year. Standard errors are clustered two-way at the household and month-year levels.

We also estimate the dynamic effect of the forbearance by replacing the  $Post_t$  variable with time dummies,  $\mathbb{1}(\text{period} = \tau)$ :

$$Y_{i,t} - \bar{Y}_{i,2019} = \sum_{\tau=-29}^{28} \beta_{\tau} \times Forbearance Amount_i \times \mathbb{1}(\text{period} = \tau) + \lambda X_{i,t} + \mu_{g,t} + \varepsilon_{i,t}. \quad (2)$$

The coefficients of interest are  $\beta_{\tau}$ , which measure the change in consumption, saving, or borrowing due to postponed debt payments in each month around the start of forbearance.

To estimate the average change in the consumption and saving rates, defined as monthly spending and saving divided by average total income, we estimate the following difference-in-differences regression:

$$\frac{Y_{i,t} - \bar{Y}_{i,2019}}{\bar{Income}_{i,2019}} = \alpha Forbearance_i \times Post_t + \lambda X_{i,t} + \mu_{g,t} + \varepsilon_{i,t}, \quad (3)$$

where the outcome variable is either the change in monthly consumption expenditures or the change in saving in total deposits for household  $i$  at time  $t$ , relative to the household's 2019 average, divided by average income in 2019. The coefficient of interest is  $\alpha$ , which measures the average change in the consumption or saving rate associated with forbearance.

## 5 Effects of Debt Forbearance on Consumption, Saving, and Borrowing

In this section, we first show the evolution of the average income, consumption, and total deposits before and after the government debt forbearance program initiated in March 2020.

Figure 1 shows the evolution of average wages, social security benefits, and total income between July 2019 and the end of the program in September 2021 for households in forbearance and outside forbearance. We adjust for seasonality using month dummies but do not control for any other variables.

Panel A of Figure 1 shows that households in forbearance were, on average, more exposed to the pandemic shock, losing about €130 of monthly wages, on average, at the onset of the pandemic, compared with about €80 for households outside forbearance. Notice that households in forbearance had lower average wages to begin with, as shown in Table 1.

Panel B of Figure 1 shows that the evolution of social security benefits at the onset of the pandemic disproportionately benefited households in forbearance and that this gap persisted until 2022. Panel C shows total income evolution, including wages and social security transfers. The figure shows that income supplements and other government transfers were sufficient to stabilize total income during this period.

Figure IA.3 of the Internet Appendix shows the evolution of wages, social security benefits, and total income separately for households receiving forbearance who meet the



eligibility criteria and for those who do not meet. We find that the large drop in wages for households in forbearance happens almost exclusively for eligible households, with almost no reduction for ineligible ones. This is somewhat mechanical, as one of the criteria for eligibility is a drop in income of at least 20%. Notably, we do not see virtually any drop for ineligible households who choose to enter forbearance. In addition, the figure shows that even after social security benefits, eligible individuals in forbearance still suffered a substantial drop in income.

Figure 2 shows the evolution of the average consumption and total deposits. Panel A shows that both groups of households cut spending right after the start of the pandemic in March 2020, likely due to a combination of demand and supply factors due to shutdowns. However, we find a positive and statistically significant difference in consumption for households in forbearance relative to households outside forbearance of about €200 per month by the summer of 2020. Interestingly, households in forbearance end up with a significantly higher average monthly consumption even compared to pre-pandemic levels. This is particularly noteworthy given that the total income was lower for households in forbearance (see Table 1).<sup>22</sup>

Figure IA.4 of the Internet Appendix shows the evolution of consumption and total deposits separately for eligible and ineligible households in forbearance. We find that ineligible households drive the positive gap in consumption that emerges for households in forbearance, whereas the consumption of eligible households in forbearance mostly tracks that of those outside of forbearance.

Panel B of Figure 2 shows the evolution of the average total deposits. The pandemic's beginning is associated with a slower growth in total deposits of households in forbearance (mostly driven by checking accounts). By mid-2020, and even more so by early 2021, households in forbearance started increasing their total deposits, i.e., accumulating balances in their checking and savings accounts, faster than households

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<sup>22</sup>The figure shows the evolution of consumption using equal weights among households. Table IA.3 of the Internet Appendix shows that the value-weighted evolution of consumption in our sample closely matches the evolution of consumption in Portugal for this period.

outside forbearance.

## 5.1 Response of Marginal Propensity to Consume and Save

In this section, we present the estimates of the effect of the debt forbearance program on household consumption and saving in deposits. Table 2 shows difference-in-differences regression estimates based on equation (1). Columns (1)-(4) show the estimates for the effect on the marginal propensity to consume (MPC) measured as the change in consumption per euro of postponed debt payment in each month relative to the household's 2019 average, and columns (5)-(8) show the estimates for the effect on the marginal propensity to save in total deposits. The main explanatory variable is the amount of postponed debt payments in euros (*Forbearance Amount<sub>i</sub>*). Columns (1) and (5) include month-year fixed effects. Columns (2)-(4) and (6)-(8) include 16 household group indicators using quartiles of income-by-total deposits interacted with month-year fixed effects. In columns (3), (4), (7), and (8), we control for  $\Delta Income$ . Additionally, since households in forbearance are on average more fragile, in columns (4) and (8) we include the  $\Delta Income \times Forbearance$  interaction term to control for potential differences in the propensities to consume/save out of income.

Column (1) of Table 2 indicates that the MPC to consume out of payments in forbearance is both positive and statistically significant, amounting to 15 cents per euro of postponed payment. This effect translates to approximately €52 at the average forbearance amount. Columns (2)-(4), which include household group-by-month-year fixed effects and total income as controls, yield similar estimates, ranging from 13 to 15 cents, all of which are also significant.

Column (5) shows a positive effect on the marginal propensity to save of 12 cents per euro of postponed payment, although this effect is statistically insignificant. In contrast, when household group-by-month-year fixed effects are included in column (6), the coefficient increases to 18 cents per euro and becomes statistically significant. Further, when changes in total income are added as a control in columns (7) and (8), the

marginal propensity to save in deposits rises to about 30 cents per euro.

Changes in total income exhibit a positive and significant impact on both consumption and total deposits. We estimate an MPC out of income changes of about 9 cents (lower than in [Ganong, Jones, Noel, Greig, Farrell, and Wheat \(2023\)](#) who obtain an MPC of 21 cents out of typical income fluctuations) and a marginal propensity to save of 70 cents per euro. In column (4), we include the  $\Delta Income \times Forbearance$  interaction term and find that the combined MPC estimate is about 0.13. This effect is comparable in magnitude to that of forbearance, indicating that households adjust their consumption in response to increased liquidity from suspended debt payments in much the same way they respond to changes in income. It remains unclear whether households perceive these changes in liquidity as temporary or permanent.

Table [IA.4](#) of the Internet Appendix presents robustness tests of the effect of forbearance on consumption and saving in Table [2](#). Column (1) shows that these results are robust to the inclusion of household-by-month fixed effects to absorb unobserved (time invariant) household-level heterogeneity and monthly seasonality. Moreover, columns (2) and (3) show that the estimated coefficients are robust when we include industry-by-month and municipality-by-month fixed effects, thus controlling for potential unobserved industry and regional shocks.

## 5.2 Response at Different Horizons

Next, we examine the response to the suspension of debt payments at different horizons after the start of forbearance. We estimate the regression in equation [\(1\)](#), replacing the *Post* dummy variable with three dummy variables: *Immediate Effect* (month 1 through month 3); *Short-run Effect* (month 4 through month 12); and *Long-run Effect* (after month 12).

Table [3](#) presents these estimates, which can be directly compared to those in Table [2](#). Columns (1)-(3) presents the estimates for the effect of forbearance on consumption. In column (1), we find that the immediate effect on the MPC is close to zero and statistically

insignificant. However, this estimate becomes significant and ranges from 13 to 15 cents per euro in forbearance for the short-term horizon. The long-term effect on consumption reaches approximately 22 cents per euro. This indicates that consumption did not respond to the amount of forbearance during the early months of the pandemic (possibly due to supply-side constraints) but increased over time.

Columns (4)-(6) of Table 3 presents the estimates for the effect of forbearance on saving in deposits. Controlling for observable differences is crucial in the saving regressions, as both income and wealth levels, along with changes in income, significantly impact the estimated saving behavior of households in forbearance (which is apparent from comparing columns (4)-(6)). Using our most comprehensive specification in column (6), we find that households save up to 35 cents per euro of postponed payments immediately following the onset of forbearance. Although the saving response to forbearance decreases to 32 cents per euro of postponed payment in the short run and 23 cents in the long run, it remains positive and strongly significant.<sup>23</sup>

Figure 3 shows the evolution of the marginal propensity to consume (Panel A) and save in deposits (Panel B) for households in forbearance and outside forbearance from 12 months before the start of the program (March 2020) up to 12 months after. The figure plots the estimates of the monthly  $\beta_\tau$  coefficients obtained from the regression in equation (2). The coefficients measure the difference between households in forbearance and households outside forbearance relative to the month before the start of the forbearance. Despite significant differences in the average characteristics of the two groups, Panel A shows no evidence of preexisting differential trends in the MPC prior to the initiation of forbearance. Additionally, there is a positive and significant consumption response shortly after the forbearance program begins. This effect increases over the first six months of the forbearance period and is persistent in the long

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<sup>23</sup>Table IA.5 in the Internet Appendix shows that the estimates of the consumption and saving response to forbearance are similar when we restrict the sample to individuals working in the private sector (i.e., we exclude public servants and other individuals working in public entities and state-owned firms). This sample includes households likely to have been more affected by the pandemic.

run.

Panel B shows the response of the marginal propensity to save in deposits around the onset of forbearance. A positive and significant saving response is clear from the beginning of the forbearance period. The effect becomes smaller in magnitude over the short-term and long-term horizons but is still positive and significant.

### 5.3 Response of Consumption and Saving Rate

Our results are robust when we use alternative definitions of the consumption and saving response to debt forbearance. Table 4 shows the effect of forbearance on consumption and saving rates, i.e., change in monthly consumption expenditure and change in monthly saving as a fraction of pre-pandemic average income. Columns (1)-(3) present the estimates using the consumption rate. Using the most stringent specification in column (3), we find that households in forbearance increased the consumption rate by about 6 percentage points. As before, the effect is stronger at 9 percentage points in the long run, which compares with an unconditional average of 76% of income allocated to consumption before the pandemic (and about 88% for households in forbearance).

Columns (4)-(6) report the estimates using the saving rate. We find that the average saving rate response is positive and significant. Households increase the saving rate by about 5 percentage points over the forbearance period. When we control for changes in income in column (6), the impact on the saving rate is similar at different horizons, ranging from 4 (immediate effect) to 5 percentage points (short-run and long-run effects).

### 5.4 Response of Marginal Propensity to Borrow

In this subsection, we estimate the marginal propensity to borrow (unsecured debt) out of postponed debt payments. Table 5 presents the estimates of the *Forbearance Amount*  $\times$  *Post* coefficient for two additional outcome variables: changes in credit card and overdraft borrowing at the bank in columns (1)-(3) and changes in

unsecured debt borrowing from other banks in columns (4)-(6). Columns (1)-(3) indicate that households in forbearance reduced borrowing in credit card and overdraft compared to households outside forbearance. Although the overall effect on credit card and overdraft borrowing is statistically insignificant, we find that households during the first quarter of forbearance (immediate effect) allocated approximately 4 cents per euro of postponed payments to reduce short-term liabilities.

Conversely, columns (4)-(6) suggest that households in forbearance paid down unsecured debt held at other banks, compared to those outside forbearance, but this effect is statistically significant only in the long term. Our estimates across all specifications indicate that households reduced borrowing at other banks by about 4 cents per euro of postponed payment. Considering that all types of unsecured loans from other banks are included, these results are consistent with the idea that households initially prioritize deleveraging high-cost loans, gradually transitioning to longer-maturity (cheaper) loans.

## 5.5 Spending Categories

Our bank transaction-level data include merchant codes for debit and credit card transactions, which allow us to categorize most of the consumption expenditures.<sup>24</sup> Table 6 shows the estimates of the forbearance effect on the MPC by category. Panel A presents the estimates of the overall effect and Panel B presents the estimates by horizon (immediate, short run and long run). The sample period is from January 2020 to September 2021 (merchant codes are unavailable in 2018 and 2019). All regressions include household group-by-month-year fixed effects, as well as  $\Delta Income$  as a control.

Column (1) shows that the estimates of the total consumption response are similar when we use the sample for which we can categorize consumption expenditures (i.e., starting in January 2020). The consumption effect is positive and significant at 10 cents

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<sup>24</sup>This is similar to the consumption categories in Cox, Ganong, Noel, Vavra, Wong, Farrell, Greig, and Deadman (2020) using credit card data from a large U.S. bank.

per euro of postponed payment, ranging between 9 cents in the short run and 17 cents in the long run.

Columns (2)-(10) report the estimates of the consumption sensitivity to forbearance for each category. During the first year after the forbearance start (immediate and short-run effects), the main drivers of the consumption response to forbearance are “Groceries” and “Clothing”, and to a smaller extent “House Maintenance”, “Furniture”, “Health Care” and “Entertainment and Education”, which have a positive and significant effect. Notably, “Restaurants” initially have a negative effect, possibly due to COVID-19-related closures. Over the long term (beyond one year after the start of the forbearance), the effects are positive and significant across all categories. The consumption response to forbearance is more pronounced for “Groceries” at 6 cents per euro of postponed payments, and for “Clothing”, “Transportation”, and “Restaurants” at about 2 cents.

## 5.6 Heterogeneous Effects

We investigate the extent to which the marginal propensity to consume or save out of postponed debt payments is heterogeneous across households with different levels of financial fragility as measured by wealth, income, and indebtedness. We estimate the *Forbearance Amount*  $\times$  *Post* interaction term coefficient using the regression in equation (1) separately for the sample of more fragile households and the sample of less fragile households. Specifically, we split households at the median of pre-pandemic wealth (proxied by total deposits), income, and indebtedness (proxied by the DTI ratio) in Panels A-C. Figure 4 reports the effects on the MPC for these groups separately, while Figure 5 provides the corresponding estimates for the marginal propensity to save in deposits.

Panels A-C of Figure 4 show that the consumption response is concentrated primarily in the most fragile households across all three financial fragility proxies. In Panel A, the low-deposits group consumption response is positive and significant at about 30 cents per euro of postponed payments, compared to about 7 cents for the high-deposits group.

In Panel B, the consumption response is stronger for low-income households than for high-income households, with similar magnitudes to those in Panel A. Panel C shows a stronger consumption response for households with a high DTI ratio than a low DTI ratio. The effect is almost 20 cents for households with a high DTI ratio and only 6 cents for households with a low DTI ratio.

We also show how households with differing levels of income volatility react to forbearance: Panel D presents estimates for households with above- and below-median income volatility, defined as the standard deviation of total monthly income normalized by its average in 2019. Panel E presents estimates for employees working in the public and private sectors, based on the primary earner in the household. This analysis addresses whether the perception of income risk and unemployment risk influences the response to forbearance. In Panel D, 4, the consumption response is lower, at about 10 cents, for households with more volatile income prior to the pandemic, compared to about 21 cents for those households with low volatility of income. Similarly, in Panel E, the consumption response is about 10 cents per euro for households in the private sector, whereas it is significantly stronger for public sector households, at about 25 cents.

Conversely, Panels A-C of Figure 5 show that the average saving response in deposits is primarily driven by high-wealth, high-income, and low-debt burden households, respectively. The propensity to save per euro of postponed payment by less fragile households is about four times higher than for the more fragile households. In Panel A, the high-deposits group's saving response is positive and significant at almost 40 cents per euro of postponed payments, compared to about 10 cents for the low-deposits group. In Panel B, the saving response is stronger for high-income households than for low-income households, with similar magnitudes to those in Panel A. Finally, Panel C shows a stronger saving response for households with a low DTI ratio than a high DTI ratio. The effect is approximately 40 cents greater for the low-DTI group compared to the high-DTI group.

Panels D and E of Figure 5 show that households with more volatile earnings or



employed by the private sector show a higher propensity to save, although the difference between the groups is not statistically significant. These findings suggest that anticipated income risk and employment risk, which is notably higher for private sector employees, play a role in shaping household responses to the debt forbearance program.

In summary, forbearance induces heterogeneous responses among households with different levels of financial fragility and income risk. The consumption response is concentrated among more fragile households and those facing lower income risk, whereas the saving response is concentrated among less fragile households and those facing higher income risk.

## 6 Forbearance Eligibility and Selection

Access to the debt forbearance program during the COVID-19 pandemic was relatively lenient, unlike the stringent criteria for loan modifications that characterized the 2008-2009 foreclosure crisis in the United States. ([Adelino, Gerardi, and Willen \(2013\)](#)).

We estimate the effect of forbearance on consumption and saving in deposits using the regression in equation (1), focusing on four groups of households based on their forbearance status and program eligibility status: (1) ineligible households outside forbearance (the omitted group); (2) eligible households outside forbearance; (3) ineligible households in forbearance; and (4) eligible households in forbearance. Specifically, we define a *Eligible* dummy variable that takes the value of one for households eligible to the forbearance program according to program rules, and zero otherwise (see Section 3 for details on the eligibility definition). We then interact the *Eligible* dummy variable with the *Forbearance* and the *Post* dummy variables.

### 6.1 Number of Eligible and Ineligible Households

Table 7 (at the bottom of the table) presents the number of observations for each of the four groups. The distribution of households across these groups confirms that access to

the forbearance program was relatively lenient during the pandemic, with implementation by banks favoring broader access to forbearance. In fact, we find that about 10% of households were eligible for forbearance, but only about 11%  $[= 1.1 \div (9.1 + 1.1)]$  of those eligible households actually entered the program (representing 1.1% of the full sample). In contrast, 5.3%  $[= 4.8 \div (85.0 + 4.8)]$  of the ineligible households entered the program (representing 4.8% of the full sample). This implies that over 80% of the households in forbearance were ineligible, while less than 20% met the eligibility criteria. These findings align with the lack of rigorous eligibility checks during the pandemic and the absence of strong incentives for bank officers to screen out ineligible forbearance applications.<sup>25</sup>

As discussed in Section 3.1, there are significant differences in observable characteristics between households in and out of forbearance, as well as between eligible and ineligible households. Ineligible households in forbearance exhibit the lowest levels of total income, consumption, and total deposits, coupled with the highest DTI ratios, among the four groups. A LASSO regression shows that total deposits are the most important factor in determining access to forbearance, even more so than wage reductions, which were part of the program's eligibility criteria, as shown in Figure IA.2 of the Internet Appendix. This indicates that ineligible households in forbearance were already the most fragile group at the onset of the pandemic crisis

## 6.2 Heterogeneous Effects by Eligibility Status

Table 7 shows substantial heterogeneity in the effects of forbearance on consumption among the four household groups. We find that eligible households outside forbearance exhibit a significantly lower MPC after the start of the forbearance period relative to ineligible individuals also outside of forbearance (the omitted group in these regressions, which constitutes 85% of the sample). This difference is about -14 cents per euro of postponed payment when we include household group-by-month-year fixed effects and

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<sup>25</sup>While we recognize that our eligibility measure may not be perfect, the proportion of the sample identified as ineligible significantly exceeds what would be expected from merely assignment errors in our measures.

total income as a control, as shown in column (3). This indicates that the burden of higher debt payments led these eligible households, who opted not to access forbearance, to significantly reduce consumption during the pandemic.<sup>26</sup> Eligible households in forbearance avoided this decline in consumption, showing a positive consumption response of 7 cents per euro in column (3); however, this response is not statistically different from that of ineligible households outside of forbearance.

In contrast, ineligible households that nonetheless entered forbearance exhibit a significantly higher MPC out of postponed payments, ranging from about 17 to 18 cents per euro of forbearance relative to the omitted group, as shown in columns (2) and (3). These findings indicate that the significant MPC reported in Table 2 is primarily driven by ineligible households that applied for and received forbearance. Furthermore, concerns about future job loss may have influenced these households' decision to seek forbearance, as they are more likely to experience job loss within 12 months after entering forbearance, as shown in Table IA.2 of the Internet Appendix.

Column (6) of Table 7 shows that ineligible households in forbearance have a marginal propensity to save in deposits of about 28 cents per euro when controlling for changes in total income. In contrast, we find no significant relationship between the saving behavior of eligible households outside of forbearance and the amount of postponed payments, unlike the results observed for consumption. Additionally, forbearance is associated with a higher marginal propensity to save among eligible households, reaching about 49 cents per euro of forbearance in column (6).

Figure 6 shows the evolution of the marginal propensity to consume (Panel A) and the marginal propensity to save in deposits (Panel B) for households in forbearance, distinguishing between eligible and ineligible households (the omitted group consists of all households outside forbearance). In Panel A, the results indicate that ineligible households

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<sup>26</sup>The decision by these households to forgo the forbearance option and instead adjust their consumption to meet mortgage payments aligns with the model presented in [Campbell, Clara, and Cocco \(2021\)](#). According to that model, opting for interest-only payments during recessions results in smaller reductions in consumption, which is consistent with our findings for borrowers who choose forbearance.

in forbearance are the primary contributors to the observed increase in consumption in response to forbearance. This effect increases during the first six months of the forbearance period and remains significant in the long term. Conversely, the consumption response among eligible households is statistically insignificant. Panel B depicts the response of the marginal propensity to save around the start of forbearance. From the beginning of the forbearance period, a positive and significant saving response is evident. Both eligible and ineligible households exhibit a similar sensitivity to forbearance.

Table [IA.6](#) of the Internet Appendix shows the effects for eligible and ineligible groups on changes in credit card and overdraft borrowing at the bank, as well as unsecured debt at other banks. We do not find significant differences in the marginal propensity to borrow, except for eligible households who do not enter forbearance with a 4% effect on unsecured debt at other banks. The other estimates in the table are not statistically significant.

Selection plays an important role in our setting as over 80% of the households in forbearance in our sample are identified as ineligible based on our proxy. The observed differences in the sensitivity of consumption (as shown in column (3)) and saving (as shown in column (6)) between eligible and ineligible households indicate that differences in forbearance program participation, when access is lenient, significantly influence the overall effects of forbearance. Specifically, ineligible households that entered the forbearance program exhibited greater sensitivity in their consumption to the amount of postponed payments compared to eligible households. These findings imply that easier access to forbearance programs may lead households with a higher sensitivity of consumption to deferred debt payments to opt for forbearance.<sup>27</sup>

Table [8](#) examines the eligibility effects separately for more and less affected household groups according to the industry they work in. The more affected subsample comprises households whose primary employer industry had below-median revenue

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<sup>27</sup>Table [IA.7](#) in the Internet Appendix shows the response by spending category for each group, akin to Table [6](#).

growth between 2019 and 2020. Even though this is not how the government selected industries for eligibility, a much larger fraction of the workers in these industries are eligible for forbearance according to the formal criteria (almost 30% in total).<sup>28</sup> Still, even for this group, only 14% of eligible households entered forbearance (4.2% of the sample of households in these industries). This implies that ineligible households make up a smaller fraction of households in forbearance in these industries (about 50% instead of 80% in the full sample). Column (1) shows that, within this group of more affected industries, the differential effect is similar to the average effect in the whole sample, with eligible households outside forbearance cutting consumption by 12 cents per euro of postponed payment; eligible households in forbearance avoiding to do so; and finally ineligible households in forbearance exhibit a positive and statistically significant MPC out of postponed payments of 14 cents.

Interestingly, a different pattern emerges when focusing on industries less affected by the pandemic (column (2)), defined as those with above-median revenue growth between 2019 and 2020, or when focusing on public servants (column (3)), who experienced no wage changes and were ineligible under the program rules.<sup>29</sup> In these sectors, ineligible households constitute a larger share of those in forbearance—76% in less affected sectors and 90% among public servants—and their behavior closely mirrors that of eligible households. Additionally, the perception of income risk plays a significant role across all household groups, with the impact on consumption (saving) showing a monotonic decrease (increase) with income risk.

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<sup>28</sup>We measure revenue growth until December 2020, and this was only available after March 2020, the date of eligibility definition by the government.

<sup>29</sup>In these less affected industries, the proportion of households meeting the eligibility criteria decreases to 14%, and to just 5% among public servants. These public servants were eligible primarily due to meeting eligibility criteria through their *secondary* employer, i.e., the household's secondary source of income.

## 7 Effect of Exit and Additional Debt Relief Measures

We examine whether the effects of the forbearance program on household behavior persisted after the program ended in September 2021. To conduct this analysis, we extend the sample period through June 2022 and introduce a dummy variable, *Exit Effect*, that takes the value of one between September 2021 and June 2022 and zero otherwise. The regression also includes indicators for the three different horizons presented before (*Immediate Effect*, *Short-run Effect*, and *Long-run Effect*).

Table 9 presents the results. Column (1) shows that consumption is affected by the end of the forbearance program. The *Long-run Effect* dummy, which is set to one starting twelve months after the initiation of forbearance and lasting until the program's end in September 2021, is associated with a 22 cents increase in consumption per euro of forbearance. However, this effect diminishes to about 13 cents after the forbearance period ends, as indicated by the coefficient on the *Exit Effect* dummy. The estimates for saving in deposits in column (3) align with those for consumption. In fact, the long-run effect of forbearance on saving changes significantly after the forbearance period, with saving decreasing from 23 cents per euro to about -12 cents post-forbearance. These findings suggest that households increased their consumption during the forbearance period and did not fully adjust consumption levels downward after its termination, resulting in a drawdown of savings (Chetty and Szeidl, 2007). Table IA.8 of the Internet Appendix further extends the analysis of the propensity to consume upon exiting forbearance, indicating that the rigidity in reducing consumption is primarily driven by spending in “Groceries”, “Clothing”, and “House Maintenance and Utilities”.

In the summer of 2021, and at the regulator's request, banks assessed the risk levels of borrowers in forbearance. For those identified as having a higher risk of default at the end of the forbearance based on a credit risk model, the bank conducted a survey to evaluate whether they should receive additional debt relief measures due to perceived default risk. Furthermore, all borrowers in forbearance were notified via SMS, email, and the bank's

app that additional assistance was available if they had difficulties meeting their debt obligations. Similar to the government forbearance program, access to these additional debt relief measures did not require on a formal verification of the borrower's income or financial hardship. In this section, we compare the evolution of consumption and saving for households that did and did not receive additional debt relief measures implemented by the bank in September 2021.

A more nuanced pattern emerges when examining the additional relief measures. Each of the three horizon dummies is interacted with an additional indicator, *Additional Relief*, to separately identify the effects on households exiting forbearance after September 2021 and those receiving additional assistance. Columns (2) and (4) of Table 9 present the results. Only about 7.4% of ineligible households in forbearance requested additional relief, compared to about 6.1% of eligible households. Column (2) shows that households receiving additional relief in September 2021 did not adjust their consumption behavior after the forbearance period ended, as the point estimate for the long-run effect remains nearly identical to the estimate after the end of forbearance (15 cents per euro of postponed payment). This is unexpected, given the temporary nature of these debt relief measures, but it aligns with the “consumption commitments” model proposed by Chetty and Szeidl (2007) for this subset of borrowers. Despite this, these households that requested additional relief, on average, exhibit lower propensity to consume from forbearance funds and higher propensity to save, as shown in columns (2) and (4). This behavior continues after the transition from the initial government forbearance program to bank-provided assistance, with the marginal propensity to save being 52 cents higher for households that remain in forbearance compared to those who exit the program.

## 8 Conclusion

Government debt relief programs during the COVID-19 pandemic provide a unique laboratory to understand borrower selection into forbearance and borrower response to preemptive interventions even before large-scale defaults. Using comprehensive bank account transaction and balance sheet data, we show that forbearance programs have a positive and significant impact on household consumption and saving. Households in forbearance increased their spending by approximately 15 cents per euro of postponed debt payments compared to those outside forbearance, and increased their saving by around 30 cents. These effects are not only substantial but also persist for over a year following the initiation of forbearance.

Importantly, our findings reveal heterogeneity in responses across different household groups. Financially fragile households, characterized by lower wealth, income, and higher indebtedness, exhibit a higher marginal propensity to consume from postponed payments. In contrast, less fragile households exhibit a greater marginal propensity to save. The response is also heterogeneous based on program eligibility. Both eligible and ineligible households in forbearance were financially more fragile even before the pandemic. Eligible households benefit from forbearance by avoiding consumption reductions due to ongoing debt payments, while ineligible households in forbearance show a relatively higher marginal propensity to consume compared to their counterparts outside forbearance.

These findings have implications for the design of debt relief programs. Our research is the first to examine the consumption and saving responses of both eligible and ineligible households within a large-scale forbearance program, while accounting for key unobserved factors such as changes in income. The results suggest that, although the Portuguese government (and many others globally) aimed to assist those most directly affected by the pandemic, the program was also accessed by households that were already financially fragile before the crisis, who were not the intended targets. Financially fragile households



were also more likely to increase their consumption, leading many to exit forbearance with higher debt burdens (due to postponed payments) and only modest improvements in savings, potentially exacerbating their financial fragility.

Our study highlights a potential unintended consequence of broad-based forbearance programs: while the comprehensive measures implemented at the start of the pandemic provided temporary relief and helped stabilize household finances, likely preventing a wave of defaults globally, these programs may have also increased indebtedness among already fragile households. Policymakers could improve the effectiveness of debt relief measures by using observable household characteristics, commonly available to lenders, and considering how different groups respond to forbearance when designing future interventions. This targeted approach could be complemented by additional policies aimed at supporting household financial health, thereby reducing the risk of long-term financial instability and ensuring that the primary goal of these programs—providing relief without exacerbating debt burdens—is achieved.

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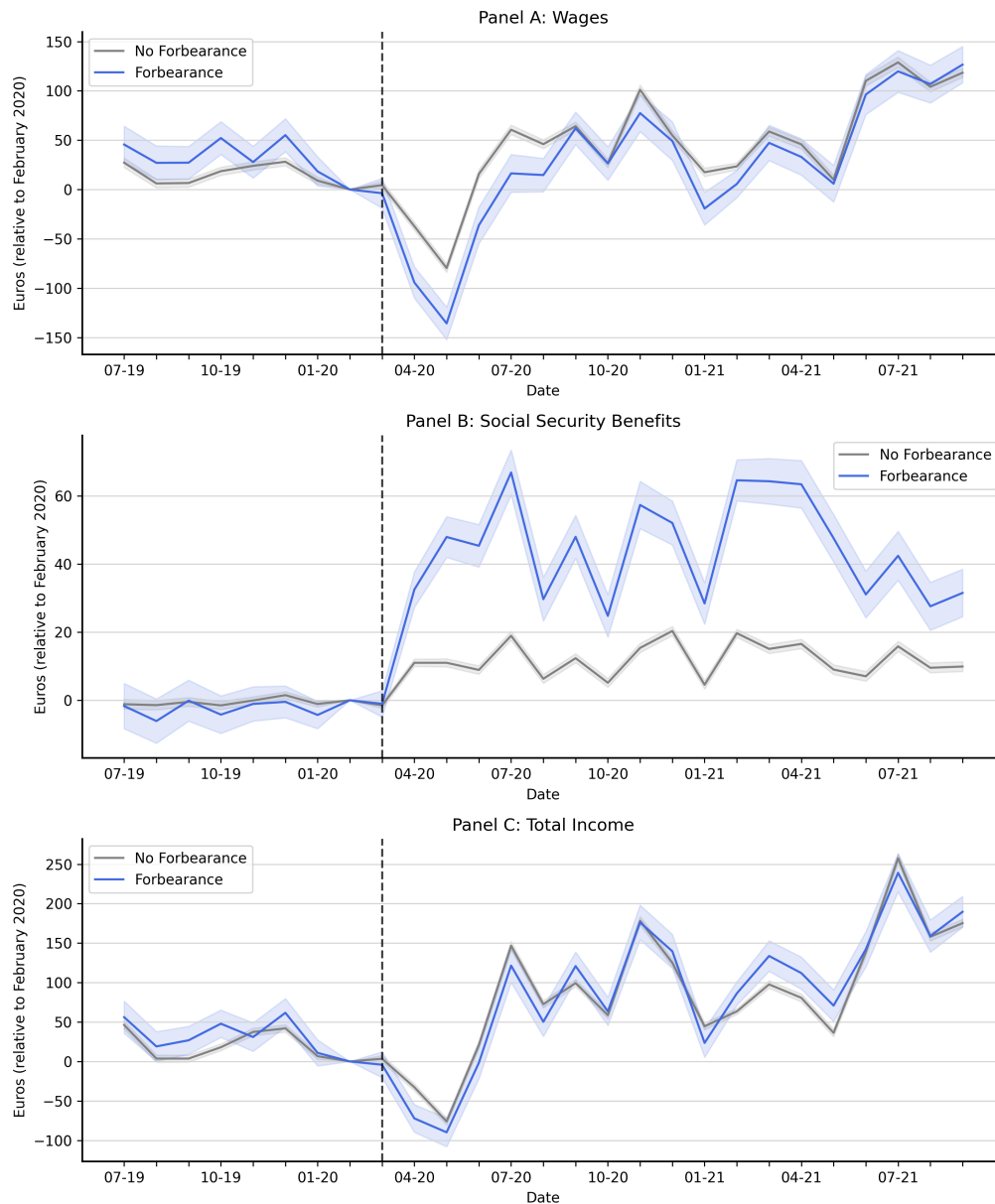
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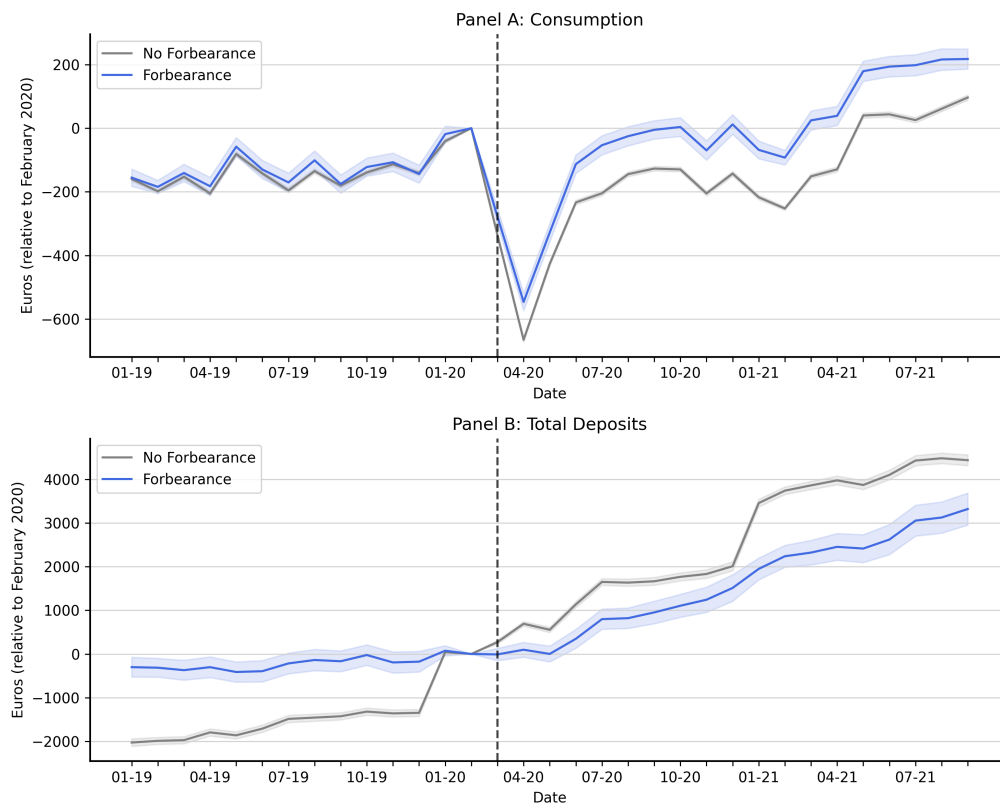
**Figure 1: Evolution of Income by Forbearance Status**



This figure shows the household average and 95% confidence interval for different components of monthly income from July 2019 to September 2021. All measures are reported in euros, seasonally adjusted and relative to a pre-pandemic baseline (February 2020). Panel A shows the change in monthly wages. Panel B shows the change in social security benefits. Panel C shows the change in total monthly income, computed as the sum between monthly wages, social security, and retirement benefits. In all panels, the average change is presented separately for households who received forbearance and those who never received forbearance. Standard errors are clustered by household.

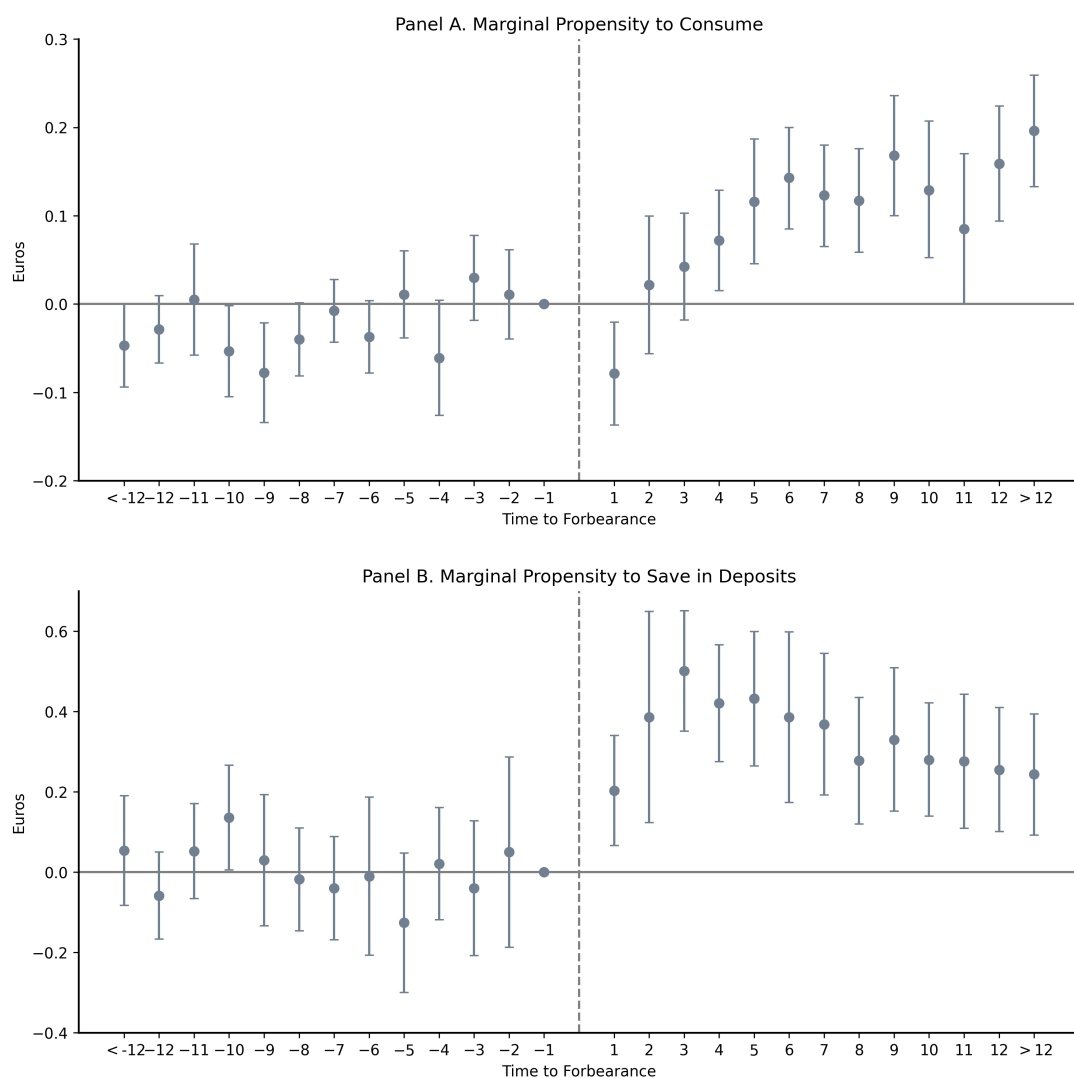


**Figure 2:** Evolution of Consumption and Deposits by Forbearance Status



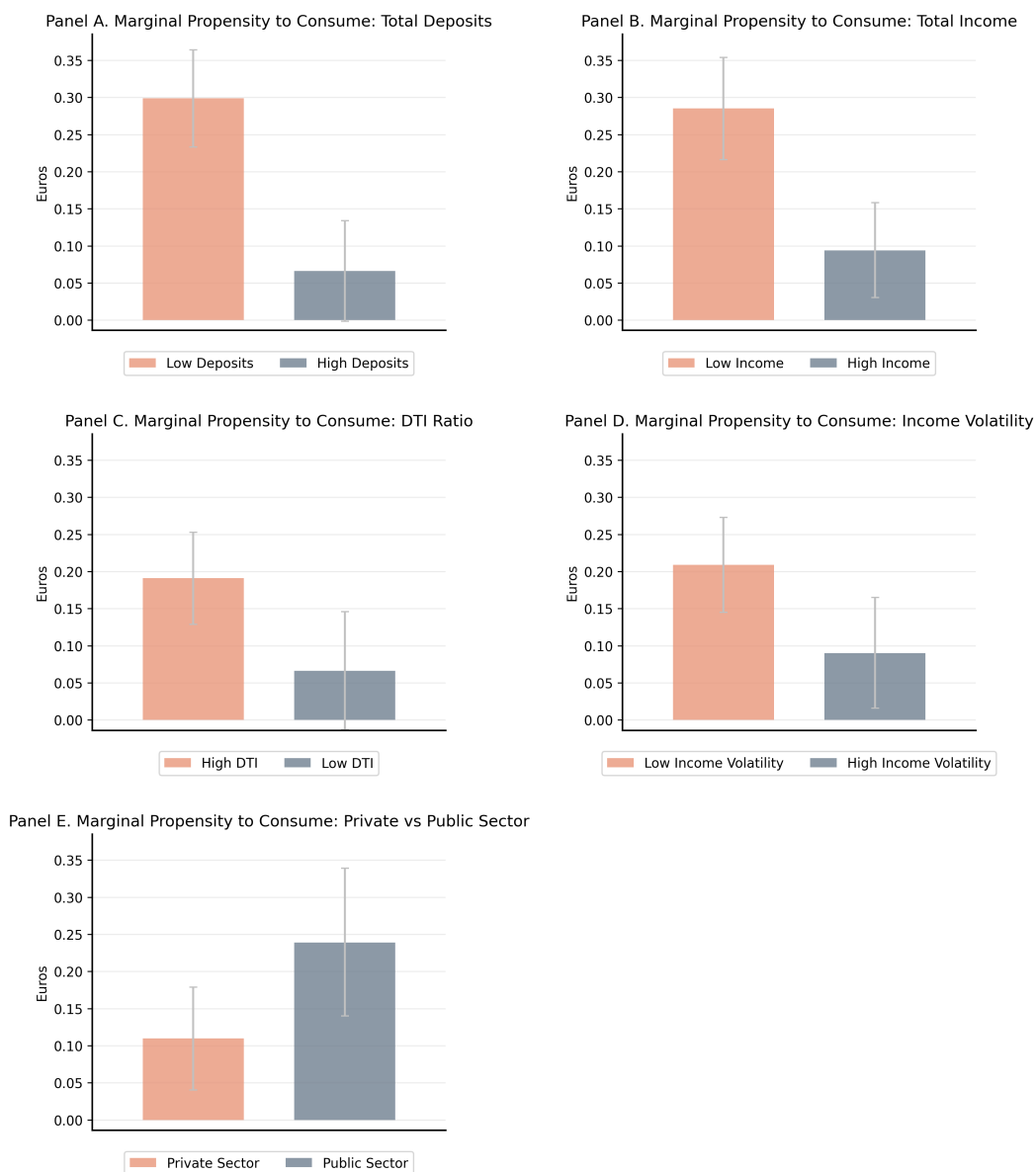
This figure shows the household average and 95% confidence interval for monthly consumption and total deposits from January 2019 to September 2021. All measures are reported in euros, seasonally adjusted, and relative to the pre-pandemic baseline (February 2020). Panel A shows the change in monthly consumption. Panel B shows the change in total deposits, computed as the sum between end-of-the-month checking and saving accounts' balances. In both panels, the average change is presented separately for households who received forbearance and those who never received forbearance. Standard errors are clustered by household.

**Figure 3:** Household Propensities to Consume and Save in Deposits



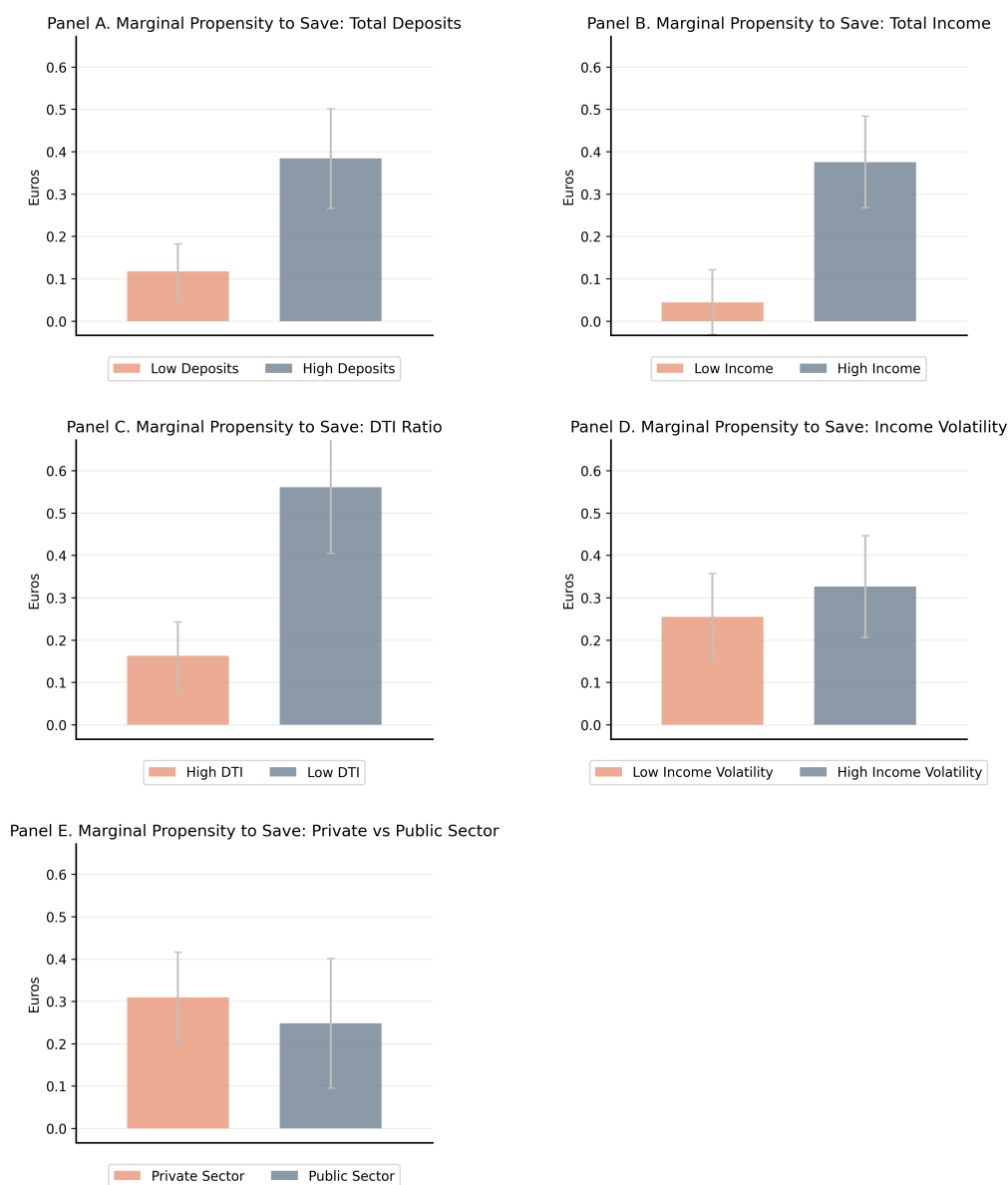
This figure shows the regression coefficients and 95% confidence intervals of the changes in monthly consumption and saving in total deposits using the difference-in-differences regression in equation (2). Panel A shows the estimates of the change in monthly spending per euro of postponed debt payment. Panel B shows the estimates of the change in saving in total deposits per euro of postponed debt payment. The specification includes group-month-year fixed effects and includes as control variables the changes in total income and the forbearance dummy variable that takes the value of one for households receiving forbearance, and zero otherwise. Standard errors are two-way clustered by household and month-year.

**Figure 4:** Household Propensity to Consume: Wealth, Income, Indebtedness and Employer Sector



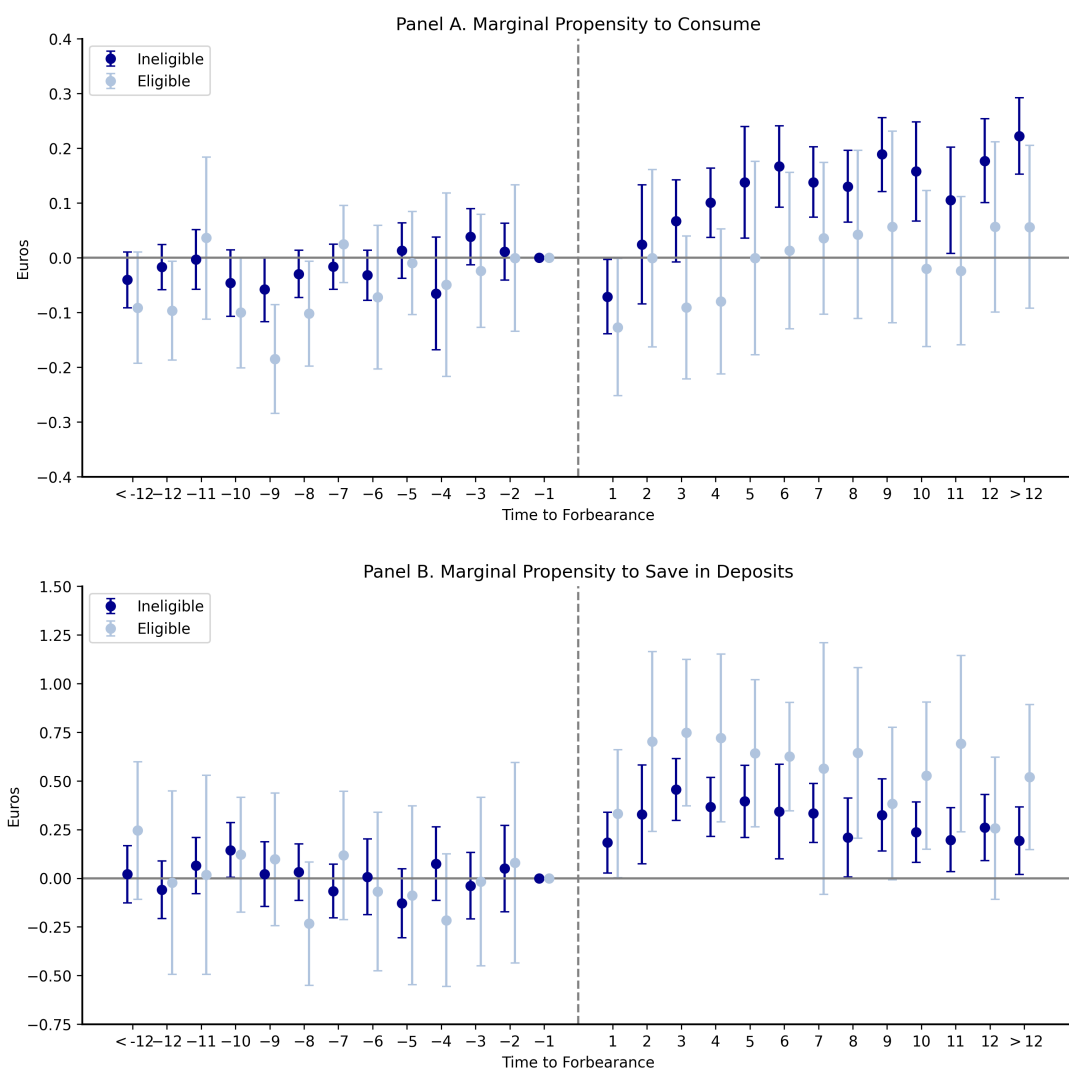
This figure shows the regression coefficients and 95% confidence intervals of the changes in monthly consumption using the difference-in-differences regression in equation (1). Panels A-D show the estimates for subgroups of households based on the pre-pandemic median of assets, income, debt payment-to-income ratio, and income volatility (2019 average). Panel E shows the estimates for subgroups of households whose primary employer is from the private and public sector. The specification includes group-month-year fixed effects and includes as control variables the changes in total income and the *Forbearance* dummy variable. Standard errors are two-way clustered by household and month-year.

**Figure 5:** Household Propensity to Save: Wealth, Income, Indebtedness and Employer Sector



This figure shows the regression coefficients and 95% confidence intervals of the changes in monthly saving in total deposits using the difference-in-differences regression in equation (1). Panels A-D shows the estimates for subgroups of households based on the pre-pandemic median of assets, income, debt payment-to-income ratio, and income volatility (2019 average). Panel E shows the estimates for subgroups of households whose primary employer is from the private or public sector. The specification includes group-month-year fixed effects and includes as control variables the changes in total income and the *Forbearance* dummy variable. Standard errors are two-way clustered by household and month-year.

**Figure 6:** Household Propensities to Consume and Save in Deposits: By Eligibility Status



This figure shows the regression coefficients and 95% confidence intervals of the changes in monthly consumption and saving in total deposits using the difference-in-differences regression in equation (2). The *Forbearance Amount* variable is further interacted with the *Eligibility* dummy variable. Panel A shows the estimates of the change in monthly spending per euro of postponed debt payment. Panel B shows the estimates of the change in saving in total deposits per euro of postponed debt payment. The specification includes group-month-year fixed effects and includes as control variables the changes in total income and the *Forbearance* dummy variable. Standard errors are two-way clustered by household and month-year.

**Table 1:** Average Household Characteristics by Groups

	No Forbearance			Forbearance		
	Total	Eligible	Ineligible	Total	Eligible	Ineligible
Wages	1,837	1,885	1,830	1,466	1,620	1,416
Social Security Benefits	346	350	345	300	303	299
Total Income	2,553	2,759	2,531	2,121	2,449	2,046
Consumption	1,516	1,615	1,506	1,343	1,441	1,320
Consumption Rate (%)	75.6	67.2	76.6	88.1	66.9	93.0
Total Deposits	18,855	17,854	18,962	7,581	7,449	7,612
Mortgage Loans	67,803	73,339	67,211	93,077	96,256	92,346
Credit Cards and Overdraft	399	375	401	749	654	771
Other Banks' Debt	7,044	7,417	7,005	13,885	13,457	13,984
Debt Payment	311	305	312	377	360	381
Debt Payment-to-Income (%)	18.7	14.7	19.1	29.8	19.6	32.2
Forbearance Amount	0	0	0	345	333	347
Observations	129,201	12,469	116,732	8,162	1,525	6,637

This table shows pre-pandemic (2019) averages for households who received forbearance and those who never received forbearance, further dividing those two groups on whether they were eligible or not according to program rules. Income, deposits, liabilities and consumption measures are winsorized at the top and bottom 1%.

**Table 2:** Household Propensities to Consume and Save in Deposits

	$\Delta$ Consumption				$\Delta$ Saving in Deposits			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Forbearance Amount $\times$ Post	0.152*** (0.033)	0.133*** (0.030)	0.147*** (0.028)	0.137*** (0.027)	0.122 (0.075)	0.182*** (0.047)	0.293*** (0.041)	0.321*** (0.042)
$\Delta$ Income			0.085*** (0.003)	0.083*** (0.003)			0.695*** (0.009)	0.704*** (0.009)
$\Delta$ Income $\times$ Forbearance				0.050*** (0.003)				-0.155*** (0.012)
Month $\times$ Year FE	Yes	No	No	No	Yes	No	No	No
Group $\times$ Month $\times$ Year FE	No	Yes	Yes	Yes	No	Yes	Yes	Yes
$R^2$	0.062	0.082	0.102	0.102	0.018	0.026	0.157	0.157
Observations	6,181,335	6,181,335	6,181,335	6,181,335	6,043,972	6,043,972	6,043,972	6,043,972

This table presents difference-in-differences estimates of regressions of changes in monthly consumption and saving in total deposits using the difference-in-differences regression in equation (1) at the household-month level. The dependent variable in columns (1)-(4),  $\Delta$ Consumption, is the difference between monthly spending at time  $t$ , relative to the corresponding household average in 2019. The dependent variable in columns (5)-(8),  $\Delta$ Saving in Deposits, is the monthly saving in total deposits at time  $t$ , relative to the corresponding household average in 2019. Forbearance Amount is the amount of postponed debt payments. Post is a dummy variable that takes a value of one for the period starting when mortgage payments are suspended and zero otherwise. All specifications include the Forbearance dummy variable as a control variable. In some specifications, the regressions include group-month-year fixed effects, with group referring to quartiles of pre-pandemic deposits and income (2019 averages), and changes in total income relative to the corresponding household average in 2019 as a control. The sample period is from January 2018 to September 2021. Two-way standard errors clustered by household and month-year are reported in parentheses. \*, \*\*, and \*\*\* indicate statistical significance at the 10%, 5%, and 1% level, respectively.

**Table 3:** Household Propensities to Consume and Save in Deposits by Horizon

	$\Delta$ Consumption			$\Delta$ Saving in Deposits		
	(1)	(2)	(3)	(4)	(5)	(6)
Forbearance Amount $\times$						
Immediate Effect (1m-3m)	0.038 (0.039)	-0.019 (0.032)	0.018 (0.035)	-0.165 (0.144)	0.045 (0.080)	0.345*** (0.055)
Short-run Effect (4m-12m)	0.153*** (0.029)	0.133*** (0.024)	0.146*** (0.023)	0.186** (0.092)	0.213*** (0.056)	0.318*** (0.042)
Long-run Effect (>12m)	0.211*** (0.029)	0.215*** (0.025)	0.218*** (0.024)	0.173 (0.110)	0.206*** (0.062)	0.226*** (0.044)
$\Delta$ Income			0.085*** (0.003)			0.695*** (0.009)
Month $\times$ Year FE	Yes	No	No	Yes	No	No
Group $\times$ Month $\times$ Year FE	No	Yes	Yes	No	Yes	Yes
$R^2$	0.062	0.082	0.102	0.018	0.026	0.157
Observations	6,181,335	6,181,335	6,181,335	6,043,972	6,043,972	6,043,972

This table presents difference-in-differences estimates of regressions of changes in monthly consumption and saving in total deposits using the difference-in-differences regression in equation (1) at the household-month level, but replacing the *Post* indicator with three different time dummy variables: the *Immediate Effect* measures the impact over the first quarter after the start of forbearance; the *Short-run Effect* measures the average effect from the 4<sup>th</sup> to the 12<sup>th</sup> month; and the *Long-run Effect* measures the average effect after one year. The dependent variable in columns (1)-(3),  $\Delta$ Consumption, is the difference between monthly spending at time  $t$ , relative to the corresponding household average in 2019. The dependent variable in columns (4)-(6),  $\Delta$ Saving in Deposits, is the monthly saving in total deposits at time  $t$ , relative to the corresponding household average in 2019. *Forbearance Amount* is the amount of postponed debt payments. All specifications include the *Forbearance* dummy variable as a control variable. In some specifications, the regressions include group-month-year fixed effects, with group referring to quartiles of pre-pandemic deposits and income (2019 averages), and changes in total income relative to the corresponding household average in 2019 as a control. The sample period is from January 2018 to September 2021. Two-way standard errors clustered by household and month-year are reported in parentheses. \*, \*\*, and \*\*\* indicate statistical significance at the 10%, 5%, and 1% level, respectively.



**Table 4:** Effect of Forbearance on Consumption and Saving Rates

	Consumption Rate			Saving Rate		
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Overall						
Forbearance $\times$ Post	0.118*** (0.011)	0.064*** (0.008)	0.063*** (0.007)	0.040* (0.020)	0.048*** (0.014)	0.045*** (0.010)
% $\Delta$ Income			0.148*** (0.009)			0.486*** (0.021)
Panel B: Horizons						
Forbearance Amount $\times$						
Immediate Effect (1m-3m)	0.055*** (0.016)	0.017 (0.011)	0.024* (0.013)	-0.023 (0.037)	0.013 (0.023)	0.037*** (0.013)
Short-run Effect (4m-12m)	0.111*** (0.008)	0.063*** (0.006)	0.062*** (0.006)	0.050** (0.024)	0.051*** (0.018)	0.048*** (0.014)
Long-run Effect (>12m)	0.163*** (0.009)	0.092*** (0.008)	0.087*** (0.007)	0.060** (0.029)	0.064*** (0.021)	0.045*** (0.013)
% $\Delta$ Income			0.148*** (0.009)			0.486*** (0.021)
Month $\times$ Year FE	Yes	No	No	Yes	No	No
Group $\times$ Month $\times$ Year FE	No	Yes	Yes	No	Yes	Yes
$R^2$	0.052	0.084	0.129	0.012	0.015	0.093
Observations	6,170,985	6,170,985	6,170,985	6,033,852	6,033,852	6,033,852

This table presents difference-in-differences estimates of regressions of changes in the consumption rate and saving rate using the difference-in-differences regression in equation (3) at the household-month level. The dependent variable in columns (1)-(3), *Consumption Rate*, is the difference between monthly spending at time  $t$  and the corresponding household average in 2019, divided by the average income in 2019. The dependent variable in columns (4)-(6), *Saving Rate*, is the monthly saving in total deposits at time  $t$  and the corresponding household average in 2019, divided by the average income in 2019. *Forbearance* is a dummy variable that takes the value of one for households receiving forbearance, and zero otherwise. *Post* is a dummy variable that takes a value of one for the period starting when mortgage payments are suspended and zero otherwise. In Panel B, the *Post* indicator is replaced by three different time dummy variables: the *Immediate Effect* measures the impact over the first quarter after the start of forbearance; the *Short-run Effect* measures the average effect from the 4<sup>th</sup> to the 12<sup>th</sup> month; and the *Long-run Effect* measures the average effect after one year. All specifications include the *Forbearance* dummy variable as a control variable. In some specifications, the regressions include group-month-year fixed effects, with group referring to quartiles of pre-pandemic deposits and income (2019 averages), and percentage changes in total income relative to the corresponding household average in 2019 as a control. The sample period is from January 2018 to September 2021. Two-way standard errors clustered by household and month-year are reported in parentheses. \*, \*\*, and \*\*\* indicate statistical significance at the 10%, 5%, and 1% level, respectively.

**Table 5: Household Propensity to Borrow**

	$\Delta$ Borrowing Credit Card & Overdraft			$\Delta$ Borrowing Other Banks' Debt		
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Overall						
Forbearance Amount $\times$ Post	-0.009 (0.011)	-0.009 (0.009)	-0.011 (0.009)	-0.053* (0.026)	-0.033 (0.025)	-0.029 (0.025)
$\Delta$ Income			-0.007*** (0.001)			0.022*** (0.002)
Panel B: Horizons						
Forbearance Amount $\times$						
Immediate Effect (1m-3m)	-0.043** (0.020)	-0.034 (0.020)	-0.037* (0.019)	-0.036 (0.027)	-0.006 (0.026)	0.003 (0.028)
Short-run Effect (4m-12m)	-0.001 (0.012)	-0.005 (0.009)	-0.006 (0.009)	-0.051 (0.037)	-0.034 (0.035)	-0.031 (0.035)
Long-run Effect (>12m)	-0.005 (0.015)	-0.004 (0.011)	-0.004 (0.011)	-0.064*** (0.021)	-0.045** (0.021)	-0.044** (0.021)
$\Delta$ Income			-0.007*** (0.001)			0.022*** (0.002)
Month $\times$ Year FE	Yes	No	No	Yes	No	No
Group $\times$ Month $\times$ Year FE	No	Yes	Yes	No	Yes	Yes
$R^2$	0.012	0.021	0.023	0.001	0.003	0.004
Observations	6,043,972	6,043,972	6,043,972	4,395,616	4,395,616	4,395,616

This table presents difference-in-differences estimates of regressions of changes in credit card and overdraft, and other banks' debt payments using the difference-in-differences regression in equation (1) at the household-month level. The dependent variable in columns (1)-(3),  $\Delta$ Borrowing Credit Card & Overdraft, is the difference in monthly changes in end-of-the-month credit card and overdraft balances at time  $t$  at the bank, relative to the corresponding household average in 2019. The dependent variable in columns (4)-(6),  $\Delta$ Borrowing Other Banks' Debt, is the difference between monthly changes in liabilities at other banks at time  $t$ , relative to the corresponding household average in 2019. *Forbearance Amount* is the amount of postponed debt payments. *Post* is a dummy variable that takes a value of one for the period starting when mortgage payments are suspended and zero otherwise. In Panel B, the *Post* indicator is replaced by three different time dummy variables: the *Immediate Effect* measures the impact over the first quarter after the start of forbearance; the *Short-run Effect* measures the average effect from the 4<sup>th</sup> to the 12<sup>th</sup> month; and the *Long-run Effect* measures the average effect after one year. All specifications include the *Forbearance* dummy variable as a control variable. All specifications include the *Forbearance* dummy variable as a control variable. In some specifications, the regressions include group-month-year fixed effects, with group referring to quartiles of pre-pandemic deposits and income (2019 averages), and changes in total income relative to the corresponding household average in 2019 as a control. The sample period in columns (1)-(3) is from January 2018 to September 2021. The sample period in columns (4)-(6), due to data limitations, is from January 2019 to September 2021. Two-way standard errors clustered by household and month-year are reported in parentheses. \*, \*\*, and \*\*\* indicate statistical significance at the 10%, 5%, and 1% level, respectively.

**Table 6: Household Propensity to Consume by Category**

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Total	Groc.	Cloth.	House Maint.	Furnit.	Transp.	Health Care	Restau.	Entert. & Educ.	Misc.
Panel A: Overall										
Forbearance Amount $\times$ Post	0.095*** (0.029)	0.055*** (0.006)	0.014*** (0.003)	0.005*** (0.002)	0.006** (0.002)	0.007 (0.006)	0.006** (0.003)	-0.000 (0.006)	0.009** (0.004)	0.002 (0.013)
$\Delta$ Income	0.078*** (0.003)	0.008*** (0.000)	0.003*** (0.000)	0.001*** (0.000)	0.003*** (0.000)	0.006*** (0.000)	0.002*** (0.000)	0.005*** (0.000)	0.003*** (0.000)	0.028*** (0.001)
Panel B: Horizons										
Forbearance Amount $\times$										
Immediate Effect (1m-3m)	-0.033 (0.039)	0.036*** (0.008)	0.004 (0.004)	0.005** (0.002)	0.003 (0.004)	-0.009 (0.008)	-0.001 (0.003)	-0.025*** (0.005)	-0.003 (0.004)	-0.027* (0.015)
Short-run Effect (4m-12m)	0.092*** (0.025)	0.057*** (0.006)	0.015*** (0.003)	0.004** (0.002)	0.005** (0.002)	0.005 (0.005)	0.008** (0.003)	-0.003 (0.007)	0.010** (0.004)	0.002 (0.013)
Long-run Effect (>12m)	0.167*** (0.026)	0.061*** (0.006)	0.020*** (0.003)	0.005** (0.002)	0.009*** (0.003)	0.020*** (0.005)	0.008** (0.003)	0.017*** (0.006)	0.013** (0.005)	0.017 (0.015)
$\Delta$ Income	0.078*** (0.003)	0.008*** (0.000)	0.003*** (0.000)	0.001*** (0.000)	0.003*** (0.000)	0.006*** (0.000)	0.002*** (0.000)	0.005*** (0.000)	0.003*** (0.000)	0.028*** (0.001)
Group $\times$ Month $\times$ Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
$R^2$	0.095	0.028	0.049	0.007	0.010	0.043	0.010	0.067	0.017	0.036
Observations	2,884,623	2,884,623	2,884,623	2,884,623	2,884,623	2,884,623	2,884,623	2,884,623	2,884,623	2,884,623

This table presents difference-in-differences estimates of regressions of changes in monthly consumption for different spending categories using the difference-in-differences regression in equation (1) at the household-month level. The dependent variable is measured as the difference between monthly spending at time  $t$ , relative to the corresponding household average in January and February of 2020, for each consumption category: (1) Total Consumption; (2) Groceries; (3) Clothing; (4) Housing Maintenance and Utilities; (5) Furniture; (6) Transport; (7) Health Care; (8) Restaurants; (9) Entertainment and Education; and (10) Miscellaneous Goods and Services. *Forbearance Amount* is the amount of postponed debt payments. *Post* is a dummy variable that takes a value of one for the period starting when mortgage payments are suspended and zero otherwise. In Panel B, the *Post* indicator is replaced by three different time dummy variables: the *Immediate Effect* measures the impact over the first quarter after the start of forbearance; the *Short-run Effect* measures the average effect from the 4<sup>th</sup> to the 12<sup>th</sup> month; and the *Long-run Effect* measures the average effect after one year. In all specifications, the regressions include group-month-year fixed effects, with group referring to quartiles of pre-pandemic deposits and income (2019 averages), and changes in total income relative to the corresponding household average in January and February 2020 as a control. The sample period is from January 2020 to September 2021. Two-way standard errors clustered by household and month-year are reported in parentheses. \*, \*\*, and \*\*\* indicate statistical significance at the 10%, 5%, and 1% level, respectively.

**Table 7:** Household Propensities to Consume and Save in Deposits by Eligibility and Forbearance Status

	$\Delta$ Consumption			$\Delta$ Saving in Deposits		
	(1)	(2)	(3)	(4)	(5)	(6)
Debt Payment $\times$						
Forbearance=0 $\times$ Eligible=1 $\times$ Post	-0.242*** (0.038)	-0.152*** (0.024)	-0.135*** (0.021)	-0.131* (0.069)	-0.133* (0.068)	0.012 (0.050)
Forbearance=1 $\times$ Eligible=0 $\times$ Post	0.195*** (0.036)	0.169*** (0.033)	0.177*** (0.030)	0.144* (0.085)	0.210*** (0.051)	0.275*** (0.044)
Forbearance=1 $\times$ Eligible=1 $\times$ Post	0.011 (0.056)	0.015 (0.052)	0.065 (0.048)	0.019 (0.112)	0.075 (0.105)	0.486*** (0.083)
Controls	No	No	Yes	No	No	Yes
Month $\times$ Year FE	Yes	No	No	Yes	No	No
Group $\times$ Month $\times$ Year FE	No	Yes	Yes	No	Yes	Yes
$R^2$	0.062	0.082	0.102	0.018	0.026	0.157
Observations	6,181,335	6,181,335	6,181,335	6,043,972	6,043,972	6,043,972
Observations:						
Forbearance=0 $\times$ Eligible=0	116,732					
% of sample	(85.0%)					
Forbearance=0 $\times$ Eligible=1	12,469					
% of sample	(9.1%)					
Forbearance=1 $\times$ Eligible=0	6,637					
% of sample	(4.8%)					
Forbearance=1 $\times$ Eligible=1	1,525					
% of sample	(1.1%)					

This table presents difference-in-differences estimates of regressions of changes in monthly consumption and saving in total deposits using the difference-in-differences regression in equation (1) at the household-month date level. The dependent variable in columns (1)-(3),  $\Delta$ Consumption, is the difference between monthly spending at time  $t$ , relative to the corresponding household average in 2019. The dependent variable in columns (4)-(6),  $\Delta$ Saving in Deposits, is the monthly saving in total deposits at time  $t$ , relative to the corresponding household average in 2019. *Debt Payment* is the pre-pandemic amount of debt payments of each household. *Forbearance* is a dummy variable that takes the value of one for households receiving forbearance and zero otherwise. *Eligible* is a dummy variable that takes the value of one if the household satisfied the legal requirements for suspending mortgage payments. *Post* is a dummy variable that takes a value of one for the period starting when mortgage payments are suspended and zero otherwise. All specifications include the *Forbearance* dummy variable as a control variable. In some specifications, the regressions include group-month-year fixed effects, with group referring to quartiles of pre-pandemic deposits and income (2019 averages), and changes in total income relative to the corresponding household average in January and February 2020 as a control. The sample period is from January 2018 to September 2021. Two-way standard errors clustered by household and month-year are reported in parentheses. \*, \*\*, and \*\*\* indicate statistical significance at the 10%, 5%, and 1% level, respectively.

**Table 8:** Propensity to Consume and Save in Deposits by Eligibility and Selection Groups: More Affected versus Less Affected

	$\Delta$ Consumption			$\Delta$ Saving in Deposits		
	More Affected (1)	Less Affected (2)	Public Servants (3)	More Affected (4)	Less Affected (5)	Public Servants (6)
Debt Payment $\times$						
Forbearance=0 $\times$ Eligible=1 $\times$ Post	-0.123*** (0.033)	-0.085** (0.036)	-0.076 (0.046)	0.013 (0.077)	0.072 (0.083)	-0.011 (0.107)
Forbearance=1 $\times$ Eligible=0 $\times$ Post	0.139*** (0.051)	0.190*** (0.061)	0.230*** (0.053)	0.180* (0.094)	0.236** (0.088)	0.269*** (0.081)
Forbearance=1 $\times$ Eligible=1 $\times$ Post	0.030 (0.069)	0.196** (0.085)	0.299** (0.114)	0.654*** (0.119)	0.135 (0.113)	0.273 (0.263)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Group $\times$ Month $\times$ Year FE	Yes	Yes	Yes	Yes	Yes	Yes
$R^2$	0.109	0.105	0.106	0.176	0.167	0.163
Observations	919,080	1,327,095	1,719,855	898,656	1,297,604	1,681,636
Observations:						
Forbearance=0 $\times$ Eligible=0	13,485	23,899	35,241			
% of sample	(66.0%)	(81.0%)	(92.2%)			
Forbearance=0 $\times$ Eligible=1	5,176	4,103	1,775			
% of sample	(25.3%)	(13.9%)	(4.6%)			
Forbearance=1 $\times$ Eligible=0	904	1,076	1,096			
% of sample	(4.4%)	(3.6%)	(2.9%)			
Forbearance=1 $\times$ Eligible=1	859	413	107			
% of sample	(4.2%)	(1.4%)	(0.3%)			

This table presents difference-in-differences estimates of regressions of changes in monthly consumption and saving in total deposits using the difference-in-differences regression in equation (1) at the household-month level. The dependent variable in columns (1)-(3),  $\Delta$ Consumption, is the difference between monthly spending at time  $t$ , relative to the corresponding household average in 2019. The dependent variable in columns (4)-(6),  $\Delta$ Saving in Deposits, is the monthly saving in total deposits at time  $t$ , relative to the corresponding household average in 2019. The *More Affected* subsample corresponds to households whose primary employer operates in an industry with revenue growth from 2019 to 2020 below the median. The *Less Affected* subsample corresponds to households whose primary employer operates in an industry with revenue growth from 2019 to 2020 above the median. The *Public Servants* subsample is defined as households whose primary employer operated in the public sector. *Debt Payment* is the amount of pre-pandemic debt payments made by households. *Forbearance* is a dummy variable that takes the value of one for households receiving forbearance and zero otherwise. *Eligible* is a dummy variable that takes the value of one if the household satisfied the legal requirements for suspending mortgage payments. *Post* is a dummy variable that takes a value of one for the period starting when mortgage payments are suspended and zero otherwise. All specifications include the *Forbearance* dummy variable as a control variable. In some specifications, the regressions include group-month-year fixed effects, with group referring to quartiles of pre-pandemic deposits and income (2019 averages), and changes in total income relative to the corresponding household average in 2019 as a control. The sample period is from January 2018 to September 2021. Two-way standard errors clustered by household and month-year are reported in parentheses. \*, \*\*, and \*\*\* indicate statistical significance at the 10%, 5%, and 1% level, respectively.

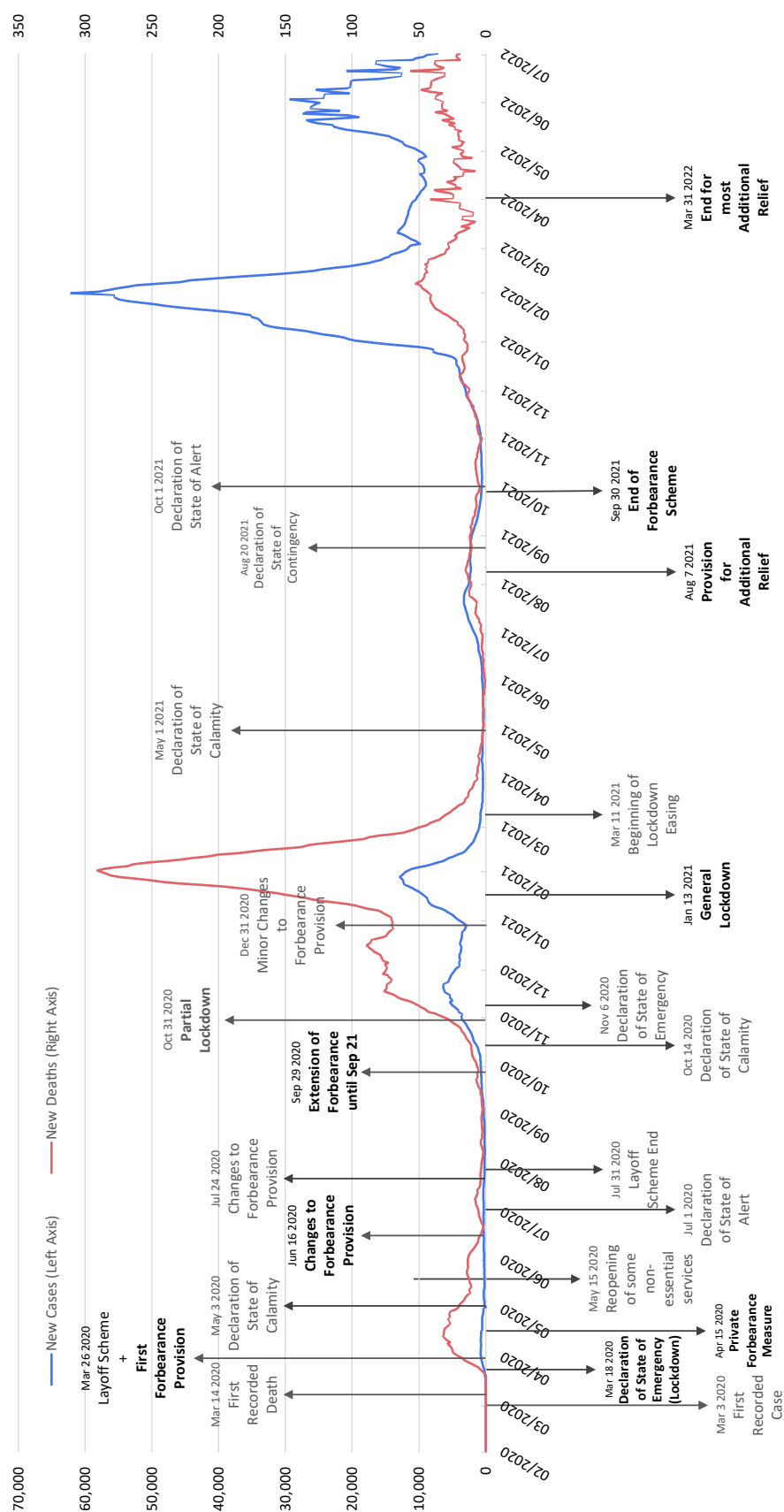
**Table 9:** Household Propensities to Consume and Save in Deposits on Exit

	$\Delta$ Consumption		$\Delta$ Saving in Deposits	
	(1)	(2)	(3)	(4)
Forbearance Amount $\times$				
Immediate Effect (1-3)	0.024 (0.036)	0.026 (0.038)	0.343*** (0.056)	0.336*** (0.058)
Short-run Effect (4-12)	0.150*** (0.024)	0.157*** (0.025)	0.319*** (0.045)	0.296*** (0.045)
Long-run Effect (>12)	0.221*** (0.025)	0.228*** (0.026)	0.228*** (0.046)	0.206*** (0.047)
Exit Effect	0.126*** (0.028)	0.124*** (0.029)	-0.120** (0.054)	-0.164*** (0.056)
Immediate Effect (1-3) $\times$ Additional Relief		-0.029 (0.077)		0.080 (0.070)
Short-run Effect (4-12) $\times$ Additional Relief		-0.083 (0.084)		0.273** (0.123)
Long-run Effect (>12) $\times$ Additional Relief		-0.075 (0.107)		0.267* (0.142)
Exit Effect $\times$ Additional Relief		0.026 (0.100)		0.522*** (0.152)
Controls	Yes	Yes	Yes	Yes
Group $\times$ Month $\times$ Year FE	Yes	Yes	Yes	Yes
$R^2$	0.101	0.101	0.152	0.152
Observations	7,417,602	7,417,602	7,280,239	7,280,239
Observations:				
Forbearance=1 $\times$ Eligible=0		6,637		
Forbearance=1 $\times$ Eligible=0 $\times$ Additional Relief=1		490		
% of group		(7.4%)		
Forbearance=1 $\times$ Eligible=1		1,525		
Forbearance=1 $\times$ Eligible=1 $\times$ Additional Relief=1		93		
% of group		(6.1%)		

This table presents difference-in-differences estimates of regressions of changes in monthly consumption and saving in total deposits using the difference-in-differences regression in equation (1) at the household-month level, but replacing the *Post* indicator with four different time dummy variables: the *Immediate Effect* measures the impact over the first quarter after the start of forbearance; the *Short-run Effect* measures the average effect from the 4<sup>th</sup> to the 12<sup>th</sup> month; the *Long-run Effect* measures the average effect after one year and until the end of the payment suspension; and the *Exit Effect* measures the average effect after the end of the payment suspension. The dependent variable in columns (1)-(2),  $\Delta$ Consumption, is the difference between monthly spending at time  $t$ , relative to the corresponding household average in 2019. The dependent variable in columns (3)-(4),  $\Delta$ Saving in Deposits, is the monthly saving in total deposits at time  $t$ , relative to the corresponding household average in 2019. *Forbearance Amount* is the amount of postponed debt payments. *Additional Relief* is a dummy variable that takes a value of one if the household requested additional relief after the forbearance end. All specifications include the *Forbearance* dummy variable as a control variable. All specifications include group-month-year fixed effects, with group referring to quartiles of pre-pandemic deposits and income (2019 averages), and changes in total income relative to the corresponding household average in 2019 as a control. The sample period is from January 2018 to June 2022. Two-way standard errors clustered by household and month-year are reported in parentheses. \*, \*\*, and \*\*\* indicate statistical significance at the 10%, 5%, and 1% level, respectively.

**Internet Appendix for**  
**“The Heterogeneous Effects of Household Debt Relief”**

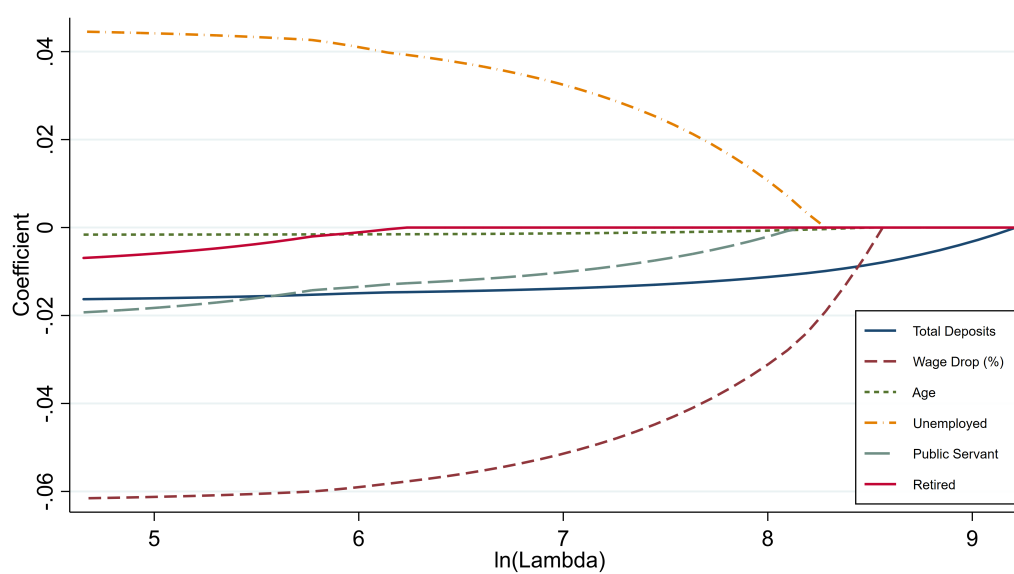
Figure IA.1: Timeline of Events



This figure shows the timeline of events (key events being identified in bold) from February 2020 until July 2022. The left axis variable (in blue) is the number of new COVID-19 cases. The right axis variable (in red) is the number of new deaths due to COVID-19.

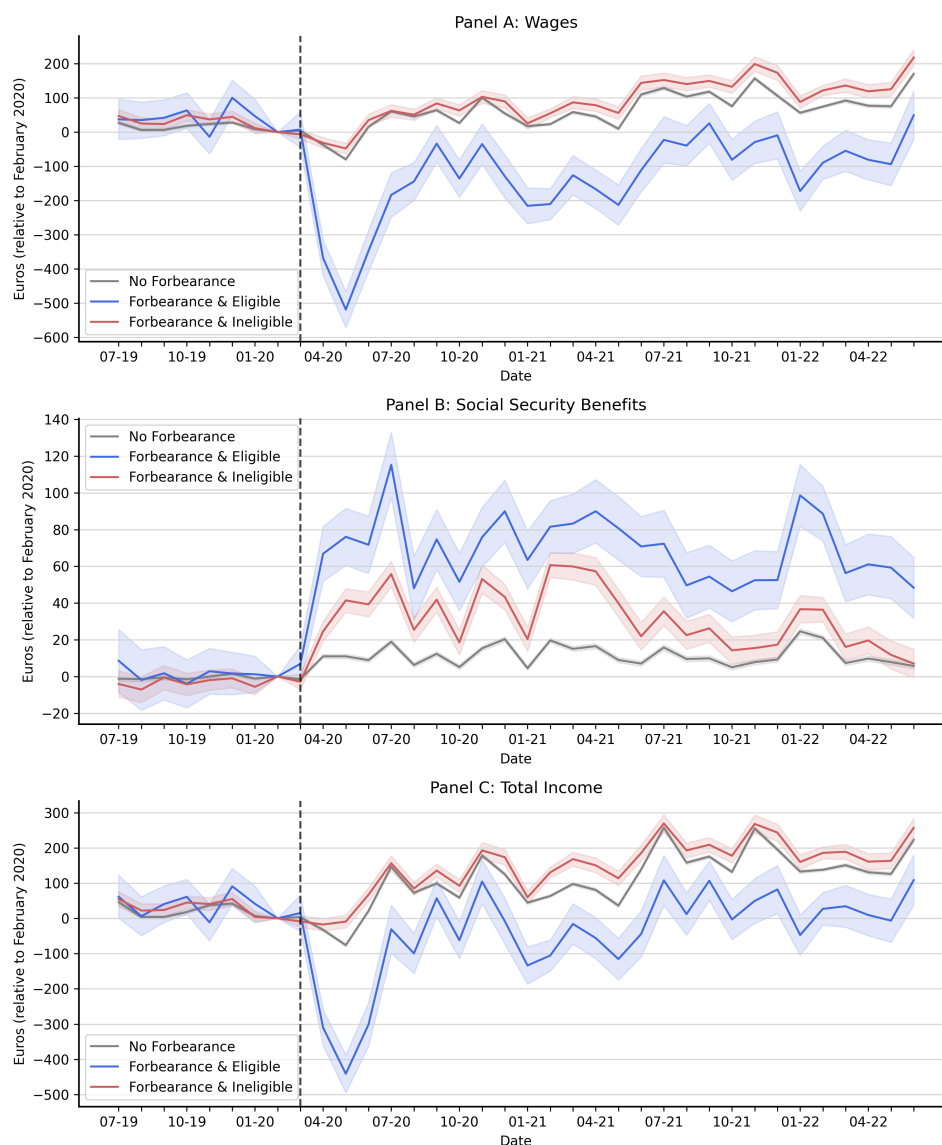


**Figure IA.2:** Probability of Accessing Forbearance: Coefficient Path



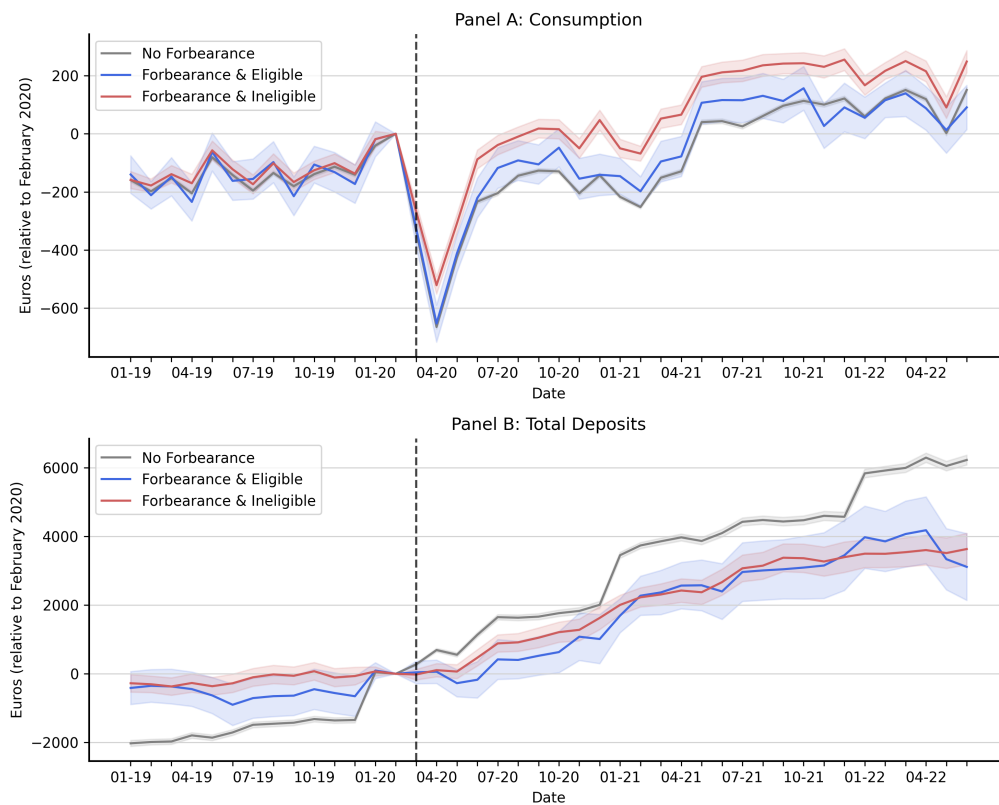
This figure plots the coefficient path of the selected variables in a LASSO regression, considering a set of pre-pandemic measures, for the probability of accessing forbearance. Coefficients of each variable are shown relative to  $\lambda$ , the overall penalty level of the regularized regression. Variables are shown in the legend according to the order by which they are selected.

**Figure IA.3:** Evolution of Income by Eligibility and Forbearance Status



This figure shows the household average and 95% confidence interval for different components of monthly income from July 2019 to June 2022. All measures are reported in euros, seasonally adjusted, and relative to the pre-pandemic baseline (February 2020). Panel A shows the change in monthly wages, while Panel B shows the change in social security benefits received. Panel C shows the change in total monthly income, computed as the sum between monthly wages, social security, and retirement benefits. In all panels, the average change is presented separately for eligible households who received forbearance, ineligible households who received forbearance, and households who never received forbearance. Standard errors are clustered by household.

**Figure IA.4:** Evolution of Consumption and Deposits by Eligibility and Selection Status



This figure plots the household average and 95% confidence interval for monthly consumption and total deposits from January 2019 to June 2022. All measures are reported in euros, seasonally adjusted, and relative to the pre-pandemic baseline (February 2020). Panel A reports monthly consumption. Panel B shows total deposits, computed as the sum between end-of-the-month checking and saving accounts' balances. In all panels the average change is presented separately for eligible households who received forbearance, ineligible households who received forbearance, and households who never received forbearance. Standard errors are clustered by household.

**Table IA.1:** Summary Statistics of Household Characteristics

Variable	Mean	SD	p10	p25	p50	p75	p90	Observations
Average Age	47.9	9.1	37.0	41.5	47.0	54.0	60.5	137,363
Number of Mortgagors	1.7	0.5	1.0	1.0	2.0	2.0	2.0	137,363
Wages	1,816.1	1,067.3	756.1	1,075.3	1,566.6	2,250.9	3,216.9	111,979
Pensions	1,313.9	928.4	390.2	629.2	1,056.7	1,767.7	2,539.4	40,258
Social Security Benefits	341.7	439.8	33.6	63.4	163.9	446.1	882.3	42,761
Other Inbound Transfers	663.2	976.0	25.6	107.1	297.5	803.0	1,693.8	137,363
Total Income	2,527.1	1,754.6	870.0	1,394.4	2,090.1	3,176.0	4,658.7	137,363
Consumption	1,505.9	932.2	559.0	860.2	1,298.3	1,915.2	2,704.5	137,363
Consumption Rate	0.76	0.75	0.32	0.46	0.62	0.80	1.13	137,133
Total Deposits	18,185.0	30,787.9	302.0	1,159.5	5,567.4	20,738.0	51,399.2	137,363
Checking Accounts	6,710.8	12,490.4	195.2	696.8	2,010.5	6,528.3	17,853.7	137,363
Savings Accounts	17,356.5	28,834.9	0.0	431.5	5,757.9	20,541.7	49,559.6	90,241
Mortgage Loans	69,304.6	52,000.4	15,173.5	30,724.2	57,206.5	95,048.2	137,247.8	137,363
Credit Cards and Overdraft	419.6	784.2	0.0	0.0	87.5	473.3	1,224.8	137,328
Other Banks' Loans	7,451.0	17,692.4	0.0	0.0	470.0	7,507.7	19,463.5	137,363
Total Debt Payment	315.3	170.1	148.7	207.4	279.4	378.1	523.5	137,363
Debt Payment-to-Income	0.19	0.25	0.06	0.09	0.14	0.21	0.32	137,133
7 Day Delinquency	0.02	0.13	0.00	0.00	0.00	0.00	0.00	137,363

This table shows pre-pandemic (2019) mean, standard deviation (SD), the 10% (p10), 25% (p25), 50% (p50), 75% (p75) and 90% (p90) percentiles, and number of households. Income, deposits, liabilities, and consumption measures are winsorized at the top and bottom 1% by date.

**Table IA.2:** Forbearance, Eligibility and Future Outcomes

	Employment Status Change				Wage Growth			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Forbearance	-0.009*** (0.003)	-0.008*** (0.003)	-0.008*** (0.003)	-0.007** (0.003)	3.114*** (0.631)	2.962*** (0.634)	2.766*** (0.635)	2.044*** (0.631)
Eligible	-0.006** (0.003)	-0.006** (0.003)	-0.006** (0.003)	-0.003 (0.003)	21.310*** (0.541)	21.360*** (0.541)	21.210*** (0.540)	20.810*** (0.569)
Forbearance × Eligible	0.004 (0.009)	0.004 (0.009)	0.003 (0.009)	0.004 (0.009)	1.821 (1.679)	1.914 (1.678)	2.091 (1.676)	1.518 (1.655)
Additional Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Group FE	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Location FE	No	No	Yes	Yes	No	No	Yes	Yes
Industry FE	No	No	No	Yes	No	No	No	Yes
$R^2$	0.004	0.006	0.011	0.014	0.113	0.114	0.121	0.141
Observations	65,658	65,658	65,658	65,658	64,351	64,351	64,351	64,351

This table presents estimates of linear probability models of losing the job and wage growth between the first quarter of 2020 and 2021, as a function of predetermined variables. Observations are at the household level and only include households employed by a single employer during the first quarter of 2020. In columns (1)-(4) the dependent variable is the difference between a dummy variable that takes the value of one if the household receives no wage payment, comparing the dummy variable value during 2021:Q1 relative to 2020:Q2. In columns (5)-(8), the dependent variable corresponds to the household wage growth between 2020:Q2 and 2021:Q1. *Forbearance* is a dummy variable that takes the value of one for households receiving forbearance and zero otherwise. *Eligible* is a dummy variable that takes the value of one if the household satisfied the legal requirements for suspending mortgage payments. In all specifications, the regressions include pre-pandemic controls such as age, total income, total deposits, credit card and overdraft balances, mortgage loan balance, balances for loans held at other banks, and debt payment. We also control for employment risk by including a private sector dummy that takes the value of one if the household's employer in the first quarter of 2020 operated in the private sector, and a measure of employer's industry volatility, computed as the standard deviation of sales at the industry level and for the previous three years, normalized by the industry's total assets. Robust standard errors are shown in parentheses. \*, \*\*, and \*\*\* indicate statistical significance at the 10%, 5%, and 1% level, respectively.

**Table IA.3:** Household Consumption Growth: Sample and Population

	Sample	Population
2019	7.2%	4.2%
2020	-4.1%	-4.7%
2021	14.8%	13.8%

This table shows the annual growth rate of consumption for the average household in our sample and the corresponding statistic for the population. The population average is the yearly growth rate of the average consumption by all resident households, measured as the final consumption expenditure divided by the number of households. National accounts data are from INE.

**Table IA.4:** Household Propensities to Consume and Save in Deposits: Robustness

	$\Delta$ Consumption			$\Delta$ Saving in Deposits		
	(1)	(2)	(3)	(4)	(5)	(6)
Forbearance Amount $\times$ Post	0.167*** (0.024)	0.150*** (0.028)	0.159*** (0.027)	0.282*** (0.030)	0.298*** (0.040)	0.294*** (0.041)
$\Delta$ Income	0.091*** (0.003)	0.086*** (0.003)	0.085*** (0.003)	0.701*** (0.007)	0.694*** (0.009)	0.695*** (0.009)
Group $\times$ Month $\times$ Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Household $\times$ Month FE	Yes	No	No	Yes	No	No
Industry $\times$ Month FE	No	Yes	No	No	Yes	No
Municipality $\times$ Month FE	No	No	Yes	No	No	Yes
$R^2$	0.423	0.103	0.107	0.508	0.158	0.159
Observations	6,181,335	6,180,996	6,181,335	6,043,972	6,043,639	6,043,972

This table presents difference-in-differences estimates of regressions of changes in monthly consumption and saving in total deposits using the difference-in-differences regression in equation (1) at the household-month level. The dependent variable in columns (1)-(3),  $\Delta$ Consumption, is the difference between monthly spending at time  $t$ , relative to the corresponding household average in 2019. The dependent variable in columns (4)-(6),  $\Delta$ Saving in Deposits, is the monthly saving in total deposits at time  $t$ , relative to the corresponding household average in 2019. *Forbearance Amount* is the amount of postponed debt payments. *Post* is a dummy variable that takes a value of one for the period starting when mortgage payments are suspended and zero otherwise. Specifications in columns (2), (3), (5), and (6) include the *Forbearance* dummy variable as a control variable. All specifications include group-month-year fixed effects, with group referring to quartiles of pre-pandemic deposits and income (2019 averages), and changes in total income relative to the corresponding household average in 2019 as a control. In columns (2) and (5) we define industry as the two-digit industry code of the primary employer, in case of employed households; and for non-employed households, the current status (either unemployed or retired). The sample period is from January 2018 to September 2021. Two-way standard errors clustered by household and month-year are reported in parentheses. \*, \*\*, and \*\*\* indicate statistical significance at the 10%, 5%, and 1% level, respectively.

**Table IA.5:** Household Propensities to Consume and Save in Deposits by Horizon: Sample of Individuals Working in the Private Sector

	$\Delta$ Consumption			$\Delta$ Saving in Deposits		
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Overall						
Forbearance Amount $\times$ Post	0.129*** (0.045)	0.110*** (0.040)	0.131*** (0.038)	0.061 (0.079)	0.120** (0.058)	0.302*** (0.059)
$\Delta$ Income			0.085*** (0.003)			0.711*** (0.010)
Panel B: Horizons						
Forbearance Amount $\times$						
Immediate Effect (1m-3m)	0.020 (0.049)	-0.032 (0.040)	0.013 (0.039)	-0.192* (0.098)	0.015 (0.049)	0.396*** (0.054)
Short-run Effect (4m-12m)	0.136*** (0.044)	0.116*** (0.037)	0.137*** (0.036)	0.090 (0.093)	0.117* (0.069)	0.294*** (0.065)
Long-run Effect (>12m)	0.175*** (0.045)	0.175*** (0.038)	0.185*** (0.036)	0.151 (0.123)	0.180** (0.084)	0.262*** (0.077)
$\Delta$ Income			0.085*** (0.003)			0.711*** (0.010)
Month $\times$ Year FE	Yes	No	No	Yes	No	No
Group $\times$ Month $\times$ Year FE	No	Yes	Yes	No	Yes	Yes
$R^2$	0.062	0.086	0.106	0.017	0.026	0.170
Observations	2,246,175	2,246,175	2,246,175	2,196,260	2,196,260	2,196,260

This table presents difference-in-differences estimates of regressions of changes in monthly consumption and saving in total deposits using the difference-in-differences regression in equation (1) at the household-month level. Panel A considers a single *Post* indicator, while in Panel B this indicator is replaced with three different time dummy variables: the *Immediate Effect* measures the impact over the first quarter after the start of forbearance; the *Short-run Effect* measures the average effect from the 4<sup>th</sup> to the 12<sup>th</sup> month; and the *Long-run Effect*, measures the average effect after one year. The dependent variable in columns (1)-(3),  $\Delta$ Consumption, is the difference between monthly spending at time  $t$ , relative to the corresponding household average in 2019. The dependent variable in columns (4)-(6),  $\Delta$ Saving in Deposits, is the monthly saving in total deposits at time  $t$ , relative to the corresponding household average in 2019. *Forbearance Amount* is the amount of postponed debt payments. *Post* is a dummy variable that takes a value of one for the period starting when mortgage payments are suspended and zero otherwise. All specifications include the *Forbearance* dummy variable as a control variable. In some specifications, the regressions include group-month-year fixed effects, with group referring to quartiles of pre-pandemic deposits and income (2019 averages), and changes in total income relative to the corresponding household average in 2019 as a control. The sample is restricted to households whose primary employer in the first quarter of 2020 is in the private sector. The sample period is from January 2018 to September 2021. Two-way standard errors clustered by household and month-year are reported in parentheses. \*, \*\*, and \*\*\* indicate statistical significance at the 10%, 5%, and 1% level, respectively.



**Table IA.6:** Household Propensity to Borrow by Eligibility and Forbearance Status

	$\Delta$ Borrowing Credit Card & Overdraft			$\Delta$ Borrowing Other Banks' Debt		
	(1)	(2)	(3)	(4)	(5)	(6)
Debt Payment $\times$						
Forbearance=0 $\times$ Eligible=1 $\times$ Post	0.002 (0.009)	0.001 (0.008)	-0.000 (0.007)	0.014 (0.017)	0.033* (0.017)	0.039** (0.017)
Forbearance=1 $\times$ Eligible=0 $\times$ Post	-0.011 (0.012)	-0.011 (0.009)	-0.012 (0.009)	-0.051* (0.028)	-0.029 (0.027)	-0.028 (0.027)
Forbearance=1 $\times$ Eligible=1 $\times$ Post	0.000 (0.018)	-0.000 (0.017)	-0.004 (0.017)	-0.109* (0.054)	-0.080 (0.054)	-0.067 (0.054)
Controls	No	No	Yes	No	No	Yes
Month $\times$ Year FE	Yes	No	No	Yes	No	No
Group $\times$ Month $\times$ Year FE	No	Yes	Yes	No	Yes	Yes
$R^2$	0.012	0.021	0.023	0.002	0.003	0.005
Observations	6,043,972	6,043,972	6,043,972	4,395,616	4,395,616	4,395,616
Observations:						
Forbearance=0 $\times$ Eligible=0	116,732					
% of sample	(85.0%)					
Forbearance=0 $\times$ Eligible=1	12,469					
% of sample	(9.1%)					
Forbearance=1 $\times$ Eligible=0	6,637					
% of sample	(4.8%)					
Forbearance=1 $\times$ Eligible=1	1,525					
% of sample	(1.1%)					

This table presents difference-in-differences estimates of regressions of changes in credit card and overdraft, and other banks' debt payments using the difference-in-differences regression in equation (1) at the household-month level. The dependent variable in columns (1)-(3),  $\Delta$ Borrowing Credit Card & Overdraft, is the difference in monthly changes in end-of-the-month credit card and overdraft balances at time  $t$  at the bank, relative to the corresponding household average in 2019. The dependent variable in columns (4)-(6),  $\Delta$ Borrowing Other Banks' Debt, is the difference between monthly changes in liabilities found in the Credit Register held at other banks at time  $t$ , relative to the corresponding household average in 2019. *Debt Payment* is the amount of pre-pandemic debt payments made by households. *Forbearance* is a dummy variable that takes the value of one for households receiving forbearance and zero otherwise. *Eligible* is a dummy variable that takes the value of one if the household satisfied the legal requirements for suspending mortgage payments. *Post* is a dummy variable that takes a value of one for the period starting when mortgage payments are suspended and zero otherwise. All specifications include the *Forbearance* dummy variable as a control variable. In some specifications, the regressions include group-month-year fixed effects, with group referring to quartiles of pre-pandemic deposits and income (2019 averages), and changes in total income relative to the corresponding household average in 2019 as a control. The sample period in columns (1)-(3) is from January 2018 to September 2021. The sample period in columns (4)-(6), due to data limitations, is from January 2019 to September 2021. Two-way standard errors clustered by household and month-year are reported in parentheses. \*, \*\*, and \*\*\* indicate statistical significance at the 10%, 5%, and 1% level, respectively.

**Table IA.7:** Household Propensity to Consume by Category and by Eligibility and Forbearance Status

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Total	Groc.	Cloth.	House Maint.	Furnit.	Transp.	Health Care	Restau.	Entert. & Educ.	Misc.
Debt Payment ×										
Forbearance=0 × Eligible=1 × Post	-0.160*** (0.023)	0.005 (0.006)	-0.007* (0.003)	0.001 (0.002)	-0.002 (0.003)	-0.028*** (0.006)	-0.010*** (0.003)	-0.023*** (0.006)	-0.015*** (0.004)	-0.065*** (0.012)
Forbearance=1 × Eligible=0 × Post	0.121*** (0.032)	0.062*** (0.007)	0.015*** (0.004)	0.005*** (0.002)	0.007** (0.003)	0.003 (0.006)	0.009** (0.003)	0.003 (0.007)	0.013*** (0.004)	0.008 (0.014)
Forbearance=1 × Eligible=1 × Post	0.016 (0.053)	0.055*** (0.012)	0.014* (0.007)	0.004 (0.004)	0.004 (0.006)	0.029** (0.010)	-0.005 (0.007)	-0.015 (0.012)	-0.006 (0.010)	-0.040 (0.031)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Group × Month × Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R <sup>2</sup>	0.095	0.028	0.049	0.007	0.010	0.043	0.010	0.067	0.018	0.036
Observations	2,884,623	2,884,623	2,884,623	2,884,623	2,884,623	2,884,623	2,884,623	2,884,623	2,884,623	2,884,623

This table presents difference-in-differences estimates of regressions of changes in monthly consumption for different spending categories using the difference-in-differences regression in equation (1) at the household-month level. The dependent variable is measured as the difference between monthly spending at time  $t$ , relative to the corresponding household average in January and February of 2020, for each consumption category: (1) Total Consumption; (2) Groceries; (3) Clothing; (4) Housing Maintenance and Utilities; (5) Furniture; (6) Transport; (7) Health Care; (8) Restaurants; (9) Entertainment and Education; and (10) Miscellaneous Goods and Services. *Debt Payment* is the pre-pandemic amount of debt payments of each household. *Forbearance* is a dummy variable that takes the value of one if the household satisfied the legal requirements for suspending mortgage payments. *Post* is a dummy variable that takes a value of one for the period starting when mortgage payments are suspended and zero otherwise. All specifications include the *Forbearance* dummy variable as a control variable. In all specifications, the regressions include group-month-year fixed effects, with group referring to quartiles of pre-pandemic deposits and income (2019 averages), and changes in total income relative to the corresponding household average in January and February 2020 as a control. The sample period is from January 2020 to September 2021. Two-way standard errors clustered by household and month-year are reported in parentheses. \*, \*\*, and \*\*\* indicate statistical significance at the 10%, 5%, and 1% level, respectively.

**Table IA.8: Household Propensity to Consume on Exit by Category**

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Total	Groc.	Cloth.	House Maint.	Furnit.	Transp.	Health Care	Restau.	Entert. & Educ.	Misc.
Forbearance Amount $\times$										
Immediate Effect (1m-3m)	-0.030 (0.039)	0.036*** (0.007)	0.004 (0.004)	0.005** (0.002)	0.003 (0.004)	-0.009 (0.008)	-0.000 (0.003)	-0.025*** (0.005)	-0.002 (0.004)	-0.026* (0.015)
Short-run Effect (4m-12m)	0.093*** (0.025)	0.058*** (0.006)	0.015*** (0.003)	0.004** (0.002)	0.005** (0.002)	0.005 (0.005)	0.008** (0.003)	-0.003 (0.007)	0.010** (0.004)	0.002 (0.013)
Long-run Effect (>12m)	0.167*** (0.026)	0.061*** (0.006)	0.020*** (0.003)	0.005*** (0.002)	0.009*** (0.003)	0.020*** (0.005)	0.008** (0.003)	0.017** (0.006)	0.013** (0.005)	0.017 (0.015)
Exit Effect	0.070** (0.027)	0.021*** (0.006)	0.014*** (0.004)	0.003** (0.002)	0.001 (0.002)	0.007 (0.005)	0.004 (0.003)	0.001 (0.006)	0.004 (0.004)	0.016 (0.016)
$\Delta$ Income	0.085*** (0.003)	0.008*** (0.000)	0.004*** (0.000)	0.001*** (0.000)	0.003*** (0.000)	0.007*** (0.000)	0.003*** (0.000)	0.006*** (0.000)	0.004*** (0.000)	0.031*** (0.001)
Group $\times$ Month $\times$ Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
$R^2$	0.087	0.029	0.044	0.006	0.010	0.037	0.009	0.053	0.015	0.037
Observations	4,120,890	4,120,890	4,120,890	4,120,890	4,120,890	4,120,890	4,120,890	4,120,890	4,120,890	4,120,890

This table presents difference-in-differences estimates of regressions of changes in monthly consumption for different spending categories using the difference-in-differences regression in equation (1) at the household-month level, but replacing the *Post* indicator with four different time dummy variables: the *Immediate Effect* measures the impact over the first quarter after one year and until the end of the payment suspension; the *Short-run Effect* measures the average effect from the 4<sup>th</sup> to the 12<sup>th</sup> month; the *Long-run Effect* measures the average effect after one year and until the end of the payment suspension; and the *Exit Effect* measures the average effect after the end of the payment suspension. The dependent variable is measured as the difference between monthly spending at time  $t$ , relative to the corresponding household average in January and February of 2020, for each consumption category: (1) Total Consumption; (2) Groceries; (3) Clothing; (4) Housing Maintenance and Utilities; (5) Furniture; (6) Transport; (7) Health Care; (8) Restaurants; (9) Entertainment and Education; and (10) Miscellaneous Goods and Services. *Forbearance Amount* is the amount of postponed debt payments. In all specifications, the regressions include group-month-year fixed effects, with group referring to quartiles of pre-pandemic deposits and income (2019 averages), and changes in total income relative to the corresponding household average in January and February 2020 as a control. The sample period is from January 2020 to June 2022. Two-way standard errors clustered by household and month-year are reported in parentheses. \*, \*\*, and \*\*\* indicate statistical significance at the 10%, 5%, and 1% level, respectively.

## Acknowledgements

We thank Paul Goldsmith-Pinkham, Isaac Hacamo, Sasha Indarte, and Amit Seru; participants at the CEPR European Workshop on Household Finance, Finance Forum, Luso-Brazilian Finance Conference (Lubrafin), NBER Summer Institute Workshop on Capital Markets and the Economy, Nova SBE-WU Workshop, Stanford Institute for Theoretical Economics (SITE) Financial Regulation Session, University of Tennessee “Smokey” Mountain Finance Conference; and seminar participants at Bayes Business School, European Central Bank, Tinbergen Institute, University of Naples Federico II (CSEF), and Washington University in St. Louis for helpful comments. The contribution by Miguel Ferreira has been prepared under the Wim Duisenberg Fellowship Program sponsored by the ECB. Any views expressed are only those of the authors and do not necessarily represent the views of the ECB or Eurosystem

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ISBN 978-92-899-7116-4

ISSN 1725-2806

doi:10.2866/4885866

QB-01-25-082-EN-N