The Heterogeneous Effects of Household Debt Relief[*]

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ABSTRACT

Large-scale debt forbearance is a key policy tool in times of economic distress, but it is challenging to design a targeted program due to information asymmetries between lenders and borrowers. Using transaction-level data from a leading Portuguese bank, we study which households access a widely advertised and easily accessible forbearance program during the COVID-19 pandemic and how it affects household consumption and savings. We show that the average household that enters forbearance is fragile, and suspending mortgage payments has positive and persistent effects on consumption and savings. The effects, however, are very heterogeneous across households. Households eligible for forbearance avoid a drop in consumption, while ineligible households, who nevertheless access forbearance, increase consumption. In addition, the consumption response is stronger among low-wealth and low-income households, while the savings response is stronger among high-wealth and high-income households.

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1 Introduction

Large-scale debt relief to distressed borrowers is riddled with information frictions between lenders and borrowers (Adelino, Gerardi, and Willen (2013), Eberly and Krishnamurthy (2014)), as well as institutional frictions such as securitization (Piskorski, Seru, and Vig (2010), Agarwal, Amromin, Ben-David, Chomsisengphet, and Evanoff (2011), and Kruger (2018)) and intermediary financial and organizational constraints (Aiello (2022)). Thus, designing debt relief programs requires making trade-offs between a quick-to-implement and catch-all approach that targets most households (minimize type II error) versus a slower and document-intensive approach that targets only "truly" distressed households (minimize type I error). The U.S. government took the latter approach in the Great Recession. In fact, even after the implementation of the Home Affordable Modification Program (HAMP) that provided incentives for financial intermediaries to modify delinquent home mortgages, as many as two-thirds of heavily indebted households never received assistance (Noel (2021)). The failure to provide debt relief to 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)).

During the initial months of the COVID-19 pandemic, in March-April 2020, and in stark contrast with the 2008-2009 foreclosure crisis, governments worldwide put in place debt forbearance programs—that is, the temporary suspension of debt payments—for a vast number of households in anticipation of potential distress and defaults. In Portugal, eligibility for debt forbearance, mainly suspension of mortgage payments, was based on whether a household was working in a sector directly affected by the lockdowns or suffered a drop in income of at least 20% relative to the pre-pandemic.[1] As of June 2020, €17 billion mortgages were on repayment moratoria, comprising about 18% of mortgages.[2]

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[1]Households were also eligible if one of the household members was infected with COVID, but this represented a minimal number of people during the first few months of the pandemic when households entered forbearance (about 42,000 cases in a population of about 10 million by the end of June of 2020, https://coronavirus.jhu.edu/region/portugal).

[2]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

This paper uses a rich microdata panel from a leading Portuguese bank to investigate both the selection into debt forbearance and the effect of forbearance on household consumption and savings. The data include transaction-level data from checking and savings accounts, credit and debit cards, and transfers in and out of each account in the 2018-2022 period. The richness of the data limits the set of unobservables that might drive the results. We study two sequential waves of debt forbearance. Our primary focus is the government program initiated in March 2020, but we also consider a subsequent private program initiated in September 2021 with additional relief measures implemented by banks at the direction of the regulator. To the best of our knowledge, we are the first to characterize who accesses forbearance in such large-scale programs using income and balance sheet data, separating eligible households from ineligible households who still obtained access, as well as how consumption and savings evolve around the start of the pandemic and forbearance.

We identify the effect of the forbearance on consumption and savings by relying on: (1) the high frequency of the data (and thus the ability to identify sharp changes in behavior around the start of the forbearance); (2) the ability to control for changes in income, the main unobserved variable in similar studies, and time-varying income and wealth bins; and (3) the validation that households were following parallel trends before the forbearance. We are interested not only in the causal effect of the programs but also in the selection of households into forbearance, a key input into the design of such programs. Thus, our estimated average effects should be interpreted as the effect of the forbearance on the set of households who choose to suspend debt payments relative to otherwise similar households who choose not to suspend payments. We then consider how selection on unobservables is likely to affect the magnitude and direction of the effects and separate the effects by eligibility for forbearance according to the government rules.

We show that, on average, households who entered forbearance suffered a drop in income in March and April of 2020 and are generally more financially fragile than those who did not.

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loans (Nicolaou (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)).

Even before the pandemic, households who later obtained forbearance had lower income and lower wealth, consistent with the experience in the United States (Cherry, Jiang, Matvos, Piskorski, and Seru (2021) and Gerardi, Lambie-Hanson, and Willen (2022)). During the pandemic, total income was less affected than wages, as households in forbearance also received relatively more government transfers.

Despite the drop in wages, the households in forbearance increased consumption relative to households outside forbearance starting in the summer of 2020. This differential increase in monthly consumption of about 8% of the pre-pandemic level persisted through the end of the government forbearance program in September 2021. We also find that forbearance is associated with increased savings (checking and savings accounts), with a differential increase in savings of about 9% over one year after the suspension of debt payments.

We also obtain estimates of the marginal propensity to consume and save, i.e., the change in consumption and savings per euro of postponed debt payments. The estimated marginal propensity to consume for households in forbearance is about 20 cents relative to households outside forbearance. The estimated marginal propensity to save for households in forbearance has a similar magnitude.

We next show that an important feature of the response to the forbearance program was how heterogeneous it was among households with different observable characteristics. We start by considering differences in the responses by household wealth and income levels. To investigate this heterogeneity, we split the sample at the median of total assets held in the bank and at the median of total income before the pandemic. We find that households in the low-asset group increased consumption by about 25 cents per euro of postponed payments shortly after the start of the forbearance (4 to 12 months). This increase persisted until the end of the forbearance program, corresponding to a higher consumption level than these households had before the pandemic. In contrast, there was a smaller (10 cents) increase in spending per euro of forbearance among high-asset households. A similar picture emerges when we split households by median income or debt payment-to-income ratio. These results are 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 other shocks to household income (e.g., Hall and Mishkin (1982), Zeldes (1989), Johnson, Parker, and Souleles (2006), Kaplan, Violante, and Weidner (2014), and Di Maggio, Kermani, Keys, Piskorski, Ramcharan, Seru, and Yao (2017)). Ganong, Jones, Noel, Greig, Farrell, and Wheat (2020) show that both income and liquid wealth matter for the sensitivity of consumption to income shocks, which is also consistent with our results.

Savings show the reverse pattern from consumption in the cross-section of households. While the low-asset group saved about 5-10 cents per dollar out of postponed repayment, the high-income and high-asset groups saved as much as 25 cents per euro after the start of the forbearance (4 to 12 months). We find qualitatively similar results when we split households by median income or debt payment-to-income ratio. This significant response in savings raises the question of whether there was a need for a mortgage moratorium for the less fragile groups that requested forbearance.

The average response by households also masks substantial heterogeneity by eligibility for the forbearance program. We construct detailed proxies of legal (or "formal") eligibility for the forbearance program using the criteria in the government legislation. Our data allow us to measure variation in wages and other sources of income, as well as the industries individuals work in, which are the two main criteria for eligibility. We estimate that about 10% of the population was eligible for the forbearance program. However, the actual enrollment in the program deviated substantially from formal eligibility.

Perhaps surprisingly, most households eligible for forbearance (according to our measure based on the bank's data) chose not to access the program. At the same time, most households who entered forbearance were not formally eligible for it. This is consistent with (intentionally) very loose screening on the part of banks and a bias toward offering forbearance to households who asked for it at the beginning of the pandemic. This means that the policy missed many target households but reached many other households who were not the intended recipients of the program.

When we separate the average consumption effect into different groups based on

eligibility, we find that the relative increase in consumption is mainly driven by ineligible households who ask for forbearance (i.e., households who were not the intended recipients of the program). Eligible households (i.e., households who were the target of the program) that entered forbearance maintained their pre-pandemic consumption levels. In contrast, eligible households who chose not to access forbearance suffered a drop in consumption relative to ineligible households outside forbearance.

Selection is important in our setting as eligible households in forbearance represent only 1.1% of the full sample, while ineligible households in forbearance represent 4.8%. Forbearance is associated with 4% higher consumption for eligible households. For ineligible households, however, forbearance is associated with a larger consumption effect of 7%. If we assume treatment effect homogeneity, this three percentage point difference between the two effects suggests that differences in who selects into large-scale forbearance programs (with easy access) materially affect the estimated forbearance effect. There are at least two interpretations of this selection. One is that the counterfactual change in consumption of the groups in forbearance would have been different in the absence of forbearance. Another is the heterogeneity of the effect of forbearance for the households who select into the program.

Finally, we study the effects of the additional debt relief offered by banks in September 2021. This additional relief took the form of reduced or suspended payments (i.e., loan maturity extensions, interest rate reductions, or additional loan payment suspension) for households who had been in forbearance during the previous 18 months and might need additional relief. As with the government moratoria, the bank had an explicit mandate to offer additional assistance to any household who *might* need additional help rather than restricting the measures to the most distressed borrowers. This included contacting all borrowers in forbearance to assess their need for relief.

Interestingly, only a small group of households who had entered forbearance in the first half of 2020 chose to take up the additional measures in September 2021. We show that the households who took the additional measures had saved less during the initial forbearance period and faced larger wage drops but still increased consumption during that period. The

increase in consumption relative to the pre-pandemic period persisted after the start of the additional measures. In contrast, the borrowers who exited forbearance in September 2021 consumed more and built up additional savings during the initial forbearance period.

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. 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. At the same time, lenders may find it optimal to perform principal reductions to reduce the incentive of borrowers to default – although the *announcement* of such a program can itself lead to strategic defaults (Mayer, Morrison, Piskorski, and Gupta (2014)).

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 has shown 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)). A notable exception is Mayer, Morrison, Piskorski, and Gupta (2014), which study a change in modification policy induced by a court decision and find that borrower delinquency rates increase when Countrywide is forced to offer more generous modification terms.

The consumption and delinquency effects of the policies during the Great Recession are also, by now, well documented (see, among many others, Agarwal, Amromin, Ben-David, Chomsisengphet, Piskorski, and Seru (2017), Abel and Fuster (2021), Agarwal, Amromin,

Chomsisengphet, Landvoigt, Piskorski, Seru, and Yao (2015), Ganong and Noel (2020)). For work on debt relief during the COVID-19 pandemic, Cherry, Jiang, Matvos, Piskorski, and Seru (2021) and Gerardi, Lambie-Hanson, and Willen (2022) show that public and private forbearance programs contributed to a low level of delinquencies in the United States. The reduction in delinquency rates was higher among low-income and minority individuals (Shi (2022), An, Cordell, Geng, and Lee (2022), Gerardi, Lambie-Hanson, Willen, et al. (2021)), and financial intermediary frictions may have prevented some borrowers from receiving forbearance (Kim, Lee, Scharlemann, and Vickery (2022) and Cherry, Jiang, Matvos, Piskorski, and Seru (2022) as during the implementation of the Home Affordable Modification Program (HAMP) in 2009. Loewenstein and Njinju (2022) finds that the Coronavirus Aid, Relief, and Economic Security (CARES) Act forbearance was largely used by borrowers who needed it and finds no evidence of strategic use of forbearance after home purchase or refinancing.[3]

Our paper is also related to the large 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 (2022) show that unemployment benefits introduced at the height of the crisis impacted spending but less so employment dynamics. Recent work also shows that the pandemic had heterogeneous effects on workers across different countries (Adams-Prassl, Boneva, Golin, and Rauh (2020)) and occupations (Barrero, Bloom, and Davis (2020)).[4]

In short, our paper provides new insights about the dynamics of individual income,

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[3]Debt forbearance may have also had positive local spillovers (Capponi, Jia, and Rios (2021), Wang, Yang, Iverson, and Kluender (2020)). In parallel, there has been an intense recent debate on the effects of student loan forgiveness programs, including its redistribution effects and how borrowers substitute between public and private debt sources (Catherine and Yannelis (2023) and Dinerstein, Yannelis, and Chen (2022)).

[4]On the heterogeneity over the implementation of the policy, Cherry, Jiang, Matvos, Piskorski, and Seru (2022) show how shadow banks affected the debt relief pass-through in the United States, exhibiting persistently lower forbearance rates than traditional lenders.

consumption, and savings before and after large-scale debt relief programs. Understanding the impact of policy choices during crises, particularly debt forbearance, is a crucial step to making better use of financial stability tools 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.[5] 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. By then, the scope of the measure was restrictive, only including mortgage loans for the acquisition of owner-occupied properties. The eligibility criteria for this initial government program also restricted forbearance to individuals who were not delinquent at the time (defined as not having payments 90 days past due) nor had outstanding tax or social security debt. Moreover, access was limited to individuals: (1) in self-isolation or providing assistance to a family member; (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) employed in activities facing closures during the state of emergency period. If individuals satisfied one of these criteria, they could request the delay of loan payments for six months, until September 2020. As the loan maturity date was deferred according to the duration of the forbearance, Portuguese banks would bear the potential cost of the policy.[6]

By April 2020, an inter-bank agreement led to a complementary and non-legislative

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[5]Figure IA.1 provides an overview of the main events related to the Portuguese government's response to the pandemic, highlighting the debt forbearance program.

[6]A back-of-envelope calculation puts the figure of postponed payments at about €60 million just for our sample, which considers less than half of the loan contracts covered by the program in this bank.

moratoria, expanding the set of loans eligible for forbearance by including other mortgage loans but also personal and auto loans. The government also soon broadened the legislative moratoria, and by mid-June the measure was extended to all individuals reporting a 20% drop in income due to the pandemic. At the same time, changes were made to the legislative program in order to include all types of mortgage loans for residential property and student loans. As a result of this change, 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, as a consequence of these amendments, the suspension of loan payments was extended until the end of March 2021, which would later be further extended until September 2021, or 18 months after the forbearance was first implemented.

During 2021, concerns over households' ability to resume payments led, however, to new regulatory guidelines on the prevention and management of arrears, demanding a more proactive role for banks. Apart from closely monitoring borrowers, banks would contribute to the prevention of arrears through renegotiation or restructuring of loans and would have to offer adequate measures to individuals at risk of defaulting. The exact nature of such measures was left at the banks' discretion and could include, as suggested by policymakers, loan maturity extensions, interest rate reductions, or additional loan payment suspension; nonetheless, only a small number of borrowers took advantage of this possibility.

3   Data

Our data comprises account-level transactions provided by a leading Portuguese bank. We apply several filters to the sample of bank customers. We restrict the sample to clients who have outstanding mortgage loans with the bank. We then group clients with a joint mortgage and who share checking accounts to define a household. Moreover, 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

chose direct deposit of wages, pensions, or social security benefits;[7] and (2) at least one member of the household regularly uses debit and credit cards held at the bank for purchases and payments (at least an average of ten transactions per month).[8] The final sample includes about 138,000 households between January 2018 and June 2022.

Our data include transactions of checking and savings accounts, as well as purchases and payments from debit or credit cards, including cash withdrawals. Given that our sample is composed of households with direct deposit of wages, pensions, and other social security benefits (e.g., unemployment insurance), 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 can categorize the type of income according to these three categories (wages, pensions, and social security benefits), and in the case of wages, we are able to identify the company employing an individual.[9]

Despite its granularity and completeness, the data on wages, pensions, and social security benefits is a lower bound on household total income, as other sources of income may not be included. For example, rents or self-employment income would not be captured. Moreover, since we only require one member of the household to have chosen wage direct deposit, we may fail to capture a fraction of the household wage income. For this reason, 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.

Our measure of consumption includes any purchases or payments using debit or credit cards, as well as cash withdrawals, using data from point-of-sale or terminal transactions.[10]

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[7]Households are offered a reduction in the mortgage spread if they choose to have wages and pensions deposited directly at the bank.

[8]On average, households in our sample made 39 transactions in a month, and the median is 35.

[9]Out the 138,000 households, we find a valid employer match for around 100,000, and about 20,000 non-employed households, which include unemployed or retired individuals. The remaining households correspond to 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 companies operating in Portugal. Firm names, as well as industry codes, are retrieved from SABI (Iberian Balance Sheet Analysis System).

[10]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

However, our consumption measures should be seen as a lower bound on total consumption as some expenditure items, such as utilities, may not be included. Thus, we complement the data with outgoing transfers issued by a third party, such as automatic payments of utilities and other services.

We are able to classify purchases by category starting in January 2020. For this period, we categorize most transactions by relying on point-of-sale terminal information, namely, the reported Merchant Category Code (MCC), which 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. The data also include additional information for liabilities, such as interest rate (as of August 2021), date of origination, maturity, and monthly installment before the pandemic.

We merge the internal information of the bank with data from the Credit Register (*Central de Responsabilidades de Crédito*), managed by Banco de Portugal, and thus we obtain outstanding loans from 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 as well as 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.

3.1 Summary Statistics

Table 1 presents pre-pandemic (as of December 2019) averages of variables for our sample of households, segmented by forbearance and eligibility status. 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. Households in our

and 5% by other means.

sample are comprised of 1.7 mortgagors on average, with negligible differences between those who sought forbearance and those who did not. The average wage in the full sample is about €1,816 per month, and it is higher for households outside of forbearance, particularly in the case of eligible households. Total income, including pensions (for 40,000 of the 137,000 households in the sample), social security benefits, and other inbound transfers (i.e., rents, business or professional income) follows 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. Table IA.1 in the Internet Appendix presents detailed summary statistics for our full sample. The 90th percentile of total income in our full sample is €4,659 per month, and the average total income is €2,527, which is significantly higher than the average household monthly income in the country, which in 2019 amounted to €1,800.[11],[12]

Average household consumption is about €1,500 per month from 39 monthly transactions per household (including cash withdrawals); the average consumption expenditure per household is about €1,560 in the whole country.[13] Average household consumption follows a similar pattern to income across groups of households, with households outside forbearance exhibiting higher spending levels.

Households hold an average checking account balance of €6,700 and an average savings account balance of €17,300 (conditional on having a savings account); the median balances are significantly lower at €2,000 and €5,700, respectively (see Table IA.1). Notably, households in forbearance show substantially lower average balances in both checking and savings accounts, consistent with the fact that they are generally more fragile regardless of eligibility. Mortgage balances are, on average, €69,000, and households in forbearance have higher average mortgage balances. Almost all clients have a credit card or an overdraft,

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[11]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*).

[12]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).

[13]Estimate for 2015, excluding actual or imputed rentals for housing (Peralta, Carvalho, and Esteves, 2021).

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 an outstanding balance of about €7,500 on average.

The average total loan payment (mostly mortgage payments), including principal and interest, is €315, higher than the country's average by the end of 2019 (€248).[14] Moreover, households in forbearance have higher loan payment commitments (€377) than households outside forbearance (€311). By entering forbearance, the average household postpones €345. Considering total income, we estimate an average debt payment-to-income (DTI) of about 19% in 2019, slightly above the country's average in 2022 (17%).[15] Average DTI appears especially high at 32% for ineligible households in forbearance. Debt delinquency is infrequent in our sample, with just 1% of households having payments more than 30 days past due. However, the low delinquency rates on loan contracts observed in our sample may be a consequence of the sample selection criteria (namely, the fact that we only consider households with wage direct deposit).

4   Empirical Methodology

We estimate a difference-in-differences regression to compare consumption and total assets between households in forbearance and households outside forbearance before and after the start of the program:

$$y_{i,t} = \delta Forbearance_i \times Post_t + X_{i,t} + \mu_{i,m} + \lambda_{g,t} + \varepsilon_{i,t}, \tag{1}$$

where $y_{i,t}$ is either the logarithm of monthly consumption expenditures or total assets (i.e., the end-of-month balance of checking and savings accounts) for household i at time t. $Forbearance_i$ is a dummy variable that takes the value of one for households receiving

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[14]Press Release INE, Interest rates implied in housing loans, January 19, 2022.

[15]*Banco de Portugal*, *Relatório de Estabilidade Financeira*, November 2022.

forbearance and zero otherwise. *Post* is a dummy variable that takes a value of one for the period starting when mortgage payments are suspended for a particular household, and zero otherwise.[16] The coefficient of interest is δ, which captures whether being in forbearance is associated with a differential change in consumption or total assets before and after the program.

We also estimate the effect at different time horizons by replacing the *Post* dummy variable with three dummies: (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 (September of 2021).

$X_{i,t}$ is a set of household-level time-varying controls such as monthly total income. The regressions also include either household fixed effects (μ_i) to account for time-invariant household heterogeneity or household-month fixed effects ($\mu_{i,m}$) to also account for differences in household-specific monthly seasonality effects. All the regressions include month-year fixed effect (λ_t) to absorb shocks that may affect all households in a calendar month or group-month-year fixed effects ($\lambda_{g,t}$) to absorb time-varying shocks for households above or below the median of pre-pandemic assets and income (2019). In some specifications, we also include municipality-month-year fixed effects to account for regional shocks. Standard errors are clustered two-way at the household and month-year levels.

We estimate the dynamic effect of the forbearance from 29 months before to 29 after the beginning of the forbearance by interacting the forbearance dummy variable with time dummies, $\mathbb{1}(\text{period} = \tau)$:

$$y_{i,t} = \sum_{\tau=-29}^{28} \delta_\tau \times Forbearance_i \times \mathbb{1}(\text{period} = \tau) + X_{i,t} + \mu_{i,m} + \lambda_{g,t} + \varepsilon_{i,t}. \tag{2}$$

The coefficients of interest are δ_τ, which measure the change in consumption or total assets due to postponed debt payments.

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[16]We drop the household subscript for expositional purposes. The *Post* variable is household-specific since households entered forbearance between April and June 2020.

To estimate marginal propensities to consume and save, defined as the average change in consumption and total assets *per euro* of postponed debt payments, we estimate the following difference-in-differences regression:

$$y_{i,t} = \delta Forbearance\ Amount_i \times Post_t + X_{i,t} + \mu_{i,m} + \lambda_{g,t} + \varepsilon_{i,t}, \tag{3}$$

where $y_{i,t}$ is either the monthly consumption expenditures or the change in total assets for household i at time t. *Forbearance Amount$_i$* is the amount of postponed debt payments (mostly mortgages, but it may include other loans for some households) for household i; it takes the value of zero for households outside forbearance. The coefficient of interest is δ, which measures the marginal propensity to consume or save associated with postponed debt payments. We also present estimates of the marginal propensity to consume and save at different horizons by replacing the *Post* dummy variable with the *Immediate Effect*, *Short-run Effect*, and *Long-run Effect* dummy variables.

5   Effects of Debt Forbearance on Consumption and Savings

In this section, we first show the evolution of income, consumption, and savings before and after the government debt forbearance program that started in March 2020.

Figure 1 shows the evolution of 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. For all outcome variables, we plot the coefficients for each month δ_τ as in equation (2); we control for seasonality using month-year fixed effects but we do not control for any additional variables.

Panel A of Figure 1 shows that households in forbearance were more exposed to the COVID-19 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 start with, as shown in Table 1. While the gap in wages was temporary and disappeared by the third quarter of 2021, this suggests that reductions in wages were one important reason that led households to request forbearance (as opposed to just perceived health risks or uncertainty).

Panel B shows that the evolution of social security benefits disproportionately benefited households in forbearance since the onset of the pandemic and that this gap persisted until 2022. Panel C shows the evolution of total income, including both 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.2 of the Internet Appendix shows the evolution of income separately for households receiving forbearance until September of 2021 but no additional relief measures (the "Forbearance and No Add. Relief" group) and households receiving forbearance until September of 2021 who applied for additional measures put in place by the bank (i.e., loan maturity extension, interest rate reductions or additional repayment forbearance) in September of 2021 (the "Forbearance & Add. Relief" group). Panel A shows that households who asked for additional measures in September of 2021 had lower wages by €50 to €100 starting at the onset of the pandemic, which persisted until 2022. Households who did not ask for additional measures recovered wages quickly and were back to pre-pandemic levels by the end of 2020. Contrary to the view that households would take any relief that is offered, wage reductions help to explain why households request additional relief, and we see that the group exiting forbearance experienced a quick convergence of wages during the pandemic.

Figure 2 shows the evolution of the average consumption and total assets for the households in forbearance and outside forbearance. Panel A shows that both groups of households cut spending right after the start of the pandemic in March 2020, which is likely to be due to a combination of demand and supply factors. However, we find a positive and statistically significant effect on consumption for households in forbearance relative to households outside forbearance of about €200 per month by the summer of 2020. Interestingly, the changes in

consumption for households in forbearance result in a significantly higher average monthly consumption even compared to pre-pandemic levels. This is particularly noteworthy given that the total monthly income (see Table 1 and Figure 1) was *not* higher for households in forbearance.[17]

Panel B of Figure 2 shows the evolution of the average household total assets (checking and savings accounts end-of-month balances). Panel A shows that the beginning of the pandemic is associated with a slower growth in total assets of households in forbearance (mostly driven by checking accounts). By mid-2020, and even more so by early 2021, households in forbearance start increasing total assets, i.e., accumulating balances in both their checking and savings accounts. In fact, this group does so more quickly than the households outside forbearance when taking into consideration their lower average level of total assets (as shown in Table 1).

Panel B of Figure IA.3 in the Internet Appendix shows the same outcome but using the logarithm of consumption and total assets. The effect is still immediate and persistent and statistically significant at all horizons. Furthermore, by the end of the forbearance period, there is a gap of about 10% of the pre-pandemic consumption between the two groups. Figure IA.4 shows the evolution of consumption separately for the households in forbearance who received and did not receive additional relief measures in September of 2021. The "Forbearance & No Add. Relief" group benefits from a large increase in consumption that results in a significant gap relative to the control group, which persists throughout the sample period. In contrast, the "Forbearance & Add. Relief" group shows an evolution similar to the "No Forbearance" group, but consuming more than in the pre-pandemic period.

5.1 Response of Consumption and Savings to Forbearance

We now estimate the effect of the government debt forbearance program initiated in March 2020 on household consumption and savings using the regression in equation (2).

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[17]The plots show the evolution of consumption using equal weights among households. Table IA.2 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.

Table 2 shows the effects of debt forbearance on consumption and assets using the difference-in-differences regression in equation (1). Columns (1)-(4) show the estimates for the effect on consumption, and columns (4)-(8) show the effect on total assets. Columns (1) and (5) include month-year and household-month fixed effects. Columns (2), (3), (6), and (7) include the household group indicators using the median of income and total assets interacted with month-year fixed effects. Columns (4) and (8) also include municipality-month-year fixed effects. Additionally, in columns (3), (4), (7), and (8), we control for changes in total income, i.e., wages, pensions, social security benefits and other sources of income.

Column (1) shows that the forbearance effect on consumption is positive and significant at 16%. This effect represents about €215 at the average consumption of the households in the forbearance group. Columns (2)-(4), including household group-month-year fixed effects and total income as control, show a consistent effect on consumption of about 8%. Column (5) shows a positive and significant forbearance effect on total assets at 17%, but this effect is insignificant in columns (6)-(8) when we control for group-month-year fixed effects. As expected, total income has a positive and significant coefficient in both the consumption and savings regressions.

Next, we examine the response to the suspension of debt payments at different time horizons after forbearance. We estimate the regression in equation (1), replacing *Post* 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 the results.

The consumption effects are comparable to those in Table 2. Columns (1)-(3) show that the forbearance effect on consumption is positive and significant across all time horizons. In columns (2) and (3), including group-month-year fixed effects, the response increases from 4% to 9% with the horizon. Columns (4)-(6) present the estimates of the savings response at different horizons. The savings effects differ significantly across horizons. There is a negative and significant immediate effect at about 20% in columns (5) and (6). In addition, columns

(5)-(6), including household group fixed effects, show a positive and significant forbearance effect on savings at about 8-9% in the long-run horizon.

Figure 3 shows how consumption (Panel A) and total assets (Panel B) change 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 (δ_τ) coefficient each month obtained from the regression in equation (1). The coefficients measure the difference between households in forbearance and households outside forbearance relative to the month prior to the start of the forbearance program. Despite the significant average differences in the characteristics of the two groups, Panel A shows similar trends in consumption in the year preceding the start of the forbearance. In addition, we find a positive and significant forbearance effect of about 10% immediately after the start of the forbearance. The effect is persistent, increasing to about 15% by the 12-month horizon.

Panel B shows the savings response around forbearance. We find a positive and significant effect on savings after the start of the forbearance. The savings effect is persistent and increases over time. In addition, households in forbearance had a reduction in savings relative to households outside forbearance just before they entered forbearance (i.e., March to May 2020). This is consistent with households withdrawing savings to make up for the wage reduction documented in Figure 1.

Tables IA.3 and IA.4 show the coefficient estimates for two additional outcomes: credit card and overdraft (the end-of-the-month balance) and the total amount of debt held at other banks (end of month). In Table IA.3, columns (1)-(4) show a negative effect on the use of credit cards after the start of the forbearance, but the effect becomes insignificant when we include household group fixed effects. Columns (5)-(8) in Table IA.3 show a positive and significant effect of forbearance on the total debt amount at other banks, suggesting some substitution across banks. We also estimate the responses of credit cards and overdraft balances and total debt at other banks for different horizons. Table IA.4 presents the results. columns (1)-(3) show a positive effect at 8% on credit card and overdraft balances at the onset of the pandemic, but the effect becomes negative and significant at 9% one year after the forbearance when we

include household group fixed effects in columns (2) and (3). Columns (4)-(6) suggest that the savings response is persistent over time between 13% (immediate effect) and 7% (long-run effect) when we include household group fixed effects.

Our bank transaction-level data include merchant codes for debit and credit card transactions, which allow us to classify most of the consumption expenditures by category. Table IA.5 shows the estimates of the average forbearance effect on consumption per category in each column. The sample period is from January 2020 to September 2021 (merchant codes are not available in 2019). All regressions include household fixed effects and household group-month-year fixed effects. During the first year after the forbearance start (immediate and short-run effects), the main drivers of consumption response to forbearance are "Housing Maintenance and Utilities" and "Miscellaneous Goods and Services" expenditures with a positive and significant effect of about 5%. The short-term effect is also positive and significant for "Groceries" at 10%. In addition, the long-term effect (beyond 12 months) is positive and significant for "Groceries" (at 10%), "Housing Maintenance and Utilities" (at 5%), "Furniture" (at 10%), and "Health Care" (at 10%).

5.2 Marginal Propensity to Consume and Save

In this section, we estimate the propensity to consume and save out of the postponed mortgage payments due to the debt forbearance program that started in March 2020. We estimate the regression in equation (3) where the dependent variable is consumption or total assets in euros and the main explanatory variable is $Forbearance\ Amount_i$, defined as the amount of postponed debt payments in euros.

Table 4 shows the estimates of the marginal propensity to consume and save out of the postponed debt payments. The regressions include the same fixed effects as in Table 2. We find that the average forbearance effect on consumption is significant across all specifications. The consumption effect is about 30 cents per euro of postponed debt payments in column (1). When we control for income and group-month-year fixed effects, this estimate drops to about 19 cents. In addition, we find a significant effect on savings, with households saving as much

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as 14 to 18 cents per euro of postponed payments in columns (6) and (7) when we control for changes in income and group-month-year fixed effects.

Next, we examine the marginal propensity to consume and save out of the postponed debt payments at different horizons after the start of the forbearance. Table 5, column (1), shows that the immediate effect on consumption is significant at about 17 cents per euro of postponed payments. However, when we control for household group-month-year fixed effect and total income in columns (2) and (3), this estimate becomes insignificant. The short-term effect is significant across all specifications at 18 to 30 cents, while the long-term effect is even stronger at 30 to 37 cents. We also find that households save as much as 11 cents per euro of postponed payments in the short run, decreasing to about 10 cents over the long run in column (1). After controlling for group-month-year fixed effects and total income in column (6), the savings response on savings becomes similar over the three horizons: 11 cents (immediate effect), 21 cents (short-term effect), and 14 cents (long-term effect). Overall, the results suggest that households consumed and saved a significant fraction of the postponed payments.

5.3   Heterogenous Effects

We investigate the extent to which the propensity to consume or save out of postponed debt payments is heterogeneous across households with different levels of fragility as proxied by wealth, income, and indebtedness. We estimate the $Forbearance\ Amount_i \times Post_t$ variable coefficient using the regression in equation (3) separately for the sample of more fragile households and the sample of less fragile households. The more fragile households are those with pre-pandemic below-median total assets and income and above-median debt payment-to-income (DTI) ratio. The less fragile households are those with pre-pandemic above-median total assets and income and below-median debt payment-to-income ratio. Figure 4 shows the effects on the average propensity to consume and save at the immediate, short-run, and long-run horizons. The figure shows very heterogeneous consumption and savings responses to debt forbearance across households with different levels of fragility.

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Panels A-C show that the consumption response is concentrated primarily in most fragile households, especially at the short-run and long-run horizons; the immediate effect is statistically insignificant for all household groups. In Panel A, the low-assets household group response is positive and significant at about 25 cents per euro of postponed payments in the short-run horizon, further increasing to about 33 cents at the long-run horizon. This shows that the consumption response to forbearance is mainly driven by poorer households. In Panel B, we show that the consumption response is stronger for low-income households than for high-income households, but the difference is economically small, which suggests that wealth is somewhat more important than income for explaining the magnitude of the response in our setting. Finally, Panel C shows a stronger consumption response for households with a high DTI ratio relative to a low DTI ratio in short-run and long-run horizons. In fact, the point estimate for the high DTI household group is almost twice as high as that for the low DTI household group.

Panels D-F show that the response to debt forbearance across different groups is also heterogeneous when we study the marginal propensity to save. Panel D shows that high-assets households use more than a quarter of the postponed payments to increase savings, especially in the short-run horizon. Over the long-run horizon, the response of the high-assets group is lower and similar to that of the low-assets group at about 10 cents. Panel D also shows that the low-assets group saves about 15 cents immediately after the forbearance starts, but the effect becomes statistically insignificant in the short-run horizon. Panel E shows a stronger savings response for the high-income group versus the low-income group in the immediate and short-run horizons, but the response is lower and similar between groups in the long-run horizon. Finally, Panel F shows a similar pattern when we split households by the median DTI ratio. As expected, the low DTI group, which exhibits a low consumption response, saves as much as 25 cents per euro of postponed payments in the short-run horizon. The effects are milder in both the immediate and long-run horizons.

In sum, there are very heterogeneous responses to debt forbearance across households with different levels of fragility as proxied by wealth, income, and indebtedness. The consumption

response is concentrated primarily in the more fragile households, in particular those with low wealth, while the savings response is concentrated in the less fragile households.

6   Forbearance Eligibility and Selection

Access to the debt forbearance program during the COVID-19 pandemic was generally lax, unlike the restrictive access to modifications that prevailed during the 2008-2009 foreclosure crisis in the United States (Adelino, Gerardi, and Willen (2013)). We use our microdata to construct a measure of whether a household was eligible to obtain forbearance according to the criteria put in place by the government. The legal criteria for an individual to be eligible for debt forbearance included: (1) individuals working in industries more affected by the COVID-19 lockdowns as defined in the government legislation; (2) individuals experiencing, or expecting, a 20% reduction in income due to the pandemic (the exact timing or definition of income were not clear in the legislation); (3) individuals infected with COVID or providing assistance to a relative infected with COVID (which represented a very small fraction of the individuals in the population early in the pandemic).

We construct the empirical analog in our data to these legal criteria by identifying the industries in which individuals work by matching employer names to a list of companies with industry codes.[18] We also measure the income drop as 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). We consider a household as eligible if any individual suffered an income drop of 20% or more or if any household member was working in more affected industries during the first quarter of 2020.

We estimate the effect of forbearance on the logarithm of consumption and savings using the regression in equation (2) separately for four household groups: (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,

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[18]The list provided by the government considers a broad industry definition, which we then match to the Portuguese industry classification code list (*CAE*, Revision 3).

we define a *Eligible* dummy variable that takes the value of one for households eligible to the forbearance program according to the legal criteria, and zero otherwise. We then interact the *Eligible* dummy variable with the *Forbearance* and the *Post* dummy variables. For households in forbearance, *Post* takes the value of one when mortgage payments are suspended (April-June 2020), and zero otherwise. For households outside forbearance, *Post* takes the value of one after March 2020, and zero otherwise.

Table 6 shows the effect of forbearance on consumption and savings for the four groups. The table also shows, at the bottom, the proportion of the sample in each of the four groups. The proportion of households in each group confirms that access to the forbearance program was lax during the pandemic and that the implementation of the program on the part of banks erred on the side of giving households access to forbearance. Specifically, we find that about 10% of households were eligible for forbearance, but only about 11% of those eligible households actually entered the program (or 1.1% of the full sample). On the other hand, 5.3% of the ineligible households entered the program (or 4.8% of the full sample). This implies that over 80% of the households in forbearance in the sample were ineligible, and less than 20% were, in fact, eligible. This is consistent with the inexistence of formal checks of the eligibility criteria during the pandemic, as well as bank officers not being incentivized to screen out debt forbearance applications.[19] As we discuss in Section 3.1, households in and outside forbearance, as well as eligible and ineligible households, are different based on their observable characteristics. Ineligible individuals in forbearance have the lowest total income of the four groups, as well as the highest debt payment-to-income ratio.

There is substantial heterogeneity in the effects of forbearance on consumption. We find that eligible households outside forbearance consume significantly less after the beginning of the forbearance period relative to ineligible individuals also outside of forbearance (the omitted group in these regressions, which corresponds to 85% of the sample). This difference is about -5% in column (1) and -2.6% when we include group-month-year fixed effects and total income

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[19]While we acknowledge that our eligibility measure may not be perfect, the proportion of the sample that we tag as ineligible seems clearly above what one would expect from purely assignment error from our measures.

as a control in columns (2) and (3). Eligible households in forbearance avoid this drop in consumption, and their consumption response is positive at 1.3% but not statistically different from that of ineligible households outside of forbearance. This indicates that debt forbearance was just enough to compensate for the pandemic shock suffered by eligible households in forbearance. However, ineligible households that anyway entered forbearance had an increase of about 7.5% in consumption relative to the omitted group in columns (2) and (3). This means that the increase in consumption we observe in Table 2 is mostly driven by ineligible households who nevertheless applied for and received forbearance.

Columns (5) and (6) of Table 6 show that ineligible households in forbearance did not save more than ineligible households outside of forbearance. At the same time, forbearance is not associated with higher savings for the eligible group: savings drop by about 4% for eligible households outside forbearance and about 8% for eligible households in forbearance (relative to the omitted group) in all specifications. This lack of savings response is consistent with Table 2 where the interaction *Forbearance × Post* is insignificant in columns (2) and (3), and with Table 3 where the savings response is only positive over the long-run horizon.

We can use the fact that we have four groups of households (eligible and ineligible, in and outside forbearance) along with an assumption of homogeneity of the effect of forbearance across eligible and ineligible households to bound the magnitude of the selection effect.[20]

Selection is important in our setting as over 80% of the households in forbearance in our sample are ineligible. For eligible households, forbearance is associated with 3.9% (= -2.6% - 1.3%) higher consumption. This estimate is significantly lower than the one for the group of ineligible households where forbearance is associated with 7.4% higher consumption. If we assume treatment effect homogeneity, the 3.5 percentage point difference between the two estimates suggests that differences in who selects into forbearance programs when access is easy significantly affect the estimated forbearance effect. In other words, our estimates

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[20]Given that the nature of the COVID shock was unanticipated, it is reasonable to assume that the average effect of forbearance on the set of households that experienced wage reductions and worked in the affected industries would be at least the same as for the rest of the population. This is not the same as saying that the effect of forbearance has to be homogeneous in the whole population, as we discuss below. To the extent that the average treatment effect for eligible households is larger than for ineligible ones, this assumption means that we underestimate the role of selection.

indicate that ineligible households who enter the forbearance program increase consumption more relative to eligible households who enter the program. There are at least two interpretations of this selection. One is that the counterfactual change in consumption of the groups in forbearance would have been different in the absence of forbearance. Another is the heterogeneity of the effect of forbearance for the households who select into the program.

Table 7 examines the eligibility effects separately for more and less affected household groups. Column (1) focuses on industries that were more affected by the pandemic, defined as those with below-median revenue growth between 2019 and 2020. We classify the *more affected* subsample by identifying households whose primary employer belongs to the below-median group of industries during the first quarter of 2020 (pre-pandemic). 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).[21] 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). Interestingly, column (1) suggests that similar households entered forbearance, as the effects on consumption do not differ with eligibility: 4.8% increase for ineligible households versus 5.1% for eligible households.

A very different picture emerges when we focus instead on industries that are less affected by the pandemic, defined as those with above-median revenue growth between 2019 and 2020, or when we focus on public servants (who suffered no changes in wages and were ineligible according to the legal criteria).[22] First, the fraction of households satisfying the eligibility criteria drops to 14% for the less affected industry workers and to 5% for public servants. In these sectors, ineligible households represent a greater proportion of households in forbearance (76% for less affected sectors and 90% for public servants), as well as their

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[21] As we measure revenue growth until December 2020, and this was obviously only available after March 2020, the date of eligibility definition by the government

[22] We observe that around 5% of public servants were eligible, the majority of which correspond to households satisfying the criteria on the *secondary* employer, i.e., on the secondary source of wages for the household.

behavior is fundamentally different from that of eligible households. While the effect on consumption for eligible households in forbearance is statistically insignificant, the effect for ineligible households in forbearance is positive and significant at about 11% in column (2). The effect is similar in the sample of public servants in column (3).

7   Effect of Additional Debt Relief Measures

During the summer of 2021, banks assessed the level of risk of borrowers in forbearance at the request of the regulator. For those deemed to be at a higher risk of default at the end of the forbearance period, the bank implemented a survey to determine whether they would qualify for additional debt relief measures. In addition, all borrowers in forbearance were informed (by SMS, email, and through the bank's app) that additional assistance was available if they had trouble meeting their obligations. As in the government forbearance program, access to additional debt relief was based on an interview with borrowers and not a formal verification process of the borrower's income or other financial difficulties. In this section, we compare the evolution of consumption for households with and without additional debt relief measures implemented by the bank in September 2021.

To study the persistence of the program on household behavior, we extend the sample through June 2022. We also extend the regression in equation (1) to include the possibility that some households may exit forbearance after September 2021. The regression includes indicators for the three different horizons presented before (*Immediate Effect*, *Short-run Effect*, and *Long-run Effect*). In addition, to account for the effect of forbearance after the end of the program, we include a *Exit Effect* indicator that takes the value of one between September 2021 and June 2022, and zero otherwise.

Table 8 presents the results. Column (1) shows that consumption is affected by the end of the forbearance program, with the differential increase in consumption dropping significantly and approaching pre-pandemic levels. We find that the long-run forbearance effect on consumption is about 8%, but the effect drops to about 2% after the end of the

forbearance. The estimates for savings in column (3) are consistent with those for consumption. In fact, we find that the long-run forbearance effect on total assets changes significantly after the forbearance ends, dropping from 13% to about -2% (statistically insignificant) after the forbearance ends.

When we consider the additional relief measures, a somewhat more subtle pattern emerges. Columns (2) and (4) of Table 8 present the results. We find that about 7.4% of ineligible households in forbearance request additional relief, whereas about 6.1% of eligible households do so. Table 8 also shows that households receiving additional relief measures in September 2021 do not adjust their consumption differential at the end of the forbearance period (column (2)). This is surprising in light of the fact that these were clearly temporary measures, and it is consistent with the "consumption commitments" model in Chetty and Szeidl (2007) for this subset of borrowers. In addition, households receiving additional relief measures exhibit an overall negative trend of savings throughout the whole forbearance period compared to other households who were also in forbearance (column (4)). This pattern persists after the initial government forbearance program is converted into private bank assistance for these households, as shown by the coefficient on *Exit Effect* in column (4), which is still very negative, although slightly less so than the *Long-run Effect*.

8   Conclusion

Government debt relief programs during the COVID-19 pandemic provide a unique laboratory to understand which borrowers select into large-scale forbearance and how borrowers respond to preemptive interventions even before large-scale defaults. In this paper, we study the household debt moratorium programs put in place in Portugal during the pandemic using bank-account transaction data and balance sheet data.

We find that households in forbearance suffered a larger drop in income at the start of the pandemic relative to households outside forbearance. In addition, forbearance has a positive and significant impact on consumption and savings, with households in forbearance increasing

spending by about 8% relative to pre-pandemic levels. Forbearance also has a positive and significant impact on savings at about 8% but only over one year after the suspension of debt payments. There is also an increase of almost 20 cents per euro of postponed debt payment in both the marginal propensity to consume and save.

The response to forbearance is heterogeneous for households who were and were not eligible for the forbearance program. For eligible households, the forbearance allowed them to maintain their consumption path and avoid a relative drop in consumption during the pandemic, but they suffered a drop in savings. For ineligible households that entered forbearance, however, we find a relative increase in consumption and no impact on savings. Thus, the increase in consumption we document is mainly driven by ineligible households who ask for forbearance. In addition, the consumption and savings response to forbearance is heterogeneous across households with different levels of fragility as proxied by wealth, income, and indebtedness. The marginal propensity to consume is significantly higher for more fragile households, while the marginal propensity to save is significantly higher for less fragile households.

Our findings provide new insights about the need for debt forbearance during crises. Households who requested forbearance suffered significant income drops and drew down their savings just before the start of forbearance. At the same time, households in forbearance had higher consumption and savings than before the pandemic. This suggests that households might have been able to meet debt payments even without the forbearance program. The same is true, even more so, for less fragile households who saved a significant fraction of their postponed debt payments. Taken together, our evidence suggests that a forbearance program with a shorter duration and more frequent renewals could have achieved the objective of avoiding loan delinquency at a lower cost to banks.

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Figure 1: Evolution of Income
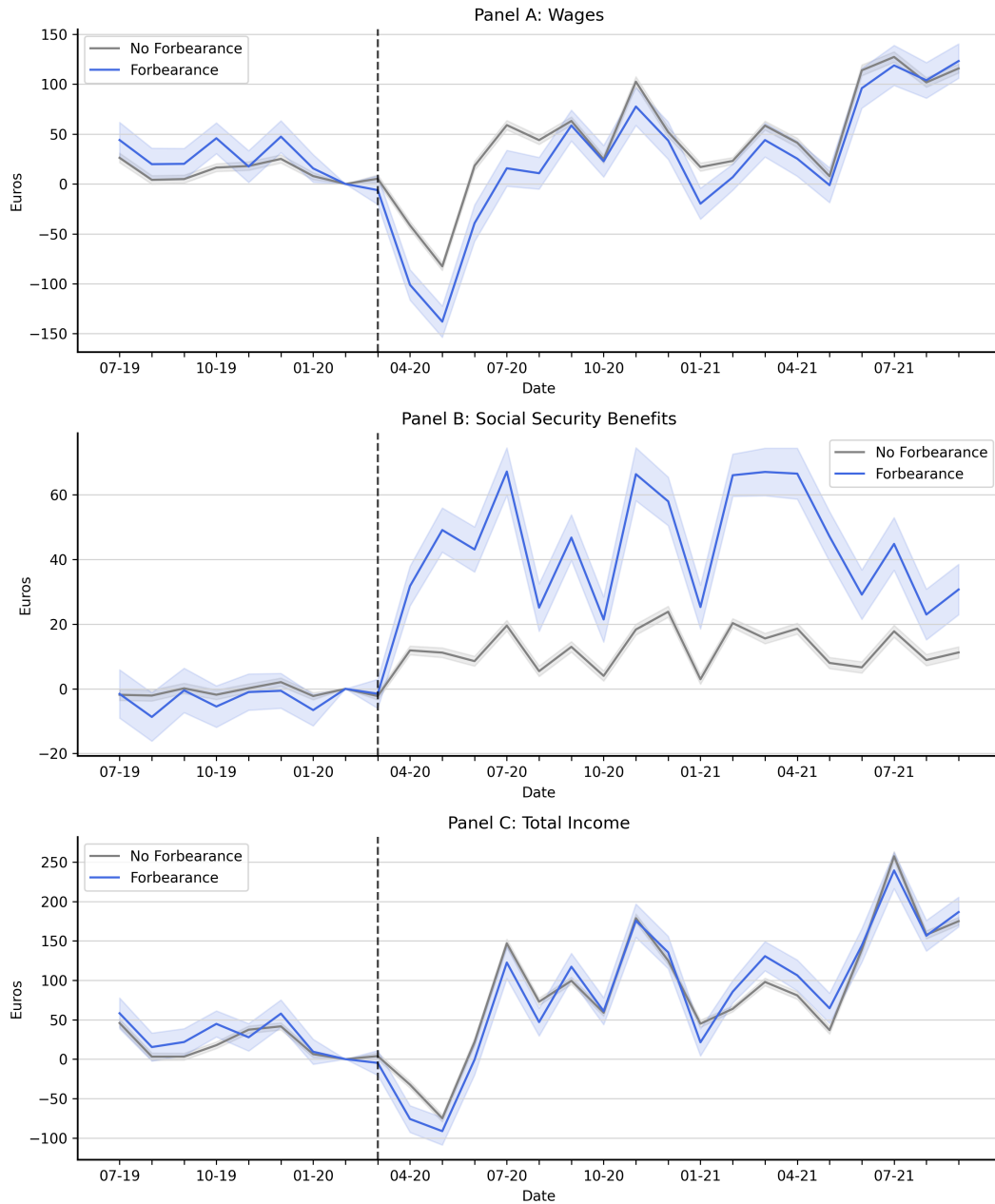
This figure plots the household average for different income sources, as well as the corresponding 95% confidence intervals, from July 2019 to September 2021. All measures are reported in euros, seasonally adjusted and relative to a pre-pandemic baseline (February 2020). Panel A reports the average change in monthly direct deposit of wages relative to the pre-pandemic baseline, while Panel B shows the change in social security benefits received. Panel C shows the change in total monthly income relative to the baseline, computed as the sum between monthly wages, social security and retirement benefits. In all panels, the average change is represented separately for households who received forbearance (in blue) and those who never entered forbearance (in grey). Standard errors are clustered at the household level.

Figure 2: Evolution of Consumption and Assets
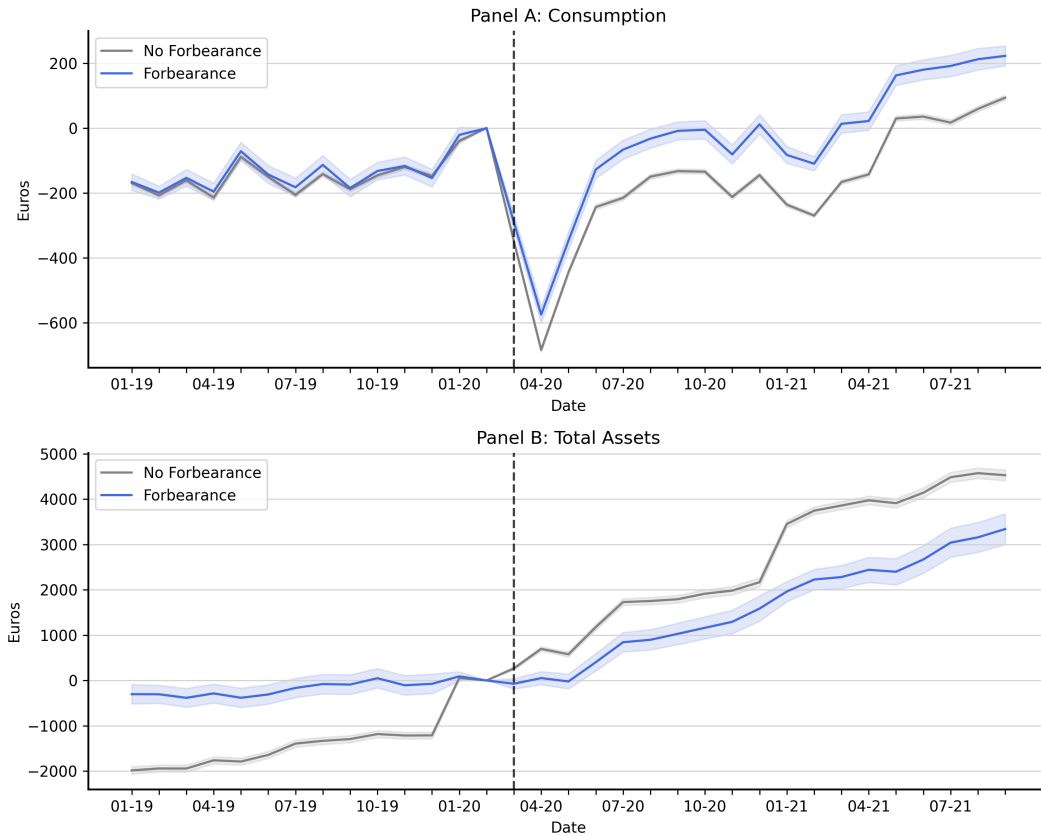
Panel A: Consumption

Panel B: Total Assets

This figure plots the household average for monthly consumption and assets, as well as the corresponding 95% confidence interval, from January 2019 to September 2021. All measures are reported in euros, seasonally adjusted and relative to a pre-pandemic baseline (February 2020). Panel A reports monthly consumption, computed as the sum between purchases and payments from either a debit or credit card at this bank. Panel B shows the evolution of total assets, computed as the sum between end-of-the-month checking and saving accounts' balances. In both panels, the trends are shown separately for households who received forbearance (in blue) and those who never entered forbearance (in grey). Standard errors are clustered at the household level.

Figure 3: Household Response in Consumption and Savings around Forbearance
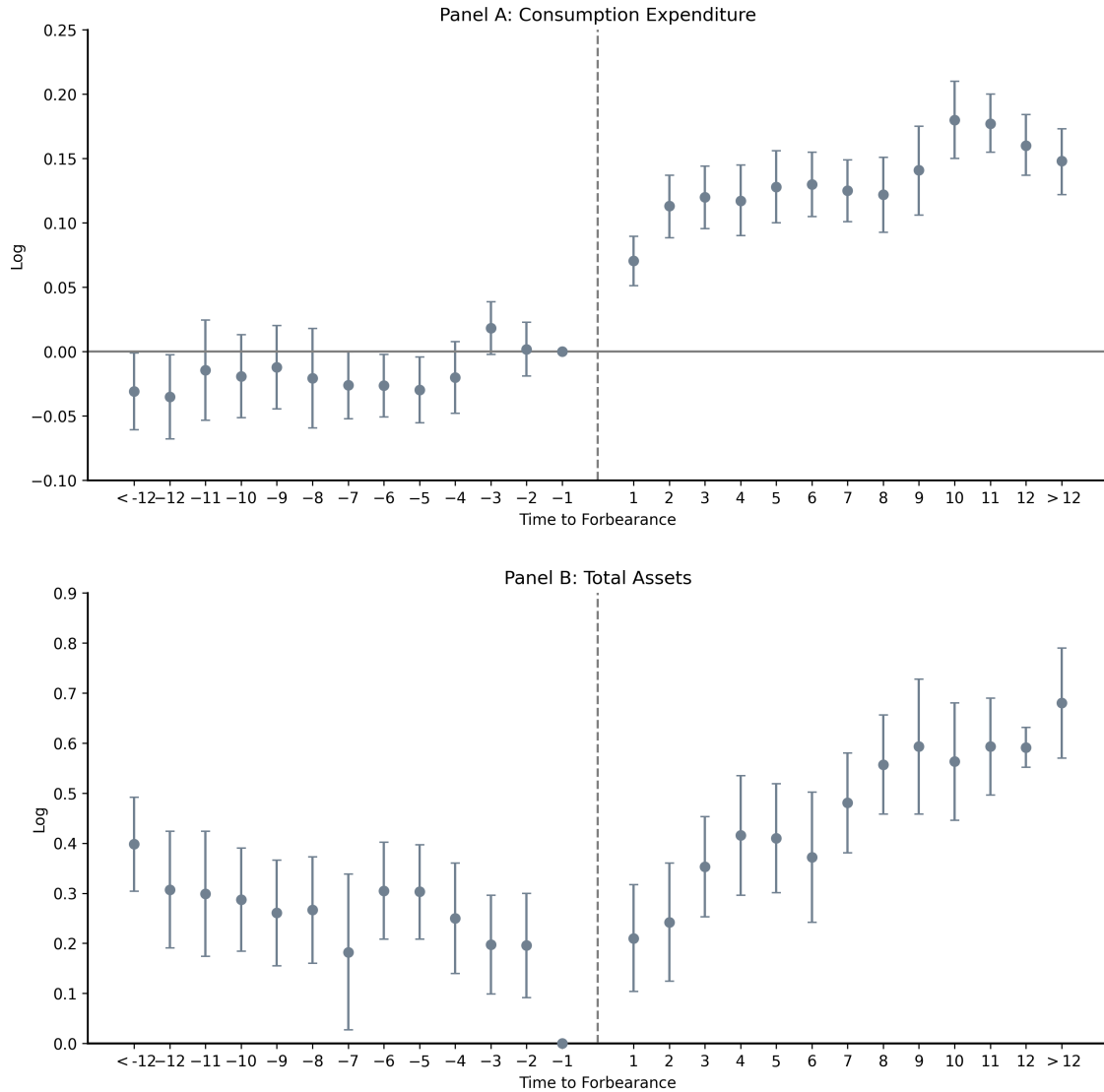
This figure plots the regression coefficients and 95% confidence intervals of the logarithm of monthly consumption and total assets, in Panel A and B, respectively, on month dummies around the start of the forbearance (which is household-specific). Both specifications follow the difference-in-differences model given by equation (2) and use the month prior to the start of forbearance as a baseline. In Panel A we show the estimates for the average percentage change on monthly spending, measured as the sum between purchases and payments from either a debit or credit card at this bank around the beginning of the forbearance measure. In Panel B we plot the average percentage change in total assets, measured as the sum between end-of-the-month checking and saving accounts' balances. This specification includes household-month and month-year fixed effects. Standard errors are computed using two-way clustering (household and month-year level).

Figure 4: Marginal Propensity to Consume and Save by Group: Wealth, Income and Debt-to-Income Ratio
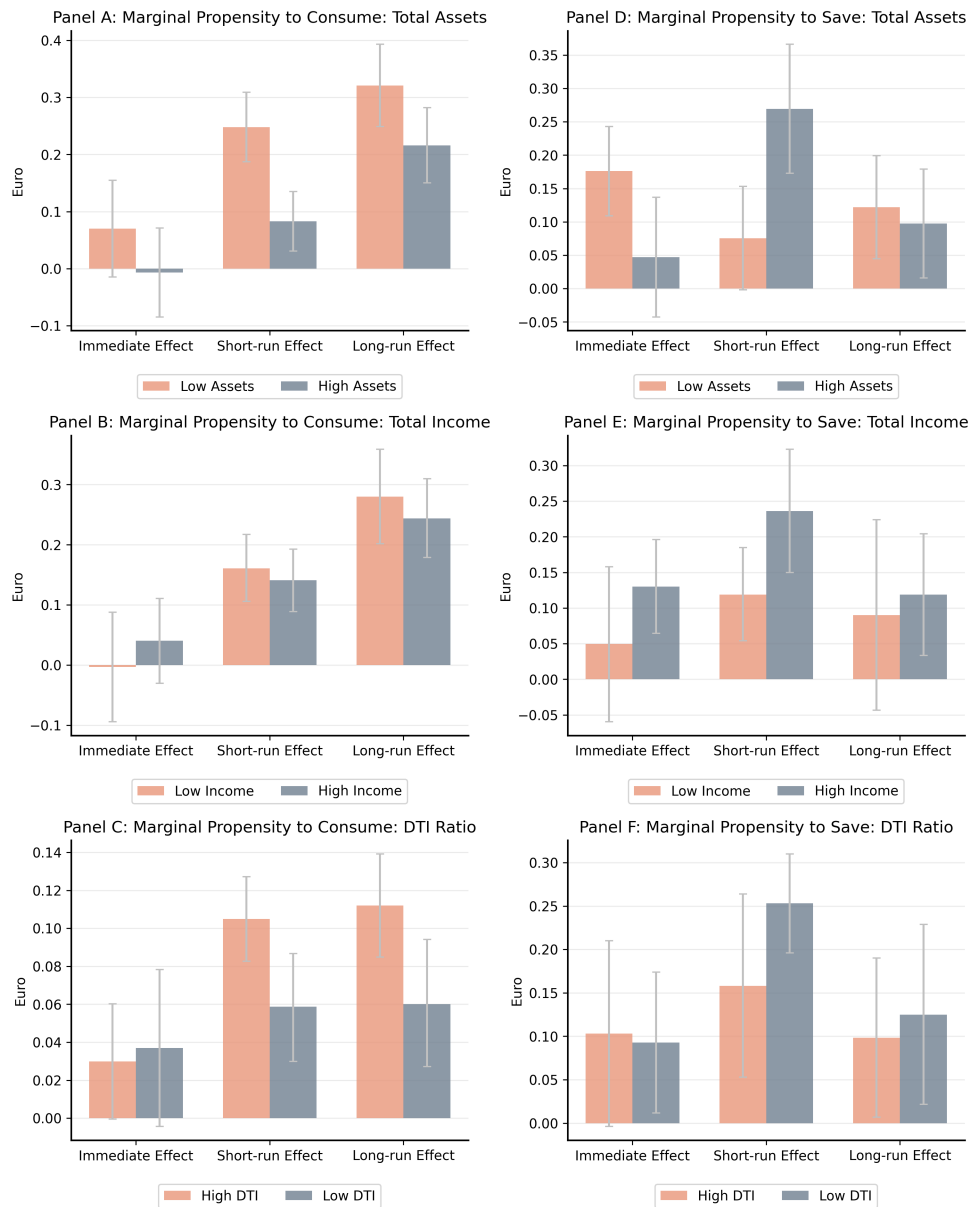
This figure plots the regression coefficients and 95% confidence intervals for monthly consumption and changes in total assets on time dummies around the start of the forbearance. All panels consider below/above median subgroups, in relation to pre-pandemic assets and income (2019 averages) and follow the difference-in-differences model given by equation (3), but using three different time dummy variables instead of a *Post* indicator: the *Immediate Effect* measures the impact over the first quarter after the start of forbearance; the *Short-run Effect* describes the average effect from the 4th to the 12th month; and the *Long-run Effect*, measuring the average effect after one year. The dependent variable on Panels A to C, monthly consumption, is measured as sum between purchases and payments from either a debit or credit card at this bank. On Panels D to F, the dependent variable is changes in total assets, measured as sum between end-of-the-month checking and saving accounts' balances. This specification includes household-month and month-year fixed effects, with the group referring to different below/above median subgroups in relation to pre-pandemic assets and income (2019 averages). As a control, we include total income, which includes wages, social security and retirement benefits. Standard errors are computed using two-way clustering (household-month and month-year level).

Table 1: Average Household Characteristics by Groups

	No Forbearance		Forbearance		Full Sample
	Ineligible	Eligible	Ineligible	Eligible	
Average Age	48.4	45.0	45.5	43.2	47.9
Number of Mortgagors	1.6	1.7	1.7	1.7	1.7
Wages	1,830.2	1,885.3	1,416.1	1,620.2	1,816.1
Pensions	1,339.1	1,128.7	1,088.5	1,020.0	1,313.9
Social Security Benefits	345.1	349.5	299.3	303.3	341.7
Other Inbound Transfers	658.7	638.8	789.7	651.0	663.2
Total Income	2,530.6	2,758.9	2,046.3	2,448.9	2,527.1
Consumption	1,505.7	1,614.8	1,320.0	1,441.5	1,505.9
Checking Accounts	6,963.2	6,732.3	3,079.5	3,022.3	6,710.8
Savings Accounts	17,898.3	16,368.5	9,159.2	8,592.8	17,356.5
Mortgage Loans	67,211.5	73,339.0	92,346.1	96,256.2	69,304.6
Credit Cards and Overdraft	401.3	375.3	770.7	653.9	419.6
Other Banks' Loans	7,004.7	7,416.8	13,983.5	13,456.6	7,451.0
Debt Payment	312.0	305.2	381.0	360.3	315.3
Debt Payment-to-Income	0.19	0.15	0.32	0.20	0.19
Forbearance	0.00	0.00	1.00	1.00	0.06
Forbearance Amount	0.0	0.0	347.2	332.9	20.5
7 Day Delinquency	0.01	0.01	0.06	0.03	0.02
30 Day Delinquency	0.01	0.01	0.03	0.01	0.01
Observations	116,732	12,469	6,637	1,525	137,363

This table shows pre-pandemic means (2019) values for which non-missing records exist, over households who requested forbearance and those who never entered forbearance, and further dividing those two groups depending on whether they were eligible or not. Income, assets, liabilities and consumption measures are winsorized at the top and bottom 1% by date. Statistics are computed on household averages over 2019.

Table 2: Effect of Forbearance on Consumption and Savings

	Log(Consumption)				Log(Total Assets)			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Forbearance × Post	0.160***	0.079***	0.075***	0.077***	0.169***	-0.019	-0.027	-0.025
	(0.011)	(0.010)	(0.010)	(0.010)	(0.039)	(0.030)	(0.029)	(0.030)
Log(Total Income)			0.035***	0.035***			0.072***	0.073***
			(0.005)	(0.004)			(0.006)	(0.006)
Household × Month FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Month × Year FE	Yes	No	No	No	Yes	No	No	No
Group × Month × Year FE	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Municipality × Month × Year FE	No	No	No	Yes	No	No	No	Yes
R^2	0.589	0.596	0.597	0.599	0.853	0.857	0.857	0.858
Observations	6,181,335	6,181,335	6,181,335	6,181,335	6,181,335	6,181,335	6,181,335	6,181,335

This table presents difference-in-differences estimates of regressions of the logarithm of monthly consumption and total assets, according to the model given in equation (1). Observations are at the household-calendar date level and the panel runs from January 2018 to September 2021. The dependent variable in columns (1) to (4) is measured as the logarithm of the sum between purchases and payments from either a debit or credit card at this bank; while from columns (5) to (8) the dependent variable is measured as the logarithm of the sum between end-of-the-month checking and saving accounts' balances. *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 the value of one at the beginning of the mortgage payment suspension and zero otherwise. As a control, we include in some specifications the logarithm of total income, which includes wages, social security, and retirement benefits. In some specifications, we include group-month-year fixed effects, with the group referring to different below/above median subgroups in relation to pre-pandemic assets and income (2019 averages). Standard errors in parentheses are computed using two-way clustering (household and month-year level). *, **, and *** indicate statistical significance at the 10%, 5%, and 1% level, respectively.

Table 3: Effect of Forbearance on Consumption and Savings by Time Horizon

	Log(Consumption)			Log(Total Assets)		
	(1)	(2)	(3)	(4)	(5)	(6)
Forbearance ×						
Immediate Effect (1m-3m)	0.129***	0.039***	0.036**	-0.078**	-0.205***	-0.212***
	(0.012)	(0.014)	(0.014)	(0.038)	(0.028)	(0.028)
Short-run Effect (4m-12m)	0.161***	0.084***	0.081***	0.170***	-0.016	-0.022
	(0.013)	(0.010)	(0.010)	(0.039)	(0.031)	(0.031)
Long-run Effect (>12m)	0.176***	0.091***	0.086***	0.327***	0.093***	0.082***
	(0.011)	(0.011)	(0.011)	(0.036)	(0.025)	(0.025)
Controls	No	No	Yes	No	No	Yes
Household × Month FE	Yes	Yes	Yes	Yes	Yes	Yes
Month × Year FE	Yes	No	No	Yes	No	No
Group × Month × Year FE	No	Yes	Yes	No	Yes	Yes
R^2	0.589	0.596	0.597	0.853	0.857	0.857
Observations	6,181,335	6,181,335	6,181,335	6,181,335	6,181,335	6,181,335

This table presents difference-in-differences estimates of regressions of the logarithm of monthly consumption and total assets, according to the model given in equation (1), but using three different time dummy variables instead of a *Post* indicator: the *Immediate Effect* measures the impact over the first quarter after the start of forbearance; the *Short-run Effect* describes the average effect from the 4th to the 12th month; and the *Long-run Effect*, measuring the average effect after one year. Observations are at the household-calendar date level and the panel runs from January 2018 to September 2021. The dependent variable in columns (1) to (3) is measured as the logarithm of the sum between purchases and payments from either a debit or credit card at this bank; while from columns (4) to (6) the dependent variable is measured as the logarithm of the sum between end-of-the-month checking and saving accounts' balances. *Forbearance* is a dummy variable that takes the value of one for households receiving forbearance and zero otherwise. As a control, we include in some specifications the logarithm of total income, which includes wages, social security, and retirement benefits. In some specifications, we include group-month-year fixed effects, with the group referring to different below/above median subgroups in relation to pre-pandemic assets and income (2019 averages). Standard errors in parentheses are computed using two-way clustering (household and month-year level). *, **, and *** indicate statistical significance at the 10%, 5%, and 1% level, respectively.

Table 4: Average Propensity to Consume and Save

	Consumption			ΔTotal Assets		
	(1)	(2)	(3)	(4)	(5)	(6)
Forbearance Amount $\times$ Post	0.295***	0.185***	0.191***	0.070*	0.137***	0.176***
	(0.026)	(0.028)	(0.028)	(0.036)	(0.023)	(0.021)
Total Income			0.083***			0.518***
			(0.006)			(0.020)
Household $\times$ Month FE	Yes	Yes	Yes	Yes	Yes	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	0.665	0.668	0.669	0.350	0.351	0.362
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 monthly consumption and changes in total assets, according to the model given in equation (3). Observations are at the household-calendar date level and the panel runs from January 2018 to September 2021. The dependent variable in columns (1) to (3) is measured as the sum between purchases and payments from either a debit or credit card at this bank; while from columns (4) to (6) the dependent variable is measured as changes in the sum between end-of-the-month checking and saving accounts' balances. *Forbearance Amount* is the amount of postponed debt payments (mostly mortgages but it may include other loans for some households), taking the value of zero before the start of the forbearance for all households, as well as after the start of the forbearance (which might vary between March and June of 2020) for households not postponing payments. *Post* is a dummy variable that takes the value of one at the beginning of the mortgage payment suspension and zero otherwise. As a control, we include in some specifications total income, which includes wages, social security, and retirement benefits. In some specifications, we include group-month-year fixed effects, with the group referring to different below/above median subgroups in relation to pre-pandemic assets and income (2019 averages). Standard errors in parentheses are computed using two-way clustering (household and month-year level). *, **, and *** indicate statistical significance at the 10%, 5%, and 1% level, respectively.

Table 5: Average Propensity to Consume and Save by Time Horizon

	Consumption			ΔTotal Assets		
	(1)	(2)	(3)	(4)	(5)	(6)
Forbearance Amount $\times$						
Immediate Effect (1m-3m)	0.169***	0.012	0.027	-0.126	0.018	0.110***
	(0.038)	(0.041)	(0.041)	(0.096)	(0.027)	(0.026)
Short-run Effect (4m-12m)	0.296***	0.183***	0.188***	0.109***	0.175***	0.206***
	(0.025)	(0.023)	(0.023)	(0.039)	(0.030)	(0.028)
Long-run Effect (>12m)	0.373***	0.299***	0.302***	0.095*	0.120***	0.141***
	(0.028)	(0.028)	(0.027)	(0.048)	(0.026)	(0.029)
Controls	No	No	Yes	No	No	Yes
Household $\times$ Month FE	Yes	Yes	Yes	Yes	Yes	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	0.665	0.668	0.669	0.350	0.351	0.362
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 monthly consumption and changes in total assets, according to the model given in equation (3), but using three different time dummies instead of a *Post* indicator: the *Immediate Effect* measures the impact over the first quarter after the start of forbearance; the *Short-run Effect* describes the average effect from the 4th to the 12th month; and the *Long-run Effect*, measuring the average effect after one year. Observations are at the household-calendar date level and the panel runs from January 2018 to September 2021. The dependent variable in columns (1) to (3) is measured as the sum between purchases and payments from either a debit or credit card at this bank; while from columns (4) to (6) the dependent variable is measured as the change in the sum between end-of-the-month checking and saving accounts' balances. *Forbearance Amount* is the amount of postponed debt payments (mostly mortgages but it may include other loans for some households), taking the value of zero before the start of the forbearance for all households, as well as after the start of the forbearance (which might vary between March and June of 2020) for households not postponing payments. As a control, we include in some specifications total income, which includes wages, social security, and retirement benefits. In some specifications, we include group-month-year fixed effects, with the group referring to different below/above median subgroups in relation to pre-pandemic assets and income (2019 averages). Standard errors in parentheses are computed using two-way clustering (household and month-year level). *, **, and *** indicate statistical significance at the 10%, 5%, and 1% level, respectively.

Table 6: Effect of Forbearance on Consumption and Savings by Eligibility and Selection Groups

	Log(Consumption)			Log(Total Assets)		
	(1)	(2)	(3)	(4)	(5)	(6)
Forbearance=0 × Eligible=1 × Post	-0.049***	-0.026***	-0.026***	-0.034***	-0.039***	-0.042***
	(0.007)	(0.007)	(0.007)	(0.011)	(0.011)	(0.011)
Forbearance=1 × Eligible=0 × Post	0.179***	0.076***	0.074***	0.185***	0.032	0.026
	(0.013)	(0.011)	(0.011)	(0.041)	(0.033)	(0.033)
Forbearance=1 × Eligible=1 × Post	0.052***	0.011	0.013	0.084*	-0.083*	-0.080*
	(0.018)	(0.017)	(0.017)	(0.046)	(0.041)	(0.041)
Controls	No	No	Yes	No	No	Yes
Household × Month FE	Yes	No	No	Yes	No	No
Month × Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Group × Month × Year FE	No	Yes	Yes	No	Yes	Yes
R^2	0.591	0.596	0.598	0.853	0.856	0.857
Observations	6,181,335	6,181,335	6,181,335	6,181,335	6,181,335	6,181,335
Observations:						
Forbearance=0 × Eligible=0	116,732					
% of sample	*(85.0%)*					
Forbearance=0 × Eligible=1	12,469					
% of sample	*(9.1%)*					
Forbearance=1 × Eligible=0	6,637					
% of sample	*(4.8%)*					
Forbearance=1 × Eligible=1	1,525					
% of sample	*(1.1%)*					

This table presents difference-in-differences estimates of regressions of the logarithm of monthly consumption and total assets, according to the model given in equation (1). In addition, we further interact these dummy variables with a *Eligible* indicator. Observations are at the household-calendar date level and the panel runs from January 2018 to September 2021. The counts presented on the bottom part of the table correspond to number of households, and the corresponding share over the total number of households in the sample. The dependent variable in columns (1)-(3) is measured as the logarithm of the sum between purchases and payments from either a debit or credit card at this bank; while in columns (4)-(6), the dependent variable is measured as the logarithm of the sum between end-of-the-month checking and saving accounts' balances. *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, for households in forbearance, the value of one at the beginning of the mortgage payment suspension and zero otherwise; and, for households outside forbearance, the value of one after March 2020 and zero otherwise. Finally, *Eligible* is a dummy variable that takes the value of one if the household satisfied the legal requirements for suspending mortgage payments. As a control, we include in some specifications the logarithm of total income, which includes wages, social security and retirement benefits. In some specifications, we include group-month-year fixed effects, with the group referring to different below/above median subgroups in relation to pre-pandemic assets and income (2019 averages). Standard errors in parentheses are computed using two-way clustering (household and month-year level). *, **, and *** indicate statistical significance at the 10%, 5%, and 1% level, respectively.

Table 7: Eligibility and Selection Groups: More Affected versus Less Affected

	Log(Consumption)			Log(Total Assets)		
	More Affected (1)	Less Affected (2)	Public Servants (3)	More Affected (4)	Less Affected (5)	Public Servants (6)
Forbearance=0 × Eligible=1 × Post	0.021*	-0.013	0.013	-0.029*	-0.009	-0.062**
	(0.012)	(0.010)	(0.017)	(0.016)	(0.016)	(0.025)
Forbearance=1 × Eligible=0 × Post	0.046**	0.112***	0.104***	0.046	0.076	-0.000
	(0.021)	(0.019)	(0.024)	(0.048)	(0.046)	(0.057)
Forbearance=1 × Eligible=1 × Post	0.051**	0.045	0.043	-0.055	-0.030	-0.014
	(0.023)	(0.030)	(0.045)	(0.048)	(0.058)	(0.148)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Household × Month FE	Yes	Yes	Yes	Yes	Yes	Yes
Group × Month × Year FE	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.591	0.602	0.588	0.848	0.861	0.868
Observations	1,001,430	1,767,870	1,719,855	1,001,430	1,767,870	1,719,855
Observations:						
Forbearance=0 × Eligible=0	14,645	32,297	35,241			
% of sample	*(65.8%)*	*(82.2%)*	*(92.2%)*			
Forbearance=0 × Eligible=1	5,642	5,009	1,775			
% of sample	*(25.4%)*	*(12.8%)*	*(4.6%)*			
Forbearance=1 × Eligible=0	1,029	1,509	1,096			
% of sample	*(4.6%)*	*(3.8%)*	*(2.9%)*			
Forbearance=1 × Eligible=1	938	471	107			
% of sample	*(4.2%)*	*(1.2%)*	*(0.3%)*			

This table presents difference-in-differences estimates of regressions of the logarithm of monthly consumption and total assets, according to the model given in equation (1). In addition, we further interact these dummy variables with a *Eligible* indicator. Observations are at the household-calendar date level and the panel runs from January 2018 to September 2021. The counts presented on the bottom part of the table correspond to number of households, and the corresponding share over the the total number of households in the sample. The dependent variable in columns (1) to (3) is measured as the logarithm of the sum between purchases and payments from either a debit or credit card at this bank; while from columns (4) to (6), the dependent variable is measured as the logarithm of the sum between end-of-the-month checking and saving accounts' balances. The *More Affected* subsample is defined by identifying households whose primary employer during the first quarter of 2020 operated in one of the more affected industries, measured by revenue growth from 2019 to 2020 and then split at the median. Conversely, the *Less Affected* subsample is defined by identifying households whose primary employer during the first quarter of 2020 operated in one of the less affected industries, defined by the same metric. Finally, the *Public Servants* subsample is defined by identifying the households whose primary employer operated in the public sector. *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, for households in forbearance, the value of one at the beginning of the mortgage payment suspension and zero otherwise; and, for households outside forbearance, the value of one after March 2020 and zero otherwise. Finally, *Eligible* is a dummy variable that takes the value of one if the household satisfied the legal requirements for suspending mortgage payments. As a control, we include in all specifications the logarithm of wages, social security and retirement benefits. In all specifications, we include group-month-year fixed effects, with the group referring to different below/above median subgroups in relation to pre-pandemic assets and income (2019 averages). Standard errors in parentheses are computed using two-way clustering (household and month-year level). *, **, and *** indicate statistical significance at the 10%, 5%, and 1% level, respectively.

**Table 8:** Effect of Forbearance Exit on Consumption and Savings

	Log(Consumption)		Log(Total Assets)	
	(1)	(2)	(3)	(4)
Forbearance ×				
Immediate Effect (1-3)	0.032**	0.031**	-0.175***	-0.174***
	(0.014)	(0.014)	(0.028)	(0.028)
Short-run Effect (4-12)	0.076***	0.075***	-0.006	-0.006
	(0.010)	(0.010)	(0.032)	(0.032)
Long-run Effect (>12)	0.081***	0.080***	0.128***	0.127***
	(0.011)	(0.011)	(0.026)	(0.026)
Exit Effect	0.019*	0.018*	-0.024	-0.025
	(0.011)	(0.011)	(0.031)	(0.031)
Immediate Effect (1-3) × Additional Relief		0.050		-0.263***
		(0.039)		(0.070)
Short-run Effect (4-12) × Additional Relief		0.071**		-0.335***
		(0.034)		(0.081)
Long-run Effect (>12) × Additional Relief		0.089**		-0.446***
		(0.042)		(0.091)
Exit Effect × Additional Relief		0.094**		-0.308***
		(0.038)		(0.092)
Controls	Yes	Yes	Yes	Yes
Household × Month FE	Yes	Yes	Yes	Yes
Group × Month × Year FE	Yes	Yes	Yes	Yes
R^2	0.568	0.567	0.843	0.843
Observations	7,386,120	7,417,602	7,386,120	7,417,602
Observations:				
Forbearance=1 × Eligible=0		6,637		
Forbearance=1 × Eligible=0 × Additional Relief=1		490		
% of group		(7.4%)		
Forbearance=1 × Eligible=1		1,525		
Forbearance=1 × Eligible=1 × Additional Relief=1		93		
% of group		(6.1%)		

This table presents difference-in-differences estimates of regressions of the logarithm of monthly consumption and savings, during and after the forbearance period, according to the model given in equation (3), but considering four time indicators as main explanatory variables: the *Immediate Effect* measures the impact over the first quarter after the start of forbearance; the *Short-run Effect* describes the average effect from the 4th to the 12th month; the *Long-run Effect*, measuring the average effect after one year and until the end of forbearance; and the *Exit Effect*, referring to the change after the end of the payment suspension. Observations are at the household-calendar date level and the panel runs from January 2018 to June 2022. The dependent variable in columns (1) and (2) is measured as the logarithm of the sum between purchases and payments from either a debit or credit card at this bank; while in columns (3) and (4), the dependent variable is measured as logarithm of the sum between end-of-the-month checking and saving accounts' balances. *Forbearance* is a dummy variable that takes the value of one for households receiving forbearance and zero otherwise; and *Additional Relief* is a dummy variable that takes a value of one if the household requested additional relief after the forbearance end. In some specifications, total income, which includes wages, social security, and retirement benefits, is included as a control. In all specifications, we include group-month-year fixed effects, with the group referring to different below/above median subgroups in relation to pre-pandemic assets and income (2019 averages). Standard errors in parentheses are computed using two-way clustering (household and month-year level). *, **, 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
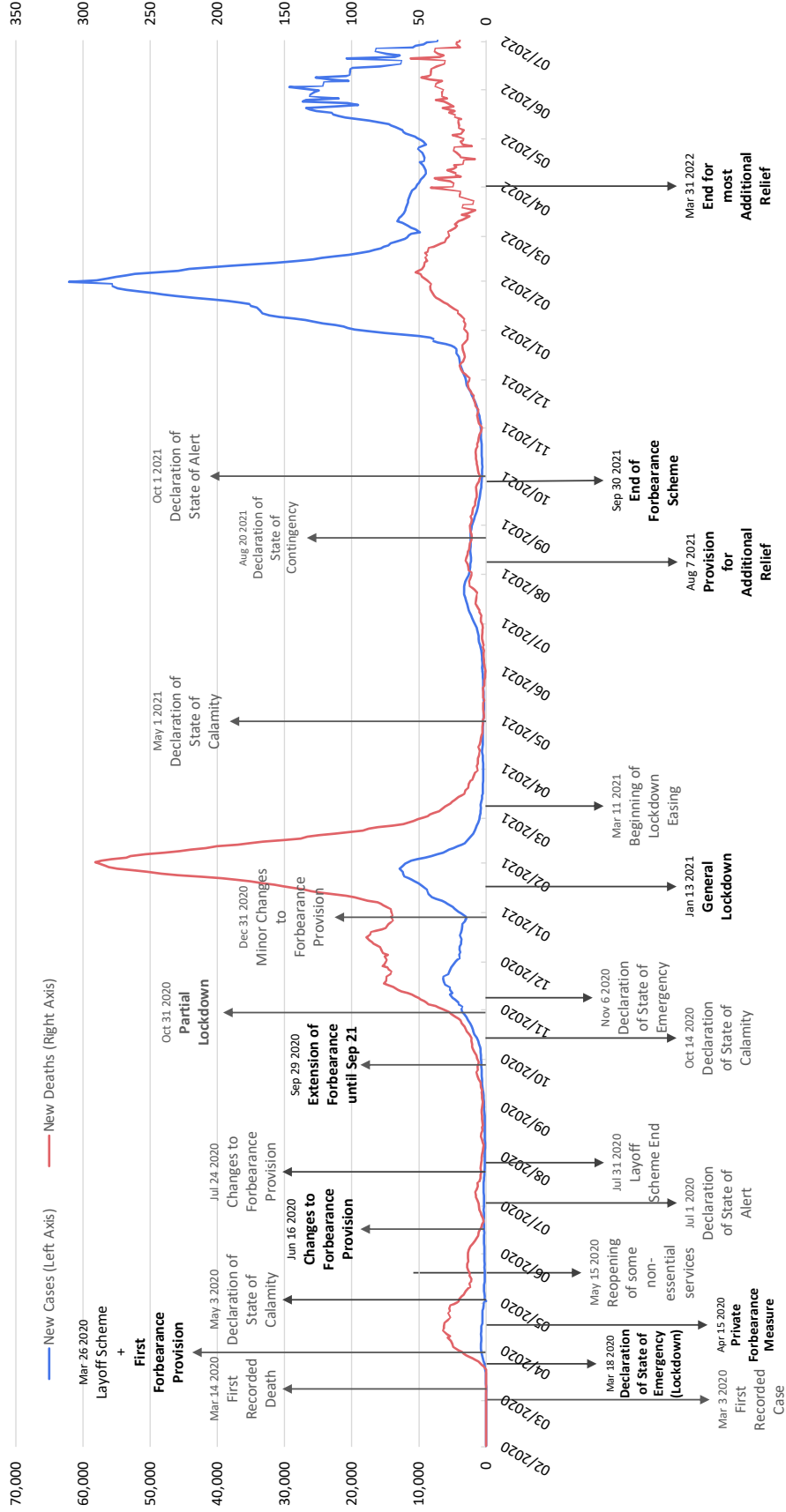
This figure shows the timeline of events, with key events being identified in bold, from February 2020 until July 2022. On the left axis and in blue we show the evolution of the number of new cases, while the right axis and in red we plot new deaths due to COVID-19.
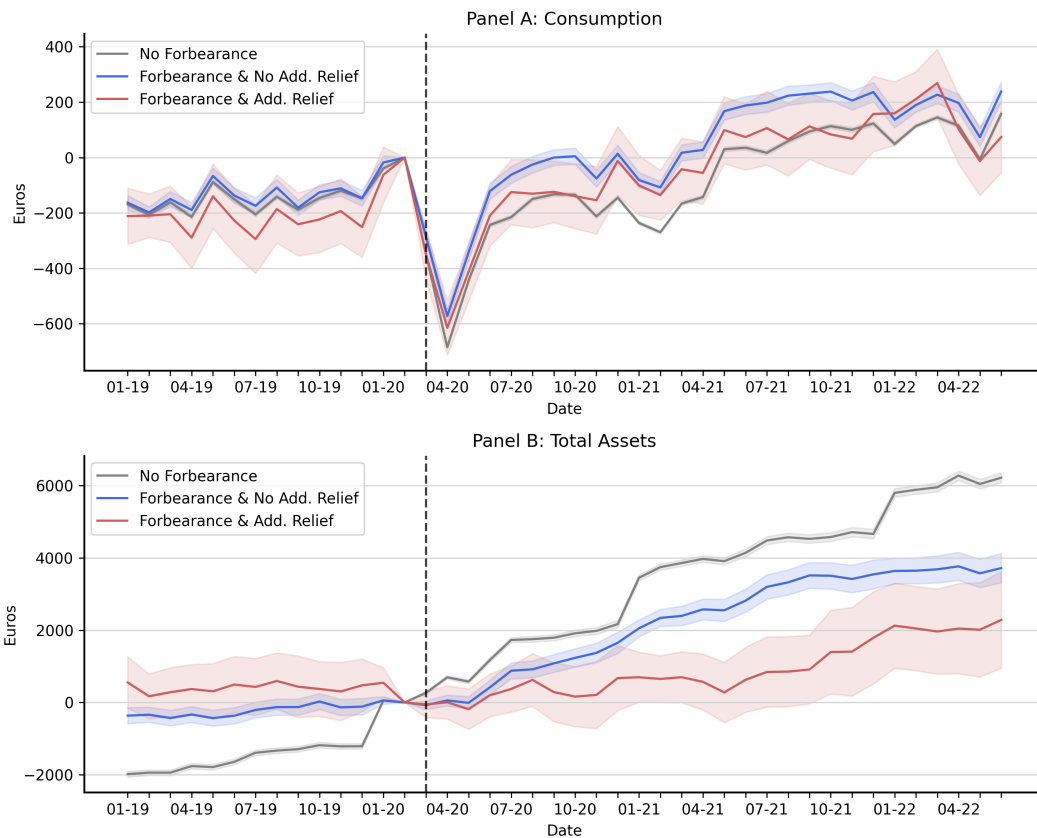
Figure IA.2: Evolution of Income
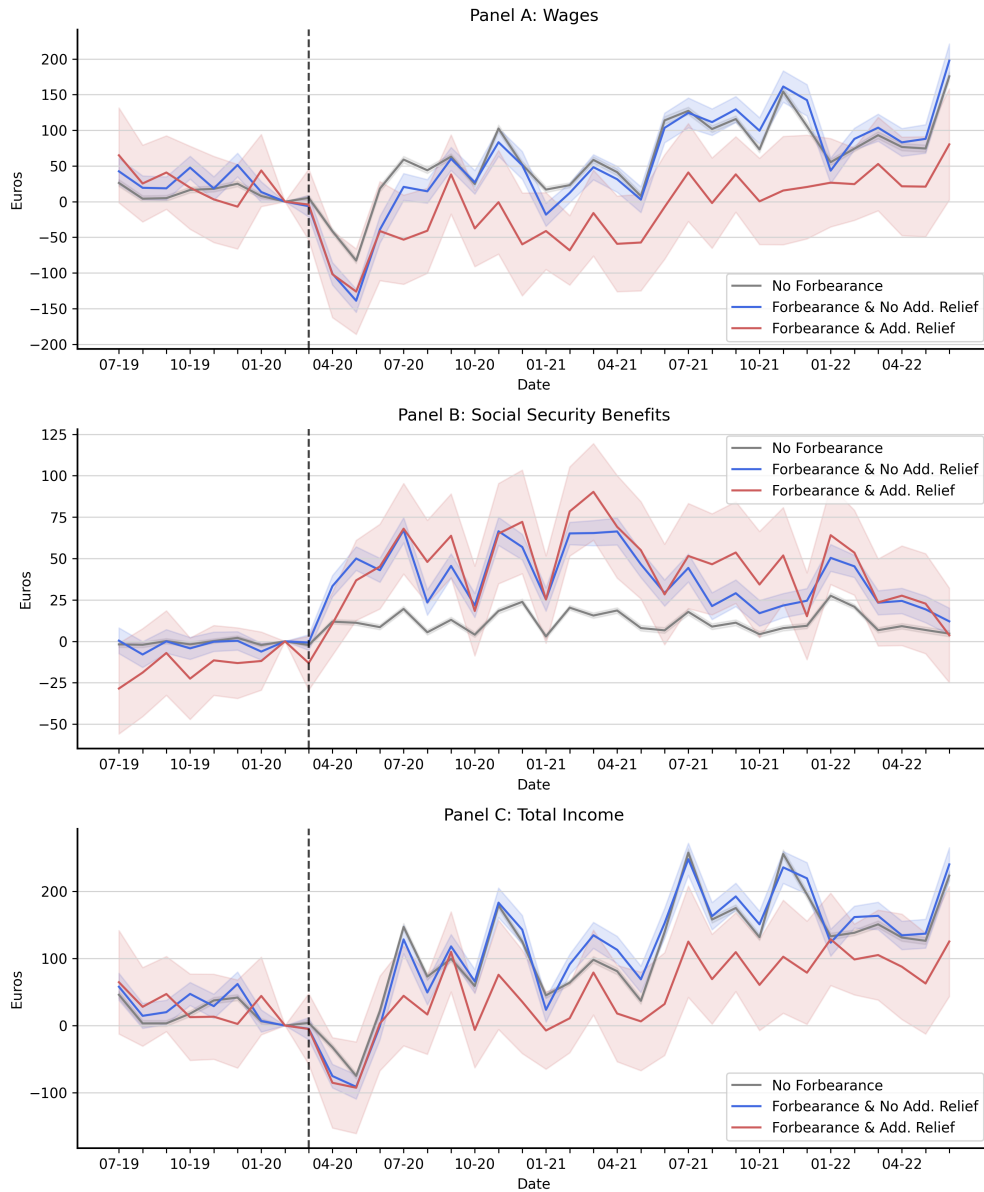
This figure plots the household average for different income sources, as well as the corresponding 95% confidence intervals, from July 2019 to June 2022. All measures are reported in euros, seasonally adjusted and relative to a pre-pandemic baseline (February 2020). Panel A reports the average change in monthly direct deposit of wages relative to the pre-pandemic baseline, while Panel B shows the change in social security benefits received. Panel C shows the change in total monthly income relative to the baseline, computed as the sum between monthly wages, social security, and retirement benefits. In all panels the average change is represented separately for households who received forbearance and requested an additional measure after its end (in red), those who received forbearance but did not request an additional relief (in blue) and those who never entered forbearance (in grey). Standard errors are clustered at the household level.

Figure IA.3: Evolution of Consumption and Assets
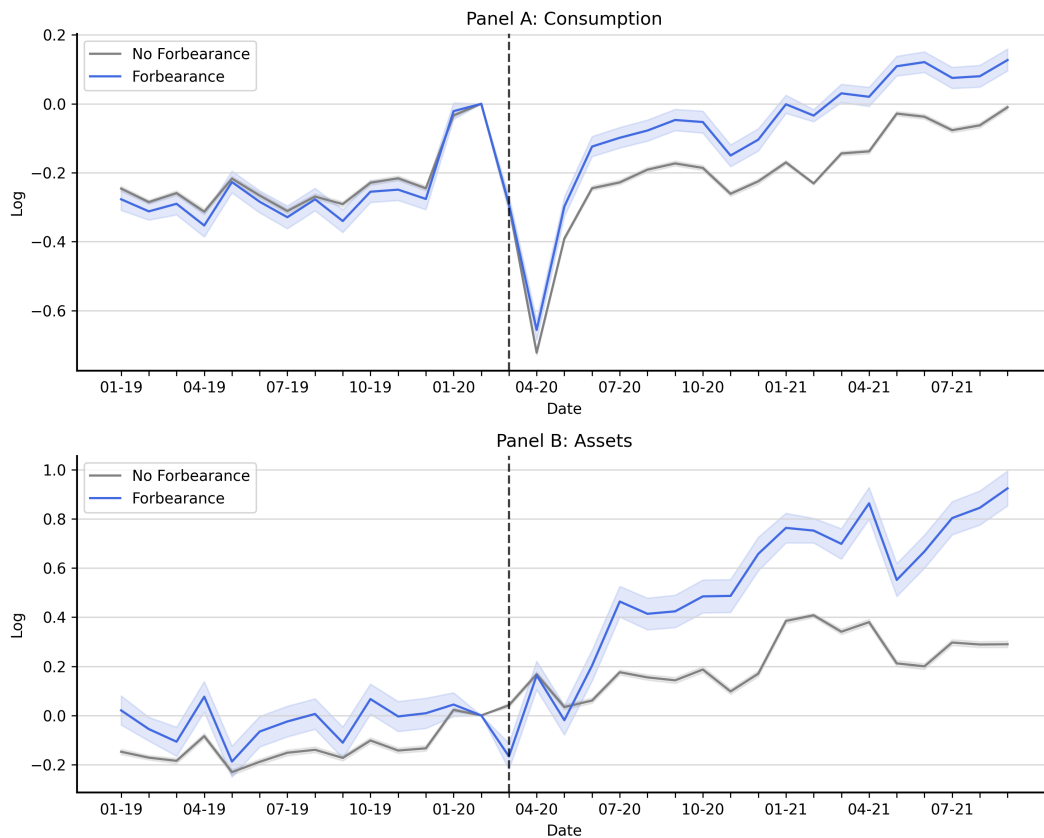
This figure plots the household average for monthly consumption and assets, as well as the corresponding 95% confidence interval, from January 2019 to September 2021. All measures are reported in logarithmic scale, seasonally adjusted and relative to a pre-pandemic baseline (February 2020), thus showing the average percentage change in consumption relative to the baseline. In Panel A, monthly consumption is computed as the logarithm of the sum between purchases and payments from either a debit or credit card at this bank. Panel B shows the logarithm of total assets, computed as the sum between the end-of-the-month checking saving accounts' balances. In both panels, the average percentage change is represented separately for households who received forbearance (in blue) and those who never entered forbearance (in grey). Standard errors are clustered at the household level.

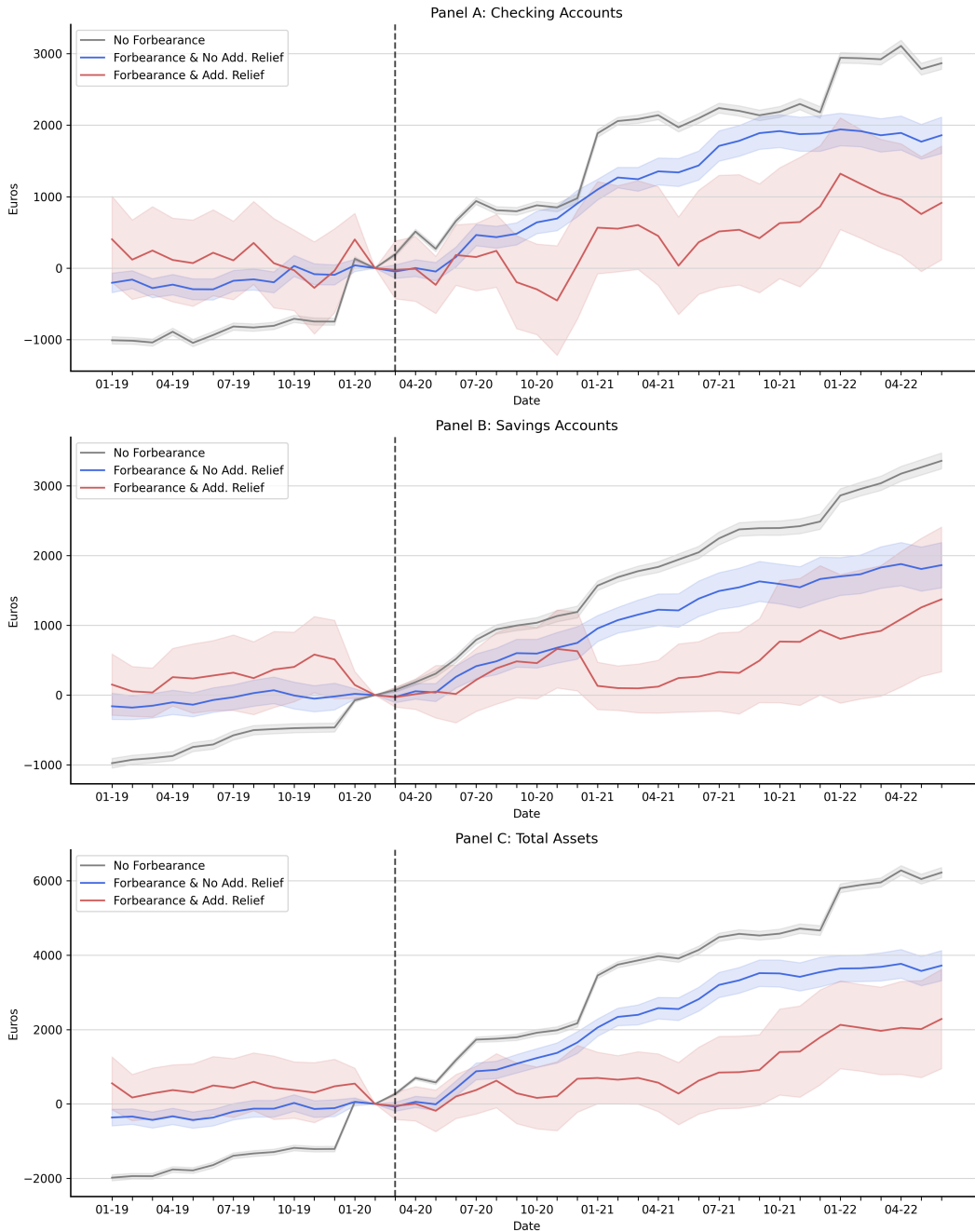**Figure IA.4:** Evolution of Consumption and Assets

This figure plots the household average for monthly consumption and assets, as well as the corresponding 95% confidence interval, from January 2019 to September 2021. All measures are reported in euros, seasonally adjusted and relative to a pre-pandemic baseline (February 2020). Panel A reports monthly consumption, computed as the sum between purchases and payments from either a debit or credit card at this bank. Panel B shows the evolution of total assets, computed as the sum between end-of-the-month checking and saving accounts' balances. In both panels the average change is represented separately for households who received forbearance and requested additional relief after its end (in red), those who received forbearance but did not request additional relief (in blue) and those who never entered forbearance (in grey). Standard errors are clustered at the household level.

Figure IA.5: Evolution of Assets

This figure plots the household average for different assets, as well as the corresponding 95% confidence intervals, from January 2019 to June 2022. All measures are reported in euros, seasonally adjusted and relative to a pre-pandemic baseline (February 2020). Panel A reports the average change in the end-of-the-month checking account balance relative to the pre-pandemic baseline, while Panel B shows the change in the end-of-the-month balance in saving accounts. Finally, Panel C shows the evolution of total assets, computed as the sum between end-of-the-month checking and saving accounts' balances. In both panels the average change is represented separately for households who received forbearance and requested additional relief after its end (in red), those who received forbearance but did not request additional relief (in blue) and those who never entered forbearance (in grey). Standard errors are clustered at the household level.

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
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
30 Day Delinquency	0.01	0.08	0.00	0.00	0.00	0.00	0.00	137,363

This table lists for each variable its mean, standard deviation, the 10%, 25%, 50%, 75% and 90% percentiles, and the number of households for which non-missing records exist. Statistics are computed on household averages over 2019. Income, assets, liabilities, and consumption measures are winsorized at the top and bottom 1% by date.

Table IA.2: Household Consumption Growth: Sample and Country Average

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

This table shows the in-sample annual growth rate of consumption for the average household and the corresponding statistic at the country level. In our sample, consumption is measured as the sum between purchases and payments from either a debit or credit card at this bank. For the country's average, we computed the yearly growth rate taking into consideration the average consumption by resident households, measured as the final consumption expenditure of resident households divided by the number of private households within the resident population. National accounts data are from INE.

Table IA.3: Effect of Forbearance on Credit Card and Overdraft

	Log(Credit Card & Overdraft)				Log(Other Banks' Debt)			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Forbearance × Post	-0.085***	-0.026	-0.025	-0.026	0.107***	0.081***	0.082***	0.088***
	(0.029)	(0.024)	(0.024)	(0.024)	(0.022)	(0.022)	(0.022)	(0.022)
Log(Total Income)			-0.012***	-0.012***			-0.005*	-0.005*
			(0.002)	(0.002)			(0.002)	(0.002)
Household × Month FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Month × Year FE	Yes	Yes	No	No	Yes	Yes	No	No
Group × Month × Year FE	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Location × Month × Year FE	No	No	No	Yes	No	No	No	Yes
R^2	0.793	0.794	0.794	0.794	0.879	0.879	0.879	0.880
Observations	6,181,335	6,181,335	6,181,335	6,181,335	4,532,979	4,532,979	4,532,979	4,532,979

This table presents difference-in-differences estimates of regressions of the logarithm of short-term liabilities held at this bank, and liabilities held at other banks, according to the model given by in equation (2), but using only a *Post* dummy variable instead of monthly indicators. Observations are at the household-calendar date level and the panel runs from January 2018 to September 2021, except for columns (5) to (8), where due to data limitations January 2019 was used as starting date. The dependent variable in columns (1) to (4) is measured as the logarithm of the end-of-the-month credit card and overdraft balances; while from columns (5) to (8) the dependent variable is measured as the logarithm of the sum of end-of-the-month balances for all liabilities found in the Credit Register held at other banks. *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 the value of one at the beginning of the mortgage payment suspension and zero otherwise. As a control, we include in some specifications the logarithm of total income, which includes wages, social security, and retirement benefits. In some specifications, we include group-month-year fixed effects, with the group referring to different below/above median subgroups in relation to pre-pandemic assets and income (2019 averages). Standard errors in parentheses are computed using two-way clustering (household and month-year level). *, **, and *** indicate statistical significance at the 10%, 5%, and 1% level, respectively.

Table IA.4: Effect of Forbearance on Credit Card and Overdraft by Time Horizon

	Log(Credit Card & Overdraft)			Log(Other Banks' Debt)		
	(1)	(2)	(3)	(4)	(5)	(6)
Forbearance $\times$						
Immediate Effect (1m-3m)	0.069**	0.082***	0.083***	0.157***	0.130***	0.131***
	(0.032)	(0.026)	(0.026)	(0.021)	(0.021)	(0.021)
Short-run Effect (4m-12m)	-0.087***	-0.028	-0.026	0.098***	0.076***	0.077***
	(0.030)	(0.024)	(0.024)	(0.022)	(0.022)	(0.022)
Long-run Effect (>12m)	-0.177***	-0.094***	-0.092***	0.101***	0.064**	0.065**
	(0.040)	(0.032)	(0.032)	(0.030)	(0.031)	(0.031)
Controls	No	No	Yes	No	No	Yes
Household $\times$ Month FE	Yes	Yes	Yes	Yes	Yes	Yes
Month-Year FE	Yes	No	No	Yes	No	No
Group $\times$ Month $\times$ Year FE	No	Yes	Yes	No	Yes	Yes
R^2	0.793	0.794	0.794	0.879	0.879	0.879
Observations	6,181,335	6,181,335	6,181,335	4,532,979	4,532,979	4,532,979

This table presents difference-in-differences estimates of regressions of the logarithm of short-term liabilities held at this bank, and liabilities held at other banks, according to the model given by in equation (1), but using three different time dummy variables instead of a *Post* indicator: the *Immediate Effect* measures the impact over the first quarter after the start of forbearance; the *Short-run Effect* describes the average effect from the 4$^{\text{th}}$ to the 12$^{\text{th}}$ month; and the *Long-run Effect*, measuring the average effect after one year. Observations are at the household-calendar date level and the panel runs from January 2018 to September 2021, except for columns (4) to (6), where due to data limitations January 2019 was used as starting date. The dependent variable in columns (1) to (3) is measured as the logarithm of the end-of-the-month credit card and overdraft balances; while from columns (4) to (6) the dependent variable is measured as the logarithm of the sum of end-of-the-month balances for all liabilities found in the Credit Register held at other banks. *Forbearance* is a dummy variable that takes the value of one for households receiving forbearance and zero otherwise. As a control, we include in some specifications the logarithm of total income, which includes wages, social security, and retirement benefits. In some specifications, we include group-month-year fixed effects, with the group referring to different below/above median subgroups in relation to pre-pandemic assets and income (2019 averages). Standard errors in parentheses are computed using two-way clustering (household and month-year level). *, **, and *** indicate statistical significance at the 10%, 5%, and 1% level, respectively.

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Table IA.5: Effect of Forbearance on Consumption per Category

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Forbearance ×									
Immediate Effect (1m-3m)	0.041	0.003	0.053*	0.019	-0.020	0.001	-0.004	-0.036	0.052***
	(0.053)	(0.061)	(0.030)	(0.052)	(0.044)	(0.057)	(0.070)	(0.073)	(0.011)
Short-run Effect (4m-12m)	0.104*	0.072	0.052**	0.069	0.067	0.077	0.090	0.041	0.049**
	(0.053)	(0.060)	(0.022)	(0.053)	(0.045)	(0.058)	(0.071)	(0.075)	(0.018)
Long-run Effect (>12m)	0.100*	0.081	0.045**	0.095*	0.073	0.104*	0.108	0.054	0.015
	(0.052)	(0.061)	(0.020)	(0.055)	(0.046)	(0.058)	(0.070)	(0.074)	(0.014)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Household FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Group × Month × Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.586	0.426	0.317	0.282	0.565	0.402	0.524	0.421	0.470
Observations	2,884,620	2,884,620	2,884,620	2,884,620	2,884,620	2,884,620	2,884,620	2,884,620	2,884,620

This table presents difference-in-differences estimates of regressions of the logarithm of monthly consumption during the forbearance period for different expenditure categories. Observations are at the household-calendar date level and the panel runs from January 2020 to September 2021. Each column shows a different consumption category as the dependent variable: (1) Groceries; (2) Clothing; (3) Housing Maintenance and Utilities; (4) Furniture; (5) Transport; (6) Health Care; (7) Restaurants; (8) Entertainment and Education; and (9) Miscellaneous Goods and Services. The logarithm of total income, which includes wages, social security, and retirement benefits, is included as a control in all specifications. In all specifications we include household and group-month-year fixed effects, with the group referring to different below/above median subgroups in relation to pre-pandemic assets and income (2019 averages). Standard errors in parentheses are computed using two-way clustering (household and month-year level). *, **, and *** indicate statistical significance at the 10%, 5%, and 1% level, respectively.