The Heterogeneous Effects of Household Debt Relief*

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ABSTRACT

Large-scale debt forbearance is an important policy tool during periods of economic distress, but effectively targeting such a policy is hindered by information asymmetries. Using transaction-level data from a leading Portuguese bank, we study a widely accessible forbearance program during the COVID-19 pandemic. We find that financially fragile households are more likely to enter forbearance regardless of their income shock. Suspending mortgage payments positively and persistently affects consumption and savings, though effects vary across households. Specifically, low-wealth and low-income households have a greater propensity to consume out of postponed payments, while high-wealth and high-income households have a greater propensity to save. Additionally, households eligible for forbearance avoid a consumption decline, whereas ineligible households who nonetheless access forbearance increase their consumption. Our findings highlight the importance of incorporating observable household characteristics in designing effective debt relief policies.

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Keywords: Household finance, Debt forbearance, Mortgages, Consumption, Savings, Income, Default, COVID-19 pandemic

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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)), 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 slower and document-intensive approach that targets only "truly" distressed households (i.e., minimizes type I error) versus a quick-to-implement and catch-all approach that reaches most households (i.e., minimizes type II error). The U.S. government took the former 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 any form of debt relief (Noel (2021)). The failure to provide assistance 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 (March-April 2020), in stark contrast with the 2007-2009 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 financial distress and defaults. In Portugal, eligibility for debt forbearance in the form of a temporary 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 period.[1] The focus on households suffering financial hardship as a result of the pandemic (as opposed to households with low income, low wealth, or high debt burden entering the crisis) mirrors

[1]Households were also eligible if one of the household members was infected with COVID, but this represented a very small 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).

the design adopted by other countries in the European Union (see, e.g., European Banking Authority (2020)), as well as the CARES Act in the United States.[2] As of June 2020, €17 billion mortgages were on repayment moratoria in Portugal, comprising about 18% of mortgages.[3]

This paper uses a new microdata panel from a leading Portuguese bank to investigate both the selection into forbearance and its effect on household consumption and savings. The data include transaction-level information from checking and savings accounts, as well as credit and debit cards in the 2018-2022 period. We study two sequential waves of 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 use household-level income, consumption, and balance sheet data to examine who accesses large-scale forbearance programs and how forbearance affects individual consumption, savings, and unsecured debt. In addition, our setting is unique as it allows us to observe the consumption and savings response of eligible households and ineligible households (who still obtained access) according to the forbearance program rules, before and after the program.[4]

We identify the effect of forbearance on consumption, savings, and unsecured debt 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 performing the comparisons within time-

[2]In the United States, the CARES Act was targeted at borrowers experiencing "financial hardship," but the program did not require any proof or documentation of hardship (National Credit Union Administration (2020)). The approach was similar across other countries in the European Union.

[3]Portugal was among the top three countries in Europe with the highest share of mortgages on repayment moratoria. According to the European Banking Authority (EBA), €365 billion in household loans (€268 billion of which were mortgages) entered moratoria in the Euro area by June 2020, about 7% of household loans (Nicolaou (2020)). 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)).

[4]Previous studies have considered the characteristics of borrowers accessing forbearance during the COVID pandemic, including differences in credit scores, race, and income levels, as well as the effects on delinquencies and debt usage(see Yannelis and Amato (2023) for a survey). Ganong and Noel (2020) study the consumption response to mortgage modifications following HAMP using end-of-month credit card balances and payments to calculate monthly expenditures. Albuquerque and Varadi (2022) consider the consumption response around the U.K. mortgage moratoria during the pandemic using data from an online personal budgeting application.

varying income-by-wealth groups; and (3) the validation that household consumption, savings, and unsecured debt were following parallel trends before the forbearance. We are interested not only in the changes in behavior due to the program but also in the selection of households into forbearance and potential heterogeneous treatment effects, a key input into the design of such programs. Thus, our estimated average effects should be interpreted as the effect of forbearance on the set of households who choose to suspend debt payments relative to otherwise similar households who choose not to suspend payments. We then consider how selection on unobservables is likely to affect the direction and magnitude of the effects and separately identify them for eligible and ineligible groups according to the program rules.

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

Despite the drop in wages, the households in forbearance increased consumption relative to those 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 forbearance program in September 2021. In addition, we find that households in forbearance increased total deposits (checking and savings accounts) in the long run, with a differential increase of about 11% over one year after the suspension of debt payments. In addition, households in forbearance decreased unsecured debt (overdraft and credit card balance) by about 11% in the long run, while they increased debt at other banks by about 5%.

We also obtain estimates of the change in consumption and savings per euro of postponed debt payments, i.e., the marginal propensities to consume and save. The estimated marginal propensity to consume for households in forbearance is about 20 cents, and the marginal propensity to save is similar. These average estimates mask substantial heterogeneity by

income, wealth (as proxied by total deposits), and indebtedness (as proxied by the DTI ratio). We find that households with below-median wealth increased consumption by about 30 cents per euro of postponed payments after the start of the forbearance. In contrast, there was a smaller (10 cents) increase in spending per euro of forbearance among households with above-median wealth. A similar picture emerges when we split households by median income or DTI 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 temporary shocks to household income (e.g., Hall and Mishkin (1982), Johnson, Parker, and Souleles (2006), and Di Maggio, Kermani, Keys, Piskorski, Ramcharan, Seru, and Yao (2017)). Ganong, Jones, Noel, Greig, Farrell, and Wheat (2020) also 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 below-median wealth group only saved about 15 cents per dollar out of postponed repayment, the above-median wealth group saved as much as 30 cents per euro after the start of the forbearance. We find qualitatively similar results when we split households by median income or DTI 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 applied for forbearance.

The response to forbearance was also heterogeneous based on eligibility under the program rules. Using our data, we construct individual proxies of legal eligibility for the program. Our data allow us to measure variation in wages and other sources of income and identify the sectors individuals work in, which are the two main criteria for eligibility. We estimate that about 10% of households in our sample were eligible for the program, but enrollment in the program deviated substantially from formal eligibility. First, and perhaps surprisingly, most households (90%) eligible for forbearance chose not to access the program. At the same time, 80% of households who suspended payments were not formally eligible for it. This is consistent with (intentionally) loose screening on the part of banks and a bias toward offering forbearance to households who asked for it. This means that the program missed many target

households but reached many other households who were not the intended recipients of the program. Ineligible households who entered forbearance were, on average, the most fragile group before the pandemic, even though we do not find that their income was the most affected by the shock itself.

When we separate the average consumption effect into different groups based on eligibility, we find that the increase in consumption is mainly driven by ineligible households who ask for forbearance. When we compare the effect of forbearance across groups, we find that forbearance is associated with 4% higher consumption for eligible households, but it is associated with a larger consumption effect of 8% for ineligible ones. This 4 percentage point difference between the two coefficients suggests that differences in who selects large-scale forbearance programs (due to easy access) materially affect the estimated average forbearance. In addition, eligible households who chose not to access forbearance suffered a drop in consumption. We find that savings by ineligible households in forbearance also grow more than those of eligible ones.

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 relief 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 fraction 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 government forbearance program and faced larger wage drops but still increased consumption during that period. In addition, ineligible households for the government moratorium were more likely to request additional measures (7.4% vs. 6.1%). The increase in consumption relative to the

pre-pandemic period persisted after the start of the additional measures but to a lower extent. In contrast, the borrowers who exited forbearance in September 2021 had built up additional savings during the government moratorium.

Taken together, our results provide new insights for the design of large-scale debt relief programs. Optimal program design should incorporate how households who strategically enter the program save and consume the additional resources they obtain. We show that households who were ineligible for the program show a large increase in consumption, implying that they will have a higher debt burden after forbearance (due to postponed principal and interest payments) which is not offset by an increase in total deposits. This means that the program has a potentially unintended consequence of producing more indebted households who were not the intended recipients of the program.

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.[5] At the same time, lenders may find it optimal to perform principal reductions to reduce the incentive for borrowers to default. The most often cited concern about providing blanket debt relief to households is strategic behavior, i.e., that "too many" households will request help, even though most do not need assistance to remain current on their debts. Recent work 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) who find that borrower delinquency rates increase

[5]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.

when Countrywide is forced by court decision to offer more generous modification terms.

The consumption and delinquency effects of the policies during the Great Recession in the post-2008 period are already well documented (see, among many others, Agarwal, Amromin, Chomsisengphet, Landvoigt, Piskorski, Seru, and Yao (2015), Agarwal, Amromin, Ben-David, Chomsisengphet, Piskorski, and Seru (2017), Ganong and Noel (2020)), Abel and Fuster (2021)). 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 low delinquencies in the United States. Hong and Lucas (2023) show that the credit policies implemented during the pandemic were an important source of incremental resources for households, in addition to governments' fiscal response. The reduction in delinquency rates was higher among low-income and minority individuals (Gerardi, Lambie-Hanson, Willen, et al. (2021), An, Cordell, Geng, and Lee (2022), Shi (2022)), but financial intermediary frictions may have prevented some borrowers from receiving forbearance (Cherry, Jiang, Matvos, Piskorski, and Seru (2022), Kim, Lee, Scharlemann, and Vickery (2022)).[6]

Our paper is also related to the literature on the consumption response to the COVID-19 pandemic. This literature focuses on the effects of (one-time or repeated) transfers rather than debt forbearance. Baker, Farrokhnia, Meyer, Pagel, and Yannelis (2020) studies the consumption response of households at different income levels and with different shocks to income around the onset of the pandemic and as a function of shelter-in-place orders. Ganong, Greig, Noel, Sullivan, and Vavra (2022) show that unemployment benefits introduced at the height of the crisis had a large impact on spending but a small impact on employment. 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)).[7]

[6]Debt forbearance can also have positive local spillovers (Wang, Yang, Iverson, and Kluender (2020), Capponi, Jia, and Rios (2021)). In addition, 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 Dinerstein, Yannelis, and Chen (2022), (Catherine and Yannelis (2023)).

[7]Intermediary frictions shaped the implementation of the CARES Act-driven debt relief during the pandemic. Cherry, Jiang, Matvos, Piskorski, and Seru (2021) and Kim, Lee, Scharlemann, and Vickery

Our paper provides new insights into the dynamics of household income, consumption, savings, and unsecured debt before and after large-scale debt relief programs. Understanding who selects debt forbearance and the heterogeneous impact of policy choices on observably different groups is an important step to better designing 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.[8] 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.[9] By then, the measure's scope was restrictive, only including mortgage loans for acquiring 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 those not having payments 90 days past due) nor had outstanding tax or social security liabilities. Moreover, access was limited to individuals: (1) 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); (2) working in companies that reduced work hours due to the pandemic and requested paycheck assistance (the "layoff" regime); (3) unemployed; (4) eligible for financial support for self-employed; or (5) individuals working in industries more affected by the COVID-19 lockdowns as defined in the government

(2024) show that shadow banks provided mortgage forbearance at lower rates than banks, while Cherry, Jiang, Matvos, Piskorski, and Seru (2022) show higher forbearance provision among better-capitalized shadow banks. Research on other pandemic relief programs also finds variation in outcomes across financial intermediaries (e.g., Granja, Makridis, Yannelis, and Zwick (2022)).

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

[9] Borrowers would restart making higher payments at the end of moratoria due to unpaid interest. The number of payments would not change, so the loan maturity was effectively extended.

legislation. If individuals satisfied one of these criteria, they could request a suspension 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.

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

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

3 Data

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

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

Our measure of consumption includes any purchases or payments using debit or credit cards using data from point-of-sale transactions and cash withdrawals.[13] We complement

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

[11] On average, households in our sample made 39 monthly transactions, and the median is 35.

[12] Out of the 137,363 households, we find a valid employer match for about 100,000 of them. The remaining households include about 20,000 non-employed households (i.e., unemployed and retired), and about 17,000 employed individuals with an unmatched employer. To achieve this, we consider the name of the entity ordering the Single Euro Payments Area (SEPA) transfer and then use the Levenshtein Distance string metric to match the employer with the universe of firms operating in Portugal. Firm names and industry codes are drawn from SABI (Iberian Balance Sheet Analysis System).

[13] 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

the data with automatic payments of utilities and other services. Our transaction-level data bypasses the concerns of using annual household wealth snapshots to calculate imputed consumption discussed in Baker, Kueng, Meyer, and Pagel (2022). Although we have an almost complete picture of the activity at the bank, we do not observe outbound transfers to other banks or their intended recipients. This could represent additional savings (if a transfer goes to accounts at other banks owned by the household itself) or additional consumption (purchases of goods and services paid for by bank transfer rather than a card, cash, or automatic withdrawal).

We are able to classify purchases by category starting in January 2020. We categorize transactions by relying on point-of-sale terminal information, namely the reported Merchant Category Code (MCC). This classifies merchants into categories based on the type of business and the reported industry code according to *Classificação das Actividades Económicas* (CAE) Revision 3.

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

We merge the internal information of the bank with data from the Credit Register (*Central de Responsabilidades de Crédito*) managed by Bank of 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.

Our data allow us to determine which households applied for and received forbearance

Central Bank, 64% of in-person retail transactions are done in cash in Portugal, as opposed to 31% by card and 5% by other means.

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

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

3.1 Summary Statistics

Table 1 presents pre-pandemic (as of December 2019) averages of variables for our sample of households, separately for households in and outside forbearance and eligible and ineligible households. This allows us to examine whether selection on observable characteristics plays an important role in applications for the government forbearance program at the beginning of the pandemic. Table IA.1 in the Internet Appendix presents detailed summary statistics for our full sample. Households in our sample comprised 1.7 mortgagors on average, with negligible differences between those who got forbearance and those who did not. The average household monthly total income in our full sample is €2,527, which is significantly higher than the average in the country, which in 2019 amounted to €1,800.[16] The 90th percentile of total income is €4,659 per month.[17]

The average wage is higher for households outside of forbearance, particularly in the case of eligible households. Total income, including pensions (for 40,000 of the 137,363 households in the sample), social security benefits, and other inbound transfers (i.e., rents, business or professional income) follow a similar pattern. For instance, the average total income for eligible

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

[16]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*).

[17]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).

households outside forbearance is substantially higher at €2,759 per month than that of eligible households in forbearance at €2,449. In addition, ineligible households in forbearance have the lowest average wage and total income at €1,416 and €2,046, respectively.

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

Households 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, holding an average balance of about €420. In contrast, only about 1% of households in our sample hold student or auto loans, and 7% hold other types of loans, such as personal loans. Finally, most households in our sample have loans with other banks, with a balance of about €7,500 on average.

The average total loan payment (mostly mortgage payments), including principal and interest, is €315 per month, higher than the country's average by the end of 2019 (€248).[19] Moreover, households in forbearance have higher loan payment commitments (€377) than households outside forbearance (€311). By entering forbearance, the average household postpones €345 per month. Considering total income, we estimate an average debt

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

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

payment-to-income (DTI) of about 19% in 2019, slightly above the country's average in 2022 (17%).[20] As expected, the average DTI is higher for households in forbearance, in particular for ineligible households at 32%. Debt delinquency is infrequent in our sample, with just 1% of households having payments more than 30 days past due. We conclude that the ineligible households in forbearance are the most fragile group with lower income, consumption, savings, and higher DTI.

4 Empirical Methodology

We estimate a difference-in-differences regression to compare consumption, total deposits, and unsecured debt between households in forbearance and households outside forbearance around the start of the debt forbearance program:

$$log(y_{i,t}) = \beta Forbearance_i \times Post_t + \lambda X_{i,t} + \mu_{i,m} + \mu_{g,t} + \varepsilon_{i,t}, \tag{1}$$

where $y_{i,t}$ is either the monthly consumption expenditures, total deposits (i.e., the end-of-month checking and savings accounts balances), or unsecured debt (i.e., the end-of-month credit card and overdraft balances and debt at other banks) for household i at time t. $Forbearance_i$ is a dummy variable that takes the value of one for households receiving forbearance and zero otherwise. *Post* is a dummy variable that takes a value of one for the period starting when mortgage payments are suspended for a particular household and zero otherwise.[21] The coefficient of interest is β, which captures whether forbearance is associated with a differential effect on consumption, total deposits or unsecured debt around the program's start. The sample period is between January 2018 and September 2021.

We also estimate the effect at different horizons by replacing the *Post* variable with three dummy variables: (1) *Immediate Effect* for months 1 through 3 after a household enters

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

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

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, including 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 seasonal effects. All the regressions include month-year fixed effect (μ_t) to absorb shocks that may affect all households in a given period or group-by-month-year fixed effects ($\mu_{g,t}$) to absorb time-varying shocks for households by quartile of pre-pandemic deposits and income (2019 averages). Thus, the regressions include a total of 16 group indicators (four quartiles of income by four quartiles of total bank deposits) interacted with dummy variables for each month-year. We also include municipality-by-month-year fixed effects in some specifications to account for regional shocks. Standard errors are clustered two-way at the household and month-year levels.

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

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

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

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

$$y_{i,t} = \alpha Forbearance\ Amount_i \times Post_t + \lambda X_{i,t} + \mu_{i,m} + \mu_{g,t} + \varepsilon_{i,t}, \quad (3)$$

where $y_{i,t}$ is either the monthly consumption expenditures or the change in total deposits 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. The coefficient of interest is α, 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, Savings, and Unsecured Debt

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

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

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

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

Figure IA.2 of the Internet Appendix shows the evolution of wages, social security benefits, and total income separately for households receiving forbearance who meet the eligibility criteria and for those who do not meet. We find that the large drop in wages for households

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

Figure 2 shows the evolution of the average consumption and total deposits 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 was likely due to a combination of demand and supply factors. However, we find a positive and statistically significant difference in consumption for households in forbearance relative to households outside forbearance of about €200 per month by the summer of 2020. Interestingly, 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 income was lower for households in forbearance (see Table 1 and Figure 1).[22]

Figure IA.3 of the Internet Appendix shows the evolution of consumption and total deposits separately for eligible and ineligible households in forbearance. We find that the gap in consumption that emerges for households in forbearance is driven by ineligible households, whereas the consumption of eligible households in forbearance mostly tracks that of those outside of forbearance.

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

[22]The figure shows 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.

This group did so more quickly than the households outside forbearance when considering their lower average level of total deposits.

Figure IA.4 in the Internet Appendix shows the evolution of the logarithm of consumption and total deposits. The effect is immediate after the start of forbearance, 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.

5.1 Response of Consumption, Savings, and Unsecured Debt to Forbearance

We now estimate the effect of the debt forbearance program on household consumption, total deposits, and unsecured debt. Table 2 shows the estimates 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 estimates for the effect on total deposits. Columns (1) and (5) include month-year and household-month fixed effects. Columns (2), (3), (6), and (7) include the 16 different household group indicators using quartiles of income and total deposits interacted with month-year fixed effects. Columns (4) and (8) also include municipality-by-month-year fixed effects. Additionally, in columns (3), (4), (7), and (8), we control for changes in total income.

Column (1) shows that the forbearance effect on consumption is positive and significant at 17%. This effect represents about €222 at the average consumption of the households in the forbearance group. Columns (2)-(4), including household group/by-month-year fixed effects and total income as control, show a lower effect on consumption at about 8% but still positive and significant. Column (5) shows a positive and significant forbearance effect on total deposits at 20%, but this effect is insignificant in columns (6)-(8) when we control for household group-by-month-year fixed effects. As expected, total income has a positive and significant coefficient in both the consumption and deposits regressions.

5.2 Response at Different Horizons

Next, we examine the response to the suspension of debt payments at different horizons after forbearance. We estimate the regression in equation (1), replacing the *Post* dummy variable with three dummy variables: *Immediate Effect* (month 1 through month 3); *Short-run Effect* (month 4 through month 12); and *Long-run Effect* (after month 12). Table 3 presents 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 horizons. In columns (2) and (3), including household group-by-month-year fixed effects and total income as controls, the response increases from 3% (immediate effect) to 9% in the short and long run. Columns (4)-(6) present the estimates of the total deposits response at different horizons. The deposits 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) and (6), including household group fixed effects, show a positive and significant forbearance effect on total deposits at about 11-12% in the long run.[23]

Figure 3 shows how consumption (Panel A) and total deposits (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 β_τ coefficients each month obtained from the regression in equation (2). The coefficients measure the difference between households in forbearance and households outside forbearance relative to the month before the start of the forbearance. Despite the significant average differences in the characteristics of the two groups, Panel A shows similar trends in consumption in the period before the start of the forbearance. In addition, we find a positive and significant forbearance effect immediately after the start of the forbearance. The effect is persistent and increasing with the horizon.

Panel B shows the total deposits response around forbearance. We find a positive and

[23]Table IA.3 in the Internet Appendix shows that the estimates of the consumption and deposits response to forbearance are similar when we restrict the sample to individuals working in the private sector (i.e., we exclude public servants and other individuals working in public entities and state-owned firms). This sample includes households that are likely to have been more affected by the pandemic.

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

Table 4 shows the coefficient estimates for two additional outcomes: credit card and overdraftand the total amount of debt held at other banks (end-of-the-month balances). Columns (1)-(3) show that households in forbearance initially increased balances by 7-8% on credit cards and overdrafts at the onset of the pandemic but then reduced those balances by a negative and significant 11% over one year after the forbearance when we include household group fixed effects in columns (2) and (3). These results are consistent with a sudden and negative liquidity shock affecting households in forbearance, who, in the long run, were also able to repay part of their short-term liabilities. Columns (4)-(6) suggest there was an increase in other banks' liabilities over the short-run at about 13% (immediate effect) when we include household group fixed effects. This response is attenuated in the long run, with the differential increase stabilizing at about 5% (long-run effect). These findings are consistent with households using the liquidity of forbearance to reduce their exposure to unsecured debt. In addition, households seem to strategically prefer to draw unsecured debt from a bank different from the one where they had the mortgage and forbearance.

5.3 Spending Categories

Our bank transaction-level data include merchant codes for debit and credit card transactions, which allow us to categorize most of the consumption expenditures. Table 5 shows the estimates of the average forbearance effect on consumption by category in each column. The sample period is from January 2020 to September 2021 (merchant codes are not available in 2018 and 2019). All regressions include household-month fixed effects and household group-by-month-year fixed effects, as well as total income as a control.

Column (1) shows that estimates of the total consumption response to forbearance are similar when we use the sample for which we can categorize consumption expenditures. During the first year after the forbearance start (immediate and short-run effects), the main drivers of consumption response to forbearance are "Groceries", "Clothing", "Restaurants", and "Entertainment and Education" expenditures with a positive and significant effect between 17% and 18%. Over the long term (beyond one year), the effect remains positive and significant across all categories. However, there is heterogeneity in the magnitude of the effect. The effect is the strongest for "Groceries", "Clothing", "Health Care", "Restaurants" and "Entertainment and Education" at 16-20%, while it is the weakest for 'Housing Maintenance and Utilities" at 7%.

5.4 Marginal Propensity to Consume and Save

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

Table 6 shows the estimates of the marginal propensity to consume and save out of the postponed debt payments. The regressions include the same fixed effects and controls as in Table 2. We find that the average forbearance effect on consumption is significant across all specifications. The marginal propensity to consume is about 30 cents per euro of postponed debt payments in column (1). When we control for total income and household group-by-month-year fixed effects, this estimate drops to about 20 cents. In addition, we estimate the marginal propensity to save with households saving as much as 16 to 20 cents per euro of postponed payments in columns (6) and (7) when we control for changes in total income and household group-by-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 7, column (1), shows

that the immediate effect on the marginal propensity to consume is significant at about 17 cents per euro of postponed payments. However, this estimate becomes insignificant when we control for household group-by-month-year fixed effect and total income in columns (2) and (3). In other words, consumption did not respond to the amount of forbearance during the initial months of the pandemic with this set of controls. The short-run (4-12 month) marginal propensity to consume (MPC) out of forbearance is significant across all specifications at 19 to 31 cents, while the long-term MPC is even stronger at 30 to 37 cents. We also find that households save as much as 12 cents per euro of postponed payments in the short run, increasing to about 15 cents over the long run in column (1). After controlling for group-by-month-year fixed effects and total income in column (6), the savings response becomes similar over the three horizons: 13 cents (immediate effect), 22 cents (short-term effect), and 19 cents (long-term effect).[24] Overall, the results suggest that households consumed and saved a significant fraction of the postponed payments.

5.5 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 financial 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. We split households at the median of pre-pandemic total deposits (wealth), income, and debt payment-to-income (DTI) ratio (indebtedness). Figure 4 shows the effects on the average propensity to consume and save for these groups separately.

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

[24] As we point out in Section 3, we do not observe outbound transfers to other bank accounts, which means our marginal propensity to consume and marginal propensity to save coefficients do not add to one.

10 cents for the high-deposits group. In Panel B, we show that the consumption response is stronger for low-income households than for high-income households, with a similar magnitude to Panel A. Finally, Panel C shows a stronger consumption response for households with a high DTI ratio than a low DTI ratio.

Panels D-F show that the response to debt forbearance differs across groups when we study the marginal propensity to save. Panel D shows that high-deposits households save almost double per euro terms relative to the low-deposits group. Panels E and F also show stronger savings responses for the high-income group versus the low-income group and for the low-DTI group versus the high-DTI group.

In sum, there are 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, 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 estimate the effect of forbearance on the logarithm of consumption and total deposits using the regression in equation (2) but are now interested in four groups of households according to forbearance and eligibility status: (1) ineligible households outside forbearance (the omitted group); (2) eligible households outside forbearance; (3) ineligible households in forbearance; and (4) eligible households in forbearance. Specifically, we define a *Eligible* dummy variable that takes the value of one for households eligible to the forbearance program according to program rules and zero otherwise (see Section 3 for details on the eligibility definition). We then interact the *Eligible* dummy variable with the *Forbearance* and the *Post* dummy variables. For households in forbearance, *Post* takes the value of one when mortgage

payments are suspended (between April and June 2020) thereafter and zero otherwise. For households outside forbearance, *Post* takes the value of one in April 2020 thereafter and zero otherwise.

6.0.1 Number of Eligible and Ineligible Households

Table 8 reports the number of observations 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 program's implementation on the part of banks erred on the side of giving households access to forbearance. In fact, 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 according to program rules. This is consistent with the lack of formal checks of the eligibility criteria during the pandemic, as well as bank officers not being incentivized to screen out debt forbearance applications.[25]

As discussed in Section 3.1, households in and outside forbearance and eligible and ineligible households are different based on their observable characteristics. Ineligible individuals in forbearance have the lowest total income, consumption, and total deposits among the four groups, and the highest debt payment-to-income ratio. Thus, this group was already the most fragile when the pandemic crisis hit.

6.1 Heterogeneus Effects by Eligibility

Table 8 shows that there is substantial heterogeneity in the effects of forbearance on consumption among the four groups. We find that eligible households outside forbearance consume significantly less after the start of the forbearance period relative to ineligible

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

individuals also outside of forbearance (the omitted group in these regressions, which corresponds to 85% of the sample). This difference is about -2.6% when we include household group-by-month-year fixed effects and total income 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. These results indicate that debt forbearance was just enough to compensate for the pandemic shock suffered by eligible households in forbearance. In contrast, ineligible households who nonetheless 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 applied for and received forbearance.

Columns (5) and (6) of Table 8 show that ineligible households in forbearance saved 5% more than ineligible households outside of forbearance but the coefficient is imprecisely estimated. At the same time, forbearance is not associated with higher deposits for the eligible group: deposits drop by about 4% for eligible households outside forbearance and about 6% for eligible households in forbearance (relative to the omitted group) in all specifications. This modest savings response is consistent with Table 2 where the interaction *Forbearance* $\times$ *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 true treatment effect of forbearance across eligible and ineligible households to bound the magnitude of the selection effect (after controlling for income, wealth, and all other observables).[26]

Selection is important in our setting as over 80% of the households in forbearance in our sample are tagged as ineligible using our proxy. For eligible households, forbearance is

[26]Given that the nature of the COVID shock was unanticipated, it is reasonable to assume that the average treatment effect of forbearance on the set of households that experienced wage reductions and worked in the affected industries would be similar to the rest of the population if forbearance had been randomly assigned. 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.

associated with higher consumption of 3.9% [= 1.3% - (-2.6%)] using the estimates in column (3). This estimate is significantly lower than the one for the group of ineligible households where forbearance is associated with 7.5% higher consumption. The 3.6 percentage point difference between the two estimates suggests that differences in who selects forbearance programs when access is easy significantly affect the forbearance effect. In other words, ineligible households who entered the forbearance program increased consumption more relative to eligible households who entered the program. Thus, our findings suggest that easy access to forbearance programs leads households with a particularly high sensitivity of consumption to postponed payments to select forbearance.

Table IA.6 in the Internet Appendix examines the eligibility effects separately for more and less affected household groups according to the industry they work in. The more affected subsample comprises households whose primary employer industry had below-median revenue growth between 2019 and 2020. Even though this is not how the government selected industries for eligibility, a much larger fraction of the workers in these industries are eligible for forbearance according to the formal criteria (almost 30% in total).[27] 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 in the most affected groups, as the effects on consumption do not differ with eligibility: 4.8% increase for ineligible households versus 5.1% for eligible households.

A 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 change in wages and were ineligible according to the program rules).[28] First, the fraction of households satisfying the eligibility

[27] We measure revenue growth until December 2020, and this was only available after March 2020, the date of eligibility definition by the government.

[28] 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.

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% in the less affected sectors and 90% for public servants), and their 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 borrowers' risk level in forbearance at the regulator's request. For those deemed to be at a higher risk of default at the end of the forbearance, the bank implemented a survey to determine whether they should receive additional debt relief measures due to perceived default risk. 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 difficulties meeting their debt obligations. As in the government forbearance program, access to additional debt relief was not based on a formal verification process of the borrower's income or financial difficulties. In this section, we compare the evolution of consumption and savings for households with and without additional debt relief measures implemented by the bank in September 2021.

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

Table 9 presents the results. Column (1) shows that consumption is affected by the end of the forbearance program, with the differential increase in consumption between groups dropping significantly but still above 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 but is still significant. The estimates for total deposits in column (3) are consistent with those for consumption. In fact, we find that the long-run forbearance effect on total deposits changes significantly after the forbearance ends, dropping from 13% to about -2% after the forbearance ends.

A somewhat more subtle pattern emerges when we consider the additional relief measures. Columns (2) and (4) of Table 9 present the results. We find that only about 7.4% of ineligible households in forbearance request additional relief, whereas about 6.1% of eligible households do so. Column (2) shows that households receiving additional relief measures in September 2021 did not adjust their consumption differential after the end of the forbearance period (column (2)). This is surprising because these debt relief measures were clearly temporary measures, but 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. This pattern persists after the initial government forbearance program is converted into bank assistance for these households, as shown by the coefficient on *Exit Effect* in column (4), which is still negative, although slightly less 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 forbearance is associated with a positive and significant impact on consumption and savings, with households in forbearance increasing spending by about 8% relative to households outside forbearance. Forbearance also has a positive and significant impact on deposits at about 8% but only over one year following the suspension of debt payments. This response to forbearance is heterogeneous across households with different levels of fragility as proxied by wealth, income, and indebtedness. While the average marginal propensity to consume and save is about 20 cents per euro of postponed payment, more fragile households consume more out of each euro of postponed payment, while the marginal propensity to save is significantly higher for less fragile households.

The response to forbearance is also heterogeneous for households who were and were not eligible for the forbearance program. Both eligible and, especially, ineligible households in forbearance earn less and are more indebted even before the pandemic than the overall sample average. Forbearance allowed eligible households to avoid a drop in consumption during the pandemic, while ineligible households increased consumption relative to ineligible households outside forbearance.

Our findings provide new insights about the need for debt forbearance during crises. Our paper is the first to characterize the individual consumption and savings response separately for both eligible and ineligible households in a large forbearance program while at the same time controlling for common unobservables like income shocks and wealth. The effects we document have important implications for the design of these programs – the Portuguese government (like many others around the world) intended the program to help households affected by the pandemic, but we show that households already observably fragile before the pandemic chose to also access the program. These households increased consumption significantly, which implies that they have exited forbearance with a higher debt burden (due to postponed principal and interest payments) and little change in deposits. If, instead, households had saved the majority of the postponed payments, one could view the policy as largely neutral from the perspective of household credit burden. Instead, we show that

most households accessing the program, who were not the intended targets and had a higher marginal propensity to consume out of forbearance, exited the forbearance program with a more indebted and fragile balance sheet than when they entered it.

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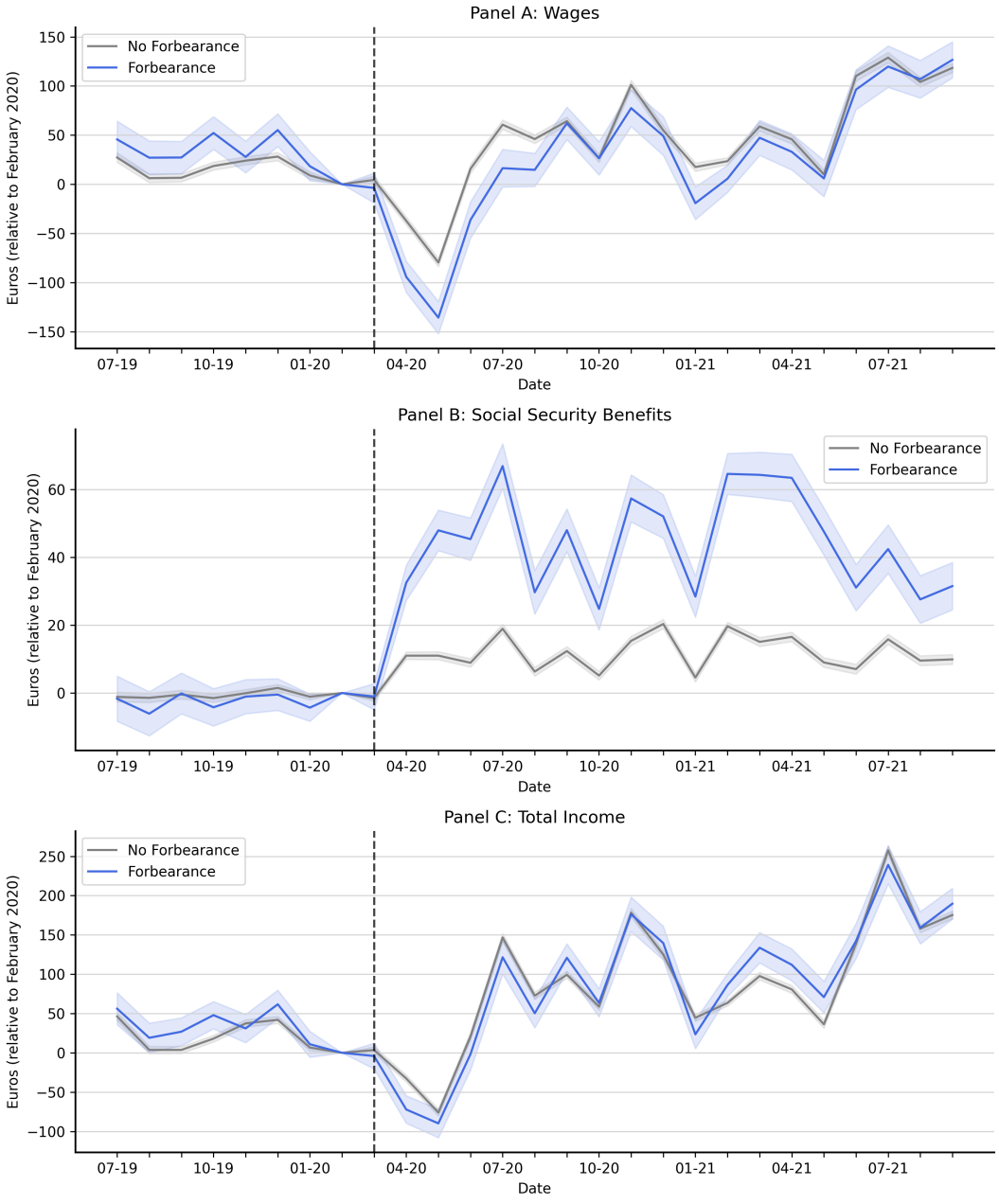
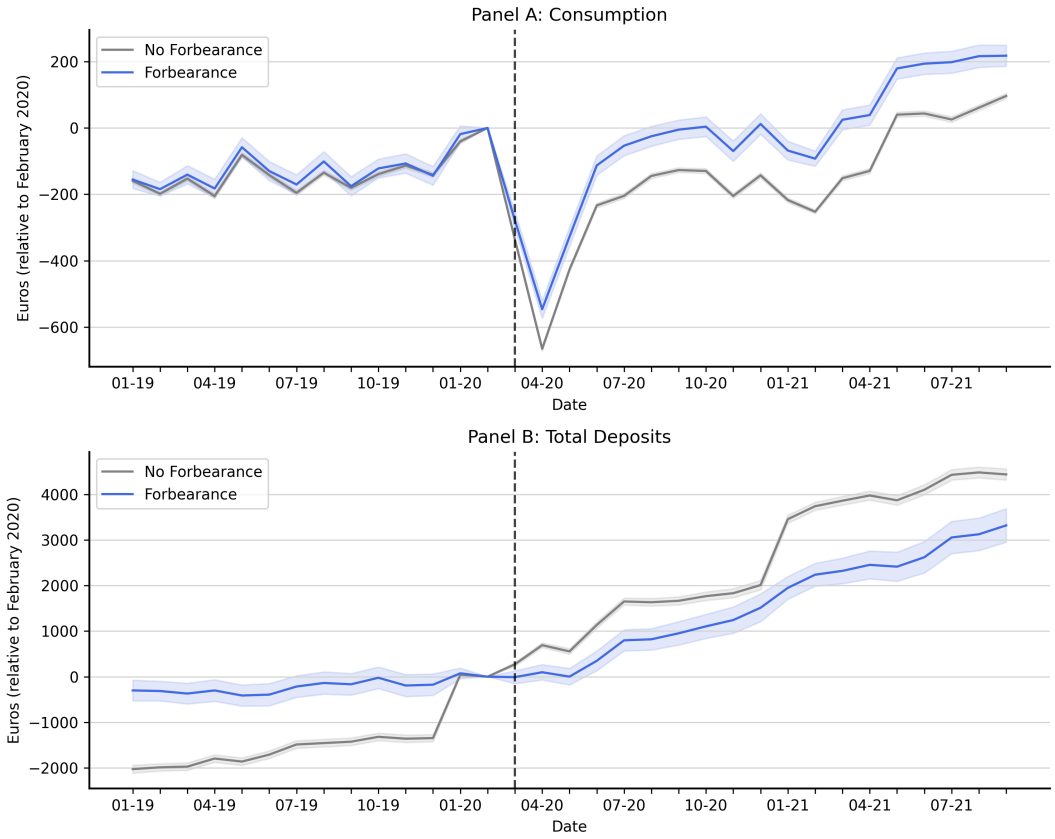
Figure 1: Evolution of Income by Forbearance Status



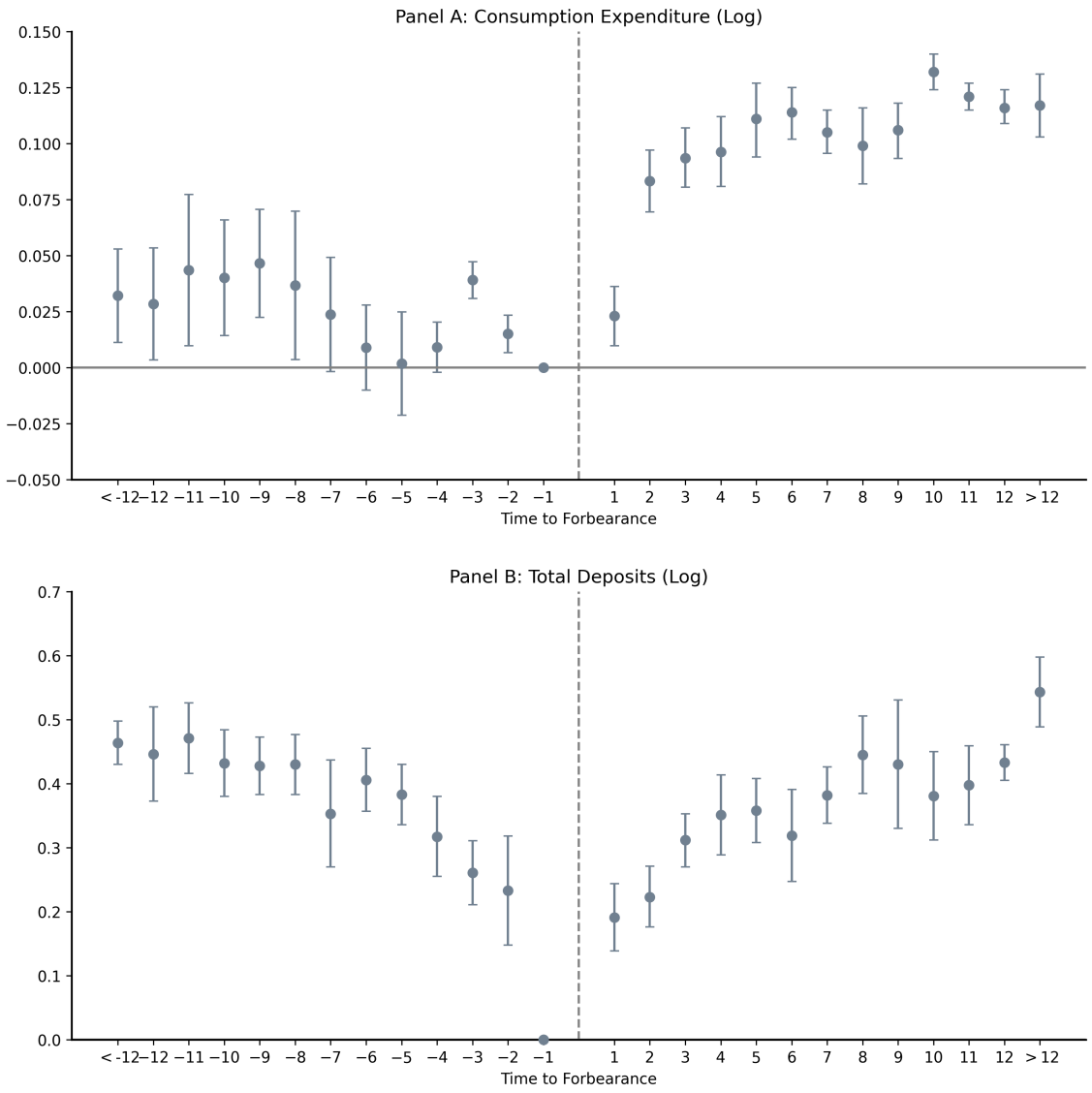
This figure plots the household average for different income sources, from July 2019 to September 2021. All measures are reported in euros, seasonally adjusted and relative to a pre-pandemic baseline (February 2020). Panel A reports the average change in monthly direct deposit of wages relative to the 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). The shaded area represents the confidence band at the 95% level, and standard errors are clustered at the household level.

Figure 2: Evolution of Consumption and Deposits by Forbearance Status



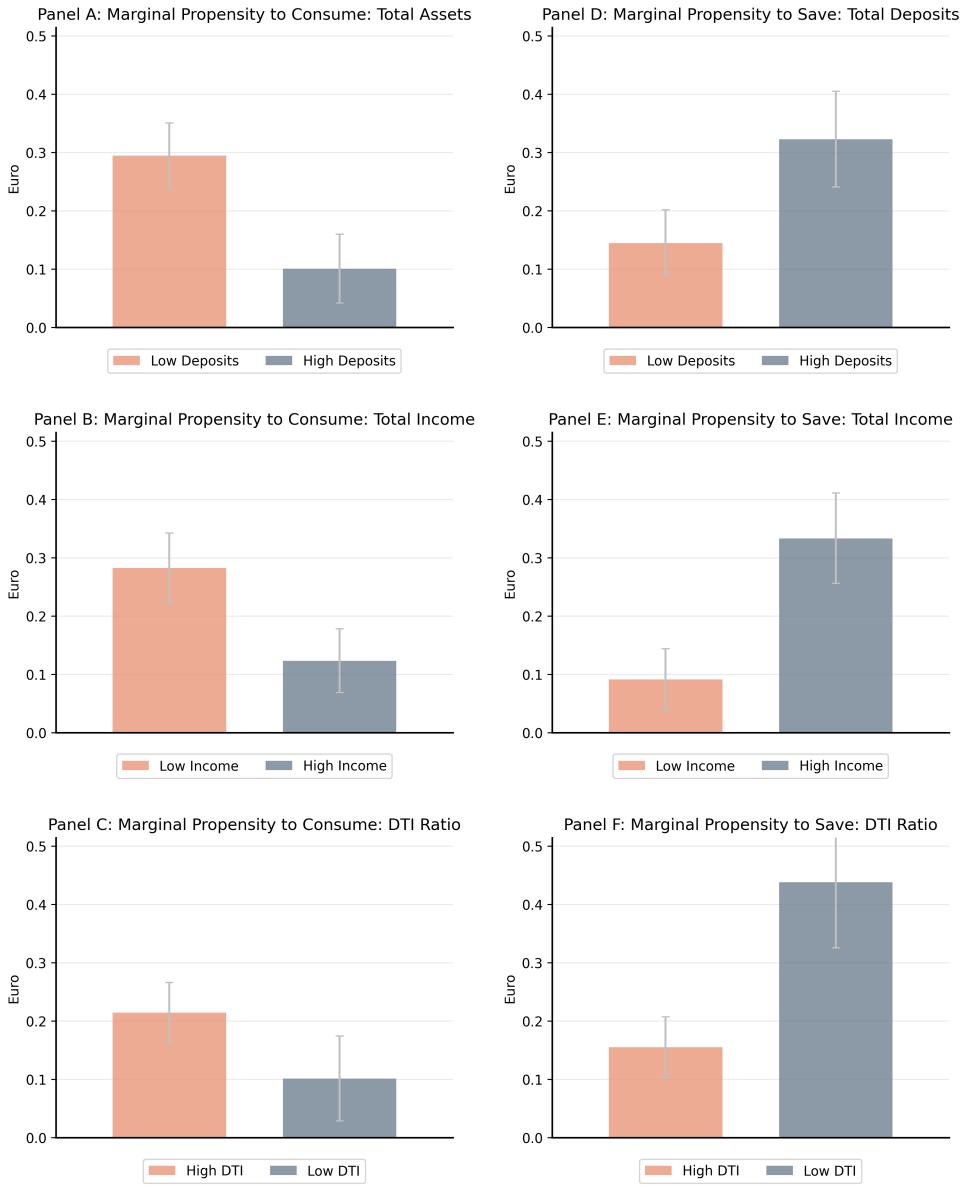
This figure plots the household average for monthly consumption and total deposits, from January 2019 to September 2021. All measures are reported in euros, seasonally adjusted and relative to a pre-pandemic baseline (February 2020). Panel A reports monthly consumption, computed as the sum between purchases and payments from either a debit or credit card at this bank. Panel B shows the evolution of total deposits, 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). The shaded area represents the confidence band at the 95% level, and standard errors are clustered at the household level.

Figure 3: Household Consumption and Deposits Response to Forbearance



This figure plots the regression coefficients and 95% confidence intervals of the logarithm of monthly consumption and total deposits, 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 deposits, measured as the sum between end-of-the-month checking and saving accounts' balances. This specification includes household-month and group-month-year fixed effects, with the group referring to different quartiles of pre-pandemic deposits and income (2019 averages). 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 Indebtedness



This figure plots the regression coefficients and 95% confidence intervals for monthly consumption and changes in total deposits, ΔTotal Deposits, 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). 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 deposits, measured as the sum between end-of-the-month checking and saving accounts' balances. This specification includes household-month and group-month-year fixed effects, with the group referring to different quartiles of pre-pandemic deposits and income (2019 averages). As a control, we include total income, which includes wages, social security, retirement benefits and other uncategorized inbound transfers. Standard errors are computed using two-way clustering (household-month and month-year level).

Table 1: Average Household Characteristics by Groups

	No Forbearance			Forbearance		
	Total	Eligible	Ineligible	Total	Eligible	Ineligible
Average Age	48	45	48	45	43	45
Number of Mortgagors	2	2	2	2	2	2
Wages	1,837	1,885	1,830	1,466	1,620	1,416
Pensions	1,325	1,129	1,339	1,079	1,020	1,089
Social Security Benefits	346	350	345	300	303	299
Other Inbound Transfers	657	639	659	764	651	790
Total Income	2,553	2,759	2,531	2,121	2,449	2,046
Consumption	1,516	1,615	1,506	1,343	1,441	1,320
Total Deposits	18,855	17,854	18,962	7,581	7,449	7,612
Checking Accounts	6,941	6,732	6,963	3,069	3,022	3,079
Savings Accounts	17,750	16,369	17,898	9,050	8,593	9,159
Mortgage Loans	67,803	73,339	67,211	93,077	96,256	92,346
Credit Cards and Overdraft	399	375	401	749	654	771
Other Banks' Loans	7,044	7,417	7,005	13,885	13,457	13,984
Debt Payment	311	305	312	377	360	381
Debt Payment-to-Income	0.19	0.15	0.19	0.30	0.20	0.32
Forbearance Amount	0	0	0	345	333	347
7 Day Delinquency	0.01	0.01	0.01	0.05	0.03	0.06
30 Day Delinquency	0.01	0.01	0.01	0.02	0.01	0.03
Observations	129,201	12,469	116,732	8,162	1,525	6,637

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, deposits, 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 Deposits

	Log(Consumption)				Log(Total Deposits)			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Forbearance × Post	0.165***	0.081***	0.077***	0.078***	0.196***	-0.002	-0.010	-0.008
	(0.011)	(0.010)	(0.010)	(0.010)	(0.039)	(0.031)	(0.030)	(0.030)
Log(Total Income)			0.035***	0.035***			0.072***	0.073***
			(0.005)	(0.005)			(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	0.591	0.598	0.599	0.601	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 deposits, 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, retirement benefits and other uncategorized inbound transfers. In some specifications, we include group-month-year fixed effects, with the group referring to different quartiles of pre-pandemic deposits 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 Deposits by Horizon

	Log(Consumption)			Log(Total Deposits)		
	(1)	(2)	(3)	(4)	(5)	(6)
Forbearance ×						
Immediate Effect (1m-3m)	0.127***	0.036**	0.033**	-0.067*	-0.196***	-0.203***
	(0.013)	(0.015)	(0.015)	(0.039)	(0.029)	(0.028)
Short-run Effect (4m-12m)	0.169***	0.088***	0.084***	0.185***	-0.010	-0.017
	(0.011)	(0.009)	(0.009)	(0.033)	(0.030)	(0.030)
Long-run Effect (>12m)	0.176***	0.090***	0.085***	0.363***	0.120***	0.109***
	(0.011)	(0.011)	(0.011)	(0.042)	(0.030)	(0.030)
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	0.591	0.598	0.599	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 deposits, 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 4^{th} to the 12^{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. 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, retirement benefits and other uncategorized inbound transfers. In some specifications, we include group-month-year fixed effects, with the group referring to different quartiles of pre-pandemic deposits 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: Effect of Forbearance on Credit Card and Overdraft by Horizon

	Log(Credit Card & Overdraft)			Log(Other Banks' Debt)		
	(1)	(2)	(3)	(4)	(5)	(6)
Forbearance ×						
Immediate Effect (1m-3m)	0.064*	0.076***	0.077***	0.153***	0.125***	0.125***
	(0.033)	(0.026)	(0.026)	(0.021)	(0.022)	(0.022)
Short-run Effect (4m-12m)	-0.095***	-0.033	-0.031	0.102***	0.078***	0.079***
	(0.028)	(0.023)	(0.023)	(0.024)	(0.023)	(0.023)
Long-run Effect (>12m)	-0.192***	-0.111***	-0.110***	0.092***	0.053	0.054*
	(0.036)	(0.031)	(0.031)	(0.031)	(0.032)	(0.032)
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	0.793	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^{th} to the 12^{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, retirement benefits and other uncategorized inbound transfers. In some specifications, we include group-month-year fixed effects, with the group referring to different quartiles of pre-pandemic deposits 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: Effect of Forbearance on Consumption by Category

	(1) Total	(2) Groc.	(3) Cloth.	(4) House Maint.	(5) Furnit.	(6) Transp.	(7) Health Care	(8) Restau.	(9) Entert. & Educ.	(10) Misc.
Forbearance ×										
Immediate Effect (1m-3m)	0.043***	0.107***	0.114***	0.088***	0.066***	0.044*	0.060**	0.082***	0.092***	0.079***
	(0.010)	(0.025)	(0.033)	(0.029)	(0.016)	(0.025)	(0.025)	(0.028)	(0.021)	(0.014)
Short-run Effect (4m-12m)	0.089***	0.170***	0.184***	0.087***	0.117***	0.131***	0.137***	0.178***	0.169***	0.076***
	(0.009)	(0.015)	(0.019)	(0.022)	(0.015)	(0.012)	(0.025)	(0.013)	(0.021)	(0.022)
Long-run Effect (>12m)	0.091***	0.164***	0.194***	0.074***	0.141***	0.138***	0.162***	0.198***	0.193***	0.040*
	(0.009)	(0.014)	(0.020)	(0.020)	(0.016)	(0.012)	(0.026)	(0.017)	(0.023)	(0.019)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Household FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Group × Month × Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.632	0.586	0.426	0.317	0.282	0.565	0.402	0.524	0.421	0.470
Observations	2,884,623	2,884,623	2,884,623	2,884,623	2,884,623	2,884,623	2,884,623	2,884,623	2,884,623	2,884,623

This table presents difference-in-differences estimates of regressions of 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) Total Consumption; (2) Groceries; (3) Clothing; (4) Housing Maintenance and Utilities; (5) Furniture; (6) Transport; (7) Health Care; (8) Restaurants; (9) Entertainment and Education; and (10) Miscellaneous Goods and Services. The logarithm of total income, which includes wages, social security, retirement benefits and other uncategorized inbound transfers, 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 quartiles of pre-pandemic deposits 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: Marginal Propensity to Consume and Save

	Consumption			ΔTotal Deposits		
	(1)	(2)	(3)	(4)	(5)	(6)
Forbearance Amount × Post	0.303***	0.194***	0.200***	0.095**	0.157***	0.195***
	(0.025)	(0.028)	(0.027)	(0.038)	(0.024)	(0.022)
Total Income			0.083***			0.518***
			(0.006)			(0.020)
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	0.663	0.666	0.667	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 coefficient estimates of the effect of forbearance on monthly consumption and monthly changes in total deposits, ΔTotal Deposits, 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, retirement benefits and other uncategorized inbound transfers. In some specifications, we include group-month-year fixed effects, with the group referring to different quartiles of pre-pandemic deposits 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: Marginal Propensity to Consume and Save by Time Horizon

	Consumption			ΔTotal Deposits		
	(1)	(2)	(3)	(4)	(5)	(6)
Forbearance Amount ×						
Immediate Effect (1m-3m)	0.170***	0.016	0.031	-0.107	0.034	0.126***
	(0.036)	(0.039)	(0.039)	(0.097)	(0.030)	(0.028)
Short-run Effect (4m-12m)	0.308***	0.193***	0.198***	0.118***	0.184***	0.216***
	(0.022)	(0.021)	(0.021)	(0.037)	(0.028)	(0.025)
Long-run Effect (>12m)	0.364***	0.291***	0.294***	0.154**	0.168***	0.189***
	(0.027)	(0.027)	(0.026)	(0.061)	(0.043)	(0.043)
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	0.663	0.666	0.667	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 deposits, ΔTotal Deposits, 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 4^{th} to the 12^{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. 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, retirement benefits and other uncategorized inbound transfers. In some specifications, we include group-month-year fixed effects, with the group referring to different quartiles of pre-pandemic deposits 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 on Consumption and Savings by Eligibility

	Log(Consumption)			Log(Total Deposits)		
	(1)	(2)	(3)	(4)	(5)	(6)
Forbearance=0 × Eligible=1 × Post	-0.049***	-0.026***	-0.026***	-0.035***	-0.040***	-0.043***
	(0.007)	(0.007)	(0.007)	(0.011)	(0.011)	(0.011)
Forbearance=1 × Eligible=0 × Post	0.184***	0.078***	0.075***	0.212***	0.050	0.045
	(0.012)	(0.012)	(0.012)	(0.041)	(0.033)	(0.033)
Forbearance=1 × Eligible=1 × Post	0.053***	0.011	0.013	0.109**	-0.066	-0.062
	(0.018)	(0.018)	(0.018)	(0.047)	(0.043)	(0.042)
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 deposits, 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, retirement benefits and other uncategorized inbound transfers. In some specifications, we include group-month-year fixed effects, with the group referring to different quartiles of pre-pandemic deposits 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 9: Effect of Forbearance Exit on Consumption and Savings

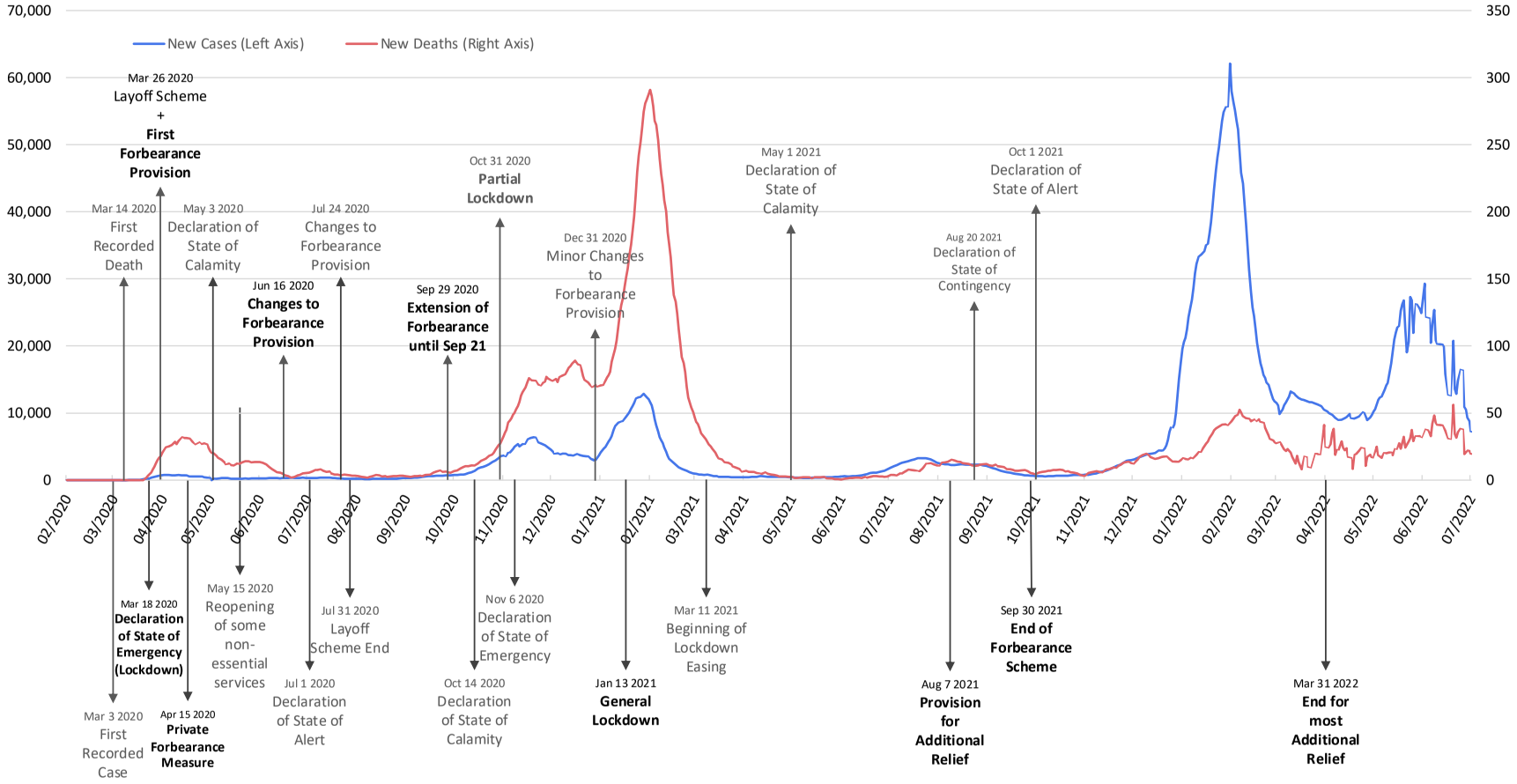
	Log(Consumption)		Log(Total Deposits)	
	(1)	(2)	(3)	(4)
Forbearance ×				
Immediate Effect (1m-3m)	0.032**	0.031**	-0.175***	-0.174***
	(0.014)	(0.014)	(0.028)	(0.028)
Short-run Effect (4m-12m)	0.076***	0.075***	-0.006	-0.006
	(0.010)	(0.010)	(0.032)	(0.032)
Long-run Effect (>12m)	0.081***	0.080***	0.128***	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 (1m-3m) × Additional Relief		0.050		-0.263***
		(0.039)		(0.070)
Short-run Effect (4m-12m) × Additional Relief		0.071**		-0.335***
		(0.034)		(0.081)
Long-run Effect (>12m) × 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 deposits, 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 4^{th} to the 12^{th} 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, retirement benefits and other uncategorized inbound transfers, is included as a control. In all specifications, we include group-month-year fixed effects, with the group referring to different quartiles of pre-pandemic deposits 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

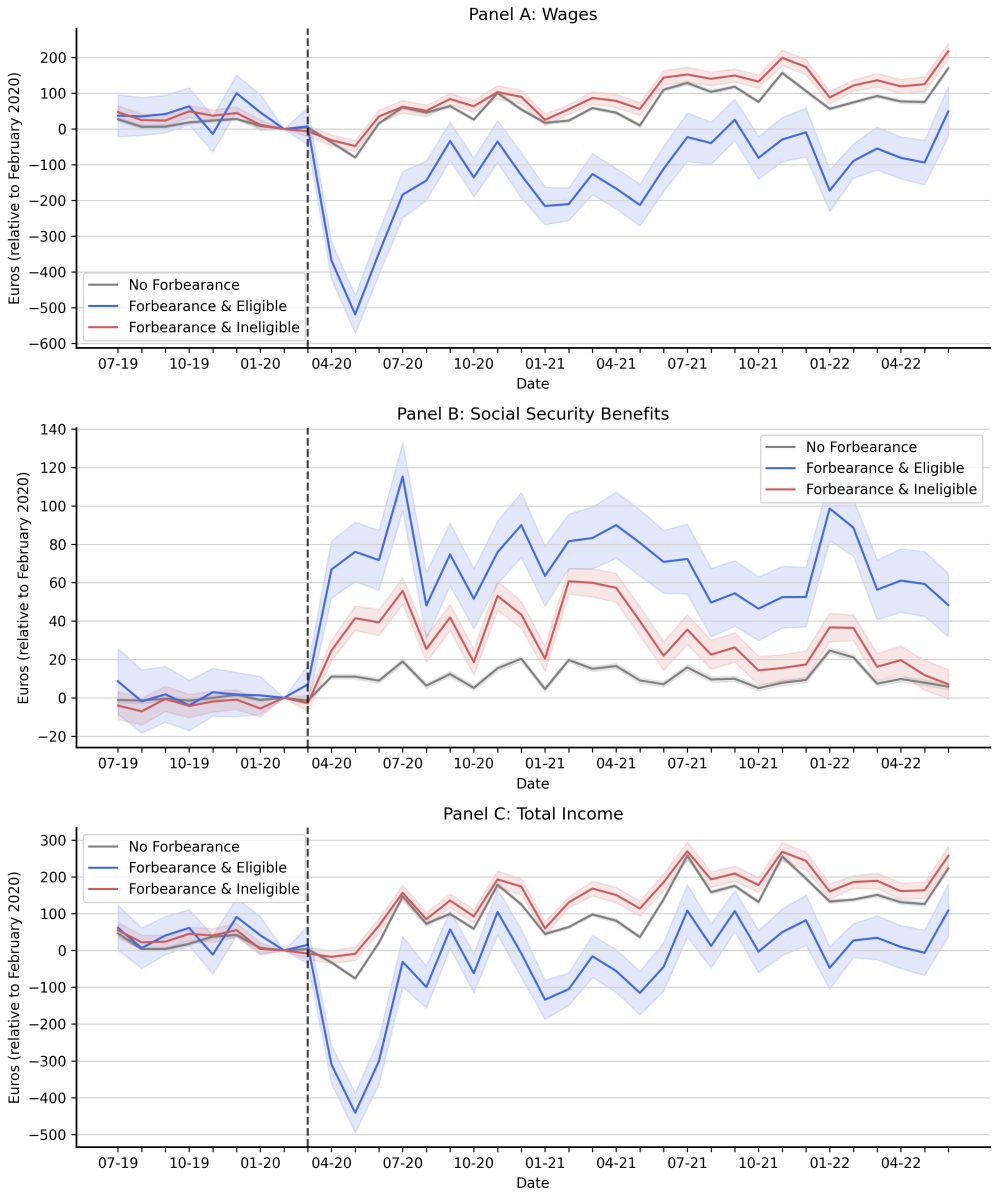
"The Heterogeneous Effects of Household Debt Relief"

Figure IA.1: Timeline of Events



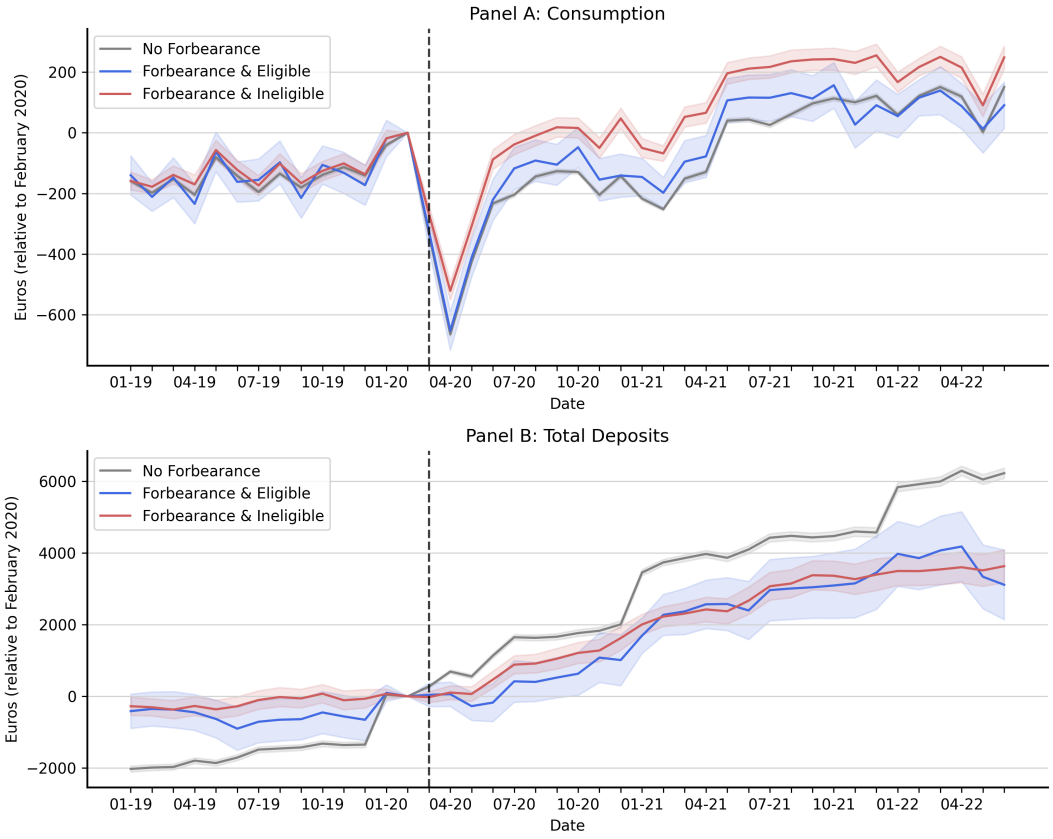
This figure shows the timeline of events, with key events being identified in bold, from February 2020 until July 2022. On the left axis and in blue we show the evolution of the number of new cases, while the right axis and in red we plot new deaths due to COVID-19.

Figure IA.2: Evolution of Income by Eligibility Status



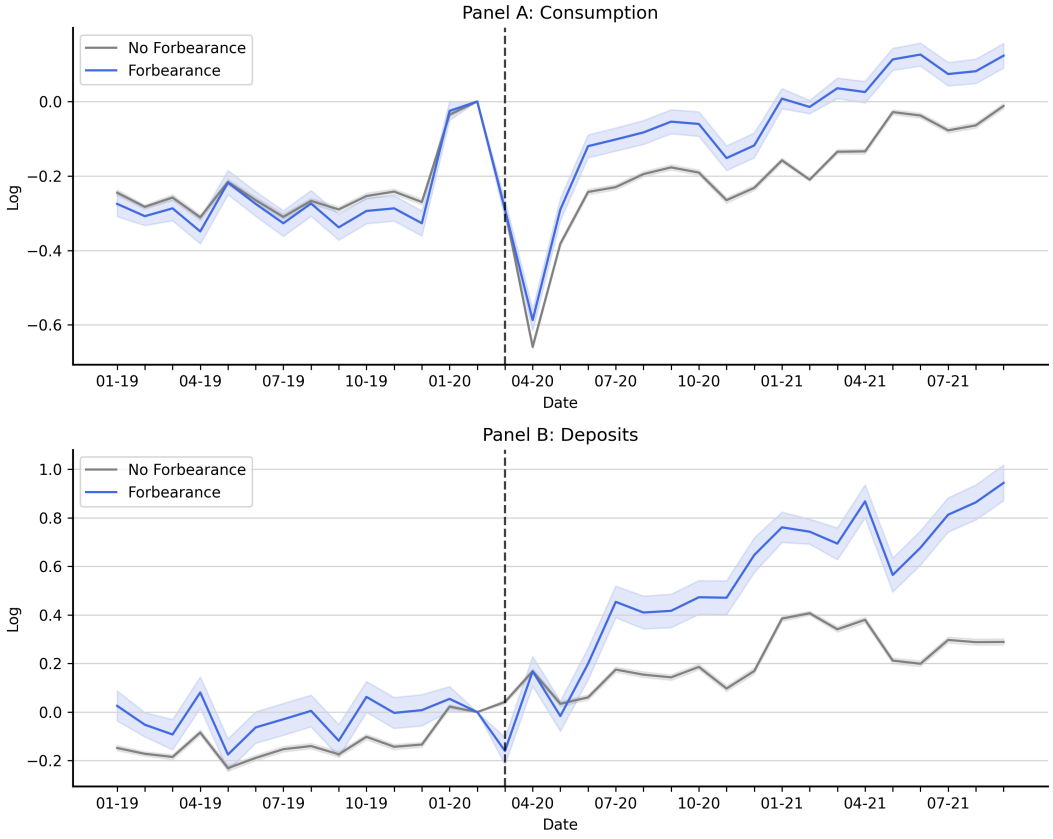
This figure plots the household average for different income sources, from July 2019 to June 2022. All measures are reported in euros, seasonally adjusted, and relative to a pre-pandemic baseline (February 2020). Panel A reports the average change in monthly direct deposit of wages relative to the pre-pandemic 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 outside forbearance (in grey), eligible households in forbearance (in blue) and ineligible households in forbearance (in red). The shaded area represents the confidence band at the 95% level, and standard errors are clustered at the household level.

Figure IA.3: Evolution of Consumption and Deposits by Eligibility Status



This figure plots the household average for monthly consumption and deposits, 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 deposits, 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 outside forbearance (in grey), eligible households in forbearance (in blue) and ineligible households in forbearance (in red). The shaded area represents the confidence band at the 95% level, and standard errors are clustered at the household level.

Figure IA.4: Evolution of Consumption and Deposits (in logs)



This figure plots the household average for monthly consumption and deposits, 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 deposits, computed as the sum between the end-of-the-month checking savings 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). The shaded area represents the confidence band at the 95% level, and 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
Total Deposits	18,185.0	30,787.9	302.0	1,159.5	5,567.4	20,738.0	51,399.2	137,363
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 provides summary statistics (mean, standard deviation, the 10%, 25%, 50%, 75% and 90% percentiles, and the number of households for which non-missing records exist), based on household averages over 2019. Income information comes from transaction-level data, as well as consumption expenditure starting January 2020. Deposits and liabilities with the bank come from end-of-the-month balances, as well as consumption expenditure prior to January 2020. Liabilities held at other banks are retrieved from the Credit Register, showing end-of-the-month balances. Income, deposits, 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. We measure as the sum between purchases and payments from either a debit or credit card at this bank. For the country's average, we compute 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 Consumption and Savings by Horizon: Sample of Individuals Working in the Private Sector)

	Log(Consumption)			Log(Total Deposits)		
	(1)	(2)	(3)	(4)	(5)	(6)
Forbearance ×						
Immediate Effect (1m-3m)	0.123***	0.051***	0.052***	-0.051	-0.186***	-0.185***
	(0.017)	(0.019)	(0.019)	(0.042)	(0.035)	(0.034)
Short-run Effect (4m-12m)	0.150***	0.088***	0.088***	0.196***	-0.004	-0.003
	(0.012)	(0.011)	(0.011)	(0.034)	(0.033)	(0.033)
Long-run Effect (>12m)	0.150***	0.090***	0.089***	0.369***	0.129***	0.126***
	(0.014)	(0.014)	(0.014)	(0.043)	(0.034)	(0.033)
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	0.591	0.597	0.598	0.852	0.856	0.857
Observations	2,769,300	2,769,300	2,769,300	2,769,300	2,769,300	2,769,300

This table presents difference-in-differences estimates of regressions of the logarithm of monthly consumption and total deposits, 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 4^{th} to the 12^{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, and only includes households whose primary employer in the first quarter of 2020 operated within the private sector. 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, retirement benefits and other uncategorized inbound transfers. In some specifications, we include group-month-year fixed effects, with the group referring to different quartiles of pre-pandemic deposits 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 Consumption by Category by Eligibility Status

	(1) Total	(2) Groc.	(3) Cloth.	(4) House Maint.	(5) Furnit.	(6) Transp.	(7) Health Care	(8) Restau.	(9) Entert. & Educ.	(10) Misc.
Forbearance=0 × Eligible=1 × Post	-0.040***	-0.005	-0.041	-0.003	-0.050**	-0.021	-0.049***	-0.049***	-0.036**	-0.055***
	(0.007)	(0.010)	(0.024)	(0.022)	(0.023)	(0.013)	(0.012)	(0.010)	(0.014)	(0.013)
Forbearance=1 × Eligible=0 × Post	0.089***	0.166***	0.186***	0.093***	0.123***	0.126***	0.141***	0.179***	0.175***	0.078***
	(0.009)	(0.016)	(0.023)	(0.018)	(0.014)	(0.015)	(0.028)	(0.019)	(0.024)	(0.015)
Forbearance=1 × Eligible=1 × Post	0.024*	0.116***	0.104***	0.038	0.052	0.068***	0.060**	0.085***	0.088**	-0.021
	(0.013)	(0.022)	(0.032)	(0.032)	(0.032)	(0.021)	(0.026)	(0.027)	(0.035)	(0.038)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Household FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Group × Month × Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.632	0.586	0.426	0.317	0.282	0.565	0.402	0.524	0.421	0.470
Observations	2,884,623	2,884,623	2,884,623	2,884,623	2,884,623	2,884,623	2,884,623	2,884,623	2,884,623	2,884,623

This table presents difference-in-differences estimates of regressions of 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) Total Consumption; (2) Groceries; (3) Clothing; (4) Housing Maintenance and Utilities; (5) Furniture; (6) Transport; (7) Health Care; (8) Restaurants; (9) Entertainment and Education; and (10) Miscellaneous Goods and Services. The logarithm of total income, which includes wages, social security, retirement benefits and other uncategorized inbound transfers, 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 quartiles of pre-pandemic deposits 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.5: Effect of Forbearance on Consumption and Savings by Eligibility and Selection Groups

	Log(Credit Card & Overdraft)			Log(Other Banks' Debt)		
	(1)	(2)	(3)	(4)	(5)	(6)
Forbearance=0 × Eligible=1 × Post	0.041**	0.050**	0.056***	0.050**	0.057***	0.061***
	(0.019)	(0.019)	(0.019)	(0.020)	(0.020)	(0.020)
Forbearance=1 × Eligible=0 × Post	-0.107***	-0.081***	-0.076***	0.133***	0.094***	0.095***
	(0.028)	(0.025)	(0.025)	(0.026)	(0.026)	(0.026)
Forbearance=1 × Eligible=1 × Post	-0.038	0.018	0.025	0.017	-0.002	0.002
	(0.055)	(0.051)	(0.051)	(0.046)	(0.046)	(0.046)
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	0.793	0.793	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
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 hort-term liabilities held at this bank, and liabilities held at other banks, 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, except for columns (4)-(6), where due to data limitations January 2019 was used as starting date. 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) 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. *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, retirement benefits and other uncategorized inbound transfers. In some specifications, we include group-month-year fixed effects, with the group referring to different quartiles of pre-pandemic deposits 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.6: Eligibility and Selection Groups: More Affected versus Less Affected

	Log(Consumption)			Log(Total Deposits)		
	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	-0.064**
	(0.013)	(0.010)	(0.017)	(0.016)	(0.016)	(0.026)
Forbearance=1 × Eligible=0 × Post	0.048**	0.114***	0.105***	0.066	0.096**	0.016
	(0.021)	(0.019)	(0.024)	(0.049)	(0.047)	(0.057)
Forbearance=1 × Eligible=1 × Post	0.051**	0.045	0.045	-0.039	-0.017	0.027
	(0.024)	(0.030)	(0.046)	(0.049)	(0.060)	(0.151)
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 deposits, 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 quartiles of pre-pandemic deposits 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.