Consumption Effects of Mortgage Payment Holidays: Evidence During the Covid-19 Pandemic*

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Abstract

We use UK transaction-level data during the Covid-19 pandemic to study whether mortgage payment holidays (PH) can act as a mechanism for smoothing household consumption following negative aggregate shocks. Our results suggest that mortgage PH were accessed by both households with pre-existing financial vulnerabilities and by those with stronger balance sheets, including buy-to-let investors. We also find that the temporary liquidity relief provided by PH allowed liquidity-constrained households to maintain higher annual consumption growth compared to those non-eligible for the policy. Finally, we find that mortgage PH led to higher saving rates for more financially-stable households.

Keywords: *Mortgage payment holidays; Household behaviour; Consumption; High-frequency data, Difference-in-differences; Panel data*

JEL classification: D14, E21, G51

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1 Introduction

Policy makers around the world deployed significant support measures in 2020 to mitigate the adverse effects of the Covid-19 shock on the real economy. One of these measures, mortgage payment deferrals, also known as mortgage payment holidays (PH), targeted directly mortgagors. It was introduced for the first time in the United Kingdom in March 2020, ahead of the first national lockdown. The initial guidance from the Financial Conduct Authority (FCA) encouraged lenders to offer a full suspension of mortgage principal and interest, without impact on households' credit risk score, and for a maximum period of three months, which was later extended to six months. The aim of this policy was to help households who struggled to keep up with mortgage payments due to the pandemic.[1]

Given the novelty of this nationwide policy, the role played by mortgage moratoria in supporting household balance sheets during the pandemic remains largely unknown. We address this gap in our paper by focusing on understanding whether, and how, mortgage PH have helped households, especially those more vulnerable, smooth their consumption during the pandemic. During the 2008-09 Great Recession mortgagors, especially those highly indebted, cut consumption to a larger extent. Understanding which policies may mitigate the impact of shocks on mortgagors is thus key to avoiding a more pronounced downturn in economic activity, and avoid a 2008-style recession.

Our work is related to the literature studying the effect of mortgage market modifications. In a loose sense, mortgage PH can be seen as a form of unemployment insurance (Ganong & Noel, 2019), or consumption insurance (Gelman *et al.* , 2020). Moreover, PH are a form of short-term liquidity relief, which has been shown to be more effective in reducing defaults than other forms of mortgage modifications that do not reduce short-term payments (Agarwal *et al.* , 2017; Ganong & Noel, 2020; McCann & O'Malley, 2020; Campbell *et al.* , 2021; Guren *et al.* , 2021).[2]

The direct impact of mortgage PH on household consumption during recessions has not yet been

[1] Unlike the 2008-09 Great Recession, when unemployment and arrears increased dramatically as a result of a collapse in aggregate demand, during the Covid-19 pandemic arrears remained contained and near historically low levels (see UK Finance data). This suggests that mortgage PH may have been successful in keeping households current on their mortgages.

[2] Ganong & Noel (2020) and Agarwal *et al.* (2017) study the US Home Affordable Modification Program (HAMP) implemented in 2009. The policy aimed to reduce mortgage payments for the first five years up to a debt-service ratio (DSR) of 31%, or cut the principal to an LTV of 115 without affecting mortgage payments. Ganong & Noel (2020) find that only reductions in mortgage payments through maturity extensions had large effects on consumption and on the probability of defaulting. In turn, Agarwal *et al.* (2017) find that the HAMP led to a lower rate of repossessions, consumer debt delinquencies, house price declines and to higher durable consumption. In addition, focusing on the Irish mortgage market, McCann & O'Malley (2020) find that borrowers' liquidity position and payment relief are key in determining the propensity for mortgage default. Finally, Campbell *et al.* (2021) build a model to show that adjustable-rate mortgages (ARM) with an option to pay only interest on loans and extend its maturity during recessions, is welfare-enhancing: this policy stabilises consumption growth over the business cycle.

examined in the literature.[3] Existing research, predominately in the United States, has focused on the predictors of mortgage PH take-up, or on the impact of PH on mortgage defaults during the Covid-19 crisis (Haughwout *et al.* , 2020; An *et al.* , 2021; Cherry *et al.* , 2021). The exception is Vihriälä (2021), who examines household commitment in debt repayments following the introduction of mortgage payment holidays by one lender in Finland in 2015. But there are a number of differences between our work and Vihriälä (2021). First, the policy allowed borrowers in Finland to reduce their minimum mortgage amortisation, but interest continued to be paid. This differs from the UK, where the policy encompassed a complete suspension in interest and principal repayments. Second, Vihriälä (2021) excluded households in severe payment difficulties, whereas those in arrears were still allowed to apply for the policy in the UK. Finally, the policy was offered by one lender in an economic expansion, while in the UK the policy was implemented nationwide during a recession.

To study mortgage PH in the UK, we use transaction-level data between January 2019 and November 2020 obtained from Money Dashboard (MDB), a UK online budgeting application. We answer two key questions regarding the impact of mortgage PH in the UK.

First, using a Probit model, we ask if PH have been accessed by those who needed them the most, i.e. by financially constrained households or by those hit hardest by the pandemic. We find that mortgage PH were used by households with varying degrees of financial strength. On the one hand, mortgage PH take-up was higher for more vulnerable households, such as those with high mortgage DSRs, negative saving rates, or by those whose income decreased during the pandemic. This is in line with the US evidence showing that households most likely to have applied for the moratorium tended to face tighter credit constraints, and were more likely to have higher pre-covid delinquency rates (Haughwout *et al.* , 2020; Cherry *et al.* , 2021). But we also find that the policy has been accessed by borrowers with stronger balance sheets, such as those with financial income or by property investors. These results suggest that some households may have accessed PH for reasons other than payment difficulties, such as precautionary or uncertainty reasons.

Second, we ask if mortgage PH have changed household behaviour. Using a quasi-experimental difference-in-differences (DiD) research design with fixed effects, we study whether the temporary

[3]Existing research during the pandemic has focused mainly on studying the the marginal propensity to consume out of direct government cash transfers, in the US, (Armantier *et al.* , 2020, 2021; Bachas *et al.* , 2020; Baker *et al.* , 2020; Carroll *et al.* , 2020; Chetty *et al.* , 2020; Coibion *et al.* , 2020; Sahm, 2021), and out of hypothetical income transfers in various advanced economies (Christelis *et al.* , 2020; Crossley *et al.* , 2020; Albuquerque & Green, 2021). One of the exceptions is An *et al.* (2021), who find that eviction moratoria during the pandemic reduced evictions and helped support US household consumption, particularly food and grocery spending.

liquidity relief from PH induced households to change their consumption decisions. A key challenge to our DiD estimates is that mortgagors can self-select into the policy, with only around one in five mortgagors having applied for it during 2020 (FCA, 2021). To minimise confounders from self-selection, we treat households non-eligible for the policy, i.e. renters and outright owners, as our control group. In turn, our treatment group includes mortgagors with a PH for at least one month during March-November 2020. We show that pre-treatment trends in consumption and debt were comparable between mortgagors on PH and non-eligible households before March 2020, which provides validity to our DiD research design. Another challenge to our DiD identification is bias from time-varying unobserved household characteristics that may determine differential household behaviour during stress. To deal with this omitted variable bias we: i) control for time-varying income level and income shocks during the pandemic; ii) check the robustness to local and regional shocks; iii) check the robustness to anticipation and delayed effects of the policy; and iv) test the sensitivity of the DiD to alternative identification approaches, such as propensity score matching and synthetic control methods, which minimise heterogeneity between treatment and control groups.

Our main finding suggests that mortgage PH supported consumption smoothing for liquidity-constrained borrowers. Among households with low saving rates, who are more likely to be liquidity-constrained, those on mortgage PH had 22 p.p. higher year-on-year real consumption growth relative to a control group with similar saving rates. For unconstrained borrowers, mortgage PH led to higher saving rates, although it had no overall effect on consumption.

We also study how consumption evolved around the expiration date of PH to determine whether the withdrawal of the policy had any impact on mortgagors' incentives. We find that liquidity-constrained households who were on PH for the maximum length of six months record a fall in consumption when mortgage repayments resume. This is in line with Vihriälä (2021), who claims that liquidity-constrained households on a six-months mortgage moratoria over-consume when the PH is active. This result can be rationalised by self-control issues. But differently from Vihriälä (2021), we find that liquidity-constrained households on shorter mortgage PH duration do not change their consumption pattern at the expiration date. We believe that the diverging behaviour of liquidity-constrained mortgagors on different mortgage PH durations may be driven by households' financial position. For instance, we show that negative income shocks are correlated with a longer PH duration. As such, losses in income during the pandemic may have further tightened financial constraints for these mortgagors who were also already liquidity-constrained.

These households would then have an incentive to have a mortgage PH for longer to be able to cope with their mortgage commitments. But once policy expires, liquidity-constrained households hardest hit by income shocks would need to under-consume to keep their mortgage payments current.

The rest of the paper is organised as follows. In Section 2 we discuss the transaction-level data we use in the paper. In Section 3 we use a Probit model to analyse which household characteristics best predict mortgage PH applications. Section 4 presents our identification strategy and research design. In Section 5 we show our results for the effect of mortgage PH on household consumption. Section 6 concludes the paper. The Online Appendix includes additional results, and describes in detail the steps we take in cleaning our dataset.

2 Data

2.1 Data description

We use data from Money Dashboard (MDB), UK's largest online personal budgeting application. The use of administrative micro data from alternative sources, such as the one used here, has been key for tracking households' consumption patterns during the pandemic (Baker & Kueng, 2021; Vavra, 2021). The MDB App is free and allows users to link their current accounts, savings accounts and credit cards on one platform. It then collates the financial transactions and groups them into nearly 290 buckets, such as mortgages, gas bills or groceries. MDB allows users to tag a transaction manually if transactions do not have automatic descriptions. The key advantages of MDB are its timeliness and the data collection method, which minimises measurement errors due to being fully electronic and automated in real-time. The use of transaction level data in the UK to understand households' financial situation has been relatively novel. The exception is Hacioğlu Hoke *et al.* (2021) and Bourquin *et al.* (2020) who use the MDB data to examine trends in aggregate spending and household financial positions during the Covid-19 pandemic.

Two features of the data are worth emphasising. First, since MDB is a transaction-level dataset, it only includes information on the flow of funds, with no information on balances of accounts, such as for mortgages, savings or credit cards. Second, the data include information only on electronic cash withdrawals and payments, so we have no way of knowing how households spend their physical cash. But this is unlikely to skew our analysis, given the lower reliance that UK households have put on cash transactions over the past decade. According to aggregate data from UK Finance, cash payments have been steadily declining in the UK since 2009.[4] Card payments and electronic transfers account for nearly 80% of all transactions in 2019, compared to only 42% in 2009.

We first aggregate transactions by month and type to carry out our analysis. Table 1 illustrates our key aggregates of financial transactions in MDB, split into debits (uses of funds) and credits (sources of funds). We do, however, consider more disaggregated measures of non-housing consumption, including non-durable goods, durables goods, services and online spending. This allows us to explore heterogeneous effects across broader measures of household consumption.

Table 1: Aggregation of financial transactions in MDB by type of funds

Sources of funds	**Uses of funds**
Income	Non-housing consumption
Labour income	Cash (ATM)
Investment/capital income	Investment/other savings
Rental income	Rental payments
Other income/benefits	Other spending
Credit	Credit repayments
Secured	Secured
Mortgage (not observed)	Mortgage
Other secured	Other unsecured
Unsecured	Unsecured
Credit card	Credit card
Personal loans	Personal loans
	Bank charges/fees

For each user in MDB we observe their transactions, year of birth, gender, a partial postcode, and the date they joined the App. For each transaction we have information on the amount, whether it is a credit or debit, the date when it was made, the merchant, a raw transaction description from the bank statement, and the automatic or manual tag that MDB uses to categorise the transaction into buckets. Since users can link multiple accounts, we can also observe an account reference, and the account type. Additionally, MDB uses specialised algorithms to derive salary income from transaction tags, including from users' multiple jobs. Finally, when a user signs up to MDB, the App automatically downloads up to three years of back data from the accounts linked. These historical transactions allow us to build a rich panel dataset and compare how trends have changed during the Covid pandemic, relative to the past.

In this paper, we follow Bourquin *et al.* (2020) and treat the data as measuring the finances of the 'nuclear family', which includes individuals, their partners and their children. Since MDB is a budgeting

[4]See UK Payment Markets 2020.

App, it gives individuals an incentive to link their partner's accounts if they share finances so as to better track spending and income in a month. Unlike Bourquin *et al.* (2020), we decide against removing users where there is a indication that their partner's accounts are missing. We do this because we believe that the accurate identification of households can be very challenging from transaction level data, since this type of data miss some household information. Additionally, we focus on the total resources of a user and how they interact with mortgage PH to determine consumption decisions. If users do not share finances with others in the household, then their spending decisions will be determined by their own financial position, and dropping them from the data would lead to an important bias.

We take a number of steps to clean the data, which we summarise in Appendix A and explain in detail in the Online Appendix. Our final monthly sample consists of a panel of 13,220 users from January 2019 to November 2020. We show descriptive statistics for the main variables in Table 13 in Appendix B. We find that the MDB data is quite representative of the mortgage population. In particular, Appendix C compares MDB statistics with those from various official national data sources. We find close matches, in particular, across mortgagors, for: the share of mortgagors relative to all households, the age distribution, and the income distribution and savings.

2.2 Identification of mortgage payment holidays in MDB

Mortgage PH were made available to all mortgagors. Since credit scores were meant to be unaffected by mortgage PH, borrowers had an incentive to apply for the policy rather than to remain in arrears or to default. Appendix D provides further details of how mortgage PH have been implemented in the UK.

In this section, we describe the steps we take to infer mortgage PH from the data. Since we do not observe directly in MDB when a particular user received the policy, we assume that households received a PH if: (i) the mortgage payment is missing from March 2020 onwards; (ii) the user has transactions in the month when the mortgage payment is missing; and (iii) the user resumes repayments within 1-6 months. We also consider borrowers who were in arrears before taking a PH, i.e. whose mortgage payment had been missing prior to March 2020 and did not resume until after March. Moreover, if users resume payments after more than six months since the date the PH started, we assume that borrowers had PH for the maximum time allowed of six months, and then moved into arrears for the subsequent months.[5] To

[5] Our data stops in November 2020 – i.e. eight months after the PH have been introduced in March 2020. As a result, we can only observe borrowers who have been in arrears for a maximum of two months after taking the PH.

make it clear, we do not include in our definition of PH those borrowers who do not resume mortgage payments by November 2020. Hence, we identify mortgage PH that start between March - October 2020 and end by November 2020. While this may underestimate the number of PH in our data, we do not believe this bias to be substantial. In November 2020, less than 2% of total outstanding mortgages in the UK were still on mortgage PH.[6]

Figure 1 compares the proportion of mortgage PH in MDB after we apply our identification method, against aggregate data from the UK Finance. Panel (a) shows that the stock of PH in force each month provides a very comparable measure between the two datasets. At the peak of the policy in May 2020, around 17% of all the mortgages in MDB were on PH, with the proportion declining gradually to around 2.5% in October 2020 (which is the last month when we identify new mortgage PH in our data). These numbers are also in line with the FCA (2021) survey showing roughly 1 in 5 borrowers have applied for a mortgage moratorium. Panel (b) shows the proportion of new mortgage PH issued on a monthly basis in MDB. New PH are slightly higher than data reported by the UK Finance in all months but April 2020. These discrepancies may be explained by a number of reasons.

First, the data collection is different. UK Finance data are based on direct reporting from a sample of lenders, while MDB captures transactions made directly by households. As such, it is possible that banks may report a successful application of a PH at a different time than when households actually stop paying their mortgage. Second, accurate timely data on new mortgage PH in early 2020 was difficult to obtain. UK Finance suggests that individual lenders' data could have been based on measurement error due to double-counting or other anomalies in daily totals reported to the UK Finance.[7] Third, it is possible that some borrowers stopped direct debits on mortgage payments before the moratorium was approved by lenders, inflating the numbers in MDB.

Although we can reliably infer when a mortgage PH was granted in the data, we have no information in MDB on mortgage PH applications. This may cause issues if lenders had an option to deny borrowers the policy, based on their characteristics. We, however, do not believe this to be a concern given the actual implementation of PH in the UK: lenders were encouraged to grant the application to all borrowers who requested a PH if the policy was in their best interest.[8] This is confirmed by data from the Understanding Society special Covid-19 Survey that shows that only 1.5% of all mortgagors who applied for a mortgage

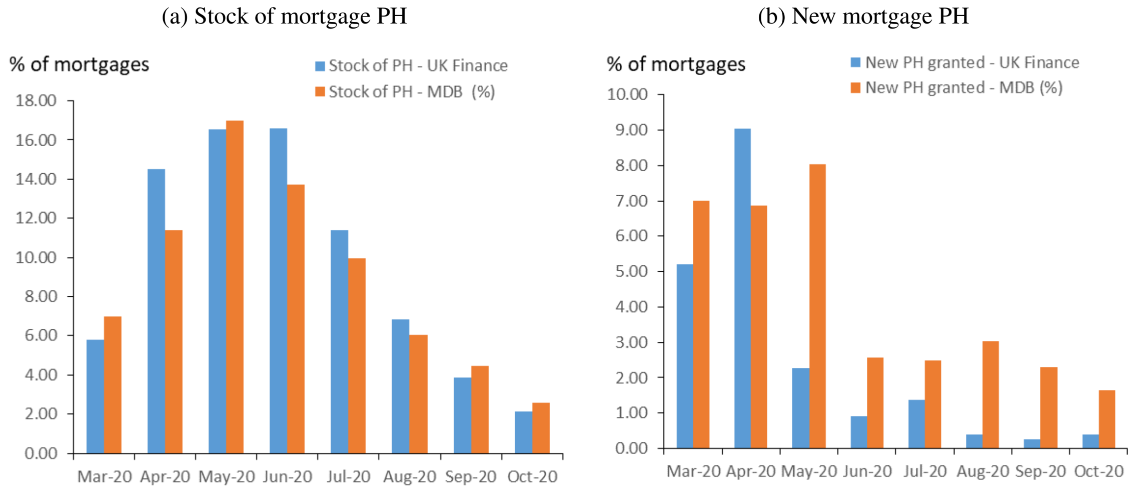
[6] See UK Finance: Household Finance Review - Q4 2020.

[7] See UK Finance press release on 19/06/2020.

[8] See the FCA's November 2020 finalised guidance on payment deferrals.

PH got their application denied in the April 2020 wave of the survey, and 0.97% in the May wave.[9] As a result, throughout the analysis, we assume that applications are nearly perfectly correlated with approvals of mortgage PH.

Figure 1: Mortgage PH in MDB versus aggregate data



Source: UK Finance and Money Dashboard.

3 Who are the mortgagors who accessed mortgage payment holidays?

In this section we examine the characteristics of borrowers who accessed mortgage PH to understand who benefited the most from this policy. It is likely that households with weaker balance sheet positions may have relied on mortgage PH to alleviate the impact of the health shock on income, particularly if they were financially constrained and thus had limited access to alternative resources to smooth consumption. It is also possible that households may have accessed the policy for reasons unrelated to financial difficulties. For instance, the elevated uncertainty about further shocks from the pandemic may have led some households to seek PH for precautionary reasons, even in the absence of negative income shocks. In addition, mortgage PHs effectively allowed households to borrow very cheaply, at a low interest rate and without having to pay any refinancing cost. Hence, even if households did have access to additional credit sources, taking out a mortgage PH allowed them to access what was very likely, the cheapest form of credit available.

We show in Table 2 the summary statistics of borrowers with mortgage PH between March and November 2020. About 20% of all mortgagors in MDB had a PH in 2020, with the majority accessing

[9] The survey data can be accessed at https://www.understandingsociety.ac.uk/documentation/covid-19.

PH for up to three months. Over half of the PH were granted in the first two months of the pandemic, when uncertainty was high amid the imposition of the first national lockdown in the UK.

Table 2: Mortgage PH statistics

No. households	13,220
No. mortgagors	5,154
No. mortgagors on PH	1183
% with $PH \leq 3m$	83%
% with $PH > 3m$	17%
Avg. PH duration (in months)	2.2
% accessed in Mar - Apr 2020	50.8%
% accessed in June - Nov 2020	49.2%

To investigate who accessed mortgage PH, we estimate the following probit model:

$$P(PH_j = 1|X) = \Phi(\beta_0 + \beta_1 X_1 + ... + \beta_k X_k) \tag{1}$$

$$\Phi(z) = P(Z \leq z),\ Z \sim N(0,1) \tag{2}$$

where we use the cumulative standard normal distribution function $\Phi(.)$ to model the binary dependent variable PH: takes the value of one if the mortgagor had a PH at any point in time. The probit coefficient β_j is the change in the z-value associated with a one-unit change in X_j, which contains a vector of pre-crisis household characteristics: age and age squared; a dummy variable for having children; a dummy variable for having more than two mortgages (a proxy for property investors); a dummy variable for having financial income pre-pandemic; a dummy for being unemployed (i.e. with zero jobs in MDB) in March or April 2020; a dummy for experiencing at least a 10% decrease in income in March or April 2020 (whichever is largest) relative to normal pre-pandemic income levels; a dummy for having negative saving rates pre-pandemic; and quintiles for pre-pandemic mortgage debt-service ratios (DSRs), pre-pandemic saving rate, and normal pre-pandemic income level. Pre-crisis variables, except normal income, represent the median values from January 2019 to February 2020. Normal pre-crisis income is calculated using the median from January 2020 to February 2020, and if that value is missing, we take the median across all months in 2019. Table 14 in Appendix B shows the distribution of pre-pandemic balance sheet indicators for the mortgage DSRs, income and saving rate. For an easier interpretation of the coefficients in the Probit model, we only show marginal effects of variables on the conditional probability of accessing mortgage PH.

3.1 Does the pre-pandemic financial position matter for policy take-up?

We start by examining the correlation between the pre-pandemic financial position of mortgagors and policy take-up in Table 3. We find that age is positively correlated with the probability of receiving a PH in columns (1) to (3), but at diminishing returns since the age squared term is negative and significant. Being financially vulnerable pre-crisis is also positively correlated with PH take-up. Column (3) shows that having DSRs ratios in the first or the second quintile of the distribution decreases the conditional probability of receiving a mortgage PH by up to 6.4 p.p. compared to having a DSR ratio at the top of the distribution. And column (4) shows that households with saving rates in the bottom quintile are 4 p.p. more likely to access PH compared to mortgagors with saving rates in the top quintile.

Nevertheless, we find that the policy was accessed not only by those struggling financially, but also by those with alternative financial resources to smooth consumption: having positive financial income makes households 2 p.p. more likely to receive a PH, when we control for quintiles of DSRs or savings. This result in in line with Chakrabarti *et al.* (2021) and Cherry *et al.* (2021) who also find that mortgage debt relief was disproportionately received by richer mortgagors in the United States.[10]

Finally, we find that borrowers are substantially more likely to receive PHs if they have more than two mortgages. These users are most likely property investors rather than owner-occupiers. The probability of receiving a mortgage moratorium increases by nearly 20 p.p. for these households, from a mean sample probability of 23 p.p. (as shown at the bottom of the table).

To understand what is driving these results, Table 4 illustrates the characteristics of mortgagors according to the number of mortgage payments they make per month. Borrowers with more than two monthly mortgage payments have higher debt levels and substantially lower median savings compared to other mortgagors.

[10]The CARES Act provided US borrowers with a six-month (renewable to twelve-month) forbearance on loans from April 2020. Cherry *et al.* (2021) find that around a third of borrowers in forbearance continued making full loan repayments, suggesting that forbearance acted as a credit line that allowed borrowers to draw on the payment deferral if needed. They also found that about 60% of the dollar amount of financial relief from forbearance was received by borrowers with above median pre-pandemic incomes, but take-up rates were correlated with tighter credit constraints. But they also find that households with a higher likelihood of COVID-19 related shocks were also found to be more likely to get debt relief. This is in line with Haughwout *et al.* (2020), who find that households most likely to have applied for the moratorium had lower credit scores, higher debt balances, and were more likely to have higher pre-covid delinquency rates.

Table 3: Impact of pre-pandemic financial position on the probability of PH at any point

	Dependent variable: P(PH = 1)			
	(1)	(2)	(3)	(4)
Age	0.0089*	0.0087*	0.0079*	0.0074
	(0.0049)	(0.0049)	(0.0047)	(0.0047)
Age sq	−0.0001**	−0.0001**	−0.0001**	−0.0001**
	(0.0001)	(0.0001)	(0.0001)	(0.0001)
Kids flag	0.0051	0.0048	0.0053	0.0032
	(0.0130)	(0.0130)	(0.0125)	(0.0124)
No mortgages > 2	0.1888***	0.1826***	0.1892***	0.1917***
	(0.0299)	(0.0297)	(0.0297)	(0.0296)
Fin. income	0.0209	0.0233*	0.0197	0.0215*
	(0.0134)	(0.0133)	(0.0129)	(0.0128)
MDSR	0.00003**			
	(0.00001)			
MDSR Q1		−0.0339*		
		(0.0182)		
MDSR Q2		−0.0647***		
		(0.0175)		
MDSR Q3		−0.0448**		
		(0.0178)		
MDSR Q4		−0.0155		
		(0.0185)		
Income Q1			0.0240	
			(0.0194)	
Income Q2			−0.0098	
			(0.0199)	
Income Q3			−0.0135	
			(0.0174)	
Income Q4			−0.0073	
			(0.0159)	
Savings Q1				0.0430*
				(0.0229)
Savings Q2				0.0161
				(0.0209)
Savings Q3				0.0230
				(0.0208)
Savings Q4				0.0236
				(0.0213)
Share on PH	22.95	22.95	22.95	22.95
Mortgagors (N)	4,687	4,687	5,034	5,150

Notes: The sample includes mortgagors. Robust standard errors in brackets. Constant is not reported. All financial variables are calculated pre-crisis. Quintile 5 is the baseline in columns 2-4.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Compared to borrowers with one mortgage payment per month, those with multiple mortgages earn 25% more, are twice as likely to have PH, and are much more likely to have financial and rental income.[11] Since these borrowers are richer and more indebted, they may rely more heavily on mortgage PH for

[11] Rental income is challenging to identify accurately in MDB since many rental payments involve transactions between different private individuals. It is thus difficult to know if these transactions are rental payments or other types of general repayments between households. As a result, only a small proportion of rental payments can be accurately identified in MDB.

two reasons. On the one hand, being more indebted and having multiple mortgages to service may have made property investors more vulnerable to economic shocks during the pandemic, particularly as rental moratoria were also introduced in the UK in 2020 (although they were voluntary, resulting in a much lower take-up). On the other hand, as these borrowers tend to have investment income, they are also more likely to be financially savvy. As a result, they may have used the funds obtained through mortgage PH at historically low mortgage rates for more lucrative investment opportunities.

Table 4: Statistics by number of monthly mortgage payments

No of mortgage payments per month	**One**	**Two**	**> Two**
Share of mortgagors (%)	77.5	16.9	5.7
Median DSR pre-crisis	23.4	26	28.6
Median DSR post-crisis	24.5	27.4	32.7
% in bottom DSR quintile	37	34.6	31.9
% in top DSR quintile	34	43	49.3
Median salary (£)	3,144	3,534	3,950
% with financial income	38.2	41.2	45.7
Median saving rate (%)	12	8.4	1.7
% with PH	19.6	32.3	40.4
Median mortgage (£)	757	968	1,164
% with rental income	9	14.1	27

Note: Values computed relative to the total number of mortgagors in each column, with the exception of the first row.

3.2 Were households hit harder by the lockdown more likely to have accessed PH?

Households with financial vulnerabilities during the first months of the national lockdown, may have relied more heavily on the mortgage PH. To examine this, we replicate Table 3 by using covariates that capture borrowers' financial position in March and April 2020, such as unemployment, negative saving rates, or adverse income shocks relative to normal pre-pandemic levels (Table 5).

We find that having experienced negative income shocks at the start of the first lockdown, or entering the crisis with negative saving rates, increases policy take-up by roughly 3 p.p. and 4 p.p. (columns 2 and 3). Borrowers are not more likely to have a mortgage PH if they have both a negative income shock and negative savings early in the lockdown, since the interaction term in column (4) is insignificant. Hence, only one of these shocks is sufficient to increase the conditional probability of applying for PH.

Table 5: Impact of pandemic shocks on the probability of PH take-up

	Dependent variable: P(PH = 1)			
	(1)	(2)	(3)	(4)
Age	0.0078*	0.0078*	0.0074	0.0075
	(0.0046)	(0.0046)	(0.0047)	(0.0047)
Age sq	−0.0001**	−0.0001**	−0.0001**	−0.0001**
	(0.0001)	(0.0001)	(0.0001)	(0.0001)
Kids flag	0.0037	0.0032	0.0021	0.0021
	(0.0124)	(0.0124)	(0.0124)	(0.0124)
No mortgages > 2	0.1981***	0.1955***	0.1966***	0.1967***
	(0.0300)	(0.0298)	(0.0298)	(0.0298)
Unemp in Mar/Apr 2020	0.0096	0.0098	−0.0017	−0.0017
	(0.0129)	(0.0129)	(0.0139)	(0.0139)
Neg. income shock Mar/Apr 2020 (< -10%)		0.0246**	0.0282**	0.0318**
		(0.0122)	(0.0123)	(0.0135)
Neg. saving rate			0.0357**	0.0422**
			(0.0169)	(0.0199)
Neg. income * Neg. saving rate				−0.0207
				(0.0306)
Share on PH	22.95	22.95	22.95	22.95
Mortgagors (N)	5,086	5,086	5,086	5,086

Notes: The sample includes mortgagors. Robust standard errors in brackets. Constant is not reported. Saving rates and the number of mortgages are calculated pre-crisis. Income shocks are changes in March or April 2020 (depending on which is larger) and pre-crisis levels.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

3.3 What determines the duration of mortgage PH?

Mortgagors could, in principle, choose a mortgage PH for a period between one and six months. For example, after the initial period of three months, if borrowers wanted to continue benefiting from PH, they had to actively request an extension of up to six months. We show in Table 2 that roughly 17% of all mortgagors that got a PH chose a PH for longer than three months. To understand what drove a subset of households to opt to extend their mortgage PH for longer, we examine which household financial characteristics predict a higher probability of extending the policy beyond three months.

Table 6 shows two important results. First, and somewhat surprisingly at face value, the likelihood of having a longer PH duration is negatively correlated with saving rates (column 4). This suggests that PH extensions may have not provided the right support for liquidity-constrained households. If they remained financially vulnerable after the initial PH term, it is plausible that lenders may have decided to offer them more tailored forbearance measures to deal with their continuous liquidity issues.

Second, income is a key driver of mortgage PH duration. The likelihood of PH extensions decreases if households have financial income, and increases if households experienced adverse income shocks during the pandemic (column 5). These results are in line with Haughwout *et al.* (2021b) who also show that US lower-income borrowers were more likely to remain in forbearance for a longer period of time.

Table 6: Determinants of PH duration

	Dependent variable: Prob(PH duration > 3 months)				
	(1)	(2)	(3)	(4)	(5)
Age	0.0009	0.0023	0.0028	0.0034	0.0024
	(0.0094)	(0.0094)	(0.0091)	(0.0088)	(0.0088)
Age sq	−0.00004	−0.0001	−0.0001	−0.0001	−0.00004
	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)
Kids flag	0.0064	0.0065	0.0078	0.0120	0.0074
	(0.0242)	(0.0241)	(0.0237)	(0.0229)	(0.0231)
No mortgages > 2	0.0369	0.0388	0.0365	0.0482	0.0391
	(0.0401)	(0.0405)	(0.0401)	(0.0400)	(0.0398)
Fin. income	−0.0377	−0.0400*	−0.0350	−0.0348	−0.0384*
	(0.0234)	(0.0232)	(0.0230)	(0.0226)	(0.0227)
MDSR	−0.00002				
	(0.00002)				
MDSR Q1		0.0638			
		(0.0394)			
MDSR Q2		−0.0250			
		(0.0357)			
MDSR Q3		0.0480			
		(0.0384)			
MDSR Q4		0.0127			
		(0.0350)			
Income Q1			−0.0178		
			(0.0324)		
Income Q2			−0.0313		
			(0.0356)		
Income Q3			−0.0042		
			(0.0335)		
Income Q4			−0.0220		
			(0.0294)		
Savings Q1				−0.0917***	
				(0.0309)	
Savings Q2				−0.0719**	
				(0.0314)	
Savings Q3				−0.0599*	
				(0.0323)	
Savings Q4				−0.0260	
				(0.0345)	
Unemp in Mar/Apr 2020					−0.0261
					(0.0250)
Neg. income shock Mar/Apr 2020 (< -10%)					0.0533**
					(0.0230)
Neg. saving rate					−0.0378
					(0.0280)
Share on PH with duration > 3m	16.99	16.99	16.99	16.99	16.99
Mortgagors on PH (N)	1,092	1,092	1,150	1,181	1,163

Notes: The sample includes only mortgagors on mortgage payment holidays. Robust standard errors in brackets. Constant is not reported. Unemployment and income shocks show performance during the pandemic. All other financial variables are calculated pre-crisis. Quintile 5 is the baseline in columns 2-4.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

3.4 Is policy take-up linked to uncertainty?

So far we have shown that mortgage PH have benefited households with pre-existing financial vulnerabilities and those with stronger balance sheets. These results suggest that some households may have applied for the mortgage moratorium for reasons other than financial constraints, such as precautionary or uncertainty reasons. While we cannot observe directly whether uncertainty about future outcomes has been a key driver of PH applications for mortgagors with stronger balance sheets, we will try to infer it from the data by examining the timing of mortgage PH.

Table 2 showed that over a half of PHs were granted in the first two months of the pandemic, when uncertainty regarding the short and medium term effects of the health crisis was at its peak. Borrowers applying at the onset of the pandemic may have been more responsive to uncertainty, as the novelty of the pandemic made its economic repercussions hard to predict. Hence, mortgagors who did not experience a negative income shock may have applied for PH out of precautionary reasons, potentially to insure against future income falls.

We investigate this conjecture in Table 7, where we compare mortgagors who applied for a PH at the very start of the crisis, in March or April 2020, with those who applied for a PH later in the year. First, we find that households who experienced a negative income shock in March or April 2020 were 10 p.p. more likely to have applied earlier in the crisis (column 5). Second, pre-crisis income and saving rates matter: being in the top quintile of the income or saving distribution increases early policy access by around 8 p.p. (column 3) and 17 p.p. (column 4) relative to being in the bottom quintile of each distribution. These results suggest that some borrowers who accessed mortgage PH early on had no immediate income or liquidity concerns that could explain an early need for liquidity relief measures. As a result, they may have been driven to apply for PH by economic uncertainty at the onset of the first lockdown. Third, we also find that early policy take-up correlates negatively with being unemployed at the start of the pandemic, presumably because these households were already being supported by pre-pandemic public support measures (such as unemployment benefits). Finally, there is some evidence that mortgagors who had financial income were less likely to have applied for PH early in the pandemic.

Table 7: Timing of successful PH application

	Dependent variable: Prob(PH Application Date < May)				
	(1)	(2)	(3)	(4)	(5)
Age	−0.0167	−0.0133	−0.0100	−0.0117	−0.0110
	(0.0135)	(0.0136)	(0.0131)	(0.0128)	(0.0130)
Age sq	0.0002	0.0001	0.0001	0.0001	0.0001
	(0.0002)	(0.0002)	(0.0002)	(0.0001)	(0.0001)
Kids flag	−0.0693**	−0.0695**	−0.0524*	−0.0398	−0.0627**
	(0.0319)	(0.0317)	(0.0312)	(0.0309)	(0.0309)
No mortgages > 2	0.0027	0.0049	0.0078	0.0252	0.0113
	(0.0495)	(0.0497)	(0.0499)	(0.0489)	(0.0494)
Fin. income	−0.0650**	−0.0661**	−0.0531*	−0.0497	−0.0471
	(0.0320)	(0.0320)	(0.0314)	(0.0311)	(0.0312)
MDSR	−0.00001				
	(0.00003)				
MDSR Q2		−0.0792			
		(0.0493)			
MDSR Q3		−0.0279			
		(0.0483)			
MDSR Q4		−0.0533			
		(0.0467)			
MDSR Q5		−0.1049**			
		(0.0461)			
Income Q2			0.0491		
			(0.0567)		
Income Q3			0.0853*		
			(0.0514)		
Income Q4			0.0187		
			(0.0479)		
Income Q5			0.0804*		
			(0.0451)		
Savings Q2				−0.0039	
				(0.0451)	
Savings Q3				−0.0275	
				(0.0446)	
Savings Q4				0.0155	
				(0.0458)	
Savings Q5				0.1701***	
				(0.0518)	
Unemp in Mar/Apr 2020					−0.0631*
					(0.0341)
Neg. income shock Mar/Apr 2020 (< -10%)					0.0977***
					(0.0300)
Neg. saving rate					0.0382
					(0.0390)
Share on PH with date < May 2020	49.62	49.62	49.62	49.62	49.62
ortgagors on PH (N)	1,092	1,092	1,150	1,181	1,163

Notes: The sample includes only mortgagors on mortgage payment holidays. Robust standard errors in brackets. Constant is not reported. Unemployment and income shocks show performance during the pandemic. All other financial variables are calculated pre-crisis. Quintile 1 is the baseline in columns 2-5.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

4 Identification of causal effects of mortgage PH on household behaviour

In this section we discuss the methods we use to identify the causal effect of mortgage PH on household consumption and savings dynamics. We use three approaches: a difference-in-differences (DiD) research design, a synthetic control method (SCM) and propensity score matching (PSM). The DiD research design is our preferred identification method due to easiness of implementation, but we use the PSM and SCM approaches to provide additional robustness for our DiD results.

4.1 Difference-in-differences research design

Our main approach for identifying the causal effects of mortgage PH relies on a quasi-experimental difference-in-differences (DiD) research design. We estimate a two-way fixed effects DiD model over January-November 2020:

$$\Delta Log(Y_{i,t}) = \alpha_i + \gamma_t + \beta_0 PH_{i,t} + \beta_1 PH_{i,t} \times Int_{i,t} + \phi Int_{i,t} + \delta X'_{i,t} + \varepsilon_{i,t} \tag{3}$$

where $\Delta Log(Y_{i,t})$ is either: i) the year-on-year change in the logarithm of real non-housing consumption; or ii) the logarithm of the saving rate. For consumption, we take year-on-year changes to deal with seasonal patterns in our data. $PH_{i,t}$ takes the value of 1 when mortgagors are on PH for at least one month during March-November 2020 (treatment group), and 0 for the control group. β_0 refers to the DiD estimator, and $\beta_1$is the marginal DiD effect of PH on the dependent variable for a given interaction term, $Int_{i,t}$. In $X'_{i,t}$ we control for the logarithm of household income, and for the percentage change in income in a given month relative to the pre-crisis median point, i.e. between January 2019 and February 2020. Fixed effects α_i control for time-invariant household-specific characteristics, and time-fixed effects γ_t account for unobserved aggregate shocks. We cluster the standard errors at the household level.

Our control group in Eq. (3) is composed of households non-eligible for the policy, i.e. renters and outright owners. This ensures that our DiD design is not biased by the potential self-selection of mortgagors into the policy. FCA (2021) shows that only one in five borrowers applied for a mortgage moratorium, although the policy was open to all mortgagors.

Aside from self-selection issues, it is possible that using mortgagors who never accessed mortgage PH as a control group may provide a better counterfactual for assessing the effect of the policy compared to renters and outright owners. For instance, Cloyne *et al.* (2020) show that mortgagors adjust expenditure significantly more than outright owners or renters following monetary policy shocks, partly due to the heterogeneity in liquid asset holdings across tenure groups. As a result, during stress periods, the behaviour of mortgagors and non-mortgagor may diverge for reasons other than government policy interventions. However, we believe that sample selection issues and omitted variable bias may be too problematic if we were to use mortgagors as a control group in our baseline specification. On the one hand, restricting our sample to mortgagors reduces the size of our control group significantly, since mortgagors account for only 30% of households. On the other hand, and most importantly, we are concerned that unobserved factors, such as financial literacy, may determine the probability of accessing PH among mortgagors, thus significantly biasing our results. In contrast, non-eligible households do not have the option of applying for mortgage deferrals; using them as control thus allows us to eliminate altogether this source of omitted variable bias.[12]

We would like to emphasise two key features of the DiD model in Eq. (3). First, the treatment period and its timing vary across households, and it is temporary, lasting for a maximum period of six months. This contrasts with the standard DiD framework with fixed-effects, where a policy intervention typically takes place at an aggregate level at a given point in time and it is typically permanent (e.g. Angrist & Pischke, 2009). We thus modify our DiD framework to deal with this, by allowing the treatment period $PH_{i,t}$ to be household-specific, referring strictly to the months when mortgage repayments are missing. Following Goodman-Bacon (2021), we exclude from the control group mortgagors who: (a) are treated at a later time in the year; and (b) have already been treated but their PH has expired. We do this to avoid biasing our DiD estimates due to self-selection. For instance, mortgagors who will be on PH in the future may anticipate some consumption expenditures before getting the PH. Adding them to the control group, as is standard in the literature on DiD with fixed effects (Pischke, 2019; Wooldridge, 2007) would bias the coefficient towards zero. Similarly, mortgagors who benefited earlier from the policy may accumulate significant savings during the PH period, which could then be used for consumption after the policy has expired. This would again bias the coefficients downwards.[13] Following Goodman-Bacon (2021), we check in Appendix E how our main results change when we extend our control group to also include early or later treated mortgagors.

[12]In Appendix G we check the robustness of our results by using mortgagors never on PH as a control group.

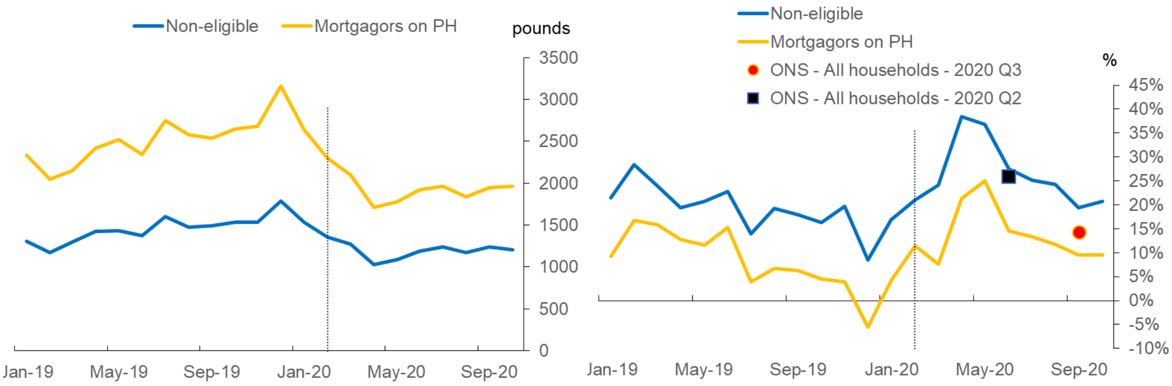
[13]Let us illustrate how we treat the early and the late treatment groups. For instance, take mortgagor A who is on PH from April to June 2020, and mortgagor B who is on PH from July to November 2020. In our baseline setting, estimated over January-November 2020, the pre-treatment period for mortgagor A is January-February, while the treatment period is April-June. Mortgagor A will thus be excluded from the sample in March and in July-November 2020. In turn, the pre-treatment period for mortgagor B is the same, i.e. January-February, and the treatment period is July-November. Mortgagor B will thus be excluded from the sample during March-June 2020.

The second feature of our DiD model is about the duration of the treatment: mortgage PH were designed in a way that allowed mortgagors to be on PH for a period ranging from one to six months. In Section 5 we first focus on average effects across mortgagors, without distinguishing between difference in the duration of the PH. But later we also allow the treatment effect to be conditional on the duration of the PH, when we evaluate the consumption dynamics around the expiration date of the policy.

The validity of our DiD research design rests on the existence of pre-treatment parallel trends between treated and control groups. If this condition does not hold, then changes in household behaviour that we would attribute to the policy could just reflect differential pre-existing trends across groups. Panels (a) and (b) of Figure 2 show pre-crisis parallel trends between treated and non-eligible in consumption and saving rate. Table 8 goes into further detail by showing that the proportion of treated and control groups are similar across many other dimensions, such as: across the bottom of the saving rate and income quintiles (i.e. more financially constrained borrowers); on employment; across regions; and on the share of households with unsecured debt.

Figure 2: Distribution of observables between groups

(a) Trends in monthly non-housing consumption (b) Trends in monthly saving rate[1]



Source: Money Dashboard and Office for National Statistics[1] Note: data updated on 30 September 2021.

We also note, however, that Table 8 illustrates some non-trivial differences across treated and control for the top income quintile and across the age distribution. It is then possible that these latter characteristics may determine different household responses during stress, and thus may bias our consumption estimates. Against this background, we test the robustness of our main DiD results by using two matching techniques for comparing treated and control units: a synthetic control method and propensity score matching. These approaches provide alternative ways of computing a control group for non-eligible which may better reproduce the characteristics of the treated units over time. We will discuss below both of these alternative identification methods.

Table 8: Pre-crisis characteristics for non-eligible and treated households

	% of non-eligible	**% of treated**
Bottom saving rate quintile	21.5	18.6
Bottom saving rate quintile and no financial income	21.3	18.5
Median saving rate quintile	17.7	24.8
Bottom income quintile	21.2	13.4
Median income quintile	18.0	16.2
Top income quintile	10.9	29.8
With personal loans	11.9	10.1
$Age < 30$	34.2	11
$Age < 30 \leq 50$	47.7	74.2
In South England (e.g. London, SE, SW)	51.4	50.7
Employed (incl. on job retention scheme)	57	68.8

Notes: Treated refers to mortgagors that will be on PH for at least one month during March-November 2020. Non-eligible refer to renters and outright owners.

4.2 Synthetic control method

The DiD method assumes that all untreated units are equally important to determine the counterfactual and, as a result, each untreated unit receives equal weight in putting together the control group. The syntethic control method (SCM) provides an alternative way to compute the control group using a weighted combination of untreated units. This approach may provide a better comparison for the treated than any single untreated unit alone (Abadie *et al.* , 2010).

The SCM extends the standard DiD estimator by allowing the effect of unobserved confounding factors on the outcome variable to vary over time. This is an important advantage since the standard DiD estimator restricts the effect of those confounders to be constant over time, so that they can be eliminated by taking time differences. The SCM uses a data-driven algorithm for each treated unit to compute an optimal control from a weighted average of potential candidates not eligible for the mortgage PH. The weights for each untreated unit depend on how well they reproduce the characteristics of the treated unit during the pre-treatment period. The outcomes for the synthetic control are then projected into the post-treatment period using the computed weights from the pre-treatment period. This is the counterfactual for the treated unit. In other words, the counterfactual shows how consumption would evolve in the absence of the PH policy. Moreover, since the weights can be restricted to be positive and sum to one, the SCM provides a safeguard against extrapolation (Abadie *et al.* , 2010).[14]

Our framework adds several dimensions to the standard SCM: (i) we have multiple treatment units; (ii) treatment occurs at different points in time; and (iii) the treatment is not permanent, i.e. it turns offs when the PH expires. To deal with (i) and (ii) we resort to an extension of the original SCM, developed by Cavallo *et al.* (2013).[15] In their paper, Cavallo *et al.* (2013) develop an algorithm that considers the impact on economic growth of catastrophic natural disasters across countries and at different points in time. Cavallo *et al.* (2013) fit separate synthetic controls for each treated unit and for each treatment period. The algorithm then averages across treatment units, and for each treatment period. By averaging over the treatment units, the procedure smooths out the noise in the estimate.[16] To deal with (iii), we select a fixed period over which the treated units are being treated. As a result, we focus our analysis on households who got PH for three months.

Our sample in this exercise is different to the one used for the DiD analysis. The SCM requires strongly balanced panel data, and non-missing data for the outcome variable. As such, we drop households for whom data on income and consumption are missing for at least one month in 2019. We also drop outliers from the treatment group for households whose monthly consumption is smaller than £280 or larger than £9,000. These are stronger assumptions than in our DiD baseline, which lead to a large decline in the number of observations. In addition, we have to restrict further our control group due to computational limitations of the SCM algorithm; we drop households whose age, income, or consumption fall outside the range of values of the treatment group. Overall, we end up with 55 households in the treatment group, and 514 households in the potential control pool.

To select our set of predictors and to evaluate their performance, we divide the pre-treatment period into an initial training period and a subsequent validation period, following Abadie (2021). After computing the weights from the training period, we use the validation sample to evaluate if our predictors minimise the mean squared prediction error (MSPE) of the outcome variable during the pre-treatment period. For each treated unit we use the last five months before treatment as the validation period, while the training period uses all the months that precede the validation period. We find that using several data

[14]In their seminal paper, Abadie *et al.* (2010) apply the SCM to a large-scale tobacco law introduced in California in 1988. It compared how cigarette sales per capita evolved in the treated unit (California) after the policy was introduced relative to a synthetic control constructed from all the other US states that did not implement a similar policy. Any differences post-treatment are attributed to the policy.

[15]We use the Stata code synth_runner made available by Galiani & Quistorff (2017).

[16]Abadie *et al.* (2010) suggest an alternative procedure for research designs with multiple treated units. Specifically, they suggest to combine the treated units first and then treat them as a single unit affected by the intervention. We prefer the method developed in Cavallo *et al.* (2013) since it may arguably provide a better counterfactual by making full use of the heterogeneity at the household level.

points for the outcome variable is able to generate a similar MSPE as larger models that also included age and income. Using just the outcome variable as a predictor is in line with Cavallo *et al.* (2013) and Abadie (2021), who argue that sometimes only one pre-treatment variable, particularly the outcome variable, is enough to ensure a good fit for the SCM. More specifically, we use the level of consumption in 2019m3, 2019m6, 2019m9, 2019m12, and 2020m1 as our pre-treatment predictors.

4.3 Propensity score matching

Another alternative identification of the treatment effect of mortgage PH is through propensity score matching (PSM). Estimating the model using PSM involves two stages. In the first stage, we estimate propensity scores $p(x)$ such that:

$$p(x) = Pr(PH = 1 \mid X) \tag{4}$$

where the probability of mortgage PH is obtained by running a probit model similar to the one in Section 3. The main difference is the inclusion of non-eligible households to the sample. To allow propensity scores to be non-zero for the non-eligible, we include only non-mortgage related covariates. This allows us to estimate the probability that the non-eligible would have received a PH, if they had had access to the policy. Observables X include: the logarithm of income; pre-crisis savings (both the pound amount and the saving rate); income shocks in March or April 2020 (depending on which is largest), relative to pre-crisis normal income levels; age; age squared; a dummy for being in the lowest quintile of the saving rate pre-crisis (i.e. being liquidity-constrained); a dummy for being in the lowest quintile of the pre-crisis normal income distribution; a dummy for having children; and a dummy for having positive financial income pre-crisis. Pre-crisis periods for computing variables are the same as in Section 3. As a result, in the first stage we control for selection bias into treatment by conditioning on observed covariates that determine policy take-up.

In the second stage, we compute the average treatment effect on the treated (ATT), which represents the difference between the mean observed outcomes for treated and untreated when $PH = 1$:

$$\widehat{ATT} = E[\{E(Y \mid PH = 1, p(x)) - E(Y \mid PH = 0, p(x))\} \mid PH = 1] \tag{5}$$

where Y is the year-on-year change in the logarithm of real non-housing consumption. To match on propensity scores, we use the nearest-neighbor estimator with and without replacement of matched control units:[17]

$$ATT = \frac{1}{N^T} \sum_{i=1,T} \left[Y_i^T - \sum_{j=1,c} w_{ij} Y_j^c \right] \tag{6}$$

where N^T is the number of treated units, c is the set of controls matched to the treated unit i, and $w_{ij} = \frac{1}{N^c}$ is the weight placed on each control matched to the treated unit i, where the total number of matched controls is N^c.[18]

5 Consumption effects of mortgage PH

In this section, we use the methods described above to identify the causal effect of mortgage PH on household consumption.

5.1 Overall consumption response across the distribution of mortgagors

To examine the effect of PH on mortgagors' consumption, we start by running our main identification strategy based on the DiD research design, as shown in Eq. (3). We use January-November 2020 data and the year-on-year change in the logarithm of real non-housing consumption as the dependent variable. Table 9 reveals three important results.

First, we find no statistical evidence that households on PH changed their consumption relative to the control group (column 1). This suggests that the average household took PH for reasons other than financial constraints, consistent with the results from Section 3. In the absence of financial constraints, households may already be consuming optimally and hence accessing PH may not affect their consumption dynamics.

Second, PH supported consumption of low-savings mortgagors, who are arguably most likely to be liquidity-constrained. These results are robust to different measures of liquidity constraints, such as using: a dummy variable if the household is in the first quintile of the pre-crisis saving rates (column 2); the pre-crisis saving rates used linearly (column 3); or a dummy variable for being in the first quintile of the pre-crisis income distribution (column 4). By providing temporary liquidity relief, our estimates suggest

[17] See Grotta & Bellocco (2014).

[18] Appendix H shows the performance of the matching approach. Overall, we find that the matching significantly reduces the bias in observables across treated and control groups, while also ensuring an acceptable range of common support.

that the policy was able to support the consumption of households who needed it the most. This result is in line with Ganong & Noel (2020), who find that households with negative housing equity who benefited from mortgage payment reductions through the 2009 US HAMP scheme, had lower defaults and higher consumption.

Third, in addition to a strong impact on liquidity-constrained borrowers, we also find that PH helped households smooth consumption following negative income shocks during the pandemic (column 5). Specifically, PH allowed mortgagors who experienced large negative income shocks in March or April 2020, to maintain a higher consumption growth relative to the control group.

In contrast, we do not find any evidence that PH had a differential impact on consumption if households entered the pandemic already unemployed (column 6).

Table 9: Average treatment effect of payment holidays

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
PH	1.398	-0.709	0.471	-0.290	1.511	0.412	-0.924	-1.480	-1.372
	(2.969)	(3.126)	(3.020)	(3.152)	(2.922)	(3.137)	(3.057)	(3.105)	(3.157)
PH*Liq		18.878**					22.427**	17.787*	20.139*
		(8.666)					(8.804)	(10.730)	(10.580)
PH*Srate			-1.362*						
			(0.801)						
PH*Low inc.				14.946*				9.638	11.920
				(8.246)				(10.229)	(11.333)
PH*Inc.shock					-0.021*		-0.033**	-0.034**	-0.037**
					(0.013)		(0.015)	(0.014)	(0.015)
PH*Unemp.						7.854			-3.492
						(8.880)			(9.965)
Controls	✓	✓	✓	✓	✓	✓	✓	✓	✓
User FE	✓	✓	✓	✓	✓	✓	✓	✓	✓
Time FE	✓	✓	✓	✓	✓	✓	✓	✓	✓
Observations	54,665	54,665	54,665	54,665	52,745	53,805	52,745	52,745	51,917
Adjusted R^2	0.084	0.084	0.084	0.084	0.085	0.084	0.085	0.085	0.085

Notes: The dependent variable is the change in log real non-housing consumption. Additional control variables are omitted from the table. All the variables in the table, with the exception of the treatment and the income shock, are measured pre-crisis. Standard errors in parentheses are clustered at the household level.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Our main result that PH seemed to have supported the consumption of more financially vulnerable households remains strongly robust to including several interaction terms simultaneously (columns 7 to 9). Our preferred specification in column (7) indicates that PH allowed liquidity-constrained mortgagors to have a 22 p.p. higher year-on-year real consumption growth relative to the control group. Furthermore, treated borrowers who had a higher negative income shock by 10 p.p. during the pandemic had 0.3 p.p.

greater consumption growth compared to the the non-eligible.[19]

In Appendix E we show that the results of our preferred specification in column (7) remain strongly robust, especially for liquidity-constrained mortgagors, to a wide range of alternative specifications, such as: i) controlling for local and regional shocks by adding country fixed effects (England, Scotland, Wales, and Northern Ireland), country-by-time fixed effects, region fixed effects, region-by-time fixed effects, postcode district fixed effects, and postcode-by-time fixed effects; ii) controlling for anticipation and delayed effects of the policy; and iii) using the Goodman-Bacon (2021) decomposition to examine the impact of allowing treated mortgagors to be part of the control group when they are not treated.

As discussed in Section 4, we provide further robustness checks on our DiD estimates by recomputing the analysis in Table 9 with two alternative identification methods of the causal impact of mortgage PH on household consumption.

First, we show that results remain consistent when we use the SCM to compute causal effects. Under the SCM, we track how consumption evolves before, during, and one month after PH expires for the treatment group versus the synthetic control group. The outcome variable for the SCM is the level of real non-housing consumption, instead of the year-on-year change in the logarithm of consumption used in Eq. (3). We use levels instead of growth rates because the SCM requires a large pre-intervention window over which to construct the synthetic control. Since our dataset starts in January 2019, using year-on-year consumption growth would only give us two pre-treatment periods (January and February 2020) for those mortgagors on PH since March 2020. Therefore, using the consumption level allows us to have at least 14 pre-treatment periods. Figure 3 shows that the SCM delivers very similar results to the DiD: we do not find any evidence that the average household on PH changed consumption differently to the non-eligible. But when we restrict the sample to liquidity-constrained households, we find that their consumption is higher relative to the synthetic control group during the three-month PH window (Figure 4).[20] In particular, at the end of the three months, real non-housing consumption of the treated is about 29% higher than when the PH started. Over the same period, consumption for the synthetic group increased only by 9%.[21] This suggests that households on PH for three months were able to enjoy 20 p.p.

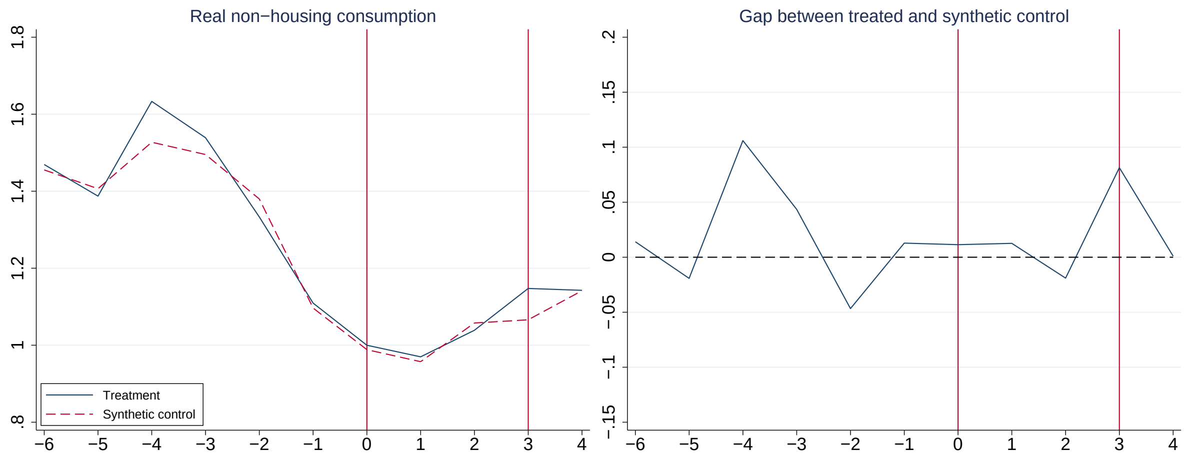
[19]Given the large standard deviation of income shocks, of 303.68, the coefficient becomes easier to interpret when we multiply it with its sample standard deviation: mortgagors that stand at a one-standard deviation below the sample mean of income shocks are associated with higher consumption growth of about 10 p.p. ($0.034 \times 303.68 = 10.3$).

[20]To increase the sample size, we define liquidity-constrained households as those belonging to the first two quintiles of the saving rate distribution. We end up with 28 households in the treatment group and 448 in the control pool.

[21]We normalise the level of consumption to deal with scale differences in the outcome variable. Since we match each treated unit with its synthetic counterpart using the level of real consumption, this means that the differences we find in the effect of PH between the treatment group and the synthetic control group will be dependent on the level of consumption. In this way, we face a scale effect, whereby the same increase in the level of consumption has a disproportional effect on households with the lowest

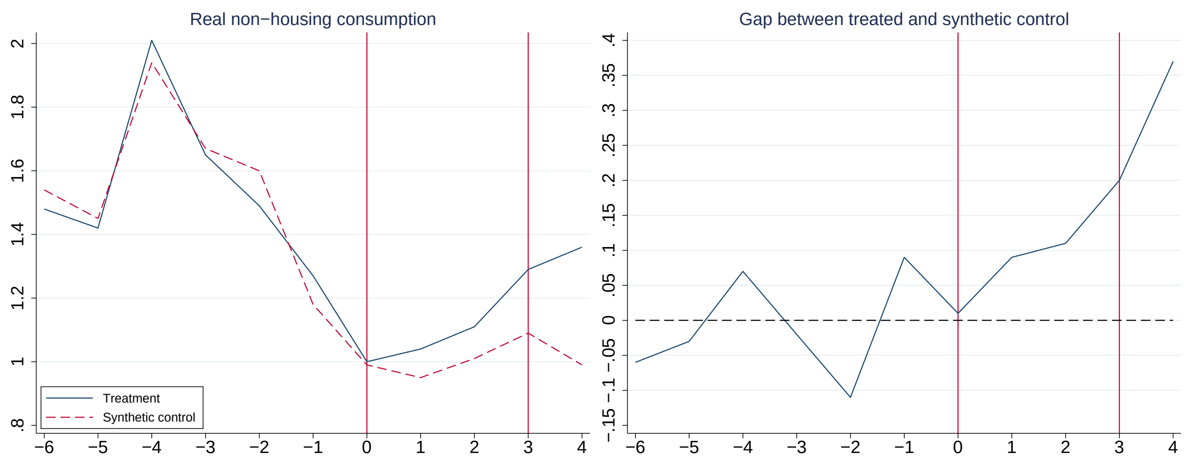
higher consumption relative to a counterfactual scenario in which there were no PH in place (right panel of Figure 4).

Figure 3: Synthetic control method



Notes: Evolution of real non-housing consumption (normalised to 1 in period 0). The treatment group (solid blue line) refers to households on PH for three months, while the synthetic control group (dashed red line) is made up of the non-eligible to the PH policy. The vertical lines represent the start of the PH (x-axis at 0) and the end of the three-month period (x-axis at 3).

Figure 4: Synthetic control method: liquidity-constrained households



Notes: Evolution of real non-housing consumption (normalised to 1 in period 0). The treatment group (solid blue line) refers to households on PH for three months, while the synthetic control group (dashed red line) is made up of the non-eligible to the PH policy. The vertical lines represent the start of the PH (x-axis at 0) and the end of the three-month period (x-axis at 3).

It is also interesting to note that the gap between the two groups continues to widen in the subsequent month after the expiration of the policy (i.e. at $t = 4$). In Appendix F, we describe our placebo tests, following Abadie *et al.* (2010) and Cavallo *et al.* (2013), to illustrate the statistical significance of the

consumption level. Given these scale effects, we have to normalise the estimates before averaging the coefficients so as to come up with the average effect of the PH. In particular, we normalise the level of consumption of the treatment unit to be equal to 1 in the first month of the PH.

estimated effects. We show that the estimated effects of PH on liquidity-constrained households are statistically significant at the 10% level from the second month of the PH. This reinforces our main baseline results that PH seemed to have helped liquidity-constrained households smooth consumption relative to the non-eligible. In addition, Figures 13 and 14 in Appendix F show that the SCM results remain consistent when we use the log of consumption as the outcome variable.

Our second robustness check for the DiD estimates relies on identification via propensity score matching. Panel (A) of Table 10 shows the average effect of the policy on year-on-year consumption growth, relative to the control group, under each of the two nearest-neighbor estimators. We again find that mortgage PH have not changed the consumption behaviour of the average mortgagor, in line with our previous findings. Panel (B) of Table 10 shows a large positive effect of mortgage PH on the consumption growth when the sample is restricted to liquidity-constrained mortgagors: those on a mortgage PH have a 20 p.p. higher consumption growth compared to those not eligible for the policy. These results are strongly consistent with the DiD effects in both direction and magnitude, as shown in column (3).

Table 10: Consumption effects using PSM

$\Delta \log C$	(1) NN	(2) NN no repl	(3) Baseline
A. Average across sample			
	-2.121	-2.754	-0.924
	(2.837)	(2.550)	(3.057)
Observations	40,702	40,702	52,745
B. Liq. constrained only			
	22.120***	20.800**	22.427**
	(9.297)	(6.758)	(8.804)
Observations	3,429	3,429	52,745

Notes: Estimation methods in columns (1) and (2) use a caliper of 0.2. When the entire sample is used, the matched sample for the nearest-neighbor with replacement includes 2,072 treated units and 1,883 control units comprised of non-eligible households. Propensity scores range from 0.0001 to 0.39. When sample is restricted to liquidity-constrained borrowers, the matched sample for the nearest-neighbor with replacement includes 245 treated units and 220 control units comprised of non-eligible households. Propensity scores range from 0.001 to 0.72. Bootstrapped standard errors in parentheses. The baseline is copied from column (7) in Table 9.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

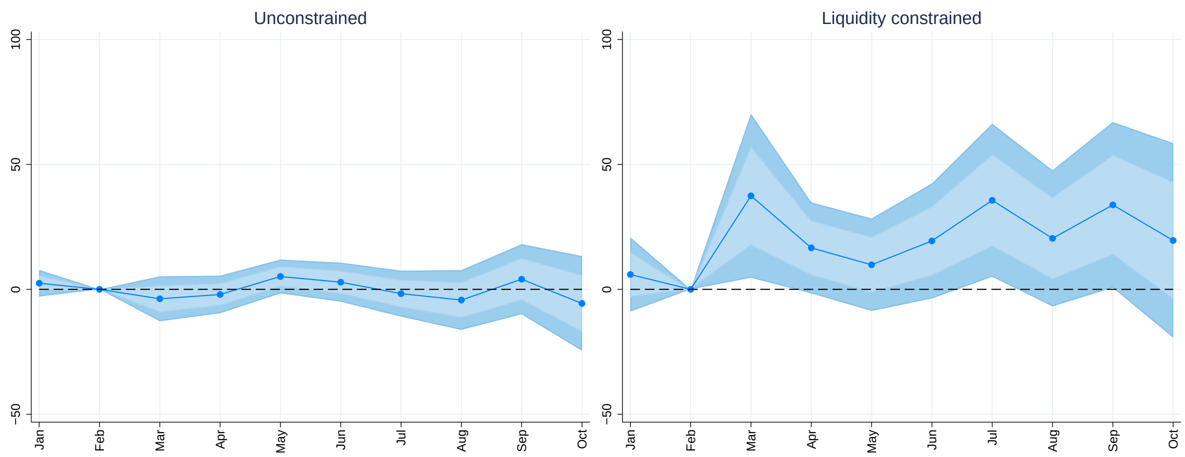
5.2 Monthly consumption response across the distribution of mortgagors

To better understand the consumption effects of mortgage PH on borrowers, we also estimate coefficients for each month of the treatment period. This allows us to examine when the PH had the strongest effect on liquidity-constrained households. We estimate the following specification:

$$\Delta LogC_{i,t} = \alpha_i + \gamma_t + \sum_{t \neq Feb20}^{Oct20} 1_{time=t} \times [\beta_{0,t} PH_{i,t} + \beta_{1,t} PH_{i,t} \times Int_i] + \delta X'_{i,t} + \varepsilon_{i,t} \tag{7}$$

where $\beta_{0,t}$ measures the average year-on-year real consumption growth differential between mortgagors on PH and the control group, for month $t \in [Jan20, Oct20]$ relative to February 2020 (the base month). Int_i stands for the interaction term for liquidity-constrained mortgagors. The left panel of Figure 5 shows the monthly estimates for unconstrained mortgagors, whereas the right panel shows the effects for liquidity-constrained mortgagors. Similarly to before, we do not find that the average mortgagor on PH increased consumption expenditures relative to the non-eligible in any month since PH were introduced. In contrast, PH allowed liquidity-constrained households to smooth the impact of the pandemic shock on consumption, with coefficients statistically significant at the 10% level in March and July 2020. These two months correspond to the introduction of the policy and to its extension by the government.[22]

Figure 5: Response of non-housing consumption for households on PH vs non-eligible



Notes: Response of year-on-year real non-housing consumption growth relative to February 2020 (base month) for households on PH relative to those not eligible for the policy (renters and outright owners). The blue areas refer to the 68% and 90% confidence bands.

[22]The policy announced on 31 June 2020 extended the deadline for applications, as well as the duration of the mortgage moratoria from a maximum of three months to six months.

5.3 Consumption response at the policy expiration date

So far we examined how mortgagors behaved when the mortgage PH were active. But another important question is to understand how households reacted when the policy expired. Vihriälä (2021) shows that consumption dropped after the expiration date of mortgage PH when it was offered by one commercial lender in Finland in 2015. He argues that liquidity-constrained households over-consumed during the PH period, which may explain why households do not behave according to rational models when they face financial constraints.

To examine the consumption response at the expiration date, we estimate the following model:

$$Log(C_{i,t}) = \alpha_i + \gamma_t + 1_{treated} \sum_{t=end-j}^{end+j,t\neq end} 1_{time=t} \times [\beta_{0,t} PH_{i,t} + \beta_{1,t} PH_{i,t} \times Int_i] + \delta X'_{i,t} + \varepsilon_{i,t} \tag{8}$$

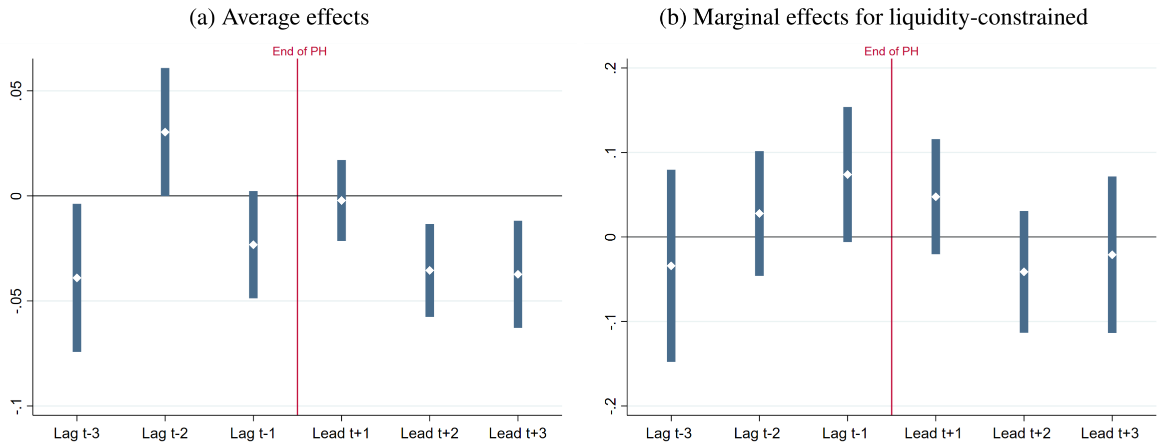
where $Log(C_{i,t})$ is the log of real non-housing consumption; *end* represents the last month that mortgage PHs are active (i.e. the omitted baseline month); $\beta_{0,t}$ measures the difference in average consumption for mortgagors on PH relative to the baseline month and relative to the control group in a period window $[-j, j]$; $\beta_{1,t}$ measures the marginal DiD effect for interaction terms. In $X'_{i,t}$, we control for log household income and the change in income in a given month relative to the median point between January 2019 - February 2020. Fixed effects α_i control for time-invariant household-specific characteristics, and time-fixed effects γ_t account for unobserved aggregate shocks. We cluster the standard errors at the household level.[23] The lags, $-j$, are computed backwards from the baseline month. This implies that we will not estimate lagged coefficients for mortgagors whose PH lasted only one month. Mortgagors whose PH lasted two months will have only one estimated lagged coefficient, and so on. But for all the mortgagors, we are able to observe the consumption behaviour in the three months following the expiration of the policy.

Figure 6 shows the consumption dynamics for mortgagors on PH, relative to the expiration month (i.e. the baseline). Panel (a) shows that the consumption effects are small and very close to zero for the average mortgagor, relative to the baseline month and to the control group. However, for liquidity-constrained households, we find some indication that relative to the expiration date, the consumption effects are positive in the first month after mortgage payments resume, as well as in the months preceding the

[23]The main difference in Eq. (8) compared to Eq. (3) is the dependent variable. Previously, we used the year-on-year change in log real consumption to address issues of seasonality and non-stationarity. Using the same specification in this exercise would complicate the interpretation of coefficients, as it would require triple-differencing: i.e. $\beta_{0,t}$ would measure the difference in yoy consumption growth for the treated, relative to the baseline month and relative to the control group. To make the interpretation easier, our dependent variable is the log level of consumption.

expiration date (panel b). Although the estimates are surrounded by large standard errors, our result offers tentative evidence consistent with what we found with the SCM in Section 5.1.

Figure 6: Consumption dynamics around expiration date

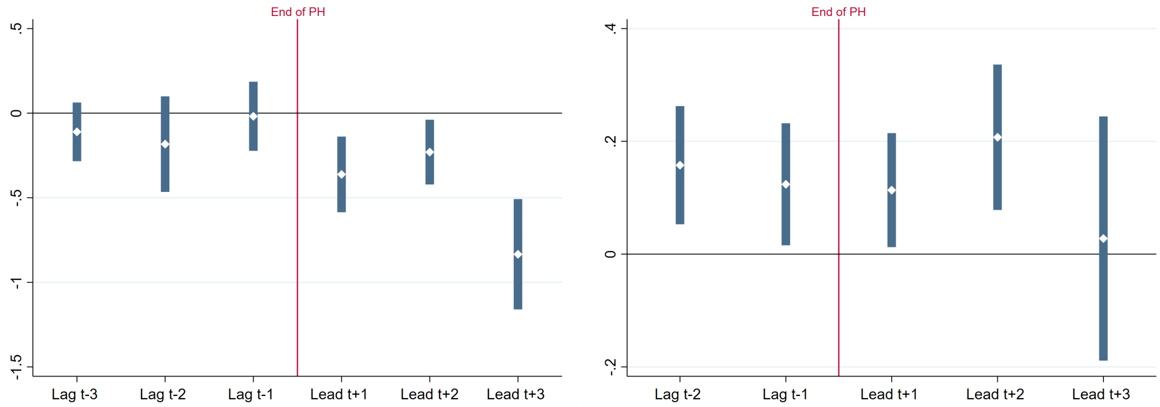


Note: The figures show the response of log real non-housing consumption relative to the last month of PH (base month) for mortgagors who accessed the policy relative to those not eligible for the policy (renters and outright owners). The dark blue bars refer to the 90% confidence bands. The regression includes controls, and user and time fixed effects. Standard errors clustered at the household level.

These results are at odds with Vihriälä (2021), who found the opposite effect – i.e. that mortgagors on PH reduced consumption after the policy ended, with the effects accentuated for households entering the policy with negative liquid assets. We check if this difference is driven by the choice for the mortgage PH duration. While thus far we ignored the length of the mortgage PH in our estimates, Vihriälä (2021) focuses only on households choosing a six-month PH. To understand if the length of mortgage PH is important in determining consumption dynamics, we compare the effects for those on three and six months payment holidays. The results are shown in Figure 7. Panel (a) shows that we obtain similar results to Vihriälä (2021) when we condition on a PH duration of six months for liquidity-constrained households: i.e. consumption is negative in the months when mortgage payments restart relative to the baseline month and to the control group. Panel (b), however, shows that this effect disappears when we look at borrowers on a shorter policy duration. These results suggest that the duration of a temporary liquidity relief policy could matter for consumption dynamics: it may be possible that a longer duration may lead households to over-consume while the policy is active, which may force them to under-consume when the mortgage payments restart. While our setup does not allow us to study what causes this phenomenon in detail, we believe that it may be driven by households' financial position. For instance, as we have shown in Table 6, negative income shocks are correlated with a longer PH duration. As such, losses in income during the pandemic may have further tightened financial constraints for some mortgagors who were already liquidity-constrained. These households would then have an incentive to have a mortgage PH for longer to be able to cope with their mortgage commitments. But once policy expires, struggling households hardest hit by income shocks would need to under-consume to keep their mortgage payments current.

Figure 7: Consumption dynamics around expiration date by PH duration

(a) Liquidity-constrained: PH duration = 6 months (b) Liquidity-constrained: PH duration = 3 months



Note: The figures show the response of log real non-housing consumption relative to the last month of PH (base month) for mortgagors who accessed the policy relative to those not eligible for the policy (renters and outright owners). The dark blue bars refer to the 90% confidence bands. The regression includes controls, and user and time fixed effects. Standard errors clustered at the household level.

5.4 Who are the financially constrained mortgagors whose consumption benefited most from mortgage PH?

Our key result so far has been that mortgage PH have primarily affected the consumption behaviour of financially constrained mortgagors. Table 11 sheds more light on the characteristics of these borrowers. Among those who accessed mortgage PH, liquidity-constrained borrowers have substantially higher DSRs, rely more heavily on personal loan finance, and are four times more likely to be unemployed relative to unconstrained households. In addition, constrained mortgagors entered the crisis with notably lower monthly incomes. However, they experienced smaller income shocks during the pandemic. This presumably reflects generous support from the furlough scheme introduced by the UK government at the end of March 2020.

These results suggest that liquidity-constrained mortgagors who benefited from mortgage moratoria were more likely to be financially vulnerable before they entered the pandemic. The temporary liquidity relief thus supported their consumption during the pandemic. However, only 12% of liquidity-constrained households opted to extend PH beyond three months, compared to 18% of unconstrained borrowers.

Table 11: Financial characteristics of households on mortgage PH

	Liquidity-constrained	**Unconstrained**
Share of mortgagors (%)	18.7	81.3
Mortgage DSR pre-crisis (median)	112	23.6
Mortgage repayment pre-crisis (median)	£1,003	£862
Saving rate pre-crisis (%)	-6.8	17.5
Personal loan finance pre-crisis (median)	£2,500	£1,425
% with personal loan fin pre-crisis	13.5	9.8
Income pre-crisis (median)	£1,566	£3,979
Unemployed pre-crisis (%)	60	14
% in 1^{st} quartile of income pre-crisis	47	7.9
% with fin income pre-crisis	57	75
Income shocks in March/Apr 2020 (%)	-5.8	-8.7
% with PH <= 3m	87.4	82
% with PH > 3m	12.6	18

Note: Values calculated relative to total number of mortgagors in each column, except for the first row which computes the shares out of all mortgagors. Pre-crisis values computed between January 2019 and February 2021 for mortgagors on mortgage PH for at least one month between March - November 2020. The last three rows show values during the pandemic. Income shocks are computed as changes in March or April 2020 income (depending on which is larger) relative to pre-crisis income.

Although somewhat counter-intuitive, this may have been driven by a number of factors. First, the UK furlough scheme may have attenuated income shocks, providing an alternative to longer duration mortgage PH. Second, it is possible that lenders were more inclined to extend mortgage PH beyond three months for borrowers with continued but temporary financial difficulties. For those with more structural vulnerabilities which are unlikely to be resolved in the medium-term, such as persistent unemployment or high debt-service ratios, lenders may have offered other forbearance options instead of extending the initial mortgage PH. This would be in line with the FCA guidance which urges lenders to provide alternative support to borrowers still struggling following the initial mortgage deferrals, such as modifying interest or capital repayments, or re-negotiating the repayment plan.[24]

5.5 What did the average unconstrained mortgagor do with mortgage PH?

In the previous section we consistently showed that the average mortgagor on PH did not exhibit a statistically different consumption behaviour relative to the control group. Instead, we showed that the policy played an important role in bolstering consumption of mortgagors facing liquidity constraints. In this context, a question remains on how the average mortgagor used her temporary liquidity relief. To investigate this, we re-run Eq. (3) with the saving rate as the dependent variable. Unfortunately, Money Dashboard does not provide enough comprehensive data on unsecured debt finance to examine how mortgage PH have affected demand for alternative sources of credit. To get a clearer picture of household

[24] See https://www.fca.org.uk/news/press-releases/fca-highlights-continued-support-consumers-struggling-payments.

access to credit card finance, one would need to observe the monthly unsecured debt balance, including credit card limits. These data are unavailable in Money Dashboard since it provides information about flows of funds and not about outstanding amounts.

Table 12 shows the causal effect of mortgage PH on the saving rate in column (2).

Table 12: Impact of PH on household balance sheets

	(1) Consumption growth	(2) Sav rate
PH	-0.924	0.194***
	(3.057)	(0.044)
PH * Liquidity constrained	22.427**	0.131
	(8.804)	(0.214)
PH * Inc shock	-0.033**	0.000
	(0.015)	(0.001)
Observations	52745	39450
Adjusted R^2	0.085	0.073

Notes: The dependent variables, all expressed in logarithms, are shown at the top of each column. Standard errors in parentheses are clustered at the household level.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

For comparison, we show our baseline estimate for the real consumption growth in column (1), taken from column (7) in Table 9. We find that saving rates are nearly 20 p.p. higher for the average unconstrained household on mortgage PH relative to the control group. In contrast, as discussed before, liquidity-constrained mortgagors in column (1), use their temporary liquidity relief from mortgage PH for consumption instead of savings purposes.

6 Conclusion

In this paper we use detailed transaction-level data from Money Dashboard to study whether mortgage PH can act as a mechanism for smoothing household consumption following negative aggregate shocks. In particular, we have examined the effect of PH on consumption for mortgagors with low savings, who tend to be more financially vulnerable.

We show that households with pre-existing financial vulnerabilities, such as a high mortgage DSRs and low saving rates, were more likely to have accessed mortgage PH. But we also show that mortgagors may have resorted to PH for reasons unrelated to financial vulnerability, as evidenced by a high take-up from buy-to-let investors and from those with stronger balance sheets. In addition, we find robust evidence that PH allowed households with low saving rates to smooth their consumption during the pandemic, relative to a control group comprised of those non-eligible for the policy – renters and outright owners.

Given that our data stops in 2020, our work focuses mainly on the short-term effects of the PH policy. But we have also provided some tentative evidence of the medium-term effects of mortgage PH on the consumption of liquidity-constrained mortgagors. On the one hand, we have shown that constrained households who accessed PH for six months tended to adjust their consumption downwards when the policy expired. This behaviour, potentially caused by self-control issues, as in Vihriälä (2021), may reduce the longer-term impact of the policy on consumer spending, as the positive consumption effects that accrue while the policy is active are reduced when mortgage repayments resume. On the other hand, we have shown that constrained households on shorter PH maintain similar consumption patterns when the policy ends. This points to a more prolonged effect of the policy on spending for these borrowers.

Our results also show that households with stronger balance sheets have not adjusted their consumption expenditures, relative to the control group, when they accessed mortgage PH. Instead, these households used the policy to boost their savings. It is an open question whether these extra savings will be used to bolster consumption in the aftermath of the pandemic. If these additional savings were to remain unused throughout the pandemic due to prevailing uncertainty about future outbreaks, then we would expect the average effects of mortgage PH on aggregate consumption to remain limited. In this context, the ability of mortgage payment deferrals to act as a tool to help mortgagors smooth consumption may be confined to the subset of liquidity-constrained households.

Overall, we believe our work provides encouraging signs about the ability of mortgage payment deferrals to aid household balance sheets through difficult times by supporting their consumption. As a result, it is possible that mortgage PH had broader utility beyond their main objective of helping struggling mortgagors with their payments. By supporting their consumption during the pandemic, mortgage PH may have helped avoid the repetition of the 2007-09 Great Recession, when unemployment and arrears increased dramatically as a result of a collapse in aggregate demand. During the 2008-09 recession, mortgagors, especially those highly indebted, cut consumption to a larger extent. The fall in aggregate demand had important ripple effects, leading to a wave of defaults that amplified the ongoing shock in the financial system. During the pandemic, however, arrears remained at historically low levels in the UK, suggesting that mortgage PH may have been successful in keeping households current on their mortgages. Nonetheless, in this paper we have not examined the impact of mortgage PH on mortgage arrears or defaults. This remains an open and an important question for further research.

Appendix

Appendix A. Summary of data cleaning

We take a number of steps to clean the data. First, we only include users who registered before March 2020 to eliminate the risk that households signed up to MDB because of the Covid-19 shock. We also remove debit transactions without tags, since we cannot identify the payment type. We also require users to have at least £200 in debits in all months between January–November 2020, and to have at least 5 transactions per month in all months but one in 2020, and in all months but two in 2019. Although we end up with an unbalanced panel, we have the advantage that our sample includes only those who consistently use MDB. All users in our data are at least 18 years old.

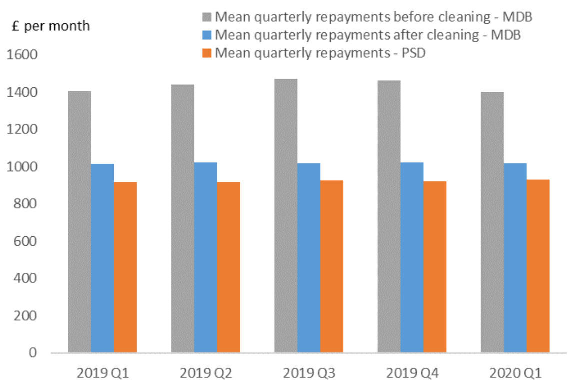
Next, we drop users with business accounts. Spending on these accounts may reflect business costs that should not be treated as consumption, and their credit amounts may be a combination of gross revenues and gross income. This is in contrast with salary-employed users, for whom we can only observe net salary income, as paid directly by their employers after PAYE. Business accounts are likely linked with users who are fully or partially self-employed, and by excluding them, we focus our analysis on salary-employed individuals. To ensure self-employed are reliably removed from the data, we exclude users who have more than three different jobs per month. This selection criterion allows us to capture families where the head of household, her partner, and another household member are all employed.

Third, we remove extreme outliers from our sample. For instance, we drop debit amounts larger than £50,000 and credit card expenses larger than £10,000. We also drop outliers in spending and income for the bottom and top 1%. The Online Appendix describes in more detail the steps we take to clean the dataset.

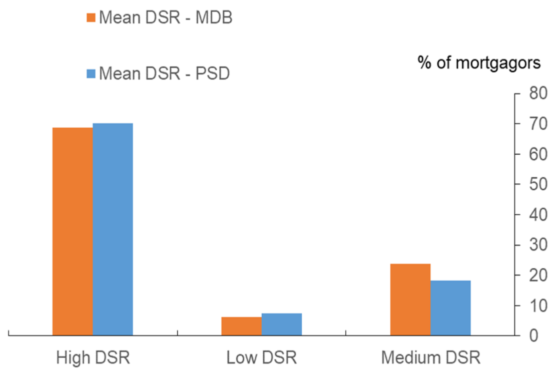
Finally, we identify mortgage payments that are regular and consistent across months. Although mortgage payments are clearly tagged in MDB, some entries may not refer to contractually monthly payments that borrowers have to make to avoid going into arrears. They are most likely transfers between the same user's accounts within the month or are early mortgage repayments, mortgage fees or other penalties which tend to be infrequent. Due to the inconsistent nature of these payments, we disregard them to avoid mistakenly labeling them as mortgage payment holidays later in the analysis. Panel (a) of Figure 8 shows the mean quarterly mortgage repayments in MDB, before and after identifying consistent mortgage payments. We compare them against the Product Sales Database (PSD), which has information on all completed owner-occupied mortgage product originations in the UK on a quarterly basis. The Figure shows an important overestimation of mortgage payments in MDB before we apply thee the aforementioned cleaning. After identifying consistent mortgage payments, the MDB data become comparable on average to the information obtained from PSD. Panel (b) goes a step further by comparing mortgage DSR in MDB to those in PSD for those mortgages we identify as consistent. It is reassuring to see that there is no substantial difference in mean DSRs across the two datasets.

Figure 8: Mortgage payments

(a) Mortgage repayments



(b) Mortgage debt-service ratios$^{(a)}$



Source: Product Sales Database and Money Dashboard. Note: The PSD excludes commercial or buy-to-let mortgages and includes only the owner-occupied newly originated flow of mortgages. (a) The Figure shows mean DSRs across three buckets: High: $50 \leq DSR < 100$; Medium: $10 \leq DSR < 50$; Low: $DSR < 10$.

Appendix B. Descriptive statistics

Table 13: Descriptive statistics, MDB (Jan 2019 - Nov 2020)

	Mean	St. Dev.	Min	P25	P75	Max
Transactions						
Transaction amount	914.2	2,763.0	0.5	55.5	799.3	104,760.8
No transactions in debit	86.2	62.0	1.0	45.0	110.0	1,479.0
No transactions in credit	17.3	14.5	1.0	9.0	22.0	579.0
Monthly amount debit	5,024.2	4,661.6	0	1,975.3	6,441.8	49,488.8
Monthly amount credit	8,517.0	10,410.1	0	2,967.6	10,093.0	176,189.8
Sources of funds						
Total salary	2,278.3	2,745.1	0	0	3,411.4	25,125.8
Total income	3,060.8	3,196.1	0	723.6	4,221.4	25,125.8
Other secured loans finance	0.5	78.6	0	0	0	45,000
Credit card finance	1,031.4	1,914.2	0	0	1,262.5	14,501.8
Personal loans finance	115.6	1,313.8	0	0	0	49,000
Uses of funds excl. mortgages						
Consumption total excl housing	2,660.9	2,435.7	0	1,081.2	3,399.8	29,510.9
Consumption total	3,212.4	2,787.1	0	1,346.1	4,169.4	33,663.7
Non durable excl. groceries	412.4	433.7	0	137.1	533.4	3,806.1
Durable	177.3	470.2	0	0	142.9	6,830.6
Services excl. restaurant	2,848.0	3,391.4	0	913.6	3,451.6	35,088.9
Groceries	431.9	439.0	0	117.9	606.6	3,098.3
Restaurant	157.7	228.8	0	21.2	201.1	2,906.0
Rent repayments	187.5	681.5	0	0	0	16,550
Other secured loan repayments	0.8	24.7	0	0	0	3,398
Personal loan repayments	244.5	760.0	0	0	157.6	13,392.9
Bank charges	39.8	185.3	0	0	18.0	4,758.6
Credit card repayments	1,135.5	1,900.6	0	0	1,448.7	15,448.8
Cash expenditure	205.7	617.5	0	0	198.2	14,050
Payments into investment accounts	327.7	1,822.2	0	0	33.1	32,075
Payment into other savings accounts	22.6	208.5	0	0	0	11,192
Online spending	165.0	289.8	0	0	195.2	3,309
Mortgages						
No mortgage payments per month	0.5	0.8	0	0	1	22
Repayments consistent mortgage	330.8	637.4	0	0	514.5	10,063.8
Repayments inconsistent mortgage	33.1	345.1	0	0	0	13,095
Savings = Income - Spending						
Monthly savings rate	−4.0	26.1	−1,653.4	−0.4	0.5	0.99
Monthly pound savings	561.1	3,709.7	−36,865.9	−802.6	1,703.5	85,718.7

Table 14: Pre-crisis variables by quintiles

	MDSR	**Income**	**Saving rate**
quintile 1	$\in [0.2, 13.5)$	$\in [33, 1128)$	< -1.3
quintile2	$\in [13.5, 20)$	$\in [1128, 2083)$	$\in [-1.3, 0)$
quintile3	$\in [20, 28)$	$\in [2083, 3125)$	$\in [0, 0.2)$
quintile4	$\in [28, 55)$	$\in [3126, 4837)$	$\in [0.2, 0.5)$
quintile5	> 55	> 4837	$\in [0.5, 0.98)$

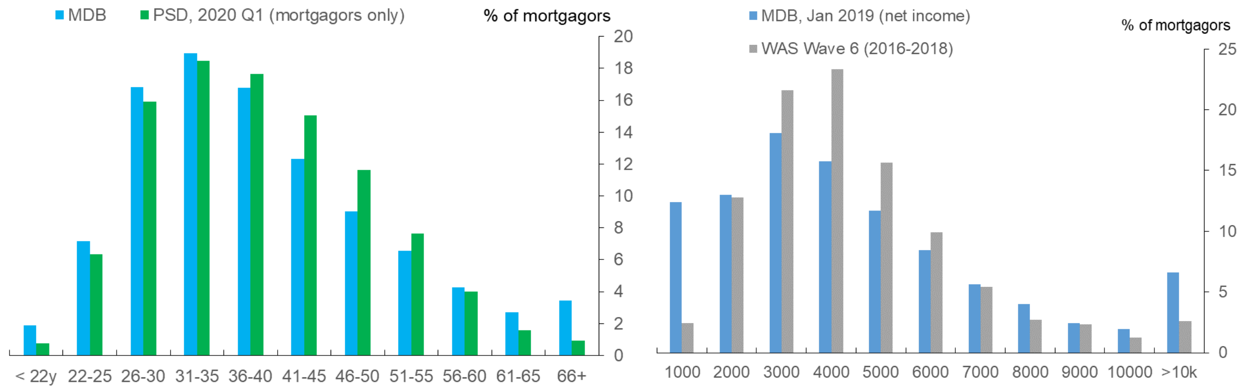
Note: MDSR refers to mortgage debt service ratio, Income to total income, and the Saving rate to 1-C/Y.

Appendix C. Representativeness of the data for UK households

MDB data is representative of the mortgage population, which is the focus of our analysis. First, the share of mortgagors in the MDB is similar to national aggregates. We find that 31% of all households in MDB in 2019 are mortgagors, compared to 28% in the Family Resource Survey[25]. This is comparable to 31.6% in the Wealth and Asset Survey across 2016-2018. Second, the age distribution of mortgagors in MDB closely matches that obtained from the PSD, as shown in Panel (a) of Figure 9. Panel (b) shows that when conditioning for mortgagors, the distribution of income in MDB in 2019 is similar to that from the Wealth and Asset Survey.

Figure 9: MDB representative of mortgagors

(a) MDB mortgagors by age$^{(a)}$ (b) Monthly income for mortgagors$^{(b)}$



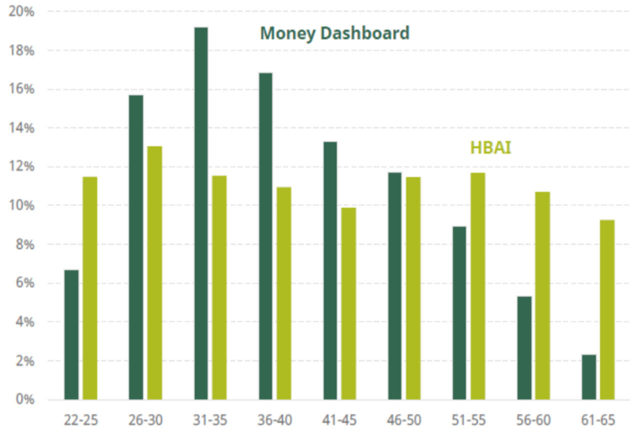
Source: (a) Money Dashboard and Product Sales Database. (b) Money Dashboard and Wealth and Asset Survey.

The data are, however, less representative for the age distribution and regional location of the entire UK household population. Figure 10 shows that the data underestimate the proportion of very young individuals (below 26 years of age) and overestimates the proportion of users located in the south of England. Nonetheless, the data capture fairly well the income and savings distributions across UK households. Panel (a) of Figure 11 shows that net monthly household income in MDB is similar to aggregate statistics from the Wealth and Asset Survey. Panel (b) shows that trends in saving rates in MDB are also comparable to the data from the ONS, with magnitudes being more comparable from late-2019 and matching ONS data nearly perfectly during the Covid pandemic.

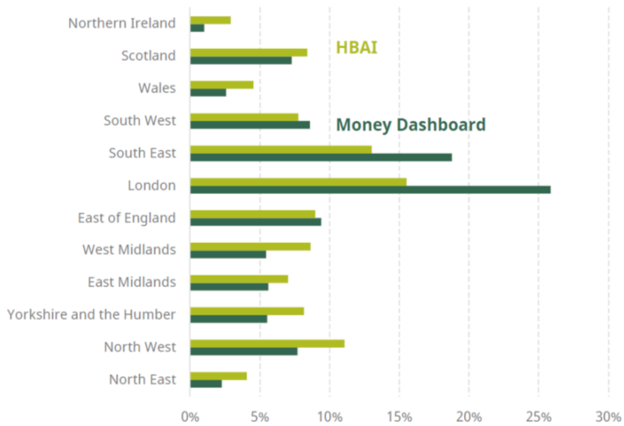
[25] See: https://assets.publishing.service.gov.uk/government/uploads/system/uploads/attachment_data/file/874507/family-resources-survey-2018-19.pdf

Figure 10: MDB is less representative of households' age and regional location

(a) Share of HBAI and MDB users by age group



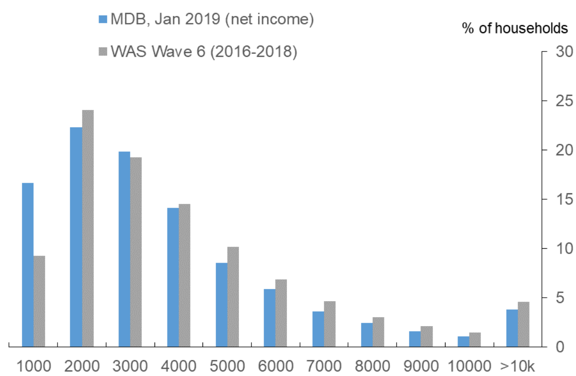
(b) Share of HBAI and MDB users by region



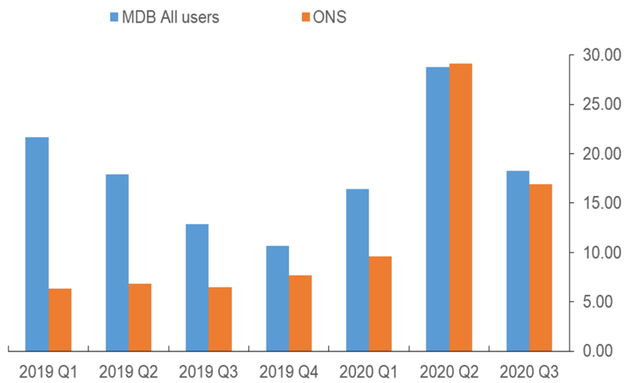
Source: Bourquin *et al.* (2020). Note: HBAI is the Households below average income (HBAI) statistics produced by the Department of Work and Pensions.

Figure 11: MDB representative of household income and savings trends

(a) Net monthly income$^{(a)}$



(b) saving ratio$^{(b)}$



Source: (a) Money Dashboard and Wealth and Asset Survey (WAS), Wave 6. (b) Money Dashboard and Office of National Statistics.

Appendix D. Implementation of mortgage PH in the UK

Mortgage PH were introduced at the start of the pandemic in the UK by the Financial Conduct Authority (FCA). The initial guidance in March 2020 encouraged lenders to offer mortgage payment deferrals to their customers for a maximum period of three months. This was later extended in June and November 2020 to six months.

Mortgage PH provide mortgagors the option to suspend temporarily their mortgage payments (principal and interest), without changing their credit risk score. The aim of this policy was to support borrowers facing temporary payment difficulties because of coronavirus pandemic[26]. In practice, all borrowers were eligible for the policy. The FCA guidance to lenders was to grant the application to all borrowers who requested a PH if the policy was in their best interest. Lenders did not have to undertake a means-based test. There was also no evidence that lenders rejected borrowers' applications if PH would be helpful to manage their financial situation. Data from the Understanding Society special Covid-19 Survey show that only 1.5% of all mortgagors who applied for a mortgage PH got their application denied in the April 2020 wave, and 0.97% in the May wave.[27] These two months correspond to the height of mortgage PH applications. As a result, we are confident that mortgage PH applications are a nearly true reflection of mortgage PH approvals.

Nevertheless, some lenders could offer borrowers an alternative forbearance measure to PH, such as restructuring the mortgage, if they thought borrowers would not be able to resume repayments at the end of the PH. This was likely to have happened to borrowers who were already in arrears for several months. According to an FCA (2021) survey of UK households in October 2020, only 1% of mortgagors that took a PH were not able to resume repayments after their PH ended. Moreover, about 54% of mortgagors on PH resumed their mortgage repayments at the same rate as before the PH. This implies that the original term of the mortgage was extended to accommodate the principal and accrued interest not paid during the PH period. About 33% of mortgagors recapitalised the shortfall from the PH into the loan amount, which implied a higher monthly payment than before. The remaining 13% resumed repayments at a reduced rate.

Appendix E. DiD robustness

E.1 Robustness to additional fixed effects

We check the extent to which our results may be affected by differential shocks at the regional or local level. For instance, UK areas hit hardest by the pandemic, particularly those areas with a higher share of high-contact industries, may have had a greater impact on household expectations. It is then possible that households in these areas cut their consumption more than other less affected areas, irrespective of being granted a PH. This would bias the coefficient downwards.

We address this concern by controlling for a set of additional fixed effects in our preferred specification

[26] See the FCA November 2020 Finalised Guidance: https://www.fca.org.uk/publication/finalised-guidance/mortgages-coronavirus-payment-deferral-guidance.pdf

[27] The survey data for different waves can be accessed at: https://www.understandingsociety.ac.uk/documentation/covid-19

in column (7) of Table 9. We include country fixed effects (England, Scotland, Wales, and Northern Ireland), country-by-time fixed effects, region fixed effects (total of 12 regions), region-by-time fixed effects, postcode district fixed effects, and postcode-by-time fixed effects.[28] When we condition on all these different fixed effects, we find that all our previous results remain strongly robust, both in terms of the size and the statistical significance of the effect of PH on liquidity-constrained households (Table 15).

Table 15: Robustness checks: additional fixed effects

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
PH	-0.924	-0.931	-0.937	0.866	-1.037	-1.383	0.924
	(3.057)	(3.057)	(3.057)	(3.477)	(3.055)	(3.028)	(3.481)
PH*Liq.	22.427**	22.427**	22.461**	22.556**	22.759***	22.734***	20.997**
	(8.804)	(8.804)	(8.805)	(9.733)	(8.813)	(8.812)	(9.833)
PH*Inc. shock	-0.033**	-0.033**	-0.033**	-0.024	-0.033**	-0.034**	-0.026
	(0.015)	(0.015)	(0.015)	(0.015)	(0.015)	(0.015)	(0.017)
Controls	✓	✓	✓	✓	✓	✓	✓
User FE	✓	✓	✓	✓	✓	✓	✓
Time FE	✓	✓	✓	✓	✓	✓	✓
Country FE		✓					
Region FE			✓				
Postocode FE				✓			
Country $\times$Time FE					✓		
Region$\times$Time FE						✓	
Postcode $\times$Time FE							✓
Observations	52,745	52,745	52,745	41,456	52,745	52,745	41,456
Adjusted R^2	0.085	0.085	0.085	0.091	0.086	0.089	0.096

Notes: The dependent variable is the change in log real non-housing consumption. Additional control variables are omitted from the table. Standard errors in parentheses are clustered at the household level. All the variables in the table, with the exception of the treatment and the income shock, are measured pre-crisis. Standard errors in parentheses.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

E.2 Anticipation and delayed effects of PH

There are two features of the PH policy which may have led to anticipation effects on consumption. First, the PH policy was widely advertised, and most households quickly became aware of it (FCA, 2021). Second, it was relatively fast to submit the PH application, was free of charge, and the application window was relatively large (since March 2020). As a result, some mortgagors may have brought forward consumption expenditures before getting the PH, for instance between the application period and the first month when mortgage repayments were paused. If this is true, the actual 'treatment' period would correspond to the application date, although we cannot observe the exact date in our dataset. Instead, we observe the 'official treatment period' corresponding to the date when the first mortgage payment is missing.

[28]For postcodes, we restrict our estimation sample to those with more than 30 mortgagors, resulting into 56 areas and 41,456 household-month observations.

To assess these anticipation effects, we extend the treatment window to the month just before households stopped paying their mortgage. This is shown in in column (2) of Table 16. We find that our main effects on liquidity-constrained mortgagors remain strongly robust, albeit the coefficient is a bit smaller. We interpret this as evidence that the effect of PH on consumption decisions comes from the official treatment period that we have assumed throughout in the paper. This is an intuitive result. Households that typically consume most of their income do not tend to have enough liquid savings to front-load consumption without additional help in place.

In a second exercise, we allow for delayed effects on consumption after the expiration of PH. Mortgagors that were on PH may have accumulated important savings during the PH window which could then be deployed to smooth consumption after the expiration of the PH. To address this, we extend the treatment window to the month right after the expiration of PH. In column (3) of Table 16 we show that our results on constrained borrowers remain consistent with our baseline findings. Finally, we obtain similar effects in column (4), where we control for both anticipation and delayed effects of PH.

Nonetheless, allowing for anticipation or delayed effects wipes out the statistical significance on the coefficient on income shocks. Both effects lead to an upward bias in the coefficient estimate. This suggests that the relationship between consumption and negative income shocks for mortgagors on PH is positive when PH are active, but then turn negative outside of the PH window. This indicates a lack of consumption smoothing behaviour for mortgagors who experience negative income shocks during the pandemic, similar to that of low-income or credit-constrained borrowers (e.g. Baker *et al.* (2020), Vihriälä (2021)). In our data, out of all mortgagors on PH who experienced a negative income shock at the start of the pandemic, 22% are below the median pre-crisis income level, while 33% have saving rates below the pre-crisis sample median. This suggests that a larger proportion of these borrowers may be financially constrained.

Table 16: Anticipation and delayed effects of payment holidays

	(1) Baseline	(2) Anticipation	(3) Delayed	(4) Both
PH	-0.924	-1.681	-2.054	-2.002
	(3.057)	(2.514)	(2.337)	(2.111)
PH*Liq	22.427**	17.173**	18.022***	15.704**
	(8.804)	(7.632)	(6.651)	(6.366)
PH*Inc.shock	-0.033**	0.002	-0.019	0.002
	(0.015)	(0.001)	(0.013)	(0.001)
Controls	✓	✓	✓	✓
User FE	✓	✓	✓	✓
Time FE	✓	✓	✓	✓
Observations	52,745	53,550	53,691	54,423
Adjusted R^2	0.085	0.086	0.085	0.086

Notes: The dependent variable is the change in log real non-housing consumption. Additional control variables are omitted from the table. Standard errors in parentheses are clustered at the household level. All the variables in the table, with the exception of the treatment and the income shock, are measured pre-crisis. Standard errors in parentheses.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

E.3 Allowing treated mortgagors to be part of the control group

In our baseline results, we used a modified version of the standard DiD estimator (Angrist & Pischke, 2009; Wooldridge, 2010). In particular, we followed Goodman-Bacon (2021) and excluded from the control group mortgagors that are treated at a future time, and mortgagors who came off their mortgage PH. We argue that including them would increase self-selection bias by mixing eligible and non-eligible households in the control group.

In this section we examine the potential bias arising if we allow treated mortgagors to also be part of the control group when they are not being treated, in line with the standard literature. This exercise is akin to the Goodman-Bacon (2021) DiD decomposition, which breaks down the standard two-way fixed-effects DiD estimator into a weighted average of all possible effects obtained by comparing: i) early treated versus control group; ii) later treated versus control group; and iii) early treated versus later treated. To replicate the Goodman-Bacon (2021) decomposition, we first add to the control group those mortgagors that are treated at a later point in time. For instance, if mortgagor B is on PH from July to November 2020, we assume that she will be part of the control group during the PH window from March to June 2020. Column (2) of Table 17 shows a decline in the coefficient estimates for liquidity-constrained borrowers and for those experiencing negative income shocks. In addition, the statistical significance of the coefficient on income shocks disappears. The downward bias on coefficient estimates is amplified further in column (3), when we include both early and later treated mortgagors in the control group, in line with the standard DiD design. The effect of PH on liquidity-constrained mortgagors drops from 22.4 p.p. in the baseline case to 14.4 p.p (column 3). In this context, our interpretation is that using the standard DiD estimator may bias the coefficient on the liquidity-constrained interaction term downwards by roughly 35%.

Table 17: DiD estimators

	(1) Baseline	(2) With later treated	(3) With early and later treated
PH	-0.924	-1.637	-1.663
	(3.057)	(2.528)	(1.928)
PH*Liq	22.427**	18.217**	14.417***
	(8.804)	(7.385)	(5.341)
PH*Inc.shock	-0.033**	-0.000	0.000
	(0.015)	(0.002)	(0.001)
Controls	✓	✓	✓
User FE	✓	✓	✓
Time FE	✓	✓	✓
Observations	52,745	54,351	57,475
Adjusted R^2	0.085	0.086	0.084

Notes: The dependent variable is the change in log real non-housing consumption. Additional control variables are omitted from the table. Standard errors in parentheses are clustered at the household level. All the variables in the table, with the exception of the treatment and the income shock, are measured pre-crisis.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

E.4 Split by consumption type

In Table 18 we explore the impact of PH by consumption type, such as on services, durable goods, non-durable goods, groceries, restaurants, and online shopping. We also include cash withdrawals to investigate whether households on PH were more likely to get cash from the ATM, thus suggesting a possible link with consumption financed by cash.[29] We find a statistically significant effect of PH only on services consumption for liquidity-constrained households. This may be related to the size of the services consumption sector which represents the bulk of total non-housing consumption in our dataset.

[29] In our baseline consumption measure we do not include cash withdrawals since we do not observe what households do with the cash withdrawn.

Table 18: Robustness checks: split by consumption type

	(1) Non-housing	(2) Services	(3) Durable	(4) Non-durable	(5) Groceries	(6) Restaurants	(7) Cash	(8) Online
PH	-0.924	-3.871	17.241	-2.605	3.258	-7.551	0.332	6.779
	(3.057)	(3.894)	(10.799)	(3.296)	(4.088)	(5.483)	(10.545)	(7.619)
PH*Liq.	22.427**	31.703***	-30.934	12.995	8.191	23.854	8.152	-3.236
	(8.804)	(11.501)	(26.236)	(9.582)	(11.622)	(17.022)	(36.132)	(21.276)
PH*Inc.shock	-0.033**	-0.050***	-0.061	-0.016	-0.012	-0.001	0.019	-0.059
	(0.015)	(0.017)	(0.066)	(0.019)	(0.019)	(0.026)	(0.070)	(0.046)
Controls	✓	✓	✓	✓	✓	✓	✓	✓
User FE	✓	✓	✓	✓	✓	✓	✓	✓
Time FE	✓	✓	✓	✓	✓	✓	✓	✓
Observations	52,745	52,189	18,450	50,445	42,442	33,045	17,740	26,380
Adjusted R^2	0.085	0.073	0.003	0.060	0.027	0.134	0.062	0.010

Notes: The dependent variable is the change in log real non-housing consumption. Additional control variables are omitted from the table. Standard errors in parentheses are clustered at the household level. All the variables in the table, with the exception of the treatment and the income shock, are measured pre-crisis. Standard errors in parentheses.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Appendix F. Robustness tests for the synthetic control method

We run placebo tests, as in Abadie *et al.* (2010), and Cavallo *et al.* (2013), to test the statistical significance of the estimated effects. We conduct inference based on permutation tests that compare the estimated main effect with the distribution of placebo tests. More specifically, we generate a corresponding set of placebo effects for each control unit, where each control is assumed to enter treatment at the same time as the treated unit. By having the same treatment period, the placebo sets will be the same for the two treated units. The resulting p-value can be thought of as the proportion of control units that have an estimated effect at least as large as the one of the actual treated unit.[30] Figure 12 shows that the estimated effects of PH on liquidity-constrained households are statistically significant at the 10% level from the second month of the PH. This reinforces our main baseline results that PH helped liquidity-constrained households smooth consumption.

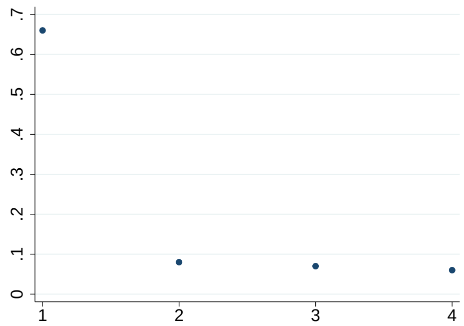
Figures 13 and 14 show the SCM results remain consistent to those reported in Section 5 when we use the log of consumption as the outcome variable.

Appendix G. Using mortgagors never on mortgage PH as the control group

We replicate the analysis in Table 10 based on propensity score matching (PSM) to examine how our

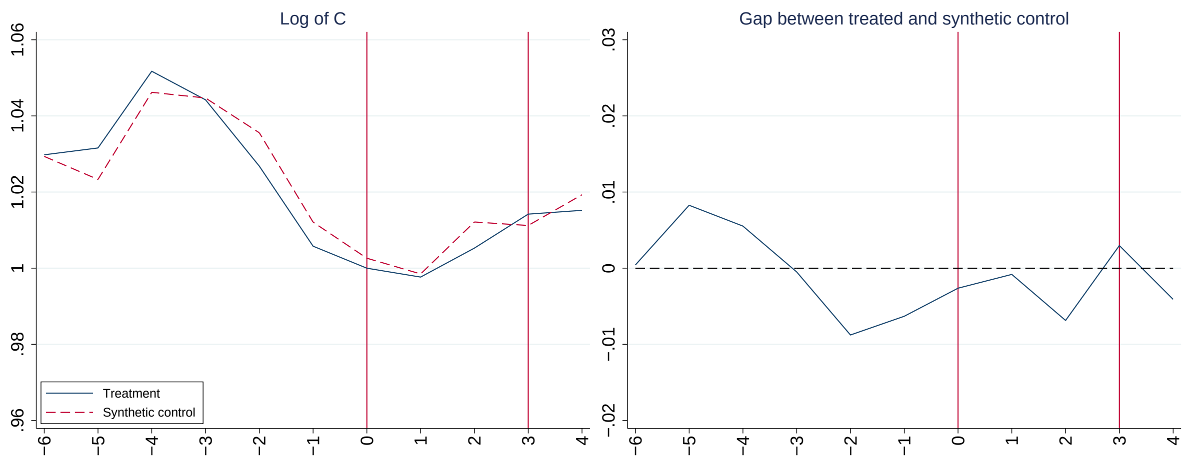
[30] In the placebo tests, treatment is assigned to the non-elligible households. To give a simple example with just ten households, four on mortgage PH and six non-eligible, the placebo tests involve assigning treatment to the six non-eligible, and then, one at a time, run the algorithm to find a suitable synthetic control from the control pool. The p-values will then essentially compare how many of those placebo treated (the non-eligible) have consumption above the ‘actual’ treated mortgagors.

Figure 12: P-values for the difference between treatment and synthetic control



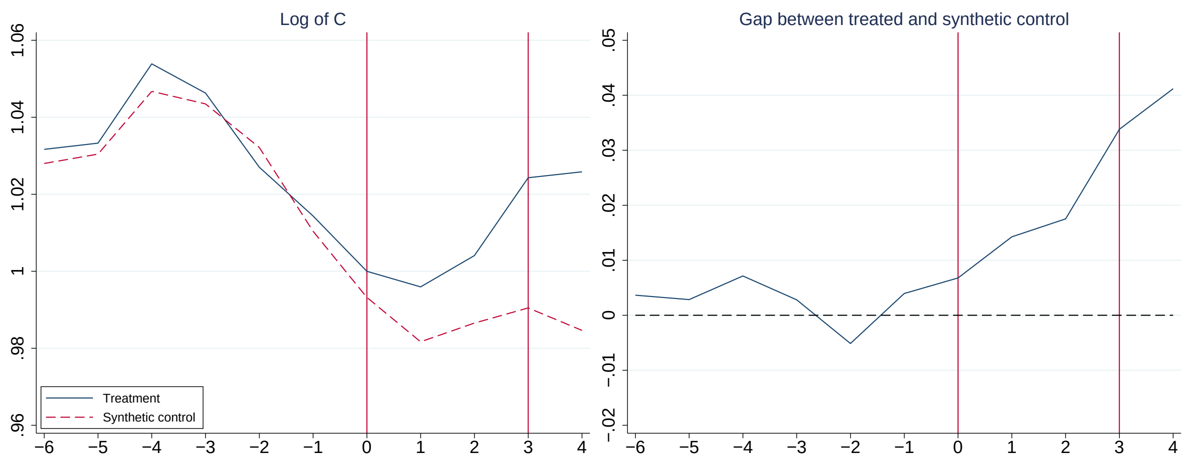
Notes: The x-axis refers to the months after taking PH.

Figure 13: Synthetic control method



Notes: Evolution of the logarithm of real non-housing consumption (normalised to 1 in period 0). The treatment group (solid blue line) refers to households on PH for three months, while the synthetic control group (dashed red line) is made up of the non-eligible to the PH policy. The vertical lines represent the start of the PH (x-axis at 0) and the end of the three-month period (x-axis at 3).

Figure 14: Synthetic control method: liquidity-constrained households



Notes: Evolution of the logarithm of real non-housing consumption (normalised to 1 in period 0). The treatment group (solid blue line) refers to households on PH for three months, while the synthetic control group (dashed red line) is made up of the non-eligible to the PH policy. The vertical lines represent the start of the PH (x-axis at 0) and the end of the three-month period (x-axis at 3).

results change if mortgagors never on PH are used as the control group, instead of non-eligible households. The PSM is better suited than the DiD approach to deal with self-selection issues into the policy. This is because the PSM matches mortgagors on a wide range of observables, and thus ensures that treated and control units have similar financial characteristics that are correlated with the probability of policy take-up. Once self-selection bias is reduced, using other mortgagors as the control group may provide a more appropriate alternative counterfactual for the treated during the pandemic, since households with similar balance sheets commitments tend to respond similarly to aggregate shocks (Cloyne *et al.* (2020)).

Table 19 shows that the consumption responses remain comparable with the DiD estimates, even when mortgagors never on PH form the control group. We do not find any statistically significant effect of the policy on the consumption of the average mortgagor. But we do find that mortgage PH provided important consumption support to liquidity-constrained borrowers, albeit the coefficient estimates being lower than the DiD baseline shown in column (3). This difference in magnitude may arise for two reasons. First, restricting our sample to mortgagors reduces the size of our control group significantly, since mortgagors account for only 30% of all UK households. Second, and most importantly, we are concerned that unobserved factors, such as financial literacy, may determine the probability of accessing the policy among mortgagors, which may bias the results. In contrast, non-eligible households do not have the option of applying for mortgage deferrals; using them as control thus allows us to eliminate altogether this source of omitted variable bias.

Table 19: Consumption effects using PSM and mortgagors never on PH as the control group

$\Delta \log C$	(1) NN	(2) NN no repl	(3) Baseline
A. Average across sample			
	-2.676	-2.125	-0.924
	(3.029)	(2.550)	(3.057)
Observations	25,190	25,190	52,745
B. Liq. constrained only			
	14.656*	14.196*	22.427**
	(8.357)	(8.535)	(8.804)
Observations	2,580	2,580	52,745

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

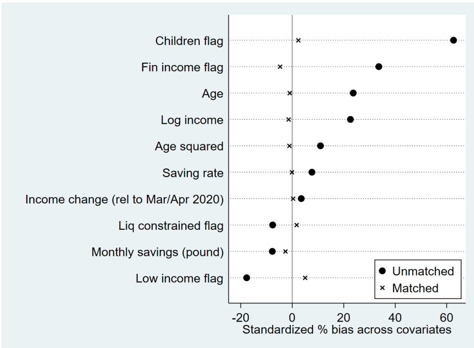
Notes: Estimation methods in columns (1) and (2) use a caliper of 0.2. When the entire sample is used, the matched sample for the nearest-neighbor with replacement includes 2,073 treated units and 1,913 control units comprised of non-eligible households. Propensity scores range from 0.003 to 0.36. When the sample is restricted to liquidity-constrained borrowers, the matched sample for the nearest-neighbor with replacement includes 246 treated units and 229 control units comprised of mortgagors never on PH. Propensity scores range from 0.02 to 0.2. Bootstrapped standard errors in parentheses. The baseline is copied from column (7) in Table 9.

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

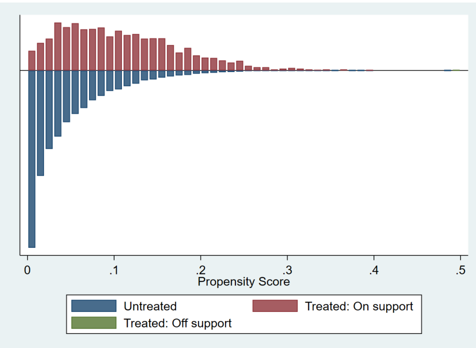
Appendix H. Matching performance for the propensity score (control group is households non-eligible for mortgage PH)

Figure 15: Matching performance across the sample

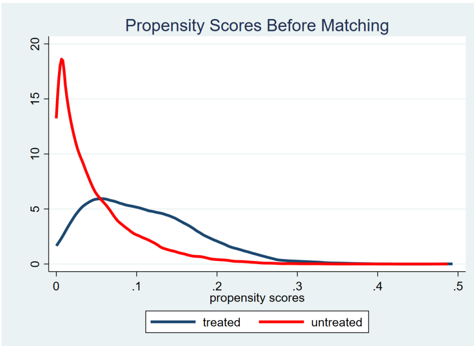
(a) Reduction in observable bias



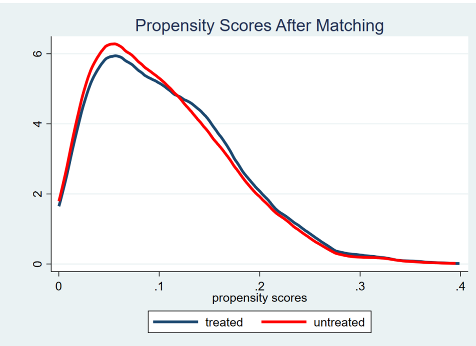
(b) Range of common support



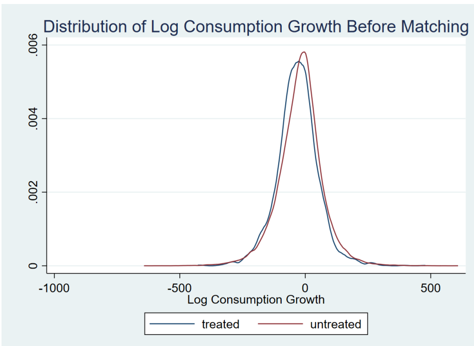
(c) Propensity scores before matching



(d) Propensity scores after matching



(e) Change in consumption before matching



(f) Change in consumption after matching

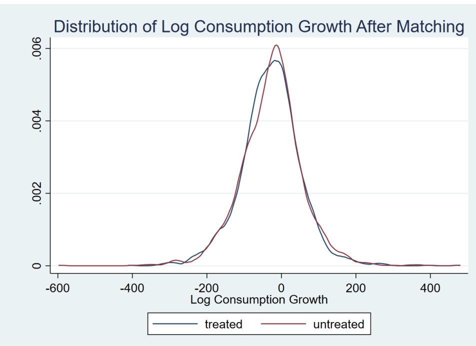
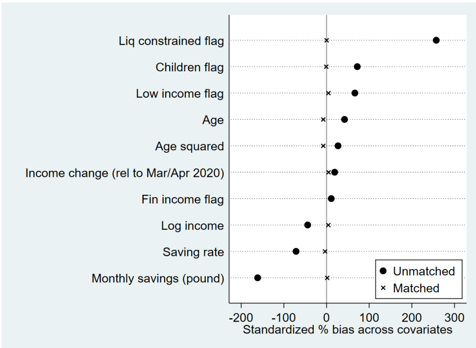
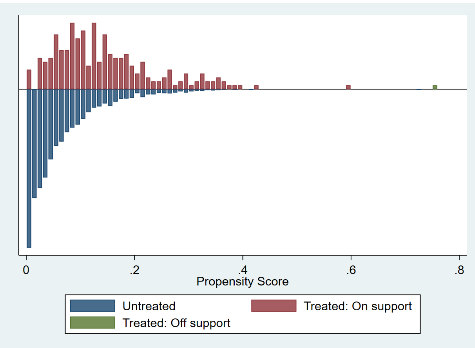


Figure 16: Matching performance across liquidity constrained households

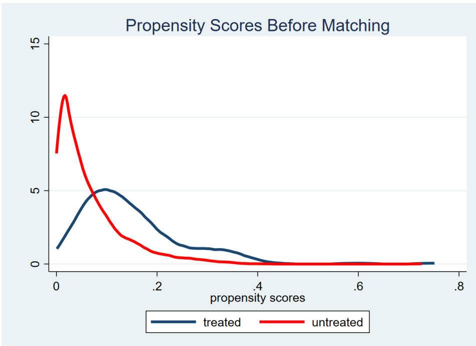
(a) Reduction in observable bias



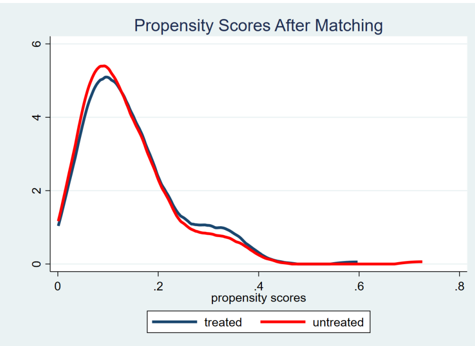
(b) Range of common support



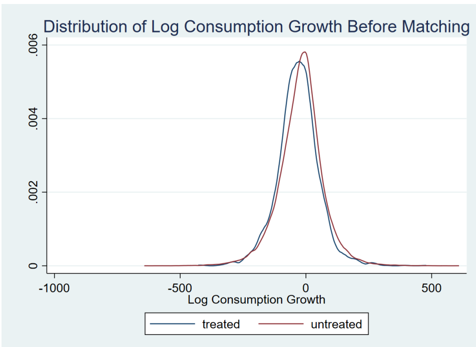
(c) Propensity scores before matching



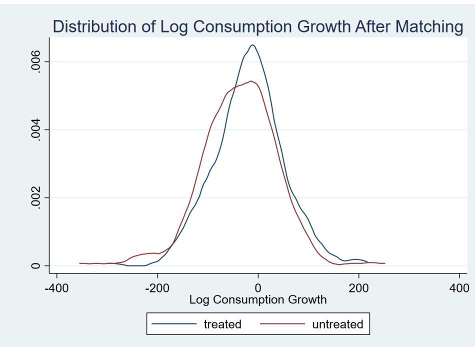
(d) Propensity scores after matching



(e) Change in consumption before matching



(f) Change in consumption after matching



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