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The consumption response to unemployment : Evidence from French bank account data*

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Lorsqu’ils perdent leur emploi, les ménages puisent-ils dans leur épargne ou réduisent-ils leurs dépenses ? – une approche par des données de comptes bancaires

La perte d'un emploi entraîne une diminution des revenus, car les allocations chômage ne compensent que partiellement la perte de salaire. Cette étude décrit comment les ménages s’adaptent financièrement à cette baisse de revenu. Puisent-ils dans leur épargne pour maintenir leur niveau de consommation ou sont-ils contraints de réduire leurs dépenses ? Ce travail utilise des données de comptes bancaires pour mesurer comment les ménages ajustent leurs dépenses suite à la perte d'un emploi. Nos résultats indiquent que la consommation diminue d’un montant équivalent à 36 % de la perte de revenu sur les six premiers mois de chômage. Plus la période de chômage se prolonge, plus les ménages réduisent leur consommation et moins ils sont enclins ou capables de puiser dans leur épargne. Un mois après la perte d'emploi, la consommation diminue peu, l’équivalent de 5 % de la perte de revenu mensuel, mais six mois après la perte d’emploi initiale la consommation diminue beaucoup plus, l’équivalent de 46 % de la perte de revenu mensuel. La diminution de la consommation est plus forte chez les ménages détenant peu d'actifs liquides, mais dépend peu du revenu du ménage.

Mots-clés : Propension marginale à consommer, Assurance chômage, Données sur les comptes bancaires

Codes JEL : C55; D12; D15; H50

The consumption response to unemployment - Evidence from French bank account data

The loss of a job results in a reduction in income, as unemployment benefits provide only partial compensation for the loss of wages. This study investigates the financial adaptations made by households in response to such circumstances. Do they draw on their savings to maintain their consumption level or are they forced to reduce their expenditures? This paper uses French high frequency bank account data to measure how spending responds to job loss. Our findings indicate that, in the first six months of unemployment, 36% of the income loss is offset by a reduction in spending, while the remainder is primarily compensated by a decrease in liquid savings. The longer the unemployment spell, the greater the reduction in spending. Only 5% of the income loss is offset by a reduction in spending one month after the job loss. However, this figure reaches 46% six months after the initial job loss. The response depends on the quantity of liquid assets held by households, while being less influenced by income.

Keywords: Marginal propensity to consume, Unemployment insurance, Bank account data

JEL Code: C55; D12; D15; H50

1 Introduction

The loss of a job leads to a significant and prolonged reduction in income, which can lead to a sharp fall in consumption.[1] Modern welfare states have introduced measures to mitigate this negative income shock: unemployment benefits (UB), such as the Aide au Retour à l'Emploi (ARE) in France, act as a replacement income, partially compensating workers for their lost earnings. However, the rest of the burden is borne by the workers. Are they able to insure themselves through their savings, or do they have to reduce their consumption as a consequence? Answering these questions allows us to assess the extent to which unemployment insurance fulfils its role as a safety net during periods of unemployment.

Despite the considerable attention paid to unemployment in both public and academic debates, empirical evidence on how household finances are affected by job loss remains limited. How do households adjust their consumption and saving behaviour during periods of unemployment? Answering this question requires access to high-frequency data on income, spending and savings, which remains difficult to obtain. However, the recent availability of bank account data to researchers in several countries has facilitated new insights. With access to bank data, it is possible to estimate the marginal propensity to consume (MPC) in response to an unemployment shock, thereby quantifying the reduction in consumption associated with income losses due to spells of unemployment.

The contribution of this article is to provide a month-by-month analysis of unemployment spells in France, using high-frequency data from the

[1] Other channels than the purely monetary ones may prevail; a self-assessed measure of welfare, subjective well-being, has been shown to fall in the aftermath of job loss (Clark et al., 2008).

last quarter of 2020 to the first quarter of 2024. This study quantifies the role of private insurance mechanisms, such as savings, in mitigating income losses. We summarise our findings on the spending response to this shock by estimating the corresponding MPC, both on average and across observable characteristics such as age, income and liquidity. In addition, we estimate the timing of the consumption adjustment as a function of the duration of unemployment. For identification purposes, our analysis is limited to unemployed persons receiving benefits.

When unemployed, households draw on their personal savings to limit the fall in consumption. Hence, one month after job loss, consumption falls by only 2%. The longer the period of unemployment, the more households reduce their consumption, and the less they are willing or able to draw on their savings. After six months of unemployment, consumption is 15% lower than before job loss. These adjustments imply that over the first six months of unemployment, 36% of the drop in income was offset by a reduction in consumption (i.e., MPC is equal to 0.36). There is considerable heterogeneity in the responses according to the quantity of liquid assets held, with MPC ranging from 0.19 for the top 25% of liquidity holders to 0.56 for the bottom 25%. Furthermore, we examine the effect of the income increase resulting from the full and final settlement in the last month of employment prior to job loss. The additional income is not entirely allocated to savings for mitigating the impact of future unemployment, as it leads to an increase in spending. However, the marginal propensity to consume (MPC) is relatively low, at 0.20.

Literature This article draws on three distinct bodies of literature. First, the studies most closely related to our work are those that examine job loss using bank account or app data, as these studies are able to analyse infra-annual consumption adjustments. This literature includes Ganong and Noel (2019) in the US, Gerard and Naritomi (2021) in Brazil and Andersen et al. (2023) in Denmark. Empirical evidence suggests an average MPC up to 24 months after a job loss of around 0.3 in the US or Denmark and 0.2 for consumer durables in Brazil. Using annual administrative data, Fagereng, Onshuus, and Torstensen (2024) find an MPC of 0.4 following an unemployment shock. Second, many papers have examined the issue of heterogeneity in the MPC using different income shocks, such as lotteries or stimulus payments. The role of liquidity has been particularly emphasised by Kaplan, Violante, and Weidner (2014). Third, the literature on the optimal level of unemployment insurance (UI) has tried to operationalise the Baily-Chetty formula (Baily, 1978) on the basis of sufficient statistics. Chetty (2006) states that the optimal level of UI is determined by three parameters: "(1) the elasticity of unemployment durations with respect to benefits, which captures the moral hazard cost of benefit provision due to behavioral response; (2) the drop in consumption as a function of UI benefits, which quantifies the consumption-smoothing benefits; and (3) the coefficient of relative risk aversion, which reflects the value of having a smoother consumption path". Since Gruber (1997), many papers have tried to quantify the second term, related to consumption reduction: a partial list includes Browning and Crossley (2001), Engen and Gruber (2001), Chetty (2008), Hendren (2017), Kolsrud et al. (2018), and Landais and Spinnewijn (2021). Our paper contributes to this literature by estimating the infra-annual consumption response in the context of France.

The rest of the paper is organized as follows. Section 2 presents the institutional setting and section 3 presents our data, including comparisons with external sources to address both representativeness and completeness concerns. Section 4 presents our empirical methods and section 5 presents the main results. Section 6 presents additional analyses focusing on the importance of access to liquidity and the duration of the unemployment spell considered. Section 7 addresses robustness concerns. Section 8 concludes.

2 Institutional setting

In France, eligibility for unemployment insurance (UI) requires a minimum period of prior employment.[2] Specifically, individuals must have been employed for at least six months during the reference period of affiliation. This reference period corresponds to 24 months preceding the termination of their employment contract for individuals below 52 years old and 36 months for those aged 53 and older. Only involuntary leavers, i.e. workers who did not deliberately leave their jobs, can claim UB.[3] Depending in particular on collective agreements and the level of seniority, the full and final settlement may include additional benefits such as severance pay or compensation for untaken leave.

UB are characterized by two parameters, their duration and their amount. The duration for which an unemployed gets benefits is equal to the number

[2]France has been reforming its unemployment insurance rules between 2019 and 2021, affecting eligibility, duration, and benefit levels. The latest measures, which affect the calculation of duration and replacement rate, came into force on October 1, 2021. Here, we outline the new rules that apply to the majority of our sample, with the earliest entry into unemployment occurring in July 2021 and the latest in August 2023. The main impact of the reform is a reduction in the value of unemployment benefits and an extension of their duration, which varies by age.

[3]There are a few exceptions to this rule, and some resignations, justified by professional or family imperatives, may be considered legitimate. Specifically, the unemployed must prove “a termination of the employment contract by dismissal, or the termination of a fixed-term employment contract (CDD), or a contractual termination, or resignation for a legitimate reason”, *European Commission*.

of calendar days (including holidays and non-working days) between the first day of the first employment contract and the last day of the last employment contract in the reference period of affiliation. This period is called the period of affiliation (in contrast to the reference period of affiliation). However, non-working days (inter-contractual periods) are limited to 75% of the number of working days.

The amount, the daily allowance, is calculated based on the daily reference wage. The daily reference wage (DRW) is calculated by dividing the earnings[4] received during the individual's period of affiliation by the total number of calendar days in that period.

$$DRW = \frac{\text{earnings in the period of affiliation}}{\text{nb calendar days in the period of affiliation}}$$

Then the daily allowance (DA) is equal to the highest amount between 57% of the daily reference wage and 40,4% of the daily reference wage plus €$12,12 \times \alpha$[5], where α corresponds to the part-time coefficient.[6][7] Moreover, the minimal allowance is set to €$29.56 \times \alpha$ and in no case can the allowance exceed 75% of the daily reference wage nor €256.96 by day. To sum up, the

[4]The earnings considered exclude various bonuses, including reimbursement of business expenses, insecurity bonuses, and payments for untaken holidays.

[5]Values are those in effect on October 1, 2021. They have been revalued over the period in line with inflation. All calculation rules can be found here https://www.cdg74.fr/sites/default/files/atoms/files/circulaire_unedic_2021-13_du_19_octobre_2021_reglementation_dassurance_chomage.pdf.

[6]In the case of part-time work, α is equal to the number of hours you work by week divided by the legal weekly working hours (35 hours).

[7]Since December 1, 2021, a measure reducing the allowance applies to beneficiaries under age 57 receiving a certain level of allowance (€91.02 per day) after an eight-month period of compensation (243 days).

daily allowance is calculated as follows:

$$DA = \min \begin{cases} \max \begin{cases} €29.56 \times \alpha \\ 40.4\% \times DRW + €12.12 \times \alpha \\ 57\% \times DRW \end{cases} \\ €256.96 \\ 75\% \times DRW \end{cases}$$

The allowance is paid once a month by multiplying the daily allowance by the number of calendar days in the month (28, 29, 30 or 31).

In the event of reemployment, it is possible to combine both unemployment benefits and salary. However, this combination is limited to the monthly amount of the reference wage: the total monthly amount received by the beneficiary (wage + unemployment benefits) cannot exceed the reference wage on which the benefit amount was calculated.

3 Data construction

The database used in this study originates from *La Banque Postale* (LBP hereafter), a public bank established in 2006 within the postal group *La Poste*, the historical monopoly responsible for mail delivery; this bank serves nearly 11 million customers. The construction of key variables follows methodologies from previous studies employing similar data (Baker, 2018; Ganong and Noel, 2019; Andersen et al., 2023). We employ transaction-level data on card payments,[8] paper checks, cash withdrawals, cash deposits, bank transfers, and direct debits, with each transaction recorded in euros. Additionally, we have access to balance sheet data, including end-of-month balances on

[8]It includes debit and credit cards. In France, the use of credit cards is limited, accounting for less than 10% of bank cards.

deposit and various savings accounts,[9] as well as life insurance, stocks, and credits (consumer loans and mortgage loans). Deposit accounts encompass joint accounts. We aggregate customers sharing a joint account into the same household, making the household our unit of observation. The data employed is high-frequency, containing transaction-level information timestamped and aggregated daily, while balances are available on a monthly basis. Finally, we observe various socio-demographics, including age, sex, marital status, occupation, *département*,[10] and location of residence (urban/rural/semiurban areas).

3.1 Initial sample

Our observation period runs from October 2020 to March 2024. Our main initial raw data is a sample of about 300,000 households. The sampling method ensures that the final sample is representative of the bank. The data provided pertain to clients meeting the following conditions: clients *engaged* with the bank (a marketing segmentation criterion of the Bank indicating that LBP is likely the main bank of the client) and adult clients born after 1923 on one of the following days (January 2nd to 5th, April 1st to 4th, July 1st to 4th, October 1st to 4th). Each month, new clients who meet these conditions are added to the sample.

[9] Savings accounts include *Livret A, Livret Jeune, Livret de Développement Durable et Solidaire, Compte Épargne Logement*, and time deposits

[10] An administrative division similar to a county in the U.S. (*départements* are on average 3.5 times bigger). Mainland France, excluding Corsica and overseas territories, is divided into 94 *départements*.

3.2 Construction of key variables

We now detail the construction of our key financial variables from this data: disposable income, spending, savings, and financial wealth.

Disposable income To construct monthly disposable income, we focus on specific types of account inflows. First, it includes all transfers from organizations that the bank has identified as income.[11] These inflows encompass wages, pensions, welfare benefits, and unemployment benefits, each of which can be separately identified. Pensions, welfare benefits, and unemployment benefits are identified by the bank based on the name of the issuing organization, while wages are identified as the remaining transfers from organizations labeled as income by the bank. Additionally, disposable income includes all incoming checks. Finally, we subtract identified tax amounts in direct debit transactions from these inflows to obtain disposable income. Note that capital income is ignored.

Spending We define total spending as the sum of various outflows. First, we include all outgoing transactions made via debit and credit card (including cash withdrawals) and paper checks. Second, we add direct debits, excluding tax payments and credit refunds to the bank (which are considered savings). This definition excludes outgoing transfers, as our data lack the granularity to differentiate between consumption (e.g., rent payments) and savings (transfers between accounts held in different banks by the same households). This measure is restrictive, and we are likely to miss a portion of spending due to transfers. Typically, rents paid via transfers (and not in cash, checks, or direct debit) are excluded from our definition. Furthermore,

[11] The definition is broad. Inflows from insurance or credit companies are excluded.

since March 2022, the bank has provided Merchant Category Codes (MCCs) for card payments, which categorize merchants depending on the types of goods and services they mainly sell.[12]

Savings We compute savings from the change in monthly balances across all financial accounts (deposit, savings, life insurance, and securities). We add up outflows net of inflows from any account in another bank with identical surnames.[13] Finally, we include credit refunds (mortgage and consumer loan repayments) and subtract amounts corresponding to new consumer loans.[14] This measure is restrictive, and we are likely to miss part of the savings due to transfers to financial institutions. We consider our measure a lower bound that excludes partially illiquid savings. Despite these potential limitations, the results of our event study demonstrate that we are able to capture most of the variations following the income shock due to job loss.

Financial wealth The financial wealth detained by households corresponds to the sum of the monthly balances over all their bank accounts, which includes both liquid (e.g. deposit and savings accounts) and illiquid (e.g. life insurance and securities) assets.

All outcomes are winsorized below the $2,5$ percentile and above the $97,5$ percentile, following the literature using bank data (Andersen et al., 2020; Ganong and Noel, 2019).

[12]The categorization is based on the information reported by merchants to the Internal Revenue Service

[13]The data we access is anonymized, but the bank flags transactions with identical surnames.

[14]We do not subtract amounts corresponding to new mortgage loans: while these reduce financial wealth, they increase housing wealth.

3.3 Completeness and representativeness issues

Despite numerous comparative advantages of bank account data, two concerns have been raised by the literature as regards external validity of such sources (Baker, 2018): representativeness and completeness. Representativeness issues arise because each bank may target specific customers, who differ from the general population (additionally, households without bank accounts are inherently excluded from the sample). Completeness issues arise because each household may hold assets and means of payment outside their primary bank, leading to only a partial observation of the household's financial situation. Therefore, we utilize several external sources to assess both the representativeness of the bank's clients and the completeness of household financial information.

Representativeness To assess representativeness issues, we use the national wealth survey which provides information on household wealth and income and on the banking institutions where deposit accounts are held. In the survey, we consider that households primarily bank at LBP if they hold their deposit account with the highest balance there. We observe in that survey that the distribution of income of households in LBP matches that of the whole population (Table A.1 in the appendix), even though customers at LBP tend to be poorer (median annual income of $26,000$ euros versus $32,000$).[15] They also tend to have lower financial wealth (Table A.2 in the appendix).

[15] There is a one-decile difference between the distribution of the general population and that of the postal bank (D2 in the postal bank is equal to D1 in the general population, D3 to D2 etc).

Completeness Households may hold accounts in multiple banks, potentially leading to measurement errors in income, consumption, and particularly financial wealth within our dataset. Households in the lowest decile of income maintain 92% of their banking assets in their primary bank, compared to 82% for those in the highest decile (Table A.3 in Appendix).

Assessing completeness and representativeness issues Nonetheless, despite these issues, comparisons between statistics derived from bank data and those from representative surveys are reassuring. The distribution of income obtained from transaction data closely matches that from surveys (Figure A.1 in Appendix). Furthermore, average household expenditures and wealth relative to their position in the standard of living distribution in bank data also closely align with those from representative surveys (Figure A.2 and A.3 in Appendix). Spending-to-income ratios appear similar (Figure A.4), decreasing from 1.6 for bottom 10% of income to 0.70 for top 10%. The spending-to-income ratios are, on average, higher in the bank data. This may be partly explained by the fact that LBP customers are, on average, poorer, and by the exclusion of capital income from our definition of income.

Overall, previous comparisons with external sources suggest that representativeness and completeness are not major concerns when tracking the response of income and spending to an unemployment shock using our data. If anything, we may underestimate financial wealth (particularly illiquid assets) and savings, both because LBP customers tend to be poorer and because a portion of financial wealth is held outside the bank. This is confirmed by Table 1, which shows that these customers have lower incomes and financial wealth than average.

3.4 Sample selection

Although we do not observe individual employment history in the data, we have access to monthly indicators that inform us whether households have received unemployment benefits (UB) and wages. Therefore, we rely on these indicators to define an unemployment spell. In our main specification, we consider a 12-months observation window: from 6 months before the loss of a job to 6 months after. To avoid mismeasurement issues, we impose three restrictions on our sample: we exclude households 1/ with transfers exceeding €$60,000$, 2/ with fewer than five outgoing transactions per month, and 3/ with less than €150 of income in the previous quarter. The first restriction is imposed because large financial transactions are unlikely to be related to the unemployment event and make it difficult to isolate saving and spending responses (these operations may be linked to housing purchases). The second and third restrictions aim to exclude customers for whom the bank may incorrectly estimate as their primary bank and whose main source of income is not reflected in the accounts held in the bank.

3.4.1 Treatment group : definition of unemployment spells in our data

For identification purposes, our analysis is limited to unemployed persons receiving benefits. A job loss is characterized as a situation where the number of monthly transfers labeled as wages declines by at least one in the household (typically, from 1 to 0 for singles and from 2 to 1 for couples if both members were initially employed and one loses his/her job). We then construct several sub-samples based on the duration of the unemployment spell. We define that a household finds a job in month m if the number of wage-labeled transfers increases in month m relative to the month of the job loss. To avoid false positives (households incorrectly identified as losing their jobs), we impose two additional mild restrictions. First, we limit our analysis to households that received fewer wage-labeled transfers[16] after the shock and more unemployment benefit transfers. Second, we exclude households who do not receive the same number of wage transfers each month before the job loss,[17] thus restricting our sample to households that had some stability in employment before the spell. The sample size is composed of $6,439$ households. In our primary specification, we concentrate on households that have been unemployed for a minimum of six months, which encompasses $2,409$ households.

3.4.2 Control group

To estimate the impact of job loss on consumption, one might simply compare the evolution of spending before and after the shock. However, this observed change may be influenced by confounding factors such as seasonality, economic conditions, or specific trends. As underlined by Fagereng, Onshuus, and Torstensen (2024), "the ideal experiment for investigating consumption responses after job loss would involve random, unexpected employee terminations". However, such an experiment is obviously impractical for ethical reasons. Consequently, researchers have traditionally relied on natural experiments, such as mass layoffs or plant closures, to obtain exogenous shocks (Schwerdt, 2011). In our dataset, we lack employer-specific information, preventing us from identifying plant closures or utilizing the mass-layoff approach. Therefore, our methodology involves constructing a counterfactual using a control group method as done by Fagereng, Onshuus, and Torstensen

[16] In number, not necessarily in amount.
[17] Except for the month before the job loss because of final and full settlement.

(2024) and Gerard and Naritomi (2021).[18]

Our objective is to create a control group whose observable variables are on average close to those of the treatment group. To do so, every worker who becomes unemployed is paired with a worker who do not experience job loss during that period: we restrict our sample to households who have received the same number of positive transfers labelled wages each month in the 12-month period but who have not received any unemployment benefits (UB) or pensions during the period. To render both groups more comparable in terms of observables, we use a propensity score matching (PSM) approach. Each household in the treatment group is matched to its sibling in the control group based on the nearest propensity score for each calendar month of the observation period. The score is computed based on observed characteristics (financial wealth, age, and the number of adults in the household) measured in the quarter preceding the observation window. Table 2 confirms that post-PSM samples are more balanced than pre-PSM ones in these respects, particularly for financial wealth. However, a latent concern remains: displaced workers and their employed counterparts may exhibit different trends in income, spending, or savings. To assess the influence of unobserved factors affecting these trajectories, we examine the pre-trends of key variables (see section 7).

[18] Andersen et al. (2020) do not use a control group and just measure the impact on treated, but they provide a robustness checks on a subsample involving mass layoffs and reports results consistent with those from their main sample.

Table 1: Summary statistics

	(1) La Banque Postale sample	(2) National surveys
# of observations	185,543	
	Sample means	
Financial variable		
Spending	2,512	2,295
Disposable income	2,664	3,138
Financial Assets	35,188	55,372
Liquid financial Assets	22,625	26,007
Illiquid financial Assets	12,563	29,364
Monthly savings	80	
Demographics		
Age	54	56
Nb adults	1.5	1.79

Notes: Pecuniary amounts: in €.
Sources: La Banque Postale sample, and national surveys: consumer survey (*Budget des Familles, 2017*, https://www.insee.fr/fr/statistiques/5371205?sommaire=5371304), wealth survey (*Histoire de vie et Patrimoine, 2017*, authors' calculations), income survey (*Enquêtes sur les Revenus Fiscaux et Sociaux, 2018*, https://www.insee.fr/fr/statistiques/5371205?sommaire=5371304) and census (*Enquêtes Annuelles de Recensement, 2019*, authors' calculations). Age is estimated on the oldest member in the households.

Table 2: Summary statistics (by treatment status, before and after PSM adjustment)

	Unadjusted		Adjusted	
	Control	*Treated*	*Control*	*Treated*
Age	48	39	39	39
# of adults	1.4	1.2	1.3	1.2
Disposable income	2,487	2,055	2,307	2,055
Spending	2,240	1,864	2,065	1,864
Savings	123	123	87	123
Financial wealth	30,676	7,655	10,753	7,655
Liquid assets	21,370	6,715	9,612	6,715
Illiquid assets	8,165	687	1,105	687

Notes: Pecuniary amounts: in €. Summary statistics on treated and control group before and after propensity score matching. Means estimated the quarter preceding the observation window and all financial variables are winsorised. The winsorisation implies that the average financial wealth is not equal to the sum of liquid and illiquid assets.
Sources: La Banque Postale sample, authors' calculations.

4 Empirical analysis: estimation method

Estimating dynamic treatment effect Our research design leverages an event study around job loss, unemployment spell being the event. We estimate dynamic treatment effects on several outcomes to provide a decomposition of the income shock along several dimensions such as spending and savings. We control for individual (α_i) and month (γ_t) fixed-effects. A two-way fixed effects approach corresponds to the estimation of the following equation:

$$y_{it} = \sum_{h=-6}^{+5} \beta_h \mathbb{1}[e_{it} = h] + \alpha_i + \gamma_t + \epsilon_{it}, \tag{1}$$

where y_{it} are the outcomes variables, e_{it} is event time defined as distance in months to job loss and ε_{it} are idiosyncratic heterogeneity terms. A large recent literature has been dedicated to investigate the biases in the treatment estimates (parameter β) that could be introduced by such estimation in a setting where all units are not treated at the same time (Borusyak and Jaravel, 2017; De Chaisemartin and d'Haultfoeuille, 2020; Callaway and Sant'Anna, 2021). In the context of event studies, Sun and Abraham (2021) show that "in settings with variation in treatment timing across units, the coefficient on a given lead or lag can be contaminated by effects from other periods". They propose an alternative method, defining cohorts by treatment dates and estimating different treatment effects for each cohort. They then estimate the average treatment effect across all cohorts using sample shares as weights. They argue that these weights are more interpretable than those underlying the traditional two-way fixed effects regression. We apply their alternative method to estimate the dynamic treatment effects for all our outcome variables.

The method consists of two steps. In the first step, a two-way fixed effects equation is estimated where time to treatment is interacted with cohorts dummies to recover cohort specific treatment effect:

$$y_{it} = \sum_{c} \sum_{h=-6}^{+5} \delta_{c,h} \mathbb{1}[e_{it} = h] \mathbb{1}[C_i = c] + \alpha_i + \gamma_t + \epsilon_{it}, \tag{2}$$

where $C_i = c$ indicates that units i belongs to cohort c, and $\delta_{c,h}$ corresponds to the treatment effect for cohort c in relative period h. In a second step, these cohort specific treatment effects are aggregated to obtain the average treatment for the treated (ATT) for each relative period. The average interaction-weighted estimator for a given relative period is then:

$$\hat{\beta}_h^{sa} = \sum_c \hat{\delta}_{c,h} \hat{s}_{hc} \tag{3}$$

where $\hat{s}_{hc}$ are sample shares. Standard errors are clustered at the household level. We normalize $\beta_{-5} = 0$, hence the reference of our event study is 5 months before the job loss. As a robustness check, we also estimate the event study using the traditional two-way fixed effects estimation, and we find similar results. Hereafter, we denote $\hat{\beta}_h^{sa}(Y)$ the average treatment effect for relative period h using this two-step procedure and variable Y as outcome (income, spending, savings, etc.).

Estimates in differences, levels or in percentage of pre-event income $\hat{\beta}_h^{sa}(Y)$ corresponds to the estimated difference in euros between the level of variable Y h months after the job loss relative to its level 5 months before (the reference month), controlling for trend and seasonality variation using the control group. To obtain average outcomes Y for each months around job loss, we add to each coefficient β the average level of the outcome variable in the treated group in the reference period -5. To express the variation of all of our outcomes as a percentage of pre-event levels, we divide $\hat{\beta}_h^{sa}(Y)$ by the average income in the treated group 5 months before job loss. Standard errors are computed using a bootstrapping procedure with resampling of individuals, rather than observations, to account for heteroskedasticity and autocorrelation within observations for the same individual.

5 Results

5.1 Event study analysis

Decomposing the income shocks In our primary analysis, we decompose the evolution of earnings around job loss for households that remain unemployed for at least six consecutive months. The institutional setting implies that the drop in disposable income due to job loss is only partially compensated by unemployment benefits (section 2).

Six months after job loss, disposable income is €630 lower than it would have been if the household had remained employed. With an average disposable income of $2,055$ euros five months prior to job loss, this decline represents a 30% reduction in income. Table 3 presents the estimates of the event study (equation 3). Figure 1 illustrates the evolution of average outcomes in levels[19] around the time of job loss (see Figure B.1 in appendix for the direct plot of the event study estimates). The reduction in income is attributed to a decline in labor earnings which are partially compensated by unemployment benefits. The average replacement rate is 65% (see Appendix C for a detailed

[19]To obtain average outcomes Y for each months around job loss, we add to each coefficient β the average level of the outcome variable in the treated group in the reference period -5, see section 4.

discussion of this result).[20] Following the loss of employment, unemployment benefits increase markedly during the initial three-month period, rising from a negligible level (€27) to an average of €868. This finding is consistent with the National Unemployment Insurance Agency's report, which indicated that the average unemployment benefit was €990 in March 2022 (Unedic, 2023). The delay in unemployment benefits (UB) reaching their maximum level immediately following job loss can be attributed to the automatic deferral of UB for households that receive payment of untaken vacation and extra-legal severance payment, in addition to potential registration delays. Other income sources, such as social security benefits like family allowances, remain stable throughout the period. Labor earnings do not fall to zero after job loss because one household member may remain employed. Furthermore, it is noteworthy that, just before the job loss, the month of the full and final settlement, disposable income increases by €460, representing an average gain of 22%.

In conclusion, three financial impacts are evident around job loss: an initial increase in disposable income one month prior to job loss, a subsequent decline due to wage loss, and a gradual increase over the subsequent three months as delayed unemployment benefits are received.

The response margins to the income shock Households may adjust both spending and savings in response to the income shock (Figure 2 shows the results in levels around job loss, Figure 3 in percentage of pre-event income, Table 3 shows the event study estimates, Figure B.2 in the appendix

[20] This figure is slightly higher than the ratio equal to 56% of the coefficient $\hat{\beta}_h^{sa}(Y)$ corresponding to spending and income in table 3. This is due to the fact that the beta measure assesses the evolution of wage and unemployment benefits in relation to a hypothetical scenario where households remained employed (and potentially received a wage increase), rather than directly in comparison to the amounts recorded prior to job loss.

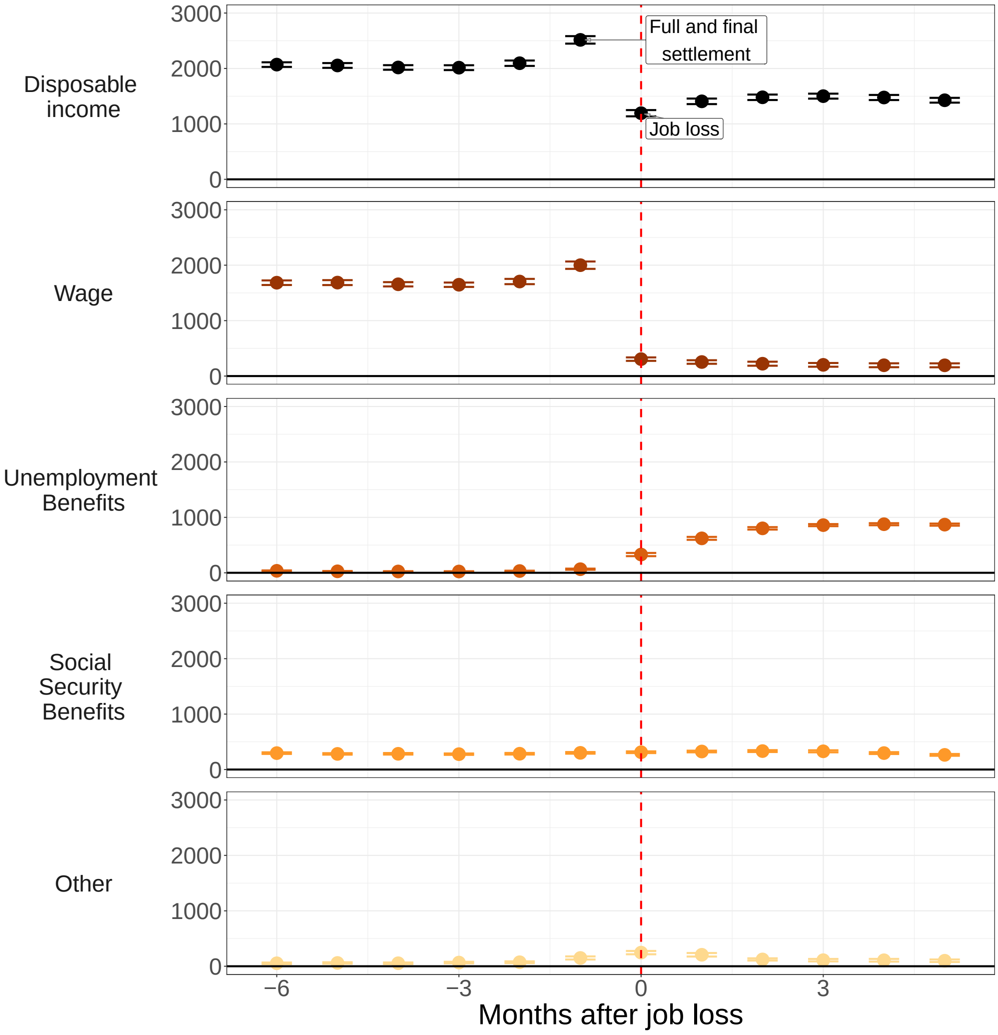

Figure 1: Trends in income, wages, unemployment benefits and social security benefits before and after job loss

Note. This graph is obtained thanks to the event study estimates around the unemployment spell using the variables income, wages, unemployment benefits and social security benefits as outcomes. The reference period in the regression is 5 months before job loss. We add the average level of each outcome variable 5 months before the shock to obtain the trajectories in levels. The dashed line corresponds to job loss. The horizontal bars around the point estimates correspond to 95% confidence intervals estimated by bootstrapping.

Source. La Banque Postale sample, authors' calculations.

corresponds to the direct plot of the event study).

After the job loss, liquid savings fall below 0 and households reduce their consumption consecutive to unemployment. In the very short-run, the first month following the onset of unemployment, spending exhibits a moderate decline, reaching a level approximately €40 lower than that observed five months prior to job loss. This represents a reduction of less than 2% of the pre-event income. Liquid savings constitute a more substantial margin of adjustment, diminishing by €700 relative to five months before job loss. Therefore, in the initial period following job loss, self-insurance accounts for approximately 80% of the income loss. However, in the medium term, six months after the commencement of an unemployment spell, spending tends to decline further, accounting for approximately 46% of the income loss (equivalent to the MPC observed in the final month).

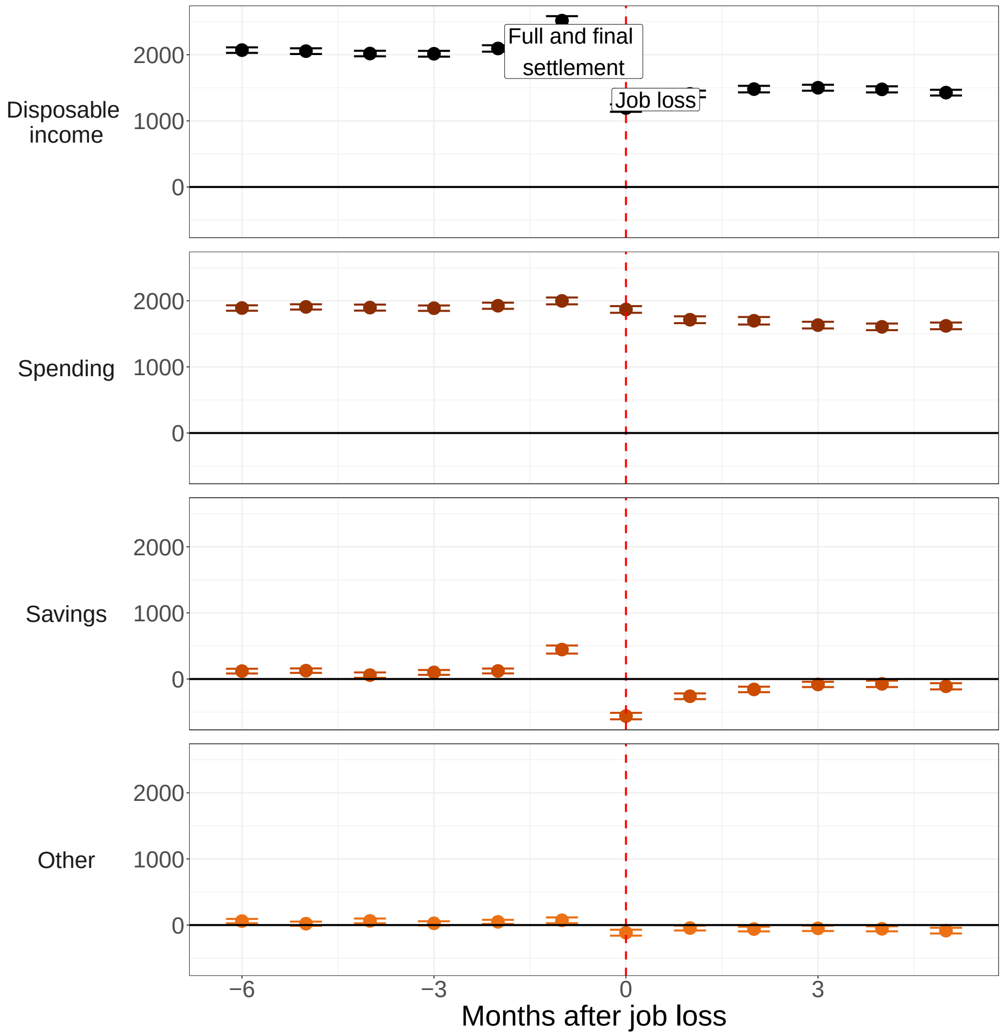

Figure 2: Trends in income, spending and savings to an unemployment spell

Note. This graph is obtained thanks to the event study estimates around the unemployment spell using the variables disposable income, savings and spending as outcomes. The reference period in the regression is 5 months before job loss. We add the average level of each outcome variable 5 months before the shock to obtain the trajectories in levels. The dashed line correspond to job loss. The horizontal bars around the point estimates correspond to 95% confidence intervals estimated by bootstrapping. The category others corresponds to financial operations that we were not able to classify such as outgoing bank transfers for example (see section 3.2)

Source. La Banque Postale sample, authors' calculations.

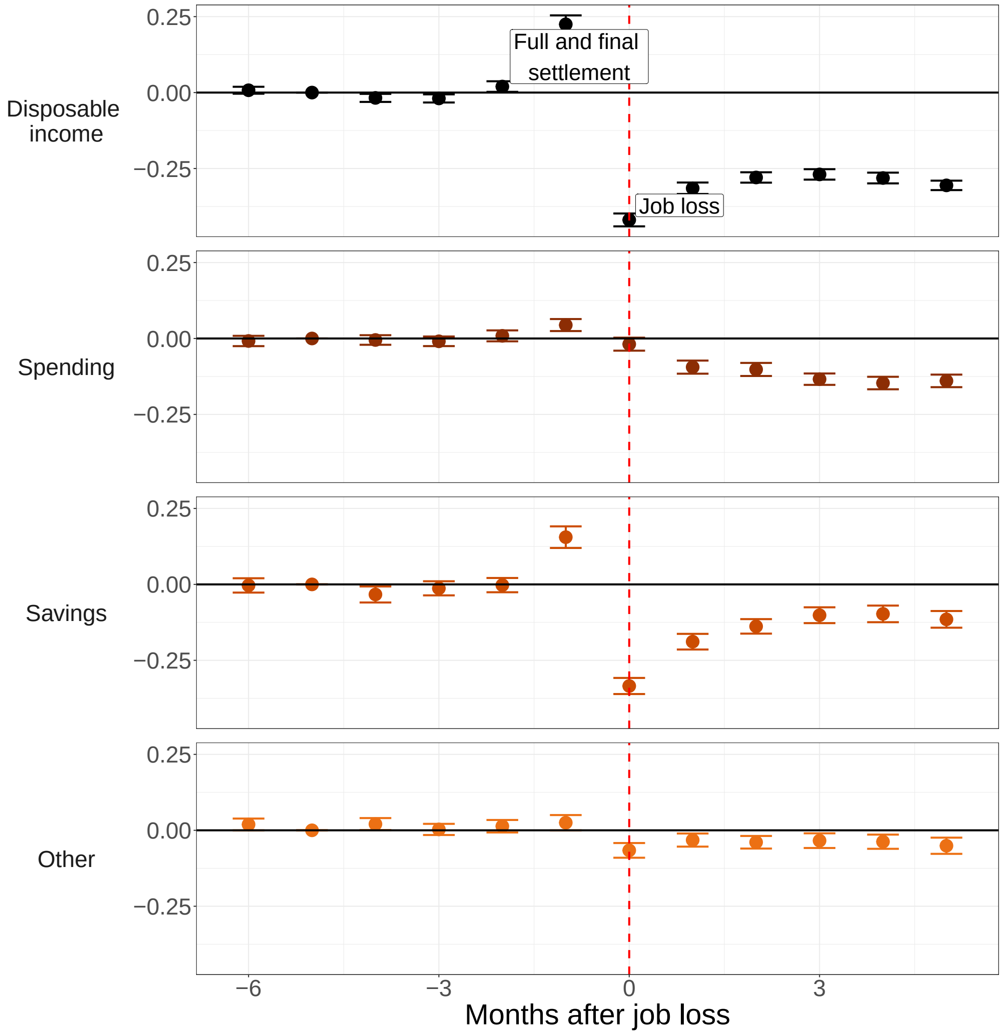

Figure 3: Response in income, spending and savings to an unemployment spell in percentage of pre-event income

Note. This graph is obtained thanks to the event study estimates around the unemployment spell using the variables disposable income, savings and spending as outcome. The coefficient in the event study are divided by the average level of disposable income 5 months before the shock to interpret parameters in deviation with respect to pre-event income. The dashed line corresponds to job loss. The horizontal bars around the point estimates correspond to 95% confidence intervals estimated by bootstrapping. The category others corresponds to financial operations that we were not able to classify such as outgoing bank transfers for example (see section 3.2).

Lecture. 3 months after job loss, spending have decrease by an amount corresponding to 12.5% of the disposable income 5 months before the job loss. *Sources.* La Banque Postale sample, authors' calculations.

Table 3: Event study: decomposing the response to the unemployment shock

	Disposable income	Wages	Unemployment Benefits	Social Security Benefits	Spending	Savings
	(1)	(2)	(3)	(4)	(5)	(6)
Time relative to job loss						
6 months before	16.23	0.4928	8.547***	13.09***	-18.01	-9.256
	(12.91)	(9.979)	(2.814)	(3.880)	(16.39)	(23.03)
5 months before	*Ref.*	*Ref.*	*Ref.*	*Ref.*	*Ref.*	*Ref.*
4 months before	-37.41***	-29.64***	-3.143	1.532	-10.76	-66.36***
	(13.34)	(9.538)	(2.444)	(3.856)	(16.06)	(23.67)
3 months before	-39.37***	-38.73***	-3.251	-4.895	-18.52	-27.31
	(14.15)	(10.01)	(2.691)	(4.277)	(16.76)	(22.68)
2 months before	40.58**	21.37	4.905	2.543	20.06	-6.768
	(17.23)	(13.78)	(3.147)	(4.799)	(18.14)	(23.92)
1 month before	462.5***	312.1***	38.73***	17.95***	95.26***	312.7***
	(26.65)	(22.16)	(4.808)	(5.385)	(19.78)	(28.92)
Job loss month	-863.1***	-1,382.0***	302.7***	30.54***	-37.09*	-691.8***
	(21.70)	(13.75)	(10.08)	(5.739)	(20.80)	(28.20)
1 month after	-647.6***	-1,433.1***	595.9***	43.04***	-191.8***	-388.3***
	(20.41)	(13.79)	(10.63)	(6.209)	(20.30)	(26.20)
2 months after	-573.4***	-1,464.8***	777.0***	50.77***	-205.9***	-284.3***
	(18.86)	(14.07)	(9.466)	(6.633)	(20.48)	(24.53)
3 months after	-556.9***	-1,485.6***	833.4***	45.54***	-273.0***	-208.8***
	(19.11)	(14.38)	(8.844)	(7.037)	(20.33)	(24.82)
4 months after	-578.9***	-1,493.1***	849.3***	14.42**	-297.8***	-202.7***
	(18.81)	(14.70)	(8.742)	(6.427)	(20.81)	(25.29)
5 months after	-628.9***	-1,493.3***	842.6***	-17.14***	-283.8***	-240.3***
	(18.56)	(14.85)	(9.016)	(6.485)	(22.04)	(25.69)
Fixed-effects						
Household	Yes	Yes	Yes	Yes	Yes	Yes
Calendar month	Yes	Yes	Yes	Yes	Yes	Yes
Fit statistics						
Observations	57,816	57,816	57,816	57,816	57,816	57,816
R^2	0.71997	0.86167	0.76613	0.80665	0.67684	0.17809
Within R^2	0.15949	0.51325	0.52844	0.02274	0.02464	0.04749

Note. Event study estimates around the unemployment spell using the variables disposable income, wage, unemployment benefits, social security benefits, savings, and spending as outcomes. The reference period in the regression is 5 months before job loss. The average values of the outcomes 5 months before the shock are: €2,054 for disposable income, 1,686 for wage, €27 unemployment benefits, €282 for social security benefits, €1,907 for spending and €127 for savings *Clustered (ID) standard-errors in parentheses. Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Source. La Banque Postale sample.

5.2 The Marginal Propensity to Consume during the period of unemployment

Estimation and results The marginal propensity to consume encapsulates households' responses to income fluctuations. It quantifies the change in consumption, measured in euros, corresponding to a one-euro change in income, reflecting either an increase or decrease. We estimate it for each relative period h as the ratio of the coefficient relative to consumption over the ratio of the coefficient relative to income:

$$MPC_h = \frac{\hat{\beta}_h^{sa}(\text{Spendings})}{\hat{\beta}_h^{sa}(\text{Disposable income})}$$

Standard errors are obtained by bootstrap resampling of households.

The marginal propensity to consume (MPC) is 0.20 for the initial positive shock resulting from the full and final settlement payment. In the month of job loss when income decreases, it is relatively low, at 0.05 (not statistically significant at 5%. Subsequently, there is an increase to 0.46 six months later (Figure 4). The mean MPC over the entire unemployment period is 0.36. This pattern indicates that during the initial months of unemployment, households do not reduce spending but instead draw on their liquid savings to mitigate the impact of the loss of income. As time progresses, they reduce their spending further. Consequently, spending drops by 2% in the month of job loss and by 15% six months later (Figure 5).

Interpretation Households reduce consumption during an unemployment spell and do not rely on private savings to entirely smooth the income shock. This phenomenon can be attributed to liquidity constraints: households may have been unable to save sufficiently in the past and cannot (or choose not

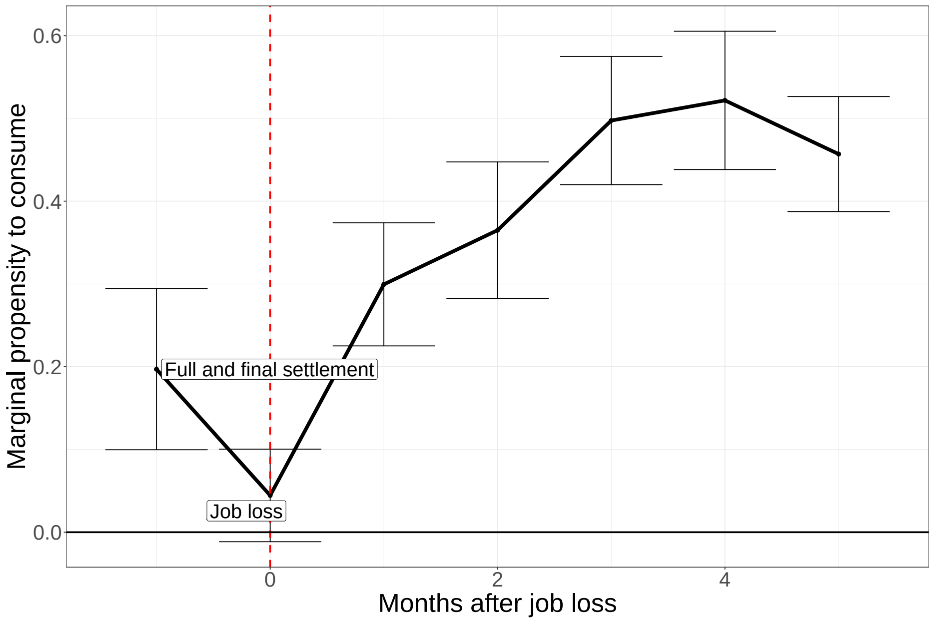

Figure 4: Monthly marginal propensity to consume

Notes: Marginal propensity to consume are computed for each month around job loss thanks to the event study estimates around the unemployment spell. MPC is the ratio of the spending estimates over disposable income estimates. The dashed line correspond to job loss. The horizontal bars correspond to 95% confidence intervals estimated by bootstrapping. MPCs are calculated from 1 month before job loss (before income does not vary, it is not possible to identify the effect).

Sources: La Banque Postale sample, authors' calculations.

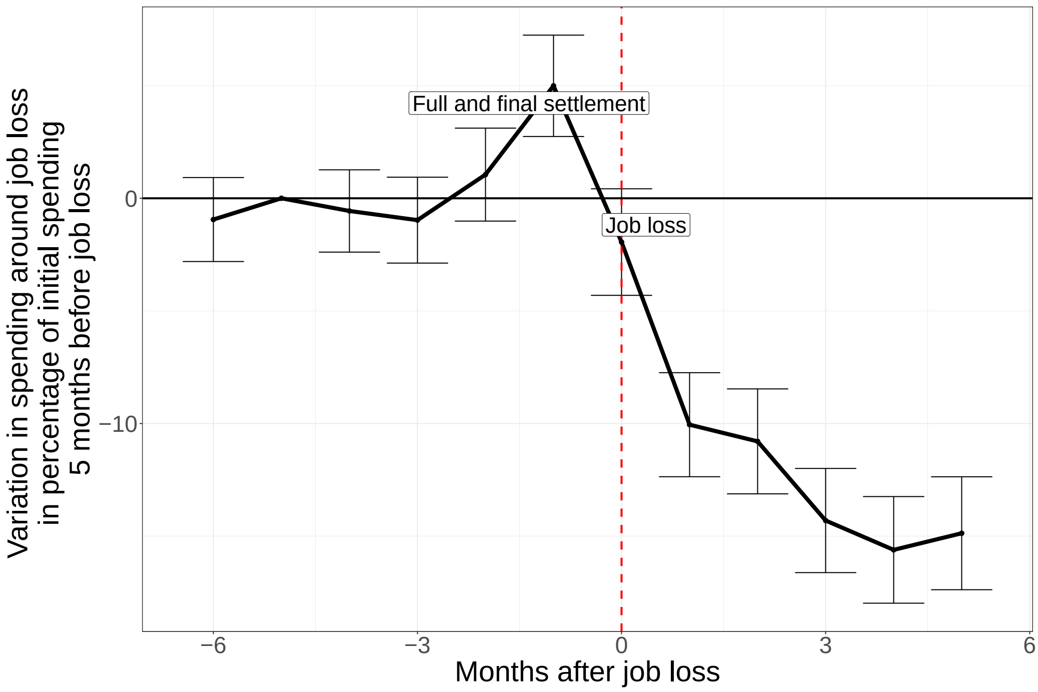

Figure 5: Variation in spending relative to spending before job loss
Notes. Variation of spending relative to the level of spending 5 months before the job loss.
Sources: La Banque Postale sample, authors' calculations.

to) borrow. This is corroborated by the fact that the marginal propensity to consume (MPC) is larger for households with low liquidity (see Section 6 and Figure D.2a). The gradual decrease in consumption over the duration of the unemployment spell could be explained by households' overoptimistic expectations of quickly finding a job (Spinnewijn, 2015). Households may not reduce consumption sufficiently in the initial months, believing they will secure employment soon. Additionally, this behavior could be attributed to present-biased preferences, where agents prioritize current consumption over future consumption (Ganong and Noel, 2019). The gradual decrease could also be explained by obligatory expenses that require time to be reduced. Direct debit payment (including energy and phone subscription) demonstrate this pattern (Table E.4 in appendix), yet are too small to entirely explain the slow decrease in consumption.

The observation that consumption increases in the month when full settlement payments are received lends support to present-biased behavioral explanations. In Brazil, Gerard and Naritomi (2021) also find an increase in consumption before job loss (positive MPC) due to severance payments despite the following decrease in income afterwards. This may appear counterintuitive, as one might expect households to save the money received from severance payments in order to maintain their consumption levels during the period of unemployment. However, these authors show that such results can be rationalized by a model of present-biased workers. This highlights the importance of the timing of payments in mitigating the welfare loss associated with unemployment.

The decline in consumption can be attributed to the necessity of adjusting spending in accordance with income levels. However, this can also be explained by changes in lifestyle habits. For example, households are no longer required to commute to work, which reduces the consumption of gasoline, and they may limit their dining out. In a different context, Aguiar and Hurst (2005) suggest that the decline in spending at retirement may be attributed to an increase in home production. The decomposition of the reduction in consumption into various categories of purchases, including groceries, fuel, pharmacies, and other leisure expenditures, reveals that the reduction in spending is particularly pronounced for goods that could complement work, such as fuel and restaurant expenses (Figure D.3 in Appendix).

6 Additional results

6.1 Heterogeneity along duration of unemployment

This section examines the differential impact based on the duration of consecutive months of unemployment. In particular, we examine the income and expenditure trajectories of households according to the length of time it took to secure new employment following unemployment spells. The probability of finding a job declines as the duration of unemployment increases. The job hazard rate during this period exhibits a decreasing pattern from 0.29 to 0.16 (Figure D.4 in Appendix). It might be consistent with observed and unobserved heterogeneity (Paserman, 2008), duration dependence (Kroft and Notowidigdo, 2016), or reference dependence (DellaVigna et al., 2017).

In the month households find employment, disposable income increases, reaching a level higher than before the initial unemployment spell (Figure 6). This phenomenon can be attributed to the fact that, during this month, households receive both wage and unemployment benefits. The subsequent months, disposable income remains higher than during the unemployment spell but lower than before the spell. This may be due to two factors: first, households may secure new jobs at lower wages with no more UB; second, some households return to unemployment.

For households unemployed for more than one month, the initial months of the spell show adjustments primarily through decreased savings with a moderate reaction in spending. The longer the unemployment spell lasts, the more pronounced the spending response becomes.

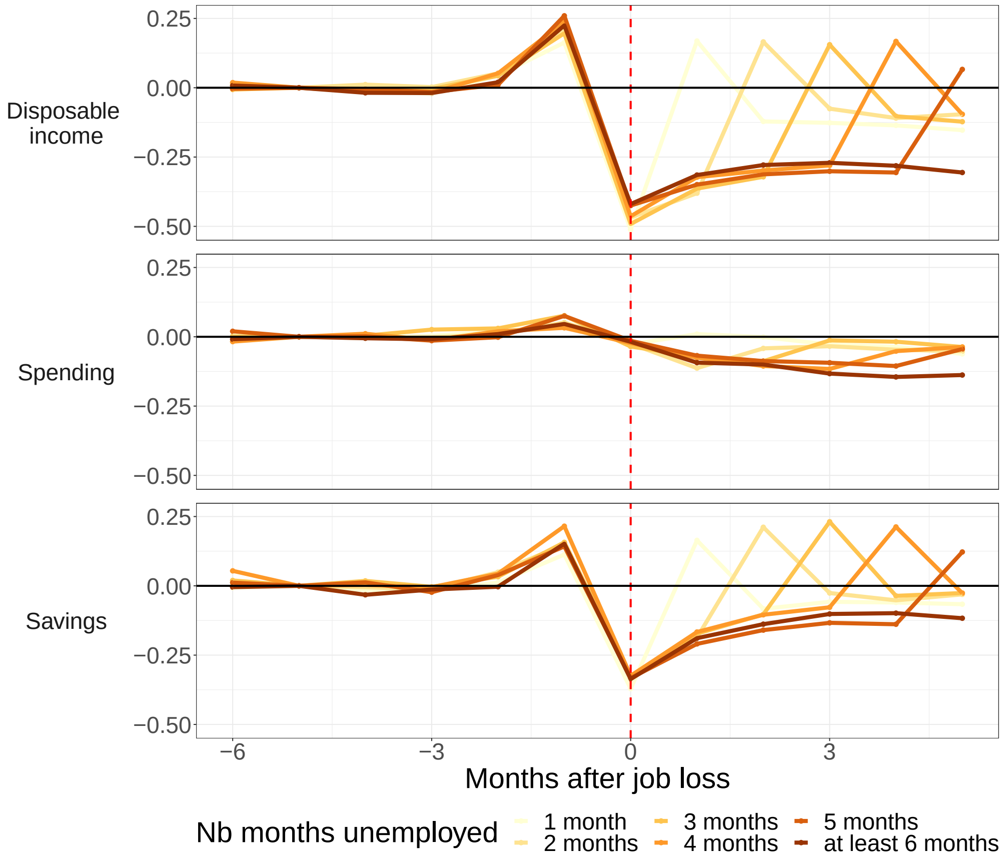

Figure 6: Income, spending, and savings response to an unemployment spell by duration of unemployment

Notes: Spells along number of consecutive months unemployed. This graph is obtained thanks to the event study estimates around the unemployment spell using the variables disposable income, savings and spending as outcome. The coefficient in the event study are divided by the average level of disposable income 5 months before the shock to interpret parameters in deviation (in percentage) with respect to pre-event income. The dashed line correspond to job loss.

Sources: La Banque Postale sample, authors' calculations.

6.2 Heterogeneity of MPC across households characteristics

We next estimate the marginal propensity to consume (MPC) out of the overall income shock along observable characteristics of the households. The computation relies on the event study estimates:

$$MPC = \frac{\sum_{h\in[0,5]} \hat{\beta}_h^{sa}(\text{Spending})/6}{\sum_{h\in[0,5]} \hat{\beta}_h^{sa}(\text{Disposable income})/6} \tag{4}$$

These MPCs show no much variation with respect to household size, age, or initial income (Figures D.1a and D.1b in the appendix and D.2c). However, they exhibit significant heterogeneity concerning liquidity and financial wealth (Figures D.2a and D.2b). MPC ranges from 0.56 in the bottom 25% of liquidity holders to 0.19 in the top 25%.

To estimate the impact of each characteristic on average MPC while holding all other factors constant, we perform the following regression on the treatment group:

$$\Delta\text{Spending}_{it} = \sum_k \beta_1(X_{it}^k)\Delta\text{Disposable income}_{it} + \sum_k \beta_0(X_{it}^k) + \mu_t + \varepsilon_{it} \tag{5}$$

where the Δ operator corresponds to the difference between the average value of the outcome in months posterior to the job loss and months before. $\beta(X)$ correspond to the estimated MPC which we allow to vary based on different explanatory variables through interactions. μ_t correspond to calendar month fixed effects. Liquidity is a crucial explanatory factor for the reduction in spending following a decrease in income (Table D.1 in the appendix).

7 Robustness

7.1 Threats to identification

The identifying assumptions of the dynamic treatment effects are (i) parallel trends and (ii) no anticipation of the treatment a months before the job loss, a is the anticipation horizon.[21]

The first assumption posits that, in the absence of treatment, outcomes would have evolved similarly for both "treated" and "control" groups. By definition, this assumption cannot be tested for months following the treatment. Nevertheless, the similar evolution of outcomes prior to the shock is reassuring. Evolution of income, spending and savings shows no clear different pre-trend from 6 to 2 months before event, i.e. before the beginning of an unemployment spell (Figure 1 and 2 for event study estimates and Figure E.1 for raw data). The coefficients of the event study regression (Table 3), from 6 months before the shock to 2 months prior, are either statistically insignificant or small in magnitude compared to the shock. If anything, it suggests that the disposable income of the treated group is slightly less dynamic than that of the control group, leading to a slight overestimation of the income shock. Reassuringly, our estimates without controls (Table E.2) yield similar results ruling out a subsequent major bias due to controls evolving with a different trend. One factor that could imply a violation of the common trend hypothesis is that the control group consists of households that have been employed for at least 12 consecutive months. Households in the control group are thus by definition stable in the labor market and may therefore experience more favorable wage trends than the rest of the population. In this respect, however, it is reassuring that the pre-trends are similar for the

[21] For more details as regards identification of two-way fixed effects models used in event study designs, see Sun and Abraham (2021).

control and treatment groups, and that we do not observe any change in trends over the period for the control group.

The second assumption implies that individuals do not anticipate the treatment by time $t + a$, thereby restricting their foresight. In practice, we set $a = 5$ months before job loss, thereby assuming away anticipation effects 5 months prior to this event. In our study, job loss is fixed at month 0, but the effects of treatment begin, at a minimum, one month before the job loss with the perception of full and final settlement. We have not observed any change in trends before this, which rules out the presence of major anticipation effects.

7.2 Parametric assumptions

Estimates have been tested for robustness across different specifications. In addition to the Sun-Abraham regressions, a traditional two-way fixed effects regression and a regression without controls or time fixed effects was conducted (Tables E.1 and E.2). The results are consistent across these methods, but adjustments are smaller without any controls, this can be explained by the slightly increasing trends, partially explained at least by inflation, in all outcome variables for treated and control group.

7.3 Generalisation of the results

It is essential to address the generalizability of our findings. The trajectory estimates presented in this study are derived from data collected from a single banking institution in a single country over a three-year period. In Section 3, we evaluated the representativeness of our data, and comparisons with other representative national surveys yielded encouraging results. However, we have to acknowledge that our findings are contingent upon a specific national context in years following the COVID crisis (with lockdowns) and unemployment benefits reforms in years 2019-2021. To test the robustness of our results, we excluded the period prior to 2021 from our sample and our findings are robust to this exclusion (Table E.3).

Moreover, our analysis is limited to unemployed individuals who are eligible for unemployment benefits. This restriction implies that our focus is on households that have experienced some degree of employment stability prior to job loss. Adjustments in spending and savings behavior for unemployed individuals who are not eligible for these benefits may differ.

Finally, in our main specification, MPCs are calculated for people who have been unemployed for 6 consecutive months. They therefore do not necessarily correspond to the average MPC of the population, due to the dynamic selection of the sample. The greater or lesser adjustment of consumption when losing a job may be directly related to the efforts made to get out of unemployment. It is possible that households that reduce their consumption more will remain unemployed for a shorter time and thus drop out of our main sample of interest. In this respect, the fact that the MPCs in the first months of unemployment are similar depending on the duration of unemployment tends to reassure us about the generalizability of the results (see Figure 6).

7.4 Other sources of adjustments

This study focuses on two primary sources of adjustment: savings and spending. However, other adjustment sources may play a significant role. Among these unobserved adjustment sources, potential candidates include spousal wages, consumer credit, or intra-family transfers. These sources of adjustment are not directly observable: it is not possible to identify which household member receives a wage-labeled transfer, distinguish transfers and direct debits to credit institutions (as only credit made to the bank is observed), or observe all intra-family inflows and outflows.

However, these margins appear, on average, to be of lesser magnitude than the spending and savings adjustments we measure, aligning with the findings of Andersen et al. (2020) in Denmark. First, there is no observable in wages following the income shock for households with one member remaining unemployed (Figure 1), which suggests that the other member of the couple does not increase their labor supply. Furthermore, marginal propensity to consume (MPC) is comparable between singles and couples (Figure D.1a). Second, there is no evidence of an increase in consumer credit made to the bank. Third, there is no evidence of significant changes in private transfers, which include intra-family transfers, around the period of unemployment (Table E.4). Additionally, residual adjustments (categorized as "Other" in Figures 2 and 3) do not show substantial evolution.

8 Conclusion

This study examines the financial adjustments of French households in response to job loss using high-frequency bank account data. Our findings indicate that unemployment benefits partially offset the income loss, with 36% compensated through reduced consumption and the remainder through decreased liquid savings. The consumption reduction intensifies with prolonged unemployment, and households with lower liquidity exhibit higher marginal propensities to consume.

Our results highlight the critical role of liquid savings in financial resilience during unemployment. Policies enhancing household savings and providing targeted support to low-liquidity households could mitigate the adverse effects of income shocks.

On the empirical side, future research could try to estimate the impact of unemployment for ineligible households (while we focus only on the compensated) or try to describe the individual heterogeneity in the responses to shocks. On the theoretical side, future research could try to model the behavior of households in line with our empirical results. This would make it possible to simulate the welfare effects of different public policies.

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Appendix

A Representativeness and completeness

The following figures and tables display information that enables us to assess the importance of representativeness and completeness issues.

A.1 Figures

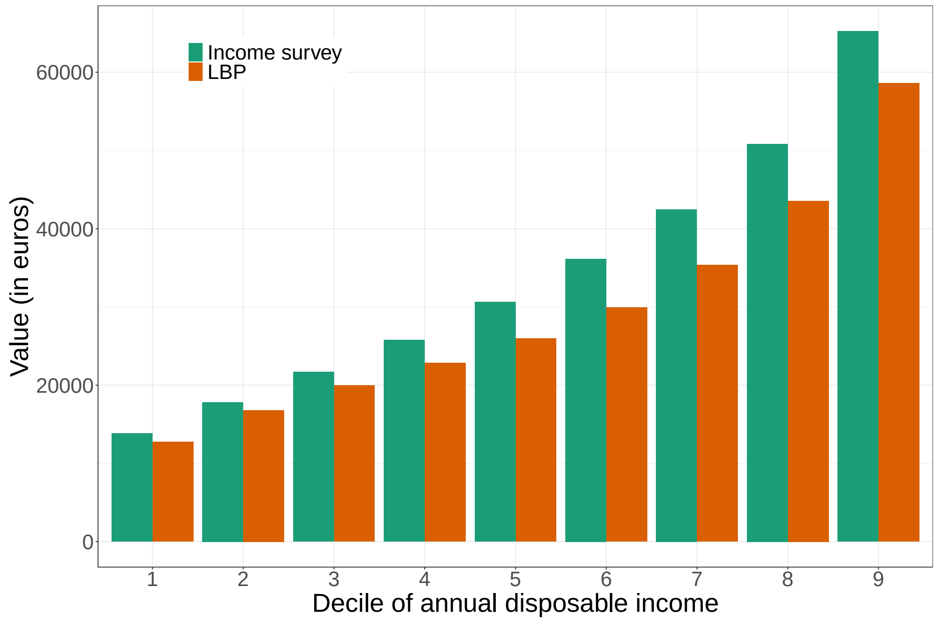


Figure A.1: Income deciles (La Banque Postale sample vs. national income survey, *enquêtes Revenus fiscaux et sociaux* 2018)

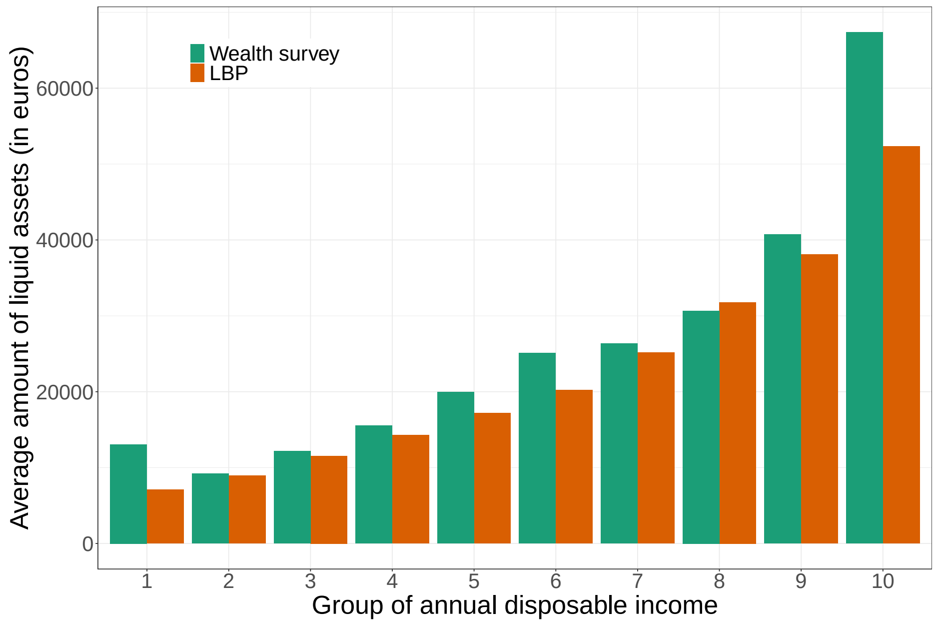


Figure A.2: Household liquid assets, by group of income (La Banque Postale sample vs. French wealth survey, *Histoire de vie et Patrimoine* 2017)

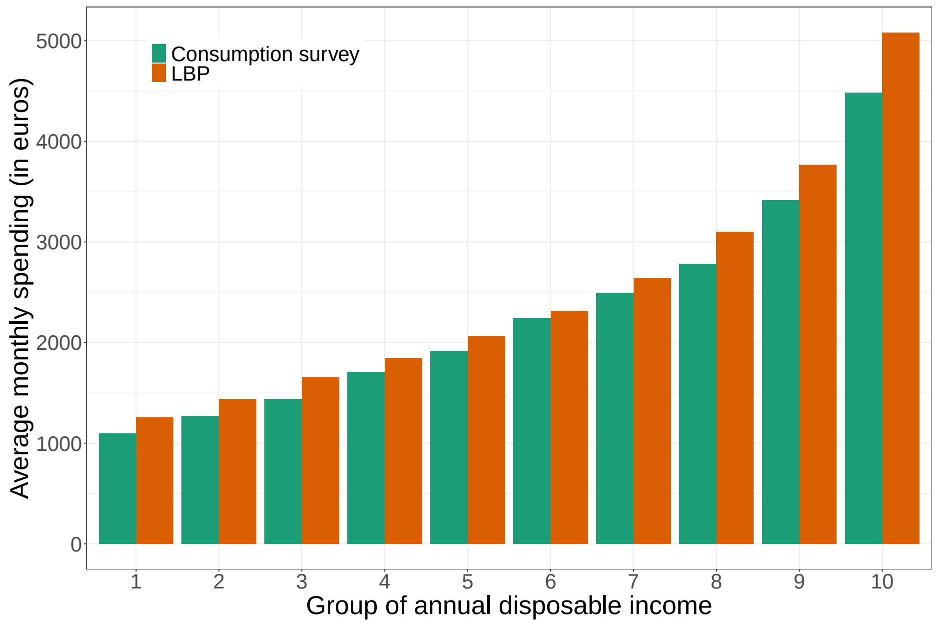


Figure A.3: Household spending, by group of income (La Banque Postale sample vs. consumption survey, *Budget des Familles* 2017)

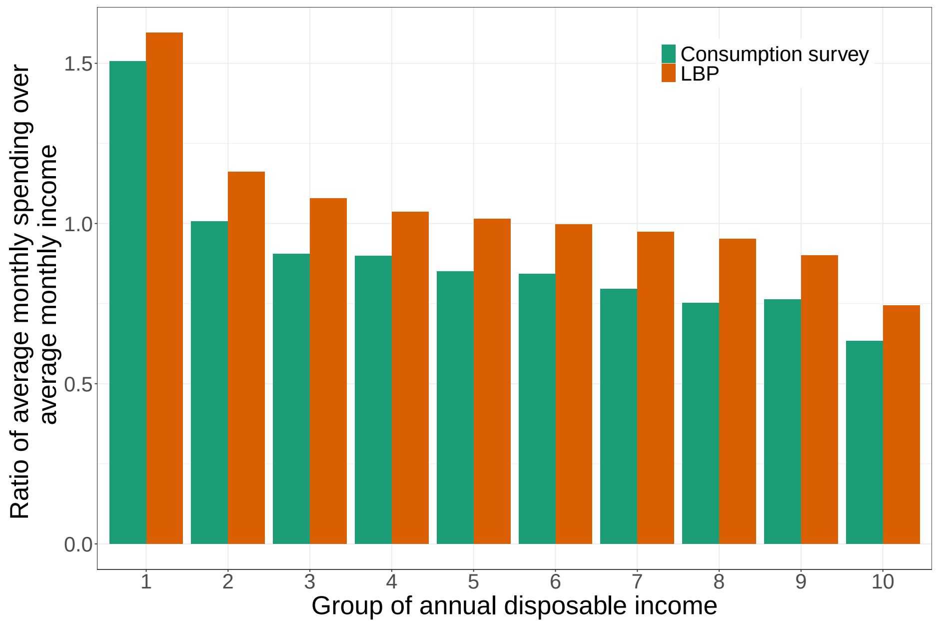


Figure A.4: Household spending-to-income ratio, by group of income (La Banque Postale sample vs. consumption survey, *Budget des Familles* 2017)

A.2 Tables

Table A.1: Income deciles (estimated from wealth national survey)

	All banks	La Banque Postale
Income group		
D1	14,500 (229)	11,930 (881)
D2	18,570 (213)	15,440 (418)
D3	22,530 (234)	18,790 (696)
D4	26,580 (330)	22,500 (779)
D5	31,580 (383)	25,890 (692)
D6	37,390 (375)	30,310 (827)
D7	43,790 (386)	36,340 (1,136)
D8	52,400 (448)	44,960 (1,216)
D9	66,370 (757)	57,260 (1,745)

Notes. Deciles: in €. Standard deviations (in parentheses). In the national survey, the median income is 31.670 in the sample and 25.890 for the households who primarily bank at *La Banque Postale*. Sample is restricted to households for which we can determined a primary bank. *Source.* French national wealth survey (*Histoire de vie et Patrimoine, 2017*).

Table A.2: Financial wealth deciles (wealth national survey)

	All banks	La Banque Postale
Income group		
D1	350 (33)	150 (30)
D2	1,033 (50)	475 (56)
D3	2,674 (152)	1,000 (105)
D4	5,640 (210)	2,200 (264)
D5	10,822 (351)	5,420 (471)
D6	18,948 (699)	10,220 (1,128)
D7	31,723 (865)	20,650 (2,282)
D8	56,090 (1657)	37,330 (2,785)
D9	116,119 (3205)	88,634 (7,045)

Notes. Deciles: in €. Standard deviations (in parentheses). In the national survey, the median financial wealth is 10.822 in the sample and 5.420 for the households who primarily bank at *La Banque Postale*. Sample is restricted to households for which we can determined a primary bank.
Source. Histoire de vie et Patrimoine, French wealth survey (2017).

Table A.3: Share of bank assets in their main bank

	Share of bank asset in the main bank
Constant	0.90 (0.01)
Income deciles group	
D1	*Ref.*
D2	-0.00 (0.01)
D3	-0.02 (0.02)
D4	-0.05 (0.02)
D5	-0.06 (0.02)
D6	-0.09 (0.02)
D7	-0.07 (0.02)
D8	-0.10 (0.02)
D9	-0.10 (0.02)
D10	-0.15 (0.02)

Note. Households in the bottom 10% of income detain 90% of their bank assets in their main bank, against 78% for those in the top 10%. On average, households detain 88.4% of their financial assets in their main bank. These results are obtained by regressing the household's share of bank assets in the main bank on a categorical variable that determines the household's income decile.
Source. Histoire de vie et Patrimoine French wealth survey (2017).

B Event study graph

This section includes the plots of the event study estimates. It corresponds to the plot of the results in table 3.

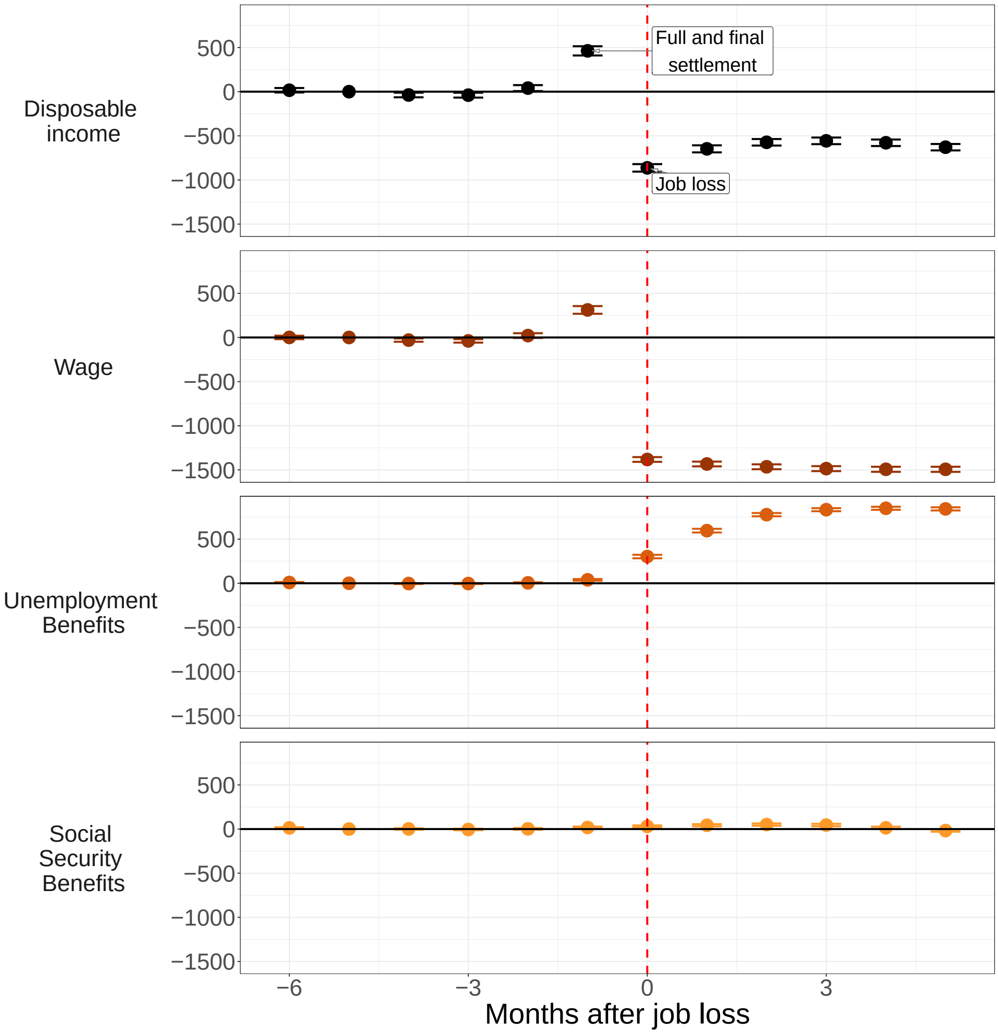


Figure B.1: Event study estimates for various outcomes: disposable income, wage, unemployment benefits and social security benefits

Note. This graph is obtained thanks to the event study estimates around the unemployment spell using the variables income, wages, unemployment benefits and social security benefits as outcomes. The reference period in the regression is 5 months before job loss. The dashed line corresponds to job loss. The horizontal bars around the point estimates correspond to 95% confidence intervals estimated by bootstrapping.

Source. La Banque Postale sample, authors' calculations.

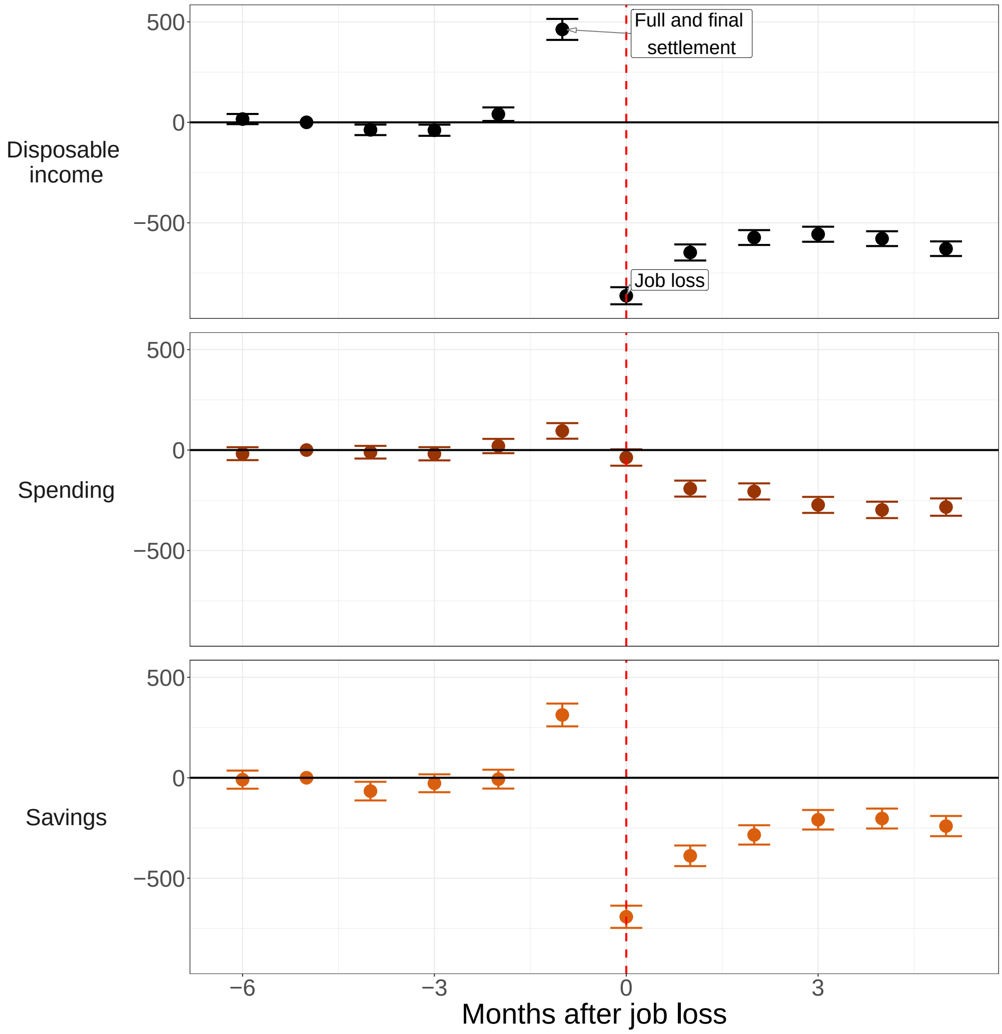


Figure B.2: Event study estimates for various outcomes: disposable income, spending and savings

Note. This graph is obtained thanks to the event study estimates around the unemployment spell using the variables disposable income, spending and savings as outcomes. The reference period in the regression is 5 months before job loss. The dashed line corresponds to job loss. The horizontal bars around the point estimates correspond to 95% confidence intervals estimated by bootstrapping.

Source. La Banque Postale sample, authors' calculations.

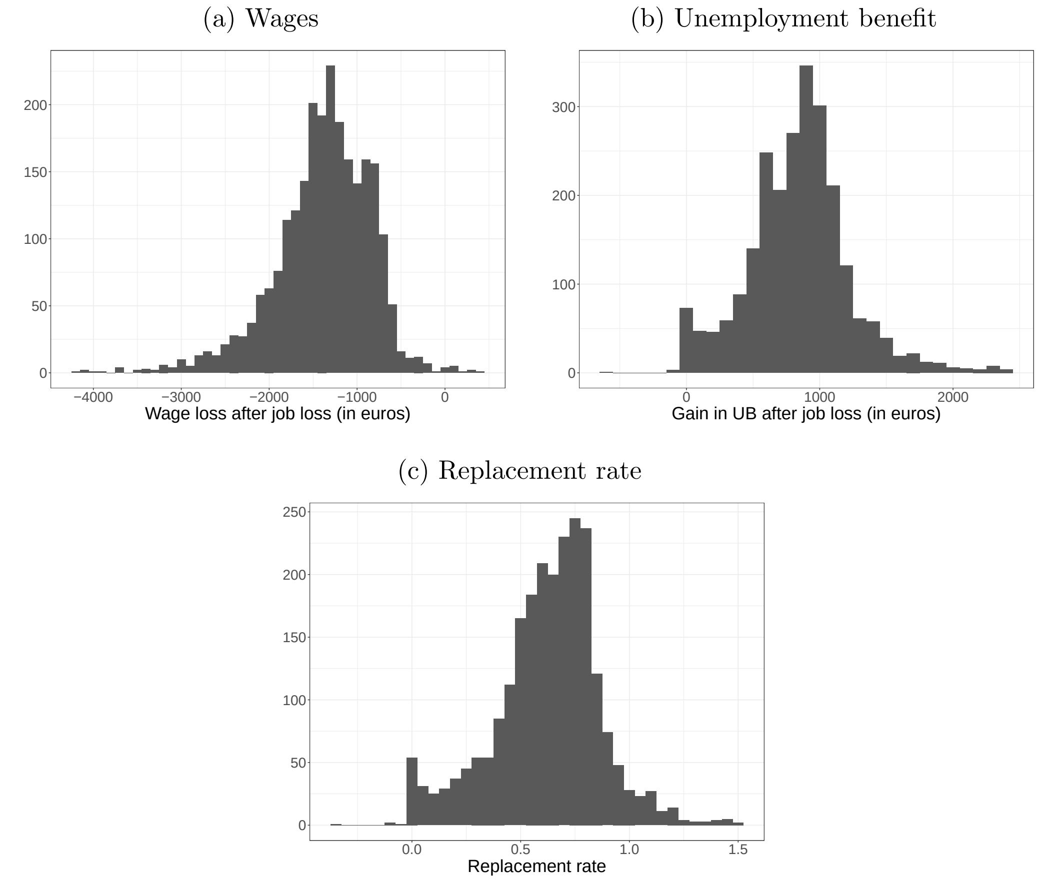
C Replacement rate

The adjustment of spending depends on the replacement rate, i.e., on the proportion of the wage loss that is compensated by unemployment benefits. In our main sample, the replacement rate is 65%. This rate is measured as the ratio of the average gain in unemployment benefits to the average loss in wages. The average gain and losses are measured by the evolution of each outcome variable from the period 6 to 2 months before job loss to 3 to 5 months after. The period from 1 month before the job loss to 2 months after is ignored because it encompasses final and full settlement as well as the delay in obtaining unemployment benefits. Figure C.1a, Figure C.1b, and Figure C.1c show the distributions of the wage loss during the unemployment spell, of unemployment benefits and of the replacement rate, respectively.

This rate is lower than the 71% net replacement rate calculated by unemployment agencies.[22] This discrepancy can be attributed to the fact that the rate was calculated before recent reforms, which tend to decrease the replacement rate. Previously, the sum of wages was divided by working days to determine the reference wage; now, it includes all calendar days since the start of the contract within the affiliation period. Additionally, our replacement rate is calculated based on monthly wages prior to job loss, whereas the national agency calculates its reference wage as the ratio of total wages earned 24 to 36 months before job loss to the number of calendar days since the beginning of the first employment in that period. Consequently, if households experienced wage increases during the two to three years before job loss or had months without employment, our estimated replacement rate would be lower than that of the national agency. Moreover, the agency's replace-

[22] This figure can be found on their website https://www.unedic.org/publications/le-montant-de-lallocation-chomage.

Figure C.1: Distribution around unemployment



Note. First and second panel show the distribution of wage loss and the increase in unemployment benefits due to job loss. Last panel shows the distribution of the replacement rate.
Source. La Banque Postale sample, authors' calculations.

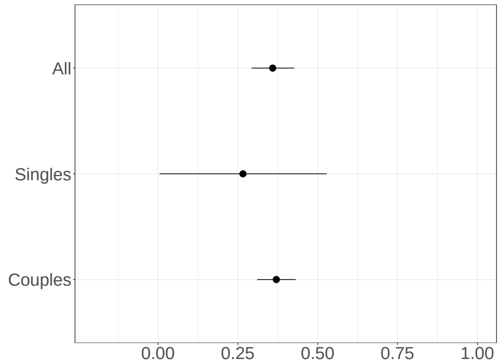
ment rate calculation excludes various bonuses that we include in our wage measure, such as reimbursement of business expenses, insecurity bonus, and holiday pay.

D Additional results

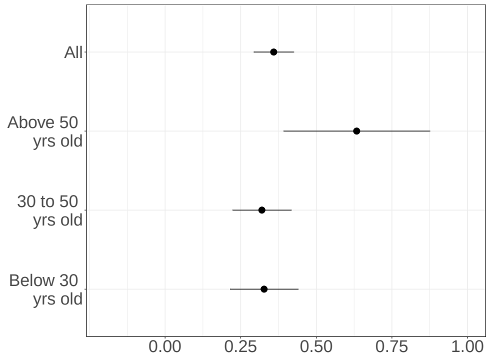
D.1 Heterogeneity in MPC

Figure D.1: Heterogeneity along age and family status

(a) Family status



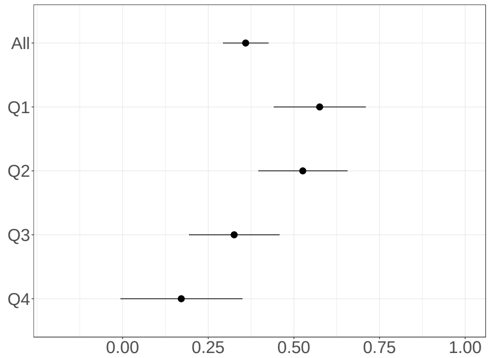
(b) Age



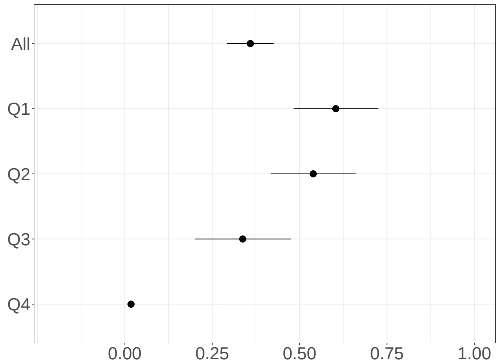
Note. Marginal propensity to consume out of a decrease in income during an unemployment spell.
Sources: La Banque Postale sample, authors' calculations.

Figure D.2: Heterogeneity along some observed characteristics

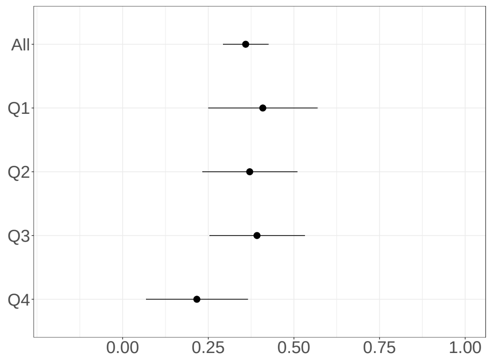
(a) Liquidity



(b) Financial wealth



(c) Income



Note. Marginal propensity to consume out of a decrease in income during an unemployment spell. Sample is divided in four groups of equal size depending on the level of liquidity assets held in the bank divided by the number of adults in the household in panel D.2a, depending on the level of financial wealth in panel D.2b and on the level of income in panel D.2c.
Source. La Banque Postale sample, authors' calculations.

Table D.1: Heterogeneity in the spending response

Dependent Variable:	Δ spending	
	(1)	(2)
Δ disposable income	0.3803***	0.4820***
	(0.0128)	(0.0469)
Family structure		
Δ disposable income $\times$ Couple		*Ref*
Δ disposable income $\times$ Single		-0.0388
		(0.0292)
Age		
Δ disposable income $\times$ Above 50 yrs old		-0.0496
		(0.0311)
Δ disposable income $\times$ 30 to 50 yrs old		*Ref*
Δ disposable income $\times$ Under 30 yrs old		-0.0376
		(0.0350)
Income group		
Δ disposable income $\times$ 1st group		*Ref*
Δ disposable income $\times$ 2nd group		0.1002**
		(0.0439)
Δ disposable income $\times$ 3rd group		0.0564
		(0.0422)
Δ disposable income $\times$ 4th group		0.0583
		(0.0391)
Liquid assets group		
Δ disposable income $\times$ 1st group		*Ref*
Δ disposable income $\times$ 2nd group		-0.1011**
		(0.0438)
Δ disposable income $\times$ 3rd group		-0.2054***
		(0.0427)
Δ disposable income $\times$ 4th group		-0.2444***
		(0.0401)
Controls	✓	✓
Calendar month fixed effects	✓	✓
Fit statistics		
Observations	4,781	4,781
R^2	0.15495	0.19855
Adjusted R^2	0.15478	0.19535

IID standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

D.2 Heterogeneity in the spending response

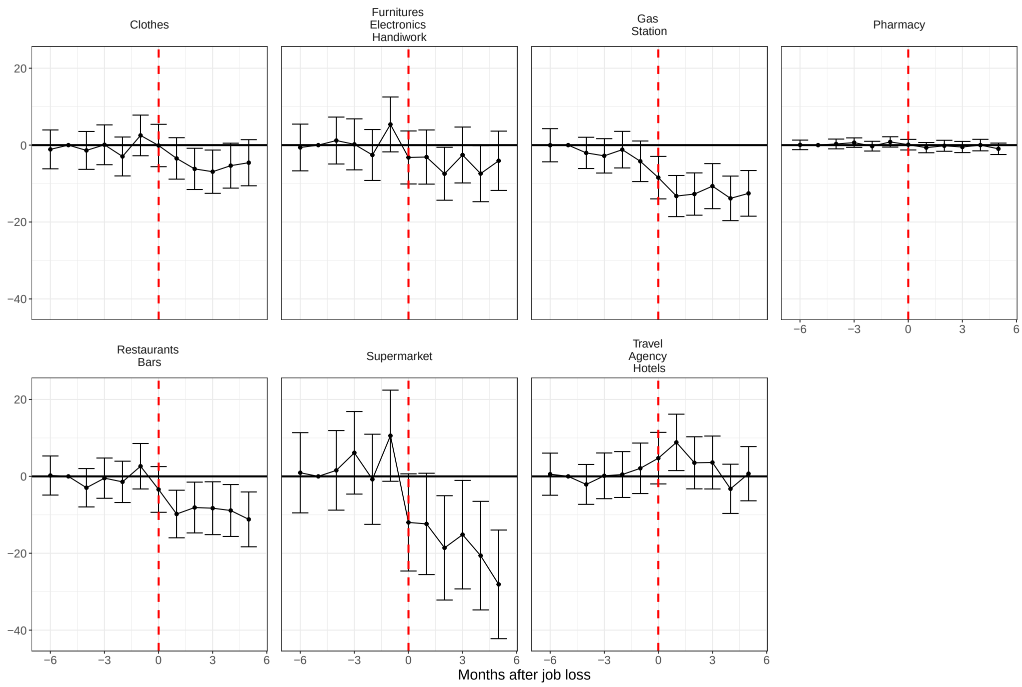


Figure D.3: Heterogeneity of spending response (by category of purchase)
Note. This graph is obtained thanks to the event study estimates around the unemployment spell using cards payments on various categories of merchants as outcomes (using MCC classifications). The reference period in the regression is 5 months before job loss. The dashed line corresponds to job loss. The horizontal bars around the point estimates correspond to 95% confidence intervals estimated by bootstrapping.
Sources: La Banque Postale sample, authors' calculations.

D.3 Descriptive statistics according to the length of the unemployment spell

Table D.2: Summary Statistics

Nb consecutive months unemployed	1	2	3	4	5	6	11
Disposable income	2178	2208	2092	2145	2211	1976	1893
Savings	88	84	63	72	123	35	18
Spending	2077	2071	1980	2039	2074	1951	1953
Financial wealth	11355	11793	9479	10860	10945	9082	9303
Liquid assets	9659	9674	8385	8721	8602	7489	6966
Illiquid assets	1695	2119	1095	2139	2342	1593	2337
Age	39	39	38	37	39	39	40
Nb adults	1.3	1.3	1.2	1.3	1.3	1.2	1.3
Sex							
... Women	46%	46%	44%	45%	44%	53%	61%
... Men	54%	54%	56%	55%	56%	47%	39%

Notes: Pecuniary amounts: in €.
Sources: LBP data, statistics are calculated 7 months before the unemployment spell. For households with several members, socio-demographics characteristics correspond to the ones of the older individual in the household.

D.4 Job finding hazard rate

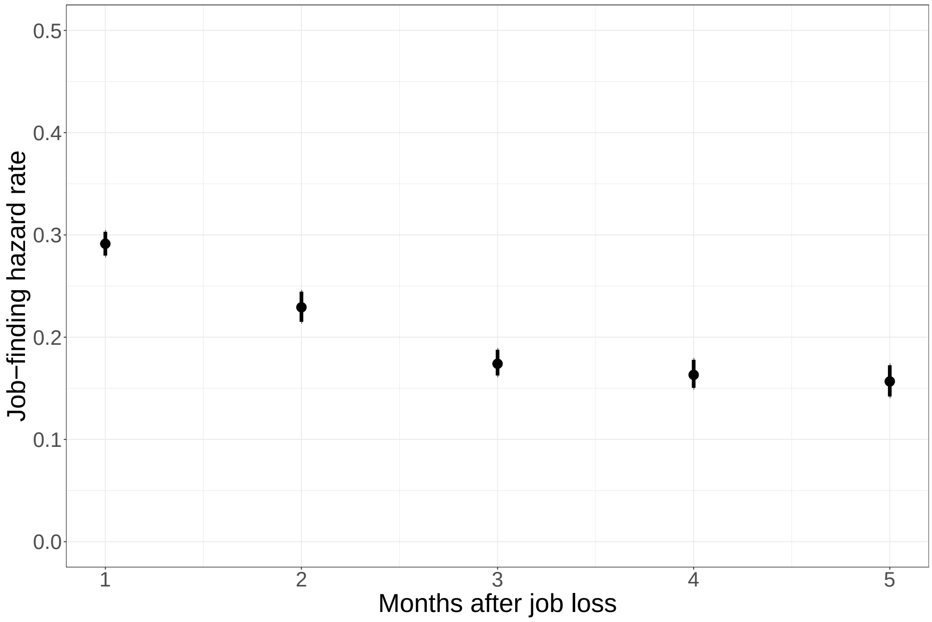


Figure D.4: Job finding hazard rate

Note. This graph illustrates the proportion of households returning to employment over time, conditional on the duration of their unemployment spell.

Lecture. 29% percent of households that experienced job loss are re-employed one month later. Among those who remained unemployed after the first month, 23% found employment two months after the initial unemployment period.

Source. La Banque Postale sample, authors' calculations.

E Robustness

Robustness tests of the parametric assumption

E.1 Figures

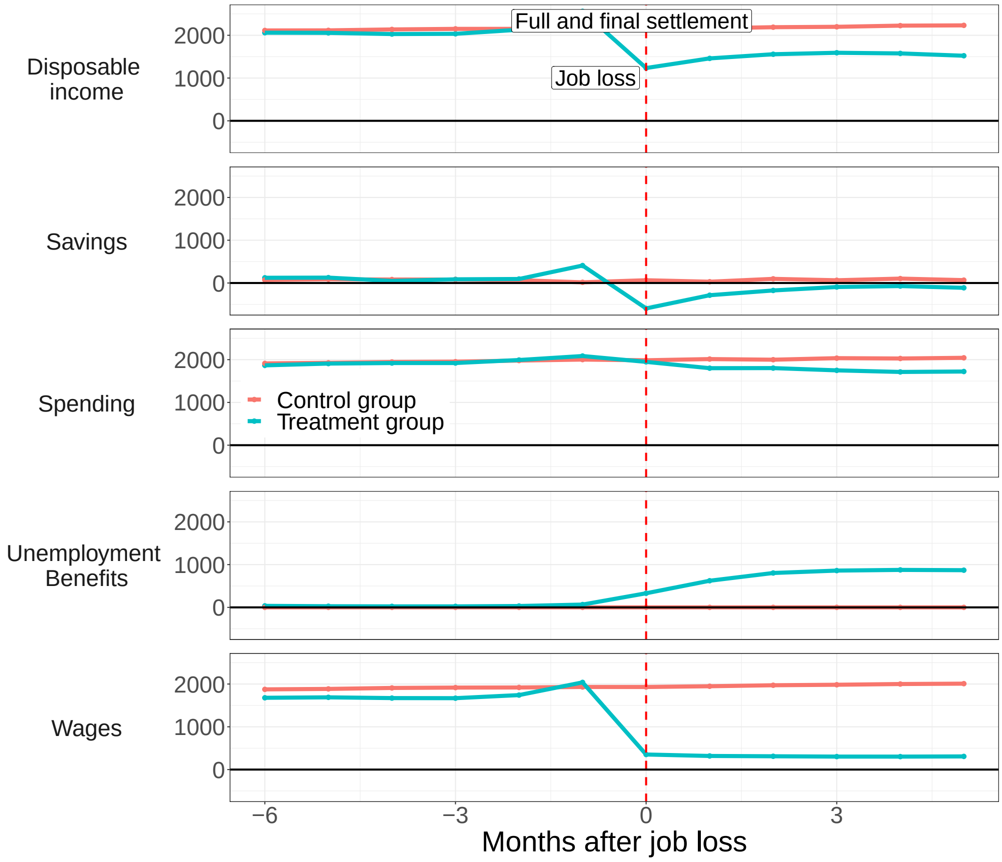


Figure E.1: Control and treatment group

Note. Evolution of main outcome for control and treatment group around unemployment spell. For the control group, the month for job loss is fictitious (drawn randomly).
Source. La Banque Postale sample, authors' calculations.

E.2 Regression tables

Table E.1: Event study: decomposing the response to the unemployment shock, two-way fixed effects

	Disposable income	Wages	Unemployment Benefits	Social Security Benefits	Spending	Savings
	(1)	(2)	(3)	(4)	(5)	(6)
Variables						
Months: -6	18.16	2.900	6.210**	14.41***	-21.50	-6.364
	(12.81)	(9.992)	(2.892)	(3.902)	(16.24)	(22.97)
Months: -5	*Ref*	*Ref*	*Ref*	*Ref*	*Ref*	*Ref*
Months: -4	-37.68***	-29.88***	-4.194	3.295	-11.18	-64.88***
	(13.38)	(9.651)	(2.570)	(3.836)	(15.94)	(23.64)
Months: -3	-44.26***	-41.45***	-5.984**	-2.980	-18.17	-30.54
	(14.23)	(10.10)	(2.922)	(4.270)	(16.66)	(22.73)
Months: -2	32.61*	17.97	1.229	2.883	21.48	-12.58
	(17.27)	(13.83)	(3.441)	(4.819)	(18.05)	(23.85)
Months: -1	453.8***	306.8***	37.87***	16.54***	96.28***	305.7***
	(26.85)	(22.42)	(4.992)	(5.405)	(19.67)	(29.07)
Months: 0	-870.8***	-1,385.2***	299.4***	30.68***	-37.73*	-695.3***
	(21.95)	(14.12)	(11.79)	(5.780)	(20.78)	(28.19)
Months: 1	-652.9***	-1,434.4***	593.1***	44.03***	-192.4***	-389.4***
	(20.50)	(14.22)	(11.71)	(6.246)	(20.32)	(26.09)
Months: 2	-576.4***	-1,465.1***	775.0***	51.63***	-205.9***	-284.8***
	(18.87)	(14.33)	(9.926)	(6.679)	(20.44)	(24.47)
Months: 3	-557.3***	-1,485.1***	831.8***	45.39***	-270.0***	-211.4***
	(19.19)	(14.62)	(9.281)	(7.068)	(20.39)	(24.80)
Months: 4	-579.1***	-1,491.8***	846.7***	14.96**	-299.3***	-201.8***
	(18.88)	(15.03)	(9.131)	(6.459)	(20.84)	(25.32)
Months: 5	-628.5***	-1,494.4***	842.4***	-14.89**	-287.4***	-238.5***
	(18.68)	(15.21)	(9.431)	(6.525)	(22.05)	(25.70)
Fixed-effects						
Household	Yes	Yes	Yes	Yes	Yes	Yes
Calendar month	Yes	Yes	Yes	Yes	Yes	Yes
Fit statistics						
Observations	57,816	57,816	57,816	57,816	57,816	57,816
R^2	0.71466	0.85851	0.72814	0.80374	0.67384	0.16882
Within R^2	0.14355	0.50214	0.45183	0.00805	0.01558	0.03675

Note. Event study estimates around the unemployment spell using the variables disposable income, savings and spending as outcomes. The reference period in the regression is 5 months before job loss. *Clustered (ID) standard-errors in parentheses. Signif. Codes: ***: 0.01, **: 0.05, *: 0.1.* *Source.* La Banque Postale sample.

Table E.2: Event study: decomposing the response to the unemployment shock, no control

	Disposable income	Wages	Unemployment Benefits	Social Security Benefits	Spending	Savings
	(1)	(2)	(3)	(4)	(5)	(6)
Variables						
Months: -6	3.293	-10.26	8.547***	11.15***	-41.52**	-3.733
	(12.67)	(9.818)	(2.822)	(4.027)	(16.47)	(22.84)
Months: -5	*Ref*	*Ref*	*Ref*	*Ref*	*Ref*	*Ref*
Months: -4	-27.99**	-17.28*	-3.143	-0.6970	12.90	-77.47***
	(13.42)	(9.679)	(2.457)	(3.957)	(16.25)	(23.70)
Months: -3	-22.32	-19.48*	-3.251	-7.028	13.01	-39.86*
	(14.14)	(10.08)	(2.699)	(4.388)	(16.83)	(22.48)
Months: -2	72.93***	52.88***	4.905	4.691	83.47***	-32.16
	(17.08)	(13.66)	(3.165)	(4.928)	(17.91)	(23.47)
Months: -1	502.8***	349.8***	38.73***	24.16***	176.9***	282.5***
	(26.60)	(22.12)	(4.838)	(5.479)	(19.27)	(28.58)
Months: 0	-822.1***	-1,336.5***	302.7***	35.47***	40.08**	-720.2***
	(21.31)	(13.74)	(12.35)	(5.751)	(19.88)	(27.43)
Months: 1	-597.1***	-1,370.4***	595.9***	46.41***	-107.3***	-411.5***
	(19.69)	(13.59)	(12.07)	(6.118)	(19.08)	(25.07)
Months: 2	-499.8***	-1,377.7***	777.0***	51.71***	-105.3***	-300.1***
	(17.65)	(13.48)	(9.994)	(6.505)	(18.51)	(22.85)
Months: 3	-465.8***	-1,385.1***	833.4***	48.19***	-158.3***	-219.2***
	(17.64)	(13.58)	(9.330)	(6.845)	(17.85)	(22.73)
Months: 4	-480.6***	-1,385.5***	849.3***	14.94**	-195.6***	-197.3***
	(16.99)	(13.71)	(9.192)	(6.043)	(17.66)	(22.66)
Months: 5	-534.8***	-1,381.0***	842.6***	-20.73***	-185.4***	-238.4***
	(16.09)	(13.46)	(9.564)	(6.018)	(18.22)	(22.51)
Fixed-effects						
Household	Yes	Yes	Yes	Yes	Yes	Yes
Fit statistics						
Observations	28,908	28,908	28,908	28,908	28,908	28,908
R^2	0.64753	0.81873	0.65218	0.74871	0.65960	0.16782
Within R^2	0.21902	0.69084	0.58694	0.01333	0.03047	0.07543

Note. Event study estimates around the unemployment spell using the variables disposable income, savings and spending as outcomes. The reference period in the regression is 5 months before job loss. *Clustered (ID) standard-errors in parentheses. Signif. Codes: ***: 0.01, **: 0.05, *: 0.1. Source.* La Banque Postale sample.

Table E.3: Event study: decomposing the response to the unemployment shock (after 2021)

	Disposable income	Wages	Unemployment Benefits	Social Security Benefits	Spending	Savings
	(1)	(2)	(3)	(4)	(5)	(6)
Variables						
Months: -6	24.12	15.32	9.641***	10.47**	-14.34	2.912
	(14.94)	(11.87)	(3.342)	(4.941)	(19.47)	(28.23)
Months: -4	-40.87**	-31.61***	-4.568	-1.078	-4.738	-84.40***
	(15.97)	(11.30)	(2.973)	(4.936)	(19.12)	(28.70)
Months: -3	-46.08***	-40.66***	-3.861	-5.177	-27.46	-30.73
	(16.53)	(12.00)	(3.298)	(5.398)	(19.73)	(27.88)
Months: -2	29.36	20.73	4.554	-1.350	5.487	-2.053
	(20.48)	(16.59)	(3.781)	(5.827)	(21.53)	(28.65)
Months: -1	438.1***	302.8***	38.23***	10.42	98.70***	278.7***
	(31.86)	(26.49)	(6.120)	(6.699)	(23.78)	(35.32)
Months: 0	-890.6***	-1,419.3***	341.4***	29.32***	-40.98*	-699.0***
	(25.62)	(16.25)	(13.03)	(7.183)	(24.91)	(34.56)
Months: 1	-666.5***	-1,469.5***	623.0***	43.75***	-193.1***	-389.9***
	(24.34)	(16.21)	(13.32)	(7.786)	(24.47)	(31.73)
Months: 2	-590.6***	-1,494.5***	795.9***	51.32***	-209.3***	-305.5***
	(22.55)	(16.89)	(11.43)	(8.332)	(24.56)	(29.64)
Months: 3	-574.6***	-1,521.9***	847.1***	46.63***	-264.0***	-210.6***
	(23.15)	(17.46)	(10.66)	(8.812)	(24.53)	(30.31)
Months: 4	-623.6***	-1,540.2***	864.3***	9.422	-306.0***	-227.2***
	(22.18)	(17.84)	(10.61)	(7.956)	(24.97)	(30.92)
Months: 5	-662.5***	-1,537.7***	857.8***	-17.26**	-313.8***	-230.5***
	(22.37)	(17.96)	(11.07)	(8.216)	(26.00)	(31.08)
Fixed-effects						
ID	Yes	Yes	Yes	Yes	Yes	Yes
month_integer	Yes	Yes	Yes	Yes	Yes	Yes
Fit statistics						
Observations	48,996	48,996	48,996	48,996	48,996	48,996
R^2	0.73307	0.86323	0.77720	0.81133	0.67805	0.17423
Within R^2	0.14899	0.48898	0.53810	0.01894	0.02108	0.03874

Note. Event study estimates around the unemployment spell using the variables disposable income, savings and spending as outcomes. The reference period in the regression is 5 months before job loss. *Clustered (ID) standard-errors in parentheses. Signif. Codes: ***: 0.01, **: 0.05, *: 0.1. Source.* La Banque Postale sample.

Table E.4: Event study: additional variables

Dependent Variables:	Private transfers	Energy Bills	Direct debit (without tax and credit)	Consumption credit
Model:	(1)	(2)	(3)	(4)
Variables				
Months: -6	-12.57***	0.6006	-2.107	0.6706
	(4.521)	(0.9771)	(3.959)	(0.7713)
Months: -4	-7.245	0.1548	1.499	0.8096
	(4.571)	(0.9346)	(3.550)	(0.6798)
Months: -3	-1.064	-0.6738	-4.229	0.4239
	(4.746)	(0.8730)	(3.911)	(0.7029)
Months: -2	-10.48**	-0.6106	-0.6128	1.780**
	(4.878)	(1.063)	(4.298)	(0.7377)
Months: -1	3.167	-0.0674	-2.074	1.317
	(5.354)	(1.034)	(4.666)	(0.9131)
Months: 0	11.78**	-0.2786	-8.694*	1.249
	(5.720)	(1.239)	(4.996)	(1.072)
Months: 1	8.653	-0.6829	-12.19**	2.624**
	(5.683)	(1.234)	(5.322)	(1.081)
Months: 2	3.108	-1.680	-16.62***	2.513**
	(5.949)	(1.364)	(5.782)	(1.259)
Months: 3	13.64**	-0.9511	-29.26***	1.716
	(6.321)	(1.375)	(6.026)	(1.323)
Months: 4	12.07*	-2.433*	-43.34***	2.555*
	(6.269)	(1.476)	(6.338)	(1.455)
Months: 5	15.38**	-1.223	-38.71***	2.600*
	(6.702)	(1.493)	(6.877)	(1.573)
Fixed-effects				
Household	Yes	Yes	Yes	Yes
Calendar month	Yes	Yes	Yes	Yes
Fit statistics				
Observations	57,816	57,816	57,816	57,816
R^2	0.48025	0.68453	0.85134	0.81907
Within R^2	0.00784	0.00648	0.01055	0.00639

Note. Event study estimates around the unemployment spell using the variables private transfers, energy bills, direct debit (without tax and credit), and consumption credit as outcomes in euros. The reference period in the regression is 5 months before job loss. *Clustered (ID) standard-errors in parentheses. Signif. Codes: ***: 0.01, **: 0.05, *: 0.1. Source* La Banque Postale sample.

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