DISCUSSION PAPER SERIES

DP16131

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LABOUR ECONOMICS

PUBLIC ECONOMICS

ISSN 0265-8003

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Discussion Paper DP16131
Published 09 May 2021
Submitted 06 May 2021

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www.cepr.org

This Discussion Paper is issued under the auspices of the Centre's research programmes:

- Labour Economics
- Public Economics

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Abstract

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JEL Classification: N/A

Keywords: Cost of unemployment, Social Insurance

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Acknowledgements
We thank Sumit Agarwal, Peter Ganong, Alessandro Martinello, Andrea Weber, as well as participants at the AEA 2021 Annual Meeting, at the CEPR workshop on New Consumption Data in Copenhagen, at the Danish central bank, at the CEPR Fourth European Workshop on Household Finance in Lund and at seminars for helpful comments and discussions. Center for Economic Behavior and Inequality (CEBI) is a center of excellence at the University of Copenhagen financed by grant DNRF134 from the Danish National Research Foundation. Financial support from the Economic Policy Research Network (EPRN), the Candys foundation, the Danish Council for Independent Research, and the Carlsberg Foundation is also gratefully acknowledged. This

research was facilitated by Adam Sheridan's Industrial PhD project, jointly financed by Danske Bank and Innovation Fund Denmark. Danske Bank did not review the conclusions of this paper before circulation and the opinions expressed are those of the authors alone and do not represent the views of Danske Bank. The use of the data for this project was approved by the Danish Data Protection Agency, Statistics Denmark, and the Ministry of Industry, Business and Financial Affairs.

How Do Households Respond to Job Loss? Lessons from Multiple High-Frequency Data Sets*

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Abstract

How do households respond to job loss, and which self-insurance channels are most important? By linking high-frequency customer data from the largest bank in Denmark with government administrative registers, we quantify a broad range of responses to job loss in a unified empirical framework. Two responses stand out: during the first 24 months after job loss, reductions in household spending account for 30% of the income loss, while lower saving in liquid assets accounts for 50%. Other response margins highlighted in the literature - spousal labor supply, private transfers, home equity extraction, mortgage refinancing, and consumer credit - are less important.

*Acknowledgments: We thank Sumit Agarwal, Peter Ganong, Alessandro Martinello, Andrea Weber, as well as seminar participants at the New Consumption Data workshop in Copenhagen, Danmarks Nationalbank, and the CEPR Fourth European Workshop on Household Finance at Lund, Sweden for helpful comments and discussions. Center for Economic Behavior and Inequality (CEBI) is a center of excellence at the University of Copenhagen financed by grant DNRF134 from the Danish National Research Foundation. Financial support from the Economic Policy Research Network (EPRN), the Candys foundation, the Danish Council for Independent Research, and the Carlsberg Foundation is also gratefully acknowledged. This research was facilitated by Adam Sheridan's Industrial PhD project, jointly financed by Danske Bank and Innovation Fund Denmark. Danske Bank did not review the conclusions of this paper before circulation and the opinions expressed are those of the authors alone and do not represent the views of Danske Bank. The use of the data for this project was approved by the Danish Data Protection Agency, Statistics Denmark, and the Ministry of Industry, Business and Financial Affairs.

People who lose their jobs typically experience a large and persistent drop in income even after social insurance is taken into account. Several studies show that household consumption also drops at the onset of unemployment.[1] But compared to the drop in income, the impact on consumption is typically moderate. For example, recent evidence for U.S. and Swedish households shows that the drop in spending amounts to only 20-30 percent of the drop in income at the onset of unemployment (Ganong and Noel 2019; Landais and Spinnewijn 2020).

The fact that reduced spending only accounts for a modest fraction of the income loss suggests that households self-insure against job loss to a significant extent, but how they do this remains an open question.[2] Any drop in income that is not matched by a drop in spending must be met by an increase in funds from other sources, a reduction in saving, or an increase in borrowing. Various response margins within each of these categories have been proposed by different strands of literature. Households can raise money inflows from other sources through an expansion of *spousal labor supply* (Lundberg 1985; Cullen and Gruber 2000; Stephens 2002; Hardoy and Schøne 2014; Halla, Schmieder, and Weber 2020) or through increases in *private transfers* from family and friends (Altonji, Hayashi, and Kotlikoff 1997; McGarry 2016). They can reduce *debt repayments* and/or increase *borrowing* by taking up alternative mortgage products or tapping into home equity (Hurst and Stafford 2004; Cocco 2013), or by borrowing more through unsecured lines of credit (Sullivan 2008). Finally, households may reduce saving by running down their buffer-stock of *liquid assets* (Carroll 1997; Basten, Fagereng, and Telle 2016). All these studies typically focus on a few response margins, with samples, data types, and methods varying across studies. Consequently, little is known about the relative importance of the various self-insurance mechanisms.

In this paper, we examine the empirical relevance and relative importance of each of

[1]For studies documenting persistent drop in income after social insurance, see Jacobson, LaLonde, and Sullivan (1993); Davis and Wachter (2011); Kawano and Lalumia (2015); Flaaen, Shapiro, and Sorkin (2019); Seim (2019). For studies documenting the drop in spending, see, e.g., Gruber (1997); Browning and Crossley (2001, 2009); Hendren (2017); Ganong and Noel (2019); Landais and Spinnewijn (2020).

[2]We use the term self-insurance to refer to all responses that weaken the contemporaneous impact of shocks to income on household consumption. These include responses that counteract the drop in income, such as higher spousal labor supply, as well as pure consumption-smoothing responses that transfer liquidity across time, i.e., borrowing or saving.

these self-insurance responses, as well as the effect on spending, in a unified empirical framework where all the margins are analyzed at the monthly frequency for the same sample of households, applying the same definition of job loss, and using the same research design. To do this, we turn to Denmark where a unique research data infrastructure makes it possible to combine data from many different sources. Specifically, we merge transaction-level data from the largest bank in Denmark with administrative data from multiple government registers. The combined data are so comprehensive that they capture all empirically relevant margins. We document this by showing that the measured self-insurance responses add up to the full size of the income loss. This feature is critical, as it allows us to assess the *relative* importance of all relevant response margins, something that has not been possible to do before. Several attractive attributes of our data are key to this result: First, the monthly frequency of the data enables sharp empirical identification in an event study design. Second, the detailed transaction data allow us to construct precise and comprehensive measures of monthly spending and saving, including subcategories of total spending.[3] Third, the data include detailed demographic information. This enables us to identify spousal labor supply responses and to study outcomes at the household level. Fourth, the data from government registers enable us to identify payments from employers and government agencies in the transaction data and thus separate them from private transfers from persons outside the household. Fifth, linking the transaction data to administrative data for the full population enables us to address concerns about completeness and representativeness that typically arise in studies using transaction data from a single provider (Baker, 2018).

Consistent with existing studies from other countries, we document a significant and persistent income loss for the average person affected by job loss.[4] Spending falls less than income: Over the two years following job loss, the cumulative spending drop amounts to 30% of the cumulative income loss. This leaves a gap of 70% that reflects the effects of

[3]Other studies impute spending from annual information about income and changes in assets and liabilities (e.g., Browning and Leth-Petersen 2003; Landais and Spinnewijn 2020) or use self-reported measures of spending (e.g., Parker and Souledes 2019; Kreiner, Lassen, and Leth-Petersen 2019).

[4]Since our focus is on the overall importance of the different response margins, the analysis concentrates on the responses to job loss for the average person. This reflects a mixture of responses for households experiencing short spells and long spells of unemployment.

household self-insurance. We find that this gap is filled by lower accumulation of liquid assets (~50%), increases in private transfers and other inflows (~10%), higher spousal labor supply (~5%), and lower net debt repayments (~5%). Mortgage borrowing and refinancing play only a small role. The combined effect of these responses closely adds up to the income loss in each month following job loss, suggesting that the analysis captures all relevant margins.

These results highlight that reduced saving in liquid assets is the most important way that households self-insure against job loss. Our analysis also documents important heterogeneity patterns related to the availability of liquidity: People who enter the job-loss event with significant liquid asset holdings primarily reduce saving in such assets while people who enter the job-loss event with few liquid assets reduce spending to a greater extent. This further emphasizes the key role of liquid assets, as it shows that the degree of self-insurance is lower among individuals with low liquidity.

Our study contributes in several ways. It is the first to provide a comprehensive assessment of the relative importance of a range of self-insurance margins by analyzing all responses for the same sample of households, applying the same definition of job loss, and using the same research design on high-frequency data. Importantly, we show that the measured responses add up to the reduction in disposable income following from the job-loss, indicating that our analysis captures all response margins that are empirically relevant. We categorize the responses into two classes of behavior: The first class comprises responses that affect the household's consumption possibilities by providing extra inflow of alternative resources to compensate for the loss of labor income (increases in spousal income, private transfers). The second class consists of responses that change the timing of consumption by adjusting borrowing and saving. Our results show that the latter class of responses is empirically far more important than the former in providing self-insurance against the income loss associated with job loss.

Another set of contributions relate to the novelty of the data. Completeness and representativeness are key issues when using transaction data from a single provider (Baker 2018; Ganong and Noel 2019). By combining bank data with data on household structure from the population registry, we can distinguish within-household transactions from external ones and measure all flows at the household level. This is important for obtaining a complete picture of the household's response to the job loss experienced by one of its members.[5] Furthermore, we link our data to annual information from the Danish Tax Authority about all accounts held in all Danish banks. This allows us to restrict the analysis to *exclusive* Danske Bank customers, i.e. households who do not have accounts at any other Danish bank. The advantage is that we, unlike previous studies based on transaction data (e.g. Gelman et al. 2014; Baker 2018; Kueng 2018; Olafsson and Pagel 2018; Ganong and Noel 2019; Gerard and Naritomi 2019), can ensure that our results hold in a sample where we know we observe the complete picture of household transactions and account balances. To address the issue of representativeness, we exploit the link between bank data and population-wide registry data and assess how our sample compares to the full population of job losers in key dimensions. We then show that our results are virtually unchanged when we re-weight observations to make the sample match the population in these dimensions.

Finally, an important benefit of our combination of transaction and administrative data is that it allows a more granular view of both income and spending than the two types of data sources can offer separately. Compared to studies based on data from government administrative registers, we can measure flows at higher frequency, break spending down on subcategories, and observe inflows that are not recorded in registers, such as person-to-person transfers. Compared to studies using transaction data only, we can characterize inflows into bank accounts with higher precision, separating salary payments, government income transfers and private transfers. This allows us to analyze the relative importance of different types of responses to job loss in much greater detail. In short, the data used in this paper are probably the most comprehensive data used to date for learning about the way households cope with job loss.

Our analysis uses data from Denmark, which is (like other Scandinavian countries)

[5] Imagine, for example, that the spouse of the person experiencing the job loss transfers money to replace the lost income 1:1, and that the latter person therefore maintains the same spending level while the spouse takes the full adjustment. Without the link to the spouse, we would wrongly conclude that total household inflows and spending were both unchanged in such cases.

known for its well-developed welfare state. Compared to most other countries, the unemployment insurance (UI) system is generous in terms of duration, providing benefits for up to two years. UI benefits replace 90 percent of previous earnings up to a cap. However, the cap is set at a level that roughly compares to the income from a full time minimum wage job. This implies that the replacement rate is low for many workers, as also witnessed by the fact that we find large and persistent drops in income following job loss for the average person in our sample. Moreover, we find spending responses that are comparable in magnitude to what is found by Browning and Crossley (2001) for Canada, Ganong and Noel (2019) for the US, and Kolsrud et al. (2018) and Landais and Spinnewijn (2020) for Sweden. Our analysis also confirms the increase in expenditures out of severance payments found by Gerard and Naritomi (2019) for Brazil. The spending effects of job loss thus appear similar across contexts and methods of expenditure measurement, suggesting that the level of self-insurance is also similar.

Other aspects of our analysis also corroborate recent findings from other countries: Landais and Spinnewijn (2020) use annual data on labor market performance, income and wealth from public administrative registers in Sweden. Like them, we find that access to liquidity matters for the spending response, and that the added-worker effect is limited. Finally, while our results emphasize the importance of adjusting saving to smooth consumption, we find that households do not increase borrowing, home equity extraction or the use of alternative mortgage products to any economically significant extent. This might seem surprising, given that Denmark has well-developed credit markets, including a mortgage market with many types of mortgage products that can potentially facilitate consumption smoothing (Campbell, 2012). However, it is consistent with recent evidence from the U.S. showing that the unemployed have high latent demand for mortgage refinancing but are constrained by their employment status (DeFusco and Mondragon, 2020). In summary, several pieces of our findings are consistent with evidence from other countries. But while the studies behind this evidence typically focus on a single or few response margins, ours is the first to provide a fully comprehensive analysis of all empirically relevant responses in a unifying framework.

The paper proceeds as follows: Sections 1-3 present background information on the institutional setting, data, and empirical methods, while section 4 presents the main results of our analysis. Section 5 presents additional analyses narrowing in on the importance of access to liquidity. Section 6 addresses concerns about completeness and representativeness. Section 7 concludes.

1 The Danish Institutional Setting

Labor market: The Danish labor market is characterized by flexible hiring and firing rules for employers combined with high income security for employees (Andersen and Svarer 2007). Dismissing workers is low-cost for employers compared to many other countries (OECD 2013). The notice period is typically 3 to 6 months for white-collar workers but shorter for blue-collar workers (Scheuer and Hansen 2011). This means that many laid-off workers have a few months to prepare for the impending drop in wage income.

The unemployment insurance system is partly funded by worker's contributions and partly by the government. Members of the insurance system receive benefits worth 90% of the pre-unemployment wage up to a cap of around \$3,000 per month. Because of this cap, actual compensation rates are considerably lower for many wage earners.[6] Benefits are taxed the same way as labor income. The maximum duration of UI benefits is two years. This provides high income security compared to many other countries, including the U.S. where the maximum duration is typically six months. Unemployed workers who are ineligible for UI benefits may receive a means-tested basic social transfer of around \$1,700 per month, with a supplement for families with children. Other government transfers, such as housing support and child benefits, are also income-dependent and may help reduce the income drop after job loss.

Financial markets: Households in Denmark buy financial services from two main types of financial institutions: retail banks and specialized mortgage banks. Retail banks

[6] In 2010, 91% of all wage earners in the age group studied in this paper had wage income exceeding the cap. 34% had wage income exceeding twice the size of the cap.

offer a wide range of financial services, including deposit accounts and various credit facilities. Mortgage banks only offer mortgage loans financed by covered bonds and they offer both fixed and adjustable-rate mortgages, with and without interest-only payments, and with a duration of up to 30 years. At origination, mortgage borrowers always face the current rate in the covered bond market. The maximum allowed loan-to-value ratio for mortgage loans is 80%.[7] Fixed-rate mortgages can be refinanced at a fairly low cost (Andersen et al. 2015). Mortgage debt is full recourse in Denmark, and defaults are rare (Kreiner, Leth-Petersen, and Willerslew-Olsen 2020).

Payment system: The payments landscape in Denmark limits the problem of "invisible" cash transactions when using bank transaction data to measure spending. Card usage is higher in Denmark than in any other European country and checks are no longer in use (Danmarks Nationalbank 2017). Almost all bill payments are made electronically, with over 95% of Danish households paying bills by direct debit (Danish Competition and Consumer Authority 2014). Only 16% of the value of point-of-sale retail transactions is in cash, compared to 39% for the U.S. (Danmarks Nationalbank 2017, Greene and Stavins 2018).

2 Data Construction

We link monthly information about individuals from six administrative data sources using a unique personal identity number assigned to all Danes at birth or first residence.[8] The combined data allow us to track individuals and their spouses from January 2009 to December 2016. This section describes the data and the construction of key variables. Appendix A contains further details about variable definitions.

Employment: We identify employment, job separations, periods of unemployment, and the individual's main employer using population-wide monthly payroll records collected by the Danish Tax Agency. Employers have to report wages for each employee

[7] Homeowners can go beyond the 80% limit by taking out additional collateralized loans from retail banks, but these are more expensive.

[8] Technically, the data providers send the data to Statistics Denmark who de-identify and store it on secure servers with remote access for researchers. Card et al. (2010) highlight the Danish micro data and data infrastructure as a blueprint for data construction.

to the tax agency and government agencies must report income transfers. Evasion is minimal (Kleven et al. 2011; Alstadsæter, Johannesen, and Zucman 2019). The records are used for tax collection and for computation of official employment statistics. Each record contains information about the gross amount paid, the month in which the amount was earned, a unique employer ID and sector code (for salary payments), and a transfer program code (for income transfers).

Disposable income, spending, saving, and non-mortgage debt repayments: We use transaction and account records from the largest bank in Denmark ("Danske Bank", henceforth just referred to as "the bank"). The data are similar to the JP Morgan Chase data used by Ganong and Noel (2019) in their recent study of spending through unemployment spells in the U.S. More than one third of the Danish adult population are in our data. The records contain information on all deposit and loan account balances, as well as detailed information about all transactions in each account.

We adopt a broad definition of household disposable income, equal to all external inflows to the household's bank accounts. To construct this measure, we focus on specific types of account inflows: First, direct deposits, which will include all labor, pension, and government transfer income. Second, person-to-person transfers that originate from outside the household, which include transfers from extended family, friends, and other external inflows. Third, cash deposits into accounts. We then break household disposable income down into salary income for each household member, income transfers from the government, and other. To do this, we combine the transaction data with the payroll data from the tax authority, as described above, allowing us to identify which income payments are from employers or government agencies (see Appendix C for details).

For spending, we focus on three types of payments – debit or credit card, in-store mobile, and bill – and cash withdrawals from ATMs. These categories account for almost 80% of all outflows in a given month for the average household (see Appendix B). For card and in-store mobile payments, we can categorize the type of spending using the four-digit Merchant Category Code (MCC) of the recipient business. MCCs are an international standard for classifying merchants by the type of goods and services they provide. For bill payments, we know the identity of the creditor for each transaction. The bank maintains a grouping of creditors into categories that correspond to the MCC grouping and we use this to categorize bill payments into the same groups as for card and mobile payments. To construct our baseline measure of monthly expenditure, we sum outgoing transactions by each of the payment methods and all cash withdrawals from ATMs. We use the categorization of spending to remove tax and debt payments, as well as fees paid to the bank.

Figure 1a compares the development in monthly card spending per person in our gross sample of bank customers vs. the full population. For the latter, we use aggregate statistics on card transactions and the number of adults in the population published by Statistics Denmark. The two series follow each other extremely closely, suggesting that our spending measure is accurate in timing and that our sample of bank customers does well in terms of representing trends in the broader population. Figure 1b shows average levels of total annual household spending across income groups based on our transaction data and compares them to estimates from Statistics Denmark's consumer expenditure survey. When averaging over households in all income groups, we get very similar spending levels across the two data sources, suggesting a high degree of completeness in our transaction data measure. Looking across groups, we see a slightly steeper income gradient in our measure than in the survey-based one, perhaps because of disproportional under-reporting at the top of the income distribution in the latter (Sabelhaus et al. 2013). But, overall, there is a strong correspondence between the two data sources in this dimension, suggesting that our transaction data measure captures cross-sectional variation in spending well.

We measure net repayments on non-mortgage loans as the change in end-of-month balances on loan accounts. Positive values correspond to net repayment, negative to net borrowing.

We define liquid assets as the sum of deposit account balances and financial securities. Consequently, our measure *of net saving* in liquid assets has two components: First, we use the change in end-of-month balances on deposit accounts to capture net saving in such accounts. Second, we add the net outflow across all accounts stemming from financial securities trades. Outflows from such trades reflect that the customer has purchased securities, while inflows reflect sales, so net outflows capture net investment. A particular advantage of this approach, as opposed to using the change in the end-of-month value of the portfolio, is that it separates the active net saving component from value changes due to capital gains and losses.

Household structure: The population register provided by Statistics Denmark contains annual demographic information about the entire Danish population. The data include information about age and gender of all individuals and, importantly, the personal ID numbers of spouses (including cohabiting partners) in each calendar year. This enables us to study outcomes at the household level rather than at the individual level where measurement can be biased by invisible intra-household effects if, for example, a spouse purchases more of the consumption goods of the household when unemployed than when employed.[9] The identity of spouses is also needed to identify spousal labor supply responses to the unemployment shocks.

Bank relationships: The Danish Tax Agency collects end-of-year information about all interest-bearing loans and deposits held in Danish banks by Danish residents. The data are third-party reported by financial institutions, and it contains account-level information about balances as well as a unique identifier for the reporting institution. With these data, we can address a key concern when working with transaction data from a single provider, namely whether the available data provides a complete picture of the activities of households who may also transact through other banks or intermediaries.

Mortgage loans: We use a loan-level data set collected from Danish mortgage banks by the Danish Ministry of Business and Growth and the Danish central bank. It provides an end-of-year snapshot of all active mortgage loans to private individuals in Denmark. It contains detailed information about the date of origin, time to maturity, original and outstanding balance, and interest rate on each loan. It also describes the type of loan,

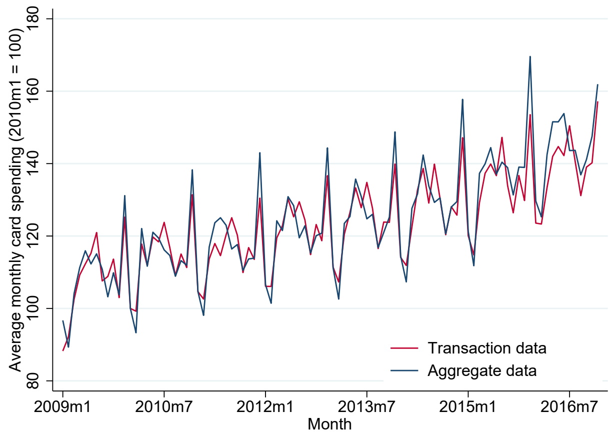
[9]We find that over 30% of actual couples are not linked to each other in the bank data, where a link is inferred from the existence of a joint account or a household identifier based on self-reporting relationships. Without information on household structure from the population register, these individuals would be treated as separate households.

including whether it is a fixed- or adjustable-rate loan and whether it is an interest-only loan. Combining the end-of-year snapshot in a given year with that of the previous year, we can detect whether there were any changes to an individual's portfolio of mortgage loans during the calendar year. We use the information on dates-of-origin for the new loan(s) to determine exactly when this change happened, and thus construct a high-frequency data set with information about mortgage loans held at the end of each month (see Appendix D for details).

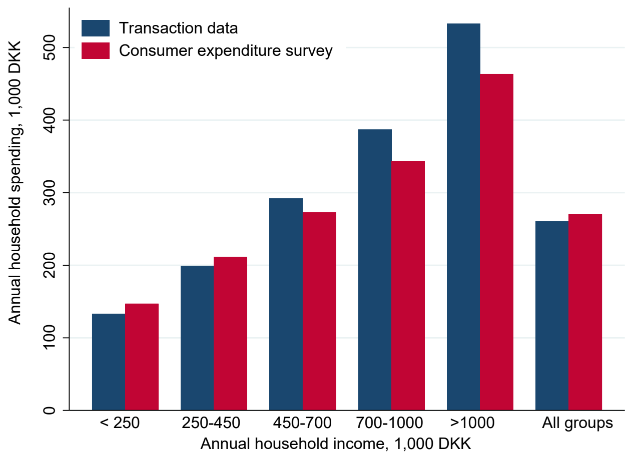
Mass layoffs: We use firm-level information about mass-layoffs reported by firms to the Ministry of Employment to isolate involuntary job losses. These data contain information about the ID of the firm, the extent of the planned layoffs, and the date of reporting to the Ministry of Employment. Through the firm ID, we can link them to the payroll data from the Tax Agency and thus construct a subsample of individuals who have been laid off shortly after their employer reported a planned mass layoff (see Appendix E for further details).

Figure 1: Measuring household spending: Transaction data vs. other sources

(a) Card spending per person



(b) Total annual spending per household, by income group



The figure compares aggregates of our measure of household spending based on transaction data to publicly available measures from existing sources. The transaction data measures are computed for our full sample of Danske Bank customers with at least five outgoing transactions per month for each adult person in the household. Panel (a) shows the development in average card spending per person in this sample (red line) vs. the full adult population (blue line), indexed relative to January 2010. The aggregate data for the full population are calculated from official statistics published by Statistics Denmark. To construct the series, we have divided total aggregate card spending in each month by the number of persons in the population above age 18. Panel (b) shows averages of total annual household spending across income groups. Income groups are defined by total annual household income in DKK. Average spending levels are computed within each group and each year and then averaged across the years 2009-16. Blue columns are based on the bank transaction data. Red columns are based on Statistics Denmark's Household Budget Survey and show total annual spending excluding the imputed value of owner-occupied housing.

3 Sample Selection and Research Design

We define an unemployment shock as a situation where the salary payments from the individual's main employer cease and total gross wage income drops below 1,000 DKK ($190, January 2010 price level), i.e. the month where the job separation occurs. The first month where these conditions are met is defined as the month of the job loss. To focus on transitions into unemployment, we require that the individual receives unemployment benefits or social insurance at some point between months -1 and 3 relative to the month of the job loss, and that he or she does not receive early retirement, sickness or parental leave benefits in this time period. Moreover, in order to identify shocks, rather than recurring events, we restrict attention to individuals who have gross wage income of at least 10,000 DKK ($1,920) for at least 18 consecutive months before the job loss and do not return to the same employer within three months after the job loss.[10]

The observation window for the event analysis is 18 months before to 24 months after the month of the job loss. The unit of analysis is individual-by-month, but outcome variables are generally measured at the household level by summing over the adult members.

Our analysis sample consists of individuals born between 1950 and 1979 who experienced an unemployment shock between July 2009 and December 2015.[11] We focus on stable households by requiring that the individual either stays single or has the same spouse in all of the months in which they enter the analysis. We exclude individuals if they or their spouse bought or sold real estate, or if they worked at the same firm as their spouse prior to the job loss. The former restriction is imposed because housing trades are associated with massive financial transactions, making it difficult to isolate the saving- and spending responses to the unemployment event. The latter restriction is

[10] The background for this approach is that administrative registry data do not contain information about whether job separations are the result of quits or layoffs. Our procedure is designed to narrow in on the subset of job separations that are likely the result of lay-offs while maintaining a sample size that allows us to estimate the effect of job loss with a relatively high level of precision. However, we also make use of data about mass layoffs, which occur less frequently, to verify that our results hold in a (much smaller) sample where all separations are almost certainly involuntary lay-offs.

[11] The payroll data cover January 2008 to March 2016. Since the definition of an event requires 18 months of data pre-event and 3 months post-event, this means that the unemployment shock must happen between July 2009 and December 2015 to satisfy all criteria.

imposed because correlated income shocks stemming from the same employer prevent us from cleanly examining the spousal labor supply effect of job loss.[12]

Finally, to produce our main results, we limit the sample to households who are active customers at the bank. Following previous literature, we define an active customer as a person with at least five spending outflows in each month of the observation window (Ganong and Noel 2019). For couples, we require that both partners are active customers.

Using outcomes based on account and transaction data of customers in one financial institution raises concerns about whether the sample is representative of the full population, and whether one captures the complete set of relevant transactions (Baker 2018). Our combined data make it possible to address these concerns. Table 1 provides summary statistics for individuals in different samples, measured six months before the month of job loss. Column (1) shows that our gross sample of job losers drawn from the full population consists of 66,844 individuals before restricting it to active customers in the bank. Introducing this restriction produces our baseline sample of 10,002 individuals, as shown in column (2). The active customers are on average slightly better educated, more likely to be single, work in the public sector, and reside in the capital region than individuals in the gross sample, and they also earn slightly higher incomes. But, overall, the two samples are quite similar in their socio-economic characteristics.

[12]In robustness analysis, we show that our results are insensitive to relaxing either of these sample restrictions.

Table 1: Sample selection and summary statistics

	(1) Gross sample	(2) Active customers (baseline sample)	(3) Exclusive customers
No. of individuals	66,844	10,002	5,224
	-------------------- Sample means ------------------------		
Female	0.43	0.47	0.48
Age	46.2	46.6	46.1
Couple	0.67	0.59	0.52
Capital region	0.33	0.44	0.42
Higher eduaction	0.23	0.28	0.27
Primary sector	0.01	0.01	0.01
Manufacturing	0.19	0.15	0.15
Homeowner	0.65	0.63	0.59
Annual gross income for person who lost job (DKK)	371,621	394,499	375,019
Share of hsh. bank deposits held at other banks	0.71	0.05	0.00
Share of hsh. retail bank loans held at other banks	0.71	0.11	0.00

Column (1) shows statistics for the gross sample of job losers drawn from the full population, i.e. with no requirements on customer status at Danske Bank. Column (2) shows statistics for the baseline sample of active customers, i.e., individuals who are customers at the bank and have at least five outgoing spending transactions in each month of the event observation window, and whose partner (if any) satisfies the same criterion. Column (3) is for the sample of exclusive customers, i.e., active customers who have no deposits or loans at other retail banks and whose partner (if any) satisfies the same criterion. All variables measured in month -6 relative to the month of job loss, except the following: Annual gross income, measured over the calendar year in which month -6 occurs; shares of household loans and deposits held at other banks, measured at end of calendar year before month -6. Appendix Table A1 provides additional summary statistics for each of the three samples.

The active customers hold a non-trivial share of their deposits (5%) and non-mortgage loans (11%) with other retail banks. Column (3) shows statistics for a subsample of *exclusive* customers, defined as active customers who do not have deposits or loans at other retail banks at any time during the observation window. In section 6, we show that our results are unchanged if we instead use this subsample, alleviating concerns about lack of completeness. The same is true if we instead impose representativeness by re-weighting observations in the sample of active customers to match the socio-economic background characteristics of the gross sample shown in column (1).

We estimate the dynamic effects of job loss using a standard event study model:

$$y_{it} = \gamma_t + \delta_i + \sum_h \beta_h \cdot \mathbb{1}[e_{it} = h] + \epsilon_{it}, \tag{1}$$

where i indexes individuals, t indexes calendar months, y_{it} is the outcome of interest, γ_t is a year-by-calendar month fixed effect, δ_i is an individual fixed effect, and e_{it} is event time, defined as distance in months to the unemployment event, with negative values indicating that individual i has not yet lost his/her job in month t. We include observations up to 18 months before and 24 months after the month of job loss. Identification in this type of model requires two reference categories (see for example Dobkin et al. 2018) so we leave out the indicator variables for $h = -18$ and $h = -6$. We normalize all nominal outcomes by measuring them relative to the household's pre-event disposable income, defined as the average disposable income in months -18 to -3. To limit the influence of extreme outliers, we censor the normalized outcome variables at the 2.5 and 97.5 percentiles within each event month. Standard errors are clustered at the level of the individual to allow for arbitrary forms of heteroskedasticity and autocorrelation across observations for the same person.

The coefficients of interest are the β_h, which capture the dynamics of the outcome variable around the time of the job loss. Each coefficient expresses the difference in the (normalized) outcome in event month h relative to the pre-event level. As summary measures of the total impact on the outcomes over the full observation window, we sum the β_h estimates for months -5 to 24. These sums capture the cumulative net effects on each outcome over the time horizon we study – expressed in multiples of pre-event monthly household disposable income – and thus facilitate comparisons of effect sizes across outcomes.[13]

4 Main Results

Figure 2 shows our main results. It is based on estimation of equation (1) and shows the impact of job loss on monthly income (markers) and the response margins (bars) for the average household on a time line centered around the month of job loss.[14]

Job loss has a large effect on the affected person's after-tax salary income (black dots in Figure 2). Salary payouts are higher than normal in the two months before job loss, due to sizable severance payments for some individuals, but then drop sharply at layoff. The average drop corresponds to about half of the household's pre-event monthly disposable income, reflecting that most households also have income from other sources, including salary income earned by the spouse. Salary payouts recover steadily in the following months, as some of the laid-off individuals return to employment, but they never catch up to the pre-displacement level within the two-year window of our analysis. In month 24, the gap remains almost half its initial size. This is in line with previous findings of persistent income losses following the transition into unemployment (Jacobson, LaLonde, and Sullivan 1993; Davis and Wachter 2011; Kawano and Lalumia 2015; Flaaen, Shapiro, and Sorkin 2019; Seim 2019). The total cumulated effect on after-tax salary over the analysis horizon amounts to a loss of seven months of pre-event household disposable income.[15]

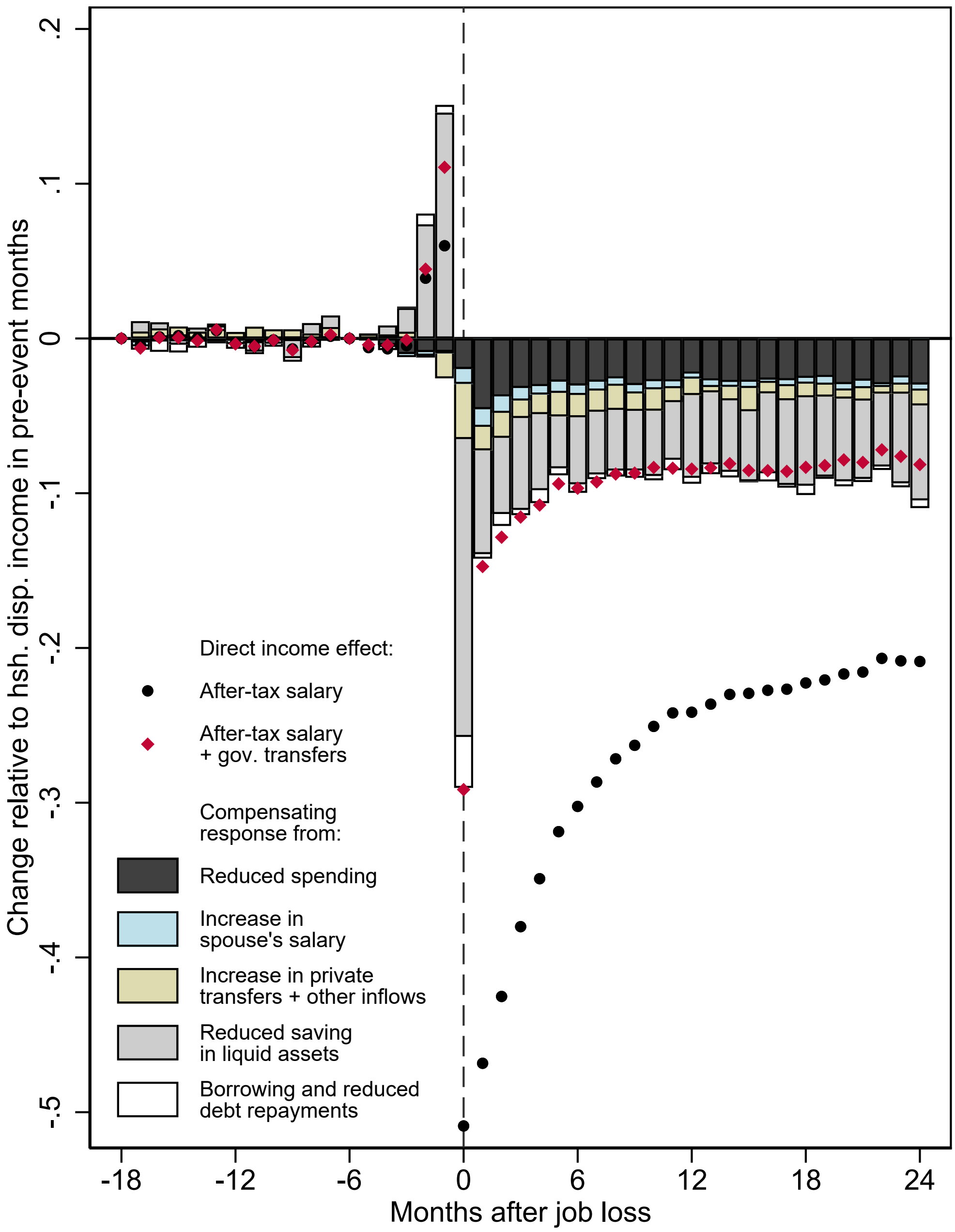
Social insurance provides significant income compensation. In Figure 2, the drop in after-tax income becomes much smaller when we include income transfers from the government (red dots). Over the full observation window, we estimate that these transfers

[13]We include the estimates for months -5 to -1 to capture effects taking place before the month of job loss, for example due to advance notices or severance payments.

[14]The key estimates underlying the figure, including their standard errors, are reported in Appendix Table A2. Appendix Figure A2 shows responses and confidence intervals separately for each outcome.

[15]The cumulative estimates, and their standard errors, are reported in column (5) of Appendix Table A2, as well as in column (1) of Table 2 below.

Figure 2: Income, spending, and self-insurance responses to job loss



The figure shows estimation results from the event study model (1) of the effects of job loss on a range of outcomes. All outcomes are measured relative to the household's average disposable income between event months -18 and -3 and winsorized at the 2.5 and 97.5 percentiles within each event month. Disposable income is defined as all external inflows to the household's bank accounts. Estimates for the effect on income are illustrated by series of shaped markers. The series labeled "After-tax salary" shows β_h coefficient estimates from a regression with after-tax salary payments for the household member who lost his/her job as the outcome. The series labeled "After-tax salary + gov. transfers" is the sum of these coefficients and the corresponding ones from a regression with income from government transfers as the outcome. Estimates for behavioral responses are shown in bars. We estimate coefficients for each outcome in separate regressions and illustrate the sums of these coefficients by the height of the stacked bars. In calculating these sums, each component is signed so that a negative value indicates a change that contributes to compensating for the loss of income. The series labeled "Borrowing and reduced debt repayments" shows the sums of coefficients for two separate outcomes: non-mortgage loan net repayments and mortgage loan repayments. Figure A2 and Table A2 in the Appendix show, respectively, full dynamics and selected coefficient estimates with standard errors for each separate outcome.

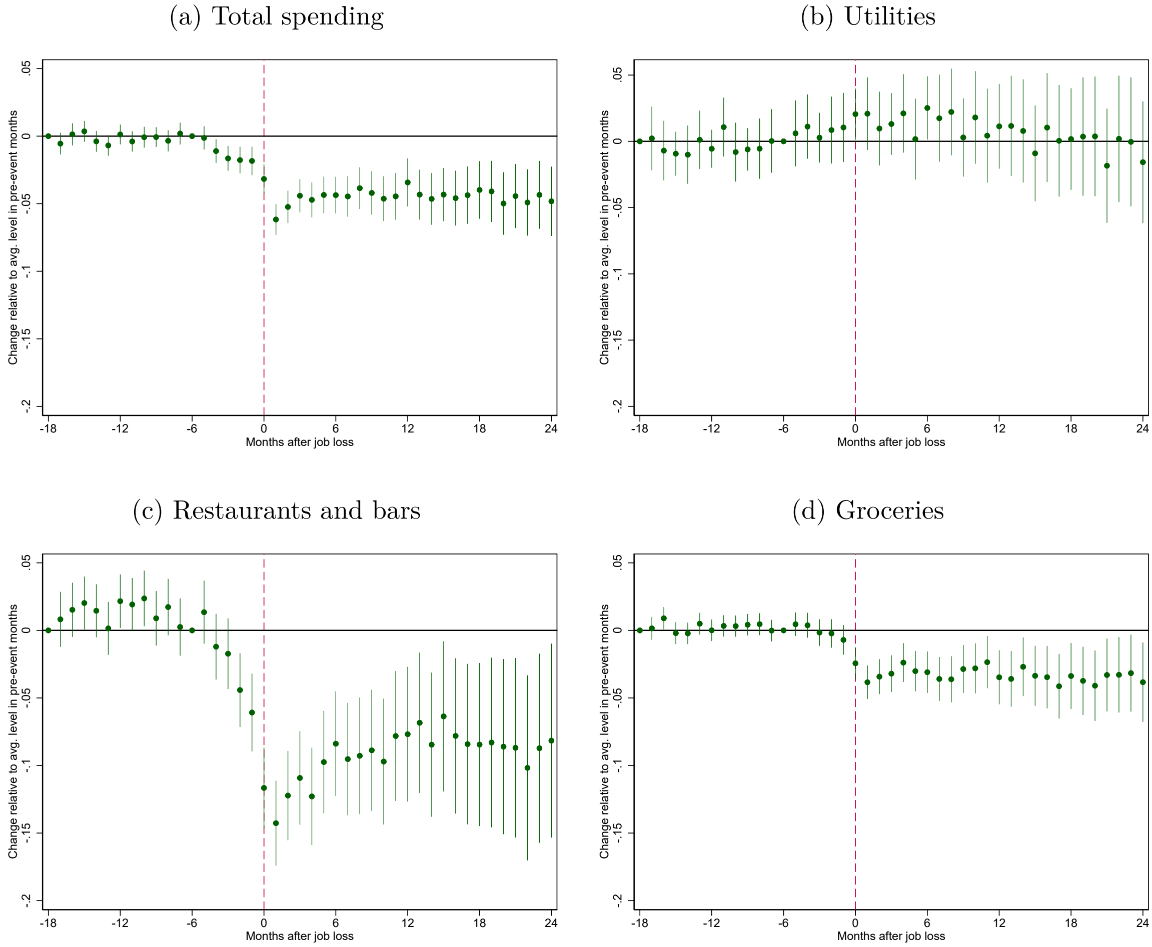
compensate for two thirds of the salary loss for the average household. Thus, the total effect on income, cumulated over the full analysis horizon, is a loss equivalent to about 2½ months of pre-event household disposable income.

The bars in Figure 2 show how households respond to compensate for this income loss. The joint effect of the compensating responses is illustrated by the height of the stacked bars. The fact that the estimated responses match the direct income loss almost perfectly – the height of the stacked bars is very close to the red dot in every month – suggests that we capture all relevant response margins.

Starting with household *spending,* we find a clear negative effect in all 24 months following job loss (black bars). Over the entire period, we estimate that the reduction in spending corresponds to 30% of the direct income loss. This aligns with the finding in the existing literature that the spending response to job loss, while significant, is substantially smaller than the corresponding effect on income. Figure 3 shows the response of spending when measured relative to it's own pre-event level and how the strength of this response varies across selected expenditure categories. As shown in panel (a), total spending drops at the time of job loss and then recovers somewhat. There is substantial variation across categories, however: In line with theory, we find that households maintain spending on consumption commitments (Chetty and Szeidl 2007, 2016), as proxied by utility bills, but cut down substantially on discretionary luxury goods, as proxied by restaurant and bar spending. In between these extremes, the relative drop in grocery spending is about the same size as for overall spending. This suggests that part of the the overall spending drop reflects an actual reduction of consumption and not merely self-insurance through postponement of luxury goods or durables purchases (Browning and Crossley 2000, 2009).

The sharp drop in spending at the time of job separation is preceded by a gradual decline in the months just before. To understand this, it is important to note that our analysis centers on the month in which we observe wage income dropping to near-zero. In reality, many employees receive notice about being laid off some months in advance, raising the possibility that they start adjusting long before that point. To assess whether this is the case, we exploit the fact that white-collar workers are employed on contracts

Figure 3: Relative responses for total spending and selected subcomponents



The figure shows estimation results from the event study model (1) of the effects of job loss on selected categories of household spending. Spending categories are defined by Merchant Category Codes, as described in Appendices A and B. All outcomes are measured relative to their own sample averages in event months -18 and -3 and winsorized at the 2.5 and 97.5 percentiles within each event month. Vertical lines represent 95% confidence intervals. Standard errors are estimated allowing for clustering at the level of the individual.

that secure them at least three months notice while blue-collar workers are employed on contracts that imply much shorter notice periods, sometimes even down to one day. As shown in Appendix Figure A3, splitting the sample by blue/white-collar status reveals that the decline in spending is indeed visible for white-collar workers as early as five months before the separation, whereas blue-collar workers do not change their spending until the separation. This supports the interpretation that the pre-event decline reflects anticipation due to notice periods.

We now turn to the various candidates that may explain the gap between the drop in income and the drop in spending following job loss. The *spousal labor supply* effect does not contribute much in this regard, as illustrated by the length of the blue bars in Figure 2. The cumulative change in spousal salary payouts over the full period covers 6% of the lost income of the laid-off person. This small increase is entirely along the intensive margin, with no significant effect on spouses' employment rate (Appendix Figure A4). In summary, the added-worker effect provides little self-insurance for the average household in our sample.

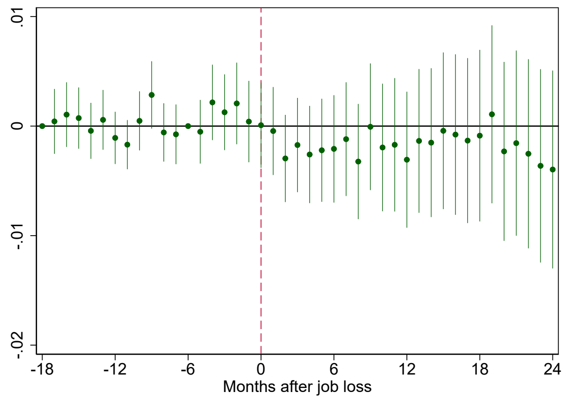
The effect on *private transfers and other inflows* is somewhat stronger as shown by the yellow bars in Figure 2. The cumulative increase in these inflows over the full analysis horizon is 0.3 months of pre-displacement disposable income, corresponding to 12% of the direct income loss. This reflects informal insurance through gifts and loans from extended family and friends but can also capture inflows stemming from sales of real assets or consumer durables.

We find only a modest impact of job loss on *borrowing* and *debt repayments* (white bars in Figure 2). This effect is strongly concentrated in month 1 after displacement where we observe a sizable increase in non-mortgage borrowing.[16]. For mortgage loans, we find a statistically significant − but economically modest − decrease in average monthly debt payments. As shown in Figure 4, this is driven by a small share of the households who convert their mortgage loans to loan types with lower debt service costs, whereas we find no impact on home equity extraction through mortgage refinancing. Over the full period, these borrowing adjustments compensate for less than 5% of the direct income loss. These findings are consistent with recent evidence in DeFusco and Mondragon (2020) showing that the unemployed have high latent demand for mortgage refinancing but are constrained by their employment status.

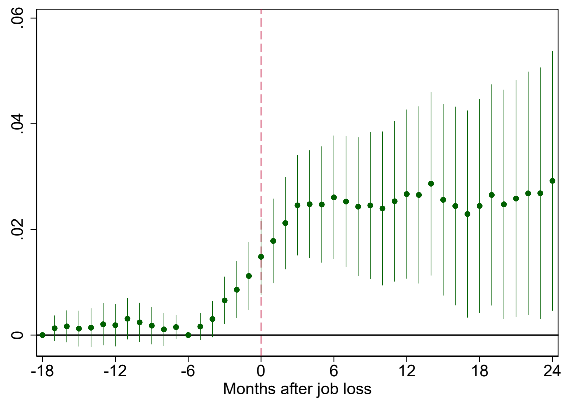
[16]Consistent with the increase in non-mortgage borrowing, we find some evidence that loan arrears become more prevalent after job loss. From the tax data, we have end-of-year information about arrears on any debt owed to Danish lenders. As shown in Appendix Figure A5, the incidence of such arrears increases by 0.5 percentage point by the end of the second calendar year following the job loss. The baseline incidence in the population is around 5 percent, implying that the estimated effect amounts to an increase in arrears of about 10 percent.

Figure 4: Mortgage loan responses (mortgagors only)

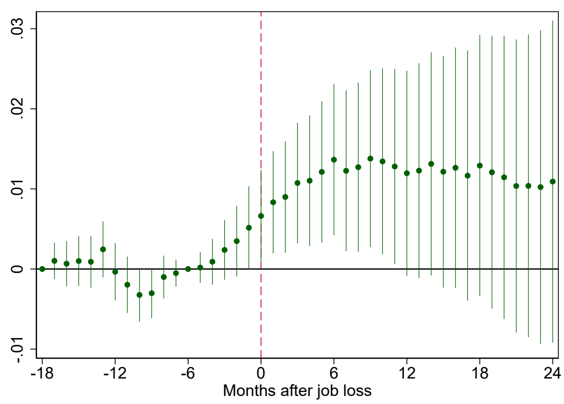
(a) Equity extraction



(b) Interest-only loan usage



(c) Adjustable-rate loan usage



The figure shows estimation results from the event study model (1) of the effects of job loss on mortgage loan outcomes. Panel a shows results from a regression where the dependent variable is a dummy for equity extraction. The dummy takes the value 1 if the household replaced an existing mortgage loan with a new one with principal exceeding $(B+20{,}000)/0.95$, where B is the outstanding balance on the existing loan in DKK. This criterion takes into account that refinancing involves a fixed fee plus a rate loss that is proportional to the principal (typically less than 5%). Panels b and c show results from regressions where the dependent variables are dummies for whether the household has at least one interest-only loan and adjustable-rate loan, respectively. We only include individuals from households with at least one mortgage loan in the estimations. Vertical lines represent 95% confidence intervals. Standard errors are estimated allowing for clustering at the level of the individual.

Saving in liquid assets (gray bars in Figure 2) is the most important self-insurance response margin. It accounts for 49% of the cumulated direct income loss, which is a significantly larger share than for any other response, economically as well as statistically.[17] The accumulation of liquid assets spikes upward just before the job loss, mirroring the increase in income from severance pay, and then drops significantly at the onset of unemployment. The effect continues to be large throughout the period. Note that this does not necessarily reflect that households continue to decumulate assets. In fact, net saving is close to zero for the average household from month five onward (Appendix Figure A6). This should be compared to a counterfactual of *positive* net saving − that is, accumulation of liquid assets − in the absence of job loss.

These results are robust to potentially important variations of our sample selection criteria. First, as shown in Appendix Table A3, we can relax the sample restrictions that exclude unstable households and those involved in real estate transactions, with no material impact on our main findings. Second, the aim of our analysis is to examine the response to involuntary lay-offs. The administrative records do not contain explicit information on whether job separations are the result of voluntary resignations or lay-offs. As explained in section 3, we impose additional restrictions to minimize the presence of the former, but the possibility that such cases occur in our sample remains. To address this concern we focus on individuals who lose their jobs concurrently with mass layoffs at their employer and re-run our analysis on this subsample. We identify mass layoffs by exploiting that firms must report directly to the Danish Ministry of Employment when they plan to lay off workers on a large scale. The results, which are illustrated in Appendix Figure A7 and summarized in columns (7)-(8) of Appendix Table A3, align with our main findings but standard errors are considerably larger due to the much smaller sample size.

In summary, we find that household self-insurance makes up for 70% of the net income loss following job loss. Our analysis investigates the relative importance of the several responses that are behind this result. These can broadly be grouped into two classes of behavior: One class involves shifting consumption across time by adjusting saving and

[17] The null that this share is numerically equal to the corresponding share for private transfers and other inflows (the second-largest response) has a p-value of 0.016 against a two-sided alternative.

debt accumulation, and the other class involves increases in the inflow of resources from other sources to compensate for the loss of income. Self-insurance mainly comes from shifting spending across time, and primarily by adjusting saving in liquid assets, whereas there is little compensation from other sources of inflows.

5 The importance of liquid assets

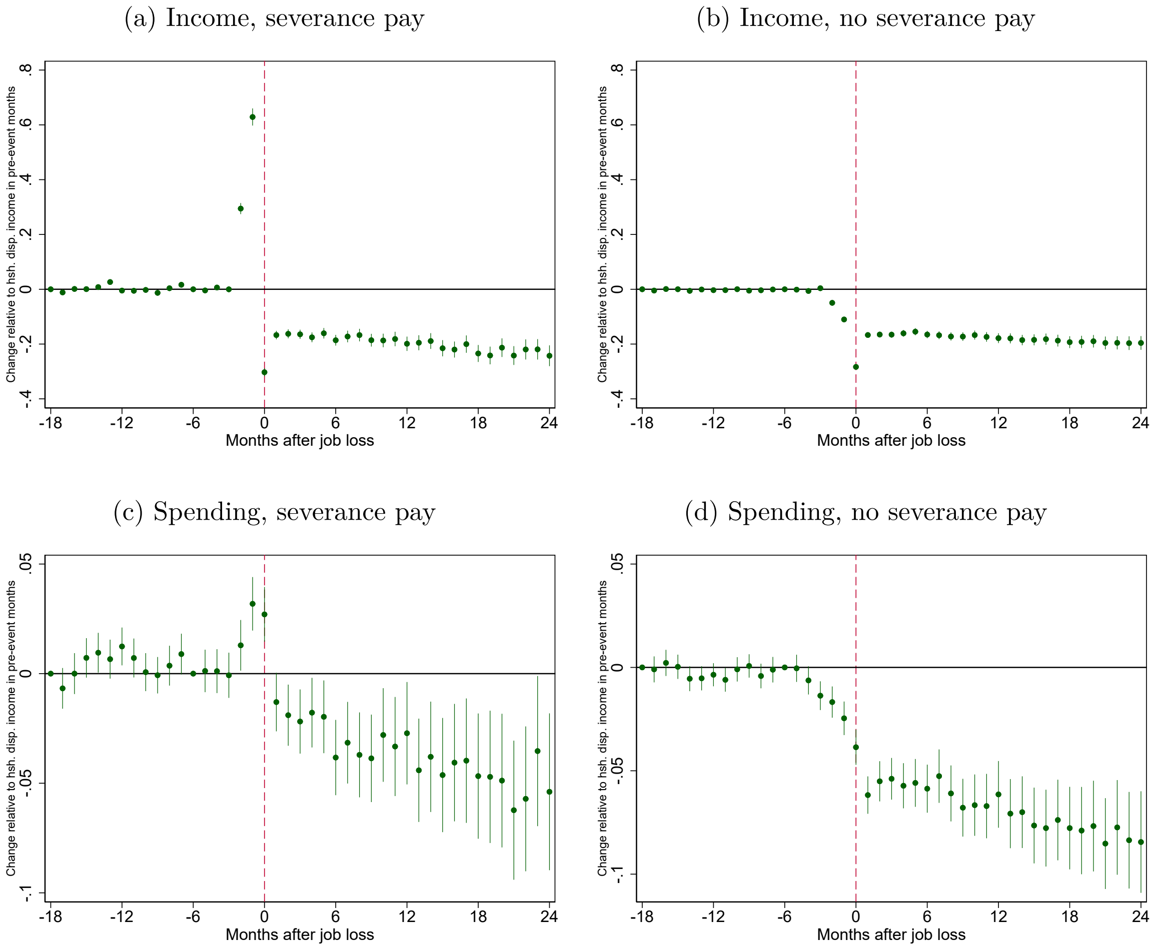
Our findings so far have pointed to adjustments in saving in liquid assets as the main important channel of self-insurance. In this section we dig further into the role of liquidity in shaping responses to job loss. We start out by investigating the effect of severance payments, which for some households provide a considerable boost to liquidity exactly at the time of job loss. This is already evident from Figure 2 where we see a clear spike in income and saving just before the month of separation. To examine how this shapes the subsequent responses, we define individuals as receiving sizable severance payments if their income in either of the two months preceding the job separation is at least 50 percent above their average income in the pre-event months. We then do a split-sample analysis based on this definition. Unlike in our main analysis (but similar to, e.g., Gerard and Naritomi 2019), we now only keep individuals in the sample for as long as they remain unemployed after having experienced job loss. This eliminates any mechanical effects from differences in the average duration of unemployment across individuals with vs. without severance payments.

The results of this analysis are displayed in Figure 5. Panels (a) and (b) show how income develops around job loss for individuals who receive and do not receive severance payments. Apart from the highly visible effect of severance payments just before the time of separation, the income shock is similar for the two groups. Panel (c) and (d) show the corresponding developments for household spending. Interestingly, spending *increases* just before the month of job separation for the severance pay group but not for the group who do not receive such payments. The two groups reduce spending by similar amounts just after separation but the level of spending remains considerably closer to the pre-event level in the severance payment group during the subsequent six months. After that, the difference between the two groups narrows.

These findings suggest that the liquidity boost that severance payments provide serves as a buffer to mitigate the consequences of job loss for spending, at least for a while. This resonates well with recent evidence by Gerard and Naritomi (2019) who find that displaced workers in Brazil increase spending at the time of layoff, despite experiencing a drop in the longer term. This is because most displaced workers in their sample receive a large lump-sum payment, in the form of government-mandated severance pay, when they are laid off.

Figure 5: Income and spending, severance pay vs. no severance pay

(a) Income, severance pay; (b) Income, no severance pay; (c) Spending, severance pay; (d) Spending, no severance pay

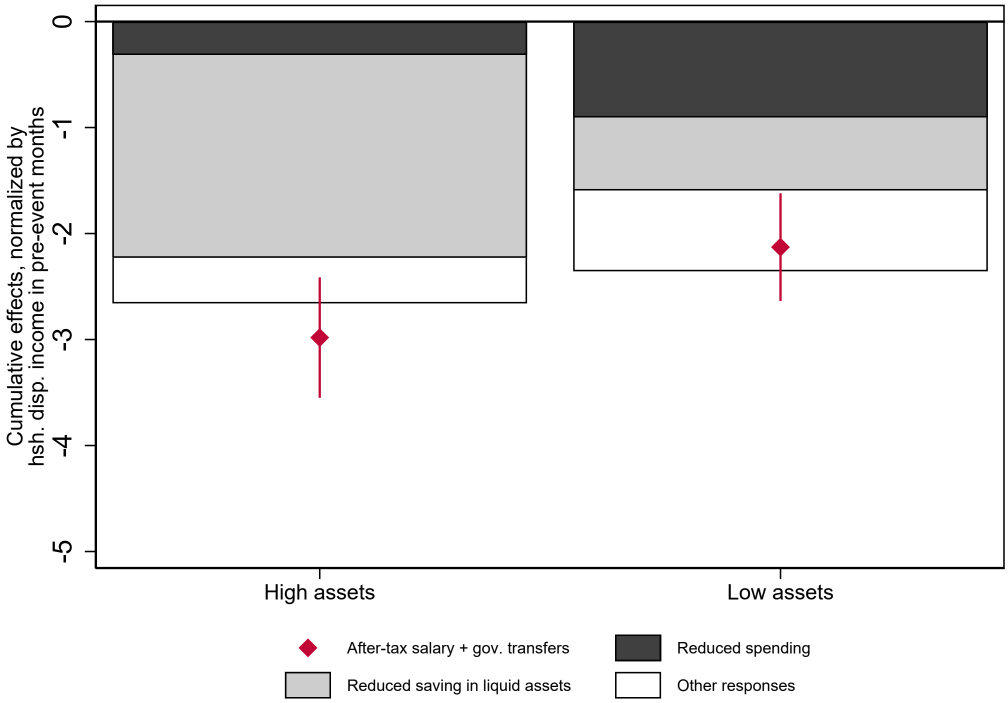


The figure shows estimation results from the event study model (1) of the effects of job loss on income (salary and government transfers) and spending. All outcomes are measured relative to the household's average disposable income between event months -18 and -3 and winsorized at the 2.5 and 97.5 percentiles within each event month. The sample is a dynamic sample of individuals that stay unemployed. In event months -18 to 0, this includes everyone in the baseline sample. For event month $t > 0$, it includes those who have not returned to employment at any point between month 0 and month t. Employment status is defined as having gross wage income above 10,000 DKK (at January 2010 price level). Income is the sum of wage income for the person experiencing job loss and government transfers for the household. Spending is measured at the household level. Panels a and c show results for the subsample of individuals whose income in month -1 or -2 is at least 50% above the average in the pre-event months. Panels b and d show corresponding results for the subsample who do not satisfy this criterion. Vertical lines represent 95% confidence intervals. Standard errors are estimated allowing for clustering at the level of the individual.

Next, we study heterogeneity in responses across households with different ex ante levels of liquidity. Specifically, we follow Zeldes (1989) and Leth-Petersen (2010) and split the sample by whether the household had liquid assets corresponding to two months' worth of disposable income 25 months ago.[18]

[18]Splitting the sample by the level of liquid assets in a particular *event* month would produce mechanical effects due to mean reversion. By using the level of liquid assets lagged 25 months, we avoid this

Figure 6: Responses by Liquid Asset Holdings



The figure shows cumulated effects of job loss for two subsamples defined by the ex ante level of liquid assets. “High assets” vs “Low assets” are defined by whether or not the household held liquid assets worth at least two months of disposable income 25 months earlier. The cumulated effects are computed by estimating model (1) on each subsample and summing the $\beta_h$coefficients for event months -5 to 24. All outcomes are measured relative to the household’s average disposable income between event months -18 and -3 and winsorized at the 2.5 and 97.5 percentiles within each event month. The red diamonds show the estimated direct income losses, i.e. the cumulative effect on after-tax income from salary and government transfers. Vertical red lines represent the 95% confidence intervals of these estimates. Estimates for responses to compensate for the direct income loss are illustrated with bars. We estimate cumulative effects for each outcome in separate regressions and illustrate the combined effects by the height of the stacked bars. All components in the stacked bars are signed so that a negative value indicates a response that contributes to compensating for the loss of income. The white bars labeled “Other responses” represent the combined effect of lower non-mortgage loan net repayments, lower mortgage loan payments, and higher spousal wage income. Lagging the value of liquid assets by 25 months means that we lose observations before February 2011. The “High assets” subsample contains 44% of the remaining observations, while the “Low assets” subsample contains 56%.

Figure 6 summarizes the results of the split-sample estimations by showing cumulative effects over the full analysis window for key response categories.[19] The figure shows the split by liquid assets to disposable income. Job loss has a significant negative impact on the household budget in both subsamples, equivalent to a loss of about 2-3 months of household disposable income over our analysis horizon. Among households with high levels of liquid assets, lower net saving in such assets is by far the most important response to this loss, while spending does not drop much. In contrast, households with low levels of liquid assets cannot reduce their saving to the same extent and hence cut more back on spending.

These findings support the main conclusion that saving in liquid assets plays a key role in shaping household responses to job loss. Households who enter the job-loss event with plenty of liquidity use it to shift spending across time, mitigating the impact on current consumption. Liquidity constrained households do not have the same option and cut back more strongly on spending.

6 Completeness and Representativeness

Our analysis relies on transaction data from a single commercial bank. This raises potential concerns about whether the data are complete, in the sense that they cover all the transactions the household is involved in, and whether they are representative, i.e., whether the customers of the bank are similar to the population at large. Because we are able to merge transaction data with data from administrative registers covering the entire population and its characteristics, as well as information about all bank connections obtained from the Danish Tax Agency, we are uniquely positioned to address these concerns.

Table 2 explores how our key estimates change as we alter sample selection criteria

problem while making sure that the split is always based on an *ex ante* value. Essentially, identification comes from comparing outcomes within a given calendar month across individuals who all had either high or low liquid assets 25 months ago but lost their job at different points in time since then (while also controlling for individual fixed effects).

[19] Appendix Figure A8 shows the dynamic results for each subsample, paralleling those shown in Figure 2 for the full sample.

and estimation methods with that specific purpose in mind. All columns report estimates of cumulative effects from month -5 to 24 relative to the month of job loss. Odd-numbered columns show these effects measured in multiples of monthly pre-event household disposable income, while even-numbered columns express them relative to the cumulated direct income loss. Columns (1)-(2) provide the estimates for our baseline sample. Columns (3)-(4) show the corresponding estimates for the subsample of households that are exclusive customers at the bank. Since these households do not bank elsewhere, results for this subsample should be free of any problems related to lack of completeness in the transaction data. Finally, to address concerns about representativeness we reproduce the baseline specification while re-weighting observations to make our sample of active bank customers match the demographic characteristics of the gross sample drawn from the full population. Weights are constructed as the inverse predicted probabilities from a probit regression where the dependent variable is a dummy for belonging to the sample of active customers and the regressors are the demographic characteristics reported in Table 1, i.e., dummy variables for age (five-year intervals), sex, couple, capital region residence, higher education, sector of employment before lay-off (seven categories), and homeownership. The results of this re-weighting exercise are reported in columns (5)-(6).

Table 2: Cumulative effects: Representativeness and completeness

	Baseline		Exclusive customers		Weighted regressions	
	(1)	(2)	(3)	(4)	(5)	(6)
	------------ Cumulative effects, months -5 to 24 -----------					
	Rel. to disp. inc. before job loss	Percent of direct income loss	Rel. to disp. inc. before job loss	Percent of direct income loss	Rel. to disp. inc. before job loss	Percent of direct income loss
Direct income effects						
[1] Salary, affected person	-6.93		-7.94		-6.47	
	(0.14)		(0.21)		(0.15)	
[2] Gov. transfers, household	4.56		5.15		4.32	
	(0.08)		(0.11)		(0.08)	
[3] Direct income loss (-[1] - [2])	**2.37**	**100.0%**	**2.79**	**100.0%**	**2.15**	**100.0%**
	(0.14)	**(0.0%)**	**(0.20)**	**(0.0%)**	**(0.15)**	**(0.0%)**
Response margins						
[4] Salary, spouse	0.15	6.2%	0.22	7.9%	0.16	7.5%
	(0.07)	(2.9%)	(0.09)	(3.5%)	(0.08)	(4.0%)
[5] Private transfers and other inflows	0.29	12.1%	0.32	11.5%	0.25	11.4%
	(0.15)	(6.4%)	(0.21)	(7.9%)	(0.15)	(7.2%)
[6] Spending	-0.72	-30.3%	-1.10	-39.6%	-0.57	-26.4%
	(0.15)	(6.8%)	(0.22)	(8.8%)	(0.16)	(7.8%)
[7] Net saving in liquid assets	-1.16	-49.2%	-1.07	-38.3%	-1.12	-52.0%
	(0.30)	(12.4%)	(0.44)	(15.8%)	(0.31)	(14.4%)
[8] Non-mortgage loan net repaym.	-0.05	-2.0%	0.06	2.0%	-0.04	-1.8%
	(0.10)	(4.5%)	(0.14)	(5.2%)	(0.11)	(5.4%)
[9] Mortgage loan repayments	-0.06	-2.7%	-0.05	-1.8%	-0.06	-2.9%
	(0.01)	(0.6%)	(0.02)	(0.7%)	(0.01)	(0.7%)
[10] Total ([4] + [5] - [6] - [7] - [8] - [9])	**2.42**	**102.4%**	**2.71**	**97.0%**	**2.20**	**102.0%**
	(0.27)	**(10.6%)**	**(0.39)**	**(12.9%)**	**(0.29)**	**(12.2%)**
Number of individuals	10,002	10,002	5,224	5,224	10,002	10,002

Notes: The table reports results from analyses aimed at assessing whether lack of representativeness or completeness in our data affect the results. Columns (1)-(2) show results for our baseline sample and estimation method; columns (3)-(4) for the subsample of exclusive customers who do not hold accounts at any other Danish bank; and columns (5)-(6) for regressions where observations are re-weighted so that our sample of active customers matches the demographic characteristics of the gross sample shown in column (1) of Table 1. All estimates are based on regressions where the reported outcomes are measured relative to the household's average disposable income in the pre-event months. Odd-numbered columns report the sum of coefficients for event months -5 to 24 from such regressions. Even-numbered columns report the ratios between these sums and the corresponding sum for the direct income loss shown in row [3]. Standard errors (in parentheses) are estimated by bootstrapping with 500 replications. The bootstrapping procedure is carried out with resampling of individuals, rather than individual observations, to account for heteroskedasticity and autocorrelation within observations for the same individual. Graphical illustrations of the results underlying columns (3)-(4) and (5)-(6), paralleling those shown in Figure 2, are shown in Appendix Figures A9 and A10

Across all columns, the estimates of the cumulated direct income loss (including the effect on government income transfers) are in the range of 2-3 months of household disposable income. The combined effect of the behavioral responses that we consider is also stable across columns and always close to the estimated direct income loss. There is some variation when it comes to the relative importance of each response but the overall conclusions are robust: Household spending drops by about 25-40% of the direct income loss, suggesting substantial self-insurance. The most important self-insurance response for all samples is reduced saving in liquid assets, which accounts for about 40-50% of the direct income loss. The compensating effects from spousal labor supply responses, borrowing, and loan repayments are small and/or insignificant. Finally, the estimates for private transfers and other inflows suggest that increases in such inflows compensate for 11-12% of the direct income loss across samples.

7 Concluding Remarks

This paper provides a comprehensive assessment of the relative importance of different types of self-insurance responses to job-loss using transaction data from a major Danish bank merged with population data with information about household composition, bank connections and more. We first document that even in Denmark, where unemployment benefits are relatively generous, the reduction in disposable income following job-loss is significant. Next, we show that about 30 percent of the income loss is accomodated by a reduction in spending, while the remaining difference is self-insured.

The self-insurance responses to unemployment shocks studied in this paper fall into two classes: First, reduced *saving in liquid assets* and increased *borrowing* are pure consumption-smoothing responses that allow consumption to be moved forward in time without changing overall consumption possibilities. Second, and in contrast, increases in *spousal labor supply* and *private transfers* from family or friends mitigate the impact of the shock by expanding the household's overall consumption possibilities. Our analysis shows that the first class of responses, in particular savings in liquid assets, is quantitatively far more important than the second type.

These findings have implications for the theoretical modelling of household responses to job loss. We show that reduced saving in liquid assets is by far the most significant channel of adjustment. This suggests that simple consumption-savings models incorporating a liquid asset can go far in capturing the most important aspects of household's responses to job loss.

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Appendix

A Variable Definitions

Income variables

Disposable income: Sum of monthly incoming transactions to bank accounts owned by the affected person and/or the spouse. The following transaction types are included: Direct deposits, person-to-person transfers from outside the household, and cash deposits. We categorize these inflows into the following subcategories (see Appendix C for details):

Salary income, affected person: Sum of monthly salary payment inflows to bank accounts owned by the person affected by job loss. Salary payments to joint accounts are attributed to the spouse if they come from the spouse's employer, and otherwise to the affected person.

Salary income, spouse: Sum of monthly salary payment inflows to bank accounts owned by the spouse. Salary payments to joint accounts are attributed to the spouse if they come from the spouse's employer, and otherwise to the affected person.

Government income transfers: Sum of monthly government income transfer inflows to bank accounts owned by the affected person and/or the spouse.

Private transfers and other inflows: The residual of disposable income minus salary (affected person and spouse) and government income transfers.

Spending

Total spending: The sum of outgoing transactions from the household's bank accounts using either of the payment methods card, mobile phone, and bill, plus cash withdrawals. Outflows categorized as tax or debt payments are excluded. We aggregate to the household level by summing spending for the person affected by job loss and the spouse, if any. Outflows from joint accounts are split evenly between the two account owners before summing to avoid double-counting. See Appendix B for further details.

Utilities: The value of the subset of transactions in total spending measure with MCC "4900", "4812", "4814", "4821", "4899", or bill payment label "utilities", "elec", "gas", "water", "heating", "internet", "cable TV", "telephone", or "TV license"

Restaurant and bar spending: The value of the subset of transactions in total spending measure with MCC "5813", 5462", "5811", "5812", or "5814".

Groceries: The value of the subset of transactions in total spending measure with MCC "5411", "5422", "5441", "5499", or "5921", or bill payment label "groceries".

Net saving and debt repayments

Net saving in liquid assets: The sum of (1) the change in end-of-month balances on deposit accounts at the bank owned by the affected person or the spouse, and (2) outflows minus inflows to all accounts from transactions with type code "securities trade".

Net repayments on non-mortgage loans: The change in end-of-month balances on loan accounts (with amounts owed coded as negative balances) owned by the affected person or the spouse.

Mortgage loan repayments: Average monthly mortgage payments with current mortgage loans. Calculated by determining the average monthly payment over a full calendar year for each loan, then summing across all mortgage loans that the household had at the end of the current month. See Appendix D for further details.

B Measuring Household Spending from Transaction Data

The basis for our measure of spending is raw transaction data from the bank's records. Each record holds information about the time and type of transaction and the amount transacted. For card transactions and bill payments, information about the type of recipient is provided in the form of Merchant Category Codes (MCCs). Before making it available to us, the bank aggregates the raw transaction to daily totals within each combination of customer account, transaction type and recipient category.

We include three types of outgoing transactions in our spending measure: Card payments (including payments initiated via mobile phone applications), bill payments and cash withdrawals. For the average person in our gross sample of active bank customers (defined as individuals who have at least five spending transactions in each month of the calendar year), these transaction types account for 80% of all transactions leaving the household in a given month. The remaining outflows include transactions that do not reflect consumption, for example fee payments to the bank and financial security purchases, and uncategorized bank transfers where the purpose is unobservable.

In a next step, we use recipient MCCs to exclude tax and debt repayments, which are not considered as spending. We then sum all the remaining outgoing transactions to construct a monthly spending measure for each individual person. For couples, household-level variables are constructed by aggregating spending for the two spouses. We split outgoing transactions from joint accounts evenly between the account owners to avoid double-counting.

Figure A1 shows the development in average quarterly spending per household for the gross sample of active bank customers, broken down by transaction type. The share of spending done by card or mobile phone transactions rises over the analysis period, from 43% in 2009Q1 to 57% in 2016Q4. Conversely, the share of cash spending gradually falls from 16% to 9% over the same years, while bills account for about 35-40% of total spending throughout the period. Cash payments account for 14% of total spending via cash or cards in 2016. In comparison, a 2017 household survey by the Danish central bank found a value-weighted cash payment share of 16% of total cash and card payments (Danmarks Nationalbank 2017).

C Categorizing Inflows to Bank Accounts

We measure household disposable income as the sum of direct deposits, person-to-person transfers and cash deposits flowing into the household's bank accounts, excluding transactions between the household's own accounts. We break this measure down into salary payments for the person affected by job loss, salary payments for the spouse (if any), government income transfers, and other. There are two main steps in this process, which we describe in detail below: first, we construct a mapping from employer IDs to the IDs of the bank branches that they use to pay out salary, and similarly for the government agencies that pay out income transfers. Second, we look at each individual's incoming transactions and use this mapping to identify payments coming from employers and government agencies. For example, if person A works for company B, which uses bank branch C for its salary payments, then we interpret all payments from bank branch C going into person A's account as salary payments from company B.

We start by linking employers and government agencies to the registration number of the bank account(s) that they use to pay out salary and income transfers, respectively. A registration number is a four-digit Danish national bank code. Each number is associated with a unique bank *branch*, but branches may have more than one registration number (e.g., one for business customers' accounts and one for personal customers' accounts). There are more than 3,000 unique numbers across all banks. Some large customers have their own unique number. For example, all payments from the central government come from accounts with the same unique registration number, which is used solely for this purpose.

We link each employer and government agency to a registration number in the following way: First, for each employer/agency j and each month t, we use the payroll data from the Danish Tax Agency to identify all individuals in our sample of bank customers who appear on the employer's/agency's payroll. Second, for each bank registration number, we use the transaction data to compute the share of individuals in that group who received a payment from an account with that registration number in month t. We record the registration number with the highest share as the one associated with payments from employer/agency j in month t.[20]

In the transaction data, we code an incoming direct deposit as a salary payment if the sender's bank registration number is associated with an employer that the recipient works for according to the payroll data. That is, if person A appears on the payroll of employer B in month t and receives a payment from an account with a registration number that has been linked to employer B through the mapping procedure described above, then we conclude that this is a salary payment from employer B to person A. In addition, we also interpret transactions with certain type codes (e.g. "salary transfers") as salary payments.

We code an incoming transaction as a government income transfer if either of the following conditions is satisfied: i) The sender's bank registration number is linked to a government agency that the person received money from in that month according to the payroll data; ii) The bank registration number is the one used by the central government, and the person does not work for the central government (in which case we code it as a salary payment).

D Constructing Monthly Mortgage Data from Annual Snapshots

The mortgage data set provides an end-of-year snapshot of all active mortgage loans to private individuals in Denmark in each year from 2009 to 2015. We use this to construct monthly measures of the type of mortgage loans in the household's portfolio and the average monthly payment on each of these loans.

[20]If the central government registration number is the top rank and j is not a central government agency, we use the second-ranked registration number

We start by mapping the household's portfolio of active loans in each month during the year by comparing the end-of-year snapshot with that of the previous year. Each loan has a unique ID that allows us to track it over time. If a loan appears in both snapshots, we conclude that it must have been part of the household's portfolio all year. If it appears only in the most recent snapshot, we use information about the date of origination to infer when it entered the portfolio. For loans that disappear from the household's portfolio during the year (e.g. because of refinancing), we assume that the date of termination coincides with the origination date for the household's new loan. Cases where a loan disappears from the portfolio without being replaced by a new one are rare but do occur in our sample. In such cases, we use data on total interest payments on mortgage loans from annual tax returns to infer when the loan was terminated.[21]

Once we have the full mapping of the household's loan portfolio in each month, we use the detailed information about each loan to characterize this portfolio. First, we use information about the loan type to infer whether the household holds any adjustable-rate loans or interest-only loans.[22] Second, we combine information about the loan's current remaining balance, time to maturity, interest rate and amortization profile (interest-only vs. amortizing) to impute the average monthly payment over the full calendar year.

E Mass Layoffs

We obtain information about mass layoff events from the Ministry of Employment. All firms with more than 20 employees are obliged to report to the Ministry if they plan to lay off workers on a large scale. The exact definition of "large scale" depends on firm size, ranging from 10 workers for firms with 20-100 employees to 30 workers for firms

[21] More precisely, we use the information about the loan's interest rate and remaining balance to calculate how much interest would have been paid on the loan over the full year. We then compare that number to information from the tax return data on how much interest on mortgage loans the household actually paid. If the former number is twice as large as the latter, we conclude that the loan was terminated after the first six months of the year.

[22] A loan can change amortization profile during the year, i.e. from amortizing to interest-only, or vice versa. The end-of-year snapshots provide information about the date of the most recent such change, allowing us to infer the loan's profile in any given month during the year. A loan cannot change from fixed to adjustable rate, or vice versa. Households need to prepay their existing loan and take out a new one if they wish to switch between these loan types.

with more than 300 employees. The data contains information about the extent of the planned layoffs, the date of reporting, and firm IDs, which we use to link it to the payroll data from the Tax Agency. From this data, we construct a subsample of individuals who have been laid off shortly after their employer reported a planned mass layoff. The report must be submitted before workers are given notice of their impending layoff. Since we do not observe when this happens for the individual worker, we include all cases where the date of reporting is within months -7 to -1 relative to the observed month of layoff. This leaves us with a subsample of 1,156 individuals.

Figure A7 shows event graphs for income and spending for the mass layoff subsample vs. the full sample of active customers. The estimated development in salary income for the person affected by job loss – as well as the ensuing increase in government transfers – are nearly identical for two samples, suggesting that there is no significant difference in the size and persistence of the shock. Spending responses also look highly similar across the two samples, although the small number of observations in the mass layoff sample implies that confidence intervals are substantially wider. Combined, these results suggest that the total amount of self-insurance is about the same in the mass layoff subsample as in the full sample.

Columns (7) and (8) of Table A3 shows results for cumulated effects over the full observation window for the mass layoffs subsample. The estimated cumulative direct income loss is almost the same as in the main analysis. This suggests that the presence of voluntary resignations (e.g., individuals who deliberately take time off between jobs) in our baseline sample is no cause for concern, since the cumulative income loss would most likely be smaller in such cases. There are some differences when it comes to the relative importance of the behavioral responses to this income loss, especially for private transfers and other inflows where we find a negative but insignificant effect in the mass layoff sample. In general, the cumulative responses are imprecisely estimated in this sample due to the limited number of observations. But the point estimates suggest that our main findings are robust: First, household spending drops by 30-45% of the direct income loss, suggesting substantial self-insurance. Second, the compensating effects from spousal labor supply, borrowing and loan repayments are small and/or insignificant. Third, the most important self-insurance response is reduced saving in liquid assets, which accounts for 50-65% of the direct income loss.

F Tables and Figures

Table A1: Sample selection and summary statistics, extended

	(1) Gross sample	(2) Active customers (baseline sample)	(3) Exclusive customers
No. of individuals	66,844	10,002	5,224
	------------------------ Sample means	-------------------	
Female	0.43	0.47	0.48
Age	46.2	46.6	46.1
Couple	0.67	0.59	0.52
Capital region	0.33	0.44	0.42
Higher eduaction	0.23	0.28	0.27
Primary sector	0.01	0.01	0.01
Manufacturing	0.19	0.15	0.15
Construction	0.07	0.06	0.06
Trade & transport	0.26	0.26	0.26
Other services	0.20	0.22	0.20
Public Sector	0.23	0.28	0.28
Arts & entertainment	0.03	0.04	0.03
Homeowner	0.65	0.63	0.59
Spouse employed	0.79	0.84	0.85
Annual gross income for person who lost job (tax data)	371,621	394,499	375,019
Household bank deposits, all banks (tax data)	161,509	165,372	131,597
Household financial securities, all banks (tax data)	66,034	65,491	51,474
Household liquid assets, all banks (tax data)	227,543	230,863	183,071
Household loan balances, all banks (tax data)	227,732	225,325	177,228
Share of hsh. bank deposits held at other banks (tax data)	0.71	0.05	0.00
Share of hsh. retail bank loans held at other banks (tax data)	0.71	0.11	0.00
Household deposit balances at Danske Bank (bank data)	41,327	137,630	131,801
Household liquid assets at Danske Bank (bank data)	62,717	201,778	192,459
Household loan balances at Danske Bank (bank data)	49,590	174,754	165,132
Household inflows to Danske Bank accounts (bank data)	11,974	40,033	36,339
- salary, affected person	5,527	19,450	18,827
- salary, spouse	2,836	9,885	8,510
- government income transfers	859	2,630	2,431
- private transfers and other inflows	2,752	8,068	6,571
Household spending from Danske Bank accounts (bank data)	7,401	25,920	24,448
Household mortgage payments, all banks (mortgage data)	2,779	3,115	2,781
Household mortgage debt, all banks (mortgage data)	685,420	747,244	654,545

The table is an extended version of Table 1 in the main text. Column (1) shows statistics for the gross sample with no requirements on customer status at Danske Bank. Column (2) shows statistics for the baseline sample of active customers, i.e., individuals who have at least five outgoing spending transactions in each month of the event observation window and whose partner (if any) satisfies the same criterion. Column (3) is for the sample of exclusive customers, i.e., active customers who have no deposits or loans at other retail banks and whose partner (if any) satisfies the same criterion. All variables are measured in month -6 relative to the month of job loss, except the following: Annual gross income, measured over the calendar year in which month -6 occurs; shares of household loans and deposits held at other banks, household mortgage debt at all mortgage banks, all measured at the end of the calendar year before month -6.

Table A2: The dynamic effects of job loss on income, saving and spending

	(1) Month 0	(2) Month 6	(3) Month 12	(4) Month 24	(5) Cumulative, months -5 to 24	(6) Cumulative, months -5 to 24
Direct income effects	--------- Relative to monthly disposable income before job loss ---------					Percent of direct income loss
[1] Salary, affected person	-0.512	-0.305	-0.243	-0.210	-6.926	
	(0.004)	(0.005)	(0.006)	(0.008)	(0.142)	
[2] Government transfers, household	0.219	0.206	0.158	0.127	4.561	
	(0.003)	(0.003)	(0.003)	(0.005)	(0.078)	
[3] Direct income loss (-[1] - [2])	**0.293**	**0.098**	**0.086**	**0.083**	**2.365**	**100.0%**
	(0.004)	**(0.005)**	**(0.006)**	**(0.008)**	**(0.139)**	**(0.0%)**
Response margins						
[4] Salary, spouse	0.010	0.006	0.004	0.004	0.146	6.2%
	(0.002)	(0.002)	(0.003)	(0.004)	(0.069)	(2.9%)
[5] Private transfers and other inflows	0.036	0.014	0.010	0.009	0.286	12.1%
	(0.004)	(0.005)	(0.006)	(0.009)	(0.148)	(6.4%)
[6] Spending	-0.019	-0.030	-0.022	-0.028	-0.717	-30.3%
	(0.004)	(0.005)	(0.006)	(0.009)	(0.154)	(6.8%)
[7] Net saving in liquid assets	-0.195	-0.045	-0.055	-0.064	-1.163	-49.2%
	(0.008)	(0.009)	(0.012)	(0.017)	(0.298)	(12.4%)
[8] Non-mortgage loan net repayments	-0.032	-0.003	-0.001	-0.002	-0.048	-2.0%
	(0.003)	(0.003)	(0.004)	(0.006)	(0.104)	(4.5%)
[9] Mortgage loan repayments	-0.001	-0.002	-0.003	-0.003	-0.063	-2.7%
	(0.000)	(0.000)	(0.001)	(0.001)	(0.013)	(0.6%)
[10] Total ([4] + [5] - [6] - [7] - [8] - [9])	**0.292**	**0.100**	**0.094**	**0.110**	**2.423**	**102.4%**
	(0.007)	**(0.009)**	**(0.011)**	**(0.016)**	**(0.274)**	**(10.6%)**

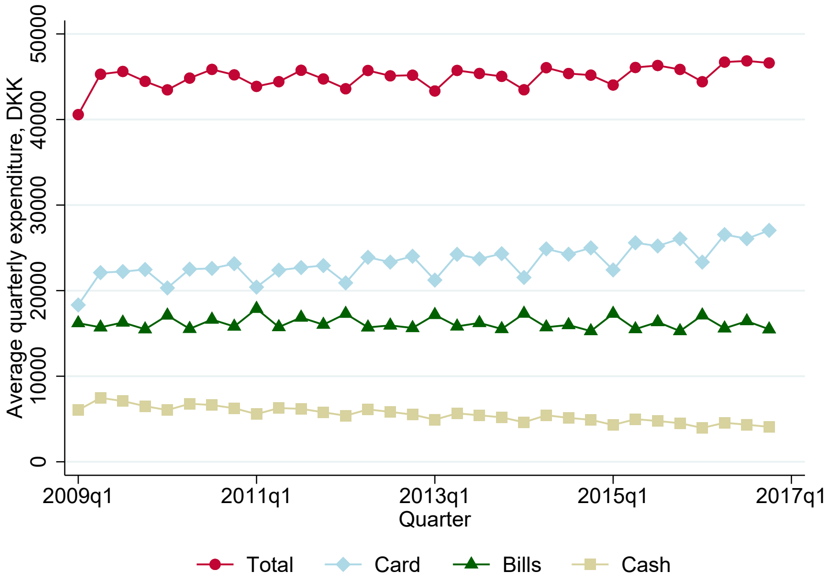
All estimates are based on regression estimates from estimation of equation (1). The reported outcomes are measured relative to the household's average income in the pre-event months. Rows in normal font show coefficient estimates from single regressions with the indicated outcomes. Rows in bold font show combination of coefficients from multiple regressions, as indicated in parenthesis. Columns (1) to (4) report coefficients on the indicator variables representing months 0, 6, 12 and 24 after the unemployment event, respectively. Column (5) reports the sum of coefficient values for event months -5 to 24. Column (6) reports the ratio between the sum shown in the same row, column (5) and the corresponding sum shown in row [3], column (5). Standard errors (in parentheses) are estimated by bootstrapping with 500 replications. The bootstrapping procedure is carried out with resampling of individuals, rather than individual observations, to account for the panel nature of the data set.

Table A3: Cumulative effects of job loss: Robustness

	Baseline		No restriction on house trades		No restriction on same partner		Mass layoffs subsample	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	------------------------------------ Cumulative effects, months -5 to 24 ------------------------------------							
	Rel. to disp. inc. before job loss	Percent of direct income loss	Rel. to disp. inc. before job loss	Percent of direct income loss	Rel. to disp. inc. before job loss	Percent of direct income loss	Rel. to disp. inc. before job loss	Percent of direct income loss
Direct income effects								
[1] Salary, affected person	-6.93		-6.66		-6.98		-6.87	
	(0.14)		(0.13)		(0.14)		(0.42)	
[2] Gov. transfers, household	4.56		4.35		4.56		4.77	
	(0.08)		(0.07)		(0.07)		(0.24)	
[3] Direct income loss (-[1] - [2])	**2.37**	**100.0%**	**2.32**	**100.0%**	**2.43**	**100.0%**	**2.10**	**100.0%**
	(0.14)	**(0.0%)**	**(0.13)**	**(0.0%)**	**(0.13)**	**(0.0%)**	**(0.39)**	**(0.0%)**
Response margins								
[4] Salary, spouse	0.15	6.2%	0.11	4.8%	0.16	6.7%	0.06	3.0%
	(0.07)	(2.9%)	(0.07)	(2.9%)	(0.06)	(2.6%)	(0.20)	(11.4%)
[5] Private transfers and other inflows	0.29	12.1%	0.48	20.8%	0.45	18.6%	-0.34	-16.0%
	(0.15)	(6.4%)	(0.16)	(6.9%)	(0.15)	(6.3%)	(0.45)	(27.1%)
[6] Spending	-0.72	-30.3%	-0.71	-30.7%	-0.86	-35.3%	-0.92	-43.9%
	(0.15)	(6.8%)	(0.15)	(6.7%)	(0.15)	(6.2%)	(0.45)	(27.5%)
[7] Net saving in liquid assets	-1.16	-49.2%	-1.03	-44.5%	-1.12	-46.1%	-1.36	-64.9%
	(0.30)	(12.4%)	(0.31)	(13.0%)	(0.29)	(11.7%)	(0.93)	(53.2%)
[8] Non-mortgage loan net repaym.	-0.05	-2.0%	-0.02	-0.8%	-0.06	-2.4%	-0.03	-1.5%
	(0.10)	(4.5%)	(0.10)	(4.4%)	(0.10)	(4.1%)	(0.30)	(17.4%)
[9] Mortgage loan repayments	-0.06	-2.7%	-0.08	-3.4%	-0.06	-2.5%	-0.01	-0.7%
	(0.01)	(0.6%)	(0.02)	(0.7%)	(0.01)	(0.5%)	(0.04)	(2.2%)
[10] Total ([4] + [5] - [6] - [7] - [8] - [9])	**2.42**	**102.4%**	**2.44**	**105.1%**	**2.71**	**111.6%**	**2.06**	**98.1%**
	(0.27)	**(10.6%)**	**(0.28)**	**(11.0%)**	**(0.28)**	**(10.3%)**	**(0.86)**	**(47.1%)**
Number of individuals	10,002	10,002	11,096	11,096	11,798	11,798	1,156	1,156

The table reports summary measures of results from estimation of equation (1) for various variations on our sample selection criteria. Columns (1)-(2) show baseline results, reproduced from columns (5)-(6) of Table A2. Columns (3)-(4) report parallel results when we relax the sample restriction that household members must not be involved in a real estate trade during the observation window. Columns (5) to (6) show results when we relax the restriction that the person experiencing job loss must have the same or no partner during the entire observation window. Columns (7) and (8) report results for the subsample of individuals who lose their job concurrently with mass layoffs at their employer. All estimates are based on regressions where the reported outcomes are measured relative to the household's average disposable income in the pre-event months. Odd-numbered columns report the sum of coefficients for event months -5 to 24 from such regressions. Even-numbered columns report the ratios between these sums and the corresponding sum for the direct income loss shown in row [3]. Standard errors (in parentheses) are estimated by bootstrapping with 500 replications. The bootstrapping procedure is carried out with resampling of individuals, rather than individual observations, to account for heteroskedasticity and autocorrelation within observations for the same individual.

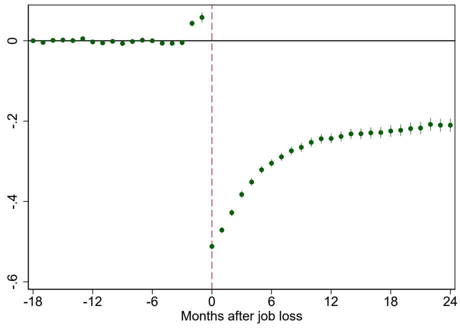
Figure A1: Average spending for active customers, by payment method and quarter



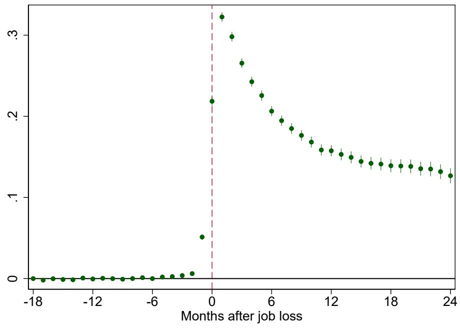
The figure shows the breakdown of the spending measure on categories of outflows for the complete sample of active customers. Card payments include payments via cellular phone.

Figure A2: Dynamic responses to job loss, individual outcomes

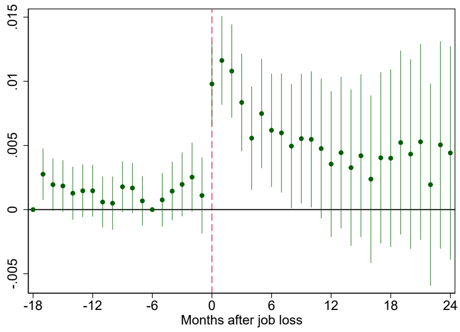
(a) Salary, affected person



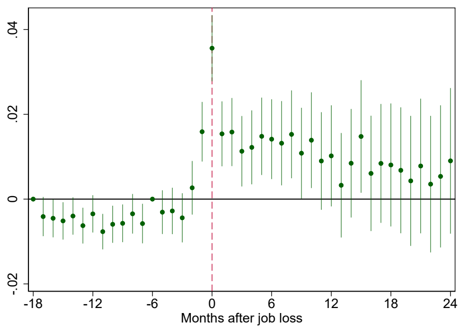
(b) Income transfers from government



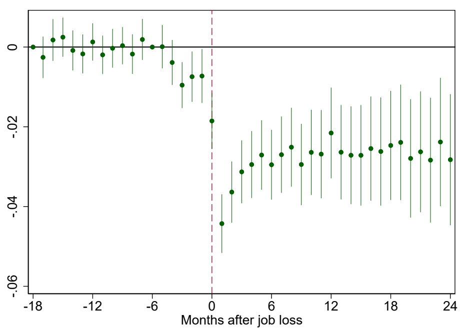
(c) Salary, spouse



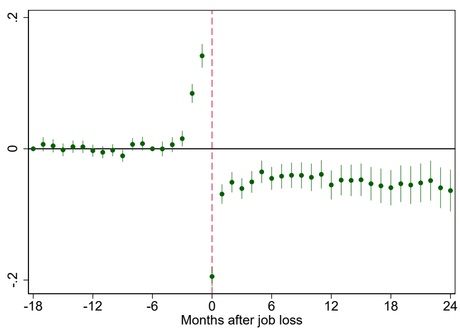
(d) Private transfers + other inflows



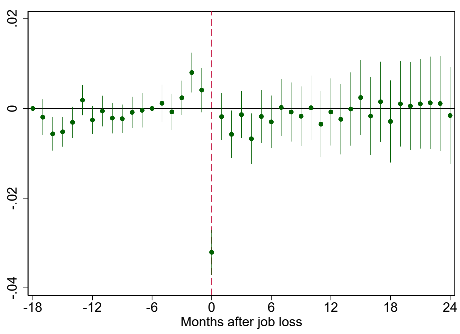
(e) Spending



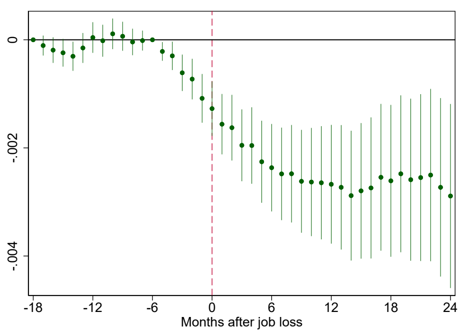
(f) Net saving in liquid assets



(g) Non-mortgage loan net repayments

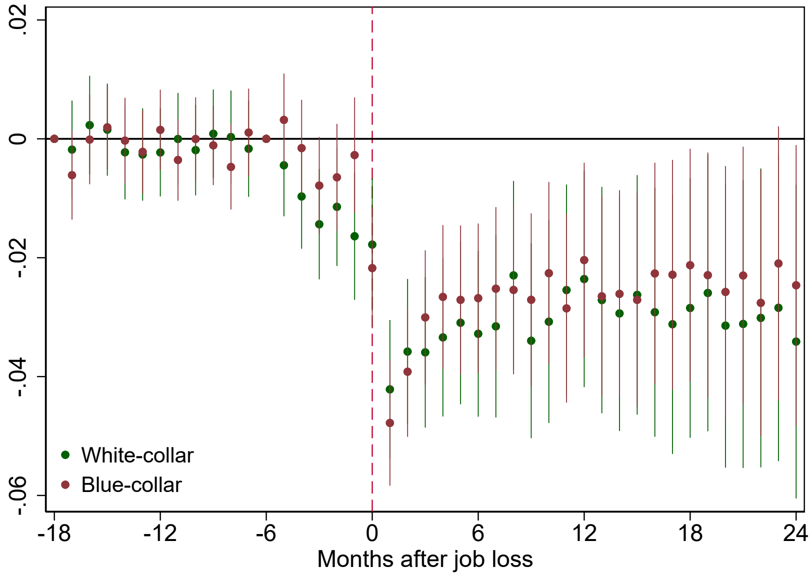


(h) Mortgage loan repayments



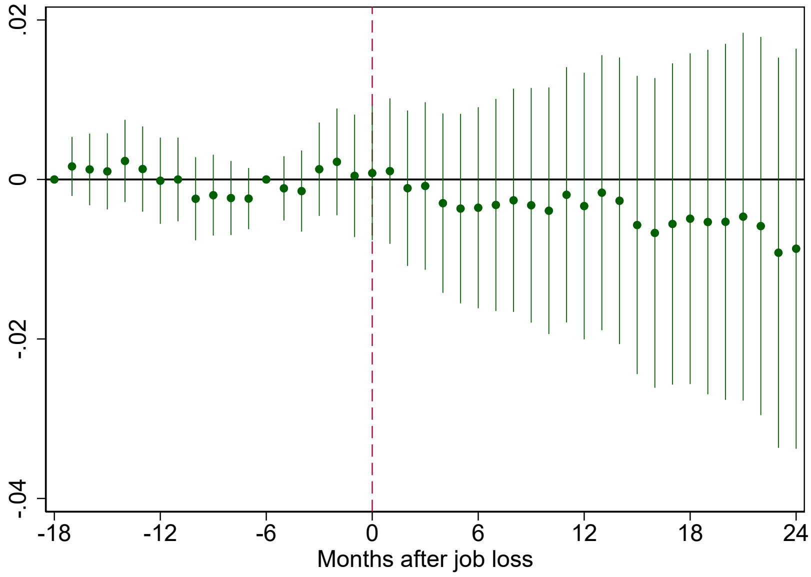
The figure shows estimation results from the event study model (1) of the effects of job loss on various outcomes. All outcomes are measured relative to the household's average disposable income between event months -18 and -3 and winsorized at the 2.5 and 97.5 percentiles within each event month. Disposable income is defined as all external inflows to the household's bank accounts. Vertical lines represent 95% confidence intervals. Standard errors are estimated allowing for clustering at the level of the individual.

Figure A3: Spending responses to job loss: Blue collar vs. white collar



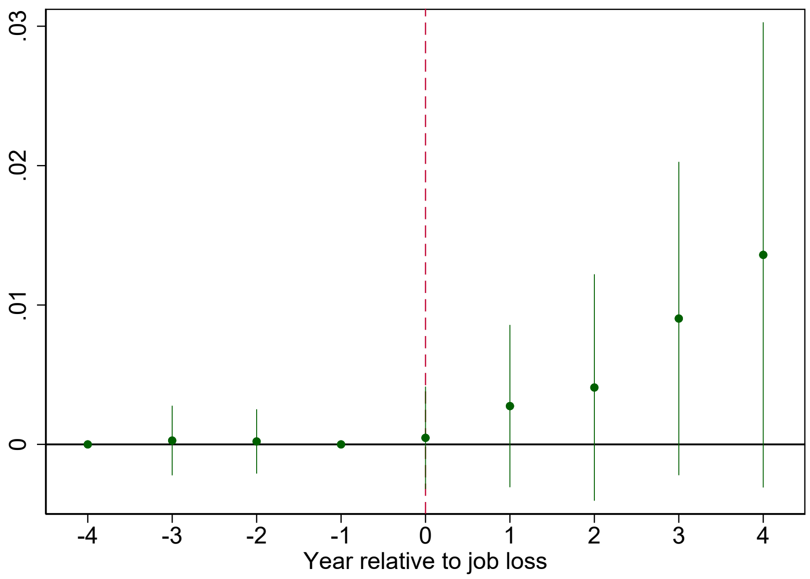
The figure shows estimation results from the event study model (1) with household spending (normalized by the household's average disposable income in the pre-event months) as the outcome for two subsamples: White-collar workers (green) are individuals who are covered by legislation guaranteeing a notice period of at least 3 months when laid off. Blue-collar workers (red) are not covered by such legislation and can have notice periods as short as one day. Data on blue- vs. white-collar status comes from the employment registry and are mainly based on information about employment contracts submitted by employers. The outcome variable is winsorized at the 2.5 and 97.5 percentiles within each event month. Vertical lines represent 95% confidence intervals. Standard errors are estimated allowing for clustering at the level of the individual.

Figure A4: Spouse employment (couples only)



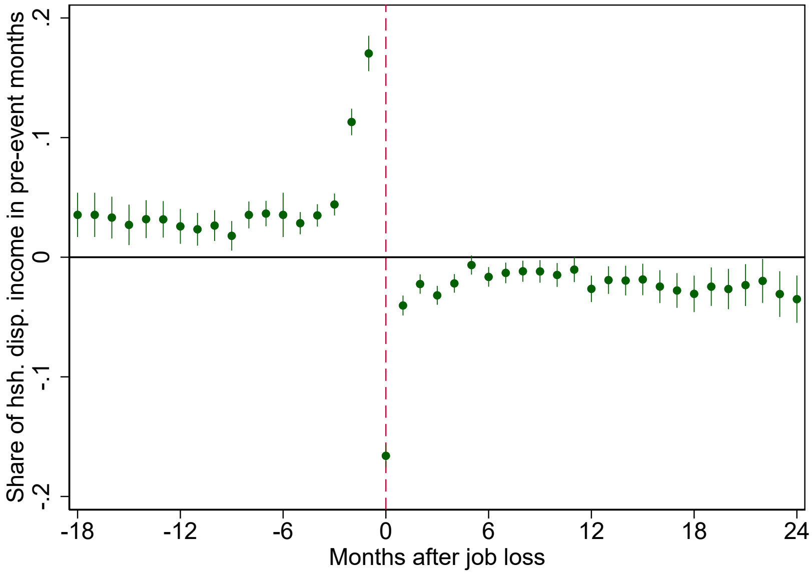
The figure shows estimation results from the event study model (1) of the effects of job loss on spouses' employment rates. The dependent variable is a dummy variable equal to 1 if the spouse of the person experiencing job loss appears on the payroll of at least one employer in the given month. Individuals with no spouse are excluded. Vertical lines represent 95% confidence intervals. Standard errors are estimated allowing for clustering at the level of the individual.

Figure A5: Loan arrears



The figure shows estimation results from an event study corresponding to model (1), but using annual data. The dependent variable is a dummy variable equal to 1 if the person experiencing job loss or his/her spouse is in arrears on any loan at the end of the year. Information on loan arrears comes from the tax data on bank relationships. Vertical lines represent 95% confidence intervals. Standard errors are estimated allowing for clustering at the level of the individual.

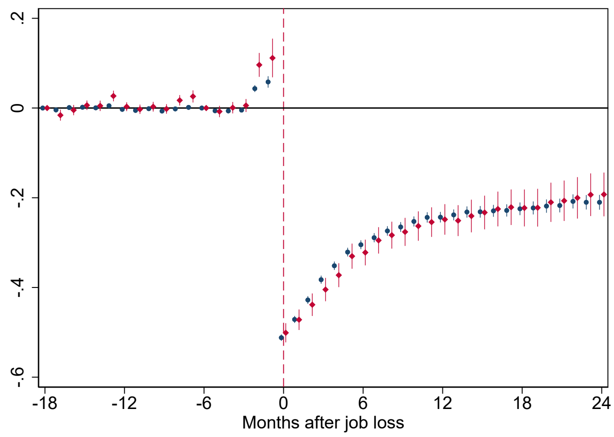
Figure A6: Net saving in liquid assets, levels



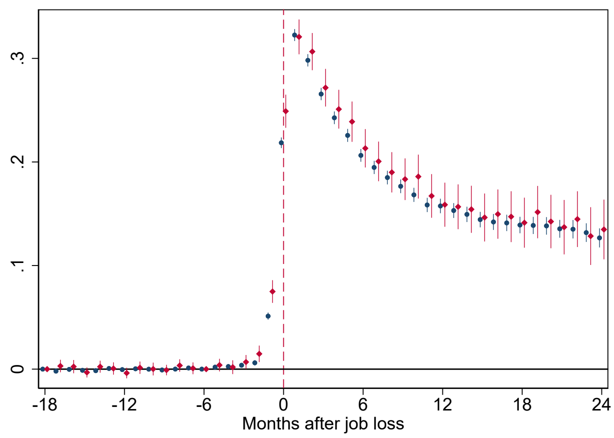
The figure shows average predicted values from the event study model (1) with net saving in liquid assets as the outcome. The dependent variable is measured relative to the household's average disposable income between event months -18 and -3 and winsorized at the 2.5 and 97.5 percentiles within each event month. Each estimate is the average predicted value when the event time variable takes the value indicated on the horizontal axis and control variables are evaluated at their actual values. Vertical lines represent 95% confidence intervals. Standard errors are estimated allowing for clustering at the level of the individual.

Figure A7: Impact of job loss on income and spending: Baseline sample vs. mass layoff sample

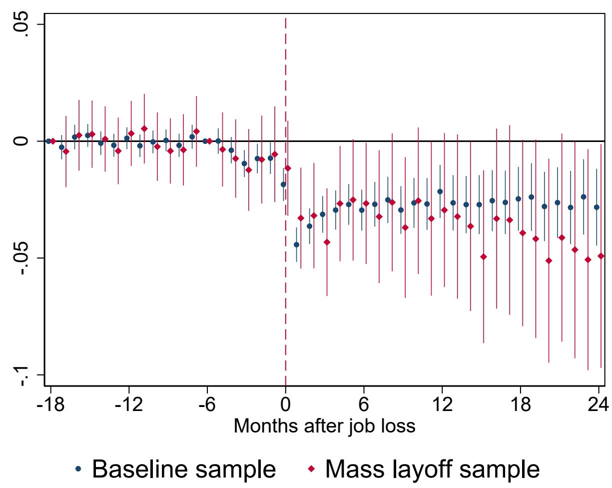
(a) Salary, affected person



(b) Government transfers

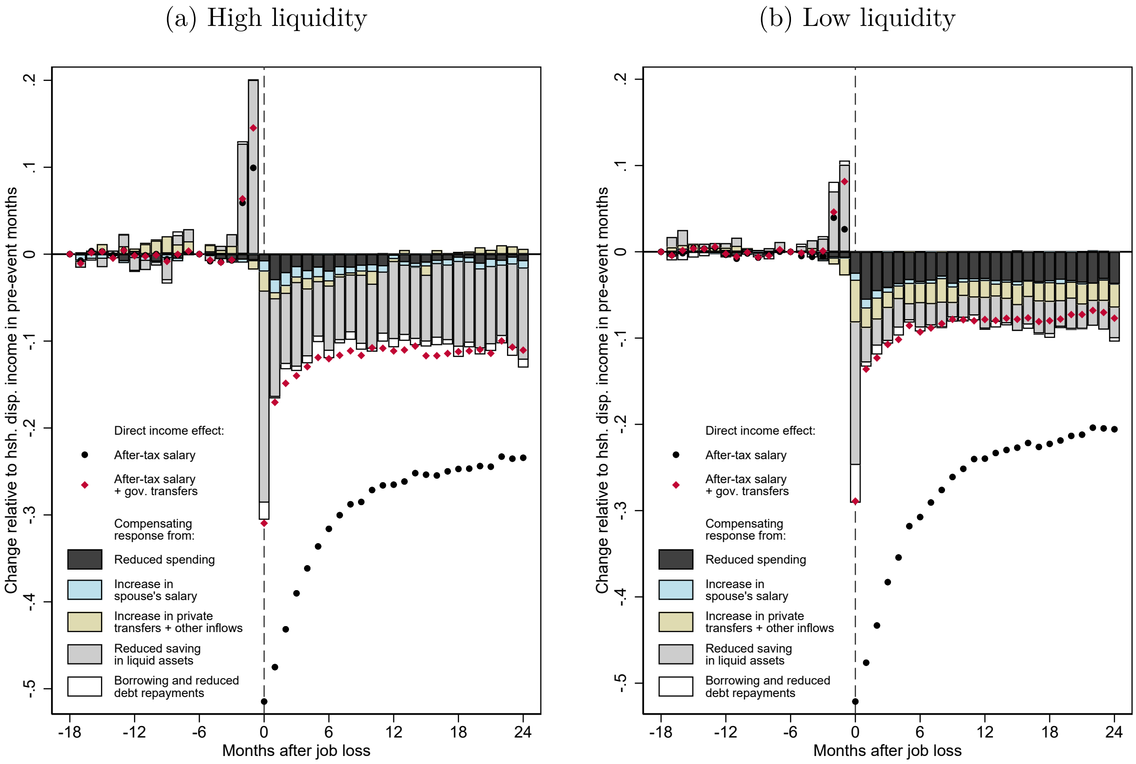


(c) Spending



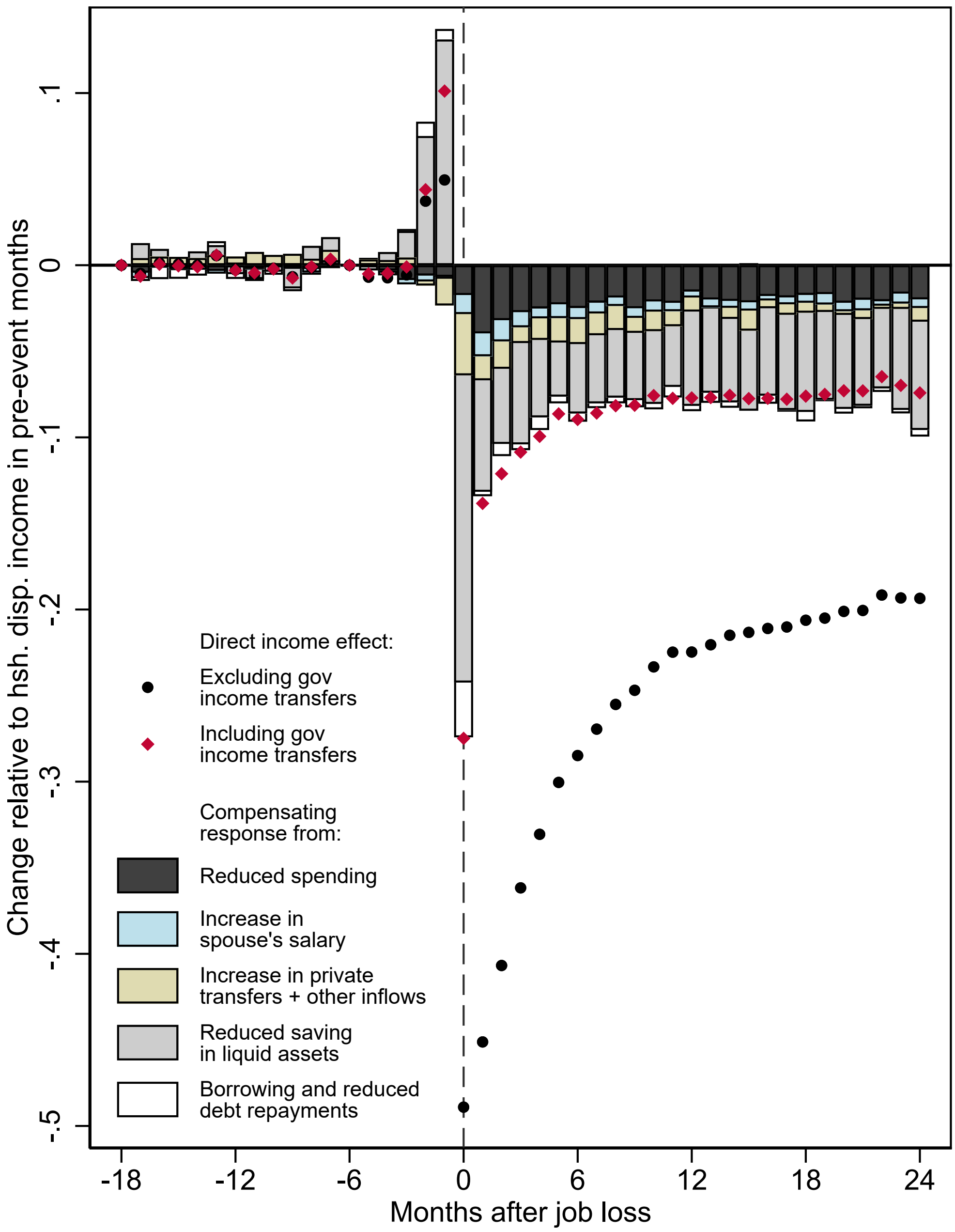
The figure shows estimation results from the event study model (1) of the effects of job loss on income and spending. Blue markers show estimates for the baseline sample of active customers. Red markers show estimates for the subsample of individuals who were laid off concurrently with a mass layoff at their employer. All outcomes are measured relative to the household's average disposable income between event months -18 and -3 and winsorized at the 2.5 and 97.5 percentiles within each event month. Vertical lines represent 95% confidence intervals. Standard errors are estimated allowing for clustering at the level of the individual.

Figure A8: Income, spending and self-insurance responses to job loss, by ex ante liquidity



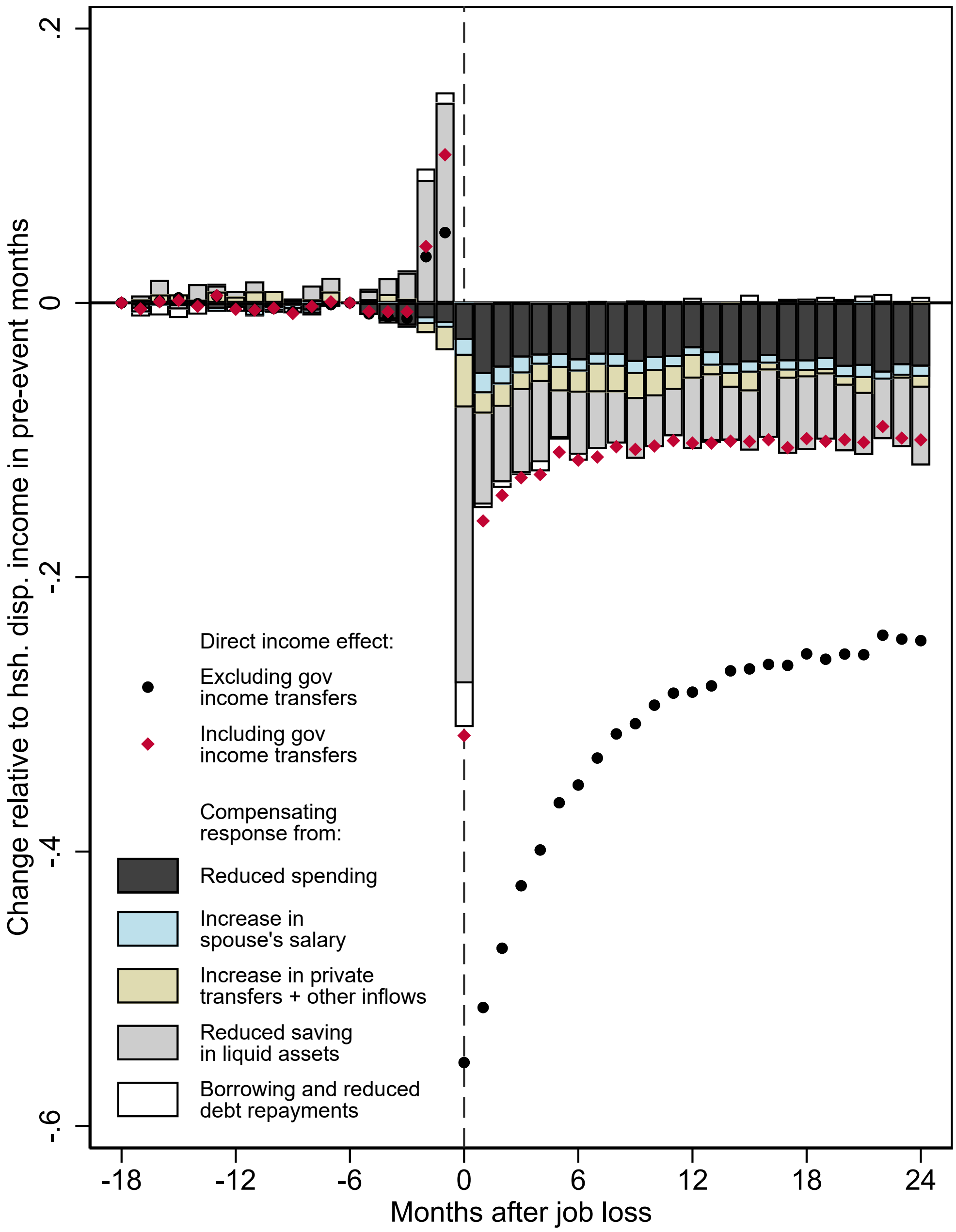
The figure shows estimation results from event study model (1) paralleling those shown in Figure 2, but splitting the sample by ex ante liquidity. Panel (a) shows results for the subsample where the household held liquid assets worth at least two months of disposable income 25 months earlier. Panel (b) shows results for the subsample where the household held liquid below that threshold 25 months earlier. Lagging the value of liquid assets by 25 months means that we lose observations before February 2011. See notes to Figure 2 in the main text for further details.

Figure A9: Income, spending and self-insurance responses to job loss, weighted regressions



The figure shows estimation results from the event study model (1) where observations are weighted so that the sample of active customers matches the characteristics of the gross sample shown in column (1) of Table 1 in the main text. The weights are the inverse predicted probabilities from a probit of active customer status on the demographic characteristics reported in that table. See notes to Figure 2 in the main text for further details.

Figure A10: Income, spending and self-insurance responses to job loss, exclusive bank customers



The figure shows estimation results from the event study model (1) of the effects of job loss on a range of outcomes, estimated on the subsample who belong to households in which all adult members are exclusive customers of the bank. See notes to Figure 2 in the main text for further details.