WORKING PAPER SERIES

The Effect of Job Loss on Bank Account Ownership

Ryan M. Goodstein

Federal Deposit Insurance Corporation

Mark J. Kutzbach

Federal Deposit Insurance Corporation

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Ryan M. Goodstein[†]
Mark J. Kutzbach[‡]

September 2022

ABSTRACT: We estimate the effect of job loss on households' bank account ownership using a novel assembly of data: FDIC-sponsored biennial supplements to the Current Population Survey (CPS), linked to respondents' work history in surrounding months constructed from the basic monthly CPS. We leverage differences in the timing of unemployment spells across respondents to plausibly identify the effect of job loss. Our estimates indicate the effects of job loss are quite large in magnitude. For example, households that experienced a job loss in the months leading up to the FDIC survey are about 18 percentage points more likely to be unbanked than households that lost a job in the subsequent year. This effect is roughly three-quarters of the sample mean unbanked rate among the lower-income, renter households that we study. Job loss also leads to increased use of other transaction products and services that might substitute for a bank account, including prepaid cards, check cashing, and money orders.

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1. INTRODUCTION

For most households in the United States, bank accounts are a fundamental tool for handling household finances. Accounts at federally insured depository institutions are covered by deposit insurance and other consumer protections, providing consumers with a safe place to keep and build savings. Bank accounts can help consumers save time and money when conducting financial transactions (Barr 2012; Armstrong 2016), and having a relationship with a depository institution facilitates access to credit products that can be used to smooth consumption in the event of income or expense shocks (Brevoort and Kambara 2017; FDIC 2018).

Recognizing the potential benefits of account ownership, expanding households' participation in the banking system has long been a focus of public policy.[1] Coincident with these efforts, the proportion of U.S. households without an account at an insured depository institution has been declining over the past few decades, as illustrated in Figure 1. Still, as of 2019 about 5.4 percent of (or 7.1 million) U.S. households were "unbanked", meaning that no one in the household had a checking or savings account at a bank or credit union (FDIC 2020). To inform the ongoing efforts of policymakers and other stakeholders, it is important to understand the barriers that contribute to households' not having an account.

This paper focuses on the role of job loss. Shocks to employment are likely to have an important effect on bank account ownership for several reasons. First, many households in the U.S. have little liquid savings and do not have an adequate financial cushion to cover income or expense shocks. Median net worth among the bottom quartile of U.S. families in 2019 was about $300 (Bhutta et al. 2020), and nearly 4 in 10 households in 2019 indicated that if faced with a $400 emergency expense, they would cover it using something other than cash or its equivalent (FRB 2019). In the event of a job loss, such households may find it difficult to manage their cash flows to avoid bank account fees or falling below minimum balance requirements.[2] Job loss

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[1] For example, the U.S. Treasury's Office of Consumer Policy has undertaken several initiatives to increase low- and moderate-income households' access to financial services and financial education dating back to (at least) 2008. The FDIC chartered an Advisory Committee on Economic Inclusion in November 2006; the committee periodically convenes to provide the FDIC with advice and recommendations on important initiatives focused on expanding access to banking services by underserved populations. More recently, the FDIC has embarked on a #GetBanked public awareness campaign to encourage unbanked households to open insured accounts, for example working with Treasury and the IRS to encourage households to open a bank account to facilitate receipt of Economic Impact Payments following the COVID-19 pandemic. (See, e.g., https://www.irs.gov/newsroom/how-to-get-an-economic-impact-payment-if-you-dont-have-a-bank-account.)

[2] Not all bank accounts have such fee structures. According to a survey by MoneyRates.com "More than 37% of checking accounts now have no monthly maintenance fees." Richard Barrington, October 5, 2021.

compounds this issue because it might result in a household losing direct deposit of a paycheck, which might have qualified the household for a waiver from such fees or balance requirements.

Understanding how economic factors, such as job loss, affect household bank account ownership is particularly timely given the COVID-19-related economic recession and uneven economic recovery. Figure 1 shows that while there has been a secular decline in the unbanked rate among U.S. households, during recessionary periods this decline has slowed or reversed.

We analyze data from biennial FDIC-sponsored supplements to the Current Population Survey (CPS), linked to the basic monthly CPS. In a novel use of these data, we take advantage of the sample rotation structure of the CPS to construct a short panel with information on households' work history in the months leading up to and after the month the FDIC survey is administered. We limit our analysis to lower-income, renter households to focus on those with low wealth who are more likely on the margin of bank account ownership. Such households made up 19.4 percent of all households and 67.9 percent of unbanked households in the U.S. in 2019. The unbanked rate among lower-income, renter households in 2019 was 18.8 percent, compared to 2.1 percent among other households.[3]

The key empirical challenge for this analysis is the potential endogeneity of job loss with respect to bank account ownership. While previous literature establishes that unbanked rates are higher among unemployed populations conditional on demographic and socioeconomic controls, the cross-sectional nature of such analyses makes it difficult to rule out the possibility that unobserved factors correlated with job loss might also affect a household's likelihood of bank account ownership.[4] For example, households with lower educational attainment or financial literacy may select into jobs with a higher turnover rate, and may also be less inclined to open and maintain a bank account.

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https://www.moneyrates.com/research-center/bank-fees/. The Cities for Financial Empowerment Fund's 2021-22 Bank On National Account Standards include a minimum balance requirement of no more than $25 and monthly maintenance fees of $5 or less.

[3] These reported unbanked rates and shares of households are weighted estimates using data from the 2019 FDIC survey. As detailed below, we define lower-income, renter households as those with less than $40,000 in annual income and that do not own their place of residence. The complement includes households that earn at least $40,000 in annual income or are homeowners (or both).

[4] A notable exception is Rhine and Greene (2013), who use the U.S. Census Bureau's Survey of Income and Program Participation (SIPP) to examine transitions out of banking using longitudinal data and finds financial shocks to be a factor. However, that analysis does not consider any confounding factors that may affect both job loss and unbanked status. See Section 2 for a discussion of the relevant literature.

To overcome this identification issue, we leverage differences across households in their job loss status or timing of job loss. In our main empirical analyses, we compare bank account ownership status for a treatment group compared to two alternative control groups. The treatment group ("Job Loss") consists of households in which at least one adult loses a job in the months leading up to the FDIC survey. The first control group ("Remain Employed") includes households in which no adults lose their job in the months leading up to the FDIC survey. The second control group ("Subsequent Job Loss") includes households that lose their job in the year subsequent to the FDIC survey.

The latter control sample, in particular, advances the empirical literature on this topic by focusing on a set of households that are likely similar to the treated households in terms of unobserved characteristics and that experience an analogous economic shock, just at a later date. However, we acknowledge that in practice there are differences in certain observable characteristics of the treatment and control groups (e.g., by race/ethnicity), which may cast doubt on our identification assumption. We address this concern in detail below, in part by showing that under reasonable assumptions the potential scope of omitted variable bias is minimal. A second limitation with our empirical approach is that the sample size of the Subsequent Job Loss control group is quite small, reducing the precision of our estimates and hindering our ability to explore heterogeneity in effects. Therefore, we present results using both control samples throughout the paper.

Our results indicate that job loss has an economically and statistically significant effect on bank account ownership across a variety of specifications and subsamples. In our preferred specification, we find that households experiencing job loss are 12.5 and 17.7 percentage points more likely to be unbanked than households in the Remain Employed and Subsequent Job Loss control samples, respectively. These differences are large relative to the sample mean unbanked rates of 15.8 and 24.1 percent, respectively. We find that the job loss effect is transitory among households that are re-employed (i.e., the adult that experienced a job loss finds employment within a year), but the effect is persistent among households that remain unemployed. Pointing toward a pecuniary mechanism, we find that effects of job loss are most pronounced among the lowest income households we analyze, and that job-losing households are especially likely to report being unbanked for reasons relating to finances. We also find evidence that job loss leads

to increased use of prepaid cards, money orders, check cashing, and pawn shop loans (though not payday loans, which typically require a bank account as well as income).

The remainder of this paper is organized as follows. Section 2 provides background including a review of the relevant literature; Section 3 describes the data and empirical specification; Section 4 presents the estimation results; and Section 5 provides concluding thoughts including potential policy implications.

2. BACKGROUND

Accounts at federally insured depositories (i.e., banks or credit unions) offer several potential benefits to consumers. They provide a safe place to store and build savings, offering deposit insurance and consumer protections such as upfront disclosures about fees and rates, provisions for financial privacy, and protections against error and fraud. Bank accounts can save consumers time and money when conducting financial transactions such as receiving income, paying bills, and purchasing goods and services.[5] Bank accounts also facilitate access to credit for "credit invisible" consumers (those that lack credit history at one of the three nationwide credit reporting agencies), as lenders may underwrite credit products using deposit account balances and transaction history (Brevoort and Kambara 2017; FDIC 2018).[6]

Despite these potential benefits, bank account ownership differs substantially across segments of the U.S. population. For example, unbanked rates are higher among lower-income households, renter households, less-educated households, Black and Hispanic households, working-age disabled households, and households with income volatility, as shown in Appendix Figure A1. Such cross-sectional differences in unbanked rates have persisted for at least the past few decades and might be attributable to a variety of factors, which we summarize here.

Households with low liquid savings or more volatile income may find traditional bank accounts difficult to manage due to minimum balance requirements, restrictions on access to

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[5] By some estimates (e.g., Barr 2004; Armstrong 2016), the costs of conducting financial transactions without a bank account may be in the hundreds of dollars per year.

[6] Some lenders are leveraging technology to underwrite credit products using bank account information. For example, Petal-branded credit cards (issued by WebBank) are underwritten using cash-flow data from bank statements. Home mortgage lenders may also use technology to underwrite using bank account information. For example, as of September 2021 Fannie Mae's Desktop Underwriter now allows lenders to, "…automatically identify recurring rent payments in the applicant's bank statement data to deliver a more inclusive credit assessment." (Fannie Mae press release, "Fannie Mae Introduces New Underwriting Innovation to Help More Renters Become Homeowners" August 11, 2021.)

funds, or potential fees including those for insufficient funds or overdrafts (e.g., Hogarth et al. 2004; Hogarth et al. 2005; Barr 2004).[7] Households that have had past difficulties in managing accounts may not be able to qualify for a new bank account (e.g., Campbell et al. 2012). Further, geographic proximity to financial institutions may also play a role. Caskey (1994) argues that a relative scarcity of bank branches in lower-income and minority neighborhoods may inhibit these households' ability to open and maintain an account.[8] Finally, households of different backgrounds have different tastes and preferences which affect their demand for having bank accounts. For example, some households may lack trust in financial institutions, may lack familiarity with banks, or may avoid accounts due to limited financial literacy or numeracy.[9] Knowing the relative importance of such factors is key for informing policy interventions to encourage households to obtain bank accounts.

This paper focuses on the role of employment. In particular, to the extent that households that suffer an unexpected or involuntary job loss have lower and more volatile household income, they may have more difficulty managing their cash flows and keeping enough money in the account to avoid fees or falling below minimum balance requirements. In short, for some low-savings households experiencing an income shock, the cost of maintaining certain bank accounts may outweigh the value added of those accounts.

While previous empirical evidence on the role of employment is somewhat limited, subjective data suggest that financial considerations are likely critical.[10] For example, among unbanked households in 2019, nearly half did not have an account because they, "Don't have enough money to meet minimum balance requirements," and for over one-in-three, "Bank

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[7] Recognizing that minimum balance requirements and other account fees might make bank accounts unattractive or difficult to manage for households with lower or more volatile income (FDIC 2016), policymakers and other stakeholders have developed a template for depository accounts that might be well-suited for such households. For example, see the FDIC's Model Safe Accounts Pilot (FDIC 2012) and the Cities for Financial Empowerment Fund's Bank On National Account Standards for 2021-2022 (CFE 2020).

[8] Goodstein and Rhine (2017) show that households residing in neighborhoods with limited access to bank branches (e.g., no branches within 5 miles) are less likely to have a bank account and more likely to use nonbank transaction services, although effects are modest in size. In related work looking at the mortgage market, Ergungor (2010) finds that originations increase and interest spreads decline when there is a bank branch located in a low- to moderate-income neighborhood.

[9] See Hogarth et al. (2004) for a survey of the early literature on these points. More recent evidence includes Northwood and Rhine (2017), who show that patterns of bank account ownership and use of nonbank financial services differ among immigrant households (and relative to native-born households), for example by whether they reside in an ethnic enclave or by the level of maturity of their home country's retail banking system.

[10] Qualitative interviews elicit a variety of rationales for closing a bank account when money is tight, as in the case of job loss. For example, some households opt to use money orders, rather than checks, to avoid nonsufficient funds fees and to make sure that certain bills are paid even if others are not (KCFED 2010 p. 6; Servon 2017 p. 7 and 17).

account fees are too high," or, "Bank account fees are too unpredictable." However, non-financial reasons are also frequently cited; over one-in-three unbanked households in 2019 didn't have an account because they, "Don't Trust Banks," or, "Avoiding a Bank Gives More Privacy."[11] A limitation of these data is that comparable information is not observed for banked households, which makes it difficult to assess the extent to which such factors contribute to households' likelihood of maintaining a bank account.

The FDIC's 2013 survey included a series of subjective questions designed to uncover causal linkages between life events and households' transitions into and out of the banking system. The results suggest that unemployment is quite important; among households that became unbanked within the previous year, about 45 percent had experienced a job loss or significant income loss over the same period, and 34 percent indicated that the job loss or significant income loss directly contributed to the household becoming unbanked (FDIC 2014).

Rhine and Greene (2013) find that banked households that suffer a loss of income or employment are likely to become unbanked, based an analysis of Survey of Income and Program Participation (SIPP) longitudinal data between 2004 and 2006. But this study suffers from the potential limitation that *changes* in employment status may be correlated with unobservable differences across households.

The main innovation of this paper is that we leverage differences in the timing of job loss across households to more cleanly identify the effect of job loss on the likelihood of being unbanked. We describe our identification strategy in detail in the next section. Further, relative to the prior literature, our analysis covers a longer and more recent period (years 2011 to 2019) that encompasses substantial developments in the provision of banking services. For example, over the past decade the number of physical bank branches in the U.S. has declined, use of online and mobile banking services has become more widespread, as has the availability of alternatives to bank accounts such as prepaid cards and online transaction accounts like PayPal and Venmo. In light of these developments, we explore whether the effects of job loss on account ownership has changed over time.

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[11] Estimates described in this paragraph are from FDIC (2020); results from earlier FDIC surveys are qualitatively similar.

3. METHODOLOGY

3.1. Data

The CPS is a monthly survey of the U.S. civilian noninstitutional population, representative at the national- and state-level. In addition to serving as the primary source of information on the labor force characteristics of the U.S. population, the CPS also collects data on demographic characteristics, and on supplemental information that varies each month.

One such supplement to the CPS is the FDIC-sponsored Survey of Household Use of Banking and Financial Services, first administered in January 2009 and then biennially in June between 2011 and 2019.[12] We use the 2011 to 2019 FDIC surveys in this study, pooling the data across survey years. The key outcome variable of interest for this analysis is whether a household is "unbanked", meaning no one in the household had a checking or savings account at the time the FDIC survey was administered.

We also examine households' use of other financial transaction services that might substitute for (or complement) the transaction services provided by a bank account, including prepaid cards, nonbank money orders, and nonbank check cashing services. Finally, we study households' use of two nonbank credit products: pawn shop loans and payday loans. Such products are disproportionately used among households that may not have access to "mainstream" credit products such as credit cards or unsecured personal loans.[13] These nonbank transaction and credit products lack the consumer protections of a bank account and mainstream credit, and may result in higher costs to the consumer.

We use household identifiers to link the FDIC surveys to the Basic Monthly CPS, which includes household data as well as data for each person in the household. Specifically, our analyses include controls for demographic characteristics (age; sex; race and ethnicity; nativity; marital status; educational attainment) and income and employment characteristics (annual household income; self-employment; full-time status; industry; number of adults and employed

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[12] Prior to 2019, the FDIC-sponsored supplement to the CPS was known as the Unbanked/Underbanked supplement. Because some of the outcomes we study are not available or measured differently in the January 2009 supplement, we exclude these data from our analysis.

[13] Unless noted otherwise, the outcomes we examine are observed in the FDIC survey in each of the supplement years between 2011 and 2019. In some years the FDIC survey also collects information about use of other nonbank financial transaction services and credit products, including bill payment services and rent-to-own services.

adults in household).[14] For simplicity we use the attributes of the "householder" (i.e., the person (or one of the people) who owns or rents the home) for demographic characteristics, and we use information from the household's first month-in-sample (MIS) in the Basic Monthly CPS to describe its employment characteristics. If the householder is not employed in the first month, we obtain employment characteristics from the first employed person in the household. (See Appendix Table A1 for a detailed list of the control variables and how they are constructed.)

Finally, we use the time series of the Basic Monthly CPS to construct a short panel with information on households' work history in the months leading up to and after the FDIC survey month.[15] To do so, we take advantage of the CPS sample rotation structure, illustrated in Figure 2.

Sampled households are in the CPS for 4 consecutive months, are then out for 8 months, and then return for another 4 consecutive months. Thus, each FDIC survey is administered in June to a sample that can be disaggregated into one of eight "rotation groups", based on the household's MIS in that survey month. Work histories begin for the second rotation group in May, the third rotation group in April, and the fourth rotation group in March.

To focus our analysis on changes in labor force status that are most likely to impact household finances and least likely to be voluntary, we limit our sample to persons aged 20 to 59 in their first month of observation (MIS=1) in the CPS. We require that persons do not attrite in the first four months-in-sample, to ensure consistency of the households in our sample.[16] We also require that each household has at least one person employed in the first month of observation.

Finally, we focus our analysis on a subset of households that are likely to be on the margin of bank account ownership due to financial constraints. Specifically, we limit the sample to renter (i.e., non-homeowner) households with annual income below $40,000. Such households made up 19.4 percent of all households and 67.9 percent of unbanked households in the U.S. in 2019 (FDIC 2020). Renter households and those in the lowest income quintile have very little wealth or savings, so loss of income might almost immediately affect such households' ability to meet

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[14] As a robustness check we also included controls for geography (e.g., Census region; metropolitan status); doing so had little effect on our estimates.

[15] To link across months, we use the household fields specified above as well as the CPSIDP field curated by CPS-IPUMS that tracks individuals across months (Flood et al. 2018).

[16] The attrition requirement is not very restrictive; 85.5 percent of persons and 90.2 percent of householders meeting the other sample requirements respond to each of the first four months in sample. The pattern of our main results is also robust to imposing more stringent restrictions on attrition. However, this comes at a cost of reduced sample size, and sample restrictions based on attrition may not be innocuous if attrition is non-random.

minimum balance requirements and absorb fees associated with overdrafts and negative balances.[17]

3.2. Empirical Specification

We estimate linear probability models of the form

$$P[Unbanked_{itr}] = \alpha + job\_loss_i\beta + X_i'\gamma + Y_i'\delta + \mu_t + \pi_r + \varepsilon_{itr} \qquad (1)$$

where i is an index for households, and t and r are indices for the FDIC survey year and CPS rotation group, respectively.[18]

The dependent variable $Unbanked_{itr}$ is a binary indicator equal to 1 if the household is unbanked, and equal to 0 otherwise. The key explanatory variable is job_loss_i, a binary indicator equal to 1 if the household experienced a job loss in the months leading up to and including the month of the FDIC survey, and equal to 0 otherwise. (Below we describe precisely how the job loss indicator is defined.) The unbanked rate among households that did not experience a job loss leading up to the FDIC survey serves as the counterfactual – or the expected outcome absent treatment. We interpret our estimate for the parameter β as the effect of job loss on the likelihood of being unbanked. A positive coefficient would be consistent with job loss inducing households to become unbanked.

X_i is a vector of household-level demographic characteristics, which may reflect households' tastes and preferences or correlated supply-side factors (e.g., geographic proximity to bank branches). Y_i is a vector of the household's income and employment characteristics in the first month the household is observed. μ_t is a vector of survey year fixed effects, and π_r is a vector of CPS rotation group fixed effects. These controls account for potential time trends and for seasonality of households starting the CPS in different months and also control for

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[17] The SIPP reports median value of assets by household characteristics (Median Value of Assets for Households, by Type of Asset Owned and Selected Characteristics: 2019). Renter households have $4,084 in net worth (compared to $305,000 for owners), with only $2,200 in a financial institution (compared to $11,700 for owners). Households in the lowest quintile of annual income (approximately equal to our cutoff) have $6,030 in net worth and only $650 in a financial institution (U.S. Census Bureau 2020).

[18] We use linear probability models for ease of interpretation, particularly in subsequent specifications that include interaction terms. We estimate the regression models using Stata 16, with the robust option for estimating standard errors (StataCorp 2019). The magnitude of effects implied by our main results is similar to estimates using probit models, as described below.

differences in month-in-sample for when a household responds to the FDIC survey. ε_{itr} is an error term.

The key empirical challenge for this study is the potential endogeneity of job loss with respect to bank account ownership. Despite the extensive set of control variables in the model, it is difficult to rule out the possibility that the error term ε_{itr} is correlated with unobserved factors that also affect households' likelihood of having a bank account. One concern is that a financial shock resulting in a negative account balance could lead to terminating a bank account and directly affect work. For example, a financial shock that prevents a worker from repairing a broken vehicle could contribute to job loss. Another potential concern is that households with unobserved, lower financial literacy or less trust in financial institutions might be less likely to maintain a bank account and might tend to select into jobs with a higher turnover rate. In either case (as well as others not described here), controlling for observable differences may not be sufficient.

To address this identification issue, we leverage differences across households in their job loss status or timing of job loss. Specifically, we take the following approach. First, we limit the sample to households in rotation groups 2, 3, or 4 in the month the FDIC survey is administered (i.e., they are in their 2nd, 3rd, or 4th MIS in the June CPS). For this subset of households, we observe labor force status in the months leading up to (and including) the FDIC survey, as well as in the subsequent year. We then compare the probability of bank account ownership between a treatment group and two alternative control groups, as illustrated in Figure 3.

The treatment group (Job Loss) consists of households in which at least one adult employed in the first month of observation (MIS=1) becomes unemployed in their second month of observation (MIS=2).[19] Depending on the specific rotation group, the job loss may occur in April, May, or June. Limiting our analysis of job loss to the recently unemployed focuses the analysis on a group experiencing a recent change in financial circumstances and with comparable prospects for re-employment, as job finding rates decline with duration of nonemployment.[20]

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[19] We consider all job types in our definition of employment, including wage and salary workers and the self-employed. In our empirical model, we include controls for self-employment status, full time (vs. part time) status, and a categorical variable for industry type. See Table 1 for details.

[20] We identify job loss using contemporaneous responses to construct a work history, as opposed to using retrospective responses on duration of unemployment. Duration responses may differ from contemporaneous responses as respondents may report the duration of search even if that search period included short-term jobs while searching. Kudlyak and Lange (2018) find that work histories explain job finding rates more completely than job search duration alone. Another concern with retrospective responses is recall bias, as is discussed by Evans and

The first control group (Remain Employed) consists of households in which no adults employed in the first month of observation (MIS=1) lose a job up through the fourth month of observation (MIS=4). Note that we cannot rule out an involuntary job change following a very brief period of unemployment. Most unemployment spells last less than two months, but from the CPS, it is not possible to measure spells lasting less than a month.

The second control group (Subsequent Job Loss) consists of households that have at least one adult employed in the fifth month of observation (MIS=5) who then suffers a job loss in the sixth month of observation (MIS=6). With this construction, the job losses in the second control group occur exactly one year later than job losses in the treatment group. These households must also have at least one person employed in the first month of observation, though that person need not be the one that ultimately loses a job.

Households that do not meet the above criteria for the treatment or control groups are dropped from the analysis sample. We estimate the regression models separately for: the Job Loss treatment group vs the Remain Employed control group (specification A), and the Job Loss treatment group vs the Subsequent Job Loss control group (specification B).[21]

Our identification assumption is that, conditional on the other control variables in the model, the job loss experienced by the treatment group is exogenous with respect to the households' decision over bank account ownership. In specification A, which parallels the approach in Rhine and Greene (2013), this assumption is somewhat tenuous as discussed above. It may be the case that households that suffer a job loss may be unobservably different than households that remain employed. Even so, we make several significant sample restrictions and include a rich set of controls to mitigate these concerns.

The identification assumption in specification B is not as susceptible to those concerns. Households in both the treatment and control groups are observed to suffer a job loss during the period of observation; the only difference is in the timing of the unemployment spell. These groups are therefore likely to be similar along unobserved dimensions, mitigating potential endogeneity concerns. However, one drawback of this second approach is that in practice the

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Leighton (1995), with regards to the Displaced Worker Supplement to the CPS. For results with alternative definitions of job loss, see the Appendix.

[21] In addition to the empirical specifications described here, we explored using a variety of other intuitively similar specifications, and found that the estimation results were qualitatively similar. We use the specifications presented in the paper because of their (relative) simplicity and because they are less restrictive in terms of sample size.

sample size of the Subsequent Job Loss control group is quite small, reducing the statistical precision of our estimates and limiting our ability to explore heterogeneity in effects. To facilitate comparisons with previous literature and to gauge the degree of selection that may be present in the first Remain Employed model, we present regression results from both specifications.

4. RESULTS

4.1. Descriptive Statistics

Table 1 presents unweighted sample means for the 179 households in our treatment sample (Job Loss) and for the control samples (A. Remain Employed and B. Subsequent Job Loss) with 5,409 and 103 households, respectively.[22] The unbanked rate for the treatment sample (32.4 percent) is substantially greater than the two controls (15.3 percent and 9.7 percent respectively). Use of other financial products and services such as prepaid cards and nonbank money orders is also higher among the treatment group compared to the control groups.

Table 1 also shows that observable characteristics of the treatment and control groups differ along certain dimensions. In Appendix Table A2, we regress job loss, our explanatory variable of interest, on the other controls. The estimates from those regressions show, in a multivariate context, which observable characteristic factors are most closely associated with treatment. In both regressions, the treatment households are more likely to be Black (non-Hispanic) and less likely to be full time employed. Relative to the remain-employed control group, treated householders are also more likely to be Hispanic and less likely to be in the higher portion of the household income distribution (20,000 - 39,999). Relative to the Subsequent Job Loss control group, treated householders are less likely to be married and more likely to work in the retail and service sectors. The treated group is also substantially less likely than the Subsequent Job Loss control group to be observed in survey year 2019.[23]

That certain observable characteristics of the Job Loss treatment group differ from the Subsequent Job Loss control group is surprising, and may cast some doubt on our argument that

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[22] Attrition of persons and households contributes to the smaller sample size of the subsequent job loss sample relative to the treatment sample. See Section 4.3 for further discussion.

[23] The Subsequent Job Loss control group disproportionately consists of observations from survey year 2019 because, for this year, the subsequent year spans April to June 2020, when COVID-19 related job losses were widespread. See Section 4.4 for further discussion on this point.

these groups are likely to be similar along unobserved dimensions. We address this concern in detail in Section 4.4 below.

4.2. Main Estimation Results

Table 2 presents OLS estimates of the effect of job loss on the probability of being unbanked. Each column presents estimates from a different regression. In panel A the control is the Remain Employed group. The estimate in the first column indicates that when controlling only for survey year and rotation group fixed-effects, households that lost a job are 16.9 percentage points (pp) more likely to be unbanked relative to households without a job loss. Adding controls for demographics, income, and employment characteristics to the model (column 2), the estimated effect falls to 12.5pp, statistically different from zero at the 5% level.

Panel B presents the results from our preferred specification, where the control is the Subsequent Job Loss group. The estimate in the third column indicates that households that had a job loss in the months preceding the FDIC survey were 18.7pp more likely to be unbanked than households that had a job loss in the year following the survey. Adding the full set of controls to the specification (column 4) has little effect; the point estimate falls to 17.7pp.

These estimates indicate that among the lower income, renter households we study, the effect of job loss on household bank account ownership is quite large in magnitude and similar across specifications. The point estimate in column 2 is about 79% of the overall unbanked rate for the Remain Employed analysis sample, and about 73% of the overall unbanked rate for the Subsequent Job Loss analysis sample. Although not directly comparable, our estimates suggest that the effect of job loss is somewhat larger in magnitude than the estimates from Rhine and Greene (2013).[24]

Further, the estimated effects of job loss are quite large compared to the other control variables in the model, shown in Appendix Table A3. For example, the point estimates on job

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[24] Reconciling our results with Rhine and Greene (2013) is not straightforward because of differences in the empirical specifications and samples being analyzed. Rhine and Greene (2013) specify a model with separate controls for job loss, income loss, and health insurance loss; in practice a job loss could result in a household losing income and/or health insurance. Under the assumption that the effects are additively separable, their estimates imply that the total effect of a job loss, loss of income of 50% or more, and loss of health insurance on likelihood of transitioning into unbanked status is about 50% of the mean rate of transition in their sample. This point estimate is smaller than the roughly 75% effect we estimate. Some of this difference may be due the samples being analyzed. Rhine and Greene (2013) estimate their model using all households in the SIPP (between 2004 and 2006), while we limit our analysis to lower-income renter households for whom effects are likely larger.

loss are of similar magnitude to the effects associated with being Black or Hispanic and are larger than the effects associated with educational attainment (i.e., having an Associate's degree or higher), household income (i.e., income of $20-40k relative to $20k or less), and nearly all of the other control variables in the model.

As discussed in the methodology section, the identification assumption for the analysis using the Remain Employed control group may be tenuous due to omitted variable concerns. However, the fact that the economic magnitudes of the effects in columns (2) and (4) are similar, especially when compared to their respective sample means, suggests that these identification concerns may not be much of an issue in practice.[25] It is also worth noting that for the Subsequent Job Loss analysis, adding controls for household demographics, income, and employment characteristics has little impact on the point estimate of the job loss effect specifications (comparing columns 3 and 4). This suggests that the scope of any omitted variable bias may be relatively small, to the extent that selection on observables is informative about selection on unobservables.[26]

We now explore heterogeneity in the effects of job loss with respect to family income, race/ethnicity, and survey year; estimates are presented in Table 3. Panel A (columns 1 and 2) of Table 3 shows results for the analysis where the control group is Remain Employed, and panel B (columns 3 and 4) shows results for the Subsequent Job Loss control group. We caution that because of the relatively small sample size used in the latter analyses, the estimated effects are statistically imprecise and in some cases may be counterintuitive. (The estimates in Table 3 are also presented graphically in Appendix Figures A2, A3, and A4.)

Panel (i) of Table 3 presents regression results from a specification where the job loss indicator is interacted with an indicator for household income between $20-40k. Perhaps unsurprisingly, the estimated job loss effect differs sharply by household income. Among households with less than $20,000 in household income (the omitted category of the interacted variable), job loss increases the likelihood of being unbanked by 20.3pp and 29.8 pp in columns (2) and (4), respectively. In both cases these effects are more than 100% of the sample mean unbanked rate. The effects of job loss are substantially smaller among households with $20-40k

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[25] An alternative interpretation is that impact of potential omitted variable bias does not appear to be larger for the Remain Employed analysis than for the Subsequent Job Loss analysis.

[26] We return to this point in Section 4.4, where we implement the methods of Oster (2019) to show that the scope of omitted variable bias is likely small.

in income. For example, the estimates in column four indicate that for this group, job loss leads to a roughly (0.298 − 0.226=) 7.2 pp increase in the likelihood of being unbanked.[27]

In contrast, the estimates in panel (ii), column (2) indicate that the effects of job loss do not systematically differ by race and ethnicity. The point estimates on the Hispanic and Other Race interaction terms are negative and is very close to zero for the interaction on Black households; none of these estimates are statistically different from zero at the 10% level. The same is true in column (4) with the exception of the interaction term on Black households; this estimate is large and statistically significant. Overall the evidence is mixed, but our reading of these results is that, conditional on the other controls in the model, the effect of job loss on probability of being unbanked is broad-based across race and ethnicity.

Panel (iii) of Table 3 explores whether the effect of job loss on being unbanked varies over the years of our analysis sample. As discussed earlier, our analysis spans a period of time that saw substantial evolution in the landscape for financial transaction products and services, including the advent of mobile banking, declining availability of bank branches, and increasing availability of other transaction products including prepaid cards and online payment accounts such as PayPal and Venmo. However, the estimates in columns 2 and 4 suggest that job loss effects are generally quite stable across survey years. This holds true even for the Subsequent Job Loss control group in survey year 2019, among whom the job loss occurred during the COVID-19 pandemic.[28]

An important question for policy is whether households that become unbanked because of a job loss remain unbanked for a long duration, or whether the effect is more transitory. We cannot directly examine the duration of households' "unbanked spells" following a job loss, because we

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[27] As a check on the breadth of our results, we re-estimated the analysis using a broader population of households. For this broader analysis (not shown), we define a Remain Employed and a Subsequent Job Loss sample without the restriction to lower-income renters (those households who are not homeowners and who have income below $40,000 in the last year). Applying the regression model to each of those samples, we find that, overall, households with a job loss are more likely to be unbanked. However, when we include an indicator for lower-income renter households and interact it with job loss, we find that the effect of job loss is substantially larger for lower-income renter households than for the complement of households (about three times larger in the broader Remain Employed sample and about six times larger for the broader Subsequent Job Loss sample). We acknowledge that effects of job loss on bank account ownership may be more widespread than the lower-income renter population we focus on here, but we believe that we can make the strongest case for causal interpretation among this subset of households.

[28] For the subsequent job loss control group sample in 2019, the job loss would have occurred in April, May, or June 2020. Because job losses were so widespread during the COVID-19 pandemic, one might be concerned about differences in selection (e.g., less negative selection) into job loss for this group. However, the estimates in panel (iii) of Table 3 suggest this is not much of an issue in practice. Further, in Section 4.4 we show that results are similar if we drop survey year 2019 from the sample.

only observe a household's bank account ownership status in the month the FDIC survey is administered.

We take a different approach, examining whether bank account ownership as observed in the FDIC survey is affected by the incidence of job loss roughly one year prior. Appendix Figure A5 provides an illustration of the identification strategy used for this "Persistence" specification. The sample consists of households that are in rotation groups 5 through 8 at the time of the FDIC survey. Among these households, we define a "Job Loss Previous Year" treatment group that includes households with an adult that was employed in their first month of observation (MIS=1) and then unemployed in the second month of observation (MIS=2). The control group "Remain Employed Previous Year" consists of households where all adults who were employed in their first month of observation remained employed through the fourth month of observation (MIS=4). Households that do not meet either criteria are dropped from the analysis.[29]

Estimation results from this analysis are presented in Table 4. In the specification with full controls (column 2), a job loss in the previous year increases the likelihood of being unbanked by 5.9 percentage points, statistically significant at the 10 percent level. The magnitude of this effect is about 41% of the sample mean unbanked rate, somewhat smaller than the magnitude of the more contemporaneous job loss effects presented in Table 2 (a 12.5pp effect, at 79 percent of the respective sample mean).

Columns 3 and 4 of Table 4 present estimated effects when we disaggregate the treatment (Job Loss Previous Year) into two mutually exclusive groups based on the labor force status of the job loser in the first month observed in the survey year (i.e., MIS=5). About one-in-three treatment households are in the "Not Re-employed" group; around two-in-three treatment households are in the "Re-employed" group. Strikingly, the entire effect of job loss in the previous year is driven by the subset of households for whom the unemployment spell was relatively longer-duration, as proxied by the Not Re-employed group. Among households for whom the job loss was a shorter-duration event (as proxied by the Re-employed group), the likelihood of being unbanked was similar in magnitude to (and not statistically different from) the control group.

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[29] We examined several alternative specifications to the Persistence specification presented here; results (not shown for brevity) were qualitatively similar. We also note that the Persistence specification described here is analogous to the main analysis using the Remain Employed control group, and acknowledge that the specification is subject to similar identification concerns.

We next explore whether households that suffer a job loss are more likely to use certain financial products and services that may substitute for the transaction services that a bank account provides. Each column and panel of Table 5 presents estimation results from a separate empirical specification similar to equation 1 (for the Remain Employed and Subsequent Job Loss control groups in panels A and B), except that the dependent variables listed as column labels indicate the use of a financial product or service.[30] The estimates in the first three columns of Table 5 indicate that job loss leads to increased use of prepaid cards (in Panel A), nonbank money orders, and nonbank check cashing services, respectively. Together with our earlier finding that job loss reduces the likelihood of bank account ownership, these results suggest that households may turn to such products to handle their financial transactions needs following a job loss.[31]

In the fourth and fifth columns of Table 5 we examine whether job loss increases the likelihood of using certain nonbank credit products, specifically pawn shop loans and payday loans.[32] Column 4 of Table 5 indicates that job loss leads to a higher likelihood of taking out a pawn shop loan, although the estimate is not statistically significant in panel B, where the control group is Subsequent Job Loss. In contrast, the effect of job loss on use of payday loans is a precisely estimated zero in both specifications. This null finding is consistent with our main result that job loss increases the probability of being unbanked, given that having a bank account and having income is generally a prerequisite for taking out a payday loan. These results suggest that households experiencing job loss may both become unbanked and turn to credit products that do not require a bank account.[33]

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[30] We note that for each of the outcome variables examined in Table 5, the FDIC supplement questionnaire asks whether the household used the product within 12 months of the FDIC supplement month (June). Thus there is a potential timing issue, in that the outcome measure may reflect use in months prior to when the treatment (job loss) actually occurred. For the estimates to be interpreted as reflecting the causal effect of job loss, we must assume that variation in use of the product in the months prior to the treatment is uncorrelated with the incidence of job loss, conditional on the other controls in the model.

[31] In unreported results, we find that job loss makes households more likely to be both unbanked and to use nonbank money orders as well as to be both unbanked and use nonbank check cashing services (we find mixed results for being unbanked and using a prepaid card).

[32] Households may use such credit products to smooth consumption after adverse shocks (see, e.g., Dobridge 2018).

[33] In unreported results, we find that job loss makes households more likely to be both unbanked and to use a pawnshop loan. We do not find that job loss is associated with an increased likelihood of being unbanked and using a payday loan. Nicolini and Cude (2019) find that households with lower financial well-being (in terms of feeling secure in their financial future), and especially unbanked households, were more likely to use a pawn shop.

4.3. Results from Supplemental Analyses

We now provide results from supplemental analyses which show that our estimated job loss effects are generally consistent with the pecuniary mechanism by which we expect job loss to affect bank account ownership. Specifically, we examine how job loss effects differ by whether a household reports a "financial" vs. "non-financial" reason for being unbanked, and whether, at person-level, effects are larger for the individual losing the job compared to others in the household. We also attempt to shed light on dynamics by exploring whether job loss effects differ by households' recency of becoming unbanked, and how effects differ by the month-to-month timing of job loss relative to the FDIC survey.

Table 6, panels (i) and (ii) show how job loss effects differ depending on whether the household cites "financial" or "non-financial" reasons for being unbanked, respectively.[34] If the effects we estimate are attributable to job loss (and not some correlated unobserved factor), we'd expect the magnitude of the effects to be larger for financial reasons compared to non-financial reasons. Consistent with this intuition, the estimates in columns (1) and (2) of panel (i) show that effects of job loss on likelihood of being unbanked for financial reasons are large in magnitude. However, the estimated effects of job loss on being unbanked for non-financial reasons are a bit mixed. For the Remain Employed specification the estimated effect of job loss on being unbanked for non-financial reasons is small and statistically insignificant, as shown in column 1 of panel (ii). But for the Subsequent Job Loss specification in column 2, the point estimate for the effect of job loss on being unbanked for non-financial reasons is about the same size as the estimate in panel (i), where being unbanked for financial reasons was the outcome.

Next we explore whether job loss effects differ by the composition of individuals within a household. First, we allow effects to differ for households with one working adult compared to households with two or more working adults. It seems plausible that job loss effects for the latter group might be smaller, to the extent that for these households the financial impact of a job loss may be less pronounced relative to total household income. The estimates in Table 7 offer some

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[34] Specifically, in panel (i) the dependent variable is an indicator equal to 1 if the household is unbanked and cites any of the following as the main reason for being unbanked: "Previously had an account but the bank closed it", "ID, credit, or banking history problems", "Do not have enough money", "Bank account fees or minimum balance requirements are too high" (in 2011); "ID, credit, or banking history problems", "Do not have enough money to keep in account or meet minimum balance", "Account fees too high or unpredictable" (in 2013); "ID, credit, or former bank account problems", "Do not have enough money to keep in account", "Account fees too high", "Account fees unpredictable" (in 2015, 2017, and 2019). All other reasons for being unbanked including nonresponse are explained in panel (ii).

support for this hypothesis. For example, looking at results for the Remain Employed control group in column 1, panels (i) and (ii), the point estimate is slightly bigger for households with one employed adult (13.8 pp) compared to households with two or more employed adults (9.3 pp). But these estimates are not statistically different from each other, and relative to the mean unbanked rates for these samples, the magnitudes of the effects are similar.

To this point, we've analyzed outcomes measured at the household level, including bank account ownership and use of other financial products and services. We do so because, to the extent that adults in a household may share a bank account (or their use of other financial products and services) to handle household finances, household-level bank account ownership is arguably the most relevant measure for informing public policy.[35]

However, understanding how job loss affects bank account ownership at person-level is also of interest. This is particularly true to the extent that job loss affects the affordability of a bank account. For example, without direct deposit of a paycheck, fees associated with a minimum balance requirement may no longer be waived.[36]

Table 8 presents person-level estimates of job loss on the probability of being unbanked. First, to facilitate comparison with the estimates in Table 7, in panels (i) and (ii) we split the sample into persons residing in households with one employed adult or two or more employed adults, respectively. While the point estimates of the person-level effects of job loss on bank account ownership are generally larger than the household-level estimates presented in Table 7, they are of similar magnitudes when expressed as a percentage of the sample mean unbanked rates.

Panel (iii) of Table 8 shows how job loss effects on person-level unbanked rates differ depending on who in the household suffers a job loss. Ex-ante, we expect that a person's likelihood of having a bank account should be more affected by that person suffering a job loss compared to a job loss suffered by another adult in the household.[37] Our results are consistent

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[35] Further, as a practical matter, most of the information collected in the FDIC supplements to the CPS is at household-level. The exception to this is bank account ownership, which is collected both at household- and person-level for years 2011 through 2019.

[36] "One of the easiest ways to circumvent checking account fees is to have your paycheck, pension or Social Security benefit electronically deposited into your account. Many banks require a minimum monthly amount in "qualifying" direct deposits from an employer, corporation, government entity or retirement benefits administrator to waive monthly account fees." Libby Wells, '8 ways to avoid monthly checking fees', Bankrate.com, Sept. 20, 2021.

[37] It's also plausible that "own" and "other" job loss effects would be similar in magnitude, to the extent that resources are shared within households, and if bank account fee structures (e.g. minimum balance requirements,

with this hypothesis. For example, the estimates in column 1 indicate that an "own" job loss increases the likelihood of a person being unbanked by 18.6 pp, compared to 11.5 pp for "other" job loss.

The last analyses presented in this section examine the dynamics of the job loss effect. Doing so is a challenge because of data limitations. While we observe work history in the months leading up and subsequent to the FDIC survey, we only observe unbanked status in the month the FDIC survey is administered. Instead, we take advantage of households' self-reported duration of being unbanked and differences in households' timing of job loss relative to the FDIC survey to shed light on how quickly job loss may lead to households' becoming unbanked.

Columns 1 and 2 of Table 9 present estimation results from alternative specifications in which the dependent variable conditions on whether the household is "recently" or is "longer-term" unbanked, respectively. Specifically, a household is categorized as recently unbanked if it became unbanked within 12 months of the FDIC survey, and as longer-term unbanked if it became unbanked more than 12 months prior (or never had an account).[38]

The specification in panel (i) is analogous to our main Remain Employed control group specification (from Table 2, panel A).[39] For this specification, because the job loss "treatment" occurs within a few months of the FDIC survey, we expect the job loss effects to be concentrated in the recently unbanked group, and the effects on being longer-term unbanked to be zero. Consistent with this intuition, the estimate in column 1 indicates job loss has a statistically significant effect on the likelihood of being recently unbanked, and the magnitude is quite large -- the point estimate is 256% of the sample mean value. However, the estimate in column 2 is counter-intuitive, indicating that job loss has a positive and statistically significant effect on likelihood of being longer-term unbanked. While the magnitude of the estimated effect in column 2 is smaller than the corresponding estimate in column 1 when expressed relative to the sample means, this result is nonetheless puzzling. One possible explanation is that job loss might

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waivers for direct deposit) do not have a meaningful impact on bank account ownership in practice. However, a finding that "other" job loss effects are larger than "own" job loss effects would be counter-intuitive.

[38] We code households with unknown duration of being unbanked as a zero; results are robust to dropping such households. Results are also similar if we drop from the analysis all households that report they never had a bank account. About two-thirds of longer-term unbanked households report they never had a bank account.

[39] Results from the main Subsequent Job Loss specification are qualitatively similar to the Remain Employed results in panel (i). We don't include these results in Table 9 both for brevity and because we cannot employ a comparable identification strategy for the "persistence" specification in panel (ii).

inhibit transitions of longer-term unbanked households into the banking system; some of these households might have opened a new account had they not suffered an employment shock.[40]

Table 9, panel (ii) presents results using the Persistence specification (analogous to Table 4). In this case the job loss "treatment" occurs at least a year prior to the month of the FDIC survey, so the job loss effects should be concentrated in the longer-term unbanked group. As expected, the estimate in column 2 indicates that a job loss in the previous year increases the likelihood of being longer-term unbanked by 5.3 pp, significant at the 10 percent level. In contrast, the estimated effect on being recently unbanked in column 1 is a precisely estimated zero.

Finally, we examine how effects vary across households based on the timing of their job loss relative to the FDIC survey. As illustrated in Figure 3 the Job Loss treatment group suffers the job loss either two, one, or zero months prior to the FDIC survey, depending on whether they are in rotation group 4, 3, or 2 respectively. To the extent that households proactively close their accounts soon after suffering a job loss then we might expect the effect to occur nearly contemporaneously with the incidence of job loss, and the magnitudes of effects to be similar across rotation groups. Alternatively, if households don't close their accounts until after drawing down their accounts and potentially being charged fees for falling below minimum balance requirements or overdrafts (or if banks are involuntary closing accounts for these reasons) then there may be some delay before account closure. In this case we'd expect effects to be largest for rotation group 4 (job loss in April) and decreasing in subsequent rotation groups.

Figure 4 plots estimates of the job loss effect by rotation group, for the Remain Employed and Subsequent Job Loss control groups in panels A and B, respectively. The figures are generated from OLS estimates of specifications that include a full interaction of the indicator for job loss with the categorical variable for rotation group. To provide another point of comparison in the figure, we add to the sample households in rotation group 1 who otherwise meet the sample requirements.[41] For rotation group 1 households in the Job Loss treatment group, the job

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[40] FDIC (2018) estimates that about 3.9 percent of U.S. households were "recently banked" in 2017, meaning they had an account at the time of the FDIC survey but did not at some point in the 12 months prior.

[41] Specifically, we apply all the sample filters used for the main analysis sample, including the requirement that at least one adult is employed in the first month-in-sample (which for rotation group 1 is the month of the FDIC survey).

loss occurs in the month following the FDIC survey; if job losses are largely unexpected, we the job loss effect should be close to zero for these households.[42]

Consistent with this intuition, Figure 4 shows that the point estimates of the job loss effect are close to zero for households in rotation group 1 for both the Remain Employed and Subsequent Job Loss control groups. We find larger point estimates in line with our main estimates for each of the rotation groups 2 through 4, without a clear time trend. While some of these rotation group estimates are statistically significant from zero, they are not (alone or in combination) statistically different from the "effect" for rotation group 1. While the point estimates are fairly similar across rotation groups 2 through 4 which suggests the effects occur fairly soon after job loss, the imprecision of the estimates inhibits drawing any strong conclusions.

4.4. Identification and potential omitted variable bias

As discussed above, Table 2 and Appendix Table A2 show that our Job Loss treatment group differs from our preferred Subsequent Job Loss control group along certain observable dimensions, including race/ethnicity, marital status, and FDIC survey year. These differences are surprising given the similarities in how the treatment and control groups are constructed.

One potential explanation is uneven attrition of households from the control sample. As detailed in the Data section, when selecting our analysis sample we filter out households with any persons that attrite in the first four months-in-sample (MIS), but don't filter out households based on subsequent attrition. Because attrition is potentially non-random, differences in observable characteristics could arise because our Job Loss treatment group includes some households that attrite after the fourth month-in-sample (MIS), while by construction households in the Subsequent Job Loss control group must remain in sample through at least the sixth MIS.[43]

To assess the potential impact of such attrition on our results, we re-estimate the model on a "No Attrition" sample where we filter out households with any adults that attrite in any of the eight MIS. However, as shown in panel A of Appendix Table A4, differences in observable characteristics across treatment and control groups of the No Attrition sample are similar to those

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[42] Alternatively, if job losses are anticipated by some households, we might see positive effects of job loss on unbanked status in the month of the FDIC survey, even though the job loss occurs subsequently to the FDIC survey.
[43] Rivera Drew, Flood, and Warren (2014) discuss patterns in attrition across monthly waves of the CPS, noting that attrition is especially prevalent between the fourth and fifth MIS.

from our main analysis sample (Table 1), suggesting that these differences aren't driven by attrition. In any case, estimating equation (1) on the No Attrition sample results in job loss effects on unbanked status (Table 10, panel (i)) that are qualitatively similar to our main results (Table 2).[44]

Another potential explanation for observable differences between our treatment and Subsequent Job Loss control group samples is that, for observations from survey year 2019, the control group's job losses occurred in April, May, or June 2020, during the COVID-19 pandemic. If the COVID-19 recession affected a broader, less "negatively selected" set of households, including these households in the control group may introduce upward bias in our estimated effects of job loss for the treatment sample. To assess this concern, we dropped year 2019 observations from our sample and re-estimated the model. Panel B of Appendix Table A4 shows that dropping year 2019 observations does not substantially reduce observable differences between the treatment and control groups.[45] And as shown in panel (ii) of Table 10, estimated job loss effects are quite similar to our main results in Table 2, mitigating selection concerns associated with the COVID-19 recession.

A third possible explanation for the differences in observable characteristics between the Job Loss treatment and Subsequent Job Loss control samples is selection related to the duration in months before the first observed job loss. Recall that to establish a set of households with similar characteristics at the start of our longitudinal analysis (e.g., income earning households), all households in our sample have at least one employed adult in the first month-in-sample. Thus, treatment households suffer a job loss in the second month while the Subsequent Job Loss control households have a job loss in month six. If duration until job loss is related to unobservable characteristics (e.g., "chronic" job losers) that also affect demand for banking services, our estimates for the Subsequent Job Loss analysis may be biased. However, as shown in Table 10 panel (iii), our results are robust to removing the sample selection requirement that at least one adult is employed in the first MIS, thereby removing the difference in duration to unemployment between the treatment and control samples. Further, very few households in our

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[44] That our estimates are not very sensitive to attrition is consistent with the findings of Neumark and Kawaguchi (2004), who mimic the CPS sample design using the SIPP and conclude that the economic magnitude of bias induced by attrition, at least for some applications, is likely to be small.

[45] One exception is that, after dropping survey year 2019 observations, the distribution of remaining observations by year is much more similar across the treatment and control groups.

sample experience more than one job loss over our (short) period of observation, suggesting that at least over this period the specific timing of job loss is not related to unobserved household characteristics.

These results indicate that in practice, selection on attrition, COVID-19-related job loss, or duration until first observed job loss does not materially affect our estimates. Because we aren't aware of other plausible explanations, we believe the differences in observable characteristics between the Job Loss and Subsequent Job Loss groups are likely attributable to random sampling error. This is especially plausible considering the small sample sizes of these groups.

As a further check to quantify the scope of potential omitted variable bias, we use the methodology proposed by Oster (2019) to produce bounds on the estimated treatment effects of job loss presented in Table 2. An important assumption underlying this analysis is the extent to which selection on observed variables is informative about selection on unobserved variables.[46] Results from this analysis (detailed in the Appendix) indicate that, under the assumption suggested by Oster (2019) that selection on unobservables is equal to selection on (carefully chosen) observables, the point estimate of the job loss effect on unbanked status is 0.110 in the Remain Employed specification and 0.171 in the Subsequent Job Loss specification. These point estimates are only about 12 percent and 3 percent smaller in magnitude than the estimates in Table 2, respectively. That these bounded treatment effect estimates are very close to the main estimate suggests that the scope of omitted variable bias is minimal. This is particularly true for the Subsequent Job Loss specification, and is consistent with our intuition that the treatment and control groups are likely similar in terms of unobservable characteristics. Both groups experience job loss; the main difference between the groups is just the timing of the job loss.

4.5. Robustness

As additional robustness checks on our main estimation results in Table 2, we re-estimated the models using alternative definitions of job loss, including entry into unemployment for reasons especially likely to be involuntary, or entry into non-employment, regardless of reason (unemployed or out of the labor force). We find a similar pattern of results using those

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[46] Another important assumption is the value of "R_{max}", the R-squared from a hypothetical regression of the outcome on treatment, observed controls, and unobserved controls. As described in the Appendix, we follow the suggestion from Oster (2019), to set R_{max} equal to 1.3 * the R-squared from the regression specification with the treatment and control variables.

alternative definitions (see Appendix Table A5).[47] We also re-estimated the main specifications using probit; estimated marginal effects of job loss (in Appendix Table A6) are quite similar to the coefficient estimates using the linear probability models.[48]

A remaining question we do not explore in this analysis is whether employment effects are symmetric, i.e., whether becoming employed increases a households' likelihood of having a bank account. One challenge with such an analysis is that the unbanked population is much smaller, so it is difficult to make inferences due to small sample sizes.

Taken together, the results presented in sections 4.4 and 4.5 show the main finding of the paper, that job loss leads to a large increase in the likelihood of being unbanked, is generally robust to a variety of sample selection criteria and empirical specifications. That these job loss effects are broad based within the sample and impact alternative outcomes in ways consistent with the pecuniary mechanism by which job loss should affect unbanked status further mitigates concern of bias in our estimator.

5. DISCUSSION

Expanding households' access to the banking system has long been a priority of policymakers in the U.S. and abroad. Understanding the factors that affect households' likelihood of opening and maintaining a bank account can help inform these efforts. This paper contributes to that understanding by estimating plausibly causal effects of a specific type of economic shock that has been posited to affect bank account ownership: job loss.

We analyze data from biennial FDIC-sponsored supplements to the CPS from 2011 to 2019, linked to information on households' work history from the basic monthly CPS in the months leading up to and subsequent to the FDIC survey. In our preferred specification, we identify the

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[47] In the CPS, unemployment can be attributed to "Job loser/ on layoff", "Other job loser", or "Temporary job ended", which we term as involuntary, or else to "Job leaver", "Re-entrant", or "New-entrant". We focus on unemployment for any reason, the definition used for the results presented in Table 2. This definition specifies that at minimum the individual is attempting to find new employment (as opposed to those out of the labor force) and therefore is more likely to have been financially affected, while at the same time not further reducing the sample size or being overly reliant on self-reported reasons for unemployment (as is the case with our "involuntary" unemployment definition).

[48] One concern with a linear probability model (relative to probit or logit) is that predicted effects may stray from the zero to one range implied by the binary outcome. This potential issue is most pronounced in cases where the sample mean is especially close to zero or one, which is not the case for our Remain Employed and Subsequent Job Loss samples (means of 0.158 and 0.241, respectively). For our main specifications, no predicted outcomes are greater than one and 9.4 and 11.3 percent (for the A and B samples respectively) fall below zero.

job loss effect by estimating the likelihood of bank account ownership for a treatment group of households that become unemployed in the months leading up to the FDIC survey, compared to a control group of households that become unemployed one year later. By comparing households that experience a similar unemployment shock but that differ by the timing of the shock, the analysis mitigates concerns of potential selection bias in unobserved household characteristics associated with both job loss and bank account ownership status. We focus on low-income, renter households, a population likely to have little savings and more likely to be on the margin of having a bank account.

Our main result is that job loss increases a household's likelihood of being unbanked, and the magnitude of this job loss effect is large – about 75 percent of the mean unbanked rate in the sample. Effects are largest among the lowest income households in our sample, but are otherwise similar across other segments of the population (e.g. by race/ethnicity) and over time. We further show that the effects of job loss on being unbanked may persist for at least a year, but only among households that are not subsequently reemployed over that period. Most households experience employment shocks that are more transitory; these households are no more likely to be unbanked in the subsequent year. We also find that job loss leads to increased use of prepaid cards and certain nonbank products including money orders and check cashing services, suggesting that households may be using these products to substitute for (at least some of) the transaction services that a bank account provides.

We acknowledge some challenges with our empirical analysis including the limited sample size and lack of longitudinal data on bank account ownership; these issues inhibit our ability to explore heterogeneity and preclude use of certain statistical techniques which might further establish the causal mechanism by which job loss affects unbanked status. We also note that households may be unbanked for reasons other than job loss, the focal mechanism of this paper. In particular, about half of unbanked households report that they have never had a bank account. The reasons that lead to such longer-term disengagement with the banking system may differ from the reasons why banked households become unbanked. Nonetheless, our focus on job loss suits the available data and our interpretation of unbanked outcomes as a transition, in principle, addresses a population more amenable to and experienced with banking.

Regarding the scope of our findings, the incidence of job loss is substantial for the at-risk population of lower-income renter households we analyze. Further, households may be subject to

other income or expense shocks (due to, e.g., changing work hours, health care expenses, loss of benefits, or other unexpected emergencies) which could similarly induce them to become unbanked.[49]

Our results have direct implications for policymakers and other stakeholders interested in helping households open and maintain federally-insured depository accounts. For example, while temporary job loss does not necessarily lead to persistent loss of a bank account, it does contribute to intermittent unbanked status and leads to use of nonbank financial transaction and credit services. This transition may worsen a household's financial circumstances at exactly the time when stability in accounts, payments, and credit access might be especially helpful in regaining a financial footing. Banks and other industry providers might alleviate such households' transitory exits from account ownership, for example, by waiving minimum balance requirements or allowing households to carry a negative account balance without incurring overdraft fees. Many banks demonstrated a willingness to do so during the onset of the COVID-19 pandemic.[50] However these efforts are sometimes ad-hoc and typically require consumers to contact their financial institution to request a waiver, which may reduce take-up due to lack of awareness or other constraints. Regulatory agencies and industry groups could consider more proactive ways to extend such help, such as leveraging technology to identify and contact consumers that may be experiencing financial hardship due to job loss (such as those applying for unemployment benefits) or other adverse financial shocks.

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[49] While the incidence of economic shocks may vary over time (e.g., with macroeconomic conditions), based on the subjective life events data in the 2013 FDIC survey, about 10 percent of all households and 17 percent of the lower-income renter households we analyze experienced a job loss within the previous year. For other types of shocks, FDIC (2014) shows that the incidence of significant income loss and significant increases in expenses were highest among households that became unbanked within the past year. And FDIC (2020) shows that unbanked rates are higher among households with monthly income that varies "somewhat" or "a lot" from month to month.

[50] For example, see the Bankrate.com article, "List of banks offering help to customers impacted by the coronavirus," May 14, 2020. https://www.bankrate.com/banking/coronavirus-list-of-banks-offering-help-to-customers-financial-hardship/

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Figure 1: Unbanked rate among U.S. households by year (percent)
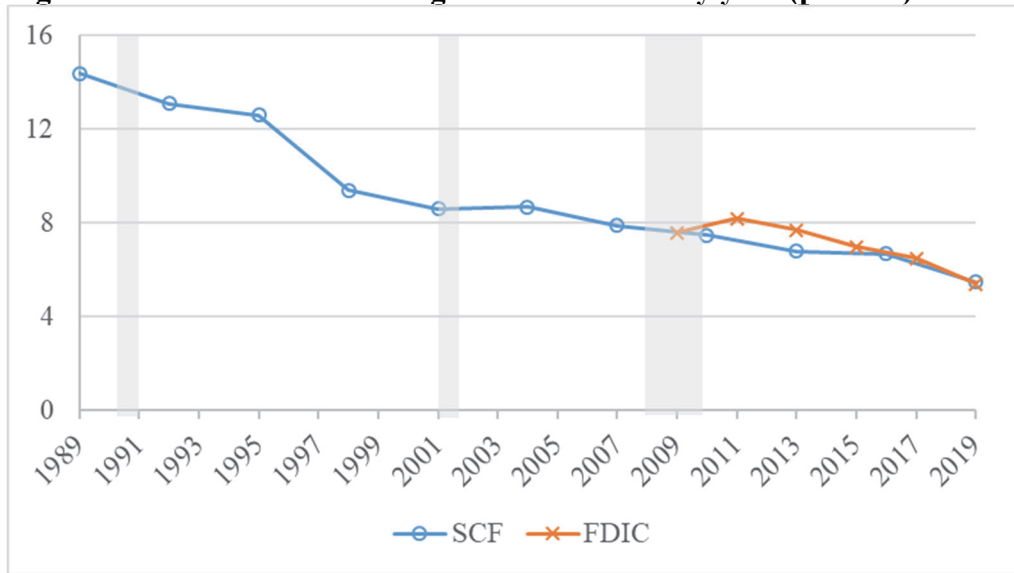
Notes: Authors' calculations using triennial data from the Survey of Consumer Finances (SCF) and biennial FDIC-sponsored supplements to the CPS (FDIC). In the SCF data, the unbanked rate is defined as the estimated proportion of households that do not have any transaction accounts, including checking, savings, and money market deposit accounts; money market mutual funds; and call or cash accounts at brokerages. The gray shaded areas indicate U.S. recessions as indicated by NBER (obtained from https://fred.stlouisfed.org/series/USRECD).

Figure 2: Sample rotation structure of the CPS in months near the June FDIC surveys

	Year before supplement (*t-1*)								FDIC supplement year (*t*)								Year after supplement (*t+1*)						
	Mar	Apr	May	Jun	Jul	Aug	Sep		Mar	Apr	May	**Jun**	Jul	Aug	Sep		Mar	Apr	May	Jun	Jul	Aug	Sep
								not observed Oct-Feb				1	2	3	4	*not observed Oct-Feb*				5	6	7	8
											1	2	3	4					5	6	7	8	
										1	2	3	4					5	6	7	8		
Months in Sample									1	2	3	4					5	6	7	8			
				1	2	3	4					5	6	7	8								
			1	2	3	4					5	6	7	8									
		1	2	3	4					5	6	7	8										
	1	2	3	4					5	6	7	8											

Notes: The figure illustrates the sample rotation structure of the Current Population Survey (CPS) in the months near the June FDIC surveys, for each year indexed as *t* of 2011, 2013, 2015, and 2019. The table indicates the number of Months in Sample (MIS) each rotation group has participated in the CPS as of that calendar month (up to a maximum of 8 months, split into two, four-month sequences a year apart). Blank cells indicate that the rotation group is not included in the CPS in that calendar month. The red rectangle indicates the rotation groups used in the main analyses presented in this paper.

Figure 3: Treatment vs control groups, main specification

Rot Grp	FDIC supplement year (t)							not observed Oct-Feb	Year after FDIC supplement (t+1)						
	Mar	Apr	May	**Jun**	Jul	Aug	Sep		Mar	Apr	May	Jun	Jul	Aug	Sep
Treatment group: "Job Loss"															
2			E	U	*	*					*	*	*	*	
3		E	U	*	*					*	*	*	*		
4	E	U	*	*					*	*	*	*			
Control group A: "Remain Employed"															
2			E	E	E	E					*	*	*	*	
3		E	E	E	E					*	*	*	*		
4	E	E	E	E					*	*	*	*			
Control group B: "Subsequent Job Loss"															
2			*	*	*	*					E	U	*	*	
3		*	*	*	*					E	U	*	*		
4	*	*	*	*					E	U	*	*			

Notes: This figure illustrates employment histories for the test and control groups for the main empirical specifications estimated in the paper. The values "E" (shaded green) and "U" (shaded red) indicate labor force status in the monthly CPS of employed and unemployed, respectively, while "*" cells (shaded gray) indicate that no selection criteria are applied to that rotation group/survey month. The red box indicates the month including the FDIC survey. To be included in the treatment group (Job Loss), at least one person in a household must satisfy the status "E" in the first month of observation (MIS=1) and "U" in the second month of observation (MIS=2). To be included in control group A (Remain Employed), all persons in the household who were employed in the first month of observation (MIS=1) must remain employed for the next three months (i.e. "E" in each month). To be included in control group B (Subsequent Job Loss), at least one person in a household must satisfy the status "E" in the fifth month of observation (MIS=5) and "U" in the sixth month of observation (MIS=6). All households must have at least one person employed in the first month of observation (MIS=1). Based on the individual job history criteria, the treatment and control group A households automatically satisfy the household employment requirement. For control group B, some individual in the household, not necessarily the individual experiencing job loss, must be employed in the first month of observation.

Figure 4: Effect of job loss by rotation group in survey month

A. Remain Employed control group
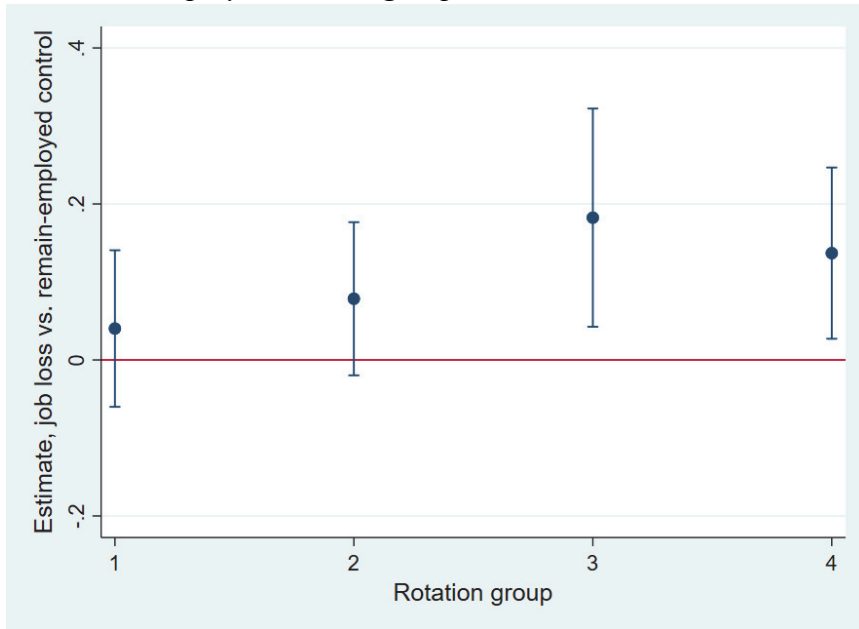
B. Subsequent Job Loss control group
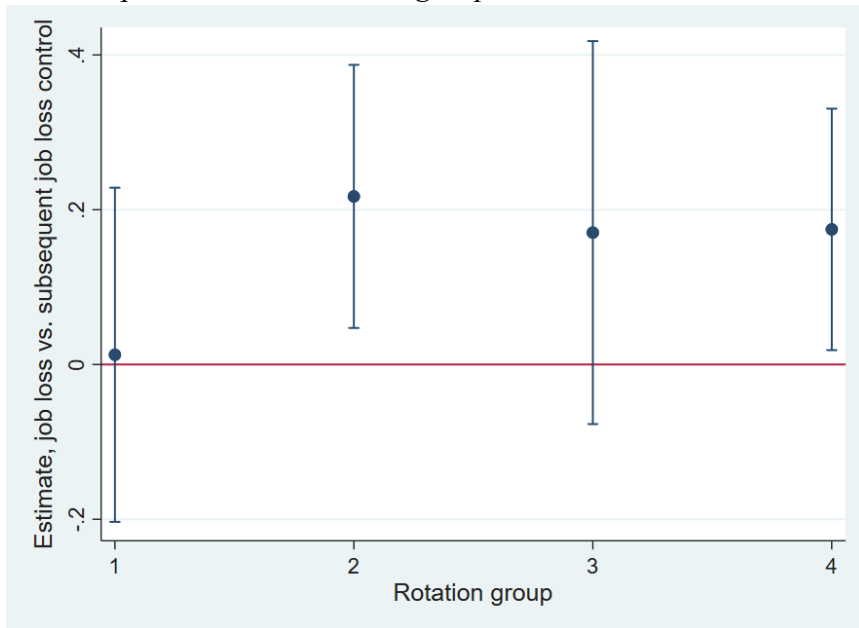
Notes: This figure plots estimates of the job loss effect by rotation group, generated from OLS estimates of specifications that include a full interaction of the indicator for job loss with the categorical variable for rotation group. The specifications also include the full set of control variables listed in Appendix Table A2. The figure plots 95% confidence intervals for each estimate. Panel A uses the Remain Employed control group and the sample includes all of the first four rotation groups, with a treatment sample size (for household job loss in month two of each rotation group) of 245, control sample size of 7,221, and total sample size of 7,466. Panel B uses the Subsequent Job Loss control group and the sample includes all of the first four rotation groups, with a treatment sample size (for household job loss in month two of each rotation group) of 245, control sample size of 123, and total sample size of 368.

Table 1: Sample means

Category	Variable	Treatment Job loss	Control A. Remain Employed	Control B. Subsequent Job Loss
Outcomes	Unbanked	0.324	0.153	0.097
	Unbanked, longer-term	0.263	0.134	0.097
	Unbanked, recently	0.056	0.014	0.000
	Prepaid card 12 mo	0.227	0.129	0.120
	Money order 12 mo	0.458	0.271	0.184
	Check cash 12 mo	0.274	0.134	0.097
	Pawn shop 12 mo	0.112	0.043	0.049
	Payday loan 12 mo	0.034	0.034	0.029
Demographics	Age 20 to 34	0.531	0.464	0.398
	Age 35 to 54	0.425	0.449	0.505
	Age 55 to 59	0.045	0.087	0.097
	Female	0.559	0.554	0.573
	White non-Hispanic	0.369	0.549	0.485
	Hispanic, any race	0.307	0.228	0.340
	Black non-Hispanic	0.251	0.153	0.097
	Other race non-Hisp.	0.073	0.070	0.078
	Native born	0.721	0.774	0.680
	Married	0.279	0.257	0.417
	Post secondary degree	0.240	0.268	0.291
Income and employment	Self employed	0.095	0.075	0.087
	Full time worker	0.480	0.690	0.660
	Industry: Ag. and mining or const.	0.128	0.096	0.136
	Industry: Manuf., utilities, or dist.	0.140	0.154	0.184
	Industry: Retail or services	0.536	0.513	0.427
	Industry: Educ., h'care, or gov't	0.196	0.237	0.252
	Income < $20k	0.480	0.344	0.437
	$20k <= Income < $40k	0.520	0.656	0.563
	Number of adults	1.922	1.690	1.816
	Number of employed adults	1.436	1.253	1.311
FDIC supplement info	Year 2011	0.285	0.249	0.107
	Year 2013	0.235	0.218	0.194
	Year 2015	0.179	0.187	0.058
	Year 2017	0.179	0.153	0.058
	Year 2019	0.123	0.194	0.583
	Rotation group 4	0.341	0.342	0.592
	Rotation group 3	0.263	0.340	0.184
	Rotation group 2	0.397	0.318	0.223
Number of observations		179	5409	103

Notes: Table presents sample means for the treatment and control groups from the main analyses presented in the paper.

Table 2: Effect of job loss on probability of being unbanked

Control group	A. Remain Employed		B. Subsequent Job Loss	
	(1)	(2)	(3)	(4)
1(Job loss)	0.169 **	0.125 **	0.187 **	0.177 **
	(0.035)	(0.034)	(0.055)	(0.055)
Effect as % of sample mean, dependent variable				
	107%	79%	78%	73%
Other controls	N	Y	N	Y
Mean, dependent variable	0.158	0.158	0.241	0.241
Number of observations	5,588	5,588	282	282

Notes: Each column presents selected OLS estimates from a different specification. For each specification, "1(Job loss)" is an indicator for treatment. The listed value is the point estimate of β from Equation 1, with the standard error in parentheses. In Panel A, the control group consists of households that are employed in their first month in sample (MIS=1) and remain employed through the month of the FDIC survey (June). In Panel B, the control group consists of households that are employed in their 1st and 5th MIS and become unemployed in their 6th MIS. All specifications include fixed effects for survey year and rotation group. Other controls include demographics, income and employment characteristics. See Appendix Table A2 for the OLS estimates associated with the control variables. The symbol ** indicates the estimate is significant at the 5% level (* = 10%).

Table 3: Heterogeneity in effects of job loss on probability of being unbanked

Control group	A. Remain Employed		B. Subsequent Job Loss	
	(1)	(2)	(3)	(4)
(i) By household Income (omitted: income < $20k)				
1(Job loss)	0.229 **	0.203 **	0.310 **	0.298 **
	(0.054)	(0.053)	(0.074)	(0.075)
1(.) X 1($20k <= Income < $40k)	-0.143 **	-0.148 **	-0.229 **	-0.226 **
	(0.069)	(0.067)	(0.094)	(0.094)
(ii) By race and ethnicity (omitted: White non-Hispanic)				
1(Job loss)	0.184 **	0.152 **	0.197 **	0.190 **
	(0.055)	(0.053)	(0.069)	(0.071)
1(.) X Hispanic	-0.107	-0.078	-0.138	-0.116
	(0.085)	(0.082)	(0.115)	(0.115)
1(.) X Black	0.000	0.015	0.204 **	0.211 **
	(0.093)	(0.088)	(0.103)	(0.103)
1(.) X Other Race	-0.134	-0.094	-0.114	-0.079
	(0.113)	(0.113)	(0.119)	(0.123)
(iii) By survey year (omitted: 2011)				
1(Job loss)	0.163 **	0.132 **	-0.056	-0.009
	(0.066)	(0.062)	(0.159)	(0.148)
1(.) X 2013	0.006	-0.008	0.347 *	0.259
	(0.100)	(0.095)	(0.183)	(0.179)
1(.) X 2015	0.050	0.029	0.399 **	0.309 *
	(0.109)	(0.106)	(0.183)	(0.187)
1(.) X 2017	-0.001	-0.027	0.225	0.227
	(0.106)	(0.096)	(0.240)	(0.239)
1(.) X 2019	-0.040	-0.041	0.190	0.175
	(0.111)	(0.108)	(0.195)	(0.184)
Other controls	N	Y	N	Y
Mean, dependent variable	0.158	0.158	0.241	0.241
Number of observations	5,588	5,588	282	282

Notes: Each column and panel (from (i) to (iv)) presents selected OLS estimates of a different empirical specification. In panel (i) the specifications include an interaction of the job loss indicator with a categorical measure of household income. In panel (ii) the interaction is with a categorical indicator of race/ethnicity. In panel (iii) the interaction is with the year of the FDIC survey. Interacted variables are also included on their own (not reported in the table). See Appendix Table A2 for control variables.

Table 4: Persistence of the job loss effect on probability of being unbanked

Control group	Remain Employed in Previous Year			
	(1)	(2)	(3)	(4)
1(job loss previous year)	0.103 **	0.059 *		
	(0.033)	(0.032)		
1(job loss previous year, not re-employed this year)			0.249 **	0.194 **
			(0.060)	(0.057)
1(job loss previous year, re-employed this year)			0.005	-0.031
			(0.036)	(0.036)
Other controls	N	Y	N	Y
Mean, dependent variable	0.141	0.141	0.141	0.141
Number of observations	5,145	5,145	5,145	5,145

Notes: Each column presents selected OLS estimates of a different empirical specification. All specifications are estimated using the "persistence" sample as illustrated in Appendix Figure A5. In columns 3 and 4 the treatment group ("job loss previous year" is split into two mutually exclusive categories (aside from those missing due to attrition), based on whether the adult with the job loss was employed in the first month of the year that the household participates in the FDIC survey (i.e., in MIS=5). See Appendix Table A2 for control variables.

Table 5: Effects of job loss on use of other financial products and services

Dependent variable	Prepaid card	Nonbank money order	Nonbank check cashing	Pawn Shop Loan	Payday Loan
	(1)	(2)	(3)	(4)	(5)
A. Control group: Remain Employed					
1(Job loss)	0.086 **	0.139 **	0.115 **	0.064 **	-0.002
	(0.037)	(0.037)	(0.033)	(0.024)	(0.014)
Effect as % of sample mean	65%	50%	83%	142%	-6%
Sample mean, dependent variable	0.132	0.277	0.139	0.045	0.034
Number of observations	4,192	5,588	5,588	5,588	5,588
B. Control group: Subsequent Job Loss					
1(Job loss)	0.057	0.163 **	0.095 *	0.061	0.005
	(0.057)	(0.065)	(0.051)	(0.045)	(0.026)
Effect as % of sample mean	31%	46%	45%	69%	15%
Sample mean, dependent variable	0.182	0.358	0.209	0.089	0.032
Number of observations	220	282	282	282	282

Notes: Each column and panel presents OLS estimates of a different empirical specification. The dependent variable of each specification is indicated at the top of each column, and indicates whether the household used the product or service within 12 months of the month of the FDIC survey. Use of prepaid cards is not observed in the 2011 FDIC survey, so data from 2011 are omitted for this regression. All specifications include the full set of control variables. See Table 2 for additional notes.

Table 6: Effects of job loss on probability of being unbanked, by reasons for being unbanked

Control group	A. Remain Employed (1)	B. Subsequent Job Loss (2)
(i) Unbanked, main reason is Financial		
1(Job loss)	0.098 **	0.087 **
	(0.029)	(0.043)
Effect as % of sample mean	133%	63%
Sample mean, dependent variable	0.074	0.138
Number of observations	5,588	282
(ii) Unbanked, main reason is Non-financial		
1(Job loss)	0.027	0.091 **
	(0.025)	(0.043)
Effect as % of sample mean	32%	88%
Sample mean, dependent variable	0.084	0.103
Number of observations	5,588	282

Notes: Each column and panel presents selected OLS estimates from a different empirical specification. See text for definitions of the outcome variables. All specifications include the full set of control variables.

Table 7: Effects of job loss on probability of being unbanked, by number of employed adults in the household

Control group	A. Remain Employed (1)	B. Subsequent Job Loss (2)
(i) One employed adult in household		
1(Job loss)	0.138 **	0.190 **
	(0.042)	(0.064)
Effect as % of sample mean	83%	74%
Sample mean, dependent variable	0.167	0.258
Number of observations	4,305	194
(ii) 2 or more employed adults in household		
1(Job loss)	0.093 *	0.159
	(0.055)	(0.132)
Effect as % of sample mean	72%	77%
Sample mean, dependent variable	0.128	0.205
Number of observations	1,283	88

Notes: Each column and panel presents selected OLS estimates from a different empirical specification. In panels (i) and (ii) the sample is stratified by the number of employed adults in the household. All specifications include the full set of control variables. See Table 2 for additional notes.

Table 8: Person-level effects of job loss on probability of being unbanked

Control group	A. Remain Employed	B. Subsequent Job Loss
	(1)	(2)
(i) One employed adult in household		
1(Job loss - self)	0.131 **	0.176 **
	(0.041)	(0.073)
Effect as % of sample mean	45%	50%
Sample mean, dependent variable	0.289	0.350
Number of observations	6,441	214
(ii) 2 or more employed adults in household		
1(Job loss - self)	0.181 **	0.325 **
	(0.057)	(0.100)
Effect as % of sample mean	63%	79%
Sample mean, dependent variable	0.290	0.411
Number of observations	3,345	107
(iii) 2 or more employed adults in household; include job-loss indicator for other adults in household		
1(Job loss - self)	0.186 **	0.322 **
	(0.057)	(0.101)
1(Job loss - other)	0.115 **	-0.186
	(0.051)	(0.230)
Effect of job loss - self as % of sample mean	64%	78%
Effect of job loss - other as % of sample mean	40%	-45%
Sample mean, dependent variable	0.290	0.411
Number of observations	3,345	107

Notes: Each column and panel presents selected OLS estimates from a different empirical specification. Unit of analysis is person-level. The indicator "job loss – self" is equal to 1 if the person suffered a job loss, and 0 otherwise. The indicator "job loss – other" is equal to 1 if another adult in the household suffered a job loss, and equal to zero otherwise. All specifications include the full set of control variables, measured at the person level (as opposed to for a reference person in the household).

Table 9: Effects of job loss on probability of being unbanked, by recency of being unbanked

Dependent variable	Unbanked recently (1)	Unbanked, longer-term (2)
(i) Main specification		
1(Job loss)	0.038 **	0.087 **
	(0.017)	(0.032)
Effect as % of sample mean	256%	63%
Sample mean, dependent variable	0.015	0.138
Number of observations	5,588	5,588
(ii) Persistence specification		
1(job loss previous year)	-0.005	0.053 *
	(0.009)	(0.031)
Effect as % of sample mean	-41%	44%
Sample mean, dependent variable	0.013	0.123
Number of observations	5,145	5,145

Notes: Each column and panel presents selected OLS estimates from a different empirical specification where the control group is "Remain employed." See text for definitions of the outcome variables. All specifications include the full set of control variables.

Table 10: Effects of job loss on probability of being unbanked, alternative samples

Control Group	A. Remain Employed	B. Subsequent Job Loss
(i) No Attrition		
1(Job loss)	0.093 *	0.119 *
	(0.048)	(0.062)
Effect as % of sample mean	67%	70%
Sample mean, dependent variable	0.140	0.171
Number of observations	3,086	170
(ii) Drop Survey Year 2019		
1(Job loss)	0.128 **	0.203 **
	(0.036)	(0.074)
Effect as % of sample mean	76%	69%
Sample mean, dependent variable	0.170	0.295
Number of observations	4,517	200
(iii) Include HHs not employed in first month-in-sample		
1(Job loss)		0.168 **
		(0.052)
Effect as % of sample mean		68%
Sample mean, dependent variable		0.248
Number of observations		306

Notes: Each column and panel presents selected OLS estimates from a different empirical specification. All specifications include the full set of control variables. In panel C the alternative analysis sample is relevant only for the Subsequent Job Loss control group. See text for descriptions of the alternative analysis samples. Sample means for the alternative analysis samples are presented in Appendix Table A4.

APPENDIX

Following Oster (2019), we provided further evidence of robustness to omitted variable bias under the assumption that the relationship between treatment and unobservables can be recovered from the relationship between the treatment and observables. For this analysis, we make use of our estimates for the specifications in Table 2, both with and without controls. Specifically, we use the coefficient estimates for β in each specification as well as the R-squared term from the model with controls.

The Oster methodology makes use of the change in coefficient value after controls are added as well as assumptions for the R-squared from a hypothetical regression of the outcome on treatment and both the observed and unobserved controls, termed R_{max}. As we discuss in Section 4.2, the coefficient estimates do not attenuate drastically once controls are added, especially for specifications using the Subsequent Job Loss control group. For a maximum R-squared, Oster suggests using $R_{max} = \Pi R^2$, where R^2 is from the specification with observed controls and $\Pi = 1.3$, based on an assumption that 90 percent of analyses using a randomized treatment should survive (Oster 2019). The final component of the bounding calculation is an assumption for the relative degree of selection on observed and unobserved variables, termed δ. The Oster (2019) analysis is not specifically designed for models with a binary outcome. However, we find that the magnitude of our estimates is not sensitive to using a probit model, rather than a linear probability model, so we expect that this aspect of model choice is of lesser importance for interpreting our results (see section 4.5 and Appendix Table A6). For further intuition, see Oster (2019), where the restricted estimator is defined on p. 192.

In Appendix Table A7 panels (i), (ii), and (iii), we provide the uncontrolled and controlled inputs to the calculations as well as the bounding estimates for our treatment effect estimates under a range of reasonable assumptions for δ (which cannot be known).[1] A typical assumption, recommended in Oster (2019), is $\delta = 1$, where carefully-chosen observables are assumed to be equally informative as unobservables. We also include bounding estimates for $\delta = 0.5$, where observables are twice as informative, and $\delta = 2$, where unobservables are twice as informative. In panel A, for the Remain Employed control, we show that the treatment effect estimated as

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[1] We use the Stata module `psacalc` (Oster 2013), with parameter inputs for δ and R_{max} as specified in Table 11 and with the survey year and rotation group indicators entered as unrelated controls to be included in all regressions (similar to our approach in Table 2, where all specifications include survey year and rotation group indicators).

0.125 in Table 2, would be bounded at 0.118, 0.110, and 0.095 under the least, middle, and most conservative scenarios. The estimate would go to zero at $\delta = 7.4$, which is quite large. There is even less attenuation for the estimates in panel B, for the Subsequent Job Loss control. The estimate of 0.177 from Table 2 would be bounded at 0.174, 0.171, and 0.165 under three scenarios and would only go to zero at $\delta = 9.6$. The lesser movement of bounding estimates in panel B can be explained by the more modest attenuation of coefficient estimates once controls are added and to the higher R^2 in the controlled regression (relative to panel A). While estimates for both controls survive this bounding analysis of omitted variable bias, the apparently less sensitive results from the Subsequent Job Loss estimates are perhaps indicative of the greater similarity of the treatment group to that control, especially in terms of factors that cannot be observed.

APPENDIX REFERENCES

Oster, Emily. 2013. "PSACALC: Stata Module to Calculate Treatment Effects and Relative Degree of Selection Under Proportional Selection of Observables and Unobservables." *Statistical Software Components* S457677, Boston College Department of Economics, revised 18 Dec 2016.

Appendix Figure A1: Unbanked rates (percent) by selected household characteristics, 2019

Category	Subgroup	Value
Family Income ($1000s)	< 20	20.6
	20 - 40	7.3
	40 - 60	2.9
	>= 60	0.7
Tenure	Renter	11.8
	Homeowner	1.9
Education	No HS Diploma	21.4
	HS Diploma	8.1
	Some College	4.3
	College Degree	0.8
Race / Ethnicity	Black	13.8
	Hispanic	12.2
	Asian	1.7
	White	2.5
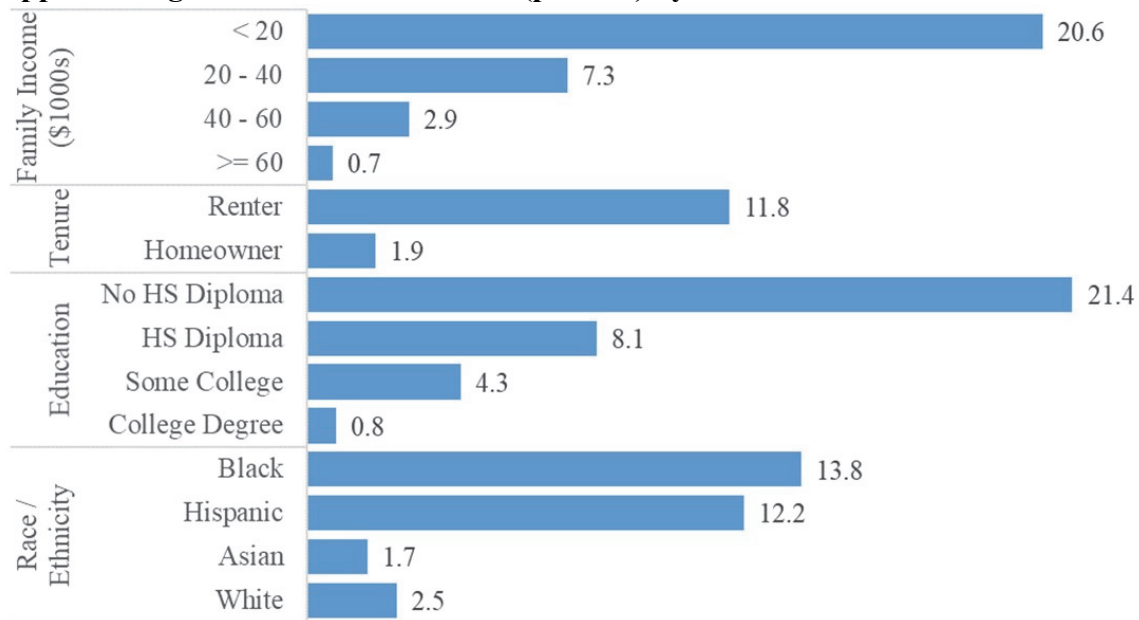
Notes: Authors' computations using 2019 FDIC survey and CPS; estimates are weighted using household supplement weights. See FDIC 2020 for more details.

Appendix Figure A2: Effect of job loss by household income
A. Remain Employed control group   *B. Subsequent Job Loss control group*
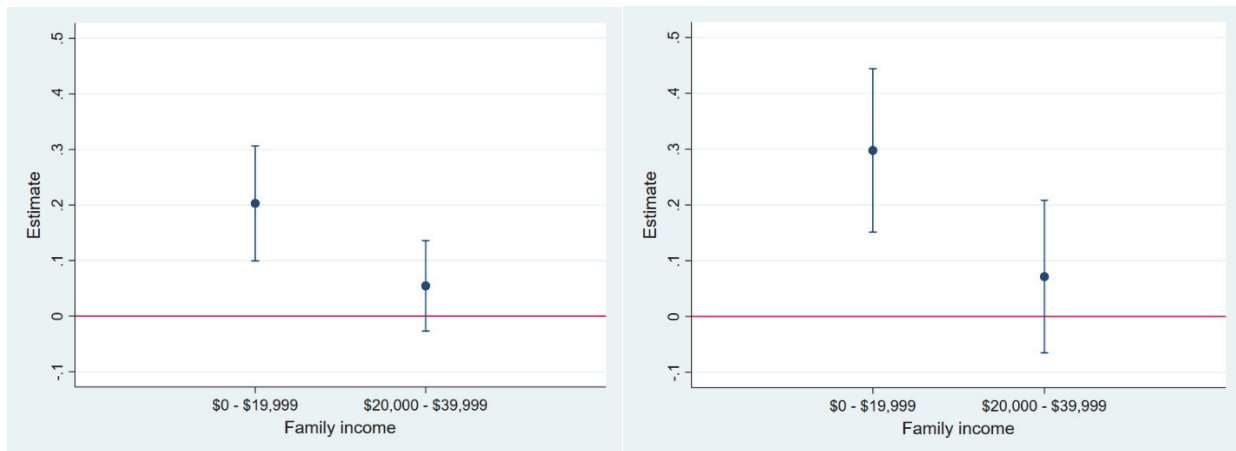
Notes: This figure plots estimates and 95% confidence intervals of the job loss effect by household income, generated from OLS estimates of a specification that includes a full interaction of the indicator for job loss with a categorical indicator for household income. See Table 3 panel (i) for the point estimates on the interacted terms. The specifications also include the full set of control variables listed in Appendix Table A2. Panel A uses the Remain Employed control group and Panel B uses the Subsequent Job Loss control group.

Appendix Figure A3: Effect of job loss by race and ethnicity

A. Remain Employed control group   *B. Subsequent Job Loss control group*
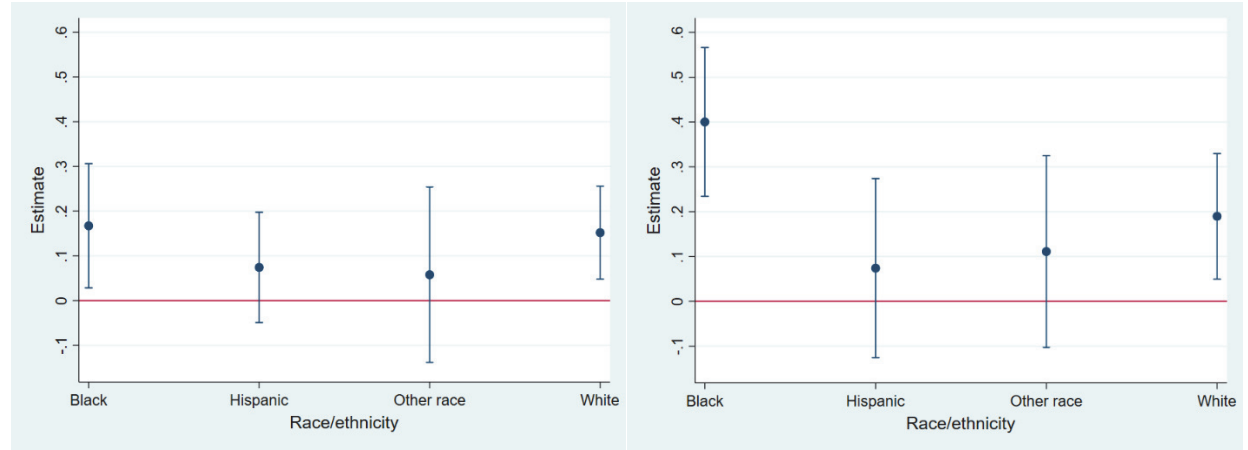
Notes: This figure plots estimates and 95% confidence intervals of the job loss effect by race and ethnicity, generated from OLS estimates of a specification that includes a full interaction of the indicator for job loss with a categorical indicator of race/ethnicity. See Table 3 panel (ii) for the point estimates on the interacted terms. The specifications also include the full set of control variables listed in Appendix Table A2. Panel A uses the Remain Employed control group and Panel B uses the Subsequent Job Loss control group.

Appendix Figure A4: Effect of job loss by survey year

A. Remain Employed control group   *B. Subsequent Job Loss control group*
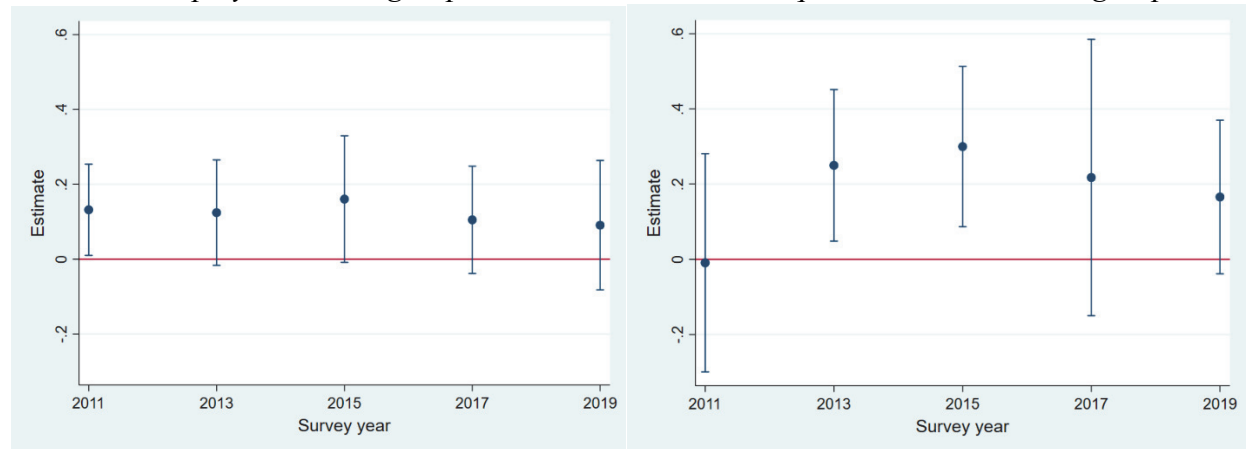
Notes: This figure plots estimates and 95% confidence intervals of the job loss effect by race and ethnicity, generated from OLS estimates of a specification that includes a full interaction of the indicator for job loss with a categorical indicator for FDIC survey year. See Table 3 panel (iii) for the point estimates on the interacted terms. The specifications also include the full set of control variables listed in Appendix Table A2. Panel A uses the Remain Employed control group and Panel B uses the Subsequent Job Loss control group.

Appendix Figure A5: Treatment vs control groups, "Persistence" specification

Rot Grp	Year before supplement (*t-1*)							not observed Oct-Feb	FDIC supplement year (*t*)			**Jun**			
	Mar	Apr	May	Jun	Jul	Aug	Sep		Mar	Apr	May	**Jun**	Jul	Aug	Sep
Treatment group: "Job Loss Previous Year"															
5				E	U	*	*					*	*	*	*
6			E	U	*	*					*	*	*	*	
7		E	U	*	*					*	*	*	*		
8	E	U	*	*					*	*	*	*			
Control group: "Remain Employed Previous Year"															
5				E	E	E	E					*	*	*	*
6			E	E	E	E					*	*	*	*	
7		E	E	E	E					*	*	*	*		
8	E	E	E	E					*	*	*	*			

Notes: This figure illustrates the test and control group for the Persistence specification estimated in the paper. The values "E" (shaded green) and "U" (shaded red) indicate labor force status in the monthly CPS of employed and unemployed, respectively, while "*" cells (shaded gray) indicate that no criteria are applicable for that rotation group/survey month. The red box indicates the month including the FDIC survey. To be included in the Treatment group (Job Loss Previous Year), at least one person in a household must satisfy the status "E" in the first month of observation (MIS=1) and "U" in the second month of observation (MIS=2). To be included in Control group (Remain Employed Previous Year), all persons in the household who were employed in the first month of observation (MIS=1) must remain employed for the next three months (i.e. "E" in each month). In contrast to Figure 3, which is based on three rotation groups (2, 3, and 4), the Persistence sample is based on four rotation groups (5, 6, 7, and 8).

Appendix Table A1: Description of variables used in the analysis

Category	Variable	Source	Fields and values	Description
Linking	Household	FDIC, CPS	hrhhid, hrhhid2	Household identifiers
	Person	CPS	cpsidp	Person identifier, pulineno used for linking to person-level BFS file
Outcomes	Unbanked	FDIC	hunbnk=="Unbanked", pes2a	Indicator for household not owning a bank account (pes2a is used to create a person-level level indicator of bank account ownership status)
	Unb. Always	FDIC	hbnkprevly	Indicator for always unbanked (zero otherwise)
	Unbanked, longer-term	FDIC	hbnkprevly	Indicator for long-term unbanked, less than a year (zero otherwise)
	Unbanked, recently	FDIC	hbnkprevly	Indicator for long-term unbanked, a year or more (zero otherwise)
	Prepaid card 12 mo	FDIC	use12pp	Indicator for using a prepaid card (zero otherwise and missing in 2011)
	Money order 12 mo	FDIC	use12mo	Indicator for using a money order (zero otherwise)
	Check cash 12 mo	FDIC	use12cc	Indicator for using check cashing (zero otherwise)
	Pawn shop 12 mo	FDIC	use12pw	Indicator for using a pawn shop loan (zero otherwise)
	Payday loan 12 mo	FDIC	use12pd	Indicator for using a payday loan (zero otherwise)
	Treated, job loss mo 2	CPS	empstat: 10-12	Household transition of employed (MIS=1) to not employed (MIS=2)
	Treated, unemp mo 2	CPS	empstat: 10-12, whyunemp: 1-6	Household transition of employed (MIS=1) to unemployed (MIS=2) (any reason)
	Treated, invol unemp mo2	CPS	empstat: 10-12, whyunemp: 1-3	Household transition of employed (MIS=1) to involuntary unemployed (MIS=2) (see footnote in main text)
	Reason unb. Financial	FDIC	hunbnkrm, hunbnkrmv2, hunbnkrmv3	Indicator for unbanked due to finance-related reasons (see footnote in main text)
	Reason unb. Other	FDIC	hunbnkrm, hunbnkrmv2, hunbnkrmv3	Indicator for unbanked due to all other reasons, as well as non-reponse regarding reasons (see footnote in main text)
	Labor force, mo 5	CPS	empstat: 10-12, whyunemp: 1-6	Indicator for employed or unemployed in MIS=5
	Employed, mo 5	CPS	empstat: 10-12	Indicator for employed in MIS=5

Appendix Table A1 [continued]

Category	Variable	Source	Fields and values	Description
Demo.	Age 20 to 34	CPS	age>=20 & age<35	Indicator, reference person, month 1
	Age 35 to 54	CPS	age>=35 & age<55	Indicator, reference person, month 1
	Age 55 to 60	CPS	age>=55 & age<60	Indicator, reference person, month 1
	Female	CPS	sex=2	Indicator, reference person, month 1
	White non-Hispanic	CPS	race==100 & hispan==0	Indicator, reference person, month 1
	Hispanic, any race	CPS	hispan>0	Indicator, reference person, month 1
	Black non-Hispanic	CPS	hispan==0 & race==200	Indicator, reference person, month 1
	Other race non-Hisp.	CPS	hispan==0 & race!=100 & race!=200	Indicator, reference person, month 1
	Native born	CPS	bpl=9900	Indicator, reference person, month 1
	Married	CPS	marst==1	Indicator, reference person, month 1
	Post secondary degree	CPS	educ>=91	Indicator, Associate's degree or higher, reference person, month 1
Inc. & emp.	Self employed	CPS	classwkr==13 | classwkr==14	Indicator, first person employed, month 1)
	Full time worker	CPS	wkstat==11	Indicator, first person employed, month 1)
	Second job	CPS	multjob==2	Indicator, first person employed, month 1)
	Industry: Ag. and mining or const.	CPS	ind: 0170-0490, 0770	Indicator, first person employed, month 1)
	Industry: Manuf., utilities, or dist.	CPS	ind: 0570-0690, 1070-3990, 4070-4590, 6070-6390	Indicator, first person employed, month 1)
	Industry: Retail or services	CPS	ind: 4670-5790, 6470-7070, 7080-7790, 8560-9290	Indicator, first person employed, month 1)
	Industry: Educ., h'care, or gov't	CPS	ind: 7860-8470, 9370-9890	Indicator, first person employed, month 1)
	Income < $20k	CPS	faminc>=100 & faminc<=500	Indicator, household, month 1
	$20k <= Income < $40k	CPS	faminc>=600 & faminc<=730	Indicator, household, month 1
	40,000 - 59,999	CPS	faminc>=740 & faminc<=820	Indicator, household, month 1
	60,000 - 99,999	CPS	faminc>=830 & faminc<=841	Indicator, household, month 1
	100,000 - 149,999	CPS	faminc==842	Indicator, household, month 1
	150,000 and over	CPS	faminc==843	Indicator, household, month 1
	Homeowners	CPS	hhtenure==1	Indicator, household, month 1
	Number of adults	CPS	pernum, age	Number persons age 20 to 59, month 1
	Number of employed adults	CPS	empstat	Number persons employed age 20 to 59, month 1
Survey	Year 2011	FDIC	hryear4	Indicator, FDIC supplement year
	Year 2013	FDIC	hryear4	Indicator, FDIC supplement year
	Year 2015	FDIC	hryear4	Indicator, FDIC supplement year
	Year 2017	FDIC	hryear4	Indicator, FDIC supplement year
	Year 2019	FDIC	hryear4	Indicator, FDIC supplement year
	Rotation group 4	CPS	mish==4	Indicator, supplement in month 4
	Rotation group 3	CPS	mish==3	Indicator, supplement in month 3
	Rotation group 2	CPS	mish==2	Indicator, supplement in month 2
	Rotation group 1	CPS	mish==1	Indicator, supplement in month 1

Note: FDIC refers to the FDIC supplement to the CPS; source is the multiyear data file (hh_multiyear_analys.csv, available at https://www.fdic.gov/analysis/household-survey/). CPS revers to the basic monthly files of the Current Population Survey. The source is IPUMS CPS (available at https://cps.ipums.org/cps/index.shtml).

Appendix Table A2: Selection into treatment

	Control group	
	A. Remain Employed	B. Subsequent Lob Loss
Age 20 to 34	0.004	0.052
	(0.005)	(0.054)
Age 55 to 59	-0.008	0.084
	(0.007)	(0.112)
Female	-0.001	-0.043
	(0.005)	(0.059)
Hispanic, any race	0.020**	0.041
	(0.008)	(0.075)
Black non-Hispanic	0.032***	0.166**
	(0.008)	(0.074)
Other race non-Hisp.	0.009	0.058
	(0.009)	(0.104)
Native born	0.003	0.033
	(0.008)	(0.076)
Married	-0.002	-0.105*
	(0.006)	(0.062)
Post secondary degree	0.003	-0.003
	(0.005)	(0.056)
Self employed	0.007	-0.038
	(0.011)	(0.094)
Full time worker	-0.027***	-0.093*
	(0.006)	(0.053)
Industry: Ag. and mining or const.	0.017	0.117
	(0.011)	(0.100)
Industry: Manuf., utilities, or dist.	0.007	0.122
	(0.007)	(0.091)
Industry: Retail or services	0.005	0.137**
	(0.006)	(0.066)
$20k <= Income < $40k	-0.014**	0.023
	(0.005)	(0.058)

Appendix Table A2 [continued]

	Control group	
	A. Remain Employed	B. Subsequent Lob Loss
Number of adults	0.004	0.004
	(0.004)	(0.040)
Number of employed adults	0.019***	0.07
	(0.007)	(0.049)
Year 2013	-0.005	-0.147*
	(0.007)	(0.076)
Year 2015	-0.006	-0.005
	(0.007)	(0.082)
Year 2017	0.001	0.039
	(0.008)	(0.075)
Year 2019	-0.012*	-0.483***
	(0.007)	(0.078)
Rotation group 4	-0.01	-0.105
	(0.006)	(0.069)
Rotation group 3	-0.015**	-0.022
	(0.006)	(0.068)
R-squared	0.02	0.321
Number of observations	5,588	282

Each column presents OLS estimates from a different specification, explaining the treatment variable in terms of control variables for each control group. The first column presents estimates explaining job loss in terms of control variables for the treatment group and control group A. The second column presents estimates explaining job loss in terms of control variables for the treatment group and control group B. See Table 1 for definitions of omitted categorical variables. The symbol *** indicates the estimate is significant at the 1% level (** = 5%; * = 10%).

Appendix Table A3: Full OLS Estimation Results, Main Specifications

	Control group	
	A. Remain Employed	B. Subsequent Job Loss
1(Job loss)	0.125***	0.177***
	(0.0337)	(0.0553)
Age 20 to 34	-0.0141	0.0135
	(0.0101)	(0.0534)
Age 55 to 59	-0.0455***	-0.0835
	(0.0155)	(0.0834)
Female	0.0145	0.0851
	(0.00947)	(0.0533)
Hispanic, any race	0.130***	0.179**
	(0.0145)	(0.0727)
Black non-Hispanic	0.143***	0.153**
	(0.0153)	(0.0736)
Other race non-Hisp.	0.00221	0.00307
	(0.0173)	(0.0835)
Native born	-0.00288	0.137*
	(0.0140)	(0.0696)
Married	-0.00346	-0.00368
	(0.0123)	(0.0539)
Post secondary degree	-0.0920***	-0.0941*
	(0.00881)	(0.0504)
Self employed	-0.0234	0.0586
	(0.0177)	(0.0914)
Full time worker	-0.00767	0.00830
	(0.0105)	(0.0513)
Industry: Ag. and mining or const.	0.147***	0.167*
	(0.0213)	(0.0968)
Industry: Manuf., utilities, or dist.	0.00737	0.0370
	(0.0148)	(0.0784)
Industry: Retail or services	0.0392***	0.0969
	(0.0108)	(0.0641)
$20k <= Income < $40k	-0.0859***	-0.123**
	(0.0109)	(0.0552)

Appendix Table A3 [continued]

	Control group	
	A. Remain Employed	B. Subsequent Job Loss
Number of adults	0.0297***	0.0871**
	(0.00819)	(0.0418)
Number of employed adults	-0.0456***	-0.0787
	(0.0111)	(0.0537)
Year 2013	0.0278**	-0.0416
	(0.0142)	(0.0806)
Year 2015	0.00712	-0.0299
	(0.0144)	(0.0945)
Year 2017	-0.00163	-0.0661
	(0.0149)	(0.0870)
Year 2019	-0.0300**	-0.0668
	(0.0133)	(0.0794)
Rotation group 4	0.00886	0.0857
	(0.0113)	(0.0595)
Rotation group 3	0.0170	0.115
	(0.0115)	(0.0719)
Constant	0.156***	-0.175
	(0.0262)	(0.144)
R-squared	0.108	0.210
Number of observations	5,588	282

Notes: This table presents OLS estimates on all control variables for the specifications in Table 2 of the paper. See Table 1 for definitions of omitted categorical variables. The symbol *** indicates the estimate is significant at the 1% level (** = 5%; * = 10%). See Table 2 for additional notes.

Appendix Table A4: Sample means of treatment and control groups, alternative samples

Variable	A. No Attrition Treatment Job Loss	A. No Attrition Control Remain Employed	A. No Attrition Control Subsequent Job Loss	B. Drop Survey Year 2019 Treatment Job Loss	B. Drop Survey Year 2019 Control Remain Employed	B. Drop Survey Year 2019 Control Subsequent Job Loss	C. Include HHs not employed in first MIS Job Loss	C. Include HHs not employed in first MIS Subsequent Job Loss
Unbanked	0.272	0.140	0.079	0.338	0.170	0.140	0.324	0.142
Unbanked, longer-term	0.198	0.125	0.079	0.268	0.148	0.140	0.263	0.134
Unbanked, recently	0.074	0.011	0.000	0.064	0.017	0.000	0.078	0.055
Prepaid card 12 mo	0.203	0.126	0.127	0.245	0.137	0.125	0.227	0.115
Money order 12 mo	0.469	0.272	0.213	0.497	0.298	0.233	0.458	0.236
Check cash 12 mo	0.198	0.129	0.101	0.299	0.148	0.140	0.274	0.118
Pawn shop 12 mo	0.086	0.037	0.045	0.115	0.050	0.070	0.112	0.071
Payday loan 12 mo	0.049	0.035	0.022	0.038	0.036	0.047	0.034	0.031
Age 20 to 34	0.444	0.387	0.360	0.554	0.484	0.419	0.531	0.449
Age 35 to 54	0.494	0.505	0.539	0.414	0.444	0.558	0.425	0.465
Age 55 to 59	0.062	0.109	0.101	0.032	0.072	0.023	0.045	0.087
Female	0.556	0.557	0.596	0.567	0.555	0.535	0.559	0.591
White non-Hispanic	0.370	0.547	0.506	0.350	0.533	0.512	0.369	0.496
Hispanic, any race	0.321	0.231	0.315	0.325	0.232	0.302	0.307	0.307
Black non-Hispanic	0.259	0.152	0.112	0.248	0.163	0.140	0.251	0.126
Other race non-Hisp.	0.049	0.070	0.067	0.076	0.072	0.047	0.073	0.071
Native born	0.704	0.758	0.708	0.713	0.767	0.698	0.721	0.701
Married	0.321	0.274	0.371	0.261	0.250	0.488	0.279	0.378
Post secondary degree	0.284	0.271	0.270	0.236	0.263	0.209	0.240	0.283
Self employed	0.099	0.089	0.079	0.102	0.070	0.140	0.095	0.071
Full time worker	0.506	0.692	0.640	0.452	0.675	0.651	0.480	0.535
Industry: Ag. and mining or const.	0.086	0.104	0.112	0.127	0.094	0.186	0.128	0.118
Industry: Manuf., utilities, or dist.	0.160	0.152	0.202	0.115	0.149	0.140	0.140	0.181
Industry: Retail or services	0.494	0.501	0.449	0.561	0.524	0.326	0.536	0.402
Industry: Educ., h'care, or gov't	0.259	0.244	0.236	0.197	0.233	0.349	0.196	0.228
Industry: Not observed	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.071
Income < $20k	0.481	0.340	0.427	0.490	0.370	0.558	0.480	0.528
$20k <= Income < $40k	0.519	0.660	0.573	0.510	0.630	0.442	0.520	0.472
Number of adults	1.889	1.617	1.753	1.936	1.700	2.047	1.922	1.787
Number of employed adults	1.370	1.219	1.292	1.452	1.258	1.302	1.436	1.063
Year 2011	0.272	0.250	0.112	0.325	0.309	0.256	0.285	0.181
Year 2013	0.198	0.216	0.191	0.268	0.270	0.465	0.235	0.181
Year 2015	0.160	0.189	0.067	0.204	0.231	0.140	0.179	0.102
Year 2017	0.222	0.153	0.067	0.204	0.190	0.140	0.179	0.047
Year 2019	0.148	0.192	0.562	0.000	0.000	0.000	0.123	0.488
Rotation group 4	0.333	0.339	0.562	0.338	0.343	0.279	0.341	0.543
Rotation group 3	0.247	0.348	0.191	0.274	0.338	0.233	0.263	0.213
Rotation group 2	0.420	0.313	0.247	0.389	0.319	0.488	0.397	0.244
Number of observations	81	3005	89	157	4517	43	179	127

Notes: Table presents sample means for alternative sample selection criteria. In Panel A the sample excludes households with any persons that attrite during any of the eight months-in-sample. In Panel B the sample excludes all observations from survey year 2019. In Panel C the Subsequent Job Loss control group does not exclude households with no employed adults in the first month-in-sample (MIS); the Job Loss treatment group is identical to the group used in the main analysis.

Appendix Table A5: Alternative definitions of job loss

Control group	A. Remain Employed		B. Subsequent Job Loss	
	(1)	(2)	(3)	(4)
(i) Main result, job loss to unemployment				
1(Job loss to unemp.)	0.169 **	0.125 **	0.187 **	0.177 **
	(0.035)	(0.034)	(0.055)	(0.055)
Effect as % of sample mean, dependent variable				
	107%	79%	78%	73%
Other controls	N	Y	N	Y
Mean, dependent variable	0.158	0.158	0.241	0.241
Number of observations	5,588	5,588	282	282
(ii) Alternative treatment, job loss to unemployment (involuntary)				
1(Job loss)	0.195 **	0.155 **	0.195 **	0.206 **
	(0.041)	(0.039)	(0.065)	(0.067)
Effect as % of sample mean, dependent variable				
	124%	99%	80%	84%
Other controls	N	Y	N	Y
Mean, dependent variable	0.157	0.157	0.246	0.246
Number of observations	5,543	5,543	228	228
(iii) Alternative treatment, job loss to nonemployment (unemployed or out of the labor force)				
1(Job loss)	0.147 **	0.118 **	0.119 **	0.110 **
	(0.023)	(0.022)	(0.037)	(0.037)
Effect as % of sample mean, dependent variable				
	90%	73%	48%	44%
Other controls	N	Y	N	Y
Mean, dependent variable	0.163	0.163	0.248	0.248
Number of observations	5,819	5,819	601	601

Notes: Each column presents selected OLS estimates from a different specification. See Table 2 for definitions of Panel A and B in terms of the timing of job loss. Panel (i) repeats the main result, where job loss is defined as entry into unemployment. Panel (ii) defined job loss as an entry into unemployment where the reason given indicates that unemployment is especially likely to be involuntary. Panel (iii) defines job loss as entry into nonemployment (unemployed or not in labor force). All specifications include fixed effects for FDIC survey year and rotation group. Other controls include demographics, income and employment characteristics. Panel (i) repeats results from Table 2, where job loss is defined. See Appendix Table 2 for the OLS estimates associated with the control variables. The symbol ** indicates the estimate is significant at the 5% level (* = 10%).

Appendix Table A6: Probit estimates of Effect of job loss on probability of being unbanked

Control group	A. Remain Employed		B. Subsequent Job Loss	
	(1)	(2)	(3)	(4)
1(Job loss)	0.559 **	0.442 **	0.708 **	0.795 **
	(0.100)	(0.106)	(0.221)	(0.243)
Marginal effect of Job Loss				
at Job loss=1	0.318	0.235	0.299	0.258
	(0.035)	(0.032)	(0.038)	(0.039)
at Job loss=0	0.151	0.122	0.108	0.074
	(0.005)	(0.005)	(0.033)	(0.028)
difference (1 vs 0)	0.167 **	0.113 **	0.191 **	0.183 **
	(0.035)	(0.032)	(0.054)	(0.050)
difference Chi^2 statistic	22.580	12.166	12.627	13.232
Other controls	N	Y	N	Y
Mean, dependent variable	0.158	0.158	0.241	0.241
Number of observations	5,588	5,588	282	282

Notes: Each column presents selected probit estimates from a different specification. For each specification, "1(Job loss)" is an indicator for treatment. The listed value is the point estimate of β from Equation 1, with the standard error in parentheses. In Panel A, the control group consists of households that are employed in their first month in sample (MIS=1) and remain employed through the June FDIC survey. In Panel B, the control group consists of households that are employed in their 1st and 5th MIS and become unemployed in their 6th MIS. All specifications include fixed effects for FDIC survey year and rotation group. Other controls include demographics, income and employment characteristics. The symbol ** indicates the estimate is significant at the 5% level (* = 10%).

Appendix Table A7: Bounding analysis of omitted variable bias (Oster 2019)

Control group	A. Remain Employed (1)	B. Subsequent Job Loss (2)
(i) Estimates without other controls		
1(Job loss)	0.169	0.187
	(0.035)	(0.055)
R-squared	0.013	0.088
(ii) Estimates including other controls		
1(Job loss)	0.125	0.177
	(0.034)	(0.055)
R-squared	0.108	0.210
(iii) Bounding estimates (Oster 2019)		
1(Job loss) | δ=0.5, R_{max}	0.118	0.174
1(Job loss) | δ=1.0, R_{max}	0.110	0.171
1(Job loss) | δ=2.0, R_{max}	0.095	0.165
R-squared max. (R_{max})	0.140	0.273
Mean, dependent variable	0.158	0.241
Number of observations	5,588	282

Notes: Each column and panel presents selected OLS estimates from a different empirical specification. See the Appendix for definitions as well as discussion of the Oster (2019) parameters.