Landlords as Lenders of Last Resort?
Late Housing Payments During Unemployment

Current Draft: February 2025

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Abstract

This paper examines the role that late housing payments play in helping households, especially renters, cope with job loss. Using a stylized model, I show that late payments can provide a source of informal credit that helps smooth consumption when facing shocks. I then empirically examine the prevalence and consequences of missed housing payments after job loss. There are three main results. First, missed housing payments are common after job loss. Second, the dollar value of these missed payments is large, providing substantial liquidity. Third, the large majority of missed payments do not lead to evictions or other forced moves.

Keywords: housing, renters, consumption smoothing, job loss, credit, eviction, unemployment
JEL D14, E21, E24, G51, J64

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[1] Address: Department of Economics, 3300 Dyer Street, Umphrey Lee Suite 301 Dallas, TX 75275. I thank Tom King, Dan Millimet, Erik Mayer, Eva Nagypal, Judith Ricks, Carly Urban, Stijn Van Nieuwerburgh, and participants at the 2020 Southern Economic Association Annual Meeting, the SMU Brownbag, the Junior HF Virtual Brownbag, the 2021 CFPB Research Conference, the 2024 WEAI Conference, and the 2024 Texas Stata Empirical Micro Conference.

1. Introduction

The welfare costs of job loss and the optimal policy responses depend on how easily households can smooth consumption by dissaving, replacing lost income, or reducing spending (Baily, 1978; Chetty, 2006). Housing is typically households' largest expenditure, so the ease with which households can adjust housing expenditure plays a critical role (Chetty and Szeidl, 2007). Moreover, this role is increasingly important as housing costs and the share of housing-cost-burdened households continue to rise (JCHS, 2024; Molloy, 2024).

There are two ways households can reduce housing expenditure when facing shocks, but research suggests both are difficult. First, households can move. Moving, however, generates large transaction costs. As a result, it is often optimal for households to remain in the same residence, even though this rigidity increases the short-term costs of moderate shocks (Chetty and Szeidl, 2007). Accordingly, households infrequently move after job displacement (Huttunen et al., 2018; Meekes and Hassink, 2019; Ransom, 2022). Second, households can stop paying their rent or mortgage. Nonpayment, however, can lead to eviction or foreclosure. Much existing research focuses on these negative outcomes. Evictions lead to homelessness, reduced earnings, and worse health (Desmond and Kimbro, 2015; Collinson et al., 2024). Foreclosure worsens health, housing conditions, and credit scores (Molloy and Shan, 2013; Currie and Tekin, 2015; Diamond et al., 2020; Guren and McQuade, 2020).

Despite these large potential costs, this paper shows that missing housing payments is a common and important consumption-smoothing mechanism, especially for renters. Examining instances of job loss in two datasets, I document several new facts: (i) households, especially renters, frequently miss housing payments after job loss, (ii) the dollar value of the missed payments is large, (iii) subsequent evictions are uncommon, and (iv) most households that miss payments continue living in the same residence (ruling out informal evictions). These facts contribute to a new understanding of the role of housing expenditure in households' response to shocks. First, although housing *consumption* remains fixed because moving is costly, the ability to miss payments makes housing *expenditure* quite flexible. Indeed, for job-losing renters, average housing and nondurable expenditures fall by the same percentage. Second, because missed payments are still legally owed, they constitute a form of informal credit borrowed from landlords and mortgage lenders. This informal credit is widely used with the amounts involved comparable to estimates of the use of formal unsecured borrowing by the unemployed. In some cases, this informal credit line can reverse the standard result that consumption commitments, such as housing, exacerbate the costs of moderate shocks (Chetty and Szeidl, 2007). Lastly, there are policy implications. The availability of informal credit can affect optimal unemployment insurance, similar to the effects of formal credit analyzed in Braxton et al. (2024). Additionally, because landlords weigh the costs of allowing missed payments against the costs of pursuing an eviction, the availability of informal credit itself may be a policy parameter that depends on the state and local rules governing evictions or foreclosures.

I begin with a simple conceptual framework to motivate the analysis. The framework adds a late payment option to the stylized consumption commitments model of Chetty and Szeidl (2007). Consumption commitments (e.g., housing, vehicle payments, utilities) are goods that are costly to adjust. As a result, households often keep commitments fixed when facing shock and so must concentrate any expenditure reductions on the subset of more flexible goods. Chetty and Szeidl (2007) shows that this inflexibility hinders consumption smoothing across goods and amplifies risk aversion and the welfare costs of moderate shocks. This paper, however, emphasizes that providers of consumption commitments, e.g., landlords and mortgage lenders, often tolerate late payments. Incorporating these late payments into the model, I highlight two points. First, late payments are an informal line of credit. Second, this line of credit can, in some cases, offset the standard result that commitments harm households' ability to smooth consumption. In particular, while commitments still inhibit consumption smoothing *across goods*, they can facilitate consumption smoothing *over time* by providing a line of credit. For this consumption smoothing benefit to be realized, it requires that late payments must be available, households must actually take-up the late payment option, and landlords or lenders must tolerate late payments (as opposed to immediately evicting). The main part of the paper empirically investigates these requirements by examining the take-up and consequences of late payments.

I use two survey datasets to examine housing payments after instances of job loss. The first is the Survey of Income and Program Participation (SIPP), which covers 1991-2014 and contains information on job loss and indicators for missed housing payments, evictions, and moves. The second is the RAND American Life Panel (ALP) Financial Crisis Surveys, a monthly and later quarterly panel covering 2008 through 2016. The ALP contains information on job loss and detailed information on monthly expenditures, including housing expenditure. These data allow me to examine the frequency, dollar amount, and consequences of missed payments around job loss. For this application, survey data have two advantages over the financial account data that is often used in recent research on consumption or expenditure. First, a significant share of those missing housing payments are unbanked or underbanked and so would be absent from most financial or bank account data. Second, through the 2010s (the period covered by this and other studies), the large majority of renters pay with paper-based methods (cash, check, or money order) which are missing or difficult to categorize in financial account data.

The identification challenge is that job loss may be endogenously related to whether households miss housing payments, leading to biased estimates of the causal effect of job loss on housing payments. I address this issue in several ways. In the SIPP analysis, I use two existing strategies – an instrumental variable strategy and restrictions on the set of job losses – that isolate plausibly exogenous job separations due to employer bankruptcies, sales of the business, or layoffs (Gerardi et al., 2018; East and Simon, 2024). The small sample size and limited information prevent me from applying these same strategies in the ALP. Instead, in the ALP, I rely on the high-frequency (monthly) observations to examine *within-household* changes immediately around instances of job loss. In both, I also conduct several other tests to address specific concerns (e.g., unobserved moves, attrition) and to assess the sensitivity to unobserved selection.

There are three main results. First, households, especially renters, frequently miss housing payments after job loss. Job loss leads to a 7.5 percentage point (pp) increase in the probability of missing payments, with larger effects for renters (9.3pp) and smaller effects for owners (6.4pp). Consistent with providing an informal source of liquidity, late payments are much more common among households with few liquid assets.

Second, the informal credit from missed payments provides significant liquidity and is repaid slowly (if at all). Upon job loss, housing expenditure falls by an average of 4.8% in the subsequent two quarters, with declines of 7.5% for renters and 3.5% for homeowners. The declines are not due to moves. Instead, they are driven by the subset of households that miss payments. Quantile regressions show that median housing payments are unchanged, reflecting that most households pay in full. The 10$^{\text{th}}$ percentile of housing payments, in contrast, falls by 30% for renters and 10% for owners. These magnitudes imply that defaulting households replace nearly 5% of lost income with missed payments (more for renters), which is similar to income replacement from formal credit among households that are not borrowing-constrained (Braxton et al., 2024). Examining the dynamics of housing expenditure, I find that households do not (on average) repay this housing debt during the three quarters following job loss. Although not repaid in this window, the back rent is legally a debt and can be pursued in state court (even if the tenant vacates). In some cases, though, collection efforts on this debt will fail or the debt may be forgiven, in which case the unemployment risk and losses would ultimately be borne by the landlord or mortgage lender.

Finally, I examine the consequences of missed housing payments. It would be inappropriate to treat missed payments as a source of credit if missed payments regularly quickly lead to severe consequences, such as eviction. Using the same identification strategy as earlier, I estimate the effect of job loss on evictions. The magnitudes imply that the large majority (90-95%) of households that miss payments in response to job loss are not subsequently evicted. Evictions, however, may be under-reported or the data may not capture informal evictions or forced moves. To investigate, I also examine the frequency of residence changes among job-losing households. Renters who do and do not miss payments move out at nearly identical rates. Owners missing payments are 4-5pp more likely to move out over the next year, but these moves could be voluntary. For both groups, those who miss payments typically remain in the same residence for at least two years after job loss. In summary, evictions and forced moves are uncommon. In more recent work following this paper, Humphries et al. (2024) also find that late payments are often tolerated by landlords, and extend this by using lease and payment data to estimate a model of landlords' decisions about who and when to evict.

One policy implication of viewing late payments as informal credit is that the amount of this informal credit may be a policy parameter. I test whether the take-up and consequences of late payments vary with state laws governing eviction and foreclosure. The results are generally inconclusive, although I find some evidence that the use of missed payments increases where renter protections are stronger.

Together, these facts reveal that the ability to miss housing payments is a common and important consumption smoothing mechanism. The stylized model shows that, for otherwise borrowing-constrained households, the gains from consumption smoothing through missed payments can offset losses caused by the committed nature of housing. As a result, the net effect of housing (bundled with implicit credit) on consumption smoothing can be ambiguous. Importantly, however, this paper does not assess the total welfare impact of missed payments as there are costs (e.g., access to credit or future housing) or general equilibrium effects (e.g., housing markets and rent prices) that I do not measure.

This paper adds to three research areas. First, existing research examines the relationship between job loss and homeowners' mortgage delinquency and default. Most closely related are the few papers studying mortgage delinquency as a method of consumption smoothing. Closest is Herkenhoff and Ohanian (2019), which examine missed mortgage payments as an implicit line of credit, building a model of the consumption insurance and labor market impact of the lengthy foreclosure delays during the Great Recession. Low (2022) shows that the potential to recover from delinquent mortgage payments is important for explaining the prevalence of foreclosures in above-water homes. Lastly, Gelman et al. (2020) shows that government workers postponed mortgage and credit card payments in response to the 2013 government shutdown, which created a small, temporary (two-week) income shock. That paper questions whether late payments would be feasible for a shock of longer duration, such as job loss, which is the focus of this paper.

A larger subset of this literature investigates the causes of mortgage default, aiming to separate the roles of negative equity from negative life events such as job loss.[2] Gerardi et al. (2018) shows that job loss is an important driver of mortgage delinquency and incorporates this into an analysis of strategic and nonstrategic motives for default. Gyourko and Tracy (2014) finds an important effect of unemployment on mortgage default after correcting for attenuation bias. Bricker and Bucks (2016) examines the intersection of unemployment, negative equity, and mobility. Recent work by Ganong and Noel (2022) and Low (2023) show that negative life events are a causal factor in the large majority of mortgage defaults. Relative to existing work on mortgage default and consumption smoothing, this paper makes two primary contributions. First, I extend the analysis to renters. Second, I go beyond the binary indicator of delinquency used in existing work to also analyze the continuous measure housing expenditure, which allow me to examine partial payments and subsequent repayment patterns.

Second, this paper also fills a gap in the literature on expenditure responses to job loss by examining housing payments. Motivated by the Baily-Chetty formula, much existing research focuses on nondurable expenditure (Gruber, 1997; East and Kuka, 2015; Hendren, 2017; Ganong and Noel, 2019). Other papers examine durable goods expenditure (Browning and Crossley, 2009), total expenditure responses (Hurd and Rohwedder, 2016), or a broader set of responses including the social safety net and other income sources such as added-worker effects and severance pay (Andersen et al., 2023; East and Simon, 2024). Others have documented that households also commonly report falling behind on non-housing payments in response to unemployment (Hurd and Rohwedder, 2013; Herkenhoff, 2019). This paper adds to this literature by estimating the changes in housing expenditure upon job loss for renters and owners. While I focus on how housing payments help households cope with unemployment, other research examines the reverse: how unemployment insurance and the safety net help households make housing payments (Hsu et al., 2018; Hobbs, 2020; McKernan et al., 2021).

Third, viewing missed payments as informal credit, this paper adds to research on borrowing during unemployment. Some papers report little to no unsecured borrowing *on average* (Hundtofte et al., 2019; Bethune, 2015; Keys et al., 2018; Andersen et al., 2023; Ganong and Noel, 2019). Braxton et al. (2024) explains zero *average* borrowing by showing that some households default and delever, while other households borrow significant amounts (replacing 5% of the income loss).[3] Sullivan (2008) finds no unsecured borrowing among

[2]See Foote and Willen (2018) for a recent review.

[3]Among households that borrow, 1-2% is replaced through bank card borrowing, with the remainder through HELOCs and other personal loans.

the lowest asset households (first asset decile) but unsecured borrowing among other low asset households (asset deciles 2 and 3). Hurst and Stafford (2004) and Sodini et al. (2023) also show that homeowners are able to borrow against housing collateral when facing unemployment. This paper adds to this research by documenting the importance of informal borrowing through late housing payments, especially for low-asset renters. This informal credit may help explain why households maintain little (formal) liquidity (Lusardi et al., 2011; Kaplan and Weidner, 2014).

2. Institutional Background

This section provides an overview of the institutional features that cause landlords and mortgage lenders to tolerate some amount of late payments. The key feature is that pursuing eviction and foreclosure can be time-consuming and costly. For eviction, the typical process requires the landlord to give notice, file with the court, obtain a court judgment, and then have a sheriff execute the eviction by removing the tenant. Delays between each step are built into the legal process, and it generally takes several months, with significant variation in local and state practices.[4] For foreclosure, the average delay between the initial delinquency and foreclosure liquidation is 22 months, and there is heterogeneity across time and states due to different economic conditions and processes (e.g. judicial vs. non-judicial foreclosure) (Cordell et al., 2015). Landlords and lenders also face financial costs. For evictions, court fees cost between \$300 and \$800 in addition to any losses from unpaid rent or damage to the property. TransUnion Smart Move estimates that the total cost to property managers to evict a tenant is \$3,500 SmartMove (2022). For foreclosures, the expenses and the costs of delays contribute to the average loss given default exceeding 40% (An and Cordell, 2021).

Given these delays and costs, landlords and mortgagors often prefer to work with the tenant/owners to resolve missed payments. In the early stages of delinquency, landlords and mortgagors want to maintain the relationship with the occupant and so provide flexibility. Researchers conducting interviews with landlords have documented several ways in which landlords work with delinquent tenants, including developing payment plans, reducing rental rates, and accepting services in lieu of rent (Balzarini and Boyd, 2021). Survey evidence, albeit during the pandemic, finds that small property owners expect the large majority of rent delinquencies that are behind by less than six months to be resolved without the tenant moving, often with repayment plans and forgiveness of some back rent (Decker, 2021). For

[4] To the extent possible, I investigate heterogeneity by these state practices in Section 5.3. Anecdotally, however, much of the variation in eviction costs arises from informal differences in local practices that are difficult to measure.

mortgages, nearly 70% of serious mortgage delinquencies become current or modified within two years (Adelino et al., 2013). These alternatives are often successful in avoiding eviction or foreclosure. Only 5% of households (7.7% of renters) with late payments report an eventual eviction (1991-2008 SIPP).

Even when the legal process of eviction begins, it is often resolved without the tenant leaving. Eviction filings are the first step in the legal process, but are often filed only to encourage payment from tenants. Serial or repeat eviction filings for the same tenant by the same landlord are common (Garboden and Rosen, 2019). For example, almost half of eviction filings in Chicago are serial filings (Leung et al., 2020). Many eviction filings do not lead to an executed eviction where the tenant is removed. In New York, 10.9% of eviction filings end in execution, and in Chicago it is around 16% (Leung et al., 2020; Collinson et al., 2024).[5] Estimating the causal effect of an eviction order on residence changes, Collinson et al. (2024) find that an eviction order increases residence changes in that year by 8.2pp from a baseline move rate of 29.3% (among those receiving an eviction filing). Thus, even tenants who receive an eviction order often do not move.

In summary, evictions and foreclosures are costly, so property owners give delinquent residents opportunities to recover and avoid eviction or foreclosure. Because missed payments are still legally owed and can be pursued in small claims court (even if the tenant vacates), they are a form of informal credit. The role of this informal credit in coping with job loss is the focus of this paper.

3. Conceptual Framework

This section introduces a simple conceptual framework that adds late payments to the standard consumption commitments model of Chetty and Szeidl (2007). The purpose is to clarify two points. First, late payments enter households' budget constraints in the same way as dissaving or formal borrowing, and therefore late payments constitute a line of informal credit. A unique feature of this credit line is that it is contingent on not moving. In a more complex, dynamic model of mortgage delinquencies, Herkenhoff and Ohanian (2019) also highlights and analyzes a similar point, namely that foreclosure delays provide an informal line of credit. Second, the availability of late payments can alter the standard view that consumption commitments, such as housing, increase the welfare costs of moderate shocks. Consumption commitments are goods that are costly to adjust, such as housing, vehicles, or some utilities. In the standard model of Chetty and Szeidl (2007), households will often

[5]These statistics are obtained by multiplying the share of eviction filings that receive an eviction order by the share of eviction orders that are executed.

keep their consumption of committed goods fixed when facing shocks, which leads to greater welfare losses (relative to an agent that could freely adjust all goods). When committed goods allow late payments, however, the net effect of commitments is ambiguous. Commitments' adjustment costs still hinder consumption smoothing *across goods*, but the informal credit line facilitates consumption smoothing *over time*.

I begin by outlining the consumption commitments model of Chetty and Szeidl (2007). A household lives for T periods and consumes two goods: an adjustable good (f_t), such as food, and a committed good (x_t), such as housing.[6] The household chooses consumption of f_t and x_t in each period to maximize

$$E_0 \sum_{t=1}^{T} \beta^{t-1} u(f_t, x_t).$$

The key feature of the model is that the committed good, x_t, has an adjustment cost kx_{t-1} for $k > 0$ that must be paid in any period where committed consumption changes (i.e., $x_t \neq x_{t-1}$). These adjustment costs represent, for example, the transaction costs of selling a home, security deposits for renters, and the costs of hiring movers. For simplicity, the model focuses on a shock ($Z < 0$) to income (y) that occurs in period 1 and analyzes consumption smoothing during that period.

I add late payments to the standard model by distinguishing between *consumption* of the committed good, x_t, and *expenditure* on the committed good, $\tilde{x}_t$. When households fall behind on payments, they reduce spending but not consumption. Specifically, a non-moving household now has the option to spend less than is consumed so that expenditure $\tilde{x}_t = x_t - L_t$, where $L_t \geq 0$ is the amount of the rent or mortgage obligation that goes unpaid in period t and becomes housing debt. The budget constraint for period 1 can be written as

$$y + Z - f_1 - x_1 - kx_0 \cdot 1\{x_1 \neq x_0\} = (1 + r)W_0 - W_1 + L_1. \tag{1}$$

The left-hand side matches the standard model and represents after-shock income ($y + Z$) minus consumption ($f_1 + x_1$) and any adjustment costs. The right-hand side represents the methods for intertemporal smoothing. As in the standard model, W_t is the net wealth available at the end of period t, which can be negative if formal borrowing is allowed, and r represents the interest rate on formal borrowing or saving. The only change from the standard model is that the option for late payments L_1 is added to the intertemporal responses on the right side. If the household does not move ($x_1 = x_0$), the constraint on

[6] Online Appendix B includes the derivations for this section and examples from a quantitative model.

informal borrowing is $L_1 \leq \alpha x_0$, where α determines the share of the housing bill that is allowed to be late (before eviction is pursued). If the household moves ($x_1 \neq x_0$), these late housing payments would be unavailable ($L_1 = 0$).

This budget constraint highlights the first point of this section. Late payments enter the budget constraint alongside the other intertemporal methods of consumption smoothing. Any take-up of late housing payments mechanically relaxes the households' contemporaneous budget constraint and so facilitates consumption smoothing by allowing less of a drop in flexible consumption f_t. That is, if a household reduces housing expenditure by missing housing payments in period 1, it is able to have a smoother path in flexible consumption f_t over time (relative to the path if they had made housing payments in full). While this paper focuses on housing, note that the ability to miss payments is also relevant for other types of committed goods (vehicle payments, some utilities).

When households actually make use of the late payment option will depend on the costs of informal credit. These can consist of both pecuniary and nonpecuniary penalties. Leases and mortgage contracts often include late payment penalties and interest rates, which can be incorporated as a combination of fixed costs and interest rates (r_x) paid when $L_1 > 0$. Mortgage lenders and landlords often waive these penalties, however, so the informal credit can be cheaper than formal credit ($r_x < r$) or even negative ($r_x < 0$) if some debt is forgiven (Decker, 2021). There may also be nonpecuniary utility costs such as stigma or hassle costs associated with falling behind on payments, exchanging services (e.g., repairs, cleaning) in lieu of rent, risking eviction or foreclosure as in Herkenhoff and Ohanian (2019), or reduced access to future housing. The tolerance and penalties for late payments vary across landlords and households, so I do not take a specific stance (Balzarini and Boyd, 2021). In any case, households may find the informal credit value if informal credit is cheaper than formal credit or if they are formally borrowing-constrained.[7]

The second point of this stylized model is that the bundling of consumption commitments with an informal line of credit can, in some cases, offset the negative effects of commitments in the standard model. In the standard model of Chetty and Szeidl (2007), without late payments ($L_1 = 0$), households optimize by keeping committed goods fixed when facing small-to-moderate shocks. This forces all expenditure reductions onto the subset of flexible goods, which, in turn, increases the risk aversion of committed households and the welfare costs of moderate shocks. When late payments are added, households gain a new option for smoothing consumption that can offset the harmful effects of commitments. In particular,

[7]For detailed treatment of this choice, Wang (2022) develops a dynamic model of the tradeoffs involved in accessing formal and informal credit within the context of entrepreneurship in developing countries.

while consumption commitments still hinder smoothing across goods, the informal credit line accompanying the committed goods facilitates consumption smoothing *over time*. In Online Appendix B, I demonstrate this formally by adapting the model of Chetty and Szeidl (2007). Specifically, I show that the risk aversion of a committed agent with late payments can be either larger or smaller than the risk aversion of an uncommitted but borrowing-constrained agent, depending on the relative importance of smoothing consumption across goods versus over time.[8] Using a quantitative example, the Online Appendix also shows that late payments can be even more valuable if households face additional barriers to adjusting x downward. For example, some households may already reside in the lowest rungs of the housing ladder and so may be unable to move into a cheaper place, or they may face difficulty passing tenant screening and securing new housing while unemployed.

The remainder of the paper investigates questions raised by this framework. First, if households miss payments ($L_1 > 0$), it mechanically relaxes their intra-period budget constraints and so facilitates consumption smoothing. How often does this occur, i.e., how frequently do households miss payments in response to job loss? Second, by treating missed payments as an informal line of credit, the model implicitly assumes that landlords do not require immediate repayment or quickly evict tenants. How regularly do households missing payments face these negative consequences?

4. Data and Motivating Evidence

4.1. Survey of Income and Program Participation

The first data source is the 1991-2008 panels of the Survey of Income and Program Participation (SIPP). An advantage of the SIPP is that it covers a long period of time (1991-2010 in my sample), allowing me to assess the use of missed payments and evictions over a twenty-year period. Each panel surveys up to 43,500 households for 3-6 years and contains demographic information, monthly employment information, and annual questions about assets and debts. In each panel, the SIPP administers an Adult Well-Being topical module that asks questions about missed housing payments and eviction that occurred over the prior year. Specifically, it asks whether there was any time in the last twelve months when the household did not pay the full amount of the rent or mortgage and whether the household was evicted for nonpayment. The analysis focuses on households during this

[8] The relative importance depends on households' curvature of utility for noncommitted vs. committed goods, as well as the duration over which housing debt can be repaid. Smoothing across goods is more valuable when utility has a high curvature over noncommitted goods. Housing debt is more valuable when it can be repaid over many periods.

twelve-month period over which missed payments are assessed, henceforth the "payment-assessment period." Following a similar analysis in Hsu et al. (2018), I convert the panel data into a repeated cross-section with one observation per household.

I restrict the sample to households that are employed and either rent or own (with a mortgage) during the pre-period, i.e., the four months immediately preceding the payment-assessment period. I restrict the sample to households that, during the pre-period, either rented or owned (with a mortgage), reported positive housing costs, had positive labor income, and had no household members reporting unemployment. I also exclude households that live in public housing, receive government housing assistance, or are missing key variables.[9] To avoid confounding job loss with retirement, I follow Sullivan (2008) and restrict the sample to households where the head (i.e., the owner or renter of record) is between the ages of 20 and 63.

The main strategy will compare households that did and did not experience a job loss during the payment-assessment period. The final analysis sample, therefore, consists of households that experience job loss in the payment-assessment period and comparison households that experience no job loss or unemployment.[10] Demographic characteristics and financial characteristics (wherever possible) are measured during the pre-period.[11]

Table 1 Panel A reports the summary statistics for households that did and did not experience job loss. Demographics and financial characteristics (whenever possible) are measured prior to the payment-assessment period, so represent pre-job-loss characteristics.[12] There are three key observations. First, missed payments are common among job losers, but evictions are not. Fifteen percent of households in the job loss sample missed housing while only 0.6% report an eviction. These statistics, of course, do not necessarily reflect the causal effect of job loss. Second, households, especially those losing jobs, had few liquid assets (checking accounts, savings accounts, interest-earning accounts, equities, or mutual funds). The median household in the job loss sample holds $1,400 in liquid assets, or enough to cover 0.3 months

[9]The 1992 SIPP panel is excluded from the main sample because the panel begins in payment-assessment period, so I cannot observe the pre-period. I further restrict the sample to households that remained in the sample through the waves covering financial distress and wealth. Additionally, I exclude households with missing variables used in the analysis, as well as the households in the 1991-1993 SIPP where the respondent's state is not observed because the SIPP grouped some less populated states together.

[10]A job loss is defined as an instance where a household member transitions from being with a job (SIPP Employment Status Recode is 1-5) to "No job all month, on layoff or looking for work all weeks." Household unemployment is defined as any household member reporting "No job all month, on layoff or looking for work all weeks" for any month.

[11]Demographic characteristics are always measured during the pre-period. Pre-period financial characteristics are available for the 1996 - 2008 panels, but not for 1991 and 1993.

[12]Asset information is available before the payment-assessment period for the 1996, 2001, 2004, and 2008 panels of the SIPP, but not for the 1991 and 1993 panels.

of income. Lastly, housing is a significant expense, with the average household in the job loss sample spending 40% of pre-unemployment monthly income on housing, and the median is 20%. Online Appendix Table A.1 Panel A shows these characteristics for renters and owners separately, and renters tend to have fewer liquid assets and greater housing expenses as a share of income. These liquidity-constrained households with high housing expenses are those households that may find the ability to postpone housing payments especially valuable.

4.2. American Life Panel

The second data source is the RAND American Life Panel (ALP) survey, which started in 2006 and is an ongoing Internet panel survey run by RAND Labor and Population that has expanded from 2,500 respondents to around 6,000 respondents (4,500 households) as of 2017. Its sample is representative of the United States Adult Population.[13] Several times each month, ALP respondents receive an email to complete a questionnaire with response rates of 80 to 95 percent. This paper makes use of the Financial Crisis Surveys, a monthly or quarterly recurring survey of around 1,800 respondents per wave beginning in November 2008 and lasting into 2016. Hurd and Rohwedder (2013) and Hurd and Rohwedder (2016) provide a detailed overview of the Financial Crisis Surveys and the collection and accuracy of the expenditure data.

The Financial Crisis Surveys has detailed information on monthly expenditures, including housing payments. Each wave collects information about the household's expenditure in the previous month across 25 spending categories, taking measures to improve accuracy and recall (Hurd and Rohwedder, 2016).[14] These high-frequency spending categories account for around 70% of total spending. I focus on housing payments. I also form aggregated bill payments (housing, utility, and auto payments), which reflects committed consumption, and nondurable expenditures (food, housekeeping, recreation, transportation, personal services, and other child or pet expenditures). The survey also asks about missed housing payments, specifically whether the household is behind on payments by two or more months.

The primary analysis uses quarterly panel observations of households. The Financial Crisis Surveys transitioned from a monthly survey schedule to a quarterly survey schedule in mid-2013. To make the timing uniform, I convert the monthly expenditure data to quarterly by taking the average of the reported monthly expenditure in each quarter and dropping missing values. In robustness checks, I find similar results using only the portion of the

[13]To remain representative, ALP participants without Internet access were provided with an Internet subscription and access. See Pollard and Baird (2017) for an overview of the survey.

[14]After entering spending in each category, the participants are prompted with a reconciliation screen in which they can revise entries and correct mistakes.

survey for which monthly data are available. I also use the monthly portion of the survey when examining the expenditure paths immediately around instances of job loss.

The final sample consists of all survey households in each quarter t that, during the pre-period (quarters $t-3$ through $t-1$), either rented or owned with a mortgage and did not experience job loss.[15] Specifically, either the head or spouse was employed during the pre-period, and neither was unemployed. I also exclude households that moved during the pre-period. To ensure normal spending responses, I also require positive total income, housing expenditure, and nondurable expenditure during the pre-period.

Within this sample, the main analysis compares outcomes for households that do and do not experience job loss in quarter t. Table 1 Panel B shows the characteristics of the sample, split by whether the head or spouse lost a job in period t. The top of the panel shows the average characteristics during the pre-period. As in the SIPP, housing expenditures take up a significant share of pre-unemployment income. The bottom of the panel shows characteristics and outcomes during the post-period, defined as quarters t and $t+1$. Even from summary statistics alone, the impact of job loss is evident. Nearly 10% of job-losing households report being behind at least two months behind on rent in this six-month period, and 2.9% receive an eviction or foreclosure notice. Note that eviction notices are just the first step and do not necessarily lead to an eviction order or enforcement (Section 2). The final set of variables shows the changes in expenditure between the pre-period and the post-period. To reduce the influence of outliers, all percent changes are winsorized at 100%. For those experiencing job loss, spending declines in most categories, including housing.

Several papers measuring expenditures use detailed transaction data from bank accounts (Ganong and Noel, 2019) or online financial account aggregation services (Olafsson and Pagel, 2018; Gelman et al., 2020; Baugh et al., 2021). When examining rent payments, however, survey data like the ALP have some advantages. First, rent payments are difficult to track in financial account data. Through the mid-2010s, a period covered in most transaction data samples, more than 75% of rent payments were made through paper-based methods (cash, check, money order) with an even higher share for low-income households (Zhang, 2016).[16] In transaction or bank account data, cash payments and money orders may be missing and paper checks are often categorized as miscellaneous payments (Baker, 2018; Ganong and Noel, 2019; Baugh et al., 2021). Second, many households missing rent payments

[15] If a household was not interviewed in all quarters, the variables are formed from the quarters in which the household is observed.

[16] Zhang (2016) uses a represented survey from 2014. Using 2017 data, Greene and Stavins (2021) report that 33% of bill payments are made using paper-based methods, with higher rates for lower-income and underbanked consumers.

are unbanked or underbanked, and so would be absent from samples of transactions appearing in financial accounts. In the SIPP, 35% of households with late housing payments report no money in any financial accounts.

4.3. Motivating Evidence

To motivate the analysis, Figure 1 provides summary statistics on the frequency of missed housing payments among households that experienced job loss in both the SIPP and the ALP samples. In the SIPP (panel A), 20% of unemployed renters and 12% of unemployed owners with a mortgage missed a housing payment. Among these households missing payments, 7% of renters and 2% of owners report an eviction. The ALP (panel B) asks about methods that households use to cope with income loss from unemployment. Twenty-eight percent of unemployed renters report missing housing payments as a method of adjusting to the income loss, which is roughly the same share that reports smoothing consumption by borrowing (31%) or by using savings (30%).[17] For unemployed owners, 15% report falling behind on housing payments to cope with job loss. There are, however, potential concerns with relying solely on the share of unemployed households reporting missed payments. In particular, these measures do not capture the causal effect of job loss on missed payments, do not measure the magnitudes of the changes in housing expenditure, and do not convey the consequences of missing housing payments, which can include eviction or foreclosure. The remainder of the paper addresses each of these issues.

5. Empirical Strategy and Results

This section investigates the prevalence and consequences of missed housing payments that occur in response to job loss. First, using the SIPP, I estimate the causal effect of job loss on the tendency to miss any housing payments (the extensive margin). Second, using the ALP, I examine the change in housing expenditure (the intensive margin) and the dynamics of housing expenditure around job loss. Finally, I use the SIPP to examine the effects of job loss and missed payments on evictions and moves. Together, these exercises test the key assumptions underlying the conceptual framework in Section 3, namely that missed payments are a method that households use to smooth consumption following shocks and that missed payments are often tolerated, thereby acting as a line of informal credit.

[17]The ALP asks how unemployed households adjusted to the income loss from unemployment and households can select multiple responses from a list. Figure 1 Panel A omits "Reduced spending," which was reported by 77% of renters and 87% of owners, and "None," which was reported by 5% of owners and 5% of renters.

5.1. Extensive Margin: Frequency of Missed Housing Payments after Job Loss

I first investigate the effect of job loss on the tendency to miss housing payments and whether this effect varies between households with different levels of liquid assets. I use multiple strategies from the literature to identify the causal effect of job loss on payments.

Empirical Strategy. Using the SIPP sample consisting of households with no unemployment in the prior four months, I estimate the following specification for household i in state s in year t

$$\texttt{missed}_{ist} = \alpha + \beta \texttt{job loss}_{ist} + \boldsymbol{X}_{ist}\gamma + \boldsymbol{Z}_{st}\xi + \delta_s + \tau_t + u_{ist}. \tag{2}$$

The dependent variable, `missed`, is an indicator for whether the household reports missing a housing payment during the payment-assessment period. The independent variable of interest is `job loss`, an indicator for whether someone in the household lost a job during that same twelve-month period. The variables $\boldsymbol{X}_{it}$ include household-level demographic controls - the household head's age, marital status, indicators for the head's race as Black and other nonwhite, an indicator for Hispanic ethnicity, indicators for the head's education group (5 categories), pre-period household income and changes in household size or marital status from the pre-period - and also household financial variables - the household's liquid assets, total net worth, unsecured debt, and housing payments as a share of monthly baseline household income.[18] State economic controls $\boldsymbol{Z}_{st}$ include the unemployment rate, max unemployment benefits, log of real GDP per capita, and average wages, all from Hsu et al. (2018), as well as the de-meaned unemployment rate and max benefits interacted with the household unemployment indicator.

The coefficient β represents the causal effect of job loss on missed housing payments. The primary concern with identifying β is that, even conditioning on observables, job loss could be endogenously related to missed housing payments. Financially fragile households may be more likely to miss housing payments and to lose their job, or households planning to move may both quit their job and skip housing payments. In this case, the estimate β would still measure the (conditional) prevalence of missed payments among job losers, but it would be a biased estimate of the *causal* effect of job loss. I address this concern by focusing on two definitions of involuntary job losses, which are more likely to be exogenous. Following Sullivan (2008), "Involuntary 1" consists of job losses due to layoff, illness or injury to the worker, being discharged or fired, employer bankruptcy, or the sale of the business. Following

[18] The pre-period consists of the four months immediately before the twelve-month period over which missed payments and unemployment are assessed. Whenever possible, financial variables are also from before the twelve-month period where housing payments are measured. Earlier financial information is available for the 1996, 2001, 2004, and 2008 panels, but not the 1991 and 1993 panels.

the definition in Gerardi et al. (2018), "Involuntary 2" is more restrictive and consists only of job losses due to layoffs, employer bankruptcies, or the sale of the business.[19]

I use these definitions of involuntary job loss in two strategies. First, similar to Gerardi et al. (2018), I instrument for the potentially endogeneous `job loss`, which includes all job losses, with an indicator for involuntary job loss. Unsurprisingly, involuntary job losses are highly correlated with the overall job loss variable (first-stage F-statistics are all above 500), so this strategy will be valid as long as involuntary job losses are uncorrelated with other unobserved factors affecting missed payments.[20] As a second strategy, similar to Sullivan (2008) and East and Simon (2024), I reestimate the OLS regressions but restrict the sample to only involuntary job losses (plus the control group of those without any job loss). Finally, in robustness checks, I examine the sensitivity to several other methods, including adding indicators for each household's number of prior unemployment spells,[21] restricting the sample to households with at least one year of prior employment, and applying the method of Oster (2019) to examine the sensitivity of the estimates to different assumptions about the importance of unobservable selection relative to the observable selection.

Results. The columns in Table 2 show estimates of the effect of job loss on missed payments from equation (2) using the sample of all respondents (Panel A), renters (Panel B), and owners with mortgages (Panel C).[22] Column (1) includes only year fixed-effects as controls. The estimate in Panel A shows that, when a household member experiences job loss, the probability of a missed payment increases by 9.6 percentage points (pp). This estimate falls to 7.8-7.9pp when each household's demographic and financial controls (column 2) and state fixed-effects and state economic controls (column 3) are successively added. Panels B and C column (3) show that the effect of unemployment on missed housing payments is 45% higher for renters (9.3pp) than for owners (6.4pp), and the p-value of the difference is 0.02. In sum, missing housing payments is a common response to job loss and is more common among renters than owners.

A concern with the OLS results in columns (1)-(3) is that job losses may be endogenously related to missed payments, leading to a biased estimate of the causal effect of job loss.

[19]Not all instances of job loss in the SIPP identify a reason for the job loss, and I classify these as not involuntary. I also examined the Bartik-style instrument of Gerardi et al. (2018) using aggregate employment flows and state-level industry analysis, but it has a weak first stage in this sample.

[20]Although not a test, the income patterns are consistent with exogeneity. Appendix Figure A.1 shows that monthly household income (from all sources) is stable in the months prior to involuntary unemployment, and then drops suddenly upon job loss.

[21]This strategy follows Gerardi et al. (2018) and is reported in Appendix Table A.2.

[22]Whether the household is a renter or owner is measured during the four months immediately prior to the twelve months over which missed payments and unemployment are assessed.

Columns (4) and (5) investigate this by instrumenting for job loss with an indicator for involuntary job losses, using the two alternative definitions of "involuntary" explained above. Similarly, columns (6) and (7) use OLS, but drop observations with job losses that are not involuntary. In all cases, the IV and subsample OLS estimates remain similar to those in the OLS regressions of column (3). In further checks, I find similar results when using other measures of involuntary job loss, when restricting the sample to households with stable prior employment, and when applying the method of Oster (2019) to investigate assumptions about the importance of unobservables.[23]

A key idea of the paper is that missing housing payments are an important source of liquidity for otherwise constrained households. This suggests that missed payments should be more common among households with fewer liquid assets. To investigate, I interact `job loss` in equation (2) with indicators for the quintile of the household's liquid assets, also directly controlling for the liquid asset quintile and the full set of controls from Table 2 column 3. Figure 2(a) reports the coefficients on these interactions, estimated on the samples of renters and owners, respectively. For both groups, the effect of job loss on missed payments is largest among those with little liquidity and falls as household liquidity increases. Those in the bottom two quintiles have less than \$1,550 in liquid wealth (median \$134), and about 10% miss housing payments in response to job loss. Those in the top two quintiles have at least \$6,240 in liquid wealth (median \$25,890) and miss payments, and about 5% miss payments in response to job loss. The figure also shows that the delinquency rates within each quintile are similar for renters and owners (except for those in the fourth quintile). Thus, the higher delinquency rates for renters seem to be explained by the lower average liquidity of renters, rather than other differences between renters and owners such as the consequences of missed payments.

5.2. Intensive Margin: Percent Changes in Housing Payments after Job Loss

The last analysis showed that missed payments are a common response to job loss, but not whether these missed payments constitute a meaningful source of liquidity. This sub-

[23]Online Appendix Table A.3 demonstrates robustness to other measures of job losses that are less likely to be planned or voluntary, and to restricting the sample to households with stable employment prior to the job loss (no unemployment in the preceding year). Online Appendix Table A.4 applies the method of Oster (2019) to examine the sensitivity of the estimates to different assumptions about the importance of unobservable selection, which requires assumptions about (i) the coefficient of proportionality, δ to capture the importance of unobservables relative to observables, and (ii) the value of $R^2_{\mathtt{max}}$, i.e., the R-squared value from a hypothetical regression of the outcome on all relevant controls (including those currently unobserved). Applying the recommended values of Oster (2019), the bias-adjusted estimates imply that job loss increases the probability of missed housing payments by 7.1pp overall, 8.3pp for renters, and 6.4pp for owners, and the values remain in a similar range when applying more conservative assumptions.

section investigates the intensive margin response, i.e., the percentage reductions in housing expenditure, and also how quickly these missed payments are repaid (if at all). I use information on monthly housing expenditures from the ALP Financial Crisis Surveys to examine the size and timing of payment reductions.

Empirical Strategy. I examine changes in housing expenditure around instances of job loss, following a standard methodology from research on changes in nondurable (food) expenditure around job loss (Gruber, 1997; East and Kuka, 2015; Hendren, 2017). The primary dependent variable is Δy_{it}, which is the percentage change in household i's housing expenditure

$$\Delta y_{it} = \frac{\overline{y}_{it}^{\text{post}} - \overline{y}_{it}^{\text{pre}}}{\overline{y}_{it}^{\text{pre}}}.$$

The post-period is average monthly expenditure in quarters t and $t+1$, while pre-period is average expenditure in quarters $t-3$ through $t-1$. [24] I similarly compute percentage changes in income, bill payments (housing, utility, and auto payments), and nondurable expenditure. To reduce the influence of extreme outliers, I truncate the percentage change at $\pm 100\%$. In additional specifications, I find the estimates are also robust to using log changes or dollar changes in expenditure.[25]

The identification strategy examines changes in expenditure among households that experience job loss compared to households that remain employed. The sample is restricted to households i in quarter t that, during the pre-period, were renters or owners with a mortgage, where the head or spouse was employed, and where neither were unemployed. I estimate the following specification using ordinary least squares (OLS):

$$\Delta y_{i,t} = \beta \texttt{job loss}_{i,t} + \tau_t + \varepsilon_{i,t} \tag{3}$$

where $\texttt{job loss}_{i,t}$ is an indicator that equals one if the head or spouse in household i lost a

[24]Not every respondent is interviewed in every wave so, if missing, I calculate the averages over the observed expenditure in the quarters.

[25]The most common strategy for food or nondurable consumption is to use log changes and a single pre-period of $t-1$ (e.g., Gruber (1997)). I deviate from this because of unique features of the ALP and housing expenditure, but I also show the results are robust to the standard method. I use exact percentage changes instead of log changes because, unlike food expenditure, housing expenditure sometimes falls to \$0. I use multiple quarters in the pre-period because, in the ALP, not every participant responds in every quarter. Additionally, averaging over multiple quarters to form the pre- and post-period can lead to efficiency gains compared to using a single period (Dube et al., 2023). None of these choices are critical, however, as the estimates are similar if the sample is restricted to respondents observed for the full pre- and post-period (Appendix Table A.5), calculating the percentage change in the difference in $log(y+1)$ (Appendix Table A.6), or using the change in dollar expenditure (Appendix Table A.8).

job in quarter t, and τ_t is a set of quarter fixed effects. The coefficient β captures the average percentage change in housing expenditure for households with job loss in quarter t relative to the change in expenditures for households that do not. I also estimate this specification separately for renters and owners.

I also examine the dynamic path of spending around job loss, which is useful for two reasons. First, although first-differencing controls for time-invariant factors within households, unbiased estimates still require the parallel trends assumption on spending between households that do and do not lose their jobs in quarter t. Examining the pre-treatment spending path helps assess this assumption. Second, the path of spending in the post-period reveals how quickly households repay debt (if at all). If debt is being repaid on average, we would expect average payments in the post-period to eventually rise above the pre-period average because households will be paying their rent plus a portion of the back rent.

To provide a more granular picture, I use the portion of the ALP survey for which monthly expenditure data are available (May 2009 - April 2013) when examining payment dynamics.[26] I use the local projections method Jordà (2005) applied in a difference-in-difference (LP-DiD) setting as in Dube et al. (2023). They show that the LP-DiD, which combines local projections with a "clean" control group, provides an alternative framework to difference-in-difference estimation and avoids problems of negative weights associated with two-way fixed effects regressions.[27] I implement this method by estimating the following set of regression equations

$$\frac{y_{i,t+h} - y_{it}^{\text{pre}}}{y_{it}^{\text{pre}}} = \beta_h \texttt{job loss}_{i,t} + \tau_t + \varepsilon_{i,t} \qquad \text{for } h = -8, \ldots, 8 \tag{4}$$

where the dependent variable is the percent change in housing expenditure in month $t+h$ relative to the pre-period expenditure.[28] The sample used to estimate equation (4) consists of (i) households whose first job loss occurs in period t and (ii) the "clean" control group consisting of households that have not reported any job loss or unemployment as of period

[26]When using the monthly data, the pre-period consists of months $t = -6$ to $t-1$ and I impose the same sample restrictions as in the quarterly analysis. Appendix Table A.7 repeats the regressions in Table 3 using this monthly sample.

[27]Another advantage of the local projections approach in this setting is that, in the ALP, not all respondents answer the survey every month. In contrast, a standard distributed lag event study model would require all respondents to be in the panel for $t = -8, \ldots, 8$, which would limit the sample. As mentioned earlier, the main results are robust to using a balanced panel (Appendix A.5).

[28]Although the dependent variable is divided by y_{it}^{pre}, it can be written as $\frac{y_{i,t+h}}{y_{it}^{\text{pre}}} - \frac{y_{it}^{\text{pre}}}{y_{it}^{\text{pre}}}$, which matches the LP-DiD specification in Dube et al. (2023). As with the earlier results, I winsorize these percentage changes at 0% and 100%. Likewise, `job loss` is an indicator that equals one only in the period of job loss, and so matches the first-difference of the treatment indicator in Dube et al. (2023).

$t + h$. Given parallel trends and no anticipation, the estimator $\hat{\beta}_h$ recovers the variance-weighted average treatment effect on treated h periods after job loss (with $h < 0$ showing pre-treatment trends).

Lastly, I examine two important extensions. First, I estimate quantile regression versions of equation (3). Only a minority of households miss housing payments. Most households repay in full. Estimates of changes in housing payments at different parts of the distribution allow me to assess the changes in housing payments among the subset of households that miss payments. Second, I account for the possibility that some changes in expenditure could be driven by households that move. I use a partial identification approach, making conservative assumptions about what households would have paid had they not moved, and estimate upper bounds on the average change in housing payments condition on not moving. Second,

Results. Table 3 shows the effect of job loss on income, housing payments, bills, and non-durable expenditure. As a benchmark, Column (1) shows that household income declines by 26% upon job loss, with slightly larger (smaller) declines for renters (owners). Next, for comparison with the SIPP estimates, I estimate the effect of job loss on an indicator for missed housing payments in the ALP which indicates whether the household is two or more months behind on rent in either quarter t or $t + 1$. Column (2) shows that job loss is associated with a 7.5pp increase in the probability of a missed payment, with slightly higher point estimates for renters, similar to the estimates from the SIPP.[29]

The main results in column (3) show that housing expenditure is fairly flexible. Upon job loss, housing expenditures fall by 4.8% relative to the pre-period expenditure, with declines of 7.5% and 3.5% for renters and owners, respectively. Column 4 broadens the expenditure outcome to bills – defined as the sum of housing, utility, and auto payments – which represent a wider set of consumption commitments. The negative estimates show that the ability to fall behind on payments is not confined only to housing. Moreover, these percentage declines represent economically meaningful amounts, as Table 1 shows that housing payments (bill payments) take up 28% (45%) of the monthly income of these households' pre-unemployment income.

Moreover, for renters, the declines in housing payments match the declines in nondurable expenditures. Column (5) shows that nondurable expenditure falls by 8% upon job loss, with declines of 7.6% for renters and 8.4% for owners. These magnitudes are consistent with estimates in the literature, including the path of nondurable expenditure after job loss

[29]The magnitudes are slightly smaller than the no-controls estimates from the SIPP (Table 2 column 1), which could reflect the differences in the questions (any missed payments vs. 2+ months of missed payments) and timing (covering a 12-month or a 6-month period).

found in Ganong and Noel (2019), and the 7-10% decline in food expenditure found in Gruber (1997) and East and Kuka (2015). Thus, although housing consumption remains committed, housing expenditure for renters is on average as flexible as nondurable expenditure.

Next, I examine the dynamic path of income and expenditures around job loss. Figure 3 shows the patterns for income, housing, bills, and nondurables using estimates from the local projection difference-in-difference specification in equation (4). There are two main takeaways. First, all show relatively stable trends in the months leading to job loss, providing some support for the parallel trends assumption. Second, the post-job-loss paths for housing payments in panel (b) reveal information about repayment patterns. Owners' payments fall by a smaller amount and recover to the pre-period levels fairly quickly. Renters' payments, in contrast, continue to fall in months 0-3 to a minimum of around -13% then gradually recover in months 4-6. Around six months of missed payments is consistent with the survey results reported in Decker (2021), which finds that 20% of landlords intended to pursue eviction for tenants 3-5 months behind, but 70% would pursue eviction for tenants with 6+ months of late payments.

The pattern of payments for renters also indicates that missed housing payments are not repaid, on average, in the observed window. Although the estimates return just below zero in months 6-8, this only indicates that housing payments in those months are similar to the amounts in the baseline pre-period. In contrast, if households were repaying the housing debt, we would observe coefficients above zero which would reflect payments that are higher than the pre-period baseline. Thus, the informal credit line allows households to borrow for at least several quarters. This debt may eventually be repaid, and some repayment plans have long time horizons Balzarini and Boyd (2021), or it may be pursued in small claims court. Some back rent, however, will likely go unpaid with the losses ultimately borne by landlords.

Quantile Regression. Only a fraction of households miss payments, and the average expenditure declines are likely driven by large payment reductions among this subset of households. I investigate this heterogeneity by estimating quantile regressions following equation (3).

Table 4 reports estimates of these quantile treatment effects for expenditure on housing and nondurable goods, reporting the estimates for all respondents and for renters and owners separately. As expected, the declines in housing expenditure are concentrated in the low end of the distribution. The 0.1 quantile of the change in housing expenditure falls by 16.4% for all respondents, 31.5% for renters, and 10.4% for owners. Most households, though, continue paying in full. Reflecting this, there are minimal effects on the 0.4 through 0.95 quantiles of the distribution of changes in housing expenditure. In contrast, reductions in nondurable payments are spread more evenly across the distribution (columns 2,4,6). As one measure of this comparison, the 0.05 quantile of housing payments falls by 25 times as much as the 0.75 quantile, but for nondurable expenditure the 0.05 quantile falls by about twice as much as the 0.75 quantile.

There are two implications. First, the fact that most households do not adjust housing expenditure, but a minority reduce housing expenditure by a lot, suggests some fixed cost of falling behind on payments. This could be late fees or the utility costs of talking with the landlord or lender. Second, for those who do miss payments, the magnitudes of the reductions are large. Given that the average monthly housing expenditure is $1,200, a 16.4% decline in monthly expenditure across the first six months after job loss would reflect unpaid rent or mortgage payments of at least $1,000. With the average pre-period housing-to-income ratio and income reduction from job loss, a 16.4% reduction in housing replaces 4.6% of the lost income. For renters, the effects are even larger since expenditure falls by larger amounts and housing makes up a higher housing-to-income ratio (0.3 in Table A.1). This implicit line of credit exceeds many existing estimates of borrowing on credit cards during unemployment. For example, Ganong and Noel (2019) finds that new credit card borrowing funds only 0.5% of consumption during unemployment, and Braxton et al. (2024) finds that the subset of borrowers with unused credit replace around 5% of lost income with borrowing on bankcards, HELOCs, and personal loans.

Accounting for Movers. Not all of the declines in housing expenditures are because households skip or postpone payments. Some households move into cheaper places. Although (most) moves are observable in the ALP, moves remain problematic because I do not observe the counterfactual amount these households would have paid if they had not moved. Moreover, selection into moving may be endogenous if, for example, households move because their landlord does not allow late payments. Such a correlation would lead to biased estimates of the changes in housing payments if I simply restrict the sample to nonmovers. To investigate these concerns, Online Appendix C constructs upper bounds on what the average household would have paid if no households had moved. To do so, I make the conservative assumption that, if households had not moved, their average housing expenditure would not have *increased* because they lost their job. This allows for the possibility that landlords of movers may have been less flexible on rent and required full payment. The resulting upper bounds indicate that housing expenditure would have fallen by at least 4.1% if no households moved, with a bound of 5.6% for renters and 3.5% for owners.[30]

[30] Some households are missing data on whether they move, and the upper bounds treat moves status as missing at random. I also construct a more conservative upper bound, treating all missing observations as

5.3. Consequences of Missed Housing Payments

The premise of the paper is that missed housing payments are a source of informal credit, and a key assumption is that some missed payments are tolerated as a form of debt. This assumption would be inappropriate if, instead, missed payments regularly lead to evictions or forced moves. This section examines the impact of missed payments on evictions and moves. I also investigate whether the probability of eviction is influenced by state policies affecting eviction and foreclosure. Allowing for the possibility that evictions are informal or underreported, I separately examine the incidence of residence changes among those missing payments.

Evictions. To examine whether job loss results in eviction, I estimate the following equation:

$$\texttt{eviction}_{ist} = \alpha + \beta \texttt{job loss}_{ist} + \boldsymbol{X}_{ist}\gamma + \boldsymbol{Z}_{st}\xi + \delta_s + \tau_t + u_{ist}.$$

The right-hand side matches the earlier equation (2), but dependent variable is now an indicator for whether the household i in state s has been evicted for nonpayment of the rent or mortgage payment during year t. The indicator `job loss` and controls are identical to those in equation (2), and the coefficient β captures the difference in the probability of eviction for households that lose jobs relative to those that do not. As in equation (2), the primary concern is that job losses may be endogenous so, as in Table 2, I investigate this using IV and subsample strategies exploiting a subset of involuntary job losses.

Table 5 reports the estimates. Overall, job loss increases the probability of eviction during the year by 0.4pp. Panels B and C reveal that these evictions are of renters, not owners. For renters, evictions rise by 0.9-1pp (columns 1-3), while for owners the effect of job loss on evictions is negligible. Compared to the rates of missed payments, evictions are rare. Job loss increases missed payments by 7.8pp (Table 2 col. 3), but evictions only increase by 0.4pp. Similarly, for renters, missed payments increase by 9.3pp while evictions rise by only 0.9pp. Dividing the additional evictions by the additional missed payments implies that only 5-10% of missed payments (that are caused by job loss) lead to evictions. Like missed payments, Figure 2 panel (b) shows that evictions are more common among those with low liquid assets.

The small effect of job loss on evictions is robust to different methods of dealing with the potential endogeneity of job loss. Columns (4)-(7) examine the sensitivity to using the more restrictive definitions of involuntary job loss as an IV (columns 4-5) and by restricting the

movers, and the estimated upper bounds are 3.2% for the overall sample, with a bound of 4.1% for renters and 2.9% for owners.

sample of job losses to those that are involuntary (columns 6-7). There are some differences in the point estimates across the two measures of involuntary job loss. When using the broader measure of Sullivan (2008), the point estimates in columns (4) and (6) are similar in magnitude to the OLS estimates. When using the more restrictive measure of job losses based on Gerardi et al. (2018) in columns (5) and (7), the point estimate is smaller and not statistically significant. The difference between the measures is that the second measure (Invol. 2) includes only job losses from layoffs and the bankruptcy or sale of the business, while the first measure (Invol. 1) adds in job losses from illness/injury or being discharged/fired. These latter two categories have higher rates of both missed payments and evictions (Online Appendix Table A.9), leading to larger point estimates when these categories are included. Online Appendix Tables A.3 and A.4 Panel B further examine the robustness to other measures of involuntary job loss and the bias-adjustment method of Oster (2019), and show that these changes further reduce the already small effect of job loss on evictions.

The result that only 5-10% of missed payments lead to evictions is also supported by alternative estimation strategies. The raw sample means in Table 1 indicate that, among job losers, 4% of missed housing payments lead to evictions. I also restrict the sample to job losers, then estimate the following specification:

$$\texttt{eviction}_{ist} = \alpha + \beta\texttt{missed}_{ist} + \boldsymbol{X}_{ist}\gamma + \boldsymbol{Z}_{st}\xi + \delta_s + \tau_t + u_{ist}.$$

Online Appendix Table A.11 reports the estimates of β, showing that, overall, 4.3% of missed payments lead to evictions and, among renters, 6.6% of missed payments lead to evictions. Both results are insensitive to additional controls.

State Policies on Eviction and Foreclosure. Next, I examine whether the incidence of late payments or evictions is affected by local policies or the characteristics of landlords, which could alter the use of late payments as a source of credit. If landlords can quickly and cheaply evict tenants, it might reduce their willingness to allow late payments. In the Online Appendix Tables A.12 and A.13, I examine heterogeneity in the frequency of missed payments and evictions across (i) states with different requirements for notifying tenants, which affect eviction filing rates (Gromis et al., 2022), (ii) eviction filing fees, and (iii) judicial foreclosure requirements, which makes foreclosure more costly and time-consuming for lenders (Feinstein, 2018). In states where landlords must notify tenants at least five days before filing an eviction, I find that renters (but not owners) are more likely to miss payments upon job loss. For the other laws, the estimates are not statistically significant. I also examine heterogeneity by the likely landlord type, using a proxy for an individual (mom-and-pop) or corporate landlord, but find imprecise estimates.[31] Overall, I find only mixed evidence that this informal credit use responds to policy, but the sample size of the SIPP is not ideal for this heterogeneity analysis.

Moves. One possibility is that, instead of being evicted, those who miss payments are pressured to move or decide to move voluntarily. Fortunately, the SIPP is well-suited to track respondents who move. As a person-based survey, it follows original sample members regardless of the household composition and moves.[32] I use this information to examine how frequently households move after missing housing payments.

I begin by comparing the unconditional move rates for job-losing households that do and do not miss housing payments in Figure 4. Renters (panel a) move frequently, with about 30% moving out within twelve months following the job loss and 40-45% moving within 24 months. Importantly, the rates at which households move are similar for those who did and did not miss housing payments. The figure indicates that 70% (55%) of renters who miss housing payments remain in the same residence for at least one year (two years). Owners (panel b) move less frequently, but those who missed housing payments moved out more frequently than those who did not. Given that informal evictions or forced moves are less common among owners, the increased move rates may reflect decisions by owners rather than the consequences of missed payments. I also examine the robustness of the results to sample attrition. Appendix Figure A.2 shows that, two years after job loss, 40% of renters have neither moved nor attritted, and the rates are again similar for those that did and did not miss payments.

Next, I examine move rates after controlling for observable characteristics of these job-losing households. I restrict the sample to households in which a member experienced job

[31] There is some evidence that mom-and-pop (individual) landlords are more willing to work with tenants and construct repayment plans than corporate landlords (Balzarini and Boyd, 2021; Decker, 2023). While the SIPP does not contain information on the landlord type, the panels up to 1996 contain information on the number of units in the property. Individual landlords own and manage most properties with fewer than five units, but are much less likely to own properties with more than five units. Analyzing the Department of Housing and Urban Development's 2018 Rental Housing Finance Survey, Cororaton (2020) reports that 72.5% of 1-4 unit properties are owned by individual investors, and the day-to-day operations in more than 70% of these properties are managed by the owner. For larger properties, individual ownership and management is much less common. For properties with at least 150 units, less only 6% are individually owned.

[32] The SIPP uses several techniques to track original sample members who move. At the first interview, the SIPP interviewer collects contact information for a person who could provide a new address if the whole household moves. The SIPP interviewer may also contact neighbors, employers, or use administrative resources to track movers. The SIPP continues to follow the original sample members as long as they are not institutionalized, living in military barracks, and do not move abroad. See SIPP User Guide, Chapter 2 for more information about the procedures for tracking movers.

loss, and I drop households that either moved in the four months prior to job loss or are not observed for at least twelve months after job loss.[33] I estimate regressions of the following form:

$$\texttt{move}_{ist} = \alpha + \beta\texttt{missed}_{ist} + \boldsymbol{X}_{ist}\gamma + \boldsymbol{Z}_{st}\xi + \delta_s + \tau_t + u_{ist}. \tag{5}$$

The dependent variable is an indicator of whether the household moves within the twelve months following the start of the unemployment spell. The coefficient β captures the effect of missing housing payments on the probability of remaining in the same residence for at least one year. The controls match those from equation (2).

Table 6 reports the results. With no controls, those missing housing payments are 6.2pp more likely to move than those who did not miss housing payments. This gap in move-out rates falls to 3.5% when the full set of household and economic controls are included in column 4, reflecting selection on observable characteristics. Panels B and C reveal that, unlike formal evictions, these differential moves are largely driven by owners. Owners who miss payments are 4.5-5pp more likely to move. In contrast, there are no statistically significant differences in move-out rates for renters who do and do not miss rent payments. Using the full sample, the point estimates range from indicating that those who miss payments are 1pp less likely to 2.6pp more likely to move. When job losses are restricted to those who experienced involuntary job losses (defined in Section 5.1), columns 5 and 6 show that job-losing households who miss payments are *less* likely to move than those who did not, although the differences are not statistically significant.

A concern is that missed payments are not randomly assigned, so differences in move rates may not reflect the causal effect of missed housing payments. Some obvious sources of bias suggest the estimates may *overstate* the effect of missed payments on moving. For example, households facing larger income shocks may be more likely to miss payments, as implied by the model of Chetty and Szeidl (2007). In line with this, Online Appendix Table A.14 finds that missed payments are correlated with longer unemployment durations and larger income shocks. As another example, households that plan to move may be more likely to miss payments because they need not maintain a good relationship with the landlord. This would also lead to upward bias. Lastly, reflecting upward bias from observable selection, the coefficient on missed payments falls slightly as controls are added (Panel A) and falls when restricting the sample to involuntary moves. As with the earlier results, I use the method of Oster (2019) to assess the sensitivity of the estimates to different assumptions about

[33] Appendix Table A.10 shows the estimates when I include households that moved prior to job loss. When these households are included, those that miss payments have slightly *lower* move rates than those that do not.

unobservable selection and find similar estimates (Online Appendix Table A.4 Panel C). External evidence also finds relatively low move rates even after eviction orders.[34] Overall, the evidence consistently shows that missed payments are infrequently followed by evictions or forced moves.

6. Conclusion

This paper examines the role that missed housing payments play in helping households smooth consumption. Upon job loss, households frequently fall behind on housing payments, reducing housing expenditure (but not consumption) and thereby accumulating debt owed to landlords and lenders. This informal credit line is a significant source of liquidity during unemployment, comparable to estimates of the use of formal credit card borrowing during unemployment. Moreover, the large majority of the missed housing payments do not lead to eviction, and households missing payments move at similar rates (for renters) or only slightly higher rates (for owners) than those not missing payments.

Together, the results show that missed housing payments constitute a widely used source of informal credit that facilitates consumption smoothing, especially for renters. The availability of this informal credit may be a policy parameters that depends on local and state eviction laws. Additionally, by providing an alternative method of consumption smoothing, the availability of informal credit has implications for optimal unemployment insurance (UI) and suggests that UI and tenant protection policies should be considered jointly.

Much more evidence is needed, however, to conduct a complete welfare analysis of the impact of missed payments. In the period observed in the data, the late payments are not (on average) repaid, so the losses may ultimately be borne by landlords. There is also likely substantial heterogeneity across landlords and lenders in their treatment of these informal loans, perhaps with variation across types of landlords and types of tenants. In recent work, Humphries et al. (2024) use lease-level data to estimate a model of landlords' decision to evict or tolerate late payments and evaluate policies to prevent evictions. Additionally, while I examine the severe consequences of evictions and moves, there may be longer-run consequences in credit markets, although these effects are limited by the infrequent reporting of rent debt to credit bureaus. Finally, there are likely general equilibrium responses to the ability to make late payments. Landlords and lenders may adjust rent prices, interest rates, or screening to compensate. While these are all important for understanding the total

[34] Collinson et al. (2024) estimate that an eviction order, which likely indicates a serious delinquency, causally increases the probability of a residence change within one year (two years) by 8.2 (11.1) percentage points from a baseline move rate of 29.2% (47.8%).

welfare impact of late payments, they do not take away from the main result of this paper: late housing payments are an important aspect of how households cope with job loss.

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Table 1: Summary Statistics

	Job Loss		No Job Loss	
	Mean	Median	Mean	Median
Panel A: Survey of Income and Program Participation				
Missed Payments and Eviction				
Missed housing payment (%)	15.0	0.0	5.2	0.0
Eviction in prior 12 months (%)	0.6	0.0	0.2	0.0
Demographic Characteristics				
Homeowner (%)	59.8	100.0	69.3	100.0
Annual HH income ($1,000s)	76.7	63.3	86.9	71.3
Age	41.3	42.0	41.6	41.0
Married (%)	60.0	100.0	61.7	100.0
Household size	3.2	3.0	2.8	3.0
Race: Black (%)	11.4	0.0	9.0	0.0
Ethnicity: Hispanic (%)	12.7	0.0	7.8	0.0
Educ.: Less Than HS (%)	13.5	0.0	7.0	0.0
Educ.: HS (%)	27.2	0.0	22.9	0.0
Educ.: Some College (%)	34.3	0.0	33.6	0.0
Educ.: College (%)	16.5	0.0	22.5	0.0
Educ.: Grad School (%)	8.5	0.0	14.0	0.0
Financial Characteristics				
Liquid assets ($1,000s)	21.3	1.4	38.7	3.3
Liquid assets / monthly income	3.2	0.3	5.1	0.6
Has unsecured debt (%)	69.3	100.0	68.9	100.0
Unsecured debt ($1,000s)	13.4	2.9	12.5	2.5
Unsecured debt / monthly income	3.3	0.5	3.1	0.4
Housing costs ($1,000s)	1.3	1.2	1.4	1.2
Housing costs / monthly income	0.4	0.2	0.3	0.2
Observations	4,465		51,088	
Panel B: RAND ALP Financial Crisis Surveys				
Pre-period Characteristics				
Owners (%)	62.4	100.0	74.5	100.0
Monthly income ($1,000s)	5.8	4.3	7.7	5.8
Credit card debt ($1,000s)	6.0	0.3	5.9	0.5
Monthly Housing exp. ($1,000s)	1.2	0.9	1.4	1.0
Housing / income (%)	28.0	21.3	21.8	17.9
Utility / income (%)	11.1	8.1	8.3	6.5
Bills / income (%)	44.5	34.6	34.7	29.5
Nondurable / income (%)	24.6	18.7	19.7	17.0
Post-period Characteristics				
Behind 2+ months (%)	9.5	0.0	2.0	0.0
Eviction notice (%)	2.9	0.0	0.8	0.0
Move (%)	9.2	0.0	3.9	0.0
Δ housing (%)	-6.1	-1.2	-1.1	-0.9
Δ utility (%)	-1.6	-2.9	2.3	0.1
Δ bills (%)	-4.6	-2.4	-0.1	-0.7
Δ nondurable (%)	-4.5	-6.6	3.4	0.4
Δ total income (%)	-23.7	-22.5	2.9	0.3
Observations	452		19,795	

SIPP data are repeated annual cross-sections of households from the 1991-2008 SIPP panels, and ALP data are quarterly observations of households from 2008-2016. See text for sample restrictions. Means and medians are separately reported for the sample of households that did and did not experience job loss in the prior year (SIPP) or in quarter t (ALP). Dollar values are deflated to 2014$. In Panel A, monthly income is the households' monthly income for the month preceding the twelve-month period over which job loss and missed housing payments are assessed. In Panel B, pre-period characteristics report the average from quarters $t-3$ to $t-1$ and post-period characteristics are reported for quarters t to $t+1$. To reduce the influence of outliers, the ratios relative to income are winsorized at the 1st- and 99th-percentile, and the Δ percent changes are winsorized at 100%.

Table 2: Frequency of Missed Housing Payments after Job Loss

	Dependent Variable: Missed Housing Payment						
	OLS	OLS	OLS	IV	IV	OLS	OLS
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: All Respondents							
Job loss	0.096***	0.079***	0.078***	0.098***	0.075***	0.097***	0.076***
	(0.006)	(0.006)	(0.006)	(0.011)	(0.014)	(0.010)	(0.012)
Mean Dep.	0.06	0.06	0.06	0.06	0.06	0.055	0.054
N Job loss	4,465	4,465	4,465	4,465	4,465	1,558	1,177
Observations	55,553	55,553	55,553	55,553	55,553	52,646	52,265
Panel B: Renters							
Job loss	0.117***	0.094***	0.093***	0.126***	0.100***	0.123***	0.100***
	(0.011)	(0.010)	(0.010)	(0.015)	(0.017)	(0.014)	(0.015)
Mean Dep.	0.096	0.096	0.096	0.096	0.096	0.089	0.087
N Job loss	1,796	1,796	1,796	1,796	1,796	595	427
Observations	17,473	17,473	17,473	17,473	17,473	16,272	16,104
Panel C: Owners with Mortgage							
Job loss	0.075***	0.065***	0.064***	0.078***	0.059***	0.077***	0.060***
	(0.007)	(0.007)	(0.007)	(0.014)	(0.016)	(0.013)	(0.015)
Mean Dep.	0.043	0.043	0.043	0.043	0.043	0.04	0.039
N Job loss	2,669	2,669	2,669	2,669	2,669	963	750
Observations	38,080	38,080	38,080	38,080	38,080	36,374	36,161
Controls							
Demo. & Financial:		X	X	X	X	X	X
State Economic:			X	X	X	X	X
Year FE	X	X	X	X	X	X	X
State FE			X	X	X	X	X
IV				Invol. 1	Invol. 2		
Sample Restriction						Invol. 1	Invol. 2
Note:					*p<0.1; **p<0.05; ***p<0.01		

Data are cross-sectional observations of households from the 1991-2008 SIPP panels with one observation per household. Estimates are from equation (2). Column (4) instruments for job loss using the indicator Involuntary Job Loss 1, which equals one if the job loss was caused by layoff, illness or injury, being discharged or fired, employer bankruptcy, or sale of the business. Column (5) instruments for job loss using the indicator Involuntary Job Loss 2, which equals one if the job loss was caused by employer bankruptcy, sale of the business, or layoffs. 1^{st}-stage F-statistics for the IVs all exceed 500. Columns (6) and (7) drop job losses that do not satisfy the definitions of Involuntary Job Loss 1 and Involuntary Job Loss 2, respectively. Demographic controls for the household head include age, marital status, race (Black), ethnicity, education group, pre-period household income, and changes in household size and marital status. Financial controls include liquid assets, total household net worth, unsecured debt, and housing payments as a share of monthly baseline household income. State economic controls include the unemployment rate, max unemployment benefits, log of real GDP per capita, and average wages, all from Hsu et al. (2018), as well as the unemp. rate and max benefits interacted with the unemployment indicator.

Table 3: Changes in Expenditures after Job Loss

	Dependent variable:				
	Δincome (%)	Behind housing 2+ months	Δhousing (%)	Δbills (%)	Δnondurable (%)
	(1)	(2)	(3)	(4)	(5)
Panel A: All respondents					
Job loss	-0.263^{***}	0.075^{***}	-0.048^{***}	-0.043^{***}	-0.080^{***}
	(0.018)	(0.014)	(0.014)	(0.013)	(0.017)
N spells:	452	452	452	452	452
Observations	20,239	20,247	20,247	20,247	20,247
Panel B: Renters					
Job loss	-0.294^{***}	0.079^{***}	-0.075^{***}	-0.065^{***}	-0.076^{**}
	(0.033)	(0.024)	(0.025)	(0.025)	(0.032)
N spells:	170	170	170	170	170
Observations	5,219	5,222	5,222	5,222	5,222
Panel C: Owners					
Job loss	-0.246^{***}	0.069^{***}	-0.035^{**}	-0.032^{**}	-0.084^{***}
	(0.021)	(0.017)	(0.016)	(0.014)	(0.020)
N spells:	282	282	282	282	282
Observations	15,020	15,025	15,025	15,025	15,025
Fixed-effects					
Year-quarter	Yes	Yes	Yes	Yes	Yes

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

Data are quarterly, household-level observations from the RAND American Life Panel Financial Crisis Surveys from 2009-2016. The dependent variables are the percentage change in household income (column 1), an indicator for being behind 2+ months of housing payments in quarters t or $t+1$ (column 2), the percentage change between the pre- and post-period in monthly housing payments (column 3), monthly bill payments (sum of housing, utilities, and auto payments) (column 4), and monthly nondurable expenditure (column 5) from the pre-period. The pre-period is the average monthly expenditure in quarters $t-3$ through $t-1$, and the post-period is the average monthly expenditure in quarters t and $t+1$. Percentage changes are truncated at -100% and 100%.

Table 4: Changes in Expenditures after Job Loss (Quantiles)

	Quantile Treatment Effect					
	All		Renters		Owners	
Quantile	housing (1)	nondurable (2)	housing (3)	nondurable (4)	housing (5)	nondurable (6)
0.05	-0.202**	-0.12***	-0.341***	-0.158*	-0.152	-0.113***
	(0.083)	(0.037)	(0.104)	(0.082)	(0.101)	(0.034)
0.1	-0.164***	-0.13***	-0.315***	-0.147***	-0.104**	-0.103***
	(0.048)	(0.025)	(0.082)	(0.055)	(0.043)	(0.03)
0.15	-0.153***	-0.118***	-0.269***	-0.133***	-0.081**	-0.108***
	(0.036)	(0.019)	(0.069)	(0.04)	(0.039)	(0.019)
0.2	-0.105***	-0.126***	-0.222***	-0.124***	-0.055*	-0.116***
	(0.033)	(0.016)	(0.067)	(0.035)	(0.029)	(0.018)
0.3	-0.037**	-0.107***	-0.046	-0.098**	-0.034**	-0.106***
	(0.015)	(0.018)	(0.039)	(0.038)	(0.015)	(0.021)
0.4	-0.005	-0.094***	0	-0.07*	-0.011*	-0.099***
	(0.005)	(0.018)	(0.008)	(0.039)	(0.006)	(0.02)
0.5	0	-0.075***	-0.001	-0.047	-0.001	-0.09***
	(0)	(0.015)	(0.001)	(0.029)	(0.002)	(0.017)
0.75	-0.008*	-0.057***	-0.011	-0.044	-0.006	-0.074***
	(0.005)	(0.021)	(0.007)	(0.042)	(0.006)	(0.025)
0.95	-0.005	0.019	0.024	-0.047	-0.024	-0.026
	(0.076)	(0.097)	(0.15)	(0.097)	(0.07)	(0.112)

Data are quarterly, household-level observations from the RAND American Life Panel Financial Crisis Surveys from 2009-2016. The table reports results from quantile regressions of equation (3). Each cell reports the coefficient from a separate regression, with the row showing the quantile and the column showing the dependent variable. The sample in columns 1-2 includes all respondents, columns 3 and 4 are restricted to renters, and columns 5 and 6 to owners. Bootstrap standard errors from 500 replications are reported. *Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Table 5: Impact of Unemployment on Eviction

	Dependent Variable: Evicted for Nonpayment						
	OLS	OLS	OLS	IV	IV	OLS	OLS
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: All Respondents							
Job loss	0.005***	0.004***	0.004***	0.005**	0.002	0.005**	0.002
	(0.001)	(0.001)	(0.001)	(0.002)	(0.002)	(0.002)	(0.002)
Mean Dep.	0.002	0.002	0.002	0.002	0.002	0.002	0.002
N Job loss	4,465	4,465	4,465	4,465	4,465	1,558	1,177
Observations	55,543	55,543	55,543	55,543	55,543	52,636	52,255
Panel B: Renters							
Job loss	0.010***	0.009***	0.009***	0.011**	0.002	0.011**	0.003
	(0.003)	(0.003)	(0.003)	(0.005)	(0.004)	(0.004)	(0.003)
Mean Dep.	0.004	0.004	0.004	0.004	0.004	0.004	0.003
N Job loss	1,796	1,796	1,796	1,796	1,796	595	427
Observations	17,470	17,470	17,470	17,470	17,470	16,269	16,101
Panel C: Owners with Mortgage							
Job loss	0.001	0.001	0.001	0.002	0.001	0.002	0.001
	(0.001)	(0.001)	(0.001)	(0.002)	(0.002)	(0.002)	(0.002)
Mean Dep.	0.001	0.001	0.001	0.001	0.001	0.001	0.001
N Job loss	2,669	2,669	2,669	2,669	2,669	963	750
Observations	38,073	38,073	38,073	38,073	38,073	36,367	36,154
Controls							
Demo. & Financial:		X	X	X	X	X	X
State Economic:			X	X	X	X	X
Year FE	X	X	X	X	X	X	X
State FE			X	X	X	X	X
IV				Invol. 1	Invol. 2		
Sample Restriction						Invol. 1	Invol. 2
Note:					*p<0.1; **p<0.05; ***p<0.01		

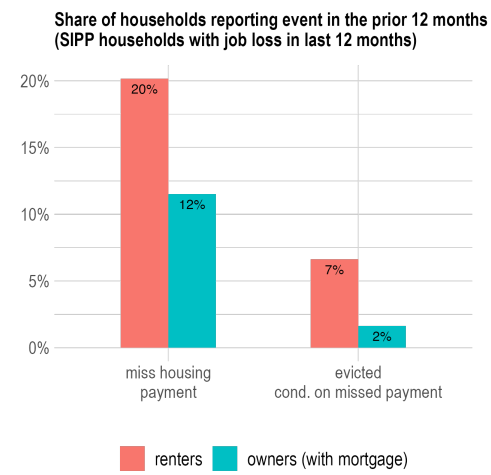
Data are repeated cross-sections of households from the 1991-2008 SIPP panels. The specifications and controls match the description in Table 2.

Table 6: Impact of Missed Payment on Moving Out

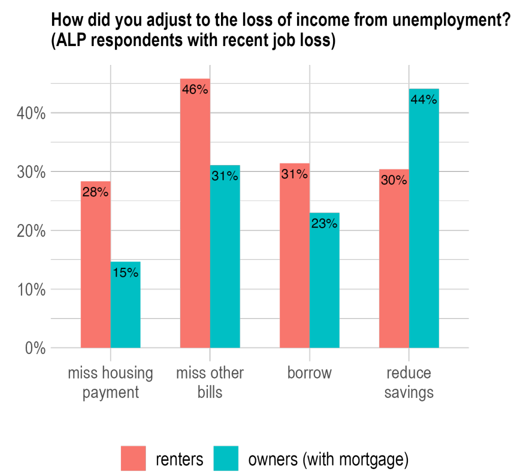
	Dependent Variable: Moved within 12 Months of Job Loss					
Model:	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: All Respondents						
Missed Payment	0.062***	0.036**	0.036**	0.035**	-0.003	-0.012
	(0.016)	(0.016)	(0.016)	(0.016)	(0.036)	(0.034)
Observations	3,206	3,206	3,206	3,206	1,102	827
Mean of Dep. Var	0.142	0.142	0.142	0.142	0.143	0.133
Panel B: Renters						
Missed Payment	-0.010	0.014	0.029	0.026	-0.012	-0.051
	(0.026)	(0.024)	(0.025)	(0.024)	(0.056)	(0.059)
Observations	1,157	1,157	1,157	1,157	391	286
Mean of Dep. Var	0.296	0.296	0.296	0.296	0.284	0.269
Panel C: Owners						
Missed Payment	0.049**	0.049**	0.046**	0.046**	0.010	0.009
	(0.020)	(0.020)	(0.019)	(0.019)	(0.031)	(0.032)
Observations	2,049	2,049	2,049	2,049	711	541
Mean of Dep. Var	0.055	0.055	0.055	0.055	0.066	0.061
Sample Restriction					Invol. 1	Invol 2.
Controls						
Demographic		X	X	X	X	X
Financial		X	X	X	X	X
State Economic				X	X	X
Fixed-effects						
Year	X	X	X	X	X	X
State			X	X	X	X

Clustered (State) standard errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Data are repeated cross-sections of households from the 1991-2008 SIPP panels. The analysis sample is restricted to households that remain in the sample for at least 12 months after the observed job loss, and did not change residence in the four months prior to job loss. Columns 5 and 6 restrict the sample to those with involuntary job losses, following the definitions in Table 2. The controls match the description in Table 2.



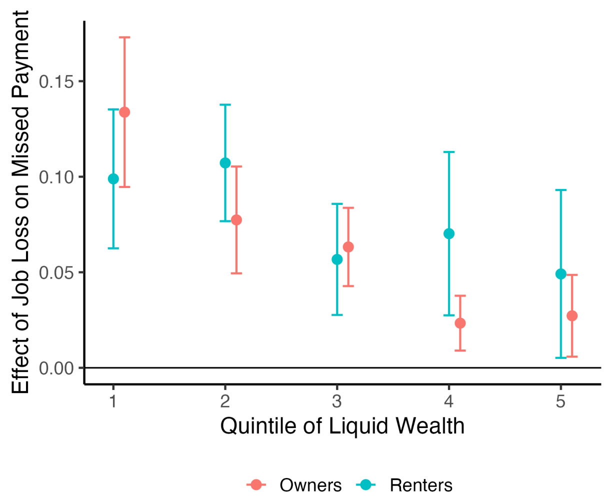
(a) SIPP



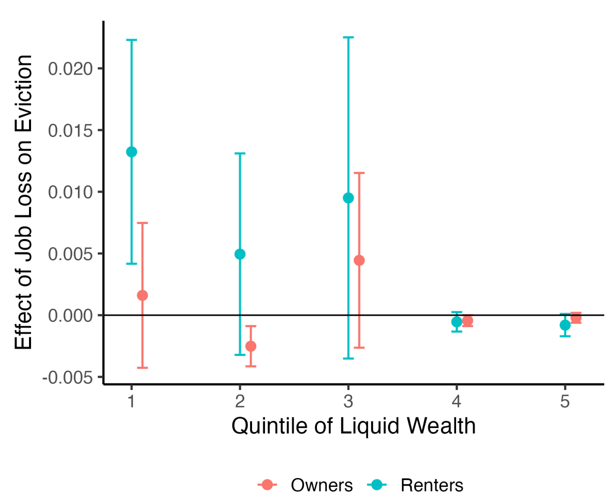
(b) ALP

Figure 1: **How households cope with income loss from unemployment**

Data are from the 1991-2008 SIPP panels and the ALP Financial Crisis Surveys. Both SIPP and ALP households are restricted to renters or owners with a mortgage. Panel (a) reports the share of job-losing households that report missed housing payments or eviction. Panel (b) reports the percentage of job-losing households that report adjusting to the loss of income by falling behind on housing payments, postponing other bills, increasing debt, or reducing savings (multiple responses allowed). Both figures are weighted to be nationally representative and drop missing observations. The ALP sample restricted observations after wave 8, when borrowing was added to the list of possible responses.



(a) Missed Payment



(b) Eviction

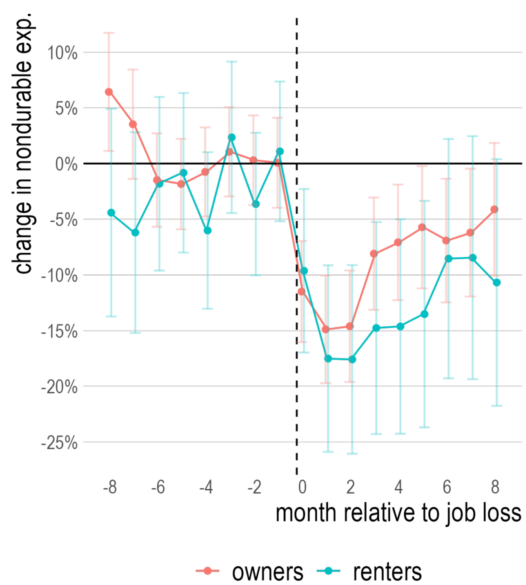
Figure 2: **Heterogeneity in the Effect of Job Loss by Liquid Assets**

Data are repeated cross-sections of households from the 1991-2008 SIPP panels. Figure reports point estimates and 95% confidence intervals for the interaction of the `job loss` indicator with quintiles of liquid wealth included within equation (2). I do not include the uninteracted `job loss` indicator, so obtain estimates for each of the five quintiles. Other controls include those in Table 2 column (3), but dropping the additional control for liquid assets.

(a) Total Income

(b) Housing

(c) Bills



(d) Nondurables

Figure 3: **Changes in Expenditure upon Job Loss**

Data are monthly, household-level observations from the RAND American Life Panel Financial Crisis Surveys from May 2009-April 2013 (waves 1-50). Estimates are from the local projections difference-in-differences specification in equation (4). Error bars show 95% confidence intervals.

(a) Renters

(b) Owners

Figure 4: **Move Rates by Missed Payment Status**

Data are repeated cross-sections of households from the 1991-2008 SIPP panels. The figure shows the cumulative share of households that have changed residence following job loss in month 0. The sample consists of households that experienced job loss. I also restrict the sample to to households (i) that did not move in the four months prior to job loss, and (ii) for which the member losing a job lived in the household for at least four months before the spell. Appendix Figure A.3 shows the figure without the sample restrictions (i) and (ii).

Landlords as Lenders of Last Resort? Late Housing Payments During Unemployment

Online Appendices

Nathaniel Pattison

Appendix A. Tables and Figures

Table A.1: Summary Statistics: Renters vs. Owners

	Renters		Owners	
	Mean	Median	Mean	Median
Panel A: Survey of Income and Program Participation				
Missed Payments and Eviction				
Unemp within household (%)	10.3	0.0	7.0	0.0
Missed housing payment (%)	9.6	0.0	4.3	0.0
Eviction in prior 12 months (%)	0.4	0.0	0.1	0.0
Demographic Characteristics				
Homeowner (%)	0.0	0.0	100.0	100.0
Annual HH income ($1,000s)	57.1	47.0	99.4	83.7
Age	37.2	35.0	43.5	43.0
Married (%)	37.0	0.0	72.8	100.0
Household size	2.4	2.0	3.0	3.0
Race: Black (%)	14.1	0.0	6.9	0.0
Ethnicity: Hispanic (%)	13.2	0.0	5.9	0.0
Educ.: Less Than HS (%)	12.1	0.0	5.4	0.0
Educ.: HS (%)	24.9	0.0	22.5	0.0
Educ.: Some College (%)	34.2	0.0	33.5	0.0
Educ.: College (%)	18.9	0.0	23.5	0.0
Educ.: Grad School (%)	9.9	0.0	15.2	0.0
Financial Characteristics				
Liquid assets ($1,000s)	12.0	0.8	48.9	5.6
Liquid assets / monthly income	2.4	0.2	6.1	0.8
Has unsecured debt (%)	62.3	100.0	72.0	100.0
Unsecured debt ($1,000s)	9.9	1.5	13.8	3.0
Unsecured debt / monthly income	3.7	0.3	2.8	0.4
Housing costs ($1,000s)	1.1	1.0	1.5	1.4
Housing costs / monthly income	0.4	0.2	0.3	0.2
Observations	17,473		38,080	
Panel B: RAND ALP Financial Crisis Surveys				
Pre-period Characteristics				
Owners (%)	0.0	0.0	100.0	100.0
Monthly income ($1,000s)	4.5	3.4	8.7	6.7
Credit card debt ($1,000s)	3.5	0.0	6.7	0.8
Monthly Housing exp. ($1,000s)	1.6	0.8	1.4	1.1
Housing / income (%)	29.8	23.0	19.5	16.5
Utility / income (%)	10.4	7.3	7.9	6.4
Bills / income (%)	46.0	36.8	31.6	27.8
Nondurable / income (%)	25.5	19.7	18.3	16.2
Post-period Characteristics				
Behind 2+ months (%)	2.9	0.0	1.9	0.0
Eviction notice (%)	0.4	0.0	1.0	0.0
Move (%)	10.3	0.0	1.9	0.0
Δ housing (%)	0.2	-0.5	-1.7	-1.0
Δ utility (%)	2.7	-0.0	2.1	0.1
Δ bills (%)	1.1	0.1	-0.7	-1.1
Δ nondurable (%)	3.0	-0.4	3.3	0.4
Δ total income (%)	2.3	0.4	2.2	0.1
Observations	5,222		15,025	

SIPP data are repeated annual cross-sections of households from the 1991-2008 SIPP panels, and ALP data are quarterly observations of households. See text for sample restrictions. Means and medians are separately reported for the sample of households that did and did not experience job loss in the prior year. Dollar values are deflated to 2014$. In Panel A, monthly income is the households' monthly income for the month preceding the twelve-month period over which job loss and missed housing payments are assessed. In Panel B, pre-period characteristics report the average from period $t-3$ to $t-1$ and post-period characteristics are reported for quarters t to $t+1$. To reduce the influence of outliers, the ratios relative to income are winsorized at the 1st- and 99th-percentile, and the Δ percent changes are winsorized at 100%.

Table A.2: Frequency of Missed Housing Payments:
Controlling for Number of Prior Spells

	Dependent Variable: Missed Housing Payment						
	OLS	OLS	OLS	IV	IV	OLS	OLS
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: All Respondents							
Job loss	0.089***	0.074***	0.074***	0.095***	0.072***	0.093***	0.072***
	(0.006)	(0.006)	(0.006)	(0.011)	(0.014)	(0.010)	(0.012)
Mean Dep.	0.06	0.06	0.06	0.06	0.06	0.055	0.054
N Job loss	4,465	4,465	4,465	4,465	4,465	1,558	1,177
Observations	55,553	55,553	55,553	55,553	55,553	52,646	52,265
Panel B: Renters							
Job loss	0.106***	0.088***	0.087***	0.121***	0.093***	0.117***	0.093***
	(0.011)	(0.010)	(0.010)	(0.014)	(0.017)	(0.013)	(0.015)
Mean Dep.	0.096	0.096	0.096	0.096	0.096	0.089	0.087
N Job loss	1,796	1,796	1,796	1,796	1,796	595	427
Observations	17,473	17,473	17,473	17,473	17,473	16,272	16,104
Panel C: Owners with Mortgage							
Job loss	0.071***	0.062***	0.062***	0.076***	0.057***	0.074***	0.058***
	(0.007)	(0.007)	(0.007)	(0.014)	(0.016)	(0.013)	(0.015)
Mean Dep.	0.043	0.043	0.043	0.043	0.043	0.04	0.039
N Job loss	2,669	2,669	2,669	2,669	2,669	963	750
Observations	38,080	38,080	38,080	38,080	38,080	36,374	36,161
Controls							
Demo. & Financial:		X	X	X	X	X	X
State Economic:			X	X	X	X	X
Year FE	X	X	X	X	X	X	X
State FE			X	X	X	X	X
Job loss FE	X	X	X	X	X	X	X
IV				Invol. 1	Invol. 2		
Sample Restriction						Invol. 1	Invol. 2
Note:					*p<0.1; **p<0.05; ***p<0.01		

This table repeats the regressions in Table 2, but adds indicators for the number of prior household unemployment spells in the SIPP, following (Gerardi et al., 2018).

Table A.3: Robustness to Sample of Job Losers

Dependent Variables:	Missed housing payment			Evicted for nonpayment		
Sample:	Baseline	Exogenous Job Loss	No unemp. $t-1$	Baseline	Exogenous Job Loss	No unemp. $t-1$
Model:	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: All Respondents						
Job loss	0.079***	0.079***	0.078***	0.004***	0.003***	0.002
	(0.006)	(0.005)	(0.007)	(0.001)	(0.001)	(0.001)
Observations	55,553	53,888	50,027	55,543	53,878	50,018
Panel B: Renters						
Job loss	0.093***	0.097***	0.088***	0.009***	0.007**	0.002
	(0.010)	(0.011)	(0.010)	(0.003)	(0.003)	(0.002)
Observations	17,473	16,792	15,141	17,470	16,789	15,138
Panel C: Owners with Mortgage						
Job loss	0.064***	0.063***	0.069***	0.0006	0.0005	0.001
	(0.007)	(0.007)	(0.009)	(0.0010)	(0.001)	(0.002)
Observations	38,080	37,096	34,886	38,073	37,089	34,880
Controls						
Demographic	Yes	Yes	Yes	Yes	Yes	Yes
Financial	Yes	Yes	Yes	Yes	Yes	Yes
State Economic	Yes	Yes	Yes	Yes	Yes	Yes
Fixed-effects						
Year	Yes	Yes	Yes	Yes	Yes	Yes
State	Yes	Yes	Yes	Yes	Yes	Yes

Clustered (State) standard errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

This table investigates the robustness of the estimates to restricting the sample to job losses which are more likely to be exogenous. Data are repeated cross-sections of households from the 1991-2008 SIPP panels. The controls match the description for column 5 in Table 2. Columns 1 and 4 report the baseline estimates from Tables 2 and 5. Columns 2 and 3 restrict the sample to job losses where the person either received unemployment insurance, or, following Sullivan (2008), reported the reasons for job loss as layoff, illness or injury, discharged or fired, employer bankruptcy or sale of the business. Columns 3 and 6 retain these restrictions on job losses, but also require all sample households to have experienced no unemployment during full year $t-1$, the year prior to the reference period over which missed payment and eviction are assessed. In contrast, the main sample in column 1 is restricted to households with no job loss in the four months prior to the reference period.

Table A.4: Bias-adjusted Treatment Effect Estimates

	(1)	(2)	(3)	(4)
	Baseline Estimate	$R^2_{\texttt{max}} = 1.3\widetilde{R}^2$	$R^2_{\texttt{max}} = 1.5\widetilde{R}^2$	$R^2_{\texttt{max}} = 2\widetilde{R}^2$
Panel A: Missed Housing Payment				
All	0.078	0.071	0.066	0.053
Renters	0.093	0.083	0.076	0.059
Owners	0.064	0.059	0.056	0.048
Panel B: Evicted for Nonpayment				
All	0.004	0.004	0.004	0.003
Renters	0.009	0.008	0.008	0.007
Owners	0.001	0.001	0.000	0.000
Panel C: Moved within 12 Months of Job Loss				
All	0.035	0.026	0.019	0.002
Renters	0.026	0.039	0.048	0.072
Owners	0.046	0.045	0.044	0.042

The table shows the robustness of the SIPP estimates after adjusting for bias following Oster (2019) under the assumption of proportional selection ($\delta = 1$). The table reports the sensitivity of the impact of job loss on missed payments from Table 2 in Panel A, the impact of job loss on evictions from Table 5 in Panel B, and the impact of missed payments on moving from Table 6 in Panel C. Column (1) reports the unadjusted baseline from column (3) of each table. Column (2) reports the bias-adjustment under the recommendations of Oster (2019), which sets the maximum R-squared value for a hypothetical regression on all observables $R^2_{\texttt{max}} = 1.3\widetilde{R}^2$, where $\widetilde{R}^2$ is the R-squared value from Table 6 column (5). Columns (3) and (4) report bias-adjusted estimates under alternative assumptions about $R^2_{\texttt{max}}$.

Table A.5: Changes in Expenditures after Job Loss (Balanced Panel)

	Dependent variable:				
	Δincome (%)	Behind housing 2+ months	Δhousing (%)	Δbills (%)	Δnondurable (%)
	(1)	(2)	(3)	(4)	(5)
Panel A: All respondents					
Job loss	−0.248***	0.077***	−0.054***	−0.043***	−0.073***
	(0.020)	(0.017)	(0.014)	(0.012)	(0.019)
N spells:	299	299	299	299	299
Observations	14,640	14,640	14,640	14,640	14,640
Panel B: Renters					
Job loss	−0.302***	0.083***	−0.066***	−0.042*	−0.080**
	(0.040)	(0.031)	(0.024)	(0.023)	(0.037)
N spells:	95	95	95	95	95
Observations	3,306	3,306	3,306	3,306	3,306
Panel C: Owners					
Job loss	−0.225***	0.072***	−0.051***	−0.046***	−0.071***
	(0.023)	(0.020)	(0.017)	(0.015)	(0.023)
N spells:	204	204	204	204	204
Observations	11,334	11,334	11,334	11,334	11,334
Fixed-effects					
Year-quarter	Yes	Yes	Yes	Yes	Yes

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

This table repeats Table 3, but restricts the ALP sample to respondents with no missing observations in quarters $t-3$ to $t+1$.

Table A.6: Changes in Expenditures after Job Loss ($log(x+1)$)

	Dependent variable:				
	Δincome	Behind housing 2+ months	Δhousing	Δbills	Δnondurable
	(1)	(2)	(3)	(4)	(5)
Panel A: All respondents					
Job loss	-0.311^{***}	0.075^{***}	-0.057^{***}	-0.052^{***}	-0.096^{***}
	(0.020)	(0.014)	(0.015)	(0.014)	(0.018)
N spells:	452	452	452	452	452
Observations	20,239	20,247	20,247	20,247	20,247
Panel B: Renters					
Job loss	-0.353^{***}	0.079^{***}	-0.092^{***}	-0.078^{***}	-0.092^{***}
	(0.037)	(0.024)	(0.027)	(0.025)	(0.033)
N spells:	170	170	170	170	170
Observations	5,219	5,222	5,222	5,222	5,222
Panel C: Owners					
Job loss	-0.287^{***}	0.069^{***}	-0.039^{**}	-0.039^{**}	-0.097^{***}
	(0.024)	(0.017)	(0.017)	(0.016)	(0.021)
N spells:	282	282	282	282	282
Observations	15,020	15,025	15,025	15,025	15,025
Fixed-effects					
Year-quarter	Yes	Yes	Yes	Yes	Yes

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

This table repeats Table 3, but replaces the dependent variable with the change in log expenditure (plus one) between the post- and pre-period.

Table A.7: Changes in Expenditures after Job Loss (Monthly Data)

	Dependent variable:				
	Δincome (%)	Behind housing 2+ months	Δhousing (%)	Δbills (%)	Δnondurable (%)
	(1)	(2)	(3)	(4)	(5)
Panel A: All respondents					
Job loss	-0.244^{***}	0.064^{***}	-0.068^{***}	-0.060^{***}	-0.127^{***}
	(0.021)	(0.016)	(0.017)	(0.014)	(0.020)
N spells:	297	297	297	297	297
Observations	29,467	29,475	29,475	29,475	29,475
Panel B: Renters					
Job loss	-0.259^{***}	0.099^{***}	-0.116^{***}	-0.075^{**}	-0.147^{***}
	(0.045)	(0.033)	(0.033)	(0.030)	(0.040)
N spells:	93	93	93	93	93
Observations	6,401	6,402	6,402	6,402	6,402
Panel C: Owners					
Job loss	-0.238^{***}	0.047^{***}	-0.047^{**}	-0.053^{***}	-0.117^{***}
	(0.024)	(0.017)	(0.020)	(0.015)	(0.022)
N spells:	204	204	204	204	204
Observations	23,066	23,073	23,073	23,073	23,073
Fixed-effects					
Year-quarter	Yes	Yes	Yes	Yes	Yes

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

This table repeats Table 3, but uses the monthly rather than quarterly data from the ALP. The pre-period is the average monthly expenditure in months $t-6$ through $t-1$, and the post-period is the average monthly expenditure in months t and $t+1$.

Table A.8: Changes in Expenditures after Job Loss (Dollars)

	Dependent variable:				
	Δincome ($)	Behind housing 2+ months	Δhousing ($)	Δbills ($)	Δnondurable ($)
	(1)	(2)	(3)	(4)	(5)
Panel A: All respondents					
Job loss	−1,401.792***	0.075***	−46.237***	−69.918***	−79.936***
	(140.600)	(0.014)	(14.232)	(19.227)	(18.686)
N spells:	452	452	452	452	452
Observations	20,239	20,247	20,247	20,247	20,247
Panel B: Renters					
Job loss	−1,025.966***	0.079***	−63.815***	−83.473***	−67.767**
	(149.503)	(0.024)	(22.573)	(30.791)	(27.438)
N spells:	170	170	170	170	170
Observations	5,219	5,222	5,222	5,222	5,222
Panel C: Owners					
Job loss	−1,628.577***	0.069***	−40.364**	−66.108***	−86.761***
	(205.036)	(0.017)	(18.349)	(24.719)	(24.915)
N spells:	282	282	282	282	282
Observations	15,020	15,025	15,025	15,025	15,025
Fixed-effects					
Year-quarter	Yes	Yes	Yes	Yes	Yes

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

This table repeats Table 3, but replaces the dependent variable with changes in dollars of expenditure between the pre-period and the post-period (instead of percentage changes).

Table A.9: Missed Payments and Eviction by Reason for Job Loss

Reason for Job Loss	Frequency	Sh. Missed Rent	Sh. Evicted	Evicted Cond. Missed
Employer Bankruptcy/Sale	99	0.152	0.000	0.000
Fired/Discharged	344	0.224	0.015	0.065
Illness/Injury	37	0.351	0.054	0.154
Layoff	1078	0.142	0.005	0.033

Data are repeated cross-sections of households from the 1991-2008 SIPP panels. The sample is job losing households that experienced involuntary unemployment following the Involuntary 1 definition, based on Sullivan (2008). The Involuntary 2 definition further restricts the job losses to those caused by Layoffs or Employer Bankruptcy/Sale.

Table A.10: Impact of Missed Payment on Moving Out (Including Prior Moves)

	Dependent Variable: Moved within 12 Months of Job Loss					
Model:	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: All Respondents						
Missed Payment	0.061***	0.024	0.024	0.025	-0.016	-0.040
	(0.019)	(0.019)	(0.018)	(0.018)	(0.049)	(0.048)
Observations	3,511	3,511	3,511	3,511	1,190	879
R^2	0.00464	0.12459	0.14644	0.14905	0.15438	0.19974
Panel B: Renters						
Missed Payment	-0.032	0.0005	0.008	0.007	-0.054	-0.105
	(0.029)	(0.028)	(0.028)	(0.028)	(0.070)	(0.077)
Observations	1,404	1,404	1,404	1,404	464	328
R^2	0.00263	0.08719	0.13831	0.14890	0.20772	0.31246
Panel C: Owners						
Missed Payment	0.041*	0.037*	0.035*	0.037*	0.008	0.0001
	(0.022)	(0.022)	(0.020)	(0.020)	(0.033)	(0.037)
Observations	2,107	2,107	2,107	2,107	726	551
R^2	0.00577	0.04231	0.07806	0.08023	0.09314	0.15793
Sample Restriction					Invol. 1	Invol 2.
Controls						
Demographic		X	X	X	X	X
Financial		X	X	X	X	X
State Economic				X	X	X
Fixed-effects						
Year	X	X	X	X	X	X
State			X	X	X	X

Clustered (State) standard errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Data are repeated cross-sections of households from the 1991-2008 SIPP panels. The analysis sample is restricted to households that remain in the sample for at 12-months after the observed job loss. Relative to Table 6, these regressions also count moves that occur in the four months *before* job loss. The controls match the description in Table 2.

Table A.11: Impact of Missed Payment on Evictions

	Dependent Variable: Evicted					
Model:	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: All Respondents						
Missed Payment	0.043***	0.043***	0.043***	0.043***	0.045***	0.031**
	(0.008)	(0.008)	(0.008)	(0.008)	(0.012)	(0.013)
Observations	4,382	4,382	4,382	4,382	1,537	1,163
R^2	0.03763	0.04016	0.05284	0.05423	0.09041	0.06193
Panel B: Renters						
Missed Payment	0.066***	0.067***	0.067***	0.066***	0.066***	0.039**
	(0.013)	(0.013)	(0.013)	(0.013)	(0.018)	(0.019)
Observations	1,763	1,763	1,763	1,763	590	425
R^2	0.05549	0.06215	0.09753	0.09974	0.15670	0.10075
Panel C: Owners						
Missed Payment	0.016**	0.015**	0.016*	0.016*	0.024	0.027
	(0.008)	(0.008)	(0.008)	(0.008)	(0.016)	(0.017)
Observations	2,619	2,619	2,619	2,619	947	738
R^2	0.01509	0.02114	0.04027	0.04320	0.09395	0.07667
Sample Restriction					Invol. 1	Invol 2.
Controls						
Demographic		X	X	X	X	X
Financial		X	X	X	X	X
State Economic				X	X	X
Fixed-effects						
Year	X	X	X	X	X	X
State			X	X	X	X

Clustered (State) standard errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Data are repeated cross-sections of households from the 1991-2008 SIPP panels. The analysis sample is restricted to households that remain in the sample for at 12-months after the observed job loss. The controls match the description in Table 2.

Table A.12: Frequency of Missed Housing Payments after Job Loss

	Dependent Variable: Missed Housing Payment				
Model:	(1)	(2)	(3)	(4)	(5)
Panel A: Renters					
Job loss (JL)	0.077***	0.088***	0.090***	0.073***	0.074***
	(0.012)	(0.012)	(0.014)	(0.016)	(0.021)
JL×high notice	0.036*			0.036*	
	(0.021)			(0.021)	
JL×high fee		0.006		0.005	
		(0.021)		(0.019)	
JL×judicial forecl.			0.003	0.003	
			(0.020)	(0.018)	
JL×1(< 5 units)					0.034
					(0.032)
1(< 5 units)					0.012*
					(0.007)
Observations	17,473	17,473	17,473	17,473	7,077
Panel B: Owners with Mortgage					
Job loss (JL)	0.062***	0.051***	0.058***	0.048***	-0.027***
	(0.011)	(0.006)	(0.008)	(0.011)	(0.010)
JL×high notice	0.0001			-0.002	
	(0.014)			(0.014)	
JL×high fee		0.019		0.019	
		(0.013)		(0.013)	
JL×judicial forecl.			0.008	0.007	
			(0.015)	(0.015)	
JL×1(< 5 units)					0.081***
					(0.013)
1(< 5 units)					0.009
					(0.010)
Observations	38,080	38,080	38,080	38,080	13,820
Controls					
Demographic	Yes	Yes	Yes	Yes	Yes
Financial	Yes	Yes	Yes	Yes	Yes
State Economic	Yes	Yes	Yes	Yes	Yes
Fixed-effects					
Year	Yes	Yes	Yes	Yes	Yes
State	Yes	Yes	Yes	Yes	Yes

Clustered (State) standard errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Data are repeated cross-sections of households from the 1991-2008 SIPP panels. Demographic, financial, and state economic controls match the description in Table 2. The first two columns examine state laws that make evictions more difficult. Column 1 interacts the job loss indicator with *high notice*, an indicator for whether the state has above-median eviction notice requirements (median is 5 days). Column 2 interacts job loss with high fee, an indicator for whether the state has above-median eviction filing fees. Column 3 examines state foreclosure requirements, interacting job loss with an indicator for whether judicial foreclosures are required, a process that increases the time and monetary costs for lenders to foreclose. Column 4 includes all three interactions. Data on eviction notice laws and fees are from Gromis et al. (2022), with county-level fee data aggregated to the state level using population weights. Data on judicial foreclosure states are from Feinstein (2018). Column 5 examines heterogeneity by whether the respondent lives in a property with fewer than five units. For renters, these smaller properties are typically owned by mom-and-pop landlords, who tend to be more lenient, while larger properties are owned by institutional landlords, who tend to be stricter (Cororaton, 2020; Balzarini and Boyd, 2021).

Table A.13: Frequency of Eviction after Job Loss

	Dependent Variable: Evicted for Nonpayment				
Model:	(1)	(2)	(3)	(4)	(5)
Panel A: Renters					
Job loss (JL)	0.006**	0.011**	0.007**	0.008*	0.009*
	(0.003)	(0.004)	(0.003)	(0.004)	(0.005)
JL×high notice	0.006			0.006	
	(0.006)			(0.006)	
JL×high fee		-0.004		-0.004	
		(0.006)		(0.006)	
JL×judicial forecl.			0.002	0.001	
			(0.005)	(0.005)	
JL×1(< 5 units)					-0.001
					(0.007)
1(< 5 units)					-0.001
					(0.001)
Observations	17,470	17,470	17,470	17,470	7,074
Panel B: Owners with Mortgage					
Job loss (JL)	0.002	0.002	0.0010	0.003	-9.77×10^{-6}
	(0.001)	(0.002)	(0.001)	(0.002)	(0.0005)
JL×high notice	-0.002			-0.002	
	(0.002)			(0.002)	
JL×high fee		-0.002		-0.001	
		(0.002)		(0.002)	
JL×judicial forecl.			-0.0008	-0.0008	
			(0.002)	(0.002)	
JL×1(< 5 units)					3.46×10^{-5}
					(0.0009)
1(< 5 units)					0.0005*
					(0.0003)
Observations	38,073	38,073	38,073	38,073	13,813
Controls					
Demographic	Yes	Yes	Yes	Yes	Yes
Financial	Yes	Yes	Yes	Yes	Yes
State Economic	Yes	Yes	Yes	Yes	Yes
Fixed-effects					
Year	Yes	Yes	Yes	Yes	Yes
State	Yes	Yes	Yes	Yes	Yes

Clustered (State) standard errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Data are repeated cross-sections of households from the 1991-2008 SIPP panels. Demographic, financial, and state economic controls match the description in Table 2. Additional controls and interactions match the description in Table A.12.

Table A.14: Correlation between Missed Payment and Income Shock

	Dependent variable:				
	Moved	Spell $\leq$ 3 months	Spell $\leq$ 6 months	$\Delta_3 log(income)$	$\Delta_6 log(income)$
	(1)	(2)	(3)	(4)	(5)
Panel A: All Respondents					
Missed Payment	0.035^{**}	-0.041^{*}	-0.047^{*}	-0.055	-0.085^{**}
	(0.016)	(0.022)	(0.024)	(0.044)	(0.040)
Observations	3,206	3,206	3,206	3,109	3,109
Panel B: Renters					
Missed Payment	0.026	-0.035	-0.028	-0.127^{*}	-0.147^{**}
	(0.024)	(0.028)	(0.041)	(0.065)	(0.060)
Observations	1,157	1,157	1,157	1,090	1,090
Panel C: Owners with Mortgage					
Missed Payment	0.046^{**}	-0.048	-0.065	-0.025	-0.063
	(0.019)	(0.035)	(0.039)	(0.050)	(0.049)
Observations	2,049	2,049	2,049	2,019	2,019
Controls					
Demo. & Financial:	X	X	X	X	X
State Economic:	X	X	X	X	X
Year FE	X	X	X	X	X
State FE	X	X	X	X	X

Note: *p<0.1; **p<0.05; ***p<0.01

Data are repeated cross-sections of households from the 1991-2008 SIPP panels. The analysis sample is restricted to households that remain in the sample for at 12-months after the observed job loss. In columns (2) and (3), the dependent variable is an indicator for whether the unemployment spell ended within 3 months or 6 months, respectively. Columns 4 and 5 show the change in monthly log household income relative to the baseline month (4 months prior to job loss). Specifically, $\Delta_j log(income) = log(\overline{y}_{0,j-1}) - log(y_{\text{base}})$ where $y_{0,j-1}$ is the average monthly income during the first j months after job loss, where $j = 3, 6$. The controls match the description in Table 2.

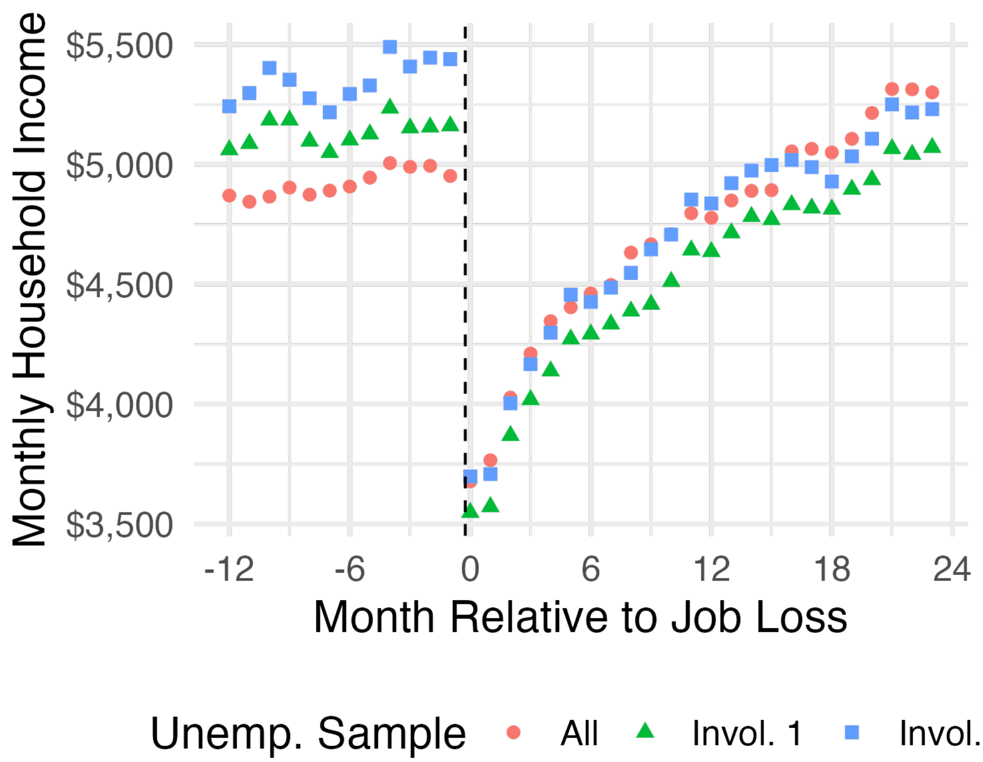


Figure A.1: **Change in Income Around Job Loss**

This figure shows the mean monthly household income around instances of job loss for the three samples of job-losing households. “All” includes all job-losing households in the main sample. Involuntary Job Loss 1 restricts the sample to job loss that was caused by layoff, illness or injury, being discharged or fired, employer bankruptcy, or sale of the business (Sullivan, 2008). Involuntary Job Loss 2 restricts the sample to job losses caused by employer bankruptcy, sale of the business, or layoffs (Gerardi et al., 2018).

(a) Renters

(b) Owners

Figure A.2: **Move or Attrition Rates by Missed Payment Status**

Data are repeated cross-sections of households from the 1991-2008 SIPP panels. The sample consists of households who experienced job loss and matches the sample from Figure 4. The graph shows the share of households that has either moved or left the sample.

(a) Renters

(b) Owners

Figure A.3: **Move Rates by Missed Payment Status**

Data are repeated cross-sections of households from the 1991-2008 SIPP panels. The sample consists of households who experienced job loss. This figure repeats Figure 4, but removes that figures sample restrictions: (i) the household did not move in the four months prior to job loss and (ii) the person who lost the job lived in the household for at least four months prior to job loss.

Appendix B. Theory

This section formally shows the effects of late payments within the consumption commitments model of Chetty and Szeidl (2007). After deriving these comparisons, I then show illustrative examples using a quantitative model.

I begin by introducing the original consumption commitments model of Chetty and Szeidl (2007). A household lives for T periods and consumes two goods: an adjustable good (f_t), such as food, and a committed good (x_t), such as housing.[35] Adjusting the committed good to x_t from x_{t-1} incurs a proportional adjustment cost kx_{t-1} with $k \geq 0$. The household chooses consumption of f_t and x_t in each period to maximize

$$E_0 \sum_{t=1}^{T} u(f_t, x_t)$$

with flow utility

$$u(f, x) = \frac{f^{1-\gamma_f}}{1-\gamma_f} + \mu \frac{x^{1-\gamma_x}}{1-\gamma_x}. \tag{B.1}$$

Assume that $\gamma_f > \gamma_x$, so that the consumer is more risk averse over adjustable goods and that, for simplicity, the discount factor and the interest rate are both zero. The household begins with exogenous housing x_0, chosen to be the level of housing that an uncommitted consumer ($k = 0$) would choose when facing no income shock ($Z = 0$, discussed below).

As in Chetty and Szeidl (2007), I make simplifying assumptions to focus the analysis on an income shock in period 1. Specifically, the household earns a steady income of y in periods $t = 1, \ldots, T-1$, but faces an income shock of size Z in period 1 so that $y_1 = y+Z$, and $y_t = y$ for $t \geq 2$. Second, while there is potentially a borrowing constraint in period 1 ($W_1 \geq \underline{W}$), there are no borrowing constraints in periods $2, \ldots, T$ other than the terminal condition $W_T = 0$. Additionally, $\beta = \frac{1}{1+r}$ and there are exogenous initial conditions on wealth, W_0, and housing, x_0. The purpose of these assumption is to ensure that consumption of f and x in periods $2, \ldots, T$ will be constant, so that the model can focus on the trade-offs between period 1, when the shock occurs, and these uniform future periods.

We can write the full consumer problem and the consumer's value function over lifetime

[35] Online Appendix B includes the derivations for this section and examples from a quantitative model.

wealth $W = Ty + Z$ as

$$\begin{aligned} v(W, x_0) = \max_{\{f_t, x_t\}} \quad & \sum_{t=1}^{T} u(f_t, x_t) \\ \text{s.t.} \quad & W_t = W_{t-1} + y_t - f_t - x_t - kx_{t-1} \cdot 1\{x_t \neq x_{t-1}\} \\ & W_0 = 0 \quad \text{(initial wealth)} \\ & W_T = 0 \quad \text{(terminal condition)} \\ & W_1 \geq \underline{W} \quad \text{(period 1 borrowing constraint)} \end{aligned} \tag{B.2}$$

Initial wealth $W_0 = 0$, and W_t represents the unused resources available after period t.

Appendix B.1. Late Payments and Consumption Smoothing

I modify the benchmark model to consider consumption commitments that are bundled with an implicit line of credit, such as housing with late payments. To incorporate the line of credit, I assume that, if the household does not move ($x_1 = x_0$), it can borrow up to a certain share α of its consumption commitments x_0. Following the assumptions in Chetty and Szeidl (2007) that all interest rates and discount rates are zero, the late payments and formal borrowing can be incorporated into a combined borrowing constraint

$$W_1 \geq \begin{cases} \underline{W} - \alpha x_0 & \text{if } x_1 = x_0 \\ \underline{W} & \text{if } x_1 \neq x_0. \end{cases} \tag{B.3}$$

As long as the household does not move, it gains access to additional credit of the amount αx_0. For example, if $\alpha = 0.5$, then the (non-moving) household can borrow up to 50% of its housing payment. This is equivalent to a late payment, i.e., the household pays only half the rent in period 1, and repays the remaining half in future periods. If consumers are otherwise borrowing-constrained, then this implicit line of credit available is the only method for smoothing consumption over time.

Chetty and Szeidl (2007) measures the impact of commitments on welfare losses by comparing the coefficient of relative risk aversion (CRRA) over wealth for agents with and without commitments. Let $v^i(W)$ denote the value function of an agent of type i (e.g., no-commitments, borrowing-constrained, etc.) with lifetime wealth W. The CRRA over wealth is defined as $\gamma_i(W) \equiv -v^i_{WW} W / v^i_W$ and is evaluated at $\overline{W} \equiv Ty$, which is the lifetime wealth when $Z = 0$. The CRRA at $\overline{W}$ reflects the curvature in utility around the first dollar of lost income, and a larger CRRA implies greater welfare losses from income fluctuations. Chetty and Szeidl (2007) shows that an agent c with consumption commitments (with adjustment cost $k > 0$) has greater risk aversion relative to a no-commitments agent n (with $k = 0$) so that the ratio of their CRRA over wealth is $\frac{\gamma_c(\overline{W})}{\gamma_n(\overline{W})} = \left(1 + \frac{\gamma_f}{\gamma_x}\frac{x}{f}\right) > 1$, reflecting reflects additional curvature in utility over wealth for the commitments agent that amplifies welfare losses from moderate shocks.

In contrast, when consumption commitments are bundled with an informal line of credit, such as an option to postpone housing payments, it can alter this result. Remaining in the same house decreases the consumer's ability to smooth consumption *across goods* by fixing x at x_0, thereby concentrating consumption reductions only on f. But the option of late payments also increases the consumer's ability to smooth consumption *over time* by borrowing up to αx_0. The trade-off can be seen directly by comparing the CRRA of an agent with the commitments-credit bundle, and therefore a relaxed borrowing constraint (γ_c), to a no-commitments agent with binding borrowing constraints from $\underline{W} = \alpha = 0$ ($\gamma_{n,BC}$):

$$\frac{\gamma_c(\overline{W})}{\gamma_{n,BC}(\overline{W})} = \underbrace{\left(1 + \frac{\gamma_f}{\gamma_x}\frac{x}{f}\right)}_{>1} \times \underbrace{\frac{1}{T}}_{<1}. \tag{B.4}$$

The first term, $\left(1 + \frac{\gamma_f}{\gamma_x}\frac{x}{f}\right)$, reflects the standard costs of commitments in preventing smoothing across goods.[36] The second term, $\frac{1}{T}$, reflects the benefits of greater smoothing over time due to the bundling of commitments with informal credit. Together, the net effect on risk aversion of this commitments-credit bundle, relative to a constrained no-commitments agent, is ambiguous as the ratio $\frac{\gamma_c(\overline{W})}{\gamma_{n,BC}(\overline{W})}$ could be less than or greater than one.[37] That is, the net effect of the commitments-credit bundle can either help or hinder consumption smoothing, depending on which term in equation (B.4) dominates. All derivations are included in Appendix B.3. below.

Appendix B.2. Quantitative Example

This section shows examples of curvature in the value function across types of agents. The first two panels of Figure B.1 show the impact of consumption commitments (panel a) and borrowing constraints (panel b) on an example agents' period zero value function over wealth (see Table notes for parameter values). Panel (a) shows that, for shocks within the

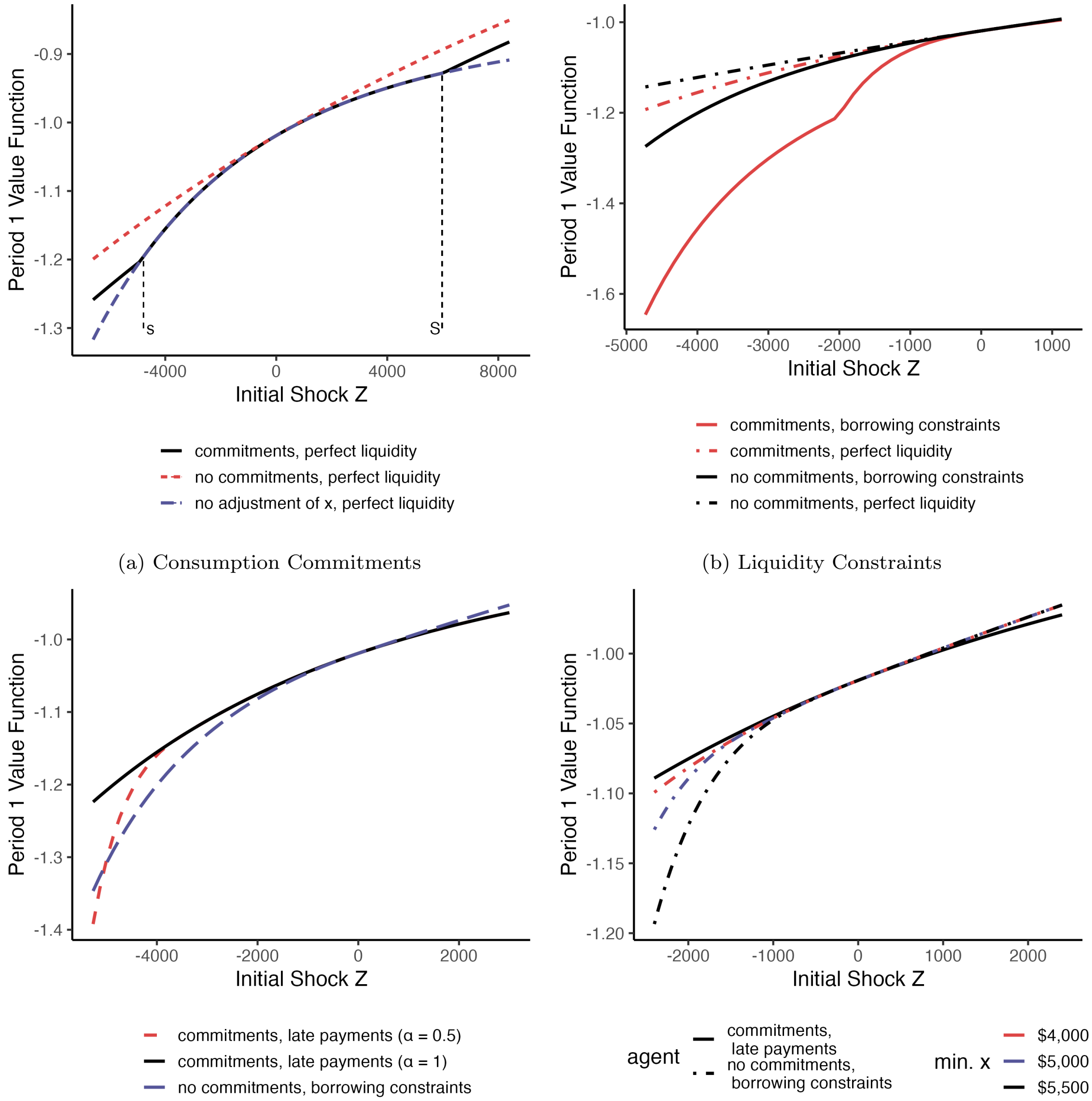
[36] One can show that $\frac{\gamma_f}{\gamma_x}\frac{x}{f} = \frac{\partial x^n/\partial W}{\partial f^n/\partial W}$, i.e., captures the adjustments that the no-commitments agent makes to x relative to f. Intuitively, the inability of the commitments agent to adjust x increases risk aversion, and this increase is proportional to the amount x would be adjusted (relative to f) if the adjustment were free.

[37] Another relevant comparison is between a commitments agent with and without the ability to make late payments. Assuming these agents are otherwise borrowing constrained, this case is covered by the effect of relaxing borrowing constraints on a commitments agent, which reduces risk aversion by at least a factor of T, i.e., $\frac{\gamma_c(W)}{\gamma_{c,BC}(W)} \geq \frac{1}{T}$.

(s, S) band, the value function of the commitments agent matches that of the agent that cannot adjust x. This leads to greater curvature of the value function for small shocks, and sharper declines in welfare relative to the no-commitments agent. For larger shocks, it is optimal for the commitments agent to move and so the value function parallels that of the no-commitments agent, but is shifted downward due to the fixed costs of moving. Panel (b) shows that borrowing constraints, modeled as an inability to borrow in period 0, magnify the costs for both types of agents. determines the curvature of the value function, which is larger than the no-commitments agent that can adjust both food and housing.

Figure B.1(c) illustrates this by comparing the no-commitments constrained agent to agents with commitments but different levels of late payments captured by $\alpha = 0.5, 1$. The value functions for the commitments agents are above those of the no-commitments agent for small shocks, showing that the ability to smooth consumption over time (but not goods) through late payments is more valuable than the ability to smooth consumption over goods (but not time). As the size of the shock increases, however, the borrowing constraints start to bind for the commitments agent with $\alpha = 0.5$, leading to increased curvature and a sharp decline in the value function.

These comparisons show late payments can help smooth consumption, and this may be even more valuable if households facing shocks cannot move into cheaper residence. For example, some households facing shocks could already reside in the lower rungs of the housing ladder, so cheaper places may be unavailable. Additionally, the shocks themselves may create barriers to moving; it is difficult to lease a new apartment while unemployed. Figure B.1(d) shows that, when there are minimum necessary housing expenditures $x \geq \underline{\mathrm{x}}$, then the ability to make late payments becomes increasingly valuable relative to the no-commitments but unconstrained case.



(a) Consumption Commitments

(b) Liquidity Constraints

(c) Late Payments

(d) Minimum Housing Expenditure

Figure B.1: **Commitments, Borrowing Constraints, and Late Payments**

This figure shows the value functions over wealth for versions of agents with and without commitments, borrowing constraints, and late payments. All agents haves $T = 6$ meant to represent six quarters, quarterly income $y = \$12,000$, and initial quarterly housing expenditure $x_0 = \$6,000$. The utility parameters are $\gamma_f = 6$, $\gamma_x = 2$, and μ is chosen so that x_0 is the optimal housing choice when the agent experiences no income shocks ($Z = 0$). For the no-commitments agent $k = 0$, and for the commitments agent $k = 0.3$, which is roughly one month's housing expenses. Borrowing constrained agents cannot borrow $\underline{W} = 0$. For agents with late payments $\alpha = 1$ (three months) and for late payments (50%), $\alpha = 0.5$ (1.5 months). In panel (d), the agents face an additional constraint that $x \geq \underline{x}$, i.e., a minimum housing expenditure, where $\underline{x}$ ranges from \$4,000 to \$5,500 per quarter.

Appendix B.3. Derivations: Comparing Risk Aversion Across Agent Types

This section derives the formulas that compare the curvature of the value function across agents, measured by the CRRA over wealth $-v^i_{WW}W/v^i_W$, with i indexing the type of agent.[38] The derivations of this section largely follow directly from Chetty and Szeidl (2007).

To begin, consider the CRRA for an agent with no commitments and perfect liquidity. The value function for this agent (n) is

$$v^n(W) = T \cdot u(f^n(W), x^n(W)).$$

Using the first-order condition that the partial derivatives $u_1 = u_2$ and the fact that, with perfect smoothing and no discounting, $\frac{\partial f^n}{\partial w} + \frac{\partial x^n}{\partial w} = \frac{1}{T}$, the marginal utility over wealth equals the marginal utility over food, i.e., $v^n(W) = u_1$. Thus,

$$\gamma^n(W) = \frac{-v^n_{WW}W}{v^n_W} = -W\frac{u_{11}\frac{\partial f^n}{\partial W} + u_{12}\frac{\partial f^n}{\partial W}}{u_1} = \gamma_f \varepsilon^n_{f,W}$$

where $u_{12} = 0$ because of the separability of f and x. Thus, the curvature of utility over food, γ_f, and the elasticity of food consumption with respect to wealth, $\varepsilon^n_{f,W} = \frac{\partial f^n}{\partial W}\frac{W}{f^n}$ are sufficient to determine the curvature over wealth. To compare across agents, it is helpful to break this elasticity into two components. Let $E = f_1 + x_1$ be the total expenditure in period 1. With this, we can write $\varepsilon^n_{f,W} = \varepsilon^n_{f,E}\varepsilon_{E,W}$, where $\varepsilon_{E,W}$ is the elasticity of expenditure with respect to wealth. Thus, for the no-commitments agent with perfect liquidity, the CRRA over wealth is

$$\gamma^n(W) = \gamma_f \ \varepsilon^n_{f,E} \ \varepsilon_{E,W}.$$

For shocks to first-period wealth that are not large enough to induce a move,[39] the CRRA over wealth for the perfect liquidity cases of the no-commitments agent (n) and the commitments agent (c), as well as the borrowing-constrained cases for the no-commitments

[38] With borrowing constraints, the timing of changes in wealth matter. Thus, when analyzing the CRRA of borrowing-constrained agents, I examine the impact of changes in wealth that are concentrated in period 1.

[39] The size of the shocks that induce a move vary across types of agents.

agent (n, BC) and the commitments agent (c, BC) take a similar form

$$\begin{aligned}
\gamma^n(W) &= \gamma_f\, \varepsilon^n_{f,E}\, \varepsilon_{E,W} \\
\gamma^c(W) &= \gamma_f\, \varepsilon^c_{f,E}\, \varepsilon_{E,W} \\
\gamma^{n,BC}(W) &= \gamma_f\, \varepsilon^n_{f,E}\, \varepsilon^{BC}_{E,W} \\
\gamma^{c,BC}(W) &= \gamma_f\, \varepsilon^c_{f,E}\, \varepsilon^{BC}_{E,W}.
\end{aligned}$$

Notice that $\varepsilon_{f,E}$ depends on the type of agent (n, c), but not whether they are borrowing-constrained, because once you fix the expenditure in period 1, borrowing constraints are irrelevant. Oppositely, $\varepsilon_{E,W}$ depends on whether the agent can borrow, but not the agent's type, because both agent types want to smooth consumption equally across periods.[40]

Before comparing agents, I characterize the elasticities $\varepsilon_{f,E}$ and $\varepsilon_{E,W}$. I begin with the elasticities of food with respect to period 1 expenditure. The consumer problem is

$$\begin{aligned}
\max_{f,x} \quad & \frac{c^{1-\gamma_c}}{1-\gamma_c} + \mu \frac{x^{1-\gamma_x}}{1-\gamma_x} \\
\text{s.t.} \quad & f + x = E
\end{aligned}$$

where, given that we are restricting to small shocks, the commitment agent has the additional constraint that $x = x_0$. The corresponding elasticities are

$$\begin{aligned}
\varepsilon^n_{f,E} &= \frac{E}{\frac{\gamma_f}{\gamma_x} x^n + f^n} < 1 \\
\varepsilon^c_{f,E} &= \frac{E}{f^c} > 1
\end{aligned}$$

Intuitively, the elasticity of food consumption with respect to expenditure is larger for the committed agent because housing expenditure is fixed at x_0 (for these small shocks). Additionally, one can show that both $\varepsilon^n_{f,E}$ and $\varepsilon^c_{f,E}$ are decreasing in expenditure E.[41]

[40]The unconstrained agent will smooth both f and x, while the constrained agent will smooth consumption of f because housing is fixed at x_0 as the shocks are not large enough to induce a move. The equal smoothing over time between borrowing constrained and unconstrained agents relies on shocks being small enough not to induce a desired move in either agent. For the constrained agents, this also excludes shocks large enough to cause them to move in period 2.

[41]The elasticity $\varepsilon^c_{f,E} = \frac{E}{f^c} = \frac{E}{E-x_0}$. Taking logs, it is easy to show that $\frac{dln(\varepsilon^c_{f,E})}{dE} < 0$, so $\varepsilon^c_{f,E}$ is decreasing in E. We can rewrite $\varepsilon^n_{f,E} = \frac{E}{E+(\frac{\gamma_f}{\gamma_x}-1)x^n}$, so that $\frac{d\varepsilon^n_{f,E}}{dE} = \frac{Ax^n(1-\varepsilon^n_{x,E})}{(E+Ax^n)^2}$ where $A = \frac{\gamma_f}{\gamma_x} - 1 > 0$. Thus, the elasticity will be decreasing if $\varepsilon^n_{x,E} > 1$. From the consumer problem, we can solve the elasticity $\varepsilon^n_{x,E} = \frac{E}{\frac{\gamma_x}{\gamma_f} f^n + x^n} < 1$ because $E = f^n + x^n$ and $\gamma_x < \gamma_f$. Thus, both elasticities are decreasing.

The elasticities of expenditure with respect to a period 1 shock reflect differences in the ability to smooth consumption. For the agent with perfect liquidity, shocks are smoothed over all periods so that $\frac{\partial E}{\partial W} = \frac{1}{T}$. Because expenditure is smoothed, $\frac{E}{W} = \frac{1}{T}$. For an agent with binding liquidity constraints, the shock is born entirely by period 1 expenditure, so $\frac{\partial E^{BC}}{\partial W} = 1$. Because the borrowing constraint binds, $\frac{E^{BC}}{W} \leq \frac{1}{T}$ As a result,

$$\varepsilon_{E,W} = \frac{\partial E}{\partial W}\frac{W}{E} = \frac{1}{T}T = 1$$
$$\varepsilon^{BC}_{E,W} = \frac{\partial E^{BC}}{\partial W}\frac{W}{E^{BC}} = 1 \cdot \frac{W}{E^{BC}} \geq T.$$

With the expressions above, we can compare the CRRA across agents with commitments, borrowing constraints, and late payments.

Commitments. First, for agents with perfect liquidity, commitments increase risk aversion. Specifically,

$$\frac{\gamma_c(W)}{\gamma_n(W)} = \frac{\varepsilon^c_{f,E}}{\varepsilon^n_{f,E}} = \frac{(\frac{\gamma_f}{\gamma_x}x^n + f^n)}{f^c}$$

In general, $f^c \neq f^n$ as the commitments and no-commitments agents will respond differently to shocks. However, as shown in Chetty and Szeidl (2007), at the level of wealth $\overline{W}$ where the no-commitments agent would optimally choose $x^n = x_0$ (which corresponds to $Z = 0$), the consumption choices of the agents would be the same. Evaluating their CRRA over wealth at this point,

$$\frac{\gamma_c(\overline{W})}{\gamma_n(\overline{W})} = 1 + \frac{\gamma_f}{\gamma_x}\frac{x}{f} > 1.$$

We can also rewrite this expression as

$$1 + \frac{\gamma_f}{\gamma_x}\frac{x^{n,BC}}{f^{f,BC}} = 1 + \frac{\partial x^n/\partial E}{\partial f^n/\partial E}.$$

This expression increases with how much the no-commitments agent adjusts x relative to f, and represents the additional costs faced by the commitments agent who, when faced with these smaller shocks, keeps $x = x_0$.

Borrowing Constraints. For both no-commitments and commitments agents, the inability to borrow also raises risk aversion. Consider shocks to wealth that are within the (s, S) bound, so it is optimal not to move, but are large enough to cause a borrowing constraint to bind. In these cases,

$$\frac{\gamma_{n,BC}(W)}{\gamma_n(W)} = \frac{\varepsilon^n_{f,E}(E^{BC})\,\varepsilon^{BC}_{E,W}}{\varepsilon^n_{f,E}(E)\,\varepsilon_{E,W}} \geq T,$$

where the inequality follows from the facts that $E^{BC} < E$ so $\frac{\varepsilon^n_{f,E}(E^{BC})}{\varepsilon^n_{f,E}(E)} \geq 1$ and $\frac{\varepsilon^{BC}_{E,W}}{\varepsilon_{E,W}} \geq T$. When $W = \overline{W}$, this inequality holds exactly so that $\frac{\gamma_{n,BC}(\overline{W})}{\gamma_n(\overline{W})} = T$. Similar calculations show the same holds for commitments agents, i.e., $\frac{\gamma_{n,BC}(W)}{\gamma_n(W)} \geq T$, with the inequality holding exactly when the borrowing constraint just begins to bind.

We can also solve for the shock Z where the liquidity constraint begins to bind. When the liquidity constraint binds, $y + Z - f - x = \underline{W}$. Additionally, given that the agent wants to smooth, the constraint just starts to bind when expenditure $E \equiv f + x$ satisfies $E = \frac{W}{T}$, where $W = Ty + Z$. With these equations, The level Z^{BC} where the borrowing constraint begins to bind is $Z^{BC} = \underline{W}\frac{T}{T-1}$.

Late Payments. Consumption commitments are often bundled with a line of credit, thereby relaxing credit constraints. Thus, an interesting comparison is between a no-commitments, borrowing-constrained agent (n, BC) and a commitments agent with perfect liquidity:

$$\frac{\gamma_c(W)}{\gamma_{n,BC}(W)} = \frac{\varepsilon^c_{f,E}(E)\ \varepsilon_{E,W}}{\varepsilon^n_{f,E}(E^{BC})\ \varepsilon^{BC}_{E,W}} = \underbrace{\frac{(\frac{\gamma_f}{\gamma_x}x^n + f^n)}{f^c}}_{\text{commitments}}\ \underbrace{\frac{1}{T}}_{\text{liquidity}}$$

The commitments term is greater than one, and reflects the additional risk aversion from fixing $x^c = x_0$. The liquidity term is less than one, and reflects the lower risk aversion coming from the ability to smooth consumption. When $W = \overline{W}$, $f^{n,BC} = f^c$, this simplifies to

$$\frac{\gamma_c(\overline{W})}{\gamma_{n,BC}(\overline{W})} = \left(1 + \frac{\gamma_f}{\gamma_x}\frac{x}{f}\right)\frac{1}{T}.$$

Appendix C. Intensive Margin: Accounting for Movers

I observe whether households moved within six months after job loss for some, but not all, households. Roughly once a year, the ALP Financial Crisis Survey asks respondents to report the dates of any changes in residence that have occurred in the last one or two years, and it allows households to report multiple moves. For these respondents, I observe the timing of each move. Some households miss these interviews, however, and their move status is unknown.

The goal is to identify how much, on average, housing payments would decline if no households had moved, which provides an estimate of the true availability of late payments. Let $M = 1$ for households that move in that six-month period, $M = 0$ for households that do not move, and $N = 1$ if the move status of the household is not observed ($N = 0$ otherwise). In potential outcomes notation, $\Delta y(0)$ is the change in housing payments (relative to the pre-period) that would occur if the household does not move ($M = 0$), $\Delta y(1)$ is the change in housing payments if the household does move ($M = 1$), and y is the observed outcome. The object of interest is $E[\Delta y(0)|\texttt{job loss}]$, where $\Delta y(0)$, in potential outcomes notation, is the change in housing payments (relative to the pre-period) that would occur if the household does not move, i.e., $M = 0$, and `job loss` indicates that the household experienced unemployment. For brevity, I suppress this conditioning on `job loss` for the rest of this section. $E[\Delta y(0)]$ can be written as

$$E[\Delta y(0)] = \underbrace{E[\Delta y(0)|M=0,N=0] \times P(M=0,N=0)}_{\Delta y(0) \text{ for stayers}} + \underbrace{E[\Delta y(0)|M=1,N=0] \times P(M=1,N=0)}_{\Delta y(0) \text{ for movers}} + \underbrace{E[\Delta y(0)|N=1] \times P(N=1)}_{\Delta y(0) \text{ for unknown}}.$$

The first of these three terms is observed, the second is an unobserved counterfactual, and, in the third term, it is unknown whether the observed y_i equals $y_i(0)$ or $y_i(1)$ because individual i's move status is unknown.

To proceed, I make assumptions about the two unobserved terms in order to provide upper bounds on $E[\Delta y(0)]$. For those who move, I assume $E[\Delta y(0)|M = 1, N = 0] \leq 0$, i.e., their rent or housing payments would not have increased on average if they had remained in their previous residence. That is, landlords and lenders would not, on average, *raise* housing payments for recent job losers. For those whose move status is unknown, I make two separate assumptions leading to two different upper bounds. The stronger assumption is that move status is missing at random, i.e., N is independent of $\Delta y(0)$ and M. This, combined with the assumption that $E[\Delta y(0)|M=1] \leq 0$, leads to the first upper bound (B1)

$$\begin{aligned} E[\Delta y(0)] &\leq E[\Delta y(0)|M=0] \times P(M=0) \\ &= E[\Delta y(0)|M=0,N=0] \times \frac{P(M=0,N=0)}{P(N=0)}, \end{aligned}$$

where the last equality uses the independence of N. Move status, however, may not be missing at random. Therefore, a more conservative assumption that $E[\Delta y(0)|N=1] \leq 0$, which essentially treats all unknown observations as movers, leads to a second upper bound (B2)

$$E[\Delta y(0)] \leq E[\Delta y(0)|M=0,N=0] \times P(M=0,N=0).$$

Table C.1 shows these probabilities, observed changes in housing payments for stayers, movers, and with unknown status, and the two upper bounds. The changes in housing payments are estimated using equation (3), but restricting the sample of job losers to movers ($M=0,N=0$), stayers ($M=1,N=0$), or unknown ($N=1$). The upper bounds are reported in final two columns. Assuming that move status is missing at random, the upper bound B1 for the overall decline is 4.1%, with a bound of 5.6% for renters and 3.5% for owners. With the more conservative assumptions in the B2 upper bounds, the estimated upper bounds are 3.2% for the overall sample, with a bound of 4.1% for renters and 2.9% for owners. These upper bounds show that the average declines in housing payments remain fairly large - around half the size of the reduction in nondurable expenditures - even under conservative assumptions about counterfactual payments by those who moved.

Table C.2 repeats the table, but using the monthly observations in the ALP, and using the two months after job loss ($t=0,1$) as the post-period. Even fewer observations either move or are unobserved in the monthly data. The upper bounds are larger, but are again close to the baseline estimates in column (1).

Table C.1: Robustness: Accounting for Movers

	Δhousing	Share of Respondents: $P(M,N)$			Group's Δhousing: $E[\Delta y_i \mid M,N]$			Upper Bounds $E[\Delta y(0)]$	
		stay	move	not obs.	stay	move	not obs.	B1	B2
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
All	-0.048	0.704	0.084	0.212	-0.046	-0.090	-0.033	-0.041	-0.032
Renters	-0.075	0.588	0.147	0.265	-0.070	-0.132	-0.046	-0.056	-0.041
Owners	-0.035	0.773	0.046	0.181	-0.037	-0.036	-0.026	-0.035	-0.029

This table reports the estimated upper bounds for the average decline in housing payments upon job loss. Column 1 reports the observed average decline in job losers from Table 3 for the full sample (All respondents), renters, and owners with mortgages. Columns 2-4 show the share of job-losing households in each sample that stayed in the same residence during the post-period ($M = 0, N = 0$), moved ($M = 1, N = 0$), or whose move status is not observed ($N = 1$). Columns 5-6 report the observed decline in housing payments within each group, estimated using equation (3) but restricting the sample of job-losing households to the respective stay, move, or not observed category. Using the estimated probabilities and declines, columns 8 and 9 construct the upper bounds. B1 assumes that missing information about move status is missing at random, and B2 more conservatively assumes that all missing move statuses are moves.

Table C.2: Robustness: Accounting for Movers (Monthly)

	Δhousing	Share of Respondents: $P(M,N)$			Group's Δhousing: $E[\Delta y_i \mid M,N]$			Upper Bounds $E[\Delta y(0)]$	
		stay	move	not obs.	stay	move	not obs.	B1	B2
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
All	-0.068	0.929	0.044	0.027	-0.054	-0.285	-0.174	-0.052	-0.051
Renters	-0.116	0.914	0.075	0.011	-0.108	-0.233	0.003	-0.100	-0.099
Owners	-0.047	0.936	0.029	0.034	-0.031	-0.358	-0.199	-0.030	-0.029

This table reports the estimated upper bounds for the average decline in housing payments upon job loss, but using the portion of the ALP for which monthly data is available . Column 1 reports the observed average decline in job losers from Table A.7 for the full sample (All respondents), renters, and owners with mortgages. Columns 2-4 show the share of job-losing households in each sample that stayed in the same residence during the post-period ($M = 0, N = 0$), moved ($M = 1, N = 0$), or whose move status is not observed ($N = 1$). Columns 5-6 report the observed decline in housing payments within each group, estimated using equation (3) but restricting the sample of job-losing households to the respective stay, move, or not observed category. Using the estimated probabilities and declines, columns 8 and 9 construct the upper bounds discussed in Section 5.3.