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

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Abstract

This paper examines the role that late housing payments play in helping households cope with job loss. The key idea is that the ability to postpone housing payments often operates as a valuable source of informal credit for households facing income or expense shocks. Using a stylized model, I first show that late payments can reduce the costs of income shocks and offset the disadvantages of consumption commitments. I then empirically examine the prevalence and consequences of missed housing payments after job loss. Missed housing payments are common, especially among renters, and provide substantial liquidity during unemployment. Indeed, the amount of informal credit from missed payments exceeds existing estimates of formal credit card borrowing during unemployment. Lastly, I examine the consequences of missed payments. The large majority of missed payments do not lead to evictions or other forced moves. Instead, households are able to fall behind on housing payments while remaining in the same residence, consistent with missed payments providing an important source of informal credit.

Keywords: consumption smoothing, job loss, housing, borrowing, credit, eviction, unemployment, household finance

JEL D14, E21, E24, G51, J64

1. Introduction

When faced with income losses or expense shocks, households sometimes fall behind on their housing payments. Much existing research focuses on the extreme negative consequences of missed housing payments: eviction or foreclosure. Evictions lead to homelessness, reduced earnings, and

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worse health (Desmond and Kimbro, 2015; Collinson et al., forthcoming). Foreclosure leads to worse health, housing conditions, and credit scores (Molloy and Shan, 2013; Currie and Tekin, 2015; Diamond et al., 2020; Guren and McQuade, 2020).

In contrast, this paper argues that falling behind on housing payments often provides an important *benefit* to households, helping them smooth consumption when facing shocks. Examining instances of job loss in two survey datasets, I show that (i) households frequently miss housing payments in response to 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. These facts are consistent with anecdotal evidence that landlords and lenders frequently "work with" delinquent tenants in order to avoid costly and time-consuming eviction and foreclosure process. By allowing households to reduce housing *expenditure* while maintaining housing *consumption*, missed payments are an informal source of credit extended by landlords and mortgage lenders. This informal credit is widely used by households facing unemployment, likely exceeding the use of formal unsecured credit by the unemployed. Moreover, landlords and lenders respond to local eviction or foreclosure policies, potentially making the availability of this informal credit a policy parameter.

Other papers examine the consumption smoothing benefits of late housing payments, though mainly for homeowners in specific settings. Gelman et al. (2018) 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 for a group of stably employed workers. That paper questions whether postponement would be viable for shocks of longer duration, such as job loss. Herkenhoff and Ohanian (2019) examines the insurance value of foreclosure delays during the Great Recession, a period of unusually long delays due to congestion from the housing crisis. A contribution of this paper is to show that the use of missed payments to cope with shocks is not confined to homeowners nor to these specific settings. Instead, missed housing payments are widely used to cope with larger shocks, namely job loss, and are more common among renters than owners.

To illustrate the potential value of late payments to households facing shocks, I modify the stylized consumption commitments model of Chetty and Szeidl (2007). In the original model without late payments, consumption commitments (e.g., housing) are costly to adjust, so for small to moderate shocks households leave consumption commitments unchanged (e.g., not moving). As a result, all expenditure changes must be concentrated on the subset of adjustable goods, and this increases welfare losses (relative to a no-commitments model) by inhibiting the household's ability to smooth *across goods* (Chetty and Szeidl, 2007). I modify the original model by bundling consumption commitments with an implicit line of credit through postponing payments. While commitments still inhibit consumption smoothing *across goods*, they can also foster smoothing *over time* by providing a line of credit. As a result, this commitments-credit bundle can potentially increase welfare.

The model shows that late payments *can* help households smooth consumption, but only when three conditions are satisfied: borrowers are otherwise borrowing-constrained, the households face shocks in a certain (s, S) bound where not moving is optimal, and landlords or lenders treat late payments as a line of credit. It is unclear how commonly these conditions arise. Households may not be borrowing-constrained, or they may not face shocks in the necessary range. Additionally, it may be inappropriate to treat late payments as a credit line if they often lead to severe consequences, such as eviction or foreclosure. To investigate, I empirically examine the prevalence and consequences of missed payments around a specific shock: job loss.

I make use of two survey datasets containing information on housing payments and expenditures. The first is the Survey of Income and Program Participation (SIPP) panels that cover 1991-2014 and contain information on job loss, whether households have missed housing payments, and whether the household has been evicted. The second dataset is the RAND American Life Panel (ALP) Financial Crisis Surveys, a monthly and later quarterly panel covering 2008 through 2016 containing information on job loss and detailed information on respondents' 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, these survey data have advantages over bank or financial account data; a significant share of those missing housing payments are unbanked or underbanked and so are absent from account data, and, through the 2010s, the large majority of renters pay with paper-based methods (cash, check, or money order) which are missing or difficult to categorize in financial transaction data.

There are three main results. First, missed housing payments are a common response to job loss. Fifteen percent of unemployed households report missing housing payments. These missed payments are not necessarily *caused* by job loss, however, since some of these households may have missed payments if they had remained employed. Thus, my preferred estimates control for a large set of household and demographic characteristics. With these controls, job loss leads to a 7.5 percentage point (pp) increase in the probability of missing payments, with larger effects for renters (9.1pp) and smaller effects for owners (6.2pp). These results are robust to a variety of methods investigating the endogeneity of job loss. Moreover, consistent with providing an informal source of liquidity, late payments are much more common among households with few liquid assets.

Second, the liquidity provided by missed payments is economically meaningful. Across all households, monthly housing expenditure falls by an average of 4.8% in the six months following job loss, with declines of 7.5% for renters and 3.5% for owners. The reductions in housing payments happen immediately upon job loss and gradually recover to prior levels in the subsequent six months. Because housing is typically a household's largest expenditure, these reductions provide substantial liquidity. The average household that reduced payments gains $4,058 (median household: $2,610) in the six months following job loss, compared to if they had made their expected payments in full. While these expenditure reductions could, in theory, be caused by moves to a cheaper residence, this plays little role in practice because few households move in this period. More formally, I construct upper bounds of the decline in housing payments, accounting for selection into moving, and find similar results.

Finally, I examine the consequences of these missed housing payments. Although late payments are commonly used and provide meaningful liquidity, it would be inappropriate to treat this as a source of credit if late payments regularly lead to severe consequences, such as eviction. I find that job loss leads to a 0.4pp increase in the probability of eviction (0.8pp for renters). When scaled by the frequency of missed payments following job loss, these estimates imply that the large majority of missed payments (90-95%) do not lead to eviction. A concern is that evictions may be underreported or do not capture informal evictions or forced moves. To investigate, I examine how missed housing payments affect the rates at which households change residence. I find little impact of missed payments on move rates. Renters who do and do not miss payments move out at nearly identical rates, while owners missing payments are 4-5pp more likely to move out over the next year. Most unemployed renters and owners who miss payments remain in the same residence for at least two years after job loss.

In summary, falling behind on housing payments is a common and quantitatively important source of informal credit used to cope with job loss.[2] One implication is that the amount of this informal credit available through late payments may be a policy parameter, and I find some, albeit imprecise, evidence that the use of missed payments increases where renter protections are stronger. Importantly, however, this paper does not assess the overall or welfare impact of missed payments. The analysis may understate the benefits to renters and owners if some of the housing arrears is ultimately forgiven. There are also costs (e.g., access to credit or future housing) or general equilibrium effects (e.g., housing markets and rent prices) that are not captured. The focus of this paper is simply to show that missed payments provide a widely used method of consumption smoothing for unemployed households.

As an informal source of credit, the availability of missed payments relates to several important aspects of consumption smoothing. First, many households have little liquidity, exposing them to large welfare losses from shocks. Around one-third of U.S. households have no liquid wealth or are near their (formal) borrowing limits (Kaplan and Weidner, 2014), and around half of households either certainly or probably could not come up with $2,000 within one month (Lusardi et al., 2011). This paper shows that formal liquidity understates the true liquidity of households, because when facing shocks these households can rely on informal credit through missed housing payments. This informal credit softens some of the expected losses for households with minimal liquidity and can help explain *why* households hold so little (formal) liquidity.

Second, the use of informal credit sheds light on surprising facts about the use of credit during unemployment. Standard models would have households using credit to smooth the (transitory) income losses (Hundtofte et al., 2019), but research commonly finds that the unemployed engage in little to no unsecured borrowing (Sullivan, 2008; Bethune, 2015; Keys et al., 2018; Braxton et al., 2019; Andersen et al., 2023).[3] In contrast, this paper shows that households widely engage in

[2] I treat the missed payments as a loan because the rent or mortgage payment is still contractually owed to the landlord or lender. However, some of these missed payments may be forgiven, in which case they operate more like insurance than credit.

[3] Most papers find that borrowing replaces 0-5% of lost income (even when credit is available), and some households repay debt. For example, Ganong and Noel (2019) finds that new credit card borrowing finances only 0.5% of consumption during unemployment. Some find borrowing in subgroups, with heterogeneity by asset holdings or unused credit (Sullivan, 2008; Braxton et al., 2019). The magnitudes in Braxton et al. (2019), however, are small. When restricted to only credit cards (Figure 15 c), borrowers in the top 3 quintiles of unused credit replace 0-2% of lost income with credit card borrowing, while those in the lower quintiles reduce balances by around 2%. The effects are larger, around 5%, when also considering borrowing through HELOCs, personal loans, and retail credits (Figure 4a).

informal borrowing around job loss, with 15% of unemployed households missing housing payments. Indeed, informal borrowing through late housing payments exceeds existing estimates of the use of credit card borrowing by the unemployed (Ganong and Noel, 2019; Braxton et al., 2019).

This paper also fills a gap in the literature on expenditure responses to job loss by examining monthly housing payments, which are often a household's largest regular expenditure. It adds to papers examining how households cope with job loss through reducing other (non-housing) expenditures and using the social safety net (Gruber, 1997; Browning and Crossley, 2009; East and Kuka, 2015; Hurd and Rohwedder, 2016; Hendren, 2017; Ganong and Noel, 2019; Andersen et al., 2023; East and Simon, 2020). While I examine how housing payments help households cope with unemployment, other research examines the reverse: how unemployment insurance and the safety net helps households make housing payments (Hsu et al., 2018; Hobbs, 2020; McKernan et al., 2021). Outside of consumption smoothing, Low (2022) shows that the potential to recover from delinquent mortgage payments is important for explaining the prevalence of foreclosures in above-water homes. Finally, this paper also relates to the broader literature on eviction and housing insecurity (Desmond and Kimbro, 2015; Collinson et al., forthcoming).

2. Institutional Background

The legal processes of 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. 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 (share of owed balance that is not recovered) exceeding 40% (An and Cordell, 2020).

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, 2020). 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 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 for the subset of cases where the legal process of eviction begins, it can resolved before the eviction or foreclosure is actually executed. Eviction filings are the first step in the legal process, but are often filed only to encourage payment from tenants. Consistent with this, 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. (forthcoming).[4] Tenants may move out before the eviction is actually executed, but a significant number remains in the same residence. Estimating the causal effect of an eviction order on residence changes, Collinson et al. (forthcoming) 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). In summary, even among tenants receiving an eviction filing or order, many remain in the same residence.

This process of falling behind on housing payments while remaining in the same residence provides an informal source of credit to renters and mortgagors. Although some debt may ultimately be forgiven, I view these missed payments as credit because the resulting debt is contractually owed by the tenant. The role of this informal credit in coping with job loss is the focus of this paper.

[4] 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.

3. Theory: Consumption Commitments with Late Payments

This section introduces a stylized model to illustrate how late payments can alter the conventional relationship between housing and consumption smoothing. Housing, because it is costly to adjust, is typically viewed as an impediment to consumption smoothing. Chetty and Szeidl (2007) shows that consumption commitments such as housing increase the welfare costs of small shocks relative to an agent that can freely adjust these goods. Similarly, the illiquidity of housing wealth plays a key role in explaining the consumption fluctuations of the "wealth hand-to-mouth" Kaplan and Weidner (2014).

In contrast, when one can postpone payments on committed consumption, e.g., falling behind on housing payments, it is possible for commitments to facilitate consumption smoothing. To demonstrate, I modify the stylized consumption commitment model of Chetty and Szeidl (2007) to incorporate late payments. Although the adjustment costs of commitments still hinder consumption smoothing *across goods*, the ability to make late payments facilitates consumption smoothing *over time*. In some situations, this commitment-credit bundle can reduce the welfare costs of small shocks relative to a no-commitments but credit-constrained agent.

3.1. Model

Following 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.[5] 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

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

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

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

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).

The consumer faces uncertain income and decides how to adjust consumption of f and x. For simplicity, all uncertainty is realized in period 1. Specifically, the household earns an 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 $t \geq 2$. The consumer may also face a borrowing constraint $\underline{\mathrm{W}}$ in this first period. As in Chetty and Szeidl (2007), I assume the consumer has perfect liquidity in periods $t \geq 2$. We can write the full consumer problem and the consumer's value function over lifetime wealth $W = Ty + Z$ as

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

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

To compare how commitments, borrowing constraints, and late payments alter the welfare losses from small shocks, I follow Chetty and Szeidl (2007) by calculating the coefficient of relative risk aversion (CRRA) over wealth for different agents. Let $v^i(W)$ denote the value function of an agent of type i (e.g., no-commitments, borrowing-constrained, etc.).[6] The CRRA over wealth is defined as $\gamma_i(W) \equiv -v^i_{WW} W / v^i_W$. In what follows, I evaluate the coefficient of relative risk aversion at $W = \overline{W} \equiv Ty$, which is the lifetime wealth when $Z = 0$. The curvature at this point captures the change in utility from the first dollar of lost income. A larger CRRA implies larger welfare losses from income fluctuations, and so is a critical measure of households ability to cope with shocks.

[6]For brevity, I suppress the dependence of $v(\cdot)$ on x_0. 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 Z, which is a shock to wealth that occurs in period 1.

3.2. Commitments with Late Payments

As a benchmark, I first summarize the results from Chetty and Szeidl (2007) for consumption commitments and borrowing constraints without late payments. This benchmark model examines the welfare of a consumption commitments agent (c), which has $k > 0$, to an otherwise identical no-commitments agent (n), which has $k = 0$, when neither face binding borrowing constraints. In response to a shock, the no-commitments agent adjusts both f and x. The commitments agent, in contrast, only adjusts f and keeps x fixed at x_0 when shocks are small to moderate, i.e., Z falls in an (s, S) bound. Concentrating consumption changes only on f increases the risk aversion of the commitments agent relative to the no-commitments agent, specifically $\frac{\gamma_c(\overline{W})}{\gamma_n(\overline{W})} = \left(1 + \frac{\gamma_f}{\gamma_x}\frac{x}{f}\right) > 1$. This is the key insight of Chetty and Szeidl (2007), as the increased risk aversion reflects additional curvature in utility over wealth that amplifies losses from moderate shocks. Adding borrowing constraints further increases the losses from shocks by preventing smoothing over time. Specifically, constraints increase risk aversion by at least a factor of T, i.e., $\frac{\gamma_{c,BC}(\overline{W})}{\gamma_c(\overline{W})} = T$, where the subscript BC indicates the borrowing constrained ($\underline{W} = 0$) versions of the agents.

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, making the new borrowing constraint

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

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.

The bundling of commitments with credit creates a trade-off in the household's ability to smooth consumption. Remaining in the same house decreases the consumer's ability to smooth consumption *across goods* by fixing x at x_0. But it also increases the consumer's ability to smooth consumption *over time* by borrowing αx_0. This credit will be useful over the range of shocks that are (i) not large enough to induce a move, and (ii) in the range where, without late payments, the agent would be borrowing-constrained. 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 ($\gamma_{n,BC}$):

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

The first term, $\left(1 + \frac{\gamma_f}{\gamma_x}\frac{x}{f}\right)$, reflects the costs of commitments in preventing smoothing across goods.[7] The second term, $\frac{1}{T}$, reflects the benefits of greater smoothing over time. 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.[8]

This comparison highlights the main point of this section: when commitments are bundled with an implicit line credit from the ability to make late payments, the combined effect can facilitate consumption smoothing and reduce the welfare costs of shocks relative to a no-commitments agent. Appendix Figure B.1(c) shows ranges of shocks where the commitments-credit bundle leads to higher welfare than a constrained, no-commitments agent. In practice, the value of late payments may be higher than suggested by the model if households face barriers to adjusting x downward. For example, some households facing shocks could already reside in the lower rungs of the housing ladder, and so may be unable to move into a cheaper place. Additionally, the shocks themselves can create barriers to moving; it is difficult to lease a new apartment while unemployed. Capturing these situations as minimum necessary housing expenditures $x \geq \underline{x}$, Appendix Figure B.1(d) shows that this makes late payments increasingly valuable. These calculations would also understate the value if some of the housing debt is ultimately forgiven by landlords or lenders Decker (2021).

The stylized model also leaves several questions unanswered. First, how important are these late payments in practice? The model suggests that late payments matter when (i) borrowers are otherwise borrowing-constrained, (ii) late payments are available, and (ii) the shock is small enough

[7] 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.

[8] 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., $\frac{\gamma_c(W)}{\gamma_{c,BC}(W)} \geq \frac{1}{T}$.

not to induce a move. The empirical analysis will help answer this question by examining the take-up of late payments, i.e., how frequently they occur. Second, is it correct to treat late payments as a loan? Instead of paying interest on the debt, the true cost of late payments may be an eviction, repossession, or forced move. The empirical analysis will also examine these consequences of late payments. However, this paper does not seek to provide a complete welfare analysis, as it ignores potentially important general equilibrium effects such as the adjustment of rent or mortgage prices and tenant screening which can offset the consumption smoothing benefits highlighted above.

4. Data

I examine the prevalence and importance of late housing payments during unemployment using household survey data from the Survey of Income and Program Participation and the RAND American Life Panel (ALP). This section provides an overview of these sources and discusses the advantages of using survey data to examine housing payments.

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). 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. Additionally, once per panel, the SIPP administers the Adult Well-Being topical module, which asks questions about missed housing payments, eviction, and other financial distress that occurred over the prior year. Regarding housing, 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 twelve-month period over which missed payments are assessed. Following a similar analysis in Hsu et al. (2018), I convert the panel data into a repeated cross-section, with one observation per surveyed household. The sample consists of households who either rent or own during the pre-period, defined as the four-month period preceding the year over which missed payments are assessed, and I exclude households that live in public housing or receive government assistance.[9] I further restrict the sample to households with

[9] I exclude households that report neither owning nor renting, and those missing data on housing costs or reporting

no unemployment during the pre-period and, to avoid confounding unemployment 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.

To examine the effects of job loss, the final analysis sample is restricted to households that experience job loss in this twelve-month period and comparison households that experience no unemployment in the same period.[10] Table 1 Panel A reports the summary statistics for the sample of households that did and did not experience job loss. Fifteen percent of households in the job loss sample missed housing, and 0.6% report an eviction. Many of these households have few liquid assets, defined as the total household assets in 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 of income. Additionally, 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.[11] Several times each month, ALP respondents receive an email to complete a questionnaire with response rates of 80 to 95 percent.

housing costs of zero. The 1992 SIPP panel is excluded from the main sample because the panel begins in the twelve-month period that missed payments are assessed, 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 data, 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] Specifically, 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] 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.

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 are well-suited for examining spending responses to job loss, and housing payments in particular. Each wave collects information about participants' expenditure in the previous month across 25 spending categories, and takes measures to improve accuracy and recall.[12] These high-frequency spending categories account for around 70% of total spending (Hurd and Rohwedder, 2016). I focus on housing payments, and also form aggregated consumption categories for bills (housing, utility, and auto payments), which reflect committed consumption, and nondurable expenditures (food, housekeeping, recreation, transportation, personal services, and other child or pet expenditures). It also asks about missed housing payments, specifically whether the household is behind on payments by two or more months. 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 category-quarter and dropping missing values. In robustness checks, I find similar results using only the portion of the survey for which monthly data are available. For respondents in each quarter t, I restrict the sample to households that are (i) either renters and owners, (ii) earned income and were not unemployed in the pre-period, defined as quarters $t-3$ to $t-1$.[13]

Table 1 Panel B shows the characteristics of the sample, split into those where the head or spouse did or did not lose a job in quarter t. The top of the panel shows the avearge 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 $t+1$. Nearly 10% are at least two months behind on rent in this six-month period, and 2.9% receive an eviction or foreclosure notice. Note that, these eviction notices are just the first step and do not necessarily lead to an eviction order or enforcement

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

[13]Additional restrictions are that, during the pre-period, either the head or head's spouse was employed and neither was unemployed, the household either rented or owned with a mortgage and reports no residence changes, and had positive total income and expenditure on housing and nondurables. If a household was not interviewed in all quarters, the variables are formed from the quarters in which the household is observed.

(Section 2). Additionally, 9.2% of households move within the first six months after job loss. The analysis will examine the relationship between job loss, missed payments, and moves, using this information as well as the extended SIPP panel. The final set of variables shows that, for those experiencing job loss, there are declines in most expenditure categories, including housing.

4.3. Comparison with Financial Account Data

Several recent 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., 2018; Baugh et al., 2021). For many research questions and expenditure categories, these transaction data have significant advantages over survey data, including larger sample sizes, more detailed expenditure categories, and less measurement error in transaction amounts.

When examining rent payments, however, survey data have some advantages. First, because of how rent is paid, rent payments will be missing or difficult to track in financial account data. Within financial accounts data, cash payments and money orders may be missing from the data and paper checks, which are difficult to characterize, and are often categorized as miscellaneous payments (Baker, 2018; Ganong and Noel, 2019; Baugh et al., 2021). As of 2014, more than 75% of rent payments are made through paper-based methods, with 22% of households paying rent in cash, 42% by check, and 16% by money order (Zhang, 2016). For the lower-income households (annual income less than $25,000), the group most likely to miss housing payments, 90% of renters use these paper-based methods. Second, households missing rent payments are more likely to be 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. Moreover, less financially prepared households tend to use cash for a significant share of their transactions (Stavins, 2021).

5. Empirical Strategy and Results

This section investigates the prevalence and consequences of missed housing payments that occur in response to job loss. The model in Section 3 suggests late payments can be valuable when households are otherwise borrowing-constrained and face shocks that are not large enough to induce a move. To understand how often such shocks occur, I examine how frequently households respond to job loss by missing payments.

To motivate the analysis, I first provide summary statistics of the incidence of missed housing payments among households that experienced job loss in Figure 1. In both the SIPP and the ALP, missed housing payments are common among unemployed households. 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%).[14] 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. First, some of these households may have missed housing payments even if they had not lost a job, so these measures may overstate the share of households that miss payments *because of* job loss. Second, these measures capture the extensive margin, i.e., the frequency of missed housing payments, but not the intensive margin, i.e., the magnitude and duration of reduced housing payments. Third, these measures do not convey the consequences of missing housing payments, which can include eviction or foreclosure. This section examines each of these three issues.

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

I begin by estimating the frequency of missed housing payments in response to job loss. This section improves on the raw summary statistics in Figure 1 by controlling for other observable characteristics, comparing the incidence of missed housing payments between observably similar households that do and do not experience job loss. I also examine heterogeneity by household liquidity, which provides evidence on whether households are more likely to miss payments when they are otherwise liquidity-constrained.

Empirical Strategy. To examine the effect of job loss on missed housing payments, I use the SIPP sample, consisting of households with no unemployment in the prior four months, to estimate the

[14]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.

following specification for household i in state s in year t

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

The dependent variable, `missed`, is an indicator for whether the household reports missing a housing payment during the twelve-month period covered by the SIPP questions on missed payments. 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 $\boldsymbol{X}_{it}$ are household-level controls for the household head's age, marital status, indicators for head's race as Black and other nonwhite, indicator for Hispanic ethnicity, indicators for head's education group (5 categories), pre-period household income and changes in household size or marital status from the pre-period.[15] The control $\boldsymbol{X}_{it}$ also includes financial variables: the household's liquid assets, total net worth, unsecured debt, and housing payments as a share of monthly baseline household income. 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. These controls are meant to control for selection into job loss, but I conduct several other robustness checks to examine the sensitivity of the estimates to unobservable selection.

Results. The columns in Table 2 show estimates of the effect of job loss on missed payments from equation (5) using the sample of all respondents (Panel A), renters (Panel B), and owners with mortgages (Panel C).[16] 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 7.6-8pp when each household's demographic controls (column 2), financial controls (column 3), state fixed-effects (column 4), and state economic controls (column 5) are successively added. Panels B and C show that the effect of unemployment on missed housing payments is nearly 40% higher for renters (9.1pp) than for owners (6.2pp), and the p-value of the column (5) difference is < 0.01. In sum, missing housing payments is a common response to job loss, and is more common among renters than owners.

[15] The pre-period consists of the four months immediately before the twelve-month period over which missed payments and unemployment are assessed.

[16] 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.

A key idea of the paper is that missing housing payments are an important source of liquidity for otherwise constrained households, implying missed payments should be more common among households with fewer liquid assets. I examine heterogeneity in the impact of job loss across different levels of liquidity by interacting `job loss` in equation (5) 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 5. 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 \$6,239 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.

One concern is that job losses may be correlated with other factors driving 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. I investigate this concern in several ways. First, some of concerns are partially addressed by the detailed financial controls in Table 2 column 5, which include many controls for the households' demographic and financial characteristics. Second, I apply 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, where observable selection is reflected by changes in the estimated effect of job loss as controls are added in Table 2.[17] Applying the recommended

[17] The method requires assumptions about (i) the coefficient of proportionality, δ, which captures the importance of unobservables relative to observables, and (ii) the value of $R^2_{\mathtt{max}}$, which is the R-squared value from a hypothetical regression of the outcome on all relevant controls (including those currently unobserved). Oster (2019) recommends the baseline values for the coefficient of proportionality $\delta = 1$, implying unobservables and observables are equally important, and $R^2_{\mathtt{max}} = 1.3\widetilde{R}$, where $\widetilde{R}$ is the R-squared value from a regression including all *observable* controls. The paper argues assuming equal importance of unobservables and observables ($\delta \leq 1$) is a reasonable bound because (i) the researcher typically selects variables they believe to be most important in explaining the outcome and (ii) the unobservables in consideration are technically only the portion of the unobserved variables that are orthogonal to the included controls. Oster (2019) recommends $R^2_{\mathtt{max}} = 1.3\widetilde{R}$ as a good bounding value based on its performance when comparing the robustness of results from randomized control trials to those from nonrandomized evaluations.

values of Oster (2019) in Online Appendix Table A.2 Panel A, the bias-adjusted estimates imply that job loss increases the probability of missed housing payments by 6.8pp overall, 8.2pp for renters, and 5.6pp for owners (column 2). Table A.2 columns 3 and 4 also examine the sensitivity to more conservative assumptions than those recommended by Oster (2019), with the effects of job loss on missed payments remaining in a similar range. Finally, Online Appendix Table A.3 shows that the estimated effect of job loss on missed payments is robust to 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, defined as no unemployment in the preceding year.[18] In sum, households, especially renters, frequently miss housing payments after job loss, and the results are robust to several tests addressing unobserved selection into job loss.

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

This section investigates the intensive margin response, i.e., the magnitudes of the reductions in housing payments that occur after job loss. Knowing this magnitudes is important for understanding how many dollars households are able to "borrow" by missing housing payments. I use information on monthly housing expenditures from the ALP Financial Crisis Surveys to examine the size and timing of payment reductions.

Empirical Strategy. The primary dependent variable is Δy_{it}, which is the percentage change in monthly housing expenditure for household i in the post-period (average monthly exp. in quarters t through $t+1$) relative to the pre-period (average monthly exp. in quarters $t-1$ through $t-3$).[19] The sample is restricted to households i in quarter t that, during the pre-period, were renters or owners with a mortgage and were not unemployed.

I estimate the following specification for household i in quarter t.

$$\Delta y_{i,t} = \alpha + \beta \texttt{job loss}_{i,t} + \tau_t + \varepsilon_{i,t} \tag{6}$$

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

[18] In particular, I restrict the sample of job losses to those that either receive unemployment insurance, which requires the job separation to occur through no fault of the worker, or for which the respondent reports the reason as layoff, illness or injury, discharged or fired, employer bankruptcy or sale of the business. In addition, I also restrict the sample to households that have experienced no unemployment during the year $t-1$, the year prior to the reference period over which missed payments are assessed.

[19] To reduce the influence of outliers, I truncate the percentage change at $\pm 100\%$.

quarter t, and τ_t is a set of quarter fixed effects. The coefficient β captures the average percentage change in housing expenditures for households with job loss in quarter t relative to the change in expenditures for households that do not. Because the dependent variable is first-differenced, the specification implicitly controls for all time-invariant characteristics of the household. I also estimate this specification separately for renters and owners, and examine variants of the dependent variable.

While the goal is to estimate the effect of missed payments, the parameter β potentially reflects changes from both missed payments and residence changes. At the end of this section (and Online Appendix C), I construct upper bounds on the average change in housing payments after job loss conditional on not moving. Reflecting that relatively few households moved, these upper bounds show that the baseline estimates are largely due to payment reductions by nonmovers.

Results. For comparison with the earlier estimates, I begin by estimating the effect of job loss on an indicator for missed housing payments in the ALP, specifically whether the household is two or more months behind on rent in either quarter t or $t+1$. Table 3 column (1) shows that job loss is associated with a 7.5pp increase in the probability of a missed payment, with slightly higher (lower) point estimates for renters (owners), which is consistent with the estimates from the SIPP.[20]

I next examine the magnitudes of the changes in housing payments. Column 2 shows that monthly housing expenditures fall by 4.8% upon job loss (relative to the pre-period), with declines of 7.5% and 3.5% for renters and owners, respectively. Column 3 broadens the expenditure outcome to bills – defined as the sum of housing, utility, and auto payments – which captures a broader measure 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 payments (bill payments) take up 28% (45%) of the monthly income of these households pre-unemployment income.

At least for renters, the declines in housing payments are roughly equal to the declines in nondurable expenditures. Column 4 shows that nondurable expenditure falls by 8% upon job loss, with declines of 7.6% for renters and 8.4% for owners. This magnitude of the consumption decline for nondurable expenditure is consistent with estimates in the literature, including the path of

[20] 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).

nondurable expenditure after job loss found in Ganong and Noel (2019), and the 7-10% decline in food expenditure found in Gruber (1997) and East and Kuka (2015).

Next, I examine the timing of the reductions in housing payments. 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), though the patterns are similar when the full, quarterly sample is used (Appendix Figure A.1). Using the monthly data, I extend equation (6) by replacing the dependent variable with $\tilde{\Delta} y_{i,t+j}$, i.e. the percentage change in the outcome in month $t+j$ relative to the pre-period (two quarters preceding month t), and estimate separate models for months $j = -8, \ldots, 8$.[21] Figure 3 shows that, for both renters and owners, housing expenditure (relative to the pre-unemp. baseline) is stable, then declines quickly upon job loss. After job loss, owners' payments recover more quickly, while renters' payments continue to fall in months 0-3 to a minimum of around -13%, then gradually recover in months 4-6. This six-month period is consistent with Decker (2021), which surveys owners of small rental properties during 2020 and 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 also indicates that, in the observed period, the missed housing payments are not repaid since there are no estimates where post-unemployment average housing expenditure exceeds the pre-unemployment baseline.

Rather than the *average* decline in housing payments, one may also be interested in the distribution of changes in housing payments. In the SIPP, only 15% of job-losing households reported missing housing payments, so most households' payments remain unchanged. To assess the magnitudes of the declines conditional on missing a housing payment, I examine the distribution of changes in housing payments. Specifically, for each household i, I calculate the ratio of realized housing payments (what was paid) in the first six months after job loss to that household's expected housing payments (what was owed).[22] Figure A.1(b) shows the empirical cumulative distribution functions (CDFs) of the share of expected housing payments that were paid for job-losing households, and for employed households as a comparison. Consistent with job loss driving missed

[21] 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.4 repeats the regressions in Table 3 using this monthly sample.

[22] The expected payments during this period equal the pre-period (and pre-unemployment) monthly housing expenditure multiplied by six. Realized payments for household i equal the sum of reported housing expenses for household i in months $t = 0, \ldots, 5$. 79% of households report housing expenditure in all six periods, and for those who report $n_i < 6$, I multiply the sum of actual expenditures by $\frac{6}{n_i}$.

payments, partial housing payments are roughly twice as common among households experiencing job loss. For example, 16.8% of job-losing households paid less than 75% of expected housing payments, compared to 7.1% of those not unemployed.

Using this 75% cutoff as a proxy for missed payments, I focus on these 16.8% of job-losing households to provide a sense of the financial magnitudes involved in missed housing payments. These households, on average, paid only 45% of expected housing payments during the six months following job loss, and the average amount of unpaid rent or mortgage payments during this period was $4,058 (median $2,610). Thus, by reducing housing expenditure, these households postponed thousands of dollars of committed expenditures, freeing resources that they could use to fund other consumption during the six months following job loss. This implicit line of credit exceeds existing estimates of borrowing on credit cards during unemployment. For example, Ganong and Noel (2019) finds that new credit card borrowing finances only 0.5% of consumption during unemployment, and Braxton et al. (2019) finds that borrowers in the top 3 quintiles of unused credit replace 0-2% of lost income with credit card borrowing, while those in the lower quintiles reduce balances by around 2%.[23]

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 observed, 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 not be related to these counterfactual housing expenditures if, for example, households may be more likely to move if their landlord is unwilling to allow late housing payments. This 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 the average change in housing expenditures *if no households had moved* by making conservative assumptions on what moving households would have paid if they had not moved. In particular, I assume that, had the job-losing households not moved, their average housing expenditure would not have *increased* in the months immediately following the job loss. This allows for the possibility that landlords of movers may have been less flexible on rent, perhaps requiring full payment, but assumes

[23]Here, I refer to the results for *credit card* borrowing. The main results of Braxton et al. (2019) also include HELOC borrowing and personal loans, and the effects are larger. Home equity borrowing, and perhaps personal loans, are unavailable for renters.

these landlords would not have systematically increased rents for households experiencing job loss. With this assumption, the estimated upper bound indicates 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.[24] I repeat this bounding analysis using the monthly data and focusing on the two months immediately following job loss, instead of the six-month post-period used in the main analysis. In the monthly data, the upper bounds for the decline in housing payments if no households move is 5.2%, or 10% for renters and 3% for owners. These upper bounds show that the average declines in housing payments remain fairly large even under conservative assumptions about counterfactual payments by those who moved. Moreover, the similarity of the upper bounds to the baseline estimates indicates that most of the reductions in housing payments are due to reductions by the households that do not move.

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 missed payments are often treated as a loan. 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.

While clearly related, evictions and moves can represent distinct consequences. Moves occur without evictions, and can include forced moves or informal evictions. Additionally, depending on one's definition of eviction, not all evictions occur alongside moves. Researchers often define an eviction as receipt of a court's eviction order, and these orders do not always lead to moves.[25] Consistent with this, among job losers reporting an eviction in the SIPP, only 55.2% move in the year of the eviction, and 75.9% move by the following year. Because of these potential differences, I examine the impact of job loss on evictions and moves separately.

[24] Some households are missing data on whether they move, and this preferred upper bound treats moves status as missing at random. I also construct a more conservative upper bound, treating all missing observations as 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.

[25] Collinson et al. (forthcoming) defines an eviction as the receipt of an eviction order. Using a judge FE strategy, they estimate that an eviction order 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%) for the non-evicted individuals in court. These estimates show that some evicted tenants, defined as those receiving an eviction order, do not move.

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

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

The dependent variable is 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 (5), and the coefficient β captures the difference in the probability of eviction for households that lose jobs relative to those that do not. Dividing this coefficient to the earlier estimates of the effect of job loss on missed payments gives a measure of the share of missed payments that lead to evictions.

Table 4 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 0.8pp, while for owners the effect of job loss on evictions is negligible. Like missed payments, Figure 2 panel (b) shows that evictions are more common among those with low liquid assets. Overall, job loss increases missed payments by 7.6pp (Table 2), but evictions only increase by 0.4pp. Similarly, for renters, missed payments increase by 9.1pp while evictions rise by only 0.8pp. Dividing the additional evictions by the additional missed payments, it implies that only 5-10% of missed payments (caused by job loss) lead to evictions.

The small effect of job loss on evictions is robust to different methods of dealing with the potential endogeneity of job loss. Indeed, applying the method of Oster (2019) to assess unobservable selection, the bias-adjusted estimates are smaller than the unadjusted estimates (Online Appendix Table A.2 Panel B), indicating that unobserved selection may bias the estimates upward. Similarly, the estimated effect of job loss on evictions is smaller when restricting the sample to job losses that are less likely to be planned or voluntary, such as layoffs (Online Appendix Table A.3). Among households with no unemployment in the year prior to job loss, the effect on eviction is not statistically different from zero for renter or owners. One explanation is that landlords may be more willing to make these informal loans to households with previously stable employment, so these households face little risk of eviction.

Additionally, result that 5-10% of missed payments lead to evictions is 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.6 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.

Finally, I examine the impact of laws or policies that alter the costs of eviction may 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.7 and A.8, 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.[26] Overall, I find only mixed evidence that this informal credit use responds to policy, but the sample size in features of the SIPP are not ideal for this heterogeneity analysis.

Moves. 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. 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

[26] 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, 2020; 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.

administrative resources to track movers.[27] 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.

Figure 4 shows the rates at which households move following a job loss. Renters (panel a) move frequently, with about 30% moving out within twelve months following the job loss and 40-45% moving within 24 months. The rates at which households move, however, are similar for those who did and did not miss housing payments during this period. Moreover, 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, since some households who move are unable to be located. 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.

Because housing delinquency is not randomly assigned, these differences in move rates may not reflect the causal effect of missed housing payments. To investigate such bias, I begin by controlling for observable characteristics of these households. I restrict the sample to households in which a member experienced job loss, and also 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.[28] Thus, the sample consists of households who had been living in their residence for at least four months when they experienced job loss. I estimate regressions of the following form:

$$\texttt{move}_{ist} = \alpha + \beta\texttt{missed}_{ist} + \boldsymbol{X}_{ist}\gamma + \boldsymbol{Z}_{st}\xi + \delta_s + \tau_t + u_{ist}. \tag{7}$$

The dependent variable is an indicator for 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 (5).

[27]See SIPP User Guide, Chapter 2 for more information about the procedures for tracking movers.

[28]Appendix Table A.5 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.

Table 5 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 5, 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. The point estimates vary between those who miss payments being 1pp *less likely* to move to being 2.6pp more likely.

These controls address selection on these observable characteristics, but the estimates may still be biased due to unobservable selection into who misses payments. As with the earlier results, I use the method of Oster (2019) to assess the sensitivity of the estimates to different assumptions about unobservable selection (Online Appendix Table A.2 Panel C). The bias-adjusted estimates under the recommended assumptions indicate that renters who miss payments are 4pp more likely to move than those who do not. Under more conservative assumptions, renters who miss payments are 7.2pp more likely to move. This estimate is close to the share of renters missing payments who report being formally evicted: 7%. Thus, after adjusting for bias, the evidence still suggests that informal evictions or forced moves in response to missed payments are uncommon. For owners, the bias-adjusted estimates differ little from the baseline estimate, reflecting the stability of the coefficient estimates in Table 5. In sum, there is no evidence that missing payments increases the rate at which renters move out. Owners missing payments are more likely to move, but given that informal evictions are less common among owners, this may reflect the decisions of these owners rather than the consequences of missed payments.

6. Conclusion

This paper examines a consumption-smoothing *benefit* provided by the ability to fall behind on housing payments. Upon job loss, households frequently fall behind on housing payments, reducing 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, exceeding 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. As a potential policy implication, the availability of this informal credit may be influenced by laws that affect the ability to evict occupants.

This analysis shows that late payments provide valuable consumption smoothing during job loss, but much more evidence is needed to conduct a complete welfare analysis. I treat missed payments as a source of credit, but the data are not suitable for examining how or whether this debt is ultimately repaid, what the effective interest rate or fees are, and whether the debt is forgiven. There is likely substantial heterogeneity across landlords and lenders in their treatment of these informal loans. Additionally, while I examine the severe consequences of evictions and moves, there may be longer-run consequences in credit markets or screening for apartments, 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, as landlords and lenders may adjust prices or screening to compensate.

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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				
Unemployment and Missed Payments				
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				
Baseline annual household 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
Total income ($1,000s)	5.8	4.3	7.7	5.8
Credit card debt ($1,000s)	6.0	0.3	5.9	0.5
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
Credit card debt / income (%)	114.5	7.6	100.5	9.1
Post-period Characteristics				
Unemp. within household (%)	100.0	100.0	0.0	0.0
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				
Model:	(1)	(2)	(3)	(4)	(5)
Panel A: All Respondents					
Job loss	0.096***	0.080***	0.079***	0.079***	0.076***
	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)
Observations	55,553	55,553	55,553	55,553	55,553
Panel B: Renters					
Job loss	0.117***	0.095***	0.094***	0.094***	0.091***
	(0.011)	(0.010)	(0.010)	(0.010)	(0.009)
Observations	17,473	17,473	17,473	17,473	17,473
Panel C: Owners with Mortgage					
Job loss	0.075***	0.066***	0.065***	0.064***	0.062***
	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)
Observations	38,080	38,080	38,080	38,080	38,080
Controls					
Demographic		Yes	Yes	Yes	Yes
Financial			Yes	Yes	Yes
State Economic					Yes
Fixed-effects					
Year	Yes	Yes	Yes	Yes	Yes
State				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 controls for the household head include age, marital status, indicators for race as Black or other nonwhite, indicator for Hispanic ethnicity, indicators for education group (5 categories), baseline household income, and changes in household size or 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:			
	Behind housing 2+ months	Δhousing (%)	Δbills (%)	Δnondurable (%)
	(1)	(2)	(3)	(4)
Panel A: All respondents				
Job loss	0.075***	−0.048***	−0.043***	−0.080***
	(0.014)	(0.014)	(0.013)	(0.017)
N spells:	452	452	452	452
Observations	20,247	20,247	20,247	20,247
Panel B: Renters				
Job loss	0.079***	−0.075***	−0.065***	−0.076**
	(0.024)	(0.025)	(0.025)	(0.032)
N spells:	170	170	170	170
Observations	5,222	5,222	5,222	5,222
Panel C: Owners				
Job loss	0.069***	−0.035**	−0.032**	−0.084***
	(0.017)	(0.016)	(0.014)	(0.020)
N spells:	282	282	282	282
Observations	15,025	15,025	15,025	15,025
Fixed-effects				
Year-quarter	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 an indicator for being behind 2+ months of housing payments in quarters t or $t+1$ (column 1), the percentage change between the pre- and post-period in monthly housing payments (column 2), monthly bill payments (sum of housing, utilities, and auto payments) (column 3), 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: Impact of Unemployment on Eviction

	Dependent Variable: Evicted for Nonpayment				
Model:	(1)	(2)	(3)	(4)	(5)
Panel A: All Respondents					
Job loss	0.005***	0.004***	0.004***	0.004***	0.004***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Observations	55,543	55,543	55,543	55,543	55,543
Panel B: Renters					
Job loss	0.010***	0.009***	0.009***	0.009***	0.008***
	(0.003)	(0.003)	(0.003)	(0.003)	(0.002)
Observations	17,470	17,470	17,470	17,470	17,470
Panel C: Owners with Mortgage					
Job loss	0.0009	0.0005	0.0005	0.0006	0.0006
	(0.0009)	(0.0010)	(0.0010)	(0.0010)	(0.0009)
Observations	38,073	38,073	38,073	38,073	38,073
Controls					
Demographic		Yes	Yes	Yes	Yes
Financial			Yes	Yes	Yes
State Economic					Yes
Fixed-effects					
Year	Yes	Yes	Yes	Yes	Yes
State				Yes	Yes

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

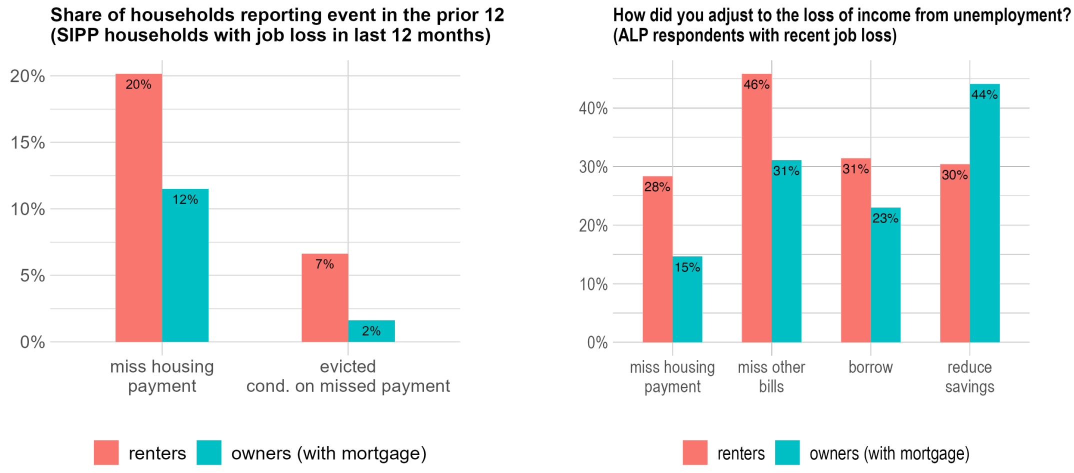
Data are repeated cross-sections of households from the 1991-2008 SIPP panels. The controls match the description in Table 2.

Table 5: Impact of Missed Payment on Moving Out

	Dependent Variable: Moved within 12 Months of Job Loss				
Model:	(1)	(2)	(3)	(4)	(5)
Panel A: All Respondents					
Missed Payment	0.062***	0.038**	0.036**	0.036**	0.035**
	(0.016)	(0.016)	(0.016)	(0.016)	(0.016)
Observations	3,206	3,206	3,206	3,206	3,206
Panel B: Renters					
Missed Payment	-0.010	0.008	0.014	0.029	0.026
	(0.026)	(0.024)	(0.024)	(0.025)	(0.024)
Observations	1,157	1,157	1,157	1,157	1,157
Panel C: Owners					
Missed Payment	0.049**	0.050**	0.048**	0.046**	0.046**
	(0.020)	(0.020)	(0.020)	(0.019)	(0.019)
Observations	2,049	2,049	2,049	2,049	2,049
Controls					
Demographic		Yes	Yes	Yes	Yes
Financial			Yes	Yes	Yes
State Economic					Yes
Fixed-effects					
Year	Yes	Yes	Yes	Yes	Yes
State				Yes	Yes

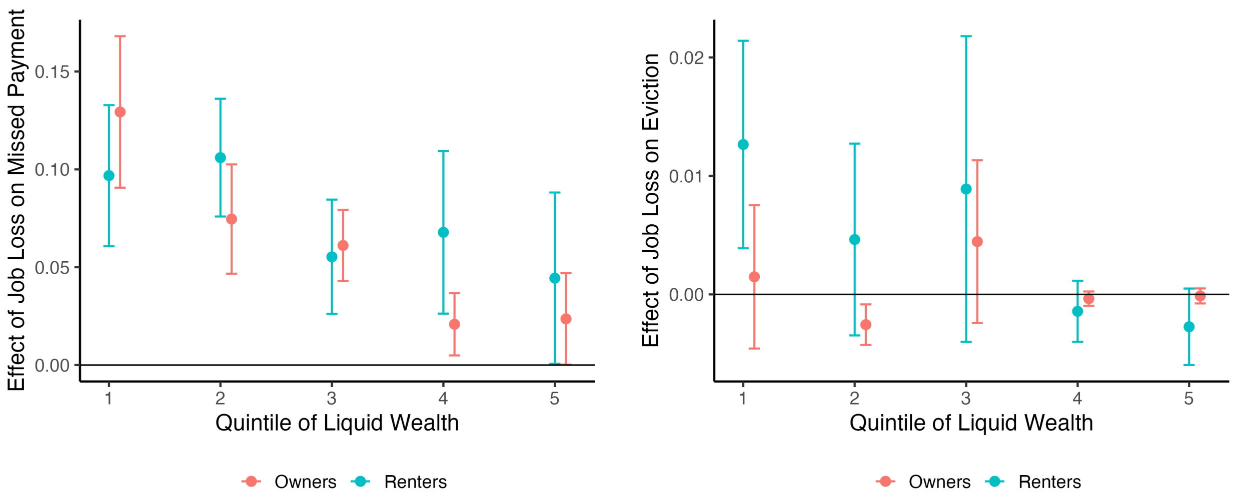
Clustered (State) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

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



(a) SIPP (b) ALP

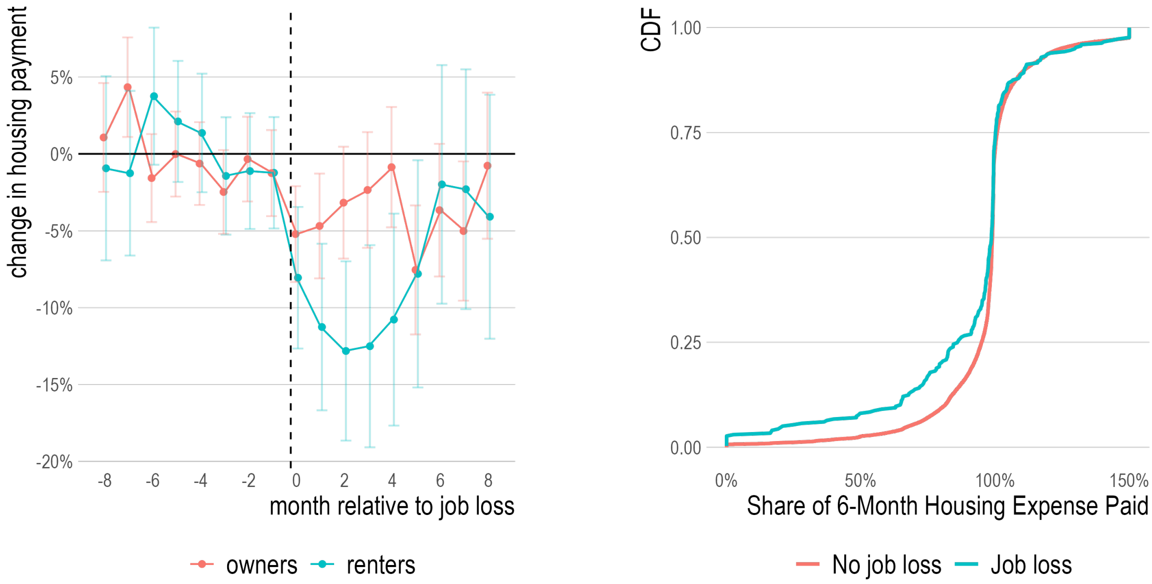
Figure 1: **How households cope with income loss from unemployment** Both SIPP and ALP households are restricted to renters or owners with a mortgage. ALP sample restricted observations after wave 8, when borrowing was added to the list of possible responses.



(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. 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 (5).

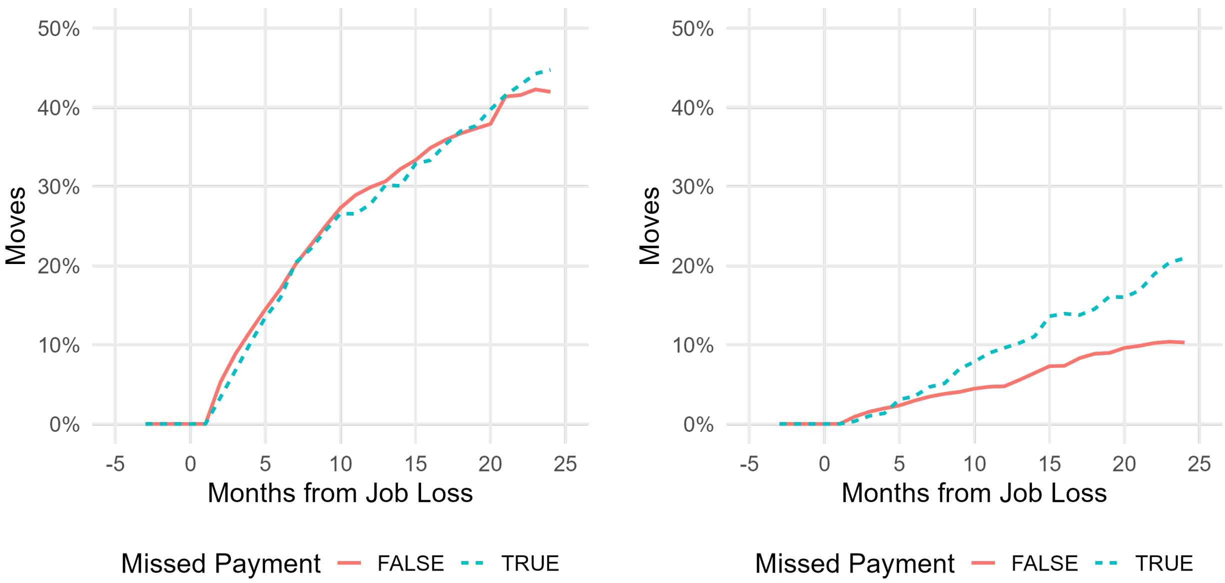


(a) Timing

(b) Distribution

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). Panel (a) reports estimates from the monthly event-study version of specification (6) for housing expenditure of renters and owners. Panel (b) shows the empirical cumulative distribution function (CDF) of the share of expected housing payments in the six-months following job loss that were actually paid, with separate distributions for households the do and do not experience job loss. Expected housing payments are estimated as six multiplied by the monthly (pre-unemp.) baseline housing expenditure. Appendix Figure A.1 repeats these figures using the full ALP panel and quarterly observations.



(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 sample consists of households who experienced job loss. I further restrict the sample where (i) the person who lost the job consistently lived in the household prior to the spell ($> 95\%$ of the sample) and (ii) the household did not move in the four months prior to job loss. For households that experience job loss in month 0, the figures show the cumulative share of household that have changed residence. Appendix Figure A.3 shows the figure without the further sample restrictions (i) and (ii).

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				
Unemployment and Missed Payments				
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				
Baseline annual household 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
Total income ($1,000s)	4.5	3.4	8.7	6.7
Credit card debt ($1,000s)	3.5	0.0	6.7	0.8
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
Credit card debt / income (%)	84.9	0.7	106.1	12.1
Post-period Characteristics				
Unemp. within household (%)	3.3	0.0	1.9	0.0
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	

NA

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: 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	0.077	0.068	0.062	0.046
Renters	0.093	0.082	0.074	0.054
Owners	0.062	0.056	0.052	0.041
Panel B: Evicted for Nonpayment				
All	0.004	0.003	0.003	0.002
Renters	0.008	0.007	0.007	0.005
Owners	0.001	0.000	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 4 in Panel B, and the impact of missed payments on moving from Table 5 in Panel C. Column (1) reports the unadjusted baseline from column (5) 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 5 column (5). Columns (3) and (4) report bias-adjusted estimates under alternative assumptions about $R^2_{\texttt{max}}$.

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	0.076***	0.075***	0.074***	0.004***	0.002**	0.001
	(0.006)	(0.005)	(0.007)	(0.001)	(0.001)	(0.0010)
Observations	55,553	53,822	49,973	55,543	53,812	49,964
Panel B: Renters						
Job loss	0.091***	0.099***	0.094***	0.008***	0.005	0.002
	(0.009)	(0.009)	(0.010)	(0.002)	(0.003)	(0.002)
Observations	17,473	16,741	15,099	17,470	16,738	15,096
Panel C: Owners with Mortgage						
Job loss	0.062***	0.057***	0.060***	0.0006	0.0003	0.0009
	(0.007)	(0.007)	(0.010)	(0.0009)	(0.0010)	(0.001)
Observations	38,080	37,081	34,874	38,073	37,074	34,868
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 4. 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: Changes in Expenditures after Job Loss (Monthly Data)

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

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

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

Table A.5: 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)
Panel A: All Respondents					
Missed Payment	0.061***	0.029	0.024	0.024	0.025
	(0.019)	(0.018)	(0.019)	(0.018)	(0.018)
Observations	3,511	3,511	3,511	3,511	3,511
R^2	0.00464	0.12220	0.12459	0.14644	0.14905
Panel B: Renters					
Missed Payment	-0.032	-0.005	0.0005	0.008	0.007
	(0.029)	(0.028)	(0.028)	(0.028)	(0.028)
Observations	1,404	1,404	1,404	1,404	1,404
R^2	0.00263	0.08085	0.08719	0.13831	0.14890
Panel C: Owners					
Missed Payment	0.040*	0.041*	0.037*	0.034*	0.037*
	(0.022)	(0.021)	(0.022)	(0.020)	(0.020)
Observations	2,107	2,107	2,107	2,107	2,107
R^2	0.00577	0.03945	0.04231	0.07806	0.08023
Controls					
Demographic		Yes	Yes	Yes	Yes
Financial			Yes	Yes	Yes
State Economic					Yes
Fixed-effects					
Year	Yes	Yes	Yes	Yes	Yes
State				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. The analysis sample is restricted to households that remain in the sample for at 12-months after the observed job loss. Relative to Table 5, these regressions also count moves that occur in the four months *before* job loss. The controls match the description in Table 2.

Table A.6: Impact of Missed Payment on Evictions

	Dependent Variable: Evicted				
Model:	(1)	(2)	(3)	(4)	(5)
Panel A: All Respondents					
Missed Payment	0.043***	0.043***	0.043***	0.043***	0.043***
	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)
Observations	4,382	4,382	4,382	4,382	4,382
R^2	0.03763	0.04009	0.04016	0.05284	0.05423
Panel B: Renters					
Missed Payment	0.066***	0.067***	0.067***	0.067***	0.066***
	(0.013)	(0.014)	(0.013)	(0.013)	(0.013)
Observations	1,763	1,763	1,763	1,763	1,763
R^2	0.05549	0.06181	0.06215	0.09753	0.09974
Panel C: Owners					
Missed Payment	0.016**	0.015**	0.015**	0.016*	0.016*
	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)
Observations	2,619	2,619	2,619	2,619	2,619
R^2	0.01509	0.02108	0.02114	0.04027	0.04320
Controls					
Demographic		Yes	Yes	Yes	Yes
Financial			Yes	Yes	Yes
State Economic					Yes
Fixed-effects					
Year	Yes	Yes	Yes	Yes	Yes
State				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. 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.7: 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.

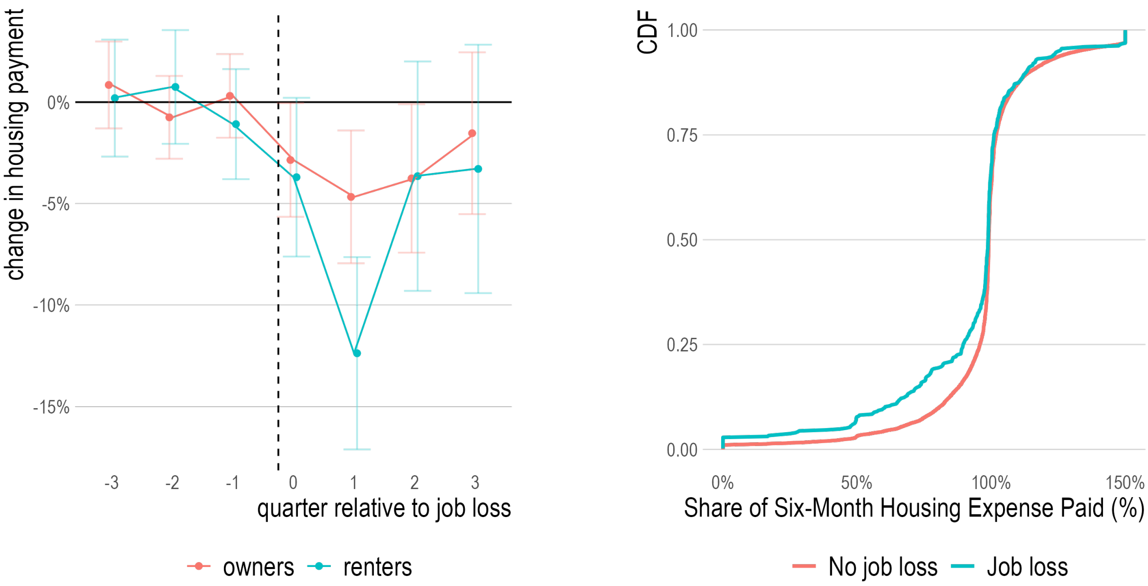
The first two columns examine state laws that make evictions more difficult. Column 1 interacts the job loss indicator with *high notice*, an indicator for whether the state has above-median eviction notice requirements (median is 5 days). Column 2 interacts job loss with high fee, an indicator for whether 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, 2020).

Table A.8: 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.7.



(a) Timing

(b) Distribution

Figure A.1: **Changes in Expenditure upon Job Loss (Quarterly)**

Data are quarterly, household-level observations from the RAND American Life Panel Financial Crisis Surveys from 2009-2016. Panel (a) reports estimates from the quarterly event-study version of specification (6) for housing expenditure. Panel (b) shows the empirical cumulative distribution function (CDF) of the share of expected housing payments in the six-months following job loss that were actually paid, with separate distributions for households the do and do not experience job loss. Expected housing payments are estimated as six multiplied by the monthly (pre-unemp.) baseline housing expenditure.

(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 that have 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 two restrictions: (i) the person who lost the job consistently lived in the household prior to the spell ($>$ 95% of the sample) and (ii) the household did not move in the four months prior to job loss.

Appendix B. Theory

The derivations of this section largely follow directly from Chetty and Szeidl (2007). I derive the comparisons made in Section 3, and then show illustrative examples using a quantitative model.

Appendix B.1. 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.[29] 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,[30] 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 agent (n, BC) and

[29] 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.

[30] The size of the shocks to induce a move vary across types of agents.

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

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

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.[31]

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

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

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

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

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.[32]

[31] 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.

[32] 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.

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,

$$\begin{aligned}\varepsilon_{E,W} &= \frac{\partial E}{\partial W}\frac{W}{E} = \frac{1}{T}T = 1\\ \varepsilon_{E,W}^{BC} &= \frac{\partial E^{BC}}{\partial W}\frac{W}{E^{BC}} = 1 \cdot \frac{W}{E^{BC}} \geq T.\end{aligned}$$

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,

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

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 $x = x_0$.

We can rewrite $\varepsilon_{f,E}^n = \frac{E}{E+(\frac{\gamma_f}{\gamma_x}-1)x^n}$, so that $\frac{d\varepsilon_{f,E}^n}{dE} = \frac{Ax^n(1-\varepsilon_{x,E}^n)}{(E+Ax^n)^2}$ where $A = \frac{\gamma_f}{\gamma_x} - 1 > 0$. Thus, the elasticity will be decreasing if $\varepsilon_{x,E}^n > 1$. From the consumer problem, we can solve the elasticity $\varepsilon_{x,E}^n = \frac{E}{\frac{\gamma_x}{\gamma_f}f^n + x^n} < 1$ because $E = f^n + x^n$ and $\gamma_x < \gamma_f$. Thus, both elasticities are decreasing.

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:

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

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

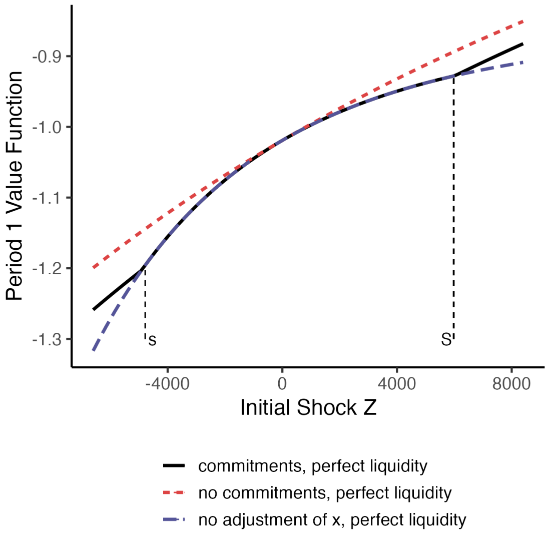
$$\frac{\gamma_c(\overline{W})}{\gamma_{n,BC}(\overline{W})} = \left(1 + \frac{\gamma_f}{\gamma_x}\frac{x}{f}\right)\frac{1}{T}.$$

Appendix B.2. Quantitative Example

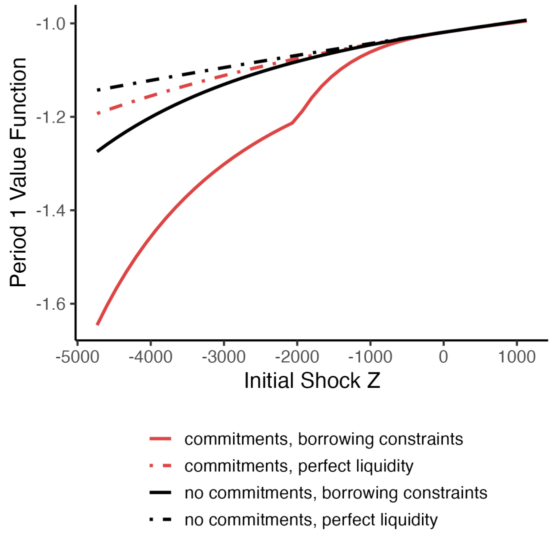
This section shows examples of curvature in the value function across types of agents. The first two panels of Figure B.1 show the impact of consumption commitments (panel a) and borrowing constraints (panel b) on an example agents' period zero value function over wealth (see Table notes for parameter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

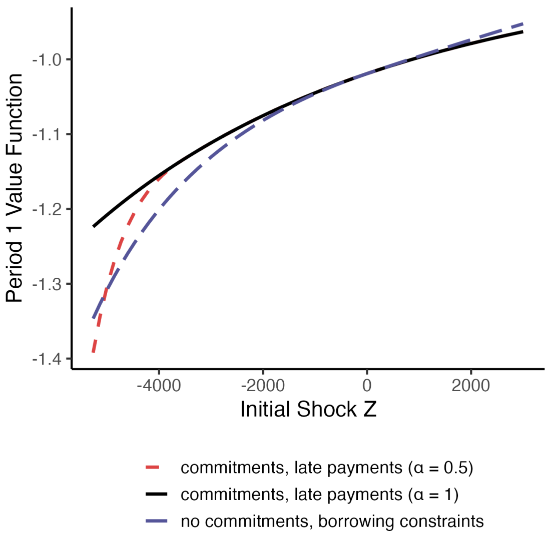
These comparisons show late payments can help smooth consumption, and this may be even more valuable if households facing shocks cannot move into cheaper residence. For example, some households facing shocks could already reside in the lower rungs of the housing ladder, so cheaper places may be unavailable. 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.



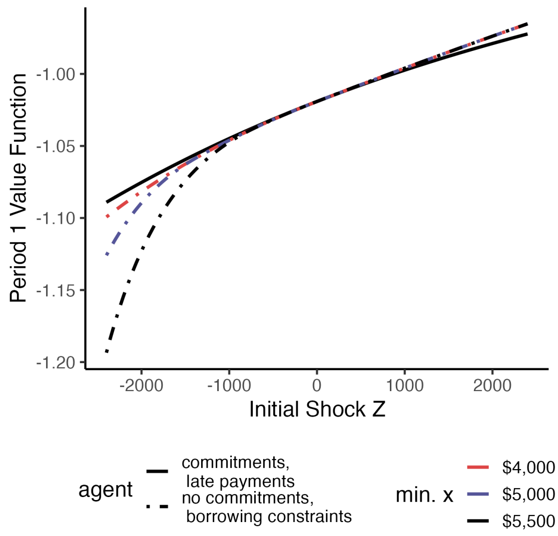
(a) Consumption Commitments



(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 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

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

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 (6), 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 (6) 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.

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.4 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 (6) 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.