How Credit Constraints Impact Job Finding Rates, Sorting & Aggregate Output*

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Abstract

We empirically and theoretically examine how consumer credit access affects displaced workers. Empirically, we link administrative employment histories to credit reports. We show that an increase in credit limits worth 10% of prior annual earnings allows individuals to take .33 to 2.67 weeks longer to find a job. Conditional on finding a job, they earn more and work at more productive firms. We develop a labor sorting model with credit to provide structural estimates of the impact of credit on employment outcomes, which we find are similar to our empirical estimates. We use the model to understand the impact of consumer credit on the macroeconomy. We find that if credit limits tighten during a downturn, employment recovers quicker, but output and productivity remain depressed as low-asset, low-productivity displaced workers being less able to self-insure. The results are consistent with displaced workers searching less thoroughly and taking more accessible jobs at less productive firms.

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Recent research by Kaplan and Violante [2014] has shown that many households, even those with large amounts of wealth, have very little liquid assets. At the same time, many of those households have significant amounts of credit access (Herkenhoff [2013]). This generates a potentially important consumption smoothing role for consumer credit if 'hand-to-mouth' households lose their jobs.

While much is known about the impact of unemployment benefits on employment outcomes (*inter alia* Katz and Meyer [1990], Ljungqvist and Sargent [1998], Acemoglu and Shimer [1998], Chetty [2008], Mitman and Rabinovich [2012], and Hagedorn et al. [2013]), little is known about the role consumer credit plays in the search decisions of unemployed households, and even less is known about how this interaction affects the macroeconomy.[1] How does consumer credit affect job finding rates, replacement earnings, or the types of jobs workers take? How does access to consumer credit affect the allocation of workers to firms, and what does this imply for labor productivity, output, and employment dynamics?

We examine these questions empirically and theoretically. Empirically, we link individual credit reports with administrative employment records. We use this dataset to measure the impact of consumer credit access on job finding rates and re-employment earnings of displaced workers. Theoretically, we develop a general equilibrium labor sorting model with consumer credit. We use the model to structurally estimate the impact of credit limits on job search behavior and to assess the impact of consumer credit on the aggregate economy.

Our results show that with increased credit displaced workers search longer for a new job and replace a higher fraction of their pre-displacement income. Empirically, we use several different instruments to control for the possibility that workers of higher quality have higher credit. These instruments include derogatory flag removals, automatic increases in credit card credit limits and housing supply elasticity in local real estate markets that has been shown to impact real estate prices.

We find that being able to replace 10% more of prior annual labor earnings with personal revolving credit allows medium-tenure displaced workers to take .33 to 2.67 weeks longer to find a job, and, among those who find a job, they obtain a .6% to 2.3% greater annual earnings replacement rate. Moreover, individuals with greater access to credit find jobs at

[1] The nascent but growing literature on the topic has focused on two mechanisms, the self-insurance role of credit (e.g. Athreya and Simpson [2006], Herkenhoff [2013], Athreya et al. [2014]) and labor demand effects of credit (e.g. Bethune et al. [2013], Donaldson et al. [2014]). The equally sparse empirical literature on unemployment and borrowing is limited due to data constraints (Hurst and Stafford [2004], Sullivan [2008] among others) but recent inroads are being made with new account data (Baker and Yannelis [2015], Gelman et al. [2015], Ganong and Noel [2015], Kolsrud et al. [2015] among others).

more productive firms, where productivity is proxied by the wage-per-worker. The dispersion in our point estimates come from different subpopulations; the largest point estimates tend to come from the most indebted and constrained sets of households.

Our results are consistent with individuals using personal credit to fund longer unemployment spells so that they can search and find better job matches. These results show that, independent of realized borrowing, the *potential* to borrow affects search decisions regardless if credit lines are actually drawn down. To our knowledge, we are the first to measure the elasticity of non-employment durations and the elasticity of earnings replacement rates with respect to consumer credit access.

In our model, heterogeneous credit-constrained workers accumulate human capital while working. When unemployed, they direct their search, as in Menzio and Shi [2010, 2011], for jobs among heterogeneous firms.[2] Firms differ with respect to capital and produce output by combining the human capital of workers with their own physical capital (for simplicity we refer to firm capital as physical capital, but this may also be thought of as intellectual capital). We assume supermodularity, meaning that firms with greater amounts of physical capital produce more with workers who have greater amounts of human capital. We therefore measure sorting in the model as the raw correlation coefficient between worker human capital and firm physical capital.[3] Which worker matches with which firm determines both output and labor productivity in this economy, and therefore the ability of unemployed households to self-insure, either through saving or borrowing, and search for higher physical capital jobs has the potential to change the path of a recovery.

Using this new theoretical framework, we make two quantitative contributions. First, we estimate the model using public data in order to provide a set of independent structural estimates of the elasticity of the duration of unemployment and the earnings replacement rate with respect to consumer credit access. We find an elasticity of duration with respect to unused credit of .61 (implying about a .7 week longer non-employment duration with a

[2]Related theoretical work includes sorting models with frictions (inter alia Bagger and Lentz [2008], Eeckhout and Kircher [2011], Hagedorn et al. [2012], Bonhomme et al. [2014]), frictionless assignment models with borrowing constraints (Fernandez and Gali [1999], Legros and Newman [2002], and Strauss [2013]), and occupational choice under credit constraints (inter alia Neumuller [2014], and Dinlersoz et al. [2015]).

Our work is also related to Lentz [2009], Krusell et al. [2010] and Nakajima [2012a] who have studied the impact of savings on search decisions, and Guerrieri and Lorenzoni [2011] among others have looked at the role household borrowing constraints play in models with frictionless labor markets.

[3]This measure is highly correlated with the Spearman Rank correlation coefficient. We also report the Spearman Rank correlation coefficient for completeness.

credit line worth 10% of prior income). This estimate falls in the middle of our reduced form estimates. We also estimate an elasticity of earnings with respect to unused credit, among those who find a job, of approximately 1.8%.

Second, given that the model produces earnings and duration elasticities that are consistent with the data, we use the model as a laboratory to examine the impact of credit on labor sorting, productivity, and the ensuing employment recovery during a recession. The main experiment we conduct using the model is to tighten borrowing limits during the 2007-2009 recession and then study the subsequent recovery. In particular, we simulate the 2007-2009 recession by feeding actual total factor productivity residuals into the model. During the recession, we permanently tighten borrowing limits, delivering roughly a 3 percentage point reduction in the fraction of households borrowing, and a 1 percentage point reduction in the aggregate debt to income ratio. Upon impact and throughout the recovery, the tighter credit limit depresses output per worker (labor productivity) by .28 percentage points and decreases overall output by .11 percentage points.

We find that when debt limits tighten, however, standard measures of sorting improve and remain elevated throughout the recovery. This happens as constrained households, who are also more likely to have low human capital, take jobs with low physical capital. Households with savings, who are more likely to have high human capital, are able to continue to search thoroughly for jobs with higher physical capital. Since sorting measures the correlation between human capital and physical capital, and since this correlation actually increases when debt limits tighten, standard measures of sorting improve. Sorting improves while output falls because the jobs that are being created during periods of tight debt limits are less-physical-capital-intensive jobs. What disconnects the positive comovement of sorting with labor productivity and output is the presence of firm investment and household credit constraints, both of which are typically absent in sorting models.

Lastly, when debt limits tighten during the recession, employment recovers more quickly. The mechanism is that when credit limits tighten, unemployed low-human-capital-borrowers lose their ability to self-insure and take relatively abundant jobs at less-capital-intensive firms. Constrained households take lower quality jobs relatively quickly, causing a tradeoff between the speed of the labor market recovery and the health of the recovery, measured by labor productivity and output.

Our contributions are both empirical and theoretical. Empirically, we build the first

dataset to merge individual credit reports with administrative employment records and measure the impact of consumer credit access on job finding rates and re-employment earnings of displaced workers. Using several different instruments, we show that with increased credit displaced workers search longer and replace a higher fraction of their pre-displacement income. Theoretically, we develop the first labor sorting model with consumer credit. We build on existing labor sorting models such as Marimon and Zilibotti [1999], Shimer and Smith [2000], Shi [2001], Shimer [2001], Lise and Robin [2013] and Eeckhout and Sepahsalari [2014], by generating interactions between heterogeneous credit histories and the allocation of workers to firms. We build on the influential work of Lise and Robin [2013] who consider sorting over the business cycle with risk neutrality, and we complement the recent innovative work of Eeckhout and Sepahsalari [2014], who study assets and sorting, by allowing for borrowing and productivity differences among workers. Our framework is tractable enough that it can be used by future researchers to study a variety of questions related to misallocation and credit access, including credit access among firms.

The paper proceeds as follows. We first describe our conceptual framework in Section 1. Section 2 describes the data. Sections 3 and 4 contain our empirical results. Section 5 presents the model, Section 6 describes the model estimation and structural estimates of the duration elasticity, Section 7 conducts the main counterfactual exercise of tightening debt limits, and Section 8 concludes.

1 Job Finding and Unsecured Credit

Unsecured credit allows unemployed households to augment today's liquid asset position by borrowing against future income. In McCall models of search, such as those studied by Athreya and Simpson [2006] and Chetty [2008], access to liquid assets allows households to search more thoroughly for higher wage jobs. While this mechanism is at the heart of the unemployment insurance literature, there is limited evidence linking access to liquid assets and job search decisions. In an influential paper, Chetty [2008] shows that workers who receive unemployment benefits take longer to find jobs, with the effect being strongest among low wealth households. He also shows that unemployed households who receive severance payments take significantly longer to find jobs.

However, to our knowledge, there are no existing studies documenting the way consumer

credit limits impact unemployment durations, subsequent wage outcomes, or the characteristics of the firms where these households ultimately take jobs. To fill this gap in the empirical literature, we test two hypotheses:

Hypothesis 1: Ceteris paribus, greater credit access among the unemployed increases non-employment durations.

Hypothesis 2: Ceteris paribus, greater credit access among the unemployed increases subsequent re-employment earnings.

It is important to note that because durations increase with greater credit access, the theoretic prediction of credit access on earnings (including zeros) is ambiguous since those who have more credit are taking longer to find jobs and so they are more likely to have zero earnings. However, *conditional* on finding a job, we test whether unemployed workers with greater credit access find higher wage jobs.

2 Data and Definitions

Our main data source is a randomly drawn panel of 4 million TransUnion credit reports which are linked by a scrambled social security number to the Longitudinal Employment and Household Dynamics (LEHD) database. All consumer credit information is taken from TransUnion at an annual frequency from 2001 to 2008. The TransUnion data includes information on the balance, limit, and status (delinquent, current, etc.) of different classes of accounts held by individuals. The different types of accounts include unsecured credit as well as secured credit on mortgages.

The LEHD database is a quarterly matched employer-employee dataset that covers 95% of U.S. private sector jobs. The LEHD includes data on earnings, worker demographic characteristics, firm size, firm age, and average wages. Our main sample of earnings records includes individuals with credit reports between 2001 and 2008 from the 11 states for which we have LEHD data: California, Illinois, Indiana, Maryland, Nevada, New Jersey, Oregon, Rhode Island, Texas, Virginia, and Washington. Since job dismissal and reason of dismissal are not recorded in the LEHD, we follow Jacobson et al. [1993] and focus on mass layoffs.[4]

We then define several labor market variables of interest. First, we define non-employment

[4]Online Appendix A includes details on the identification of mass layoffs.

duration to be the number of quarters it takes an individual to find a job following a mass displacement.[5] Non-employment duration therefore takes values ranging from 0 (indicating immediate job finding) to 9 (all spells longer than 9 quarters of non-employment are assigned a value of 9).[6]

Second, we define replacement earnings as the ratio of annual earnings 1 year after layoff over annual pre-displacement earnings. Suppose a worker is displaced in year t, then we define the replacement earnings ratio to be the ratio of annual earnings in the year after layoff, in year $t+1$, to the pre-displacement annual earnings, in year $t-1$. To avoid confounding the duration of non-employment with replacement earnings, when we measure replacement earnings, we condition on individuals who have a full year of earnings in year $t+1$. We consider longer-term measures of replacement earnings (e.g. in year $t+2$) in Online Appendix C.

We focus on revolving credit from TransUnion because it can be drawn down on short notice following job loss and paid-off slowly over time without any additional loan-applications or income-checks. Our main measure of credit access is therefore an individual's unused credit limit across all types of revolving debt (excluding mortgage related revolving debt) over annual earnings, measured prior to displacement.[7] We call this ratio the 'unused revolving credit limit ratio.'[8] The main components of revolving credit include bank revolving (bank credit cards), retail revolving (retail credit cards), and finance revolving credit (other personal finance loans with a revolving feature). In Online Appendix B.5 we use alternate measures of credit access prior to layoff including (i) credit scores, (ii) unused revolving credit inclusive of HELOCs, and (iii) total secured and unsecured unused credit.

[5] We follow Abowd et al. [2009] (Appendix A, Definitions of Fundamental LEHD Concepts) to construct our measures of job accessions and employment at end-of-quarter. See Online Appendix A for more discussion.

[6] Very few workers in our sample of displaced workers remain non-employed for longer than 4 quarters. Changing the censored value to 8 or 10 has no impact on the results.

[7] The reason that our main measure of credit access excludes mortgage related credit is because we want to isolate the impact of credit access on employment, independent of housing wealth. To control for the component of housing wealth that can be drawn down upon job loss, we include directly HELOC limits and home equity proxies as controls in our empirical analysis.

[8] Online Appendix A includes details on the construction of this ratio.

3 Empirical Approach

The goal of this section is to estimate the impact of credit access on employment outcomes of displaced workers. While many authors, including Jacobson et al. [1993], have argued that mass layoffs are exogenous to worker characteristics, credit access upon layoff certainly is not. To solve this issue, we need to find a characteristic of workers that impacts credit limits and only impacts employment prospects through its impact on credit limits. To isolate such exogenous variation in credit limits, we use three sets of orthogonal instruments which we discuss in detail in the following sections: (i) derogatory flag removals, (ii) individual-specific account ages and (iii) geography instruments.

3.1 Derogatory Flag Removals

Our first strategy is to follow Musto [2004] who exploits the fact that bankruptcy flags are removed, by law, after ten years, and this gives rise to a large exogenous increase in credit access which does not reflect changes in underlying credit worthiness or unobserved quality of the individual.[9] The fair credit reporting act requires most derogatory credit events to be removed after 7 years (Ch. 7 bankruptcy is an exception). What matters for us is that the removal is statutory and a function of time, not the exact amount of time for which the flag is on the record. We therefore include all derogatory flag removals in our regressions to expand the sample and improve the external validity of our point estimates.

Consider the set of displaced workers who have a constant, but positive, number of derogatory flags on their record prior to displacement, and let $Removal_{i,t}$ equal 1 if the worker has all of their derogatory flags removed in the year before, or year of displacement. Our empirical approach is to compare those who have their derogatory flags removed in the year of displacement (the treatment group) to those whose flags remain on their record in the year of displacement (the control group). We therefore implement the following specification in order to isolate the impact of increased credit access on outcome variables such as duration, $D_{i,t}$,

$$D_{i,t} = \gamma Removal_{i,t} + \beta X_{i,t} + \epsilon_{i,t} \tag{1}$$

[9] Chapter 7 Bankruptcy flags (the most pervasive type of bankruptcy) remain on a worker's credit report for a statutory 10 years, whereas Chapter 13 flags are removed after 7 years. We cannot differentiate between these types of flag removal, but what matters for us is that the removal is statutory.

In this specification γ represents how much longer displaced workers take to find a job if their flag is removed relative to the control group of workers whose flags are not removed. The key identifying assumption for this specification is that the treatment and control group have identical outcomes if no treatment had occurred. We formally test this assumption in Online Appendix B.1 by showing that the main outcome variables for the treatment and control group are statistically indistinguishable prior to the flag removal. In particular, we show that mean earnings growth, time spent non-employed, firm size, and firm wage prior to layoff are statistically indistinguishable across the treatment and control group prior to layoff.

3.2 Gross and Souleles Instrument

Our second approach is based on the identification strategy of Gross and Souleles [2002] who exploit the fact that credit card limits increase automatically as a function of the length of time an account has been open.[10] We exploit these time-contingent changes in credit access by using the age of the oldest account as an instrument for credit limits.

More formally, let i denote individuals and t denote years. The first-stage regression is to predict the unused credit limit ratio prior to layoff ($l_{i,t-1}$) as a function of the age of the oldest account, $s_{i,t}$, and a vector of controls $X_{i,t}$.

$$l_{i,t-1} = \pi s_{i,t} + BX_{i,t} + u_{i,t} \tag{2}$$

These first-stage estimates of π and B are used to isolate the exogenous component of the unused credit limit ratio, $\hat{l}_{i,t-1}$. The second stage regression is then used to estimate how this exogenous variation in credit impacts employment outcomes such as duration, $D_{i,t}$.

$$D_{i,t} = \gamma \hat{l}_{i,t-1} + \beta X_{i,t} + \epsilon_{i,t} \tag{3}$$

The main challenge to exogeneity for this instrument is that account ages are related to physical ages. Unlike credit scoring companies, however, we observe physical age. We argue and provide supporting evidence with over-identification tests that, after controlling for

[10] As we discuss in Online Appendix B.4, the general mechanism is that credit issuers revise account limits based on set time intervals. The subsequent limit revision is a function of credit scores, and credit scores, by construction, positively weight account ages.

physical age as well as a host of other individual characteristics, that account age satisfies the exclusion restriction. In Online Appendix B.4 we discuss this instrument and the over-identification tests in more detail.

3.3 Saiz Instrument

Our last approach is based on Mian and Sufi [2012] who show that geographic constraints, such as the Saiz [2010] housing supply elasticity, significantly impact house price growth as well as leverage and are orthogonal to labor markets except through their impact on leverage (excluding real estate related sectors). Our analysis relies on the arguments made in Mian and Sufi [2012], but, rather than focusing on *realized leverage* (realized borrowing), the channel we emphasize is that geographic constraints impact house price growth, and house price growth is a determinant of *credit access*, and in particular, credit limits. We therefore use the MSA-level housing supply elasticity as the instrument, $s_{i,t}$, in the first stage regression (2) for credit limits.

There are two reasons why house prices determine access to revolving credit: (i) workers have more access to capital and are less likely to default, increasing the propensity of lenders to extend any type of credit, and (ii) lenders expect workers to consume more, and therefore offer more credit cards since they profit from transaction volume (not just balances). In the first-stage regression, from a purely statistical point of view, we show that the Saiz [2010] geographic constraint instrument is a strong predictor of the unused credit limit ratio of individual workers for the 38 MSAs present in our sample. In the second stage regression, the predicted unused credit limit ratios from the first stage are used to measure the impact of credit on non-employment durations and annual earnings replacement rates.

The two main challenges to exogeneity of the Saiz [2010] instrument are (i) aggregate conditions, and (ii) housing wealth. We conduct a thought experiment to address the first challenge. While Mian and Sufi [2012] argue that there is no correlation between the supply elasticity and aggregate conditions except through leverage, if there is a correlation, it should bias our results toward zero. Suppose MSAs with low supply elasticities have quickly rising house prices and have better labor markets, then credit should expand and non-employment durations should be shorter in those MSAs. As we will show below, our IV estimates imply the opposite relationship.

To mitigate concerns about housing wealth, we include an equity proxy (the highest mortgage balance ever observed less the current balance) and HELOC limits (home equity lines of credit) in all specifications. We argue that HELOCs isolate the amount of home equity that can be used as an ATM immediately following a job loss. In other words, the HELOC credit limit just prior to a job loss is a good proxy for access to liquid housing assets during the non-employment spell. Furthermore, we do not see workers disproportionately take out new mortgages (which would indicate potential cash-out refinancing) or pay off mortgages (indicating a sale) during or after layoff.

To further address concerns over housing wealth and wealth more generally, we conduct two additional exercises. First, in Online Appendix B.2, we report OLS regressions of unemployment duration on unused credit, directly controlling for the OFHEO house price index. We show that the inclusion of house prices does not affect our point estimates. Second, in Online Appendix B.3, we use the 1998 to 2007 Survey of Consumer Finances (SCF) surveys, and we show that the relationship between non-employment duration and unused credit card limits (the only limit available in the SCF) is similar to our IV estimates and unaffected by the direct inclusion of self-reported home values, liquid assets, or illiquid assets.

Lastly, in Online Appendix B.4 we provide the closest possible test of whether or not the Saiz instrument is exogenous by using Hansen's J-test (over-identification test). The over-identification tests show that the level component of unused credit which is being isolated by the Saiz instrument satisfies the exclusion restriction.

3.4 Sample Descriptions and Summary Statistics

Based on these identification strategies, we use three samples in this paper.

i. **All Displacements Sample:** Our first sample includes all prime-age displaced workers who had at least 3 years of tenure at the time of displacement. These are standard restrictions used in the literature, e.g. Davis and Von Wachter [2011], to mitigate any issues associated with seasonal employment or weak labor-force attachment. Under these criteria we end up with a sample of 81,000 individuals (to the nearest thousand). Given the way we identify displacements, and our use of lagged credit prior to displacement, this sample covers the years 2002-2006.

ii. **Displaced Mortgagor Sample:** Our second sample includes prime-age displaced

workers with mortgages who had at least 3 years of tenure at the time of displacement, and worked in a non-real-estate industry. Given these criteria we end up with a sample of 30,000 individuals (to the nearest thousand). This sample covers the years 2002-2006.

iii. **Displaced with Derogatory Flag Sample:** Our third sample includes prime-age displaced households who had a constant number of derogatory flags (including bankruptcy, tax liens, foreclosures, civil judgements etc.) on their record in the two years prior to displacement. We then split this sample into a treatment group of 4,000 individuals (to the nearest thousand) whose flags are removed in the year before displacement or year of displacement, and a control group of 17,000 individuals whose flags remain on their records throughout the displacement.[11] In order to garner enough observations to disclose this sample, we could only impose a tenure requirement of 1 year. This sample covers the years 2002-2006.

Table 1 includes summary statistics for each sample. All variables are deflated by the CPI, and the top 1% (and bottom 1% if the variable is not bounded below) of continuous variables are winsorized. Columns (1) and (2) of Table 1 summarize the displaced derogatory sample. Our displaced derogatory sample is split into a treatment and control group. On average the demographic characteristics between these two groups are quite close: imputed years of education is 13.6 years for those who have their flag removed (the treatment group) and 13.4 for those who do not have their flag removed (the control group) and average tenure is 3.9 years in both the treatment and control groups.

Individuals whose flags are removed are naturally older, with an average age of 43.2 in the treatment group and an average age of 42.1 in the control group. The treatment group earned \$46.3k before displacement whereas the control group earned \$42.1k. While there is a level difference in earnings, in Online Appendix B.1, we show that the main outcome variables for the treatment and control group, including wage growth and time spent non-employed, are statistically indistinguishable prior to the flag removal. The treatment group takes 2.58 quarters to find a job on average after displacement, and their earnings replacement rate is 76%, whereas the control group takes 2.39 quarters on average to find a job after displacement, and their earnings replacement rate is 79%. We show in the regression analysis that the difference in durations is significant after adjusting for composition, whereas the

[11] Census requires sample numbers to be rounded off to the nearest hundred to ensure no individual data is disclosed or can be inferred. We round to the nearest thousand to allow for quicker disclosure of results.

difference in earnings replacement rates is reversed (i.e. the treatment group has a higher replacement rate).

Turning to the displaced mortgagor sample, Column (3) of Table 1 shows that average annual labor earnings prior to layoff was about $54.7k and that the average worker could replace 45% of their prior annual labor earnings with unused revolving credit.[12] After layoff, they took roughly 2.53 quarters to find a new job, and their annual earnings replacement rates were 79% one year after mass displacement, including zeros, similar to what Davis and Von Wachter [2011] find. Finally, the age of the oldest account is approximately 14.6 years on average in our sample. Column (4) of Table 1 shows that if we condition on displaced mortgagors who have a job in the year following displacement (i.e. in period $t+1$), the average duration is .77 quarters, and the average earnings replacement rate is 1.06 (meaning a more-than-full recovery of earnings). By conditioning on employment, this sample drops earnings replacement rates equal to zero and thus parses out any effects of duration on earnings.

For the entire sample of displaced individuals, Column (5) of Table 1 shows that they were on average 38.8 years old, took 2.43 quarters to find a job, and had an earnings replacement rate of 81%. They could replace roughly 29% of their prior earnings with unsecured revolving credit, and the average age of their oldest account is 11.58 years. Column (6) shows the same sample conditional on the individual finding a job in the preceding year (at t+1). On average, their earnings replacement rate is above 1, their lagged prior income is slightly higher, and they could replace about 27% of their prior earnings with unsecured credit.

To illustrate composition corrected correlations between access to credit, unemployment durations, and replacement rates, we run regressions of unemployment durations and replacement rates on quintiles of unused credit to income, controlling for basic demographics such as age, sex, and education. In particular, Figure 1 plots the coefficients, β_j, from a regression of duration on unused revolving credit to income quintiles prior to layoff for the full sample of displaced workers ($dur_{i,t} = \sum_j \beta_j \mathbb{I}_{i,t}(j) + \Gamma X_{i,t} + \epsilon_{i,t}$ where $\mathbb{I}_{i,t}(j)$ is a dummy if individual i is in quintile j of the unused credit to income distribution). Relative to the second quintile of credit access, those in the fifth quintile take approximately .5 quarters longer to find a job. Figure 2 plots the coefficients, β_j, from a regression of replacements rates on unused revolving credit to income quintiles prior to layoff for the sample of displaced

[12]The distribution of available credit is skewed. In the SCF, *unused credit card limits* to annual *family* income among the unemployed peaks at 38% in 1998, and among the employed it peaks at 33% in 2007.

workers who are employed at t+1 ($reprate_i = \sum_j \beta_j \mathbb{I}_{i,t}(j) + \Gamma X_{i,t} + \epsilon_{i,t}$). Relative to the second quintile of credit access, those in the fifth quintile have a 10% higher replacement rate. Both figures reveal a generally monotone increasing relationship between unused credit prior to layoff and both durations and earnings replacement rates, with a pronounced rise in the last quintile of unused credit. The empirical analysis that follows is designed to draw causal inference about the relationships seen in Figures 1 and 2, and express the relationship as an elasticity that can be mapped to models.

4 Empirical Results

For each dependent variable, we show four sets of results corresponding to (1) OLS for the derogatory flag-drop sample, (2) OLS for all displacements, (3) the account age IV (GS-IV) for all displacements, and (4) the geography IV (Saiz-IV) for the displaced mortgagor sample. In every specification, our vector of controls ($X_{i,t}$) includes quadratics in age and tenure as well as sex, race and education dummies, lagged annual income, cumulative lagged earnings (to proxy for assets), 1-digit SIC industry dummies, lagged characteristics of the previous employer including the size, age, and wage per worker, a dummy for the presence of auto loans, an equity proxy (the highest mortgage balance observed less the current balance), HELOC limits (to proxy for available housing wealth upon layoff), as well as year dummies, the MSA unemployment rate, and MSA income per capita.

4.1 Duration Results

Table 2 illustrates the impact of unused credit on durations. Column (1) uses the displaced derogatory sample, and shows the impact of a derogatory flag drop on duration. The point estimates imply that if an individual's derogatory flag is dropped in the year of displacement, they take on average .112 quarters longer to find a job (about 1.3 weeks longer), relative to the control group. Column (2), which uses the displaced mortgagor sample, illustrates the results from a simple OLS regression of duration on unused credit limits. The estimates reveal a duration elasticity of .279, which implies that being able to replace 10% more of prior annual income with unused credit is associated with an increase in duration of .33

weeks.[13]

In Column (3) we instrument the unused credit limit with the Gross and Souleles [2002] instrument. We find a duration elasticity of .442 which implies that being able to replace 10% more of prior annual income with unused credit allows workers to take roughly .53 weeks longer to find a job. In Column (4), we instrument the unused credit limit with the Saiz [2010] instrument. We find a duration elasticity of 2.23 which implies that being able to replace 10% more of prior annual income with unused credit allows workers to take roughly 2.66 weeks longer to find a job.

To convert the derogatory flag removal result into an elasticity, Table 3 shows that a derogatory flag is associated with a contemporaneous increase in revolving credit limits equal to 10.8% of prior annual income and an increase in credit scores of 123 pts. This implies that being able to replace 10% of prior annual income allows displaced workers to take 1.24 weeks longer to find a job (=.1*.112*12/.108).

Our estimates imply that $1 of additional unused credit limit is about half to three-quarters as potent for unemployment durations as $1 of unemployment benefit. Being able to replace 5% of annual earnings on a credit card is equivalent to a 10% increase in UI replacement rates for the typical 6-month duration of unemployment benefits. In the empirical UI literature, the impact of a 10% increase in the UI replacement rate for 6 months is to increase unemployment durations by .3 to 2 weeks with the modal estimate lying between .5 and 1 for the US (see Nakajima [2012b] and Card et al. [2015] for a summary of recent empirical and quantitative elasticities). Our estimates imply an equivalent elasticity with respect to credit of .15 to 1.3 weeks.

For robustness, Online Appendix C.1 merges our sample with Schedule C tax records to adjust the non-employment spells for self-employment. Online Appendix C.1 also uses the earnings gap method to infer partial quarters of non-employment. Under either of these definitions of non-employment duration, we find that the main results hold.

[13].33 weeks comes from multiplying the 10% increase in credit access to income by the OLS coefficient and then multiplying again by 12 weeks per quarter (i.e. .33 weeks=.1*.279*12).

4.2 Earnings Replacement Rates

Earnings Replacement Rates Excluding Zeros: To avoid confounding annual replacement earnings with durations, in Table 4 we isolate the set of households who have positive earnings in each quarter during the year after layoff. Table 4 reveals that conditional on finding a job, those with greater credit access find higher wage jobs.

Column (1) of Table 4 shows that if an individual's derogatory flag is dropped in the year of displacement, their replacement rate 2.3% higher. Columns (2) through (4) of Table 4 show that being able to replace 10% more of prior annual income increases the earnings replacement rate of job finders by .6% in the OLS regressions, by 1.34% using the Gross and Souleles instrument, and by 2.33% using the Saiz Instrument.

Earnings Replacement Rates Including Zeros: Table 5 illustrates the impact of unused credit on replacement rates of annual earnings, including zeros for those who do not find a job. While Column (3) reveals a significant positive impact of credit on replacement rates inclusive of zeros, the point estimates in Column (1), (2), and (4) of Table 5 suggest that the impact of additional credit access on replacement earnings is statistically indistinguishable from zero. There are two competing forces generating this result: (i) durations increase with more credit access, depressing replacement earnings, (ii) of those who find a job, those who have more credit access find higher wages, increasing replacement earnings. In the sections that follow, we show that the model's self-insurance mechanism generates the same offsetting forces.

These results are, in general, in line with US estimates in the UI literature. Studies that have considered the impact of unemployment benefits on re-employment earnings have found positive and significant but mixed-magnitude effects in US data (see Addison and Blackburn [2000] for a summary), whereas European studies have found both positive and insignificant effects, as well as negative effects in one case (see Nekoei and Weber [2015] for a summary). In Online Appendix C, we explore earnings replacement rates at longer horizons for this sample.

4.3 Productivity of Firms

We show that among individuals who find a job in the year after displacement, those with greater credit access are more likely to work at more productive firms. Our main dependent

variables are indicator functions for whether the worker finds a job at a firm which is ranked above the 50th, 75th, or 90th percentile of the wage-per-worker distribution in the year after job loss.[14] This is our proxy for productivity.

Panel (A) of Table 6 illustrates the impact of unused credit on the odds that a worker finds a job at a firm above the 50th/75th/90th wage-per-worker percentile following derogatory flag removal. After flag removal, individuals are 2.2% more likely to work for a firm in the 75th percentile of the wage distribution or better. The impact of credit on a worker's ability to find employment at firms above the 90th percentile of the wage-per-worker distribution is smaller and insignificant.

Using the Gross and Souleles instrument in Panel (B) reveals a similar pattern– being able to replace 10% more of prior annual income increases the odds that a worker finds a job at a firm in the 50th percentile of the wage-per-worker distribution or better by 1.2%, in the 75th percentile of the wage-per-worker distribution or better by 1.59%, and in the 90th percentile of the wage-per-worker distribution or better by .8%. Again, credit has a weaker impact on finding jobs at the far-right tail of the wage-per-worker distribution, but credit helps workers find jobs at firms in the top half and top quartile of the productivity distribution.

Using the Saiz instrument, Panel (C) shows that being able to replace 10% more of prior annual income increases the odds that a worker finds a job at a firm in the 50th percentile of the wage-per-worker distribution or better by 2.8% and in the 75th percentile of the wage-per-worker distribution or better by 1.15% (although that point estimate is insignificant). There is no impact of credit on the odds that a worker finds a job in the 90th percentile of the wage-per-worker distribution using the Saiz instrument. Overall, the modal estimates are positive for the 50th and 75th percentile, implying that those with greater credit access prior to layoff are more likely to be re-employed at more productive firms.

4.4 Borrowing by Displaced Workers

One important point of our empirical section is that regardless of realized borrowing, the *potential* to borrow affects job search decisions regardless if the credit line is actually drawn

[14] What we call firms in the text are State Employment Identification Numbers (SEINs) in the LEHD. SEINs aggregate all plants within a state. Wage-per-worker is calcuated as the aggregate wage bill divided by total employees.

down. Workers know that if their buffer stock of liquid assets is depleted, they can borrow, and this affects their job search decisions even if they never borrow. Existing work by Sullivan [2008] using the PSID and SIPP has shown that about 20% of workers borrow during unemployment, and it is precisely low wealth workers who borrow during unemployment. Recent work by Collins et al. [2015] has updated Sullivan [2008] through the great recession and found similar results, with greater borrowing occurring among low wealth households in the crisis.

We plot the distribution of bankcard borrowing among displaced workers. Figure 3, which is a smoothed density, plots the change in bankcard balance among displaced workers in the year of layoff relative to one year before layoff. The graph reveals significant heterogeneity in borrowing responses among displaced workers. Some workers borrow, consistent with Sullivan [2008], whereas some workers save, also consistent with Sullivan [2008]'s regression results. As a result, the net amount borrowed among displaced workers is close to zero, consistent with recent findings (e.g. Gelman et al. [2015], Ganong and Noel [2015], Kolsrud et al. [2015]). However, this masks quite large and economically significant heterogeneity in borrowing by displaced workers.

As more direct evidence of borrowing by the unemployed, we include Table **??**, which is from Herkenhoff [2013]. Table **??** reveals that over one-quarter of job losers borrow to replace income, and over one-third of job losers skip their obligated monthly payments and become delinquent (I will refer to this as default) to smooth consumption. This evidence supports the mechanism that unconstrained unemployed individuals borrow, whereas among job losers who are indebted or have other obligated payments, they become delinquent to smooth consumption (I will also refer to this as default). The ability to default separates credit from unemployment insurance or other safety-net programs, and this evidence reveals that it is used frequently by job losers.

For our sample, Table 7 illustrates regression results for the relationship between non-employment duration and borrowing, controlling for as many characteristics of workers as possible. The coefficient in Column (1) implies that for every additional quarter of non-employment, workers borrow on average $84, which is a relatively small average amount. However, as discussed above, this regression confounds the offsetting impact of borrowing (which increases balances) and default (which tends to reduce balances). The result is a small average impact even though both mechanisms provide consumption smoothing to job losers via credit markets. Column (2) shows that for every additional quarter of non-employment,

workers are .14% more likely to enter collection on their debts (i.e. they have defaulted and their delinquent accounts have been sold to a collection agency).

5 Model

Given our IV estimates are inherently local estimates, we build a structural model that we use to obtain independent 'global' estimates of the unemployment duration and earnings replacement rate elasticities. We then use the model to conduct our main experiment which is to consider how changes in aggregate borrowing limits impact the allocation of workers to firms, output, and productivity.

Let $t = 0, 1, 2, \ldots$ denote time. Time is discrete and runs forever. There are three types of agents in this economy. A unit measure of risk averse finitely-lived households, a continuum of risk neutral entrepreneurs that run the endogenously chosen measure of operating firms, and a unit measure of risk neutral lenders.

As in Menzio et al. [2012], there are $T \geq 2$ overlapping generations of risk averse households that face both idiosyncratic and aggregate risk. Each household lives T periods deterministically and discounts the future at a constant rate $\beta \in (0, 1)$. Every period households first participate in an asset market where they make asset accumulation, borrowing, and bankruptcy decisions. After the asset market closes, households enter the labor market where they direct their search for jobs.[15] Let $c_{t,t+t_0}$ and $L_{t,t+t_0}$ respectively denote the consumption and hours worked of an agent born at date t in period $t + t_0$. The objective of a household is to maximize the expected lifetime flow utility from non-durable consumption and leisure.

$$\mathbb{E}_t \left[\sum_{t_0=1}^{T} \beta^{t_0} u(c_{t,t+t_0}, 1 - L_{t,t+t_0}) \right]$$

From this point on we will drop time subscripts and focus on a recursive representation of the problem. We assume that labor is indivisible, such that the household consumes its entire time endowment while employed $L = 1$, and vice verse for the unemployed.

[15] The way directed search is modeled in this paper rules out the possibility that wage gains may simply reflect differences in bargaining power and outside options.

Households are heterogeneous along several dimensions. Households are either employed or unemployed, where employed value functions are denoted W and unemployed value functions are denoted U. Let $e \in \{W, U\}$ denote employment status. Let $b \in \mathcal{B} \equiv [\underline{b}, \bar{b}] \subset \mathbb{R}$ denote the net asset position of the household, where $b > 0$ denotes that the household is saving, and $b < 0$ indicates that the household is borrowing. Let $h \in \mathcal{H} \equiv [\underline{h}, \bar{h}] \subset \mathbb{R}_+$ denote the human capital of the worker. Workers also differ with respect to the capital $k \in \mathcal{K} \equiv [\underline{k}, \bar{k}] \subset \mathbb{R}_+$ of the firm with which they are matched, and with respect to their credit access status $a \in \{G, B\}$ where $a = G$ denotes good standing, and $a = B$ denotes bad standing. Let $\mathbb{N}_T = \{1, 2, \ldots, T\}$ denote the set of ages.

The aggregate state of the economy includes three components: (i) total factor productivity (TFP) $y \in \mathcal{Y} \subset \mathbb{R}_+$ and (ii) the borrowing limit $\underline{b} \subset \mathbb{R}_-$, and (iii) the distribution of agents across states $\mu : \{W, U\} \times \{G, B\} \times \mathcal{B} \times \mathcal{H} \times \mathcal{K} \times \mathbb{N}_T \to [0, 1]$. Let $\Omega = (y, \underline{b}, \mu) \in \mathcal{Y} \times \mathbb{R}_- \times M$ summarize the aggregate state of the economy where M is the set of distributions over the state of the economy. Let $\mu' = \Phi(\Omega, \underline{b}', y')$ be the law of motion for the distribution, and assume productivity and the borrowing limit follow a Markov process. It is important to note that even though there is an exogenously imposed borrowing limit $\underline{b}$, debt will be individually priced as in Chatterjee et al. [2007], and many workers will have 'effective borrowing limits' where the bond price reaches zero well before $\underline{b}$.

Let $M(u, v)$ denote the matching function, and define the labor market tightness to be the ratio of vacancies to unemployment. Since there is directed search, there will be a separate labor market tightness for each submarket. In each submarket, there is a job finding rate for households, $p(\cdot)$, that is a function of the labor market tightness $\theta_t(h, k; \Omega)$, such that $p(\theta_t(h, k; \Omega)) = \frac{M(u_t(h,k;\Omega), v_t(h,k;\Omega))}{u_t(h,k;\Omega)}$. On the other side of the market, the hiring rate for firms $p_f(\cdot)$ is also a function of the labor market tightness and is given by $p_f(\theta_t(h, k; \Omega)) = \frac{M(u_t(h,k;\Omega), v_t(h,k;\Omega))}{v_t(h,k;\Omega)}$. When households enter the labor market, they choose which capital intensity (k) submarket to search in.[16] Once matched with a firm, a worker produces $f(y, h, k) : \mathcal{Y} \times \mathcal{H} \times \mathcal{K} \to \mathbb{R}_+$ and keeps a share α of this production.[17]

[16] This is the only dimension along which households optimize since their own human capital h and age t are predetermined states.

[17] This a similar assumption to Kaplan and Menzio [2013], and is only made for tractability purposes. Directed search models with commitment to one submarket, including Shi [2001], find that firms optimally post unique wages that are monotone in workers' types, but other models in which firms do not commit to any given submarket, such as Shimer [2001], find non-monotone wages in workers' types within any given job, in some cases. Empirically, wage profiles are concave in education and decreasing for higher levels of education. We can allow for this by introducing flexible functional forms for production.

At the beginning of every period, households with debt positions $b < 0$ make a default decision. In the present formulation, the default punishment is similar to Ch. 7 bankruptcy in the United States. A household in bankruptcy has a value function scripted by B and cannot save or borrow. With probability λ, a previously bankrupt agent regains access to asset markets. If a household is in good standing (i.e. they have regained access to asset markets), its value function is scripted with a G, and the household can freely save and borrow.

The problem of an unemployed household in good standing is given below. To suppress an additional state variable, we allow unemployment benefits $z(k)$ to be a function of the worker's prior wage, but only through its dependence on k.[18]

$$U_t^G(b,h,k;\Omega) = \max_{b' \geq \underline{b}} u(c,1) + \beta \mathbb{E}\Big[\max_{\tilde{k}} p(\theta_{t+1}(h',\tilde{k};\Omega'))W_{t+1}(b',h',\tilde{k};\Omega') \\ + \big(1 - p(\theta_{t+1}(h',\tilde{k};\Omega'))\big)U_{t+1}(b',h',k;\Omega')\Big], \quad t \leq T$$

$$U_{T+1}^G(b,h,k;\Omega) = 0$$

Such that

$$c + q_{U,t}(b',h,k;\Omega)b' \leq z(k) + b$$

We assume that human capital abides by the following law of motion (note that the process is indexed by employment status U):

$$h' = H(h,U)$$

And the shock processes and aggregate law of motion are taken as given:

$$y' \sim F(y' \mid y), \quad \underline{b}' \sim F(\underline{b}' \mid \underline{b}), \quad \mu' = \Phi(\Omega, y', \underline{b}'), \quad \Omega' = (y', \underline{b}', \mu') \tag{4}$$

For households who default, they are excluded from both saving and borrowing. There is an

[18] Shocks to k during unemployment could proxy expiration of unemployment benefits.

exogenous probability λ that they regain access to asset markets:

$$U_t^B(b,h,k;\Omega) = u(c,1)+\lambda\beta\mathbb{E}\Big[\max_{\tilde{k}} p(\theta_{t+1}(h',\tilde{k};\Omega'))W_{t+1}(0,h',\tilde{k};\Omega') + \big(1-p(\theta_{t+1}(h',\tilde{k};\Omega'))\big)U_{t+1}(0,h',k;\Omega')\Big] + (1-\lambda)\beta\mathbb{E}\Big[\max_{\tilde{k}} p(\theta_{t+1}(h',\tilde{k};\Omega'))W_{t+1}^B(0,h',\tilde{k};\Omega') + \big(1-p(\theta_{t+1}(h',\tilde{k};\Omega'))\big)U_{t+1}^B(0,h',k;\Omega')\Big], \quad t \leq T$$

$$U_{T+1}^B(b,h,k;\Omega) = 0$$

Such that

$$c \leq z(k)$$

and the law of motion for human capital and aggregates are taken as given. For households in good standing, at the start of every period, they must make a default decision:

$$U_t(b,h,k;\Omega) = \max\Big\{U_t^G(b,h,k;\Omega), U_t^B(b,h,k;\Omega) - \chi\Big\}$$

Let $D_{U,t}(b,h,k;\Omega)$ denote the unemployed household's default decision. Due to the finite life cycle, a utility penalty of default, χ, is necessary to support credit in equilibrium.

A similar problem holds for the employed. The value functions are denoted with a W for employed households, and at the end of every period, employed households face layoff risk δ. If they are laid off, since the period we will ultimately use is 1 quarter, we must allow the workers to search immediately for a new job.[19] We relegate the employed value functions to Online Appendix D.

5.1 Lenders

There is a continuum of potential lenders who are risk neutral and can obtain funds, without constraint, at the risk free rate r_f. Lenders may lend to households or firms. Recall $e \in \{W,U\}$ denotes employment status. The price of debt for households must therefore satisfy

[19] This allows the model to match labor flows in the data.

the inequality below:

$$q_{e,t}(b', h, k; \Omega) \leq \frac{\mathbb{E}\Big[1 - D_{e',t}(b', h', k'; \Omega')\Big]}{1 + r_f} \tag{5}$$

Under free entry, the price of debt must yield exactly the risk free rate, r_f, and this equation holds with equality.

The price of debt for firms follows a similar form. For the sake of brevity, and the necessity for additional notation, this bond price will be shown below in the firm section. Since lenders earn zero profit for each contract in equilibrium, lenders are indifferent between lending to a firm or a household.

5.2 Firms

There is a continuum of risk neutral entrepreneurs that operate constant returns to scale production functions. The entrepreneurs invest in capital $k \in \mathcal{K} \subset \mathbb{R}_+$ and post vacancies to attract workers in the frictional labor market. We assume capital is denominated in units of the final consumption good.

The entrepreneur, when attempting to create a firm, is subject to a financing constraint. When a firm is not yet operational, the firm does not have access to perfect capital markets. The firm must borrow the money, $b_f < 0$ to finance the initial capital investment. We assume the firm is not subject to the aggregate debt limit.[20] If the firm fails to find an employee, the firm defaults and the capital is lost.[21]

When deciding whether or not to post a vacancy, the firm solves the following problem. It chooses capital $k \in \mathcal{K}$ and what types of workers, indexed by human capital and age $(h, t) \in \mathcal{H} \times \mathbb{N}_T$, to hire. In the event that the worker is hired, the firm has access to perfect capital markets and repays b_f immediately. In the event that no worker can be found, the firm defaults. Let $J_t(h, k; \Omega)$ be the profit stream of a firm that has k units of physical capital and is matched with an age t worker with human capital h. Let $q_{f,t}(b, k, h; \Omega)$ denote the

[20] This is because we want to isolate the impact of household credit limits on the macroeconomy. In future work, we are exploring the role of firm constraints on sorting.

[21] We are envisioning specific assets with low liquidation value, however, in Online Appendix J.4 we allow for an explicit partial liquidation by the lender (capital is denoted in units of the final consumption good, and so this amounts to capital reversibility).

bond price faced by the firm. Then the problem a firm solves when attempting to recruit a worker is given below (recall b is negative if borrowing),

$$\kappa \leq \max_{k,h,t} p_f(\theta_t(h,k;\Omega))[J_t(h,k;\Omega) + b_f] + (1 - p_f(\theta_t(h,k;\Omega))) \cdot 0 \tag{6}$$

such that

$$-k \geq q_{f,t}(b_f, k, h; \Omega) b_f \tag{7}$$

With free entry in the lending market, the price of debt must be given by (note that k is implicitly related to b_f in the equation above),

$$q_{f,t}(b_f, k, h; \Omega) = \frac{p_f(\theta_t(h,k;\Omega))}{1 + r_f} \tag{8}$$

Using the fact that Equation (6) holds with equality under free entry and that Equation (7) must also hold with equality, the market tightness in each submarket which is entered with positive probability is given by,

$$\theta_t(h,k;\Omega) = p_f^{-1}\left(\frac{\kappa + (1+r_f)k}{J_t(h,k;\Omega)}\right) \tag{9}$$

For tractability, we assume that workers and firms split output according to a constant piece-rate α. We assume the firm keeps a share $1-\alpha$ of its production, and workers receive the remaining share α of production. Of that remaining output, firms must then pay a fixed cost f_c.[22] The value function for the firm is given by,

$$J_t(h,k;\Omega) = (1-\alpha)f(y,h,k) - f_c + \beta\mathbb{E}\big[(1-\delta)J_{t+1}(h',k;\Omega')\big], \qquad \forall t \leq T$$

$$J_{T+1}(h,k;\Omega) = 0$$

There are three stark assumptions implicit in this value function, (i) zero liquidation value of capital, (ii) static capital, and (iii) no on-the-job search. In Online Appendix J we allow capital to have a nonzero liquidation value and we allow firms to dynamically invest in capital. We do not explicitly model on-the-job search due to tractability (it would require

[22] The representative entrepreneur will make exactly zero profits across plants and over time, even if some firms are temporarily making negative profits. When calibrating the model this fixed cost will serve to generate a small surplus for firms, and help the model match quantitative features of the data.

firms knowing workers' asset policy functions, see Herkenhoff [2013] for a model with one sided heterogeneity, credit, and OJS), but by allowing firms to invest in capital, we mitigate workers' incentives to switch jobs. In fact, with frictionless capital adjustment, firms set capital to the surplus maximizing value and workers have no incentive to leave the firm.

5.3 Equilibrium: Definition, Existence and Uniqueness

Let $\mathbf{x}$ summarize the state vector of a household. An equilibrium in this economy is a set of household policy functions for saving and borrowing $(\{b'_{e,t}(\mathbf{x})\}_{t=1}^{T})$, bankruptcy $(\{D_{e,t}(\mathbf{x})\}_{t=1}^{T})$, and a capital search choice $\{k_t(\mathbf{x})\}_{t=1}^{T}$, a debt price $(\{q_{e,t}(\mathbf{x})\}_{t=1}^{T})$ for both the employed $(e = W)$ and unemployed $(e = U)$, a debt price for firms $(\{q_{f,t}(\mathbf{x})\}_{t=1}^{T})$, a market tightness function $\theta_t(h, k; \Omega)$, processes for aggregate shocks $(y, \underline{b})$, and an aggregate law of motion $\Phi(\Omega, y', \underline{b}')$ such that

i. Given the law of motion for aggregates, the bond price, and market tightness function, households' decision rules are optimal.

ii. Given the law of motion for aggregates and the bond price, the free entry condition in the labor market (9) holds.

iii. Given household policy functions, the labor market tightness function, and the law of motion for aggregates, the free entry conditions for lenders making loans to households (5) and firms (8) both hold.

iv. The aggregate law of motion is consistent with household policy functions.

We use the same tools as Shi [2009] and Menzio and Shi [2011] to solve for a Block Recursive Equilibrium in which policy functions and prices do not depend on the aggregate distribution μ (even though it fluctuates over time and can be recovered by simulation). However, policy functions still depend on aggregate productivity, y, and the borrowing limit, $\underline{b}$.

In Online Appendix E, we prove two propositions which we will use in the numeric portion of the paper. First, a Block Recursive Equilibrium exists in this economy, and thus to solve the model economy, we only need to solve the first 'block' of the equilibrium i.-iii. ignoring iv., and then we can simulate to recover the dynamics of μ. Furthermore, we establish that certain classes of utility, matching, and production functions yield uniqueness.

6 Calibration

The parameters are calibrated so that the model's stochastic steady state is consistent with 1970-2007 averages. Stochastic steady state means that aggregate total factor productivity (y) still fluctuates but that the borrowing limit ($\underline{b}$) is constant forever.[23] The period is set to one quarter. We calibrate the productivity process to match the Fernald et al. [2012], non-utilization adjusted total factor productivity series. The series is logged and band pass filtered to obtain deviations from trend with periods between 6 and 32 quarters. Aggregate productivity deviations are assumed to fluctuate over time according to an AR(1) process:

$$\ln(y') = \rho \ln(y) + \epsilon_1 \quad \text{s.t.} \quad \epsilon_1 \sim N(0, \sigma_e^2)$$

Estimation yields $\rho = 0.894$ and $\sigma_e = 0.00543$, and the process is discretized using Rouwenhurst's method.

We set the annualized risk free rate to 4%. In stochastic steady state, we set $\underline{b} = -.5$, which is non-binding for all agents in our simulations. We set the job destruction rate to a constant 10% per quarter, $\delta = .1$ (Shimer [2005]). For the labor market matching function, we use a constant returns to scale matching function that yields well-defined job finding probabilities:

$$M(u, v) = \frac{u \cdot v}{(u^\zeta + v^\zeta)^{1/\zeta}} \in [0, 1)$$

The matching elasticity parameter is chosen to be $\zeta = 1.6$ as in Schaal [2012].

Preferences are given below, where η is the flow from leisure, and L=1 for employed persons and L=0 otherwise:

$$u(c, 1 - L) = \frac{c^{1-\sigma} - 1}{1 - \sigma} + \eta(1 - L)$$

We set the risk aversion parameter to a standard value, $\sigma = 2$. The life span is set to $T = 80$ quarters (20 years), and newly born agents are born unemployed, with zero assets, in good credit standing, and with a uniform draw over the grid of human capital. The household share of income, α, is set to $\frac{2}{3}$, and the production function is Cobb-Douglas,

[23] A long sequence of productivity shocks is drawn according to the AR(1) process for y and held fixed. A large number of agents (N=30,000) is then simulated for a large number of periods (T=270 quarters, burning the first 100 quarters). Averages are reported over the remaining 170 quarters across $R = 10$ repetitions. Online Appendix K describes the solution algorithm in detail.

$f(y,h,k) = yh^a k^{(1-a)}$ with parameter $a = \frac{2}{3}$. The bankruptcy re-access parameter $\lambda = .036$ generates the statutory 7 year exclusion period.

The remaining 8 parameters including the discount factor β, the unemployment benefit z, the utility penalty of bankruptcy χ, the entry cost of firms κ, the fixed cost of operations f_c, the flow from leisure η, the human capital appreciation $p_{+\Delta}$ rate, and the human capital depreciation $p_{-\Delta}$ rate are calibrated jointly to match 8 moments: the fraction of households with liquid asset to income ratios less than 1%, the immediate consumption loss from unemployment, the bankruptcy rate, the unemployment rate, the relative volatility of unemployment to productivity, the autocorrelation of unemployment, the wage growth of 25 year olds, and the long term consumption losses from layoff. We do not directly target the duration elasticity or earnings replacement rate elasticity.

The household discount factor $\beta = .988$, which implies a discount rate of about 5% per annum, is calibrated to match the fact that 25.4% of households have a ratio of liquid assets to annual gross income less than one percent.[24] We argue that for high frequency search models (in which unemployment durations are very short), that liquid wealth is the relevant calibration target since it isolates the portion of wealth that can be drawn down in response to a temporary income shock (such as job loss). As Kaplan and Violante [2014] show, it is precisely access to liquid assets that determines the response of households to income shocks.

The unemployment benefit is set to a constant, $z(k) = .101 \ \forall k$, in order to match the observed consumption losses following job loss.[25] This value of z yields an average UI replacement rate of approximately 40% for the lowest human capital workers (Shimer [2005]), but implies significantly lower UI replacement rates of 10% for higher human capital workers, in line with Chodorow-Reich and Karabarbounis [2013].

The labor vacancy posting cost $\kappa = .034$ is chosen to target a mean U6 unemployment rate of 8.9% which is the 1994-2007 average,[26] and we set the bankruptcy utility penalty

[24] See Kaplan and Violante [2014] and Herkenhoff [2013]. The data is from the SCF (and it predecessor survey the Survey of Consumer Credit). For each household, we sum cash, checking, money market funds, CDS, corporate bonds, government saving bonds, stocks, and mutual funds less credit card debt over annual gross income. We take the mean of this liquid asset to income ratio across households in each survey year, and then we average over 1970 to 2007 to arrive at the moment.

[25] Browning and Crossley [2001] find 16% consumption losses after 6 months of unemployment for Canadians, and as they explain, scaling food consumption losses in Gruber [1994] results in 15% consumption losses in the year of layoff for US households in the PSID. We therefore target a 15% consumption loss from the quarter prior to initial displacement until the end of the 1st year of layoff, 4 quarters after initial displacement.

[26] Since there is no concept of "marginally-attached" workers or part-time employment in the model, U6

$\chi = .077$ to generate the average bankruptcy rate in the US from 1970-2007 of approximately .1% per quarter.[27]

The processes for human capital are calibrated to generate 1.05% wage growth per quarter while employed, as well as the long term consumption losses of displaced households.[28] These processes are governed by two parameters $p_{-\Delta}$ and $p_{+\Delta}$.

$$H(h, U) = h' = \begin{cases} h - \Delta & \text{w/ pr. } p_{-\Delta} \text{ if unemployed} \\ h & \text{w/ pr. } 1 - p_{-\Delta} \text{ if unemployed} \end{cases}$$

$$H(h, W) = h' = \begin{cases} h + \Delta & \text{w/ pr. } p_{+\Delta} \text{ if employed} \\ h & \text{w/ pr. } 1 - p_{+\Delta} \text{ if employed} \end{cases}$$

In the calibration below, the grid for human capital, $h \in [.5, .6, .7, .8, .9, 1]$, as well as the step size, $\Delta = .1$, between grid points are taken as given. Our estimates are $p_{-\Delta} = .143$ and $p_{+\Delta} = .077$, which imply that once every year-and-a-half, unemployed agents in the model expect to fall one rung on the human capital ladder. This generates between 10% to 20% earnings losses (depending on the initial human capital) per annum, which is smaller than the 30% per year Ljungqvist and Sargent [1998] target.

In terms of the flow utility of leisure, we follow most of the quantitative search and matching literature by setting η to target a labor market moment. We choose $\eta = .237$ to match the autocorrelation of unemployment since the flow utility of leisure determines unemployed households' willingness to remain out of work.

We calibrate the fixed cost of operations for firms $f_c = .100$, which determines how sensitive firms are to productivity shocks, to match the observed volatility of unemployment to productivity.

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is a better measure of unemployment for the model. The data is available from 1994:Q1 to present.

[27] The bankruptcy rate is .41% per annum from 1970-2007 according to the American Bankruptcy Institute (accessed via the Decennial Statistics).

[28] Our measure of wage growth in the data is the median 2-year real-income growth rate for households aged between 25 and 30 in the PSID between 2005 and 2007. In the data, the median growth rate among this subset of households was 8.8% (we condition on at least 1k of earnings in each year). Converting this estimate to quarters yields a quarterly income growth rate of 1.05%. Assuming agents are born at 25, our model measure of wage growth is at the midpoint of that interval, measured among 27.5 year olds in the model. Using the 2005-2011 PSID, we calculate a full consumption recovery (1% higher relative to pre-layoff consumption) 2 years after layoff for unemployed households who have zero duration spells. For distressed layoffs, Saporta-Eksten [2013]'s estimates long-run consumption losses of approximately 8% two years after initial displacement. We take the average of these two estimates and target a 3% consumption loss.

Table 8 summarizes the parameters, and Table 9 summarizes the model's fit relative to the targeted moments. The key targeted moments that control the sensitivity of agents to credit are the consumption drop upon layoff, which is most directly controlled by z (UI), and the long term consumption loss which is controlled by $p_{-\Delta}$ (the human capital depreciation rate), and the model matches those targeted moments quite well. While the model produces less volatility than the data in response to productivity shocks, as we show in the next section, the model succeeds at replicating the elasticity of replacement rates and durations with respect to credit, both of which were non-targeted moments. In other words, the responsiveness of agents to credit in our model matches the observed responsiveness in the data, independent of the model's ability to generate volatility.

The fundamental tension in the model is between generating borrowing/bankruptcies and matching the business cycle facts: intuition would suggest that lowering the discount factor would be the best way to generate borrowing/bankruptcies but doing so only exacerbates the models ability to match business cycle facts.[29] The more impatient are agents, the more they want to work immediately, regardless of productivity, which dampens business cycle dynamics. Because of the two sided heterogeneity, we cannot deploy the simple fixes in Hagedorn and Manovskii [2008]; raising the value of leisure will, at best, make only the lowest-human-capital agents indifferent between working and not, and reducing firm surplus will, at best, make only the lowest-capital firms sensitive to productivity movements. The remainder of the distribution of workers and firms will not, in general, respond to productivity shocks. We discuss this more in Online Appendix F.

6.1 Non-Targeted Moments: Model Estimates of Duration and Earnings Replacement Rate Elasticities

In this section, we use the model generated policy functions to simulate a large mass of agents and estimate the 'global' duration and replacement earnings elasticities with respect to credit access.[30] Since the model's debt pricing schedule does not have an explicit credit

[29] To then generate more borrowing, the calibration moves into regions the parameters which penalize default.

[30] We calculate the duration and replacement earnings elasticities using 30,000 agents simulated for 270 periods (burning the first 100 periods), while holding the aggregate state fixed at $y = 1$, and defining $\underline{b}_H$=-.1 and $\underline{b}_L$=-.5. Agents hold the same rational beliefs over the transition rate $P_{\underline{b}}$ between $\underline{b}_H$ and $\underline{b}_L$ as Section 7.

limit, we define the credit limit to be the minimum of the level of debt where the bond interest rate first exceeds 30% per quarter and the exogenous debt limit $\underline{b}$.[31] We isolate newly laid off agents, and then we compute each agent's optimal search decision under loose ($\underline{b} = \underline{b}_L$) and tight exogenous debt limits ($\underline{b} = \underline{b}_H > \underline{b}_L$), ceteris paribus.

This calculation is feasible as the policy function of each agent is contingent on the realization of Ω which includes the exogenous debt limit $\underline{b}$. So at each decision node, encoded in this policy function is the search decision of the agent if debt limits tighten as well as if debt limits remain slack. What makes this experimental design valid is the block recursive nature of the model; the menu of job choices faced by the household is not a function of $\underline{b}$. This allows us to determine the impact of changing debt limits, holding all else constant, including the set of jobs from which households can choose.[32] Online Appendix G provides more detail on the way we calculate the model's elasticities.

Table 10 compares the model's global elasticities to the data. The first row of Table 10 shows that if unused credit to income increases by 10% in the model, then agents take .72 (=.608*12*.1) weeks longer to find a job. This falls in the mid-range of our OLS and IV estimates. However, the elasticity calculated in the model is a 'global' elasticity and is conceptually different from the local average treatment effect identified by the IVs.

The second row of Table 10 shows that the model replacement rate elasticity (inclusive of 0s) is -.024, which is quite small and in line with the insignificant coefficients found in the data. To understand why this is the case, we decompose earnings losses in the model into two offsetting components: (i) access to additional credit depresses job finding rates which tends to lower replacement earnings, and (ii) access to additional credit increases the capital intensity of submarkets searched by agents which tends to raise replacement earnings.

To capture the first component, the third row of Table 10 shows that when debt limits expand by 10% of prior annual income, job finding rates fall by 1.1% as workers can better self-insure while searching more thoroughly for jobs. This tends to decrease the replacement earnings of agents, since unemployed workers have an earnings replacement rate of zero. To capture the second component, the fourth row of Table 10 shows that being able to replace 10% more of prior income with credit allows agents in the model to search in submarkets with

[31] Only .03% of the agents in the model will ever borrow at real quarterly rates above 30%. The results are robust to alternate definitions of this effective debt limit.

[32] The intuition is simple and is formally shown in the existence proof. $J_T(h, k; y) = f(y, h, k)$ does not depend on $\underline{b}$, and working back, neither does $J_t(h, k; y)$ for arbitrary t. Therefore, using the free entry condition, $\theta_t(h, k; y)$, which pins down the menu of operating submarkets, will not either.

2.7% greater intellectual or physical capital intensity. This tends to increase the replacement earnings of agents. The combination of the two effects, namely the negative influence of job finding rates and positive influence of capital intensity on replacement earnings, yields the near-zero replacement earnings elasticity observed in the model.

The fifth row of Table 10 is designed to remove any duration effects from the replacement earnings elasticity. This row shows that among job finders at t+1 (the year after layoff), being able to replace 10% more of prior income with credit results in a 1.8% greater earnings replacement rate, which falls toward the high end of our IV estimates.

Lastly, we can compare the impact of bankruptcy flag removal on duration and replacement rates in both the model and data (we compare this to derogatory flag removals in the data). The final two rows of Table 10 show that following flag removal, displaced workers take .066 quarters longer to find a job, whereas in the data this number is .112. Following flag removal, agents earn .7% more in the model, whereas in the data, the impact on replacement earnings is insignificant.

Overall, we believe the model's self-insurance mechanism generates replacement rate elasticities (both inclusive and exclusive of 0s) as well as duration elasticities that are in line with our IV estimates.

7 Main Quantitative Experiment

Based on the model's success at replicating key non-targeted micro moments, we now aggregate across individual agents to explore how credit access impacts the macroeconomy. In particular, we study the way changes in borrowing limits impact the path of output, labor market sorting and therefore productivity during the 2007-2009 recession. We do so by comparing aggregate outcomes across two economies, both of which have the same beliefs about debt limit transitions $P_{\underline{b}}$:

1. **Tight Debt Limit Economy:** The debt limit tightens from $\underline{b} = -.5$ (a non-binding value) to $\underline{b} = -.1$ in 2008-Q4 (the first quarter in which the aggregate consumer credit limit declined[33]), and stays there permanently. As we discuss below, the new debt limit

[33] 2014 Q1 New York Fed Consumer Credit Report, Page 7, time series entitled 'Credit Limit and Balance for Credit Cards and HE Revolving.'

generates a similar debt to income (DTI) and borrowing contraction as the data.

2. **Constant Debt Limit Economy:** The debt limit $\underline{b} = -.5$ remains constant throughout the simulation.

In the experiments below, both economies are simulated in their ergodic stochastic steady states with the non-binding debt limit, $\underline{b} = -.5$, for a large number of periods. We then feed in a realized set of shocks that replicates, as approximated on a grid, the path of the Fernald et al. [2012] productivity residuals from 1974-Q1 to 2012-Q4. The borrowing limit is held constant at $\underline{b} = -.5$ through 2008-Q4 in both economies, for simplicity. In 2008-Q4, one economy has the limit tighten to $\underline{b} = -.1$, and it remains there permanently.

We impose that both economies have the same beliefs over debt transitions. Let $p_{l,l}$ be the probability of remaining in the 'low' debt limit state, $\underline{b} = -.1$, and let $p_{h,h}$ be the probability of remaining in the 'high' debt limit state, $\underline{b} = -.5$. Then the transition matrix for the debt limit $\underline{b}$ is given by $P_{\underline{b}} = \begin{pmatrix} p_{l,l} & 1 - p_{l,l} \\ 1 - p_{h,h} & p_{h,h} \end{pmatrix}$. Agents understand that if the debt limit tightens, it is permanent, so we set $p_{l,l} = 1$. And, agents also understand that once every 34 years (from 1974 to 2008), debt limits will tighten, so we set $p_{h,h} = .9926$. On average, therefore, agents are rational.

We show in Table 11 that the model economy replicates the liquid wealth of displaced households leading up to the crisis. Furthermore, approximately 14% of agents in the model borrow, and of those that borrow, they replace 9.8% of lost income using unsecured credit which is in line with Sullivan [2008]'s findings. We then document in Table 12 that the new debt limit $\underline{b} = -.1$ delivers roughly the same magnitude decline in both borrowing and debt to income (DTI) ratios as the data.

7.1 Model Results

Figure 5 illustrates the path for the exogenous component of productivity y and the path for the borrowing limit $\underline{b}$. These are the two inputs in the experiment. Each plot contains two dashed lines that correspond to differing degrees of debt limit tightening. The dashed blue line corresponds to the economy where limits tighten to $\underline{b} = -.1$, and, for the sake of robustness, we also include the dash-dot red line which corresponds to the economy where limits tighten to $\underline{b} = -.2$ (this delivers a drop in DTIs half as severe as the $\underline{b} = -.1$ economy).

Table 12 shows that the fraction of households borrowing falls by 3.09 percentage points in the economy in which the aggregate limit tightens to $\underline{b} = -.1$ and 1.21 percentage points when the aggregate debt limits tightens to $\underline{b} = -.2$. Economy-wide debt to income ratios fall by 1.09 percentage points and .53 percentage points, respectively. In the data, the fraction of households that stopped borrowing fell by 6.77 percentage points from 2007 to 2010 (measured in the SCF) while the debt to income ratio fell by .86 percentage points from 2007 to 2010 (measured in the SCF).[34]

Figure 6 plots the percentage change in employment during the 2007-2009 recession across the economy with a tighter debt limit versus the economy with a fixed debt limit. When debt limits tighten, employment tends to increase, persistently. The mechanism is that with looser credit limits, unemployed households borrow to smooth consumption while thoroughly searching for capital-intensive jobs. If debt limits tighten, they lose their ability to self-insure, and, as a result, take low-capital-intensity jobs that are relatively quick to find. In other words, when limits tighten, low-asset job losers take relatively less productive employment opportunities. This introduces a strong tension between recovery speed and recovery health, as workers find jobs more quickly but these jobs are of lower quality.

As Figure 9 shows, the aggregate capital stock held by entrepreneurs drops severely relative to the economy in which debt limits are held constant. This is entirely driven by new entrepreneurial entrants posting more vacancies in submarkets with less capital, and constrained households searching for jobs in those submarkets. The time it takes for the aggregate capital stock to recover to its pre-recession levels is as much as 6 quarters longer in the economy in which debt limits tighten.

Because households become more constrained and take jobs in which there is less capital per unit of labor, Figure 7 shows that measured labor productivity, defined as output over employment, declines when debt limits are tightened. The economy in which debt limits tighten the most has a .28 percentage point lower labor productivity as compared to the economy with constant debt limits, and this productivity gap persists throughout the recovery. In Online Appendix J, when we allow for capital investment to change over the course of a match, the labor productivity decline is slightly less pronounced because firms have the ability to invest more in existing matches during the recovery.

In terms of production, the impact of tighter debt limits on aggregate output is theoret-

[34]While not reported here for the sake of space, the bankruptcy rate reaches .97% in the model in the quarter in which limits are tightened, which is in line with ABI bankruptcies per capita.

ically ambiguous: households find jobs faster, but the jobs workers find are less productive. However, Figure 8 shows that quantitatively the reduction in capital per worker is so severe that output falls by .11 percentage points.

The mechanism at the heart of the output decline involves a reallocation of workers from high capital firms to low capital firms. To understand this reallocation in greater detail, we now turn to standard measures of sorting. Figure 10 plots the percentage change in the correlation between human capital, h, and firm capital, k, during the 2007-2009 recession. Figure 11 plots the corresponding percentage change in the Spearman rank correlation coefficient between human capital, h, and firm capital, k, as well (workers are ranked by h, and firms are ranked by k, and the Spearman Rank correlation coefficient is the resulting correlation between the numeric ranks of workers and firms). The raw correlation coefficient between worker human capital and firm physical capital is approximately +.33.

Figures 10 and 11 show that in the economy in which debt limits are tighter, these standard measures of sorting *improve*. The mechanism behind this sorting improvement is that in the economy in which debt limits tighten, unemployed agents with low-human-capital cannot borrow to smooth consumption while thoroughly searching for jobs. Therefore, they take jobs that are less-capital-intensive, but more abundant. On average, since low human capital workers are less productive (recall the assumption of supermodularity), tighter debt limits force these 'low quality' workers to take 'low quality' jobs. As such, standard measures of sorting improve, even as output falls, since they do not take into account the investment decisions of firms. In this economy, these standard measures of sorting are not good proxies for either productivity or output, even with a supermodular production function.[35] In Online Appendix H, we show that households would be willing to give up .09% of lifetime consumption to live in a world in which borrowing limits do not tighten in 2007, even though standard measures of sorting would be lower in that world.

Allowing firms to invest and workers to save are standard assumptions in most neoclassical business cycle models but are often difficult to incorporate in search theoretic models. The fact that standard measures of sorting move in the opposite direction of output and productivity under these mild assumptions raises important questions about the welfare implications of sorting patterns derived from search theoretic models with linear utility or

[35] In Online Appendix I we explore alternate measures of sorting in more detail, including other measures of mismatch proposed in the literature. We show that alternate measures of mismatch, such as distance from surplus maximizing match (Lise and Robin [2013]), are countercyclical in the model. This general pattern of countercyclical mismatch is in line with the data, e.g. Moscarini and Vella [2008] and Şahin et al. [2012].

under the assumption of fixed firm types.

7.2 Robustness: Capital Investment and Liquidation Value

We conduct two robustness exercises in Online Appendix J. First, we allow for the entrepreneurs to invest in capital over time, mitigating concerns about both quits and on-the-job-search. With costless adjustments to entrepreneur capital, there would never be a reason to quit or change jobs. We find that our main results are largely unchanged, but the ability to invest in capital during recoveries marginally dampens the response of capital, productivity, output, and sorting to business cycle shocks. Second, we allow for a liquidation value of firm capital, and again, the main predictions of the model still hold.

8 Conclusions

Our paper provides the first estimates of the impact of credit constraints on job finding rates and subsequent replacement wages of displaced workers. Using new administrative data, we find that medium-tenure displaced mortgagors, in response to being able to replace 10% of their annual income with revolving credit, take .33 to 2.67 weeks longer to find a job but obtain earnings replacement rates that are .6 to 2.3% greater. Furthermore, displaced individuals with greater credit access tend to find jobs at more productive firms, proxied by wages-paid-per-worker.

We develop a labor sorting model with credit to provide structural estimates of the impact of credit on the duration of unemployment and how much income displaced individuals replace in their new job. The model yields estimates of approximately .7 and 1.8%, respectively for the duration of unemployment and the income replacement rate, which are similar to our empirical estimates.

We then use the model to understand the impact of consumer credit on productivity, output, and employment. The model shows that tighter debt limits during recessions may increase employment during the recovery, but depress both productivity and output. This tension between the speed of recovery and health of recovery is at the heart of the mechanism: tighter debt limits force constrained households to cut their job search shorter, taking relatively unproductive jobs that are more abundant.

Our empirical and quantitative findings have implications for the way both policy-makers and economists think about the optimal provision of unemployment insurance (Marimon and Zilibotti [1999], Acemoglu and Shimer [1998], Shimer and Werning [2005]) and the response of labor markets to monetary policy (Gornemann et al. [2012], Auclert [2014]). The fact that increases in credit access can *reduce* job finding rates by easing household credit constraints brings into question the ability of the Federal Reserve Bank to effectively meet the mandate of "maximum employment, stable prices and moderate long-term interest rates."

We view this paper as the beginning of a research agenda that uses new micro data and theory to understand how consumer credit impacts the allocation of households to firms. The next step in our research agenda is to measure the impact of consumer credit constraints on entrepreneurs and small firm employers (Herkenhoff et al. [2016]).

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Table 1: Summary Statistics for Main Samples

	Displaced w/ Flag		Displaced Mortgagor		All Displacements	
	(1) Treatment (Flag Removed)	**(2)** Control (Flag Persists)	**(3)** Entire Sample	**(4)** Employed, t+1	**(5)** Entire Sample	**(6)** Employed, t+1
Age	43.2	42.1	42.6	41.9	38.8	38.5
Tenure	3.9	3.9	4.4	4.3	3.6	3.7
Imputed Education	13.6	13.4	13.7	13.8	13.2	13.3
Lagged Annual Earnings	$ 46,359	$ 42,065	$ 54,782	$ 57,084	$ 44,317	$ 46,772
Lagged Unused Revolving Credit to Income	0.17	0.10	0.45	0.40	0.29	0.27
Lagged Unused Total Credit to Income	0.37	0.25	0.79	0.70	0.51	0.47
Duration of Non-Employment (In Quarters)	2.58	2.39	2.53	0.77	2.43	0.78
Replacement Rate (Annual Earnings Year t+1/Annual Earnings Year t-1)	0.76	0.79	0.75	1.06	0.81	1.14
Lagged Months Since Oldest Account Opened	156	159	175	173	139	140
Observations (Rounded to 000s)	4000	13000	30000	14000	81000	36000

Table 2: Dependent Variable is Duration. Columns: (1) OLS with Derogatory Flag Drop, (2) OLS with Displaced Mortgagors, (3) IV using Gross and Souleles Instrument, (4) IV using Saiz instrument.

	(1)	**(2)**	**(3)**	**(4)**
	Dependent Variable is Duration			
	OLS-Flag Drop	OLS	IV-GS	IV-Saiz
Unused Revolving Credit to Income Ratio		0.279***	0.442***	2.226***
		(0.0207)	(0.0929)	(0.437)
Flag Drop (d)	0.112*			
	(0.0581)			
Demographic, Industry, MSA, & Lagged Earnings Controls	Y	Y	Y	Y
HELOC Limits and Equity Proxy	Y	Y	Y	Y
R2 (1st Stage for IVs)	0.060	0.052	0.0433	0.00799
Angirst Pischke FStat Pval	-	-	0	1.93e-06
Round N	17000	81000	81000	30000

*Notes. Std. errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1. Cols. (1) to (3) use robust std. errors, and Col. (4) uses MSA clustered std. errors. Col. (1) uses the flag drop sample, Cols. (2) and (3) use all displacements. Col. (4) uses the mortgagor sample. Unused Revolving Credit to Income measured 1 year prior to layoff. Demographic controls include quadratic in age & tenure, race, sex and education dummies as well as year & auto loan dummies. Industry controls include 1-digit SIC dummies and size, age, and wage per worker of prior firm. MSA controls include real per capita GDP and the MSA unemployment rate. Lagged earnings controls include prior real annual earnings and cumulative real annual earnings to proxy for assets. Equity proxy is highest observed mortgage balance less current mortgage balance. HELOC limits include combined home equity limits.*

Table 3: Impact of Derogatory Flag Removal on Credit Access, OLS. Column (1) Dependent Variable is Revolving Credit Limit to Income, and Column (2) Dependent Variable is Credit Score.

	(1) Revolving Credit Limit To Income	**(2)** Credit Score
Flag Drop (d)	0.108***	123.1***
	(0.0117)	(4.003)
Demographic, Industry, MSA, & Lagged Earnings Controls	Y	Y
HELOC Limits and Equity Proxy	Y	Y
R2	0.066	0.180
Round N	17000	17000

*Notes. Robust Std. errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1. Same control definitions as Table 2.*

Table 4: Dependent Variable is Replacement Rate, Measured 1 Year After Layoff Relative to 1 Year Before Layoff. Sample Includes Those With Jobs 1 Year After Layoff.

	(1)	**(2)**	**(3)**	**(4)**
	DV is Replacement Rate, Among Employed at t+1			
	Flag Drop	OLS	IV-GS	IV-Saiz
Unused Revolving Credit to Income Ratio		0.0605***	0.134***	0.233**
		(0.00477)	(0.0183)	(0.101)
Flag Drop (d)	0.0232**			
	(0.0111)			
Demographic, Industry, MSA, & Lagged Earnings Controls	Y	Y	Y	Y
HELOC Limits and Equity Proxy	Y	Y	Y	Y
R2 (1st Stage for IVs)	0.191	0.229	0.0396	0.00676
Angirst Pischke FStat Pval	-	-	0	1.17e-05
Round N	7000	36000	36000	14000

Notes. Same as Table 2.

Table 5: Dependent Variable is Replacement Rate, Measured 1 Year After Layoff Relative to 1 Year Before Layoff. Sample includes everyone, even those with replacement rates of 0.

	(1)	**(2)**	**(3)**	**(4)**
	Dependent Variable is Replacement Rate			
	Flag Drop	OLS	IV-GS	IV-Saiz
Unused Revolving Credit to Income Ratio		0.00286	0.0592***	-0.168
		(0.00418)	(0.0159)	(0.116)
Flag Drop (d)	-0.00257			
	(0.0102)			
Demographic, Industry, MSA, & Lagged Earnings Controls	Y	Y	Y	Y
HELOC Limits and Equity Proxy	Y	Y	Y	Y
R2 (1st Stage for IVs)	0.083	0.086	0.0433	0.00799
Angirst Pischke FStat Pval	-	-	0	1.93e-06
Round N	17000	81000	81000	30000

Notes. Same as Table 2.

Table 6: Dependent Variable is Dummy if Firm is in 50th/75th/90th Decile of Wage Per Worker Distribution or Greater ('Productive Firm Dummy'). Sample Includes Those With Jobs 1 Year After Layoff.

(A)	**OLS-Flag Drop**		
	Dep. Var. is Percentile of Firm Wage per Worker Distribution		
	p50	p75	p90
Flag Drop (d)	0.000117	0.0222*	0.000662
	(0.00988)	(0.0130)	(0.00950)
R2 (1st Stage for IVs)	0.154	0.190	0.125
N	7000	7000	7000

(B)	**IV-GS**		
	Dep. Var. is Percentile of Firm Wage per Worker Distribution		
	p50	p75	p90
Unused Revolving Credit to Income Ratio	0.121***	0.159***	0.0806***
	(0.0177)	(0.0230)	(0.0169)
R2 (1st Stage for IVs)	0.0396	0.0396	0.0396
Angirst Pischke FStat Pval	0	0	0
N	36000	36000	36000

(C)	**IV-Saiz**		
	Dep. Var. is Percentile of Firm Wage per Worker Distribution		
	p50	p75	p90
Unused Revolving Credit to Income Ratio	0.280**	0.115	-0.0374
	(0.121)	(0.230)	(0.251)
R2 (1st Stage for IVs)	0.00676	0.00676	0.00676
Angirst Pischke FStat Pval	1.17e-05	1.17e-05	1.17e-05
N	14000	14000	14000

Notes. Same as Table 2.

Table 7: Dependent Variable is Change in Real Revolving Debt 1 Year After Layoff Minus 1 Year Before Layoff.

	(1) Change in Revolving Debt: Year of Layoff minus 1 Year Before	**(2)** Odds of Entering Collections at t+1
Duration of Unemployment	84.83***	0.00149***
	(23.37)	(0.000507)
Demographic, Industry, MSA, & Lagged Earnings Controls	N	Y
HELOC Limits and Equity Proxy	N	Y
R-squared	0.030	0.083
Round N	81000	81000

*Notes. Robust Std. errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1. Same control definitions as Table 2.*

Table 8: Summary of Model Parameters.

Pre-Calibrated		
Variable	Value	Description
ρ	0.894	Autocorrelation of Productivity Process
σ_e	0.00543	Std. Dev. Of Productivity Process
r_f	4%	Annualize Risk Free Rate
δ	10%	Quarterly Layoff Rate
ζ	1.6	Matching Function Elasticity
σ	2	Risk Aversion
α	0.66	Household share of income
a	0.66	Cobb-Douglas Labor Share
λ	0.036	Bankruptcy Re-Access
$\underline{b}$	-0.5	Non-binding debt limit

Jointly-Estimated		
Variable	Value	Description
κ	0.034	Firm Entry Cost
z	0.101	UI
$p_{-\Delta}$	0.143	Depreciation Rate of Human Cap.
$p_{+\Delta}$	0.077	Appreciation Rate of Human Cap.
β	0.988	Discount Factor
f_c	0.100	Fixed Cost
η	0.237	Flow Utility of Leisure
χ	0.077	Bankruptcy Utility Penalty

Table 9: Model Calibration

	Model	**Target**	**Variable**	**Value**	**Source**
Unemployment Rate	8.93%	8.90%	κ	0.034	BLS, U6 1994-2007
Consumption Drop 1 Yr After Layoff	0.846	0.84	z	0.101	Browning & Crossley (2001)
Consumption Drop 2 Yrs After Layoff	0.971	.97	$p_{-\Delta}$	0.143	Saporta-Eksten (2013) / PSID 2005-2011
Quarterly Income Growth Rate 25yo	1.078%	1.05%	$p_{+\Delta}$	0.077	PSID, 2005-2007
Fraction of Households with Liquid assets to Income Ratio<1%	0.093	0.254	β	0.988	SCF, 1974-2007
Vol U/ Vol y	2.686	9.5	f_c	0.100	Shimer (2005)
Autocorr Unempl	0.730	0.94	η	0.237	Shimer (2005)
Bankruptcy rate	0.01%	0.10%	χ	0.077	ABI, 1970-2007

Table 10: Non-Targeted Moments: Model Elasticities vs. Data Elasticities

	Model	**Data: OLS**	**Data: IV-GS**	**Data: IV-Saiz**	**Data: Flag Removal**
Duration Elasticity	0.608	.279***	.442***	2.226***	-
Replacement Elasticity (including 0s)	-0.024	.00286	0.0592***	-0.168	-
Job Finding Elasticity	-0.11	-	-	-	-
Capital Elasticity	0.27	-	-	-	-
Replacement Elasticity, Job Finders at t+1	0.18	.0605***	.134***	.233**	-
Duration after Flag Removal	0.066	-	-	-	.112*
Replacement rate after Flag Removal	0.007	-	-	-	-0.00257

Notes. Model estimates derived from simulating 30,000 agents in steady state with productivity held at $y = 1$ for T=270 (discarding the first 100 periods) and computing job finding behavior under counterfactually looser limits. See Section 6.1 and Online Appendix G for more details. Data estimates from Tables 2 through 4.

Table 11: Liquid Wealth Distribution: Model vs. Data, 2007

	Model Job Losers, 2007	Data Job Losers, 2007	Model All HHs, 2007	Data All HHs, 2007
p10	-0.04	-0.02	0.03	-0.07
p25	0.05	0.00	0.17	0.00
p50	0.18	0.01	0.28	0.04
p75	0.36	0.08	0.42	0.30
p90	0.51	0.21	0.54	1.60
Mean	0.21	0.22	0.29	0.59
N	120,000	57	120,000	4385

Notes. 2007 SCF liquid wealth calculated as the sum of savings, checking, money market, mutual funds, CDs, bonds, and stocks less credit card debt, taken as ratio to gross family income. Job losers defined as those who made the transition from employed to unemployed in the last quarter, in both model and data.

Table 12: Reduction in Borrowing When Borrowing Limit Tightens, Model v. Data.

	Δ Fraction of HHs Borrowing	Δ DTI
Debt limit tightened from $\underline{b} = -.5$ to $\underline{b} = -.1$	-3.09%	-1.09%
Debt limit tightened from $\underline{b} = -.5$ to $\underline{b} = -.2$	-1.21%	-0.53%
Data	-6.77%	-0.86%

Notes: All Differences Computed using 2007 and 2010 SCF. DTI is Change Unsecured Revolving Consumer Credit to Annual Family Income. Fraction Borrowing Change is Difference in Fraction of Households Carry Positive Balances. Means Weighted Using Survey Weights. Model statistics calculated as difference in average of quarterly values over same corresponding years.

Figure 1: Non-Employment Duration by Unused Revolving Credit to Income Decile, *prior to layoff*



Figure 2: Replacement Earnings 1 Year After Layoff (Including 0s) by Unused Revolving Credit to Income Decile, *prior to layoff*

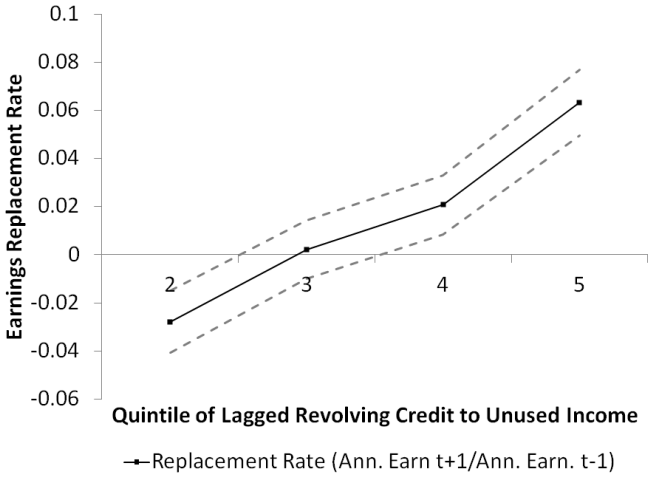


Figure 3: Change in Real Revolving Debt between Year of Layoff Minus 1 Year Before Layoff

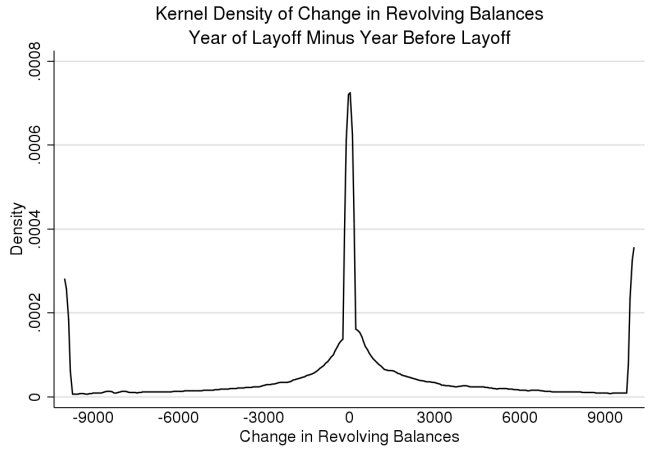


Figure 4: Replacement Rate Distribution (Annual Earnings t+1/ Annual Earnings t-1)

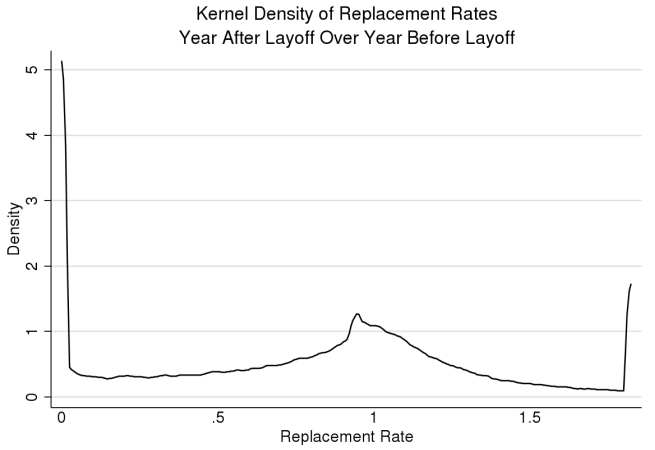


Figure 5: Experiment Input: Exogenous Aggregate Productivity (y) and Borrowing Limit $\underline{b}$, 2007-2009 Recession

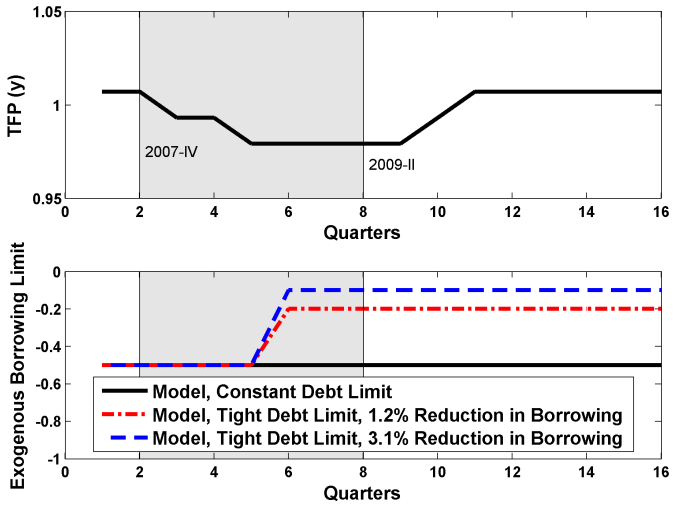


Figure 6: Percentage Change in Employment Per Capita

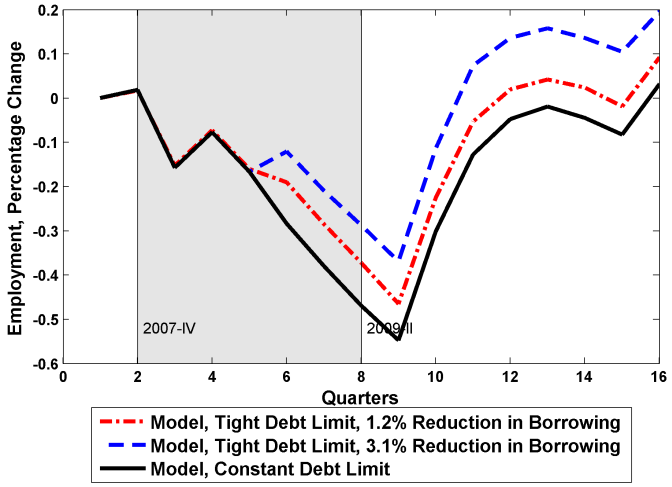


Figure 7: Labor Productivity, 2007-2009 Recession

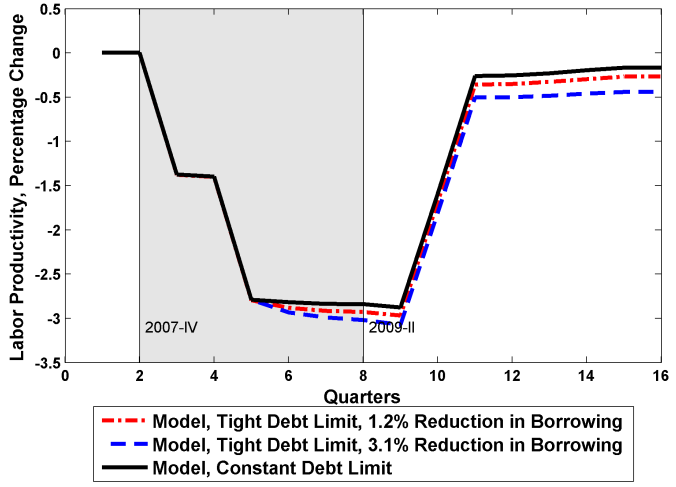


Figure 8: Aggregate Output

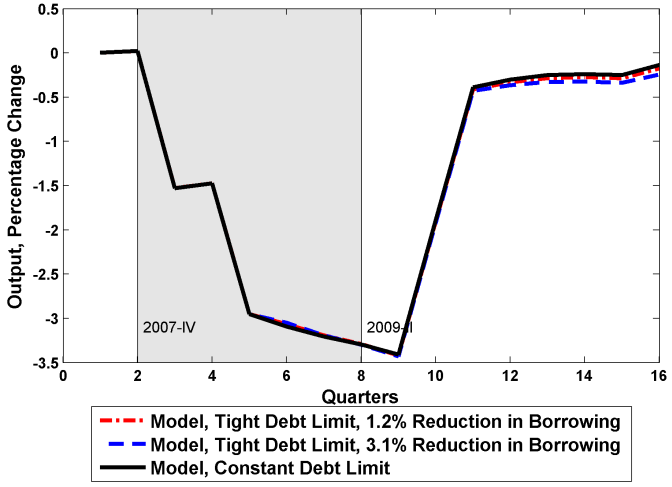


Figure 9: Aggregate Firm Capital, 2007-2009 Recession

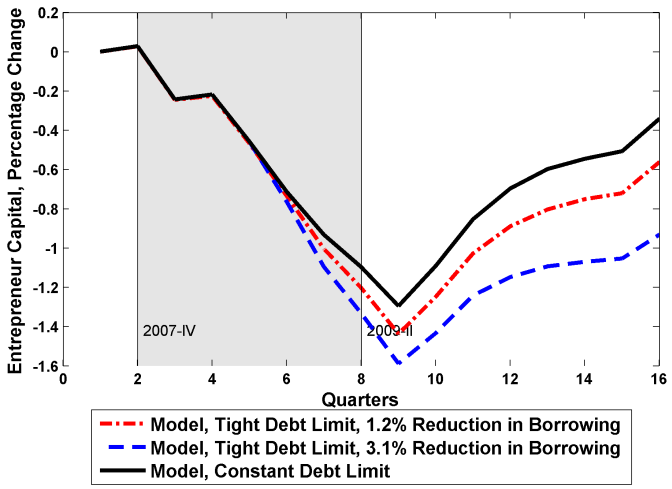


Figure 10: Correlation Between Human Capital (h) and Firm Capital (k)

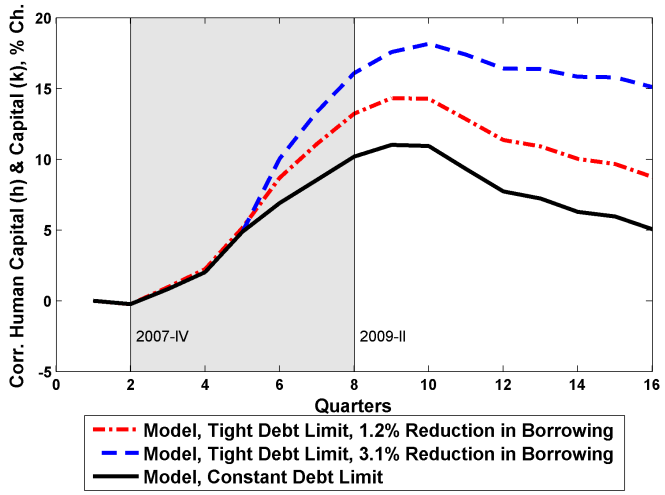


Figure 11: Spearman Rank Correlation Coefficient Between Human Capital (h) and Firm Capital (k)

