The Impact of Consumer Credit Access on Self-Employment and Entrepreneurship[*]

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Abstract

We examine how consumer credit affects entrepreneurship by linking three million earnings and pass-through tax records to credit reports. In the cross-section, we show that self-employment without employees and employer firm ownership increase monotonically with credit limits and credit scores. We then isolate individuals who have had discrete increases in credit limits after the exogenous removal of bankruptcy flags to measure the effects of personal credit on entrepreneurship. Following bankruptcy flag removal, individuals are more likely to start a new employer business and borrow extensively. Those who own businesses with employees borrow $40,000 more after bankruptcy flag removal, a 33% gain relative to the sample average.

Keywords: Entrepreneurship, Start-ups, credit access, personal bankruptcy.

JEL classification: M13, D13, G32, G33.

1.   Introduction

We examine how consumer credit affects entrepreneurship and borrowing by self-employed individuals and new firm owners. We first show several new correlations between credit limits and entrepreneurship through self-employment and firm ownership in the overall population. We then measure the effects of personal credit on entrepreneurship by examining individuals who have had discrete increases in credit limits after the exogenous removal of bankruptcy flags. We contribute to the recent literature on consumer credit and start-ups (e.g., inter ali Hurst and Pugsley, 2011; Robb and Robinson, 2014) by linking occupation outcomes (self-employment with no employees; self-employment with employees, which we call "firm ownership"; and formal work) to personal credit and credit limits.[1]

We begin our analysis by showing the positive relation between available personal credit and both self-employment flows and firm ownership for a large sample of three million prime-age individuals. Prior studies such as Hurst and Lusardi (2004) have proxied access to capital markets using wealth and shown that business ownership rates are largely unresponsive to wealth, except among the very wealthy. We complement this prior work by directly measuring credit access using credit reports and showing that self-employment increases monotonically with available personal credit. In addition, we show that firm ownership sharply increases with an owner's personal credit. Our initial findings for this broad sample of individuals are in agreement with Robb and Robinson (2014), who find that many start-ups receive debt financing through the personal balance sheets of the entrepreneur and that borrowers in states with higher personal bankruptcy exemptions, and thus implicitly less credit available, have lower ratios of debt to personal capital.

We provide several measures of population semi-elasticities of self-employment to unused revolving credit and unused total credit. We find that if an individual's unused revolving

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[1]The topic of start-ups and access to consumer credit following the housing bust has also been a large area of recent research (see Fairlie and Krashinsky, 2012; Chatterji and Seamans, 2012; Schmalz et al., 2017; Adelino et al., 2015; Jensen et al., 2014; Kerr et al., 2014) as well as Greenstone et al. (2020) for bank credit.

credit increases by 10% in the current year, their firm ownership rate increases by 0.021 percentage points in the subsequent two years. This represents a 7% increase relative to the sample average rate of firm ownership, suggesting a large elasticity of firm ownership to borrowing capacity. We also find that self-employment rates also increase by 0.66 percentage points in the year following a 10% increase in an individual's unused revolving credit. Relative to the sample average self-employment rate of 10.6%, this represents a 6% increase. These elasticities suggest a potentially large role for consumer credit as a determinant of start-ups.

However, the central issue with determining the impact of personal credit on job and start-up outcomes is that personal credit is highly correlated with an individual's earnings and job history as well as their wealth. Thus, it is hard to separate out fundamental ability and wealth effects from access to credit. Our approach to this question is to examine individuals after bankruptcy flags are removed from consumer credit reports, similar to Musto (2004). These removals occur, by law, no more than ten years after bankruptcy and give rise to large increases in credit ratings while not reflecting large changes in an individual's creditworthiness.

Our empirical approach is to compare bankrupt individuals over time, before and after flag removal. Consistent with prior studies such as Musto (2004) and Han and Li (2011), we show that access to credit increases among the subgroup of individuals who have their bankruptcy flags removed. Our analysis of bankruptcy flag removal is focused on the credit access effect: credit constraints loosen after flag removal, allowing individuals to potentially finance entry into business ownership. We also explore the impact of flag removal on employment rates.

We first study self-employment. We find that bankruptcy flag removal has a limited effect on the stock of self-employed individuals, as some individuals leave self-employment for formal employment and other individuals enter self-employment. We find that those who transition into self-employment after a bankruptcy flag removal borrow $15k more than those

who transition into self-employment prior to flag removal. This finding represents a 12.4% increase in borrowing relative to the sample average. They earn approximately $1,000 more Schedule C net income at any time horizon we observe (an increase of about 4% relative to the sample average).

We then use the new Integrated Longitudinal Business Database (ILBD) to measure transitions from self-employment to hiring paid employees in the Longitudinal Business Database (LBD). We believe focusing on this conditional sample of self-employed individuals examines people who are closer to the active margin of having demand for credit and shows how extra credit affects the tendency to start a new firm that has employees. We find that after flag removal, individuals are more likely to own a business with employees, that is, enter firm ownership. Among these firm owners, they borrow, on average, $40,000 more after flag removal, a 33% gain relative to the sample average.

We test several competing hypotheses for our results showing the importance of consumer credit to individual self-employment and entrepreneurship. The first hypothesis is that our results are driven by credit constraints, and so once the bankruptcy flag is removed, individuals enter self-employment because they can now borrow to finance their business. The second hypothesis is that rising borrowing among the self-employed simply reflects realized intertemporal consumption smoothing. The third hypothesis is that credit provides insurance against bad self-employment outcomes, and so individuals with greater unused credit buffers enter self-employment at a higher rate. Borrowing will take place ex ante even if income is high but volatile. Lastly, the observed borrowing could simply be signaling. Individuals borrow after their flag is removed to signal their type and obtain future increases in credit limits and credit scores.

We test these hypotheses by isolating the self-employed population and measuring various correlations between self-employed income and consumer credit. To test the role of credit constraints versus consumption smoothing, we measure the covariance between self-

employment income changes and borrowing changes. consumption-smoothing motives imply a negative relation, since the self-employed should borrow more when income falls to maintain consumption. On the other hand, we show in an illustrative model that credit constraints give rise to a positive relation between self-employed income and borrowing.

In both our population and bankrupt samples, we find that changes in self-employed income and borrowing are positively and significantly related, providing evidence against the consumption-smoothing channel and in support of a credit constraints channel. Second, we test whether or not credit markets are used to insure income variability by measuring the correlation between the coefficient of variation of self-employed income and credit limits. If credit is used to insure risky self-employment ventures, we would expect that those with the greatest income variability would have the largest buffers of credit (higher limits), similar to models of precautionary saving (e.g., Carroll and Samwick, 1998).

We find that self-employed income risk is negatively correlated with credit limits and unused credit. Moreover, we find that those with greater income variability borrow less. These findings suggest that entry into self-employment after flag removal is unlikely to be driven by the fact that credit provides insurance for self-employed income volatility. Last, signaling would imply a positive relation between borrowing changes today and future credit score and credit limit changes. We find a negative relation in the data, suggesting that signaling is not driving our results. While these tests are unable to directly measure why the self-employed borrow and what the marginal borrowed dollar is financing, individuals who enter self-employment after removal of their bankruptcy flags have greater access to credit markets, borrow more, and earn more, all of which are consistent with credit constraints being relaxed.

To further test the hypothesis that the post removal relaxation of credit constraints is driving our results, we explore the relation between predetermined characteristics and entry into self-employment and borrowing. We first explore demographics. There is a large

literature arguing that age and education are important determinants of entrepreneurship. Age tends to increase entrepreneurship (ceteris paribus) and a college education is a positive but much weaker predictor of entry into entrepreneurship than it was in the past (inter alia Karahan et al., 2019 and Salgado, 2017). When we split our sample by age and college education, we find very similar responses of entrepreneurship to increased credit access. These results suggest that age and education do not interact with increased credit access. A consequence of these findings is that the flow into entrepreneurship after bankruptcy flag removal is unlikely to be driven by selection of entrepreneurs along these dimensions.

Our next set of tests isolates individuals who receive the greatest increase in credit limits after their bankruptcy flag is removed. We find that predetermined financial characteristics are the best predictors of subsequent credit limit increases after bankruptcy flag removal. We find that those individuals whose credit is not "maxed-out" and have above-median unused credit and credit scores (measured in the year prior to flag removal) have the largest increase in limits. These individuals also enter self-employment at the greatest rate, consistent with the hypothesis that increased access to credit relaxes financial constraints.

One final hypothesis for our results that we explore is that higher quality entrepreneurs wait until their flags are removed to enter self-employment. We address this in several ways. First, we provide direct tests of selection on prior entrepreneurial profitability. We find that those who enter self-employment before their bankruptcy flag is removed compared to those who enter after removal have similar prior entrepreneurial profitability. Second, we narrow the window of our analysis to +/- 1 year around bankruptcy flag removal, thus estimating individual fixed effects only on recent data around flag removal. These narrow window analyses may better capture unobserved heterogeneity around the removal, and they yield very similar results. Third, we show our main results are robust to controlling for both the profitability and prior entrepreneur experience 7 to 12 years before flag removal. We also acknowledge this concern in the relevant sections, and we discuss where selection may be present and the potential biases that may result.

Last, we provide an assessment of whether our results are externally valid by comparing the responsiveness of start-up rates to credit in our bankrupt sample versus our large sample drawn from the overall population. We show that the elasticity of firm ownership and formal employment with respect to credit are insignificantly different in the bankrupt sample compared to the population estimates. However, the responsiveness of self-employment is weaker in the bankrupt sample than in the general population estimates. Our estimates using the bankrupt sample of the responsiveness of firm ownership to credit are likely to be representative, whereas our estimates of self-employment elasticities from the bankrupt sample likely provide a lower bound.

Our main contribution is to study initial firm ownership using a database that shows when owners hire their initial employees. We provide new evidence that consumer credit is used to fund new businesses and that borrowing increases substantially after owners have their bankruptcy flags removed. We provide additional evidence in a number of tests that show that our results are consistent with credit constraints driving the increased borrowing and that other explanations for the increased credit and borrowing do not explain our results. We view our results throughout as robust evidence that consumer credit matters for firm ownership.

Our paper contributes to several literatures, including the theoretical and empirical literature on credit constraints and start-up rates, cited on the first page of the introduction. Of particular note is the concurrent, independent work by Bos, Breza, and Liberman (2018) and Dobbie et al. (2020). Bos, Breza, and Liberman (2018) focus on the way reported delinquencies, that is, skipped payments as opposed to debt discharge, in the Swedish pawn registry affect earnings and self-employment. Bos, Breza, and Liberman (2018) show that individuals whose past defaults are publicly available for longer are less likely to have a job, are more likely to be self-employed, and earn lower incomes. on average.

Dobbie et al. (2020) merge bankruptcy court records with Social Security Administration

earnings and study the impact of bankruptcy flag removal on labor market outcomes. Dobbie et al. (2020) find insignificant impacts of flag removal across most of their specifications, but where our papers overlap, our point estimates fall within their confidence intervals. One key advantage of our data set is that we observe credit bureau records and see the actual date on which bankruptcy flags are removed from credit reports, whereas Dobbie et al. (2020) must infer removal of bankruptcy flags from the original court filing records, and as we show, there are frequently leads and lags in the credit bureau removals of flags, leading to some measurement error in the removal date. Thus, Dobbie et al. (2020) suffers from a bias towards insignificant results, while we measure the exit from having a bankruptcy flag with actual credit reports.

Relative to existing and concurrent work, our most important contribution is to go beyond previous research and examine early entrepreneurial firms from the Integrated Longitudinal Business Database to show where consumer credit has the largest impact. We show that there is a large impact of personal credit on new employer businesses. In particular, we show that transitions from nonemployer to employer businesses increase sharply as credit access expands - a new result that, to our knowledge, has not been shown anywhere previously.

We also show a large increase in borrowing by the owners of these firms that occurs in the year of hiring their first employee. We thus are the first, to our knowledge, to measure the impact of consumer credit access, independent of entrepreneurial ability, on the rate at which individuals move from being a nonemployer to being an employer business and on the amount that owners of new employer firms borrow. We thus add to the work of Robb and Robinson (2014), who show that small entrepreneurial firms borrow from banks, as we show they also borrow using their own personal credit. Overall, we provide a full characterization of the consumer credit choices of these new entrants given that we are the first to merge credit reports with firm ownership records. We believe this new evidence to be an advance over survey data, which often aggregate sources of credit or do not measure all sources of consumer credit.

2. Theoretical explanations for the importance of credit

There are several potential theoretical explanations for why personal credit can be important for self-employment and for entrepreneurs starting a new employer firm. We present four different explanations here, and in later sections, we test the importance of these explanations for self-employed individuals, both in and out of bankruptcy.

The first explanation we consider is that credit can be used to relax financial constraints. If individuals want to become entrepreneurs (either self-employed or a small business owner) but lack the necessary wealth to do so, they borrow. If credit markets are closed to these individuals, they remain formally employed or unemployed. When these individuals experience an exogenous increase in the availability of credit, they borrow and transition into self-employment and small firm ownership. We expect that individuals borrow more when the returns to entrepreneurship are higher. In Appendix ??, we present a model with working capital constraints that can generate this type of positive correlation between self-employment income and borrowing.

We measure the relation between self-employed income and borrowing for all individuals in the population as well as for the sample of bankrupt individuals. Of course, borrowing may be correlated with earnings ability and quality, and so for the full population, we will not be able to distinguish borrowing due to quality and borrowing due to credit constraints. We thus also examine a sample of bankrupt individuals pre and postbankruptcy where the bankruptcy flag removal arises exogenously and increases credit access. If bankrupt individuals are constrained, we expect to see that when individuals have their bankruptcy flag removed, they enter self-employment and also start new firms while increasing the amount they borrow.

The second explanation we consider is that credit can be used by self-employed individuals to smooth consumption. Under a consumption-smoothing motivation, individuals with access to credit borrow when their self-employed income is low. Thus, we expect to

see a negative correlation between self-employment income and borrowing. Again, we expect this pattern both for the full population and for bankrupt individuals, though it would be mitigated for individuals in bankruptcy if bankruptcy kept individuals from borrowing the full amount they wish to borrow. When individuals emerge from bankruptcy, under a consumption-smoothing hypothesis, we expect borrowing to increase more for individuals with lower self-employment income. Our model in Appendix **??** shows that uninsurable self-employment earnings risk can generate this type of negative correlation between self-employment earnings and borrowing.

The third explanation is that individuals may accumulate credit as insurance if they foresee high income volatility. The second and third hypothesis are related but distinct. Both are based on consumption smoothing: the second hypothesis is about realized borrowing in response to an income loss, while the third hypothesis is about building a credit buffer prior to the realization of the shocks. Without credit insurance, individuals will hold off starting a business because of volatility risk even if current income is high enough. When we test the third hypothesis, we focus on how the variance or risk of self-employment income is related to the demand for credit, parallel to precautionary saving in the Bewley literature (e.g., Carroll and Samwick, 1998). Under this explanation, individuals with a higher variance of self-employment income will obtain higher credit limits that they can draw down later if they experience negative outcomes. Without credit insurance, individuals will hold off starting a business because of volatility risk even if current income is high enough. Empirically, in the full population we expect credit limits to be positively related to the volatility of self-employment income, measured as the coefficient of variation for self-employment income. We expect that while in bankruptcy, the relation will become insignificant or negative and on the removal of the bankruptcy flag, the positive relation will return.

The fourth explanation we examine is whether credit is used by the self-employed to signal unobserved quality. Under this explanation, a self-employed individual who is of higher quality will borrow more than needed to signal their ability. In subsequent periods,

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they will then have a higher capacity to borrow more. Empirically, if this explanation is a key motivation for the use of credit, we will see a positive relation between today's borrowing and tomorrow's credit score (an indicator of future quality) and also the change in credit limits tomorrow.

To summarize, here are the four theoretical explanations for the use of personal credit by the self-employed.

1. Credit constraints: Credit is used by individuals to help finance self-employment and new firm creation. As their credit standing improves, they can borrow more to finance self-employment and new firm creation.

2. Consumption smoothing: Individuals borrow when their self-employed income is low to smooth consumption.

3. Credit access as insurance: Those with more volatile self-employed incomes have greater buffers of credit (e.g., higher credit limits).

4. Credit signaling: Credit is used to signal quality by self-employed individuals and thus increase their capacity for subsequent borrowing.

In subsequent sections, we examine these four motivations for the use of credit using both an overall large sample of three million individuals and a sample of 240,000 bankrupt individuals. The advantage of examining both samples of individuals is that we can see if patterns that exist in the general population of those with credit reports are similar in the bankrupt sample, since bankrupt individuals are the focus of our subsequent analysis. These comparisons thus examine whether the bankrupt sample can be used to learn about the population more generally.

3.   Data description and empirical approach

Our self-employment and firm ownership measures are derived from the Integrated Longitudinal Business Database (ILBD). This database integrates self-employment records (identified by a unique scrambled version of an individual's Social Security number) with the employer firms that are subsequently created and owned by the same individuals. We use the nonemployer/employer links built by Davis et al. (2007). In Appendix **??**, we briefly describe the way the nonemployer/employer universes were linked. In short, the links are derived from administrative data sources and are therefore of the highest possible quality. The self-employment income comes from the universe of Schedule C tax records for sole proprietors across all US states. We therefore have net self-employment income annually from 1998-2010 as well as indicators of whether or not the self-employed individual began employing others.

All consumer credit information is taken from TransUnion at an annual frequency from 2001 to 2010. TransUnion is one of the three largest credit scoring companies in the United States, and it has a market share similar to that of Equifax and Experian. Our main sample is three million individuals with credit reports from the 11 states for which we have Longitudinal Employer-Household Dynamics (LEHD) data. We over sampled bankrupt individuals, so when we compute population estimates, we reweight the sample to match yearly aggregate bankruptcy, delinquency, and foreclosure rates in the 13 states for which we have data. The TransUnion data are then merged based on an anonymized unique identifier to the LEHD. Our data include information on the balance, limit, and status (delinquent, current, etc.) of different classes of accounts held by individuals. We do note an important caveat that concerns bank loans. If bank loans increase after flag removal and they do not appear on the consumer credit records, then our estimates of the elasticity of entrepreneurship with respect to consumer credit may be biased upward as individuals may also be using consumer credit to supplement bank loans.

Our data on unemployment-insured (UI) jobs (or formal sector jobs) come from the LEHD database (see Abowd et al., 2009). The LEHD, which is a matched employer-employee data set that covers 95% of US private sector jobs, includes information on worker flows between UI jobs as well as quarterly earnings. Our employment and earnings data span from 1995 (or 1998 in some cases) to 2008 for 11 states: California, Maryland, Illinois, Texas, Indiana, Nevada, New Jersey, Oregon, Rhode Island, Virginia, and Washington.

Each database contains the same anonymized unique identifiers that can be used to link the data sets together. Our resulting panel is unbalanced and contains earnings (1998-2008), self-employment income (1998-2010), and credit reports (2001-2010) at an annual frequency.

3.1. Variable definitions

All nominal variables, such as self-employed net income, credit balance, and labor earnings, are deflated by the Consumer Price Index (CPI) (expressed in 2008 dollars), and we winsorize the top 1% of each continuous variable, except variables pertaining to the ILBD (since fewer than 1% of our sample has admissible values).

We define an individual to be self-employed in a given year if they earn at least $1,000 of real Schedule C net income throughout the year, and we define an individual to be formally employed if they earn at least $1,000 of real labor earnings throughout the year in an unemployment-insured job. Transitions are defined at an annual frequency; for example an individual is counted as transitioning into self-employment if they earn less than $1,000 of real Schedule C net income in the prior year and then earn at least that much in the current year.

To measure firm ownership, we isolate those who own a firm in the LBD and thus have paid employees. An individual is counted as owning a firm in the LBD if their Social Security number or any other comparable identifier is linked to the ownership of a firm. Links are made to firms, not establishments (Davis et al., 2007, and our Appendix ?? contains full

details on these links). The *1+ years ownership* variable equals 1 if the individual has a valid *firmid* variable in that year. The *2+ years ownership* variable equals 1 if the individual has a valid *firmid* for at least two years in a row. We define two measures of firm ownership, the first of which requires only one year of ownership (ownership in the contemporaneous year t) and includes potentially transitory businesses. Our second definition is more stringent and requires at least two years of ownership (ownership in years t and $t+1$). Thus, our central variable, *Firm ownership, 2+ years*, is forward looking and covers the current year and the subsequent year.

Rather than using a traditional credit risk score, we use the TransUnion bankruptcy score, which is designed to be a measure of bankruptcy propensity. The bankruptcy score lies between 0 and 1000, and higher scores reveal lower odds of bankruptcy. Bankruptcy scores are used only by more sophisticated lenders, and when they are used, they are used in conjunction with a traditional credit risk score. The *Revolving balance* variable includes any type of credit that can be rolled over at a preset interest rate (including bankcards, revolving personal finance loans, and other revolving lines of credit). The combined sum of home equity lines of credit (HELOCs) is included in the *HELOC balance* variable. Traditional unsecured credit cards that are issued by banks are included in the *Bankcard balance* variable.

4. Population relation between credit constraints, employment, and self-employment

In this section, we assess the relation between credit constraints, self-employment, and new firm ownership in the full TransUnion-LEHD sample (we will refer to this sample as the "population" or the "100% Sample"). We impose minimal restrictions on the data. The sample includes prime-age individuals ages 24 to 65 between 2002 and 2007 who earned at least $1,000 of self-employment or labor earnings in any year in the sample window. Our

restrictions yield 16.4 million person-year observations generated by roughly three million individuals.

Table 1 presents data that show that the mean prior-year bankruptcy score (which we will refer to as the "credit score" is 440, and, on average, self-employed individuals in our sample earned $29.1k per annum. Those who work in the formal employment sector earned $40.1k per annum. The self-employment rate is 10.6%, and the transition rate into self-employment is 3.6% per annum. The transition rate out of self-employment is 3.0% per annum. Very few individuals (0.4%) own a firm with an employee, and even fewer own a firm that survives for 2 or more years (0.3%). Note that the low frequency of firm ownership will result in many of our R-squared estimates having low values, but our relatively large samples versus those of previous studies help with statistical significance. Roughly 79.1% of those in our sample are employed in the formal sector, 5.8% are simultaneously self-employed, and 16.1% are nonemployed.

In Table 2, we regress outcome variables such as self-employment and formal employment on deciles of unused revolving credit, controlling for the marginal cost of funds as proxied by the credit score, as well as other forms of available credit. We examine revolving credit, which includes HELOCs, since the borrowing limit is well defined for these credit products. We also examine our results for combined total credit access of the household. Let i index individuals, t index years, and j index pooled deciles of unused revolving credit. We estimate regressions of the following form, which include fixed effects (α_i), year dummies (γ_t), and dynamic controls ($X_{i,t}$):

$$Y_{i,t+1} = \alpha_i + \gamma_t + \sum_{j=2}^{10} \beta_j I(Unused\ credit_{i,t}\ in\ decile\ j) + \Gamma X_{i,t} + \epsilon_{i,t}.$$

These regressions are designed to capture the correlation between the current stock of credit (measured at date t) and future labor market outcomes (measured at date $t+1$). Our regressions include credit scores as a control for the marginal cost of credit, unused mortgage

credit to proxy for available housing wealth, and the unused balance of all other nonrevolving and nonmortgage forms of credit. Unused mortgage credit is defined as the difference between the highest observed mortgage balance and the current mortgage balance. We also include deciles of cumulative lagged earnings as controls, where the deciles are computed using cumulative earnings since 1998. This cumulative lagged earnings control is designed to proxy for all other forms of accumulated wealth other than through home equity. Our remaining dynamic controls include quadratics in both age and tenure. In all specifications we include individual fixed effects to capture nondynamic heterogeneity.

Column (1) of Table 2 demonstrates that the stock of self-employed individuals, measured in year $t + 1$, rises as available credit increases, measured in year t. Moreover, this relation is monotonic. The first two deciles of unused revolving credit correspond to $0 of available credit, while the third decile in Table 2 corresponds to $101 dollars in unused revolving credit and the 9th decile corresponds to $32k in unused revolving credit.

Figure 1 plots the coefficients on the unused revolving credit deciles in Table 2. Prior studies such as Hurst and Lusardi (2004) have proxied access to capital markets using wealth; those studies find that business ownership rates are largely unresponsive to wealth, with a pronounced rise only among the very wealthy. In our data set, in which we directly observe credit constraints, self-employment is increasing across all deciles of credit access.

Columns (2) and (3) of Table 2 show that the transition rate into self-employment rises with available credit, and the transition rate out of self-employment is not impacted. Columns (4) and (5) illustrate a positive relation between new firm ownership, which indicates that the individual hired an employee, and the stock of available credit. Similar to the nonlinear relation between wealth and business ownership in Hurst and Lusardi (2004), the strongest relation between credit and ownership of firms is in the last decile of credit.

Figure 2 plots the coefficients for the relation between firm ownership and available credit. Moving from the 3rd decile to the 10th decile of available credit corresponds to an increase

in firm ownership of 0.05 percentage points, which is a 10% increase relative to the sample average. In Column (6), we show that as self-employment rises, formal employment falls, as expected. We include additional analysis in Appendix Table **??** that includes the full set of coefficients on the controls for the other stocks of credit (which are present in each regression but suppressed) as well as the coefficients on credit scores, our proxy for the marginal cost of funds.

Our main specifications focus on revolving credit balances, since increases are always associated with an increase in liquid resources available to the household. While there may be some advantages to focusing on total unused credit, there are significant measurement issues associated with mortgages. A new first mortgage involves a down payment transfer of money from a household to a bank and is not associated with an increase in liquid resources available to the household. Cash refinancings do increase liquidity to borrowers. Thus, despite not being able to separate out first mortgages from cash out refinancing, we do present results that include total mortgage credit in Online Appendix Table C1. In addition, in Online Appendix Table **??** we report the correlation of various entrepreneurship measures and total unused credit deciles. We find very similar results.

4.1. Population semi-elasticities

In Table 3 we report the population elasticities of self-employment and firm ownership with respect to unused credit. The point estimate in Column (1) is a semi-elasticity, since the dependent variable is binary. The coefficient implies that if an individual's unused revolving credit increases by 10% in the prior year t-1, their firm ownership rate increases by 0.021 percentage points in year t and the subsequent year t-1. That represents a 7% increase relative to the sample average firm ownership rate for two plus years. The magnitude is in line with Figure 2. This elasticity is new to the literature and may provide a useful benchmark for future studies that incorporate links between consumer credit and entrepreneurship.

We repeat the same exercise using total unused credit, and we find a similar result. The magnitude in Column (3) implies a significantly larger elasticity of self-employment with respect to unused revolving credit. If an individual's unused revolving credit increases by 10% in the prior year t-1, their self-employment rate increases by 0.66 percentage points in year t. Relative to our sample average of 10.6% self-employed individuals, that represents a 6.2% increase. Column (4) is the corresponding - and nearly identical - elasticity of self-employment with respect to total unused credit. A consequence of the result in Columns (3) and (4) is that formal employment falls at a similar rate. Column (5) shows that if an individual's unused revolving credit increases by 10% in the prior year t-1, their formal employment rate falls by 0.58 percentage points in year t.

These regressions suggest that consumer credit, self-employment, and formal employment comove in meaningful ways. However, these regressions are not causal, as access to credit is likely correlated with underlying worker characteristics that are time varying and unobserved. One example includes the stock of wealth. We use a number of proxies to control for wealth, but we are unable to directly observe it. We therefore turn to a natural experiment, bankruptcy flag removal, to isolate the impact of consumer credit access on both self-employment and new firm ownership.

5.   Bankruptcy and bankruptcy flag removals

To isolate exogenous changes in credit, our empirical strategy is to compare previously bankrupt individuals before and after removal of their bankruptcy flags. Our discussion of the bankruptcy institutions in the United States is abbreviated and based largely on the discussion by Han and Li (2007), Li and White (2009), and Han and Li (2011). There are two main types of bankruptcy filings in the United States, Chapter 7 (liquidation) and Chapter 13 (repayment plan); however, we are unable to differentiate between the two in our data set. As Han and Li (2007) discuss, more than 70% of bankruptcy filings in the US

are Chapter 7 filings, and of those filings that initially begin as Chapter 13 filings, many are subsequently converted into Chapter 7 filings.[2] As Han and Li (2011) explain, the Fair Credit Reporting Act (FCRA) and the original Bankruptcy Code itself largely govern how bankruptcy filings appear on a credit report. Chapter 7 bankruptcy information is removed up to ten years after the date of filing, whereas Chapter 13 is removed up to seven years after filing.[3]

What is important for the purpose of our regression design is that the removal of the bankruptcy flag follows a cutoff rule. One key advantage of our data set is that we observe credit bureau records, and so we can identify the date on which bankruptcy flags are actually removed from credit reports, since there are sometimes leads and lags in the flag removals given some differences in court bureaucracies, reporting of filings, and the conversions of Chapter 13 into Chapter 7.

We show the importance of observing the actual time to removal of bankruptcy flags in Figure 3. Figure 3 is a histogram of bankruptcy flag removal times. The TransUnion data are not limited by the availability of labor income information, so we have extended the data from 2001 to 2012. With this extension of the data, we can see one cohort enter bankruptcy and potentially exit bankruptcy (recall, bankruptcies can be converted and there are repeat filers). As the graph makes clear, many people have their flags removed after 7 years (Chapter 13), and the majority have their flags removed in ten years (Chapter 7). However, significant masses of flags are removed in years other than year seven and year

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[2]In short, Chapter 7 involves the liquidation of an individual's assets and the discharge of certain debts (student debt, for instance, cannot be discharged, and home equity is often protected up to a state-specific limit. So we include individual fixed effects to absorb this variation), whereas Chapter 13 is essentially a repayment plan and it allows individuals to repay all or part of their debts. See Li and White (2009) for discussion of the way repayments are used strategically to save one's home.

[3]Quoting from Han and Li (2011): "The FCRA states: '605 (a) Information excluded from consumer reports. (1) Cases under title 11 [United States Code] or under the Bankruptcy Act that, from the date of entry of the order for relief or the date of adjudication, as the case may be, antedate the report by more than ten years'; and '(5) Any other adverse item of information, other than records of convictions of crimes which antedates the report by more than seven years.' The FCRA has no rule on the minimum period of time that credit bureaus have to report a bankruptcy filing. Indeed, it is common that credit bureaus remove a Chapter 13 bankruptcy record from a credit report after only seven years. Also, the Act has no time restrictions on using the bankruptcy record that is maintained in the creditor's proprietary database."

ten. Since we observe credit reports, this kind of measurement error is not an issue - credit scores respond to what is on the credit report, and we condition on the flag being dropped from the report. This uncertainty about the actual date of emergence from bankruptcy can create measurement error for **?** and other studies that infer removal times from the time individuals enter bankruptcy using public filing records.

Our sample window is 2001-2007 (2008 is not included as we use forward lags of variables). We restrict our attention to 24-65 year olds. Even though our sample window stops in 2007, our credit data allow us to identify flag removals between 2002 and 2010. We include all flag removal cohorts in our analysis. While our time period includes individuals whose flags are removed before and after the bankruptcy reform act of 2005, our research design is unaffected, since everyone in our sample previously filed for bankruptcy before 2005. In results that are available upon request, we limit the sample window to 2001-2005, and we use alternate variable definitions; our main results are nearly identical.

Let i index individuals and t index years (from 2001 to 2007). Let α_i denote a set of individual fixed effects and γ_t denote year dummies. Let $Y_{i,t}$ denote the outcome of interest (a self-employment indicator variable, earnings, etc.) of individual i in year t. Let $D_{x,i,t}$ be an indicator variable taking the value of one when an individual is x periods before (if x is negative) or after (if x is positive) flag removal in year t. For example, $D_{-2,i,t}$ is an indicator variable indicating if an individual is two periods before flag removal; likewise, $D_{0,i,t}$ takes a value of one if the individual is in the year of flag removal, and $D_{1+,i,t}$ takes a value of one if the individual is one or more years past flag removal. The specifications we use are of the following form:

$$Y_{i,t} = \alpha_i + \gamma_t + \beta_{-2}D_{-2,i,t} + \beta_{-1}D_{-1,i,t} + \beta_0 D_{0,i,t} + \beta_{1+}D_{1+,i,t} + \Gamma X_{i,t} + \epsilon_{i,t}. \qquad (1)$$

The objects of interest are β_0 and β_{1+}, which summarize the impact of flag removal on the outcome variable in the year of removal as well as subsequent years, respectively, relative

to the omitted group of individuals who are three or more years prior to bankruptcy flag removal.

Our identification exploits individual-level variation in bankruptcy status, and thus we cluster our standard errors at the individual level. We also cluster the standard errors at a higher level than the individual level (cohort by zip). We show that our main results persist in Online Appendix **??**.

A limitation of this study, as well as all other regression designs, is that one cannot simultaneously include time-since-bankruptcy, cohort fixed effects, and time fixed effects (e.g., in labor economics, one cannot distinguish age, cohort, and time effects in Mincer wage regressions). Our individual fixed effects include cohort effects. The individual fixed effect along with the year dummies can be combined to recover time-since-bankruptcy (since it increases linearly, and the fixed effect allows for an arbitrary intercept), thus generating collinearity.

While we cannot separately identify time-since-bankruptcy and cohort effects, our fixed effects are removing variation that is linear in time-since-bankruptcy. Thus, our exclusion restriction is that variables satisfy a linear trend in time-since-bankruptcy. A necessary (but not sufficient) condition for causal inference is that there are no pretrends prior to flag removal (i.e., β_{-2} and β_{-1} are not statistically different from zero, or β_{-2} and β_{-1} are statistically different from zero but $\beta_{-2} \approx \beta_{-1}$).

Thus, we test the presence of observable pretrends that are not captured by our specifications by showing that β_{-2} and β_{-1} are not statistically different from zero in the majority of our specifications.

5.1. *Summary statistics surrounding bankruptcy flag removals*

Table 4 compares the mean values of our main variables of interest one year before bankruptcy flag removal to one year after bankruptcy flag removal. This analysis compares

the pooled outcomes of everyone in our sample one year before flag removal to one year after flag removal. Since some of the later cohorts (2007 through 2010) of flag removals are not observed one year after the flag removal, the sample sizes differ across the columns of Table 4.[4] This section is designed to provide raw averages of important variables and summarize broad changes in those variables. In the sections that follow, we will address cohort and compositional issues by including fixed effects and dynamic controls in all regressions.

Panel (A) of Table 4 describes the main "stock" (or "level") variables. If we define self-employment and formal employment based on a \$1,000 earnings threshold, Column (1) of Table 4 shows that 9.0% of individuals are self-employed one year before bankruptcy flag removal and 78.7% of individuals are formally employed. Following flag removal, Column (2) shows that the self-employment rate increases by 0.6% to 9.6%, whereas the formal employment rate decreases by 0.1% to 78.6%. Column (4) shows that the change in self-employment is significant at the 10% level, while the change in formal employment is not.

Roughly 6.1% of individuals in our sample are simultaneously formally employed and self-employed (SE), and roughly 18.4% of the individuals in our sample are nonemployed. Following flag removal, the fraction that holds two jobs increases, whereas nonemployment moves insignificantly.

Prior to flag removal, 0.4% of our sample own a firm in the LBD for one plus years, whereas 0.2% of our sample own a firm in the LBD for two plus years (e.g., we require ownership in years t and $t+1$). Following flag removal, we see a significant increase in the latter definition of firm ownership by 0.1%.

Panel (B) of Table 4 describes the main flow variables. Prior to flag removal, 3.1% of individuals transition into self-employment, whereas 2.8% transition out of self-employment. In the year after flag removal, the transition rate into self-employment increases by 0.3%

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[4]We include these cohorts to maximize power, since these cohorts still allow us to inform at least one of the coefficients $\{D_{-2,i,t}, D_{-1,i,t}, D_{0,i,t}, D_{1+,i,t}\}$ and are thus used in the main regression analysis. So while the total number of individuals in our sample frame is 220k, only 170k reach one year before flag removal, and only 110k reach one year after flag removal.

per annum to 3.4%, which is significant at the 10% level. The rate at which individuals transition out of self-employment also increases from 2.9% before flag removal to 3.1% after flag removal, and this difference is significant at the 10% level.

Panel (C) of Table 4 describes the main earnings variables. Per capita self-employment income is about \$2,140 per annum. If we adjust for the fact that most individuals are not self-employed, annual self-employed net income per self-employed individual is \$23,800 (=\$2,140/0.009). Following flag removal, self-employed net income per capita increases by \$161. Per capita formal earnings in the sample are \$32,683. Following flag removal, real annual labor income increases significantly, by \$300. If we adjust for the fact that some individuals are not working, annual labor earnings per worker is approximately \$41,500 (=\$32,683/0.0787). Real annual total income is the sum of both self-employed (SE) net income and labor (non-SE) earnings.

Panel (D) of Table 4 describes the main credit variables. We see large credit balances prior to flag removal, since the individuals have a partial recovery in credit access before their flag is removed (for more discussion, see Cohen-Cole et al., 2013). Following flag removal, however, we see a large increase across all types of credit, especially mortgage credit (see Han and Li, 2011 for more results on credit portfolios after flag removal).

5.2. Bankruptcy flag removal: baseline regression results

We begin our regression results for bankruptcy flag removal and examine changes in credit scores, entry into self-employment, and hiring the first employees. Table 5 illustrates the impact of flag removal on credit scores, self-employment, and firm ownership. The coefficients in the table correspond to $(\beta_{-2}, \beta_{-1}, \beta_0, \beta_{1+})$ in Eq. 1. The final two rows of Table 5 test equality of coefficients (e.g., *Sig Diff 1+yr & -2yr* at 10%' is equal to y if we can reject equality of β_{1+} and β_{-2} at a 10% significance level).

In all regressions of Table 5, we include year fixed effects and individual fixed effects to

correct for time trends and compositional differences in state laws, industry, occupation, and any other static characteristics of the individual. We also include dynamic controls such as quadratics in age and tenure.

Columns (1) and (2) of Table 5 demonstrate the large increase in credit scores following bankruptcy flag removal. This finding corroborates the prior work of Musto (2004) and Han and Li (2011) and is at the core of the credit access effect we study below. To visualize this change in credit scores, Fig. 4 illustrates the regression coefficients from Column (1), showing the stable trend in credit scores leading up to the flag removal, followed by a punctuated one-time level shift in credit scores. Column (2) illustrates that after we take out a quadratic age trend, individuals' credit scores one or more years after flag removal are close to preflag removal scores; however, this subsequent mean reversion in scores is largely due to the increased borrowing following flag removal.

Column (3) of Table 5 defines self-employment using a $1,000 annual Schedule C net income cutoff. Flag removal has no significant effect on self-employment. Column (4) of Table 5 shows that the transition rate into self-employment is significant and positive in the year of flag removal. Relative to the omitted group, individuals are 0.161% more likely to transit into self-employment in the year of flag removal. Relative to the sample average, this 0.161% represents a 5% increase (=0.161/3.1). However, this increase is quite transitory, and we are not able to reject equality of β_{-2} and β_{1+} at a 10% significance level.

Columns (5) and (6) of Table 5 illustrate the impact of bankruptcy flag removal on whether the individual owns a firm in the LBD database. Firms in the LBD database must have at least one employee. In Column (5), we define firm ownership to be at least one or more years of firm ownership (from date t onward). This definition includes relatively transitory firm ownership spells of one year and less. We find that following flag removal, ownership of new firms increases but insignificantly. In Column (6), we define firm ownership to be at least two or more years of firm ownership (from date t and $t+1$ onward). Column (6)

illustrates that under this more stringent definition, there is now a significant and positive increase in firm ownership following bankruptcy flag removal, relative to the omitted group. Following flag removal, the odds that an individual owns a firm in the LBD increases by 0.05% (or approximately 1,000 start-ups in our sample of 1.5 million person-year observations). Relative to the sample average, the 0.05% represents a 25% increase (=0.05/0.02). We can reject equality of β_{-2} and β_{1+} (as well as equality of β_{-2} and β_0) at a 10% significance level. Fig. 5 plots the results from Column (6), illustrating the rise in employer firms following flag removal.

Column (1) illustrates that among individuals who transition into self-employment, those who transition into self-employment one or more years after flag removal earn, on average, $991 (=(3376-158.2)-(2274-47.61)) more in Schedule C net income relative to those who transition into self-employment two years prior to flag removal. Relative to the sample average, self-employed net income of $23,800 (adjusting for zeros in Table 4), $991 represents a 4% gain. We arrive at this number by first computing the net income gain of an individual who transits into self-employment one or more years after flag removal. For such an individual, three coefficients are nonzero and must be summed to obtain the overall effect of $11,740 (=1+ years after removal (d) x Trans. into self-employed, $1,000+ (d) + 1+ years after removal (d) +Transition into self-employed, $1,000+ (d) = 3376-158+8522). Repeating this exercise for those who transition two years before flag removal, the overall effect is $10,748 (=2274-48+8522). Taking the difference yields $991 (= $11,739.8-$10,748.39). Since the unconditional transition term (*Transition into self-employed, $1,000+ (d)* cancels in these calculations, we omit it in the remainder of the paper. This combined difference of coefficients between the year after flag removal and two years before is the key statistic from the transition tables, since it captures the impact of flag removal on transition outcomes. Therefore, it is reported in the bottom two rows of every table along with its significance level.

Figure 6 plots the summed coefficients from Column (1) of Table 7. The points on the

plotted line can be interpreted as the gain in Schedule C net income from entering self-employment, relative to a nontransitioner in the omitted group (i.e., those who are three or more years prior to flag removal). In particular, we add the coefficients on the flag removal indicator (e.g., *2 Years before removal (d)'*, interaction term (e.g., *2 years before removal (d) x Trans. Into self-employed, $1,000+ (d)'* and the transition term (e.g., *Transition Into self-employed, $1,000+ (d)'*, and we compute standard errors using the delta method. As the figure demonstrates, there is a stable trend for self-employed income prior to flag removal. Following flag removal, the net income gain for those who enter self-employment increases rapidly. The difference in self-employed income for those who transition into self-employment one or more years after removal versus two years prior to removal is, as we saw before, $991. This calculation is illustrated on the graph.

Given the importance of housing equity in survey evidence of entrepreneur balance sheets (e.g., Robb and Robinson, 2014 and Adelino et al., 2015), we separately consider HE-LOCs. Column (2) of Table 7 shows HELOC balances of individuals who transition into self-employment. Those who transition into self-employment following a bankruptcy flag removal borrow $3,766 (=1,253+3,551-(278+760)) more using HELOCs relative to those who transition into self-employment prior to flag removal.

Column (3) of Table 7 shows the total borrowing of individuals who transition into self-employment. Those who transition into self-employment following a bankruptcy flag removal borrow $15,337 (=16,195+14,373-(6,422+8,809)) more relative to those who transition into self-employment prior to flag removal.

Finally, we assess whether those who transition into self-employment following flag removal are "marginal" entrepreneurs. We address this question by focusing on the turnover rates of new entrants, pre- and post-flag removal. Column (4) of Table 7 regresses future exits (those who exit in year $t + 1$) on self-employed entrant dummies (those who enter in year t) interacted with the bankruptcy flag removal dummies. Column (4) shows that newly

self-employed individuals are transitioning out of self-employment at a very high rate, 38% per annum, unconditionally. However, following flag removal, we see no disproportionate change in the subsequent rate at which these individuals exit self-employment. The finding of no change in exit from self-employment suggests that the post-flag removal new entrants are not marginal, and we can make this statement with a high degree of confidence since our point estimates in Column (4) are precisely estimated.

5.3. Borrowing by firm owners

We now examine the borrowing behavior of individuals who are firm owners and therefore have paid employees. Similar to our previous analysis, we recognize that there may be selection effects into firm ownership post-flag removal. We discuss these selection concerns in subsequent sections.

Panel (B) of Table 6 presents summary statistics on the borrowing behavior of firm owners before and after flag removal. For those individuals who are firm owners, we see their borrowing total $205,800 prior to flag removal and then increase by $33,200 one year after flag removal. They also borrow more using revolving credit and HELOCs.

Table 8 formalizes these summary statistics by regressing borrowing on a window of dummies around flag removal interacted with an indicator for firm ownership (two plus years). We include the same set of controls as in our prior regressions. Column (1) of Panel (B) shows that firm owners who are one or more years after flag removal borrow $1,591 (=(1,067+904.1)-(205+175.4)) more using unsecured ("bank card") credit than firm owners who are two years prior to flag removal. Column (2) shows that firm owners who are one or more years after flag removal borrow $8,138 (=(8,645+4,291)-(3,683+1,116)) more using revolving credit than firm owners who are two years prior to flag removal.

The next two columns of Table 8 isolate housing credit. Column (3) shows that firm owners who are one or more years after flag removal borrow $29,693 (=(37,997+8,462)-

(7,493+9,273)) more using mortgage credit than firm owners who are two years prior to flag removal. In terms of HELOCs, Column (4) shows that firm owners who are one or more years after flag removal borrow $9,208 (=(7,716+3,576) - (1,317+767)) more using HELOCs than firm owners who are two years prior to flag removal. Turning to total debt balances (including secured and unsecured debts), Column (5) shows that firm owners who are one or more years after flag removal borrow $39,835 (=(47,332+14,812)-(13,318+8,991)) more across all lines of credit than firm owners who are two years prior to flag removal.

5.4. Transitions into self-employment

We now examine characteristics of individuals who enter self-employment. We use the ILBD to measure transitioners' Schedule C net income as well as exit rates, and we use TransUnion credit reports to measure borrowing behavior.

Panel (A) of Table 6 describes summary statistics for the borrowing patterns of those who transition into self-employment. Those who transition into self-employment have total credit balances of $149,300 one year prior to flag removal versus $169,900 after flag removal, a raw difference of $20,600. We see similar patterns for revolving balances and home equity loans.

Table 7 regresses the income and borrowing of bankrupt individuals surrounding bankruptcy flag removals with indicator variables for transitions into self-employment. We include fixed individual characteristics, age, tenure, and year effects and individual fixed effects. While flag removal is exogenous, the decision to transition into self-employment is not, so these results may contain selection effects into self-employment. Those individuals who flow into self-employment post-flag removal may be higher quality entrepreneurs, thus biasing the coefficients on profitability upward. We provide direct tests of selection based on prior entrepreneur profitability and experience in Sections 6.2. and 8.; however, we cannot rule out selection on time varying unobservables. To meaningfully interpret the interaction terms, we

compare those who transition into self-employment two years before flag removal to those who transition into self-employment one year after flag removal.

Figure 7 plots the summed coefficients from Column (5) of Table 8. The points on the plotted line can be interpreted as the increase in total credit balances among firm owners, relative to nonowners in the omitted group (i.e., those who are three or more years before flag removal).[5] As the figure demonstrates, there is a stable trend in borrowing prior to flag removal. Following flag removal, relative borrowing among firm owners increases rapidly. The difference in borrowing for those who are firm owners one or more years after removal versus two years prior to removal is $39,835 (=(47332+14812)-(13318+8991)). This represents an approximately one third increase in borrowing relative to the sample average real total balances of $121,000.

These results suggest that firm owners, as opposed to the self-employed, rely heavily on credit markets. Firm owners borrow nearly five times as much using personal credit. This large increase in borrowing confirms the innovative cross-sectional survey work by Robb and Robinson (2014), who show that many start-ups rely on secured and unsecured consumer credit to finance their businesses.

6. Mechanisms

Following bankruptcy flag removal (a) credit scores increase, (b) new firm ownership increases, (c) the flow rate into self-employment marginally increases, (d) self-employed entrants earn more post-flag removal, (e) self-employed entrants borrow more post-flag removal, and (d) firm owners borrow five times more than self-employed entrants post-flag removal.

In this section we explore several hypotheses for our results. We first entertain the hypothesis that our results are driven by unobserved heterogeneity. We narrow our window,

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[5]In particular, we add the coefficients on the flag removal indicator (e.g., *2 years before removal (d)*), interaction term (e.g., *2 years before removal (d) x Firm ownership, 2+ yrs. (d)*) and the ownership term (e.g., *Firm ownership, 2+ Yrs (d)*), and we compute standard errors using the delta method.

estimating our fixed effects using $+/-$ one year of flag removal; we find that our results persist. The robust results suggest that our results are not driven by comparing those with flag removals to cohorts that are much closer to their entry into bankruptcy, who may have different unobservable characteristics, e.g., health status or wealth.

We then interact our flag removal variables with predetermined characteristics to assess the potential role of credit constraints and selection. We find that subsets of individuals who receive the largest increase in credit limits enter self-employment at a higher rate. We find no differential response by age or education, suggesting our results regarding flows into self-employment and subsequent borrowing are not driven by selection on these characteristics.

We then test three other competing hypotheses for the rise in self-employment and borrowing after flag removal: (1) credit increases after flag removal because of consumption-smoothing motives, (2) self-employment increases because credit provides insurance against shocks, and (3) individuals may be borrowing to signal that they are "high quality" and thus, borrowing may not reflect immediate credit constraints. We provide evidence that refutes each of these three hypotheses.

Last, despite the fact that we do not directly observe employer screening (e.g., credit checks) in our data, we briefly discuss the formal and self-employment flows following flag removal. Flows into formal employment and out of self-employment increase, which is qualitatively consistent with credit checks being present. These flows are unlikely to affect our point estimates or interpretation of increased borrowing and profitability of new self-employed entrants following bankruptcy flag removal.

6.1. *Unobserved heterogeneity*

Since our benchmark results use relatively wide panels of individuals, one may be concerned that individual "types" are changing over time in a way not reflected by individual fixed

effects, year dummies, or age. One concern may be that those who are still years away from leaving bankruptcy have different unobserved characteristics than those who just leave bankruptcy, for example, health status, wealth, marital status. To address concerns of unobserved heterogeneity, we narrow the window of observations around $+/-one$ year around bankruptcy flag removal. In this specification, we are only comparing individuals before and after flag removal, excluding all observations prior to that time window. The omitted group is those one year prior to flag removal. Table 9 reports our main results for the narrow window.

Inspection of Table 9 reveals similar effects of flag removal for our main outcome variables. As in our benchmark specifications, following flag removal, Column (1) shows that the credit score increases by 79 points in the year of flag removal. Column (2) shows that the self-employment rate increases by 0.5% in the year of flag removal. Column (3) shows that the transition rate into self-employment increases by 0.3% in the year of flag removal. Column (4) implies an insignificant decline in firm ownership for one or more years, which tends to include transitory business owners. In the year after the removal, the coefficient becomes negative. However, our preferred specification in Column (5) implies a positive and significant 0.06% increase in firm ownership for two or more years. Overall, these narrow window results are broadly consistent with our benchmark findings in Table 5.

6.2. Heterogeneous response of firm ownership

In this section, we examine the heterogeneous response of firm ownership to bankruptcy flag removal. We view these heterogeneous responses as informative of various mechanisms, including selection and credit constraints. We split individuals according to (i) age, (ii) education (which is imputed in the LEHD, see Abowd et al., 2009 for more discussion), (iii) unused revolving credit in the year before flag removal, (iv) unused total credit in the year before flag removal, and (v) credit score in the year before flag removal. If individuals are selecting into postflag-removal self-employment or new firm ownership based on age and

education, then we would expect a positive and significant interaction between these variables and flag removal. If preflag-removal characteristics, including credit utilization and credit score, predict greater credit access after the flag is removed, we expect greater transitions into self-employment among those subgroups.

We present the heterogeneous response of new firm ownership in Table 10. Note that the predetermined characteristics are constant throughout the panel and are thus absorbed into the individual fixed effects. To facilitate exposition and interpretation, we limit our sample to individuals within $+/- one$ year of flag removal. Column (1) shows our results when we split the sample among those who are younger/older than 40 years of age at the date of flag removal. We find a small and precisely estimated zero interaction effect between flag removal and age. Older households are not selecting into firm ownership at a higher rate following bankruptcy flag removal. Likewise, Column (2) shows our results when we split the sample among those who have a college education versus those who do not. Again, we find a small and precisely estimated zero interaction effect between flag removal and college education. These results suggest that individuals are not differentially selecting into firm ownership after bankruptcy flag removal based on age or education. Our results are similar for alternative age and education splits. While other studies have shown that age and education are important determinants of self-employment (e.g., Karahan et al., 2019 and Salgado, 2017), our results indicate no interaction between age, education, and credit constraints.

Columns (3) and (4) of Table 10 split the sample by credit characteristics in the year before flag removal based on the following criteria: (i) above median unused revolving credit, (ii) above-median unused total credit, and (iii) above-median credit score. As we show in Table 11, these characteristics predict the largest subsequent increases in credit access after bankruptcy flag removal. The cutoff for above-median unused revolving credit in the year before flag removal is $1,000 in 2008 dollars (rounded to the nearest significant digit), and the cutoff for median unused total credit is $8,000 (rounded to the nearest significant digit).

The cutoff for median credit score is 200 (rounded to the nearest significant digit), which is extremely low. Column (3) of Table 10 shows that those with above-median unused revolving credit are 50% more likely to be firm owners relative to the baseline effect. The same is true in Column (4) for unused total credit. Last, Column (5) shows that those with above-median credit scores in the year before flag removal are much more likely to be firm owners. The magnitude of the interaction suggests a 100% increase in the transition rate into firm ownership for those who have above-median credit scores prior to flag removal.

6.3. Heterogeneous credit responses

Table 11 examines heterogeneous changes in credit limits and borrowing responses for the same predetermined credit variables: (i) above-median unused revolving credit, (ii) above-median unused total credit, and (iii) above-median credit score. We examine which characteristics predict the largest subsequent increases in credit access after bankruptcy flag removal. The only portion of the credit history that is being erased is the bankruptcy flag, and so individuals who have unused credit and are not likely to pose default risk based on other credit characteristics will be impacted the most and have higher credit limit increases and greater borrowing. Those individuals who pose substantial default risk along other dimensions, such as being "maxed-out" on their credit lines, will retain low scores after flag removal and will thus be impacted less and will have smaller credit limit increases. For those with poor observable credit characteristics, their default probability is high and remains high regardless of their bankruptcy flag. We expect the smallest credit responses among these individuals (which we will see in Table 11).

Columns (1) to (3) of Table 11 examine revolving credit limits. The results in column (1) show that individuals with above-median unused revolving credit in the year before flag removal have limit increases that are roughly four times larger than those with below-median unused revolving credit. Individuals with below-median unused revolving credit have less than $1,000 in unused credit prior to flag removal and are extremely constrained. Despite

flag removal, preremoval high-utilization individuals have very small credit limit increases. Column (2) shows that this pattern is also true for total unused credit. Column (3) shows that an above-median credit score prior to removal is also extremely predictive of a large increase in credit limits following flag removal.

Columns (4) through (6) of Table 11 examine total real revolving balances. The results show that these individuals with unused existing credit and higher scores are the ones who borrow the most. In Column (4) the dependent variable is real revolving credit balances. Those individuals who have above-median unused revolving credit balances prior to flag removal borrow an additional $2,514 in the year after flag removal (their limits increase by $4,155). Column (5) also shows an increase in borrowing when individuals are split by their total unused credit. Column (6) provides the strongest result. Those with above-median credit scores in the year prior to flag removal borrow an additional $3,521 after flag removal. Those below the median borrow much less.

One concern may be the fact that credit is correlated with entrepreneurial propensity. Are those who have above-median credit access prior to flag removal also those who had past self-employment experience? We explore this in Tables 12, as well as in Online Appendix ??. In Table 12, we repeat our exercise in Table 10 with additional controls for entrepreneurial experience prior to entry into bankruptcy. Since we cannot distinguish Chapter 13 (filed seven years before flag removal) and Chapter 7 (filed ten years before flag removal) bankruptcies, we include controls for real self-employed earnings 7, 8, 9, 10, 11, and 12 years prior to flag removal (Columns (1) through (5) of Table 12). To further support the hypothesis that it is prior entrepreneurship instead of predetermined credit access that drives the results in Table ??, we include a full set of interactions between flag removal dummies and real self-employed earnings 7, 8, 9, 10, 11, and 12 years prior to flag removal (Columns (6) through (10) of Table 12).

After controlling for prior entrepreneurial experience in Columns (1) through (5) of Table

12, we find results very similar to our benchmark specifications. Nearly all coefficient magnitudes remain unchanged. Nearly all of the coefficients also remain significant, with one exception (Column (3), t-stat of 1.62), which is the interaction between unused revolving credit and flag removal. While the coefficient is marginally insignificant, the point estimate on the interaction term remains very stable. It is likely that we need more power to identify the large number of coefficients included in these regressions. However, when we include a full set of interactions in Columns (6) through (10) of Table 12, all of the coefficients retain their significance, sign, and magnitude relative to our benchmark specifications.

6.4. Tests of alternative rationales for increased borrowing

Our results show that individuals with the largest credit limit increases after bankruptcy flag removal enter entrepreneurship at a higher rate. These results are consistent with credit constraints being relaxed. However, as we discussed in Section 2. there may still be alternative explanations that can explain our results. These explanations include (1) consumption-smoothing motives that coincidentally imply increased borrowing post-flag removal; (2) better ability to insure post-flag removal, allowing individuals to coincidentally enter self-employment; or (3) signaling motives that coincidentally drive borrowing. We test each of these mechanisms among self-employed individuals in both the population sample and our bankrupt sample.

We first assess whether self-employed individuals smooth consumption with credit by measuring the correlation between changes in self-employed income and changes in borrowing. A consumption-smoothing motive (as well as if credit was being used as insurance) would imply that in response to self-employed income losses, individuals should borrow more. Table 13 reports the results for the population sample in Columns (1) and (2) and for the bankrupt sample in Columns (3) and (4). In Column (1), we find a positive correlation between self-employed income and changes in revolving credit balances. In Column (2) we find a positive correlation between changes in self-employed income and log revolving

credit balances (in levels). These results strongly refute the hypothesis that self-employed individuals are using credit to smooth consumption.

Our model in Appendix ?? predicts a positive correlation between changes in self-employed income and credit due to a working-capital constraint. We take the strong positive correlation as suggestive evidence that credit constraints are relevant among the self-employed.

We further test consumption-smoothing motives by measuring whether individuals with greater self-employment income volatility obtain greater credit limits to insure against potential income losses. To test whether individuals build a precautionary credit buffer, we measure the relation between self-employed income volatility and the unused credit capacity of individuals. Our measure of self-employed income volatility is the coefficient of variation (standard deviation over mean) of self-employed income in our sample period. If credit were being used to provide insurance against losses, we would expect those individuals with the greatest self-employed income volatility to have the largest buffers of credit limits and unused credit, and conditional on receiving a shock they should borrow more. We measure these correlations in Table 14. We find negative relations between self-employed income volatility and balances (Column (1)), limits (Column (2)), and unused credit (Column (3)) for the full sample. The same is true for the bankrupt sample (Columns (4) through (6)). These results suggest that credit is not being used to insure volatile income processes. Individuals with the most volatile self-employed income processes are not applying for greater credit limits, nor are they borrowing more.

Last, we test the signaling hypothesis that self-employed individuals are borrowing to signal that they are high quality, thus increasing their future access to credit. To test this hypothesis, we examine two different correlations: the correlation between changes in revolving credit balances and (i) future changes in credit scores and (ii) future changes in credit limits. The signaling hypothesis would imply a positive correlation between borrowing

today and future credit access. Table 15 reports these two correlations. Columns (1) and (2) show that among the population of self-employed individuals who borrow more between years t and t-1, they have negative credit score growth and credit limit growth between years t+1 and t. This result also holds true in our bankrupt sample (Columns (3) and (4)). These results suggest that signaling is unlikely to play a significant role in generating the borrowing patterns we observe after flag removal among the self-employed.

6.5. Formal employment flows

In this section, we discuss formal employment job transitions to provide a complete characterization of the effects of flag removal. We present these results in Online Appendix Table ??.3. Credit can impact flows into formal employment through two channels. First, employees may be able to use credit to help get a new job. Second, employers may use credit checks and the existence of bankruptcy flags to screen potential employees. We do not observe credit checks directly, and thus our analysis and discussion are limited to flows into and out of formal and self-employment. Given that we do not observe credit checks, we leave direct evidence of screening to future research.

We examine two testable implications of employer screening: (1) flows into formal employment increase since employers no longer see a bankruptcy flag on the worker's record, and (2) flows out of self-employment and unemployment increase if individuals can enter the formal employment sector. We find that formal employment increases, as do gross flows both into and out of formal employment. We also find that flows out of self-employment increase. This analysis reconciles our findings with Bos et al. (2018) by showing that flows into and out of self-employment increase in response to bankruptcy flag removal, suggesting there may be a role for both credit constraints and screening. Other phenomena could be consistent with higher flows into formal employment and greater flows out of self-employment, for example if credit facilitates job search (Herkenhoff et al., 2015); however, we are unable to distinguish these phenomena from screening in our context.

7. Representativeness and external validity

Our identification strategy focuses on the population of bankrupt individuals who have had their bankruptcy flag removed. Nearly 10% of Americans have ever filed for some type of bankruptcy protection (Indarte, 2019). To test how representative the bankrupt sample is, in Table 16 we examine how the full population and bankrupt sample differ in terms of their responsiveness of self-/formal employment to credit. In particular, we reestimate the population semi-elasticities from Table 3, except now we include an interaction term between unused credit and a time-invariant flag for whether the individual is in our bankrupt sample. This interaction term captures the difference in responsiveness of bankrupt and nonbankrupt individuals to credit.

In Column (1) of Table 16, we measure the elasticity of firm ownership in years t and $t+1$ with respect to unused revolving credit at year t-1. We find that bankrupt individuals have moderately weaker firm ownership responsiveness to unused revolving credit; however, the point estimate is small and insignificant. In Column (2), we repeat this exercise for self-employment. We find that the elasticities of self-employment with respect to unused revolving credit are roughly 50% smaller in the bankrupt sample. Last, in Column (3) we study the relation between formal employment and unused credit. We find that the interaction between bankruptcy and unused credit is insignificant but imprecisely estimated.

Overall, we find insignificant differences between bankrupt individuals and the broader population (among those with credit reports) in the response of firm ownership and formal employment to credit. However, bankrupt individuals appear to be less responsive to credit when making the choice to enter self-employment. These findings suggest that our estimates relating responsiveness of firm ownership and formal employment to credit in the bankrupt sample may be representative, whereas our estimates in the bankrupt sample regarding self-employment may be a lower bound.

8. Robustness

We discuss three additional robustness exercises in this section. We consider alternative definitions of self-employment, test for differences in lagged earnings among pre- and post-flag removal new self-employed entrants, and show that nonself-employed job transitioners borrow much less than new self-employed entrants.

We reestimate our benchmark specifications for alternative definitions of self-employment and present these results in Online Appendix Table **??**. We examine self-employment definitions that require (i) $5,000 of Schedule C net income, (ii) two consecutive years of $1,000 Schedule C net income, (iii) two consecutive years of $5,000 Schedule C net income, and (iv) 50% of total income (defined as the sum of formal labor market earnings and Schedule C net income). These additional results confirm that while the stock of self-employed individuals remains constant, we do see marginal changes in both the flow rate in and flow rate out of self-employment. The flow rate into self-employment is positive and marginally significant after bankruptcy flag removal in three of six definitions considered in this paper. Despite the relatively small effect of flag removal on self-employment flows, individuals who subsequently flow into self-employment following bankruptcy flag removal borrow more and earn more net income.

Since flag removal is foreseeable, there may be concern that better entrepreneurs who anticipate the need for credit deliberately wait until the flag is removed to start a business. We address this potential issue in two ways: (i) if "better" entrepreneurs are waiting until their flag is removed to borrow at a lower cost to start a business, then waiting until credit is cheaper simply reinforces the point that credit matters for start-ups, and (ii) to test for selection over and above the results in Section 6.2., we take advantage of the panel dimension to our data and compare prebankruptcy entrepreneurial performance and labor earnings among those who enter entrepreneurship before and after flag removal. In Appendix **??** we show that the entrepreneurs who transition into self-employment following flag removal are

very similar in terms of prior self-employment income and prior labor earnings.

Moreover, recent independent work by Gross et al. (2020) has also provided formal tests of the anticipation of bankruptcy flag removal by looking at credit application behavior. They show that rather than waiting an additional quarter for credit at more favorable rates after their flag is removed, individuals continue to apply for credit normally prior to the removal, indicating a lack of foresight.

Finally, are all job transitioners more likely to borrow simply because they have (and expect) earnings gains, or do the newly self-employed rely particularly heavily on credit?[6] As another test of the importance of credit for the self-employed, Appendix ?? compares borrowing by those who transition into formal sector employment and those who transition into self-employment. Both sets of individuals realize income gains (new self-employed entrants have net income that increases by roughly $1,000, and new formal sector entrants earn roughly $1,500 more; see Appendix Table ??). However, those who transition into formal employment borrow less than nontransitioners. In contrast, those who transition into self-employment after flag removal borrow $15,337 more relative to those who transition prior to flag removal. Thus, even though self-employed entrants have smaller earnings gains than new formal employment entrants after flag removal, the self-employed borrow much more heavily following flag removal. This evidence is consistent with the credit access effect being an important determinant of self-employment.

9. Conclusions

We examine how consumer credit affects employment prospects, earnings, and entrepreneurship. Using a sample of three million prime-age individuals, we show that self-employment and firm ownership are increasing functions of available credit. In contrast, formal employment declines as access to credit increases. Our results, which are based on di-

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[6]We thank Nawid Siassi for suggesting this exercise.

rect measures of credit constraints, contrast with prior studies, including Hurst and Lusardi (2004), who find that business ownership and self-employment are largely flat functions of wealth. Our findings agree with those of Robb and Robinson (2014), who find that many start-ups receive debt financing through the personal balance sheets of the entrepreneur.

We use individual bankruptcy flag removals and compare them to individuals still in bankruptcy to isolate a large discrete increase in credit access not directly associated with differences in creditworthiness, wealth, or any other unobserved characteristics of the individual. We find that following a bankruptcy flag removal, (1) credit scores and credit access increase, (2) the fraction of individuals who start businesses that hire employees (firm ownership) increases, (3) the transition rate into self-employment is higher, (4) new self-employed entrants earn more, (5) new self-employed entrants borrow more, and (6) firm owners borrow five times more than self-employed entrants.

We test several explanations for the rise in self-employment and borrowing after bankruptcy flag removal, including (1) credit constraints are relaxed after flag removal, (2) credit increases after flag removal because of consumption-smoothing motives, (3) borrowing to provide insurance against shocks, and (4) individuals borrow to signal that they are 'high quality'; thus, borrowing may not reflect immediate credit constraints.

Our evidence and tests provide support for credit constraints in explaining the use of credit for the self-employed as well as new firm owners in both the general population as well as for our bankrupt sample. In particular, we find strong positive correlations between self-employment income changes and borrowing, suggesting a limited role of credit as a consumption-smoothing device for the self-employed. Models with working capital financial constraints are consistent with this positive correlation. Moreover, we show that income volatility and credit limits are negatively correlated, limiting the role for precautionary credit accumulation or insurance as an explanation for the increased borrowing. Last, we see that contemporaneous borrowing is associated with lower subsequent credit access, inconsistent

with signaling higher future quality as an explanation for the increased borrowing we show.

Overall, our findings contribute to the understanding of how individuals use credit in entrepreneurship by linking direct measures of secured and unsecured consumer credit access (including limits and scores) to new firms and measuring the reliance on personal credit to each stage of entrepreneurship. By merging the credit reports with the ILBD, we add to the work of Robb and Robinson (2014) by providing the first set of summary statistics that describe the relation between unsecured and secured credit limits, credit scores, and entry into nonemployer and employer start-ups. Combining these new data with bankruptcy flag removals allows us to provide a unique characterization of the way credit access affects entry into nonemployer and employer businesses.

Our results can also be used to test the growing class of models that link consumer credit to self-employment dynamics. In terms of policy, our results are consistent with entrepreneurs in bankruptcy being credit constrained. On exit from bankruptcy, entrepreneurs use significant amounts of consumer credit when starting a business, suggesting, by revealed preference, that alternative sources of credit are more expensive and that they face credit constraints in alternative sources of finance as well. Our research suggests that making bankruptcy more prodebtor and shortening time in bankruptcy may have positive effects on entrepreneurship.

Our research also suggests that recent legislation that makes consumer bankruptcy more procreditor may also be contributing to the reduced rates of entrepreneurship shown in research by Decker et al. (2014). The data we have used in this project can be used to address these questions and a range of future questions related to entrepreneurial finance and subsequent outcomes.

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Figure 1: Self-employment and formal employment by unused revolving credit decile in population.

This graph plots the coefficients from unsecured revolving credit deciles in Table 2, columns (1), (2), (3), and (6).
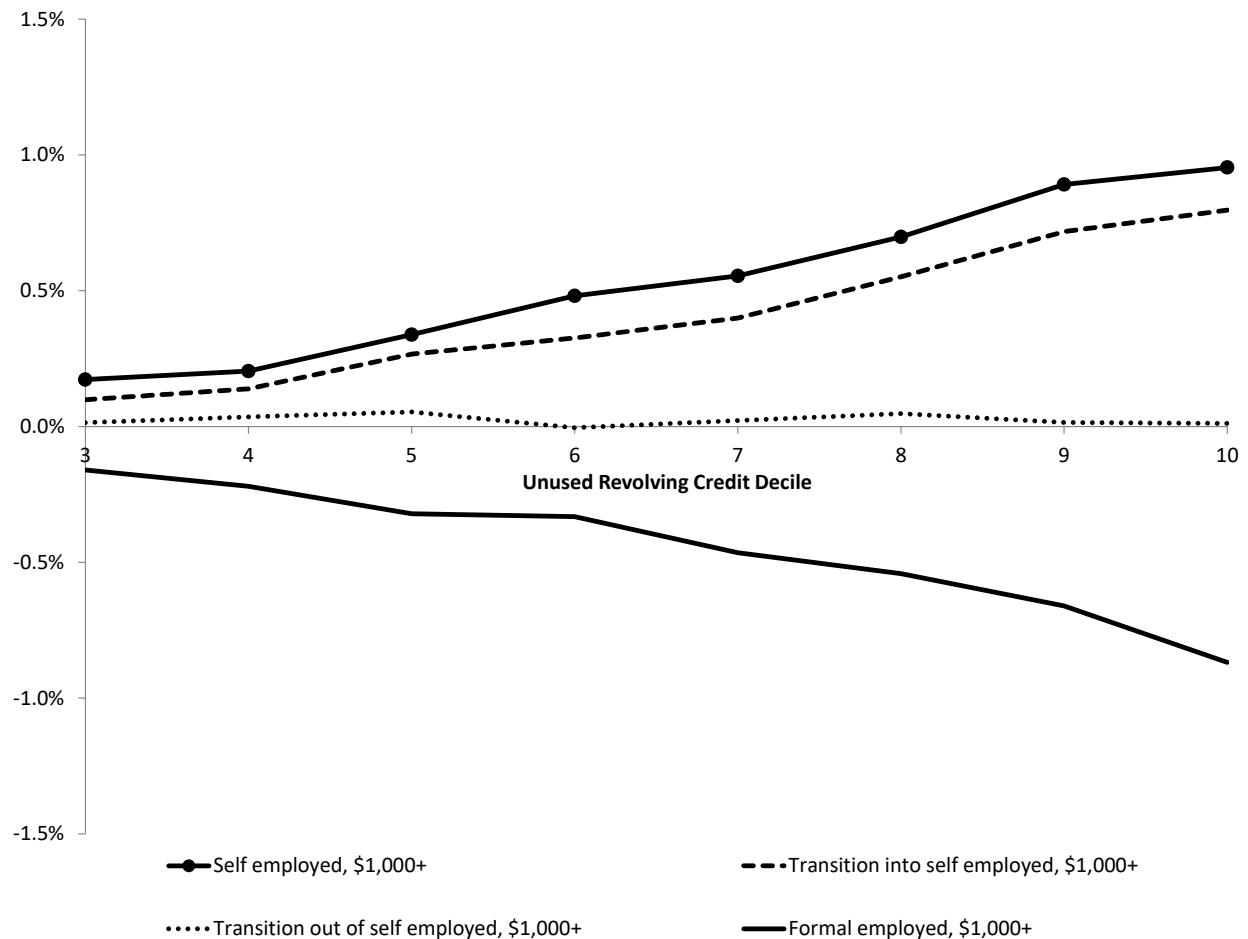
Figure 2: Firm ownership by unused revolving credit decile in population.

This graph plots the coefficients from unsecured revolving credit deciles in Table 2, columns (4) and (5). *"Firm ownership, 1+ (2+ yrs)"* are forward-looking indicator variables that take the value of one if an individual owns a firm in year t (and in the subsequent year $t+1$), and 0 otherwise.
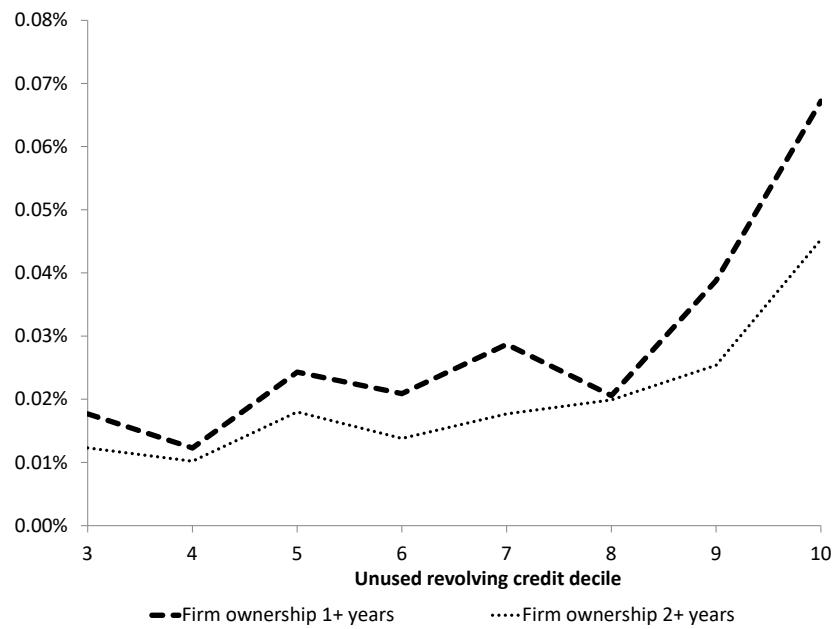
Figure 3: Histogram of years until bankruptcy flag removal.

This graph is based on the extended sample of TransUnion credit reports between 2001 and 2012.
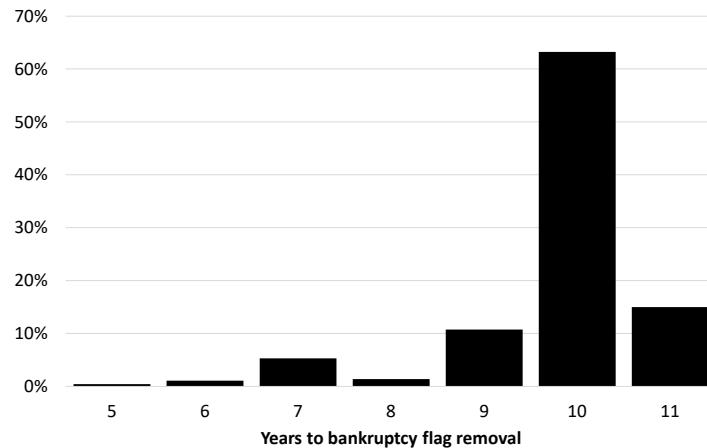
Figure 4: Credit score following bankruptcy flag removal.

This graph plots the coefficients from Table 5, column (1). Standard errors are clustered at the individual level.
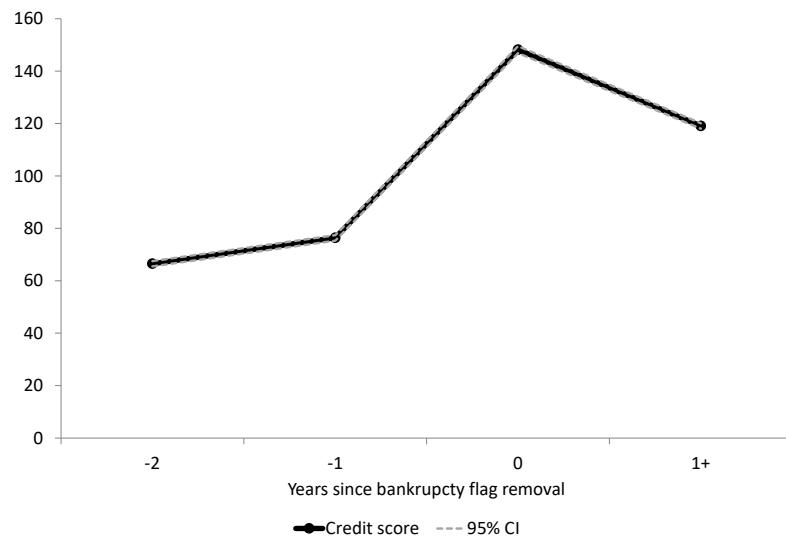
Figure 5: Firm ownership following bankruptcy flag removal.

This graph plots the coefficients from Table 5, column (6). Standard errors are clustered at the individual level. *"Firm ownership, 2+ yrs"* is a forward-looking indicator variable that takes the value of one if an individual owns a firm in year t and $t+1$, and 0 otherwise.
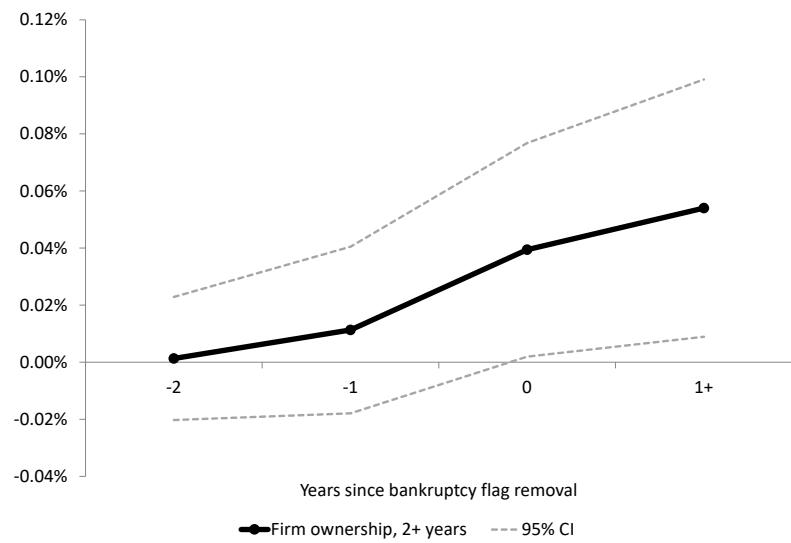
Figure 6: Schedule C net income among new self-employed entrants following bankruptcy flag removal.

This graph plots the coefficients from column (1), Table 7. (d) denotes a 0,1 binary indicator variable. Each point is the sum of coefficients on the flag removal indicator (e.g., *"2 years before removal (d)"*), interaction term (e.g., *"2 years before removal (d) x Trans. into self-employed, $1,000+ (d)"*) and the transition term (e.g., *Transition into self-employed, $1,000+ (d)"*). We compute standard errors using the delta method. The points on the plotted line can be interpreted as the differential gain in self-employed income from entering self-employment, relative to a nontransitioner in the omitted group, where the omitted group is those who are three or more years prior to flag removal. Standard errors are clustered at the individual level.

$991

-2   -1   0   1+

Years since bankruptcy flag removal

Real annual self-employed net income for those who transit into self employment, relative to omitted group

48
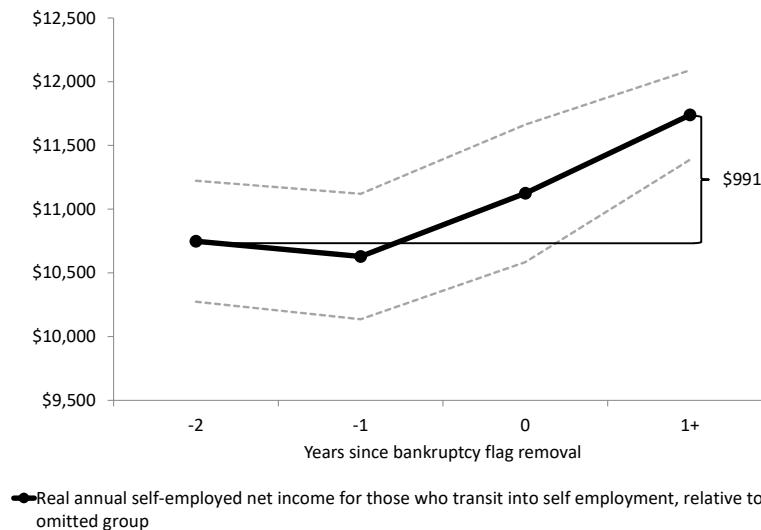
Figure 7: Total credit balance among firm owners following bankruptcy flag removal.

This graph plots the coefficients from column (5), Table 8. (d) denotes a 0,1 binary indicator variable. Each point is the sum of coefficients on the flag removal indicator (e.g., *2 years before removal (d)*), interaction term (e.g., *2 years before removal (d) x Firm ownership, 2+ Yrs. (d)*) and the ownership term (e.g., *Firm ownership, 2+ Yrs (d)*). We compute standard errors using the delta method. The points on the plotted line can be interpreted as the differential increase in borrowing from firm owners, relative to nonowners in the omitted group, where the omitted group is those who are 3+ years prior to flag removal. Standard errors are clustered at the individual level.

Real total credit balance for firm owners (2+ yrs.), relative to omitted group
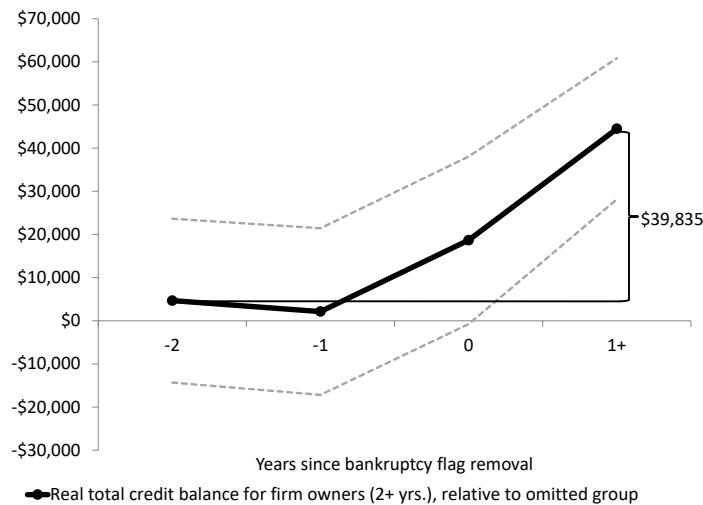
Table 1: Population summary statistics (source: 100% sample).

The table shows population summary statistics calculated by weighting observations to match yearly aggregate bankruptcy, delinquency, and foreclosure rates in sampled states. The symbol (d) denotes a 0,1 binary indicator variable. *"Firm ownership, 1+ (2+ yrs)"* are forward-looking variables that take the value of one if an individual owns a firm in year t (and the subsequent year $t+1$).

Variable	Mean	Variable	Mean
Lagged credit score	440.0	Transition into self-employed, $1,000+ (d)	3.6%
Real annual self-employed net income ($29,056 if self-employed)	3,080	Transition out of self-employed, $1,000+ (d)	3.0%
Real annual labor earnings ($40,139 if formal employed)	31,750	Firm ownership, 1+ yrs	0.4%
Age	40.8	Firm ownership, 2+ yrs	0.3%
Imputed years of education	13.1	Both self- and formal employed, $1,000+ (d)	5.8%
Total credit balance	95,570	nonemployed, $1,000+ (d)	16.1%
Self-employed, $1,000+ (d)	10.6%	Formal employed, $1,000+ (d)	79.1%
Observations (millions)	16.40		

Table 2: Population relationships between self-/formal employment outcomes and credit (source: 100% sample).

The table presents population summary statistics calculated by weighting observations to match yearly aggregate bankruptcy, delinquency, and foreclosure rates in sampled states. All variables are 0,1 binary indicator variables. *"Firm ownership, 1+ (2+ yrs)"* are forward-looking indicator variables that take the value of one if an individual owns a firm in year t (and in the subsequent year $t+1$) and zero otherwise. Controls include deciles of credit scores dummies, deciles of unused mortgage credit dummies, deciles of unused nonmortgage and nonrevolving credit dummies, lagged labor earnings and self-employed income, deciles of cumulative lagged earnings dummies and quadratics in age and tenure. Fixed effects include individual fixed effects and year dummies. Mean unused credit by decile: decile 1 $0; decile 2 $0; decile 3 $101; decile 4 $491; decile 5 $1,537; decile 6 $3,790; decile 7 $8,086; decile 8 $16,120; decile 9 $32,460; decile 10 $66,010. Standard errors are in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

	(1) self-employed $1,000+, t+1	(2) Transition into self-employed $1,000+, t+1	(3) Transition out of self-employed $1,000+, t+1	(4) Firm ownership 1+ yrs	(5) Firm ownership 2+ yrs, t and t+1	(6) Formal-employed $1,000+, t+1
Unused revolving credit decile 3, t	0.00173***	0.000989***	0.000148	0.000177***	0.000123***	-0.00160***
	(0.000343)	(0.000287)	(0.000253)	(5.34e-05)	(4.03e-05)	(0.000463)
Unused revolving credit decile 4, t	0.00204***	0.00139***	0.000358	0.000123**	0.000102**	-0.00220***
	(0.000328)	(0.000267)	(0.000237)	(5.76e-05)	(4.38e-05)	(0.000436)
Unused revolving credit decile 5, t	0.00338***	0.00267***	0.000538**	0.000243***	0.000180***	-0.00321***
	(0.000357)	(0.000288)	(0.000255)	(6.80e-05)	(5.36e-05)	(0.000466)
Unused revolving credit decile 6, t	0.00481***	0.00326***	-4.19e-05	0.000209***	0.000138**	-0.00332***
	(0.000388)	(0.000308)	(0.000274)	(7.61e-05)	(5.96e-05)	(0.000498)
Unused revolving credit decile 7, t	0.00555***	0.00400***	0.000223	0.000287***	0.000177***	-0.00465***
	(0.000422)	(0.000331)	(0.000296)	(8.58e-05)	(6.80e-05)	(0.000534)
Unused revolving credit decile 8, t	0.00698***	0.00552***	0.000474	0.000206**	0.000199**	-0.00542***
	(0.000471)	(0.000364)	(0.000327)	(0.000101)	(8.20e-05)	(0.000585)
Unused revolving credit decile 9, t	0.00891***	0.00718***	0.000150	0.000388***	0.000254**	-0.00661***
	(0.000538)	(0.000410)	(0.000372)	(0.000123)	(0.000101)	(0.000657)
Unused revolving credit decile 10, t	0.00954***	0.00797***	0.000115	0.000672***	0.000453***	-0.00868***
	(0.000644)	(0.000488)	(0.000444)	(0.000162)	(0.000135)	(0.000762)
fixed effects	Y	Y	Y	Y	Y	Y
Controls	Y	Y	Y	Y	Y	Y
R-squared	0.019	0.059	0.073	0.002	0.001	0.170
Individuals (millions)	3.06	3.06	3.06	3.06	3.06	3.06
Total person-year obs. (millions)	16.40	16.40	16.40	16.40	16.40	16.40

The table shows population elasticities to unused credit. All dependent variables are 0,1 binary indicator variables. *"Firm ownership, 2+ yrs"* is a forward-looking indicator variable that takes the value of one if an individual owns a firm in year t and subsequent year $t+1$ and zero otherwise. Controls include lagged labor earnings and self-employed income, deciles of cumulative lagged earnings dummies, and quadratics in age and tenure. Fixed effects include individual fixed effects and year dummies. Observations are weighted to match yearly aggregate bankruptcy, delinquency, and foreclosure rates in sampled states. Standard errors are clustered at the individual level. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

	(1) Firm ownership, 2+ yrs	(2) Firm ownership, 2+ yrs	(3) Self-employed, $1,000+	(4) Self-employed, $1,000+	(5) Formal employed, $1,000+	(6) Formal employed, $1,000+
Log unused revolving credit, t-1	2.15e-05***		0.000659***		-0.000581***	
	(5.93e-06)		(3.85e-05)		(5.03e-05)	
Log unused total credit, t-1		1.49e-05***		0.000643***		-0.000671***
		(5.10e-06)		(3.72e-05)		(5.29e-05)
Fixed effects	Y	Y	Y	Y	Y	Y
Controls	Y	Y	Y	Y	Y	Y
R-squared	0.001	0.001	0.018	0.018	0.170	0.170
Round N	1.640e+07	1.640e+07	1.640e+07	1.640e+07	1.640e+07	1.640e+07
N indiv	3.060e+06	3.060e+06	3.060e+06	3.060e+06	3.060e+06	3.060e+06

Table 4: Summary statistics before and after flag removal.

The table presents summary statistics for bankrupt individuals. Column (1) computes averages using the individuals in our sample who are one year before bankruptcy flag removal. Column (2) computes averages using the individuals in our sample who are one year after bankruptcy flag removal. Column (3) is the difference in means between Columns (1) and (2), and Column (4) indicates if that difference in means is significant at the 10% level. The symbol (d) denotes a 0,1 binary indicator variable. *"Formal-employed, $1,000"* is an indicator that equals one when an individual earned at least $1,000 in a UI-insured job covered by the LEHD. *"Self-employed, $1,000+ (d)"* is an indicator that equals one when an individual earned at least $1,000 in net income on their 1040 Schedule C. *"Firm ownership, 1+ (2+ yrs)"* are forward-looking indicator variables that take the value of one if an individual owns a firm in year t (and in the subsequent year $t + 1$) and zero otherwise. For all other definitions, see Section 3.1.

	Sample averages			
	(1)	(2)	(3)	(4)
	1 yr. before flag drop	1 yr. after flag drop	Diff. ((1)-(2))	Sig. diff.
(A) Employment stocks				
Self-employed, $1,000+ (d)	9.00%	9.60%	0.60%	*
Formal employed, $1,000+ (d)	78.70%	78.60%	-0.10%	
Both SE and formal employed, $1,000+ (d)	6.10%	6.40%	0.30%	*
Nonemployed, $1,000+ (d)	18.40%	18.20%	-0.20%	
Firm ownership, 1+ yrs. (d)	0.40%	0.40%	0.00%	
Firm ownership, 2+ yrs. (d)	0.20%	0.30%	0.10%	*
(B) Employment flows				
Transition into self-employed, $1,000+ (d)	3.10%	3.40%	0.30%	*
Transition out of self-employed, $1,000+ (d)	2.80%	2.80%	0.00%	
Transition out of self-empl. next year, $1,000+ (d)	2.90%	3.10%	0.20%	*
(C) Earnings				
Real annual self-employed net income ($23,800 without $0s)	$2,140	$2,300	$161	*
Real annual labor earnings ($41,500 without $0s)	$32,683	$33,005	$323	*
Real annual total income (SE and non-SE)	$34,822	$35,305	$483	*
(D) Credit variables				
Credit score	288.0	351.8	63.8	*
Real bank card balance	$3,441	$4,467	$1,027	*
Real revolving balance	$7,601	$10,475	$2,874	*
Real mortgage balance	$92,417	$104,000	$11,583	*
Real HELOC balance	$3,355	$5,181	$1,825	*
Observations	170,000	110,000		

Table 5: Credit scores and entrepreneurial outcomes after bankruptcy flag removal.

The table examines the relation between entrepreneurial outcomes and the TransUnion bankruptcy score. *"Transition into self-employed, \$1,000+ (d)"* is an indicator variable that takes the value of one when an individual earns less than \$1,000 of self-employed earnings this year and more than \$1,000 of self-employed earnings this year. *"Firm ownership, 1+ (2+ yrs)"* are forward-looking indicator variables that take the value of one if an individual owns a firm in year t (and in the subsequent year $t + 1$) and zero otherwise. For more details on the firm ownership measures, see Section 3.1. Age and tenure controls include quadratics in age and tenure. The symbol (d) denotes a 0,1 binary indicator variable. The row "Sig diff 1+yr & -2yr at 10%" is an indicator that the coefficients are different on the terms *"1+ years after removal (d)"* and *"2 years before removal (d)"* at the 10% level. "Sig diff 0yr & -2yr at 10%" is a row indicating that the coefficients are different on the terms 'Year of removal (d)' and '2 years before removal (d)' at the 10% level. Standard errors are in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

	(1) Credit score	(2) Credit score	(3) Self-employed, \$1,000+ (d)	(4) Transition into self-employed, \$1,000+ (d)	(5) Firm ownership, 1+ yrs (d)	(6) Firm ownership, 2+ yrs (d)
2 years before removal (d)	66.52***	19.70***	0.000600	0.000527	-0.000126	1.30e-05
	(0.513)	(0.444)	(0.000701)	(0.000576)	(0.000172)	(0.000110)
1 year before removal (d)	76.39***	13.26***	-0.000384	2.64e-05	-0.000110	0.000113
	(0.528)	(0.592)	(0.000903)	(0.000635)	(0.000215)	(0.000149)
Year of removal (d)	148.2***	68.70***	0.000950	0.00161**	0.000179	0.000394**
	(0.675)	(0.798)	(0.00112)	(0.000740)	(0.000259)	(0.000191)
1+ years after removal (d)	119.1***	7.046***	0.00108	0.000649	0.000297	0.000540**
	(0.524)	(0.939)	(0.00137)	(0.000891)	(0.000334)	(0.000230)
Individual fixed effects	N	Y	Y	Y	Y	Y
Year fixed effects	N	Y	Y	Y	Y	Y
Age and tenure controls	N	Y	Y	Y	Y	Y
R-squared	0.116	0.134	0.003	0.000	0.001	0.000
Round N	1.500e+06	1.500e+06	1.500e+06	1.500e+06	1.500e+06	1.500e+06
N indiv	220,000	220,000	220,000	220,000	220,000	220,000
Sig diff 1+yr & -2yr at 10%	Y	Y	N	N	N	Y
Sig diff yYr & -2yr at 10%	Y	Y	N	N	N	Y

Table 6: Summary statistics among self-employed and LBD business owners.

The table presents summary statistics for self-employed individuals and LBD business owners. *"Firm ownership, 2+ yrs"* is a forward-looking indicator variable that takes the value of one if an individual owns a firm in year t and in the subsequent year $t+1$ and zero otherwise. Column (1) computes averages using the individuals in our sample who are one year before bankruptcy flag removal. Column (2) computes averages using the individuals in our sample who are one year after bankruptcy flag removal. Column (3) is the difference in means between Columns (1) and (2), and Column (4) indicates if that difference in means is significant at the 10% level.

	1 year before	1 year After	Diff.	Sig diff?
(A) Self-employed				
Total balance, Transition into self-employed $1,000+	$149,300	$169,900	$20,600	*
Revolving balance, Transition into self-employed $1,000+	$9,361	$12,202	$2,841	*
Home equity, Transition into self-employed $1,000+	$4,669	$6,686	$2,017	*
(B) Firm owners				
Total balance, Firm ownership 2+ yrs.	$205,800	$239,000	$33,200	*
Revolving balance, Firm ownership 2+ yrs.	$15,344	$19,461	$4,117	*
Home equity, Firm ownership 2+ yrs.	$10,558	$12,884	$2,326	*

Table 7: Transitions into self-employment and earnings, borrowing, and turnover.

The table examines the relation between individual outcomes, credit, and bankruptcy flag removal interacted individual transitions into self-employment. (d) denotes a 0,1 binary indicator variable. Age and tenure controls include quadratics in age and tenure. The row titled "Combined coeff diff 1+ yrs & -2 yrs" calculates the difference in the summed coefficients for those who transition one year after flag removal (sum the coefficients on *"1+ years after removal (d)"* + *"1+ yrs. after removal (d) x Trans into self-empl, \$1,000+ (d)"* =3376-158.2) minus the summed coefficients for those who transition two years before flag removal (=2274-47.61). Taking the difference yields \$991 (=(3376-158.2)-(2274-47.61)), which is the additional amount earned by those who transition into self-employment one year after flag removal, relative to two years before. Fixed effects include individual fixed effects and year dummies. The row titled "Combined coeff diff Sig at 10%" is an indicator if that difference is significant at the 10% level. Standard errors are in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

	(1) Real self-employed earnings	(2) Real HELOC balance	(3) Real total balance	(4) Transition out of self-employment next yr., \$1,000+ (d)
2 yrs. before removal (d) x Trans into self-employed, \$1,000+ (d)	2,274***	277.8	6,422***	-0.00672
	(268.1)	(270.1)	(1,955)	(0.00830)
1 yr. before removal (d) x Trans into self-employed, \$1,000+ (d)	2,185***	575.8*	11,815***	0.00761
	(274.8)	(298.1)	(2,011)	(0.00850)
Yr. of removal (d) x Trans into self-employed, \$1,000+ (d)	2,704***	640.2*	8,511***	-0.0128
	(297.7)	(331.9)	(2,162)	(0.00889)
1+ yrs. after removal (d) x Trans into self-employed, \$1,000+ (d)	3,376***	1,253***	16,195***	0.00437
	(212.7)	(267.9)	(1,594)	(0.00649)
Transition into self-employed, \$1,000+ (d)	8,522***	-486.9***	-6,483***	0.380***
	(123.4)	(88.90)	(790.1)	(0.00392)
2 years before removal (d)	-47.61**	759.8***	8,809***	0.000481
	(20.08)	(47.46)	(350.1)	(0.000456)
1 year before removal (d)	-78.49***	1,377***	13,038***	0.000153
	(27.70)	(66.35)	(475.3)	(0.000509)
Year of removal (d)	-101.3***	1,978***	15,800***	0.000583
	(34.61)	(84.78)	(598.6)	(0.000602)
1+ years after removal (d)	-158.2***	3,551***	14,373***	0.00117
	(43.52)	(98.35)	(728.1)	(0.000754)
Individual fixed effects	Y	Y	Y	Y
Year fixed effects	Y	Y	Y	Y
Age and tenure controls	Y	Y	Y	Y
R-squared	0.077	0.026	0.105	0.158
Round N	1.500e+06	1.500e+06	1.500e+06	1.500e+06
No. person-yr. obs.	1.500e+06	1.500e+06	1.500e+06	1.500e+06
No. indiv. obs.	220,000	220,000	220,000	220,000
Combined coeff diff 1+ yrs & -2 yrs	991	3,766	15,337	1.18%
Combined coeff diff sig at 10%	Y	Y	Y	N

Table 8: Firm ownership and borrowing.

The table examines the relation between firm ownership and borrowing. *"Firm ownership, 2+ yrs"* is a forward-looking variable that takes the value of one if an individual owns a firm in year t and $t+1$ and zero otherwise. (d) denotes a 0,1 binary indicator variable. Age and tenure controls include quadratics in age and tenure. fixed effects include individual fixed effects and year dummies. "Combined coeff diff 1+ yrs & -2 yrs" compares the overall effect of firm ownership one or more years after flag removal to the overall effect of firm ownership two years before flag removal. Standard errors are in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

	(1) Real bank card balance	(2) Real revolving balance	(3) Real mortgage Balance	(4) Real HELOC balance	(5) Real total balance
2 yrs. before removal (d) x Firm ownership, 2+ yrs. (d)	175.4	3,682***	7,493	1,317	13,318
	(300.3)	(1,269)	(9,526)	(1,408)	(10,209)
1 yr. before removal (d) x Firm ownership, 2+ yrs. (d)	449.7	4,326***	4,209	4,520**	6,382
	(334.4)	(1,646)	(10,123)	(2,149)	(10,937)
Yr. of removal (d) x Firm ownership, 2+ yrs. (d)	902.0**	4,843***	18,303*	5,953**	20,255*
	(383.7)	(1,742)	(10,899)	(2,437)	(11,717)
1+ yrs. After removal (d) x Firm ownership, 2+ yrs. (d)	1,067***	8,645***	37,997***	7,716***	47,332***
	(406.2)	(1,487)	(9,793)	(2,030)	(10,710)
Firm ownership, 2+ yrs. (d)	-195.6	-3,055***	-16,342**	-3,674***	-17,637**
	(172.8)	(808.7)	(6,410)	(989.6)	(6,862)
2 years before removal (d)	205.0***	1,116***	6,189***	766.9***	8,991***
	(12.96)	(40.32)	(326.3)	(47.19)	(347.2)
1 year before removal (d)	338.4***	1,812***	9,273***	1,388***	13,406***
	(18.10)	(56.18)	(441.9)	(66.23)	(474.3)
Year of removal (d)	586.0***	2,745***	10,597***	1,988***	16,045***
	(23.73)	(72.00)	(555.6)	(84.50)	(598.0)
1+ years after removal (d)	904.1***	4,291***	8,462***	3,576***	14,812***
	(28.44)	(83.73)	(675.5)	(98.32)	(728.0)
Individual fixed effects	Y	Y	Y	Y	Y
Year fixed effects	Y	Y	Y	Y	Y
Age and tenure controls	Y	Y	Y	Y	Y
R-squared	0.027	0.050	0.092	0.026	0.105
No. person-yr obs.	1.500e+06	1.500e+06	1.500e+06	1.500e+06	1.500e+06
No. of indiv.	220,000	220,000	220,000	220,000	220,000
Combined coeff diff 1+ yrs & -2 yrs	1,591	8,138	32,777	9,208	39,835
Combined coeff diff sig at 10%	Y	Y	Y	Y	Y

Table 9: Baseline results, narrow window around bankruptcy.

This table examines the relation between bankruptcy flag removal and credit and individual employment and firm ownership. *"Firm ownership, 1+ (2+ yrs)"* are forward-looking indicator variables that take the value of one if an individual owns a firm in year t (and in the subsequent year $t+1$) and zero otherwise. (d) denotes a 0,1 binary indicator variable. The sample includes only those in our baseline sample within +/- one year of bankruptcy flag removal. The interpretation is similar to Table 5, except the omitted group is one year prior to flag removal. Standard errors are clustered at the individual level. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

	(1) Credit score	(2) Self-employed, $1,000+ (d)	(3) Transition into self-employed, $1,000+ (d)	(4) Firm ownership, 1+ yrs (d)	(5) Firm ownership, 2+ yrs (d)
Year of removal (d)	78.86***	0.00519***	0.00312***	-0.000221	0.000548***
	(0.539)	(0.000713)	(0.000709)	(0.000181)	(0.000105)
1 year after removal (d)	70.07***	0.0101***	0.00342***	-0.000586**	0.000923***
	(0.787)	(0.00100)	(0.000859)	(0.000281)	(0.000154)
Individual fixed effects	Y	Y	Y	Y	Y
Year fixed effects	Y	Y	Y	Y	Y
Age and tenure Controls	Y	Y	Y	Y	Y
R-squared	0.103	0.001	0.000	0.001	0.000
No. person-yr. obs.	410,000	410,000	410,000	410,000	410,000
No. indiv. obs.	170,000	170,000	170,000	170,000	170,000

Table 10: Firm ownership and the interaction between flag removal and age, education, and predetermined credit variables.

The table examines the relation between firm ownership and the interaction of bankrupt flag removal with individual characteristics. All dependent variables are indicator 0,1 binary variables. *"Firm ownership, 2+ yrs"* is a forward-looking indicator variable that takes the value of one if an individual owns a firm in year t (and in the subsequent year $t+1$) and zero otherwise. (d) denotes a 0,1 binary indicator variable. All interaction variables for age, education, unused credit, and scores are measured in the year prior to flag removal. The cutoff for median unused revolving credit is \$1,000 in 2008 dollars rounded to the nearest significant digit. The cutoff for median unused total credit is \$8,000 rounded to the nearest significant digit. The cutoff for median credit score is 200 rounded to the nearest significant digit. The sample includes only those in our baseline sample within +/- one year of bankruptcy flag removal. See notes to Table 5 for controls and variable definitions. Standard errors are clustered at the individual level. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

	(1) Firm ownership, 2+ yrs	(2) Firm ownership, 2+ yrs	(3) Firm ownership, 2+ yrs	(4) Firm ownership, 2+ yrs	(5) Firm ownership, 2+ yrs
Year of flag removal	0.000485***	0.000608***	0.000376***	0.000347***	0.000341***
	(0.000127)	(0.000133)	(0.000108)	(0.000115)	(0.000122)
1 year after flag removal (d)	0.000870***	0.000962***	0.000737***	0.000736***	0.000660***
	(0.000184)	(0.000183)	(0.000172)	(0.000172)	(0.000184)
Year of flag removal × age≥ 40 (d)	0.000132				
	(0.000210)				
1 year after flag removal × age≥ 40 (d)	0.000119				
	(0.000296)				
Year of flag removal × College educ (d)		-0.000176			
		(0.000210)			
1 year after flag removal × College educ (d)		-0.000120			
		(0.000296)			
Year of flag removal × Unused revolv.≥ p50 (d)			0.000350*		
			(0.000208)		
1 year after flag removal (d) × Unused revolv.≥ p50 (d)			0.000382		
			(0.000287)		
Year of flag removal × Unused total credit ≥ p50 (d)				0.000409**	
				(0.000207)	
1 year after flag removal × Unused total credit ≥ p50 (d)				0.000385	
				(0.000287)	
Year of flag removal × score ≥ p50 (d)					0.000419**
					(0.000207)
1 year after flag removal × score ≥ p50 (d)					0.000540*
					(0.000284)
Individual fixed effects	Y	Y	Y	Y	Y
Year fixed effects	Y	Y	Y	Y	Y
Age and tenure controls	Y	Y	Y	Y	Y
R-squared	0.000	0.000	0.000	0.000	0.000
No. person-yr. obs	410,000	410,000	410,000	410,000	410,000
No. indiv. obs.	170,000	170,000	170,000	170,000	170,000

Table 11: Credit access and the interaction between flag removal and age, education, and predetermined credit variables.

The table examines the relation between borrowing and bankruptcy flag removal interacted with individual characteristics. (d) denotes an indicator 0,1 binary variable. The cutoff for median unused revolving credit is \$1,000 in 2008 dollars rounded to the nearest significant digit. The cutoff for median unused total credit is \$8,000 rounded to the nearest significant digit. The cutoff for median credit score is 200 rounded to the nearest significant digit. All interaction variables for age, education, unused credit, and scores are measured in the year prior to flag removal. The sample includes only those in our baseline sample within $+/-$ one year of bankruptcy flag removal. Standard errors are clustered at the individual level. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

	(1) Revolving credit limit	(2) Revolving credit limit	(3) Revolving credit limit	(4) Real revolving balances	(5) Real revolving balances	(6) Real revolving balances
Year of flag removal (d)	1,060***	1,612***	386.0***	302.4***	581.7***	-195.8***
	(41.14)	(47.06)	(45.13)	(29.46)	(33.76)	(33.55)
1 year after flag removal (d)	2,607***	3,661***	1,408***	1,210***	1,683***	225.4***
	(76.35)	(84.03)	(81.59)	(52.14)	(57.20)	(56.38)
Year of flag removal (d) × Unused revolv.≥ p50 (d)	4,155***			2,514***		
	(108.6)			(81.07)		
1 year after flag removal (d) × Unused revolv.≥ p50 (d)	8,333***			4,390***		
	(165.0)			(117.2)		
Year of flag removal (d) × Unused total credit ≥ p50 (d)		3,016***			1,937***	
		(108.4)			(80.81)	
1 year after flag removal (d) × Unused total credit ≥ p50 (d)		6,144***			3,407***	
		(164.4)			(116.5)	
Year of flag removal (d) × Score ≥ p50 (d)			5,514***			3,521***
			(106.5)			(79.48)
1 year after flag removal (d) × Score ≥ p50 (d)			10,750***			6,388***
			(161.4)			(114.4)
R-squared	0.060	0.054	0.070	0.029	0.026	0.038
No. person-yr obs.	410,000	410,000	410,000	410,000	410,000	410,000
No. indiv. obs.	170,000	170,000	170,000	170,000	170,000	170,000

Table 12: Robustness for heterogeneous effects of flag removal.

This table examines heterogeneous effects of flag removal adding controls for self-employed earnings 7 to 12 years before flag removal and a full set of interactions between flag removal dummies and prior self-employed earnings. All dependent variables are indicator 0,1 binary variables. *"Firm ownership, 2+ yrs"* is a forward-looking indicator variable that takes the value of one if an individual owns a firm in year t (and in the subsequent year $t+1$) and zero otherwise. The sample includes only those in our baseline sample within +/- one year of bankruptcy flag removal. All interaction variables for age, education, unused credit, and scores are measured in the year prior to flag removal. Columns (1) through (5) include simultaneous controls for real self-employed earnings 7, 8, 9, 10, 11, and 12 years prior to flag removal. Columns (6) through (10) include a full set of interactions between flag removal dummies and real self-employed earnings 7, 8, 9, 10, 11, and 12 years prior to flag removal. (d) denotes a 0,1 binary indicator variable. Standard errors are clustered at the individual level. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

	(1) Firm ownership, 2+ yrs	(2) Firm ownership, 2+ yrs	(3) Firm ownership, 2+ yrs	(4) Firm ownership, 2+ yrs	(5) Firm ownership, 2+ yrs	(6) Firm ownership, 2+ yrs	(7) Firm ownership, 2+ yrs	(8) Firm ownership, 2+ yrs	(9) Firm ownership, 2+ yrs	(10) Firm ownership, 2+ yrs
Year of flag removal (d)	0.000473***	0.000590***	0.000365***	0.000333***	0.000327***	0.000447***	0.000573***	0.000351***	0.000329***	0.000329***
	(0.000127)	(0.000131)	(0.000107)	(0.000114)	(0.000121)	(0.000125)	(0.000130)	(0.000105)	(0.000110)	(0.000118)
1 year after flag removal (d)	0.000847***	0.000930***	0.000718***	0.000717***	0.000636***	0.000795***	0.000891***	0.000686***	0.000702***	0.000644***
	(0.000181)	(0.000178)	(0.000172)	(0.000170)	(0.000183)	(0.000173)	(0.000175)	(0.000166)	(0.000163)	(0.000176)
Year of flag removal (d) × age≥ 40 (d)	0.000119					0.000164				
	(0.000210)					(0.000206)				
1 year after flag removal (d) × age≥ 40 (d)	0.000101					0.000187				
	(0.000295)					(0.000281)				
Year of flag removal (d) × College educ (d)		-0.000177					-0.000125			
		(0.000211)					(0.000207)			
1 year after flag removal (d) × College educ (d)		-0.000119					-9.77e-06			
		(0.000296)					(0.000281)			
Year of flag removal (d) × Unused revolv.≥ p50 (d)			0.000338					0.000366*		
			(0.000208)					(0.000204)		
1 year after flag removal (d) × Unused revolv.≥ p50 (d)			0.000358					0.000413		
			(0.000284)					(0.000273)		
Year of flag removal (d) × Unused total credit ≥ p50 (d)				0.000400*					0.000411**	
				(0.000208)					(0.000203)	
1 year after flag removal (d) × Unused total credit ≥ p50 (d)				0.000361					0.000384	
				(0.000286)					(0.000272)	
Year of flag removal (d) × Score ≥ p50 (d)					0.000413**					0.000410**
					(0.000206)					(0.000202)
1 year after flag removal (d) × Score ≥ p50 (d)					0.000525*					0.000508*
					(0.000282)					(0.000269)
Individual FE, year FE, age and tenure Controls	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Controls for prior entrepreneur earnings, 7 to 12 years ago	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Interact prior entrepreneur earnings with prior credit access	N	N	N	N	N	Y	Y	Y	Y	Y
R-squared	0.000	0.000	0.000	0.000	0.000	0.001	0.001	0.001	0.001	0.001
No. person-yr. obs	410,000	410,000	410,000	410,000	410,000	410,000	410,000	410,000	410,000	410,000
No. indiv. obs.	170,0000	170,0000	170,0000	170,0000	170,0000	170,0000	170,0000	170,0000	170,0000	170,0000

Table 13: Correlation of self-employed income growth and revolving balance growth.

The table examines the correlation between self-employment and revolving credit growth. Columns (1) and (2) include all individual person-year observations that are within +/- one year of a self-employment spell in the population sample. Columns (1) and (2) are weighted to match yearly aggregate bankruptcy, delinquency, and foreclosure rates in sampled states. Columns (3) and (4) include person-year observations that are within +/- one year of a self-employment spell in the bankrupt sample. Fixed effects include individual and time fixed effects. Controls include lagged labor and self-employment earnings as well as a quadratic in age. Standard errors are clustered at the individual level. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

	(1) Δ Log revolving balance t minus t-1	(2) Log revolving balance t	(3) Δ Log revolving balance t minus t-1	(4) Log revolving balance t
Δ Log income t minus t-1	0.00248***	0.00479***	0.00365**	0.00712***
	(0.000584)	(0.000414)	(0.00160)	(0.00117)
Sample	Population	Population	Bankrupt	Bankrupt
Fixed effects	Y	Y	Y	Y
Controls	Y	Y	Y	Y
R-squared	0.000	0.001	0.002	0.016
Observations	1,840,000	1,840,000	190,000	190,000
Individuals	670,000	670,000	50,000	50,000

Table 14: Correlation of self-employed income with credit borrowing and utilization.

The table examines the correlation of the coefficient of variation of self-employed income with measures of credit borrowing and utilization. Columns (1) to (3) include all individual person-year observations that are within +/- one year of a self-employment spell in the population sample. Columns (1) to (3) are weighted to match yearly aggregate bankruptcy, delinquency, and foreclosure rates in sampled states. Columns (4) to (6) include individual person-year observations that are within +/- one year of a self-employment spell in the bankrupt sample. Fixed effects include individual and time fixed effects. Controls include lagged labor and self-employment earnings as well as a quadratic in age. Standard errors are clustered at the individual level. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

	(1) Log revolving balance t	(2) Log revolving limit t	(3) Log unused revolving credit t	(4) Log revolving balance t	(5) Log revolving limit t	(6) Log unused revolving credit t
Log of coefficient of variation of self-employed income	-0.709***	-0.882***	-0.944***	-0.757***	-0.812***	-0.843***
	(0.0155)	(0.0156)	(0.0165)	(0.0477)	(0.0488)	(0.0497)
Sample	Population	Population	Population	Bankrupt	Bankrupt	Bankrupt
Fixed effects	N	N	N	N	N	N
Controls	Y	Y	Y	Y	Y	Y
R-squared	0.082	0.114	0.118	0.058	0.070	0.075
Observations	1,840,000	1,840,000	1,840,000	190,000	190,000	190,000
Individuals	670,000	670,000	670,000	50,000	50,000	50,000

Table 15: Correlation of revolving credit balance growth with future credit.

This table examines the correlation of revolving credit balance growth with future credit score and credit limit growth. Columns (1) and (2) include all individual person-year observations that are within +/- one year of a self-employment spell in the population sample. Columns (1) to (2) are weighted to match yearly aggregate bankruptcy, delinquency, and foreclosure rates in sampled states. Columns (3) and (4) include individual person-year observations that are within +/- one year of a self-employment spell in the bankrupt sample. Fixed effects include individual and time fixed effects. Controls include lagged labor and self-employment earnings as well as a quadratic in age. Standard errors are clustered at the individual level. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

	(1) Δ Log score t+1 minus t	(2) Δ Log revolving limit t+1 minus t	(3) Δ Log score t+1 minus t	(4) Δ Log revolving limit t+1 minus t
Δ Log revolving balance t minus $t-1$	-0.00204***	-0.155***	-0.00862***	-0.174***
	(0.000174)	(0.00103)	(0.000699)	(0.00269)
Sample	Population	Population	Bankrupt	Bankrupt
Fixed effects	Y	Y	Y	Y
Controls	Y	Y	Y	Y
R-squared	0.002	0.047	0.012	0.050
Observations	1,840,000	1,840,000	190,000	190,000
Individuals	670,000	670,000	50,000	50,000

Table 16: Representativeness of the bankrupt sample.

This table examines if the bankrupt sample is similar to the overall population. The sample consists of the 100% sample and the bankrupt sample. All dependent variables are binary 0,1 indicator variables. *"Firm ownership, 2+ yrs"* is a forward-looking indicator variable that takes the value of one if an individual owns a firm in year t (and in the subsequent year $t+1$) and zero otherwise. Controls include lagged labor earnings and self-employed income, deciles of cumulative lagged earnings dummies, quadratics in age and tenure. fixed effects include individual fixed effects and year dummies. *"Log unused revolving credit, $t-1$ × Bankrupt sample (d)"* is the interaction between log unused revolving credit at date $t-1$ and a flag for whether or not the individual is in our bankrupt sample (this flag is time invariant). Observations are weighted to match yearly aggregate bankruptcy, delinquency, and foreclosure rates in sampled states. Standard errors are clustered at the individual level. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

	(1) Firm ownership, 2+ yrs	(2) Self-employed, $1,000+	(3) Formal employed, $1,000+
Log unused revolving credit, $t-1$	2.15e-05***	0.000688***	-0.000568***
	(6.21e-06)	(4.04e-05)	(5.26e-05)
Log unused revolving credit, $t-1$ × Bankrupt sample (d)	-5.24e-07	-0.000344***	-0.000154
	(2.00e-05)	(0.000126)	(0.000167)
Fixed effects	Y	Y	Y
Controls	Y	Y	Y
R-squared	0.001	0.018	0.170
Round N	1.640e+07	1.640e+07	1.640e+07
N indiv	3.060e+06	3.060e+06	3.060e+06