# The Impact of Consumer Credit Access on Employment, Earnings and Entrepreneurship*

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**Abstract**

How does consumer credit access impact job flows, earnings, and entrepreneurship? To answer this question, we build a new administrative dataset which links individual employment and entrepreneur tax records to TransUnion credit reports, and then we exploit the discrete increase in consumer credit access following bankruptcy flag removal. After flag removal, individuals flow into self-employment and are more likely to become an employer business in the LBD. New entrants earn more, borrow significantly using unsecured and secured consumer credit, and are more likely to become an employer business. In addition, non-employed or self-employed individuals are more likely to find unemployment-insured "formal" jobs with larger firms after flag removal. These results suggest that consumer credit access matters for each stage of entrepreneurship and credit-checks may be limiting formal sector employment opportunities. Calibrating a directed search model to our empirical estimates, we show that firms believe bankrupt workers are 3.8% less productive than non-bankrupt workers, on average.

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While much is known theoretically and empirically about the interaction between credit constraints and startup rates (*inter alia* Cooley and Quadrini [2001], Hurst and Lusardi [2004], Buera et al. [2009], Hurst and Pugsley [2011], Greenstone et al. [2014], Buera et al. [2015]), little is known about the way access to consumer credit affects individual job flows, startup decisions, or subsequent earnings.[1] How does consumer credit access affect the transition rate into and out of employment and self-employment? What are the consequences of these transitions for labor earnings and business income?

The central issue with determining the causal impact of personal credit on employment outcomes is that personal credit is highly correlated with an individual's quality as well as their wealth and access to funds. Thus, it is hard to separate out wealth effects and fundamental ability 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 credit worthiness.

We use a standard difference-in-difference approach in which we compare cohorts of bankrupt individuals whose flags are removed to adjacent cohorts of bankrupt individuals whose flags are not yet removed. Similar to the medical literature, our approach isolates a group of 'sick' individuals (i.e. the bankrupt) and then compares outcomes of people who are treated (have their bankruptcy flag removed) to those who aren't yet treated (their bankruptcy flag stays on their record). We apply this methodology to a new dataset which merges millions of credit histories and self-employment tax records to administrative US Census employment records. We show that consistent with prior studies such as Musto [2004], access to credit increases dramatically among the subgroup of individuals who have their bankruptcy flags removed. We show that these increases in credit access affect an individual's employment outcomes. We then verify our results in the Survey of Consumer Finances (SCF), a public cross-sectional survey.[2]

[1] While the topic of startups and credit has received much attention following the housing bust (*inter alia* Fairlie and Krashinsky [2012], Chatterji and Seamans [2012], Schmalz et al. [2013], Adelino et al. [2013], Jensen et al. [2014], Kerr et al. [2014], Mehrotra and Sergeyev [2015]), only recently have studies emerged which asses the impact of consumer credit on unemployment theoretically, e.g. Athreya and Simpson [2006], Karahan and Rhee [2011], Midrigan and Philippon [2011], Chen [2012], Carroll et al. [2012] , Chen et al. [2013], Herkenhoff [2013], Schott [2013], Kehoe et al. [2014] as well as empirically, e.g. Bethune [2015], Bos et al. [2015], and Herkenhoff et al. [2015].

[2] See Appendix F.

We frame the subsequent discussion in terms of two competing economic forces generated by a bankruptcy flag removal: (i) the *credit-access* effect: credit constraints loosen after flag removal allowing individuals to start self-employed businesses or borrow to smooth consumption while searching for an unemployment-insured (UI) job (we will refer to UI jobs as formal sector jobs, and non-UI jobs such as self-employment as informal sector jobs) (ii) the *credit-check* effect: individuals who were non-employed or self-employed subsequently find jobs in the formal sector after flag removal. Our main contribution is to provide suggestive evidence of these two economic forces as well as provide a complete picture of how the discrete rise in consumer credit following bankruptcy flag removal affects job flows, the transition rate from non-employmer to employer businesses, job creation, and earnings. Our approach is to build a set of facts that when viewed together, provide consistent evidence that the credit-access and credit-check effects influence employment outcomes.

We first study self-employment, and we show that the self-employment rate does not change among individuals whose bankruptcy flag is removed versus those whose flag is not removed. However, the lack of movement in the self-employment rate masks offsetting movements in gross flows. Relative to the control group whose flags are not removed, those whose flags are removed have both gross flows into self-employment increase by .16% per annum and gross flows out of self-employment increase by about .17% per annum. As a result, these flows offset and the stock remains constant.

Examining individuals who transition into self-employment, we show that cohorts who transition into self-employment after a bankruptcy flag removal borrow $15k more than cohorts who transition into self-employment prior to flag removal. This represents a 12.4% increase in borrowing relative to the sample average.[3] They borrow mainly in the form of mortgages, HELOCs and credit cards, and they earn ∼$1k more Schedule C net income at any time horizon we observe (an increase of about 4% relative to the sample average).[4]

We then use the new Integrated Longitudinal Business Database (LBD) to measure transitions from self-employment to employer firms. Those who enter self-employment after a flag drop are .7% more likely to own an employer firm in the LBD, a 200% increase over the sample average LBD firm ownership rate. Among those who own an employer firm in the LBD, they borrow on average $40k more after flag drop, a 33% gain relative to the sample

[3]The average total balance across all forms of credit one year prior to removal is $121k in our sample.

[4]Relative to national averages, this is still a 2.2% treatment effect. The median self-employment income is $45,000 and calculated as the pooled average of all heads of household who have positive self-employment income from the 1998 SCF through the 2010 SCF, weighted.

average. Both payroll and employment increase for firm owners whose flags are removed, however, we lack power to statistically discriminate these effects from zero.

We take this set of facts – (a) increased flow rates into self-employment, (b) disproportionate borrowing by new self-employed entrants relative to other job-transitioners, (c) the increased likelihood of starting an employer business, and (d) the large amount borrowed by new employer businesses – as strong evidence of the credit-access effect.

We then examine formal sector jobs (i.e. jobs that are unemployment-insured (UI)). We find that the formal-sector employment rate of individuals whose bankruptcy flags are removed increases by .32% relative to those whose flags remain on their record. Measured relative to the control group, gross flows into the formal sector increase by .24% per annum. While average earnings of formally employed workers remains constant around flag removal, we find that those who make the transition into formal employment following a bankruptcy flag removal earn $1.8k more per annum relative to individuals who transition into formal sector employment prior to flag removal. This earnings gain represents an increase of over 4.3% relative to the sample average. What is striking is that conditional on flowing into the formal sector after flag removal, individuals are 1.5% more likely to work for large firms (1000+ employees) and less likely to subsequently exit the formal sector to non-employment or self-employment. In other words, those who get jobs after the flag removal are not 'bad' workers.

While evidence on firm size and credit checks is scarce, the existing evidence from the UK (e.g. Zibarras and Woods [2010]) indicates that small firms are less likely to conduct background checks (although sample sizes prevent this statement from being statistically significant). The fact that workers disproportionately flow into larger firms following flag removal provides suggestive evidence that credit-checks may have previously prevented these individuals from obtaining jobs at those firms. These results are also consistent with individuals using consumer credit to smooth consumption while searching for higher paying jobs at larger and more productive firms. In particular, recent work by Herkenhoff et al. [2015] shows that displaced workers borrow and take longer to find a job if they have more credit access. However, the goal of this paper is to assess the impact of credit access on all types of employment flows, and so our current sample includes very few displaced workers. For this broader sample, we are able to show that those who find a new formal job do not increase borrowing. Since these new job finders are not borrowing, our evidence thus suggests that individuals may be obtaining better jobs after flag removal because credit-checks by employ-

ers were previously limiting employment opportunities. Since we do not directly observe credit checks, we are unable to conclusively distinguish between these two forces, but at the bare minimum, our results indicate that a combination of both the credit check effect and credit access effect influence formal-employment outcomes.

In terms of bankruptcy policy implications, we calibrate a directed search model using our empirical estimates, and we show that firms, ex-post, would be willing to pay approximately $17.6k in net present value to be able to decipher between a potential hire who has a bankruptcy record versus a potential hire with no bankruptcy record. What drives this result is the large difference in wage payments between bankrupt and non-bankrupt workers, which can be used to back-out that firms believe bankrupt workers are 3.8% less productive than non-bankrupt workers. This implies that banning credit checks of potential hires, as many states have done (e.g. Cortes et al. [2016] and Shoag and Clifford [2016]), may be costly to firms, ex-post. We are unaware of any comparable existing calculation, and our resulting estimates can be used by lawmakers and policymakers for cost-benefit analysis of credit-check bans.

Our paper proceeds as follows. Section 1 describes the related literature, and Section 2 describes the institutional background. Section 3 summarizes the data and our empirical approach. Section 4 presents the baseline 'stock' or 'level' results. Section 5 analyzes self-employed transitioners and LBD firm owners, and Section 6 analyzes those who obtain a job in the formal sector. Section 7 discusses policy implications, and Section 8 concludes.

# 1 Literature Review

Our paper contributes to several literatures, including the theoretical and empirical literature on credit constraints and startup rates (*inter alia* Evans and Jovanovic [1989], Evans and Leighton [1989], Berger and Udell [1998], Cooley and Quadrini [2001], Black and Strahan [2002], Hurst and Lusardi [2004], Buera et al. [2009], Hurst and Pugsley [2011], Greenstone et al. [2014], Buera et al. [2015]) as well as the specific empirical literature on consumer credit access and startups (*inter alia* Fairlie and Krashinsky [2012], Chatterji and Seamans [2012], Schmalz et al. [2013], Adelino et al. [2013], Jensen et al. [2014], Kerr et al. [2014], Mehrotra and Sergeyev [2015]) as well as employment (*inter alia* Mian and Sufi [2012], Dobbie and

Goldsmith-Pinkham [2014], Herkenhoff et al. [2015]).[5] The idea of using bankruptcy flag removals as a natural experiment is not new, and was pioneered by Musto [2004] and more recently applied, and assessed, by Cohen-Cole et al. [2009] and Han and Li [2011] as well as Gross et al. [2016].

Of particular note is the concurrent, independent, and innovative work by Bos et al. [2015] and Dobbie et al. [2016]. Bos et al. [2015] focus on the way delinquencies, i.e. skipped payments as opposed to debt discharge, affect earnings and employment in Sweden. Using changes in the way delinquencies are registered in Sweden, they find that overall self-employment falls upon delinquency flag removal and they find overall employment increases. Dobbie et al. [2016] merge bankruptcy court records with SSA administrative earnings and study the impact of bankruptcy flag removal on the stock of formal employment and self-employment as well as earnings. They compare Chapter 13 filers to Chapter 7 filers using a difference-in-difference approach. They find that following flag removal, the stock of employment, the stock of self-employment, and average earnings increase slightly, but these increases are statistically insignificant. Overall, our point estimates are broadly consistent with Bos et al. [2015] and Dobbie et al. [2016], even though each paper was developed independently using different research designs.

Relative to Bos et al. [2015], Dobbie et al. [2016], and the existing literature, to our knowledge, we are the first to measure gross employment transitions into and out of self- and formal-employment in response to Chapter 7 and Chapter 13 bankruptcy flag removals, and we are the first, to our knowledge, to measure the causal impact of consumer credit access, inclusive of both unsecured and mortgage credit, on the rate at which individuals move from being a non-employer to employer business. Since we are the first to merge credit reports with LBD firm ownership, we also provide the most complete characterization of the consumer-credit choices of these new entrants (which we believe to be an advance over survey data which often aggregates or does not measure all sources of consumer credit, e.g. Census CBO/SBO and SCF).

There is also a relatively young theoretical and empirical literature that builds on Chatterjee et al. [2007] and looks at the impact of bankruptcy institutions and consumer credit on labor supply (e.g. Livshits et al. [2007], Han and Li [2007], Chen [2012], Chatterjee and Gordon [2012], Herkenhoff and Ohanian [2012], Dobbie and Song [2013], Athreya et al. [2014]) as

[5] See also Karlan and Zinman [2010] and Banerjee et al. [2013] for randomized controlled trials of micro finance in developing countries.

well as the impact of credit information structures on employment (Chatterjee et al. [2008], Athreya et al. [2012], Chen et al. [2013], and Corbae and Glover [2015]). In particular, Chen et al. [2013] develop a model in which credit scores reveal information about the productivity of a worker, leading employers to discriminate based on credit scores. Using survey data, they also show that nearly 60% of the human resource representatives surveyed in the 2009 Survey by the Society for Human Resource Management used credit checks for new hires. Our empirical findings provide support for this mechanism and suggest these credit checks are binding in some cases. Concurrent and independent work by Cortes et al. [2016] and Shoag and Clifford [2016] study regional variation in employment following state passage of credit-check bans. Cortes et al. [2016] find greater churn and Shoag and Clifford [2016] find moderate aggregate employment gains among minority groups following bankruptcy flag removal.

Our paper also broadly relates to models which incorporate personal bankruptcy policy into entrepreneurship decisions (Glover and Short [2010] and Akyol and Athreya [2011]), financial frictions and labor market outcomes (*inter alia* Petrosky-Nadeau and Wasmer [2013] and Blanco and Navarro [2014]), as well as theories that focus on credit constraints and subsistence entrepreneurship (e.g. Donovan et al. [2014] and Dinlersoz et al. [2015]).

# 2 Institutional Background

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 dataset. 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.[6] 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

[6]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, and 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.

up to 10 years after the date of filing, whereas Chapter 13 is removed up to 7 years after filing.[7] What is important for the purpose of our regression design is that the removal of the bankruptcy flag is statutory and follows a cutoff rule.

One important benefit of our dataset that gives us significant power is that we observe the actual date of flag removal. These dates can vary due to timing of filings and court proceedings as well as repeat filings. By observing the actual date of removal, we are able to estimate effects of flag removal precisely.

# 3 Data Description and Empirical Approach

Our data on unemployment-insured (UI) jobs (or formal sector jobs) comes from the Longitudinal-Employer Household Dynamics (LEHD) database. The LEHD, which is a matched employer-employee dataset that covers 95% of U.S. private sector jobs, includes information on worker flows between UI jobs as well as quarterly earnings.[8] 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.

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 their social security number) with the employer-firms that are subsequently created and owned by the same individuals. Appendix A briefly describes the way the non-employer/employer universes are linked, but the construction and details of this dataset are described extensively in Davis et al. [2007]. The self-employment income comes from the universe of Schedule C tax records for sole-proprietors across all U.S. 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.

[7] 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 10 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 creditors proprietary database."

[8] See Abowd et al. [2009] for an extensive description of the LEHD.

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 similar market share to Equifax and Experian. Our main sample is an approximately 5% random sample of individuals with credit reports from the 11 states for which we have LEHD data. The TransUnion data is then merged based on an anonymized unique identifier to the LEHD. Our data includes information on the balance, limit, and status (delinquent, current, etc.) of different classes of accounts held by individuals. Our credit data is measured as of September in each year, so there are instances in which flags are removed in October, November, or December of the prior year (i.e. prior fiscal year ending Dec. 31 through which we measure earnings and self-employment earnings), but the flag removal is classified as a removal only in the following year. In Appendix G, we attempt to capture these early transitions by using beginning-of-year employment (e.g. if an individual earned $1k last year and $1k this year, then they were employed at the beginning of the year, and they transited at some point in the prior year). Under these alternate beginning-of-year definitions of employment and self-employment, our main results persist.

Each database contains the same anonymized unique identifiers that can be used to link the datasets 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 Empirical Approach

Our empirical strategy is to compare previously bankrupt individuals before and after their bankruptcy flag removal to a subset of individuals whose flags are removed later in the sample, i.e. we implement a difference-in-difference analysis. In particular, our sample window is 2001-2007[9] and we always restrict our attention to 24-65 year olds.[10] Even though our sample window stops in 2007, our credit data allows us to identify flag removals between 2002 and 2010. We include all flag removal cohorts in our analysis.

Let $i$ index individuals and $t$ index years (from 2001 to 2007). Let $\alpha_i$ denote a set of

[9]Since we use several forward lags of variables, we cannot include 2008 in our sample window. However, our 2007 variables that are forward looking are using 2008 data.

[10]We do note that 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 bankruptcy before 2005. In Appendix G we limit the sample window to 2001-2005, and we use alternate variable definitions. See Albanesi and Nosal [2015] for more analysis of the how the reform affected new delinquency behavior.

individual fixed effects, and $\gamma_t$ denote year dummies. Let $Y_{i,t}$ denote the outcome of interest (a self employment dummy, earnings, wages, etc.). Let $D_{x,i,t}$ be a dummy variable taking the value 1 when an individual is $x$ periods before (if $x$ is negative) or after (if $x$ is positive) flag removal. E.g. $D_{-2,i,t}$ is a dummy indicating if an individual is 2 periods before flag removal, likewise $D_{0,i,t}$ takes a value of 1 if the individual is in the year of flag removal, and $D_{1+,i,t}$ takes a value of 1 if the treated individual is 1 or more years past flag removal. The specifications we use are of the following form:

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

The objects of interest are $\beta_0$ and $\beta_{1+}$ which summarize the impact of flag removal on the outcome variable in the year of removal as well as subsequent years, respectively. For some variables such as bankcard limits, bankcard balances, etc., bankruptcy flag removal has an immediate impact. For other variables such as employment, startup rates, employment at small firms, etc., bankruptcy flag removal has a delayed effect.[11]

The analogy in the medical literature is that we are comparing a group of sick individuals who receive the treatment (flag removal), to a group of sick individuals who have not yet received the treatment (e.g. those who are 3 or more years prior to flag removal). To check whether our point estimates are valid, we show that the treatment and control group have parallel trends prior to flag removal, (i.e. $\beta_{-2}$ and $\beta_{-1}$ are not statistically different from zero).

## 3.2 Variable Definitions

All nominal variables such as labor earnings, credit balance, and self-employed net income are deflated by the CPI (expressed in 2008 dollars), and we winsorize the top 1% of each continuous variable, except variables pertaining to the LBD (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 \$1k of real Schedule C net income throughout the year, and we define an individual to be formally employed if they earn at least \$1k of real labor earnings throughout the year in an

[11] If these variables were also "jump variables" then we would be able to use a simple difference estimator, as the bulk of the existing literature has chosen to do when studying credit outcomes.

unemployment-insured job. Transitions are defined at an annual frequency, e.g. a individual is counted as transitioning into self-employment if they earn less than $1k of real Schedule C net income in the prior year and then earn at least that much in the current year.

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 an LBD firm. The ownership links were constructed by Davis et al. [2007].[12] We define two measures of LBD firm ownership, the first of which only requires one year of ownership and includes potentially transitory businesses. Our second definition is more stringent and requires at least two years of ownership.

A new job accession occurs if the individual begins working at an employer that they previously have not worked for in our sample period.[13] Individuals may have multiple job accessions in a given year, and some job accessions may not necessary result in a separation from a prior employer (in the case of holding two jobs). Employer measures of size are taken as the monthly average of 4th quarter employment.

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 (this includes bankcards, revolving personal finance loans, and other revolving lines of credit). The combined sum of Home Equity Lines of Credit (HELOCs) are included in the HELOC Balance variable. Traditional unsecured credit cards that are issued by banks are included in the Bankcard Balance variable.

[12] Links are made to firmids, which refer to firms, not establishments. See Appendix A and Davis et al. [2007] for more details on the links.

[13] We use an end-of-quarter accession definition (Abowd et al. [2009]) that requires the individual to earn at least $500 dollars from the new employer in this year and next.

## 3.3 Summary Statistics

Table 1 compares the mean values of our main variables of interest one year before bankruptcy flag removal to one year after bankruptcy flag removal.[14] 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 compositional issues by including fixed effects and dynamic controls such as age and tenure in each regression. These controls capture cohort effects as well as static and dynamic differences across individuals.

Panel (A) of Table 1 describes the main 'stock' (or 'level') variables. The employment rate, self-employment rate, LBD firm ownership rate, and employer size among individuals one year before bankruptcy flag removal and one year after bankruptcy flag removal. If we define formal and self employment based on a $1,000 dollar earnings threshold, Column (1) of Table 1 shows that 78.7% of individuals are formally employed and 9.0% of individuals are self-employed one year before bankruptcy flag removal. Following flag removal, Column (2) shows that the formal-employment rate decreases by .1% to 78.6%, whereas the self-employment rate increases by .6% to 9.6%. 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 non-employed. Following flag removal, the fraction who hold two jobs increases significantly, whereas non-employment moves insignificantly. Prior to flag removal, .4% of our sample own a firm in the LBD for 1+ years, whereas .2% of our sample own a firm in the LBD for 2+ years. Following flag removal, we see a significant increase in the latter definition of LBD firm ownership by .1%. In terms of employer characteristics, roughly 1/3 of our workers are employed at large firms with 500+ employees, and this fraction increases significantly following flag removal.

Panel (B) of Table 1 describes the main flow variables. Approximately 4.6% of individuals transit into formal-employment in the year before flag removal, whereas 4.4% of individuals transit out of formal-employment. In both instances, there is an insignificant change in

[14] The 2006 and 2007 flag removal cohorts are not in our sample one year after their flag removal, but they are still used as controls. Therefore, the two sample sizes differ by 60k, where 60k is the combined number of individuals in the 2006 and 2007 cohorts. Likewise, the 2009 cohort and 2010 cohorts (approximately 50k individuals) will never be in our sample one year before or after their flag removal. But they are still used as controls in the main tables. 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.

flows following flag removal. 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 increases by .3% per annum to 3.4%, which is significant at the 10% level.

Panel (C) of Table 1 describes the main earnings variables. Per capita labor income in the sample is $32,683. Following flag removal, real annual labor income increases by $300, and this is a significant change. If we adjust for the fact that some individuals are not working, annual labor earnings per worker is approximately $41,529 (=$32,683/.787). 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 $23,778 (=$2,140/.09). Following flag removal, self-employed net income per capita increases by $161. Real annual total income is the sum of both self-employed (SE) net income and labor (non-SE) earnings.

Panel (D) of Table 1 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. [2009]). 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).

# 4 Level Results

The layout of the next several sections is to establish a series of facts that we will ultimately use to separate the credit-access effect from the credit-check effect. We begin our analysis with what we will call the 'stock' (or 'level') results, meaning that we only consider the impact of bankruptcy flag removal on the levels of employment, self-employment, and LBD firm ownership, and for the time being, we ignore flows. We will then turn to our analysis of gross flows and show that individual flow rates into and out of formal and self-employment change after bankruptcy flag removal, and we further characterize subsequent borrowing, earnings gains, and transitions into LBD firm ownership.

Table 1: Summary Statistics Before and After Flag Removal

| | Sample Averages | | | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| | 1 Yr. Before Flag Drop | 1 Yr. After Flag Drop | Diff. ((1)-(2)) | Sig. Diff. |
| **(A) Employment Stocks** | | | | |
| Formal-Employed, 1k (d) | 78.70% | 78.60% | -0.10% | |
| Self-Employed, 1k (d) | 9.00% | 9.60% | 0.60% | * |
| Both SE and Formal-Employed, 1k (d) | 6.10% | 6.40% | 0.30% | * |
| Non-Employed, 1k (d) | 18.40% | 18.20% | -0.20% | |
| LBD Firm Ownership, 1+ Yrs. (d) | 0.40% | 0.40% | 0.00% | |
| LBD Firm Ownership, 2+ Yrs. (d) | 0.20% | 0.30% | 0.10% | * |
| Employer Size≥500 (d) | 31.70% | 32.00% | 0.30% | * |
| Employer Size≥1000 (d) | 25.40% | 25.60% | 0.20% | |
| **(B) Employment Flows** | | | | |
| Transition into Formal-Employed, 1k (d) | 4.60% | 4.50% | -0.10% | |
| Transition out of Formal-Employed, 1k (d) | 4.40% | 4.50% | 0.10% | |
| Transition into Self-Employed, 1k (d) | 3.10% | 3.40% | 0.30% | * |
| Transition out of Self-Employed, 1k (d) | 2.80% | 2.80% | 0.00% | |
| New Formal Job Accession Next Year (d) | 17.30% | 17.00% | -0.30% | * |
| **(C) Earnings** | | | | |
| Real Annual Labor Earnings ($41.5k without 0s) | $32,683 | $33,005 | $323 | * |
| Real Annual Self-Employed Net Income ($23.8k without 0s) | $2,140 | $2,300 | $161 | * |
| 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 Bankcard 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 | 170000 | 110000 | | |

*Notes: Column (1) computes averages using the individuals in our sample who are 1 year before bankruptcy flag removal. Column (2) computes averages using the individuals in our sample who are 1 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) indicates a dummy variable. Formal-Employed, 1k (d) is a dummy that equals one when an individual earned at least $1k in a UI insured job covered by the LEHD. Self-Employed, 1k (d) is a dummy that equals one when an individual earned at least $1k in net income on their 1040 Schedule C. LBD Firm Ownership, 1+ Yrs (d) is a dummy for LBD firm ownership of 1 or more years. LBD Firm Ownership, 2+ Yrs (d) is a dummy for LBD firm ownership of 2 or more years. For all other definitions, see Section 3.2.*

## 4.1 Levels of Employment and Self-Employment

Table 2 illustrates the baseline results for the 'stocks' (or 'levels') of employment. The coefficients in Table 2 correspond to $(\beta_{-2}, \beta_{-1}, \beta_0, \beta_{+1})$ in Equation 1, and throughout the paper we will estimate coefficients using OLS, and we cluster standard errors at the individual level. In all regressions, we include year fixed effects and individual fixed effects in order 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.

Table 2 illustrates the large spike in bankruptcy scores in Columns (1) and (2) 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 bankruptcy scores, Figure 1 illustrates the regression coefficients from Column (1), showing the stable trend in bankruptcy scores leading up to the flag removal, followed by a punctuated one-time level shift in bankruptcy scores. Column (2) illustrates that after we take out a quadratic age trend, individuals' credit scores are close to pre-flag removal scores; however, this subsequent mean-reversion in scores is due to the increased borrowing following flag removal.

Figure 1: Bankruptcy Score (Coefficients plotted from Table 2, Column (1))

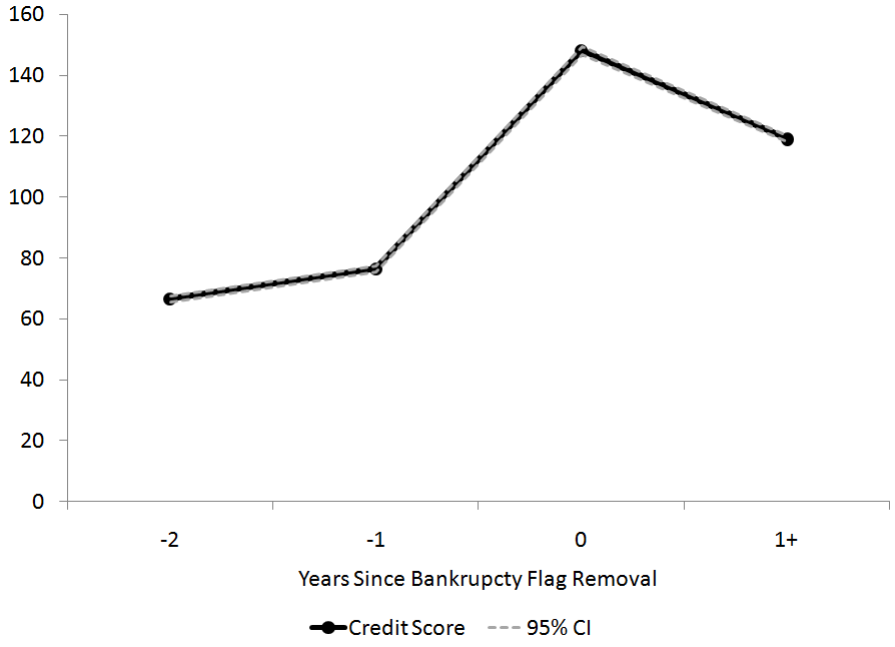
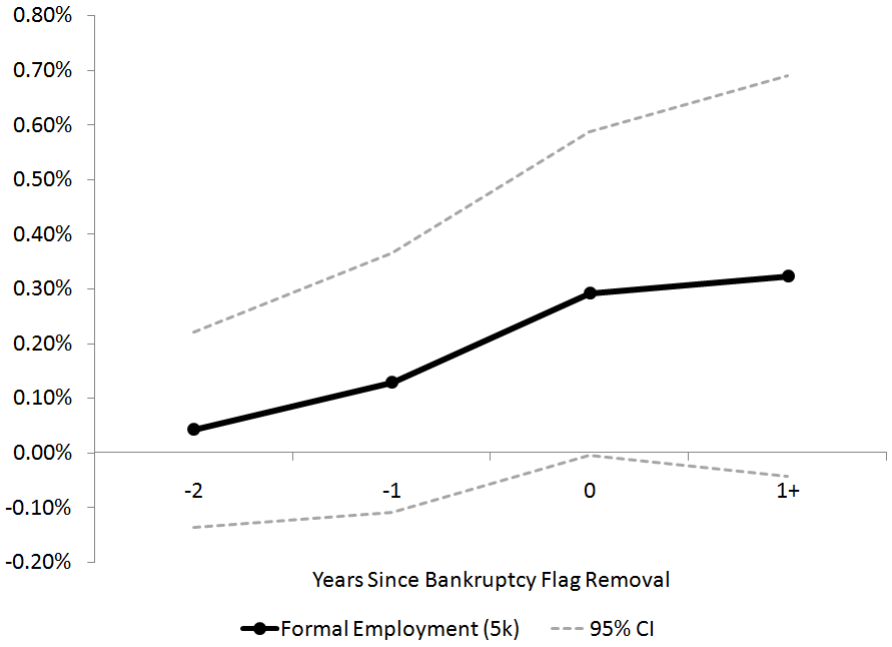


Table 2: Baseline Employment Levels and Bankruptcy Scores

| | **(1)** Credit Score | **(2)** Credit Score | **(3)** Formal-Employed, 1k (d) | **(4)** Formal-Employed, 5k (d) | **(5)** Self-Employed, 1k (d) | **(6)** Self-Employed, 5k (d) |
|---|---|---|---|---|---|---|
| 2 Years Before Removal (d) | 66.52*** | 19.70*** | 0.000308 | 0.000425 | 0.000600 | 0.000919 |
| | (0.513) | (0.444) | (0.000897) | (0.000914) | (0.000701) | (0.000597) |
| 1 Year Before Removal (d) | 76.39*** | 13.26*** | 0.00154 | 0.00129 | -0.000384 | 0.000353 |
| | (0.528) | (0.592) | (0.00120) | (0.00121) | (0.000903) | (0.000772) |
| Year of Removal (d) | 148.2*** | 68.70*** | 0.00289* | 0.00292* | 0.000950 | 0.00108 |
| | (0.675) | (0.798) | (0.00149) | (0.00151) | (0.00112) | (0.000955) |
| 1+ Years After Removal (d) | 119.1*** | 7.046*** | 0.00465** | 0.00323* | 0.00108 | 0.000983 |
| | (0.524) | (0.939) | (0.00185) | (0.00187) | (0.00137) | (0.00117) |
| 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 | 0.122 | 0.122 | 0.003 | 0.003 |
| Indiv-Yr Obs. | 1.500e+06 | 1.500e+06 | 1.500e+06 | 1.500e+06 | 1.500e+06 | 1.500e+06 |
| No. of Indiv. | 220000 | 220000 | 220000 | 220000 | 220000 | 220000 |
| Sig Diff 1+Yr & -2Yr at 10% | Y | Y | Y | Y | N | N |
| Sig Diff 0Yr & -2Yr at 10% | Y | Y | Y | Y | N | N |

*Notes: SE in parentheses. *** p<0.01, ** p<0.05, * p<0.1. Age and Tenure controls include quadratics in age and tenure. The symbol (d) indicates a dummy variable. Credit score refers to the TransUnion bankruptcy score. For formal-employment and self-employment definitions, see Section 3.2. '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 an indicator that the coefficients are different on the terms 'Year of Removal (d)' and '2 Years Before Removal (d)' at the 10% level.*

Figure 2: Formal employment (Coefficients plotted from Table 2, Column (4))




Columns (3) and (4) of Table 2 show the impact of flag removal on formal-employment. Column (3) defines formal employment to be those who have earned at least $1,000 in an unemployment-insured job, whereas Column (4) defines formal employment to be those who have earned at least $5,000 in an unemployment-insured job. Using the $1k threshold, Column (3) shows that the stock of formally employed individuals increases by .465% for those whose bankruptcy flags were removed relative to the control group (those who are 3 or more years before flag removal). Using the 5k definition in Column (3), formal employment increases by .323%. The coefficients from Column (3) are plotted in Figure 2.

Columns (4) and (5) of Table 2 show the impact of flag removal on self-employment, defined using $1k and $5k annual net income thresholds, respectively. Both columns reveal a small, but insignificant increase in self-employment following bankruptcy flag removal. However, this relatively stable stock of self-employment masks offsetting changes in gross flows. As we will see in Section 5, following flag removal, flows into and out of self-employment increase, but the stock remains constant.

## 4.2 Levels of Non-Employment

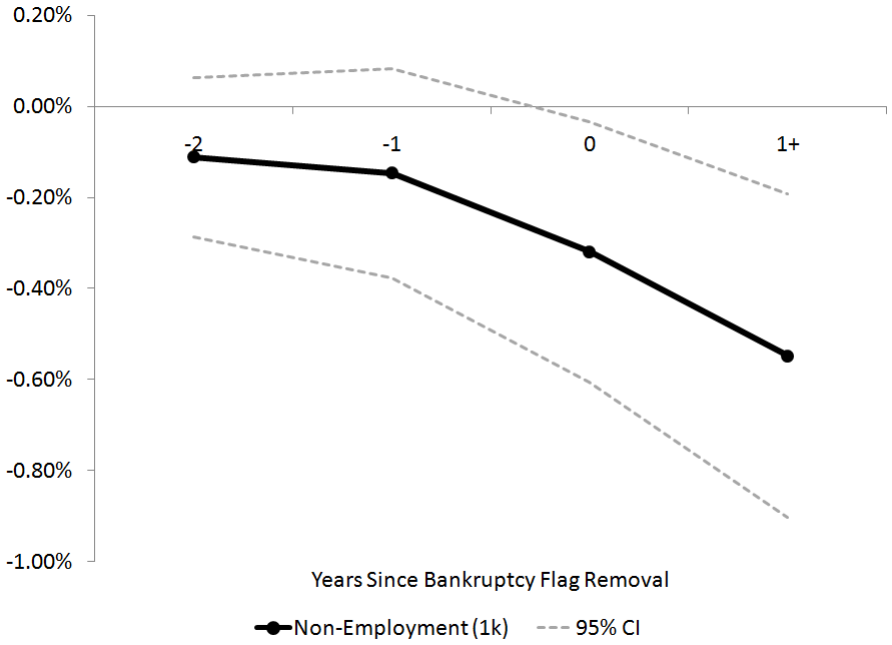
Table 3 illustrates the impact of bankruptcy flag removal on non-employment, defined to be those who are not formal-employed and are also not self-employed. Column (1) of Table 3 shows that non-employment (using a $1k earnings threshold) declines by .548% following bankruptcy flag removal, relative to the control group. In this case, we can reject equality of coefficients on the dummy one year prior to flag removal and one year following flag removal. Figure 3 plots the coefficients from Column (1). The figure shows a stable trend in non-employment prior to flag removal and then a rapid decline in non-employment following flag removal. Column (2) of Table 3 illustrates a similar pattern using the $5k definitions of formal employment and self-employment.

Table 3: Baseline Non-Employment Results

| | **(1)** Non-Employed, 1k (d) | **(2)** Non-Employed, 5k (d) |
|---|---|---|
| 2 Years Before Removal (d) | -0.00112 | -0.000723 |
| | (0.000887) | (0.000914) |
| 1 Year Before Removal (d) | -0.00147 | -0.00101 |
| | (0.00117) | (0.00120) |
| Year of Removal (d) | -0.00320** | -0.00278* |
| | (0.00146) | (0.00150) |
| 1+ Years After Removal (d) | -0.00548*** | -0.00354* |
| | (0.00181) | (0.00185) |
| Individual Fixed Effects | Y | Y |
| Year Fixed Effects | Y | Y |
| Age and Tenure Controls | Y | Y |
| R-squared | 0.096 | 0.098 |
| Indiv-Yr Obs. | 1.500e+06 | 1.500e+06 |
| No. of Indiv. | 220000 | 220000 |
| Sig Diff 1+Yr & -2Yr at 10% | Y | Y |
| Sig Diff 0Yr & -2Yr at 10% | Y | Y |

*Notes: SE in parentheses. *** p<0.01, ** p<0.05, * p<0.1. Age and Tenure controls include quadratics in age and tenure. The symbol (d) indicates a dummy variable. Non-employment (d) is a dummy which equals one when individuals are simultaneously not formal-employed and not self-employed. For formal-employment and self-employment definitions, see Section 3.2.*

Figure 3: Non-employment (Coefficients Plotted from Table 3, Column (1))




## 4.3 Ownership of LBD Firms

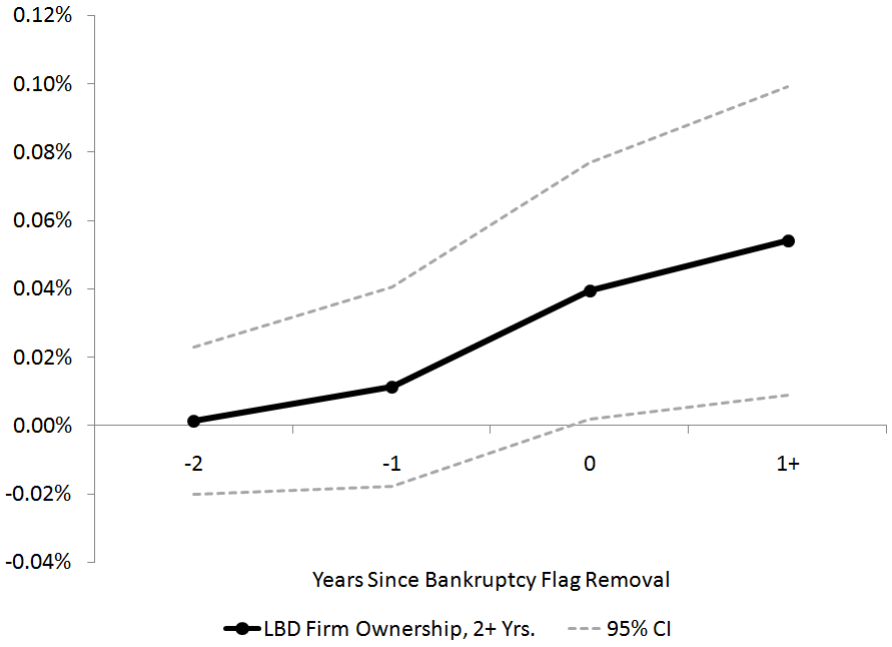
Table 4 illustrates the impact of bankruptcy flag removal on ownership of employer firms, i.e. ownership of an employer firm in the Longitudinal Business Dynamics (LBD) database. The ownership links were constructed by Davis et al. [2007] and are discussed in Appendix A. In Column (1), we define LBD firm ownership to be at least one or more years of firm ownership. This definition includes relatively transitory firm ownership spells of 1 year and less. We find that following flag removal, ownership of LBD firms increases, but insignificantly. In Column (2), we define LBD firm ownership to be at least two or more years of firm ownership. Column (2) illustrates that under this more stringent definition, there is now a significant and positive increase in ownership following bankruptcy flag removal, relative to the control group. The magnitude of this increase, however, is economically quite small; following flag removal, the odds that an individual owns a firm in the LBD increases by .05% (or approximately 1000 startups in our sample of 1.5m person-year observations) and we cannot reject equality of the coefficients on the dummies one year prior to removal and one year after removal. Figure 4 plots the results from Column (2), illustrating the rise in employer firms following flag removal.

Table 4: Ownership of LBD Firms

| | **(1)** LBD Firm Ownership, 1+ Yrs (d) | **(2)** LBD Firm Ownership, 2+ Yrs (d) |
|---|---|---|
| 2 Years Before Removal (d) | -0.000126 | 1.30e-05 |
| | (0.000172) | (0.000110) |
| 1 Year Before Removal (d) | -0.000110 | 0.000113 |
| | (0.000215) | (0.000149) |
| Year of Removal (d) | 0.000179 | 0.000394** |
| | (0.000259) | (0.000191) |
| 1+ Years After Removal (d) | 0.000297 | 0.000540** |
| | (0.000334) | (0.000230) |
| Individual Fixed Effects | Y | Y |
| Year Fixed Effects | Y | Y |
| Age and Tenure Controls | Y | Y |
| R-squared | 0.001 | 0.000 |
| Indiv-Yr Obs. | 1.500e+06 | 1.500e+06 |
| No. of Indiv. | 220000 | 220000 |
| Sig Diff 1+Yr & -2Yr at 10% | N | Y |
| Sig Diff 0Yr & -2Yr at 10% | N | Y |

*Notes: SE in parentheses. *** p<0.01, ** p<0.05, * p<0.1. The symbol (d) indicates a dummy variable. Age and Tenure controls include quadratics in age and tenure. LBD Firm Ownership, 1+ Yrs (d) is a dummy for LBD firm ownership of 1 or more years. LBD Firm Ownership, 2+ Yrs (d) is a dummy for LBD firm ownership of 2 or more years. For more details on LBD firm ownership measures, see Section 3.2.*

Figure 4: LBD Firm Ownership (Coefficients Plotted from Table 4, Column (2))




## 4.4 Earnings

Appendix B measures the impact of bankruptcy flag removal on labor earnings and Schedule C net income. We find that labor earnings and self-employed net income are approximately constant following flag removal. However, in the sections that follow, we show that those who transition into new formal sector jobs and those who start new businesses after flag removal earn significantly more. We then use information on subsequent outcomes and the borrowing behavior of these transitioners to understand the mechanisms underlying the earnings gains.

## 4.5 Summary of Level Results

In summary, the 'stock' or 'level' results indicate that while formal-employment and non-employment respond to flag removal, self-employment is stagnant, and LBD firm ownership increases only slightly. At first pass, these findings suggest that credit-access may not matter for entrepreneurship, and we can say very little about the importance of credit-checks. But these level results are misleading, and this is where we make our main contribution: our insight is to decompose these stock results into 'flows in' and 'flows out'. And, it is precisely

by studying flows that we can make inferences about the importance of credit access and credit-checks.

# 5 Transitions Into and Out of Self-Employment

In this section, we study gross flows into and out of self-employment. We examine transitioners earnings, borrowing behavior, and the subsequent rate at which non-employers become employer businesses in the LBD. By doing so, we attempt to disentangle the two competing forces following a bankruptcy flag removal: (i) the credit-access affect which allows previously constrained individuals to start a business, and the (ii) credit-check affect allows individuals who were previously unable to find a formal sector job due to poor credit to enter the formal labor force.

Table 5 measures self-employment flows following flag removal. Column (1) illustrates that flows into self-employment, using the $1k threshold, increase by .16% per annum following a bankruptcy flag removal relative to individuals whose flag is not removed. This increase is quite transitory, and relatively small in economic magnitude. However, individuals who subsequently flow into self-employment following bankruptcy flag removal, as we show below, borrow more, earn more net income, and are more likely to become an employer firm. We will argue in the sections that follow, that the increased flow rate into self-employment is due to the credit-access effect. Column (2) shows that transitions out of self-employment increase following bankruptcy flag removal as well. But, the table reveals a significant pretrend in the time series (we will address this in the next two columns as well as in Appendix G).

In the next two columns of Table 5, we use a $5k earnings threshold to define self-employment. Column (3) illustrates that flows into self-employment still increase by .1%; however, this coefficient is significant only at the 10% level. Column (4) of Table 5 shows that flows out of self-employment still exhibit a weak pretrend, but the same general patten emerges: individuals are exiting self-employment following bankruptcy flag removal. In Appendix C we show that the rate at which individuals transition from self-employment to formal-employment increases by .12% after the flag drop. As we will discuss in the following sections, the increased flow rate out of self-employment following flag removal, and the subsequent flow into formal-employment, is consistent with credit checks precluding

bankrupt individuals from finding formal-sector jobs.

Table 5: Baseline Self-Employment Flows

| | **(1)** Transition into Self-Employed, 1k (d) | **(2)** Transition out of Self-Employed, 1k (d) | **(3)** Transition into Self-Employed, 5k (d) | **(4)** Transition out of Self-Employed, 5k (d) |
|---|---|---|---|---|
| 2 Years Before Removal (d) | 0.000527 | 0.000898* | 0.000536 | 0.000644 |
| | (0.000576) | (0.000540) | (0.000491) | (0.000458) |
| 1 Year Before Removal (d) | 2.64e-05 | 0.00137** | 0.000219 | 0.000939* |
| | (0.000635) | (0.000598) | (0.000540) | (0.000509) |
| Year of Removal (d) | 0.00161** | 0.00169** | 0.00107* | 0.00131** |
| | (0.000740) | (0.000691) | (0.000630) | (0.000590) |
| 1+ Years After Removal (d) | 0.000649 | 0.00222*** | 0.000119 | 0.00149** |
| | (0.000891) | (0.000837) | (0.000757) | (0.000708) |
| Individual Fixed Effects | Y | Y | Y | Y |
| Year Fixed Effects | Y | Y | Y | Y |
| Age and Tenure Controls | Y | Y | Y | Y |
| R-squared | 0.000 | 0.001 | 0.000 | 0.001 |
| Indiv-Yr Obs. | 1.500e+06 | 1.500e+06 | 1.500e+06 | 1.500e+06 |
| No. of Indiv. | 220000 | 220000 | 220000 | 220000 |
| Sig Diff 1+Yr & -2Yr at 10% | N | Y | N | N |
| Sig Diff 0Yr & -2Yr at 10% | N | N | N | N |

*Notes: SE in parentheses. *** p<0.01, ** p<0.05, * p<0.1. Age and Tenure controls include quadratics in age and tenure. Fixed Effects include individual fixed effects and year dummies. Transition into Self-Employed, 1k (d) is a dummy that takes the value 1 when an individual earns less than $1k of self-employed earnings this year, and more than $1k of self-employed earnings this year.*

## 5.1 Earnings After Transitioning into Self-Employment

To isolate the net income of new entrants, Table 6 reports the coefficients on the window of dummies surrounding the bankruptcy flag removal in Equation 1 interacted with a dummy of whether the individual transitioned into self-employment. How should these interaction terms be interpreted? Over and above the individual effects of transitioning into self-employment and having a bankruptcy flag removed, the interaction terms capture the additional effect of having both events occur simultaneously. To meaningfully interpret the interaction terms, we will compare those who transition into self-employment 2 years before flag removal to those who transition into self-employment 1 year after flag removal.

Column (1) is the easiest to interpret since all interaction terms and all coefficients are negative and monotone, meaning that formal sector employment earnings drop when individuals enter self-employment following flag removal. This is intuitive since individuals have less time for a formal sector job if they are running their own business. Column (2) illustrates that among individuals who transition into self-employment, those who transition into self-employment 1+ 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 2 years prior to flag removal. We arrive at this number by first computing the net income gain of an individual who transits into self-employment 1+ years after flag removal. For such an individual 3 coefficients are non-zero and must be summed to obtain the overall effect of \$11,739.8: '1+ Years After Removal (d) x Trans. into Self-Employed, 1k (d)' + '1+ Years After Removal (d)' +'Transition into Self-Employed, 1k (d)' = 3376-158.2+8522). Repeating this exercise for those who transition 2 years before flag removal, the overall effect is \$10,748.39 (=2274-47.61+8522). Taking the difference yields \$991 (= \$11,739.8-\$10,748.39). Since the unconditional transition term ('Transition into Self-Employed, 1k (d)') cancels in these calculations, we omit it in the remainder of the paper. Relative to the sample average self-employed net income of \$23.8k (adjusting for 0s in Table 1), \$991 represents a 4% gain.

Column (3) of Table 6 shows that among the individuals who transition into self-employment, unconditionally they have incomes that are \$7,016 greater, where total income is defined to be the sum of self-employed and formal labor earnings. However, among those who transition into self-employment after flag removal, their total income actually declines by \$-670 (=(1667-761.7)-(1629-53.66)) relative to those who transition prior to flag removal. This

indicates that the marginal self-employed entrant may not actually be benefiting from increased credit access.

Figure 5 plots the summed coefficients from Column (2) of Table 6. This allows us to measure the increase in self-employed income for individuals who transition into self-employment, relative to non-transitioners in the control group (i.e. those who are 3 or more years prior to flag removal). In particular, we add the coefficients on the flag removal dummy (e.g. 2 Years Before Removal (d)), interaction term (e.g. 2 Years Before Removal (d) x Trans. Into Self-Employed, 1k (d)) and the transition term (e.g. Transition Into Self-Employed, 1k (d)), and 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 non-transitioner in the control group. As the figure demonstrates, there is a stable trend in 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 vs. 2 years prior to removal is, as we saw before, $991. This calculation is illustrated on the graph.

Figure 5: Impact of Flag Removal on Self-Employed Income, for Those who Transition into Self-Employment (Summed Coefficients Plotted from Col. (2), Table 6)

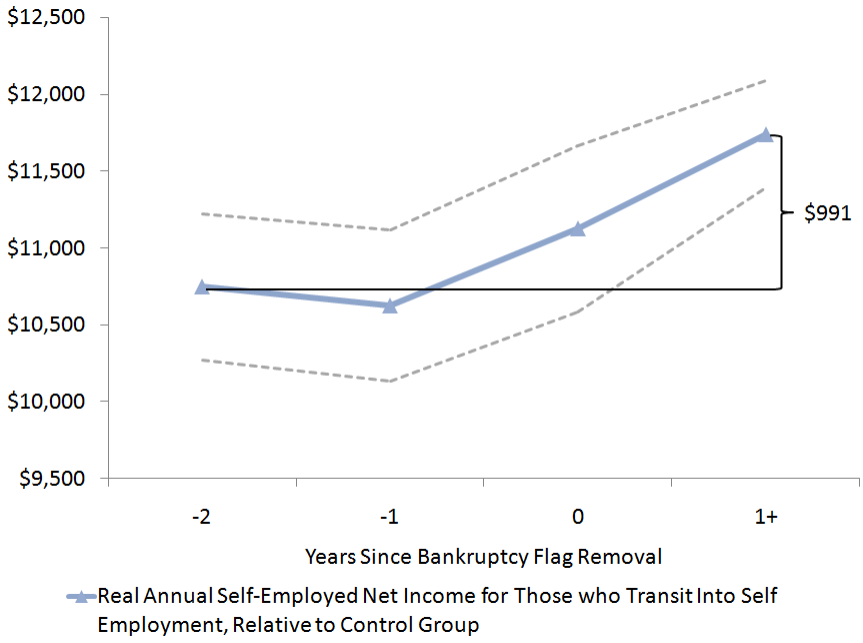


Table 6: Transitions into Self-Employment: Earnings

| | (1) Real Annual Earnings | (2) Real Self-Employed Net Income | (3) Real Total Income (SE and Non-SE) |
|---|---|---|---|
| 2 Years Before Removal (d) | -6.046 | -47.61** | -53.66 |
| | (44.85) | (20.08) | (47.42) |
| 1 Year Before Removal (d) | -111.5* | -78.49*** | -190.0*** |
| | (61.86) | (27.70) | (65.21) |
| Year of Removal (d) | -205.6*** | -101.3*** | -306.9*** |
| | (77.99) | (34.61) | (82.02) |
| 1+ Years After Removal (d) | -603.5*** | -158.2*** | -761.7*** |
| | (96.19) | (43.52) | (101.2) |
| Transition Into Self-Employed, 1k (d) | -1,506*** | 8,522*** | 7,016*** |
| | (116.6) | (123.4) | (154.0) |
| 2 Yrs. Before Removal (d) x Trans Into Self-Employed, 1k (d) | -645.0** | 2,274*** | 1,629*** |
| | (273.8) | (268.1) | (345.9) |
| 1 Yr. Before Removal (d) x Trans Into Self-Employed, 1k (d) | -1,138*** | 2,185*** | 1,048*** |
| | (272.8) | (274.8) | (346.5) |
| Yr. of Removal (d) x Trans Into Self-Employed, 1k (d) | -1,534*** | 2,704*** | 1,170*** |
| | (295.2) | (297.7) | (376.4) |
| 1+ Yrs. After Removal (d) x Trans Into Self-Employed, 1k (d) | -1,709*** | 3,376*** | 1,667*** |
| | (216.4) | (212.7) | (273.8) |
| Individual Fixed Effects | Y | Y | Y |
| Year Fixed Effects | Y | Y | Y |
| Age and Tenure Controls | Y | Y | Y |
| R-squared | 0.122 | 0.077 | 0.105 |
| No. Person-Yr Obs. | 1.500e+06 | 1.500e+06 | 1.500e+06 |
| No. of Indiv. | 220000 | 220000 | 220000 |
| Combined Coeff Diff 1+Yr & -2Yr | -1,661 | 991 | -670 |
| Combined Coeff Diff Sig at 10% | Y | Y | Y |

*Notes: SE in parentheses. *** p<0.01, ** p<0.05, * p<0.1. Age and Tenure controls include quadratics in age and tenure. Fixed Effects include individual fixed effects and year dummies. The row titled 'Combined Coeff Diff 1+ Yrs & -2 Yrs' calculates the difference in the summed coefficients for those who transition 1 year after flag removal (Sum the coefficients on '1+ Years After Removal (d)' +'1+ Yrs. After Removal (d) x Trans into Self-Empl, 1k (d)'=3376-158.2) minus the summed coefficients for those who transition 2 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 1 year after flag removal, relative to 2 years before. The titled 'Combined Coeff Diff Sig at 10%' is an indicator if that difference is significant at the 10% level.*

## 5.2 Borrowing Among Those Who Transition into Self-Employment

Table 7 illustrates the borrowing behavior of individuals who transition into self-employment. Individuals who transit into self-employment following a bankruptcy flag removal borrow heavily using secured credit (mortgages and HELOCs) as well as non-bankcard revolving credit. This provides a plausible mechanism for the increased earnings of individuals who transition into self-employment following a bankruptcy flag removal – they simply have more capital to work with.

Column (1) shows that individuals who transit into self-employment, regardless of whether their flag is removed or not, borrow very little using their bankcards (note, 'bankcards' refers to traditional unsecured credit cards issued by banks). Following flag removal, those who transition into self-employment borrow significantly using revolving credit (e.g. revolving personal finance loans) as shown in Column (2). They also take out large amounts of mortgage credit as shown in Column (3) and HELOCs as shown in Column (4). Those who transition into self-employment following a bankruptcy flag removal borrow \$3,766 (=1253+3551-(277.8+759.8)) more using HELOCs relative to those who transition into self-employment prior to flag removal. Figure 6 graphically illustrates the summed HELOC borrowing coefficients in Column (4) (i.e. Figure 6 plots the sum of the coefficients on the flag removal term, the transition term, and the corresponding interaction). There is a mild pretrend in the graph, but following flag removal, there is a significant rise in HELOC borrowing. Turning to the total balance across all types of consumer credit, Column (5) shows that those who transition into self-employment 1 or more years after flag removal borrow \$15,337 (=(16195+14373)-(6422+8809)) more than those who transition into self-employment 2 years prior to flag removal.

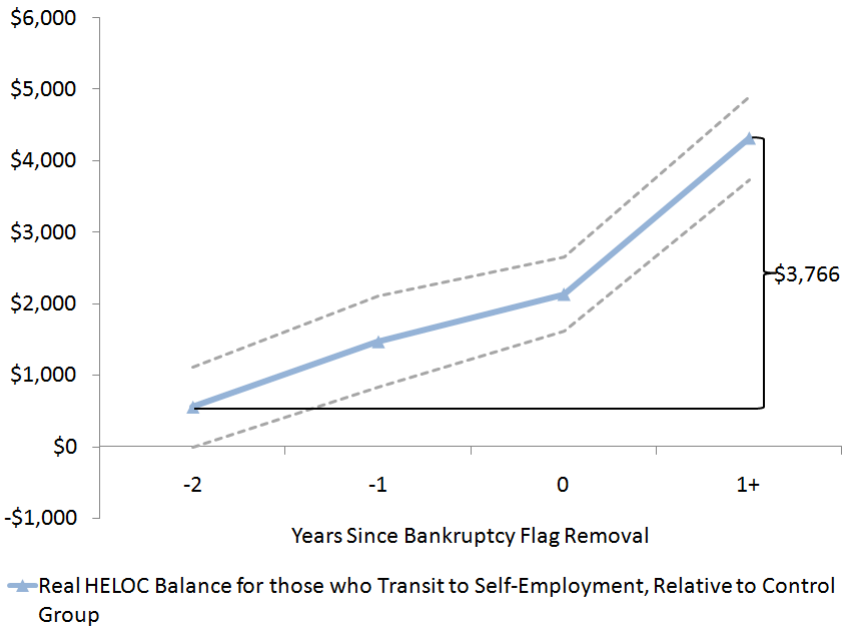
Our findings confirm survey based studies on mortgage borrowing by entrepreneurs such as Fairlie and Krashinsky [2012] and Adelino et al. [2013], as well as Decennial Census results, e.g. Kerr et al. [2014]. Relative to these studies, we measure the causal impact of generic consumer credit access including unsecured and revolving credit access, not just those induced by changes in house prices. We are also able to directly measure the types of credit used by the self-employed as well as LBD firm owners (see Section 5.5).

Table 7: Transitions into Self-Employment: Borrowing

| | (1) Real Bankcard Balance | (2) Real Revolving Balance | (3) Real Mortgage Balance | (4) Real HELOC Balance | (5) Real Total Balance |
|---|---|---|---|---|---|
| 2 Years Before Removal (d) | 202.1*** | 1,112*** | 6,023*** | 759.8*** | 8,809*** |
| | (13.09) | (40.60) | (329.1) | (47.46) | (350.1) |
| 1 Year Before Removal (d) | 336.3*** | 1,793*** | 8,957*** | 1,377*** | 13,038*** |
| | (18.21) | (56.30) | (443.1) | (66.35) | (475.3) |
| Year of Removal (d) | 586.4*** | 2,735*** | 10,387*** | 1,978*** | 15,800*** |
| | (23.83) | (72.19) | (556.3) | (84.78) | (598.6) |
| 1+ Years After Removal (d) | 892.7*** | 4,257*** | 8,084*** | 3,551*** | 14,373*** |
| | (28.47) | (83.77) | (675.8) | (98.35) | (728.1) |
| Transition Into Self-Employed (d) | -62.55*** | -461.9*** | -5,635*** | -486.9*** | -6,483*** |
| | (24.01) | (75.81) | (742.6) | (88.90) | (790.1) |
| 2 Yrs. Before Removal (d) x Trans Into Self-Employed, 1k (d) | 99.73 | 298.4 | 5,642*** | 277.8 | 6,422*** |
| | (66.47) | (216.5) | (1,835) | (270.1) | (1,955) |
| 1 Yr. Before Removal (d) x Trans Into Self-Employed, 1k (d) | 91.64 | 822.2*** | 10,086*** | 575.8* | 11,815*** |
| | (74.23) | (252.3) | (1,875) | (298.1) | (2,011) |
| Yr. of Removal (d) x Trans Into Self-Employed, 1k (d) | 46.52 | 598.9** | 7,348*** | 640.2* | 8,511*** |
| | (86.61) | (271.6) | (2,026) | (331.9) | (2,162) |
| 1+ Yrs. After Removal (d) x Trans Into Self-Employed, 1k (d) | 416.6*** | 1,610*** | 13,714*** | 1,253*** | 16,195*** |
| | (74.15) | (211.2) | (1,483) | (267.9) | (1,594) |
| 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. | 220000 | 220000 | 220000 | 220000 | 220000 |
| Combined Coeff Diff 1+ Yrs & -2 Yrs | 1,007 | 4,457 | 10,133 | 3,766 | 15,337 |
| Combined Coeff Diff Sig at 10% | Y | Y | Y | Y | Y |

*Notes: SE in parentheses. *** p<0.01, ** p<0.05, * p<0.1. 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 transitioning 1+ years after flag removal to the overall effect of transitioning 2 years before flag removal. See Table 6 for more details.*

Figure 6: Impact of Flag Removal on Self-Employed HELOC Borrowing, for Those who Transition into Self-Employment (Summed Coefficients Plotted from Col. (4), Table 7 )




## 5.3 Importance of Credit for Self- vs. Formal- Employed Workers

Are all job-transitioners more likely to borrow, or do the newly self-employed rely particularly heavily on credit?[15] To further test the importance of credit for the self-employed, Table 8 compares borrowing by those who transition into formal sector employment and those who transition into self-employment. Table 8 illustrates that those who transition into formal-employment after flag removal borrow $4,526 relative to those who transition prior to flag removal; however, the interaction terms are negative, indicating that formal transitioners are just like everyone else, and if anything, they borrow less than non-transitioners (this is an important point that we will revisit shortly). In contrast, those who transition into self-employment after flag removal borrow $15,337 more relative to those who transition prior to flag removal. Comparing these two numbers, it is clear that the self-employed borrow much more heavily following flag removal, nearly ∼10k more than other job-transitioners. This evidence is consistent with the credit-access effect being an important determinant of self-employment.

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

Table 8: Comparison of Total Borrowing by Newly Formal-Employed and Newly Self-Employed.

| | (1)<br>Total Balance | | (2)<br>Total Balance |
|---|---|---|---|
| | **Formal Trans.** | | **Self-Empl Trans.** |
| 2 Years Before Removal (d) | 9,234*** | 2 Years Before Removal (d) | 8,809*** |
| | (353.2) | | (350.1) |
| 1 Year Before Removal (d) | 13,483*** | 1 Year Before Removal (d) | 13,038*** |
| | (478.8) | | (475.3) |
| Year of Removal (d) | 16,355*** | Year of Removal (d) | 15,800*** |
| | (602.0) | | (598.6) |
| 1+ Years After Removal (d) | 15,220*** | 1+ Years After Removal (d) | 14,373*** |
| | (729.9) | | (728.1) |
| Transition into **Formal-Employed**, 1k (d) | -1,976*** | Transition Into **Self-Employed** (d) | -6,483*** |
| | (536.2) | | (790.1) |
| 2 Yrs. Before Removal (d) x Trans into **Formal-Empl**, 1k (d) | -5,095*** | 2 Yrs. Before Removal (d) x Trans Into **Self-Empl**, 1k (d) | 6,422*** |
| | (1,361) | | (1,955) |
| 1 Yr. Before Removal (d) x Trans into **Formal-Empl**, 1k (d) | -1,600 | 1 Yr. Before Removal (d) x Trans Into **Self-Empl**, 1k (d) | 11,815*** |
| | (1,453) | | (2,011) |
| Yr. of Removal (d) x Trans into **Formal-Empl**, 1k (d) | -6,143*** | Yr. of Removal (d) x Trans Into **Self-Empl**, 1k (d) | 8,511*** |
| | (1,616) | | (2,162) |
| 1+ Yrs. After Removal (d) x Trans into **Formal-Empl**, 1k (d) | -6,555*** | 1+ Yrs. After Removal (d) x Trans Into **Self-Empl**, 1k (d) | 16,195*** |
| | (1,164) | | (1,594) |
| Individual Fixed Effects | Y | Individual Fixed Effects | Y |
| Year Fixed Effects | Y | Year Fixed Effects | Y |
| Age and Tenure Controls | Y | Age and Tenure Controls | Y |
| R-squared | 0.105 | R-squared | 0.105 |
| No. Person-Yr Obs. | 1.500e+06 | No. Person-Yr Obs. | 1.500e+06 |
| No. of Indiv. | 220000 | No. of Indiv. | 220000 |
| Combined Coeff Diff 1+ Yrs & -2 Yrs | 4,526 | | 15,337 |
| Combined Coeff Diff Sig at 10% | Y | | Y |

*Notes: SE in parentheses. *** p<0.01, ** p<0.05, * p<0.1. 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 transitioning 1+ years after flag removal to the overall effect of transitioning 2 years before flag removal. See Table 6 for more details.*

## 5.4 Newly Self-Employed Turnover Rates and LBD Firm Ownership

Does access to credit affect the subsequent transitions of self-employed individuals? The evidence suggests that the post-flag removal entrants are more likely to make the transition into an employer-firm in the LBD. However, even though they have access to more capital, the odds that they subsequently exit self-employment is stable and statistically indistinguishable from those who transition into self-employment prior to flag removal. Table 9 illustrates these results more formally. Column (1) of Table 9 illustrates the odds that a newly self-employed individual becomes the owner of an employer-firm in the LBD (under the 2+ years definition), and Column (2) of Table 9 illustrates the subsequent turnover rate among newly self-employed individuals. Column (1) shows that individuals are .7% (=(.00979+.000204)-(.00312-.0000893)) more likely to own a firm in the LBD if they transition into self-employment 1+ years following a flag removal relative to those who transition into self-employment 2 years prior to flag removal. Column (2) shows that newly self-employed individuals are transitioning out of self-employment at a very high rate, 38%, unconditionally. However, following flag removal we see no disproportionate change in the subsequent rate at which these individuals exit self-employment.

## 5.5 Borrowing by LBD Owners

Table 10 illustrates the borrowing behavior of LBD firm owners. Column (1) shows that they borrow moderate amounts of bankcard credit following flag removal. Column (2) shows that they increase revolving credit significantly following flag removal, and Column (3) illustrates that they borrow significant amounts of mortgage credit. Column (3) shows that LBD firm owners who are 1 or more years after flag removal borrow $29,693 (=(37997+8462)-(7493+9273)) more using mortgage credit than LBD firm owners who are 2 years prior to flag removal. A significant fraction of their increased borrowing comes in the form of Home Equity Lines of Credit (HELOCs), as shown in Column (4). Turning to total debt balances (including secured and unsecured debts), Column (5) shows that LBD firm owners who are 1 or more years after flag removal borrow $39,835 (=(47332+14812)-(13318+8991)) more across all lines of credit than LBD firm owners who are 2 years prior to flag removal.

Figure 7 plots the summed coefficients from Column (5) of Table 10. This allows us to

Table 9: LBD Firm Ownership and Subsequent Turnover Among Newly Self-Employed

| | LBD Firm Ownership, 2+Yrs (d) | Transition out of Self-Employment Next Yr., 1k (d) |
|---|---|---|
| 2 Years Before Removal (d) | -8.93e-05 | 0.000481 |
| | (9.33e-05) | (0.000456) |
| 1 Year Before Removal (d) | -2.84e-05 | 0.000153 |
| | (0.000132) | (0.000509) |
| Year of Removal (d) | 0.000145 | 0.000583 |
| | (0.000171) | (0.000602) |
| 1+ Years After Removal (d) | 0.000204 | 0.00117 |
| | (0.000223) | (0.000754) |
| Transition Into Self-Employed (d) | 0.00936*** | 0.380*** |
| | (0.000692) | (0.00392) |
| 2 Yrs. Before Removal (d) x Trans Into Self-Employed, 1k (d) | 0.00312* | -0.00672 |
| | (0.00162) | (0.00830) |
| 1 Yr. Before Removal (d) x Trans Into Self-Employed, 1k (d) | 0.00451*** | 0.00761 |
| | (0.00172) | (0.00850) |
| Yr. of Removal (d) x Trans Into Self-Employed, 1k (d) | 0.00694*** | -0.0128 |
| | (0.00191) | (0.00889) |
| 1+ Yrs. After Removal (d) x Trans Into Self-Employed, 1k (d) | 0.00979*** | 0.00437 |
| | (0.00145) | (0.00649) |
| Individual Fixed Effects | Y | Y |
| Year Fixed Effects | Y | Y |
| Age and Tenure Controls | Y | Y |
| R-squared | 0.006 | 0.158 |
| No. Person-Yr Obs | 1.500e+06 | 1.500e+06 |
| No. Indiv. | 220000 | 220000 |
| Combined Coeff Diff 1+ Yrs & -2 Yrs | 0.70% | 1.18% |
| Combined Coeff Diff Sig at 10% | Y | N |

*Notes: SE in parentheses. *** p<0.01, ** p<0.05, * p<0.1. 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 transitioning 1+ years after flag removal to the overall effect of transitioning 2 years before flag removal. See Table 6 for more details.*

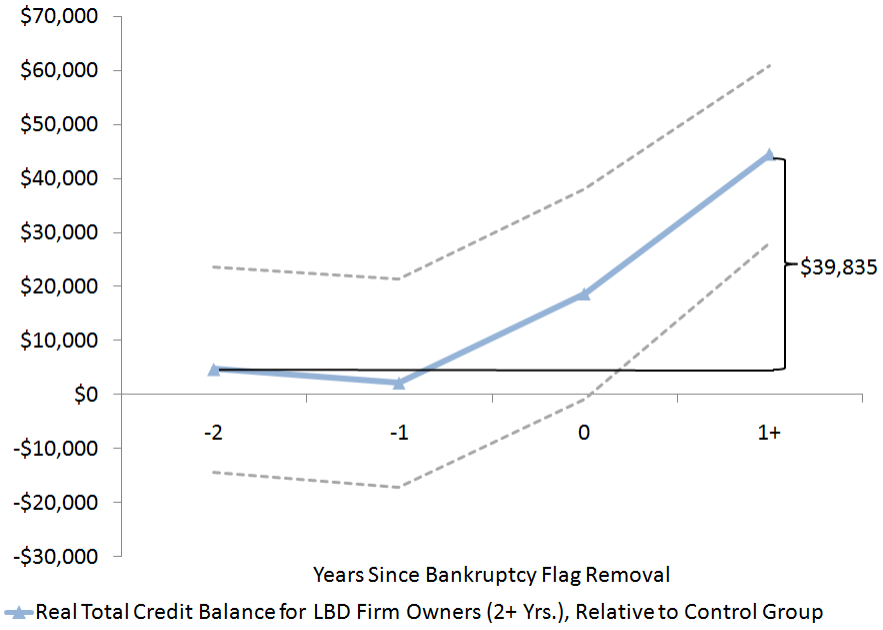
Table 10: LBD Ownership and Borrowing

| | (1) Real Bankcard Balance | (2) Real Revolving Balance | (3) Real Mortgage Balance | (4) Real HELOC Balance | (5) Real Total Balance |
|---|---|---|---|---|---|
| 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) |
| LBD 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 Yrs. Before Removal (d) x LBD 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 LBD 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 LBD 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 LBD Ownership, 2+ Yrs. (d) | 1,067*** | 8,645*** | 37,997*** | 7,716*** | 47,332*** |
| | (406.2) | (1,487) | (9,793) | (2,030) | (10,710) |
| 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. | 220000 | 220000 | 220000 | 220000 | 220000 |
| 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 |

*Notes: SE in parentheses. *** p<0.01, ** p<0.05, * p<0.1. 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 transitioning 1+ years after flag removal to the overall effect of transitioning 2 years before flag removal. See Table 6 for more details.*

measure the increase in borrowing among LBD firm owners, relative to non-transitioners in the control group. In particular, we add the coefficients on the flag removal dummy (e.g. 2 Years Before Removal (d)), interaction term (e.g. 2 Years Before Removal (d) x LBD Ownership, 2+ Yrs. (d)) and the ownership term (e.g. LBD Ownership, 2+ Yrs (d)), and we compute standard errors using the delta method. The points on the plotted line can be interpreted as the differential increase in total credit balances among LBD firm owners, relative to an individual in the control group. As the figure demonstrates, there is a stable trend in borrowing prior to flag removal. Following flag removal, relative borrowing among LBD firm owners increases rapidly. The difference in borrowing for those who are LBD firm owners one or more years after removal vs. 2 years prior to removal is $39,835 (=(47332+14812)-(13318+8991)).

Figure 7: Impact of Flag Removal on Total Credit Balances, for Those who Own Firms in LBD (2+ Yrs.) (Summed Coefficients Plotted from Col. (5), Table 10)



## 5.6 LBD Pay and Employment

Finally, Appendix D illustrates the impact of bankruptcy flag removal on the payroll and employment of LBD firm owners. Our results indicate that there is an increase in both LBD payroll and employment, however this increase is insignificant at standard levels. The lack of power is presumably from the small fraction of bankrupt individuals who own employer-firms in the LBD. We therefore see Appendix D as inconclusive evidence regarding the importance of consumer credit access for payroll and hiring decisions. In future research, we plan to explore the impact of credit access on hiring patterns in more detail with a broader sample of firm owners.

## 5.7 Discussion of Selection Issues

Since flag removal is foreseeable, there is concern that better entrepreneurs who anticipate the need for credit deliberately wait until the flag is removed to start a business. There are two ways we address this: (i) if 'better' entrepreneurs are waiting until their flag is removed in order to borrow and start a business, this simply reinforces the point that credit matters for startups, (ii) to test for selection more formally, we take advantage of the panel dimension to our data. In Appendix H we show that the entrepreneurs who transition into self-employment following flag removal are statistically indistinguishable in terms of prior self-employment income and prior labor earnings.

Moreover, recent independent work by Gross et al. [2016] has also provided formal tests of the anticipation of bankrupty flag removal by looking at credit application behavior; 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.

## 5.8 Taking Stock: The Credit-Access Effect

By analyzing gross flows as opposed to levels, we were able to establish several facts in Sections 5.1 to 5.6. Namely, following bankruptcy flag removal there is (a) an increased flow rate into self-employment, (b) disproportionate borrowing by new self-employed entrants relative to other job-transitioners, (c) an increased likelihood of starting an employer business, and

(d) a large amount borrowed by new employer businesses. We believe that these facts, taken together, provide strong evidence of the credit-access effect. Our findings also indicate that credit access not only affects the self-employment decision, but also the decision to become an employer firm. The credit-access effect influences both stages of entrepreneurship.

As robustness, we verify that our results regarding entrepreneurship and credit access hold in pooled SCF cross-sections from 1998-2010 in Appendix F.

# 6 Transitions Into and Out of Formal-Employment

We now turn our attention to gross formal-employment flows where we will provide another set of facts that allows us to partially disentangle the credit-access effect from the credit-check effect. Among bankrupt individuals who transition into formal employment, we find that if they make that transition after flag removal as opposed to prior to flag removal, they have (i) significantly greater earnings, (ii) work for larger firms, and (iii) as we show in Appendix F in the SCF, they are more likely to work for firms with non-wage benefits such as pensions.

Table 11 illustrates the impact of bankruptcy flag removal on formal-employment flows. Columns (1) and (2) show that for the baseline definition of formal employment, the flows in and flows out are insignificant. We attribute the lack of significance to the sample size and churn, since the levels increase significantly, but the flow regressions are essentially estimating coefficients on rare events with noise (since many of these individuals are marginally attached to the labor force, they may flow in and out of formal employment several times in the span on a few years).

In Table 11, if we define formal employment using a more stringent earnings threshold of \$5k, we do see flows into and out of formal employment increase significantly following flag removal. The flow rate into formal employment increases by .24% in the year of removal, relative to non-transitioners the control group. We can reject equality of coefficients on the dummy for the year of removal and the dummy for 2 years prior to removal, but the increase is short lived. The flow rate out of formal employment also increases following flag removal, suggesting that some individuals may be leaving formal employment to start businesses once they have credit access.

In Appendix C we illustrate the impact of flag removal on flows from self-employment to formal-employment, and vice versa. While point estimates imply that the transition rate from formal employment to self-employment increases, the results cannot be distinguished from zero. This suggests that if individuals are leaving formal employment to start businesses after flag removal, they are first going through a spell of non-employment. For completeness, Appendix C also shows how bankruptcy flag removal impacts the odds of holding both a formal sector job and self-employed job, as well as the odds of being only formal-employed and only self-employed.

Table 11: Baseline Formal-Employment Flows

| | **(1)** Transition into Formal-Employed, 1k (d) | **(2)** Transition out of Formal-Employed, 1k (d) | **(3)** Transition into Formal-Employed, 5k (d) | **(4)** Transition out of Formal-Employed, 5k (d) |
|---|---|---|---|---|
| 2 Years Before Removal (d) | -0.000892 | 0.000709 | 0.000788 | 0.000595 |
| | (0.000687) | (0.000686) | (0.000711) | (0.000703) |
| 1 Year Before Removal (d) | 0.000127 | 0.000421 | 0.00118 | 0.000927 |
| | (0.000763) | (0.000764) | (0.000786) | (0.000784) |
| Year of Removal (d) | -0.000380 | 0.000932 | 0.00241*** | 0.00224** |
| | (0.000878) | (0.000887) | (0.000907) | (0.000912) |
| 1+ Years After Removal (d) | -0.00121 | 0.00168 | 0.000713 | 0.00303*** |
| | (0.00107) | (0.00108) | (0.00110) | (0.00111) |
| Individual Fixed Effects | Y | Y | Y | Y |
| Year Fixed Effects | Y | Y | Y | Y |
| Age and Tenure Controls | Y | Y | Y | Y |
| R-squared | 0.026 | 0.026 | 0.018 | 0.011 |
| Indiv-Yr Obs. | 1.500e+06 | 1.500e+06 | 1.500e+06 | 1.500e+06 |
| No. of Indiv. | 220000 | 220000 | 220000 | 220000 |
| Sig Diff 1+Yr & -2Yr at 10% | N | N | N | Y |
| Sig Diff 0Yr & -2Yr at 10% | N | N | Y | Y |

*Notes: SE in parentheses. *** p<0.01, ** p<0.05, * p<0.1. Age and Tenure controls include quadratics in age and tenure. Fixed Effects include individual fixed effects and year dummies. Transition into Formal-Employed, 1k (d) is a dummy that takes the value 1 if the individual earned less than $1k in formal sector earnings last year, and greater than $1k in formal sector earnings this year.*

## 6.1 Earnings Among New Formal Sector Entrants

Table 12 includes interaction terms between the dummies surrounding the bankruptcy flag removal and an indicator for whether the individual transitioned into formal employment. Table 12 illustrates that among individuals who transition into formal employment, earnings rise significantly, and this increase is largely driven by the interaction of having a bankruptcy

flag removed and simultaneously transitioning into a formal sector job. For example, Column (1) shows that individuals who transition into formal employment 1 or more years after bankruptcy flag removal earn $1,816 (= (4033-847)- (1459-89.74) ) more than individuals who transition into formal employment 2 years prior to bankruptcy flag removal. Relative to the sample average of labor earnings which is $41.5k (see Table 1), these labor earnings gains represent a 4.3% increase.

Column (2) of Table 12 shows that those who transition into formal sector employment earn less from self-employment. This is an intuitive result, since the individual is taking a formal sector job, they have less time to devote to self-employment.

Column (3) of Table 12 looks at the sum of labor earnings and self-employment earnings. Column (3) shows that individuals who transition into formal employment 1 or more years after bankruptcy flag removal have a total annual income that is $1,696 (= (3726-870.7)- (1209-49.87) ) more than individuals who transition into formal employment 2 years prior to bankruptcy flag removal. Relative to the sample average of total income which is $34.8k, these gains are quite large, approaching 5% of the average individual's total income.

Figure 8 plots the summed coefficients from Column (3) of Table 12.[16] By summing the coefficients, we can compare those who transition into formal-employment to non-transitioners in the control group (3 or more years before flag removal). In particular, we add the coefficients on the flag removal dummy (e.g. 2 Years Before Removal (d)), interaction term (e.g. 2 Years Before Removal (d) x Trans. Into Formal-Employed, 1k (d)) and the transition term (e.g. Transition Into Formal-Employed, 1k (d)), and we compute standard errors using the delta method. The points on the plotted line can be interpreted as the increase in total income from entering formal employment, relative to an individual in the control group. As the figure demonstrates, there is a stable trend in total income prior to flag removal. Following flag removal, the gains from transitioning into formal-employment increase rapidly. The difference in total income for those who transition into formal employment one or more years after removal vs. 2 years prior to removal is $1,696 (= (3726-870.7)- (1209-49.87) ). This calculation is illustrated on the graph.

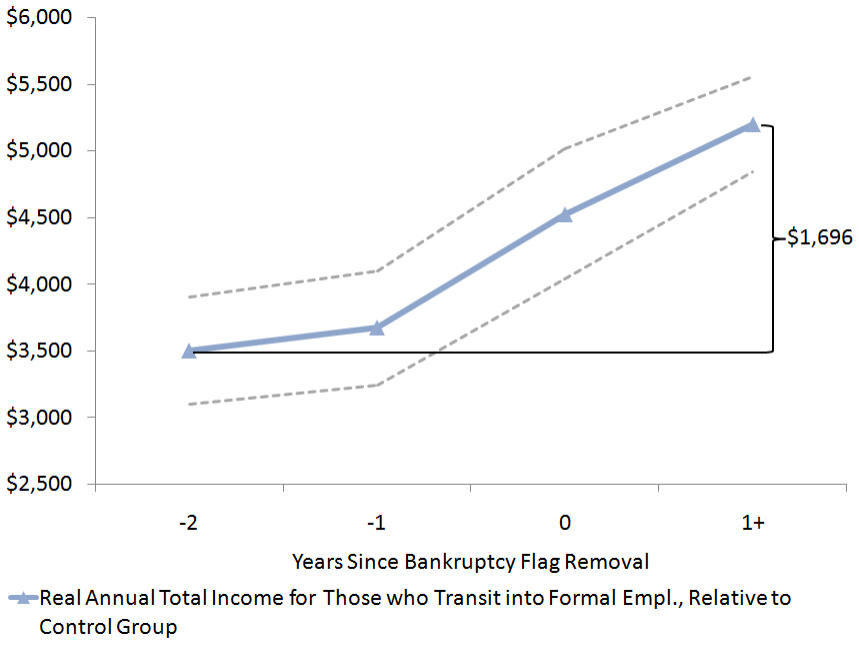
[16] Standard errors are computed using the delta method.

Table 12: Transitions into Formal Employment: Earnings

| | **(1)** Real Annual Labor Earnings | **(2)** Real Annual Self-Employed Net Income | **(3)** Real Annual Total Income (SE and Non-SE) |
|---|---|---|---|
| 2 Years Before Removal (d) | -89.74** | 39.87* | -49.87 |
| | (44.94) | (22.45) | (48.18) |
| 1 Year Before Removal (d) | -239.8*** | 11.64 | -228.1*** |
| | (62.11) | (29.39) | (65.74) |
| Year of Removal (d) | -381.2*** | 10.57 | -370.6*** |
| | (78.42) | (36.76) | (82.76) |
| 1+ Years After Removal (d) | -847.1*** | -23.63 | -870.7*** |
| | (96.50) | (44.79) | (101.7) |
| Transition into Formal-Employed, 1k (d) | 2,673*** | -332.2*** | 2,341*** |
| | (91.15) | (47.31) | (98.08) |
| 2 Yrs. Before Removal (d) x Trans. into Formal-Employed, 1k (d) | 1,459*** | -249.9** | 1,209*** |
| | (207.1) | (110.8) | (225.6) |
| 1 Yr. Before Removal (d) x Trans. into Formal-Employed, 1k (d) | 2,013*** | -455.0*** | 1,558*** |
| | (215.0) | (117.5) | (235.6) |
| Yr. of Removal (d) x Trans. into Formal-Employed, 1k (d) | 2,695*** | -140.5 | 2,554*** |
| | (237.3) | (131.0) | (259.2) |
| 1+ Yrs. After Removal (d) x Trans. into Formal-Employed, 1k (d) | 4,033*** | -307.3*** | 3,726*** |
| | (169.9) | (91.28) | (185.4) |
| Individual Fixed Effects | Y | Y | Y |
| Year Fixed Effects | Y | Y | Y |
| Age and Tenure Controls | Y | Y | Y |
| R-squared | 0.126 | 0.004 | 0.100 |
| No. Person-Yr Obs. | 1.500e+06 | 1.500e+06 | 1.500e+06 |
| No. of Indiv. | 220000 | 220000 | 220000 |
| Combined Coeff Diff 1+ Yrs & -2 Yrs | 1,817 | -121 | 1,696 |
| Combined Coeff Diff Sig at 10% | Y | N | Y |

*Notes: SE in parentheses. *** p<0.01, ** p<0.05, * p<0.1. 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 transitioning 1+ years after flag removal to the overall effect of transitioning 2 years before flag removal. See Table 6 for more details.*

Figure 8: Impact of Flag Removal on Total Income (Labor Earnings plus Self-Employed Net Income), Among Those Who Transition into Formal Employment (Summed Coefficients Plotted from Col. (3), Table 12)




## 6.2 Firm Size After Transitioning into Formal Employment

Column (1) of Table 13 shows that individuals who transition into formal employment following bankruptcy flag removal are more likely to work at a firm with 1000+ employees relative to individuals who transition into formal employment prior to flag removal. Column (1) shows that individuals who transition into formal employment 1 or more years after bankruptcy flag removal are 1.48% (= (.0357-.0034)- (.0188-.00126) ) more likely to work at a firm with 1000+ employees than individuals who transition into formal employment 2 years prior to bankruptcy flag removal. Column (2) illustrates a similar result, showing that individuals who transition into formal employment following bankruptcy flag removal are more likely to work for firms with greater than 500 employees relative to those who transition into formal employment prior to bankruptcy flag removal. Column (3) shows that among those who transition into formal sector employment, the fraction of individuals who work for small and young firms (firms with 1 employee or less and 1 year in age or less) remains unchanged. However, regardless of labor market transitions, the fraction of individuals who work at young small firms drops by a small, but statistically significant amount .0855%.

These results suggest that individuals are searching for jobs at larger firms which may provide better job security, health insurance, pensions etc. Since the LEHD does not cover healthcare or pensions, we show in Appendix F that in the SCF, following flag removal, individuals are more likely to work at larger firms that provide pensions; however, this result is only significant at the 10% level and occurs with a significant lag. Nonetheless, this suggests that there are non-wage considerations being taken into account when individuals search for a job, and bankruptcy flag removal allows them to find jobs at firms that provide these types of non-wage benefits.

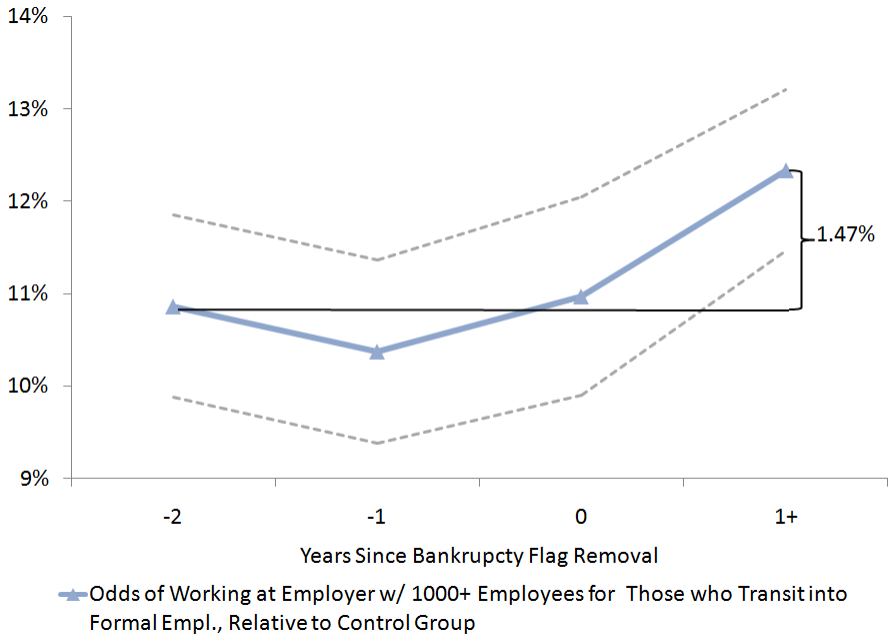
Figure 9 plots the summed coefficients from Column (1) of Table 13. By summing the coefficients, we can compare those who transition into formal-employment to those in the control group (3 or more years before flag removal). In particular, we add the coefficients on the flag removal dummy (e.g. 2 Years Before Removal (d)), interaction term (e.g. 2 Years Before Removal (d) x Trans. Into Formal-Employed, 1k (d)) and the transition term (e.g. Transition Into Formal-Employed, 1k (d)), and we compute standard errors using the delta method. The points on the plotted line can be interpreted as the increased odds of working at a large firm if an individual enters formal employment, relative to an individual in the control group. As the figure demonstrates, there is a stable trend in the odds of working at a large firm. Following flag removal, the odds of working at a large firm increase rapidly. The difference in the odds that an individual works at a large firm if they transition into formal employment one or more years after removal vs. 2 years prior to removal is 1.48% (= (.0357-.0034)- (.0188-.00126) ). This calculation is illustrated on the graph.

Table 13: Transitions into Formal Employment: Firm Size

| | **(1)** Employer Size≥1000 (d) | **(2)** Employer Size≥500 (d) | **(3)** Employer Size≤1 & Age≤1 Yr. (d) |
|---|---|---|---|
| 2 Years Before Removal (d) | -0.00126 | -0.00121 | 2.43e-05 |
| | (0.000868) | (0.000950) | (0.000286) |
| 1 Year Before Removal (d) | -0.00205* | -0.000788 | -3.53e-05 |
| | (0.00115) | (0.00125) | (0.000346) |
| Year of Removal (d) | -0.00199 | 0.000167 | -0.000559 |
| | (0.00143) | (0.00155) | (0.000409) |
| 1+ Years After Removal (d) | -0.00344* | -0.000773 | -0.000855* |
| | (0.00178) | (0.00193) | (0.000515) |
| Transition into Formal-Employed, 1k (d) | 0.0911*** | 0.114*** | 0.0350*** |
| | (0.00196) | (0.00214) | (0.00117) |
| 2 Yrs. Before Removal (d) x Trans into Formal-Employed, 1k (d) | 0.0188*** | 0.0292*** | -0.00261 |
| | (0.00454) | (0.00499) | (0.00258) |
| 1 Yr. Before Removal (d) x Trans into Formal-Employed, 1k (d) | 0.0147*** | 0.0210*** | 0.00481* |
| | (0.00452) | (0.00498) | (0.00274) |
| Yr. of Removal (d) x Trans into Formal-Employed, 1k (d) | 0.0206*** | 0.0268*** | 0.00329 |
| | (0.00490) | (0.00537) | (0.00295) |
| 1+ Yrs. After Removal (d) x Trans into Formal-Employed, 1k (d) | 0.0357*** | 0.0511*** | 0.00310 |
| | (0.00357) | (0.00394) | (0.00208) |
| Individual Fixed Effects | Y | Y | Y |
| Year Fixed Effects | Y | Y | Y |
| Age and Tenure Controls | Y | Y | Y |
| R-squared | 0.013 | 0.017 | 0.009 |
| No. Person-Yr Obs. | 1.500e+06 | 1.500e+06 | 1.500e+06 |
| No. of Indiv. | 220000 | 220000 | 220000 |
| Combined Coeff Diff 1+ Yrs & -2 Yrs | 1.47% | 2.23% | 0.48% |
| Combined Coeff Diff Sig at 10% | Y | Y | Y |

*Notes: SE in parentheses. *** p<0.01, ** p<0.05, * p<0.1. Age and Tenure controls include quadratics in age and tenure. Fixed Effects include individual fixed effects and year dummies. Employer Size≥1000 (d) is a dummy that takes the value 1 when an individual works at an employer with 1000+ other employees. Employer size is measured with respect to the SEIN and taken as the average of 4th quarter monthly employment. 'Combined Coeff Diff 1+ Yrs & -2 Yrs' compares the overall effect of transitioning 1+ years after flag removal to the overall effect of transitioning 2 years before flag removal. See Table 6 for more details.*

Figure 9: Impact of Flag Removal on Odds of Working At Employer with 1000+ Employees, Among Those Who Transition into Formal Employment (Summed Coefficients Plotted from Col. (1), Table 13)



## 6.3 Formal Sector Exit Rates and Job Turnover

Are those who transition into formal employment marginal workers? In Appendix E we explore this question by computing rates at which workers transit out of formal employment after finding a new job. In general, these newly employed workers are attached to the formal sector and are less likely to exit the formal sector after flag removal. Their new job accession rate within the formal sector increases after flag removal, but their large and persistent wage gains suggest that these subsequent accessions are simply reflecting the fact that these workers are climbing the job ladder.

## 6.4 Taking Stock: The Credit-Check Effect

In Section 5.3 and Sections 6.1 to 6.3 we demonstrated that if a bankrupt individual transitions into formal employment after flag removal as opposed to prior to flag removal, they (a) earn more, (b) work for larger firms with greater non-wage benefits, and (c) do *not* borrow more than other transitioners (recall Section 5.3). We argue that these facts, taken together, point toward credit-checks as the mechanism generating the formal-employment flows we observe in our data.

The fact that workers disproportionately flow into larger firms after flag removal may be because of two reasons: (1) credit-checks may have previously been preventing these individuals from obtaining jobs at large firms, or (2) following flag removal access to consumer credit allows individuals to smooth consumption while searching for higher paying job at larger and more productive firms. Existing evidence from the UK (e.g. Zibarras and Woods [2010]) may support the credit-check explanation since small firms are less likely to conduct background checks.[17] On the other hand, related work by Herkenhoff et al. [2015] shows that displaced workers borrow more and take longer to find a job if they have more credit access, providing support for the consumption smoothing explanation. Unlike Herkenhoff et al. [2015], however, we assess the impact of credit access on all types of employment flows, and so we use a broad sample that includes very few displaced workers. For this broader sample in the current paper, Section 5.3 shows that those who transition into formal employment

[17] Zibarras and Woods [2010] show that micro firms (1-9 employees) are 45% less likely to conduct criminal background checks than very large firms (1000+ employees). Patterns in the middle of the size distribution are less clear. The probability of conducting a background check is 23.4% for micro firms vs. 34.1% for very large firms, however their sample sizes prevent them from statistically distinguishing between these two numbers.

after flag removal do not borrow disproportionately relative to other transitioners. This suggests that the consumption smoothing role is less important in the sample studied in the current paper, and individuals may be obtaining better jobs after flag removal because credit-checks by employers were previously limiting employment opportunities.

We are unable to observe credit check directly, and so we take our set of facts as supportive, but not conclusive, evidence of credit-checks limiting employment opportunities of bankrupt individuals.

# 7 Bankruptcy Policy Implications: Implied Productivity of Bankrupt Workers

Our results have important policy implications, especially for the debate over credit checks (Chen et al. [2013], Cortes et al. [2016], and Shoag and Clifford [2016]). In this section, we use the free entry condition from the canonical directed search model, e.g. Moen [1997] and Menzio and Shi [2011], to calculate firms' beliefs about the productivity of bankrupt workers relative to non-bankrupt workers. Using our wage and job-finding estimates from our empirical analysis, we show that firms would be willing to pay approximately \$17.6k, in net present value, to be able to decipher between a potential hire who has a bankruptcy record versus a potential hire with no bankruptcy record, ex-post. We are unaware of any comparable existing calculation, and our resulting estimates can be used by lawmakers and policymakers for cost-benefit analysis of credit-check bans.

Let $i \in \{B, N\}$ summarize the bankruptcy status of a worker at match formation (we can allow for alternate information structures or stochastic types), where $B$ denotes bankrupt and $N$ denotes non-bankrupt, e.g. $y_B$ is the productivity of a bankrupt worker and $y_N$ is the productivity of a non-bankrupt worker. Likewise, let $w_i$ denote the wage, and let $J_i$ denote the value of a firm matched with a worker that has bankruptcy status $i$ at match formation.

Let $\kappa$ denote the vacancy cost, and let $\theta_i$ denote market tightness of submarket $i$, i.e. $\theta_i = v_i/u_i$ where $v_i$ is the level of vacancies posted by firms in submarket $i$ and $u_i$ is the number of workers looking for a job in that submarket. The number of worker-firm matches that occur in submarket $i$ any given period is given by $M(u_i, v_i) = \frac{u_i \cdot v_i}{(u_i^\zeta + v_i^\zeta)^{1/\zeta}} \in [0, 1)$.

Define the firm contact rate in submarket $i$ as $q_i = \frac{M(u_i, v_i)}{v_i}$. The free entry condition

implies that the expected profits of matching ($q_i J_i$) in either submarket equals the vacancy cost ($\kappa$), $\kappa = q_i J_i$.[18]

Let $\delta_i$ be the associated transition rate out of formal employment. Assume that both productivity and wages are constant throughout the duration of a match so that $J_i$ can be expressed as $J_i = \frac{y_i - w_i}{1-\beta(1-\delta_i)}$. Using the free entry condition in conjunction with the expression for $J_i$ yields the implied productivity of a worker with bankruptcy status $i$,

$$y_i = w_i + \frac{\kappa(1-\beta(1-\delta_i))}{q_i} \tag{2}$$

How can $y_i$ be interpreted? $y_i$ is simply a residual. It may capture unmodeled costs or other unobserved characteristics of workers that affect profitability (tardiness, ability to handle money, etc.). In what follows, we will broadly interpret $y_i$ as firms' belief about the per-period productivity of a type $i$ worker.

## 7.1 Type Structure

Some discussion of the Bankrupt (B)/Non-bankrupt (N) type structure is warranted. We are not writing down the individual's problem, but the implicit assumption in the current formulation is that individuals switch types, and they only do so when they switch jobs. Bankrupt workers who remain in a match that was formed when the worker was initially bankrupt face a flat wage profile dictated by their status at match formation. This assumption is supported by the null average affects on wages shown in Appendix B, Table 14; only transitioners realize the wage gains shown in Table 12. This assumption may also be justified if employers do not repetitively check credit scores during a match. When an individual's flag is removed, the worker can switch jobs and search for a job in the 'non-bankrupt' submarkets.

[18] Screening costs are quite low for small businesses, ranging from \$30 to \$300 (depending on the range of checks conducted). E.g. www.businessnewsdaily.com/7638-best-background-check-services.html includes a list of providers of these services and costs. We could incorporate these costs in the model, but it would only complicate the calculation and have little influence on the results.

## 7.2 Calibration and Measurement

Let the period be annual, and let $\beta = .96$ (implying an annual interest rate of 4%). We set $\delta_N = .0559$ (=.044/.787) based on transition rate out of formal employment (into non-employment or self-employment) prior to bankruptcy flag removal.[19] We measure the vacancy cost following Hagedorn and Manovskii [2008] as 4.5% of the quarterly wage of a new hire, i.e. $\kappa = .045 w_B$.[20] We use the same matching elasticity, $\zeta = 1.6$, as Schaal [2012].

To measure the contact rate between firms and workers, we first recover the job finding rate for workers. The transition rate into formal employment is 4.6% per annum prior to flag removal, which, after adjusting for the fact that only 18.4% of the sample are non-employed and 9.0% are self-employed, yields an annual job finding rate of $16.79\% = (.046/(.184 + .09))$. Using the definition of the matching function, the worker contact rate is $p_B = M(u_B, v_B)/u_B = \theta_B(1+\theta_B)^{-1/\zeta} = .1679$ which implies $\theta_B = 0.1947$. Therefore the firm contact rate is $q_B = M(u_B, v_B)/v_B = (1+\theta_B)^{-1/\zeta} = .862$.

After flag removal, the flow transition rate into formal employment increases by .241% per annum, which, after adjusting for composition, translates into a 17.67% (=(.046+.00241)/(.184+.09)) annual job finding rate. The firm contact rate with non-bankrupt workers is therefore $q_N = .855$.

The final object that needs to be measured is the difference in wages of those who find jobs following flag removal versus those who find jobs with a flag still on their record. Using our wage gain estimates from Table 12, we measure the wage difference to be $w_N - w_B = \$1,696$. Using equation (2), we can now calculate the implied productivity difference:

$$y_N - y_B = \underbrace{w_N - w_B}_{\cong \$1,696} + \underbrace{\kappa\left(\frac{(1-\beta(1-\delta_N))}{q_N} - \frac{(1-\beta(1-\delta_B))}{q_B}\right)}_{\cong -40} = \$1656 \tag{3}$$

The relatively small differences in firm contact rates drives the second term in equation (3) to zero. The productivity difference is almost entirely summarized by the wage gap. In net present value the productivity difference in quite large, $\frac{y_N - y_B}{(1-\beta(1-\delta_N))} = \$17,676$. In absolute value, according to our calibration, non-bankrupt workers produce $43,387 per

[19] The 4.4% transition rate is the odds that any worker transitions out of employment. We adjust for the fact that only the formal-employed individuals can transition out of employment, and 78.7% of individuals in our sample are formally employed.

[20] Which wage we use as the base wage does not alter the results.

annum and bankrupt workers produce on average $41,732. Our productivity estimates imply that firms believe bankrupt workers are approximately 3.8% (=1.6k/43.3k) less productive than non-bankrupt workers. In net present value, firms in our sample would be willing to pay approximately $17.6k in order to avoid hiring a bankrupt worker, ex-post. However, free entry implies that firms are indifferent between hiring either type of worker, ex-ante.

# 8 Conclusions

We construct a new administrative dataset in order to examine how consumer credit access impacts employment prospects, earnings, and entrepreneurship. In particular, we use bankruptcy flag removals to isolate a large discrete increase in credit access which is not directly associated with credit worthiness, wealth, or any other unobserved characteristics of the individual. However, bankruptcy flag removals not only increase credit access, they also change the set of potential jobs available to a individual. We call these two effects from bankruptcy flag removal (i) the *credit access effect*, which is the way increased credit access following flag removal allows previously constrained individuals to start businesses or smooth consumption while searching for a job, and (ii) the *credit check effect*, which is the way bankruptcy flag removal gives individuals previously excluded from unemployment-insured jobs, which we call formal sector jobs, the opportunity to obtain a formal sector job.

We attempt to separate the two effects of bankruptcy flag removal using gross flows. We demonstrate that following flag removal there is (a) an increased flow rate into self-employment, (b) disproportionate borrowing by new self-employed entrants relative to other job-transitioners, (c) an increased likelihood of starting an employer business, and (d) disproportionate borrowing by new employer businesses. Taken together, we view these facts as strong evidence of the credit-access effect. On the other hand, we also study gross flows into new formal sectors jobs. Post-flag removal, entrants in the formal sector (a) earn more, (b) work for larger firms with greater non-wage benefits, but (c) do *not* necessarily borrow more. This last fact, in conjunction with limited evidence on background checks by firm size, points toward credit-checks preventing bankrupt workers from obtaining jobs at large firms.

Our results have important policy implications, especially for the debate over credit

checks (Chen et al. [2013], Cortes et al. [2016], and Shoag and Clifford [2016]). Using our empirical findings, we calibrate a directed search model and provide the first estimates of the implied productivity of bankrupt versus non-bankrupt workers. Our estimates reveal that firms would be willing to pay $17.6k, ex-post, in order to tell the difference between a bankrupt and non-bankrupt worker. In future work, which is beyond the scope of the paper, we believe the tools used in Chen et al. [2013] and Dávila [2014] can be used to assess the optimal information structure following bankruptcy.

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# Online Appendix, Not For Publication

"The Impact of Consumer Credit Access on Employment, Earnings and Entrepreneurship"

Herkenhoff, Phillips, and Cohen-Cole

July 15, 2016

# A Details on the Integrated Longitudinal Business Database (ILBD)

The ILBD merges two different databases. The first database is the Longitudinal Business Database (LBD) which tracks the universe of all U.S. establishments that have paid employees.[21] The second database is the universe of IRS non-employer tax records (i.e. those who fill out 1040 Schedule C tax returns). As soon as an entity hires a non-contractor, full-time employee, the business owner must obtain an EIN and will enter the LBD.[22] Davis et al. [2007] construct the ILBD using the SSN-EIN link found on the application for an EIN, and they also use exact business name matches. This yields a crosswalk between non-employers and the subsequent businesses they become.[23] We subsequently merge the ILBD using anonymized unique identifiers to our credit bureau data and the LEHD.

[21] "Overview: The Longitudinal Business Database (LBD) is a research dataset constructed at the Center for Economic Studies (CES) in the U.S. Census Bureau. The LBD contains the universe of all U.S. business establishments with paid employees listed in the Census Bureaus business register." http://maryannfeldman.web.unc.edu/data-sources/longitudinal-databases/longitudinal-business-database/

[22] According to the IRS, "As a business owner, when another person performs work for you, you must first correctly classify that person as an independent contractor or employee. If the person is an independent contractor, refer to Forms and Associated Taxes for Independent Contractors for your tax responsibilities. If the person is classified as an employee you must have an Employer Identification Number (EIN). Your tax responsibilities include withholding, depositing, reporting, and paying employment taxes. You must also give certain forms to your employees, they must give certain forms to you, and you must send certain forms to the IRS and SSA." https://www.irs.gov/businesses/small-businesses-self-employed/businesses-with-employees

[23] Quoting from Davis et al. [2007] "...we create a set of firm-level matches between employers and nonemployers for our selected industries. These matches rely on numeric identifiers and exact literal matches on business names. In matching on numeric identifiers, we exploit the fact that many business records contain both an EIN and an SSN. For example, when a business owner or officer applies for an EIN, he or she must fill out an SS-4 form for the IRS. This form includes the business name, the EIN and the SSN of the business owner or chief officer, all of which are included in Census Bureau business registers. These data allow us to build a crosswalk between EINs and SSNs, which we then use to match business records across universes... we rely only on the EIN-SSN crosswalk and exact literal matches on business name. As an example of how our matching algorithm works, consider all establishments with employees in our selected industries as of 2000. Using the longitudinal links in the LBD, we first create a set of identifiers (EINs, SSNs and business names) associated with each establishment with employees in 2000 for each year back to 1992... About 17 percent of our employer-nonemployer matches rely on exact literal matches on business name strings."

# B TransUnion/LEHD Additional Results: Earnings

Table 14 illustrates the baseline impact of bankruptcy flag removal on earnings, ignoring worker flows. Column (1) shows that real annual labor earnings exhibit a small significant downward trend that does not respond to bankruptcy flag removal. Similarly, Column (2) shows no significant trend in real annual self-employed net income, while Column (3) is the sum of the series in Columns (1) and (2). The overall trend is that real income is falling among these individuals, even though nominal earnings are fairly flat. However, as we will showed in the main text, individuals who transition into formal employment or self-employment following a bankruptcy flag removal have large earnings gains.

Table 14: Baseline Earnings

| | **(1)** Real Annual Labor Earnings | **(2)** Real Annual Self-Employed Net Income | **(3)** Real Annual Total Income (SE and Non-SE) |
|---|---|---|---|
| 2 Years Before Removal (d) | -27.08 | 28.92 | 1.843 |
| | (44.66) | (22.31) | (47.87) |
| 1 Year Before Removal (d) | -147.2** | -9.304 | -156.5** |
| | (61.80) | (29.40) | (65.46) |
| Year of Removal (d) | -259.9*** | 4.096 | -255.8*** |
| | (78.12) | (36.81) | (82.51) |
| 1+ Years After Removal (d) | -662.4*** | -37.86 | -700.3*** |
| | (96.31) | (44.82) | (101.5) |
| Individual Fixed Effects | Y | Y | Y |
| Year Fixed Effects | Y | Y | Y |
| Age and Tenure Controls | Y | Y | Y |
| R-squared | 0.121 | 0.003 | 0.097 |
| Indiv-Yr Obs. | 1.500e+06 | 1.500e+06 | 1.500e+06 |
| No. of Indiv. | 220000 | 220000 | 220000 |
| Sig Diff 1+Yr & -2Yr at 10% | Y | Y | Y |
| Sig Diff 0Yr & -2Yr at 10% | Y | N | Y |

*Notes: SE in parentheses. *** p<0.01, ** p<0.05, * p<0.1. Age and Tenure controls include quadratics in age and tenure. Fixed Effects include individual fixed effects and year dummies. Real Annual Total Income is the sum of Real Annual Labor Earnings and Real Annual Self-Employed Net Income.*

# C TransUnion/LEHD Additional Results: Transitions from Self-Employment to Formal Employment and Vice Versa

Table 15 includes the transition rates between self-employment and formal-employment, as well as the incidence of dual jobs and single jobs. To reduce noise we require the individual to be in their previous sector for at least 2 years before the transitioning. Column (1) illustrates an insignificant increase in the transition rate from formal employment to self-employment. Column (2) illustrates an increase in the transition rate from self-employment to formal-employment of .171% following flag removal; however, the transition rate moves prior to the flag removal. Column (3) shows that odds of being both formally employed and self employed increases following flag removal, but insignificantly. Likewise Column (3) shows that the odds of being only self-employed are hardly affected by flag removal. But Column (4) shows that the odds of being solely employed in a formal sector job increases by .44% following flag removal. This suggests that individuals are finding formal-sector jobs that allow them to quit their self-employed jobs.

Table 15: Transitions from Self-Employment to Formal Employment

| | **(1)** Transition from Formal-Employed (2yr) to Self-Employment, 1k (d) | **(2)** Transitions from Self-Employed (2yr) to Formal-Employed, 1k (d) | **(3)** Both SE and Formal-Employed (d) | **(4)** Only Self-employed (d) | **(5)** Only Formal-Employed (d) |
|---|---|---|---|---|---|
| 2 Years Before Removal (d) | 0.000173 | 0.000244 | -0.000208 | 0.000808* | 0.000516 |
| | (0.000614) | (0.000322) | (0.000636) | (0.000426) | (0.00102) |
| 1 Year Before Removal (d) | 0.000555 | 0.00114*** | -0.000309 | -7.45e-05 | 0.00185 |
| | (0.000756) | (0.000418) | (0.000807) | (0.000545) | (0.00134) |
| Year of Removal (d) | 0.00126 | 0.00126** | 0.000632 | 0.000318 | 0.00225 |
| | (0.000909) | (0.000506) | (0.000990) | (0.000677) | (0.00166) |
| 1+ Years After Removal (d) | 0.00137 | 0.00171*** | 0.000247 | 0.000830 | 0.00441** |
| | (0.00110) | (0.000619) | (0.00121) | (0.000832) | (0.00205) |
| 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 | 0.002 | 0.003 | 0.013 | 0.083 |
| No. Person-Yr Obs. | 1.500e+06 | 1.500e+06 | 1.500e+06 | 1.500e+06 | 1.500e+06 |
| No. of Indiv. | 220000 | 220000 | 220000 | 220000 | 220000 |

*Notes: SE in parentheses. *** p<0.01, ** p<0.05, * p<0.1. Age and Tenure controls include quadratics in age and tenure. Fixed Effects include individual fixed effects and year dummies.*

# D TransUnion/LEHD Additional Results: LBD Pay and Employment

Finally, Table 16 illustrates the impact of bankruptcy flag removal on payroll (in thousands of 2008 dollars) and employment for LBD firm owners. Column (1) of Table 16 shows that there is an increase in both LBD payrolls of $2.9k following flag removal among LBD firm owners, however this increase is insignificant at standard levels. Column (2) of Table 16 shows that there is an increase in employment of .66 employees (i.e. 2/3 of an employee on average) following flag removal among LBD firm owners, but again, this increase is insignificant. As we mentioned in the main text, the lack of power is presumably from the small fraction of bankrupt individuals who own employer-firms in the LBD, and in future research, we plan to explore the impact of credit access on hiring patterns in more detail with a broader sample of firm owners.

Table 16: LBD Employment and Pay

| | **(1)** LBD Pay | **(2)** LBD Employment |
|---|---|---|
| 2 Years Before Removal (d) | 0.105 | 0.00251 |
| | (0.118) | (0.00378) |
| 1 Year Before Removal (d) | 0.127 | 0.00240 |
| | (0.197) | (0.00476) |
| Year of Removal (d) | 0.242 | 0.00304 |
| | (0.207) | (0.00617) |
| 1+ Years After Removal (d) | 0.163 | 0.00123 |
| | (0.278) | (0.00642) |
| LBD Ownership, 2+ Yrs. (d) | -9.136*** | -0.316 |
| | (3.181) | (0.266) |
| 2 Yrs. Before Removal (d) x LBD Ownership, 2+ Yrs. (d) | 11.24 | -0.635 |
| | (12.12) | (0.564) |
| 1 Yr. Before Removal (d) x LBD Ownership, 2+ Yrs. (d) | 7.121 | -0.673 |
| | (6.496) | (0.704) |
| Yr. of Removal (d) x LBD Ownership, 2+ Yrs. (d) | 17.20 | -0.328 |
| | (12.90) | (0.637) |
| 1+ Yrs. After Removal (d) x LBD Ownership, 2+ Yrs. (d) | 14.12 | 0.0269 |
| | (12.54) | (0.302) |
| Individual Fixed Effects | Y | Y |
| Year Fixed Effects | Y | Y |
| Age and Tenure Controls | Y | Y |
| R-squared | 0.000 | 0.000 |
| No. Person-Yr Obs. | 1.500e+06 | 1.500e+06 |
| No. of Indiv. | 220000 | 220000 |
| Combined Coeff Diff 1+ Yrs & -2 Yrs | 2.94 | 0.66 |
| Combined Coeff Diff Sig at 10% | N | N |

*Notes: SE in parentheses. *** p<0.01, ** p<0.05, * p<0.1. Age and Tenure controls include quadratics in age and tenure. Fixed Effects include individual fixed effects and year dummies. LBD Pay measures the total payroll of the firm and is expressed in thousands of 2008 dollars. LBD employment refers to the number of workers employed at the firm.*

# E Formal Sector Exit Rates and Job Turnover

Are those who transition into formal employment marginal workers? To explore this question, we compute rates at which workers transit out of formal employment for those who find a new job after flag removal. In general, these newly employed workers are attached to the formal sector and are less likely to exit after flag removal. Table 17 illustrates job transitions among those who recently found a job. Column (1) of Table 17 shows that among those who transition into formal employment following flag removal, their odds of exiting formal employment in the following year actually declines slightly. Column (1) shows that individuals who transition into formal employment 1 or more years after bankruptcy flag removal are -.76% (= (-.0267+.0047)- (-.0153+.0009) ) less likely to subsequently exit formal sector employment than individuals who transition into formal employment 2 years prior to bankruptcy flag removal. We find a similar result using the $5k cutoff in Column (2). Column (3) shows that individuals who transition into formal employment 1 or more years after bankruptcy flag removal are 2.96% (= (.0526-.0009)- (.0226-.0005) ) more likely to start a new formal sector job than individuals who transition into formal employment 2 years prior to bankruptcy flag removal. The increased odds of switching to a new employer may reflect either (i) increased odds of being laid off, or (ii) climbing the job ladder. The persistent wage gains point to the latter explanation, but since the LEHD does not provide reason of separation, we will leave this to future research.

Table 17: Transitions Out of Formal Sector Employment and Between Employers

| | **(1)** Transition out of Formal Empl. Next Year, 1k (d) | **(2)** Transition out of Formal Empl. Next Year, 5k (d) | **(3)** New Formal Job Accession Next Year (d) |
|---|---|---|---|
| 2 Years Before Removal (d) | 0.000963 | 0.000921 | -0.000513 |
| | (0.000669) | (0.000699) | (0.00122) |
| 1 Year Before Removal (d) | 0.00150** | 0.00178** | -0.000469 |
| | (0.000759) | (0.000785) | (0.00141) |
| Year of Removal (d) | 0.00266*** | 0.00275*** | -0.00112 |
| | (0.000893) | (0.000917) | (0.00165) |
| 1+ Years After Removal (d) | 0.00476*** | 0.00408*** | -0.000948 |
| | (0.00109) | (0.00111) | (0.00201) |
| Transition into Formal-Employed, 1k (d) | 0.109*** | 0.0534*** | 0.00822*** |
| | (0.00227) | (0.00192) | (0.00259) |
| 2 Yrs. Before Removal (d) x Trans into Formal-Employed, 1k (d) | -0.0153*** | -0.00924** | 0.0226*** |
| | (0.00501) | (0.00428) | (0.00574) |
| 1 Yr. Before Removal (d) x Trans into Formal-Employed, 1k (d) | -0.0150*** | -0.00822* | 0.0213*** |
| | (0.00503) | (0.00429) | (0.00571) |
| Yr. of Removal (d) x Trans into Formal-Employed, 1k (d) | -0.0203*** | -0.00334 | 0.0186*** |
| | (0.00547) | (0.00475) | (0.00626) |
| 1+ Yrs. After Removal (d) x Trans into Formal-Employed, 1k (d) | -0.0267*** | -0.0187*** | 0.0526*** |
| | (0.00389) | (0.00330) | (0.00446) |
| Individual Fixed Effects | Y | Y | Y |
| Year Fixed Effects | Y | Y | Y |
| Age and Tenure Controls | Y | Y | Y |
| R-squared | 0.043 | 0.037 | 0.011 |
| No. Person-Yr Obs. | 1.500e+06 | 1.500e+06 | 1.500e+06 |
| No. of Indiv. | 220000 | 220000 | 220000 |
| Combined Coeff Diff 1+ Yrs & -2 Yrs | -0.76% | -0.63% | 2.96% |
| Combined Coeff Diff Sig at 10% | N | N | Y |

*Notes: SE in parentheses. *** p<0.01, ** p<0.05, * p<0.1. Age and Tenure controls include quadratics in age and tenure. Transition out of Formal Empl. Next Year, 1k (d) is a dummy that takes the value 1 if the individual earned at least $1k in the formal sector this year, and less than $1k next year. New Formal Job Accession Next Year (d) is a dummy that takes the value 1 if the individual becomes end-of-quarter employed at an SEIN they have never worked at previously.*

# F Verification of Results in Cross-Sectional Public Data: Survey of Consumer Finances 1998-2010

To compare our results to public data, we turn to the Survey of Consumer Finances (SCF) from 1998 to 2010. We consider households who have filed for bankruptcy in the last 19 years (to protect the identity of survey respondents, the year of bankruptcy filing is restricted to be 0-1, 2-3, 4-5, etc. and therefore so is the removal year). To keep the studies comparable, we limit ourselves to prime age (24-65) heads of household. Our main independent variable is years since flag removal which takes values from -9 to +9 (-9 is 9 years before flag removal).

## F.1 SCF Summary Statistics

Table 18 summarizes the sample of household heads used in the SCF analysis. Panel (A) describes the demographic characteristics of households. Approximately 27% have a college degree, the average age is 45, and the modal household head is white. Turning to Panel (B) which describes financial characteristics, on average, household heads with a prior bankruptcy record earn $65k per year (this is gross family income). Individuals have limited liquid asset positions but have relatively large (and skewed) illiquid asset positions. On average, households filed for bankruptcy 8 years ago, and roughly 48% of these households had credit denied when they applied. Bankcard limits total $8k and bankcard balances total $2k, despite the fact that many of these households have an active bankruptcy flag on their records. Panel (C) describes the employment and business ownership characteristics of households. Nearly 17% work for companies with less than 10 employees, and 12% own their own business.[24] Approximately 77% of the household heads are employed, and nearly 33% are working for employers with pensions or retirement plans.

## F.2 SCF Results

Table 19 illustrates a distributed lag model around bankruptcy flag removal. Column (1) and (2) study employed households only. The general pattern in Columns (1) and (2) is that after

[24] Our Census Self-employment Records cover 1040 Schedule C income, and so for comparability, we exclude SCF households who own multiple businesses are unlikely to be using 1040 Schedule C returns to report business income.

Table 18: Summary Statistics, Prime-Age Heads of Household with Prior Bankruptcy in Last 20 years (Source: 1998-2010 SCF)

| **(A) Demographic Characteristics** | | | |
|---|---|---|---|
| **Variable** | **Mean** | **p50** | **Std. Dev** |
| College Degree (d) | 0.26 | 0 | 0.44 |
| No College Degree (d) | 0.24 | 0 | 0.43 |
| 12 or Less Years of Education (d) | 0.50 | 1 | 0.50 |
| Age | 45.60 | 45 | 9.71 |
| White (d) | 0.72 | 1 | 0.45 |
| Black (d) | 0.17 | 0 | 0.37 |
| Hispanic (d) | 0.09 | 0 | 0.28 |

| **(B) Financial Characteristics** | | | |
|---|---|---|---|
| **Variable** | **Mean** | **p50** | **Std. Dev** |
| Income | 65628 | 40000 | 242598 |
| Liquid Assets to Income | 0.16 | 0.025 | 0.66 |
| Illiquid Assets to Income | 3.29 | 2.08 | 3.88 |
| Years Since Filing | 7.93 | 7 | 5.03 |
| Denied Credit (d) | 0.48 | 0 | 0.50 |
| Bankcard Limits, Combined | 8012 | 800 | 18916 |
| Bankcard Balance, Combined | 2099 | 0 | 5522 |

| **(C) Employment and Business Ownership** | | | |
|---|---|---|---|
| **Variable** | **Mean** | **p50** | **Std. Dev** |
| Work for Company with Less Than 10 Employees (d) | 0.17 | 0 | 0.38 |
| Single Firm Owner (d) | 0.12 | 0 | 0.33 |
| Employed (d) | 0.77 | 1 | 0.42 |
| Job Pension (d) | 0.33 | 0 | 0.47 |
| Observations | 1775 | | |

flag removal, households are finding jobs at larger firms where they are more likely to have benefits such as a pension; however, point estimates and patterns are unstable in Column (1). Column (3) shows that self-employment initially rises during bankruptcy and rises once again when the bankruptcy flag is removed. There are two competing forces at play: (i) the flag removal gives households access to more non-self-employment opportunities, and (ii) flag removal gives households access to more credit which may facilitate business formation and self-employment. In terms of employment, Column (4) shows that employment rises following flag removal. For credit related outcomes, Column (5) shows that credit denials fall after flag removal, Column (5) shows that bankcard limits expand, and Column (6) shows that bankcard balances rise.

The small sample sizes limit the types of inference and experiments that can be conducted; moreover many of the point estimates and patterns appear unstable. While this analysis is merely describing correlations, we take the patterns to be consistent with the patterns observed in our LEHD/TransUnion dataset.

Table 19: Distributed lags around bankruptcy flag removal. Dependent variables are (1) Job Benefits (2) Size of Firm Employee Works for (3) Self-Employment (4) Employment (5) Non-Employment (6) Loan Denial, (7) Credit Limits, and (8) Credit Balances. (Source: 1998-2010 SCF)

| | **(1)** Job Pension (d) | **(2)** Work for Company with Less Than 10 Employees (d) | **(3)** Single Firm Owner (d) | **(4)** Employed (d) | **(5)** Denied Credit (d) | **(6)** Bankcard Limits, Combined | **(7)** Bankcard Balance, Combined |
|---|---|---|---|---|---|---|---|
| 7 Years Before Flag Removal | -0.025 | 0.046 | 0.042 | 0.020 | 0.039 | 661.0 | -19.3 |
| | (-0.49) | (1.40) | (1.62) | (0.57) | (0.90) | (0.77) | (-0.05) |
| 5 Years Before Flag Removal | -0.012 | 0.053 | 0.054* | -0.016 | -0.006 | 3,025.5*** | 620.0 |
| | (-0.22) | (1.45) | (1.93) | (-0.41) | (-0.14) | (3.07) | (1.52) |
| 3 Years Before Flag Removal | -0.039 | 0.044 | 0.033 | 0.023 | -0.033 | 2,276.3** | 735.7* |
| | (-0.73) | (1.32) | (1.22) | (0.64) | (-0.73) | (2.50) | (1.90) |
| 1 Year Before Flag Removal | -0.085 | 0.100** | 0.018 | 0.029 | 0.001 | 3,256.9*** | 573.7 |
| | (-1.50) | (2.54) | (0.65) | (0.75) | (0.03) | (3.04) | (1.38) |
| 1 Year After Flag Removal | 0.045 | 0.092** | 0.061** | 0.063* | -0.073 | 5,809.6*** | 1,856.5*** |
| | (0.82) | (2.51) | (2.10) | (1.71) | (-1.57) | (4.93) | (3.51) |
| 3 Years After Flag Removal | 0.097 | 0.067 | 0.066* | 0.023 | -0.089* | 9,612.6*** | 2,748.7*** |
| | (1.57) | (1.61) | (1.91) | (0.51) | (-1.65) | (4.80) | (3.92) |
| 5 Years After Flag Removal | -0.034 | 0.050 | 0.011 | -0.047 | -0.123** | 8,804.7*** | 1,535.7*** |
| | (-0.47) | (1.19) | (0.32) | (-0.95) | (-2.25) | (3.88) | (2.68) |
| 7 Years After Flag Removal | 0.154* | 0.001 | 0.024 | -0.094 | -0.143** | 14,721.4*** | 3,131.1** |
| | (1.65) | (0.03) | (0.53) | (-1.50) | (-2.20) | (3.36) | (2.32) |
| 9 Years After Flag Removal | -0.082 | 0.054 | 0.096* | 0.075 | -0.055 | 7,190.5*** | 2,081.9** |
| | (-0.93) | (0.96) | (1.77) | (1.29) | (-0.76) | (2.65) | (2.06) |
| Demographic Controls | Y | Y | Y | Y | Y | Y | Y |
| Year Dummies | Y | Y | Y | Y | Y | Y | Y |
| Wealth Controls | Y | Y | Y | Y | Y | Y | Y |
| Employed Households Only | Y | Y | N | N | N | N | N |
| Observations | 1,167 | 1,167 | 1,775 | 1,775 | 1,775 | 1,775 | 1,775 |
| R-squared | 0.051 | 0.036 | 0.077 | 0.110 | 0.053 | 0.287 | 0.069 |

Notes: Robust t-statistics in parentheses. *** p<0.01, ** p<0.05, * p<0.1. Demographic controls include a quadratic in age, race dummies, and education dummies. Wealth controls include liquid assets to income, illiquid assets to income, and income.

# G Alternate Empirical Design: Non-Overlapping Treatment and Control Groups with Sample Window from 2001-2005

For robustness, we employ an alternate empirical strategy designed to address concerns about bankruptcy reform. We compare previously bankrupt individuals before and after the bankruptcy flag removal to a subset of individuals that **never** have their flags removed in our sample period, i.e. we implement a difference-in-difference analysis with non-overlapping treatment and control groups. In particular, our sample window is 2001-2005, and we always restrict our attention to 24-65 year olds. The two groups of individuals we compare are labeled the treatment group, for those whose bankruptcy flags are removed, and control group, for those whose bankruptcy flags are not removed.

- **Treatment Group:** Flag removals between 2001 and 2005 (the earliest date we can identify a flag removal is 2002 due to data limitations).
- **Control Group:** Flag removals between 2006 and 2010.

Let $i$ index individuals and $t$ index years (from 2001 to 2005). Let $\alpha_i$ denote a set of individual fixed effects, and $\gamma_t$ denote year dummies. Let $Y_{i,t}$ denote the outcome of interest (a self employment dummy, earnings, wages, etc.). Let $D_{x,i,t}$ be a dummy variable taking the value 1 when a member of the treatment group is $x$ periods before (if $x$ is negative) or after (if $x$ is positive) flag removal. E.g. $D_{-2,i,t}$ is a dummy indicating if a treated individual is 2 periods before flag removal, likewise $D_{0,i,t}$ takes a value of 1 if the treated individual is in the year of flag removal, and $D_{1+,i,t}$ takes a value of 1 if the treated individual is 1 or more years past flag removal. The specifications we run are of the following form:

$$Y_{i,t} = \alpha_i + \gamma_t + \beta_{-2}D_{-2,i,t} + \beta_{-1}D_{-1,i,t} + \beta_0 D_{0,i,t} + \beta_{1+}D_{1+,i,t} + \Gamma X_{i,t} + \epsilon_{i,t} \tag{4}$$

## G.1 Alternate Empirical Design: Summary Stats

Table 20 describes the means and standard deviations of several key variables across the treatment ('Flag Drop') and control group ('No Flag Drop'), as of 2001.[25] As the table reveals, several of the mean values are significantly different across these two groups; however, what we show in the sections that follow, and what is essential for identification, is that the *trends* in these variables are parallel.[26] In the sections that follow, our analysis reveals parallel trends for the majority of outcome variables.

The variables in Table 20 are identical to those in Table 1, except the the alternate definitions of self-employment and formal employment. Since our credit reports are measured in September of each year, we may mistakenly classify a removal as occurring in the present year when in fact it had occured between October-December of the prior year.[27] To address any concerns that there may be people who transition right away into self-employment or formal-employment following an October flag removal, we also use an alternate definition of self-employment and formal employment (labeled in the tables as 'Self Employed, Alternate Def. (d)' and 'Formal-Employment, Alternate Def. (d)') which counts the individual as self-employed if they earned at least $1k in Schedule C income last year and $1k Schedule C income this year. Likewise, we count a individual as formally employed under this alternate definition if they earned at least $1k in labor earnings last year and $1k labor earnings this year.

[25] Less than 5% of the panel enter at a later date, and in those situations we report the first observed value in the summary statistics.

[26] Due to the large size of the sample, even small differences in levels are statistically significant, even if they are economically insignificant.

[27] This is a problem, if for example, in September 2005 an individual has a bankruptcy flag on record and in September 2006 they do not; we would mark flag removal to occur in year 2006, however it may have been removed in October 2005, and they then immediately started a business.

Table 20: Alternate Empirical Design: Summary Statistics as of 2001, Treatment (Flag Drop) and Control (No Flag Drop) Groups

| | (A) Employment Levels and Flows | | | | |
|---|---|---|---|---|---|
| | Flag Drop | | No Flag Drop | | |
| | Mean | SD | Mean | SD | Diff. Means p<.05 |
| Self-Employed, 1k (d) | 8.1% | 0.27 | 7.4% | 0.26 | * |
| Self-Employed, Alternate Def. (d) | 5.4% | 0.23 | 4.6% | 0.21 | * |
| Formal-Employed, 1k (d) | 78.7% | 0.41 | 78.8% | 0.41 | |
| Formal-Employed, Alternate Def. (d) | 74.3% | 0.44 | 74.2% | 0.44 | |
| Both SE and Formal-Employed (d) | 5.7% | 0.23 | 5.2% | 0.22 | * |
| Non-Employed, 1k (d) | 18.9% | 0.39 | 19.0% | 0.39 | |
| Only Self-employed, 1k (d) | 2.4% | 0.15 | 2.2% | 0.15 | * |
| Only Formal-Employed, 1k (d) | 73.0% | 0.44 | 73.6% | 0.44 | * |
| Transition into Self-Employed, 1k (d) | 2.8% | 0.16 | 2.8% | 0.16 | |
| Transition out of Self-Employed, 1k (d) | 2.5% | 0.16 | 2.6% | 0.16 | |
| Transition into Formal-Employed, 1k (d) | 4.4% | 0.21 | 4.6% | 0.21 | * |
| Transition out of Formal-Employed, 1k (d) | 4.4% | 0.21 | 4.7% | 0.21 | * |
| | **(B) Earnings Characteristics** | | | | |
| | Flag Drop | | No Flag Drop | | |
| | Mean | SD | Mean | SD | Diff. Means p<.05 |
| Formal Earnings Q4 | $6,997.5 | 11209.3 | $6,436.7 | 7719.7 | * |
| Real Annual Labor Earnings (Adjusted for 0s: $41k) | $32,097.5 | 28704.7 | $29,759.9 | 26482.2 | * |
| Real Annual Self-Employed Net Income (Adjusted for 0s: $24k) | $2,009.2 | 9893.8 | $1,631.8 | 8645.0 | * |
| Real Total Annual Income (SE and Non-SE) | $34,106.7 | 29744.4 | $31,391.8 | 27291.8 | * |
| Self-Employed Income to Total Income | 0.041 | 0.18 | 0.037 | 0.17 | * |
| Arc Total Earnings Growth | -0.059 | 0.60 | -0.056 | 0.64 | |
| | **(C) Credit Characteristics** | | | | |
| | Flag Drop | | No Flag Drop | | |
| | Mean | SD | Mean | SD | Diff. Means p<.05 |
| Bankruptcy Score | 262.7 | 181.8 | 153.9 | 104.1 | * |
| Number of Accounts Opened | 1.6 | 1.8 | 1.3 | 1.5 | * |
| Real Bankcard Balance | $3,311.2 | 5681.6 | $1,891.4 | 3820.0 | * |
| Real Revolving Balance | $4,809.5 | 9022.4 | $2,627.1 | 5988.2 | * |
| Real Installment Balance | $17,513.5 | 26472.7 | $13,843.3 | 22543.1 | * |
| Real Mortgage Balance | $55,166.1 | 98186.3 | $37,271.4 | 76750.1 | * |
| Real HELOC Balance | $684.9 | 6813.7 | $368.8 | 4786.9 | * |
| | **(D) Demographics and Employer Characteristics** | | | | |
| | Flag Drop | | No Flag Drop | | |
| | Mean | SD | Mean | SD | Diff. Means p<.05 |
| Age | 42.4 | 8.9 | 40.2 | 9.4 | * |
| Imputed Years of Education | 13.7 | 2.6 | 13.6 | 2.6 | * |
| Tenure | 2.4 | 2.3 | 2.3 | 2.3 | * |
| Unemployment Rate | 4.3 | 0.8 | 4.5 | 1.0 | * |
| Employer Size ≥1000 (d) | 0.243 | 0.429 | 0.244 | 0.43 | |
| Employer Size ≥500 (d) | 0.305 | 0.46 | 0.306 | 0.461 | |
| Number of Observations | 90000 | | 140000 | | |

*Notes: Formal sector employment refers to those who earned at least $1k in a UI insured job covered by the LEHD. Self-Employment refers to those who earned at least $1k on their 1040 Schedule C. The Alternative Definition of Formal sector employment refers to those who earned at least $1k in a UI insured job covered by the LEHD for 2 consecutive years. The Alternative Definition of Self-Employment refers to those who earned at least $1k on their 1040 Schedule C for 2 consecutive years. Transitions defined as earnings more than $1k dollars in a sector in which you previously earned zero. Number of accounts opened refers to accounts opened in last 12 months.*

## G.2 Alternate Empirical Design: Stocks and Flows of Employment and Self-Employment

Table 21 illustrates the baseline 'stock' or levels of employment results using our alternate empirical design and using alternate definitions of formal and self employment. Table 21 generates very similar results to Table 2, and the alternate definitions of formal and self-employment results are similar to the baseline definitions. Following flag removal formal-employment under the alternate definition increases by .655%, and self-employment under the alternate definition remains flat. Table 22 and 23 illustrate the corresponding self-employment flows and formal employment flows for our alternate empirical design. Self-employment flows under the alternate definition increase by .2%. Flows out of self-employment under the alternate definition increase, but the increase is insignificant. In both tables, however, the broad pattern is the same: flows into self and formal employment increase, and flows out of self-employment increase (weakly). Generally, Table 22 and 23 support the results shown in Tables 5 and 11 shown in the main text.

Table 21: Alternate Empirical Design: Employment Levels and Bankruptcy Scores

| | **(1)** Bankruptcy Score | **(2)** Bankruptcy Score | **(3)** Formal-Employed (d) | **(4)** Formal-Employed, Alternate Def. (d) | **(5)** Self-Employed (d) | **(6)** Self-Employed, Alternate Def. (d) |
|---|---|---|---|---|---|---|
| 2 Years Before Removal (d) | 77.04*** | -13.18*** | 6.45e-05 | 0.000457 | 0.00175 | 0.000299 |
| | (0.732) | (0.755) | (0.00150) | (0.00140) | (0.00117) | (0.000916) |
| 1 Year Before Removal (d) | 80.50*** | -26.04*** | 0.00157 | 0.00116 | 0.000554 | 4.99e-05 |
| | (0.655) | (0.887) | (0.00174) | (0.00166) | (0.00132) | (0.00109) |
| Year of Removal (d) | 147.2*** | 29.45*** | 0.00397** | 0.00353* | 0.00170 | -0.00107 |
| | (0.798) | (1.047) | (0.00193) | (0.00185) | (0.00147) | (0.00122) |
| 1+ Years After Removal (d) | 110.1*** | -34.51*** | 0.00623*** | 0.00655*** | 0.00320* | 0.00126 |
| | (0.684) | (1.162) | (0.00222) | (0.00213) | (0.00168) | (0.00140) |
| Fixed Effects (Individual and Year) | N | Y | Y | Y | Y | Y |
| Age and Tenure Controls | N | Y | Y | Y | Y | Y |
| R-Squared | 0.120 | 0.122 | 0.113 | 0.221 | 0.002 | 0.003 |
| Indiv.-Yr Obs | 1,150,000 | 1,150,000 | 1,150,000 | 1,150,000 | 1,150,000 | 1,150,000 |
| Number of Indiv. | 240,000 | 240,000 | 240,000 | 240,000 | 240,000 | 240,000 |

*Notes: SE in parentheses. *** p<0.01, ** p<0.05, * p<0.1. Age and Tenure controls include quadratics in age and tenure. Fixed Effects include individual fixed effects and year dummies.*

Table 22: Alternate Empirical Design: Self-Employment Flows

| | **(1)** Transition into Self-Employed (d) | **(2)** Transition out of Self-Employed (d) | **(3)** Transition into Self-Employed, Alternate Def (d) | **(4)** Transition out of Self-Employed, Alternate Def (d) |
|---|---|---|---|---|
| 2 Years Before Removal (d) | 0.00145 | 0.000755 | 0.000228 | 0.000301 |
| | (0.000993) | (0.000929) | (0.000722) | (0.000667) |
| 1 Year Before Removal (d) | 0.000504 | 0.00196** | 0.00107 | 0.000647 |
| | (0.000969) | (0.000924) | (0.000745) | (0.000675) |
| Year of Removal (d) | 0.00278*** | 0.00272*** | -8.44e-05 | 0.000901 |
| | (0.00104) | (0.000988) | (0.000775) | (0.000721) |
| 1+ Years After Removal (d) | 0.00194* | 0.00271** | 0.00205** | 0.000660 |
| | (0.00117) | (0.00111) | (0.000895) | (0.000809) |
| Fixed Effects (Individual and Year) | Y | Y | Y | Y |
| Age and Tenure Controls | Y | Y | Y | Y |
| R-Squared | 0.000 | 0.000 | 0.001 | 0.000 |
| Indiv.-Yr Obs | 1,150,000 | 1,150,000 | 1,150,000 | 1,150,000 |
| Number of Indiv. | 240,000 | 240,000 | 240,000 | 240,000 |

*Notes: SE in parentheses. *** p<0.01, ** p<0.05, * p<0.1. Age and Tenure controls include quadratics in age and tenure. Fixed Effects include individual fixed effects and year dummies. Transition into self-employed if SE annual earnings greater than $1k this year, zero last year. Transition out of self-employed if SE annual earnings greater than $1k last year, zero this year.*

Table 23: Alternate Empirical Design: Formal-Employment Flows

| | **(1)** Transition into Formal-Employed (d) | **(2)** Transition out of Formal-Employed (d) | **(3)** Transition into Formal-Employed, Alternate Def. (d) | **(4)** Transition out of Formal-Employed, Alternate Def. (d) |
|---|---|---|---|---|
| 2 Years Before Removal (d) | -0.000392 | 0.00110 | 0.00138 | 0.000427 |
| | (0.00118) | (0.00126) | (0.00114) | (0.00116) |
| 1 Year Before Removal (d) | 0.000413 | 0.000474 | 0.000970 | 1.20e-05 |
| | (0.00118) | (0.00124) | (0.00114) | (0.00116) |
| Year of Removal (d) | 0.000437 | 0.000679 | 0.00258** | 0.000395 |
| | (0.00125) | (0.00131) | (0.00121) | (0.00122) |
| 1+ Years After Removal (d) | -0.000325 | 0.00238 | 0.00263* | 0.00162 |
| | (0.00143) | (0.00149) | (0.00138) | (0.00139) |
| Fixed Effects (Individual and Year) | Y | Y | Y | Y |
| Age and Tenure Controls | Y | Y | Y | Y |
| R-Squared | 0.025 | 0.032 | 0.002 | 0.037 |
| Indiv.-Yr Obs | 1,150,000 | 1,150,000 | 1,150,000 | 1,150,000 |
| Number of Indiv. | 240,000 | 240,000 | 240,000 | 240,000 |

*Notes: SE in parentheses. *** p<0.01, ** p<0.05, * p<0.1. Age and Tenure controls include quadratics in age and tenure. Fixed Effects include individual fixed effects and year dummies. Transition into self-employed if SE annual earnings greater than $1k this year, zero last year. Transition out of self-employed if SE annual earnings greater than $1k last year, zero this year.*

# H Tests for Selection Among Self-Employed Transitioners

In this appendix, we discuss the role of selection for the transition results. Since flag removal is forseeable, there is concern that better entrepreneurs who anticipate the need for credit deliberately wait until the flag is removed to start a business. However, such selection still suggests credit plays an important role for business startups. Nonetheless, we show that the entrepreneurs who transition into self-employment following flag removal are identical (or slightly worse) in terms of 1, 2, and 3 year lags of annual labor earnings relative to those who do not flow into self-employment. This set of results suggests that the new self-employed individuals have similar labor productivity. We repeat the same exercise using 1, 2, and 3 year lags of annual self-employed income. Again we find that those who transition into self-employment have very similar levels of previous self-employed earnings (i.e. they are not people who have previously failed disproportionately or succeeded disproportionately at entrepreneurship in the past). This suggests that they have similar prior levels of entrepreneurial talent. However, we cannot definitively rule out differences in unobserved talent.

Table 24 provides the signs and significance levels from these regressions. Regression coefficients are available upon request.

Table 24: Measures of selection for those who transition into self-employment. Regressions of past labor earnings and self-employed earnings on transition dummies.

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| | 1 Yr. Lagged Labor Earnings | 2 Yr. Lagged Labor Earnings | 3 Yr. Lagged Labor Earnings | 1 Yr. Lagged Self-Employed Income | 2 Yr. Lagged Self-Employed Income | 3 Yr. Lagged Self-Employed Income |
| 2 Years Before Removal (d) | n | p* | p | p | p | p |
| 1 Year Before Removal (d) | n | p | p** | p** | p | p |
| Year of Removal (d) | n | p | p | p* | p | p |
| 1+ Years After Removal (d) | n* | n | p | p | p | p |
| Transition Into Self-Employed (d) | p | p | p* | n*** | n*** | n |
| 2 Yrs. Before Removal (d) x Trans Into Self-Empl., 1k (d) | p | n | n | n | p | n |
| 1 Yr. Before Removal (d) x Trans Into Self-Empl., 1k (d) | p | n | n | n | p*** | n |
| Yr. of Removal (d) x Trans Into Self-Empl., 1k (d) | p | n | n | n | n | n |
| 1+ Yrs. After Removal (d) x Trans Into Self-Empl., 1k (d) | n | n | n | p | p** | p |
| Individual Fixed Effects | Y | Y | Y | Y | Y | Y |
| Year Fixed Effects | Y | Y | Y | Y | Y | Y |
| Age and Tenure Controls | Y | Y | Y | Y | Y | Y |

*Notes: *** p<0.01, ** p<0.05, * p<0.1. Age and Tenure controls include quadratics in age and tenure. The letter 'p' indicates a positive coefficient, and the letter 'n' indicates a negative coefficient.*