Personal Wealth and Self-Employment*

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Abstract

We examine the effect of wealth windfalls on self-employment decisions using a novel data set on unexpected payments to individuals from the fracking revolution in Texas. Individuals who receive large wealth shocks (greater than $50,000) have 59% greater self-employment rates relative to individuals who receive small wealth shocks or no wealth shock. We evaluate several economic channels that could drive these results and find that wealth shocks do not alleviate entrepreneurial financial constraints or reduce risk aversion, but drive self-employment through non-pecuniary channels, such as leisure or job autonomy. Our results indicate that heterogeneity in self-employment types is important when assessing the impact of entrepreneurship on the broader economy.

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1 Introduction

Economists have long sought to estimate how labor supply responds to changes in personal wealth. Within this literature, the role of wealth changes on the self-employment decision is of particular interest because self-employment spells are potentially important for entrepreneurship and are prevalent. Indeed, forty percent of U.S. workers experience at least one period of self-employment during their lifetime (Astebro et al. (2014) and Parker (2009)). At the same time, the impacts of wealth on self-employment and its nature are not well understood. On one hand, a strand of the entrepreneurship literature has argued that greater personal wealth alleviates liquidity constraints for self-employment and business formation.[1] On the other hand, recent work by Levine and Rubinstein (2017) shows that self-employment attracts a mix of different types of entrepreneurs: those with high-growth aspirations and those that are motivated by non-pecuniary benefits, such as leisure. Given this tension, we evaluate how large cash windfalls from shale natural gas discoveries affect self-employment and its nature in a broad sample of American workers. Our findings cast doubt on the notion that self-employment has general benefits for the economy, and suggest that a better understanding of entrepreneurship types is critical for evaluating the importance of self-employment.

Identifying the effect of wealth on self-employment decisions is empirically challenging. First, data on self-employment spells is difficult to obtain, as individuals engaged in entrepreneurship span a wide range of economic activities, ranging from freelancers to technology firm entrepreneurs with venture capital backing. Second, wealth and employment decisions are jointly determined, which makes it difficult to identify a causal impact of wealth on employment decisions. Third, even if a relationship can be identified, it is difficult to isolate the economic channels driving the relationship. That is, it is hard to identify whether the impact of wealth is due to alleviating liquidity constraints, reducing risk aversion, enabling an individual's preference for a leisure job, or some other factor.

In this paper, we examine a setting that allows us to make progress on each of these empirical challenges. Specifically, we study how shale natural gas mineral windfalls affect decisions to engage in self-employment. Our data consist of individual-level payments to people from shale gas extraction, merged with data on employment (self-employment versus employment, and in what

[1] Examples from this literature include Evans and Jovanovic (1989), Cagetti and De Nardi (2006), Lindh and Ohlsson (1996).

industry) from Experian. As the fracking revolution was unexpected and mineral rights are held diffusely across the population, this setting enables us to cleanly identify how wealth windfalls affect self-employment decisions for a broadly-relevant sample of American workers. In support of empirical strategy, we provide evidence that the mineral windfalls we employ for identification are plausibly exogenous to other factors that might matter for self-employment decisions, and importantly, the distinction between individuals who receive large payments (greater than $50,000) versus small payments is completely external to factors chosen by the individual (i.e., extraction companies determine the timing and intensity of drilling, not the mineral leaseholders we study). Lastly, we use the credit attributes available from the credit bureau to assess the role of different economic channels in affecting self employment decisions.

We find that wealth windfalls have economically significant effects on the decision to become self-employed. Specifically, individuals who receive large windfalls in excess of $50,000 have a 59% greater self-employment rate compared to a control sample of individuals who receive smaller payments or no payments, even after accounting for other important characteristics that likely determine self-employment decisions (i.e., geography, age and income bin fixed effects, credit scores, mineral acreage owned). We find similar effects both for mineral owners that reside in the area of shale development as well as for those that live in other areas, suggesting that local business investment opportunities linked with shale are unlikely to be the driver of this result. Figures 1 and 2 plot the location of our mineral owners across the United States.

We evaluate several different economic forces that could drive the self-employment decisions we observe. First, entrepreneurs may be financially constrained. That is, they may have a positive net present value (NPV) project, but lack access to funding to pursue it (Evans and Jovanovic (1989), Cagetti and De Nardi (2006)). Second, self-employment could be viewed as a risky activity. If individuals are risk averse and if risk aversion is a function of wealth, then higher wealth may result in transitions into entrepreneurship (Kihlstrom and Laffont (1979); Hvide and Panos (2014)). Lastly, increased wealth could alter an individual's preference for leisure and other non-pecuniary benefits (Hurst and Lusardi (2004)). We construct tests to evaluate support for each of these economic channels.

To assess whether the wealth shocks in our study affect self-employment by alleviating a binding liquidity constraint, we test whether the effect of wealth is more pronounced in sub-samples in which entrepreneurs are more or less liquidity constrained. Specifically, we identify two proxies for individuals that have binding liquidity constraints – whether the individual has initially-subprime credit (credit score < 620), or if the individual has high debt-to-income. According to the financial constraints view, the influx of wealth is likely to matter more for these more constrained borrowers. Thus, we should see liquidity-constrained individuals enter self-employment at greater rates for constrained versus unconstrained individuals. We find constrained individuals do not enter self-employment at higher rates, and if anything, they are less likely to become entrepreneurs after receiving the wealth shock than unconstrained individuals. This result contrasts with the financial constraints view of entrepreneurship, and is consistent with recent evidence that ample bank financing to entrepreneurs is available, as documented by Robb and Robinson (2014).

Next, we evaluate whether reduced risk aversion can explain the substantial impact of wealth on self-employment rates by examining transitions into and out of self-employment. According to the "reduced risk aversion" view, self-employment rates increase after an influx of wealth because individuals now have a financial buffer that enables them to quit their job. In this way, reduced risk aversion makes the prediction that the self-employment rate increases – in large part – because more people transition from employed to self-employed. In contrast to this view, we estimate a precise zero effect of wealth on the transition rate from employed to self-employed. That is, self-employment rates increase with large wealth shocks because the influx of wealth enables individuals who already chose to be self-employed to extend their self-employment spells for longer. Indeed, in some specifications, we even find that large wealth shocks to individuals currently in normal employment, actually make an individual less likely to be-self employed.

Finally, we undertake several tests to evaluate whether the higher self-employment rates we identify are related to individual preferences for leisure. We find that, once individuals stop receiving shale royalty payments, they tend to switch back to normal employment, consistent with the idea that shale royalty payments were subsidizing their income in a way that allowed them to be self-employed. This evidence also supports the view that the wealth shocks were not being used to fund self-sustaining or otherwise productive projects. We also find evidence that individuals who choose to remain self-employed after receiving a mineral windfall have lower income than individuals who transition to normal employment. Taken together, these results suggest that mineral windfalls, and wealth more broadly, are being used to subsidize marginally successful businesses, consistent with leisure and non-pecuniary benefits of entrepreneurship.

An important alternative view on the apparent low returns to self-employment is that there are substantial gains to experimentation in self-employment – either through learning about uncertain self-employment earnings or learning valuable lessons that can be applied upon returning to regular employment. Allowing for experimentation, the low average earnings of self-employed individuals cannot be interpreted as evidence of non-pecuniary motives (Manso (2016); Dillon and Stanton (2017)). To speak to this issue, we estimate the effect of wealth windfalls on self-employment rates separately by different age cohorts. We find that there is a flat life-cycle profile of the effect of wealth on self-employment, indicating that the effect on self-employment is not concentrated among younger individuals for whom the gains to experimentation are greatest. Indeed, we contrast this profile with retirement decisions to assess whether self-employment and retirement are capturing distinct decisions and we find large and striking effects of wealth windfalls on retirement propensities for individuals nearing or exceeding normal retirement age (age 65) as opposed to the flat profile of self-employment. Taken together, these findings provide novel evidence that wealth windfalls lead to self-employment through an individual's non-pecuniary motives.

We also test whether there is variation in the effect of wealth shocks on self-employment across different industry types and education levels. Overall, we find that the effect of wealth on self-employment rates is similar across different industries and for individuals across different education levels. This is further evidence that wealth shocks in our setting are unrelelated to high growth entrepreneurship. A high growth entrepreneurship hypothesis would have predicted outsized effects in industries that are high growth, as well as individuals with greater human capital (high education levels). However, we estimate broad based effects across industries and education levels , which suggests that high human capital entrepreneurship is not the primary driver of our results.

Our paper relates to several strands of literature. Existing research on entrepreneurship has tended to equate self-employment with entrepreneurship (Glaeser (2007)). However, recent research has suggested that different self-employment types may be important in understanding broader macro trends, as well as policies and initiatives which are designed to support entrepreneurship. For example, Levine and Rubinstein (2017) present some evidence that self-employment selects two types of entrepreneurs: those who value leisure and those who are more likely to generate high-growth entrepreneurship. Empirically, however, it is difficult to know which of these types of entrepreneurship dominates. Our paper provides evidence regarding how wealth shocks, a central feature of models of entrepreneurial behavior, relate to these self-employment types.

More broadly, our paper helps reconcile several existing findings in the literature. For example, on the one hand, access to wealth and collateral has been shown to increase entrepreneurship rates (Adelino, Schoar, and Severino (2015) and Lindh and Ohlsson (1996)), but on the other hand, other papers have found evidence that bank debt financing is readily available to entrepreneurs (Robb and Robinson (2014)). Our findings suggest that the type of entrepreneur is important when considering these relationships, and that wealth-collateral effects may be most salient when focusing on settings that have a substantial composition of individuals who may classify as unincorporated self-employed, and that such self-employment decisions are more likely to be driven by leisure preferences than liquidity constraints.

In addition, our paper contributes to the literature by providing novel evidence on how individual wealth shocks, distinct from local economic activity, affect entrepreneurial outcomes, as well as labor supply more broadly (e.g., Cesarini et al. (2017); Jones and Marinescu (2018)). Specific to entrepreneurial activity, recent work on self-employment tends to exploit regional shocks to identify the effect of wealth on self-employment (e.g., real estate wealth as in Adelino, Schoar, and Severino (2015) or regional windfalls as in Bermejo et al. (2019)). However, as our tests that rely on windfalls to out-of-area individuals show, the self-employment effects we observe are not driven by improvements to local economic opportunities. The fact that we observe individual wealth shocks is an advantage that is shared by Lindh and Ohlsson (1996), who study how individual lottery windfalls affect self-employment using Swedish microdata, but our setting enables us to study how the size of the wealth shocks and their timing relate to self-employment decisions (and whether these decisions are moderated by personal financial constraints). As such, our results shed unique insight into the individual mechanisms behind self-employment choices, distinct from regional development.

Separately, our analysis of a broad sample of individuals – those who start in typical employment arrangements as well as those who are initially self-employed – is rather unique in the literature, which frequently analyzes situations in which individuals are selected on either being an entrepreneur at the beginning of the sample or eventually transitioning into entrepreneurial work.

For example, our paper relates to Cespedes, Huang, and Parra (2019) who show that existing business owners/entrepreneurs (proprietors of stores that sell lottery tickets) use unexpected windfalls to start new businesses. Our evidence from a broad sample of potential entrepreneurs allows us to examine transitions into and out of entrepreneurship, which is informative of the underlying mechanisms behind the impact of wealth on entrepreneurship.

Finally, our analysis complements recent research on the effects of personal credit constraints and credit market information on entrepreneurial activity (e.g., Andersen and Nielsen (2012); Fracassi et al. (2016)). Some papers are especially related to our work in that they employ data from credit reporting agencies. Bos, Breza, and Liberman (2018) finds that negative information on individuals' credit reports increases self-employment. Herkenhoff, Phillips, and Cohen-Cole (2018) shows that individuals whose bankruptcy flags have been removed are more likely to become self-employed. In this way, their analysis provides direct evidence that financial constraints can be important for self-employment and entrepreneurship decisions. An important distinguishing feature of our analysis is that we analyze wealth shocks, which can alleviate financial constraints or operate through other important mechanisms (leisure entrepreneurship or risk aversion). That is, though previous research shows that financial constraints can matter, our evidence provides insight into the nature of the impact of wealth on entrepreneurship. In so doing, we provide novel evidence on the relative weight of the financial constraint view versus the leisure entrepreneurship view in a broad sample of American workers.

2 Setting and Data

The analysis uses several data sets that are novel to the literature. For a detailed discussion of this data merge in the context of household debt, see Cookson, Gilje, and Heimer (2019).[2] Below we outline the data and its construction, as it pertains to our study of the impact of wealth on self-employment.

[2] Although Cookson, Gilje, and Heimer (2019) uses the same data merge as this paper, the identifying variation from the wealth windfalls is quite distinct. One of the main lessons of Cookson, Gilje, and Heimer (2019) is that small-to-moderate payments matter more than larger payments when it comes to affecting the propensity of individuals to repay debt. By contrast, the present paper gets most of its identifying variation from very large shocks (in excess of $50,000 in most specifications, but sometimes, payments exceeding $1 million). Apart from this difference in identifying variation from the shale windfalls, the lessons from these two papers apply to substantively different domains of life.

2.1 Oil and Gas Lease and Royalty Data

When an oil and gas firm decides to drill and develop an oil and gas reservoir, it must first negotiate a contract, often with a private individual for the right to do so. These are the individuals in our sample. Contracts to develop oil and gas compensate a mineral owner on two different dimensions. First, prior to any extraction, a mineral owner will receive an upfront bonus payment, which will typically be a dollar per acre value. For example, a person receiving a $5,000 per acre bonus that owns 10 net mineral acres would receive a check for $50,000. Second, once extraction commences, individuals receive a royalty stream based on their share in a well. In our sample royalty percentages range from 12.5% to 30%, with 18.75% being the most common. An individual's dollar royalty payment is also scaled by their interest percentage in a drilling unit. Royalties are computed based on gross revenues, and no costs can legally be deducted from the gross revenue. For example, if a well generates gross revenue of $10,000 in a month, and an individual owns 10 net mineral acres at a 20% royalty on a 400 acre drilling unit, that individual would receive a check for $10,000*10/400*20% = $50 for that month.

Accurate data on payments that individuals receive is exceedingly difficult to obtain and compute. In all states except Texas, royalty ownership interests in wells are held by private companies and not released to the public. Public county court records can be used to compute ownership percentages, but this often requires manually searching county indices and filings, and oil and gas firms typically pay an average of $50,000 per well to compile accurate royalty owner information from these public records. To put this in perspective, the number of wells in our sample is 7,041. Fortunately, in the state of Texas, producing royalty interests are required to pay property tax, unlike other states. Texas requires all oil and gas firms to turn over their so-called "pay decks" with detailed well-by-well ownership interest information to the state. This royalty interest information is then used to compute an ownership value based on the production profile of each well. Because property tax information is public information in the state of Texas, one can conduct open record requests to obtain the detailed title and ownership information that private firms paid millions of dollars to construct. The data is often provided in PDF format, and requires substantial data manipulation to translate the data into a format conducive to analysis. In our study, we focused on compiling mineral appraisal roll data for the four main producing counties in the Barnett Shale going back to the year 2000.

Mineral roll appraisal data is highly attractive to work with because the address provided on the rolls is the address at which people receive their tax bills. This accurate address is useful for ensuring a high quality merge with credit bureau data. However, it is not enough to simply know a person's name, address, and well ownership percentage. One must match these percentages with well production and natural gas pricing. For each well in our sample, we compile monthly production data from the oil and gas regulatory body in Texas, the Texas Railroad Commission. We then multiply production by prevailing spot natural gas prices reported by the U.S. energy information administration for a given month, this computation gives us the total gross revenue of a well, which is sufficient to calculate the amount of each individual check.

In our sample, royalty payments from production account for 60% of total payments. The remaining payments are the bonus payments that mineral owners received at the time a lease was signed. To compute bonus payments, we conducted public record requests for all oil and gas leases from the four counties in our study, as well as county indexes. The lease bonus payment in many cases is not reported on a lease because it is not required to be. However, many leases do have this information, as well as net acreage amounts. Based on the leases that do have lease bonus information we estimate a regression which attempts to predict the dollar per acre amount a lease bonus is based on time fixed effects, county fixed effects, and operator fixed effects. The R-squared we obtain from the regression is 0.82. We then use this predicted amount to estimate the lease bonus amounts for the rest of our sample for which we do not have this information.

Once we have computed lease bonus payments and royalty payments for the sample, we then merge the royalty payment data and the lease bonus payment data to obtain our overall payment amounts. Overall the payment someone receives is a function of prevailing natural gas prices, the amount of net mineral acreage they own, and the amount of natural gas produced on their mineral acreage.

2.2 Barnett Shale Overview

The focus of our study is the sample of oil and gas mineral owners who own minerals in the Barnett Shale from 2005 through 2015. The Barnett Shale was the first shale gas development in the United

States. Shale gas had historically been uneconomic to drill and develop. However, the combination of horizontal drilling with hydraulic fracturing ("fracking"), by Devon Energy and George Mitchell, led to a technological breakthrough which allowed vast new quantities of natural gas to be developed. According the U.S. Energy Information administration, shale gas production was less than 1% of total U.S. natural gas production in the year 2000, but by 2015 accounted for 46.2% of total U.S. gas production. Moreover, the Barnett shale was the first, and among the most prolific shale development in the United States, and the four Barnett Shale counties we focus on in our study accounted for 17.3% of total U.S. shale gas production when production from the shale field peaked in 2012. There is a 14-fold increase in shale wells during the time period of our study. We start in 2005 largely because that is towards the beginning of the shale discovery (only 6.7% of our mineral owners were getting any payments at that time), and it is the first time period which high quality credit bureau data was available to us. As can be seen, there is a high degree of spatial heterogeneity that existed over time, as development ramped up.

The development of the Barnett Shale offers several attractive features. First, shale development was unexpected by the industry, and even less expected by households in our study. Indeed, Chevron CEO John Watson was famously quoted as saying "'fracking' took the industry by surprise (2011 WSJ)." Accordingly, mineral ownership in the Barnett Shale represented a deep out of the money option, which had minimal value until there was a technological breakthrough. For those fortunate to own minerals, which typically occurred through family ancestry, the shale breakthrough led to the deep out of the money option becoming a vary valuable cash flow stream when natural gas was drilled. Therefore, although people who own minerals are certainly different than the average credit profile in the United States, the shock they experience "within" person was due to an exogenous technological breakthrough over which they had not control.

2.3 Experian Data Overview

From the raw data we compiled, we identified approximately 500,000 mineral rights owners, and computed a monthly panel data set of the payments received by rights owners from 2000 onward. We contracted with Experian to merge the mineral rights data with individual-level credit bureau data.[3] We provided information on payments, names and addresses, and Experian conducted the merge on name and address. In addition, Experian provided us with two control samples, (i) a sample matched on the geography and age distribution of our Experian records, and (ii) a nationally representative sample. The merge with credit bureau data returned an 80 percent hit rate, leaving us with approximately 400,000 consumers who received mineral rights payments. Each of our control samples has approximately 300,000 individuals, leaving us with approximately 1.1 million credit histories.

For each individual in our sample, we observe an annual snapshot of credit bureau characteristics (credit score, estimated personal incomes modeled using actual W2 statements, an internal debt-to-income measure, plus 250 credit attributes). In addition to standard credit bureau characteristics, from 2010 to 2015 the credit bureau also provides information on employment status of individuals and demographic characteristics. The employment status field listed by the credit bureau lists the actual name of the employer of an individual (for example, "Fort Worth Independent School District"). If an individual is self-employed, the credit bureau data lists the individual as being "self-employed."

To provide more context on the types of individuals switching between self-employment and employment, we manually extract and classify the names of the firms that employ individuals who switched into or out of self-employment. Though we can only examine firm names for individuals during their period of regular employment, these jobs provide useful and granular contextual information on the skills and professions for individuals who choose self-employment in our sample. This analysis is complementary to the analysis we perform on the broad industry and educational categorizations available from Experian because it is more precise about the types of individuals who choose self-employment. For firms we can classify industry or skillset reliably, the most common industry for switchers is Real Estate, followed by Government, Construction, and Medical. These four categories combined account for over half of the switchers. By contrast, individuals who work for Technology firms account for less than 5% of switchers. This classification exercise provides some additional context on the types of indivduals driving the self-employment decisions we identify in our main tests.

[3]

As a check of data validity, we benchmark the propensity to be self employed from the credit bureau to two other data sources used in prior literature, the American Community Survey and Current Population Survey. To illustrate this comparison, we plot self employment propensity at the state-year level from these different data sources and find a high degree of correlation with measures of unincorporated self-employment (Figures 3 and 4). Overall, it appears that the credit bureau data offers a measure of self-employment consistent with these other data sources at the levels of aggregation in which we can draw the comparison. However, one major advantage of the self-employment field for our study is that it is observable at the individual-year level, and we have this individual-level measure of self-employment linked with actual payments to individuals from natural gas extraction. Furthermore, our study has the unique advantage, that we are able to observe a substantial number of other attributes on self employed individuals, including credit characteristics and oil and gas royalty wealth shocks, which affords us unique micro-level variation to exploit.

As an alternative check on data validity, we use the textual employment data field to construct a measure of retirement by searching for the string "retired." As retirement has a distinct lifecycle (with a normal retirement age of 65) from self-employment, this variable allows us to verify whether the timing of the employment field is informative. Consistent with the employment data field providing useful insight, we find that retirement propensities are greater among individuals of typical retirement age. Moreover, we also find that the impact of wealth windfalls on retirement propensities follows this life-cycle pattern (greater in near-retirement cohorts and normal retirement age cohorts than younger individuals). As we present in Figure 5, the impact of self-employment propensities is relatively flat over the life-cycle, which concords with our intuition that self-employment and retirement derived from the textual employment data field capture economically distinct activities.

2.4 Summary Statistics

Our royalty windfall data provides us with payment information to individuals for royalty payments between 2005 and 2015, however, the credit bureau data only has employment information from 2010 to 2015. Consequently, our analyses include focusing both on panel and cross-sectional results. All of the comparisons we do are across individuals, some of whom serve as "treated" which in our setting means they receive more than $50,000 in cash payments (we run sensitivities around this) and control which in our setting means individuals received no payments or payments less than $50,000.

A central concern for identification is whether the payments individuals received are meaningfully correlated with other characteristics.

In Table 1, we report summary statistics on the whole sample (Panels A and B) how observable characteristics compare across the two groups (Panels C and D), treatment and control, for two sample sets we will use in the study. One being all sample individuals, which as of 2005 both treatment and control have not received payments. The second being individuals who have not received a payment as of 2010, but who subsequently do receive a payment, this sample allows us to focus on the subset of data where we have complete employment information and can construct a meaningful panel. As can be seen the raw differences in both sample sets are both economically and statistically meaningful. To control for these differences, as best we can, we saturate our regressions with granular fixed effects to control for age, zip code, income, mineral acreage size, ex ante credit score. We can compute an adjusted difference where we look at differences after controlling for these fixed effects. In both sample sets most observable differences are reduced dramatically, and there are only a few differences that remain statistically meaningful, though in most instances are not economically meaningful.

Our main specifications rely on both cross-sectional and time series comparisons to individuals receiving wealth windfall shocks. As we report in Table 1, there are some observable differences between individuals who receive large wealth shocks, and individuals who do not. We employ a wide set of fixed effects to soak up this variation, including granular controls for mineral acreage owned. However, it still could be possible that our treatment (high payment group) and control (low payment group) have differential trends. To explicitly evaluate this possibility we look at the propensity to be self employed in event time (time t = 0 is the year the first payment is received), and plot the coefficients before and after treatment. As can be seen in Figure 7, there is no differential pre-trend prior to the initial wealth windfall. This provides some supportive evidence for our research design.

3 Empirical specifications and Results

We examine how large wealth shocks affect self-employment rates using the following linear probability model, which we estimate by OLS:

$$self_i^{2015} = \gamma_z + \gamma_{age} + \gamma_{income} + \gamma_{acreage} + \beta_1 large\ payment_i + \mathbf{X}_i' \beta + \varepsilon_i, \tag{1}$$

where the dependent variable $self_i^{2015}$ is an indicator (=1) for whether the individual is self-employed in 2015. The variable, *large payment*$_i$, is an indicator (=1) for whether individual i receives a large payment (> $50,000 in aggregate over the period 2005 to 2015). Given this specification, the coefficient of interest β_1 reflects the (conditional) difference in self-employment rates between (treated) individuals who receive large payments versus (control) individuals who receive small payments. To account for local clustering of payments, standard errors are clustered by ZIP3.

This specification conditions the effect receiving a large windfall on a set of granular fixed effects: γ_z are ZIP3 fixed effects, γ_{age} are age fixed effects (dummies for each age), γ_{income} are fixed effects for quintiles of the initial (2005, pre-shock) income distribution, and $\gamma_{acreage}$ are fixed effects for quintiles of the distribution of acreage owned. The acreage, age, and initial income fixed effects, in particular, account flexibly for individual differences that can lead to large payments. In this specification, therefore, the residual variation in payments is driven entirely by factors external to the individual – the timing and intensity of drilling, as well as macro fluctuations in the price of natural gas.

3.1 Main Results on Self-Employment

Table 2 presents the results from estimating equation (1). We include several different functional forms of "large payment" so that we can assess whether the relationship between self-employment and wealth is linear, discontinuous (dummy variable), or log. Overall, the results indicate a non-linear relationship. Although the coefficient estimate is statistically significant when we employ a linear specification for payment size in columns (1) and (2), the other specifications, which allow for various types of non-linearities provide a better fit of the data. Notably, the economic interpretation of the coefficient estimate from column (5) is that individuals who receive a windfall of more than $50,000 have 1.15 percentage points higher self-employment rates than individuals who receive smaller (or zero) payments. These specifications account for granular age, income, and acreage owned fixed effects (as well as initial credit score). We obtain a similar 1.17 percentage effect when including a wide set of individual-level controls measured as of 2015.[4] Overall specifications (5) and (6) suggest an increased self employment rate of approximately 59% to 60% of the baseline rate (baseline rate of 1.95%).

The additional specifications are also instructive on the relationship between wealth and self-employment. Specifically, the impact of wealth on self-employment is largest for large shocks, confirming intuition that small shocks should matter less for choices related to important life events, such as the decision to become or remain self-employed. This is borne out in the dummy variable specifications in (7) and (8), which shows that the larger payment amounts have larger impacts on self-employment rates. Moreover, the effects are pervasive throughout the sample (i.e., not driven by outliers or tail events). This pervasive relationship is perhaps most striking in the bin scatter of self-employment rates on logged wealth in Figure 6, which shows a robust, positive relationship between wealth and self-employment.

We undertake two sets of tests to address potential threats to identification beyond the tests in Table 2. First, one concern is that individuals who own mineral rights may be unobservably different than the control group of people who do not own mineral rights. These unobservable differences may matter despite saturating our model with a wide range of fixed effects. Our first test drops the individuals who received no payments from natural gas exploration, such that the comparison we draw is between individuals who received large versus small payments, which after controlling for mineral acreage, is driven entirely by the timing and intensity of drilling – factors external to the individuals in our sample.

We report the estimates of our main specifications that rely on this within-mineral-owner variation in in Table 3. By dropping the zero-windfall control individuals from the sample, the estimated effect of receiving a large windfall (in excess of $50,000) changes slightly from 1.15 in specification (5) of Table 2 to 1.09 in specification (5) of Table 3. Across all specifications in Table 3, the statistical and economic significance remain the same as in the main table of results.

A second potential concern is that our main results in Table 2 may not picking up wealth shocks, but instead changes in local investment opportunities. To evaluate this possibility, we limit

[4] Across all cross-sectional specifications where we indicate accounting for individual-level controls, these controls include 2015 values of the individual's credit score, debt-to-income, fraction of accounts 90 days past due, revolving utilization (%), and indicators for whether the individual is subprime, has a mortgage, has collections debt, and has an auto loan. In panel specifications, we control for each of these variables in a time-varying manner.

the sample to mineral owners who reside outside of the Barnett shale to see if there is any differential effect on our main results. Table 4 presents the results from estimating the main specifications on the sub-sample of individuals who do not reside in the Barnett Shale area. According to the specifications in columns (5) and (6), large payments have a similar magnitude effect when restricting attention to the out-of-area sub-sample, with estimates of the effect of a large payment that range from 1.17 to 1.21 percentage points. This pattern of results implies that it is the effect of wealth on the person's choice to become self-employed, not changes to regional income or local economic opportunities that drives our central result. Panels B and C of Table 2 report how the treatment and control groups differ on observables for the outside of Barnett Shale test.

3.2 Self-Employment and Liquidity Constraints

To directly assess whether the impact of wealth on self-employment is due to alleviating liquidity constraints, we augment our specification to evaluate whether the effect of wealth on self-employment is heterogeneous with empirical proxies for personal liquidity constraints Specifically, we estimate:

$$self_i^{2015} = FE + \beta_1 large\ payment_i + \beta_2 large\ payment_i \times cross\ var_i + \varepsilon_i, \tag{2}$$

in which the variable *cross* var_i is a measure for whether an individual is financially constrained prior to the fracking revolution (i.e., the debt-to-income ratio, the credit score, or an indicator for whether an individual is subprime). The prediction, if wealth affects self-employment by alleviating financial constraints, is that individuals with greater ex ante financial constraints should exhibit a greater effect of wealth on self-employment. We report the results of the interactive specifications in Table 5.

Throughout the table of results, interactions with proxies for financial constraints typically result in negative coefficients, or in the cases where the estimate is not negative, the estimated coefficient is very small and statistically insignificant. The specifications with negative and significant interaction coefficients suggest that, if anything, individuals facing binding financial constraints are *less* likely to become self-employed. These results contrast with the view that the impact of wealth on self-employment is attributable to the fact that wealth alleviates liquidity constraints.

3.3 Self-Employment and Risk Aversion

To evaluate whether changes to risk aversion can explain the impact of wealth on self-employment, we examine how the influx of wealth affects transitions into and out of self-employment in a panel structure. According to the risk aversion mechanism, an influx of wealth tends to make individuals less risk averse, which then encourages them to quit their regular job to become self employed. The unit of observation for these tests is at the individual-year level between 2010 and 2015. We estimate:

$$self_{i,t} = FE + \beta_1 Post\ treat_{i,t} + \beta_2 Post\ treat_{i,t} \times high\ payment_i + \varepsilon_i, \tag{3}$$

We estimate equation (4) separately for individuals who were initially self-employed in 2010 versus those who were regularly employed in 2010. In the initially self-employed sub-sample, the estimate β_2 reflects the propensity for an individual to remain self-employed after receiving a large wealth inflow (i.e., sticking with self employment). In the initially regularly-employed sub-sample, the estimate β_2 reflects the propensity to switch into self-employment after receiving a large influx of wealth.

Table 6 reports the results from estimating how large wealth shocks influence transition rates into and out of self-employment. Specifically, in columns (1) and (2), we obtain positive, economically large and highly statistically significant coefficient estimates for the sticking with self-employment effect. That is, individuals who receive a large wealth windfall are 9.9 to 10.8 percentage points more likely to remain self-employed than an individual who received a small mineral payment or no payment at all. When we consider different ranges of payment sizes (lower and greater than \$50,000) in column (3) of Table 6, the effect is driven by payments in excess of \$50,000.

By contrast, when we consider whether wealth windfalls lead individuals switch from regular employment into self-employment (columns (4), (5) and (6) of Table 6), we either estimate no effect or a small negative effect for payments greater than $1 million. The finding that wealth windfalls do not lead to significantly more regular employment to self-employment transitions contrasts with the risk aversion mechanism, while being consistent with the view that wealth enhances opportunities to pursue leisure entrepreneurship.

3.4 Self-Employment and Leisure

We undertake several tests to assess whether the self-employment decisions we observe are consistent with a leisure hypothesis, that is individuals become or remain self-employed for preference/leisure purposes. If wealth shocks subsidize a leisure life style/marginal businesses, one might expect that once the wealth shocks run out that we observe individuals switching back to normal employment from self-employment. To evaluate this hypothesis, we augment our cross-sectional regression specification to estimate this effect, in Table 7. Specifically we estimate:

$$self_i^{2015} = FE + \beta_1 large\ payment_i + \beta_2 large\ payment_i \times run\ out_i + \varepsilon_i, \tag{4}$$

The interaction term identifies the relative effect of having a large payment after the payment runs out. As can be seen in the specifications in Table 7, the effect of receiving a windfall is fully reversed once the windfall statements stop. One can see this, for example, in column (1) by adding together the coefficients 1.161 (main payment effect) + 0.148 (run out effect) - 1.768 (interaction effect) = -0.459. That is, taking these point estimates at face value, the total effect of receiving large payments that eventually run out is to slightly reduce the propensity to be self-employed. Had the windfall payments been used to fund positive NPV projects one might have expected the self-employment spells individuals have to become more self sustaining as cash flows from the positive NPV projects are realized. The fact that the self-employment effects we observe are short-lived suggests that the wealth windfalls were being used to subsidize marginal projects.

As another assessment of the leisure hypothesis, we test whether individuals who receive large payments and are self employed experience reductions in income. We report these results in Table 8. The point estimate on specification (1) is -7.29, which would be interpreted as a $7,292 drop in income. This result is not statistically significant. However, when we consider different cutoffs for the impact of wealth on income, several dummy variables in the payment bin specification in column (2) are negative and statistically significant, suggesting large declines income for individuals receiving wealth shocks relative to those that do not. Interestingly, we find no meaningful effect when we look at this income variable for individuals who are regularly employed in column (3). The reduction in income we observe is consistent with wealth shocks subsidizing individuals who now exert less effort in their self-employment and earn less.

Relating to these findings on income, it is important to note that – without accounting for the benefits of experimentation, e.g., Manso (2016); Dillon and Stanton (2017) – the low average earnings of self-employed individuals cannot be interpreted as evidence of non-pecuniary motives. To address this concern, we estimate the effect of wealth windfalls on self-employment rates separately by different age cohorts using our main specification in equation (1). The estimated coefficients and 95% confidence intervals are pictured in Figure 5. In contrast to the effect being concentrated among younger cohorts with greater potential gains to experimentation, we estimate a flat life-cycle profile of the effect of wealth on self-employment. Indeed, as a complement to this main finding, we also consider how retirement propensities relate to wealth windfalls in Figure 5. Consistent with wealth windfalls being deployed toward leisure (this time, outside of self-employment), we find large and striking effects of wealth windfalls on retirement propensities for individuals nearing or exceeding normal retirement age (age 65). Taken together, these findings provide novel evidence that wealth windfalls lead to self-employment and retirement through an individual's non-pecuniary motives.

3.5 Heterogeneity by Education and Industry

To further evaluate how heterogeneity in self-employment could be linked with different entrepreneurship types, we estimate the impact of wealth on self employment propensities by educational level. As we present in Figure 8, we find broadly similar effects of receiving a wealth windfall on self-employment rates regardless of education level. Specifically, we find that individuals with graduate degrees are just as likely to see an increase in self-employment rates as individuals who have less than a high school diploma. When we formally test whether there is a differential effect for having a college degree in propensities to be self-employed from wealth shocks we find a coefficient that is negative (meaning college educated individuals are less likely to become self employed upon receiving a wealth windfall), but not statistically significant. To the extent education is a proxy for human capital, and that high human capital is required for high growth businesses, this result again suggests a substantial role for non-high growth, leisure entrepreneurs driving the variation we observe in our sample.

We also test whether there are meaningful differences in self-employment propensity across industry type using a personal industry classification from Experian. The industry classifications that Experian provides are somewhat coarse, however we can still infer some conclusions. If people who are self-employed are starting businesses that are high-growth entrepreneurs, we ought to see bigger effects in Professional/Technical industries relative to Blue Collar or Farm related industries. However, as we show in Figure 9, we find similar propensities to become self employed across all industries, again suggesting the entrepreneurship types driving self-employment in our setting are not creating high growth companies.

4 Conclusion

Entrepreneurship encompasses a wide range of economic activities. Recent literature has begun to explore the implications of this heterogeneity as it relates to different entrepreneurship types. We highlight important dynamics at the intersection of entrepreneurship types and frictions that have been hypothesized to impede entrepreneurship. Specifically, we employ novel micro-level wealth shocks to document that wealth shocks to not appear to alleviate binding financial constraints for self-employed individuals. Instead, wealth shocks appear to facilitate self-employment decisions that lead to leisure entrepreneurship. Overall our results provide important evidence on how entrepreneurship types need to be considered when evaluating frictions that might adversely affect entrepreneurship decisions.

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5 Tables and figures

Figure 1: Spatial distribution of people that receive oil and gas mineral payments

Note: This figure plots the spatial distribution of the people in the sample that have received an oil and gas royalty payment. The figure is a heatmap where each individual is represented by a square. The darker (lighter) is the square, the more (less) density of people there is. The location of the individual is defined as follow: it is the centroid of the 5 digit zipcode of his personal location the day he signs the lease.

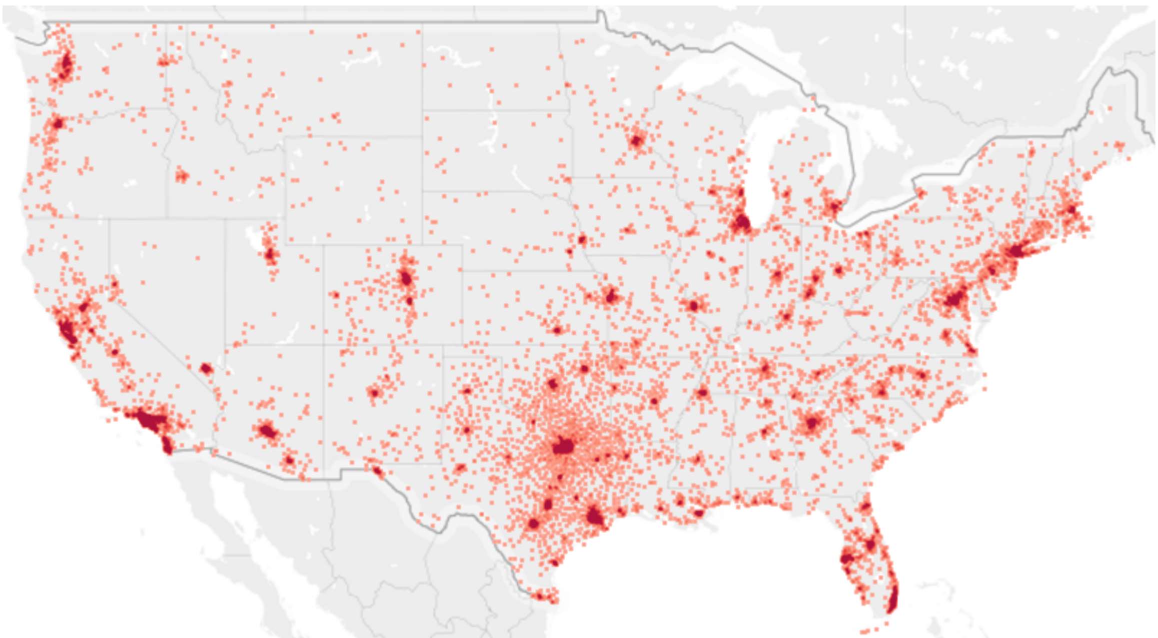

Figure 2: Spatial distribution of people that receive large oil and gas royalty payments

Note: This figure plots the spatial distribution of the people in the sample that have received an oil and gas royalty payment that is above $50,000. The figure is a heatmap where each individual is represented by a square. The darker (lighter) is the square, the more (less) density of people there is. The location of the individual is defined as follow: it is the centroid of the 5 digit zipcode of his personal location the day he signs the lease.

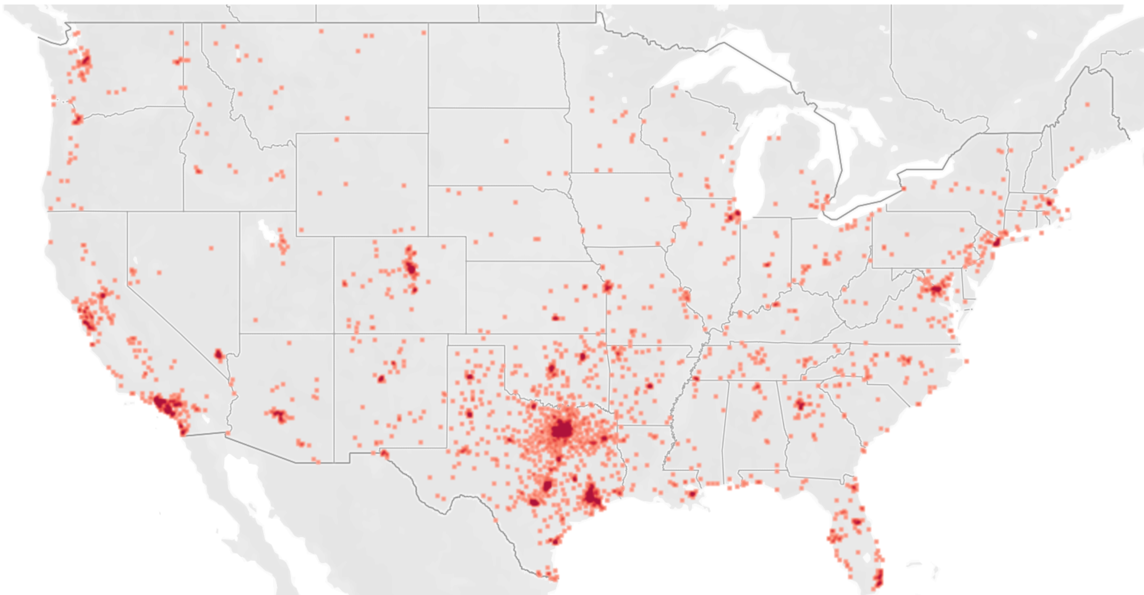

Figure 3: American Community Survey Self-Employment versus Credit Bureau Self Employment

Note: This figure plots the fraction of the workforce that is self-employed as reported by the American Community Survey (y-axis) compared to the Credit Bureau (x–axis). The unit of observation is at the state-year level.

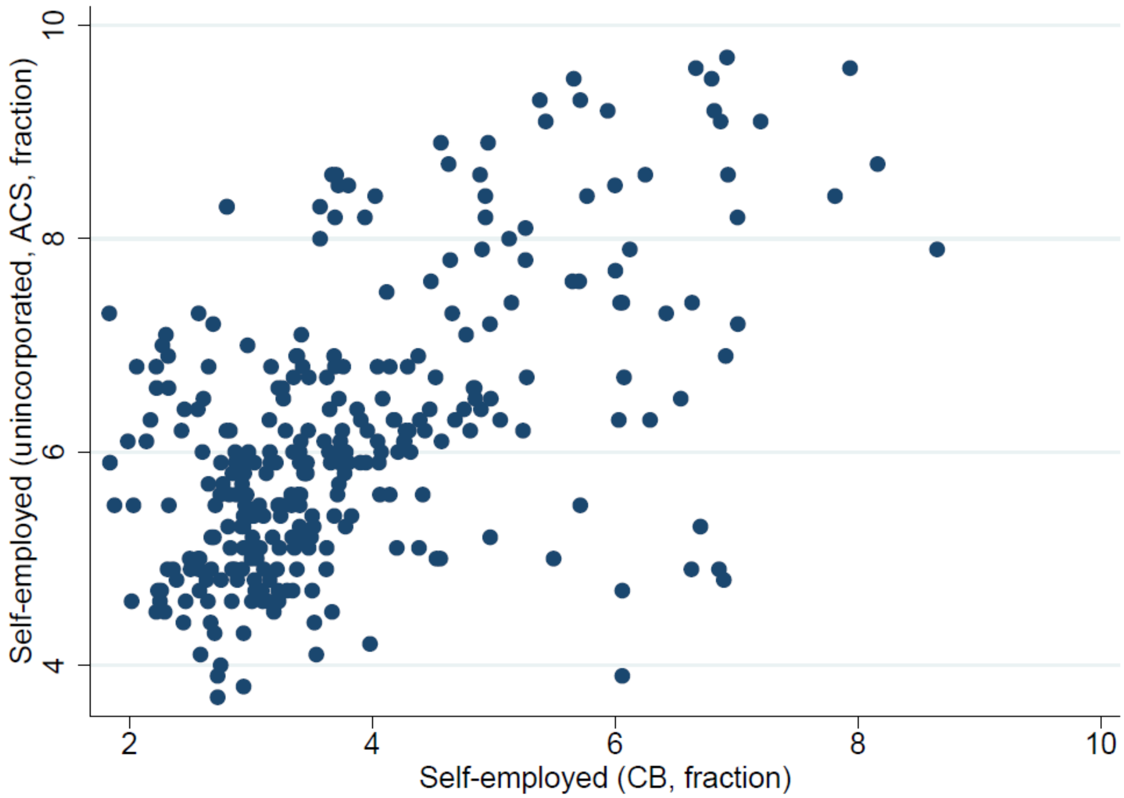

Figure 4: Current Population Survey Self-Employment versus Credit Bureau Self-Employment

Note: This figure plots the fraction of the workforce that is self-employed as reported by the Current Population Survey (y-axis) compared to the Credit Bureau (x–axis). The unit of observation is at the state-year level.

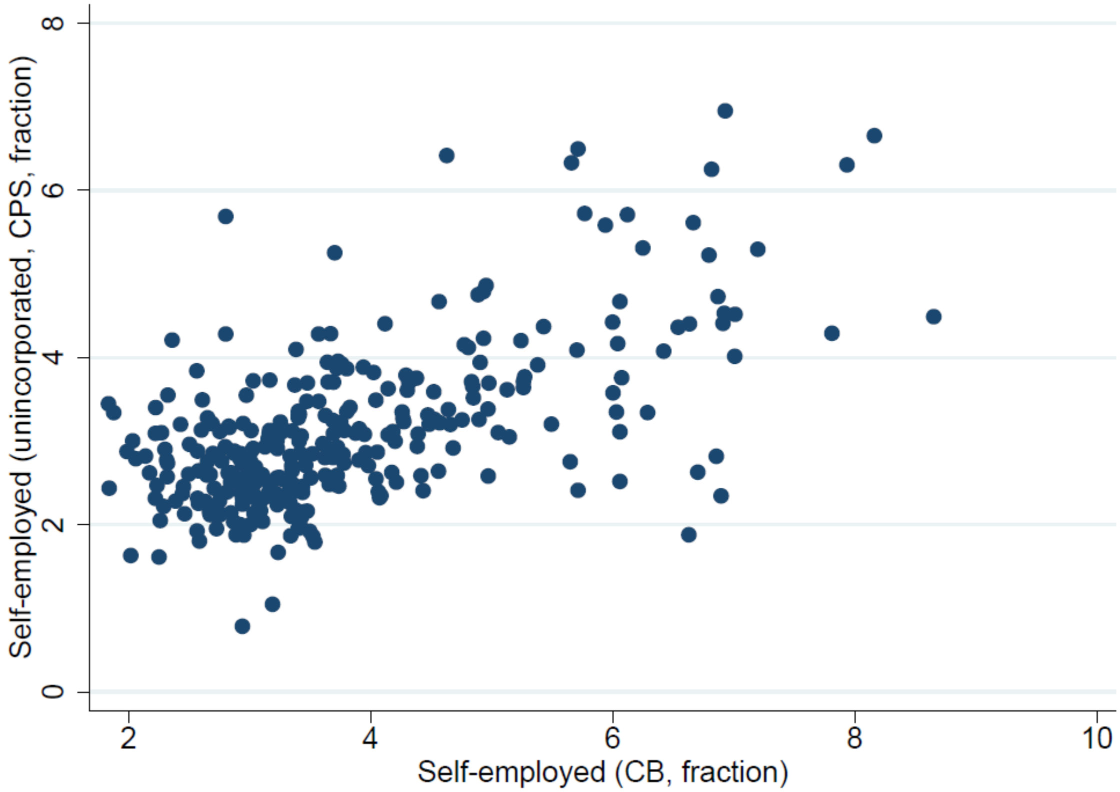

Figure 5: The Effect of Windfall Wealth on Self-employment and Retirement over the Lifecycle

Note: This figure presents the estimated effect and 95% confidence interval for the effect of logged windfall wealth on self-employment (and separately retirement), estimated separately for different age ranges (based on age in year 2005).

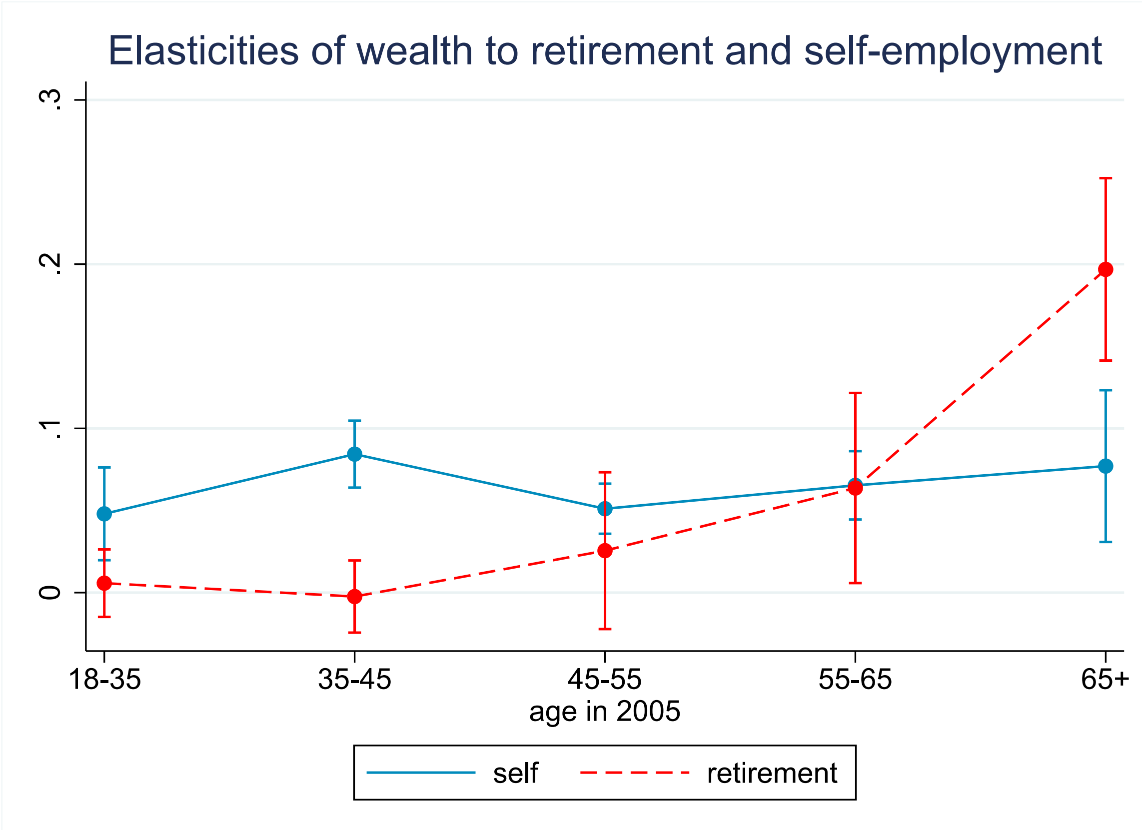

Figure 6: Wealth Shocks and Self-Employment

Note: This figure plots the propensity to be self-employed in percentage terms on the y-axis, relative to the log wealth shock received (x-axis).

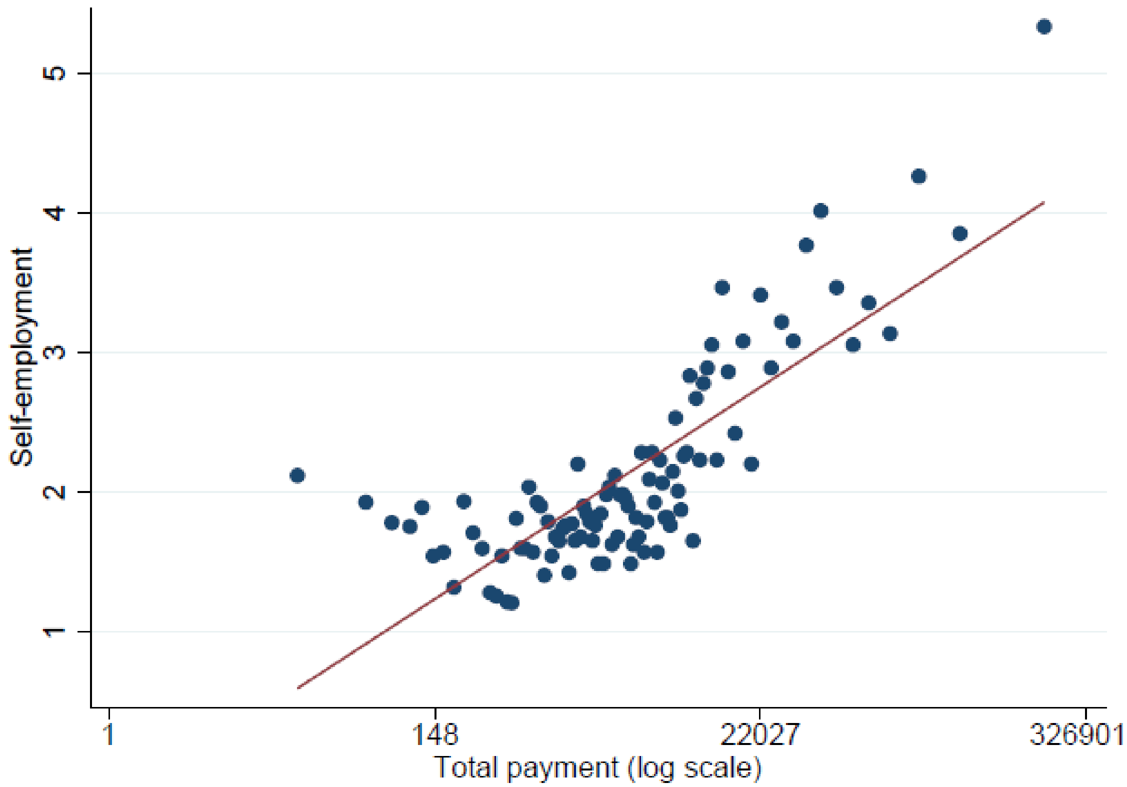

Figure 7: Self-employment in Event Time

Note: This figure plots the propensity to be self-employed relative to the first windfall and treatment individual receives, compared to the control group.

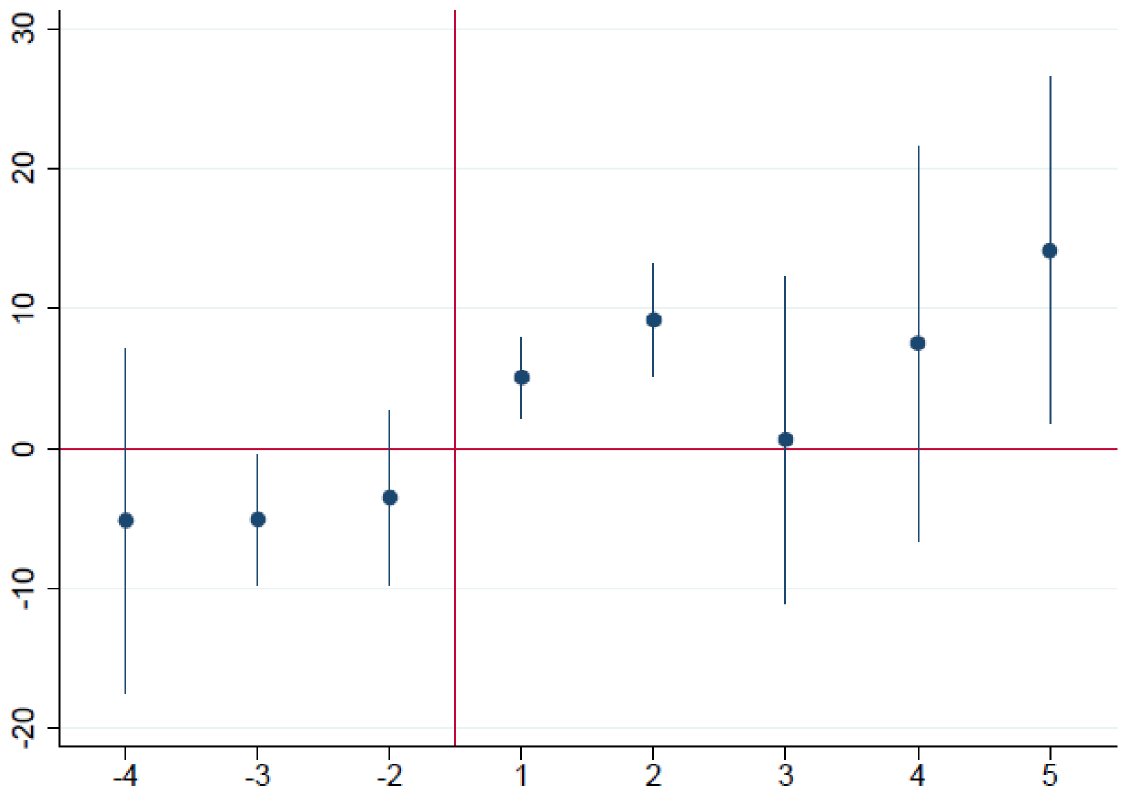

Figure 8: The Effect of Windfall Wealth on Self-employment and Retirement over the Lifecycle

Note: This figure presents the estimated effect of receiving a large wealth windfall (> $50,000) on self-employment rates, estimated separately for subsamples of individuals by industry. For details on the empirical specifications, see Table A.1.

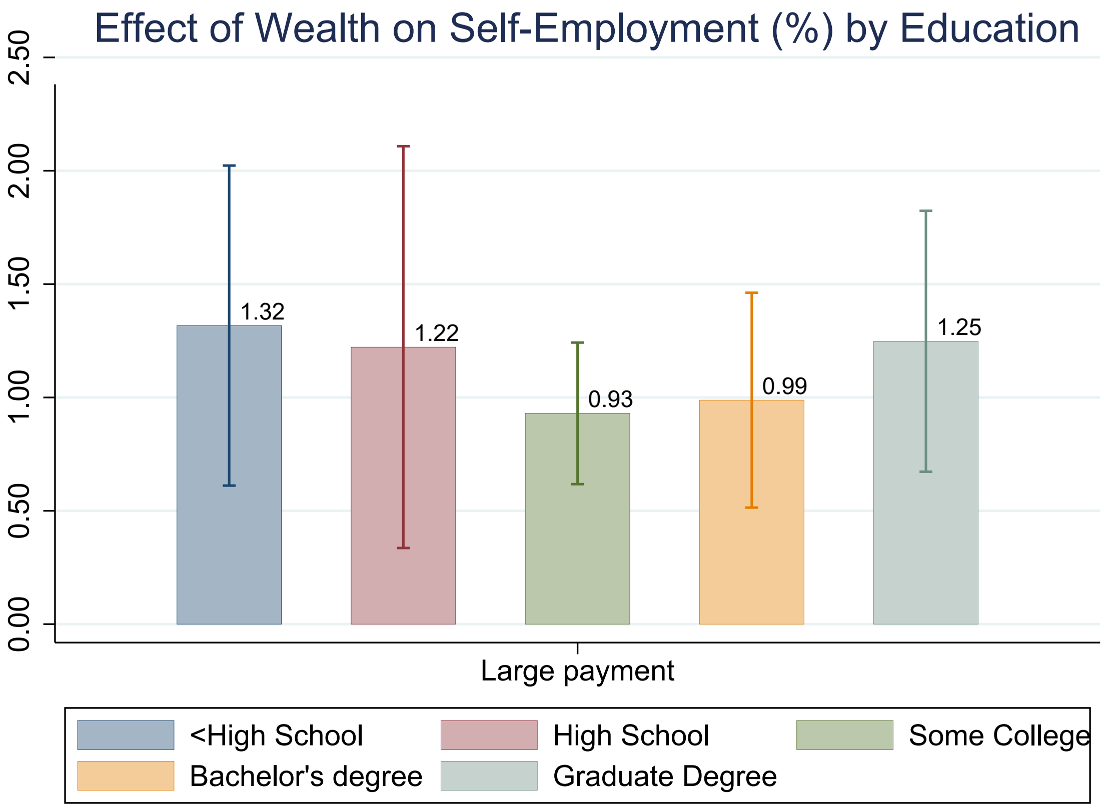

Figure 9: The Effect of Windfall Wealth by Industry

Note: This figure presents the estimated effect of receiving a large wealth windfall (> $50,000) on self-employment rates, estimated separately for subsamples of individuals by industry. For details on the empirical specifications, see Table A.2.

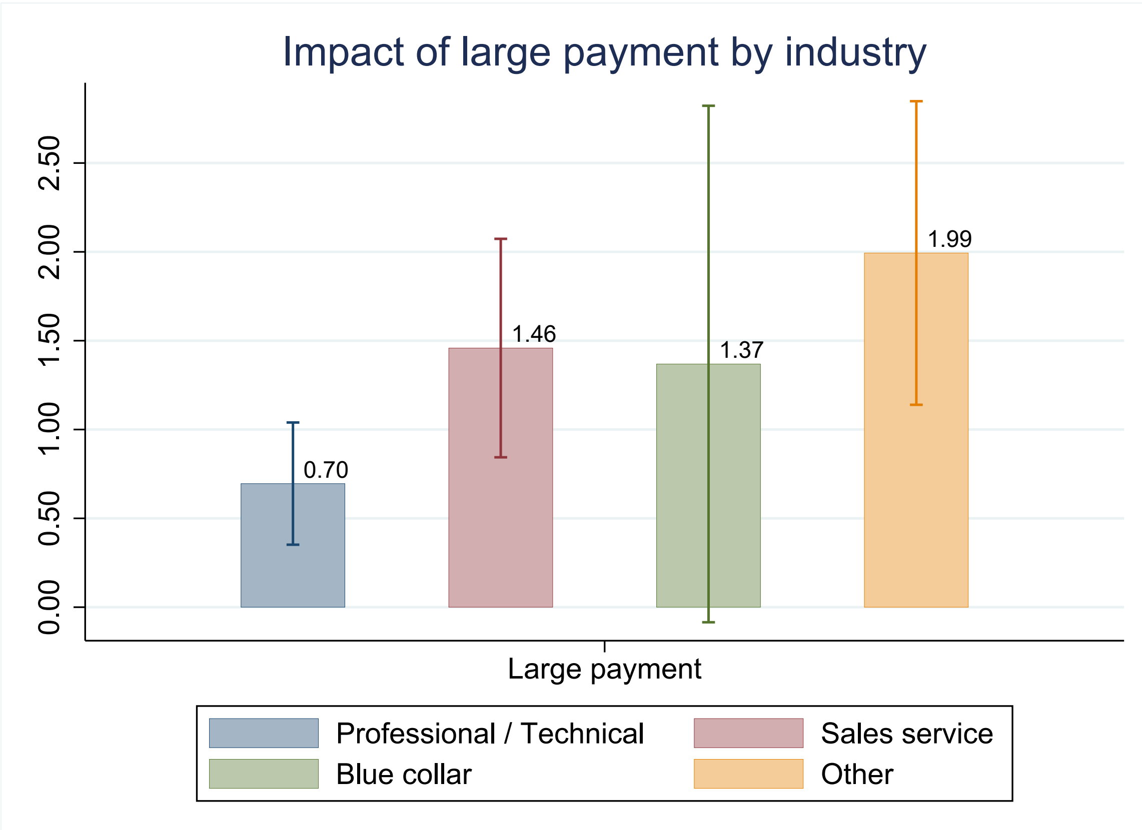

Table 1: Initial differences between treated and controls

Note: This table shows the descriptive statistics of our databases (Panel A and B) as well as the differences between the treated and control group before the treatment happens (Panel C and D). We define our treated group as the people who received an amount of wealth above $50k. Our control group is made of people that received either a low amount of wealth or no wealth at all. Standard errors for the t-test are clustered at the zipcode 3 digit level. The column Raw diff presents the differences when we control for the fixed effects used in the econometric specifications: Age fixed effects include a fixed effect for each age. acre and income fixed effects are for the quantiles and credit score fixed effects are made at the centile level. Standard errors are clustered by ZIP3 in parentheses. *, **, and *** indicate statistical significance at the 10%, 5% and 1% level respectively.

Panel A: Sample in 2005

	mean	standard deviation	q25	median	q75
self-employed	1.8	13.29	0	0	0
retired	4.54	20.82	0	0	0
Income	57.99	25.70	41	52	67
Credit score	738.77	81.69	681	763	808
Age in 2005	49.97	13.56	41	49	59
debt to income	14.37	13.07	2	12	22
Derogatory trades	.13	.68	0	0	0
Delinquent trades	.01	.15	0	0	0
total number of trades	19.55	11.39	11	18	26
Revolving Utilization	.25	.28	.03	.13	.42
Subprime	.13	.34	0	0	0

Panel B: Sample in 2010

	mean	standard deviation	q25	median	q75
Income	58.82	25.9	42	53	68
Credit score	737.88	81.43	680	761	808
Age in 2005	48.07	13.66	38	48	57
Debt to income	14.61	12.91	3	13	23
Derogatory trades	.14	.7	0	0	0
Delinquent trades	.010	.15	0	0	0
total number of trades	19.43	11.17	11	18	26
Revolving Utilization	.27	.29	.037	.14	.44
Subprime	.15	.36	0	0	0
retired	4.65	21.06	0	0	0
self-employed	1.95	13.84	0	0	0
sector: blue collar	.13	.34	0	0	0
sector: farm	.003	.054	0	0	0
sector: sales service	.26	.44	0	0	1
sector: technical	.4	.49	0	0	1
education: less high school	.1	.29	0	0	0
education: high school	.29	.42	0	0	0
education: college degree	.28	.45	0	0	1
education: bachelor degree	.25	.43	0	0	0
education: graduate degree	.12	.33	0	0	0

Panel C: Full sample in 2005

	Group treated	Control group	Raw diff.	p	Adjusted diff	p	Obs.
Income	57.00	49.10	-7.91^{***}	0.00	1.404^{***}	0.000	688736
Credit score	724.98	701.92	-23.06^{***}	0.00	0.001	0.955	688736
Age in 2005	52.00	49.75	-2.25^{***}	0.00	0	.	685723
debt to income	18.19	17.47	-0.72^{***}	0.00	0.245	0.231	655788
Derogatory trades	0.12	0.19	0.07^{***}	0.00	-0.005	0.259	683323
Delinquent trades	0.01	0.02	0.01^{***}	0.00	-0.001	0.406	678404
total number of trades	20.75	18.49	-2.26^{***}	0.00	-0.044	0.869	688736
Revolving Utilization	0.24	0.27	0.03^{***}	0.00	-0.001	0.881	597065
subprime	0.14	0.21	0.07^{***}	0.00	0.0002	0.566	688736

Panel D: Post 2010 sample in 2010

	Group treated	Control group	Raw diff.	p	Adjusted diff	p	Obs.
Income	60.54	52.82	-7.71^{***}	0.00	-0.363	0.739	170606
Credit score	737.34	704.02	-33.32^{***}	0.00	0.063	0.248	170606
Age in 2005	51.52	48.44	-3.08^{***}	0.00	0	.	169818
debt to income	12.79	16.22	3.43^{***}	0.00	-1.268^{**}	0.018	155509
Derogatory trades	0.14	0.29	0.15^{***}	0.00	-0.0197	0.337	164023
Delinquent trades	0.00	0.02	0.02^{***}	0.00	-0.008^{***}	0.006	161002
total number of trades	18.18	17.97	-0.21	0.65	-1.407^{***}	0.005	170606
Revolving Utilization	0.24	0.28	0.04^{***}	0.00	-0.004	0.617	135037
subprime	0.15	0.21	0.07^{***}	0.00	0.016	0.334	170606
retired	3.97	3.76	-0.21	0.78	-0.502	0.530	170606
sector: blue collar	0.15	0.11	-0.04^{***}	0.00	0.003	0.768	170606
sector: farm	0.23	0.23	0.00	0.91	-0.014	0.447	170606
sector: sales service	0.30	0.30	-0.00	0.94	0.033^{*}	0.081	170606
sector: technical	0.21	0.21	-0.00	0.98	-0.005	0.743	170606
education: less high school	0.10	0.12	0.02^{*}	0.09	-0.008	0.473	170606
education: high school	0.21	0.21	-0.00	0.98	-0.005	0.743	170606
education: college degree	0.30	0.30	-0.00	0.94	0.0326^{*}	0.081	170606
education: bachelor degree	0.23	0.23	0.00	0.91	-0.014	0.447	170606
education: graduate degree	0.15	0.11	-0.04^{***}	0.00	0.003	0.768	170606
self-employed	3.24	1.99	-1.24^{*}	0.07	0.535	0.569	170606

Table 2: Wealth Shocks and Self-Employment

Note: The sample is a cross-sectional regrssion on all 2015 observations. The dependent variable is an indicator for self-employed (x 100 for percentage interpretation). Large payment is an indicator for whether the total payment exceeds \$50,000. The estimates are from a linear probability model, estimated using OLS. Age fixed effects include a fixed effect for each age. acre and income fixed effects are for the quantiles and credit score fixed effects are made at the centile level. Controls include 2015 values of the individual's credit score, debt-to-income, fraction of accounts 90 days past due, revolving utilization (%), and indicators for whether the individual is subprime, has a mortgage, has collections debt, and has an auto loan. Standard errors clustered by ZIP3 in parentheses. *, **, and *** indicate statistical significance at the 10%, 5% and 1% level respectively.

	Dependent variable:							
	100 × self							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Total payment (Normalized)	0.168**	0.139**						
	(0.072)	(0.055)						
Total payment (log)			0.066***	0.081***				
			(0.008)	(0.006)				
Large payment					1.147***	1.166***		
					(0.185)	(0.135)		
payment: \$5k- \$20k							0.447***	0.450***
							(0.035)	(0.046)
payment: \$20k-\$50k							0.970***	0.948***
							(0.283)	(0.218)
payment: \$50k-\$100k							1.150***	1.125***
							(0.306)	(0.242)
payment: \$100k-\$1million							1.306***	1.396***
							(0.135)	(0.182)
payment above \$1million							3.477***	3.247***
							(0.579)	(0.471)
Observations	735419	579771	735419	579771	735419	579771	735419	579771
Age Fixed Effects	x	x	x	x	x	x	x	x
ZIP3 Fixed Effects	x	x	x	x	x	x	x	x
Income quantile Fixed Effects	x	x	x	x	x	x	x	x
Acre quantile Fixed Effects	x	x	x	x	x	x	x	x
controls		x		x		x		x
Credit score centile Fixed Effects	x	x	x	x	x	x	x	x

Table 3: Wealth Shocks and Self Employment – Treated only

Note: The sample is a cross-sectional regression on all 2015 observations that receive a royalty or bonus payment between 2005 and 2015 (individuals that receive some dollar payment). The variables are defined as in Table 2. The dependent variable is an indicator for self-employed (x 100 for percentage interpretation). Large payment is an indicator for whether the total payment exceeds $50,000. The estimates are from a linear probability model, estimated using OLS. Age fixed effects include a fixed effect for each age. acre and income fixed effects are for the quantiles and credit score fixed effects are made at the centile level. Controls include 2015 values of the individual's credit score, debt-to-income, fraction of accounts 90 days past due, revolving utilization (%), and indicators for whether the individual is subprime, has a mortgage, has collections debt, and has an auto loan. Standard errors clustered by ZIP3 in parentheses. *, **, and *** indicate statistical significance at the 10%, 5% and 1% level respectively.

	Dependent variable:							
	100 × self							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Total payment (Normalized)	0.162**	0.132**						
	(0.073)	(0.056)						
Total payment (log)			0.207***	0.202***				
			(0.027)	(0.022)				
Large payment					1.089***	1.084***		
					(0.181)	(0.131)		
payment: $5k- $20k							0.418***	0.386***
							(0.035)	(0.051)
payment: $20k-$50k							0.942***	0.868***
							(0.282)	(0.227)
payment: $50k-$100k							1.124***	1.055***
							(0.310)	(0.249)
payment: $100k-$1million							1.277***	1.322***
							(0.150)	(0.188)
payment above $1million							3.439***	3.151***
							(0.598)	(0.478)
Observations	363873	306628	363873	306628	363873	306628	363873	306628
Age Fixed Effects	x	x	x	x	x	x	x	x
ZIP3 Fixed Effects	x	x	x	x	x	x	x	x
Income quantile Fixed Effects	x	x	x	x	x	x	x	x
Acre quantile Fixed Effects	x	x	x	x	x	x	x	x
controls		x		x		x		x
Credit score centile Fixed Effects	x	x	x	x	x	x	x	x

Table 4: Wealth Shocks and Self Employment – Outside of Barnett

Note: Panel A is a cross-setional regression on all 2015 observations that are located outside the barnett region. The variables are defined as in Table 2. The dependent variable is an indicator for self-employed (x 100 for percentage interpretation). Large payment is an indicator for whether the total payment exceeds $50,000. The estimates are from a linear probability model, estimated using OLS. Age fixed effects include a fixed effect for each age. acre and income fixed effects are for the quantiles and credit score fixed effects are made at the centile level. Controls include 2015 values of the individual's credit score, debt-to-income, fraction of accounts 90 days past due, revolving utilization (%), and indicators for whether the individual is subprime, has a mortgage, has collections debt, and has an auto loan. Standard errors clustered by ZIP3 in parentheses. *, **, and *** indicate statistical significance at the 10%, 5% and 1% level respectively. Panel B and C present the descriptive statistics between the treated and control for the people living outside barnett in a way similar to panel C and D of table 1 .

Panel A

	Dependent variable:							
	100 × self							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Total payment (Normalized)	0.114	0.112						
	(0.071)	(0.077)						
Total payment (log)			0.073***	0.080***				
			(0.019)	(0.022)				
Large payment					1.168***	1.209***		
					(0.325)	(0.364)		
payment: $5k- $20k							0.233	0.123
							(0.212)	(0.217)
payment: $20k-$50k							0.438	0.516
							(0.373)	(0.425)
payment: $50k-$100k							1.529***	1.427***
							(0.488)	(0.527)
payment: $100k-$1million							0.945**	1.019*
							(0.467)	(0.529)
payment above $1million							2.614***	2.864**
							(0.969)	(1.127)
Observations	88967	72399	88967	72399	88967	72399	88967	72399
Age Fixed Effects	x	x	x	x	x	x	x	x
ZIP3 Fixed Effects	x	x	x	x	x	x	x	x
Income quantile Fixed Effects	x	x	x	x	x	x	x	x
Acre quantile Fixed Effects	x	x	x	x	x	x	x	x
controls		x		x		x		x
Credit score centile Fixed Effects	x	x	x	x	x	x	x	x

Panel B

	Group treated	Control group	Raw diff.	p	Adjusted diff	p	Obs.
Income	59.86	51.31	-8.56***	0.00	1.30	0.008***	73216
Credit score	735.77	707.43	-28.34***	0.00	-.043	0.4	73216
Age in 2015	64.64	57.78	-6.86***	0.00	0	.	. 72293
debt to income	15.21	16.14	0.93***	0.00	-.62	0.044 **	69196
Derogatory trades	0.08	0.13	0.05***	0.00	-.01	0.39	72554
Delinquent trades	0.01	0.01	0.01***	0.019	-.00	0.94	72016
total number of trades	19.72	18.07	-1.65	0.580	-.90	0.001 ***	73216
rev_util	0.23	0.27	0.04***	0.00	.011	0.043 **	64223
subprime	0.12	0.18	0.07***	0.00	.001	0.233	73216

Panel C

	Group treated	Control group	Raw diff.	p	Adjusted diff	p	Obs.
Income	63.63	55.26	-8.36***	0.00	1.46	0.37	23741
Credit score	734.60	704.68	-29.92***	0.00	.041	0.68	23741
Age in 2005	54.15	48.17	-5.98***	0.00	0	.	. 23437
debt to income	12.87	15.14	2.27***	0.01	-.21	0.77	21421
Derogatory trades	0.18	0.30	0.12**	0.01	.00	0.94	22802
Delinquent trades	0.01	0.02	0.01**	0.02	-.00	0.87	22412
total number of trades	18.38	17.82	-0.56	0.50	-.49	0.57	23741
rev_util	0.24	0.28	0.04**	0.02	-.01	0.28	18860
subprime	0.14	0.19	0.05**	0.02	.02	0.41	23741
Retired	4.44	3.73	-0.71	0.56	.39	0.75	23741
sector: blue collar	0.08	0.14	0.06***	0.00	-.021	0.25	23741
sector: farm	0.02	0.01	-0.01	0.13	.01	0.30	23741
sector: sales service	0.27	0.30	0.04	0.22	-.01	0.83	23741
sector: technical	0.40	0.34	-0.06*	0.08	.02	0.43	23741
education: less high school	0.11	0.13	0.02	0.28	-.01	0.49	23741
education: high school	0.23	0.22	-0.00	0.87	.00	0.91	23741
education: college degree	0.29	0.28	-0.00	0.91	.03	0.32	23741
education: bachelor degree	0.21	0.21	-0.00	0.95	.00	0.97	23741
education: graduate degree	0.16	0.13	-0.04	0.12	-.01	0.82	23741
self-employed	2.39	2.18	-0.21	0.82	-.39	0.733	23741

Table 5: Wealth Shocks and Self Employment – Heterogeneity by Financial Constraints

Note: The sample is a cross-sectional regression on all 2015 observations and we interact the treatment variable with balance sheet variables that proxy for financial constraints at the beginning of the sample. The dependent variable is an indicator for self-employed (x 100 for percentage interpretation). Large payment is an indicator for whether the total payment exceeds $50,000. The estimates are from a linear probability model, estimated using OLS. Age fixed effects include a fixed effect for each age. acre and income fixed effects are for the quantiles and credit score fixed effects are made at the centile level. (Z) indicates that the continuous variable has been normalized to a mean of zero and a standard deviation of 1 to ease the interpretation of the interactive coefficient estimates. Standard errors clustered by ZIP3 in parentheses. *, **, and *** indicate statistical significance at the 10%, 5% and 1% level respectively.

	Dependent variable:					
	100 × self					
	(1)	(2)	(3)	(4)	(5)	(6)
large payment	1.149***	1.116***	1.236***	1.380***	1.097***	1.043***
	(0.183)	(0.323)	(0.213)	(0.354)	(0.183)	(0.334)
large payment × Debt to income in 2005 (Z)	-0.028	−0.586**				
	(0.142)	(0.265)				
large payment × subprime_2005			−0.697**	−1.882***		
			(0.313)	(0.635)		
large payment × Credit score in 2005 (Z)					0.199***	0.380
					(0.072)	(0.273)
Debt to income in 2005 (Z)	−0.078***	−0.234***				
	(0.026)	(0.064)				
subprime_2005			-0.104	0.081		
			(0.156)	(0.170)		
Credit score in 2005 (Z)					-0.114	−0.250***
					(0.083)	(0.078)
Observations	646449	88967	646449	88967	646449	88967
Outside of Barnett		x		x		x
Credit score centile Fixed Effects	x	x	x	x	x	x
Age Fixed Effects	x	x	x	x	x	x
ZIP3 Fixed Effects	x	x	x	x	x	x
Income quantile Fixed Effects	x	x	x	x	x	x
Acre quantile Fixed Effects	x	x	x	x	x	x

Table 6: Self Employment Transitions

Note: The sample is a panel of all observations between 2010 and 2015 that did not receive a payment in 2010 or before. It compares the flows in and out self-employment. The dependent variable is an indicator for self-employed (x 100 for percentage interpretation). Large payment is an indicator for whether the total payment exceeds $50,000. The estimates are from a linear probability model, estimated using OLS. Age fixed effects include a fixed effect for each age. acre and income fixed effects are for the quantiles and credit score fixed effects are made at the centile level. Standard errors clustered by ZIP3 in parentheses. *, **, and *** indicate statistical significance at the 10%, 5% and 1% level respectively.

	Dependent variable: 100 × self					
	Self-employed in 2010			Regular employment in 2010		
	(1)	(2)	(3)	(4)	(5)	
post	-0.000	-0.000	-0.000	-0.000	-0.000	-0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
post × Large payment	10.767***	9.854***		0.031	-0.015	
	(2.991)	(2.871)		(0.202)	(0.208)	
post × payment: $5k- $20k			1.242			0.000
			(1.845)			(0.042)
post × payment: $20k-$50k			-0.473			-0.026
			(6.143)			(0.125)
post × payment: $50k-$100k			10.598**			0.080
			(5.228)			(0.308)
post × payment: $100k-$1million			17.579***			-0.186
			(6.180)			(0.142)
post × payment above $1million			42.008			−0.252**
			(37.924)			(0.109)
Individual Fixed Effects	x	x	x	x	x	x
year	x			x		
Acre quantile Fixed Effects × year		x	x		x	x

Table 7: Wealth and Self Employment: Impact of Mineral Payments Ending

Note: The sample is a cross-section of individuals, observed in 2015. The variable *run_out* is a variable that takes 1 if the payment occurred but stopped in 2015 and 0 otherwise. The dependent variable is an indicator for self-employed (x 100 for percentage interpretation). Large payment is an indicator for whether the total payment exceeds $50,000. The estimates are from a linear probability model, estimated using OLS. Age fixed effects include a fixed effect for each age. acre and income fixed effects are for the quantiles and credit score fixed effects are made at the centile level. Controls include 2015 values of the individual's credit score, debt-to-income, fraction of accounts 90 days past due, revolving utilization (%), and indicators for whether the individual is subprime, has a mortgage, has collections debt, and has an auto loan. Standard errors clustered by ZIP3 in parentheses. *, **, and *** indicate statistical significance at the 10%, 5% and 1% level respectively.

	Dependent variable: 100 × self			
	(1)	(2)	(3)	(4)
Large payment	1.161***		1.180***	
	(0.184)		(0.133)	
run_out	0.148	0.224*	0.149	0.219**
	(0.130)	(0.120)	(0.107)	(0.097)
Large payment × run_out	-1.768***		-1.837***	
	(0.613)		(0.620)	
payment: $5k- $20k		0.450***		0.451***
		(0.034)		(0.045)
payment: $20k-$50k		0.984***		0.966***
		(0.288)		(0.224)
payment: $50k-$100k		1.180***		1.163***
		(0.306)		(0.244)
payment: $100k-$1million		1.307***		1.394***
		(0.132)		(0.178)
payment above $1million		3.505***		3.269***
		(0.579)		(0.468)
run_out × payment: $5k- $20k		-0.100		0.344
		(0.591)		(0.867)
run_out × payment: $20k-$50k		-1.644**		-2.135**
		(0.796)		(0.971)
run_out × payment: $50k-$100k		-2.397***		-2.937***
		(0.775)		(0.579)
run_out × payment: $100k-$1million		-0.058		0.633
		(1.204)		(1.505)
run_out × payment above $1million		-5.883***		-6.057***
		(0.673)		(0.564)
Observations	735419	735419	579771	579771
Age Fixed Effects	x	x	x	x
ZIP3 Fixed Effects	x	x	x	x
Income quantile Fixed Effects	x	x	x	x
Acre quantile Fixed Effects	x	x	x	x
Credit score centile Fixed Effects	x	x	x	x
controls			x	x

Table 8: Wealth Shocks, Self-Employment and Income

Note: The sample is a panel with observations between 2010 and 2015. The dependent variable is the income at the individual level. The estimates are from a linear probability model, estimated using OLS. Age fixed effects include a fixed effect for each age, acre and income fixed effects are for quantiles. Income is measured as of 2005. Standard errors clustered by ZIP3 in parentheses. *, **, and *** indicate statistical significance at the 10%, 5% and 1% level respectively.

	Dependent variable: Income ($1,000s)		
	(1)	(2)	(3)
post	0.266	0.385	-0.170^*
	(0.30)	(0.31)	(0.096)
post × Large payment	-7.292		0.194
	(5.06)		(0.85)
post × payment: $5k- $20k		-3.175^{**}	
		(1.41)	
post × payment: $20k-$50k		0.577	
		(2.09)	
post × payment: $50k-$100k		-16.36^*	
		(9.89)	
post × payment: $100k-$1million		2.011	
		(3.25)	
post × payment above $1million		-44.67^{***}	
		(1.92)	
Observations	20442	20442	1003194
Self Employed (2010)	x	x	
Acre quantile Fixed Effects × year	x	x	x
Individual Fixed Effects	x	x	x

Appendix to:

Personal Wealth and Self-Employment

Figure A.1: Self-employment (CPS) validation after controlling for unemployment

Note: This figure plots the fraction of the workforce that is self-employed as reported by the Current Population Survey (y-axis) compared to the Credit Bureau (x–axis), after controlling for the unemployment (from the LBS). The unit of observation is at the state-year level. The correlation between the two variables is equal to 0.30.

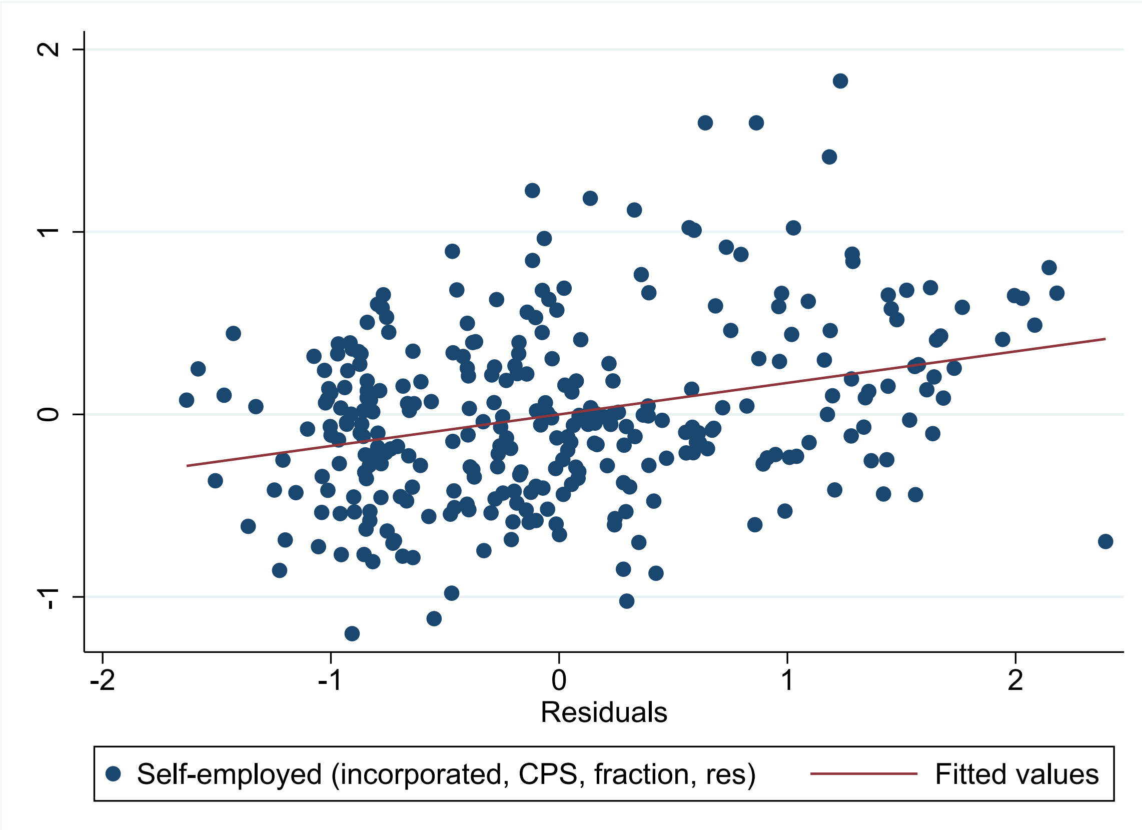

Figure A.2: Self-employment (CPS) validation after controlling for unemployment

Note: This figure plots the fraction of the workforce that is self-employed as reported by the Current Population Survey (y-axis) compared to the Credit Bureau (x–axis), after controlling for the unemployment (from the LBS). The unit of observation is at the state-year level. The correlation between the two variables is equal to 0.62

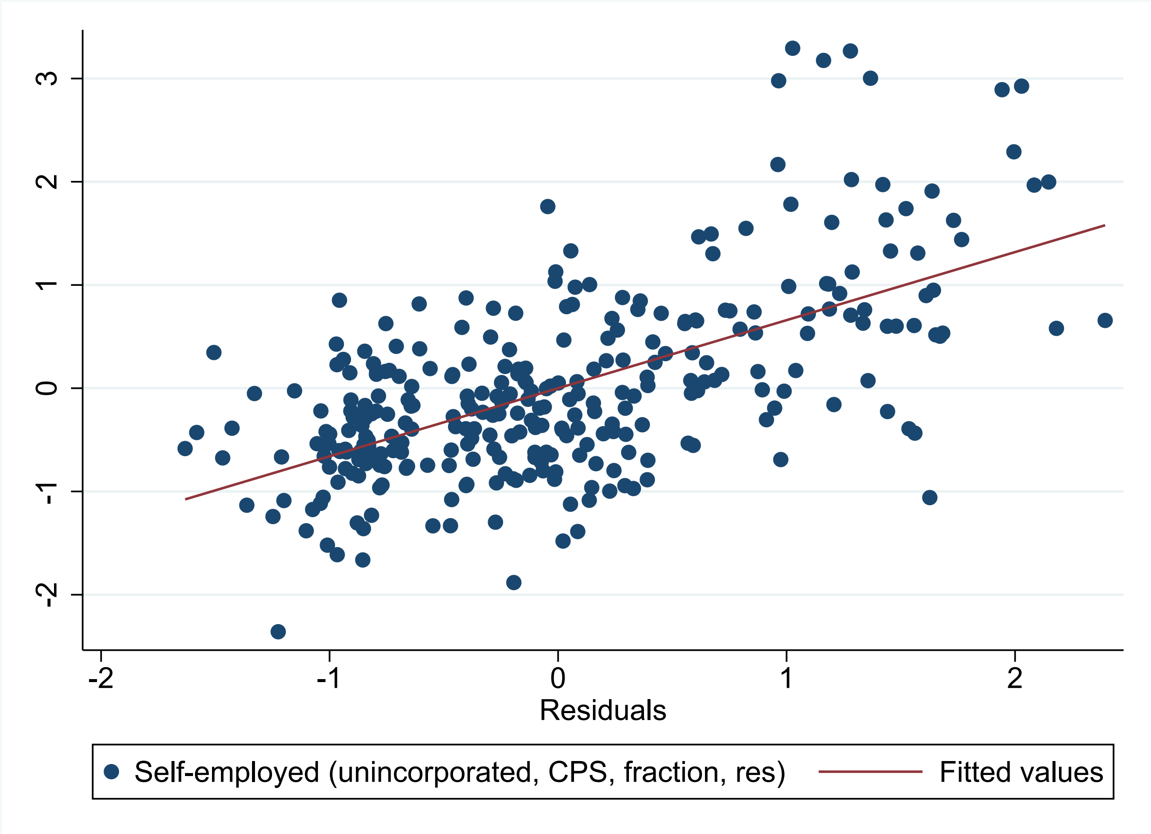

Figure A.3: Self-employment (ACS) validation after controlling for unemployment

Note: This figure plots the fraction of the workforce that is self-employed as reported by the American Community Survey (y-axis) compared to the Credit Bureau (x–axis), after controlling for the unemployment (from the LBS). The unit of observation is at the state-year level. The correlation between the two variables is equal to 0.69.

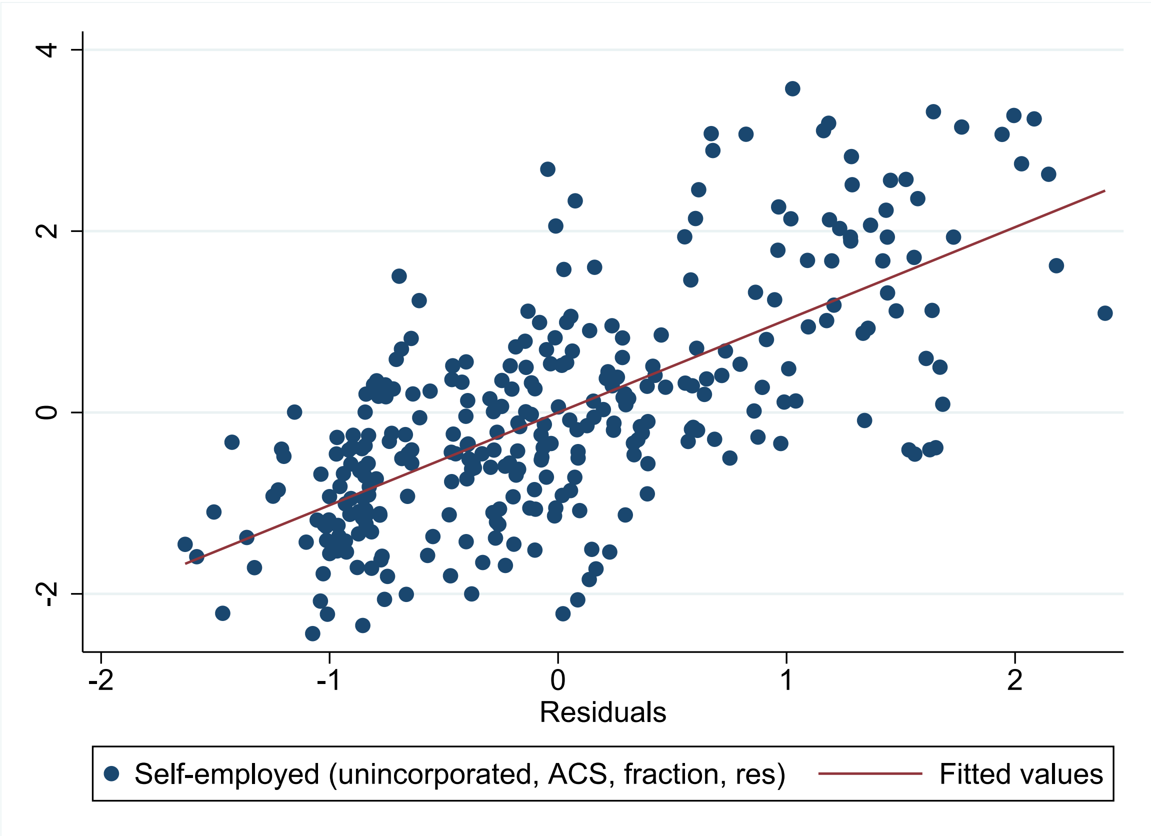

Figure A.4: unemployed_self_CB

Note: This figure plots the fraction of the workforce that is unemployed (from the LBS) (y-axis) compared to our measure of self-employment from Credit Bureau (x–axis). The unit of observation is at the state-year leve.l. The correlation between the two variables is equal to -0.35

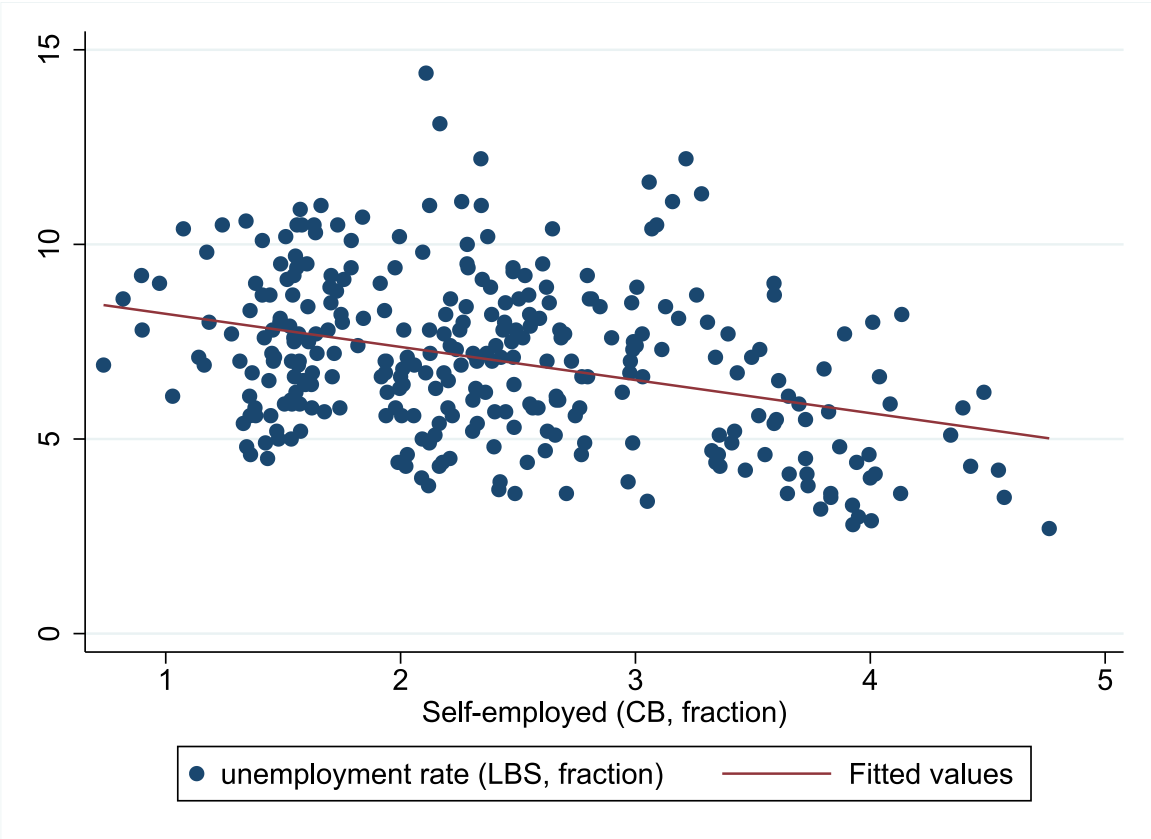

Figure A.5: Fraction fo people that change their occupation

Note: This figure plots the fraction (over the active workforce) of the workforce that remained in their occupation from one year to another one in the Credit Bureau (y-axis) to the yearly average fraction of people that stayed in their job in the Job-to-Job Flows (J2J) Data ($\frac{\text{JobStayS}}{((\text{MainB}+\text{MainE})/2)}$) (x–axis). The unit of observation is at the state-year level. The correlation between the two variables is equal to 0.47

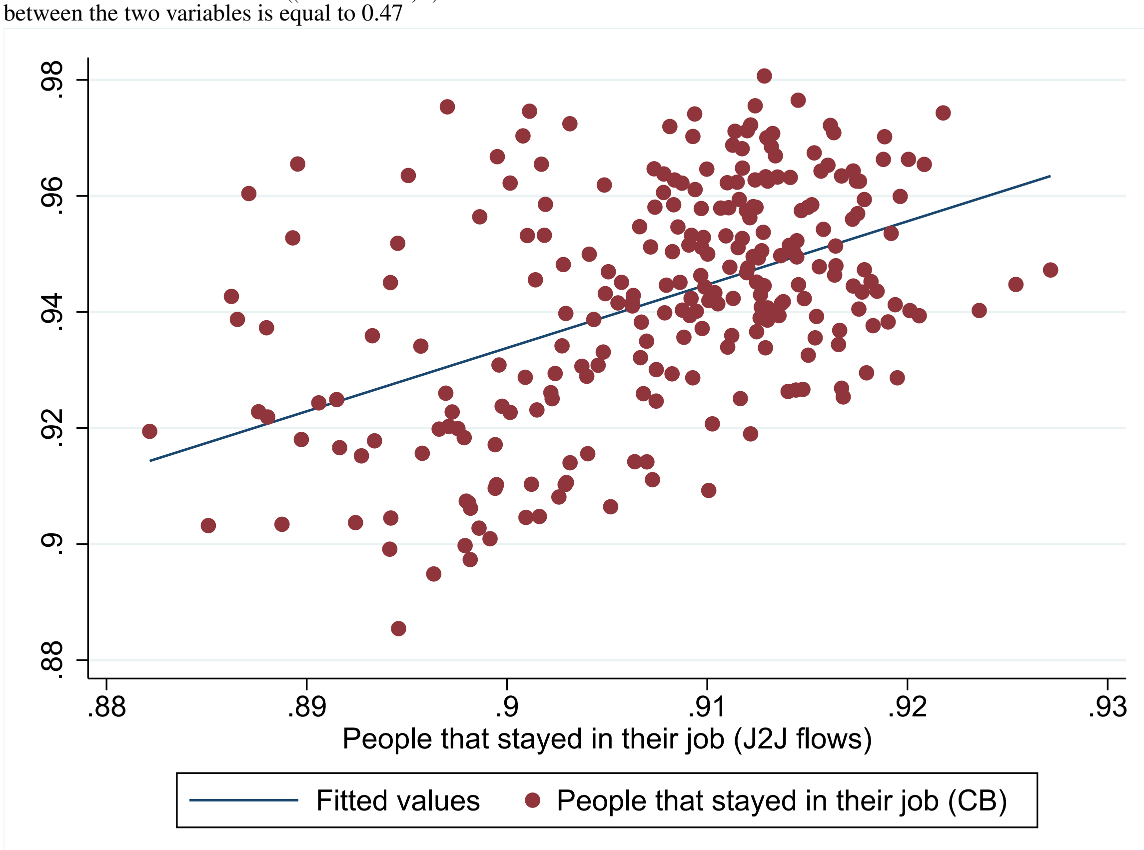

Figure A.6: Comparison of income for the group of people that stayed in their occupation

Note: This figure plots the yearly income of people that stayed in their job in the Job-to-Job Flows (J2J) Data (variable JobStaySEarn_Orig) (y-axis) to the yearly income of people that stayed in their job in the credit bureau Data. The unit of observation is at the state-year level. The correlation between the two variables is equal to 0.58.

Table A.1: Wealth Shocks, Self-Employment and Education

Note: The sample is a cross-sectional regressions on all 2015 observations. The dependent variable is the income at the individual level. The estimates are from a linear probability model, estimated using OLS. Age fixed effects include a fixed effect for each age, acre and income fixed effects are for quantiles. Standard errors clustered by ZIP3 in parentheses. *, **, and *** indicate statistical significance at the 10%, 5% and 1% level respectively. Education is a dummy that is equals to 0 if the diploma is less than high school, 1 if it is high school, 2 for some college degrees, 3 for bachelor degree and 4 for graduate degree. The dummy college degree or above takes the value 1 if the education code is strictly above 1.

	Dependent variable: 100 × self					
	(1)	(2)	(3)	(4)	(5)	(6)
Large payment	1.317***	1.222***	0.930***	0.988***	1.248***	1.317***
	(0.359)	(0.451)	(0.159)	(0.241)	(0.293)	(0.414)
Large payment × college_degree						-0.266
						(0.380)
college_degree						0.174***
						(0.023)
Observations	79174	194216	204087	156384	77671	735419
education	0	1	2	3	4	
Acre quantile Fixed Effects	x	x	x	x	x	x
ZIP3 Fixed Effects	x	x	x	x	x	x
Income quantile Fixed Effects	x	x	x	x	x	x
Age Fixed Effects	x	x	x	x	x	x
Credit score centile Fixed Effects	x	x	x	x	x	x

Table A.2: Wealth Shocks, Self-Employment and Industry

Note: The sample is a cross-sectional regression on all 2015 observations. The dependent variable is the a dummy of self-employment as of 2015. The estimates are from a linear probability model, estimated using OLS. Age fixed effects include a fixed effect for each age, acre and income fixed effects are for quantiles. Standard errors clustered by ZIP3 in parentheses. *, **, and *** indicate statistical significance at the 10%, 5% and 1% level respectively.

	(1)	(2)	(3)	(4)
	self	self	self	self
Large payment	0.696***	1.459***	1.369*	1.994***
	(0.175)	(0.313)	(0.740)	(0.435)
Observations	246023	190163	103922	23374
occupation	Professional/Technical	Sales/service	Blue Collar	Other
Acre quantile Fixed Effects	x	x	x	x
ZIP3 Fixed Effects	x	x	x	x
Income quantile Fixed Effects	x	x	x	x
Age Fixed Effects	x	x	x	x
Credit score centile Fixed Effects	x	x	x	x
Methodology Fixed Effects	x	x	x	x

Table A.3: Baseline results estimated on the sample of people that made at least one credit inquiry in 2015

Note: The sample is a cross-sectional regression on all 2015 observations that made at least one credit inquiry in 2015. The dependent variable is the a dummy of self-employment as of 2015. The estimates are from a linear probability model, estimated using OLS. Age fixed effects include a fixed effect for each age, acre and income fixed effects are for quantiles. Standard errors clustered by ZIP3 in parentheses. *, **, and *** indicate statistical significance at the 10%, 5% and 1% level respectively.

	(1) self	(2) self	(3) self	(4) self	(5) self	(6) self	(7) self	(8) self
Total payment (Normalized)	0.212*	0.164*						
	(0.123)	(0.091)						
Total payment (log)			0.063***	0.074***				
			(0.007)	(0.007)				
Large payment					1.140***	1.092***		
					(0.264)	(0.211)		
payment: \$5k- \$20k							0.443***	0.411***
							(0.047)	(0.055)
payment: \$20k-\$50k							1.183***	1.182***
							(0.382)	(0.325)
payment: \$50k-\$100k							1.217**	1.190***
							(0.495)	(0.453)
payment: \$100k-\$1million							1.330***	1.320***
							(0.160)	(0.193)
payment above \$1million							3.518***	3.087***
							(1.245)	(0.876)
Observations	449004	384321	449004	384321	449004	384321	449004	384321
Age Fixed Effects	x	x	x	x	x	x	x	x
ZIP3 Fixed Effects	x	x	x	x	x	x	x	x
Income quantile Fixed Effects	x	x	x	x	x	x	x	x
Acre quantile Fixed Effects	x	x	x	x	x	x	x	x
controls		x		x		x		x
Credit score centile Fixed Effects	x	x	x	x	x	x	x	x