Personal Wealth and Self-Employment*

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Abstract

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Abstract

We examine how wealth windfalls affect self-employment decisions using data on cash payments from claims on Texas shale drilling to people throughout the United States. Individuals who receive large wealth shocks (greater than $50,000) have 51% higher self-employment rates. The increase in self-employment rates is driven by individuals who lengthen existing self-employment spells, and not by individuals who leave regular employment for self-employment. Moreover, the effect of wealth reverts for individuals whose payments run out. Rather than alleviating a financial constraint, our evidence suggests that unrestricted cash windfalls affect self-employment decisions primarily through self-employment's non-pecuniary benefits.

1 Introduction

Forty percent of U.S. workers go through at least one self-employment spell during their lifetime (Astebro et al. (2014) and Parker (2009)). Recent literature shows that alleviating barriers to self-employment, such as financial constraints or fear of downside risk, can lead individuals to enter self-employment, spurring job creation and business dynamism (e.g., Hombert et al. (2020); Herkenhoff, Phillips, and Cohen-Cole (2018)). An important class of policies – namely, cash transfers – does not affect barriers to self-employment directly, but rather indirectly through the impact on personal wealth. Although cash windfalls undoubtedly alleviate financial constraints, an influx of personal wealth may lead people to want to work less and take leisure in non-employment. On the other hand, if people derive utility from owning a business, personal wealth could help sustain self-employment activities through this "consuming self-employment" channel (Hurst and Pugsley (2011, 2017)). Thus, the net effects of personal wealth on self-employment are unclear.

This paper evaluates the effects of personal wealth on self-employment by using large cash windfall payments to individuals from the discovery and extraction of shale natural gas during the fracking revolution. Crucially, though the payments come from mineral rights claims in Texas, windfall recipients in our sample reside in all 50 states and are not necessarily land owners because claims to mineral rights are often distinct from the ownership of surface property (see Figure 1). We find that large positive shocks to personal wealth increase self-employment through lengthening existing self-employment spells, not through entry into self-employment, and that individuals exit self-employment when wealth shocks dissipate. In this way, our evidence stands in contrast to the view that personal wealth discourages work. Indeed, our evidence suggests that individuals respond to an influx of personal wealth by "consuming self-employment" rather than forsaking employment altogether, or taking substantially more employment risk (e.g., through starting high-growth ventures).

Our focus on cash windfalls from the extraction of shale natural gas has a number of notable empirical advantages. First, the shale natural gas setting is attractive for identification. We compile novel data on payments to individuals from shale gas extraction, which we merge with self-employment data from Experian. Because the timing and amounts of individual payments from shale are difficult to anticipate, this setting enables clean identification of how wealth windfalls affect self-employment decisions. In support of using cash payments from shale extraction to identify the effect of personal wealth on self-employment, we provide evidence of parallel trends in self-employment decisions around the timing of individual payments for those who receive them versus matched controls. Furthermore, after conditioning on mineral acreage owned and geographic fixed effects, most observable characteristics are similar for those who receive payments versus those who do not. Importantly, our tightest identification rests on the distinction between individuals who receive large payments (greater than $50,000) versus small payments, which 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).[1] Our sample of Barnett Shale mineral owners resides all over the U.S., which enables our tests to distinguish the effect of of cash windfalls from the effects of expanding local opportunities due to the shale shock.

Second, studying windfalls from shale natural gas is also attractive from the standpoint of external validity. Though our sample of mineral owners is different from a national random sample, mineral ownership is prevalent in the U.S. (12 million Americans have received mineral payments according to the National Association of Royalty Owners) and there is significant overlap between our sample and the characteristics of a nationally representative sample of individuals. Specifically, many mineral have both good and bad financial health (14.6% of windfall mineral owners are subprime while 30.8% of in the nationally representative sample are subprime), and a subsample of mineral owners rent and do not own a home. Exploiting this significant overlap, we re-weight our analysis to match the national representative sample characteristics (age, income and credit score) and we find similar results, providing support for the applicability of our estimates to a broader sample. Therefore, our results complement contemporaneous work that focuses on how windfalls affect individuals in specific samples such as lottery winners (Mikhed, Raina, and Scholnick (2019)), or in other countries (Bermejo et al. (2019)), or localized area shocks (Decker, McCollum, and Upton Jr. (2018)).

We find that wealth windfalls have economically significant effects on self-employment rates. Specifically, individuals who receive large windfalls in excess of $50,000 have a 51% greater self-

[1] We describe each of these tests as they relate to our identification strategy in more detail in Section 5. In particular, we observe that individuals who receive small payments versus large payments, but nonetheless have the same mineral acreage, are on similar trends with respect to self-employment decisions around the timing of first payment. We also conduct a placebo exercise for individuals who receive small payments (below $10,000), finding a small, but not economically and statistically meaningful effect.

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.

There are several potential mechanisms for why personal wealth could increase self employment: personal wealth could alleviate financial constraints (e.g., Herkenhoff, Phillips, and Cohen-Cole (2018)), personal wealth reduces individuals' need to worry about the downside risk of business failures (e.g., Hombert et al. (2020)), or personal wealth could make self-employment more attractive through demand for its non-pecuniary benefits (e.g., Hurst and Pugsley (2017)). Of these channels proposed by the literature, our evidence is most consistent with the non-pecuniary benefits channel. Three empirical tests support this interpretation. First, when we examine the effect separately for initially self-employed versus initially regularly employed, we find that most of the effect of personal wealth is on extending existing self-employment spells, not spurring new self-employment activities. Second, we find that, once individuals stop receiving shale royalty payments, they tend to exit self-employment for regular 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. Third, we find similar or larger effects of personal wealth on self-employment for unconstrained borrowers, suggesting that the main effect is not driven primarily by shocks to personal wealth alleviating a financial constraint. Taken together, these results suggest that cash windfalls from shale extraction, and wealth more broadly, are being used to subsidize marginally successful businesses, consistent with leisure and non-pecuniary benefits of self-employment. This evidence, which contrasts with the financial constraints view, is consistent with recent evidence that ample bank financing to individuals is available, as is shown by Robb and Robinson (2014).

Finally, aside from non-pecuniary motives, 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 skills 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 a similar effect of wealth on self-employment for younger cohorts versus older cohorts, 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 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.[2]

Our paper makes a number of notable contributions. First, our paper contributes to the understanding of the economic barriers to self-employment. Broadly, this literature focuses on two classes of barriers: financial constraints coming from limited access to credit markets and protections against the downside risk of personal-business failures. The literature on financial constraints and self-employment considers the effects of shocks to the banking sector (e.g., Black and Strahan (2002); Kerr and Nanda (2009); Chatterji and Seamans (2012); Hanspal (2016); Fracassi et al. (2016)), shocks to personal collateral (e.g., Adelino, Schoar, and Severino (2015); Naaraayanan (2019)), and shocks to personal credit constraints (e.g., Bos, Breza, and Liberman (2018); Herkenhoff, Phillips, and Cohen-Cole (2018); Dobbie et al. (2020)), the latter of which, like our work, combines personal credit data with data on self-employment.[3] On the other hand, the literature on

[2]We also estimate 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 unrelated to high growth entrepreneurship and is consistent with the results on the nature of unincorporated self-employed in Levine and Rubinstein (2017).

[3]Related to the literature on personal collateral, there is a well-developed literature on collateral constraints from housing wealth that originated out of Hurst and Lusardi (2004)'s proposal to use housing wealth shocks to understand how wealth affects self-employment and entrepreneurship outcomes. Although a few papers directly tie self-employment to house price appreciation (Corradin and Popov (2015); Harding and Rosenthal (2017)), research has taken a critical view of the ability of households to realize housing wealth (Bhutta and Keys (2016)), and the literature has come to understand that housing wealth affects self-employment primarily through alleviating a collateral constraint (Kerr, Kerr, and Nanda (2015); Schmalz, Sraer, and Thesmar (2017)). This collateral constraint is particularly relevant because early-stage entrepreneurs often use their personal credit to secure bank financing at the time of business formation (Robb and Robinson (2014)). Related work studies the effects of constraints on existing entrepreneurs and within firms (e.g., Howell (2017)).

downside protections focuses on how social insurance can encourage increased risk-taking by potential new business owners (e.g., Gottlieb, Townsend, and Xu (2016); Hombert et al. (2020)). Our paper tests the effects of shocks to personal wealth on self-employment. Greater wealth can increase self-employment by alleviating financial constraints or by reducing downside risk, but in contrast to other barriers, personal wealth might have an effect on self-employment through affecting the individual's preference for self-employment. To the extent that self-employment is a normal good, as is argued by Hurst and Pugsley (2017), personal wealth could lead to more self-employment through this alternative "consuming self-employment" channel. Our paper's contribution is to provide rich evidence on the net effect of personal wealth on self-employment and self-employment entry and exits. We find that personal wealth shocks primarily affect self-employment levels by extending self-employment spells, and provide evidence consistent with self-employment being a normal good (i.e., having non-pecuniary benefits).

Our interpretation, that wealth shocks support the non-pecuniary benefits of self-employment, also relates to the literature that studies the motivations of business owners and the different types of self-employed (e.g., Blanchflower and Oswald (1998); Schoar (2010); Levine and Rubinstein (2017)). Much prior literature equates self-employment with a desire to be entrepreneurial (e.g., Glaeser (2007)). If entrepreneurship is the objective of a self-employment spell, then drivers of self-employment can include personal characteristics, such as confidence, risk-tolerance, and abilities (e.g., Kihlstrom and Laffont (1979); Lazear (2004); Puri and Robinson (2007); Andersen and Nielsen (2012); Hvide and Panos (2014); De Meza et al. (2019)), as well as rational responses to demand shocks and labor market restructuring (e.g., Bernstein et al. (2018); Bermejo et al. (2019); Kleiner and Hacamo (2019); Babina (2020)). However, recent work augments prior literature by showing that many self-employed do not aspire to be entrepreneurs (in the classical sense) and instead enjoy the non-pecuniary benefits of business ownership (Hurst and Pugsley (2011)). Related, there is documented evidence of "serial entrepreneurs" (e.g., Lafontaine and Shaw (2016)), and though their persistent pursuit of new ventures can be consistent with project experimentation (e.g., Manso (2016); Dillon and Stanton (2017)), it could also be evidence of non-pecuniary motives, such as a desire to be one's own boss. In our empirical setting, we show the empirical relevance of the "consuming self-employment" channel, and distinguish it from these notable alternatives from the literature – namely, experimentation, tolerance for risk/failure, and local demand shocks.

Finally, our paper relates to prior work on the effects of personal wealth shocks on labor market decisions and business formation by individuals.[4] Papers in this literature most often rely on wealth shocks brought about by lottery winnings and interpret the lottery winnings as relaxing financial constraints on business entry (e.g., Lindh and Ohlsson (1996); Cesarini et al. (2017); Mikhed, Raina, and Scholnick (2019)). We contribute to this literature by introducing a different type of wealth shock, royalty payments from natural resource extraction, which reaches a broad segment of the U.S. population and has considerable variation in size, timing, and geography.[5] Furthermore, several of these recent papers only consider the effects of wealth shocks on individuals who are predisposed to self-employment, namely existing business owners (e.g., Cespedes, Huang, and Parra (2019)). These papers find that wealth shocks alleviate financial constraints on existing business owners, whereas we find no evidence that wealth shocks increase transitions into self-employment from the general population. Instead, the positive relation between wealth and self-employment we document is driven by wealth shocks merely extending pre-existing self-employment spells, a finding that is new to this literature.

2 Setting and Data

2.1 Overview of Barnett Shale and Mineral Rights

Our study focuses on a sample of oil and gas mineral owners with claims to mineral extraction in the Barnett Shale of Texas from 2005 through 2015. The Barnett Shale was the first shale gas development in the United States. Before the mid-2000s, shale gas had been uneconomic to drill and develop. However, the combination of horizontal drilling with hydraulic fracturing ("fracking"), by

[4]Beyond labor market outcomes, a kindred line of research studies the effects of wealth shocks on a variety of other outcomes including asset market participation, credit market outcomes, personal happiness, and health (Hankins, Hoekstra, and Skiba (2011); Cesarini et al. (2016); Cookson, Gilje, and Heimer (2019); Briggs et al. (2020); Agarwal, Mikhed, and Scholnick (2020); Lindqvist, Östling, and Cesarini (2020)). Additionally, there is a literature in economic development that studies the effects of cash transfers on business formation and entrepreneurship (see e.g., Bianchi and Bobba (2013); Blattman, Fiala, and Martinez (2014)).

[5]Related work examines how aggregate new business opportunities originating from the Fracking Boom affects the entry of new firms and the expansion of preexisting firms (e.g., see Decker, McCollum, and Upton Jr. (2018)). Though our work shares an interest in the entry and exit dynamics linked to fracking activity, our measurement of the shock at the individual level allow us to speak to the role of personal income, as distinct from industrial responses to regional opportunities, which has been the focus of most of the research on the role of new firms in responding to the shale shock. More broadly, our work uses unique variation within the literature that has exploited variation from the shale oil and gas extraction context. For example Gilje (Forthcoming), Gilje, Loutskina, and Strahan (2016), Cunningham, Gerardi, and Shen (2017), and Feyrer, Mansur, and Sacerdote (2017).

Devon Energy and George Mitchell, led to a technological breakthrough which allowed vast new quantities of natural gas to be developed. According to 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 shale development in the United States. It was also among the most prolific – the four Barnett Shale counties in our analysis accounted for 17.3% of total U.S. shale gas production when shale production 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 receiving payments at that time), and it is the first year in which credit bureau data was available to us.

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)." Moreover, there was a broadly held view in the United States that natural gas supply was running out and no new sources of supply (i.e., shale) would be developed. For example, in Senate testimony in 2003, Alan Greenspan stated:

> "Today's tight natural gas markets have been a long time in coming, and distant futures prices suggest that we are not apt to return to earlier periods of relative abundance and low prices anytime soon."

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. In the context of our empirical design, Section 5 provides evidence that individuals did not anticipate the payments they would eventually receive (as of 2005 and even after 2010) – we find parallel trends in self-employment rates between those who receive large windfalls against our control samples.

Who owns mineral rights in the United States? These rights typically reside with individuals whose families were involved in the initial permanent settlements of oil and gas producing regions of the United States. The mineral right were then often severed from surface rights when those families migrated elsewhere. According to news articles and other anecdotes about mineral ownership, the discovery of shale and the resulting wealth shock was largely unexpected by individual recipients. For example, the following characterization by Kiplinger's Magazine is relatively common:

> "Pam Cooner, 42, an occupational therapist in Houston, has collected about $15,000 in the past year for a fractional ownership of mineral rights. Cooner was surprised when contacted by a landman about the rights. She didn't know she'd inherited them—as had 13 other distant family members. In August, Cooner got a $400 royalty check for rights on another property, owned jointly with a different, non-family group—again, a total surprise."

For those fortunate to own minerals, which typically occurred through family ancestry, the shale breakthrough caused the mineral rights, previously a deep out-of-the-money option, to become a valuable cash-flow stream when natural gas was drilled. Therefore, although people who own minerals are not a purely random sample of U.S. individuals, the increase in personal wealth these mineral owners experience was due to an exogenous technological breakthrough over which these individuals did not have control.

2.2 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 private individuals constitute our sample of royalty recipients. Contracts to develop oil and gas compensate a mineral owner in two ways. 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. In Texas, the law is such that royalties are computed based on gross revenues, and no costs can legally be deducted from the gross revenue.[6] For example, if a well generates gross revenue of $10,000 in a month, and an

[6]There are popular press accounts of individuals with mineral rights in other U.S. regions who had a dispute with an extraction company over the costs that the company could deduct before paying the leaseholder (e.g., "Millions Own Gas and Oil Under Their Land. Here's Why Only Some Strike It Rich" from NPR, *March 15, 2018*). However, these "deduct

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. Fortunately, in the state of Texas, unlike in other states, mineral owners are required to pay property tax. 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. We use this royalty interest information 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, we used open record requests to obtain the detailed title and ownership information that private firms paid millions of dollars to construct. The data are provided in PDF format, which required us to convert the images into usable data. 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. Though shale drilling eventually expanded to states outside of Texas, the identities of individuals who own mineral rights to oil and gas wells in other states is not easily attainable. 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.

A crucial feature of our mineral roll appraisals is that it provides the address at which the mineral owner receives tax bills. This accurate address is useful for ensuring a high quality merge with credit bureau data. Furthermore, we used the well ownership percentages to calculate individuals royalty payments. To do so, we matched 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 combined with ownership information from the mineral rolls, is sufficient to calculate the amount of each individual check.

clauses" are extremely rare, and it is highly unlikely that an extraction company would be successful in seeking them informally because Texas is a state with "no implied duty of marketability." In practice, this feature of Texas state law implies that – if the parties did not spell it out explicitly in the original contract – the default contract is to not deduct costs. From a practical standpoint, we separately see bonus versus royalty payments. If we only consider bonus payments for our tests – which are unaffected by the issue of deducting costs – we obtain the same main result.

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 to predict the lease bonus payment on a dollar-per-acre basis using 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 remaining sample in which we do not have direct data on bonus payments.

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. Crucially, as shale development increased over time, there was a high degree of spatial heterogeneity in well production. This variation frequently caused individuals to receive vastly different payment amounts, even when these individuals are from the same region and own similar levels of mineral acreage.

2.3 Experian Data Overview

From the raw data, 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.[7] We provided information on payments, names and addresses, and Experian conducted the merge on name and address, and returned the merged data to us without the personally-identifying data fields. 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 match 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.[8]

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 is a textual field that 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 people who transition into or out of self-employment. By contrast, individuals who work for Technology firms account for less than 5% of these switchers. This classification exercise provides some additional context on the types of individuals driving the self-employment decisions we identify in our main tests.

To our knowledge, our research is the first academic use of Experian's employment data. As such, we conduct a battery of tests to confirm the validity of the data. In particular, we benchmark the propensity to be self-employed from the credit bureau to two other data sources used throughout

[8] For a detailed discussion of this data merge in the context of household debt, see Cookson, Gilje, and Heimer (2019). 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.

the literature — the American Community Survey and Current Population Survey. To illustrate this comparison, we plot self employment propensity at the state-year level drawn from our nationally representative sample from Experian and find a high degree of correlation with measures of unincorporated self-employment in the public use microdata (Figure 2). Overall, the credit bureau data offer a measure of self-employment consistent with these other data sources at the levels of aggregation in which we can draw the comparison. However, our self-employment field is useful because 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 data is uniquely positioned to observe a substantial number of other attributes on self employed individuals, including credit characteristics and oil and gas royalty wealth shocks.

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 3, the impact of personal wealth on 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 provide us with payment information to individuals for royalty payments between 2005 and 2015. However, the credit bureau data only have employment information from 2010 onward. We account for this discrepancy by utilizing both a cross-sectional analysis of our entire sample of mineral owners, as well as a panel analysis on a sub-sample of owners who begin receiving payments after 2010. All of our subsequent tests compare the effects of wealth windfalls across individuals. We define the "treatment group" as those who received more than $50,000 in cash payments (we perform sensitivity analysis around this cutoff) and define the "control group" as individuals who 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 cross-sectional sample as of 2015. The mean self-employment rate in our sample of 1.77% is somewhat lower than self-employment rates reported in public use microdata (ACS or CPS). The lower self-employment is also present in the nationally representative sample (see the range on the horizontal axis in Figure 2), which indicates that the self-employment measure provided by Experian undercounts some forms of self-employment (e.g., part-time self-employed).

In addition, individuals in our sample have an average income of $57,000, an average credit score of 738, and are 49 years old on average. These summary characteristics indicate that our sample tilts slightly toward older individuals who have higher income and higher credit scores. However, as we show in Figure 4, there is substantial overlap among individuals in our sample who receive mineral payments versus the distribution from a nationally representative sample drawn from Experian. From the standpoint of external validity, we exploit this overlap to re-weight our regressions to the representative sample characteristics, as a robustness exercise in Section 5. To enhance the validity of our empirical tests, we construct a matched control sample using a propensity score matching on initial credit characteristics in 2005.

The variable of interest in our regressions is whether an individual receives a large payment. To give context for the underlying variation in payments, Figure 5 presents a histogram of total payments received between 2005 and 2015, grouped by $10,000 bins. The distribution is right skewed, with a significant number of people receiving small windfall amounts. We choose to set $50,000 as the cutoff for large payments because $50,000 is approximately as large as a typical individual's annual income. Approximately 10 percent of the sample of people who received mineral payments received large payments exceeding $50,000. In a sensitivity analysis, we find that the choice of this cutoff does not qualitatively affect our results.[9]

To compare the matched control sample to the mineral owners who receive large payments, Table 2 reports how observable characteristics compare across the treatment and control groups for

[9]Indeed, as nonparametric visual evidence, the bin scatter plot presented in Figure 6 indicates that the effect of wealth is persistent throughout the sample. We also explicitly vary the cutoff for large payment in a series of robustness exercises.

our cross-sectional estimation (Panel A) and panel estimation (Panel B). 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. In our two samples, the raw differences in individual characteristics are economically and statistically meaningful. As such, we control for these differences by saturating our regressions with granular fixed effects to control for age, zip code, income, mineral acreage size, and ex ante credit score. We can compute an adjusted difference where we look at differences after controlling for these fixed effects. In both samples, 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 2, there are some observable differences between individuals who receive large wealth shocks, and individuals who do not. We employ numerous fixed effects to absorb 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. Figure 7 shows that there is no differential pre-trend prior to the initial wealth windfall, which provides evidence in support of our research design.

3 Effects of Shale Royalties on Self-Employment

3.1 Empirical Specification

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

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

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 by three digit ZIP codes.

This specification conditions the effect of receiving a large windfall on a set of granular fixed effects: γ_z are three digit zip fixed effects, γ_{age} are age fixed effects (dummies for each year of 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 mineral 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.

3.2 Main Results on Self-Employment

Table 3 presents the results from estimating equation (1). We include several different functional forms of "large payment" to 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 0.90 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 0.93 percentage point effect when including a wide set of individual-level controls measured as of 2015.[10] Overall specifications (5) and

[10]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

(6) suggest an increased self employment rate of approximately 51% to 53% of the baseline rate (baseline rate of 1.77%). Our identification relies on the assumption that in the absence of receiving treatment, self-employment rates would be similar for treatment relative to control. Section 5 provides evidence in support of this assumption.

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.

4 Mechanisms

The prior section finds that increased personal wealth from shale royalties has a large positive effect on self-employment. This result provides evidence that on net, increased personal wealth does not discourage people from being self-employed. This section establishes a mechanism for the positive relation between personal wealth and self-employment. We consider three plausible mechanisms that are relevant to our setting – financial constraints, downside risks of business failures, and non-pecuniary benefits – each of which would have distinct predictions about the nature of the personal-wealth-self-employment relationship.

First, much prior literature views wealth shocks as alleviating financial constraints for aspiring entrepreneurs. In the financial constraints view of self-employment, the population contains a set of aspiring entrepreneurs with positive NPV projects who have insufficient capital to initiate such ventures (Evans and Jovanovic (1989)). For these financially constrained individuals, the additional personal wealth is the source of investment capital to start a business. As such, if the positive relation between wealth and self-employment we document was evidence of such a mechanism,

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.

we would expect to find that personal wealth increases the rate of entry into self-employment (i.e., increased transitions from regular employment to self-employment). We would also expect to find that individuals who are more financially constrained when they begin receiving wealth windfalls to have the largest positive response to large wealth shocks.

Related to the financial constraints view, recent literature suggests that the decision to become self-employment is often deterred by the downside risks of business failures. Because starting a business is inherently more risky than regular employment, some individuals may be reluctant to enter self-employment if they are sensitive to earnings risk, for example, because they dislike periods of low consumption. The downside-risk mechanism would have a similar prediction as the financial constraints mechanism in that downside protections would increase entry into self-employment (e.g., unemployment insurance, as Hombert et al. (2020) documents). Here too, we would observe shale windfalls cause an increase in individuals transitioning from regular employment to self-employment.

Instead, our evidence is most consistent with a "consuming self-employment" channel, in which individuals derive utility from being self-employed (Hurst and Pugsley (2011, 2017)). Under this view, individuals choose self-employment over regular employment, because they value the non-pecuniary benefits, such as job autonomy. These individuals are also willing to accept lower risk-adjusted incomes in order to avoid regular employment. As such, we expect that personal wealth shocks would allow those who have previously decided to become self-employed to continue being self-employed even if their business is not self-sustaining. This would lead us to observe personal wealth shocks lengthen preexisting self-employment spells. We would also observe individuals transition back to regular employment when they no longer receive supplemental income from shale royalties. In addition, these "consuming self-employment" types are less motivated by the growth prospects of their business. Consequently, in contrast to the financial constraints and downside risk mechanisms, we would expect to find the effects of wealth shocks to be less sensitive to the growth prospects of the business. Our empirical evidence in the following section supports the "consuming self-employment" interpretation of the positive relation between personal wealth from shale windfalls and self-employment.

4.1 Transitions Into and out of Self-Employment

We provide two complementary tests of transitions into versus out of self-employment. First, we examine transitions into versus out of self-employment in a panel setting. Because the self-employment data begin in 2010, our individual-year panel ranges from years 2010 to 2015. We estimate:

$$self_{i,t} = FE + \beta_1 Post\ treat_{i,t} + \beta_2 Post\ treat_{i,t} \times high\ payment_i + \varepsilon_i, \tag{2}$$

We estimate equation (2) separately for individuals who were initially self-employed in 2010 versus those who were regularly employed in 2010. If initially-regularly-employed individuals transition into self-employment, it would be consistent with a financial constraints channel – i.e., the wealth windfall has given an employed individual the necessary start-up capital to pursue self-employment. Alternatively, if wealth windfalls extend self-employment spells of currently self-employed individuals it would be more consistent with the windfall being used to subsidize a marginal business, consistent with non-pecuniary motives of self-employed individuals (Hurst and Pugsley (2011, 2017)). 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. Note that the direct effect of $high\ payment_i$ is subsumed by individual fixed effects.

Table 4 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 8.4 to 9.6 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 4, the effect is driven by payments in excess of $50,000.

By contrast, when we consider whether wealth windfalls lead individuals to switch from regular employment into self-employment (columns (4), (5) and (6) of Table 4), we either estimate no effect or a small negative effect for payments greater than \$50,000. This evidence highlights important dynamics in self-employment behavior that are consistent with the non-pecuniary motives of self-employment: wealth windfalls increase self-employment rates by extending existing self-employment spells, as opposed to creating new self-employed individuals from currently employed individuals.

As a second test of the non-pecuniary motives mechanism, we evaluate whether individuals revert to normal employment from self employment once the wealth windfall payments run out. This can happen once a well's production has been exhausted. If wealth shocks are used to support self-employment activities of individuals who primarily derive utility from owning a business, we would expect to observe people exit self-employment when this stream of payments runs out. To evaluate this hypothesis, Table 5 augments our cross-sectional regression specification to estimate this effect. Specifically we estimate:

$$self_i^{2015} = FE + \beta_1 large\ payment_i + \beta_2 large\ payment_i \times run\ out_i + \varepsilon_i, \tag{3}$$

where the coefficient of interest, β_2, captures the difference in self-employment rates between two individuals who both receive large windfalls (exceeding \$50,000), but one individual whose payment stream has been exhausted.

Referring to Table 5, the effect of receiving a windfall is fully reversed once the windfall payments stop. One can see this, for example, in column (1) by adding together the coefficients 0.92 (main payment effect) + 0.15 (run out effect) - 2.10 (interaction effect) = -1.03. 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. If the windfall payments had been used to fund positive NPV projects, we would expect these self-employment spells to become 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, consistent with “consumption” of self-employment for non-pecuniary motives.

4.2 Self-Employment and Liquidity Constraints

To provide further evidence on the potential role of wealth windfalls in alleviating liquidity constraints, we augment our main cross-sectional specification to evaluate whether the effect of wealth on self-employment is heterogeneous with empirical proxies for personal liquidity constraints. Specifically, we estimate:

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

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 and credit score). The prediction, if wealth affects self-employment by alleviating financial constraints (e.g., by providing start-up capital), 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 6.

Throughout the table of results, interactions with debt-to-income result in negative and statistically insignificant coefficient estimates (e.g., columns (1) and (2) of Table 6). We also estimate payment interactions with credit scores, to the extent lower credit scores are associated with more financially constrained individuals one would expect a negative coefficient on the interaction term, in fact the estimated interaction coefficients are positive, though not statistically significant (see Table 6, columns (3) and (4)). These results suggest, if anything, individuals facing financial constraints before the fracking revolution are *less* likely to become self-employed for the same large windfall. These results contrast with the view that the impact of wealth on self-employment is attributable to the fact that wealth alleviates liquidity constraints. We are cautious in the interpretation of the results of Table 6, as the proxies we use for financial constraints can correlate with other individual characteristics. However, this evidence, combined with the tests in Tables 4 and 5, provide some evidence against liquidity constraint motives being the primary mechanism that links windfall payments to increased self-employment.

4.3 Heterogeneity by Education and Industry

To further contrast the effect of personal wealth on self-employment with alternative mechanisms (namely, high-growth entrepreneurship), we estimate the impact of wealth on self employment propensities by education 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 increase 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 that education proxies for human capital, and that high human capital is required for high growth businesses, this result suggests that "consuming self-employment" types drive the relation between personal wealth and self-employment in our sample. We report the regression results associated with this figure in Appendix Table A1.

We also use coarse industry classifications from Experian to test whether high-growth sectors affect the relation between personal wealth and self-employment. If people who are self-employed are starting businesses that have the potential for high-growth, we would expect to see larger effects in Professional/Technical industries relative to Blue Collar or Farm related industries. However, as reported in Appendix Table A2, and as we show in Figure 9, we find similar propensities to become self-employed across all industries, suggesting that self-employment rates respond to wealth regardless of industry focus.

5 Robustness and External Validity

5.1 Tests on Validity of Empirical Design

Our identification strategy relies on the conjecture that in the absence of receiving mineral royalty wealth windfalls, treatment individuals who receive large windfalls would have self-employment rates that trended similarly to control individuals who did not receive cash windfalls. However, mineral ownership, and therefore treatment, in our sample is not random (ownership typically relates to family ancestry as these assets are passed down through generations), and there are important observable differences between mineral owners and a national random sample of individuals (e.g., see Figure 4). In this section, we undertake a series of tests to assess whether treatment individuals would have behaved similarly to control individuals if they were not treated. The robustness tests in this section are reported in the Appendix, and in each instance, the main estimate of interest is reported graphically in Figure 10.

First, we document parallel trends in a panel setting between treatment individuals and control individuals for our main tests on self-employed individuals prior to treatment. Our evidence on parallel trends is reported in a leads-and-lags plot that allows the difference-in-difference coefficient in equation (2) to differ by lag (lead) relative to the date of first payment. Figure 7 reports the dynamic results that speak to parallel trends. Consistent with our tabular evidence, we separately evaluate this plot for the initially self-employed sample (left panel) and the initially regularly employed sample (right panel). We also separately report this result for the full estimation sample (Panel A), and the estimation sample restricted to individuals who received some mineral payment (Panel B, treatment-on-treated specifications). Regardless of the specification, we see no evidence of a pre-trend leading up to the year of first payment, supporting the view that the payment (at least whether the windfall would be large) is unanticipated by the individuals in our sample.

Second, there are few observable differences between the treatment group that receives mineral payments and control individuals receiving no payments or low payments after conditioning on the set of fixed effects and controls we employ in our main specifications. This evidence of conditional covariate balance supports our empirical strategy of comparing high payment individuals to the control group, particularly for the cross-sectional specifications (see Table 2 for these comparisons).

Third, we undertake a "treatment on the treated" test that drops from our control sample the individuals who received no payments from natural gas exploration. In this test, we draw a comparison 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. Following the same tabular structure of Table 3, these tests are reported in Appendix Table A4, and we report the key coefficient of interest in this test in Figure 10. We obtain coefficients and bound on these coefficients well within the initial estimates in Table 3.[11]

Fourth, we address the possibility that our main results in Table 3 do not reflect the effects of wealth shocks, but instead capture changes in local investment opportunities. To evaluate this possibility, we limit the sample to mineral owners who live outside of the Barnett shale, and thus, are unlikely to see local opportunities arise from the shale shock. Appendix Table A5 presents the results from estimating the main specifications on the sub-sample of individuals who do not reside in the Barnett Shale area, and we report the key coefficient of interest in this test in Figure 10. Coefficients and confidence intervals fall within the range estimated in Table 3. 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.

Fifth, we evaluate the effects of heterogeneous expectations on payment streams by exploring differences in the fraction of overall payments that come from the upfront bonus versus the royalty bonus stream. To do so, we estimate our main tests using bonus payments only and royalty payments only to compute the high payment mineral windfall variable. We report these results in Appendix Table A6 and we report the key coefficient of interest in this test in Figure 10. We again obtain coefficients with bounds close to our main results in Table 3, suggesting the impact of these potential factors is not a primary driver of our main results.[12]

Sixth, we address the possibility that "stale" employment data from Experian could be driving our results. If receiving a windfall leads to more inertia in our self-employment measure – perhaps through less interaction with the credit registry – our results on extending self-employment spells could be due to having stale employment records. We address this possibility by restricting our sample to individuals who consistently have credit inquiries throughout the sample period, and therefore, have less potential for stale records. For this "Credit inquiry" subsample, we report the results in Appendix Table A7, and we summarize the key coefficient of interest in this test in Figure 10. As is apparent in the results, our findings on the "Credit inquiry" subsample are well within

[11] Related, we have also estimated the model without saturating it with fixed effects and controls, finding very similar magnitudes. The coefficient stability that we have observed in these tests is another measure of comfort against a possible omitted variable bias (Oster (2019)). We report these findings in Appendix Table A3.

[12] We also report results in Appendix Table A6 assuming flat royalty rates across all mineral owners, and average bonus per acre and royalty percentage amounts in a given year for mineral owners, so as to alleviate potential issues of certain types of mineral owners negotiating better terms and self sorting into high windfall categories. These tests fix lease terms constant and re-estimate our main tests. We obtain similar results to our main tests in Table 3.

the standard errors of our main test in Table 3. As further evidence that stale records do not drive our estimates, we find that Experian routinely updates employment status even individuals do not have updates to their credit reports. This finding is consistent with Experian's deep database on individuals and background check services.

Lastly, we include a series of robustness tests in Appendix Table 8 where we estimate our main specifications from Table 3 using different dollar cutoffs to define the High Payment Dummy. We obtain coefficients and interpretations consistent with those in Table 3.

5.2 External Validity

We also undertake two tests that relate to the broader applicability and interpretation of our results.

First, we evaluate whether individuals who received their first payment after 2010 (our panel sample) react to the receipt of large payments as if the payments were unexpected. People in this post-2010 sample may have anticipated wealth windfalls because fracking technologies had been employed for at least five years. To understand whether recipients of later windfalls exhibit anticipation effects, we perform a placebo exercise that mirrors the parallel trends plot in Figure 7. Namely, we identify people who received small windfalls (<$10,000) as treated relative to the control group who received no payments, and we produce a leads-and-lags plot of the effect of receiving a small payment. Figure 11 reports this evidence, which exhibits no effect for initially self-employed, nor initially regularly employed. Crucially, Panel B re-weights this exercise such that the distribution of acreage for small payment recipients matches the distribution for our main treatment group, and we continue to find no effect. If the small payment individuals have similar acreages, they plausibly have similar expectations regarding the eventual payments. Thus, anticipation effects due to forming expectations about eventual payments are unlikely to be driving our findings.

Second, though our sample of mineral owners covers a broad swath of the population, we provide further evidence that our findings can generalize to a nationally representative sample of U.S. individuals. Referring to the the distributions of key characteristics in comparison to the nationally representative sample (Figure 4), our sample of mineral owners is statistically different from a nationally representative sample along several important dimensions. At the same time, we observe significant overlap between our sample and the national random sample.[13] To alleviate the concern that our conclusions would differ in a more representative sample, we re-estimate our main specification, using sample weights to match the national random sample distribution on age, income, and credit score (see Appendix Figure A.5 for an illustration of how our re-weighting matches the national random sample's distribution for age, income and credit scores). We report estimates from this weighted regression in Appendix Table A9, and we report the key coefficient of interest in this test in Figure 10. When we weight to match the national random sample characteristics, our coefficient estimates remain close to our initial estimates, providing support for the applicability of our estimates to a broader sample.

6 Conclusion

Entrepreneurship encompasses a wide range of economic activities. Using a broadly relevant data set of cash windfalls from payments related to shale natural gas extraction, our paper studies the impact of wealth on self-employment decisions and finds evidence consistent with personal wealth affecting self-employment decisions through a non-pecuniary "consuming self-employment" channel (Hurst and Pugsley (2011, 2017)). Recent literature has identified several important barriers to self-employment, including financial constraints and downside risk of business failures (Herkenhoff, Phillips, and Cohen-Cole (2018); Hombert et al. (2020)). When evaluating the impact of wealth on self-employment rates, we find that these alternative channels are not the primary drivers of our results. Indeed, our results are most consistent with the idea that wealth enhances individuals' preferences for the non-pecuniary benefits of self-employment, and that the corresponding increase in self-employment rates resembles a personal consumption choice (i.e., individuals choose to become self-employed because of the non-monetary benefits, such as leisure or job autonomy).

The insights from how each of these channels is operative in the economy are important, as each channel corresponds to different policy targeting and responses. For example, shocks to personal wealth most resemble unrestricted cash transfer programs, whereas other barriers to self-

[13] Using an analysis of the names from the mineral roll data, we were able to impute race of the individuals in our sample to provide some description of the distribution of individuals across various ethnic backgrounds. The imputations from Nameprism are reported in a frequency distribution in Appendix Figure A.4. Unfortunately, the Experian merged data set does not have the race field appended to it, because of federal regulations regarding the use of race in credit services. Thus, we cannot construct sample weights to match a nationally representative sample. However, the data – though tilted toward whites – does indicate a mix of races in our sample of mineral owners.

employment studied in the literature (e.g., unemployment insurance and financial constraints) ought to inform policies that either insure downside risk or alleviate financial constraints directly via the banking sector. Though a systematic comparison of policy options is beyond the scope of our study, our core insight that unrestricted cash windfalls tend to facilitate consumption of the personal benefits of self-employment rather than benefits to the broader economy is important to keep in mind.

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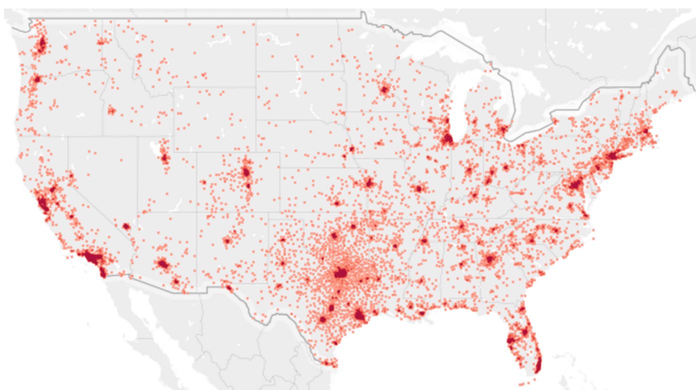
Figure 1: Geographical distribution of the payment

Panel A and B contain 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 their personal location the day they receive their first payment. Panel A plots the spatial distribution of the people in the sample that have received a wealth windfall above $0. Panel B plots the spatial distribution of the people in the sample that have received an oil and gas royalty payment that is above $50,000.

Panel A: Mineral owners



Panel B: High payment

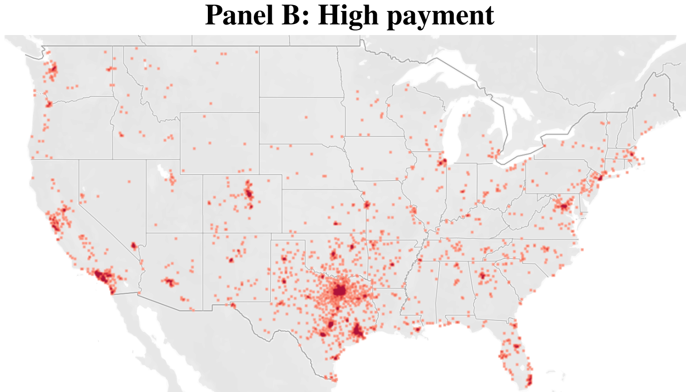
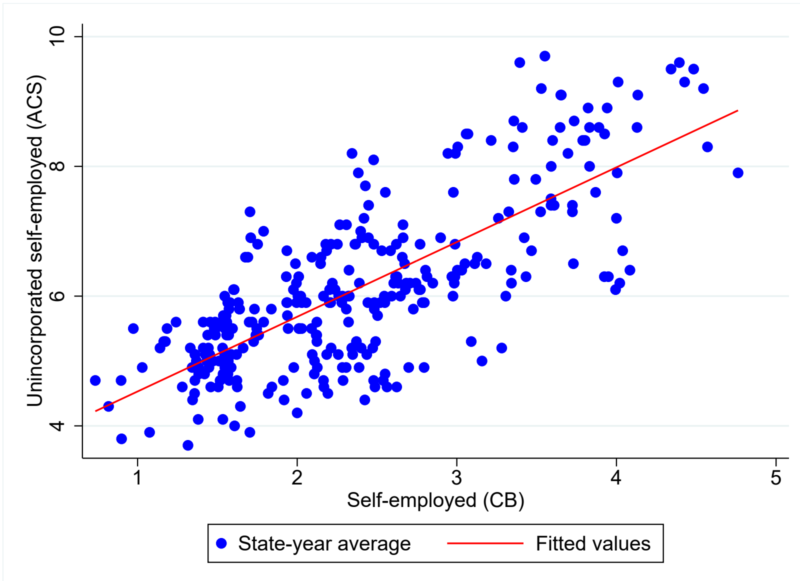


Figure 2: Self-Employment Surveys versus Credit Bureau Self Employment

Panel A 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. Panel B 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.

Panel A: American Community Survey versus Credit Bureau



Panel B: Current Population Survey versus Credit Bureau

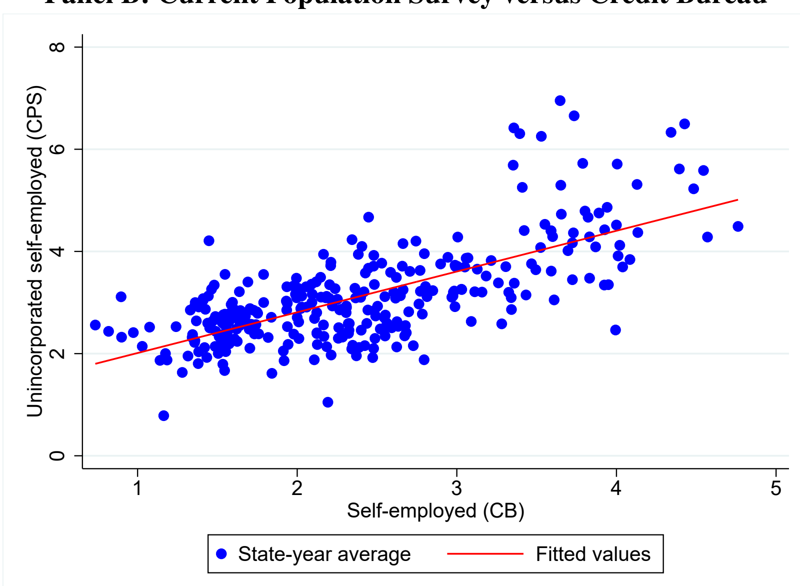


Figure 3: The Effect of Windfall Wealth on Self-employment and Retirement over the Lifecycle

This figure reports the effect of logged wealth windfall on self-employment (and separately retirement), estimated separately for different age ranges (based on age in year 2005). The figure contains both the point estimate effect and 95% confidence interval

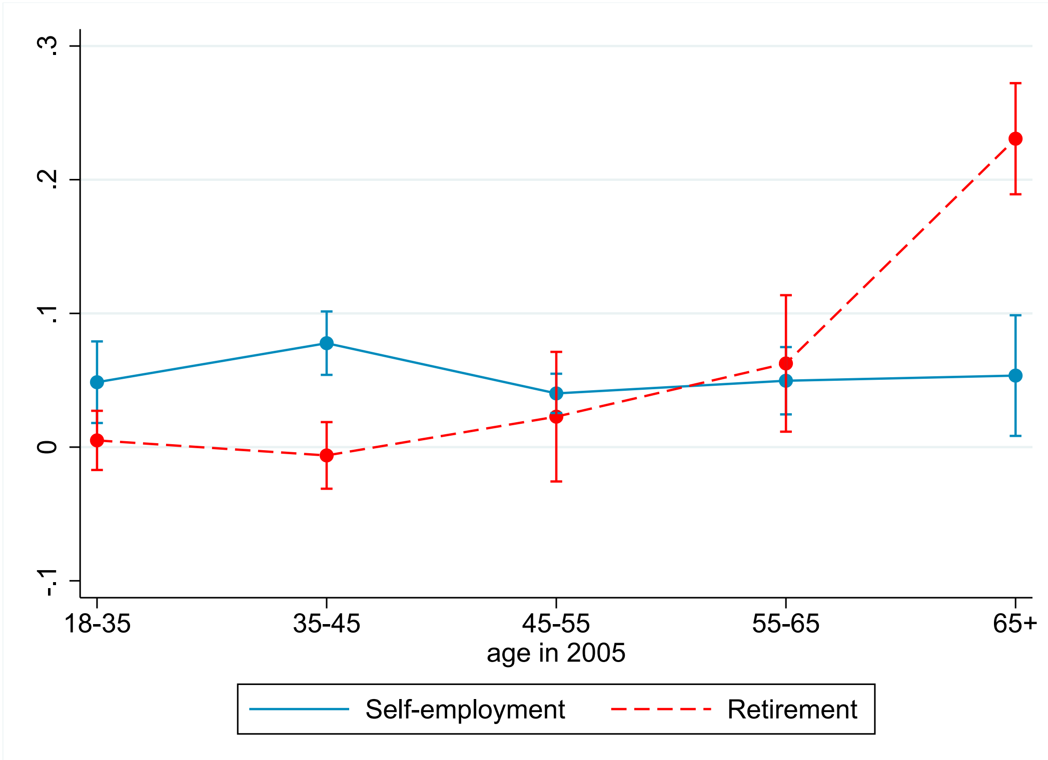
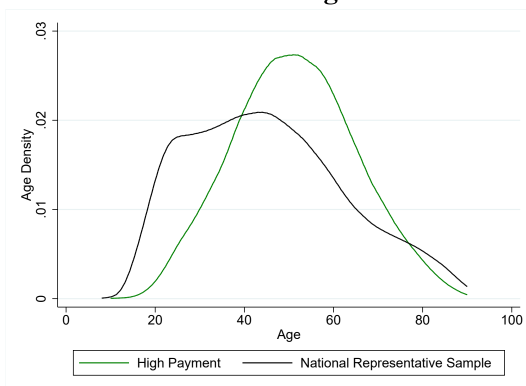


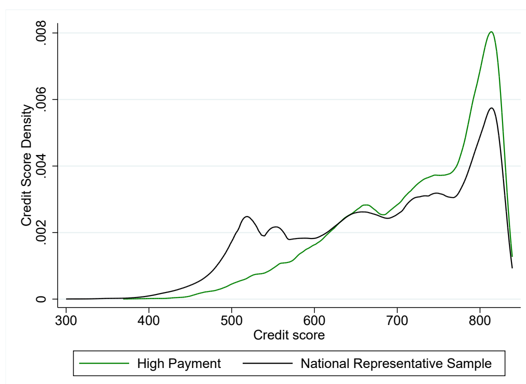
Figure 4: Distribution Comparison: High Payment Mineral Owners vs National Representative Sample

Panel A, B and C report the distribution of the age, credit score and W2 income between individuals that receive a mineral payment above $50,000 and the national random sample.

Panel A: Age



Panel B: Credit Score



Panel C: W2 income

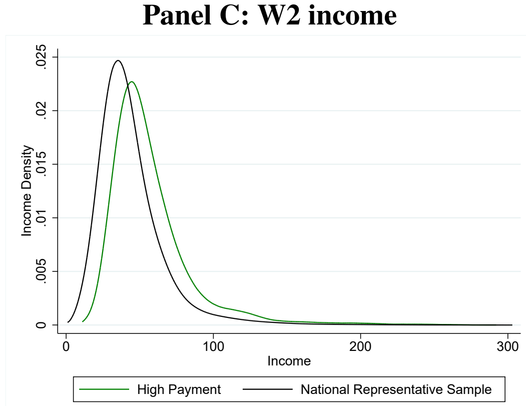


Figure 5: Mineral Payment Distribution

This figure reports the distribution of payments. The first bin represents the number of people that receive a payment between 0 and \$10,000. Similarly, all other bins group people by interval of \$10,000, except for the last one that is composed of all the people that have a payment above \$150,000.

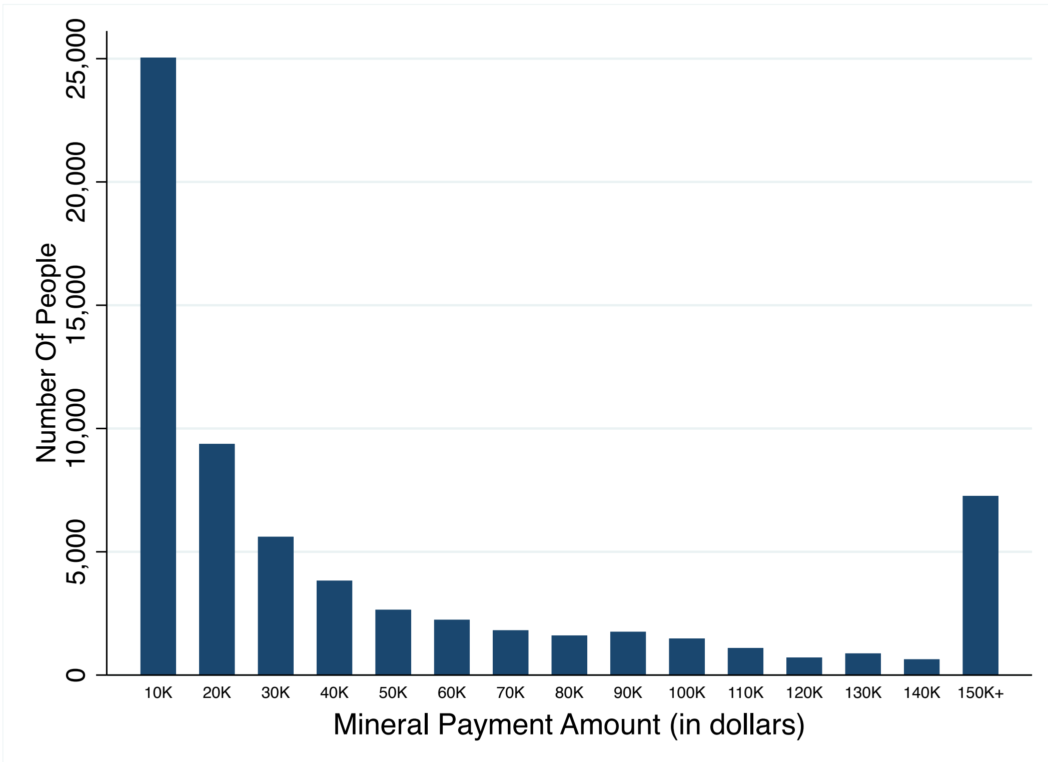


Figure 6: Wealth Shocks and Self-Employment

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) in 2015. This is the raw data with no controls.

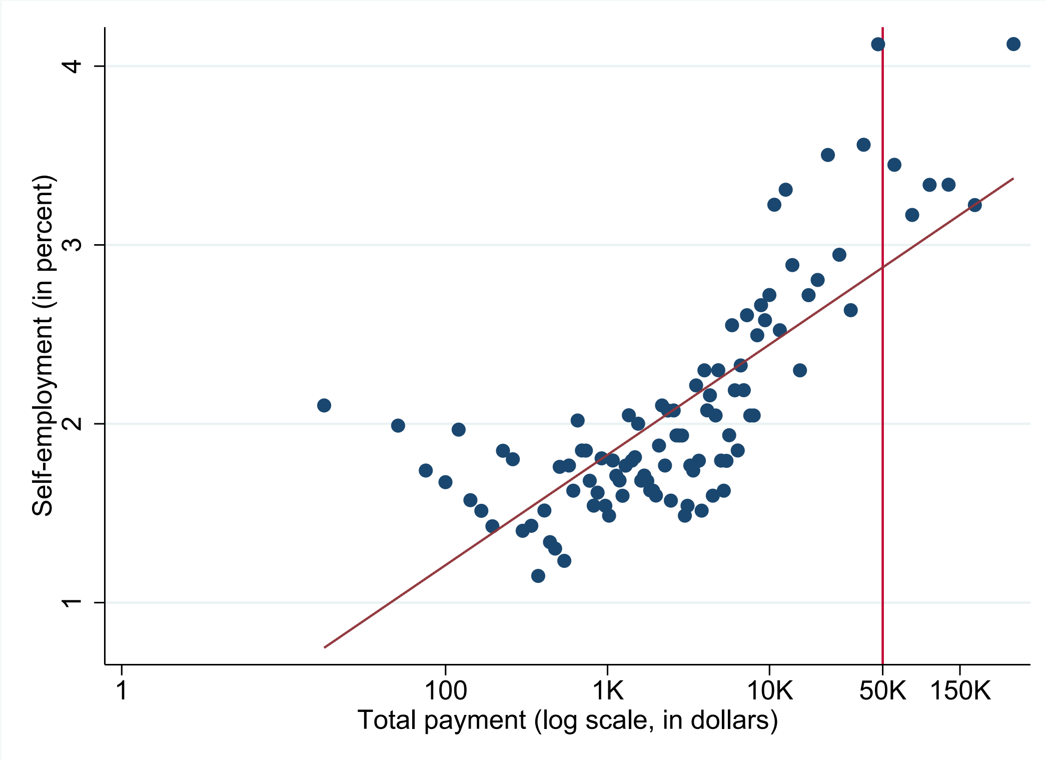
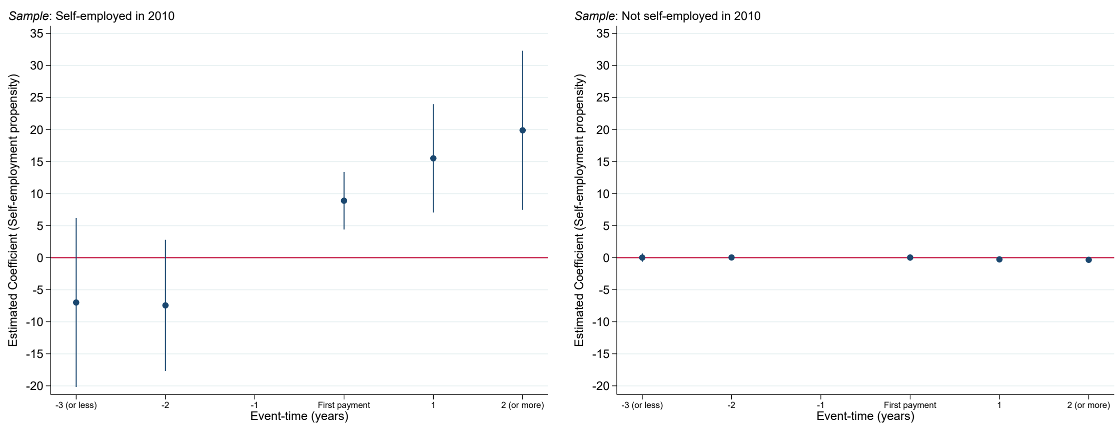


Figure 7: Wealth Shocks and Self-Employment in Event-Time

Panel A and B plot the dynamic graph of the impact of wealth on the decision to become self-employed on the post 2010 sample in a event-time setting. The left (right) figure plots the decision for the group of people that are self-employed (respectively employed) in 2010. In Panel A, the control group is all people that have no payment or a small payment. In Panel B, the control group is all people that have a payment below \$50,000 but strictly positive. The treatment group is all persons that receive a payment above \$50,000 between 2010 and 2015. Confidence intervals at the 95% level around point estimates are plotted. Standard errors are clustered at the three-digit zipcode level.

Panel A: Baseline



Panel B: Within treated

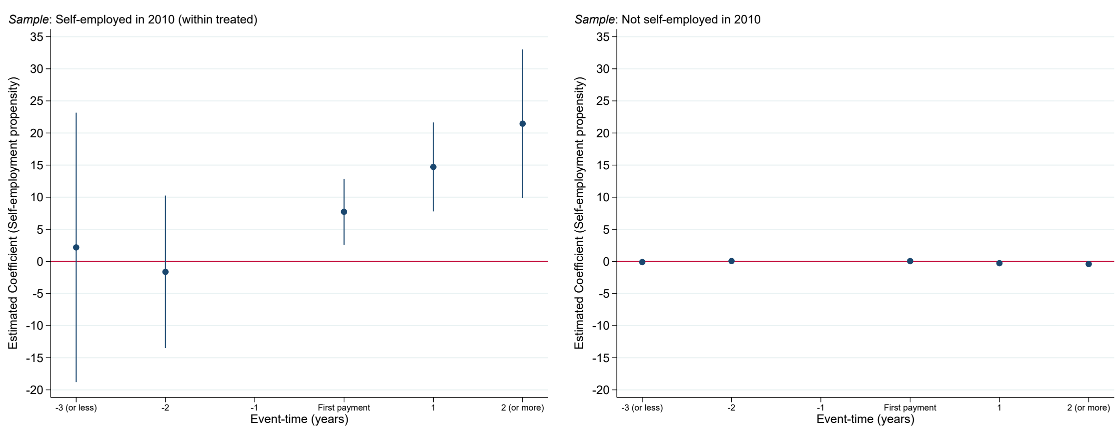


Figure 8: Personal Wealth, Self-Employment and Education

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. The empirical specifications are reported in Appendix Table A1.

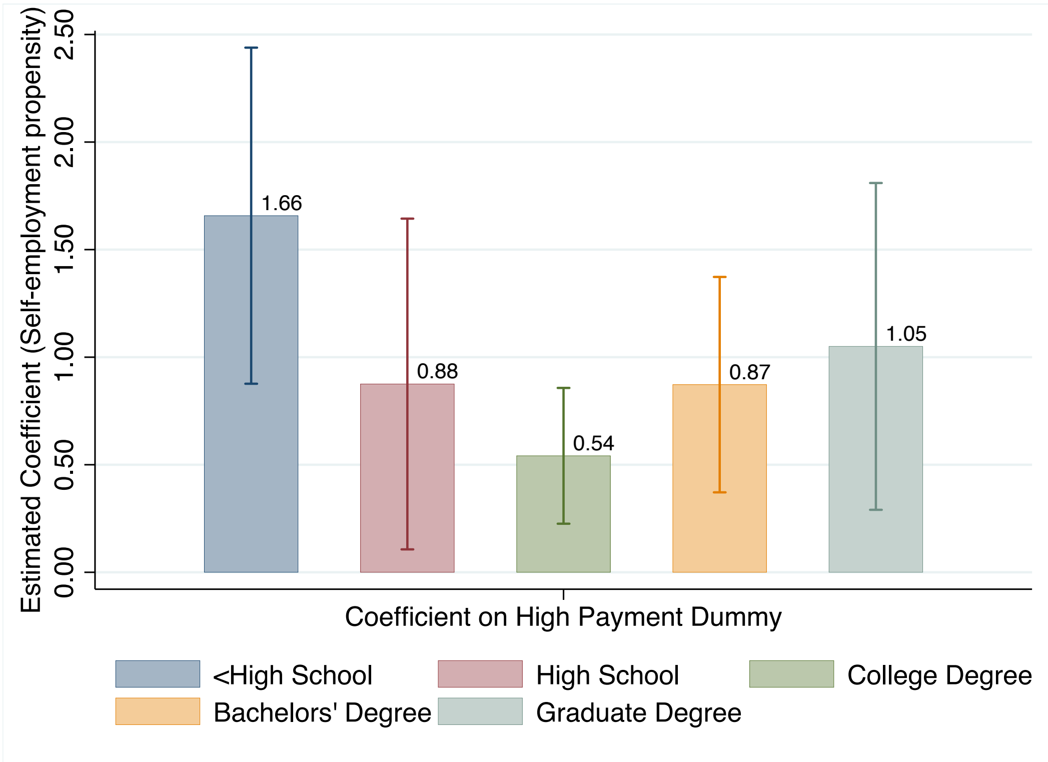


Figure 9: Personal Wealth, Self-Employment and Industry

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. The empirical specifications are reported in Appendix Table A2.

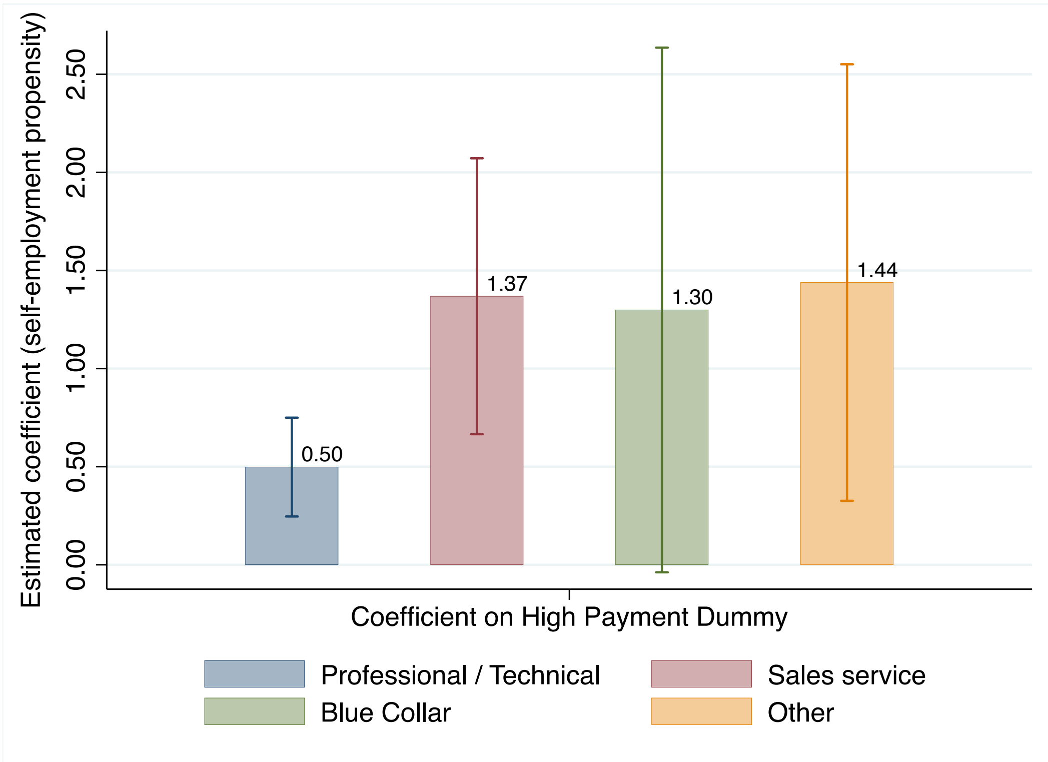


Figure 10: Summary of Robustness Tests

This figure plots the point estimates and confidence intervals of the impact of wealth on the decision to become self-employed using the 2015 cross-sectional sample across a set of robustness tests. More specifically, "Weighted" reports the impact when the national sample weights are used. "Minerals owners" is when the sample on people that receive a payment strictly positive is used. That is, the specification compares mineral owners that receive large payment windfalls with those receiving small payment windfalls. "Outside Barnett" is when we restrict the regression to the people living outside Barnett. "Royalty" (or "Bonus") are estimated when the treatment variable is defined as having royalty (or bonus) above \$50k. "Credit inquiry" restricts the sample to people that have had a credit inquiry in 2015. Finally, ">\$10k" and ">\$100k" estimate the baseline regression when different thresholds are constructed for the treated group, respectively above \$10,000 or \$100,000.

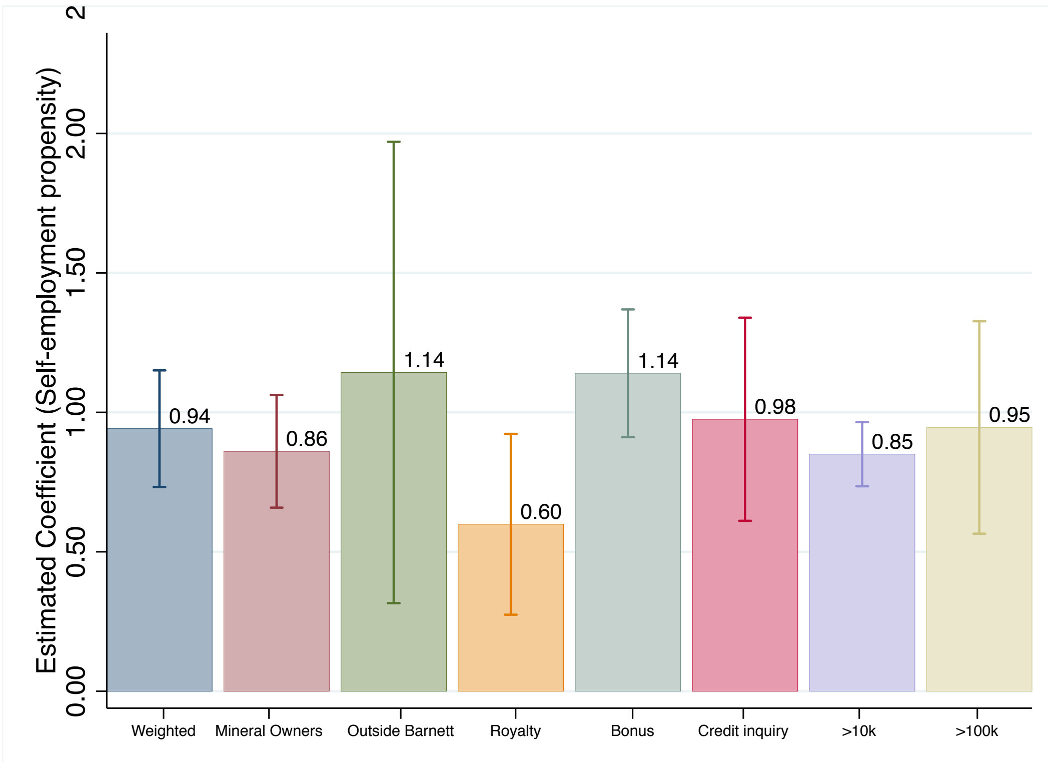
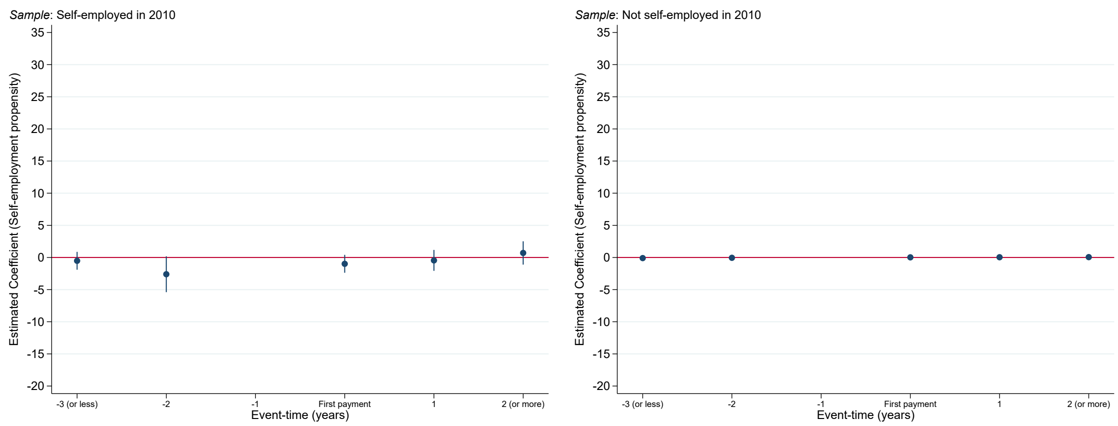


Figure 11: Event Time Placebo Tests

These Figures plot the dynamic graph of the impact of receiving a low wealth windfall (below \$10,000) on the decision to become self-employed using the post 2010 sample. The left (right) figure plots the decision for the group of people that are self-employed (respectively employed) in 2010. In Panel A, the control group is all people that have no payment. In Panel B, we re-weight the control and treated groups so that the distribution of the mean acreage matches the one when the high payment people are included. Confidence intervals are at the 95% level.The y-axis is the same as in Figure 6 to ease the comparison of the magnitudes.

Panel A: Baseline Placebo



Panel B: Matched Acreage Placebo

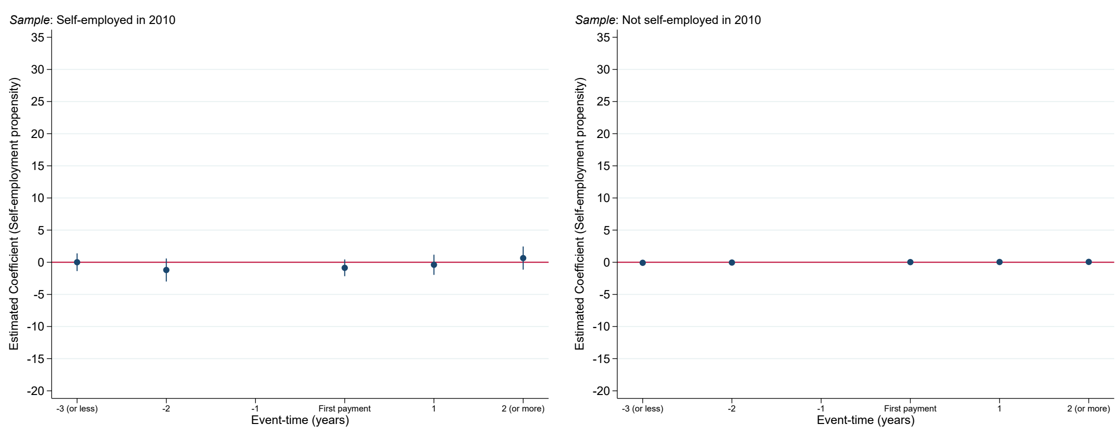


Table 1. Summary Statistics

This table reports the summary statistics for the sample used in the main cross-sectional regressions reported in this study. The unit of observation is at the individual level as of 2015.

	Mean	Std Dev	p25	Median	p75
Self-Employed (%)	1.77	13.18	0.00	0.00	0.00
Retired (%)	4.51	20.74	0.00	0.00	0.00
Income	57.92	25.58	41.00	52.00	67.00
Credit Score	738.49	81.78	681.00	763.00	808.00
Age in 2005	49.89	13.54	40.00	49.00	59.00
Debt to Income	14.41	13.07	2.00	12.00	23.00
Derogatory Trades	0.14	0.68	0.00	0.00	0.00
Delinquent Trades	0.01	0.15	0.00	0.00	0.00
Total Number of Trades	19.56	11.39	11.00	18.00	26.00
Revolving Utilization	0.26	0.28	0.03	0.13	0.42
Subprime	0.14	0.35	0.00	0.00	0.00

Table 2. Comparisons of Treatment and Control

This table reports the differences between treatment and control individuals used in our main cross-sectional (Panel A) and panel tests (Panel B). 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 three digit zip code level. The column Difference reports the raw difference and whether the raw difference is statistically significant. The Adjusted Difference column reports differences when we control for the fixed effects used in the econometric specifications: Age fixed effects, acre fixed effects (quartile), income fixed effects (quartile), and credit score fixed effects (centile). *, **, and *** indicate statistical significance at the 10%, 5% and 1% level respectively.

Panel A: Comparison of Treatment and Control for Cross-sectional tests (2005)

	Treatment Group Mean	Control Group Mean	Difference	Adjusted Difference
Income	57.22	49.09	8.130***	1.654***
Credit Score	722.78	701.89	20.889***	0.015
Age in 2005	51.39	49.74	1.650**	0.000
Debt to Income	18.87	17.47	1.403***	0.986***
Derogatory Trades	0.12	0.19	-0.068***	-0.003
Delinquent Trades	0.01	0.02	-0.006***	-0.001
Total Number of Trades	21.08	18.49	2.583***	0.468*
Revolving Utilization	0.25	0.27	-0.020***	0.000
Subprime	0.15	0.21	-0.063***	0.002

Panel B: Comparison of Treatment and Control for Panel Regression Tests (2010)

	Treatment Group Mean	Control Group Mean	Difference	Adjusted Difference
Retired	3.61	3.75	0.14	-0.770
Self-employed	2.34	1.99	-0.34	-0.139
Income	61.04	52.82	-8.22***	0.831
Credit Score	740.23	703.98	-36.25***	0.071
Age in 2005	51.14	48.43	2.706***	0.00
Debt to Income	12.98	16.22	3.24***	-1.332**
Derogatory Trades	0.10	0.29	0.19***	-0.042**
Delinquent Trades	0.00	0.02	0.02***	-0.007**
Total Number of Trades	18.42	17.97	-0.46	-1.284**
Revolving Utilization	0.22	0.28	0.05***	-0.013
Subprime	0.13	0.21	0.08***	0.010

Table 3. Wealth Shocks and Self-Employment

This table reports coefficient estimates from a cross-sectional regression on all 2015 observations. The unit of observation is individual i in the year 2015. The dependent variable is an indicator for self-employed (x 100 for percentage interpretation). High Payment Dummy 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 at the quintile level 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 three digit zip code are reported in parentheses. (Z) indicates that the continuous variable has been normalized to a mean of zero and a standard deviation of 1 to ease the interpretation the coefficient estimates. *, **, and *** indicate statistical significance at the 10%, 5% and 1% level respectively.

	Dependent variable: 100 × Self Employment Dummy							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
(β_1) Total Payment $(Z)_i$	2.405***	2.415***						
	[0.237]	[0.337]						
(β_2) Log(Total Payment)$_i$			0.054***	0.070***				
			[0.009]	[0.007]				
(β_3) High Payment Dummy$_i$					0.897***	0.928***		
					[0.146]	[0.106]		
(β_4) Payment Dummy \$5k-20k$_i$							0.454***	0.454***
							[0.038]	[0.048]
(β_5) Payment Dummy \$20k-50k$_i$							0.996***	0.971***
							[0.304]	[0.237]
(β_6) Payment Dummy \$50k-100k$_i$							1.155***	1.141***
							[0.319]	[0.257]
(β_7) Payment Dummy \$100k-250k$_i$							1.039***	1.157***
							[0.157]	[0.198]
(β_8) Payment Dummy >\$250k$_i$							1.794***	1.746***
							[0.317]	[0.401]
Age FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
ZIP FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Income Quintile FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Acreage Quintile FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Credit Score Centile FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls		Yes		Yes		Yes		Yes
R^2	0.006	0.007	0.006	0.007	0.006	0.007	0.006	0.007
N	728,121	573,357	728,121	573,357	728,121	573,357	728,121	573,357

Table 4. Self Employment Transitions

This table reports coefficient estimates from a panel regression. The sample is a panel of observations of individuals between 2010 and 2015 that did not receive a payment in 2010 or before. The unit of observation is at the individual i, year t level. The table compares the flows in and out self-employment. The dependent variable is an indicator for self-employed (x 100 for percentage interpretation). High Payment Dummy is an indicator for whether the total payment exceeds $50,000 for a mineral owner between 2010 and 2015. The estimates are from a linear probability model, estimated using OLS. Individual fixed effects are included in the estimate, and depending on the specification year fixed effects and acreage quintile interacted with year fixed effects are included. Standard errors clustered by three digit zip code are reported in parentheses. *, **, and *** indicate statistical significance at the 10%, 5% and 1% level respectively.

	Dependent variable: 100 × Self Employment Dummy					
	Self-Employed as of 2010			Regular Employed as of 2010		
	(1)	(2)	(3)	(4)	(5)	(6)
(β_1) $Post_t$	0.239	0.442	0.420	0.011	0.010	0.011
	[0.634]	[0.560]	[0.576]	[0.009]	[0.009]	[0.009]
(β_2) $Post_t$ × High Payment $Dummy_i$	9.590***	8.403***		0.005	-0.045	
	[3.011]	[2.944]		[0.178]	[0.185]	
(β_3) $Post_t$ × Payment Dummy $5k-20k$_i$			0.893			-0.004
			[1.914]			[0.042]
(β_4) $Post_t$ × Payment Dummy $20k-50k$_i$			-1.413			-0.032
			[5.935]			[0.127]
(β_5) $Post_t$ × Payment Dummy $50k-100k$_i$			8.799**			0.074
			[4.183]			[0.306]
(β_6) $Post_t$ × Payment Dummy $100k-250k$_i$			13.994***			-0.130
			[1.004]			[0.204]
(β_7) $Post_t$ × Payment Dummy >$250k$_i$			5.868			-0.304***
			[7.874]			[0.046]
Individual FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes			Yes		
Acreage Quintile × Year FE		Yes	Yes		Yes	Yes
R^2 Within	0.624	0.624	0.624	0.560	0.560	0.560
N	20,442	20,442	20,442	1,003,194	1,003,194	1,003,194

Table 5. Wealth and Self Employment: Impact of Mineral Payments Ending

This table reports coefficient estimates from a cross-sectional regression on all 2015 observations. The unit of observation is individual i in the year 2015. The variable Run Out_i 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). High Payment dummy 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 at the quintile level 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 three digit zip code are reported in parentheses. *, **, and *** indicate statistical significance at the 10%, 5% and 1% level respectively.

	Dependent variable: 100 × Self Employment Dummy			
	(1)	(2)	(3)	(4)
(β_1) High Payment $Dummy_i$	0.916***		0.945***	
	[0.144]		[0.103]	
(β_2) Run Out_i	0.151	0.226*	0.151	0.220**
	[0.132]	[0.122]	[0.108]	[0.098]
(β_3) High Payment $Dummy_i$ × Run Out_i	-2.068***		-1.807***	
	[0.587]		[0.672]	
(β_4) Payment Dummy $5k-$20k_i$		0.456***		0.455***
		[0.037]		[0.047]
(β_5) Payment Dummy $20k-$50k_i$		1.009***		0.989***
		[0.309]		[0.244]
(β_6) Payment Dummy $50k-$100k_i$		1.194***		1.179***
		[0.324]		[0.260]
(β_7) Payment Dummy $100k-$250k_i$		1.034***		1.147***
		[0.152]		[0.192]
(β_8) Payment Dummy >$$250k_i$		1.814***		1.769***
		[0.317]		[0.405]
(β_9) Payment Dummy $5k-$20k_i$ × Run Out_i		-0.081		0.371
		[0.600]		[0.871]
(β_{10}) Payment Dummy $20k-$50k_i$ × Run Out_i		-1.640**		-2.131**
		[0.813]		[0.996]
(β_{11}) Payment Dummy $50k-$100k_i$ × Run Out_i		-3.087***		-2.924***
		[0.716]		[0.593]
(β_{12}) Payment Dummy $100k-$250k_i$ × Run Out_i		0.704		1.608
		[1.957]		[2.377]
(β_{13}) Payment Dummy >$$250k_i$ × Run Out_i		-4.490***		-4.692***
		[0.270]		[0.288]
Age FE	Yes	Yes	Yes	Yes
ZIP FE	Yes	Yes	Yes	Yes
Income Quintile FE	Yes	Yes	Yes	Yes
Acreage Quintile FE	Yes	Yes	Yes	Yes
Credit Score Centile FE	Yes	Yes	Yes	Yes
Controls			Yes	Yes
R^2 Within	0.006	0.006	0.007	0.007
N	728,121	728,121	573,357	573,357

Table 6. Wealth Shocks and Self-Employment - Heterogeneity by Financial Constraints

This table reports coefficient estimates for mineral windfall and personal balance sheet variables from a cross-sectional regression on all 2015 observations. The unit of observation is individual i in the year 2015. The dependent variable is an indicator for self-employed (x 100 for percentage interpretation). High Payment Dummy is an indicator for whether the total payment exceeds $50,000. The estimates are from a linear probability model, estimated using OLS. Acre and income fixed effects are for at the quintile level 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 three digit zip code are reported in parentheses. *, **, and *** indicate statistical significance at the 10%, 5% and 1% level respectively.

	Dependent variable: 100 × Self Employment Dummy			
	(1)	(2)	(3)	(4)
(β_1) High Payment Dummy$_i$	0.900***	1.083***	0.888***	1.030***
	[0.147]	[0.377]	[0.147]	[0.393]
(β_2) High Payment Dummy$_i$ × Debt to Income in 2005$_i$	-0.005	-0.378		
	[0.094]	[0.314]		
(β_3) High Payment Dummy$_i$ × Credit Score in 2005$_i$			0.048	0.202
			[0.068]	[0.294]
(β_4) Debt to Income in 2005$_i$	-0.078***	-0.231***		
	[0.026]	[0.065]		
(β_5) Credit Score in 2005$_i$			-0.117	-0.259***
			[0.082]	[0.078]
Outside of Barnett		Yes		Yes
Age FE	Yes	Yes	Yes	Yes
ZIP FE	Yes	Yes	Yes	Yes
Income Quintile FE	Yes	Yes	Yes	Yes
Acreage Quintile FE	Yes	Yes	Yes	Yes
Credit Score Centile FE	Yes	Yes	Yes	Yes
R^2 Within	0.006	0.023	0.006	0.023
N	728,121	87,068	728,121	87,068

Appendix to:

Personal Wealth and Self-Employment

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

The left 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 Bureau of Labor Statistics). The unit of observation is at the state-year level. The correlation between the two variables is equal to 0.30. The right 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 Bureau of Labor Statistics). The unit of observation is at the state-year level. The correlation between the two variables is equal to 0.69.

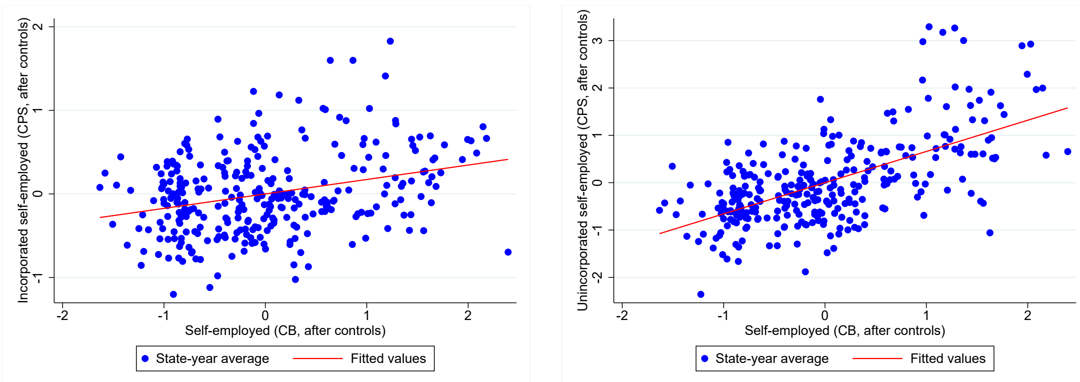


Figure A.2: Self-employment (CPS) validation after controlling for unemployment

The left 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 Bureau of Labor Statistics). The unit of observation is at the state-year level. The correlation between the two variables is equal to 0.69. The right figure plots the fraction of the workforce that is unemployed (from the Bureau of Labor Statistics) (y-axis) compared to our measure of self-employment from Credit Bureau (x–axis). The unit of observation is at the state-year level. The correlation between the two variables is equal to -0.35.

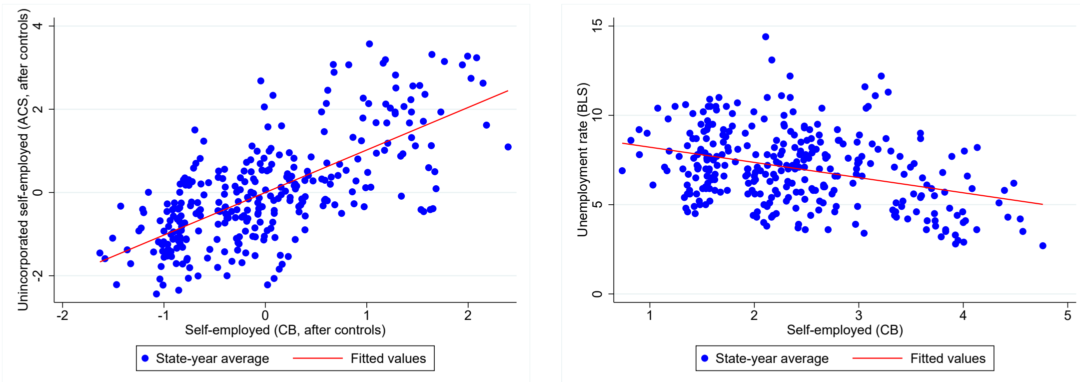


Figure A.3: Analysis of the flows

The left 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. The right 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.

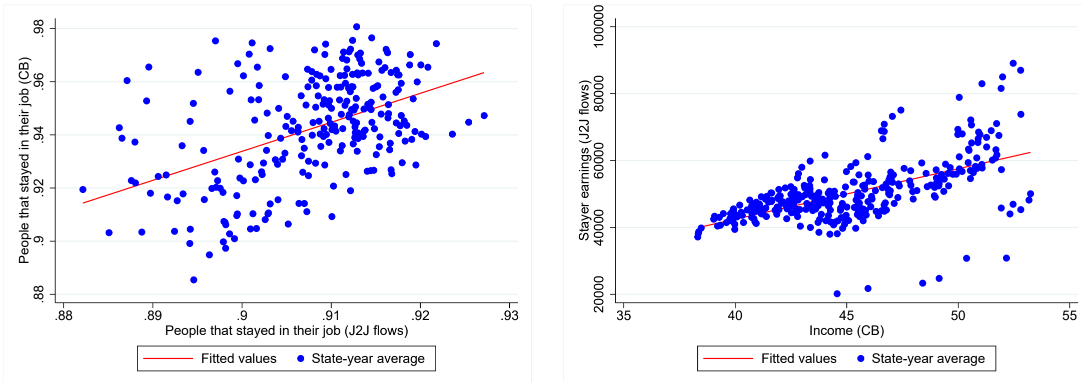


Figure A.4: Ethnicity of the treated group

This histogram plots the estimated race of the people that sign an oil and gas lease. The race is found using Nameprism, a classification algorithm using name embeddings.

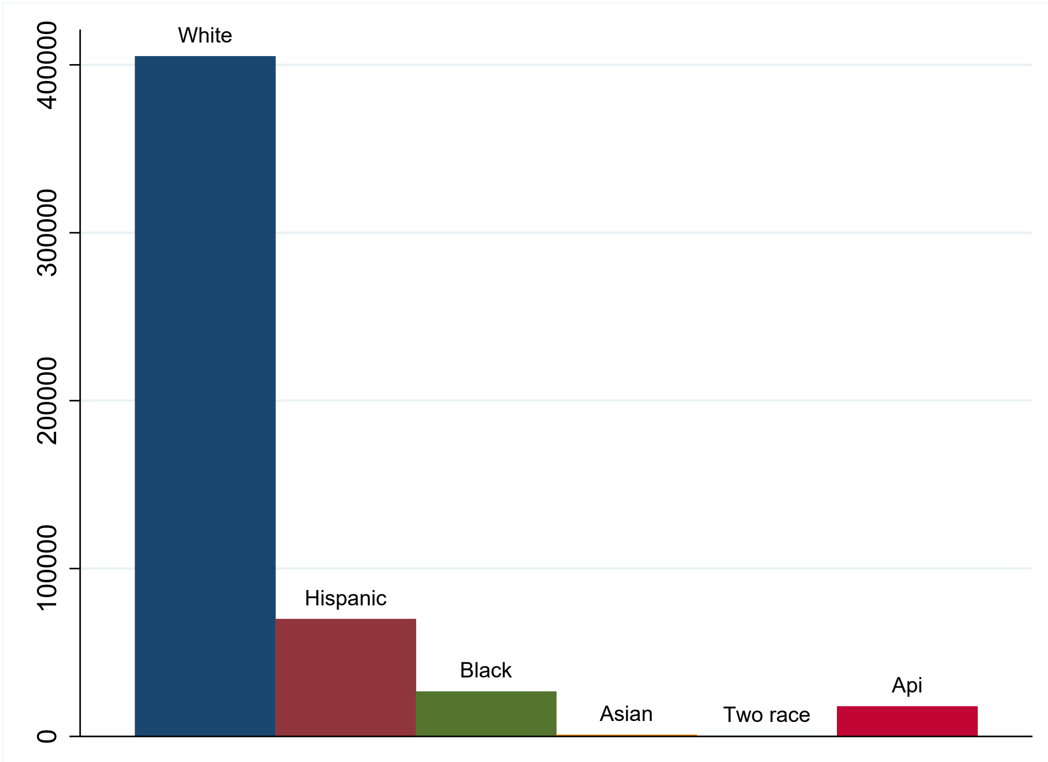
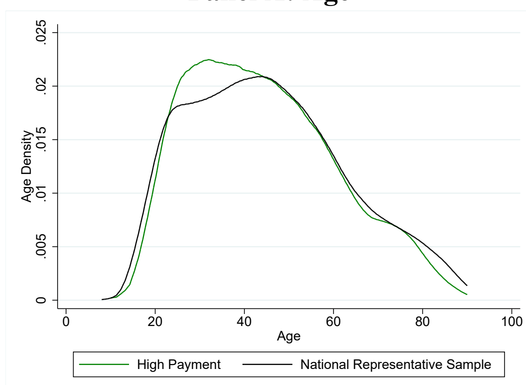


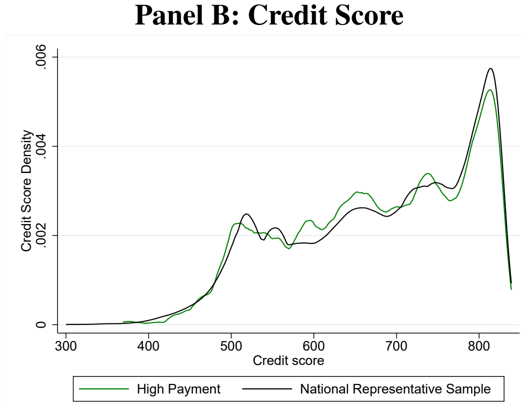
Figure A.5: Representativeness

Panel A, B and C plot the empirical distribution of age, credit score and W2 income from our matched sample once weights are applied to make the sample representative. The weights are constructed using a stratified sampling methodology. First, we group each observations into 10 deciles of age, credit score and W2 income for the representative sample. Then, we calculate the frequency of occurence for each combination of decile. We do the same for our matched sample that includes both the treated group and the control group using the same cutoffs as in the representative sample. Finally we multiply our matched sample by the inverse of the matched sample weights times the weights of the representative sample. The intuition is that it gives more importance to observations that are scarce in the matched sample but appears a lot in the representative sample. We are careful to apply the weights to both the control and treated group, so that we do not create an endogeneity problem.

Panel A: Age



Panel B: Credit Score



Panel C: W2 income

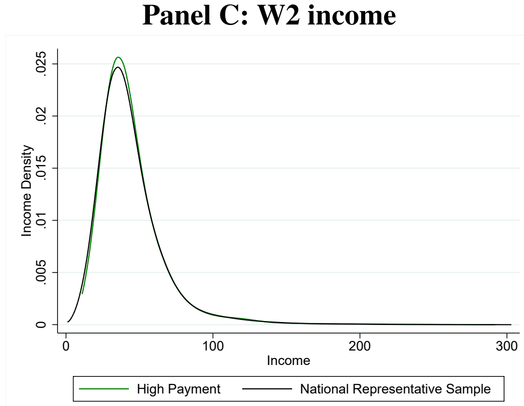


Table A1. Wealth Shocks, Self-Employment, and Education

This table reports coefficient estimates from a cross-sectional regression on all 2015 observations assessing heterogeneity linked to education and the relationship between mineral windfalls and self-employment decisions. The unit of observation is individual i in the year 2015. The dependent variable is an indicator for self-employed (x 100 for percentage interpretation). The estimates are from a linear probability model, estimated using OLS. Acre and income fixed effects are for the quintiles and credit score fixed effects are made at the centile level. Standard errors clustered by three digit zip code are reported in parentheses. The dummy college degree takes the value of 1 if the individual has obtained a college degree, and is 0 otherwise. *, **, and *** indicate statistical significance at the 10%, 5% and 1% level respectively.

	Dependent variable: 100 × Self Employment Dummy					
	Less Than High School	High School	Some College Degree	Bachelor	Graduate	All
	(1)	(2)	(3)	(4)	(5)	(6)
(β_1) High Payment Dummy$_i$	1.658***	0.875**	0.541***	0.872***	1.050***	1.131***
	[0.398]	[0.392]	[0.161]	[0.255]	[0.387]	[0.354]
(β_2) High Payment Dummy$_i$ × College Degree$_i$						-0.365
						[0.362]
(β_3) College Degree$_i$						0.175***
						[0.022]
Age FE	Yes	Yes	Yes	Yes	Yes	Yes
ZIP FE	Yes	Yes	Yes	Yes	Yes	Yes
Income Quintile FE	Yes	Yes	Yes	Yes	Yes	Yes
Acreage Quintile FE	Yes	Yes	Yes	Yes	Yes	Yes
Credit Score Centile FE	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.018	0.010	0.014	0.013	0.023	0.006
N	78,513	192,245	202,264	154,807	76,629	728,121

Table A2. Wealth Shocks, Self-Employment, and Industry

This table reports coefficient estimates from a cross-sectional regression on all 2015 observations assessing heterogeneity linked to industry/job type and the relationship between mineral windfalls and self-employment decisions. The unit of observation is individual i in the year 2015. 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 at the quintile level. Standard errors clustered at the three digit zipcode are reported in parentheses. *, **, and *** indicate statistical significance at the 10%, 5% and 1% level respectively.

	Dependent variable: 100 × Self Employment Dummy			
	Professional/ Technical	Sales/ Service	Blue Collar	Other
	(1)	(2)	(3)	(4)
(β_1) High Payment Dummy_i	0.498***	1.369***	1.299*	1.438**
	[0.128]	[0.358]	[0.681]	[0.566]
Age FE	Yes	Yes	Yes	Yes
ZIP FE	Yes	Yes	Yes	Yes
Income Quintile FE	Yes	Yes	Yes	Yes
Acreage Quintile FE	Yes	Yes	Yes	Yes
Credit Score Centile FE	Yes	Yes	Yes	Yes
R^2	0.012	0.014	0.018	0.043
N	243,865	188,573	103,292	23,044

Table A3. Stability of Coefficient when adding Fixed Effects

This table reports a specification similar to specifcation (5) of Table 3, but adds additional fixed effects one-by-one across specifications to assess coefficient stability

	Dependent variable: 100 × Self Employment Dummy				
	(1)	(2)	(3)	(4)	(5)
(β_1) High Payment Dummy$_i$	1.012***	0.967***	0.918***	0.888***	0.897***
	[0.182]	[0.169]	[0.147]	[0.144]	[0.146]
Age FE		Yes	Yes	Yes	Yes
ZIP FE			Yes	Yes	Yes
Income Quintile FE				Yes	Yes
Acreage Quintile FE	Yes	Yes	Yes	Yes	Yes
Credit Score Centile FE					Yes
R^2	0.001	0.003	0.005	0.005	0.006
N	728,140	728,138	728,121	728,121	728,121

Table A4. Wealth Shocks and Self-Employment - Treated Only

This table reports similar specifications to Table 3, except on a limited sample of mineral owners only. The identification is coming from comparing mineral owners that receive high payments versus small payments.

	Dependent variable: 100 × Self Employment Dummy							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
(β_1) Total Payment$_i$	2.279***	2.203***						
	[0.260]	[0.354]						
(β_2) Log(Total Payment)$_i$			0.174***	0.170***				
			[0.035]	[0.029]				
(β_3) High Payment Dummy$_i$					0.852***	0.860***		
					[0.143]	[0.103]		
(β_4) Payment Dummy $5k-20k$_i$							0.429***	0.395***
							[0.037]	[0.054]
(β_5) Payment Dummy $20k-50k$_i$							0.975***	0.898***
							[0.305]	[0.249]
(β_6) Payment Dummy $50k-100k$_i$							1.141***	1.083***
							[0.328]	[0.269]
(β_7) Payment Dummy $100k-250k$_i$							1.013***	1.089***
							[0.165]	[0.197]
(β_8) Payment Dummy >$250k$_i$							1.803***	1.697***
							[0.304]	[0.379]
Age FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
ZIP FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Income Quintile FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Acreage Quintile FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Credit Score Centile FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls		Yes		Yes		Yes		Yes
R^2	0.008	0.009	0.008	0.009	0.008	0.009	0.008	0.009
N	356,625	300,250	356,625	300,250	356,625	300,250	356,625	300,250

Table A5. Wealth Shocks and Self-Employment - Outside Barnett

This table reports similar specifications to Table 3, except that the sample is limited to mineral owners that reside outside of the Barnett Shale.

	Dependent variable: 100 × Self Employment Dummy							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
(β_1) Total Payment$_i$	1.485*	1.568						
	[0.876]	[0.958]						
(β_2) Log(Total Payment)$_i$			0.064***	0.070***				
			[0.020]	[0.022]				
(β_3) High Payment Dummy$_i$					1.091***	1.143***		
					[0.379]	[0.421]		
(β_4) Payment Dummy \$5k-20k$_i$							0.219	0.102
							[0.216]	[0.224]
(β_5) Payment Dummy \$20k-50k$_i$							0.499	0.576
							[0.378]	[0.430]
(β_6) Payment Dummy \$50k-100k$_i$							1.534***	1.560***
							[0.503]	[0.551]
(β_7) Payment Dummy \$100k-250k$_i$							1.050*	1.119
							[0.628]	[0.697]
(β_8) Payment Dummy >\$250k$_i$							0.814	0.793
							[0.908]	[0.971]
Age FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
ZIP FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Income Quintile FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Acreage Quintile FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Credit Score Centile FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls		Yes		Yes		Yes		Yes
R^2	0.022	0.027	0.022	0.027	0.022	0.027	0.023	0.027
N	87,068	70,745	87,068	70,745	87,068	70,745	87,068	70,745

Table A6. Wealth Shocks and Self-Employment - Payment Type Robustness

This table reports specifications similar to specifications (5) and (6) of Table 3, except payment amounts are derived from different components of the mineral payment received. Specifications (1) and (2) derive the High Payment Dummy from the bonus payment only. Specifications (3) and (4) derive the High Payment Dummy from the total royalty amounts received only. Specifications (5) and (6) derive the High Payment Dummy from both bonus and royalty amounts, but adjusts the royalty to assume everyone in the sample signed a lease with a 20% royalty. Specifications (7) and (8) applies the yearly average bonus per acre and royalty percentage to the year of the first payment received to normalize the terms an individual receives in a given sample year to compute the High Payment Dummy.

	Dependent variable: 100 × Self Employment Dummy							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
(β_1) Bonus Only High Payment Dummy$_i$	0.594***	0.599***						
	[0.180]	[0.165]						
(β_2) Royalty Payment Only High Payment Dummy$_i$			1.076***	1.140***				
			[0.119]	[0.117]				
(β_3) High Payment Dummy 20% Royalty$_i$					1.112***	1.109***		
					[0.197]	[0.167]		
(β_4) High Payment Dummy Yearly Avg Terms$_i$							1.130***	1.163***
							[0.229]	[0.209]
Age FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
ZIP FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Income Quintile FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Acreage Quintile FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Credit Score Centile FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls		Yes		Yes		Yes		Yes
R^2	0.006	0.007	0.006	0.007	0.006	0.007	0.006	0.007
N	728,121	573,357	728,121	573,357	728,121	573,357	728,121	573,357

Table A7. Wealth Shocks and Self-Employment - Credit Inquiry Sample Only
This table reports specifications similar to Table 3, except limits the sample to 2015 observations in which a credit inquiry has occurred.

	Dependent variable: 100 × Self Employment Dummy							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
(β_1) Total Payment$_i$	2.972***	2.733***						
	[0.477]	[0.326]						
(β_2) Log(Total Payment)$_i$			0.056***	0.067***				
			[0.008]	[0.007]				
(β_3) High Payment Dummy$_i$					1.006***	0.975***		
					[0.237]	[0.186]		
(β_4) Payment Dummy \$5k-20k$_i$							0.466***	0.426***
							[0.048]	[0.056]
(β_5) Payment Dummy \$20k-50k$_i$							1.239***	1.213***
							[0.419]	[0.361]
(β_6) Payment Dummy \$50k-100k$_i$							1.254**	1.210**
							[0.536]	[0.491]
(β_7) Payment Dummy \$100k-250k$_i$							1.143***	1.176***
							[0.189]	[0.234]
(β_8) Payment Dummy >\$250k$_i$							2.299***	2.047***
							[0.467]	[0.408]
Age FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
ZIP FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Income Quintile FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Acreage Quintile FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Credit Score Centile FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls		Yes		Yes		Yes		Yes
R^2	0.008	0.010	0.008	0.010	0.008	0.010	0.008	0.010
N	445,035	380,692	445,035	380,692	445,035	380,692	445,035	380,692

Table A8. Wealth Shocks and Self-Employment - Payment Cutoff Robustness
This table reports specifications similar to Table 3 specification (6), and applies different cutoffs to compute the definition of the High Payment Dummy.

	Dependent variable: 100 × Self Employment Dummy						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
(β_1) Payment Dummy >$10k$_i$	0.850***						
	[0.059]						
(β_2) Payment Dummy >$20k$_i$		0.951***					
		[0.136]					
(β_3) Payment Dummy >$30k$_i$			0.874***				
			[0.129]				
(β_4) Payment Dummy >$40k$_i$				1.036***			
				[0.132]			
(β_5) Payment Dummy >$50k$_i$					0.928***		
					[0.106]		
(β_6) Payment Dummy >$100k$_i$						0.946***	
						[0.194]	
(β_7) Payment Dummy >$250k$_i$							1.355***
							[0.379]
Age FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
ZIP FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Income Quintile FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Acreage Quintile FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Credit Score Centile FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.007	0.007	0.007	0.007	0.007	0.007	0.007
N	573,357	573,357	573,357	573,357	573,357	573,357	573,357

Table A9. Wealth Shocks and Self-Employment - Weighted to National Representative Sample
This table reports similar specifications to Table 3, except that it applies weights to the regression specification such that age, income, and credit score are weighted to match a national representative sample of individuals provided by Experian.

	Dependent variable: 100 × Self Employment Dummy							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
(β_1) Total Payment$_i$	1.671***	2.422***						
	[0.342]	[0.529]						
(β_2) Log(Total Payment)$_i$			0.025***	0.060***				
			[0.005]	[0.006]				
(β_3) High Payment Dummy$_i$					0.643***	0.942***		
					[0.213]	[0.106]		
(β_4) Payment Dummy $5k-20k$_i$							0.282***	0.417***
							[0.070]	[0.082]
(β_5) Payment Dummy $20k-50k$_i$							1.097**	0.720***
							[0.499]	[0.104]
(β_6) Payment Dummy $50k-100k$_i$							0.950**	0.946***
							[0.446]	[0.165]
(β_7) Payment Dummy $100k-250k$_i$							0.758***	1.301***
							[0.145]	[0.222]
(β_8) Payment Dummy >$250k$_i$							1.126***	1.588***
							[0.416]	[0.524]
Age FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
ZIP FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Income Quintile FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Acreage Quintile FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Credit Score Centile FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls		Yes		Yes		Yes		Yes
R^2	0.010	0.009	0.010	0.009	0.010	0.009	0.010	0.009
N	Weighted Sample							