Shale Shocked:

The Long Run Effect of Wealth on Household Debt*

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Abstract

We study the long-run effect of unanticipated wealth shocks on the distribution of household debt. Specifically, we focus on how $14.6 Billion in oil and gas shale royalty windfalls over 11 years affect the balance sheets of 404,937 consumers. We find that initially-subprime consumers decrease their credit utilization by paying down preexisting revolving debts, whereas prime consumers increase their revolving, mortgage and auto debts. Overall, household balance sheets become less risky on average (as measured by credit scores). The effects are similar for consumers that reside inside or outside shale areas, which suggests our findings are driven by household decision making, not local economic conditions. Our findings highlight the role of heterogeneity in household balance sheets when considering how positive economic shocks affect future household debt levels.

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1 Introduction

There is longstanding interest in understanding how wealth shocks affect households in the long run. Indeed, higher wealth is robustly correlated with important outcomes, such as health, mortality and human capital accumulation.[1] However, there is less evidence that wealth shocks improve behaviors in domains that are more cognitively challenging, including important household financial decisions.[2] Given such evidence, there is uncertainty about how wealth shocks should affect households' use of debt and under what conditions. Resolving this uncertainty has important implications because the distribution of debt is crucial for a broad set of aggregate outcomes, such as labor supply, consumption, entrepreneurship and household formation (Bernstein (2016); Baker (2018); Schmalz, Sraer, and Thesmar (2017); Goodman, Isen, and Yannelis (2018)). In this paper, we construct a new data set of oil and gas royalty payments to individuals to study how unexpected wealth shocks affect household borrowing choices and the riskiness of household balance sheets in the long run.

Despite much interest, considerable empirical challenges have made it difficult to disentangle the long-run effect of wealth on household debt. In particular, household debt tends to change in response to changes in macroeconomic conditions. For example, the 2000s financial crisis saw increases in household debt that coincided with both relaxed credit constraints (Favara and Imbs (2015)) and the growth of housing market speculation (Chinco and Mayer (2015)). As such, it is essential to identify economic shocks that are not correlated with other macroeconomic events. Furthermore, estimates of economic shocks on household debt usage could depend on the state of household balance sheets. To capture the effects of such heterogeneity we would need to know precisely who experiences such shocks, whereas prior studies tend to rely on aggregate sources of variation (e.g., the ZIP code-level variation in Mian and Sufi (2009)).

[1] For a classic survey on the interrelation between health, wealth and mortality, see Smith (1999). For a recent study on the relation between human capital and economic well-being, see Manuelli and Seshadri (2014).

[2] For example, Briggs et al. (2015) finds that stock market participation increases following lottery winnings in Sweden, but not by as much as predicted by a canonical consumption smoothing model. In a related vein, Hankins, Hoekstra, and Skiba (2011) finds that lottery winnings merely delay the incidence of bankruptcy. More recently, Bernstein, McQuade, and Townsend (2018) find that housing wealth shocks affect households' incentives to innovate.

We focus on an empirical setting that enables significant progress on these empirical challenges. Specifically, we study how the debt usage and financial risk of 404,937 individuals changes in response to $14.6 Billion in oil and gas royalty windfalls over 11 years for Barnett Shale mineral owners. The windfalls in our sample range from less than $1 dollar per month at the lowest to $703,030.30 dollars per month. Households that receive these shocks live in every U.S. state. With 45,242 mineral owners in our data residing outside of the Barnett Shale area, we can attribute changes in household debt behavior to the individual wealth shock, distinct from changes to local conditions after the discovery of shale. Further, individuals in our sample span every credit score category, including 84,073 subprime individuals. Our sample has significant overlap with demographics across the entire U.S. population. Moreover, these payments were unexpected at the beginning of our sample and are largely driven by factors external to the individuals we study (i.e., the price of natural gas and the number of wells drilled). In this way, our setting enables us to address the most salient endogeneity issues in estimating the effect of wealth on household debt, as well as understanding sources of heterogeneity in how households respond to these wealth shocks.

Our first set of empirical tests highlights how heterogeneity with respect to initial credit ratings (e.g., subprime versus prime) affects household debt responses to wealth shocks. Individuals who had a subprime credit rating in 2005 (credit score $\leq$ 620) begin our sample with average revolving balances of $11,173, which represents nearly 50 percent utilization of their revolving credit limits. By 2015, we estimate that initially-subprime individuals who received mineral payments reduce their revolving credit utilization by 6.9 percentage points (approximately 20 percent), relative to initially-subprime individuals who received no wealth shock. Initially-subprime individuals also reduce their mortgage balances slightly, and exhibit a reduction of 3.35 percentage points in their debt-to-income ratio.

By contrast, individuals who had a prime credit rating in 2005 (credit score $\geq$ 720) begin our sample with average revolving balances of $6,317, which represents roughly 15 percent utilization of their revolving credit limits. By 2015, we estimate that initially-prime consumers *increase* their revolving credit utilization by 2.6 percentage points (approximately 25 percent), relative to initially-prime consumers who receive no wealth shock. Prime consumers also increase their use of installment credit. Their mortgage balances increase by 6.5 percent and their automobile loans are 2 percent larger. In contrast to subprime consumers who use wealth shocks to pay down debt, these findings suggest prime consumers use debt markets as a complement to consumption. These differential credit behaviors have important consequences. By 2015, receiving mineral rights payments more than halves the likelihood that a subprime individual would fail to qualify for a mortgage based on debt-to-income (Qualifying Mortgage threshold (DTI>43%)), but it increases the likelihood that an initially prime credit individual would fail to qualify for a mortgage on the basis of debt-to-income. Furthermore, all of these findings are similar for individuals who live outside of the Barnett shale. Therefore, we can attribute our findings to individualistic debt decision-making, rather than decision-making spurred by changes to the local economy.

Next, we examine how these changes in debt usage affect the riskiness of consumer balance sheets. Studying the riskiness of household balance sheets is important because it can affect the willingness of lenders to extend credit to households, which can itself slow consumption and investment. For example, lenders would be more reluctant to provide mortgage loans to consumers who are at greater risk of default. We study consumer's financial risk in two ways. First, we test for the incidence of payment delinquencies and derogatory accounts. We find that subprime consumers who receive wealth shocks are 44 percent less likely to have credit accounts that are at least 90 days past due than control individuals who receive no wealth shock. Similarly, the fraction of severe derogatory accounts falls by 13 percent for subprime consumers who receive mineral payments. On the other hand, consumers who initially have near-prime creditworthiness – those with credit scores between 620 and 720 – are more likely to have delinquent and derogatory accounts if they received a wealth shock. The fraction of accounts at least 90 days past due increases by 63 percent and the fraction of severe derogatory accounts increases by 16 percent relative to control individuals. Prime consumers also increase their incidence of delinquent and derogatory accounts, but the effects are not as large and in some cases statistically insignificant.

Second, we estimate the effect of royalty payments on consumer credit scores, which the credit bureaus use to measure consumers' implied probability of default. We find that royalty payments translate into significant improvements to credit scores, regardless of initial credit rating. On average, credit scores of individuals who receive royalty wealth shocks increase by 10 points more than individuals who received no wealth shock. This increase is economically meaningful, and is similar to effects identified by Dobbie, Goldsmith-Pinkham, and Yang (2017) and Brown, Cookson, and Heimer (2018).[3] Turning to heterogeneity, the credit score effect is greater for initially subprime individuals than for prime or near-prime individuals. For initially-subprime individuals, we estimate a 15–18 point increase in credit scores, whereas the effect is between 10–15 points for near-prime consumers and 7–8 points for prime consumers. Contrasting with the results on delinquencies, the financial health benefits to near-prime credit consumers offset the negative effects of the increase in delinquent and derogatory accounts. This evidence suggests that, despite the financial mistakes and consumption effects noted in the literature, wealth shocks result in economically large reductions in the financial risk of households, particularly for subprime individuals.

Looking beyond these average effects, we exploit the wide variation in payments to examine the size of the wealth shocks that matter for household financial risk. We find that even relatively small payment sizes have large effects on consumer default risk, and that the effect of payments levels out in the $20,000 to $50,000 aggregate payment range. Specifically, we find that payments of over $1 million do not result in any increase in credit scores, relative to payments in the $20,000 range. We obtain similar results looking at both Barnett shale residents and non-Barnett residents. The implication of these findings is that individuals benefit from significant improvement even from relatively small increases in cash transfers of $20,000 over 11 years or $151/month on average.

Our primary contribution is to provide novel evidence on how wealth shocks affect the level of household debt over the long-run. Prior work has shown that high levels of household debt place important constraints on real outcomes and the macroeconomy (Mian, Rao, and Sufi (2013); Favara and Imbs (2015); Bernstein (2016); Melzer (2017)). At the same time, a complete understanding of what determines household debt levels has remained elusive. Existing work has identified the role of increased credit supply (Mian and Sufi (2011); Di Maggio and Kermani (2017)) or, on the demand-side, beliefs about future prices (Bailey et al. (2018)) in driving household debt levels. Relative to this literature, we provide a systematic investigation of how personal wealth affects household borrowing choices in the long-run and how these borrowing decisions affect the riskiness of household balance sheets.

We also furnish new evidence on the long-run effects of non-labor wealth shocks on household outcomes. For the most part, this literature draws on evidence from two settings: lottery

[3] In the context of Native American reservations Brown, Cookson, and Heimer (2018) find that an increase of this magnitude results in a decrease in the cost of mortgage financing of 5.1%.

winners in Sweden and recipients of payments from the Alaska Permanent Fund. Lottery winners in Sweden report higher subjective well-being decades after winning, have reduced labor earnings, and increase their stock market participation rates (Briggs et al. (2015); Cesarini et al. (2017); Lindqvist, Östling, and Cesarini (2018)). However, Swedish lottery winners do not experience improved health and their children do not have improved developmental outcomes (Cesarini et al. (2016)). Anticipated cash transfers from the Alaska Permanent Fund lead to increased consumption by high-income households, but do not significantly decrease aggregate employment (Kueng (Forthcoming); Jones and Marinescu (2018)).[4] We differ from these studies in the following ways. The royalty payments in our sample are frequent, long-lasting, and large for a significant number of individuals in our sample, whereas the Swedish lottery winners receive a one-time wealth shock and the payments distributed by the Alaska Permanent Fund average less than $2,000 per person per year. Furthermore, we exploit the substantial heterogeneity in financial conditions of households at the time that they begin to receive royalty payments. To our knowledge, we are also the first in this literature to study the long-run effects of non-labor income on credit market outcomes.

A much lengthier literature studies individuals' near-term response to expected and unexpected income shocks. Many papers in this area attempt to estimate a marginal propensity to consume out of income. These studies rely on a variety of natural experiments including government shutdowns (Baker and Yannelis (2017)), mortgage payment resets (Di Maggio et al. (2017); Jørring (2017)), tax refunds and stimulus checks (Johnson, Parker, and Souleles (2006); Baugh et al. (2014)), and lottery windfalls (Kuhn et al. (2011)).[5] Other papers specifically study how unanticipated income shocks affect household borrowing (Agarwal, Liu, and Souleles (2007); Agarwal and Qian (2014)). In contrast to our work, these other papers study the short-term rather than long-run effects on debt usage, they study income shocks that are one-time payments rather than recurring, and the payments they study are much smaller, and have less variation, than our royalty payments. Additional papers study the joint effect of income shocks on consumption and debt (e.g., Aaronson, Agarwal, and French (2012); Baker (2018)), while related research extends these settings to consider the effect on neighboring households that do not receive income shocks (Agarwal, Mikhed,

[4] Early research finds no effect of the Alaska Permanent Fund on consumption (Hsieh (2003)). However, Kueng (Forthcoming) uses new transaction-level data from a personal finance website to update this finding.

[5] Some papers study the effect of cash transfers to households in developing countries. For example, Haushofer and Shapiro (2016) find that unconditional cash transfers to poor households in rural Kenya lead to increased consumption and higher subjective well-being.

and Scholnick (2018)). We differ from these near-term studies in that we focus on long run consequences. In this way, our analysis extends the current literature by showing the degree to which the benefits of wealth shocks persist beyond shorter horizons.

Our paper also contributes to an emerging literature on the determinants of household's financial risk. This literature contains mixed evidence on the effectiveness of various policies on the financial health of households. For example, exposure to financial institutions at a young age leads to long-lasting improvements in debt management and financial health (Brown, Cookson, and Heimer (2018)). Credit counseling, credit monitoring, and restrictions on payday lending can all improve credit outcomes (e.g., Roll and Moulton (2016); Blascak et al. (2016); Baugh (2017)), as can repayment reminders targeted to borrowers (Bracha and Meier (2014); Bursztyn et al. (forthcoming)). On the other hand, financial literacy programs are found to have had only modest and short-lived effects on financial well-being (e.g., Brown et al. (2016); Fernandes, Lynch, and Netemeyer (2014)). Related to this literature, some papers study the determinants of consumer credit access and the subsequent effects on financial well-being. Important determinants of credit access include the political economy (Akey, Heimer, and Lewellen (2017); Akey et al. (2018)), as well as debt collection and bankruptcy protection laws (Fedaseyeu (2013); Severino and Brown (2017)). More closely related to our study, Brown (2018) and Haughwout et al. (2016) examine the local effects of the oil and gas boom in the regional U.S. on consumer debt accumulation and financial distress, respectively. These studies use the FRBNY - CCP/Equifax panel data set to measure credit outcomes, but without an individual-level match to mineral payments, these studies must rely on local aggregates. Because we link royalty payments to individual credit bureau information, our findings are distinct in highlighting the role of ex-ante heterogeneity in household balance sheets. Further, our work effectively disentangles the individual responses to wealth shocks from changes to the local economy by studying the household debt changes of non-local beneficiaries of the Shale boom.

Our paper also relates to a growing literature on the economic effects of shale development. Existing literature has documented that natural gas shale development has led to job growth (Feyrer, Mansur, and Sacerdote (2017)), lending (Gilje (Forthcoming) and Gilje, Loutskina, and Strahan (2016)), and changes in house prices (Muehlenbachs, Spiller, and Timmins (2017)). Our paper is the first paper to use individual level monthly oil and gas royalty payments to trace out the effects of shale development on household outcomes.

2 Data and Institutional Setting

The analysis uses several data sets that are novel to the literature. Below we outline the data and its construction.

2.1 Oil and Gas Lease and Royalty Data

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

Accurate data on payments that individuals receive is exceedingly difficult to obtain and compute. In all states except Texas, royalty ownership interests in wells are held by private companies and not released to the public. Public county court records can be used to compute ownership percentages, but this often requires manually searching county indices and filings, and oil and gas firms typically pay an average of $50,000 per well to compile accurate royalty owner information from these public records. To put this in perspective, the number of wells in our sample is 7,041. Fortunately, in the state of Texas, producing royalty interests are required to pay property tax, unlike other states. Texas requires all oil and gas firms to turn over their so-called "pay decks" with detailed well-by-well ownership interest information to the state. This royalty interest information is then used to compute an ownership value based on the production profile of each well. Because property tax information is public information in the state of Texas, one can conduct open record requests to obtain the detailed title and ownership information that private firms paid millions of dollars to construct. Appendix Figure A.1 provides an example of the raw mineral appraisal rolls which are used in this study. The data is often provided in PDF format, and requires substantial data manipulation to translate the data into a format conducive to analysis. In our study we focused on compiling mineral appraisal roll data for the four main producing counties in the Barnett Shale going back to the year 2000.

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

In our sample, royalty payments from production account for 60% of total payments. The remaining payments are the bonus payments that mineral owners received at the time a lease was signed. To compute this, 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, some leases do have this information, as well as net acreage amounts. Based on the leases that do have lease bonus information we estimate a regression which attempts to predict the dollar per acre amount a lease bonus is based on time fixed effects, county fixed effects, and operator fixed effects. The R-squared we obtain from the regression is 0.82. We then use this predicted amount to estimate the lease bonus amounts for the rest of our sample for which we do not have this information. An example of a lease in our sample is provided in Appendix Exhibit A.2.

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. The first panel in Table 1 provides and overview of the distribution of payments. 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. The high correlation between monthly payments and natural gas prices can be seen in Figure 1, which plots the aggregate monthly payments in our sample versus the prices of natural gas. For our sample we compute the monthly correlation of payments and natural gas prices to be 0.61.

2.2 Barnett Shale Overview

The focus of our study is the sample of oil and gas mineral owners who own minerals in the Barnett Shale from 2005 through 2015. The Barnett Shale was the first shale gas development in the United States. Shale gas had historically been uneconomic to drill and develop. However, the combination of horizontal drilling with hydraulic fracturing ("fracking"), by Devon Energy and George Mitchell, led to a technological breakthrough which allowed vast new quantities of natural gas to be developed. According the U.S. Energy Information administration shale gas production was less than 1% of total U.S. natural gas production in the year 2000, but by 2015 accounted for 46.2% of total U.S. gas production. Moreover, the Barnett shale was the first, and among the most prolific shale development in the United States, and the four Barnett Shale counties we focus on in our study accounted for 17.3% of total U.S. shale gas production when production from the shale field peaked in 2012. Figure 2 plots the number of Barnett Shale wells over time. There is a 14-fold increase in shale wells during the time period of our study. We start in 2005 largely because that is towards the beginning of the shale discovery (only 6.7% of our mineral owners were getting any payments at that time) and it is the first time period which high quality credit bureau data was available to us. To provide a spatial perspective of the development over the Barnett over time, we plot shale well development over different years on a map in Figure 3. As can be seen, there is a high degree of spatial heterogeneity that existed over time, as development ramped up.

The development of the Barnett Shale offers several attractive features. First, because shale development was unexpected by the industry (Acknowledging the unexpected nature of shale gas development John Watson, CEO of Chevron, stated in a Wall Street Journal (2011) interview, that the technological advances associated with fracking took the industry by surprise), mineral ownership in the Barnett Shale represented a deep out of the money option, which had minimal value until there was a technological breakthrough. For those fortunate to own minerals, which typically occurred through family ancestry, the shale breakthrough led to the deep out of the money option becoming a very valuable cash flow stream when natural gas was drilled. Therefore, while people that own minerals are certainly different than the average credit profile in the U.S., the shock they experience "within" person was due to an exogenous technological breakthrough over which they had not control.

2.3 Royalty Owners vs. Average Household Nationally

A question central to the identification we use in our study is why some people own mineral royalties while others do not. The National Association of Royalty Owners estimates that 12 million people in the United States own oil and gas minerals. Mineral interests can be associated with real estate ownership, but often is not. In many instances mineral interests are severed from surface rights, and retained by the initial family ancestry that settled an area. Because undeveloped minerals represent a deep out of the money option, little value is ascribed to minerals until there is drilling activity. Therefore it is common in surface real estate transactions for minerals to be severed as buyers, especially in areas with shale, as no development was expected, would pay little extra to own the minerals and surface.

Figure 4 plots the locations of mineral owners in our sample. These individuals live in all 50 states, the District of Columbia, and three U.S. territories. In total, 16.6% of the royalty payments in our sample are received by people that do not live in the four Barnett Shale counties of our study. The state in our sample with the second highest gross mineral payments is California, consistent with the mass migration patterns of Texans during the Dust Bowl. In most instances, mineral interests can be traced back generations, as families pass down deep out of the money options. Later in our study, we undertake a series of tests to document that our main results are robust to focusing only on treated individuals (individuals with same net mineral acreage plots, but some that receive large payments due to more prolific wells and some who receive small payments due to less prolific wells). The purpose of this section is to document, that while mineral owners may be different than the U.S. population on average, there is significant overlap of our sample with key parts of the overall U.S.

distribution of borrowers. For this, we will compare our sample of oil and gas royalty owners to a random sample of U.S. borrowers provided by Experian.

As Figure 5 shows, our sample has 84,073 people that are subprime. Our sample has a significant number of observations in each credit category. Table 1 reports detailed summary statistics on the distribution of payments in the sample and credit characteristics of the individuals in our sample.

2.4 Experian Data Overview

From the raw data we compiled, we identified approximately 500,000 mineral rights owners, and computed a monthly panel data set of the payments received by rights owners from 2000 onward. We contracted with Experian to merge the mineral rights data with individual-level credit bureau data.[6] We provided information on payments, names and addresses, and Experian conducted the merge on name and address. In addition, Experian provided us with two control samples, (i) a sample matched on the geography and age distribution of our Experian records, and (ii) a nationally representative sample. The merge with credit bureau data returned an 80 percent hit rate, leaving us with approximately 404,000 consumers who received mineral rights payments. Each of our control samples has approximately 300,000 individuals, leaving us with approximately one million credit histories.

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). Our primary outcome variables are the total amounts of debt usage across different product categories (e.g., unsecured credit lines, auto loans, and mortgage debt), as well as measures of the financial risk on consumer balance sheets (e.g., delinquencies and credit scores). Because our focus is on long run outcomes in this study, we restrict attention to two snapshots of the data – year 2005 and year 2015.

For our main tests, it will be useful to contrast individuals who receive mineral rights payments to those in our control samples. To ensure covariate balance, we refine the geography-age matching provided by Experian with a propensity score matching procedure in which we match on initial credit score in 2005 and length of credit history. We select controls with replacement and

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we restrict matched controls to be individuals who live in the same three digit zip as the mineral owner. Table 2 reports how the treatment (mineral owner) sample compares to the matched (control) sample. Across a wide range of 2005 characteristics, the propensity score matching procedure does well, even for credit characteristics that were not targeted in the matching procedure (e.g., mortgage and credit card balances).

3 Hypothesis Development

Theories of household borrowing behavior have divergent predictions regarding how unanticipated wealth shocks should affect household credit usage. On one hand, households might increase their borrowing in response to receiving mineral rights payments. According to the standard consumption smoothing rationale, agents borrow against future income to consume today, thereby increasing their current level of debt. This tendency to increase debt is amplified in models that consider behavioral agents in an extrapolative expectations framework. In these theories, a precipitating event leads some economic agents to become optimistic about future cash flows (e.g., Minsky (1977)). Such optimism leads to greater household debt because some agents over-extrapolate the value or duration of the stream of payments (e.g., Fuster, Laibson, and Mendel (2010); Bordalo, Gennaioli, and Shleifer (2018)).

On the other hand, there is equally sound theoretical basis that predicts unanticipated wealth shocks will decrease household debt levels. Some households may be burdened by their current debt levels, such that it would be optimal to put an unexpected windfall toward debt payments. For example, consider a consumer who carries a large month-to-month balance on a high-interest credit card. Given this initial state, it is a reasonable decision to repay these high-interest debts upon receiving a wealth shock. Related to this point, Gross and Souleles (2002) show that many households have high revolving debt balances, even households who have significant liquid savings in low-yield accounts that could be used to pay down debt. Thus, although debt pay down is a reasonable prediction for consumers with high revolving balances, this revolving balances puzzle casts skepticism that high-debt consumers would indeed pay down debts upon receiving a wealth shock. Lastly, it is plausible that wealth shocks do not affect household debt levels at all, as impatient households consume the unanticipated windfall, but do not adjust debt levels.

Ultimately, it is an empirical question how unanticipated wealth shocks affect households' debt usage. Our empirical tests examine how wealth shocks affect both the level of household debt (revolving balances, mortgage, auto and overall debt-to-income), and the riskiness of household balance sheets (credit scores and delinquencies). Jointly, these tests are informative of underlying mechanisms. If households rationally use debt markets to smooth consumption, wealth shocks should lead to greater debt and improvements to credit scores and delinquencies in the long run. If households over-extrapolate past shocks, wealth shocks should lead to more debt and a decline in credit quality in the long run. If households experience burdensome debts that wealth can alleviate, wealth shocks should reduce debts while improving overall credit quality in the long run.

Moreover, different household types (e.g., subprime consumers versus prime consumers) may follow different models of behavior, such that the average relation between wealth shocks and household debt decisions represents a mix of these potential channels. To distinguish the mixing of different debt behavior types from the possible null prediction (i.e., households just consume the windfall with no implication for debt behavior), we consider heterogeneity in initial credit rating – subprime, near-prime and prime consumers. These subsample splits are informative of mechanisms in their own right. If subprime credit ratings tend to reflect greater initial debt burden, wealth shocks should lead to more debt pay down and improvements to financial risk for subprime relative to prime. However, initial subprime credit may reflect greater impatience or propensity to extrapolate past shocks. In this case, wealth shocks would lead to less debt pay down (greater consumption) and greater financial risk.

In the following section, we systematically analyze the impact of wealth shocks from mineral payments on the distribution of debt and household financial risk. This is an important empirical exercise because understanding the link between wealth shocks and household debt is central to understanding the potential effects of policies that affect the income profiles of households (e.g., fiscal stimulus, tax cuts, universal basic income). Further, our analysis of the heterogeneity of these responses is helpful to understand the distributional consequences of these wealth transfers.

4 Results on Household Debt Usage

In this section, we provide evidence on how consumers affected by the mineral payments shock change their debt usage behavior. Specifically, we examine how consumers' use of revolving credit, mortgage instruments, and total debt-to-income change after receiving mineral rights payments.

4.1 Revolving Credit

We start by examining the effects of wealth on revolving credit balances, because many consumers use revolving credit card accounts to pay monthly expenses. At the same time, revolving credit with high interest rates presents a significant challenge for constrained borrowers who carry a credit card balance from month to month.

We estimate the effect of mineral payments on revolving credit choices by estimating the following specification:

$$Rev\ Credit_{it} = \gamma + \gamma_{zt} + \beta_1 treatment_i \times post_t + \epsilon_{it}, \tag{1}$$

where $Rev\ Credit_{it}$ is either the utilization of revolving credit (balance/limit) or the logged revolving credit balance for individual i in year t. The independent variable $treatment_i$ is an indicator for whether individual i is an individual who received a bonus payment (=1) versus an individual in the propensity-matched control sample (=0). The $post$ variable is an indicator for the post period (=1 for year 2015, =0 for year 2005), γ_i are individual fixed effects, and γ_{zt} are ZIP3-year fixed effects. To allow for local correlation in errors, we cluster standard errors by ZIP3.

The specification includes main effects for $post_i$ and $treatment_i$ when these effects are identified separately from fixed effects. The coefficient of interest is β_1, which captures how the credit balances of treated individuals changes in the long term relative to control individuals.

Panel A of Table 3 presents the results on revolving credit utilization. In the full sample, there is no significant effect of mineral payments on utilization, but there is a striking dimension of heterogeneity across initial creditworthiness. Subprime and near prime credit individuals dramatically reduce revolving credit utilization by 6.9 and 3.7 percentage points, respectively, whereas consumers with initially prime credit records increase their utilization by 2.6 percentage points. These changes to revolving credit utilization are statistically significant at the 1 percent level. As the triple interaction in columns (5) and (6) indicates, the heterogeneity of the effect of mineral payments by initial creditworthiness is statistically significant on the full sample, as well as the subsample of residents who live outside of the Barnett shale area.

Turning to magnitudes, the 6.9 percentage point effect for subprime individuals corresponds to reducing revolving utilization by greater than one-sixth of the initial revolving utilization of 48.4%. Similarly, the 2.6 percentage point increase in utilization by prime credit individuals corresponds to an 18 percent increase over the baseline utilization of 14.6%. These effects are economically meaningful and point to a sensible form of heterogeneity in the ways in which the mineral payments are used by consumers of differing creditworthiness. Subprime consumers – who have greater benefit to paying down revolving debts – do indeed pay down debts, whereas prime credit consumers who more likely pay off revolving balances monthly do not. Indeed, the month-to-month variation among these individuals seems to represent a natural increase in consumption by these consumers.

Panel B of Table 3 presents the results of an analogous specification, which uses the logged revolving balances as the dependent variable instead. These specifications portray a similar qualitative picture to the utilization results. Mineral payments translate to significant reductions to the balances of subprime and near prime credit consumers (21.7% and 29.5%, respectively), but a significant increase for prime credit consumers. Moreover, the magnitudes of the estimates on balances are similar to those magnitudes computed from utilization, which also accounts for credit limits. The consistency of the findings across these specifications suggests that the findings are not due to changes in credit limits, but actual debt paydown and increasing consumption behavior.[7]

[7] In the appendix, we perform several important robustness checks on these main results. First, we show that the effect of payments on revolving credit and credit scores is similar for individuals who receive only a one-time bonus payment (see Tables A.1 and A.2). Thus, our effects are less likely to be driven by expectations that the stream of payments will continue into the future. Second, we show that the effect is similar to our main specifications when restricting the sample to individuals who begin receiving payments before 2009 (see Tables A.5 and A.6). These individuals are less likely to be influenced by news of the shale boom prior to receiving their first payments. Finally, we also verify that the effect is similar for consumers who own less than 5 acres (see Tables A.3 and A.4). These findings indicate that the results are not being driven by selection into the sample – i.e., individuals with less than 5 acres have no market power to allow or block the decision of an extraction company to drill.

4.1.1 Heterogeneity in Payments

One of the attractive features of our setting is the wide variation in payments we observe. The distribution of mineral payments ranges from small payments (less than \$5,000) to very large payments (totaling more than \$1 million over the decade). Estimating how different payment amount translate into consumer debt usage is important for understanding the degree to which we can translate the lessons from experiments of a smaller magnitude (i.e., shocks on the order of \$1,000s) to much larger scale policies.

To quantify the impact of heterogeneity in payments, we estimate a specification for credit score using the following specification.

$$Rev\ Credit_{it} = \gamma_i + \gamma_{zt} + \gamma_{at} + \sum_{b=1}^{B} \beta_1^b pmt\_bin_i^b \times post_t + \beta_2 InitScore_i \times post + \epsilon_{it}, \quad (2)$$

which has two notable departures from the initial difference-in-difference design: (1) we replace the treatment dummy with payment bin dummies $pmt\_bin_i^b$ to flexibly estimate the effect of payments for different payment amounts, and (2) we estimate these specifications for the treated sample only, using individuals who received very small payments (<\$5000) as controls within the treated sample. This specification choice holds constant unobservable determinants of signing a mineral lease, while benchmarking the effects against individuals who receive small payments (due to drilling decisions and natural gasprice fluctuations beyond the consumer's control). In this specification, the coefficients of interest are β_1^b, which capture the long-term effect on revolving credit usage for the payment bin b relative to individuals who receive very small payments.

In these within-treated specifications, we enrich the specification to account for important potential confounding differences that could lead to larger payments. For example, better credit quality individuals may own greater acreage and hence receive larger payments. We account directly for this potential mechanism by including *acreage quintile x year* fixed effects in some specifications. Beyond this direct control, we also account for initial credit quintiles interacted with $post$, as well as *income* and *age quintile x year* fixed effects.

Table 4 presents the estimation results on heterogeneity in payments, separately by initially subprime, near prime and prime credit consumers. Consistent with the broad evidence, the effects on revolving balance tend to be negative among initially subprime consumers and near prime consumers, and the effects tend to be positive for initially prime consumers. We also find that the effects are most pronounced among moderate payments and are muted for larger payments. Figure 6 presents graphs of the estimates with 90% confidence bands, which more clearly conveys the muted effect among large payments.

4.2 Mortgage Credit

We now turn to understanding the effects of mineral payments on mortgage balances and utilization of mortgage financing. Specifically, we estimate the main specification (3), but with mortgage measures as the dependent variable. Our specifications consider two complementary outcome variables – mortgage utilization (balance/limit) and the log of (one plus) mortgage balances.

Panel A of Table 5 presents the estimates for mortgage utilization. Consumers who receive mineral payments have nearly one percentage point greater utilization of mortgage credit, on average, than matched control consumers. This effect is statistically significant at the 1 percent level, and because of the size and importance of mortgage debt, one percentage point is an economically significant increase. The increase in mortgage utilization is strongest among the initially prime creditworthiness consumers, and is absent among the subsample of initially subprime consumers. The estimate for subprime consumers is a small negative estimate that is not statistically different from zero.

Panel B of Table 5 presents the estimates for mortgage balances. Overall, consumers who receive mineral payments increase their mortgage balance by 5.9%, an effect that is statistically significant at the one percent level. Using the in-sample average mortgage balance of $134,970, the increase in mortgage balance is equivalent to an average increase of $7,963. Turning to heterogeneity, subprime consumers reduce their balances slightly (-4.25%, p-value of 0.108), whereas near prime consumers and prime consumers increase their mortgage balance in a statistically significant manner by greater than 5 percent each.

4.3 Automobile Loans

Next, we examine how mineral payments affect automobile loans. Specifically, we estimate the main specification (3), but with the log of (one plus) the consumer's auto loan limit as the dependent variable. We employ the auto loan limit as the dependent variable to more accurately measure the full size of the auto loan, which is closely related to durable goods consumption.

Table 6 presents the results from estimating the effect of mineral rights payments on auto loans. Overall, consumers who receive mineral rights payments increase their auto loan balance by approximately 1.7% relative to matched control consumers. This effect is statistically significant at the 1 percent level. Like our finding on mortgages, this effect on automobile lending is strongest among the subsample of prime creditworthiness consumers, and it is absent among subprime consumers. This finding is consistent with broad interpretation that prime consumers expand their use of debt markets to complement their consumption, whereas subprime consumers primarily pay down debts with the influx of wealth.

4.4 Debt-to-Income

We conclude the discussion of credit usage by analyzing how total debt-to-income responds to mineral payments. As in the mortgage credit specifications, we estimate the main specification (3), but using Experian's measure of total debt to W2 income as the outcome variable.

Panel A of Table 7 presents our estimation results on debt-to-income overall (column 1), and split by different initial credit score bins (columns 2 through 4). Overall, individuals who receive mineral payments reduce their debt-to-income ratio by 1.25 percentage points, which is a sizable effect, relative to the average debt-to-income in 2005 of 15.1%. This estimate is statistically significant at the 1 percent level, clustering standard errors at the ZIP3 level. Turning to heterogeneity, subprime consumers reduce their debt-to-income by the most – an effect of 3.4 percentage points on a base of 20.3%. Notably, both subprime and near prime credit consumers reduce debt-to-income significantly, but initially prime credit consumers do not.

To address the concern that these shifts in debt-to-income are not economically important, Panel B presents a series of specifications in which the dependent variable is an indicator for whether the debt-to-income ratio exceeds the Qualified Mortgage threshold of 43 percent. In 2005, 10.66%

of subprime consumers, 6.37% of near prime credit consumers, and 1.39% of prime credit consumers had a debt-to-income ratio exceeding this threshold.

Consistent with the broad reductions in debt-to-income, mineral rights payments significantly reduce the percentage of consumers with debt-to-income that would disqualify them from receiving a mortgage. The average effect is a reduction of 1.6 percentage points in the likelihood of not qualifying for a mortgage. For subprime consumers, the effect is more than triple the average effect at 5.9 percentage points, and this reduction amounts to more than half of the subprime consumers who did not qualify for a mortgage in 2005. By contrast, receiving mineral rights payments increases the likelihood that an initially prime credit consumer exceeds the Qualified Mortgage threshold by 0.33 percentage points, which is significant compared to the baseline rate of 1.39% for prime creditworthiness consumers.

The results on the Qualified Mortgage threshold provides tangible evidence on one benefit of reducing the debt-to-income ratio, but access to mortgage financing is but one benefit from having a low debt-to-income ratio. The effects we observe for mortgages likely translate into the access to and cost of other products as well.

5 Results on Household Financial Risk

This section presents the main results on the consequences of mineral rights payments for financial risk, which we measure using observed delinquencies, as well as Experian credit score. Beyond providing evidence on the treatment effect of mineral rights payments, we investigate natural sources of heterogeneity in initial creditworthiness, initial income, and heterogeneity in the payment amount.

5.1 Delinquencies and Derogatory Accounts

We begin our analysis of the consequences of mineral rights payments for consumer financial risk by examining the long-run impact on observed delinquencies and severe derogatory accounts. We follow the same treatment-control strategy as in our analysis of debt usage, and estimating equation 1 using the percent of accounts delinquent as the dependent variable.

Panel A of Table 8 presents the results on delinquencies in which the payment is 90 days past due. Overall, the delinquency result suggests that the mineral rights payments lead to a slight decline, on average, in the likelihood of a consumer going delinquent on an account, relative to the matched control sample. Turning to heterogeneity, the decline in the likelihood of going delinquent is concentrated among initially-subprime borrowers who nearly halve their likelihood of going delinquent. On the other hand, near prime households have a substantially *higher* propensity to have delinquent accounts if they received mineral rights payments versus matched control.

As a complement to these results on delinquencies, Panel B uses a more severe form of delinquency, an indicator for whether the consumer has at least one account that is in severe derogatory status. By this measure of household financial risk, the average effect of mineral rights payments is to increase slightly the propensity to have a severe derogatory account (by 0.4 percentage points, statistically significant at the 10 percent level). Similar to the findings on delinquencies, subprime consumers who receive mineral rights payments exhibit a strong reduction in their propensity to have a severe derogatory account relative to matched controls. Consistent with some risk of their greater consumption, near prime and prime consumers exhibit a higher likelihood of having an account in severe derogatory status.

5.2 Credit Scores

The empirical cumulative distribution functions (CDFs) in Figure 7 provide a visual depiction of the effect of mineral rights shocks on the distribution of credit scores. The figure presents the distribution function of credit scores, separately for before (year 2005) and after (year 2015) the shale payments. Consistent with the mineral rights payments translating into better financial well being throughout the distribution of credit scores, the distribution of credit scores shifts markedly to the right. In contrast, the distribution of credit scores for matched controls does not show a similar shift.In Table 9, we subject this relation between mineral payments and financial well-being to more stringent specifications that account for individual and ZIP3-year fixed effects. Specifically, the main specification for credit score, $Score_{it}$ is:

$$Score_{it} = \gamma_i + \gamma_{zt} + \beta_1 treatment_i \times post_t + \epsilon_{it}. \quad (3)$$

This specification is analogous to equation (1), but replaces the dependent variable with $Score_{it}$, which is the Experian Vantage score for individual i in year t. Mirroring our tests of debt usage, we employ individual and ZIP3-year fixed effects and cluster the standard errors by ZIP3. To consider overall and heterogeneous effects, we estimate the specification on the full sample and on sub-samples split by initial credit score (subprime, near prime and prime).

Columns (1) through (4) in Table 9 present regression estimates for the full sample, with fixed effects of increasing granularity. The coefficient estimate ranges between 8.86 and 10.03 credit score points, and is statistically significant at the 1 percent level across all specifications. These estimates represent an important average long-term improvement of credit profiles by individuals who receive payments. Brown, Cookson, and Heimer (2018) quantify the impact of an increase in the credit score of 10 points to reduce the cost of mortgage financing by approximately 5.1 percent.

Columns (4) through (8) in Table 9 present the estimates on the sub-sample of individuals who reside outside of the Barnett Shale. The estimated magnitudes are similar on this subsample (ranging from 8.9 to 12.0 credit score points), and are statistically significant at the 1 percent level across specifications. The similarity of the estimates inside and outside of the Barnett shale suggest the effects we observe are not due to shocks that hit the Barnett shale, more generally.

5.3 Heterogeneity by Initial Credit Score and Income

Next, we examine heterogeneity in the types of consumers who most benefit from the mineral rights shock. Ex ante, it is unclear whether low credit quality consumers or high credit quality consumers would benefit more from a significant wealth shock. From the standpoint of credit score as an indicator of quality, one might expect a smaller effect on low credit score individuals. On the other hand, to the extent that poor credit leads to a debt trap, individuals with low credit scores may gain more than high creditworthiness consumers because the mineral payments alleviate a constraint.

We examine heterogeneity by the initial credit score by estimating equation (3) separately for subprime credit (initial score < 620), near prime credit (initial score between 620 and 720), and prime credit (initial score > 720). We conduct the same exercise using data on income provided by Experian, splitting the sample into five income quintiles. Across all specifications, we include individual and ZIP3-year fixed effects, and cluster standard errors by ZIP3.

Panel A of Table 10 presents the heterogeneity in the effect by initial credit score. Consistent with the hypothesis that mineral payments alleviate consumer credit constraints, the effects are approximately twice as strong among initially subprime borrowers than borrowers with initially prime credit. Nevertheless, even among prime credit individuals, mineral payments translate into a large, statistically significant improvement to creditworthiness – treated individuals see an improvement of approximately 8 credit score points. As the specifications estimated on individuals outside of the Barnett shale indicate (even columns), this pattern holds robustly outside of the Barnett shale area.

Panel B of Table 10 presents the heterogeneity in the effect by initial income across quintiles of the 2005 income distribution. Again, consistent with constraints, the effect of mineral payments on creditworthiness is much stronger among individuals in the bottom quintile of the income distribution than it is at higher incomes. The effect monotonically decreases in income, becoming very small (1.46 credit score points) for the top quintile of the income distribution.

In Panel C of Table 10, we perform a double sort on initial income and initial creditworthiness. To simplify the exposition, we pool the bottom two quintiles into "Low Income" and pool the top two quintiles into "High Income." Using this categorization, we estimate the specification (3) with individual and ZIP3-year fixed effects separately by each combination of income (low or high) and creditworthiness (subprime, near prime, prime).

The estimates indicate that both initial creditworthiness and initial income are important dimensions of heterogeneity, but that low initial income matters more than initially subprime credit. Within credit category, the effect of mineral payments on credit scores is approximately 10 credit score points higher for low income individuals than for high income individuals. Interestingly, low initial income appears to be a pre-condition for creditworthiness (subprime versus prime credit) to matter. This is sensible. For an inflow of mineral rights payments to matter for creditworthiness through alleviating a constraint, the individual would need to be constrained in the first place. Put differently, for an individual who is subprime despite having high initial income, it is difficult to imagine how mineral rights payments alleviate consumer financial risk by alleviating the income constraint.

5.4 Heterogeneity in Payments

We also estimate specifications that allow the effect of mineral payments to vary by payment size. Specifically, we estimate a specification for credit score using the following specification.

$$Score_{it} = \gamma_i + \gamma_{zt} + \gamma_{at} + \sum_{b=1}^{B} \beta_1^b pmt\_bin_i^b \times post_t + \beta_2 InitScore_i \times post + \epsilon_{it}, \tag{4}$$

which is perfectly analogous to the specification for revolving credit in equation (2). As in the previous specification, we estimate these specifications for the treated sample only, using individuals who received very small payments (<$5000) as controls within the treated sample. The only difference is the dependent variable $Score_{it}$ – Experian Vantage Score. In this specification, the coefficients of interest are β_1^b, which capture the long-term effect on credit scores for the payment bin b relative to individuals who receive very small payments.

Table 11 presents the estimation results on heterogeneity in payments, separately by initially subprime, near prime and prime credit consumers. Consistent with the broad evidence, the effects are largest among initially subprime and near prime credit consumers. Indeed, prime credit individuals exhibit small and statistically insignificant effects across the distribution of payments. Regardless of the specification and controls, the effect of payments on creditworthiness is largest for low and moderate payments. Payments between $5,000 and $20,000 have a similar effect to much larger payments (e.g., $100,000 to $1 million), and across specifications are larger than the effect among individuals who receive $1 million or more in payments. Figure 8 presents these estimates visually, with 90 percent confidence bands. This finding suggests that – after a moderate shock – additional payments do not significantly translate into improvements to a consumer's creditworthiness.

6 Conclusions and Discussion

This paper provides new evidence of the long-run effects of wealth on household outcomes. We combine new data on $14.6 billion in oil and gas royalty payments over 11 years with individuals' credit reports from one of three large credit reporting agencies. These royalty payments were unanticipated by households because they are the result of the fracking revolution that shocked the U.S.

oil and gas industry in the mid- to late-2000s. Recipients of these royalty payments reside in all 50 states and span the range of creditworthiness prior to receiving these payments. The royalty payments vary in size from less than $1 dollar to hundreds of thousands of dollars per month. Hence, the coverage of the data, combined with the variation in the size and timing of payments provides an ideal setting to test for the credit market effects of wealth shocks.

We find substantial heterogeneity in how households use these unexpected wealth shocks. Subprime households use the additional wealth to pay down debt, consistent with these households facing a significant debt burden. This result suggests that household financial stresses may blunt the consumption effects of unanticipated cash flows. On the other hand, initially-prime households increase debt levels across credit categories, including revolving balances installment credit, such as mortgages and auto loans. Although the increase in revolving credit is consistent with using credit as a payment technology, the effects on mortgages and auto loans suggest that greater wealth complements the demand for credit for those who are in good financial standing.

Finally, we consider the effects of these royalty payments and the changes in debt usage on households' financial risk. We find that these royalty payments lead to large improvements in the state of the consumer balance sheets a decade after the payments commence. The financial-health improvements are largest for those that were initial sub-prime, but we observe improvements for all credit score ranges. These results suggest that positive wealth shocks can lead to marked, broad reductions in financial risk. Taken together, these findings provide important new evidence on how unanticipated cash flows affect household balance sheets.

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Figures

Figure 1: Mineral Rights Payments versus Natural Gas Prices

Note: This figure plots the aggregate monthly payments received by minerals over time (primary y-axis), relative to the price of natural gas ($/mmbtu, secondary y-axis). The mineral payment data is computed using the payment data compiled from our study and the natural gas price data is obtained from the U.S. Energy Information Administration.

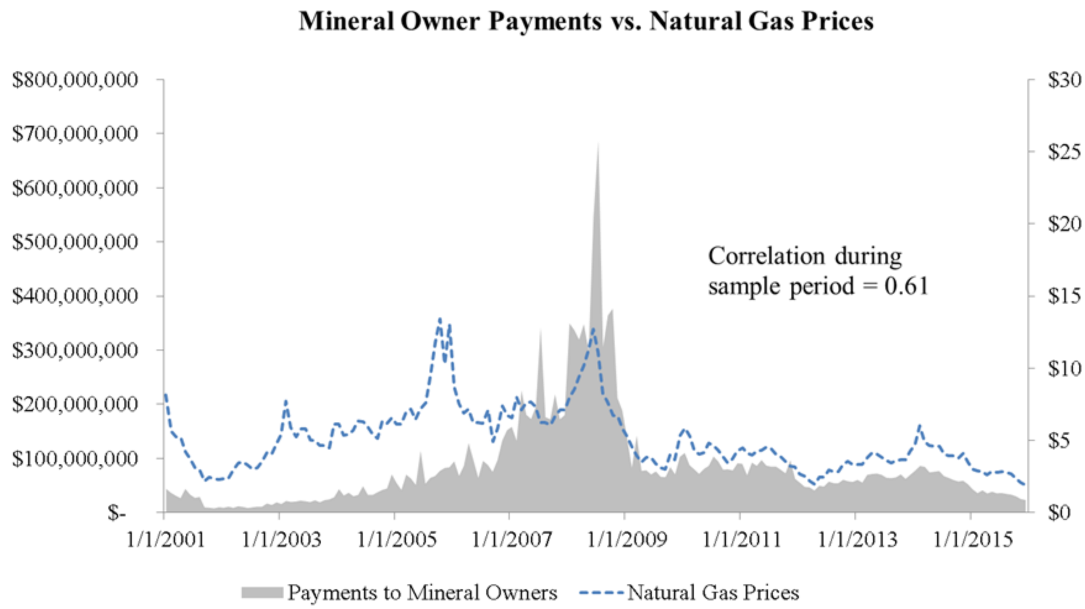


Figure 2: Wells under Production in the Barnett Shale over Time

Note: This figure plots the number of Barnett Shale wells over time in the four counties of our study: Wise, Denton, Tarrant, and Johnson. The data on well numbers was obtained from Smith International Corporation and the Texas Railroad Commission.

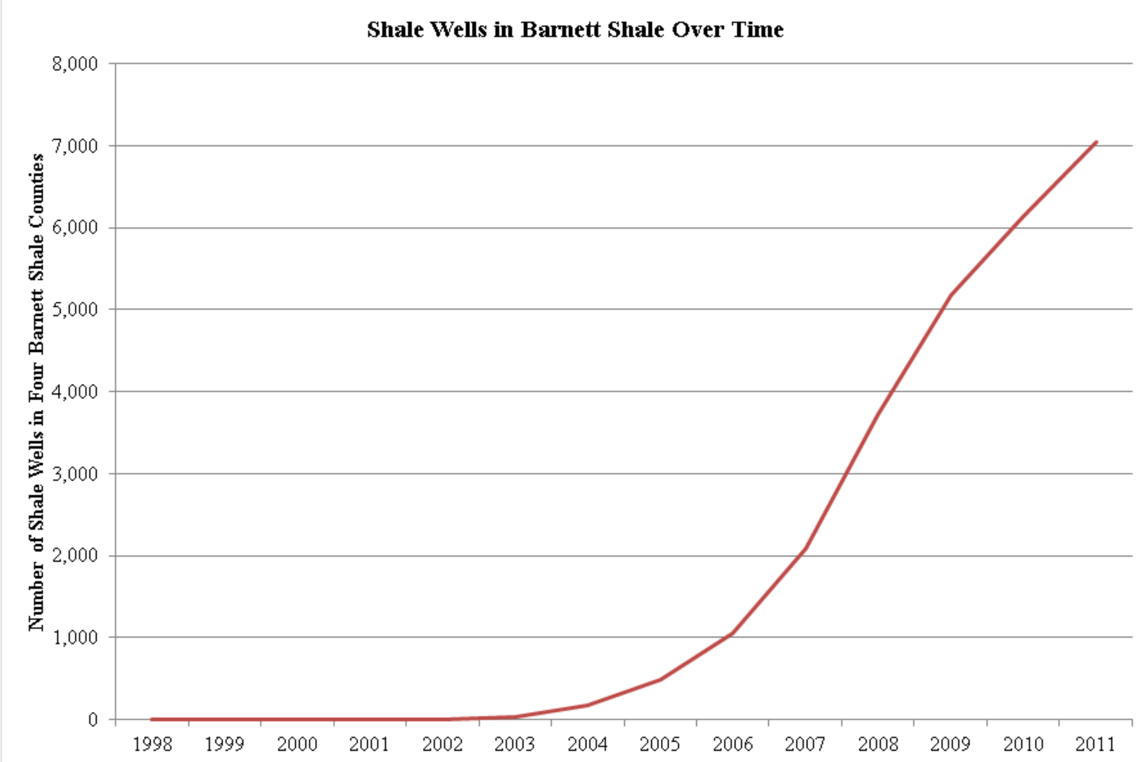


Figure 3: The Spatial Distribution of Wells (Johnson County)

Note: This figure plots a series of maps of snapshots of shale drilling activity over time. The yellow lines represent the horizontal wellbores of the Barnett Shale wells.

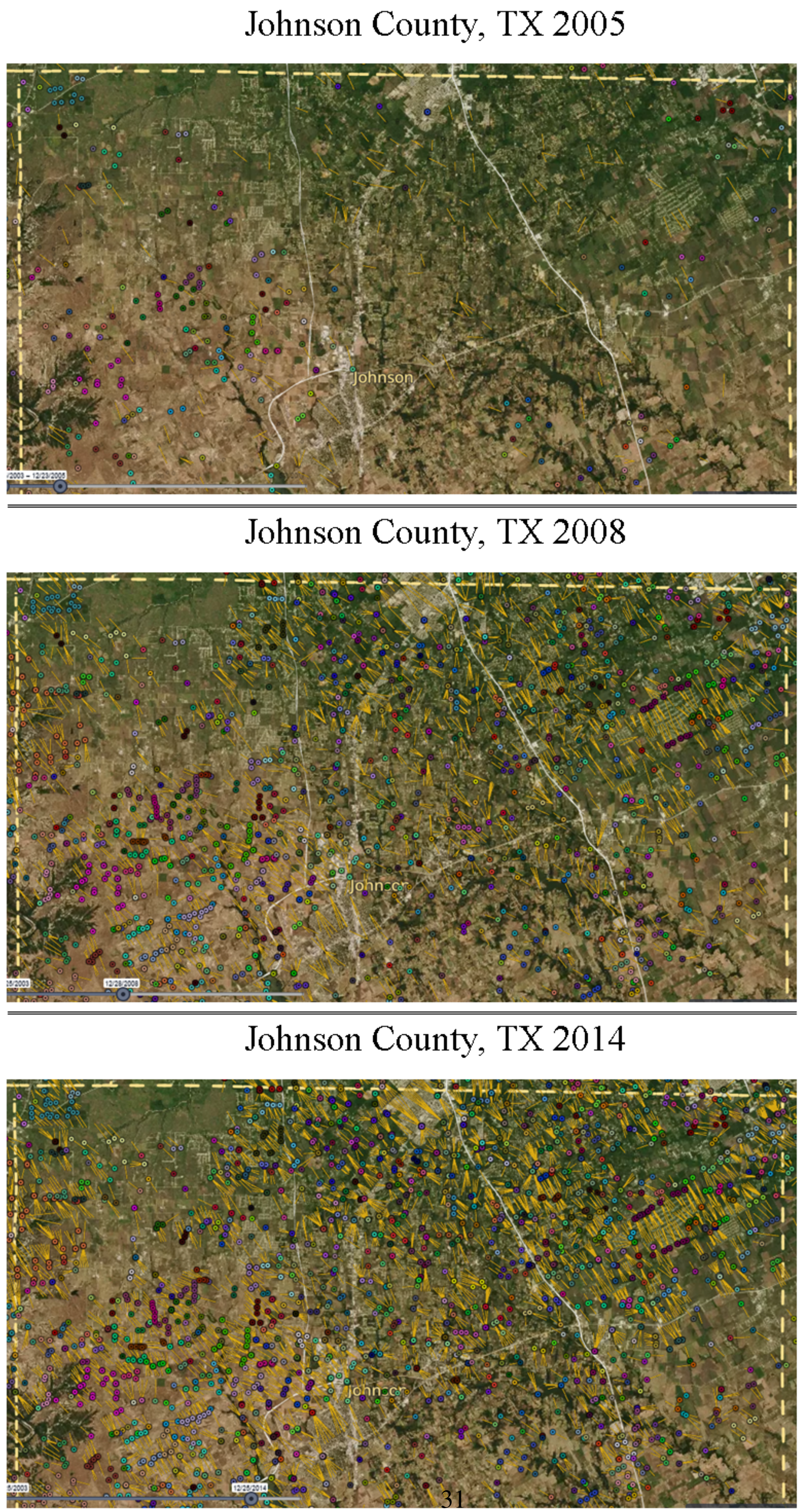


Figure 4: Locations of Individuals Receiving Mineral Rights Payments

Note: This figure plots the location of the different mineral owners in our study who own minerals in the Barnett Shale. The location data is based on the zip code that mineral owners reside at according to property tax and credit bureau records.

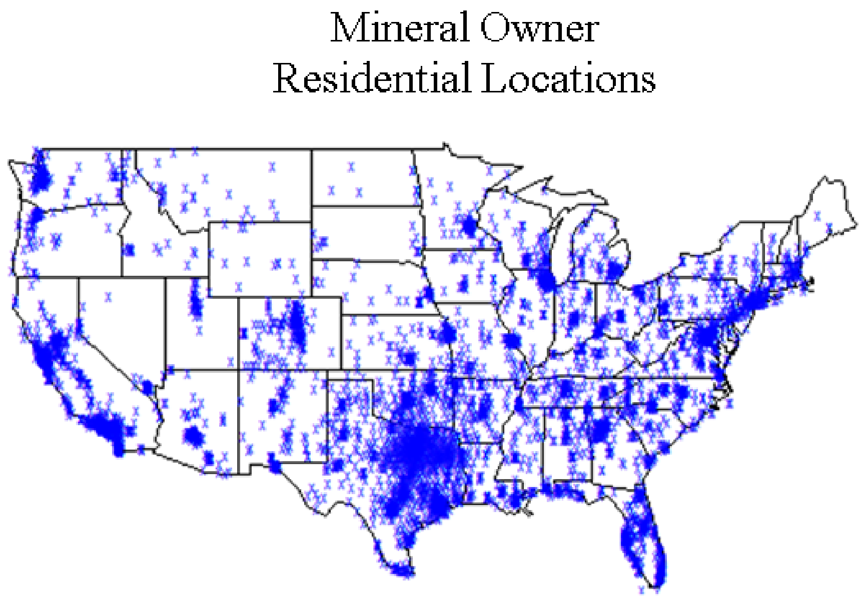


Figure 5: Comparison of Mineral Rights Owners to the Nationally Representative Sample

Note: This figure plots the distribution of credit scores of the mineral owners in our sample relative to a national random sample of people in the United Stats as of 2005. The blue bars represent the national random sample and the tan bars represent the mineral owner sample. The national random sample is based on a national random sample of 259,634 people.

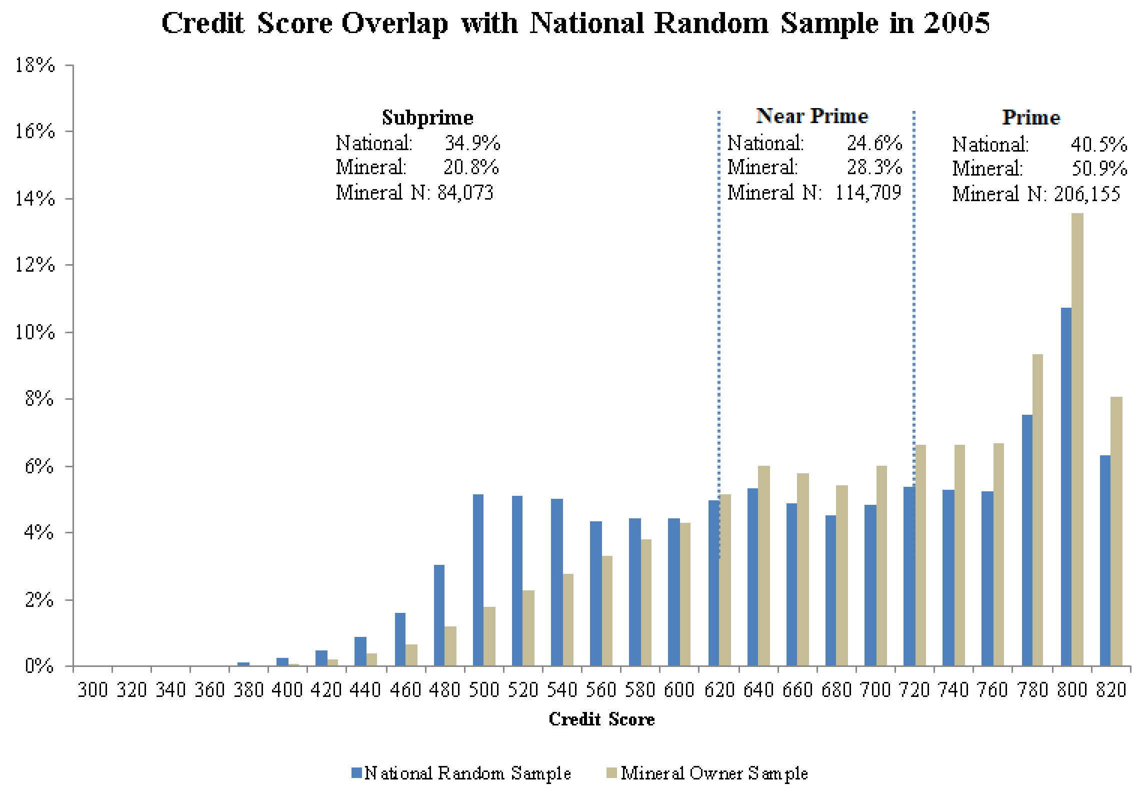


Figure 6: Long Run Effects of wealth Shocks on Revolving Balances – Heterogeneous Payment Amounts, Split by Initial Credit Quality

Note: This figure presents plots of the heterogeneous effect of mineral rights payments on revolving balance using the estimates in Table 4. The dependent variable is 100 x log of (one plus) the revolving balance to admit a percentage change interpretation. The baseline category in these specifications is the set of individuals who receive small payments (below $5,000 in aggregate). The blue dashed lines are 90% error bands, standard errors clustered by ZIP3.

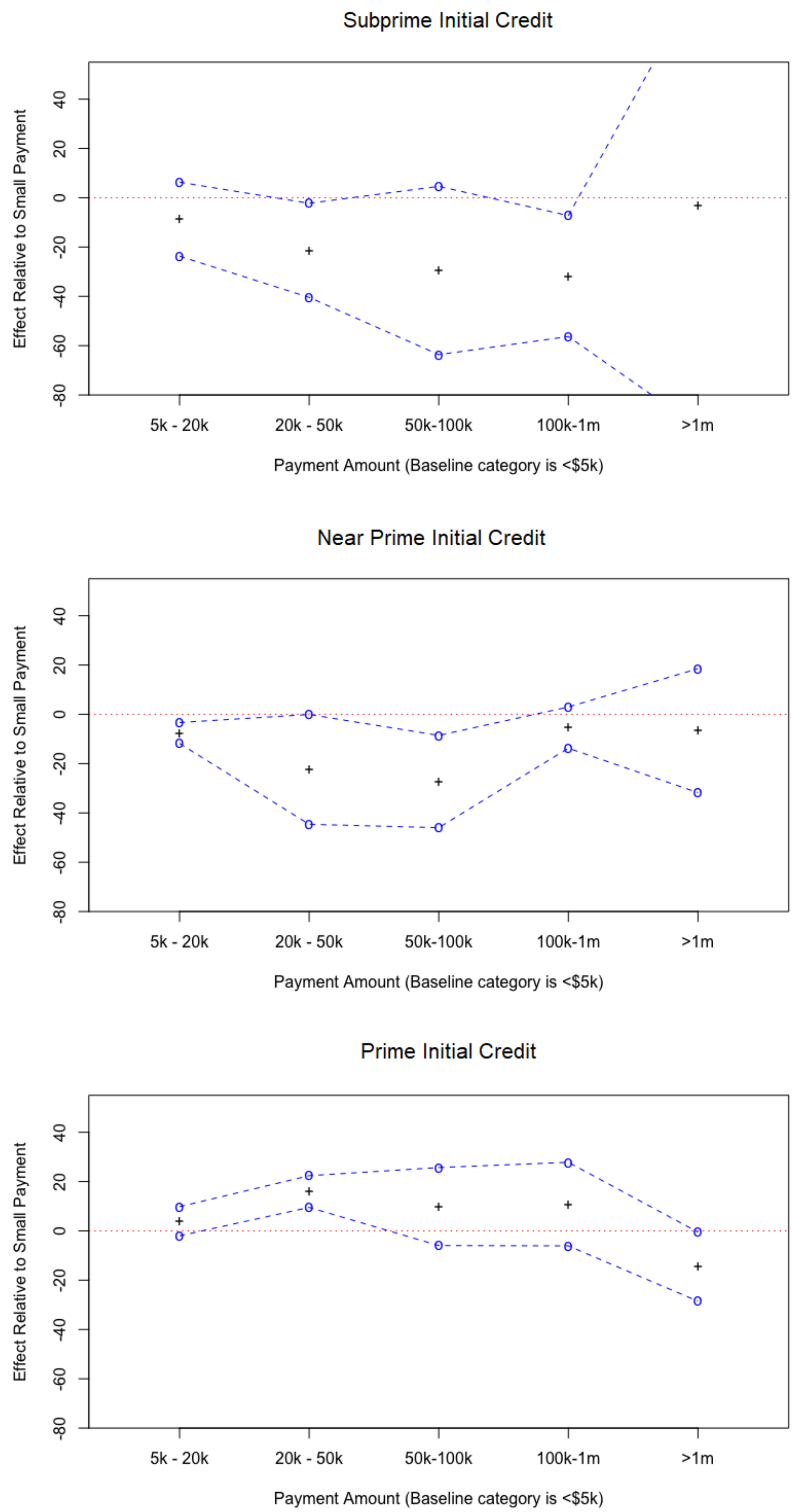


Figure 7: Cumulative Distribution of Credit Scores – Treated versus Control Sample, Before versus After Payments

Note: This figure presents plots of the empirical cumulative distribution functions for the credit score distribution in 2005 (before payments) and for the credit score distribution in 2015 (after payments), separately for the treatment sample and the propensity matched control sample. These plots provide an unconditional depiction of how the distribution of credit scores changes in the treatment and control samples.

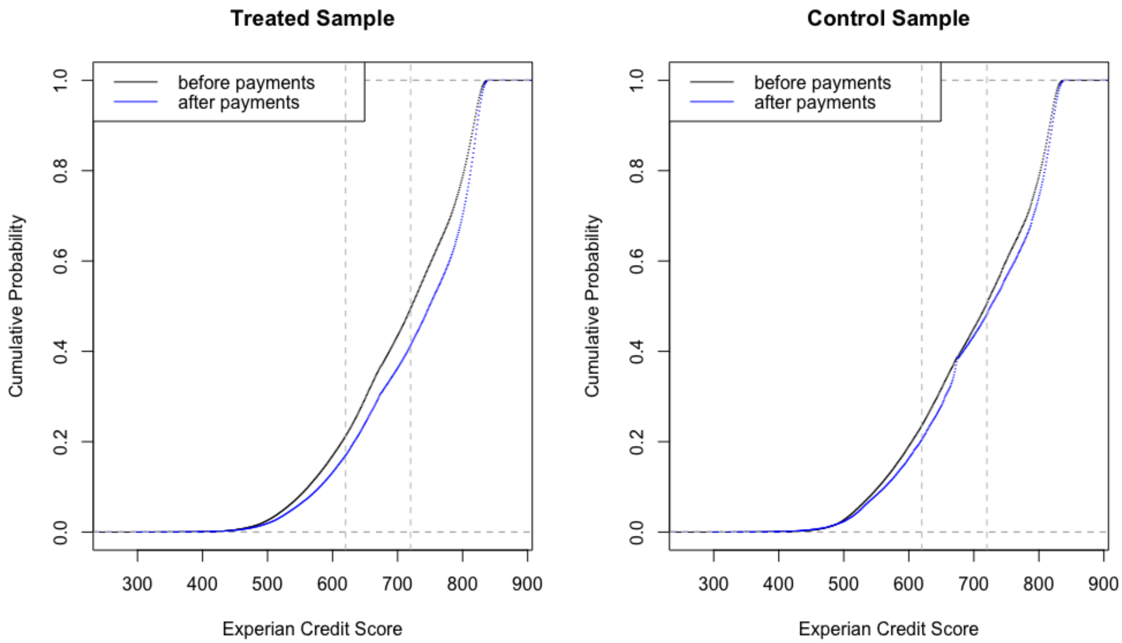
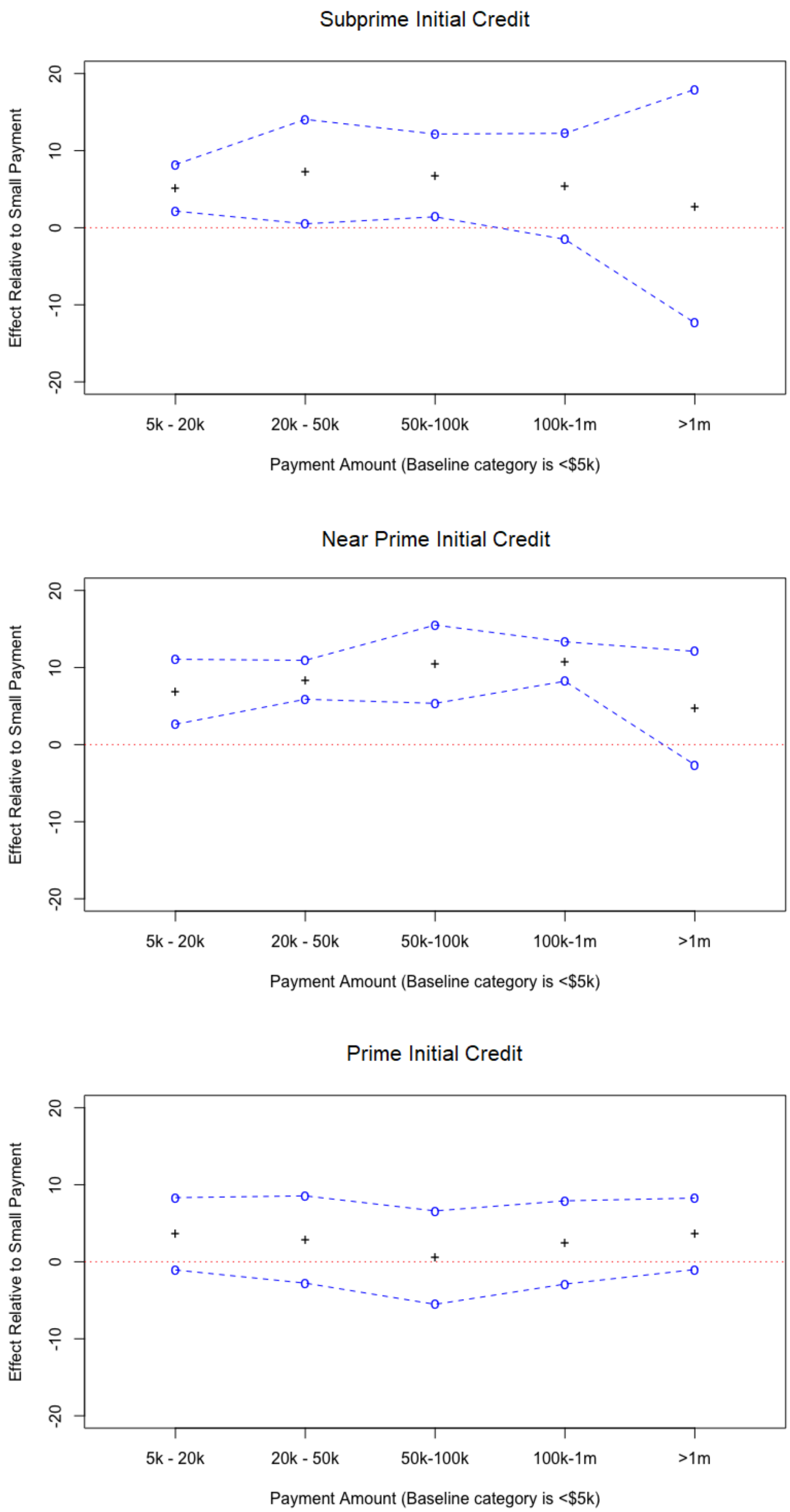


Figure 8: Long Run Effects of wealth Shocks on Credit Scores – Heterogeneous Payment Amounts, Split by Initial Credit Quality

Note: This figure presents plots of the heterogeneous effect of mineral rights payments on credit score using the estimates in Table 10. The baseline category in these specifications is the set of individuals who receive small payments (below $5,000 in aggregate). The blue dashed lines are 90% error bands, standard errors clustered by ZIP3.



Tables

Summary Statistics and Balance

Table 1: Summary Statistics

Note: This table reports summary statistics for our mineral payment and credit bureau data. The Mineral Payment data has a unit of observation at the mineral owner level and provides summary statistics on the payments that mineral owners receiv along with the amount of net mineral acres they own. The Credit Data provides summary statistics on the credit data used in our main regressions, and has a unit of observation at the individual-year level (the two years being 2005 and 2015). It includes both mineral owners and matched control individuals used in our panel.

Mineral Payment Data	*N*	Mean	Std Dev	p1	p10	p25	p50	p75	p90	p99
Total Payments to Mineral Owners	404,937	$45,831	$580,198	–	$180	$756	$2,584	$7,554	$33,949	$751,507
Acres Owned by Mineral Owners	255,784	3.97	69.15	0.02	0.10	0.20	0.31	0.63	2.63	80.00

Credit Data – Panel	*N*	Mean	Std Dev	p1	p10	p25	p50	p75	p90	p99
Credit Score (Vantage Score)	1,591,543	709	97	474	566	640	726	798	819	832
W2 Income	1,591,543	$52,285	$24,521	$22,000	$30,000	$37,000	$46,000	$61,000	$79,000	$147,000
Mortgage Balance	732,055	$135,064	$172,598	$1,101	$27,631	$57,599	$98,010	$157,836	$260,727	$737,330
Mortgage Limit	731,921	$159,503	$242,045	$19,768	$50,000	$75,100	$115,648	$180,000	$294,368	$844,650
Mortgage Utilization	717,775	82%	21%	10%	52%	77%	90%	96%	99%	99%
Credit Card Balance	1,324,757	$9,547	$27,635	–	$34	$707	$3,188	$10,065	$24,166	$82,786
Credit Card Balance	1,324,757	$42,708	$58,148	$250	$2,391	$10,430	$29,120	$58,300	$96,224	$213,200
Credit Card Utilization	1,187,992	28%	28%	0%	2%	5%	17%	46%	75%	98%

Table 2: Comparison of Outcomes for Treatment and Matched Controls

Note: This Table reports differences in key variables from credit data across our treatment (Mineral Owner) and control (matched sample) groups, as well as our treatment (Mineral Owner) as of 2005. The t-test comparisons are done for each variable and the p-value, based on clustering by 3 digit zip (similar to our main tests) is reported.

Variable	Treatment	Control	Difference	p-value
Credit Score (Vantage Score)	705	701	4	0.8092
W2 Income	$50,771	$47,389	$3,382	0.6668
Mortgage Balance	$130,874	$123,951	$6,924	0.9341
Mortgage Limit	$146,962	$140,678	$6,284	0.9465
Mortgage Utilization	87%	85%	2%	0.5649
Credit Card Balance	$10,099	$8,321	$1,778	0.7515
Credit Card Limit	$43,401	$37,113	$6,288	0.6810
Credit Card Utilization	28%	28%	0%	0.9366

Credit Card Utilization, Balances and Limits

Table 3: Long-Run Effects on Revolving Card Utilization

Note: In Panel A, the dependent variable is percentage revolving card utilization (balance as a percentage of available revolving card limits, between 0 and 100%). In Panel B, the dependent variable is $100 \times log(1 + rev.balance)$, where the 100 x log functional form yields an approximate percentage change interpretation. The unit of observation is an individual-year in which two years are considered, 2005 (pre) and 2015 (post). The variable *treatment* is an indicator for whether the individual received royalty or bonus payments as part of our sample from the Barnett Shale between 2005 and 2015. The variable *post* is an indicator for whether the individual-year observation is in 2015. Individuals who are not treated are matched controls (propensity score matching on ZIP3, 2005 credit score, and length of credit history) drawn from the control sample from Experian. The interaction *treatment x post* captures the average difference in the change in credit scores between those receiving mineral payments and those in our control sample. Standard errors clustered by ZIP3 in parentheses. *, **, and *** indicate statistical significance at the 10%, 5% and 1% level respectively.

Panel A: Percent Revolving Card Utilization

	(1)	(2)	(3)	(4)	(5)	(6)
subsample	all	$S < 620$	$620 < S < 720$	$S > 720$	all	outside of Barnett
treatment x post	-0.29	-6.86^{***}	-3.73^{***}	2.64^{***}	-1.35^{***}	-2.89^{***}
	(0.20)	(0.79)	(0.46)	(0.09)	(0.30)	(0.75)
treatment x post x initial credit score (Z)	–	–	–	–	4.24^{***}	5.18^{***}
					(0.33)	(0.85)
post x initial credit score (Z)	–	–	–	–	3.83^{***}	3.30^{***}
					(0.31)	(0.65)
Fixed Effects	individual	individual	individual	individual	individual	individual
	ZIP3-year	ZIP3-year	ZIP3-year	ZIP3-year	ZIP3-year	ZIP3-year
Adj. R-squared	0.5299	0.3923	0.4012	0.4084	0.5469	0.5274
Observations	1,372,961	193,551	390,501	781,372	1,372,961	158,920
Baseline Rate	25.41%	48.43%	35.43%	14.62%	25.41%	24.75%

Panel B: Logged Revolving Balances (100 x log)

	(1)	(2)	(3)	(4)	(5)	(6)
subsample	all	S < 620	$620 < S < 720$	S > 720	all	outside of Barnett
treatment x post	4.87^{***}	-21.69^{***}	-29.54^{***}	26.81^{***}	-2.02	-19.72^{***}
	(0.99)	(7.88)	(1.84)	(2.54)	(1.33)	(6.89)
treatment x post x initial credit score (Z)	–	–	–	–	30.25^{***}	48.91^{***}
					(3.68)	(7.66)
post x initial credit score (Z)	–	–	–	–	-17.10^{***}	-18.02^{***}
					(4.44)	(6.30)
Fixed Effects	individual	individual	individual	individual	individual	individual
	ZIP3-year	ZIP3-year	ZIP3-year	ZIP3-year	ZIP3-year	ZIP3-year
Adj. R-squared	0.4673	0.3484	0.4290	0.5099	0.4683	0.4627
Observations	1,393,988	207,360	395,711	783,280	1,393,988	161,268

Table 4: Long-Run Effects on Revolving Balances, Heterogeneity by Payment Size

Note: The dependent variable is the balance across all revolving card accounts (100 x log of balances in odd columns, dollar value in even columns). The unit of observation is an individual-year in which two years are considered, 2005 (pre) and 2015 (post). In these specifications, the variable *treatment* is replaced by a set of indicator variables for whether total royalty or bonus payments between 2005 and 2015 is in the specified interval. The baseline category for these indicator variables is small payments ($<$ \$5000). The variable *post* is an indicator for whether the individual-year observation is in 2015. The interaction *treatment x post* captures the average difference in the change in credit scores between those receiving mineral payments and those in our control sample. Standard errors clustered by ZIP3 in parentheses. *, **, and *** indicate statistical significance at the 10%, 5% and 1% level respectively.

	Subprime Credit ($S < 620$)		Near Prime Credit ($620 < S < 720$)		Prime Credit ($S > 720$)	
	(1)	(2)	(3)	(4)	(5)	(6)
post x payment $\in$ {5k, 20k}	-8.71	-1712.7^{***}	-7.57^{***}	-534.3	3.90	462.8
	(9.12)	(581.2)	(2.58)	(516.0)	(3.59)	(304.5)
post x payment $\in$ {20k, 50k}	-21.33^{*}	-2358.8	-22.35^{*}	-1351.6	15.95^{***}	1339.1^{**}
	(11.60)	(1871.2)	(13.54)	(1042.1)	(3.91)	(636.2)
post x payment $\in$ {50k, 100k}	-29.52	-4038.7^{**}	-27.27^{**}	-1228.4	9.87	1568.0^{**}
	(20.75)	(1786.0)	(11.38)	(2161.2)	(9.62)	(611.6)
post x payment $\in$ {100k, 1mm}	-31.77^{**}	-2999.50	-5.39	-550.6	10.86	497.4
	(14.92)	(3323.8)	(5.08)	(1863.2)	(10.30)	(724.2)
post x payment $\geq$ 1mm	-3.04	-2871.30	-6.62	-1807.0	-14.53^{*}	2124.9
	(54.76)	(2378.6)	(15.28)	(2211.5)	(8.52)	(2110.5)
Fixed Effects	individual	individual	individual	individual	individual	individual
	ZIP3-year	ZIP3-year	ZIP3-year	ZIP3-year	ZIP3-year	ZIP3-year
	acreage bin-year	acreage bin-year	acreage bin-year	acreage bin-year	acreage bin-year	acreage bin-year
	credit bin-year	credit bin-year	credit bin-year	credit bin-year	credit bin-year	credit bin-year
Adj. R-squared	0.2787	0.2900	0.3537	0.3576	0.4392	0.2688
Observations	65,018	65,018	120,981	120,981	249,330	249,330
Average Revolving Balance in 2005	\$11,173	\$11,173	\$16,866	\$16,866	\$6,317	\$6,317

Mortgages

Table 5: Long-Run Effects on Mortgage Utilization and Balances

Note: In Panel A, the dependent variable is the percentage mortage utilization (balance / credit limit). In Panel B, the dependent variable is the log of one plus the mortgage balance, and the coefficient estimates are multiplied by 100 to elicit an approximate percentage change interpretation. The unit of observation is an individual-year in which two years are considered, 2005 (pre) and 2015 (post). The variable *treatment* is an indicator for whether the individual received mineral payments as part of our sample from the Barnett Shale between 2005 and 2015. The variable *post* is an indicator for whether the individual-year observation is in 2015. Individuals who are not treated are matched controls (propensity score matching on ZIP3, 2005 credit score, and length of credit history) drawn from the control sample from Experian. The interaction *treatment x post* captures the average difference in the change in credit scores between those receiving mineral payments and those in our control sample. Standard errors clustered by ZIP3 in parentheses. *, **, and *** indicate statistical significance at the 10%, 5% and 1% level respectively.

Panel A: Mortgage Utilization as a Percentage of Loan

	(1)	(2)	(3)	(4)
subsample	all	$S < 620$	$620 < S < 720$	$S > 720$
treatment x post	0.86^{***}	-0.20	0.80^{*}	1.01^{**}
	(0.32)	(0.63)	(0.47)	(0.43)
Fixed Effects	individual	individual	individual	individual
	ZIP3-year	ZIP3-year	ZIP3-year	ZIP3-year
Adj. R-squared	0.4338	0.4261	0.4243	0.4206
Observations	764,659	130,351	243,574	386,071
Baseline Rate	81.58%	86.65%	84.78%	77.81%

Panel B: Logged Mortgage Balances (100 x log)

	(1)	(2)	(3)	(4)
subsample	all	S $<$ 620	$620 < S < 720$	S $>$ 720
treatment x post	5.92^{***}	-4.25	5.09^{***}	6.51^{**}
	(1.96)	(2.64)	(1.26)	(3.13)
Fixed Effects	individual	individual	individual	individual
	ZIP3-year	ZIP3-year	ZIP3-year	ZIP3-year
Adj. R-squared	0.4663	0.5751	0.5155	0.4560
Observations	764,794	130,371	243,642	386,117
Average Mortgage Balance in 2005	\$134,970	\$111,398	\$136,557	\$138,466

Automobile Lending

Table 6: Auto Loans

Note: The dependent variable is the log of one plus the consumer's auto loan limit. The unit of observation is an individual-year in which two years are considered, 2005 (pre) and 2015 (post). The variable *treatment* is an indicator for whether the individual received royalty or bonus payments as part of our sample from the Barnett Shale between 2005 and 2015. The variable *post* is an indicator for whether the individual-year observation is in 2015. Individuals who are not treated are matched controls (propensity score matching on ZIP3, 2005 credit score, and length of credit history) drawn from the control sample from Experian. The interaction *treatment x post* captures the average difference in the change in credit scores between those receiving mineral payments and those in our control sample. Standard errors clustered by ZIP3 in parentheses. *, **, and *** indicate statistical significance at the 10%, 5% and 1% level respectively.

	(1)	(2)	(3)	(4)	(5)	(6)
subsample	all	$S < 620$	$620 < S < 720$	$S > 720$	all	outside of Barnett
treatment x post	0.017^{***}	0.002	0.01	0.019^{***}	0.014^{**}	-0.06
	(0.005)	(0.03)	(0.02)	(0.007)	(0.006)	(0.05)
treatment x post x initial credit score (Z)	–	–	–	–	0.008	0.04
					(0.01)	(0.05)
post x initial credit score (Z)	–	–	–	–	-0.06^{***}	-0.08^{**}
					(0.01)	(0.04)
Fixed Effects	individual	individual	individual	individual	individual	individual
	ZIP3-year	ZIP3-year	ZIP3-year	ZIP3-year	ZIP3-year	ZIP3-year
Adj. R-squared	0.3833	0.3774	0.3814	0.3942	0.3852	0.3639
Observations	1,372,961	193,551	390,501	781,372	1,372,961	158,920

Total Debt-to-Income

Table 7: Long-Run Debt-to-Income Effects overall and split by credit score bin

Note: In Panel A, the dependent variable is the total debt-to-income of the consumer provided by Experian. In Panel B, the dependent variable is an indicator for whether the consumer's debt-to-income ratio exceeds the Qualified Mortgage threshold of 43%, multiplied by 100 to admit a percentage change interpretation. The unit of observation is an individual-year in which two years are considered, 2005 (pre) and 2015 (post). The variable *treatment* is an indicator for whether the individual received royalty or bonus payments as part of our sample from the Barnett Shale between 2005 and 2015. The variable *post* is an indicator for whether the individual-year observation is in 2015. Individuals who are not treated are matched controls (propensity score matching on ZIP3, 2005 credit score, and length of credit history) drawn from the control sample from Experian. The interaction *treatment x post* captures the average difference in the change in credit scores between those receiving mineral payments and those in our control sample. Standard errors clustered by ZIP3 in parentheses. *, **, and *** indicate statistical significance at the 10%, 5% and 1% level respectively.

Panel A: Total Debt-to-Income Ratio

	(1)	(2)	(3)	(4)
credit score bin	all	$S < 620$	$620 < S < 720$	$S > 720$
treatment x post	-1.25^{***}	-3.35^{***}	-2.39^{***}	0.02
	(0.12)	(0.44)	(0.10)	(0.14)
Fixed Effects	individual	individual	individual	individual
	ZIP3-year	ZIP3-year	ZIP3-year	ZIP3-year
Adj. R-squared	0.5035	0.3426	0.4316	0.5350
Observations	1,508,375	276,184	429,752	794,118
Average Debt-to-Income in 2005	15.1%	20.3%	19.0%	11.3%

Panel B: Percentage of Consumers with DTI Exceeding Qualified Mortgage Threshold

	(1)	(2)	(3)	(4)
credit score bin	all	$S < 620$	$620 < S < 720$	$S > 720$
treatment x post	-1.63^{***}	-5.91^{***}	-2.51^{***}	0.33^{***}
	(0.21)	(1.18)	(0.27)	(0.09)
Fixed Effects	individual	individual	individual	individual
	ZIP3-year	ZIP3-year	ZIP3-year	ZIP3-year
Adj. R-squared	0.1436	0.1088	0.1303	0.1299
Observations	1,508,375	276,184	429,752	794,118
% of Consumers ¿ 43% in 2005	4.49%	10.66%	6.37%	1.39%

Financial Risks

Table 8: Severe Delinquencies and Derogatory Accounts

Note: The dependent variable is the percentage of accounts that are more than 90 days past due (Panel A) or an indicator for having any severe derogatory accounts (Panel B; these are defaults, settled for less than the amount, etc.). The unit of observation is an individual-year in which two years are considered, 2005 (pre) and 2015 (post). The variable *treatment* is an indicator for whether the individual received royalty or bonus payments as part of our sample from the Barnett Shale between 2005 and 2015. The variable *post* is an indicator for whether the individual-year observation is in 2015. Individuals who are not treated are matched controls (propensity score matching on ZIP3, 2005 credit score, and length of credit history) drawn from the control sample from Experian. The interaction *treatment x post* captures the average difference in the change in credit scores between those receiving mineral payments and those in our control sample. Standard errors clustered by ZIP3 in parentheses. *, **, and *** indicate statistical significance at the 10%, 5% and 1% level respectively.

Panel A: Percent of Accounts 90 or More Days Past Due

	(1)	(2)	(3)	(4)	(5)	(6)
subsample	all	$S < 620$	$620 < S < 720$	$S > 720$	all	outside of Barnett
treatment x post	-0.02^{*}	-0.19^{***}	0.05^{***}	0.002	-0.02^{**}	0.01
	(0.01)	(0.03)	(0.02)	(0.003)	(0.01)	(0.03)
treatment x post x initial credit score (Z)	–	–	–	–	0.08^{***}	-0.001
					(0.02)	(0.06)
post x initial credit score (Z)	–	–	–	–	0.05^{***}	0.09^{*}
					(0.004)	(0.05)
Fixed Effects	individual	individual	individual	individual	individual	individual
	ZIP3-year	ZIP3-year	ZIP3-year	ZIP3-year	ZIP3-year	ZIP3-year
Adj. R-squared	0.1674	0.1470	0.1424	0.0690	0.1693	0.2429
Observations	1,372,961	193,551	390,501	781,372	1,372,961	158,920
Baseline Rate	0.120%	0.430%	0.080%	0.015%	0.120%	0.099%

Panel B: Indicator for Severe Derogatory Accounts

	(1)	(2)	(3)	(4)	(5)	(6)
subsample	all	$S < 620$	$620 < S < 720$	$S > 720$	all	outside of Barnett
treatment x post	0.004^{*}	-0.04^{***}	0.03^{***}	0.004^{**}	0.003^{**}	-0.005
	(0.002)	(0.004)	(0.002)	(0.002)	(0.002)	(0.007)
treatment x post x initial credit score (Z)	–	–	–	–	0.01^{***}	0.015^{*}
					(0.001)	(0.008)
post x initial credit score (Z)	–	–	–	–	0.01^{***}	-0.006
					(0.003)	(0.006)
Fixed Effects	individual	individual	individual	individual	individual	individual
	ZIP3-year	ZIP3-year	ZIP3-year	ZIP3-year	ZIP3-year	ZIP3-year
Adj. R-squared	0.2751	0.1852	0.1830	0.1590	0.2772	0.3566
Observations	1,372,961	193,551	390,501	781,372	1,372,961	158,920
Baseline Rate	0.108	0.309	0.109	0.026	0.108	0.086

Table 9: Long-Run Credit Score Effects

Note: The unit of observation is an individual-year in which two years are considered, 2005 (pre) and 2015 (post). Columns (1) through (4) present results using the full sample of individuals. Columns (5) through (8) present results for only individuals who reside outside of the Barnett Shale area. The variable *treatment* is an indicator for whether the individual received royalty or bonus payments as part of our sample from the Barnett Shale between 2005 and 2015. The variable *post* is an indicator for whether the individual-year observation is in 2015. Individuals who are not treated are matched controls (propensity score matching on ZIP3, 2005 credit score, and length of credit history) drawn from the control sample from Experian. The interaction *treatment x post* captures the average difference in the change in credit scores between those receiving mineral payments and those in our control sample. Standard errors clustered by ZIP3 in parentheses. *, **, and *** indicate statistical significance at the 10%, 5% and 1% level respectively.

	Full Sample				Resides Outside of Barnett			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
treatment x post	10.03^{***}	9.52^{***}	8.86^{***}	9.03^{***}	12.00^{***}	11.45^{***}	9.55^{***}	8.90^{***}
	(0.83)	(0.77)	(0.79)	(0.75)	(1.05)	(1.00)	(1.21)	(1.29)
post	7.74^{***}	7.59^{***}	10.07^{*}	–	2.59	2.05^{*}	4.45^{***}	–
	(1.44)	(1.45)	(1.77)		(1.08)	(1.04)	(1.21)	
treatment	3.55^{***}	3.55^{***}	–	–	-0.52	-0.51	–	–
	(0.32)	(0.34)			(0.68)	(0.64)		
Fixed Effects	none	ZIP3	individual	individual ZIP3-year	none	ZIP3	individual	individual ZIP3-year
Adj. R-squared	0.0070	0.0152	0.6988	0.6997	0.0042	0.0325	0.7022	0.7094
Observations	1,647,856	1,647,856	1,647,856	1,647,856	186,274	186,274	186,274	186,274

Table 10: Long-Run Credit Score Effects – Heterogeneity by Initial Credit Score and Initial Income

Note: The unit of observation is an individual-year in which two years are considered, 2005 (pre) and 2015 (post). In Panel A, the results are from subsamples based on initial credit score (Subprime $S < 620$, columns (1) and (2); Near prime $620 < S < 720$, columns (3) and (4); Prime $S > 720$, columns (5) and (6)). In addition, the even columns in Panel A consider only individuals who reside outside of the Barnett Shale. In Panel B, the results are from estimating the specification on individuals within each quintile of the income distribution in 2005. Panel C presents the estimate on *treatment x post* using observations within a double sort on initial income and initial credit score. The variable *treatment* is an indicator for whether the individual received royalty or bonus payments as part of our sample from the Barnett Shale between 2005 and 2015. The variable *post* is an indicator for whether the individual-year observation is in 2015. Individuals who are not treated are matched controls (propensity score matching on ZIP3, 2005 credit score, and length of credit history) drawn from the control sample from Experian. The interaction *treatment x post* captures the average difference in the change in credit scores between those receiving mineral payments and those in our control sample. Standard errors clustered by ZIP3 in parentheses. *, **, and *** indicate statistical significance at the 10%, 5% and 1% level respectively.

Panel A: Heterogeneity by Initial Credit Score

	Subprime Credit ($S < 620$)		Near Prime Credit ($620 < S < 720$)		Prime Credit ($S > 720$)	
	(1)	(2)	(3)	(4)	(5)	(6)
treatment x post	15.13***	18.27***	10.75***	14.40***	8.17***	7.45***
	(0.57)	(5.06)	(0.58)	(4.57)	(1.39)	(1.50)
Fixed Effects	individual ZIP3-year	individual ZIP3-year	individual ZIP3-year	individual ZIP3-year	individual ZIP3-year	individual ZIP3-year
Adj. R-squared	0.3912	0.4657	0.2303	0.2938	0.2925	0.3768
Observations	246,163	21,188	242,067	22,679	659,145	79,585

Panel B: Heterogeneity by Initial Income

	Bottom Quintile	2nd Quintile	3rd Quintile	4th Quintile	Top Quintile
	(1)	(2)	(3)	(4)	(5)
treatment x post	17.57***	11.62***	7.36***	4.05***	1.46*
	(0.68)	(0.60)	(1.48)	(1.22)	(0.79)
Fixed Effects	individual ZIP3-year credit bin-year	individual ZIP3-year credit bin-year	individual ZIP3-year credit bin-year	individual ZIP3-year credit bin-year	individual ZIP3-year credit bin-year
Adj. R-squared	0.6882	0.7124	0.6999	0.6871	0.6556
Observations	329,637	347,809	338,367	316,365	315,678

Panel C: Double Sort by Initial Credit and Initial Income

	Subprime	Near Prime	Prime	Subprime - Prime
	(1)	(2)	(3)	
Low Income	16.03***	12.91***	12.56***	3.47***
	(0.68)	(0.60)	(1.48)	(1.27)
High Income	4.36***	3.69***	3.55***	0.81
	(0.68)	(0.60)	(1.48)	(2.06)
Low - High	11.67***	9.22***	9.01***	
	(1.29)	(1.48)	(2.05)	

Table 11: Long-Run Credit Score Effects – Heterogeneity in Payments, Split by Initial Credit Quality

Note: The unit of observation is an individual-year in which two years are considered, 2005 (pre) and 2015 (post). To focus on payments with a long enough horizon to affect the credit history, we restrict attention to the subsample of individuals who receive their first payment before 2009. In these specifications, the variable *treatment* is replaced by a series of indicators for the total mineral payments received from Barnett Shale production between 2005 and 2015. The baseline category is very small aggregate payments (< $5,000). The variable *post* is an indicator for whether the individual-year observation is in 2015. The interaction *treatment bin x post* captures the average difference in the change in credit scores between those receiving very small mineral payments and those in the specified payment bin. When they are included, characteristic bin-year fixed effects, the bins are constructed from the sample quintiles of the characteristic in 2005. Standard errors clustered by ZIP3 in parentheses. *, **, and *** indicate statistical significance at the 10%, 5% and 1% level respectively.

	Subprime Credit ($S < 620$)		Near Prime Credit ($620 < S < 720$)		Prime Credit ($S > 720$)	
	(1)	(2)	(3)	(4)	(5)	(6)
post x payment $\in$ {5k, 20k}	7.31***	4.28***	8.52***	5.18***	4.43	2.64
	(2.68)	(1.41)	(3.28)	(1.64)	(3.20)	(2.21)
post x payment $\in$ {20k, 50k}	7.96*	5.22	9.90***	5.57***	3.21	1.69
	(4.18)	(3.80)	(1.31)	(1.98)	(3.57)	(2.74)
post x payment $\in$ {50k, 100k}	7.17**	4.80	11.86***	7.29***	0.84	−0.29
	(3.16)	(2.98)	(3.58)	(2.75)	(3.80)	(3.18)
post x payment $\in$ {100k, 1mm}	5.74	3.15	12.05***	7.67***	2.78	1.97
	(4.15)	(4.27)	(2.04)	(1.53)	(3.32)	(2.99)
post x payment $\geq$ 1mm	2.74	0.13	6.02	0.90	3.88	3.31
	(8.91)	(9.41)	(4.78)	(4.47)	(3.82)	(2.89)
Fixed Effects	individual ZIP3-year credit bin-year	individual ZIP3-year credit bin-year age bin-year inc bin-year acreage bin-year	individual ZIP3-year credit bin-year	individual ZIP3-year credit bin-year age bin-year inc bin-year acreage bin-year	individual ZIP3-year credit bin-year	individual ZIP3-year credit bin-year age bin-year inc bin-year acreage bin-year
Adj. R-squared	0.3825	0.3905	0.2266	0.2386	0.3111	0.3260
Observations	100,608	100,608	136,954	136,954	257,376	257,376

Internet Appendix to:

Shale Shocked: The Long Run Effect of Wealth on Household Debt

A Examples of Raw Data

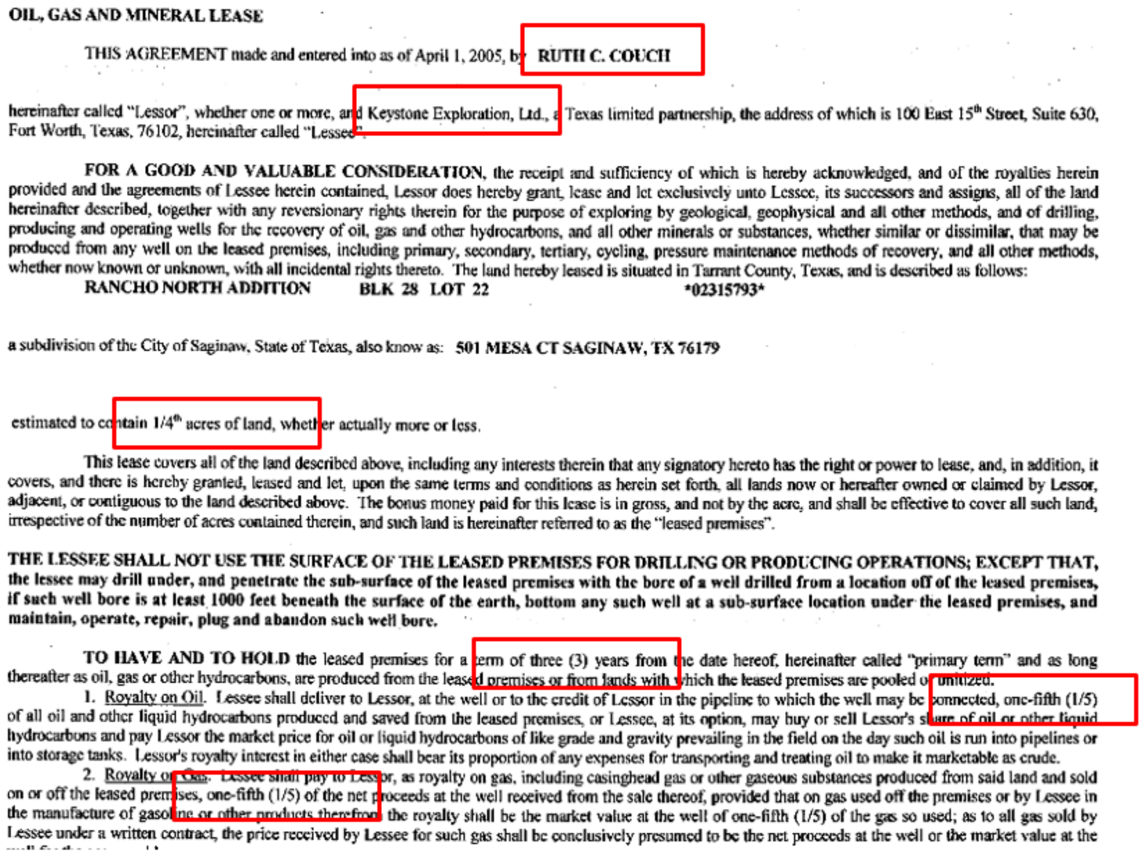
Figure A.1: Example of Raw Mineral Appraisal Roll

Note: This figure presents an example of the raw data from the tax appraisal rolls. We processed the raw text into our mineral payments data by using appraisal rolls to merge with production data in order to compute precise values for monthly mineral rights payments.

OWN/GEO	NAME AND ADDRESS	LEASE#	PROPERTY DESCRIPTION		OPERATOR/JURISDICTION	VALUE
68713592		5947 G1	HIGHTOWER # 2H KELWAT A-1010 MANN W SUR RRC-09-259285 KELWAT-.3967	.000138 RI - 281.460 ACRES	CHESAPEAKE OPER LLC 00-01-70-71-56-32	20 ***
						220 *
137480 67449212	AARON DAVID 4021 J RENDON RD BURLESON TX 76028-3629	5236 G1	BIG DADDY # 1H A-1341 RAMEY R R SUR RRC-09-257711	.000115 RI -- 342.320 ACRES	CHESAPEAKE OPER LLC 00-01-53-14-70-71-73	130
66505607		7308 G1	BOBCAT # 1H A- 425 DAVIS S SUR RRC-09-265910	.000141 RI - 349.010 ACRES	CHESAPEAKE OPER LLC 00-01-02-45-70-71	420
66505615		7539 G1	BOBCAT # 2H A- 425 DAVIS S SUR RRC-09-269124	.000071 RI - 349.010 ACRES	CHESAPEAKE OPER LLC 00-01-02-45-70-71	200
					*****	750 *
20506 13301667	AARON DAVID & MARGARET 4021 J RENDON RD BURLESON TX 76028-3629	2820 G1	RAFAEL UT #A 1H A-1263 RENDON J SUR RRC-09-240685	.002230 RI -- 392.800 ACRES	XTO ENERGY INC 00-01-59-74-70-71-72	940
13301691		2916 G1	RAFAEL UT #B 1H A-1263 RENDON J SUR RRC-09-243133	.002230 RI - 457.600 ACRES	XTO ENERGY INC 00-01-59-74-70-71-72	980
13301705		3721 G1	RAFAEL UT #B 2H A-1263 RENDON J SUR RRC-09-246148	.002230 RI - 457.600 ACRES	XTO ENERGY INC 00-01-59-74-70-71-72	1,870
13301675		2821 G1	RAFAEL UT #B 3H A-1263 RENDON J SUR RRC-09-241008	.002230 RI - 457.600 ACRES	XTO ENERGY INC 00-01-59-74-70-71-72	2,110
61139378		5894 G1	RAFAEL UT #B 4H A-1263 RENDON J SUR RRC-09-260498	.002230 RI - 459.690 ACRES	XTO ENERGY INC 00-01-59-74-70-71-72	1,820
61139386		5895 G1	RAFAEL UT #B 5H A-1263 RENDON J SUR RRC-09-260500	.002230 RI - 459.690 ACRES	XTO ENERGY INC 00-01-59-74-70-71-72	1,160
13301683		2822 G1	RAFAEL UT #B 6H A-1263 RENDON J SUR RRC-09-241353	.002230 RI - 459.690 ACRES	XTO ENERGY INC 00-01-59-74-70-71-72	2,820
61139394		5896 G1	RAFAEL UT #B 7H A-1263 RENDON J SUR RRC-09-2260582	.002230 RI - 459.690 ACRES	XTO ENERGY INC 00-01-59-74-70-71-72	660
						12,360 *
18382	AARON DELVIN E & AMY T 620 BLUFF SPRINGS RD FORT WORTH TX 76108-7600	2845 G1	BARONE 1H A 1661 WILCOX J (PARKER CO) RRC-09-	.002930 RI --	GRAND OPERATING INC 84	0

Figure A.2: Example of a Mineral Rights Lease

Note: This figure presents an example of a mineral rights lease, with key information highlighted. We processed a large sample of these leases to augment our tax appraisal data set, as well as to compute estimates of lease bonus payments.



OIL, GAS AND MINERAL LEASE

THIS AGREEMENT made and entered into as of April 1, 2005, by **RUTH C. COUCH**

hereinafter called "Lessor", whether one or more, and Keystone Exploration, Ltd., a Texas limited partnership, the address of which is 100 East 15th Street, Suite 630, Fort Worth, Texas, 76102, hereinafter called "Lessee".

FOR A GOOD AND VALUABLE CONSIDERATION, the receipt and sufficiency of which is hereby acknowledged, and of the royalties herein provided and the agreements of Lessee herein contained, Lessor does hereby grant, lease and let exclusively unto Lessee, its successors and assigns, all of the land hereinafter described, together with any reversionary rights therein for the purpose of exploring by geological, geophysical and all other methods, and of drilling, producing and operating wells for the recovery of oil, gas and other hydrocarbons, and all other minerals or substances, whether similar or dissimilar, that may be produced from any well on the leased premises, including primary, secondary, tertiary, cycling, pressure maintenance methods of recovery, and all other methods, whether now known or unknown, with all incidental rights thereto. The land hereby leased is situated in Tarrant County, Texas, and is described as follows:
RANCHO NORTH ADDITION BLK 28 LOT 22 *02315793*

a subdivision of the City of Saginaw, State of Texas, also know as: **501 MESA CT SAGINAW, TX 76179**

estimated to contain 1/4th acres of land, whether actually more or less.

This lease covers all of the land described above, including any interests therein that any signatory hereto has the right or power to lease, and, in addition, it covers, and there is hereby granted, leased and let, upon the same terms and conditions as herein set forth, all lands now or hereafter owned or claimed by Lessor, adjacent, or contiguous to the land described above. The bonus money paid for this lease is in gross, and not by the acre, and shall be effective to cover all such land, irrespective of the number of acres contained therein, and such land is hereinafter referred to as the "leased premises".

THE LESSEE SHALL NOT USE THE SURFACE OF THE LEASED PREMISES FOR DRILLING OR PRODUCING OPERATIONS; EXCEPT THAT, the lessee may drill under, and penetrate the sub-surface of the leased premises with the bore of a well drilled from a location off of the leased premises, if such well bore is at least 1000 feet beneath the surface of the earth, bottom any such well at a sub-surface location under the leased premises, and maintain, operate, repair, plug and abandon such well bore.

TO HAVE AND TO HOLD the leased premises for a term of three (3) years from the date hereof, hereinafter called "primary term" and as long thereafter as oil, gas or other hydrocarbons, are produced from the leased premises or from lands with which the leased premises are pooled or unitized.

1. Royalty on Oil. Lessee shall deliver to Lessor, at the well or to the credit of Lessor in the pipeline to which the well may be connected, one-fifth (1/5) of all oil and other liquid hydrocarbons produced and saved from the leased premises, or Lessee, at its option, may buy or sell Lessor's share of oil or other liquid hydrocarbons and pay Lessor the market price for oil or liquid hydrocarbons of like grade and gravity prevailing in the field on the day such oil is run into pipelines or into storage tanks. Lessor's royalty interest in either case shall bear its proportion of any expenses for transporting and treating oil to make it marketable as crude.

2. Royalty on Gas. Lessee shall pay to Lessor, as royalty on gas, including casinghead gas or other gaseous substances produced from said land and sold on or off the leased premises, one-fifth (1/5) of the net proceeds at the well received from the sale thereof, provided that on gas used off the premises or by Lessee in the manufacture of gasoline or other products therefrom the royalty shall be the market value at the well of one-fifth (1/5) of the gas so used; as to all gas sold by Lessee under a written contract, the price received by Lessee for such gas shall be conclusively presumed to be the net proceeds at the well or the market value at the

B Appendix Tables

Table A.1: Long-Run Effects on Revolving Card Utilization – Restricting to Consumers with Only Bonus Payments

Note: In Panel A, the dependent variable is percentage revolving card utilization (balance as a percentage of available revolving card limits, between 0 and 100%). In Panel B, the dependent variable is $100 \times log(1 + rev.balance)$, where the 100 x log functional form yields an approximate percentage change interpretation. The unit of observation is an individual-year in which two years are considered, 2005 (pre) and 2015 (post). The variable *treatment* is an indicator for whether the individual received royalty or bonus payments as part of our sample from the Barnett Shale between 2005 and 2015. The variable *post* is an indicator for whether the individual-year observation is in 2015. Individuals who are not treated are matched controls (propensity score matching on ZIP3, 2005 credit score, and length of credit history) drawn from the control sample from Experian. The interaction *treatment x post* captures the average difference in the change in credit scores between those receiving mineral payments and those in our control sample. Standard errors clustered by ZIP3 in parentheses. *, **, and *** indicate statistical significance at the 10%, 5% and 1% level respectively.

Panel A: Percent Revolving Card Utilization

	(1)	(2)	(3)	(4)	(5)	(6)
subsample	all	$S < 620$	$620 < S < 720$	$S > 720$	all	outside of Barnett
treatment x post	0.305	-5.81^{***}	-2.36^{***}	2.99^{***}	-0.78	-2.08^{***}
	(0.43)	(1.27)	(0.98)	(0.40)	(0.66)	(1.49)
treatment x post x initial credit score (Z)	–	–	–	–	4.00^{***}	5.39^{***}
					(0.39)	(1.63)
post x initial credit score (Z)	–	–	–	–	4.15^{***}	3.50^{***}
					(0.16)	(1.06)
Fixed Effects	individual	individual	individual	individual	individual	individual
	ZIP3-year	ZIP3-year	ZIP3-year	ZIP3-year	ZIP3-year	ZIP3-year
Adj. R-squared	0.4788	0.3057	0.3313	0.3402	0.4973	0.4654
Observations	416,145	96,195	119,004	118,676	416,145	45,231
Baseline Rate	25.41%	48.43%	35.43%	14.62%	25.41%	24.75%

Panel B: Logged Revolving Balances (100 x log)

	(1)	(2)	(3)	(4)	(5)	(6)
subsample	all	S < 620	$620 < S < 720$	S > 720	all	outside of Barnett
treatment x post	4.51	-19.56^{***}	-30.25^{***}	28.26^{***}	-29.69^{***}	-11.50^{***}
	(2.87)	(5.64)	(4.66)	(4.44)	(14.22)	(13.27)
treatment x post x initial credit score (Z)	–	–	–	–	31.13^{***}	56.96^{***}
					(4.64)	(14.41)
post x initial credit score (Z)	–	–	–	–	-17.98^{***}	-23.67^{***}
					(2.30)	(10.78)
Fixed Effects	individual	individual	individual	individual	individual	individual
	ZIP3-year	ZIP3-year	ZIP3-year	ZIP3-year	ZIP3-year	ZIP3-year
Adj. R-squared	0.4109	0.2721	0.3685	0.4627	0.4119	0.3856
Observations	416,145	96,195	119,004	118,676	416,145	45,231

Table A.2: Long-Run Credit Score Effects – Heterogeneity by Initial Credit Score and Initial Income – Only Bonus Payments

Note: The unit of observation is an individual-year in which two years are considered, 2005 (pre) and 2015 (post). In Panel A, the results are from subsamples based on initial credit score (Subprime $S < 620$, columns (1) and (2); Near prime $620 < S < 720$, columns (3) and (4); Prime $S > 720$, columns (5) and (6)). In addition, the even columns in Panel A consider only individuals who reside outside of the Barnett Shale. In Panel B, the results are from estimating the specification on individuals within each quintile of the income distribution in 2005. Panel C presents the estimate on *treatment x post* using observations within a double sort on initial income and initial credit score. The variable *treatment* is an indicator for whether the individual received royalty or bonus payments as part of our sample from the Barnett Shale between 2005 and 2015. The variable *post* is an indicator for whether the individual-year observation is in 2015. Individuals who are not treated are matched controls (propensity score matching on ZIP3, 2005 credit score, and length of credit history) drawn from the control sample from Experian. The interaction *treatment x post* captures the average difference in the change in credit scores between those receiving mineral payments and those in our control sample. Standard errors clustered by ZIP3 in parentheses. *, **, and *** indicate statistical significance at the 10%, 5% and 1% level respectively.

Panel A: Heterogeneity by Initial Credit Score

	Subprime Credit ($S < 620$)		Near Prime Credit ($620 < S < 720$)		Prime Credit ($S > 720$)	
	(1)	(2)	(3)	(4)	(5)	(6)
treatment x post	10.08***	7.25	7.46***	12.26	13.54***	13.44***
	(3.03)	(13.18)	(2.88)	(12.17)	(1.32)	(3.73)
Fixed Effects	individual	individual	individual	individual	individual	individual
	ZIP3-year	ZIP3-year	ZIP3-year	ZIP3-year	ZIP3-year	ZIP3-year
Adj. R-squared	0.3512	0.4056	0.1654	0.2459	0.2268	0.3095
Observations	70,630	5,893	61,110	5,962	157,829	17,748

Table A.3: Long-Run Effects on Revolving Card Utilization – Restricting to Consumers with Less Than 5 Acres

Note: In Panel A, the dependent variable is percentage revolving card utilization (balance as a percentage of available revolving card limits, between 0 and 100%). In Panel B, the dependent variable is $100 \times log(1 + rev.balance)$, where the 100 x log functional form yields an approximate percentage change interpretation. The unit of observation is an individual-year in which two years are considered, 2005 (pre) and 2015 (post). The variable *treatment* is an indicator for whether the individual received royalty or bonus payments as part of our sample from the Barnett Shale between 2005 and 2015. The variable *post* is an indicator for whether the individual-year observation is in 2015. Individuals who are not treated are matched controls (propensity score matching on ZIP3, 2005 credit score, and length of credit history) drawn from the control sample from Experian. The interaction *treatment x post* captures the average difference in the change in credit scores between those receiving mineral payments and those in our control sample. Standard errors clustered by ZIP3 in parentheses. *, **, and *** indicate statistical significance at the 10%, 5% and 1% level respectively.

Panel A: Percent Revolving Card Utilization

	(1)	(2)	(3)	(4)	(5)	(6)
subsample	all	$S < 620$	$620 < S < 720$	$S > 720$	all	outside of Barnett
treatment x post	-0.34^{*}	-6.79^{***}	-3.72^{***}	2.68^{***}	-1.28^{***}	-2.81^{***}
	(0.20)	(0.76)	(0.47)	(0.10)	(0.66)	(0.75)
treatment x post x initial credit score (Z)	–	–	–	–	4.23^{***}	5.21^{***}
					(0.34)	(0.82)
post x initial credit score (Z)	–	–	–	–	3.82^{***}	3.33^{***}
					(0.32)	(0.65)
Fixed Effects	individual ZIP3-year	individual ZIP3-year	individual ZIP3-year	individual ZIP3-year	individual ZIP3-year	individual ZIP3-year
Adj. R-squared	0.5290	0.3934	0.4013	0.4098	0.5457	0.5263
Observations	1,645,989	339,490	467,156	830,381	1,645,989	183,124
Baseline Rate	25.41%	48.43%	35.43%	14.62%	25.41%	24.75%

Panel B: Logged Revolving Balances (100 x log)

	(1)	(2)	(3)	(4)	(5)	(6)
subsample	all	S < 620	$620 < S < 720$	S > 720	all	outside of Barnett
treatment x post	5.21^{***}	-20.45^{***}	-28.81^{***}	27.23^{***}	-1.19	-18.10^{***}
	(0.99)	(7.54)	(1.81)	(2.64)	(1.16)	(6.98)
treatment x post x initial credit score (Z)	–	–	–	–	29.63^{***}	50.13^{***}
					(3.79)	(7.38)
post x initial credit score (Z)	–	–	–	–	-17.11^{***}	-17.93^{***}
					(4.49)	(6.20)
Fixed Effects	individual ZIP3-year	individual ZIP3-year	individual ZIP3-year	individual ZIP3-year	individual ZIP3-year	individual ZIP3-year
Adj. R-squared	0.4674	0.3494	0.4298	0.5104	0.4683	0.4627
Observations	1,645,989	339,490	467,156	830,381	1,645,989	183,124

Table A.4: Long-Run Credit Score Effects – Heterogeneity by Initial Credit Score and Initial Income – Less Than 5 Acres

Note: The unit of observation is an individual-year in which two years are considered, 2005 (pre) and 2015 (post). In Panel A, the results are from subsamples based on initial credit score (Subprime $S < 620$, columns (1) and (2); Near prime $620 < S < 720$, columns (3) and (4); Prime $S > 720$, columns (5) and (6)). In addition, the even columns in Panel A consider only individuals who reside outside of the Barnett Shale. In Panel B, the results are from estimating the specification on individuals within each quintile of the income distribution in 2005. Panel C presents the estimate on *treatment x post* using observations within a double sort on initial income and initial credit score. The variable *treatment* is an indicator for whether the individual received royalty or bonus payments as part of our sample from the Barnett Shale between 2005 and 2015. The variable *post* is an indicator for whether the individual-year observation is in 2015. Individuals who are not treated are matched controls (propensity score matching on ZIP3, 2005 credit score, and length of credit history) drawn from the control sample from Experian. The interaction *treatment x post* captures the average difference in the change in credit scores between those receiving mineral payments and those in our control sample. Standard errors clustered by ZIP3 in parentheses. *, **, and *** indicate statistical significance at the 10%, 5% and 1% level respectively.

Panel A: Heterogeneity by Initial Credit Score

	Subprime Credit ($S < 620$)		Near Prime Credit ($620 < S < 720$)		Prime Credit ($S > 720$)	
	(1)	(2)	(3)	(4)	(5)	(6)
treatment x post	14.84***	18.42***	11.02***	14.65***	16.69***	12.48***
	(0.61)	(5.17)	(0.49)	(4.61)	(2.62)	(1.48)
Fixed Effects	individual	individual	individual	individual	individual	individual
	ZIP3-year	ZIP3-year	ZIP3-year	ZIP3-year	ZIP3-year	ZIP3-year
Adj. R-squared	0.3930	0.4646	0.2270	0.2876	0.2952	0.3615
Observations	244,448	20,849	241,469	22,320	661,424	78,165

Table A.5: Long-Run Effects on Revolving Card Utilization – First Payment Received Before 2009

Note: In Panel A, the dependent variable is percentage revolving card utilization (balance as a percentage of available revolving card limits, between 0 and 100%). In Panel B, the dependent variable is $100 \times log(1 + rev.balance)$, where the 100 x log functional form yields an approximate percentage change interpretation. The unit of observation is an individual-year in which two years are considered, 2005 (pre) and 2015 (post). The variable *treatment* is an indicator for whether the individual received royalty or bonus payments as part of our sample from the Barnett Shale between 2005 and 2015. The variable *post* is an indicator for whether the individual-year observation is in 2015. Individuals who are not treated are matched controls (propensity score matching on ZIP3, 2005 credit score, and length of credit history) drawn from the control sample from Experian. The interaction *treatment x post* captures the average difference in the change in credit scores between those receiving mineral payments and those in our control sample. Standard errors clustered by ZIP3 in parentheses. *, **, and *** indicate statistical significance at the 10%, 5% and 1% level respectively.

Panel A: Percent Revolving Card Utilization

	(1)	(2)	(3)	(4)	(5)	(6)
subsample	all	$S < 620$	$620 < S < 720$	$S > 720$	all	outside of Barnett
treatment x post	-0.32^{*}	-6.40^{***}	-3.73^{***}	2.40^{***}	-1.24^{***}	-2.44^{***}
	(0.18)	(1.24)	(0.37)	(0.17)	(0.27)	(0.87)
treatment x post x initial credit score (Z)	–	–	–	–	3.98^{***}	4.48^{***}
					(0.36)	(1.20)
post x initial credit score (Z)	–	–	–	–	3.84^{***}	3.11^{***}
					(0.48)	(0.98)
Fixed Effects	individual ZIP3-year	individual ZIP3-year	individual ZIP3-year	individual ZIP3-year	individual ZIP3-year	individual ZIP3-year
Adj. R-squared	0.5413	0.4120	0.4167	0.4185	0.4473	0.5521
Observations	1,645,989	339,490	467,156	830,381	1,645,989	183,124
Baseline Rate	25.41%	48.43%	35.43%	14.62%	25.41%	24.75%

Panel B: Logged Revolving Balances (100 x log)

	(1)	(2)	(3)	(4)	(5)	(6)
subsample	all	S < 620	$620 < S < 720$	S > 720	all	outside of Barnett
treatment x post	-0.40	-28.58^{***}	-35.39^{***}	21.41^{***}	-6.75^{***}	-17.93^{*}
	(1.78)	(11.90)	(2.88)	(3.93)	(1.98)	(9.67)
treatment x post x initial credit score (Z)	–	–	–	–	30.72^{***}	45.37^{***}
					(3.96)	(11.13)
post x initial credit score (Z)	–	–	–	–	-15.25^{***}	-16.18^{*}
					(5.99)	(9.58)
Fixed Effects	individual ZIP3-year	individual ZIP3-year	individual ZIP3-year	individual ZIP3-year	individual ZIP3-year	individual ZIP3-year
Adj. R-squared	0.4868	0.3694	0.4508	0.5104	0.4877	0.4906
Observations	1,645,989	339,490	467,156	830,381	1,645,989	183,124

Table A.6: Long-Run Credit Score Effects – Heterogeneity by Initial Credit Score and Initial Income – First Payment Received Before 2009

Note: The unit of observation is an individual-year in which two years are considered, 2005 (pre) and 2015 (post). In Panel A, the results are from subsamples based on initial credit score (Subprime $S < 620$, columns (1) and (2); Near prime $620 < S < 720$, columns (3) and (4); Prime $S > 720$, columns (5) and (6)). In addition, the even columns in Panel A consider only individuals who reside outside of the Barnett Shale. In Panel B, the results are from estimating the specification on individuals within each quintile of the income distribution in 2005. Panel C presents the estimate on *treatment x post* using observations within a double sort on initial income and initial credit score. The variable *treatment* is an indicator for whether the individual received royalty or bonus payments as part of our sample from the Barnett Shale between 2005 and 2015. The variable *post* is an indicator for whether the individual-year observation is in 2015. Individuals who are not treated are matched controls (propensity score matching on ZIP3, 2005 credit score, and length of credit history) drawn from the control sample from Experian. The interaction *treatment x post* captures the average difference in the change in credit scores between those receiving mineral payments and those in our control sample. Standard errors clustered by ZIP3 in parentheses. *, **, and *** indicate statistical significance at the 10%, 5% and 1% level respectively.

Panel A: Heterogeneity by Initial Credit Score

	Subprime Credit ($S < 620$)		Near Prime Credit ($620 < S < 720$)		Prime Credit ($S > 720$)	
	(1)	(2)	(3)	(4)	(5)	(6)
treatment x post	13.56^{***}	18.55^{***}	10.07^{***}	13.46^{***}	15.97^{***}	9.73^{***}
	(0.80)	(7.78)	(0.54)	(8.02)	(2.62)	(2.07)
Fixed Effects	individual	individual	individual	individual	individual	individual
	ZIP3-year	ZIP3-year	ZIP3-year	ZIP3-year	ZIP3-year	ZIP3-year
Adj. R-squared	0.3924	0.5397	0.2401	0.3883	0.3058	0.3825
Observations	148,926	10,652	151,499	11,863	446,779	47,609