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SHALE SHOCKED: CASH WINDFALLS AND HOUSEHOLD DEBT REPAYMENT

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ABSTRACT

How do persistent cash flow shocks affect debt repayment across the distribution of households? Using individual data on natural gas shale royalty payments matched with credit bureau data for 215,639 consumers, we estimate that individuals repay 33 cents of debt per dollar of windfall, and that initially-subprime individuals repay approximately 5 times more debt than initially-prime individuals do. This difference in debt repayment is driven by changes to revolving debt balances. Finally, we show that debt repayment precedes durable goods consumption, particularly for households who were initially financially constrained. These results shed new light on how deleveraging affects household consumption.

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1 Introduction

Household debt is a crucial determinant of macroeconomic outcomes. Indeed, when faced with an unexpected negative shock, like a recession, households with high leverage curtail their consumption the most (Mian, Rao, and Sufi (2013); Baker (2018)).[1] Theoretically, the key friction that high-leverage imposes on consumption responses is that liquidity- or financially-constrained households first have to repay debts (Eggertson and Krugman (2012)). Yet, two features of how household leverage varies in the cross-section call into question whether indebted households are truly constrained and whether such households would actually deleverage in response to shocks. Namely, household debt is greater for wealthy individuals who are presumably less financially constrained (Mason (2018)) and many wealthy households revolve substantial amounts of unsecured debt (Laibson, Repetto, and Tobacman (2003); Kaplan and Violante (2014)). To help resolve this tension, this paper provides direct estimates of the marginal propensity to repay debt (MPR) out of unanticipated cash flows, and shows how debt repayment depends on whether the household is initially constrained.

It is challenging to evaluate how cash flow shocks affect household deleveraging because cash flow shocks are difficult to disentangle from changes to other economic outcomes. In particular, the economic recovery from the Global Financial Crisis coincided with declines in household debt, falling unemployment, rising asset prices, and low interest rates. Recent papers make progress on identifying channels by using macroeconomic models to quantify the effects of financial constraints (see e.g., Korinek and Simsek (2016); Guerrieri and Lorenzoni (2017); Jones, Midrigan, and Philippon (2018)). Our paper complements this structural-modeling approach by studying a setting in which cash flow shocks are unrelated to macro factors that typically confound inference about household deleveraging.

To address the central identification challenge, we study the dynamics of debt repayment for a novel sample of individuals who receive unexpected and continuing payments from the discovery and extraction of natural gas at the onset of the Fracking Revolution. Specifically, we study how \$1.2 Billion in royalty windfalls over 11 years affect the debt repayment decisions of 215,639 individuals who own subsurface mineral rights in the Barnett Shale in Texas. The cash windfalls in our sample

[1]Not only does household debt affect consumption, but it holds back investment, weakens labor supply, and amplifies asset prices (Melzer (2017); Bernstein (2016); Favara and Imbs (2015)).

vary widely, ranging from $78 in the first percentile to $46,245 in the 99th percentile. Further, our sample of windfall recipients includes individuals who live in *every* U.S. state. The out-of-area individuals in our sample receive payments because subsurface rights are separate from surface rights (e.g., land), and are inherited through family ties.

With this uniquely-detailed information, we attribute variation in household debt repayment to the cash received by individuals, which in our setting, is distinct from changes to local conditions after the discovery of shale. Further, individuals in our sample span every credit score category, including 43,829 subprime individuals. Our sample has significant overlap with demographics of the U.S. population. Crucially, 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 dynamics of deleveraging, as well as understanding sources of heterogeneity in household deleveraging decisions.

Our main specification examines how the size of unexpected windfalls (payments/income, annualized) affects household leverage (debt payments/income). We estimate a treatment intensity difference-in-difference specification, which contrasts how household leverage differs for individuals for different payments-to-income before (2005) versus after the Fracking Revolution (2015). Using this benchmark specification, we estimate the marginal propensity to repay debt (MPR) out of the mineral rights windfalls to be 0.33. That is, on average across individuals in our sample, 33% of an additional dollar of mineral payments effectively goes toward repaying existing debts. We also estimate the analogous specification in the 2005-2015 panel, exploiting differential timing for when individuals begin receiving payments. In these dynamic specifications, we observe a smooth adjustment from no effect in the year of first payment to the long-run effect six years after receiving the first payment.[2]

We find that there is substantial heterogeneity in the MPR, depending on the initial creditworthiness of individuals receiving the shock. Specifically, we consider heterogeneity by whether an individual is initially subprime (credit score < 620) versus initially prime (credit score > 720). For subprime individuals, we estimate a MPR that is roughly 5 times the MPR for the prime sub-

[2] In addition to exploiting variation in mineral payments / income, our sample also contains a set of control individuals who do not receive cash payments. Although mineral ownership is not randomly assigned, the dynamic specifications show no differential pre-trends, which supports our identification assumption in the difference-in-difference tests.

sample. Specifically, we estimate that 77% of the wealth shock is used for debt repayment for subprime individuals, but only 14% is used for debt repayment for prime individuals. The nature of this heterogeneity is consistent with subprime individuals responding to significant debt overhang. Furthermore, under the assumption that none of the windfall is directed toward direct saving, the subprime estimate places an upper bound on subprime individuals' marginal propensity to consume (MPC) of 0.23.

Digging deeper into the difference between subprime and prime debt repayment, we examine how the cash flow shocks affect major categories of debt. Using these specifications, we attribute much of the overall difference between subprime and prime debt repayment to different effects of cash flows on revolving debt balances (e.g., credit cards). For subprime individuals, a standard deviation increase in payments (1.42 pp) leads to roughly a 5.5% reduction in revolving debt ($462 on an initial base of $8,345). By contrast, prime individuals *increase* revolving debt balances by approximately 7% (nearly $600) in response to a standard deviation increase in cash windfall. These differences are consistent with subprime individuals using the cash flow shock to repay revolving debts, whereas prime individuals use the shock mostly for additional consumption that is reflected in greater monthly credit card balances.[3]

Finally, we show how debt repayment facilitates consumption out of cash flow shocks by studying an important aspect of durable goods consumption — automobile purchases inferred from individuals' credit histories (Benmelech, Meisenzahl, and Ramcharan (2017) and Dupor et al. (2019) validate this measure of consumption). As a first step, we confirm that subprime individuals exhibit a positive and significant sensitivity of automobile consumption to cash flow shocks, whereas prime individuals do not. Then, we highlight the importance of debt repayment for this consumption response: subprime individuals repay significantly more debt than prime individuals before making their first purchase, suggesting that deleveraging prior to consumption is important for understanding the consumption response of subprime consumers. Lastly, we use data on credit inquiries to understand whether the consumption response for subprime individuals is the result

[3]The change in revolving debt balances in response to wealth shocks reflects an unknown mix between debt repayment and consumption. However, the heterogeneous response of revolving debt to the wealth shock suggests that subprime consumers forgo consumption relative to prime consumers. Given the likely negative effects on credit scores from high credit card utilization, the revolving balances of subprime individuals are more likely to reflect higher initial balances for expensive debts (i.e., individuals holding credit card balances month-to-month) than greater transactional consumption using credit cards (i.e., individuals who pay the full credit card balance each month).

of credit constraints. Credit constraints matter: subprime individuals are significantly more likely than prime individuals to have been denied credit prior to their first post-shock auto purchases, accounting for other factors. These findings imply that it is important to understand the debt response to unexpected cash flow shocks. Apart from debt overhang, which affects households beyond its consumption implications, the interaction between consumption and debt responses is important to understand, yet understudied in the broader literature.

The primary contribution of this paper is to provide novel estimates of how unanticipated cash flows affect household deleveraging. Our findings emphasize the crucial role of heterogeneity in credit access and the effect of debt repayment on the consumption response to shocks. Though we are not the first paper to study the effects of cash flow shocks on household borrowing, there are fundamental differences between our paper and prior research on debt repayment. Agarwal, Liu, and Souleles (2007) and Agarwal and Qian (2014) study debt repayment out of one-time income shocks, which are qualitatively different than the unexpected and persistent stream of royalties from shale natural gas extraction. Dynamic theories of consumption choice predict different responses depending on whether the shocks are transitory or persistent. As such, our analysis of initially unexpected and continuing cash flows provides guidance for policies that involve recurring payments, such as extended unemployment insurance or a universal basic income, while prior work is best suited to study one-time transfers, such as tax rebates or stimulus checks. Similar to our work, some papers estimate the effects of sustained shocks to wealth on household leverage, using fiscal shocks (Demyanyk, Loutskina, and Murphy (2019)) and shocks to import competition (Barrot et al. (2017)). However, these papers rely on regional aggregates, which cannot distinguish local economic effects (e.g., employment opportunities) from the effect of the shock. Our individual-level data provide two distinct advantages. First, we more cleanly estimate the elasticity of deleveraging with respect to cash payments, separate from local area confounding variation. Second, our individually-matched data enable us to evaluate individual-level heterogeneity.

Though our paper focuses primarily on debt repayment, our paper is closely linked via consumption theory to recent work studying heterogeneity in households' marginal propensity to consume out of income and wealth. Understanding the consumption response to shocks (expected or unexpected, and permanent or transitory) has important implications for understanding consumer theory and for policy-making. The canonical permanent income hypothesis suggests that consumption should be proportional to disposable resources and that all consumers respond similarly to shocks (Jappelli and Pistaferri (2010)). However, numerous empirical studies document that consumers consistently deviate from the predictions of theory. To reconcile these findings, papers have argued for the role of credit and liquidity constraints (e.g., Zeldes (1989); Baker (2018)) and a litany of behavioral rules (e.g., Campbell and Mankiw (1989); Kueng (2018)). When applied to policy, recent papers estimate the consumption response over several years to government spending (Dupor et al. (2019)), tax rebates (Sahm, Shapiro, and Slemrod (2015); Baugh et al. (2018)), housing variables (e.g., Berger et al. (2017); Ganong and Noel (2018)), and employment (e.g., Ganong and Noel (2019)). This literature argues that household balance sheets are an important source of heterogeneity in the observed consumption responses. However, the majority of this literature tends to treat the household balance sheet as exogenous. Relative to these works, our paper finds that household balance sheets respond dynamically to cash windfalls, suggesting that debt repayment decisions and household leverage are important outcomes unto themselves.

In recognizing the importance of household debt, we relate to an emerging literature on how household debt responds to a variety of shocks to household budget constraints, as well as to other variables that affect household decisions. For example, previous work has examined factors that affect household liquidity, such as resets to adjustable rate mortgages (Fuster and Willen (2017); Di Maggio et al. (2017)) and government policies targeted to mortgage owners (Scharlemann and Shore (2016); Abel and Fuster (2020)). In a complementary vein, a separate line of work considers how demand-side factors affect household leverage, such as beliefs, personal experiences, and social networks (Bailey et al. (2019); Brown, Cookson, and Heimer (2019); Kalda (2020)). Other papers study precisely how households develop behavioral rules for debt repayment (Gathergood et al. (2019); Argyle, Nadauld, and Palmer (2020)). We contribute to this literature by showing the debt repayment effects of a pure expansion to the household budget constraint in the form of cash transfers, as opposed to other shocks to household finances that may require individuals to be financially sophisticated (e.g., mortgage modifications) or have questions about external validity (e.g., lottery winners). In light of prior research on large wealth shocks, which has found little evidence of improvements to financial decisions (Hankins, Hoekstra, and Skiba (2011); Briggs et al. (2020)), our finding that households improve their household balance sheet by repaying debt out of their shale cash windfalls is not obvious, ex ante.

Finally, our paper 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, Loutskina, and Strahan (2016); Gilje (2019)), and changes in house prices (Muehlenbachs, Spiller, and Timmins (2017)). Our work is the first to use individual level data on cash payments from shale extraction to trace out the effects of shale development on household outcomes.[4] More closely related to our study, Brown (2018) examines the local effects of the oil and gas boom in the U.S. on consumer debt accumulation, while Haughwout et al. (2016) and Cunningham, Gerardi, and Shen (2017) study the effects on financial distress. Some of these studies use the FRBNY - CCP/Equifax panel data set to measure credit outcomes. However, these studies rely on local aggregates of drilling intensity, rather than trace out the effects of royalty payments to individuals. As such, our paper is in a unique position to evaluate the role of ex-ante heterogeneity in households' balance sheets and consider the effects on non-local beneficiaries of the Shale boom.

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. Royalty percentages

[4] Bellon et al. (2020) estimate the impact of large cash windfalls from shale extraction on self-employment decisions using the same data on individual-level cash windfalls as in this paper. Bellon et al. (2020)'s identification contrasts large windfall recipients (>$50,000) with others. By contrast, this paper explicitly focuses on the deleveraging decisions out of moderately-sized cash payments (i.e., less than annual income), which more closely resemble the size of the fiscal policy shocks considered in macro models.

in our sample 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. The data requires substantial effort to translate into a format conducive to analysis. In our study, we focused on compiling mineral appraisal roll data for the four main producing counties in the Barnett Shale going back to the year 2000.

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

In our sample, royalty payments from production account for 60% of total payments. The remaining payments are the bonus payments that mineral owners received at the time a lease was signed. To compute bonus payments, we conducted public record requests for all oil and gas leases from the four counties in our study, as well as county indexes. The lease bonus payment in many cases is not reported on a lease because it is not required to be. However, many leases do have this information, as well as net acreage amounts. Based on the leases that do have lease bonus information we estimate a regression which that predicts 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 to the U.S. Energy Information administration, shale gas production was less than 1% of total U.S. natural gas production in the year 2000, but by 2015 accounted for 46.2% of total

U.S. gas production. Moreover, the Barnett shale was the first, 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 of Johnson county 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, shale development was unexpected by the industry, and even less expected by households in our study. Indeed, Chevron CEO John Watson was famously quoted as saying "'fracking' took the industry by surprise" (2011 WSJ). Accordingly, mineral ownership in the Barnett Shale represented a deep out of the money option, which had minimal value until there was a technological breakthrough. For those fortunate to own minerals, which typically occurred through family ancestry, the shale breakthrough led to the deep out of the money option becoming a vary valuable cash flow stream when natural gas was drilled. Therefore, although people who own minerals are certainly different than the average credit profile in the United States, the shock they experience "within" person was due to an exogenous technological breakthrough over which they had little control.

2.3 Royalty Owners versus 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 it is not. Mineral rights are frequently severed from surface rights, and held by individuals whose family lived in the area generations ago. 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. This is especially true in areas with shale because buyers would not have expected development and would therefore 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, 10% of the royalty payments in our sample are received by people who 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 the mineral rights. Further, the variation we use across most of our empirical tests is not the extensive margin of owning mineral rights, but the intensive margin of how large the payments are. That is, conditioning on individuals with the same mineral acreage ownership, we examine the impact of receiving large payments versus small payments, which is driven entirely by the timing of drilling and the intensity of production, factors external to the households we study.

Beyond these desirable features of the variation in mineral payments, the sample of mineral rights owners – though different than the U.S. population on average – has significant overlap with the overall U.S. distribution of borrowers. Accordingly, our findings have a stronger claim for external validity than analyses that rely on very particular shocks. To highlight this overlap, we 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 overall sample has 43,829 people who are subprime. Our sample has a significant number of observations in each credit category.

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.[5] 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

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representative sample. The merge with credit bureau data returned an 80 percent hit rate, leaving us with approximately 400,000 consumers who received mineral rights payments. Each of our control samples has approximately 300,000 individuals, leaving us with approximately 1.1 million credit histories.

For each individual in our sample, we observe an annual snapshot of credit bureau characteristics (credit score, estimated personal incomes modeled using actual W2 statements from the IRS, an internal debt-to-income measure, plus 250 credit attributes). Our primary outcome variable is the omnibus debt-to-income ratio from Experian, which reports the percentage of an individual's annual debt obligations relative to annual income. By scaling debt *payments* to income, rather than overall debt levels to income, this measure captures the percentage of income devoted to the individual's debt payments, and is similar to the metrics of debt burden used in screening credit applications. In other tests, we also evaluate the impact of shale wealth shocks on specific categories of debt, i.e., the total amounts of revolving credit, auto loan balances and mortgage debt, as well as measures of the financial risk on consumer balance sheets (e.g., delinquencies, bankruptcies and credit scores). For simplicity of presentation, the main tests restrict attention to two snapshots of the data – year 2005 and year 2015 – but we also use the annual snapshots to examine the dynamics of how the short run effects translate into longer run effects on household debt.

2.5 Sample Summary

From the full sample of data, we make a number of sampling restrictions to standardize the sample characteristics and measurement of the key variables. Starting from a sample with 404,937 individuals in our mineral payment sample, we restrict attention to individuals (and matches) for whom we have valid data for the entire 11-year panel. We also restrict attention to treated individuals for whom we observe their bonus payment to ensure we reliably identify the first date at which the individual begins receiving payments. Beyond these broad choices, our final analysis sample focuses on the sample of individuals who receive moderate payments (< 100% of 2005 income).

These sampling restrictions leave us with a final sample of 215,639 treated individuals (matched with 215,639 control individuals). Panel (c) of Table 1 reports the number of observations dropped by each sampling restriction. The sample size is reduced the most by choices that ensure the payments and their timing are measured accurately (i.e., the observe bonus payment filter and the restriction on mineral acreage owned).

Table 1, Panel (a) reports detailed summary statistics on the distribution of payments in the sample and credit characteristics of the individuals in the sample with valid data on mineral acreage owned. We note that total mineral payments and mineral payments as a percentage of income are both right skewed. For example, although the median payment-to-income percentage is 5.7%, the mean payment-to-income percentage is 11.2%.

Table 1, Panel (b) reports summary statistics on credit characteristics from the Experian data. The main dependent variable, debt payments-to-income (DTI)(%), has a relatively symmetric distribution, with a median DTI of 13.0% and a mean DTI of 15.6%. Consistent with broader samples of credit bureau data, the distribution of credit scores in our data is left skewed. However, consistent with our contextual description, the individuals in our sample have credit scores that are slightly above national averages, with a mean of 718 and a median of 739.0.

To provide context for our main tests, it is useful to contrast mineral rights payments recipients with those in our control sample. To construct a useful sample of control individuals, 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 we restrict matched controls to be individuals who live in the same three digit zip as the mineral owner.

Table 2 reports how recipients of cash windfalls compare to control individuals who do not own mineral rights. The central identification assumption of empirical tests is that in the absence of receiving cash windfalls from mineral ownership treatment and control individuals would have had outcomes (e.g., DTI or revolving balances) that would have trended similarly. Tests later in the paper provide evidence supporting the parallel trends assumption.

In addition to having parallel trends, Table 2 presents summary statistics prior to treatment (year 2005) for individual characteristics related to debt (i.e., DTI, revolving balances, credit scores, and the share of subprime individuals) and other personal characteristics (i.e., age, income and amount of mineral acreage owned), separately for mineral windfall recipients and control individuals. For the outcomes of interest, there is substantial overlap in the distributions of mineral windfall recipients and control individuals. For example, the interquartile range of DTI for mineral payment recipients is from 8% to 29%, whereas it ranges from 2% to 24% for our control sample. Comparing the means of the two samples, we observe some initial differences in the observed characteristics between our sample of cash windfall recipients and the control sample.[6] Indeed, the fixed effects we employ in our empirical tests reduce the possibility that level differences in treatment and control groups affect the magnitudes of the estimates we present in the subsequent analysis, in particular the use of individual fixed effects subsumes any remaining individual level differences. Ultimately, our empirical design relies on the absence of differential pre-trends for mineral owners versus our sample of non-mineral owners, not random assignment of mineral ownership. This approach is widely used in the literature (e.g., Yagan (2015)). In our setting, this approach is well supported. We show that after adjusting for fixed effects, and plotting our key outcomes in event time (see section 4.1) the empirical evidence is consistent with parallel trends.

3 Conceptual Framework

Our focus in this study is to estimate the marginal propensity to repay debt (MPR) and understand its heterogeneity across individuals. The MPR is the amount of debt repaid per additional dollar of wealth received by the household.

Household debt tends to be viewed within the context of canonical theories of intertemporal consumption choice. In this framework, individuals have preferences over initial and future consumption given by

$$u_i(c_{0i}) + \delta \cdot E_i[u(c_{1i})] \tag{1}$$

where c_{0i} is consumption at time 0, the discount factor δ is less than 1, and E_i is the expectations of individual i. The individual's utility function $u_i(\cdot)$ is an increasing and concave function. Household

[6] In the appendix Table A.1, we present a more formal balancing analysis. We observe that the *raw* differences in these individual characteristics that are statistically significant, but the large size of our sample is a contributing factor to the statistical significance. However, most of these differences become statistically insignificant upon including the set of fixed effects that are included in our subsequent regression analysis. More important than the statistical differences, Table A.1 also shows that the differences in these individual characteristics are small in economic magnitude. For example, the largest differences are in DTI and revolving credit, which are just 2.7% and 1.6% of a standard deviation larger for the treatment group relative to the control group.

debt enters into the consumer's decision-making as part of the intertemporal budget constraint:

$$c_{0i} = y_{0i} - Rb_{0i} + b_{1i}. \tag{2}$$

The individual's initial wealth is y_{0i} and she chooses c_{0i} in order to maximize her lifetime utility. In this formulation, period 0 debt outstanding b_{0i} is a state variable. Debt can be rolled over to subsequent periods at cost R. Subsequent period debts b_{1i} are used to finance consumption in $t = 1$ and beyond. In the model, debt enters into the individuals' decision-making only as a means to facilitate future consumption.

The literature uses this framework to develop predictions about the impulse response of consumption to anticipated and unanticipated cash flows. For simplicity of presentation, consider the following general estimating equation:

$$\triangle log\, c_{it} = \lambda_i + \beta_{1,1} \triangle log\, y_{it} + \varepsilon_{1,it} \tag{3}$$

where λ_i is an individual specific fixed effect that would include factors such as unobserved preferences and ε is the usual error term. The coefficient $\beta_{1,1}$ is the consumption response to cash flow changes, which is determined by structural parameters that are unobservable to the empiricist, particularly beliefs about the persistence of the shock.

The predictions on $\beta_{1,1}$ are well-known and they are summarized by Jappelli and Pistaferri (2010) and Mian, Rao, and Sufi (2013). First, known as the permanent income hypothesis, *predictable* changes in cash flows should have no effect on current consumption. The best predictor of current consumption is past consumption.

However, in our setting, the cash windfalls from shale natural gas were unforeseen, which leads to a more nuanced set of predictions, which we describe as follows. As Mian, Rao, and Sufi (2013) outline, under the strong assumption of complete markets, individuals hedge against cash flows shocks and $\beta_{1,1}$ is expected to be close to zero. On the other hand, when consumers lack access to credit or have limited borrowing capacity there will be cross-sectional variation in $\beta_{1,1}$ reflecting individuals' ability to smooth shocks.[7] As such, the aggregate consumption response to

[7]As a technical note, Mian, Rao, and Sufi (2013) describes how precautionary savings motives (e.g., Carroll and Kimball (1996)) and liquidity constraints (e.g., Bernanke and Gertler (1989); Kiyotaki and Moore (1997)) can generate

cash flows will vary depending on the distribution of the shocks. Recent work finds support for this prediction. Mian, Rao, and Sufi (2013) document heterogeneous consumption responses to the reduction in housing wealth during the Great Recession, with housing wealth and variation in leverage as proxies for financial constraints. Baker (2018) uses ratios of household debt to assets (or income) for variation in financial constraints and shows that it explains the cross-sectional variation in MPC to negative income shocks.

These theoretical predictions and studies of heterogeneous MPCs caused by financial constraints serve as a springboard for our analysis. Connecting these studies back to theory, the cross-sectional variation in $\beta_{1,1}$ from equation 3 is determined by variation in initial debt b_0, a state variable in the households' intertemporal budget constraint. Related to this implication, there is an important unexplored prediction of the financial constraints view: household debt will be repaid out of positive cash flow shocks, particularly for households who are initially constrained and seek to unwind these financial constraints. This prediction can be captured in the following general estimating equation:

$$\triangle log\, b_{it} = \lambda_i + \beta_{2,1} \triangle log\, y_{it} + \varepsilon_{2,it} \tag{4}$$

in which $\beta_{2,1}$ is the MPR that our tests explicitly estimate.

Despite the clear theoretical predictions, the extent to which individuals would actually repay debts in response to cash flow shocks is less clear because the prediction rests on the assumption that household debt constitutes a financial constraint. Indeed, several facts about the cross-section of household debt and household borrowing behaviors cast doubt on this assumption. First, household leverage is greater for individuals who are unlikely to have difficulty accessing credit markets. Using data from the Survey of Consumer Finances, Mason (2018) shows that the bottom 20% of the income distribution holds almost no debt at all, while the top 20% holds the most debt. As such, levels of household leverage and debt to income ratios do not purely reflect a household's degree of financial constraint. Second, there is substantial evidence that unconstrained households carry expensive debts that are difficult to explain using standard theory. In particular, Laibson, Repetto, and Tobacman (2003) and Gross and Souleles (2002) document a "debt puzzle" in which a substan-

similar predictions. In particular, low net worth households would have a larger consumption response under the precautionary savings motive, but low net worth also implies greater financial constraints (e.g., insufficient collateral) and such constrained households would also have a larger consumption response to shocks.

tial fraction of unconstrained households carry revolving balances on their credit cards. Laibson, Repetto, and Tobacman (2003) explains this behavior using a consumption model with impatient consumers. Such consumers would, instead of paying down debts, use unanticipated cash flows to increase near-term consumption, a finding supported by Olafsson and Pagel (2019) who estimates an MPC greater than one. In a similar vein, Kaplan and Violante (2014) point to many households appearing liquidity constrained because they hold substantial illiquid assets. Such households are not credit constrained in the traditional sense because illiquid assets are frequently used as collateral. Third, there is evidence that individuals use heuristics to determine how much debt they should repay (see e.g., Gathergood et al. (2019); Argyle, Nadauld, and Palmer (2020)). This finding suggests that households may have target debt levels and therefore b_0 represents the individuals' preferences towards debt.

Given the above considerations, our goal is to test the unexplored prediction of the financial constraints view that households deleverage in response to unexpected positive cash flow shocks. As such, the following presents an organizational framework and road map for our empirical tests:

1. We estimate heterogeneity in the MPR (equation 4) while sorting individuals by a *direct* measure of financial constraints: individuals' credit scores. To the extent that individuals use unanticipated cash windfalls to repay burdensome debts and ease financial constraints, we would expect to find that a negative MPR. The magnitude of the MPR response will vary by whether or not people have good (prime) or bad (subprime) credit scores.

2. Next, we document the implications of debt repayment by estimating cross-sectional variation in MPC (equation 3) also based on individuals' credit scores. In accordance with theory, and like Mian, Rao, and Sufi (2013) and Baker (2018), we expect to find a greater elasticity of consumption out of cash flow shocks for constrained individuals. We build upon this work by testing how the consumption response parallels the debt response, with such tests precisely capturing their interdependence and joint dynamics.

3. We conclude with tests of how the dynamics of the consumption response depend on the binding nature of credit constraints when individuals carry burdensome debts. Completely unlike prior studies, our data allow us to test which individuals have been denied credit.

> We use our data to show that the consumption dynamics are intertwined with individuals' successes and failures in obtaining credit.

The following section proceeds to our tests.

4 Main Results

In this section, we provide evidence on how wealth shocks from mineral payments affect household leverage. In addition to estimating the marginal propensity to repay debt (MPR), the tests in this section also provide evidence on the heterogeneity in the MPR – both in terms of initial creditworthiness and the size of the wealth shock.

4.1 Household Leverage

As a first step towards estimating the effect of wealth windfalls on debt repayment, we estimate a coarse specification, estimated separately for subsamples of individuals who received moderate payments (<100% of annual income in 2005) versus large payments (>100% of annual income in 2005). Specifically we estimate the following regression:

$$DTI_{it} = \gamma_i + \gamma_{zt} + \gamma_{at} + \gamma_{kt} + \beta_1 Treatment_i \times post_t + \mathbf{X}'\eta + \varepsilon_{it}, \tag{5}$$

where DTI_{it} is annual debt payments as a percentage of income for individual i in year t. The key term of interest is the difference-in-difference term $Treatment_i \times post_t$. The variable $Treatment_i$, is a dummy for whether an individual received mineral payments ($Treatment_i = 0$ for propensity-matched control individuals), and $post_t$ is a dummy for whether the observation occurs after the Fracking Revolution (2015). The specification is a long difference that compares individuals' DTI in 2015 to their DTI in 2005. To understand the role of heterogeneity, we also separately estimate this specification on subsamples based on the initial credit scores of individuals (i.e., subprime versus prime).

Table 3 reports the results of estimating equation (5). For the full sample of initial credit scores (columns 1 and 2), the coefficient estimate of interest, β_1, equals approximately -1.55. Economically, this means that individuals that receive moderate (or large) payments have a reduction of 1.55 of their DTI relative to control individuals, which is approximately 9% of the sample average DTI and 11% of the sample standard deviation for DTI. We see stark differences in debt repayment based on initial credit. Initially-subprime individuals (columns 3 and 4) reduce their DTI by approximately ten times as much as initially-prime individuals (columns 5 and 6). These differences in debt repayment by initial credit status are a first-order feature of our setting, and thus, we consider heterogeneity by initial credit status in all subsequent tests.

Importantly, the estimated amount of debt repayment from Table 3 is similar for individuals who receive large payments (> 100% of annual income) to individuals who receive smaller payments (< 100% of annual income). The odd numbered specifications restrict the sample to individuals who receive moderate payments (up to 100% of annual income), while the even specifications include only individuals who receive large payments (greater than 100% of annual income). For the full sample, as well as the split samples of subprime and prime initial credit, we obtain similar coefficient estimates for large and moderate payments. These similar estimates imply that additional unexpected wealth (greater than 100% of annual income) does not lead to additional debt repayment.[8] As such, we focus the remainder of our tests on estimating the elasticity of wealth on DTI for moderate-sized windfall payments up to 100% of annual income in 2005. Further, this restriction ensures that our estimated elasticities are more relevant to policy parameters, as the moderate payment sizes in our sample are more similar to the scope of typical fiscal policy interventions than the very large windfalls in the right tail.

To identify the MPR from this set of unexpected mineral payments (<100% of annual income), we estimate the following treatment intensity difference-in-difference specification:

$$DTI_{it} = \gamma_i + \gamma_{zt} + \gamma_{at} + \gamma_{kt} + \beta_1 Avg\ PTI_i \times post_t + \mathbf{X}'\eta + \varepsilon_{it}, \tag{6}$$

[8]To further support the choice of trimming the sample at 100% of annual income, we perform a non-parametric analysis (specifically, we estimate a generalized additive model, GAM) in Appendix B in which we allow the estimated elasticity of debt repayment to amount of debt repayment (in specification (6)) to depend flexibly on the size of the payment relative to income. In this more flexible model, we find a diminishing marginal propensity to repay debt that is effectively flat after 100% of annual income.

where DTI_{it} is annual debt payments as a percentage of income for individual i in year t. The independent variable $Avg\ PTI_i$ is the annualized amount of payments received (between 2005 and 2015) as a percentage of income for individual i. The *post* variable is an indicator for the post period (equal to one for year 2015, and equal to zero for year 2005). The coefficient of interest is β_1, which measures the impact of a one percentage point increase in annual mineral payments relative to income ($Avg\ PTI_i$) on the percentage of debt payments relative to income. Because both variables are scaled by income and are annual measures, $-\beta_1$ measures the marginal propensity to repay debt (i.e., the fraction of an additional dollar of mineral payments that is used to pay down debt). To allow for local correlation in errors as well as serial correlation over time, we cluster standard errors by ZIP3.

Our preferred estimates of MPR come from our most restrictive specification in which we include fixed effects that account for background determinants of household leverage. All specifications include individual fixed effects γ_i. In addition, this specification includes ZIP3-year fixed effects (γ_{zt}) to allow for differential trends in economic conditions across locations, and also includes age quintile by year and income quintile by year fixed effects (γ_{at} and γ_{kt}, respectively) to account for different life-cycle and income profile effects on household leverage. In addition, our preferred estimates are based on a sample of mineral payment recipients and matched controls who are precisely matched (i.e., we retain observations in which the propensity-score matched individual has a credit score within 100 points of the treatment individual). We will also present robustness to these specification choices.

Panel (a) of Table 4 presents our preferred estimates of the MPR. We separately estimate the MPR for subsamples split by treatment individuals' initial credit scores (i.e., their credit scores in 2005). For the full sample of initial credit scores (columns 1 and 2), we estimate the MPR to be approximately 0.3. The coefficient estimate equals 0.29 in the specification with individual fixed effects and ZIP3-year fixed effects and it increases to 0.33 when we include the age and income fixed effects. In context, this estimate implies that approximately 30% of each additional dollar from mineral payments is used by individuals to repay debt. Under the assumption that saving and labor are unaffected, we can attribute the remaining share of wealth to changes in consumption. This estimate implies a MPC of approximately 0.70, which is comparable to the short run estimate of 0.80 from Agarwal and Qian (2014).

In columns (3) through (6), we examine heterogeneity in the effect of cash flow shocks by initial creditworthiness. We find striking differences in the MPR based on the initial state of the household's balance sheet. In columns (3) and (4), we estimate that initially-subprime individuals have a MPR that is approximately 0.7, which implies that 70% of every dollar of unexpected cash flows goes to repaying debt. By contrast, our estimates from columns (5) and (6) imply that the MPR for prime-credit individuals is approximately equal to 0.1. These results show that consumers with weaker financial health use additional cash flows to shore up their balance sheets, whereas those with good financial health can use additional cash flow for other purposes.

We complement these long run difference-in-difference estimates with evidence on the dynamics of debt repayment in the yearly panel data set. Specifically, we estimate leads and lags of the effect of $Avg\ PTI_i$ on DTI_{it} around the year of first payment. On average, roughly 40% of the total windfall is received in the year of first payment, with the remaining payments accumulating over the subsequent six years (see Figure 6). In addition, the date of the first windfall is when households learn that they will receive the full stream of payments. This payment profile is similar for initially-prime and initially-subprime individuals. Figure 7 presents our estimates of the dynamics of debt repayment visually, with approximate 95% confidence bands. Apart from the different long-run effects by initially-subprime and initially-prime consumers, the graph indicates a smooth transition from no effect to the full long-term effect roughly five years after the first payment is received. Moreover, the dynamics are not indicative of a short-run overreaction and subsequent reversion, nor do they indicate any problematic pre-trends in the effect before payments have been issued.

Next, we examine the robustness of our estimates to our assumptions about the sample restrictions. Table 4, Panel (b) reports the same set of specifications as Panel (a), but employs the full sample of treatment-control matches, rather than just those precisely matched on initial credit score. We find that the estimated MPR is stable and not statistically different from the estimates on the more restrictive sample – the estimated MPR is 0.33 in column (1) and 0.37 in column (2). Also, consistent with our preferred sample frame, initially-subprime households display a significantly greater MPR than initially-prime households. Examining the heterogeneity based on initial credit, the MPR for initially-subprime individuals increases slightly to above 0.8 (columns 3 and 4). However, the increase is not necessarily statistically meaningful as the smaller sample size of the initially-subprime group comes with larger standard errors on the coefficient estimates. For the initially-prime individuals (columns 5 and 6), the MPR estimates on the full sample are not statistically different from the restricted sample estimates.

At the local level, a potential explanation for the relation between wealth windfalls received and debt repayment is that the windfalls reflect an improvement to the local area economy. Our main specifications account for general improvements to the local area economy by identifying the MPR from the difference between mineral recipients and control individuals and including ZIP3 by year fixed effects. However, as an alternative technique to evaluate this explanation, Table 5 compares estimates using the full set of individuals' locations (odd numbered columns) to a restricted sample of individuals who reside outside of the Barnett Shale exploration area (even numbered columns).[9]

The estimates of MPR (columns 1 and 2) are similar when we restrict to the out-of-area sample, which drops more than 90% of the full sample's observations. Further, our finding of greater MPR among initially-subprime households than the initially-prime households (columns 3 through 6) remains robust and significant in the out-of-area subsample. In fact, a one standard error bound around the estimated elasticities on the out-of-area sample contains the estimated full sample elasticity for all three estimated MPRs. These specifications employ individual fixed effects, but not the time-varying fixed effects from our main specification in Table 4, Panel (a). We do not include time-varying fixed effects in these tests. These fixed effects leave little variation to precisely estimate a MPR because the out-of-area sample is widely spread across the country.[10] The fact that we obtain our core results on the out-of-area subsample gives us added confidence that the estimated MPR reflects household leverage choices in response to receiving a cash flow windfall rather than the result of changing economic conditions in geographic areas that experience drilling.

4.2 Types of Debts

Using data on balances from different categories of debt from Experian, this section sheds additional insight into the composition of debt repayment across different debt types (i.e., revolving, auto and mortgage credit). To understand the categories of debt that are most important for the heterogene-

[9]The Barnett Shale exploration area consists of Denton, Tarrant, Johnson and Wise counties in Texas. Individuals residing outside of this exploration area comprise our "out of area" sample. In addition to the sample of out-of-area mineral payment recipients, our control sample from Experian was constructed specifically to include individuals from these out of area locations to serve as controls.

[10]Notably, in areas with few recipients, the time-varying fixed effects – e.g., ZIP3 by year FE – closely resemble individual by year fixed effects, which would leave our specification tenuously identified. In practice, these fixed effects do not much affect the estimated magnitudes, but their inclusion dramatically increases the standard errors.

ity in MPR we documented in the previous section, we estimate the following treatment intensity difference-in-difference specification:

$$\log(1+balances_{it}) = \gamma_i + \gamma_{zt} + \gamma_{at} + \gamma_{kt} + \beta_1 Avg\ PTI_i \times post_t + \mathbf{X}'\eta + \varepsilon_{it}, \tag{7}$$

where the dependent variable is log transformed balances from either revolving, auto loan, or mortgage debt categories for individual i in year t. The independent variable $Avg\ PTI_i$ is the annualized amount of payments received (between 2005 and 2015) as a percentage of income for individual i. The *post* variable is an indicator for the post period (=1 for year 2015, =0 for year 2005), γ_i are individual fixed effects, γ_{zt} are ZIP3 x year fixed effects, γ_{at} are age quintile by year fixed effects, and γ_{kt} are income quintile by year fixed effects. The specification also controls for the mineral acreage owned by individual i. To allow for local correlation in errors, as well as serial correlation over time, we cluster standard errors by ZIP3.

As in the main specification, the coefficient of interest in this specification is β_1, which approximately equals the percentage impact on balances of a one percentage point increase in annual mineral payments relative to income ($Avg\ PTI_i$). We use the transformation $\log(1+balances_{it})$ to account for zeros in the balances of some credit categories. As such, β_1 can be interpreted as approximately a percentage change increase in $balances_{it}$. In our sample, there are not many zeros and the scale of the variable is typically far from zero. Thus, alternative solutions to the problem of log transforming are likely to give quantitatively similar estimates.

Table 6 presents the results from estimating equation (7), separately for subprime individuals and prime individuals for revolving credit, auto loans, and mortgages. Across these credit categories, the most striking difference between subprime and prime repayment of debts is that subprime individuals reduce their revolving debts, whereas prime individuals increase their revolving debts. To interpret the coefficient estimate for the subprime subsample, a one percentage point increase in average payments-to-income leads subprime individuals to reduce their revolving balances by approximately 3.9%. As the average revolving balance among subprime individuals is \$8,345, this represents a significant reduction in debt: a standard deviation increase in average payments-to-income (1.42 pp) implies that subprime individuals repay \$462 of their revolving debts.[11] By contrast, initially-prime individuals increase their revolving debt by 5.1% per one percentage point increase in payments-to-income (an increase of approximately \$580 for a standard deviation increase in average payments-to-income). When we examine other categories of credit, we note that the changes to auto loan and mortgage balances for both subprime and prime samples are insignificant, statistically and economically.

Figure 8 presents a dynamic plot of how revolving credit changes around the year of first payment. As in the analogous debt-to-income plot, the dynamic transition to the long-term effect is smooth and does not appear to revert. Although there is some indication of a pre-trend for the initially-prime sample, the initially-prime and initially-subprime samples exhibit similar patterns in the years before receiving mineral payments. Thus, the main conclusion from this analysis – that subprime repay revolving debts while prime increase them – does not stem from changes to revolving credit or trends that occur prior to the year in which individuals start receiving mineral payments.

5 Debt Repayment and Auto Consumption

This section evaluates the implications of debt repayment by showing how it affects the consumption response to unexpected wealth shocks. Existing literature has tied household debt levels to consumption by households both at aggregated levels (Mian, Rao, and Sufi (2013)) and at the individual level (Baker (2018)). Broadly, this literature shows that individuals with higher debt burdens exhibit a greater sensitivity of consumption with respect to income. Tests of such an effect tend to be conducted by estimating cross-sectional variation in the marginal propensity to consume for individuals that vary by their debt levels at the time they experience the shock. However, this literature leaves open lingering questions about the dynamics of repayment and how that tracks alongside the consumption response. In particular, individuals may want to pay off all of their existing debts before consuming or they may prefer to retain some debts using the unexpected cash flow as a source of liquidity. This section illustrates how the dynamics of debt and consumption operate jointly. The

[11] Summary statistics by initial debt category (subprime versus prime) are reported in Appendix Table A.2.

joint response illustrates precisely how debt acts as a financial constraint impeding the consumption response to unexpected cash flow shocks.

To measure a consumption response, we first develop a reliable measure of consumption from the credit bureau data. Prior work has often relied on revolving balances (from bank issued cards) to measure consumption levels (Di Maggio et al. (2017)). However, a change in revolving balances can capture a mixture of consumption responses and increased debts (i.e., rollovers) that individuals are unable to repay, and even with the most comprehensive data on income, consumption is difficult to measure accurately (Baker et al. (2018)). Given these challenges, we turn to a measure of durable goods consumption from the credit bureau data that has recently been established in the literature: car purchases. We proxy for an individual having purchased a new car by identifying increases in year-over-year automobile loan balances. Two prior papers, Benmelech, Meisenzahl, and Ramcharan (2017) and Dupor et al. (2019), have validated this measure by comparing automobile obligations in the credit bureau data to new vehicle registrations. We confirm the quality of our measure by showing that our estimate of the time series of new vehicle purchases is visually identical to that in Dupor et al. (2019) (see Appendix Figure A.3). Additionally, our focus on automobile consumption is appealing because it centers on the largest single consumption good that most households would purchase (aside from a home purchase) and because automobile consumption is a significant component of the aggregate cyclicality of consumption (see e.g., Benmelech, Meisenzahl, and Ramcharan (2017)).

We begin by estimating the following cross sectional treatment intensity specification:

$$Auto_i = \gamma_z + \gamma_a + \gamma_k + \beta_1 Avg\ PTI_i + \varepsilon_i, \tag{8}$$

where $Auto_i$ is an indicator equal to one if individual i's auto loan balance increased year-over-year at any point between 2005 and 2015.[12] The independent variable $Avg\ PTI_i$ is the annualized amount of payments received (between 2005 and 2015) as a percentage of income for individual i. γ_z are ZIP3 by year fixed effects, γ_a are age cohort by year fixed effects, and γ_k are income quintile by

[12]Given the repayment schedules for the typical auto loan, it is a strong indication of making a car purchase during a year if an individual's outstanding auto loan balance increases from one year to the next. For each year in our panel, we identify year-over-year increases in auto balances as automobile purchases, and our primary specification is an indicator for whether the individual has at least one of these identified auto purchases between 2005 and 2015.

year fixed effects. The specification controls for a vector of control variables, including the mineral acreage owned by individual i. To allow for local correlation in errors, we cluster standard errors by ZIP3.

Consistent with the literature's finding of excess sensitivity of consumption by indebted households, we find significant heterogeneity by initial credit status in the auto consumption response to mineral payments – subprime individuals exhibit much greater sensitivity than prime individuals (Table 7). According to the coefficient estimates, a one standard deviation increase in average PTI results in an 11.5% and 17.3% increase in the likelihood of purchasing a car for subprime individuals (columns 3 and 4). There is no statistically significant effect for prime individuals (columns 5 and 6).

To assess how this consumption result relates to debt repayment, we undertake two additional exercises. First, we examine cohorts of individuals whose first car purchase after beginning to receive mineral payments occurs in the years $\{1, 2, ..., 6\}$. For each first-time car purchaser, we compute how much debt has been repaid (in terms of DTI) as of the time of purchase. Figure 9 summarizes how this cumulative debt repayment varies by car purchase cohort and initial credit score category. We find that subprime individuals repay more debt than prime individuals at the time of the first auto purchase, especially for individuals who wait several years after receiving their first mineral payment to make a car purchase. This result shows how subprime individuals engage in a slow process of deleveraging, reducing debt overhang, before subsequently increasing consumption by purchasing a new automobile.

Second, in Figure 10, we demonstrate how the trajectory of first auto purchases is shaped by the credit status of individuals. The figure plots the fraction of new automobile purchases made by initially-subprime individuals. Similar to the debt repayment figure, it plots this fraction by each year relative to the year that individuals start receiving royalty payments. The figure shows that initially-subprime individuals increasingly comprise a larger share of new automobile purchases, from two percentage points more in years 2 to 5, to over three percentage points more in year 6 after mineral payments begin.

We take our analysis a step further and trace out how credit access changes in response to royalty payments in the lead up to new auto purchases. Figure 11 examines how access to credit changes in the years after royalty owners begin receiving payment. We measure credit access by taking the number of new credit lines divided by hard credit inquiries over the past year. This measure of credit access has been validated by prior studies—it correlates with known measures of credit supply in both the time series and cross-section (see e.g., Bhutta and Keys (2016); Akey, Heimer, and Lewellen (2020)). A lower value for this variable means that a consumer has been rejected from credit lines that they applied for. Two striking patterns emerge. First, subprime consumers are more likely to have been denied credit prior to an auto purchase relative to prime borrowers (hence the subprime line is below the prime line). Second, subprime borrowers who wait several years to purchase a car tend to have been declined more prior to the purchase (the coefficient for subprime at year 6 is below the coefficient at year 0 or year 1). This is consistent with the deleveraging mechanism that motivates our work: subprime owners wait longer to purchase a vehicle after receiving a payment experienced greater credit constraints in the form of credit denials, and that the cash windfalls went toward debt repayment, prior to auto consumption.

6 Conclusion

An important class of macro models posits that households must repay debt before consumption can recover from a recession with significant household debt burden. Yet, these debt repayment dynamics have not been, to date, studied in response to unexpected and persistent shocks to cash flow. Using a novel set of such shocks to individuals from the extraction of shale natural gas, we find substantial heterogeneity in how households use unexpected cash to repay debt. Subprime households use the additional cash flow to repay debt, consistent with these households facing a significant debt burden. We link the debt repayment behavior in our setting to consumption responses of consumers for durable goods. We find that debt repayment out of cash flow is important for the consumption choices of subprime individuals, while being less important for prime individuals who are less credit constrained before receiving the cash windfall.

There has been substantial interest in both measuring a household's MPC and, to an additional extent, understanding how this MPC interacts with household debt overhang. Our paper takes these findings a step further, to directly trace out how consumption can be facilitated by deleveraging of household balance sheets for subprime borrowers over time. Taken together, these results provide important insight into how models of household leverage and consumption relate to household characteristics. Our evidence suggests that high initial household leverage may blunt the consumption effects of unanticipated cash flows, as subprime individuals in particular pay down their debt prior to consuming. These findings provide a novel perspective on why it is challenging to stimulate consumption by subprime individuals and link marginal propensity to repay debt with classical marginal propensity to consume estimates.

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7 Tables

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 receive 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.

(a) Summary Statistics on Mineral Payments

Variable	*N*	mean	Std. Dev.	p1	p25	p50	p75	p99
Total Payment ($)	215,639	5,683.90	9,338.45	77.89	1,003.68	2,717.39	6,281.98	46,245.85
Income in 2005 ($1,000s)	215,639	51.40	22.22	22.00	38.00	46.00	59.00	138.00
Total Payments / Income (%)	215,639	11.16	15.66	0.14	2.16	5.70	12.97	82.54
Avg Payments / Income (%)	215,639	1.01	1.42	0.01	0.20	0.52	1.18	7.50
Net Mineral Acres Owned	215,639	0.64	7.15	0.00	0.00	0.16	0.34	6.05
Year of First Payment	214,845	2007.95	1.53	2005	2007	2008	2008	2012
Reside in Barnett	215,639	0.90	0.30	0.00	1.00	1.00	1.00	1.00

(b) Summary Statistics on Credit Characteristics

Variable	*N*	mean	Std. Dev.	p1	p25	p50	p75	p99
Income in 2005 ($1,000s)	785,884	50.75	21.45	22.00	37.00	46.00	58.00	134.00
Age in 2005	783,669	49.51	13.46	22	40	49	58	84
Credit Score (Vantage Score)	785,884	718.24	92.97	478.00	651.00	739.00	801.00	833.00
... Subprime (=1)	785,884	0.19	0.39	0.00	0.00	0.00	0.00	1.00
... Prime (=1)	785,884	0.51	0.50	0.00	0.00	1.00	1.00	1.00
Total Debt-to-Income (%)	785,884	15.60	13.53	0.00	3.00	13.00	24.00	53.00
Revolving Balance	723,513	9,420.65	20,185.53	0.00	741.00	3,281.00	10,246.00	80,975.88
Auto Loan Balance	333,716	20,469.92	16,974.78	788.00	9,478.00	16,662.00	26,652.00	79,430.50
Mortgage Balance	411,687	132,040	157,078.05	1,270.86	56,715.00	96,073.00	155,801.00	704,578.02

(c) Treatment Observations Satisfying Data Quality Filters

Sampling Restriction	Number of Treated Individuals	Treatment Obs. Dropped
Full Sample	404,937	–
Valid Matches with nonmissing data in 2005-2015 panel	367,727	37,210
Observe bonus payment	234,385	133,342
Restrict to moderate payments (< 100% of 2005 income)	215,639	18,746

Table 2: Initial Characteristics for Windfall Recipients versus Others

Note: This table reports characteristics in key variables observed in year 2005 prior to receipt of mineral payments for recipients of cash windfalls versus control individuals based on a propensity score matching of credit score and length of credit history. For each sample and characteristic, we report the mean, median, 25th percentile and 75th percentile.

	Mineral Payment Recipients			
Variable	Mean	Median	25th Percentile	75th Percentile
Debt-to-Income (%)	19.8	19.0	8.0	29.0
Revolving Balance	10,214.0	3,645.0	891.0	11,374.0
Credit Score (Vantage Score)	705.1	720.0	637.0	790.0
Initially Subprime (%)	20.3	0.0	0.0	0.0
Net Mineral Acres Owned	0.64	0.16	0.00	0.34
Income ($1,000s)	51.4	46.0	38.0	59.0
Age	48.6	48.0	39.0	58.0

	Control Sample Individuals			
Variable	Mean	Median	25th Percentile	75th Percentile
Debt-to-Income (%)	15.2	13.0	2.0	24.0
Revolving Balance	9,134.0	8,337.8	2,619.0	437.0
Credit Score (Vanage Score)	701.5	717.0	629.0	790.0
Initially Subprime (%)	22.4	0.0	0.0	0.0
Net Mineral Acres Owned	0.00	0.00	0.00	0.00
Income ($1,000s)	47.8	43.0	35.0	55.0
Age	51.7	50.0	42.0	60.0

Table 3: Long-Run Debt-to-Income Effects for Moderate versus Large Payments

Note: The dependent variable is the total debt-to-income of the consumer provided by Experian. 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 and bonus payments as part of our sample from mineral owners in 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 debt to income ratios between those receiving mineral payments and those in the control sample. Mineral acres owned (Z) are the number of mineral acres owned by the individual, standardized to have a mean of 0 and standard deviation of 1 for ease of interpretation. The odd columns present this effect for moderate payments (total payments / annual income < 100%), whereas the even columns present this effect for large payments (total payments / annual income > 100%). Standard errors clustered by ZIP3 in parentheses. *, **, and *** indicate statistical significance at the 10%, 5% and 1% level respectively.

dependent variable:	Total Debt-to-Income					
pre-period credit sample:	Full Sample		Subprime		Prime	
size of payments sample (more or less than 100% of annual income):	less	more	less	more	less	more
	(1)	(2)	(3)	(4)	(5)	(6)
treatment x post	-1.579^{***}	-1.540^{***}	-3.309^{***}	-3.852^{***}	-0.366^{***}	-0.356
	(0.154)	(0.143)	(0.532)	(1.118)	(0.142)	(0.326)
mineral acreage owned (Z) x post	0.050^{**}	0.056	-0.068	0.317	0.089^{***}	0.066^{**}
	(0.024)	(0.066)	(0.066)	(0.367)	(0.032)	(0.033)
ZIP3-year FE	x	x	x	x	x	x
age-year FE	x	x	x	x	x	x
income-year FE	x	x	x	x	x	x
individual FE	x	x	x	x	x	x
Observations	785,884	68,587	146,298	9,212	403,572	40,985
Adjusted R^2	0.484	0.519	0.333	0.382	0.521	0.525

Table 4: Long-Run Debt-to-Income Effects

Note: The dependent variable is the total debt-to-income of the consumer provided by Experian. The unit of observation is an individual-year in which two years are considered, 2005 (pre) and 2015 (post). The treatment intensity variable *Avg payments/income* is average annual bonus and royalty payment from Barnett Shale extraction between 2005 and 2015 as a percentage of 2005 income. 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 *Avg payments/income x post* captures how a one percentage point increase in payments/income translates into a percentage change in debt-to-income (i.e., the marginal propensity to pay down debt). Mineral acres owned (Z) are the number of mineral acres owned by the individual, standardized to have a mean of 0 and standard deviation of 1 for ease of interpretation. The sample is restricted to individuals who receive payments up to 100% of their annual income in 2005. Panel (a) reports results on the sample with matched control and treatment individuals that have credit scores within 100 credit score units in 2005. Panel (b) relaxes restrictions on matched controls and includes the full sample provided by Experian. Standard errors clustered by ZIP3 in parentheses. *, **, and *** indicate statistical significance at the 10%, 5% and 1% level respectively.

(a) Estimates of MPR using precisely matched controls

dependent variable:	Total Debt-to-Income					
pre-period credit sample:	Full Sample		Subprime		Prime	
	(1)	(2)	(3)	(4)	(5)	(6)
Avg PTI x post	-0.291^{***}	-0.330^{***}	-0.687^{***}	-0.772^{***}	-0.096	-0.141^{**}
	(0.046)	(0.041)	(0.100)	(0.118)	(0.068)	(0.057)
mineral acres owned (Z) x post	0.023	0.083^{***}	-0.068	0.028	0.060^{*}	0.102^{***}
	(0.025)	(0.030)	(0.114)	(0.078)	(0.031)	(0.025)
individual FE	x	x	x	x	x	x
ZIP3-year FE	x	x	x	x	x	x
age quintile-year FE		x		x		x
income quintile-year FE		x		x		x
Observations	682,627	682,627	119,567	119,567	365,471	365,471
Adjusted R^2	0.467	0.490	0.255	0.304	0.503	0.524

(b) Full sample estimates of MPR

dependent variable:	Total Debt-to-Income					
pre-period credit sample:	Full Sample		Subprime		Prime	
	(1)	(2)	(3)	(4)	(5)	(6)
Avg PTI x post	-0.325^{***}	-0.367^{***}	-0.891^{***}	-0.856^{***}	-0.044	-0.108^{**}
	(0.041)	(0.041)	(0.104)	(0.127)	(0.053)	(0.044)
mineral acres owned (Z) x post	0.004	0.073^{***}	-0.132	-0.017	0.052^{*}	0.101^{***}
	(0.027)	(0.028)	(0.086)	(0.067)	(0.031)	(0.027)
individual FE	x	x	x	x	x	x
ZIP3-year FE	x	x	x	x	x	x
age quintile-year FE		x		x		x
income quintile-year FE		x		x		x
Observations	785,884	785,884	146,298	146,298	403,572	403,572
Adjusted R^2	0.458	0.483	0.281	0.328	0.497	0.521

Table 5: Long-Run Debt-to-Income Effects – Estimates in Areas Unexposed to Drilling

Note: The dependent variable is the total debt-to-income of the consumer provided by Experian. The unit of observation is an individual-year in which two years are considered, 2005 (pre) and 2015 (post). The sample is restricted to individuals who receive payments up to 100% of their annual income in 2005. Sample and variable definitions are the same as in Table 4. Standard errors clustered by ZIP3 in parentheses. *, **, and *** indicate statistical significance at the 10%, 5% and 1% level respectively.

dependent variable:	Total Debt-to-Income					
pre-period credit sample:	Full Sample		Subprime		Prime	
out of area subsample?	no	yes	no	yes	no	yes
	(1)	(2)	(3)	(4)	(5)	(6)
Avg PTI x post	-0.370^{***}	-0.460^{***}	-0.949^{***}	-1.607^{**}	-0.075	-0.040
	(0.039)	(0.177)	(0.085)	(0.739)	(0.054)	(0.180)
mineral acres owned (Z) x post	0.016	-0.034	-0.102	-0.114	0.054^{**}	-0.121
	(0.023)	(0.156)	(0.082)	(0.597)	(0.026)	(0.182)
individual FE	x	x	x	x	x	x
Observations	785,884	71,014	146,298	11,639	403,572	36,897
Adjusted R^2	0.452	0.378	0.270	0.233	0.491	0.409

Table 6: Effects of Cash Windfalls on Debt Repayment by Category

Note: The dependent variables are log transformed balances of consumer credit categories reported to Experian. Specifically, we estimate how receiving mineral payments affects revolving balances, auto loan balances and mortgage balances. The unit of observation is an individual-year in which two years are considered, 2005 (pre) and 2015 (post). We restrict attention to individuals (and their matched controls) who receive less than 100% of their income in 2005. The variable *Avg payments/income* is average bonus and royalty payment from Barnett Shale extraction between 2005 and 2015 as a percentage of 2005 income. 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. For subsamples based on credit score, the subsample is based on the treated individual's initial credit status. Standard errors clustered by ZIP3 in parentheses. *, **, and *** indicate statistical significance at the 10%, 5% and 1% level respectively.

dependent variable:	%Δ Revolving Credit		%Δ Auto Loan Balance		%Δ Mortgage Balance	
pre-period credit sample:	Subprime	Prime	Subprime	Prime	Subprime	Prime
	(1)	(2)	(3)	(4)	(5)	(6)
Avg PTI x post	-3.908^{**}	5.052^{***}	2.474	-0.176	-0.942	0.161
	(1.522)	(0.525)	(1.534)	(0.999)	(1.324)	(1.713)
mineral acres owned (Z) x post	-1.124	-0.034	-0.697	-1.533	-1.121	0.213
	(3.192)	(0.647)	(2.179)	(1.645)	(1.315)	(1.155)
individual FE	x	x	x	x	x	x
ZIP3-year FE	x	x	x	x	x	x
income-year FE	x	x	x	x	x	x
age-year FE	x	x	x	x	x	x
Observations	87,306	361,399	61,512	129,882	57,740	184,920
Adjusted R^2	0.274	0.479	0.240	0.234	0.602	0.382

Table 7: Effect of Windfalls on Automobile Consumption

Note: The dependent variable is an indicator variable for whether the individual purchased a car, measured by whether the individual's auto loan balanced increased year over year between 2005 and 2015, as in Dupor et al. (2019). The specification is a changes specification in which the unit of observation is an individual observed in 2015, and the flow of consumption is measured over the period 2005 (pre) through 2015 (post). We restrict attention to individuals (and their matched controls) who receive less than 100% of their income in 2005. The variable *Avg payments/income* is average bonus and royalty payment from Barnett Shale extraction between 2005 and 2015 as a percentage of 2005 income. 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. For subsamples based on credit score, the subsample is based on the treated individual's initial credit status. Standard errors clustered by ZIP3 in parentheses. *, **, and *** indicate statistical significance at the 10%, 5% and 1% level respectively.

dependent variable:	Auto purchase (=1)					
pre-period credit sample:	Full Sample		Subprime		Prime	
	(1)	(2)	(3)	(4)	(5)	(6)
Avg PTI	0.288***	0.037	0.757***	0.508***	−0.001	−0.302
	(0.102)	(0.135)	(0.196)	(0.143)	(0.155)	(0.193)
mineral acres owned (Z)	−0.169**	−0.053	−0.059	−0.197	−0.156*	−0.029
	(0.076)	(0.088)	(0.169)	(0.174)	(0.094)	(0.120)
ZIP3 FE	x	x	x	x	x	x
age quintile FE		x		x		x
income quintile FE		x		x		x
Observations	431,278	431,278	87,658	87,658	215,116	215,116
Adjusted R^2	0.006	0.087	0.007	0.077	0.006	0.088

8 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.

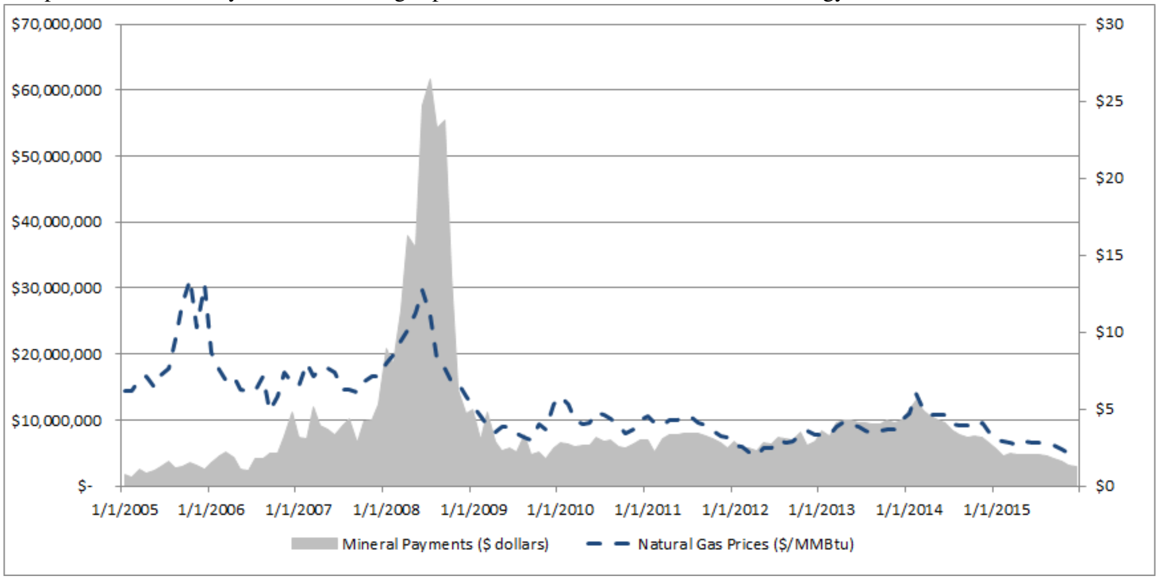


Figure 2: Wells under Production in the Barnett Shale over Time

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.

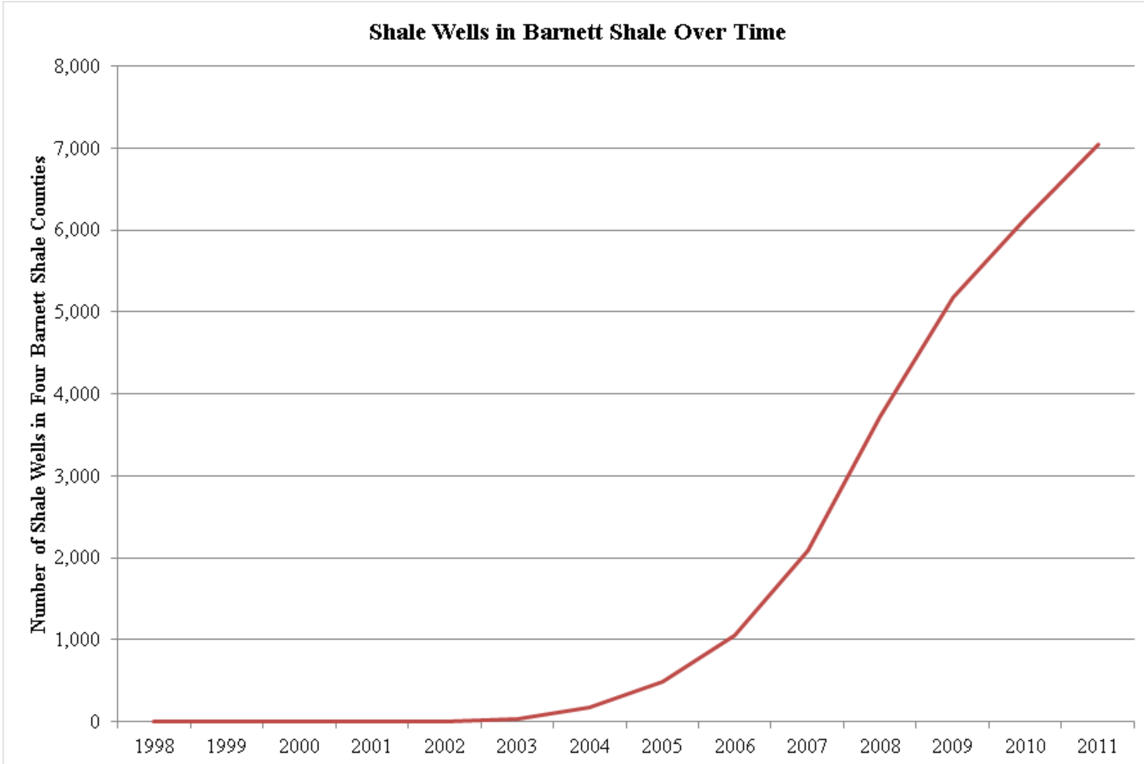


Figure 3: The Spatial Distribution of Wells (Johnson County)

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.

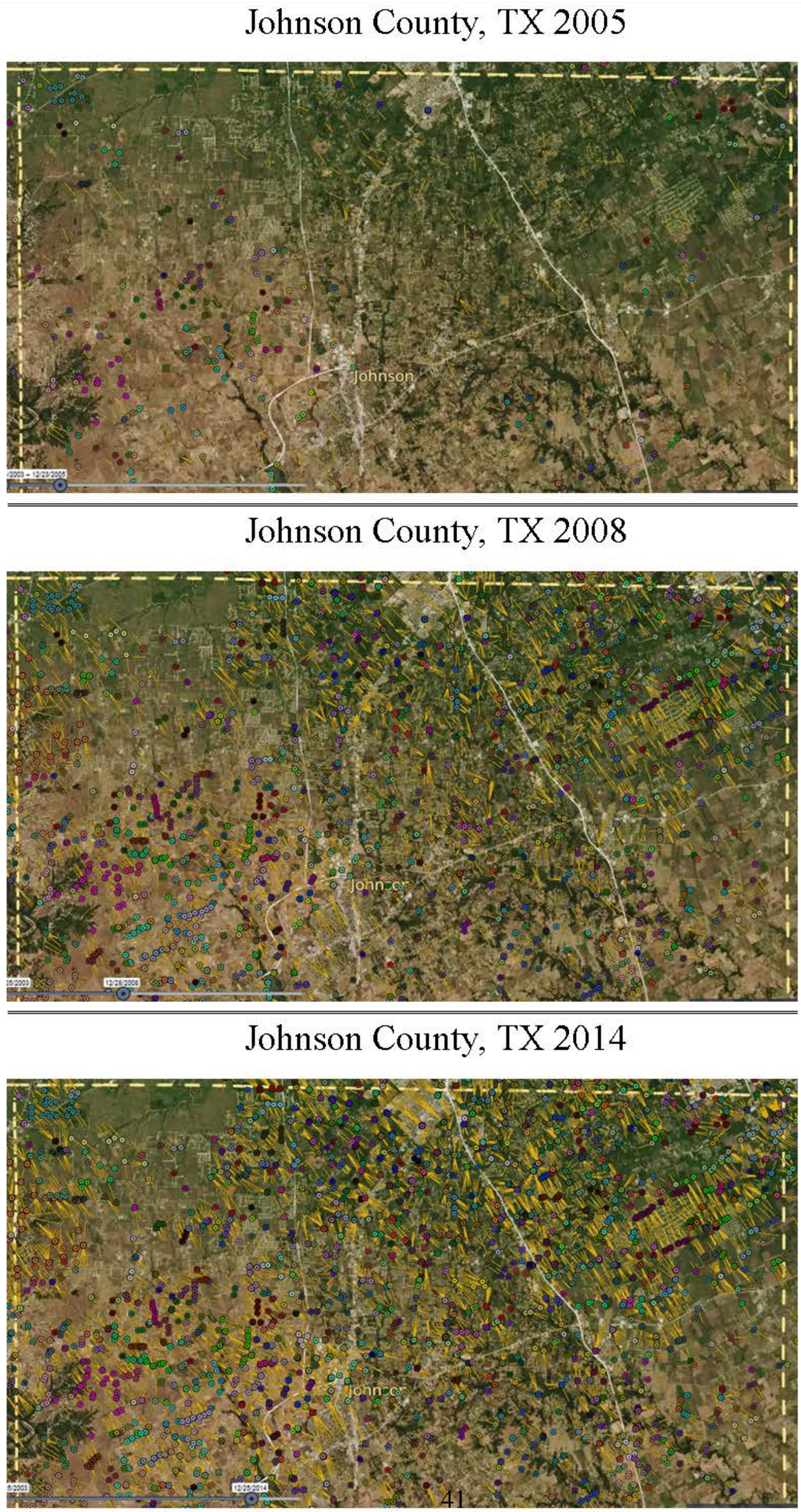


Figure 4: Locations of Individuals Receiving Mineral Rights Payments

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. Darker points indicate greater numbers of mineral owners in a location.

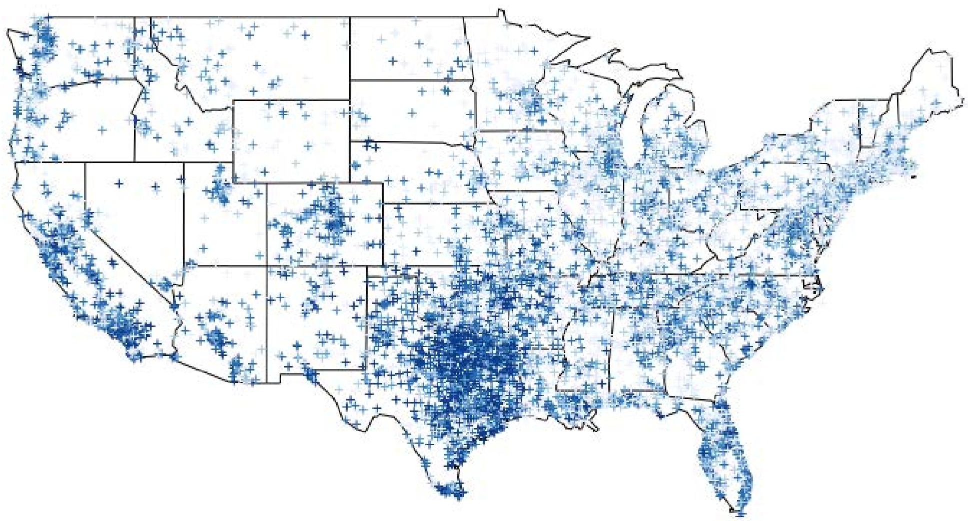


Figure 5: Comparison of Mineral Rights Owners to the Nationally Representative Sample

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.

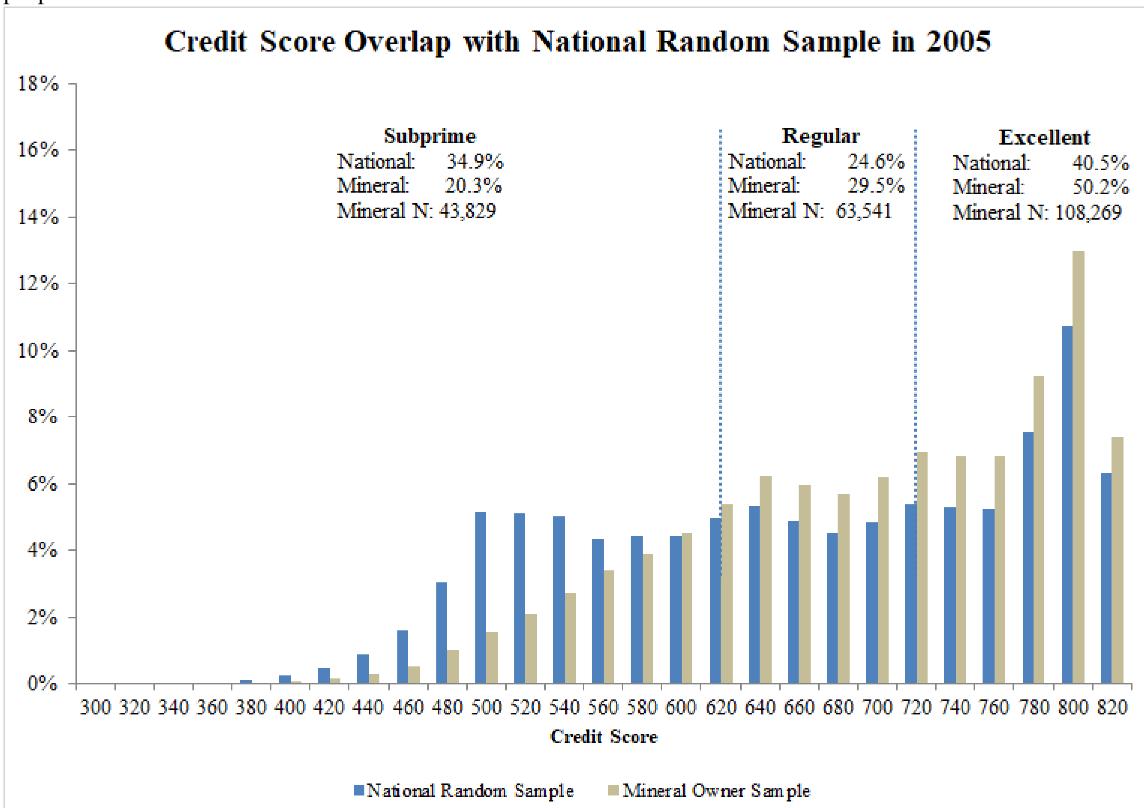


Figure 6: Average Mineral Payment Profile – Subprime versus Prime

Note: This figure presents the cumulative amount of the total payment as of years 1 through 6 after the year of first payment by individuals who are subprime in 2005 (credit score less than 620) versus prime individuals in 2005 (credit score greater than 720), using the time series of payments in our mineral payment sample.

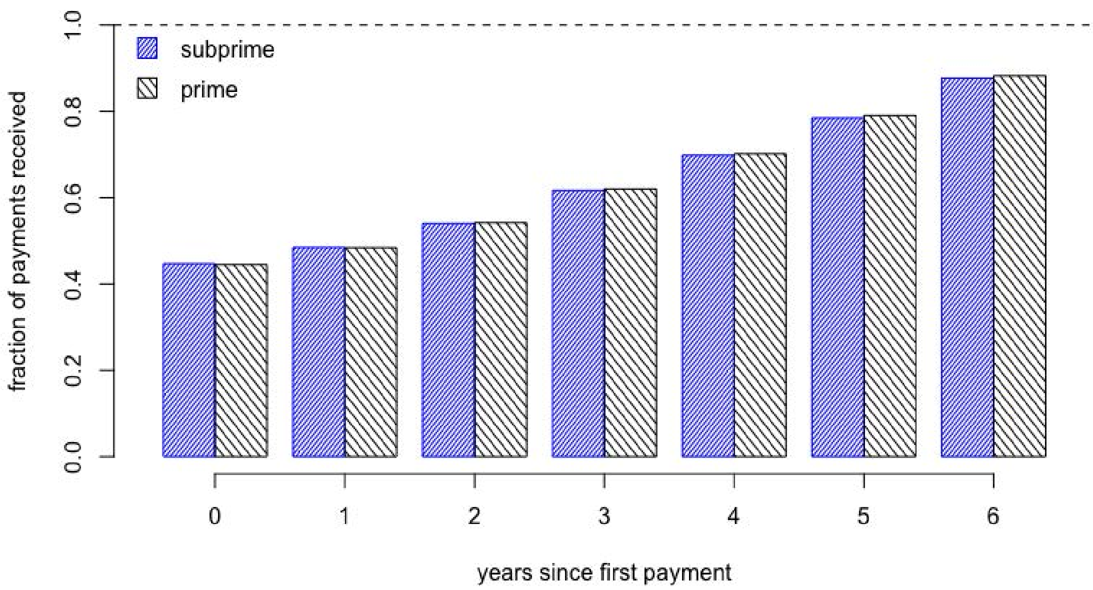


Figure 7: Debt-to-Income Dynamics

Note: This figure presents the dynamics in the effect of average payments-to-income on debt-to-income. Specifically, the figure plots the coefficient estimates regression of debt-to-income on Average PTI interacted with leads and lags relative to the first year the individuals receives a payment. We estimate separate leads-and-lags regressions for subprime individuals and prime individuals. As in the analogous specifications in Table 4, we restrict attention to individuals (and their matched controls) whose matched controls are within 100 credit score points in 2005 and individuals who receive aggregate payments less than 100% of their income in 2005. Subprime coefficient estimates are indicated as black diamonds, whereas the prime coefficient estimates are indicated as blue squares. Approximate 95% confidence intervals are depicted by the dotted lines. Standard errors are clustered by ZIP3.

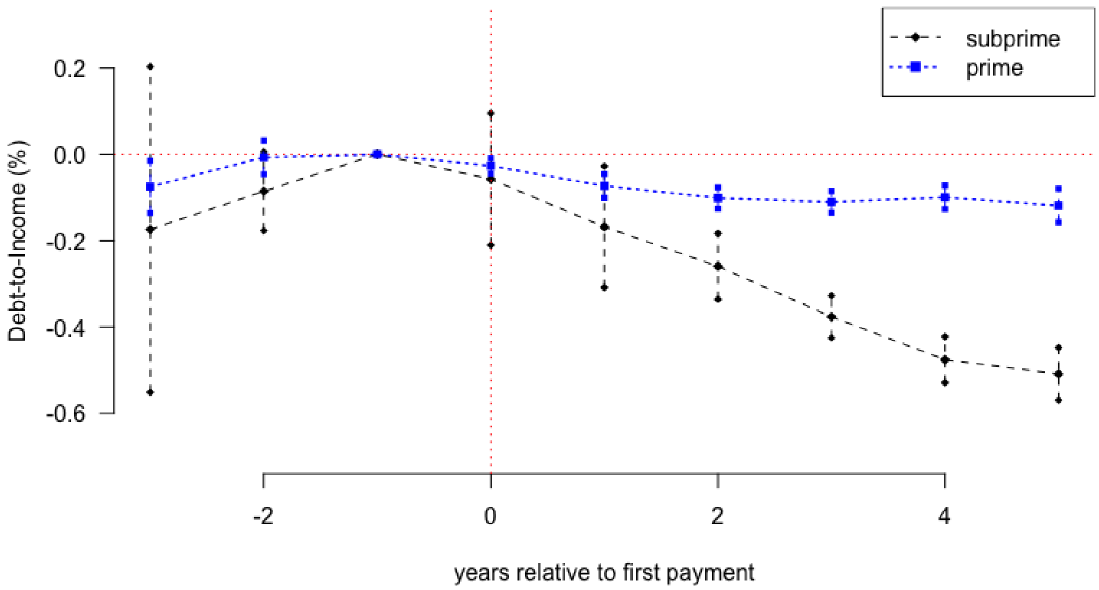


Figure 8: Revolving Credit Dynamics

Note: This figure presents the dynamics in the effect of receiving mineral payments on logged (1+) revolving balances. Specifically, the figure plots the coefficient estimates from a leads-and-lags regression of logged revolving balances on Average PTI interacted with leads and lags relative to the first year the individuals receives a payment. We estimate separate leads-and-lags regressions for subprime individuals and prime individuals. As in the analogous specifications in Table 6, we restrict attention to individuals (and their matched controls) whose matched controls are within 100 credit score points in 2005 and treatment individuals who receive aggregate payments less than 100% of their annual income. Subprime coefficient estimates are indicated as black diamonds, whereas the prime coefficient estimates are indicated as blue squares. Approximate 95% confidence intervals are depicted by the dotted lines. Standard errors are clustered by ZIP3.

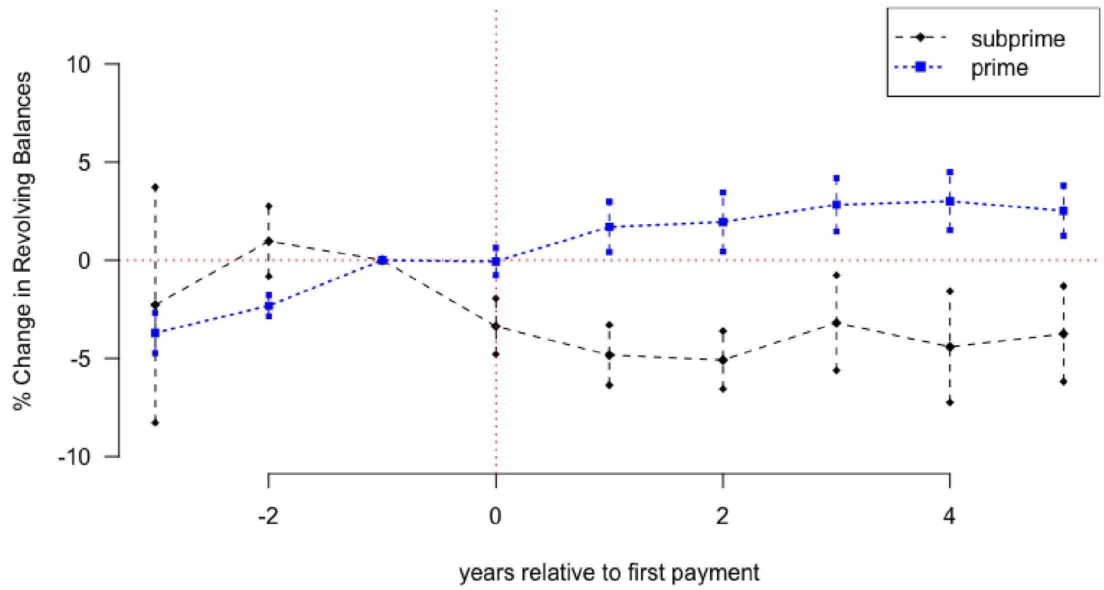


Figure 9: Cumulative Debt Repayment as of First Auto Purchase

Note: This figure presents a plot of the cumulative amount of debt repayment – difference in DTI relative to DTI upon receipt of first mineral windfall – in the year of first automobile purchase after beginning to receive mineral windfalls. Subprime coefficient estimates are indicated as black diamonds, whereas the prime coefficient estimates are indicated as blue squares. The estimates are drawn from fixed effects specifications with cumulative debt repayment as the dependent variable, and are conditioned on ZIP3 by year FE, income bin by year FE, and age bin by year FE. Approximate 95% confidence intervals are depicted by the dotted lines. Standard errors are clustered by ZIP3.

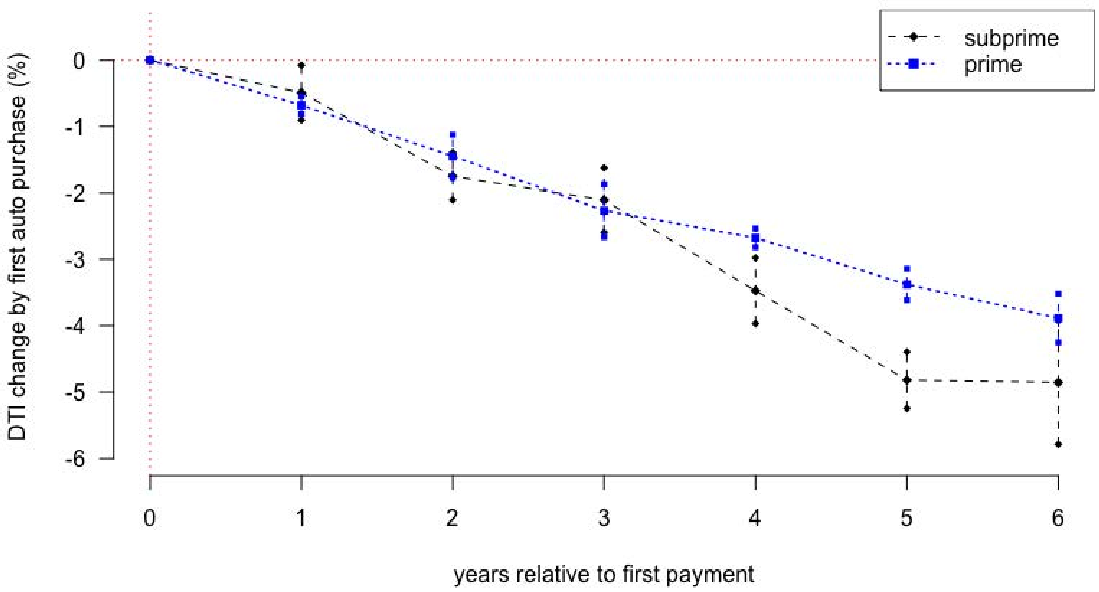


Figure 10: Likelihood of Initially Subprime Individuals Making Auto Purchase

Note: This figure presents estimates of the likelihood of initially subprime individuals to purchase an automobile for the first time in year t after the date of receipt of first payment in year $t = 0$. The plot presents estimates and 95% confidence intervals for the difference in the likelihood of auto purchases by subprime individuals, drawn from a linear probability model with ZIP3 by year FE, income bin by year FE, and age bin by year FE. Standard errors are clustered by ZIP3.

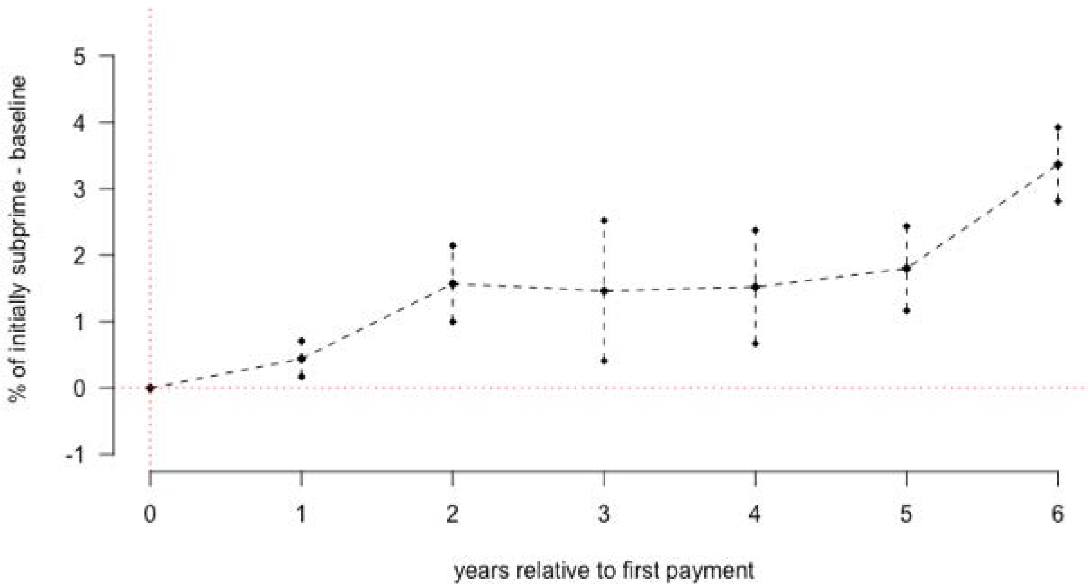
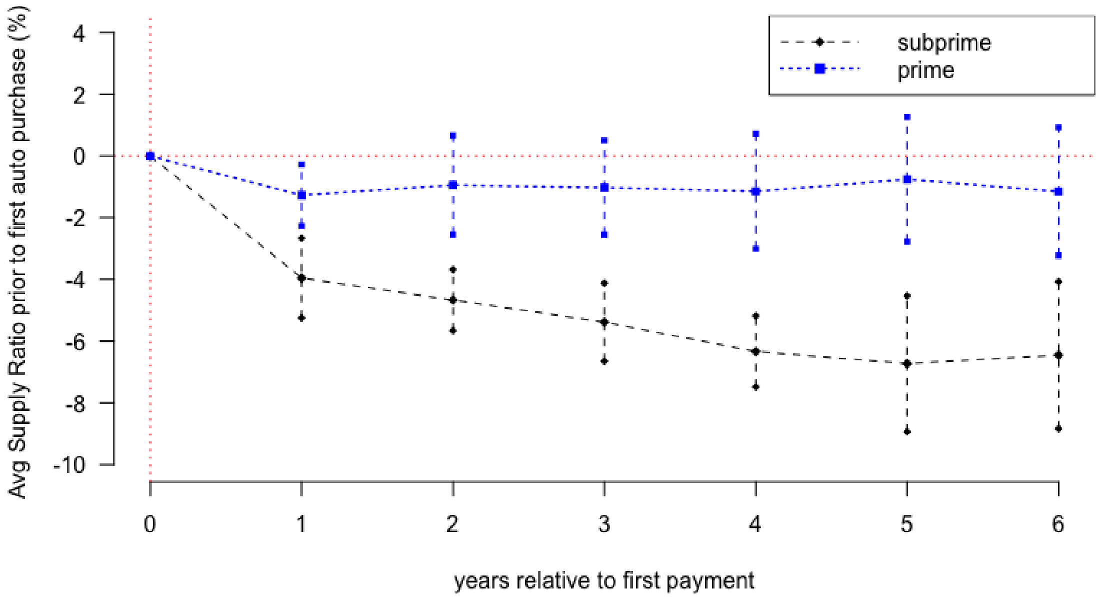


Figure 11: Credit Denials Prior to First Automobile Purchase

Note: This figure presents estimates of how the timing of purchasing one's first automobile after the date of first receipt in year $t = 0$ relates to the average annual supply ratio ($\frac{new\ credit\ lines_t}{hard\ credit\ inquiries_t}$) taken over all years in the credit panel prior to the auto purchase year. The plot presents estimates and 95% confidence intervals for the difference in the average supply ratio (as compared to the baseline year $t = 0$). The specification includes with ZIP3 by year FE, income bin by year FE, and age bin by year FE. Standard errors are clustered by ZIP3.



Internet Appendix to:

Shale Shocked: Cash Windfalls and Household Debt Repayment

A Additional tables and figures

A.1 Examples of Raw Data

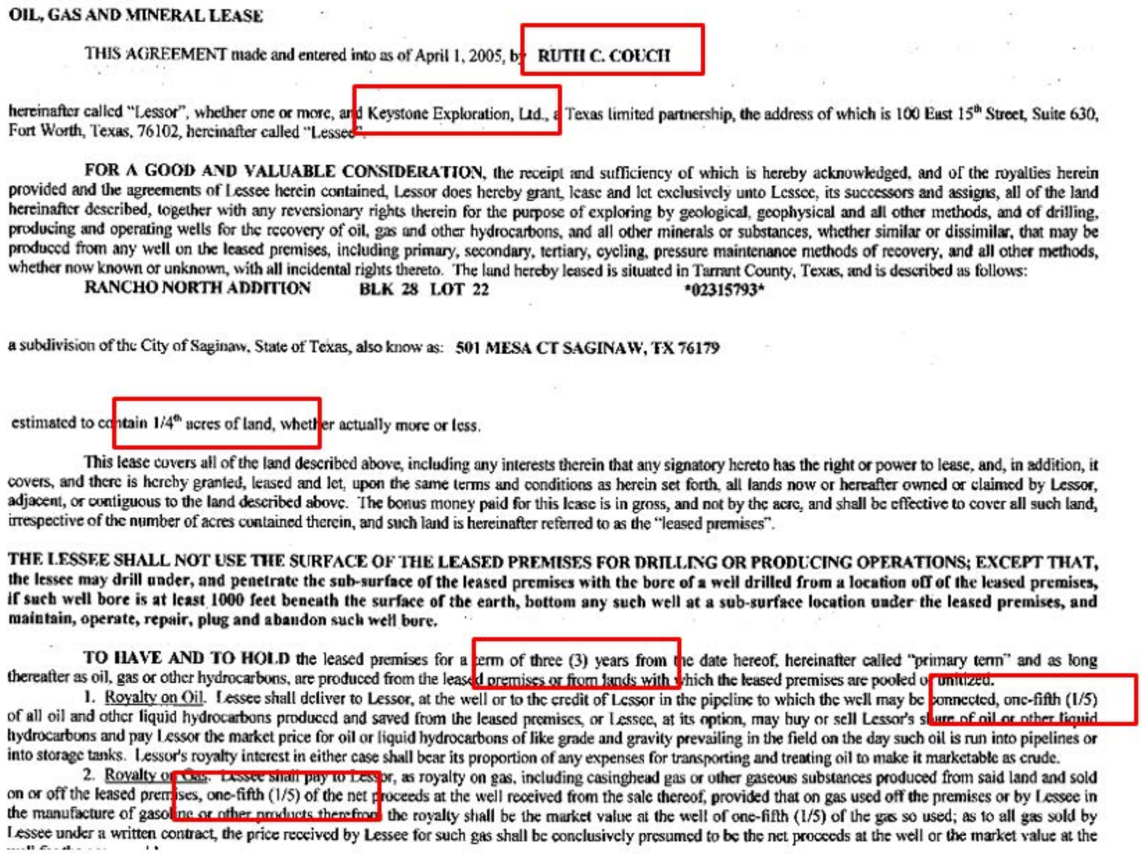
Figure A.1: Example of Raw Mineral Appraisal Roll

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

A.2 Summary Statistics

Table A.1: Comparison of Initial Characteristics for Windfall Recipients versus Control Sample

Note: This table reports differences in key variables observed in year 2005 prior to receipt of mineral payments for treated individuals versus control individuals based on a propensity score matching of credit score and length of credit history. The Adjusted Difference column reports the difference between treatment and control samples after controlling for mineral acreage and an indicator for whether the individual has a mortgage in 2005, and including ZIP3, age quintile, and income quintile fixed effects. Statistical significance is based on clustering by 3 digit zip (similar to our main tests) is reported. Statistically significant differences at the 5% level are indicated by $*$ and 1% by **.

Variable	Treatment	Control	Difference	Adjusted Difference	% of Std. Deviation
Debt-to-Income (%)	19.8	15.2	4.6^{**}	0.4^{**}	2.71%
Revolving Credit ($1,000s)	10.2	8.3	1.9^{**}	0.4^{*}	1.88%
Credit Score (Vantage Score)	705.1	701.5	3.6^{**}	1.2	1.29%
Initially Subprime (%)	20.3	22.4	-2.1^{**}	0.1	0.17%
Net Mineral Acres Owned	0.6	0.0	0.6^{**}	0.0	0.00%
Income ($1,000s)	51.4	47.8	3.6^{**}	0.2	0.83%
Age	48.6	51.7	-3.1^{**}	0.0	0.00%

Table A.2: Summary Statistics by Initial Credit Score

Note: This table reports summary statistics for credit bureau debt variables, split by initially-subprime individuals versus initially-prime individuals. The data have a unit of observation at the individual-year level (the two years being 2005 and 2015), and include both mineral owners and matched control individuals used in our panel.

Variable	*N*	mean	Std. Dev.	p1	p25	p50	p75	p99
Subprime Sample								
Total Debt-to-Income (%)	146,298	20.50	14.93	0.00	9.00	19.00	31.00	59.00
Revolving Balance	110,449	8,345.44	16,540.41	0.00	561.00	2,770.00	9,425.00	71,856.12
Auto Loan Balance	73,090	20,725.57	16,628.14	808.00	9,835.00	16,857.50	26,862.00	79,918.00
Mortgage Balance	71,292	109,967.90	120,110.49	4,926.28	55,459.00	86,892.50	131,171.25	507,913.76
Prime Sample								
Total Debt-to-Income (%)	403,572	11.70	11.39	0.00	1.00	10.00	19.00	45.00
Revolving Balance	397,770	8,027.36	19,511.59	0.00	669.00	2,749.00	7,978.00	76,372.00
Auto Loan Balance	146,292	19,368.93	16,374.30	725.91	8,883.00	15,835.00	25,315.00	75,068.00
Mortgage Balance	205,324	137,878.90	174,508.48	0.00	53,593.00	97,382.50	164,424.00	769,219.83

A.3 Debt-to-Income Robustness

Table A.3: Long-Run Debt-to-Income Effects – Restricting to Individuals who Own 0.5 or More Mineral Acres

Note: The dependent variable is the total debt-to-income of the consumer provided by Experian. The unit of observation is an individual-year in which two years are considered, 2005 (pre) and 2015 (post), restricted to individuals who receive payments less than 100% of their annual income. Sample and variable definitions are as in Table 4, with the additional restriction that the individual owns at least half an acre of mineral rights.Standard errors clustered by ZIP3 in parentheses. *, **, and *** indicate statistical significance at the 10%, 5% and 1% level respectively.

dependent variable:	Total Debt-to-Income					
pre-period credit sample:	Full Sample		Subprime		Prime	
	(1)	(2)	(3)	(4)	(5)	(6)
Avg PTI x post	−0.265***	−0.284***	−0.446***	−0.449**	−0.092	−0.113**
	(0.025)	(0.031)	(0.133)	(0.178)	(0.066)	(0.056)
acres (Z) x post	0.055	0.156***	−0.354	−0.097	0.153**	0.215***
	(0.070)	(0.058)	(0.415)	(0.282)	(0.078)	(0.064)
individual FE	x	x	x	x	x	x
ZIP3-year FE	x	x	x	x	x	x
age quintile-year FE		x		x		x
income quintile-year FE		x		x		x
Observations	121,114	121,114	13,171	13,171	65,578	65,578
Adjusted R^2	0.495	0.516	0.296	0.341	0.514	0.531

A.4 Categories of Credit – Broad Sample

Table A.4: Categories of Credit – Out-of-area subsample

Note: The dependent variables are log transformed balances of consumer credit categories reported to Experian. Specifically, we estimate how receiving mineral payments affects revolving balances, auto loan balances and mortgage balances. The unit of observation is an individual-year in which two years are considered, 2005 (pre) and 2015 (post). We restrict attention to individuals (and their matched controls) who receive less than 100% of their income in 2005, and for this test whether the individual resides outside of the Barnett Shale area. Other variable definitions and sample choices are the same as in Table 6. Standard errors clustered by ZIP3 in parentheses. *, **, and *** indicate statistical significance at the 10%, 5% and 1% level respectively.

dependent variable:	%Δ Revolving Credit		%Δ Auto Loan Balance		%Δ Mortgage Balance	
pre-period credit sample:	Subprime	Prime	Subprime	Prime	Subprime	Prime
	(1)	(2)	(3)	(4)	(5)	(6)
Avg PTI x post	−12.450	6.075	−0.135	−0.839	−2.193	1.153
	(11.631)	(4.456)	(8.449)	(5.064)	(7.151)	(7.679)
acres (Z) x post	−7.654	−2.881	−6.211	0.719	−4.929	0.311
	(13.085)	(5.418)	(5.896)	(5.863)	(5.928)	(7.298)
individual FE	x	x	x	x	x	x
Observations	9,202	36,362	5,540	11,814	4,397	19,594
Adjusted R^2	0.309	0.407	0.205	0.216	0.293	0.350

A.5 Debt-to-Income Changes Specification

Table A.5: Long-Run Debt-to-Income Effects — Changes Specification

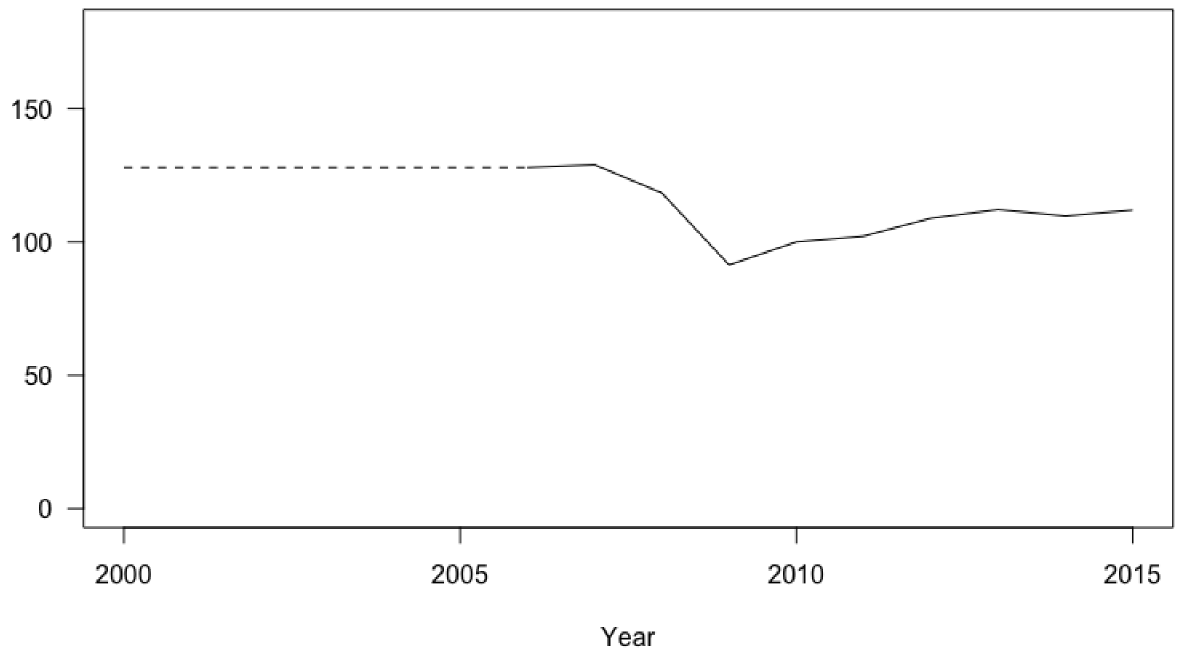
Note: The dependent variable is the cumulative debt repayment from the year of first mineral payment to 2015, computed from the credit file provided by Experian. The unit of observation is an individual observed in 2015, restricted to individuals who receive payments less than 100% of their annual income. The variable *Avg PTI* is average bonus and royalty payment from Barnett Shale extraction between 2005 and 2015 as a percentage of 2005 income. 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. For subsamples based on credit score, the subsample is based on the treated individual's initial credit status. Standard errors clustered by ZIP3 in parentheses. *, **, and *** indicate statistical significance at the 10%, 5% and 1% level respectively.

dependent variable:	Cumulative debt repayment					
pre-period credit sample:	Full Sample		Subprime		Prime	
	(1)	(2)	(3)	(4)	(5)	(6)
Avg PTI	-0.708^{***}	-0.667^{***}	-1.170^{***}	-1.092^{***}	-0.330^{***}	-0.305^{***}
	(0.040)	(0.028)	(0.059)	(0.054)	(0.031)	(0.024)
mineral acres owned (Z)	0.127^{***}	0.150^{***}	0.088	0.145^{***}	0.122^{***}	0.132^{***}
	(0.024)	(0.031)	(0.057)	(0.055)	(0.026)	(0.031)
ZIP3 FE	x	x	x	x	x	x
age quintile FE		x		x		x
income quintile FE		x		x		x
Observations	429,690	429,690	87,368	87,368	214,228	214,228
Adjusted R^2	0.007	0.027	0.011	0.034	0.006	0.032

A.6 Debt-to-Income Changes Specification

Figure A.3: Time Series Validation of Auto Purchases

Note: This figure presents the time series plot of aggregate auto purchases, plotted as an index relative to auto purchases in 2010, based on the auto purchase classification that we use in our analysis of auto consumption. The time series pattern matches closely with the time series pattern with vehicle registrations, as is reported in Figure A-2 from Dupor et al. (2019)'s analysis of auto consumption.



B Nonparametric Effects of Royalty Payment Size

The specification in equation (6) assumes a constant MPR regardless of the size of the payment, but as we motivated in the hypothesis development section, it is natural to expect that the MPR declines as payment size increases.

To estimate the nonparametric relationship between $Avg\ PTI_i$ and DTI_{it}, we estimate a generalized additive model (GAM) for debt payments relative to income, separately for $t = 2005$ and $t = 2015$:

$$DTI_{i(t)} = g_{(t)}\left(Avg\ PTI_i\right) + s_{1(t)}\left(acres_i\right) + s_{2(t)}\left(age_i\right) + s_{1(t)}\left(income_i\right) + \varepsilon_{i(t)}, \tag{9}$$

where the dependent variable DTI_i is the percentage of debt payments relative to income for individual i, $Avg\ PTI_i$ is the annualized mineral payment relative to income in 2005, $acres_i$ is the number of net mineral acres owned by individual i, age_i is the individual's age in 2005, and $income_i$ is the individual's income (provided by Experian) in 2005. The subscript (t) indicates variables and functions that differ for the separate estimations – e.g., the fitted nonparametric function of $Avg\ PTI_i$ in 2005 is denoted as $g_{(2005)}\left(Avg\ PTI_i\right)$, whereas the estimated nonparametric function of $Avg\ PTI_i$ in 2005 is $g_{(2015)}\left(Avg\ PTI_i\right)$.

We estimate the difference-in-difference effect of $Avg\ PTI_i$ by taking the difference (post minus pre) of these estimated nonparametric functions that relate average payments-to-income to debt-to-income:

$$\beta_1\left(Avg\ PTI_i\right) = g_{(2015)}\left(Avg\ PTI_i\right) - g_{(2005)}\left(Avg\ PTI_i\right) \tag{10}$$

Because each generalized additive model conditions also on the nonparametric relationship of DTI_i to age, income and mineral acreage owned, the resulting nonparametric estimates are free of confounds related to lifecycle effects, income effects or unobserved characteristics that lead an individual to own more mineral acreage. We compute standard errors using a block bootstrap by ZIP3 to account estimation error in this multi-stage estimator and to account for local correlation of the errors.

Figure B.1 portrays the results from nonparametrically estimating the relation between $Avg\ PTI_i$ and DTI_i. Consistent with our motivating hypotheses, the marginal propensity to repay debt is greatest for relatively modest wealth shocks and diminishes quickly as payment size increases. By summarizing the slope of the best fitting line to the nonparametric relationship, Table B.1 provides a numerical summary of this diminishing effect of additional wealth on the marginal propensity to repay. For relatively small payments ($Avg\ PTI_i$ < 25% of income), we estimate that 97.6% of additional wealth goes toward repaying debt. By contrast, for relatively large payments ($Avg\ PTI_i$ > 50% of income), only 4.6% of the marginal dollar goes toward debt repayment. Using standard errors from a block bootstrap by ZIP3, this decline in the MPR along the payment size distribution is highly statistically significant.

Figure B.1: Nonparametric Estimation of the Effect of Payment Size on Debt-to-Income

Note: This figure presents the results from a nonparametric estimation of the impact of mineral payments-to-income on the debt-to-income ratio. Specifically, we estimate the effect of payments/income using a generalized additive model – estimated separately in the 2005 pre-period and in the 2015 post-period – that also controls nonparametrically for age, income and mineral acres owned. For this plot, we compute the effect of payments at each payment-to-income ratio as the difference between the smoothed functions (year 2015 minus year 2005). Dashed blue lines indicate the best linear approximation to the nonlinear fit for payments/income less than 50% and greater than 50%. Standard errors are computed as the standard deviation of the bootstrap sampling distribution using 200 bootstrap replications using a block bootstrap by ZIP3. Dotted lines indicate approximate 95% confidence intervals.

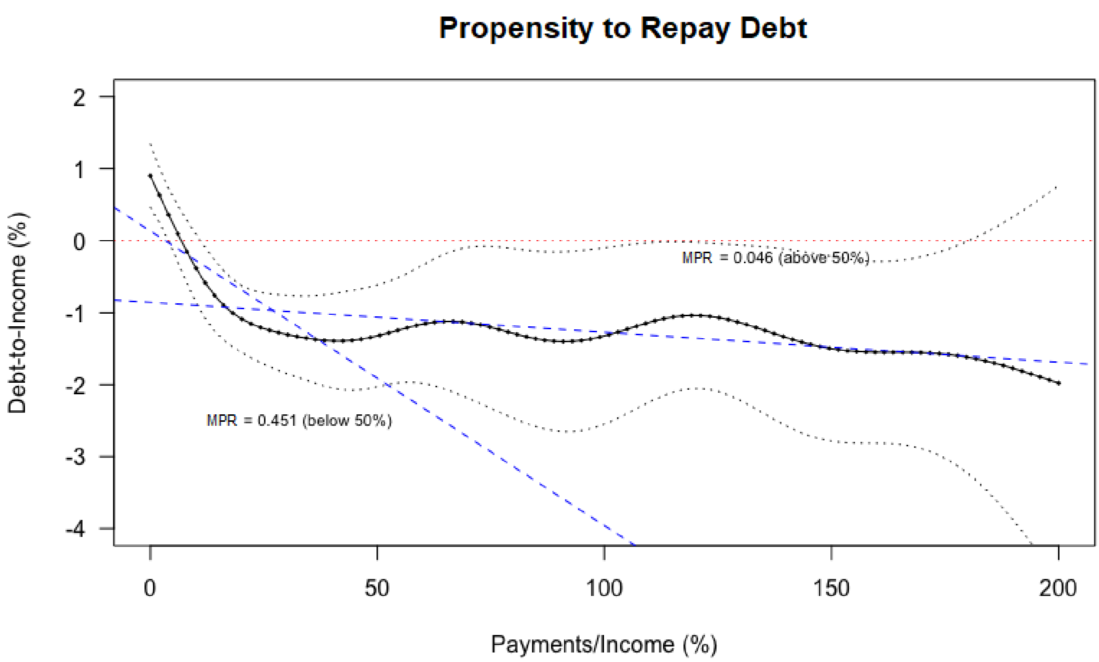


Table B.1: Long-Run Debt-to-Income Effects, Heterogeneity by Payment Intensity

Note: This table presents several statistical tests to gauge the heterogeneity in the propensity to pay down debt by the size of the payment. Specifically, we construct estimates for the MPR for four alternative ranges of payments: (i) below 25% of annual income, (ii) below 50% of annual income, (iii) above 50% of annual income, and (iv) above 100% of annual income. For each range of *Payments/income (%)*, we estimate the marginal propensity to pay down debt by computing the slope of the best fitting straight line to the nonparametric function in Figure B.1. To alleviate concern about outliers and to improve the precision of the nonparametric method, we restrict attention to individuals who receive total payments less than 200% of their annual income in 2005, and their matched controls. We cluster standard errors by ZIP3 using a block bootstrap procedure that computes standard errors as the standard deviation of the bootstrap sampling distribution using 200 bootstrap replications. *, **, and *** indicate statistical significance at the 10%, 5% and 1% level respectively.

	Range of Payments/Income (%)			
	Below 25%	Below 50%	Above 50%	Above 100%
$\frac{\Delta DTI}{\Delta Avg\ PTI}$	-0.975^{***}	-0.451^{***}	-0.046^{***}	-0.092^{***}
	(0.019)	(0.013)	(0.005)	(0.011)
Nonparametric controls x year				
acreage owned	x	x	x	x
age	x	x	x	x
income	x	x	x	x
T-statistics for differences in marginal propensity to pay down debt				
Below 25%	–	36.9^{***}	50.7^{***}	49.4^{***}
Below 50%	–	–	33.0^{***}	28.6^{***}
Above 50%	–	–	–	-6.5^{***}