Essays on Consumer Finance and Political Economy

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In Chapter One, I study the causal effect of consumer credit access on households' healthcare expenditures. I estimate such an effect using a simulated instrumental variable design on my assembled MEPS-credit data and measure the policy effect of a stimulus loan using a life-cycle model.

In Chapter Two, my coauthor and I study the impact of mortgage credit standards on households' homeownership. We discover such an impact using a difference-in-differences regression on the Experian consumer credit panel and Freddie Mac's Single-family Loan-level dataset. We construct a life-cycle housing model to quantitatively measure the impact.

In Chapter Three, my coauthors and I study the polarization of the Ukrainian Parliament. We compare two methodologies in identifying polarization using roll call votes data: the ideal points model and the community detection algorithm.

Table of Contents

Preface . . . xii

1.0 The Impact of Consumer Credit Access on Healthcare Expenditures . . . 1

- 1.1 Introduction . . . 1
- 1.2 Theory characterization . . . 5
- 1.3 Data . . . 8
  - 1.3.1 Measuring the credit access of MEPS respondents . . . 9
  - 1.3.2 Summary statistics . . . 16
- 1.4 Empirical strategy . . . 17
  - 1.4.1 Constructing the instrumental variable . . . 19
  - 1.4.2 Instrumental variable model . . . 20
- 1.5 Main result . . . 23
  - 1.5.1 First stage . . . 24
  - 1.5.2 Effect on annual total medical expenditure . . . 24
  - 1.5.3 Effect on emergency room visits . . . 31
  - 1.5.4 Effect on private insurance coverage . . . 36
  - 1.5.5 Robustness check . . . 41
- 1.6 Role of credit constraint in public health policies . . . 42
  - 1.6.1 Life cycle model . . . 43
  - 1.6.2 Calibration . . . 45
  - 1.6.3 Policy evaluation . . . 45
- 1.7 Conclusion . . . 47

2.0 Home Financing: Distributional Implications of Mortgage Credit Standards . . . 48
2.1 Introduction . . . 48
2.2 Empirical evidence . . . 50
2.2.1 Shift in mean credit score at mortgage origination . . . 50
2.2.2 Shift in mortgage inquiries and approval . . . 54
2.2.2.1 Likelihood of mortgage inquiries . . . 56
2.2.2.2 Likelihood of mortgage approval . . . 58
2.2.2.3 Payment to income ratio . . . 60
2.3 Model . . . 62
2.3.1 Housing market . . . 63
2.3.2 Mortgage financing . . . 64
2.3.3 Labor endowments and student loan debt . . . 65
2.3.4 Household decision problems . . . 66
2.4 Calibration . . . 68
2.4.1 Stochastic steady-state comparison . . . 70
2.5 Evaluating the implications of mortgage standards . . . 74
2.5.1 The effect of mortgage standards on homeownership . . . 74
2.6 Conclusion . . . 77
3.0 Detecting Disruption: Identifying Structural Changes in the Verkhovna Rada . . . 79
3.1 Introduction . . . 79
3.2 Data . . . 81
3.3 Ideal Points . . . 82
3.3.1 Background . . . 82
3.3.2 Methodology Used for Ukraine . . . 84

3.4 Faction Change Detection 86
3.4.1 Background 86
3.4.2 Methodology Used for Ukraine 87
3.5 Results 89
3.5.1 Legislative Change Points 89
3.5.1.1 Ideal Points 89
3.5.1.2 Factions 89
3.5.2 Discussion 89
3.6 Conclusion 92
Appendix. Appendix 94
A.1 Solution to the life cycle model 94
A.2 Tables and figures 96
Bibliography 109

List of Tables

1 Regression: the credit access proxies on the MEPS debt factors. Dependent variables are on the column names. 12
2 Summary statistics: outcome and explanatory variables in the MEPS. . 17
3 First stage: regression of individual's credit access on their county credit access. Dependent variables are on the column names. 25
4 Effect on medical expenditure: regression of medical expenditure on credit access. The dependent variable is the log of the amount of annual total medical expenditure. 30
5 Effect on emergency care: regression of emergency room visits on credit access. The dependent variable is whether the individual has visited an emergency room in a reference year. 35
6 Effect on coverage: regression of private health insurance coverage on credit access. The dependent variable is whether a Medicaid-ineligible person has private health insurance. 39
7 Life cycle model: effect of relaxing the credit constraint. 46
8 Regression result: effect of credit score on the interest rate at origination. 53
9 Regression result: effect of credit score on the likelihood of making mortgage inquiries. 57
10 Regression result: effect of credit score on the likelihood of getting a mortgage. 59
11 Regression result: effect of credit score on payment to income ratio at mortgage origination. 61

12 Summary of parameters, 2007 steady state calibration. 71
13 Simulated moments, 2007 steady state calibration. 72
14 1997 versus 2013 steady states comparison. 72
15 Summary of the data for each convocation. 82
16 Structural breaks 92
17 Correlation: delinquency and default flag against debt, income, and age. 96
18 Comparison of summary statistics: credit access measures. 98
19 Summary statistics: county-level credit access measures by age-income groups. 99
20 Effect on the young population age 21-35: regression of medical expenditures on credit access. Dependent variables are the same as in the regressions in Section 5 main results. 100
21 Robustness check: regression of households' opinion on credit access. The dependent variables are households' opinions on the need and the worth of health insurance. 101
22 Robustness check: regression of county healthcare access on county credit access. The dependent variables are on the column names. 102
23 Estimates for the wage process of high-school households. 107
24 Estimates for the wage process of college-degree households. 108

List of Figures

1 Variation in credit card limit by income and age, as a visualization of the estimation sample of Table 1. A random subsample of $n = 1000$ points. . . 15
2 Choropleth graphs for the instrumental variables. The higher degree of color darkness represents a higher level of credit access in the county. . . 22
3 Effect of credit access on annual total medical expenditure, sample plots. 28
4 Effect of credit access on emergency room visits, sample plots. 33
5 Effect of credit access on private insurance coverage, sample plots. . . . 38
6 Median FICO score by type of mortgage at origination. Source: calculation from Freddie Mac's Single-Family Loan-Level Dataset. 52
7 Fitted linear correlation between the mortgage interest rate of 30-year fixed-rate home mortgages at origination and FICO scores. Source: calculation from Freddie Mac's Single-Family Loan-Level Dataset. 55
8 Simulated life-cycle dynamics for different mortgage credit standards. . . 75
8 Simulated life-cycle dynamics for different mortgage credit standards (cont.) 76
9 85
10 90
11 91
12 Comparison of the residuals with and without the default flags in regressions. 97
13 Average student loan balance, CPI-adjusted to 2015 value. Source: calculation from Experian Consumer Credit Panel. 103

14 Fraction of population holding student loan debt. Source: calculation from Experian Consumer Credit Panel. 104

15 Wage income by age, CPI-adjusted to 2015 value. Source: calculation from the Panel Study of Income Dynamics. 105

16 Wage income by year, CPI-adjusted to 2015 value. Source: calculation from the Panel Study of Income Dynamics. 106

Preface

This dissertation studies two novel aspects of the economic impact of consumer credit access in the US and one of the many important political economy analyses for Ukraine.

I would like to thank numerous support from my chair advisor, my dissertation committee, the Economics Department, and the Center for Research Computing at the University of Pittsburgh.

1.0 The Impact of Consumer Credit Access on Healthcare Expenditures

1.1 Introduction

Extensive literature has studied the interaction between public health policies and consumer finance. At the center of the interaction is how large the elasticity of households' healthcare consumption to their credit access is. To the best knowledge, this question has not been answered due to data limitations. This paper estimates the causal effect of households' credit access on healthcare expenditure and health insurance coverage using an instrumental variable design on two robust household-level datasets.

Credit access, or the ability to borrow, affects households' healthcare expenditures for two significant reasons. First, credit-constrained low-income households prioritize daily goods or other critical expenses and often postpone non-emergency medical care. They would be willing to spend more on their health if they could borrow. Second, young households who are credit-constrained may spend less than enough on their health or skip their health insurance as they expect their income to grow and they can spend more on healthcare in the future.

I assess the quantitative importance of this mechanism using my combined dataset. I investigate three measures of households' ability to borrow: the credit score, the total credit amount on credit cards, and the number of inquiries per new loan opened. I study the impact of these credit access measures on three aspects of households' healthcare expenditures, including the annual total medical expenditure, likelihood of visiting an emergency room, and private health insurance coverage.

The challenge in identifying the impact of credit access is the two-way causality of household finance and healthcare expenditure. Unpaid medical bills will appear in a person's credit history, which affects his ability to borrow for up to seven years[1]. Moreover, the illness underlying the need for medical care causes impaired ability to work and lowers income, further depressing a person's ability to borrow. My primary approach to addressing this issue is constructing an instrumental variable that isolates the one-way causality of credit access on households' medical expenditures.

I combine the MEPS and Experian Credit Panel by linking the MEPS respondent to the Experian data. I estimate credit access proxies using the Experian data and use the function to impute the credit access of the MEPS respondents. The credit access proxies serve two purposes in my analysis. First, it generates an unbiased estimate of credit access for the MEPS respondents. Second, it produces the county fixed effect, conveniently used as the instrumental variable for imputed credit access.

I conduct several robustness checks on the validity of the credit access proxies. These robustness checks show that the credit access proxies have similar predictive power to the full information scoring function. The distribution of the imputed credit access for the MEPS respondents resembles the distribution in the Experian data.

I use the county fixed effect generated from the credit access proxies as the instrumental variable to the MEPS respondents' imputed measure of credit access. The instrumental variable is uncorrelated with the respondents' income, age and debt, and asset information by construction. The instrumental variable also exhibits sufficient geographic and time variation that is not correlated with geographic characteristics such as population density and county average income. I conduct several

[1] as stated by one of the major official credit bureaus (https://www.equifax.com/personal/education/debt-management). In my data, I estimate the duration to be about one to two years.

robustness checks and verify the exogeneity condition of the instrumental variable to potential heterogeneity in the household preference and the county health resources.

The main result shows a significantly positive effect of credit access on households' healthcare expenditures. A higher credit score, more dollars on the credit card limit, and fewer inquiries per new loan opened significantly affect households' healthcare expenditures. The healthcare expenditures are measured by annual total medical expenditure, insurance coverage, and reduced likelihood of emergency room visits. My preferred estimate indicates that among households without access to public health insurance, a 10 points increase in the credit score, equivalent to an increase in credit rating by good credit records for a month, raises total medical expenditure by 1.305 log points on a basis of \$1,254. It also reduces the likelihood of emergency room visits by 2.59% on a base of 13.0% and improves insurance coverage by 6.59% on a base of 79.4%.

Using credit card limit as the credit access measure, I find that a log point increase in households' credit card limit raises total medical expenditure by 0.255 log points. It also reduces the likelihood of emergency room visits by 0.135 and improves insurance coverage by 0.131. All estimates are significantly stronger for low-income and young households.

I use an additional source of variation due to the credit expansion after the Great Recession of 2007 to provide evidence on the mechanism using a difference-in-difference strategy. The pro-cyclical credit expansion generated substantial variance in credit supply across US counties. I find that households in high credit supply counties are more likely to increase their healthcare expenditures. The finding also implies that the procyclical consumer credit market contributes to household healthcare heterogeneity.

Consistent with the theory, the elasticity of healthcare expenditure to credit access suggests that households' willingness to purchase insurance is independent of the insurance's affordability. I quantify the willingness to purchase and evaluate relaxing credit access as an option for public health policies. By calibrating the intertemporal Grossman model, I show that consumer finance policy such as allowing \$1,000 more credit to households improves willingness to pay by 5.9%, and health insurance coverage by 6.7% among households aged 25-35, at zero policy cost. The consumer finance policy operates on the willingness to purchase, which is different from the Medicaid expansion.

The paper directly relates to recent papers in the literature (for example, [39, 41, 28, 43, 11]) on the significant correlation between health insurance and household financial status among US households. Those papers show that uninsured households' health is significantly dependent on households' financial well-being. Low-income households heavily rely on their ability to file personal bankruptcy on medical bills to gain medical treatment instead of purchasing health insurance ([39]). Once low-income households are covered by Medicaid due to the Affordable Care Act Medicaid expansion and related reforms, their financial status is also significantly improved ([41, 28]). Consistent results have also been generated for the elderly population covered by Medicare, where once covered, elderly households get instant relief on their debts ([11]).

The paper overcomes data and identification difficulties in estimating the elasticity of the demand for household insurance and health care services ([35]) by employing both imputation and the instrumental variable approach. The results of this paper are consistent with the previous literature. For example, by studying a health shock conditionally independent of household characteristics, [17] found that the uninsured receive less health care than the insured as they are less likely to pay for the care.

As the Grossman model predicts, none credit-constrained households can insure themselves against unexpected illness ([26]). Previous studies in the consumer finance literature also provide supportive evidence, including the fact that health-related issues are among the top reasons for households being financially depressed ([8, 36, 3]), and that credit access varies across the income and wealth distribution ([19, 25, 34]). Credit scores, credit cards, and new loan inquiries correlate with each other ([54]) and reflect a household's ability to borrow.

Based on my empirical findings, my model simulation suggests that many young households are unwilling to purchase health insurance because of credit constraints. This result is comparable to previous literature on the willingness of households to purchase health insurance ([6, 20]) and on the crowding out effect of public health insurance on private insurance ([9, 2]).

The rest of the paper proceeds as follows. Section II discusses the mechanism in detail using an intertemporal Grossman model. Section III provides an overview of the data and presents the credit access proxies linking the MEPS respondents to the Experian data. Section IV discusses the identification strategy. Section V presents the main empirical results. Additional evidence using over-time variation during the credit expansion after the Great Recession is discussed in Section VI. Section VII calibrates the intertemporal Grossman model and provides policy evaluation. Section VIII concludes.

1.2 Theory characterization

I derive my hypothesis from an intertemporal Grossman model with a borrowing constraint. Adopting the classic Grossman model setting ([26]), I assume households enjoy health as a good and use it as human capital. Consider an agent who lives for T years and plans for his lifetime consumption, saving, and level of health. Therefore the agent solves the following intertemporal problem:

$$\max_{b_t, h_t} \sum_{t=t_0}^{T} \beta^t E_{t_0}[u(c_t, h_t)] \tag{1}$$

$$\text{s.t. [budget constraint] } c_t + I'p + (1 - Ik)m(h_{t-1}, x_t, h_t) + \frac{b_{t+1}}{1+r} = b_t + w(h_t, y_t) \tag{2}$$

$$\text{[terminal condition] } b_{T+1} = 0 \tag{3}$$

$$\text{[borrowing constraint] } b_{t+1} \geq \underline{b}(w_t) \tag{4}$$

where c_t denotes the agent's consumption at age t, h_t denotes how healthy he is at t, b_t denotes his net asset (saving when $b_t > 0$ or borrowing when $b_t < 0$), r denotes the interest rate, m denotes the medical expenditures and cost of health insurance, and w denotes the income the agent earns.

x_t is the health shock that hits the agent annually t of unexpected different degrees. It represents illness (when $x_t > 0$) and self-resolved recovery from the illness (when $x_t < 0$). Given the shock x_t, the health level the agent has been before the shock h_{t-1}, and the health level the agent would like to maintain after the shock h_t, he must choose how much medical treatment he needs and incur a medical bill of $m(h_{t-1}, x_t, h_t)$. Lastly, $I \in \{0, 1\}$ represents the health insurance status, and k is the parameter for the copay.

First, I discuss the scenario when there is no health insurance available. Without the borrowing constraint (4), a rational agent would solve the above intertemporal problem of only (1)-(3). The agent would borrow money from a bank when his income is lower than his average lifetime income and save his money when his income is higher than his lifetime income. If the agent sufficiently cares about his health (in the utility function), he would always fully insure his health and pay for necessary medical care when hit by illness by spending his savings or borrowing more from a bank. In other words, the agent would perfectly smooth his consumption and health.

However, even a rational agent cannot fully insure his health when a borrowing constraint is present if his income and credit limit is low and the borrowing constraint is binding. It can be seen from solving the intertemporal problem of (1)-(4). The Lagrangian intertemporal decision when $t < T$ is:

$$\lambda_{t+1} = \frac{\lambda_t}{1+r} - \eta$$

with respect to b_t, and

$$\begin{aligned} u_2'(h_t) &= \lambda_t[m_3'(h_t) - w_1'(h_t)] - \underbrace{\lambda_{t+1}}_{=\frac{\lambda_t}{1+r}-\eta<\frac{\lambda_t}{1+r}} m_1'(h_t) \\ &> \lambda_t[m_3'(h_t) - w_1'(h_t)] - \frac{\lambda_t}{1+r} m_1'(h_t) = u_2'(h_t^{unconstrained}) \end{aligned}$$

with respect to h_t. I impose the standard assumption that the utility function $u(\cdot)$ is continuously differentiable and concave. Hence

$$h_t < h_t^{unconstrained}$$

where λ and η are positive Lagrangian multipliers on constraints (2) and (4), respectively. Therefore, the agent cannot borrow as much as he wants when the borrowing constraint is binding from below. The constrained optimal h_t^* is lower than unconstrained perfect health, which means the constrained agent cares less about their current health and spends less on medical care despite illness.

The scenario with health insurance has a similar result. I will discuss that scenario with the calibrated version of the model in Section 1.6.

1.3 Data

My main analysis examines the causal effect of households' credit access on their healthcare expenditures. To this end, I require information on household-level medical costs and financial status. I use the MEPS as my primary dataset. The MEPS not only contains the required information on the dependent and explanatory variables, but its residence information is also necessary for my instrumental variable design of the causality identification, as discussed below.

The MEPS is a rotating panel with survey waves introduced each year and followed for a short two-year period. It has publicly available medical costs, insurance coverage, and restricted-access information on wealth and state of residence. I use the data from 2006-2016, which contains significant macroeconomic and policy changes from the 2007 economic recession and the 2014 health insurance reform.

My analysis requires another dataset to supplement the MEPS because of two reasons. First, the MEPS financial status data only contains the asset and debt information of the respondents, which is not equivalent to their credit access, e.g., how much these respondents can borrow on their credit cards. Second, I expect the reverse causality to be strong, as unpaid healthcare expenditures will show on households' credit history and affect their ability to borrow[2]. Therefore, the causality of interest cannot be identified using only the information in the MEPS data.

The second dataset I use is the Experian credit report panel, which sufficiently complements the MEPS to identify the causality of interest. The Experian dataset is a panel that records individual persons' credit history by quarter. It is a US nationwide representative data[3]. Its original purpose is to provide a basis for banks

[2]In many cases, the unpaid medical bill will be directly sent to collection. It will then be recorded in households' credit history and affect how much they can borrow in the next few months.

[3]The Experian dataset only contains individuals who have obtained at least some loan in their

and lending facilities to evaluate new loan applicants' creditworthiness. The lenders can investigate their debt portfolio and payment history at the application stage and evaluate the likelihood of non-repay. I use the data from 2006-2016, matching the data from the MEPS.

I use the Experian dataset to estimate credit access proxies, a reduced version of loan lenders' credit evaluation process. My estimated credit access proxies are based on factors also present in the MEPS, producing a "credit score", which measures how much a person can borrow from a bank or a lending facility. I then apply these credit access proxies to impute the credit access of the MEPS respondents. Finally, I use county-level Experian measures of credit access as an instrumental variable to the credit access of the MEPS respondents.

1.3.1 Measuring the credit access of MEPS respondents

The MEPS does not contain direct information on respondents' credit access. I impute the credit access of each MEPS respondent by comparing their debt information to the Experian data, where both debt information and ability to borrow are observed. Specifically, I use the Experian data to estimate credit access proxies, and then I use these to impute the credit access of the MEPS respondents. As the original purpose of the Experian data is to provide the basis for banks and lending facilities to evaluate the creditworthiness of new loan applicants, It is expected that my credit access proxies to be highly predictive. However, it uses a smaller set of debt information than the actual credit evaluation process because of the limited

past, whether it is a credit card, mortgage, student loan, auto loan, or unpaid medical bills. However, the fraction of the population without any debt history is tiny (about 10% of the total US population in 2005). It is common practice to consider the data as nationally representative. See [10] for reference.

MEPS debt information. As discussed below, the robustness of this measurement approach is carefully verified despite the smaller set of information.

Five factors in the credit record affect the creditworthiness of an individual, including utilization of credit balance, delinquency or default history, age of the credit record, diversity of the debt portfolio, and inquiries for new loans. All factors are present in the Experian data. In the MEPS data, only the actual balances of the significant debt types are reported. Nevertheless, the imputation predictive power remains strong using the limited MEPS debt information, as I discuss below.

I use the Experian data to estimate credit access proxies for the MEPS respondents. The dependent variables are direct measures of credit access, and the explanatory variables are the debt information that appears in the MEPS and the Experian datasets. For a person i at time t, the credit access proxies is:

$$\begin{aligned} CreditAccess_{i,t} =& (\beta_1 MortgageBalance_{i,t} + \beta_0)\mathbb{1}\{MortgageBalance_{i,t} > 0\} + \\ & (\gamma_1 AutoLoanBalance_{i,t} + \gamma_0)\mathbb{1}\{AutoLoanBalance_{i,t} > 0\} + \\ & (\theta_1 UnsecuredLoanBalance_{i,t} + \theta_0)\mathbb{1}\{UnsecuredLoanBalance_{i,t} > 0\} + \\ & \kappa Income_{i,t} + \lambda Age_{i,t} + CountyFE_{i,t} + c + \epsilon_{i,t} \end{aligned} \tag{5}$$

where $CreditAccess_{i,t}$ is one of the three measures of households' credit access that I will focus on, which are the log of credit card limit, the ratio of the number of inquiries to newly opened loans (namely the failure rate), and the credit score. Among the dependent variables, $MortgageBalance_{i,t}$ is the log of person i's mortgage balance, $AutoLoanBalance_{i,t}$ is the log of their car loan balance, $UnsecuredBalance_{i,t}$ is the log of their unsecured debt balance including credit cards, student loan, and medical bills, $Age_{i,t}$ and $Income_{i,t}$ are person i's log income and age[4], and $CountyFE_{i,t}$ is the

[4]income and age are not directly used in the three major credit scoring bureaus' official credit scoring process. However, they are used in most loan lenders' application reviewing process, and they strongly correlate with other used factors. Here in Function 5, they serve also as proxies for

county-level time-trend fixed effect. c is the constant term representing the baseline credit access of a young person with no prior credit history and zero income. Lastly, $\epsilon_{i,t}$ is the error term caused by the rest of the credit scoring factors missing from the MEPS dataset that are not directly observed in the MEPS. I estimate Function 5 using the OLS.

Table 1 reports the estimated credit access proxies from the Experian data and compares it to a complete factors version from the same sample. As shown in Columns (1), (3), and (5), all three measures significantly depend on the debt, income, and age information. Specifically, all three measures strongly depend on a person's income and age. For a log increase in a person's income, that person has, on average, a 135.1 points higher credit score, a 1.783 log increase in their credit card limit, and a -0.0465 decrease in the number of inquiries per newly opened loan. Columns (2), (4), and (6) report the estimation results of the complete information credit access proxies that also include the delinquency and default history missing from the MEPS. Comparison of Columns (1), (3), (5) to Columns (2), (4), (6) shows that including the MEPS-missing factors in the regressions does not change the significance nor the magnitude of the estimated parameters on the MEPS-existing factors. However, the missing information on delinquency and default history also plays a critical role in affecting credit access. I further examine these missing factors below.

Figures 1a to 1c depict the variation in the credit access measures by income and age in the Experian sample. As shown in Figure 1a, the higher the income, the less likely the person will miss a payment on their borrowing, thus the more that person can borrow from a bank or lending facilities. The age 21-30 population

missing factors that are used, i.e., age of credit history and delinquency and default flags. See [46] for reference.

Table 1: Regression: the credit access proxies on the MEPS debt factors. Dependent variables are on the column names.

Model: OLS	Score (1)	Score (2)	Limit (3)	Limit (4)	Ratio inquiry (5)	Ratio inquiry (6)
Log mortgage	-20.03***	-18.54***	-0.0746***	-0.0606***	0.568***	0.567***
	(0.0390)	(0.0352)	(0.000551)	(0.000531)	(0.0260)	(0.0260)
Has mortgage	231.0***	214.7***	0.903***	0.749***	-7.093***	-7.076***
	(0.454)	(0.410)	(0.00644)	(0.00621)	(0.304)	(0.304)
Log auto loan	-13.77***	-13.58***	-0.0994***	-0.0964***	0.310***	0.304***
	(0.0356)	(0.0327)	(0.000525)	(0.000507)	(0.0233)	(0.0234)
Has auto loan	126.0***	124.4***	0.890***	0.870***	-2.666***	-2.624***
	(0.337)	(0.309)	(0.00499)	(0.00482)	(0.223)	(0.223)
Log unsecured loan	-12.66***	-11.09***	0.153***	0.165***	-0.113***	-0.124***
	(0.0113)	(0.0133)	(0.000175)	(0.000171)	(0.00812)	(0.00846)
Has unsecured loan	153.4***	148.2***	-0.0432***	-0.0954***	1.270***	1.236***
	(0.104)	(0.0983)	(0.00201)	(0.00198)	(0.0925)	(0.0925)
Log income	135.1***	118.7***	1.783***	1.671***	-0.0465	-0.0464
	(0.0483)	(0.0515)	(0.000774)	(0.000766)	(0.0370)	(0.0387)
Age	-0.155***	-0.0870***	0.00310***	0.00390***	-0.0157***	-0.0155***
	(0.00152)	(0.00143)	(0.0000237)	(0.0000232)	(0.00116)	(0.00116)
Bank card utilization		-0.438***				0.252***
		(0.119)				(0.0389)
Past due flag		-116.4***		-1.117***		-1.224***
		(0.118)		(0.00196)		(0.125)
Default flag		-78.20***		-0.822***		-1.245***
		(0.0865)		(0.00208)		(0.120)
County-year fixed effect	Y	Y	Y	Y	Y	Y
Constant	-845.0***	-663.7***	-11.82***	-10.61***	50.46***	50.55***
	(0.488)	(0.523)	(0.00803)	(0.00796)	(0.382)	(0.401)
R^2	0.440	0.520	0.459	0.487	0.00184	0.00232
Observation	22755850	22755850	16426999	16426999	879283	879283

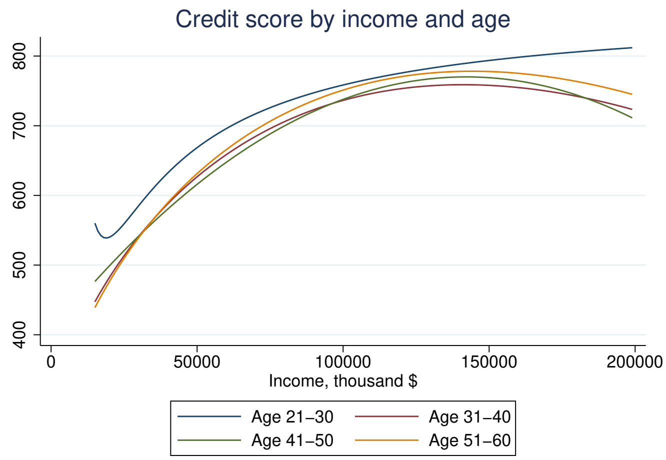
Note: * denotes 10% significance level. ** denotes 5%, and *** denotes 1%. Standard errors are in parentheses. The score is the Vantage credit score. The credit limit is the log of the credit card limit. Ratio inquiry is the ratio of inquiries to newly opened loans. The data source is the Experian consumer credit panel 2006-2016.

is concentrated on the lower end of the income axis and has lower average credit scores. As shown in Figure 1b, the credit card limit also positively depends on income and age. Figure 1c shows the negative dependency of inquiries per new loan on income and age. The age 21-30 population remains an outliner because the young population has a higher demand for new loans, such as car loans or home mortgages.

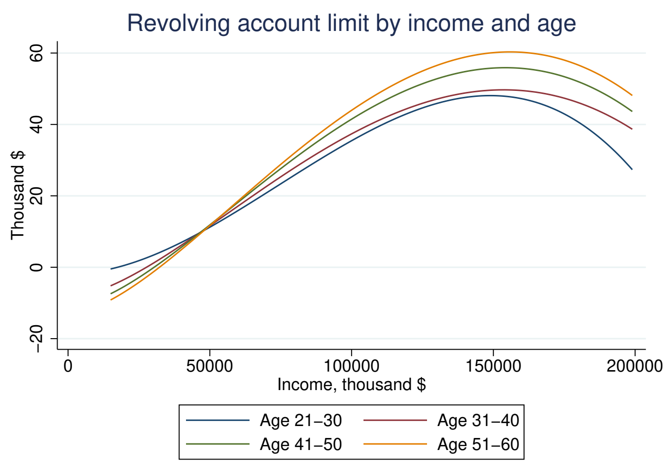
One of the critical and yet missing factors from Function 5 is person i's past delinquency and default history on any debt for the past year. Other significant credit evaluation factors, including debt balance, income, and age information, are also observed in the MEPS and used in Function 5. A person with a default flag will be punished by a lowered score that causes a more unfavorable reviewing process for a new loan and less favorable loan terms. Nevertheless, I present three reasons that confirm the robustness of using the prediction Function 5 in my analysis:

First, the debt balance, income, and age information significantly correlate with the MEPS-missing past default history. Indeed, Table 17 in the Appendix shows the significant correlation between delinquency and default history and the debt balance, income, and age information in the Experian data. As shown in Table 17, delinquency and default history are significantly negatively dependent on a person's income and age, and delinquency is also significantly positively dependent on a person's unsecured loan balances. Because of such a high correlation, I expect the variance of the OLS estimation of Function 5 to be close to the variance of the complete information version of Function 5.

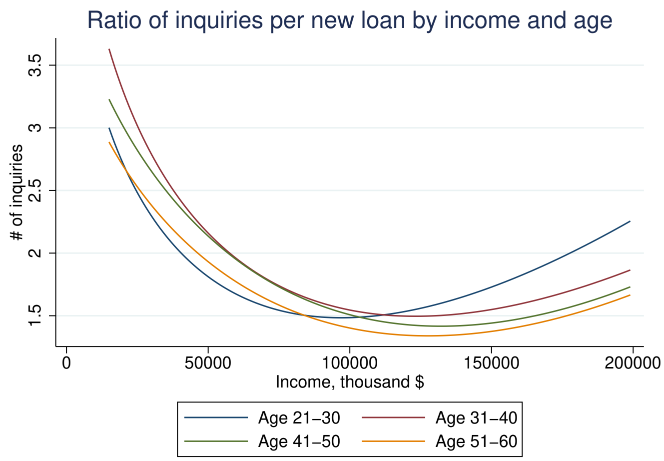
Second, the predictive power of Function 5 without the missing default history is close to the full-information function with the default history. We see this by comparing Columns (1) to (2), (3) to (4), and (5) to (6) in Table 1. Figure 12 in the Appendix compares the predicted residuals from these two versions of the regression. As shown in Figure 12, the predicted residuals are close. Therefore Function 5 has a



(a) Credit score.



(b) Credit card limit.



(c) Number of inquiries per new loan opened.

Figure 1: Variation in credit card limit by income and age, as a visualization of the estimation sample of Table 1. A random subsample of $n = 1000$ points.

good predictive power close to the full information version.

Third, I compare the imputed credit access for the MEPS respondents to the credit access data of the Experian sample. The credit access measures in the two datasets have similar distributions. The MEPS sample has a slightly left-shifted wealth distribution than the Experian sample, as shown in Table 18 in the Appendix. Taken together, I conclude that Function 5 has good predictive power on the MEPS data close to the full information version on the Experian credit panel data.

1.3.2 Summary statistics

I conduct my primary analysis at the individual level because the Experian data is individual-level. I use the household annual total income to measure income, as it is more accurate in credit scoring. I exclude from the baseline sample the MEPS respondents with public insurance, including Medicaid, or those eligible for Medicare (i.e., age 65 or older). I also drop the first year of each survey because the MEPS debt information is only collected in the second year. After data restrictions, there are 49042 MEPS respondents and 49042 respondent-year observations from 2006-2016 in my primary analysis.

I focus on three outcome variables that can be affected by households' restricted credit access as predicted by my model: annual healthcare expenditures, emergency room visits, and private health insurance coverage. As predicted by my model, the impact of credit access can vary significantly by income and age. Therefore, I study the effect on the entire sample and the effect on the low-income households defined in the MEPS as households with income below the 200% Federal Poverty Line.

Table 2 presents the summary statistics for my main analysis. Compared to the actual distributions of credit score, credit card limit, and ratios of inquiries in

Table 2: Summary statistics: outcome and explanatory variables in the MEPS.

Variables	Mean	Std.	p25	p50	p75	min	max
Healthcare charge	4983.044	22894.96	0	451	2358	0	1279622
Emergency payment	2969.875	11618.53	0	438	2096	0	580640
Emergency charge	363.5075	2687.727	0	0	0	0	251732
Insurance coverage	.7151041	.4513688	0	1	1	0	1
Imputed credit score	667.0866	146.1467	582.6449	668.9729	746.8567	-631.905	1292.836
Imputed card limit	8.272226	1.733487	7.264579	8.424769	9.443541	-8.863572	13.64017
Imputed ratio of inquiries	49.01146	.7589892	48.67327	49.21355	49.52932	40.53638	50.92411
Income	10.93447	.8797518	10.4631	11.02674	11.51333	1.609438	13.38468
Age	41.35048	12.15981	31	42	52	21	69
Observation	49042						

the Experian data, the imputed distribution for the MEPS respondents is slightly skewed to the left. It matches the fact that the MEPS respondents also report a left-skewed household income distribution. Before conducting my analysis, I drop the two-sided fraction of prediction errors (1% on either side) which contains those with an imputed credit score below 300 or above 900, a negative or too high imputed credit card limit, or a negative or too high imputed number of inquiries.

1.4 Empirical strategy

I discuss my empirical strategy to examine the central predictions of the mechanism. The mechanism is that households with limited ability to borrow are more likely to postpone non-emergency healthcare expenditures to maintain a smooth consumption of non-healthcare goods. This mechanism is particularly enhanced by the strong correlation between income and the ability to borrow and, therefore, can be more evident among low-income or young households.

Several identification challenges need to be addressed in my empirical strategy. First, imputation error exists in the independent variables of interest, affecting the estimation accuracy. Omitted variables also affect households' healthcare decisions and their ability to borrow. Examples include the households' unobserved heterogeneous preferences, as shown in my theoretical model. Second, there is solid reverse causality, as households' unpaid medical bills will show up in their credit report and affect their ability to borrow. Third, measurement errors can occur in the survey data as households may not report their medical expenses or debt balances. Studies have shown that many households under-report their actual credit card debt in survey data such as the Survey of Consumer Finance ([10]). This measurement error is not present in the administration of Experian data. Finally, the loan term offered by the lending facilities may reflect factors other than the likelihood of the borrowers' non-repay and thus obscure the true borrowing constraint that a low-income household faces.

To address these identification challenges, I construct an instrumental variable from the Experian dataset representing the credit supply in the county where a household resides. The county credit supply directly affects households' ability to borrow. Theoretically, it can only affect their healthcare expenditure through borrowing, conditional on the households' income and the county's average income. I follow [39, 16] and construct a simulated instrument by taking the entire sample of households and calculating the mean credit access as though this sample faced the credit supply in each county. The mean credit access is equivalent to the county fixed effect in the credit proxy estimation. I compute this instrumental variable by taking out the county-level time-trended fixed effect of county j at time t from Function 5. For household i, this instrumental variable is:

$$z_{j,t} = \frac{1}{|\mathbb{N}|} \sum_{i \in \mathbb{N}} CreditAccess_{i,j,t} \sim CountyFE_{j,t} \tag{6}$$

which represents the county credit supply to the MEPS households i at time t. By construction, the variable is independent of households' debt, income and age, and the county average income and age.

1.4.1 Constructing the instrumental variable

The instrumental variable is the county credit supply to a MEPS respondent, which I argue only indirectly impacts their healthcare expenditures through their ability to borrow, conditional on a whole set of control variables, including income, age, and geographic and time-fixed effects. Therefore, it is a valid instrumental variable to study the impact of households' ability to borrow on their healthcare expenditures. I will examine the robustness of my instrumental variable design below in Section 1.5.5.

The credit supply to a MEPS respondent is not directly observed in the MEPS data. Therefore, I construct the instrumental variable by matching a MEPS respondent to the county they reside in and use the estimated county-level time-trended fixed effect of the Experian credit access in the county as the measure of the credit supply to the MEPS respondent. I use the restricted-use MEPS geographic identifier to match the two data sources.

Figure 2 shows the variation in the instrumental variable across the US counties map. The map shows that credit access does not correlate with geographic or demographic characteristics and contains substantial heterogeneity within a state.

Table 19 in the Appendix presents the summary statistics of these county-level instrumental variables.

1.4.2 Instrumental variable model

The first-stage equation for person i that resides in county j in time period t is given by:

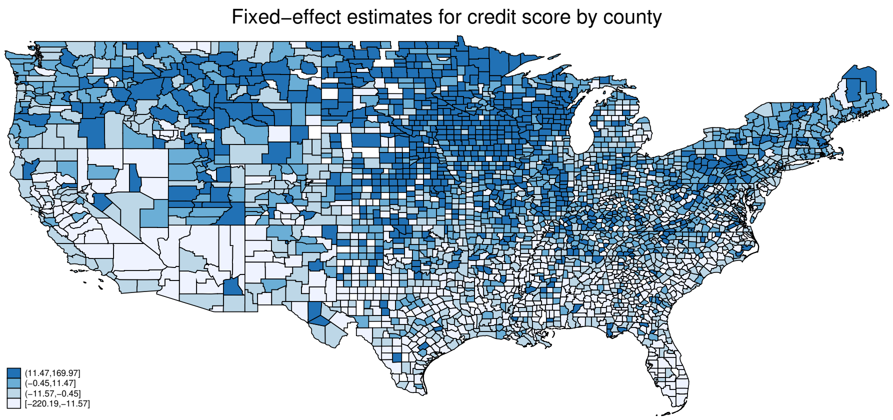
$$Access_{i,j,t} = \alpha_C z_{j,t} + \alpha_I Income_{i,t} + \alpha_A Age_{i,t} + \alpha_t + \alpha_{state} + \xi_{i,t} \tag{7}$$

where $Access_{i,j,t}$ is the credit access measure of person i, $z_{j,t}$ is the instrumental variable that is the mean credit access of county j that i resides, $Income_{i,t}$ and $Age_{i,t}$ are person i's income and age, and α_t and α_{state} are the time and state fixed effects. Note that within a county there is no variation of the instrumental variable, and therefore county fixed effect is dropped from the model. $\xi_{i,j,t}$ is the error term.

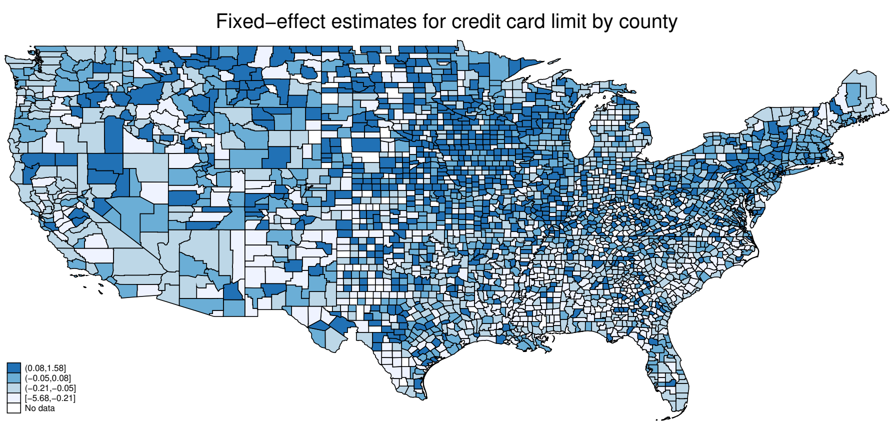
The second-stage equation for person i's healthcare expenditure $Expenditure_{i,j,t}$ is given by:

$$Expenditure_{i,j,t} = \beta_A \widehat{Access}_{i,j,t} + \beta_I Income_{i,t} + \beta_A Age_{i,t} + \beta_t + \beta_{state} + \nu_{i,t} \tag{8}$$

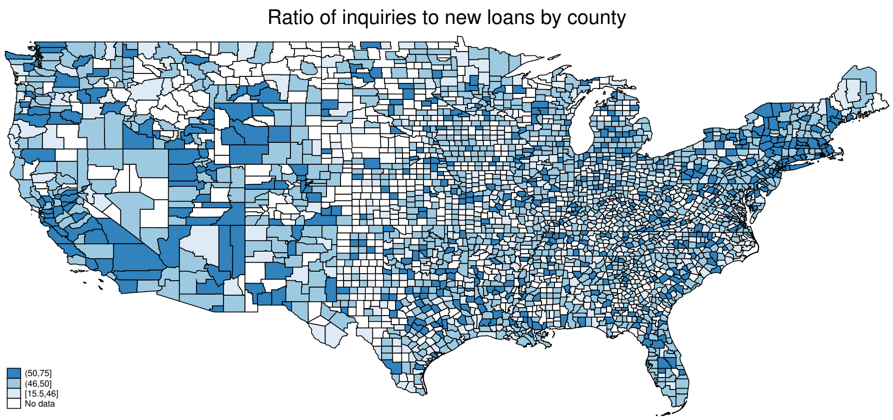
where $\widehat{Access}_{i,j,t}$ is person i's instrumented credit access measure from the first stage. β_t and β_{state} are the time and state fixed effects. $\nu_{i,j,t}$ is the error term. I estimate the model on medical expenses by the 2SLS. To study the effect on emergency visits and insurance coverage, I use an IV-probit functional form and maximum likelihood instead.



(a) Credit score.



(b) Credit card limit.



(c) Number of inquiries per new loan opened.

Figure 2: Choropleth graphs for the instrumental variables. The higher degree of color darkness represents a higher level of credit access in the county.

I examine the effect on annual healthcare expenditures by regressing the log annual total medical expenses on the three measures of credit access. The credit score and credit card limit measures are direct credit access measures. The ratio of inquiries to new loans is more representative among middle-income households. My primary analysis focuses on non-public insured households with non-zero medical expenses, and I also study the effect on low-income or young households.

I examine the effect on emergency room visits by regressing whether households with positive medical expenses have visited an emergency room in the same year. The logic is that unconstrained households should be more likely to use non-emergency medical care than the emergency room. I use a probit functional form in the preferred specification because the dependent variable is unitary. I also study the effect on low-income or young households.

I examine the effect on private health insurance coverage by regressing whether households are insured on the three measures of credit access. I use a probit functional form in the preferred specification because the dependent variable is unitary. I exclude households eligible for public insurance from the baseline sample and study the effect on low-income or young households.

1.5 Main result

I present my main result using the instrumental variable design and several robustness checks on this design. In the following section, I also present supplement results using different approaches.

1.5.1 First stage

I start by presenting estimates of the implied first stage. Table 3 shows the first-stage estimation result of the effect of county credit access on a MEPS respondent's credit access. Column 1 shows the fixed effect of the county credit score on the respondent's credit score. Column 2 and Column 3 show the effect of the county credit card limit FE, and the county number of inquiries FE on the respondent's credit card limit and the number of inquiries per new loan opened, respectively. Because the IV is taken directly from the credit access proxies 5, the first stage is robust, with an F-statistic above 100 in all specifications. For all three credit access measures, the instrumental county fixed effects coefficients are significant at the 1% level with a positive sign, suggesting a strong positive correlation between the county credit access supply and the resident's credit access.

1.5.2 Effect on annual total medical expenditure

My sample includes households without access to public health insurance. There are two types of households in my sample: those that are financially constrained and those that are not. The borrowing constraint varies by household age and income and is more likely to bind for lower-income and young households. Wealthy households are less likely to be credit-constrained; therefore, their non-binding credit access should have little impact on their healthcare expenditures. On the other hand, low-income or young households are more likely to have binding borrowing constraints. The limited credit access will significantly impact their non-essential healthcare expenditures.

Therefore, I expect a significant effect on those MEPS respondents with a binding constraint, varying degrees depending on how binding the credit constraint is. To

Table 3: First stage: regression of individual's credit access on their county credit access.

Dependent variables are on the column names.

First stage: OLS	Credit score (1)	Credit card limit (2)	Ratio of inquiries (3)
County FE for credit score	0.991***		
	(0.0284)		
County FE for credit card limit		0.878***	
		(0.0788)	
County FE for ratio of inquires			1.018***
			(0.0151)
Self-reported health control	Y	Y	Y
Insurance coverage control	Y	Y	Y
Income and age controls	Y	Y	Y
Year and county fixed effects	Y	Y	Y
Constant	Y	Y	Y
F-statistic	89437	9427.89	16399
R^2	0.995	0.951	0.966
Observation	76918	76923	16399

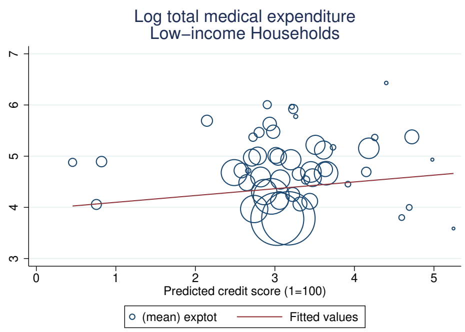
Note: * denotes 10% significance level. ** denotes 5%, and *** denotes 1%. Standard errors are in parentheses. The credit score is the imputed MEPS respondent's credit score. The credit card limit is the log of the imputed MEPS respondent's credit card limit. The ratio of inquiry is the imputed MEPS respondent's ratio of loan inquiries over new loans opened. The data source is the MEPS panel 2006-2016.

narrow the local treatment effect on those with a binding constraint, I further study the sub-sample defined in MEPS as low-income[5].

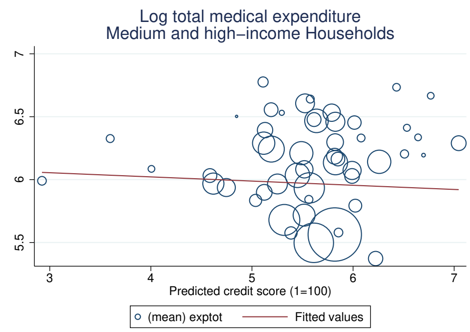
Figures 3a to 3f show the cross-state correlation between medical expenditure and credit access measures. To account for the tightness of the credit constraint, I split the sample into households with income below or above the MEPS low-income level. The data are averaged by state, with circles proportional to the number of observations. Figure 3a and 3b show the correlation between the log total medical expenditure and the imputed credit score. An upward-sloping correlation in the low-income group is consistent with the binding credit constraint effect on medical expenditure. The downward-sloping correlation in the entire sample reflects the potential reverse causality and wealth effect. Figures 3c to 3f show the correlation between the log total medical expenditure, the imputed credit card limit, and the number of inquiries per new loan. They have similar patterns as in Figures 3a and 3b. These graphs suggest a strongly negative effect of the binding credit constraint on medical expenditure, especially among low-income households. However, as shown in the following estimation table, the binding credit constraint effect is also significant in the entire sample after controlling the reverse causality and wealth effect.

Table 4 presents the regression estimates of the effect on annual total healthcare expenditure. Panel a uses credit score as the credit access measure, and Panel b and c use credit card limit and the number of inquiries per new loan opened, respectively. Across all panels, the estimation results show a robust causal effect of households' credit access on healthcare expenditures. Columns (1) and (3) show the OLS regression, which measures the correlation between total healthcare expenditure and credit access, controlling for households' total medical charge, income, age, and state and year fixed effects. Columns (2) and (4) show the instrumental variable regression,

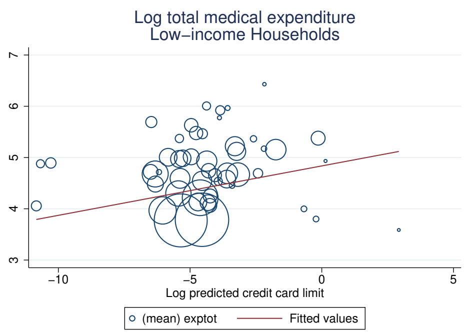
[5]Those are the households with income below 200% of the Federal Poverty Line



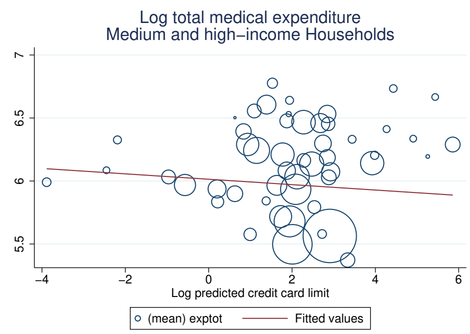
(a) Line fit: credit score, low-income.



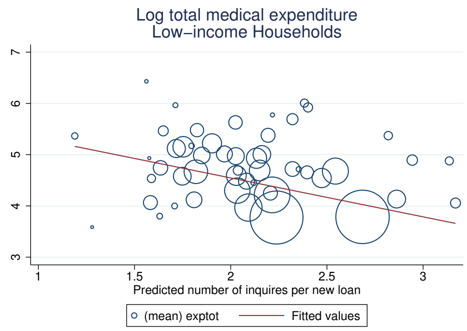
(b) Line fit: credit score, high-income.



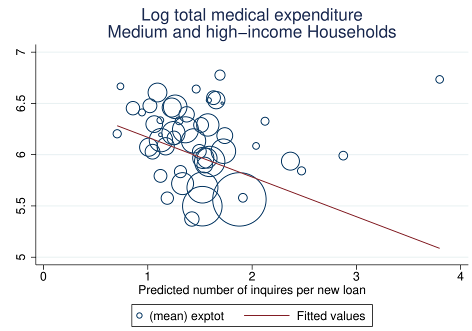
(c) Line fit: credit card limit, low-income.



(d) Line fit: credit card limit, high-income.



(e) Line fit: number of inquiries per new loan, low-income.



(f) Line fit: number of inquiries per new loan, high-income.

Figure 3: Effect of credit access on annual total medical expenditure, sample plots.

Notes: Panels a and b plot log total medical expenditure against the predicted credit score by the state for low- ($\leq$ 200%FPL) and medium and high- ($\geq$ 200%FPL) income households. Panel c and d plot the total medical expenditure against the state's log predicted credit card limit for low- and medium- and high-income households. Panels e and f plot log total medical expenditure against the state's predicted number of inquiries per new loan opened for low- and medium- high-income households. The circles in Panels a-f are proportional to the number of observations in each state. Pooled 2007-2016 MEPS, excluding individuals with public insurance or age 65 or older.

which measures the causal effect of credit access on healthcare expenditure. Columns (1) and (2) show an estimation of the entire sample aged 25-71 who are ineligible for public health insurance, and Columns (3) and (4) show the sub-sample of low-income households.

As shown in Table 4, all three measures of credit access have a robust causal effect on households' medical expenses. Panel a shows an economically significant effect of households' credit access, measured by the imputed credit score, on the medical expenditure for households with income below 40% national level. The IV estimate in Column (2) indicates that a 100 points increase in the credit score, equivalent to an increase in credit rating rank (sub-prime, below prime, prime, above prime), raises total medical expenditure by 1.305 log points on a base of $1,254. The estimates are much smaller in the OLS specifications. The estimate is even stronger for the low-income sub-sample in Column (4), at 1.547 log points increase of medical expenditure for a 100 points increase in the credit score.

Panel b shows an economically significant effect of credit access on medical expenditure, where the imputed credit card limit measures credit access. The effect is significant for the entire sample but insignificant for the low-income sub-sample. The IV estimate in Column (2) indicates that a log point increase in households' credit card limit raises total medical expenditure by 0.255 log points on a base of $1,270. The estimate is much smaller in the OLS specifications.

Panel c shows a significant effect of credit access on medical expenditure, measured by the imputed number of inquiries per new loan opened. The effect is significant for the entire sample but insignificant for the low-income sub-sample. The IV estimate in Column (2) indicates that one additional inquiry per new loan opened reduces total medical expenditure by 0.456 log points on a base of $1,287. The estimate is slightly larger in the OLS specifications.

Table 4: Effect on medical expenditure: regression of medical expenditure on credit access.

The dependent variable is the log of the amount of annual total medical expenditure.

Second stage: 2SLS	OLS, full sample (1)	IV, full sample (2)	OLS, low-income (3)	IV, low-income (4)
Panel a: credit access measure is imputed credit score				
imputed credit score	0.540***	1.305***	1.083***	1.547***
	(0.0429)	(0.185)	(0.0945)	(0.327)
Mean medical expenditure	$1,254	$1,254	$1,254	$1,254
Health and insurance controls	Y	Y	Y	Y
Age and income controls	Y	Y	Y	Y
Year and state fixed effects	Y	Y	Y	Y
R^2	0.215	0.208	0.216	0.214
Observations	76,918	76,918	87,760	87,760
Panel b: credit access measure is log imputed credit card limit				
imputed credit card limit	0.119***	0.255***	0.185***	0.0377
	(0.00566)	(0.0941)	(0.0127)	(0.127)
Mean medical expenditure	$1,270	$1,270	$1,270	$1,270
Health and insurance controls	Y	Y	Y	Y
Age and income controls	Y	Y	Y	Y
Year and state fixed effects	Y	Y	Y	Y
R^2	0.221	0.209	0.224	0.213
Observations	76,923	76,923	87,765	87,765
Panel c: credit access measure is the imputed number of inquiries per new loan opened				
imputed number of inquiries	-0.586***	-0.456***	-1.035***	-0.436
	(0.0506)	(0.144)	(0.128)	(0.288)
Mean medical expenditure	$1,287	$1,287	$1,248	$1,248
Health and insurance controls	Y	Y	Y	Y
Age and income controls	Y	Y	Y	Y
Year and state fixed effects	Y	Y	Y	Y
R^2	0.214	0.214	0.212	0.210
Observations	71,387	71,387	85,507	85,507

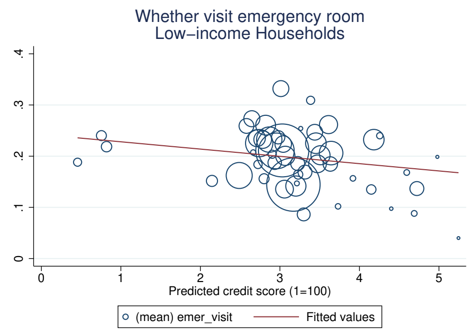
Note: * denotes 10% significance level. ** denotes 5%, and *** denotes 1%. Standard errors are in parentheses. Low-income households are households with annual income below 200% Federal Poverty Line, as defined in the MEPS. The credit score is the imputed MEPS respondent's credit score. The credit card limit is the log of the imputed MEPS respondent's credit card limit. The ratio of inquiries is the imputed MEPS respondent's number of loan inquiries per new loan opened. The data source is the MEPS panel 2006-2016.

1.5.3 Effect on emergency room visits

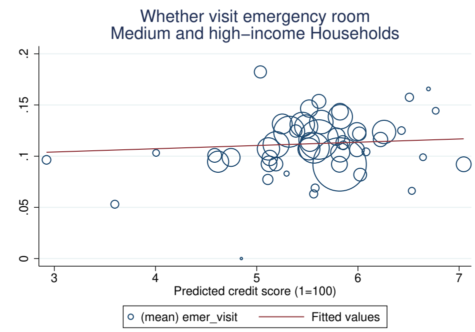
I turn to the estimation result on emergency room visits. The raw data of the number of emergency room visits is highly skewed, with more than 70% of MEPS respondents not visiting in a year, while 5% have visited more than ten times per year. Because of this, I measure emergency room visits by a zero-one indicator of whether a person has chosen to visit an emergency room, conditional on the person having positive medical charges.

Figures 4a to 4f show the correlation between emergency room visits and credit access measures. To account for the tightness of the credit constraint, I split the sample into households with total income below or above the 200% poverty line. The data are averaged by state with circles proportional to the number of observations. Note that among medium and high-income households and for the credit score (Figures 4a and 4b) and the number of inquiries per new loan (Figures 4e and 4f) the correlation is positive. At the same time, my theory predicts a negative causal effect. The positive correlation indicates a strong reverse causality and wealth effect, as emergency room visits usually imply impaired work ability.

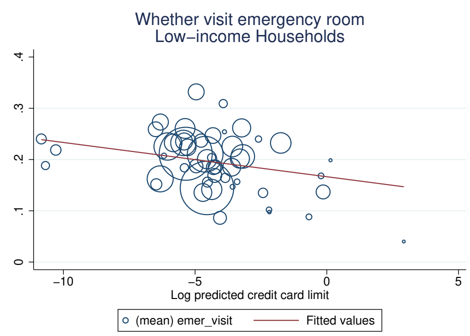
Table 5 presents the regression estimates of the effect on emergency room visits, conditional on positive medical charges. Across all panels, the estimation results show a robust causal effect of households' credit access on their likelihood of visiting an emergency room. Columns (1) and (3) show the OLS regression, which measures the probability measure transformed correlation between emergency room visits and credit access, controlling for households' total medical charge, income, age, and state and year fixed effects. Columns (2) and (4) show the instrumental variable regression, which measures the causal effect of credit access on emergency room visits. Columns (1) and (2) show an estimation of the entire sample aged 25-71 who are ineligible for



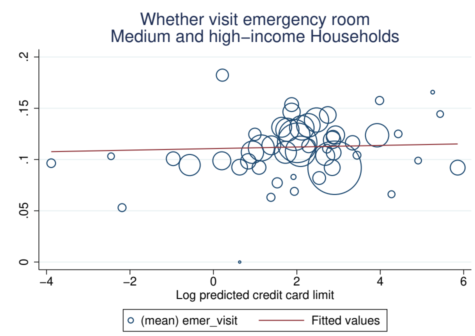
(a) Line fit: credit score, low-income.



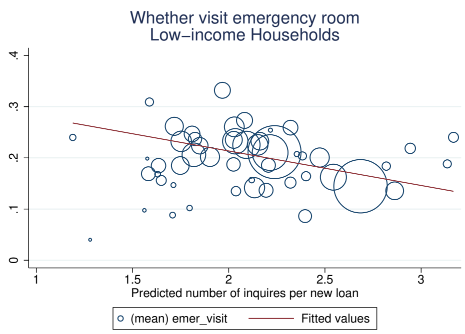
(b) Line fit: credit score, high-income.



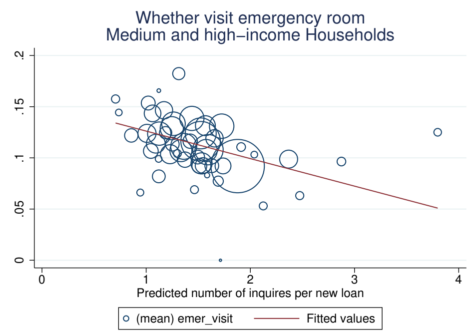
(c) Line fit: credit card limit, low-income.



(d) Line fit: credit card limit, high-income.



(e) Line fit: inquiries per new loan, low-income.



(f) Line fit: inquiries per new loan, high-income.

Figure 4: Effect of credit access on emergency room visits, sample plots.

Notes: Panels a and b plot the fraction of the population with emergency room visits against the predicted credit score by the state for low- ($\leq$ 200%FPL) and medium and high- ($\geq$ 200%FPL) income households. Panels c and d plot the fraction of the population with emergency room visits against the state's log predicted credit card limit for low- and medium- high-income households. Panels e and f plot the fraction of the population with emergency room visits against the predicted number of inquiries per new loan opened by the state for low- and medium, and high-income households. The circles in Panels a-f are proportional to the number of observations in each state. Pooled 2007-2016 MEPS, excluding individuals with public insurance or age 65 or older.

public health insurance, and Columns (3) and (4) show the sub-sample of low-income households.

As shown in Table 5, all three measures of credit access have a robust causal effect on households' likelihood of emergency room visits. Panel a shows an economically significant effect of credit access, measured by the imputed credit score, on emergency room visits for the entire sample and the sub-sample of low-income households. The IV estimate in Column (2) indicates that a 100 points increase in the credit score, equivalent to an increase in credit rating rank (sub-prime, below prime, prime, above prime), reduces the likelihood by 0.259 on a base of 0.130. The estimates in the OLS specifications have the opposite sign, showing the strong presence of reverse causality. The estimate is even stronger for the low-income sub-sample in Column (4), at 0.637 reductions of the likelihood for a 100 points increase in the credit score.

Panel b shows an economically significant effect of credit access on emergency room visits, measured by the imputed credit card limit. The effect is significant for the entire sample and the low-income sub-sample. The IV estimate in Column (2) indicates that a log point increase in households' credit card limit reduces the likelihood of emergency room visits by 0.135 on a base of 0.130 for the entire sample. The IV estimate in Column (4) indicates that a log point increase in households' credit card limit reduces the likelihood of emergency room visits by 0.160 on a base of 0.126 for the sub-sample of low-income households. The estimates in the OLS specifications are significant while having the opposite sign, showing the strong presence of reverse causality.

Panel c shows a significant effect of credit access on emergency room visits, measured by the imputed number of inquiries per new loan opened. The effect is not significant. However, a comparison of the IV estimates in Columns (2) and (4) to the OLS estimates in Columns (1) and (3) shows that the IV estimates suggest a positive

Table 5: Effect on emergency care: regression of emergency room visits on credit access.

The dependent variable is whether the individual has visited an emergency room in a reference year.

Second stage: MLE	probit, all (1)	IV-probit, all (2)	probit, low-income (3)	IV-probit, low-income (4)
Panel a: credit access measure is imputed credit score				
imputed credit score	0.0916***	-0.259***	0.0610	-0.637***
	(0.0261)	(0.0938)	(0.0523)	(0.147)
Marginal effect at all means	0.0185	-0.259		
Fraction emergency room visits	0.130	0.130	0.127	0.127
Health and charge controls	Y	Y	Y	Y
Age and income controls	Y	Y	Y	Y
Year and state fixed effects	Y	Y	Y	Y
F-statistic	75.28	46.49	11.90	25.61
Observations	57,474	57,474	78,821	78,821
Panel b: credit access measure is imputed credit card limit				
imputed credit card limit	0.0209***	-0.135***	0.0287***	-0.160***
	(0.00327)	(0.0362)	(0.00660)	(0.0380)
Marginal effect at all means	0.0042	-0.135		
Fraction emergency room visits	0.130	0.130	0.126	0.126
Health and charge controls	Y	Y	Y	Y
Age and income controls	Y	Y	Y	Y
Year and state fixed effects	Y	Y	Y	Y
F-statistic	77.21	35.38	11.38	85.24
Observations	57,476	57,476	78,823	78,823
Panel c: credit access measure is the imputed number of inquiries per new loan				
imputed number of inquiries	-0.104***	0.0562	-0.210***	0.0965
	(0.0323)	(0.0676)	(0.0665)	(0.124)
Marginal effect at all means	-0.0475	-0.0661		
Fraction emergency room visits	0.129	0.129	0.128	0.128
Health and charge controls	Y	Y	Y	Y
Age and income controls	Y	Y	Y	Y
Year and state fixed effects	Y	Y	Y	Y
F-statistic	79.13	72.96		803.77
Observations	71,387	71,387	77,244	77,244

Note: sample is conditional on the MEPS respondents who have positive medical charges in a year. * denotes 10% significance level. ** denotes 5%, and *** denotes 1%. Standard errors are in parentheses. The credit score is the imputed MEPS respondent's credit score. The credit card limit is the log of the imputed MEPS respondent's credit card limit. The number of inquiries is the imputed MEPS respondent's number of loan inquiries per new loan opened. The data source is the MEPS panel 2006-2016.

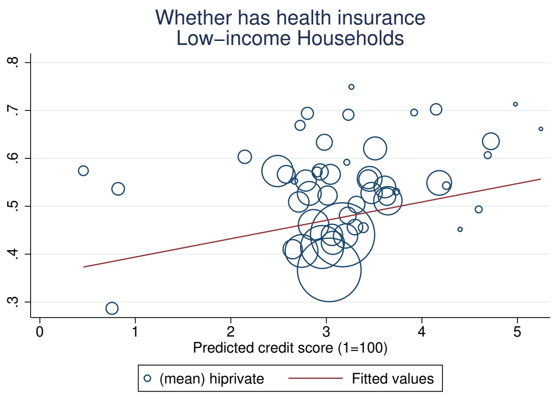
effect on the number of inquiries on emergency room visits, which is consistent with the other two panels above.

1.5.4 Effect on private insurance coverage

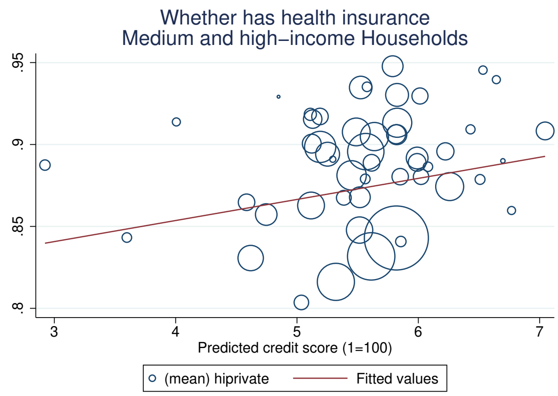
I now present the estimation result on private insurance coverage. Like the previous analysis, my sample only includes MEPS respondents who are not eligible for Medicaid or Medicare. Figures 5a to 5f show the correlation between private insurance coverage and credit access measures. The data are averaged by state with circles proportional to the number of observations. As shown in Figures 5a to 5f, the positive correlation of credit access on insurance coverage is solid and consistent for both low-income households and the entire sample.

Table 6 presents the regression estimates of the effect on private health insurance coverage. Across all panels, the estimation results show a robust causal effect of households' credit access on their likelihood of purchasing private health insurance. Columns (1) and (3) show the OLS regression, which measures the probability measure transformed correlation between health insurance coverage and credit access, controlling for households' total medical charge, income, age, and state and year fixed effects. Columns (2) and (4) show the instrumental variable regression, which measures the causal effect of credit access on health insurance coverage. Columns (1) and (2) show an estimation of the entire sample aged 25-71 who are ineligible for public health insurance, and Columns (3) and (4) show the sub-sample of low-income households.

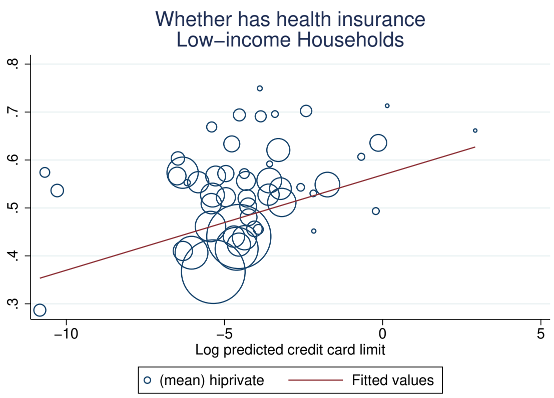
As shown in Table 6, all three measures of credit access have a robust causal effect on households' health insurance coverage. Panel a shows an economically significant effect of credit access, measured by the imputed credit score, on health insurance



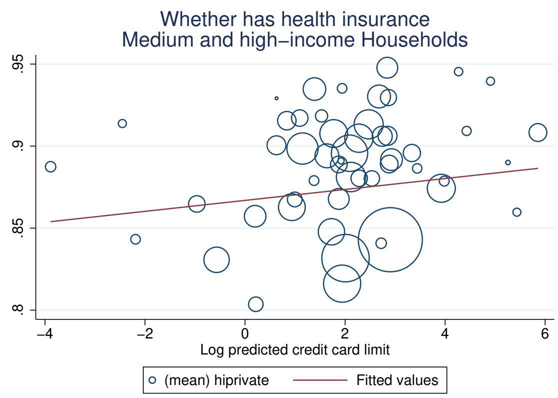
(a) Line fit: credit score, low-income.



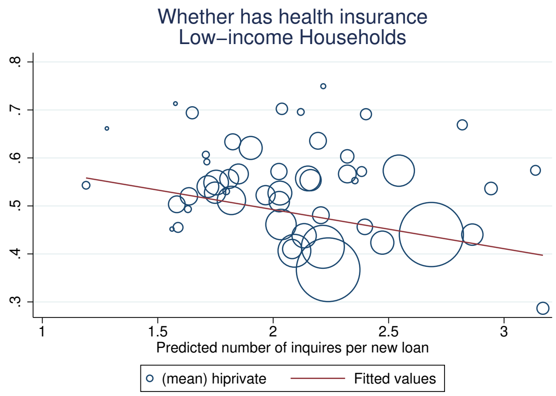
(b) Line fit: credit score, high-income.



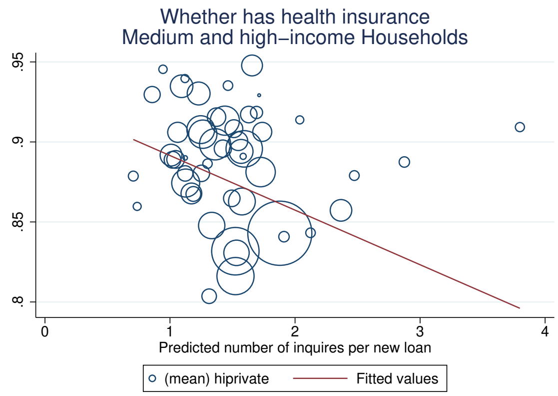
(c) Line fit: credit card limit, low-income.



(d) Line fit: credit card limit, high-income.



(e) Line fit: inquiries per new loan, low-income.



(f) Line fit: inquiries per new loan, high-income.

Figure 5: Effect of credit access on private insurance coverage, sample plots.

Notes: Panels a and b plot private insurance coverage against the predicted credit score by the state for low- ($\leq$ 200%FPL) and medium and high- ($\geq$ 200%FPL) income households. Panel c and d plot private insurance coverage against the state's log predicted credit card limit for low- and medium- and high-income households. Panels e and f plot private insurance coverage against the predicted number of inquiries per new loan opened by the state for low- and medium- high-income households. The circles in Panels a-f are proportional to the number of observations in each state. Pooled 2007-2016 MEPS, excluding individuals with public insurance or age 65 or older.

Table 6: Effect on coverage: regression of private health insurance coverage on credit access.

The dependent variable is whether a Medicaid-ineligible person has private health insurance.

Second stage: MLE	probit, all (1)	IV-probit, all (2)	probit, low-income (3)	IV-probit, low-income (4)
Panel a: credit access measure is imputed credit score				
imputed credit score	0.376***	0.659***	0.460***	0.758***
	(0.0252)	(0.106)	(0.0421)	(0.135)
Marginal effect at all means	0.099	0.659	0.181	0.758
Fraction has private insurance	0.794	0.794	0.807	0.807
Health control	Y	Y	Y	Y
Age and income controls	Y	Y	Y	Y
Year and state fixed effects	Y	Y	Y	Y
F-statistic	50.23	46.94	16.10	14.43
Observations	76,918	76,918	87,760	87,760
Panel b: credit access measure is imputed credit card limit				
imputed credit card limit	0.0553***	0.168***	0.0581***	0.131**
	(0.00347)	(0.0419)	(0.00583)	(0.0512)
Marginal effect at all means	0.0146	0.168	.0229	0.131
Fraction has private insurance	0.793	0.793	0.807	0.807
Health control	Y	Y	Y	Y
Age and income controls	Y	Y	Y	Y
Year and state fixed effects	Y	Y	Y	Y
F-statistic	54.04	58.18	13.67	13.16
Observations	76,923	76,923	87,765	87,765
Panel c: credit access measure is the imputed number of inquiries per new loan				
Imputed ratio of inquiries	-0.181***	-0.0661	-0.264***	0.0210
	(0.0303)	(0.106)	(0.0592)	(0.148)
Marginal effect at all means	-0.047	-0.066	0.181	0.758
Fraction has private insurance	0.795	0.795	0.807	0.807
Health control	Y	Y	Y	Y
Age and income controls	Y	Y	Y	Y
Year and state fixed effects	Y	Y	Y	Y
F-statistic	79.13	72.96	16.10	14.43
Observations	71,387	71,387	87,760	87,760

Note: * denotes 10% significance level. ** denotes 5%, and *** denotes 1%. Standard errors are in parentheses. The credit score is the imputed MEPS respondent's credit score. The credit card limit is the log of the imputed MEPS respondent's credit card limit. Ratio inquiry is the imputed MEPS respondent's ratio of loan inquiries over new loans opened. The data source is the MEPS panel 2006-2016.

coverage for the entire sample and the sub-sample of low-income households. The IV estimate in Column (2) indicates that a 100 points increase in the credit score, equivalent to an increase in credit rating rank (sub-prime, below prime, prime, above prime), improves the likelihood of coverage by 0.659 on a base of 0.794. The estimate is even stronger for the low-income sub-sample in Column (4), at 0.758, improvements of the likelihood of coverage for a 100 points increase in the credit score.

Panel b shows an economically significant effect of credit access measured by the imputed credit card limit on health insurance coverage. The effect is significant for both the entire sample and the low-income sub-sample. The IV estimate in Column (2) indicates that a log point increase in households' credit card limit improves the likelihood by 0.168 on a base of 0.793 for the entire sample. The IV estimate in Column (4) indicates that a log point increase in households' credit card limit improves the likelihood by 0.131 on a base of 0.807 for the sub-sample of low-income households. The estimates in the OLS specifications are also significant but one-third smaller.

Panel c shows the effect of credit access measured by the imputed number of inquiries per new loan opened on health insurance coverage. The effect is not significant. A possible reason is that low-income households who do not have private health insurance can be so credit-constrained that many do not make any loan inquiries. This fact weakens the correlation between the instrumental variable and the outcome variable.

To summarize, I have shown a significant causal effect of credit access on households' medical expenditures. The 3-by-3-by-2 regression result is comprehensive. All three credit access measures strongly affect households' medical expenditures, including total medical expenditure, emergency room visits, and private health insurance coverage. These effects are more substantial for low-income households than for the entire sample.

Table 20 in the Appendix presents the regression results for the sub-sample of young households aged 21-35 and shows a significant effect of credit access on their medical expenditures. These effects are stronger for young households than for the entire sample.

1.5.5 Robustness check

I further check the robustness of the exogeneity assumption underlying the instrumental variable design. The motivation for this test is the concern that a third variable may impact the instrumental variable, i.e., county credit access, and the outcome variable, i.e., personal healthcare expenditures. There are two possible variables that I am aware of, which are the heterogeneity in households' preferences and the county's health resources.

Should the heterogeneity in households' preference correlate with the county credit access fixed effects, households' opinion on healthcare service and health insurance would also correlate with the fixed effects, conditional on the set of control variables. I investigated the households' opinions on health insurance recorded in the MEPS data. As Table 21 in the Appendix shows, the correlation between households' opinion on health insurance and the county fixed effects is very weak if one replaces the outcome variables in the previous main result with the two opinion variables.

County health resources and credit access directly depend on the local economy. However, conditional on the county average income, the correlation between health resources and credit access is weak. To conduct this robustness check, I regress county health resources on county credit access. Indeed, Table 22 in the Appendix shows a weak correlation. As shown in Columns (1)-(3), the county number of hospitals, the total number of hospital beds, and medical providers per capita are all significantly dependent on the county average income but insignificantly or weakly dependent on the county credit access measures. The table suggests that conditional on the controlled exogenous variables, the county health resources are most weakly related to the instrumental variable. Previous literature ([18]) has found a weak correlation between hospital altruism and public medical spending.

1.6 Role of credit constraint in public health policies

As shown in the previous sections, low-income households are often credit constrained and unwilling to purchase health insurance due to their tightened credit. If these households receive more credit from a credit expansion policy, they will likely purchase health insurance. Credit expansion policy, therefore, improves health insurance enrollment, especially among the intertemporally constrained young population. I calibrate the model in Section 1.2 and calculate the lifetime reduction in health insurance coverage due to limited credit access for an average person—the calibration abstracts from other factors that influence healthcare expenditure and saving behavior. I exclude households with public insurance and do not model tax exemptions or premium subsidies to health insurance.

Life cycle model analysis is also helpful in evaluating public health policies such as the Affordable Care Act Medicaid expansion. I use the calibrated model to compare the role of households' credit constraints to the effectiveness of public health policies.

1.6.1 Life cycle model

I calibrate the intertemporal Grossman model ([26, 49]) as in Section 1.2. Households have a representative agent with expected utility preferences over consumption c and level of health h. As in the literature, health is modeled as a human capital affecting utility and productivity ([27]). They face illness shocks and choose whether to purchase health insurance to protect against this health risk. Medical providers must provide medical services m and attempt to recover the costs.

Model timing proceeds as follows: (i) households live T periods deterministically, starting from age 21 to age 65, (ii) households start with maximum health $h = 1$ and zero assets $b = 0$ at period 1, (iii) they choose the optimal consumption c and level of health h at every period t, and (iv) they face an age-dependent income shock y_t and illness shock x_t at every period t. Households also face a credit constraint, which limits the maximum amount they can borrow as proportional to their income. Together with the hump-shaped age-dependent income profile, the credit constraint is binding for young households facing negative income shocks. For those households, the optimal marginal utility of consumption $u'(c_t)$ today is higher than tomorrow. Therefore it is optimal for them not to purchase health insurance today[6].

I assume standard CRRA utility function, linear medical cost function, and log-linear wage determination function for parsimony. I also assume the yearly model

[6] Here, I am making a reasonable assumption that the price of the health insurance is actuarially fair for households without a binding constraint.

period to be consistent with the MEPS data. The life cycle model is as follows:

$$
\begin{aligned}
&\max_{b_t, h_t} \sum_{t=t_0}^{T} \beta^t E_{t_0}[u(c_t, h_t)] \\
&\text{s.t. } c_t + I'p + (1 - Ik)m(h_{t-1}, x_t, h_t) + \frac{b_{t+1}}{1+r} = b_t + w(h_t, y_t) \\
&\qquad b_{T+1} = 0 \\
&\qquad b_{t+1} \geq \underline{b}(w_t)
\end{aligned}
$$

where I assume the following functional form:

$$
\begin{aligned}
u(c_t, h_t) &= \frac{(c_t^{1-\lambda} h_t^{\lambda})^{1-\delta}}{1-\delta} \\
m(h_{t-1}, x_t, h_t) &= \mu(h_t - h_{t-1} + x_t) \\
w(h_t, y_t) &= \phi_j h_t y_t \\
\underline{b}(w_t) &= \nu_j w_t
\end{aligned}
$$

and x_t and y_t are the random health shock and income shock, respectively.

This simple model captures the main empirical findings. Households reduce their medical expenditure and are more likely to skip health insurance when they are credit-constrained, especially among the low-income and young population. For the young population, the effect on health insurance enrollment is more concentrated because they expect their income to increase while their illness risk is low.

1.6.2 Calibration

The model parameters are calibrated based on the same Experian-imputed MEPS sample as in the previous sections. The life cycle model is solved using backward induction with a yearly period. The level of health is identified by the MEPS categorical variable of self-reported health status, and it is normalized to between zero (poor health) and one (healthy). The illness shock x_t is identified from the MEPS question of whether the respondent has had an illness or severe illness in the past 12 months. It is coded as a three-state Markovian process. The medical expenditure functions $m(\cdot)$ is then estimated using these variables.

The income process $w(\cdot)$ comprises an age-profile ϕ_j, households' level of health h_t, and an AR(1) process. It is also estimated from the MEPS. Lastly, the credit constraint $\underline{b}(\cdot)$ is identified as the ratio of credit card limit to income in the Experian credit report data. Appendix A.1 discusses the solution to the model and the estimation of the $m(\cdot)$, $w(\cdot)$ and $\underline{b}(\cdot)$ functions as well as the random processes x_t and y_t.

1.6.3 Policy evaluation

Table 7 compares several policy experiments based on the calibrated model. Specifically, I examine the effect of relaxing the credit constraint on improving lifetime insurance coverage and medical expenditure compared to the effect of the ACA Medicaid expansion. To make the magnitudes of the policy experiments interpretable, I study the effect of a one-time credit expansion that increases a household's credit limit by $1,000. In contrast, in the data, the median imputed credit limit for MEPS respondents is $3,314. Panel a presents the baseline setting that matches the pre-Medicaid expansion. Panel b presents the policy experiments on relaxing households' credit constraints.

Table 7: Life cycle model: effect of relaxing the credit constraint.

	Insurance coverage		Can not afford		Is not willing		Policy cost
	age 25-65	age 25-35	age 25-65	age 25-35	age 25-65	age 25-35	
Panel a: baseline							
(1) w/o Medicaid expansion	82.7%	85.1%	2.6%	3.6%	14.6%	11.2%	-
(2) with Medicaid expansion	93.5%	99.5%	0%	0%	6.5%	0.5%	0.148
Panel b: counterfactual							
(3) Unconstrained, w/o expansion	84.5%	91.9%	0%	0%	15.5%	8.1%	0
(4) Credit +$1,000, w/o expansion	84.5%	91.8%	2.4%	2.8%	13.1%	5.3%	0
(5) Credit +$1,000, with expansion	93.6%	99.6%	0%	0%	6.4%	0.4%	0.148

Note: Simulated model output in the table. Can not afford represents the fraction of the population that cannot afford health insurance within their budget constraint. Is not willing represents the fraction of the population that can afford health insurance but chooses not to purchase it. Policy cost is normalized as a flat income tax on the average annual income of the working-age population.

Table 7 has two significant results. First, by comparing Rows (3) to (1), we see that the credit constraint has both a limiting effect on the affordability of health insurance and a discouraging effect on young households' willingness to purchase health insurance. The limiting effect generates a difference of 2.6% for the working-age population and 3.6% for the young. The discouraging effect generates an 11.2%-8.1%=3.1% difference between the baseline and the counterfactual of completely unconstrained borrowing. Overall, removing households' borrowing constraint improves overall insurance coverage by 1.8% points and the coverage of households aged 25-35 by 6.8% points at zero policy cost.

Second, by comparing Row (4) to (2), we see that a moderate increase in credit improves insurance coverage through a different channel than the Medicaid expansion. Allowing $1,000 more credit to households increases the willingness to purchase health insurance for all households by 1.5% and especially for young households aged 25-35 by 5.9%. Contrary to the Medicaid expansion, allowing more credit does not effectively improve affordability among very low-income households, but this policy also has zero policy cost.

1.7 Conclusion

Measuring the response of households' healthcare expenditure to credit access is very useful for analyzing healthcare policy. This paper provides such measures as elasticity, long-term effects, and life cycle experiments.

In the paper's main result, I estimate the elasticity of healthcare expenditures to credit access using an instrumental variable design on high-quality household data. The result shows a significantly strong and positive effect of credit access on medical expenditure, insurance coverage, and a reduced likelihood of emergency room visits.

I next investigate such effects in the long term and over households' life cycle. Exploiting the variation across US counties after the 2007 Great Recession, I show that households living in high credit supply counties are more likely to increase their medical expenditures.

The paper's final part examines credit access as an option for public health policies. By calibrating the intertemporal Grossman model, I show that consumer finance policy, such as allowing $1,000 more credit to households, improves willingness to purchase by 5.9% and health insurance by 6.7% among households age 25-35, at lower policy cost compared to the Medicaid expansion. Unlike the Medicaid expansion, the consumer finance policy improves the willingness to purchase.

2.0 Home Financing: Distributional Implications of Mortgage Credit Standards

2.1 Introduction

The Great Recession of 2007-2009 was the most significant economic contraction in the United States' history since the Great Depression. Real GDP declined by 4%, and the employment-to-population ratio dropped from 63% to 58%. Though real GDP returned to pre-recession levels by 2011 and continued to grow after that, labor markets remained stagnant, with no substantial growth in employment until 2014. Household consumption also fell by 5% and remained stagnant until well into 2013 ([47]). Additionally, consumer credit also contracted substantially, with mortgage debt falling by 13% between 2008 and 2014 and other household debt by 6% between 2008 and 2012, with no systematic recovery in either category before 2015.[1]

This paper aims to isolate and quantify the Great Recession's impact on young households via these unique channels and trace out the long-run implications for lifetime earnings, housing markets, and economic performance. The primary mechanism through which the Great Recession affects young households in the analysis is the response of credit markets to the downturn, notably the tightening of credit standards combined with the history-dependent credit scoring system that governs the allocation and price of consumer credit in the United States.

We begin the analysis by presenting evidence that younger households were

[1]Authors' calculations from the Financial Accounts of the United States, Board of Governors of the Federal Reserve System.

severely restricted in their borrowing capacity in the aftermath of the Great Recession. We show that compared to older households and older cohorts after previous recessions, mortgage terms for younger households during and after the Great Recession were much more stringent. Additionally, these households had lower net worth due to a higher student debt burden, resulting from the rise in college tuition over the past decade. the central hypothesis is that with more outstanding debt, stricter borrowing limits, and substantial income loss during the 2007-2009 recession, this constrained generation had to delay the recession.

We assess the hypothesis quantitatively with a life cycle heterogeneous agents model with aggregate shocks, where households' access and price of credit depend on their history of borrowing and defaults. Similar to [33], in the model, households receive labor income shocks and choose housing and non-durable consumption, where homes can be rented or purchased. Mortgage financing is available, and mortgage terms and borrowing limits are determined endogenously in a way that mirrors current practice in US consumer credit markets. the model implies a generational difference in mortgage access and conditions, which amplifies differences in the pattern of recovery of consumption and homeownership for younger and older generations after a recession.

This paper contributes to the literature on intergenerational distributional effects of the Great Recession, as in [23, 29, 42]. We concentrate on the role of the response of mortgage market conditions to the Great Recession in combination with the impact of student debt burdens on credit scores as a critical mechanism for amplifying the effects of the recession for young households.

We directly measure access to and pricing of mortgage credit for different cohorts during and after the Great Recession compared to earlier periods from Experian credit report data and the Freddie Mac Single-Family Loan Database.

We show through the model that the credit market extended the negative effect of the Great Recession for young households. This result is related to the discussion on debt accumulation of the younger generations, such as [51].

2.2 Empirical evidence

We present empirical evidence from two separate sources which jointly motivate the model's mechanism: (i) from the perspective of the mortgage lenders, the median credit score at mortgage origination for new homes rose significantly after 2013, (ii) from the perspective of potential borrowers, the likelihood of both making a mortgage inquiry and opening a new mortgage shifted after 2013 when prime borrowers fully recovered their mortgage balance to the pre-Great Recession level but sub-prime borrowers did not recover. Taken together, the empirical evidence suggests a significant role of the shift in mortgage credit standards in the recovery of the mortgage market.

2.2.1 Shift in mean credit score at mortgage origination

We utilize Freddie Mac's Single-Family Loan-Level Dataset to study the credit score thresholds at mortgage origination for new homes. This data contains single-family mortgages that Freddie Mac purchased or guaranteed from 1999 to 2019. Figure 6 shows the median FICO score of all 30-year fixed-rate Freddie-Mac purchased mortgages at origination by year. The 2007 Great Recession caused a sudden increase in the median FICO score of new mortgage holders, which did not fall back to the pre-2007 level after 2010. Specifically, before 2007, the median FICO score of new mortgage holders was about 720, which rapidly increased to 760 in 2010 and only slightly decreased to about 750 in 2019[2]. Before 2007, the median credit score for first-time homebuyers was almost the same as the median score for non-first-time buyers. After 2010, the median score for first-time buyers was 20 points less than for non-first-time buyers. This indicates even more stringent credit standards for non-first-time home buyers after 2010.

Using the same sample, we estimate this structural shift in credit scores at mortgage origination before and after the 2007 Great Recession. Specifically, we estimate the following difference-in-difference model with time dummies for the Great Recession start and end dates and credit rank dummies:

$$\begin{aligned} InterstRateMinusTreasury_{i,t} = &\beta_{ScoreRank} + \beta_{ScoreRank \times Phase} + \beta_{Phase} + \\ &\beta_t + \beta_{state} + \epsilon_{i,t} \end{aligned} \tag{9}$$

where $InterstRateMinusTreasury_{i,t}$ is the spread between the mortgage interest rate at the origination for mortgage holder i at time t and the market yield of the 10-year maturity treasury bill. $\beta_{ScoreRank}$ is the dummy variable for the rank of the FICO score of mortgage holder i at time t, and β_{Phase} is the dummy for whether time t is before 2007q3, between 2007q4 and 2013q3, or after 2013q4. β_t and β_{state} are the time and state dummy variables, and $\epsilon_{i,t}$ is the error term. The summary statistics of the regression are in Table.

Table 8 reports the regression results. The idea of the regression is to test whether low credit score mortgage holders received a much higher increase in mortgage interest rate than high credit mortgage score holders after 2013 due to the structural shift in credit standards. Indeed, while Columns (1) and (2) show a significant negative correlation between credit score and mortgage interest rate, Column

[2] A credit score of 720 is the upper bar of the "good" score rank of Fico score between 690-720, while 720-850 is the "excellent" score rank which is also the highest rank.

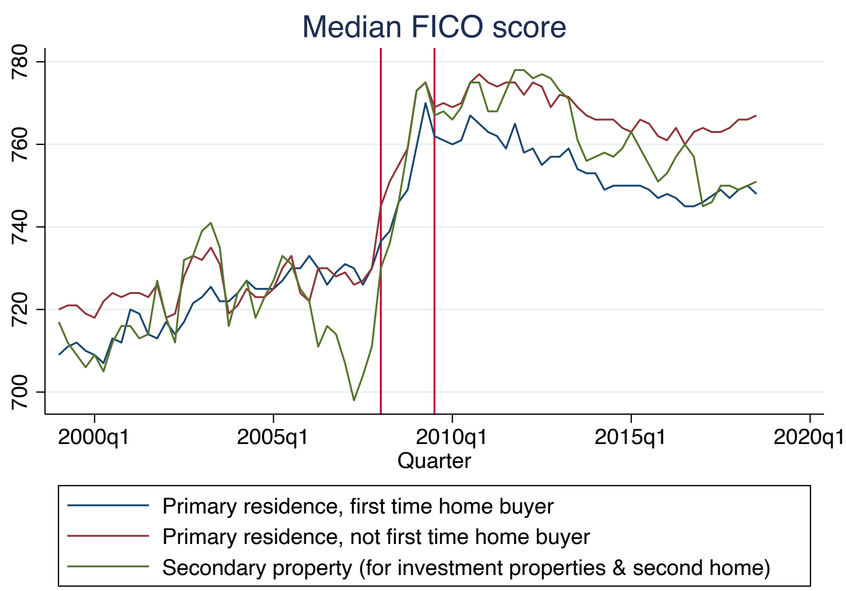


Figure 6: Median FICO score by type of mortgage at origination. Source: calculation from Freddie Mac's Single-Family Loan-Level Dataset.

Table 8: Regression result: effect of credit score on the interest rate at origination.

Score rank	(1) before 2007q3 β_{Score}	(2) 2007q4-2013q3 $\beta_{Score} + \beta_{Score \times Phase2} + \beta_{Phase2}$	(3) after 2013q4 $\beta_{Score} + \beta_{Score \times Phase3} + \beta_{Phase3}$
Deep subprime	0.21***	0.70***	not in sample
	(0.25)	(0.10)	-
Subprime	0.18***	0.66***	0.25***
	(0.01)	(0.04)	(0.06)
Near prime	0.13***	0.59***	0.35***
	(0.01)	(0.02)	(0.01)
Prime	0.06***	0.36***	0.10***
	(0.01)	(0.01)	(0.01)
Super prime	0***	0.18***	-0.12***
	(0)	(0.01)	(0.01)
N		112,932	
R^2		0.3333	

Robust standard errors in parentheses, *** $p < 0.001$, ** $p < 0.05$, * $p < 0.1$. The sample includes Freddie Mac purchased or guaranteed 30-year fixed rate mortgage at the origination of first-time home buyers from 1999-2019. The dependent variable is the spread between the mortgage interest rate at origination and the market yield of a 10-year maturity treasury bill.

(3) shows that after the Great Recession, the mortgage rate fell disproportionately across the credit ranks. Specifically, low credit score borrowers faced a much higher increase in mortgage interest rate ($0.25 - 0.18 = 0.07$) compared to prime borrowers ($0.10 - 0.06 = 0.04$), and subprime borrowers are excluded from the Freddie Mac mortgage pool after 2013. This result implies that the mortgage market became disproportionately more stringent for subprime borrowers after 2013.

Figure 7 visualizes the regression results of Table 8. The fitted lines between the mortgage interest rate at origination and FICO scores for 1999-2007 and 2013-2018 were almost parallel. However, Freddie Mac's mortgage sample for the 2013-2018 mortgage origination only covered FICO scores above 580 as a consequence of the tightened mortgage credit standards.

2.2.2 Shift in mortgage inquiries and approval

We now turn to the Experian credit panel to measure the consequences of the change in credit standards. We model the change in credit standards as an exogenous policy shock to competitive mortgage pricing. While the main focus is the role of mortgage credit standards, households' decision-making remains the central part of the mechanism in determining the average homeownership rate.

We use the Experian consumer credit panel to study how the shift in credit standards affected households' decisions on making mortgage inquiries and their likelihood of getting a mortgage. The credit panel data tracks an individual's debt portfolio by quarter, and it is generally considered representative of the US population [3]. While the credit panel data only contains limited demographic information

[3] The Experian consumer credit panel data only contains individuals who have at least some credit history in their past, i.e. a credit card or a mortgage inquiry. This includes about 90% of the US population in 2005. See [] for a detailed comparison between consumer credit panel data

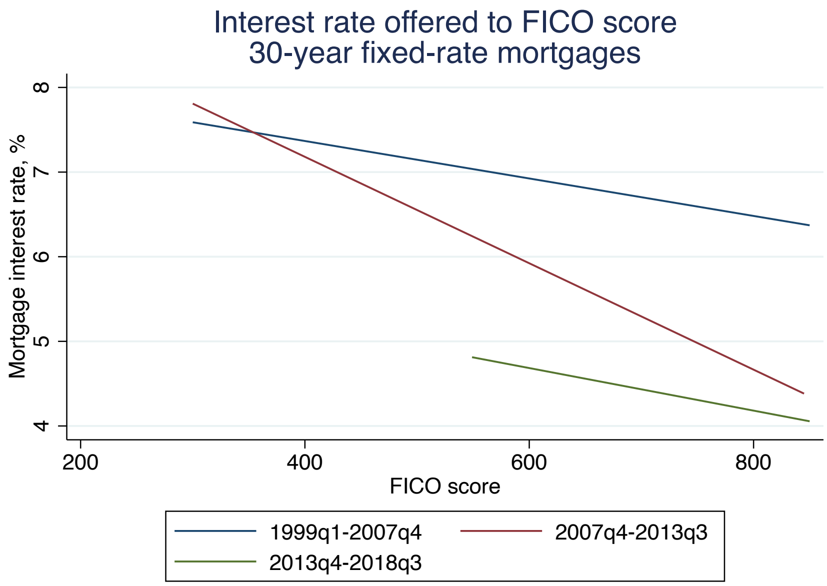


Figure 7: Fitted linear correlation between the mortgage interest rate of 30-year fixed-rate home mortgages at origination and FICO scores. Source: calculation from Freddie Mac's Single-Family Loan-Level Dataset.

such as age and household imputed income, I can observe accurate changes in individuals' mortgage balances and credit scores. We use these changes in the household debt portfolio to input households' financial status such as homeowners, potential home buyers, or student loan holders.

2.2.2.1 Likelihood of mortgage inquiries

A typical borrower makes several mortgage inquiries as they start to apply for a mortgage. We first study the effect of changing credit standards on households' likelihood of making mortgage inquiries. In the credit report data, we define non-homeowners as individuals with zero mortgage balance in their past two years. We estimate the effect of credit standards on making mortgage inquiries using the following model:

$$\begin{aligned} Inquiries_{i,t} = &\beta_{ScoreRank} + \beta_{ScoreRank \times Phase} + \beta_{Phase} + \beta_I Income_{i,t} + \\ &\beta_A Age_{i,t} + \beta_t + \epsilon_{i,t} \end{aligned} \tag{10}$$

where $Inquiries_{i,t}$ is the 0-1 indicator of whether a non-homeowner individual i makes a mortgage inquiry at time t. $\beta_{ScoreRank}$ is the dummy variable for the rank of credit score of the individual i at time t, and β_{Phase} is the dummy for whether t is before 2007q3, between 2007q4 and 2013q3, or after 2013q4. $Income_{i,t}$ and $Age_{i,t}$ are the income and age control variables, β_t is the time dummy variable, and $\epsilon_{i,t}$ is the error term.

Table 9 reports the regression results. Column (1) shows the estimated coefficient for credit score ranks before 2007, Column (2) shows the estimated coefficient for credit score ranks during the Great Recession, and Column (3) shows the estimated

and the Survey of Consumer Finances.

Table 9: Regression result: effect of credit score on the likelihood of making mortgage inquiries.

	(1)	(2)	(3)
Score rank	before 2007q3	2007q4-2013q3	after 2013q4
	β_{Score}	$\beta_{Score} + \beta_{Score \times Phase2} + \beta_{Phase2}$	$\beta_{Score} + \beta_{Score \times Phase3} + \beta_{Phase3}$
Deep subprime	0.032***	0.006***	0.016***
	(0.001)	(0.001)	(0.001)
Subprime	0.042***	0.011***	0.018***
	(0.001)	(0.001)	(0.001)
Near prime	0.062***	0.034***	0.038***
	(0.001)	(0.001)	(0.001)
Low prime	0.048***	0.035***	0.036***
	(0.001)	(0.001)	(0.001)
High prime	0.028***	0.031***	0.034***
	(0.001)	(0.001)	(0.005)
Super prime	0***	0.015	0.016***
	(0)	(0.001)	(0.007)
N		7,070,803	
R^2		0.0091	

Robust standard errors in parentheses, *** $p < 0.001$, ** $p < 0.05$, * $p < 0.1$. The sample includes the Experian credit panel individuals who have had zero mortgage balance in their past two years from 2006-2017. The dependent variable is the indicator variable of whether the individual makes one or more mortgage inquiries in the past six months.

coefficient for credit score ranks after 2013. By comparing Column (3) to (1), I observe that prime borrowers recovered faster than near-prime and subprime borrowers in their likelihood of making mortgage inquiries after the Great Recession. Specifically, before 2007q3, near-prime borrowers were the most active consumers making mortgage inquiries, about $6.2\% - 0\% = 6.2\%$ more likely than super-prime borrowers, while low-prime borrowers were only $4.8\% - 0\% = 4.8\%$ more likely than super-prime borrowers. After 2013q4, they became much less active in making mortgage inquiries, only $3.8\% - 1.6\% = 2.2\%$ more likely than super-prime borrowers, while low prime borrowers still were $3.6\% - 1.6\% = 2.0\%$ more likely than super-prime borrowers.

2.2.2.2 Likelihood of mortgage approval

After making mortgage inquiries, a potential borrower may receive a mortgage loan offer from various lenders. We do not observe mortgage interest rates at origination in the Experian data. Instead, we study the effect of changing credit standards on households' likelihood of getting a mortgage after making inquiries. We estimate the effect of credit standards on getting a mortgage after making inquiries using the following model:

$$\begin{aligned} NewMortgage_{i,t} =& \alpha_{ScoreRank} + \alpha_{ScoreRank \times Phase} + \alpha_{Phase} + \alpha_I Income_{i,t} + \\ & \alpha_A Age_{i,t} + \alpha_t + \xi_{i,t} \end{aligned} \tag{11}$$

where $NewMortgage_{i,t}$ is the 0-1 indicator of whether a non-homeowner individual i acquires a new mortgage at time t.

Table 10 reports the regression results. Column (1) shows the estimated coefficient for credit score ranks before 2007, Column (2) shows the estimated coefficient for credit score ranks during the Great Recession, and Column (3) shows the estimated coefficient for credit score ranks after 2013. By comparing each Column for

Table 10: Regression result: effect of credit score on the likelihood of getting a mortgage.

	(1)	(2)	(3)
Score rank	before 2007q3	2007q4-2013q3	after 2013q4
	β_{Score}	$\beta_{Score} + \beta_{Score \times Phase2} + \beta_{Phase2}$	$\beta_{Score} + \beta_{Score \times Phase3} + \beta_{Phase3}$
Deep subprime	-0.009*	-0.017***	0.012**
	(0.005)	(0.006)	(0.006)
Subprime	0.006	-0.011*	0.013**
	(0.005)	(0.006)	(0.006)
Near prime	0.061***	0.019***	0.037***
	(0.006)	(0.006)	(0.006)
Low prime	0.060***	0.032***	0.057***
	(0.006)	(0.006)	(0.006)
High prime	0.027***	0.014***	0.041***
	(0.006)	(0.006)	(0.006)
Super prime	0***	-0.009	0.016**
	(0)	(0.006)	(0.007)
N		365,070	
R^2		0.0271	

Robust standard errors in parentheses, *** $p < 0.001$, ** $p < 0.05$, * $p < 0.1$. The sample includes the Experian credit panel individuals who have had zero mortgage balance in their past two years from 2006-2017 and have made one or more mortgage inquiries in the past six months. The dependent variable is the indicator variable of whether the individual has a positive mortgage balance.

the credit ranks, I observe that the likelihood of prime borrowers getting a mortgage recovered faster than near-prime borrowers. Specifically, before 2007q3, near-prime borrowers were the most likely to acquire a new mortgage, 6.1% more likely than super-prime borrowers. After 2013q4, they became much less likely to acquire a new mortgage, 3.7%-1.6%=2.1% more likely than super-prime borrowers. I also observe confounding results as deep subprime borrowers and subprime borrowers increased their likelihood of acquiring a new mortgage. It is because those subprime borrowers select themselves in other factors I do not observe that increase their likelihood of getting a mortgage.

2.2.2.3 Payment to income ratio

Finally, for new mortgage holders, we study the effect of changing credit standards on their payment-to-income ratio at mortgage origination. In the Experian data, we calculate the mortgage payment to income ratio by taking the ratio of the mortgage payment in the next quarter after an individual has got a new mortgage, over their Experian-imputed annual income. We then estimate the effect of changing credit standards on payment to income ratio using the following model:

$$\begin{aligned} PayToIncome_{i,t} =& \gamma_{ScoreRank} + \gamma_{ScoreRank \times Phase} + \gamma_{Phase} + \gamma_I Income_{i,t} + \\ & \gamma_A Age_{i,t} + \gamma_t + \eta_{i,t} \end{aligned} \tag{12}$$

where $PayToIncome_{i,t}$ is the payment-to-income ratio for an individual i who has just acquired a new mortgage in the past three months.

Table 11 reports the regression results. Column (1) shows that the payment-to-income ratio is the highest among subprime and near-prime borrowers after controlling their income. However, Column (3) shows that after 2013q4, near-prime

Table 11: Regression result: effect of credit score on payment to income ratio at mortgage origination.

	(1)	(2)	(3)
Score rank	before 2007q3	2007q4-2013q3	after 2013q4
	β_{Score}	$\beta_{Score} + \beta_{Score \times Phase2} + \beta_{Phase2}$	$\beta_{Score} + \beta_{Score \times Phase3} + \beta_{Phase3}$
Deep subprime	0.0011	-0.0004	-0.0048**
	(0.0024)	(0.0020)	(0.0022)
Subprime	-0.0013	-0.0043**	-0.0048**
	(0.0021)	(0.0020)	(0.0020)
Near prime	-0.032	-0.0056***	-0.0055***
	(0.0020)	(0.0020)	(0.0020)
Low prime	-0.0050**	-0.0063***	-0.0060***
	(0.0020)	(0.0020)	(0.0020)
High prime	-0.0050**	-0.0064***	-0.0061***
	(0.0020)	(0.0020)	(0.0020)
Super prime	0***	-0.0043**	-0.0052***
	(0)	(0.0020)	(0.0020)
N		69,079	
R^2		0.4146	

Robust standard errors in parentheses, *** $p < 0.001$, ** $p < 0.05$, * $p < 0.1$. The sample includes Experian credit panel individuals who have had zero mortgage balance in their past two years from 2006-2017 and have acquired a mortgage in the past six months. The dependent variable is the indicator variable of whether the individual has a positive mortgage balance.

borrowers have a payment-to-income ratio of $-0.55\% - (-0.60\%) = 0.05\%$ higher than low-prime borrowers. This piece of evidence suggests that mortgage terms are much more stringent for subprime and near-prime after the structural shift in the mortgage market since Great Recession.

2.3 Model

Time is discrete and runs forever. There are $A \geq 2$ overlapping generations. Each generation has a continuum of agents of measure one. Agents live for A periods deterministically and discount the future at a constant rate β. Let c and s denote the consumption and units of housing service of agents who derive utility in each period from

$$u(c, s) = \frac{((1-\phi)c^{1-\gamma} + \phi s^{1-\gamma})^{\frac{1-\sigma}{1-\gamma}} - 1}{1-\sigma} \tag{13}$$

where ϕ measures the relative taste for housing, $1/\gamma$ measures the elasticity of substitution between housing services and non-durables, and $1/\theta$ measures the IES. Because we focus on the stationary equilibrium, the cohort index is omitted and the agents' problem is described in terms of age a. At $a = 0$, agents are born with an initial wealth b, level of education e, and student loan debt s. At $a = 1, ..., A$, agents are in the housing market, either renting or owning a house. Renters choose the optimal size of housing to rent every period, and owners can upgrade or downgrade their housing size by selling their current house.

2.3.1 Housing market

Agents are heterogeneous in terms of their education level (college or high-school degrees), wealth b, student loan debt s, mortgage debt m, age a, idiosyncratic income shock y, and the size of their current house (if owning a house).

At period a, a renter chooses between renting a new house of size h for the period or buying a new house of size h. The unit housing rental price is denoted by ρ_h and the unit price of a house is denoted by p_h. If they buy a new house, they become a homeowner and can take out a mortgage. All mortgages are long-term and amortized over the remaining life of the mortgage holder at the mortgage interest rate r_m. The competitive mortgage price q_m is based on the default risk of the borrower. The down payment satisfies a maximum loan-to-value ratio limit: the initial principal balance m must be less than a fraction λ^m of the value of the house.

The amortization formula at age a with remaining mortgage balance m is:

$$\pi_{min} = \frac{r_m(1 + r_m)^{A-1-a}}{(1 + r_m)^{A-1-a} - 1} m \tag{14}$$

At period a, a homeowner chooses between keeping the current house, selling the house, or defaulting on the mortgage debt[4]. Owner-occupied houses carry a per-period maintenance and tax cost of $(\delta_h + \tau_h)p$. Maintenance fully offsets the physical depreciation of the dwelling. When a homeowner sells their house, it incurs a transaction cost κ_h. A homeowner who has sold their current house can immediately buy a new house. A homeowner who has defaulted on their mortgage need to wait until the next period to buy a new house.

[4]In the baseline model we do not model refinance options for homeowners, as there is no aggregate shock to the unit house price or mortgage rate, and therefore homeowners in the model have no incentives to refinance.

2.3.2 Mortgage financing

We assume a competitive mortgage market where the lenders have exogenous funding at a constant cost $\bar{r}$. Mortgage lenders on the market offer competitive mortgage price that generates zero profit on a loan-by-loan basis in expectation. The equilibrium price has a similar formulation as in [13]. The price of a mortgage at origination is determined jointly by the down payment and the mortgage applicant's creditworthiness. With a larger down payment and a higher income, the borrower's default risk is lower, leading to a lower price.

The competitive price of a mortgage at origination for a new homeowner with state variables $q*(a, b, m, s, e, y)$ is:

$$q* = \frac{1-\nu}{(1+r_m)m'} E\{(1+r_m)m'g^s + (1-\delta_h-\tau_h-\kappa_h)pg^d + (\pi'_m + q'm'')g_p\} \quad (15)$$

which is the ratio of the next-period expected return on the mortgage over the value of the mortgage at origination, net of the proportional transaction cost ν. In case the household sells the house ($g^s = 1$) in the next period, the return is the full balance of the mortgage in the next period. In case the household defaults ($g^d = 1$), the house is repossessed and the return is the remaining market value of the house. In case the household pays the mortgage ($g^p = 1$), the return is the mortgage payment plus the rolled-over value of the mortgage in the next period.

We model the shift in credit standards after the Great Recession as an exogenous policy shock to the competitive pricing scheme. The zero profit condition 15 only holds in expectation ([33]). Our empirical evidence suggests that mortgage companies in the aftermath of the Great Recession reacted by raising their credit standards on mortgage applications as the market recovered. As a consequence, subprime borrowers were all excluded from the market. Therefore, we model the shift in credit standards as a lower bound $\rho > 0$ on the mortgage price at origination:

$$q = \begin{cases} q*, & \text{if } q* \geq \rho \\ 0, & \text{if } q* < \rho \end{cases} \tag{16}$$

where the mortgage price $q = q*$ if the competitive mortgage price is above the lower bound ρ (thus the riskiness of the borrower is below a threshold) and the mortgage price $q = 0$ if the competitive mortgage price is below ρ, which means the riskiness of a borrower is beyond a threshold. We note that ρ does not alter the competitiveness of the mortgage pricing scheme, as the lenders with $\rho > 0$ still make zero profit in expectation on a loan-by-loan basis.

2.3.3 Labor endowments and student loan debt

Working-age households earn income endowment w_a^e given by

$$\log w_a^e(y_{i,t}) = \phi_a^e + y_{i,t}^e + z_t^e \tag{17}$$

where high school $e = HS$ and college degree $e = C$ households have different levels of income endowments. Conditional on their education level, individual income has three orthogonal components: a deterministic age profile ϕ_a^e, an idiosyncratic AR(1) process $y_{i,t}^e$, and the aggregate productivity shock z_t.

Agents are born with a high school or college degree, and college-degree agents are born with student loan debt whose distribution is calibrated to the data. Student loan debt is not dischargeable. I assume households repay their student loan debt according to a fixed repayment plan which is the main feature of the federal student loan program. The fixed repayment plan requires borrowers to make the same payment y in each period until age a^{FIX}.

The payment formula at age a with remaining student loan balance s is:

$$y^{FIX} = \frac{r_s(1+r_s)^{a^{FIX}-1-a}}{(1+r_s)^{a^{FIX}-1-a}-1}s \tag{18}$$

2.3.4 Household decision problems

The household decision problems are formulated recursively. Let V^n denote the value function of households who start the period without owning any housing. These households choose between being a renter and buying a house to become an owner by solving:

$$V_a^{n,e}(b,s;y,z) = \max\{V_t^{r,e}(b,s;y,z), V_t^{b,e}(b,s;y,z)\} \tag{19}$$

and I let $g^b \in \{0,1\}$ denote the decision to buy a house.

Those who choose to rent solve:

$$\begin{aligned} V_t^{r,e}(b,s;y,z) = \max_{c,b',s'} &\{u(c,h) + \beta E[V_{t+1}^{r,e}(b',s';y',z'))|y,z]\} \\ \text{s.t. } & c + q_b b' + \pi_s \leq b + (1-\tau)w_j^e(y,z) \\ & b \geq 0 \\ & \pi_s = (1+r_s)s - s' \geq \pi_s^{min} \end{aligned} \tag{20}$$

And those who choose to buy solve:

$$V_t^{b,e}(b,s;y,z) = \max_{c,b',m',s'} \{u(c,h) + \beta E[V_{t+1}^{h,e}(b',m',s';y',z'))|y,z]\} \tag{21}$$

$$\text{s.t. } c + q_b b' + \pi_m + \pi_s + p \leq b + (1-\tau)w_j^e(y,z) + m' \tag{22}$$

$$b \geq -\lambda_b p \tag{23}$$

$$m' \leq \lambda_h p \tag{24}$$

$$\pi_m = (1+r_m)m - m' \geq \pi_m^{min} \tag{25}$$

$$\pi_s = (1+r_s)s - s' \geq \pi_s^{min} \tag{26}$$

where V^h denotes the value function of homeowner households.

A homeowner has the option to keep the house and make its mortgage payment, sell the house, or default. Therefore, the problem for a homeowner is:

$$V_t^{h,e}(b,s;y,z) = \max\{V_t^{p,e}(b,s;y,z), V_t^{s,e}(b,s;y,z), V_t^{d,e}(b,s;y,z)\} \tag{27}$$

and I let $g^p \in \{0,1\}$ denote the decision to pay the mortgage, $g^s \in \{0,1\}$ the decision to sell the house, and $g^d \in \{0,1\}$ the decision to default on the mortgage.

Those who choose to pay the mortgage solve:

$$V_t^{p,e}(b,m,s;y,z) = \max_{c,b',m',s'} \{u(c,h) + \beta E[V_{t+1}^{h,e}(b',m',s';y',z'))|y,z]\} \tag{28}$$

$$\text{s.t. } c + q_b b' + \pi_m + \pi_s \leq b + (1-\tau)w_j^e(y,z) \tag{29}$$

$$b \geq -\lambda_b p \tag{30}$$

$$\pi_m = (1+r_m)m - m' \geq \pi_m^{min} \tag{31}$$

$$\pi_s = (1+r_s)s - s' \geq \pi_s^{min} \tag{32}$$

Those who choose to sell the house solve:

$$V_t^{s,e}(b,m,s;y,z) = \max_{c,b',h',m',s'} \{u(c,h) + \beta E[V_{t+1}^{r,e}(b',m',s';y',z'))|y,z]\} \tag{33}$$

$$\text{s.t. } c + q_b b' + \pi_s + m \leq b + (1-\tau)w_j^e(y,z) + p \tag{34}$$

$$b \geq 0 \tag{35}$$

$$\pi_s = (1+r_s)s - s' \geq \pi_s^{min} \tag{36}$$

And those who choose to default on the mortgage solve:

$$V_t^{d,e}(b,m,s;y,z) = \max_{c,b',m',s'} \{u(c,h) + \beta E[V_{t+1}^{r,e}(b',m',s';y',z'))|y,z]\} \tag{37}$$

$$\text{s.t. } c + q_b b' + \pi_s + \chi \leq b + (1-\tau)w_j^e(y,z) \tag{38}$$

$$b \geq 0 \tag{39}$$

$$\pi_s = (1+r_s)s - s' \geq \pi_s^{min} \tag{40}$$

2.4 Calibration

We use a two-step calibration strategy. In the first step, the parameters except for the credit standard parameter ρ are calibrated so that the model-generated moments are consistent with the 2007 data. In the second step, we keep all the other parameters fixed and calibrate ρ to match the model-generated homeownership rate to the 2013 data. The two-step strategy identifies the shift in the mortgage credit standards not directly observed in the post-Great Recession data.

We exogenously estimate the households' income process using data from the Panel Study of Income Dynamics. Specifically, we assume the income process can be decomposed into a deterministic age-income profile ϕ_a, an individual AR(1) process

$y_{i,t}$ and the aggregate productivity history z_t, and estimate the process separately for high-school and college-degree households. We restrict the sample to male household heads in the PSID aged 21-81 from 1990-2015. The specification for the income process is

$$\log(w^e(a, y_{i,t}, z_t)) = \phi^e_a + y^e_{i,t} + z^e_t \tag{41}$$

$$y_{i,t} = \alpha y_{i,t-1} + \beta + \epsilon_t \tag{42}$$

$$z_t \in \{z_1, z_2, z_3, ...\} \tag{43}$$

$$e \in \{\text{HS, C}\} \tag{44}$$

Tables 23 and 24 in the Appendix summarize the estimated parameters for the income process. Figure 15 in the Appendix visualizes the difference between high-school and college-degree households, and Figure 16 visualizes the persistent impact of the Great Recession. The estimated difference in mean log wages between high-school and college-degree households aged 21-65 is 0.57. The estimated difference in mean log wages between the trough of the Great Recession and the base year is -0.11.

We model student debt repayment to reflect the main features of the federal student loan program which accounts for 80% of the total volume. The fixed repayment plan requires borrowers to make the same payment y^{FIX} in each period until age t^{FIX}. We set the repayment period to 10 years as for the standard federal student loan plan and the student loan interest rate to 2% above the treasury bill rate following [30]. We match the initial distribution of student loan debt to the Experian consumer credit report data.

Households enter the economy at age 21, retire at age 65, and exit at 81. For parameters related to the mortgage and housing market, we follow [33] and set the loan-to-value ratio at mortgage origination to 0.85, and the depreciation rate of a foreclosed house to 0.33. The proportional property tax is 0.03, the mortgage origination cost is 0.07, and the house maintenance cost is 0.03. The transaction cost for purchasing a house is 0.08 and the intermediation cost for mortgage loans is 0.05. The rental sector operating cost per rental home is $500. Finally, we set the risk-free rate to 4% per year.

For parameters related to households' preference, we choose a standard risk aversion value of 2 and set bequest as a luxury good of $400,000 following [33]. We calibrate the utility of owning a house to 6.9 by targeting an average homeownership rate of 0.66 in 2007 in the Current Population Survey. We calibrate the discount rate to 0.95 by targeting a mortgage debt-to-income ratio of 0.34, and the disutility of defaulting to 0.12 by targeting a mortgage default rate of 0.003.

2.4.1 Stochastic steady-state comparison

We begin the analysis by considering the steady state of two economies with different levels of mortgage credit standards. We calibrate the 2013 steady state by holding all other parameters fixed at 1997 and calibrating $\rho_{2013} = 0.87$ to match the fraction of mortgage holders in 2013. Then, we compare the aggregate economic variables with ρ_{1997} and ρ_{2013}.

Table 14 illustrates the stochastic steady-state results. The model predicts that the average homeownership rate in 2013 is 0.0041 less than in 1997, and the fraction of mortgage holders in 2013 is 0.053 less than in 1997. The mechanism driving this difference is the tightened credit standards parameter that increases the mortgage price for agents with a higher likelihood of default. Young households are more affected by the tightened credit standards because they have near-zero wealth and

Table 12: Summary of parameters, 2007 steady state calibration.

Parameter	Value	Description
Non-calibrated		
A	60	Life span in yearly units
σ	2	Risk aversion
λ_m	0.85	Loan-to-value ratio at origination
δ_{hd}	0.33	Foreclosed houses depreciation rate
τ_h	0.03	Proportional property tax
α_h	0.07	Origination cost
δ_h	0.03	Maintenance cost
κ_h	0.08	Transaction cost
ν	0.05	Intermediation cost
ξ	\$500	Rental sector operating cost
ψ	100	Strength of bequest motive
$\bar{B}$	\$400,000	Bequest as a luxury good
r_b	0.04	Annualized risk free rate
r_m/r_b	1.33	Mortgage rate premium
ρ^{SS}	0.5	Social Security replacement rate
Calibrated		
ϕ	1.08	Utility of owning a house
ρ	0	Lower bound on mortgage price
β	0.964	Discount factor
χ	0.8	Disutility of default

Table 13: Simulated moments, 2007 steady state calibration.

Parameter	Target	Data	Model	Source
ϕ	Average homeownership rate	0.66	0.69	CPS
ρ	Fraction of mortgage holders	0.34	0.23	Experian
β	Mortgage debt to income ratio	7.3	6.4	Experian
χ	Mortgage default rate	0.003	0.0005	Experian

Table 14: 1997 versus 2013 steady states comparison.

	2013	1997	Difference(2013-1997)
Lower bound on mortgage price	0	0.87	0.87
Average homeownership rate	0.5495	0.59	-0.041
Fraction of mortgage holders	0.2097	0.2629	-0.053
Mortgage debt to income ratio	2.3469	2.9236	-0.577
Mortgage default rate	0.0011	0.0009	0.0002
High-school newborns: fraction of lifetime consumption willing to forego to move from 2013 to 1997			0.0831
College-degree newborns: fraction of lifetime consumption willing to forego to move from 2013 to 1997			0.1464
Renters: fraction of lifetime consumption willing to forego to move from 2013 to 1997			0.1068
Homeowners: fraction of lifetime consumption willing to forego to move from 2013 to 1997			-0.0004

face upward-sloping income-age profiles, and therefore they are more likely to default on mortgages upon receiving negative income shocks. We note that the tightened credit standards do not decrease the overall likelihood of mortgage default (the default rate in 2013 is 0.0002 higher than in 1997) because the credit standards only affect mortgage origination while default can happen after the mortgage transaction is closed.

To calculate the welfare changes from the shift in mortgage credit standards, I follow [37] and consider the fraction of ex-ante lifetime consumption newborn agents living in an economy with 2013 levels of mortgage credit standard would give up in order to be born in the economy with 1997 levels of credit standard. The bottom panel of Table 14 shows that newborn high-school agents would be willing to give up 8.3% of lifetime consumption to move from the economy with 2013 levels of credit access to 1997 levels of credit access, while college-degree households are willing to give up 14.64% of lifetime consumption to live in an economy with 1997 levels of credit access. Overall, renters would be willing to give up 10.68% of lifetime consumption to move from the economy with 2013 levels of credit access to 1997 levels of credit access, while homeowners are almost not affected by the tightening credit standards. College-degree households are more affected by the tightened mortgage credit standards than high-school households because many of them carry student loan debt and face steeper income-age profiles. Both factors increase their likelihood of defaulting on mortgages upon receiving negative income shocks.

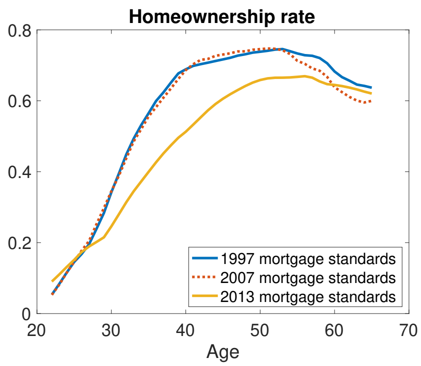
2.5 Evaluating the implications of mortgage standards

We now use the estimated model to conduct quantitative analyses. We first study the effect of mortgage standards on homeownership in partial equilibrium to illustrate the economic mechanism. We then conduct impulse response analyses in general equilibrium to shed light on the role of mortgage credit standards in the boom and bust of the housing market.

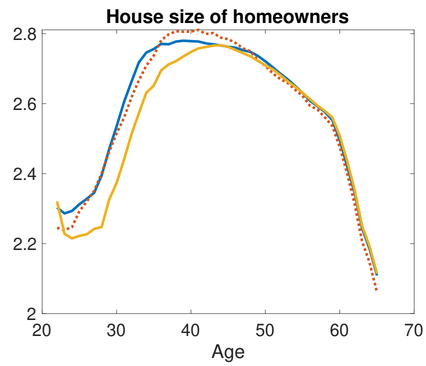
2.5.1 The effect of mortgage standards on homeownership

In this subsection, we focus on the effect of mortgage standards on homeownership and mortgage borrowings. To illustrate the economic mechanism of mortgage credit standards, we conduct a partial equilibrium analysis where the distribution of agents entering the housing market at $t = 1$ and they expect the aggregate variables (the aggregate productivity shock z and the housing price p_h) to be stable.

We begin by investigating the effect of mortgage standards on homeownership under the 2007 mortgage credit standards. Following [33], we set the maximum loan-to-value ratio to 1 and origination cost to 0. Panel (a) of Figure 8 shows that compared to the pre-housing boom mortgage supply, the housing boom credit standards do not have a significant effect on promoting homeownership, where homeownership steadily increases as households get older and peaks at age 54. Panel (b) shows that compared to the pre-housing boom mortgage supply, the housing boom does allow young homebuyers to purchase larger houses. The average house size increases by 0.2 units for young homebuyers between ages 21 and 40 under the 2007 mortgage standards compared to the 1997 baseline. While homebuyers are purchasing larger houses under the 2007 mortgage credit, Panel (c) shows that they do not utilize the



(a) Homeownership rate.

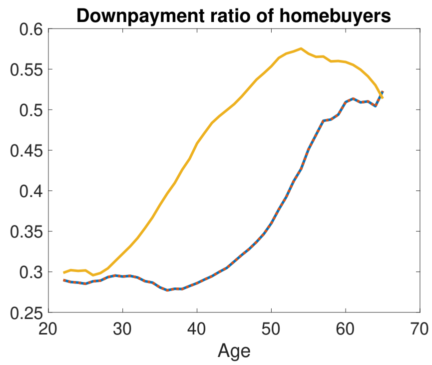


(b) House size.

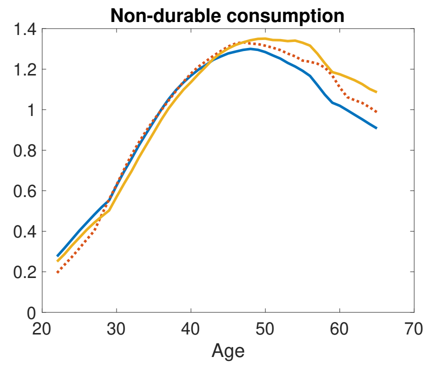
Figure 8: Simulated life-cycle dynamics for different mortgage credit standards.

expanded maximum loan-to-value ratio when applying for a mortgage. Finally, Panel (d) shows that, as a result of households spending more on their houses, young households before age 45 spend less on non-durable consumption, and older households after age 45 spend more on non-durable consumption.

The effect of the 2013 tightening of mortgage credit is completely different from the 2007 expansion. Following the calibration, we keep the maximum loan-to-value ratio at the baseline of 0.85 and the origination cost at the baseline of 0.07 but set the lower-bound of mortgage price to 0.88. Panel (a) of Figure 8 shows that compared to the pre-housing boom mortgage supply, the post-boom credit standards have a significant negative effect on homeownership, where homeownership decreases for all age between 21-65, and is more pronounced at age 40. Panel (b) shows that compared to the pre-housing boom mortgage supply, the tightened credit forces young homebuyers to purchase smaller houses. The average house size decreases by



(c) Home equity percentage.



(d) Non-durable consumption.

Figure 8: Simulated life-cycle dynamics for different mortgage credit standards (cont.)

Note: Panel A plots the aggregate homeownership rate across high-school and college-degree households between the ages of 21 and 65. Panel B plots the house size of homeowners across the population. Panel C plots the downpayment-to-price ratio of homebuyers at mortgage origination. Panel D plots the aggregate non-durable consumption. The blue line plots the variables under the 1997 mortgage credit standards which are the baseline credit standards when the maximum Loan-to-value ratio equals 0.85 and origination cost equals 0.07. The red dotted line plots the 2007 mortgage standards when the maximum Loan-to-value ratio equals 1 and origination cost equals 0. The yellow line plots the calibrated 2013 mortgage standards when the maximum Loan-to-value ratio equals 0.85 and origination cost equals 0.07, and the lower-bound of mortgage price is set to 0.88.

0.2 units for young homebuyers between ages 21 and 40 under the 2013 mortgage standards compared to both the 2007 housing boom and the 1997 baseline. While homebuyers are purchasing smaller houses under the 2007 mortgage credit, Panel (c) shows that it is because they have to make a higher down payment when applying for a mortgage, in order to decrease their likelihood of default. Finally, Panel (d) shows that young households before age 45 spend less on non-durable consumption, and older households after age 45 spend more on non-durable consumption. This is because young households need to spend more on their mortgage and old households are less interested in buying or upgrading their houses.

Intuitively, under the 2013 tightened mortgage credit standards, homebuyers make higher down payments and choose smaller houses, in order to reduce their likelihood of default. For households of low income, they are forced to postpone buying a house until they have accumulated a higher level of wealth. During the housing market boom around 2007, the relaxed maximum loan-to-value ratio and lowered origination cost has a limited effect on promoting homeownership because households understand that a mortgage is a long-term debt beyond the lowered cost at origination. However, an increased cost at origination, such as the post-recession tightened mortgage credit standards, effectively decreases homeownership as young and low-income households are completely excluded from getting a mortgage due to their reasonably-higher likelihood of default.

2.6 Conclusion

In this paper, we have argued that the current US credit market extended and amplified the negative effect of the Great Recession on the younger generation. Combined with persistent wage loss from the Great Recession, and rising college tuition, these effects caused the slow recovery of household consumption and homeownership. To support the argument, we have constructed a life cycle housing model with an aggregate shock, emphasizing the role of the credit market. The model results show a generational difference in mortgage terms conditional on all other household characteristics and therefore imply sub-optimal credit tightness for younger households in the aftermath of the Great Recession.

3.0 Detecting Disruption: Identifying Structural Changes in the Verkhovna Rada

3.1 Introduction

In 2014, there was a revolution in Ukraine triggered by the decision by the president to stop negotiations on the association agreement with the EU. The president fled to Russia after violent protests in which a number of protesters and police were killed. Shortly after that, Russia annexed Crimea and Russia backed a security conflict in the East of Ukraine. The government of Ukraine lost control over the territories in parts of two regions: Donetsk and Lugansk. The international community has mediated two agreements between the government of Ukraine and separatists - Minsk I and Minsk II. Both agreements have not been successful at resolving the crisis.

The political power in Ukraine is centralized. Despite recent legislation implemented after 2014 that serves to decentralize some economic power to the regions (budgets, service provision), the majority of legislative and executive power is concentrated at the national level. Verkhovna Rada is the national parliament. It appoints the government (subject to nominations by the president), votes on national laws that can regulate the economy, allocate the national budget, and has the power to approve constitutional amendments. Minsk agreements between the separatists and the government of Ukraine require such amendments.

In this paper, we study disruptions in the voting patterns of the parliament of Ukraine (Verkhovna Rada) over the period of 2007-2018. This period covers three convocations, prior to, during, and after the revolution. The prior convocation is important because the president who left office because of the revolution was elected in the middle of this convocation. We employ two methods: a standard ideal points model from political science and a network science approach to identify factions in the Ukrainian parliament and structural changes in the composition and behavior of these factions.

The paper makes two contributions. First, we identify the time periods of substantive changes in the Ukrainian Parliament: (1) the election of the president prior to the revolution, (2) the revolution, and (3) voting for constitutional amendments to implement Minsk II agreements. We find that during these moments, the Ukrainian parliament has become polarized. Polarization can be a sign of conflict, in-fighting, and weakness within the Ukrainian political elites. Second, we compare the performance of two different methods of identification of disruption in the parliament's behavior. We show that the network science faction detection method picks up structural changes prior to the revolution (election of the president whose tenure was ended early by the revolution), while the ideal points method performs stronger after 2014 identifying a disruption around voting on constitutional changes to implement Minsk II agreements between separatists and Ukraine. The ideal point method is better at detecting position changes of the members of parliament, while the faction method is better at detecting changes in relationships between different MPs. These results suggest that the Ukrainian parliament has become more consolidated, but the distribution of its political positions continues to evolve in response to changes in geopolitical conditions.

The ideal points model is the standard approach to understanding a legislative body of government. It models the decision of individual voters, and estimates, for each voter, a point which represents the voter's relative degree of conservativeness/liberality in the government. Researchers have developed the classical static ideal points model [14] into several dynamic forms [12, 31, 40], and used this approach to analyze the US Congress [53] and legislation in other countries [5].

We contrast ideal point results with those of an alternate modeling perspective: network science. Under a network science framework, MPs are connected to each other based on how often they agree on bills. From this network, underlying communities can be detected automatically in a variety of ways [38]. Finally, disruptions in the underlying structure of the parliament are measurable through changes in faction assignment.

These two approaches complement each other. The ideal points method can see fine-grained change since it operates on individuals, allowing for earlier detection of changes in the legislature. Faction analysis, on the other hand, is typically more interpretable, as it simplifies the entire legislature into a small number of groups. In the following sections, we discuss the data set, walk through the details of both ideal points and faction disruption, display the results of both methods and finally put them in context with Ukraine's political landscape.

3.2 Data

We use the publicly available roll call votes in Ukraine's Parliament, the Verkhovna Rada [1]. The parliament is split into convocations, roughly terms for the parliamentarians (MPs). In this work, we analyze the roll calls from convocations 6, 7, and 8. The data was analyzed on a month-to-month basis. Summary statistics for each convocation, including their start and end dates, can be found in Table 15. MPs that are not present throughout the entire convocation are not considered, since these members cannot be analyzed through ideal points. This keeps the number of MPs

Table 15: Summary of the data for each convocation.

Convocation	Start Date	End Date	Months	Bills	MPs	Parties
6	11/2007	12/2012	59	3974	539	8
7	12/2012	10/2014	22	1062	479	8
8	11/2014	07/2018*	21	1267	325	9

*Note: Convocation 8 is ongoing, but our data is truncated.

consistent through the methods of analysis.

The Verkhovna Rada has unique voting rules. For each bill, MPs have 6 voting options: Vote For, Vote Against, No Vote, Absence, Do Not Vote, and Abstain. Votes in favor of bills are common, while votes against bills are not. Local experts suggest that unless an MP feels very strongly in opposition to a bill, they will use one of the other non-for-voting options.

3.3 Ideal Points

3.3.1 Background

The ideal points model estimates a political space from the roll call votes data by directly modeling voters' decisions. We assume that each voter has an ideal point x_i in the policy space R^d with the dimensionality $d \in \{1, 2, ...\}$ representing the issues that are independent of each other. The issues do not necessarily have a direct interpretation, but they are, by the model set-up below, pairwise independent of each other. Commonly, $d = 1$ is used. The position of x_i indicates Voter i's political ideology, i.e. relative degree of conservativeness or liberality in the parliament for each identified issue.

Denote MPs $i \in \{1, ..., I\}$, Bills $j \in \{1, ..., J\}$. We code Vote For as 1, Vote Against as -1, and other votes as 0, denoted as $y_{i,j} \in \{1, -1, 0\}$. When abstention is modeled as a neutral attitude, we model instead a multi-probit. The results of each roll call are either passed ξ_j or not passed ψ_j. ξ_j, ψ_j are vectors in R^d, representing the political consequences of a bill which vary across bills. The utility of a voter i of having bill j passed is $U_i(\xi_j) = -||x_i - \xi_j||^2 + \eta_{i,j}$, and the utility of a voter i of not having bill j passed is $U_i(\psi_j) = -||x_i - \psi_j||^2 + \nu_{i,j}$. Given the setup, the utility maximization of each voter implies

$$y_{i,j} = \begin{cases} 1 \text{ , if } U_i(\xi_j) > U_i(\psi_j) \\ -1 \text{ , if } U_i(\xi_j) \leq U_i(\psi_j) \end{cases} \tag{45}$$

The voters vote independently. Thus, the error terms are normalized to be $\eta_{i,j} - \nu_{i,j} \sim N(0, 1)$. The probability of Legislator i voting yea on Bill j is

$$\begin{aligned} P(y_{i,j} = 1) = P(U_i(\xi_j) > U_i(\psi_j)) &= P(\nu_{i,j} - \eta_{i,j} < ||x_i - \psi_j||^2 - ||x_i - \xi_j||^2) \\ &= \Phi(\beta_j' x_i - \alpha_j) \end{aligned}$$

where x_i is the ideal point of MP i, and β_j is the parameter that describes the characteristics of Bill j. The likelihood across all MPs and all bills is

$$L = \prod_{i=1}^{n} \prod_{j=1}^{m} \Phi(\beta_j' x_i - \alpha_j)^{|y_{i,j} + \frac{1}{2}| - \frac{1}{2}} (1 - \Phi(\beta_j' x_i - \alpha_j))^{|y_{i,j} - \frac{1}{2}| - \frac{1}{2}} \tag{46}$$

where $y_{i,j}$ is the observed votes, and x_i, β_j, α_j are unknown parameters. We use Bayesian inference, assuming a normal (0,1) prior distribution of the parameters for each MP before the estimation of each convocation.

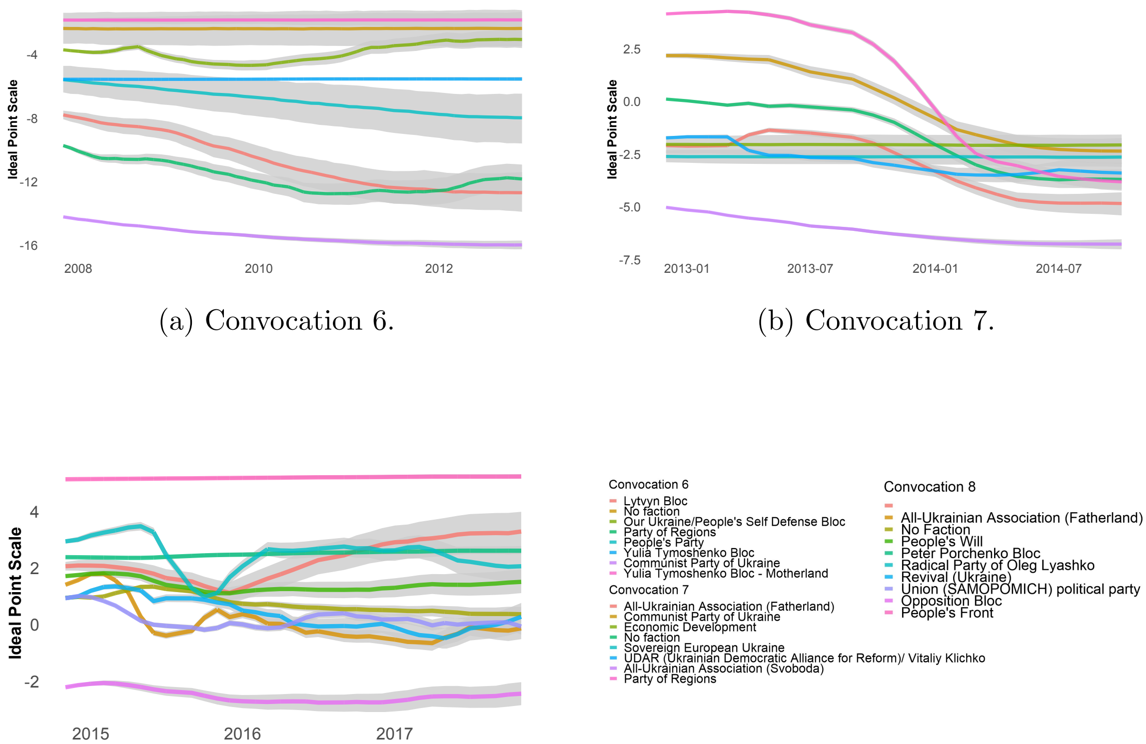
The Ideal Point Scale (y-axis of Figure 9) is a measure of relative liberty/ conservation with respect to an unpredictable, perfectly-neutral median voter whose measure is 0. A score of x on the Scale implies a difference from the median voter by a fraction $\Phi(x)$ of random voters, where $\Phi(\cdot)$ is the cumulative density function of the normal distribution with mean 0 and variance 1. We set the positive direction on y to mean more liberty, by manually putting the right-most parties which we know beforehand onto the positive half of the y-axis.

3.3.2 Methodology Used for Ukraine

We first describe the political space of the Ukraine Parliament from 2007-present. Figure 9 shows the random-walk ideal points of factions of the Parliament in Convocation 6, 7, and 8.

In Figure 9a we present the estimation result for parties in the Convocation 6. We estimate the ideal points dynamically assuming a random walk process for each party while treating the members of each party as one voter. Through Convocation 6 we observe gradual divergence in parties, with the Communist Party of Ukraine at one end and the Yulia Tymoshenko Bloc at the other end. The scale on the vertical axis is the normalized standard deviation from 0 which is the ideal point of a perfect median voter.

Convocation 7 witnessed the Ukrainian Revolution of 2014. The revolution was followed by a series of changes in Ukraine's political system, including the formation of an interim government and the restoration of the 2004 constitution. As Figure 9b shows, our estimation identifies the revolution in the winter of 2014. The biggest party, the Party of Regions, almost flipped its political standing from far left (pro-Russian) to slightly right. Other parties in the Parliament also moved to the right,



(a) Convocation 6.

(b) Convocation 7.

(c) Convocation 8.

(d) Legend: Parties in each Convocation.

Figure 9: Ideal points by party.

showing the profound impact of the Euromaidan Revolution.

At the beginning of Convocation 8, five of the parliament's pro-European parties, namely the Petro Poroshenko Bloc, People's Front, Self Reliance, Fatherland, and Radical Party, signed a coalition agreement. Per the coalition agreement, the current convocation is tasked with passing major reforms to ensure Ukrainian membership in European institutions such as the EU and NATO, while dealing with the threat of further Russian aggression and intervention against Ukraine. As Figure 9c shows, our estimation identifies a disruption in the Parliament in the late summer of 2015. We associate this disruption inside the Parliament with the historical event of the "Grenade Attack" that happened at that time.The attack occurred on the day parliament voted for the constitutional amendments required to implement the Minsk II agreements. The nationalist Radical Party of Oleg Lyashko was the most affected by the disruption.

3.4 Faction Change Detection

3.4.1 Background

An alternate method of discovering legislative change points is to first find groups of aligned MPs or *factions*. More formally, a faction is defined as a recognized political group with a defined political agenda and sometimes with formal membership requirements [21, 22]. Large changes in faction membership, then, can be thought of as legislative change points.

Much of legislative analysis relies on co-voting, or the instances that MPs cast the same vote on the same bill [32, 48]. One approach to faction detection, then, is through network analysis, where the network links MPs based on their co-voting frequency. After a network is created, a number of empirically verified community detection algorithms could be applied to find factions [7, 15, 45]. Comparison of network creation and grouping algorithm combinations has been completed in [38], which we use to inform our choice of method.

Once the factions are defined, a method of group-based change detection must then be applied. This problem is a simplified version of dynamic community detection, which seeks to understand how communities evolve in time [4, 50]. The two most popular approaches to dynamic community detection are Generalized Louvain, and GraphScope [44, 52]. GraphScope takes an *online* approach, meaning that it allows for real-time change detection and can account for MPs leaving and entering. However, it cannot leverage the full timeline, and as such is susceptible to noise. Generalized Louvain utilizes the full timeline, but does not account for MPs leaving or entering, and requires user-defined parameters. Instead, we use a simple alternative method described in Section 3.4.2.

3.4.2 Methodology Used for Ukraine

In [38], it was found that a Gower-Mean Shift method is both intuitive and gives similar results to many other possible methods. As such, we will apply it here. First, a brief summary of the method.

A network is constructed where nodes represent MPs. The nodes are linked by their Gower similarity: $S_{ij} = \frac{\sum_{k=1}^{N} w_k \delta(x_{ik}, x_{jk})}{\sum_{k=1}^{N} w_k}$, where w_k is a weighting on bill k, x_{ik} represents the vote cast on bill k by MP i, and δ is the Kronecker delta function [24]. A bills weight, w_k, is calculated based on the roll call's Shannon entropy: $w_k = \sum_v p_v \log(p_v)$, where v, is the type of vote cast (for, against, abstain, etc.), and p_v is the proportion of the parliament casting vote type v. Entropy, then, is a measure of how contentious a bill is. Bills that split the parliament in half, are weighted higher than bills for which everyone agrees. It should be noted that entropy weighting has a limitation which can be demonstrated through an example: If a bill gets 95% votes "for," it will receive a very low weight, however, the 5% in opposition are showing signs of strong ties. Finally, Gower's similarity matrix is clustered using

Mean Shift [15], classifying each of the MPs into a faction. This procedure was run on Convocations 6-8.

After defining factions, the change-point analysis can be performed. The basis of the analysis is that time segments should: one, not contain major changes in group membership, and two, have different group memberships from adjacent time segments. These competing goals balance the number of segments the timeline is split into.

More formally, a co-group network, G_t, is defined at each time segment, t. This network links nodes if they were placed in the same faction for a time period t. Then, a pairwise similarity matrix is defined as $H_{t_1,t_2} = s(G_{t_1}, G_{t_2})$, where s is the Product-Moment Correlation between the graphs. The goals described, then, can be formalized as internal and external similarities:

$$s_{\text{internal}}(H, \mathbf{t}) = \sum_{p=1}^{P-1} H_{t_p:t_{p+1}, t_p:t_{p+1}};\ s_{\text{external}}(H, \mathbf{t}) = \sum_{p=1}^{P-2} H_{t_p:t_{p+1}, t_{p+1}:t_{p+2}}, \tag{47}$$

where $\mathbf{t}$ is a vector indicating the time segments, and P is the number of partitions+2, for each of the ends. The value of P is initially 3. The partitions are placed such that s_{internal} is maximized. The number of partitions is then increased, and placement is repeated. If the gain in internal similarity outpaces that of external similarity, the process repeats. If not it is terminated. The second-to-last iteration is then taken as the time segmentation.

3.5 Results

3.5.1 Legislative Change Points

3.5.1.1 Ideal Points

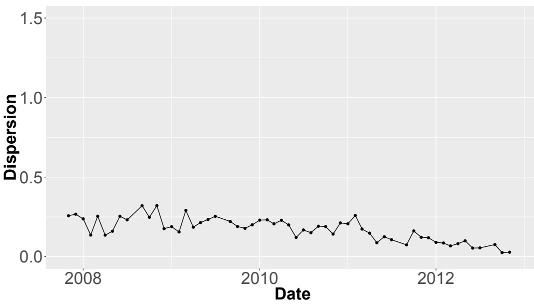
From the ideal points perspective, divergence is identified by the variance of all MPs in the Parliament. We measure the polarization of the Ukraine Parliament by the auto-variance of party ideal points. Specifically, we calculate the following measure: $V_t = [\sum_i (x_{i,t} - x_{i,t-1})^2]^{\frac{1}{2}}$. From our dispersion measure, we identify two structural breaks in the Parliament, shown in Table 16.

3.5.1.2 Factions

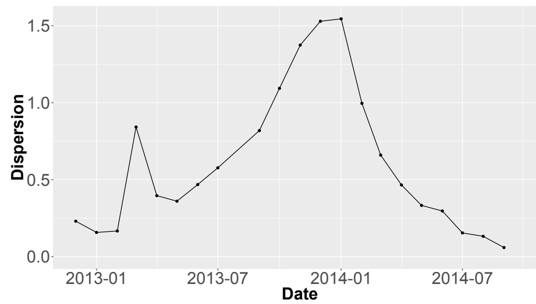
The data for convocations 6, 7, and 8 were fed through the faction detection and network partitioning algorithms. The resulting partitions of the faction similarity matrix are shown in Figure 11 and resulting breaks are given in Table 16. This figure shows the temporal structure of factions. In Convocation 6, we see that there is a major change after April 2010, after which the factions are extremely stable. This stability is seen through the highly correlated block in the similarity matrix after the partition. Convocation 7 shows the opposite: stable faction structure until the break in February 2014. We also see two partitions in convocation 8, on April 2015 and April 2016, though they are far less dramatic than those of prior convocations.

3.5.2 Discussion

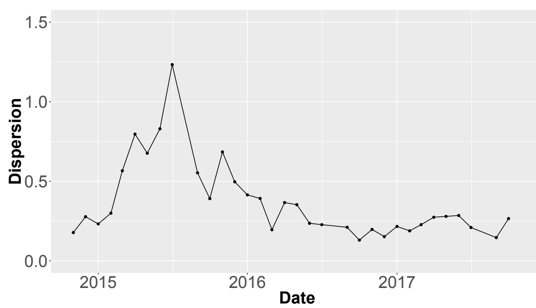
We first investigate polarization in Convocation 8 using the ideal points model. We find that polarization is most visible among new MPs to the convocation. Through



(a) C6. No spike in party dispersion.



(b) C7. Spike is found on December 2013.

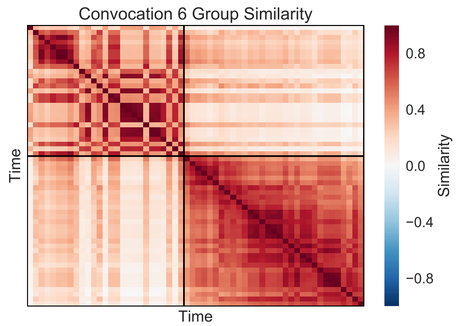


(c) C8. Spike is found on June 2015.

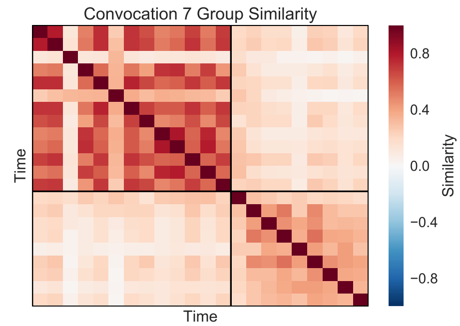
Figure 10: Dispersion of parties ideal points.

the Convocation, most party leaders maintained their own stances (which are also consistent with their party ideology), while many independent MPs changed their position on the political spectrum over time.

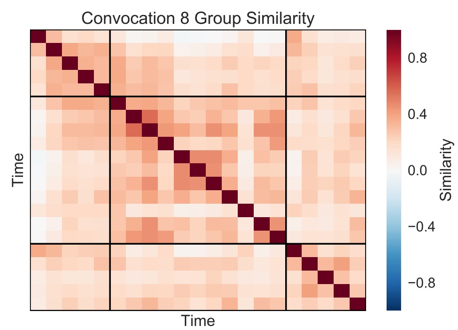
In Convocation 6, one large faction initially dominates the legislature. After April 2010, however, this faction splits roughly in half, creating a more balanced parliament. In convocation 7, we see large changes in membership from the presidential



(a) C6. Partition on April 2010.



(b) C7. Partition on February 2014.



(c) C8. Partitions on April 2015 and 2016.

Figure 11: Induced partitions in the group similarity matrix for each convocation.

faction, to the opposing faction. In this shift, we see the opposing faction gaining a majority in the parliament. In Convocation 8, the minority faction is jockeying for power. They gain a significant number of members after April 2015. In April 2016,

Table 16: Structural breaks

Method	**Date of Break**	**Event description**
Ideal Points	1/1/2014	Euromaidan Revolution
Ideal Points	7/1/2015	Grenade Attack
Factions	4/1/2010	Presidential Election
Factions	2/1/2014	Euromaidan Revolution
Factions	4/1/2015	No Clear Event
Factions	4/1/2016	No Clear Event

there are just minor trades between factions.

3.6 Conclusion

Both methods identify the revolution in Ukraine in 2014. The faction detection method also detects structural changes prior to the revolution (election of the president whose tenure was ended early by the revolution), while the ideal points method performs stronger after 2014 identifying a disruption around voting on constitutional changes to implement Minsk II agreements between separatists and Ukraine. The ideal point method is better at detecting position changes of the members of parliament, while the faction method is better at detecting changes in relationships between different MPs. The results suggest that after 2014, the Ukrainian parliament has become more consolidated, but the distribution of its political positions continues to evolve in response to changes in geopolitical conditions.

Appendix Appendix

A.1 Solution to the life cycle model

The life cycle model is computationally solved using backward induction from the agent's last period T. The agent has a binding constraint only when he has little or negative asset b and is hit by negative shocks x_t, y_t. Therefore, a numerical solution is necessary.

I estimate the main components of the model from my Experian-imputed MEPS data. I estimate the income process using the panel MEPS data, assuming an AR(1) process. The specification for the income process is:

$$\begin{aligned} log(w_{i,j,k,t}) &= \phi_j + h_k + y_{i,t} \\ y_{i,t} &= \alpha y_{i,t-1} + \epsilon_{i,t} \\ \epsilon_{i,t} &\sim N(0, \sigma_\epsilon^2) \end{aligned}$$

where ϕ_j is the deterministic age profile that depends on age j, h_k is the deterministic health effect that depends on health level k, and $y_{i,t}$ follows the AR(1) process of slope α and intercept β. The estimation result is in the online Table.

I estimate λ in the borrowing constraint according to a reduced version of the credit access proxies 5 that only depends on income and age. The specification of the borrowing constraint is:

$$log(CreditCard_{i,t}) = \alpha Age_{i,t} + \beta Income_{i,t} + \epsilon_{i,t}$$

where $Age_{i,t}$ and $Income_{i,t}$ are the age and income and $CreditCard_{i,t}$ is the credit card limit. The estimation result is in the online Table.

I estimate the medical cost function using the MEPS, utilizing its short panel structure. Because each MEPS respondent is only observed consecutively twice, I assume $x_{i,t}$ is Markovian and can be captured by the MEPS variables of self-reported incidence of needing regular, specialist's, or immediate care[1]. The health cost function can be transformed into the health process below:

$$h_t = h_{t-1} + pm_t - x_t$$

where $p = 1/\mu$. I then estimate the following instrumental variable specification:

$$\begin{aligned} m_{i,t} &= Income_{i,t} + Insured_{i,t} + \eta_{i,t} \\ \Delta h_{i,t} &= p\widehat{m}_{i,t} + \gamma x_t + \epsilon_{i,t} \end{aligned}$$

where $\Delta h_{i,t}$ is the change in the self-reported health level, $\widehat{m}_{i,t}$ is the instrumented medical expenditure, and $x_{i,t}$ is the self-reported incidence of illness needing levels of care. γ_j is the marginal effect of the illness shock $x_{i,t}$. The estimation result is in the online Table.

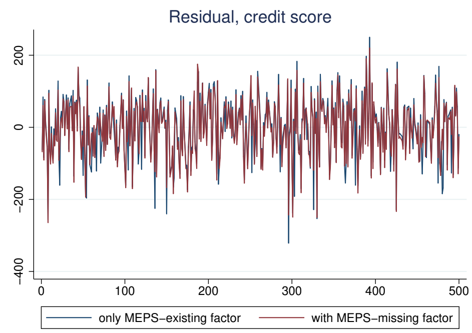
[1]This assumption fits the purpose of this model as the main mechanism is the local treatment effect of credit access on medical expenditure.

A.2 Tables and figures

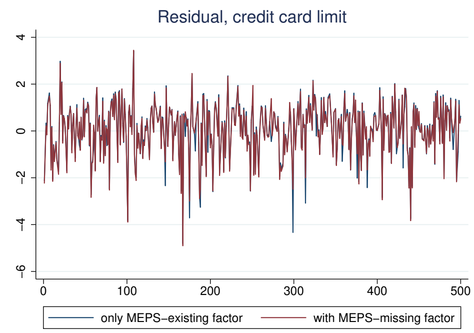
Table 17: Correlation: delinquency and default flag against debt, income, and age.

Correlation	past due	default	mortgage	car loan	unsecured loan	income	age
past due	1	0.184***	-0.0148***	0.0164***	0.114***	-0.116***	-0.0595***
default		1	-0.0851***	-0.0214***	-0.105***	-0.164***	-0.0602***
mortgage			1	0.186***	0.332***	0.487***	0.158***
car loan				1	0.240***	0.149***	-0.0156***
unsecured loan					1	0.435***	0.0787***
income						1	0.447***
age							1

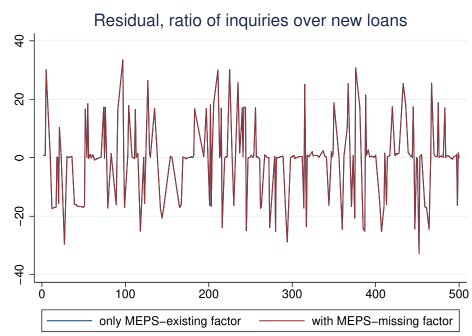
Note: * denotes 10% significance level. ** denotes 5%, and *** denotes 1%. Standard errors are in parentheses. Past due is the one-period lag indicator for having any past due debt. Default is the one-period lag indicator for having any default debt. The data source is the Experian consumer credit panel 2006-2016.



(a) Credit score.



(b) Credit card limit.



(c) Number of inquiries per new loan opened.

Figure 12: Comparison of the residuals with and without the default flags in regressions.

Table 18: Comparison of summary statistics: credit access measures.

Statistics	Mean	Std.	p25	p50	p75	min	max
Panel a: imputations for the MEPS							
Mortgage	34735.2	70928.8	0	0	48000	0	2000000
Car loan	2830.58	5943.242	0	0	3500	0	200000
Unsecured debt	4182.341	21598.74	0	0	1250	0	2009000
Income	9.758434	3.386697	10.08581	10.83408	11.40756	0	13.58138
Age	41.78306	12.3299	31	42	52	21	70
Credit score	517.4462	458.4236	535.3419	645.0459	732.2331	-919.6591	1292.836
Credit card limit	10172.24	22291.02	764.8925	3314.364	10208.2	6.29e-06	839168.1
Ratio of inquiries	49.04681	.7762228	48.70067	49.2375	49.55679	40.53638	51.10061
Panel b: the Experian data							
Mortgage	49105.48	101272.4	0	0	64849	0	7700000
Car loan	5175.4	11417.61	0	0	6597	0	5054568
Unsecured debt	14741.67	35082.76	0	2486	14430	0	11900000
Income	68715.58	36989.93	42000	61000	86000	15000	199000
Age	43.80435	13.46806	32	44	55	22	69
Credit score	665.7692	114.6332	576	675	763	1	839
Credit card limit	26538.42	29884.94	4850	16720	38068	1	719101
Ratio of inquiries	49.44303	12.69389	50	50	50	3	90

Note: * denotes 10% significance level. ** denotes 5%, and *** denotes 1%. The data source is the MEPS public use files 2006-2016 and the Experian consumer credit panel 2006-2016.

Table 19: Summary statistics: county-level credit access measures by age-income groups.

Measures	Mean	Std.	Measures	Mean	Std.	Measures	Mean	Std.
score11	583.6194	25.77138	credit limit11	4062.322	1284.152	ratio inquiries11	50.80258	3.453974
score21	542.5756	26.65608	credit limit21	4161.74	2844.208	ratio inquiries21	51.47817	5.750031
score31	542.6819	28.59084	credit limit31	4559.587	3856.297	ratio inquiries31	51.13022	6.944355
score41	551.8751	38.47494	credit limit41	5154.185	6340.643	ratio inquiries41	51.17112	8.597897
score51	565.4423	57.57637	credit limit51	5381.767	7757.011	ratio inquiries51	50.0492	9.382549
score12	650.9164	25.79332	credit limit12	8054.599	2420.122	ratio inquiries12	50.72082	3.365927
score22	598.9288	27.2228	credit limit22	7905.803	2930.843	ratio inquiries22	51.04206	4.615455
score32	590.2275	28.024	credit limit32	8022.899	3924.23	ratio inquiries32	51.1673	5.166892
score42	602.535	31.02767	credit limit42	8543.016	4990.03	ratio inquiries42	50.48442	5.466717
score52	621.7662	43.4446	credit limit52	8964.356	6720.722	ratio inquiries52	49.99199	7.032523
score13	703.8438	26.9599	credit limit13	16025.77	4949.427	ratio inquiries13	50.63769	4.604067
score23	658.3699	29.06922	credit limit23	15709.13	4814.05	ratio inquiries23	50.75607	3.943757
score33	638.6928	31.93352	credit limit33	15493.38	5797.458	ratio inquiries33	50.45803	4.757093
score43	651.1612	33.13582	credit limit43	16110.25	6067.424	ratio inquiries43	50.19778	4.611744
score53	674.5954	36.10082	credit limit53	16962.14	8205.891	ratio inquiries53	49.60057	6.340053
score14	726.4089	33.48923	credit limit14	26321.53	8011.237	ratio inquiries14	50.64458	5.899489
score24	714.3789	26.87045	credit limit24	27978.7	6710.621	ratio inquiries24	50.40466	4.050053
score34	709.6876	27.67512	credit limit34	30151.77	7894.034	ratio inquiries34	50.23852	3.981043
score44	726.649	25.80097	credit limit44	32164.6	7703.829	ratio inquiries44	49.94037	3.792344
score54	751.6354	23.91267	credit limit54	32609.41	7900.857	ratio inquiries54	49.61176	4.651764
score15	733.6198	47.38928	credit limit15	36705.86	15934.77	ratio inquiries15	50.47937	9.18811
score25	737.9114	28.4007	credit limit25	42150.91	9063.058	ratio inquiries25	50.41393	4.776689
score35	743.9538	22.97425	credit limit35	47962.56	8727.795	ratio inquiries35	49.809	3.802323
score45	760.6002	18.71417	credit limit45	52227.97	8620.462	ratio inquiries45	49.61083	3.401958
score55	778.5586	17.776	credit limit55	52739.44	8974.626	ratio inquiries55	49.35802	4.108096

Note: * denotes 10% significance level. ** denotes 5%, and *** denotes 1%. The first digit represents the age bin and the second digit represents the income bin. The data source is the Experian consumer credit panel 2006-2016.

Table 20: Effect on the young population age 21-35: regression of medical expenditures on credit access. Dependent variables are the same as in the regressions in Section 5 main results.

Second stage:	Expenditure (1)	Expenditure, IV (2)	Emergency visit (3)	Emergency visit, IV (4)	Insurance (5)	Insurance, IV (6)
Panel a: credit access measure is imputed credit score						
imputed credit score	1.035***	1.943***	0.107**	-0.478***	0.467***	0.741***
	(0.0739)	(0.267)	(0.0480)	(0.164)	(0.0445)	(0.137)
Controls	S	S	S	S	S	S
R^2/F-statistic	0.174	0.167	12.71	12.63	24.84	29.08
Observations	86,977	86,977	77,978	77,978	86,977	86,977
Panel b: credit access measure is imputed credit card limit						
imputed credit card limit	0.175***	0.398***	0.0201***	-0.192***	0.0730***	0.179***
	(0.00965)	(0.106)	(0.00576)	(0.0395)	(0.00533)	(0.0424)
Controls	S	S	S	S	S	S
F-statistic	0.184	0.154	13.67	13.16	25.13	40.27
Observations	86,980	86,980	87,765	87,765	86,980	86,980
Panel c: credit access measure is imputed number of of inquiries per new loan						
Imputed ratio of inquiries	-1.028***	-0.662***	-0.171***	-0.0532	-0.216***	-0.133
	(0.0899)	(0.221)	(0.0563)	(0.128)	(0.0499)	(0.144)
Controls	S	S	S	S	S	S
F-statistic	0.172	0.171	41.15	39.39	113.22	108.28
Observations	85,034	85,034	76,676	76,676	85,017	85,017

Note: S stands for "health and insurance controls, age and income controls, and year and state fixed effects". * denotes 10% significance level. ** denotes 5%, and *** denotes 1%. Standard errors are in parentheses. The credit score is the imputed MEPS respondent's credit score. The credit card limit is the log of the imputed MEPS respondent's credit card limit. Ratio inquiry is the imputed MEPS respondent's ratio of loan inquiries over new loans opened. The data source is the MEPS panel 2006-2016.

Table 21: Robustness check: regression of households' opinion on credit access. The dependent variables are households' opinions on the need and the worth of health insurance.

Second stage: 2SLS	need, OLS (1)	need, IV (2)	worth, OLS (3)	worth, IV (4)
Panel a: credit access measure is imputed credit score				
imputed credit score	-0.0772***	0.0892	0.0824***	0.150
	(0.0174)	(0.0740)	(0.0225)	(0.0912)
Health control	Y	Y	Y	Y
Age and income controls	Y	Y	Y	Y
Year and state fixed effects	Y	Y	Y	Y
Constant	Y	Y	Y	Y
R^2	0.119	0.117	0.0415	0.0413
Observations	75,472	75,472	75,248	75,248
Panel b: credit access measure is imputed credit card limit				
imputed credit card limit	-0.0127***	0.0241	0.00681**	-0.0307
	(0.00231)	(0.0316)	(0.00278)	(0.0441)
Health control	Y	Y	Y	Y
Age and income controls	Y	Y	Y	Y
Year and state fixed effects	Y	Y	Y	Y
Constant	Y	Y	Y	Y
R^2	0.119	0.113	0.0413	0.0366
Observations	75,477	75,477	75,253	75,253
Panel c: credit access measure is the imputed number of inquiries per new loan				
Imputed ratio of inquiries	0.0804***	0.0133	-0.0699**	0.0601
	(0.0219)	(0.0522)	(0.0291)	(0.0651)
Health control	Y	Y	Y	Y
Age and income controls	Y	Y	Y	Y
Year and state fixed effects	Y	Y	Y	Y
Constant	Y	Y	Y	Y
R^2	0.119	0.118	0.0419	0.0412
Observations	70,047	70,047	69,831	69,831

Note: * denotes 10% significance level. ** denotes 5%, and *** denotes 1%. Standard errors are in parentheses. The opinion on need is on a 1-5 scale, with 1 meaning strongly disagree and 5 strongly agree, how strongly the respondent believes that they are healthy enough that they don't really need health insurance. The opinion on worth is on a 1-5 scale, with 1 meaning strongly disagree and 5 strongly agree, how strongly the respondent believes that health insurance is not worth the money it costs. The credit score is the imputed MEPS respondent's credit score. The credit card limit is the log of the imputed MEPS respondent's credit card limit. Ratio inquiry is the imputed MEPS respondent's ratio of loan inquiries over new loans opened. The data source is the MEPS panel 2006-2016.

Table 22: Robustness check: regression of county healthcare access on county credit access.

The dependent variables are on the column names.

FE panel	Number of hospitals (1)	Number of beds (2)	Number of ICU beds (3)
Panel a: credit access measure is county credit score			
County credit score	6.184	-6295.2	-41.37
	(28.96)	(3897.1)	(39.74)
Log county average income	-7.988***	-1605.7***	-22.01***
	(1.030)	(138.6)	(1.414)
Year and county fixed effects	Y	Y	Y
R^2	0.00352	0.00382	0.00579
Observations	35866	35866	35866
Panel b: credit access measure is imputed credit card limit			
County credit card limit	-6.564***	-368.9	-3.535
	(1.674)	(225.9)	(2.271)
Log county average income	-8.330***	-1651.6***	-22.76***
	(1.046)	(141.1)	(1.419)
Year and county fixed effects	Y	Y	Y
R^2	0.00155	0.00333	0.00607
Observations	35488	35488	35488
Panel c: credit access measure is the imputed number of inquiries per new loan			
County ratio of inquiries	-0.0112	-1.737	0.0263*
	(0.0108)	(1.481)	(0.0122)
Log county average income	-11.38***	-1952.2***	-14.03***
	(1.159)	(159.1)	(1.308)
Year and county fixed effects	Y	Y	Y
R^2	0.0000190	0.00178	0.0118
Observations	29328	29328	29328

Note: Dependent variables are denoted per millions of people. * denotes 10% significance level. ** denotes 5%, and *** denotes 1%. Standard errors are in parentheses. The credit score is the imputed MEPS respondent's credit score. The credit card limit is the log of the imputed MEPS respondent's credit card limit. Ratio inquiry is the imputed MEPS respondent's number of loan inquiries per new loan opened. The data source is the MEPS Area Health Resources Files 2006-2016.

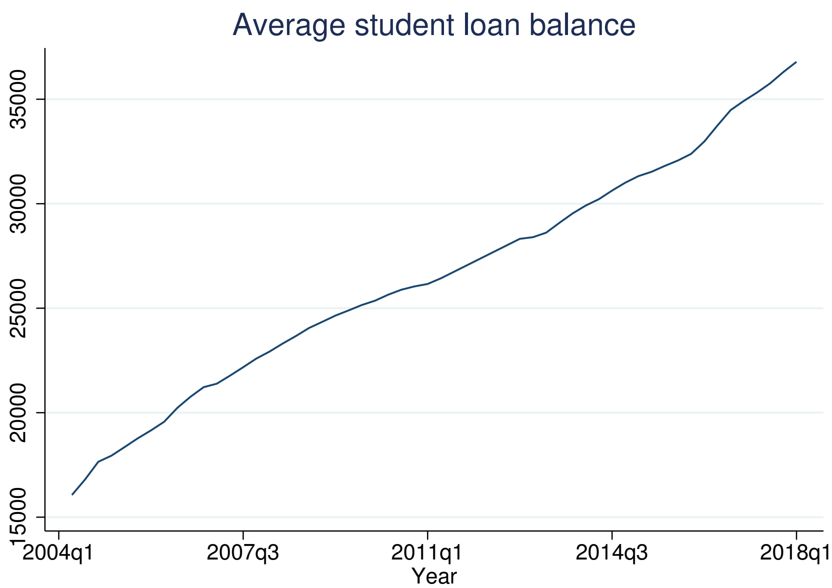


Figure 13: Average student loan balance, CPI-adjusted to 2015 value. Source: calculation from Experian Consumer Credit Panel.

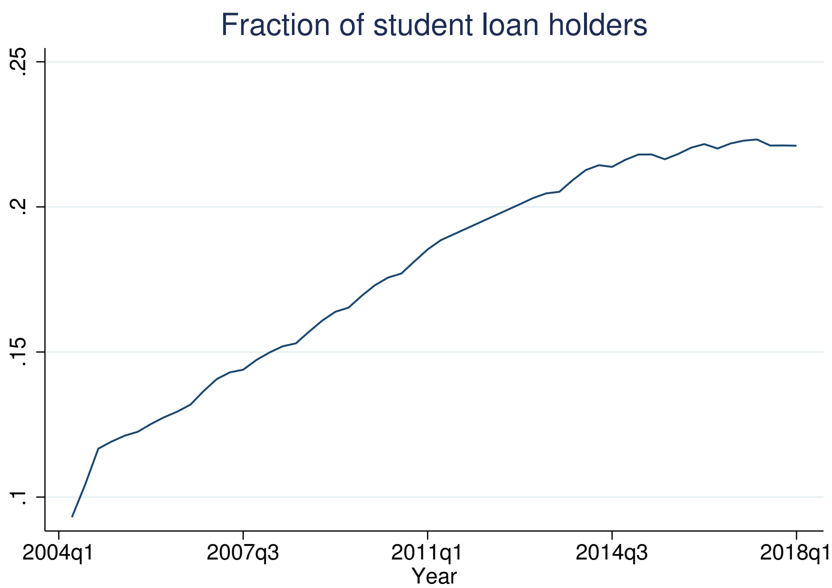


Figure 14: Fraction of population holding student loan debt. Source: calculation from Experian Consumer Credit Panel.

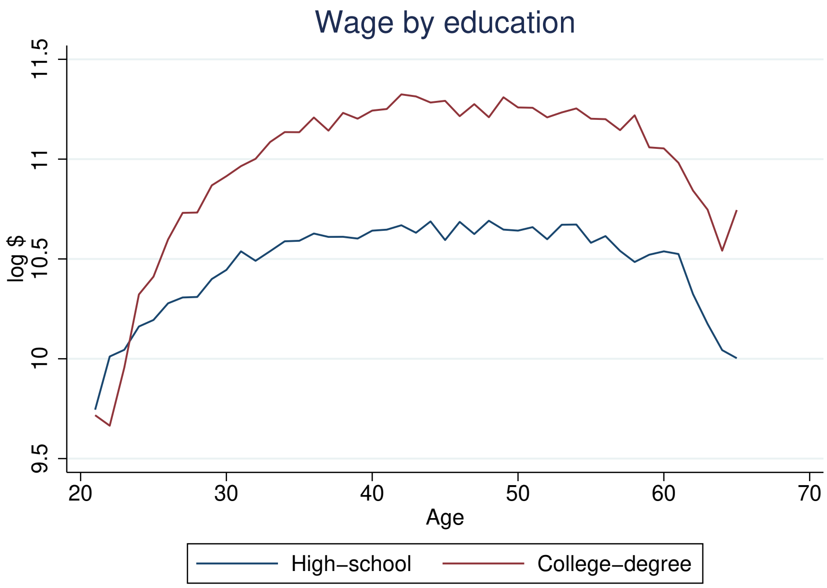


Figure 15: Wage income by age, CPI-adjusted to 2015 value. Source: calculation from the Panel Study of Income Dynamics.

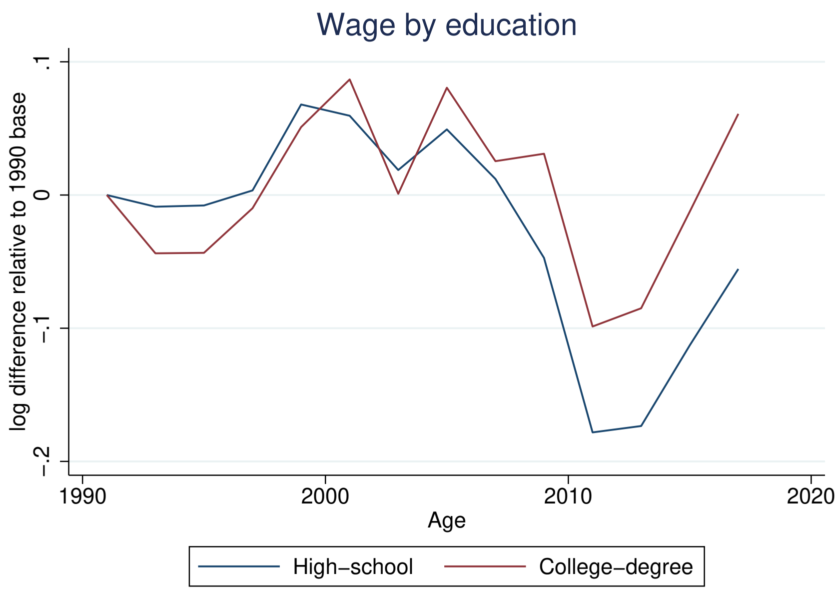


Figure 16: Wage income by year, CPI-adjusted to 2015 value. Source: calculation from the Panel Study of Income Dynamics.

Table 23: Estimates for the wage process of high-school households.

ϕ_a	estimate	std. error	ϕ_a	estimate	std. error
22	0.245	0.053	44	0.881	0.049
23	0.284	0.046	45	0.814	0.050
24	0.386	0.050	46	0.866	0.050
25	0.454	0.047	47	0.834	0.050
26	0.526	0.049	48	0.858	0.051
27	0.521	0.047	49	0.859	0.051
28	0.548	0.048	50	0.804	0.052
29	0.624	0.047	51	0.845	0.052
30	0.640	0.048	52	0.794	0.052
31	0.746	0.047	53	0.854	0.053
32	0.707	0.048	54	0.811	0.053
33	0.761	0.047	55	0.786	0.054
34	0.793	0.048	56	0.766	0.054
35	0.800	0.048	57	0.736	0.055
36	0.834	0.048	58	0.642	0.056
37	0.839	0.048	59	0.675	0.057
38	0.815	0.048	60	0.667	0.058
39	0.840	0.048	61	0.685	0.059
40	0.825	0.049	62	0.500	0.060
41	0.871	0.049	63	0.321	0.062
42	0.869	0.049	64	0.245	0.065
43	0.859	0.049	65	0.152	0.068
constant	9.738	0.043			
Z_t	estimate	std. error			
1	0.030	0.013			
2	-0.001	0.021			
3	-0.052	0.013			
4	-0.020	0.020			
5	-0.041	0.013			
6	-0.029	0.018			
7	0.021	0.016			
y_t	estimate				
α	0.280				
σ_ϵ	0.585				
σ_y	0.644				

Table 24: Estimates for the wage process of college-degree households.

ϕ_a	estimate	std. error	ϕ_a	estimate	std. error
22	-0.016	0.174	44	1.739	0.150
23	0.298	0.150	45	1.746	0.149
24	0.737	0.152	46	1.701	0.150
25	0.817	0.149	47	1.743	0.150
26	1.018	0.150	48	1.690	0.150
27	1.147	0.149	49	1.776	0.150
28	1.161	0.149	50	1.757	0.151
29	1.292	0.149	51	1.739	0.150
30	1.332	0.149	52	1.669	0.151
31	1.388	0.149	53	1.725	0.151
32	1.454	0.149	54	1.723	0.152
33	1.506	0.149	55	1.669	0.151
34	1.579	0.149	56	1.647	0.152
35	1.559	0.149	57	1.615	0.152
36	1.639	0.149	58	1.663	0.153
37	1.622	0.149	59	1.500	0.153
38	1.630	0.149	60	1.497	0.153
39	1.665	0.149	61	1.395	0.154
40	1.676	0.149	62	1.295	0.155
41	1.708	0.149	63	1.118	0.155
42	1.751	0.150	64	0.967	0.157
43	1.742	0.149	65	1.090	0.158
constant	9.519	0.146			
Z_t	estimate	std. error			
1	0.041	0.017			
2	-0.038	0.029			
3	-0.064	0.017			
4	-0.085	0.028			
5	-0.035	0.017			
6	-0.062	0.024			
7	0.008	0.022			
y_t	estimate				
α	0.345				
σ_ϵ	0.601				
σ_y	0.657				

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