

The Impact of Social Insurance on Household Debt *

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Abstract

This paper investigates how the expansion of social insurance affects households' accumulation of debt. Insurance can reduce reliance on debt by lessening the financial impact of adverse events like illness and job loss. But it can also weaken the motive to self-insure through savings, and households' improved financial resilience can increase access to credit. Using data on 10 million borrowers and a quasi-experimental research design, we estimate the causal effect of expanded insurance on household debt, exploiting ZIP code-level heterogeneity in exposure to the staggered expansions of one of the largest US social insurance programs: Medicaid. We find that a one percentage point increase in a ZIP code's Medicaid-eligible population increases credit card borrowing by 0.82%. Decomposing this effect in a model of household borrowing, we show that increased credit supply in response to households' improved financial resilience fully accounts for this rise in borrowing and contributed 33% of the net welfare gains of expanding Medicaid.

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1 Introduction

Unsecured debt is an important tool that households rely on to smooth their consumption—not only across time, but also across states of the world. For example, of the four in ten US adults that report anticipating difficulty in meeting an unexpected \$400 expense, credit cards are the most cited tool they expect to rely on.¹ And when experiencing an income shortfall, 43% of US households report turning to borrowing, including credit cards.² When unsecured debt functions as a de facto source of insurance, changes to social insurance could significantly impact households’ desire and ability to access credit.

Whether social insurance crowds in or crowds out household debt is theoretically ambiguous. The total effect depends on the strength of competing channels. On the one hand, improved access to social insurance can reduce households’ reliance on debt to cope with adverse shocks such as job loss and illness, crowding out debt. On the other hand, credit demand and supply channels may also work in the opposite direction. Social insurance can increase the demand for debt by weakening the precautionary savings motive. Additionally, when social insurance makes households less likely to default, this reduction in risk can make creditors more willing to lend. Expanded insurance can improve households’ financial resilience, which can in turn increase access to credit.

This paper sheds new light on how credit markets shape the impact of social insurance on household debt and welfare. We first estimate the causal effect on credit card borrowing of the expansion of Medicaid under the Affordable Care Act (ACA), one of the largest changes to the US social safety net in recent years. We find that a one percentage point increase in the share of a ZIP code’s population eligible for Medicaid increases revolving credit card balances by 0.82 percentage points. Next, we develop a heterogeneous-agent model where households face both income and medical expenditure shocks, while having the ability to borrow and default on unsecured debt. Simulating the expansion of Medicaid in the model, we find that increased credit supply is fully responsible for the overall increase in household debt. Moreover, the credit supply response to expanding Medicaid accounts for 33% of the

¹Source: 2019 SHED.

²Source: 2016 SCF.

net welfare gain.³ Our findings demonstrate that expanding social insurance can *crowd in* the supply of unsecured debt, and that this improvement in credit access can lead to first order welfare gains. We conclude that policymakers may significantly underestimate the benefits of social insurance programs if they overlook potential increases in credit access spurred by improved financial resilience.

Medical expenses are an important source of financial distress for US households. In a survey of bankruptcy filers, 29% named medical bills as the reason for their bankruptcy (Himmelstein, Thorne, Warren and Woolhandler, 2009). In a recent survey of US residents, unexpected medical bills ranked first in terms of what kind of expenses they worry most about being able to afford, ahead of rent or mortgages, transportation, and food.⁴ Health expenditures comprise a large and growing share of US GDP, reaching 18.3% in 2021.⁵ Despite the importance of health expenses, they have received less attention in the macroeconomics literature compared to other sources of risk such as job loss and housing.⁶

We begin by estimating the causal effect of Medicaid eligibility on a variety of credit outcomes using credit bureau data on 10 million US borrowers over 2010-2021 in a continuous difference-in-difference analysis. We exploit granular variation in the *size* of eligibility changes induced by states' staggered expansions of Medicaid under the ACA. This variation arises from two sources. The first comes from states' pre-expansion Medicaid policies. Expanding Medicaid under the ACA required states to implement a common set of eligibility requirements. As a result, ZIP codes with similar distributions of income saw larger increases in eligibility if their state previously had stricter requirements. The second source of variation comes from within-ZIP differences in the distribution of income among low-income households. Eligibility increased more ZIP codes that had more low-income households clustered between the pre and post-expansion income eligibility thresholds.

Our difference-in-difference estimator compares credit outcomes before and after expand-

³Our measure of net welfare reflects improvements stemming from the financial benefits of the Medicaid expansion (net of the tax costs of expansion). We do not model mortality nor utility as a function of health. Therefore, our welfare estimates do not reflect gains from improved health or reduced mortality.

⁴Source: Kaiser Family Foundation Health Tracking Poll (February 2020). Link: <https://www.kff.org/health-costs/poll-finding/data-note-public-worries-about-and-experience-with-surprise-medical-bills/>.

⁵Source: Centers for Medicare and Medicaid Services NHE Fact Sheet.

⁶Notable exceptions include Livshits, MacGee and Tertilt (2007); Chatterjee, Corbae, Nakajima and Ríos-Rull (2007); Attanasio, Kitao and Violante (2010); De Nardi, French and Jones (2010).

ing across ZIP codes with bigger versus smaller changes in eligibility. The within-state variation in the impact of expansions makes it possible to use state-time fixed effects in our analysis, which help account for other unobserved state-level policy changes that may have coincided with expansions and also impacted credit outcomes. Identification requires that the change in eligibility is not correlated with other shocks coinciding with the expansion.

We find that expanded Medicaid eligibility increases credit card borrowing. A one percentage point increase in eligibility leads to a 0.36 percentage point increase in the share of households with at least one credit card and raises revolving credit card balances by 0.82 percentage points. Consistent with a positive credit supply response, we document that increased eligibility also led to increases in both credit limits and the success rate of credit card applications. On the demand side, we also find that the number of credit card inquiries rose as well. In line with our financial resilience hypothesis, we show that expanding Medicaid also reduced default (both delinquencies and debt in collections) and increased credit scores.

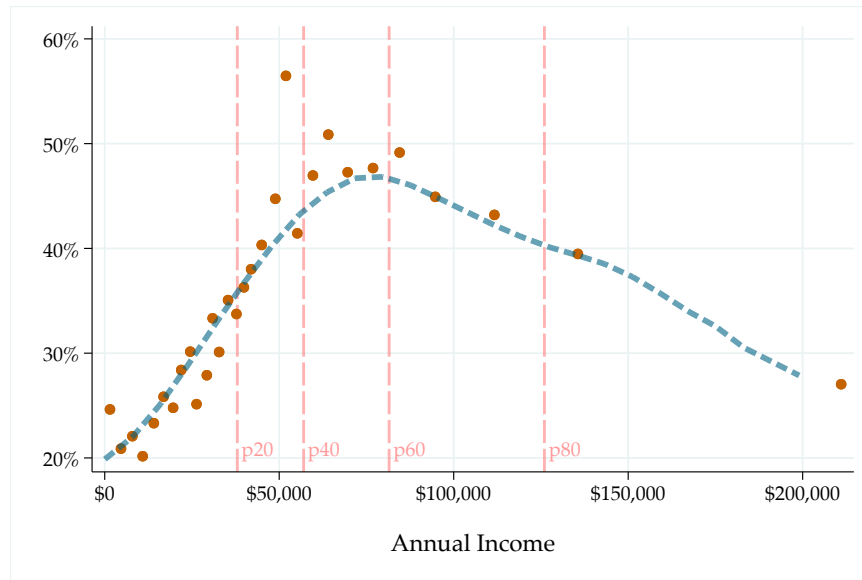
We then construct a heterogeneous-agent model in which households have access to credit card debt. Households face idiosyncratic income shocks as well as idiosyncratic expenditure shocks. They can save using a risk-free asset or borrow via credit card debt, which they can decide not to repay. Households incur debt both by choosing credit card borrowing and as a result of experiencing expenditure shocks that they are unwilling or unable to cover on impact. The interest rate paid on credit card debt is endogenous and depends on the probability households do not pay their owed debt.

The nature of credit card debt in our model is a hybrid of one-period and long-term debt used in the literature on consumer bankruptcy and sovereign debt. When households are not delinquent, they must roll over their debt that period. That is, credit card debt must be repaid in full to avoid delinquency. However, households have the option not to repay their debt and enter a delinquent state. In that state, they cannot take on more debt before paying their current debt in full.⁷ After a stochastic amount of time, households in a delinquent state get a haircut on their debt. Since financial intermediaries who hold claims on delinquent debt are not repaid immediately, the pricing of debt has a long-term component to it.

⁷While households cannot choose to take on more debt in the delinquent state, uninsured expenditure shocks add to the households' total debt.

The delinquency option on debt allows us to capture a key aspect of the data. The relationship between having credit card debt and income follows an inverse U-shape, as displayed in Figure 1. Less than 25% of households with annual income below \$25,000 have any credit card debt. This share rises to 50% for households with an annual income of \$70,000, and declines as household income rises. Delinquency, together with endogenous credit supply, restricts low-income households' access to credit.

Figure 1: Share of Households with Credit Card Debt (2017)



Notes: This figure plots a binscatter of the share of households with a non-zero amount of credit card debt. Data come from the 2017 PSID.

Capturing the inverse U-shape relationship between income and credit card debt is important for studying policies that target low-income households, such as Medicaid. This is because means-tested social insurance policies generally target households whose default risk limits their credit access. By reducing this risk, social insurance can lead to better credit access. We model Medicaid as policy that covers a fraction of households' expenditure shocks, where eligibility for Medicaid depends on their income.

Health insurance policies affect the aggregate level of credit card debt through three channels. First, the direct effect of more generous health insurance is increasing households' disposable income. Households can achieve the same consumption levels while borrowing less. Therefore, the direct effect of health insurance is a reduction in debt levels.

The second channel is through credit demand. Health insurance affects demand for credit

even if credit terms remain unchanged. Lower medical expenditures reduce households' precautionary savings motive, and as a result, increase their borrowing. However, households are more likely to repay their debt in the future. This results in a higher level of *expected* repayment, which discourages borrowing. These competing forces mean that the effect of the credit demand channel is theoretically ambiguous.

The third channel is the credit supply channel. The reduction in delinquency rates leads to lower interest rate spreads in equilibrium. Lower interest rates induce households to take on more credit, leading to an increase in the aggregate level of credit card debt.

While we ground our model in the context of health insurance, the economic channels we study can arise in many other contexts. Our model and approach can be readily adapted to study the impact of other types of social insurance (unemployment insurance, minimum wages, disability insurance, etc.). Additionally, a similar model could be used to study how changes to insurance/risk exposure affect sovereign and corporate credit outcomes. Quantitatively, which channels dominate in equilibrium may vary with context.

Our model abstracts away from several features of health. Notably: "physical" health that enters the utility function, price-sensitive demand for health services ("moral hazard"), and correlations between income and health expenditures. As we discuss in detail in Section 3, this likely makes our counterfactual analysis conservative regarding the welfare and borrowing responses.

Consistent with our empirical findings, our model predicts that expanding Medicaid led to a 1.6% overall increase in credit card debt. We find that both the direct channel and the credit demand channel reduce the aggregate level of credit card debt. Together, these two channels decrease credit card debt by 2.2%. The overall increase in credit card debt is solely due to the credit supply channel, which increases aggregate credit card debt by 3.9%.

We use our model to study the welfare benefits associated with the expansion of Medicaid. We find that the policy is equivalent to a permanent increase in consumption of 9 basis points. Per newly eligible person, this is a 5.8 percentage point consumption equivalent increase. 33% of the welfare gains are due to the reduction in interest rate spreads households face on their debt. This result suggests policy makers should take into account the effect social insurance has on credit supply. Disregarding the credit supply channel could substantially

underestimate the welfare gains of social insurance programs.

Related Literature: This paper contributes to four strands of literature by bringing a new macroeconomic perspective to the effects of social insurance. First, our model builds on the macroeconomic literature on consumer bankruptcy and default (e.g., [Chatterjee, Corbae, Nakajima and Ríos-Rull, 2007](#); [Livshits, MacGee and Tertilt, 2007](#); [Mitman, 2016](#)). This literature focuses on the drivers of default, in particular the role of bankruptcy policy. We study a policy that does not directly target bankruptcy or default: public health insurance. This policy can reduce default and increase both credit access and borrowing.

Second, we add to a large macroeconomic literature on heterogeneous agent models with uninsurable risk ([Bewley, 1986](#); [Aiyagari, 1994](#); [Huggett, 1993](#)). This literature highlights how precautionary savings play a key role in shaping the macroeconomy. Our work studies how insurance provision can reduce the precautionary savings motive and traces the macroeconomic implications. We take a partial equilibrium approach, i.e., the risk-free rate in the economy is taken as given. In this sense, our approach is similar to the work of [Imrohoroglu \(1989\)](#), [Zeldes \(1989\)](#), [Deaton \(1991\)](#), and [Hubbard, Skinner and Zeldes \(1995\)](#) who also study how social insurance affects precautionary savings motive. We build on these works by taking into account the impact social insurance has on the endogenous debt pricing schedule. We also build on [Krueger and Perri \(2011\)](#). Their model features contingent contracts, resulting in no default occurring in equilibrium and credit supply *decreasing* when social insurance expands (financial autarky becomes less costly and thus default more tempting). Our model instead features non-contingent defaultable debt and leads to a positive credit supply response, which is consistent with our empirical estimates. Additionally, our analysis of the impact of social insurance on credit markets complements concurrent work studying the "opposite" question: the impact of credit access on the design of optimal UI ([Braxton, Herkenhoff and Phillips, 2022](#)).

Third, we build on an empirical microeconomic literature studying the consumer finance consequences of expanding social insurance. We present new evidence on *revolving* credit card debt and, using our model, quantify the welfare effects and decompose the underlying channels. Prior work on Medicaid finds that expansions reduced medical debt, missed

debt and bill payments, reliance on payday loans, and debt in collections (Allen, Swanson, Wang and Gross, 2017; Hu, Kaestner, Mazumder, Miller and Wong, 2018; Miller, Hu, Kaestner, Mazumder and Wong, 2018; Gallagher, Gopalan and Grinstein-Weiss, 2019b; Goldsmith-Pinkham, Pinkovskiy and Wallace, 2020b). Reduced debt and delinquency improved FICO scores after expansions, leading to lower interest rates on credit card offers (Brevoort, Grodzicki and Hackmann, 2017) and increased mortgage application approval rates (C  lerier and Matray, 2017). Our focus on borrowing complements Gallagher, Gopalan and Grinstein-Weiss (2019a), which finds that health insurance reduces savings among households experiencing financial hardship. Less saving does not immediately imply higher gross or net borrowing.⁸ Research on unemployment insurance also finds evidence of improved financial resilience (lower mortgage default) and expanded credit access (lower credit card offer rates) (Hsu, Matsa and Melzer, 2018). Our paper also relates to Aaronson, Agarwal and French (2012), which documents strong and positive spending and collateralized debt increases among minimum wage workers following rises in the minimum wage. We build on their findings by documenting that revolving credit card debt also rises after the expansions of another part of the social safety net (Medicaid). We show that this rise is accompanied by a decline in default, improvements in credit scores, and increases in measures of credit supply (e.g., credit card approval rates).

Finally, our work is related to a recent literature that investigates the relationship between household debt and the macroeconomy. This literature finds that increases in household debt can portend macroeconomic downturns and financial crises (Jord  , Schularick and Taylor, 2015, 2016; Mian, Sufi and Verner, 2017; Gomes, Grotteria and Wachter, 2019; Mian, Sufi and Verner, 2020). By focusing on *how* institutional features such as social insurance affect borrowing across households, this paper sheds new light on the relationship between household debt and the macroeconomy. In particular, a high *level* of household debt can indicate that households are well-insured and financially resilient.

This paper is organized as follows. Section 2 presents background on the Medicaid ex-

⁸In contrast to standard models of consumption and saving, household borrowing presents a "credit card puzzle" in that households tend to hold both high-interest credit card debt and low-interest savings simultaneously (Gross and Souleles (2002)). This behavior is consistent with a motive to maintain a liquidity buffer in the presence of incomplete markets (Telyukova, 2013; Druedahl and J  rgensen, 2018).

pansions and the empirical analysis of the impact of expansions on credit card debt. Next, Section 3 presents the model. Section 4 analyzes policy counterfactuals on debt (its distribution and aggregate level) as well as welfare. We decomposes the effect of expanding health insurance on borrowing into its direct impact on debt for households experiencing adverse shocks and its general equilibrium impacts through credit demand and supply. Section 5 concludes.

2 The Effect of Social Insurance on Household Debt: Evidence from Medicaid Expansions

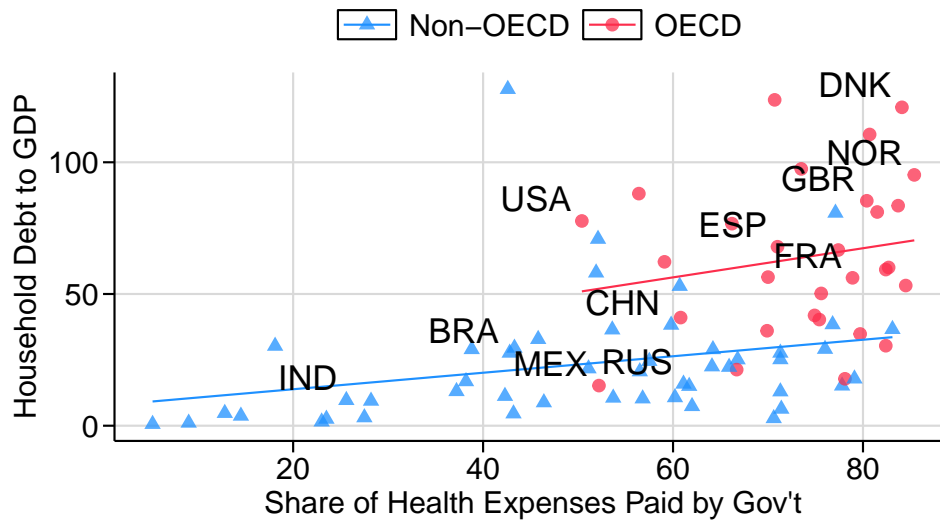
At the macro-level, cross-country comparisons indicate the generosity of social insurance is *positively* related to household debt. Figure 2 plots the ratio of household debt to GDP versus the share of total health expenses paid by the government, a proxy for social insurance provision, across countries. Countries with more generous insurance have significantly higher levels of household debt. On the top right corner are the Scandinavian countries, which have both a high provision of social insurance and high levels of household debt. Through the lens of our hypothesis, one reason Scandinavian households take on higher levels of debt is because they are more financially resilient—they are better insured against adverse events.

This section estimates the effect of expanded social insurance on household debt. We study one of the largest changes to the US social safety net in recent decades: the expansion of Medicaid under the Affordable Care Act (ACA). Our empirical strategy exploits both the staggered nature of the Medicaid expansions across states and within-state granular heterogeneity in the impact of the expansions. Examining a variety of credit outcomes, we find a positive relationship between credit card debt and Medicaid eligibility, as well as supporting evidence consistent with credit supply playing an important role in shaping the equilibrium response.

2.1 Institutional Background: Medicaid

Medicaid is a program, administered jointly by state and federal governments, that offers low-income households free or low-cost health insurance. By the end of 2019, 64.3 million individuals were receiving health insurance through Medicaid, nearly 20% of the US popu-

Figure 2: Household Debt and Social Insurance Across Countries



Notes: Data come from the IMF Global Debt and WHO Global Health Observatory data repository.

lation. In wake of Covid-19, the number of enrollees has steady climbed to 85.3 million as of December 2022.⁹ Medicaid spending is a large fraction of total US healthcare expenses, totaling \$597.4 billion in 2018 (or comprising 16% of aggregate health expenditures). To qualify for Medicaid, a household must have income below a specified threshold.¹⁰

The ACA expanded Medicaid in participating states by requiring these states to set the income eligibility threshold to at least 138% of the federal poverty level (FPL) for *all* adults. Participating states receive federal funds to support the costs of the Medicaid expansion. Prior to the ACA, only a handful states offered Medicaid to adults aged 64 or under without dependents. After expanding, the uninsured population decreased on average 30% among participating states. The share of the population enrolled in Medicaid rose 4.37 percentage points in expanding states. This average change in enrollment is within range of the 3-5 percentage point estimated *causal* effect of the expansions on enrollments.¹¹ Crowd out of private and other sources of government insurance appears limited; the fraction receiving non-Medicaid

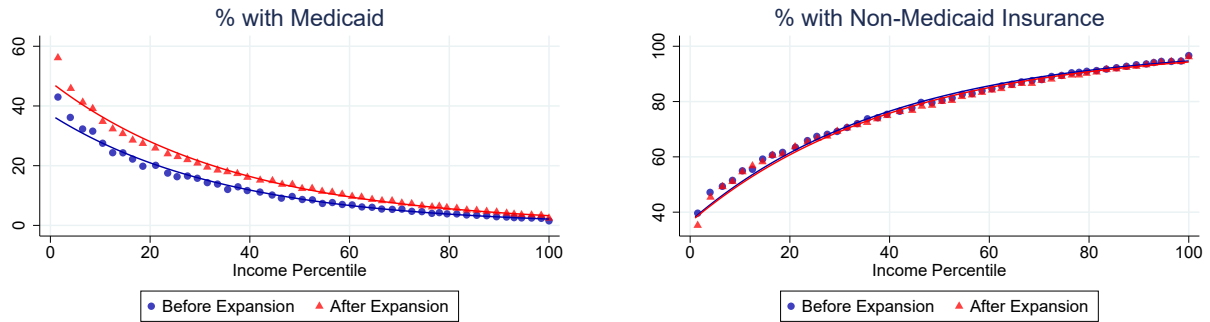
⁹We obtain enrollment count data from the Kaiser Family Foundation: <https://www.kff.org/other/state-indicator/medicaid-and-chip-monthly-enrollment>.

¹⁰Several states also use an asset-based means test in addition to the income threshold to determine eligibility. For a state to expand Medicaid under the ACA, they were required to remove any asset-based means tests.

¹¹See, for example, Sommers et al. (2014); Frean et al. (2017); Kaestner et al. (2017); Courtemanche et al. (2019).

sources of insurance fell 0.59 percentage points.¹² Figure 3 plots insurance type by income both before and after expanding.

Figure 3: Health Insurance Type by Income (Pre and Post Expansion)



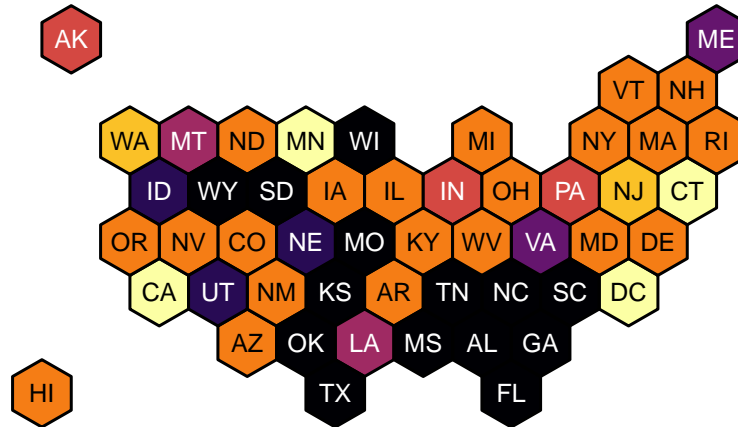
Notes: The graphs above plot the fraction of people with either Medicaid (left) or other non-Medicaid sources of insurance (right), such as private insurance or Medicare. We report averages both before (blue circles) and after (red triangles) states expanded Medicaid. We include non-expanding states in the "before" group. Insurance type is measured at the end of the year while income is measured over the entire year. Note that some "high income" households may qualify for Medicaid if they, for example, lost their job during the year and were eligible by the end of the year. This, and aggregation across household sizes, time, and states, is likely why the share with Medicaid is smooth with respect to income (as opposed to being discontinuous at eligibility thresholds). Source: American Community Survey (ACS), 2010 to 2020.

Participating in the expansion is optional, and the timing of adoption varied significantly across time (see Figure 4). 2014 was the most common year of adoption, but some states opted to expand as early as 2010. As of 2019, 34 states have expanded Medicaid eligibility under the ACA, with Idaho, Nebraska, and Utah set to expand in 2020.

The ACA was primarily financed by cuts to federal government spending on healthcare, taxes on insurers, and taxes on individuals. Congressional Budget Office (CBO) estimates projected the ACA to *reduce* budget deficits overall during 2013—2022 by \$109 billion dollars (Congressional Budget Office, 2012). The ACA's projected costs to the Federal Government of expanding Medicaid, the Children's Health Insurance Program (CHIP), Health Insurance Exchanges, and other non-coverage provisions totaled \$1,455 billion (approximately 0.6% of 2020's GDP per year). Healthcare-related government spending cuts were projected to save \$741 billion over this same time period, accounting for 50.9% of the total cost. The primary source of these cuts were reductions in payment rates for hospital services rendered to Medi-

¹²These figures are calculated using ACS data from 2010 to 2020.

Figure 4: Medicaid Expansion Dates



Notes: Dates come from the Kaufman Family Foundation.

care and Medicare Advantage patients and reductions in Disproportionate Share Hospital (DSH) payments. DSH payments help compensate hospitals for the cost of uncompensated care (i.e., missing or partial payments owed by patients).

2.2 Data

This section describes the two main datasets used in our reduced-form analysis. The first is credit bureau data. The second dataset records Medicaid eligibility at the ZIP-level. Throughout, we deflate nominal variables to real 2020 dollars.

Experian Data. To study credit outcomes, we use credit bureau data from Experian for a random sample of ten million US residents. Our Experian sample is an annual panel spanning 2010 to 2021 and contains over 90 million borrower-level observations. A subset of outcomes are measured at a quarterly frequency over 2010–2020. Our sample is geographically representative in the sense that individuals are randomly sampled from ZIP codes in proportion to the their ZIP code’s population share. In our empirical analysis, we aggregate the borrower-level data to the ZIP code-level. We drop ZIP codes with fewer than 150 borrowers reporting

data and ZIP codes that cannot be matched to our Medicaid eligibility data (these are mainly small ZIP codes). After these restriction, we have 12,931 unique ZIP codes over twelve years.

Experian Summary Statistics. Table 1 presents summary statistics for our main credit outcomes of interest in the ZIP-level sample. On average, 85% of borrowers in a ZIP have at least one credit card and 22% obtained a new credit card in the past year. Average borrower-level credit card balances are \$4,276. We measure revolving (i.e., unpaid) balances as the difference between average monthly balances and average monthly payments. These monthly averages are available at a quarterly frequency in the Experian data. In our sample, borrowers on average carry \$3,662 in revolving balances each month. 60% of the sample has unpaid balances at least once per quarter; the rate is 71% among credit cardholders.

Our measure of unpaid balances may be subject to measurement error that depends on when balances are measured relative to the consumer's scheduled payment date.¹³ Average use of revolving credit card debt in our sample is similar to estimates from regulatory data, suggesting that measurement error is close to mean zero.¹⁴ Importantly, this measurement error will *unlikely* bias the regression analysis. Revolving balances will be a response variable in our regressions, and the timing of a consumer's billing cycle is likely unrelated to their state's Medicaid expansion.

We consider several additional variables that reflect credit access. In our sample, average credit card utilization is 31% and average limits are \$17,897. Credit limits vary significantly across ZIP codes; the standard deviation is \$8,745. The average ratio of new accounts per credit card inquiry is 0.52, indicating that typically two inquiries are necessary to obtain one credit card. Credit card inquiries per year average 0.44 per borrower.

For default, we examine both measures of delinquency and whether borrowers have debt in collections. On average, 10% and 7% of borrowers have some debt that is 30 and 90 or more days past due (respectively). These measures of delinquency exclude debt in collec-

¹³Ideally, we would subtract payments from the *statement* balance at the end of the billing cycle. Average current balances will tend to understate statement balances when measured after the end of the billing cycle (and may overstate it when measured shortly before).

¹⁴Using regulatory data, [Adams, Bord and Katcher \(2022\)](#) reports that around 68% of active credit card accounts are used to revolve in a given year and that approximately 75% of credit card balances are revolving at a given point in time. Alternatively, the share of revolvers is 52% in the 2019 SHED and 45% in the 2019 SCF. However, [Zinman \(2009\)](#) argues that the SCF significantly under-reports credit card debt.

Table 1: Summary Statistics for ZIP-Level Credit Outcomes

	Mean	SD	25th %	50th %	75th %	N
1[Has CC] (%)	84.62	8.78	79.37	86.16	91.4	119,293
1[New CC] (%)	21.73	4.62	18.64	21.82	24.81	119,293
CC Bal. (All)	4276.4	1745.35	3030.78	3962.42	5193.14	377,729
CC Rev. Bal.	3662.28	1449.06	2638.59	3417.59	4428.19	377,729
1[Revolver] (%)	60.34	11.49	52.39	60.62	68.95	377,729
CC Util. (%)	31.43	12.15	21.98	30.22	39.59	119,293
CC Lim.	17896.83	8744.96	11529.78	15854.57	22510.16	119,293
New CC to Inq.	0.52	0.12	0.44	0.52	0.6	119,293
CC Inq. (#)	0.44	0.14	0.34	0.43	0.53	119,293
30+ Delinq. (%)	10.1	4.03	7.18	9.78	12.57	119,293
90+ Delinq. (%)	7.19	3.25	4.84	6.86	9.12	119,293
1[Any Col.]	23.79	11.9	14.6	22.08	31.17	119,293
Col. Bal.	667.47	436.75	353.45	570.01	877.42	119,293
Vantage Score	683.39	37.18	658.46	684.11	709.85	119,293

Notes: This table reports ZIP-level summary statistics for the main credit outcomes of interest from the Experian data. All variables are reported annually (June of each year) except for credit card balances (total and revolving) and percent of people that have revolving balances ("revolvers"). The latter are reported quarterly. We calculate revolving (i.e., unpaid) balances by subtracting credit card payments from total credit card balances. Nominal variables are CPI-adjusted to be in terms of 2020 dollars. Throughout, "CC" denotes "credit card". The "Has CC" variable indicates the fraction of borrowers with at least one credit card while the "New CC" variable captures people that obtained at least one new credit card in the past year. Balances, limits, and utilization are calculated across all credit cards. Utilization and limit calculations include households with zero credit cards. The "inquiries" variables reflect total activity in the past year. The delinquency variables reflect if borrowers had any debt 30+ or 90+ days past due (excluding collections). The collections variables indicate the fraction of households with some debt in collections (non-medical and medical debt are tabulated separately). Our credit score measure is the Vantage Score.

tions, which is even more common. On average 24% of borrowers have at least some debt in collections.

Medicaid Eligibility Data. We combine several data sources to estimate Medicaid eligibility at the ZIP-level. Adult household members are eligible for Medicaid if their income falls below a threshold, where the income threshold depends on household size and the number of dependents. Directly calculating eligibility requires individual-level data containing ZIP codes, income, and household composition. Because such data is difficult to access, we instead estimate the eligible population share. Our estimation approach first estimates the income distribution for each ZIP-year using data from the IRS Statistics of Income (SOI). Next, relying on the law of iterated expectations and Bayes' rule, we use the joint distribution of income and household size from the 2010 American Community Survey (ACS) to estimate the

share of households with income below their corresponding Medicaid threshold. For details on our procedure see Appendix B. We use IRS SOI data from 2009–2019 in our calculations. We obtain annual ZIP-level data on Medicaid enrollment from the ACS.

Eligibility Summary Statistics. Table 2 reports summary statistics for our eligibility and enrollment data.

Table 2: Summary Statistics for ZIP-Level Eligibility and Income Data

	Mean	SD	25th %	50th %	75th %	N
Elig. (%)	17.93	12.1	6.46	14.46	28.14	119,293
New Elig.	11.51	11.78	0	7.7	22.15	7879
Avg. HH Inc.	79.34	62.9	50.87	62.71	85	119,293

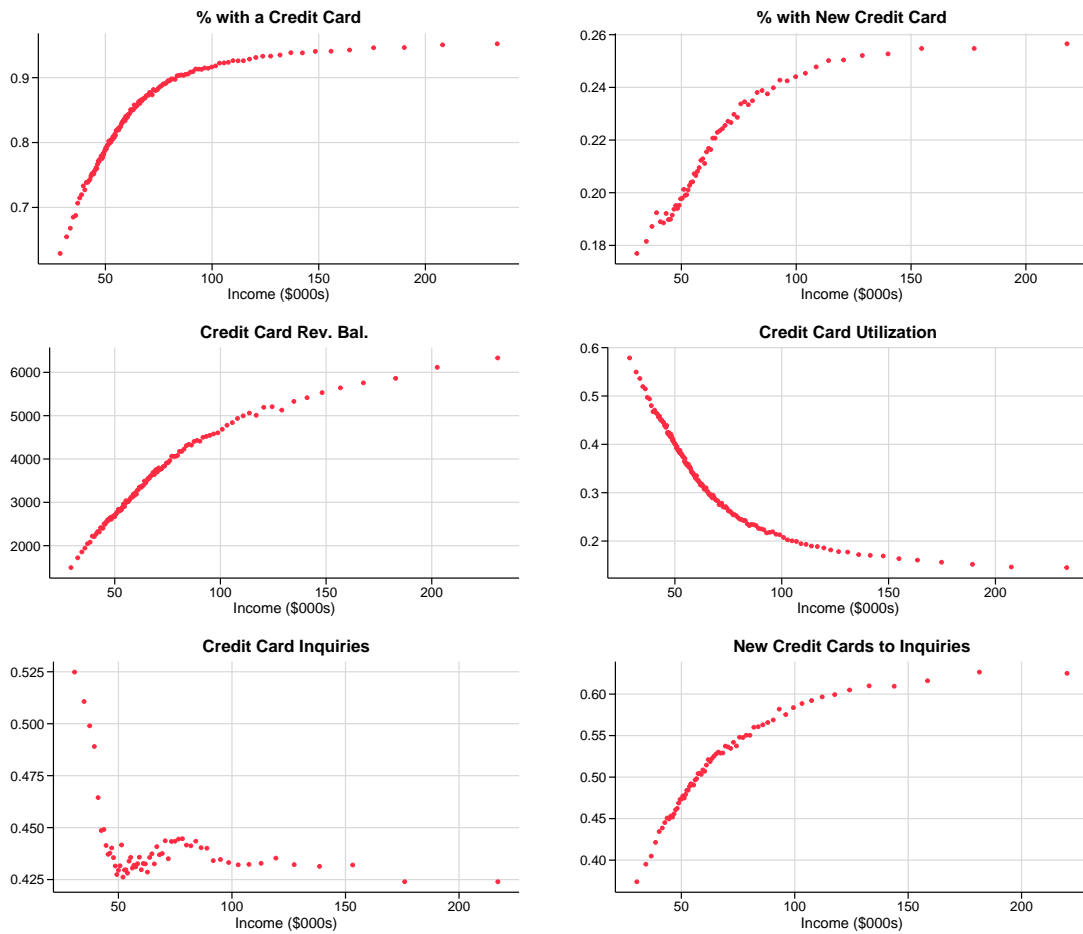
Notes: This table reports ZIP-level summary statistics for the eligibility measures and income. Eligibility is calculated as described in the text, with additional details provided in Appendix B. The first row reports average eligibility across all ZIP codes and years. The second row reports the average change in eligibility in the first year of expansion (this excludes ZIP codes in states that did not expand). Average household income is the average adjusted gross income (AGI) reported in the ZIP code. Nominal variables are CPI-adjusted to be in terms of 2020 dollars.

2.3 Empirical Strategy: Continuous Difference-in-Difference

Identifying the causal effect of Medicaid eligibility is challenging. Eligibility depends on household income, which is likely correlated with omitted variables. Figure 5 shows that income is generally positively related to credit access and use. Comparing credit outcomes of eligible and non-eligible people would likely *understate* the causal effect of Medicaid eligibility, as such comparisons would conflate the causal effect with the direct effect of income and other factors correlated with income.

To overcome these identification challenges, we exploit quasi-experimental variation in the *size* of Medicaid expansions in a difference-in-difference (DID) analysis. We draw on two sources of variation. First, we exploit cross-state variation arising from states’ *pre-existing* Medicaid eligibility rules. Expanding under the ACA required states to raise the income eligibility limit to a common threshold of 138% of the federal poverty level for all adults aged 64 or less. States with previously stricter eligibility rules, such as lower income thresholds or rules precluding adults without dependents from eligibility, tended to experience larger changes in eligibility when expanding. Second, we exploit within-state variation due to differences in

Figure 5: ZIP-Level Credit Access and Income Facts



Notes: This figures plots average income against credit outcomes, with all variables measured as the average within a ZIP code for a given year. All nominal variables deflated to 2020 dollars.

the *distribution* of income among low-income households. Specifically, ZIP codes where more households had income lie between the pre and post-expansion income eligibility thresholds experienced a larger increase in eligibility.

Because we have a continuous measure of treatment intensity, we estimate a continuous DID. The DID compares ZIP codes with larger versus smaller shares of people that became newly eligible as a result of their state expanding Medicaid under the ACA. This approach is similar in spirit to that of [Goodman-Bacon \(2018, 2021b\)](#), which uses state-level variation in exposure to Medicaid expansions to estimate the impact on mortality. An advantage of using ZIP-level variation is that we can include state-time fixed effects to account for the impact of state-level events coinciding with expansions. Motivated by similar concerns, [Dranove et al. \(2016\)](#) and [Garthwaite et al. \(2019\)](#) also use a continuous ZIP-level DID to study the impact on healthcare utilization. We further refine their approaches by constructing an eligibility measure that takes into account how Medicaid eligibility cutoffs vary with family size.

We estimate

$$Y_{zct} = \alpha_1 \text{Post}_{st} + \alpha_2 \text{NewElig}_{zs} + \beta (\text{Post}_{st} \times \text{NewElig}_{zs}) + \phi_{ct} + \gamma X_{zct} + \varepsilon_{zct}. \quad (1)$$

Above, Y_{zct} is an average credit outcome in ZIP code z (located in county c of state s) in year t .¹⁵ Post_{st} equals one if state s has expanded Medicaid as of year t . We capture the intensity of treatment with a continuous variable, NewElig_{zs} , which corresponds to the fraction of ZIP z 's population that became newly eligible for Medicaid over the first year of state s 's expansion (relative to the year before its expansion). All specifications include county-time fixed effects to account for the effects of other observed time-varying county and state-level shocks. We include time-varying controls (X_{zct}) such as logged ZIP-level income per household. The coefficient of interest is β on the interaction term, which reflects the causal effect of an expansion that induced a one percentage point increase in eligibility. Throughout, we cluster our standard errors at the state-level because adopting the expansion occurs at the state-level.

When does OLS estimation of the above equation identify the causal effect β ? The key

¹⁵Most of our outcome variables are measured annually, but a subset are observed at a quarterly frequency. The quarterly variables are credit card balances (both total and revolving) and the share of households that are revolvers.

identifying assumption is that household credit outcomes would have evolved in parallel—across locations with high versus low changes in eligibility—if Medicaid had not expanded. Phrased differently, we assume that the size of the newly eligible population is uncorrelated with other factors *changing* at the time of the expansion. This assumption would fail if other policies were implemented along with the expansions that also targeted the newly-eligible population.¹⁶ Another advantage of the granular, ZIP-level variation is that it is possible to include county-year fixed effects. These fixed effects help account for the impact of other state or county-level policy changes on credit outcomes.

2.4 Empirical Results: The Effects of Medicaid Eligibility on Household Debt

Credit Card Debt. We first examine the response of credit card debt. Table 3 reports DID estimation results for these outcomes. We first document positive effects of increased eligibility on the extensive margin of credit card use. A one percentage point increase in a ZIP code’s Medicaid-eligible population leads to a 0.33 percentage point increase in the fraction of borrowers that has a credit card. The fraction of households that received a new credit card in the last year grows by 0.21 percentage points in response to the same shock. Per newly-eligible household, these correspond to a 33 and 21 percentage point increase (respectively).

In terms of total credit card balances, average ZIP-level balances increase 1.08 percentage point in response to a one percentage point increase in eligibility. This corresponds to approximately a \$46.18 increase in average balances per capita. Examining *revolving* balances, we find that a one percentage point increase in eligibility increases credit card borrowing by 0.82 percentage points. The borrowing response constitutes a \$30.03 increase in average balances. Per newly-eligible household, this corresponds to a \$3,003 increase in credit card borrowing, which is approximately 42% of total household credit card balances.¹⁷ Additional heterogeneity analyses reveal that both the extensive and intensive margin responses

¹⁶In support of this assumption, we find evidence that credit card borrowing evolved similarly across both high and low income ZIP codes after expanding. This suggests it is unlikely that other policies altering low-income households’ credit use coincided with the Medicaid expansions. Our test augments the regression above to include an interaction between an indicator for whether the ZIP code has above median income and a post expansion indicator. For credit card debt, we estimate a precise null effect for the interaction with a high income indicator (a coefficient of 0.02) but still obtain large and statistically significant estimates for β .

¹⁷The 42% figure comes from dividing \$3,003 by \$7,210, where the \$7,210 is average household-level credit card balances as of 2019. We calculate this figure using the aggregate credit card balance data from the Federal Reserve Bank of New York’s Consumer Credit panel and household population count data from the American Community Survey.

are concentrated in low income ZIP codes.¹⁸

Table 3: DID Results for Credit Card Debt

	1[Has CC] (1)	1[New CC] (2)	log(CC Bal.) (3)	log(CC Rev. Bal.) (4)
NewElig _{zs} × Post _{st}	0.36*** (0.05)	0.23*** (0.03)	1.08*** (0.24)	0.82*** (0.19)
NewElig _{zs}	-0.50*** (0.07)	-0.26*** (0.04)	-1.38*** (0.25)	-1.15*** (0.23)
log(AGI _{zcs})	0.11*** (0.01)	0.02*** (0.003)	0.62*** (0.02)	0.553*** (0.02)
Obs	119,288	119,288	377,705	377,705
R2	0.78	0.69	0.85	0.82
Mean	85%	22%	\$4,276	\$3,662

Notes: This table reports results from estimating the DID in Equation (1). Each specification uses county-time fixed effects and controls for logged ZIP-level average adjusted gross income (AGI). Note that data are annual in columns 1-2 and quarterly in columns 3-4 (due to data availability). Standard errors are clustered by state. Nominal variables are CPI-adjusted to be in terms of 2020 dollars. The dependent variable is labeled above the column number and its mean is reported in the bottom row. Statistical significance: 10%*, 5%** , and 1%***.

While large, the estimated borrowing response is plausible. First, this large response is consistent with households going from little-to-no credit card usage to newly having access to a credit card, and beginning to catch up to more typical levels of credit card balances.

Second, the DID estimates the credit card response for up to twelve years before and after expanding. For each household made newly eligible *at the time* of expansion (our explanatory variable), more households likely end up eligible in the *following years* due to adverse events such as job loss. Putting our estimates in terms of *newly* eligible households likely overstates the impact in terms of the number of households that access Medicaid at some point *over* the next twelve years.

Third, credit demand and supply may also change for borrowers that never become eligible during the period of study. For example, households at risk of adverse events may expect that they are more likely to qualify for Medicaid if an adverse event occurs. Even if these particular households do not experience a adverse event, credit supply may respond positively to non-eligible households if underwriting models suggest that they present a lower risk of

¹⁸See Appendix Table A.4.

default after the expansion. In our structural analysis, our model allows these effects to be present.

Fourth, our large estimates are consistent with other empirical evidence on spending responses to social insurance. Notably, [Aaronson et al. \(2012\)](#) estimate strong spending responses to minimum wage hikes. They find that a \$1 rise in the minimum wage increases quarterly income and spending by \$250 and \$800 (respectively) among households receiving over 20% of their income from minimum wage labor.

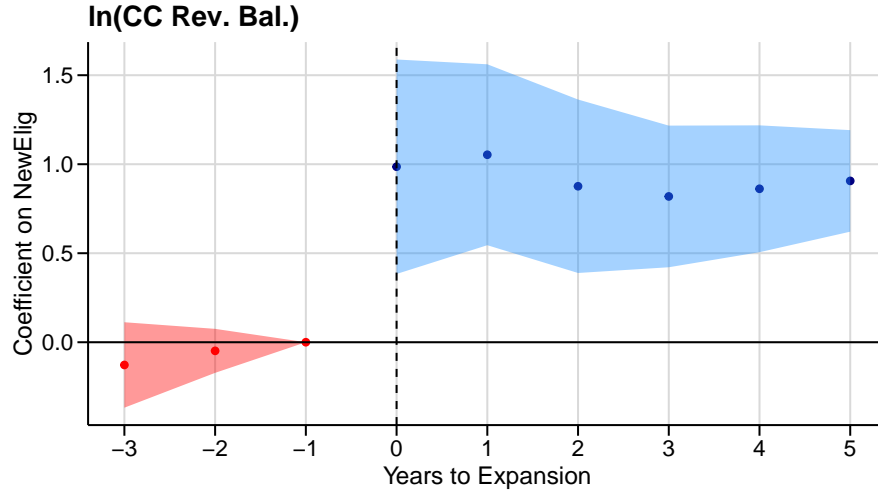
Pre-trend Testing and Dynamic Estimates. To explore the possibility of time-varying treatment effects, we obtain dynamic DID estimates for our main outcome of interest. On impact, we find a coefficient for log credit card borrowing of approximately one. The estimated effect remains near one (decreasing slightly) in the next five years after the expansion, indicating a persistent increase borrowing after expanding Medicaid. Prior to expanding, we see no evidence of pre-trends. That is, the subsequent change in Medicaid eligibility does not predict differential trends in credit card debt prior to the expansion.

Credit Supply and Demand Proxies. Next, to shed further light on the role of supply and demand, we examine proxies for these two forces in Table 4. For our supply proxies, we consider ratio of credit card balances to credit limits (utilization), credit limits themselves, and the ratio of new credit cards to credit card inquiries (over the past year).

Overall we find evidence consistent with a positive credit supply response to expansion. A one percentage point increase in a ZIP code's eligibility leads to a 0.60 percentage point decrease in utilization, indicating a relaxation of credit constraints. Given the positive effects documented for balances, this suggests that this decline in utilization is due to an increase in credit card limits. Examining limits directly, we estimate a the same 1% eligibility increase leads to a 1.38 percentage point increase in credit card limits (a \$247 increase). Lastly, we find that after expanding Medicaid, credit card inquiries are more likely to result in a new credit card. A one percentage point increase in eligibility leads to a 0.31 percentage point increase in the ratio of new credit cards to inquiries.

Our proxy for credit card demand is the number of credit card inquiries per borrower

Figure 6: Dynamic Estimates for Revolving Balances.



Notes: The figure above plots the estimated change in log revolving credit card balances per percentage point increase in the newly eligible share of the population in blue (to the right of the dashed line) at various horizons. In red (left of the dashed line), the figure plots estimates of pre-trends. The shaded area is a 95% confidence interval. Dynamic DID estimates are obtained using the imputation estimator described in the text (based on [Borusyak et al., 2022](#)), which is robust to treatment effect heterogeneity. Pre-trend estimates are obtained following the procedure in [Borusyak et al. \(2022\)](#).

Table 4: DID Results for Credit Card Supply and Demand Proxies

	Supply			Demand
	CC Util. (%) (1)	log(CC Lim.) (2)	New CC to Inq. (3)	CC Inq. (#) (4)
$\text{NewElig}_{zs} \times \text{Post}_{st}$	-0.60*** (0.07)	1.38*** (0.19)	0.31*** (0.05)	0.23*** (0.05)
NewElig_{zs}	0.76*** (0.08)	-1.73*** (0.26)	-0.42*** (0.06)	-0.18** (0.06)
$\log(\text{AGI}_{zcsst})$	-0.16*** (0.01)	0.78*** (0.03)	0.10*** (0.01)	-0.08*** (0.01)
Obs	119,288	119,288	119,288	119,288
R2	0.82	0.86	0.71	0.86
Mean	31%	\$17,896	0.52	0.44

Notes: This table reports results from estimating the DID in Equation (1). Each specification uses county-year fixed effects and controls for logged ZIP-level average adjusted gross income (AGI). Standard errors are clustered by state. Nominal variables are CPI-adjusted to be in terms of 2020 dollars. The dependent variable is labeled above the column number and its mean is reported in the bottom row. Statistical significance: 10%*, 5%** , and 1%***.

in the past year. We estimate that a one percentage point increase in a ZIP code's eligible population increases inquiries by 0.0023 per borrower (approximately 1 new inquiry per 400 individuals). This suggests demand may also be contributing to the equilibrium rise in credit card balances. However, we note that inquiries are an equilibrium outcome that may be influenced by marketing practices of credit card lenders and households' perceptions of lending standards, making them an imperfect proxy for demand. We ultimately rely on our model (in upcoming sections) to definitively separate supply and demand.

Financial Resilience. Finally, we turn to measures of default and find evidence indicating that increased eligibility improved households' financial resilience in Table 5. We find that increased eligibility reduces delinquency. A one percentage point increase in eligibility reduces the likelihood of being 30 and 90 days delinquent by 0.11 percentage points (separately). Examining more severe measures of default, we also find a reduction in debt in collections. The same 1% increase in eligibility reduces the likelihood of having debt in collections by 0.43 percentage points. The average amount of debt in collections decreases 0.89 percentage points (\$ 5.93) in response to a 1% increase in eligibility.

The improved financial resilience of borrowers appears to translate into higher credit scores. We estimate that a one percentage point increase in eligibility leads to a 1.25 point increase in borrowers' average Vantage Score. This is approximately a 0.18 percentage point increase relative to the average Vantage Score of 683.

To sum up, we find that expanding Medicaid eligibility increased both the prevalence of credit cards and total credit card balances. We find evidence suggesting both supply and demand contributed to the increase in equilibrium credit card usage. Lastly, we document that default decreased and credit scores improved, which could drive an increase in credit supply in response to expanded Medicaid eligibility.

Treatment Effect Heterogeneity Robustness. Recent econometric work highlights that when the timing of treatment is staggered and treatment effects are heterogeneous, standard DID estimators can identify a weighted average treatment effect (e.g., [Goodman-Bacon, 2021a](#)). These weights can sometimes be *negative*, biasing estimates in the opposite direction of the

Table 5: DID Results for Default and Credit Scores

	1[Delinquency]		Debt in Collections		Credit Score
	30+ Days (1)	90+ Days (2)	1[Any Col.] (3)	log(Col. Bal.) (4)	Vantage Score (5)
$\text{NewElig}_{zs} \times \text{Post}_{st}$	-0.11*** (0.03)	-0.11*** (0.03)	-0.43*** (0.06)	-0.89*** (0.25)	1.25*** (0.13)
NewElig_{zs}	0.15*** (0.04)	0.15*** (0.04)	0.58*** (0.08)	1.95*** (0.34)	1.86*** (0.20)
log(Avg. Inc.)	-0.05*** (0.003)	-0.04*** (0.003)	-0.16*** (0.01)	-0.79*** (0.05)	58.29*** (2.64)
Obs	119,288	119,288	119,288	119,288	119,288
R2	0.73	0.71	0.82	0.78	0.84
Mean	10%	7%	24%	\$667	683

Notes: This table reports results from estimating the DID in Equation (1). Note that the delinquency measure here is not restricted to delinquency on credit cards. It will also reflect delinquency on mortgages, student loans, etc. Each specification uses county-year fixed effects and controls for logged ZIP-level average adjusted gross income (AGI). Standard errors are clustered by state. Nominal variables are CPI-adjusted to be in terms of 2020 dollars. The dependent variable is labeled above the column number and its mean is reported in the bottom row. Statistical significance: 10%*, 5%** , and 1%***.

true (unweighted) average treatment effect. Settings like this paper’s, where there are many *early* treated units, are especially vulnerable in theory to having negative weights (Roth, Sant’Anna, Bilinski and Poe, 2022). This suggests that the estimates above may tend to understate the true treatment effects.

To address this concern, we verify that our results are robust to using an imputation estimator adapted from Borusyak, Jaravel and Spiess (2022) that is robust to treatment effect heterogeneity. The Borusyak et al. (2022) imputation estimator works by first splitting the data into untreated (never and not-yet-treated) and treated observations. The untreated sample is used to predict counterfactual outcomes for the treated units. The average treatment effect is calculated as the average difference between treated units’ realized and imputed counterfactual outcome.

We adapt the Borusyak et al. (2022) estimator to our setting in two ways. First, because treatment is continuous in our setting, we calculate the treatment effect per unit of treatment as the difference between the realized and potential outcome divided by the newly eligible

share of the population. Second, in order to include county-time fixed effects, we reclassify ZIP codes that had less than 0.1% of their population become eligible to having 0% become eligible.¹⁹ Appendix Tables A.1, A.2, and A.3 report results using the imputation estimator. Overall, estimates are similar and suggest that bias results in slight understatement of most effects. For example, the coefficient on revolving balances increases from 0.82 to 0.91 and remains statistically significant at the 1% level.

Comparison to Prior Evidence on Financial Outcomes. By testing whether the credit demand and supply channels can dominate in equilibrium, we build on prior work analyzing partial equilibrium effects of expanded health insurance access. The key mechanism underlying these channels is that insurance enhances financial resilience, incentivizing both borrowing and lending. Prior work documents that insurance access significantly reduces default and medical expenses, suggesting that insurance can significantly enhance financial resilience.²⁰ In support of the credit supply channel, Brevoort et al. (2017) estimates that the reduction in delinquencies induced by the Medicaid expansions led to improvements in credit card terms worth \$520 million per year.

Analyses of savings behavior suggest that Medicaid eligibility on average can reduce savings, but increase them for households experiencing financial hardship. Gallagher et al. (2019a) estimates that newly eligible households on average either reduced or left savings out of tax refunds unchanged. But this masks heterogeneity among those whose precautionary savings motive decrease and those experiencing adverse financial events (e.g., households reporting skipping meals for financial reasons), as the latter group on average increased savings. This suggests the average response to insurance access may differ from the direct effect.

Our analysis builds on this work by focusing on borrowing rather than saving. Many households simultaneously hold both high-interest credit card debt and low-interest savings

¹⁹Without doing so, it is not feasible to include county-time fixed effects in a Borusyak et al. (2022) estimator for our setting. This is because treatment timing only varies at the state-time level, even though treatment intensity varies at the ZIP-level. Without our adaptation, there would be no untreated ZIP codes post-expansion to serve as counterfactuals for the high-treatment ZIP codes. This would preclude estimating county-time (or state-time) fixed effects post expansion. Our rounding approach is conservative because it may slightly overstate the counterfactual outcome, understating the treatment effect.

²⁰For example, expanded access to Medicaid and Medicare reduce delinquency and collections (for medical and non-medical debt), unpaid bills, out-of-pocket medical expenses, and bankruptcy filings (Gross and Notowidigdo, 2011; Finkelstein et al., 2012a; Barcellos and Jacobson, 2015; Hu et al., 2018; Gallagher et al., 2019b; Goldsmith-Pinkham et al., 2020a).

([Gross and Souleles, 2002](#)), which presents a "credit card puzzle" for standard models of consumption and saving. This may be driven by a liquidity motive to avoid states of the world in which the household has no liquid savings and no access to additional credit card borrowing.²¹ It is not obvious if gross borrowing would always rise when households reduce gross savings, as households might reduce borrowing if they were doing so in order to maintain a target level of gross savings. This makes our analysis of borrowing complementary to [Gallagher et al. \(2019a\)](#), and together informative about the joint dynamics of borrowing and saving.

3 Model

In this section, we develop an incomplete-markets heterogeneous-agents model with health expenditure shocks, credit delinquency, and health insurance. Households face idiosyncratic income shocks as well as idiosyncratic health expenditure shocks. They can save and borrow using a one-period non-state-contingent asset, and can choose not to repay their debt obligations. After renegeing on their debt obligations, their debt enters a delinquent state, and the household is excluded from financial markets and suffers a utility loss. In every period, delinquent debt can stochastically get a haircut, i.e., be reduced by some percent. Households can exit the delinquency state by repaying their debt.

Credit to households is supplied by credit card companies. These companies have access to funds at the risk-free interest rate, which households take as given. The assumption that the risk-free rate is constant, rather than the aggregate net supply of debt, allows us to study how different policies affect the aggregate stock of household debt in the economy. We assume that credit card companies are risk neutral and behave competitively so that the spread they charge on a household's loan is such that their expected profits equal zero.

We use the model to study the effects of different insurance policies on the aggregate stock of debt, wealth inequality, and welfare.

²¹For liquidity-based explanations of the credit card puzzle, see [Telyukova \(2013\)](#), [Druehl and Jørgensen \(2018\)](#), and [Fulford \(2015\)](#).

3.1 Household problem

There is a continuum of measure one of households in the economy, denoted by $i \in (0,1)$. Household's i income at time t is denoted by y_{it} and evolves according to a compound Poisson process:

$$\ln y_{it} = \begin{cases} \rho \ln y_{it-1} + \epsilon_{it}^y & \text{w.p. } \lambda_y, \\ \rho \ln y_{it-1} & \text{w.p. } 1 - \lambda_y. \end{cases} \quad (2)$$

where λ_y is the probability that an income shock arrives in a period. If such shock does not arrive, the household's income does not change. Given an income shock, the household income follows an AR(1) process, where ρ is the degree of persistence and ϵ_{it}^y is an idiosyncratic income shock with mean zero and variance $\sigma_{\epsilon,y}^2$.

In addition to income risk, households are subject to stochastic medical expenditure shocks, denoted by m_{it} . The stochastic expenditure shocks follow a log-normal distribution with mean μ_e and variance σ_e^2 .

Households medical bills are partially covered by their health insurance. We allow insurance to vary exogenously with the household income. Health insurance covers a share of the medical bill due. We denote the share of medical bills the households have to pay out-of-pocket as $o(y_{it})$. Note that the out-of-pocket share depends only on the current household income, as we assume insurance varies only with household income.

Each household has access to non-state-contingent one-period debt, denoted by b_{it} . A negative value of b_{it} represents household savings. In the beginning of each period, the household can choose to repay or renege on its debt obligations. The household's decision to repay debt depends on their total debt, their level of income, and the size of their current medical bill. Households face an interest rate schedule for the amount of borrowing they choose, which we denote by $r(b', y)$.²² The price of debt is denoted by $q(b', y) = \frac{1}{1+r(b', y)}$. We denote the total amount of repayments owed by the household by $\tilde{b}_{it} = b_{it} + o(y_{it})m_{it}$. Note that \tilde{b} is the relevant state variable from the perspective of the household.

At the beginning of each period, a household learns its current income and medical expenditure shock, and then chooses whether to repay its debt or to renege and declare delin-

²²The size of medical bill does not appear in the interest rate schedule as it is independent across periods.

quency. Its present discounted value is denoted by $V(\tilde{b}, y)$, where $\{\tilde{b}, y\}$ are its individual state variables. This value function is given by the max between the present discounted value of repaying debt obligations, denoted by $V^r(\tilde{b}, y)$, and the value of renegeing and declaring delinquency, denoted by $V^d(\tilde{b}, y)$:

$$V(\tilde{b}, y) = \max \left\{ V^r(\tilde{b}, y), V^d(\tilde{b}, y) \right\}. \quad (3)$$

Conditional on the decision to repay its debt obligations, the recursive problem of the household with total debt obligations \tilde{b} and income y is given by

$$\begin{aligned} V^r(\tilde{b}, y) &= \max_{c, b'} u(c) + \beta \mathbb{E} V(b' + o(y')m', y'), \\ \text{s.t.} \quad c + \tilde{b} &\leq y + q(b', y)b', \end{aligned} \quad (4)$$

where $u(\cdot)$ is the utility of the household from consumption, which is assumed to be strictly increasing, concave, and continuously differentiable. The household's discount factor is β .

When a household renegees on its debt obligations, the debt moves into a delinquency state. A household with debt in a delinquency state cannot save or borrow, and suffers a utility cost ξ . At the end of the period, the household receives a stochastic haircut on its debt obligations. The haircut, denoted by δ , is assumed to follow a compound Poisson process. With probability, λ_δ , the household receives a positive haircut. Conditional on a haircut, the share of debt forgiven follows a Beta distribution with shape parameters α_1 and α_2 . The value of the household in the delinquent state is given by

$$V^d(\tilde{b}, y) = u(y) - \xi + \beta \mathbb{E} V((1 - \delta)\tilde{b} + o'(y')m', y'). \quad (5)$$

The timeline in every period is as follows. First, the household learns its current income and medical bill. Then, it decides whether to repay its outstanding debt obligations or not. If it decides to repay its debt obligations, it chooses the level of consumption and borrowing. If it decides to renege on its debt obligations, it consumes its income, suffers a utility loss, and at the end of the period has a chance of drawing a stochastic haircut rate.

We denote the delinquency policy function of a household with total debt obligations \tilde{b} by

$$d(\tilde{b}, y) = \mathbf{1} \left[V^r(\tilde{b}, y) < V^d(\tilde{b}, y) \right], \quad (6)$$

where $\mathbf{1}$ is the indicator function. The function $d(\tilde{b}, y)$ equals 1 when the household defaults. When indifferent, we assume the household repays its debt obligations.

3.2 Credit supply

Credit to households is supplied by risk-neutral credit card companies. Perfect competition among these companies ensures an expected zero-profits condition holds in equilibrium. We assume credit card companies have unlimited access to funds at the risk-free interest rate, which we denote by r^f .

Debt in the economy is a hybrid of short-term and long-term debt. Debt is of short-maturity in nature, as households need to repay their debt obligations in the following period. However, when debt becomes delinquent, credit card companies do not receive payments within that period. Instead, they need to wait until the household decides to repay its debt. Either because it received a haircut, or because its income level changes so that it decides to repay.

The zero-profits condition that pins down the price of debt, $q(b', y)$, is

$$\begin{aligned} q(b', y)(1 + r_f) &= [1 - \mathbb{E}(d(b' + o(y')m', y'))] \\ &+ \mathbb{E}[d(b' + o(y')m', y')(1 - \delta')q((1 - \delta')(b' + o(y')m'), y')]. \end{aligned} \quad (7)$$

That is, the credit companies equate the cost of their loan (LHS) with its return (RHS). The return from the loan is the sum of two parts. If the debt does not go delinquent, the credit company receives its full face value. If the debt goes delinquent, which happens with probability $\mathbb{E}(d(b' + o(y')m', y'))$, then the creditor is left with a claim on a delinquent debt. The worth of such claim, following a possible haircut, is given by $(1 - \delta')q((1 - \delta')(b' + o(y')m'), y')$. The fixed point function $q(\cdot)$ that solves equation (7) is the debt pricing schedule.

Proposition 1. *Given a default policy function, $d(\tilde{b}, y)$, there exists a unique pricing schedule $q(b', y)$ which satisfies equation (7).*

3.3 Stationary Equilibrium

We denote the joint distribution of households across total debt obligations and income levels at the beginning of the period, after the expenditure shock realization, by $\Lambda(\tilde{b}, y)$. Four components characterize the law of motion for this joint distribution: (i) the borrowing decision of households – $b'(\tilde{b}, y)$, (ii) the debt pricing schedule – $q(b', y)$, (iii) the household's default decision – $d(\tilde{b}, y)$, and (iv) the exogenous processes of income, expenditure shocks, and haircuts. The law of motion for the joint distribution is defined as follows. For all Borel sets $\mathcal{B} \times \mathcal{Y} \subset \mathbb{R} \times \mathbb{R}^+$,

$$\begin{aligned} \Lambda'(\mathcal{B} \times \mathcal{Y}) &= \int_{m'} \int_{y' \in \mathcal{Y}} \int_{\mathcal{B}(\tilde{b}, y, m')} d\Lambda(\tilde{b}, y) dF(y'|y) dG(m') \\ &\quad + \int_{\delta} \int_{m'} \int_{y' \in \mathcal{Y}} \int_{y, ((1-\delta)\tilde{b} + o(y')m') \in \mathcal{B}} \mathbb{1}[d(\tilde{b}, y)] d\Lambda(\tilde{b}, y) dF(y'|y) dG(m') dH(\delta), \end{aligned} \quad (8)$$

and

$$\mathcal{B}(\tilde{b}, y, m') = \{(\tilde{b}, y) \text{ s.t. } d(\tilde{b}, y) = 0 \text{ and } b'(\tilde{b}, y) + o(y')m' \in \mathcal{B}\},$$

where $F(\cdot)$, $G(\cdot)$, and $H(\cdot)$, are the CDF of the different exogenous variables. In the stationary equilibrium, the distribution $\Lambda(\tilde{b}, y)$ is constant over time. The definition of the stationary Markov-perfect equilibrium is as follows.

Definition 1 (Equilibrium). *A stationary Markov-perfect equilibrium is given by a default policy function $d(\tilde{b}, y)$, a borrowing policy function $b'(\tilde{b}, y)$, a debt pricing schedule $q(b', y)$, and a joint distribution of households across total debt and income levels, $\Lambda(\tilde{b}, y)$ such that*

1. *The default and borrowing policy functions solve the household's problem given the debt pricing schedule.*
2. *The debt pricing schedule satisfies the zero-profits condition, (7).*
3. *The joint distribution of households across debt and income levels is stationary.*

3.4 Calibration

We calibrate the risk-free interest rate in the economy to 2%. There are five sets of structural parameters in the model, in addition to the risk-free rate. First, there are three preference

parameters - the discount factor β , the CRRA γ , and the disutility of delinquency ξ . Second, there are three haircut process parameters - the arrival rate of haircuts λ_δ , and the two shape parameters of haircuts α_1 and α_2 . Third, there are three parameters governing the income process - the arrival rate of income shocks λ_y , the persistence of income ρ , and the variance of income shocks $\sigma_{\epsilon,y}^2$. Fourth, there are two parameters governing the health expenditure process - its log mean μ_e and variance σ_e^2 . Finally, we need to calibrate the out-of-pocket share function $o(y)$.

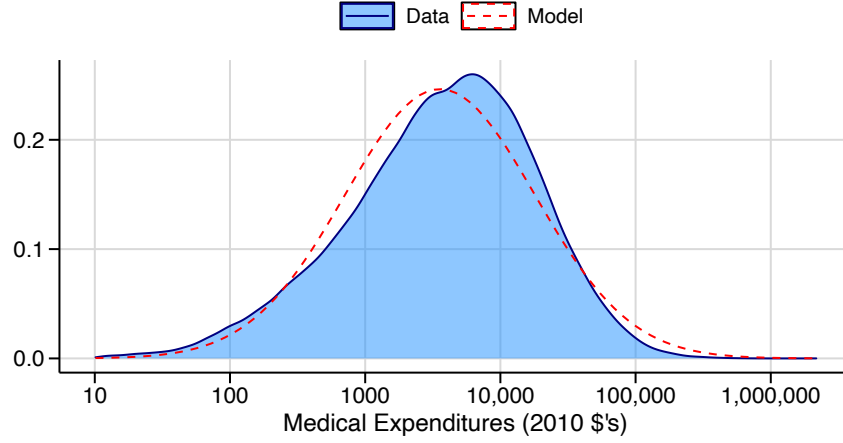
We proceed as follows. We use the Medical Expenditure Panel Survey (MEPS) to calibrate the parameters governing health expenditure shocks and the out-of-pocket function. We then calibrate the income process, preference, and haircut parameters to match several features of credit card debt in the data.

Health Expenditure and Insurance Parameters. We calibrate the mean log expenditure and its variance to match the distribution of annual medical expenditures of households in the MEPS data over 2000 to 2017. We find a mean expenditure of 8% of median income and a variance of 2.62. Figure 7 compares the calibrated distribution to its empirical counterpart. The calibrated parameters imply that a medical expenditure one s.d. above the average equals 40% of median income. Recall that the expenditure shocks, both in the data and in the model, are not the amounts that households have to pay out-of-pocket.

The MEPS dataset also contains information on households' income, insurance types, and out-of-pocket (OOP) expenditures. To construct the out-of-pocket share as a function of income we proceed in two steps. First, we split insurance types into three categories: Medicaid, "other" insurance, and uninsured. Other insurance includes households with private insurance (including employer-sponsored) and Medicare. We compute the average OOP share for each insurance type. The average OOP share in Medicaid is 6.8%, 27.5% under other insurance types, and 63% for the uninsured.

Second, using a log-linear function, we approximate the share of households with each insurance type. Figure 3 displays the empirical shares alongside our approximation. The share of households with Medicaid falls with income while the share with other sources of insurance rises with income.

Figure 7: Distribution of annual medical expenditure



Notes: Data source—Medical Expenditure Panel Survey (MEPS).

We combine the relationship between insurance type and income along with the OOP share by insurance type to impute the average OOP share by income. Specifically, the OOP share in our model is:

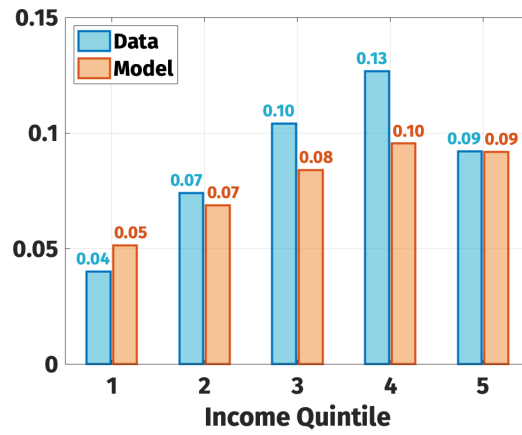
$$oop(y) = \Pr(\text{Medicaid}|y) \times 6.8\% + \Pr(\text{Other}|y) \times 27.5\% + \Pr(\text{Uninsured}|y) \times 62.7\% \quad (9)$$

A key advantage of this two-step approach, relative to directly estimating the OOP share along the income distribution, is that we can conduct counterfactuals in which we change the share of households under each insurance type.

Income, Preference, and Haircut Parameters. (*Preliminary*) There are nine remaining structural parameters: the income process parameters $(\lambda_y, \rho_y, \sigma_{\epsilon,y}^2)$, preference parameters (β, γ, ζ) and the haircut parameters $(\lambda_\delta, \alpha_1, \alpha_2)$. Our initial calibration targets the distribution of credit card debt across households of different income levels. This results in the following parameterization. An income shock arrives on average every 2.3 years $(\lambda_y = 0.42)$. The income shock persistence is 0.88 (ρ_y) and the volatility of the innovation is 7.3% $(\sigma_{\epsilon,y}^2)$. The calibrated discount rate is 0.92 (β) , which induces households to borrow. The disutility of delinquency is 0.34 (ζ) , and the coefficient of relative risk aversion is 3 (γ) . Finally, the annual probability of a haircut is 0.92 (λ_d) , and the shape parameters of the Beta distribution of haircuts are 1.7

and 9 (α_1, α_2).

Figure 8: Credit card debt along the income distribution



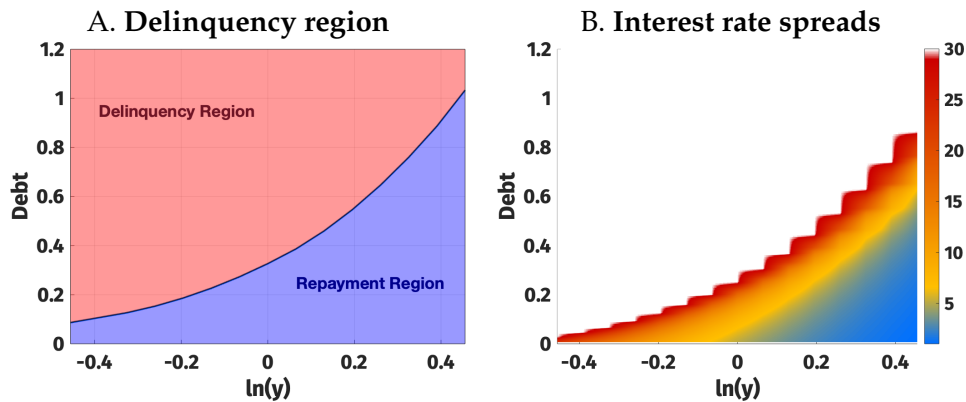
Notes: Data source - Panel Study of Income Dynamics (PSID)

3.5 Equilibrium properties

We solve the model globally, and compute the stationary distribution across income and debt levels. Panel A of Figure 9 presents the regions of the state space where households choose to repay or renege on their debt obligations. In general, lower income implies a lower debt threshold, above which households do not repay their debt. Because the utility function is concave, low-income households are more tempted not to repay their debt and increase their contemporaneous consumption. As a result, low-income households cannot maintain a high level of debt.

The probability of delinquency affects the interest rate spreads households face. Panel B

Figure 9: Equilibrium properties



Notes: This figure presents the regions where households choose to repay their debt or go delinquent.

of Figure 9 presents the interest rates spreads households face in equilibrium. The horizontal axis is the current income level of the households and the vertical axis represent the debt obligations promised to be repaid in the following period. Because high-income households are less likely to renege on their debt obligations, they face lower interest rate spreads. In a similar fashion, low-income households face very high interest rate spreads.

The interest rate schedule faced by low-income households effectively limit their access to credit. Policies that reduce the delinquency probability of these households expand their credit access by lowering these interest rate spreads. We now turn to study the effects of one such policy - the expansion of Medicaid.

Model Abstractions. Our model abstracts away from several aspects of health, which will generally make our policy counterfactual analysis conservative. First, we do not model households' "health." Therefore, our welfare calculations reflect improvements in financial health, rather than physical health. In reality, the welfare benefits from improved physical health are also likely large, as Medicaid expansions significantly reduced both child and adult mortality ([Goodman-Bacon, 2018, 2021b](#); [Miller et al., 2021](#)).

Second, our model does not feature moral hazard in terms of risky health behaviors nor use of medical services. Prior analysis of the ACA's Medicaid expansion do not find effects on risky behaviors (e.g., smoking and lack of exercise, [Simon, Soni and Cawley, 2017](#)). However, research generally estimates moderate moral hazard responses in terms of overall health expenditures ([Manning et al., 1987](#); [Finkelstein et al., 2012b](#); [Garthwaite et al., 2019](#)). Although, a recent analysis of the Medicaid expansion under the ACA finds no impact on total (pre-insurance) medical expenditures ([Shupe, 2023](#)). Because households do not increase total medical expenditures in our model, the model likely understates the borrowing response. While the additional costs of the expansion that would arise from increased expenditure is not included in our welfare calculation, these costs should be weighed against the welfare gains from improved physical health.

Third, income and medical expenditure shocks are uncorrelated in our model. In reality, the health events resulting in large medical expenditures may reduce income and employment ([Dobkin et al., 2018](#); [Stepner, 2019](#)). If expanding Medicaid improves access to care and

reduces the likelihood of major illnesses, then our model would understate the financial benefits from higher income associated with these health improvements. It likely also understates the borrowing response, as avoiding low-income states of the world improves credit access.

4 The Effect of Social Health Insurance on Household Debt and Welfare

In this section we study how different health insurance policies shape the distribution of debt across households in the economy, and study their welfare implications. We start by studying the channels through which health insurance policy can affect households' accumulation of debt. We then study the effect of Medicaid expansion in our model. The policy broadens Medicaid health insurance and increases the share of insured households by 1.56%. This is the increase we identified Medicaid expansion had in our state-level empirical analysis.

4.1 Theoretical analysis

We model health insurance as a change in the out-of-pocket share of medical expenditure households of different income levels face, $o(y)$. A more generous health insurance policy corresponds to a reduction in the out-of-pocket share different households pay for medical shocks.

Health insurance policy affects households' accumulation of debt in several ways. The direct effect of more generous health insurance is increasing households' disposable income. Households can achieve the same consumption levels while borrowing less. Therefore, the direct effect of health insurance is a reduction in debt levels. To study the indirect channels, consider the household's optimality condition with respect to debt accumulation, which is given by

$$u'(c) \frac{\partial (q(b', y)b')}{\partial b} = \beta \mathbb{E} \mathbf{1}_{V^r \geq V^d} u'(c(b' + o(y')m', y')) + \beta \mathbb{E} \mathbf{1}_{V^r < V^d} V_1^d (b' + o(y')m', y') \quad (10)$$

The household equated the benefits from borrowing (LHS) to the costs of borrowing (RHS). By increasing debt obligations, b' , the household increases its current funds by $\frac{\partial (q(b', y)b')}{\partial b}$.

Note that the household internalizes how its borrowing decision affects the interest rate it pays on debt. There are two potential costs of borrowing. If the household repays its debt in the following period ($V^r \geq V^d$), the marginal cost of debt obligations is simply the marginal utility of consumption. Alternatively, if the household goes delinquent ($V^d > V^r$), the household's cost of debt obligations are $\frac{\partial V}{\partial b}$.

The first indirect channel through which social insurance affects household debt is a reduction in the *precautionary savings motive*. A reduction in $o(y')$ reduces the volatility of out-of-pocket medical expenditure, $o(y')m'$. This results in a lower volatility of future consumption. If the utility function features prudence ($u'''(\cdot) > 0$), as it does in our calibration, then such reduction in volatility results in a smaller cost of borrowing. The reduction in the marginal cost of borrowing induces households to take on more debt. That is, through the precautionary savings channel, social insurance raises household debt levels.

The second indirect channel is the *debt aversion* channel. Borrowing is more costly in the states where households repay their debt obligations. Debt is less costly in the delinquency state as households expect to pay it only in the future, and potentially after a haircut. More generous insurance policy raises the probability of repayment, $\mathbb{E} \mathbb{1}_{V^r \geq V^d}$, as medical expenditures that would have pushed households into delinquency are now partially insured. The increase in the repayment probability increases the cost of borrowing, as households are more averse to debt when they are more likely to repay it. Therefore, through the debt aversion channel, social insurance reduces household debt levels.

Both the precautionary savings motive and the debt aversion channels do not depend on lenders changing their behavior. That is, they do not depend on the supply side of loans. We refer to the combined effect of these two channels as the *credit demand* channel.

The final indirect channel is the *credit supply* channel. The reduction in delinquency probability induces lenders to lower interest rate spreads, $q(b', y)$. This raises the benefits from debt obligations b' . For each unit of consumption promised to be repaid in the following period, households receive more units of consumption in the current period. This induces households to increase their debt obligations. So, through the credit supply channel, social insurance increases household debt levels.

Overall, the effect of social insurance on the aggregate level of household debt is ambigu-

ous. The direct channel as well as the debt aversion channel lead to a reduction in debt levels, while the precautionary savings motive and the credit supply channels lead to an increase in debt levels. We now turn to study the expansion of Medicaid in our model, and quantitatively assess the strength of the different channels.

4.2 Medicaid expansion

Our benchmark specification assumes that the share of households covered by Medicaid insurance is log-linear in income. Low-income households are more likely to be covered by Medicaid. In this section we consider a policy that mimics the expansion of Medicaid as part of the Affordable Care Act in the data. We change the intercept of the Medicaid coverage function, so that an additional 1.56% of households are covered by Medicaid. This magnitude corresponds to our empirical estimate for the effect of Medicaid expansion on the share of insured households. We solve the model and compute the stationary distribution. We assume out-of-pocket share of the Medicaid policy remains unchanged at a rate of 6.8%.

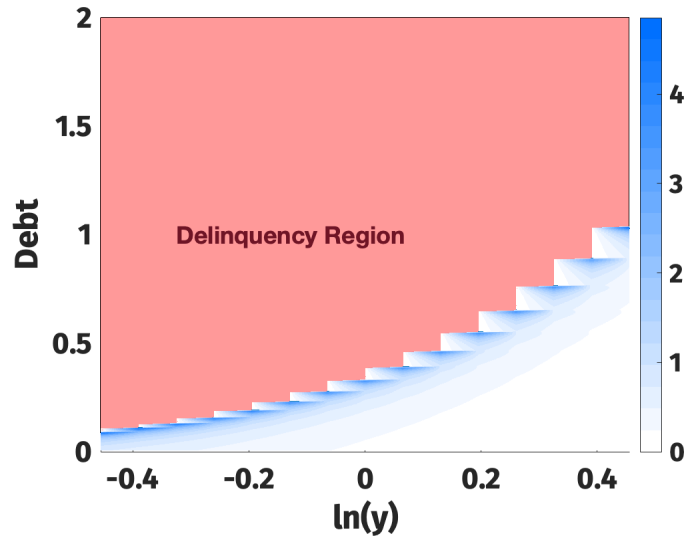
The expansion of Medicaid reduces the delinquency probability of households, as health expenditure shocks that would push households into the delinquency region are not partially covered by their health insurance. This results in lower interest rate spread in equilibrium. The reduction in equilibrium spreads as a result of the policy is plotted in Figure 10. Households who are close to the delinquency region are now facing interest rate spreads up to 5 percentage points lower relative to the interest rate spreads before the expansion of Medicaid.

The reduction in interest rate spreads affects households who tend to be close to the default frontier. So, it primarily affects the behavior of low- and medium-income households. High income households, who often do not hold any debt, are less affected by the expansion of Medicaid.

The expansion of Medicaid in our model leads to a long-run increase of 1.33% in credit card debt. Consistent with our empirical findings, the overall impact on credit card debt is positive. While the expansion of Medicaid reduces the amount of medical debt, its total impact on household debt is also positive. The policy increases household debt by 0.86%.

Our model allows us to decompose the effect Medicaid expansion has on credit card debt

Figure 10: Reduction in interest rate spreads due to policy



Notes: This figure presents the reduction in equilibrium interest rate spreads in percentage points.

Table 6: Decomposing the effect of Medicaid expansion

Not budget neutral				
	Total impact	Direct impact	Credit demand	Credit supply
Credit card debt	+1.32%	-1.14%	-1.43%	+3.9%
Total welfare	+0.18%	+0.15%	+0.0001%	+0.03%
Budget neutral				
	Total impact	Direct impact	Credit demand	Credit supply
Credit card debt	+1.63%	-1.00%	-1.25%	+3.88%
Total welfare	+0.09%	+0.06%	+0.0001%	+0.03%

to the three theoretical channels we laid out in the previous subsection: the direct channel, the credit demand channel, and the credit supply channel. The decomposition results are summarized in the top panel of Table 6.

To get the direct impact of Medicaid expansion, we keep the debt pricing schedule as well as policy functions of households as they are prior to the expansion. The only change we consider here is the change to the out-of-pocket share of medical expenditure, $o(y)$. The direct impact of the policy on debt is a decline of 1.14% in the aggregate level of credit card debt.

The credit demand channel is computed by keeping the debt pricing schedule at its levels prior to the expansion of Medicaid but allowing households to re-optimize given the orig-

inal pricing schedule. The credit demand channel reduces the aggregate level credit by an additional 1.43%. This implies that the debt aversion channel is much stronger than the precautionary savings channel.

Finally, the credit supply channel is computed by updating also the debt pricing schedule so that financial intermediaries make zero profits. The reduction in interest rates, which can be seen in Figure 10, leads to a large increase in credit card debt. The credit supply channel raises the aggregate level of credit card debt by 3.9%, dominating over the cumulative effect of the direct and the credit demand channels.

Our model also allows us to study the welfare impact of the policy. If the policy is not budget-neutral, all households will benefit from it. The model is useful in studying how important are the different channels in driving welfare, as well as comparing the welfare benefits across different households. Following [Chatterjee et al. \(2007\)](#), we calculate welfare by computing what is the percentage drop in consumption in all periods following the expansion of Medicaid which would make households indifferent between implementing and not implementing the policy. On average, the policy leads to a welfare benefit of 29 basis points in consumption equivalent terms. That is, the average household in the economy is willing to incur a 0.29% drop in consumption in all periods so that Medicaid expansion remains in place. In terms of wealth, on average, households are willing to pay a one-time payment of 94% of the median income in order to implement an expansion of Medicaid starting from the following period.

Our model allows us to decompose the effects of the Medicaid expansion to the three channels. The results are presented in the second row of Table 6. The reduction in out-of-pocket medical expenditure accounts for the majority of welfare benefits. This is expected—we assumed households do not pay any cost to implement the policy. The direct channel accounts for 83% of the welfare gains.

The credit demand channel has only negligible welfare effect. This is simply the envelope theorem. Households were optimizing their borrowing decision prior to the policy. So adjusting their borrowing decision following the policy can only lead to second-order welfare gains.

Unlike the credit demand channel, the credit supply channel leads to sizable welfare ben-

efits, 0.03% out of a total of 0.18%. That is, 17% out of the total welfare gains of the expansion of Medicaid can be attributed to the reduction in interest rate spreads households pay. This result suggests policy makers should take into account the impact of social insurance policies on the supply of credit. Disregarding the effect of social insurance on the supply of credit understates its welfare benefits.

Finally, we consider an alternative in which the policy is financed through a uniform tax levied on all households. Medicaid expansion reduces uncompensated medical care. We choose the uniform tax rate so that the tax revenues are equal to the cost of enacting the policy net of the reduction in uncompensated medical care.²³ The resulting uniform tax is 0.08%. We find that 66% of the expansion of Medicaid is financed through a reduction in uncompensated medical care. The bottom panel of Table 6 presents the effect of the budget-neutral Medicaid expansion on debt and welfare.

The aggregate impact on credit card debt is larger under the budget-neutral policy. As households disposable income is lower due to the tax, their debt level goes up. The direct welfare benefits from the program falls from 0.15% to 0.06%. Note, however, that the welfare benefits attributed to the credit supply channel remains approximately the same. The credit supply channel amplifies the direct welfare benefits of Medicaid expansion by 50%.

5 Conclusion

This paper investigates how social insurance affects household debt. We exploit the staggered expansions of Medicaid as a source of quasi-experimental variation in households' access to health insurance. Using an instrumental variables strategy, we estimate that a one percentage point increase in health insurance coverage leads to a 1.4% increase per capita credit card debt. Our estimates imply that expanding Medicaid (to non-elderly adults with no dependents) on average increased credit card debt by 2.2% (\$20.4 billion dollars in terms of aggregate debt).

Our paper builds on prior empirical work by focusing on general equilibrium channels and both macroeconomic and distributional outcomes. We develop a heterogeneous-agent model where households face permanent and transitory differences in their income, health

²³Uncompensated medical care is defined as the sum of unpaid out-of-pocket medical bills net of the market value of medical debt due to unpaid medical bills.

expenditure shocks, and incomplete markets. Households incur debt both by choosing how much to borrow on a credit card and as a result of health expenditure shocks. Using the model, we explore the impact of expanding health insurance.

While insurance can help households avoid taking on debt when experiencing adverse events like job loss and illness, it can also increase borrowing by enhancing households' financial resilience. Insurance softens the financial impact of adverse events, making it easier to avoid default and/or states of the world in which consumption is extremely low. In doing so, insurance can dampen households' precautionary savings motive and raise lenders' expected returns, increasing both credit demand and supply. Our empirical evidence suggests that these credit demand and supply channels dominate the direct impact on borrowing in equilibrium. Our model is also able to match this finding. Our findings suggest that institutions like social insurance can have an important impact on the quantity and distribution of household debt as well as welfare.

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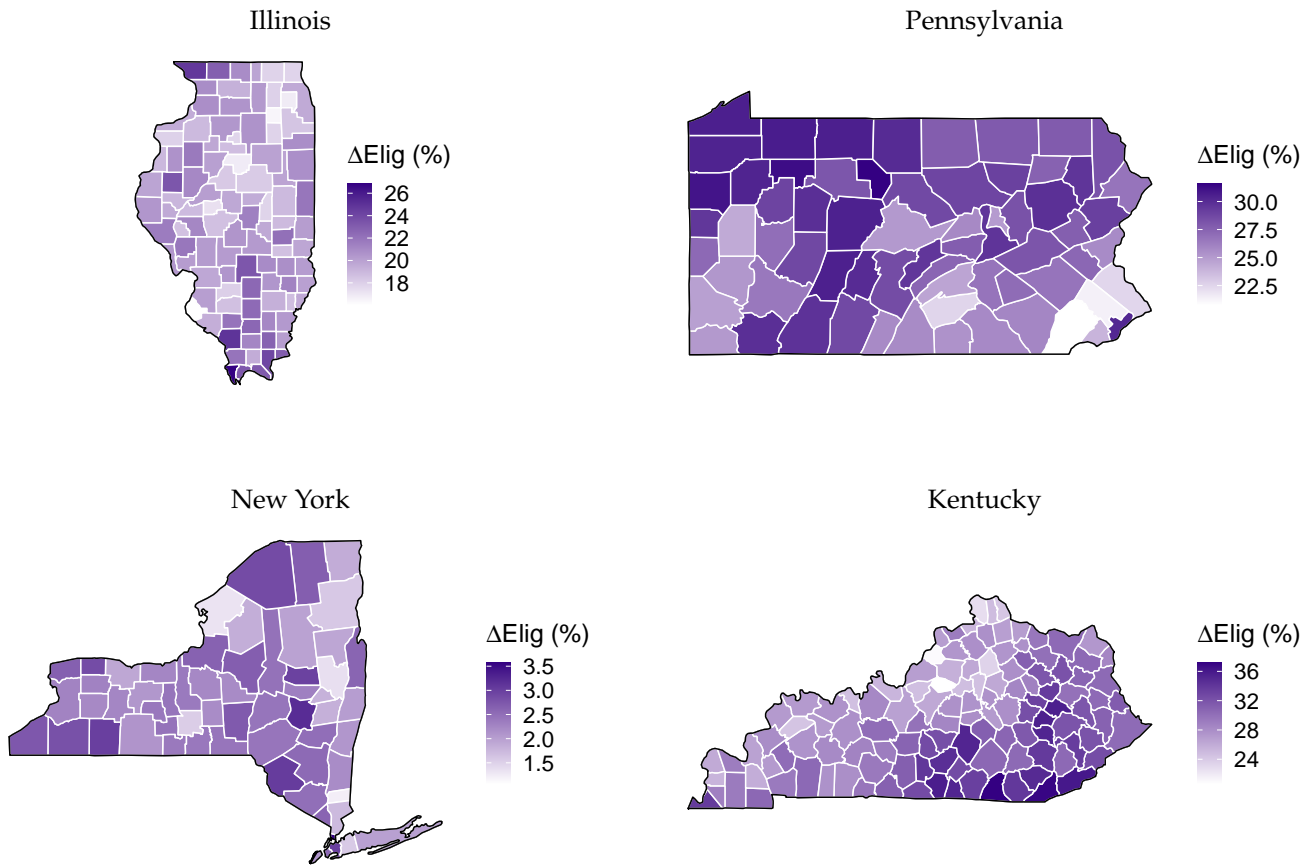
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Appendix

A Additional Figures and Tables

Figure A.1: County-Level Variation in Impact of Expansions on Eligibility



Notes: These maps display the change in the share of households eligible for Medicaid. The change is measured from the year prior to the state's expansion of Medicaid to the year of the expansion.

A.1 Treatment Effect Heterogeneity Robustness

Table A.1: Credit Card Debt (Borusyak et al. (2022) Estimates)

	1[Has CC] (1)	1[New CC] (2)	log(CC Bal.) (3)	log(CC Rev. Bal.) (4)
NewElig _{zs} × Post _{st}	0.08*** (0.03)	0.02 (0.06)	0.62*** (0.15)	0.91*** (0.19)
Obs	93,133	93,133	282,476	282,473
Mean	85%	22%	\$4,276	\$3,662

Table A.2: Credit Card Supply and Demand Proxies (Borusyak et al. (2022) Estimates)

	Supply			Demand
	CC Util. (%) (1)	log(CC Lim.) (2)	New CC to Inq. (3)	CC Inq. (#) (4)
NewElig _{zs} × Post _{st}	-0.10 (0.09)	0.43*** (0.12)	0.11 (0.11)	0.15 (0.14)
Obs	93,133	93,133	93,133	93,133
Mean	31%	\$17,896	0.52	0.44

Table A.3: Default and Credit Scores (Borusyak et al. (2022) Estimates)

	1[Delinquency]		Debt in Collections		Credit Score
	30+ Days (1)	90+ Days (2)	1[Any Col.] (3)	log(Col. Bal.) (4)	Vantage Score (5)
NewElig _{zs} × Post _{st}	-0.06*** (0.01)	-0.05*** (0.01)	0.04*** (0.02)	-0.63*** (0.23)	0.45*** (0.11)
Obs	93,133	93,133	93,133	93,133	93,133
Mean	10%	7%	24%	\$667	683

Notes: The above tables report results from the imputation estimator based on Borusyak et al. (2022). We exclude ZIP codes with a newly eligible share below 10% when taking the average of the treatment effects in order to limit noise from small denominators (recall that the imputation estimator divides the difference in realized and imputed counterfactual outcomes by the share of newly eligible). Each specification uses county-year and ZIP fixed effects and controls for logged ZIP-level average adjusted gross income (coefficient omitted above for brevity). Standard errors are clustered by state and estimated by bootstrapping the unstudentized t-statistic via a (state) clustered pairs bootstrap (using 1,000 draws). Nominal variables are CPI-adjusted to be in terms of 2020 dollars. The dependent variable is labeled above the column number and its mean is reported in the bottom row. Statistical significance: 10%*, 5%***, and 1%***.

A.2 Credit Card Response by Income

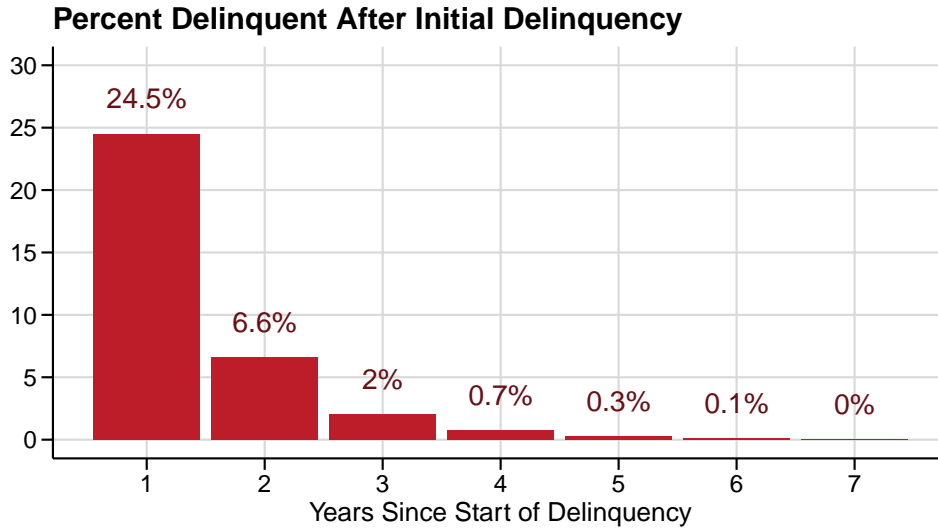
Table A.4: Credit Card Debt (High versus Low Income)

	1[Has CC] (1)	1[New CC] (2)	log(CC Bal.) (3)	log(CC Rev. Bal.) (4)
$\text{NewElig}_{zs} \times \text{Post}_{st}$	0.648*** (0.08)	0.248*** (0.03)	1.567*** (0.26)	1.644*** (0.26)
$\text{NewElig}_{zs} \times \text{Post}_{st} \times 1[\text{high inc.}]_{zt}$	-0.581*** (0.10)	-0.198*** (0.07)	-1.128*** (0.32)	-1.538*** (0.31)
NewElig_{zs}	-0.740*** (0.09)	-0.271*** (0.03)	-1.757*** (0.30)	-1.870*** (0.30)
$\log(\text{AGI}_{zcs})$	0.076*** (0.01)	0.009** (0.00)	0.521*** (0.02)	0.455*** (0.02)
$\text{NewElig}_{zs} \times 1[\text{high inc.}]_{zt}$	0.378** (0.15)	0.09 (0.08)	0.728 (0.48)	1.192** (0.51)
Obs	119,288	119,288	377,705	377,701
R2	0.829	0.738	0.88	0.849
Low Inc. Mean	0.79%	0.20%	\$3158.57	\$2789.26
High Inc. Mean	0.91%	0.24%	\$5464.95	\$4590.54

Notes: This table reports results from estimating an augmented version of the DID in Equation (1). We modify the specification to interact the coefficient of interest with an indicator for whether or not the ZIP code has above median income ("1[high inc.]"). Each specification uses county-time fixed effects and controls for logged ZIP-level average adjusted gross income (AGI). Our modified version also interacts the county-time fixed effects with the high income indicator, allowing these fixed effects to flexibly differ across the subsample. Note that data are annual in columns 1-2 and quarterly in columns 3-4 (due to data availability). Standard errors are clustered by state. Nominal variables are CPI-adjusted to be in terms of 2020 dollars. The dependent variable is labeled above the column number and its mean is reported in the bottom row. Statistical significance: 10%*, 5%***, and 1%***.

A.3 Delinquency Duration

Figure A.2: Duration of Delinquency



Notes: This figure plots the fraction of people that are delinquent one to seven years later (after an initial delinquency). Here, delinquency means that a person has debt 90 or more days past due. We use a subsample of people that are continuously observed for at least three years (totalling 2,306,609 unique individuals).

Table A.5: Persistence of Delinquency

Years	Percent of People Still Delinquent
0	100%
1	24.5%
2	6.59%
3	2.02%
4	0.73%
5	0.25%
6	0.08%
7	0.010%
8	0.005%
9	0.002%
10	0.0005%
Average Duration:	1.34 years

Notes: This table reports the fraction of people that are delinquent zero to ten years later (after an initial delinquency). Here, delinquency means that a person has debt 90 or more days past due. We use a subsample of people that are continuously observed for at least three years (totalling 2,306,609 unique individuals).

B Eligibility Estimation

This appendix describes how we generate a measure of Medicaid eligibility at the zip \times year level.

Data: We use the following data sets from 2009 to 2016:

1. IRS SOI: ZIP-level individual income tax statistics
2. KFF: Medicaid eligibility limits
3. ACS: joint distribution of income and household-size
4. KFF: Medicaid expansion date data

B.1 Estimand

We want estimates of the fraction of adults eligible for Medicaid in year t and zip z . Let E denote the event a household is eligible for Medicaid. Denote the probability that a household is eligible in (t, z) by $P_{tz}(E)$. $P_{tz}(E) = P(E|t, z)$. The probability that a household of size n is eligible depends on the probability that their income is below the relevant cutoff c_{tz}^n . We can write out the probability above into the sum of probabilities for these different cases using the law of iterated expectations:

$$P_{tz}(E) = P_{tz}(y \leq c) = \sum_{n=1}^N P_{tz}(y \leq c_{tz}^n | c = c_{tz}^n) P(c = c_{tz}^n)$$

where y is income and c_{tz}^n is the eligibility cutoff for a household of size n in $\{z, t\}$. We can rewrite each term using Bayes' rule to get:

$$P_{tz}(y \leq c_{tz}^n | c = c_{tz}^n) P(c = c_{tz}^n) = P_{tz}(c = c_{tz}^n | y \leq c_{tz}^n) P(y \leq c_{tz}^n).$$

The first term is the probability that a household is of a given size:

$$P_{tz}(c = c_{tz}^n | y \leq c_{tz}^n) = P_{tz}(\tilde{N} = n | y \leq c_{tz}^n) = g_{tz}(n | y \leq c_{tz}^n)$$

where g_{tz} is the probability mass function of a household size conditional on income being below the relevant threshold. The second term $P(y \leq c_{tz}^n)$ is the probability that the household's income is below the threshold. Let $F_{tz}(\cdot)$ denote the cumulative distribution function of income in $\{t, z\}$. Then

$$P(y \leq c_{tz}^n) = F_{tz}(c_{tz}^n).$$

Household Size Notation: Households of size two may be comprised of either a head and a dependent or a head and a non-dependent (e.g., a parent and child versus a childless couple). The eligibility thresholds can differ across these situations. To capture both, we use 2 to indicate a household with a head and a dependent and 2* to indicate a two-person household with no dependents. Let n denote household size, and we write $n \in n\{1, 2, 2^*, 3, 4, \dots, N\}$, where N is the largest observed household size.

Note on Cutoffs: Income cutoffs vary over time due to changes in the threshold and federal poverty line. They also differ across states, specifically Alaska and Hawaii generally have higher thresholds in each year.

B.2 Estimation

We estimate $P_{tz}(E)$ using the following steps:

1. We procure the number of tax returns and annual gross income by income bins for each zip code and year using IRS SOI.
2. Then, we interpolate the CDF $\hat{F}_{tz}(\cdot)$ of income between the known bins. With that, we estimate $\hat{F}_{tz}(c_{tz}^n)$ where c_{tz}^n is obtained from medicaid eligibility limits. Note that this equals the second term of the probability, $P(y \leq c_{tz}^n)$. We generate this for $n \in N$ household sizes where N is defined above.
3. We keep one observation per zip and year.
4. We use the ACS data to calculate $P(\tilde{N} = n | y \leq c_{tz}^n)$ for each n . We do this using income and household size information available by zip code and year. Note that this equals the first term of the probability, $g_{tz}(n | y \leq c_{tz}^n)$.

5. We can calculate the probability that a household of size n is eligible for medicaid by multiplying the first and second terms for each n .
6. Then, $P_{tz}(E)$ is equal to the sum of the probabilities calculated in the step above.

C Results from Alternative Empirical Analyses

C.1 State-Level Analysis of Credit Card Borrowing

C.1.1 Empirical Strategy: Instrumental Variables

The goal of our analysis is to estimate the causal effect of health insurance coverage on credit card borrowing. Specifically, we estimate the following model:

$$\ln(\text{CC}_{s,t}) = \text{Insured}_{s,t}\beta + X_{s,t}\gamma + \theta_s + \tau_t + \varepsilon_{s,t} \quad (11)$$

where $\text{CC}_{s,t}$ is credit card debt per capita, $\text{Insured}_{s,t}$ is the share of the population with health insurance, and $X_{s,t}$ is a vector of controls. The indexes s and t denote state and time, respectively. Our coefficient of interest is β . Directly estimating (11) directly with OLS would likely yield biased estimates understating the true value of β . We anticipate a negative bias because credit card borrowing is countercyclical – people use it to smooth out shocks in downturns – while insurance coverage is procyclical – likely due to the widespread reliance on employer-provided insurance.²⁴

Identification. To identify the causal effect of insurance coverage, we use an instrumental variables strategy. For each observation we construct an indicator for whether or not state s has expanded Medicaid as of time t (denoted $1[\text{Expanded}]_{s,t}$). We instrument for the insured share of the population with this indicator in a two-stage least squares (TSLS) estimation. Formally, our first stage is

$$\text{Insured}_{s,t} = 1[\text{Expanded}]_{s,t}\pi + X_{s,t}\tilde{\gamma} + \tilde{\theta}_s + \tilde{\tau}_t + \eta_{s,t}. \quad (12)$$

²⁴In our sample, health insurance coverage is positively correlated with GDP (both measured at the state-level), with a correlation coefficient of 0.10. Credit card debt is negatively correlated with GDP, with a correlation coefficient of -0.05. The correlations strengthen and retain the same sign after partialing out state and time fixed effects.

This approach exploits the staggered timing of the Medicaid expansions to obtain plausibly exogenous variation in insurance coverage. The staggered timing means that we can use time fixed effects to absorb macroeconomic trends, such as the recession and recovery, that also affect borrowing and insurance coverage. Additionally, by using state fixed effects we can net out the effect of any persistent cross-state differences related to borrowing and insurance coverage. The key identifying assumption, the exclusion restriction, is that expanding Medicaid only affects credit card debt through health insurance coverage. In practice, this assumption requires that the timing of expansions is unrelated to other events affecting credit card debt.

Data. We build an annual state-level panel dataset, where credit card debt per capita and insurance coverage rates are our primary variables of interest. The panel includes all 50 states and DC. The sample spans 2003 to 2017, making for 765 observations in total. The American Community Survey (ACS) is the underlying data source for our state-level measures of the insured population share. To measure credit card debt per capita, we use the Federal Reserve Bank of New York's state-level aggregates of their Consumer Credit Panel (CCP). The CCP's credit aggregates are calculated from a 5% random sample of individual-level credit bureau data. We obtain state-level control variables from the ACS and National Income and Product Accounts (NIPA). Our control variables are the unemployment rate, log population, log house prices, annual house price growth, and annual GDP growth (measured at the state-level).

C.1.2 Results: The Effect of Health Insurance Coverage on Debt

We find that increased health insurance coverage leads to more credit card debt per capita. Table C.1 presents estimation results for the second and first stage, as well as OLS estimates. Our preferred specification (column 4) implies that a one percentage point increase in the insured share of the population leads to 1.41 percentage point increase in credit card debt per capita.

Our preferred specification is column 4, which includes a variety of controls as well as state and time fixed effects. Including state fixed effects helps absorb persistent differences across states that are related to both insurance coverage and credit card borrowing. The time fixed effects account for time-varying factors like the recession and recovery that also im-

pacted credit card debt and insurance coverage.

An especially useful control is state-level GDP growth because it helps address a measurement limitation in the CCP data. The CCP's measure of credit card debt reflects credit card balances, which includes both revolving and non-revolving balances. Revolving balances reflect actual borrowing – i.e. unpaid balances on which the borrower pays interest. Total balances might conflate spending and borrowing. Controlling for state-level GDP helps account for state-level changes in aggregate spending, which means our estimates more likely reflect a response driven by borrowing rather than spending.

Table C.1: Effect of Insurance Coverage on Credit Card Borrowing

	TSLS				OLS			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	2nd Stage: outcome = $\ln(\text{CC})_{s,t}$				outcome = $\ln(\text{CC})_{s,t}$			
Insured _{s,t}	-2.60*** (0.29)	-1.79*** (0.31)	2.69*** (0.78)	1.41*** (0.35)	-2.00*** (0.15)	-1.05*** (0.16)	-0.76*** (0.14)	0.06 (0.09)
	1st Stage: outcome = insured% _{s,t}							
1[Expanded] _{s,t}	5.05*** (0.31)	3.37*** (0.20)	2.61*** (0.38)	1.56*** (0.19)				
Controls	✓	✓	✓	✓	✓	✓	✓	✓
Year FE			✓	✓			✓	✓
State FE		✓		✓		✓		✓
Stage 1 F	262.05	287.67	46.97	65.8				
Observations	765	765	765	765	765	765	765	765

Notes: Time-varying, state-level controls include the unemployment rate, log(population), log(house prices), annual house price growth, and GDP growth. All nominal variables and their growth rates are computed in real 2010 dollars. Coefficients are scaled so that 2nd stage estimate describes approximately the percent change in credit card debt associated with a 1% change in the insurance rate. The 1st stage coefficient reflects the percentage point increase in insurance coverage associated with adopting the Medicaid expansion. Statistical significance: 0.10+, 0.05*, 0.01*, 0.001***.

In the first stage of the preferred specification, we estimate that expanding Medicaid increased the insured share of the population by 1.56 percentage points on average. Combined, the first and second stage estimates imply that expanding Medicaid increased credit card borrowing by 2.2 percentage points. The first stage F-statistic is 65.8 in the preferred specification, which is well above the threshold of 10, indicating that bias due to weak instruments is unlikely. Additionally, the analogous OLS estimate is 0.06, which is much smaller than our TSLS

estimate of 1.41. This suggests that if there is bias due to weak instruments, if any our estimate would understate the effect of insurance on borrowing. The 1.56% increase in coverage is smaller than the 6.4% average change post-expansion visible in Figure ???. The estimated growth in coverage induced by expanding medicaid diminishes as we add state and time fixed effects (columns 1-4 in Table C.1). This is because states that opted to expand Medicaid on average had a smaller uninsured population. Additionally, insurance was generally growing over this time period in response to other incentives such as the ACA tax penalty for lacking health insurance.

The point estimate implies expanding Medicaid had an economically significant effect on credit card debt. By means of a back-of-the-envelope calculation (assuming a uniform treatment effect across debt levels), the estimated 2.2 percentage point increase corresponds to a \$20.4 billion increase in credit card debt.²⁵ The estimate implies that the overall 6.4% rise in insurance coverage following the expansions increased credit card debt 9.02%, corresponding to a \$83.65 billion increase in credit card debt.

Our finding of a positive effect on credit card borrowing suggests that the credit demand and supply channels dominate the "direct" effect of increased health insurance. Insurance incentivizes borrowing through a credit demand channel by reducing households' precautionary savings motives. Additionally, insurance can increase credit supply by reducing households' default risk, which in turn increases creditors' expected returns and incentivizes lending. Together, these two forces result in a positive relationship between borrowing and insurance coverage. In contrast, the "direct" effect on households using insurance likely implies a negative relationship between borrowing and insurance coverage. When insurance reduces the share of medical expenses borne by households, these directly affected households may now incur less medical and credit card debt than they otherwise would have.

C.2 County-Level Analysis of Medicaid Eligibility and Household Debt

C.2.1 Empirical Strategy: Treatment-Intensity Difference-in-Difference

There are several important limitations to empirical strategies using only the state-level variation in the timing of Medicaid expansions, motivating a new empirical strategy that we

²⁵This is calculated relative to the aggregate amount of credit card debt in 2019 of \$927 billion.

introduce here. Inference can be more challenging as the aggregate nature of a state-level shocks leads to less variation across households. Additionally, pre-existing differences in states' economies and Medicaid programs resulted in Medicaid expansions under the ACA having significantly different impacts on Medicaid eligibility. Rich within-state heterogeneity in treatment underlies the binary state-level adoption indicator. This granular variation in treatment intensity can improve statistical precision and support heterogeneity analyses estimating treatment effects in sub-populations. In terms of identification, bias can arise in state-level analyses when the timing of states opting into Medicaid is non-random or correlated with other economic events. Many of the later Medicaid expansions occurred after the election of a Democratic governor or by ballot measure, which can coincide with other major state-level changes in policies.

Approach. These limitations motivate our novel use of a treatment-intensity difference-in-difference approach to estimate the causal effect of Medicaid Eligibility on household debt. This approach exploits rich heterogeneity in the impact of Medicaid expansions on eligibility. The granular nature of this data also makes it possible to include state-year fixed effects, which help net out the effect of other state-level trends that affect household borrowing.

This difference-in-difference strategy exploits heterogeneity across counties and expansions in the impact of adopting the Medicaid expansion. Geographic variation in the change in the eligible share of the population was driven by differences in states' pre-ACA Medicaid income limits and differences in the distribution of income within locations. Expanding under the ACA required states to raise the income eligibility limit to 138% of the federal poverty level for all adults aged 64 or less. This primarily impacted adults without dependents, who generally faced stricter income limits prior to the expansion. All else equal, counties in states with a lower pre-ACA limit (for adults with no dependents) experienced a larger rise in the eligible population. The impact of expansions also differed within and across states due to variation in distribution of income among low-income households. Counties with more households whose income fell between the pre and post ACA eligibility limits, all else equal, experienced a larger rise in eligibility.

Our approach compares borrowing in counties with larger versus smaller changes in eli-

gibility before and after expanding Medicaid. We estimate

$$DTI_{c,t} = 1[\text{Adopted}_{s,t}]\alpha_1 + \Delta\text{Elig}_c\alpha_2 + \Delta\text{Elig}_c \times 1[\text{Adopted}_{s,t}]\beta + \kappa_c + \tau_t + \xi_{s,t} + \varepsilon_{s,t} \quad (13)$$

where $DTI_{c,t}$ is the ratio of household debt-to-income (DTI) in county c in year t . The variable $1[\text{Adopted}_{s,t}]$ indicates whether state s has adopted the Medicaid expansion as of year t . Our measure of treatment intensity is ΔElig_c , which denotes the change in the percentage of the population eligible for Medicaid, induced by the Medicaid expansion, in county c . We estimate this specification using OLS.

The coefficient of interest is β , which captures the effect on borrowing of a $\Delta\text{Elig}_c\%$ increase in the Medicaid-eligible population. This specification exploits variation in both the timing and impact of the Medicaid expansions. The county fixed effect nets out persistent differences in borrowing across counties that experienced high versus low changes in eligibility. The time fixed effect accounts for impact of macroeconomic trends on borrowing. A key strength of this specification is that we can also include state-year fixed effects, which absorbs the effect of other time-varying state-level factors influencing household borrowing.

Identification. Our approach to identification makes use of both variation in the timing and intensity of the change in Medicaid eligibility induced by the adoption of expansions. Variation in the eligible population share comes from two sources. First, state-level differences in pre-expansion Medicaid income eligibility thresholds resulted in greater *rises* in eligibility where pre-expansion threshold were *lower*. Adopting the expansion brought states up to a common threshold of 138% of the federal poverty level. Second, variation in the distribution of income among low-income households also shapes the treatment intensity of Medicaid expansions. Counties with a larger mass of low-income households concentrated just under the 138% threshold, all else equal, experienced larger increases in eligibility.

Additionally, by estimating the *change* in the relationship between eligibility and borrowing before versus after a state's expansion, we exploit variation in the timing of treatment. The difference-in-difference aspect of the regression means that we can account for persistent differences in places that would tend to have a larger versus smaller treatment effects induced

by expanding Medicaid. Because we have within-state variation in treatment intensity, we can compare households in the same state policy environment but with different exposure to one specific policy change: Medicaid eligibility.

The key identifying assumption for this empirical strategy is that household borrowing would have evolved in parallel – across locations with high versus low changes in eligibility – if Medicaid had not expanded. Intuitively the identifying assumption boils down to treatment intensity being uncorrelated with other factors *changing* at the time of the expansion. The identifying assumption would be violated if another event coinciding with expansions systematically affected counties with high versus low changes in eligibility differently.

Data. Our measure of household DTI is calculated as the county-level sum of credit card, residential mortgage, and auto debt divided by the sum of income. We obtain this data from the Board of Governors of the Federal Reserve System.²⁶ To calculate our treatment intensity measure we use data on income and Medicaid eligibility rules. We obtain Medicaid income eligibility limits from the Kaiser Family Foundation (KFF).²⁷ To calculate eligibility, we use data on the distribution of income within ZIP codes from the US Internal Revenue Service’s Statistics on Income (IRS SOI).²⁸

Summary Statistics. Table C.2 presents county-level summary statistics. In the average county, 18.4% of households are eligible for Medicaid while 22.9% of the population is enrolled. The enrollment figure is higher because the denominator is population, and households enrolled in Medicaid are more likely to have children (making their households larger than the average). The average increase in the eligible share of the population, from the year before to the year of the expansion, is 10.4 percentage points.

The impact on eligibility varied significantly; its standard deviation was 11.5 percentage

²⁶Source: https://www.federalreserve.gov/releases/z1/dataviz/household_debt/county/map. The county-level debt measures are calculated from individual-level data available in the Federal Reserve Bank of New York’s Consumer Credit Panel.

²⁷Data on income limits is available for parents from 2002-2020 at: <https://www.kff.org/medicaid/state-indicator/medicaid-income-eligibility-limits-for-parents>. Data on income limits for adults that are non-elderly, non-disabled, and with no dependents is available for 2011-2020 at: <https://www.kff.org/medicaid/state-indicator/medicaid-income-eligibility-limits-for-other-non-disabled-adults>.

²⁸The ZIP-level income data are available at: <https://www.irs.gov/statistics/soi-tax-stats-individual-income-tax-statistics-zip-code-data-soi>.

points. Compared to the average increase of 10.4%, the median rise was 2.6%. Appendix Figure A.1 displays county-level maps of changes in the eligible population for several states. Household debt averages 174.8% of income, and also varies significantly across counties (with a standard deviation of 87.3%).

Table C.2: County-Level Summary Statistics

<i>Variable</i>	Mean	SD	25 th	50 th	75 th	<i>N</i>
Pop. Enrolled in Medicaid (%)	18.4	6.6	13.9	17.9	21.9	9,741
Pop. Eligible for Medicaid (%)	22.9	11.0	10.2	26.9	31.7	12,991
ΔElig_c (%)	10.4	11.5	0.0	2.6	21.5	12,886
DTI (%)	174.8	87.3	116.6	162.0	211.8	12,978
Average AGI (\$000s)	65.7	24.2	49.9	60.1	72.4	12,991

Notes: The first two variables are the share of the county population enrolled in and eligible for Medicaid (respectively). The next variable, ΔElig_c is the change in the share of county population eligible for Medicaid from the year prior to the year after expansion. DTI is the ratio of household debt (mortgage, auto, and credit card) to income. Average AGI is the county-level average of households' adjusted gross income (AGI). Nominal variables are CPI-adjusted to be in terms of 2010 dollars. All statistics are calculated using population weights, where population is measured using the number of tax returns filed in the county.

C.2.2 Results: The Effect of Medicaid Eligibility on Household Debt

We find that expanding Medicaid eligibility increases household debt. Table C.3 reports results from estimating the treatment-intensity DID specified in equation (13). Our preferred specification (column 3) includes state-year and county fixed effects. The point estimate of column 3 implies that a one percentage point increase in the share of the population eligible for Medicaid leads to a 0.85 percentage point rise in households' DTI. Dividing this point estimate by the average DTI of 174.83% implies that a one percentage point rise in the eligible population share leads to a 0.49% increase in household debt. The positive effect of Medicaid eligibility on household debt is consistent with either increased credit supply or a reduced precautionary savings motive (and increased credit demand) resulting from households' improved financial resilience.

Take-Up. To what extent does a rise in Medicaid eligibility lead to a rise in enrollment? Take-up can be less than 100% simply due to low-income households already having insurance, for example, by receiving it through an employer and or as a young adult through a parent's

Table C.3: Effect of Medicaid Eligibility on Household DTI

	(1)	(2)	(3)
$\text{Adopted}_{s,t} \times \Delta\text{Elig}_c$	0.27 (0.18)	0.53 (0.54)	0.85* (0.43)
$\text{Adopted}_{s,t}$	5.40+ (3.03)		
ΔElig_c	-11.80*** (2.35)	-11.92*** (2.42)	
$\ln(\text{income}_{c,t})$	-12.84* (5.13)	-12.72* (5.20)	-11.69* (5.69)
State \times Year FE		✓	✓
County FE			✓
Observations	12,873	12,873	12,873
R ²	0.18	0.19	0.97

Notes: The outcome variable is county-level household debt-to-income (DTI) in percentage points. The change in eligibility (ΔElig_c) is scaled in percentage points, so its point estimate correspond to the percentage point change in DTI for a given percentage point change in eligibility. Our county-level measure of income is the (demeaned) log of the average adjusted gross income (AGI) reported in the IRS SOI data. We weight observations by the number of households (measured as the number of tax returns filed in the county each year). All specifications include state and year fixed effects. Standard errors are clustered by county. Statistical significance: 0.10+, 0.05*, 0.01*, 0.001***.

insurance. Additionally, stigma, inattention, misperceived ineligibility, and complexity in the sign-up process can also deter participation.²⁹

We employ our treatment-intensity DID strategy to estimate the average take-up rate of the Medicaid expansions under the ACA. We obtain county-level data on Medicaid enrollment from the American Community Survey (ACS). An estimate of a positive effect, especially one similar to prior estimates of take-up, helps validate our empirical strategy. Table C.4 reports our estimates. Column 3's estimate implies a take-up rate of 19%: for every 100 newly-eligible people, 19 enrolled in Medicaid. This estimate is similar in magnitude to estimates from expansions to pregnant women in the 1980s (Currie and Gruber, 1996) and low-income parents (Busch and Duchovny, 2005) of 34% and 15%, respectively. Multiplying the average change in eligibility (10.4%) by the point estimate implies that the average expansion increased the share of the population enrolled in Medicaid by 1.97 percentage points (an

²⁹See for example Aizer (2003); Currie (2004); Baicker, Congdon and Mullainathan (2012); Desmond, Laux, Levin, Huang and Williams (2016); Wright, Garcia-Alexander, Weller and Baicker (2017).

8.61% rise in enrollees).

Table C.4: Effect of Medicaid Eligibility on Medicaid Enrollment (Take-Up)

	(1)	(2)	(3)
$\text{Adopted}_{s,t} \times \Delta\text{Elig}_c$	0.02** (0.01)	0.20*** (0.05)	0.19*** (0.02)
$\text{Adopted}_{s,t}$	-0.76*** (0.22)		
ΔElig_c	1.05*** (0.17)	0.93*** (0.18)	
$\ln(\text{income}_{c,t})$	-2.60*** (0.53)	-2.62*** (0.54)	-1.24*** (0.29)
State \times Year FE		✓	✓
County FE			✓
Observations	9,667	9,667	9,667
R ²	0.65	0.66	0.99

Notes: The outcome variable is county-level Medicaid enrollment, measured as the fraction of population enrolled in Medicaid (in percentage points). The change in eligibility (ΔElig_c) is scaled in percentage points, so its point estimate correspond to the percentage point change in the Medicaid enrollment for a given percentage point change in eligibility. Our county-level measure of income is the (demeaned) log of the average adjusted gross income (AGI) reported in the IRS SOI data. We weight observations by the number of households (measured as the number of tax returns filed in the county each year). All specifications include state and year fixed effects. Standard errors are clustered by county. Statistical significance: 0.10*, 0.05*, 0.01*, 0.001***.