Insurance Subsidies, the Affordable Care Act, and Financial Stability

Samuel Dodini
Cornell University
snd46@cornell.edu

September 21, 2020

Abstract

This paper measures the effects of subsidies in the Affordable Care Act on adverse financial outcomes and consumer welfare using administrative tax data and financial outcomes from credit data. Using multiple identification strategies, I find that ACA premium tax credits reduced the rate of foreclosures by 13%, consumer bankruptcies by 6%, and rates of other severely delinquent debts, particularly for credit-constrained consumers. The subsidies reduced the right tail of the debt distribution, including debts in third-party collections. The value of the risk protections against foreclosure, bankruptcy, and medical debt from these subsidies amounts to approximately 20-32% of their cash costs.

JEL Codes: I13, D14 , H24, H51, H53

*I am grateful to Mike Lovenheim, Evan Riehl, Maria Fitzpatrick, and Corbin Miller for helpful comments and guidance. I am also grateful to Claudia Sahm, Jeff Larrimore, Ellen Merry, Barbara Robles, Mike Zabek, Erin Troland, David Buchholz and Anna Tranfaglia for their comments and help with data access. Thank you to Sean Nicholson, Kurt Wrobel, Colleen Carey, Carly Urban, and seminar participants at Cornell University for helpful discussion.
The analysis and conclusions set forth are those of the author and do not represent the views of the Federal Reserve Board, Equifax, or the Federal Reserve System.

1 Introduction

One of the core purposes of insurance is to protect against the financial risk of high-cost and relatively low-probability events. In the case of health insurance, because sickness and injury are often unpredictable and can result in large, concentrated expenditures, insured individuals and families are protected against severe financial shocks. While there is a large literature across the fields of economics, sociology, public health, and epidemiology that highlights the positive health effects of health insurance, there is comparatively less research on the effects of such insurance on financial outcomes.

The Patient Protection and Affordable Care Act (ACA) was passed in 2010 and was designed to increase health insurance coverage by incentivizing insurance enrollment through legislated guarantees against rejection for pre-existing conditions, an individual mandate to have insurance, and a series of Medicaid expansions and refundable tax credits for low- and moderate-income households. Individual tax credits are defined in the ACA as the difference between the legislated maximum for a household's spending on basic health insurance premiums as a percentage of their income (2-9%, depending on their income) and the cost of health insurance available to the household in their local area. These tax credit subsidies were designed to bring down the cost of private insurance to the consumer while the federal government made up the difference to the insurer.

These premium tax credits represent a substantial expenditure for the federal government and a significant transfer to households. Despite the centrality of these premium tax credits to the ACA's stated goals of universal and affordable insurance coverage, no work has been done that directly measures the effect of these subsidies on household financial well-being. Work to date on the effects of insurance has focused on public health insurance programs, but the overall financial effects of purchasing private health insurance, with its various monthly premiums, copays, coinsurance, and deductible requirements, may differ substantially.

In this paper, I ask the following questions: 1) What is the effect of public money directed toward lowering the cost of private insurance on the incidence of catastrophic financial outcomes such as bankruptcy, foreclosure, or collections debt? 2) Where in the distribution of debt and credit health are these effects most concentrated? 3) What are the implied consumer welfare gains of these effects? 4) What are the implied externalities of these effects on outside parties such as other homeowners, local governments, and mortgage lenders?

To address these questions, I use rich information from three administrative datasets aggregated to ZIP codes. As the treatment variable, I use data from the IRS on actual tax credits received by residents. As outcomes, I focus on measures of financial health from administrative credit bureau data. In my analysis, I use the Federal Reserve Bank of New York/Equifax Consumer Credit Panel (CCP), which is a 5 percent random sample of all Equifax credit files in the United States and contains data on credit card use, mortgage products, auto loans, foreclosures, bankruptcy filings, payment histories on important accounts, and Equifax 3.0 Risk Scores (credit scores). Together, these datasets provide a broad accounting of the consumer finance effects of the premium tax credits with high-quality data. The granularity of the data also allows me to measure how these causal effects differ over various credit scores (Equifax Risk Score) as well as over segments of the debt distribution to understand intensive margin effects.[1] Finally, I include data on the health insurance plans offered by insurers after the implementation of the ACA in 2014.

I use two main empirical strategies to measure the effects of the tax credits over the entire distribution of eligibility from 100 percent of the Federal Poverty Line (FPL)—or 138 percent in Medicaid expansion states—to 400 percent FPL. First, I use a dose-response difference-in-differences approach. Identification in these estimates comes from differences in the intensity of treatment (tax credits received) each year across ZIP codes and rests on the assumption that low-receipt areas act as a valid counterfactual for high-receipt areas because their trends are parallel prior to the 2014 rollout of the tax credits and high-receipt areas ex-

[1] Here and in all other instances in the paper, "credit score" refers to the Equifax Risk Score.

perienced no differential shocks at treatment. To address any differential pre-trends which may threaten this assumption, I incorporate a two-step estimation approach which mechanically accounts for differential trends in the pre-2014 period (Goodman-Bacon, 2016; Kuka et al., 2020; Lee and Solon, 2011). My difference-in-differences results are robust to a variety of specifications, including unit-specific time trends, state-by-year interacted fixed effects, and using individual-level data in an individual fixed effects version of the model.

Second, to address the possibility of localized shocks that are correlated with greater subsidy eligibility and worse financial outcomes, I also use a simulated instrument (Currie and Gruber, 1996) which leverages local variation in the determinants of subsidy eligibility and is exogenous to local shocks. Subsidies for any individual or family vary based on their income, current insurance coverage, Medicaid expansion in their state, and the premium for a benchmark insurance plan specific to each household's age and family structure.[2] Using health insurance plan data, I calculate the subsidies that the national distribution of households would be eligible to receive based on locally determined eligibility rules for every ZIP code in the United States. Because one might worry that insurers set their premiums based on endogenous cost factors, I condition my simulated instrument on choices made by insurers, namely their baseline premiums and their choice of which local markets to enter.[3] The remaining variation in the simulated instrument comes from cross-state differences in the rules that govern how insurers may set premiums based on age and family structure.

Next, I use my estimates in the expected utility framework in Finkelstein and McKnight (2008) to calculate the implied consumer welfare gains (i.e. the utility value of risk protections) by comparing the change in financial risks faced by consumers in a pre-ACA state of the world versus a post-ACA state of the world. This allows me to compare program costs with program benefits to beneficiaries in relation to a set of well-defined–albeit

[2] The ACA created homogenized insurance plan tiers: Platinum, Gold, Silver, Bronze. Platinum plans have the highest monthly premiums and lowest cost-sharing, while Bronze plans have relatively low premiums and high cost-sharing requirements. Tax credits are pegged to the local "second lowest-cost Silver plan." I discuss these in detail in Section 2 and the implications for my simulated instrument in Section 4.

[3] I discuss these choices in Section 2.

narrow–risks. Lastly, I use my calculations of the subsidy effects to estimate the implied spillovers to outside parties: in particular, mortgage lenders, local homeowners, local governments, and creditors.

My results suggest that, at an average outlay of approximately $59 per capita per year in a ZIP code, premium tax credits reduced the foreclosure rate per 1,000 credit files by 13%, the consumer bankruptcy filing rate (Chapter 7 and Chapter 13) by 6% and the rate of having severely delinquent debt on a mortgage, credit card, or auto loan by 13%, 5%, and 3%, respectively. The amount of delinquent debt conditional on having any such debt decreased substantially at the top of the distribution, indicating concentrated protections against right-tail catastrophic losses. My results also indicate that, conditional on having debts in third-party collections, premium tax credits reduced amounts at the 95th-99th percentile by $1,100-$1,400. Meaningful negative effects on these debt amounts are detectable moving down the distribution until approximately the 60th percentile, below which effects are a precisely estimated zero. Contrary to the results of the ACA Medicaid expansion found in other work, the number of credit files with nonzero amounts in third-party collections *increased* modestly by 0.5% as a result of these premium tax credits. One plausible explanation is that this is due to an increase in take-up of healthcare among the insured population combined with nonzero cost-sharing provisions in private insurance plans. These results together suggest that the small increase at the extensive margin is predominantly among those with low collections balances.

As a measure of overall financial health, I find that premium tax credits led to an overall upward shift in the distribution of credit scores (Equifax Risk Score) below the 60th percentile, with the largest effects concentrated around the 30-40th percentile where the typical credit score is between 620 and 660. At the treatment average, the distribution at the 30th percentile shifted upward approximately 1.5 points. Underlying this upward shift, my results on nearly every outcome show that the largest absolute effects are driven by those with credit scores below 660 who are more likely to be credit constrained.

ACA subsidies notably shifted downward the risks of third-party collections debt, foreclosure, and bankruptcy, which are costly to consumers.[4] In an expected utility framework, risk-averse consumers newly protected from such risk experience a gain in utility through a change in their risk premium. Using the shift in risk from my causal estimates in the framework in Finkelstein and McKnight (2008), I calculate average welfare gains from risk protection of $720-$1,012 per person per year for the lowest-income eligible population. Compared to average costs of $3,168-$3,528 per recipient, protection against this narrow set of financial risks can account for nearly a third of program costs.

The external benefits of the ACA subsidies to outside parties are large. Using empirical findings from the literature on the costs of foreclosure and bankruptcy, I find total implied indirect subsidies to mortgage lenders, local governments, creditors, and other homeowners of $17.8 billion per year compared to a direct expenditure of $15.5 billion on average per year from 2014-2016.[5] Thus, externalities alone almost entirely pay for these subsidies.

In answering these questions, I contribute to the literature on the effects of public spending for insurance on consumer welfare and financial outcomes as well as the literature on the effects of health insurance. I am the first to directly estimate the effect of premium tax credits on a wide set of financial outcomes. More broadly, I am the first to estimate the national financial effects of directly subsidizing the purchase of private health insurance rather than the effects of public insurance. I also am the first to calculate the implied consumer welfare gains for recipients per public dollar spent as well as the implied per dollar externalities of these subsidies as they accrue to outside parties. These contributions are important in light of political debates about the future of the Affordable Care Act, how governments facilitate the expansion of health insurance coverage, and who benefits from the features of the

[4]The internal costs of a foreclosure to a consumer sum to approximately $11,000 (2013 dollars) (Moreno, 1995), while bankruptcy costs a consumer approximately $3,300 after filing fees (Dobbie et al., 2017; White, 2007).

[5]Estimated costs are approximately $28,000 per foreclosure on nearby home values (Campbell et al., 2011; Harding et al., 2009; Immergluck and Smith, 2006; Gallagher et al., 2019); $95,000 per foreclosure for lenders (Hsu et al., 2018); and $19,000-$34,000 per foreclosure for local government revenues. Creditors stand to lose approximately $42,000 per bankruptcy (Li, 2007; Eraslan et al., 2017; Norberg and Velkey, 2005; Jiménez, 2009). I infer bankruptcy costs based on various facts presented in the literature.

ACA. This paper provides a basis for understanding the distribution of benefits of paying to expand private health insurance coverage in relation to the cash cost of the transfers.

Prior research using a variety of methods indicates that there are substantial effects of insurance coverage on utilization of medical care (Newhouse, 1993; Aron-Dine et al., 2013; Anderson et al., 2012; Card et al., 2008); self-reported health and depression (Finkelstein et al., 2012); reductions in mortality (Card et al., 2009); and consumption via reductions in out-of-pocket spending (Finkelstein and McKnight, 2008; Engelhardt and Gruber, 2011; Finkelstein et al., 2012; Baicker et al., 2013; Barcellos and Jacobson, 2015).[6] The literature also documents large effects on outside parties, such as medical care providers, who often treat the uninsured without compensation (Mahoney, 2015; Finkelstein et al., 2015).

Work measuring the financial effects of public programs for insurance has almost exclusively focused on the Medicaid program (Hu et al., 2016; Brevoort et al., 2020; Finkelstein et al., 2012; Argys et al., 2017; Gross and Notowidigdo, 2011). This literature documents broad consensus that cost volatility, debts in collections, bankruptcies, and other types of delinquencies fall while creditworthiness increases as a result of Medicaid eligibility. The effects include a reduction in third-party collections debt of $1,140 for every new Medicaid enrollee (Hu et al., 2016); annual aggregate increases in creditworthiness valued at $670 million as well as a drop of 50,000 bankruptcies annually (Brevoort et al., 2017); and a decline in bankruptcy among the eligible population by as much as 8% with a 10% increase in Medicaid eligibility (Gross and Notowidigdo, 2011). On the other hand, *losing* Medicaid has substantial negative consequences for the financial health of recipients, and those effects are larger than the gains from being newly insured (Argys et al., 2017).

The preceding papers focused on quasi-experimental methods. By contrast, the Oregon Health Insurance Experiment, a randomized experiment that allocated finite slots for Medicaid benefits, found large reductions in cost volatility and catastrophic financial outcomes among those who were treated, along with improvements in mental health and take-up of

[6] Finkelstein et al. (2018) review these and other papers related to the effects of health insurance.

preventative health care (Finkelstein et al., 2012). Most notably for my analysis, the calculated welfare gain from the risk protections of Medicaid in Finkelstein et al. (2015) using a common measure of consumption from the Consumer Expenditure Survey (CEX) as a benchmark was $126 per recipient per year.[7] The Oregon experiment outlines the real risk protection effects of the Medicaid program on recipients and suggests a key role for public policy to help ensure household financial stability by providing insurance broadly.

Unlike the Medicaid program, which is publicly administered health insurance, premium tax credits are a means of lowering the costs of *private* health insurance and thus may differ from Medicaid in important ways, especially considering the differences in the target populations and the existence of premiums, coinsurance, and deductibles. Recent research suggests premium tax credits in the ACA significantly increased health insurance take-up among the eligible population (Courtemanche et al., 2017; Hinde, 2017) and that these credits explain approximately a quarter of coverage gains from 2012-2015 (Frean et al., 2017). Gallagher et al. (2019) show that, in comparison to those just below the eligibility threshold in states that did not expand Medicaid, those who qualified for subsidies were 25 percent less likely to have difficulty making home payments and had significantly less out-of-pocket medical spending.

To date, no research has examined the effects of the ACA's subsidies on financial well-being directly. My analysis, therefore, extends the literature in an important way by directly measuring their effects across the entire distribution of eligibility, accounting for a broad set of outcomes from administrative data, and generating estimates based on dollars spent rather than the marginal effects of crossing the eligibility threshold. Importantly, this analysis presents evidence that publicly funding private insurance is a viable option for promoting financial stability among lower-income households and that the benefits spill over to outside parties as well.

[7] Notably, this is only the risk protection component of the benefit and represents one of several aspects of consumer welfare tested in that paper. This is virtually identical to the welfare gains of $120 I find for the ACA tax credits when considering only medical debt payments using a different analytical approach. See Table 6.

2 Policy Context

The Affordable Care Act was passed in 2010 and included reforms to the way health insurance markets operated, including bans on not offering coverage to those with pre-existing conditions, mandated coverage for certain products/services, and the elimination of price discrimination based on health history or sex. The key features of the law, however, did not take effect until January 1, 2014. The law attempted to expand health insurance coverage through two main channels: expanding Medicaid eligibility and premium tax credits to help consumers purchase private insurance. Beginning in 2014, health insurance marketplace websites, or "exchanges," were designed to provide a one-stop-shop for people who lack affordable health insurance through an employer or third party to compare available plans and to find information on their eligibility for Medicaid, premium tax credits, and cost-sharing reduction (CSR) subsidies. The ACA also created homogenized "metal" tiers of plans which differ in covered procedures, cost-sharing, and monthly premiums. These are the Platinum (the most generous and expensive), Gold, Silver, and Bronze plans (the least generous and expensive). Silver plans were a middle tier that balanced coverage and cost. Silver plan enrollees also received the greatest CSR subsidies, and the vast majority of enrollees choose this tier (DeLeire et al., 2017).[8] In states that expanded Medicaid, those under 138% FPL became eligible for Medicaid, and those above 138% until 400% FPL were eligible for subsidies. In Medicaid non-expansion states, the lower limit was 100% FPL.[9]

For the subsidy-eligible population, the ACA set limits on household spending on health insurance premiums as a percentage of their income, and those limits increase with income before phasing out at 400% FPL. Panel A of Figure 1 shows these expenditure limits for 2016

[8] Those making under 250% FPL were eligible to receive CSR subsidies, which were payments that lowered deductibles, coinsurance, copays, and out-of-pocket spending limits on Silver plans only. Unfortunately, these payments are not reported in the IRS data.

[9] Depending on the state, those under 100% FPL may have been subject to the "coverage gap," which left many low-income adults uninsured as they were not eligible for Medicaid under their state laws and not eligible for subsidies under the federal law.

in the states that did not expand Medicaid. Under the ACA, as long as they meet income requirements, anyone who does not have insurance available through an employer or third party or whose expenditures on premiums for their current plan are above 9.5 percent of their income is eligible for subsidies.[10]

In order to calculate tax credits, these expenditure limits were benchmarked against the annual premium for what was termed the "second lowest-cost Silver plan." This is defined as the premium, specific to each household's age and family structure, for the Silver plan ranked second in cost within each household's "Rating Area," which is a county or 3-digit ZIP code. Premium tax credits are the difference between this annual premium and their expenditure limit. So, for a household with structure h with age(s) a in Rating Area r and income i: $PTC_{hari} = Silver2_{har} - Limit_{ih}$, where $Silver2$ is the cost for the benchmark plan for the household and $Limit$ is their maximum spending on basic premiums in the ACA given income level i.

Panel B in Figure 1 shows a concrete example of subsidy eligibility for a hypothetical family of four living in a Medicaid non-expansion state in 2016 facing two hypothetical premiums for the benchmark plan ($Silver2$). As income grows, the size of the subsidy falls because the household's contribution to their own premiums grows. Households in areas with higher costs for the benchmark plan receive more in subsidies to make up the difference.[11] Given each of these inputs, areas may receive more in premium tax credits (PTC) per capita if they have a greater share of lower-income residents, if they have more residents who lack health insurance, or if the benchmark Silver plan premiums are more expensive.

Silver plan premiums at the local level ($Silver2_{har}$ above), which form the basis for calculating ACA subsidies, are a function of two main choices by the state as well as three

[10]Nearly half of Americans in 2014 were eligible for premium tax credits based on income. 400% FPL was $46,680 for an individual or $62,920 for a two-person household. The average household income for the middle quintile of the distribution was $69,000 before taxes and transfers for the average 2.6 person household. See https://www.cbo.gov/publication/53597 (Accessed June 1, 2020).

[11]Subsidies can be paid directly to insurers ("Advanced") or paid during tax season. Overpayment of subsidies relative to realized annual income must be repaid during tax filing with some limits.

main choices by insurers. First, each state decided whether or not to expand Medicaid. Second, each state decided if they wanted to create their own "age curve" and "family tier ratios," apart from federal guidelines. This allowed states to differentially set limits on premiums charged to families (as opposed to individuals) as well as limits on how different premiums could be for younger enrollees in relation to older enrollees. For example, in Minnesota, insurers may differentially charge premiums to 64 year-olds and 21 year-olds at a 3:1 ratio, while in Massachusetts, that ratio is 2:1. As another example, in Vermont, a family of any size with at least one child can be charged at most 2.85 times the base individual rate.

Each insurer had three main questions to answer each year. First, given what they expect the enrolling population to look like and any state-specific regulations, what base premium would they charge at the state level for a basic individual Silver plan? Second, in which Rating Areas should they offer their Silver plans? Third, how should they adjust their statewide base premium across different parts of the state? The results of the first choice reflect the insurers' best guess of what their costs would be to cover the enrolling population as well as state-specific regulations that drive costs up or down. The results of the second choice created different levels of competition across areas within states. The third choice, called a "geographic factor," was constrained by ACA rules to only reflect the differences in costs for delivering medical services and not the morbidity risk of the local population. These costs are most often a function of existing contracts between medical providers and certain insurers, differences in the way medical practitioners order and bill services, as well as competition among providers.[12]

The interaction of these choices is straightforward. Medicaid expansion choices by states influence insurer choices about statewide base premiums for individual Silver plans (e.g. an individual age 30). Entry choices and competition in different Rating Areas and "geographic factors" influence the premiums insurers charge for that basic age 30 individual Silver plan

[12] Based on personal conversations with an active actuary tasked with calculating exchange premiums.

in each county. Finally, how that basic individual Silver plan in each Rating Area translates into a specific premium for each household given their age and family structure depends on the "age curve" and "family tier ratio" policies of the state. In terms of the $Silver2_{har}$, the choices insurers make set the terms for the r portion of the benchmark Silver plan, while *states* determine the remaining h and a components of that plan cost. An individual could, therefore, face two different benchmark Silver plan premiums across state lines even if the base premium for a 30-year old individual is the same because two states differ in their age curves. A family of four could face two different benchmark premiums across state lines because one state limits the premiums for an entire family regardless of size and the other allows insurers to charge per-child premiums. Differences in these premiums then drive differences in the tax credits a household receives.

My simulated instrument, which I describe in detail later and which acts as a check on my difference-in-differences estimate, controls for these three insurer choices and relies on cross-state variation in these two state regulations to estimate the effects of the subsidies.

3 Data

I bring together three rich administrative data sources aggregated to small geographic levels to test the effects of the ACA's tax credit provisions on financial well-being. The administrative datasets are: 1) IRS Statistics of Income (SOI) data for premium tax credits; 2) the Federal Reserve Bank of New York/Equifax Consumer Credit Panel (CCP) for financial outcomes; and 3) ACA exchange data on actual health plans offered on the state or federal exchanges. I also include data from the American Community Survey (ACS) and the Decennial Census along with data on medical providers from the Area Health Resource File (AHRF) produced by the Department of Health and Human Services' Health Resources and Services Administration.

3.1 Tax Credits from IRS Records

In order to accurately measure premium tax credit subsidies, I use IRS tax records to track actual credits received. The IRS began including premium tax credits in their published Statistics of Income products in 2014 when the tax credits took effect. The IRS produces SOI data that are aggregated to the ZIP code level. My main treatment variable is the premium tax credit amount (net of repayments or additional subsidies received during tax season) received per person under 65 in the ZIP code, including ZIP codes where total subsidies were zero. I focus on this population because those over 65 were eligible for Medicare, and tax credits could be used to subsidize the health insurance of children as well as adults.

My sample reports average aggregate annual expenditures of $15.5 billion, but there is substantial variation from year to year. Importantly, as enrollment expanded after the initial rollout, there is a large increase in total tax credits paid from 2014 to 2016: from approximately $9 billion to $17 billion to $21 billion.

3.2 Financial Outcomes from Credit Data

To accurately measure financial health and other outcomes of interest, I use individual credit file data from the Federal Reserve Bank of New York/Equifax Consumer Credit Panel (CCP). This panel follows a 5 percent random sample of all Equifax credit files in the United States and contains data on a variety of account activities as well as and Equifax 3.0 Risk Scores (credit scores). In my analysis, I limit the individual data to those age 18-64 that are part of the primary 5% sample for years 2005-2016.[13]

Because I am unable to link IRS data and the CCP, I aggregate all financial measures to the ZIP code level. Individual counts are scaled by 20 to capture population levels from the

[13] My study is limited by the coverage of the Consumer Credit Panel. Recent research by the Consumer Financial Protection Bureau notes that approximately 11 percent of US adults are not represented in credit bureau data. These consumers are more likely to reside in lower-income areas, have lower incomes, be a part of a racial or ethnic minority, and be at the extremes of the adult age distribution compared to the population included in credit bureau data (Brevoort et al., 2016). Given that the population eligible for PTC is slightly higher income than the Medicaid population, this is less of a concern in this study.

5% random sample. For each binary outcome (below), I construct a rate by dividing the total number of credit files with each adverse event by the number of credit files. In order to match the quarterly CCP data to the annual SOI data, I use only CCP data from the 4th quarter of each year, which is reported in December, to represent the effects of the subsidies received that calendar year. This exercise also makes calculations less computationally intensive because analyses of the quarterly CCP impose prohibitively large computing costs.

I focus on the set of catastrophic financial events recorded in the CCP data that health insurance is, in part, designed to mitigate. These are debts in third-party collections; home foreclosures; having accounts in severe delinquency on a credit card, mortgage, or auto loan; and new bankruptcy filings (Chapter 7 and Chapter 13).[14] I define "severely delinquent" or "severe delinquency" as an account which is currently at least 120 days past due or contains a "major derogatory event" on the credit file related to the account such as repossession, is in collections, or is in consideration for bankruptcy. Among the population age 18-64, I use the ratio of the number of credit files experiencing each event divided by total credit files in the ZIP code as my main outcomes of interest. This measures the extensive margin of these events. I examine the distribution of intensive margin effects (in dollar amounts) below.[15] I also examine credit score (Equifax Risk Score) as an overall measure of financial health.

For continuous outcomes such as debt amounts in severe delinquency or third-party collections, I examine the intensive margin effects separately. I analyze where in the distribution of outcomes the effects of premium tax credits are largest. I construct measures of the within-ZIP code percentiles (1-99) of the amounts of each type of debt conditional on a positive balance and plot the coefficients from each outcome similar to the way quantile treatment effects are presented in the larger economics literature.[16]

[14] I calculate new bankruptcies as the change in bankruptcy status between years. For this reason, the sample of measured bankruptcies runs from 2006-2016.

[15] If a person experiences multiple events, say, multiple severely delinquent credit cards, I count these as a single occurrence for that person. All rates are based on the total adult population age 18-64 with a credit file, which captures the equilibrium effects of the policy rather than effects on specific populations such as homeowners.

[16] I exclude zeroes because severe derogatory events are relatively rare in the total adult population. The most common event in my sample is the existence of debt in third-party collections, which affects less than

The large sample size of the CCP allows me to create financial health measures for different subgroups to investigate heterogeneous effects. I break down each geographic unit into subgroups by tiers of credit score (Equifax Risk Score). These tiers are: below 550, 550 to 649, 650 to 699, 700 to 749, 750 to 799, and 800 to 850. Having a lower credit score entails information about one's credit limits, the utilization rate of the credit they do have, how long they have held various accounts, and their payment consistency. In addition, adept use of credit may be a marker of financial sophistication regardless of income. A lower credit score implies constrained levels of credit access or ability to absorb costs going down the score scale either because their credit utilization rates are high, because they cannot open additional lines of credit or face higher costs of credit, or because they lack the financial knowledge to adjust. Those with lower credit scores may be unable to absorb even a moderate unexpected medical bill without insurance coverage, let alone a large one. Small, unexpected debts can be deleterious for financial health and may be widespread (CFPB, 2014).[17] Uninsured households are more likely to have such emergency bills, and credit-constrained households have less room to borrow.

I provide more detailed information on variable construction and sources in Appendix B. Summary statistics are listed in Table 1. Overall, the average ZIP code had approximately 7,900 credit files for those 18-64. Just over a third of credit files have debts in third-party collections, and the mean amount of these debts was just over $3,000 from 2005-2016. About 75 files per 1,000 have severely delinquent credit card debt with average amounts of about $5,000. The typical bankruptcy and foreclosure rates from 2005-2016 were 6.2 and 4.5 per 1,000 respectively. The mean average credit score across ZIP codes is 678. Notably, there is a relatively large variance in the incidence of foreclosures, bankruptcies, and severely delinquent mortgage debts: the standard deviations in these variables are larger

a third of credit files in any given year. The inclusion of zeroes simply compresses my effects further up the percentile distribution, making visualization more difficult.

[17]For example, in 2014, 46 percent of households in the United States said they would either not be able to cover an emergency $400 expense or would borrow (via credit card or other means) to cover the cost (Federal Reserve Board, 2015).

than their means.

3.3 Simulated Instrument: CPS and Health Insurance Plans

As a check on my primary specification, I construct a simulated instrument in the spirit of Currie and Gruber (1996). For my fixed, national sample, I use the 2013 Current Population Survey Annual Social and Economic Supplement (CPS ASEC), which contains detailed information on health insurance coverage and premium payments, the availability of employer-sponsored insurance plans, family structure, age, and income—in short, the characteristics upon which all the legislated determinants of tax credit eligibility on the consumer side depend.[18]

The legislated determinants of subsidy eligibility also depend on the "Second lowest-cost Silver plan" available on a consumer's ACA exchange website and each state's Medicaid expansion status. For information on these plans, I use the public use Qualified Health Plan (QHP) Landscape files produced by the Center for Medicare and Medicaid Services (CMS). These data files contain the universe of health insurance plans available on the federal health exchange website in each insurance Rating Area, which is usually a county or 3-digit ZIP code. For state-based exchanges, similar measures are available from the CMS Center for Consumer Information and Insurance Oversight (CCIIO) in the form of the State-based Exchange Public Use Files (SBE PUF). When these are not available, as in some states in 2014-2015, I include public-use data from the Robert Wood Johnson Foundation HIX Compare datasets. If an ACA Rating Area is listed at the county level, I allocate these values to ZIP codes using the share of the population in each ZIP code as a weight.[19] I also pull each state's age curve and family tier rules from the CMS database.[20]

[18] The underestimation of health insurance premiums in the CPS in comparison to administrative records in Larrimore and Splinter (2019) will not bias my simulated instrument estimates because it will be applied broadly to all ZIP codes in my sample.

[19] Because I am missing some states' exchange information for 2014, the sample size of each regression using this simulated instrument is slightly smaller. In this list are Colorado, Connecticut, District of Columbia, Hawaii, Kentucky, Maryland, Massachusetts, Minnesota, Nevada, New York, Oregon, Rhode Island, Vermont, and Washington. To make sure that sample composition is not driving any major differences in my estimates, I show OLS results with the full sample as well as the IV sample.

[20] See https://www.cms.gov/CCIIO/Programs-and-Initiatives/Health-Insurance-Market-Reforms/

My simulated instrument, which is constructed from these datasets, uses *statutory* eligibility as an instrument for *actual* subsidy receipt. To construct the instrument, I first take the 2013 CPS ASEC supplement as a fixed sample. Using data on household health insurance coverage, income, and family structure, I run each household in the CPS through the exchange rules applicable to them in every ZIP code for which I have premium data. Based on Medicaid expansion in each ZIP code's state in each year and the benchmark premium in each ZIP code-year-age-family structure cell, I calculate the per capita eligibility for premium tax credits in every ZIP code-year cell for that fixed sample. The instrument then reveals what the per capita eligibility for tax credits would be in every ZIP code if that ZIP code had the distribution of income, family structure, and health insurance coverage of the nationally representative 2013 ASEC.

As additional controls, I include ZIP-code level statistics from the American Community Survey (ACS) and the decennial Census. Among these are the total population, racial/ethnic makeup of the ZIP code, the age distribution of the ZIP code, the unemployment rate, the share of adults with a Bachelor's degree or more, family structure, median household income, and median house price as a measure of the cost of living and housing market stability. From the Health Resource and Services Administration's Area Health Resource File (AHRF) dataset, I include measures of healthcare provider supply that may influence insurance or medical costs. These include the number of primary care physicians and other care workers such as physician assistants and nurse practitioners. Variables originally provided at the county level are allocated to ZIP codes based on population weights.[21]

In total, my primary estimation sample consists of 20,625 unique ZIP codes spanning the years 2005-2016. My sample ZIP codes cover 139 million total tax returns of the 148 million filed in 2014 and 272.5 million of the 284 million total personal exemptions claimed nationally. Thus, my sample covers 96% of the total tax filer population and 94% of tax

state-rating. (Accessed June 1, 2020).

[21] This allocation ensures that, even if a medical provider is not located in an exact ZIP code, they are apportioned to a ZIP code if they are located in the same county.

returns.[22]

Though my sample covers all but a few tax filers in the United States (excluding those living abroad), it does not cover all ZIP codes. To start, I am limited in ZIP code coverage by the IRS data. Coverage of populated ZIP codes is not complete because the IRS takes various steps to limit disclosure risk. In addition, I limit my sample to ZIP codes with at least 30 credit file observations for those age 18-64. This excludes approximately 4,800 sparsely populated ZIP codes (or 57,600 total observations) from my sample as well as some large retirement communities. Next, I keep areas that have complete ACS and Census data, and I limit my sample to a balanced panel for years 2005-2016. These limitations, though excluding a fair number of ZIP codes, exclude very few residents.

4 Empirical Strategy

I use the panel nature of my constructed dataset and the 2014 rollout of the premium tax credit subsidies to identify the effects of the subsidies on these financial outcomes. My main empirical strategy is a dose-response difference-in-differences approach. The base model is expressed for ZIP code z in year t as:

$$y_{zt} = \beta_0 + \beta_1 TaxCredits_{zt} + X'_{zt}\beta_2 + \delta_z + \tau_t + \varepsilon_{zt} \tag{1}$$

The $TaxCredits$ variable is a measure of the premium tax credits received per resident under age 65 in the ZIP code; it implicitly contains a $Post$ interaction common to difference-in-differences designs and is set to zero for all years before 2014. Variation in this variable, or the intensity of the treatment, comes mostly through differences in the pop-

[22] I mention this coverage to distinguish geographic coverage from population coverage. These tax returns are for the full filing population of any age, though the outcomes I focus on are for those ages 18-64. Approximately 10 percent of residents in the US do not file their taxes each year (Larrimore et al., 2019), so I am limited in coverage to the filing population. However, according to Cilke (2014), 38% of the non-filer population is over 65, while 60% of non-filers with wages had wages below the filing requirement for a single person. Like those without credit files, this population is most likely to align with the Medicaid program or else be eligible for Medicare, suggesting the missing non-filer population is unlikely to strongly affect my analysis.

ulations eligible for tax credits across ZIP codes. Areas that have more low- and moderate-income residents, areas with higher concentrations of the uninsured or part-time workers without employer-sponsored health insurance, and areas with a higher share of those with pre-existing conditions that were not able to purchase health insurance prior to the ACA have higher numbers of eligible residents.

The X vector contains the various controls mentioned previously, such as median household income and race/ethnic composition, family structure, education, and home values as well as my measures of medical provider supply.[23] The vector includes an indicator for living in a Medicaid expansion state in the 2014-2016 period, which is mechanically related to tax credits. While some states expanded Medicaid in 2014, others waited until later or did not expand at all. The year fixed effects (τ_t) take into account any national shocks to financial and health insurance markets such as the Great Recession or the imposition of the ACA's individual mandate and other reforms in 2014. The δ parameter takes into account time-invariant characteristics of the ZIP code. The coefficient of interest, β_1, measures the change in the outcome within high-receipt areas for 2014-2016 relative to the change in low-receipt areas. I interpret this coefficient as an "intent-to-treat" estimate as I cannot identify which people within the ZIP code actually received treatment. Standard errors are clustered at the ZIP code level.

The key assumptions underlying this approach are that low-receipt areas act as a valid counterfactual for high-receipt areas because of parallel trends before treatment and that high- and low-receipt areas do not experience differential shocks. There are two main threats to identification in this design. The first is if high-receipt areas were on a different trend before the implementation of the ACA in 2014 than low-receipt areas. The direction of any bias would depend on the direction of the pre-trends. The second is differential shocks in high-receipt areas in the 2014-2016 period correlated with both PTC receipt and financial outcomes. Because receiving more in PTC is most likely correlated with a higher prevalence

[23] Race and ethnicity come from the ACS. Neither race nor ethnicity are identifiable in tax files or credit files.

of negative financial outcomes, this may bias my estimates closer to zero. Below, I explain my approaches for addressing these two possibilities.

Any analysis of the ACA in 2014—and any panel analysis covering part of the post-2007 period—must tackle the empirical challenge posed by the Great Recession. The 2008-2011 period was extremely tumultuous economically and socially, and some states and local areas were demonstrably harder hit by the financial crisis, the foreclosure crisis, unemployment, and loss of local government fiscal capacity. The labor market consequences of the recession, with its far-reaching effects on other aspects of the local economy, hit low- to moderate-income residents the hardest (Grusky et al., 2011; Smeeding et al., 2011).

In my data, areas that saw the highest peaks of foreclosures and delinquent mortgage debt were also those which received the most in premium tax credits. Panel A of Figure 2 shows how areas that received more in PTC were systematically harder hit by the foreclosure crisis. The recovery for those areas in the top quartile of treatment was faster in the 2011-2013 stage in part because the 2009 peak was nearly 50% higher in these areas than in quartile three.[24]

In order to systematically test for violations of parallel pre-treatment trends for each outcome, I estimate a regression of the form:

$$y_{zt} = \beta_0 + \sum_{t \neq 2013, t=2005}^{2016} \alpha_t \mathbb{I}_t * \overline{TaxCredits}_{z(2014-2016)} + X'_{zt}\beta_1 + \delta_z + \tau_t + \varepsilon_{zt} \quad (2)$$

This is similar to Equation 1 except I interact year dummy variables ($\mathbb{I}_t$) with 2014-2016 average annual PTC per capita (a measure of average treatment intensity). A sign of a violation of parallel pre-treatment trends would be if the coefficients for 2005-2013 trend up or down, especially closer to 2013, indicating that high-receipt areas deviated from the trend in low-receipt areas just before the ACA was implemented. Panel B of Figure 2 demonstrates how strongly high-treatment areas deviated from low-treatment areas in their foreclosure

[24] I adjust for Medicaid expansion in my diagnostic figures by regressing PTC per person under age 65 on a dummy variable for Medicaid expansion and then calculating the predicted residuals.

rates during the Great Recession and just prior to 2014. It is clear from this analysis that there are not parallel pre-trends for high- and low-PTC areas with regard to foreclosures.

To address these differential pre-trends, I follow a modified two-step approach similar to Goodman-Bacon (2016) and Kuka et al. (2020). In their applications, they regress their outcomes on their control variables and a linear time trend interacted with eventual treatment status during the pre-treatment period only. This mechanically accounts for differential pre-trends for treatment and control groups. They then calculate the predicted residuals of that regression for both the pre- and post-treatment periods. Their subsequent regressions use these residuals as the outcome. This approach differences out the variation that is due to different pre-trends across treatment status and projects the adjusted, parallel trends into the post-treatment period.

In a similar fashion, my first step is to estimate:

$$
\begin{aligned}
y_{zt} = \gamma_0 + &\gamma_1 t * \overline{TaxCredits}_{z(2014-2016)} + \gamma_2 t^2 * \overline{TaxCredits}_{z(2014-2016)} \\
&+ \sum_{t=2008}^{2011} \alpha_t \mathbb{I}_t * \overline{TaxCredits}_{z(2014-2016)} \\
&+ X'_{zt}\gamma_2 + \delta_z + \tau_t + \nu_{zt} \text{ for t=2005-2013} \qquad (3)
\end{aligned}
$$

Because the PTC treatment variable is continuous, I interact annual average PTC per capita from 2014-2016 with a flexible quadratic time trend for the pre-treatment period ($\gamma_1 t$ and $\gamma_2 t^2$). Because the Great Recession resulted in large trend breaks, I add an additional modification: I include interactions between this average treatment and dummy variables for peak recession years in 2008-2011 ($\alpha_{2008} - \alpha_{2011}$). These interactions address the fact that the differential effects of the recession during these peak years are not well captured by any single functional form. Together, the recession dummies and flexible quadratic time trend mechanically eliminate the differential trends before the implementation of the ACA. In the second step, I use the predicted residuals for the entire sample period from 2005-2016

($\hat{\nu}_{zt}$) as the outcome in Equation 1.[25]

Empirically, the results of this exercise reveal far closer trends during the 2005-2013 pre-treatment period than the raw values, which is substantial improvement in model fit.[26] Panel C of Figure 2 shows that despite a slightly elevated foreclosure rate in quartile four, this difference does not dramatically peak with the recession, and the shape of the curve is nearly identical. Panel D shows the relevant coefficient graph. Contrary to Panel B, there is a relatively flat pre-trend and a noticeable structural break in 2014 with the introduction of the ACA.[27]

These trend-adjusted figures form the basis of my preferred estimates. The raw and adjusted time series values and parallel trends test graphs for other outcomes are in Figures 3-7, and each reveals improvements in trend match. In practice, this exercise changes most estimates from the Equation 1 base model little with the exception of delinquent credit card debt (the sign switches) and bankruptcy (the magnitude nearly doubles).[28]

Outside of differential pre-trends, a separate threat to identification would be if a ZIP code experienced a shock correlated with financial health and PTC eligibility. My setting is similar to Currie and Gruber (1996) in that premium tax credit eligibility might also

[25] Alternatively, I also estimate the basic panel model incorporating ZIP code-specific quadratic time trends in Appendix Table A2. The results, while similar to my residualized and IV estimates, are theoretically based on fitting a single time trend to both pre- and post-treatment periods. This captures some of the effects of the treatment and could result in biased estimates (Lee and Solon, 2011).

[26] Regressions using a flexible quadratic trend without the dummies for peak recession years still result in noticeable deviations during the Great Recession (see Appendix Figures A1 and A2). This provides key justification for the inclusion of recession-year dummies.

[27] For some outcomes, the coefficient for 2012 is slightly higher than other coefficients, while 2013 is mechanically zero, leading to some visual deviation from a flat pre-trend. The quadratic trend slightly under-predicts the severity of remaining recession conditions in 2012 in the highest-receipt areas.

[28] One alternative to including peak recession year indicators is to fit a time trend to the pre-ACA data completely excluding peak recession years. For results using this approach, see Appendix Table A3. For additional robustness, I also generate estimates with state by year fixed effects for the residualized, pre-trend corrected outcome regressions. Those estimates are in Appendix Table A4. As a final robustness check, I also perform a similar estimate as that in Equation 1 at the individual level, expressed as:

$$y_{izt} = \beta_0 + \beta_1 TaxCredits_{zt} + X'_{zt}\beta_2 + \delta_i + \tau_t + \varepsilon_{izt}$$

In this specification, I apply ZIP code PTC per capita to the individual and include individual fixed effects rather than ZIP code fixed effects and cluster the standard errors at the ZIP code level. For computational ease, I take a 25% random sample of the full CCP to perform this estimate. These estimates are similar to my ZIP code estimates. Those results are in Appendix Table A5.

relate to credit outcomes through local shocks to overall employment or the availability of employer-sponsored health insurance. If ZIP codes experience a shock in 2014-2016 that is correlated with PTC receipt and also leads to worse financial outcomes, this will bias my estimates toward zero. My simulated instrument procedure bypasses these concerns about local shocks and measures what the per capita eligibility would be if the population of each ZIP code matched the CPS ASEC population in 2013 before the implementation of the ACA.

I estimate a two-stage least squares model in which ZIP code simulated per capita PTC eligibility from the CPS acts as an instrument for actual PTC receipt per capita:

$$TaxCredits_{zt} = \alpha_0 + \alpha_1 SimulatedPTC_{zt} + X_{zt}'\alpha_2 + E_{zt}'\alpha_3 + \delta_z + \tau_t + \eta_{zt} \tag{4}$$

$$y_{zt} = \beta_0 + \beta_1 \widehat{TaxCredits}_{zt} + X_{zt}'\beta_2 + E_{zt}'\beta_3 + \delta_z + \tau_t + \varepsilon_{zt} \tag{5}$$

This approach uses simulated PTC per capita in the first stage to predict actual take-up of premium tax credits per capita in each ZIP code. The E vector is a set of important controls relevant to the simulated instrument, which I explain below. The identifying assumptions of the simulated instrument design are, first, that the drivers of statutory PTC only affect financial outcomes through their effects on PTC take-up (the exclusion restriction); and second, that simulated PTC strongly predicts actual take-up of PTC (the relevance criterion).

Variation in simulated PTC per capita comes from two features: the Medicaid expansion status for each state, and the premium charged for the "second lowest-cost Silver plan" in the ZIP code for each specific age band and family structure. Importantly, how a baseline individual premium in each ZIP code translates into age-specific premiums or family structure-specific premiums is a function of state policy. Isolating this exogenous variation in eligibility from possibly endogenous insurer choices is the goal of the E vector of controls.

Simulated instruments like mine are special applications of a Bartik/shift-share instrument. In the canonical wage and employment setting common to many labor models, Bartik instruments implicitly rely on industry shares as instruments for exposure to a treatment, which may be problematic if industry shares predict outcomes through unobserved or alternative channels outside the treatment being measured (Goldsmith-Pinkham et al., 2020). In simulated instruments, the "industry shares" portion is an assumed exogenous policy variation across space. In my setting, these are the premiums for the "second lowest-cost Silver plan" that determine statutory eligibility, which may be responsive to endogenous insurer choices that predict financial outcomes outside of their effects on simulated eligibility. In a Bartik setting, if the determinants of industry shares that could affect wages or employment outside of their driving the local intensity of a national shock were known, the researcher could control for those endogenous determinants with fixed controls (Goldsmith-Pinkham et al., 2020). With simulated instruments, endogenous determinants of policy variation can be known and controlled in the model.

Because the premium set for baseline individual Silver plans may be heavily affected by the choices of insurers and therefore may be correlated with the financial outcomes of local residents, it is important to control in the model for these choices. The E vector consists of proxies for the choices discussed in Section 2. To proxy for the state base (single age) individual premium set by insurers that reflects state-specific rules that drive costs, insurer expectations about enrollment, and within-state competition, I include a control for the average Silver premium in each state for an individual age 30. To control for entry choices in Rating Areas in each state, I include the number of insurers offering Silver plans in each ZIP code.[29] Finally, to control for the effects of each insurer's geographic factors as well as other unobserved local determinants of insurance costs on the benchmark Silver plan for individuals, I include the premium for benchmark Silver plan for an individual age 30. These controls remove variation from simulated PTC that is attributable to insurer pricing and entry choices. Local deviations from this single age premium over age and

[29] I include this as a linear term because dummy variables for the number of insurers result in linear effects.

family structure are set by the state. Therefore, conditional on these controls, the remaining variation driving simulated PTC comes from state policies. These policies affect eligibility for premium tax credits independently of other considerations and thus are likely to satisfy the exclusion restriction.[30] A violation of this restriction requires that these state policies affect financial outcomes directly through a channel outside their effects on PTC eligibility. Because the policies only apply to the individual exchange market, this is unlikely.

The second assumption of the simulated instrument, which is testable, is that simulated eligibility is a relevant predictor of actual take-up. I test this by testing the features of the first stage regression (Equation 4). There are two reasons this test is necessary. First, fewer than ten states set their own age curves and family tier ratios, so variation across these states must be sufficient to drive meaningful variation in take-up across states. Second, the control variables in my regression are quite restrictive. Nevertheless, there is a strong first-stage correlation between statutory eligibility and actual PTC take-up even conditional on these restrictive controls (32 cents in take-up for every dollar of simulated PTC). The high Kleibergen-Paap rk Wald statistic value of 475 suggests that the simulated instrument satisfies the relevance criterion (see Appendix Table A1).

5 Results

5.1 Extensive Margin Effects

I begin by presenting results from the baseline models measuring the rates of negative financial shocks for the overall population, or the extensive margin effects. In each of Tables 2 and 3, Column 1 is the basic difference-in-differences model without controls (the X vector), Column 2 adds controls from the ACS, Census, and Area Health Resource File (AHRF). Column 2 results ensure that the difference-in-differences results are not driven by differential changes in ZIP code composition or providers. Column 3 uses the "residualized" or pre-trend corrected outcome from the two-step correction procedure discussed in Section

[30]For more information on state-specific policies regarding age and family premium pricing, see Appendix B.

4. As mentioned previously, 2014 health insurance exchange data are missing for 14 states in 2014. To ensure that sample composition does not drive any differences between the results of my IV and my OLS estimates, Column 4 presents the same estimates as Column 3 but limiting the sample to the same sample as the IV estimates by excluding ZIP codes with missing exchange data. Column 5 shows the IV estimate using my simulated instrument.

Table 2 shows the extensive margin for the first three catastrophic outcomes: severe mortgage delinquency, foreclosure, and consumer bankruptcy, while Table 3 shows the extensive margin results for third-party collections, severe credit card delinquency, and severe auto delinquency. Panel A of Table 2 shows that every $1 per capita in premium tax credits received in a ZIP code reduces the rate of severe mortgage delinquency per 1,000 by approximately 0.03. At the treatment mean (58.86), this translates to a reduction of 13 percent of the sample mean. The IV estimate for Column 5, while slightly smaller in magnitude, is not statistically significantly different from Columns 3-4. The results are similar in direction and relative magnitude in Panel B for the foreclosure rate. According to Column 3, at the treatment mean, the foreclosure rate fell by approximately 0.59 foreclosures per 1,000 credit files, or 13 percent, as a result of ACA tax credits. In Panel C for consumer bankruptcy, the magnitude of the effect at the treatment mean is approximately 0.38 bankruptcies per 1,000, or 6 percent. For bankruptcies, the relative magnitude of the effects nearly doubles after correcting for pre-trends (Column 3). The results in Column 5 for Panel B are larger in magnitude than those in Columns 3-4, but the relatively wide standard errors indicate the differences are not statistically significant across the three models at conventional levels. In Panel A of Table 3, I show the results for the rate of having any debts in third-party collections. As the receipt of premium tax credits increases, the rate of third-party collections increases slightly. At the treatment mean, this translates into an increase of 1.7-2 third-party collections per 1,000, or a 0.5% increase. Although the incidence of having debts in third-party collections increased, the *amounts* of such debts appear to decline substantially, as I will show in Section 5.3, particularly at the top of the distribution. One explanation for this phenomenon is that the newly insured population may increase their use of medical care and thus incur small costs such as co-pays or deductibles that are subsequently sent to collections after a period of non-payment.[31]

Panel B for severe credit card delinquency reveals much of the utility of using the residualized outcome variable as well as the simulated instrument. Columns 3-5 reverse the sign of the effect from positive to negative, and, in fact, are close to an order of magnitude larger in absolute terms. This sign reversal is evident in the difference between the coefficient graphs in Panels B and D in Figure 6. Panel B shows a clear differential pre-trend. After correcting for this pre-trend, the rate of severe credit card payment delinquency fell by 3.9 per 1,000, or approximately 5% of the treatment mean. Across columns of Panel C in the table, there is notable agreement of the coefficients. Column 3 is the most conservative of the estimates. The IV results in Column 5 are too noisy to make any clear inference. The results here suggest that the rate of having severely delinquent auto debt fell by just over 3% at the treatment mean.

It is apparent from both tables that the premium tax credits provided through the ACA substantially reduced the extensive margin incidence of catastrophic loss. Provision of private insurance through these channels substantially lowered the rate of severe mortgage delinquency, foreclosure, and consumer bankruptcy with smaller but still notable reductions in delinquent credit card and auto debt.

5.2 Heterogeneity by Credit Score (Equifax Risk Score)

I next present estimates broken down by the risk score buckets mentioned in Section 3. This heterogeneity is particularly important given that credit-constrained consumers may not have access to credit that may otherwise help them absorb a financial shock, especially if they are uninsured.

[31]The CFPB stated in a recent report: "Medical debts occur and are collected through unique circumstances and practices... In particular, the complexity of medical billing and the third-party reimbursement processes faced by most patients and their families is a potential source of confusion or misunderstanding... That complexity could lead some consumers to be unaware of when, to whom, or for what amount they owe a medical bill." (CFPB, 2014).

Tables 4 and 5 present results for each risk score bin. The estimates are based on the pre-trend correction procedure described earlier. Panel A of Table 4 indicates that the effects of ACA tax credits on severe mortgage delinquency are similar in percent terms for people with credit scores below 700, while effects above 700 are not distinguishable from zero. However, the absolute effect on those with lower credit scores is far larger. For those below 550, the effect at the treatment mean is -7.3 incidents of severe mortgage delinquency per 1,000, while the effect for those 550-650 is less than half of that effect: -3.18 severe delinquencies per 1,000.

There are similar patterns across all panels of both tables. Panel B of Table 4 indicates that the effect on foreclosures per 1,000 at the treatment mean is -2.77 and -0.87 for those below 550 and at 550-650 respectively. Although the absolute level of the downward shift for those with scores 650-750 is not as large, given the low baseline foreclosure rate for these groups, the proportional change is large at -53%. For relatively sophisticated consumers with more financial stability at 700-750, the proportional effect is approximately -90% at the treatment mean, though the absolute effect is small.

Panel C shows the results for consumer bankruptcies. For those with scores between 550 and 650, at the treatment mean, insurance subsidies reduced the rate of bankruptcy by over 0.8 per 1,000. For those 650-700, this reduction is 1 per 1,000, or almost 20% of the baseline. This departure from the pattern of other outcomes makes sense if one's credit score is correlated with having assets that need protection from creditors. In Table 5, the results suggest rather consistent proportional effects of the tax credits for those with credit scores at 650 or below across the panels, but each panel re-emphasizes the point that the largest absolute effects are concentrated among those with low credit scores.

Overall, these results show that the vast majority of the overall average treatment effect of premium tax credits are loading upon those whose credit scores (Equifax Risk Score) are below 650. In other words, tax credits appear to most heavily prevent catastrophic financial loss among those who are most likely to be credit constrained or least sophisticated in their financial management in the formal sector. These results echo the general pattern in Argys et al. (2017), who find that subprime borrowers were hit hardest by sudden disenrollment from Medicaid in Tennessee in 2005.

5.3 Intensive Margin Effects Across the Distribution

The incidence of catastrophic events may not capture heterogeneity in the monetary effects of adverse health shocks once they occur. For example, the balance of severely delinquent debt can vary between borrowers, as can the value of the debts sent to third-party collections agencies. To capture heterogeneity across the distribution of each outcome, I present results of 99 separate regressions corresponding with percentiles 1-99 of the within-ZIP code distribution of each outcome. I test the balance of severely delinquent mortgage debts, credit card debts, and auto debts; the balance of accounts sent to third-party collections; and credit score (Equifax Risk Score). Each of these represents the intensive margin rather than the extensive margin effects measured earlier.

I present the results of these regressions for negative financial outcomes in Figure 8. In each of the panels, the blue line represents the point estimate for each separate regression with that percentile as the outcome, and the green vertical bars denote the average value across ZIP codes at the 10th, 25th, 50th, 75th, and 99th percentiles. These vertical bars provide some context on the size of the effects relative to their baseline values.[32] Each of these corresponds with the pre-trend corrected, residualized outcome models mentioned previously with standard errors clustered at the ZIP code level.

Panel A shows that from about the 40th percentile upward, premium tax credits meaningfully lowered the balance of severely delinquent mortgage debt. At the 99th percentile, $1 spent per capita on premium tax credits lowered the balance of severely delinquent mortgage debt by $725. At the treatment mean, this equates to a reduction of approximately $43,000 relative to a sample mean of $315,000 at that percentile, or about a 14% reduction. This is

[32]Full tables of the percentile-specific point estimates and standard errors for each outcome are in Appendix Tables A6 and A7.

closely comparable to the percent effect on the extensive margin mentioned earlier. Below the 8th percentile in the distribution, there is a small upward shift in severe mortgage debts. At the treatment mean for this group, the results point to an increase in severely delinquent mortgage debt of approximately $4,000, suggestive of small, newly delinquent debts. A small proportion of the adult population may have struggled to make mortgage payments as a result of new payment requirements for premiums or deductibles.

The results for third-party collections (Panel B) show a similar pattern above the 40th percentile. At the 99th percentile, each dollar of premium tax credits per capita led to a reduction of $24 in debts in third-party collections, or a reduction of approximately $1,400 at the treatment mean (5% of the 99th percentile). This is in line with Hu et al. (2016), who find that new enrollees into Medicaid experienced reductions in third-party collections of about $1,100, as well as Brevoort et al. (2020), who find that newly insured Medicaid recipients reduced their medical debts by an average of $1,231 per person per year. These coefficients on the shift in third-party collections debt are particularly important for my calculation of the consumer welfare effects of these subsidies, which I describe in Section 6.1.

The same pattern holds for severely delinquent credit card and auto debts as presented in Panel C and Panel D. At the 99th percentile, the mean treatment is associated with a reduction of $1,648 in severely delinquent credit card debt (6%) and $912 in severely delinquent auto debt (5%). These relative changes are again close to the relative size of the extensive margin effects.

As a composite measure of overall financial health, Figure 9 shows the change in the within-ZIP code distribution of credit scores (Equifax Risk Score) that results from $1 of PTC per capita. This captures where in the distribution of credit health the effects of the subsidies load. There is an upward shift across most of the distribution of credit scores in areas that received more in premium tax credits from 2014-2016. The size of the effect is most pronounced between the 30th and 40th percentiles (where scores are 620-660), with minimal effects above the 60th percentile (720-760). At the treatment mean, the 30th percentile of the credit score distribution shifted upward by approximately 1.5 points.[33]

Overall, the effects across the distribution suggest that the premium tax credits in the ACA strongly affected the risk of catastrophic loss both on the extensive margin and on the intensive margin. Even though private health insurance plans require premiums and cost sharing, these do not have deleterious effects on the financial health of the vast majority of recipients. We see uniformly positive gains for credit scores below the 60th percentile, and no negative effects across the credit score distribution. The only notable exception is the small proportion of mortgage borrowers whose delinquent balances grew modestly.[34]

6 Discussion

6.1 Value of Risk Reduction

Following Finkelstein and McKnight (2008), I contextualize my empirical results in an expected utility framework to simulate the value of risk reduction for recipients. As shown previously, ACA subsidies shifted downward the distribution of third-party collections debts

[33]This 1.5 point increase is marginally larger than the one-year increase attributed to Medicaid expansion in the ACA (Brevoort et al., 2020), which also affects a similar credit score range most strongly. However, Medicaid expansion also exhibits strong upward pressure on credit scores up to the 75th percentile (760), while the effects of premium tax credits fade more quickly and have negligible effects above the 60th percentile (720) (Brevoort et al., 2020). The 1.5 point increase from premium tax credits is interesting in light of the 2.78 point decline that occurred in Tennessee in 2005 when individuals were suddenly *disenrolled* from Medicaid (Argys et al., 2017). The current evidence suggests there is meaningful asymmetry in the effects of gaining vs losing insurance.

[34]These effects could theoretically be driven by an "insurance" effect or a "liquidity" effect due to the simple cash value of the transfer. I argue the vast majority of these effects are driven by insurance effects because, 1) the effects only appear in the right-tail of the distribution, consistent with protections against catastrophic risk, and 2) if the effects were driven by liquidity, we should see effects driven by inframarginal changes in insurance coverage and some substitution from employer-sponsored plans with premiums over 10% of household income toward non-group insurance plans, which includes exchange enrollment. In the US as a whole, employer-sponsored insurance coverage *increased* from 55% of the population below age 65 in 2013 to 56% in 2016, while the non-group share increased from 6% to 8% over the same period. Medicaid increased from 18% to 22% while the uninsured rate fell from 17% to 10%. See https://www.kff.org/other/state-indicator/nonelderly-0-64/?activeTab=graph&currentTimeframe=2&startTimeframe=5&selectedDistributions=employer--non-group--medicaid--uninsured&selectedRows=%7B%22wrapups%22:%7B%22united-states%22:%7B%7D%7D%7D&sortModel=%7B%22colId%22:%22Location%22,%22sort%22:%22asc%22%7D. The primary effects of the subsidies appear to be on the extensive margin of coverage. Enrollment for lower-income adults drops precipitously as subsidy generosity falls, though enrollment is still incomplete even with generous subsidies (Finkelstein et al., 2019).

and the probability of foreclosure and bankruptcy, which are costly to consumers.[35] In an expected utility framework, consumers experiencing this protection from risk will experience a gain in utility. This gain in utility is calculated by examining the change in their risk premium between two states of the world. The risk premium is the difference between the expected outcome for a choice that includes risk and what a consumer would be willing to accept if there were no risk.

I consider a simple framework in which utility is a function of consumption income net of costs associated with medical debts and catastrophic outcomes—particularly foreclosure and bankruptcy. Because bankruptcy is considered a form of implicit insurance (Brevoort et al., 2020; Mahoney, 2015), it is worth considering this outcome with respect to the costs of bankruptcy to the filer.

I calculate the change in consumer welfare as the difference in the risk premium before and after the premium tax credits subsidized insurance ($\pi_b - \pi_a$). The risk premium depends on consumption income (y), the probability distribution of various medical costs and catastrophic negative outcomes ($f(o)$), and the shape of the utility function $u()$.

There are two π parameters, one for before the receipt of PTC and one after—in other words, when facing the different distributions of probabilities and costs in two states of the world. Analogous to Finkelstein and McKnight (2008), who examine the shift in cost distributions between 1963 and 1970 (before and after the implementation of Medicare), I consider two periods: before, b (2013), or after receiving subsidies, a (2014-2016). Here, o represents the distribution of costly outcomes, including payments on medical debts, the internal costs of foreclosure, and the internal costs of consumer bankruptcy.

$$u(y - \pi_b) = \int_0^{\bar{o}_b} u(y - o_b) f(o_b) do_b | Year = 2013 \tag{6}$$

[35] Also note that there are implicit costs associated with collections, foreclosures, and bankruptcies, such as the increased cost of credit, and psychic and social costs which are not captured in this analysis. The analysis also excludes the costs of vehicle repossession and eviction because they are not captured in the credit bureau data.

$$u(y - \pi_a) = \int_0^{\bar{o}_a} u(y - o_a) f(o_a) do_a | \overline{PTC} \quad (7)$$

My dataset is limited because I cannot directly observe out-of-pocket medical spending—either those paid without being sent to collections or those actually paid after being sent to collections. I can, however, infer out-of-pocket spending based on third-party collections information and the recent literature. Two recent papers from researchers at the CFPB, whose consumer credit dataset does contain medical debts separate from other types of collections debt, inform my assumptions. First CFPB (2014) finds that approximately half (52%) of all third-party collections in the United States are owed to medical providers. Second, Brevoort et al. (2020) show that the one-year repayment rates of medical debts in collections are about 36-38% of the face value of the debt they hold and that this proportion is relatively constant across quantiles of medical debt. Taken together, I assume that out-of-pocket spending annually for debts in third-party collections are 19% of the total value of the debt in collections (52% * 36% * third-party collections debt) and that this proportion is uniform across the distribution.[36] After these assumptions, this out-of-pocket spending distribution is designed to be analogous to the measures in Finkelstein and McKnight (2008) and Finkelstein et al. (2015), though my measure is admittedly less precise.

To capture an additional important aspect of consumer financial well-being, I also include the financial risks of bankruptcy and foreclosure in my analysis. To calculate the expected utility of these events, I consult outside estimates of the costs of foreclosure to the homeowner as well as the internal costs of consumer bankruptcy. One estimate suggests that the costs of foreclosure to the homeowner, including administrative and moving costs, were approximately $11,000 in 2013 dollars (Moreno, 1995). Attorney fees after the passage of the Bankruptcy Abuse Prevention and Consumer Protection Act (BAPCPA) were $3,000

[36]While this 52% assumption leads to an admittedly coarse estimate of annualized out-of-pocket medical payments, there is no work of which I am aware which breaks down specific shares of third-party collections debt due to medical debts across the debt distribution. This 19% total reduction relative to third-party collections totals is, however, in line with Finkelstein (2007), who notes that uninsured individuals pay only 20% of the cost of the medical care they consume out-of-pocket.

(Dobbie et al., 2017; White, 2007). Filing fees are $300 on average.[37] I thus calculate a conservative total cost of bankruptcy filing of $3,300. The inclusion of the risk of foreclosure and bankruptcy adds substantially to the measured consumer welfare gains from risk protection.

In the two states of the world (2013 vs post-PTC), I consider the shift in outcomes at the treatment mean attributable to the subsidies and subtract these effects from the observed 2013 distribution of costs and probabilities. For example, I subtract the effects of the tax credits using my pre-trend corrected specification (Equation 3) on foreclosure risk at the treatment mean from the 2013 mean to get the "post PTC" measure of foreclosure risk. I do the same for bankruptcy risk. For the distribution of third-party collections debts, I subtract the percentile-specific PTC effects (by multiplying the effects by a scalar of 0.19 to match the assumed distribution of out-of-pocket debt payments) from the observed 2013 distribution of debts.

The change in the risk premium that occurs with this shift in risk is my measure of consumer welfare gains. In this consumption framework, by assumption, a one dollar increase in out-of-pocket medical spending or spending on bankruptcy or foreclosure leads to a 1 dollar decrease in consumption. This assumption is similarly invoked in Finkelstein et al. (2015) and Finkelstein and McKnight (2008). There are two main choices for the researcher in this exercise: assumed risk aversion parameters and assumed income. I consider constant relative risk aversion (CRRA) utility functions with a risk aversion parameter, θ, of 2, 3, and 4 as is common in the literature with the functional form:

$$u(c) = \frac{1}{1-\theta} c^{1-\theta} \text{ if } \theta > 1 \tag{8}$$

Based on consumption income measures in the Consumer Expenditure Survey, I also vary the assumed per capita consumption at levels of $14,000, $16,000, $18,000, and $20,000.[38]

[37] I rely on industry estimates for this number https://www.natlbankruptcy.com/how-much-does-it-cost-to-file-bankruptcy-2/ (Accessed June 15, 2020).

[38] This is based on Finkelstein et al. (2015), who anchor consumption to the Consumer Expenditure Survey

These dimensions of income and risk aversion parameters provide a broad view of possible gains in consumer welfare due to the premium tax credits.[39]

I present a table of estimates of the consumer welfare gains for these four income levels and three CRRA risk aversion parameters and including vs excluding foreclosure and bankruptcy risk in Table 6. Excluding bankruptcy and foreclosure risk makes my estimates the PTC analog of the Medicaid estimates in Finkelstein et al. (2015). When considering only the change in risk premium that occurs with the shift in medical spending (Panel A), the consumer welfare gains are \$120 for assumed consumption income of \$14,000 at a CRRA risk aversion parameter of 3. This is remarkably similar to Finkelstein et al. (2015), who estimate welfare gains based on a CEX consumption measure and a pure insurance component for medical spending through the Medicaid program of \$126 using a different estimation approach. After including the change in the probability of foreclosure and bankruptcy (Panel B), the overall consumer welfare gain at $\theta = 3$ is \$721 at the lowest income level, and \$378 at the highest income level for a risk aversion parameter of 3. At $\theta = 4$, the consumer welfare gains from risk protection are as high as \$1,012 per year. Because there is evidence that low-income individuals and those experiencing acute stress have higher levels of risk aversion (Haushofer and Fehr, 2014; Cahlíková and Cingl, 2017), a value for θ of 4 may be appropriate considering the low income of PTC recipients.

I reiterate here that these estimates represent what I interpret to be an "intent-to-treat" effect because I cannot observe exactly who in each ZIP code actually received these benefits, who in the ZIP code actually owns their home (rather than renting), nor who owns assets that could be protected in the case of bankruptcy. Though the costs of eviction are large, I cannot observe those in my dataset because evictions are not listed on credit reports. These

(CEX) estimate of non-medical consumption of \$13,541 per capita for the uninsured population below the Federal Poverty Line. Because the population eligible for tax credits had slightly higher incomes, I increase this level incrementally.

[39] This analysis does not explicitly account for the premiums paid by recipients but does so implicitly. Inasmuch as monthly premiums place a burden on household budgets which could translate into negative financial outcomes, I account for these premiums in my estimates of the change in the distribution of risk, which incorporate the net effect of new payment obligations and risk protections.

welfare effect estimates therefore plausibly represent lower bounds, even when considering this narrow set of outcomes.

As with any analysis of public policy, we want to know the relative costs and benefits. Because the exact size of the recipient population of tax credits is not clear in tax data, I infer costs from two sources: HHS reports and the adult population in each tax filing unit in my sample area multiplied by the number of returns receiving PTC. According to HHS reports, the average recipient of tax credits received $264 per month in 2014, $268 per month in 2015, and $294 per month in 2016 in premium tax credits in the states on the federal exchange.[40] Given that costs are similar in states with their own exchanges, I can compare the recipient costs to the value of the risk protections.[41] Panel C presents the full range of estimates for the average annual cost per recipient of the program from HHS reports, the individual insurance value or welfare gain from my various Panel B cells, the share of the costs realized in risk protections to recipients, aggregate annual average costs from my sample area, and aggregate insurance value/welfare gain in my sample area.[42] The middle line in Panel C suggests that risk protections for recipients sum to as much as 32% of the annual cost of subsidies per recipient. At average consumer welfare gains of $721-$1,012 per year for those at risk parameters of three or four and income of $14,000 compared to average costs of $3,168-$3,528 per recipient, 20-32% of the annual cost of the tax credits is realized in protections against risk from medical debt, foreclosure, and bankruptcy alone. On a total average annual cost from 2014-2016 of $15.5 billion in my sample, annual aggregate welfare gains in my sample area sum to between $1.8 billion and $5.46 billion. These figures notably do not include any health effects (physical or mental), effects on renters' ability to avoid eviction, protections against vehicle repossession, or effects operating through in-

[40] See, for example, https://aspe.hhs.gov/report/health-insurance-marketplace-2015-average-premiums-after-advance-premium-t Accessed June 10, 2020).

[41] The average PTC per recipient for the federal exchange population and the national population including state-based exchanges appears to be virtually identical. See, for example, https://www.cms.gov/newsroom/fact-sheets/june-30-2015-effectuated-enrollment-snapshot.

[42] I infer the recipient population in the tax data by multiplying the number of recipient returns by the average non-dependent exemptions per return (1.33). Multiplying this total by average individual welfare gains yields aggregate welfare gains.

creases in credit-worthiness. Protection against this narrow set of financial risks can account for close to a third of the costs.

6.2 External Spillovers

Using back of the envelope calculations, I find that the external spillovers of ACA subsidies are substantial. With regard to foreclosures, several papers estimate price discounts of 1% of the price of homes within a 300-600 foot radius of a foreclosed home, or a total annual indirect transfer of approximately $28,700 per foreclosure (Campbell et al., 2011; Harding et al., 2009; Immergluck and Smith, 2006; Gallagher et al., 2019). Using this figure, I calculate a total transfer to nearby homeowners of $2.76 billion through the ACA tax credits. Based upon estimates of the cost to private investors and GSEs of foreclosures of $95,000 during the Great Recession (Hsu et al., 2018), ACA subsidies provide mortgage lenders an indirect transfer of approximately $9.12 billion annually. Local governments face costs commonly in the $19,000 to $34,000 range per foreclosure depending on severity (Apgar et al., 2005), meaning that ACA premium tax credits subsidize local municipal budgets by a total of $1.8 billion to $3.3 billion each year. Using a conservatively estimated cost of bankruptcy to creditors of $42,000 per bankruptcy using insights from various papers, I calculate that the ACA premium tax credits provide an indirect subsidy to creditors of at least $2.6 billion annually.[43] In total, indirect subsidies sum to as much as $17.8 billion, or 113% of the cash cost of the subsidies. The direct benefit to recipients and indirect benefits to outside parties together total as much as $23.26 billion, or 150% of the cash cost of the transfers.

[43]Work using a national sample of bankruptcy filings suggests that the median unsecured debt in Chapter 7 asset cases was $61,916 (Jiménez, 2009) at the beginning of 2007 near the end of the housing boom and just before the housing bust. Notably, 93% of cases were "no asset" cases, meaning recovery of debts to unsecured creditors was even less likely. The recovery rate on unsecured debts may be as little as 8 cents on the dollar (Jiménez, 2009). For Chapter 13 bankruptcy cases, recovery rates on secured and unsecured debt may be approximately 20-30 percent of the face value of the debts (Li, 2007; Eraslan et al., 2017; Norberg and Velkey, 2005). Given that Chapter 13 requires repayments to creditors of unsecured debts be equal to that of Chapter 7 filings, I assume this recovery rate holds across filing types. I assume a conservative 30% recovery rate on average debts of $61,900 per filing, for a total loss of $42,000 per filing.

7 Conclusion

In this paper, I have measured the causal effects of premium tax credits as a funding mechanism for private health insurance on catastrophic financial outcomes using a dose-response difference-in-differences design as well as a simulated instrument.

Among the non-elderly adult population, premium tax credits from 2014-2016 substantially reduced the rates of severe delinquency on a mortgage, foreclosure, bankruptcy, and severe delinquency on credit cards and auto loans. On the other hand, these tax credits marginally increased the share of non-elderly adults with debts in third-party collections but decreased the amounts of these debts substantially at the top of the distribution. Debt amounts that are severely delinquent fall substantially at the top of the debt distribution across mortgage accounts, credit cards, auto debts, and collections debt. The extensive margin effects load heavily on those with low credit scores, and there is substantial improvement in credit scores for those below 660.

I find that consumer welfare as measured by the change in the risk premium from reduced medical debts and protection against foreclosure and bankruptcy costs (the value of risk reductions) increased, particularly for the lowest-income recipients. When comparing to the Medicaid program, despite the differences in implementation and cost-sharing rules, welfare gains from ACA tax credits through pure insurance from medical debts are similar. Using conservative assumptions, 20-32% of the monetary costs of the credits are realized in welfare gains to recipients. These welfare gains are substantial despite being very narrow in scope (medical debt, bankruptcy, foreclosure) and excluding several other channels through which subsidies may provide welfare gains to recipients.

I also find that there are substantial implied spillovers of these tax credits to outside parties such as nearby homeowners, mortgage lenders, local governments, and creditors whose debtors discharge obligations through bankruptcy. In all, these indirect transfers to these four groups totaled just over $17.8 billion per year. The benefits of these premium tax credits do not accrue to recipients alone but may act as housing market stabilizers as well as buffers to local governments and the financial system.

I am limited in my analysis in my ability to directly connect subsidy receipt with individual credit outcomes. Thus, my estimates are an "intent-to-treat" effect. In addition, I do not detail the economic incidence of the taxes levied to fund this program, nor of the cross-subsidization of health insurance costs that came with the other ACA provisions. My analysis focuses intentionally on answering the first-order question of what the immediate financial benefits are. Future work in this area may attempt to detail the full cost-benefit calculation of these subsidies in a general framework and full social welfare context.

This paper sheds light on the balance of direct cash costs and risk protection benefits from providing subsidies for private health insurance. It gives a broad accounting of the effects of this type of policy regime on consumer financial outcomes and provides clarity on who benefits most from this policy approach. The benefits do not just accrue to recipients, but to a broad set of agents in the economy. Current debates around methods of expanding health insurance coverage most often focus on expanding current public programs like Medicaid or Medicare, while policies focusing on expanding access to private health insurance tend to lag behind in terms of popularity or political airtime. As the first paper to analyze the effects of this type of national policy change, this analysis can contextualize that debate for policymakers and interested parties by showing that there is a viable role for public funding to support the purchase of private health insurance, with meaningful positive effects on recipients' financial health and minimal negative effects. The pure financial insurance value to recipients accounts for a large portion of program costs, even when excluding possible benefits via health, creditworthiness, and protections against eviction or vehicle repossession. That the policy indirectly benefits other actors may provide some sense of who might be tapped to bear some of the funding responsibility in order to internalize the costs associated with their received benefits.

References

Anderson, Michael, Carlos Dobkin, and Tal Gross. 2012. "The Effect of Health Insurance Coverage on the Use of Medical Services." *American Economic Journal: Economic Policy*, 4(1): 1–27.

Apgar, William C, Mark Duda, Rochelle Nawrocki Gorey, and Housing Policy Council. 2005. "The Municipal Cost of Foreclosures: A Chicago Case Study." *Homeownership Preservation Foundation Housing Finance Policy Research Paper*, 1.

Argys, Laura M, Andrew Friedson, M Melinda Pitts, and Daniel Tello-Trillo. 2017. "Losing Public Health Insurance: TennCare Disenrollment and Personal Financial Distress." *FRB Atlanta Working Paper*.

Aron-Dine, Aviva, Liran Einav, and Amy Finkelstein. 2013. "The RAND Health Insurance Experiment, Three Decades Later." *Journal of Economic Perspectives*, 27(1): 197–222.

Baicker, Katherine, Sarah L Taubman, Heidi L Allen, Mira Bernstein, Jonathan H Gruber, Joseph P Newhouse, Eric C Schneider, Bill J Wright, Alan M Zaslavsky, and Amy N Finkelstein. 2013. "The Oregon Experiment: Effects of Medicaid on Clinical Outcomes." *New England Journal of Medicine*, 368(18): 1713–1722.

Barcellos, Silvia Helena, and Mireille Jacobson. 2015. "The Effects of Medicare on Medical Expenditure Risk and Financial Strain." *American Economic Journal: Economic Policy*, 7(4): 41–70.

Brevoort, Kenneth, Daniel Grodzicki, and Martin B Hackmann. 2017. "Medicaid and Financial Health."Technical report, National Bureau of Economic Research.

Brevoort, Kenneth, Daniel Grodzicki, and Martin B Hackmann. 2020. "The Credit Consequences of Unpaid Medical Bills." *Journal of Public Economics*, 187, p. 104203.

Brevoort, Kenneth P, Philipp Grimm, and Michelle Kambara. 2016. "Credit Invisibles and the Unscored." *Cityscape*, 18(2): 9–34.

Cahlíková, Jana, and Lubomir Cingl. 2017. "Risk Preferences Under Acute Stress." *Experimental Economics*, 20(1): 209–236.

Campbell, John Y, Stefano Giglio, and Parag Pathak. 2011. "Forced Sales and House Prices." *American Economic Review*, 101(5): 2108–31.

Card, David, Carlos Dobkin, and Nicole Maestas. 2008. "The Impact of Nearly Universal Insurance Coverage on Health Care Utilization: Evidence from Medicare." *American Economic Review*, 98(5): 2242–58.

Card, David, Carlos Dobkin, and Nicole Maestas. 2009. "Does Medicare Save Lives?" *The Quarterly Journal of Economics*, 124(2): 597–636.

CFPB. 2014. "Consumer Credit Reports: A Study of Medical and Non-Medical Collections." *Consumer Financial Protection Bureau*.

Cilke, James. 2014. "The Case of the Missing Strangers: What We Know and DonâĂŹt Know About Non-Filers." In *107th Annual Conference of the National Tax Association, Santa Fe, New Mexico*. JSTOR.

Courtemanche, Charles, James Marton, Benjamin Ukert, Aaron Yelowitz, and Daniela Zapata. 2017. "Early Empacts of the Affordable Care Act on Health Insurance Coverage in Medicaid Expansion and Non-Expansion States." *Journal of Policy Analysis and Management*, 36(1): 178–210.

Currie, Janet, and Jonathan Gruber. 1996. "Saving Babies: The Efficacy and Cost of Recent Changes in the Medicaid Eligibility of Pregnant Women." *Journal of Political Economy*, 104(6): 1263–1296.

DeLeire, Thomas, Andre Chappel, Kenneth Finegold, and Emily Gee. 2017. "Do Individuals

Respond to Cost-Sharing Subsidies in Their Selections of Marketplace Health Insurance Plans?" *Journal of Health Economics*, 56 71–86.

Dobbie, Will, Paul Goldsmith-Pinkham, and Crystal S Yang. 2017. "Consumer Bankruptcy and Financial Health." *Review of Economics and Statistics*, 99(5): 853–869.

Engelhardt, Gary V, and Jonathan Gruber. 2011. "Medicare Part D and the Financial Protection of the Elderly." *American Economic Journal: Economic Policy*, 3(4): 77–102.

Eraslan, Hülya, Gizem Koşar, Wenli Li, and Pierre-Daniel Sarte. 2017. "An Anatomy of US Personal Bankruptcy Under Chapter 13." *International Economic Review*, 58(3): 671–702.

Federal Reserve Board. 2015. "Report on the Economic Well-Being of U.S. Households in 2014."Technical report.

Finkelstein, Amy. 2007. "The Aggregate Effects of Health Insurance: Evidence from the Introduction of Medicare." *The Quarterly Journal of Economics*, 122(1): 1–37.

Finkelstein, Amy, Nathaniel Hendren, and Erzo FP Luttmer. 2015. "The Value of Medicaid: Interpreting Results from the Oregon Health Insurance Experiment."Technical report, NBER Working Paper.

Finkelstein, Amy, Nathaniel Hendren, and Mark Shepard. 2019. "Subsidizing Health Insurance for Low-Income Adults: Evidence from Massachusetts." *American Economic Review*, 109(4): 1530–67.

Finkelstein, Amy, Neale Mahoney, and Matthew J Notowidigdo. 2018. "What Does (Formal) Health Insurance Do, and for Whom?" *Annual Review of Economics*, 10 261–286.

Finkelstein, Amy, and Robin McKnight. 2008. "What Did Medicare Do? The Initial Impact of Medicare on Mortality and Out of Pocket Medical Spending." *Journal of Public Economics*, 92(7): 1644–1668.

Finkelstein, Amy, Sarah Taubman, Bill Wright, Mira Bernstein, Jonathan Gruber, Joseph P Newhouse, Heidi Allen, Katherine Baicker, and Oregon Health Study Group. 2012. "The Oregon Health Insurance Experiment: Evidence from the First Year." *The Quarterly journal of economics*, 127(3): 1057–1106.

Frean, Molly, Jonathan Gruber, and Benjamin D Sommers. 2017. "Premium Subsidies, the Mandate, and Medicaid Expansion: Coverage Effects of the Affordable Care Act." *Journal of Health Economics*, 53 72–86.

Gallagher, Emily A, Radhakrishnan Gopalan, and Michal Grinstein-Weiss. 2019. "The Effect of Health Insurance on Home Payment Delinquency: Evidence from ACA Marketplace subsidies." *Journal of Public Economics*, 172 67–83.

Goldsmith-Pinkham, Paul, Isaac Sorkin, and Henry Swift. 2020. "Bartik Instruments: What, When, Why, and How." *American Economic Review*, 110(8): 2586–2624.

Goodman-Bacon, Andrew. 2016. "The Long-Run Effects of Childhood Insurance Coverage: Medicaid Implementation, Adult Health, and Labor Market Outcomes."Technical report, National Bureau of Economic Research working paper 22899.

Gross, Tal, and Matthew J Notowidigdo. 2011. "Health Insurance and the Consumer Bankruptcy Decision: Evidence from Expansions of Medicaid." *Journal of Public Economics*, 95(7-8): 767–778.

Grusky, David B, Bruce Western, and Christopher Wimer. 2011. *The Great Recession.*: Russell Sage Foundation.

Harding, John P, Eric Rosenblatt, and Vincent W Yao. 2009. "The Contagion Effect of Foreclosed Properties." *Journal of Urban Economics*, 66(3): 164–178.

Haushofer, Johannes, and Ernst Fehr. 2014. "On the Psychology of Poverty." *Science*, 344(6186): 862–867.

Hinde, Jesse M. 2017. “Incentive (Less)? the Effectiveness of Tax Credits and Cost-Sharing Subsidies in the Affordable Care Act.” *American Journal of Health Economics*, 3(3): 346–369.

Hsu, Joanne W, David A Matsa, and Brian T Melzer. 2018. “Unemployment Insurance as a Housing Market Stabilizer.” *American Economic Review*, 108(1): 49–81.

Hu, Luojia, Robert Kaestner, Bhashkar Mazumder, Sarah Miller, and Ashley Wong. 2016. “The Effect of the Patient Protection and Affordable Care Act Medicaid Expansions on Financial Wellbeing.”Technical report, NBER Working Paper 22170.

Immergluck, Dan, and Geoff Smith. 2006. “The External Costs of Foreclosure: The Impact of Single-Family Mortgage Foreclosures on Property Values.” *Housing Policy Debate*, 17(1): 57–79.

Jiménez, Dalié. 2009. “The Distribution of Assets in Consumer Chapter 7 Bankruptcy Cases.” *American Bankruptcy Law Journal*, 83, p. 795.

Kuka, Elira, Na’ama Shenhav, and Kevin Shih. 2020. “Do Human Capital Decisions Respond to the Returns to Education? Evidence from DACA.” *American Economic Journal: Economic Policy*, 12(1): 293–324.

Larrimore, Jeff, Jacob Mortenson, and David Splinter. 2019. “Household Incomes in Tax Data: Using Addresses to Move from Tax Unit to Household Income Distributions.” *Journal of Human Resources* 0718–9647R1.

Larrimore, Jeff, and David Splinter. 2019. “How Much Does Health Insurance Cost? Comparison of Premiums in Administrative and Survey Data.” *Economics Letters*, 174 132–135.

Lee, Jin Young, and Gary Solon. 2011. “The Fragility of Estimated Effects of Unilateral Divorce laws on Divorce Rates.” *The BE Journal of Economic Analysis & Policy*, 11(1): .

Li, Wenli. 2007. “What Do We Know About Chapter 13 Personal Bankruptcy Filings?” *Business Review*(Q4): .

Mahoney, Neale. 2015. “Bankruptcy as Implicit Health Insurance.” *American Economic Review*, 105(2): 710–46.

Moreno, Ana. 1995. “The Cost-Effectiveness of Mortgage Foreclosure Prevention.” *Minneapolis: Family Housing Fund*.

Newhouse, Exp. Group, Joseph P. 1993. *Free for All?: Lessons from the RAND Health Insurance Experiment*.: Harvard University Press.

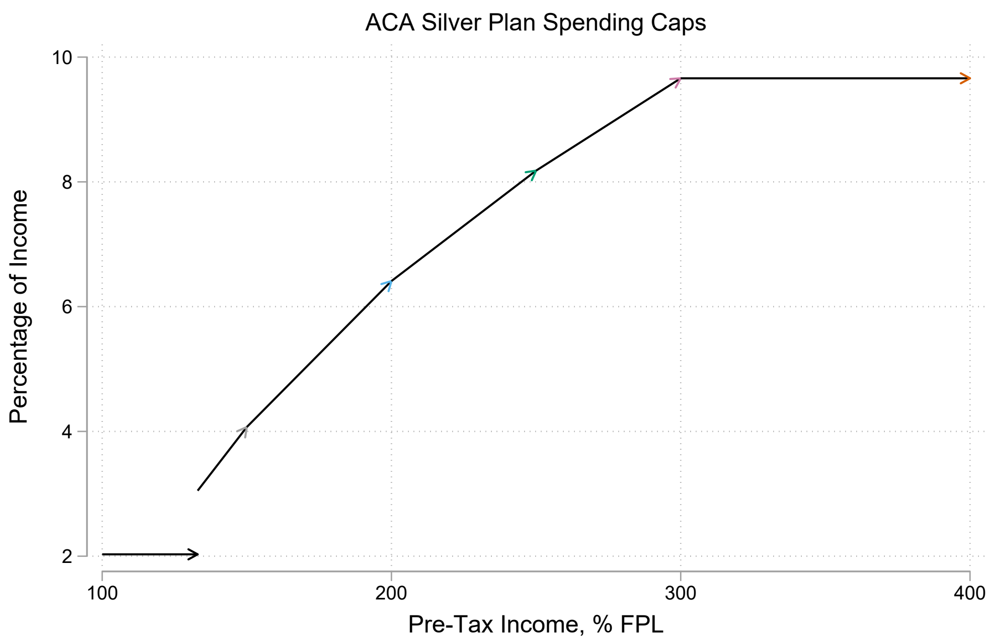
Norberg, Scott F, and Andrew J Velkey. 2005. “Debtor Discharge and Creditor Repayment in Chapter 13.” *Creighton Law Review*, 39, p. 473.

Smeeding, Timothy, Jeffrey P Thompson, Asaf Levanon, Burak Ho, and Beste Esra. 2011. “Poverty and Income Inequality in the Early Stages of the Great Recession.”

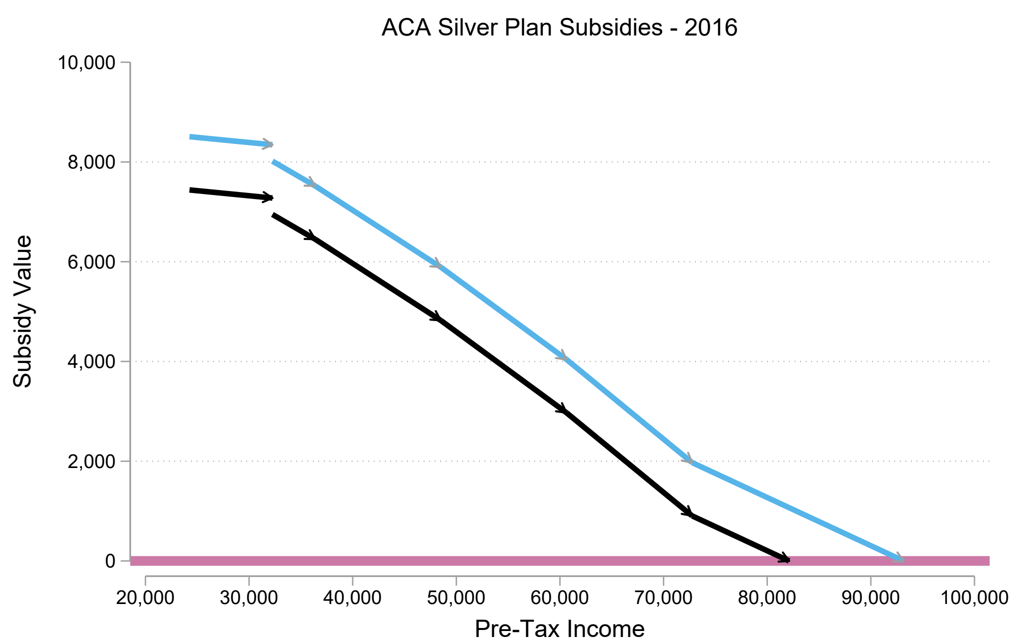
White, Michelle J. 2007. “Bankruptcy Reform and Credit Cards.” *Journal of Economic Perspectives*, 21(4): 175–200.

Figure 1: ACA Legislated Spending Caps and Example Subsidy Payments for a Family of 4 in a Medicaid Non-Expansion State Facing Benchmark Premiums of $7,932 (Black) or $9,000 (Blue)

Panel A

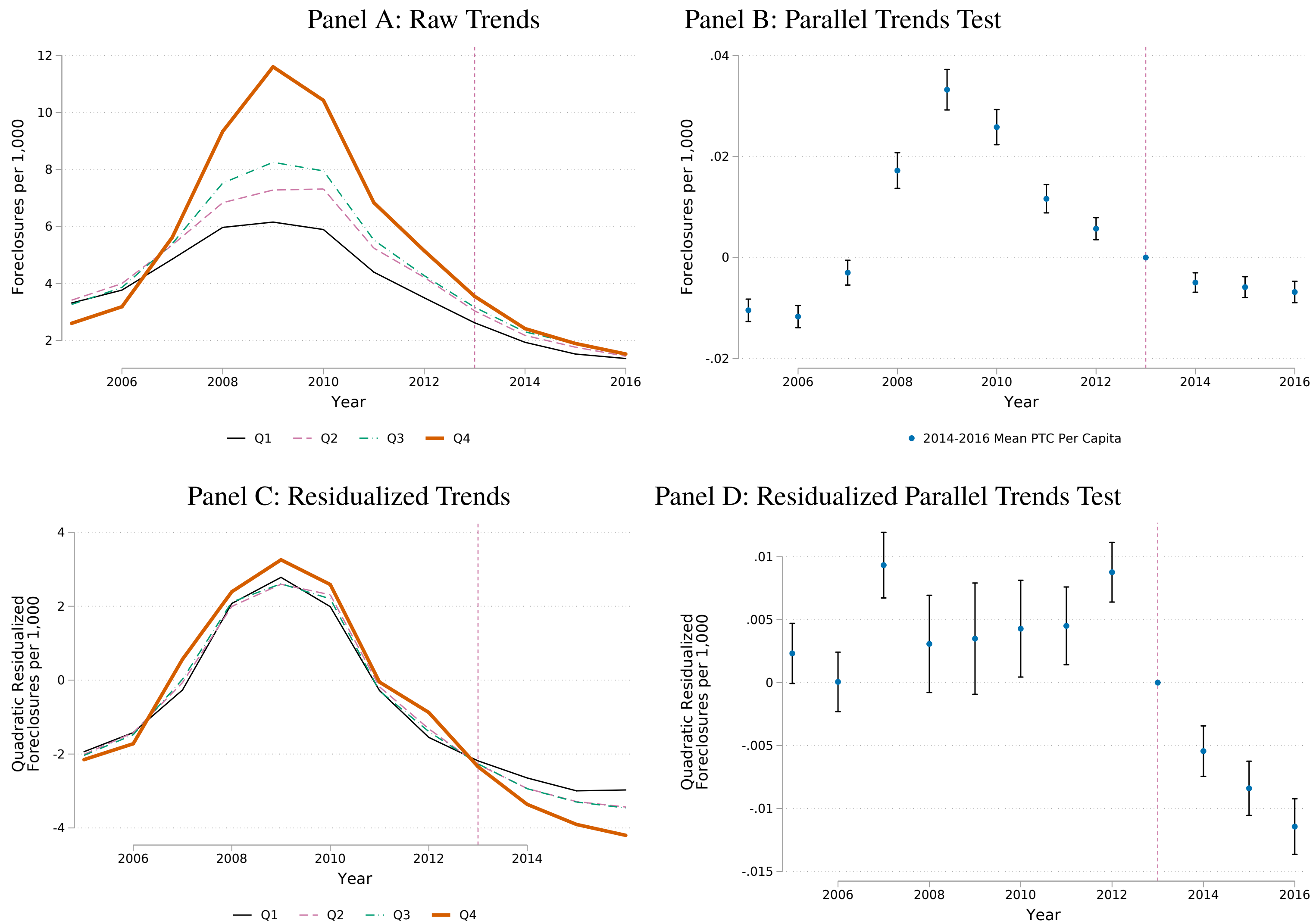


Panel B



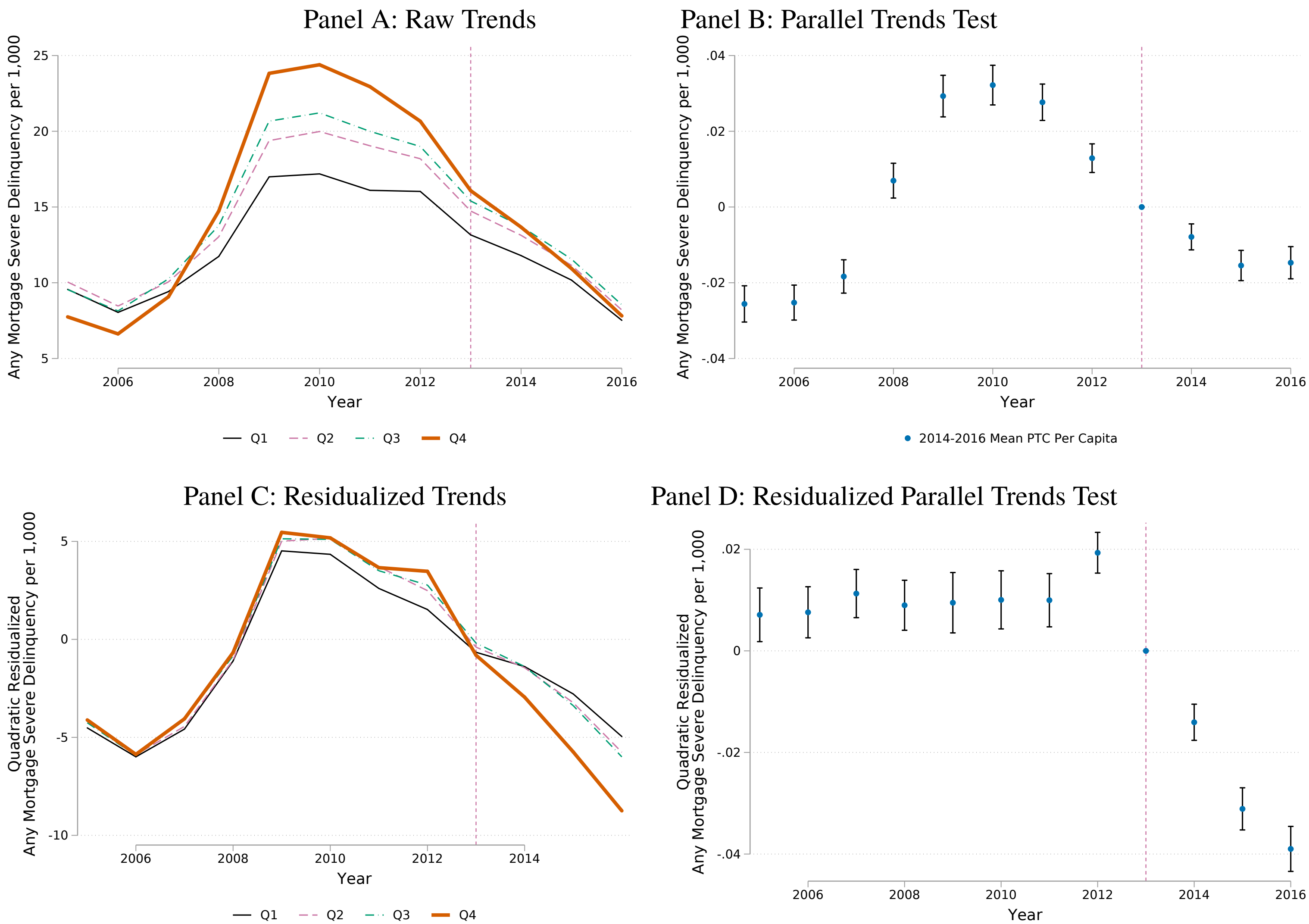
Source: Author's example calculations of legislated income limits and subsidies in the Affordable Care Act.
Note: Pre-tax income here is Modified Adjusted Gross Income (MAGI). Spending limits and the premium for the "second lowest-cost Silver plan" are from 2016. Arrows mark kinks or discontinuities in the subsidy schedule. The jump at 138% FPL is due to a large level shift in spending limits in Medicaid non-expansion states.

Figure 2: Foreclosure Rate Trends and Parallel Trends Test



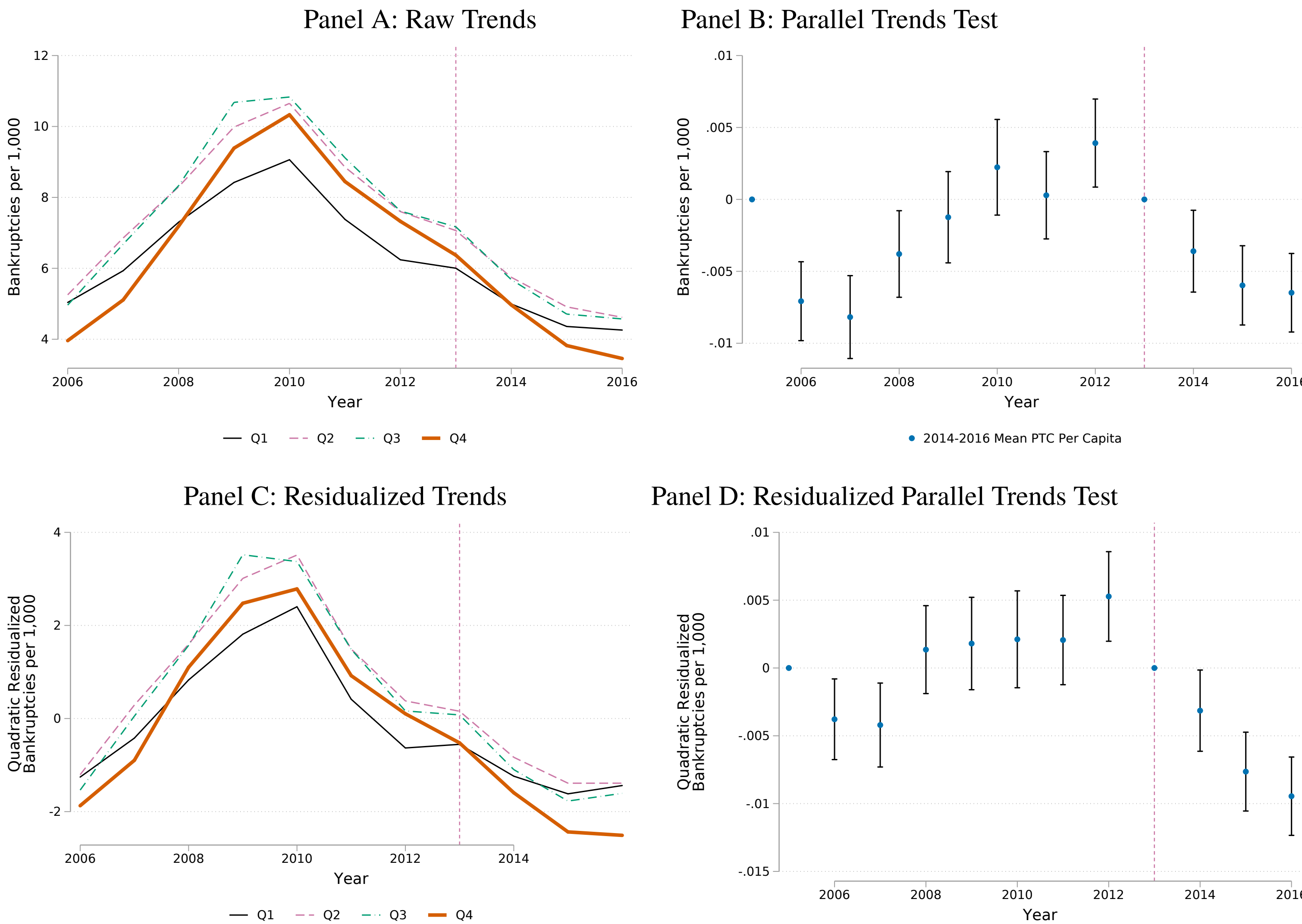
Source: Author's calculations based on Federal Reserve Bank of New York/Equifax Consumer Credit Panel data for those age 18-64 and IRS Statistics of Income data at the ZIP code level.
Note: Q1-Q4 are quartiles of average PTC per capita from 2014-2016 (Panels A and C). The parallel trends test coefficients come from Equation 2. The residualized outcomes are the predicted residuals from Equation 3. Standard errors are clustered at the ZIP code level.

Figure 3: Severe Mortgage Delinquency Rate Trends and Parallel Trends Test



Source: Author's calculations based on Federal Reserve Bank of New York/Equifax Consumer Credit Panel data for those age 18-64 and IRS Statistics of Income data at the ZIP code level.
Note: Q1-Q4 are quartiles of average PTC per capita from 2014-2016 (Panels A and C). The parallel trends test coefficients come from Equation 2. The residualized outcomes are the predicted residuals from Equation 3. Standard errors are clustered at the ZIP code level.

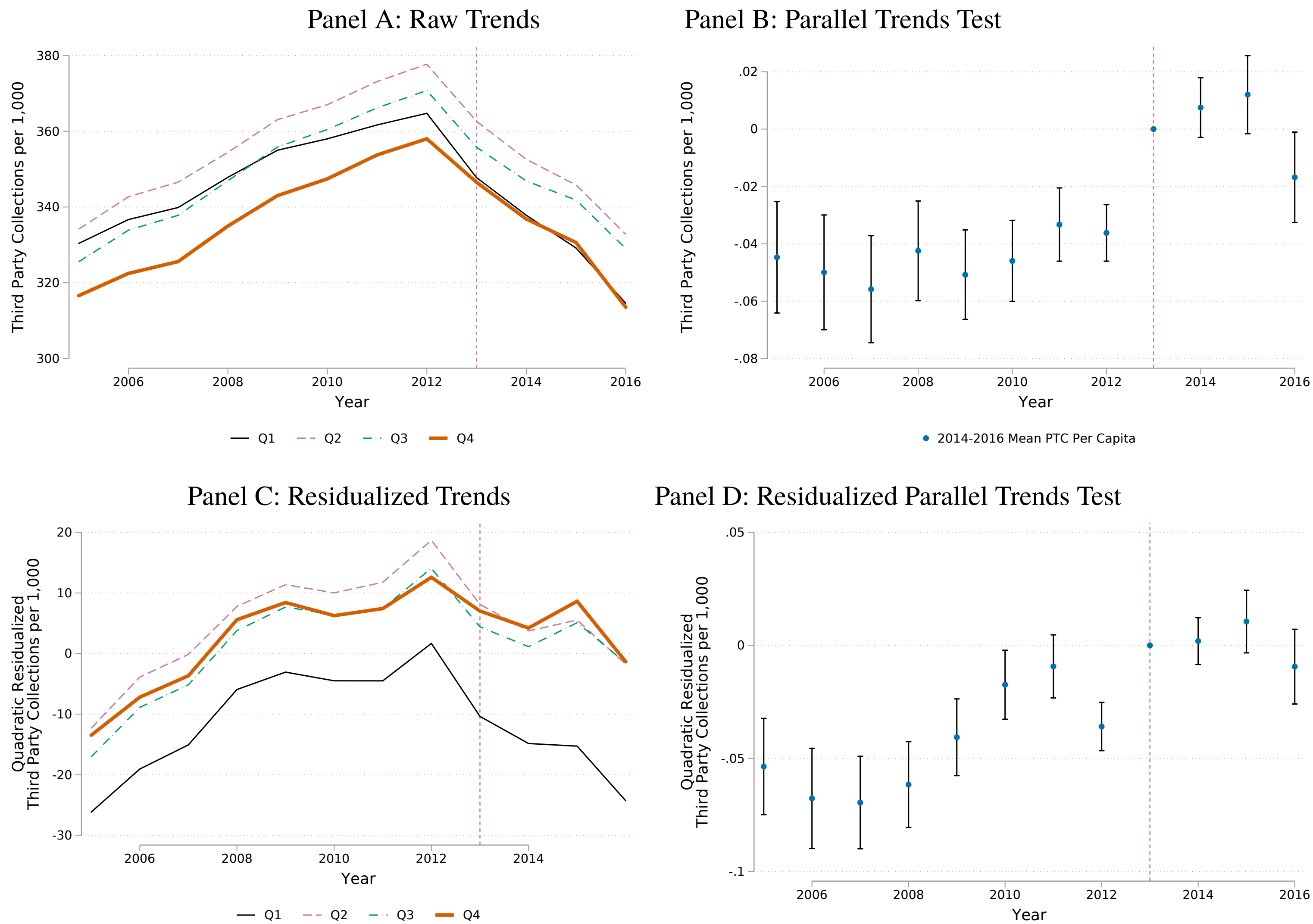
Figure 4: Bankruptcy Rate Trends and Parallel Trends Test



Source: Author's calculations based on Federal Reserve Bank of New York/Equifax Consumer Credit Panel data for those age 18-64 and IRS Statistics of Income data at the ZIP code level.

Note: Q1-Q4 are quartiles of average PTC per capita from 2014-2016 (Panels A and C). The parallel trends test coefficients come from Equation 2. The residualized outcomes are the predicted residuals from Equation 3. Standard errors are clustered at the ZIP code level.

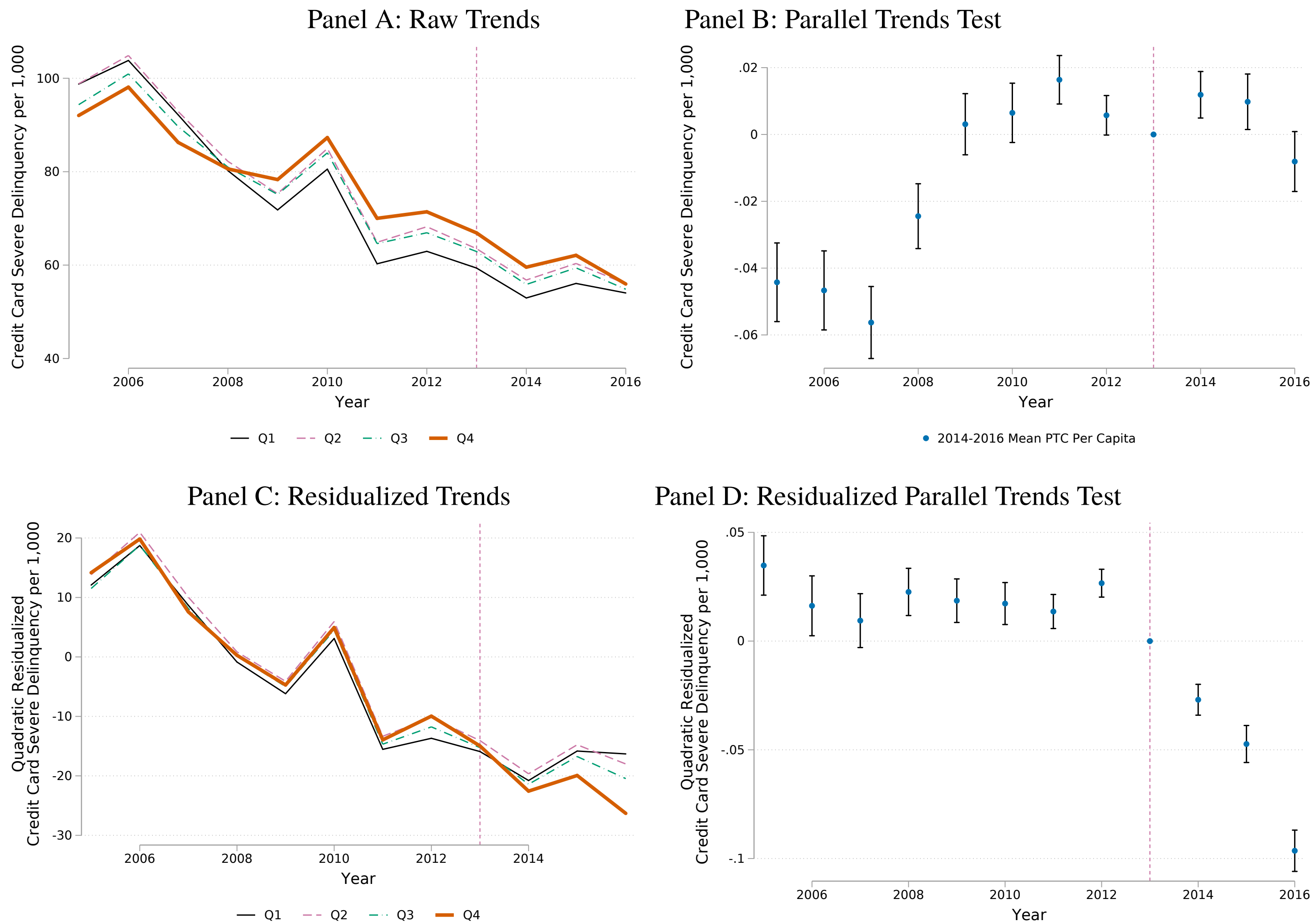
Figure 5: Third-Party Collections Rate Trends and Parallel Trends Test



Source: Author's calculations based on Federal Reserve Bank of New York/Equifax Consumer Credit Panel data for those age 18-64 and IRS Statistics of Income data at the ZIP code level.

Note: Q1-Q4 are quartiles of average PTC per capita from 2014-2016 (Panels A and C). The parallel trends test coefficients come from Equation 2. The residualized outcomes are the predicted residuals from Equation 3. Standard errors are clustered at the ZIP code level.

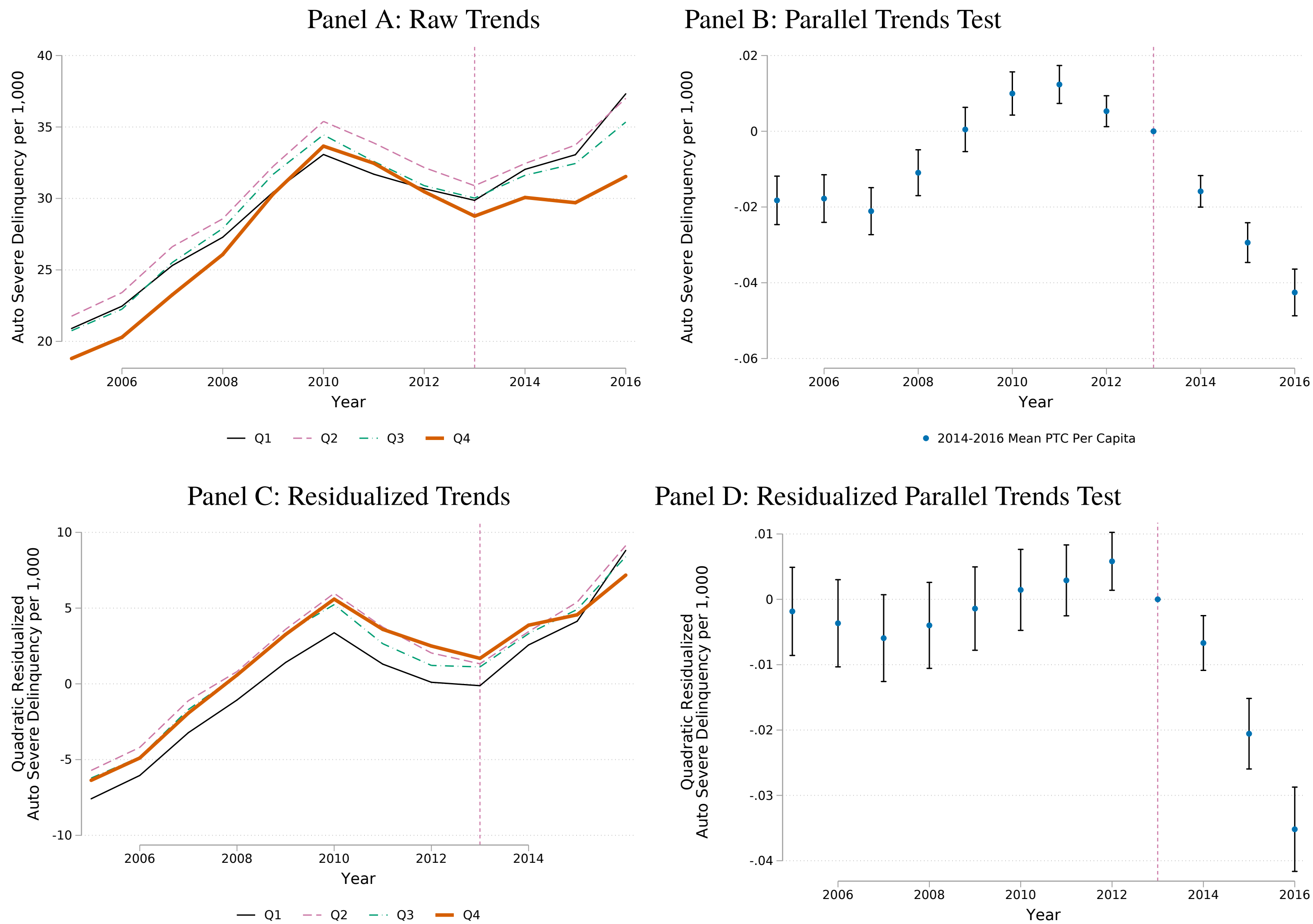
Figure 6: Severe Credit Card Delinquency Rate Trends and Parallel Trends Test



Source: Author's calculations based on Federal Reserve Bank of New York/Equifax Consumer Credit Panel data for those age 18-64 and IRS Statistics of Income data at the ZIP code level.

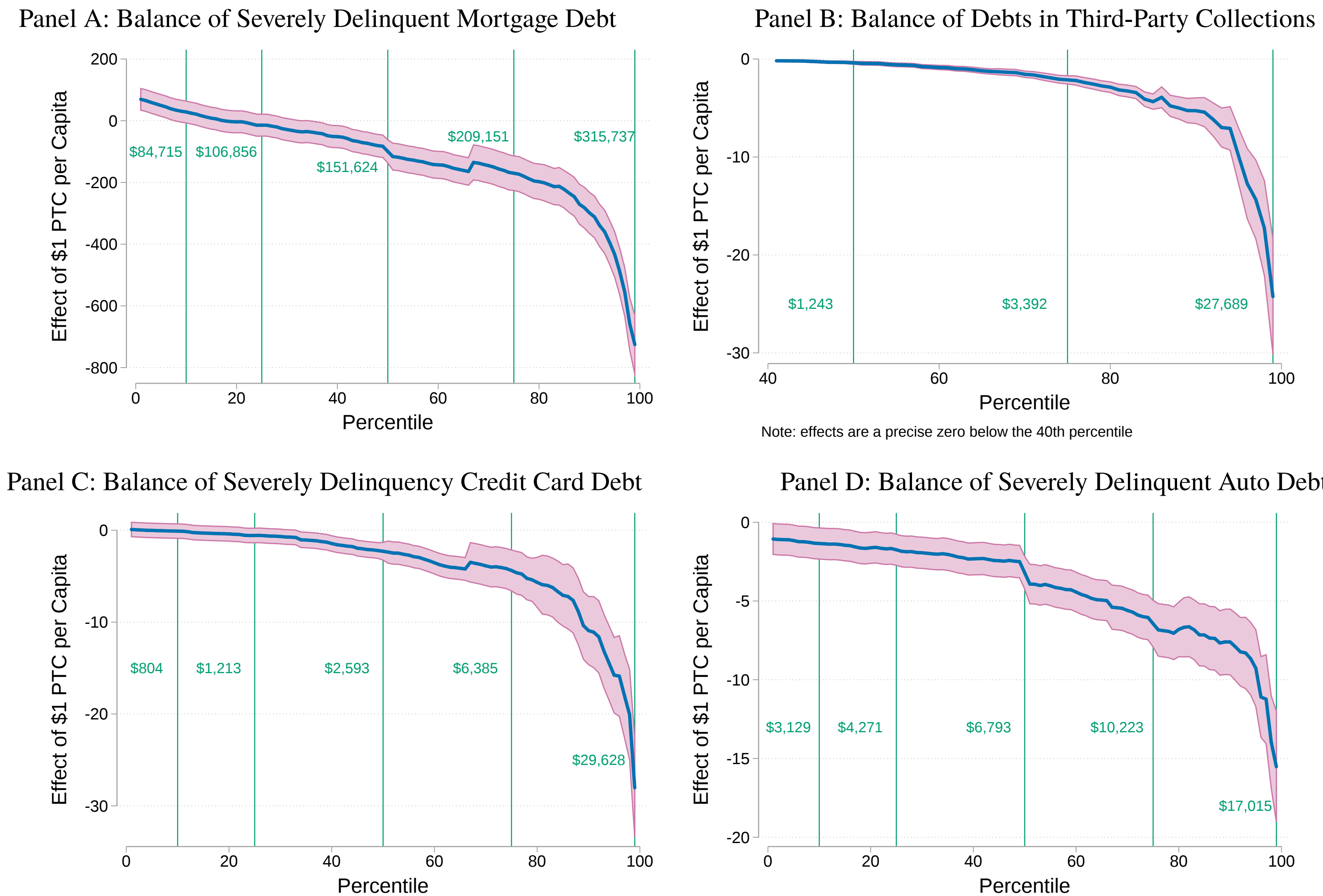
Note: Q1-Q4 are quartiles of average PTC per capita from 2014-2016 (Panels A and C). The parallel trends test coefficients come from Equation 2. The residualized outcomes are the predicted residuals from Equation 3. Standard errors are clustered at the ZIP code level.

Figure 7: Severe Auto Delinquency Rate Trends and Parallel Trends Test



Source: Author's calculations based on Federal Reserve Bank of New York/Equifax Consumer Credit Panel data for those age 18-64 and IRS Statistics of Income data at the ZIP code level.
Note: Q1-Q4 are quartiles of average PTC per capita from 2014-2016 (Panels A and C). The parallel trends test coefficients come from Equation 2. The residualized outcomes are the predicted residuals from Equation 3. Standard errors are clustered at the ZIP code level.

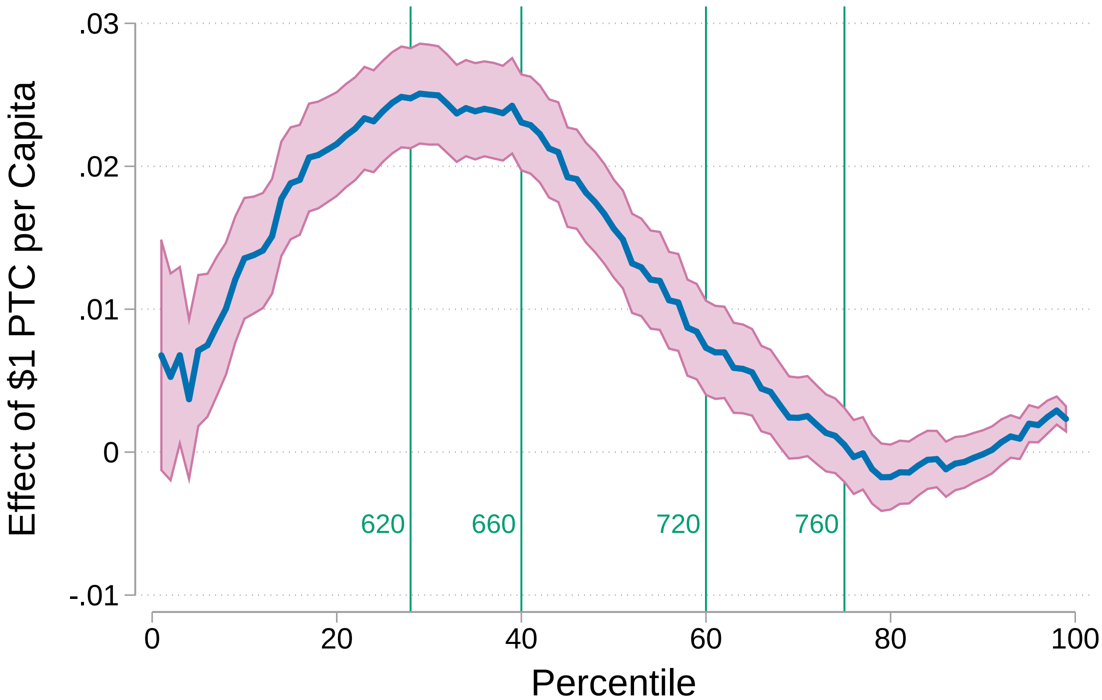
Figure 8: Distributional Effects on Negative Financial Outcomes



Source: Author's calculations based on Federal Reserve Bank of New York/Equifax Consumer Credit Panel data for those age 18-64 and IRS Statistics of Income data at the ZIP code level.

Note: Coefficients are for separate regressions for the nth percentile of the within-ZIP code distribution of each outcome conditional on having a positive balance. Estimates correspond to the residualized outcomes from Equation 3. The vertical bars correspond to the mean values of 10th, 25th, 50th, 75th, and 99th percentiles of the distribution across ZIP codes.

Figure 9: Distributional Effects: Credit Score (Equifax Risk Score)



Source: Author's calculations based on Federal Reserve Bank of New York/Equifax Consumer Credit Panel data for those age 18-64 and IRS Statistics of Income data at the ZIP code level.
Note: Coefficients are for separate regressions for the nth percentile of the within-ZIP code distribution of each outcome conditional on having a positive balance. Estimates correspond to the residualized outcomes from Equation 3. The vertical bars correspond to the mean values of 10th, 25th, 50th, 75th, and 99th percentiles of the distribution across ZIP codes.

Tables

Table 1: Summary Statistics of Key Analysis Variables

Treatment Variables	Mean	SD
PTC Per Capita (Age<65)	58.86	59.42
Average Total PTC per Year in Sample (2014-2016)	15.5 billion	
2005-2016 Outcomes	Mean	SD
Total Credit Files	7898.42	8280.67
Third-Party Collections per 1,000	345.35	151.97
Credit Card Severe Delinquency per 1,000	74.59	38.57
Auto Severe Delinquency per 1,000	29.44	23.56
Any Mortgage Severe Delinquency per 1,000	13.82	13.91
Foreclosures per 1,000	4.51	7.18
Bankruptcies per 1,000	6.24	8.23
Mean Credit Score (Equifax Risk Score)	677.62	36.78
Mean Amount in Third-Party Collections	3042.08	1987.07
Mean Amount of Severe Derogatory CC Debt	4981.94	5047.07
Mean Amount of Severe Derogatory Auto Debt	7717.55	4691.78
Mean Amount of Severe Derogatory Mortgage Debt	159820.30	158351.50
AHRF Supplier Variables		
# Primary Care Physicians	465.04	1012.72
# OBGYN Specialists	80.94	182.91
# Physician Assistants	158.26	308.04
# Nurse Practitioners	236.04	440.12
# Clinical Nurse Specialists	10.80	24.36
Number of ZIP Codes	20,625	

Source: Author's calculations based on Federal Reserve Bank of New York/Equifax Consumer Credit Panel data for those age 18-64, IRS SOI data, AHRF data, and ACS data at the ZIP code level.
Note: Analysis sample is described in Section 3. Debt amounts are conditional on having any positive debt amount.

Table 2: Results for Catastrophic Outcomes

	Panel A				
	(1)	(2)	(3)	(4)	(5)
VARIABLES	Severe Mortgage Delinquency Rate per 1,000				
PTC Per Capita	-0.0147***	-0.0135***	-0.0303***	-0.0314***	-0.0244***
	(0.00136)	(0.00131)	(0.00130)	(0.00132)	(0.00901)
Observations	247,500	247,500	247,500	243,078	243,078
Census + AHRF		X	X	X	X
Residualized Outcome			X	X	
Excluding Missing 2014 Data				X	X
IV					X
Dep. Mean	13.82	13.82	13.82	13.82	13.82
Effect at Treatment Mean (58.86)	-0.87	-0.79	-1.78	-1.85	-1.44
Pct Effect at Treatment Mean	-6%	-6%	-13%	-13%	-10%

	Panel B				
	(1)	(2)	(3)	(4)	(5)
VARIABLES	Foreclosure Rate per 1,000				
PTC Per Capita	-0.0118***	-0.0108***	-0.0100***	-0.0106***	-0.0139***
	(0.000719)	(0.000684)	(0.000686)	(0.000697)	(0.00406)
Observations	247,500	247,500	247,500	243,078	243,078
Census + AHRF		X	X	X	X
Residualized Outcome			X	X	
Excluding Missing 2014 Data				X	X
IV					X
Dep. Mean	4.51	4.51	4.51	4.51	4.51
Effect at Treatment Mean (58.86)	-0.69	-0.64	-0.59	-0.62	-0.82
Pct Effect at Treatment Mean	-15%	-14%	-13%	-14%	-18%

	Panel C				
	(1)	(2)	(3)	(4)	(5)
VARIABLES	Bankruptcy Rate per 1,000				
PTC Per Capita	-0.00347***	-0.00333***	-0.00644***	-0.00607***	-0.0150***
	(0.000612)	(0.000630)	(0.000629)	(0.000655)	(0.00463)
Observations	226,875	226,875	226,875	222,453	222,453
Census + AHRF		X	X	X	X
Residualized Outcome			X	X	
Excluding Missing 2014 Data				X	X
IV					X
Dep. Mean	6.24	6.24	6.24	6.24	6.24
Effect at Treatment Mean (58.86)	-0.20	-0.20	-0.38	-0.36	-0.88
Pct Effect at Treatment Mean	-3%	-3%	-6%	-6%	-14%

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Source: Author's calculations based on Federal Reserve Bank of New York/Equifax Consumer Credit Panel data for those age 18-64 and IRS SOI data. Estimates for Columns 1-2 are from the Equation 1, Column 3 from Equation 1 with the residualized, pre-trend corrected outcome from the two-step approach in Section 4, Column 4 excludes all ZIP codes with missing state-based insurance exchange data for 2014, and Column 5 from Equation 5.

Table 3: Results for Third-Party Collections and Other Debts

	Panel A				
	(1)	(2)	(3)	(4)	(5)
VARIABLES	Third-Party Collections Rate per 1,000				
PTC Per Capita	0.0347***	0.0286***	0.0300***	0.0309***	0.0120
	(0.00592)	(0.00583)	(0.00583)	(0.00589)	(0.0391)
Observations	247,500	247,500	247,500	243,078	243,078
Census + AHRF		X	X	X	X
Residualized Outcome			X	X	
Excluding States Missing 2014 Exchange Data				X	X
IV					X
Dep. Mean	345.35	345.35	345.35	345.35	345.35
Effect at Treatment Mean (58.86)	2.04	1.68	1.77	1.82	0.71
Pct Effect at Treatment Mean	1%	0%	1%	1%	0%

	Panel B				
	(1)	(2)	(3)	(4)	(5)
VARIABLES	Severe Credit Card Delinquency Rate per 1,000				
PTC Per Capita	0.0183***	0.0131***	-0.0663***	-0.0679***	-0.121***
	(0.00303)	(0.00302)	(0.00307)	(0.00309)	(0.0221)
Observations	247,500	247,500	247,500	243,078	243,078
Census + AHRF		X	X	X	X
Residualized Outcome			X	X	
Excluding States Missing 2014 Exchange Data				X	X
IV					X
Dep. Mean	74.59	74.59	74.59	74.59	74.59
Effect at Treatment Mean (58.86)	1.08	0.77	-3.90	-4.00	-7.12
Pct Effect at Treatment Mean	1%	1%	-5%	-5%	-10%

	Panel C				
	(1)	(2)	(3)	(4)	(5)
VARIABLES	Severe Auto Delinquency Rate per 1,000				
PTC Per Capita	-0.0245***	-0.0230***	-0.0174***	-0.0178***	0.00625
	(0.00197)	(0.00199)	(0.00199)	(0.00201)	(0.0134)
Observations	247,500	247,500	247,500	243,078	243,078
Census + AHRF		X	X	X	X
Residualized Outcome			X	X	
Excluding States Missing 2014 Exchange Data				X	X
IV					X
Dep. Mean	29.44	29.44	29.44	29.44	29.44
Effect at Treatment Mean (58.86)	-1.44	-1.35	-1.02	-1.05	0.37
Pct Effect at Treatment Mean	-5%	-5%	-3%	-4%	1%

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Source: Author's calculations based on Federal Reserve Bank of New York/Equifax Consumer Credit Panel data for those age 18-64 and IRS SOI data. Estimates for Columns 1-2 are from the Equation 1, Column 3 from Equation 1 with the residualized, pre-trend corrected outcome from the two-step approach in Section 4, Column 4 excludes all ZIP codes with missing state-based insurance exchange data for 2014, and Column 5 from Equation 5.

Table 4: Results for Catastrophic Outcomes by Credit Score (Equifax Risk Score)

Panel A

	Severe Mortgage Delinquency Rate per 1,000				
VARIABLES	<550	550-649	650-699	700-749	750-799
PTC Per Capita	-0.124***	-0.0540***	-0.0146***	0.000883	0.000228*
	(0.00897)	(0.00349)	(0.00213)	(0.000938)	(0.000129)
Observations	246,430	247,479	247,448	247,395	247,362
Dep. Mean	63.01	21.92	5.940	0.580	0.0100
Effect at Treatment Mean	-7.30	-3.18	-0.86	0.05	0.01
Pct Effect at Treatment Mean (58.86)	-12%	-15%	-14%	9%	134%

Panel B

	Foreclosure Rate per 1,000				
VARIABLES	<550	550-649	650-699	700-749	750-799
PTC Per Capita	-0.0471***	-0.0147***	-0.0105***	-0.00230***	-8.86e-06
	(0.00505)	(0.00214)	(0.000710)	(0.000185)	(1.79e-05)
Observations	246,430	247,479	247,448	247,395	247,362
Dep. Mean	23.50	6.310	1.170	0.150	0.000
Effect at Treatment Mean	-2.77	-0.87	-0.62	-0.14	0.00
Pct Effect at Treatment Mean (58.86)	-12%	-14%	-53%	-90%	0%

Panel C

	Bankruptcy Rate per 1,000				
VARIABLES	<550	550-649	650-699	700-749	750-799
PTC Per Capita	-0.000869	-0.0136***	-0.0170***	-0.000275	-3.89e-05
	(0.00288)	(0.00263)	(0.00149)	(0.000463)	(2.40e-05)
Observations	225,874	226,856	226,828	226,775	226,747
Dep. Mean	12.31	15.47	5.250	0.310	0.000
Effect at Treatment Mean	-0.05	-0.80	-1.00	-0.02	0.00
Pct Effect at Treatment Mean (58.86)	0%	-5%	-19%	-5%	0%

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Source: Author's calculations based on Federal Reserve Bank of New York/Equifax Consumer Credit Panel data for those age 18-64 and IRS SOI data. Estimates are from the Equation 1 using the residualized outcome, coinciding with Column 3 estimates from Tables 2 and 3.

Table 5: Results for Third-Party Collections and Other Debts by Credit Score (Equifax Risk Score)

Panel A

VARIABLES	Third-Party Collections Rate per 1,000 <550	550-649	650-699	700-749	750-799
PTC Per Capita	0.0695***	0.110***	0.108***	0.0132	0.0257***
	(0.0195)	(0.0129)	(0.0140)	(0.01000)	(0.00578)
Observations	246,430	247,479	247,448	247,395	247,362
Dep. Mean	804.4	650.5	281.9	117.6	38.96
Effect at Treatment Mean	4.09	6.47	6.36	0.78	1.51
Pct Effect at Treatment Mean (58.86)	1%	1%	2%	1%	4%

Panel B

VARIABLES	Severe Credit Card Delinquency Rate per 1,000 <550	550-649	650-699	700-749	750-799
PTC Per Capita	-0.153***	-0.0800***	-0.0128**	0.00870***	0.000192
	(0.0183)	(0.00908)	(0.00524)	(0.00193)	(0.000160)
Observations	246,430	247,479	247,448	247,395	247,362
Dep. Mean	299.1	129.8	27.74	3.270	0.0500
Effect at Treatment Mean	-9.01	-4.71	-0.75	0.51	0.01
Pct Effect at Treatment Mean (58.86)	-3%	-4%	-3%	16%	23%

Panel C

VARIABLES	Severe Auto Delinquency Rate per 1,000 <550	550-649	650-699	700-749	750-799
PTC Per Capita	-0.0333**	-0.0235***	-0.000458	0.000402	0.000104
	(0.0132)	(0.00559)	(0.00256)	(0.000811)	(0.000103)
Observations	246,430	247,479	247,442	247,395	247,362
Dep. Mean	110.4	47.09	7.650	0.860	0.0200
Effect at Treatment Mean	-1.96	-1.38	-0.03	0.02	0.01
Pct Effect at Treatment Mean (58.86)	-2%	-3%	0%	3%	31%

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Source: Author's calculations based on Federal Reserve Bank of New York/Equifax Consumer Credit Panel data for those age 18-64 and IRS SOI data. Estimates are from the Equation 1 using the residualized outcome, coinciding with Column 3 estimates from Tables 2 and 3.

Table 6: Consumer Welfare Gains Based on Change in Risk Premium ($\Delta\pi$) by Consumption Income and CRRA Parameter

Panel A: Excluding Foreclosure and Bankruptcy

	CRRA Parameter		
Consumption Assumption	2	3	4
$14,000	86	120	127
$16,000	82	111	113
$18,000	79	104	104
$20,000	77	100	98

Panel B: Including Bankruptcy and Foreclosure Risk

	CRRA Parameter		
Consumption Assumption	2	3	4
$14,000	392	721	1012
$16,000	347	500	652
$18,000	334	417	483
$20,000	334	378	399

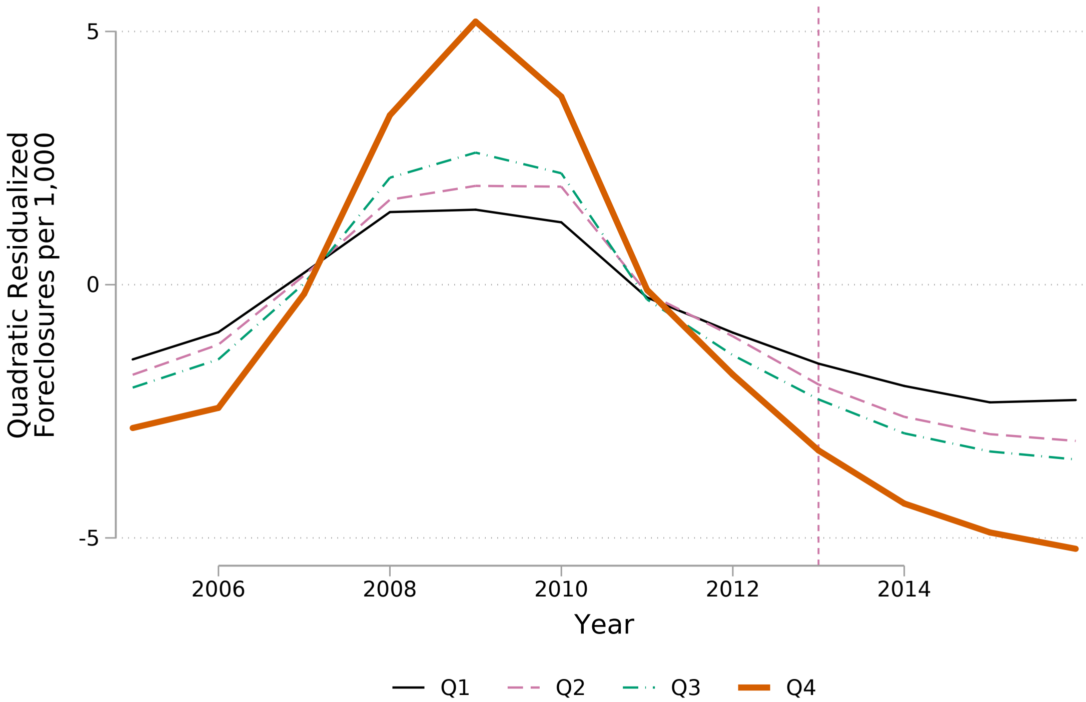
Panel C: Program Costs vs Estimated Benefits, Including Bankruptcy and Foreclosure

Annual Cost Per Recipient (HHS Range)	[3,168; 3,528]
Individual Insurance Value (Est. Range)	[334; 1012]
Share of Cost Realized in Risk Protections	[9.5%;32%]
Aggregate Annual Cost (Sample Area, Billions)	15.5
Aggregate Insurance Value (Sample Area, Billions)	[1.8; 5.46]

Source: Author's calculations based on Federal Reserve Bank of New York/Equifax Consumer Credit Panel data for those age 18-64 and IRS SOI data. Estimates are from the change in risk premium from 2013 baseline distributions from the causal estimates from Equation 1 using the residualized, pre-trend corrected specifications. Assumed consumption income levels come from the Consumer Expenditure Survey and Finkelstein et al. (2015). Calculations are based on expected utility framework in Equations 6 and 7. Costs per recipient are estimated from HHS reports. Aggregate welfare gains in my sample come from multiplying the number of tax returns receiving tax credits by the average non-dependent exemptions claimed on returns (1.33) and the range of insurance value estimates [334,1012].

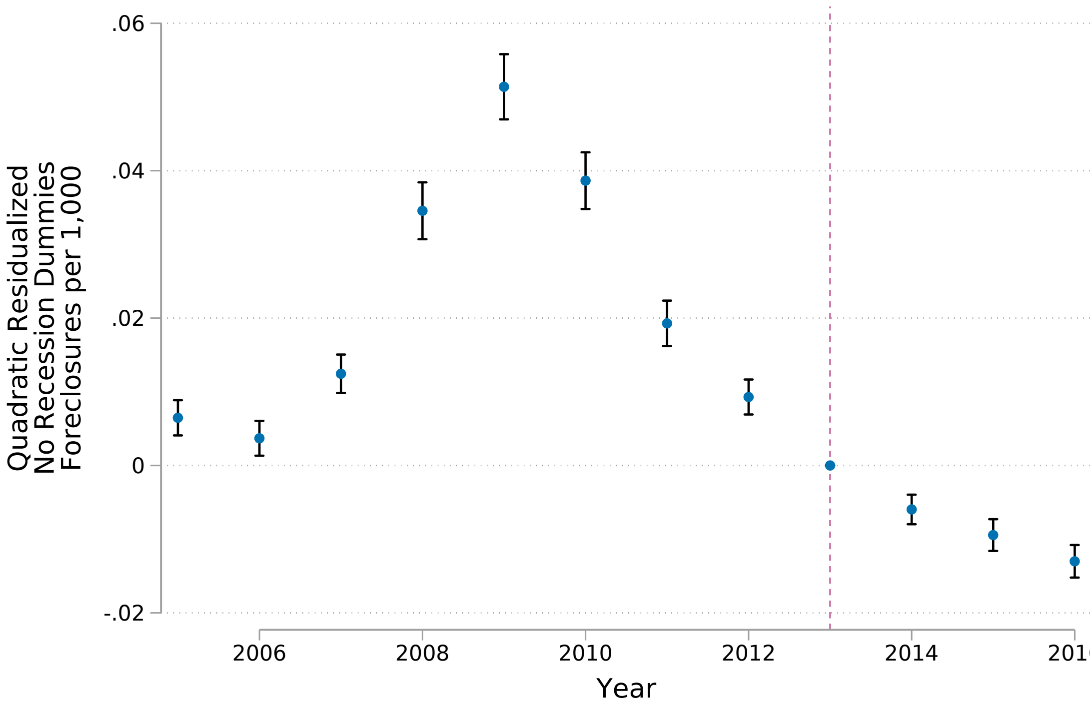
A Figures and Tables Appendix (Not For Publication)

Figure A1: Residualized Foreclosure Rate by Quartile of 2014-2016 PTC:
Quadratic Trend without Peak Recession Indicators



Source: Author's calculations based on Federal Reserve Bank of New York/Equifax Consumer Credit Panel data for those age 18-64 and IRS Statistics of Income data at the ZIP code level.
Note: Q1-Q4 are quartiles of average PTC per capita from 2014-2016 (Panels A and C). The residualized outcomes are the predicted residuals from Equation 3 without the interactions with peak recession years (α). See Figure 2 for the result of including these indicators for peak recession years.

Figure A2: Residualized Foreclosure Rate Parallel Trends Test



Source: Author's calculations based on Federal Reserve Bank of New York/Equifax Consumer Credit Panel data for those age 18-64 and IRS Statistics of Income data at the ZIP code level.
Note: The residualized outcomes are the predicted residuals from Equation 3 without the interactions with peak recession years (α_t). See Figure 2 for the result of including these indicators for peak recession years. Standard errors are clustered at the ZIP code level.

Table A1: First Stage Results from Two-Stage Least Squares Estimate

VARIABLES	(1) Actual PTC Per Capita
Simulated PTC Per Capita	0.323***
	(0.0148)
Medicaid Expansion	-21.82***
	(0.876)
Benchmark Silver Plan Premium (30 Yr Old Adult)	-0.212***
	(0.0148)
Number of Issuers on Local Exchange	-2.184***
	(0.107)
Statewide Mean Silver Plan Premium (30 Yr Old Adult)	-0.0319***
	(0.00666)
Constant	60.93***
	(6.180)
Observations	243,078
R-squared	0.677
K-P Wald F statistic	475.1

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Source: Author's calculations of QHP landscape files for the second lowest-cost Silver plan, the 2013 CPS ASEC supplement, ACS/Census, and the Area Health Resource File.

Note: Results are from the first stage regression of actual PTC per person on simulated PTC per person. Standard errors are clustered at the ZIP code level. The Kleibergen-Paap rk Wald statistic tests for weak identification when errors are not homoskedastic i.i.d. The value indicates strong first-stage performance.

Table A2: Results Including ZIP-Specific Quadratic Time Trends

Panel A

VARIABLES	(1) Severe Mortgage Delinquency Rate per 1,000	(2) Foreclosure Rate per 1,000	(3) Bankruptcy Rate per 1,000
PTC Per Capita	-0.0404***	-0.0187***	-0.00815***
	(0.00246)	(0.00103)	(0.00112)
Observations	247,500	247,500	226,875
Dep. Mean	13.82	4.51	6.24
Effect at Treatment Mean	-2.38	-1.10	-0.48
Treatment Mean (2014-2016)	58.86	58.86	58.86

Panel B

VARIABLES	(1) Third-Party Collections Rate per 1,000	(2) Severe Credit Card Delinquency Rate per 1,000	(3) Severe Auto Delinquency Rate per 1,000
PTC Per Capita	0.000309	-0.0613***	-0.0255***
	(0.00720)	(0.00450)	(0.00271)
Observations	247,500	247,500	247,500
Dep. Mean	345.35	74.59	29.44
Effect at Treatment Mean	0.02	-3.61	-1.50
Treatment Mean (2014-2016)	58.86	58.86	58.86

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Source: Author's calculations based on Federal Reserve Bank of New York/Equifax Consumer Credit Panel data for those age 18-64 and IRS SOI data. Estimates are from the Equation 1 adding ZIP-code specific quadratic time trends.

Table A3: Results Using Residualized Outcomes Excluding Peak Recession Years (2008-2011)

Panel A

	(1)	(2)	(3)
VARIABLES	Severe Mortgage Delinquency Rate/1,000	Foreclosure Rate/1,000	Bankruptcy Rate/1,000
PTC Per Capita	-0.0434***	-0.0204***	-0.00653***
	(0.00130)	(0.000677)	(0.000652)
Observations	247,500	247,500	226,875
Dep. Mean	13.8	4.5	6.8
Effect at Treatment Mean	-2.55	-1.20	-0.38
Treatment Mean (2014-2016)	58.86	58.86	58.86

Panel B

	(1)	(2)	(3)
VARIABLES	Third-Party Collections Rate/1,000	Severe Credit Card Delinquency Rate/1,000	Severe Auto Delinquency Rate/1,000
PTC Per Capita	0.0373***	-0.0833***	-0.0201***
	(0.00582)	(0.00309)	(0.00199)
Observations	247,500	247,500	247,500
Dep. Mean	345.4	74.59	29.44
Effect at Treatment Mean	2.20	-4.90	-1.18
Treatment Mean (2014-2016)	58.86	58.86	58.86

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Source: Author's calculations based on Federal Reserve Bank of New York/Equifax Consumer Credit Panel data for those age 18-64 and IRS SOI data. Estimates are from the Equation 1 using residualized outcomes from a quadratic time trend excluding peak recession years (2008-2011). Standard errors are clustered at the ZIP code level.

Table A4: Results Including State by Year Fixed Effects, Residualized Outcomes

Panel A

VARIABLES	(1) Severe Mortgage Delinquency Rate/1,000	(2) Foreclosure Rate/1,000	(3) Bankruptcy Rate/1,000
PTC Per Capita	-0.0147***	-0.000246	-0.00306***
	(0.00144)	(0.000767)	(0.000827)
Observations	247,500	247,500	226,875
Dep. Mean	13.8	4.5	6.8
Effect at Treatment Mean	-0.87	-0.01	-0.18
Treatment Mean (2014-2016)	58.86	58.86	58.86

Panel B

VARIABLES	(1) Third-Party Collections Rate/1,000	(2) Severe Credit Card Delinquency Rate/1,000	(3) Severe Auto Delinquency Rate/1,000
PTC Per Capita	0.0389***	-0.0659***	-0.00619**
	(0.00747)	(0.00393)	(0.00249)
Observations	247,500	247,500	247,500
Dep. Mean	345.4	74.59	29.44
Effect at Treatment Mean	2.29	-3.88	-0.36
Treatment Mean (2014-2016)	58.86	58.86	58.86

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Source: Author's calculations based on Federal Reserve Bank of New York/Equifax Consumer Credit Panel data for those age 18-64 and IRS SOI data. Estimates are from the Equation 1 with the residualized outcomes noted in Section 4.

Table A5: Results Using Individual Data Applying ZIP Code Treatment

	Panel A		
VARIABLES	(1) Pr(Severe Mortgage Delinquency)	(2) Pr(Foreclosure)	(3) Pr(Bankruptcy)
PTC Per Capita	-4.14e-05***	-2.37e-05***	-5.12e-06***
	(2.52e-06)	(1.43e-06)	(1.20e-06)
Observations	22,098,717	22,098,717	19,938,454
Dep. Mean	0.01523	0.00532	0.00694
Effect at Treatment Mean	-0.0024	-0.0014	-0.0003
Percent Effect at Treatment Mean	-16%	-26%	-4%
Treatment Mean (2014-2016)	58.86	58.86	58.86

	Panel B		
VARIABLES	(1) Pr(Third-Party Collections)	(2) Pr(Severe Credit Card Delinquency)	(3) Pr(Severe Auto Delinquency)
PTC Per Capita	7.51e-05***	1.74e-06	-2.03e-05***
	(6.13e-06)	(4.38e-06)	(2.74e-06)
Observations	22,098,717	22,098,717	22,098,717
Dep. Mean	0.33790	0.07915	0.02956
Effect at Treatment Mean	0.0044	0.0001	-0.0012
Percent Effect at Treatment Mean	1%	0%	-4%
Treatment Mean (2014-2016)	58.86	58.86	58.86

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Source: Author's calculations based on Federal Reserve Bank of New York/Equifax Consumer Credit Panel data for those age 18-64 and IRS SOI data. Estimates are from the Equation 28 using individual CCP data. Standard errors are clustered at the ZIP code level.

Table A6: Distributional Effects Regressions
Severely Delinquent Debts and Third-Party Collections

Percentile	Mortgage Debt Coef.	Mortgage Debt SE	Collections Coef	Collections SE	Credit Card Coef.	Credit Card SE	Auto Coef.	Auto SE
1	69.374	17.915	0.030	0.026	0.090	0.401	-1.062	0.501
2	65.355	17.934	0.031	0.026	0.053	0.401	-1.094	0.501
3	59.699	17.945	0.031	0.026	0.029	0.401	-1.105	0.502
4	54.756	17.972	0.030	0.026	-0.001	0.401	-1.112	0.502
5	49.620	17.992	0.028	0.026	-0.011	0.401	-1.161	0.502
6	44.829	18.014	0.031	0.027	-0.037	0.401	-1.233	0.503
7	38.578	18.043	0.030	0.027	-0.045	0.401	-1.239	0.503
8	34.229	18.072	0.026	0.027	-0.067	0.401	-1.274	0.503
9	31.013	18.076	0.028	0.027	-0.077	0.401	-1.330	0.504
10	28.393	18.054	0.029	0.027	-0.090	0.401	-1.349	0.504
11	24.625	18.073	0.027	0.028	-0.106	0.401	-1.368	0.505
12	21.343	18.074	0.028	0.028	-0.159	0.401	-1.392	0.505
13	16.221	18.092	0.030	0.028	-0.248	0.401	-1.386	0.505
14	12.059	18.122	0.030	0.028	-0.278	0.401	-1.412	0.506
15	8.316	18.144	0.028	0.028	-0.304	0.401	-1.460	0.506
16	5.843	18.182	0.028	0.029	-0.318	0.401	-1.486	0.506
17	1.326	18.155	0.026	0.029	-0.344	0.401	-1.567	0.506
18	-1.265	18.191	0.019	0.029	-0.358	0.401	-1.637	0.506
19	-2.919	18.189	0.015	0.030	-0.373	0.401	-1.659	0.506
20	-3.556	18.113	0.009	0.030	-0.397	0.401	-1.622	0.505
21	-3.347	18.115	0.010	0.031	-0.433	0.402	-1.592	0.506
22	-6.310	18.153	0.007	0.032	-0.456	0.402	-1.653	0.506
23	-10.422	18.163	-0.002	0.032	-0.543	0.402	-1.691	0.506
24	-14.408	18.197	-0.022	0.033	-0.569	0.402	-1.669	0.506

Table A6: Distributional Effects Regressions
Severely Delinquent Debts and Third-Party Collections

Percentile	Mortgage Debt Coef.	Mortgage Debt SE	Collections Coef	Collections SE	Credit Card Coef.	Credit Card SE	Auto Coef.	Auto SE
25	-14.297	18.063	-0.020	0.033	-0.562	0.403	-1.747	0.503
26	-14.395	18.157	-0.017	0.034	-0.555	0.407	-1.842	0.505
27	-17.593	18.183	-0.024	0.035	-0.591	0.407	-1.871	0.505
28	-20.264	18.198	-0.039	0.036	-0.629	0.407	-1.866	0.505
29	-25.454	18.235	-0.056	0.037	-0.640	0.407	-1.924	0.506
30	-28.426	18.263	-0.070	0.037	-0.676	0.407	-1.939	0.506
31	-31.349	18.302	-0.082	0.039	-0.729	0.407	-1.970	0.506
32	-34.344	18.332	-0.086	0.039	-0.746	0.407	-2.005	0.506
33	-36.006	18.347	-0.092	0.040	-0.781	0.408	-2.027	0.507
34	-35.046	18.165	-0.076	0.046	-1.036	0.431	-2.001	0.517
35	-37.100	18.181	-0.085	0.046	-1.059	0.431	-2.042	0.517
36	-39.802	18.197	-0.116	0.047	-1.098	0.431	-2.117	0.517
37	-41.962	18.210	-0.129	0.048	-1.140	0.431	-2.205	0.518
38	-48.217	18.267	-0.133	0.048	-1.226	0.431	-2.249	0.518
39	-50.888	18.298	-0.153	0.050	-1.292	0.431	-2.337	0.518
40	-51.553	18.256	-0.166	0.051	-1.440	0.431	-2.323	0.519
41	-53.281	18.294	-0.179	0.053	-1.572	0.433	-2.310	0.522
42	-57.654	18.302	-0.183	0.056	-1.641	0.433	-2.304	0.522
43	-64.340	18.357	-0.192	0.058	-1.725	0.435	-2.367	0.523
44	-66.895	18.372	-0.199	0.058	-1.771	0.435	-2.426	0.523
45	-71.147	18.407	-0.237	0.060	-1.951	0.435	-2.441	0.523
46	-73.199	18.425	-0.270	0.062	-2.016	0.436	-2.471	0.524
47	-77.305	18.460	-0.319	0.064	-2.098	0.436	-2.427	0.524
48	-80.517	18.491	-0.328	0.065	-2.138	0.436	-2.472	0.524

Table A6: Distributional Effects Regressions
Severely Delinquent Debts and Third-Party Collections

Percentile	Mortgage Debt Coef.	Mortgage Debt SE	Collections Coef	Collections SE	Credit Card Coef.	Credit Card SE	Auto Coef.	Auto SE
49	-82.631	18.515	-0.339	0.065	-2.205	0.436	-2.498	0.524
50	-99.389	19.161	-0.389	0.075	-2.285	0.492	-3.211	0.530
51	-116.219	22.186	-0.439	0.090	-2.377	0.617	-3.925	0.639
52	-118.047	22.193	-0.448	0.090	-2.480	0.618	-3.939	0.639
53	-121.454	22.209	-0.463	0.092	-2.490	0.618	-4.017	0.639
54	-125.315	22.217	-0.540	0.094	-2.614	0.618	-3.948	0.639
55	-127.448	22.228	-0.593	0.095	-2.707	0.618	-4.035	0.639
56	-130.862	22.240	-0.613	0.097	-2.878	0.619	-4.143	0.640
57	-133.214	22.245	-0.643	0.097	-2.953	0.619	-4.195	0.640
58	-137.904	22.310	-0.772	0.104	-3.142	0.621	-4.270	0.642
59	-141.792	22.326	-0.810	0.106	-3.325	0.622	-4.288	0.642
60	-142.992	22.358	-0.865	0.110	-3.536	0.621	-4.430	0.642
61	-143.932	22.516	-0.892	0.118	-3.746	0.628	-4.582	0.645
62	-148.335	22.532	-0.986	0.120	-3.897	0.629	-4.683	0.645
63	-153.956	22.588	-1.013	0.126	-4.017	0.630	-4.831	0.647
64	-157.421	22.639	-1.097	0.130	-4.062	0.631	-4.917	0.648
65	-160.889	22.669	-1.201	0.134	-4.133	0.632	-4.939	0.648
66	-164.476	22.678	-1.267	0.136	-4.212	0.633	-4.978	0.648
67	-135.016	28.850	-1.301	0.160	-3.486	1.097	-5.396	0.716
68	-137.416	28.858	-1.352	0.161	-3.603	1.097	-5.428	0.716
69	-141.731	28.855	-1.389	0.163	-3.717	1.098	-5.469	0.717
70	-145.530	28.852	-1.557	0.166	-3.870	1.098	-5.599	0.718
71	-150.072	28.859	-1.619	0.171	-4.004	1.100	-5.704	0.718
72	-156.218	29.048	-1.771	0.189	-3.970	1.111	-5.889	0.721

Table A6: Distributional Effects Regressions
Severely Delinquent Debts and Third-Party Collections

Percentile	Mortgage Debt Coef.	Mortgage Debt SE	Collections Coef	Collections SE	Credit Card Coef.	Credit Card SE	Auto Coef.	Auto SE
73	-160.821	29.066	-1.916	0.197	-4.055	1.112	-5.994	0.722
74	-167.262	29.088	-2.063	0.202	-4.171	1.113	-6.045	0.722
75	-170.119	28.576	-2.127	0.215	-4.375	1.135	-6.440	0.756
76	-173.126	28.939	-2.198	0.240	-4.623	1.187	-6.831	0.852
77	-180.197	28.925	-2.400	0.255	-4.756	1.187	-6.885	0.852
78	-188.029	29.027	-2.556	0.264	-5.232	1.191	-6.926	0.854
79	-195.142	29.067	-2.747	0.268	-5.391	1.193	-7.046	0.855
80	-197.387	29.195	-2.872	0.278	-5.670	1.391	-6.804	0.883
81	-201.019	29.898	-3.152	0.299	-5.922	1.630	-6.671	0.958
82	-207.186	29.985	-3.263	0.310	-5.999	1.635	-6.634	0.968
83	-213.429	29.998	-3.426	0.316	-6.229	1.636	-6.831	0.968
84	-212.559	31.045	-4.093	0.374	-6.667	1.700	-7.146	1.005
85	-222.450	31.060	-4.351	0.399	-7.075	1.702	-7.160	1.008
86	-234.819	32.026	-3.909	0.543	-7.210	1.806	-7.353	1.024
87	-246.294	32.042	-4.780	0.552	-7.629	1.808	-7.374	1.026
88	-270.493	33.012	-4.994	0.577	-8.819	1.847	-7.667	1.043
89	-281.878	33.809	-5.261	0.634	-10.369	1.872	-7.597	1.058
90	-298.601	34.010	-5.273	0.667	-10.920	1.890	-7.599	1.066
91	-311.765	34.532	-5.423	0.758	-11.089	1.973	-7.902	1.091
92	-339.153	34.984	-6.155	0.861	-11.596	2.003	-8.227	1.112
93	-359.424	35.817	-6.990	1.014	-13.149	2.042	-8.297	1.154
94	-394.425	36.312	-7.086	1.133	-14.458	2.065	-8.662	1.177
95	-433.191	37.651	-9.935	1.460	-15.779	2.097	-9.268	1.246
96	-488.625	38.644	-12.720	1.827	-15.870	2.238	-11.095	1.309

Table A6: Distributional Effects Regressions
Severely Delinquent Debts and Third-Party Collections

Percentile	Mortgage Debt Coef.	Mortgage Debt SE	Collections Coef	Collections SE	Credit Card Coef.	Credit Card SE	Auto Coef.	Auto SE
97	-554.975	40.221	-14.345	2.053	-18.001	2.337	-11.233	1.440
98	-659.200	43.497	-17.264	2.476	-20.070	2.514	-13.954	1.493
99	-725.367	48.710	-24.245	3.007	-28.033	2.749	-15.519	1.796

Note: Estimates correspond with those in Figure 8. Standard errors are clustered at the ZIP code level.

Table A7: Distributional Effects Regressions
Credit Score (Equifax Risk Score)

Percentile	Credit Score Coef.	Credit Score SE
1	0.007	0.004
2	0.005	0.004
3	0.007	0.003
4	0.004	0.003
5	0.007	0.003
6	0.007	0.003
7	0.009	0.002
8	0.010	0.002
9	0.012	0.002
10	0.014	0.002
11	0.014	0.002
12	0.014	0.002
13	0.015	0.002
14	0.018	0.002
15	0.019	0.002
16	0.019	0.002
17	0.021	0.002
18	0.021	0.002
19	0.021	0.002
20	0.022	0.002
21	0.022	0.002
22	0.023	0.002
23	0.023	0.002
24	0.023	0.002
25	0.024	0.002
26	0.024	0.002
27	0.025	0.002
28	0.025	0.002
29	0.025	0.002
30	0.025	0.002
31	0.025	0.002
32	0.024	0.002
33	0.024	0.002
34	0.024	0.002
35	0.024	0.002
36	0.024	0.002
37	0.024	0.002
38	0.024	0.002
39	0.024	0.002
40	0.023	0.002
41	0.023	0.002
42	0.022	0.002
43	0.021	0.002
44	0.021	0.002
45	0.019	0.002
46	0.019	0.002
47	0.018	0.002
48	0.017	0.002
49	0.017	0.002

Table A7: Distributional Effects Regressions
Credit Score (Equifax Risk Score)

Percentile	Credit Score Coef.	Credit Score SE
50	0.016	0.002
51	0.015	0.002
52	0.013	0.002
53	0.013	0.002
54	0.012	0.002
55	0.012	0.002
56	0.011	0.002
57	0.010	0.002
58	0.009	0.002
59	0.008	0.002
60	0.007	0.002
61	0.007	0.002
62	0.007	0.002
63	0.006	0.002
64	0.006	0.002
65	0.006	0.002
66	0.004	0.002
67	0.004	0.002
68	0.003	0.001
69	0.002	0.001
70	0.002	0.001
71	0.003	0.001
72	0.002	0.001
73	0.001	0.001
74	0.001	0.001
75	0.001	0.001
76	0.000	0.001
77	0.000	0.001
78	-0.001	0.001
79	-0.002	0.001
80	-0.002	0.001
81	-0.001	0.001
82	-0.001	0.001
83	-0.001	0.001
84	-0.001	0.001
85	0.000	0.001
86	-0.001	0.001
87	-0.001	0.001
88	-0.001	0.001
89	0.000	0.001
90	0.000	0.001
91	0.000	0.001
92	0.001	0.001
93	0.001	0.001
94	0.001	0.001
95	0.002	0.001
96	0.002	0.001
97	0.002	0.001
98	0.003	0.000

Table A7: Distributional Effects Regressions
Credit Score (Equifax Risk Score)

Percentile	Credit Score Coef.	Credit Score SE
99	0.002	0.000

Note: Estimates correspond with those in Figure 9. Standard errors are clustered at the ZIP code level.

B Data Appendix

This section contains key details about the construction of the main sample as well as detailed notes on the generation of each key variable.

All variables are constructed for the 2005-2016 time period with the exception of bankruptcies, which cover 2006-2016.

The base sample of SOI ZIP code data includes a total of 145 million tax returns filed in 2014 out of a total of 148 million that year and 284 million personal exemptions compared to a total US population of 318 million that year. Coverage of populated ZIP codes is not complete because the IRS takes various steps to limit disclosure risk. These include masking the actual ZIP code identity of any ZIP codes with fewer than 100 returns and those that are nonresidential or single buildings; excluding those living abroad; excluding items with fewer than 20 returns filled for that item; excluding returns with negative adjusted gross income; rounding the number of returns to the nearest 10; and excluding returns filed without a ZIP code or whose ZIP did not match their state code.

The IRS Statistics of Income figures are available for roughly 25,545 ZIP codes with complete AHRF and Census data. This total is out of 40,000 or so total ZIP codes in the United States. By comparison, there are approximately 32,000 ZIP Code Tabulation Areas designated by the Census Bureau as containing any residents. I restrict my sample further to areas with at least 30 credit file records in the CCP for individuals age 18-64 in order to avoid complications of small samples. This eliminates another 4,800 ZIP codes (57,600 total observations) from my sample, most of which are sparsely populated or do not have large under 65 populations because they represent large retirement communities.

In total, my primary estimation sample consists of 20,625 unique ZIP codes spanning the years 2005-2016. My sample ZIP codes cover 139 million total tax returns of the 148 million filed in 2014 and 272.5 million of the 284 million total personal exemptions claimed nationally. Thus, my sample covers 96% of the total tax filer population and 94% of tax returns.

Though my sample covers nearly the whole tax filing population in the United States, it does not cover all ZIP codes.

Data on the ACA exchanges

While data on all health plans offered on the Federal insurance exchange (Healthcare.gov) are available for all years in 2014-2016, there are several states who are missing 2014 data on the health plans available on their state-based exchanges. These states are excluded from the estimates which use the simulated instrument (columns 4-5 in the main regressions). These states are Colorado, Connecticut, District of Columbia, Hawaii, Kentucky, Maryland, Massachusetts, Minnesota, Nevada, New York, Oregon, Rhode Island, Vermont, and Washington. Eliminating these ZIP codes from my OLS estimates does not materially change any of my estimates, suggesting that state composition is not particularly important to my results.

In terms of state-specific policies that drive exogenous variation in simulated eligibility, New York and Vermont both set their age curves to be completely flat, i.e. that there was no differential pricing by age for individuals on the exchange. Notably, New York and Vermont both of these states set these policies before the implementation of the ACA in 2014 and continued to use those policies. District of Columbia, Massachusetts, Minnesota, Utah, and New Jersey each set their own age curves for different groups, including children. In DC, Minnesota, and Utah, the 3:1 ratio for age 64 to age 21 was maintained, but differences within the 22-63 range were meaningful.

Two states enacted their own family tiers on their exchanges. In New York, a single parent with one or more children under 21 could be charged at maximum of 1.7 times the “base” adult individual rate. For a two-parent household, the maximum ratio an insurer could charge was 2.85 times the base individual rate. In Vermont, these ratios were 1.93 and 2.81 for a single parent and two-parent household with one or more children.

Table B1 lists each variable used in the analysis, its source, and a description of details relevant to its construction.

Table B1: Variable Sources and Description

Variable	Source	Description
PTC Per Capita	IRS	This is the total amount in premium tax credits received by filers in the ZIP code divided by the population below age 64 according to the American Community Survey or Decennial Census.
Total Credit Files	CCP	The total number of credit files for residents of the ZIP code. This is the total number of scored files in the full CCP multiplied by 20 to scale up the 5% random sampling. For separate credit score ranges (Equifax Risk Score), this is the total number of credit files with scores in the particular range.
Third-Party Collections per 1,000	CCP	The total number of credit files with a positive debt amount in third-party collections divided by total number of credit files multiplied by 1,000.
Credit Card Severe Delinquency per 1,000	CCP	The total number of credit files with a positive debt amount designated as being 120 days past due, with a "severe derogatory" event such as repossession, in collections, or as part of a bankruptcy divided by total number of credit files multiplied by 1,000.
Auto Severe Delinquency per 1,000	CCP	The same construction as credit card severe delinquency, but for auto loans from a bank as well as other auto financing reported to credit bureaus.
Any Mortgage Severe Delinquency per 1,000	CCP	The same construction as credit card severe delinquency, but for debts on any mortgage including first mortgages, HELOANs, HELOCs, or junior liens.
Foreclosures per 1,000	CCP	The number of credit files with a foreclosure in the last 12 months divided by total credit files in the ZIP code. At scale, this number is lower than that reported in private sector estimates of foreclosure because it does not capture foreclosure for the population 65+. These appear to be relatively conservative estimates in comparison to other data sources which capture the number of properties rather than the number of credit files.
Bankruptcies per 1,000	CCP	The number of individual credit files which a change in bankruptcy status from year to year. This isolated "new" bankruptcies as opposed to having a bankruptcy on the credit file. I exclude 2005 because of the Bankruptcy Abuse Prevention and Consumer Protection Act, which made bankruptcy much more difficult to obtain and drastically cut the filing rate.
Mean Credit Score (Equifax Risk Score)	CCP	The mean of all Equifax 3.0 risk scores in the ZIP code.
Mean Amount in Third-Party Collections	CCP	Mean amount in third-party collections in a ZIP code conditional on having a positive balance.
Mean Amount of Credit Card Debt	CCP	Mean amount of credit card debt on credit files in a ZIP code conditional on having a positive balance.
Mean Amount of Severe Derogatory CC Debt	CCP	Mean amount of credit card debt that is 120 days past due or worse on credit files in a ZIP code conditional on having a positive balance.

Mean Amount of Severe Derogatory Auto Debt	CCP	Mean amount of auto debt that is 120 days past due or worse on credit files in a ZIP code conditional on having a positive balance.
Mean Amount of Severe Derogatory Mortgage Debt	CCP	Mean amount of mortgage debt that is 120 days past due or worse on credit files in a ZIP code conditional on having a positive balance.
Percentiles of continuous outcomes (Equifax Risk Score/credit score, delinquent balances)	CCP	For various outcomes (Equifax Risk Score/credit score, debt in third-party collections, severely delinquent debts), this is the nth percentile (1-99) taken within the ZIP code. Each of these are used as an outcome in a separate regression.
Median HH Income	ACS	Median household income in the ZIP code (2016 dollars using the PCE deflator); for all variables from the ACS, I rely on the midpoint of 5-year estimates. For example, year 2009 data come from 2007-2011 5-year estimates for the ZIP code.
Median House Value	ACS	Median house value in the ZIP code (2016 dollars using the PCE deflator)
% Unemployed Persons in CLF	ACS	Percent of the civilian labor force that is unemployed in the ZIP code
% Bachelor degree or higher	ACS	Percent of adults over 25 with a Bachelor's degree or higher
% White alone	ACS	Percent of the total population that identifies as "white"
% Black alone	ACS	Percent of the total population that identifies as "black"
% Asian alone	ACS	Percent of the total population that identifies as "Asian"
% Hispanic or Latino of any race	ACS	Percent of the total population that identifies as "Hispanic" or "Latino"
% Single Mothers	ACS	Percent of households headed by a single mother with children under age 18
% 20 to 24	ACS	Percent of the population age 20-24
% 25 to 34	ACS	Percent of the population age 25-34
% 35 to 44	ACS	Percent of the population age 35-44
% 45 to 54	ACS	Percent of the population age 45-54
% 55 to 64	ACS	Percent of the population age 55-64
# Primary Care Physicians	AHRF	The number of primary care physicians active in the ZIP code. All incomplete AHRF data are linearly interpolated between years. Any AHRF variables based on county-level data are apportioned to ZIP codes via population weights.
# OBGYN Specialists	AHRF	The number of OBGYN specialists active in the ZIP code.
# Physician Assistants	AHRF	The number of Physician Assistants active in the ZIP code.
# Nurse Practitioners	AHRF	The number of Nurse Practitioners active in the ZIP code.
# Clinical Nurse Specialists	AHRF	The number of Clinical Nurse Specialists active in the ZIP code.

Second Lowest-Cost Silver Plan Premium	CMS RWJF	This is the second lowest cost Silver plan for a 30 year old individual in each ZIP code. This is calculated based upon either the posted age-specific premium for every plan, or the standardized "age curve" required by states. If Rating Areas are the county level, each plan premium is apportioned to ZIP codes based on population weights.
Statewide Average Silver Plan Premium	CMS RWJF	The average Silver plan premium for an individual age 30 across all Rating Areas. This approximates the statewide expected cost of insuring the newly enrolling marketplace population. States with systematically higher premiums may have a different proportion of sick people entering the exchanges or else have other regulations that determine exchange premiums.
Number of Insurers on the Exchange	CMS RWJF	This is the number of "issuers" listed as offering a Silver plan in each Rating Area in the QHP or SBE.