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The effect of health insurance on home payment delinquency: Evidence from ACA Marketplace subsidies☆

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ABSTRACT

We use administrative tax data and survey responses to quantify the effect of subsidized health insurance on rent and mortgage delinquency. We employ a regression discontinuity (RD) design, exploiting the income threshold for receiving Marketplace subsidies in states that did not expand Medicaid under the Affordable Care Act. Among households targeted by the policy, eligibility for subsidies is associated with a roughly 25 % decline in the delinquency rate and reduced exposure to out-of-pocket medical expenditure risk. IV treatment effects are significant, indicating that the decline in the delinquency rate is related to participation in health insurance. We show that, under plausible assumptions, the social benefits implied by our RD estimates, in terms of fewer evictions and foreclosures, are substantial.

1. Introduction

A growing body of research finds large internalized and externalized costs associated with a household's inability to make timely home payments. Nonpayment of rent can make a household ineligible for public housing assistance and limit its access to credit and rental markets (Greiner et al., 2013). Moreover, delinquent home payments are antecedents of foreclosure and eviction, which can impose large costs not only on the delinquent household but also on the local community and the government.[1] Financially constrained households may, therefore, rationally prioritize home payments over other bills, such that missed rent and mortgage payments are indicative of extreme financial strain. It is striking then to note that 19% of the low-income households analyzed in this paper fall delinquent on their rent or mortgage within a 6-month period. Despite its prevalence and economic importance, there has been little research

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[1] By some estimates, a single foreclosure reduces local house prices by anywhere from 1% to 8.7% (Campbell et al., 2011; Lin et al., 2009). When house prices fall and properties are abandoned, local governments lose property tax revenue (Elliott and Kalish, 2016; Schuetz et al., 2016). Evictions, too, can also impose costs on society — having been linked to poor child development outcomes, greater exposure to high-crime neighborhoods, and higher rates of mental illness. See, e.g., Burgard et al. (2012), Desmond and Kimbro (2015), Ziol-Guest and McKenna (2014), Desmond and Shollenberger (2015), and references therein.

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into the causes of delinquent home payments, particularly among renters.

Anecdotal reports have long suggested that health shocks cause some households to fall behind on home payments.[2] Formal evidence on the link between health costs and home delinquency is, however, lacking.[3] To the extent that the most immediate role of health insurance is to protect against large medical expenditures, we expect that health insurance may negatively affect the delinquency rate on rent and mortgage payments. Our study provides an estimate of the magnitude of this effect within the context of a major reform to the U.S. health system.

We exploit the roll-out of the Patient Protection and Affordable Care Act (ACA) to generate a quasi-random shock to the cost of health insurance. Our method relies on the expansion in access to affordable, non-group private health insurance sold through the ACA's insurance "Marketplaces." Under the law, the federal government subsidizes the premiums ("advanced tax credits") of households earning between [100%, 400%) of the federal poverty line (FPL), as well as the out-of-pocket costs ("cost-sharing reductions") of households earning between [100%, 250%) FPL. Equally important to our identification strategy is the decision of 22 state governments not to fully expand Medicaid to their low-income adult population, as mandated under the ACA in 2014. In these "nonexpansion" states, some adults have incomes that fall into the "coverage gap" – meaning they earn too much to qualify for Medicaid but too little to be eligible for Marketplace subsidies. Put together, these features of the ACA's design and roll-out create an exploitable disparity in insurance access along income and state lines.

We leverage this disparity using a regression discontinuity (RD) design on a sample of households in nonexpansion states with incomes near 100% FPL. The intuition is simple: the availability of subsidies above the income threshold should discontinuously increase the share of households receiving quality non-group private health insurance, which, in turn, should discontinuously reduce home delinquency rates, if home delinquencies are influenced by health (costs). To test this hypothesis, we run reduced form and fuzzy RD regressions, in which the subsidy threshold is the instrument and insurance coverage is the first-stage endogenous variable. Given the subsidy inherent in the Marketplace policies, our estimates capture the change in the rate of home delinquency due both to receiving subsidies to purchase/use insurance coverage (the wealth effect) and to receiving insurance coverage at actuarially fair prices (the insurance effect).

Our analysis is conducted on a novel administrative tax dataset with income information for a large sample of low-income households, the majority of which rent their homes. These are households that use a free online tax-preparation software in 2014, 2015 or 2016 to prepare their tax returns and that participate in a survey about their finances at the end of the tax filing process. The tax return data provides us each household's adjusted gross income (AGI), allowing us to approximate its eligibility for Marketplace subsidies.[4] The linked survey provides us with information on the household's health insurance status, home delinquency, and recent medical expenditures. Our ability to capture rent delinquency for these households is important since, unlike mortgage delinquencies, rent delinquencies are generally not reported to credit bureaus and are often missing from public survey datasets. We have 15,967 observations from nonexpansion states and 24,531 observations from expansion states in the pooled 2014–2016 sample.

First, we test for changes in insurance status at the 100% FPL threshold in nonexpansion states. Note that to be eligible for Marketplace subsidies, a participant cannot have access to an alternative form of affordable health insurance, such as an employer plan. Consistent with the policy design, we find no significant evidence that the Marketplace subsidy policy crowded-out other forms of insurance (e.g., employer plans). Therefore, we perform our analyses both in the full sample as well as in a subsample of households that report not having the types of insurance that would typically disqualify them from receiving Marketplace subsidies. We call the latter group the "intent-to-treat" (ITT) sample as it includes those households that were targeted by the policy. We do this to increase the power of our instrument as the full sample includes a majority of households whose insurance status is unaffected by the instrument. We expect our estimates to be larger for the ITT subsample.

We find a significant increase in the fraction of households from nonexpansion states that report health insurance coverage at the 100% FPL threshold. In particular, the share of the full sample reporting any form of insurance coverage during 2014–2016 jumps by 4.0 percentage points (a relative 6% increase). Similarly, the share specifically reporting non-group private insurance coverage increases by 3.7 percentage points (a relative 35% increase). Within the ITT sample, the share reporting non-group private insurance rises by about 9.4 percentage points (a relative 46% increase) and appears to grow over time. In placebo tests, we do not find a significant increase in the probability of coverage at 100% FPL among households living in states that expanded Medicaid. This confirms that the increase in coverage that we document is because of the Marketplace subsidies.

Next, we turn to estimating the impact of the subsidy policy on home delinquency rates. Our primary specification is a reduced form RD, which offers policymakers a sense of the overall impact of the subsidy policy irrespective of actual enrollment. We find that being above the poverty line in a nonexpansion state is associated with a 2.4–7.8 percentage point (or a relative 9%–33%) decline in the rate of home payment delinquency among the full sample of households, with the effect being larger in magnitude and statistically significant only in 2016. Within the ITT sample, the effect is large and significant in all years. Our most conservative ITT sample estimate suggests that gaining access to Marketplace subsidies is associated with an 8.3 percentage point (or a relative 25%) decline in the home delinquency rate among households that lack access to alternative insurance plans. We find that our effects also appear to be concentrated in the subsample of households that are at a greater likelihood of health problems. As expected, we find no significant effects in expansion states.

It should be noted that the home payment delinquency rate varies with FPL with a peak at around 90% FPL, regardless of state or year. If a similar heterogeneity exists in the treatment effect, then that may affect the applicability of our estimates to other parts of the FPL distribution. Our estimates are also noisy, with the 95% confidence interval indicating somewhere between a 2% and 78% relative decline in the home delinquency rate due to the policy.

Using a fuzzy RD design, we estimate the treatment effect for households with a health insurance status that is correlated with the instrument. If the subsidy threshold affects delinquencies exclusively through health insurance, the IV model estimates the causal effect of treatment on this group. Our preferred specification indicates that households in the full sample that go from being uninsured to having insurance coverage due to the threshold are 12.4 percentage points less likely to report a home payment delinquency.

[2] For instance, in his book on eviction, Mathew Desmond recounts the events that lead to the evictions of several Milwaukee residents, including Teddy. Desmond writes: "They had fallen behind [on rent] two months ago, when a neck X-ray and brain scan set Teddy back $507. Teddy's health problems began a year earlier, when he woke up in the hospital after tumbling down some steps…." See also Tirado (2014).

[3] Although a number of studies document how housing instability affects health, we are aware of only one analysis of the effect of health costs on housing instability (Gupta et al., 2016) and that analysis is narrowly focused on the foreclosure decisions of cancer patients in Washington state.

[4] In Section 4.2, we test for potential manipulation of our assignment variable around the subsidy threshold and do not find any evidence of that. Furthermore, in Appendix Table A3 (Panel C), we confirm that our results are robust to the exclusion of self-employed households — the subsample that has been shown to sometimes sort around program thresholds, including this one (Kucko et al., 2017; Saez, 2010).

One channel through which health insurance may affect home delinquency is through expense shocks. We document a large reduction in the upper quantiles of the out-of-pocket medical spending distribution at the subsidy threshold. This effect is found in nonexpansion states only. Thus, as designed, health insurance appears to prevent health shocks from becoming significant liquidity shocks and, thereby, reduces the probability of home delinquency.

Back-of-the-envelope calculations based on our estimates suggest that the ACA subsidy program may generate incidental welfare benefits that are substantial when juxtaposed with the transfer costs of the subsidies. For example, if we assume that an eviction carries a social cost of around $5,000, then delinquency probabilities predicted from our RD model would imply social benefits of $441 per eligible household. For a reference point, that amount forms 27% of the transfer cost of the insurance subsidies per eligible household. This numerical exercise is admittedly suggestive as it relies on strong assumptions about the probability and cost of eviction and ignores the social costs associated with financing the subsidies, including the dead-weight cost of taxes.

Our paper is related to the literature that uses experimental or quasi-experimental designs to understand the effect of health insurance on household finance (Gross and Notowidigdo, 2011; Finkelstein et al., 2012; Barcellos and Jacobson, 2015; Mazumder and Miller, 2016; Brevoort et al., 2018). These studies have uncovered certain downstream financial benefits of expanded insurance access — including lower out-of-pocket medical spending, fewer bankruptcies, improved credit scores, and fewer unpaid medical and non-medical bills. By relating home delinquency to health insurance, we contribute to this body of work.

Our paper is also unique in terms of its setting. We exploit variation in the cost of health insurance created by the ACA's Marketplace subsidy program. Existing research on the ACA focuses heavily on the effect of Medicaid expansion on financial outcomes (e.g., Hu et al., 2016), labor market effects (e.g., Duggan et al., 2017), or the effect of ACA on insurance coverage rates (e.g., Courtemanche et al., 2016). By, instead, exploiting the Marketplace subsidy policy in its design, our paper applies a different lens to evaluations of the ACA's impact on household financial outcomes.

A final contribution of our study is to document a source of plausibly exogenous variation in health insurance status that could be used to estimate the effects of the ACA's subsidy policy on other outcomes. While a number of papers use quasi-experimental designs based on Medicaid expansion, their variation is usually generated at the state-year level and is potentially subject to identification challenges. Our RD design, which is greatly facilitated by administrative income information, circumvents several of the identification challenges and also allows for a natural placebo comparison using expansion states.

Our paper proceeds as follows. Section 2 provides background information about the ACA. Section 3 describes the dataset and our strategy for constructing variables. Section 4 details our empirical approach. Section 5 presents our results. Section 6 discusses the broader significance of our findings for policy. Section 7 concludes.

2. Background on the ACA

The ACA represents the most substantial change to health policy since Medicare and Medicaid were enacted in 1965. By the end of March 2016, an additional 20 million people were believed to have gained coverage; 11.1 million were covered through the ACA's Marketplaces, of which 85% received subsidies.[5]

Prior to the passage of the ACA, Medicaid was primarily a program for children, pregnant women, older adults, and the disabled living in low-income households. States typically did not offer Medicaid to childless adults and offered it only to parents with incomes that were well below the poverty line (Garfield and Damico, 2016). Through large federal subsidies, the ACA encourages states to expand Medicaid to include able-bodied adults. These include adults earning at or below 138% FPL. As of 2016, 31 states (and Washington, DC) fully expanded Medicaid and 19 states did not.

In states that chose not to expand Medicaid, some households were left in the insurance "coverage gap." As visually represented in Fig. 1, the coverage gap includes adults who earn too much to qualify for Medicaid but too little to qualify for Marketplace insurance subsidies, which begin at 100% FPL in nonexpansion states. Health insurance is usually very expensive for people in the coverage gap.[6] Adults in the coverage gap who are unable to obtain insurance through an employer or a family member often go uninsured or purchase "catastrophic" (low-premium, high-deductible) plans. The Kaiser Family Foundation estimates that 2.6 million Americans are living in the coverage gap (Garfield et al., 2016). Our research design exploits this coverage gap, and the associated income threshold above which a household exits the gap, to identify the influence of subsidized health insurance on the prevalence of delinquent home payments.

The ACA provides two types of subsidies – premium assistance and cost-sharing reductions (CSRs) – for households to acquire non-group private insurance sold on the "Marketplace" (also called the "Exchange"). Fig. 2 helps to visualize the rather complicated subsidy eligibility rules for households from different states and with different income levels.

Premium subsidies, which are often referred to as "advance premium tax credits," are paid in advance by the Internal Revenue Service (IRS) to insurers. They are subsequently reconciled based on any difference between projected income at the time of enrollment and actual income as reported on tax returns filed the following year. If a participant receives a premium subsidy and then ends up earning less than 100% FPL during the enrollment year, the participant does not have to pay back the subsidy at tax-time the following year.[7,8]

At incomes just above 100% FPL, the premium subsidies are substantial. According to the Kaiser Family Foundation, a 40-year-old single adult earning just over 100% FPL (or about $12,000) would have paid an average monthly premium of $20 ($240 per year) after subsidies in 2016. Without the subsidies, that same participant would have paid about $299 per month for the same plan ($3,588 per year), equal to 30% of the participant's annual income. These subsidies fall continuously, but non-linearly, after 100% FPL, such that the amount of premium assistance becomes negligible at incomes of above 300% FPL.

In expansion states, there is no jump in premium subsidy at 100% FPL as households that earn at or below 138% FPL are not eligible for

[5] These estimates are from the Centers for Medicare and Medicaid Services. See: https://www.cms.gov/Newsroom/MediaReleaseDatabase/Fact-sheets/2016-Fact-sheets-items/2016-06-30.html.

[6] For example, if a family of four making $20,000 per year (82% FPL) and living in a nonexpansion state chooses to insure the two children through either the Children's Health Insurance Program or Medicaid and to insure the two adults (both 30 years old) through a silver plan purchased on the Marketplace, that family could expect to pay the following for the silver plan: premiums of $531 per month ($6,371 per year, or 31.86% of the family's annual income) and 30% of the cost of the adults' medical services, up to an out-of-pocket limit of $13,700 (Kaiser Family Foundation 2016 Calculator).

[7] See: https://www.healthinsurance.org/faqs/if-my-income-is-less-than-expected-this-year-i-might-be-eligible-for-medicaid-what-can-i-do-during-open-enrollment-to-cover-my-bases/.

[8] Households that earn *more* than they had projected may be required to repay a portion of their subsidy at tax-time after the coverage year. The repayment amount is capped at $300 for single filers and $600 for married filers as long as actual income remains below 200% FPL. See footnote 4 in the letter from IRS Commissioner John Koskinen to Congress available at: https://www.irs.gov/pub/newsroom/commissionerletteracafilingseason.pdf.

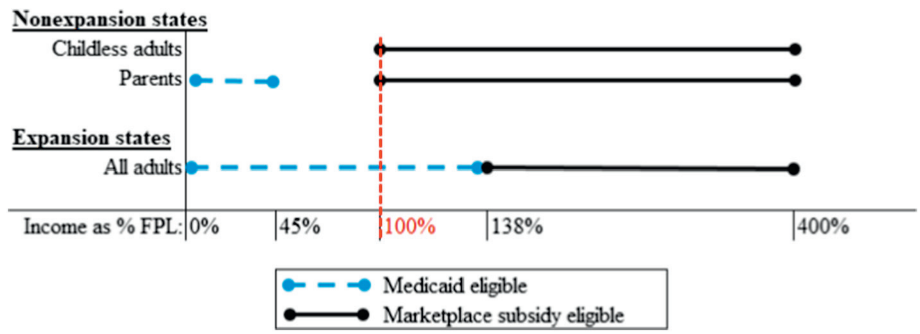

Fig. 1. Medicaid and Marketplace subsidy income thresholds. This figure shows income eligibility income thresholds for Medicaid and Marketplace subsidies in Medicaid nonexpansion and expansion states, separately. The vertical dashed line represents the threshold of interest in this paper (100% FPL). States that did not expand Medicaid have different thresholds for parents. The average threshold in 2016 for states that did not expand Medicaid is 45% FPL, calculated using data from the Kaiser Family Foundation.

Marketplace subsidies because they are, instead, eligible for Medicaid. As the second vertical line illustrates, households in expansion states that earn in the range (138%, 400%) FPL are eligible to receive premium subsidies.[9,10]

Participants in nonexpansion states that earn in the range [100%, 250%) FPL also qualify for CSRs. For participants in expansion states, eligibility for CSRs begins at 138% FPL. CSRs reduce out-of-pocket costs, such as from deductibles and copayments, paid when health care services are used. According to the Kaiser Family Foundation 2016 Calculator, CSRs decrease the out-of-pocket maximum from \$6,850 to \$2250 (mostly through a reduction in the deductible), on average, for a 30-year-old single adult earning just over 100% FPL in 2016. The federal government pays these subsidies directly to the insurer, and the participant does not have to refund the cost-sharing subsidy if his/her projected income at the time of enrollment differs from the actual income reported in the tax return.

The effect of CSRs is to raise the actuarial value of the insurance plan. As represented by the red step function line in Fig. 2, the base actuarial value of a "silver" plan on the Marketplace is 70%, meaning that the plan pays 70% of the typical participant's health care costs. CSRs lift the effective actuarial value of a silver plan to 94% for participants with income in the range [100%, 150%) FPL, to 87% for incomes in the range [150%, 200%) FPL, and to 73% for incomes in the range [200%, 250%) FPL.

Thus, for participants that earn under 200% FPL, the combined benefit of the two subsidies is substantial — the subsidies cover the majority (at least 58%) of the premium and the effective actuarial value of the plan rises from 70% to at least 87%. By comparison, a household that earns just under 250% FPL gets assistance toward a more modest 33% of the premium and the actuarial value rises only slightly to 73%. Meanwhile, households that earn at or above 300% FPL get a 70% actuarial value plan with very little premium assistance, at most just 5% of the \$299 premium is subsidized. These estimates highlight the fact that the Marketplace subsidy policy is extremely progressive. As we will later discuss, our analysis is restricted to households that earn under 200% FPL.

In summary, the 100% FPL threshold in nonexpansion states is a line between subsidized and non-subsidized health insurance. The subsidy comes in the form of both the front-end premiums and back-end deductibles. We will not be able to isolate the effect of one subsidy versus the other.[11] Thus, the changes in the financial well-being of households at 100% FPL as estimated with our reduced form model could be equally attributable to a jump in the prevalence of health insurance (an extensive margin effect) as well as to a decline in the deductible for previously insured participants (an intensive margin effect).

3. Data and variables

3.1. Data

We obtain our data from the tax records and survey responses of households that use a free online tax-preparation software to prepare their tax returns during the fiscal years 2014, 2015 and 2016 and who consent to their anonymous data being used for research. Filers with an adjusted gross income of less than \$31,000 and/or who qualify for the Earned Income Tax Credit (EITC) qualify to file their returns with this particular form of IRS Free File Alliance software (henceforth, "IRS-FFA").[12] Because the income ceiling for families to be eligible for the EITC roughly corresponds with 200% FPL, only 9.2% of our sample has income above 200% FPL and we have no observations with income over 400% FPL. Those who earn over 200% FPL but under \$31,000 are majority single adults. Thus, we restrict our analysis to respondents earning under 200% FPL. Doing so also ensures a uniform level of subsidy (see Fig. 2) among those in the "treated" sample.

Immediately following the tax-filing process (typically around late March, early April), a randomly selected sample of filers are invited to participate in a household financial survey (henceforth, "HFS") and are offered a small financial incentive for completion (an Amazon gift card of varying value). The survey includes a wide array of questions about filers' assets, liabilities, financial behaviors, use of social services, experiences of hardship, and health insurance status. Our sample includes data on households that complete the survey and provide consent. Over our sample period, 9% of households that were offered the survey completed the survey.

The HFS is not longitudinal in nature and only about 4% of the households in our sample complete the survey in consecutive years.

9 Not all expansion states use the 138% FPL threshold. Some have higher thresholds, in which case the second vertical line in Fig. 2 would shift rightward.

10 Medicaid rules somewhat complicate the Marketplace eligibility threshold in expansion states. Medicaid eligibility is usually based on current *monthly* income (at an annualized rate) and verified by Medicaid administrators roughly every 6 to 12 months. In contrast, access to the Marketplace subsidies is based on the household's projected *annual* income and being deemed no longer eligible for Medicaid. It is, therefore, possible to have someone who actually earns over 138% FPL for the year, but is on Medicaid because their annualized *monthly* income was below 138% as of the last time it was verified. Thus, the line between Medicaid and Marketplace subsidies will blur when annualized monthly income at the time of Medicaid enrollment differs from annual income. See: http://www.healthreformbeyondthebasics.org/key-facts-income-definitions-for-marketplace-and-medicaid-coverage/.

11 Evidence suggests that both monthly premium subsidies and CSRs are highly salient to consumers when choosing plans (DeLeire et al., 2017).

12 The particular vendor of the software asks to remain anonymous. However, the vendor serves filers throughout the United States. Parties interested in replication can be put in touch with the vendor through the Institute for Social Policy at Washington University in St. Louis. For more on the Free File Alliance, see: https://www.irs.gov/uac/about-the-free-file-program.

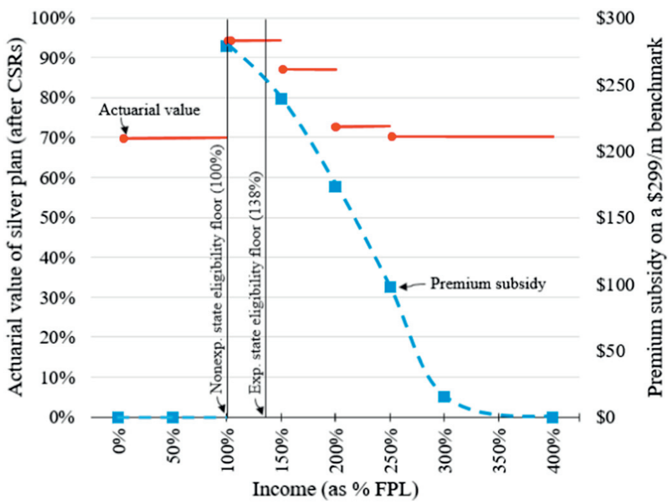

Fig. 2. Marketplace insurance subsidy schedule for hypothetical adult. Figure shows the actuarial value of Marketplaces "silver" plans (LHS axis, red lines), which are subject to cost-sharing reductions (CSRs) that affect the effective actuarial value of the plan, moving it from its 70% baseline. The figure also shows that amount of premium subsidy (RHS, blue dashed lines) that a hypothetical 40-year-old, single, adult could expect when purchasing a $299/month benchmark Marketplace silver plan. Black vertical lines mark the income floors at which households in nonexpansion and expansion states gain access to both CSRs and premium subsidies, which simultaneously become available when a households earns over 100% FPL in nonexpansion states or over 138% FPL in expansion states. In all states, CSRs end at 250% FPL and premium subsidies end at 400% FPL.
Source: authors' tabulations of the subsidy schedule provided by the Kaiser Family Foundation (https://www.kff.org/health-reform/issue-brief/explaining-health-care-reform-questions-about-health/) and the *estimated* premium subsidy from the Kaiser Family Foundation's 2016 calculator.

We pool the data for the first three years the Marketplaces were open to conduct our analysis. We repeat some of our tests in the last year of available data, 2016, to evaluate if there are any differences over time in the effects we document. The sample for the regressions includes low-income filers who are U.S. citizen civilians aged 19 to 64 at the time of data collection. This results in a sample of 15,967 observations from nonexpansion states and 24,531 observations from expansion states in the full, pooled 2014–2016 sample.

To be eligible for Marketplace subsidies, a household must not have access to "affordable" coverage (currently defined to be a plan with an actuarial value of at least 60% and with an employee contribution of less than 9.7% of income) through an employer, which includes a family member's employer. The household also cannot be eligible for insurance through Medicare, Medicaid, or other government insurance programs. We refer to the subsample of households that do not have access to such insurance as the "intent-to-treat" (ITT) subsample and repeat our tests in this subsample. Although access to Medicaid would make a household ineligible to receive Marketplace subsidies, we retain households that are eligible for Medicaid in the ITT sample to make it comparable across expansion and nonexpansion states.[13] We have 6443 observations from nonexpansion states and 10,390 observations from expansion states in the ITT, pooled 2014–2016 sample.

For two reasons, we repeat our tests in the 2016 sample. First, as we will later document, the discontinuity in non-group private insurance coverage is larger in 2016 than in earlier years. This is likely because the penalty for not being insured became more onerous in 2016. The accuracy of self-reported insurance types may also have improved over time as the different components of the ACA became better understood.[14] The strong 2016 discontinuity means a stronger instrument for non-group private insurance, particularly in the ITT sample, but at the cost of a much smaller sample size. Second, certain input variables, discussed in the next subsection, are not available from the 2014 and/or the 2015 surveys.

To generate a discontinuity in the cost of health insurance, we focus our analysis on nonexpansion states — defined in our study as states with a gap between the income threshold for adult Medicaid eligibility and for Marketplace subsidies.[15] In these states, the Medicaid eligibility ceiling for childless adults and, in most cases, parents is less than 100% FPL. According to the data from the Kaiser Family Foundation, 25 states met this criterion at some point during the 2014–2016 period. They are listed in Appendix Table A1.

Appendix Table A2 summarizes the health insurance coverage of respondents across income buckets and state Medicaid expansion status.[16] This table also provides preliminary evidence of the effect of the Marketplace subsidy policy on increasing access to non-group health insurance. Also see Appendix Table B1, for a detailed demographic comparison of the HFS and American Community Survey (ACS) samples. To control for oversampling (Solon et al., 2015) of younger adults and students, in certain specifications we include controls for income, age, race, number of dependents, college completion, gender, marital status, employment status, and student status.

Despite its novelty, there are three potential problems with our data. First, there is possible misreporting of survey data due to participant inattention or due to survey questions being subject to multiple interpretations. However, we take some comfort in the fact that we observe a substantial discontinuity in the probability of non-group private health insurance at the 100% FPL. Also, our key outcome variable (home delinquency) is binary and generated from a straightforward question.

Second, there is a time mismatch between when insurance status is measured (at tax-time) and the period when delinquency is captured (the 6-months ending at tax-time). To the extent that enrollment in the Marketplaces increases over time, it is likely that we overestimate the share of participants that were insured at the time of a recent past delinquency. This measurement error should bias the RD coefficients toward zero.

Third, there is an unknown degree of sample selection bias. If the factors that influence the likelihood of participating in the HFS are correlated with financial decision-making, it would affect the external validity of our estimates. We note that our tests do not reveal significant selection on observables. For example, we find that the characteristics of individuals that take the survey are statistically similar to the characteristics of the population that uses the tax

[13] We make this accommodation for Medicaid because there are systematic differences in Medicaid access for adults across expansion and nonexpansion states that are not clearly present for other forms of insurance (see Appendix Table A2). In particular, we retain in our ITT sample the person who is eligible for Medicaid but goes uninsured as well as the person who is eligible and enrolls. In expansion states, everyone with income under 138% FPL is eligible for Medicaid, which means that we cannot restrict the sample based on eligibility. For affordability rules, see: http://kff.org/health-reform/issue-brief/explaining-health-care-reform-questions-about-health/.

[14] Indeed, we find across all surveys that some households select "Other insurance" and, then, voluntarily write in the word "Obamacare," as though they are unsure of how to classify their new insurance. Moreover, nearly twice as many people report other insurance in 2014 as they do in later years.

[15] In common usage, the term "nonexpansion state" refers to a state that has an adult Medicaid income eligibility ceiling of less than 138% FPL — the federally mandated minimum under the ACA. All other states are referred to as "expansion states." We depart from this definition, classifying Wisconsin, for example, as an expansion state because its Medicaid eligibility ceiling is 100% FPL, the eligibility floor for Marketplace subsidies.

[16] We obtain this data in response to the multiple-choice question: "What kind of health insurance do you currently have?" Participants have the option of selecting "I am currently uninsured" as well as eight other forms of insurance, including "Non-group private insurance purchased directly (includes healthcare.gov/ACA exchanges)."

software. We also find that the samples generated by changing the participation reward (e.g., \$0, \$5, \$20) are similar on observables.

Overall, we believe that the concerns we identify are overwhelmed by the novelty of the information contained in this dataset — permitting the study of a policy and outcome that have been difficult to evaluate with standard datasets. Moreover, our research design allows us to conduct placebo tests in expansion states to overcome some of the drawbacks of using survey data. Additional details on the dataset, and the steps we take to mitigate any potential sample problems are available in Appendix B.

3.2. Variables

Our first variable of interest is household income as a percentage of FPL. We calculate this by combining the household's AGI as reported on its tax return, its household size from the linked survey, and the federal poverty guidelines for each state and for each year.[17] This is the running variable in our RD design, with treatment (eligibility for Marketplace subsidies) assigned at incomes above 100% FPL in nonexpansion states.

Eligibility for Marketplace subsidies (both for cost-sharing subsidies and for advanced premium tax credits) is based on a household's "projected" modified adjusted gross income (MAGI) for the calendar year during which the advance premium tax credit is received. We, on the other hand, use the household's AGI for the previous calendar year to construct our running variable. For example, the open enrollment for 2015 Marketplace plans occurred from November 15, 2014, through February 15, 2015. We use the AGI for the fiscal year 2014 to construct our running variable for this cycle.[18] To the extent that Marketplace providers verify the reasonableness of income projections through credit bureaus, wage slips or prior tax forms, prior year income reported on the tax form should closely approximate projected income.[19] Nonetheless, this approximation is likely to introduce noise in our running variable. Despite this noise, we will later document a significant discontinuity in insurance coverage at the subsidy threshold in nonexpansion states.

Our main outcome variable captures self-reported difficulty in making rent or mortgage payments on time ("home delinquency" hereafter). This variable, *Home delinquency*, is constructed from a straightforward survey question: "Was there a time in the past 6 months when you or someone in your household did not pay the full amount of the rent or mortgage because you could not afford it?" *Home delinquency* identifies households that provide a positive response to this question. Note that our running variable could potentially be correlated with both the current year's projected MAGI and/or the previous year's projected MAGI. To this extent, our instrument is likely to identify households eligible for Marketplace subsidies during the six months we use to measure delinquency.

Since we are focused on low-income households, we are dealing primarily with a sample of renters. Table 1 provides the breakdown of our sample from nonexpansion states based on reported housing status. Among all respondents, 53% are renters while 17% own their home. A substantial portion (31%) report living rent-free with friends/family, in dormitories, or in employee housing. We include such households in our analysis because their current housing status could reflect recent evictions or foreclosures due to difficulties making home payments. Our main results are stronger when we remove such participants from the sample (see Appendix Table A3, Panel B). Furthermore, compared to our control sample, an (insignificantly) larger fraction of our treated sample rents or owns.

Because our dataset lacks a good measure of current health status, we construct an indicator by combining all health-related information on the survey. First, we identify households with medical debt in excess of 5% of annual income — equal to the top quartile among non-group privately insured households. High amounts of medical debt may indicate either an ongoing chronic condition (e.g., diabetes) that predates the ACA or a more recent severe health shock. Next, we include the 11% of households that report that medical spending caused a variation in their monthly expenses. A positive response to this question may indicate frequent medical treatments or prescriptions. Finally, we use responses to the following question: "In the last 6 months, have you or has any member of your household (the people on your tax form) had an unexpected major out-of-pocket medical expense (e.g., from hospitalization or emergency room visit)?"[20] We create a binary measure, *HealthProb*, that equals one if a household meets any of these three criteria. As only the 2016 survey asked all three of these questions, results using *HealthProb* are for the 2016 sample only. Based on this variable, we identify 39% of the full 2016 sample and 45% the ITT 2016 sample as having experienced a recent health problem.

Table 1 provides descriptive statistics for households from nonexpansion states as well as the sample balance (Imbens and Wooldridge, 2009) of the 2016 ITT subsample. The only variable that is clearly seriously unbalanced between treatment and control samples is non-group private insurance coverage ($diff/s.d. = 0.47$ standard deviations), which is by design. A strong indicator of the representativeness of our sample is that the overall share of the potential Marketplace (i.e., the treated, ITT subsample) that is enrolled from our sample nearly equals that same statistic estimated using the 2015 Current Population Survey by the Kaiser Family Foundation (42% vs. 40%).[21]

We find a very high level of *Home delinquency* in our sample. In our full sample, 19% report recent hardship meeting rent or mortgage payments. For comparison, this number is 21% (28%) in the treated (control) 2016 ITT subsamples. A much higher fraction of the households in our sample face hardship meeting regular bill payments (e.g., 47% in the full sample). Bankruptcy is markedly rarer than these other forms of hardship, affecting just 1% of the sample over the past year (see Appendix Table A5 for further analysis of bankruptcy).

Overall, the households in the treated sample appear to be in a slightly healthier financial condition as compared to the households in the control sample, consistent with their higher incomes. On average, the households in our full sample spend \$1,561 on medical care. This variable is significantly right-skewed with the median medical spending at \$500. On average, the households in our sample have \$3,142 in liquid assets. This variable is also significantly right-skewed with the median value at \$647. Not surprisingly, treated households have more liquid assets as compared to control households, although the difference is not statistically significant. We find that

[17] Federal poverty guidelines are obtained from the U.S. Department of Health and Human Services website (available at https://aspe.hhs.gov/poverty-guidelines). Following enrollment rules, we use the prior year's federal poverty guidelines to estimate subsidy eligibility during the enrollment year.

[18] Apart from the timing difference, AGI varies from the MAGI along three dimensions: non-taxable social security benefits, tax-exempt interest, and excluded foreign income. Since we lack information on these three components, following standard practice in literature, we use AGI instead. Nevertheless, we expect differences between AGI and MAGI to be negligible in our sample. This assumption is validated in an analysis of 2014 IRS data by Hinde (2017).

[19] Projections that differ substantially from past income must be supported by documentation. Enrollees are granted presumptive eligibility for a limited time while the discrepancy is reconciled. We thank Amy Crews Cutts (Chief Economist, Equifax) for discussing the Marketplace income verification process with us.

[20] Ideally, we would have preferred a question that does not combine the expense and the medical event. Nonetheless, about the same portion of respondents with non-group private insurance report positive responses to this question compared to respondents that are uninsured. In Appendix Table A4, we exclude this variable from our definition of *HealthProb*. Regression results are similar, albeit statistically weaker.

[21] See Kaiser Family Foundation, State Health Facts, Marketplace Enrollment as a Share of the Potential Marketplace Population, available at http://www.kff.org/statedata/.

Table 1
Summary statistics.

Variable	Full, 2014–2016 N	Mean	s.d.	ITT control, 2016 N	Mean	ITT treated, 2016 N	Mean	Diff/s.d.
Own	15,962	0.17	0.37	1598	0.14	784	0.23	0.23
Rent	15,962	0.53	0.50	1598	0.52	784	0.55	0.07
Other housing	15,962	0.31	0.46	1598	0.34	784	0.22	0.26
Income≥100% FPL	15,967	0.43	0.50	1598	0.00	784	1.00	n.a.
Non-group private ins.	15,967	0.11	0.31	1598	0.21	784	0.42	0.47
Home delinquency	15,967	0.19	0.39	1598	0.28	784	0.21	0.16
Bill delinquency	15,965	0.47	0.50	1597	0.59	783	0.53	0.13
Bankrupt	n.a.	n.a.	n.a.	1514	0.01	746	0.01	0.00
HealthProb	n.a.	n.a.	n.a.	1598	0.47	784	0.40	0.15
Medical spending	15,607	1561	2426	1580	1326	776	1619	0.13
Liquid assets	15,967	3142	6698	1598	1913	784	2928	0.18
Age	15,967	33.60	12.35	1598	34.79	784	36.77	0.17
White	15,967	0.85	0.36	1598	0.92	784	0.92	0.02
Male	15,967	0.46	0.50	1598	0.44	784	0.47	0.06
Single	15,967	0.85	0.36	1598	0.87	784	0.79	0.20
Student	15,967	0.33	0.47	1598	0.26	784	0.18	0.20
College graduate	15,967	0.44	0.50	1598	0.38	784	0.47	0.18
Number of kids	15,967	0.49	0.96	1598	0.59	784	0.46	0.14
Employed	15,967	0.80	0.40	1598	0.73	784	0.80	0.15

Table reports means and standard deviations using the full, 2014–16 sample of respondents living nonexpansion states and with incomes in the range (0%, 200%) FPL. The table also reports statistics for a subsample of 2016 intent-to-treat (ITT) households divided by treatment status. Treatment is assigned at incomes above 100% FPL (i.e., $Income \geq 100\% = 1$). The last column reports the difference in sample means (by treatment status), normalized by the standard deviation of the combined sample. Samples with normalized differences above 0.25 standard deviations (denoted with an asterisk) are considered unbalanced. The variables *Bankruptcy* – an indicator of bankruptcy in the last year – and *HealthProb* – which indicates a likelihood of health problems – are generated from survey questions that were not asked in 2014.

the treated and control samples are statistically similar along demographic characteristics, with small differences likely attributable to income. As discussed in the next section, we will flexibly control for income as well as demographics in our regressions.

4. Research design

This section describes our RD framework and details the process we employ for selecting bandwidths and polynomial orders. We also describe the results of the tests that verify the assumptions underlying our RD design.

4.1. RD: conceptual basis and estimation

We now introduce some notation to explain our empirical model. Let D_i represent an indicator variable that takes a value of one for households with insurance (which could be "any insurance" or, more specifically, "non-group private insurance"); let Z_i denote household income; let c be the threshold, 100% FPL, such that $1 > P(D = 1|Z \geq c) > P(D = 1|Z < c)$; and let T_i be an indicator of households with income above the threshold, $T_i = 1(Z_i \geq c)$.

With this framework, our primary specification is a reduced form RD that takes the form:

$$Y_i = a + \tau T_i + f(Z_i - c) + T_i \times f(Z_i - c) + \beta \upsilon_i + \delta_i \qquad (1)$$

where $f(Z_i - c)$ is an optimally chosen polynomial function of income (measured as a percentage of FPL) that varies discontinuously at the threshold and controls for the relationship between income and the outcome variable. The characteristics that we include in v are age, race, dependents, education level, marital status, employment status and student status of the head of household. Their inclusion will reduce the residual variance and lead to efficiency gains (Imbens and Lemieux, 2008; Calonico et al., 2016). Y_i is an indicator of a home delinquency.

In Eq. (1), our coefficient of interest is τ, which captures the difference in the probability of home delinquency among households that are eligible for the Marketplace subsidies compared to those that are ineligible. In the full sample, "eligible" refers to those who are eligible based on their income. Note that the majority of income eligible households may, in reality, be ineligible due to their access to alternative forms of insurance. In the ITT sample, "eligible" refers to those who are eligible on both grounds. The reduced form coefficient, τ, in the full sample is the weighted average effect on households with and without access to alternate forms of insurance. In the ITT sample, the reduced form estimate will capture the weighted average change in the rate of home delinquency due to both the gain in insurance coverage at the cutoff and any difference in cost/benefits package associated with subsidized Marketplace insurance relative to other forms of coverage. Note that the results throughout this paper capture the combined effect of receiving subsidies to purchase/use insurance coverage (the wealth effect) and of receiving insurance coverage at actuarially fair prices (the insurance effect). We cannot differentiate the two effects in this research setting.[22]

Despite the availability of subsidies and the federal mandate to purchase insurance, all uninsured households in nonexpansion states with income above 100% FPL do not buy health insurance. Thus, in a second test, we measure the effect of insurance coverage on home delinquency by estimating the following system of equations:

$$D_i = b + \pi T_i + g(Z_i - c) + T_i \times g(Z_i - c) + \beta_1 \upsilon_i + \epsilon_i \qquad (2)$$

$$Y_i = a + \gamma D_i + f(Z_i - c) + T_i \times f(Z_i - c) + \beta_2 \upsilon_i + \delta_i \qquad (3)$$

where the first equation models the probability that household i has health insurance coverage as a function of income, household (or head of household) characteristics, and the indicator variable T_i. In the second equation, γ is an estimate of the effect of insurance coverage on home delinquency for the subset of households with income near

[22] Since a low-income population is unlikely to purchase quality health insurance that is not subsidized (Finkelstein et al., 2012), a randomized control trial, in which one group receives insurance and the other receives the cash equivalent, would likely be necessary to convincingly disentangle the wealth effect from the insurance effect. We leave that task to future research.

the threshold, whose insurance status is correlated with whether they are above or below the threshold (commonly called "compliers"). If the exclusion restriction holds, the resulting IV estimand, γ, can be interpreted causally, as the LATE of gaining insurance coverage (in a subsidized form) on the probability of home delinquency for the subsample of compliers. Given the measurement error in our assignment variable, we interpret our estimate of γ cautiously — as evidence suggesting a causal relationship and its direction.

We estimate a fuzzy RD using a bivariate probit model (Heckman, 1978). We do so because both our outcome variable (*Home delinquency*) and the endogenous variable (*Any insurance*) are binary. Based on their simulation analysis, Chiburis et al. (2012) recommend a bivariate probit when treatment probabilities are low, when sample sizes are below 5000, or when covariates are included in the model.[23] However, in Appendix Table A9, we also show estimates from a 2SLS IV model, which tend to be larger in magnitude and less consistent across specifications.

Since outreach by state governments and non-profits, as well as political views, could affect the extent of enrollment in non-group private insurance across states, the standard errors in the first stage could be correlated across participants from the same state. It is also reasonable to expect that housing and rental markets within a state-year could be subject to common shocks. Hence, we report estimates in which we cluster the standard errors at the state level and also show that our results are robust to the inclusion of within-state-year effects. In settings with a binary outcome variable and an endogenous binary treatment variable, Chiburis et al. (2012) recommend reporting bootstrapped standard errors when sample sizes are less than 10,000. We follow their advice, while taking into account the potential dependence of the observations within states, and report our main fuzzy RD estimates with block bootstrapped errors (Cameron et al., 2008).

We show that our results are robust to a variety of bandwidths and polynomial orders, selected from data-driven approaches. According to the mean-squared error (MSE) optimal bandwidth selector of Calonico et al. (2014), under a local linear specification with a uniform kernel and controls, the optimal regression window (below and above the cutoff) in the full sample for our key variable of interest, *Home delinquency*, is [69%, 125%] FPL. The optimal window in the full sample for the endogenous variable, *Any insurance*, is wider at [42%, 165%] FPL. Finally, as a robustness check, we also repeat our tests on a wider bandwidth range of (0%, 200%) FPL. Following Lee and Lemieux (2010), we select the optimal polynomial order, *P*, to be the one that minimizes the AIC. Our procedure results in us selecting mostly linear and quadratic functions of income.[24]

Throughout our main analysis, we take the transparent approach of using a uniform kernel and estimating a standard polynomial regression over a range around the cutoff. However, in Appendix Table A8, we show that RD estimates using a triangular kernel are similar.

4.2. Validation of RD

In this section, we test for possible manipulation of the assignment variable (Lee and Lemieux, 2010). Note that while our assignment variable is based on previous year's AGI, eligibility for cost-sharing subsidies and advanced premium tax credits is based on projected MAGI. As such, the incentive for a household to manipulate its prior year taxable AGI is less obvious. However, one reason to manipulate prior year taxable AGI could be to convince Marketplace providers of the reasonableness of projected MAGI, given that providers sometimes use prior year tax forms to verify income projections (see Section 3.2). We, therefore, test for manipulation of our assignment variable. Note, however, that failure to find evidence of manipulation of taxable AGI does not mean that households are not manipulating projected MAGI (which we cannot observe). Manipulating projected MAGI to gain eligibility should weaken our instrument.[25]

We begin with the tests documented in Kucko et al. (2017). These authors test for manipulation of taxable AGI at 100% FPL using a much larger sample — the universe of Internal Revenue Service (IRS) Form 1040 tax returns from 2014 and 2015. They find some evidence for upward income manipulation at 100% FPL but only among a small subset of self-employed filers.[26] Less than 9% of our filers are self-employed and we find that our results are robust to excluding these filers (Appendix Table A3, Panel C).

Notwithstanding the results in Kucko et al. (2017), we independently test for jumps in the marginal density of incomes in our smaller dataset using the methods developed by McCrary (2008) and Cattaneo et al. (2017). Although there is a jump in the density at the threshold, it is not statistically significant (*p*-value is greater than 0.13 across all samples and methods).[27] Appendix Fig. A1 presents histograms of the density of sample incomes around the subsidy threshold in both nonexpansion and expansion states. These also indicate that any jump in the density of incomes at 100% FPL is not particularly large in context. We conclude that there is insufficient evidence of precise manipulation of prior year AGI around the Marketplace subsidy threshold during the 2014–2016 period. Nonetheless, we show that our results hold under a doughnut RD specification (Barreca et al., 2011), in which we exclude filers with incomes within 5 percentage points (about the width of the bump) of the threshold (Appendix Table A3, Panel D).

Appendix Fig. A2 offers a visual check of the smoothness of demographic characteristics (covariates) around the threshold. Covariates appear smooth. More formally, in Appendix Table A6, we present the results of local linear regressions that test for locally balanced covariates around 100% FPL. We view the evidence of discontinuities in covariates to be sufficiently weak so as to validate our RD design. Nevertheless, we control for demographic factors in our regressions.[28]

5. Results

This section presents our main empirical results. We begin with an evaluation of insurance crowd-out at the 100% FPL threshold. This is followed by our first-stage estimate of the effect of the subsidy policy on insurance coverage. Third, we document the RD effects of the increase in health insurance under the subsidy program on the

[23] We confirm that some predicted values of treatment lie outside the unit interval. Moreover, the standard errors of our bivariate probit estimates go down with the inclusion of covariates suggesting that this model is well-specified. We also perform a Rao score test (Murphy, 2007), which confirms the null hypothesis of bivariate normality.

[24] This is fortunate since Gelman and Imbens (2016) caution against using higher-order polynomials, recommending that researchers rely primarily on results generated from local linear or quadratic functions of the running variable.

[25] For example, a household that has an income of 95% FPL according to their 2014 tax form may, at the time of enrollment, project an income of 101% FPL for the 2015 coverage year. If the Marketplace accepts that projection, the household will receive subsidies during the coverage year and we would misclassify this household to the control group.

[26] Kucko et al. (2017) report that the excess mass at 100% FPL is attributable to about 26,000 additional returns (representing 1.1% of the 2,336,101 self-employed filers with income in the range 50% to 150% FPL).

[27] We also test for sorting in the 2015 ACS dataset after restricting the ACS sample to resemble our ITT sample. Based on a sample size 57,034 observations, we find no evidence of upward income sorting at the poverty line in the ACS data.

[28] Demographic covariates may help limit bias from including observations some distance from the threshold; and, when demographic factors are correlated with the outcome variable, covariates improve precision (Imbens and Lemieux, 2008; Kim, 2013). Finally, demographic controls help adjust for the higher sampling probability of younger adults (Solon et al., 2015).

probability of a delinquent home payment. Finally, we document a potential channel through which a lack of health insurance might drive a household to miss a rent or mortgage payment.

5.1. Insurance crowd-out

In this subsection, we use our RD design to test whether the Marketplace subsidy policy crowds-out non-group forms of insurance coverage. The subsidy policy was designed *not* to crowd-out other forms of quality coverage as individuals with affordable insurance options are generally ineligible to receive the subsidies (see Section 3.1). Employer plans, for example, are usually considered to be affordable. Even before the ACA, the vast majority of enrollees in employer plans – upwards of 98% – were enrolled in plans with an actuarial value of at least 60% (Yong et al., 2011). To this extent, we would expect crowd-out, if present, to be limited.[29]

Table 2 documents the results of estimating Eq. (2), with indicators for various types of insurance coverage as the dependent variable and treatment assignment at incomes above 100% FPL. All regressions use the full, 2014–2016 sample of households and we run separate regressions by state Medicaid expansion status. The specification with the outcome variable "*Any (excl. non-group private)*," which indicates having a form of health insurance coverage other than non-group private insurance, is likely to capture the aggregate extent of crowd-out. Our preferred bandwidth range, [42%, 165%] FPL, is the MSE-optimal bandwidth for *Any insurance* under a linear specification of the running variable. For robustness, the table also presents local linear and local quadratic RD estimates under a narrower and wider bandwidth, respectively, and tests for treatment effects on the underlying insurance types (e.g., employer insurance).

There is no significant discontinuity in "*Any (excl. non-group private)*" at 100% FPL in the nonexpansion states. The effects for the underlying insurance types are either marginally positive or insignificant, suggesting that there is no crowd-out of other forms of insurance at 100% FPL. One exception is "*Other*" insurance coverage, which exhibits a marginal dip at 100% FPL, although the effect is economically insignificant.[30] The table also indicates that there is no statistically important change at 100% FPL in the prevalence of these insurance types in expansion states. These results also hold in the 2016 subsample (not shown).[31]

In sum, the results do not indicate any significant crowd-out of other forms of insurance at around 100% FPL. This also assuages concern that our tests with the ITT sample could potentially be biased by unobserved heterogeneity around the threshold.

5.2. First stage effects

Fig. 3 shows the share of respondents that report having "any" insurance coverage or "non-group private" insurance coverage within small bins of income in nonexpansion and expansion states, separately. Top panels include the full sample while bottom panels include the ITT subsample — i.e., households that do not report insurance through an employer, family member, school, Medicare or the VA. The ITT sample results are shown for the pooled 2014–2016 sample as well as for just the 2016 sample. There is clear visual evidence of a roughly 4 percentage point jump in the share of the full sample from nonexpansion states that report any insurance (Panel a) as well as non-group private insurance (Panel c). Within the ITT subsample, the discontinuity in the non-group private insurance share is larger — approximately 10 percentage points during 2014–2016 (Panel e) and 13 percentage points in 2016 (Panel g).[32]

In the states that expanded Medicaid, most participants with incomes around 100% FPL should qualify for Medicaid and be ineligible for Marketplace subsidies. As expected, not only is the prevalence of non-group private insurance much lower in these states but there is no discontinuity around 100% FPL (Panels d, f, and h). Instead, the non-group private share begins to increase at around 138% FPL (Panel h), the upper limit for Medicaid eligibility and the floor for Marketplace subsidies in many (but not all) expansion states.[33] This further reinforces the conclusion that the discontinuity in the probability of non-group private insurance in nonexpansion states at 100% FPL is likely due to federal subsidy and not due to other unobserved factors.

In Table 3, we present the results of estimating Eq. (2), where the dependent variable is either an indicator of having "*Any*" insurance or of having "*Non-group private*" insurance. The discontinuity in *Any* insurance coverage at 100% FPL, shown in Panel A (rows 1–6), is statistically significant in nonexpansion states in all but the narrowest sample specification (row 1). Our preferred specification is that of the second row, which corresponds to the MSE-optimal bandwidth for *Any* insurance, [42%, 165%] FPL. The estimate of π in this row suggests that the subsidy threshold is associated with a 4.0 percentage point increase in the share of the sample from nonexpansion states that report having any insurance coverage. Relative to the share insured to the left of the threshold (72%), this is a 6% increase in the insurance rate. Note, however, that the F-statistic is at or above 10 (10.03) only in row 2 (Stock et al., 2002), which suggests that the subsidy threshold is not a particularly strong instrument for *Any* insurance coverage in the full sample.

The subsidy threshold does appear to be a somewhat stronger instrument for *Non-group private insurance* coverage in nonexpansion states. The F-statistic for the intermediate bandwidth specification rises to 19.00 (Panel A, row 8). Correspondingly, the rate of *Non-group private* insurance jumps by about 3.7 percentage points at the threshold. In relative terms, this effect is quite large, indicating a 35% increase in non-group private coverage at the subsidy threshold.

Among households targeted by the subsidy policy (the ITT subsample), the share with non-group private insurance rises by 9.4 percentage points at the threshold or 46% in relative terms (Panel B, row 2). This effect increases to 14.1 percentage points in 2016 (Panel C, row 2). The strength of the instrument also grows, with an

[29] The one exception is with regard to high-deductible non-group plans, which we cannot differentiate from other non-group private insurance plans based on the survey question.

[30] The small drop in people reporting "other insurance" in nonexpansion states is largely confined to the 2014 survey (not shown). One possibility is that people with catastrophic or COBRA plans may have reported "other" rather than "non-group private" insurance. In other words, this might be evidence of the crowd-out of high-deductible non-group plans that we would expect to find had the survey explicitly asked about them. Regardless, the effect is too small and inconsistent across years to be of much interest.

[31] This lack of statistical evidence of crowd-out is visually confirmed in Appendix Figs. A4 (2014–2016) and A5 (2016 only). These graphs plot the share of the sample reporting a particular insurance type within small bins of income around the subsidy cutoff. Most importantly, the relationship between income and employer insurance is smooth at the threshold.

[32] Appendix Fig. A3 shows these result broken out for each year. They indicate a growing discontinuity in *Non-group private* insurance relative to the discontinuity in *Any* insurance, over time. As discussed in Section 3.1, this is likely due to improvements in the accuracy of self-reported insurance types as people learn that healthcare.gov actually sells private plans. The implication is that the 100% FPL threshold is a better instrument for *Non-group private* insurance in 2016 relative to earlier years, which is consistent with the F-statistics in Panels B and C of Table 3.

[33] As discussed in Footnote 10, the 138% FPL threshold is not a clean break point between Medicaid and Marketplace subsidies in expansion states, which might explain the absence of a sharper discontinuity in take-up of non-group private insurance at 138% FPL in expansion states (Fig. 3). Even if there were clear evidence of substitution between Medicaid and Marketplace insurance at 138% FPL, its expected effect on financial well-being is not clear.

Table 2
Crowd-out of other insurance types at the Marketplace subsidy threshold.

Dependent variable (insurance type)	Nonexpansion states			Expansion states				
	Coeff. (π)			Coeff. (π)				
	Inc ≥ 100%	Std. err.	N	*Inc ≥ 100%*	Std. err.	N	Bandwidth	*P*
Any (excl. non-group private)	−0.017	(0.026)	4884	0.029	(0.022)	7157	[69, 125]	1
	0.002	(0.015)	10,395	0.020	(0.013)	15,399	[42, 165]	1
	0.002	(0.021)	15,967	0.028*	(0.016)	24,531	(0, 200)	2
Employer	0.002	(0.020)	10,395	0.028*	(0.015)	15,399	[42, 165]	1
Family member	0.033*	(0.018)	10,395	0.014	(0.013)	15,399	[42, 165]	1
Medicaid	−0.024	(0.017)	10,395	−0.010	(0.014)	15,399	[42, 165]	1
Medicare	−0.005	(0.004)	10,395	−0.004	(0.003)	15,399	[42, 165]	1
VA	−0.002	(0.005)	10,395	−0.001	(0.003)	15,399	[42, 165]	1
Student	0.005	(0.006)	10,395	−0.007	(0.005)	15,399	[42, 165]	1
Other	−0.006*	(0.004)	10,395	−0.000	(0.003)	15,399	[42, 165]	1

These are RD treatment-effect (Income≥100%) estimates (Eq. (2)) on the full, 2014–16, sample of households. The dependent variables are indicators of various types of insurance coverage. The running variable (income as a percentage of FPL) is allowed to vary on both sides of the cutoff and includes a polynomial of order (*P*). The intermediate regression window, [42%, 165%] FPL, corresponds to the MSE-optimal bandwidth (below and above the cutoff) for *Any insurance*. Results are also shown for a narrower window, [69%, 125%] FPL, which is the optimal bandwidth for *Home delinquency*, and for a chosen upper bound window, (0%, 200%) FPL. Regressions include demographic covariates and state-year fixed effects. Standard errors, clustered on state, are shown in parentheses: $^{*}p = 0.1$; $^{**}p = 0.05$; $^{***}p = 0.01$ (statistically significant).

F-statistic of 20.54 in the second row of Panel C. As expected, there is no corresponding treatment effect in expansion states.

Given the potential noise in our assignment variable (see Section 3.2) and the measurement error in our survey responses, these results serve as a strong check on the precision of our treatment identifier, *Income* ≥ 100%. In sum, the discontinuity in non-group private insurance coverage in nonexpansion states is sizable and offers a source of variation in insurance that we exploit

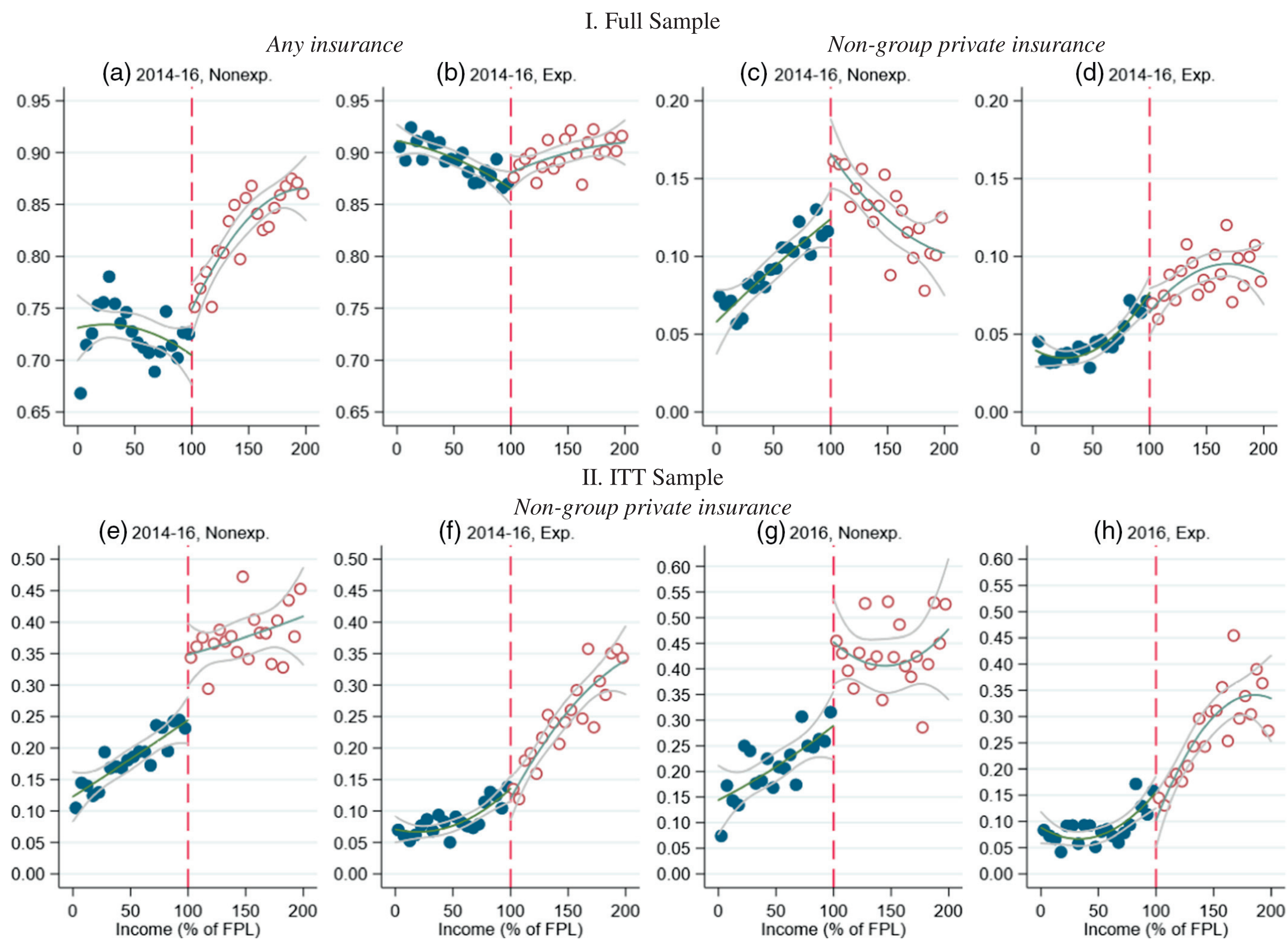

Fig. 3. Discontinuity in insurance coverage at the subsidy threshold. Graphs show the share of respondents within 5 percentage point bins of income that report "any" insurance or "non-group private" insurance coverage around the subsidy threshold (x-axis). Fit lines are based on a quadratic function of income. Outside lines represent the 95% confidence intervals around the bin mean. Data include households with incomes in the range (0%, 200%) FPL. Panel I summarizes the full sample of households. Panel II summarizes the intent-to-treat (ITT) sample (i.e., households without coverage through an employer, school, VA, Medicare, or family member). The sample for each graph is restricted by the year of the sample (i.e., 2014–2016 or 2016) and by whether the state of residence did ("Exp.") or did not ("Nonexp.") expand Medicaid.

Table 3
First stage impact of the Marketplace subsidy threshold on insurance coverage.

Dependent variable (insurance type)	Coeff. (π)								
	Inc ≥ 100%	Std. err.	F-stat	Mean to left	Rel. effect	State sample	N	Bandwidth	*P*
A. Full sample; 2014–2016									
Any	0.028	(0.021)	1.74	0.72	0.04	Nonexp.	4884	[69, 125]	1
	0.040***	(0.012)	10.03	0.72	0.06	Nonexp.	10,395	[42, 165]	1
	0.044***	(0.016)	7.85	0.73	0.06	Nonexp.	15,967	(0, 200)	2
	0.013	(0.019)	0.45	0.88	0.01	Exp.	7157	[69, 125]	1
	0.016	(0.012)	1.93	0.88	0.02	Exp.	15,399	[42, 165]	1
	0.016	(0.014)	1.36	0.89	0.02	Exp.	24,531	(0, 200)	2
Non-group private	0.045**	(0.018)	6.13	0.11	0.40	Nonexp.	4884	[69, 125]	1
	0.037***	(0.009)	19.00	0.11	0.35	Nonexp.	10,395	[42, 165]	1
	0.042***	(0.012)	12.39	0.10	0.44	Nonexp.	15,967	(0, 200)	2
	−0.016	(0.010)	2.74	0.06	−0.26	Exp.	7157	[69, 125]	1
	−0.003	(0.009)	0.13	0.05	−0.06	Exp.	15,399	[42, 165]	1
	−0.012	(0.010)	1.66	0.05	−0.26	Exp.	24531	(0, 200)	2
B. ITT subsample; 2014–2016									
Non-group private	0.096**	(0.039)	5.89	0.23	0.42	Nonexp.	2169	[69, 125]	1
	0.094***	(0.022)	18.00	0.21	0.46	Nonexp.	4380	[42, 165]	1
	0.101***	(0.023)	19.06	0.19	0.53	Nonexp.	6443	(0, 200)	2
	−0.024	(0.022)	1.21	0.11	−0.21	Exp.	3407	[69, 125]	1
	−0.001	(0.021)	0.00	0.09	−0.01	Exp.	6886	[42, 165]	1
	−0.014	(0.019)	0.56	0.09	−0.16	Exp.	10,390	(0, 200)	2
C. ITT subsample; 2016									
Non-group private	0.153**	(0.074)	4.31	0.27	0.57	Nonexp.	763	[69, 125]	1
	0.141***	(0.031)	20.54	0.23	0.60	Nonexp.	1595	[42, 165]	1
	0.166***	(0.032)	26.83	0.22	0.75	Nonexp.	2382	(0, 200)	2
	−0.046	(0.039)	1.35	0.12	−0.38	Exp.	1573	[69, 125]	1
	−0.019	(0.031)	0.38	0.10	−0.20	Exp.	3131	[42, 165]	1
	−0.051	(0.034)	2.30	0.09	−0.55	Exp.	4779	(0, 200)	2

These are first-stage RD estimates (Eq. (2)). The dependent variables are indicators of "any" insurance coverage (Panel A) and "non-group private" insurance coverage (Panels B–C). The sample includes respondents living in nonexpansion and expansion states. Panels A uses the full sample while Panels B and C use the intent-to-treat (ITT) sample (i.e., households without coverage through an employer, school, VA, Medicare, or family member). Treatment is assigned at incomes above 100% FPL. The running variable (income as a percentage of FPL) is allowed to vary on both sides of the cutoff and includes a polynomial of order (*P*). The intermediate regression window, [42%, 165%] FPL, corresponds to the MSE-optimal bandwidth (below and above the cutoff) for *Any insurance*. Results are also shown for a narrower window, [69%, 125%] FPL, which is the optimal bandwidth for *Home delinquency*, and for a chosen upper bound window, (0%, 200%) FPL. Regressions include demographic covariates and state-year fixed effects. The Kleibergen-Paap F-stat (weak instrument test) from a 2SLS IV regression with home delinquency as the final outcome variable is shown beside each regression estimate, as is the mean of the dependent variable to the left of 100% FPL and the implied relative effect of the coefficient. Standard errors, clustered on state, are shown in parentheses: $^{*}p = 0.1$; $^{**}p = 0.05$; $^{***}p = 0.01$ (statistically significant).

to establish the effect of subsidized health insurance on home delinquency.

5.3. Main results: insurance and home delinquency rates

In Fig. 4, we plot the average level of *Home delinquency* within bins of income around the threshold. Consistent with expanded health insurance access reducing home delinquency, at the poverty line, the prevalence of delinquent home payments among the households in nonexpansion states falls by between 4 and 10 percentage points, depending on the sample cut [Panels a, c, and e]. This effect expands in Panel (g) to roughly 14 percentage points when the 2016 ITT sample is restricted to households that we identify as having a higher likelihood of health problems (*HealthProb* $= 1$). As expected, no such discontinuity is apparent in states that expanded Medicaid (panels b, d, f, and h). Put together, these graphs signal a possible causal relationship between health insurance and financial hardship.

In Table 4, we present our reduced form estimates. Recall, τ in Eq. (1) captures the effect of being income eligible for the subsidies, on *Home delinquency*, regardless of enrollment status. Panel A presents the results in nonexpansion states. Within the full sample, our estimates indicate that *Home delinquency* is 2.4–7.8 percentage points lower among households with income above 100% FPL as compared to households with income less than 100% FPL. This effect is only statistically significant, however, in 2016, coinciding with when the discontinuity in non-group private insurance is the largest (Table 3). In 2016, the 7.2 percentage point decline in the home delinquency share in column (4) corresponds to a relative 30% decline in the home delinquency rate. Within the ITT sample, the threshold effect is larger and statistically strong in all specifications, ranging from 8.3 to 14.7 percentage points. As compared to the probability of *Home delinquency* among households with income just below the threshold, our estimates indicate a 25–42 % decline in the rate of home delinquency. Furthermore, Panel B shows that such effects are not present in states that expanded Medicaid.

Although there is compelling visual and statistical evidence of a discontinuous decline in the rate of home delinquency at the subsidy threshold, we note that our estimates are quite imprecise. As documented at the bottom of Panel A, the implied treatment effect, relative to the mean delinquency rate, ranges from −9% to −42%, depending on the sample and specification. Moreover, the 95% confidence interval around the relative treatment effect ranges from +4% to −54% for the full sample and from −2% to −78% for the ITT sample. This should be taken into account when interpreting our point estimates.

Through the interaction with *HealthProb*, columns (5) and (10) document that this effect is concentrated among households that potentially face some health problems. Recall, however, that we do not have good indicators of health status in the survey and must, therefore, rely on financial proxies such as the presence of medical debt (see Section 3.2). Still, this finding is the first indication that health insurance might mitigate home delinquency, in part, by reducing health expenditures.

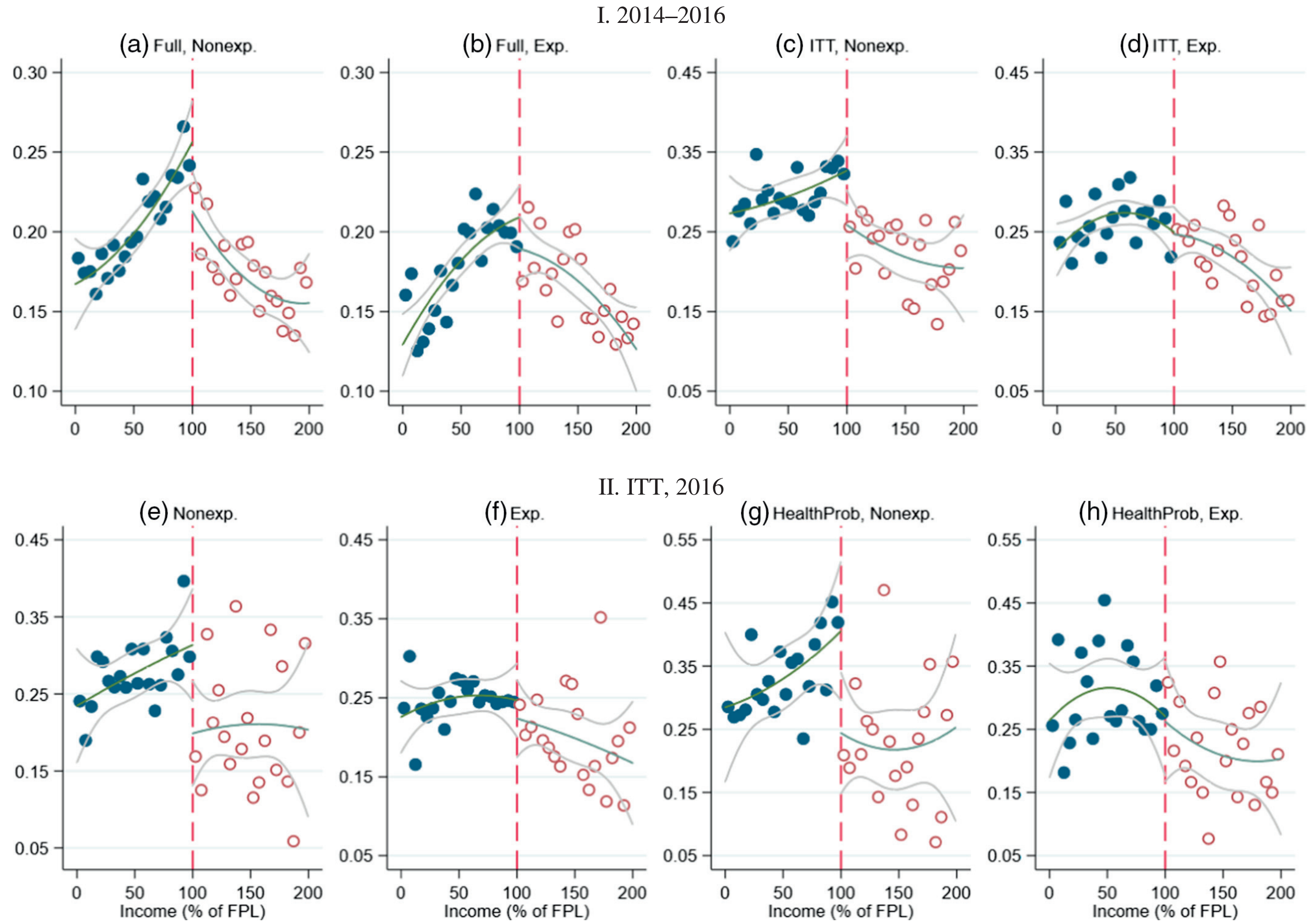

Fig. 4. Discontinuity in home delinquency at the subsidy threshold. Graphs show the mean share of respondents within 5 percentage point bins of income that report a delinquent rent or mortgage payment (home delinquency) around the subsidy threshold (x-axis). Fit lines are based on a quadratic function of income. Outside lines represent the 95% confidence intervals around the bin mean. Data include households with incomes in the range (0%, 200%) FPL. As noted above each graph, the figure separately summarizes the pooled 2014–2016 period (Panel I), as well as the 2016 period only (Panel II). In addition, the figure separately summarizes the full sample and the intent-to-treat (ITT) sample (i.e., households without coverage through an employer, school, VA, Medicare, or family member), The sample for each graph is also restricted by whether the state of residence did ("Exp.") or did not ("Nonexp.") expand Medicaid. Panels (g) and (h) further restrict the 2016 ITT sample to households that are more likely to have experienced a recent health problem (*HealthProb* $=1$).

A unique feature of our data is our ability to identify rental delinquency. Exploiting this, Appendix Table A7 reports the estimates of τ for renters and owners separately.[34] Because our reduced form effects are strongest in 2016 (Table 4), we focus on the 2016 sample. We also remove any household that does not either rent or own. The bottom row of the table reports *p*-values from an F-test for the equality of reported coefficients. While the discontinuity in home delinquency appears to be statistically concentrated in the sample of renters, the difference in coefficients is never statistically significant. This indicates that the subsidy policy likely affects the home payment delinquency rates of both renters and owners to some extent.

In Table 5, we present fuzzy RD estimates, generated from a bivariate probit model and the 2014–2016 sample in nonexpansion states. The negative and significant coefficient on *Any insurance* (our estimate of γ) is consistent with subsidized health insurance reducing *Home delinquency*. Estimates are economically significant. Our preferred specification is in column (2), which corresponds to the specification with the largest first-stage F-statistic in the full sample with *Any insurance* as the endogenous variable (see Table 3). The average marginal effect estimate [$\gamma = -0.124$] signals that going from being uninsured to having health insurance due to crossing the subsidy threshold lowers the probability of reporting a recent *Home delinquency* by 12.4 percentage points. When *Non-group private insurance* is the endogenous outcome variable (columns 4–6) and the ITT subsample is used (columns 7–9), the treatment effect roughly doubles and triples, respectively. The implication is that going from being uninsured to receiving subsidized health insurance has a profound impact on the likelihood of making timely home payments. This is particularly true for households in the ITT sample, which lack employer plans and tend to have a higher base rate of home delinquency near the subsidy threshold (33%).

Given possible bias in the IV estimate, we caution against putting too much stock in the precision of these point estimates. One should view the fuzzy RD estimates merely as supporting a causal interpretation of the reduced form relationship documented earlier. Measurement error from our method of assigning households to treatment (i.e., based on past rather than projected income) will bias both our first stage and reduced form regression estimates toward zero. Ideally, the two biases could cancel each other out in the

[34] Note that if the subsidy policy affects a household's housing status, then splitting the sample on housing status will affect the interpretation of our estimates in Appendix Table A7. In unreported tests, we test for discontinuities in the rates of homeownership, renting, and living rent-free at the subsidy threshold in the 2016 full and ITT samples and find no significant treatment effects.

Table 4
Reduced form RD estimates of the effect of the subsidy threshold on home delinquency.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Panel A. Nonexpansion states; dependent variable: *Home delinquency*										
Income ≥ 100%	−0.024	−0.027	−0.078***	−0.072**	−0.049	−0.083**	−0.095**	−0.132**	−0.147**	−0.096
	(0.017)	(0.020)	(0.026)	(0.028)	(0.030)	(0.034)	(0.035)	(0.060)	(0.059)	(0.058)
Income ≥ 100% × HealthProb					−0.054**					−0.089***
					(0.024)					(0.029)
HealthProb					0.088***					0.120***
					(0.019)					(0.029)
N	4884	10,395	1826	3923	3923	2169	4380	763	1595	1595
Mean to left	0.25	0.25	0.24	0.24	0.29	0.33	0.33	0.35	0.35	0.44
Relative effect (%)	−0.09	−0.11	−0.33	−0.30	−0.22	−0.25	−0.29	−0.38	−0.42	−0.31
Rel. eff. 95%CI	[−0.23, 0.04]	[−0.27, 0.06]	[−0.54, −0.11]	[−0.54, −0.06]	[−0.74, 0.03]	[−0.46, −0.04]	[−0.50, −0.07]	[−0.74, −0.02]	[−0.78, −0.07]	[−0.85, 0.00]
Panel B. Expansion states; dependent variable: *Home delinquency*										
Income ≥ 100%	−0.009	−0.007	−0.033	−0.034	−0.032	0.003	0.002	−0.031	−0.042	−0.028
	(0.017)	(0.016)	(0.035)	(0.033)	(0.030)	(0.034)	(0.030)	(0.057)	(0.052)	(0.048)
Income ≥ 100% × HealthProb					−0.005					−0.042
					(0.023)					(0.025)
HealthProb					0.037**					0.077***
					(0.017)					(0.024)
N	7157	15,399	3233	6891	6891	3407	6886	1573	3131	3131
Sample	Full 2014–16	Full 2014–16	Full 2016	Full 2016	Full 2016	ITT 2014–16	ITT 2014–16	ITT 2016	ITT 2016	ITT 2016
Bandwidth	[69, 125]	[42, 165]	[69, 125]	[42, 165]	[42, 165]	[69, 125]	[42, 165]	[69, 125]	[42, 165]	[42, 165]
P	1	2	1	2	2	1	2	1	2	2

Table shows reduced form RD estimates for respondents living in nonexpansion (Panel A) and expansion states (Panel B). The dependent variable, *Home delinquency*, is binary and indicates a delinquent housing payment. Treatment is assigned at incomes above 100% FPL (i.e., "Income ≥ 100%" = 1). The running variable (income as a percentage of FPL) is allowed to vary on both sides of the cutoff and includes a polynomial of order (*P*). The smallest regression window, [69%, 125%] FPL, corresponds to the MSE-optimal bandwidth (below and above the cutoff) under a local linear RD estimator for *Home delinquency*. Local quadratic estimates are shown for the window [42%, 165%] FPL (which is the optimal bandwidth for *Any insurance*). Regressions include demographic covariates and state-year fixed effects. *HealthProb* (available in 2016 only) is an indicator that the household is more likely to have experienced health problems. Panel A reports the mean of *Home delinquency* within the 90%–100% FPL bin, the implied treatment effect relative to that mean, and the implied 95% confidence interval (CI) on the relative treatment effect. In columns (5) and (10), the mean is calculated for households with *HealthProb* $= 1$ and the treatment effect is the sum of the two coefficients (and confidence intervals) on *Income ≥ 100%*. Standard errors, clustered on state, are shown in parentheses: $^{*}p = 0.1$; $^{**}p = 0.05$; $^{***}p = 0.01$ (statistically significant).

fuzzy RD estimator. However, if the error with respect to predicting insurance status is greater than the error with respect to predicting home delinquency, then our second stage effect would be inflated (Pei and Shen, 2016).

To summarize, eligibility for subsidized Marketplace plans – reflecting a change in the likelihood of coverage as well as in the cost/benefits of available plans – is associated with a meaningful decline in the rate of *Home delinquency*. Effects are largest in 2016 (7.2 percentage points or 30%) and in the subsample of households without access to alternative forms of affordable coverage (8.3 percentage points or 25%). IV results suggest that an extensive margin increase in health insurance at the subsidy threshold has a large effect

Table 5
Fuzzy RD estimates of the effect of insurance on home delinquency (bivariate probit).

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Final outcome:	*Home delinquency*								
Insurance	−0.495*	−0.470**	−0.390**	−0.909**	−0.803***	−0.711***	−1.548***	−1.205***	−1.068***
	(0.284)	(0.187)	(0.177)	(0.420)	(0.279)	(0.218)	(0.537)	(0.301)	(0.366)
	[−0.134]	[−0.124]	[−0.098]	[−0.257]	[−0.219]	[−0.185]	[−0.491]	[−0.383]	[−0.337]
Endog. outcome:	*Any insurance*			*Non-group private insurance*					
Income ≥ 100%	0.093*	0.145***	0.153***	0.222**	0.177***	0.212***	0.295*	0.231***	0.251***
	(0.055)	(0.039)	(0.049)	(0.093)	(0.034)	(0.044)	(0.154)	(0.066)	(0.063)
	[0.028]	[0.045]	[0.046]	[0.047]	[0.036]	[0.039]	[0.097]	[0.074]	[0.076]
Sample	Full	Full	Full	Full	Full	Full	ITT	ITT	ITT
P	1	1	2	1	1	2	1	1	2
Bandwidth	[69, 125]	[42, 165]	(0, 200)	[69, 125]	[42, 165]	(0, 200)	[69, 125]	[42, 165]	(0, 200)
N	4884	10,395	15,967	4884	10,395	15,967	2169	4380	6443

Table shows fuzzy RD estimates, based on a bivariate probit model, for the 2014–2016 sample living in nonexpansion states. The dependent variable, *Home delinquency*, is binary and indicates a delinquent housing payment. The key explanatory variables are *Any insurance* coverage and *Non-group private insurance* coverage, which are instrumented by incomes of above 100% FPL (i.e., "Income ≥ 100%" = 1). The running variable (income as a percentage of FPL) is allowed to vary on both sides of the cutoff and includes a polynomial of order (*P*). Regressions include demographic covariates and state-year fixed effects. Average marginal effects, holding all variables at their means, are shown in brackets. Results are shown for a small regression window, [69%, 125%] FPL, which corresponds to the MSE-optimal bandwidth (below and above the cutoff) for *Home delinquency*, an intermediate window, [42%, 165%] FPL, which is the optimal bandwidth for *Any insurance*, and for a chosen upper bound, (0%, 200%) FPL. Standard errors, shown in parentheses, are calculated by state-based block bootstrap: $^{*}p = 0.1$; $^{**}p = 0.05$; $^{***}p = 0.01$ (statistically significant).

Table 6
Reduced form RD estimates in late expanding states (Alaska, Indiana, Montana).

Dependent variable: *Home delinquency*	(1)	(2)	(3)	(4)	(5)	(6)
Income≥100%	−0.052	−0.020	−0.046	−0.081	−0.048	−0.057
	(0.041)	(0.036)	(0.035)	(0.082)	(0.076)	(0.102)
Income≥100% × PreExp	−0.158**	−0.076**	−0.072**	−0.206**	−0.117*	−0.130**
	(0.056)	(0.023)	(0.028)	(0.066)	(0.051)	(0.048)
PreExp	0.118***	0.096***	0.107***	0.111***	0.095***	0.136***
	(0.032)	(0.014)	(0.016)	(0.025)	(0.013)	(0.028)
Sample	Full	Full	Full	ITT	ITT	ITT
Bandwidth	[69, 125]	[42, 165]	(0, 200)	[69, 125]	[42, 165]	(0, 200)
P	1	2	3	1	2	3
N	590	1218	1838	264	500	709

Table shows reduced form difference-in-difference RD estimates for respondents living in three "late-expanding" states during the 2014–2016 period. These are the three states that expanded after the start of the 2015 tax season but before the start of the 2016 tax season (Alaska 9/1/2015; Indiana 2/1/2015; Montana 1/1/2016). The dependent variable, *Home delinquency*, is binary and indicates a delinquent housing payment. Treatment is assigned at incomes above 100% FPL (i.e., "Income ≥ 100%" = 1) in the periods pre-Medicaid expansion (2014 and 2015). In the case of Alaska, parents are assigned to the control condition (never associated with a "pre" expansion indicator) because Alaska is one of the three "nonexpansion" states that offered Medicaid to parents earning above the poverty line since 2014. Regressions control only for the running variable (income as a percentage of FPL), which is allowed to vary on both sides of the cutoff, and state fixed effects. The smallest regression window, [69%, 125%] FPL, corresponds to the MSE-optimal bandwidth (below and above the cutoff) under a local linear RD estimator for *Home delinquency*. Local quadratic and cubic estimates, respectively, are shown for an intermediate window, [42%, 165%] FPL (which is the optimal bandwidth for *Any insurance*), and for a wider window, (0%, 200%) FPL. Robust standard errors are in parentheses. $^{*}p = 0.1$; $^{**}p = 0.05$; $^{***}p = 0.01$ (statistically significant).

on *Home delinquency*. We find no robust evidence of a differential impact of the subsidy policy on renters as opposed to homeowners.

5.4. Robustness

In this subsection, we discuss the results of a number of robustness tests.[35]

First, we exploit the fact that three states – Alaska, Indiana, and Montana – expanded Medicaid after the start of tax-time 2015 but before the start of tax-time in 2016. If access to affordable health insurance is what drives the discontinuity in home delinquency at 100% FPL, then we would not expect to find a discontinuity after Medicaid expansion in these states. Table 6 documents the test of this conjecture. The sample is restricted to households in the three "late-expansion" states. We interact with our treatment identifier, *Income* ≥ 100%, an indicator that identifies households sampled in the pre-expansion period, *PreExp* (2014–2015). In all specifications, the coefficient on the interaction term is negative and significant, ranging from 7.2 to 20.6 percentage points, while that of *Income* ≥ *100%* is insignificantly different from zero. Consistent with our expectation, these estimates signal that the reduction in home delinquency at 100% FPL is because of access to subsidized health insurance.

Finally, we allow the data to objectively choose the location of the threshold using a search technique put forth in Hansen (2000). This has obvious advantages over arbitrarily testing placebo thresholds (e.g., 75% FPL). In particular, we estimate $Y_i = a_i + d1[Z_i > c*] + g(Z_i - c*) + 1[Z_i > c*] \times g(Z_i - c*) + e_i$ for $0 < c_i < 200\%$ FPL. We allow the dependent variable to be either *Non-group private insurance* or *Home delinquency* and $g(Z_i - c*)$ to be a quadratic function of income. We look for the value of $c*$ – tested at each 1 percentage point of income – that maximizes the R-squared of this above equation. Sample size is important for this procedure to not select obvious outliers, thus we run the analysis only on the full, 2014–2016 sample. Results are presented in Appendix Fig. A6. This procedure clearly selects 100% FPL as the threshold point in the relationship between *Non-group private insurance* and income. With respect to *Home delinquency*, the procedure selects a range around 100% FPL (from 83% to 105%) as the location of the threshold, which is a fairly remarkable result given the weakness of our reduced form estimates within the full, 2014–2016 sample.

We would like to note that in our sample, the home delinquency rate peaks just to the left of the subsidy threshold, at around 90% FPL, regardless of state of residence or health insurance status (see Fig. 4). If the treatment effect exhibits a similar heterogeneity, then one should exercise caution in extrapolating our estimates to other points in the income distribution.

5.5. Exploring a mechanism: medical expenditure shocks

There are several possible channels through which insurance coverage may, in the short-term, influence a household's decision to fall behind on a home payment. The channel we focus on in this paper is what we refer to as the "medical expenditure shock" channel. By limiting medical expenses consequent to health shocks, health insurance may protect a household's liquid assets from health shocks. Presumably this, in turn, will increase the household's ability to keep up with household payments. This channel implies a testable prediction: insurance should reduce extreme values of medical expenditure shocks.[36] We test this prediction in this section.

We estimate the impact of the Marketplace subsidy policy on the distribution of medical expenditures by estimating quantile treatment effects á la Finkelstein and McKnight (2008). Fig. 5 plots quantile regression estimates from the reduced form model in Eq. (1) based on the ITT subsample of households surveyed in 2014–2016. The graphs capture the impact of subsidy eligibility on different percentiles of *Medical spending ($)* in nonexpansion and expansion states, separately. Subsidy eligibility begins to reduce the dollar

[35] On top of the robustness checks described in this subsection, Appendix Table A3 presents reduced form RD estimates after excluding four types of households that could have confounding influences on our estimate of τ. Results are largely unaffected. In addition, Appendix Table A8 shows estimates generated from the bias-corrected local polynomial RD estimator and confidence intervals of Calonico et al. (2014) under both a uniform and triangular kernel. Estimates presented in our main analysis are similar in magnitude and statistical significance.

[36] If access to uncompensated care (through hospital emergency rooms, for example) permits low-income households to fully avoid large medical expenditures, then we might expect health insurance status to have little effect on health expenditure risk. To test this conjecture, in Appendix Fig. A7, we analyze data from the 2016 Medical Expenditure Panel Survey (MEPS). We show that lower-income households are, in fact, subject to substantial health expenditure risk. Uninsured individuals earning under 100% FPL have by far the largest standard deviation in medical spending of all income groups at $3,878. Moreover, the gap between the expenditure risk of the uninsured ($3,878) versus the insured ($1,569) is largest among individuals earning under 100% FPL.

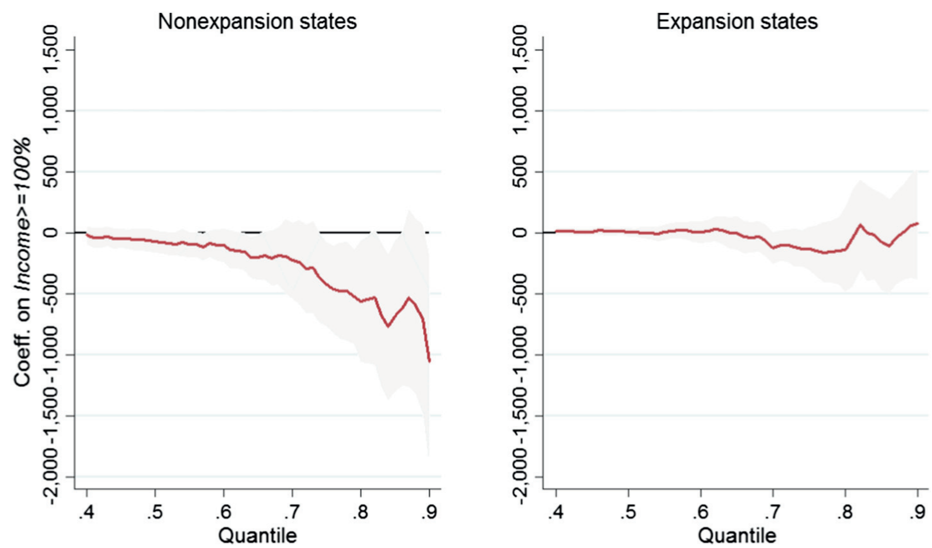

Fig. 5. Quantile regression estimates of the reduced form effect of the subsidy threshold on out-of-pocket medical spending. Figure shows the predicted effect of the subsidy threshold on out-of-pocket medical expenditures (in dollars) at different quantiles. The figure shows the coefficient estimates at every 2nd quantile, from the 40th to the 90th. The shaded 90% CI is recovered from bootstrapping. Data include the intent-to-treat sample of households from nonexpansion states with incomes in the range (0%, 200%) FPL. Quantile estimation of the reduced form Eq. (1) includes a quadratic polynomial ($P = 2$) and demographic covariates.

value of out-of-pocket spending at around the 60th percentile of the spending distribution. The estimated decline in out-of-pocket spending is monotonic until the 90th percentile (when estimates become too noisy to interpret and are therefore suppressed). At the 90th percentile, the subsidy policy is associated with an average decline of \$1,054 per household (p-value = 0.045) or 73% relative to the control mean. These results are remarkably similar to those presented in studies of Medicare and Medicaid (Finkelstein and McKnight, 2008; Finkelstein et al., 2012; Barcellos and Jacobson, 2015).[37] We observe no such effect in expansion states.[38]

In sum, the evidence in this section highlights that subsidized Marketplace insurance has a significant moderating role on large medical expenditure shocks among households targeted by the subsidy policy. This is likely an important channel through which subsidized insurance affects delinquency.

6. Discussion

In this section, we use a simple framework to estimate the benefits associated with fewer home payment delinquencies and compare it to the direct transfer costs of the subsidies. Let $P(D|S)$ and $P(D|-S)$ denote the probability of delinquency with and without access to subsidized Marketplace insurance, respectively. Let $E[C^D]$ denote the total expected cost of a delinquency to the participant and to the larger society. Thus, the benefit from lower delinquency can be represented as: $[P(D|S)\text{-}P(D|-S)] \times E[C^D]$.

Our most conservative estimate of τ in the ITT sample (Table 4, column 6) gives us an estimate of $[P(D|-S) - P(D|S)] = 0.083$. If delinquencies are due to liquidity shocks resulting from health shocks and if health shocks arrive at a uniform Poisson frequency, then the number (or probability) of shocks and, hence, the number (or probability) of potential delinquencies should increase in an approximately linear fashion with time. If health insurance prevents delinquencies caused by health shocks, then the extent of reduction in delinquency should linearly increase with time. Therefore, we annualize the fall in delinquency rate by multiplying our six-month estimate by two. To capture the average cost of delinquency among both renters and owners, we decompose the expected cost of a delinquency, $E[C^D]$ as follows:

$$E[C^D] = P(R|D) \times P(E|D^R) \times C^E + P(O|D) \times P(F|D^O) \times C^F$$

where $P(R|D)$ is the probability that the delinquency is of a renter; $P(E|D^R)$ is the probability of eviction given a rental delinquency; C^E is the total cost of an eviction; $P(O|D)$ is the probability of mortgage delinquency by an owner; $P(F|D^O)$ is the probability of a foreclosure given mortgage delinquency; and C^F is the total cost of a foreclosure. In our sample, we find $P(R|D)$ to be 0.81 and $P(O|D)$ to be 0.19. The other components of this equation must be estimated from a variety of data sources discussed below. Additional information on these datasets and our estimation assumptions for each component are presented in Appendix C.

First, we use loan-level performance data from Fannie Mae to generate the probability of transitioning from a delinquency to a foreclosure completion, $P(F|D^O)$. In particular, we take all mortgage loans with a delinquency at some point in 2014 and calculate the probability of a foreclosure completion on that mortgage by the end of 2016 (our period of interest). Based on this method, we set $P(F|D^O) = 0.18$.[39]

Although we are not aware of any formal estimates of the total costs of a foreclosure to society, C^F, there are estimates of some of its components. We begin by calculating the average loss on first-lien mortgages during foreclosure. Fannie Mae reports a loss severity of 46.3% on loans with disposition dates during 2014–2016 (Fannie Mae, 2018). We apply this loss rate to the median unpaid balance

[37] For example, Barcellos and Jacobson (2015) estimate that at age-65 and the 90th percentile of the spending distribution eligibility for Medicare was associated an \$865 (36% relative to the mean) decline in medical expenditures based on 2007–2010 data. Finkelstein and McKnight (2008) estimate that, for the top quartile of out-of-pocket medical spending, "the introduction of Medicare was associated with a 40% decline in out-of-pocket spending by 1970, relative to pre-Medicare levels."

[38] As a robustness check, we verify that health insurance has a differential effect on medical expenditure shocks for ITT households subject to health problems in Appendix Table A10.

[39] This estimate is comparable to the foreclosure completion transition rate used in Hsu et al. (2018), which they estimate to be 20% based on the 2010 and 2012 waves of the National Longitudinal Survey of Youth (see their online Appendix Table A14).

Table 7
Benefits of lower home delinquency relative to the expected subsidy.

Cost of Eviction	\$5,000	\$10,000	\$15,000	\$20,000
Benefits due to lower delinquency	\$441	\$521	\$602	\$683
Transfer cost of subsidy	\$1,607	\$1,607	\$1,607	\$1,607
Delinquency benefit (%)	27%	32%	37%	42%

Table provides estimates of the expected social benefit from lower home delinquency (per eligible person) due access to subsidies. For context, this value is compared to the expected transfer cost of the subsidy (per eligible person). This is done at different hypothetical social costs of eviction.

in our sample, giving a \$34,669 lender loss on the average first-lien mortgage. To this figure, we add an estimate of the neighborhood spillover effect. Campbell et al. (2011) estimate a 1% price discount on properties located within a 300 feet radius from the foreclosure. This implies an aggregate loss on nearby properties of \$28,727 giving a total cost of a foreclosure of, $C^F = \$63{,}397$.

It is important to note that our estimate of C^F likely includes both real dead-weight loss and externalities that are pecuniary. For example, part of the loss on the first-lien mortgage may be pecuniary due to a fire-sale of the property. On the other hand, the loss in the value of nearby properties may arise from the reduced attractiveness of the neighborhood and represent real externalities. As noted in Campbell et al. (2011), discounts on home values can represent both a transfer to the buyer and a change in the fundamental value of the property.[40] We are unable to make this distinction when estimating C^F.

Unfortunately, data on rent delinquencies and subsequent evictions are largely nonexistent since many, if not the majority, of such occurrences are never reported to credit bureaus or courts (Desmond, 2016). In a follow-up survey that occurred six months after the 2016 tax season, we ask households that responded to the original HFS survey: "In the last 12 months, were you or a person you were staying with forced to move by a landlord when you did not want to?" Of the 401 low-income, renter households that reported a delinquency at tax-time and that took both waves of the survey, 12% report 6-months later that they had been forced to move by a landlord in the last 12 months. We, therefore, set $P(E|D^R) = 0.12$.

Although a number of studies have empirically identified certain consequences of eviction for affected families, we are aware of no formal estimates of the associated costs. Consequently, we cannot reasonably estimate C^E. Hence, in Table 7, we provide estimates of the social benefits from lower home payment delinquency based on a range of hypothetical values for the cost of eviction, ranging from \$5,000 to \$20,000.

To put the benefits from lower delinquency into greater context, one can compare them to the transfer cost of the Marketplace subsidies. In Table 7, we estimate the transfer cost of the subsidy policy as $S \times P(S)$, where S is the annual cost of the subsidy provided to a household and $P(S)$ is the probability an eligible household avails of the subsidy. This representation abstracts from the other potential indirect costs of the subsidy policy, say due to higher taxes or higher interest rates. For example, if the subsidies are financed with taxes, then the deadweight losses from additional taxes can increase the total cost of the subsidy. Abstracting from such deadweight costs and considering only the monetary outlay, we set $S = \$4{,}342$. This amount reflects the sum of the average annual premium assistance given to a person in our sample (\$3,156) and the average per person cost-sharing assistance received by the Marketplace population (\$1,186). Next, we focus on the 2014–2016 ITT sample of households and set $P(S) = 0.37$, which corresponds to the share of the eligible sample that reports non-group private insurance coverage — i.e., the enrolled share of the potential Marketplace population. This provides an expected transfer cost of the subsidy policy, $E[S]$, equal to \$1,607.

From Table 7, we find that the expected benefit from reduced delinquency per subsidy eligible person ranges from \$441 to \$683. These benefits form a large fraction of the transfer cost of subsidies. If the cost of eviction is \$10,000, the benefits amount to 32% of the subsidy transfer cost. In sum, the calculations in this section indicate that lower home payment delinquency may be an important benefit from subsidized Marketplace insurance. Such incidental benefits of the ACA subsidy program should be considered when evaluating its welfare implications.

7. Conclusion

Our results confirm that health insurance through the ACA's Marketplaces helps protect low-income households from falling behind on rent or mortgage payments. We apply an RD design to a large sample of tax filers living near the poverty line in states that did not expand Medicaid. The probability of having any form of health insurance jumps by about 4 percentage points, or 6% relative to the control mean, at the income threshold for receiving subsidies for Marketplace plans. Among households targeted by the policy (i.e., that lack employer plans), the effect is large: a 46% relative increase in the rate of non-group private insurance coverage at the subsidy threshold.

This variation in insurance status could be used to explore any number of outcomes. We document a 25% drop in the rate of home delinquency as otherwise uninsured households qualify for subsidized Marketplace plans. This effect may operate, in part, through medical expenditures: at the 90th percentile of out-of-pocket medical spending distribution, the subsidy policy is associated with an average decline in spending of \$1,054 per household.

Overall, our RD estimate suggests that the subsidy policy may have indirect financial benefits to the participant and to the broader society that go beyond health costs. To the extent that fewer delinquent home payments translate into fewer evictions and foreclosures, social benefits may accrue. For example, under plausible assumptions and a hypothetical social cost of eviction in the range \$5,000– \$20,000, we estimate that the social benefits from fewer delinquencies would amount to \$441–\$683 per subsidy eligible person (representing 27%–42% of the transfer cost of the subsidies).

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Appendix A. Supplementary data

Supplementary data to this article can be found online at https://doi.org/10.1016/j.jpubeco.2018.12.007.

[40] If a foreclosure is merely a signal of risk, in terms of property or neighborhood quality, then the buyer is compensated for this risk through a price discount. If the price discount is, instead, due to owners actively depreciating the properties (e.g., selling fixtures at fire-sale prices), the depreciation to the property represents a dead-weight loss that will impose unnecessary costs on the new owner (see more discussion in HUD, 2010).

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