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Health Insurance as an Income Stabilizer

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Abstract

We evaluate the effect of health insurance on the incidence of negative income shocks using the tax data and survey responses of nearly 14,000 low income households. Using a regression discontinuity (RD) design and variation in the cost of nongroup private health insurance under the Affordable Care Act, we find that eligibility for subsidized Marketplace insurance is associated with a 16% and 9% decline in the rates of unexpected job loss and income loss, respectively. Effects are concentrated among households with past health costs and exist only for "unexpected" forms of earnings variation, suggesting a health-productivity link. Calculations based on our fuzzy RD estimate imply a $256 to $476 per year welfare benefit of health insurance in terms of reduced exposure to job loss.

Keywords: regression discontinuity, Affordable Care Act, subsidies, labor supply, income volatility, productivity

JEL Codes: D10, H51, I13, J22, G51, G52

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1 Introduction

The stability of cash flows is vital to the financial stability of households. Recent employment data show that around 1% of U.S. workers on average are laid off from their jobs in a typical month, and evidence points to employment becoming less stable over time (Hollister, 2011).[1,2] Beyond the unexpected loss of a job, volatility in income flows is an increasingly common experience for U.S. households, particularly among low income households. For example, an analysis of JPMorgan Chase accounts found that over half of customers regularly experienced income fluctuations of over 30% on a month-to-month basis (Farrell and Greig, 2016). Moreover, the standard deviation in a household's annual income grew by about 30% from 1971 to 2008 (Dynan, Elmendorf, and Sichel, 2012). Despite its relevance, little is known about the mechanisms that influence earnings instability. This paper asks whether health insurance plays any role in mitigating income and employment shocks within a sample of low income households.

The institutional attachment of health insurance to employment in the U.S. leaves few experimental settings available to researchers to study how health insurance affects income shocks. The experiment with the most rigorous design found that the 2008 expansion of Medicaid to low income adults in Oregon was associated with *insignificant* increases in total annual earnings and the likelihood of working more than 20 hours per week (Baicker, Finkelstein, Song, and Taubman, 2014; Finkelstein, Taubman, Wright, Bernstein, Gruber, Newhouse, Allen, Baicker, Group et al., 2012, Appendix). It is not clear how to interpret this result, however. As the authors note, any boost in labor supply from a health-productivity link could be offset by households reducing their incomes to qualify for Medicaid. Moreover, to the extent that households can later recuperate lost income, for example, by taking on extra hours, annual labor outcomes could mask substantial intrayear variation in household earnings streams following insured versus uninsured health events.

Our empirical design helps to overcome these issues. It relies on the Patient Protection and Affordable Care Act (ACA), which substantially increased the percentage of working-age adults with health insurance coverage in the U.S. To identify a causal effect of health insurance on income shocks, we use a regression discontinuity (RD) design, exploiting ACA "Marketplace" insurance

[1] Data are from the Bureau of Labor Statistics. See `https://www.bls.gov/news.release/archives/jolts_09122017.pdf`.

[2] Income volatility may have aggregate demand consequences. For example, Chetty (2008) shows that households have difficulty smoothing consumption at the onset of unemployment, contributing to a 7% drop in consumption expenditures (Gruber, 1997).

(i.e., nongroup private insurance) plan subsidies to generate quasirandom variation in the cost of health insurance. Key to our empirical design is (a) the decision of 22 state governments not to expand Medicaid to able-bodied, low income adults under the ACA, (b) the income eligibility cliff for receiving federal assistance to purchase nongroup private health insurance plans through the ACA's Marketplaces, and (c) the restricted availability of these subsidies to only households without access to employer plans.

In states that did *not* expand Medicaid, the income threshold for Marketplace subsidies is at 100% of the federal poverty level (FPL), which, in 2016, was roughly $12,000 for an individual and $24,000 for a family of four. Households with incomes *above* 100% FPL (and that do not have affordable insurance through an employer plan) qualify for generous subsidies toward Marketplace insurance plans. Without these subsidies, eligible low income households often go uninsured because the cost of Marketplace plans (and health insurance more generally) is typically too expensive for this population. This feature of the ACA's design and its implementation creates a powerful shock to health insurance access for households without an employer plan in states that did not expand Medicaid. In states that *did* expand Medicaid, there is no change to the cost of insurance at 100% FPL, implying that there should be no change in the probability of income shocks at 100% FPL. While it is possible that intrayear income shocks might reduce annual income, pushing income below 100% FPL and making it less likely that a household qualifies for subsidies, we would not expect to find a *discontinuity* in the probability of income shocks at 100% FPL after controlling for a smooth function of income.

To implement this design, we use a unique data set containing the 2015 and 2016 Form 1040 of a large sample of low income, online tax filers. These data contain precise information on households' adjusted gross income (AGI), which closely approximates the income measure used by the Marketplaces to validate eligibility. We also gather information on health insurance status and experiences of income shocks from a linked survey taken at the end of the tax-filing process. Participants receive a small financial reward for taking the survey and consent to their deidentified tax and survey data being used for research. The sample contains just under 14,000 households that lack employer plans (the target population of the Marketplace), of which nearly 5,000 live in states that did not expand Medicaid. We are interested in the likelihood that these households experience income shocks within the recent past. To create our outcome variables, we use all available information in our tax and survey data set on disruptions in workers' income streams. In particular, we create indicators for unexpected loss of a job, unexpected loss of income, and indicators of income variation throughout the year. If health insurance coverage reduces absenteeism and improves worker performance, for example, households gaining coverage under the ACA should be less likely to unexpectedly lose their jobs or experience dips in income.

We estimate intent-to-treat (ITT) effects using a reduced form RD specification. Within states that did not expand Medicaid, our most conservative estimates indicate that eligibility for subsidized Marketplace insurance is associated with an 8 percentage point (or a relative 29%) decline in the rate of unexpected job loss among sample households. Similarly, we find a 7 percentage point (or a relative 18%) decline in the share of households reporting unexpected reductions in income. Using an RD difference-in-difference (RD DiD) model comparing households in nonexpansion states with similar households in expansion states, the discontinuities at 100% FPL fall in magnitude but remain substantial: We find relative declines in the rates of unexpected job loss and unexpected income loss of 16% and 9%, respectively, as households become eligible for the subsidized Marketplace. We also find a significant drop in the rate of households reporting that they experienced monthly income variation because of periodic unemployment.

Through an instrumental variables approach, we show that this relationship is likely causal. Relative to being uninsured, households that get the subsidized coverage through the Marketplaces are significantly *less* likely to report an unexpected job or income loss. We apply our fuzzy RD estimate for job loss to the price of private unemployment insurance, obtained from a private unemployment insurance provider based in Wisconsin. Back-of-the-envelope calculations suggest a $256 to $476 per year welfare benefit of health insurance in reduced exposure to job loss.

Placebo tests show that these results are unique to the 100% FPL threshold and exist only in states that did not take up Medicaid expansion. Our findings are also robust to alternate (data-driven) bandwidth choices, controlling for income in a flexible manner with polynomials of different orders; triangular and uniform kernel weighting; including household demographics as well as state-year fixed effects as controls; and alternate methods of estimating standard errors. Our results also hold when we account for potential income manipulation around the eligibility threshold through a doughnut RD design (Barreca, Guldi, Lindo, and Waddell, 2011).

The most direct explanation is that health insurance limits negative earning shocks by improving worker health and, in turn, the quantity, quality, and reliability of labor output. The literature strongly supports the hypothesis that better perceptions of one's physical and mental health can translate into increased labor supply (Frijters, Johnston, and Shields, 2014; Bubonya, Cobb-Clark, and Wooden, 2017; Dizioli and Pinheiro, 2016). Perceptions of one's health are likely influenced by insurance status. The Oregon health insurance experiment, which used a lottery to randomly assign Medicaid coverage to participants, found that access to health insurance leads to substantial improvements in self-reported physical health as well as in mental health (Finkelstein et al., 2012). The literature also documents large reductions in mortality rates after Medicaid expansions (Miller et al., 2019), signaling a long-term clinical impact. Consistent with this narrative, our effects are concentrated in households with a greater likelihood of health problems, as approximated through indicators of past health-related expense shocks and accumulated medical debt.

Since the timing and duration of health shocks are difficult for households to anticipate, we expect that health insurance would affect only those forms of earnings disruptions that are unanticipated. Indeed, we find no significant effect of the subsidy policy on the prevalence of income variation attributed to ex-ante predictable causes, such as seasonal jobs. Moreover, this result is robust to controlling for whether the household is financially constrained (defined as "unable to come up with $2,000 in an emergency"). Studies show that insurance reduces financial stress (e.g., Gross and Notowidigdo, 2011), which, in turn, can boost labor productivity (Mullainathan and Shafir, 2013; Bernstein et al., 2019; Maturana and Nickerson, 2019).[3] Therefore, we ask whether a significant interaction effect between subsidy eligibility and health problems might simply reflect a lack of financial resources among sicker households, which limits productivity and is alleviated by insurance. However, after absorbing the effect of constraint, the treatment effect continues to be magnified within the subsample of plausibly sicker households. Hence, our findings do not appear to operate purely through a financial stress mechanism. Instead, access to affordable health insurance appears to help households, particularly sicker households, to stabilize their earnings streams.

It is also possible that health insurance has no effect on workers' earnings streams but that the act of trying to obtain health insurance does.[4] In particular, households may attempt to obtain insurance assistance by manipulating their incomes upward to be on the optimal side of the income eligibility threshold for Marketplace subsidies. Income manipulation of this type may translate

[3]For example, Bernstein et al. (2019) find that those patent inventors who lost housing wealth during the financial crisis produced fewer patents and patents of lower quality compared with other inventors in the same firm and metropolitan area. Maturana and Nickerson (2019) find that the test scores of students taught by Texas public school teachers that file for bankruptcy fall by 1.7%.

[4]For example, households may seek jobs that come with insurance benefits and perform well at those jobs to hold onto their insurance benefits, in turn, improving earnings stability. The focus of our paper, however, is on those households that report not having health insurance through an employer, family member, school, or through the Veterans Administration (VA). These households were the target of several reforms to the nongroup private insurance market enacted under the ACA.

indirectly into a lower likelihood of negative income shocks. We explore this possibility in detail, testing for bumps in the density just above the 100% FPL threshold. We find no significant evidence of income manipulation in our sample. Moreover, we find that our results are unchanged after we exclude self-employed households (i.e., the subset of households with the greatest ability to manipulate their incomes around policy thresholds, including the threshold studied in this paper (Saez, 2010; Kucko et al., 2017)).

Our study makes three important contributions. Foremost, it contributes to the broader literature on the labor market effects of public health insurance initiatives. Recent analyses have found that the ACA's coverage provisions had little effect on aggregate labor supply or demand (Duggan, Goda, and Jackson, 2017; Heim, Lurie, and Simon, 2018; Heim, Hunter, Isen, Lurie, and Rammath, 2016).[5] Yet to date, there has been no test of the impact of the ACA on the *intrayear* labor market experiences of individual households. To the extent that our findings stem from a relationship between health insurance and productivity, they signal important economic consequences from expanding or eliminating federal health insurance programs. Additional research is needed, however, to convincingly isolate a productivity link, as our data do not capture hours worked, work output, or work performance. Instead, we only observe the downstream shocks to employment and income. Second, we quantify the effect of health insurance on a new dimension of household well-being: the stability of earnings. Income volatility is an emerging topic in household finance and a growing concern for policymakers.[6] Finally, our study provides evidence of the indirect financial effects of the Marketplace subsidy program, a component of the ACA that has been relatively underexamined compared with the Medicaid expansions.

2 Background on the ACA

The Affordable Care Act, passed by President Barack Obama in 2010, aimed to decrease the number of uninsured Americans by implementing a wide array of changes to the U.S. health-care system, including expanding eligibility for Medicaid and restructuring the market for nongroup private health insurance plans. Historically, the Medicaid program primarily covered children, pregnant women, older adults, and disabled individuals living in low income households. The

[5]While Duggan et al. (2017) finds that the ACA had no effect on aggregate labor market supply, the authors show that this may be because of netting from a reduction (increase) in labor force participation linked to the Marketplace subsidies (Medicaid expansion).

[6]For example, the annual *Report on the Economic Well-Being of U.S. Households*, released annually by the Board of Governors of the Federal Reserve System, now includes a subsection devoted to the topic of income volatility. This inclusion began with the 2015 report and has since continued.

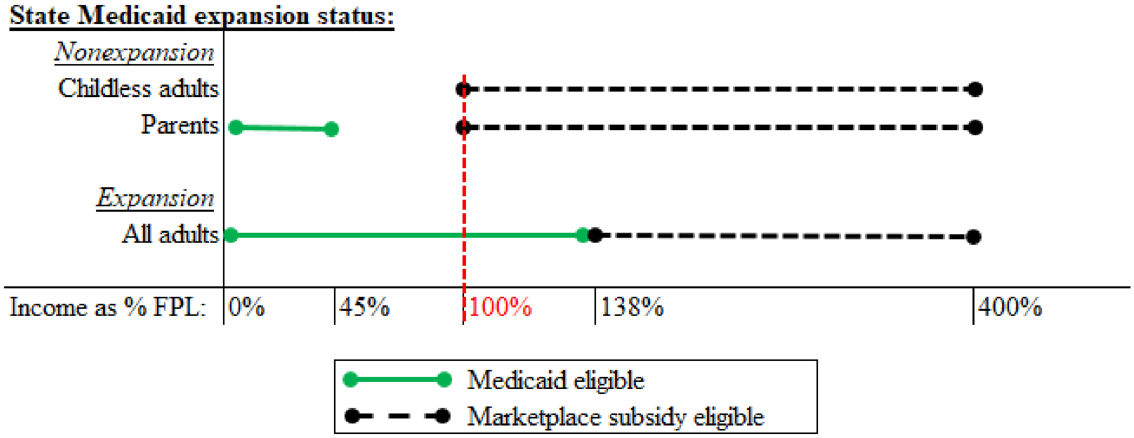
program was typically not available to childless adults, and parents of children under the age of 18 could only qualify if their incomes were substantially below the poverty line. The ACA, which went into full effect in 2014, provided large federal subsidies to encourage states to expand Medicaid availability to adults earning up to 138% FPL. As of 2016 (the end of our sampling period), 31 states and Washington, D.C. opted to expand Medicaid (hereafter “expansion” states) while 19 did not (hereafter “nonexpansion” states).

Purchasing private nongroup health insurance prior to the ACA was expensive because insurance companies could use health information in the underwriting process to determine both coverage and premium amount. The ACA implemented a number of large-scale changes to this market, including mandating that insurers accept all applicants, charge the same rate premiums to individuals regardless of preexisting health conditions in a given geography, and provide a minimum amount of benefits. Along with the changes to insurance coverage, 15 states and the federal government set up online health insurance “exchanges” in which individuals can buy coverage. To assist low income households in purchasing these plans, the ACA provided federal subsidies for households with incomes between 100% and 400% FPL. Over 11 million individuals in 2016 purchased insurance though an exchange (HHS, 2016).

Despite the increased insurance coverage under the ACA, some low income households were left in an insurance “coverage gap” when some states opted to not expand Medicaid. As illustrated in Figure 1, this coverage gap includes 2.6 million childless adults and parents (Garfield et al., 2016) whose income is too high to qualify for Medicaid (above 45% FPL) but too little to qualify for Marketplace insurance subsidies, which begin at 100% FPL. Health insurance is typically very expensive for people in the coverage gap, particularly when these costs are considered as a fraction of income.[7] Adults in this coverage gap may obtain insurance through an employer or family member or, absent these options, go uninsured (during our period of analysis, households at this income level were usually exempt from the penalties associated with the insurance mandate) or purchase “catastrophic” (low-premium, high-deductible) plans. This coverage gap is critical to our research design, as we use the income threshold at which a household exits the gap to identify the effect of health insurance access on earnings stability.

[7] For example, consider a family of four living in a nonexpansion state making $20,000 per year (82% FPL) that chooses to insure two children through either the Children’s Health Insurance Program or Medicaid (which carry no premiums) and to insure the two adults (both 30 years old) through a midlevel (“Silver”) plan purchased on the Marketplace. This family would expect to pay the following costs for the Silver Plan: premiums of $531 per month ($6,371 per year, or 31.9% 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. These numbers are based on the Kaiser Family Foundation’s 2016 online calculator, available at `https://www.kff.org/interactive/subsidy-calculator-2016/`.

Figure 1: Medicaid and Marketplace Subsidy Income Thresholds



Notes: 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. A similar version of this figure first appeared in Gallagher, Gopalan, and Grinstein-Weiss (2019b).

Beyond the expansion of Medicaid, the ACA provides two types of subsidies – premium assistance and cost-sharing reductions (CSRs) – to households purchasing nongroup private insurance sold on the "Marketplace" (also called the "Exchange"). These subsidies reduce both the premium and out-of-pocket expenses for households that do not have access to Medicaid or affordable employer plans. Figure 2 helps to visualize the level of subsidy available at different income levels in different states.

Premium subsidies, or advance premium tax credits, are paid in advance to insurers by the Internal Revenue Service (IRS). For an enrollee, any difference between the advanced subsidy and the final subsidy that is caused by a gap between projected income at the time of enrollment and actual income as reported on tax returns filed the following year is reconciled at tax time after the coverage year. However, if an enrollee receives a premium subsidy and then earns *less* than 100% FPL during the enrollment year, the participant does not have to pay back the subsidy at tax time the following year.[8]

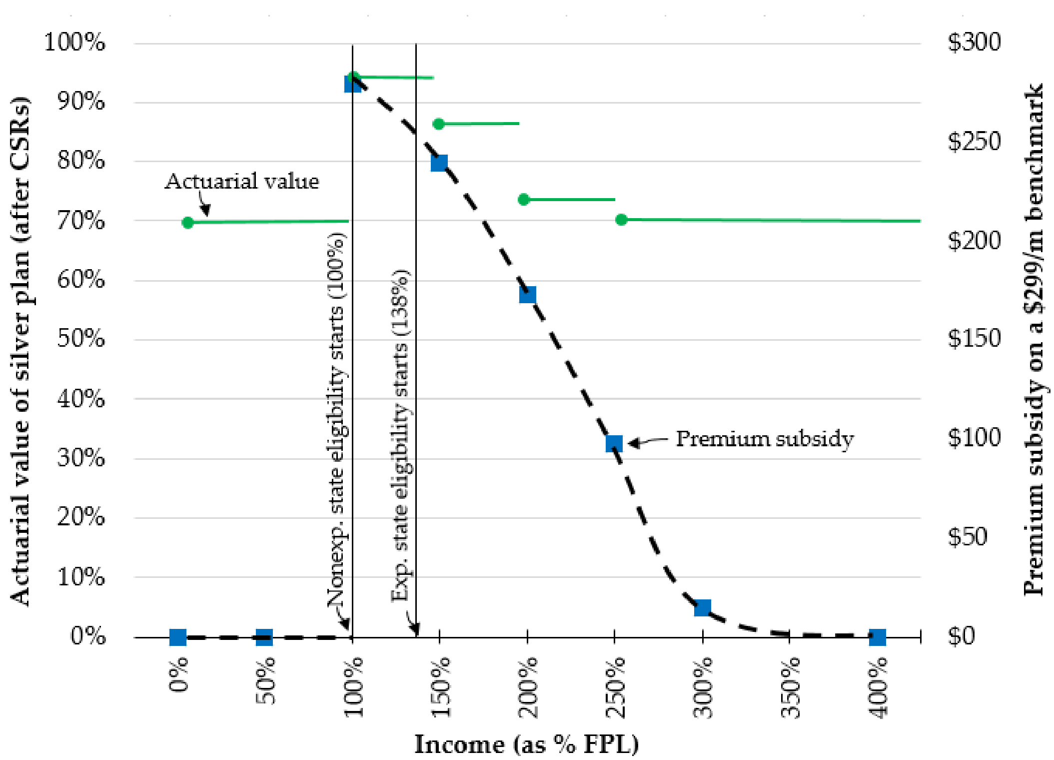
The premium subsidies are substantial at incomes just above 100% FPL. A 40-year-old single adult earning just over 100% FPL (or about $12,000) would have paid an average monthly pre-

[8]Households that end up earning *more* than they had projected may be obliged to repay a portion of their subsidy at tax time after the coverage year. The repayment is capped at $300 for single filers and $600 for married filers with income 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`. Also see `https://www.healthinsurance.org/faqs/`.

mium of $20 ($240 per year) after subsidies in 2016, according to a calculator offered by the Kaiser Family Foundation (see footnote 7). In the absence of subsidies, that same participant would have paid about $299 per month for the same plan ($3,588 per year), or 30% of the participant's annual income. Above 100% FPL, these subsidies fall continuously, but precipitously, such that the amount of premium assistance becomes very small at incomes of above 300% FPL.

In expansion states, there is no jump in premium subsidy at 100% FPL, since households that earn at or below 138% FPL are eligible for Medicaid and, thus, by design, ineligible for Marketplace subsidies. As illustrated by the second vertical line in Figure 2, households in expansion states with incomes in the range of (138%, 400%) FPL are eligible for premium subsidies.

Figure 2: Marketplace Insurance Subsidy Schedule for a Hypothetical Adult



Notes: This figure shows the actuarial value of a Marketplace "silver" plan (LHS axis, green lines) after cost-sharing reductions (CSRs), which alter the effective actuarial value of the plan, moving it from its 70% baseline. The figure also shows the amount of premium subsidy (RHS, black dashed lines) that a hypothetical 40-year-old, single adult could expect to help pay for a $299/month benchmark Marketplace silver plan. Vertical lines mark the minimum income to gain access to both CSRs and premium subsidies, which simultaneously become available when a household earns over 100% FPL in nonexpansion states or over 138% FPL in expansion states. CSRs end at 250% FPL and premium subsidies end at 400% FPL in all states. These estimates are tabulated using the subsidy schedule provided by the Kaiser Family Foundation (KFF) (`https://www.kff.org/health-reform/issue-brief/explaining-health-care-reform-questions-about-health/`) and the *estimated* premium subsidy from the KKF's 2016 calculator (see footnote 7). A similar version of this figure first appeared in Gallagher et al. (2019b).

Participants living in nonexpansion states with incomes in the range of [100%, 250%) FPL also qualify for CSRs. By contrast, in expansion states, eligibility for CSRs begins at 138% FPL. CSRs reduce out-of-pocket costs (primarily from deductibles) associated with using health-care services.[9] Similar to premium subsidies, CSRs are paid by the federal government directly to the insurer. The participant does not have to refund the CSRs if projected income at the time of enrollment differs from the actual income reported on the tax return the following year.

In effect, CSRs raise the actuarial value of insurance plans. This feature is represented in Figure 2 by the step function. The base actuarial value of a "silver" plan (a midlevel plan) on the Marketplace is 70% (i.e., the plan pays 70% of the typical participant's health-care costs). CSRs effectively raise the 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 of [150%, 200%) FPL, and to 73% for incomes in the range [200%, 250%) FPL.

Thus, for participants who earn under 200% FPL, the combined benefit of the two subsidies is large. The premium subsidies cover at least 58% of the premium (with this percentage increasing as incomes approach 100% FPL); similarly, the effective actuarial value of the plan rises from 70% to at least 87%. Compare that level of subsidy with that of a household that earns just under 250% FPL – this household gets assistance toward much less (33%) of the premium and the actuarial value rises only marginally to 73%. Hence, we can think of the Marketplace subsidy policy as being extremely progressive. For this reason, our analysis is restricted to households that earn under 200% FPL.

It is important to note that, although the subsidy at 100% FPL comes both in the form of both the front-end premiums and back-end deductibles, we cannot isolate the independent effect of either form of subsidy. Thus, any reduced form changes in the earnings stability of households at 100% FPL could be equally attributable to a jump in the penetration of health insurance (an extensive margin effect) as well as to a decline in the health costs of previously insured participants (an intensive margin effect).

3 Literature

Our paper finds evidence of a link between health insurance access and income shocks within a sample of low income households. As such, we contribute to three strands of literature: (a)

[9] According to the Kaiser Family Foundation 2016 Calculator (see footnote 7), CSRs decrease the out-of-pocket maximum from $6,850 to $2,250 (mostly through a reduction in the deductible), on average, for a 30-year-old single adult earning just over 100% FPL in 2016.

studies testing the effect of health insurance on household financial well-being, (b) explorations of the factors contributing to rising income volatility, and (c) analyses of the linkages between health and productivity.

Foremost, this paper joins a large body of work that uses experimental or quasiexperimental designs to relate health insurance to financial outcomes. These studies typically find that affordable health insurance access leads to substantial improvements in financial well-being, including improvements in credit scores, credit offers, and reductions in various forms of debt and delinquency (Gross and Notowidigdo, 2011; Finkelstein et al., 2012; Barcellos and Jacobson, 2015; Mazumder and Miller, 2016; Brevoort et al., 2019; Blascak and Mikhed, 2019; Hu et al., 2018). Note that two other papers have used our same data set to analyze the household financial effects of the ACA. Gallagher et al. (2019a) evaluate the effect of Medicaid access on households' tax refund savings decisions. Second, using our same empirical approach, Gallagher et al. (2019b) identify a link between the ACA subsidy policy and reductions in delinquency rates for rent and mortgage payments, as well as significant declines in medical expenditure risk.

Although a number of studies have explored the labor market participation effects from public insurance expansions (Baicker et al., 2014; Finkelstein et al., 2012; Duggan et al., 2017; Heim et al., 2018; Garthwaite et al., 2014; Akosa Antwi et al., 2013), these studies focus on the effect of health insurance on incentives to work, typically evaluating only the extensive margin of employment (e.g., labor force participation) and/or annualized measures of labor output (e.g., earnings or average hours per week). In contrast, we study within-year variation in unexpected earnings and job loss. This is an important distinction for two reasons. First, if households can recuperate lost income, annual measures of earnings may mask intrayear earnings variation. Second, part of the hypothesis of these other studies is that households may engage in strategic downward manipulation of their labor supply or may increase their labor attachment to obtain health insurance, such as by qualifying for Medicaid or by maintaining access to an employer plan. Up front, we present evidence that households are not widely manipulating their income upward to gain access to the type of insurance exploited in our study. Moreover, intrayear earning shocks should be less directly affected by the confounding influence of income manipulation (if it were to exist), since Medicaid eligibility is assessed annually. Hence, a major contribution of our paper is to offer evidence on labor market experiences using a source of health insurance that is designed for people without employer plans and for which incomes must be *above* (not below) a certain threshold to access (perhaps explaining the lack of income manipulation around the subsidy threshold).

Instead of focusing on the extensive margin of employment or on hours worked, we contribute to the burgeoning discussion around the causes and effects of income volatility by studying the stability of a household's earnings.[10] Employment and income have become less stable over time, particularly for U.S. households in the lowest income brackets (Dynan et al., 2012). Acs, Loprest, and Nichols (2009) find that over 40% of households in the lowest income quintile experienced an income drop of 25% or more in a given year. Low-income households are both disproportionately exposed to negative income shocks and least able to weather them through emergency savings (Lusardi, Schneider, and Tufano, 2011). In a review of the research, Kalleberg (2009) identifies a number of factors leading to increased employment insecurity over time, including a decline in attachment to employers, a growth in perceived job insecurity, and the growth of contract-based or temporary employment arrangements. Little attention has been paid, however, to policies that may alleviate income volatility.

Health insurance is one such policy. The most obvious way that health insurance might prevent slumps in income is by improving access to early stage treatment for physical and mental health problems, which, in turn, might enable more stable work. This mechanism, however, relies on two assumptions. The first assumption is that individuals with access to health insurance use their insurance benefits. In the Oregon health insurance experiment, Finkelstein et al. (2012) find that individuals randomly assigned Medicaid coverage had higher utilization of the emergency room and had more doctor's office visits. Consistent with these findings, studies link health insurance expansions to increased use of primary and preventative care services, as well as adherence to prescription drugs (Simon, Soni, and Cawley, 2017; Sommers, Blendon, Orav, and Epstein, 2016).[11]

The second assumption is that health insurance improves one's health. Finkelstein et al. (2012) find that individuals randomly assigned to receive coverage through Oregon's Medicaid program experienced large improvements in self-reported physical and mental health. The authors attribute this result to either a significant increase in health-care utilization, including preventative care, or simply a general sense of improved well-being. It is important to note, however, that there has been no comprehensive assessment of the *clinical* health impact of health insurance. As noted

[10] The consequences of income volatility are numerous, having been linked to declines in physical and mental health (Prause, Dooley, and Huh, 2009; Halliday, 2016) as well as in the ability to afford food, housing, health care, and essential expenses (Dahl, DeLeire, and Mok, 2014; Heflin, 2016).

[11] Studies also show improved medication adherence and increased screening for diabetes and other diseases (Ghosh, Simon, and Sommers, 2017; Sommers et al., 2016; Wherry and Miller, 2016; Sommers et al., 2017). Several studies find that insured households are more likely to spend on medical care (Manning et al., 1987; Shen, 2013; Lipton and Decker, 2015; Dunn, 2016; Martin et al., 2017).

by Finkelstein, Mahoney, and Notowidigdo (2018), "The set of potential clinical, non-mortality health benefits is large, and only a subset of them have proven measurable."[12] Nonetheless, a growing body of empirical evidence points to dramatic reductions in mortality from health insurance (Sommers, 2017; Miller et al., 2019), implying that the perceived health benefits of insurance are real. For example, an RCT with 3.9 million participants found that just 6 months of health insurance reduced mortality by about 1 percentage point among middle-aged participants (Goldin, Lurie, and McCubbin, 2019).

Perceptions of one's physical and mental health have been linked to labor productivity. For example, Bubonya et al. (2017) find that absence rates are 5% higher among workers who report being in poor mental health (see also Frijters et al., 2014; Babiarz, Widdows, and Yilmazer, 2013, and Smith, 1999). However, it is difficult to find settings in the U.S. where health and/or health insurance are not endogenous to labor supply (Currie and Madrian, 1999). Ettner et al. (1997) use the hereditary nature of mental illness to develop a plausibly exogenous instrument. They find small effects of mental illness on hours worked but a large, 10% to 30%, decline in income.

In an event study approach, Dobkin et al. (2018) study the rather extreme case of hospitalizations and show that a hospitalization translates into declines in the probability of being employed and in the annual labor market earnings of those with health insurance. The impact of hospitalizations on annual earnings appears to be increasing over time and is consistent with the fact that substantial public insurance for earnings losses due to health shocks does not exist in the U.S. until individuals become age-eligible for Social Security. These researchers do not, however, explore the role of health insurance on earnings streams after a health event nor do they use a framework in which insurance is quasirandomized. One possible exception is Dizioli and Pinheiro (2016). Using U.S. Census region of residence as instrument variables for the probability of holding health insurance coverage, these authors find that workers with health insurance missed 76% fewer workdays than comparable workers without health insurance.

We contribute to this literature by assessing whether exogenous variation in health insurance access predicts income shocks in a low income sample. If there is a link between health and productivity, then there are two reasons to think that the link might be stronger in the context of low income households, the population of interest in our study. First, the frequency and severity of health shocks may be greater in low income samples, since health is positively correlated with

[12] The results of a large randomized control trial in Oregon suggest little impact of insurance access on short-term clinical outcomes (such as blood pressure and cholesterol). However, the experiment did reveal increased rates of diabetes detection, diabetes management, and led to lower rates of depression (Baicker et al., 2013).

income (Egen et al., 2017). Second, most low-wage workers do not have access to paid sick leave. By design, employee leave policies buffer against income shocks, and the absence of these policies for the low-wage workers may exacerbate the economic consequences of illness.

4 Data and Variables

4.1 Data

The data used in this study come from the linked tax records and survey responses of a large sample of low income households that use an online tax-preparation program offered through the IRS' Free File Alliance (FFA) to prepare their tax returns during the 2015 and 2016 tax-filing seasons. To qualify for the FFA program, tax filers must have an AGI of less than $31,000 or qualify for the Earned Income Tax Credit (EITC).[13] The income ceiling for EITC-eligible families roughly corresponds with 200% FPL. As such, less than 10% of our sample has an income above 200% FPL, and we have no observations with income over 400% FPL. Note that households that earn over 200% FPL but under $31,000 are majority single adult households. To ensure a fairly uniform level of subsidy within those in the "treated" sample, we restrict our analysis to respondents earning under 200% FPL.

Immediately following the tax-filing process, a random sample of FFA filers are invited to participate in a household financial survey (abbreviated as HFS). Participants are offered an Amazon gift card (typically, $5 in value) as compensation for completing the survey, which includes a wide array of questions about filers' assets, liabilities, financial behaviors, experiences of hardship, financial shocks, and health insurance status. The response rate for the survey, including only those filers who completed the survey, was 9.3%. As part of completing the survey, tax filers consent to the use of their anonymized tax return data for research.

Our sample may be subject to an unknown degree of selection bias if households that participate in the survey differ in unobserved ways (e.g., risk aversion) from those who do not participate. The appendix of Gallagher et al. (2019b) offers a detailed discussion of the data set, as well as an evaluation of the potential for sample selection bias. Briefly, these authors' tests reveal no significant differences between the 2013 tax forms of households that self-select into the survey

[13]Additional detail about the construction of this data set is available in the appendix of Gallagher et al. (2019b). Due to the sensitive nature of tax data, the data for this study are not publicly available. Parties interested in replication can be put in touch with the vendor through the Social Policy Institute at Washington University in St. Louis. For more on the Free File Alliance, see `https://www.irs.gov/uac/about-the-free-file-program`.

and those that do not on observable variables. The survey responses of households are also statistically similar when cut according to the size of the participation reward offered (e.g., $0, $5, $15, or $20). Although these tests indicate that our results are unlikely to be qualitatively affected by sample selection bias, the magnitude of our estimates could be biased to an unknown degree.

We include data from both the 2015 and 2016 tax-filing seasons – the only two seasons in which all necessary variables are present in the survey and individual-level tax information is attached to the survey responses. The HFS is not longitudinal across years, and only about 4% of the sampled households completed the survey in both years. We pool the data for the two years to conduct our analysis; sampled households are similar on observable characteristics across the two years. Our sample includes filers who are U.S. citizens aged 19 to 64 at the time of the survey, with income in the (0%, 200%) FPL range. Much of our analysis focuses on nonexpansion states, which we define as states with a gap between the income threshold for adult Medicaid eligibility and for Marketplace subsidies.[14] Twenty-two states met this criterion at some point during the 2015 and 2016 tax seasons, based on data from the Kaiser Family Foundation. These states are listed in Appendix Table A1.[15]

To be eligible for Marketplace subsidies, a household must not have access to "affordable" employer coverage, currently defined as a plan with an employee contribution of less than 9.7% of income and with an actuarial value of at least 60%. The vast majority of employer plans meet this threshold (Yong et al., 2011). They must also not have access to insurance through a family member's employer or through a government insurance program. This fact poses a problem for our identification strategy because it means that the full sample includes a majority of households with insurance status that is unaffected by the instrument (the Marketplace subsidy threshold). It is not surprising then to note that, when we include households with employer plans in our analysis, our first-stage and reduced form estimates are heavily attenuated and the reduced form is not statistically significant.[16]

[14] The term "nonexpansion state" commonly 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 nonexpansion states as those with a Medicaid eligibility ceiling that is below 100% FPL, the eligibility floor for Marketplace subsidies in states that did not expand Medicaid.

[15] Appendix Table A1 shows the annual number of sample households from each nonexpansion state as well as the state's associated Medicaid ceilings for childless adults and parents. As we later discuss, we oversample younger adult households, the majority of which have no children. Although we include parent households in our analytic sample, we ensure that our main results are robust to excluding the 64 parent households from Alaska, Maine, and Tennessee (which had Medicaid ceilings of above 100% FPL for adult parents).

[16] For the first-stage estimates using the full sample, see Gallagher et al. (2019b). For brevity, we do not show the reduced form estimates using the full sample (i.e., including households with employer plans). These estimates are negative but statistically insignificant and a fraction of the size of our estimates based on the restricted sample of

To increase the power of our instrument, we restrict our sample to households to the "potential Marketplace" population (the analytic sample), which are households that do not have disqualifying alternative insurance plans. Although access to Medicaid would make a household ineligible to receive Marketplace subsidies, we retain households that are eligible for Medicaid in the analytic sample to make it comparable across expansion and nonexpansion states.[17]

It is reasonable to ask whether restricting our sample to households without employer plans may bias our estimates. If the Marketplace subsidy policy crowds out other forms of insurance (e.g., employer-sponsored insurance), there might exist unobserved heterogeneity around the threshold. Recall, however, that the subsidy policy was designed *not* to crowd out other forms of quality coverage (since eligibility is conditional on not having access to alternative forms of affordable insurance). Still, households may choose to quit jobs that come with health insurance in favor of alternative (perhaps, less stable) jobs and Marketplace insurance, thus biasing our estimates. If crowd out were occurring, however, we would expect to observe a discontinuous reduction in the probability of having employer insurance at 100% FPL in the nonexpansion states. We observe no such effects; the relationship between income and employer insurance is smooth at the threshold. A full battery of regression discontinuity tests of insurance crowd out using our data is presented in Gallagher et al. (2019b). As these authors note, the jump in nongroup private insurance in our sample appears to come almost entirely from a reduction in the uninsurance rate.[18]

After these restrictions, we have 13,990 survey participants, divided roughly equally between the two tax years. Within nonexpansion states, there are 4,975 households that fit these criteria, of which 3,270 have income below 100% FPL and 1,705 have income greater than or equal to 100% FPL.

Our study is limited to households that filed their taxes through an online tax-filing program. This limitation may generate differences between our sample and the broader low income population. For example, online tax-filing appears to attract a disproportionate number of young adults or students. We control for the oversampling by following Solon, Haider, and Wooldridge (2015). In particular, we control for observable characteristics that may be skewed in our sample compared with the broader low income population, including income, age, race, number of

households targeted by the policy.

[17]In expansion states, everyone with an 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/`.

[18]Appendix Table A2 reports respondents' health insurance coverage across income levels by state Medicaid expansion status.

dependents, college completion, gender, marital status, employment status, and student status.[19]

4.2 Variables

Table 1 summarizes the variables used in our analysis. The running variable in our RD design is a household's income as a percentage of FPL, which is a function of a tax filer's AGI as reported on the tax return, its household size, and the federal poverty guidelines in a given state and year. Using this variable, we assign a household to the treatment group when its income is above 100% FPL and to the control group when its income is below 100% FPL.

Table 1: Summary Statistics

	All		Treated	Control	
Variable	mean	(s.d.)	mean	mean	diff / s.d.
Income≥100% FPL (treatment)	0.343	0.475	1.000	0.000	n.a.
Nongroup private insurance	0.271	0.445	0.401	0.203	0.444
Job shock*	0.249	0.433	0.208	0.271	0.146
Income shock*	0.351	0.477	0.316	0.369	0.111
Periodic unemployment*	0.179	0.383	0.128	0.205	0.201
Health problem	0.420	0.494	0.364	0.450	0.174
Constrained	0.626	0.484	0.559	0.660	0.208
Number of W2s	1.661	1.140	1.840	1.567	0.240
Full-time	0.378	0.485	0.553	0.287	0.549
Age	35.695	11.809	36.888	35.074	0.154
White (non-Hispanic)	0.811	0.392	0.833	0.799	0.087
Number of kids	0.544	0.976	0.445	0.595	0.154
College graduate	0.407	0.491	0.467	0.376	0.185
Male	0.461	0.499	0.492	0.445	0.094
Single	0.838	0.368	0.798	0.860	0.168
Employed	0.746	0.436	0.807	0.714	0.213
Student	0.240	0.427	0.188	0.267	0.185
N	4,975		1,705	3,270	

Notes: This table reports means and standard deviations of a variety of variables using the potential Marketplace sample of respondents living nonexpansion states. The three outcome variables of primary interest in this study (i.e., those that potentially capture unexpected shocks to income because of health events) are denoted with a "*". For each variable, the table also 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 are considered unbalanced. Data include respondents with incomes in the range of (0%, 200%) of FPL. Treatment is assigned at incomes above 100% FPL (i.e., "Income≥100%"=1).

Eligibility for Marketplace subsidies (both for cost-sharing subsidies and for advanced premium tax credits) is determined based on a household's "projected" modified adjusted gross in-

[19]For more discussion of the data set, its weaknesses, as well as an evaluation of the potential for sample selection bias, see the Appendix of Gallagher et al. (2019b).

come (MAGI) for the coverage year. We approximate this figure using a household's AGI for the previous year (as reported on its 2015 or 2016 tax form). As such, our running variable is only a proxy for Marketplace subsidy eligibility. A household's AGI may vary from its MAGI in three ways: nontaxable Social Security benefits, tax-exempt interest, and excluded foreign income. As we lack information on these three components, following standard practice in literature (e.g., Hinde, 2017), we use AGI instead. This approximation is justified, given that we study working-age adults with incomes near the poverty line – i.e., households that receive very little by way of Social Security, interest, or foreign income. Also, one tool that Marketplace providers use to verify the reasonableness of income projections is prior-year tax forms. Although this approximation is likely to introduce noise in our running variable, we still observe a large discontinuity in insurance status at the Marketplace subsidy threshold of 100% FPL for households living in nonexpansion states (see Section 6.1). We observe no such discontinuity in expansion states.

As expected, 40% of the sample with incomes greater than or equal to 100% FPL (the treated sample) reports having private health insurance compared with 20% of the sample with incomes less than 100% FPL (the control sample). Based on the 2015 Current Population Survey, the Kaiser Family Foundation estimates that 40% of the "potential" Marketplace population is enrolled.[20] The similarity of our enrollment estimate serves as a strong indicator of the reliability of our survey data.

Treated and control samples appear to be statistically similar along demographic characteristics, with modest differences likely attributable to income. For example, respondents in the treated sample are less likely to be students and more likely to be employed with a college education. The average age in our sample is 36, which is consistent with the expectation that our sample is skewed toward younger households; the average age among a similar subsample of American Community Survey (ACS) households is 41.

To develop a proxy for a household's health, we combine all health-related expense and debt information in the survey into one indicator variable.[21] First, we include households with medical debt in excess of 5% of their incomes, a level equal to the top quartile of medical debt among privately insured households. This level of medical debt may signal a recent medical shock or

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

[21] The only medical question on the survey that is not included in our *Health problem* indicator is a question asking how much the household "spent out-of-pocket on medical care." The issue with incorporating this question is that it asks the participant to include "insurance premiums and over-the-counter drugs." These costs are often not indicative of health problems.

be indicative of an ongoing chronic condition (e.g., diabetes) predating the ACA. Next, we include the 11% of households reporting that medical spending causes variation in their monthly expenses, since an affirmative response to this question may indicate frequent medical treatments or prescriptions.[22] Finally, we include households that respond affirmatively to the 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)?" We create a binary measure, *Health problem*, that equals one if a household meets any of these three criteria. By this measure, 42% of households in our analytic (i.e., "potential Marketplace") sample are assumed to have experienced a recent health problem.

There is no standard way to measure being financial constrained in the household finance literature. We proxy for being constrained using the response to the question, "How confident are you that you could come up with $2,000 if an unexpected need arose within the next month?" Households that respond that they could "probably not" or "certainly not" come up with $2,000 are classified as financially constrained using the dummy *Constrained*. Although imperfect, an advantage of using this measure, rather than a household's liquid assets, for example, is that it takes into account network effects. For example, a young worker may have little in his bank account but may have generous parents who can cover expenses. Using this variable, we test (a) whether treatment effects vary within households that lack the resources with which to absorb a medical expense, and (b) whether a significant interaction effect between treatment and health problems might merely reflect a lack of resources among sicker households, which may independently affect and be affected by earning shocks. By this definition, 63% of our potential Marketplace sample from nonexpansion states are *Constrained*. Considering that our sample is low income, this degree of constraint is in line with expectations.[23]

Income volatility has been characterized in a variety of ways in the literature.[24] In this paper, we use indicators of unexpected job loss, unexpected income loss, and self-reported variation in

[22] The survey question first asks "Which of the following best describes your household's expenses over the last 6 months?" Households that report their expenses "vary from one month to the next" are then asked: "What caused your household's expenses to vary over the last 6 months?" One of several possible responses to this question is "Medical expenses: medical or dental bills, health insurance, supporting ill family member."

[23] A Federal Reserve survey estimated that almost half of U.S. households could not easily handle an emergency expense of just $400. See Board of Governors of the Federal Reserve System, "Report on the Economic Well-Being of U.S. Households in 2016," May 2017, www.federalreserve.gov/publications/files/2016-report-economic-well-being-us-households-201705.pdf.

[24] Researchers sometimes measure income volatility as short-term income variations relative to some long-term average (e.g., Gottschalk and Moffitt, 1994), as month-to-month fluctuations in income (e.g., Farrell and Greig, 2016), or as large income drops associated with events like unemployment or illness (Gosselin and Zimmerman, 2008).

income. We would expect income effects generated from uninsured health events to affect some measures more than others. For example, the survey asks about "unexpected" shocks to income and job status, as well as variation in income caused by more predictable factors, such as seasonal employment. For brevity, we narrow much of our discussion to the former set of variables, while also showing main results for the latter. The first outcome indicator of interest is *Job shock*, which measures whether the household recently lost a job unexpectedly. Similarly, we study *Income shock*, which indicates whether the household experienced an unexpected reduction in income.[25] Table 1 shows that 25% and 35% of households in our potential Marketplace sample experienced these two shocks, respectively. The prevalence of both shocks is about 6 percentage points higher in the control sample than in the treatment sample.

Next, the survey asks about variation (either positive or negative) in monthly income. Households that select that their income is *not* "roughly the same amount each month" but instead varies, are asked a follow-up question about the reasons for that variation. Participants select from a menu of provided reasons: "irregular work schedule," "periodic unemployment," "odd jobs," "seasonal employment," "commissions," "bonuses," and "investment income." Participants can choose several reasons simultaneously and may choose "other," in which case they can write in their own explanation. Table 2 shows the frequency of each reason among the subsample of households that report income variation. The last row of the table also shows statistics on the share of households that experience income variation for any reason. The most common reason selected is "irregular work schedule" (63%), and the second most-common reason is "periodic unemployment" (37%). Since the menu does not offer "health problems" as a prefilled reason, we expect that people experiencing income volatility due to health reasons might select this second reason or they might also select "other." After examining the textual answers, 19% of those who selected "other" as a reason for monthly income variation noted a health-related reason.[26] Although substantial, a 19% frequency is unlikely to be sufficient for insurance effects to overcome

[25] In particular, the survey asks, "In the last 6 months, have you or has any member of your household (the people on your tax form) lost a job unexpectedly?" And, "In the last 6 months, have you or has any member of your household (the people on your tax form) had an unexpected reduction in income?" Several questions in the HFS ask about events over the last "6 months" rather than over the last year. The "6 month" rate is likely to be a conservative estimate of the annual rate. As evidence, a later survey revealed that the vast majority ($\rho = 0.86$) of people who responded affirmatively (negatively) to shock-related question referencing the last 6 months responded similarly to the same question when it referenced the last 12 months instead. This effect could be related to the difficulty in recalling the exact timing of a recent past event as well as to positive autocorrelation in shocks within a household.

[26] Some of the more specific health-related responses include "cancer," "alcoholism and depression," "anxiety and transportation," "back injury," and "frequently sick - no paid sick leave." Some other common reasons involved attending school, student loan receipts, tips, weather, child support receipts, and overtime hours.

noise in the "other" category. Given this set of responses, we concentrate our analysis on the indicator of income variation that is because of *Periodic unemployment* (while still showing key results for the other reasons).

Table 2: Reasons for Monthly Income Variation

	All			Treated		Control		
Reason	N	mean	(s.d.)	N	mean	N	mean	diff
Irregular work schedule	2,423	0.634	0.482	750	0.664	1,673	0.621	0.043
Periodic unemployment	2,423	0.366	0.482	750	0.291	1,673	0.400	-0.109
Odd jobs	2,423	0.207	0.405	750	0.151	1,673	0.232	-0.081
Seasonal employment	2,423	0.207	0.406	750	0.163	1,673	0.227	-0.064
Other	2,423	0.126	0.332	750	0.161	1,673	0.110	0.051
Commissions	2,423	0.064	0.244	750	0.073	1,673	0.060	0.013
Bonuses	2,423	0.044	0.205	750	0.052	1,673	0.040	0.012
Investment income	2,423	0.012	0.107	750	0.015	1,673	0.010	0.005
Income variation (all reasons)	4,975	0.488	0.500	1,705	0.442	3,270	0.513	-0.071

Notes: Conditional on reporting income variation during the year and being in the potential Marketplace sample from nonexpansion states, this table reports means and standard deviations of binary variables indicating a selected reason for that income variation. The last row summarizes these statistics using an indicator of having experienced monthly income variation for any reason. The variable of primary interest above is is *Periodic unemployment*, which is an indicator of experiencing monthly income variation because of periods of unemployment. For each variable, the table also reports the difference in sample means (by treatment status). Data include respondents with incomes in the range of (0%, 200%) of FPL. Treatment is assigned at incomes above 100% FPL (i.e., Income$\geq$100%=1).

Descriptive statistics appear to support this choice. The difference in means between the control and treated sample for *Periodic unemployment* is the largest among the reasons in Table 2 (11 percentage points). Moreover, according to Appendix Table A3, this variable is much more correlated with unexpected income disruptions (*Job shock* and *Income shock*) than the other explanations for income variation, signaling a common component.

5 Empirical Framework

This section describes our regression discontinuity framework as well as the results of the tests that verify the assumptions underlying our RD design.

5.1 Regression discontinuity design and estimation

Direct regressions of income shocks on insurance status would generate biased conclusions if insurance status is endogenous to labor decisions.[27] The ACA's Marketplace subsidy policy offers a plausible source of exogenous variation in insurance status among households that lack access to employer plans. Moreover, while income shocks might reduce annual income, making it less likely that a household's income is above the 100% FPL threshold for subsidies, we would not expect to observe a *discontinuity* in the probability of income shocks at 100% FPL after controlling for a smooth polynomial of income around the threshold.

Following the RD strategy of Gallagher et al. (2019b), we introduce some notation to explain the empirical model. Let D_i represent an indicator variable that takes a value of one for households with insurance. In the context of potential Marketplace respondents, insurance refers to nongroup private insurance. Let Z_i denote household income and let c be the eligibility threshold, denoted 100% FPL, such that $1 > P(D = 1|Z \geq c) > P(D = 1|Z < c)$. T_i is an indicator for households with income above the threshold, $T_i = 1(Z_i \geq c)$. Finally, $f(Z_i - c)$ 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 earning shocks. Using this notation, we write the reduced form equation as:

$$Y_i = a + \tau T_i + f(Z_i - c) + T_i \times f(Z_i - c) + \beta X_i + \mu_i, \tag{1}$$

where the vector X_i contains the characteristics of the tax filer in household i, including age, race, dependents, education level, marital status, employment status and student status of the head of household, and unobservable characteristics, such as risk aversion, are absorbed in μ_i.

Our coefficient of interest, τ, captures the "intent-to-treat" (ITT) effects of the ACA's subsidy program on income shocks, Y_i. It can be interpreted as the weighted average change in the rate of income shocks attributable to both the gain in insurance coverage at the cutoff and any difference in the cost/benefits package associated with subsidized Marketplace insurance relative to other forms of coverage (e.g., a catastrophic or COBRA plan).

In some specifications, we adjust our reduced form RD to include our analytic sample of households from states that expanded Medicaid and directly compare treatment effects based on

[27] For example, if a household recognizes that it has a strong likelihood of health trouble, it might be more likely both to get insurance and to miss work because of illness. Such adverse selection would bias our coefficients in the direction of a positive relationship between insurance coverage and income shocks.

expansion status:

$$Y_i = a + \tau_D T_i \times \delta_{ns} + \beta_1 T_i + \beta_2 \delta_{ns} + f(Z_i - c) + T_i \times f(Z_i - c) + \beta_3 X_i + \mu_i, \tag{2}$$

where τ_D captures the effect of having an income above the poverty line in a nonexpansion state rather than in an expansion state. This is essentially an RD difference-in-difference (RD DiD) specification, in which treatment is assigned only when households with income above 100% FPL also reside in nonexpansion states ($T_i \times \delta_{ns} = 1$).

Next, for outcome variables that show significant reduced form effects, we measure the effect of insurance coverage on income shocks by estimating the following set of equations:

$$D_i = b + \pi T_i + h(Z_i - c) + T_i \times h(Z_i - c) + \beta_1 X_i + \epsilon_i \tag{3}$$

$$Y_i = a + \phi D_i + f(Z_i - c) + T_i \times f(Z_i - c) + \beta_2 X_i + \mu_i, \tag{4}$$

where the first equation in the system models the probability of having insurance ($D_i = 1$) as a function of income, household characteristics, and treatment, T_i. In the second equation, the IV estimand, ϕ, is an estimate of the effect of insurance coverage on the probability of income shocks for a subset of households. These are households with income near the threshold with an insurance status that is correlated with whether their income is above or below the threshold ("compliers"). Under the exclusion restriction, ϕ can be interpreted causally as the local average treatment effect (LATE) of having insurance coverage (in a subsidized form) on the probability of income shocks for the subsample of compliers.

We estimate this fuzzy RD specification via a bivariate probit model (Heckman, 1978) and a two-stage least squares (2SLS) model. A bivariate probit model is useful when both the outcome variables (earnings shock indicators) and the endogenous variable (*Insurance*) are binary. Chiburis, Das, and Lokshin (2012) advocate reporting the bivariate probit estimate when sample sizes are below 5,000 and covariates are included in the model.

Since political views, as well as enrollment efforts by state governments and nonprofits, could drive variation in Marketplace enrollment across states, the standard errors in the first-stage could be correlated across households from the same state. It is also reasonable to expect that labor markets within a state-year to be subject to common shocks. Therefore, we report standard errors clustered at the state-year level and show that our results are robust to the inclusion of within-state-year effects. In our fuzzy RD analysis, we demonstrate that our results are robust to bootstrapped errors, taking into account the potential dependence of the observations within state-years through block bootstrapping.[28]

We take the transparent approach of using a uniform kernel (promoted over other kernels by Lee and Lemieux, 2010) and estimating a standard polynomial regression over a given range around the cutoff. Compared with pre-coded RD estimators, this procedure also allows us to study the interactions between treatment effects and indicators of health problems or financial constraint. Such interactions are key to understanding the channel through which health insurance may influence income shocks. Another advantage of this procedure is that it allows for an RD DiD specification. However, for robustness, we show estimates in Appendix A for *Job shock* generated from a triangular kernel and the bias-corrected local polynomial RD estimator and confidence intervals of Calonico, Cattaneo, and Titiunik (2014).

We select three income bandwidths and apply an appropriate polynomial order for each bandwidth using a data-driven approach. We begin this process by calculating the mean-squared error optimal bandwidth under a uniform kernel and linear function of income (Imbens and Kalyanaraman, 2012). The sensitivity to the bandwidth of τ for the main outcome variables is shown in Appendix Figure A1. The optimal bandwidth for the endogenous variable, *Insurance*, is 43 percentage points of FPL around the threshold. The optimal bandwidths for the earnings shock measures are similar, ranging from 28 to 54 percentage points. Therefore, we select the bandwidth ranges of [70%, 130%] FPL, [45%, 155%] FPL, and (0%, 200%) FPL. Next, we select the optimal polynomial order, P, for each bandwidth and outcome variable following a method suggested by Lee and Lemieux (2010). In particular, we repeat the regression in Equation (1) using ever higher-order polynomials and select the order that minimizes the AIC. Selected polynomial orders based on this procedure are shown in Table A4. We select mostly linear and quadratic functions of income.[29]

5.2 Validation of RD

If workers manipulate their labor output to push their income above the eligibility threshold for insurance assistance, a byproduct of this manipulation process might be a lower probability of

[28] Chiburis et al. (2012) recommend reporting bootstrapped standard errors in settings with a binary outcome and binary endogenous variable and when sample sizes are less than 10,000. In block bootstrap, one resamples entire state-years rather than individual observations.

[29] This is fortunate since, according to Gelman and Imbens (2019), higher-order polynomials can produce unreliable estimates.

unexpected job and income loss.[30] Workers at greater risk of health problems would have more incentive to follow this strategy. We run a battery of statistical tests to investigate possible income manipulation in our setting. Before doing so, however, we note that Kucko et al. (2017) test for manipulation of taxable AGI at 100% FPL using the *entire universe* of IRS Form 1040 tax returns from 2014 and 2015. These authors find evidence of income manipulation (based on an excess mass in the income distribution at 100% FPL) only among a small subsample of self-employed tax filers.[31] Presumably, the self-employed are better able to adjust their reported incomes (Saez, 2010). Indeed, Kucko et al. (2017) also show that the manipulation they find is unlikely to reflect changes in actual labor supply but rather changes in reported income. As documented in our main regression tables (Tables 4–6), when we eliminate all self-employed tax filers from our sample (roughly 8% of the sample), our results are unaffected.

Our research design might also help mute the influence of any income manipulation. We assign households to treatment based on previous year's AGI (which is hard to manipulate), while eligibility for subsidies is based on projected MAGI. Our instrument would be weakened if households succeed in manipulating projected MAGI without changing the previous year's AGI. For example, a household that has an income of 98% FPL, according to its 2015 tax form, may, at the time of enrollment, project an income of 102% FPL for the 2016 coverage year. If the Marketplace accepts that projection, the household will qualify for subsidies during the coverage year and we would misassign this household to the control group.

Our first test of income manipulation involves visually examining the density of households around the subsidy threshold. The histogram in Figure 3 shows a small bump in the density just after the threshold. However, it is similar in size to a several of other bumps along the density plot, suggesting a degree of randomness. We apply the technique developed by Cattaneo, Jansson, and Ma (2019) to formally test the significance of this bump in the marginal density of incomes.[32] The p-value for the analytical sample from nonexpansion states with nonzero income is 0.15 (using a uniform kernel and a quadratic polynomial), which indicates that the observed bump in the den-

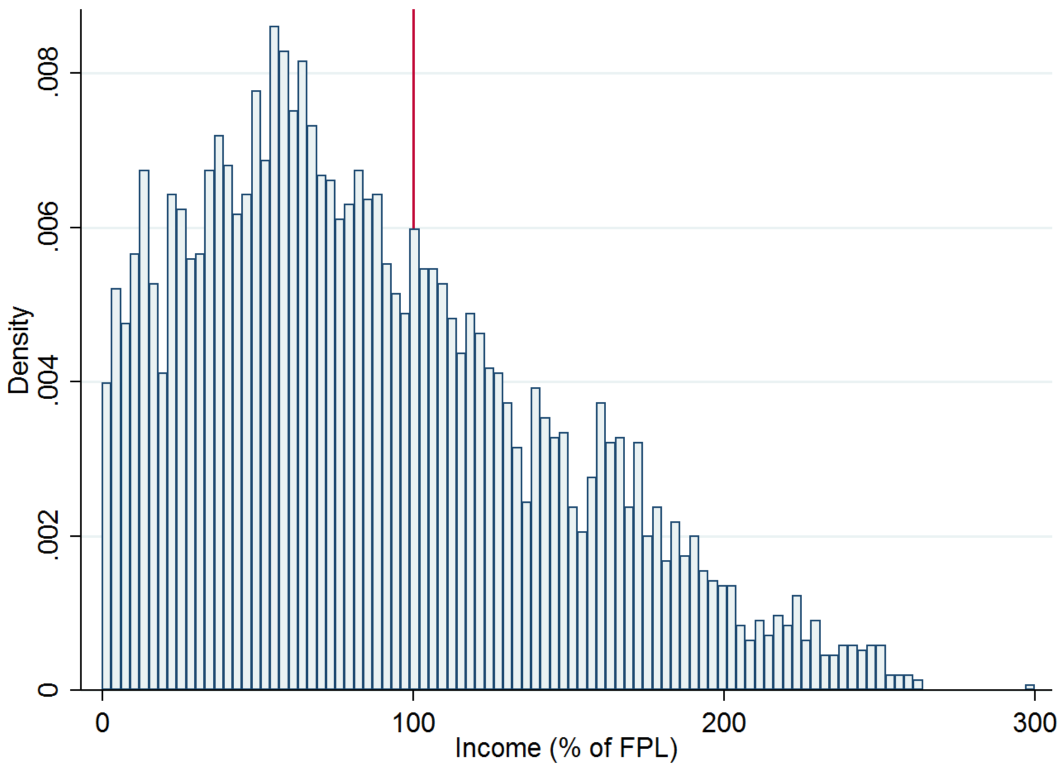
[30]For example, a worker who wants subsidized health insurance and is aware of how to qualify for subsidies may take on extra jobs to insure eligibility. In turn, this additional labor effort might diversify his income sources, making a serious income shock less likely and/or reducing the likelihood of being selected for a layoff.

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

[32]The Cattaneo et al. (2019) manipulation test automatically generates a boundary-corrected kernel density estimator. It requires the choice of only the bandwidth (which the authors' Stata program calculates automatically based on a data-driven technique). In contrast, McCrary (2008) requires choosing additional histogram tuning parameters: the bin length and number of bins.

sity at the threshold is statistically insignificant.[33] Since sicker households are likely to face greater financial incentives to get insured, we also condition the sample on those that are more likely to have experienced a health problem. Nonetheless, the Cattaneo et al. (2019) test continues to indicate that the change in the density near the subsidy cutoff is insignificant (p-value = 0.22). One possibility is that sicker households pursue insurance through an employer, rather than through subsidized private insurance.

Figure 3: Histogram of the Assignment Variable (Income as a % of FPL)



Notes: The figure plots the density of observations along income (as a percentage of FPL). Data include the potential Marketplace sample of households from nonexpansion states with nonzero incomes. Each bar spans 3 percentage points of FPL. The red line corresponds to the subsidy threshold (100% FPL).

In theory, since income manipulators may take on additional jobs and/or hours at their current employer, income manipulation might appear as a discontinuity in the number of W2s that households file as part of Form 1040 or in the share of households that report working full-time. Appendix Table A5 presents estimates from reduced form RD regressions using these two outcome variables. The table indicates no robust discontinuity at the subsidy threshold in *Number of W2s* or *Full-time*.

[33] The bump in the density is also insignificant using a triangular kernel (p-value = 0.14) and using a linear polynomial (p-value = 0.47).

Next, setting aside the evidence against the income manipulation hypothesis, we test whether our main results would likely persist if we could identify and remove manipulators from the data set. We do this by estimating a doughnut RD (Barreca et al., 2011). In particular, we run a reduced form RD specification after eliminating all households with income in the (95%, 105%) FPL window – selected because the size of the density bump at the threshold is roughly 5 percentage points of FPL (Figure 3). Appendix Table A7 displays the doughnut RD estimates. Despite losing nearly 10% of observations, in most cases, treatment estimates remain statistically significant and roughly similar in magnitude to those based on the full sample (see Tables 4 and 5).

Finally, we check the validity of our RD design by ensuring there is no tendency for households with certain demographic characteristics to bunch on one side of the subsidy threshold. A violation of this could signal that certain types of households adjust their incomes to receive subsidies. Appendix Figure A2 offers a visual check. The figure shows the mean of each variable, along with the 95% confidence interval, within small bins of income (each of width 2 percentage points of FPL). With the possible exception of *Age*, which is included among the control variables in our regressions, covariates are smooth around the threshold. More formally, Appendix Table A6 shows the results of local linear regressions that test for balanced covariates around 100% FPL. Although there is a significant difference in *Age* at the subsidy threshold, the F-statistics on all covariates are small. Moreover, the *Age* discontinuity is not significant when using a bandwidth range of (0%, 200%) FPL and a quadratic function of income.[34] Overall, these results are unfavorable to the hypothesis that our findings merely reflect manipulated labor supply to obtain subsidized health insurance.

6 Results

This section presents our main empirical findings. First, in subsection 6.1, we document the first-stage effect of the subsidy threshold on the private insurance rate. Then in subsections 6.2–6.3, we estimate the ITT effect and the LATE of subsidized insurance coverage on income shocks. We also run a number of robustness tests to ensure that our results are not because of random chance.

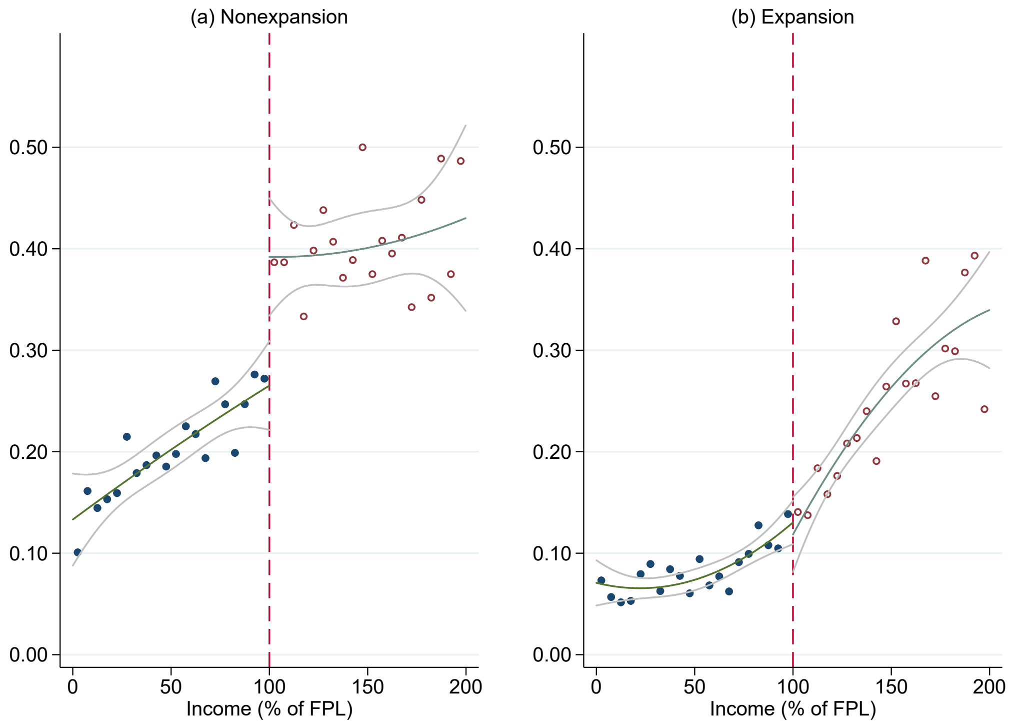
[34]To determine whether the *Age* discontinuity is attributable to random chance, we follow Lee and Lemieux (2010) and use a seemingly unrelated regression design to produce a single test statistic (a χ^2 test). The null hypothesis is that changes in all covariates at the threshold are jointly zero. We cannot reject the null hypothesis (p-value of 0.17). Thus, we conclude that any discontinuities in covariates are sufficiently weak so as not to invalidate our RD design.

6.1 First-stage effects of the subsidy policy

Figure 4 plots the share of potential Marketplace respondents who report having nongroup private insurance coverage within income bins of width equal to 2 percentage points of FPL.[35] Panels (a) and (b) include households from Medicaid nonexpansion states and expansion states, respectively. To provide a sense of the variation in the underlying data, gray lines show the 95% confidence intervals around the mean. There is an economically and statistically large discontinuity in insurance coverage at the threshold for Marketplace subsidies. The prevalence of private insurance among households without access to employer insurance appears to rise by over 10 percentage points at the threshold for Marketplace subsidies. The graph also confirms that the insurance-income relationship near the threshold is well approximated by a linear function of income. As expected, we see no discontinuity in states that expanded Medicaid to households earning over 100% FPL (Panel B of Figure 4); households in these states within this income range are generally covered by Medicaid. This is further evidence that the observed discontinuity in Panel A of Figure 4 is attributable to the Marketplace subsidy policy.

[35]For an analysis of the change in *all* forms of insurance coverage at 100% FPL in the full sample (which includes households that would be disqualified from Marketplace subsidies based on their access to employer plans), see Gallagher et al. (2019b).

Figure 4: Discontinuity in Insurance Coverage at the Subsidy Threshold



Notes: These graphs show the share of potential Marketplace respondents reporting nongroup private insurance coverage within small bins of income around the subsidy threshold (x-axis). Bin width is set to 5 percentage points of FPL. Lines are based on a quadratic function of income ($P = 2$). Data include households with incomes in the range of (0%, 200%) of FPL and without coverage through an employer, school, or family member from the 2015 and 2016 tax filer cross-sections. Graphs restrict the sample to states that did not (a) and did (b) expand Medicaid to households earning up to at least 100% FPL.

More formally, Table 3 documents that the observed discontinuity in nonexpansion states is statistically significant. This table presents estimates of π in Equation (3) over the three bandwidths, controlling for income. The subsidy threshold is associated with about an 11 percentage point increase in the share of the sample from nonexpansion states that report having insurance coverage. For potential Marketplace respondents from nonexpansion states, this coverage is almost always nongroup private insurance. Again, there is no corresponding treatment effect in expansion states. Moreover, the Kleibergen-Paap F-stat (weak instrument test) is greater than 10 in all nonexpansion state specifications, indicative of a strong first-stage effect of the subsidy policy on insurance coverage (Stock, Wright, and Yogo, 2002). These results are robust to the inclusion of demographic covariates and state-year fixed effects.[36] These results suggest that our treatment

[36]Gallagher et al. (2019b) also show the robustness of these results to the inclusion of covariates and fixed effects, as well as the evolution of the discontinuity over time. As expected, the discontinuity has grown since the Marketplace first opened near the beginning of 2014.

identifier, *Income* $\geq$ 100%, is well approximated, despite using past AGI rather than projected MAGI (see Section 4.2). In the next subsections, we exploit the discontinuity in insurance coverage documented here to measure the relationship between health insurance and income shocks.

Table 3: Impact of the Marketplace Subsidy Threshold on Insurance Coverage

Dependent variable: Insurance coverage

	Nonexpansion states			Expansion states		
Income≥100%	0.115***	0.114***	0.127***	-0.007	-0.005	-0.012
	(0.042)	(0.027)	(0.031)	(0.025)	(0.019)	(0.023)
N	1692	3080	4975	3157	5623	9024
KP F-stat	18.18	13.22	12.68	0.09	0.02	0.15
Bandwidth range	[70,130]	[45,155]	(0,200)	[70,130]	[45,155]	(0,200)
P	1	1	2	1	1	2

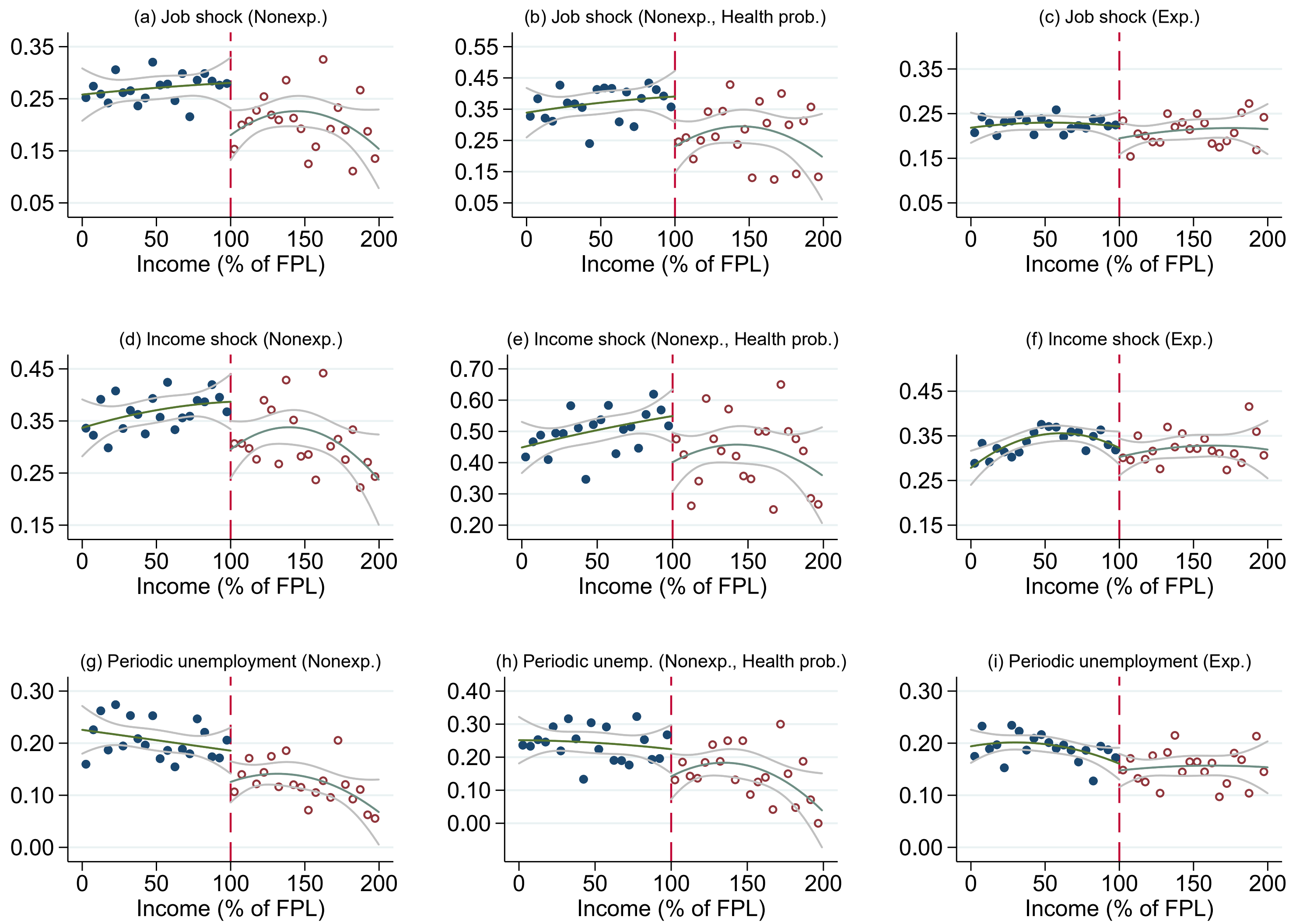
Notes: These are linear probability model estimates using the RD design specified in Equation (3) and based on the analytic sample of respondents living in nonexpansion and expansion states separately. The dependent variable is a binary indicator of any form of insurance coverage. Treatment is assigned at incomes above 100% FPL. The running variable (income as a percentage of FPL) is allowed to vary on both sides of the cutoff. The Kleibergen-Paap F-stat (weak instrument test) from a 2SLS IV regression with *Job Shock* as the final outcome variable is shown below each regression estimate. The bandwidth range and polynomial order, *P*, of the running variable are specified at the bottom of the table. Standard errors, shown in parentheses, are clustered on state-year. *p = 0.1; **p = 0.05; ***p = 0.01 (statistically significant).

6.2 Intent-to-treat effects of the subsidy policy

If access to health insurance affects the earnings stability of households, we should observe a discontinuous change in the share of households reporting income shocks at the Marketplace subsidy threshold. As discussed in Section 4.2, we measure income shocks using three main outcome variables – *Job shock*, *Income shock*, and *Periodic unemployment* – while also showing results for related indicators. We begin by looking for visual evidence of discontinuities in these outcome variables.

Figure 5 plots the share of respondents who report having each type of earnings shock, within income bins equal to 5 percentage points of FPL. For households from nonexpansion states (the left column), these graphs point to a sizable decline in the probability that a respondent experiences *Job shock* and *Income shock*, but a smaller discontinuity in *Periodic unemployment* at the threshold for subsidies. According to the middle column, the prevalence of all three earnings shock indicators rises marginally when the sample is conditioned on households with a greater likelihood of health problems ($Health\ prob. = 1$). Particularly in the case of *Job shock* and *Income shock*, the discontinuity appears to widen compared with the corresponding LHS panel plot. Panels on the RHS use observations from expansion states. We observe no clear discontinuities in expansion states for any of the three indicators, which is consistent with what we would expect if the discontinuities in nonexpansion states were related to the subsidy policy.

Figure 5: Discontinuity in Income Shocks at the Subsidy Threshold



Notes: These graphs show the mean share of potential Marketplace households within small bins of income around the subsidy threshold (x-axis) that report an unexpected loss of a job (*Job shock*), an unexpected reduction in income (*Income shock*), or variation in monthly income because of periods of unemployment (*Periodic unemployment*) (y-axis). Bin width is set to 5 percentage points of FPL. Lines are based on a quadratic function of income ($P = 2$). Data include the analytic sample of households with incomes in the range of (0%, 200%) of FPL. Graphs on the RHS include households from states that expanded Medicaid (Exp.). All other graphs include observations from nonexpansion states (Nonexp.) only. Middle graphs restrict the sample to households from nonexpansion states that are more likely to have experienced a recent health problem.

6.2.1 RD specification

To quantify these observed discontinuities, we use a reduced form RD specification. Tables 4–6 present estimates of τ from Equation 1 for each outcome variable. These can be interpreted as the ITT effects of the subsidy policy. Each of the following tables contains results for one outcome variable, using a variety of bandwidths, functional forms of income, and interaction terms.

We begin by analyzing the effects of the subsidy threshold on *Job shock* (Table 4). Columns 1–4 of Panel A indicate a large, statistically significant treatment effect, τ, of at least 8 percentage points in nonexpansion states. Relative to the mean rate of *Job shock* in the control sample (27%), this corresponds to a 29% decline in the rate of *Job shock* at the subsidy threshold.

We proxy for the likelihood that a household faces health problems on the extensive margin by including the indicator variable $Health\ prob.$ as a regressor in Equation 3. In Column 5, after controlling for $Health\ prob.$, we find a positive association with the probability of an unexpected job loss. The interaction term with treatment ($Health\ prob.$ x $Income \geq 100\%$) captures the subsidy policy effect on *Job shock* for households that are most likely to have health problems. In each case, the effect of the threshold appears to be heavily concentrated among households with possible health issues. This result is consistent with the idea that access to health insurance reduces the influence of health issues on income shocks.

Financial stress could affect, and be affected by, earnings shocks. Financial stress may also be correlated with both health and insurance access, creating a spurious relationship in the previous regressions. In the final column, we employ the indicator $Constrained$, which is a measure of access to financial resources. Unsurprisingly, Column 6 shows a correlation between job loss and having limited access to emergency resources. However, the statistically insignificant interaction term between $Constrained$ and $Income \geq 100\%$ in Column 6 suggests that the treatment effect of insurance access does not differ between constrained and unconstrained households. Instead, similar to the results in Column 5, the interaction effect ($Health\ prob.$ x $Income \geq 100\%$) remains significant and similar in magnitude. These results indicate that the magnifying effect of potential health issues on the relationship between insurance access and earnings stability does not merely reflect a lack of resources among sicker and/or uninsured households.

Effects are unique to nonexpansion states. Panel B in Table 4 repeats the regressions using the analytic sample from states that expanded Medicaid. Consistent with our results being related to health insurance access, we find no significant coefficients on treatment ($Income \geq 100\%$). The

Table 4: Reduced Form RD Estimates of the Effect of the Subsidy Threshold on *Job Shock*

Dependent variable: *Job shock*

	Panel A. Nonexpansion states						Panel B. Expansion states					
	(1)	(2)	(3)	(4)	(5)	(6)	(1)	(2)	(3)	(4)	(5)	(6)
Income≥100%	-0.120***	-0.137***	-0.075**	-0.044	-0.048	-0.061*	-0.028	-0.036	-0.022	-0.012	-0.008	-0.002
	(0.044)	(0.042)	(0.036)	(0.038)	(0.036)	(0.036)	(0.031)	(0.036)	(0.027)	(0.027)	(0.028)	(0.029)
Income≥100% x Health prob.				-0.080***	-0.069**	-0.063*				-0.026	-0.001	0.002
				(0.021)	(0.031)	(0.032)				(0.019)	(0.032)	(0.034)
Health prob.				0.156***	0.142***	0.145***				0.144***	0.124***	0.119***
				(0.014)	(0.021)	(0.021)				(0.010)	(0.019)	(0.020)
Income≥100% x Health prob. x Constrained					-0.021	-0.023					-0.037	-0.040
					(0.051)	(0.050)					(0.044)	(0.045)
Income≥100% x Constrained					0.003	0.014					-0.007	-0.002
					(0.037)	(0.039)					(0.022)	(0.024)
Constrained					0.119***	0.118***					0.094***	0.094***
					(0.018)	(0.021)					(0.011)	(0.011)
Health prob. x Constrained					-0.006	-0.007					0.011	0.016
					(0.026)	(0.023)					(0.022)	(0.024)
Sample	All	All	All	All	All	Ex. Self-Emp	All	All	All	All	All	Ex. Self-Emp
N	1692	3078	4973	4973	4973	4582	3157	5623	9024	9024	9024	8376
Bandwidth range	[70, 130]	[45, 155]	(0, 200)	(0, 200)	(0, 200)	(0, 200)	[70, 130]	[45, 155]	(0, 200)	(0, 200)	(0, 200)	(0, 200)
P	1	2	2	2	2	2	1	2	2	2	2	2

Notes: This table shows reduced form RD (linear probability model) estimates based on the potential Marketplace sample of respondents from nonexpansion (Panel A) and expansion states (Panel B). The dependent variable, *Job shock*, is binary and indicates a recent unexpected loss of a job. 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. Bandwidth range, polynomial order (P), and use of demographic covariates as well as state-year fixed effects are specified at the bottom of the table. "Nonexp." indicates that a household resides in a Medicaid nonexpansion state. "Health prob." indicates those households that are more likely to have experienced health problems. "Constrained" indicates those households that report *not* having access to $2,000 in case of an emergency. The final column of each panel excludes tax filers who report being self-employed. All regressions include demographic covariates and state-year fixed effects (not shown). Standard errors, shown in parentheses, are clustered on state-year. *p = 0.1; **p = 0.05; ***p = 0.01 (statistically significant).

Table 5: Reduced Form RD Estimates of the Effect of the Subsidy Threshold on *Income Shock*

Dependent variable: *Income shock*

	Panel A. Nonexpansion states						Panel B. Expansion states					
	(1)	(2)	(3)	(4)	(5)	(6)	(1)	(2)	(3)	(4)	(5)	(6)
Income≥100%	-0.095**	-0.072**	-0.078**	-0.055	-0.080*	-0.084**	-0.012	-0.023	-0.014	0.001	-0.003	-0.006
	(0.036)	(0.031)	(0.033)	(0.035)	(0.041)	(0.041)	(0.036)	(0.027)	(0.030)	(0.028)	(0.032)	(0.032)
Income≥100% x Health prob.				-0.056**	-0.060	-0.072				-0.041	-0.022	-0.003
				(0.025)	(0.049)	(0.052)				(0.025)	(0.035)	(0.037)
Health prob.				0.218***	0.193***	0.200***				0.206***	0.182***	0.168***
				(0.016)	(0.028)	(0.028)				(0.011)	(0.020)	(0.020)
Income≥100% x Health prob. x Constrained					-0.011	0.002					-0.032	-0.057
					(0.064)	(0.066)					(0.040)	(0.044)
Income≥100% x Constrained					0.043	0.035					0.008	0.010
					(0.042)	(0.043)					(0.025)	(0.026)
Constrained					0.118***	0.126***					0.131***	0.134***
					(0.021)	(0.023)					(0.013)	(0.013)
Health prob. x Constrained					0.009	0.007					0.008	0.021
					(0.030)	(0.033)					(0.026)	(0.026)
Sample	All	All	All	All	All	Ex. Self-Emp	All	All	All	All	All	Ex. Self-Emp
N	1692	3078	4973	4973	4973	4582	3157	5623	9024	9024	9024	8376
Bandwidth range	[70, 130]	[45, 155]	(0, 200)	(0, 200)	(0, 200)	(0, 200)	[70, 130]	[45, 155]	(0, 200)	(0, 200)	(0, 200)	(0, 200)
P	1	1	2	2	2	2	1	1	2	2	2	2

Notes: This table shows reduced form RD (linear probability model) estimates based on the potential Marketplace sample of respondents from nonexpansion (Panel A) and expansion states (Panel B). The dependent variable, *Income shock*, is binary and indicates a recent unexpected reduction in income. 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. Bandwidth range, polynomial order (*P*), and use of demographic covariates as well as state-year fixed effects are specified at the bottom of the table. "Nonexp." indicates that a household resides in a Medicaid nonexpansion state. "Health prob." indicates those households that are more likely to have experienced health problems. "Constrained" indicates those households that report *not* having access to $2,000 in case of an emergency. The final column of each panel excludes tax filers who report being self-employed. All regressions include demographic covariates and state-year fixed effects (not shown). Standard errors, shown in parentheses, are clustered on state-year. *p = 0.1; **p = 0.05; ***p = 0.01 (statistically significant).

Table 6: Reduced Form RD Estimates of the Effect of the Subsidy Threshold on Income Variation Because of *Periodic Unemployment*

Dependent variable: *Periodic unemployment*

	Panel A. Nonexpansion states						Panel B. Expansion states					
	(1)	(2)	(3)	(4)	(5)	(6)	(1)	(2)	(3)	(4)	(5)	(6)
Income≥100%	-0.070*	-0.088**	-0.087**	-0.077*	-0.066	-0.064	-0.025	-0.032	-0.036	-0.029	-0.032	-0.028
	(0.035)	(0.039)	(0.038)	(0.040)	(0.042)	(0.043)	(0.026)	(0.026)	(0.027)	(0.027)	(0.029)	(0.030)
Income≥100% x Health prob.				-0.020	-0.038	-0.048				-0.017	-0.013	-0.024
				(0.020)	(0.039)	(0.036)				(0.021)	(0.037)	(0.037)
Health prob.				0.057***	0.076**	0.081***				0.090***	0.061***	0.070***
				(0.012)	(0.028)	(0.029)				(0.010)	(0.021)	(0.022)
Income≥100% x Health prob. x Constrained					0.023	0.036					-0.002	0.011
					(0.047)	(0.046)					(0.058)	(0.057)
Income≥100% x Constrained					-0.016	-0.015					-0.001	-0.010
					(0.022)	(0.022)					(0.025)	(0.025)
Constrained					0.076***	0.071***					0.042***	0.050***
					(0.018)	(0.020)					(0.014)	(0.014)
Health prob. x Constrained					-0.041	-0.040					0.033	0.020
					(0.035)	(0.036)					(0.029)	(0.028)
Sample	All	All	All	All	All	Ex. Self-Emp	All	All	All	All	All	Ex. Self-Emp
N	1692	3078	4973	4973	4973	4582	3157	5623	9024	9024	9024	8376
Bandwidth range	[70, 130]	[45, 155]	(0, 200)	(0, 200)	(0, 200)	(0, 200)	[70, 130]	[45, 155]	(0, 200)	(0, 200)	(0, 200)	(0, 200)
P	1	2	3	3	3	3	1	2	3	3	3	3

Notes: This table shows reduced form RD (linear probability model) estimates based on the potential Marketplace sample of respondents from nonexpansion (Panel A) and expansion states (Panel B). The dependent variable, *Periodic unemployment*, is binary and indicates recent variation in income because of periods of unemployment. 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. Bandwidth range, polynomial order (*P*), and use of demographic covariates as well as state-year fixed effects are specified at the bottom of the table. "Nonexp." indicates that a household resides in a Medicaid nonexpansion state. "Health prob." indicates those households that are more likely to have experienced health problems. "Constrained" indicates those households that report *not* having access to $2,000 in case of an emergency. All regressions include demographic covariates and state-year fixed effects (not shown). The final column of each panel excludes tax filers who report being self-employed. Standard errors, shown in parentheses, are clustered on state-year. *p = 0.1; **p = 0.05; ***p = 0.01 (statistically significant).

final column of each panel documents that these estimates change very little when we exclude tax filers that report being self-employed (and, therefore, better able to manipulate their income) from the sample.

Table 5 presents regression results for the next outcome variable, *Income shock*. Estimates are similar, though not quite as statistically significant as those for *Job shock*. The treatment effect starts at 7 percentage points in Panel A, Columns 1–4. In relative terms, this translates into an 18% decline in the probability of unexpected loss of income at the subsidy threshold. We also observe a statistically significant interaction effect between treatment and *Health prob.* in Column 5. In Column 6, we find that treatment effects are, once again, unrelated to being financially constrained, even among the sample with a greater likelihood of health problems. Adding the *Constrained* interaction appears to make the effect of *Health prob.* x *Income* $\geq$ 100% slightly larger but noisier.

In Columns 1–4 of Table 6, we find that the treatment is associated with a 7 percentage point decline, or 37% in relative terms, in the probability of reporting *Periodic unemployment*. However, unlike the results for *Job shock* and *Income shock*, some of the treatment coefficients are only marginally statistically significant. Moreover, there appears to be no change in the treatment effect associated with the health problems indicator (Column 5). There is also no change in the treatment effect from being financially constrained (Column 6).

One possible explanation for the weaker effects of the Marketplace subsidy threshold on *Periodic unemployment* is that, unlike in the case of the two prior outcome variables, a household need not experience an "unexpected" form of unemployment to respond "yes" to the *Periodic unemployment* question on the survey. If some of the households experiencing *Periodic unemployment* anticipated this outcome – for instance, because of a restaurant closing, a slump in construction business, or a conflation of periodic unemployment with seasonal work – treatment effects would likely be statistically weaker. Presumably, since the exact timing of most health problems is unpredictable, health problems (and any moderating effects of health insurance) should be revealed through unpredictable forms of earnings disruption.

6.2.2 Regression discontinuity difference-in-difference estimates

Table 7 shows the results of our reduced form RD DiD specification from Equation 2 for all three of our outcome variables. All regressions use an income range of (0%, 200%) FPL and include either a quadratic or cubic polynomial function of income, as well as demographic controls and state-year fixed effects. The treatment effect, τ_D, is the coefficient on $Income \geq 100\% \times Nonexp.$, as well as its triple interaction with $Health\ prob.$

For a household with a given income above the poverty line, living in a nonexpansion state ($Income \geq 100\% \times Nonexp.$) reduces the probability of an earnings shock by an additional 3–5 percentage points, depending on the outcome measure. Relative to the mean of the control group in nonexpansion states, these estimates correspond to relative declines in the probabilities of *Job shock*, *Income shock*, and *Periodic Unemployment* of 16%, 9%, and 22%, respectively. Although the magnitudes of our estimates are smaller in our RD DiD specification compared with our pure RD specification, they still are economically meaningful. For the triple interactions between treatment and $Health\ prob.$, our coefficient estimates possess the correct sign but are only statistically significant in the case of *Job shock*.[37]

[37]Estimates are very similar when we control for *Constraint* and its interaction with treatment (not shown for brevity). As before, we find no combined effect of having insurance access and being financially constrained on income shocks.

Table 7: Reduced Form RD DiD Estimates of the Effect of the Subsidy Threshold on Income Shocks

Dependent variable:	*Job shock*		*Income shock*		*Periodic unemployment*	
	(1)	(2)	(5)	(6)	(9)	(10)
Income≥100%	-0.024	-0.018	-0.024	-0.014	-0.039*	-0.033
	(0.022)	(0.022)	(0.024)	(0.023)	(0.022)	(0.022)
Income≥100% x Nonexp.	-0.046***	-0.016	-0.033**	-0.012	-0.041***	-0.037**
	(0.012)	(0.014)	(0.015)	(0.019)	(0.016)	(0.016)
Income≥100% x Health prob.		-0.026		-0.040		-0.017
		(0.019)		(0.025)		(0.020)
Income≥100% x Health prob. x Nonexp.		-0.052*		-0.014		-0.003
		(0.029)		(0.035)		(0.028)
Health prob. x Nonexp.		0.013		0.013		-0.032**
		(0.017)		(0.019)		(0.015)
Health prob.		0.144***		0.206***		0.090***
		(0.010)		(0.011)		(0.010)
N	13999	13999	13999	13999	13999	13999
P	2	2	2	2	3	3

Notes: This table shows reduced form RD (linear probability model) estimates based on the analytic sample of respondents from all states. The dependent variables are: *Job shock* (which indicates a recent unexpected loss of a job), *Income shock* (which indicates a recent unexpected reduction in income), and *Periodic unemployment* (which indicates recent variation in income because of periods of unemployment). 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. The polynomial order (P) of income, chosen to be mean squared error optimal, is specified at the bottom of the table. "Nonexp." indicates that a household resides in a state that has not yet expanded Medicaid. "Health prob." indicates those households that are more likely to have experienced health problems. All regressions use a bandwidth range of (0%, 200%) of FPL and include demographic covariates and state-year fixed effects. To avoid collinearity with state-year fixed effects, a "Nonexp." dummy is not included. Standard errors, shown in parentheses, are clustered on state-year. *p = 0.1; **p = 0.05; ***p = 0.01 (statistically significant).

6.2.3 Other outcome variables

The nature of the earnings shock (i.e., whether it is unexpected or expected) may influence the treatment effect. Thus far, we have analyzed only those outcome variables from the survey where the question explicitly conditions on an "unexpected" cause. Now, we turn to the set of survey questions that ask respondents who report experiencing monthly income variation to select among a set of prefilled reasons for that variation (see Section 4.2).

Table 8 shows reduced form RD results (using the model in Equation 1). Among the provided reasons, only in the case of *Periodic unemployment* is there consistent evidence of a discontinuity at the subsidy threshold for the average household. For example, all coefficients on *Seasonal employment* are small and statistically equivalent to zero. However, after conditioning the sample on *Health prob.* in Column 5, there is a significant treatment effect at the 5% level for *Commissions* and *Bonuses*, which may conceivably vary according to health insurance access if health events influence a worker's performance. Nonetheless, only a small share of our sample reports income variation because of commissions or bonuses. These findings suggest that access to subsidized insurance is associated with fewer income shocks only to the extent that income shocks are driven by unexpected causes or reflect a worker's performance.

Putting all the reduced form results together, health insurance access appears to have a sizable effect on the probability of certain forms of income shocks – namely, unexpected forms as well as those that reflect performance. The concentration of the treatment effect among households with a greater likelihood of health problems, in most specifications, is consistent with the hypothesis that health insurance might act on labor output, in part, by reducing the frequency and duration of health shocks that may prevent work.

Table 8: Reduced Form RD Estimates of the Effect of the Subsidy Threshold on Income Variation by Associated Reason in Nonexpansion States

	(1)	(2)	(3)	(4)	(5)
Income variation (all reasons)					
Income≥100%	-0.037	-0.074	-0.061	-0.050	-0.046
	(0.054)	(0.060)	(0.062)	(0.062)	(0.090)
Irregular work schedule					
Income≥100%	-0.010	-0.009	0.000	0.007	-0.009
	(0.053)	(0.061)	(0.061)	(0.063)	(0.089)
*Periodic unemployment**					
Income≥100%	-0.070*	-0.102***	-0.088**	-0.089**	-0.121**
	(0.035)	(0.035)	(0.039)	(0.037)	(0.054)
Odd jobs					
Income≥100%	-0.010	-0.011	-0.010	-0.010	0.071*
	(0.022)	(0.025)	(0.026)	(0.027)	(0.038)
Seasonal employment					
Income≥100%	-0.002	-0.025	-0.021	-0.020	-0.008
	(0.028)	(0.031)	(0.031)	(0.032)	(0.040)
Other					
Income≥100%	0.031	0.009	0.008	0.025	0.050
	(0.023)	(0.026)	(0.026)	(0.024)	(0.039)
Commissions					
Income≥100%	0.018	0.015	0.012	0.006	-0.040**
	(0.017)	(0.019)	(0.019)	(0.020)	(0.019)
Bonuses					
Income≥100%	0.007	0.002	0.002	0.001	-0.063**
	(0.018)	(0.020)	(0.020)	(0.019)	(0.029)
Investment income					
Income≥100%	0.008	0.010	0.010	0.008	0.008
	(0.008)	(0.008)	(0.008)	(0.007)	(0.007)
N	1692	3080	3080	4975	2091
Health prob. restriction	No	No	No	No	Yes
Bandwidth range	[70, 130]	[45, 155]	[45, 155]	(0, 200)	(0, 200)
P	1	2	2	3	3
Covariates	No	No	Yes	Yes	Yes
State-year F.E.	No	No	Yes	No	No

Notes: This table shows reduced form RD (linear probability model) estimates based on the potential Marketplace sample of respondents from Medicaid nonexpansion states. The sample includes all such households, regardless of whether they experienced income variation. The first dependent variable, *Income variation*, is an indicator of monthly income variation for any reason. The remaining variables are binary indicators of experiencing income variation for a selected reason. 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. Sample restrictions, bandwidth range, polynomial order (*P*), and use of demographic covariates as well as state-year fixed effects are specified at the bottom of the table. "Health prob." refers to those households that are more likely to have experienced health problems. Standard errors, shown in parentheses, are clustered on state-year. *p = 0.1; **p = 0.05; ***p = 0.01 (statistically significant).

6.3 Local average treatment effects of subsidized insurance coverage

The previous subsection documents a consistently strong ITT effect of the subsidy policy on *Job shock* and *Income shock*.[38] Assuming the exclusion restriction holds, we now measure the causal relationship between insurance coverage and these two outcome variables. In interpreting our IV results, we assign more importance to estimates that are smaller in magnitude because measurement error in our assignment variable could inflate the IV estimates by some unknown margin. Our assignment of households to treatment based on prior tax year income, rather than projected future income, necessarily generates some error. Measurement error should attenuate both our first-stage and reduced form regressions estimates. However, if the bias is larger with respect to predicting insurance status, then our second-stage estimate would be inflated. Despite this, we observe a marked (non-S-shaped) first-stage discontinuity (Section 6.1), which indicates that our IV estimate remains indicative of the correct direction and rough magnitude of the true relationship (Pei and Shen, 2017).

Table 9 presents fuzzy RD estimates of ϕ in Equation 4, estimated using a bivariate probit model.[39] Panel A shows estimates for *Job shock* as the outcome variable. The LATE is economically and statistically large. The smallest coefficient, seen in Column 5, implies that going from being uninsured to receiving insurance coverage because of the subsidy policy reduces the probability of unexpected job loss by 25 percentage points relative to being uninsured.[40] This effect is large. To compare this with sample means in nonexpansion states, the rate of *Job shock* among all insured (irrespective of insurance type) households in our full sample is 11% versus 28% among the uninsured.

As mentioned previously, our IV estimate could be inflated or it could simply be correcting for positive bias in the direct estimate. Appendix Table A9 presents estimates from direct probit and OLS regressions of income shocks on *Insurance*. Direct estimates are negative and significant, yet they are an order of magnitude smaller than those estimated through the fuzzy RD in Table 9 using

[38] We also estimate these effects for *Periodic unemployment*. Results are available upon request. We find evidence of a negative causal effect. However, the result is statistically sensitive to bandwidth/polynomial order and appears to be unrelated to health status.

[39] Chiburis et al. (2012) suggest running a score test to validate the null hypothesis of bivariate normality. We perform a Rao score test (Murphy, 2007); we cannot reject the null (p-value=0.50).

[40] Appendix Table A8 presents 2SLS IV results, which tend to be larger in magnitude than the bivariate probit, and introduces an interaction term between insurance and the health problems indicator ($Insurance \times Health\,prob$). In particular, we add two terms in our 2SLS IV model, $Health\,prob.$ and $Insurance \times Health\,prob.$ In the first-stage regression (not shown), we use $Income \geq 100\%$ as an instrument for *Insurance* and the interaction term, $Income \geq 100\% \times Health\,prob.$, as an instrument for $Insurance \times Health\,prob.$ There is a negative and significant coefficient on the interaction term.

exogenous variation in insurance status. For example, the estimated effect of insurance coverage on the probability of *Job shock* in Column 5 is 11 percentage points, based on the probit model, and 25 percentage points, based on the bivariate probit model. Thus, unobserved heterogeneity across households appears to bias the direct estimate in a positive direction, consistent with the direction of adverse selection: Those who are more likely to get sick and miss work may also tend to enroll in insurance plans.

Conditioning on the sample with a higher likelihood of health problems in Column 3, the coefficient grows in magnitude, suggesting that insurance coverage reduces the likelihood of unexpected job loss, in part, by mitigating the positive effect of *Health prob*. on job loss. Results in Panel B, where *Income shock* is the outcome variable, are similar.[41]

[41] These results are robust to using bootstrapped standard errors. For example, Figure A3, plots the predicted probability of both outcome variables at different probabilities of *Insurance*, generated from the bivariate probit models in Column 2 of Table 9. Standard errors are calculated via state-year-based block bootstrap using 200 repetitions.

Table 9: Fuzzy RD Estimates of the Effect of Insurance Coverage on Income Shocks (Biprobit)

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. Dependent variable = *Job shock*						
Insurance	-1.673***	-1.461***	-1.586***	-1.269**	-0.900*	-1.449***
	(0.103)	(0.205)	(0.100)	(0.588)	(0.523)	(0.175)
	[-0.509]	[-0.455]	[-0.522]	[-0.371]	[-0.254]	[-0.430]
Panel B. Dependent variable = *Income shock*						
Insurance	-1.566***	-1.408***	-1.582***	-1.530***	-1.582***	-0.834**
	(0.103)	(0.316)	(0.051)	(0.364)	(0.029)	(0.383)
	[-0.499]	[-0.467]	[-0.519]	[-0.498]	[-0.510]	[-0.290]
Sample restriction	No	No	Health prob.	No	No	No
N	1692	3080	1234	3080	3080	4975
Bandwidth range	[70, 130]	[45, 155]	[45, 155]	[45, 155]	[45, 155]	(0, 200)
P	1	1	1	1	1	2
Covariates	No	No	No	Yes	Yes	Yes
State-year F.E.	No	No	No	No	Yes	Yes

Notes: This table shows fuzzy RD estimates, based on a bivariate probit (Biprobit) model. Data include the potential Marketplace sample of respondents living in nonexpansion states. The dependent variables, *Job shock* and *Income shock*, are binary and indicate a recent unexpected job loss or reduction in income, respectively. The key explanatory variable is insurance coverage. Average marginal effects, holding all variables at their means, are shown in brackets. The running variable (income as a percentage of FPL) is allowed to vary on both sides of the cutoff. In the first-stage (not shown), private insurance coverage is instrumented by incomes above 100% FPL (i.e., "Income≥100%"=1). In Column 3, the sample is restricted based on *Health prob.*, an indicator that a household that is more likely to have experienced health problems. The Kleibergen-Paap rk Wald F statistics (weak instrument test) are reported in Table 3. The estimates are based on the optimal bandwidth/polynomial order selected for the endogenous variable, *Insurance* (see Table A4). Sample restrictions, bandwidth range, polynomial order (*P*), and use of covariates and fixed effects are also specified at the bottom of the table. Standard errors, shown in parentheses, are clustered on state-year: *p = 0.1; **p = 0.05; ***p = 0.01 (statistically significant).

6.4 Robustness checks

In all of the tables discussed previously, we show that our results remain fairly stable under a variety of bandwidth and polynomial combinations, as well as under the inclusion of demographic covariates and state-year fixed effects. We also document that statistically significant treatment effects are unique to households in Medicaid nonexpansion states and hold when we exclude the self-employed (Tables 4–7). Consistent with there being no reduction in the cost of health insurance at 100% FPL in states that expanded Medicaid, we find no consistent change in the probability of income shocks at 100% FPL in these states. This finding signals that our treatment effects are driven by the Marketplace subsidy policy, rather than by some other federal social program.

Next, we conduct placebo tests and present the results in Appendix Table A10. These tests do two things: (a) they indicate that the income polynomials we selected properly control for the relationship between the running variable and the outcome variable, and (b) they signal that discontinuities observed at 100% FPL are most likely related to the subsidy policy rather than to random bumps in the probability of income shocks along income lines. We estimate a reduced form RD using two placebo thresholds, one at 75% FPL and the other at 125% FPL. We do not find any discontinuous change in income shocks at these points.

Next, in Appendix Table A11, we verify that there is no strong tendency for discontinuities to exist in 2015 but not in 2016, and vice versa, as this might signal that something other than the Marketplace subsidy program is responsible for our treatment effects. Although unpooling the two cross-sectional samples results in a meaningful loss of statistical power, we observe no such tendency.

Appendix Table A12 shows reduced form and fuzzy RD estimates for *Job shock* generated from the bias-corrected local polynomial RD estimator and confidence intervals of Calonico et al. (2014) (the existing procedure does not take into account nonlinearities or permit interactions with treatment). We allow the Calonico et al. (2014) procedure to select two different MSE-optimal bandwidths (below and above the cutoff) and apply a triangular or uniform kernel. Reduced form estimates are statistically significant in all specifications and similar in magnitude to those in our main analysis (Table 4). Fuzzy RD estimates are significant in five out of seven specifications and remain large in magnitude.[42]

7 Discussion

The results in the previous section show that eligibility for subsidized Marketplace plans is associated with a large and robust discontinuous decline in unexpected income shocks. Households have been found to take pay cuts to have jobs that provide guaranteed hours throughout the year (Morduch and Schneider, 2017). Our RD estimates, therefore, suggest a welfare benefit to health insurance in terms of greater earnings stability. The dollar value that the average low income household places on earnings stability is unclear, however.

Premiums paid for unemployment insurance might provide an estimate of the value of at least one form of earnings instability. However, unemployment insurance has traditionally been a publicly provided good (Hendren, 2016), making it difficult to estimate the average willingness to

[42]The inflation in the Calonico et al. (2014) IV estimates as well as their weaker statistical significance may be related to the smaller magnitudes of the corresponding first-stage estimates and their larger standard errors. This contrasts with the clear evidence of strong first-stage effect of at least 11 percentage points in Figure 4.

pay for it. In recent years, however, a handful of new private unemployment and wage insurance plans have come to market.[43] SafetyNet is a start-up offering an insurance product that is particularly relevant to the forms of income shocks examined in this paper.[44] SafetyNet offers "income flow" protection – a lump-sum, severance-like payout in the event of unexpected job loss or inability to work for 30 days because of unexpected illness or injury. According to our conversations with a company spokesman, the average client paid a monthly premium of $25 to gain $7,500 in protection in 2016.

Combining our RD estimate for *Job shock* with the valuation implied by SafetyNet's product offers a back-of-the-envelope estimate of the welfare benefit, W, of health insurance in terms of employment stability:

$$W = \phi \times E[C^{JL}],$$

where ϕ is our fuzzy RD estimate and the $E[C^{JL}]$ is the expected cost of a job loss.

The premium for a year of SafetyNet's protection is $300. This is an estimate of the expected cost of a job loss for a household in which the cost of a job loss, C^{JL}, is $7,500. These figures imply a probability of job loss, P^{JL}, of 4% in a given year (i.e., since $E[C^{JL}] = P^{JL} \times C^{JL}$). The probability of job loss is substantially higher in our analytic sample at 25%, which is consistent with our analytic sample comprising households near the poverty line with the types of jobs that do not provide health benefits. For simplicity, we assume that $7,500 is still an appropriate upper-bound level of protection for households in our sample. This amount of protection equates to about 5 months' worth of the annual gross income of the average household just above the poverty line in our analytic sample. For comparison, the average unemployment duration in the U.S. was just over 6 months at the end of 2016.[45] As a lower-bound, we set $C^{JL} = \$4,025$, which is about a quarter of the average annual income of a household in our analytic sample. Combined with public unemployment insurance, which generally covers 50% of income for 6 months, this amount should fully smooth income through a 6-month job loss. We adjust the SafetyNet premium to compensate for the higher probability of job loss in our analytic sample, such that the expected cost of job loss is now $E[\hat{C}^{JL}] = 25\% \times \$7,500 = \$1,875$ per year for the upper-bound estimate of

[43]See the *New York Times*, May 27, 2016, "Finally, Private Unemployment Insurance. But Will Anyone Buy It?" by Ron Lieber. Available at: `https://www.nytimes.com/2016/05/28/your-money/finally-private-unemployment-insurance-but-will-anyone-buy-it.html?_r=1`.

[44]See `https://safetynet.com/product-basics`.

[45]FRED Economic Data for "Average (Mean) Duration of Unemployment," available at `https://fred.stlouisfed.org/series/UEMPMEAN`.

C^{JL}. Using the lower-bound estimate of C^{JL}, $E[\hat{C}^{JL}] = \$1,006$ per year.

Next, we multiply these expectations by our most conservative fuzzy RD estimate from Table 9, $\phi = 25.4$, which is the percentage point reduction in the probability of an unexpected job loss attributable to going from being uninsured to having subsidized private health insurance coverage. The resulting welfare benefit of insurance is $W = \phi \times E[\hat{C}^{JL}] = 25.4\% \times \$1,006$ *or* $\$1,875 =$ \$256–\$476 per year.

A welfare benefit of \$256 to \$476 per year, in terms of reduced exposure to job loss, is substantial relative to the premium paid for subsidized Marketplace insurance (\$244 per year for a 30-year-old single adult earning just over 100% FPL). However, it is small compared with the unsubsidized premium for private health insurance for the same adult (\$3,186 per year). These rudimentary estimates suggest that a reduction in the probability of job loss is a meaningful incentive for households to get health insurance when the coverage is subsidized.

8 Conclusion

Income volatility and employment insecurity are increasingly common experiences for low income U.S. households (PEW, 2017; Dynan et al., 2012) and the consequences can be severe. We ask whether health insurance has any bearing on the stability of the earnings stream of households. Our results confirm that health insurance reduces the probability of unexpected negative shocks to employment and income. We use an RD design, exploiting the income threshold to receive subsidies for nongroup private health insurance under the ACA as a source of exogenous variation in health insurance status. Our most conservative estimates (from a regression discontinuity difference-in-difference design) indicate that eligibility to purchase subsidized private insurance is associated with a 16% and 9% relative decline in the rates of unexpected job and income loss, respectively. Our fuzzy RD estimate, combined with information on the premiums paid for private unemployment insurance, implies a \$256–\$476 per year welfare benefit of health insurance in terms of reduced exposure to job loss.

We offer suggestive evidence on the mechanism. The evidence is consistent with a story in which health insurance reduces the probability of income shocks by affecting physical and/or mental health episodes and, thus, labor output. Effects are strongest among households with rough indicators of health issues. Furthermore, we do not find any reduction in the prevalence of more predictable income shocks, such as because of seasonal employment. Unfortunately, our data are insufficient to formally test for a health-performance mechanism since we lack measures of productivity, such as hours worked. Instead, we cast doubt on the most likely alternative explanations. Namely, our results do not appear to be driven by a correlation between health and financial distress, which may independently affect and be affected by income shocks. We also find no evidence to support the theory that a discontinuity in income shocks at the Marketplace subsidy threshold stem from households manipulating their incomes upward to qualify for the subsidies.

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A For Online Publication Only: Appendix Tables and Figures

Table A1: Sample Size and Medicaid Eligibility Thresholds for Nonexpansion States

	"Potential Marketplace" sample size			Medicaid income ceilings			
				Childless adults		Parents	
Nonexpansion state:	2015	2016	Total	2015	2016	2015	2016
ALABAMA	69	63	132	0%	0%	18%	18%
ALASKA	10		10	0%	138%	146%	143%
FLORIDA	278	360	638	0%	0%	34%	34%
GEORGIA	238	260	498	0%	0%	38%	37%
IDAHO	84	80	164	0%	0%	27%	26%
INDIANA	241		241	0%	139%	24%	139%
KANSAS	44	46	90	0%	0%	38%	38%
LOUISIANA	46	58	104	0%	0%	24%	24%
MAINE	33	35	68	0%	0%	105%	105%
MISSISSIPPI	38	38	76	0%	0%	28%	27%
MISSOURI	201	200	401	0%	0%	23%	22%
MONTANA	24		24	0%	138%	51%	138%
NEBRASKA	34	33	67	0%	0%	55%	63%
NORTH CAROLINA	248	248	496	0%	0%	45%	44%
OKLAHOMA	105	88	193	0%	0%	46%	44%
SOUTH CAROLINA	119	121	240	0%	0%	67%	67%
SOUTH DAKOTA	16	10	26	0%	0%	53%	52%
TENNESSEE	121	109	230	0%	0%	103%	101%
TEXAS	326	336	662	0%	0%	19%	18%
UTAH	93	97	190	0%	0%	46%	45%
VIRGINIA	210	185	395	0%	0%	45%	39%
WYOMING	9	10	19	0%	0%	58%	57%
Sample total	2,587	2,377	4,964				

Notes: This table shows the number of households in our potential Marketplace sample from nonexpansion states with incomes in the range of (0%, 200%) of FPL, by the household's state of residence. The table also show Medicaid income eligibility ceilings (as a % of FPL) for childless adults and parents as of January of the designated year.

Table A2: Sample Insurance Types by FPL

FPL Bin:	[0,25)	[25,50)	[50,75)	[75,100)	[100,125)	[125,150)	[150,175)	[175,200)	[>200)	Total	Wgt. Total
Panel A. Nonexpansion states:											
Employer	3.8%	7.4%	13.5%	23.5%	29.6%	42.3%	49.4%	55.6%	62.4%	28.1%	31.2%
Family & Student	42.7%	42.0%	30.5%	24.9%	23.6%	19.7%	17.4%	15.9%	13.8%	27.3%	14.1%
Medicaid	9.1%	9.4%	9.1%	6.2%	3.4%	2.2%	0.7%	0.9%	0.2%	5.3%	7.7%
VA, Medicare, & Other	10.1%	8.1%	7.6%	7.7%	5.7%	6.0%	5.4%	4.6%	6.7%	7.1%	9.1%
nongroup private	8.8%	9.0%	11.4%	12.0%	16.7%	14.4%	11.8%	10.7%	8.8%	11.4%	10.9%
Uninsured	25.6%	24.0%	27.8%	25.7%	21.1%	15.5%	15.3%	12.3%	8.1%	20.7%	26.9%
Total	12.9%	13.9%	14.2%	12.1%	11.3%	9.9%	8.9%	7.4%	9.3%	100.0%	100.0%
Panel B. Expansion states:											
Employer	3.7%	5.6%	10.3%	16.6%	27.6%	36.9%	46.1%	52.8%	58.3%	24.6%	25.4%
Family & Student	46.0%	48.1%	37.2%	29.4%	24.2%	22.4%	19.4%	18.0%	16.5%	31.3%	18.5%
Medicaid	28.6%	27.4%	30.5%	29.2%	24.8%	17.8%	9.2%	6.3%	4.1%	21.7%	30.1%
VA, Medicare, & Other	8.6%	6.3%	6.3%	6.3%	5.9%	3.9%	4.9%	4.6%	5.2%	6.0%	8.3%
nongroup private	4.1%	3.9%	4.4%	6.7%	7.7%	9.4%	10.5%	9.2%	8.2%	6.6%	6.2%
Uninsured	9.0%	8.7%	11.3%	11.8%	9.8%	9.7%	9.9%	9.1%	7.6%	9.7%	11.5%
Total	14.4%	14.1%	13.8%	12.2%	10.3%	9.7%	8.4%	7.1%	9.9%	100.0%	100.0%

Notes: This table shows the portion of full, 2015–16 sample respondents reporting various forms of health insurance coverage within bins of income (measured as a percentage of FPL). The top and bottom panels show respondents from Medicaid nonexpansion and expansion states, respectively. The final column applies national demographic weights based on the demographic characteristics of the 2015 American Community Survey (ACS) sample. In nonexpansion states, the uninsurance rate is 20.7% compared with 9.7% in the states that expanded Medicaid. As expected, this difference increases as respondents' incomes decrease. This difference is largely attributable to Medicaid, as the share of households with Medicaid coverage is 21.7% in expansion states and only 5.3% in nonexpansion states. However, this gap is partly offset by a higher incidence of employer-based and private health insurance coverage in nonexpansion states. Also of note in this table is the shift in nongroup private insurance coverage between households with incomes just below the Marketplace subsidy threshold (100% FPL) and households with incomes just above the subsidy threshold in nonexpansion states. The rate of private insurance coverage is 12% for those just below the threshold and 16.7% for those just above; a difference of almost 5 percentage points. By comparison, the shift in private insurance coverage in expansion states across these same income levels is only 1 percentage point. These results provide preliminary evidence that Marketplace subsidies are reducing the cost of private health insurance and, thus, increasing the rate of private insurance coverage.

Table A3: Correlation Among Earnings Shock Variables

	Job shock*	Income shock*	Income variation (all)	Irreg. work schedule	Periodic unemploy.*	Odd jobs	Seasonal employment	Other	Commissions	Bonuses	Investment income
Job shock*	1.00										
Income shock*	0.56	1.00									
Income variation (all)	0.16	0.20	1.00								
Irreg. work schedule	0.06	0.13	0.68	1.00							
Periodic unemploy.*	0.39	0.30	0.48	0.22	1.00						
Odd jobs	0.12	0.10	0.34	0.20	0.31	1.00					
Seasonal employment	0.08	0.09	0.34	0.21	0.25	0.25	1.00				
Other	0.01	0.07	0.26	-0.01	0.01	0.02	0.02	1.00			
Commissions	0.06	0.07	0.18	0.04	0.06	0.08	0.05	0.01	1.00		
Bonuses	-0.01	-0.01	0.15	0.11	0.03	0.07	0.08	0.00	0.15	1.00	
Investment income	-0.01	-0.01	0.08	0.00	0.02	0.06	0.02	-0.01	0.10	0.06	1.00

Notes: This table reports coefficients of correlation on all variables capturing income shocks and variation in the HFS. The three outcome variables of primary interest in this study (i.e., those that potentially capture unexpected shocks to income because of health events) are denoted with a "*". Data include the potential Marketplace sample of respondents living nonexpansion states with incomes in the range of (0%, 200%) of FPL. The sample includes all such households, regardless of whether they experienced income variation.

Table A4: Selected Polynomial Order, P

Variable	Bandwidth range	P
Insurance	[70%,130%] FPL	1
	[45%,155%] FPL	1
	(0%,200%) FPL	2
Job shock	[70%,130%] FPL	1
	[45%,155%] FPL	2
	(0%,200%) FPL	2
Income shock	[70%,130%] FPL	1
	[45%,155%] FPL	1
	(0%,200%) FPL	2
Periodic unemployment	[70%,130%] FPL	1
	[45%,155%] FPL	2
	(0%,200%) FPL	3

Notes: This table lists the optimal polynomial order, P, of income corresponding to each income bandwidth range. The selection is made by running regressions of the outcome variable on various polynomials of income, which are allowed to vary on either side of the subsidy threshold (100% FPL), and selecting the polynomial order that minimizes the AIC.

Table A5: Reduced Form RD Estimates of the Effect of the Subsidy Threshold on *Number of W2s* and *Full-time*

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A. Dependent variable = *Number of W2s*							
Income≥100%	-0.032	-0.058	-0.031	0.099	0.156	0.098	0.050
	(0.081)	(0.085)	(0.099)	(0.071)	(0.114)	(0.071)	(0.077)
Income≥100% x Health prob.							0.125
							(0.075)
Health prob.						0.119***	0.078**
						(0.033)	(0.038)
Panel B. Dependent variable = *Full-time*							
Income≥100%	0.082*	0.093*	0.054	0.022	0.019	0.022	0.037
	(0.043)	(0.047)	(0.047)	(0.038)	(0.059)	(0.038)	(0.039)
Income≥100% x Health prob.							-0.042
							(0.025)
Health prob.						0.023*	0.036*
						(0.013)	(0.019)
N	1692	3080	3080	4975	2090	4975	4975
Sample restriction	No	No	No	No	Health prob.	No	No
Bandwidth range	[70, 130]	[45, 155]	[45, 155]	(0, 200)	(0, 200)	(0, 200)	(0, 200)
P	1	1	1	2	2	2	2
Covariates	No	No	Yes	Yes	Yes	Yes	Yes
State-year F.E.	No	No	Yes	No	No	No	No

Notes: This table shows reduced form RD (linear probability model) estimates based on the potential Marketplace sample of respondents from nonexpansion states. The dependent variable in Panel A, *Number of W2s*, is the number of W2s included on the household's 1040 form. The dependent variable in Panel B, *Full-time*, is binary and indicates whether the household reports full-time (rather than part-time) employment on the survey. 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. Sample restrictions, bandwidth range, polynomial order (*P*), and use of demographic covariates as well as state-year fixed effects are specified at the bottom of the table. "Health prob." refers to those households that are more likely to have experienced health problems. Standard errors, shown in parentheses, are clustered on state-year. *p = 0.1; **p = 0.05; ***p = 0.01 (statistically significant).

Table A6: Tests for Smoothness of Covariates at the Subsidy Threshold

Dependent variable	Coef. (Income≥100%)	Std. error	F-stat	N
Age	1.752**	(0.825)	3.145	3,080
White (non-Hispanic)	-0.011	(0.025)	5.884	3,080
Number of kids	-0.083	(0.081)	11.439	3,080
College graduate	0.052	(0.037)	3.117	3,080
Male	0.013	(0.038)	2.027	3,080
Single	-0.032	(0.032)	4.025	3,080
Employed	0.020	(0.025)	3.427	3,080
Student	-0.002	(0.033)	5.469	3,080

Notes: This table reports local linear RD estimates based on the potential Marketplace sample of respondents living in nonexpansion states and with incomes in the bandwidth range of [45%, 155%] FPL. The dependent variables are various demographic covariates. 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. Standard errors, shown in parentheses, are clustered on state-year. *p = 0.1; **p = 0.05; ***p = 0.01 (statistically significant).

Table A7: Test of Robustness of RD Estimates to a Doughnut RD

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dependent variable:	*Job shock*				*Income shock*			
Income≥100%	-0.124*	-0.165**	-0.073	-0.038	-0.153**	-0.078*	-0.109**	-0.080
	(0.066)	(0.071)	(0.050)	(0.049)	(0.063)	(0.045)	(0.054)	(0.051)
Income≥100% x Health prob.				-0.089***				-0.071***
				(0.025)				(0.025)
Health prob.				0.181***				0.244***
				(0.014)				(0.016)
N	1406	2792	4687	4687	1406	2792	4687	4687
Bandwidth range	[70,130]	[45,155]	(0,200)	(0,200)	[70,130]	[45,155]	(0,200)	(0,200)
P	1	2	2	2	1	1	2	2

Notes: This table shows estimates from a doughnut reduced form RD specification, in which households with incomes in (95%, 105%) FPL are removed. Data include the potential Marketplace sample of households living in Medicaid nonexpansion states, subject to the aforementioned restrictions. The dependent variables, *Job shock* and *Income shock*, are binary and indicate a recent unexpected job loss or reduction in income, respectively. 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 cutoffs. Bandwidth range and polynomial order (*P*) are specified at the bottom of the table. Regressions do not include demographic covariates or fixed effects. Standard errors, shown in parentheses, are clustered on state-year. *p = 0.1; **p = 0.05; ***p = 0.01 (statistically significant).

Table A8: Fuzzy RD Estimates of the Effect of Insurance Coverage (2SLS IV)

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. Dependent variable = *Job shock*						
Insurance	-0.644*	-0.646	-0.330	-0.332	-0.718	-0.570
	(0.351)	(0.404)	(0.399)	(0.404)	(0.444)	(0.452)
Insurance x Health prob.			-0.666***	-0.660***		-0.434***
			(0.183)	(0.173)		(0.134)
Health prob.		0.107***	0.294***	0.292***		0.222***
		(0.021)	(0.052)	(0.050)		(0.039)
Panel B. Dependent variable = *Income shock*						
Insurance	-0.651**	-0.774*	-0.509	-0.513	-0.738*	-0.629
	(0.329)	(0.416)	(0.406)	(0.418)	(0.417)	(0.416)
Insurance x Health prob.			-0.560**	-0.557**		-0.310**
			(0.254)	(0.253)		(0.145)
Health prob.		0.178***	0.335***	0.335***		0.259***
		(0.024)	(0.074)	(0.074)		(0.041)
Sample restriction	No	No	No	No	No	No
N	3080	3080	3080	3080	4975	4975
Bandwidth range	[45, 155]	[45, 155]	[45, 155]	[45, 155]	(0, 200)	(0, 200)
P	1	1	1	1	2	2
Covariates	No	Yes	Yes	Yes	Yes	Yes
State-year F.E.	No	No	No	Yes	Yes	Yes

Notes: This table shows fuzzy RD estimates, based on a linear IV (2SLS) model. Data include the potential Marketplace sample of respondents living in nonexpansion states. The dependent variables, *Job shock* and *Income shock*, are binary and indicate a recent unexpected job loss or reduction in income, respectively. The key explanatory variable is insurance coverage. The running variable (income as a percentage of FPL) is allowed to vary on both sides of the cutoff. In the first-stage (not shown), private insurance coverage is instrumented by incomes above 100% FPL (i.e., "Income$\geq$100%"=1) and by its interaction with *Health prob.*, an indicator of a household that is more likely to have experienced health problems. The Kleibergen-Paap rk Wald F statistic (weak instrument test) are reported in Table 3. Sample restrictions, bandwidth range, polynomial order (*P*), and use of covariates and fixed effects are also specified at the bottom of the table. Standard errors, shown in parentheses, are clustered on state-year: *p = 0.1; **p = 0.05; ***p = 0.01 (statistically significant).

Table A9: Direct Estimates of the Effect of Insurance Coverage (Probit & OLS)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	Probit						OLS					
Panel A. Dependent variable = *Job shock*												
Insurance	-0.468***	-0.380***	-0.325***	-0.361***	-0.353***	-0.354***	-0.112***	-0.098***	-0.097***	-0.093***	-0.100***	-0.094***
	(0.070)	(0.049)	(0.083)	(0.053)	(0.053)	(0.046)	(0.013)	(0.014)	(0.014)	(0.014)	(0.012)	(0.012)
	[-0.143]	[-0.119]	[-0.118]	[-0.112]	[-0.109]	[-0.109]						
Insurance x Health prob.									-0.103***	-0.105***		-0.095***
									(0.029)	(0.029)		(0.021)
Health prob.								0.144***	0.182***	0.184***		0.175***
								(0.015)	(0.019)	(0.019)		(0.015)
Panel B. Dependent variable = *Income shock*												
Insurance	-0.297***	-0.236***	-0.181**	-0.222***	-0.219***	-0.225***	-0.086***	-0.069***	-0.069***	-0.067***	-0.078***	-0.069***
	(0.049)	(0.044)	(0.092)	(0.043)	(0.044)	(0.043)	(0.016)	(0.016)	(0.016)	(0.016)	(0.015)	(0.016)
	[-0.110]	[-0.087]	[-0.072]	[-0.081]	[-0.079]	[-0.081]						
Insurance x Health prob.									-0.091***	-0.094***		-0.070***
									(0.031)	(0.031)		(0.023)
Health prob.								0.204***	0.238***	0.241***		0.231***
								(0.022)	(0.024)	(0.024)		(0.017)
Sample restriction	No	No	Health prob.	No	No	No	No	No	No	No	No	No
N	1692	3080	1234	3080	3080	4975	3080	3080	3080	3080	4975	4975
Bandwidth range	[70, 130]	[45, 155]	[45, 155]	[45, 155]	[45, 155]	(0, 200)	[45, 155]	[45, 155]	[45, 155]	[45, 155]	(0, 200)	(0, 200)
P	1	1	1	1	1	2	1	1	1	1	2	2
Covariates	No	No	No	Yes	Yes	Yes	No	Yes	Yes	Yes	Yes	Yes
State-year F.E.	No	No	No	No	Yes	Yes	No	No	No	Yes	Yes	Yes

Notes: This table shows estimates from direct regressions of the outcome variables on *Insurance* using a probit and OLS model. Data include the potential Marketplace sample of respondents living in nonexpansion states. The dependent variables, *Job shock* and *Income shock*, are binary and indicate a recent unexpected job loss or reduction in income, respectively. Average marginal effects, holding all variables at their means, are shown in brackets. The running variable (income as a percentage of FPL) is allowed to vary on both sides of the cutoff. *Health prob.* is an indicator that a household is more likely to have experienced health problems. Sample restrictions, bandwidth range, polynomial order (*P*), and use of covariates and fixed effects are specified at the bottom of the table. Standard errors, shown in parentheses, are clustered on state-year: *p = 0.1; **p = 0.05; ***p = 0.01 (statistically significant).

Table A10: Reduced Form RD Estimates of the Effect of Placebo Thresholds

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Dependent variable:	*Job shock*				*Income shock*				*Periodic unemployment*			
Income≥75%	0.042	0.036	0.058	0.037	0.038	0.044	0.040	0.043	0.055	0.052	0.108	0.056
	(0.033)	(0.030)	(0.040)	(0.031)	(0.036)	(0.034)	(0.059)	(0.034)	(0.041)	(0.041)	(0.069)	(0.041)
Income≥125%	0.015	0.020	0.078	0.019	0.040	0.044	0.051	0.043	0.056	0.046	0.121*	0.043
	(0.043)	(0.041)	(0.077)	(0.041)	(0.048)	(0.049)	(0.092)	(0.050)	(0.040)	(0.039)	(0.067)	(0.039)
N	4975	4975	2091	4975	4975	4975	2091	4975	4975	4975	2091	4975
Health prob. restriction	No	No	Yes	No	No	No	Yes	No	No	No	Yes	No
Bandwidth range	(0, 200)	(0, 200)	(0, 200)	(0, 200)	(0, 200)	(0, 200)	(0, 200)	(0, 200)	(0, 200)	(0, 200)	(0, 200)	(0, 200)
P	2	2	2	2	2	2	2	2	3	3	3	3
Covariates	No	Yes	Yes	Yes	No	Yes	Yes	Yes	No	Yes	Yes	Yes
State-year F.E.	No	No	No	Yes	No	No	No	Yes	No	No	No	Yes

Notes: This table shows reduced form RD estimated (linear probability model) based on placebo thresholds. Treatment is assigned in separate regressions at incomes above 75% FPL and 125% FPL. The three dependent variables employed are designated at the top of the table. Data include the potential Marketplace sample of households living in nonexpansion states. The running variable (income as a percentage of FPL) is allowed to vary on both sides of the cutoffs. Bandwidth range, polynomial order (*P*), and controls are specified at the bottom of the table. Standard errors, shown in parentheses, are clustered on state-year. *p = 0.1; **p = 0.05; ***p = 0.01 (statistically significant).

Table A11: Reduced Form RD Estimates of the Effect of the Subsidy Threshold, by Year

Dependent variable: *Job shock*

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Dependent variable:	*Job shock*						*Income shock*					
Income≥100%	-0.098*	-0.079	-0.042	-0.059	-0.016	-0.001	-0.112**	-0.099**	-0.031	-0.040	-0.007	-0.004
	(0.052)	(0.050)	(0.029)	(0.048)	(0.054)	(0.033)	(0.043)	(0.045)	(0.035)	(0.050)	(0.055)	(0.030)
Income≥100% x Health prob.		-0.047*	-0.034		-0.114***	-0.029		-0.031	-0.039		-0.090***	-0.053
		(0.027)	(0.025)		(0.029)	(0.028)		(0.038)	(0.036)		(0.030)	(0.035)
Health prob.		0.145***	0.166***		0.163***	0.128***		0.241***	0.214***		0.197***	0.203***
		(0.020)	(0.010)		(0.020)	(0.015)		(0.027)	(0.017)		(0.017)	(0.014)
Income≥100% x Nonexp.			-0.020			-0.011			-0.027			0.014
			(0.020)			(0.018)			(0.024)			(0.029)
Income≥100% x Health prob. x Nonexp.			-0.012			-0.083**			0.009			-0.035
			(0.036)			(0.041)			(0.052)			(0.046)
Health prob. x Nonexp.			-0.019			0.035			0.026			-0.005
			(0.022)			(0.025)			(0.029)			(0.021)
Nonexp.			0.021			0.023**			-0.013			0.000
			(0.014)			(0.010)			(0.014)			(0.019)
N	2592	2592	6837	2383	2383	7162	2592	2592	6837	2383	2383	7162
Expansion status	Nonexp	Nonexp	All	Nonexp	Nonexp	All	Nonexp	Nonexp	All	Nonexp	Nonexp	All
Year	2015	2015	2015	2016	2016	2016	2015	2015	2015	2016	2016	2016

Notes: This table shows reduced form RD (linear probability model) estimates based on the analytic sample of respondents by year. The dependent variables, *Job shock* and *Income shock*, are binary and indicates a recent unexpected loss of a job or of income, respectively. 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. "Health prob." refers to those households that are more likely to have experienced health problems. All regressions use a bandwidth range of (0%, 200%) FPL and a quadratic polynomial. All regressions include demographic covariates but exclude state-year fixed effects. The Medicaid expansion status of included states is designated at the bottom of the table. Standard errors, shown in parentheses, are clustered on state-year. *p = 0.1; **p = 0.05; ***p = 0.01 (statistically significant).

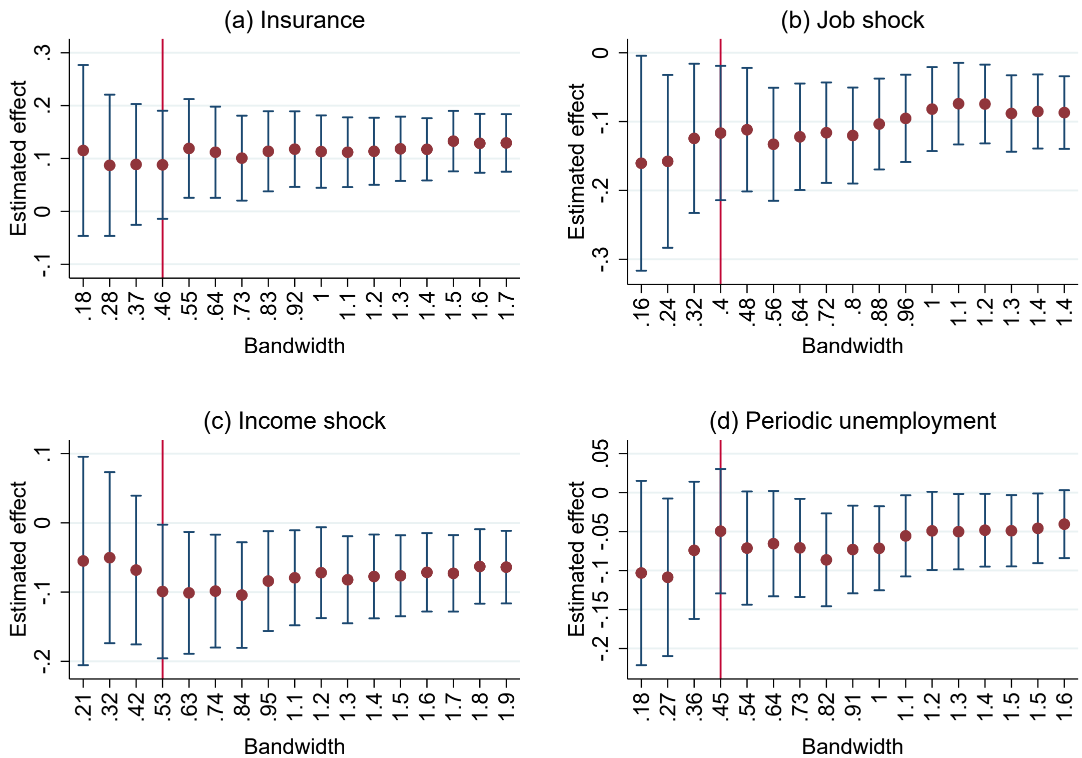
Table A12: Calonico et al. (2014) Reduced Form and Fuzzy RD estimates of the Effect of Insurance Coverage on *Job Shock*

Dependent variable: *Job shock*

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A. Reduced form RD (ITT) estimates:							
Income≥100%	-0.127***	-0.132***	-0.139***	-0.130***	-0.119**	-0.134**	-0.092***
	(0.045)	(0.045)	(0.042)	(0.042)	(0.042)	(0.050)	(0.035)
Bandwidth	[66, 117]	[70, 115]	[56, 123]	[62, 121]	[70, 130]	[45, 155]	(0, 200)
N	1554	1360	2117	1857	1692	3080	4975
Panel B. Fuzzy RD (LATE) estimates:							
Insurance	-1.167**	-1.095*	-1.138**	-1.177*	-1.198	-1.395	-1.023*
	(0.559)	(0.486)	(0.530)	(0.579)	(0.684)	(0.943)	(0.565)
First-stage	0.114**	0.125**	0.121*	0.110*	0.099	0.096	0.090*
	(0.048)	(0.048)	(0.052)	(0.050)	(0.051)	(0.061)	(0.041)
Bandwidth	[64, 124]	[64, 121]	[57, 125]	[61, 126]	[70, 130]	[45, 155]	(0, 200)
N	1790	1706	2117	1980	1692	3080	4975
Kernel	Uniform	Uniform	Triangular	Triangular	Triangular	Triangular	Triangular
P	1	1	1	1	1	2	2
Covariates	No	Yes	No	Yes	Yes	Yes	Yes

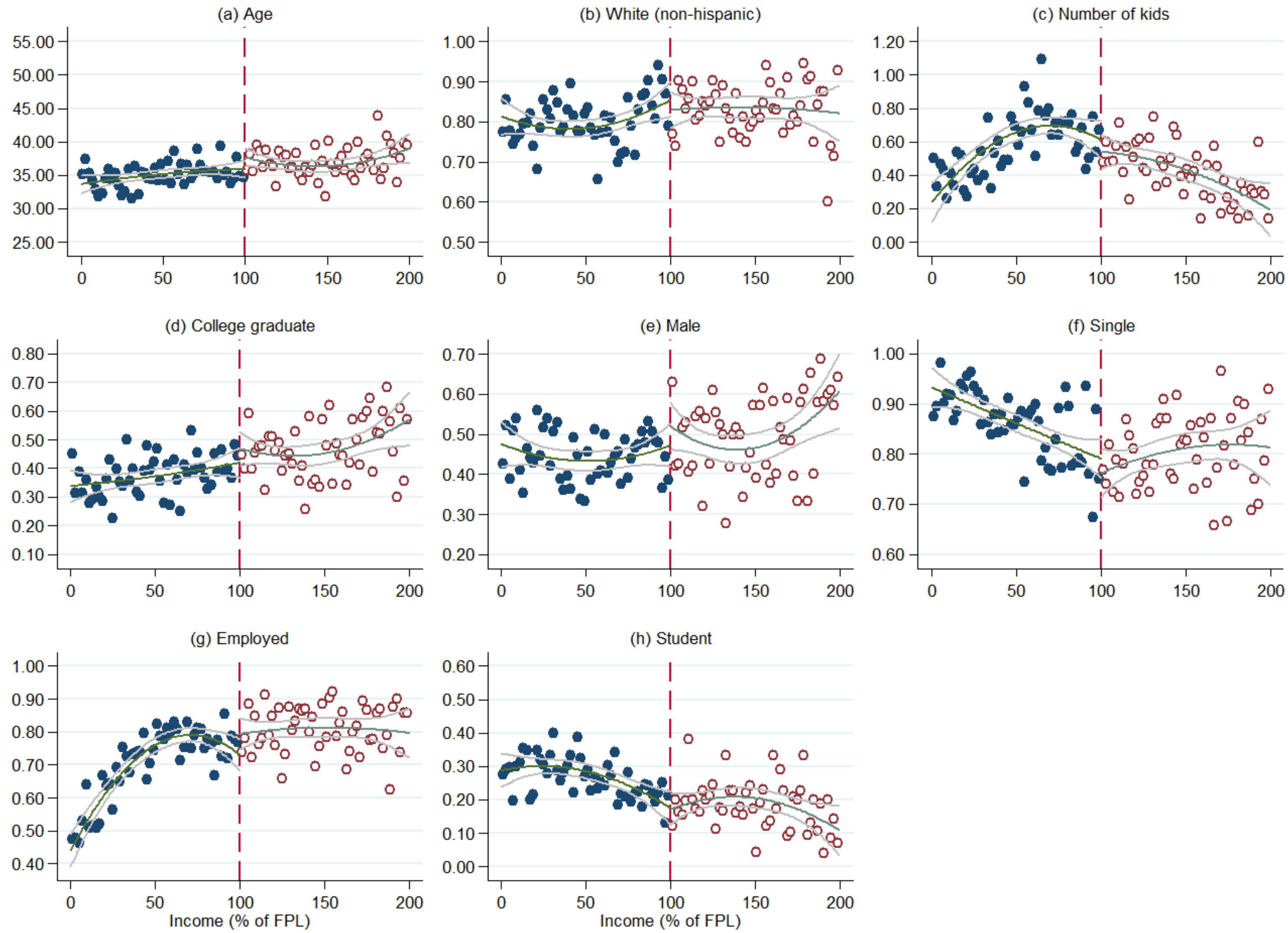
Notes: This table shows reduced form RD estimates (Panel A) and fuzzy RD (2SLS IV) estimates (Panel B) using the bias-corrected local polynomial RD estimator and confidence intervals of Calonico et al. (2014) and either a uniform or triangular kernel, as specified. Data include the potential Marketplace sample of households living in nonexpansion states. The dependent variable, *Job shock*, indicates a recent unexpected loss of a job. In Panel B, the key explanatory variable is insurance coverage. Insurance coverage is instrumented by incomes above 100% FPL (i.e., "Income≥100%"=1). First-stage estimates are shown below. Bandwidth ranges are specified at the bottom of the table. In Columns 1–4, we allow the Calonico et al. (2014) procedure to select two different MSE-optimal bandwidths (below and above the cutoff) for the RD estimator. For comparison purposes, in the remaining columns, we apply the same bandwidths and polynomial orders as used in Tables 4 and 9. Demographic covariates are employed when specified. Standard errors are shown in parentheses. P-values are calculated from the Calonico et al. (2014) confidence intervals: *p = 0.1; **p = 0.05; ***p = 0.01 (statistically significant).

Figure A1: Sensitivity to Bandwidth of the Effect of the Subsidy Threshold on Outcomes



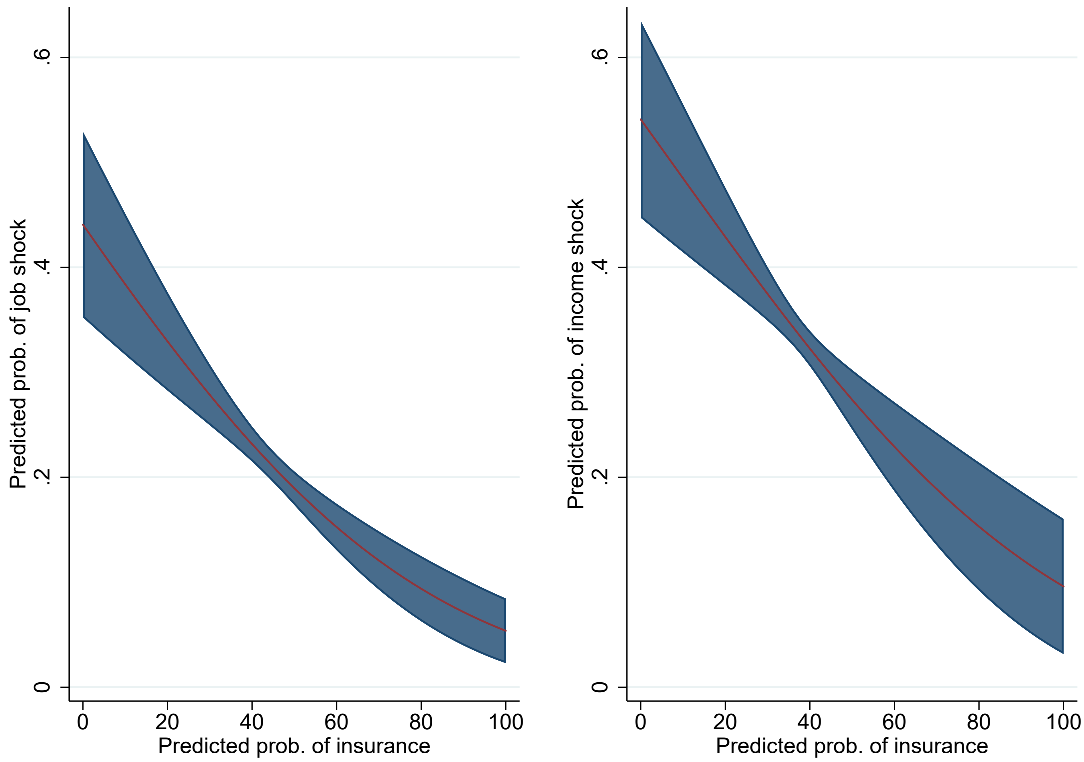
Notes: This figure shows the sensitivity to chosen bandwidth of the reduced form RD estimate (i.e., τ_r in Equation 1) for each dependent variable. The red line shows the bandwidth selected using the method in Imbens and Kalyanaraman (2012).

Figure A2: Smoothness of Demographic Covariates at the Subsidy Threshold



Notes: This figure shows the mean value or mean share of households reporting a variety of demographic characteristics within small bins of income around the subsidy threshold (x-axis). Bin width is set to 2 percentage points of FPL. Lines are based on a quadratic function of income ($P = 2$). Data include the potential Marketplace sample of households living in nonexpansion states with incomes in the range (0%, 200%) FPL.

Figure A3: Predicted Effect of Insurance Coverage on the Likelihood of *Job Shock* and *Income Shock*



Notes: This figure shows the predicted probability of *Job shock and Income shock* at various probabilities of insurance coverage (in increments of 2%) – estimated using "Income>100%" as an instrument. Predictions are generated using the bivariate probit model shown in Column 2 of Table 9. This model uses a bandwidth range of [45%,155%] FPL, a linear polynomial, and excludes demographic covariates. All variables are held at their means. Standard errors are calculated via state-year-based block bootstrap using 200 repetitions. The figures shows the 95% confidence bounds.