Medicaid and Household Savings Behavior: New Evidence from Tax Refunds*

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Abstract

Using data on over 57,000 low-income tax filers, we estimate the effect of Medicaid access on the propensity of households to save or repay debt from their tax refunds. We instrument for Medicaid access using variation in state eligibility rules. We find substanital heterogeneity across households in the savings response to Medicaid. Households that are not experiencing financial hardship behave in a manner consistent with a precautionary savings model, meaning they save less under Medicaid. In contrast, among households experiencing financial hardship, Medicaid eligibility increases refund savings rates by roughly 5 percentage points or $102. For both sets of households, effects are stronger in states with lower bankruptcy exemption limits – consistent with uninsured, financially constrained households using bankruptcy to manage health expenditure risk. Our results imply that expansions to the social safety net may affect the magnitude of the consumption response to tax rebates.

Keywords: health insurance, Affordable Care Act (ACA), precautionary savings, strategic default, bankruptcy

JEL: D11, D14, H51, I13

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1 Introduction

The 2014 Patient Protection and Affordable Care Act (ACA) extended subsidized health insurance coverage through Medicaid to an additional 16.3 million people. About 21% of the U.S. population now receives health insurance through Medicaid. Access to subsidized health insurance may not only affect a household's utilization of health care but also its finances and, thereby, its incentives to save and consume. The expansion of Medicaid coverage under the ACA and the current policy debate around "Medicare for all" has enhanced the importance of understanding if and how subsidized health insurance affects household financial decisions. To evaluate the effect of Medicaid on household savings, we employ a unique dataset on 57,000 low-income tax filers and their self-reported plans to save from their tax refunds. More broadly, we seek to better understand the extent to which the expansion of public safety net programs, such as Medicaid, may interact with current bankruptcy protections to influence personal savings behavior.

To the extent that households save to self-insure against (income and) expenditure risk, becoming eligible for Medicaid should reduce the precautionary motive to save (Carroll et al., 1992). Since Medicaid is a heavily subsidized form of health insurance, eligibility for Medicaid should also increase perceived household wealth and, consequently, current and future consumption. The existing empirical evidence on whether subsidized health insurance crowds-out private savings is mixed, however (Starr-McCluer, 1996; Gruber and Yelowitz, 1999; Maynard and Qiu, 2009; Gittleman et al., 2011; Guariglia and Rossi, 2004; Chou et al., 2003).

We bring three innovations to the literature. First, we evaluate the effect of Medicaid on a low-income household's self-reported intention to save or pay down debt (to not consume) from the tax refund. Tax refunds represent the largest annual cash infusion for most low-income households and are an important source of savings.[1] Second, we exploit the expansion of Medicaid – historically a program for children, the disabled, and pregnant women from low-income households – to the broader low-income adult population through the ACA to generate quasi-random variation in Medicaid eligibility. The savings response to Medicaid may vary based on whether the insurance is directed at the primary income earner or her dependents (Fafchamps, 2008). Finally,

[1] Roughly three-fourths of tax filers receive a tax refund (IRS, 2017). Farrell et al. (2018) report that the average total tax refund for a sample of JP Morgan Chase account holders was $3,100, representing 2.6 times the average payroll deposit. Additionally, for 40 percent of their account holders (which includes higher income clients), a tax refund payment represents "the largest single cash infusion into their accounts for the whole year."

we test for possible heterogeneity in the effect of Medicaid on savings according to the degree of financial hardship facing the household.

Our focus on financial hardship follows a number of studies that document significant heterogeneity in household financial decisions according to income, wealth, and liquidity constraint (e.g., Chetty, 2008). Studies show that financially constrained households have a greater marginal propensity to consume from both anticipated (e.g., Souleles, 1999) and unanticipated, (e.g., Jappelli and Pistaferri, 2014) transitory cash infusions. Moreover, Mahoney (2015) argues that uninsured households may consider bankruptcy to be a form of implicit high-deductible health insurance. This would imply that the household's proximity to bankruptcy may influence its savings behavior.

Key to our identification strategy is the substantial variation in Medicaid access of able-bodied, low-income adults across states and time. This variation comes primarily from the ACA's expansion of Medicaid to adults earning up to 138% of the federal poverty line (FPL) as well as the decision of 22 state governments *not* to expand Medicaid in 2014. Put together, these properties of the ACA's design and implementation create exploitable variation in Medicaid eligibility across income and state lines over the 2013–2017 period.

We conduct our analysis on a proprietary administrative tax and survey dataset for a large sample of low-income households (N≈57,000 households). These are households that used an online tax preparation software from the IRS free-file alliance at some point during the 2013–2017 period to prepare their tax returns. Sample households both consent to their anonymous tax data being used to conduct research and participate in a survey about their finances at the end of the tax filing process. About a fifth of the sample takes a follow-up survey 6-months after tax time, which we use to validate our findings. The tax return data includes each household's adjusted gross income (AGI), which is approximately equivalent to the income measure used to determine Medicaid eligibility. The linked survey provides each household's size, health insurance status, the share of the tax refund that each household expects to save (not consume), as well as information on the household's finances.

Note that any evaluation of Medicaid's effects on household savings must contend with the possibility that households may manipulate their income to obtain Medicaid eligibility. To overcome this challenge, we follow Currie and Gruber (1996) and construct a simulated probability of Medicaid eligibility using state Medicaid rules each year and the income distribution of demographic groups in a fixed national sample. Thus, our instrument does not depend on a household's income. We further show that our instrument is not correlated with financial characteristics, such as homeownership rates, of demographic groups within a state-year. This ensures that any effect we find is due to Medicaid and not other characteristics that are correlated with state Medicaid eligibility rules.

Since our instrument varies at the state-year-demographic group level, we are able to include within-state year fixed effects in our specifications. This reduces concern that our estimates are confounded by non-parallel state economic trends that coincide with Medicaid expansion. Still, an important assumption underlying our analysis is that a state's Medicaid eligibility rules do not update in response to the savings rates of the state's demographic groups. We verify this by showing that current values of our instrument are not correlated with the past savings rates of demographic groups within a state.

On average, households in our sample express an intention to save or pay down debt with 72% of their tax refund payments, which is roughly in line with estimates in Souleles (1999).[2] The refund savings intentions in our sample are correlated with actual savings. A one standard deviation, or 36 percentage point increase in the self-reported propensity to save from the refund at tax time is associated with a $161 increase in liquid assets six-months later.

Our first result is that Medicaid eligibility does not have a significant effect on the propensity of the average low-income household to save from its tax refund. Neither refund savings nor liquid assets respond, on average, to changes in Medicaid eligibility. This is true both in the reduced form and in the two-stage instrumental variables (IV) approach. Relevant to policymakers, this result suggests that any aggregate crowding out of private savings among low-income households from the Medicaid expansions is likely to be economically small. As we now discuss, however, this finding masks substantial heterogeneity across households.

We differentiate households based on extent of financial constraint (henceforth "hardship") with an index constructed using five indicators of financial difficulty. We find that low-income households in the top tercile of hardship express an intention to consume a greater share (6.7 per-

[2]Souleles (1999) calculate a marginal propensity to consume from the tax refund of at least 35% based on the self-reported consumption expenditures of 7,622 households from the 1980-1991 Consumer Expenditure Surveys. This would imply a savings rate of 65%.

centage points) of their tax refund payment than those in the bottom tercile of hardship. This result is consistent with the literature on how financial constraints affect consumption from transitory income shocks. Importantly, hardship appears to separate the savings response to Medicaid. Our IV estimates indicate that, among households in the top tercile of financial hardship, being eligible for Medicaid increases the refund savings share by roughly 5 percentage points or $102 dollars on average.

Our results are consistently significant and larger in magnitude when, instead, we use a household's level of assets as our measure of savings. In particular, for a household in hardship, we find that Medicaid eligibility increases liquid assets and net worth by $524 and $2,182, respectively. These effects are substantial given that the average household in our sample that is in hardship has $898 (-$3,186) in liquid assets (net worth), respectively. A large increase in liquid assets and net worth (relative to savings from the tax refund) is consistent with an upward bias in our estimates due to our inability to control for past medical expenses.[3]

Our results are broadly consistent with the predictions of a "strategic default" model of the type presented in Appendix A. In the model, uninsured households in hardship use bankruptcy as a high-deductible health plan, broadly consistent with evidence in Mahoney (2015). Such households will have little incentive to save to insure against health shocks. Since Medicaid obviates the need for such a household to declare bankruptcy to get out of a medical bill, access to Medicaid should increase its intention to save. Consistent with this mechanism, we find that our estimates vary based on the state laws that govern asset exemptions in bankruptcy. Among households in financial hardship, access to Medicaid produces a consistently more positive savings response if the household lives in a state with a high financial cost of bankruptcy (a low asset exemption limit).

We also find evidence consistent with a fall in precautionary savings among subsamples of unconstrained households when they become eligible for Medicaid. First, unconstrained households with a college degree tend to save substantially less of their tax refund under Medicaid. Thus, the extent of education appears to affect households incentives to save for health shocks. Second, unconstrained households that live in states with a higher cost of bankruptcy save sub-

[3]In the absence of Medicaid, medical expenses are likely to depress liquid assets and net worth. As we are unable to control for past medical expenses, these are likely to impart an upward bias to our estimated effect of Medicaid on liquid assets and net worth.

stantially less when they become eligible for Medicaid. A possible explanation is that the high cost of bankruptcy in these states prompts uninsured, unconstrained households to save more to guard against bankruptcy. When such households obtain Medicaid access, their savings rate drops.

Prior research finds that constrained households drive much of the consumption response to fiscal stimulus payments (e.g., Johnson et al., 2006). Our results indicate that constrained households may have a lower propensity to consume their tax refund if they enjoy access to Medicaid. Together, these results imply that the effect of fiscal stimulus on aggregate demand may, to some extent, depend on the extent of Medicaid coverage.

To evaluate this hypothesis, we regenerate our instrument for the 2008 period and use it to relate Medicaid access to the consumption response to tax rebates under the Economic Stimulus Act of 2008. Consistent with our main results above, we find that constrained households consume less (save more) of their tax rebate when they are eligible for Medicaid. To quantify the possible fiscal implications of these findings, we use back of the envelope calculations based on the coefficient estimates from our IV model. We estimate that a hypothetical fiscal stimulus program of 2% of GDP will generate 10% less demand growth if the country moves from no Medicaid access to complete Medicaid expansion for all low-income households. Note that our estimates are clearly partial equilibrium.

The paper proceeds as follows: Section 2 reviews the literature. Section 3 provides background information about the ACA. Section 4 describes the dataset, our strategy for constructing variables, and presents summary statistics. Section 5 explains our empirical method. Section 6 presents the results. Section 7 interprets our results through a policy lens. Section 8 concludes.

2 Literature

Our contribution to the literature is three fold. Most directly, we contribute to the academic debate over the effect of public health insurance on savings. We propose that the inconsistent results across earlier studies can, perhaps, be reconciled by the heterogeneity in the savings response to insurance according to extent of financial hardship. Second, we offer a new data point to the literature exploring the effect of the ACA on household finances – a literature that, until now, has focused on the liability side of the household balance sheet. Third, we add to the literature linking health costs to bankruptcy – namely, we show that interactions between bankruptcy protections and public insurance programs can have implications, not just for household savings behavior, but potentially for macroeconomic policy.

First, a substantial body of empirical work examines the link between health insurance and household savings. In an early non-experimental test, Starr-McCluer (1996) document a strong, positive association between insurance and savings, which runs contrary to the precautionary savings view. In an influential paper, Gruber and Yelowitz (1999) find evidence in support of a precautionary savings effect. These authors analyze the 1984–93 period, when a number of states expanded Medicaid to children and pregnant women, and find that Medicaid eligibility for children reduces household net worth and increases consumption. According to their analysis, asset tests for Medicaid eligibility more than double the negative impact of Medicaid on net worth.

In a re-examination, Gittleman et al. (2011) show that Gruber and Yelowitz (1999) estimates are sensitive to the choice of instrument and data source.[4] Maynard and Qiu (2009) employ an instrumental quantile regression model to show that the negative relationship between Medicaid and net worth is not present among households with low wealth or income. This result is puzzling considering that poorer households are more likely to benefit from Medicaid. Evidence from quasi-experiments in international contexts are also inconclusive (Guariglia and Rossi, 2004; Chou et al., 2003; Chou et al., 2004). In sum, the literature leaves unresolved the direction and magnitude of the effect of public health insurance initiatives, like Medicaid, on household savings. Our paper highlights the importance of conditioning on household financial hardship in evaluating the effect of Medicaid on household savings.

Another unique contribution of our paper is to study the effect of Medicaid on households' intention to save out of their tax refund. In contrast, the prior work primarily evaluates changes in net worth or liquid assets. An advantage of our measure is that it is less likely to be affected by our inability to observe and consequently control for the magnitude of health expenditure shocks. In evaluating tax refund savings, our paper belongs to the line of work that evaluates responses to

[4]Gittleman's criticism of the instrument primarily concerns the calculation of the expected dollar value of Medicaid to a household (which is estimated using the number of children as well as their ages and genders). The instrument in Gruber and Yelowitz (1999) is constructed as the product of the expected dollar value of Medicaid and the simulated probability of being Medicaid eligible. Our instrument, the simulated probability of Medicaid eligibility, does not include the dollar value of Medicaid and thus, is not subject to Gittleman's criticism.

tax refunds in order to test canonical consumption models (e.g., Souleles, 2002; Bracha and Cooper, 2014; Baugh et al., 2014). Most notably, Souleles (1999) uses the transitory nature of tax refunds to test the validity of the life-cycle consumption model. Inconsistent with the permanent income hypothesis, he reports that consumption increases at the time of tax refund receipt, particularly for constrained households.[5]

Second, our paper offers new insights to the burgeoning literature analyzing the effect of ACA Medicaid expansion on household financial well-being. Unlike our paper, this literature is heavily focused on household debt outcomes. Notably, Hu et al. (2016) and Brevoort et al. (2017) uncover downstream financial benefits of Medicaid, including fewer non-medical bills in collections and better loan offers. To our knowledge, the only other paper relating ACA Medicaid expansion to the asset side of the balance sheet is Lee (2017). He finds that low-income households living in expansion states have higher interest income – possibly from higher savings – and borrow less from friends/family after the Medicaid expansion.

Third, our study is related to the literature tying health costs to bankruptcy. Here again there is lack of consensus. Using a randomized control trial in Oregon, Finkelstein et al. (2012) finds that Medicaid has no effect on bankruptcies in the first year. In contrast, Gross and Notowidigdo (2011) and Hu et al. (2016) study changes in certain states' income thresholds for Medicaid eligibility and uncover lower bankruptcy rates in the most affected geographic areas. Similarly, using individual credit data, Brevoort et al. (2018) estimate that the ACA's Medicaid expansion reduced the number of personal bankruptcies by 25,000 per year.[6] More broadly, there is an active debate in health economics regarding the share of bankruptcies that are truly medically-induced, with estimates ranging widely – from 4% to 62% (Himmelstein et al., 2009; Dranove and Millenson, 2006; Himmelstein et al., 2011; Dobkin et al., 2018b).

Our paper differs from the above literature in that our focus is not on the empirical realization of the health-bankruptcy relationship, but on the household behavioral response to it. Mahoney

[5]In this sense, our paper is also related to the literature on the propensity to consume out of transient income changes (Zeldes, 1989; Jappelli and Pistaferri, 2014; Kaplan and Violante, 2014). For example, using a quasi-experiment Johnson et al. (2006) and Parker et al. (2013) show that households spent over two-thirds of their tax rebate payments during the 2001 and 2008 fiscal stimulus programs. The largest consumption response came from low-income, low-asset households.

[6]The varying estimates across studies may partly reflect the fact that bankruptcies are rare events, making them difficult to model. Only about 1% of low-income populations declare bankruptcy in a given year. There may also be a long lag between when a medical bill arrives and when a household files for bankruptcy.

(2015) offers empirical evidence in support of the idea that households factor bankruptcy laws into health spending decisions. In particular, he documents that households with greater wealth at risk during bankruptcy are more likely to have health insurance or, if they remain uninsured, make higher out-of-pocket medical payments. Similarly, Brevoort et al. (2018) explore the puzzle of why outstanding *non*-medical debt tends to fall when households gain health insurance, arguing that strategic default incentives around bankruptcy laws might drive excessive borrowing when a household is uninsured. Our study contributes to this latter body of work by documenting how the savings response to Medicaid changes according to a household's proximity to bankruptcy.

While bankruptcy is an extreme event, Medicaid may also have large welfare benefits by allowing households to smooth consumption. Surprisingly, with the exception of Gruber and Yelowitz (1999), there has been little work on the effect of Medicaid on household consumption. To the extent that more savings enables more consumption smoothing, our paper provides indirect evidence for the effect of Medicaid on households' ability to smooth consumption.

3 Background: Medicaid and the ACA

The ACA sharply reduced the share of uninsured Americans (Courtemanche et al., 2016). It did this, in large part, by raising the income threshold for adults to qualify for Medicaid and by providing low-income households that do not qualify for Medicaid with subsidies to purchase private insurance. Prior to passage of the ACA, Medicaid was primarily a program for children, pregnant women, older adults, and the disabled living in low-income households. Most states did not offer Medicaid to childless adults and provided Medicaid to only the poorest of parents. With the ACA's passage, Medicaid's focus widened to include able-bodied adults from low-income households. The ACA also eliminated asset tests, sometimes called "resource thresholds," for able-bodied adults (as well as several other classifications of income-eligible participants) to determine Medicaid eligibility.

By providing states with large federal subsidies per participant, the ACA encourages states to expand Medicaid to their adult populations with incomes under 138% FPL.[7] As of 2016, 31 states (and Washington, DC) had expanded Medicaid, and 19 states had not. Variation in the income

[7] In 2016, 100% of the FPL was $11,880 for an individual and $24,300 for a family of four; 400% of the FPL was $47,520 for an individual and $97,200 for a family of four (Annual Update of the HHS Poverty Guidelines, 2016).

eligibility limits for Medicaid across states and time is illustrated in Figure 1.[8] The figure shows that state eligibility limits changed very little between 2010 and 2013 but changed dramatically between 2013 and 2016. After the implementation of the ACA's Medicaid expansions, which began in 2014, about half the states have very high eligibility limits (darker) while the rest have very low eligibility limits (lighter). This figure highlights the opportunity presented by the 2013–2017 era, in terms of variation in Medicaid eligibility across households (both within and across states), to measure the effect of Medicaid access on savings.

It is important to note that Medicaid offers a minimum level of financial protection for low-income households. While Medicaid generosity varies by state, there are some common rules imposed at the federal level.[9] These rules mean that Medicaid remains a comparatively low-cost form of insurance in all states.

4 Data and Summary Statistics

Data used in this paper come from the tax records and survey responses of households that use an IRS free-file alliance online tax-preparation software to prepare their tax returns during the fiscal years 2013–2017.[10] To be eligible for IRS free-file alliance software, filers must have an AGI of less than $31,000 or qualify for the Earned Income Tax Credit (EITC). Immediately after the tax-filing process, a random sample of filers are invited to participate in a survey about their finances and are offered a small-value Amazon gift card for completion. The survey asks questions about filers' balance sheets, financial behavior, use of social services, experiences of hardship, and health insurance status. Participants who complete the survey consent to the use of their anonymized tax return and survey data for research. Our sample includes data on households that complete the survey and provide such consent. The dataset is not longitudinal in nature, we pool the cross-sectional data for the four years (2013-17) to conduct our analysis. We restrict the sample to U.S.

[8] For more granular detail, Tables IA-1 and IA-2 in the Internet Appendix (IA) document for each state and year the income eligibility limit for parents and childless adults. Table IA-3 lists the Medicaid asset limits as of 2013.

[9] Seven expansion states were granted "Section 1115 demonstration waivers," allowing them to charge higher premiums. For more information, see Kaiser Family Foundation article "Key Themes in Section 1115 Medicaid Expansion Waivers" available at: http://www.kff.org/Medicaid/issue-brief/key-themes-in-section-1115-Medicaid-expansion-waivers/

[10] The particular vendor of this tax-preparation platform asks to remain anonymous. Additional detail about the construction of this dataset is available in the appendix of Gallagher et al. (2019). The data are not publicly available. For more on the Free File Alliance, see https://www.irs.gov/uac/about -the-free-file-program.

citizen civilians, aged 19 to 64. This results in a sample of roughly 57,000 households.

The survey has a 7% response rate, on average over 2013–2017. Of serious concern is the potential for sample selection bias, which could arise if households that participate in the survey differ from those that do not participate in terms of unobservable factors, like health or risk-aversion. A detailed discussion of the dataset, as well as an evaluation of the potential for sample selection bias, is provided in the Appendix of Gallagher et al. (2019). These authors' tests reveal little evidence of sample selection bias along observable dimensions. In particular, there are no significant differences between the 2013 tax forms of households that self-select into the survey and those that do not. Also, there are no significant differences between the survey responses of households based on the size of the participation reward offered (e.g., $0, $5, $15, or $20). While these tests indicate that our results are unlikely to be *qualitatively* affected by sample selection issues, caution is warranted when interpreting the magnitude of our estimates.

Table 1 presents the summary statistics for the key variables we use in our analysis. About 41% of the households in our pooled sample of low-income households are eligible for Medicaid (*Med*) with a wide standard deviation of 49 percentage points. Our instrument for Medicaid eligibility, described in the next section, is the simulated probability of Medicaid eligibility, *ProbNTL(Med)*. Its average is smaller at 11% because it is based on a national sample that includes middle- and high-income households. When we divide our sample of tax filers into above and below median *ProbNTL(Med)*, we see that the rate of actual medicaid eligibility (*Med*) is 69% in the high *ProbNTL(Med)* group as compared to 12% in the low *ProbNTL(Med)* group. This not only signals that our instrument is strong but also highlights substantial variation in Medicaid eligibility across our sample.

On average, households expect to use 74% of their tax refund to save and/or pay down debt (to not consume) (as seen from the mean value of *Saving*). The intended savings rate varies substantially, however, with a standard deviation of 36 percentage points. The average refund in our sample is $1,686, which includes tax withholding as well as any federal EITC payment. A quarter of households receive a refund in excess of $1,880. Consistent with our sample comprising of low-income households, we find that a quarter of our sample has less than $157 in liquid assets. We define liquid assets as the sum of the amount held in checking accounts, savings accounts, on prepaid cards, money market accounts, and cash saved at home. We calculate net worth as total assets minus unsecured debt. Although average net worth is $35,020, almost half of our sample has debt exceeding assets (negative net worth). Median net worth is $464. The average household in our sample has income of 103% FPL. On average, 18% of the households in our sample express difficulty making rent on-time (*LateRent*), 48% have negative net worth (*LowNW*), 36% were food-insecure in the last 6-months (*HardFood*), 29% had at least one account with an overdraft in the last 6-months (*Overdraft*), and 14% had a credit card declined or a credit card application denied in the last 6-months (*CCDecline*).[11]

5 Identification Strategy

In Appendix A, we outline a standard two-period model to highlight both the effect of subsidized health insurance on household saving and also document how this effect will vary with household net worth, which we use as a proxy for hardship. The model generates two predictions. For a household with high net worth (not in financial hardship), Medicaid eligibility decreases savings. On the other hand, for a household in financial hardship – that finds the option to declare bankruptcy to get out of paying for medical care attractive – Medicaid eligibility increases savings. In this section, we describe our tests of these predictions.

5.1 Instrument for Medicaid eligibility

Medicaid eligibility depends on three sets of factors: state eligibility rules, household demographics (e.g., household size and parent status), and household income. Our first identification challenge is the potential for households to manipulate income to become eligible (Saez, 2010). Incentives to manipulate may be correlated with household savings decisions – say through unobserved risk aversion or health condition – and could potentially bias our estimates. To overcome this challenge, we follow Currie and Gruber (1996) and instrument for Medicaid eligibility using a simulated probability that varies only with state eligibility rules and household demographics.

We use a one-time national sample, namely the 2013 American Community Survey (ACS)

[11] In untabulated results, we recalculate the mean for key variables after adjusting by demographic sampling weights at the national, low-income level (using the ACS). Results indicate that our sample is significantly younger, more educated, more likely to be childless, and more likely to be white than the national low-income population. This is expected as we draw from online tax filers. We deal with these imbalances by controlling for socio-demographic covariates following Solon et al. (2015).

sample, and segment it into demographic blocks based on parent status, age, gender, race, and education level.[12] We select these variables because, parent status is part of the criteria for Medicaid eligibility in some states while the other variables are determined "pre-treatment" and are potentially correlated with a household's income and, hence, its Medicaid eligibility. Using the sample's income distribution within each block, we calculate the percentage of households that would be eligible for Medicaid based on each state's eligibility rules for each year of our sample. The instrument is given by:

$$ProbNTL(Med)_{j,s,t} = \frac{\sum I\left(y_{i,j} < \bar{y}_{s,t}\right)}{N_j}$$

where $y_{i,,j}$ is the income for household i belonging to demographic block j; $\bar{y}_{s,t}$ is the income-based Medicaid eligibility ceiling in a given state-year; and $N_{,j}$ are the number of households in the demographic block. Thus our instrument varies with household demographic characteristics, the national income distribution within a demographic block, and a state's Medicaid eligibility rules. We control for our sample's observable household demographic characteristics; hence, the residual variation in our instrument is due only to the national income distribution and state eligibility rules. Our tests suggest that the the instrument satisfies the monotonicity requirement. In Figure IA-1, we plot the fraction of households that are eligible for Medicaid within groups formed by each state-year-parent status combination (leading to 380 groups). We plot these factions against the corresponding average value of our instrument. Visually, there is a clear monotonic relationship between the two variables. The correlation between the two is 0.89.

To the extent state eligibility rules are either related to pre-existing differences across demographic blocks within states or affect within-state economic conditions for different demographic blocks, say through migration, our instrument will fail the exclusion restriction. We overcome this challenge using multiple approaches.

First, in all of our specifications we include state-year fixed effects, $\delta_s \times \delta_t$. This helps to control for all state-time specific macroeconomic conditions and ensures that our results are estimated

[12] We use the 2013 ACS to simulate the probability of eligibility for all years of our sample to ensure our results are not affected by changes in the income distribution over time (e.g., an increasingly poor population within certain demographic cells). In other words, we keep the income distribution around the poverty line constant over time.

using only within-state-time variation in our instrument.

Second, to ensure that the state Medicaid rules for different demographic blocks are not correlated with the average financial characteristics of those blocks in the state, we conduct falsification tests wherein we relate the average observable financial characteristics (those that may be correlated with savings and that are available within the ACS) of the different demographic blocks within states to our instrument. Results are presented in Table IA-4 in the Internet Appendix (IA). We find that our instrument is uncorrelated with the average income and homeownership rates of state-demographic blocks.

Third, we test whether a state's Medicaid rules change in response to the savings rates of its residents. To do so, in Table IA-5 of the IA, column (3), we impute over the 2013 sample the simulated probabilities of Medicaid eligibility based on 2014 Medicaid rules (after ACA expansion) and repeat our tests. This is effectively a test of a pre-trend as it compares the savings response of households treated by the ACA to the control households a year before ACA expansion. The results indicate an insignificant relationship between *future* Medicaid access and 2013 tax refund savings, irrespective of hardship. The absence of a pre-trend validates the assumption that the changes in household savings behavior that we observe, occur concurrently with major changes in eligibility under the ACA and not before.[13]

The last concern with our empirical method is that the changes to Medicaid rules resulting from the ACA coincide with other policy changes (e.g., to taxes, minimum wages, or food stamps) or spur migration trends that may affect the savings rate of the different demographic groups. To overcome this challenge, we repeat our tests employing an instrument constructed using Medicaid rules as of 2013, $ProbNTL2013(Med)$. In this test, we exploit only the pre-existing differences in the eligibility rules across states. As documented in Table IA-6, our key findings hold.

Implicitly, we assume that: (a) Medicaid significantly reduces a household's economic burden of health care, (b) households are aware of Medicaid expansion and their respective eligibility. To test the validity of the first assumption, in Table IA-7, we report OLS estimates that relate the inverse hyperbolic sine (IHS) transformation of out-of-pocket medical spending, *IHS($MedSpend)*, and medical debt, *IHS($MedSpend+$MedDebt)* to Medicaid status. We find that being eligible (*Med*)

[13] Along similar lines in untabulated tests, we collapse our sample to the block-state-time level and then relate average savings at time $t-1$ to the average value of our instrument at time t. We find that our instrument is not related to past savings rates.

or being enrolled (*MedEnroll*) in Medicaid is associated with significantly lower medical spending and medical debt.[14] Second, in Figure IA-2 of the IA, we document a sharp increase, particularly in 2014, in the share of low-income households in our sample that report having Medicaid. The timing corresponds with a marked decline in the share of uninsured households, consistent with substantial Medicaid program awareness.

Throughout our analysis, we focus on Medicaid eligibility rather than enrollment because, as Mahoney (2015) argues, enrollment is not particularly relevant when households are implicitly insured. Eligible households are covered retroactively for up to 3 months prior to the month of application, allowing Medical providers to enroll individuals retroactively and bill Medicaid for the care. We note, however, that if most households are aware of their Medicaid access, then eligible households would behave as enrolled ones. In Table IA-8, we verify the robustness of our main results to a 2SLS with enrollment as the endogenous outcome variable.

The size of the wealth shock (the subsidy) from obtaining Medicaid eligibility through the ACA will vary with the number of adult individuals in the household as well as their health status. There are two ways to handle this issue. One is to follow Gruber and Yelowitz (1999) and estimate the subsidy value that a household might expect and take that into account in constructing the instrument. The problem with this approach, as illustrated by Gittleman et al. (2011), is that the results may be sensitive to the way the dollar value of subsidy is calculated. To avoid such issues, we follow the second method, which is to assume homogeneity in treatment and explicitly control for the sources of heterogeneity. Specifically, we control for the number of adults in the household and for differences in average health costs through age-bin and gender fixed effects and their interactions.

5.2 Regression model

We match individual households, i, in our sample of 2013–2017 tax filers, described in the next section, to their corresponding $ProbNTL(Med)_{j,s,t}$ generated from the 2013 ACS (for simplicity, we replace subscript j, s, t with i going forward). To estimate the effect of Medicaid eligibility on a

[14] Our transformed estimates are also economically significant. For example, medical expenditure plus medical debt is $172 ($563) lower among households that are Medicaid eligible (enrolled). The estimation is based on the coefficients in the last two columns of Table IA-7 and the IHS marginal effect transformation shown in footnote 15, using mean *$MedSpend+$MedDebt* of $2,632.

household's propensity to save, we estimate the following two-stage IV model (2SLS):

$$Med_i = \beta_0 + \beta_1 ProbNTL(Med)_i + X'\varphi + \delta_{s,t} + \xi_i$$
$$Saving_i = \beta_0 + \beta_1 \hat{Med}_i + X'\gamma + \delta_{s,t} + \xi_i$$

where Med_i is an indicator variable that identifies the actual Medicaid eligibility of the household. We construct this from each state's eligibility rules in a given year, which are based on household size, parental status, and adjusted gross income (see Section 3). $ProbNTL(Med)_i$ is the simulated probability that household i is eligible for Medicaid. X is a vector of predetermined socio-demographic controls. These include dummies for parent status, the number of kids in the household, the number of adults in the household, education level, race, bins of age, gender, and interactions of age bins and gender. We include a state-year fixed effect, $\delta_{s,t}$. We present standard errors that are clustered at the state level, since both eligibility and savings rates are likely to be correlated within states (Bertrand et al., 2004). Apart from the 2SLS, we also implement a reduced form analysis, wherein we directly relate our outcome variables to $ProbNTL(Med)_i$.

5.3 Variables

Our key outcome variable, $Saving_i$, is the percentage of the tax refund that household i expects to save at tax time (of the sampling year). We construct our dependent variable, $Saving_i$, as 1-consumption, where consumption is the sum of the first two options in the following survey question: "What do you plan to do with your tax refund? What percentage do you plan to..."

1. Spend within 1 month of receiving the refund _____ %
2. Hold at least 1 month, but spend before 6 months _____ %
3. Pay down debt you owe now _____ %
4. Save at least 6 months _____ %

Note that the question is explicitly designed to avoid any potential bias towards selecting the savings or debt option. It achieves this by offering the savings and debt repayment options last, enabling flexibility in the consumption choice, and allowing the respondent to divvy up the refund into four buckets. An alternative outcome variable is an IHS transformation of the intended refund savings share multiplied by the dollar value of the tax refund the household is due to receive, $IHS(\$Saving_i)$. We use the IHS transformation to deal with extreme observations and zeros (Burbidge et al., 1988).[15]

The tax refund is often the largest lump-sum cash inflow that a household receives during the year and is, consequently, an important source of savings and debt repayment (Mendenhall et al., 2012).[16] However, a critical assumption in our analysis is that reported refund savings intentions are a good measure of actual savings. We verify this assumption by relating a household's liquid assets six-months after tax time, $IHS(\$LiqAssets_{i,t+1})$, to its intended refund savings at tax time, $Saving_{i,t}$. In this regression, we control for the household's liquid assets at tax time along with a vector of socio-demographic information (X).

$$IHS(\$LiqAssets_{i,t+1}) = \alpha + \underset{\substack{306.825 \\ (39.005)}}{\beta_1} Saving_{i,t} + \beta_2 IHS(\$LiqAssets_{i,t}) + X'\lambda + \delta_{s,t} + \epsilon_i$$

We find that our estimate for β_1 is statistically and economically significant and the R-squared of the regression is 0.45. A one standard deviation increase in $Saving_i$ (36 percentage points) is associated with \$161 higher level of liquid assets six-months later (i.e., evaluated per footnote 15 at the mean of liquid assets, $\bar{w} = \$3,519$, and with $\beta = 306.8$ and $\theta = 0.0003$).

An advantage of evaluating planned tax refund decisions (a reported-preference) rather than changes in assets or net worth (a revealed-preference) is that our estimates are less affected by our inability to control for the magnitude of health shocks. Households with Medicaid will necessarily face lower out-of-pocket medical expenditure risk compared to uninsured households, which may affect the amount of liquid assets and potentially bias the estimates. In other words, as explained by Parker and Souleles (2017): "Part of the attraction of the reported-preference approach is that unlike traditional revealed-preference estimation, inference based on reported preferences does not require plausibly exogenous real-world variation in situations."

[15] The inverse hyperbolic sine function is: $y = IHS(w) = log(\theta w + \sqrt{\theta^2 w^2 + 1})/\theta$ where θ is a positive location parameter. The results presented in this paper use $\theta = 0.0003$, which is consistent with Pence (2006). We also tried other values for $\theta = \{1; 0.01\}$ but we found that $\theta = 0.0003$ is the one that is associated with the highest likelihood function of the reduced form regression. Following Pence (2006), the marginal effect of independent variable (x) – at the mean value of the transformed dependent variable ($\bar{w}$) – is estimated as : $\frac{\partial w}{\partial x} = \frac{\partial w}{\partial y}\beta = 1/2\,(exp(IHS(\bar{w})\theta) + exp(-IHS(\bar{w})\theta))\,\beta$.

[16] We consider debt repayment to be a form of savings since it is net worth increasing. Moreover, the debt burden of low-income households has been shown to be an important factor in explaining their savings rates as well as the rise in wealth inequality (Saez and Zucman, 2016).

We identify households in financial hardship through an index constructed using a principal component analysis that combines a set of variables that proxy for financial stress: a dummy variable that identifies households with negative net worth ($LowNW$); an indicator that identifies households that report skipping meals in last 6 months due to affordability issues ($SkipFood$); an indicator of having been delinquent on a rent or mortgage payment during the last 6 months ($LateRent$); an indicator that identifies households with at least one bank account in over-draft during the last 6 months ($Overdraft$); and an indicator of having had a credit card declined in the last 6 months ($CCDecline$). As documented in Table IA-9, the first principal component explains 40% of the variation in these variables and has positive loadings on all the variables. We label the variable constructed from this first principal component, $Hardship$. We employ both a continuous measure of hardship and, a discrete measure using dummies to indicate terciles ($LowHardship$, $MidHardship$ and $HighHardship$).

Our measure of financial constraint (*Hardship*) is not standard in the literature. More common measures are, for example, based on income, net worth, or liquid assets. We employ our measure because we believe it better captures all sources of pledgeable net worth of the household. That is, we believe our hardship measure is a sufficient condition for low net worth – a measure of household's proximity to bankruptcy. On the other hand, other measures such as "low liquid assets" may not be a sufficient condition for financial constraint. A household with low levels of liquid assets may be rich in terms of the social support network or illiquid assets and, thus, may not experience financial hardship (Schoeni, 2002; Lusardi et al., 2011). Nonetheless, we show the robustness of our results to more standard measures, such as (*Liquid asssets/Income*) in Table IA-5 of the IA, column (4), as well as to a simple indicator of delinquent rent/mortgage payments (Table 3).

Apart from net worth, other unobserved factors may be different about households that we categorize as in high hardship. In Table IA-10, we compare the observable characteristics of households with high levels of hardship with the rest of the sample. Indeed, we find that households in financial hardship are significantly different from the rest of the sample along some dimensions: the size of the refund that they receive, age, college attainment, and parent status. In tests, we control for these factors and our results hold. Notwithstanding that, the existence of observable differences according to hardship status means that we cannot interpret our results as implying a causal link between hardship and the savings response to Medicaid. Put differently, we cannot say our empirical analysis is a test of our model in Appendix A.

6 Results

This section presents the results of tests that relate savings behavior to Medicaid eligibility.

6.1 Average effect: Medicaid eligibility on savings

We begin with nonparametric analysis of the relationship between Medicaid and savings at the state-level. Figure 2 plots the 2013 to 2017 change in state-level average savings (y-axis) against the corresponding change in the state-level average of our simulated instrument for Medicaid eligibility (x-axis). Each plot uses a different measures of savings, weighting the fit line by the sample size within the state. While the top panel provides the plots for *Saving* and *IHS($Saving)*, the bottom graphs present results for our two alternative measures of savings: *IHS($LiqAssets)*, and *IHS($NetWorth)*.

Across all four savings measures, the figure shows no significant relationship between Medicaid and savings. Many states underwent double digit changes in their average simulated probability of Medicaid.[17] However, none of these changes appear correlated with average household savings behavior. Of course, the analysis does not adjust for changes in state economies, asset tests for program eligibility, or sample composition over time.

In Table 2, we present estimates from a reduced form model with *Saving* as our outcome of interest. Our main independent variable is our simulated instrument, *ProbNTL(Med)*. All regressions include socio-demographic controls. While Columns (1) and (2) include state and year fixed effects, Columns (3) and (4) include within-state year effects. Given the important role of asset tests in affecting household savings and consumption (Hubbard et al., 1995; Powers, 1998; Gruber and Yelowitz, 1999), we control for the influence of asset tests through an interaction term between

[17]Most of these changes were positive in direction; however, certain small states, like Vermont, that had expanded Medicaid prior to the ACA reduced their eligibility rules to match those of the ACA. Several nonexpansion states, like Missouri, also slightly reduced their Medicaid income ceilings over this period. In Table IA-5 column (2), we repeat our tests after dropping parents living in the 21 states that reduced parent coverage as well as childless adults in Vermont. We find that our estimates are unaffected by this sample restriction. This reassures us that our results are not driven by a loss of Medicaid coverage in certain states.

a dummy variable that identifies households living in states that had asset tests for Medicaid eligibility in place at the time of sampling and our simulated instrument.[18]

The coefficient on *ProbNTL(Med)* suggests that, for the average household, there is no relationship between the probability of Medicaid eligibility and the propensity to save from the tax refund. This finding contrasts with a pure-precautionary savings hypothesis and suggests that Medicaid is not significantly crowding-out the savings of the average low-income household.[19] We verify the robustness of this conclusion using alternative measures of savings in Section 6.3.

We find that households in states that have an asset test in place, save less of their tax refund when they become eligible for Medicaid. For example, according to column (3), a one standard deviation (13 percentage point) increase in the simulated probability of Medicaid eligibility is associated with a 2.2 percentage point reduction in the refund savings share, given the presence of an asset test. This result reinforces evidence from other settings (e.g., Hubbard et al., 1995; Gruber and Yelowitz, 1999) that suggest that asset tests for social insurance deter savings. This result also helps validate our intention based savings measure.

6.2 Heterogeneity according to financial hardship

In this subsection, we differentiate households based on financial hardship to test if hardship affects the relationship between Medicaid eligibility and savings. Our model in Appendix A would predict that, on gaining Medicaid eligibility, households in financial hardship – those that are using bankruptcy as a form of health insurance – would save more.

We begin with non-parametric evidence in Figure 3. We divide our sample into quintiles based on hardship. Within each hardship quintile, we further divide households based on the level of our simulated instrument. The bars in Figure 3 represent the average *Savings* of households in the bottom or top quintile of simulated Medicaid eligibility within each hardship quintile. The line represents the difference in savings rate between the top and the bottom Medicaid eligibility quintile. In the table below, we present the average values of simulated Medicaid eligibility and

[18] As of 2013, 17 states still had an asset test in place for able-bodied adults. Such tests were eliminated under the ACA starting in 2014. Note that a separate control for $AssetTest_{s,t}$ is excluded in certain specifications due to collinearity with the state-year fixed effects.

[19] Note that our insignificant estimate for the average low-income household is consistent with quantile regression evidence in Maynard and Qiu (2009) estimated during the expansion of Medicaid to children and pregnant women in the early 1990s.

hardship across hardship quintiles. The plot shows that, at low levels of hardship, high eligibility households save an average of 3.0 percentage points less of their refund than do low eligibility households. The reverse is true at high levels of hardship. For example in the highest hardship quintile, high eligibility households save 4.2 percentage points more of their refund than low eligibility households. This figure offers preliminary evidence that households that are not in hardship behave in a manner consistent with a precautionary savings model, while those in hardship behave in a manner consistent with a strategic default model.

In Table 3, we repeat the reduced form estimates after including *Hardship* and an interaction term between *ProbNTL(Med)* and *Hardship.* Since coefficients are similar under both sets of fixed effects, we center our discussion on estimates based on within state-year variation in our instrument (columns 4–9). The coefficient on *Hardship* in column (4) signals that a household in hardship expects to save less of its tax refund, which is in line with prior research. The coefficient on the interaction term *ProbNTL(Med)*$\times$ *Hardship* is positive and significant. Thus, a household in hardship expects to save more of its tax refund when it gains Medicaid access. From the coefficient in column (4), keeping Medicaid eligibility at its mean value, a one standard deviation increase in *Hardship*, is associated with a 5.2 percentage point increase in the intended propensity to save from the tax refund. After transformation of the coefficient in column (4), the implied dollar effect from the mean is $101. Thus, the economic significance of this estimate seems modest.

In column (5), we test for possible nonlinearities in the interaction effect by employing dummy variables that indicate terciles of *Hardship*. Independent of Medicaid, we find that households in high (low) levels of hardship expect to save less (more) from their tax refund as compared to households with average levels of hardship. This indicates a monotonic relationship between hardship and savings. We also find that the positive relationship between Medicaid eligibility and the savings share is mostly due to households in extreme hardship. The coefficient on the interaction term *ProbNTL(Med)*$\times$ *HighHardship* of 10.694 implies that a one standard deviation higher likelihood of Medicaid eligibility for a household with a high level of hardship correlates with an additional savings of 1.4 percentage points of the fraction of the refund saved. In column (6), as a robustness check, we repeat our tests with just *LateRent* as a measure of hardship and obtain consistent results.[20]

[20]For additional robustness tests, see Figure 4, wherein net worth acts as a proxy for hardship, and see Table IA-5,

When *IHS($Savings)* is the dependent variable (columns 7–9), the standard errors become very large due to substantial variation across households in the size of the tax refund, but we still find that hardship is associated with more savings under Medicaid.

In Table 4, we present 2SLS IV estimates. Since the reduced form coefficients are only significant when we interact *ProbNTL(Med)* with hardship measures, we focus our analysis on the interaction effects in the 2SLS IV specification. Panel A presents first stage estimates. We run two first stage regressions with *Med* and *Med* $\times$ *HighHardship* as the outcome variables and *ProbNTL(Med)* and *ProbNTL(Med)* $\times$ *HighHardship* as the respective instruments. The Kleibergen-Paap Wald F statistics (weak instrument test) for both first stage equations are large, indicating a strong instrument. Panel B displays the results of our second stage regressions. The coefficients on $Med \times \widehat{HighHardship}$ are positive and significant. These estimates are economically modest. Among households in substantial financial stress, Medicaid eligibility increases the propensity to save from the tax refund by nearly 5 percentage points. According to the transformed coefficient on *IHS($Saving)*, this represents an additional $102 in implied savings from the mean.

To evaluate the potential bias from the endogenous nature of Medicaid eligibility, in Table IA-11, we present the corresponding OLS estimates. The OLS estimates indicate that households in the highest tercile of hardship save 2.9 percentage points more (vs. +5.0 percentage points under the IV approach) of their refund when they become eligible for Medicaid, while those not in hardship save 6.3 percentage points less (vs. +0.3 percentage points based on our IV estimates). Thus, our OLS estimates are, on average, biased downward as compared to our IV estimates. This is reasonable if households that manipulate their income downward to become eligible for Medicaid experience other (unobserved) forms of hardship prompting them to save less from tax refunds. The downward bias is consistent across all savings measures.

6.3 Robustness

In this section, we describe the results of multiple robustness tests.

First, we evaluate the effect of Medicaid and financial hardship on alternative measures of savings. In Table 5, we repeat our reduced form and 2SLS IV regressions with the IHS transformed version of liquid assets and net worth, separately. As mentioned earlier, Medicaid may

column (4), wherein hardship is measured using *Liquid asssets/Income*.

mechanically increase a household's liquid assets if it lowers healthcare costs and, in turn, limits the negative effect of a health shock on liquid assets or net worth. This mechanical effect should bias our estimates upward.

Panel A presents our reduced form estimates. The coefficient on $ProbNTL(Med)$ in columns (1) and (2) suggests that there is no robust relationship between Medicaid eligibility and assets for the average household. While the coefficient is insignificant in column (1) with *IHS($LiqAssets)* as the outcome variable, it is positive and significant in column (2) with *IHS($NetWorth)* as the outcome variable. A positive and significant coefficient may reflect upward bias in our estimates. Regardless, we observe no evidence of a crowd-out of private savings for the average low-income household in our sample. Among households in hardship, we find that gaining Medicaid eligibility is associated with an increase in both liquid assets and net worth. Our estimates are also economically large. For households in the upper tercile of financial hardship, a one standard deviation increase in $ProbNTL(Med)$ is associated with an increase in liquid assets of $141 (column 3) and an increase in net worth of $565 (column 4).[21] We find similar results in columns (5) and (6) using *LateRent* as our measure of hardship.

In Panel B we present the 2SLS IV estimates. Note that the first stage regressions are similar to those in Panel A of Table 4 and, hence, are suppressed. IV results are consistent with the reduced form estimates and are also economically significant. Again, we find that gaining Medicaid eligibility has no significant effect on liquid assets for the average low income household; however, the coefficient in column (2) is positive and significant and implies that gaining Medicaid eligibility is associated with $3,269 increase in net worth for the average low-income household. For a household in financial hardship, the coefficients in columns (3) and (4) imply that gaining Medicaid eligibility is associated with an additional $524 ($2,182) in liquid assets (net worth). The large magnitude of these effects is further evidence of an upward bias in estimates based on stock measures of savings.

Second, we address the concern that our results may, in part, depend on the size or the source of the tax refund. If propensities to save/consume vary according to the size of payments (e.g., Kueng, 2015) and if the size of payments were correlated with financial hardship, then the correct

[21] Throughout the paper, IHS estimates are converted to dollar values from the mean following the transformation shown in Footnote 15. Because we use $\theta = 0.0003$, rather than $\theta = 1$, these estimates cannot be directly interpreted as an approximate logarithmic dependent variable.

interpretation of our findings might be quite different from that presented above. Similarly, if only unconstrained households choose to overwhithold – either because they feel less incentive to adjust their W-4s or because they overwithhold as a self-control mechanism (Thaler, 1994; Neumark, 1995; Fennell, 2006) – then the source of the tax refund may be a relevant omitted variable.[22]

Our results are robust to controlling for both the source and size of the refund. Evidence, presented in Table 6, shows that our results do not change after controlling for whether or not the household received the EITC (column 1), the size of total refund (column 2), and the size of EITC relative to the total refund (column 3). Furthermore, we find that our results are similar across subsamples identified based on EITC status (columns 4 and 5), controlling for total refund size. The existence of financially constrained households within the subsample that does not receive the EITC is somewhat puzzling – households should try to minimize their tax refund (avoid overwithholding), especially if they are financially constrained. However, if one takes into account behavioral biases, such as inertia, a household may be constrained and overwhithold at the same time (Highfill et al., 1998; Thaler, 1994; Neumark, 1995; Fennell, 2006). Notably, Jones (2012), shows that, in the years following a change in the number of dependents, few households adjust their withholding patterns, which he attributes to an inertia problem. Adjustment rates are particularly slow for low-income filers. Jones (2012) notes that default withholding rules tend to lead individuals to overwhithold (receive refunds), which compounds the problem of inertia.[23]

A simple alternative explanation for our findings could be that uninsured households in financial hardship tend to defer medical care in anticipation of the tax refund. If such households gain Medicaid, then they can mechanically save a greater share of their tax refund since they no longer need that money for medical care. In support of this mechanism, Farrell et al. (2018) show descriptively that households with lower checking account balances tend to spend more of their tax refund on medical care. The authors do not differentiate households by insurance status, however.

We run several tests of this explanation for our findings, none of which yields supportive evidence. Most directly, in Table IA-13, we find that households in hardship do not report higher medical expenditures in the 6-months following tax time, as compared to households not in hard-

[22]We find that 41%, of our sample received the EITC. For this group, the EITC represents 72% of their refund, on average.

[23]For additional analysis relating Medicaid, hardship, and savings to the the size of the tax refund, see Table IA-12 in the Internet Appendix.

ship. This is true even after we control for their simulated probability of Medicaid and for insurance status. If households in hardship defer care in anticipation of a tax refund, one would expect them to have higher medical expenditure following receipt of the refund. Next, in Table IA-14 of the IA, we repeat our tests within the subsample of households that report skipping and not skipping medical care during the past six months. The coefficients are not significantly different across the subsamples. In other words, even among households that do not defer care, gaining Medicaid access is associated with an increase in the savings rate. In summary, we find no evidence to suggest that our results are driven, by uninsured households in financial hardship postponing health care in anticipation of a tax refund.

As a final robustness check, we run a placebo test to ensure that our results are unlikely to arise from random chance. In particular, we assign individuals to a random state and apply that state's Medicaid eligibility rules for that year to construct our instrument. We use the household's actual demographics in this construction. We do this for every household in our sample and re-estimate our savings model, recording the coefficient on $ProbNTL(Med) \times HighHardship$. Based on 10,000 iterations, we find that only 4.64% of the coefficients we obtain are larger than our estimate (the simulated p-value is .0464). Note that the power of this test is limited given the commonalities in state Medicaid eligibility rules for demographic groups. This results in a positive and significant correlation (conditional on demographics) between the instrument constructed using random allocation and the original instrument. Despite this, we are able to reject the null at less than 5% level. This result confirms that our results are driven by Medicaid access rather than by unobserved characteristics of households in hardship.

6.4 Financial constraint: the mechanism

In this subsection, first, we try to isolate the strategic default mechanism, then we explore alternative explanations for the increase in the savings rate of households in financial hardship upon gaining Medicaid access.

To isolate a strategic default mechanism, we rely on substantial variation in state bankruptcy rules. States differ in terms of the amount of assets that individuals are allowed to retain when they declare personal bankruptcy (Fay et al., 2002). These differences are likely to affect an uninsured household's intention to save. If the household contemplates bankruptcy as a high-deductible escape hatch from medical bills – where the deductible depends on how much is saved at the time of bankruptcy – the household may choose to save comparatively less when it lives in a state with a less generous exemption limit. Medicaid will obviate the need for bankruptcy and increase that household's savings rate relative to a household living in a state with more generous asset exemptions.[24]

To test this prediction, we borrow the cross-state simulated instrument of Mahoney (2015). This instrument, which we refer to as $CostB_s$, is a parameterization of the asset exemption laws of each state. Like our instrument for Medicaid eligibility, it is generated from financial data on a national sample of households.[25] The instrument is calculated as the mean financial cost of bankruptcy as though the national sample faced the asset exemption rules of each state. For example, if a household has home equity above the state's homestead exemption, the difference is considered part of the household's seizable assets. Each form of seizable asset is tallied for each household and then averaged across households. This process is repeated for each state. We assume that the higher the value of $CostB_s$ the greater is the cost of bankruptcy to the average household, since a greater fraction of savings is likely to be lost in bankruptcy.

We divide the states in our sample into those with above and below median values of $CostB_s$ and repeat our tests within each subsample and present the results in Table 7. We employ three alternate dependent variables, including our main savings measure, *Saving*, as well as, the IHS transformation of *Saving* in dollars, *IHS($Saving)*, and liquid assets, *IHS($LiqAssets)*. Presumably, state asset exemption rules act on savings behavior primarily through asset accumulation rather than through debt repayment. The p-values from F-tests that compare the key coefficients across the two samples are shown at the bottom of the table.

From columns (1) and (2) we find that the effect of Medicaid on the refund savings rate of a household in financial hardship is almost thrice that in states with a higher cost of bankruptcy as compared to in states with a lower cost (i.e., 14.6 vs. 5.5 percentage points). The coefficients are marginally statistically different from one another (p-value=0.061). Columns (3) and (4) doc-

[24]An underlying assumption here is that households are aware of the relative cost of bankruptcy in their state. For literature supporting this view see: Guiso et al. (2013); Kalda (2018); Kleiner et al. (2019).

[25]Mahoney (2015) constructs the instrument using the 1996–2005 SIPP, adjusted to 2005 dollars, and using households under age 65 and without public insurance (few able-bodied adults had Medicaid access during this period). While this instrument is constructed using survey responses from at least 8 years before our survey, Mahoney (2015) observes very little time-variation in state bankruptcy laws: "real exemption levels have been remarkably stable over time since 1920."

ument that the positive effect of Medicaid access on the dollar amount saved from tax refund by households in financial hardship is only present in states with a high cost of bankruptcy. We find similar, albeit statistically weaker, results when we focus on *IHS($LiqAssets)* in columns (5) and (6). Overall, the results indicate that the positive effect of Medicaid on savings among households in hardship is stronger in states where bankruptcy is costly. This is consistent with our conjecture that the increase in savings among households in hardship may reflect a reduced necessity to resort to bankruptcy in the event of a medical shock – i.e., the predictions of our model in Appendix A.

The table also offers some evidence that access to Medicaid is associated with reduced savings among households that are *not* in financial hardship when in states with a high cost of bankruptcy. In specifications that use refund savings intentions, the coefficient on *ProbNTL(Med)* is more negative in $HighCostB$ states relative to $LowCostB$ states. The difference is statistically significant (p-value = 0.007) in columns (1) and (2). This finding is reasonable if uninsured households from $HighCostB$ states that are not in hardship save to avoid bankruptcy in case of health shocks. Given Medicaid access, such households save less. In other words, to the extent that bankruptcy (due to a health shock) imposes greater financial losses in $HighCostB$ states, precautionary savings incentives should be stronger.

One concern with the above analysis is that states that differ in the amount of bankruptcy exemption may also vary along other economic dimensions. Such unobserved heterogeneity could potentially bias our estimates. In an attempt to overcome this, we conduct an analysis wherein we confine the sample to households that live in border counties of states that vary in their extent of bankruptcy exemption. In these tests, we include a border county pair-time fixed effect so that we compare the savings response to the tax refund only across households that live on either side of the border at a particular point in time. Our underlying assumption is that households in counties that are adjacent to each are subject to reasonably uniform economic conditions. Results of this test are presented in Table IA-15 in the Internet Appendix. Although our sample reduces to 10% of the original, our estimates are similar to those with the original sample. Due to the smaller size of the sample, we find our interaction effect coefficients are not significantly different from one another but that their signs and magnitudes are similar to those generated from the full sample. And, as in the full sample, we find that unconstrained households save significantly less under Medicaid in states with a higher cost of bankruptcy. This latter result is consistent with a precautionary savings motive.

6.5 Isolating a precautionary savings response

In the previous subsection, we established that unconstrained households engage in more precautionary savings behavior when they live in states with a high financial cost of bankruptcy. In this subsection, we dig deeper to characterize the (uninsured) household that saves for health shocks.

Quantile regressions are useful tool to understand the effect of Medicaid across the wealth distribution. At any chosen quantile, we ask how a marginal increase in the simulated probability of Medicaid affects net worth. Net worth is selected because it functions both as a savings outcome and as an indicator of proximity to bankruptcy. Coefficient estimates at the 10th through 90th quantiles of net worth are presented in Figure 4. At lower conditional quantiles of net worth, the graph clearly shows a positive marginal effect of a change in Medicaid access on net worth – consistent with a strategic default motive. At around the 45th percentile – near the point where assets surpass unsecured debt – the relationship between Medicaid access and net worth turns negative. The shape is convex with a local minimum at around the 70th percentile of net worth (corresponding to net worth of $15,311). At around the 85th percentile ($96,568), the relationship turns weakly positive again. One interpretation of this graph is that households in the 45th to 85th percentiles of net worth are actively saving for future uninsured health shocks. When granted Medicaid access, they limit this precautionary behavior.

Next, in Table 8, we repeat our estimates within subsamples of socio-demographic groups identified based on educational attainment, gender, race, marital status, parent status, and income. We control for the hardship interaction effect, $HighHardship \times ProbNTL(Med)$, and, focus our attention on the non-interaction effect, $ProbNTL(Med)$, which measures the effect on households not in hardship. Of all the demographic characteristics, we find that only educational attainment separates the Medicaid-savings relationship. We find that households with a college degree save 1.6 (1.6=12.6 x 0.13) percentage points less of their tax refund per standard deviation increase in their simulated Medicaid eligibility. This effect is highly statistically significant and similar in magnitude to the reduced form effect of $HighHardship \times ProbNTL(Med)$. One interpretation is that it is more educated (uninsured) households that tend to save for future health shocks. When such households become eligible for Medicaid, they reduce these savings.

In sum, there are three characteristics of low-income households that appear to be predictive of a stronger precautionary savings effect: (1) having more wealth; (2) living in a state with a higher financial cost of bankruptcy; and (3) having completed college.

7 Discussion

7.1 Estimates in the context of savings interventions

To put our estimates into context, we look to interventions explicitly designed to encourage saving among low-income households. On average, savings rates among low-income households are low – just 1 to 3.5 percent of the permanent income near retirement (Lusardi, 1998) – and there has been limited success in identifying interventions that increase it. A plausible explanation is offered by Shah et al. (2015). These authors contend that, when faced with scarcity, people focus on trade-offs, thus, aligning their preferences with economic predictions, and becoming less suceptible to "nudges."

Consistent with this theory, Bronchetti et al. (2011) find that low-income households tend to remove themselves from a savings default in a tax refund saving assignment experiment. The authors interpret this finding as evidence that financially constrained households have preexisting plans on how to use their refund, making them less susceptible to "nudges." Similarly, when Grinstein-Weiss et al. (2014) exposed low-income tax filers to anchors and promts to deposit refunds into savings accounts, take up increased from 6.8% to 7.6%. Only in the case of savings lotteries (Tufano, 2008; Cookson, 2018) or savings match programs (Duflo et al., 2006), in which people are rewarded with money (or the potential for a large payout) for saving, does one find a substantial boost in the savings take-up rates of low-income households. For example, when a 50% savings match on tax refunds was offered to low- and middle-income households through an experiment with H&R Block, Duflo et al. (2006) find that the individual retirement account savings take-up rate rose from 3% to 14%.

Viewed in this context, a 5 percentage point increase in the refund savings rate of constrained households due to Medicaid (a program that is not explicitly designed to alter savings) is relatively large.

7.2 Fiscal policy implications

We posit that the generosity of public health insurance programs could affect the ability of tax rebates to stimulate consumption. In particular, since constrained households have the greatest propensity to consume from fiscal stimulus payments (Johnson et al., 2006) and since constraint rises during recessions, we ask whether households may consume less from their stimulus payments as public insurance expands.

To test this hypothesis, we replicate Parker et al. (2013) – using the same Bureau of Labor Statistics (BLS) 2008 Consumer Expenditure Survey – differentiating households based on probability of Medicaid eligibility. Specifically, we regenerate our instrument for the 2008 period, the year of the last tax rebate program, using the national income distribution and the state Medicaid ceilings for childless adults and parents that were in place in 2008. Following Parker et al. (2013), we examine the effect of the economic stimulus payments (*ESP*) on changes in consumption using the following model:

$$C_{i,t+1} - C_{i,t} = \beta_1 ESP_{i,t+1} + \beta_2 \left(ESP_{i,t+1} \times ProbNTL(Med)_{i,t}\right) + \beta_3 ProbNTL(Med)_{i,t} + \delta_t + \gamma' X_{i,t} + u_{i,t+1}$$

where C is consumption and *ESP* is the dollar value of the tax rebate received by an individual.[26] Four types of consumption are modeled: a) food, b) strictly nondurables, c) nondurables and d) all goods and services.

We present the results in Table 9. From Panel A we find that across all categories of consumption, the results imply that households consume less of the tax refund when they are more likely to be eligible for Medicaid. For example, non-durable consumption, per dollar of *ESP*, is \$0.26 (\$0.08) among households with a low (high) simulated probability of Medicaid eligibility. In Panel B, we differentiate households based on the amount of liquid assets (a measure of financial constraint) and find that among constrained households, Medicaid access has a significant negative effect on MPCs. In other words, constrained households consume less and save more of their tax rebate when they are more likely to be eligible for Medicaid. This result is consistent with a strategic default mechanism. Among unconstrained households (those with high liquidity), Medicaid ac-

[26]Note that, similar to the tax refund, the average value of the *ESP*, conditional on receipt, is about \$1,000, and about two-thirds of households received the payments. Also note that the data are not restricted to a low-income sample.

cess appears to have a positive effect on MPCs, which is consistent with a precautionary savings motive. These findings are fairly consistent across the different forms of consumption. Note that the significant reduction in sample size in Panel B is because of missing observations for liquid assets in the BLS data.

In summary, the results based on the 2008 tax rebate program are highly consistent with our main results based on tax *refunds*. They suggest that economic stimulus programs, such as the 2008 tax rebate, could be less effective in stimulating consumption if public insurance programs are more prevalent.

Next, we illustrate the partial-equilibrium implications of our estimates based on tax refunds in Table 10. We study the marginal propensity to consume from a hypothetical stimulus program under different Medicaid scenarios. As stimulus programs tend to be progressive, we consider a hypothetical debt-financed stimulus program of 2% of GDP targeted at low-income households earning less than 200% of the federal poverty line (a third of the U.S. population). The table documents the implied impact on consumption as we move from a society with no Medicaid access to one with full Medicaid access for low-income households. Since the default rates on credit cards, mortgages, and consumer loans more than doubled during the 2008-2009 crisis, we multiply the rate of high hardship in our sample by 2.5.[27] We compute the MPC as one minus the predicted savings rate for different levels of Medicaid access using the coefficients from the 2SLS IV model in Table 4.

The table shows that the MPC drops by about 4.4 percentage points when one moves from no Medicaid to full Medicaid for all low-income adults. In this hypothetical scenario, aggregate consumption growth would fall from 1.24% to 1.11%.[28] Thus, Medicaid access would reduce the economic impact of the stimulus by roughly 10%.

It must be stressed that this table is merely suggestive of the direction of the effect of Medicaid on the demand generated by fiscal stimulus payments. Our analysis cannot speak to the general equilibrium as we ignore multipliers and price effects. We also abstract from how Medicaid may influence the rate of hardship. Moreover, it is also unclear if a model that captures changes in

[27]See Federal Reserve Bank of St. Louis data for delinquency rates available at: https://fred.stlouisfed.org/categories/32440

[28]Similar to our hypothetical program, the 2008 rebate program amounted to 2.2% of GDP. For comparison, Parker et al. (2013) estimate that, in partial equilibrium, the 2008 program stimulated extra demand of 1.3–2.3% of personal consumption expenditures in Q2 2008.

intended consumption from tax *refunds* can be applied to actual consumption behavior from tax *rebates*. Finally, as discussed in Section 4, due to the possibility of sample selection bias, caution is warranted when extrapolating from our coefficient estimates.

8 Conclusion

This paper tests whether the provision of Medicaid to low-income adults influences their propensity to save. In comparison to much of the extant research, we use an intentions-based savings measure and the ACA's Medicaid expansions to able-bodied adults as our source of exogenous variation in Medicaid eligibility. Our empirical tests are designed to manage the potential endogenous relationship between Medicaid and savings as well as the confounding influence of Medicaid's redistributional impact on savings stocks.

We find that Medicaid eligibility does not have a significant effect on the savings intentions of the average low-income household in our sample. We do, however, find evidence of a heterogeneous response based on financial hardship. Households that are are not experiencing financial hardship behave in a manner consistent with a precautionary savings model, meaning they save less under Medicaid. In contrast, among the households in financial hardship, being eligible for Medicaid increases the expected share of the tax refund saved by roughly 5 percentage points, or $102 on average. We also find that this effect is stronger in states with a lower bankruptcy exemption limit – where strategic disincentives to savings are greatest. Our results are consistent with financially constrained, uninsured households using bankruptcy as a last resort to overcome medical expenses.

The estimates documented in this paper, while small in absolute magnitude, are substantial when compared to the impact of interventions that are explicitly designed to nudge low-income households to save. Moreover, a reduced propensity to consume by financially constrained households under Medicaid may have implications for the effectiveness of fiscal policy through stimulus payments. Our estimates suggest that financially constrained households consume less (save more) under Medicaid. Thus, as the social safety net expands, we expect that fiscal stimulus programs will be less effective in boosting aggregate demand.

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A note from the tax preparation company: Statistical compilations disclosed in this document relate directly to the bona fide research of, and public policy discussions concerning, savings behavior as it relates to tax compliance. Compilations are anonymous and reflect taxpayer-level data with the prior explicit consent from taxpayers or do not disclose information containing data from fewer than 10 tax returns. Compilations follow the tax preparer's protocols to help ensure the privacy and confidentiality of customer tax data.

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Table 1: Summary statistics

This table documents summary statistics for key variables. In the last three columns, separate statistics are shown for households with high versus and low simulated probabilities of Medicaid eligibility, *ProbNTL(Med)*. To test for balance, in the last column, we follow Imbens and Wooldridge (2009) and calculate the normalized difference in variable means between the two groups (normalized by the standard deviation of the combined sample). A difference in means of more than 0.25 standard deviations is considered unbalanced (denoted with an asterisk).

							Mean by *ProbNTL(Med)*		
	Mean	Std. Dev.	p10	p25	p75	p90	<p50	>p50	diff/sd
Savings (%)	74.02	35.99	0.00	51.00	100.00	100.00	74.00	75.00	0.03
Refund ($)	1,686	2,047	147	415	1,880	4,909	1,691	1,682	0.00
Savings ($)	1,331	1,797	0.00	202	1,495	3,967	1,327	1,336	0.01
LiqAssets ($)	3,499	6,880	15	157	3,264	9,650	3,616	3,383	0.03
Net worth ($)	35,020	331,025	-52,300	-17,750	27,530	158,500	29,984	40,000	0.03
Income (% FPL)	103	66	20	49	153	196	110	97	0.20
LateRent	0.18	0.38	0.00	0.00	0.00	1.00	0.18	0.17	0.03
LowNW	0.48	0.50	0.00	0.00	1.00	1.00	0.48	0.48	0.00
SkipFood	0.36	0.48	0.00	0.00	1.00	1.00	0.39	0.34	0.10
Overdraft	0.29	0.45	0.00	0.00	1.00	1.00	0.30	0.28	0.04
CCDecline	0.14	0.35	0.00	0.00	0.00	1.00	0.15	0.13	0.06
Med	0.41	0.49	0.00	0.00	1.00	1.00	0.12	0.69	1.16*
ProbNTL(Med)	0.11	0.13	0.00	0.00	0.18	0.32	0.01	0.21	1.54*
Age	33.01	11.90	21.00	24.00	40.00	53.00	35.06	30.98	0.34*
College grad	0.48	0.50	0.00	0.00	1.00	1.00	0.58	0.39	0.38*
White	0.84	0.36	0.00	1.00	1.00	1.00	0.87	0.82	0.14
Parents	0.22	0.41	0.00	0.00	0.00	1.00	0.21	0.23	0.05
Male	0.51	0.50	0.00	0.00	1.00	1.00	0.55	0.47	0.16
N	57,562						28,617	28,945	

Table 2: The effect of Medicaid on tax refund savings, reduced form estimates

This table presents reduced form OLS estimates. The dependent variables are the fraction of the tax refund that a household elects to save, *Saving* (measured in percentage points), and an IHS transformation of the fraction saved, measured in dollars, *IHS($Saving)*. Key explanatory variables included are household's simulated Medicaid eligibility, as detailed in Section 5.1, $ProbNTL(Med)$, and an indicator for whether the state has an asset test in place at the time of sampling, $AssetTest_{s,t}$, which is not separately controlled for in columns (3) and (4) because it is collinear with the state-year fixed effects. All regressions include socio-demographic controls as well as state, year, or state-year fixed effects (not shown). Standard errors, shown in parentheses, are clustered on state. *p = 0.1; **p = 0.05; ***p = 0.01 (statistically significant)

Dependent:	*Saving*	*IHS($Saving)*	*Saving*	*IHS($Saving)*
	(1)	(2)	(3)	(4)
$ProbNTL(Med)$	0.833	71.744	1.635	73.397
	(2.120)	(121.540)	(2.870)	(149.831)
$ProbNTL(Med) \times AssetTest_{s,t}$	-6.580*	-1673.920***	-17.136***	-2323.288***
	(3.761)	(543.849)	(4.303)	(491.811)
$AssetTest_{s,t}$	-0.960	-145.348**		
	(0.841)	(68.112)		
N	57,648	57,648	57,648	57,648
Adj. R-squared	0.065	0.514	0.065	0.515
F.E.	State, Year		State x Year	

Table 3: The effect of Medicaid and financial hardship on tax refund savings, reduced form estimates

This table presents reduced form OLS estimates. The dependent variables are the fraction of the tax refund that a household elects to save, *Saving* (measured in percentage points), and an IHS transformation of the fraction saved, measured in dollars, *IHS($Saving)*. Key explanatory variables include household's simulated Medicaid eligibility, $ProbNTL(Med)$, an indicator for whether the state has an asset test in place at the time of sampling, $AssetTest_{s,t}$, an indicator of financial strain, $Hardship$, a dummy variable for households that face difficulties making rent/mortgage payments on time, $LateRent$, and tercile dummies of $Hardship$: $LowHardship$, $MidHardship$ and $HighHardship$. All regressions include controls for $ProbNTL(Med) \times AssetTest_{s,t}$, socio-demographics, as well as state, year, or state-year fixed effects (not shown). Standard errors, shown in parentheses, are clustered on state. *p = 0.1; **p = 0.05; ***p = 0.01 (statistically significant)

Dependent:	*Saving*						*IHS($Saving)*		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
$ProbNTL(Med)$	0.581	-3.125	-1.351	1.070	-2.502	-0.654	66.577	67.377	53.670
	(2.212)	(3.113)	(2.459)	(2.964)	(3.818)	(3.253)	(152.621)	(178.052)	(151.276)
$ProbNTL(Med) \times Hardship$	3.665***			3.625***			66.537**		
	(0.837)			(0.850)			(25.540)		
$ProbNTL(Med) \times LowHardship$		0.450			0.317			-87.740	
		(2.726)			(2.730)			(90.077)	
$ProbNTL(Med) \times HighHardship$		10.890***			10.694***			128.590	
		(3.210)			(3.250)			(101.816)	
$ProbNTL(Med) \times LateRent$			10.604***			10.435***			87.896
			(3.449)			(3.471)			(85.864)
$Hardship$	-2.162***			-2.166***			-26.355***		
	(0.140)			(0.140)			(3.569)		
$LowHardship$		3.602***			3.593***			36.837***	
		(0.421)			(0.422)			(10.910)	
$HighHardship$		-3.044***			-3.062***			-43.267***	
		(0.379)			(0.381)			(12.414)	
$LateRent$			-5.712***			-5.741***			-84.056***
			(0.393)			(0.393)			(10.401)
$ProbNTL(Med) \times AssetTest_{s,t}$	-8.837**	-8.759**	-7.755**	-18.981***	-19.183***	-18.213***	-2378.871***	-2385.337***	-2329.511***
	(3.776)	(3.807)	(3.786)	(4.361)	(4.365)	(4.279)	(494.330)	(494.390)	(489.595)
$AssetTest_{s,t}$	-1.041	-1.023	-1.026						
	(0.840)	(0.837)	(0.832)						
N	57,648	57,648	57,648	57,648	57,648	57,648	57,648	57,648	57,648
Adj. R-squared	0.072	0.071	0.068	0.072	0.071	0.069	0.516	0.516	0.516
F.E.		State, Year			State x Year			State x Year	

Table 4: The effect of Medicaid and financial hardship on tax refund savings, 2SLS IV estimates

This table presents 2SLS IV regression estimates. The endogenous outcome variable in the first stage (Panel A) is Medicaid eligibility, *Med*, as well as its interaction with $HighHardship$. The second stage outcome variables are the fraction of the tax refund that a household elects to save, *Saving* (measured in percentage points), and an IHS transformation of the dollar amount elected to save, *IHS($Saving)*. All regressions include a control for $ProbNTL(Med)$ *or* $Med_{i,} \times AssetTest_{s,t}$, socio-demographic controls, as well as state-year fixed effects (not shown). The Kleibergen-Paap F-stat (weak instrument test) from a 2SLS IV regression with *Saving* as the final outcome variable is shown below each first stage regression estimate. Standard errors, shown in parentheses, are clustered on state. *p = 0.1; **p = 0.05; ***p = 0.01 (statistically significant)

Panel A. First stage estimates			Panel B. 2SLS IV estimates		
Dependent:	*Med*	$Med \times$ $HighHardship$	Dependent:	*Saving*	*IHS($Saving)*
$ProbNTL(Med)$	0.863***	-0.467***	$\hat{Med}$	0.313	60.585
	(0.056)	(0.031)		(3.537)	(179.114)
$ProbNTL(Med) \times HighHardship$	-0.062*	2.051***	$Med \times Hi\hat{g}hHardship$	4.975***	91.453**
	(0.035)	(0.131)		(1.437)	(40.533)
$HighHardship$	0.042***	0.425***	$HighHardship$	-7.464***	-107.841***
	(0.006)	(0.025)		(0.679)	(23.988)
F-stat	125.70	127.51			
N	57,648	57,648	N	57,648	57,648

Table 5: The effect of Medicaid and financial hardship on alternative savings measures, reduced form and 2SLS IV estimates

This table presents reduced form and 2SLS IV regression estimates. The dependent variables are a household's liquid assets, *IHS($LiqAssets)*, and household's net worth, *IHS($NetWorth)* – both variables are transformed using IHS. The instrument is the simulated probability of being eligible for Medicaid, $ProbNTL(Med)$. In the first stage regressions (not shown for brevity), the endogenous outcome variables include an indicator of whether a household is Medicaid eligible (*Med*) and, in some specifications, an interaction between *Med* and $HighHardship$ or $LateRent$. All regressions include controls for $ProbNTL(Med)$ *or* $Med_{i,s,t} \times AssetTest_{i,s,t}$, socio-demographics, as well as state-year fixed effects (not shown). Standard errors, shown in parentheses, are clustered on state. *p = 0.1; **p = 0.05; ***p = 0.01 (statistically significant)

Dependent:	*IHS($LiqAssets)*	*IHS($NetWorth)*	*IHS($LiqAssets)*	*IHS($NetWorth)*	*IHS($LiqAssets)*	*IHS($NetWorth)*
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. Reduced form estimates						
$ProbNTL(Med)$	139.060	1983.647***	-322.456	547.577	-69.092	1236.224*
	(175.050)	(738.512)	(194.864)	(691.979)	(191.988)	(723.928)
$ProbNTL(Med) \times HighHardship$			1042.930***	3142.846***		
			(168.479)	(572.918)		
$HighHardship$			-1622.220***	-5699.838***		
			(41.555)	(113.048)		
$ProbNTL(Med) \times LateRent$					899.281***	3232.982***
					(185.243)	(632.462)
$LateRent$					-1358.544***	-4813.123***
					(37.120)	(120.489)
Adj. R-squared	0.108	0.070	0.186	0.134	0.146	0.102
Panel B. 2SLS IV estimates						
$\hat{Med}$	165.635	2,362.726**	-100.171	1,488.779*	77.708	2,035.398**
	(206.351)	(958.821)	(214.526)	(887.001)	(216.569)	(924.501)
$Med \times Hi\hat{g}hHardship$			505.396***	1,577.076***		
			(100.992)	(338.721)		
$HighHardship$			-1,832.826***	-6,432.197***		
			(86.311)	(270.229)		
$Med \times \hat{L}ateRent$					465.947***	1,753.179***
					(110.204)	(353.022)
$LateRent$					-1,556.186***	-5,622.941***
					(84.442)	(275.680)
N	57,648	57,648	57,648	57,648	57,648	57,648

Table 6: Controlling for the size and source of tax refunds

This table presents reduced form estimates. The the dependent variable is the fraction of the tax refund that a household elects to save, *Saving* (measured in percentage points). Key explanatory variables included are household's simulated Medicaid eligibility, $ProbNTL(Med)$, tercile dummies of *Hardship*: $LowHardship$, $MidHardship$ and $HighHardship$. New controls include a dummy variable that identifies households that do not receive the EITC, the IHS transformation of the *Refund* measured in dollars, and the EITC share of the total refund, $EITC/Refund$. In columns (4) and (5), the sample is split according to the EITC share of the refund. This slit is performed as follows: 41% of households are classified as *No EITC/Refund* because they do not receive the EITC. The remaining households are split into below and above median subsamples of *EITC/Refund.* As we are interested in comparing households for which the EITC is unimportant with households for which the EITC is a substantial share of their refund, only results for the *High EITC/Refund* subsample, which includes 29.5% of households, is shown. All regressions include controls for $ProbNTL(Med) \times AssetTest_{s,t}$, socio-demographics, as well as state-year fixed effects (not shown). Standard errors, shown in parentheses, are clustered on state. *p = 0.1; **p = 0.05; ***p = 0.01 (statistically significant)

Dependent variable: *Saving*

Sample split:	*All*	*All*	*All*	*No EITC*	*High EITC/Refund*
	(1)	*(2)*	*(3)*	*(4)*	*(5)*
$ProbNTL(Med)$	-4.40	-4.07	-4.08	-2.80	-4.32
	(3.56)	(3.20)	(3.21)	(4.59)	(6.30)
$ProbNTL(Med) \times HighHardship$	10.03***	10.00***	10.00***	8.79*	11.76*
	(3.36)	(3.26)	(3.26)	(4.94)	(6.48)
$ProbNTL(Med) \times LowHardship$	1.18	1.23	1.22	3.77	1.81
	(2.67)	(2.66)	(2.66)	(3.61)	(5.64)
$HighHardship$	-2.95***	-3.05***	-3.05***	-2.97***	-3.12***
	(0.38)	(0.38)	(0.38)	(0.55)	(0.79)
$LowHardship$	3.43***	3.29***	3.28***	3.21***	3.17***
	(0.42)	(0.42)	(0.42)	(0.53)	(0.86)
$NoEITC$	3.59***	5.14***	5.11***		
	(0.31)	(0.34)	(0.34)		
$IHS(\$Refund)$		0.01***	0.01***	0.01***	0.00***
		(0.00)	(0.00)	(0.00)	(0.00)
$EITC/Refund$			-0.04		
			(0.05)		
N	57561	57561	57555	34001	11782
Adj. R-squared	0.07	0.09	0.09	0.09	0.11

Table 7: The impact of state bankruptcy and Medicaid rules on savings behavior, reduced form estimates

This table presents reduced form OLS estimates. The dependent variables are the fraction of the tax refund that a household elects to save, *Saving* (measured in percentage points), refund savings measured in dollars, *IHS($Saving)*, and household's liquid assets, *IHS($LiqAssets)* – dollar values are transformed using the IHS. Key explanatory variables include a household's simulated Medicaid eligibility, $ProbNTL(Med)$ and an indicator of financial strain, $HighHardship$. The sample is split according to the Mahoney (2015) parameterization of state bankruptcy rules, $CostB$ – which is calculated as the mean financial cost of bankruptcy as though the national sample faced the asset exemption rules of each state. The data is divided into states with a high or a low cost of bankruptcy ($HighCostB$ and $LowCostB$). All regressions include controls for $ProbNTL(Med) \times AssetTest_{s,t}$, socio-demographics, as well as state-year fixed effects (not shown). The bottom row of the table reports p-values from an F-test for the equality of reported coefficients. Standard errors, shown in parentheses, are clustered on state. *p = 0.1; **p = 0.05; ***p = 0.01 (statistically significant)

Dependent variable:	*Saving*		*IHS($Saving)*		*IHS($LiqAssets)*	
	(1)	(2)	(3)	(4)	(5)	(6)
$ProbNTL(Med)$	4.86	-14.06***	381.08*	-311.04	-666.68**	-566.49*
	(4.96)	(4.75)	(217.23)	(228.70)	(269.83)	(299.18)
$ProbNTL(Med) \times HighHardship$	5.47	14.63***	-47.98	307.72***	1266.18***	1698.03***
	(3.30)	(3.52)	(160.28)	(83.23)	(231.01)	(301.82)
$HighHardship$	-5.38***	-5.71***	-83.42***	-67.09***	-1472.28***	-1796.52***
	(0.42)	(0.61)	(15.55)	(13.27)	(47.26)	(53.87)
N	29118	28440	29118	28440	29118	28440
Adj. R-squared	0.07	0.07	0.52	0.51	0.19	0.18
Sample	$LowCostB$	$HighCostB$	$LowCostB$	$HighCostB$	$LowCostB$	$HighCostB$
Difference p-value:						
$ProbNTL(Med)$	0.007		0.023		0.873	
$ProbNTL(Med) \times HighHardship$	0.061		0.045		0.250	

Table 8: The effect of Medicaid on tax refund savings, reduced form estimates, by demographic groups

This table presents reduced form estimates. The dependent variable is the fraction of the tax refund that a household elects to save, *Saving* (measured in percentage points). Key explanatory variables included are household's simulated Medicaid eligibility, $ProbNTL(Med)$. All regressions include controls for $HighHardship$, and $HighHardship \times ProbNTL(Med)$, $ProbNTL(Med) \times AssetTest_{s,t}$, socio-demographics, as well as state-year fixed effects (not shown). The sample is split according to socio-demographic characteristics: educational attainment, race, marital status, parent status, gender, and income group. Standard errors, shown in parentheses, are clustered on state. *p = 0.1; **p = 0.05; ***p = 0.01 (statistically significant)

Dependent variable: *Saving*

	Full Sample			
Sample split	Coef. *ProbNTL(Med)*	Std Err.	N	Adj. R2
Panel A: Education level				
No college degree	-3.23	3.21	29692	0.06
College degree	-12.57***	4.21	27867	0.06
Panel B: Race				
White	-3.08	3.08	44403	0.07
Black	-5.70	7.81	3782	0.10
Asian	-9.87	7.56	2132	0.04
Other	0.85	6.19	7151	0.06
Panel C: Marital status				
Married	-2.75	2.51	50444	0.07
Single	7.06	6.02	7114	0.09
Panel D: Parent status				
Childless	-2.61	2.82	45025	0.06
Parents	0.35	3.69	12533	0.10
Panel E: Gender				
Female	-1.90	3.16	28147	0.07
Male	-3.33	2.94	29413	0.06
Panel F: Income-level:				
Low-low-income	-4.17	3.72	19188	0.06
Mid-low-income	-1.70	3.82	19185	0.09
High-low-income	1.13	3.37	19187	0.06

Table 9: Tax rebate regressions from Parker et al. (2013)

This table presents reduced form estimates from a replication of Parker et al. (2013), after allowing for an interaction term between tax refund and Medicaid eligibility (see the model in Section 7.2). Data are from the Bureau of Labor Statistics' 2008 Consumer Expenditure (CE) Survey. The dependent variable captures the three-month dollar change in consumer spending on: "Food" (which includes food consumed away from home, food consumed at home, and purchases of alcoholic beverages), "Strictly Non-Durables", "Non Durables" (which includes semi-durable categories like apparel, health and reading materials), or on "All Goods & Services" (which includes durable goods, such as home furnishings, entertainment equipment, and auto purchases). The key explanatory variable in Panel A is the economic stimulus payment received in dollars, ESP, where the sample is split according to the individual's Medicaid eligibility probability (which we regenerate based on states' 2008 Medicaid rules). In Panel B, ESP is interacted with our instrument for Medicaid eligibility and the sample is split by low versus high terciles of liquid assets (a measure of constraint in Parker et al., 2013). The reduction in sample size seen in Panel B is due to missing values of liquid assets. Regressions include a full set of dummies for every month in the CE sample, δ_t, and controls for age and changes in family size (number of adults and children, separately), $X_{i,t}$. Robust standard errors, shown in parentheses, are clustered on household. *p = 0.1; **p = 0.05; ***p = 0.01 (statistically significant)

Panel A. Sample split by low versus high simulated Medicaid eligibility

Dependent variable:	*Food*		*Strictly Non-Durables*		*Non-Durables*		*All Goods & Services*	
$ProbNTL(Med)$ *Split:*	*Low*	*High*	*Low*	*High*	*Low*	*High*	*Low*	*High*
ESP	0.041	-0.002	0.203**	0.039	0.258***	0.080	0.708**	0.513*
	(0.051)	(0.038)	(0.081)	(0.067)	(0.095)	(0.081)	(0.321)	(0.274)
N	7121	7118	7121	7118	7121	7118	7121	7118
Adj. R-squared	0.001	0.001	0.011	0.008	0.007	0.005	0.001	0.001

Panel B. Sample split by low versus high liquid assets

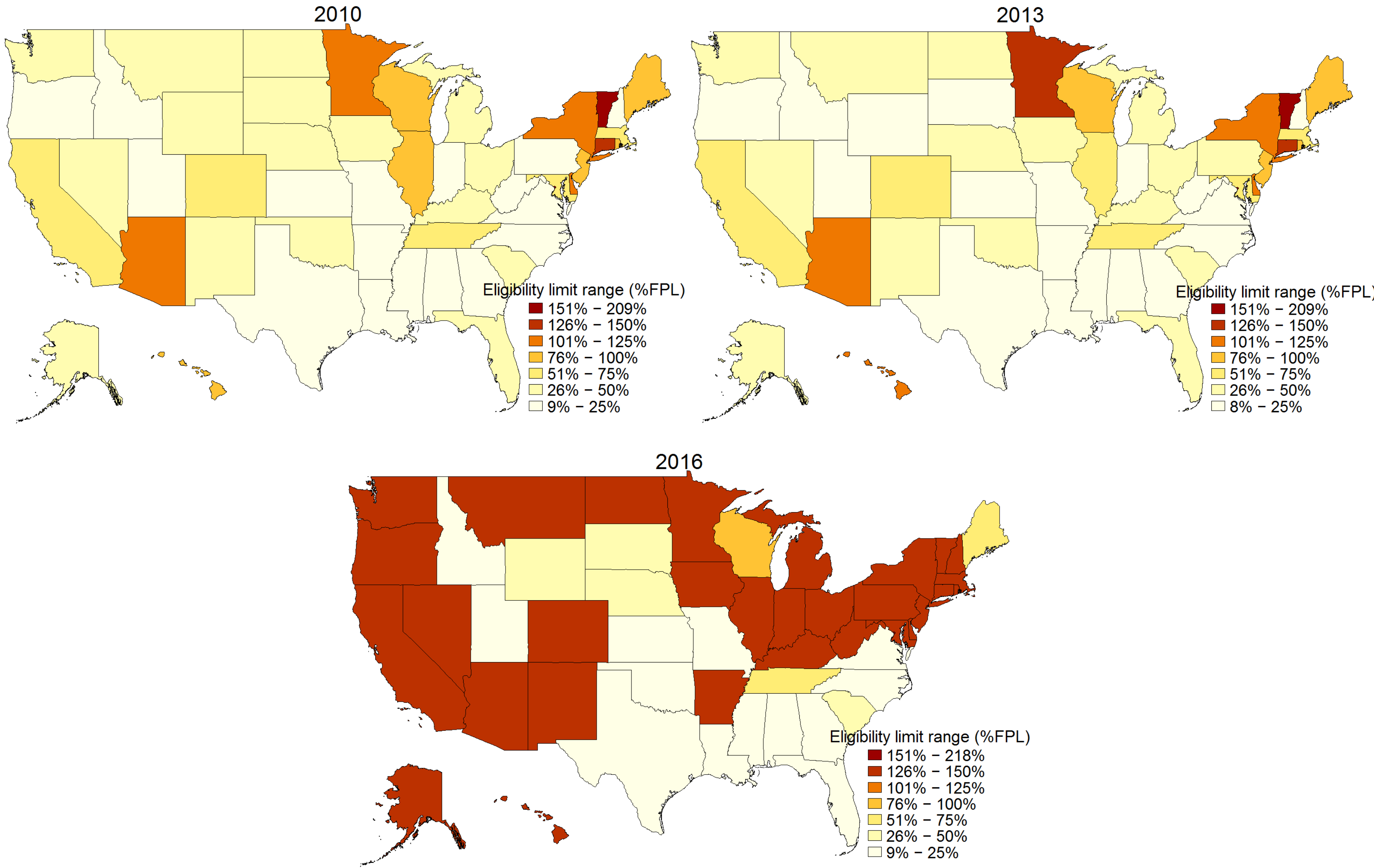
Dependent variable:	*Food*		*Strictly Non-Durables*		*Non-Durables*		*All Goods & Services*	
Liquid asset split:	*Low*	*High*	*Low*	*High*	*Low*	*High*	*Low*	*High*
ESP	0.091	-0.193	0.800**	-0.445	0.658	-0.756	0.345	-0.576
	(0.219)	(0.215)	(0.361)	(0.466)	(0.460)	(0.589)	(0.971)	(1.838)
$ProbNTL(Med) \times ESP$	-3.806	4.721**	-7.954*	4.842***	-12.950***	5.255***	-21.479*	8.709*
	(2.450)	(2.340)	(4.244)	(1.449)	(4.613)	(1.477)	(12.110)	(4.877)
N	500	498	500	498	500	498	500	498
Adj. R-squared	0.369	0.461	0.067	0.157	0.067	0.157	0.091	0.001

Table 10: Consumption stimulus effect with and without Medicaid

This table documents how a hypothetical stimulus program of 2% of GDP directed at low-income households might effect aggregate consumption with and without Medicaid. To calculate the MPC under different Medicaid policies, we set the base MPC (assuming no Medicaid or hardship) to 100% minus the constant (i.e., 100-63.5) in the 2SLS IV results in Table 4. Then, we adjust that number for the three other combinations of Medicaid and hardship, using the coefficients on Medicaid eligibility, hardship, and their interaction. Finally, we weight the adjusted values by the hardship rate and take the average with and without Medicaid. Aggregate consumption growth is measured as the MPC times the total stimulus payout divided by total personal consumption expenditures in the economy (assumed to by 69% of GDP at the time of the stimulus). We assume a hardship rate of 2.5 times the hardship rate in our sample (which was captured during a period of economic growth).

Medicaid policy for low-income adults	MPC	Aggregate consumption growth	% Change in consumption impact of stimulus
No Medicaid	42.66%	1.24%	
Full Medicaid	38.24%	1.11%	
Difference	-4.42	-0.13	-10.36%

Figure 1: State Medicaid eligibility limits for adults, by year



This figure shows the income limit for an able-bodied, adult to receive Medicaid in each state by year. Eligibility limits are presented as ranges of income, measured as a percentage of the FPL. Darker shades represent higher limits. Since eligibility limits often differ for parents and childless adults within a state, we take the average of the two. For example, if a state offers Medicaid to parents earning up to 100% FPL but does not offer Medicaid to childless adults (i.e., 0% FPL), that state is assigned an eligibility limit of 50% FPL.

Figure 2: Household savings changes by states and Medicaid eligibility

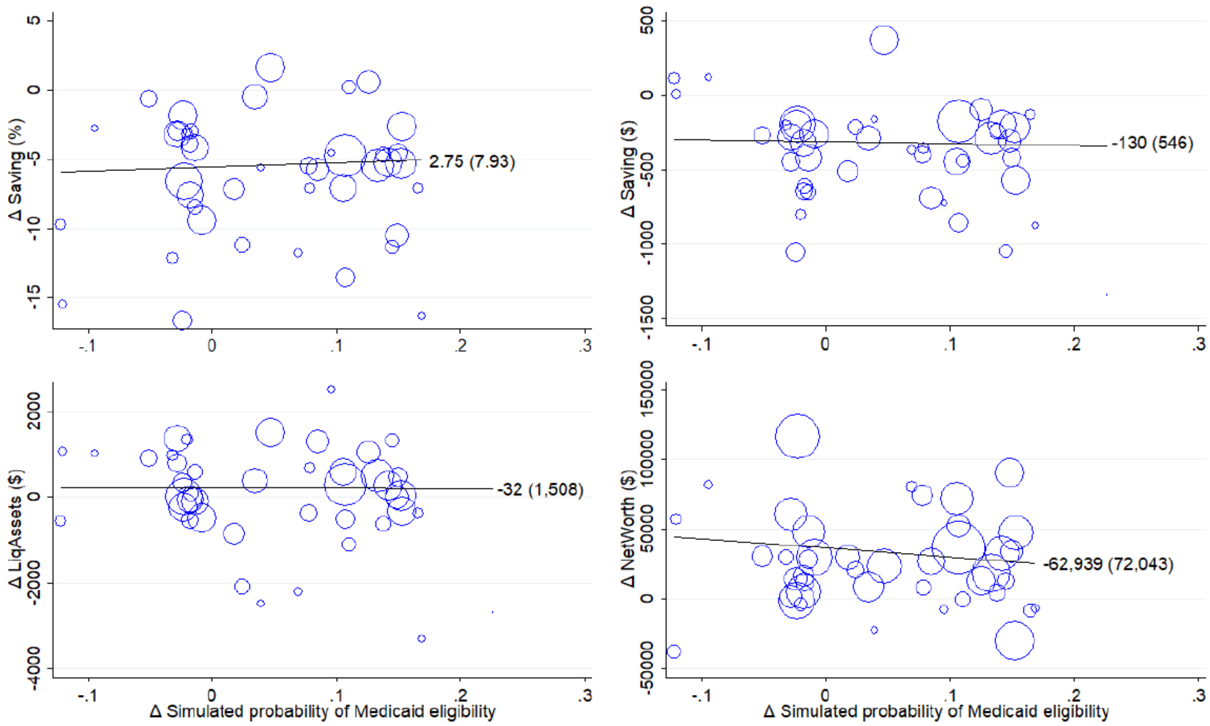
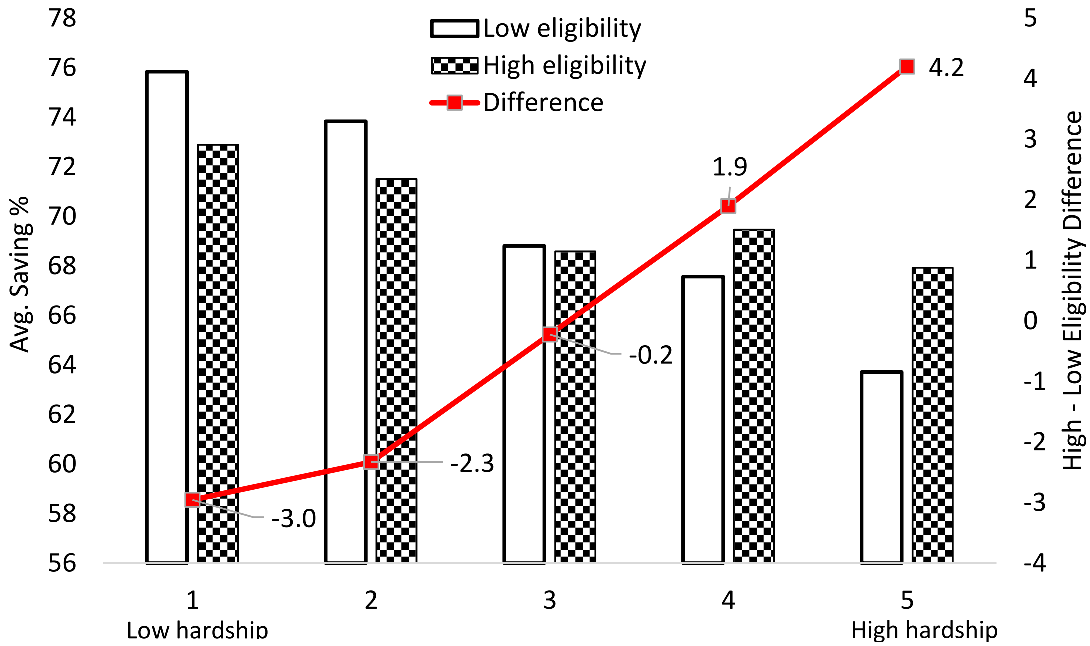


Figure plots the the 2013 to 2017 change in state-level average savings (y-axis) against the 2013 to 2017 change in state-level average Medicaid probabilities (x-axis). Each observation represents one state, where the bubble size (and, therefore, the fit line regression weighting) corresponds to the average number of observations per state in our sample of tax filers. Each plot uses a different measure of savings (y-axis): the intended percentage of the tax refund saved or used to pay down debt, *Saving* (%), the IHS transformation of the implied dollar amount, *Saving* ($), and the IHS transformation of household liquid assets, *LiqAssets* ($), and net worth, *NetWorth* ($). The beta and (standard error) are reported.

Figure 3: Nonparametric evidence on the role of hardship and Medicaid eligibility on savings



Average Medicaid eligibility across quintiles

Eligibility quintiles:	1	2	3	4	5
Avg. ProbNTL(Med)	0.00	0.02	0.07	0.15	0.34

Average hardship across quintiles

Hardship quintiles:	1	2	3	4	5
Normalized value	-1.405	-0.773	-0.148	0.733	2.370

Figure plots the average intended savings rate from the tax refund (*Savings*) (y-axis, LHS) for households in the lowest and highest Medicaid eligibility quintiles (bars) at quintiles of financial hardship (x-axis). The line represents the difference between the two bars (y-axis, RHS). Levels of Medicaid eligibility and hardship at different quintiles are documented in the tables below the graph.

Figure 4: Quantile regression estimates of the effect of Medicaid on net worth

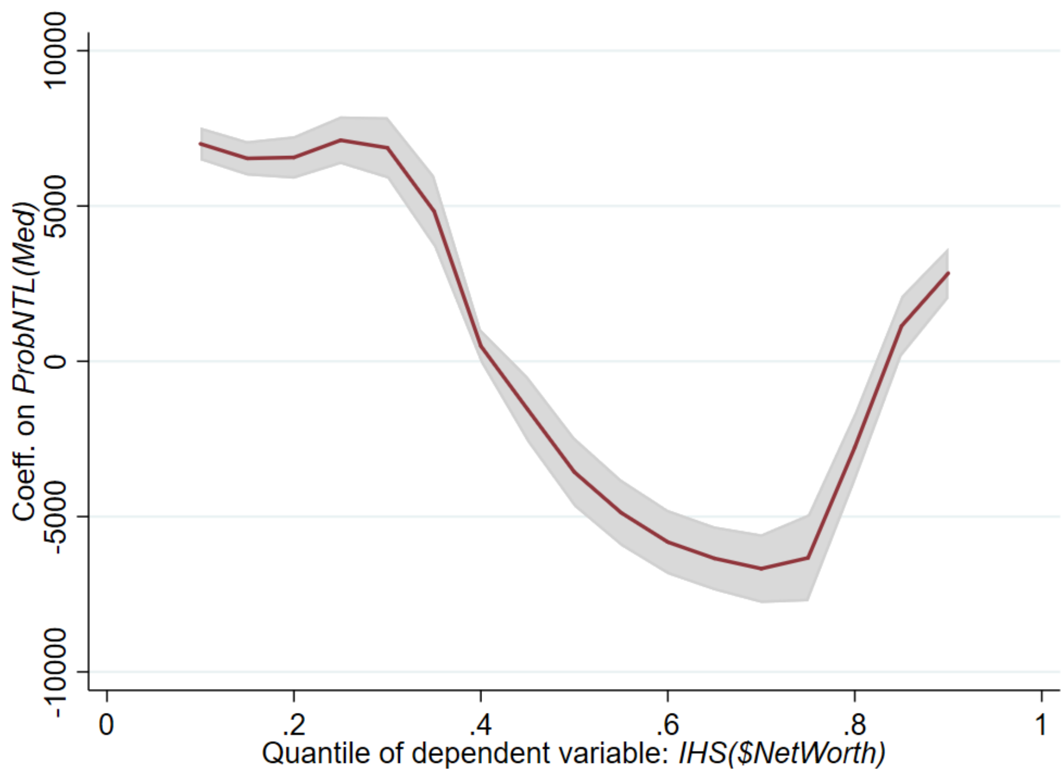


Figure plots the predicted marginal effect of the simulated probability of Medicaid eligibility, *ProbNTL(Med)*, on household net worth at different quantiles of net worth. Net worth is normalized using the IHS transformation with $\theta = 0.0003$, making the magnitude of coefficients not directly interpretable. The figure shows the coefficient estimates at every 5th quantile, from the 10th to the 90th. The shaded 95% CI is recovered through bootstrap (50 repetitions). The regression specification mathes that in Table 5, Panel A, column (4).

A Theory Model

In this section, we present a model to illustrate the mechanisms that influence a household's savings response conditional on health insurance status.

We consider a standard two-period (period 0 and 1) model of household consumption. Households select their period 0 consumption, C_0, so as to maximize their expected utility with respect to consumption in both periods. Any wealth not consumed in period 0 is saved, such that saving is just the mirror image of consumption. Households have constant relative risk aversion preferences with a relative risk aversion parameter γ. Households start with an initial wealth, W_0, and for simplicity, we assume no income. In period 1, households face a health shock with a probability p_s. If a household is uninsured, then the health shock imposes a cost, $\epsilon > 0$. Let the interest rate be 0 and the household's subjective discount factor be β. Within this set-up, the utility maximization problem in period 0 for a household without health insurance (subscript "U") can be written as:

$$\max_{C_{0,U}\geq 0} \quad \frac{C_{0,U}^{(1-\gamma)}-1}{1-\gamma} + \beta \left\{ [1-p_s]\frac{(W_0 - C_{0,U})^{(1-\gamma)}-1}{1-\gamma} + p_s \frac{(W_0 - C_{0,U} - \epsilon)^{(1-\gamma)}-1}{1-\gamma} \right\} \tag{A1}$$

Where the first term represents utility from period 0 consumption while the second term presents the expected utility from period 1 consumption. The solution to this problem is given by the solution to the following Euler equation:

$$C_{0,U}^{*-\gamma} = \beta \left\{ [1-p_s]\left(W_0 - C_{0,U}^*\right)^{-\gamma} + p_s \left(W_0 - C_{0,U}^* - \epsilon\right)^{-\gamma} \right\} \tag{A2}$$

While it is not possible to analytically solve this equation for the optimal consumption at time zero, in Table A1 we present some comparative statics that inform us, generally, about the direction of the effect of Medicaid on consumption and savings. Using this setup, we derive Proposition 1, regarding optimal savings. All the proofs are presented in Appendix Section A.2 below.

Proposition 1. *Household's optimal consumption (saving) at period 0, $C^*_{0,U}$ $(W_0 - C^*_{0,U})$, is strictly decreasing (increasing) in ϵ.*

The intuition for this proposition is simple. A higher ϵ, all else equal, implies a lower utility. The risk-averse household would wish to smooth this shock by shifting more wealth from period 0 to period 1. Note that a change in ϵ affects both household wealth and its incentives to engage in precautionary saving. Thus *Proposition 1* highlights the combined effect of the wealth and precautionary savings channel.

The above result helps us evaluate the effect of Medicaid on saving. We assume that Medicaid costlessly insures the household against health expenditure risk. One can, therefore, think of Medicaid as a special case of the above problem when ϵ decreases all the way to zero. From *Proposition 1*, we see that household saving will be lower with Medicaid than without – i.e., when $\epsilon > 0$. Thus, the combined effect of the wealth and precautionary savings channel is to reduce a household's savings when it becomes eligible for Medicaid.

Empirically, one can isolate the wealth effect from the precautionary savings effect on household consumption only if one can control for the wealth transfer resulting from Medicaid – the actuarially fair value of the insurance. In reality, the actuarially fair value of insurance from the perspective of the consumer depends on unobservables (such as risk aversion). Consequently, empirical estimation of causal effects from exogenous changes in Medicaid coverage always measures a combined effect of both the health insurance and the subsidy (Finkelstein et al., 2012).

We now extend the model to incorporate personal bankruptcy. Federal law mandates hospitals to provide emergency treatment on credit and, in some cases, provide even non-emergency care without an upfront payment. Households also have the means to avoid paying medical debt by declaring bankruptcy. The ability to obtain hospital care and then default on medical bills, declare bankruptcy, and wipe away debt could reduce a household's incentive to save for a future shock. Current bankruptcy laws allow households to discharge medical debt by giving up assets above a certain exemption limit. Mahoney (2015) argues that such bankruptcy protection, by limiting the cost of health shocks, serves the same role as health insurance. One can incorporate bankruptcy protection into the above model by changing the uninsured household's period 1 consumption in the state in which it experiences a health shock, such that:

$$C^B_{1,U} = \begin{cases} W_0 - C_{0,U} - \epsilon & \text{if} \quad W_0 - C_{0,U} - \epsilon > \delta \\ \delta & \text{if} \quad W_0 - C_{0,U} - \epsilon \leq \delta \end{cases}$$

where the superscript B indicates the ability to declare bankruptcy. Intuitively, bankruptcy puts a lower bound, δ, on household consumption (the subsistence level of consumption) in period 1. Note that the effect of bankruptcy protection on household consumption will depend on household wealth. We consider a polar case to illustrate the effect of bankruptcy on the household savings decision. Specifically, we consider households with initial wealth $W_0 < \underline{W} = \epsilon + \delta$. Such households have sufficiently low initial wealth such that any health shock in period 1 is likely to push them into bankruptcy. For such households's the period 0 problem without health insurance can be written as (we drop the superscript B for simplicity):

$$\max_{C_{0,U} \geq 0} \quad \frac{C_{0,U}^{(1-\gamma)} - 1}{1-\gamma} + \beta \left\{ [1 - p_s] \frac{(W_0 - C_{0,U})^{(1-\gamma)} - 1}{1-\gamma} + p_s \delta \right\} \tag{A3}$$

The following proposition compares the savings rate of the constrained household with and without Medicaid:

Proposition 2. *The savings of a financially constrained household* $(W_0 - C^*_{0,U})$ *is strictly larger with Medicaid than without.*

Proposition 2 says that, for the constrained household without health insurance, any period 0 saving is useful only in the state in which it does not experience a health shock. In the state in which it experiences a health shock, the period 0 savings does not affect its consumption as the consumption is floored at δ. On the other hand, Medicaid allows the household to enjoy the fruits of its savings even when it experiences a health shock as Medicaid insures against the associated health expenditure.

One important caveat to the model presented above is that it relies on rationality and immediate adjustment. A neoclassical model may not be representative of household savings' behavior. Behavioral finance models predict that households are insensitive to changes in incentives because of adjustment costs that generate inertia, difficulties processing information, and procrastination resulting from hyperbolic discounting (Carroll et al., 2009). To the extent that these forces operate in our sample, they could bias our analysis against finding results that are consistent with the predictions of a neoclassical model.

A.1 Comparative statics

In the absence of analytical solutions, we illustrate predictions from the consumption-savings model in Section A. To do this, we solve the partial equilibrium model numerically using a set of calibrated parameters. Let us assume the common risk-aversion parameter of $\gamma = 2$ and a subjective discount of $\beta \approx 0.7031$ (Laibson et al., 2007). Let us also assume, for simplicity, an interest rate of $r = -ln(\beta)$ and a constant, lower-bound on consumption of approximately zero, $\delta = 1e(-7)$. We start with two agents with high and low initial wealth, $W_{0,h} = \$3,000$ and $W_{0,l} = \$640$, respectively equal to the 75th percentile and the median liquid assets that we observe in our data. We calibrate a health shock probability of $p = 0.138$, based on the prevalence of health shocks in our sample. We set the cost of the health shock to $\epsilon = \$1,674$, the average out-of-pocket health spending in our data. We assume that bankruptcy is allowed.

The results of this exercise are shown in Table A1. We analyze two type of households according to whether or not they are financially constrained based on their initial wealth. For a financially unconstrained household ("No Hardship") that would otherwise be uninsured, receiving Medicaid increases its consumption rate (i.e., C_0/W_0 goes from 0.45 to 0.58) and reduces its savings rate ($(W_0 - C_0)/W_0$) by 24.4%. In contrast, for a constrained household ("Hardship"), receiving Medicaid would lead to a 4.47% increase in its savings rate.

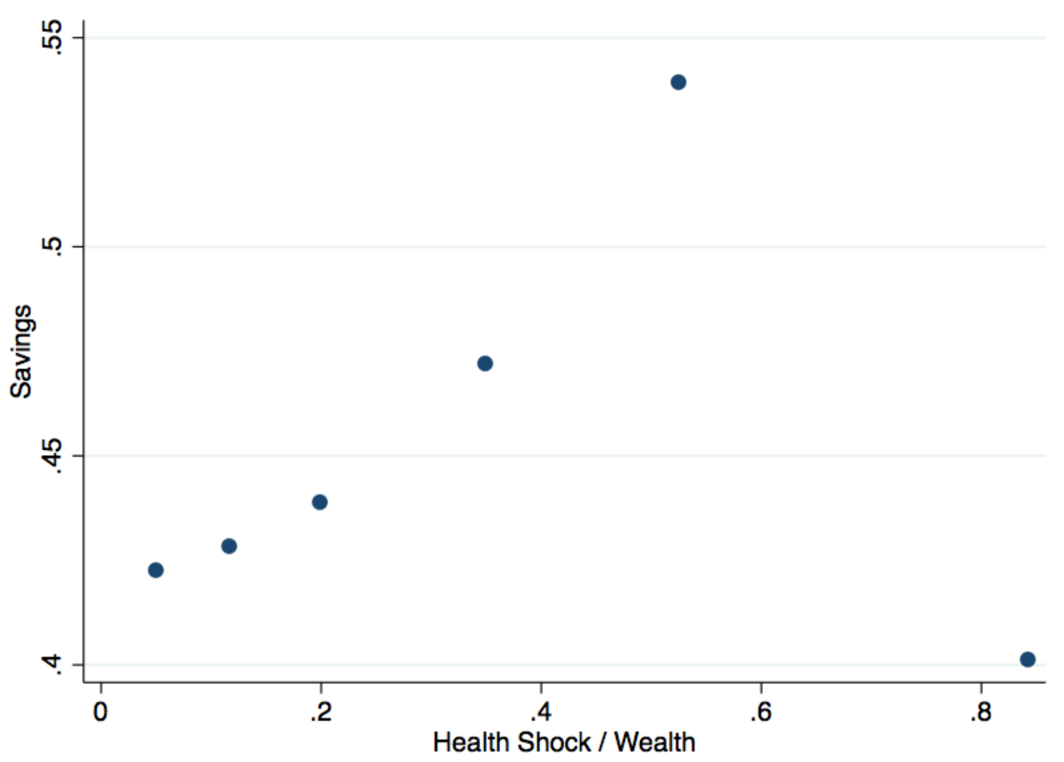
Figure A1 also illustrates this prediction. In this figure, we numerically solve for $(W_0 - C_{0,U}^{*B})/W_0$ for different values of ϵ and a set amount of initial wealth, $W_0 = 1$. The calibration of the other parameters is discussed above. We find that while households' period 0 savings rates initially increase with the severity of the potential health shock, savings rates discontinuously decrease for potentially large, catastrophic health shocks. Hence, bankruptcy protection is likely to reduce, if not eliminate, the precautionary savings motive.

Table A1: Comparative statics

This table shows the comparative statics for the counterfactual change in savings rates, $\Delta\% \,(W_0 - C_0)$, using no insurance as the base case. It is produced from calibrating the consumption-savings model in Section A.

		C_0/W_0	$C_{1,s}/W_0$	$C_{1,h}/W_0$	$(W_0 - C_0)/W_0$	$\Delta\% \,(W_0 - C_0)$
No Hardship	Uninsured	0.45	0.19	0.75	0.55	
	Medicaid	0.58	0.57	0.57	0.42	-24.40%
Hardship	Uninsured	0.60	0.00	0.54	0.40	
	Medicaid	0.58	0.57	0.57	0.42	4.47%

Figure A1: Optimal savings under varying health shock costs



The figure presents the savings rate prediction of the calibrated model from Section A for uninsured households with access to bankruptcy and different degrees of exposure to a health shock. The Y-axis represents the normalized level of savings after optimal consumption, $(W_0 - C_0^*)/W_0$, and the X-axis represents the size of the health shock as a proportion of the initial wealth, ε/W_0.

A.2 Proofs

Proof of Proposition 1. As shown in Section A, the Euler equation of the uninsured household's problem is:

$$C_{0,U}^{*-\gamma} = \beta \left\{ [1-p_s] \left(W_0 - C_{0,U}^* \right)^{-\gamma} + p_s \left(W_0 - C_{0,U}^* - \epsilon \right)^{-\gamma} \right\} \tag{A4}$$

By implicit differentiation of the first order condition:

$$\begin{aligned} -\gamma C_{0,U}^{*(-\gamma-1)} \frac{\partial C_{0,U}^*}{\partial \epsilon} = \quad & \beta \left\{ \gamma [1-p_s] \left(W_0 - C_{0,U}^* \right)^{(-\gamma-1)} \frac{\partial C_{0,U}^*}{\partial \epsilon} \right. \\ & \left. -\gamma p_s \left(W_0 - C_{0,U}^* - \epsilon \right)^{(-\gamma-1)} \left(-\frac{\partial C_{0,U}^*}{\partial \epsilon} - 1 \right) \right\} \end{aligned}$$

Solving for the derivative of consumption on the health shock,

$$\frac{\partial C_{0,U}^*}{\partial \epsilon} = - \left[\frac{\beta p_s \left(W_0 - C_{0,U}^* - \epsilon \right)^{(-\gamma-1)}}{C_{0,U}^{*-\gamma} + [1-p_s] \beta \left(W_0 - C_{0,U}^* \right)^{(-\gamma-1)} + \beta p_s \left(W_0 - C_{0,U}^* - \epsilon \right)^{(-\gamma-1)}} \right]$$

Given the standard preference's assumption of constant relative risk aversion and assuming $W_0 > \epsilon$, the right-hand side of the derivate above will be always negative. Therefore, we must have lower consumption, $C_{0,U}^*$, or equivalently higher savings, $W_0 - C_{0,U}^*$, as the size of the health shock, ϵ, increases.

Proof of Proposition 2. The Euler equation of the uninsured and constrained (subscript $\mathbb{C}$) household's problem in Equation A3 (described in Section A) is:

$$C_{0,\mathbb{C}}^{*\,-\gamma} = \beta [1-p_s] \left(W_0 - C_{0,\mathbb{C}}^* \right)^{-\gamma}$$

In this case, we can solve for the optimal consumption analytically, such that:

$$C^*_{0,\mathbb{C}} = \frac{[\beta(1-p_s)]^{(-1/\gamma)}\, W_0}{1+[\beta(1-p_s)]^{(-1/\gamma)}}$$

Theoretically, providing Medicaid (subscript $\mathbb{M}$) implies that $\epsilon = 0$, which makes the probability of a health shock irrelevant to households. Providing Medicaid also makes the household's problem in Equation A4 analytically solvable. Therefore, optimal consumption given Medicaid eligibility is given by:

$$C^*_{0,\mathbb{M}} = \frac{\beta^{(-1/\gamma)} W_0}{1+\beta^{(-1/\gamma)}}$$

Comparing consumption with and without Medicaid shows us that:

$$C^*_{0,\mathbb{M}} - C^*_{0,\mathbb{C}} = \frac{\beta^{(-1/\gamma)} W_0}{1+\beta^{(-1/\gamma)}} - \frac{[\beta(1-p_s)]^{(-1/\gamma)}\, W_0}{1+[\beta(1-p_s)]^{(-1/\gamma)}}$$

This expression can be rewritten as:

$$C^*_{0,\mathbb{M}} - C^*_{0,\mathbb{C}} = \beta^{(-1/\gamma)} W_0 \left(\frac{1}{1+\beta^{(-1/\gamma)}} - \frac{1}{(1-p_s)^{(1/\gamma)} + \beta^{(-1/\gamma)}} \right) \tag{A5}$$

The assumptions that p_s is a probability defined between 0 and 1 and the risk aversion parameter (γ) is a strictly positive number implies that:

$$0 < (1-p_s)^{(1/\gamma)} < 1$$

which makes the right-hand side of Equation A5 always negative. Therefore, as is stated in Proposition 2, we have proved that a household that is at risk of bankruptcy will always decrease their consumption, or equivalently increase their savings $\left(W_0 - C^*_0\right)$, after being eligible for Medicaid.

Internet Appendix: Additional Tables and Figures

Table IA-1: Medicaid income eligibility limits for childless adults, % of FPL

This table documents income eligibility limits (as a % of FPL) across states for non-disabled adults over time. Data is collected by The Kaiser Family Foundation.

Location	2011	2012	2013	2014	2015	2016	2017
Alabama	0	0	0	0	0	0	0
Alaska	0	0	0	0	0	1.38	1.38
Arizona	1.1	1.1	1	1.38	1.38	1.38	1.38
Arkansas	0	0	0	1.38	1.38	1.38	1.38
California	0	0	0	1.38	1.38	1.38	1.38
Colorado	0	0	0.2	1.38	1.38	1.38	1.38
Connecticut	0.73	0.72	0.7	1.38	1.38	1.38	1.38
Delaware	1.1	1.1	1.1	1.38	1.38	1.38	1.38
District of Columbia	2.11	2.11	2.11	2.15	2.15	2.15	2.15
Florida	0	0	0	0	0	0	0
Georgia	0	0	0	0	0	0	0
Hawaii	1	1	1	1.38	1.38	1.38	1.38
Idaho	0	0	0	0	0	0	0
Illinois	0	0	0	1.38	1.38	1.38	1.38
Indiana	0	0	0	0	0	1.39	1.39
Iowa	0	0	0	1.38	1.38	1.38	1.38
Kansas	0	0	0	0	0	0	0
Kentucky	0	0	0	1.38	1.38	1.38	1.38
Louisiana	0	0	0	0	0	0	1.38
Maine	0	0	0	0	0	0	0
Maryland	0	0	0	1.38	1.38	1.38	1.38
Massachusetts	0	0	0	1.38	1.38	1.38	1.38
Michigan	0	0	0	1.38	1.38	1.38	1.38
Minnesota	0	0.75	0.75	2	1.38	1.38	1.38
Mississippi	0	0	0	0	0	0	0

Continued...

Location	2011	2012	2013	2014	2015	2016	2017
Missouri	0	0	0	0	0	0	0
Montana	0	0	0	0	0	1.38	1.38
Nebraska	0	0	0	0	0	0	0
Nevada	0	0	0	1.38	1.38	1.38	1.38
New Hampshire	0	0	0	0	1.38	1.38	1.38
New Jersey	0	0	0	1.38	1.38	1.38	1.38
New Mexico	0	0	0	1.38	1.38	1.38	1.38
New York	1	1	1	1.38	1.38	1.38	1.38
North Carolina	0	0	0	0	0	0	0
North Dakota	0	0	0	1.38	1.38	1.38	1.38
Ohio	0	0	0	1.38	1.38	1.38	1.38
Oklahoma	0	0	0	0	0	0	0
Oregon	0	0	0	1.38	1.38	1.38	1.38
Pennsylvania	0	0	0	0	1.38	1.38	1.38
Rhode Island	0	0	0	1.38	1.38	1.38	1.38
South Carolina	0	0	0	0	0	0	0
South Dakota	0	0	0	0	0	0	0
Tennessee	0	0	0	0	0	0	0
Texas	0	0	0	0	0	0	0
Utah	0	0	0	0	0	0	0
Vermont	1.6	1.5	1.6	1.38	1.38	1.38	1.38
Virginia	0	0	0	0	0	0	0
Washington	0	0	0	1.38	1.38	1.38	1.38
West Virginia	0	0	0	1.38	1.38	1.38	1.38
Wisconsin	0	0	0	1	1	1	1
Wyoming	0	0	0	0	0	0	0

Table IA-2: Medicaid income eligibility limits for parents, % of FPL

This table documents income eligibility limits (as a % of the Federal Poverty Line) across states for parents over time. Data is collected by The Kaiser Family Foundation.

Location	2006	2007	2008	2009	2010	2011	2012	2013	2014	2015	2016	2017
Alabama	0.19	0.26	0.26	0.25	0.24	0.24	0.24	0.23	0.16	0.18	0.18	0.18
Alaska	0.81	0.81	0.81	0.85	0.81	0.81	0.81	0.78	1.28	1.46	1.43	1.41
Arizona	2	2	2	2	1.06	1.06	1.06	1.06	1.38	1.38	1.38	1.38
Arkansas	0.19	0.18	0.18	0.17	0.17	0.17	0.17	0.16	1.38	1.38	1.38	1.38
California	1.07	1.07	1.06	1.06	1.06	1.06	1.06	1.06	1.38	1.38	1.38	1.38
Colorado	0.38	0.67	0.66	0.66	0.66	1.06	1.06	1.06	1.38	1.38	1.38	1.38
Connecticut	1.57	1.57	1.91	1.91	1.91	1.91	1.91	1.91	2.01	2.01	1.55	1.55
Delaware	1.07	1.07	1.06	1.21	1.21	1.2	1.19	1.2	1.38	1.38	1.38	1.38
District of Columbia	2	2.07	2.07	2.07	2.07	2.07	2.06	2.06	2.21	2.21	2.21	2.21
Florida	0.6	0.58	0.56	0.55	0.53	0.59	0.58	0.56	0.35	0.34	0.34	0.33
Georgia	0.56	0.55	0.53	0.52	0.5	0.5	0.49	0.48	0.39	0.38	0.37	0.37
Hawaii	1	1	1	1	1	1	1	1.38	1.38	1.38	1.38	1.38
Idaho	0.3	0.43	0.42	0.28	0.27	0.39	0.39	0.37	0.27	0.27	0.26	0.26
Illinois	1.92	1.92	1.91	1.85	1.85	1.91	1.91	1.39	1.38	1.38	1.38	1.38
Indiana	0.28	0.27	0.26	0.26	0.25	0.36	0.24	0.24	0.24	0.24	1.39	1.39
Iowa	0.79	0.77	0.89	0.86	0.83	0.83	0.82	0.8	1.38	1.38	1.38	1.38
Kansas	0.37	0.36	0.34	0.34	0.32	0.32	0.32	0.31	0.38	0.38	0.38	0.38
Kentucky	0.68	0.66	0.64	0.62	0.62	0.62	0.59	0.57	1.38	1.38	1.38	1.38
Louisiana	0.2	0.2	0.2	0.26	0.25	0.25	0.25	0.24	0.24	0.24	0.24	1.38
Maine	1.57	2.07	2.06	2.06	2.06	2	2	2	1.05	1.05	1.05	1.05
Maryland	0.39	0.38	0.37	1.16	1.16	1.16	1.16	1.22	1.38	1.38	1.38	1.38
Massachusetts	1.33	1.33	1.33	1.33	1.33	1.33	1.33	1.33	1.38	1.38	1.38	1.38
Michigan	0.58	0.61	0.61	0.66	0.64	0.64	0.63	0.64	1.38	1.38	1.38	1.38
Minnesota	2.75	2.75	2.75	2.75	2.15	2.15	2.15	2.15	2.05	1.38	1.38	1.38
Mississippi	0.34	0.33	0.32	0.46	0.44	0.44	0.44	0.29	0.29	0.28	0.27	0.27

continued...

Location	2006	2007	2008	2009	2010	2011	2012	2013	2014	2015	2016	2017
Missouri	0.42	0.4	0.39	0.26	0.25	0.37	0.36	0.35	0.24	0.23	0.22	0.22
Montana	0.64	0.62	0.6	0.58	0.56	0.56	0.55	0.54	0.52	0.51	1.38	1.38
Nebraska	0.6	0.58	0.59	0.58	0.58	0.58	0.57	0.58	0.55	0.55	0.63	0.63
Nevada	0.84	0.86	0.94	0.91	0.88	0.88	0.87	0.84	1.38	1.38	1.38	1.38
New Hampshire	0.58	0.56	0.55	0.51	0.49	0.49	0.49	0.47	0.75	1.38	1.38	1.38
New Jersey	1	1.15	1.33	2	2	2	2	2	1.38	1.38	1.38	1.38
New Mexico	0.67	0.65	0.63	0.69	0.67	0.67	0.85	0.85	1.38	1.38	1.38	1.38
New York	1.5	1.5	1.5	1.5	1.5	1.5	1.5	1.5	1.38	1.38	1.38	1.38
North Carolina	0.56	0.54	0.52	0.51	0.49	0.49	0.49	0.47	0.45	0.45	0.44	0.44
North Dakota	0.67	0.65	0.63	0.62	0.59	0.59	0.59	0.57	1.38	1.38	1.38	1.38
Ohio	0.9	0.9	0.9	0.9	0.9	0.9	0.9	0.96	1.38	1.38	1.38	1.38
Oklahoma	0.44	0.43	0.5	0.48	0.47	0.53	0.53	0.51	0.48	0.46	0.44	0.44
Oregon	1	1	1	1	0.4	0.4	0.4	0.39	1.38	1.38	1.38	1.38
Pennsylvania	0.63	0.61	0.59	0.36	0.34	0.46	0.46	0.58	0.38	1.38	1.38	1.38
Rhode Island	1.92	1.92	1.91	1.81	1.81	1.81	1.81	1.81	1.38	1.38	1.38	1.38
South Carolina	0.97	0.97	1	0.9	0.89	0.93	0.91	0.89	0.67	0.67	0.67	0.67
South Dakota	0.59	0.58	0.56	0.54	0.52	0.52	0.52	0.5	0.54	0.53	0.52	0.51
Tennessee	0.81	0.8	0.8	1.34	1.29	1.27	1.26	1.22	1.11	1.03	1.01	0.99
Texas	0.3	0.29	0.28	0.27	0.26	0.26	0.26	0.25	0.19	0.19	0.18	0.18
Utah	0.5	0.49	0.47	0.68	0.44	0.44	0.44	0.42	0.47	0.46	0.45	0.44
Vermont	1.92	1.92	1.91	1.91	1.91	1.91	1.91	1.91	1.38	1.38	1.38	1.38
Virginia	0.31	0.31	0.31	0.3	0.29	0.31	0.31	0.3	0.52	0.45	0.39	0.38
Washington	0.81	0.79	0.76	0.77	0.74	0.74	0.73	0.71	1.38	1.38	1.38	1.38
West Virginia	0.37	0.36	0.35	0.34	0.33	0.33	0.32	0.31	1.38	1.38	1.38	1.38
Wisconsin	1.92	1.92	1.91	2	2	2	2	2	1	1	1	1
Wyoming	0.59	0.57	0.55	0.54	0.52	0.52	0.51	0.5	0.59	0.58	0.57	0.56

Table IA-3: Medicaid asset limits, 2013

This table documents the state asset limits in 2013. These are the maximum amount of assets that a household could hold and still be eligible for Medicaid. States excluded from this table had no corresponding asset test. Data is collected by The Kaiser Family Foundation.

Location	Upper limit
Alaska	$2,000
Arkansas	$1,000
California	$3,150
Florida	$2,000
Georgia	$1,000
Hawaii	$3,250
Idaho	$1,000
Indiana	$1,000
Iowa	$2,000
Kentucky	$2,000
Maine	$2,000
Michigan	$3,000
Minnesota	$20,000
Montana	$3,000
Nebraska	$6,000
Nevada	$2,000
New Hampshire	$1,000
North Carolina	$3,000
Oregon	$2,500
South Carolina	$30,000
South Dakota	$2,000
Tennessee	$2,000
Texas	$2,000
Utah	$3,025
Vermont	$3,150
Washington	$1,000
West Virginia	$1,000

Table IA-4: Test of correlation between financial variables and the instrument for Medicaid eligibility, state vs. national income distribution

This table shows the results of regressions that relate $ProbNTL(Med)$ with average financial characteristics of demographic groups within state-years. The dependent variable is the probability of Medicaid eligibility for a given demographic group and state-year combination, $ProbNTL(Med)$. The independent variables are the average income of the corresponding group, $\bar{Income}$, and the fraction of households in the corresponding group that owns a house, $\bar{HomeOwn}$, based on the 2013–2016 ACS. All regressions include socio-demographics fixed effects, δ_j, and state-year fixed effects, $\delta_{s,t}$, (not shown). Standard errors, shown in parentheses, are clustered on state. *p = 0.1; **p = 0.05; ***p = 0.01 (statistically significant)

Dependent variable:	$ProbNTL(Med)$	$ProbNTL(Med)$
$\bar{Income}$	-0.001	
	(0.001)	
$\bar{HomeOwn}$		-0.004
		(0.003)
N	274858	274858
Adj. R-squared	0.785	0.785

Table IA-5: Reduced form estimates, multiple robustness checks

This table presents reduced form estimates. The dependent variable is the fraction of the tax refund that a household elects to save, *Saving* (measured in percentage points). Key explanatory variables include household's simulated Medicaid eligibility, $ProbNTL(Med)$, and tercile dummies of *Hardship*: $LowHardship$, $MidHardship$ and $HighHardship$. All regressions include controls for $ProbNTL(Med) \times AssetTest_{s,t}$, socio-demographics, as well as state-year fixed effects (not shown). Standard errors, shown in parentheses, are clustered on state. *p = 0.1; **p = 0.05; ***p = 0.01 (statistically significant). Columns (1)-(3) test the robustness of the main specification, measuring hardship according to the principal components analysis detailed in Section 5.3. Column (1) presents the main specification in the paper. Column (2) presents estimates after removing parents living in the 21 states that reduced parent coverage as well as childless adults in Vermont. This test assures us that our main estimates are not not entirely powered by a loss of Medicaid coverage in certain states following the ACA. Column (3) shows the results of a test, wherein the instrument, $ProbNTL(Med)$, is generated using future, rather than current, Medicaid eligibility. In this test, we run our main specification on the 2013 sample, using the 2014 simulated probabilities of Medicaid eligibility. In Column (4), the ratio of *Liquid Assets/Income* is used in place of the *Hardship* measure from our main analysis. In particular, we use tercile dummies of liquid assets as a share of income.

	Dependent variable: *Saving*			
	(1)	(2)	(3)	(4)
	Original	Reduced Eligibility	Future Eligibility	Liquidity/Income Hardship
$ProbNTL(Med)$	-2.502	-2.20	-3.11	-3.12
	(3.818)	(3.06)	(4.89)	(3.11)
$ProbNTL(Med) \times HighHardship$	10.694***	12.06***	4.71	10.89***
	(3.250)	(3.50)	(5.14)	(3.21)
$ProbNTL(Med) \times LowHardship$	0.317	0.73	-1.42	0.45
	(2.730)	(2.79)	(5.98)	(2.73)
$HighHardship$	-3.062***	-3.09***	-6.90***	-3.04***
	(0.381)	(0.42)	(0.77)	(0.38)
$LowHardship$	3.593***	3.57***	6.63***	3.60***
	(0.422)	(0.49)	(0.76)	(0.42)
N	57,648	51031	11912	57560
Adj. R-squared	0.071	0.07	0.05	0.07

Table IA-6: Reduced form estimates using the 2013 probability of Medicaid eligibility

This table presents reduced form estimates. The dependent variable is the fraction of the tax refund that a household elects to save, *Saving* (measured in percentage points). Key explanatory variables include a household's simulated Medicaid eligibility based on 2013 eligibility thresholds, $ProbNTL2013(Med)$, an indicator for whether the state has an asset test in place at the time of sampling, $AssetTest_{s,t}$, an indicator of top tercile financial strain, $HighHardship$. Regressions include the sample of parents only because very few states provided any coverage to childless adults in 2013. All regressions include socio-demographic controls as well as state-year fixed effects (not shown). Standard errors, shown in parentheses, are clustered on state. *p = 0.1; **p = 0.05; ***p = 0.01 (statistically significant)

Dependent variable: *Saving*		
	(1)	(2)
$ProbNTL2013(Med)$	0.241	-2.926
	(4.471)	(4.280)
$ProbNTL2013(Med) \times HighHardship$		6.568**
		(2.784)
$HighHardship$		-6.354***
		(0.510)
$ProbNTL2013(Med) \times AssetTest_{s,t}$	-15.852***	-17.980***
	(5.598)	(5.946)
N	14,023	13,952
Adj. R-squared	0.088	0.097
Sample	Parents	Parents

Table IA-7: Correlation between medical spending/debt and Medicaid eligibility/enrollment, OLS estimates

This table shows OLS estimates. The dependent variables capture a household's total out-of-pocket medical spending during the last year, $IHS(\$MedSpend)$, and the same variable plus its amount of medical debt, $IHS(\$MedSpend + \$MedDebt)$ – both variables are transformed using the IHS. The first explanatory variable is Medicaid eligibility (*Med*), estimated from a household's annual adjusted gross income, household size, and corresponding state eligibility threshold. The second explanatory variable is survey-reported Medicaid enrollment, $MedEnroll$. All regressions include socio-demographic controls as well as state-year fixed effects (not shown). Standard errors, shown in parentheses, are clustered on state. *p = 0.1; **p = 0.05; ***p = 0.01 (statistically significant)

Dependent variable:	$IHS(\$MedSpend)$		$IHS(\$MedSpend + \$MedDebt)$	
Med	-270.587***		-147.417***	
	(22.844)		(21.154)	
$MedEnroll$		-619.051***		-509.155***
		(26.710)		(35.608)
N	56457	50493	57645	51538
Adj. R-squared	0.053	0.067	0.090	0.092

Notes on Table IA-8:

It is important to instrument for enrollment. A reasonable concern is that households may not be aware of their Medicaid access until after a health event. If greater awareness comes from health shocks, then "awareness," as measured through enrollment, might be correlated with the household's financial condition (and its savings decision) through both an income (Dobkin et al., 2018a) channel and a health spending (Finkelstein et al., 2012) channel. This issue poses an identification challenge.

To overcome this issue, we run a 2SLS regression with enrollment as the endogenous outcome variable and either actual or simulated Medicaid eligibility as the instrument. Results are presented in Table IA-8.[29] The interaction coefficients have the same signs as in our main 2SLS regression, in Table 4 (in which Medicaid *eligibility*, rather than *enrollment*, is the first stage endogenous variable). Panels correspond to whether actual (Panel A) or simulated (Panel B) Medicaid eligibility is used as the instrument for enrollment. In both panels, the interaction effects are statistically significant, and large in magnitude – indicating that constrained households save 19.6 percentage points *more* of their tax refund when they are *enrolled* in Medicaid. Compare this amount to the 5 percentage point effect in the 2SLS regression in Table 4.

There is also some evidence of a precautionary savings effect. Households that are not in hardship save 11–38 percentage points less of their tax refund when they are enrolled in Medicaid. This effect is statistically significant only in Panel A, however, where actual Medicaid eligibility is used as the instrument for Medicaid enrollment. Clearly, actual Medicaid eligibility is a stronger instrument for enrollment than simulated eligibility (see the F-statistics at the bottom of the tables); although, actual eligibility may result in biased 2SLS estimates if households manipulate their income to qualify for Medicaid.

[29]Note that these tests are subject to an unknown degree of measurement error. Due to the way the question was worded on the survey, we cannot perfectly distinguish the adult's Medicaid enrollment status from the child's. Missclassification is possible in about 8% of cases (a detailed discussion of this problem is available in the appendix of Gallagher et al. 2019). Our simulated instrument is, of course, specific to the adult in the household only.

Table IA-8: The effect of Medicaid enrollment on tax refund savings, 2SLS IV estimates

This table presents 2SLS IV regression estimates. The dependent variables are the fraction of the tax refund that a household intends to save, *Saving* (measured in percentage points), as well as the IHS transformation of *Saving* in dollars, *IHS($Saving)*, or of liquid assets, *IHS($LiqAssets)*. The endogenous outcome variable in the first stage (not shown) is Medicaid enrollment (*MedEnroll*) as well as its interaction with top tercile of our financial hardship index ($HighHardship$). First stage regression F-statistics, based on the Kleibergen-Paap weak instrument test, are shown below each table. The instrument in Panel A is actual Medicaid eligibility (*Med*), as determined by the 1040-tax form, as well as its interaction with $HighHardship$. The instrument in Panel B is simulated Medicaid eligibility, $ProbNTL(Med)$, as well as its interaction with $HighHardship$. All regressions include controls for $ProbNTL(Med) \times AssetTest_{s,t}$, socio-demographics, as well as for state-year fixed effects (not shown). Standard errors, shown in parentheses, are clustered on state. *p = 0.1; **p = 0.05; ***p = 0.01 (statistically significant)

Panel A: Actual Medicaid eligibility as the instrument for Medicaid enrollment

	Saving	*Saving*	*IHS($Saving)*	*IHS($Saving)*	*IHS($LiqAssets)*	*IHS($LiqAssets)*
$Med\hat{E}nroll$	-31.460***	-37.636***	-1,610.981***	-1,849.674***	-2,311.755***	-2,306.799***
	(2.703)	(2.991)	(178.041)	(202.347)	(194.082)	(236.958)
$Med\hat{E}nroll \times HighHardship$		19.635***		625.590***		1,285.426***
		(2.660)		(124.156)		(190.208)
$HighHardship$		-6.561***		-69.798**		-1,788.019***
		(0.574)		(28.398)		(65.349)
N	51,481	51,481	51,481	51,481	51,481	51,481
First stage F-test ($MedEnroll$)	443.17	237.06	443.17	237.06	443.17	237.06
First stage F-test ($MedEnroll \times HardRent$)		211.57		211.57		211.57

Panel B: Simulated Medicaid eligibility as the instrument for Medicaid enrollment

	Saving	*Saving*	*IHS($Saving)*	*IHS($Saving)*	*IHS($LiqAssets)*	*IHS($LiqAssets)*
$Med\hat{E}nroll$	-13.692	-11.394	-571.465	-551.870	-1,056.753	-199.272
	(15.416)	(16.510)	(657.854)	(698.028)	(1,038.220)	(1,068.951)
$Med\hat{E}nroll \times HighHardship$		19.167**		571.198		1,871.447***
		(7.565)		(349.541)		(609.003)
$HighHardship$		-8.371***		-151.939***		-2,079.999***
		(1.236)		(43.587)		(158.707)
N	51,481	51,481	51,481	51,481	51,481	51,481
First stage F-test ($MedEnroll$)	27.10	30.89	27.10	30.89	27.10	30.89
First stage F-test ($MedEnroll \times HardRent$)		28.91		28.91		28.91

Table IA-9: Principal components analysis of financial hardship

This table describes the principal components of variables that proxy financial hardship: *LateRent*, *LowNW*, *SkipFood*, *Overdraft*, and *CCDecline*. In Panel A, the eigenvalues for different components and a variance decomposition are reported. In Panel B, the factor loadings used to construct our index of financial hardship are reported.

Panel A. Eigen values of the correlation matrix

	Eigenvalue	Difference	Proportion	Cum.
Comp1	2.02	1.12	0.40	0.40
Comp2	0.91	0.12	0.18	0.59
Comp3	0.79	0.14	0.16	0.74
Comp4	0.65	0.02	0.13	0.87
Comp5	0.63		0.13	1.00

Panel B. Corresponding eigen vectors

	Comp1	Comp2	Comp3	Comp4	Comp5
LateRent	0.48	-0.01	-0.50	0.38	0.60
LowNW	0.32	0.87	0.38	0.02	0.06
SkipFood	0.48	0.10	-0.51	-0.22	-0.67
Overdraft	0.48	-0.31	0.29	-0.71	0.30
CCDecline	0.45	-0.38	0.51	0.56	-0.29

Table IA-10: Summary statistics by hardship tercile

Table documents summary statistics for key variables. The sample is split by low versus high tercile of hardship. Statistically significant differences are marked with an asterisk.

	LowHardship		*HighHardship*		Difference	
	Mean	Median	Mean	Median	Mean	Median
Savings (%)	77.31	100.00	69.93	90.00	7.38	10.00
Refund ($)	1,368.08	783.00	2,193.17	1,120.00	825.09*	337.00
Savings ($)	1,137.46	610.00	1,639.20	797.00	501.74*	187.00
LiqAssets ($)	5,596.52	2,200.00	898.48	195.00	4,698.04*	2,005.00
Net worth ($)	61,235.65	5,627.00	-3,169.17	-6,875.00	64,404.82	12,502.00
Income (% FPL)	1.06	0.99	1.00	0.93	0.06	0.06
LateRent	0.00	0.00	0.56	1.00	0.56*	1.00
LowNW	0.36	0.00	0.68	1.00	0.32*	1.00
SkipFood	0.00	0.00	0.78	1.00	0.78*	1.00
Overdraft	0.00	0.00	0.70	1.00	0.7*	1.00
CCDecline	0.00	0.00	0.43	0.00	0.43*	0.00
Med	0.41	0.00	0.40	0.00	0.01	0.00
ProbNTL(Med)	0.12	0.07	0.11	0.06	0.01	0.01
ProbSTATE(Med)	0.12	0.05	0.11	0.03	0.01	0.02
Age	31.39	27.00	35.00	32.00	3.61*	5.00
College grad	0.55	1.00	0.40	0.00	0.15*	1.00
White	0.86	1.00	0.81	1.00	0.05	0.00
Parents	0.14	0.00	0.34	0.00	0.20*	0.00
Male	0.53	1.00	0.50	0.00	0.03	1.00
N	27,352		17,277			

Table IA-11: The effect of Medicaid eligibility on savings measures, OLS estimates

This table presents OLS regression estimates. The dependent variables are the fraction of the tax refund that a household elects to save, *Saving* (measured in percentage points), the IHS transform of the implied dollar amount of the tax refund saved, *IHS($Savings)*, a household's liquid assets, *IHS($LiqAssets)*, and household's net worth, *IHS($NetWorth)*. There is no instrument. Instead these savings measures are regressed directly on a binary indicator of whether the household is eligible for Medicaid, according to their 1040 tax form adjusted gross income, family size, and state of residence (*Med*). Certain specifications include and interaction between *Med* and *HighHardship*, our indicator of extreme financial constraint. All regressions include controls for $Med_i \times AssetTest_{s,t}$, socio-demographics, as well as state-year fixed effects (not shown). Standard errors, shown in parentheses, are clustered on state. *p = 0.1; **p = 0.05; ***p = 0.01 (statistically significant)

Dependent:	*Saving*		*IHS($Savings)*		*IHS($LiqAssets)*		*IHS($NetWorth)*	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Med	-5.757***	-6.282***	-288.513***	-291.626***	-410.786***	-363.759***	-1774.006***	-1463.941***
	(0.342)	(0.339)	(26.970)	(23.959)	(39.826)	(47.299)	(125.266)	(136.341)
$Med \times HighHardship$		2.879***		21.893		152.674***		20.460
		(0.706)		(29.198)		(41.622)		(146.491)
HighHardship		-6.299***		-63.694***		-1675.449***		-5665.863***
		(0.418)		(17.013)		(44.012)		(119.587)
Adj. R-squared	57560	57560	57560	57560	57560	57560	57560	57560
N	0.069	0.073	0.526	0.526	0.112	0.188	0.075	0.136

Notes on Table IA-12:

Table IA-12 helps address concerns about possible feedback effects between medical spending-related tax deductions, the size of the tax refund, and the savings response to Medicaid. Indeed, medical spending and medical debt are negatively correlated with Medicaid (see to Table IA-7) and, in principal, this correlation could influence the size of the tax refund. A number of studies observe variation in MPCs according to the size of the tax refund (Browning and Collado, 2001; Hsieh, 2003; Kueng, 2015). However, such concerns are aleviated by the fact that we do not observe a strong relationship between Medicaid and health-related tax deductions.

From the line items in the 1040 Form, we are able to compare the association between healthcare-related deductions and Medicaid eligibility. We find that a 1 percentage point increase in the probability of Medicaid eligibility decreases health-related deductions by just $13 (medical and dental expenses, health savings accounts, and self-employed health insurance premiums). Relative to the average health-related deductions, this is just a 3% decline. This may be because lower-income households tend to take the standard deduction. Regardless, such a small change in health-related deductions due to Medicaid is unlikely to bias our estimates, even if there is a higher MPC out of large payments relative to small payments.

As a more formal test of this conclusion, in Table IA-12, Panel A, we repeat our reduced form estimates with the *size of the tax refund* (both in inverse hyperbolic sine and in dollars, separately) as the dependent variable. We find no statistically significant relationship between Medicaid eligibility and the magnitude of the tax refund. We conclude that differences in medical expenditures created by Medicaid do not translate into meaningful differences in the size of the tax refund.

We also evaluate whether our estimates vary by total refund payment. In Panel B, we run our main reduced form regressions with *Savings* as the dependent variable, but controlling for the size of the tax refund (column 1). We also repeat the main regressions within subsamples identified based on the size of the refund (columns 2-4). After controlling for the size of the refund, results are unchanged. The coefficient on the control, $IHS(\$Refund)$, in column 1 is significantly positive, which suggests that smaller payments might have have higher MPCs (less *Savings*). While the magnitude of the estimates on $ProbNTL(Med)$ are reasonably similar across the refund-size subsamples, the estimate on the interaction term ($ProbNTL(Med) \times HighHardship$) is statistically strongest within the *Large* refund group. Although that might indicate a statistically stronger tendency to save out of large payments, the coefficients are not statistically different across subsamples.

Table IA-12: The effect of Medicaid on tax refund savings, reduced form estimates, by refund size

This table presents reduced form estimates. In Panel A, the dependent variable is the IHS transformation of the *Refund* measured in dollars (*columns 1-4*), and the refund measured in dollars (*columns 1-4*). In Panel B, the dependent variable is the fraction of the tax refund that a household elects to save, *Saving* (measured in percentage points), and the sample is split into terciles according to the size of the refund. Key explanatory variables included are household's simulated Medicaid eligibility, $ProbNTL(Med)$, a top tercile dummy of *Hardship*: $HighHardship$, and a control for the IHS transormation of the dollar size of the refund, $IHS(\$Refund)$. All regressions include controls for $ProbNTL(Med) \times AssetTest_{s,t}$, socio-demographics, as well as state-year fixed effects (not shown). Standard errors, shown in parentheses, are clustered on state. *p = 0.1; **p = 0.05; ***p = 0.01 (statistically significant)

Panel A. Dependent variable: *$Refund*

Dependent variable tranform:	*IHS($)*	*IHS($)*	$	$
	(1)	*(2)*	*(3)*	*(4)*
$ProbNTL(Med)$	155.56	140.20	249.44	246.67
	(108.11)	(111.28)	(153.09)	(155.50)
$ProbNTL(Med) \times HighHardship$		54.86		14.01
		(79.98)		(111.78)
$HighHardship$		14.51		20.69
		(11.57)		(15.81)
N	57560	57560	57560	57560
Adj. R-squared	0.61	0.61	0.61	0.61

Panel B. Dependent variable: *Saving*

Refund size sample:	All	Small	Medium	Large
	(1)	(2)	(3)	(4)
$ProbNTL(Med)$	-3.34	-0.25	-2.51	-0.51
	(2.24)	(4.54)	(4.65)	(3.12)
$ProbNTL(Med) \times HighHardship$	9.98***	8.88	7.18	9.02**
	(2.74)	(6.09)	(5.51)	(3.45)
$HighHardship$	-5.41***	-4.53***	-5.79***	-5.78***
	(0.37)	(0.85)	(0.53)	(0.48)
$IHS(\$Refund)$	0.01***			
	(0.00)			
N	57640	19227	19202	19213
Adj. R-squared	0.12	0.04	0.08	0.10

Table IA-13: The relationship between health expenditure over the 6-months following tax time and simulated Medicaid eligibility, OLS estimates

This table shows regression estimates from the model:
$IHS(\$SpendMed_{i,t+1}) = \alpha + \beta_1 HighHardship_{i,t} + \beta_2 ProbNTL(Med)_{i,t} + IHS(\$SpendMed_{i,t}) + X'\gamma + \delta_{s,t} + \epsilon_i$
The dependent variable, $IHS(\$SpendMed_{i,t+1})$, measures household i's out-of-pocket health expenditure over the six months after filing their taxes (i.e., the period in which households receive their tax refund and can spend from it) transformed using the IHS. This measure comes from a follow-up survey that is conducted every August-September. The key independent variable is an indicator that a household is in the top tercile of our index of financial hardship, $HighHardship_t$. We control for a household's simulated probability of Medicaid eligibility, $ProbNTL(Med)_{i,t}$, as of tax time, as well as it's healthcare spending over the 6-months ending at tax time, $IHS(\$SpendMed_{i,t})$ (not shown). As insured households may be less likely to defer care until receipt of the tax refund, the sample is split according to whether the household is insured or uninsured as of tax time. All regressions control for socio-demographics and state-year fixed effects (not shown). *p = 0.1; **p = 0.05; ***p = 0.01 (statistically significant)

Dependent variable: $IHS(\$SpendMed_{i,t+1})$				
	(1)	(2)	(3)	(4)
$HighHardship_{i,t}$	93.852	94.551	-30.482	-30.666
	(72.901)	(72.920)	(34.252)	(34.202)
$ProbNTL(Med)_{i,t}$		328.025		205.804
		(556.515)		(205.546)
Sample:	Uninsured		Insured	
N	854	854	6792	6792
Adj. R-squared	0.087	0.086	0.187	0.187

Table IA-14: Reduced form effects of Medicaid on savings, by skipped medical care

This table presents reduced form estimates. The dependent variables are the fraction of the tax refund that a household intends to save, *Saving* (measured in percentage points), as well as the IHS transformation of *Saving* in dollars, *IHS($Saving)*, or of liquid assets, *IHS($LiqAssets)*. Explanatory variables include household's simulated Medicaid eligibility probability, $ProbNTL(Med)$, an indicator for whether the state has an asset test in place at the time of sampling, $AssetTest_{s,t}$, and an indicator of top tercile financial strain, $HighHardship$. Regression samples are split according to an indicator of whether the households reports having skipped medical care in past 6 months. All regressions include socio-demographic controls as well as state-year fixed effects (not shown). Standard errors, shown in parentheses, are clustered on state. *p = 0.1; **p = 0.05; ***p = 0.01 (statistically significant)

Dependent variable:	*Saving*		*IHS($Saving)*		*IHS($LiqAssets)*	
Subsample:	SkipMed	No SkipMed	SkipMed	No SkipMed	SkipMed	No SkipMed
$ProbNTL(Med)$	-1.64	-0.57	-46.47	63.20	-324.30	-226.06
	(4.37)	(3.86)	(216.57)	(147.61)	(280.63)	(243.94)
$ProbNTL(Med) \times HighHardship$	10.88***	7.52**	158.44	176.57	576.21**	1453.42***
	(4.05)	(3.21)	(102.03)	(105.70)	(257.20)	(213.31)
$HighHardship$	-3.81***	-5.95***	-38.95**	-69.64***	-1015.58***	-1771.24***
	(0.49)	(0.44)	(16.20)	(15.58)	(41.16)	(41.20)
$ProbNTL(Med) \times AssetTest_{s,t}$	-11.37	-22.36***	-2314.72***	-2225.04***	368.09	144.82
	(6.87)	(7.42)	(631.54)	(349.68)	(436.76)	(315.37)
N	18190	39362	18190	39362	18190	39362
Adj. R-squared	0.07	0.07	0.49	0.53	0.16	0.16
Difference p-value:						
$Prob(Med)$	0.844		0.582		0.823	
$ProbNTL(Med) \times HighHardship$	0.459		0.910		0.005	

Table IA-15: The impact of state bankruptcy and Medicaid rules on savings behavior, reduced form estimates on border counties

This table presents reduced form estimates. The dependent variables are the fraction of the tax refund that a household elects to save, *Saving* (measured in percentage points), refund savings measured in dollars, *IHS($Saving)*, and household's liquid assets, *IHS($LiqAssets)* – dollar values are transformed using the IHS. Key explanatory variables include a household's simulated Medicaid eligibility, $ProbNTL(Med)$ and an indicator of financial strain, $HighHardship$. The sample is split according to the Mahoney (2015) parameterization of state bankruptcy rules, *CostB* – which is calculated as the mean financial cost of bankruptcy as though the national sample faced the asset exemption rules of each state. The data is restricted to include only counties that border a county in a different state, where the two states differ in their classification as either a high or low cost of bankruptcy state (*HighCostB* and *LowCostB*). To ensure sufficient sample size within each border county-pair, we further restrict the sample to only metropolitan statistical area (MSA) counties. All regressions include controls for $ProbNTL(Med) \times AssetTest_{s,t}$, socio-demographics, as well as border county-pair-year fixed effects (not shown). The bottom row of the table reports p-values from an F-test for the equality of reported coefficients. Standard errors, shown in parentheses, are clustered on state. *p = 0.1; **p = 0.05; ***p = 0.01 (statistically significant)

Dependent variable:	*Saving*		*IHS($Saving)*		*IHS($LiqAssets)*	
	(1)	(2)	(3)	(4)	(7)	(8)
$ProbNTL(Med)$	-0.25	-20.97***	493.81**	-140.22	-362.17	-641.36
	(6.74)	(6.93)	(182.72)	(294.66)	(319.25)	(688.26)
$ProbNTL(Med) \times HighHardship$	3.63	16.97**	-229.17	92.07	1512.79***	1999.18***
	(13.17)	(7.50)	(548.85)	(253.38)	(463.45)	(379.78)
$HighHardship$	-4.78***	-3.45***	-111.41**	-46.53	-1493.76***	-1879.25***
	(1.39)	(1.01)	(43.48)	(35.10)	(90.53)	(115.65)
N	3656	3797	3656	3797	3656	3797
Adj. R-squared	0.08	0.07	0.51	0.51	0.21	0.21
Sample	*LowCostB*	*HighCostB*	*LowCostB*	*HighCostB*	*LowCostB*	*HighCostB*
Difference p-value:						
$ProbNTL(Med)$	0.030		0.040		0.728	
$ProbNTL(Med) \times HighHardship$	0.358		0.563		0.398	

Figure IA-1: Evidence of a monotonic instrument

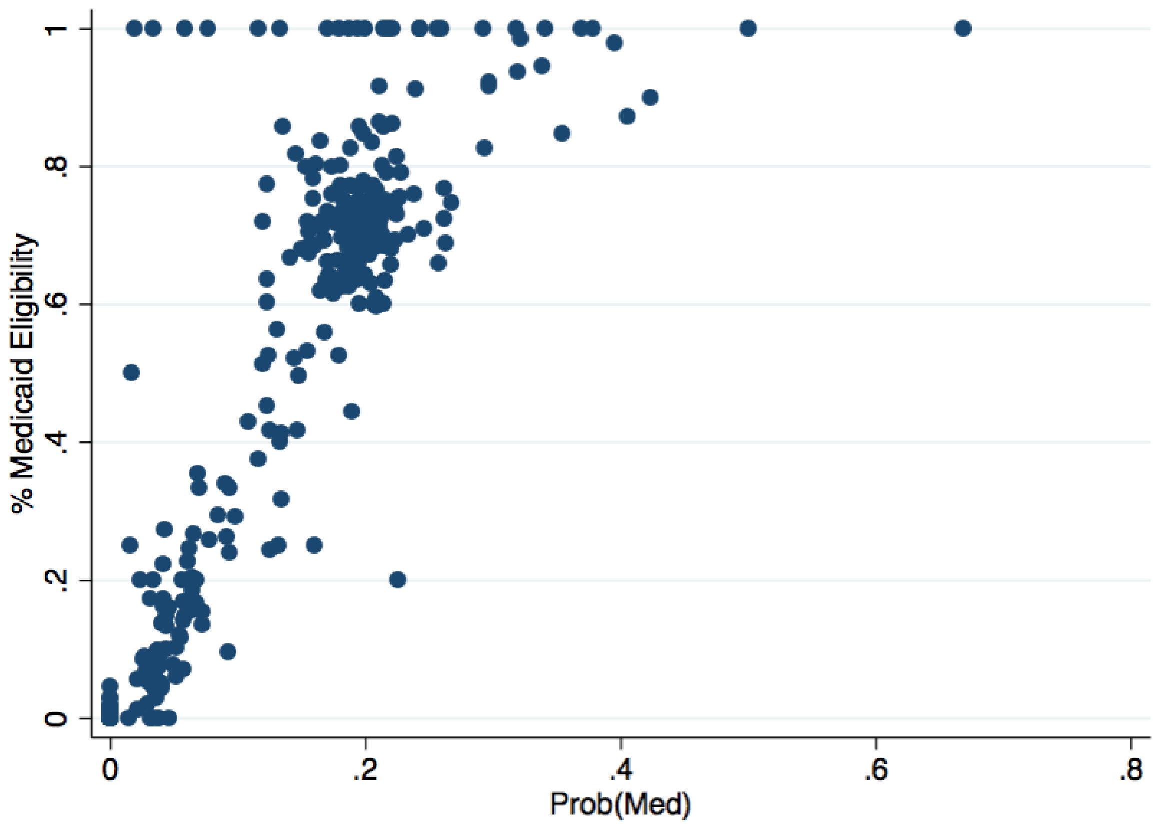
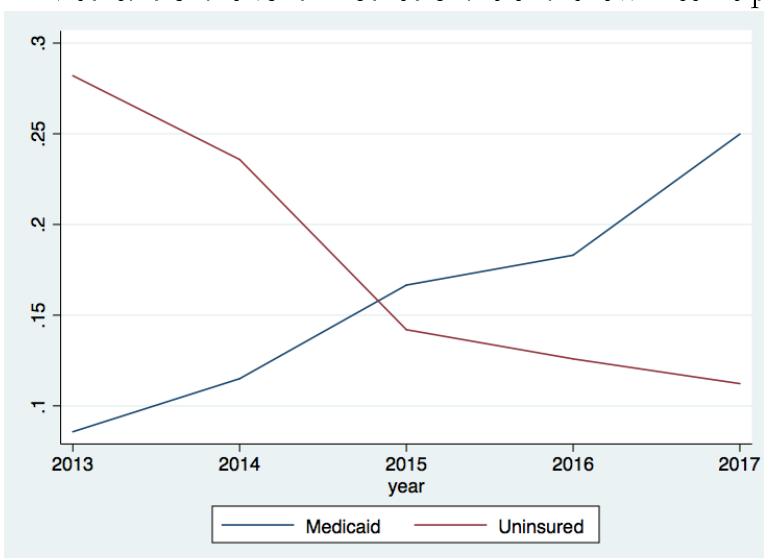


Figure plots the actual Medicaid eligibility share (y-axis) against the average simulated probability of Medicaid eligibility (x-axis) within groups – where groups are formed be splitting the sample according to parent status, year, and state. Actual Medicaid eligibility is determined by the household's adjusted gross incomes as reported on the 1040 Form. Simulated Medicaid eligibility, $ProbNTL(Med)$, is generated as described in Section 5.1. Note that we use the full national sample, which is unrestricted by income, to simulate the Medicaid probabilities of our low-income tax filer sample. This is why the slope is steep (not at 45 degrees) and there are occasions of 100% Medicaid eligible within certain state-years that have very high eligibility ceilings.

Figure IA-2: Medicaid share vs. uninsured share of the low-income population



This figure shows the percentage of low-income households enrolled in Medicaid and the percentage of low-income households that are uninsured, by year. These statistics are based on our sample of tax filers, weighted according to the ACS sample of low-income households.