# Does Medicaid Expansion Increase the Ability of Low-Income Households to Self-Insure?

Daeyong Lee*

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## ABSTRACT

This article examines the effects of the Medicaid expansion on household unearned income by focusing on the Affordable Care Act. The Affordable Care Act extended Medicaid coverage to childless adults and eliminated the asset-test for its eligibility. Using the March Current Population Survey Supplement data, the author finds that households with no dependent children and income below the 100% federal poverty level living in Medicaid-expansion states significantly increased the annual dividend (interest) income by $63 ($84) after the Medicaid expansion. At the same time, the financial assistance these households received from relatives or friends decreased by $159 after the expansion.

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* *Affiliation: Human Development & Family Studies, Iowa State University, Ames, IA 50011.*
*E-mail: daelee@iastat.edu.*

## I. Introduction

When the Affordable Care Act (ACA) expanded Medicaid coverage to childless adults and eliminated the asset-test to simplify the Medicaid application process in January 2014, the concern was that individuals with lots of money spread across bank accounts and real estate but with low taxable income would take advantage of the Medicaid program.[1] Yet the expansion advocates believed that the negative effect from this loophole would be trivial because the majority of Medicaid beneficiaries have very low levels of life-time wealth, and thus these low-income and low-asset households would mostly benefit from the asset-test elimination under the ACA expansion and increase their ability to self-insure against health-related risk.

In this article, I investigate whether the ACA Medicaid expansion increased unearned income, such as dividends and interest from savings and investments for low-income households, and identify which group of households (low-income and low-asset vs. low-income but high-asset) increased their income in response to the ACA Medicaid expansion. On the one hand, Medicaid beneficiaries who were bound to an asset-test cutoff (e.g., $1,000) and thus had restrictions on asset holdings before the reform would increase their savings and investments after the expansion. On the other hand, Medicaid beneficiaries who were far below the asset-test cutoff and thus had optimal level of savings would have no incentive to change their behavior after the expansion. Moreover, newly eligible low-income but high-asset households may not use the Medicaid program because of a "welfare stigma" (Moffitt, 1983). Depending on how these groups of households responded, the extent of the ACA Medicaid expansion effect would differ, and thus it is worthwhile to investigate empirically.

To analyze how the ACA Medicaid expansion affected household income, this article

[1] ***Abbreviations:*** **ACA**: Affordable Care Act, **AFDC**: Aid to Families with Dependent Children, **CD**: Certificate of Deposit, **CPS**: Current Population Survey, **DD**: Difference-in-Differences, **FPL**: Federal Poverty Level.

focuses on five different types of household income: (1) total income from all sources of receipts and gains; (2) labor income (i.e., wage and salary); (3) dividend income from stocks and mutual funds; (4) interest income from interest earning accounts, funds, savings bonds, T-notes, individual retirement accounts, certificates of deposit (CDs), or other investments; and (5) financial assistance from friends or relatives.[2] As the ACA Medicaid expansion covered low-income, childless adults and eliminated the asset-test, low-income households with no dependents living in the expansion states are expected to increase dividend and interest income from their savings and investments and to reduce financial assistance from other people after the expansion. Yet these households would not increase their total income after the expansion because the income-test for Medicaid eligibility still exists.

To identify the effects of the Medicaid expansion on household income under ACA, I use the difference-in-differences (DD) framework (i.e., pre- and post-Medicaid expansion, for expansion and non-expansion states). That is, I compare a treatment group of households with no dependent children and family income below 100% of federal poverty level (FPL) living in the expansion-adopted states with a control group of counterparts living in the states opting out of the expansion, before and after the expansion. Analyzing the March Current Population Survey (CPS) Supplement data from 2011 to 2016, I find that households in the treatment group significantly increased dividend and interest income by $63 and $84, respectively, after the expansion, compared with counterparts in the control group. At the same time, the households in the treatment group significantly reduced the financial assistance from relatives or friends by $159 after the expansion. In addition, due to the income-test, total or labor income for the treatment group households did not significantly increase after the expansion. Finally, the empirical

[2] Although the March Current Population Survey Supplement data include several other types of income, such as survivor income and child support, I do not include these inconsequential income categories in the empirical analysis.

findings suggest that the increases in dividend and interest income after the expansion are brought by both low-income and low-asset households and low-income but high-asset households, though to a greater extent by the latter group.

This article contributes to the literature in two major ways. First, to the best of my knowledge, this article is the first to present the ACA Medicaid effects on savings and investments for low-income, childless adults. Furthermore, this article indirectly identifies that low-income and low-asset households increased savings and benefited from the ACA Medicaid expansion, thus meeting the key issue of the asset-test elimination. The ACA provision of health insurance for childless adults is unprecedented in Medicaid history, and so it is crucial to correctly understand how low-income and low-asset households changed their savings and investment decisions in response to the expansion because they generally had a low ability to self-insure against medical expenditure risk. According to the 2014 Medical Expenditure Panel Survey data, approximately 15% of non-elderly households with income below 100% of FPL with medical expenses had difficulty in paying medical bills and 70% were paying off medical bills over time. Yet existing research is limited in scope, investigating the ACA Medicaid expansion on labor supply decisions or health-related outcomes (Courtemanche et al., 2017; Frean et al., 2016; Gooptu et al., 2016).

Second, to examine the Medicaid effects on household financial decisions, this article focuses on unearned income, rather than just the level of savings, with the income-test in mind. Although low-income households do not face the asset-test for Medicaid eligibility after the expansion, the income-test is still in place for its eligibility. Prior studies (Greenhalgh-Stanley, 2012; Owens and Baum, 2012; Sullivan, 2006) examining the effects of the asset-test for welfare programs such as Aid to Families with Dependent Children (AFDC) or Medicaid long-term care

coverage have mainly focused on the level of savings or real-estate assets that generate little or no income (e.g., primary residential housing and vehicles). Unlike the asset-test exemption on housing or vehicles, households generate unearned income through an increase in savings and financial investment in response to an asset-test elimination, which consequentially is bound to the income-test cutoff. Accordingly, under the existence of the income-test for Medicaid eligibility, focusing on household unearned income more precisely captures the effects of the asset-test elimination on household saving and investment behavior.

The rest of this article proceeds as follows: Section II introduces the new changes in the Medicaid program by the ACA, reviews previous literature on the asset-test for eligibility of public welfare programs, and explains how the ACA Medicaid expansion with the asset-test elimination affected household income composition. Section III describes the March CPS Supplement data and presents the descriptive statistics of the sample. Section IV establishes the empirical strategy for identifying the effects of the ACA Medicaid expansion on household income. Section V provides the empirical results, and Section VI provides concluding remarks with a brief discussion of further research direction.

## II. Medicaid Expansion and Its Implications for Household Income

### A. *History of Medicaid and Its ACA Expansion in 2014*

Medicaid is the largest public health insurance program in the United States and has provided health insurance coverage for the non-elderly poor population for more than 40 years.[3] Medicaid is a means-tested program, administered by the state government, while the federal government

[3] I briefly summarize the history and eligibility rule changes of Medicaid in this section; further details of these are well summarized in the studies of Gruber (2003) and De Nardi et al. (2011).

provides matching funds for states.[4] Historically, Medicaid was enacted in 1965 under Title XIX of the Social Security Amendments, and state governments implemented their Medicaid programs between 1966 and 1970. At the time of introduction, Medicaid eligibility was closely tied to welfare receipts through programs such as AFDC and Supplemental Security Income. Single-parent families and the aged, blind, deaf, or disabled were the main populations covered by Medicaid until the mid-1980s.

Since 1984, the Medicaid program has expanded its eligibility for pregnant women and children. Specifically, with the Deficit Reduction Act of 1984, Medicaid eliminated the categorical test for certain pregnant women and children. In 1986, the Omnibus Budget Reconciliation Act allowed for the coverage of pregnant women and infants under the age of two with income up to 100% of FPL. A decoupling of the Medicaid program from AFDC began in 1987 through substantial increases in the income cutoff and child age cutoff for Medicaid eligibility. For example, the Omnibus Budget Reconciliation Act of 1989 mandated states to cover children under the age of six and all pregnant women with income up to 133% of FPL by 1992, and states were allowed to cover these populations up to 185% of FPL at their discretion. These laws permitted states to expand Medicaid programs on their own and thus generated substantial variation in Medicaid programs across the states. In 1997, the Balanced Budget Act created the Children's Health Insurance Program and permitted states to extend eligibility to children under the age of 19 with incomes above Medicaid limits, either through newer, more flexible state programs or through further expansions of Medicaid.

On March 2010, as one of the ACA provisions, the Medicaid program was scheduled to extend its eligibility, covering individuals and families with income up to 138% of FPL in all

[4] Federal matching rates vary across states depending on the level of Medicaid benefits and state per capita income.

states, beginning on January 1, 2014. In an effort to achieve universal health insurance coverage, the ACA Medicaid expansion allowed the coverage of non-elderly adults aged below 65 years without dependent children (i.e., childless adults), who historically had a relatively low rate of health insurance coverage.[5] Another important feature of the ACA Medicaid expansion forced states to eliminate the asset-test for Medicaid eligibility. To restrict abuse of the Medicaid program, the asset-test had been imposed for Medicaid eligibility, in addition to the income-test. Although states established different asset-test rules for Medicaid eligibility, households were required to hold assets valued at less than its threshold, with a modal value of $2,000, generally counting savings and financial assets in bank accounts along with real-estate assets, with exemptions on one vehicle and one primary resident home.[6]

Yet opponents of the ACA provisions have challenged the constitutionality of the legislation since the ACA passage on 2010. On June 28, 2012, the U.S. Supreme Court ruled in *National Federation of Independent Business v. Sebelius* (2012) that the significant Medicaid expansion exceeded the legitimate power of Congress and that the federal government must allow states to continue at pre-ACA levels of funding and eligibility if they so choose. As a result, the Supreme Court ruling left the Medicaid expansion to increase income cutoff up to 138% of FPL and to cover childless adults optional for each state. Yet all states were still required to eliminate the asset-test for Medicaid eligibility for the coverage of routine and preventive care from January 2014, regardless of whether the state adopted the expansion for childless adults with income up

[5] According to the March CPS Supplement data, the average uninsured rate for non-elderly adults with no dependent children and income below 100% of FPL was 70% before the ACA reform (i.e., years between 2010 and 2014), while it was 58% for low-income adults with dependent children.

[6] Before the ACA Medicaid expansion, many states eliminated the asset-test when determining Medicaid eligibility for children but were slower to eliminate or relax the asset-test requirement for low-income parents. According to the report of the Center for Medicare and Medicaid services, some states that relaxed the asset-test before 2014 still do not exempt certain crucial asset classes, such as savings in individual retirement accounts for the Medicaid asset-test.

to 138% of FPL.[7]

As a result of the optional ruling of the Medicaid expansion, states have made different decisions about extending its eligibility to childless non-elderly adults. Table 1 illustrates each state's adoption status of the Medicaid expansion and the specific implementation date for the expansion. According to the information from the Kaiser Family Foundation, 25 states, including Washington, DC, chose to expand as of January 1, 2014. Adults with no dependent children and family income up to 100% of FPL living in these 25 states serve as the treatment group. Childless adults with income between 100% and 138% of FPL living in these 25 states are not included in the sample because their counterparts living in the non-expansion states are also affected by the ACA reform in that they receive the federal government premium subsidy, and thus they cannot be used as a pure control group. Among the 25 states, 13 states experienced limited prior expansion of the Medicaid program, and five states had comprehensive prior expansion similar to the ACA provisions. For example, California and Connecticut expanded their Medicaid eligibility to childless adults earlier than 2014, but their prior expansion was limited in that California eliminated the asset-test after January 2014 and Colorado capped its program at 10,000 in 2012. In the empirical analysis, I incorporate this heterogeneity of the prior-expansion status across states. The rest of 26 states did not opt into Medicaid expansion to childless adults as of January 2014, and thus those childless adults with income below 100% of FPL living in non-expansion states serve as the control group. Among them, seven states (Alaska, Indiana, Louisiana, Michigan, Montana, New Hampshire, and Pennsylvania) expanded later than January 2014. As such, these states are dropped from the analysis for the post-2014 period.[8]

[7] Both the asset-test and real-estate recovery for Medicaid coverage for long-term institutional care (e.g., nursing home) remain after the ACA expansion.

[8] The main results still hold when these seven states with their late post-treatment date are included in the analysis.

[Insert Table 1 here]

### B. *How the Medicaid Expansion Affects Household Income*

When the ACA Medicaid expansion allowed for the coverage of low-income, childless adults and eliminated the asset-test for its eligibility, it affected the savings and financial investment decisions of those households in various ways.[9] First, low-income households bound to an asset-test threshold before the expansion would increase savings in bank accounts and financial assets such as stocks and bonds after the 2014 expansion. Thus, these households are expected to increase the amount of dividend income from stocks and mutual funds as well as interest income from savings, CDs, bonds, and other investment after the expansion. In addition, because the pre-existing asset-test made exemptions on real-estate assets, if these households held primary residences or vehicles greater than its optimal, they would reallocate their real-estate assets to financial assets after the expansion.[10] As these low-income households are still affected by the income-test for Medicaid eligibility, their total household income would not change after the expansion in 2014. Second, low-income and low-asset households that did not have a binding condition of the asset-test cutoff before the expansion would keep the same amount of unearned income after the expansion. Because these households originally held the optimal level of savings and financial portfolio, they would have no incentive to adjust their savings and investments in response to the ACA Medicaid expansion. Third, low-income but high-asset households that were not eligible for Medicaid before but become eligible after the expansion could affect the average amount of household unearned income in the post-reform period. If

[9] Although low-income households are financially illiterate relative to high-income and well-educated households (Lusardi and Mitchell, 2011), I assume that they respond to the Medicaid expansion rationally.

[10] Since the March CPS Supplement data do not include the market value of real-estate assets, it is not feasible to test whether these households reduced their proportion of real-estate assets after the expansion.

these households join the Medicaid program, income from financial assets in the treatment group would be higher after the expansion. Conversely, if these newly eligible households did not use the Medicaid program because of a welfare stigma (Moffitt, 1983), they would not contribute to the higher level of unearned income in the treatment group after the expansion. In summary, depending on how these groups of households responded to the ACA Medicaid expansion, the extent of the expansion effects on household income would differ. Considering the Medicaid coverage for childless adults and asset-test elimination under the ACA, I test the following hypotheses:

**Hypothesis 1.** The ACA Medicaid expansion in 2014 increased dividend income for low-income households without dependent children (i.e., $\alpha_1 > 0$).

**Hypothesis 2.** The ACA Medicaid expansion in 2014 increased interest income for low-income households without dependent children (i.e., $\alpha_1 > 0$).

Moreover, I examine whether these households reduced financial assistance from relatives and friends after the 2014 expansion. As low-income households without dependent children were not eligible for Medicaid before the expansion, they had a low ability to self-insure against medical expenditure risk and tended to rely on financial assistance from relatives and friends. With these childless adults becoming entitled to Medicaid after the expansion, they should reduce their financial assistance from other people.

**Hypothesis 3.** The ACA Medicaid expansion in 2014 decreased financial assistance for low-income households without dependent children (i.e., $\alpha_1 < 0$).

Last, I investigate how the Medicaid expansion affected total and labor income for these households. Although the Medicaid eligibility rule eliminated the asset-test after 2014, it still maintains the income-test. Thus, the ACA Medicaid expansion would not induce these low-

income households without dependents to increase their total income substantially. In addition, because many studies (Dague et al., 2014; Frisvold and Jung, 2016; Gooptu et al., 2016; Kaestner et al., 2015) investigate how the ACA Medicaid affected labor supply for low-income households without dependent children, I focus on these households' labor income as well. With previous studies finding little or no evidence of the effect of Medicaid expansion on labor supply for low-income, childless households, I test the following two non-directional hypotheses:

**Hypothesis 4.** The ACA Medicaid expansion in 2014 did not affect total income for low-income households without dependent children (i.e., $\alpha_1 = 0$).

**Hypothesis 5.** The ACA Medicaid expansion in 2014 did not affect labor income for low-income households without dependent children (i.e., $\alpha_1 = 0$).

### C. *Previous Literature*

Regarding asset-tests of welfare programs, prior studies have primarily focused on single mothers relying on AFDC or the elderly using long-term care (e.g., nursing home) coverage by Medicaid. First, Hubbard et al. (1995) demonstrate theoretically that social insurance programs with a means-test based on income and assets discourage savings for households with a low level of lifetime income. Regarding the asset-test effects of AFDC on savings for single mothers, the empirical evidence is mixed. Powers (1998) examined the impact of AFDC's asset-test on female-headed households' saving behavior and suggested that a $1 additional increase in the AFDC asset-test threshold leads to an increase in savings of approximately $0.25. In contrast, Hurst and Ziliak (2006) reported that changes in asset restrictions of AFDC have no measurable effect on changes in liquid assets for female-headed households with children. Similarly, Sullivan (2006) examined whether the AFDC asset-test affected the asset holdings of low-

educated single mothers. He showed that vehicle exemptions have an important effect on vehicle assets but found little evidence that asset limits have an effect on the amount of liquid assets that single mothers hold. Owens and Baum (2012) examined the effects of the increase in the limit for vehicle asset exemptions under the 1996 welfare reform on household vehicle assets and found that liberalizing asset rules increases vehicle assets.

Second, regarding the Medicaid program, prior studies have examined the effects of its asset-test on the elderly behavior of "spending-down" assets for Medicaid eligibility or the effects of real-estate recovery for long-term care coverage by Medicaid on savings and housing assets for the elderly. The empirical evidence on the extent to which the elderly transfer assets or adjust real-estate assets for Medicaid eligibility is mixed. Brown et al. (2007) estimated that a $10,000 increase in the level of assets a household can retain while qualifying for Medicaid coverage of long-term care expenditures would crowd out a 1.1 percentage point in private long-term care insurance coverage. Greenhalgh-Stanley (2012, 2015) investigated the effects of real-estate recovery and spousal protection laws for long-term care provided by Medicaid on elderly housing assets and other portfolio items and found that the estate recovery program of Medicaid makes the elderly decrease home equity and homeownership.[11]

In contrast, other studies have not found evidence that the elderly transferred or spent down their assets in response to the asset-test or real-estate recovery laws. Norton (1995) argued that the actual time of spending-down assets would be much longer than a predicted time from a base model with no behavioral effects. This is because the "welfare-averse" elderly try to avoid Medicaid eligibility by receiving transfers from relatives. De Nardi et al. (2010) analyzed a life-cycle model of saving on single, retired elderly people and suggested that the minimum

[11] For the effects of the Medicaid asset-test on savings of the non-elderly households, Gruber and Yelowitz (1999) also found that Medicaid eligibility exerts a sizable and significant negative effect on wealth holdings and is positively associated with consumption expenditures.

consumption level ("consumption floor") guaranteed by Medicaid and other public welfare programs causes those people to accumulate assets to self-insure. Gardner and Gilleskie (2012) examined a dynamic empirical model of health insurance coverage, long-term care arrangement, and asset and gift behavior for the elderly over time. Their long-term simulation results suggest that Medicaid eligibility and the generosity policy associated with nursing home services have no effect on Medicaid enrollment and asset transfer behavior.

Last, regarding the Medicaid expansion under ACA, most of the recent studies have examined its effects on labor supply (Frean et al., 2016; Gooptu et al., 2016; Janicki, 2014; Kaestner et al., 2015; Levy et al., 2016b), health insurance coverage (Courtemanche et al., 2017; Frean et al., 2016; Levy et al., 2016a), or heath conditions and behavior (Na and Slusky, 2016; Simon et al., 2016) for low-income, childless adults. They found that the Medicaid expansion had little or no effects on labor supply but significantly increased health insurance coverage and improved health conditions for childless adults. To my knowledge, only one recent study (Hu et al., 2016) has examined the effects of the ACA Medicaid expansion on household financial well-being, finding that the Medicaid expansion significantly reduced the number of unpaid bills and the amount of debt sent to third-party collection agencies among low income, uninsured individuals.[12]

## III. Data

To examine how the ACA Medicaid expansion affected household income, I use the 2011–2016 March CPS Supplement data, which covers the periods before and after the Medicaid expansion under the ACA. The CPS is a representative sample of the nationwide U.S. population

[12] Regarding the other ACA provisions (e.g., the dependent coverage mandate), previous studies have also investigated effects on health conditions, medical care utilization, labor supply, and savings of young adults (Akosa Antwi et al., 2015; Barbaresco et al., 2015; Depew, 2015; Depew and Bailey, 2015; Lee, 2016a, 2016b).

surveyed and provides the primary source of the monthly labor force status of the population. In addition, the CPS Supplement collects data for a variety of economic and social well-being studies on the entire U.S. population and specific population subsets. Notably, the March CPS Supplement data provide detailed information of health insurance coverage and household income. As the March CPS Supplement data asked survey respondent to provide income in the previous year, the sample covers the calendar years from 2010 to 2015.[13] A limitation of investigating the Medicaid expansion effect on household income with the March CPS Supplement data is that I am unable to control for unobservable household characteristics that might systemically affect the certain type of income that households prefer to hold because the March CPS Supplement data are not of a panel structure.

The study sample includes heads of households aged 26 to 55 years with no dependent children. Since October 2010, the ACA has mandated that private health insurance companies provide health insurance for employees' dependent children aged less than 26 years, and thus I exclude young adults aged up to 25 years from the sample. In addition, because the incidence of health problems (e.g., stroke, cancer, heart disease) and medical expenditures for individuals aged above 55 years increased sharply (Gruber and Madrian, 1996) and thus their savings and labor supply decisions are systematically different from younger households, I exclude households whose head is over 55 years. In the "robustness check" section, I also vary the different age cutoff levels and confirm that the main empirical results still qualitatively hold.

In addition, I restrict the sample to households with income below 100% of FPL. Although households with FPLs between 100% and 138% living in the expansion states are eligible for

[13] In 2014, the CPS provided the additional data regarding the questionnaires of health insurance coverage. The traditional data asked the status of respondents' health insurance coverage *in the previous year*. In 2014, the additional data asked the health insurance coverage for the respondent and households *at the time of interview*. I used the traditional March CPS Supplement data for the main analysis. The main estimation results still hold by including the additional 2014 data.

Medicaid, I do not include these households in the sample. As the counterparts living in the non-expansion states are affected by the ACA reform as well, in the sense that they receive the federal government premium subsidy for health insurance purchase through the health insurance marketplace (known as "health exchanges"), these households cannot be a pure control group in the analysis. Among the childless adults with income below 100% of FPL, I divide these households into two groups on the basis of state of residence. Households living in the Medicaid-expansion states are in the treatment group, with those living in states without expansion in the control group, as childless adults living in the non-expansion states are still not eligible for Medicaid after the ACA expansion.

Table 2 shows the summary statistics of the sample. All the statistics and estimates given herein are weighted by the March CPS Supplement household sampling weights. The average total income for the households in the treatment group before the 2014 expansion was $7,960 and $8,543 for the households in the control group. All the monetary values in the analysis are adjusted in 2010 U.S. dollars. After 2014, the total household income decreased overall to $7,562 for households in the treatment group and $7,928 for households in the control group. The annual dividend income from stocks and mutual funds was $18 on average in the treatment group before 2014 and increased to $36 after the Medicaid expansion. In contrast, the annual dividend income for households in the control group was $65 and decreased to $27 after 2014. Similarly, the average amount of interest income from bank accounts, CDs, bonds, and other investments increased for households in the treatment group and decreased in the control group after the expansion. Last, the annual amount of household financial assistance from relatives or friends decreased from $202 to $46 after 2014 for households in the treatment group but increased from $76 to $112 for those in the control group.

[Insert Table 2 here]

Figure 1 illustrates the unconditional patterns of the amount of total, labor, dividend, interest, and financial assistance income before and after the Medicaid expansion. As Panel A of Figure 1 shows, total household income increased after the expansion for households in the treatment group, while there was no increasing pattern in the amount of total income for households in the control group. Interest income displays increasing patterns after the expansion for households in both treatment and control groups (shown in Panel D). In contrast, financial assistance income decreased after the expansion for households in both groups (shown in Panel E). To verify equality of the pre-reform trends between treatment and control groups for each type of income, I conducted a formal statistical test and found no statistical difference in pre-trends between the treatment and control groups for each income.[14]

[Insert Figure 1 here]

## IV. Econometric Framework

To analyze the Medicaid expansion effects on household income more precisely, I use a standard DD framework. For the identification strategy, I compare households with no dependent children and income below 100% of FPL living in the Medicaid-expansion states with those living in the non-expansion states before and after the expansion in 2014. Because this DD identification strategy might simply display the difference in dynamics of household income for childless adults between the treatment and control groups during the sample period (i.e., *placebo* effects), I conduct a series of placebo tests in the "robustness check" section and confirm that the DD framework helps precisely identify the ACA Medicaid expansion effects on household income portfolios.

[14] Details of the test procedures and results are in Section V.

In addition, there is a potential concern that using household residential states might not be appropriate for the identification strategy because low-income households without dependent children that need Medicaid benefits are more likely to move into the states adopting the Medicaid expansion. To check this potential endogeneity of the residential states, I examine whether a significant increase in migration occurred after 2014 for households with income below 100% of FPL and no dependent children, compared with their counterparts with dependent children. The estimation result in the "robustness check" section suggests that the Medicaid expansion did not significantly induce the low-income, childless households to move into Medicaid-expansion states; this result is consistent with the findings of Schwartz and Sommers (2014). Thus, the DD identification framework using the residential states is pertinent to estimate the ACA Medicaid expansion effects on household income.

Under the DD framework, I estimate the following model:

$$(1) \quad y_{i,s,t} = \alpha_1 I(Exp.States) \cdot I(Post\ 2014) + \alpha_2 I(Exp.States) + \alpha_3 I(Post\ 2014) + X'_{i,s,t}\alpha_4 + T'_t\alpha_5 + \vartheta'_s\alpha_6 + \varepsilon_{i,s,t},$$

where $y_{i,s,t}$ is the annual amount of a certain income (i.e., total, labor, dividend, interest, or financial assistance) for a household *i* living in state $s$ at time $t$; $I(Exp.States)$ is an indicator for households living in the Medicaid-expansion states; $I(Post\ 2014)$ is an indicator for the post-treatment period (i.e., since January 2014); $X_{i,s,t}$ is a vector of heads of households' demographic characteristics that possibly affect households' income portfolio, including education, age, square of age, race, sex, and marital status[15]; $T_t$ is the vector of year dummies; and $\vartheta_s$ controls for state fixed effects, which reflect differences in state Medicaid rules before the ACA expansion, and thus standard errors are clustered at the state level (Akosa Antwi et al.

[15] The variable of age square controls for potential non-linearity in the relationship between unearned income portfolio and household characteristics (Faig and Shum 2002; Shum and Faig 2006).

2013).[16] The parameter of interest is $\alpha_1$, which measures the average impact of the ACA Medicaid expansion on household income. For ease of interpretation and computation of marginal effects of interacted variables in the model with clustered standard errors, I use the linear model as a baseline. In addition, I use the type-I Tobit model for the Medicaid expansion effect on labor, dividend, interest, and financial assistance income because the values of the dependent variable are censored at zero. For example, when households do not hold any stocks, their dividend income is censored at zero.

## V. Empirical Results

### A. *Main Results*

Before examining the Medicaid expansion effect on household income, I investigate how the Medicaid expansion affected health insurance coverage and health conditions for low-income, childless households. I estimate equation (1) with the dependent variables of household health insurance coverage and health conditions. Table 3 provides the estimates for the ACA Medicaid expansion effects on the probability of being covered by Medicaid. The Probit estimates in column (2) of Table 3 suggest that the Medicaid expansion significantly increased the Medicaid coverage for households with no dependent children and income below 100% of FPL ($\alpha_1 = 0.266, p < 0.01$). Column (3), which presents the marginal effect of the Probit model, shows that the result still qualitatively holds ($\alpha_1 = 0.084, p < 0.05$).[17] These findings are consistent with those from other studies (Courtemanche et al., 2017; Frean et al., 2016; Levy et al., 2016a). Yet, as column (5) shows, the Medicaid expansion did not significantly improve the average

[16] I also estimate equation (1) with the clustering standard errors at the treatment group and year level in the "robustness" check section.

[17] I also estimate correcting the magnitude and standard errors of the interaction effect in a non-linear model with clustered standard errors (Ai and Norton, 2003). The results are still consistent with the baseline model ($\alpha_1 = 0.122, p < 0.01$).

health conditions for those households ($\alpha_1 = 0.018$).

[Insert Table 3 here]

Table 4 presents the main estimates for the ACA Medicaid expansion effects on household income. In column (1), the coefficient for the interaction term between the treatment states and the post-expansion period is positive but statistically non-significant ($\alpha_1 = 172.82$), in support of Hypothesis 4. That is, the ACA Medicaid expansion did not significantly increase total income for households with no dependent children and income below 100% of FPL living in the expansion states, because the Medicaid eligibility rule still limits the amount of household adjusted gross income through the income-test. Similarly, as column (2) shows, labor income for these households increased after the Medicaid expansion but is not statistically significant ($\alpha_1 = 277.74$), confirming Hypothesis 5.

[Insert Table 4 here]

Regarding unearned income, the estimates in column (3) of Table 4 suggest that the ACA Medicaid expansion significantly increased the annual dividend income for households with no dependent children and income below 100% of FPL living in the expansion states by \$63 ($\alpha_1 = 62.89, p < 0.10$). As some of the households do not hold stocks and thus have zero dollars of dividend income, I also estimate equation (1) with the Tobit specification; the estimate in column (7) is consistent with that from the linear baseline model (i.e., the Medicaid expansion significantly increased the dividend income for those households by \$58; $\alpha_1 = 58.13, p < 0.01$). In addition, the estimates in column (4) suggest that these households significantly increased interest income after the expansion by \$86 ($\alpha_1 = 86.35, p < 0.01$). These empirical results provide support for Hypotheses 1 and 2.

In accordance with the increase in unearned income of dividends and interest, these

households significantly reduced the amount of financial assistance from relatives and friends after the Medicaid expansion. The estimates in column (5) of Table 4 show that the coefficient is negative and statistically significant for the amount of financial assistance ($\alpha_1 = -159.27, p < 0.05$), in support of Hypothesis 3. The Tobit estimate for the financial assistance is also significantly negative ($\alpha_1 = -106.45, p < 0.05$; column (9)). In summary, these empirical results imply that the ACA Medicaid expansion with asset-test elimination had a positive effect on financial investment and savings and reduced financial dependency for low-income households.

One caveat for these estimation results is that it is unclear whether the increase in dividend and interest income after the Medicaid expansion resulted from behavioral changes by (1) low-income and low-asset households that were originally eligible for Medicaid before the expansion increased their savings and financial assets, (2) low-income but high-asset households that joined the Medicaid program after the expansion and thus contributed to the higher level of dividend and interest income in the treatment group, or (3) both. Because the March CPS Supplement data do not track the same individuals over time, it is not feasible to answer this question directly in the dynamic framework. To identify which group of low-income households (low-asset vs. high-asset) contributed to the main estimates, I exploited the variable of "how many months during the previous year the respondent was covered by Medicaid" from the data. It is more likely that low-income households that were originally eligible for Medicaid (i.e., low-asset households) used Medicaid benefits during the entire 12 months in the previous year. Thus, using the information of this benefit period, I divide low-income households into two groups for the post-expansion period. I denote the households covered by Medicaid for the entire 12 months in the past year as the low-asset households and those that did not receive the full 12-month Medicaid

benefits consecutively as high-asset households (i.e., new entrants to Medicaid after the expansion). Then, I estimate equation (1) separately for each group of low-income households.

Table 5 reports the estimation results. For dividend income, the estimates in column (3) suggest that households defined as original Medicaid beneficiaries living in the Medicaid-expansion states significantly increased dividend income by approximately $53 after the expansion ($\alpha_1 = 52.80, p < 0.05$). Although the increase in dividend income for households denoted as new entrants is $75, which is greater than that of the first group, it is not statistically significant ($\alpha_1 = 74.77$; column (8)). Interest income for low-income households in both groups significantly increased after the expansion ($\alpha_1 = 89.07, p < 0.01$; column (4); $\alpha_1 = 104.39, p < 0.01$; column (9)), but to a greater extent for the new entrant group. For the level of financial assistance from other people, low-income households in both groups also saw a significant reduction after the expansion ($\alpha_1 = -118.78, p < 0.10$; column (5); $\alpha_1 = -171.75, p < 0.10$; column (10)). In summary, these results imply that the policy-targeted group (i.e., households with low-income and low-assets) benefited from the asset-test elimination under the ACA Medicaid expansion, albeit to a lesser degree than the low-income but high-asset households.

[Insert Table 5 here]

### B. *Robustness Checks*

First, to affirm that using residential states as the identification strategy is appropriate, I investigate whether households with no dependent children and income below 100% of FPL were more likely to move into the expansion states after 2014 than those with dependent children and income below 100% of FPL. The estimation result in column (1) of Table 6 suggests that the

low-income households with no dependents did not significantly move in or out more frequently after the 2014 expansion than their counterparts with dependents ($\alpha_1 = 0.089$). In addition, the results in column (2) suggest that, among the low-income households that moved, the low-income households with no dependents did not significantly move into the Medicaid-expansion states after 2014 ($\alpha_1 = 0.034$). Furthermore, I investigate whether the residential states are endogenous by comparing households with no dependents and income below 100% of FPL on the basis of their health conditions. Among the low-income, childless households, those suffering from poor health conditions should be more likely than healthy households to move into the expansion states to take advantage of Medicaid benefits. The estimation results in columns (3) and (4) suggest that the Medicaid expansion did not significantly induce less-healthy households to move into the Medicaid-expansion states ($\alpha_1 = -0.046$; $\alpha_1 = -0.019$, respectively). In summary, these results are consistent with the findings of Schwartz and Sommers (2014) and certify that using the residential states as the identification strategy is appropriate to identify the Medicaid expansion effects.

[Insert Table 6 here]

In addition to the main estimates with equation (1), I further split the treatment group into three parts depending on whether the state experienced *no*, *limited,* or *comprehensive* expansion before the ACA Medicaid reform. First, I replace the variable $I(Exp.States)$ in equation (1) with the three treatment indicators and estimate the following model:

$$
\begin{aligned}
(2) \qquad y_{i,s,t} = {} & \beta_1 I(Exp.States_{NO}) \cdot I(Post\ 2014) + \beta_2 I(Exp.States_{LIM}) \cdot I(Post\ 2014) \\
& + \beta_3 I(Exp.States_{COM}) \cdot I(Post\ 2014) + \beta_4 I(Exp.States_{NO}) \\
& + \beta_5 I(Exp.States_{LIM}) + \beta_6 I(Exp.States_{COM}) + \beta_7 I(Post\ 2014) + X'_{i,s,t}\beta_8 \\
& + T'_t\beta_9 + \vartheta'_s\beta_{10} + \varepsilon_{i,s,t},
\end{aligned}
$$

where $I(Exp.States_{NO})$ is an indicator for households living in the ACA Medicaid-expansion states with no prior reform. The other two indicator variables for the prior-expansion status, $I(Exp.States_{LIM})$ and $I(Exp.States_{COM})$, are defined similarly, and all other variables are defined as in equation (1).

Table 7 presents the estimates for the ACA Medicaid expansion effects on household income with a three-treatment group specification. In general, the estimation results suggest that the effects of the ACA Medicaid expansion on household unearned income are greatest for low-income, childless households living in the states with *no* prior expansion. First, regarding dividend income, the Medicaid expansion effect is positive and statistically significant only for low-income households living in the states with no prior expansion ($\beta_1 = 136.13, p < 0.05$; column (3)). Second, the estimates in column (4) suggest that, regardless of whether states had some degree of expansion before 2014, the ACA Medicaid expansion in 2014 significantly increased interest income for households in the treatment groups ($\beta_1 = 130.32, p < 0.05;\ \beta_2 = 72.27, p < 0.05;\ \beta_3 = 78.47, p < 0.01$), but to the greatest extent for the no prior-expansion states. Third, households without dependent children and income below 100% of FPL living in states with limited prior expansion significantly reduced the amount of financial assistance received from relatives or friends after the ACA expansion in 2014 ($\beta_2 = -184.87, p < 0.05$; column (5)). Furthermore, I merged the two treatment groups of limited and comprehensive prior expansion into one. As a result, there are two treatment groups depending on whether the states *adopted* or *did not adopt* expansion before the ACA reform. The estimation results with the two treatment groups presented in Table 8 are consistent with the results using a three-treatment-group specification in Table 7.

[Insert Tables 7 and 8 here]

Second, because the ACA Medicaid expansion might affect household income differently across various households, I test the heterogeneity impacts of the Medicaid expansion on household income for different sub-groups by race (white vs. non-white), gender (male vs. female), and marital status (married vs. unmarried). For example, because men are less risk averse than women (Barber and Odean, 2001; Jianakoplos and Bernasek, 1998), the impact of the ACA Medicaid expansion on household income for male heads of households might be greater than that for their female counterparts. Table 9 presents the estimation results of heterogeneity effects on household income. Regarding interest income, the estimates in Panel D show that male ($\alpha_1 = 147.88, p < 0.05$), white ($\alpha_1 = 141.0, p < 0.05$), married ($\alpha_1 = 85.0, p < 0.10$), and unmarried ($\alpha_1 = 68.0, p < 0.10$) heads of households significantly increased interest income after the expansion. Yet there are no statistically significant differences in the effects of Medicaid expansion on interest income between male and female, between white and non-white, or between married and unmarried heads of households. The significant heterogeneity impacts across the sub-groups appear in labor income (Panel B) and financial assistance (Panel E). Female heads of households significantly increased labor income compared with male heads of households ($\Delta\alpha_1 = 2022.04, p < 0.10$), and white heads of households significantly reduced the financial assistance from other people after the expansion compared with non-white heads of households ($\Delta\alpha_1 = -337.88, p < 0.10$).

[Insert Table 9 here]

Third, I examine whether the household income patterns between households in the treatment and control groups are similar in the pre-reform period. If income patterns for households in the two groups differed in the pre-reform period, the main estimation results would merely exhibit differences in income patterns between the two groups, not the ACA Medicaid expansion effects.

Exploiting the March CPS Supplement data from 2011 to 2014 (covering the pre-reform periods), I analyze a model with the same specification of equation (1) by replacing the indicator for the post-2014 period with the linear year trends. The estimation results in Table 10 show that there are no significant disparities in patterns for all five types of household income between the treatment and control groups before the ACA Medicaid expansion.

[Insert Table 10 here]

Fourth, because the effects of Medicaid expansion on household income decisions might be attributed to dynamics in household income structures across different households over time, I also run a series of placebo tests by setting artificial reform periods. I replace the indicator for the post-ACA period, $I(Post\ 2014)$, in equation (1) with an indicator for a placebo date by falsely assuming that the asset-test elimination occurred on different years before 2014. Specifically, I re-estimate equation (1) for each of the three placebo years between the calendar years of 2011 and 2013. The estimates for placebo dates appear in Table 11. All the placebo test results except the placebo year of 2011 for total income indicate no statistical significance in the coefficients for the interaction term of indicators between the treatment states and placebo date. These results confirm that the main estimates for unearned income and financial assistance stemmed not from the dynamics in financial asset investment across different households but from the Medicaid expansion with the asset-test elimination.[18]

[Insert Table 11 here]

Fifth, I re-estimate the baseline model for different age cutoffs for heads of households with the lower bounds from age 19 to 25 and upper bounds from age 56 to 60 years. These estimates

[18] In addition to these placebo tests with regards to time, I examine the placebo effects of the Medical expansion on unearned income by using the sample that is not targeted by the policy. That is, I reestimate equation (1) for low-income households with dependent children, and confirm that the Medicaid expansion neither increased unearned income nor reduced financial assistance from friend and relatives for those households who are not affected by the ACA Medicaid expansion.

appear in Table 12; the main results with the age cutoff of 26 to 55 years still qualitatively hold with the different age cutoffs. The estimates suggest that non-elderly households with income below 100% of FPL and no dependent children living in the expansion states significantly increased in the amount of dividend and interest income while financial assistance from other people fell after the Medicaid expansion.

[Insert Table 12 here]

Finally, I re-estimate equation (1) to investigate whether the main estimates from the DD framework are robust to the treatment-year-level clustering standard errors. Because a *t*-distribution is derived from a small number of treatment-year clusters (i.e., 4 clusters, and thus the degrees of freedom are equal to 3), the critical values used for the hypothesis tests are more conservative than those using state-clustered robust standard errors in the main analysis.[19] In Table 13, the estimation results with the treatment-year-clustered robust standard errors are qualitatively consistent with the main estimates.

[Insert Table 13 here]

## VI. Conclusions

This study examines how the ACA Medicaid expansion involving the elimination of the asset-test for Medicaid eligibility affected household income. For the identification strategy, I used the DD framework and divided households into treatment and control groups depending on whether households reside in states that adopted the Medicaid expansion under ACA. I compared total, labor, dividend, interest, and financial assistance income for households in the treatment and control groups before and after the expansion in 2014. I find that households with no

[19] The block bootstrap (Bertrand et al., 2004) or wild cluster bootstrap (Cameron et al., 2008) methods are not feasible because of the extremely small number of groups.

dependent children and income below 100% of FPL living in the Medicaid-expansion states significantly increased in the amount of dividend and interest income by $63 and $84, respectively, after the expansion. Notably, the increase in dividend and interest income is partially attributed to the increase in savings and financial investment by low-income and low-asset households. At the same time, these households reduced the amount of financial assistance from relatives and friends after the expansion by $159. In summary, these empirical findings suggest that the ACA Medicaid expansion with asset-test elimination increased the ability of low-income households to self-insure. In addition, the placebo tests suggest that the baseline DD framework is pertinent to precisely identify the Medicaid-expansion effects on household income. During his presidential election campaign in 2016, Donald J. Trump announced his intention to repeal or replace many key provisions of the ACA. Now that Trump is president, analyzing the effects of a potential future change or abolishment of the ACA Medicaid program on household income would be a fruitful future research direction.

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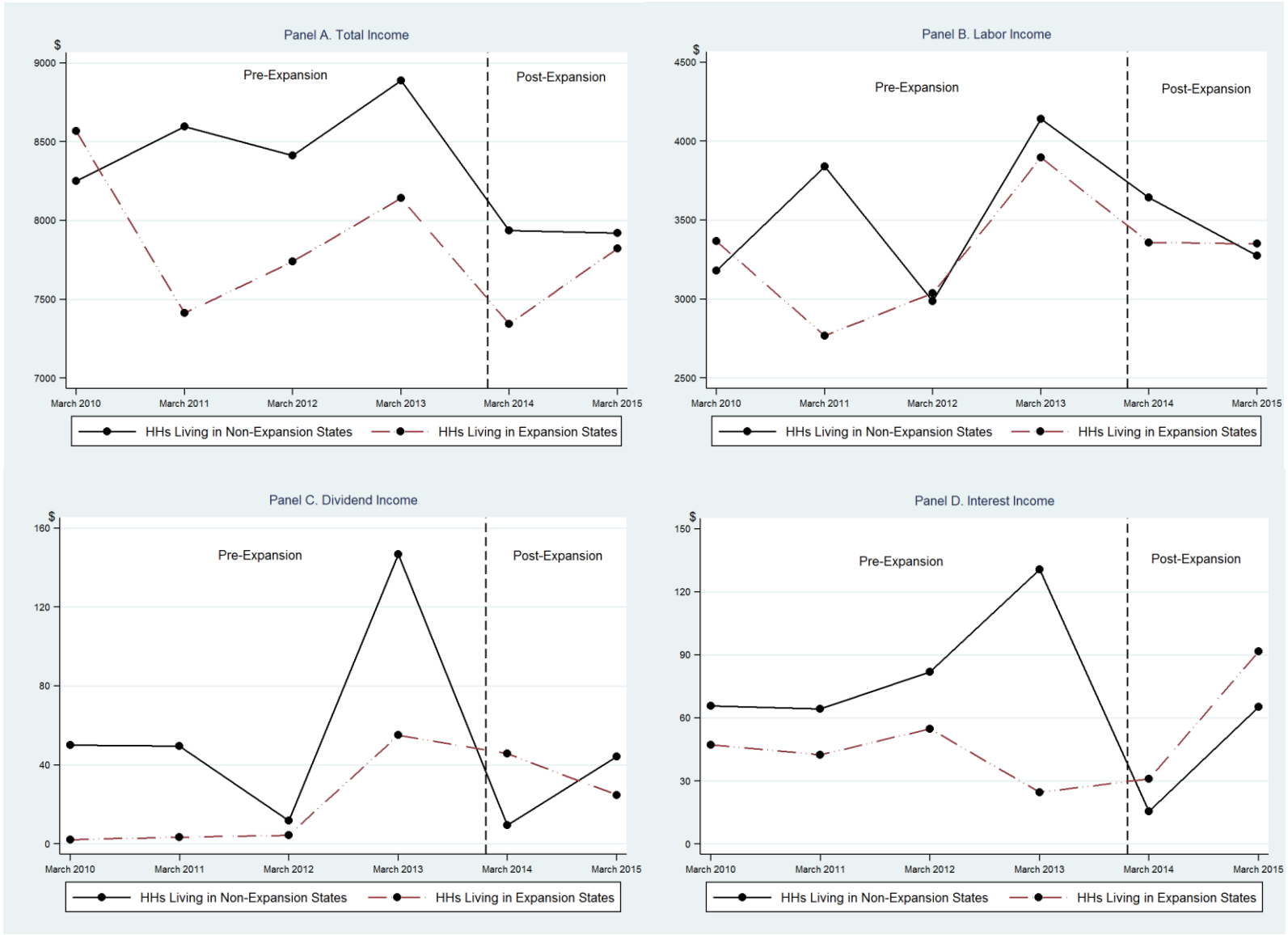


**FIGURE 1.** HOUSEHOLD INCOME PATTERNS BETWEEN TREATMENT AND CONTROL GROUP

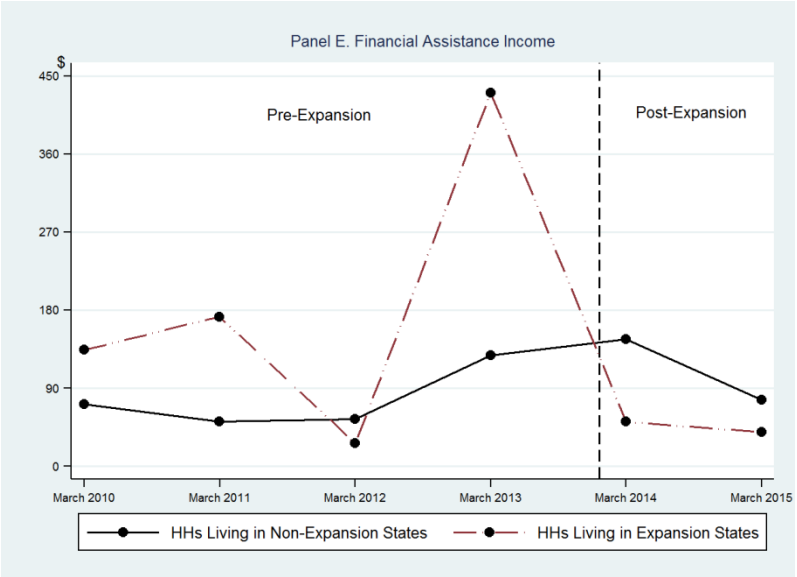


**FIGURE 1.** HOUSEHOLD INCOME PATTERNS BETWEEN TREATMENT AND CONTROL GROUP (CONTINUED)

Notes: Figure 1 illustrates the *unconditional* patterns of the amount of total, labor, dividend, interest, and financial assistance income before and after the Medicaid expansion. To verify equality of the *pre-reform trends* between treatment and control groups for each type of income, I conducted a formal statistical test and found *no statistical difference* in pre-trends between the treatment and control groups for each income.

TABLE 1—MEDICAID-EXPANSION STATUS BY STATES AS OF JANUARY 2014

| Control Group | | | Treatment Group | | | | | |
|---|---|---|---|---|---|---|---|---|
| Not Expanded | Expanded Later | | No Prior Expansion | | Limited Prior Expansion | | Comprehensive Prior Expansion | |
| Alabama | Alaska | 09/01/2015 | Arkansas | 01/01/2014 | Arizona | 01/01/2014 | Delaware | 01/01/2014 |
| Florida | Indiana | 02/01/2015 | Kentucky | 01/01/2014 | California | 07/01/2011 | Washington, DC | 07/01/2010 |
| Georgia | Louisiana | 07/01/2016 | Nevada | 01/01/2014 | Colorado | 01/01/2014 | Massachusetts | 01/01/2014 |
| Idaho | Michigan | 04/01/2014 | New Mexico | 01/01/2014 | Connecticut | 04/01/2010 | New York | 01/01/2014 |
| Kansas | Montana | 01/01/2016 | North Dakota | 01/01/2014 | Hawaii | 01/01/2014 | Vermont | 01/01/2014 |
| Maine | New Hampshire | 08/15/2014 | Ohio | 01/01/2014 | Illinois | 01/01/2014 | | |
| Mississippi | Pennsylvania | 01/01/2015 | West Virginia | 01/01/2014 | Iowa | 01/01/2014 | | |
| Missouri | | | | | Maryland | 01/01/2014 | | |
| Nebraska | | | | | Minnesota | 03/01/2011 | | |
| North Carolina | | | | | New Jersey | 04/14/2011 | | |
| Oklahoma | | | | | Oregon | 01/01/2014 | | |
| South Carolina | | | | | Rhode Island | 01/01/2014 | | |
| South Dakota | | | | | Washington | 01/03/2011 | | |
| Tennessee | | | | | | | | |
| Texas | | | | | | | | |
| Utah | | | | | | | | |
| Virginia | | | | | | | | |
| Wisconsin | | | | | | | | |
| Wyoming | | | | | | | | |

*Source*: Kaiser Family Foundation: Status of State Action on the Medicaid Expansion Decision http://kff.org/medicaid/state-indicator/state-activity-around-expanding-medicaid-under-the-affordable-care-act/; Kaiser Family Foundation: States getting a jump start on health reform's Medicaid expansion. http://kff.org/health-reform/issue-brief/states-getting-a-jump-start-on-health/.

TABLE 2—SUMMARY STATISTICS: MARCH CPS SUPPLEMENT DATA FROM 2011 TO 2016

| | All | | | Pre-2014 | | | | | | Post2014 | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | | | | Expansion States | | | Non-Expansion States | | | Expansion States | | | Non-Expansion States | | |
| | Mean | Min | Max | Mean | Min | Max | Mean | Min | Max | Mean | Min | Max | Mean | Min | Max |
| Total income | 8100.47 | -16149.20 | 36942.0 | 7959.99 | -16149.20 | 26885.0 | 8542.93 | -9999.0 | 36942.0 | 7562.28 | -9199.08 | 27784.0 | 7928.47 | -14685.04 | 30823.68 |
| Labor income | 3413.47 | 0 | 36848.0 | 3290.26 | 0 | 26600.0 | 3547.30 | 0 | 36848.0 | 3354.72 | 0 | 25760.0 | 3459.43 | 0 | 23000.0 |
| Dividend income | 38.78 | 0 | 13800.0 | 18.11 | 0 | 7600.0 | 65.03 | 0 | 9700.0 | 36.12 | 0 | 4600.0 | 26.64 | 0 | 13800.0 |
| Interest income | 59.94 | 0 | 10000.0 | 41.44 | 0 | 3538.56 | 86.05 | 0 | 10000.0 | 58.72 | 0 | 5387.52 | 40.28 | 0 | 3335.0 |
| Financial assistance | 120.96 | 0 | 18400.0 | 201.95 | 0 | 12000.0 | 76.49 | 0 | 9400.0 | 45.69 | 0 | 5520.0 | 111.55 | 0 | 18400.0 |
| Age | 45.34 | 26 | 55.0 | 45.09 | 26 | 55.0 | 45.57 | 26 | 55.0 | 44.84 | 26 | 55.0 | 46.04 | 26 | 55.0 |
| Education | 12.22 | 0 | 21.0 | 12.32 | 0 | 21.0 | 12.04 | 0 | 21.0 | 12.33 | 0 | 21.0 | 12.29 | 2.5 | 21.0 |
| Sex (male=0, female=1) | 0.56 | 0 | 1 | 0.55 | 0 | 1 | 0.54 | 0 | 1 | 0.57 | 0 | 1 | 0.60 | 0 | 1 |
| Marital status (unmarried=0, married=1) | 0.58 | 0 | 1 | 0.58 | 0 | 1 | 0.57 | 0 | 1 | 0.58 | 0 | 1 | 0.59 | 0 | 1 |
| Race | | | | | | | | | | | | | | | |
| Hispanic | 0.20 | 0 | 1 | 0.22 | 0 | 1 | 0.15 | 0 | 1 | 0.24 | 0 | 1 | 0.22 | 0 | 1 |
| White | 0.50 | 0 | 1 | 0.50 | 0 | 1 | 0.50 | 0 | 1 | 0.53 | 0 | 1 | 0.47 | 0 | 1 |
| Black | 0.23 | 0 | 1 | 0.18 | 0 | 1 | 0.30 | 0 | 1 | 0.15 | 0 | 1 | 0.27 | 0 | 1 |
| Asian | 0.04 | 0 | 1 | 0.07 | 0 | 1 | 0.02 | 0 | 1 | 0.05 | 0 | 1 | 0.02 | 0 | 1 |
| Other | 0.03 | 0 | 1 | 0.02 | 0 | 1 | 0.03 | 0 | 1 | 0.03 | 0 | 1 | 0.02 | 0 | 1 |
| Number of households | 2691 | | | 1014 | | | 875 | | | 465 | | | 337 | | |

*Notes*: The estimates are from the March CPS Supplement data and the author's calculation. The data cover the calendar years between 2010 and 2015. All averages are weighted by the March CPS household sampling weights. All the monetary values are adjusted to thousands of 2010 US dollars.

TABLE 3—EFFECTS OF THE MEDICAID EXPANSION ON HEALTH INSURANCE COVERAGE AND HEALTH STATUS

| | Health Insurance Coverage | | | Health Status | | |
|---|---|---|---|---|---|---|
| | OLS | Probit | | OLS | Ordered Logit | Ordered Probit |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| I(Exp. States) × I(Post-2014) | 0.092*** | 0.266*** | 0.084** | 0.009 | 0.018 | 0.015 |
| | (0.031) | (0.087) | (0.038) | (0.089) | (0.173) | (0.093) |
| I(Exp. States) | 0.098*** | 0.999*** | 0.292*** | -0.168*** | -0.619*** | -0.311*** |
| | (0.029) | (0.049) | (0.019) | (0.056) | (0.089) | (0.049) |
| I(Post-2014) | 0.078** | 0.226** | 0.076** | 0.103 | 0.215 | 0.104 |
| | (0.033) | (0.093) | (0.036) | (0.092) | (0.185) | (0.094) |
| Age | 0.025 | 0.074 | 0.018 | -0.044* | -0.079* | -0.048* |
| | (0.019) | (0.056) | (0.017) | (0.024) | (0.048) | (0.026) |
| Age square | -0.000 | -0.001 | -0.000 | 0.000 | 0.000 | 0.000 |
| | (0.000) | (0.001) | (0.000) | (0.000) | (0.001) | (0.000) |
| Education | -0.017*** | -0.051*** | -0.019*** | 0.054*** | 0.098*** | 0.057*** |
| | (0.005) | (0.014) | (0.004) | (0.012) | (0.021) | (0.012) |
| Sex (male=0, female=1) | 0.069*** | 0.196*** | 0.077*** | 0.032 | 0.061 | 0.038 |
| | (0.025) | (0.070) | (0.024) | (0.050) | (0.093) | (0.053) |
| Marital status (unmarried=0, married=1) | -0.169*** | -0.476*** | -0.040** | -0.051 | -0.098 | -0.048 |
| | (0.022) | (0.063) | (0.018) | (0.052) | (0.087) | (0.054) |
| Hispanic | -0.087* | -0.231 | -0.138*** | 0.196*** | 0.333** | 0.206*** |
| | (0.050) | (0.142) | (0.040) | (0.073) | (0.131) | (0.076) |
| White | -0.147*** | -0.402*** | -0.113*** | -0.119 | -0.226 | -0.125 |
| | (0.034) | (0.092) | (0.028) | (0.088) | (0.160) | (0.089) |
| Asian | -0.153** | -0.462** | -0.161* | 0.290** | 0.507** | 0.317** |
| | (0.066) | (0.190) | (0.089) | (0.121) | (0.221) | (0.127) |
| Other | 0.002 | 0.024 | 0.020 | -0.135 | -0.295 | -0.143 |
| | (0.063) | (0.178) | (0.052) | (0.121) | (0.215) | (0.125) |
| $R^2$ | 0.159 | | | 0.124 | | |
| Pseudo-$R^2$ | | 0.125 | | | 0.026 | 0.027 |
| N | 2691 | 2691 | 2691 | 2691 | 2691 | 2691 |

*Notes*: All the estimates are weighted by the March CPS household sampling weights. State and time fixed effects are included in the estimation but not reported. State-clustered robust standard errors are in parentheses. Asterisks denote statistical significance at the 1% (***), 5% (**), and 10% (*) levels.

TABLE 4—EFFECTS OF THE MEDICAID EXPANSION ON HOUSEHOLD INCOME

| | Total | Labor | Dividend | Interest | Financial Assistance | Labor | Dividend | Interest | Financial Assistance |
|---|---|---|---|---|---|---|---|---|---|
| | OLS | | | | | Tobit | | | |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) |
| I(Exp. States) × I(Post-2014) | 172.816 | 277.742 | 62.886* | 86.349*** | -159.265** | 242.847 | 58.125*** | 71.034*** | -106.452*** |
| | (503.444) | (379.200) | (38.014) | (26.063) | (74.152) | (437.802) | (8.816) | (17.638) | (6.837) |
| I(Exp. States) | -308.127 | 928.721*** | -57.050** | -249.527*** | -4.573 | -54.621 | -495.302*** | -173.764*** | -1202.511*** |
| | (304.526) | (206.514) | (22.043) | (29.236) | (24.664) | (166.725) | (54.369) | (13.516) | (101.583) |
| I(Post-2014) | -530.367 | -215.204 | -25.835 | -30.544 | 20.391 | -263.563 | -39.721*** | 16.573 | 58.109*** |
| | (568.236) | (466.117) | (31.636) | (32.893) | (64.157) | (519.481) | (3.119) | (23.045) | (12.167) |
| Age | 392.695** | 10.095 | -12.831 | -23.287* | -55.447* | 64.539 | -26.794*** | -16.830* | -9.873*** |
| | (193.565) | (180.285) | (12.372) | (13.046) | (31.279) | (163.804) | (2.973) | (9.438) | (0.813) |
| Age square | -4.291* | -0.557 | 0.200 | 0.317* | 0.669* | -1.241 | 0.350*** | 0.224* | 0.123*** |
| | (2.186) | (2.036) | (0.166) | (0.166) | (0.364) | (1.898) | (0.039) | (0.116) | (0.011) |
| Education | -172.875*** | -74.699** | 17.175* | 11.344** | 9.204 | -36.500 | 16.965*** | 14.028*** | 6.181*** |
| | (54.263) | (33.499) | (9.566) | (4.756) | (5.658) | (31.036) | (1.929) | (3.289) | (0.627) |
| Sex (male=0, female=1) | 294.607 | 187.723 | -12.408 | -24.096 | 0.280 | 262.996 | -8.138*** | -9.775 | 3.006* |
| | (255.407) | (258.336) | (23.158) | (21.599) | (42.213) | (249.672) | (0.640) | (15.241) | (1.590) |
| Marital status (unmarried=0, married=1) | -202.185 | 426.450* | 48.585*** | 35.911** | -12.145 | 233.996 | 44.607*** | 38.778*** | -15.589*** |
| | (202.245) | (239.703) | (15.925) | (15.169) | (55.644) | (236.968) | (5.398) | (10.751) | (1.001) |
| Hispanic | 1749.412*** | 2357.422*** | 13.834 | 8.423 | 3.463 | 1990.055*** | -2.894** | 9.332 | -4.782* |
| | (376.725) | (354.644) | (26.282) | (15.774) | (52.971) | (273.425) | (1.189) | (18.084) | (2.558) |
| White | 408.934 | 108.704 | 19.802 | 49.273** | -16.001 | 175.687 | 38.717*** | 68.585*** | -50.299*** |
| | (398.841) | (304.898) | (26.927) | (18.507) | (58.961) | (296.773) | (4.929) | (20.605) | (4.407) |
| Asian | -2.278 | 1290.904** | -42.706 | 93.846 | 204.278 | 1087.048* | 5.767*** | 79.559*** | 73.556*** |
| | (967.338) | (625.420) | (28.584) | (66.239) | (157.268) | (597.259) | (1.264) | (26.904) | (4.813) |
| Other | 333.870 | -394.363 | -29.886 | 57.680 | -95.808* | -577.678 | 37.097*** | 40.898 | -265.920*** |
| | (614.239) | (433.281) | (26.751) | (75.692) | (52.526) | (544.497) | (4.567) | (39.895) | (24.329) |
| $R^2$ | 0.059 | 0.067 | 0.048 | 0.041 | 0.040 | | | | |
| Pseudo-$R^2$ | | | | | | 0.007 | 0.10 | 0.032 | 0.031 |
| N | 2691 | 2691 | 2691 | 2691 | 2691 | 2691 | 2691 | 2691 | 2691 |

*Notes*: All the estimates are weighted by the March CPS household sampling weights. State and time fixed effects are included in the estimation but not reported. State-clustered robust standard errors are in parentheses. Asterisks denote statistical significance at the 1% (***), 5% (**), and 10% (*) levels.

TABLE 5—EFFECTS OF THE MEDICAID EXPANSION ON HOUSEHOLD INCOME: LOW-ASSET VS. HIGH-ASSET HOUSEHOLDS

| | Existing Medicaid-Eligible Households | | | | | Newly Medicaid-Eligible Households | | | | |
|---|---|---|---|---|---|---|---|---|---|---|
| | Total | Labor | Dividend | Interest | Financial Assistance | Total | Labor | Dividend | Interest | Financial Assistance |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) |
| I(Exp. States) × I(Post-2014) | 1648.011* | 1706.067** | 52.796** | 89.065*** | -118.784* | -679.417 | 3.784 | 74.765 | 104.385*** | -171.753* |
| | (866.300) | (661.069) | (23.460) | (24.548) | (62.307) | (582.287) | (533.500) | (46.770) | (34.243) | (91.604) |
| I(Exp. States) | -1278.689*** | 1366.663*** | -39.535* | -239.826*** | 26.434 | 1715.277*** | 961.009*** | -56.667*** | -230.517*** | 5.518 |
| | (342.370) | (264.957) | (22.753) | (45.208) | (30.599) | (247.125) | (194.493) | (17.749) | (9.951) | (26.331) |
| I(Post-2014) | -1371.485** | -2182.058*** | -34.456 | -89.400*** | 3.216 | -409.964 | 579.465* | -36.601 | -94.214*** | 71.470 |
| | (587.055) | (565.063) | (22.868) | (27.835) | (62.655) | (429.665) | (329.375) | (29.518) | (31.216) | (112.347) |
| Age | 303.532* | -45.258 | -10.358 | -17.536 | -58.990 | 423.372** | 80.374 | -13.654 | -25.874* | -57.436* |
| | (174.661) | (153.431) | (15.632) | (15.747) | (39.158) | (196.097) | (203.053) | (14.275) | (14.274) | (33.706) |
| Age square | -3.362 | -0.043 | 0.169 | 0.243 | 0.696 | -4.700** | -1.449 | 0.212 | 0.350* | 0.695* |
| | (2.054) | (1.793) | (0.215) | (0.199) | (0.448) | (2.202) | (2.301) | (0.190) | (0.182) | (0.394) |
| Education | -154.631*** | -61.799* | 18.055 | 10.436* | 13.335* | -172.872*** | -79.742* | 18.473* | 11.918** | 9.589 |
| | (56.268) | (33.949) | (11.042) | (6.078) | (7.437) | (55.562) | (44.084) | (10.343) | (5.161) | (6.183) |
| Sex (male=0, female=1) | 221.980 | 251.474 | -4.924 | -23.033 | -16.382 | 251.361 | 207.562 | -12.462 | -22.743 | 1.790 |
| | (290.420) | (291.333) | (31.215) | (28.251) | (53.157) | (277.073) | (271.782) | (25.458) | (23.275) | (45.656) |
| Marital status (unmarried=0, married=1) | 40.824 | 604.403** | 49.201*** | 46.108*** | 3.350 | -210.796 | 459.439* | 51.832*** | 38.092** | -3.457 |
| | (250.790) | (252.302) | (16.915) | (14.128) | (69.462) | (220.813) | (271.651) | (16.879) | (16.260) | (62.535) |
| Hispanic | 1726.248*** | 2481.145*** | 18.776 | 10.155 | 5.798 | 1836.763*** | 2297.732*** | 12.878 | 6.372 | -15.599 |
| | (437.812) | (358.241) | (29.633) | (16.431) | (56.598) | (382.762) | (401.294) | (28.968) | (16.269) | (56.494) |
| White | 321.776 | 164.208 | 19.903 | 39.641** | -43.020 | 440.263 | -38.647 | 19.631 | 51.944*** | -28.471 |
| | (472.856) | (377.990) | (31.805) | (18.292) | (65.412) | (416.915) | (327.442) | (29.623) | (19.108) | (64.090) |
| Asian | 87.144 | 1395.693** | -43.139 | 110.385 | 153.739 | 68.584 | 1366.453** | -48.510 | 92.605 | 205.909 |
| | (857.744) | (579.716) | (32.539) | (82.228) | (197.581) | (985.453) | (661.575) | (31.632) | (70.637) | (171.196) |
| Other | 702.791 | -546.375 | -3.938 | 80.804 | -131.590** | 526.689 | -504.742 | -34.232 | 58.918 | -108.401* |
| | (677.568) | (600.263) | (23.706) | (95.708) | (60.959) | (664.621) | (434.676) | (30.125) | (81.331) | (56.973) |
| $R^2$ | 0.056 | 0.083 | 0.046 | 0.039 | 0.050 | 0.061 | 0.070 | 0.050 | 0.043 | 0.042 |
| N | 2157 | 2157 | 2157 | 2157 | 2157 | 2423 | 2423 | 2423 | 2423 | 2423 |

*Notes*: All the estimates are weighted by the March CPS household sampling weights. State and time fixed effects are included in the estimation but not reported. State-clustered robust standard errors are in parentheses. Asterisks denote statistical significance at the 1% (***), 5% (**), and 10% (*) levels.

TABLE 6—EFFECTS OF THE MEDICAID EXPANSION ON MIGRATION

| | Migration | | | |
|---|---|---|---|---|
| | With Dependents vs. Childless | | Healthy vs. Sick | |
| | Whether to Move | Move to Expansion States | Whether to Move | Move to Expansion States |
| | (1) | (2) | (3) | (4) |
| I(Childless) × I(Post-2014) | 0.089 | 0.034 | | |
| | (0.056) | (0.058) | | |
| I(Childless) | -0.083 | 0.022 | | |
| | (0.054) | (0.033) | | |
| I(Healthy) × I(Post-2014) | | | -0.046 | -0.019 |
| | | | (0.073) | (0.052) |
| I(Healthy) | | | 0.096 | -0.023 |
| | | | (0.059) | (0.024) |
| I(Post-2014) | -0.131*** | 0.034 | -0.027 | 0.216 |
| | (0.045) | (0.024) | (0.270) | (0.149) |
| Age | -0.097*** | 0.007 | -0.073 | 0.007 |
| | (0.020) | (0.008) | (0.056) | (0.032) |
| Age square | 0.001*** | -0.000 | 0.001 | -0.000 |
| | (0.000) | (0.000) | (0.001) | (0.000) |
| Education | 0.011** | 0.013*** | -0.013 | 0.022* |
| | (0.004) | (0.004) | (0.021) | (0.013) |
| Sex (male=0, female=1) | -0.028 | -0.032* | -0.070 | -0.046 |
| | (0.025) | (0.017) | (0.054) | (0.051) |
| Marital status (unmarried=0, married=1) | -0.146*** | 0.032 | -0.021 | -0.000 |
| | (0.033) | (0.019) | (0.099) | (0.061) |
| Hispanic | -0.126*** | -0.018 | -0.039 | -0.052 |
| | (0.046) | (0.021) | (0.138) | (0.048) |
| White | -0.089* | -0.013 | -0.155 | -0.025 |
| | (0.052) | (0.022) | (0.128) | (0.040) |
| Asian | 0.040 | 0.094** | 0.310* | 0.352*** |
| | (0.063) | (0.035) | (0.175) | (0.110) |
| Other | -0.064 | -0.037 | -0.228 | -0.032 |
| | (0.129) | (0.027) | (0.240) | (0.082) |
| $R^2$ | | 0.147 | | 0.406 |
| Pseudo-$R^2$ | 0.047 | | 0.092 | |
| N | 18183 | 2758 | 2673 | 319 |

*Notes*: All the estimates are weighted by the March CPS household sampling weights. State and time fixed effects are included in the estimation but not reported. State-clustered robust standard errors are in parentheses. Asterisks denote statistical significance at the 1% (***), 5% (**), and 10% (*) levels.

TABLE 7—EFFECTS OF THE MEDICAID EXPANSION ON HOUSEHOLD INCOME: THREE TREATMENT GROUPS

| | Total | Labor | Dividend | Interest | Financial Assistance |
|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) |
| I(Exp. States with no prior exp.) × I(Post-2014) | 515.184 | 863.102 | 136.126** | 130.324** | -180.525 |
| | (994.389) | (674.167) | (53.557) | (54.767) | (129.286) |
| I(Exp. States with limited prior exp.) × I(Post-2014) | 513.825 | 270.263 | 40.798 | 72.274** | -184.865** |
| | (392.400) | (376.072) | (35.676) | (28.650) | (89.205) |
| I(Exp. States with comprehensive prior exp.) × I(Post-2014) | -1242.902** | -367.824 | 45.689 | 78.467*** | -58.053 |
| | (464.363) | (335.084) | (36.697) | (21.550) | (51.570) |
| I(Exp. States with no prior exp.) | -1404.597*** | 198.527 | -56.861** | -227.790*** | -2.549 |
| | (400.990) | (297.246) | (21.798) | (24.094) | (58.217) |
| I(Exp. States with limited prior exp.) | -773.076* | 410.742 | -21.865 | -259.178*** | 43.644 |
| | (431.345) | (314.545) | (18.867) | (31.953) | (70.343) |
| I(Exp. States with comprehensive prior exp.) | 2415.647*** | 2213.358*** | -52.387** | -216.957*** | -12.541 |
| | (272.830) | (184.933) | (19.861) | (11.317) | (16.412) |
| I(Post-2014) | -518.534 | -202.186 | -24.465 | -29.731 | 19.602 |
| | (567.631) | (464.861) | (31.460) | (32.963) | (63.653) |
| Age | 388.013** | 7.062 | -13.032 | -23.401* | -55.119* |
| | (190.879) | (178.868) | (12.198) | (13.033) | (30.957) |
| Age square | -4.248* | -0.529 | 0.202 | 0.318* | 0.666* |
| | (2.161) | (2.022) | (0.165) | (0.166) | (0.361) |
| Education | -173.811*** | -75.652** | 17.079* | 11.288** | 9.267 |
| | (54.129) | (33.491) | (9.560) | (4.752) | (5.734) |
| Sex (male=0, female=1) | 312.656 | 204.988 | -10.735 | -23.110 | -0.943 |
| | (269.646) | (264.852) | (23.435) | (21.612) | (41.886) |
| Marital status (unmarried=0, married=1) | -190.474 | 443.134* | 50.553*** | 37.088** | -12.897 |
| | (208.267) | (241.355) | (16.579) | (15.175) | (54.403) |
| Hispanic | 1749.034*** | 2353.458*** | 13.219 | 8.049 | 3.462 |
| | (374.595) | (353.933) | (26.285) | (15.795) | (52.611) |
| White | 410.491 | 102.215 | 18.663 | 48.576** | -16.166 |
| | (403.196) | (306.299) | (27.000) | (18.500) | (57.956) |
| Asian | -50.622 | 1268.058** | -43.423 | 93.498 | 207.728 |
| | (937.966) | (608.538) | (28.808) | (66.423) | (159.068) |
| Other | 324.503 | -412.845 | -32.287 | 56.235 | -95.244* |
| | (607.624) | (432.296) | (26.967) | (75.923) | (51.167) |
| $R^2$ | 0.060 | 0.068 | 0.049 | 0.041 | 0.040 |
| N | 2691 | 2691 | 2691 | 2691 | 2691 |

*Notes*: All the estimates are weighted by the March CPS household sampling weights. State and time fixed effects are included in the estimation but not reported. State-clustered robust standard errors are in parentheses. Asterisks denote statistical significance at the 1% (***), 5% (**), and 10% (*) levels.

TABLE 8—EFFECTS OF THE MEDICAID EXPANSION ON HOUSEHOLD INCOME: TWO TREATMENT GROUPS

| | Total | Labor | Dividend | Interest | Financial Assistance |
|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) |
| I(Exp. States with no prior exp.) × I(Post-2014) | 511.883 | 861.903 | 136.135** | 130.335** | -180.287 |
| | (994.792) | (674.015) | (53.549) | (54.755) | (129.287) |
| I(Exp. States with prior exp.) × I(Post-2014) | 76.214 | 111.311 | 42.016 | 73.817*** | -153.275** |
| | (508.070) | (351.009) | (35.317) | (24.904) | (73.797) |
| I(Exp. States with no prior exp.) | -1406.929*** | 197.680 | -56.854** | -227.782*** | -2.381 |
| | (402.184) | (297.204) | (21.789) | (24.086) | (58.079) |
| I(Exp. States with prior exp.) | -822.320*** | 682.986*** | 93.705*** | -174.239*** | -0.927 |
| | (283.052) | (222.361) | (11.693) | (17.373) | (55.830) |
| I(Post-2014) | -523.953 | -204.154 | -24.450 | -29.712 | 19.993 |
| | (567.587) | (464.630) | (31.450) | (32.950) | (63.557) |
| Age | 391.714** | 8.406 | -13.042 | -23.414* | -55.386* |
| | (191.982) | (179.407) | (12.198) | (13.019) | (31.223) |
| Age square | -4.282* | -0.542 | 0.202 | 0.318* | 0.668* |
| | (2.170) | (2.027) | (0.165) | (0.166) | (0.364) |
| Education | -173.326*** | -75.476** | 17.077* | 11.286** | 9.232 |
| | (54.377) | (33.371) | (9.560) | (4.751) | (5.741) |
| Sex (male=0, female=1) | 302.484 | 201.293 | -10.706 | -23.074 | -0.209 |
| | (267.043) | (264.356) | (23.410) | (21.574) | (41.933) |
| Marital status (unmarried=0, married=1) | -193.042 | 442.201* | 50.560*** | 37.097** | -12.712 |
| | (210.713) | (240.768) | (16.576) | (15.174) | (54.504) |
| Hispanic | 1746.598*** | 2352.573*** | 13.226 | 8.058 | 3.638 |
| | (376.826) | (353.841) | (26.283) | (15.798) | (52.531) |
| White | 403.745 | 99.765 | 18.681 | 48.600** | -15.679 |
| | (400.054) | (305.233) | (27.001) | (18.493) | (57.988) |
| Asian | -6.166 | 1284.206** | -43.546 | 93.341 | 204.519 |
| | (963.512) | (620.122) | (28.801) | (66.359) | (156.954) |
| Other | 322.779 | -413.471 | -32.282 | 56.241 | -95.120* |
| | (611.335) | (432.772) | (26.966) | (75.913) | (51.062) |
| $R^2$ | 0.059 | 0.067 | 0.049 | 0.041 | 0.040 |
| N | 2691 | 2691 | 2691 | 2691 | 2691 |

*Notes*: All the estimates are weighted by the March CPS household sampling weights. State and time fixed effects are included in the estimation but not reported. State-clustered robust standard errors are in parentheses. Asterisks denote statistical significance at the 1% (***), 5% (**), and 10% (*) levels.

TABLE 9—EFFECTS OF THE MEDICAID EXPANSION ON HOUSEHOLD INCOME: HETEROGENEITY BY SUB-GROUPS

| | | | |
|---|---|---|---|
| **Panel A. Total** | | | |
| *Male vs. Female* | Male | Female | Difference |
| I(Exp. States) × I(Post-2014) | 105.557<br>(866.751) | 158.193<br>(791.125) | -52.636 |
| *White vs. Non-White* | White | Non-white | Difference |
| I(Exp. States) × I(Post-2014) | 126.293<br>(769.295) | 152.256<br>(635.268) | -25.963 |
| *Married vs. Unmarried* | Married | Unmarried | Difference |
| I(Exp. States) × I(Post-2014) | 13.384<br>(772.196) | 412.566<br>(752.983) | -399.182 |
| **Panel B. Labor** | | | |
| *Male vs. Female* | Male | Female | Difference |
| I(Exp. States) × I(Post-2014) | -830.532<br>(883.635) | 1191.506**<br>(519.253) | -2022.038* |
| *White vs. Non-White* | White | Non-white | Difference |
| I(Exp. States) × I(Post-2014) | 508.326<br>(645.693) | -189.070<br>(470.797) | 697.396 |
| *Married vs. Unmarried* | Married | Unmarried | Difference |
| I(Exp. States) × I(Post-2014) | -23.414<br>(551.079) | 503.306<br>(487.172) | -526.720 |
| **Panel C. Dividend** | | | |
| *Male vs. Female* | Male | Female | Difference |
| I(Exp. States) × I(Post-2014) | 117.632<br>(80.155) | 4.916<br>(26.539) | 112.716 |
| *White vs. Non-White* | White | Non-white | Difference |
| I(Exp. States) × I(Post-2014) | 119.513<br>(72.324) | 3.340<br>(7.548) | 116.173 |
| *Married vs. Unmarried* | Married | Unmarried | Difference |
| I(Exp. States) × I(Post-2014) | 108.793<br>(65.900) | 4.334<br>(5.654) | 104.459 |
| **Panel D. Interest** | | | |
| *Male vs. Female* | Male | Female | Difference |
| I(Exp. States) × I(Post-2014) | 147.881**<br>(63.650) | 11.788<br>(49.375) | 136.093 |
| *White vs. Non-White* | White | Non-white | Difference |
| I(Exp. States) × I(Post-2014) | 140.997**<br>(58.907) | 26.527<br>(33.106) | 114.470 |
| *Married vs. Unmarried* | Married | Unmarried | Difference |
| I(Exp. States) × I(Post-2014) | 85.000*<br>(45.817) | 67.999*<br>(35.851) | 17.001 |
| **Panel E. Financial Assistance** | | | |
| *Male vs. Female* | Male | Female | Difference |
| I(Exp. States) × I(Post-2014) | -168.675*<br>(98.829) | -169.350<br>(126.062) | 0.675 |
| *White vs. Non-White* | White | Non-white | Difference |
| I(Exp. States) × I(Post-2014) | -344.192**<br>(145.658) | -6.309<br>(46.726) | -337.883* |
| *Married vs. Unmarried* | Married | Unmarried | Difference |
| I(Exp. States) × I(Post-2014) | -75.799<br>(103.528) | -302.721<br>(220.779) | 226.922 |

*Notes*: All the estimates are weighted by the March CPS household sampling weights. State and time fixed effects are included in the estimation but not reported. State-clustered robust standard errors are in parentheses. Asterisks denote statistical significance at the 1% (***), 5% (**), and 10% (*) levels.

TABLE 10—HOUSEHOLD INCOME TRENDS IN THE PRE-REFORM PERIODS

| | Total | Labor | Dividend | Interest | Financial Assistance |
|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) |
| I(Exp. States) × Trend | -155.015 | 1.019 | -20.051 | -34.215 | 57.679 |
| | (268.469) | (266.493) | (37.657) | (27.936) | (68.585) |
| I(Exp. States) | -1006.014 | 1398.489** | 7.859 | -174.822** | -100.559 |
| | (636.678) | (632.251) | (68.436) | (79.091) | (168.815) |
| Trend | 139.530 | 241.689 | 36.393 | 23.397 | 16.276 |
| | (197.144) | (228.294) | (35.576) | (26.965) | (22.724) |
| Age | 343.161* | 3.594 | -10.823 | -19.628 | -62.383 |
| | (174.451) | (170.274) | (16.959) | (17.831) | (43.219) |
| Age square | -3.897* | -0.688 | 0.178 | 0.272 | 0.738 |
| | (2.065) | (1.997) | (0.233) | (0.225) | (0.494) |
| Education | -157.491*** | -61.489 | 20.039 | 11.458* | 14.186* |
| | (57.961) | (42.930) | (12.278) | (6.683) | (8.065) |
| Sex (male=0, female=1) | 226.011 | 252.809 | -5.481 | -23.754 | -16.984 |
| | (335.816) | (316.373) | (34.215) | (31.024) | (58.152) |
| Marital status (unmarried=0, married=1) | 29.785 | 707.801** | 56.010*** | 54.350*** | 14.723 |
| | (284.276) | (287.254) | (19.508) | (15.639) | (78.213) |
| Hispanic | 1826.369*** | 2527.458*** | 17.379 | 8.746 | -18.452 |
| | (513.040) | (402.813) | (32.075) | (17.683) | (61.835) |
| White | 280.144 | 38.303 | 20.574 | 45.938** | -67.406 |
| | (502.449) | (392.524) | (36.363) | (20.228) | (74.171) |
| Asian | 58.854 | 1535.357** | -48.296 | 117.618 | 142.579 |
| | (956.540) | (622.724) | (36.843) | (87.989) | (214.534) |
| Other | 838.112 | -596.975 | -5.545 | 88.977 | -154.136** |
| | (719.390) | (617.691) | (27.303) | (104.704) | (64.085) |
| $R^2$ | 0.055 | 0.083 | 0.048 | 0.042 | 0.053 |
| N | 1889 | 1889 | 1889 | 1889 | 1889 |

*Notes*: All the estimates are weighted by the March CPS household sampling weights. State and time fixed effects are included in the estimation but not reported. State-clustered robust standard errors are in parentheses. Asterisks denote statistical significance at the 1% (***), 5% (**), and 10% (*) levels.

TABLE 11—EFFECTS OF THE MEDICAID EXPANSION ON HOUSEHOLD INCOME: PLACEBO TESTS

| | Total | Labor | Dividend | Interest | Financial Assistance |
|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) |
| I(Exp. States) × I(Post-2011) | -956.602* | -309.625 | -16.099 | -53.867 | 72.224 |
| | (478.489) | (609.183) | (54.257) | (43.546) | (134.963) |
| I(Exp. States) × I(Post-2012) | -59.142 | 283.062 | -23.403 | -61.202 | 56.283 |
| | (584.717) | (551.798) | (70.786) | (57.502) | (127.774) |
| I(Exp. States) × I(Post-2013) | -38.717 | -72.533 | -82.759 | -90.166 | 228.575 |
| | (793.799) | (643.161) | (109.057) | (85.589) | (193.560) |

*Notes*: All the estimates are weighted by the March CPS household sampling weights. State and time fixed effects are included in the estimation but not reported. State-clustered robust standard errors are in parentheses. Asterisks denote statistical significance at the 1% (***), 5% (**), and 10% (*) levels.

TABLE 12—EFFECTS OF THE MEDICAID EXPANSION ON HOUSEHOLD INCOME: DIFFERENT AGE CUTOFFS

| | Total | Labor | Dividend | Interest | Financial Assistance |
|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) |
| $\mathbf{26 \leq Age\ Cutoff \leq 56}$ | | | | | |
| I(Exp. States) × I(Post-2014) | 69.971 | 130.907 | 58.472*** | 69.906*** | -80.946*** |
| | (506.952) | (428.040) | (9.088) | (19.105) | (5.951) |
| $\mathbf{26 \leq Age\ Cutoff \leq 57}$ | | | | | |
| I(Exp. States) × I(Post-2014) | 238.441 | 219.228 | 77.861*** | 60.281*** | -76.979*** |
| | (499.640) | (450.233) | (10.549) | (16.051) | (5.573) |
| $\mathbf{26 \leq Age\ Cutoff \leq 58}$ | | | | | |
| I(Exp. States) × I(Post-2014) | 64.942 | 165.268 | 55.964*** | 46.446*** | -76.109*** |
| | (522.212) | (437.609) | (7.166) | (15.368) | (4.985) |
| $\mathbf{26 \leq Age\ Cutoff \leq 59}$ | | | | | |
| I(Exp. States) × I(Post-2014) | -43.490 | 112.012 | 70.599*** | 55.960*** | -45.670*** |
| | (475.041) | (461.333) | (7.652) | (17.819) | (5.163) |
| $\mathbf{26 \leq Age\ Cutoff \leq 60}$ | | | | | |
| I(Exp. States) × I(Post-2014) | -43.612 | -8.587 | 71.609*** | 66.896*** | -50.874*** |
| | (440.407) | (427.320) | (7.194) | (21.938) | (4.177) |
| $\mathbf{19 \leq Age\ Cutoff \leq 55}$ | | | | | |
| I(Exp. States) × I(Post-2014) | -176.455 | -0.842 | 39.298*** | 56.639*** | -80.304*** |
| | (550.363) | (477.509) | (6.637) | (17.241) | (8.299) |
| $\mathbf{20 \leq Age\ Cutoff \leq 55}$ | | | | | |
| I(Exp. States) × I(Post-2014) | -164.601 | -50.528 | 39.581*** | 58.601*** | -90.066*** |
| | (539.845) | (465.427) | (6.701) | (17.225) | (8.460) |
| $\mathbf{21 \leq Age\ Cutoff \leq 55}$ | | | | | |
| I(Exp. States) × I(Post-2014) | -244.325 | -52.859 | 41.607*** | 60.298*** | -74.555*** |
| | (516.700) | (450.680) | (6.939) | (17.197) | (8.110) |
| $\mathbf{22 \leq Age\ Cutoff \leq 55}$ | | | | | |
| I(Exp. States) × I(Post-2014) | -195.750 | -68.355 | 42.501*** | 63.482*** | -76.998*** |
| | (517.826) | (442.037) | (7.092) | (16.958) | (7.302) |
| $\mathbf{23 \leq Age\ Cutoff \leq 55}$ | | | | | |
| I(Exp. States) × I(Post-2014) | -171.807 | 17.694 | 48.595*** | 63.261*** | -99.533*** |
| | (552.282) | (449.403) | (7.955) | (16.703) | (6.735) |
| $\mathbf{24 \leq Age\ Cutoff \leq 55}$ | | | | | |
| I(Exp. States) × I(Post-2014) | 75.148 | 216.155 | 53.505*** | 68.588*** | -100.993*** |
| | (517.340) | (462.213) | (8.221) | (17.847) | (6.584) |
| $\mathbf{25 \leq Age\ Cutoff \leq 55}$ | | | | | |
| I(Exp. States) × I(Post-2014) | 162.152 | 249.986 | 55.067*** | 71.179*** | -107.507*** |
| | (499.436) | (439.774) | (8.411) | (17.650) | (6.731) |

*Notes*: All the estimates are weighted by the March CPS household sampling weights. State and time fixed effects are included in the estimation but not reported. State-clustered robust standard errors are in parentheses. Asterisks denote statistical significance at the 1% (***), 5% (**), and 10% (*) levels.

TABLE 13—EFFECTS OF THE MEDICAID EXPANSION ON HOUSEHOLD INCOME: CLUSTERING AT TREATMENT-YEAR LEVEL

| | Total | Labor | Dividend | Interest | Financial Assistance |
|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) |
| I(Exp. States) × I(Post-2014) | 172.816* | 277.742** | 62.886*** | 86.349*** | -159.265*** |
| | (64.058) | (52.962) | (4.396) | (3.267) | (6.333) |
| I(Exp. States) | -308.127 | 928.721 | -57.050 | -249.527*** | -4.573 |
| | (1108.947) | (450.546) | (27.134) | (24.498) | (33.584) |
| I(Post-2014) | -530.367 | -215.204* | -25.835** | -30.544 | 20.391 |
| | (321.412) | (78.961) | (5.359) | (18.296) | (28.028) |
| Age | 392.695*** | 10.095 | -12.831 | -23.287 | -55.447 |
| | (58.347) | (159.070) | (9.097) | (19.178) | (28.853) |
| Age square | -4.291*** | -0.557 | 0.200 | 0.317 | 0.669 |
| | (0.635) | (1.822) | (0.135) | (0.258) | (0.348) |
| Education | -172.875*** | -74.699 | 17.175 | 11.344** | 9.204 |
| | (13.575) | (35.103) | (8.655) | (3.219) | (5.391) |
| Sex (male=0, female=1) | 294.607** | 187.723 | -12.408 | -24.096 | 0.280 |
| | (86.807) | (468.223) | (13.118) | (28.904) | (15.060) |
| Marital status (unmarried=0, married=1) | -202.185 | 426.450 | 48.585* | 35.911 | -12.145 |
| | (208.124) | (283.858) | (17.815) | (16.705) | (39.560) |
| Hispanic | 1749.412*** | 2357.422*** | 13.834* | 8.423 | 3.463 |
| | (290.352) | (364.285) | (5.274) | (7.051) | (20.339) |
| White | 408.934 | 108.704 | 19.802*** | 49.273** | -16.001 |
| | (275.861) | (207.363) | (0.905) | (11.203) | (50.779) |
| Asian | -2.278 | 1290.904** | -42.706 | 93.846* | 204.278 |
| | (296.166) | (389.839) | (26.477) | (33.271) | (128.150) |
| Other | 333.870 | -394.363 | -29.886 | 57.680 | -95.808 |
| | (742.598) | (293.571) | (24.566) | (53.366) | (64.665) |
| $R^2$ | 0.059 | 0.067 | 0.048 | 0.041 | 0.040 |
| N | 2691 | 2691 | 2691 | 2691 | 2691 |

*Notes*: All the estimates are weighted by the March CPS household sampling weights. State and time fixed effects are included in the estimation but not reported. Treatment-year-clustered robust standard errors are in parentheses. Asterisks denote statistical significance at the 1% (***), 5% (**), and 10% (*) levels.