Incentive(less)? The Effectiveness of Tax Credits and Cost-Sharing Subsidies in the Affordable Care Act

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ABSTRACT

Several prominent ACA components went into effect in 2014: individual and large employer mandates, private health insurance exchanges, and expansion of the Medicaid program. Initial evidence has suggested large impacts on insurance coverage, but the contribution of each component was not clear. This study applied a regression discontinuity design to identify separate and combined effects of premium tax credits and cost-sharing subsidies available on health insurance exchanges on private insurance coverage. Income relative to the Federal Poverty Level (FPL) was used as an exogenous source of variation against strict eligibility cutoffs for tax credits at 400% FPL, cost-sharing subsidies at 250% FPL, and the combined effect at 100% or 138% FPL. Data are drawn from the Current Population Survey. I found a large increase in individually purchased private insurance coverage just above 138% FPL in Medicaid expansion states of 7 to 8 percentage points and a smaller effect above 100% FPL in non-expansion states, where consumer were initially eligible for tax credits and cost-sharing subsidies and the incentives were the strongest. There were no discernible differences in coverage at 250% FPL attributable to the cost-sharing subsidies-only and only suggestive differences at 400% FPL attributable solely to the tax credits. Coverage increases did not appear to be driven by adverse selection and no effects were found for a variety of labor market outcomes. The results suggest that the tax credits cost-sharing subsidies would need to be raised at higher incomes to induce more participation.

INTRODUCTION

The Patient Protection and Affordable Care Act of 2010 (ACA) implemented a complex, broad set of changes in the U.S. health insurance (HI) and health care system. In 2014, several prominent ACA components went into effect: individual and large employer mandates, private HI exchanges, and expansion of the Medicaid program. A wave of early, nationally representative polls have suggested that the ACA has reduced the proportion of the uninsured population (e.g., Sommers et al., 2014, 2015a). These early investigations into the impact of the ACA to date have used pre-post, difference-in-difference, and interrupted time series methods. Identifying a feasible control group is a challenge given the widespread reach of the ACA policies. Although preliminary evidence suggests that insurance coverage increased substantially, disentangling the mechanisms by which consumer behaviors are affected is of critical importance.

In this study, I used a regression discontinuity (RD) design to identify impacts of the premium tax credits and cost-sharing subsidies available on private HI exchanges on individually purchased insurance. To the author's knowledge, this is the first study to examine the impacts of the tax credits and cost-sharing subsidies implemented under the ACA. I found strong evidence of increases in individually purchased private insurance (IPI) just above 138% of the Federal Poverty Level (FPL) in Medicaid expansion states, where subsidized insurance coverage is first available and individuals are just ineligible for the expanded Medicaid program. In non-expansion states, the effects above 100% FPL were slightly smaller in magnitude and not as robust. I also found suggestive evidence of an increase in IPI around 400% FPL attributable to the premium tax credits. Stratifying by demographic characteristics, I did not find strong evidence of adverse selection or differential effects. Effect sizes were similar across married and single individuals and younger and older adults. There were differential effects for health status, but these effects did not signal adverse selection. Individuals self-reporting excellent or very good health had similar effect sizes to the main results, while individuals reporting good, fair or poor health had much smaller effects.

Beyond the clear policy effect of the ACA tax credits and cost-sharing subsidies on IPI, this analysis contributes to the broader literature in several ways. First, the tax credits and cost-sharing subsidies represent a newer form of income transfer in health policy, in between a conditional transfer, such as the earned income tax credit, and an in-kind transfer, such as the traditional Medicare/Medicaid program. While similar tax subsidies have only been used in past programs related to self-employment and for newly unemployed individuals, the results here suggest that these types of transfers may have broader appeal for lower-income individuals.

Second, while the effect of the combined incentive for lower-income individuals was clear, the results were at best suggestive for the cost sharing– and tax credit–only effects. Given that the individual mandate penalty and premiums increased in 2015 while the tax credits/cost-sharing benefits did not, these results suggested that the long-term impact beyond the lowest-income group could be minimal.

Finally, I did not find evidence of labor market effects near the cutoff points. The broader income transfer literature suggests that transfer programs that use income-based eligibility have mixed labor supply effects on the intensive and extensive margin, as individuals adjust labor supply to maximize transfer benefits. A secondary goal of the ACA, however, was to reduce the reliance on employer-sponsored insurance (ESI) and job lock, which could increase rates of self-employment, part-time employment, and/or job mobility. There was no evidence of any effect on labor force participation, unemployment, part-time status, self-employment, or job mobility. This finding is similar to other recent studies focusing on the labor market impacts of Medicaid expansions (Gooptu et al., 2016).

BACKGROUND

The ACA introduced a relatively new type of income transfer for health care to attempt to increase participation in the IPI market: premium tax credits and cost-sharing subsidies. The tax credits were designed to reduce the costs of obtaining HI, while the cost-sharing subsidies reduced the burden of subsequent medical consumption. This type of transfer was new for health care in terms of the policy lever and implementation scale, as it combined two distinct types of traditional transfers. The combined benefit was between a conditional income transfer, such as the earned income tax credit, and an in-kind benefit, such as fully subsidized health care consumption per the Medicaid program. The impact of conditional income transfers on welfare is well-studied, and because eligibility is often income-based, these programs also tend to have subsequent effects on labor supply. On the other end of the spectrum, in-kind transfers, such as the Medicaid program, are also well-studied and fully cover the consumption effects of free medical consumption. The large consumption benefit also affects labor supply.

Little empirical work has examined tax credits for HI premiums. The most relevant literature focuses on state-level variation in self-employment subsidies for HI and for subsidies to continue ESI benefits after employer separation (e.g., Barber & Moffett, 2015; Heim & Lurie, 2009, 2010; Moriya & Simon, 2014). These policies have had modest, positive effects, but they are limited in scope and benefit. Prior HI subsidies did not actually affect the cost of HI. The self-employment subsidies afforded a tax credit toward the volatile individual market prices, and the COBRA-subsidies offset the full cost of an ESI benefit, which generally resulted in more expensive HI premiums. Furthermore, the affected populations are narrowly defined.

Beyond the question of whether incentives affected participation, this study contributes to the broader literature on income and in-kind transfers. HI is not a costless benefit to obtain, as individuals must invest in premium payments for access to reduced future medical costs. With rising costs of medical care, the benefits of HI may have substantial wealth and welfare effects for lower-income individuals. Recent studies have suggested the importance of including HI benefits as a part of total income measures, and that HI benefits may reduce inequality (Burkhauser et al., 2013). Thus, the benefits of the dual incentive may provide large health and income benefits to the intended recipients.

Components of the ACA

The primary focus of this analysis is to examine the impact of tax credits and cost-sharing subsidies that were available first in 2014 to certain income bands of the population and were obtained through state-based exchanges (SBE) or a federally facilitated exchange (FFE). The ACA initiated HI exchanges, a marketplace to facilitate small group and individual HI plan purchases. Given the historically higher premiums individuals and small groups face, the exchanges were intended to mimic the large risk pools of large companies and provide more affordable premiums. States were required to either design, regulate, and implement an SBE or defer to the FFE. In some cases, states opted for a partnership arrangement, whereby the state incorporated some components of the SBE but still deferred to the FFE for the enrollment process. In 2014, 17 states chose SBEs, 27 chose FFEs, and 7 chose a partnership arrangement.

The mechanisms by which the SBE and Medicaid expansion states increase IPI coverage are likely grounded in consumer assistance efforts. SBE states funded consumer advocacy and outreach efforts to enroll eligible consumers, suggesting awareness of the SBE, the tax credits and the cost-sharing subsidies was likely to be higher. Furthermore, many expanding states directly referred individuals to the SBE when ineligible for Medicaid. Consumers in non-SBE states still had access to the FFE, but they may not have had the same access to information and assistance as the SBE states (Dash et al., 2013; Long et al., 2015).

To increase affordability of HI plans, the ACA subsidized to a varying degree individuals with incomes 100% to 400% of the FPL. The ACA implemented both a *premium tax credit* to individuals between 100% and 400% FPL and a *cost-sharing* subsidy to individuals between 100% and 250% FPL. For 2014, the FPL cutoffs for single individuals were $11,490 (100% FPL), $15,856 (138% FPL), $28,725 ($250% FPL) and $45,960 (400% FPL) (KFF, 2014a). The tax credits fell on a sliding scale, where individuals received a higher relative subsidy at lower income levels. At 400% FPL, the income cap was 9.2%, or $4,320 for an individual. The amount of the credit was the difference between the total annual premium and the income cap. The credit was normalized to the 70% actuarially valued plan (the silver tier), so that individuals did not receive a higher subsidy for choosing a gold or platinum tier plan. The tax credit could have been applied at the time of enrollment to reduce monthly payments or collected in a lump sum through income tax filings.

The cost-sharing subsidy was available to individuals 100%–250% FPL and increased the actuarial value of the silver plan to 94% for those 100%–150% FPL, 87% for those 150%–200% FPL and 73% for those 200%–250% FPL. Again, the cost-sharing subsidy was normalized to the silver plan. When an individual below 250% FPL chose an insurance plan on the exchange, the subsidy reduced the face value of the deductible, the out-of-pocket maximum, and co-payments associated with the plan. For example, an insurance plan on the exchange might have had a $2,000 deductible, a $6,400 out-of-pocket maximum, and a $45 co-payment for primary care physician visits. For an individual with income 150%–200% FPL, the subsidy would have reduced the deductible to $500, the out-of-pocket maximum was capped at $2,250, and the co-payment is reduced to $15. Other than regulations on the out-of-pocket maximum, insurers could choose how to balance the deductible/co-payment mix to achieve an actuarial value of 87% for the 150%–200% FPL cost-sharing subsidy.

In this analysis, I focus on consumer behavior around each of three eligibility cutoffs: 100%/138% FPL, 250% FPL, and 400% FPL. Table 1 describes how program eligibility changed across the different cutoffs. I use 138% FPL for Medicaid expansion states instead of 100% FPL to avoid overlap with expanded Medicaid eligibility. The RD design compares individuals just above and below each of the three FPL cutoffs. In what follows, I refer to the changes around the 400% FPL cutoff as the effect of the tax credits, comparing individuals just below 400% FPL that are eligible for tax credits and individuals just above 400% FPL that are ineligible. An effect around the 250% FPL cutoff is attributed to cost-sharing subsidies. Individuals just below and just above 250% FPL have access to the tax credits, but individuals just below 250% FPL also gain eligibility for cost-sharing subsidies. An effect around the cutoff at 250% FPL would capture the differential impact of the cost-sharing subsidies. Lastly, changes around the 100%/138% FPL cutoffs are a combined effect of the tax credits and cost-sharing subsidies. Just above 100%/138% FPL, individuals gain eligibility to the dual incentive.

A second incentive to participation was an individual mandate that required all individuals to obtain a minimum 60% actuarial value HI plan or pay a lump sum tax ($95 or 1% of income per adult in 2014, $325 per adult in 2015, and $695 per adult in 2016) (KFF, 2014b). There were restrictions on the amount of the tax imposed at low income levels, but the tax was binding to most individuals who file an income tax return. Given the low level of the tax in 2014, the contamination of this component is assumed to be zero for this analysis.

This analysis does not formally examine Medicaid expansion. Medicaid expansion interacts with the analyses here, since individuals are eligible just below 138% FPL, but this analysis does not focus on the effect of Medicaid expansion. An intended effect of the research design is that individuals were not eligible for Medicaid coverage above 138% FPL and that there should be a negative effect due to ineligibility. This is not a policy effect in as much as a validity check.

This analysis builds on a waxing literature studying the impacts of the ACA. Several organizations conducted nationally representative surveys to track early impacts of the 2014 ACA components, Medicaid expansion, individual and large employer mandates and private HI exchanges. Descriptive results from the Health Reform Monitoring Survey indicated a regression-adjusted increase in the insured rate of 5.3 percentage points among adults with an income 138%–399% FPL through June 2014 and a 7.4 percentage point increase through March 2015 (Long et al., 2014a, 2015a). The gains varied by age, race/ethnicity, and gender and were potentially larger in Medicaid expansion states. Among those uninsured in the 138%–399% FPL range, almost half of respondents were unaware of the incentives, approximately 60% were uninsured primarily due to costs of insurance, and 20% did not want insurance or would rather pay the nonparticipation fine (Shartzer et al., 2014). Estimates from the Gallup Poll and National Health Interview Survey found similar reductions in the proportion of uninsured (e.g., Black & Cohen, 2014; Sommers et al., 2015).

Beyond impacts of the ACA on the overall HI coverage, several studies have focused on the impact of the broader ACA on ESI. Early survey data from the Urban Institute found little evidence of changes in ESI availability, ESI take-up, and ESI coverage, but offered suggestive evidence that ESI coverage increased for employees of small employers and low incomes

(Blavin et al., 2015). The 2015 Employer Health Benefits Survey found an increase in ESI premiums consistent with increases from previous years and observed little change in benefit design (Claxton et al. 2015). These rapid response surveys provided suggestive evidence of anticipatory changes in offer and benefit design to meet ACA requirements, but little overall impact on ESI.

Quasi-experimental analyses of different ACA components have focused on early adopting states and ACA components implemented prior to 2014. Golberstein et al. (2015) found large increases in public HI (PHI) coverage associated with Medicaid expansion in California. Kaestner and colleagues (2015) used difference-in-differences and synthetic control methods to estimate an approximately 4 percentage point increase in PHI. Early evidence from the dependent-coverage mandate indicated a marked increase in insurance coverage among those less than 26 years of age (Antwi et al., 2012).

Conceptual Model of the Effects of ACA Incentives

This analysis is motivated by a partial equilibrium approach, comparing consumer choice pre- and post-2014. The intuition behind and the implications of the ACA reforms are described in Figure 1, a graphical analysis of the tax credits. Consider a simple two-good economy, health/HI and all other goods, where consumers maximize their utility by spending their income between these two goods. The left panel represents a pre-2014 situation, and the right panel is post-2014. BL_1 refers to a choice set at 400% FPL. The consumer must choose whether or not to purchase insurance for the coming year and must allocate income based on expected health expenditures. Under this choice set, some consumers choose to not purchase health/HI (U_1), and other consumers choose varying levels of health/HI (U_2 and U_3).

In the right panel for post-2014, BL_1 (dashed, gray line) is replaced by BL_2 (solid black line), which is shifted and has a small gap where certain levels of health/HI cannot be consumed. The left-segment of BL_2 is out of pocket (OOP) consumption only and shifts inward due to the non-participation tax. Again, the effect of the non-participation penalty is small in 2014 and assumed to be negligible. Figure 1 highlights the fact that non-participants will be worse off as the non-participation penalty increases. The right segment of BL_2 shifts outward due to the tax credit. The gap represents the consumption floor; to have HI, one must purchase a 60% actuarial value plan.

Consumers with U_1 still do not purchase HI and are worse off due to the non-participation tax, while consumers with U_3 gain the most benefit, as the tax credit is a lump sum income transfer. It is unclear what consumers located on the dashed portion of BL_1 do. Someone with an indifference curve U_2 is practically indifferent between insuring and not insuring in the new market. Individuals with a slightly higher marginal rate of substitution (MRS) between non-health consumption and health/HI may increase health/HI purchases, while individuals with lower MRS may choose not to purchase insurance. In summary, there are likely individuals who would not purchase HI and are worse off, individuals who would purchase HI even without the ACA incentives and are better off, and a third group of individuals who could be better off or worse off by purchasing insurance. The impact of the tax credits depend on the fraction of consumers in the third bin, those consumers who are on the margin of purchasing HI under the new regulations, and the average MRS of those marginal consumers.

The simplified example in Figure 1 covers consumer effects around 400% FPL. As mentioned earlier, the non-participation tax is trivial in 2014. Therefore, the assumption is that the influence of the non-participation tax is minimal and there is only a tax credit effect at 400%

FPL that compares whether the income transfer is large enough to increase health/HI purchases relative to the average MRS of consumers.

The insurance exchanges also subsidized coverage for consumers between 100% and 250% FPL with a cost-sharing subsidy. Individuals between 100% FPL and 250% FPL receive both a tax credit and a cost-sharing subsidy. The cost-sharing subsidy modifies BL_2 in the right panel of Figure 1 by reducing the price of medical care consumption, flattening the slope of the right segment of BL_2. Compared to the scenario around 400% FPL, the flattened slope of BL_2 would suggest that individuals with a higher MRS would be more willing to purchase health/HI given the consumption benefit incurred by the price change. It is anticipated that effects around 250% FPL should be stronger than at 400% FPL based on the price change alone, but it must also be emphasized that the relative level of tax credits is also higher at 250% FPL than at 400% FPL.

A third cutoff at 138% FPL or 100% FPL determines whether individuals are eligible for Medicaid expansion. Below 138% FPL, consumers in expansion states get access to fully subsidized insurance, while there was a coverage gap below 100% FPL in non-expansion states. At this cutoff, it is a combined policy effect capturing whether the tax credits and cost-sharing subsidy increase income and reduce the price of future medical consumption enough to induce consumers on the margin to participate. The relative benefit of the tax credits and cost-sharing subsidies were also highest near this cutoff.

The empirical model focuses on consumers just eligible for the tax credits at 400% FPL, just eligible for cost-sharing subsidies at 250% FPL and individuals who are first eligible for tax credits and cost-sharing subsidies at 100% or 138% FPL. The primary question I will answer is whether the tax credit and cost-sharing subsidy amounts were high enough to induce participation in the HI exchanges relative to the average MRS of consumers. It is anticipated that the effects of the subsidies are likely to be strongest around 250% and 100%/138%, where the tax credits and cost-sharing subsidies offered the greatest potential welfare benefit. A recent working paper by Pauly et al. (2015) simulated financial implications and welfare changes associated with the exchanges and tax credits/cost-sharing subsidies. Their results indicated that the additional financial burden of purchasing HI was offset by increases in welfare due to the expected medical care prices for individuals below 250%. Pauly and colleagues' (2015) model implied that average consumer MRS was higher for consumers with lower incomes. Thus, the model provides some calibration to the conceptual model presented here and supports the hypothesis of larger potential impacts at the lower FPL cutoffs.

METHODS

Data

The Current Population Survey (CPS) Annual Social and Economic Supplement (ASEC) was used for this analysis because it captures income, HI status, and demographics representatively at the national and state level (Flood et al., 2015). The analyses focused on 2014, the first year the tax credits and cost-sharing subsidies were implemented. As a validity check, I used a pre-reform period pooling data from the calendar years 2010–2012. The ASEC HI questions were redesigned starting in calendar year 2013, and the Census Bureau recommends against directly comparing HI measures before and after 2013 until methods are developed to correct for differences in the series (Pascale et al. 2016). The pre-period was not used as a benchmark for changes in IPI, ESI, or PHI. Furthermore, I excluded 2013 from the pre-period due to concerns about respondents reporting IPI or PHI coverage associated with the early enrollment period in fall/winter 2013.

Several exclusion restrictions were made to the sample: age, veterans, work disabled, and allocated HI status. The sample included adults aged 26 to 64. Veterans and work disabled individuals were excluded per guidance from the literature. Any individual with an allocated HI status was dropped; allocated HI meant that the insurance status was assigned based on other answers and information on the respondent's record or imputed if the interview was not fully completed. Allocation did not include logical imputation for PHI.

The main outcome was a binary indicator for having HI in the past year. There were four exclusive categories for HI: IPI, ESI, and PHI, or uninsured. If an individual reported ESI coverage during a given year, he or she was excluded from IPI or PHI. Individuals who reported any ESI or IPI were not included in PHI. The primary independent variable was the respondent's income relative to the FPL. FPL is the ratio of the total family income to the federally determined poverty threshold. The poverty threshold was based on the size of the family. Binary indicators were used to denote incomes that fell below 400% and 250% and above 100%/138% FPL; these capture the eligibility cutoffs in the RD design.

Technically, subsidy eligibility was based on modified adjusted gross income (MAGI) and not gross income as reported in the ASEC. To calculate FPL, I used a Census Bureau-provided measure of adjusted gross income (AGI) that was created using statistical matching with Internal Revenue Service (IRS) tax records. AGI removes certain tax deductions and exemptions from gross income; AGI is lower than gross income. The difference between MAGI and AGI is not generally large and captures foreign assets and other investment incomes. At lower income levels, this type of income is not as prevalent and AGI should be considered a good approximation to MAGI. Statistical matching used to link IRS and ASEC is a potential source of measurement error, but there were not better sources that capture AGI beyond the IRS data (conversely, the IRS data do not historically measure HI coverage well).

Using AGI in the income-relative FPL calculation reduces the ASEC population around the 250% and 400% by roughly half. To date, most studies of the ACA that have reported statistics stratified by the FPL have used the gross income definition. While incentive eligibility was not based on gross income, there was concern that individuals going on the exchange may have missed the fine print and used their gross income in determining eligibility. A separate set of analyses used gross income-relative FPL definition to be consistent with the existing literature and are available upon request.

The AGI statistical match was done mainly on household heads. I logically assigned the imputed AGI to household members since the eligibility decision is generally made at the household level. The results do not change if only household heads were used. Rather, the standard errors improved and provided more robust results, providing indirect evidence of measurement error. Thus, the results presented here are conservative.

Another source of potential measurement error was the difference between predicted and actual income earned by families. Families and individuals had to project income they would earn the following year when signing up for HI on the exchanges to determine tax credit and cost-sharing subsidy eligibility. There was likely variation between expected and actual income. Because individuals had to pay back advanced tax credits and cost-sharing subsides if actual income was above eligibility thresholds, there was little incentive to cheat when projecting income. It is assumed that these variations are random. To account for these random differences in projected and actual income, all models excluded observations within 2.5% FPL of the cutoff to conservatively estimate the policy effects. The results were not sensitive to alternative models that including observations within 2.5% FPL

The estimation approach logically separated the sample into two groups: expansion and non-expansion states. The 138% cutoff only applied to expansion states, and the 100% FPL cutoff to non-expansion states, dictating separation when examining the lowest cutoff. Furthermore, nearly all states that expanded Medicaid adopted an SBE or partnership arrangement for a state marketplace and vice versa for the non-expansion states and the FFE.

A series of covariates were also used to control for potential confounding factors: age, gender, race, ethnicity, marital status, family size, living in a metropolitan statistical area, education, self-reported health status, Census region, and state of residence. Age and family size were treated as continuous variables, while binary indicators were used for the remaining individual controls.

Empirical Methods

The empirical approach was an RD design, using the 400%, 250%, and 100%/138% FPL cutoffs as exogenous forcing variables.* RD was first estimated non-parametrically using local linear regression with a triangle kernel density estimator. Secondly, RD was estimated using a parametric, linear specification where the FPL cutoff and the continuous FPL measure interacted. The following equation for parametric specification references the 400% FPL cutoff:

$$HI_i = \alpha + \beta_1 SUB(FPL < 400)_{\,i} + \beta_2 FPL(x - 400)_i + \beta_3 SUB(FPL < 400)_{\,i} * FPL(x - 400)_i + \delta \boldsymbol{X}_i + \tau + \varepsilon_i$$

* Another study used this design for the 2006 Massachusetts reform (Hinde, in press). Chandra, Gruber, and McKnight (2010) also used FPL cutoffs to examine the effects of the Massachusetts reform on insurance premiums and health care utilization.

where *HI* is a binary HI indicator, and *SUB* is a binary indicator for below 400% FPL, *FPL* is centered at 400% FPL, $\boldsymbol{X}$ is a vector of individual demographics described above, and τ are year fixed effects. ε_i is assumed to be an independently and identically distributed error term. β_1 represents the treatment effect at the discontinuity. The nonparametric model estimates the equivalent of β_1 but without imposing linearity. I report detailed treatment effects for IPI—the category directly affected by the tax credits and cost-sharing subsidies—in Tables 3 through 5, as well as estimates for ESI and PHI. The above equation was also estimated for 2010–2012 period separately and presented in the appendix.

To test for improvements in fit of the parametric form, I used higher order FPL terms in the parametric model. Models were estimated with and without the vector of individual-level controls. The models were not generally sensitive to higher order terms or covariate inclusion. Standard errors were clustered on the FPL for all models (Lee & Card, 2008). Results were also not sensitive to robust standard errors or standard errors clustered at the state level. All models used ASEC supplement probability weights.

Five sensitivity and falsification tests were used to test the robustness of the results (Imbens & Lemieux, 2008). First, I used a search procedure to move the cutoff around arbitrarily and test for treatment effects. The "false" cutoffs should have smaller treatment effects in absolute magnitude and smaller test statistics than the actual cutoff (Imbens & Lemieux, 2008). The cutoff is arbitrarily moved from 38% FPL to 238% FPL, 150% FPL to 250% FPL, and 300% FPL to 500% FPL in 5% increments, and potential discontinuities were examined at each arbitrary cutoff.

Second, different bandwidths around the cutoffs were tested to examine the sensitivity of the results to bandwidth selection. There is no theoretical guidance on optimal bandwidth selection. There is a tradeoff between bias and precision in determining the bandwidth: wider bandwidths are more likely to be biased and are more precise, whereas narrower bandwidths are less likely to be biased and are less precise. The selected bandwidth was 75%, and the bandwidth was allowed to vary between 25% and 100%.

Third, I examined nonrandom heaping with the FPL, a concern raised by Barecca et al. (2011, 2012; see also Almond et al., 2011). This test deals with the fact that respondents tend to report income in $1,000 or $10,000 increments, potentially leading to blips in the disaggregated data series. This is distinct from a discontinuity in the density of the sample distribution, which may indicate manipulation of the forcing variable. Nonrandom heaping close to the cutoff can potentially bias the treatment effects. Barecca et al. (2011) recommend a donut-hole RD, where the heap is dropped from the estimation procedure. The exclusion of observations within 2.5% FPL constituted a donut-hole RD.

Fourth, I estimated the McCrary (2008) test for discontinuities in the distribution of the forcing variable, which assesses a violation of the continuity assumption of the forcing variable and potential manipulation. Finally, I examined concurrent discontinuities in covariates at the cutoff that could threaten identification.

RESULTS

Main Results

Table 2 presents summary statistics around each cutoff for expansion and non-expansion states. Across all states, any HI and ESI increased greatly as income increased, while IPI decreased slightly from the lower to higher incomes. Across both expansion and non-expansion states, IPI was similar at each cutoff. The main difference in HI measures across expansion and non-expansion states was the 21% PHI coverage around 138% FPL in expansion states. There were some minor differences in other demographic characteristics between lower and high incomes. Namely, as income increased individuals were more likely be older, married, white and well-educated.

To begin to tease out the effects of the combined tax credits and cost-sharing subsidies, Figure 2 shows IPI coverage across the FPL distribution. The symbols represent the unconditional proportion covered by IPI within a 10% FPL bin. Figure 2 also imposes splines between the cutoffs to visualize potential treatment effects near the cutoffs.

For expansion states in the top panel of Figure 2, IPI hovered at 0.10 up to 138% FPL. There was noticeable increase in the scatter plot just above 138% FPL and the splines suggested a large, positive effect on IPI coverage. Beyond 138% FPL, the scatter plot and splines trended downward until 400% FPL where it appeared to flatten out. There was not visual evidence of a treatment effect near 250% in the scatter plot, but the splines did indicated a small, negative effect just below 250% FPL. Near 400% FPL, the splines indicated a small, positive effect. The scatter plot suggested that the break may have occurred slightly lower than 400%, at approximately 390% FPL.

For non-expansion states in the bottom panel of Figure 2, the plot looked quite similar to expansion states. There was an apparent effect just above 100% FPL, similar in magnitude to the effect above 138% FPL in expansion states. Between 100% FPL and 250% FPL, the IPI trend declined until it flattened out above 250% FPL. There was a small, positive effect just below 250% FPL, although it appears there may have been larger break in the trend around 260% FPL. There was no visual evidence of an effect just below 400% FPL.

Statistical estimates of the treatment effects are presented in Table 3. The combined treatment effect just above 138% FPL in expansion states was 7.7 percentage points in the non-parametric model and 6.9 percentage points in the linear model. Both estimates were statistically significant. Among the non-expansion states, the treatment effect just above 100% FPL was a smaller 3.6 percentage points and only significant at the 10% level. Still, the combined benefit of reduced premiums and co-insurance provided a strong incentive at lower income levels. Given the positive correlation between SBE adoption and Medicaid expansion, the difference between expansion and non-expansion states provided convincing evidence that outreach, assistance, and framing efforts of marketplaces significantly affected uptake of IPI. Likewise, this also suggested that individuals just ineligible for Medicaid were effectively recruited to subsidized private coverage.

Confirming the visual evidence in Figure 2, I did not find evidence of a cost-sharing treatment effect for IPI just below 250% FPL. For expansion states, there was an insignificant 2 percentage point reduction in IPI just below 250% FPL. Contrary to the visual evidence of a positive effect just below 400% FPL in expansion states, the statistical estimate was positive but small and insignificant. There were negligible differences at both 250% and 400% cutoffs in non-expansion states. Based on the visual evidence, I tested other cutoff points in the region below 400% FPL in expansion states and above 250% FPL in non-expansion states. I did not find meaningful statistical estimates of a treatment effect. A last model focusing solely on the SBE states found a roughly 4 to 5 percentage point increase in IPI just below 400% FPL and the effect was significant at the 10% level.

In summary of the IPI results, I found strong evidence of a combined effect of the tax credits/cost-sharing subsidies just above 138% FPL in expansion states and slightly less robust evidence of a combined effect just above 100% FPL in non-expansion states. The tax incentives were strongest at the lower FPL as were the future medical consumption benefits. The stronger effects in expansion states suggest that consumer participation was aided by state-funded outreach and assistance. Although there was tantalizing visual evidence of a potential cost-sharing effect in non-expansion states and tax credit effect in expansion states, there was not robust statistical evidence to support a cost-sharing effect and only suggestive evidence of a tax credit effect in SBE states. Loosely, the positive effects found for the combined incentive and tax credits-only implied that the tax credits could be the driving incentive for consumers on the margin.

Beyond IPI coverage, I examined changes in ESI and PHI at the same three cutoffs in expansion and non-expansion states. Figure 3 shows ESI coverage across the FPL distribution. ESI coverage increased greatly as the FPL increased, but there was little evidence of any jumps around the cutoffs in either state grouping. For expansion states, the RD estimates in Table 3 were relatively small and insignificant at each cutoff. In non-expansion states, RD estimates were larger at just above 100% FPL, an approximately 3 to 4 percentage point increase, and an approximately 4 to 5 percentage point decrease just below 250% FPL. The estimate near 250% FPL was statistically significant at the 10% level only in the non-parametric model.

The last set of main models explored potential effects on PHI near the cutoffs. According to Figure 4, there was a large drop-off in PHI just above 138% FPL in expansion states, and no effects at the other two cutoffs. There was little evidence of treatment effects for PHI at any of the cutoffs for non-expansion states. The RD results in Table 3 showed an approximately 6.1 percentage point drop in PHI in expansion states and little elsewhere. The negative effect for PHI in expansion states just below 138% FPL was expected because individuals were not eligible for the expanded Medicaid program above 138% FPL.

Looking across all three outcomes for expansion states in Table 3, there was no change in the insured rate just above 138% FPL from the non-parametric model and a small net decrease in the insured rate from the linear model. Non-expansion states had a net increase in the insured rate just above 100% FPL. The overall changes in the insured rate were not statistically significant, and for expansion states, suggested a minimal level of crowding out from Medicaid expansion.

As a validity check, a separate set of analyses reproduced the main results for the 2010–2012 period, available in Appendix Table A.1 and Appendix Figures A.1–A.3. There were no effects near 138% or 100% FPL in the pre-period. There was statistical evidence of a 1.6 percentage point increase in IPI just below 250% in the pre-period in non-expansion states, but not visual evidence. When disaggregated by year, the effect dissipated. There was also statistical evidence of an approximately 3.8 percentage point increase in ESI just below 400% FPL in expansion states, which again dissipated when disaggregated by year. Given the lack of visual evidence, the sensitivity of the effects across years, and the fact that these effects were concentrated in the pre-period at the cutoffs where there were no 2014 effects, there was little concern that the design was invalid.

Who Used the Incentives?

Long-term sustainability of the marketplaces is in many ways tied to conformation by younger, healthier individuals to diversify the risk pool of the exchanges. To test whether the effects observed above 138% FPL in expansion states and above 100% FPL in non-expansion states were differences across marital status, adverse selection or age differences. I stratified the models in Table 3 by relationship status, self-reported health status, and age group. The estimates are presented in Table 4 for expansion States and Table 5 for non-expansion states.

Starting with expansion states, the combined effect of the tax credits/cost-sharing subsidies for IPI was slightly higher for married (approximately 8 percentage points) than non-married individuals (7-8 percentage points), but not practically different. Rather, there were differences across ESI and IPI. A PHI reduction of 13 percentage points was estimated for married individuals, whereas non-married individuals had a reduction in ESI of approximately 8.5 to 9.5 percentage points. The PHI drop-off is consistent with Medicaid ineligibility, but the ESI drop-off for single individuals was unexpected. This could be evidence of switching away from ESI toward IPI. Looking across the three outcomes, there was a net increase in the insured rate for married individuals and a net decrease in non-married individuals.

The next stratification was by self-reported health status, comparing individuals who reported being in excellent or very good health against individuals who reported being in good, fair, or poor health. Referring back to Table 2, there were too few individuals in fair and poor health to analyze separately. When stratified by health status, the combined effect was unchanged for the higher self-reported health group, and somewhat attenuated for the lower self-reported health group. A reduction in PHI was observed only for the lower self-reported health group. Overall, there was a net increase in the insured rate among the higher self-reported health group and a net decrease in the lower self-reported health group. Although the lower self-reported health group may be under-represented in this sample, there was no evidence of adverse selection in IPI take-up.

The bottom portion of Table 4 compared the combined effect for individuals aged 26 to 39 and individuals aged 40 to 64. There was little difference in the approximately 6.1 to 6.9 percentage point effect across groups. The older group had a reduction in PHI between 9.2 and

10.9 percentage points attributable to Medicaid ineligibility. Overall, the younger group saw a net increase in the insured rate, whereas older individuals saw a net decline.

Table 4 provided three implications. First, there was no evidence of differential policy effects on IPI by marital status and age group. Perhaps surprisingly, the effect was slightly larger among self-reported healthier individuals. Second, there was an interesting dynamic of non-married individuals dropping off ESI coverage just above 138% FPL. Third, the non-married, lower self-reported health, and older age groups saw net declines in the insured rate that were associated with Medicaid ineligibility. In one sense, the results suggested that the desired effect of incentivizing, young, single and healthy individuals worked. In another sense, the net decrease in the insured rate for potentially vulnerable groups, such as those in poor health or of older age, suggested that affordability could still be an issue.

For the non-expansion states in Table 5, there were less interesting findings. The combined effect for IPI was higher among married individuals, individuals reporting a higher health status, and the 26- to 39-year-old age group. The effects for the latter two groups were statistically significant at the 10% and 5% levels, respectively. The effects for non-married and 40- to 64-year-old respondents were between 1.8 and 2.8 percentage points but not statistically significant, and the effects for the low self-reported health status group were negligible. The non-expanding states did not have dynamic effects for ESI or PHI, and all groups except for the low self-reported health status group saw a net increase in the insured rate.

HI Premiums and Medical Spending

The results so far have focused largely on the extensive margin of obtaining IPI. This next section examined the impacts on premiums and OOP medical expenditures. The limited sample size in the CPS prevented in-depth statistical examination of the impact on premiums and

OOP medical expenditures conditional on having IPI. Instead, descriptive results are presented. Figures 5 and 6 graphically present the average non-zero log HI premiums and log OOP spending for IPI-covered individuals before and after the exchanges and incentives went into effect in 2014, along with the splines checking for discontinuities. The cost measures have not changed and are comparable across time, but are generally noisy.

Figure 5 showed that IPI premium payers in 2014 had lower average log premiums than 2010–2012 payers across the FPL distribution in both expansion and non-expansion states. In expansion states, the splines suggest large drop-offs after each cutoff, although they were not statistically significant. The pattern was similar in non-expansion states, except for a smooth trend across 250% FPL. The pre-periods did not exhibit large changes near any of the cutoffs. The reduction in log premiums just above 138% FPL/100% FPL was consistent with receipt of the tax credits, while the increase just below 250% and 400% FPL was not. The increases in average log premiums just below the 250% and 400% FPL cutoffs instead suggested maximizing behavior. For individuals just below 250% who were eligible for the cost-sharing subsidies, they could have been willing to pay higher up-front costs for HI to get reduced costs for future medical consumption. Figure 6 provided suggestive evidence for this hypothesis, showing that while log OOP expenditures were lower across time, there was a large increase in log OOP expenditures just below 250% FPL in expansion states in 2014. Another explanation was adverse selection: while the extensive effect of obtaining IPI was minimal just below 250%, the intensive effect may have been more pronounced.

Although statistical tests were imprecise, the reduction in premiums and OOP expenditures was clear visually across the FPL distribution in comparison to pre-2014 data. There was suggestive visual evidence for premium reductions above 138% FPL in expansion states and 100% FPL in non-expansion states. The demographic stratifications did not suggest adverse selection on the extensive margin, but the effects below 250% FPL were weakly suggestive of adverse selection on the intensive margin. Secondarily, the increase in premiums just below 400% in expansion states was suggestive of maximizing behavior in plan choice. Even though the premiums increased for this group, OOP expenditures were still lower compared to the pre-period. This was suggestive of broader welfare benefits to consumers.

Labor Market Impacts

As a secondary analysis, I examined labor market outcomes at each of the cutoffs. The statistical estimates are not formally presented but available upon request. There was little evidence of any impact on labor force participation, unemployment, self-employment, part-time status, and whether the individual switched jobs. Overall, there did not appear to be any negative, distortionary effects on labor force participation and unemployment. Likewise, potential positive effects of increased self-employment and job mobility were not found.

Robustness Checks

I implemented a wide range of robustness checks and sensitivity analyses to attempt to refute the main HI results presented in the previous section. Results from all robustness checks are summarized here. A selection of figures and tables for robustness checks are included in the appendix and full results available upon request. The first robustness test involved arbitrarily moving the cutoff around the FPL distribution to create false cutoffs. The cutoffs near 138% or 100% FPL, 250%, and 400% FPL should have had the largest effect size in absolute magnitude and the largest test statistic. There were no other large effects in the FPL range around 138% FPL for IPI in expansion states or 100% FPL in non-expansion states (see Appendix Figure A-4). Near 250% FPL for IPI in both expansion and non-expansion states, the permutation test was suggestive of a positive effect in the range of 230% to 245% FPL (see Appendix Figure A-5). The permutation coefficients near 400% FPL were consistently small and insignificant for non-expansion states and positive just below 400% FPL in expansion states, in the 385% to 395% FPL range (see Appendix Figure A-6). Among the ESI and PHI outcomes, the permutation testing did not alter interpretation of the main results at any cutoff.

The second robustness test altered the bandwidth for the model, ranging from 25% FPL on either side of the cutoff to 100% FPL on either side of the three cutoffs. There is not robust guidance on the appropriate bandwidth to use with an RD design. Should the results be sensitive to the bandwidth, it may cast doubt on the design. The results were appropriately sensitive to bandwidth selection (see Appendix Figure A-7). Coefficient magnitude was at least constant or decreasing in absolute magnitude as the bandwidth increased. At wider bandwidths, the combined effect of the tax credits and cost-sharing subsidies in non-expansion states were statistically significant at the 5% level.

The third robustness test assessed non-random heaping. Assessing heaping in this context was more complex due to the calculation of FPL. Heaping might normally occur with income in discrete increments (e.g., individuals round to $5,000 increments). Because income-relative FPL was calculated by dividing income by the poverty threshold, income heaping could not be directly assessed. I assessed bunching using disaggregated scatter plots across FPL ranges for each outcome and cutoff and assessed this against histograms around the cutoff. I did not find evidence of heaping.

The fourth robustness test—checking for discontinuities in the FPL sample distribution at the cutoff—was estimated using McCrary's test (2008) and assessing visual evidence from histograms. There was not visual evidence of mass points occurring near the cutoffs that would indicate manipulation (see Appendix Figure A-8). The distribution was noisy in both periods, but there were no systematic visual breaks in the data. The McCrary test indicated a small and statistically significant break at 250% FPL for expansion states, although a histogram did not show signs of bunching. I report the results based on recommendations from the literature, but the McCrary test generally performed poorly. I arbitrarily ran the McCrary test at a wide range of FPL points in both the 2014 and 2010-2012 periods and a majority were statistically significant. It appears the McCrary test was picking up the noise in the distribution and not systematic manipulation near the FPL cutoffs.

The fifth and final robustness test examined potential effects of demographic shifts near the cutoff. There was little visual evidence of demographic breaks near the cutoffs, but three demographic characteristics did have statistically significant differences in a few models: race/ethnicity, marital status, and family size. The proportion of non-white and Hispanic, not currently married, and average family size were noisy and decreasing in FPL in both expansion and non-expansion states, which helped to explain why some models pick up a statistically significant effect. More importantly, the effects were small and there was no visual evidence of a demographic shift near any of the cutoffs.

In summary of the five robustness tests, there was little evidence to draw serious concerns about the design. Beyond the robustness tests, these analyses still have several limitations. First, there were several sources of measurement error: statistically matched AGI, logical imputation of AGI to families, and projected versus actual income. It is assumed that these were cases of classical measurement error that magnified the standard errors and did not introduce bias. To check for sensitivity to AGI definition, I re-estimated the models using a gross income-based FPL definition. The gross income definition yielded results that were only significant for the tax credit effect just below 400% FPL for IPI (available upon request). The treatment effect was similar in magnitude to the AGI definition results. The narrative was quite similar for cost-sharing effects near 250% FPL, and there were no combined effects at 138% FPL using the gross income definition.

When the AGI definition was used without logical imputation for non-household heads, the IPI results for the combined effect in expansion states were in a similar magnitude and the combined effects above in non-expansion states were more robust. This was suggestive of classical measurement error. The final source of measurement error—projected versus actual income—could not be addressed with the CPS. Given the incentives against cheating in the tax code, the possibility of non-random measurement error was likely weakest.

A second limitation was that this data did not directly measure receipt of tax credits and cost-sharing subsidies or capture whether IPI was obtained through the exchanges. I assumed that the cutoffs were binding and the demand for non-exchange coverage did not correlate with the ACA cutoffs. It was possible that non-exchange IPI coverage was wrapped up in the estimates. Built into this limitation was also the fact that the CPS income and HI questions were redesigned recently to better capture income and HI dynamics. Respondents could have potentially confused IPI coverage obtained through SBE exchanges or the FFE as PHI. As an anecdotal example, Kentucky and Colorado branded their exchanges as to not be associated with "Obamacare."

As a final limitation, while the CPS provided a large sample size overall, using only 2014 data limited the relative sample size within FPL bins. The estimates could potentially be improved by additional years of data. The visual and statistical evidence supported the main results of a combined effect, but more data is always better.

DISCUSSION

This analysis examined the effectiveness of ACA tax credits and cost-sharing subsidies implemented in 2014 to increase IPI coverage and found robust, positive effects of the combined tax credit/cost-sharing effects above 138% FPL in expansion states and above 100% FPL in non-expansion states. This was a combined effect because consumers were initially eligible for tax credits and cost-sharing subsides just above 138%/100% FPL. The tax credit amount was highest and the cost-sharing subsidy was most valuable at lower income levels, so it made sense that the effects were largest where the incentives were strongest. Perhaps surprisingly, there was not a detectable effect near the 250% FPL cutoff, above which consumers lost eligibility for the cost-sharing subsidies. There was suggestive evidence of an effect just below the 400% FPL cutoff, concentrated in SBE states, which implied that the policy effect of tax credits drove the results.

Despite the limitations noted in the previous section, the broad story painted by these estimates was a positive narrative of the initial effects of the combined incentive for lower income individuals. The difference in effect size and significance between expansion and non-expansion states also highlighted previously identified coverage gaps among states opposing federal ACA policies. The stronger results in expansion/SBE states was also consistent with early evidence on the impact of messaging and outreach among states actively supporting the ACA components (Cox et al., 2015). For individuals who were not eligible for Medicaid, just above 138% FPL, outreach and enrollment advocacy directly provided by the SBE may have increased enrollment.

I caution that the null results at 250% FPL did not suggest ineffectiveness of the cost-sharing subsidies overall. Rather, the results indicated that perceived future consumption benefits from cost-sharing subsidies were not differentiable from the premium tax credit. If at all, the insignificant negative effects were suggestive of risk aversion. The cost-sharing subsidies could have provided significant medical care consumption benefits and effects on wealth. Given the possibility of wealth effects, the potential loss of these benefits may have acted as a disincentive to participation for more risk-averse individuals close to 250% FPL

At 400% FPL, there was suggestive evidence in SBE states of a tax credit effect. Separate analyses that used the gross income calculation of FPL indicated a strong tax credit effect (available upon request). As noted earlier, roughly half the population in the FPL range around 400% FPL using gross income had a much lower AGI. Much of this movement was concentrated among non-household heads and non-single tax filers. Among household heads and single tax filers, the difference between AGI and gross income was only a few thousand dollars. This limits the conclusions that the main results here were solely reflective of consumers with gross incomes near 400% having an AGI conversion closer to 138% FPL; a contamination effect. Rather, possible explanations for the insignificant effects near 400% FPL using AGI include measurement error concerns from logical imputation at the household level and reduced power from a loss of sample size. Regardless of definition, the results loosely implied that the tax credit was driving IPI increases.

This analysis assumed negligible effects of the individual mandate penalty in 2014. After 2015, the penalty increased significantly. Because the mandate penalty was also on a sliding scale, higher incomes were much more susceptible to the increase in the penalty and future studies should consider whether the countervailing effects of the individual mandate penalty increased the appeal of IPI.

Furthermore, the tax credits and cost-sharing incentives did not increase over time, while the marketplace prices increased. If there were not visible effects in this design below 250% FPL and only weak effects below 400% FPL, the increased mandate penalty could be outweighed by the price increases on the marketplace. The results from this study imply that the long-term impact beyond lower-income groups could be minimal unless the individual mandate is binding.

Finally, no changes in labor market outcomes were found around the cutoffs. This finding is consistent with other recent studies on the impacts of the ACA on labor markets (e.g., Gooptu et al., 2016). Given the precedence of income-based transfers affecting labor supply on the extensive and intensive margins, the null finding had a positive spin for the ACA. In the short-term, there was no evidence of distortionary impacts on labor force participation or unemployment to gain access to the ACA incentives. Likewise, reductions in job lock were not captured in this study. Future studies should examine whether long-term effects on labor market outcomes accrue.

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TABLES

Table 1. ACA Program Eligibility

FPL Range	Cost-Sharing Subsidies	Premium Tax Credits	Expanded Medicaid Eligibility
0-99%	N	N	**Y***
100-138%	**Y**	**Y**	**Y***
138% -250%	**Y**	**Y**	N
251-400%	N	**Y**	N
>400%	N	N	N

Notes: * only applies to states that expanded their Medicaid program.

Table 2. Weighted Summary Statistics

	Expansion States						Non-Expansion States					
	138% FPL N=6,227		250% FPL N=5,372		400% FPL N=4,169		135% FPL N=7,238		250% FPL N=6,890		400% FPL N=5,050	
	Mean	**SE**	**Mean**	**SE**	**Mean**	**SE**	**Mean**	**SE**	**Mean**	**SE**	**Mean**	**SE**
Any Health Insurance	0.78	(0.784)	0.88	(0.005)	0.95	(0.004)	0.70	(0.006)	0.84	(0.005)	0.94	(0.004)
Any IPI	0.12	(0.121)	0.11	(0.005)	0.08	(0.005)	0.12	(0.004)	0.09	(0.004)	0.08	(0.004)
Any ESI	0.45	(0.452)	0.70	(0.007)	0.85	(0.007)	0.48	(0.007)	0.71	(0.006)	0.85	(0.006)
Any Public Insurance	0.21	(0.211)	0.07	(0.004)	0.02	(0.003)	0.10	(0.004)	0.04	(0.003)	0.02	(0.002)
Age	41.67	(41.673)	42.53	(0.183)	43.88	(0.205)	41.55	(0.152)	43.25	(0.156)	43.82	(0.178)
Female	0.54	(0.541)	0.51	(0.008)	0.51	(0.009)	0.55	(0.007)	0.51	(0.007)	0.50	(0.008)
Race												
White	0.73	(0.728)	0.77	(0.007)	0.81	(0.007)	0.75	(0.006)	0.79	(0.006)	0.83	(0.006)
Black	0.12	(0.121)	0.10	(0.005)	0.07	(0.005)	0.18	(0.005)	0.15	(0.005)	0.11	(0.005)
Other/Multiple Race	0.15	(0.151)	0.13	(0.005)	0.12	(0.006)	0.07	(0.004)	0.07	(0.004)	0.06	(0.004)
Hispanic	0.37	(0.365)	0.22	(0.006)	0.12	(0.006)	0.28	(0.006)	0.17	(0.005)	0.10	(0.005)

Marital Status												
Currently Married	0.50	(0.504)	0.57	(0.008)	0.68	(0.009)	0.50	(0.007)	0.59	(0.007)	0.72	(0.008)
Previously Married	0.18	(0.184)	0.17	(0.006)	0.12	(0.006)	0.22	(0.006)	0.20	(0.006)	0.14	(0.006)
Never Married	0.31	(0.312)	0.27	(0.008)	0.19	(0.008)	0.28	(0.006)	0.21	(0.006)	0.14	(0.006)
Household Size	3.44	(3.440)	2.97	(0.027)	2.82	(0.026)	3.24	(0.024)	2.88	(0.022)	2.82	(0.023)
Education												
Less than HS	0.20	(0.197)	0.08	(0.004)	0.03	(0.003)	0.19	(0.005)	0.08	(0.004)	0.04	(0.003)
HS Diploma/GED	0.33	(0.334)	0.30	(0.007)	0.22	(0.008)	0.36	(0.007)	0.33	(0.007)	0.24	(0.007)
Some College	0.18	(0.183)	0.20	(0.007)	0.16	(0.007)	0.18	(0.005)	0.19	(0.006)	0.16	(0.006)
Associate's Degree	0.10	(0.104)	0.13	(0.006)	0.14	(0.007)	0.10	(0.004)	0.13	(0.005)	0.14	(0.006)
Bachelor's Degree	0.13	(0.132)	0.21	(0.007)	0.29	(0.008)	0.12	(0.005)	0.19	(0.006)	0.27	(0.007)
Graduate Degree	0.05	(0.050)	0.07	(0.004)	0.16	(0.007)	0.04	(0.003)	0.07	(0.004)	0.14	(0.006)
Self-Rated Health Status												
Excellent	0.25	(0.246)	0.28	(0.007)	0.32	(0.009)	0.25	(0.006)	0.28	(0.006)	0.33	(0.008)
Very Good	0.35	(0.346)	0.37	(0.008)	0.43	(0.009)	0.34	(0.006)	0.38	(0.007)	0.39	(0.008)

Good	0.33	(0.325)	0.29	(0.007)	0.21	(0.008)	0.32	(0.006)	0.28	(0.006)	0.23	(0.007)
Fair	0.07	(0.071)	0.05	(0.004)	0.03	(0.003)	0.08	(0.004)	0.06	(0.003)	0.04	(0.003)
Poor	0.01	(0.012)	0.01	(0.001)	0.00	(0.001)	0.01	(0.001)	0.01	(0.001)	0.00	(0.001)

Notes: Data are drawn from the IPUMS-CPS. All means are weighted using the ASEC supplemental probability weights ESI = Employer-Sponsored Insurance; IPI = Directly Purchased Private Insurance; FPL= Federal Poverty Level.

Table 3. Regression Discontinuity Estimates at 138% FPL/100% FPL, 250% FPL and 400% FPL for HI Outcomes, 2014

	Expansion States				Non-Expansion States		
138% FPL				**100% FPL**			
N=5,998	**IPI**	**ESI**	**PHI**	N=6,285	**IPI**	**ESI**	**PHI**
Non-parametric	0.077***	−0.013	−0.061**	**Non-parametric**	0.036*	0.037	−0.012
	(0.021)	(0.032)	(0.025)		(0.020)	(0.029)	(0.020)
Linear	0.069***	−0.027	−0.060**	**Linear**	0.035	0.028	−0.017
	(0.023)	(0.039)	(0.027)		(0.022)	(0.032)	(0.022)
250% FPL				**250% FPL**			
N=5,141				N=6,608			
Non-parametric	−0.019	0.020	−0.014	**Non-parametric**	0.007	−0.048*	−0.017*
	(0.021)	(0.032)	(0.015)		(0.017)	(0.027)	(0.010)
Linear	−0.020	0.024	−0.012	**Linear**	0.008	−0.039	−0.018
	(0.026)	(0.036)	(0.018)		(0.022)	(0.033)	(0.013)
400% FPL				**400% FPL**			
N=4,044				N=4,903			

Non-parametric	0.025	−0.020	−0.008	**Non-parametric**	−0.005	−0.009	0.013
	(0.022)	(0.030)	(0.012)		(0.020)	(0.026)	(0.010)
Linear	0.021	−0.013	0.005	**Linear**	−0.004	−0.009	−0.014
	(0.025)	(0.035)	(0.014)		(0.023)	(0.031)	(0.012)

Notes: * p<0.10, **p<0.05, ***p<0.01. Data come from the IPUMS-CPS. IPI = directly purchased private insurance. Observations within 70% on either side of the cutoff are included. Standard errors are in parentheses. Non-parametric RD is calculated using a triangle kernel. Standard errors are clustered on FPL. Each OLS model includes the cutoff indicator interacted with FPL. Models are weighted using the ASEC supplement probability weights. Covariates include age, gender, race, marital status, family size, education level, self-reported health status, MSA status, and state and year indicators.

Table 4. RD Estimates for Expansion States at 138% FPL by Key Demographics, 2014

		Expansion States					
		IPI		ESI		PHI	
138% FPL	N	Non-parametric	Linear	Non-parametric	Linear	Non-parametric	Linear
Marital Status							
Currently Married	3,145	0.082***	0.080**	0.069	0.037	−0.133***	−0.120***
		(0.028)	(0.036)	(0.042)	(0.052)	(0.035)	(0.044)
Not Married	2,806	0.076**	0.068**	−0.094**	−0.085*	0.001	−0.009
		(0.032)	(0.031)	(0.048)	(0.050)	(0.034)	(0.032)
Heath Status							
Excellent/Very Good	3,432	0.079***	0.088***	−0.042	−0.056	0.001	0.004
		(0.029)	(0.031)	(0.042)	(0.046)	(0.030)	(0.034)
Good/Fair/Poor	2,397	0.053*	0.038	−0.002	−0.021	−0.130***	−0.140***
		(0.031)	(0.035)	(0.050)	(0.056)	(0.041)	(0.044)
Age Group							
26–39	2,754	0.069**	0.061**	−0.011	−0.007	0.019	0.000

		(0.031)	(0.030)	(0.048)	(0.052)	(0.036)	(0.040)
40–64	2,992	0.066**	0.064*	−0.039	−0.061	−0.109***	−0.092**
		(0.030)	(0.036)	(0.046)	(0.052)	(0.035)	(0.038)

Notes: * p<0.10, **p<0.05, ***p<0.01. Data come from the IPUMS-CPS. IPI = directly purchased private insurance. Observations within 70% on either side of the cutoff are included. Standard errors are in parentheses. Non-parametric RD is calculated using a triangle kernel. Standard errors are clustered on FPL. Each OLS model includes the cutoff indicator interacted with FPL. Models are weighted using the ASEC supplement probability weights. Covariates include age, gender, race, marital status, family size, education level, self-reported health status, MSA status, and state and year indicators.

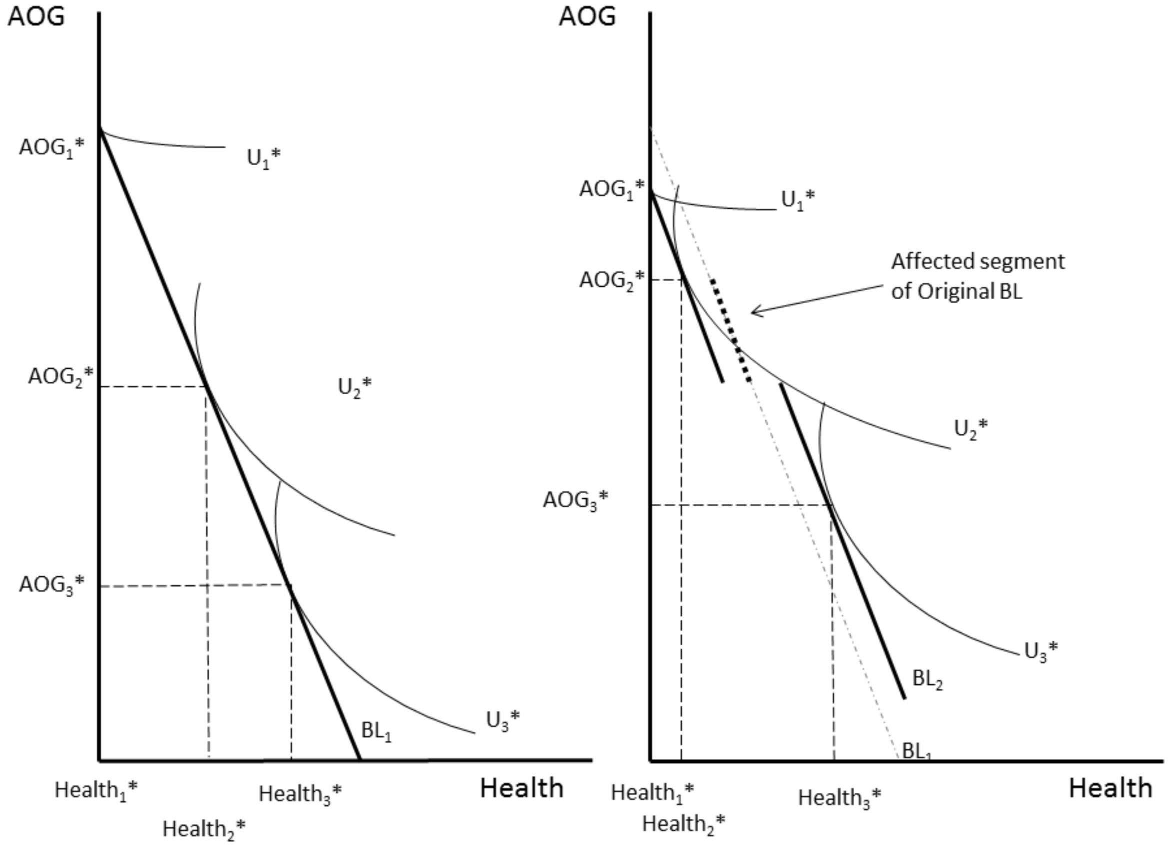
Table 5. RD Estimates for Non-Expansion States at 100% FPL by Key Demographics, 2014

		Non-Expansion States					
		IPI		ESI		PHI	
100% FPL	**N**	**Non-parametric**	**Linear**	**Non-parametric**	**Linear**	**Non-parametric**	**Linear**
Marital Status							
Currently Married	3,050	0.050	0.057	0.056	0.030	−0.032	−0.040
		(0.030)	(0.037)	(0.041)	(0.050)	(0.032)	(0.037)
Not Married	3,174	0.026	0.018	0.031	0.026	−0.000	0.006
		(0.028)	(0.028)	(0.041)	(0.040)	(0.025)	(0.025)
Heath Status							
Excellent/Very Good	3,459	0.056*	0.051*	0.065	0.056	−0.016	−0.015
		(0.029)	(0.031)	(0.041)	(0.042)	(0.026)	(0.030)
Good/Fair/Poor	2,626	−0.003	0.004	0.011	−0.001	−0.022	−0.033
		(0.029)	(0.029)	(0.044)	(0.047)	(0.032)	(0.033)
Age Group							
26–39	3,148	0.043*	0.050**	0.031	0.032	−0.037	−0.041
		(0.023)	(0.025)	(0.042)	(0.047)	(0.029)	(0.032)
40–64	2,817	0.028	0.024	0.044	0.026	0.004	0.005
		(0.036)	(0.038)	(0.045)	(0.047)	(0.029)	(0.031)

Notes: * p<0.10, **p<0.05, ***p<0.01. Data come from the IPUMS-CPS. IPI = directly purchased private insurance. Observations within 70% on either side of the cutoff are included. Standard errors are in parentheses. Non-parametric RD is calculated using a triangle kernel. Standard errors are clustered on FPL. Each OLS model includes the cutoff indicator interacted with FPL. Models are weighted using the ASEC supplement probability weights. Covariates include age, gender, race, marital status, family size, education level, self-reported health status, MSA status, and state and year indicators.

FIGURES

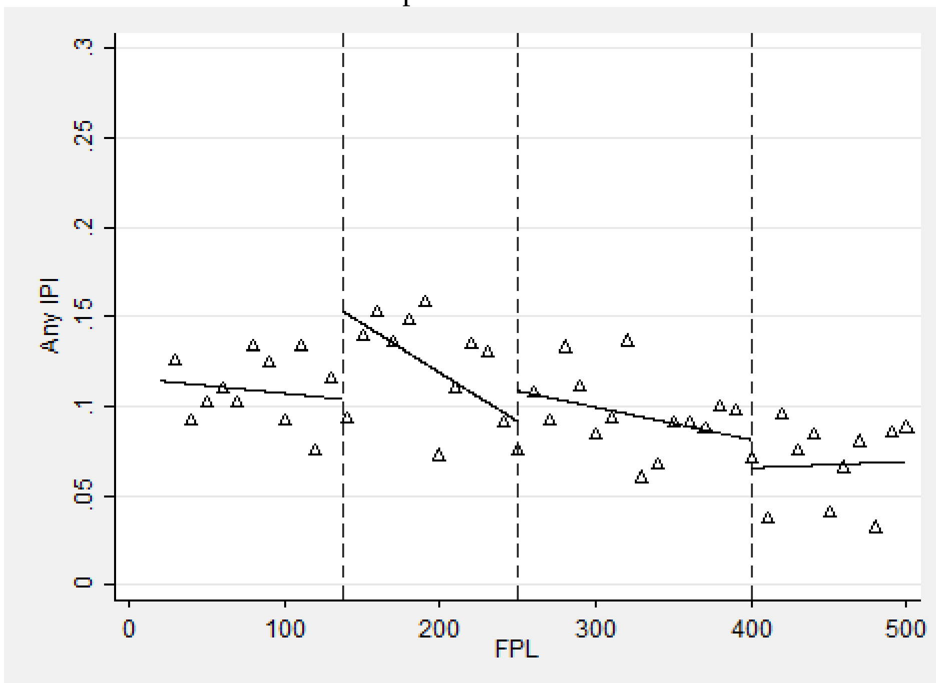
Figure 1. Graphical Analysis of ACA Tax Credit



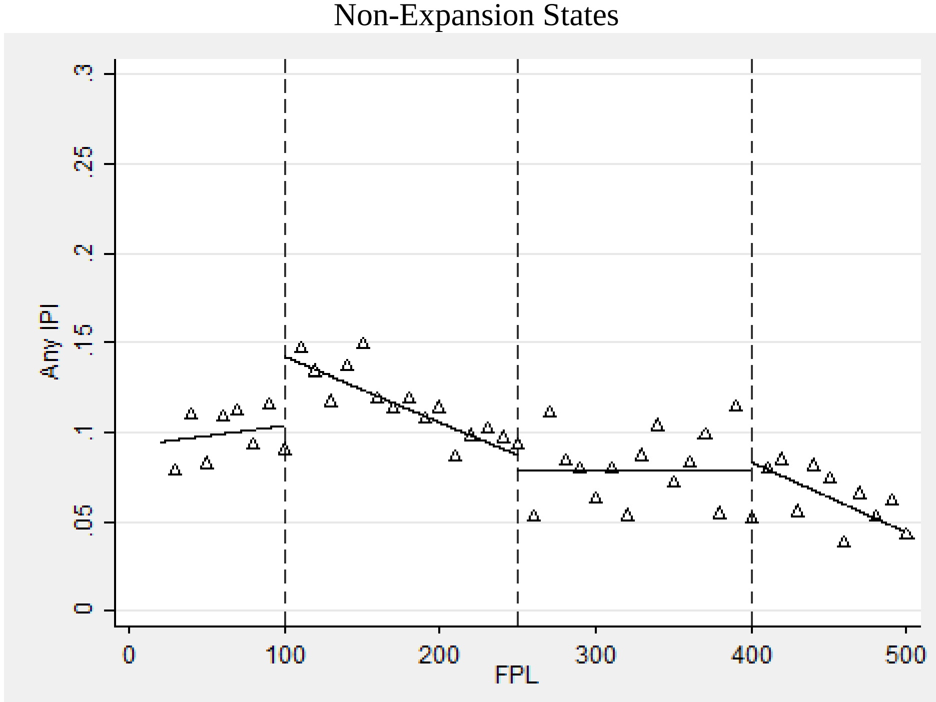
Notes: The left panel refers to a pre-2014 economy and the right panel refers to the post-2014 economy.

Figure 2. IPI Coverage by 10% FPL Bins in 2014

Expansion States

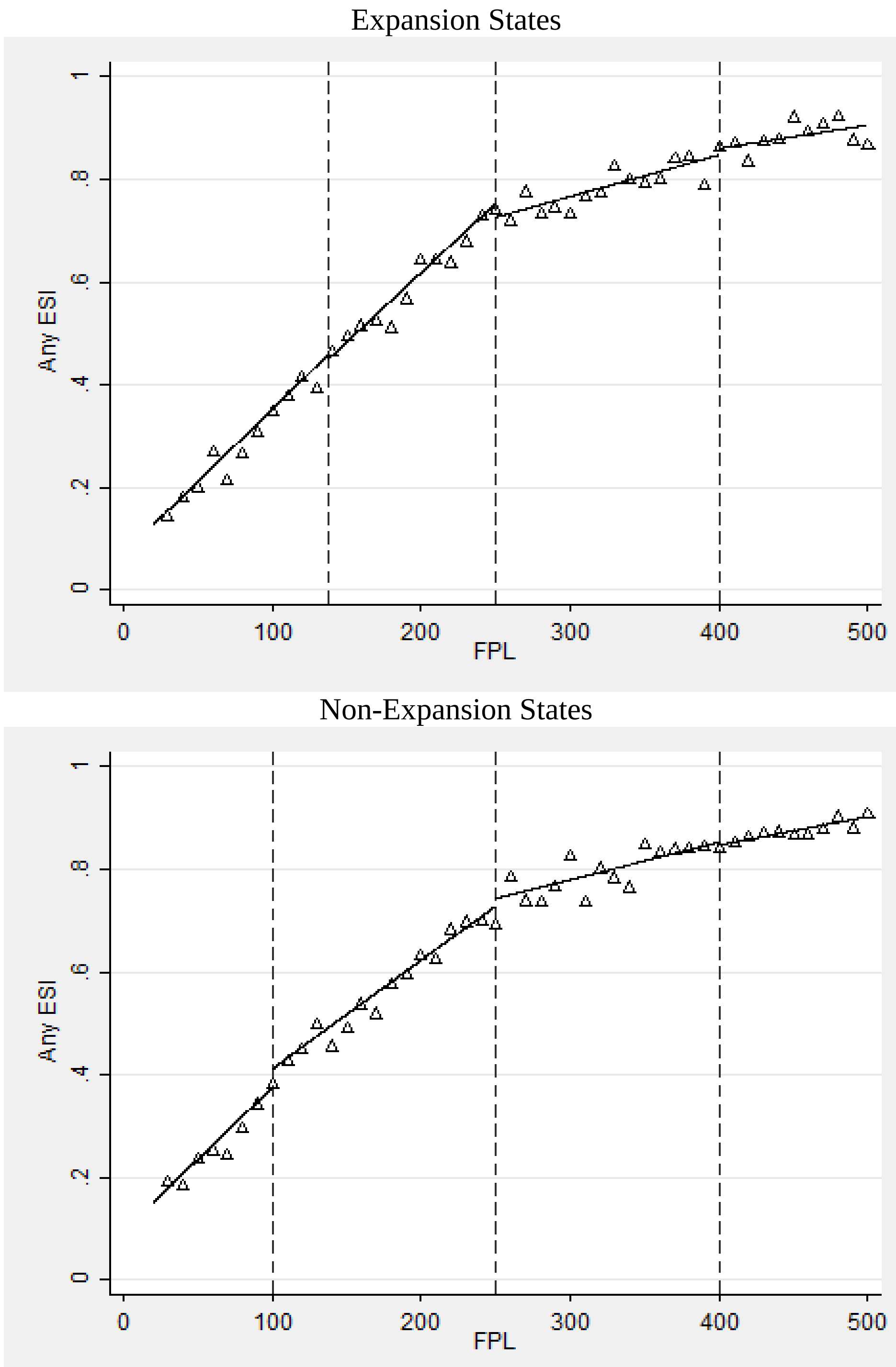


Non-Expansion States



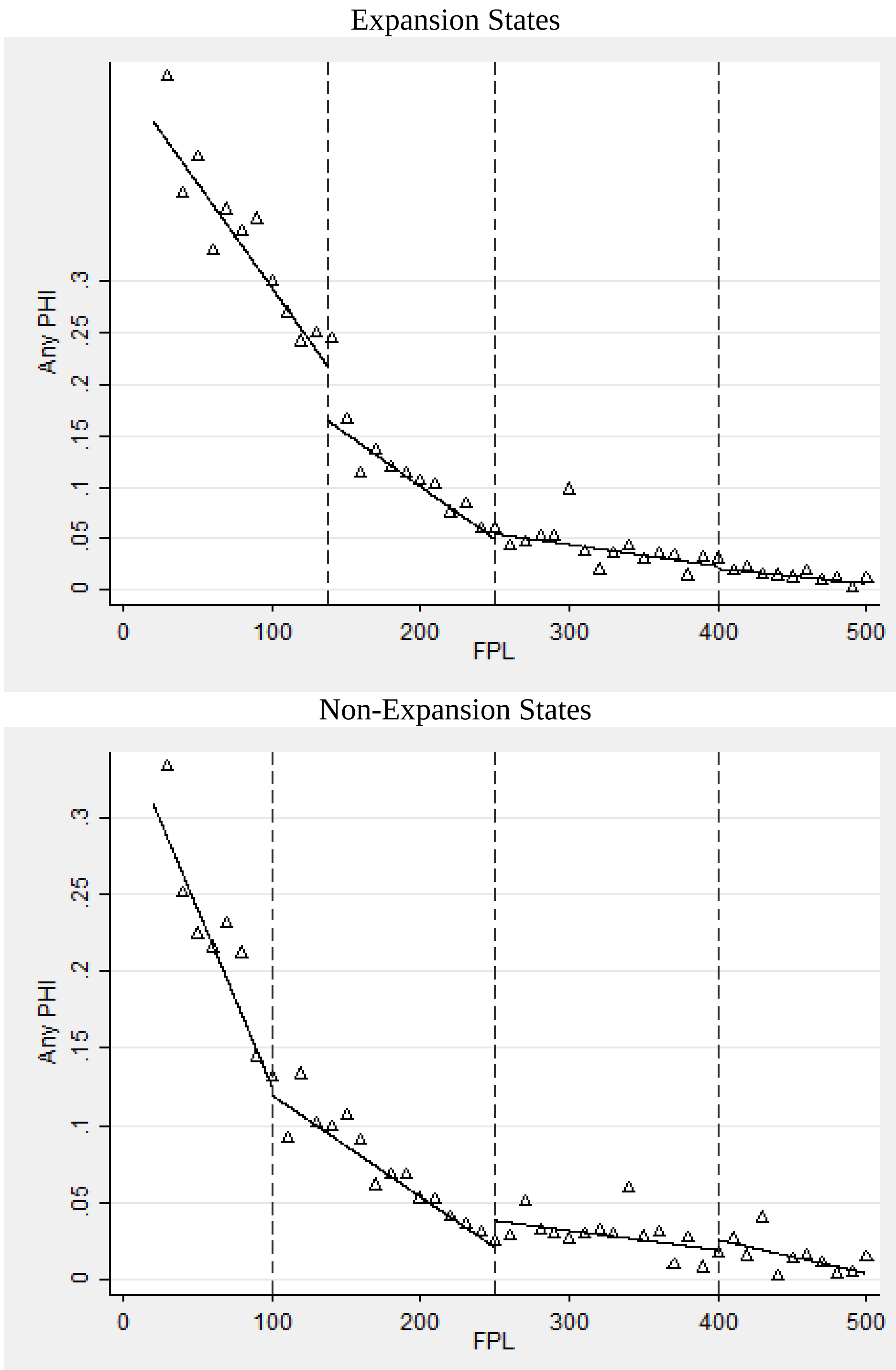
Notes: Data come from the IPUMS-CPS. Symbols represent the proportion covered in a 10% FPL bin. Vertical dashed lines represent the 138%/100%, 250% and 400% FPL cutoffs. Linear splines are imposed with steps at each cutoff. Estimates are weighted using ASEC supplement probability weights.

Figure 3. ESI Coverage by 10% FPL Bins in 2014



Notes: Data come from the IPUMS-CPS. Symbols represent the proportion covered in a 10% FPL bin. Vertical dashed lines represent the 138%/100%, 250% and 400% FPL cutoffs. Linear splines are imposed with steps at each cutoff. Estimates are weighted using ASEC supplement probability weights.

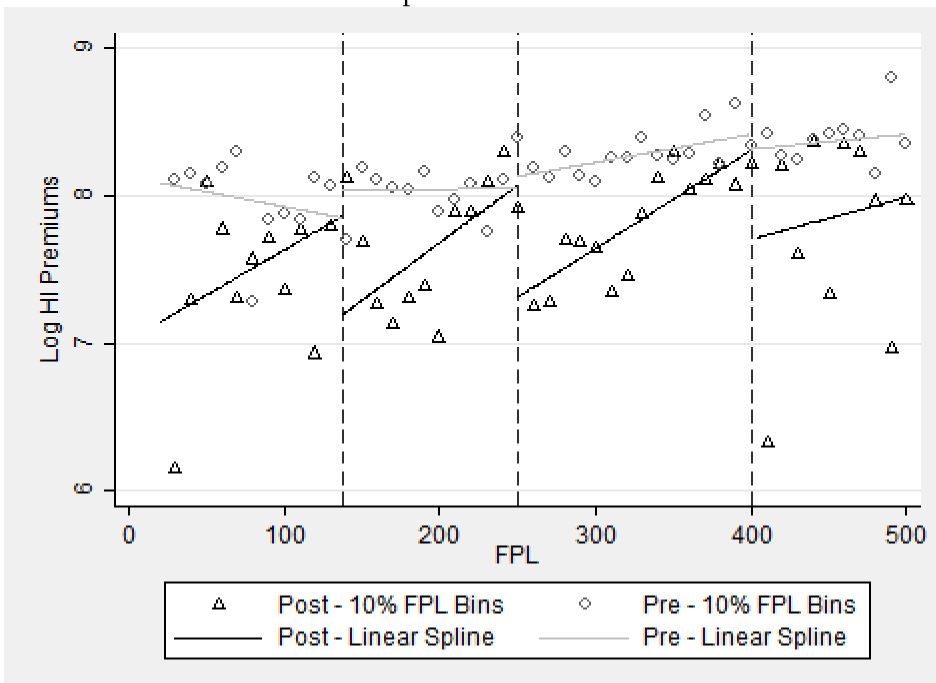
Figure 4. PHI Coverage by 10% FPL Bins in 2014



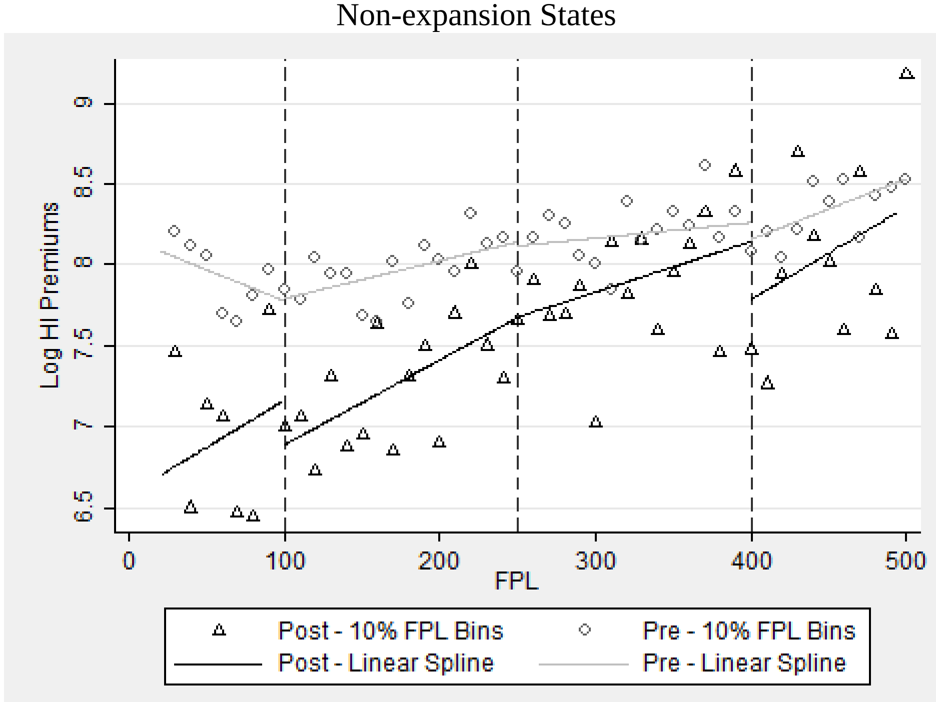
Notes: Data come from the IPUMS-CPS. Symbols represent the proportion covered in a 10% FPL bin. Vertical dashed lines represent the 138%/100%, 250%, and 400% FPL cutoffs. Linear splines are imposed with steps at each cutoff. Estimates are weighted using ASEC supplement probability weights.

Figure 5. Log Non-Zero HI Premiums for IPI-covered Individuals in 2014

Expansion States



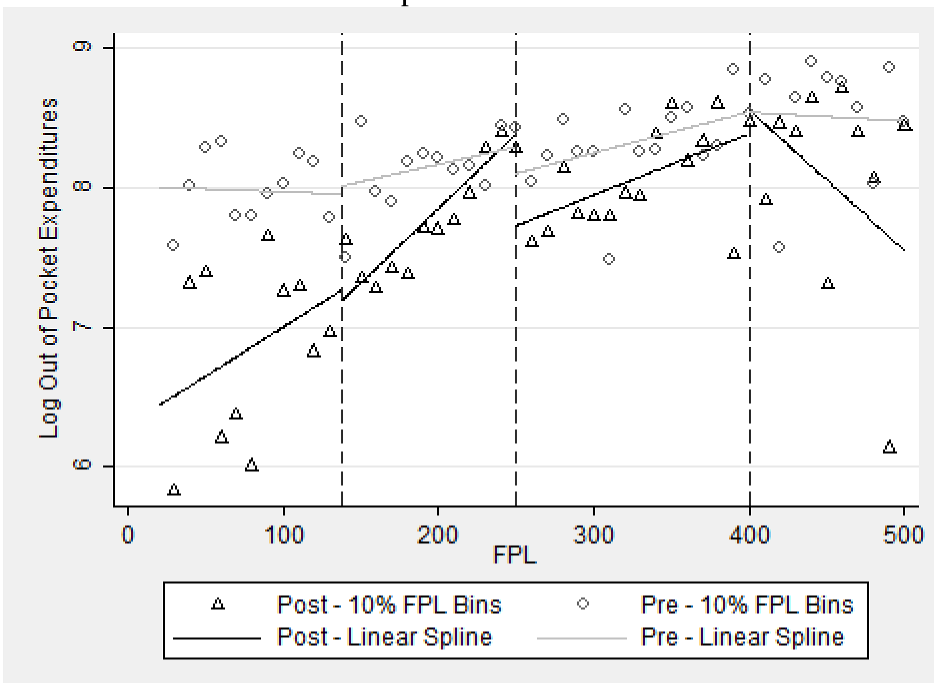
Non-expansion States



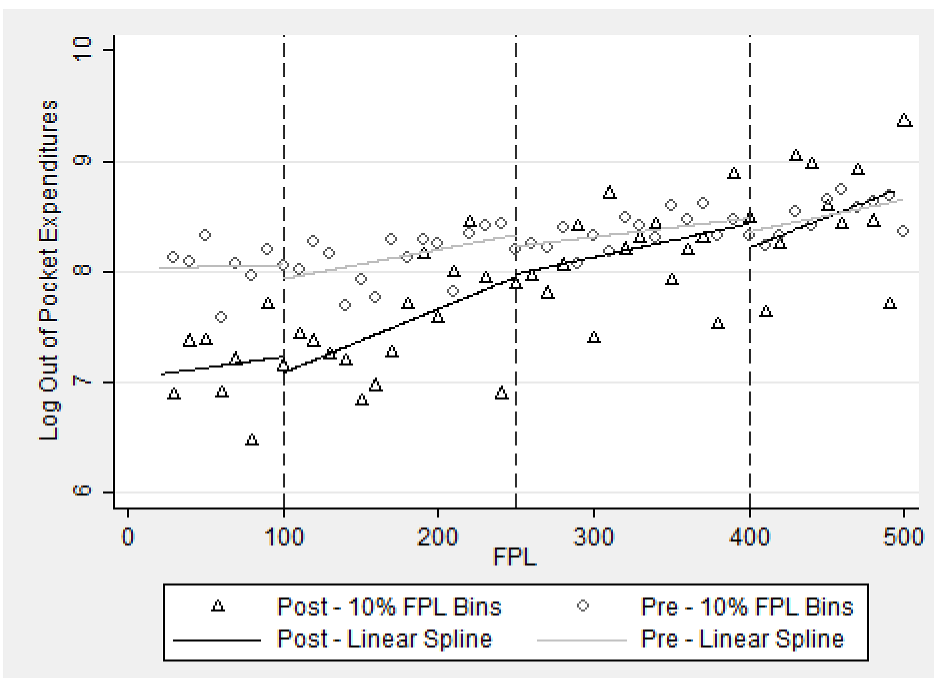
Notes: Data come from the IPUMS-CPS. Symbols represent the proportion covered in a 10% FPL bin. Vertical dashed lines represent the 138%/100%, 250%, and 400% FPL cutoffs. Linear splines are imposed with steps at each cutoff. Estimates are weighted using ASEC supplement probability weights.

Figure 6. Log OOP Expenditures for IPI-covered Individuals in 2014

Expansion States



Non-Expansion States



Notes: Data come from the IPUMS-CPS. Symbols represent the proportion covered in a 10% FPL bin. Vertical dashed lines represent the 138%/100%, 250%, and 400% FPL cutoffs. Linear splines are imposed with steps at each cutoff. Estimates are weighted using ASEC supplement probability weights.

APPENDIX

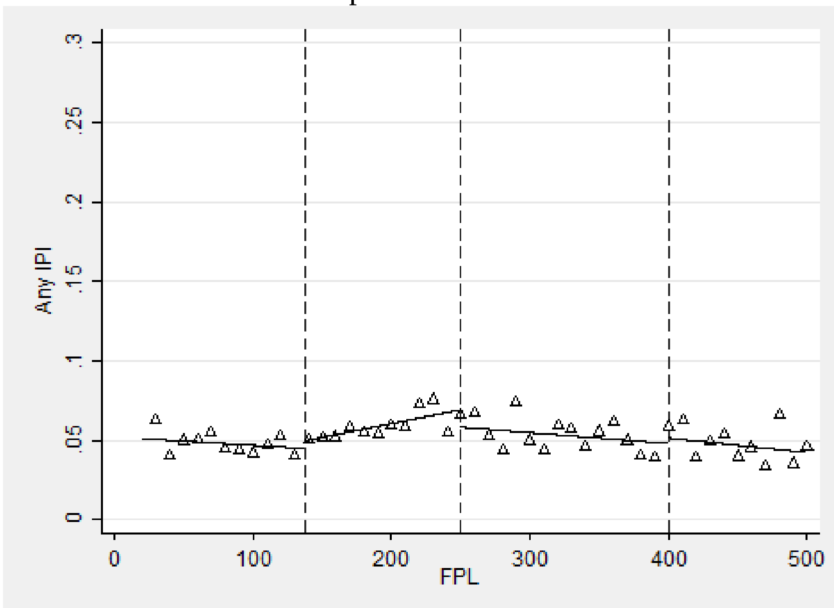
Table A-1. Regression Discontinuity Estimates at 138% FPL/100% FPL, 250% FPL, and 400% FPL for HI Outcomes, 2010–2012

	Expansion States				Non-Expansion States		
138% FPL N=21,315	**IPI**	**ESI**	**PHI**	**100% FPL** N=21,121	**IPI**	**ESI**	**PHI**
Non-parametric	0.000	0.005	0.005	**Non-parametric**	−0.008	−0.002	0.003
	(0.008)	(0.016)	(0.010)		(0.008)	(0.015)	(0.009)
Linear	0.001	−0.004	0.008	**Linear**	−0.007	0.002	0.003
	(0.008)	(0.018)	(0.012)		(0.008)	(0.016)	(0.010)
250% FPL N=19,555				**250% FPL** N=21,615			
Non-parametric	−0.013	−0.010	−0.002	**Non-parametric**	0.016**	0.000	−0.000
	(0.009)	(0.017)	(0.006)		(0.008)	(0.016)	(0.005)
Linear	0.011	0.012	−0.002	**Linear**	0.015*	0.005	−0.002
	(0.010)	(0.020)	(0.007)		(0.009)	(0.018)	(0.006)
400% FPL N=15,575				**400% FPL** N=16,610			
Non-parametric	−0.012	0.038***	0.003	**Non-parametric**	−0.013	0.011	−0.003
	(0.009)	(0.014)	(0.005)		(0.008)	(0.013)	(0.004)
Linear	−0.010	0.032*	0.003	**Linear**	−0.015	0.014	−0.001
	(0.011)	(0.017)	(0.006)		(0.010)	(0.016)	(0.005)

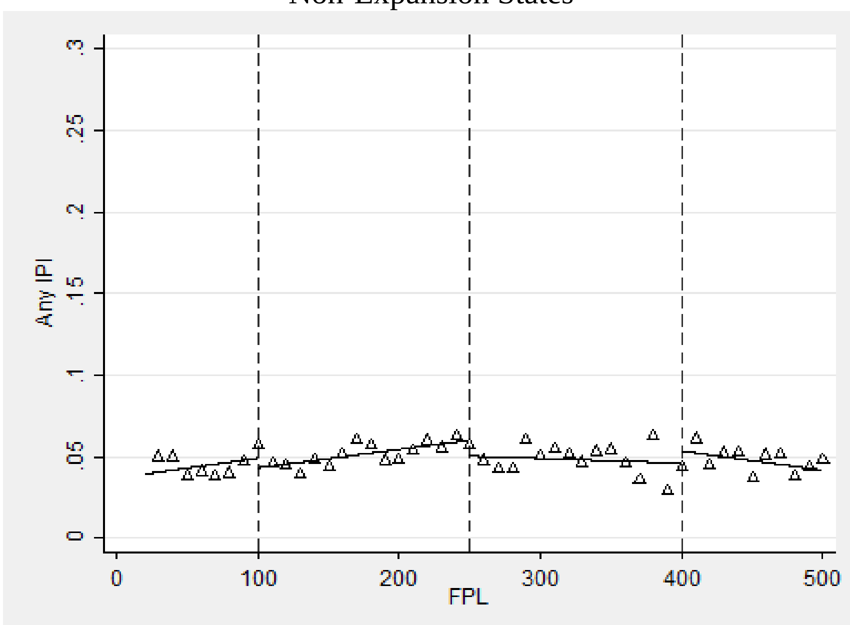
Notes: * p<0.10, **p<0.05, ***p<0.01. Data come from the IPUMS-CPS. IPI = individually purchased private insurance. Observations within 70% on either side of the cutoff are included. Standard errors are in parentheses. Non-parametric RD is calculated using a triangle kernel. Standard errors are clustered on FPL. Each OLS model includes the cutoff indicator interacted with FPL. Models are weighted using the ASEC supplement probability weights. Covariates include age, gender, race, marital status, family size, education level, self-reported health status, MSA status, and state and year indicators.

Figure A-1. IPI Coverage by 10% FPL Bins in 2010–2012

Expansion States



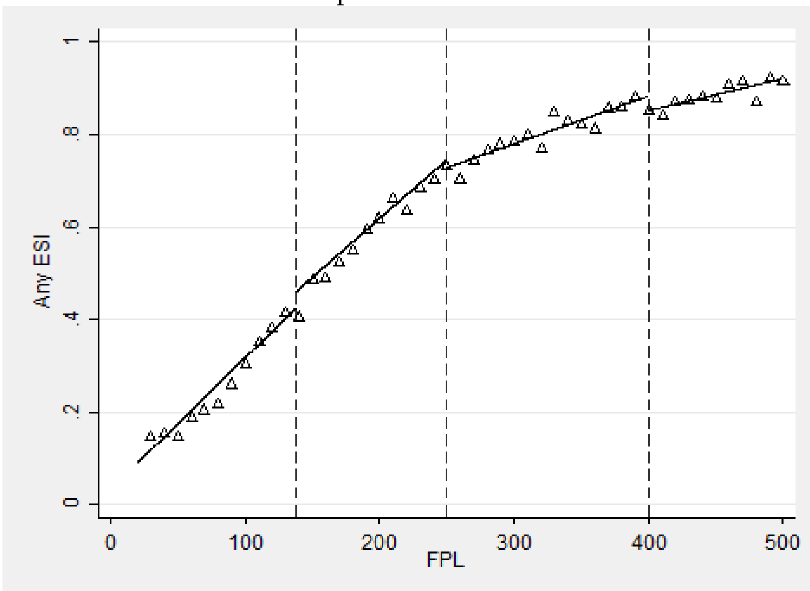
Non-Expansion States



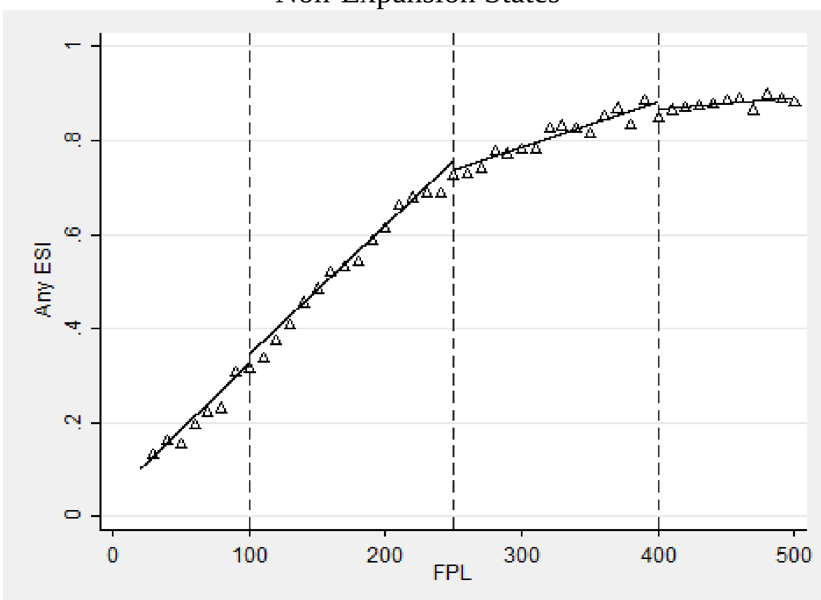
Notes: Data come from the IPUMS-CPS. Symbols represent the proportion covered in a 10% FPL bin. Vertical dashed lines represent the 138%/100%, 250%, and 400% FPL cutoffs. Linear splines are imposed with steps at each cutoff. Estimates are weighted using ASEC supplement probability weights.

Figure A-2. ESI Coverage by 10% FPL Bins in 2010–2012

Expansion States



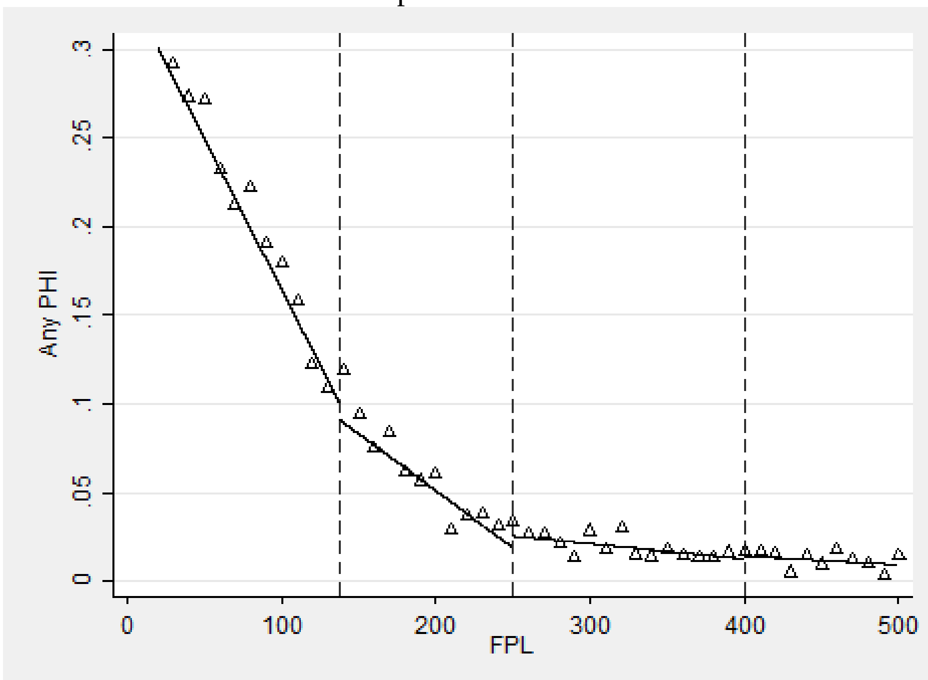
Non-Expansion States



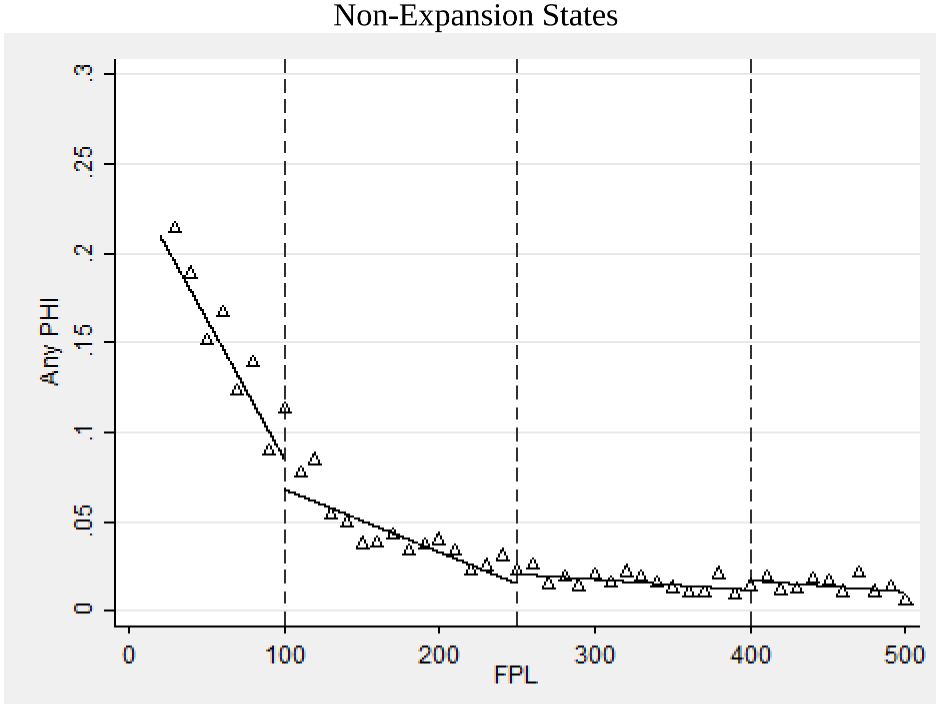
Notes: Data come from the IPUMS-CPS. Symbols represent the proportion covered in a 10% FPL bin. Vertical dashed lines represent the 138%/100%, 250%, and 400% FPL cutoffs. Linear splines are imposed with steps at each cutoff. Estimates are weighted using ASEC supplement probability weights.

Figure A-3. PHI Coverage by 10% FPL Bins in 2010-2012

Expansion States

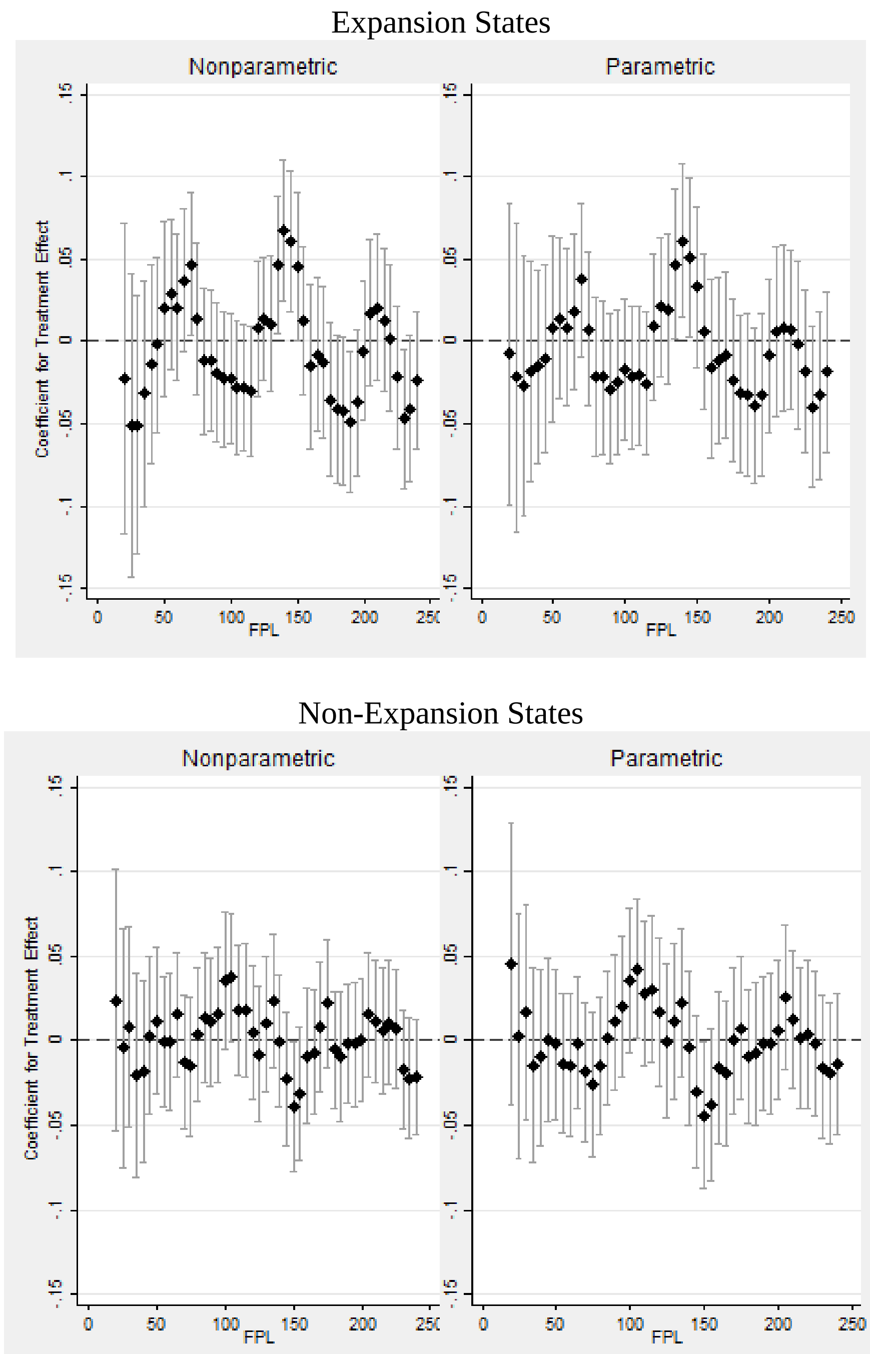


Non-Expansion States



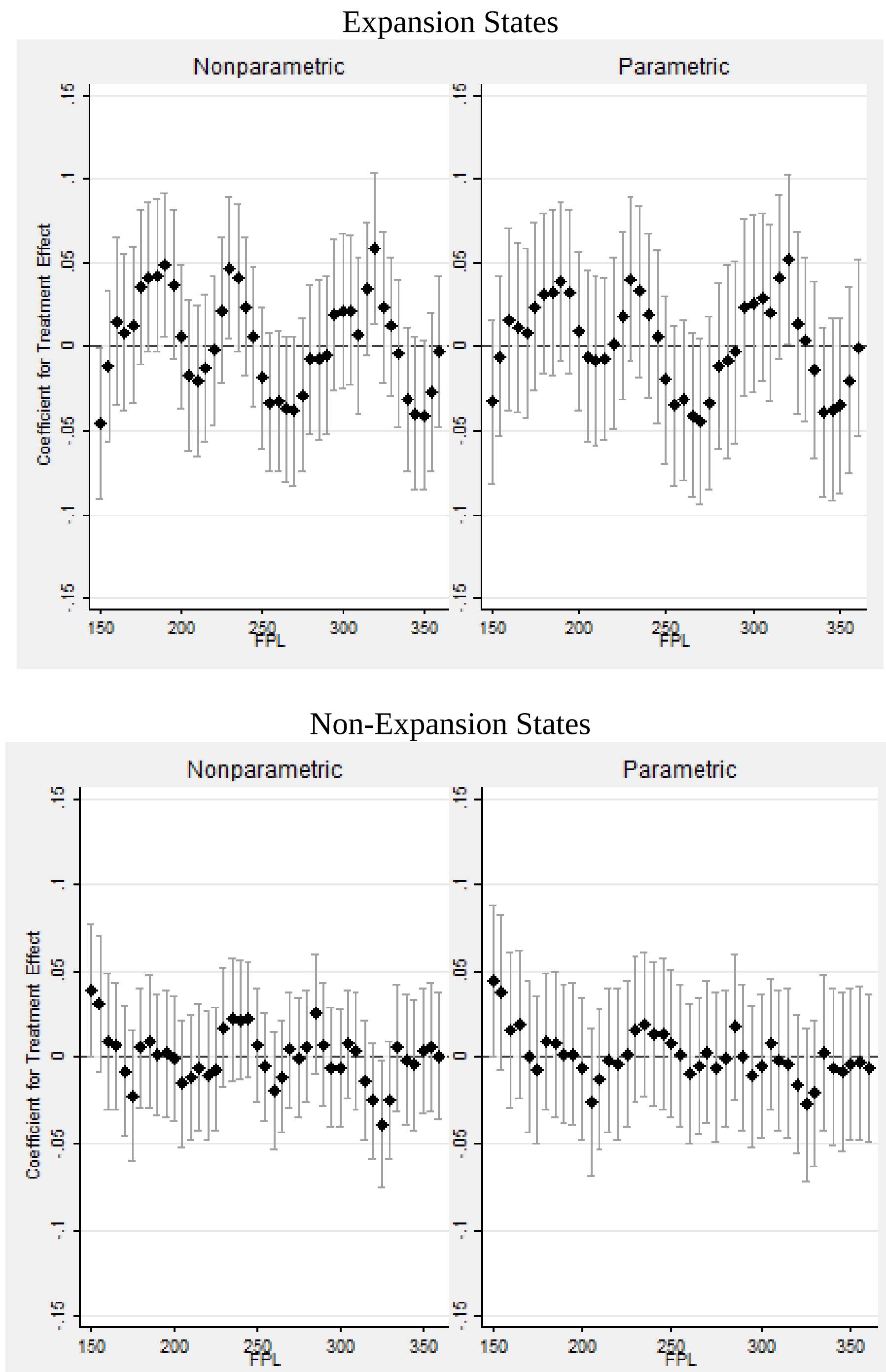
Notes: Data come from the IPUMS-CPS. Symbols represent the proportion covered in a 10% FPL bin. Vertical dashed lines represent the 138%/100%, 250%, and 400% FPL cutoffs. Linear splines are imposed with steps at each cutoff. Estimates are weighted using ASEC supplement probability weights.

Figure A-4. Permutation Testing for Different FPL Cutoffs for the Probability of having IPI in 2014, 38%-238% FPL



Notes: Points represent the coefficient estimate for the treatment effect using different FPL cutoffs. Vertical bars are 95% confidence intervals

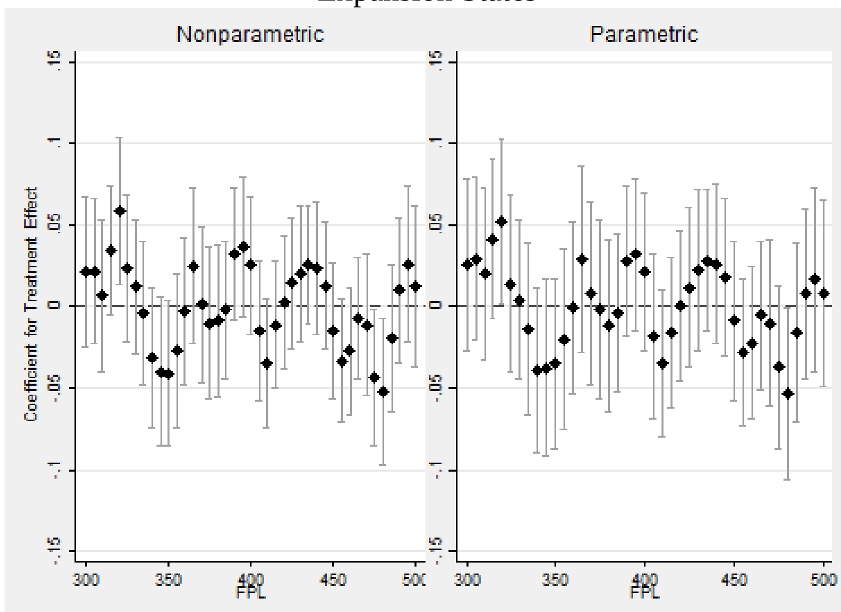
Figure A-5. Permutation Testing for Different FPL Cutoffs for the Probability of having IPI in 2014, 150%-350% FPL



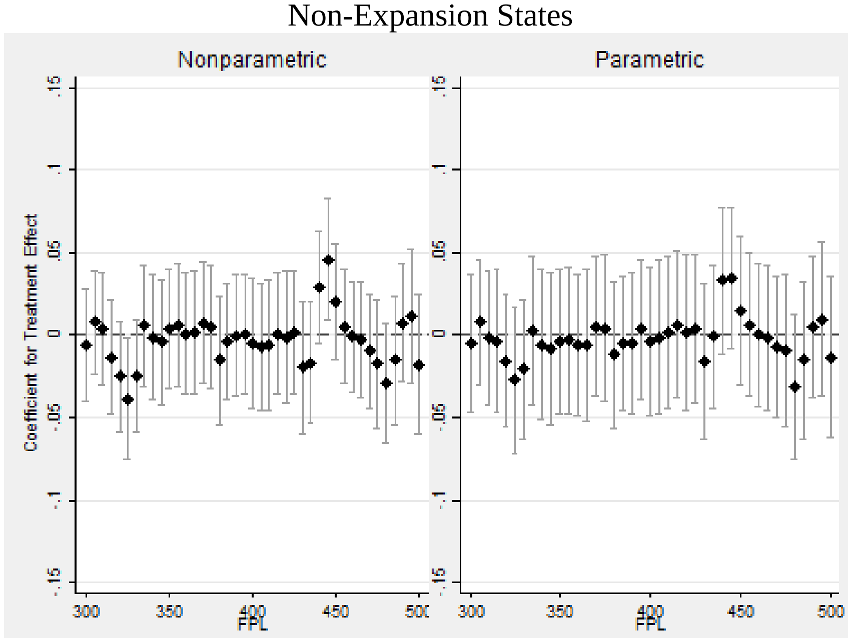
Notes: Points represent the coefficient estimate for the treatment effect using different FPL cutoffs. Vertical bars are 95% confidence intervals

Figure A-6. Permutation Testing for Different FPL Cutoffs for the Probability of having IPI in 2014, 300%-500% FPL

Expansion States



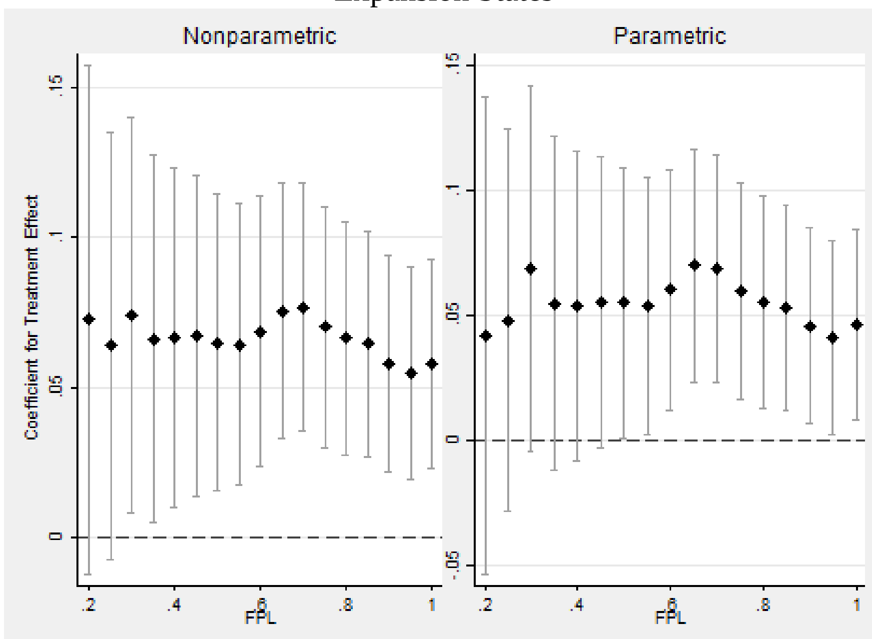
Non-Expansion States



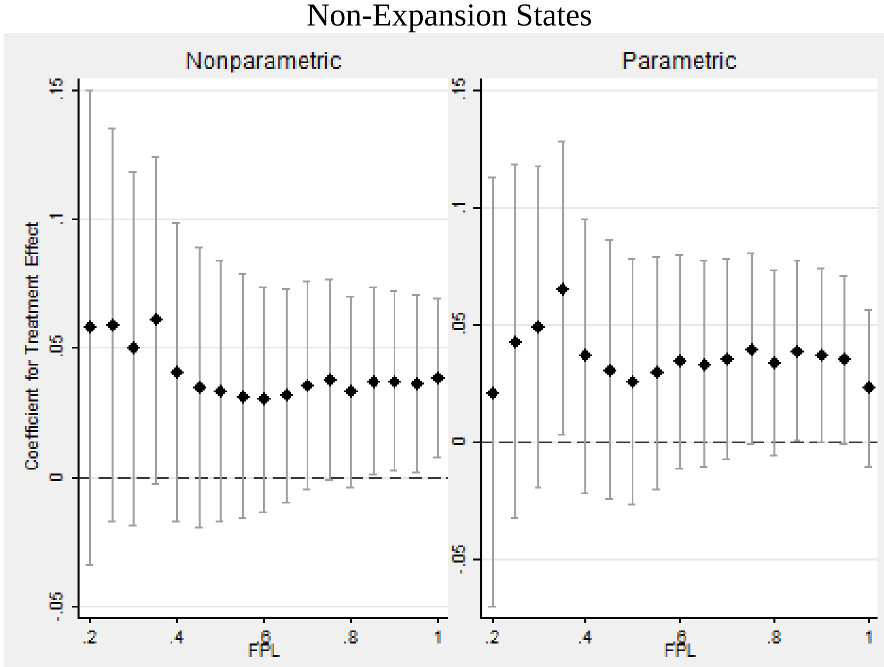
Notes: Points represent the coefficient estimate for the treatment effect using different FPL cutoffs. Vertical bars are 95% confidence intervals

Figure A-7. Bandwidth Testing for the 138%/100% FPL Cutoff for the Probability of having IPI in 2014

Expansion States



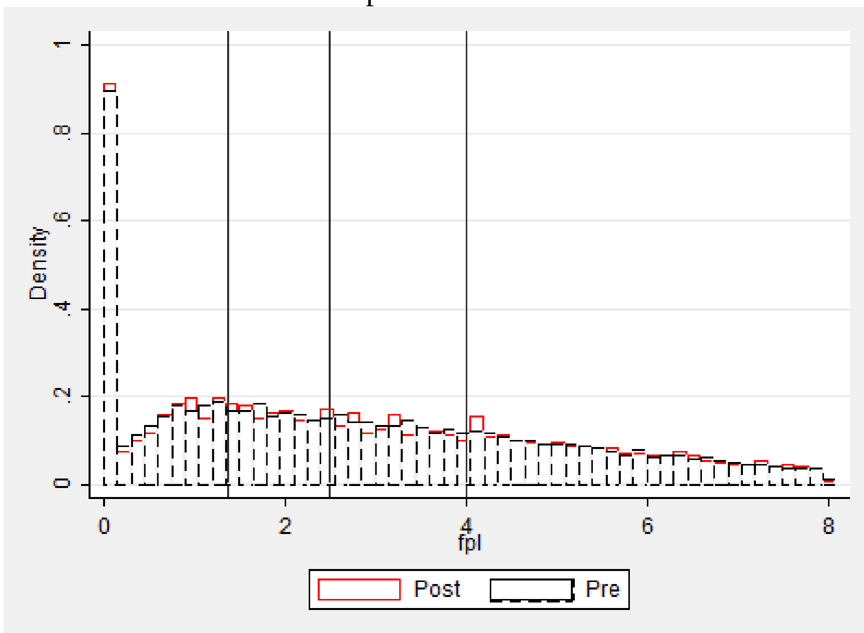
Non-Expansion States



Notes: Points represent the coefficient estimate for the treatment effect using the bandwidth indicated on the x-axis. Vertical bars are 95% confidence intervals.

Figure A-8. Density Estimates in 2014

Expansion States



Non-Expansion States