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

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**ABSTRACT**

The Patient Protection and Affordable Care Act (ACA) introduced several new policies in 2014, including an individual mandate, expanded Medicaid eligibility, and subsidized private coverage. Private subsidies include advance premium tax credits (APTCs) and cost-sharing reductions (CSRs). Individuals gain eligibility for APTCs and CSRs at 100% (138% in Medicaid expansion states) of the Federal Poverty Level (FPL), lose eligibility for CSRs at 250% FPL, and lose eligibility for the APTCs at 400% FPL. Using the Current Population Survey (CPS) and a regression discontinuity design, this study exploits the exogenous differences in subsidy eligibility in 2014 at three cutoffs to identify the separate and combined effects of the APTCs and CSRs on private insurance coverage. I estimate a 4.8 to 5.4 percentage point increase in private insurance coverage just above 138% FPL in Medicaid expansion states and a smaller effect above 100% FPL in non-expansion states attributable to the combined incentives. I calculate a demand elasticity for health insurance of -0.65 to -0.58, which is higher than most estimates in the literature, suggesting low-income individuals may be relatively more price responsive. There is no evidence of an effect on private health insurance at 250% FPL, attributable solely to the CSRs, and suggestive effects at 400% FPL, attributable to only the APTCs. Coverage increases do not appear to be driven by adverse selection, and there is no evidence of crowding-out or income manipulation around the cutoffs. APTC and CSR levels would need to be raised at higher incomes to induce more participation.

Key words: health reform, premium tax credits; cost-sharing; health insurance; regression discontinuity.

JEL Codes: H2; I1

## INTRODUCTION

The Patient Protection and Affordable Care Act of 2010 (ACA) implemented a complex, broad set of changes in the U.S. health insurance and health care system. In 2014, several prominent ACA components went into effect. First, insurance mandates require individuals to obtain and large employers to offer coverage or pay a penalty. Second, states could choose to expand Medicaid eligibility to childless, low-income adults. Third, individuals could purchase private insurance through online marketplaces and a receive a subsidy in the form of advance premium tax credits (APTCs) and cost-sharing reductions (CSRs) if the income falls between 100% and 400% of the Federal Poverty Level (FPL). APTCs reduce monthly premium payments and CSRs reduce certain elements of cost-sharing, such as co-payments or out-of-pocket maximums. The amount of the APTCs and CSRs vary considerably between 100% and 400% FPL.

A systematic review of early evidence suggests that ACA policies have greatly reduced the proportion of the population that is uninsured (French et al., 2016). Although early evidence suggests that insurance coverage increased substantially, disentangling the mechanisms by which consumer behavior is affected is of critical policy importance. Recent working papers use triple difference methods to attempt to overcome the challenge of separating the effects of the 2014 ACA components (Frean, Gruber & Sommers, 2016, Courtemanche et al. 2016). Preliminary results from these studies indicate that Medicaid expansion drives much of the increase in the insured rate but the other components, including the APTCs and CSRs, contribute substantially to the increase in the insured rate as well.

Beyond the policy impact on the overall insured rate, the APTCs and CSRs provide an opportunity to better understand the elasticity of demand for health insurance. Tax credits have been used in the past to incentivize employer-sponsored insurance (ESI) coverage (e.g., Moriya & Simon 2016) and individually purchased insurance (IPI) among the self-employed (e.g., Heim & Lurie 2009), and have typically yielded relatively low elasticities between -0.6 and -0.3. Under the ACA, the APTCs and CSRs represent a large expansion of tax credits to a low-income population, for which there are few elasticity estimates.

In this study, I exploit the discrete changes in eligibility by income relative to the FPL with a regression discontinuity (RD) design to identify the combined and separate effects of the APTCs and CSRs. Individuals gain eligibility for both APTCs and CSRs at 100% FPL, lose eligibility for CSRs at 250% FPL, and lose eligibility for the APTCs at 400% FPL. In Medicaid expansion states, individuals initially gain eligibility at 138% FPL instead of 100% FPL in non-expansion states. This creates three plausibly exogenous cutoffs where subsidy eligibility changes dramatically: 138%/100% FPL with highly subsidized APTCs and CSRs; 250% FPL where CSRs are no longer available; and 400% FPL where APTCs are no longer available. In this way, the lowest cutoff tests the combined APTC/CSR subsidy, the middle cutoff tests for changes associated with the CSRs, and the highest cutoff tests for an APTC-only effect.

I use data from the Current Population Survey (CPS) to test for effects on health insurance take up at each of the three cutoffs in 2014. As a validity check, I also examine pre-2014 data. An important assumption for RD is that the forcing variable cannot be manipulated. Given potential concerns that income can be manipulated, my approach robustly tests for evidence of bunching around the cutoffs. Prior studies focusing on the Massachusetts reform use RD and find no evidence of income manipulation (Hinde 2016, Chandra, Gruber & McKnight 2010, 2014). In contrast to other studies that examine APTCs and CSRs, I use an income definition more consistent with actual APTC/CSR eligibility. The design also does not require the identification of control group, which is problematic for ACA studies using difference-in-differences given the widespread reach of the ACA. By focusing on the discrete changes at each cutoff, it possible to separately examine the APTCs from the CSRs.

I find strong evidence of a 4.8 to 5.4 percentage point increase in IPI just above 138% FPL in Medicaid expansion states, where subsidized insurance coverage is first available and individuals are just ineligible for the expanded Medicaid program. At the 138% FPL cutoff, I estimate an elasticity of demand for health insurance ranging from -0.65 to -0.58. In non-expansion states, the effects above 100% FPL are slightly smaller in magnitude and not statistically significant for the general population, but is instead concentrated among 20-to-39 year olds. There is no evidence of an effect at the 250% FPL cutoff attributable solely to the CSRs. I do find suggestive evidence of an increase in IPI at the 400% FPL cutoff attributable to the APTC in states that implemented a state-based exchange.

More broadly, my results suggest that there are negligible effects on the overall insured rate in Medicaid expansion states at each cutoff, and positive, but insignificant, effects at each cutoff in non-expansion states. This signals a minimal level of crowding-out. Stratifying by demographic characteristics, I do not find strong evidence of adverse selection based on self-reported health status. Positive effect sizes for IPI are similar across married / single individuals and younger / older adults in expansion states, but the increases are offset by reductions in public health insurance (PHI) for married individuals and employer-sponsored insurance (ESI) for non-married individuals.

While similar tax credits have been used in past programs related to self-employment and for recently unemployed individuals, the results here suggest that these ACA tax credits may have broader appeal for lower-income individuals. The estimated elasticities are also on the high end of existing estimates, suggesting low-income individuals may be more price responsive than previous studies have found. There is no evidence for changes in IPI coverage at 250% FPL and weak evidence for changes at 400% FPL, consistent with existing low elasticity estimates for higher income individuals.

One policy implication is that the APTC and CSR levels would need to be raised at higher incomes to induce more participation. Furthermore, these results suggest the long-term impact beyond the lowest-income group could be minimal. However, given that the individual mandate penalty and the exchange premium increases in 2015 could further incentivize participation, consumer awareness of and responsiveness to these changes are a key determinant of how much the APTC and CSR levels would need to be raised in the future.

## BACKGROUND

### Institutional setting

The primary focus of this analysis is to examine the impact of APTCs and CSRs that are available first in 2014 to certain income bands of the population and are obtained through state-based exchanges (SBE) or a federally-facilitated exchange (FFE). The ACA initiated HI exchanges, online marketplaces 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 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.

To increase affordability of exchange plans, the ACA subsidized premiums to a varying degree for individuals with incomes between 100% and 400% of the FPL. The ACA implemented APTCs for individuals between 100% and 400% FPL and CSRs for individuals between 100% and 250% FPL. For 2014, income thresholds for single individuals were $11,490 (100% FPL), $15,856 (138% FPL), $28,725 ($250% FPL) and $45,960 (400% FPL) (KFF, 2014a). The value of APTCs fall on a sliding scale, where individuals receive a higher relative subsidy at lower income levels. At 400% FPL, the income cap in 2014 was 9.5%, yielding a $4,320 maximum annual premium for an individual, or $363 monthly. At the bottom end at 100% FPL, the cap was 2%, yielding a maximum annual premium of $230, or $20 monthly. The amount of the APTC was the difference between the total annual premium and the income cap. and was normalized to the price of the second lowest silver tier plan, so that individuals did not receive a higher subsidy for choosing a gold or platinum tier plan. The APTC could be applied at the time of enrollment to reduce monthly payments (referred to as the advanced premium tax credit) or collected in a lump sum through income tax filings. In 2014, 85% of consumers who enrolled in the exchange received the APTC (ASPE 2014).

The CSR subsidy was available to individuals between 100% and 250% FPL and increased the actuarial value of the silver plan to 94% for those between 100%–150% FPL, 87% for those 150%–200% FPL, and 73% for those 200%–250% FPL. Again, CSRs were normalized to the silver plan. When an individual below 250% FPL chose an exchange plan, the subsidy reduced the face value of the deductible, the out-of-pocket maximum, and co-payments associated with the plan. For example, an exchange plan 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 between 150%–200% FPL, the cost-sharing 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 health insurance decisions around each of three eligibility cutoffs: 100%/138% FPL, 250% FPL, and 400% FPL. Table 1 describes how program eligibility changes across the different FPL 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 changes around the 100%/138% FPL cutoffs as a combined effect of the APTCs and CSRs. Just above 100%/138% FPL, individuals gain eligibility to the dual incentive. For expansion states, those who fall below 138% FPL are potentially eligible for Medicaid, so this effect may be capturing changes in preferences between public and private coverage. In non-expansion states, a coverage gap exists, where individuals below 100% FPL have no access to APTCs/CSRs and are unlikely to be newly eligible for Medicaid. Thus, the incentive is different and potentially much more valuable in non-expansion states.

An effect at the 250% FPL cutoff would be attributed to the CSRs. Individuals just below and just above 250% FPL both have access to the APTCs, while individuals just below 250% FPL are eligible for CSRs and individuals above 250% FPL do not. The APTC does not change discretely at 250% FPL, only the availability of the CSR. Lastly, I refer to the changes around the 400% FPL cutoff as the effect of the APTC only, comparing individuals just below 400% FPL that are eligible for APTCs and individuals just above 400% FPL that are ineligible.

A second incentive to health insurance participation is an individual mandate that requires 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). Furthermore, the penalty is not applied to individuals with incomes that fall below the tax filing threshold or 138% FPL in states that do not expand Medicaid, Native Americans, or if the lowest exchange premium available is greater than 8% of income. Given the low level of the tax in 2014, the contamination of this component is assumed to be zero for this analysis. This is consistent with preliminary evidence that the mandate had little effect on insurance coverage (Frean, Gruber, & Sommers, 2016).

This analysis does not formally examine Medicaid expansion, which extends Medicaid eligibility to childless adults under 138% FPL. Medicaid expansion interacts with the analyses here, since many individuals are newly eligible just below 138% FPL. An intended effect of the research design is that many individuals should lose eligibility for Medicaid coverage above 138% FPL in states that choose to expand. This is not a policy effect in the context of the current study in as much as a validity check.

**Prior Literature**

This analysis contributes to the expanding empirical evidence of the impacts of the ACA. Several organizations conducted nationally representative surveys to track early impacts of the 2014 ACA components including Medicaid expansion, individual and large employer mandates, and private HI exchanges. Descriptive results from the Health Reform Monitoring Survey indicate 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., 2014, 2015). The gains vary by age, race/ethnicity, and gender and are potentially larger in Medicaid expansion states. Among those uninsured between 138%–399% FPL, 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 find similar reductions in the proportion of uninsured (e.g., Black & Cohen, 2014; Sommers et al., 2015).

Two recent studies use a triple difference method, taking advantage of pre-2014 variation in the local area uninsured rate, to separate the effects of the different ACA components on insurance coverage. Courtemanche et al. (2016) use cross-state variation in Medicaid expansion status and estimate a 5.9 percentage point increase in the insured rate. They attribute half of the increase to Medicaid expansion and the other half to ACA components. Frean, Gruber & Sommers (2016) use variation in premiums across geographic regions to separate the effects of APTCs, individual mandate, and Medicaid expansion. They find the APTCs account for 37% of the observed reduction in the uninsured rate and Medicaid expansion accounts for 63%. They further describe that most of the Medicaid expansion effects are driven by a woodwork effect – increased uptake by previously eligible individuals. For the APTCs, Frean, Gruber and Sommers (2016) estimate a small average price elasticity of -0.05. While Courtemanche et al. (2016) find evidence for a partial crowding out of public insurance, Frean, Gruber & Sommers (2016) find no evidence of crowding out.

Other quasi-experimental analyses of specific ACA components focus on early expanding states and other components implemented prior to 2014, such as the dependent care mandate. For example, Golberstein et al. (2015) find 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 due to early Medicaid expansions. Evidence from the dependent-coverage mandate indicates a marked increase in insurance coverage among those less than 26 years of age (e.g., Antwi et al., 2013).

Several other studies examine the impact of the ACA on ESI. Survey data from the Urban Institute show little evidence of changes in ESI availability, ESI take-up, and ESI coverage, but offer suggestive evidence that ESI coverage increases for employees of small employers and low incomes (Blavin et al., 2015). The 2015 Employer Health Benefits Survey indicates an increase in ESI premiums consistent with increases from previous years and notes little change in benefit design (Claxton et al. 2015). The rapid response surveys provide suggestive evidence of anticipatory changes in offer and benefit design to meet ACA requirements, but little overall impact on ESI.

Beyond the policy effects of the ACA itself, this study links to the broader literature on the demand elasticity for health insurance. A wide range of empirical studies have produced varying elasticity estimates across ESI and IPI plans, ranging from almost zero to above one. Early studies focusing on variation in employee contributions and the tax deductibility of employee premiums estimate highly inelastic demand in the range of -0.05 to -0.02 (e.g., Blumberg, Nichols & Banthin 2001, Chernew, Frick & McLaughlin 1997; Gruber & Washington 2005). These early studies estimate the elasticity based off the employee portion of the premium. Over time, a separate literature focusing on the relationship between ESI costs and wages suggests that employees bear the full cost of changes in ESI premium, and thus the relevant base for the elasticity estimate should be the total premium cost. This shift in thought suggests the early estimates are potentially low (e.g., Baicker & Chandra 2006).

For non-ESI elasticities, another literature focuses on subsidies for self-employed and recently unemployed. Gruber and Poterba (1994) compare how changes in tax deductibility affect insurance among self-employed individuals compared to employed individuals and estimate an elasticity between -3 and -1. Using an individual fixed effects model, a more recent study by Heim & Lurie (2009) estimates a smaller elasticity for the self-employed, between -0.6 and -0.3. The American Recovery and Reinvestment Act of 2008 provided health insurance subsidies to recently unemployed individuals who previously had access to ESI. The ARRA subsidy lets individuals pay 35 percent of the full ESI premium while the employer is repaid 65 percent of the subsidy by the government. Moriya & Simon (2016) estimate an elasticity of -0.38 to -0.27. These studies yield moderate price elasticities for narrowly defined populations – self-employed and recently unemployed individuals. The APTC apply to a broader portion of the population, and a potentially different population – lower-income individuals.

The APTCs and CSRs in the ACA are modeled after the 2006 Massachusetts reform. A recent study applies a similar RD design and methods used in this analysis to examine the impact of APTCs implemented in Massachusetts in 2006 (Hinde 2016). As a part of the Massachusetts reform, APTCs were offered to individuals below 300% FPL. Using a regression discontinuity design, the study estimates a 7 to 9 percentage point increase in IPI just below 300% FPL associated with the APTCs. A pair of RD studies by Chandra, Gruber & McKnight (2010, 2014) uses the change in CSRs at several FPL cutoffs as exogenous cutoffs to estimate demand elasticities for medical care services. They estimate an elasticity of -0.16 across various medical services, similar to the elasticity estimated in the seminal RAND Health Insurance Experiment (Newhouse 1993).

A recent working paper by Pauly et al. (2015) simulates financial implications and welfare changes associated with the 2014 APTCs and CSRs. Their results indicate that the additional financial burden of purchasing HI are offset by increases in welfare due to expected medical care prices for individuals below 250%. Aligning with these projections, I hypothesize the effects may be strongest at 100%/138% and 250%, where consumers have access to the APTCs and CSRs. Combined with the low elasticity estimate from Chandra, Gruber & McKnight (2014), the effect at 250% FPL is likely to be weaker, since the change in the CSR is lower across that cutoff.

## METHODS

### Data

I use the Current Population Survey (CPS) Annual Social and Economic Supplement (ASEC) because it captures income, HI status, and demographics representatively at the national and state level (Flood et al., 2015). The analyses focus on 2014, the first year the APTC and CSR subsidies are available. As a validity check, I use a pre-reform period pooling data from calendar years 2010–2012. The ASEC was redesigned for the March 2014 survey so that the health insurance questions better match the American Community Survey (ACS). The ACS questions include current coverage, while the old ASEC questions include whether household members had coverage in the past year. The redesigned ASEC includes information about current and previous year coverage. The redesigned questions also include whether specific household members are covered by a specific type of insurance, and once coverage type is identified, whether other household members are covered by that specific type. The Census Bureau recommends against directly comparing ASEC HI measures before and after 2013 until methods are developed to correct for differences in the series (Pascale et al. 2016). Therefore, the pre-period cannot currently be used as a baseline for 2014 changes. Furthermore, I exclude calendar year 2013 from the pre-period due to concerns about respondents reporting current coverage as of March 2014 instead of past year coverage (Swartz 1986).

The analytic sample includes adults aged 26 to 64. Individuals over 64 are almost universally covered by Medicare. Any individual with an allocated HI status is also dropped; HI status is allocated for some respondents based on other answers and information on the respondent's record or imputed if the interview was not fully completed. Allocation does not include logical imputation for PHI. Lastly, I also drop respondents residing in Massachusetts due to pre-existing health reform policies that directly overlap with the ACA policies.

The main outcome is past year HI status. I measure whether respondents had any HI and four exclusive categories of HI: IPI, ESI, and PHI, or uninsured. If an individual reports ESI coverage during a given year, he or she is not assigned IPI or PHI. Individuals who report any ESI or IPI are not assigned PHI. The primary independent variable is 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 is based on the size of the family. Binary indicators are used to denote incomes that fell below 400% and 250% and above 100%/138% FPL; these capture the eligibility cutoffs for different components of the ACA in the RD design. As noted, there is a difference in the lowest cutoff between expansion (138% FPL) and non-expansion states (100% FPL).

Subsidy eligibility is based on modified adjusted gross income (MAGI), not gross income as directly reported in the ASEC. Because of this, I calculate income relative to the FPL using a Census Bureau-provided measure of adjusted gross income (AGI) that is created using statistical matching with Internal Revenue Service (IRS) tax records (O'Hara 2004). AGI removes certain tax deductions and exemptions from gross income; AGI is lower than gross income. MAGI includes foreign-earned income, tax-exempt interest and non-taxable social security benefits. At lower income levels, the difference between MAGI and AGI is likely small.[1] Statistical matching introduces a potential source of measurement error, but there are not better sources that capture AGI beyond the IRS data (conversely, the IRS data do not historically measure broader HI coverage well). To account for differences between MAGI and AGI, all models exclude observations within 1% FPL of the cutoff to conservatively estimate the policy effects. The results are not sensitive to alternative models that include observations within 1% FPL

The AGI statistical match is made on household heads. I logically assign the imputed AGI to household members since the eligibility decision is made at the household level. The results do not change if only household heads are used. Rather, the standard errors improve, providing indirect evidence of measurement error. The results presented here are conservative.

A series of covariates are 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 are treated as continuous variables, while binary indicators are used for the remaining individual controls.

**Empirical Methods**

To estimate the effects of the APTCs and CSRs on coverage, a sharp RD design is applied using the 100%/138%, 250%, and 400% FPL cutoffs in 2014 as exogenous forcing

[1] Based on author's calculations using the 2014 IRS SOI (https://www.irs.gov/uac/soi-tax-stats-individual-income-tax-returns-publication-1304-complete-report#_IndReturns). For reported AGI's less than $50,000, the average foreign-earned income is $52 and the average tax-exempt interest is $143. For AGIs less than $100,000 the average difference between MAGI and AGI is less than 1%. Above $100,000 the average difference is between 1% and 2%. Non-taxable social security benefits are excluded given the sample restriction to the non-elderly (Supplemental social income is not included in the MAGI calculation)

variables.[2] The estimation approach logically separates the sample into two groups: expansion and non-expansion states. The 138% cutoff only applies to expansion states, and the 100% FPL cutoff to non-expansion states, requiring separation when examining the lowest cutoff. Twenty-eight states expanded Medicaid by 2014 to include childless adults below 138% FPL.

RD compares individuals just below and just above each FPL cutoff, assuming that the only difference between individuals is eligibility for the APTCs, CSRs, or Medicaid. Hinde (2016) uses exact design and data source to estimate the impact of the tax credits available below 300% FPL in Massachusetts in 2006 (Hinde 2016) and Chandra, Gruber & McKnight (2010, 2014) use FPL cutoffs as an exogenous source of variation to examine CSRs used in the 2006 Massachusetts reform.

RD is first estimated non-parametrically using local linear regression with a triangle kernel density estimator. Multiple bandwidths are used for the local linear estimation to examine sensitivity to the bandwidth (Lee & Lemieux 2010). RD is also estimated using a standard linear specification. The following specification references the 100% FPL cutoff, but applies similarly to the other cutoffs.[3]

$$HI_i = \alpha + \beta_1 SUB(FPL > 100)_{\ i} + \beta_2 FPL(x - 100)_i + \beta_3 SUB(FPL > 100)_{\ i} * FPL(x - 100)_i + \delta \boldsymbol{X}_i + \gamma_s + \varepsilon_i$$

where *HI* is a binary HI indicator, and *SUB* is a binary indicator for above 100% FPL, *FPL* is centered at 100% FPL, $\boldsymbol{X}$ is a vector of the individual demographics described above, and $\gamma_s$ are state fixed effects. $\varepsilon_i$ is assumed to be an independently and identically distributed error term.

[2] One could argue that a fuzzy RD would be better in this context given the measurement errors concerns described in the previous section. For a fuzzy RD design, one would need to know whether an individual receives APTC and CSR subsidies to serve as the first-stage outcome. Since the CPS does not capture whether or not an individual receives the APTCs and CSRs, the outcome for the first-stage is missing and a fuzzy RD is not possible.

[3] For the 250% and 400% FPL cutoff, the *SUB* variable refers to being just below the cutoff, reversing the inequality in the above equation.

The FPL cutoff indicator and the continuous FPL measure are interacted to allow the slope of the FPL trend to vary on either side of the cutoff. $\beta_1$ represents the treatment effect at the discontinuity. The nonparametric model estimates the equivalent of $\beta_1$ but without imposing linearity of trends. I report detailed treatment effects for any HI and IPI, the categories directly affected by the APTCs and CSRs. For completeness, I also report estimates for ESI and PHI. The above equation is also estimated for the pre-period separately and presented in Appendix Table A.1 and Appendix Figures A.1 to A.4. Pre-period estimates include year fixed effects.

To test for improvements in fit of the parametric form, I use higher order FPL terms in the parametric model. Models are estimated with and without the vector of individual-level controls. The models are not generally sensitive to higher order terms or covariate inclusion. Standard errors are clustered on the FPL for all models, as recommended by Lee & Card (2008) to account for the potential discreteness of the forcing variable. Results are not sensitive to alternative standard error calculations, such as heteroscedastic-robust standard errors or standard errors clustered at the state level. All reported models use ASEC supplement probability weights to account for oversampling in the CPS. The probability weights may cause imprecision, so I re-estimate the main models without weighting (Solon, Haider & Wooldridge 2015). For the unweighted models, the standard errors are not different, but the effects are smaller in magnitude and sometimes insignificant.

A potential concern with this application of RD is that income can be manipulated, which would threaten identification. Historically, programs enforced through the tax code, such as the EITC, have been known to cause kink points in the income distribution (e.g., Saez 2010). Unlike other tax-based policies, such as the EITC, the APTC and CSR are not pure income transfers. In this context, there is also a temporal disconnect between the enrollment decision and tax reconciliation. The enrollment period for the exchanges occurs in the fall months prior to the beginning of the next calendar year.[4] Thus, individuals prospectively decide to enroll based on projected income. The final amount of the APTC is not determined until tax filing the following year, where a repayment penalty occurs if individuals underestimate income.

The RD design is focused on the availability of the APTCs and CSRs at certain FPL thresholds, not the actual receipt of the incentives. To manipulate income to maintain eligibility, one could alter labor supply to affect earnings or take advantage of various tax credits and deductions, such as individual retirement account contributions, at tax filing to get under a threshold. To test for this type of manipulation, I look for evidence of income bunching around the FPL thresholds and changes in labor market behavior. I also estimate the McCrary (2008) test for discontinuities in the distribution near the cutoffs. To preview results of the manipulation tests, I do not find evidence that incomes are manipulated and argue that the design is valid given the prospective nature of the enrollment decision. This is consistent with a previous study on the Massachusetts reform (Hinde 2016).

Four other standard sensitivity and falsification tests are used to test the robustness of the results (Imbens & Lemieux, 2008). First, I use 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.

[4] Open enrollment for calendar year 2014 lasted from October 2013 through March 2014.

Second, different bandwidths around the cutoffs are 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 is 70%, and the bandwidth is allowed to vary between 25% and 100%.

Third, I examine nonrandom heaping with the FPL, a concern raised by Barreca 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. Barreca et al. (2011) recommend a donut-hole RD, where the heap is dropped from the estimation procedure. The exclusion of observations within 1% FPL constitutes a donut-hole RD.

Finally, I examine 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, respectively. Across all states, any HI and ESI coverage increases as income increases, while IPI decreases slightly from the lower to higher incomes. Across both expansion and non-expansion states, IPI coverage is similar at each cutoff. The main difference in HI coverage across expansion and non-expansion states comes from the 8 percentage point difference for PHI around 138% FPL in expansion states. There are some minor differences in other demographic characteristics between lower and high incomes. Namely, as income increases individuals are more likely be older, married, white, and well-educated.

A critical assumption for an RD design is that there is no manipulation in the forcing variable. This assumption can be visually assessed with histograms by checking for discontinuities in the FPL sample distribution at the cutoff and estimating the McCrary (2008) test for manipulation. Figure 1 presents a histogram for the expansion and non-expansion states for 2010-2012 and 2014. There is no visual evidence of mass points occurring near the cutoffs that would indicate manipulation nor large changes across time. Likewise, the McCrary test does not indicate large or significant differences in the FPL density at any cutoff. To further assess manipulation, I examine labor market outcomes at each of the cutoffs, since altering labor supply is one way to alter income. I find no differences at the cutoffs in labor force participation, unemployment, self-employment, or part-time status (results available upon request). There is no evidence of any income manipulation that would invalidate the RD design.

To visually assess the effects of the combined APTCs and CSRs, Figures 2 through 5 show HI coverage across the FPL distribution for the four types of HI. The hollow circle symbols represent the unconditional proportion covered by the HI type within a 5% FPL bin. The figures also impose a local linear trend between the cutoffs to visualize potential treatment effects near the cutoffs.

In Figure 2, while the proportion with any HI increases over the FPL distribution, there are no clear breaks at any of the cutoffs, except for a potential dip in any HI coverage just below 250% FPL in non-expansion states. For expansion states in the top panel of Figure 3, there is a noticeable increase in the scatterplot just above 138% FPL and the local linear trends suggest a large, positive effect on IPI coverage relative to those below 138% FPL. Moving further above 138% FPL, the scatterplot and local linear curve trend downward until 400% FPL where it appeared to flatten out. There is no visual evidence of a treatment effect near 250% in the scatterplot, but the local linear curves indicate a small, negative effect just below 250% FPL. Near 400% FPL, the change in the FPL trends indicate a small, positive effect.

For non-expansion states in the bottom panel of Figure 3, the plot looks quite similar to expansion states. There is an apparent effect just above 100% FPL, similar in magnitude to the effect above 138% FPL in expansion states, although not as clean. Between 100% FPL and 250% FPL, the IPI trend declines until it flattens out above 250% FPL. There is a small, negative effect just below 250% FPL and just below 400% FPL according to the local linear trends, but again, the visual evidence for an effect is weak.

Beyond IPI coverage, I examine changes in ESI and PHI at the same three cutoffs in expansion and non-expansion states. Figure 4 shows ESI coverage across the FPL distribution. ESI coverage increases greatly as the FPL increases, but there is little evidence of any jumps around the cutoffs in either state grouping. In Figure 5 describing PHI coverage, there is a noticeable drop-off in PHI just above 138% FPL in expansion states and just above 100% FPL in non-expansion states. There are no effects at the other two cutoffs.

Statistical estimates of the treatment effects are presented in Table 3. For expansion states, there are negligible changes in any HI coverage at all three cutoffs. The overall changes in the insured rate are not statistically significant, and for expansion states, suggest a minimal level of crowding out from Medicaid expansion. The increase in IPI is offset by a 1.3 to 2.3 percentage point drop in ESI and a 3.1-3.2 percentage point drop in PHI. For IPI, the combined treatment effect just above 138% FPL in expansion states is 5.4 percentage points in the non-parametric model and 4.8 percentage points in the linear model. Both estimates are statistically significant. The proportion covered by IPI between 68% and 138% FPL in 2014 is 0.104. The percentage increase associated with the combined incentive, therefore, ranges from 46.6% to 52.5%. Per ASPE reports from the FFE, the APTC reduced the average premium by 80%, implying an elasticity ranging from -0.65 to -0.58 (ASPE 2014).

Among the non-expansion states, there is a non-parametric 4.3 percentage point effect and a linear 3.4 percentage point effect for any HI at 100% FPL, although it is insignificant. The combined incentive effect for IPI just above 100% FPL is a smaller 2.3 percentage points and statistically insignificant. However, there is a similar increase in ESI of 1.7 to 2.6 percentage points.

Confirming the visual evidence in Figure 3, I do not find evidence of an effect on any HI coverage or a cost-sharing treatment effect for IPI just below 250% FPL. For expansion states, there is an insignificant 1.3 percentage point reduction in IPI just below 250% FPL. There is a marginally significant drop in any HI coverage of 3.9 percentage points in non-expansion states just below 250% FPL, but the IPI effects are negligible. Instead, the decrease is driven by an insignificant 3.6 to 4.2 percentage point decrease in ESI just below 250% FPL.

Contrary to the visual evidence of a positive effect just below 400% FPL in expansion states, the statistical estimate is positive but small and insignificant. A separate model focusing solely on the SBE states estimates a 3.6 percentage point increase in IPI just below 400% FPL and the effect was significant at the 10% level. Again, there is no evidence of any effects near 400% FPL in non-expansion states

In summary of the IPI results, I find strong evidence of a combined effect of the APTCs and CSRs just above 138% FPL in expansion states and less robust evidence of a combined effect just above 100% FPL in non-expansion states, where the incentives are strongest. There is no robust statistical evidence to support a CSR effect and only suggestive evidence of an APTC effect in SBE states. The positive effects for the combined incentive and APTC-only imply that the APTCs could be the driving incentive for consumers on the margin.

As a validity check, a separate set of analyses reproduce the main results for the 2010–2012 period, available in Appendix Table A.1 and Appendix Figures A.1–A.3. No effects are found in the pre-period at 100%/138% FPL and 400% FPL. There is weak statistical evidence of a 2.4 percentage point increase in any HI coverage just below 250% FPL in expansion states and 1.5-1.6 percentage point increase in IPI just below 250% in non-expansion states. In both cases, there is not strong visual evidence of a jump in coverage. When disaggregated by year, all 3 effects dissipate. Given the sensitivity of the effects across years and the lack of visual evidence, there is little concern that the design is invalid for the 250% FPL cutoff.

**Heterogeneous Effects**

Long-term sustainability of the marketplace is in many ways tied to conformation by younger, healthier individuals to diversify the risk pool of the exchanges. To test whether the observed effects above 138% FPL in expansion states and above 100% FPL in non-expansion states are concentrated among a particular demographic, I stratify the models in Table 3 by three key characteristics: 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 in Table 4, there is a net increase in the insured rate for married individuals and a net decrease in non-married individuals. The combined effect of the APTCs and CSRs for IPI is slightly higher for married (approximately 5.5 percentage points) than non-married individuals (4.6 to 5.3 percentage points), but not practically different. The differences in any HI coverage across marital status is driven by ESI and PHI. There is a reduction of 5.9 to 6.2 percentage points in PHI for married individuals, whereas non-married individuals have a reduction in ESI of approximately 5.4 to 6.6 percentage points. The PHI drop-off is consistent with Medicaid ineligibility, but the ESI drop-off for single individuals is unexpected. This could be evidence of switching away from ESI toward IPI.

The next stratification is 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 are too few individuals in fair and poor health to analyze separately. When stratified by health status, the combined effect is unchanged for the higher self-reported health group, and is somewhat attenuated for the lower self-reported health group for the linear specification. A reduction in PHI is observed only for the lower self-reported health group. Overall, there are negligible effects on the insured rate among the higher self-reported health or the lower self-reported health group. At least the extensive margin, there is no evidence of adverse selection in IPI take-up.

The bottom portion of Table 4 compares the effects for individuals aged 26 to 39 and individuals aged 40 to 64. While imprecise, there is a marginally significant increase in any HI for the younger group and a negative, insignificant decrease in any HI in the older group. There is a small difference in the 3.6 to 4.7 percentage point combined effect and 5.8 to 6.1 percentage effect on IPI between younger and older groups, respectively. As with the marriage stratification, the older group experiences a reduction in PHI between 6.1 and 7 percentage points attributable to Medicaid ineligibility above 138% FPL, while the younger group does not see a countervailing reduction in ESI comparable to the non-married group.

Table 4 provides three implications. First, there are only minor differences in the effect of the combined incentives on IPI across marital status and age group. Second, there is an interesting dynamic of non-married individuals dropping off ESI coverage just above 138% FPL. Third, the non-married, older age groups see small net declines in the insured rate that are associated with Medicaid ineligibility. In one sense, the results suggest 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, signals a small crowding out effect.

For the non-expansion states in Table 5, there are three interesting findings. First, there is an increase in any HI for all groups except those reporting good, fair or poor health. Thus, the any HI significant for those in self-reported excellent or very good health is large, positive and significant. The 6.5 to 9 percentage point effect is driven by approximately equal increases in IPI and ESI. However, the increase in IPI and ESI is not significant. There is not the dynamic tradeoff in ESI and PHI as with the expansion states and little evidence of crowd-out.

Second, there is a significant combined effect on IPI for the 26- to 39-year-old age group of 5.1 to 5.3 percentage points. The 5.1 to 5.3 percentage point increase in IPI among young adults is a slightly larger than estimate of the combined effect for young adults in expansion states. The effects for 40- to 64-year-old respondents are negligible. Since older adults face higher premium levels, the relative value of the subsidy should be higher for older adults and the lack of an effect is counterintuitive. It may point to issues in navigating the FFE and minimal outreach and navigational assistance in most non-expanding states, given the positive correlation between Medicaid expansion and adoption of a state-based exchange.

**HI Premiums and Medical Spending**

The results so far focus on the extensive margin of obtaining IPI. Beginning with the 2011 ASEC, respondents are asked to self-report HI premiums and out-of-pocket medical expenditures. The limited sample size in the CPS prevented in-depth statistical examination of the impact on premiums and out-of-pocket (OOP) medical expenditures conditional on having IPI. Instead, descriptive results of the impacts on premiums and OOP medical expenditures are presented. Figures 6 and 7 graphically present the average non-zero log premiums and log OOP spending for IPI-covered individuals before and after the exchanges and incentives went into effect in 2014, along with the local linear curves checking for discontinuities. These cost measures have not changed and are comparable across time, but are generally noisy.

Figure 6 shows 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. For expansion states in 2014, the line is relatively smooth up to 250% FPL. Premiums drop slightly after 250% FPL and then exhibit a larger drop-off above 400% FPL. The trend lines are smooth in non-expansion states in 2014. For both state groupings, the pre-periods do not show large changes near any of the cutoffs.

The increases in average log premiums just below the 250% and 400% FPL cutoffs in expansion states suggest CSR-maximizing behavior. Figure 7 provides suggestive evidence for this hypothesis. Log OOP expenditures are lower across time below 250% FPL, and then converge back to pre-204 levels. This is suggestive of broader welfare benefits to consumers. There is also an increase in log OOP expenditures just below 400% FPL in expansion and non-expansion states in 2014. This last fact could be evidence of adverse selection. The demographic stratifications in Tables 4 and 5 do not suggest adverse selection on the extensive margin, but the effects below 400% FPL are weakly suggestive of adverse selection on the intensive margin.

**Robustness Checks**

I implement a wide range of robustness checks and sensitivity analyses to attempt to refute the main results presented in the previous section. Results from the 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 involves 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 the largest effect size in absolute magnitude and the largest test statistic. There are no other large effects in the FPL range around 138% FPL for IPI in expansion states. Just above 100% FPL in non-expansion states there is a large, positive effect (see Appendix Figure A-4). Near 250% FPL and 400% FPL for IPI in both expansion and non-expansion states, the permutation test is not suggestive of false effects (see Appendix Figure A-5[5] and A-6). Among the ESI and PHI outcomes, the permutation testing do not alter interpretation of the main results at any cutoff.

The second robustness test alters 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 no 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 are appropriately sensitive to bandwidth selection (see Appendix Figure A-7). Coefficient magnitude is at least constant or decreasing in absolute magnitude as the bandwidth increase.

[5]Appendix Figure A-5 also shows that there are no effects associated with the CSRs at 200% FPL in expansion states and at 150% and 200% FPL in non-expansion states. The CSR drops from 94% actuarial value to 87% actuarial value at 150% FPL and further drops to 73% actuarial value at 200 FPL.

The third robustness test assesses non-random heaping. I assess bunching using disaggregated scatter plots across FPL ranges for each outcome and do not find evidence of heaping.

The fourth and final robustness test examines potential effects of demographic shifts near the cutoffs. There is little visual evidence of demographic breaks near the cutoffs, but three demographic characteristics do 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 are noisy and decreasing in FPL in both expansion and non-expansion states, which help to explain why some models pick up a statistically significant effect. More importantly, the effects are small and there is no visual evidence of a demographic shift near any of the cutoffs.

In summary of the four robustness tests, there is little evidence to draw serious concerns about the design. Beyond the robustness tests, these analyses still have several limitations. First, there are several potential sources of measurement error: statistically matched AGI, logical imputation of AGI to families, and projected versus actual income. It is assumed that these are cases of classical measurement error that magnified the standard errors and do not introduce bias. To check for sensitivity to AGI definition, I re-calculate AGI using the National Bureau of Economic Research TaxSim 9.0 program. Model estimates with the TaxSim version of AGI are slightly smaller in magnitude compared to the matched AGI definition results. The final source of measurement error—projected versus actual income—could not be addressed with the CPS. Given the prospective incentives against cheating through repayment policies, the possibility of non-random measurement error is likely weakest in this study.[6]

[6] As noted earlier, individual retirement account contributions and other tax deductions/credits could be applied at the time tax filing to maintain eligibility. Tax avoidance behavior cannot be observed here and is not critical to the

A second limitation is that the data do not directly measure receipt of tax credits and cost-sharing subsidies or capture whether IPI is obtained through the exchanges. I assume that the cutoffs are binding and the demand for non-exchange coverage does not correlate with the ACA cutoffs. It is possible that non-exchange IPI coverage is wrapped up in the estimates. Built into this limitation is also the fact that the CPS income and HI questions were redesigned recently to better capture income and HI dynamics. Respondents could potentially confuse 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." There may be some concern that the family size used in the FPL definition here exactly capture family size used in determining tax credit/cost-sharing eligibility. However, when the results are stratified by marital status in Table 5, the estimates are statistically indistinguishable.

As a final limitation, while the CPS provides a large sample size overall, using only 2014 data limits the relative sample size within FPL bins. The estimates could potentially be improved by additional years of data. The visual and statistical evidence support the main results of a combined effect, but more data is always better. In testing for income manipulation, I examine changes in labor market outcomes around the cutoffs. The null 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 (e.g., Bitler et al. 2006), short-term labor responses may not be detectable with 2014 data alone, but should be monitored as more data become available. Future studies should examine whether long-term effects on labor market outcomes accrue.

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research design. Self-employed individuals are most able to manipulate income. Separate analyses exclude self-employed individuals and the results are not different.

## DISCUSSION

This analysis examines the effectiveness of ACA APTCs and CSRs implemented in 2014. Overall, the APTCs and CSRs are not associated with sharp changes in any HI coverage at any cutoff in expansion states, but are associated with insignificant, positive changes in any HI in non-expansion states. For IPI, however, I find robust, positive effects of the combined APTC/CSR incentive just above 138% FPL in expansion states and weaker effects above 100% FPL in non-expansion states. This is a combined effect because consumers were initially eligible for APTCs and CSRs just above 138%/100% FPL. The APTC amount is highest and the CSR is most valuable at lower income levels, resulting in large effects where the incentives were strongest. Of particular policy importance is the finding that the increase in IPI in expansion states just above 138% FPL is offset by reductions in ESI and PHI. This suggests a minimal level of crowd-out and could have significant implications for public health care expenditures.

Despite the limitations noted in the previous section, the broad story painted by these estimates is 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 highlights previously identified coverage gaps among states opposing federal ACA policies. With a positive relationship between SBE adoption and Medicaid expansion, the difference in the effect between expansion and non-expansion states could indicate that outreach, assistance, and framing efforts of marketplaces could significantly affect uptake of IPI. SBE states funded consumer advocacy and outreach efforts to enroll eligible consumers, suggesting awareness of the SBE, the APTCs and the CSRs is likely to be higher (Sommers et al. 2015, Cox et al., 2015). 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).

Tying into the broader literature examining the demand for health insurance, I estimate an elasticity of demand for health insurance of -0.65 to -0.58 for expansion states just above 138% FPL. This estimate is much larger than the -0.05 elasticity in Frean, Gruber & Sommers (2016), which is calculated using the average subsidy level (as in this study), but the treatment effect is for the broader 100-400% FPL population. Because the elasticity here is estimated on the margin of APTC and CSR eligibility, it suggests that low-income consumers on the margin are much more price-responsive than low-income individuals subject to the more gradual decline in the subsidy value.

My elasticity estimate is also higher relative to the overall -0.38 to -0.27 elasticity from the ARRA subsidies in Moriya & Simon (2016), although they acknowledge that the ACA APTCs and CSRs could produce higher elasticities. Using a sub-sample of their data, Moriya & Simon (2016) estimate a similar treatment effect of 6.1 percentage points for the 138% FPL to 400% FPL subsample, but the subsample elasticity of -0.41 is still lower than my estimate. One key difference between this study and Moriya & Simon (2016) is that their population consists of recently unemployed individuals. Recently employed individuals choose whether to maintain current coverage (with a 65% subsidy reduction) or lose coverage. Risk-averse consumers may be less price sensitive when faced with losing HI as opposed to a decision to become newly insured through the exchanges.

My estimate is at the upper range of the -0.6 to -0.3 elasticity estimated by Heim & Lurie (2009) for self-employment premium subsidies. Heim et al. (2015) estimate that the after-tax exchange premiums (including the APTC) are on average 42 percent lower than comparable after-tax self-premiums paid by the self-employed, which may account for the difference in the range of the elasticities. In summary, the elasticity estimate of -0.65 to -0.58 just above the 138% FPL cutoff in expansion states is much higher than a similar study of the ACA (Frean, Gruber & Sommers, 2016) but aligns with other elasticities estimated among the self-employed and recently unemployed.

While there are clear effects of the combined APTCs and CSRs in expansion states, the lack of robust findings at 100% FPL in non-expansion states is puzzling given the coverage gap. In expansion states, the difference from 137% FPL and 139% is fully subsidized coverage to highly subsidized coverage, whereas in non-expansion states, the difference is unsubsidized coverage at 99% FPL to highly subsidized coverage at 101% FPL. In one sense, this could imply a much lower elasticity. I do estimate a similar effect size for IPI above 100% FPL for young adults, which yields an elasticity of -0.80 to -0.77.[7] Alternatively, this could be related to technical issues with FFE and other navigation/awareness issues highlighted earlier in FFE states.

Perhaps unsurprisingly, there is no detectable effect on IPI near the 250% FPL cutoff, above which consumers lost eligibility for the cost-sharing subsidies. At 250% FPL, the actuarial value only drops from 73% to 70%; a relatively small amount. The permutation testing by default tests the 150% FPL and 200% FPL cutoffs, where the drop in the CSR is more valuable. There are still no visible or statistical effects (see Appendix Table A-5). I caution that the null results at 250% FPL attributable to the CSRs do not suggest overall ineffectiveness. Referring back to Figure 4, the unconditional proportions with IPI between 138%/100% FPL and 250% FPL are higher than the 250%-400% FPL and the greater than 400% FPL samples. Rather, the

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[7]The proportion of the sample below 100% with IPI is 0.073, indicating a 70% to 74% increase. Based on a 2014 report from ASPE, the average benchmark FFE monthly premium for a 27-year old with the second lowest silver tier plan was $214. With a $20/month income cap at 100% FPL, the subsidy represents 90% of the total premium.

results indicate that CSRs are not necessarily differentiable from the APTC and the suggestive evidence of an effect just below the 400% FPL cutoff, concentrated in SBE states, implies that the APTCs drive the results. Still, a basic policy implication from this study is that the APTC and CSR levels would need to be raised at higher incomes to induce more participation.

The results from this study imply that the long-term impact for income groups above 250% FPL could be minimal unless the individual mandate is binding or the relative value of the APTC/CSR subsidy increases due to overall premium increases. This analysis assumes negligible effects of the individual mandate penalty in 2014. After 2015, the penalty increases significantly. Because the mandate penalty is also on a sliding scale, higher incomes are much more susceptible to the increase in the penalty and future studies should consider whether the countervailing effects of the individual mandate penalty increase the appeal of IPI. Furthermore, as currently written in law, the APTC and CSR levels do not increase over time, but increases in marketplace premiums potentially increase the relative value of the APTCs since the caps are relatively flat across time. If there are not visible effects in this design just below 250% FPL and only weak effects below 400% FPL in 2014, the increased mandate penalty in 2015 could be further re-enforced by the price increases in the marketplace to increase the attractiveness of exchange plans, creating effects beyond 2014.  The dynamic responses to these changes hinges on consumer awareness of and response to the individual mandate and premium increases.

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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**[a] |
| 100–138% | **Y** | **Y** | **Y**[a] |
| 138–250% | **Y** | **Y** | N |
| 251–400% | N | **Y** | N |
| >400% | N | N | N |

[a]Only applies to 28 states that expanded their Medicaid program.

**Table 2. Weighted Summary Statistics**

| | Expansion States | | | | | | Non-Expansion States | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | 68-208% FPL N=8,575 | | 180-320% FPL N=7,417 | | 330-470% FPL N=5,939 | | 68-208% FPL N=6,351 | | 180-320% FPL N=6,590 | | 330-470% FPL N=4,826 | |
| Characteristic | Mean | SE | Mean | SE | Mean | SE | Mean | SE | Mean | SE | Mean | SE |
| Any Health Insurance | 0.79 | (0.005) | 0.88 | (0.004) | 0.95 | (0.003) | 0.65 | (0.007) | 0.83 | (0.006) | 0.93 | (0.004) |
| Any IPI | 0.11 | (0.004) | 0.10 | (0.004) | 0.07 | (0.004) | 0.12 | (0.005) | 0.08 | (0.004) | 0.08 | (0.005) |
| Any ESI | 0.46 | (0.006) | 0.71 | (0.006) | 0.86 | (0.005) | 0.38 | (0.007) | 0.70 | (0.007) | 0.81 | (0.007) |
| Any public insurance | 0.23 | (0.005) | 0.07 | (0.003) | 0.03 | (0.003) | 0.15 | (0.005) | 0.05 | (0.003) | 0.04 | (0.003) |
| Age | 42.58 | (0.144) | 43.50 | (0.156) | 44.53 | (0.169) | 42.08 | (0.169) | 43.63 | (0.161) | 44.75 | (0.186) |
| Female | 0.53 | (0.006) | 0.50 | (0.007) | 0.49 | (0.008) | 0.55 | (0.007) | 0.50 | (0.007) | 0.49 | (0.008) |
| Race | | | | | | | | | | | | |
| White | 0.75 | (0.005) | 0.79 | (0.006) | 0.83 | (0.006) | 0.72 | (0.007) | 0.77 | (0.006) | 0.82 | (0.006) |
| Black | 0.12 | (0.004) | 0.10 | (0.004) | 0.07 | (0.004) | 0.22 | (0.006) | 0.17 | (0.005) | 0.12 | (0.006) |
| Other/multiple race | 0.13 | (0.004) | 0.11 | (0.004) | 0.10 | (0.004) | 0.07 | (0.004) | 0.06 | (0.004) | 0.05 | (0.004) |
| Hispanic | 0.32 | (0.006) | 0.20 | (0.005) | 0.10 | (0.004) | 0.27 | (0.006) | 0.17 | (0.005) | 0.10 | (0.005) |
| Marital Status | | | | | | | | | | | | |
| Currently married | 0.52 | (0.006) | 0.58 | (0.007) | 0.70 | (0.007) | 0.48 | (0.007) | 0.61 | (0.007) | 0.74 | (0.008) |
| Previously married | 0.19 | (0.005) | 0.18 | (0.005) | 0.13 | (0.005) | 0.25 | (0.006) | 0.20 | (0.006) | 0.14 | (0.006) |
| Never married | 0.29 | (0.006) | 0.25 | (0.006) | 0.17 | (0.006) | 0.28 | (0.007) | 0.19 | (0.006) | 0.12 | (0.006) |
| Household Size | 3.30 | (0.023) | 2.93 | (0.022) | 2.81 | (0.021) | 3.30 | (0.026) | 2.86 | (0.022) | 2.78 | (0.023) |
| Education | | | | | | | | | | | | |
| Less than high school | 0.18 | (0.005) | 0.08 | (0.003) | 0.03 | (0.003) | 0.22 | (0.006) | 0.08 | (0.004) | 0.04 | (0.003) |
| High school diploma/GED | 0.35 | (0.006) | 0.32 | (0.006) | 0.24 | (0.007) | 0.36 | (0.007) | 0.33 | (0.007) | 0.25 | (0.007) |
| Some college | 0.19 | (0.005) | 0.20 | (0.006) | 0.17 | (0.006) | 0.20 | (0.006) | 0.20 | (0.006) | 0.18 | (0.006) |
| Associate’s degree | 0.10 | (0.004) | 0.13 | (0.005) | 0.14 | (0.005) | 0.10 | (0.004) | 0.13 | (0.005) | 0.15 | (0.006) |
| Bachelor’s degree | 0.13 | (0.004) | 0.20 | (0.005) | 0.27 | (0.007) | 0.10 | (0.004) | 0.19 | (0.006) | 0.26 | (0.007) |
| Graduate degree | 0.05 | (0.003) | 0.07 | (0.003) | 0.14 | (0.005) | 0.03 | (0.003) | 0.07 | (0.004) | 0.13 | (0.006) |

| Characteristic | Expansion States: 68-208% FPL N=8,575 Mean | SE | 180-320% FPL N=7,417 Mean | SE | 330-470% FPL N=5,939 Mean | SE | Non-Expansion States: 68-208% FPL N=6,351 Mean | SE | 180-320% FPL N=6,590 Mean | SE | 330-470% FPL N=4,826 Mean | SE |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| Self-Rated Health Status | | | | | | | | | | | | |
| Excellent | 0.23 | (0.005) | 0.27 | (0.006) | 0.30 | (0.007) | 0.22 | (0.006) | 0.26 | (0.006) | 0.31 | (0.008) |
| Very good | 0.32 | (0.006) | 0.36 | (0.007) | 0.41 | (0.008) | 0.30 | (0.007) | 0.37 | (0.007) | 0.38 | (0.008) |
| Good | 0.32 | (0.006) | 0.29 | (0.006) | 0.23 | (0.006) | 0.33 | (0.007) | 0.29 | (0.007) | 0.24 | (0.007) |
| Fair | 0.10 | (0.004) | 0.07 | (0.003) | 0.05 | (0.004) | 0.12 | (0.005) | 0.08 | (0.004) | 0.05 | (0.004) |
| Poor | 0.03 | (0.002) | 0.02 | (0.002) | 0.01 | (0.002) | 0.03 | (0.003) | 0.02 | (0.002) | 0.01 | 0.002 |

Notes: Data are drawn from the IPUMS-CPS. Twenty-eight states expanded their Medicaid program by 2014. All means are weighted using the ASEC supplemental probability weights. ESI = Employer-Sponsored Insurance; IPI = Individually Purchased 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** N=8,429 | **Any HI** | **IPI** | **ESI** | **PHI** | **100% FPL** N=6,237 | **Any HI** | **IPI** | **ESI** | **PHI** |
| **Non-parametric** | 0.008 | 0.054*** | -0.013 | -0.031 | **Non-parametric** | 0.043 | 0.023 | 0.026 | -0.004 |
| | (0.021) | (0.017) | (0.026) | (0.021) | | (0.028) | (0.019) | (0.029) | (0.020) |
| **Linear** | -0.006 | 0.048** | -0.023 | -0.032 | **Linear** | 0.034 | 0.022 | 0.017 | -0.005 |
| | (0.025) | (0.019) | (0.032) | (0.024) | | (0.032) | (0.021) | (0.032) | (0.022) |
| **250% FPL** N=7,307 | | | | | **250% FPL** N=6,495 | | | | |
| **Non-parametric** | -0.009 | -0.013 | 0.002 | 0.004 | **Non-parametric** | -0.039* | 0.007 | -0.034 | -0.013 |
| | (0.019) | (0.017) | (0.025) | (0.012) | | (0.023) | (0.015) | (0.027) | (0.011) |
| **Linear** | -0.011 | -0.016 | 0.002 | 0.003 | **Linear** | -0.036 | 0.012 | -0.042 | -0.006 |
| | (0.021) | (0.020) | (0.029) | (0.014) | | (0.030) | (0.018) | (0.034) | (0.014) |
| **400% FPL** N=5,864 | | | | | **400% FPL** N=4,784 | | | | |
| **Non-parametric** | 0.010 | 0.011 | -0.002 | 0.004 | **Non-parametric** | -0.018 | -0.005 | -0.009 | -0.003 |
| | (0.015) | (0.015) | (0.022) | (0.010) | | (0.017) | (0.020) | (0.027) | (0.013) |
| **Linear** | 0.008 | 0.013 | -0.003 | -0.002 | **Linear** | -0.021 | -0.008 | -0.007 | -0.006 |
| | (0.018) | (0.018) | (0.027) | (0.013) | | (0.020) | (0.024) | (0.032) | (0.015) |

Notes: * p<0.10, **p<0.05, ***p<0.01. Data come from the IPUMS-CPS. Twenty-eight states expanded their Medicaid program by 2014. 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 fixed effects.

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

| | | Any HI | | IPI | | ESI | | PHI | |
|---|---|---|---|---|---|---|---|---|---|
| 138% FPL | N | Non-parametric | Linear | Non-parametric | Linear | Non-parametric | Linear | Non-parametric | Linear |
| Marital Status | | | | | | | | | |
| Currently married | 4,590 | 0.025 | 0.005 | 0.055** | 0.054* | 0.034 | 0.010 | -0.062** | -0.059 |
| | | (0.026) | (0.032) | (0.022) | (0.029) | (0.034) | (0.042) | (0.029) | (0.037) |
| Not married | 3,839 | -0.010 | -0.014 | 0.053** | 0.046* | -0.066* | -0.054 | 0.005 | -0.006 |
| | | (0.033) | (0.034) | (0.026) | (0.026) | (0.040) | (0.041) | (0.029) | (0.028) |
| Heath Status | | | | | | | | | |
| Excellent/very good | 4,568 | -0.007 | -0.010 | 0.053** | 0.058** | -0.052 | -0.061 | -0.013 | -0.007 |
| | | (0.028) | (0.029) | (0.025) | (0.027) | (0.035) | (0.039) | (0.026) | (0.030) |
| Good/fair/poor | 3,861 | 0.022 | -0.008 | 0.053** | 0.040 | 0.021 | 0.017 | -0.049 | -0.066* |
| | | (0.031) | (0.035) | (0.023) | (0.026) | (0.038) | (0.042) | (0.033) | (0.037) |
| Age Group | | | | | | | | | |
| 26–39 | 3,830 | 0.057* | 0.042 | 0.047** | 0.036 | 0.006 | -0.006 | 0.006 | 0.012 |
| | | (0.032) | (0.034) | (0.024) | (0.025) | (0.039) | (0.043) | (0.032) | (0.036) |
| 40–64 | 4,599 | -0.031 | -0.040 | 0.061** | 0.058** | -0.030 | -0.028 | -0.061** | -0.070** |
| | | (0.027) | (0.032) | (0.024) | (0.027) | (0.035) | (0.040) | (0.028) | (0.032) |

Notes: * p<0.10, **p<0.05, ***p<0.01. Data come from the IPUMS-CPS. Twenty-eight states expanded their Medicaid program by 2014. 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 fixed effects.

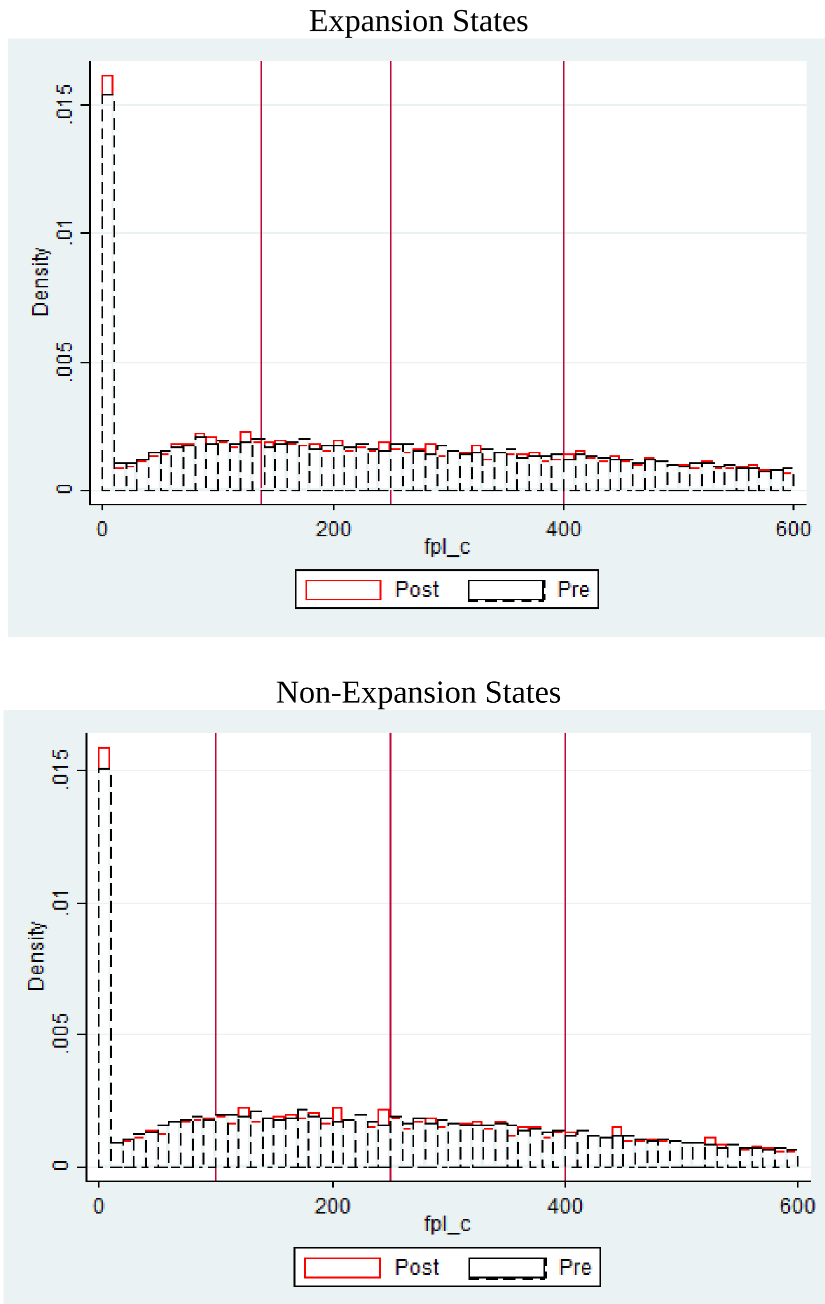
**Table 5. RD Estimates for Non-Expansion States at 100% FPL by Key Demographics, 2014**

| | | Any HI | | IPI | | ESI | | PHI | |
|---|---|---|---|---|---|---|---|---|---|
| **100% FPL** | **N** | **Non-parametric** | **Linear** | **Non-parametric** | **Linear** | **Non-parametric** | **Linear** | **Non-parametric** | **Linear** |
| Marital Status | | | | | | | | | |
| Currently Married | 3,134 | 0.062 | 0.040 | 0.021 | 0.031 | 0.024 | -0.006 | 0.019 | 0.016 |
| | | (0.041) | (0.044) | (0.029) | (0.035) | (0.040) | (0.048) | (0.030) | (0.037) |
| Not Married | 3,103 | 0.034 | 0.026 | 0.024 | 0.015 | 0.040 | 0.034 | -0.029 | -0.023 |
| | | (0.040) | (0.044) | (0.027) | (0.026) | (0.041) | (0.040) | (0.026) | (0.026) |
| Heath Status | | | | | | | | | |
| Excellent/Very Good | 3,294 | 0.090** | 0.065 | 0.039 | 0.029 | 0.050 | 0.034 | 0.003 | 0.002 |
| | | (0.038) | (0.041) | (0.027) | (0.030) | (0.040) | (0.042) | (0.025) | (0.028) |
| Good/Fair/Poor | 2,943 | -0.011 | 0.005 | 0.006 | 0.011 | 0.001 | -0.001 | -0.015 | -0.005 |
| | | (0.042) | (0.043) | (0.028) | (0.028) | (0.040) | (0.045) | (0.031) | (0.034) |
| Age Group | | | | | | | | | |
| 26–39 | 2,988 | 0.061 | 0.064 | 0.051** | 0.053** | 0.031 | 0.028 | -0.020 | -0.017 |
| | | (0.042) | (0.045) | (0.022) | (0.025) | (0.043) | (0.046) | (0.027) | (0.030) |
| 40–64 | 3,249 | 0.026 | 0.013 | -0.003 | -0.004 | 0.022 | 0.002 | 0.007 | 0.015 |
| | | (0.038) | (0.040) | (0.031) | (0.033) | (0.040) | (0.043) | (0.029) | (0.030) |

Notes: * p<0.10, **p<0.05, ***p<0.01. Data come from the IPUMS-CPS. Thirty-two states had not expanded their Medicaid program as of 2014. 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 fixed effects.

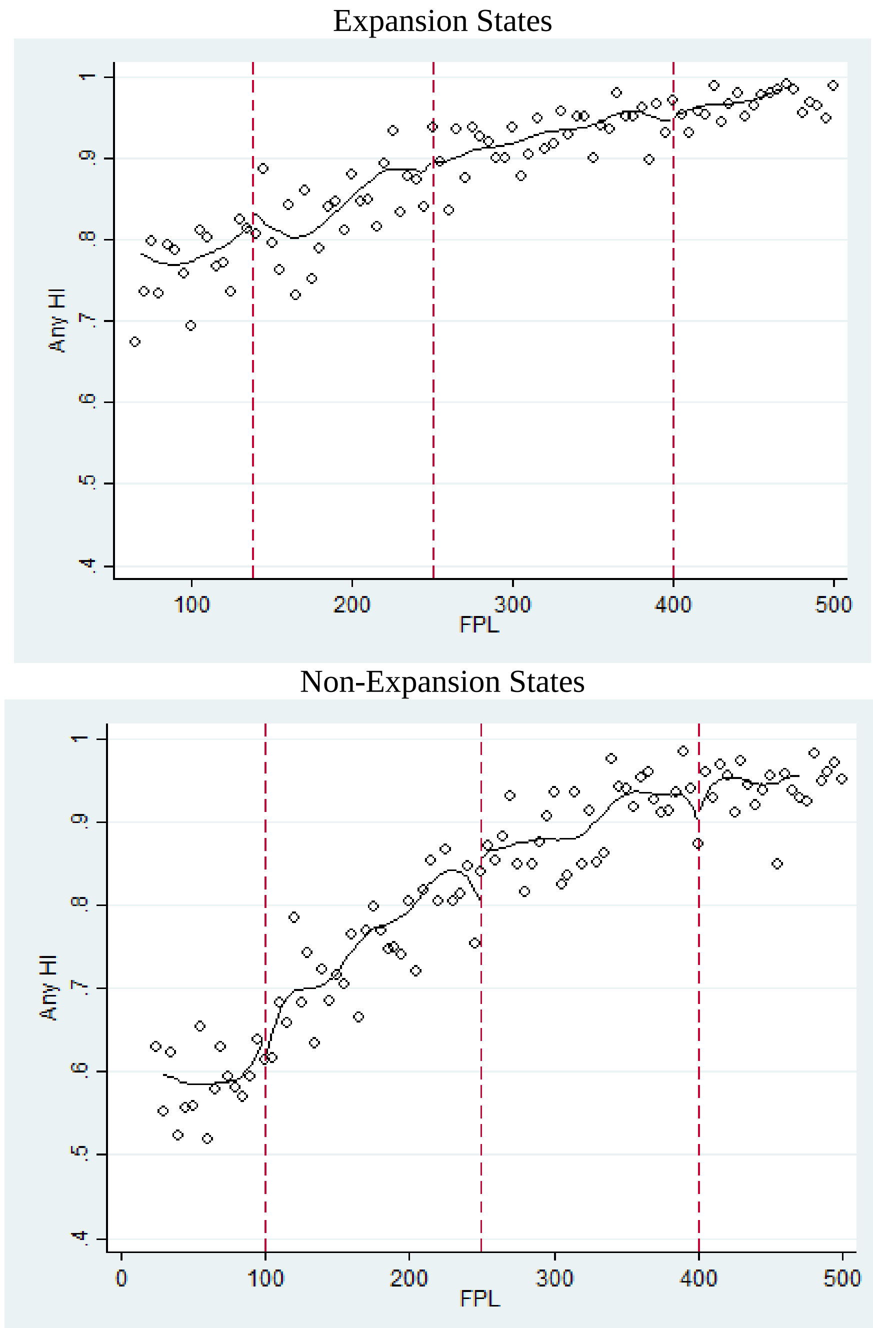
## FIGURES

### Figure 1. FPL Density Estimates, Post- and Pre-2014




Notes: Data come from the IPUMS-CPS. Bars represent a 10% FPL bin. Vertical lines represent the 138%/100%, 250% and 400% FPL cutoffs.

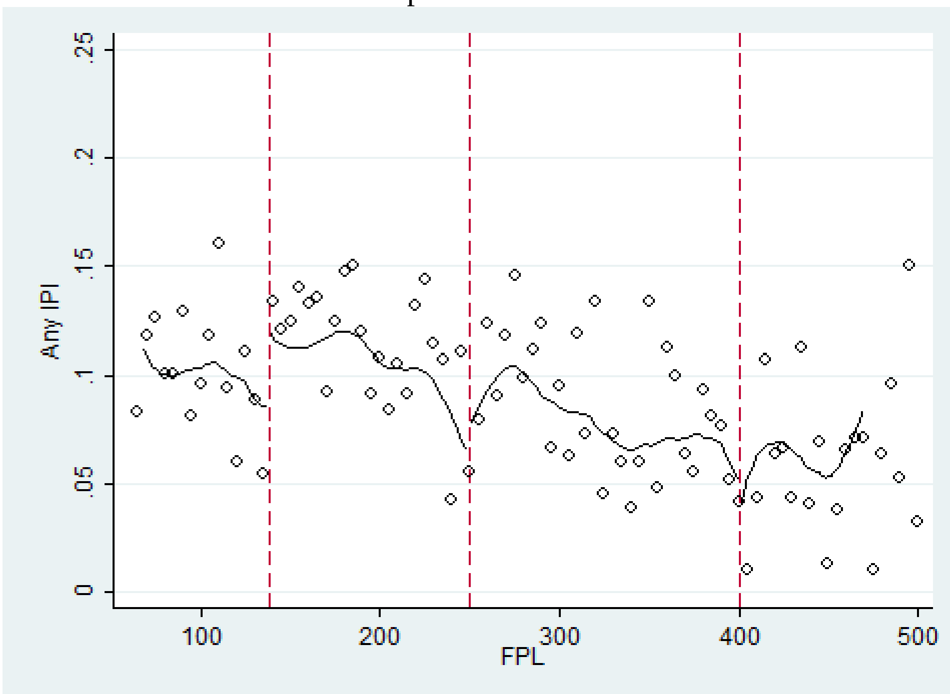
**Figure 2. Any HI coverage by 5% FPL Bins in 2014**



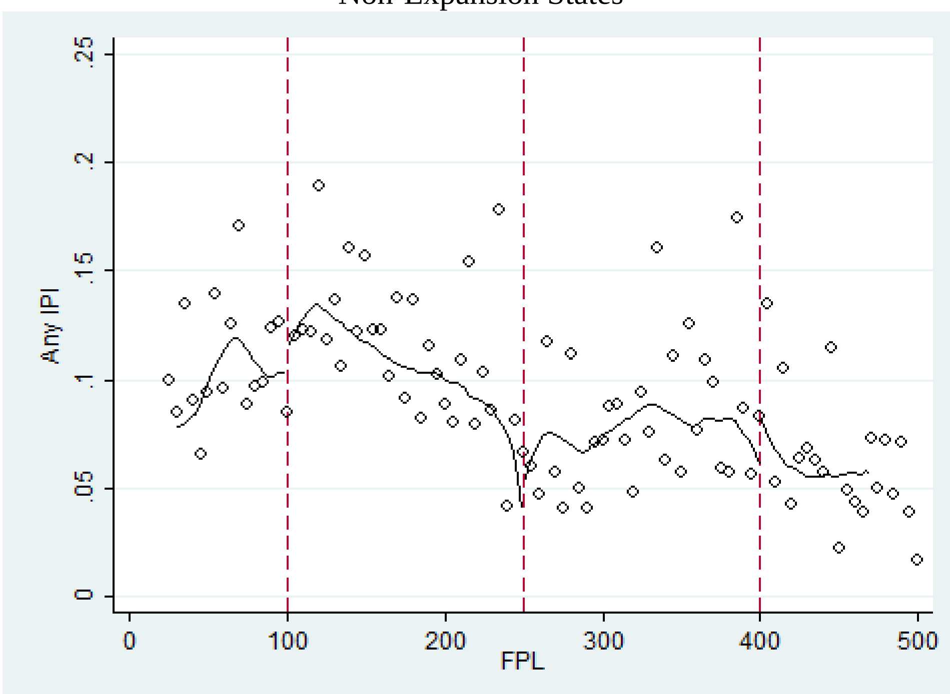

Notes: Data come from the IPUMS-CPS. Symbols represent the proportion covered in a 5% FPL bin. Vertical dashed lines represent the 138%/100%, 250% and 400% FPL cutoffs. Local linear trends are imposed between each cutoff. Estimates are weighted using ASEC supplement probability weights.

**Figure 3. IPI Coverage by 5% FPL Bins in 2014**

Expansion States

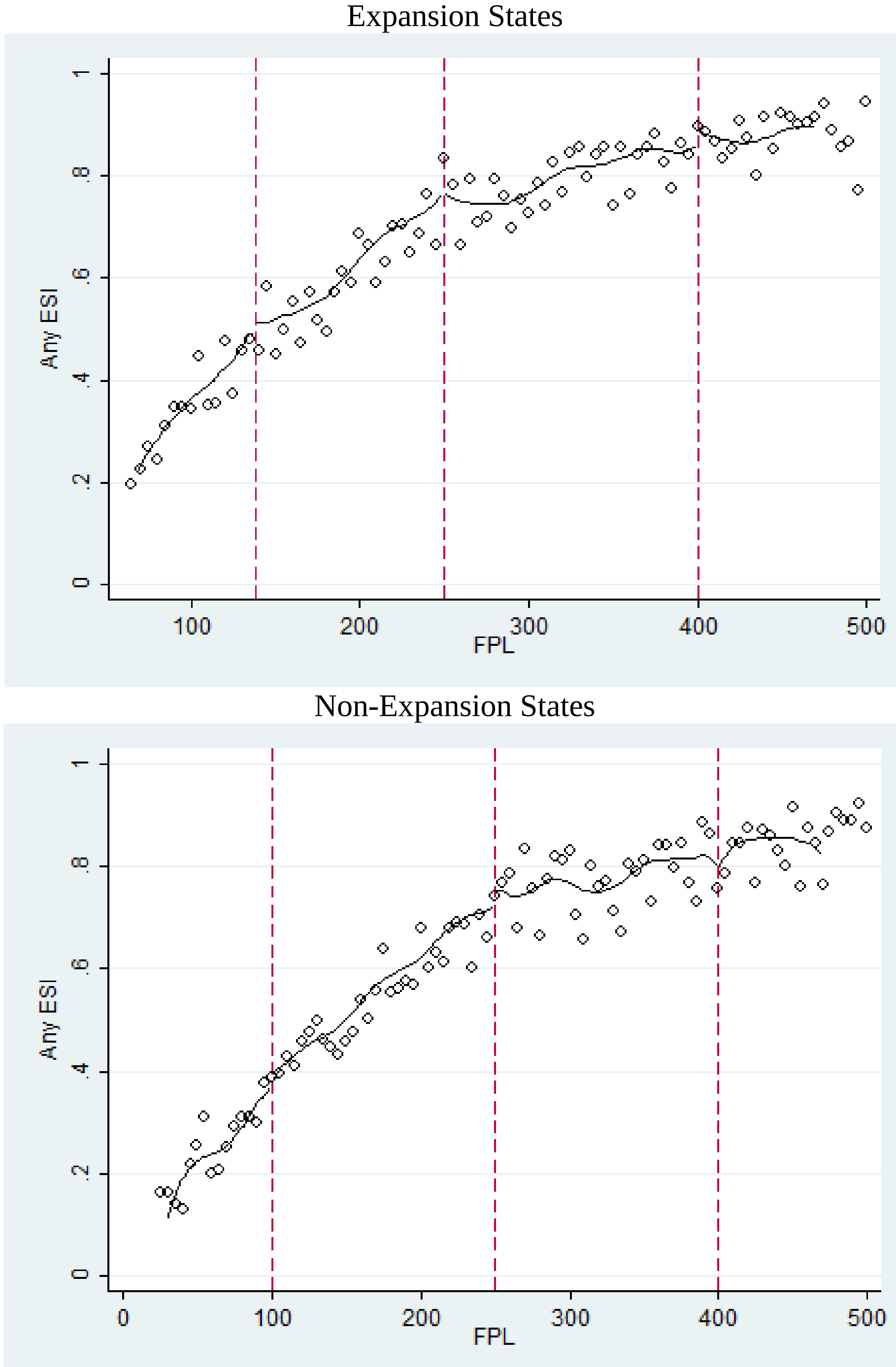



Non-Expansion States




Notes: Data come from the IPUMS-CPS. Symbols represent the proportion covered in a 5% FPL bin. Vertical dashed lines represent the 138%/100%, 250% and 400% FPL cutoffs. Local linear trends are imposed between each cutoff. Estimates are weighted using ASEC supplement probability weights.

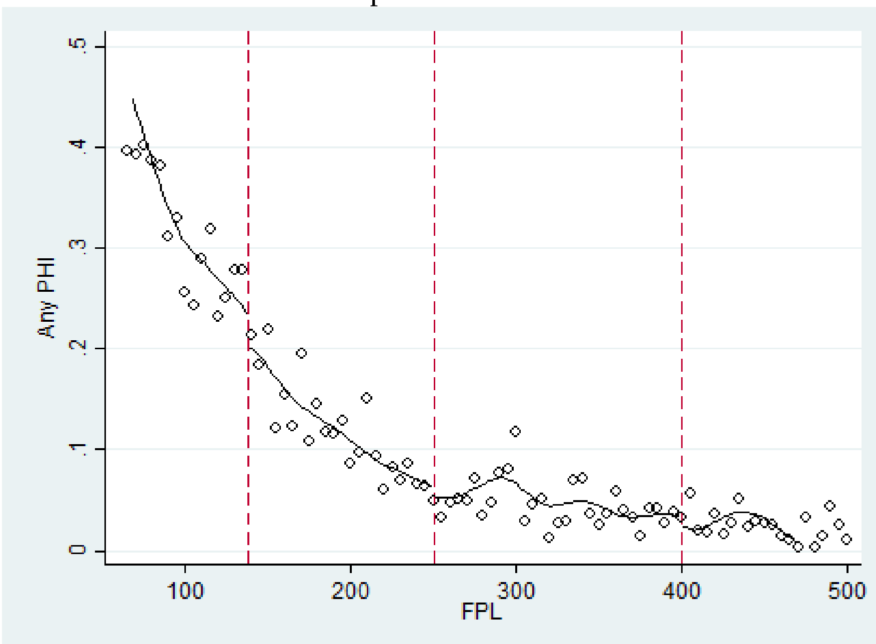
**Figure 4. ESI Coverage by 5% FPL Bins in 2014**



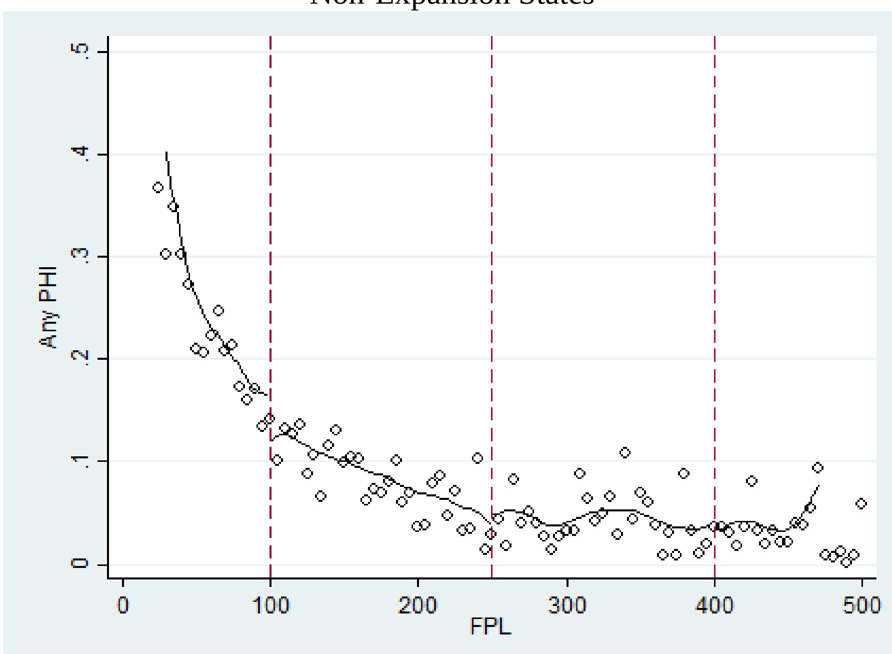

Notes: Data come from the IPUMS-CPS. Symbols represent the proportion covered in a 5% FPL bin. Vertical dashed lines represent the 138%/100%, 250% and 400% FPL cutoffs. Local linear trends are imposed between each cutoff. Estimates are weighted using ASEC supplement probability weights.

**Figure 5. PHI Coverage by 5% FPL Bins in 2014**

Expansion States

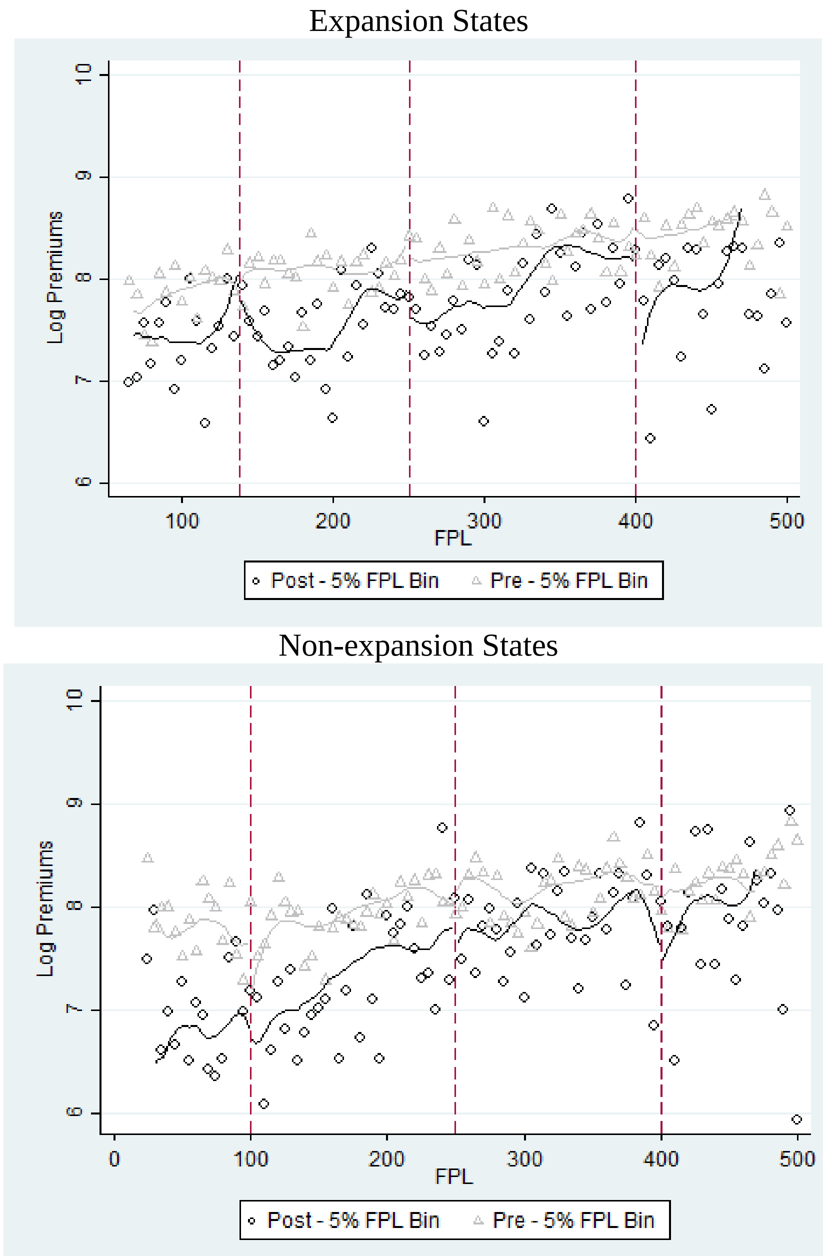



Non-Expansion States




Notes: Data come from the IPUMS-CPS. Symbols represent the proportion covered in a 5% FPL bin. Vertical dashed lines represent the 138%/100%, 250% and 400% FPL cutoffs. Local linear trends are imposed between each cutoff. Estimates are weighted using ASEC supplement probability weights.

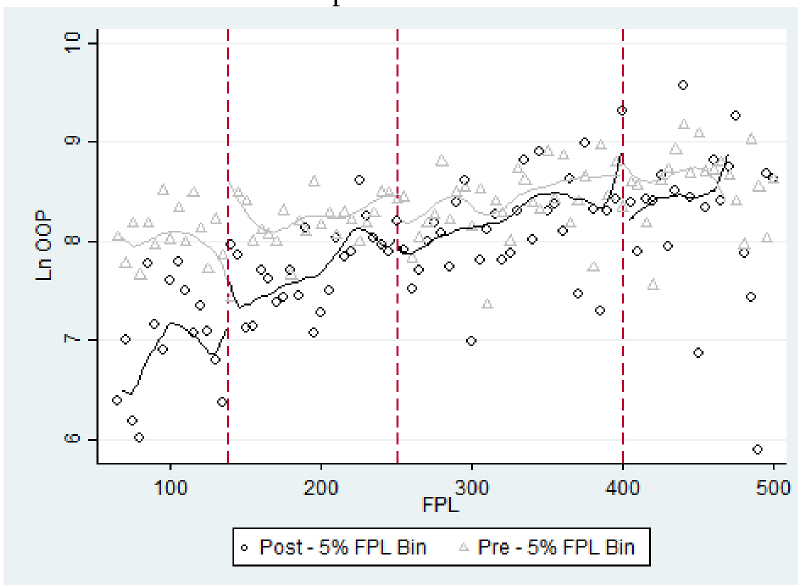
**Figure 6. Log Non-Zero HI Premiums for IPI-covered Individuals in 2014**



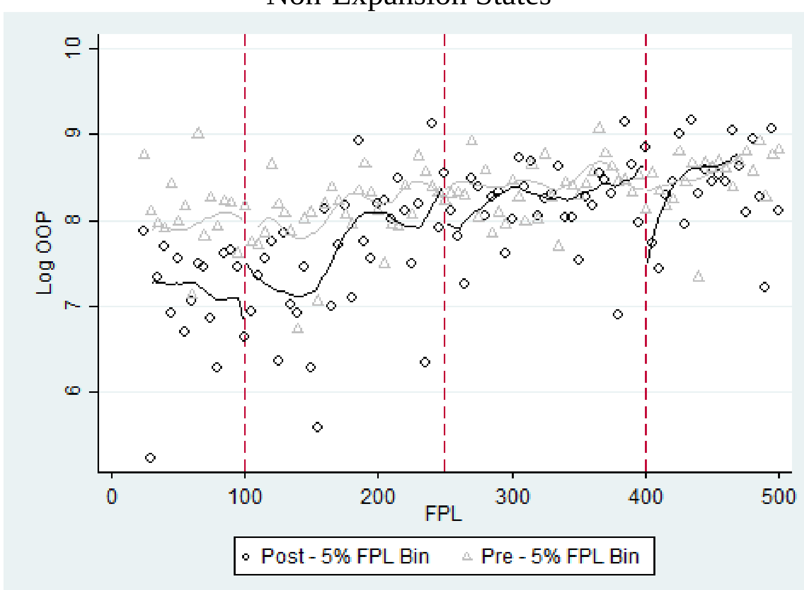

Notes: Data come from the IPUMS-CPS. Symbols represent the proportion covered in a 5% FPL bin. Vertical dashed lines represent the 138%/100%, 250%, and 400% FPL cutoffs. Local linear trends are imposed between each cutoff. Estimates are weighted using ASEC supplement probability weights.

**Figure 7. 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 5% FPL bin. Vertical dashed lines represent the 138%/100%, 250%, and 400% FPL cutoffs. Local linear trends are imposed between each cutoff. Estimates are weighted using ASEC supplement probability weights.

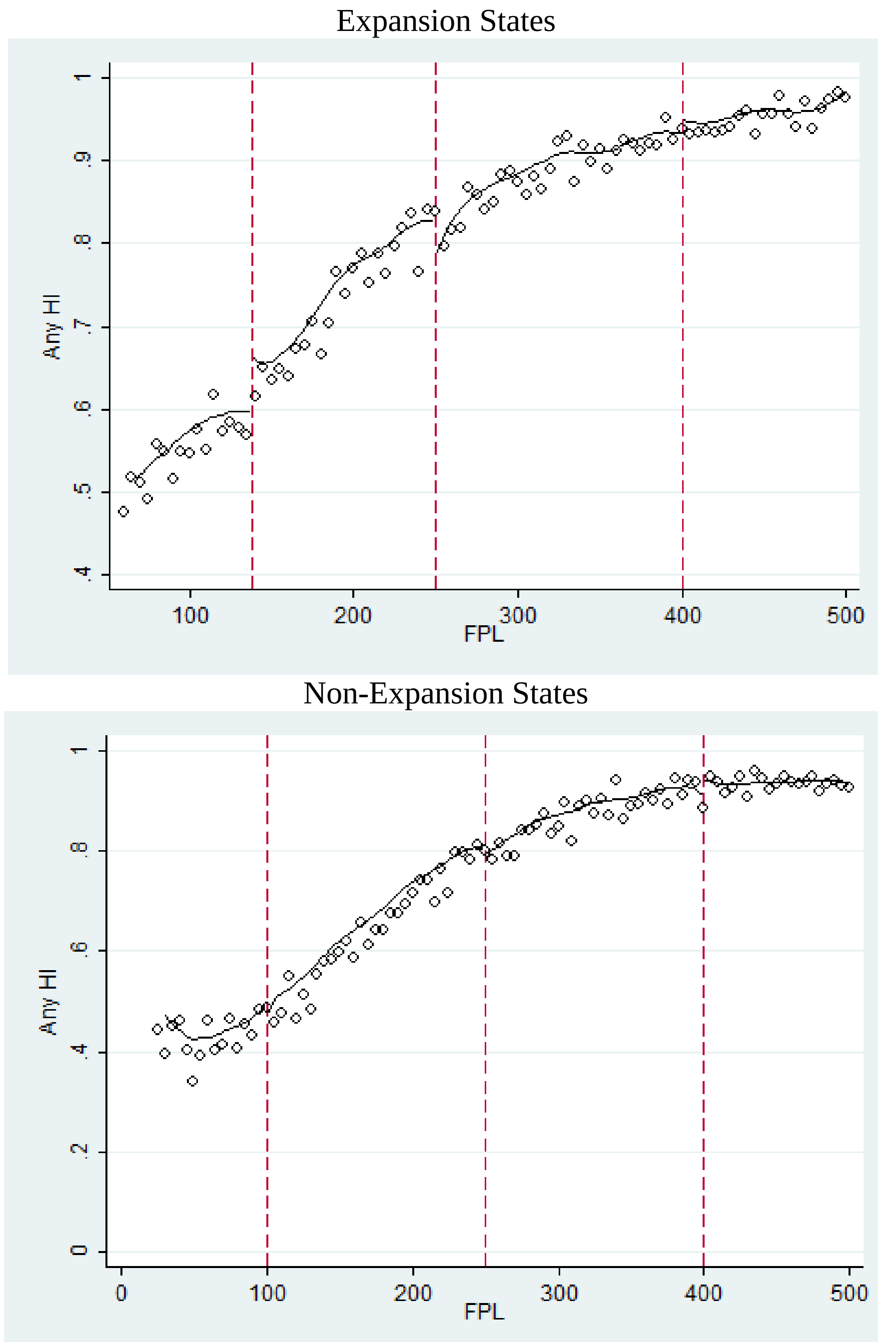
**APPENDIX**

**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 Sta | | |
|---|---|---|---|---|---|---|---|---|
| **138% FPL** N=30,603 | **Any HI** | **IPI** | **ESI** | **PHI** | **100% FPL** N=20,877 | **Any HI** | **IPI** | **ESI** |
| **Non-parametric** | 0.024* | 0.005 | 0.010 | 0.008 | **Non-parametric** | -0.001 | -0.010 | -0.005 |
| | (0.013) | (0.006) | (0.014) | (0.008) | | (0.016) | (0.007) | (0.015) |
| **Linear** | 0.022 | 0.005 | 0.007 | 0.010 | **Linear** | 0.007 | -0.009 | 0.002 |
| | (0.015) | (0.007) | (0.015) | (0.010) | | (0.018) | (0.008) | (0.017) |
| **250% FPL** N=27,440 | | | | | **250% FPL** N=20,977 | | | |
| **Non-parametric** | 0.024** | 0.008 | 0.020 | -0.003 | **Non-parametric** | 0.026* | 0.015** | 0.011 |
| | (0.012) | (0.007) | (0.013) | (0.005) | | (0.014) | (0.008) | (0.016) |
| **Linear** | 0.014 | 0.006 | 0.012 | -0.005 | **Linear** | 0.018 | 0.016* | 0.006 |
| | (0.014) | (0.008) | (0.015) | (0.006) | | (0.016) | (0.009) | (0.018) |
| **400% FPL** N=22,449 | | | | | **400% FPL** N=15,872 | | | |
| **Non-parametric** | 0.009 | -0.006 | 0.015 | 0.001 | **Non-parametric** | -0.007 | -0.011 | -0.001 |
| | (0.009) | (0.007) | (0.011) | (0.005) | | (0.011) | (0.008) | (0.014) |
| **Linear** | 0.008 | -0.006 | 0.015 | -0.001 | **Linear** | -0.001 | -0.011 | 0.005 |
| | (0.010) | (0.008) | (0.013) | (0.006) | | (0.012) | (0.010) | (0.017) |

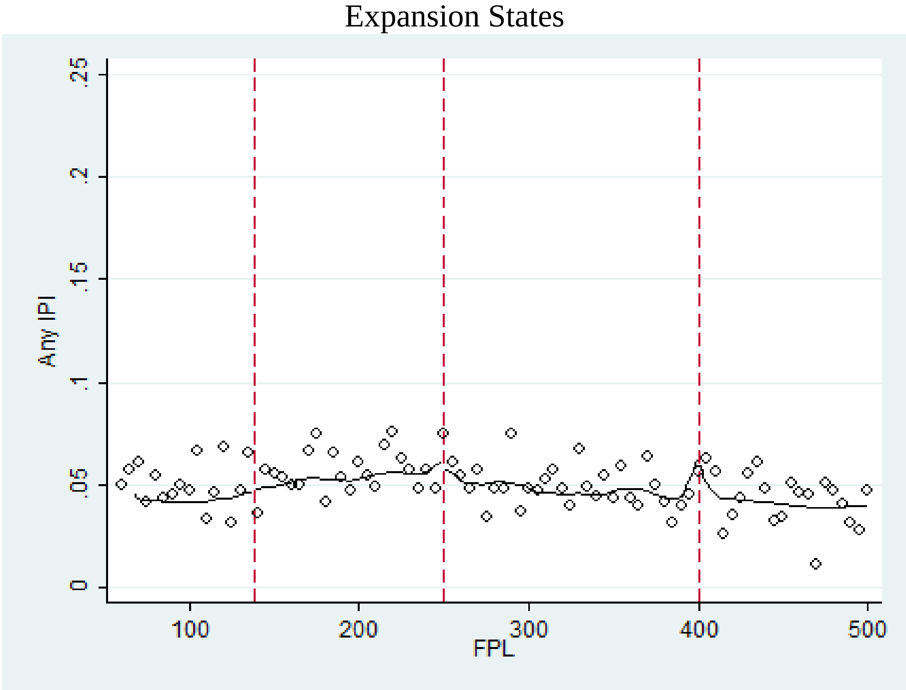
Notes: * p<0.10, **p<0.05, ***p<0.01. Data come from the IPUMS-CPS. Twenty-eight states had expanded their Medicaid program by 2014. IPI = individually purchased private insurance. Observations within 70% on either side of the cutoff are included. Standard errors are in parentheses. Non-parametric RD 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 fixed effects.

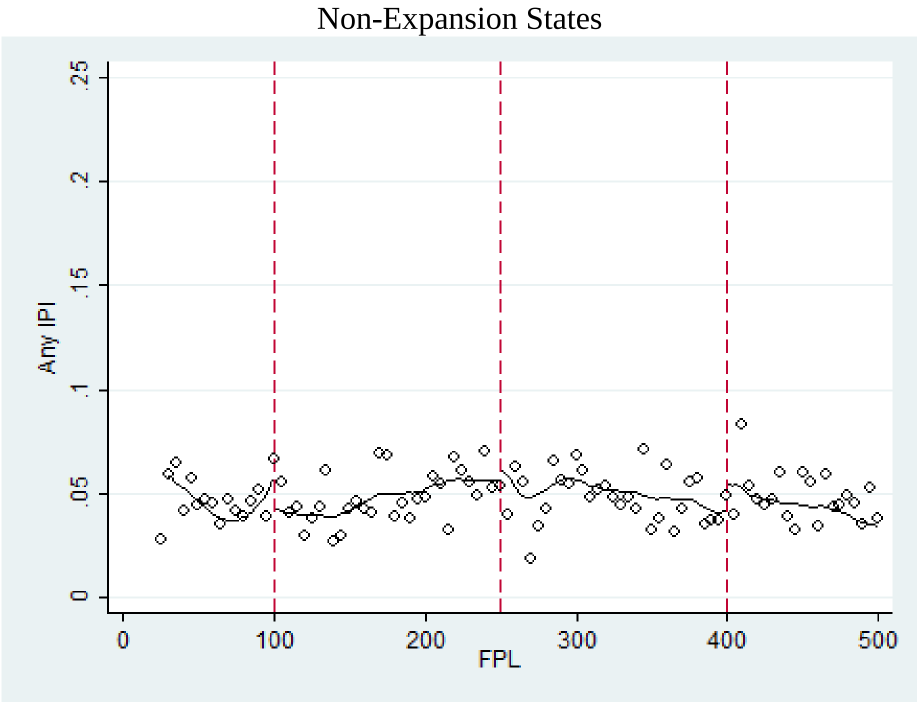
**Figure A-1. Any HI Coverage by 5% FPL Bins in 2010–2012**




Notes: Data come from the IPUMS-CPS. Symbols represent the proportion covered in a 5% FPL bin. Vertical dashed lines represent the 138%/100%, 250% and 400% FPL cutoffs. Local linear trends are imposed between each cutoff. Estimates are weighted using ASEC supplement probability weights.

**Figure A-2. IPI Coverage by 5% FPL Bins in 2010–2012**

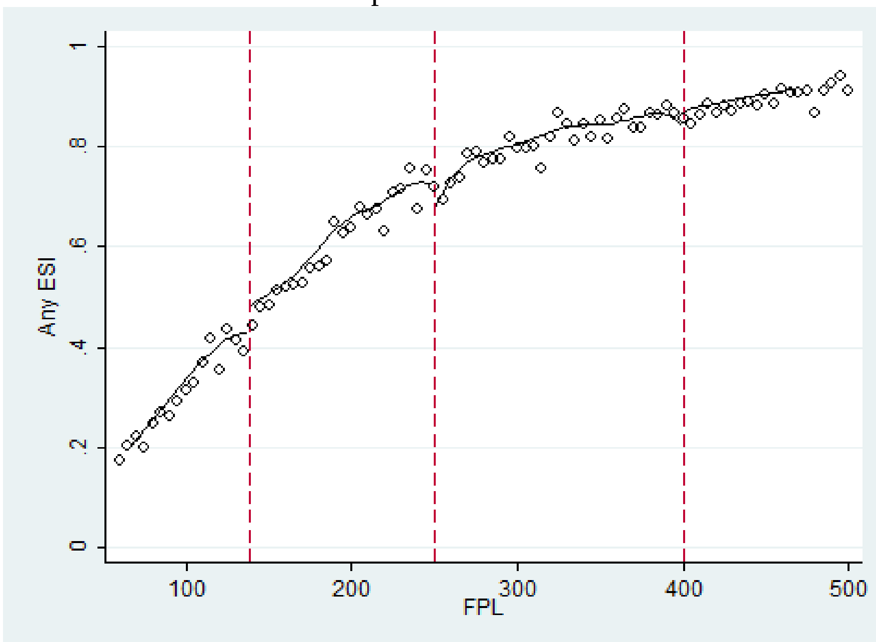


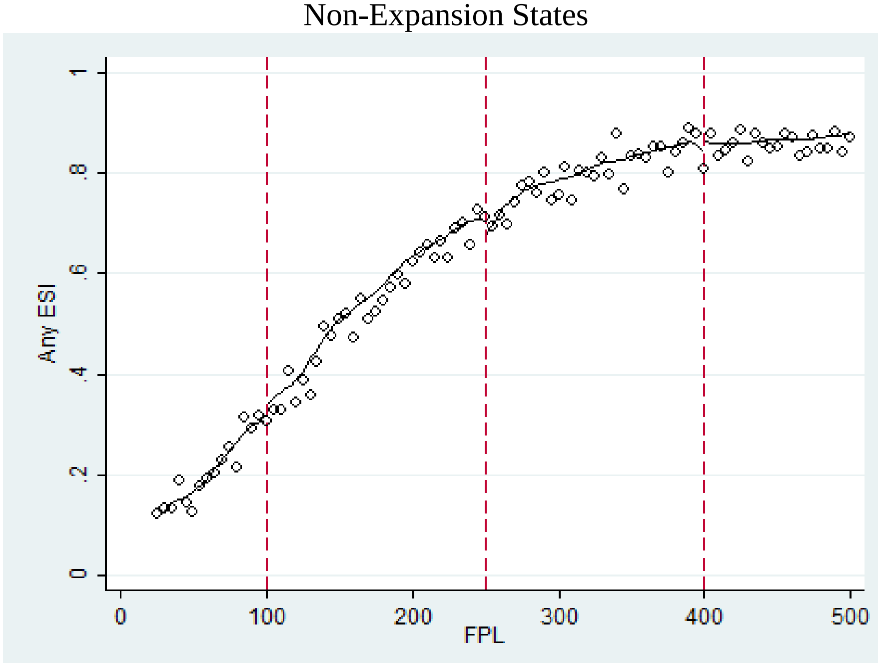

Notes: Data come from the IPUMS-CPS. Symbols represent the proportion covered in a 5% FPL bin. Vertical dashed lines represent the 138%/100%, 250% and 400% FPL cutoffs. Local linear trends are imposed between each cutoff. Estimates are weighted using ASEC supplement probability weights.

**Figure A-3. ESI Coverage by 5% FPL Bins in 2010–2012**

Expansion States



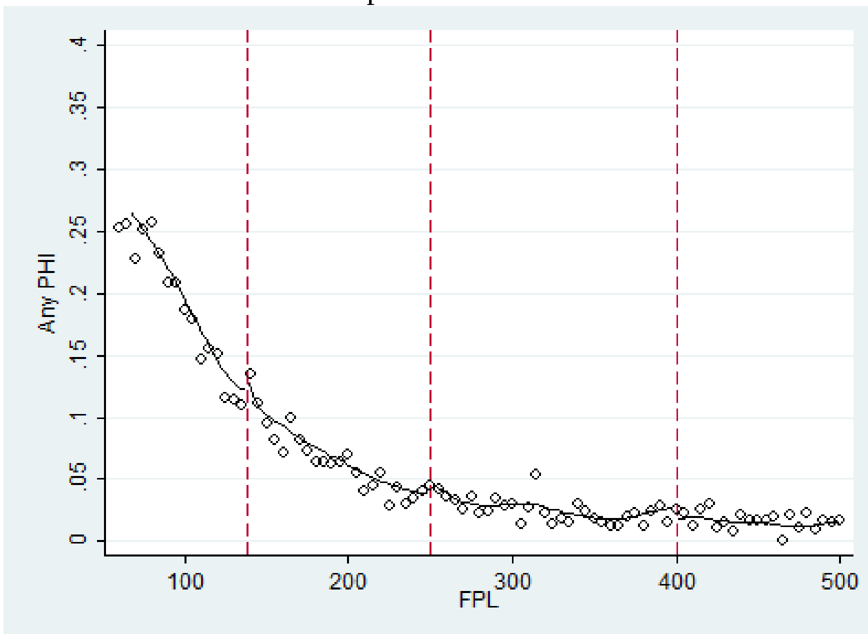

Non-Expansion States



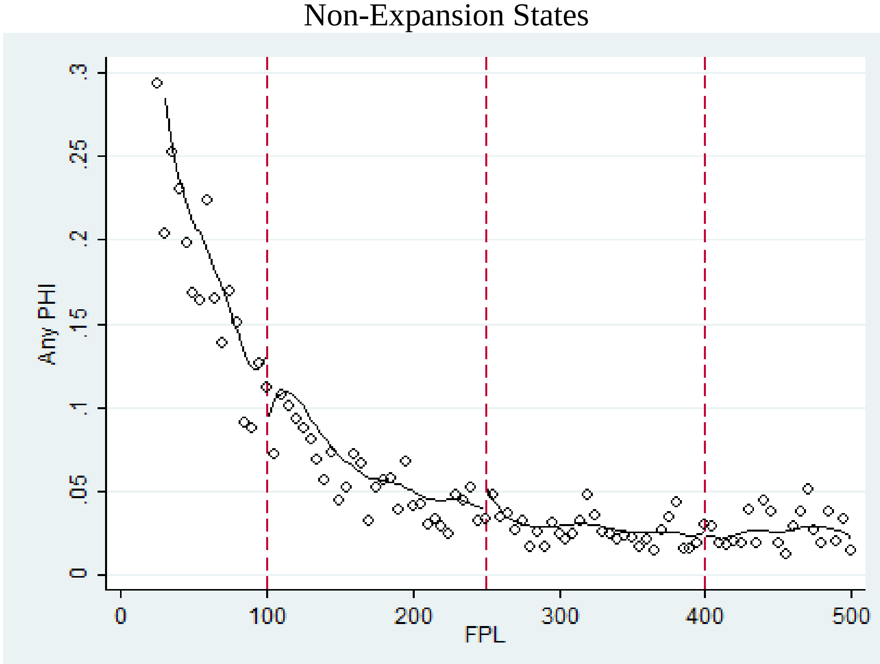

Notes: Data come from the IPUMS-CPS. Symbols represent the proportion covered in a 5% FPL bin. Vertical dashed lines represent the 138%/100%, 250% and 400% FPL cutoffs. Local linear trends are imposed between each cutoff. Estimates are weighted using ASEC supplement probability weights.

**Figure A-4. PHI Coverage by 5% FPL Bins in 2010-2012**

Expansion States



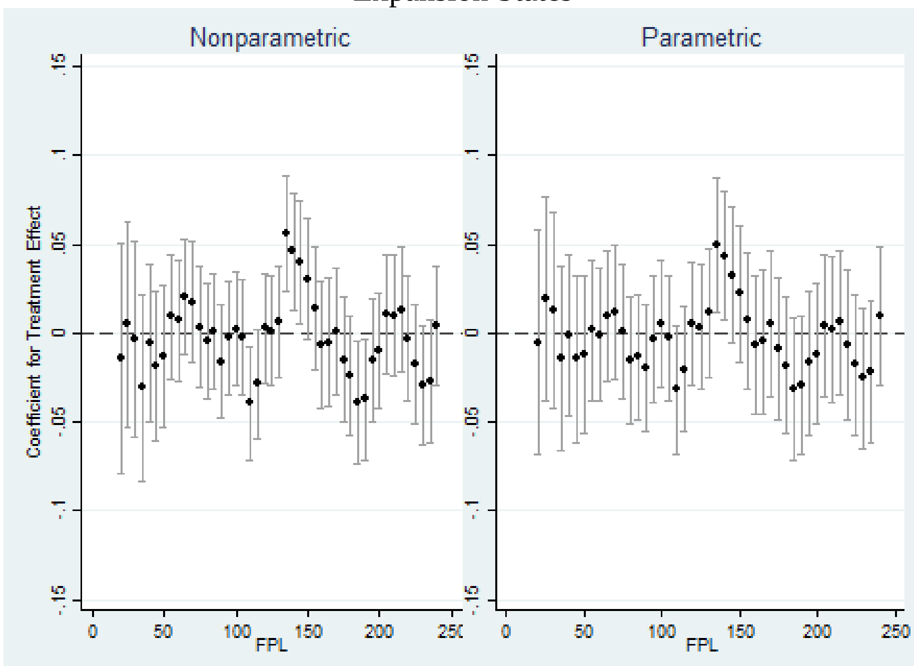

Non-Expansion States



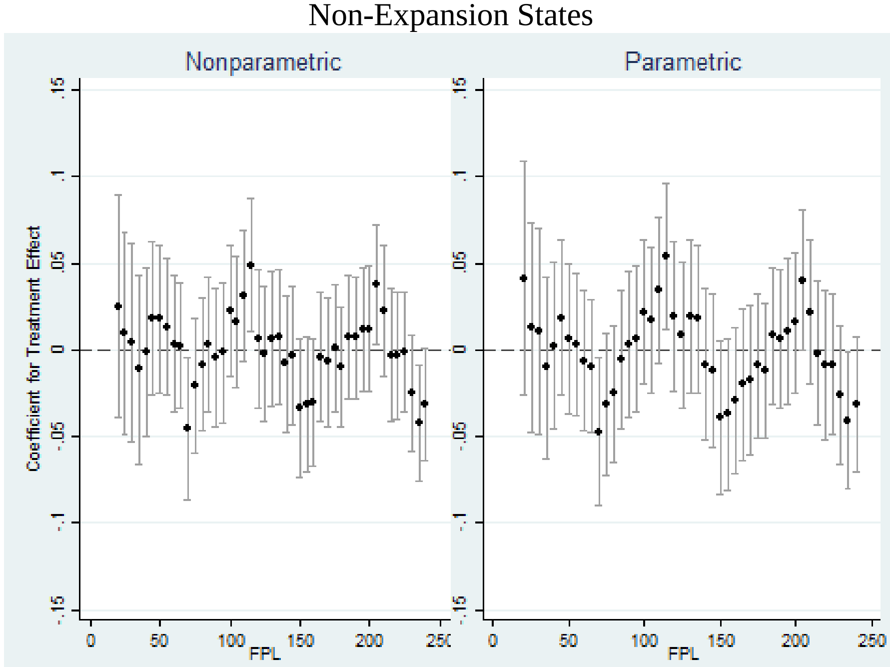

Notes: Data come from the IPUMS-CPS. Symbols represent the proportion covered in a 5% FPL bin. Vertical dashed lines represent the 138%/100%, 250% and 400% FPL cutoffs. Local linear trends are imposed between each cutoff. Estimates are weighted using ASEC supplement probability weights.

**Figure A-5. Permutation Testing for Different FPL Cutoffs for the Probability of having IPI in 2014, 38%-238% FPL**

Expansion States



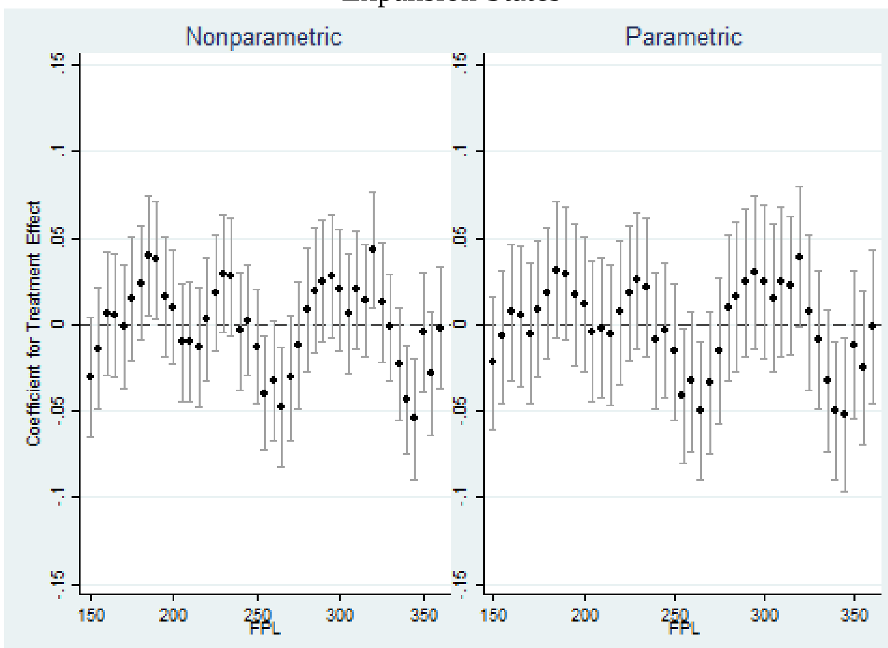

Non-Expansion States



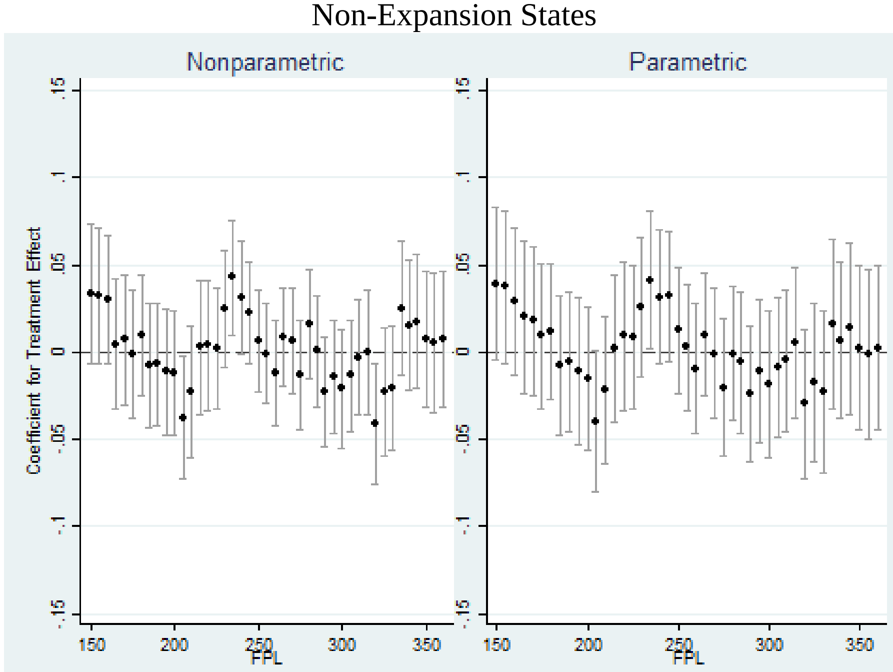

Notes: Points represent the coefficient estimate for the treatment effect using different FPL cutoffs. Vertical bars are 95% confidence intervals

**Figure A-6. Permutation Testing for Different FPL Cutoffs for the Probability of having IPI in 2014, 150%-350% FPL**

Expansion States



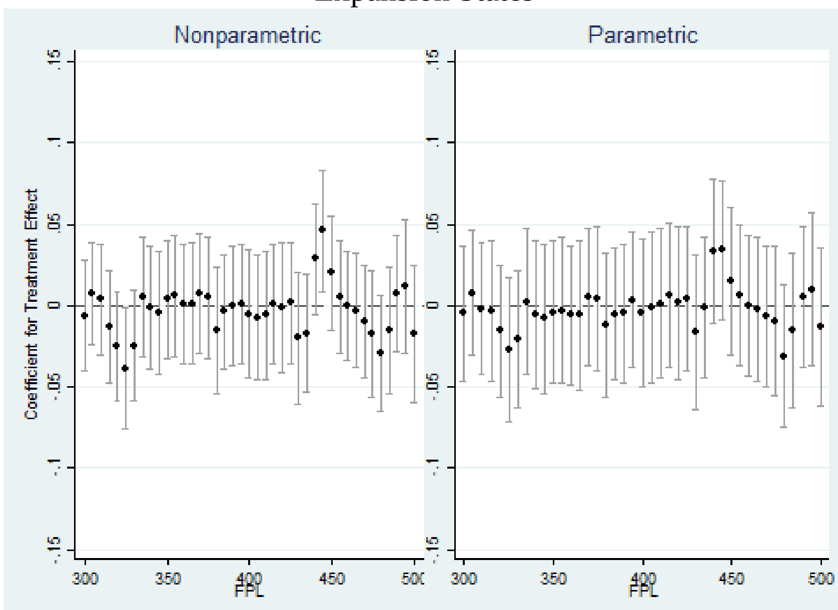

Non-Expansion States



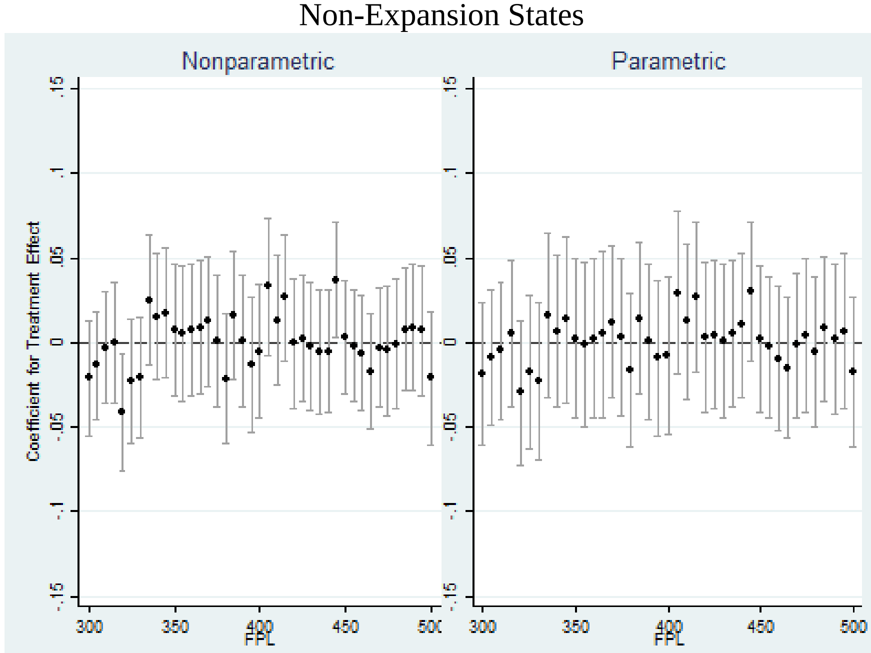

Notes: Points represent the coefficient estimate for the treatment effect using different FPL cutoffs. Vertical bars are 95% confidence intervals

**Figure A-7. Permutation Testing for Different FPL Cutoffs for the Probability of having IPI in 2014, 300%-500% FPL**

Expansion States



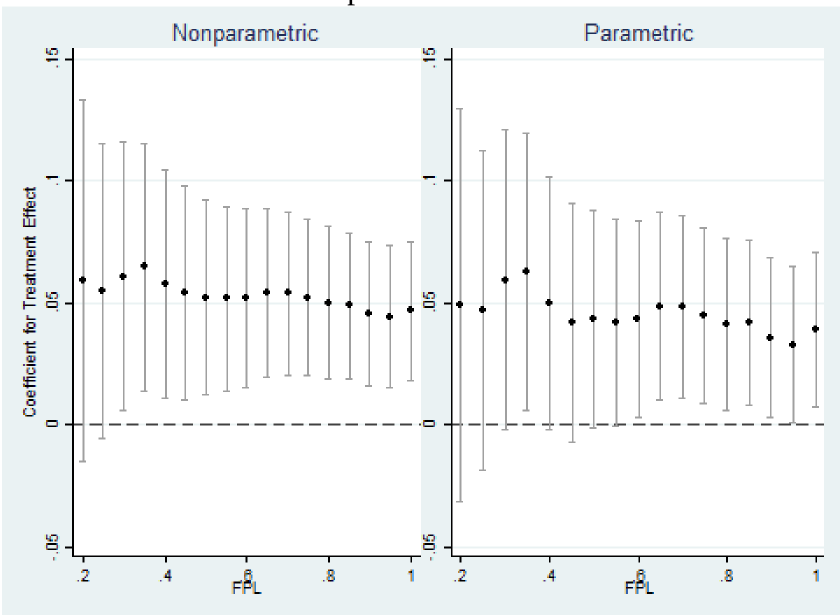

Non-Expansion States



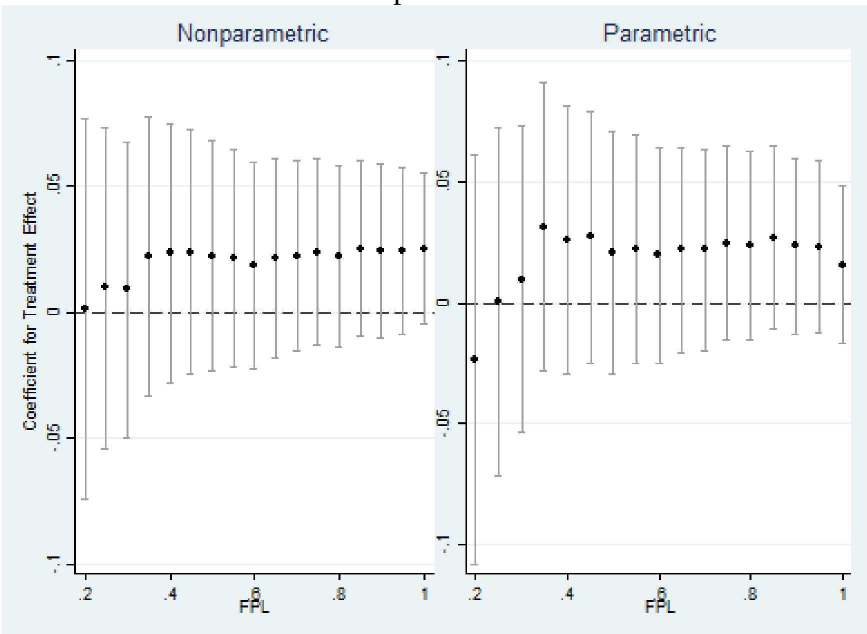

Notes: Points represent the coefficient estimate for the treatment effect using different FPL cutoffs. Vertical bars are 95% confidence intervals

**Figure A-8. 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.