EXAMINING THE IMPACTS OF HEALTH INSURANCE COSTS AND HEALTH REFORM ON PRIVATE INSURANCE COVERAGE, EMPLOYMENT, AND WAGES

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ABSTRACT

Jesse Michael Hinde: Examining the Impacts of Health Insurance Costs and Health Reform on Private Insurance Coverage, Employment, and Wages
(Under the guidance of Christine Piette Durrance)

This dissertation is focused on private health insurance coverage, health reform and labor market outcomes. Using novel and rigorous empirical strategies, the first two essays estimate the impact of health insurance tax credits adopted during Massachusetts's 2006 health reform and as a part of the Affordable Care Act (ACA) in 2014 on non-group private health insurance coverage. In Massachusetts, I find a large response on the margin for the tax credits. For the ACA, I document robust, positive effects on private coverage at the lowest eligibility threshold and weak evidence of effects at higher thresholds. Separating these effects from other important ACA policies, such as Medicaid expansion or the individual mandate, is vital to future efforts to modify and sustain the progress made by the ACA.

The third essay addresses a significant gap in the literature, examining how employer-sponsored health insurance (ESI) affects the earnings distribution. I examine the role of sample selection and selection bias as an explanation for the inconsistent findings in the literature. Using quantile regression, I show that that cost-shifting due to compensating wage differentials occurs and that cost-shifting can be offset for higher earnings due to higher marginal tax rates, producing net-positive effects. Together, my dissertation indicates that reducing reliance on ESI may have beneficial effects on earnings for low- and middle-income individuals and that health insurance tax credits provide an appealing, alternative coverage option.

This dissertation is dedicated to my wife, Tori. I cannot thank her enough for pushing me to pursue my doctorate and being my partner in this endeavor. Although the writing and analysis presented herein is my own, she deserves most of the credit for its completion. I also dedicate this dissertation to my parents, Jim and Michele. My father instilled in me the importance of a steady work ethic and taught me to value the process, not just the result. Hard work is not a sufficient condition for success, but it is necessary. To my mother, who always showed unwavering confidence in my abilities and passed on a principle of fairness and justice, I thank you for mentally and emotionally preparing me to survive this process. I am proud to be their son and this dissertation is as much reflection on their investment in me. Finally, I dedicate this dissertation to my children, Natalie and Jocelyn. I will always cherish that you think I just completed high school. In twenty years, I hope you may find this, not be embarrassed, and find some inspiration for your own lives.

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TABLE OF CONTENTS

LIST OF TABLES

LIST OF FIGURES

LIST OF ABBREVIATIONS

CPS: Current Population Survey

ESI: employer-sponsored insurance

FPL: federal poverty level

HI: health insurance

IPI: individually purchased insurance

PHI: public health insurance

RD: regression discontinuity

ESI PH: Employer-sponsored insurance policy holder

FTFY: Full-time, full year

PTPY: Part-time or part-year

FULL: refers to the sample of all workers, including both FTFY and PTPY workers

FTFY-ESI: Refers to the sample of FTFY workers that are ESI PHs or ESI dependents

PTPY-ESI: Refers to the sample of PTPY workers that are ESI PHs or ESI dependents

CPS: Current Population Survey

ASEC: Annual Social and Economic Supplement

CHAPTER 1: INTRODUCTION

Recent health reform policies in the United States (US) focus on increasing access to, improving the quality of, and reducing the cost of medical care. Access, quality and cost are in many ways governed by health insurance. Given rising medical costs, health insurance not only provides indemnity against unexpected health shocks but affordable access to basic and routine medical services. Health insurance in the US is predominately provided through employment, with more than 60% of the population covered by an employer-sponsored health insurance (ESI) plan (DeNavas et al. 2009). ESI is appealing in large part because it offers a significant price reduction relative to private, non-group market prices. Individuals without access to ESI must rely on a volatile and expensive non-group market, obtain coverage through a public program if eligible, or be uninsured. Thus, although the Patient Protection and Affordable Care Act of 2010 (ACA) requires that employers offer health insurance, a major push of the ACA is to increase the availability and affordability of insurance through public insurance coverage expansions and through subsidy programs for private non-group insurance available on online marketplaces.

In 2014, the ACA implemented premium tax credits to individuals with incomes below 400% of the federal poverty level (FPL) and cost-sharing subsidies to individuals below 250% FPL. Subsidized plans were available on state or federal health insurance (HI) exchanges, i.e., online marketplaces. Nearly 8 million people signed up for coverage in 2014 and more than 80% of individuals who enrolled in the exchanges received either tax credits or cost-sharing subsidies. The average tax credit received was approximately $3,000 ($243/month) (ASPE 2014). The cost-sharing subsidies reduced co-insurance, deductibles, and out-of-pocket maximums. These two subsidies represent the broadest offering of subsidies for non-group health insurance. The ACA subsidies build on the subsidy plan first offered in Massachusetts as part of an earlier health reform in 2006.

There is limited evidence of the effectiveness of insurance subsidies for private, non-group health insurance. Existing subsidies are largely built into the tax system, providing general deductions for health care costs above 7.5% of adjusted gross income or more specific deductions (e.g., the self-employed). Two recent studies focused on subsidies available to the self-employed (tax-based) and those recently unemployed (not tax-based) and have found modest, positive effects on insurance coverage (Heim & Lurie 2009, Moriya & Simon 2016).

In the context of the ACA, while eligible individuals receive a substantial subsidy, the monthly premiums may still be higher than individuals are willing to pay. Additionally, the search costs of navigating the exchanges may provide additional disincentive. By examining the effectiveness of the tax credits and cost-sharing combined and separately, this dissertation provides evidence on how consumers respond to differing levels of subsidies and how the incentives could be altered to increase participation. Two chapters in this dissertation examine the Massachusetts and broader ACA subsidy schemes.

A secondary line of inquiry in my dissertation focuses on the tradeoff between increasing ESI premiums and wages. As noted, ESI is the predominate form of health insurance in the US and offers a substantial price reduction relative to the individual market. Because insurance is largely linked to employment, microeconomic theory implies an unambiguous negative effect on earnings and an uncertain effect on employment levels. The ACA was politically framed as a threat to employment and wages but initial evidence suggests that the impacts on employment are minimal. The impact on earnings has not yet been studied.

More broadly, while the hypothetical tradeoff between ESI and earnings is well studied, the empirical evidence for a tradeoff is mixed and the mechanism by which increasing ESI premiums reduces wages is not well understood (Currie & Madrian 1999). ESI premiums increased more than 150% in the two decades prior to the implementation of the ACA and premiums markedly increased shortly after the ACA was passed. Since ESI remains the prevalent source of insurance coverage, the indirect consequences for earnings is an important consideration for policy makers. An offer of ESI voids eligibility for the ACA subsidies, which provides low- and middle-income little alternative to the coverage their employer offers in the face of an earnings tradeoff. As the existing literature focuses largely on the average effects of ESI premium increases on earnings, the third paper in this dissertation examines the distributional effects of ESI premium increases. Identifying vulnerable parts of the earnings distribution prior to the ACA can help to simulate and identify potential welfare impacts of the ACA policies.

CHAPTER 2: DO PREMIUM TAX CREDITS INCREASE PRIVATE HEALTH INSURANCE COVERAGE? EVIDENCE FROM THE 2006 MASSACHUSETTS HEALTH CARE REFORM

Introduction

The costs of health care and health insurance (HI) have increased dramatically over the past several decades in the United States. Many individuals and families have relied on employer-sponsored insurance (ESI) for affordable HI, but ESI has eroded recently as costs climb. To attempt to address this, many states have expanded public health insurance (PHI) to cover low-income families. In 2006, Massachusetts implemented a novel health reform that provided a marketplace for individuals to purchase HI directly. The marketplace was coupled with an individual mandate that ensured a large enough risk pool to contain premiums. To further incentivize participation, Massachusetts subsidized premiums for individuals below 300% of the federal poverty level (FPL).

Extensive literature has examined the broad impact of the Massachusetts reforms on the insured rate (e.g., Pande et al., 2011) and a variety of health and health care utilization outcomes (e.g., Kolstad & Kowalski, 2012). A methodological difficulty with such an extensive set of policies is to isolate the effects of different policy components. No study to date has looked directly at the tax credits. This study uses regression discontinuity (RD) to compare non-group private insurance coverage of individuals just below 300% FPL who were eligible for a tax credit to individuals just above who were not eligible.

The tax credits reduce the up-front cost of obtaining HI, but they still require the individual to contribute some of the cost. Tax credits represent a new form of income transfer, and their effect has little empirical evidence. Evidence to date has focused on individuals who are laid off or are self-employed, and associated subsidies have produced modest, positive impacts. Given static premium costs, some consumers may not want HI regardless of the subsidy, and some may want it without a subsidy. From a policy maker's perspective, the population of interest is those on the margin of purchasing insurance. The tax credit must be large enough to encourage participation for consumers who want insurance but not at pre-reform prices. I test whether the tax credits were large enough to increase participation.

Materials and Methods

The Current Population Survey (CPS) was chosen because it captures income, HI, and demographics before and after the 2006 Massachusetts health reform (Flood et al., 2015). The pre-reform period comprises calendar years 1999 through 2006, and the post-reform period comprises calendar years 2007 through 2009. The sample includes adults aged 18 to 64 and excludes veterans and individuals with imputed HI responses.

Although individuals can report multiple types of HI in a year, I used three exclusive categories for HI based on guidance from the literature: the primary outcome, individually purchased insurance (IPI); ESI; and PHI. If an individual reports ESI, they are excluded from being in the IPI or PHI. Individuals who report any ESI or IPI are not included in the PHI.

Using a RD design, the forcing variable is the respondent's income relative to the FPL. FPL is the ratio of total family income to the federally determined poverty threshold. The threshold is based on the size of the family. I focus on 300% FPL, which is the upper limit for tax credit eligibility. The tax credit had an average value of approximately $1,500 just below 300% FPL. A series of individual variables is also used to control for potential confounding factors: age, gender, race, ethnicity, marital status, family size, urbanicity, education, and self-reported health status.

I estimated the RD model at 300% FPL using both parametric and nonparametric models. The base parametric specification is:

$$HI_i = \alpha + \beta_1 SUB(FPL < 300)_{\,i} + \beta_2 FPL(x - 300)_i + \beta_3 SUB(FPL < 300)_{\,i} * FPL(x - 300)_i + \delta \boldsymbol{X}_i + \tau_i + \varepsilon_i$$

where *HI* is a binary HI indicator, *SUB* is a binary indicator for below 300% FPL. *FPL* is centered at 300%, $\boldsymbol{X}$ is a vector of individual demographics described above, and τ_i are year fixed effects. ε_i is assumed to be an independently and identically distributed error term. β_1 is the treatment effect at the discontinuity. All models use the HI-specific probability weight. I estimate the above equation with and without $\boldsymbol{X}$ and τ_i and with higher-order FPL terms. Standard errors are clustered on the FPL for the parametric models (Lee & Card, 2008). I also pooled each model and computed a difference-in-differences effect at the cutoff. Lastly, a non-parametric RD was estimated using local linear regression with a triangle kernel density estimator.

Following Imbens and Lemieux (2008), four sensitivity and falsification tests were used to test the robustness of the results: checking for false cutoffs, changing the bandwidth around the cutoff, McCrary's (2008) test manipulation of the forcing variable, and discontinuities in demographic characteristics. An additional test examined nonrandom heaping (Barecca et al., 2011). The sensitivity tests do not meaningfully alter the results of this study.

One concern suggested by Shu (2016) was manipulation in the FPL in Massachusetts around 300% FPL using American Community Survey data. I did not find visual or statistical evidence of manipulation in the CPS using more years than Shu (2016). With self-reported income, families tend to report incomes rounded to the nearest $1,000 or $5,000 increment. Since the FPL variable is created by dividing income by the poverty cutoff, and the poverty cutoff is determined by family size, this creates lumpiness in the histogram (see Online Appendix Figures 1 and 2).

Results

Table 2.1 presents weighted summary statistics for the 1999–2006 and 2007–2009 samples between 230% and 370% FPL. The summary statistics demonstrated a slight increase in IPI and PHI across time. There was little change in demographic characteristics of the sample across time, including education and self-reported health (not presented).

Figure 2.1 presents the main RD results graphically for the post-reform periods for all outcomes, and Table 2.2 presents statistical estimates for the effect shown in Figure 2.1. The bottom left panel of Figure 2.1 shows an increase in IPI just below 300% FPL in the post-reform period and no detectable effect in the pre-reform period. The nonparametric estimates for IPI are a statistically significant increase of 8.4 percentage points, and the cubic model estimates a 19.4 percentage point effect. The linear and difference-in-differences models are similar in magnitude to the non-parametric model, but they are not statistically significant.

Although IPI is the primary outcome, the remainder of Figure 2.1 and Table 2.2 present the broader effects on other HI outcomes. The upper left panel of Figure 2.1 shows that any HI coverage decreased slightly in the post-reform period just below 300% FPL. The estimate for that effect was 4 to 5 percentage points, and it was not statistically significant. Although imprecise, this result suggests that the increase in coverage in IPI due to the subsidies was offset by a general decrease in coverage.

The right two panels of Figure 2.1 explain the decrease in any HI coverage. There was a small decrease in the post-reform period for ESI just below 300% FPL, but it was not statistically significant. There was a much larger decrease in PHI, but the visual evidence in the PHI panel was not as convincing as the IPI panel: there was not a clear break in the PHI trend and much more noise. Still, Table 2.2 suggests that the reduction in PHI was statistically significant in the post-reform period, ranging in effect size from 12 to 18 percentage points.

One potential explanation for the overall decrease in HI and large decrease in PHI is crowd-out. However, there were not concurrent Medicaid policy changes at 300% FPL. These effects could instead be explained by volatility between ESI and PHI due to the Great Recession. For the bin proportions of ESI and PHI in Figure 2.1, spikes in ESI coverage line up with decreases in PHI and vice versa. There were not enough observations to test this hypothesis by looking at years separately.

The permutation testing also provided a meaningful check for interpreting the ESI/PHI effect. The effect for IPI was largest in magnitude and the test statistic relative to all surrounding points in the FPL distribution (see Online Appendix Figure 3), suggesting a valid treatment effect. The permutation tests were much less clear for ESI and PHI where there were large effects in both directions at multiple false cutoffs between 220% and 300% FPL, suggesting the large PHI effects seen at 300% were not associated with the tax credits.

Discussion

This study examined the effectiveness of premium tax credits on IPI associated with the 2006 Massachusetts health reform. I find evidence of an increase in IPI participation among those below the 300% FPL cutoff at which individuals become ineligible for subsidized insurance but a statistically insignificant decrease in any HI coverage.

Using the CPS for a single state limits the statistical power and produces multiple insignificant findings. Beyond statistical power, the results have several limitations. Receipt of subsidies was not directly measured, and the sample size was not large enough to examine single years of data. The latter point prevents any analysis before the Great Recession or to better investigate volatility in ESI/PHI coverage. There is also a chance that individuals misreported IPI as PHI or vice versa, given the strong advocacy and state-branding that occurred with health reform.

Like the Massachusetts health reform, the Affordable Care Act implemented premium tax credits for individuals between 138% FPL and 400% FPL in 2014 and cost-sharing subsidies for individuals between 138% and 250% FPL. The results presented here indicate that tax credits may be an effective means of increasing IPI and bode well for the Affordable Care Act. An RD design could be applied nationally to consider both the effects of the tax credits and cost-sharing subsidies in the Act.

Tables

Table 2.1 Weighted summary statistics, 230%–370% FPL

Characteristic	1999–2006 N=2,578		2007–2009 N=804	
	Mean	SE	Mean	SE
Any HI	0.82	(0.01)	0.93	(0.010)
ESI	0.74	(0.01)	0.74	(0.017)
IPI	0.04	(0.00)	0.06	(0.009)
PHI	0.04	(0.00)	0.13	(0.013)
Age	38.53	(0.26)	39.45	(0.498)
Female	0.53	(0.01)	0.52	(0.019)
Race				
White	0.86	(0.01)	0.83	(0.014)
Black	0.08	(0.01)	0.09	(0.011)
Other/multiple	0.06	(0.01)	0.08	(0.009)
Hispanic	0.10	(0.01)	0.08	(0.009)
Marital Status				
Married	0.49	(0.01)	0.49	(0.019)
Previously married	0.14	(0.01)	0.14	(0.013)
Never married	0.37	(0.01)	0.37	(0.019)
Household Size	3.07	(0.04)	3.06	(0.071)

Note: Summary statistics before and after health reform.

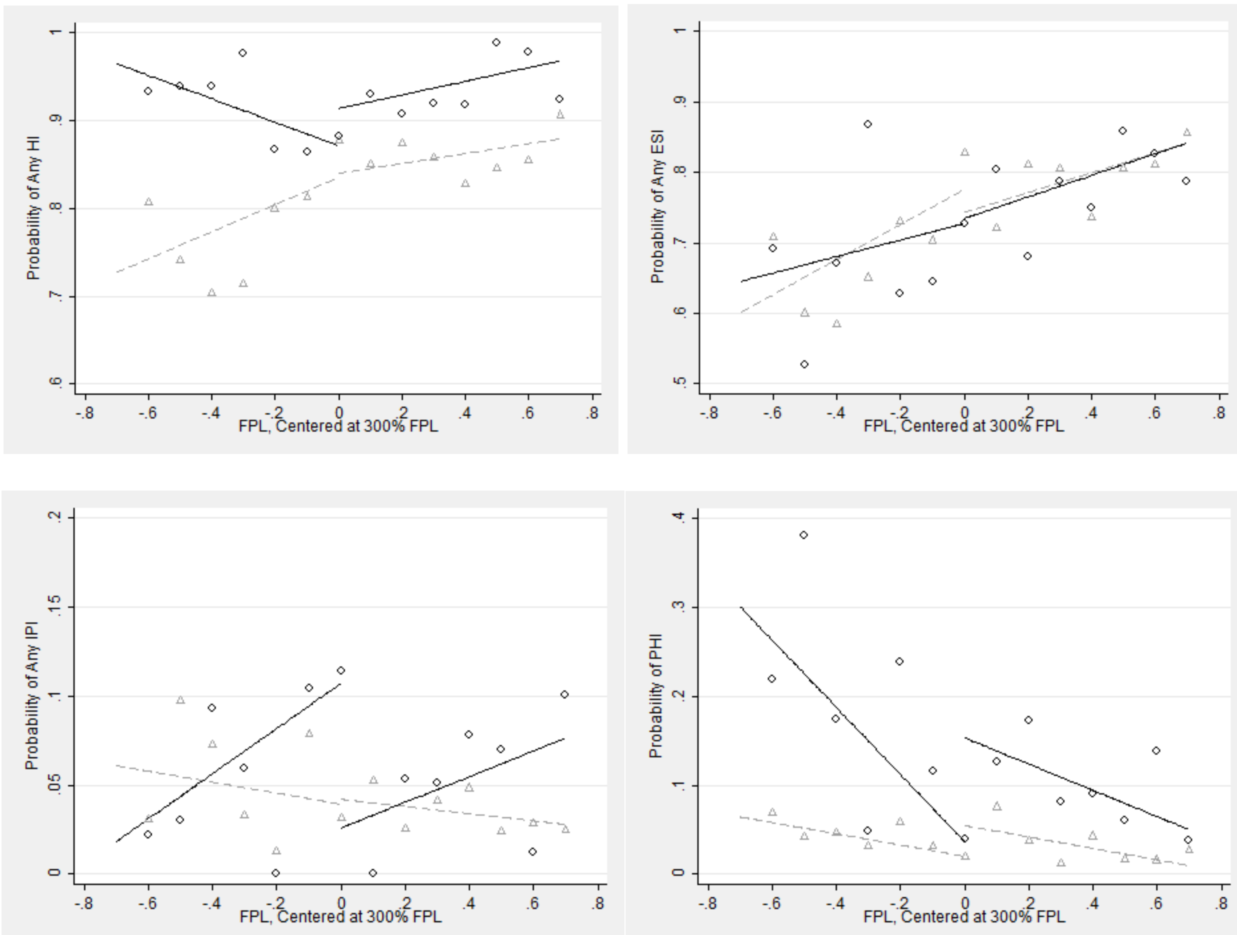
Table 2.2. Regressions discontinuity estimates of health insurance uptake at 300% FPL

	Post-Reform: 2007–2009			Pre-Reform: 1999–2006			Difference-in-Differences	
	Non-parametric	**Linear**	**Cubic**	**Non-parametric**	**Linear**	**Cubic**	**Linear**	**Cubic**
Any HI	−0.048	−0.041	−0.128	0.008	−0.021	0.010	−0.068	−0.049
	(0.051)	(0.055)	(0.104)	(0.032)	(0.034)	(0.063)	(0.075)	(0.143)
Any IPI	0.084**	0.070	0.194**	−0.002	−0.003	0.050	0.082	0.161
	(0.039)	(0.043)	(0.092)	(0.017)	(0.020)	(0.033)	(0.057)	(0.120)
Any ESI	−0.013	0.044	−0.132	0.043	0.014	0.032	−0.031	−0.054
	(0.072)	(0.082)	(0.161)	(0.036)	(0.039)	(0.075)	(0.101)	(0.205)
Any PHI	−0.118**	−0.155***	−0.186*	−0.038**	−0.032*	−0.070	−0.119*	−0.155
	(0.049)	(0.056)	(0.098)	(0.016)	(0.019)	(0.043)	(0.063)	(0.113)

Notes: * $p < 0.10$ ** $p < 0.05$ *** $p < 0.01$. The sample size is 804 in the post period and 2,578 in the pre-period.

Figures

Figure 2.1. RD estimates at the 300% FPL cutoff



Notes: Lines are the predicted trends and the symbols are the unconditional sample proportions, aggregated into 10% bins. Black lines/symbols are for the post-reform period, and gray lines/symbols are for the pre-reform period.

CHAPTER 3: INCENTIVE(LESS)? THE EFFECTIVENESS OF TAX CREDITS AND COST-SHARING SUBSIDIES IN THE AFFORDABLE CARE ACT

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 and 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 and Simon 2016) and individually purchased insurance (IPI) among the self-employed (e.g., Heim and 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 and 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 3.1 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, and 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 and 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 and 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 and 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 and Banthin 2001, Chernew, Frick and McLaughlin 1997; Gruber and 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 and 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 and 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 and 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 and 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 and 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.

[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)

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 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 and 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 and 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]

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[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.

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

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 γ_s are state fixed effects. ε_i is assumed to be an independently and identically distributed error term. 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. β_1 represents the treatment effect at the discontinuity. The nonparametric model estimates the equivalent of β_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 1 and Appendix Figures 4 to 7. 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 and 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 and 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 and 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

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

in absolute magnitude and smaller test statistics than the actual cutoff (Imbens and 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 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 3.2 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 3.1 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 3.2 through 3.5 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 3.2, 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 3.3, 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 3.3, 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 3.4 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 3.5 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 3.3. 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 3.3, 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 1 and Appendix Figures 4–6. 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 3.3 by three key characteristics: relationship status, self-reported health status, and age group. The estimates are presented in Table 3.4 for expansion states and Table 5 for non-expansion states.

Starting with expansion states in Table 3.4, 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 3.2, 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 3.4 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 3.4 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 3.5, 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 3.6 and 3.7 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 3.6 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 3.7 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 3.4 and 3.5 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 8). 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 Figures 9[5] and 10). 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

[5]Appendix Figure 8 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.

to the bandwidth, it may cast doubt on the design. The results are appropriately sensitive to bandwidth selection (see Appendix Figure 11). Coefficient magnitude is at least constant or decreasing in absolute magnitude as the bandwidth increase.

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]

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 3.5, 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

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

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.

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 and 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 and Simon (2016), although they acknowledge that the ACA APTCs and CSRs could produce higher elasticities. Using a sub-sample of their data, Moriya and 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 and 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 and 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 and 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.

[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.

There are still no visible or statistical effects (see Appendix Figure 8). I caution that the null results at 250% FPL attributable to the CSRs do not suggest overall ineffectiveness. Referring back to Figure 3.4, 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 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.

Tables

Table 3.1. 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 3.2. 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						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	
	Mean	SE	Mean	SE	Mean	SE	Mean	SE	Mean	SE	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 3.3. 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 3.4. RD estimates for expansion states at 138% FPL by key demographics, 2014

138% FPL	N	Any HI Non-parametric	Any HI Linear	IPI Non-parametric	IPI Linear	ESI Non-parametric	ESI Linear	PHI Non-parametric	PHI 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 3.5. RD Estimates for non-expansion states at 100% FPL by key demographics, 2014

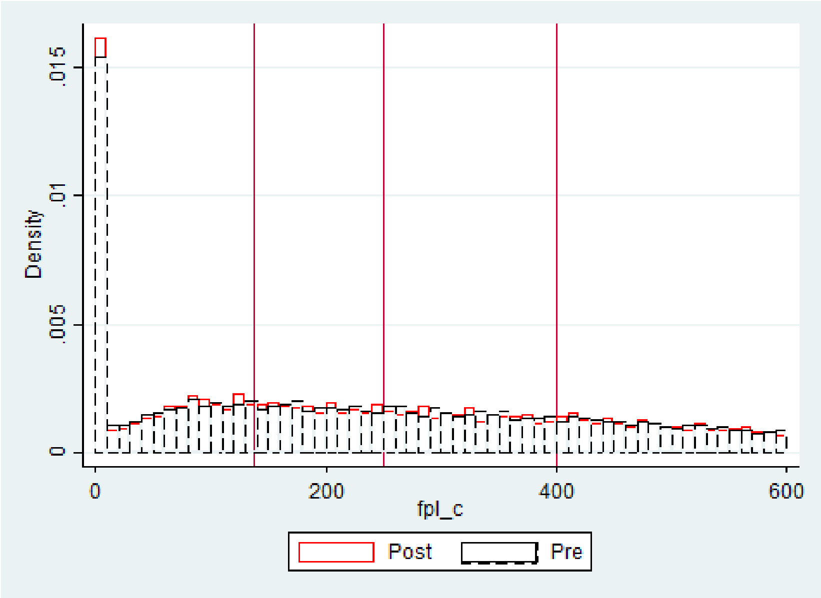
		Any HI		IPI		ESI		PHI	
100% FPL	**N**	**Non-parametric**	**Linear**	**Non-parametric**	**Linear**	**Non-parametric**	**Linear**	**Non-parametric**	**Linear**
Marital Status									
Currently Married	3,134	0.062	0.040	0.021	0.031	0.024	-0.006	0.019	0.016
		(0.041)	(0.044)	(0.029)	(0.035)	(0.040)	(0.048)	(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.

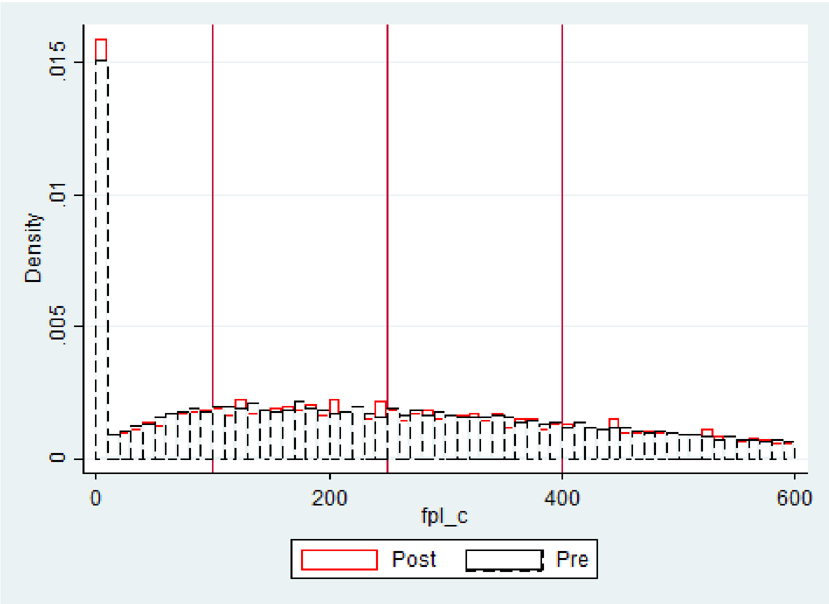
Figures

Figure 3.1. FPL density estimates, post- and pre-2014

Panel A. Expansion States



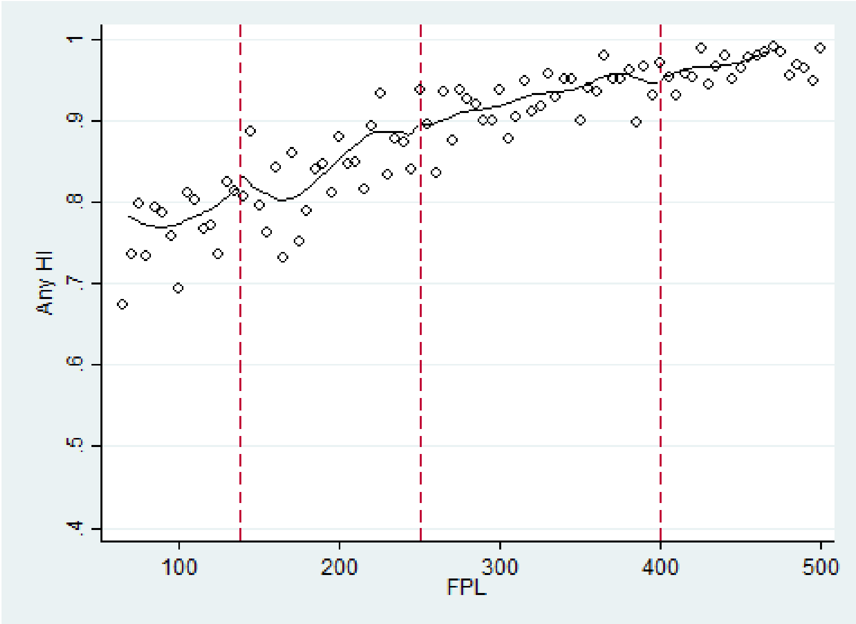
Panel B. Non-Expansion States



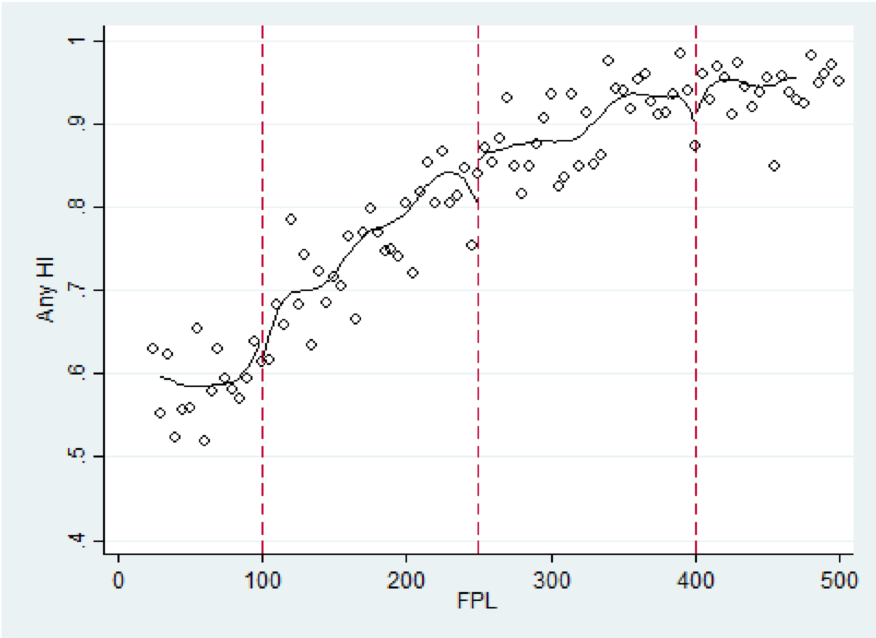
Notes: Data come from the IPUMS-CPS. Bars represent a 10% FPL bin. Vertical lines represent the 138%/100%, 250% and 400% FPL cutoffs.

Figure 3.2. Any HI coverage by 5% FPL bins in 2014

Panel A. Expansion States



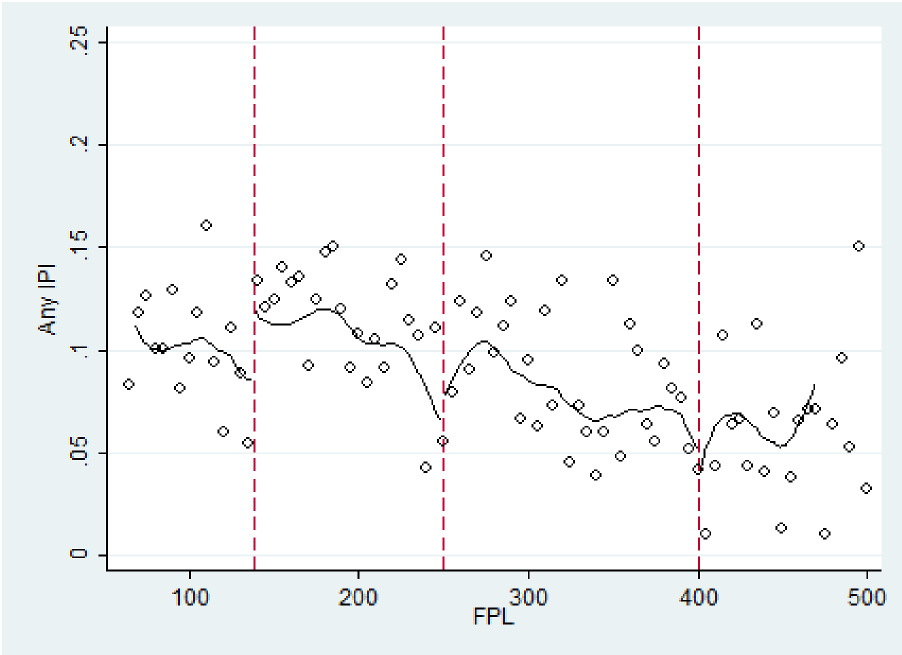
Panel B. Non-Expansion States



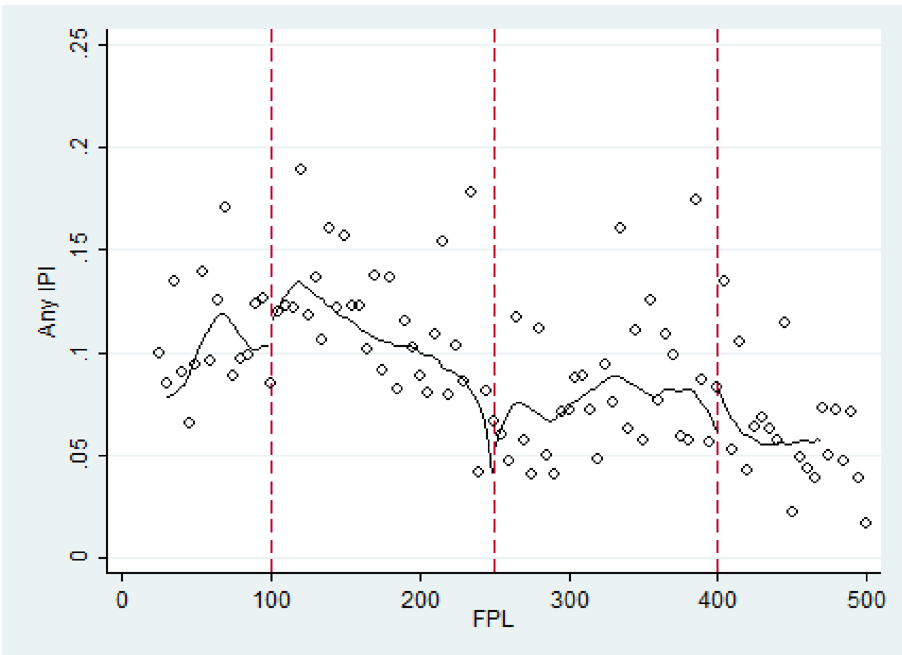
Notes: Data come from the IPUMS-CPS. Symbols represent the proportion covered in a 5% FPL bin. Vertical dashed lines represent the 138%/100%, 250% and 400% FPL cutoffs. Local linear trends are imposed between each cutoff. Estimates are weighted using ASEC supplement probability weights.

Figure 3.3. IPI coverage by 5% FPL bins in 2014

Panel A. Expansion States



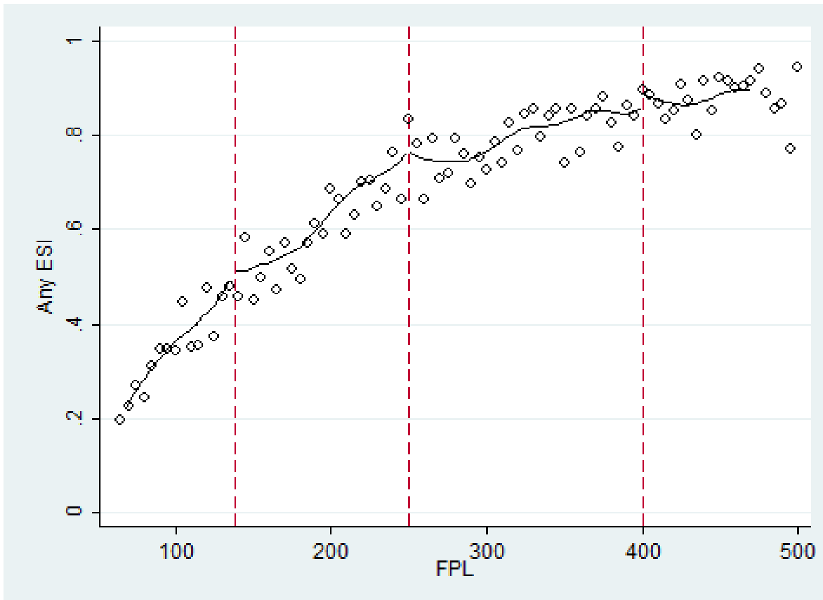
Panel B. Non-Expansion States



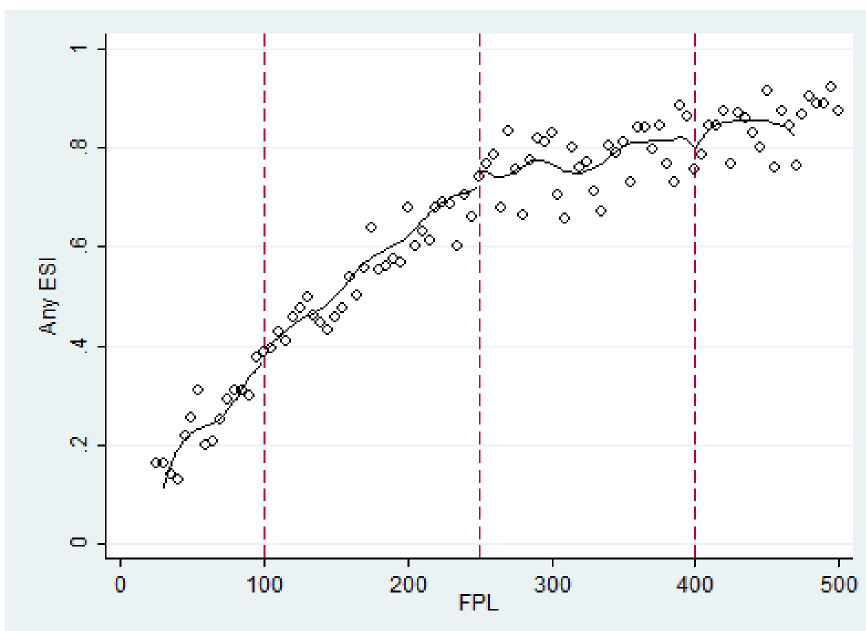
Notes: Data come from the IPUMS-CPS. Symbols represent the proportion covered in a 5% FPL bin. Vertical dashed lines represent the 138%/100%, 250% and 400% FPL cutoffs. Local linear trends are imposed between each cutoff. Estimates are weighted using ASEC supplement probability weights.

Figure 3.4. ESI coverage by 5% FPL bins in 2014

Panel A. Expansion States



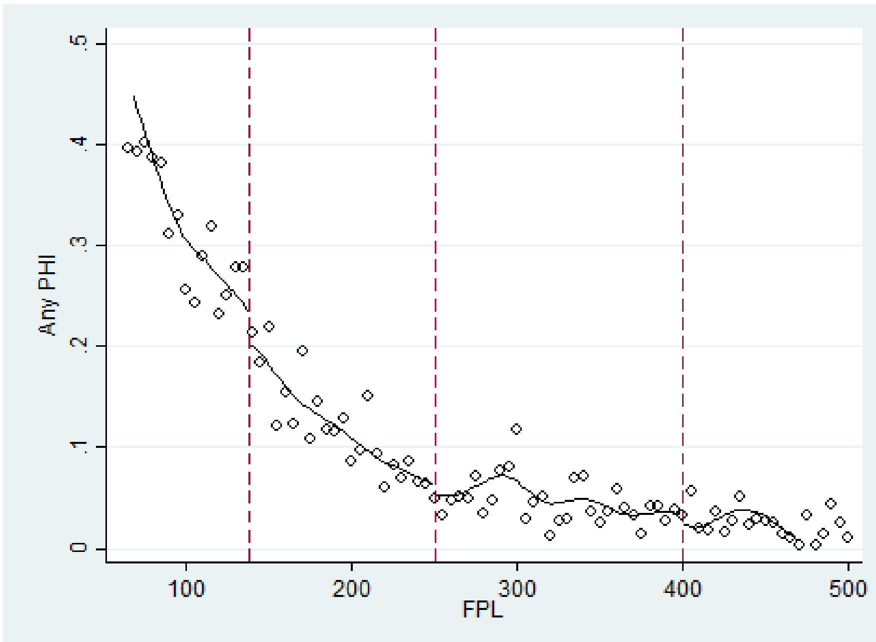
Panel B. Non-Expansion States



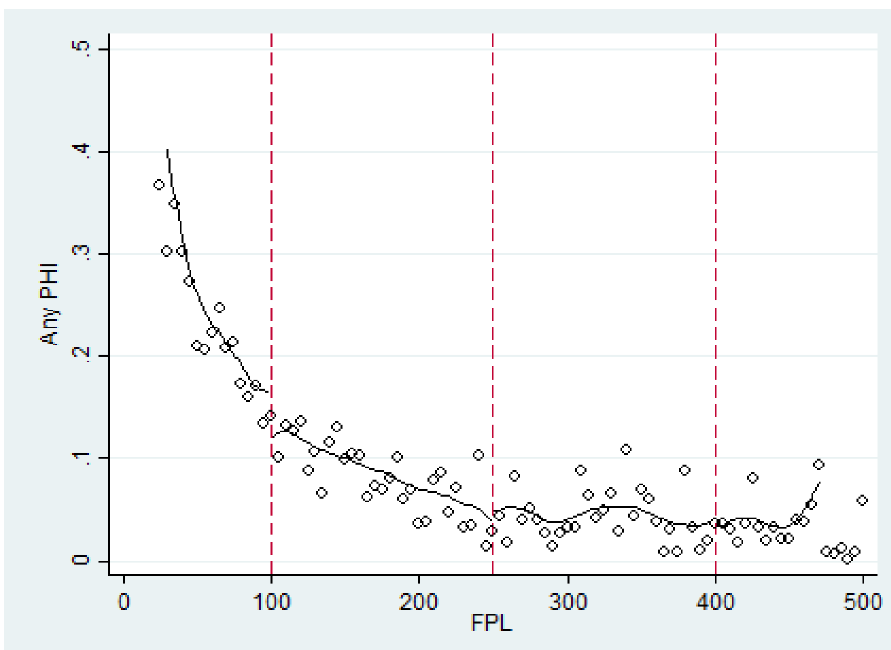
Notes: Data come from the IPUMS-CPS. Symbols represent the proportion covered in a 5% FPL bin. Vertical dashed lines represent the 138%/100%, 250% and 400% FPL cutoffs. Local linear trends are imposed between each cutoff. Estimates are weighted using ASEC supplement probability weights.

Figure 3.5. PHI coverage by 5% FPL bins in 2014

Panel A. Expansion States



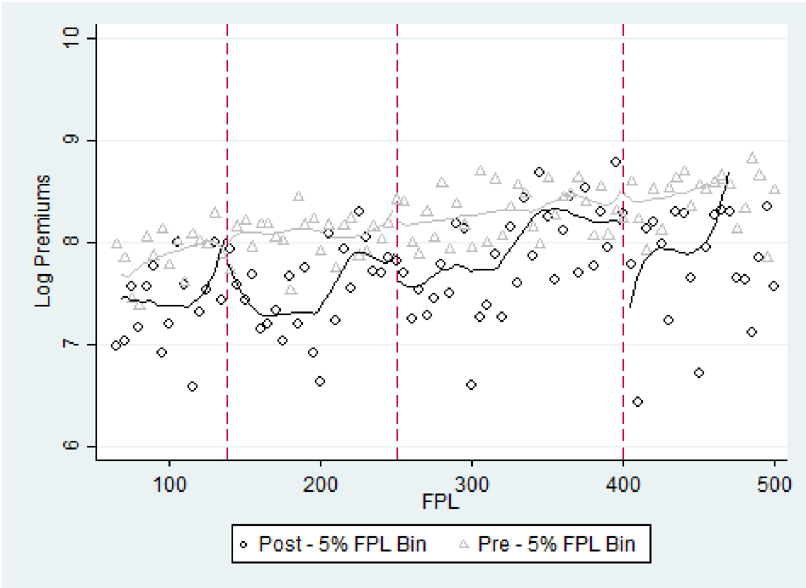
Panel B. Non-Expansion States



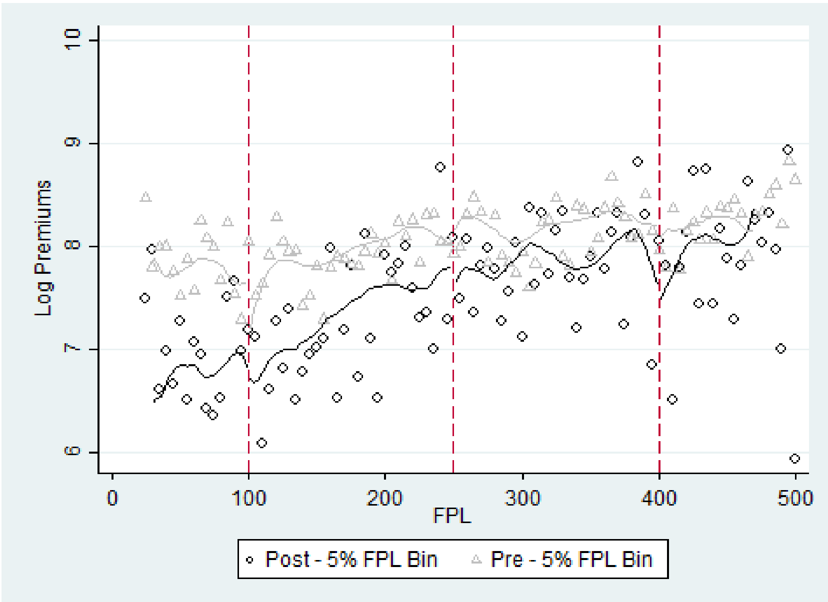
Notes: Data come from the IPUMS-CPS. Symbols represent the proportion covered in a 5% FPL bin. Vertical dashed lines represent the 138%/100%, 250% and 400% FPL cutoffs. Local linear trends are imposed between each cutoff. Estimates are weighted using ASEC supplement probability weights.

Figure 3.6. Log non-zero HI premiums for IPI-covered individuals in 2014

Panel A. Expansion States



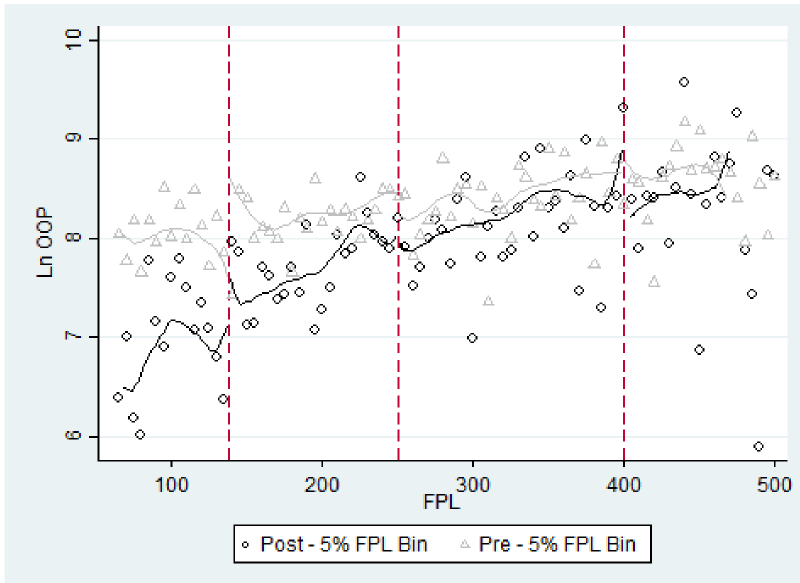
Panel B. Non-expansion States



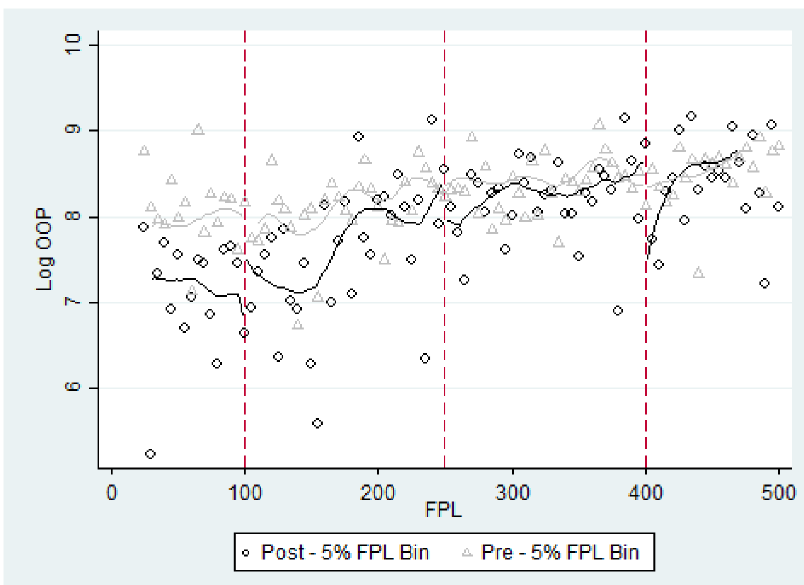
Notes: Data come from the IPUMS-CPS. Symbols represent the proportion covered in a 5% FPL bin. Vertical dashed lines represent the 138%/100%, 250%, and 400% FPL cutoffs. Local linear trends are imposed between each cutoff. Estimates are weighted using ASEC supplement probability weights.

Figure 3.7. Log OOP expenditures for IPI-covered individuals in 2014

Panel A. Expansion States



Panel B. 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.

CHAPTER 4: DISENTANGLING THE EFFECTS OF EMPLOYER-SPONSORED HEALTH INSURANCE ON THE U.S. EARNINGS DISTRIBUTION

Introduction

Unlike nearly every other developed country, health insurance in the United States remains ostensibly tied to employment for the nonelderly population. Almost 60% of the U.S. population remains covered by employer-sponsored health insurance (ESI) (DeNavas et al., 2009). Reliance on ESI is thought to have several consequences in the labor market. Compensating earnings differentials suggest increases in ESI costs should put downward pressure on earnings. Empirical evidence for an earnings penalty, however, is mixed, with many studies finding positive or insignificant effects. This is especially puzzling given the large increase in premiums over the last three decades.

Exploiting the large increases in ESI premiums between 1995 and 2007 using a difference-in-differences framework, this study shows how earnings growth changes for ESI policy holders (PH) both on average and across the earnings distribution. Drawing on methodological approaches from the earnings equality literature, I focus on changes between two years, 1995 and 2007, using data from the Current Population Survey (CPS). Studies of earnings inequality often focus on the changes in the earnings distribution across discrete points in time. 1995 is the first year the CPS collects detailed health insurance information, and 2007 is the last calendar year before the Great Recession. Also, this period largely matches the existing estimates of the ESI-earnings penalty in the literature.

This study makes three contributions in understanding the ESI-earnings penalty. First, I provide evidence that both sample selection and selection bias have contributed to the mixed findings in the literature. I comprehensively assess changes in demographics and earnings across full-time, full-year workers (FTFY), part-time or part-year workers (PTPY), and the combined sample of all workers (referred to as the FULL sample). Existing studies vary significantly in whether PTPY workers are included. Based on descriptive analysis, I find that the preferred model includes FTFY workers and compares ESI PHs to ESI dependents (referred to as the FTFY-ESI sample). Ordinary least squares (OLS) estimates from this preferred model indicate that increases in ESI costs over time are associated with a modest earnings penalty of 3%. I show that the FULL sample overstates the earnings penalty from increasing ESI premiums, yielding an estimate of nearly 10% that is consistent with almost full cost-shifting onto employees, similar to Baicker and Chandra (2006). I also examine the role of selection into ESI by using inverse propensity and entropy balancing weighting and find that weighting produces estimates similar to the FTFY worker sample.

Second, I use quantile regression to examine heterogeneous effects of increasing ESI costs on earnings, moving beyond existing estimates of the average effects. For the FTFY-ESI sample, quantile models show that the earnings penalty is larger, approximately 5%, but applies only to earners below the 75th earnings percentile. When weighting methods are used, the penalty for the lowest quarter of the distribution dissipates, suggesting the penalty is concentrated in the middle half of the earnings distribution.

Third, I estimate models that use a continuous measure of premiums, separated into employer and employee contributions. It has been assumed that the primary mechanism by which earnings are reduced is through employer contributions. Recent studies (e.g., Lubotsky and Olson, 2015) using firm-level data have suggested that the mechanism is instead increases in employee contributions. My results suggest that increases in employer contributions do have a small, negative effect that applies to all earners, but that increases in employee contributions yield net positive effects on earnings growth for the upper 50% of earners. The offset for higher earners provides unifying evidence that is consistent with cost-shifting from employers to employees due to compensating wage differentials, but also shows that higher earners may benefit from increasing employee contributions relative to lower earners due to higher marginal tax rates.

Background

In the United States, nearly 60% of the population receives health insurance through an employer-sponsored plan (DeNavas et al., 2009). Generally, the employer and employee both contribute to the insurance premiums—there is a cost to the employer in the form of non-pecuniary compensation, and there is a direct cost to the employee and a potential indirect cost through reduced earnings. From 2000 through 2010, the annual cost of an average ESI plan increased by more than 125%, and the contributions from employees to these plans increased by more than 150%, while nominal earnings increased by only 35% (Kaiser Family Foundation, 2013).

The trade-off between ESI premiums and earnings is theoretically grounded in compensating earnings differentials (Rosen, 1986). In a perfectly competitive labor market with no institutional constraints, employees who value health insurance would be willing to accept lower earnings in exchange for health insurance. Gruber (2000) notes two issues with health insurance that cause a departure from the traditional model: firms cannot necessarily set employee-specific health insurance packages, and firms face heterogeneous pricing for purchasing health insurance. These constraints may hamper job mobility among workers, who may prefer to retain current employment for a given firm's ESI benefits than switch to a more productive, higher paying job with a different set of ESI benefits. In a seminal paper, Summers

(1989) argues that rising ESI costs will be fully shifted onto employees through earnings if employees value health insurance. In the context of the United States, this effect is reinforced through the non-taxability of ESI benefits.

While microeconomic theory suggests that the large increase in the cost of premiums should lead to a reduction in earnings, the empirical literature has not robustly confirmed this prediction (Currie & Madrian, 1999). Focusing on mandated ESI benefits at the state level, the earliest studies find that the costs of mandated benefits are shifted onto groups targeted by the mandate (e.g., Gruber & Krueger, 1991; Gruber, 1994). Most pre-2000 studies, however, produce moderate-to-small effect sizes with weak identification strategies (Currie & Madrian, 1999).

The common empirical model of the ESI-earnings gap starts with the Mincer equation:

$$\ln(Earnings_{it}) = \beta_0 + \beta_1 ESI_{it} + \beta' X_i + \varepsilon_{it} \qquad (1)$$

where $\ln(Earnings_{it})$ are log pre-tax, pre-transfer annual earnings for person *i* at year *t*; ESI_{it} is a measure of ESI; and X_i is a vector of demographic, human capital, and geographic characteristics. β_1 is the reduced form wage return to having ESI. The identification of β_1 has been proposed in the literature through three estimation methods: difference-in-differences (DD) (relying on a control group), fixed effects (relying on time invariant unobserved heterogeneity), and instrumental variables (relying on exogeneity assumptions) (Currie & Madrian. 1999). None of these approaches directly address selection bias.

In addition to identification, the measure of ESI is not consistent across studies, including both binary measures of ESI and continuous premium contributions. Across both measures, a persistent issue is the accounting of the referent group. For the binary measure of ESI, permutations of the referent group could be ESI dependents, those without ESI, those with non-group insurance, or those uninsured. The operationalization of ESI itself, therefore, may be endogenous or could influence the estimates.

Recent studies have attempted to address this empirical deficiency. For example, Baicker & Chandra (2006) find substantial negative effects of employer premium contributions on earnings using instrumental variables (IV). They are limited by the use of imputed premiums and weak instruments. Qin and Chernew (2014) focus on public workers in the CPS, exploiting how public unions can affect the government's ability to adjust wages and benefits. Using inverse propensity weight matching to adjust for differences and selection among workers with and without ESI, they find moderate but imprecise evidence of a penalty.

Lluis and Abraham (2013) is the only study that explicitly models for selection into ESI, using panel data from the Medical Expenditure Panel Survey (MEPS). Using a generalized methods of moments approach, they find weak evidence of an ESI-wage gap, concentrated largely among employees who have ESI and no other fringe benefits (e.g., pension or retirement plan). They find a positive earnings return to ESI when other fringe benefits are accounted for, suggesting the effects of increasing ESI costs may be spread across other fringe benefits. Employees may be willing to accept lower non-ESI benefits to account for increasing ESI costs.

Other studies, such as DeVaro and Maxwell (2014) and Lubotsky and Olsen (2015), point to specific empirical concerns using narrower administrative data sets. DeVaro and Maxwell (2014) note that firm size is a critical determinant of insurance pricing and that the existing literature does not adequately control for firm size. They find evidence of a negative gap when firm size is interacted with ESI status in sample of California firms. Beyond firm size, they develop a model that separates multi-establishment firms from single establishment firms. Whereas a given establishment can set earnings according to local labor market conditions, health insurance decisions are often made at the firm level. Firms may be constrained across establishments in passing along the premium costs. Still, they show an earnings penalty exists across both multi-establishment and single establishment firms. Lubotsky and Olsen (2015) use detailed administrative data for Illinois public schools to assess the relationship between employee premium contributions and earnings. They find no evidence that increases in total premium costs reduce earnings. Rather, increases in premiums are passed on in the form of employee premium contributions.

Lastly, a study by Cowan and Schwab (2016) using difference-in-differences-in-differences (DDD) suggests that given higher average medical costs, females face an earnings penalty through ESI coverage. This study uses a standard Heckman selection model as a robustness check and focuses on a narrow sample from the 1979 National Longitudinal Survey of Youth (NLSY).

Across this broad literature, all studies examine the average effects of ESI; no studies have focused on the distributional effects. Lubotsky and Olsen (2015) come close by stratifying their models across skill levels and find negative earnings effects concentrated among low- and middle-skilled workers, with no effects in the highest skill category. The focus on the average in this literature is in stark contrast to a separate but related literature that has emerged over the last decade focusing on measures of disposable income inequality. Disposable income is similar in nature to the Haig-Simons measure of economic income. The Haig-Simons measure includes consumption plus change in net worth. The following equation describes three broad components included in the measure of disposable income:

$$\begin{aligned} Disposable\ Income &= [1 - Taxrate(Total\ Income)] * Total\ Income \\ &\quad + Public\ Transfers + Nontaxable\ In - Kind\ Benefits \quad (2) \end{aligned}$$

Total income comprises earnings, which is wage and salary income, and unearned private income, including non-labor income sources and private transfers. Due to taxes, only a share of total income is included in disposable income. The tax rate, however, is endogenous to the income level. Disposable income also includes the value of public transfer payments and in-kind benefits to incorporate all financial resources available to an individual or family. The latter two benefits are not included in taxable earnings.

Rather than include health insurance on the right-hand side of the estimating equation, the disposable income measure includes the value of health insurance as a part of the dependent variable. Chung (2003) and Pierce (2001) first examined this with a measure of compensation inequality (fringe benefits, but no transfers), and, more recently, an emerging literature has added transfers and a valuation of public health insurance. Burkhauser, Larrimore, and Simon (2012, 2013) and Kaestner and Lubotsky (2016) show that including the value of public health insurance and transfers greatly reduces disposable income inequality. They also document a large gain in the upper income tail due to the tax benefits of ESI when accounting for post-tax income.

However, the disposable income measure aggregates individual incomes to family, household, or tax units thus incorporating information on workers partially attached to the labor force. Most of the findings in the ESI-earnings literature are based on a FTFY worker sample, and identification concerns become more muddled once aggregating to a higher unit of observation that includes part-time or part-year workers. This study provides a link between pre-tax earnings and post-tax, post transfer disposable earnings by highlighting the role of sample selection and selection bias before aggregation and points to potential mechanisms related to the U.S. tax system as a source of the earnings penalty associated with increasing premiums.

Referring back to Equation 2, ESI affects disposable income through three possible mechanisms: in-kind benefits, total earnings, and the tax rate. First, ESI provides a nontaxable, in-kind benefit that increases disposable income. Second, earnings could be affected directly through employee contributions to ESI and indirectly through employer contributions. This second mechanism is the focus of this study. Employment contracts are negotiated on pre-tax terms and contributions to ESI are determined annually. The employee portion directly reduces the salary or wage earnings, and if there is a gap between earnings and employer contributions, then total earnings could be reduced through lower salaries or wages if employer contributions rise. The latter, indirect mechanism is predominately studied.

Third, employers could offset increases in their portion by increasing employee contributions, alleviating the downward pressure on earnings growth. Thus, the earnings gap could be positively correlated with increasing employee contributions. This consideration is critical when investigating the final mechanism, the tax rate. The tax rate is endogenous to total earnings, and reductions in total earnings could lower the tax liability. At higher tax liabilities, reductions in earnings are potentially appealing and could increase post-tax disposable income, as suggested in Kaestner and Lubotsky (2016).

Data

The CPS Annual Social and Economic Supplement (ASEC) is the primary data set for this study, as it provides measures of both income and insurance status. I focus on the changes between two years, 1995 and 2007. Descriptive trends include all years, but model estimates include 1995 and 2007 only. The sample excludes veterans, self-employed individuals, work-disabled individuals, and individuals younger than age 16 and older than age 64. I also exclude workers who reported zero weeks of work or reported being a family worker.

A common approach in the earnings inequality and earnings-ESI literatures is to restrict the analytic samples to FTFY workers. Full-time work means working 35 or more hours per week and full-year workers work at least 40 weeks. FTFY workers are the primary sample, but I also examine two other samples: PTPY and FULL. All workers (FULL) include FTFY and PTPY workers. I examine each of the three samples to describe the sensitivity of the model estimates to the inclusion of PTPY workers. FTFY workers are more likely to have higher earnings and more likely to have ESI. Thus, the effect of increasing premiums may differ by FTFY and PTPY workers, and the amalgamation of FTFY and PTPY workers creates a fundamentally different earnings distribution than in the subsamples.

The dependent variable is the logarithm of pre-tax, pre-transfer individual earnings from all jobs. Earnings include salary and wage income and excludes all other sources of unearned income, such as rent or private transfers. All earnings are adjusted to 1999 dollars using the Consumer Price Index.

The primary independent variable is ESI, measured as either a binary indicator or a continuous measure of premium contributions. For the binary definition, I compare ESI primary policy holders (ESI PH) to one of two potential referent groups: ESI dependents and all else. The referent group for ESI PHs is not consistently defined in the literature. ESI dependents are covered by another household members' ESI plan. The "all else" group includes ESI dependents and individuals without ESI. Individuals without ESI may have individually-purchased insurance (IPI) or public health insurance, or be uninsured. All insurance categories are mutually exclusive with the following hierarchy: ESI PH, ESI dependent, and does not have ESI.

As an alternative to a binary measure, I impute premium contributions and employee premium contributions by replicating the method used in Burkhauser, Larrimore, and Simon

(2013). I start with state-level estimates from MEPS for employee and employer premium contributions. Although ASEC contains a self-reported measure of employer contributions, it does not historically capture the employee portion. MEPS is therefore commonly used as source for premiums for pre-2010 studies using ASEC. However, using only state-level estimates yields a relatively tight premium distribution with little overall variation. To flatten the distribution, I use firm size-specific estimates for single and family plans. Per Burkhauser, Larrimore, and Simon (2013), I also use detailed occupation estimates from the National Compensation Survey Employer Costs for Employee Compensation Index to scale premium costs by occupation type. All contributions are adjusted to 1999 dollars to match earnings, but are adjusted using the medical Consumer Price Index. Because the resulting premium distribution contains three mass points, I also include a binary indicator for non-ESI PHs to capture a mass point at zero in the premium distribution and binary indicators for whether the plan type is a single or family plan.

Other included labor market characteristics are potential experience; education; job transitions; union status; and firm size, occupation, and industry fixed effects. Potential experience is calculated by subtracting from the individual’s age their years of education plus six (Autor et al., 2008). Individuals with negative potential experience or potential experience greater than 39 years are dropped. Job transitions are measured with an indicator for working for more than on employer during the year. Education is categorically assigned to six categories: less than high school, high school diploma/GED, some college, associate’s degree, bachelor’s degree, and graduate degree. Indicators for firm size (5 types), occupation (7 types), and industry (14 types) are also included in all models.[8]

[8] Firm size is broken out into less than 10 employees, 10 to 24 employees, 25 to 99 employees, 100 to 999 employees, and 1,000 or more employees. Occupation types include management; professional; service; sales; office/administrative; natural resource, construction and mining; and production, transportation and material moving. For 1995, the last two categories are combined into a single blue-collar occupation category. Industry types

Other individual characteristics of the sample include age and age squared, an ordinal measure of self-reported health status (excellent, very good, good, fair and poor), gender, marital status (married, previously married, and never married), household size, race (white, black, and other/multiple races), an indicator for Hispanic ethnicity, Census region (Northeast, Midwest, South, West), and an indicator for living in a metropolitan statistical area. State fixed effects are also included. All models are weighted using the ASEC supplemental weights, unless otherwise stated.

Methods

This study applies several empirical methods for assessing changes in the earnings *growth* associated with increasing ESI premiums between 1995 and 2007. The base approach uses DD and a standard OLS model, varying the analytic sample and referent group. I then directly address selection into ESI by incorporating inverse propensity weighting and entropy balancing methods. Finally, I use quantile regression to examine heterogeneous effects. The following sections describe the methods used for each approach.

Difference-in-differences approach

The DD approach is motivated by the earnings inequality literature that assesses the changes in returns to human capital and other characteristics at distinct points in time, capturing factors associated with earnings growth or decline. These studies focus on changes as time evolves and not specific policies or events. Thus, I estimate the changes in earnings across time for ESI PHs compared to non-ESI PHs:

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are agriculture, forestry, fishing and hunting, and mining; construction; manufacturing; wholesale trade; retailed trade; transportation and warehousing, and utilities; information; finance and insurance, and real estate and rental and leasing; educational services, and health and social services; arts, entertainment, and recreation, and accommodation and food services; other services, except publication administration; public administration; and active military duty.

$$\ln(Earnings_{it}) = \beta_0 + \beta_1 ESI\ PH_{it} + \beta_2 2007_{it} + \beta_3 ESI\ PH_{it} * 2007_{it} + \beta' X_i + \varepsilon_{it} \quad (3)$$

Equation 3 is a standard DD, relying on changes across time between the ESI group and the referent group to identify the effect of ESI on earnings. β_1 measures the gap in earnings between ESI PHs and the referent group in 1995 (the "'pre"' period), and β_2 captures secular changes in earnings for the referent group in 2007 (or the "'post"' period). β_3 is the coefficient of interest, the change in the earnings growth over time. Given that ESI premiums have increased across time, compensating differentials imply that the coefficient β_3 should be negative. As premiums increase, there should be an earnings penalty if employers are able to shift these costs on to employees. Also included in the model is X_i, a vector of demographic and work characteristics described above. ε_{it} is assumed to be a normally distributed random error term.

As noted earlier, a challenge in identifying β_3 (or β_1) is that $ESI\ PH_{it}$ is likely correlated with ε_{it}, either through omitted variable bias, endogeneity, or selection. Although not a full solution to the identification issue, my approach attempts to address all three concerns. A key assumption in my approach is that ESI premium costs have increased for all potential employees over time, providing exogenous variation for a DD design. I rely on an extensive set of controls to minimize omitted variables bias. To address selection into ESI, I use inverse propensity weighting and entropy balancing methods. In combination, these may approximate conditional independence of $ESI\ PH_{it}$ from the error term.

For DD, the specific identifying assumption is that changes in earnings across time are not correlated with unobserved differences between ESI PHs and non-ESI PHs. In this context, the underlying counterfactual assesses how earnings change in the absence of increasing ESI costs. The validity of this assumption hinges on the definition of the referent group and analytic sample. Therefore, a primary purpose of this analysis is to document the sensitivity of estimates of ESI-earnings penalty to the choice of the referent group and analytic sample. Given evidence in the literature, the FTFY sample is the likeliest sample to experience a wage penalty, but the choice of the referent group may affect the size of the penalty or even produce a wage gain. I argue that using ESI dependents as a referent group is the best strategy and that the FTFY-ESI sample that includes only ESI PHs and ESI dependents is preferred. FTFY workers that are ESI dependents are much more likely to be *offered* ESI but do not take it.[9] They also implicitly value ESI by maintaining coverage. Thus, a plausible identifying assumption is that changes in earnings for ESI dependents across time are not correlated with increases ESI costs across time.

A potential threat to the validity of this assumption is that increasing ESI costs affect the negotiation of earnings for ESI dependents. One could argue that, depending on the negotiation process, declining coverage could affect earnings and induce unobserved correlation across time. However, firms are often constrained to offering homogenous ESI packages and may have more flexibility in earnings adjustments. In that sense, if dependents are able to negotiate earnings increases by declining coverage, it provides the necessary counterfactual about how earnings grow in the absence of rising ESI costs. If there are more mechanical pressures, such that increasing ESI costs mechanically limit increases in earnings at a firm level for all employees, using ESI dependents as a referent group conditions out the mechanical component and offers a more unbiased test of compensating differentials.

Broadening the referent group to include FTFY workers without ESI is a slightly less desirable comparison. Individuals with IPI, public health insurance, or who are uninsured may not have an offer of ESI or may not value health insurance. They present an interesting hypothetical counterfactual. Without an offer of ESI, ESI costs do not factor into the earnings

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[9] The ASEC does not indicate whether respondents had an offer of ESI, so this is unobserved in the model.

decision. Likewise, if individuals do not value ESI, increases in premiums should not induce an earnings penalty through compensating differentials. However, these unobserved preferences for ESI could be correlated with earnings and potentially bias estimates of the penalty. Within the context of the FTFY sample, this bias could be minimal inasmuch as preferences for jobs that offer ESI or preferences to be uninsured do not change as premiums increase (i.e., individuals value an ESI offer or uninsurance more or less as premiums increase) or that changes in these preferences are weakly correlated with changes in earnings.

While the identification for the FTFY samples are more straightforward, the identification for the PTPY and FULL samples are less clear. An ESI offer is much less prevalent among PTPY workers, and the reasons or preferences for part-time or part-year work are unobserved. Comparing ESI PHs to ESI dependents among PTPY workers is not as clean a comparison as with FTFY workers, since PTPY workers may not be primary earners and jobs that are PTPY and provide ESI may be fundamentally different in earnings structure than PTPY jobs that do not offer ESI. Moreover, structural changes in the labor market associated with increasing ESI costs may shift workers to PTPY status. Once the referent group is broadened to include non-ESI PTPY workers, the selection issues are even more muddled. The impact of increasing premiums on earnings for PTPY workers is therefore interesting only in how they affect the FULL sample. There is not a strong argument that the DD assumption is valid for the FULL sample, and it is included to highlight the influence of sample selection and selection bias on estimates of the earning penalty.

Inverse propensity and entropy balancing weights

Estimating the earnings penalty for different samples and referent groups provides indirect evidence on the influence of selection. To directly address selection into ESI, I use inverse propensity weighting and weights derived through entropy balancing (Hainmueller, 2012). Both methods control for selection on observable characteristics. Although I use a large set of covariates to model the selection process, the weighting methods require the strong assumption that unobserved characteristics are not correlated with the selection process. Given that I do not observe ESI offers or a comprehensive measure of the respondent's health that would be related to ESI status, the method may not fully address the selection problem. Weighting methods, however, will reduce bias associated with selection based on a large set of observable characteristics, improving model identification.

With longitudinal data, the standard approach balances the treatment and control groups at baseline and applies the weights to each subsequent time point. Since the sample is stable across time, balance should hold at each time period. This study does not have longitudinal data on respondents, which raises concerns about whether balance is maintained across time. With repeated cross-sections, respondents in the follow-up period could be different from the baseline sample due to changes in observable characteristics across time or due simply to random sampling of the population.

In this study, four groups should be assessed for balance: (1) ESI PHs in 1995, (2) non-ESI PHs in 1995, (3) ESI PHs in 2007, and (4) non-ESI-PHs in 2007. Starting with the inverse propensity weighting, I use a method recommended by Stuart et al. (2014) to ensure balance across these four groups by estimating three sets of inverse propensity weights and assessing covariate balance between the four groups for each set of inverse propensity weights. Covariate balance across the four groups is assessed using a 0.1 standardized difference threshold for each covariate. The inverse propensity weights from the model that best balances covariates are used instead of the ASEC sampling weights in the regression models.

The first set of inverse propensity weights are estimated with a logit where the outcome is 1 if the respondent is an ESI PH in 1995 and 0 if they fall into the other three groups. I include in the model the all covariates described earlier plus FTFY status. This process treats ESI PHs in 2007 as "untreated" for the inverse propensity weight estimation and includes both 1995 and 2007 respondents in the untreated group. The second set of inverse propensity weights are estimated with two independent logits. First, I estimate a logit for the 1995 data with the outcome defined as 1 for ESI PHs and 0 for non-ESI PHs. Then, I calculate inverse propensity weights for the 1995 data. The same procedure is then independently applied to 2007. This process ensures balance between the treated and untreated groups within each year, but does not necessarily balance observable characteristics across time.

The third set of inverse propensity weights is estimated with a multinomial logit with four categories. This last model is recommended by Stuart et al. (2014) since it addresses selection on observables across time and treatment condition. 1995 ESI PHs are the base category with the three remaining groups as the other outcomes. In a multinomial framework, each respondent has a predicted probability of being in each of the four groups. The inverse propensity weights are normalized to the base category, 1995 ESI PHs, such that:

$$Propensity\ Score_i = \frac{\Pr(Y_i = 1)}{\Pr(Y_i = k)}$$

where *Y* is the outcome for the group, *i* refers to the individual, *k* refers to the group the individual was in, and *k*=1 refers to ESI PHs in 1995. This approach produces a weight of one for 1995 ESI PHs. The inverse propensity weights for the other three groups are proportionally weighted to likelihood of being a 1995 ESI PH.

As an alternative to inverse propensity weighting, I also use weights derived through entropy balancing. Rather than rely on the functional form assumptions for estimating inverse propensity weights, entropy balancing uses a pre-processing algorithm to assign weights such that the control group is balanced across multiple moments (Hainmueller, 2012). The algorithm produces a similar weight for the control group as an inverse propensity weight, but always assigns a weight of one to the treatment group. Similar to the inverse propensity weights, I estimate three sets of entropy balancing weights to assess which set best balances the four groups. Since I cannot use a multinomial framework for the third set of weights, I independently run the entropy balancing algorithm three times to balance ESI PHs in 1995 again each of the three other groups. As with the inverse propensity weights, the entropy balancing weights from the preferred set are applied to the regression models.

Quantile methods

A final alteration to the DD framework assesses heterogeneous effects on the earnings distribution using quantile regression. Conditional quantile methods estimate a coefficient that can be interpreted as a rate of return at different points of the earnings distribution. Buchinsky (1994) first suggested using quantile regression to assess changes in the conditional earnings distribution associated with the returns to education. The conditional quantile model is specified as such:

$$Q_{\tau}[\ln(wage)_{it}\,|Z_{it}] = Z'\beta(\theta) + F_{\varepsilon_{it}}^{-1}(\tau)$$

where $Q_{\tau}[\ln(wage)_{it}\,|Z_{it}]$ is the conditional quantile function of log earnings given Z_{it}, the full set of characteristics, and τ is used to denote a specific quantile. τ=0.5 would represent the conditional median function. $\beta(\theta)$ is a vector of coefficients to be estimated. The additive error term used in the regression model is been replaced with $F_{\varepsilon_{it}}^{-1}(\tau)$, the inverse of the cumulative distribution function for the loss function evaluated at quantile τ. This specification allows the parameters in the model to differ at each τ and accounts for a heteroskedastic error term (Angrist and Pischke, 2009; Cameron and Trivedi, 2010).

Results

Descriptive analysis

This section comprehensively describes the characteristics and earnings trends of the different worker samples and highlights differences across the samples that may influence the size and direction of the ESI-earnings penalty. Summary statistics are presented in Table 4.1 across the FTFY, PTPY, and FULL samples for the years 1995 and 2007. Mean earnings increase by approximately $5,000 for the FTFY and FULL samples between 1995 and 2007 and by approximately $2,000 for the PTPY sample. ESI PH coverage declined by 1 to 2 percentage points across all samples between 1995 and 2007. In the PTPY sample, the uninsured rate increases by 3 percentage points between 1995 and 2007. Across the three samples, real employer contributions increase by approximately 50% and real employee contributions increase by 65%.

Tables 4.2 through 4.4 present detailed summary statistics for groups within the FTFY, PTPY, and FULL samples. Each table breaks the sample into ESI PHs, ESI dependents, and individuals without ESI, with the latter two groups representing the proposed control groups for the DD design. Starting with Table 4.2, FTFY-ESI dependents are more likely to be female, married, and work for a smaller firm compared with FTFY-ESI PHs. Otherwise, ESI dependents are naturally similar in observable characteristics to ESI PHs. Compared with ESI PHs, individuals without ESI are younger; less similar in marital status; more likely to be Hispanic; less educated; more likely to work in a construction, service, or blue collar industry; less likely to work in professional service industries; less likely to be in a management or professional occupation; more likely to work in a service or blue collar occupation; and more likely to work in smaller firms. Individuals without ESI are approximately 80% uninsured. Table 4.2 suggests that individuals without ESI are much different from ESI PHs and may not serve as a good control group.

Table 4.3 stratifies the PTPY sample across ESI PHs and control groups. Unlike the FTFY sample, there are demographic differences in the PTPY sample between ESI PHs and ESI dependents. ESI dependents are younger, less likely to be married, live in a larger household, have less potential experience, are less educated, work in different industries (most notably retail), more likely to work in sales or services occupations, and more likely work in a smaller firm. Substantial demographic differences exist between ESI PHs and non-ESI individuals as well. The evidence in Table 4.3 suggests that both ESI dependents and individuals without ESI are different than ESI PHs, limiting their potential as a control group in the PTPY sample.

For the FULL sample in Table 4.4, the viability of ESI dependents as a control group is further lessened. ESI dependents are similar to ESI PHs in racial composition, but differ substantially across other demographic, family, human capital, and job characteristics. ESI dependents are much less likely to be FTFY as well. Individuals without ESI remain quite different on observable characteristics.

The detailed summary tables suggest that ESI dependents are naturally similar to ESI PHs in the FTFY sample, while the PTPY and FULL samples do not yield an obvious choice for a control group. A priori, the preferred specification is the FTFY-ESI sample that includes only

ESI PHs and ESI dependents, matching the conceptual discussion in the methods section. To balance the groups more explicitly, Appendix Table 2 presents covariate balance checks for the three sets of inverse propensity weights. For each set, I present the standardized difference for ESI PHs in 1995 against the three remaining groups. Only the third set using a multinomial framework yields standardized differences across all covariates are less than 0.1 and is thus the preferred model.[10] Appendix Table 3 shows covariate balance checks for each of the three entropy balancing weight sets, and again, the third set that balances ESI PHs in 1995 to the remaining three groups independently provides the best match.

Trends analysis

The next set of results focuses on the equality of pre-trends assumption for DD and the viability of using two years of data, 1995 and 2007, instead of the full panel of years between 1995 and 2007. Figures 4.1 through 4.3 illustrate the broader trends between 1995 and 2012. Starting with health insurance coverage, Figure 4.1 describes trends across types of coverage. This figure shows that all groups have a relatively flat trend through 2007, after which coverage for ESI PHs and ESI dependents declines slightly and the percentage of uninsured increases. Nothing in the trends suggest that 1995 and 2007 are anomalous years, although more dynamic changes occur after 2007 due to the Great Recession.

Figure 4.2 presents the real earnings trends across sample definitions. I also present earnings estimates from the Bureau of Labor Statistics for FTFY workers as a reference point. My FTFY real earnings trend is similar to the national estimates for FTFY workers from the Bureau of Labor Statistics across the study period. For all study samples, earnings have a weakly positive trend across time with no obvious deviations across samples, though the slope of the

[10] This model also meets the overlap condition for propensity score methods.

trend is slightly more positive for PTPY workers. The relatively flat growth in real earnings is consistent with the literature for the 1995–2007 time period (Autor et al., 2008).[11] Across the samples, there are also large level differences in the trends, with FTFY workers' earnings remaining much higher PTPY workers. Both the FTFY-ESI and PTPY-ESI subsamples have slightly higher earnings their respective samples that include individuals without ESI.

Once the worker samples are broken out across health insurance status in Figure 4.3, several differences in earnings growth emerge. Panel A with FTFY workers shows small level differences across groups, but the level differences do not invalidate the DD design. ESI dependents have a slightly more positive trend than ESI PHs and individuals without ESI, but match well overall and likely satisfy the equality of pre-trends assumption. Figure 4.3A also shows the log premium trend across the study period. The premium trend is flat from 1995 to 1998 and then steadily increases through 2007. Comparing the premiums trend to the earnings trend, the ESI PH earnings trend flattens after 1999. The timing of the trend flattening in ESI PH earnings correlates with the large increase in premiums, whereas the trend for ESI dependents does not change as premiums begin to increase in 1999.

In Panel B for PTPY workers, the slopes of the trend lines match well across the three groups, but it appears that the choice of the referent group has less meaning for the PTPY sample given the almost identical earnings trend for ESI dependents and individuals without ESI. In Panel C for the FULL sample, ESI dependents have a slightly more positive trend than ESI PHs and individuals without ESI. This is in contrast to the FTFY and PTPY panels, where the slope

[11] The flat growth in earnings between 2007 and 2012 is also consistent with Bureau of Labor Statistics estimates (for example, see https://www.bls.gov/opub/ted/2014/ted_20140423.htm).

of the trend lines match well. The equality of pre-trends may not hold for ESI dependents in the FULL sample panel.

In summary of the trends analysis, there are level differences in earnings between ESI PHs and non-ESI PHs, but the trends match well in the FTFY and PTPY samples, strengthening the validity of a DD approach. There is less evidence that ESI dependents make a strong control group for the FULL sample. Finally, there is no “policy intervention” in this study for which there is a true pre-period, but Figures 4.2 and 4.3 highlight that 1995 and 2007 are not anomalous in the context of broader earnings trends.

Based on the descriptive and trends analysis, the FTFY-ESI sample is the preferred sample since it best satisfies the equality of pre-trends assumption and is naturally similar in demographic composition. The FTFY sample including individuals without ESI meets the pre-trends assumption, but is less similar in demographic composition and is not as ideal as the FTFY-ESI sample. While the PTPY samples appear to meet the pre-trends assumption, the sample composition is unquestionably different between ESI PHs and non-ESI PHs. The FULL sample may not address either the pre-trends or sample composition concerns.

Changes in the earnings distribution

Moving beyond average earnings, this next section graphically assesses differences in the earnings distribution between 1995 and 2007, highlighting where the different samples and groups experience changes in their respective distributions and where expected earning penalties might appear in the quantile models. Figure 4.4 describes the change in the log earnings distribution between 1995 and 2007 across the FTFY, PTPY, and FULL samples.[12] Starting with

[12] Appendix Figure 12 shows the actual log earnings at each percentile for each sample in 1995 and 2007, from which the differences are calculated.

the conventional FTFY samples, the earnings gains are roughly flat until the 75th percentile, where the earnings gains increase. The 90th percentile increased approximately twice as much as the 50th and 10th percentiles. These ratios are consistent with the earnings inequality literature, suggesting an increase at the top of the earnings distribution and compression in the middle (Autor et al., 2008[13]).

The shapes and patterns of the earnings gains in the PTPY and FULL samples are markedly different. The PTPY sample experiences larger growth in the lower 50% of the distribution. The gains are decreasing up to the 75th percentile, after which earnings growth increases again. Compared to the PTPY trend, the FULL sample has much higher growth in the bottom 10% with a steeper decline through the 50th percentile. While there are largely static earnings gains in the FTFY sample, the PTPY and FULL samples have much more dynamic growth patterns in the lower portion of the earnings distribution. This conclusion is itself noteworthy, as these two groups have not been well-studied in the earnings inequality literature.

Breaking out each sample across ESI status, Figure 4.5 provides several further insights.[14] In Panel A for the FTFY sample, the ESI dependent profile is similar to the ESI PH profile, but shifted higher across the majority of the distribution, whereas there are fewer differences between ESI PHs and individuals without ESI. Since ESI PHs experience lower growth than ESI dependents over most of the distribution, this indicates an earnings penalty for ESI PHs relative to ESI dependents. Conversely, the PTPY sample in Panel B shows few differences in earnings growth across the distribution between ESI PHs and individuals without ESI and there are only

[13] The FTFY plot close resembles Figure 4.11b from Autor et al. (2008) that describes changes in real earnings between 1990 and 2000. Appendix Table 4 also describes common earnings dispersion measures for every year between 1995 and 2012.

[14] Appendix Figure 13 shows the actual log earnings for each percentile for each sample in 1995 and 2007, from which the differences are calculated.

lower tail differences between ESI PHs and dependents. The difference in growth between ESI PHs and ESI dependents in the lower tail is positive and not indicative of an earnings penalty.

In the FULL sample in Panel C of Figure 4.5, ESI dependents have a concave earnings gain profile that is much higher than ESI PHs. The ESI PH profile is relatively flat except for the tails, and the profile for individuals without ESI decreases across most of the distribution. For the FULL sample, ESI PHs have lower earnings growth than ESI dependents across almost the entire distribution and the difference is larger towards the middle of the distribution. Comparing ESI PHs and individuals without ESI, the difference is decreasing until approximately the 70th percentile.

Figure 4.5 suggests that the sample definition influences the pattern and magnitude of the differences in earnings growth between ESI PHs and the referent groups. There is a relatively constant earnings penalty for ESI PHs in Panel A when comparing ESI PH earnings growth to ESI dependents, but among the PTPY workers in Panel B there is no evidence of an earnings penalty. With the FULL sample, ESI PHs experience lower growth than both ESI dependents and individuals without ESI and the magnitude of the difference changes across the distribution. Thus, it might be expected that the FULL sample yields more dynamic estimates from the quantile models with larger effect sizes than with the FTFY sample and that the PTPY sample yields little evidence of an earnings penalty.

Conditional average and quantile difference-in-differences results

The descriptive analyses in the previous section re-affirm that the FTFY-ESI sample including only ESI PHs and ESI dependents is preferred and that an earnings penalty may exist both on average and across the earnings distribution. This section now presents statistical estimates of the ESI-earnings penalty, starting with conventional OLS models for the average effect and building to quantile models for heterogeneous effects across the earnings distribution. Table 4.5 presents OLS estimates for the binary (Panel A) and continuous measures of ESI (Panel B). Although they are not the preferred samples, I include models with the PTPY and FULL samples to show the sensitivity of the estimates to the inclusion of PTPY workers.

Across all models in Panel A, being an ESI PH in 1995 is associated with statistically significant higher earnings. The positive effect is much more pronounced in the less preferred PTPY and FULL samples, upwards of a 75% increase in earnings, relative to the roughly 25% increase in the FTFY samples.[15] The binary DD coefficient is negative and statistically significant for the FTFY-ESI sample (~3% earnings penalty) and FULL sample (~10% earnings penalty). DD coefficients are not significant for the FTFY sample that includes individuals without ESI and for the PTPY sample. The last two columns of Panel A present the inverse propensity- and entropy balancing-weighted regressions to account for selection into ESI. The entropy balancing-weighted regression model estimates comparable effects to the preferred FTFY-ESI sample, while the inverse propensity-weighted model is somewhat similar in is magnitude but the DD coefficient is not statistically significant. Given the natural similarity between FTFY ESI PHs and dependents, it makes sense the weighting methods produce similar results to the second column of Panel A.

For the continuous measures of ESI premiums in Panel B, the positive coefficient pattern for 1995 and negative DD coefficient pattern among the FTFY and FULL sample transfers to log employer contributions; however, the log employer contribution DD effect is only marginally significant for the FTFY-ESI sample. For log employee contributions, there is a negative initial

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[15] Coefficients from the regressions have been transformed by calculating $e^{\beta} - 1$. For example, the coefficient for the FULL sample in the first row of Table 4.5 is 0.588: $e^{0.588} - 1 = 0.80$

effect in 1995 of approximately 3% in the FTFY and FULL samples and larger, positive, and insignificant effects across the models for the DD effect. The positive DD effect for employee contributions is marginally significant at the 10% level only in the model with the FULL sample. Although the positive effect is insignificant for the FTFY samples, it suggests that increases in employee contributions could mitigate the earnings penalty from employee contributions.

The OLS results indicate a moderate earnings penalty of 3% for the preferred FTFY-ESI sample, but I cannot precisely decompose that effect across employer and employee contributions. Including both FTFY and PTPY workers in the model yields a much larger earnings penalty of approximately 10%. When broken out by the type of contribution, the FULL sample yields a similar sign patter to the FTFY-ESI sample with higher magnitudes. While the standalone PTPY models do not yield an effect, their inclusion in the FULL sample appear to bias the estimate of the earnings penalty.

The bias in the FULL sample earnings penalty is driven largely by an implicit interaction with FTFY status. Statistically, one might expect that the coefficient estimates for the FULL sample in Table 4.5 be a weighted average of the coefficients from the FTFY and PTPY models. Consistent with other studies (e.g., Baicker & Chandra 2006), the FULL sample does not include a control for FTFY status. Appendix Table 5 compares the coefficients for the FTFY, PTPY, and the FULL sample with and without the FTFY interaction. Given the proportion that are ESI PHs are different across FTFY and that FTFY and PTPY earnings evolve differently, the final column of Appendix Table 5 shows how these influence the FULL sample and drive up the magnitude of the earnings penalty. FTFY ESI PHs have lower earnings (-0.345) and FTFY workers had lower earnings than PTPY workers in 2007 (-0.09). Once FTFY status is accounted for in the FULL sample, the earnings penalty is back down to -0.02 and is consistent with the separate FTFY and PTPY models.

Again, these OLS models estimate the average earnings penalty and do not inform whether the penalty is different for higher and lower earners. With the quantile models, the earnings penalty is assessed at different percentiles of the earnings distribution and the estimates are presented graphically. Figure 4.6 presents the DD quantile regression coefficients across the earnings distribution for the FTFY samples. Panel A for the FTFY sample that includes individuals without ESI shows a significant earnings penalty of approximately 3% between 25th and 75th percentile that is similar to the OLS estimates. The average effect was smaller and statistically significant, masking the larger penalty in the middle of the earnings distribution. At several percentiles in the tails of the distribution, the effect is positive but insignificant. For the preferred FTFY-ESI sample in Panel B, there is a relatively constant earnings penalty of approximately 5% up to the 75th percentile. The two panels look mostly similar except for the different behavior in the lower tail.

Quantile DD coefficients for the PTPY samples are presented in Figure 4.7. Both panels show few statistically significant estimates. In Panel A, the DD effects are negative except for the tails and are statistically significant between the 60th and 80th earnings percentiles. The sign of the effect for the PTPY-ESI sample switches from positive to negative at the 30th percentile, but the only significant effects are around the 80th percentile. The relative lack of effects in the PTPY samples, regardless of whether individuals without ESI are included, is consistent with the minimal differences in the unconditional distribution in Panel B of Figure 4.5. Since the PTPY sample is not preferred, the null effects by themselves are not interesting, but are important to understanding the effects in the FULL sample.

For the FULL sample in Figure 4.8, the quantile effects in Panel A are much more dynamic and indicate a more severe earnings penalty in the lower tails. The earnings penalty is decreasing (in absolute terms) and ranges from 22% to 0. The penalty is much larger in magnitude compared with the FTFY models. Referring back to Panel C of Figure 4.5, earnings gains for ESI dependents and individuals without ESI are much larger than ESI PHs in the lower portion of the earnings distribution, which is the root cause for the increased magnitude of the earnings penalty.

The increased magnitude of the earnings penalty in the FULL sample is influenced by the inclusion of PTPY workers, since these individuals have lower earnings and are less likely to have insurance (as noted in Table 4.3). When inverse propensity weighting is used (Figure 4.8B), the coefficients closely resemble the FTFY sample (Figure 4.6A) except for the bottom of the distribution where the DD coefficient turns positive. Likewise, the entropy balancing weights (Figure 4.8C) yield a graph similar to the FTFY-ESI (Figure 4.6C) sample with a relatively constant earnings gap of 5%. Comparing Figures 4.8B and 4.8C to Figure 4.8A shows that not controlling for selection into ESI may lead to an overstatement of the ESI-earnings penalty.

When the preferred FTFY models are disaggregated by employer and employee contributions in Figure 4.9, there are again more dynamic patterns across the earnings distribution. In Panel A for the FTFY sample, there is a fairly static negative DD effect of employer contributions on earnings. For employee contributions, the positive DD quantile effects trend upward across the earnings distribution and are mainly significant in the upper 50% of the earnings distribution. The pattern across employer and employee contributions is mostly similar in Panel B for the FTFY-ESI sample. Figure 4.9 suggests a uniform, negative effect on the earnings distribution over time associated with increasing employer premiums, consistent with cost-shifting. The positive effects of increases in employee contributions over time are suggestive of net-positive effect on earnings at the upper end of the distribution. Higher income individuals actually recover pre-tax and potentially post-tax earnings directly from cost-shifting through employee contributions due to the tax treatment of ESI costs.

Finally, Figure 4.10 presents quantile coefficients of the effects of the continuous ESI measures on earnings for the PTPY and FULL samples. The PTPY samples show little DD effects across time for both employer and employee contributions. As with the OLS models, the FULL sample produces effects that are larger in magnitude than the FTFY samples, consistent with the other evidence presented in this section.

Triple difference results using gender

Building on an approach to study the gender earnings gap, a final set of models estimates DDD, with the third difference taken across females and males (Schwab & Cowan, 2016). The DDD equation is defined as follows:

$$\ln(Earmings_{it}) = \beta_0 + \beta_1 ESI_{it} + \beta_2 POST_{it} + \beta_3 FEMALE_i + \beta_4 ESI_{it} * POST_{it} + \beta_5 ESI_{it} * FEMALE_i + \beta_6 POST_{it} * FEMALE_i + \beta_7 ESI_{it} * POST_{it} * FEMALE_i + \beta' X_i + \varepsilon_{it} \qquad (4)$$

Schwab and Cowan (2016) argue that the effects of ESI on earnings should not vary across females and males over time except for the higher expected medical costs for females. I further examine this assumption here since the descriptive results show that FTFY and ESI status vary slightly across gender. The ASEC sample also includes a wider age range of workers than included in the Schwab and Cowan (2016) cohort in the NLSY79, which captures only older adults.

Table 4.6 presents regression coefficients from the DDD OLS models for binary ESI coverage (Panel A) and continuous measures of ESI premiums (Panel B). For the FTFY and FTFY-ESI samples in Panel A, there is a noticeable difference in the sign pattern of the DD and DDD coefficients. The FTFY sample has a negligible coefficient estimate (−0.002) for the difference across time, a marginally significant difference for female ESI PHs of −0.023, and a triple difference of −0.025 that is insignificant. However, the sign pattern reverses for the preferred FTFY-ESI sample, with a negative, marginally significant difference across time of −0.032 and positive and insignificant differences across females and for the triple difference. The FTFY model including individuals without ESI is indicative of a female ESI earnings penalty, while the preferred FTFY-ESI does not, highlighting the sensitivity of the results to the sample definition.

The final three columns of Panel A in Table 4.6 focus on the PTPY and FULL samples. Unlike the FTFY and FTFY-ESI samples, there are not noticeable differences between the PTPY and PTPY-ESI sample in the sign pattern or significance. These two samples yield insignificant DD and DDD effects and the sign pattern indicates negative differences across time and for females, but a positive DDD effect. In the last column for the FULL sample, the difference across time of −0.091 signals a strong ESI earnings penalty, whereas the difference for females is positive and significant at 0.06, indicating that the female ESI PHs fair better than males. For the FULL sample, the negligible and insignificant DDD coefficient suggests little difference in the earnings gap associated with increasing ESI costs over time. In summary of Panel A, the gender earnings penalty appears in the FTFY, PTPY and PTPY-ESI samples, but the sign reverses in the FTFY-ESI and FULL samples, providing inconsistent evidence of a gender-specific penalty associated with ESI.

Appendix Table 6 replicates the actual specification in Cowan and Schwab (2016)—a DD comparing individuals with ESI to individuals without ESI and males to females for the years 1995 through 2007—to assess comprehensively the gender penalty in the CPS. I find that the FTFY sample including individuals without ESI produces similar results to the Cowan and Schwab analyses, but the FTFY-ESI sample produces robust positive effects for women, similar to the results in Table 4.6. Overall, my results suggest that the increases in ESI costs over time are not affecting earnings differentially by gender and cast doubt on whether increasing ESI costs can explain the gender earnings penalty.

Moving a step beyond Cowan and Schwab (2016), Panel B of Table 4.6 uses a continuous measure of ESI premiums to break out employer and employee contributions.[16] Three findings stand out. First, employer contributions introduce an earnings penalty, whereas employee contributions appear to help recover that penalty, matching the main results from Table 4.5 that do not incorporate a third difference. The first two rows of Panel B show that across models, the *Log Employer Prem * 2007* coefficient is negative, and the *Log Employee Prem * 2007* coefficient is positive. Here, the results are not significant except for the FULL model that is likely biased.

Second, the middle two rows of Panel B show the differential effect of premium contributions for females. The coefficients are positive for employer premiums and negative for employee premiums, and these are significant in the FTFY, FTFY-ESI, and FULL models. Women may experience an earnings benefit through employer contributions; however, this does not entirely offset the personal contribution.

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[16] Appendix Table 7 replicates a DD analyses for the full 1995–2007 period.

Third, the DDD effects for employer and employee premiums in the last two rows of Panel B are small and insignificant. Women and men saw similar reductions in earnings over time due to increases in employer premiums. Increases in ESI costs for employers negatively affect earnings across time for both men and women, but it appears that the employee contributions may only affect women's earnings. This is weakly suggestive that employee contributions may be a mechanism for the gender earnings gap discussed in Cowan and Schwab (2016).

Discussion

This study offers three contributions in understanding the earnings penalty due to increasing ESI premiums. First, I provide evidence that many of the null or positive effects found in the literature may in part be explained by the choice of the analytic sample and the reference group. In a preferred OLS model that uses only FTFY workers and compares ESI PHs to ESI dependents, I estimate a moderate earnings penalty of 3.1%. Including non-ESI workers reduces that effect size to a statistically insignificant 1.5%. Models focusing on the PTPY sample produce differing signs of the effect. When FTFY and PTPY workers are combined into a single sample, the estimated earnings penalty is 9.3%, three times the magnitude of the preferred model. The magnitude of my estimates for the FULL sample in Panel A of Table 4.5 aligns with the OLS and IV estimates from Baicker and Chandra (2006), who also use a FULL sample. They find that a 10% increase in premiums is associated with a 2.3% reduction in earnings and conclude that increasing costs are fully shifted onto employees. The real increase in premiums in my sample is 51% and, using the Baicker and Chandra elasticity, would predict a reduction in earnings of 11.7% that is similar my OLS estimate of 9.3%. I conclude that the FULL sample overestimates the magnitude of cost-shifting, and the preferred FTFY model produces less biased estimates consistent with a more moderate level of cost-shifting.

Second, this study also moves beyond average effects using quantile regression. Results from the preferred FTFY-ESI model estimates an earnings gap of approximately 5% below the 75th earnings percentile. The average real earnings for FTFY workers grew 15% between 1995 and 2007, meaning that the ESI earnings penalty was roughly one-third of overall earnings growth. This evidence is not consistent with full cost-shifting onto employees, but it is still economically meaningful. As with the OLS models, quantile models for the FULL sample estimate a much larger earnings penalty concentrated in the lower tail. This raises an important consideration for studies of disposable income that aggregate incomes within a tax unit or household and include PTPY workers.

The third and final contribution of this study is to reconcile the differing mechanisms by which earnings are reduced through increasing ESI premiums. In decomposing employer and employee contributions, the preferred FTFY-ESI quantile model provides evidence of compensating differentials through cost-shifting and evidence that higher earners may not face an earnings penalty. My preferred quantile model estimates a small, negative, and static effect of increasing employer contributions on earnings across the entire earnings distribution. This negative effect is consistent with compensating wage differentials and cost-shifting and may simply be mechanical in nature. If employer contributions are growing faster than wage and salary pools, then earnings growth may stagnate. Conversely, employee contributions had a positive effect in the upper half of the earnings distribution, indicating that increases in employee contributions may offset the downward pressure from employer contributions. The offset for higher earners provides unifying evidence that is both consistent with cost-shifting from employers to employees due to compensating wage differentials, but also shows that higher earners may benefit from increasing employee contributions relative to lower earners due to higher marginal tax rates. The countervailing pressures of increasing employer and employee premiums are also an alternative explanation for the mixed evidence in the literature.

Although the study period pre-dates the implementation of the Affordable Care Act (ACA), my results suggest that the ACA employer mandate could increase the earnings penalty among the lower and middle classes. Recent health reform efforts emphasize alternatives for those without access to ESI through public health insurance coverage via an expanded Medicaid program or through subsidized private coverage through state exchanges. Mandates require employers to offer ESI coverage and individuals to obtain insurance or pay penalties. Under the ACA, individuals receiving an offer of ESI are not eligible for public insurance expansions or subsidized exchange coverage, but are likely subject to the individual mandate penalty if they decline an offer of ESI. The individual mandate penalty may reinforce the decision to accept an ESI offer. Ultimately, the financial protection from health insurance could be beneficial for those who value or need it, but the earnings penalty may be reinforced or deepen existing inequalities in earnings. From a policy perspective, the earnings gap for lower- and middle-class points to unintended distributional implications of health reform, especially since the majority of the population remains covered by ESI.

Expansions of publicly financed coverage also emphasize the need for broader disposable income measures that may better reflect the welfare gains associated with having health insurance. The inconsistencies in the results for PTPY and non-ESI samples portend selection issues implicit in the disposable income literature, since these studies aggregate FTFY and PTPY individuals into tax or household units. A potential danger is that positive selection into public health insurance in the lower tail may overstate reductions in inequality associated with the inclusion of health insurance value in disposable income.

This study has several limitations. First, the increase in ESI costs might not be entirely exogenous, and other methods, such as IV, may better address the endogeneity problem. Identifying a valid instrument, however, remains a challenge. To date, a single study has used medical malpractice claims as an instrument (Baicker & Chandra 2006). I examined the use of medical malpractice claims as an instrument for the current study and confirmed that medical malpractice claims are weak instruments. Even if a suitable instrument is identified, IV does not address selection unless the exogenous variation from the instrument strongly correlates with that selection. Although I do not incorporate a more generalized model of selection or address selection into employment itself, I do demonstrate the role of both sample selection and selection bias. Inverse propensity weighting and entropy balancing address selection into ESI based on observable characteristics, but it cannot address unobservable characteristics, which remains a limitation.

Second, the earnings gap is measured using two years of data, and the year-to-year variations could produce different results. I argue that 1995 and 2007 are not anomalous years using a basic trends analysis of earnings and health insurance status. As a sensitivity analysis, I re-estimate the models in Table 4.5 using alternating boundary years: 1995 and 2006, 1996 and 2007, and 1996 and 2006. These alternate specifications produce similar results. Third, premiums are calculated using an imputation process. Fourth, I cannot distinguish between single and multi-establishment firms. DeVaro and Maxwell (2014) note that there is significant heterogeneity across these firm types.

Despite these limitations, this study makes progress in understanding the complex relationship between ESI and earnings. Future research should examine the post-tax implications of increasing premiums on earnings and the impacts on broader measures of disposable income.

Early studies of the ACA suggest that the labor market effects are minimal, but no studies have yet examined the earnings impacts. The distributional consequences of premium increases due to the ACA are an important, potentially unintended effect to the majority of the U.S. population covered by ESI and warrant careful monitoring.

Tables

Table 4.1. Summary statistics

	FTFY				PTPY				FULL			
	1995		**2007**		**1995**		**2007**		**1995**		**2007**	
	N=33,971		N=60,495		N=13,401		N=19,118		N=47,372		N=79,613	
	Mean	SE	Mean	SE	Mean	SE	Mean	SE	Mean	SE	Mean	SE
Annual Earnings	34,163	(201)	39,210	(212)	10,288	(201)	12,405	(171)	27,445	(165)	32,928	(173)
FTFY	-		-		-		-		0.72	(0.002)	0.77	(0.002)
Any Insurance	0.85	(0.002)	0.84	(0.002)	0.73	(0.004)	0.70	(0.004)	0.82	(0.002)	0.81	(0.002)
ESI PH	0.71	(0.003)	0.68	(0.002)	0.24	(0.004)	0.23	(0.004)	0.58	(0.003)	0.57	(0.002)
ESI Dependent	0.10	(0.002)	0.11	(0.001)	0.36	(0.005)	0.34	(0.004)	0.18	(0.002)	0.17	(0.002)
IPI	0.02	(0.001)	0.02	(0.001)	0.05	(0.002)	0.05	(0.002)	0.03	(0.001)	0.03	(0.001)
Public	0.01	(0.001)	0.02	(0.001)	0.08	(0.003)	0.08	(0.002)	0.03	(0.001)	0.04	(0.001)
Employer Contribution to ESI[a]	4,232	(13)	6,197	(16)	3,963	(37)	5,901	(49)	4,200	(12)	6,169	(15)
Employee Contribution to ESI[a]	1,455	(6)	2,389	(8)	1,314	(16)	2,168	(24)	1,439	(6)	2,369	(7)
Age	37.83	(0.064)	40.34	(0.057)	31.68	(0.121)	33.47	(0.120)	36.10	(0.059)	38.73	(0.053)
Female	0.48	(0.003)	0.47	(0.002)	0.65	(0.005)	0.64	(0.004)	0.53	(0.003)	0.51	(0.002)
Marital Status												
Currently Married	0.60	(0.003)	0.58	(0.002)	0.43	(0.005)	0.40	(0.004)	0.55	(0.003)	0.54	(0.002)
Previously Married	0.15	(0.002)	0.15	(0.002)	0.10	(0.003)	0.10	(0.003)	0.14	(0.002)	0.14	(0.001)
Never Married	0.24	(0.003)	0.27	(0.002)	0.47	(0.005)	0.49	(0.004)	0.31	(0.002)	0.32	(0.002)
Household Size	2.91	(0.009)	2.83	(0.007)	3.33	(0.016)	3.17	(0.014)	3.03	(0.008)	2.91	(0.006)
Race												
White	0.84	(0.002)	0.81	(0.002)	0.84	(0.004)	0.81	(0.003)	0.84	(0.002)	0.81	(0.002)
Black	0.12	(0.002)	0.12	(0.002)	0.11	(0.003)	0.11	(0.003)	0.12	(0.002)	0.12	(0.001)
Other/Multiple Race	0.05	(0.001)	0.07	(0.001)	0.05	(0.002)	0.07	(0.002)	0.05	(0.001)	0.07	(0.001)
Hispanic	0.11	(0.002)	0.16	(0.002)	0.12	(0.003)	0.15	(0.003)	0.11	(0.001)	0.15	(0.001)
Switched Jobs	0.13	(0.002)	0.10	(0.001)	0.23	(0.004)	0.18	(0.003)	0.16	(0.002)	0.12	(0.001)

Union	0.27	(0.003)	0.20	(0.002)	0.19	(0.004)	0.14	(0.003)	0.25	(0.003)	0.19	(0.002)
Years of Potential Experience	2.86	(0.047)	3.48	(0.044)	2.50	(0.079)	3.06	(0.085)	2.76	(0.041)	3.39	(0.039)
Education												
Less than HS	0.11	(0.002)	0.09	(0.001)	0.23	(0.004)	0.18	(0.003)	0.14	(0.002)	0.11	(0.001)
HS Diploma/GED	0.33	(0.003)	0.29	(0.002)	0.28	(0.004)	0.26	(0.004)	0.32	(0.002)	0.29	(0.002)
Some College	0.19	(0.002)	0.18	(0.002)	0.26	(0.004)	0.26	(0.004)	0.21	(0.002)	0.20	(0.002)
Associate's Degree	0.09	(0.002)	0.10	(0.001)	0.06	(0.002)	0.08	(0.002)	0.08	(0.001)	0.09	(0.001)
Bachelor's Degree	0.19	(0.002)	0.22	(0.002)	0.13	(0.003)	0.16	(0.003)	0.18	(0.002)	0.21	(0.002)
Graduate Degree	0.09	(0.002)	0.12	(0.002)	0.04	(0.002)	0.06	(0.002)	0.08	(0.001)	0.10	(0.001)
Industry												
Agriculture, Forestry, Fishing and Hunting, and Mining	0.02	(0.001)	0.01	(0.001)	0.03	(0.002)	0.01	(0.001)	0.02	(0.001)	0.01	(0.000)
Construction	0.05	(0.001)	0.07	(0.001)	0.05	(0.002)	0.06	(0.002)	0.05	(0.001)	0.07	(0.001)
Manufacturing	0.20	(0.002)	0.13	(0.002)	0.09	(0.003)	0.05	(0.002)	0.17	(0.002)	0.12	(0.001)
Wholesale Trade	0.04	(0.001)	0.03	(0.001)	0.02	(0.001)	0.01	(0.001)	0.04	(0.001)	0.03	(0.001)
Retail Trade	0.14	(0.002)	0.10	(0.001)	0.30	(0.004)	0.17	(0.003)	0.18	(0.002)	0.12	(0.001)
Transportation and Warehousing, and Utilities	0.06	(0.001)	0.06	(0.001)	0.03	(0.002)	0.03	(0.002)	0.05	(0.001)	0.05	(0.001)
Information	0.02	(0.001)	0.03	(0.001)	0.01	(0.001)	0.02	(0.001)	0.02	(0.001)	0.03	(0.001)
Finance and Insurance, and Real Estate and Rental and Leasing	0.08	(0.002)	0.08	(0.001)	0.04	(0.002)	0.04	(0.002)	0.07	(0.001)	0.07	(0.001)
Professional, Scientific, and Management, and Administrative, and Waste Management Services	0.07	(0.002)	0.10	(0.001)	0.07	(0.003)	0.09	(0.003)	0.07	(0.001)	0.10	(0.001)
Educational Services, and Health Care and Social Assistance	0.20	(0.002)	0.22	(0.002)	0.23	(0.004)	0.25	(0.004)	0.21	(0.002)	0.23	(0.002)
Arts, Entertainment, and Recreation, and Accommodation and Food Services	0.02	(0.001)	0.07	(0.001)	0.05	(0.002)	0.18	(0.003)	0.03	(0.001)	0.09	(0.001)

Other Services, Except Public Administration	0.03	(0.001)	0.04	(0.001)	0.05	(0.002)	0.05	(0.002)	0.04	(0.001)	0.04	(0.001)
Public Administration	0.06	(0.001)	0.06	(0.001)	0.02	(0.001)	0.02	(0.001)	0.05	(0.001)	0.05	(0.001)
Occupation												
Management	0.17	(0.002)	0.16	(0.002)	0.06	(0.002)	0.05	(0.002)	0.14	(0.002)	0.13	(0.001)
Professional	0.20	(0.002)	0.22	(0.002)	0.15	(0.003)	0.19	(0.003)	0.18	(0.002)	0.22	(0.002)
Services	0.11	(0.002)	0.14	(0.002)	0.23	(0.004)	0.27	(0.004)	0.14	(0.002)	0.17	(0.002)
Sales	0.10	(0.002)	0.10	(0.001)	0.16	(0.004)	0.15	(0.003)	0.12	(0.002)	0.11	(0.001)
Office and Administrative Support	0.15	(0.002)	0.15	(0.002)	0.16	(0.004)	0.16	(0.003)	0.15	(0.002)	0.15	(0.001)
Blue Collar	0.28	(0.003)	0.24	(0.002)	0.23	(0.004)	0.18	(0.003)	0.27	(0.002)	0.23	(0.002)
Natural Resources, Construction & Maintenance	-		0.11	(0.001)	-		0.08	(0.002)	-		0.10	(0.001)
Production, Transportation & Material Moving	-		0.14	(0.002)	-		0.10	(0.003)	-		0.13	(0.001)
Firm size												
Less than 10 employees	0.11	(0.002)	0.11	(0.002)	0.19	(0.004)	0.18	(0.003)	0.13	(0.002)	0.13	(0.001)
10 to 24 employees	0.09	(0.002)	0.09	(0.001)	0.13	(0.003)	0.12	(0.003)	0.10	(0.002)	0.10	(0.001)
25 to 99 employees	0.14	(0.002)	0.14	(0.002)	0.14	(0.003)	0.13	(0.003)	0.14	(0.002)	0.14	(0.001)
100 to 99 employees	0.23	(0.003)	0.22	(0.002)	0.18	(0.004)	0.18	(0.003)	0.22	(0.002)	0.21	(0.002)
1,000 or more employees	0.43	(0.003)	0.43	(0.002)	0.36	(0.005)	0.39	(0.004)	0.41	(0.003)	0.42	(0.002)

Notes: [a]Conditional on being an ESI PH. Data come the Current Population Survey. Earnings are adjusted to 1999 dollars. Estimates are weighted using the ASEC supplemental probability weights. FTFY = Full-time, full year worker sample. PTPY = Part-time or part-year worker sample. FULL = Full sample. ESI PH = Employer-sponsored insurance policy holder. ESI Dep. = Employer-sponsored insurance dependent. IPI = Individually-purchased insurance. Full-time work is defined by working 35 or more hours per week. Full-year workers work at least 40 weeks in the prior year.

Table 4.2. Summary statistics for FTFY sample by ESI status

	ESI PH				ESI Dependent				Non-ESI			
	1995		2007		1995		2007		1995		2007	
	N=24,041		N=41,112		N=3,633		N=7,535		N=6,297		N=11,848	
	Mean	SE	Mean	SE	Mean	SE	Mean	SE	Mean	SE	Mean	SE
Annual Earnings	38,275	(253)	44,401	(284)	28,284	(456)	35,471	(425)	21,626	(351)	24,066	(294)
Any Insurance	1.00	-	1.00	-	1.00	-	1.00	-	0.20	(0.006)	0.22	(0.004)
ESI PH	1.00	-	1.00	-	-		-		-		-	
ESI Dependent	-		-		1.00	-	1.00	-	-		-	
IPI	-		-		-		-		0.13	(0.005)	0.11	(0.003)
Public	-		-		-		-		0.07	(0.004)	0.11	(0.003)
Age	38.77	(0.074)	41.50	(0.068)	37.89	(0.193)	41.45	(0.159)	34.18	(0.151)	35.85	(0.126)
Female	0.47	(0.004)	0.47	(0.003)	0.64	(0.009)	0.60	(0.007)	0.43	(0.007)	0.41	(0.005)
Marital Status												
Currently Married	0.61	(0.004)	0.58	(0.003)	0.90	(0.006)	0.91	(0.004)	0.40	(0.007)	0.39	(0.005)
Previously Married	0.16	(0.003)	0.17	(0.002)	0.01	(0.001)	0.01	(0.001)	0.20	(0.006)	0.17	(0.004)
Never Married	0.23	(0.003)	0.25	(0.003)	0.09	(0.006)	0.09	(0.004)	0.40	(0.007)	0.44	(0.005)
Household Size	2.82	(0.010)	2.71	(0.008)	3.47	(0.023)	3.38	(0.017)	2.97	(0.027)	2.91	(0.020)
Race												
White	0.84	(0.003)	0.81	(0.002)	0.87	(0.007)	0.85	(0.005)	0.79	(0.007)	0.77	(0.005)
Black	0.11	(0.002)	0.12	(0.002)	0.08	(0.006)	0.08	(0.004)	0.16	(0.006)	0.15	(0.004)
Other/Multiple Race	0.04	(0.002)	0.07	(0.001)	0.04	(0.004)	0.07	(0.003)	0.05	(0.003)	0.09	(0.003)
Hispanic	0.08	(0.002)	0.11	(0.002)	0.08	(0.004)	0.10	(0.004)	0.22	(0.005)	0.33	(0.005)
Switched Jobs	0.11	(0.002)	0.08	(0.002)	0.15	(0.007)	0.11	(0.004)	0.19	(0.006)	0.13	(0.004)
Union	0.29	(0.004)	0.22	(0.003)	0.24	(0.009)	0.18	(0.006)	0.22	(0.007)	0.16	(0.004)
Years of Potential Experience	2.97	(0.056)	3.60	(0.054)	2.39	(0.121)	3.52	(0.127)	2.73	(0.118)	3.09	(0.091)
Education												
Less than HS	0.08	(0.002)	0.05	(0.001)	0.08	(0.005)	0.05	(0.003)	0.24	(0.006)	0.24	(0.005)
HS Diploma/GED	0.31	(0.003)	0.27	(0.003)	0.37	(0.009)	0.30	(0.006)	0.38	(0.007)	0.38	(0.005)

Some College	0.18	(0.003)	0.18	(0.002)	0.21	(0.008)	0.19	(0.005)	0.19	(0.006)	0.17	(0.004)
Associate's Degree	0.09	(0.002)	0.10	(0.002)	0.10	(0.005)	0.11	(0.004)	0.06	(0.003)	0.07	(0.003)
Bachelor's Degree	0.22	(0.003)	0.26	(0.003)	0.18	(0.007)	0.22	(0.006)	0.10	(0.004)	0.11	(0.003)
Graduate Degree	0.11	(0.002)	0.14	(0.002)	0.07	(0.005)	0.11	(0.004)	0.03	(0.002)	0.03	(0.002)
Industry												
Agriculture, Forestry, Fishing and Hunting, and Mining	0.02	(0.001)	0.01	(0.001)	0.02	(0.002)	0.01	(0.001)	0.04	(0.003)	0.03	(0.002)
Construction	0.04	(0.001)	0.06	(0.001)	0.06	(0.004)	0.06	(0.003)	0.10	(0.004)	0.14	(0.004)
Manufacturing	0.23	(0.003)	0.15	(0.002)	0.14	(0.006)	0.11	(0.004)	0.14	(0.005)	0.09	(0.003)
Wholesale Trade	0.04	(0.001)	0.03	(0.001)	0.05	(0.004)	0.03	(0.002)	0.04	(0.003)	0.03	(0.002)
Retail Trade	0.11	(0.002)	0.09	(0.002)	0.17	(0.007)	0.12	(0.004)	0.25	(0.006)	0.13	(0.004)
Transportation and Warehousing, and Utilities	0.06	(0.002)	0.06	(0.001)	0.04	(0.004)	0.04	(0.003)	0.04	(0.003)	0.05	(0.002)
Information	0.02	(0.001)	0.03	(0.001)	0.01	(0.002)	0.02	(0.002)	0.01	(0.002)	0.02	(0.001)
Finance and Insurance, and Real Estate and Rental and Leasing	0.08	(0.002)	0.08	(0.002)	0.08	(0.005)	0.10	(0.004)	0.05	(0.003)	0.05	(0.002)
Professional, Scientific, and Management, and Administrative, and Waste Management Services	0.07	(0.002)	0.09	(0.002)	0.08	(0.005)	0.11	(0.004)	0.08	(0.004)	0.10	(0.003)
Educational Services, and Health Care and Social Assistance	0.22	(0.003)	0.24	(0.002)	0.24	(0.008)	0.25	(0.006)	0.13	(0.005)	0.13	(0.004)
Arts, Entertainment, and Recreation, and Accommodation and Food Services	0.02	(0.001)	0.05	(0.001)	0.02	(0.003)	0.06	(0.003)	0.03	(0.003)	0.15	(0.004)
Other Services, Except Public Administration	0.02	(0.001)	0.03	(0.001)	0.05	(0.004)	0.05	(0.003)	0.07	(0.004)	0.07	(0.003)
Public Administration	0.07	(0.002)	0.07	(0.001)	0.04	(0.004)	0.04	(0.003)	0.02	(0.002)	0.02	(0.001)
Occupation												
Management	0.19	(0.003)	0.18	(0.002)	0.16	(0.007)	0.17	(0.005)	0.08	(0.004)	0.07	(0.003)
Professional	0.22	(0.003)	0.26	(0.003)	0.18	(0.007)	0.23	(0.006)	0.09	(0.004)	0.09	(0.003)

Services	0.08	(0.002)	0.10	(0.002)	0.10	(0.006)	0.12	(0.004)	0.21	(0.006)	0.25	(0.005)
Sales	0.09	(0.002)	0.09	(0.002)	0.12	(0.006)	0.12	(0.004)	0.12	(0.005)	0.11	(0.003)
Office and Administrative Support	0.15	(0.003)	0.15	(0.002)	0.20	(0.007)	0.18	(0.005)	0.11	(0.005)	0.11	(0.003)
Blue Collar	0.26	(0.003)	0.22	(0.002)	0.23	(0.008)	0.18	(0.005)	0.38	(0.007)	0.36	(0.005)
Natural Resources, Construction & Maintenance	-		0.09	(0.002)	-		0.08	(0.004)	-		0.19	(0.004)
Production, Transportation & Material Moving	-		0.13	(0.002)	-		0.10	(0.004)	-		0.17	(0.004)
Firm size												
Less than 10 employees	0.05	(0.002)	0.06	(0.001)	0.18	(0.007)	0.17	(0.005)	0.26	(0.006)	0.26	(0.005)
10 to 24 employees	0.06	(0.002)	0.07	(0.001)	0.13	(0.006)	0.12	(0.005)	0.16	(0.005)	0.17	(0.004)
25 to 99 employees	0.13	(0.002)	0.13	(0.002)	0.15	(0.007)	0.16	(0.005)	0.16	(0.005)	0.17	(0.004)
100 to 99 employees	0.25	(0.003)	0.24	(0.002)	0.21	(0.008)	0.21	(0.005)	0.18	(0.006)	0.16	(0.004)
1,000 or more employees	0.49	(0.004)	0.51	(0.003)	0.33	(0.009)	0.34	(0.006)	0.23	(0.006)	0.24	(0.005)

Notes: Data come the Current Population Survey. Earnings are adjusted to 1999 dollars. Estimates are weighted using the ASEC supplemental probability weights. FTFY = Full-time, full year worker sample. PTPY = Part-time or part-year worker sample. FULL = Full sample. ESI PH = Employer-sponsored insurance policy holder. ESI Dep. = Employer-sponsored insurance dependent. IPI = Individually-purchased insurance. Full-time work is defined by working 35 or more hours per week. Full-year workers work at least 40 weeks in the prior year.

Table 4.3. Summary statistics for PTPY sample by ESI status

	ESI PH				ESI Dependent				Non-ESI			
	1995		2007		1995		2007		1995		2007	
	N=3,212		N=4,158		N=4,734		N=6,885		N=5,455		N=8,075	
	Mean	SE	Mean	SE	Mean	SE	Mean	SE	Mean	SE	Mean	SE
Annual Earnings	17,512	(386)	21,761	(467)	8,083	(162)	9,901	(179)	7,992	(408)	9,509	(258)
Any Insurance	1.00	-	1.00	-	1.00	-	1.00	-	0.33	(0.007)	0.31	(0.006)
ESI PH	1.00	-	1.00	-	-		-		-		-	
ESI Dependent	-		-		1.00	-	1.00	-	-		-	
IPI	-		-		-		-		0.13	(0.005)	0.12	(0.004)
Public	-		-		-		-		0.20	(0.006)	0.19	(0.005)
Age	37.09	(0.255)	39.83	(0.260)	30.02	(0.203)	31.58	(0.205)	29.97	(0.173)	31.65	(0.167)
Female	0.67	(0.009)	0.66	(0.009)	0.71	(0.008)	0.69	(0.007)	0.58	(0.008)	0.58	(0.007)
Marital Status												
Currently Married	0.54	(0.010)	0.50	(0.009)	0.53	(0.008)	0.52	(0.007)	0.28	(0.007)	0.27	(0.006)
Previously Married	0.14	(0.007)	0.16	(0.007)	0.00	(0.001)	0.01	(0.001)	0.17	(0.006)	0.15	(0.005)
Never Married	0.32	(0.009)	0.35	(0.009)	0.46	(0.008)	0.47	(0.007)	0.55	(0.008)	0.58	(0.006)
Household Size	2.89	(0.030)	2.67	(0.026)	3.87	(0.020)	3.77	(0.018)	3.11	(0.030)	2.96	(0.023)
Race												
White	0.86	(0.007)	0.82	(0.007)	0.90	(0.005)	0.88	(0.005)	0.78	(0.007)	0.76	(0.006)
Black	0.10	(0.006)	0.11	(0.006)	0.06	(0.004)	0.06	(0.004)	0.17	(0.006)	0.15	(0.005)
Other/Multiple Race	0.05	(0.004)	0.07	(0.004)	0.04	(0.003)	0.06	(0.003)	0.05	(0.003)	0.08	(0.003)
Hispanic	0.09	(0.005)	0.10	(0.005)	0.06	(0.003)	0.08	(0.003)	0.19	(0.006)	0.23	(0.005)
Switched Jobs	0.20	(0.008)	0.16	(0.007)	0.22	(0.007)	0.15	(0.005)	0.25	(0.007)	0.20	(0.005)
Union	0.24	(0.011)	0.18	(0.009)	0.19	(0.007)	0.13	(0.006)	0.17	(0.006)	0.12	(0.005)
Years of Potential Experience	4.38	(0.223)	4.97	(0.225)	1.64	(0.097)	2.21	(0.121)	2.16	(0.115)	2.74	(0.122)
Education												
Less than HS	0.11	(0.006)	0.07	(0.004)	0.23	(0.007)	0.18	(0.005)	0.30	(0.007)	0.24	(0.005)
HS Diploma/GED	0.29	(0.009)	0.24	(0.008)	0.25	(0.007)	0.21	(0.006)	0.31	(0.007)	0.31	(0.006)

Some College	0.23	(0.008)	0.21	(0.008)	0.30	(0.008)	0.31	(0.007)	0.24	(0.007)	0.26	(0.006)
Associate's Degree	0.08	(0.005)	0.11	(0.006)	0.06	(0.004)	0.08	(0.004)	0.05	(0.003)	0.06	(0.003)
Bachelor's Degree	0.21	(0.008)	0.25	(0.008)	0.12	(0.005)	0.16	(0.005)	0.08	(0.004)	0.11	(0.004)
Graduate Degree	0.09	(0.006)	0.13	(0.006)	0.04	(0.003)	0.06	(0.003)	0.02	(0.002)	0.03	(0.002)
Industry												
Agriculture, Forestry, Fishing and Hunting, and Mining	0.02	(0.003)	0.01	(0.002)	0.03	(0.003)	0.01	(0.001)	0.05	(0.003)	0.02	(0.002)
Construction	0.06	(0.005)	0.05	(0.004)	0.03	(0.003)	0.03	(0.003)	0.06	(0.004)	0.09	(0.004)
Manufacturing	0.11	(0.006)	0.07	(0.005)	0.06	(0.004)	0.03	(0.003)	0.10	(0.005)	0.06	(0.003)
Wholesale Trade	0.02	(0.003)	0.02	(0.002)	0.02	(0.002)	0.01	(0.002)	0.02	(0.002)	0.01	(0.002)
Retail Trade	0.18	(0.008)	0.11	(0.006)	0.34	(0.008)	0.19	(0.006)	0.33	(0.007)	0.18	(0.005)
Transportation and Warehousing, and Utilities	0.04	(0.004)	0.05	(0.004)	0.02	(0.002)	0.02	(0.002)	0.03	(0.003)	0.03	(0.002)
Information	0.02	(0.003)	0.03	(0.003)	0.01	(0.002)	0.02	(0.002)	0.01	(0.001)	0.01	(0.002)
Finance and Insurance, and Real Estate and Rental and Leasing	0.05	(0.004)	0.06	(0.004)	0.05	(0.004)	0.04	(0.003)	0.03	(0.002)	0.03	(0.002)
Professional, Scientific, and Management, and Administrative, and Waste Management Services	0.07	(0.005)	0.09	(0.006)	0.07	(0.004)	0.08	(0.004)	0.09	(0.004)	0.11	(0.004)
Educational Services, and Health Care and Social Assistance	0.34	(0.009)	0.38	(0.009)	0.25	(0.007)	0.28	(0.006)	0.16	(0.006)	0.17	(0.005)
Arts, Entertainment, and Recreation, and Accommodation and Food Services	0.04	(0.004)	0.08	(0.005)	0.06	(0.004)	0.20	(0.006)	0.05	(0.003)	0.21	(0.005)
Other Services, Except Public Administration	0.02	(0.003)	0.03	(0.003)	0.05	(0.004)	0.05	(0.003)	0.07	(0.004)	0.06	(0.003)
Public Administration	0.03	(0.003)	0.03	(0.003)	0.02	(0.002)	0.02	(0.002)	0.02	(0.002)	0.01	(0.001)
Occupation												
Management	0.09	(0.006)	0.09	(0.005)	0.06	(0.004)	0.05	(0.003)	0.04	(0.003)	0.04	(0.002)
Professional	0.25	(0.009)	0.31	(0.009)	0.16	(0.006)	0.21	(0.006)	0.09	(0.004)	0.11	(0.004)
Services	0.14	(0.007)	0.17	(0.007)	0.24	(0.007)	0.27	(0.006)	0.28	(0.007)	0.33	(0.006)
Sales	0.11	(0.006)	0.10	(0.006)	0.19	(0.006)	0.17	(0.005)	0.16	(0.006)	0.16	(0.005)
Office and Administrative Support	0.18	(0.008)	0.16	(0.007)	0.19	(0.006)	0.18	(0.006)	0.13	(0.005)	0.13	(0.004)

Blue Collar	0.22	(0.008)	0.17	(0.007)	0.17	(0.006)	0.12	(0.005)	0.30	(0.007)	0.24	(0.006)
Natural Resources, Construction & Maintenance	-		0.06	(0.005)	-		0.04	(0.003)	-		0.12	(0.004)
Production, Transportation & Material Moving	-		0.11	(0.006)	-		0.07	(0.004)	-		0.12	(0.004)
Firm size												
Less than 10 employees	0.10	(0.006)	0.10	(0.006)	0.20	(0.006)	0.18	(0.006)	0.24	(0.007)	0.23	(0.005)
10 to 24 employees	0.08	(0.005)	0.08	(0.005)	0.13	(0.006)	0.13	(0.005)	0.14	(0.005)	0.13	(0.005)
25 to 99 employees	0.13	(0.007)	0.11	(0.006)	0.13	(0.006)	0.13	(0.005)	0.15	(0.005)	0.13	(0.005)
100 to 99 employees	0.22	(0.008)	0.21	(0.008)	0.19	(0.006)	0.18	(0.006)	0.15	(0.006)	0.16	(0.005)
1,000 or more employees	0.47	(0.010)	0.50	(0.009)	0.34	(0.008)	0.38	(0.007)	0.31	(0.007)	0.34	(0.006)

Notes: Data come the Current Population Survey. Earnings are adjusted to 1999 dollars. Estimates are weighted using the ASEC supplemental probability weights. FTFY = Full-time, full year worker sample. PTPY = Part-time or part-year worker sample. FULL = Full sample. ESI PH = Employer-sponsored insurance policy holder. ESI Dep. = Employer-sponsored insurance dependent. IPI = Individually-purchased insurance. Full-time work is defined by working 35 or more hours per week. Full-year workers work at least 40 weeks in the prior year.

Table 4.4. Summary statistics for the FULL sample by ESI status

	ESI PH				ESI Dependent				Non-ESI			
	1995		2007		1995		2007		1995		2007	
	N=27,253		N=45,270		N=8,367		N=14,420		N=11,752		N=19,923	
	Mean	SE	Mean	SE	Mean	SE	Mean	SE	Mean	SE	Mean	SE
Annual Earnings	35,872	(233)	42,314	(264)	16,712	(248)	23,398	(270)	15,323	(276)	18,300	(213)
FTFY	0.88	(0.002)	0.91	(0.002)	0.43	(0.006)	0.53	(0.005)	0.54	(0.005)	0.60	(0.004)
Any Insurance	1.00	-	1.00	-	1.00	-	1.00	-	0.26	(0.005)	0.26	(0.004)
ESI PH	1.00	-	1.00	-	-		-		-		-	
ESI Dependent	-		-		1.00	-	1.00	-	-		-	
IPI	-		-		-		-		0.13	(0.004)	0.12	(0.003)
Public	-		-		-		-		0.13	(0.003)	0.14	(0.003)
Age	38.57	(0.072)	41.34	(0.066)	33.38	(0.151)	36.79	(0.137)	32.23	(0.116)	34.19	(0.102)
Female	0.50	(0.003)	0.48	(0.003)	0.68	(0.006)	0.65	(0.005)	0.50	(0.005)	0.48	(0.004)
Marital Status												
Currently Married	0.60	(0.003)	0.58	(0.003)	0.69	(0.006)	0.72	(0.004)	0.35	(0.005)	0.34	(0.004)
Previously Married	0.16	(0.002)	0.17	(0.002)	0.00	(0.001)	0.01	(0.001)	0.18	(0.004)	0.16	(0.003)
Never Married	0.24	(0.003)	0.26	(0.003)	0.30	(0.006)	0.27	(0.004)	0.47	(0.005)	0.49	(0.004)
Household Size	2.83	(0.010)	2.70	(0.008)	3.70	(0.015)	3.57	(0.013)	3.04	(0.020)	2.93	(0.015)
Race												
White	0.85	(0.003)	0.81	(0.002)	0.89	(0.004)	0.86	(0.003)	0.78	(0.005)	0.77	(0.004)
Black	0.11	(0.002)	0.12	(0.002)	0.07	(0.003)	0.07	(0.003)	0.16	(0.004)	0.15	(0.003)
Other/Multiple Race	0.04	(0.001)	0.07	(0.001)	0.04	(0.002)	0.06	(0.002)	0.05	(0.002)	0.08	(0.002)
Hispanic	0.08	(0.002)	0.11	(0.002)	0.07	(0.003)	0.09	(0.003)	0.21	(0.004)	0.29	(0.004)
Switched Jobs	0.12	(0.002)	0.09	(0.002)	0.19	(0.005)	0.13	(0.003)	0.22	(0.004)	0.16	(0.003)
Union	0.28	(0.004)	0.22	(0.003)	0.21	(0.006)	0.16	(0.004)	0.20	(0.005)	0.15	(0.003)
Years of Potential Experience	3.13	(0.056)	3.72	(0.054)	1.96	(0.076)	2.90	(0.088)	2.46	(0.083)	2.95	(0.073)
Education												
Less than HS	0.08	(0.002)	0.05	(0.001)	0.17	(0.005)	0.11	(0.003)	0.27	(0.005)	0.24	(0.003)

HS Diploma/GED	0.31	(0.003)	0.27	(0.002)	0.30	(0.006)	0.26	(0.004)	0.34	(0.005)	0.35	(0.004)
Some College	0.19	(0.003)	0.18	(0.002)	0.26	(0.005)	0.25	(0.004)	0.21	(0.004)	0.20	(0.003)
Associate's Degree	0.09	(0.002)	0.10	(0.002)	0.08	(0.003)	0.10	(0.003)	0.06	(0.002)	0.07	(0.002)
Bachelor's Degree	0.22	(0.003)	0.26	(0.002)	0.14	(0.004)	0.20	(0.004)	0.09	(0.003)	0.11	(0.003)
Graduate Degree	0.11	(0.002)	0.14	(0.002)	0.05	(0.003)	0.09	(0.003)	0.03	(0.002)	0.03	(0.001)
Industry												
Agriculture, Forestry, Fishing and Hunting, and Mining	0.02	(0.001)	0.01	(0.001)	0.02	(0.002)	0.01	(0.001)	0.05	(0.002)	0.03	(0.001)
Construction	0.04	(0.001)	0.05	(0.001)	0.04	(0.002)	0.05	(0.002)	0.08	(0.003)	0.12	(0.003)
Manufacturing	0.21	(0.003)	0.14	(0.002)	0.09	(0.004)	0.07	(0.003)	0.12	(0.003)	0.08	(0.002)
Wholesale Trade	0.04	(0.001)	0.03	(0.001)	0.03	(0.002)	0.02	(0.001)	0.03	(0.002)	0.02	(0.001)
Retail Trade	0.12	(0.002)	0.10	(0.002)	0.27	(0.005)	0.15	(0.004)	0.28	(0.005)	0.15	(0.003)
Transportation and Warehousing, and Utilities	0.06	(0.002)	0.06	(0.001)	0.03	(0.002)	0.03	(0.002)	0.03	(0.002)	0.04	(0.002)
Information	0.02	(0.001)	0.03	(0.001)	0.01	(0.001)	0.02	(0.001)	0.01	(0.001)	0.02	(0.001)
Finance and Insurance, and Real Estate and Rental and Leasing	0.08	(0.002)	0.08	(0.002)	0.06	(0.003)	0.07	(0.003)	0.04	(0.002)	0.04	(0.002)
Professional, Scientific, and Management, and Administrative, and Waste Management Services	0.07	(0.002)	0.09	(0.002)	0.07	(0.003)	0.09	(0.003)	0.08	(0.003)	0.10	(0.003)
Educational Services, and Health Care and Social Assistance	0.23	(0.003)	0.25	(0.002)	0.25	(0.005)	0.26	(0.004)	0.14	(0.004)	0.15	(0.003)
Arts, Entertainment, and Recreation, and Accommodation and Food Services	0.02	(0.001)	0.05	(0.001)	0.04	(0.002)	0.13	(0.003)	0.04	(0.002)	0.17	(0.003)
Other Services, Except Public Administration	0.02	(0.001)	0.03	(0.001)	0.05	(0.003)	0.05	(0.002)	0.07	(0.003)	0.07	(0.002)
Public Administration	0.06	(0.002)	0.07	(0.001)	0.03	(0.002)	0.03	(0.002)	0.02	(0.001)	0.01	(0.001)
Occupation												
Management	0.18	(0.003)	0.17	(0.002)	0.10	(0.004)	0.11	(0.003)	0.06	(0.003)	0.06	(0.002)

Professional	0.23	(0.003)	0.27	(0.002)	0.17	(0.005)	0.22	(0.004)	0.09	(0.003)	0.10	(0.002)
Services	0.09	(0.002)	0.11	(0.002)	0.18	(0.005)	0.19	(0.004)	0.24	(0.004)	0.28	(0.004)
Sales	0.10	(0.002)	0.09	(0.002)	0.16	(0.004)	0.14	(0.003)	0.14	(0.004)	0.13	(0.003)
Office and Administrative Support	0.15	(0.002)	0.15	(0.002)	0.19	(0.005)	0.18	(0.004)	0.12	(0.003)	0.12	(0.003)
Blue Collar	0.26	(0.003)	0.21	(0.002)	0.19	(0.005)	0.15	(0.004)	0.34	(0.005)	0.31	(0.004)
Natural Resources, Construction & Maintenance	-		0.08	(0.002)	-		0.06	(0.002)	-		0.16	(0.003)
Production, Transportation & Material Moving	-		0.13	(0.002)	-		0.09	(0.003)	-		0.15	(0.003)
Firm size												
Less than 10 employees	0.06	(0.002)	0.06	(0.001)	0.19	(0.005)	0.17	(0.004)	0.25	(0.005)	0.25	(0.004)
10 to 24 employees	0.07	(0.002)	0.07	(0.001)	0.13	(0.004)	0.13	(0.003)	0.15	(0.004)	0.15	(0.003)
25 to 99 employees	0.13	(0.002)	0.13	(0.002)	0.14	(0.004)	0.15	(0.003)	0.16	(0.004)	0.16	(0.003)
100 to 99 employees	0.25	(0.003)	0.24	(0.002)	0.20	(0.005)	0.19	(0.004)	0.17	(0.004)	0.16	(0.003)
1,000 or more employees	0.49	(0.003)	0.50	(0.003)	0.33	(0.006)	0.36	(0.005)	0.27	(0.005)	0.28	(0.004)

Notes: Data come the Current Population Survey. Earnings are adjusted to 1999 dollars. Estimates are weighted using the ASEC supplemental probability weights. FTFY = Full-time, full year worker sample. PTPY = Part-time or part-year worker sample. FULL = Full sample. ESI PH = Employer-sponsored insurance policy holder. ESI Dep. = Employer-sponsored insurance dependent. IPI = Individually-purchased insurance. Full-time work is defined by working 35 or more hours per week. Full-year workers work at least 40 weeks in the prior year.

Table 4.5. DD regression estimates by sample and referent group

Panel A: Binary ESI

DV: Log Annual Earnings	FTFY	FTFY-ESI	PTPY	PTPY-ESI	FULL	IPW	EB
ESI PH	0.219***	0.182***	0.558***	0.525***	0.588***	0.263***	0.266***
	(0.009)	(0.009)	(0.018)	(0.019)	(0.012)	(0.011)	(0.011)
2007	0.111***	0.124***	0.216***	0.173***	0.229***	0.125***	0.133***
	(0.008)	(0.011)	(0.013)	(0.017)	(0.009)	(0.008)	(0.010)
ESI PH*2007	-0.015	-0.031**	-0.020	0.015	-0.098***	-0.017	-0.027**
	(0.010)	(0.013)	(0.023)	(0.022)	(0.010)	(0.010)	(0.012)

Panel B: Continuous Premiums

DV: Log Annual Earnings	FTFY	FTFY-ESI	PTPY	PTPY-ESI	FULL	IPW	EB
Log Employer Prem.	0.051***	0.030***	0.071**	0.052	0.098***	-	-
	(0.009)	(0.010)	(0.035)	(0.036)	(0.012)		
Log Employee Prem.	-0.028***	-0.010	-0.003	0.014	-0.031**	-	-
	(0.010)	(0.012)	(0.041)	(0.042)	(0.015)		
2007	0.108***	0.117***	0.215***	0.172***	0.226***	-	-
	(0.008)	(0.010)	(0.013)	(0.017)	(0.009)		
Log Employer Prem.*2007	-0.017	-0.021*	-0.078	-0.072	-0.035***	-	-
	(0.012)	(0.011)	(0.052)	(0.053)	(0.012)		
Log Employee Prem.*2007	0.017	0.020	0.083	0.081	0.025*	-	-
	(0.013)	(0.013)	(0.059)	(0.061)	(0.013)		
N	94,466	76,321	32,519	18,989	126,985	126,985	126,985

Notes: * p<.10, ** p<.05, *** p<.01. Data come the Current Population Survey. Earnings are deflated to 1999 dollars. The full sample includes 126,985 observations. The FTFY though FULL columns are weighted using the ASEC supplemental probability weights. FTFY = Full-time, full year worker sample. PTPY = Part-time or part-year worker sample. FULL = Full sample. ESI PH = Employer-sponsored insurance policy holder. ESI Dep. = Employer-sponsored insurance dependent. Full-time work is defined by working 35 or more hours per week. Full-year workers work at least 40 weeks in the prior year.

Table 4.6. DDD regression estimates by sample and referent group

Panel A: Binary ESI

DV: Log Annual Earnings	FTFY	FTFY-ESI	PTPY	PTPY-ESI	FULL
ESI PH*2007	-0.002	-0.032*	-0.038	0.002	-0.091***
	(0.013)	(0.018)	(0.051)	(0.064)	(0.014)
ESI PH*Female	-0.023*	0.031	-0.043	-0.041	0.066***
	(0.013)	(0.019)	(0.041)	(0.056)	(0.015)
ESI PH*Female*2007	-0.025	0.010	0.027	0.019	-0.001
	(0.016)	(0.024)	(0.064)	(0.081)	(0.020)

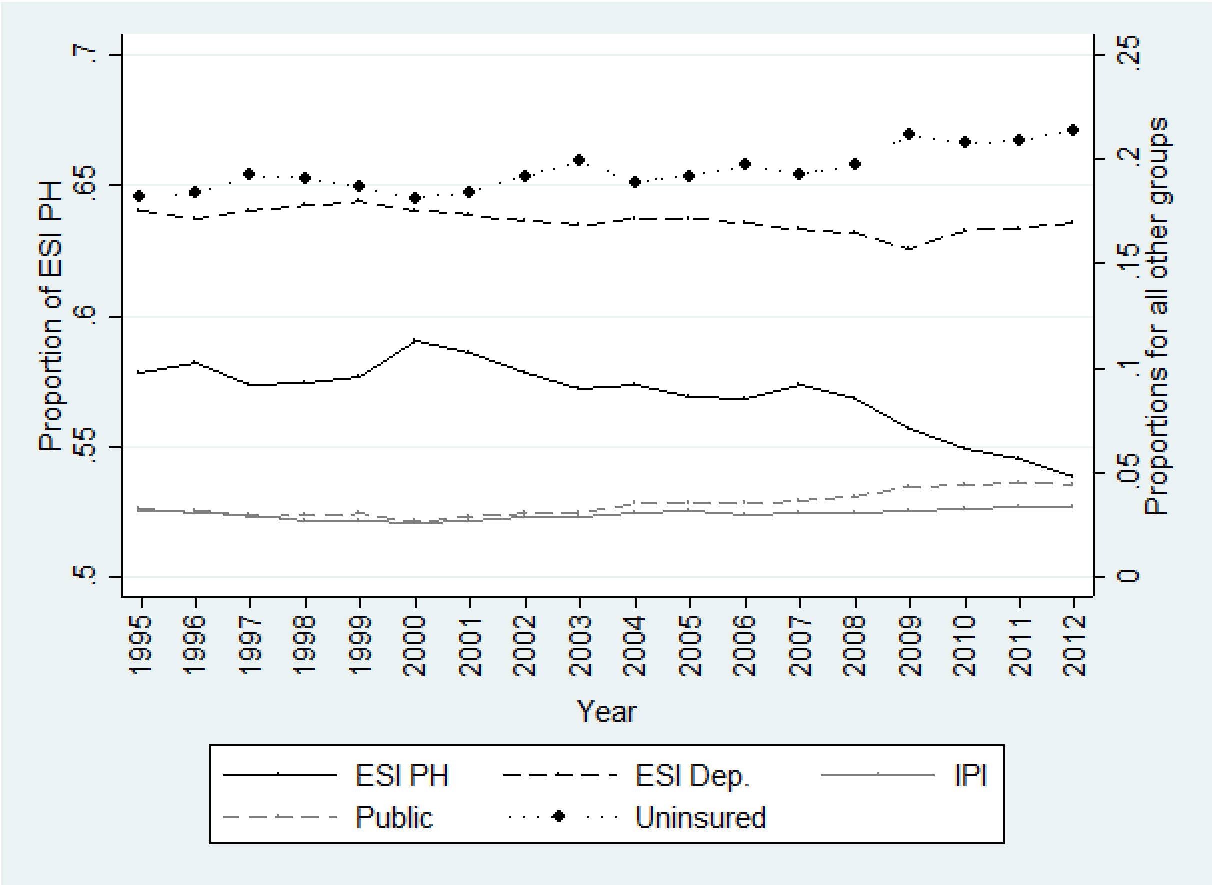
Panel B: Continuous Premiums

DV: Log Annual Earnings	FTFY	FTFY-ESI	PTPY	PTPY -ESI	FULL
Log Employer Prem.*2007	-0.020	-0.023	-0.065	-0.061	-0.035***
	(0.016)	(0.015)	(0.091)	(0.094)	(0.013)
Log Employee Prem.*2007	0.023	0.024	0.066	0.065	0.027*
	(0.018)	(0.017)	(0.106)	(0.107)	(0.014)
Log Employer Prem.*Female	0.054***	0.059***	-0.044	-0.032	0.074***
	(0.019)	(0.019)	(0.069)	(0.069)	(0.017)
Log Employee Prem.*Female	-0.067***	-0.064***	0.048	0.036	-0.076***
	(0.021)	(0.021)	(0.081)	(0.079)	(0.019)
Log Employer Prem.*Female*2007	0.009	0.013	-0.015	-0.019	0.002
	(0.020)	(0.019)	(0.112)	(0.114)	(0.020)
Log Employee Prem.*Female*2007	-0.012	-0.013	0.016	0.018	-0.002
	(0.023)	(0.022)	(0.130)	(0.129)	(0.024)
N	94,466	76,321	32,519	18,989	126,985

Notes: * p<.10, ** p<.05, *** p<.01. Data come the Current Population Survey. Earnings are adjusted to 1999 dollars. The full sample includes 126,985 observations. Estimates are weighted using the ASEC supplemental probability weights. FTFY = Full-time, full year worker sample. PTPY = Part-time or part-year worker sample. FULL = Full sample. ESI PH = Employer-sponsored insurance policy holder. ESI Dep. = Employer-sponsored insurance dependent. Full-time work is defined by working 35 or more hours per week. Full-year workers work at least 40 weeks in the prior year.

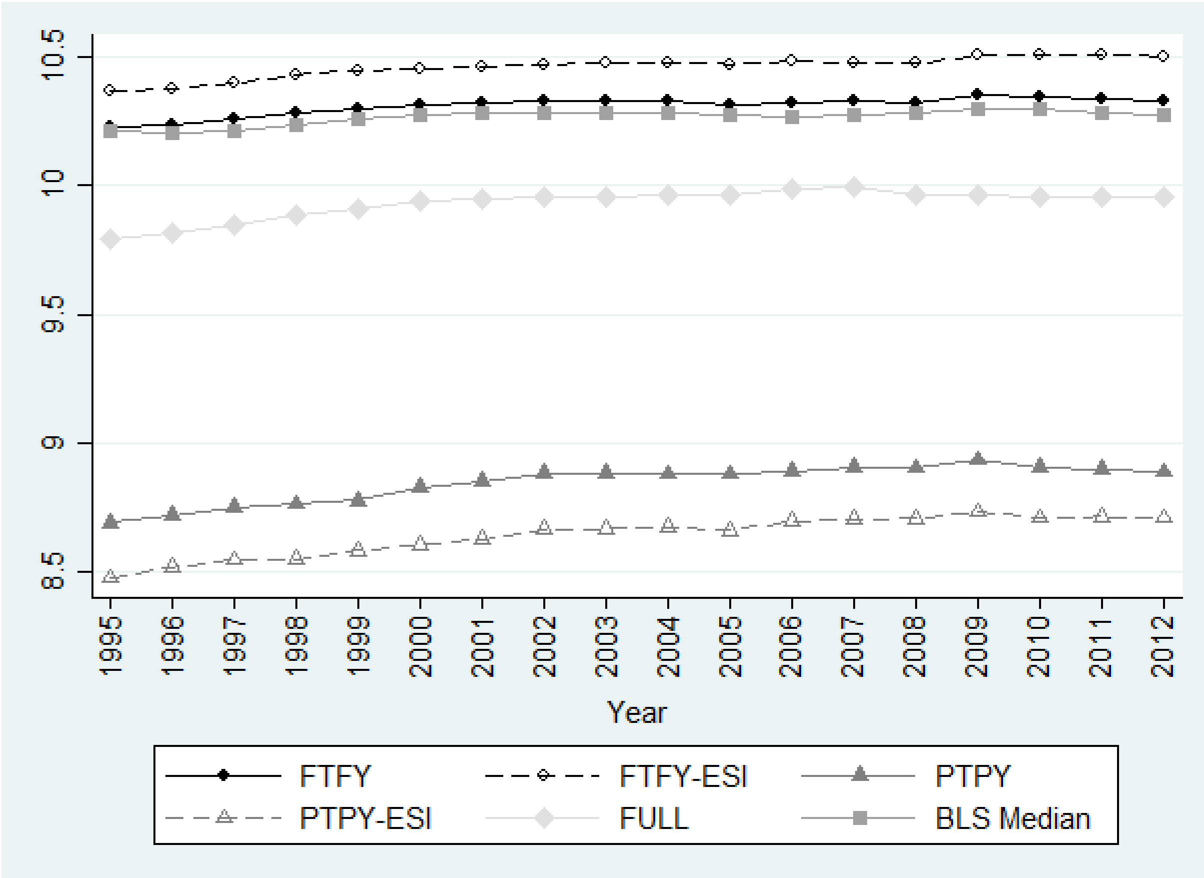
Figures

Figure 4.1. Health insurance coverage, 1995–2012



Notes: Data come the Current Population Survey. ESI PH = Employer-sponsored insurance policy holder. ESI Dep. = Employer-sponsored insurance dependent. IPI = Individually-purchased insurance. This figure presents trends in insurance coverage by type of coverage between 1995 and 2012. ESI PHs are on the left axis and all other insurance types follow the right axis.

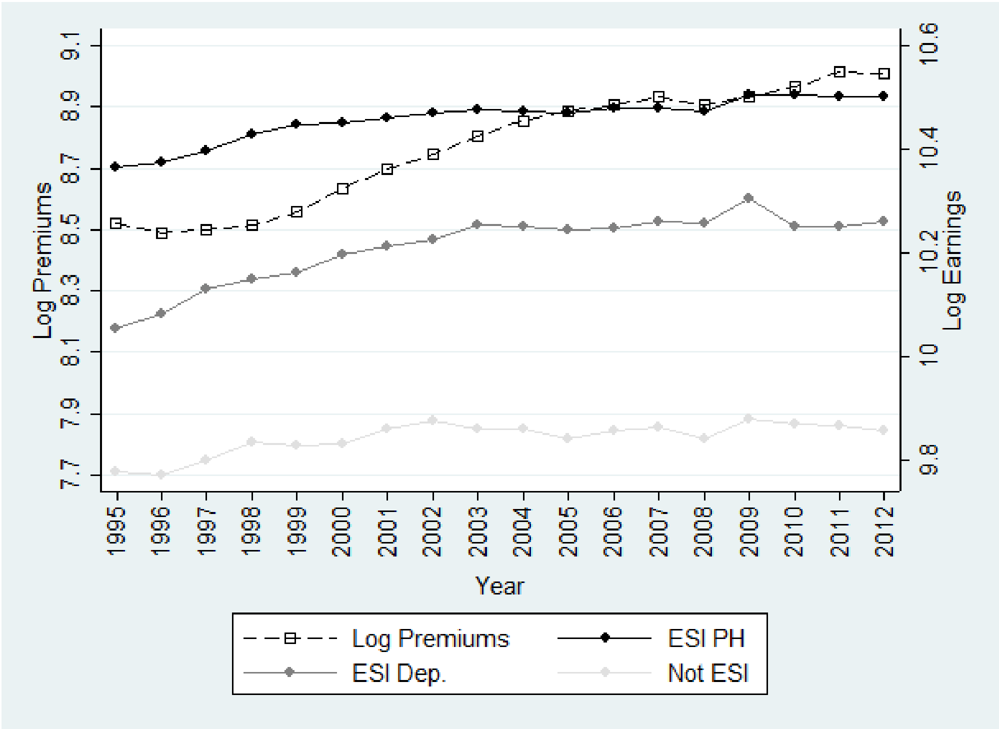
Figure 4.2. Log earnings by sample definition



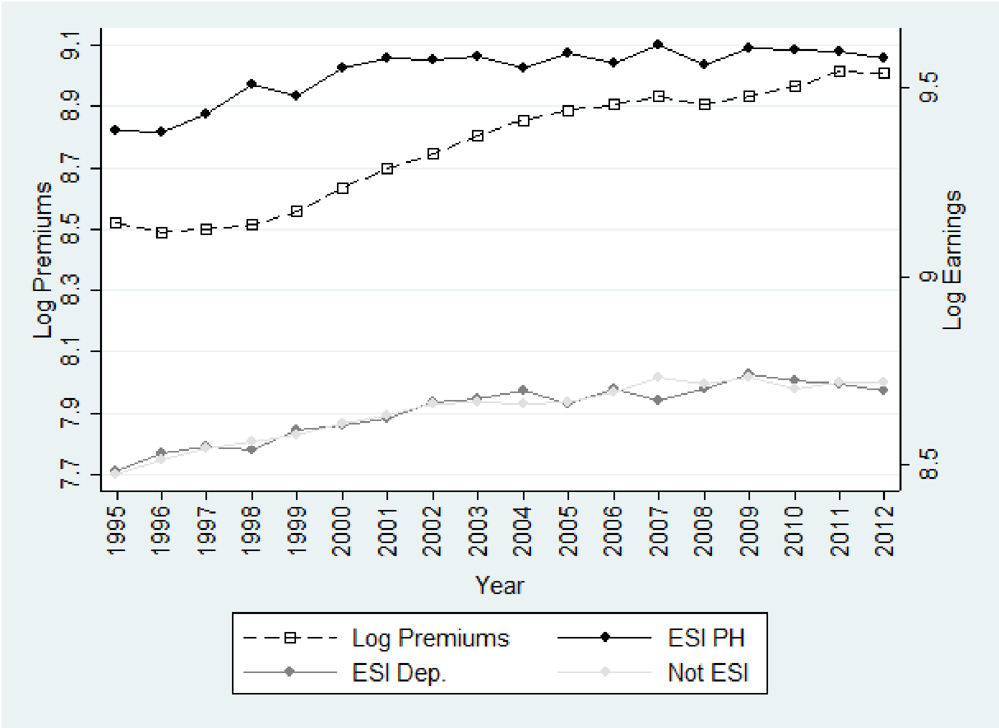
Notes: Data come the Current Population Survey. Earnings are adjusted to 1999 dollars. FTFY = full-time, full year worker sample. FTFY-ESI = full-time, full year workers covered by employer-sponsored insurance. PTPY = part-time or part-year workers. PTPY-ESI = part-time or part-year workers covered by employer-sponsored insurance. FULL = full sample. BLS = Bureau of Labor Statistics. This figure presents log annual average earnings between 1995 and 2012 for the full sample and four subsamples based on work status. Full-time work is defined by working 35 or more hours per week. Full-year workers work at least 40 weeks in the prior year. Also included are median earnings for full-time employees from CPS data series LEU0252881500 to document that the author's calculations of earnings are consistent with national estimates produced by the BLS. The FTFY series closely resembles the BLS FTFY median earnings series.

Figure 4.3. Log earnings across ESI policy holders and referent groups, by sample definition

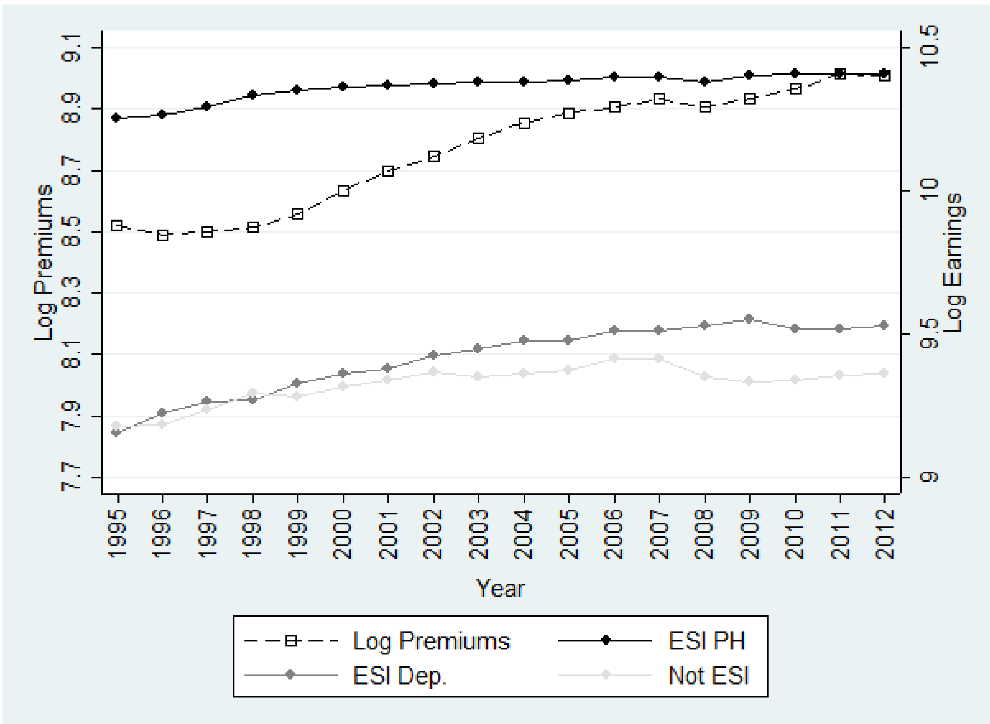
Panel A. FTFY sample



Panel B. PTPY sample

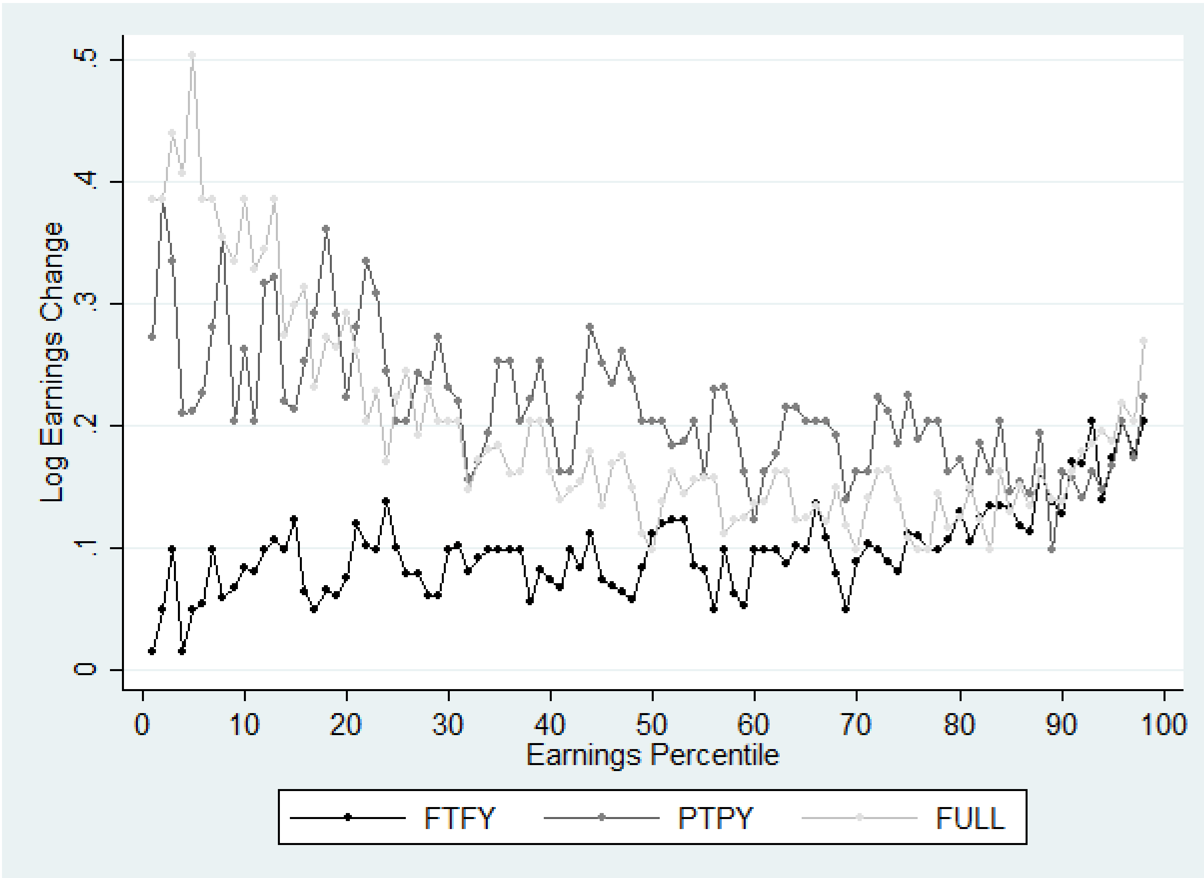


Panel C. FULL sample



Notes: Data come the Current Population Survey. Earnings are adjusted to 1999 dollars. FTFY = Full-time, full year worker sample. PTPY = Part-time or part-year worker sample. FULL = Full sample. ESI PH = Employer-sponsored insurance policy holder. ESI Dep. = Employer-sponsored insurance dependent. Full-time work is defined by working 35 or more hours per week. Full-year workers work at least 40 weeks in the prior year. This figure presents log premiums on the left axis and log annual earnings on the right axis. The series ranges from 1995 to 2012 and earnings are presented by health insurance status for the FTFY, PTPY, and FULL samples.

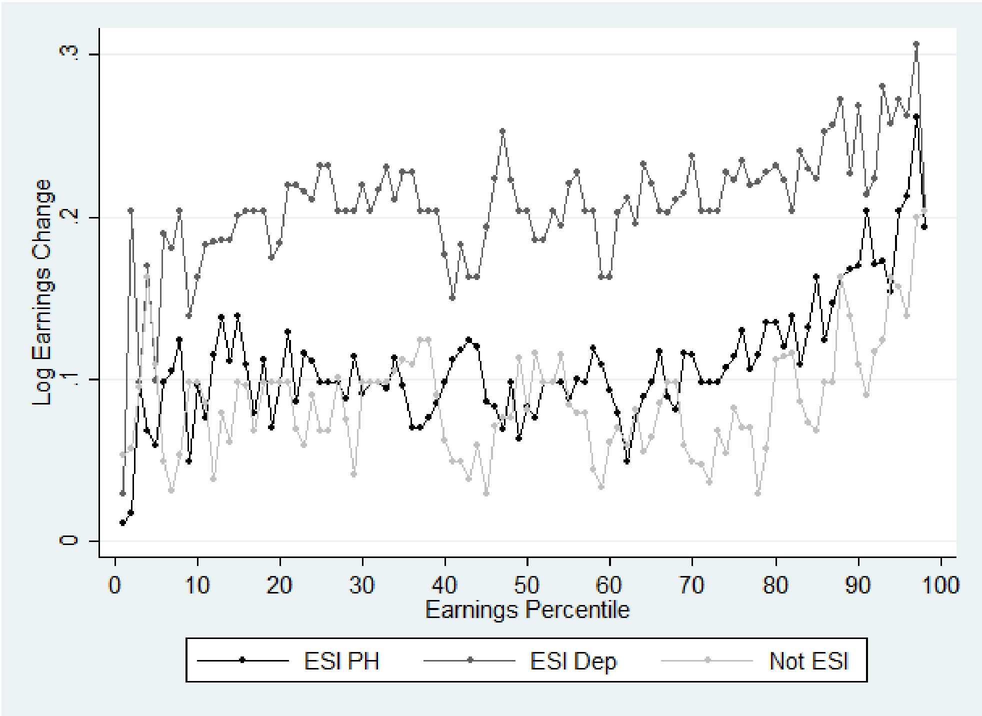
Figure 4.4. Log earnings change by percentile by sample, 1995 and 2007



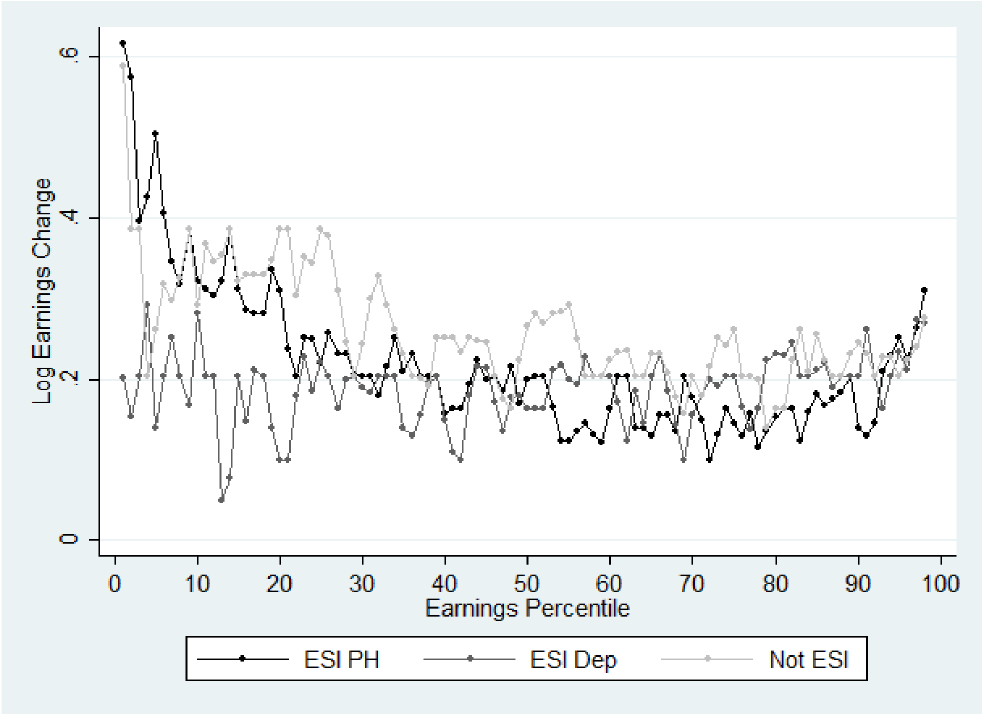
Notes: Data come the Current Population Survey. Earnings are adjusted to 1999 dollars. FTFY = Full-time, full year worker sample. PTPY = Part-time or part-year worker sample. FULL = Full sample. ESI PH = Employer-sponsored insurance policy holder. ESI Dep. = Employer-sponsored insurance dependent. Full-time work is defined by working 35 or more hours per week. Full-year workers work at least 40 weeks in the prior year. This figure presents changes in log annual earnings at each earnings percentile across 1995 and 2007. Earnings changes are grouped by the FTFY, PTPY and FULL samples.

Figure 4.5. Log earnings change by percentile by sample and referent group, 1995 and 2007

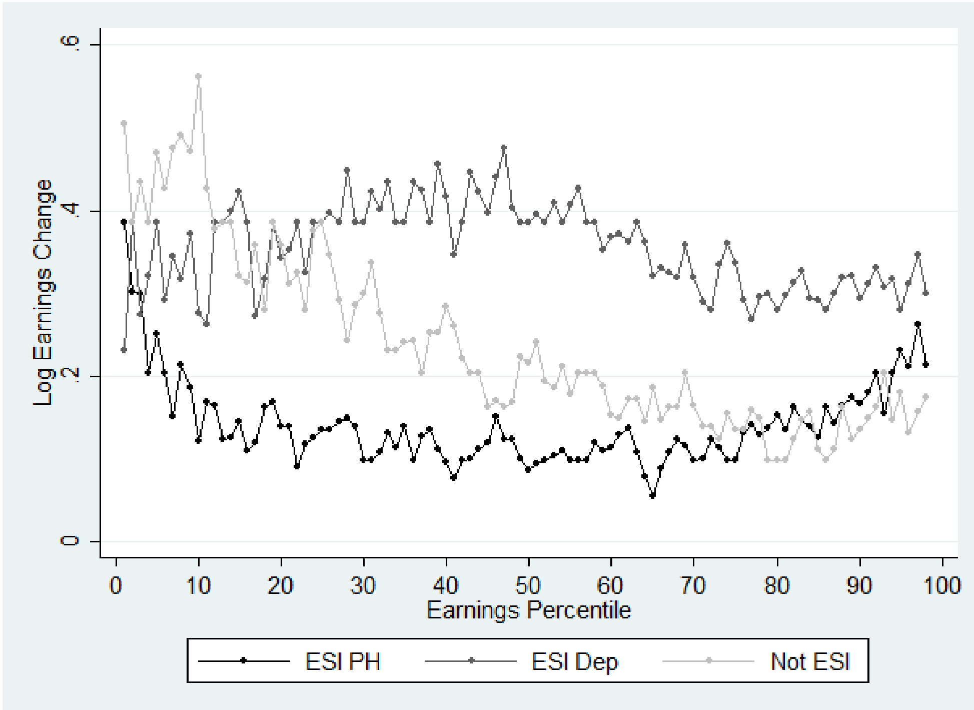
Panel A. FTFY sample.



Panel B. PTPY sample



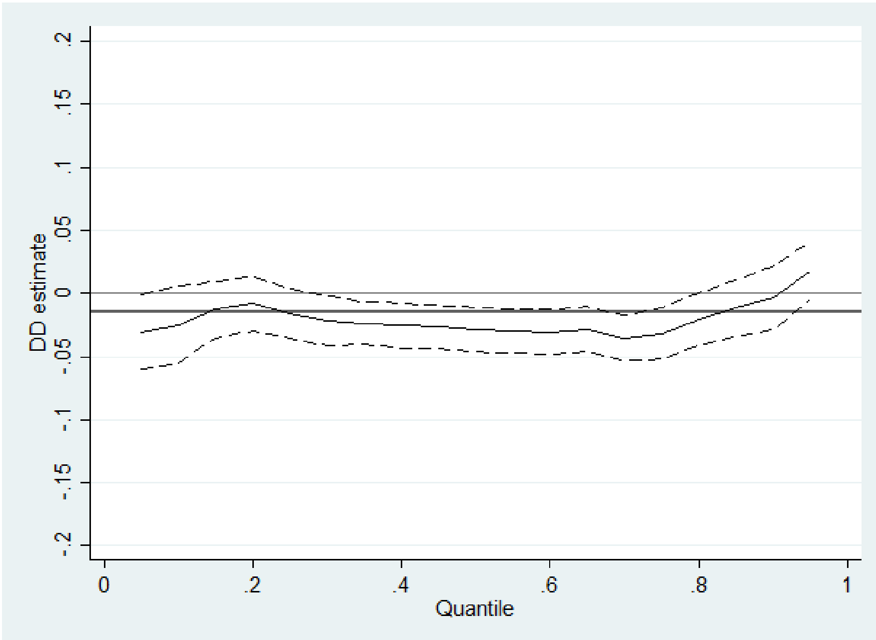
Panel C. FULL sample



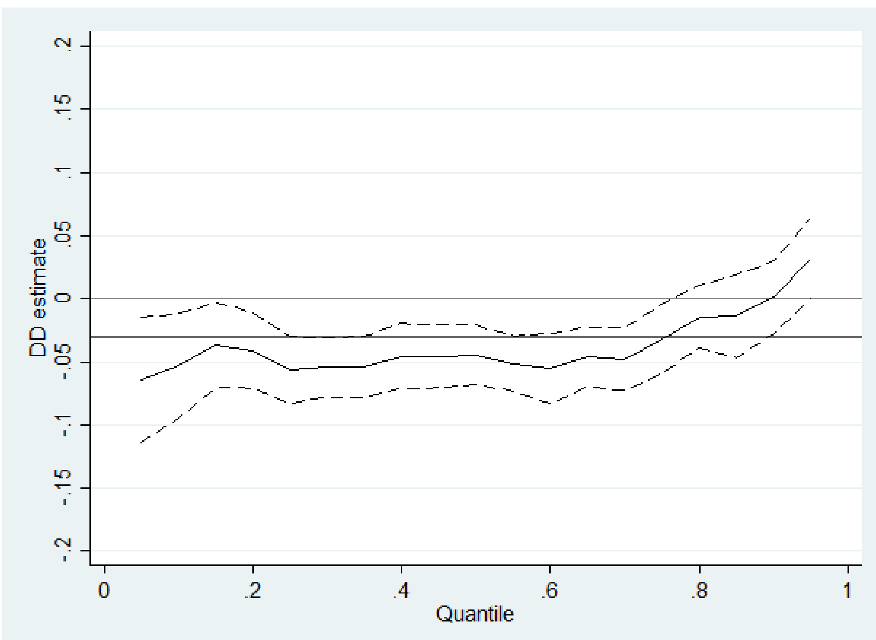
Notes: Data come the Current Population Survey. Earnings are adjusted to 1999 dollars. FTFY = Full-time, full year worker sample. PTPY = Part-time or part-year worker sample. FULL = Full sample. ESI PH = Employer-sponsored insurance policy holder. ESI Dep. = Employer-sponsored insurance dependent. Full-time work is defined by working 35 or more hours per week. Full-year workers work at least 40 weeks in the prior year. This figure presents changes in log annual earnings at each earnings percentile across 1995 and 2007. Earnings changes are grouped by health insurance type for the FTFY, PTPY, and FULL samples.

Figure 4.6. DD quantile regression estimates using binary ESI, 1995 and 2007, preferred FTFY models

Panel A. FTFY sample

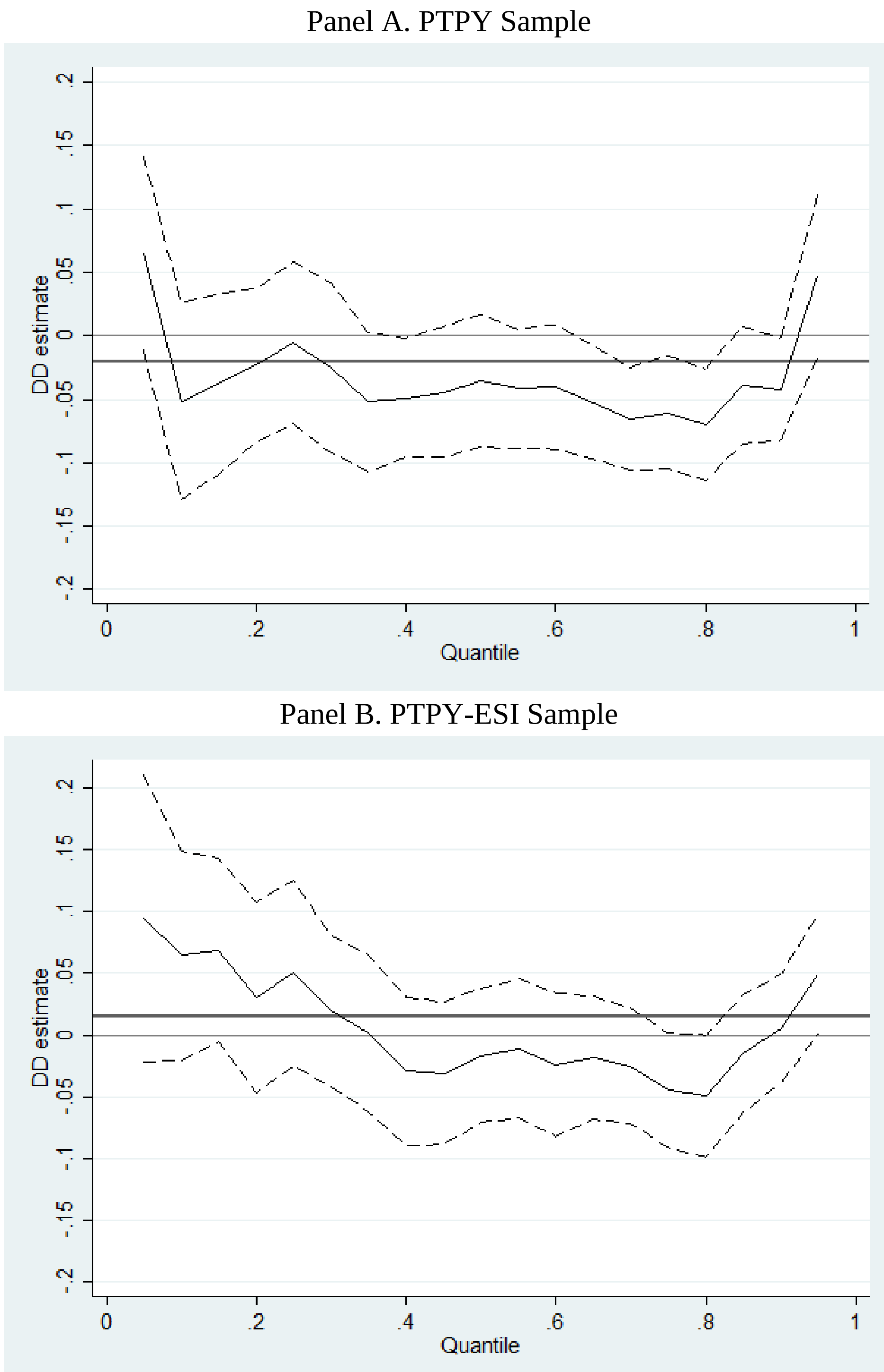


Panel B. FTFY-ESI sample



Notes: Data come the Current Population Survey. Earnings are adjusted to 1999 dollars. FTFY = full-time, full year worker sample. FTFY-ESI = full-time, full year workers covered by employer-sponsored insurance. This figure plots quantile regression coefficients at .05 quantile increments. Dashed lines indicate 95% confidence intervals and the thick grey line represents the OLS DD coefficients.

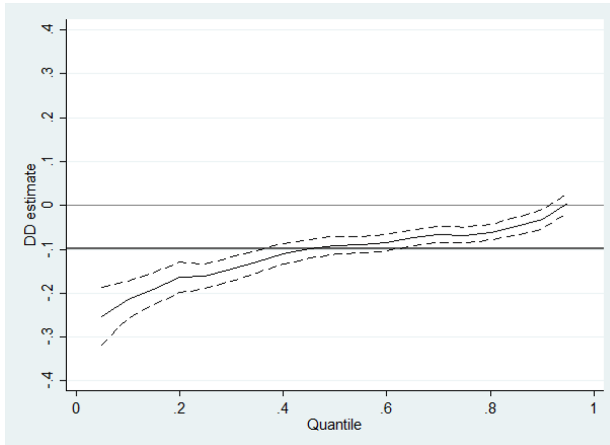
Figure 4.7. DD quantile regression estimates using binary ESI, 1995 and 2007, PTPY sample



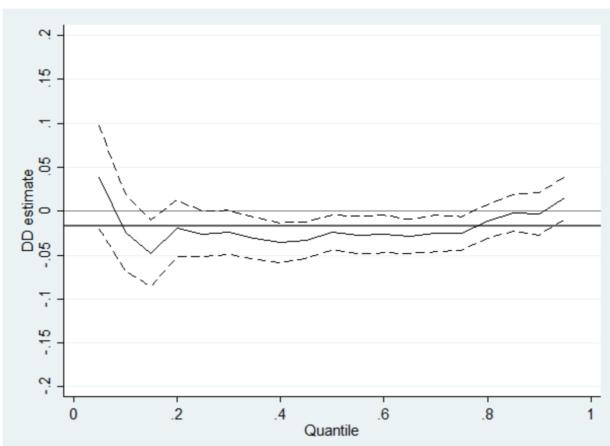
Notes: Data come the Current Population Survey. Earnings are adjusted to 1999 dollars. PT/PT = part-time or part year worker sample. PTPY-ESI = part-time or part year workers covered by employer-sponsored insurance. This figure plots quantile regression coefficients at .05 quantile increments. Dashed lines indicate 95% confidence intervals and the thick grey line represents the OLS DD coefficients.

Figure 4.8. DD quantile regression estimates using binary ESI, 1995 and 2007, FULL and quasi-experimental weighted samples

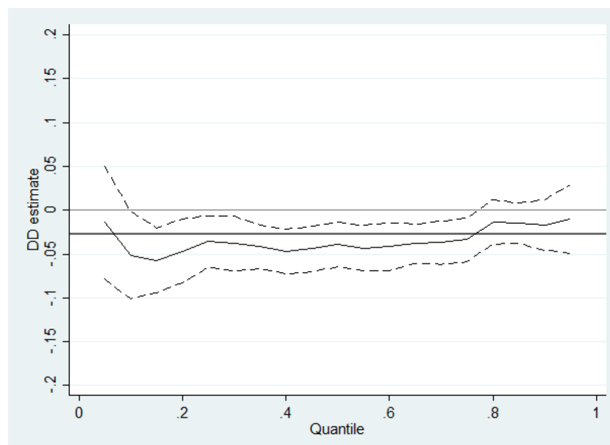
Panel A. FULL sample



Panel B. FULL sample with IPW



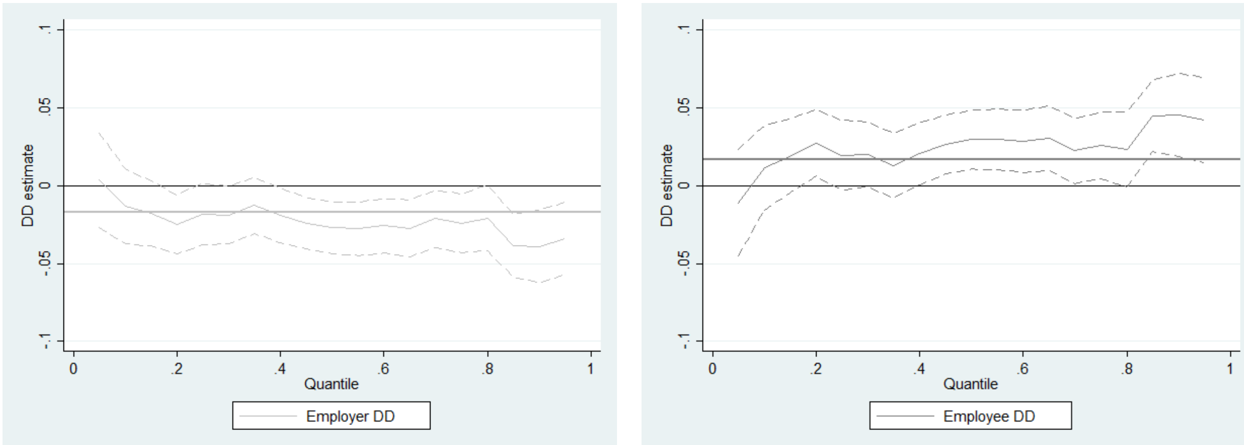
Panel C. FULL sample with EB weighting



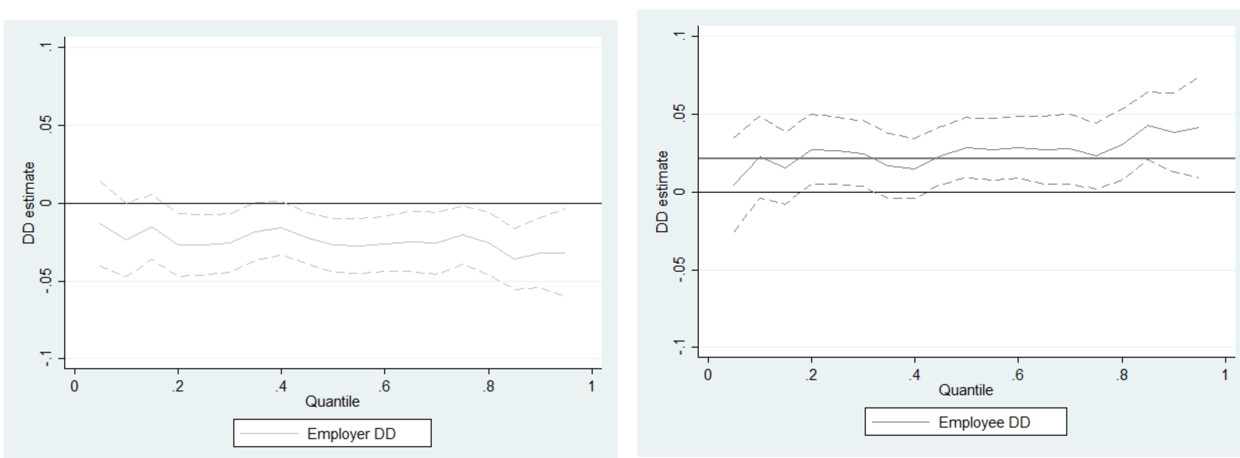
Notes: Data come the Current Population Survey. Earnings are adjusted to 1999 dollars. FULL = Full sample. IPW = inverse propensity weighting, EB = entropy balancing. This figure plots quantile regression coefficients at .05 quantile increments. Dashed lines indicate 95% confidence intervals and the thick grey line represents the OLS DD coefficients.

Figure 4.9. DD quantile regression estimates using employer and employee premiums, 1995 and 2007, preferred models

Panel A. FTFY sample



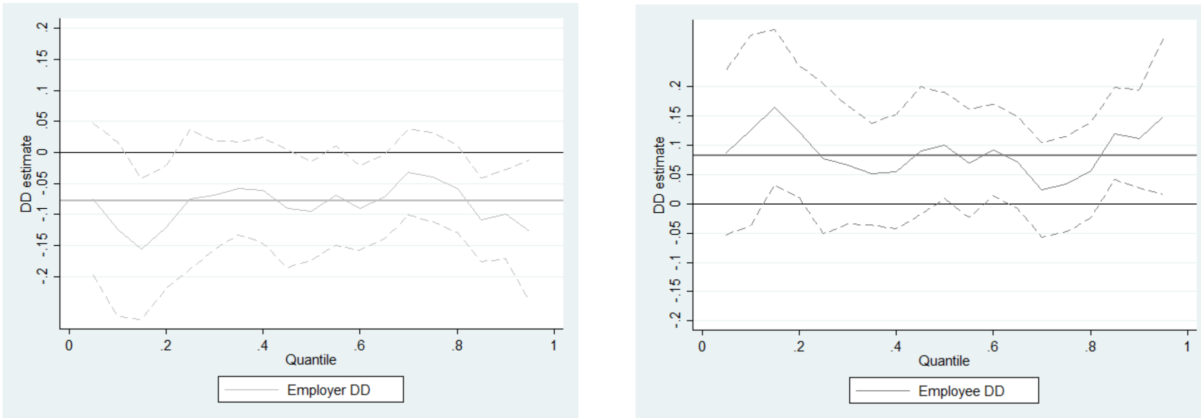
Panel B. FTFY-ESI sample



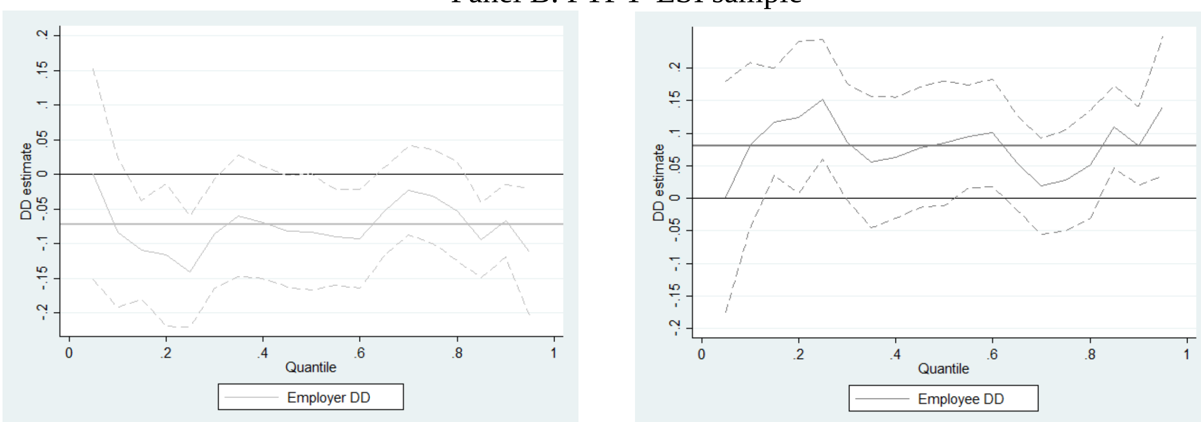
Notes: Data come the Current Population Survey. Earnings are adjusted to 1999 dollars. FTFY = full-time, full year worker sample. FTFY-ESI = full-time, full year workers covered by employer-sponsored insurance. This figure plots quantile regression coefficients at .05 quantile increments. Dashed lines indicate 95% confidence intervals and the thick grey line represents the OLS DD coefficients.

Figure 4.10. DD quantile regression estimates using employer and employee premiums, 1995 and 2007, non-preferred models

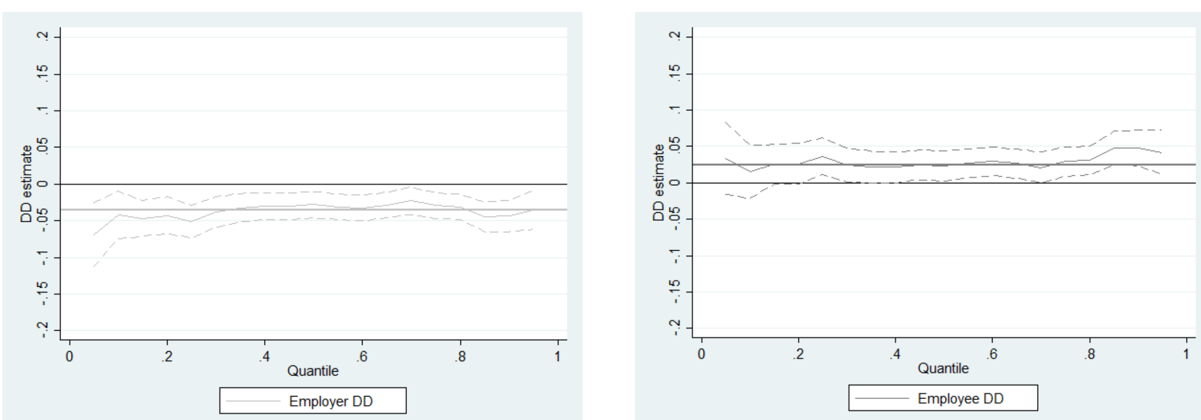
Panel A. PTPY sample



Panel B. PTPY-ESI sample



Panel C. FULL sample



Notes: Data come the Current Population Survey. Earnings are adjusted to 1999 dollars. PT/PT = part-time or part year worker sample. PTPY-ESI = part-time or part year workers covered by employer-sponsored insurance. FULL = Full sample. This figure plots quantile regression coefficients at .05 quantile increments. Dashed lines indicate 95% confidence intervals and the thick grey line represents the OLS DD coefficients.

CHAPTER 5: CONCLUSION

The ACA represents an unprecedented change in the US health system and has greatly increased the insured rate to over 90% (French et al. 2016). Using novel and rigorous empirical strategies, the second and third chapters of my dissertation provide a valuable contribution on the benefits of premium tax credits and cost-sharing subsidies. In Massachusetts, I found a large response on the margin for the tax credits. For the ACA, I document robust, positive effects on private coverage at the lowest eligibility threshold and weak evidence of effects at higher thresholds. Separating these effects from other important ACA policies, such as Medicaid expansion or the individual mandate, is vital to future efforts to modify and sustain the progress made by the ACA.

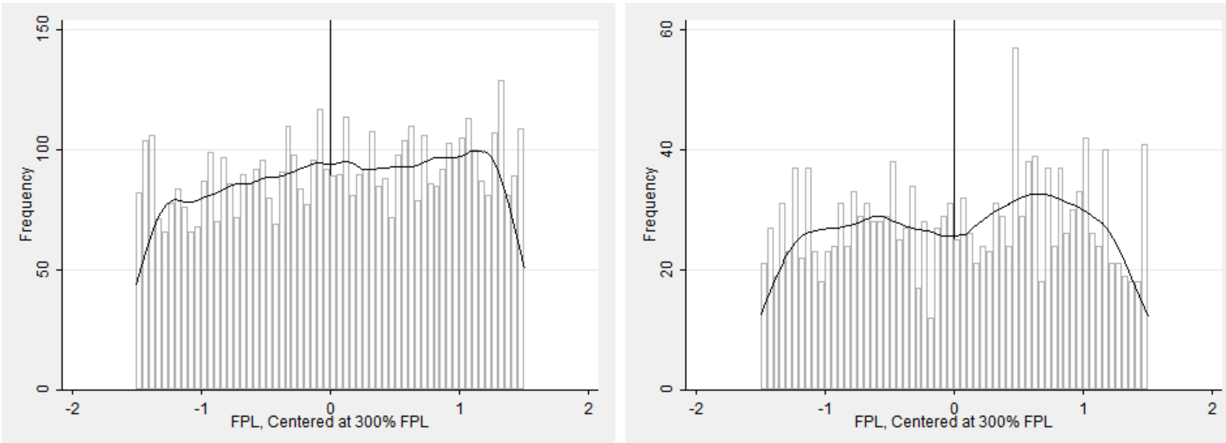
The fourth chapter addresses a significant gap in the literature, examining how employer-sponsored health insurance (ESI) affects the earnings distribution. I examine the role of sample selection and selection bias as an explanation for the inconsistent findings in the literature and show that the inclusion of part-time or part-year workers leads to estimates that vastly overstate the earnings penalty. I also provide evidence consistent with cost-shifting from employers to employees. The use of quantile regression shows that cost-shifting due to compensating wage differentials occurs, but is also offset for higher earnings due to higher marginal tax rates. Together, my dissertation indicates that reducing reliance on ESI may have beneficial effects on earnings for low- and middle-income individuals and that health insurance tax credits provide an appealing, alternative coverage option.

Going forward, I am building on my dissertation in at least two ways. First, the ACA chapter focuses only on 2014 outcomes. I am examining planned changes beyond 2014, such as increases in the individual mandate penalty, and unplanned changes, such as insurer dynamics on the exchanges. Second, building on the fourth chapter, I plan to investigate the effects of the ACA on the earnings distribution and others measures of income that account for the tax implications of ESI.

APPENDIX 1: SUPPORTING MATERIAL FOR CHAPTER 2

Appendix Figure 1. Density estimates around the 300% FPL cutoff, Massachusetts

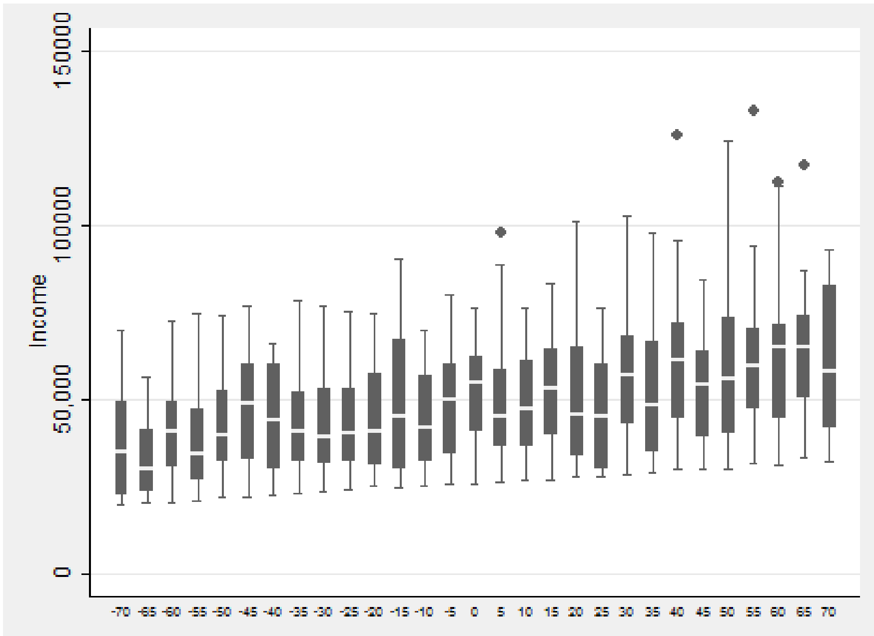
1999–2006 **2007–2009**



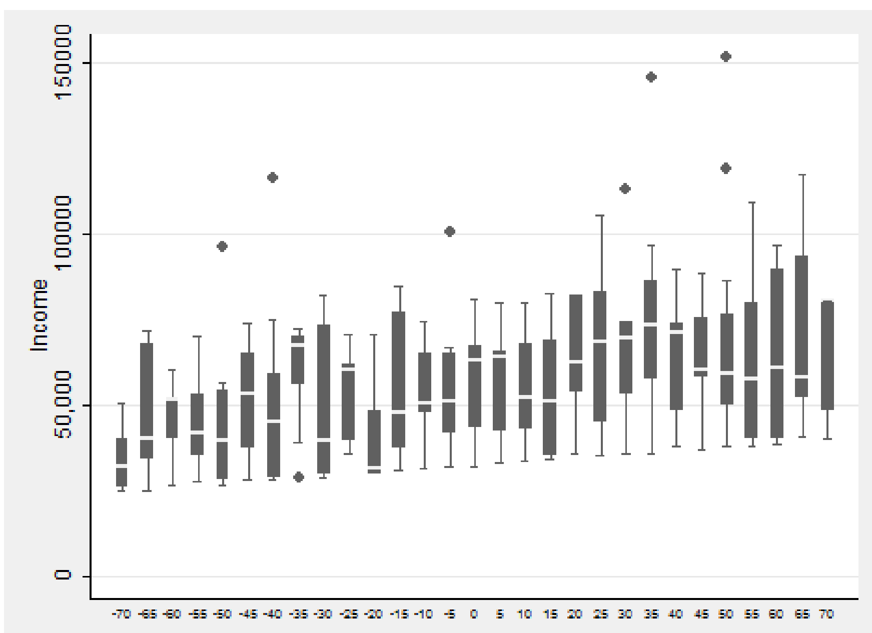
Notes: FPL = federal poverty level. FPL is centered at 300%. The presented range is 150% FPL to 450% FPL. The histogram bins have a width of 5% FPL. An Epanechnikov kernel density is overlaid on each diagram. There is no visual evidence of bunching or income manipulation near 300% FPL. There are several spikes across the distribution in the pre- and post-reform periods.

Appendix Figure 2. Income distribution within 5% FPL bins, pre- and post-reform

Pre



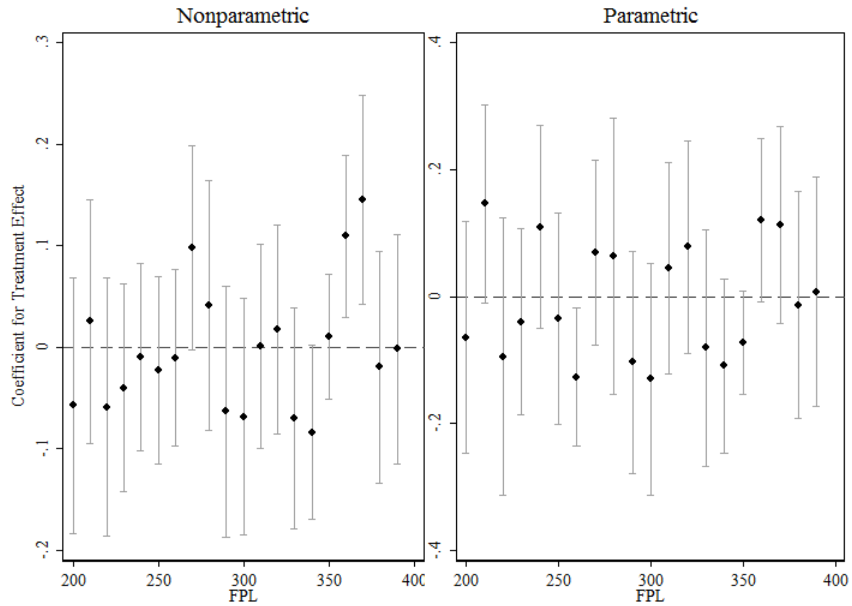
Post



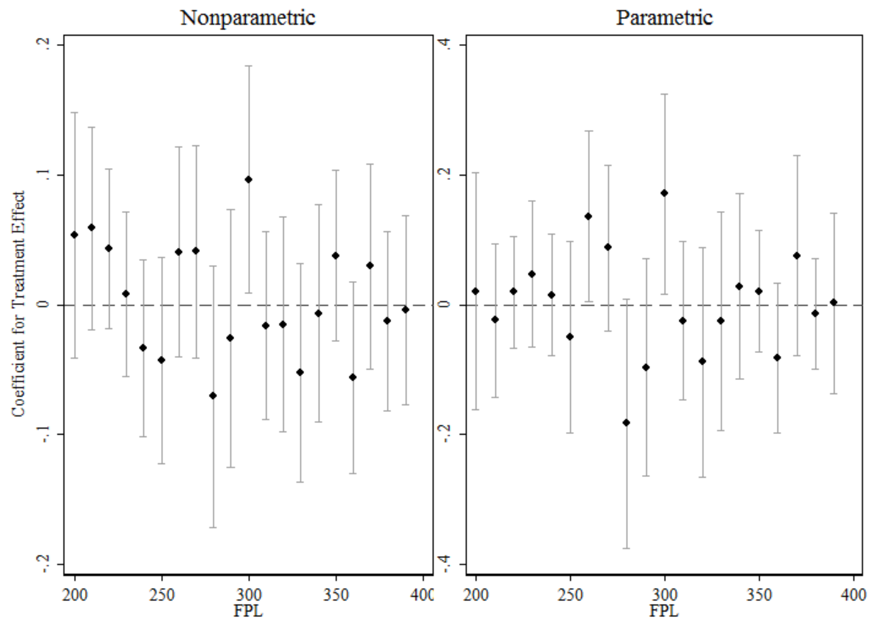
Notes: FPL = federal poverty level. This figure presents box plots of the income distribution within 5% FPL bins. The average income fluctuates across 5% bins due to the change in the poverty cutoff. In the pre-period below 300%, the average income rises to $50,000 and then falls several times, signifying an additional family member increase the poverty cutoff.

Appendix Figure 3. Permutation tests for the post-period

Any HI

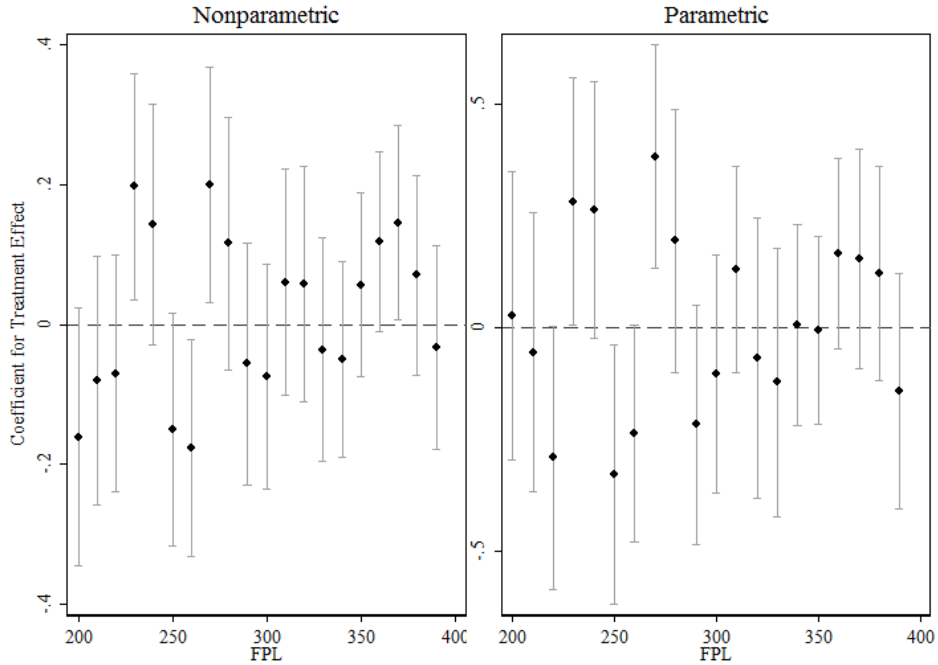


IPI

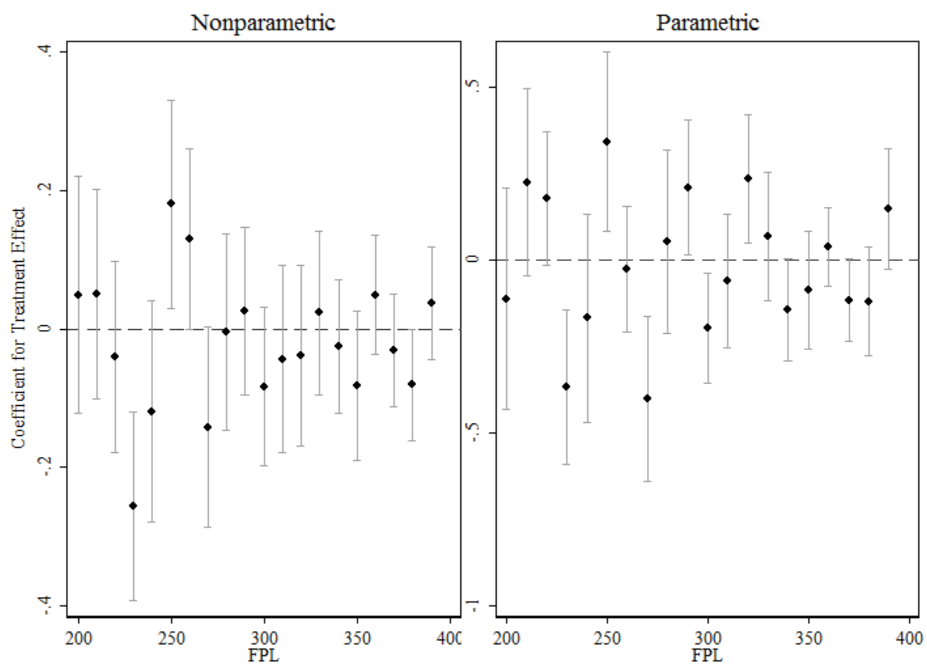


(continued)

ESI



PHI



Notes: ESI = employer-sponsored insurance; HI = health insurance; IPI = individually purchased insurance; PHI = public health insurance. Points represent the coefficient estimate for the treatment effect using different FPL cutoffs. Vertical bars are 95% confidence intervals. The largest effect size should occur near the 300% FPL cutoff. This is only the case of the IPI panel. The PHI panel is quite noisy, showing large effects elsewhere.

APPENDIX 2: SUPPORTING MATERIAL FOR CHAPTER 3

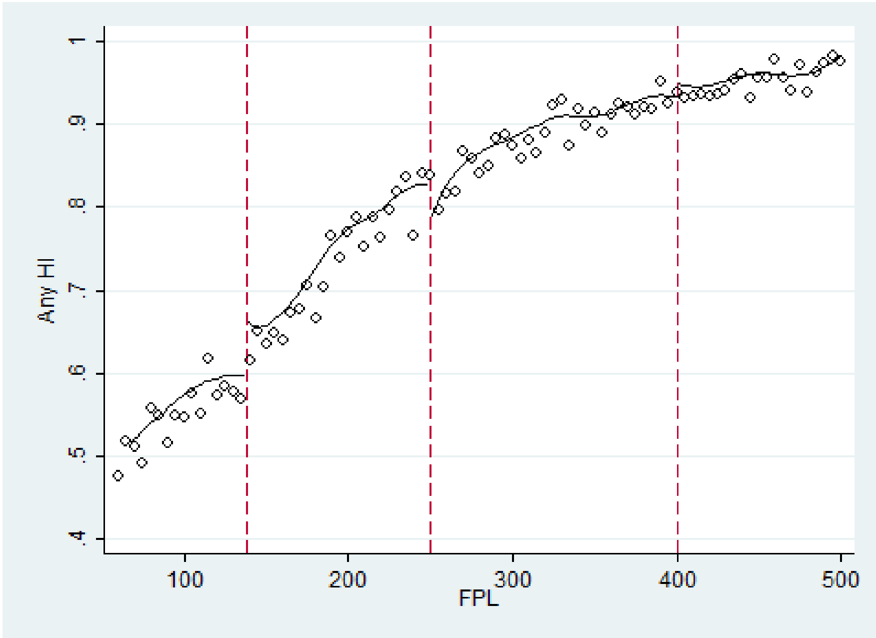
Appendix Table 1. Regression discontinuity estimates at 138% FPL/100% FPL, 250% FPL, and 400% FPL for HI outcomes, 2010–2012

	Expansion States					Non-Expansion States			
138% FPL N=30,603	**Any HI**	**IPI**	**ESI**	**PHI**	**100% FPL** N=20,877	**Any HI**	**IPI**	**ESI**	**PHI**
Non-parametric	0.024*	0.005	0.010	0.008	**Non-parametric**	-0.001	-0.010	-0.005	0.014
	(0.013)	(0.006)	(0.014)	(0.008)		(0.016)	(0.007)	(0.015)	(0.010)
Linear	0.022	0.005	0.007	0.010	**Linear**	0.007	-0.009	0.002	0.014
	(0.015)	(0.007)	(0.015)	(0.010)		(0.018)	(0.008)	(0.017)	(0.011)
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	-0.002
	(0.012)	(0.007)	(0.013)	(0.005)		(0.014)	(0.008)	(0.016)	(0.007)
Linear	0.014	0.006	0.012	-0.005	**Linear**	0.018	0.016*	0.006	-0.003
	(0.014)	(0.008)	(0.015)	(0.006)		(0.016)	(0.009)	(0.018)	(0.008)
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	0.006
	(0.009)	(0.007)	(0.011)	(0.005)		(0.011)	(0.008)	(0.014)	(0.006)
Linear	0.008	-0.006	0.015	-0.001	**Linear**	-0.001	-0.011	0.005	0.005
	(0.010)	(0.008)	(0.013)	(0.006)		(0.012)	(0.010)	(0.017)	(0.007)

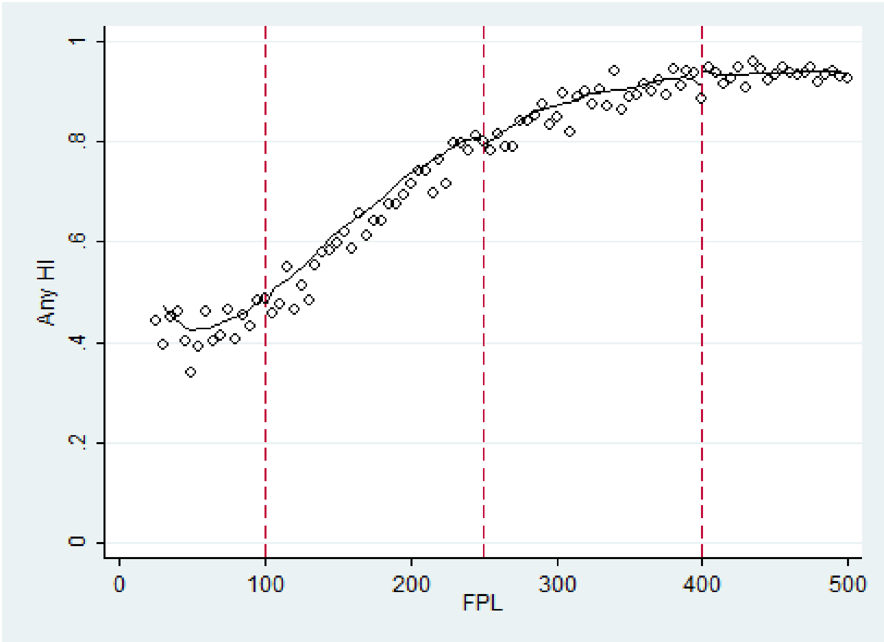
Notes: * p<0.10, **p<0.05, ***p<0.01. Data come from the IPUMS-CPS. Twenty-eight states had expanded their Medicaid program by 2014. IPI = individually purchased private insurance. Observations within 70% on either side of the cutoff are included. Standard errors are in parentheses. Non-parametric RD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

Appendix Figure 4. Any HI coverage by 5% FPL bins in 2010–2012

Expansion States



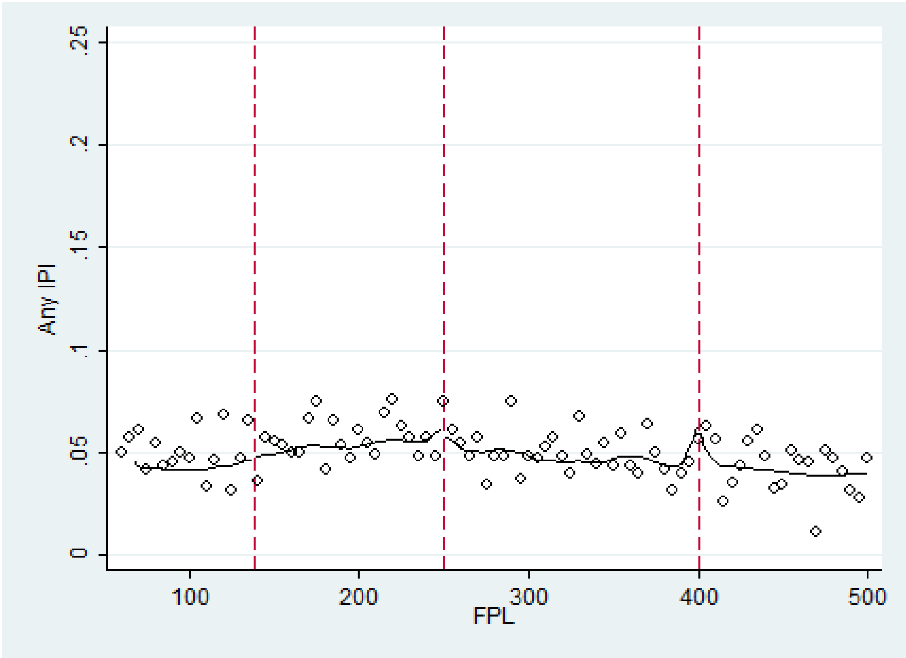
Non-Expansion States



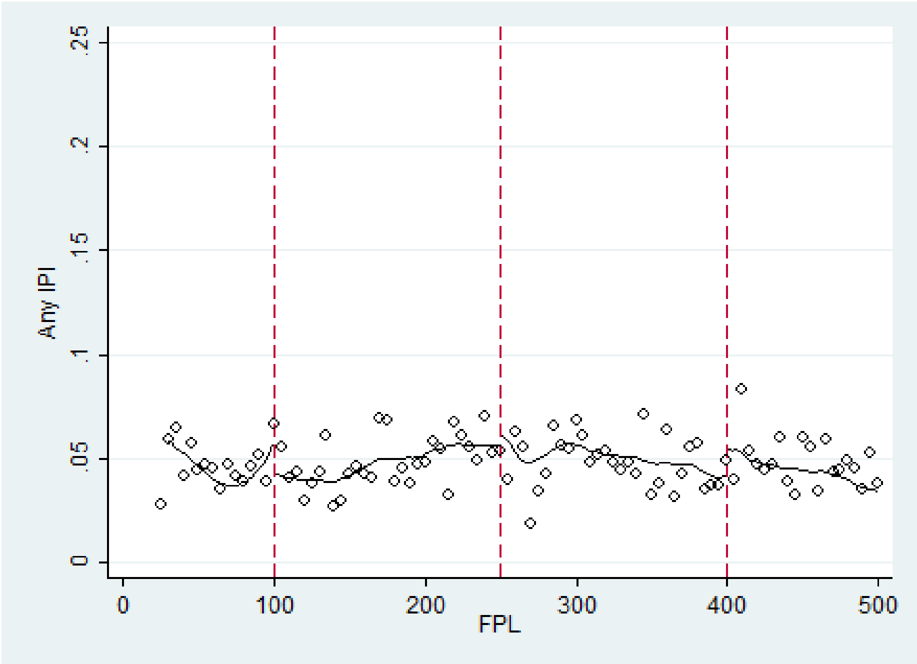
Notes: Data come from the IPUMS-CPS. Symbols represent the proportion covered in a 5% FPL bin. Vertical dashed lines represent the 138%/100%, 250% and 400% FPL cutoffs. Local linear trends are imposed between each cutoff. Estimates are weighted using ASEC supplement probability weights.

Appendix Figure 5. IPI coverage by 5% FPL bins in 2010–2012

Expansion States



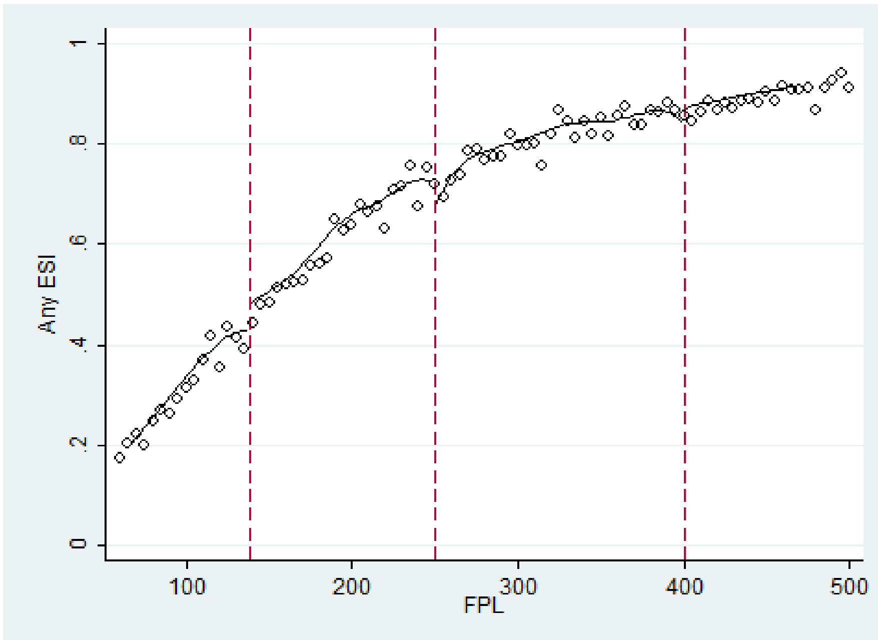
Non-Expansion States



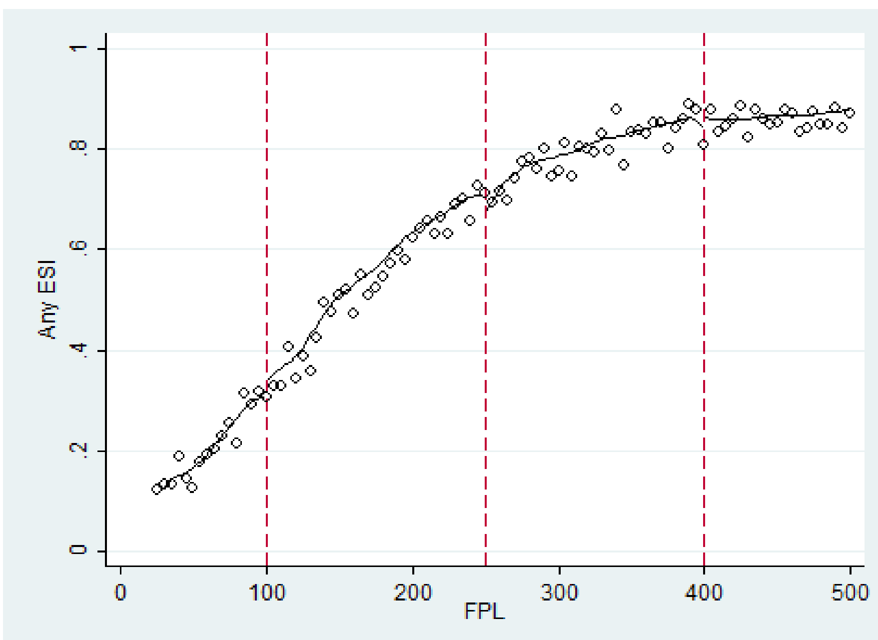
Notes: Data come from the IPUMS-CPS. Symbols represent the proportion covered in a 5% FPL bin. Vertical dashed lines represent the 138%/100%, 250% and 400% FPL cutoffs. Local linear trends are imposed between each cutoff. Estimates are weighted using ASEC supplement probability weights.

Appendix Figure 6. ESI coverage by 5% FPL bins in 2010–2012

Expansion States



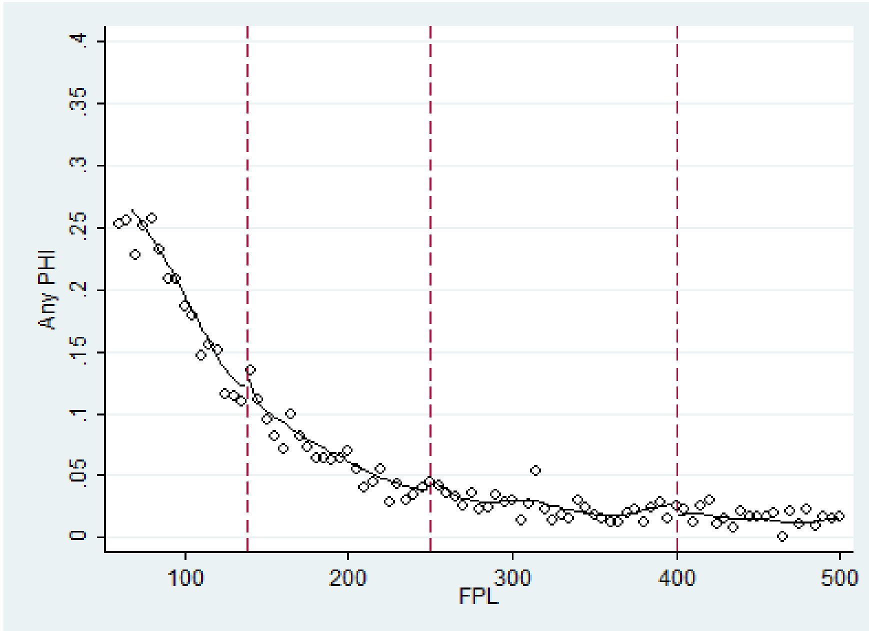
Non-Expansion States



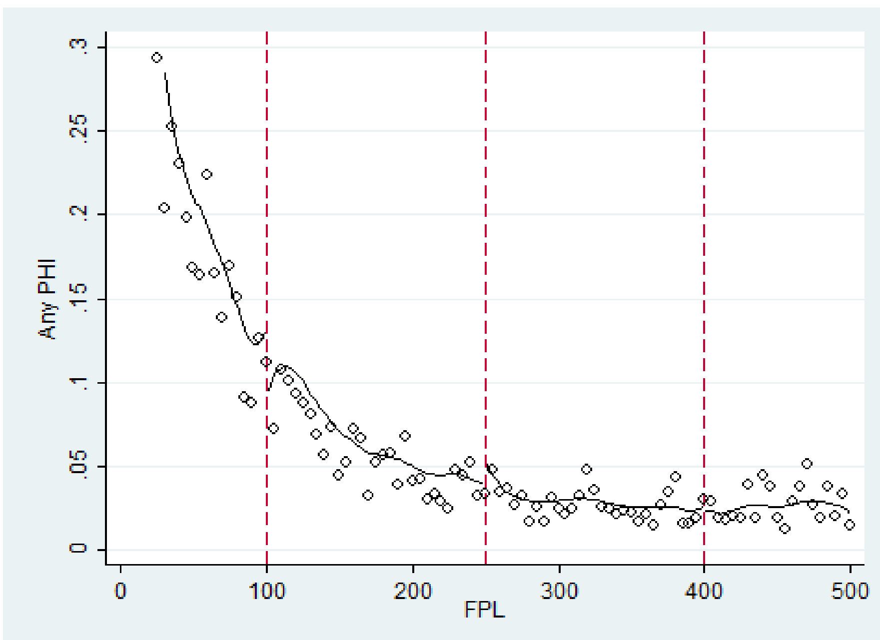
Notes: Data come from the IPUMS-CPS. Symbols represent the proportion covered in a 5% FPL bin. Vertical dashed lines represent the 138%/100%, 250% and 400% FPL cutoffs. Local linear trends are imposed between each cutoff. Estimates are weighted using ASEC supplement probability weights.

Appendix Figure 7. PHI coverage by 5% FPL bins in 2010-2012

Expansion States



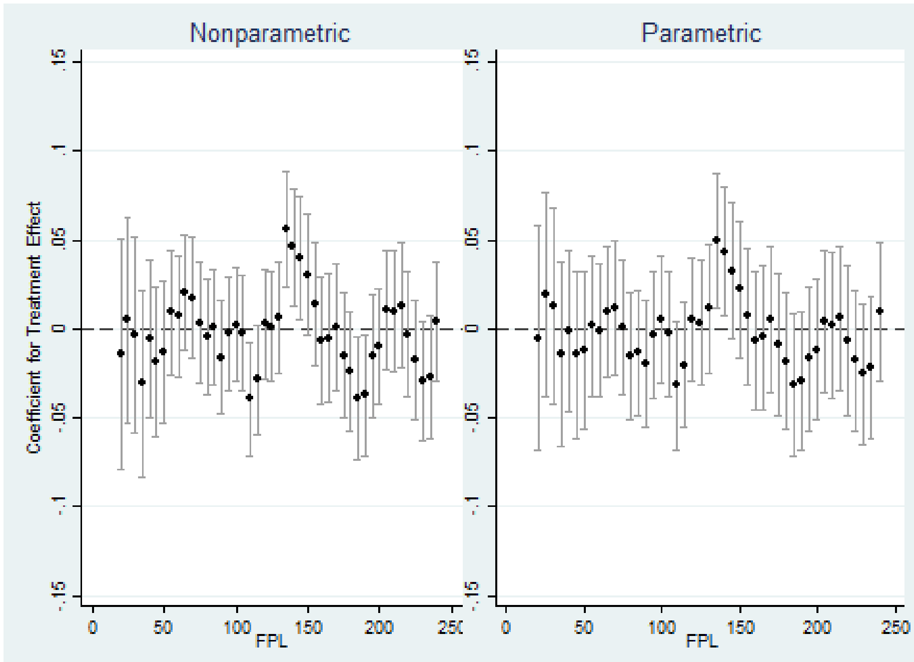
Non-Expansion States



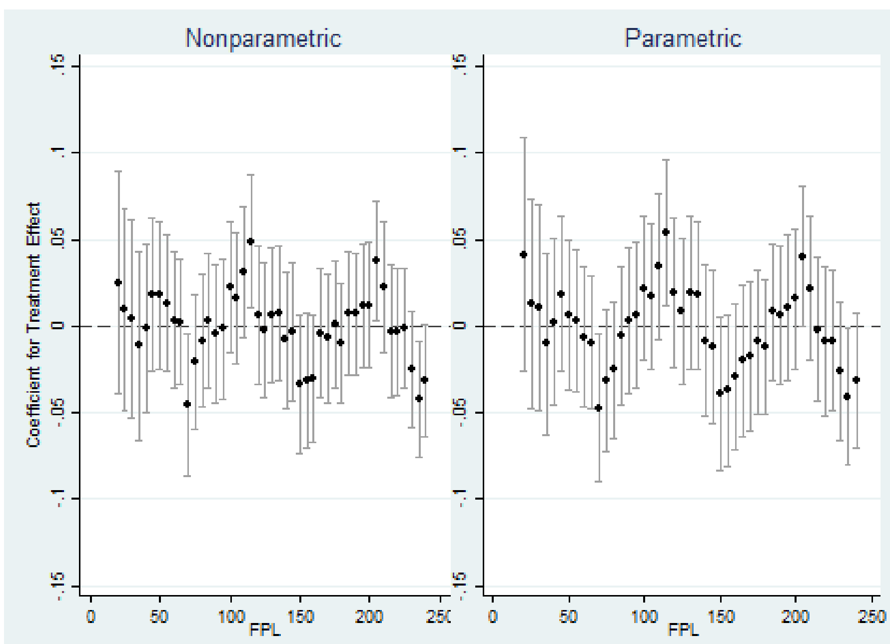
Notes: Data come from the IPUMS-CPS. Symbols represent the proportion covered in a 5% FPL bin. Vertical dashed lines represent the 138%/100%, 250% and 400% FPL cutoffs. Local linear trends are imposed between each cutoff. Estimates are weighted using ASEC supplement probability weights.

Appendix Figure 8. Permutation testing for different FPL cutoffs for the probability of having IPI in 2014, 38%-238% FPL

Expansion States



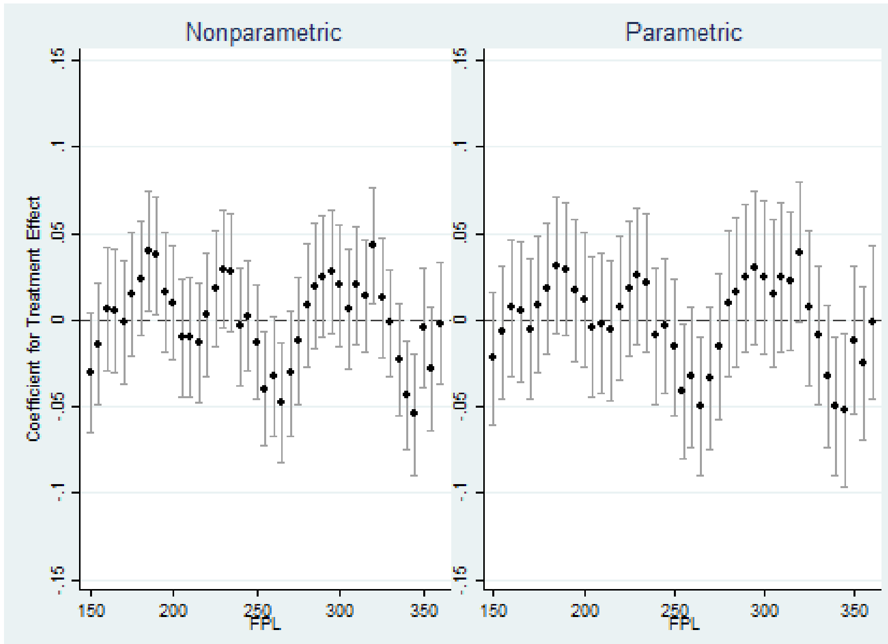
Non-Expansion States



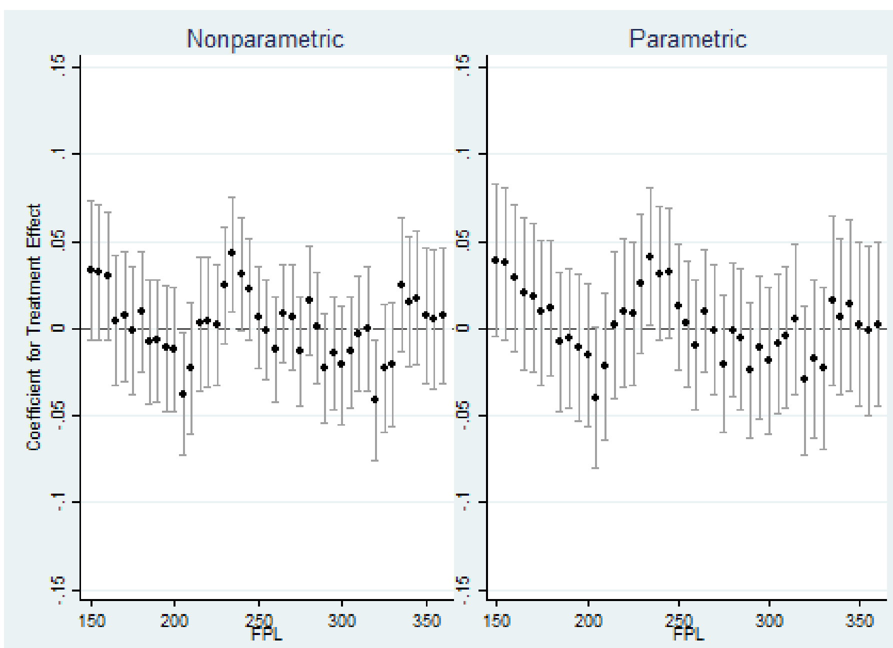
Notes: Points represent the coefficient estimate for the treatment effect using different FPL cutoffs. Vertical bars are 95% confidence intervals

Appendix Figure 9. Permutation testing for different FPL cutoffs for the probability of having IPI in 2014, 150%-350% FPL

Expansion States



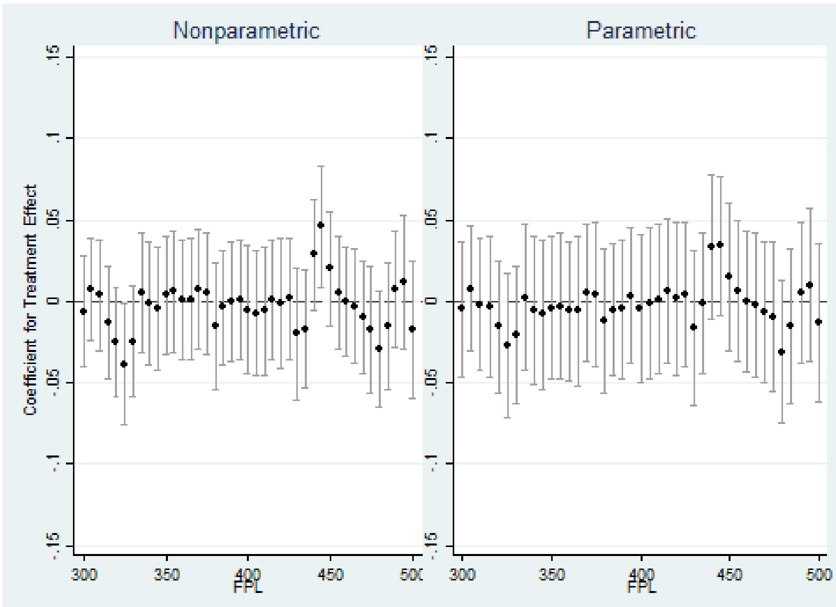
Non-Expansion States



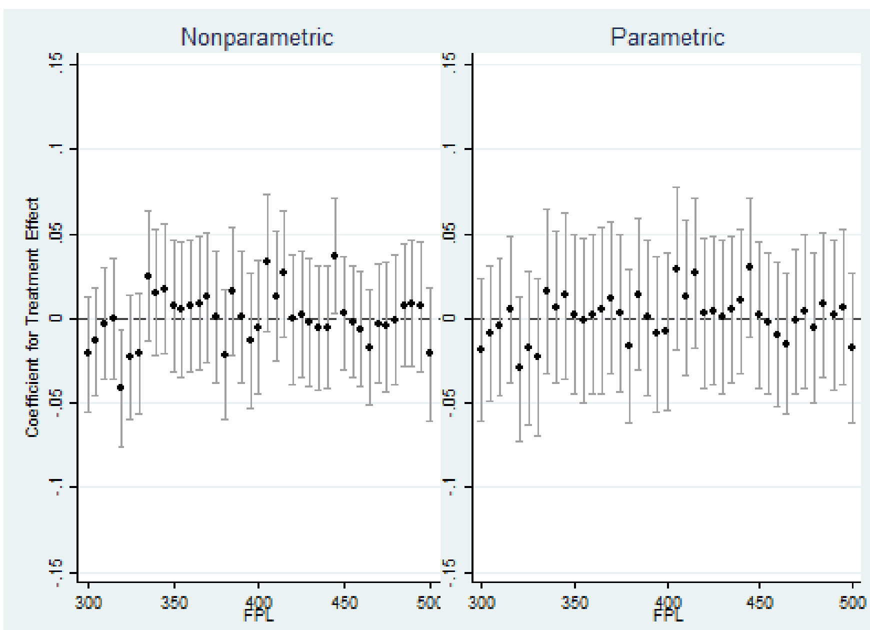
Notes: Points represent the coefficient estimate for the treatment effect using different FPL cutoffs. Vertical bars are 95% confidence intervals

Appendix Figure 10. Permutation testing for different FPL cutoffs for the probability of having IPI in 2014, 300%-500% FPL

Expansion States



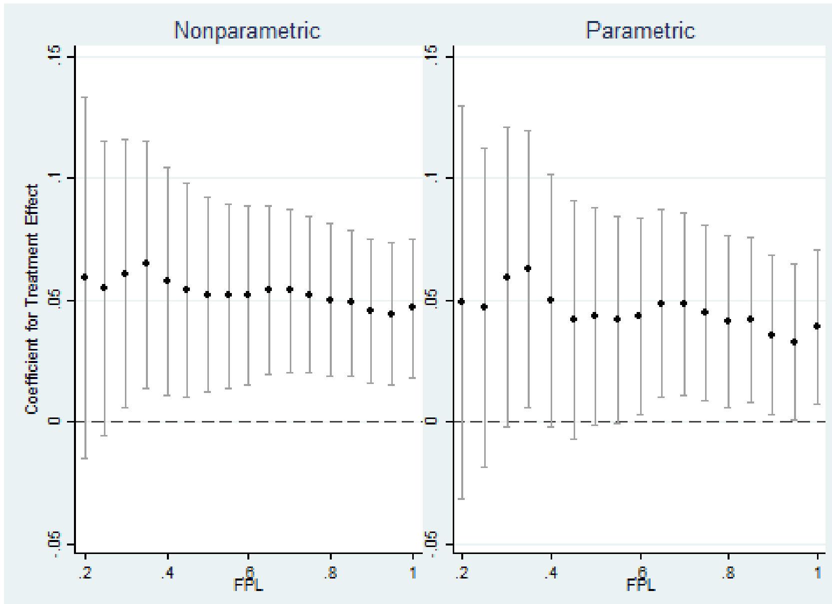
Non-Expansion States



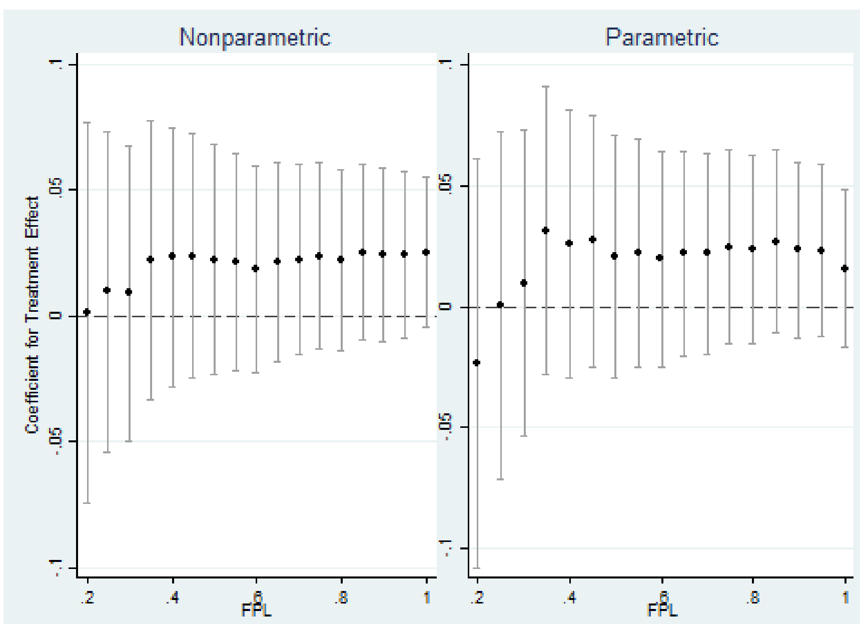
Notes: Points represent the coefficient estimate for the treatment effect using different FPL cutoffs. Vertical bars are 95% confidence intervals

Appendix Figure 11. 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.

APPENDIX 3: SUPPORTING MATERIAL FOR CHAPTER 4

Appendix Table 2. Balance checks using inverse propensity score weighting

	Set #1: Balance to 1995 ESI PHs			Set #2: Balance to ESI PHs in each year			Set #3: Balance to 1995 ESI PHs independently		
	1995 All else	2007 ESI PH	2007 All Else	1995 All else	2007 ESI PH	2007 All Else	1995 All else	2007 ESI PH	2007 All Else
FTFY	0.46	-0.47	0.33	-0.05	-0.13	-0.15	0.01	0.00	0.02
Age	0.46	-0.21	0.28	-0.02	-0.25	-0.23	0.00	0.00	-0.04
Female	-0.12	0.09	-0.08	0.00	0.04	0.05	-0.05	0.00	-0.01
Marital Status									
Currently Married	0.12	-0.05	0.11	-0.03	-0.03	-0.03	-0.05	-0.01	-0.07
Previously Married	0.08	-0.08	0.11	0.02	0.01	0.02	0.04	0.00	0.03
Never Married	-0.19	0.12	-0.20	0.02	0.02	0.02	0.03	0.00	0.06
Household Size	-0.13	0.14	-0.13	0.00	0.01	-0.01	0.00	0.00	-0.03
Race									
White	-0.12	0.01	0.01	0.01	0.16	0.16	0.01	0.00	0.01
Black	0.04	-0.02	0.00	-0.01	-0.07	-0.06	0.01	0.00	0.01
Other/Multiple Race	0.14	0.00	-0.01	0.00	-0.15	-0.16	-0.03	0.00	-0.03
Hispanic	-0.06	0.13	-0.14	0.02	-0.05	-0.02	0.01	0.00	0.00
Switched Jobs	-0.18	0.13	-0.05	0.03	0.13	0.14	-0.02	0.00	-0.03
Union	-0.02	-0.05	0.12	0.00	0.13	0.14	0.01	-0.01	0.01
Years of Potential Experience	0.11	-0.02	0.06	-0.01	-0.05	-0.04	0.01	0.00	-0.03
Education									
Less than High School	-0.28	0.24	-0.19	0.02	0.12	0.12	0.00	0.00	0.00
High School Diploma/GED	-0.08	0.05	-0.04	0.00	0.07	0.08	0.02	0.00	0.02
Some College	-0.07	0.03	-0.05	0.00	0.03	0.03	0.01	0.01	0.02
Associate’s Degree	0.09	-0.05	0.03	0.00	-0.06	-0.06	-0.01	0.00	0.00
Bachelor’s Degree	0.22	-0.12	0.14	-0.02	-0.09	-0.10	-0.02	0.00	-0.02
Graduate Degree	0.25	-0.12	0.16	-0.01	-0.10	-0.11	-0.02	-0.01	-0.03
Industry									
Agriculture, Forestry, Fishing and Hunting, and Mining	-0.10	0.04	-0.01	-0.01	0.06	0.06	0.03	0.00	0.00
Construction	0.00	0.05	-0.07	0.01	-0.05	-0.05	-0.01	0.00	0.01
Manufacturing	0.01	-0.08	0.13	0.01	0.13	0.13	0.05	0.01	0.01

Wholesale Trade	-0.01	-0.01	0.04	0.00	0.05	0.06	0.02	0.00	-0.01
Retail Trade	-0.29	0.17	-0.02	0.02	0.21	0.21	0.04	0.00	-0.02
Transportation and Warehousing, and Utilities	0.08	-0.06	0.04	-0.01	-0.02	-0.01	-0.01	0.00	0.02
Information	0.07	-0.05	0.04	0.00	-0.05	-0.05	0.02	0.00	-0.01
Finance and Insurance, and Real Estate and Rental and Leasing	0.08	-0.05	0.05	-0.01	-0.02	-0.02	-0.01	0.00	-0.02
Professional, Scientific, and Management, and Administrative, and Waste Management Services	0.04	0.00	-0.04	-0.01	-0.10	-0.09	-0.01	0.00	0.01
Educational Services, and Health Care and Social Assistance	0.12	-0.03	0.08	0.00	-0.03	-0.03	-0.06	-0.01	0.01
Arts, Entertainment, and Recreation, and Accommodation and Food Services	0.10	0.08	-0.27	-0.01	-0.27	-0.27	-0.04	0.00	0.01
Other Services, Except Public Administration	-0.08	0.10	-0.07	0.00	-0.01	-0.01	0.00	0.00	0.00
Public Administration	0.13	-0.11	0.14	-0.01	-0.02	-0.05	-0.01	-0.01	-0.02
Occupation									
Management	0.14	-0.12	0.15	-0.01	0.00	-0.01	0.02	0.01	-0.04
Professional	0.24	-0.12	0.15	-0.02	-0.10	-0.11	-0.03	-0.01	0.00
Services	-0.13	0.15	-0.21	0.02	-0.07	-0.05	0.03	0.00	0.03
Sales	-0.12	0.07	-0.08	0.00	0.04	0.04	-0.02	0.01	0.01
Office and Administrative Support	0.00	0.00	0.02	0.00	0.02	0.02	-0.04	0.00	0.01
Blue Collar	-0.10	0.05	0.00	0.01	0.09	0.10	0.05	0.00	0.01
Firm size									
Less than 10 employees	-0.21	0.28	-0.18	0.07	0.06	0.09	-0.01	-0.01	-0.01
10 to 24 employees	-0.13	0.13	-0.14	0.00	-0.01	0.01	-0.01	0.00	-0.01
25 to 99 employees	-0.04	0.04	-0.04	-0.02	0.00	0.00	0.00	0.00	-0.01
100 to 99 employees	0.08	-0.06	0.09	-0.01	0.01	0.00	0.02	0.00	-0.01
1,000 or more employees	0.22	-0.21	0.19	-0.03	-0.04	-0.06	-0.01	0.00	0.03
Self-Reported Health Status									
Excellent	-0.06	-0.01	-0.01	-0.01	0.07	0.06	-0.03	0.01	0.00
Very Good	0.06	-0.03	0.04	0.00	-0.03	-0.03	0.02	0.00	0.00
Good	0.00	0.04	-0.02	0.01	-0.02	-0.02	0.02	0.00	0.00
Fair	-0.01	0.02	-0.02	-0.01	-0.04	-0.03	0.00	-0.01	-0.01
Poor	0.00	0.02	-0.02	-0.01	-0.02	-0.02	0.00	0.00	0.00

Notes: Data come the Current Population Survey. Earnings are adjusted to 1999 dollars. The full sample includes 126,985 observations. FTFY = Full-time, full year worker sample. PTPY = Part-time or part-year worker sample. FULL = Full sample. ESI PH = Employer-sponsored insurance policy holder. ESI Dep. = Employer-sponsored insurance dependent. Full-time work is defined by working 35 or more hours per week. Full-year workers work at least 40 weeks in the prior year.

Appendix Table 3. Balance checks using entropy balance weighting

	Set #1: Balance to 1995 ESI PHs			Set #2: Balance to ESI PHs in each year			Set #3: Balance to 1995 ESI PHs independently		
	1995 All else	**2007 ESI PH**	**2007 All Else**	**1995 All else**	**2007 ESI PH**	**2007 All Else**	**1995 All else**	**2007 ESI PH**	**2007 All Else**
FTFY	0.00	-0.42	-0.08	0.00	-0.08	-0.08	0.00	0.00	0.00
Age	0.00	-0.55	-0.27	0.00	-0.23	-0.23	0.00	0.00	0.00
Female	0.00	0.28	0.11	0.00	0.03	0.03	0.00	0.00	0.00
Marital Status									
Currently Married	0.00	0.11	-0.04	0.00	0.00	0.00	0.00	0.00	0.00
Previously Married	0.00	-0.17	0.03	0.00	0.01	0.01	0.00	0.00	0.00
Never Married	0.00	0.04	0.02	0.00	-0.01	-0.01	0.00	0.00	0.00
Household Size	0.00	0.22	-0.01	0.00	-0.03	-0.03	0.00	0.00	0.00
Race									
White	0.00	0.09	0.11	0.00	0.17	0.17	0.00	0.00	0.00
Black	0.00	-0.01	-0.03	0.00	-0.07	-0.07	0.00	0.00	0.00
Other/Multiple Race	0.00	-0.11	-0.13	0.00	-0.17	-0.17	0.00	0.00	0.00
Hispanic	0.00	0.16	-0.06	0.00	-0.03	-0.03	0.00	0.00	0.00
Switched Jobs	0.00	0.24	0.10	0.00	0.09	0.09	0.00	0.00	0.00
Union	0.00	-0.02	0.15	0.00	0.15	0.15	0.00	0.00	0.00
Years of Potential Experience	0.00	-0.10	-0.08	0.00	-0.03	-0.03	0.00	0.00	0.00
Education									
Less than High School	0.00	0.30	0.06	0.00	0.12	0.12	0.00	0.00	0.00
High School Diploma/GED	0.00	0.25	0.08	0.00	0.10	0.10	0.00	0.00	0.00
Some College	0.00	0.11	0.06	0.00	0.02	0.02	0.00	0.00	0.00
Associate’s Degree	0.00	-0.05	-0.04	0.00	-0.06	-0.06	0.00	0.00	0.00
Bachelor’s Degree	0.00	-0.20	-0.07	0.00	-0.08	-0.08	0.00	0.00	0.00
Graduate Degree	0.00	-0.30	-0.10	0.00	-0.10	-0.10	0.00	0.00	0.00
Industry									
Agriculture, Forestry, Fishing and Hunting, and Mining	0.00	0.04	0.01	0.00	0.03	0.03	0.00	0.00	0.00
Construction	0.00	0.04	-0.11	0.00	-0.06	-0.06	0.00	0.00	0.00
Manufacturing	0.00	-0.01	0.11	0.00	0.17	0.17	0.00	0.00	0.00
Wholesale Trade	0.00	0.06	0.04	0.00	0.05	0.05	0.00	0.00	0.00
Retail Trade	0.00	0.27	0.14	0.00	0.07	0.07	0.00	0.00	0.00

Transportation and Warehousing, and Utilities	0.00	-0.07	-0.02	0.00	0.00	0.00	0.00	0.00	0.00
Information	0.00	-0.12	-0.06	0.00	-0.05	-0.05	0.00	0.00	0.00
Finance and Insurance, and Real Estate and Rental and Leasing	0.00	-0.03	-0.02	0.00	-0.01	-0.01	0.00	0.00	0.00
Professional, Scientific, and Management, and Administrative, and Waste Management Services	0.00	0.00	-0.05	0.00	-0.09	-0.09	0.00	0.00	0.00
Educational Services, and Health Care and Social Assistance	0.00	-0.01	0.01	0.00	-0.04	-0.04	0.00	0.00	0.00
Arts, Entertainment, and Recreation, and Accommodation and Food Services	0.00	-0.01	-0.20	0.00	-0.14	-0.14	0.00	0.00	0.00
Other Services, Except Public Administration	0.00	0.09	0.00	0.00	-0.03	-0.03	0.00	0.00	0.00
Public Administration	0.00	-0.18	0.02	0.00	-0.02	-0.02	0.00	0.00	0.00
Occupation									
Management	0.00	-0.15	0.00	0.00	0.02	0.02	0.00	0.00	0.00
Professional	0.00	-0.23	-0.05	0.00	-0.09	-0.09	0.00	0.00	0.00
Services	0.00	0.14	-0.04	0.00	-0.06	-0.06	0.00	0.00	0.00
Sales	0.00	0.15	0.04	0.00	0.03	0.03	0.00	0.00	0.00
Office and Administrative Support	0.00	0.12	0.09	0.00	0.01	0.01	0.00	0.00	0.00
Blue Collar	0.00	0.10	-0.02	0.00	0.09	0.09	0.00	0.00	0.00
Firm size									
Less than 10 employees	0.00	0.28	0.01	0.00	0.00	0.00	0.00	0.00	0.00
10 to 24 employees	0.00	0.23	-0.01	0.00	0.00	0.00	0.00	0.00	0.00
25 to 99 employees	0.00	0.19	0.00	0.00	0.02	0.02	0.00	0.00	0.00
100 to 99 employees	0.00	0.06	0.01	0.00	0.02	0.02	0.00	0.00	0.00
1,000 or more employees	0.00	-0.38	-0.01	0.00	-0.03	-0.03	0.00	0.00	0.00
Self-Reported Health Status									
Excellent	0.00	0.07	0.09	0.00	0.08	0.08	0.00	0.00	0.00
Very Good	0.00	-0.10	-0.05	0.00	-0.04	-0.04	0.00	0.00	0.00
Good	0.00	0.02	-0.04	0.00	-0.04	-0.04	0.00	0.00	0.00
Fair	0.00	0.03	-0.01	0.00	-0.02	-0.02	0.00	0.00	0.00
Poor	0.00	0.02	-0.03	0.00	0.00	0.00	0.00	0.00	0.00

Notes: Data come the Current Population Survey. Earnings are adjusted to 1999 dollars. The full sample includes 126,985 observations. FTFY = Full-time, full year worker sample. PTPY = Part-time or part-year worker sample. FULL = Full sample. ESI PH = Employer-sponsored insurance policy holder. ESI Dep. = Employer-sponsored insurance dependent. Full-time work is defined by working 35 or more hours per week. Full-year workers work at least 40 weeks in the prior year.

Appendix Table 4. Earnings dispersion between 1995 and 2012

	FTFY				PTPY				FULL			
Year	GINI	log(p90) - log(p10)	log(p90) - log(p50)	log(p50) - log(p10)	GINI	log(p90) - log(p10)	log(p90) - log(p50)	log(p50) - log(p10)	GINI	log(p90) - log(p10)	log(p90) - log(p50)	log(p50) - log(p10)
1995	0.35	1.56	0.79	0.78	0.52	2.73	1.20	1.53	0.44	2.53	0.92	1.61
1996	0.36	1.54	0.77	0.77	0.52	2.59	1.17	1.42	0.45	2.41	0.89	1.52
1997	0.37	1.61	0.76	0.85	0.52	2.67	1.16	1.51	0.45	2.39	0.89	1.50
1998	0.37	1.55	0.74	0.80	0.50	2.70	1.19	1.51	0.45	2.40	0.87	1.53
1999	0.37	1.61	0.77	0.84	0.51	2.62	1.19	1.43	0.45	2.48	0.92	1.57
2000	0.39	1.61	0.85	0.76	0.52	2.65	1.16	1.49	0.46	2.37	0.91	1.46
2001	0.40	1.60	0.81	0.79	0.53	2.74	1.25	1.49	0.47	2.38	0.92	1.47
2002	0.38	1.61	0.85	0.76	0.52	2.67	1.23	1.45	0.45	2.40	0.93	1.47
2003	0.38	1.61	0.80	0.81	0.52	2.71	1.22	1.49	0.45	2.44	0.94	1.50
2004	0.38	1.65	0.80	0.85	0.51	2.71	1.20	1.50	0.45	2.36	0.92	1.45
2005	0.39	1.64	0.83	0.81	0.52	2.71	1.16	1.55	0.45	2.33	0.91	1.42
2006	0.40	1.66	0.85	0.81	0.52	2.68	1.13	1.55	0.46	2.25	0.92	1.33
2007	0.38	1.61	0.81	0.80	0.51	2.63	1.16	1.47	0.45	2.28	0.96	1.32
2008	0.39	1.65	0.84	0.82	0.52	2.65	1.22	1.43	0.45	2.30	0.95	1.35
2009	0.39	1.62	0.83	0.80	0.51	2.62	1.20	1.43	0.47	2.37	0.96	1.40
2010	0.38	1.65	0.85	0.80	0.51	2.68	1.22	1.46	0.45	2.36	0.94	1.42
2011	0.39	1.71	0.91	0.80	0.50	2.64	1.18	1.46	0.46	2.36	0.98	1.39
2012	0.39	1.71	0.90	0.81	0.51	2.61	1.14	1.47	0.46	2.42	1.00	1.42

Notes: Data come the Current Population Survey. Earnings are adjusted to 1999 dollars. FTFY = Full-time, full year worker sample. PTPY = Part-time or part-year worker sample. FULL = Full sample. Full-time work is defined by working 35 or more hours per week. Full-year workers work at least 40 weeks in the prior year. This table shows the GINI coefficient and the log differences across the 90th, 50th and 10th percentiles for each sample.

Appendix Table 5. FULL sample DD regression estimates with an interaction with FTFY status

	FTFY	PTPY	FULL	FULL
ESI PH	0.219***	0.558***	0.588***	0.569***
	(0.009)	(0.018)	(0.012)	(0.017)
2007	0.111***	0.216***	0.229***	0.205***
	(0.008)	(0.013)	(0.009)	(0.013)
FTFY				1.113***
				(0.012)
ESI PH*2007	-0.015	-0.020	-0.098***	-0.022
	(0.010)	(0.023)	(0.010)	(0.021)
FTFY*ESI PH				-0.345***
				(0.016)
FTFY*2007				-0.090***
				(0.014)
FTFY*ESI_PH*2007				0.007
				(0.023)
N	94,466	32,519	126,985	126,985

Notes: * p<.10, ** p<.05, *** p<.01. Data come the Current Population Survey. Earnings are deflated to 1999 dollars. The full sample includes 126,985 observations. The FTFY though FULL columns are weighted using the ASEC supplemental probability weights. FTFY = Full-time, full year worker sample. PTPY = Part-time or part-year worker sample. FULL = Full sample. ESI PH = Employer-sponsored insurance policy holder. ESI Dep. = Employer-sponsored insurance dependent. Full-time work is defined by working 35 or more hours per week. Full-year workers work at least 40 weeks in the prior year.

Appendix Table 6. Regression estimates using binary ESI, 1995 and 2007 by sample and referent group

Panel A – No Gender Differences

DV: Log Annual Earnings	FTFY	FTFY-ESI	PTPY	PTPY -ESI	FULL
ESI PH	0.209***	0.155***	0.536***	0.508***	0.527***
	(0.007)	(0.003)	(0.009)	(0.011)	(0.009)
N	662,112	536,124	231,630	138,167	893,742

* p<.10, ** p<.05, *** p<.01

Panel B – Gender Differences

DV: Log Annual Earnings	FTFY	FTFY-ESI	PTPY	PTPY -ESI	FULL
ESI PH	0.229***	0.137***	0.557***	0.555***	0.488***
	(0.008)	(0.004)	(0.010)	(0.014)	(0.008)
ESI PH*Female	-0.040***	0.030***	-0.031	-0.065***	0.072***
	(0.004)	(0.005)	(0.019)	(0.020)	(0.007)
Female	-0.221***	-0.284***	-0.127***	-0.101***	-0.362***
	(0.005)	(0.005)	(0.008)	(0.009)	(0.004)
N	662,112	536,124	231,630	138,167	893,742

Notes: * p<.10, ** p<.05, *** p<.01. Data come the Current Population Survey. Earnings are deflated to 1999 dollars. The full sample includes 893,742 observations. Estimates are weighted using the ASEC supplemental probability weights. FTFY = Full-time, full year worker sample. PTPY = Part-time or part-year worker sample. FULL = Full sample. ESI PH = Employer-sponsored insurance policy holder. ESI Dep. = Employer-sponsored insurance dependent. Full-time work is defined by working 35 or more hours per week. Full-year workers work at least 40 weeks in the prior year.

Appendix Table 7. Regression estimates using employer and employee premiums, 1995 and 2007, by sample and referent group

Panel A – No Gender Differences

DV: Log Annual Earnings	FTFY	FTFY-ESI	PTPY	PTPY -ESI	FULL
Log Employer Prem.	0.044***	0.022***	0.053***	0.035***	0.091***
	(0.004)	(0.003)	(0.010)	(0.013)	(0.005)
Log Employee Prem.	-0.023***	-0.005	0.013	0.029*	-0.033***
	(0.004)	(0.003)	(0.012)	(0.015)	(0.006)
N	662,112	536,124	231,630	138,167	893,742

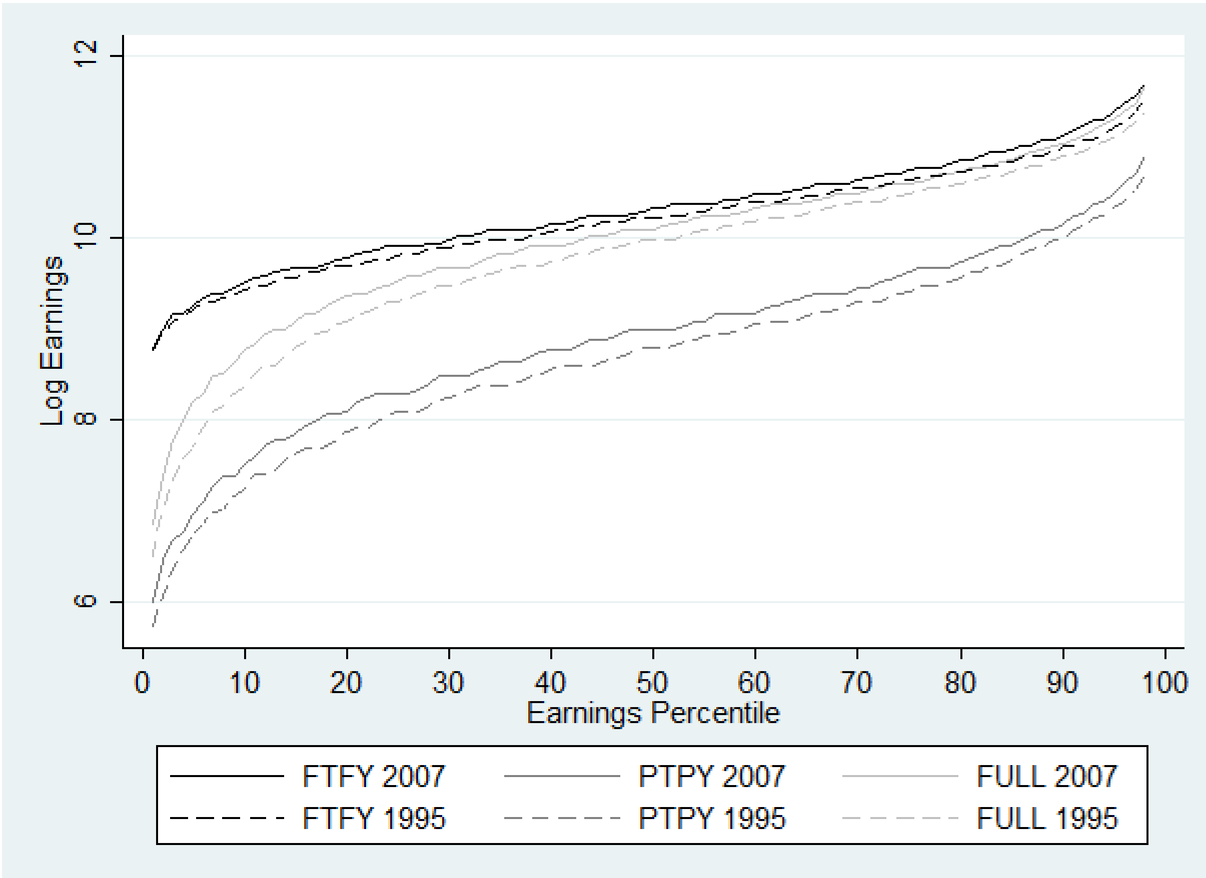
* p<.10, ** p<.05, *** p<.01

Panel B – Gender Differences

DV: Log Annual Earnings	FTFY	FTFY-ESI	PTPY	PTPY -ESI	FULL
Log Employer Prem.	-0.032***	-0.045***	0.073***	0.049**	-0.026***
	(0.004)	(0.005)	(0.021)	(0.023)	(0.009)
Log Employer Prem.*Female	0.056***	0.063***	0.024	0.026	0.083***
	(0.007)	(0.007)	(0.016)	(0.017)	(0.009)
Log Employee Prem.	0.012***	0.015***	0.031*	0.060***	0.013
	(0.004)	(0.004)	(0.017)	(0.019)	(0.009)
Log Employee Prem.*Female	-0.072***	-0.071***	-0.033*	-0.039**	-0.087***
	(0.008)	(0.008)	(0.018)	(0.019)	(0.010)
Female	-0.220***	-0.277***	-0.126***	-0.103***	-0.361***
	(0.005)	(0.005)	(0.008)	(0.009)	(0.004)
N	662,112	536,124	231,630	138,167	893,742

Notes: * p<.10, ** p<.05, *** p<.01. Earnings are adjusted to 1999 dollars. The full sample includes 893,742 observations. Estimates are weighted using the ASEC supplemental probability weights. FTFY = Full-time, full year worker sample. PTPY = Part-time or part-year worker sample. FULL = Full sample. ESI PH = Employer-sponsored insurance policy holder. ESI Dep. = Employer-sponsored insurance dependent. Full-time work is defined by working 35 or more hours per week. Full-year workers work at least 40 weeks in the prior year.

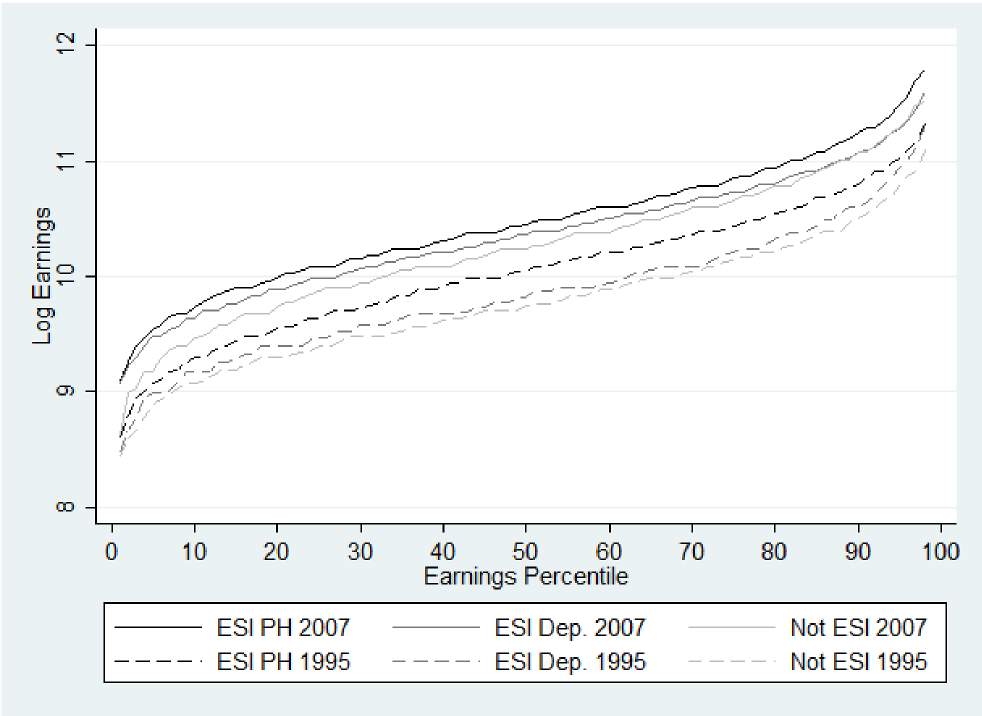
Appendix Figure 12. Log earnings at each percentile by sample, 1995 and 2007



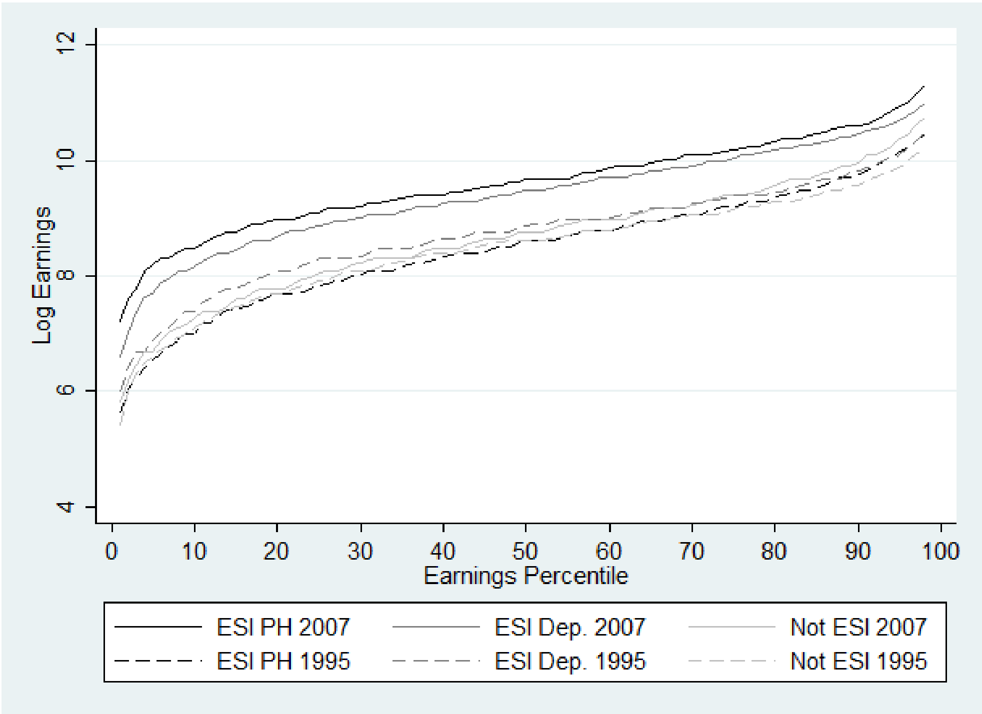
Notes: Data come the Current Population Survey. Earnings are adjusted to 1999 dollars. FTFY = Full-time, full year worker sample. PTPY = Part-time or part-year worker sample. FULL = Full sample. Full-time work is defined by working 35 or more hours per week. Full-year workers work at least 40 weeks in the prior year. This figure presents the log annual earnings at each percentile in 1995 and 2007. The difference between solid lines (2007) and dashed lines (1995) within each group are plotted in Figure 4.4.

Appendix Figure 13. Log earnings at each percentile by sample and referent group, 1995 and 2007

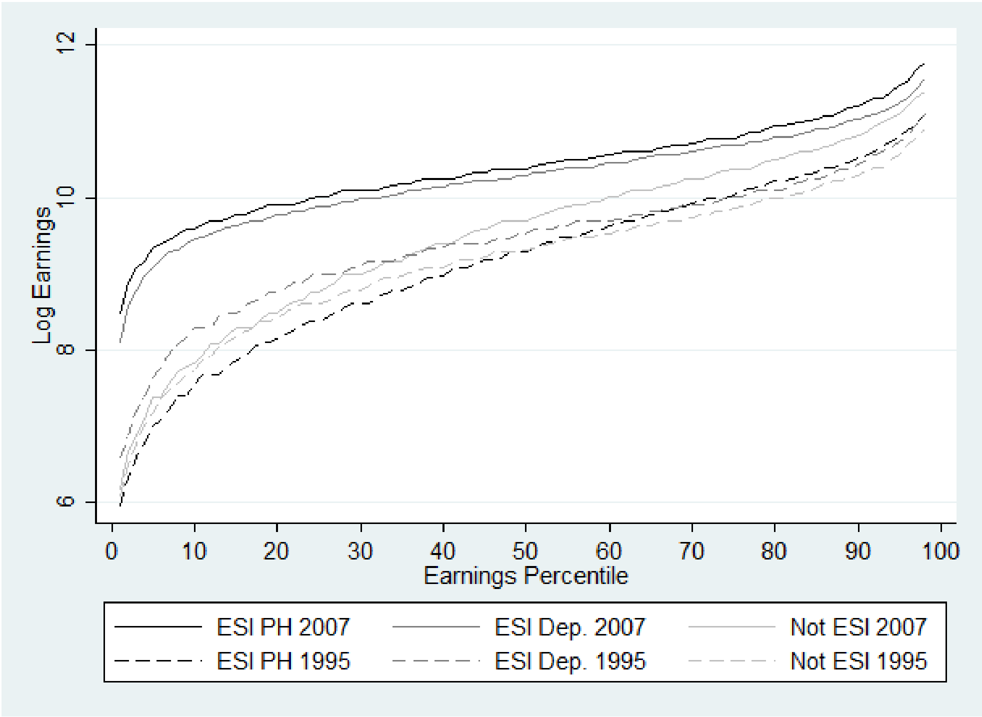
Panel A. FTFY sample.



Panel B. PTPY sample



Panel C. FULL sample



Notes: Data come the Current Population Survey. Earnings are adjusted to 1999 dollars. FTFY = Full-time, full year worker sample. PTPY = Part-time or part-year worker sample. FULL = Full sample. ESI PH = Employer-sponsored insurance policy holder. ESI Dep. = Employer-sponsored insurance dependent. Full-time work is defined by working 35 or more hours per week. Full-year workers work at least 40 weeks in the prior year. This figure presents the log annual earnings at each percentile in 1995 and 2007. The difference between solid lines (2007) and dashed lines (1995) within each group are plotted in Figure 4.5. Earnings are grouped by health insurance type for the FTFY, PTPY, and FULL samples.

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