

Does the individual mandate affect insurance coverage? Evidence from the population of tax returns*

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Abstract

We estimate the effect of the ACA's individual mandate penalty on insurance coverage using Regression Discontinuity and regression kink designs and tax return data for the population of single, childless tax filers. We have four key results. First, the penalty paid per uninsured month is less than half the statutory amount. Second, nonetheless, we find visually clear and statistically significant responses to the individual mandate. Coverage rises both in response to extensive margin exposure to the individual mandate and to marginal increases in the mandate penalty. Third, we find substantial heterogeneity in who responds; young people, men, and people without markers of serious health problems are all especially responsiveness. Fourth, our estimates imply fairly small quantitative responses to the individual mandate, especially in the Health Insurance Exchanges.

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One of the most contentious features of the Affordable Care Act (ACA) is the individual shared responsibility provision, colloquially known as the individual mandate, which requires that Americans obtain insurance coverage or pay a tax penalty. The individual mandate's supporters argue that it is a critical tool for achieving universal health insurance coverage and reducing adverse selection. Opponents of the individual mandate view it as an unreasonable assault on liberty, an encroachment of government authority, and an onerous burden on low-income Americans. The individual mandate was effectively repealed by the Tax Cuts and Jobs Act of 2017, which set the penalty for non-coverage to zero.

Despite the controversy, little is known about the effect of the ACA's mandate on insurance coverage. Understanding the coverage response is critical for at least three reasons. First, it tests whether the mandate works as intended—by raising coverage through a greater penalty. Second, the coverage response is critical for forecasting the effects of repeal on coverage, premiums, and government spending on insurance subsidies. Third, in the absence of a federal mandate, several states are now considering enacting their own mandates.¹ The ACA experience offers a guide for current policy making. We provide quasi-experimental evidence on the effect of the individual mandate penalty on insurance coverage using administrative data.

Our empirical approach exploits two nonlinearities in the mandate penalty function. The first nonlinearity is that people are exempt from the penalty if their income is below 138 percent of the federal poverty line (FPL) and they live in a state that did not expand Medicaid. This creates a discontinuity in the mandate penalty. Second, the penalty amount is a kinked function of income, with a kink for single taxpayers at \$26,550 in 2015 and \$38,150 in 2016. We implement regression discontinuity and regression kink designs, looking for discontinuities and kinks in coverage corresponding to the discontinuities and kinks in the penalty amount.

We use income and insurance data derived from the population of tax returns. We observe exact income, as well as both self-reported and third-party verified insurance

¹See <https://www.wsj.com/articles/states-look-at-establishing-their-own-health-insurance-mandates>.

coverage. These comprehensive administrative data offer a clear advantage over previously used data sources. They are free from the measurement error that plagues survey data, and they record all sources of coverage, unlike administrative data from a single employer or Health Insurance Marketplace. Our discontinuity analysis focuses on a sample of about 2.1 million people filing single-person tax returns in non-expansion states, with income between 110 and 160 percent of FPL in 2015 and 2016. Our kink analysis focuses on a sample of 1.1 million people with income close to the mandate kink point, filing single-person tax returns without signs of offers of employer-sponsored insurance (an imperfect proxy for actual offers).

We begin by documenting a clear first stage. We find large discontinuities in the penalty paid per uninsured month at 138 percent of FPL, and clear kinks at the mandate kink points. However, these discontinuities and kinks are much less than the statutory amounts. For example, for a single adult in 2015, the penalty per month of uninsurance was about \$59, but we estimate a discontinuity of about \$10 when people become subject to the mandate penalty. Although the mandate penalty has bite, it is not as sharp as the statutory amount might indicate.

Given the clear nonlinearities in penalty amount, we next turn to estimating coverage effects. We find visually clear, statistically significant discontinuities in coverage at the 2015 and 2016 eligibility thresholds. When people become subject to the individual mandate, insurance coverage increases by about 1 percentage point in 2015, and by about 2.5 percentage points in 2016. These responses imply a semi-elasticity of about 0.06 with respect to the statutory penalty. Given the relatively low penalty actually paid, however, the semi-elasticity with respect to the observed penalty is about 0.2 in 2015 and 0.3 in 2016, meaning each extra dollar of penalty per uninsured month raises coverage by 0.2-0.3 percent. This estimate is on the low end of the literature, which finds semi elasticities from 0.2 to 1.0. The discontinuity evidence therefore shows a clear coverage response to a discontinuous increase of the mandate penalty from zero to a large, positive amount. On a per-dollar basis we find fairly small responses. Some researchers have suggested that the first-dollar response to the individual mandate may be especially large, because some people may exhibit a “taste for compliance” (Saltzman et al., 2015; Saltzman, 2018). Our

findings suggest that if present, such a taste for compliance is not large.

We further estimate the response to marginal increases in the mandate penalty amount by examining behavior around the mandate kink points. We find a visually clear kink in coverage at the 2015 kink point, but less clear evidence at the 2016 kink point. The estimates at the 2015 kink point (\$26,550) imply a semi-elasticity of about 0.9 with respect to the statutory penalty, and at the 2016 kink point we estimate a statistically insignificant semi-elasticity of 0.35. Thus we find that coverage respond on the margin to a greater mandate penalty, not only to the presence of the penalty.²

An important goal of the individual mandate was not only to raise coverage but especially to induce healthy, low-cost people into coverage, to ease adverse selection concerns in the individual market. We therefore investigate heterogenous responses to the individual mandate. We find that young people (below median age) and men are especially responsive, consistent with the idea that the mandate helps bring “young invincibles” into coverage. On the other hand, people with an indicator of poor health—itemized medical expenditures on prior tax returns—do not respond to the mandate penalty. We also find that people with lower income over the past three years are much more responsive to the mandate penalty. This heterogeneity helps explain the lower sensitivity at the 2016 kink point, which occurs at a higher income level. Overall, the evidence suggests that the individual mandate induces low-spending people to obtain insurance coverage.

However, we find that the mandate does not have a quantitatively large effect on coverage individual market. Specific, we estimate fairly low coverage responses in both the Exchange and Off-Exchange segment. To illustrate this, we conducted simulations based on our estimated responsiveness to the individual mandate penalties. We find that eliminating the individual mandate would reduce exchange coverage by 4-10 percent (at fixed premiums). Thus, although the mandate does bring healthy people into coverage, it does not necessarily bring them into the individual market.

Our estimated discontinuities and kinks coverage reflect the causal effect of a greater mandate penalty under the identification assumption that the counterfactual relationship

²These estimate reflect responses among people without signs of an ESI offer. We find no evidence that a greater mandate penalty increases coverage among people with signs of an ESI offer, nor does it appear to increase ESI offer rates around the mandate kink point.

between income and insurance coverage would be smooth, absent any discontinuity kink in the mandate penalty. This assumption could fail if people manipulate their income to reduce their mandate penalty, or if insurance coverage is inherently kinked or discontinuous. We find no evidence for income manipulation: the income distribution is smooth around the mandate discontinuity and kink points. We also find no evidence for discontinuities or kinks in predetermined predictors of insurance demand.

We believe this is the first quasi-experimental evidence that the ACA's individual mandate increased insurance coverage. Other papers have found coverage effects of the Massachusetts mandate, or looked the ACA but found no coverage effects. Our estimates complement work by Hackmann et al. (2015) and Jaffe and Shepard (2017), who find the Massachusetts mandate increased coverage in the individual insurance market. Our work is also related to research on the coverage responses to premium subsidies (Tebaldi, 2017; Finkelstein et al., 2017; Saltzman, 2018). Our results are not strictly comparable to these past estimates, however, because we estimate overall coverage effects, rather than effects on individual market or Exchange coverage, which past literature has focused on. Our results contrast with Frean et al. (2017), who find the individual mandate has a small, negative coverage effect. A likely reason for this difference is the difference in our identification strategy and data. Frean et al. (2017) estimate triple difference models using survey data with self-reported income, so their estimates pertain to the population as a whole, whereas our estimates are a local average treatment effect. However, measurement error in self-reported income could lead to spurious findings because the mandate penalty is negatively correlated with Medicaid eligibility and PTC coverage, so that the negative response to the mandate could reflect a response to these other provisions of the ACA.

1 The individual mandate

1.1 The individual mandate

The ACA's individual mandate requires that Americans pay a fee for each month they go without health insurance. The goal of the mandate is to raise insurance coverage, particularly among healthy people, thereby limiting adverse selection in the individual health insurance market. Adverse selection was a concern under the ACA because it included regulations sharply limiting insurers' ability to engage in risk-based pricing. Prior state experiments with similar regulations had resulted in serious adverse selection problems (Buchmueller and DiNardo, 2002; Congdon et al., 2008; Herring and Pauly, 2006; Lo Sasso and Lurie, 2009).

The ACA's individual mandate operates as follows. For each month that a taxpayer (or a dependent) is uninsured, an "individual shared responsibility payment" is owed, unless the individual qualifies for an exemption.³ We refer to this payment as the mandate penalty. For each month of uninsurance, the mandate penalty is given by

$$Penalty = 1/12 \max \{ \min \{ [A + .5C]F, 3F \}, S(\text{MAGI} - \text{tax filing threshold}) \}, \quad (1)$$

where A is the number of uninsured adults, C is the number of uninsured children, F is a fixed dollar amount, S is a share of income, and MAGI is modified adjusted gross income.⁴ People with income below the filing threshold are exempt. For simplicity, we refer to modified adjusted gross income as "income." The fixed dollar amount (F) was \$95 in 2014, \$325 in 2015, and \$695 in 2016, and is indexed to inflation thereafter. The share of income (S) was 1% in 2014, 2% in 2015, and 2.5% in 2016 and thereafter. For single filers, the filing threshold was \$10,300 in 2015 and \$10,350 in 2016. The penalty is waived if a person is uninsured for less than three continuous months, and it is capped at the national average price of the cheapest bronze plan available.⁵

³See Lurie and McCubbin (2016) for more detail.

⁴Modified adjusted gross income is adjusted gross income (AGI) plus tax exempt interest and foreign earned income and housing excluded from AGI.

⁵The penalty is calculated in the instructions to Form 8965, and is paid when the taxpayer files their annual tax return.

For a single tax payer with no children, the formula for the penalty simplifies considerably. Figure 1 plots the mandate penalty as a function of income for single tax payers in 2015 and 2016. The penalty is a kinked function of income, with a kink occurring where the fixed penalty equals the dollar share of income, \$26,550 (with a kink of 0.02) in 2015, and \$38,150 (with a kink of 0.025) in 2016.⁶ We plot vertical lines at 200, 250, 300, and 400 percent of FPL because, as we explain in more detail below, there are other relevant policy nonlinearities at these thresholds. The figure shows that the mandate kink point occurs between these other policy kinks. The basic idea in this paper is to look for a corresponding kink in insurance coverage at these income thresholds.

Tax payers may obtain an exemption from the mandate penalty depending on their circumstances. Importantly, taxpayers are exempt from the penalty if their income is below 138 percent of the poverty line and they live in a state that expand Medicaid. That is, if a person should be qualified for Medicaid based on her income, but her state did not expand Medicaid, she is exempt from the penalty. This exemption creates a discontinuity in the penalty amount at 138 percent of the poverty line in non-expansion states. We illustrate this discontinuity in Figure 1.

1.2 Other income-based insurance inducements

Several other policies also encourage insurance coverage in a way that is tied to income. Some of these policies generate kinks near the mandate kink points, although the other kink points are distinct from the mandate kink points.

The Premium Tax Credit and the Advanced Premium Tax Credit The Premium Tax Credit (PTC) is a subsidy which may be used for purchasing an insurance plan on the Health Insurance Marketplaces. The PTC is equal to the difference between a household's "benchmark premiums"—the second lowest-cost silver-tier health insurance plan available to it in the Health Insurance Marketplaces—and its expected contribution, a percent of income specified by law that ranges from 2 to almost 10 percent. The expected contribution is a kinked and discontinuous function of income, so for the PTC is also a

⁶As we explain in Appendix A, for multiperson households, the penalty creates more complex incentives.

kinked and discontinuous function of income, with potential kinks at 100, 133, 150, 200, 250, 300, and 400 percent of the FPL, and discontinuities at 100, 133, and 400 percent of FPL. There is also a kink in the PTC at the income level at which the expected contribution exactly equals the benchmark premium; this kink point varies across markets, since the benchmark premium varies across markets.⁷ To help taxpayers manage liquidity, the PTC is paid in advance, throughout the coverage year, in the form of the Advanced Premium Tax Credit (APTC). APTC payment amounts are based not on realized MAGI, but on project income, which Marketplace enrollees report to the Marketplace at the time of signing up for insurance. If APTC payment are too high (because realized income exceeded projected income), taxpayers must repay the excess, with repayment limits that depend on realized income. These repayment limits are discontinuous functions of income, with discontinuities at 200, 300, and 400 percent of FPL. Heim et al. (2017) provide more detail on the PTC, APTC, and repayment requirements, and document income responses to the premium tax credit at the 400 percent of FPL discontinuity.

Cost-sharing reductions Whereas the PTC helps subsidizes premiums, cost-sharing reductions (CSRs) subsidize out-of-pocket health care expenses. For every standard silver plan that insurers offer on the Marketplace, they must offer three additional CSR plans, which are identical in all aspects except their cost sharing. A standard silver plan has an actuarial value of 70 percent, meaning it covers 70 percent of expected health care costs. The CSR plans have actuarial value of 73 percent, 87 percent, and 93 percent. Insurers must charge the same premium for these more generous plans as they do for the base silver plan; the government pays for the additional cost-sharing. Only low-income people are eligible to purchase these more generous plans. People with income between 100 and 150 percent of FPL may purchase the 93 percent actuarial value plans; people with income between 150 and 200 percent of FPL may purchase the 87 percent actuarial value plans; and people with income between 200 and 250 percent of FPL may purchase the 73 percent actuarial value plans.⁸

⁷The PTC and the mandate penalty are assessed using slightly different modifications of AGI. The definition of MAGI for the purpose of PTC is similar to the mandate penalty, but also includes the non-taxable Social Security income. We focus on a population aged 27-64 with MAGI between \$29,425 and \$47,080, so we expect that non-taxable Social Security income is zero for nearly all our sample.

⁸DeLeire et al. (2017) show that these subsidies influence plan choice on the Exchanges.

Other policies We believe that the PTC and CSRs are the most important potential threats to identification, in the sense that they create meaningful nonlinearities in the incentives to obtain insurance near the mandate kink point. Several other programs might also be relevant. Medicaid eligibility of course depends on income, although eligibility is determined in terms of rolling income throughout the year, rather than realized income. For single, childless adults that we study, nearly all states which offer Medicaid coverage have an income limit of 138 percent of FPL.⁹ This is not a problem for our analysis of the 138 percent FPL discontinuity, because we only examine non-expansion states.

Another kink in the incentive to obtain insurance comes from the tax deductibility of employer sponsored insurance, which creates a kink in the incentive to obtain ESI at each kink point in the income tax code. These kinks turn out not to be close to our nonlinearity points, because the income tax applies to taxable income rather than MAGI. For example, for a single taxpayer in 2016, the 15 percent tax bracket ran from \$9,275 to \$37,650 of taxable income. For a single tax payer with one exemption claiming the standard deduction, this works out to \$19,625 to \$48,000 in MAGI. Other programs such as SNAP, TANF, and the EITC may affect insurance demand through income effects. But we focus on single people without dependents, for whom these programs generally provide only small amounts of benefits.

Summary The PTC, APTC, repayment requirements, and CSRs all create kinks or discontinuities in the incentive to obtain health insurance coverage.¹⁰ These nonlinearities occur at even increments of the FPL: 133, 150, 200, 250, 300, and 400 percent. The mandate discontinuity and kink points occurs between these critical values, as shown in Figure 1. It is therefore possible to separately identify the coverage effect of the individual mandate from the coverage effects of these policies by looking within narrow windows of FPL (133-150 percent, 200-250 percent, and 300-400 percent).

⁹Washington, D.C., is the exception, with a limit of 215 percent of FPL. See <https://www.kff.org/health-reform/state-indicator/medicaid-income-eligibility-limits-for-adults-as-a-percent-of-the-federal-poverty-level/>

¹⁰Tebaldi (2017) studies coverage responses to the PTC in the California Marketplace, and Frean et al. (2017) study coverage responses at a national level using the in the ACS. DeLeire et al. (2017) study coverage responses to the CSRs.

2 Empirical Approach

2.1 Econometric specification

We estimate standard regression discontinuity and regression kink designs. The effect of the penalty on coverage can be obtained as the ratio of the kink or discontinuity in coverage to the kink or discontinuity in the penalty (Hahn et al., 2001; Card et al., 2015). We focus on regressions of the form regression:

$$y_i = \beta_0 + \beta_1 v_i + \beta_2 1\{v_i \geq 0\} + \beta_3 v_i 1\{v_i \geq 0\} + \varepsilon_i, \quad (2)$$

where y_i is an outcome of interest, such as months of insurance coverage, and v_i is income (in thousands of dollars) relative to the mandate kink point or discontinuity. Our estimate of the coverage discontinuity is β_2 and our estimate of the coverage kink is β_3 .

For the regression discontinuity estimates, we use the optimal bandwidth selection procedure of Calonico et al. (2014). This procedure yields a bandwidth of less than five FPL points, so we avoid looking across the 133 percent FPL policy discontinuity. To ease the computational burden, we estimate the regression discontinuity models on bin-level data, aggregated to bins of 0.1 FPL points (about \$12).

We estimate Equation 2 using people with income between 200 and 250 percent of FPL when examining the 2015 kink point, and between 300 and 390 percent of FPL in 2016. We focus on these ranges to avoid including any other policy nonlinearities in our estimation window.¹¹ To choose this specification, we conducted a Monte Carlo study to examine the performance of several different candidate estimators, following Card et al. (2017). We simulated a data generating process that closely resembles our actual data, but imposed a true kink corresponding to a semi-elasticity of 0.5, similar to what the literature estimates. In each of 1000 iterations, we implemented several estimators. We found that the simple piecewise linear specification using the full range of data performed better than other procedures, with 25 percent lower mean squared error, but slightly worse coverage rates.

¹¹We cut the sample off at 390 percent of FPL, rather than 400 percent, because Heim et al. (2017) document bunching in the income distribution at 400 percent of FPL, and we want to exclude the (highly selected) bunchers from our estimation sample.

See Appendix B for more details.¹²

Translating our reduced form coverage kinks and discontinuities into penalty sensitivities requires that we scale by the first stage kink or discontinuity in the penalty amount. We consider two approaches to the first stage. First, we treat the first stage as known and given by the statute kinks and discontinuities (e.g. a \$325/12 discontinuity and \$20/12 kink in 2015). In practice the statutory amounts are too large because some people claim exemptions or fail to pay the mandate penalty. Therefore we also consider an empirical first stage, given by the kink or discontinuity in the penalty amount paid per month of uninsurance. We obtain this first stage by estimating Equation 2 with the observed penalty amount paid per month of uninsurance as the dependent variable. (We can only estimate this regression for people with at least one month of uninsurance.) For consistency with past research, we report semi-elasticities, the percentage point increase in insurance coverage caused by a \$1 per month increase in the penalty.

2.2 Identifying assumption and tests of validity

We would like to interpret any estimated coverage kink or discontinuity in Equation 2 as caused by the kink or discontinuity in the mandate penalty. Our key identifying assumption is that in the absence of a kink or discontinuity in the mandate penalty, the relationship between income and insurance coverage would be smooth. This assumption can fail for several reasons. First, other policy nonlinearities near the mandate kink could create a kink in insurance coverage. Second, people may manipulate their income to reduce their mandate penalty. Third, the identification assumption also requires that there is no coincidental kink in the relationship between income and insurance coverage near the mandate kink point.

We attempt to address each of these concerns. We chose our estimation sample to avoid looking across policy kinks, as discussed in Section 1. Below, we test for income manipulation by looking at the income distribution around the mandate kink point (McCrary, 2007), and we test a necessary condition for smoothness of the income-insurance

¹²Our specification choices are generally similar to those used in the empirical RKD literature, e.g. Gelber et al. (2017b); Landais (2015).

relationship by examining the smoothness of correlates of insurance coverage around the mandate kink point.

2.3 Penalty sensitivity and forecast errors

Our baseline approach for translating the estimated coverage kink or discontinuity into a penalty sensitivity parameter makes an implicit assumption on people's information sets. Specifically we assume that people fully anticipate their mandate penalty at the time they sign up for insurance. To see why this assumption could fail, note that insurance coverage is determined before income or the mandate penalty is fully realized. For example, the open enrollment period in the Health Insurance Marketplaces ran from November 15, 2014 until February 15, 2015, but people may not have learned their penalty until they paid their taxes in early 2016.¹³ This issue is likely more accurate for the kink design than for the discontinuity design, because the kink point moves from year to year (making it harder to anticipate its exact location) whereas the discontinuity is relatively stable.

Perfect foresight is an appealing assumption because it leads to a straightforward specification—our estimation strategy becomes a sharp RKD. It is a strong assumption, however. Several considerations suggest that at least some people could anticipate their 2015 mandate penalty in time for their 2015 coverage. First, income is fairly (but not perfectly) stable from year to year. For example, among people whose 2014 income is between 200 and 250 percent of FPL (and who otherwise meet our inclusion criteria), about a third saw their income change by less than 10 percent. Second, at least some tax preparers and tax software systems notified filers in 2014 of their projected 2015 penalty amounts. Finally, although income uncertainty is resolved throughout the year, people also have opportunities throughout the year to respond to the mandate—for example, they can choose to drop Marketplace coverage. Dickstein et al. (2018) document considerable mid-year dropout in California's Marketplace, and argue that this reflects strategic behavior.

¹³See <https://www.kff.org/health-reform/issue-brief/explaining-the-2015-open-enrollment-period/>.

Nonetheless, it is likely that some people do not perfectly forecast their income or mandate penalty. In that case, our estimates likely understate their true responsiveness, as such forecast errors act as classical measurement error (Dickstein and Morales, 2018). Thus our estimates likely represent a lower bound on the response to the mandate penalty.

Beyond our baseline case of perfect foresight, we consider two alternative approaches to handling imperfect expectations. In general, we would like to relate coverage decisions in a given period to the perceived penalty in that period, $\widetilde{penalty}_{it}$:

$$Coverage_{it} = \gamma_0 + \gamma_1 v_{it-1} + \gamma_2 1\{v_{it-1} > 0\} + \gamma_3 \widetilde{penalty}_{it} + \eta_{it}. \quad (3)$$

The difficulty in implementing this equation is that $\widetilde{penalty}_{it}$ is unobserved. We consider two alternative assumptions on expectations, in addition to perfect foresight: that people form rational expectations about the period t penalty, given their income in $t - 1$, or that people naively project their $t - 1$ income one-period ahead. These assumptions capture the idea that people use readily available information to forecast their mandate penalty. Of course many other expectations processes are plausible; we view these assumptions as a straightforward way to account for expectations errors.

To operationalize rational expectations, we estimate

$$penalty_{it} = \alpha_0 + \alpha_1 v_{it-1} + \alpha_2 1\{v_{it-1} > 0\} + \alpha_3 v_{it-1} 1\{v_{it-1} > 0\} + \eta'_{it}, \quad (4)$$

and we set $\widetilde{penalty}_{it} = \widehat{penalty}_{it}$. Under rational expectations, the perceived penalty is the predicted penalty given prior year income, and Equations 3 and 4 represent a fuzzy RKD set up, with Equation 3 the structural equation and Equation 4 the first stage.

To operationalize naive expectations, we assume that

$$\widetilde{penalty}_{it} = Penalty_t(income_{it-1}), \quad (5)$$

where $Penalty_t(\cdot)$ is the function which translates income into a penalty (i.e. Equation 1). Under naive expectations, people simply map their prior year's income into a mandate penalty, without accounting for any dynamics. Under naive expectations, Equation 4

represents a sharp RKD, with a known first stage kink of 0.02 in 2015 and 0.025 in 2016.

2.4 Interpretation of the estimand

Several caveats govern the interpretation of our estimate. First, when we use the statutory kink or discontinuity at the first stage, we obtain the average response to a \$1 increase in the mandate penalty, averaging over people who are and are not subject to the mandate penalty, and accounting for evasion and avoidance. It is not a structural parameter in the sense that a price elasticity would be. We believe that this parameter is policy relevant, as it gives the in-sample response to a \$1 change in the legislated penalty amount. However the response to observed kinks or discontinuities (rather than statutory ones) is likely more comparable to past estimates of semi-elasticities of insurance demand, so we report those as well.

Second, our estimate is a local average marginal effect of the mandate penalty, specific to people with income near the mandate kink point or discontinuity. The estimated coverage kinks reflect the average marginal effect of a small increase in the mandate penalty. The effect of a small change in the mandate penalty could be different for people at different income levels, and the (per-dollar) effect of fully repealing the mandate could differ from the effect of a small change, for example because of a “taste for compliance” that is independent of the penalty amount (Saltzman et al., 2015). However the coverage discontinuities may include a taste for compliance (if any), as they reflect responses to the first dollar of penalty.

Finally, we limit our kink sample to people without signs of an ESI offer. ESI offers may themselves increase because of the mandate, however, as it raises the compensating differential of ESI (Kolstad and Kowalski, 2016). This possibility is not a threat to the internal validity of our estimator if the probability of having a job with an ESI offer is not kinked at the mandate kink point. We test for and fail to find a significant impact of the mandate kink on ESI offers, or on coverage conditional on an ESI offer. However, our sample selection approach does change the interpretation of our estimates. They reflect the effect of the mandate penalty on insurance coverage, conditional on not receiving an

ESI offer. If the penalty increases ESI offers, then our estimates understate the total effect of the individual mandate.

3 Data

For our data, we extract information from the full population of U.S. individual income tax returns in 2015 and 2016, maintained by the Internal Revenue Service (IRS) for the full population of U.S. individual income tax returns in 2015 and 2016.¹⁴

3.1 Variable definitions

Coverage measures Since 2015, insurers have filed Form 1095, which reports monthly coverage. Marketplace insurers file Form 1095-A, self-insured employers file Form 1095-C, and all other health insurance providers file Form 1095-B. Each form 1095 reports all months of coverage, and also indicates type of coverage. Form 1095-A always represents Marketplace coverage, and Form 1095-C always represents large employer coverage. Form 1095-B contains insurer identifier codes from which we infer other coverage types: small employer plans, public plans (Medicaid, Medicare, or VA), and off-Exchange plans. As a third-party information return, Form 1095 likely accurately measures coverage.

We also observe self-reported full year all family health insurance coverage on Form 1040, the main tax return. Filers may check a box to indicate whole year coverage. If they check this box, they might be able to avoid the mandate penalty. This self-reported information is more likely to be misreported. However, Form 1040 can indicate legitimate coverage even without verification from Form 1095, because insurers may file Form 1095 late or not at all.

We define months of “any coverage”, equal to 12 if the 1040 full-year coverage box is checked, and equal to the number of months with coverage reported on Form 1095 otherwise. We define “verified coverage” as months with 1095-reported coverage. We disaggregate coverage into different types using Form 1095. These coverage types include:

¹⁴We do not use 2014 because third-party verification did not begin until tax year 2015.

Medicaid, Veteran’s Affairs (VA), non-VA ESI (including military/Tricare), Exchange, and off-Exchange. Our definition of “Medicaid” coverage pools together all non-Medicare, non-VA governmental insurance plans. Lurie and Pearce (2018) provide more detail on these data, and show their validity. The tax-based coverage measures are quite similar to survey-based measures such as those in the ACS, CPS, or MEPS, although there are some differences. In general our “verified” measure yields a slightly lower coverage rate than survey-based measures, while our “any” measure yields a slightly higher rate. The tax based rate is higher for public and lower for ESI than are survey based measures.

Mandate penalty data Individuals who report that they were not covered all year are instructed to fill out a worksheet to determine their mandate penalty. We define the dummy variable “paid penalty” as an indicator for whether a positive mandate penalty amount was reported on Form 1040.

Income data Our income measure is modified adjusted gross income (MAGI) as it is calculated for determining the mandate penalty that a person would pay if she were uninsured. To obtain our income measure, we start with adjusted gross income and add tax exempt interest, foreign earned income, and housing income (all reported on Form 1040) to generate reported MAGI.

Covariates We observe age and gender in the Death Master File of Social Security, and we merge these fields into our data. In some specifications, we split the sample based on prior income, which we define as average taxable income in the prior three years. We also split the sample based on health status. Although our administrative data lacks direct measures of health, we observe two variables closely related to health: an indicator for any itemized medical expenses in the past three years, and an indicator for ever receiving Social Security disability insurance income (since 1999) a proxy for disability. As a measure of the cost of coverage, we impute the benchmark Exchange premiums using tax return zipcodes and premium data from the HIXCompare database.¹⁵ (Currently we lack data on several of these covariates for the discontinuity sample.)

Signs of ESI offers We do not observe ESI offers directly, because small employers are

¹⁵Available at hixcompare.org. We lack premium data for about two percent of tax returns with missing zip code information.

not required to report ESI offers to the IRS. Instead, we identify people who have signs of an ESI offer, meaning an employment situation in which an ESI offer is likely. The first sign of an ESI offer is employment in a large employer. Among people working for a large employer in our data, 80 percent had ESI coverage in 2015 and 96 percent in 2016. Second, among people not working for large employers, we observe several signs of ESI offers. Specifically, for each employer, we construct flags indicating whether any of the W2s for its employees contain HSA contributions, employee retirement plan indicators, or health insurance premium payments. We then say that any person with a W2 from a flagged employer has a sign of an ESI offer.¹⁶

3.2 Sample selection

We begin by drawing individual tax returns (Form 1040) from tax years 2015 and 2016 that were filed by single individuals with no dependents, meaning no children were claimed on the tax return. We study single-person tax returns because the individual mandate creates complicated, possibly non-salient coverage incentives for larger households. (See Appendix A.) We next limit the sample to individuals age 27-64, dropping people whose age makes them eligible to obtain coverage through their parents or Medicare. Finally, we limit the sample to include only those in a window around the relevant nonlinearities. For the discontinuity analysis, we limit the sample to people with income between 110 and 160 percent of FPL (although in most specifications we end up zooming in to 133 to 143 percent of FPL). The discontinuity sample is limited to people in non-expansion states. We end up with about 1.1 million people in 2015 and 1.0 million in 2016.

For the kink sample, we look at 200 to 250 percent of FPL in 2015, and 300 to 390 percent of FPL in 2016.¹⁷ For the kink sample, we further limit the sample to people without signs of ESI offers. Such people have access to highly subsidized insurance and had high coverage rates even prior to the ACA (see, e.g. Kaiser Family Foundation and

¹⁶Among people with W2s from flagged employers and not 1095-Cs, about 50 percent have ESI coverage, while people without such W2s or 1095-Cs, the 25 percent have ESI.

¹⁷We cut off the sample at 390 percent of FPL in 2016 because Heim et al. (2017) find that people manipulate their income to get below the 400 percent FPL notch. We chose this cutoff to exclude most bunchers.

Health Research and Educational Trust (2012)), meaning the mandate likely had a small and difficult-to-detect effect on their coverage decision. Indeed, we show below that a greater mandate penalty does not increase insurance coverage for this group. (If anything, it slightly reduces coverage.) We also show that a greater penalty does not induce greater selection into the signs-of-ESI sample; we find no kink in the probability of having a sign of an ESI offer at the mandate kink point. After these exclusions, our kink analysis sample consists of 653,891 observations in 2015 and 656,056 observations in 2016.

Table 1 reports summary statistics for the analysis samples. On average people had about 8 months of coverage. Exchange coverage and ESI coverage are both fairly common.¹⁸ Medicaid is also a non-trivial coverage source in the kink sample. This is surprising because most states extended Medicaid eligibility for childless adults only up to 138 percent of FPL. The main explanation for the non-trivial Medicaid coverage in our sample is that Medicaid eligibility is assessed on a continuous basis, but we observe income annually. A person with a spell of unemployment in 2014 or 2015, for example, could be temporarily eligible for Medicaid, and remain on its rolls even after her income recovers to above 200 percent of FPL.

Twenty-one percent of our kink sample paid the mandate penalty in 2015 and 13 percent in 2016. A lower fraction paid the penalty in the discontinuity sample, despite similar coverage rates, because about half our discontinuity sample has income below 138 percent of FPL and is exempt from the penalty. The average penalty paid per uninsured month is low even in the kink sample—about \$15 in 2015 and \$26 in 2016, relative to statutory amounts of \$27 and \$59. From this we conclude that, although many people end up paying the mandate penalty and it therefore likely has real coverage incentives, these incentives may be small relative to the statutory amounts.

¹⁸Although we exclude people with signs of ESI offers in our kink sample, we end up with some ESI coverage because, while no one in our sample has ESI in their own name, people can obtain coverage through others, such as domestic partners.

4 Results

4.1 Smoothness tests

We begin by examining the validity of our RD and RK designs, showing smoothness of the income distribution and covariates around the mandate discontinuity and kink points. Figure 2 shows the income distribution for each sample. There is no obvious kink or discontinuity. Close inspection reveals perhaps a slight downward dip in the income distribution in the 2016 discontinuity sample. We test for smoothness by regressing the bin count on income, allowing for a kink and discontinuity at the mandate kink point. The estimated kinks and discontinuities are small and statistically insignificant in all samples.

Next we turn to showing smoothness of pre-determined covariates. Our goal is to show that demand for insurance is not inherently kinked at the mandate kink point, so we focus on variables which we believe are most closely correlated with insurance coverage: prior itemized medical expenses and benchmark premiums. Figure 3 shows the binned scatter plots of these variables in the kink samples. (Currently we lack data on them for the discontinuity samples.) They are clearly correlated with income, but again there is no obvious kink or discontinuity at the mandate kink point, and the estimated kinks are statistically insignificant. We report smoothness tests for a larger set of predetermined outcomes in Table 2. We estimate insignificant and fairly small kinks across these outcomes. We conclude that there is no evidence of income manipulation, and the predictors of insurance demand are smoothly distributed around the mandate kink point.¹⁹

4.2 First stage

Next we show that empirically there are discontinuities and kinks in the strength of the penalty. We plot the observed penalty amount paid per month of uninsurance in Figure 4. We highlight two important facts about penalty paid. First, there are visually obvious discontinuities in penalty paid in the discontinuity samples, and visually obvious kinks in

¹⁹It is not surprising that we find no evidence of income manipulation at the mandate kink point, despite bunching at the PTC notch (Heim et al., 2017; Kucko et al., 2017). Unlike the PTC notch, the mandate kink is small and even small earnings adjustment frictions would be enough to prevent bunching (Gelber et al., 2017a).

the kink samples. We report the estimated kinks in Table 3. The second important fact is that the estimated kinks and discontinuities are far below their statutory levels. We would expect statutory discontinuities of about \$27 in 2015 and \$58 in 2016, and statutory kinks of 1.67 and 2.08. The estimated discontinuities and kinks are less than half as large. These differ from statutory amounts both because people can obtain coverage exemptions, and because some people declined to pay a penalty amount even though they were uninsured and claimed no exemptions. Thus, although exemptions and limited enforcement weaken the bite of the mandate, empirically we do see a sharp increase in penalties around the discontinuities and kinks.

4.3 Coverage effects of the mandate penalty

We now turn to our main results, the estimated discontinuities and kinks in insurance coverage around the mandate discontinuity and kink points. Figure 5 and Figure 6 show months of insurance coverage (any or verified) as a function of income, by year, for the discontinuity and kink samples. In both years there are visually obvious discontinuities in any coverage, as well as discontinuities (that are somewhat less distinct) in third party verified coverage. We report the estimated discontinuities and implied semi-elasticities in Table 4. The “statutory” semi-elasticity is about 0.07 in both years. However, this low response reflects the fact the many people do not pay the penalty even when uninsured. Using the estimated first stage, we obtain an observed semi-elasticity of 0.24 in 2015 and 0.33 in 2016. These estimates are on the low end of those in the literature, which generally looks at the semi-elasticity of individual market or Exchange coverage. The estimates of Hackmann et al. (2015) imply a semi-elasticity of about 0.2 in the context of the Massachusetts mandate. Tebaldi (2017) estimates semi-elasticities in California of 0.2 to 0.5 for subsidized households. Jaffe and Shepard (2017) estimate a semi elasticity of about 1, looking at a lower income population in Massachusetts. Finkelstein et al. (2017) also find a semi-elasticity of individual market coverage of about 1, identified off of discontinuities in PTC generosity in Massachusetts. Overall we find statistically significant coverage responses to the individual mandate, but low rates of paying the penalty end up dampening

the elasticity of coverage with respect to the statutory amount.

Turning now to the kink estimates, we see a In 2015, there is a clear upward kink in months of overall insurance coverage at the mandate kink point. The estimated kink, reported in Panel A of Table 4, is a statistically significant 0.051, meaning that for each extra \$1000 of income above the mandate kink point, coverage rises by 0.05 months, relative to the trend below. This estimate implies that each \$1 of mandate penalty per month raises insurance coverage by 0.03 months, or about 0.41 percent. About 60 percent of the response comes from verified coverage, which has a marginally significant kink of 0.029 ($p=0.09$), for a semi-elasticity of 0.28. At the 2016 kink point, there is a smaller and less clear response to the mandate penalty, reported in Panel B of Table 4. We estimate a statistically insignificant kink of 0.020 in overall coverage, corresponding to a semi-elasticity of 0.14. The kink in third-party verified coverage in 2016 is essentially zero, with a large standard error. Although the statutory semi-elasticities are fairly low, the observed semi-elasticities are comparable to past estimates.

The overall response masks important heterogeneity across different categories of coverage. In Figure 7 and Figure 8, we show months of Medicaid, Exchange, and ESI coverage as a function of income in the kink and discontinuity samples. We report estimated kinks by category in Table 4, columns (3)-(8). In the discontinuity sample (which consists of non-expansion states only), we find clear exchange responses, with observed semi-elasticities of 0.23 in 2015 and 0.46 in 2016. In the kink samples (which includes non-expansion states) we find statistically insignificant exchange responses. Strikingly, we find in the 2015 kink sample that Medicaid coverage responds sharply to the mandate penalty, accounting for 45 percent of the overall response. We also find a modest (but statistically insignificant) response for ESI in 2015.

4.4 Heterogeneous responses by observable characteristics

A key goal of the individual mandate is not only to increase coverage, but especially to encourage healthy people to obtain coverage, to ease adverse selection. To look for such heterogeneous responses, we re-estimate our RD and RK models, stratifying on co-

variates related to health and spending: sex, age, prior itemized medical expenses, and disability insurance income. In Table 7 we present the heterogeneous RD estimates. We pool 2015 and 2016 to maximize power. We find that men are nearly twice as responsive to the mandate discontinuity as are women. Younger people are also substantially more responsive than are older people.

We find similar differences in the kink sample, albeit with less precision. The results are in Panels A-D of Table 8.²⁰ We find that men are about three times as responsive as women, and young people—below the median age of 45—are more than twice as responsive as older people. People with clear markers of poor health—itemized medical expenses or disability insurance income—have a negative and statistically insignificant response. Across multiple dimensions, we find that signals of good health are correlated with responses to the mandate penalty. These differences are not all statistically significant, but they point towards the conclusion that groups with lower coverage rates, and lower expected health care expenses, respond most to the mandate penalty.

4.5 Alternative expectations: Relating 2015 coverage to 2014 income

Our analysis so far uses current-year income as the running variable, because current-year income determines the mandate penalty. However, this variable does not capture possible errors in people’s forecast of their 2015 income and mandate penalty at the time of enrollment. As an alternative running variable, we used lagged income, under the hypothesis that people use their prior year income to forecast their current year income and mandate penalty. For this analysis, we select a sample of people whose 2014 income is between 200 and 250 percent of FPL, without signs of ESI offers in 2014.

Figure 12 plots months of coverage in 2015 as a function of 2014 income. We see a clear kink in both reported and verified coverage. We report estimated kinks in Table 9. We begin in column (1) by showing the kink in the 2015 mandate penalty as a function of 2014 income. It is about 0.009, meaning that each additional dollar of 2014 income above the kink is associated with a \$0.01 higher mandate penalty in 2015 - about half what we

²⁰We do not examine 2016 because we lack the power to detect cross-group differential responses.

would expect if income were constant from year to year. The remaining columns show the kinks in months of any coverage and coverage by type. The overall coverage kink is fairly similar to our estimate when we use 2015 income as the running variable. However the category-specific responses differ somewhat. We find a larger individual market response (especially off-Exchange) and a smaller (and now statistically insignificant) Medicaid response. Our interpretation of this difference is that the estimator is picking up a different margin of response when we use lagged income as the running variable. In this specification, the estimator is doing a better job picking up responses before the coverage year has begun, and a worse job picking up within-year responses as income changes. By contrast, the estimator using current-year income does a better job picking up within year responses and a worse job picking up responses before the year has begun.

Translating these kinks into semi-elasticities requires an assumption about how people use their 2014 income to forecast their 2015 penalty. Under the assumption that people make a totally naive forecast—they assume that their 2015 income will equal their 2014 income—then the reduced form kinks should be scaled by $1/20$ (since there is a kink of \$20 in the mandate penalty). If, alternatively, people have rational expectations and forecast their 2015 income and mandate penalty using the OLS predicted value, then the reduced form kink should be scaled by roughly $1/.0091$. We report the semi-elasticities both ways. Accounting in this parametric way for expectation errors results in semi-elasticities that are roughly double what we find in the baseline case.

4.6 Digging into the Medicaid and individual market responses

We dig further into the large Medicaid response and small individual market response by re-estimating our RK models, stratifying on Medicaid expansion status.²¹ We expect larger Medicaid responses in states that expanded Medicaid, and larger individual market responses in non-expansion states. We report the estimates in Table 5. The Medicaid

²¹The Medicaid expansion states in 2015 are Arkansas, Arizona, California, Colorado, Connecticut, Washington (D.C.), Delaware, Hawaii, Iowa, Illinois, Kentucky, Massachusetts, Maryland, Michigan, Minnesota, North Dakota, New Hampshire, New Jersey, New Mexico, Nevada, New York, Ohio, Oregon, Pennsylvania, Rhode Island, Vermont, Washington, and West Virginia. The 2016 expansion states are all the 2015 expansion states, plus Alaska, Indiana, and Montana.

response occurs almost entirely in Medicaid expansion states, in both 2015 and in 2016. Likewise, in 2016, we find the Exchange and off-Exchange responses are concentrated in non-expansion states. In 2015, however, we find no individual market response even in non-Expansion states. The point estimates are all statistically insignificant, fairly small, and some are wrong signed. These estimates suggest that people respond to a greater mandate penalty by obtaining Medicaid coverage if at all possible. Only at fairly high income levels and in non-expansion states do we see an individual market response.

The substantial Medicaid response raise an important question: how is it that people with income above 200 percent of FPL obtain Medicaid coverage? Medicaid eligibility is assessed based on rolling income, with infrequent recertification, rather than on realized annual income. It is likely that people in our sample obtain Medicaid coverage because they found or lost a job during the year, and were temporarily eligible for Medicaid. We expect to see the biggest increases in partial year Medicaid coverage—people with a few more months of coverage, rather than an increase from 0 to 12 months of coverage. To test this hypothesis, we estimate regressions of the form

$$Pr(\text{Medicaid Months}_i \leq m) = \beta_0^m + \beta_1^m v_i + \beta_2^m 1\{v_i \geq 0\} + \beta_3^m v_i 1\{v_i \geq 0\} + \varepsilon_i^m. \quad (6)$$

This is an RKD where the dependent variable is an indicator for having at most m months of Medicaid coverage. We expect to find larger effects on the probability having an intermediate number of months of coverage (1-11). This implies that we should find less negative kinks as m grows larger. We present the estimates graphically in Figure 9.²² Specifically we show the baseline CDF at the 2015 kink point, and the new CDF induced by a \$10 per month increase in the mandate penalty, along with the new CDF's 95% confidence interval. The baseline CDF is given by the estimates of β_m^0 from Equation 6. We obtain the new CDF by adding the implied effect of a \$10 penalty increase to the baseline CDF. The new CDF is lower everywhere than the old CDF, implying that the penalty shifts people towards more months of Medicaid. However the distance between the CDFs is greatest for relatively low months of coverage. The mandate penalty increases months of

²²We report the estimates in Appendix Table C.1 presents the estimated kinks and the implied effect of a \$1 increase in the monthly mandate penalty. The effect is largest for 0-5 months of coverage.

Medicaid coverage primarily at the bottom end of the coverage spectrum, pulling people up from zero months of coverage to 1-6 months coverage, with a relatively smaller effect higher up.

4.7 Exploring the ESI offer sample

Our main kink sample excludes people with signs of an ESI offer. Hence, our main estimates do not reflect two channels through which a greater mandate penalty could affect insurance coverage: by changing ESI offer rates, or by changing take up among people offered ESI. Here we present evidence that neither of these channels is quantitatively important, at least in the context of the kink. To do so, we expand our sample to include people with signs of ESI offers.

We begin by showing that there is no kink in the probability of having a sign of ESI offer at the mandate kink point. Figure 10 plots the fraction of people who have a sign of an ESI offer, as a function of income. In both 2015 and 2016 the offer rate is increasing in income but essentially smooth through the mandate kink point. In 2015 the estimated kink is -0.06, with a standard error of 0.18, meaning that each thousand dollars of income above the kink point reduces the offer rate by 0.06 percentage points—an economically small, statistically insignificant and wrong-signed amount. In 2016, the estimated kink is -0.21 (standard error=0.13)—larger in absolute value, but still small and statistically insignificant. Although it is certainly possible that the mandate increased ESI offers, it does not do so in a sharp way around the mandate kink point.

Next we show that, among people with signs of an ESI offers, a greater mandate penalty does not increase insurance coverage. Figure 11 shows the coverage rate among people with signs of an ESI offer. Unsurprisingly, the overall coverage rate is much higher – on average people have around 10 months of coverage. There is no upward kink in the coverage; indeed, if anything there is a negative kink in the number of months of coverage at the mandate kink point. We reported estimated kinks in coverage in Table 6. We estimate a negative (and statistically significant) kink in coverage for the ESI offer sample. This kink is driven almost entirely by a negative kink in ESI coverage. We believe

this negative kink is likely due to concavity in the income-coverage relationship, rather than a true negative effect of the mandate penalty on coverage, because it is difficult to see how a greater penalty could reduce coverage. We therefore conclude that the greater mandate penalty above the kink point does not induce a greater ESI offer rate, nor does it increase take up of ESI among people with ESI offers.

4.8 Reconciling the disparate kink estimates

We find a substantially larger—but not statistically significantly different—response to the mandate kink in 2015 than in 2016. We interpret this difference as an income effect, reflecting the fact that the 2015 kink point is at a lower income level. In general we expect that price sensitivities fall as income rises. Direct evidence for this view comes from the estimates in Panel E of Table 8, which shows that people with lower prior income have substantially higher responsiveness to the mandate penalty, providing direct evidence of an association between income and responsiveness. Of course we cannot completely rule out alternative explanations such as declining enforcement or taste-for-compliance. Indeed, the first stage becomes weaker in 2016 than in 2015, in the sense that the estimated penalty kink is a smaller ratio of the actual penalty kink in 2016 than in 2015 (and the same is true for the discontinuity). However even the observed semi-elasticity is smaller in 2016 than in 2015, so simple underpayment does not explain the lower sensitivity. We also show in Figure C.4 that support for the ACA did not fall from 2015 to 2016, suggesting falling support for the mandate does not explain the 2016 decline. (What about the discontinuity estimates? The discontinuity occurs at a lower income level than even the 2015 kink, so perhaps we should find a larger coverage sensitivity there. We note that the kink and discontinuity samples differ because the discontinuity sample both excludes people in Medicaid expansion states, and includes people with signs of ESI offers. This latter inclusion likely pushes downward the observed response to the mandate, since such people have high coverage rates and low responsiveness to the penalty.)

4.9 Quantitative implications: simulating mandate repeal

The estimated coverage responses imply that the mandate has a quantitatively important effect on coverage. To illustrate, we consider a simple simulation: how much lower would overall coverage in our sample be if we set the mandate penalty to zero? We separately simulate the effect implied by each of our samples (2015 or 2016, kink or discontinuity). For these simulations, we use the statutory penalty amount as legislated for 2017, because we do not know the penalty amount paid. The simulations are therefore based on responses to the statutory penalty rather than the observed penalty. We report results of this simulation in Panels A and C of Table 10. Repeal would reduce the average penalty by about \$30/month in the discontinuity sample and \$60/month in the kink sample. Given our estimated sensitivities, this entails a 1-2 percent fall in coverage in the discontinuity sample and a 7-24 percent fall in coverage in the kink sample. There is a lower coverage response in the discontinuity sample because the penalty amount is lower (as we average over some people who are exempt from the penalty) and the sensitivity is lower. In the discontinuity sample this simulation is essentially in-sample, since the variation we use for identification comes from comparing people who are subject to the penalty to people who are not. However we caution that this calculation in the kink sample entails extrapolating far outside the range of variation identifying our mandate sensitivity parameters. We present it only to show the quantitative implications of our estimates.

These simulations give overall coverage responses. The goal of the mandate, however, may have been to increase coverage in the individual market in particular. We repeat our simulation exercise but using Exchange coverage responsiveness (rather than any coverage) in Panel B and D of Table 10. Our estimates imply that setting the mandate penalty to zero would reduce Exchange coverage by about 1 percentage point in the discontinuity sample, and up to 5 percentage points in the kink sample. The largest estimate (2016 kink sample) suggests perhaps a 30 percent decline in Exchange enrollment. However, this likely overstates Exchange coverage responsiveness because it puts too much weight on the 2016 kink point, around which our sample spans 90 FPL points, versus 50 FPL points around the 2015 kink point. Put differently, the income distribution is thicker around

the 2015 kink point than around the 2016 kink point, so we should weight 2015 higher. If we weight by the density of the income distribution around each kink point, our estimates imply that setting the penalty to zero would reduce Exchange coverage by about 1 percentage point, or about 6.4 percent.

4.10 Further validity and robustness tests

Permutation test A key concern with the RKD is the possibility of finding spurious kinks, simply because of curvature in the relationship between income and insurance coverage (Ganong and Jäger, 2017). We assess this concern in Appendix C by re-estimating our RKD models, but varying the kink point across a fine grid of placebo locations. If the kink is spurious, then we expect that our estimate is unexceptional in the distribution of placebo estimates. We find that our kink for any coverage in 2015 is larger than 92.2 percent of placebo estimates, and the kink for verified coverage in 2015 is larger than 90.2 percent of the placebo estimates.

Alternative bandwidths We consider robustness to bandwidth and to alternative specification choices. Figure C.2 shows the estimated discontinuity and its 95% confidence interval, as a function of the bandwidth. As we vary the bandwidth we never use data below 133 percent of FPL, to avoid looking across the PTC notch. We consider (possibly asymmetric) bandwidths as small as \$200 or as wide as \$1400. In both years the point estimates are fairly stable across bandwidths.

We plot the analogous figure for the 2015 kink estimate in Figure C.3. The point estimate is stable over a wide range of bandwidths. At small enough bandwidths, the point estimate fluctuates and its confidence interval becomes quite large. The mean-squared error optimal bandwidth of Calonico et al. (2014) is about \$900. At this bandwidth, the point estimate is 0.17, thrice as large as our main estimate, but the confidence interval is larger still, and so the estimate is marginally significant ($p = 0.08$). Looking across the different coverage types, the point estimates are fairly stable until the bandwidth becomes small, at which point the estimates become less stable and much less precise.

Alternative controls or samples We consider robustness to alternative specification

choices: allowing for nonlinearities in income (i.e. a quadratic or cubic), controlling for demographics (female dummy and a quadratic in age), imposing continuity at the kink point, and excluding people with ESI. The results are in Appendix Tables C.2 - C.8. The RD estimates are fairly robust to alternative functional forms; the point estimates change little in magnitude or significance. For the kink sample, the nonlinear income terms generally produce larger estimates, and sometimes substantially larger ones. For example the kink in any coverage in 2015 increases by 60 percent when we include a quadratic term, and it more than doubles when we include a cubic. The standard errors also rise, consistent with the findings from our Monte Carlo analysis that these nonlinear terms substantially increase the MSE of our estimates. We focus on the linear specifications because of these large standard errors, although we find it reassuring that allowing for higher order terms would, if anything, strengthen the conclusion that the mandate penalty increases coverage. In general the results are not sensitive to other choices of controls. They change little when we impose continuity or add demographic controls. The estimates typically rise when we drop people with ESI coverage.

5 Conclusions

The individual mandate has generated considerable attention and controversy, with protests, legal challenges, and numerous repeal efforts ultimately culminating in the Tax Cuts and Jobs Act of 2017, which set the mandate penalty to zero. Despite all this attention, relatively little is known about whether the individual mandate actually causes people to obtain coverage. We provide new evidence on this question using regression discontinuity and regression kink designs and data derived from the population of tax returns.

Our results show that the mandate penalty influences coverage decisions. Our RD estimates show a clear coverage response to the existence of the mandate penalty. Our RK estimates show that coverage also responds to marginal increases in the penalty amount. People with indicators of good health appear especially responsive to the mandate penalty. Medicaid coverage responds heavily (in expansion states), and we find small Exchange

coverage responses.

Our estimates imply economically meaningful coverage effects. In simple, partial equilibrium simulations, we find that mandate repeal would reduce coverage by about 1 percentage point around the discontinuity and about 10 percentage points around the mandate kink. On the other hand, we find small responses in the individual market. Our estimates imply that repeal would reduce Exchange coverage by perhaps 1-2 percentage points or 6-12 percent.

If the goal of the individual mandate is to raise insurance coverage, then our evidence suggests it has succeeded. If the goal is to bring reduce adverse selection in the individual insurance market, then the evidence is mixed: although healthy people are especially responsive to the mandate penalty, they do not necessarily respond by enrolling in the individual market. If, however, the goal is to achieve universal coverage, then the individual mandate has clearly failed. In principle the goal of universal coverage could be achieved with a higher penalty, as we find that a greater mandate penalty leads to higher coverage. Our estimates suggest that it would have to be much higher indeed. Ignoring any response of premiums, and extrapolating crudely, the mandate penalty would have had to be about \$2000 at the 2015 kink point to achieve full coverage. This is a very large increase—about three times the current level. Interestingly, it is in the range of recently proposed policies. For example, Scott Morton (2018) proposes an individual health care responsibility payment for Connecticut that ranges from 4 to 9.6 percent of income—which is roughly twice as large as the federal mandate penalty at low incomes and more than twice as large at high incomes. Our estimates suggest that such a high penalty could have a large effect on insurance coverage.

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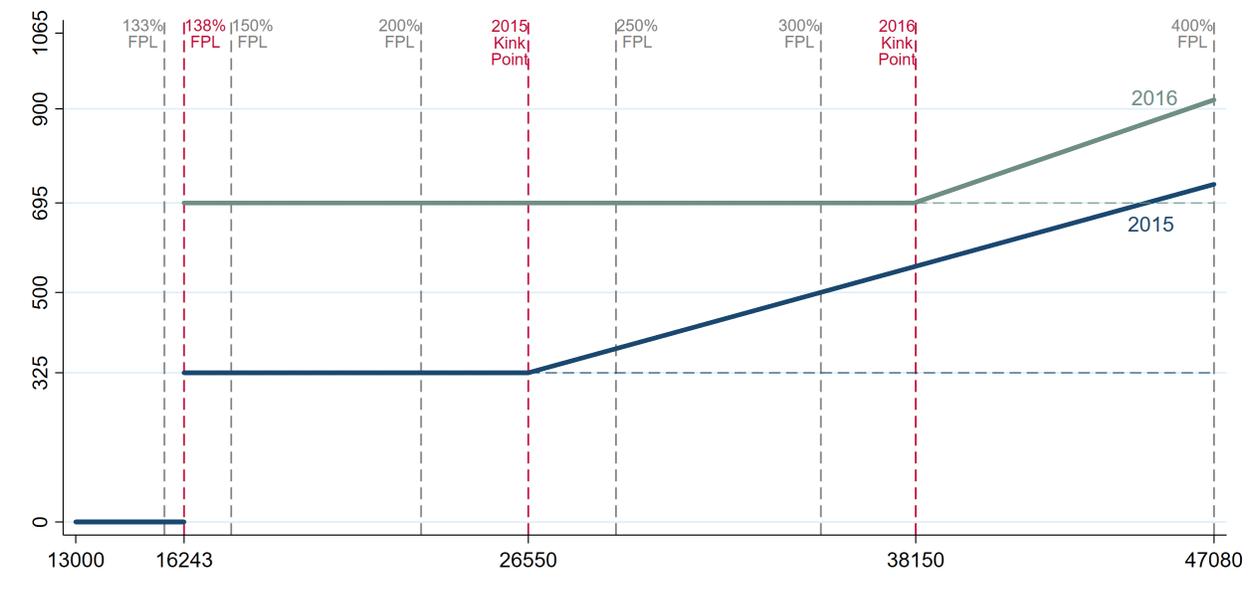
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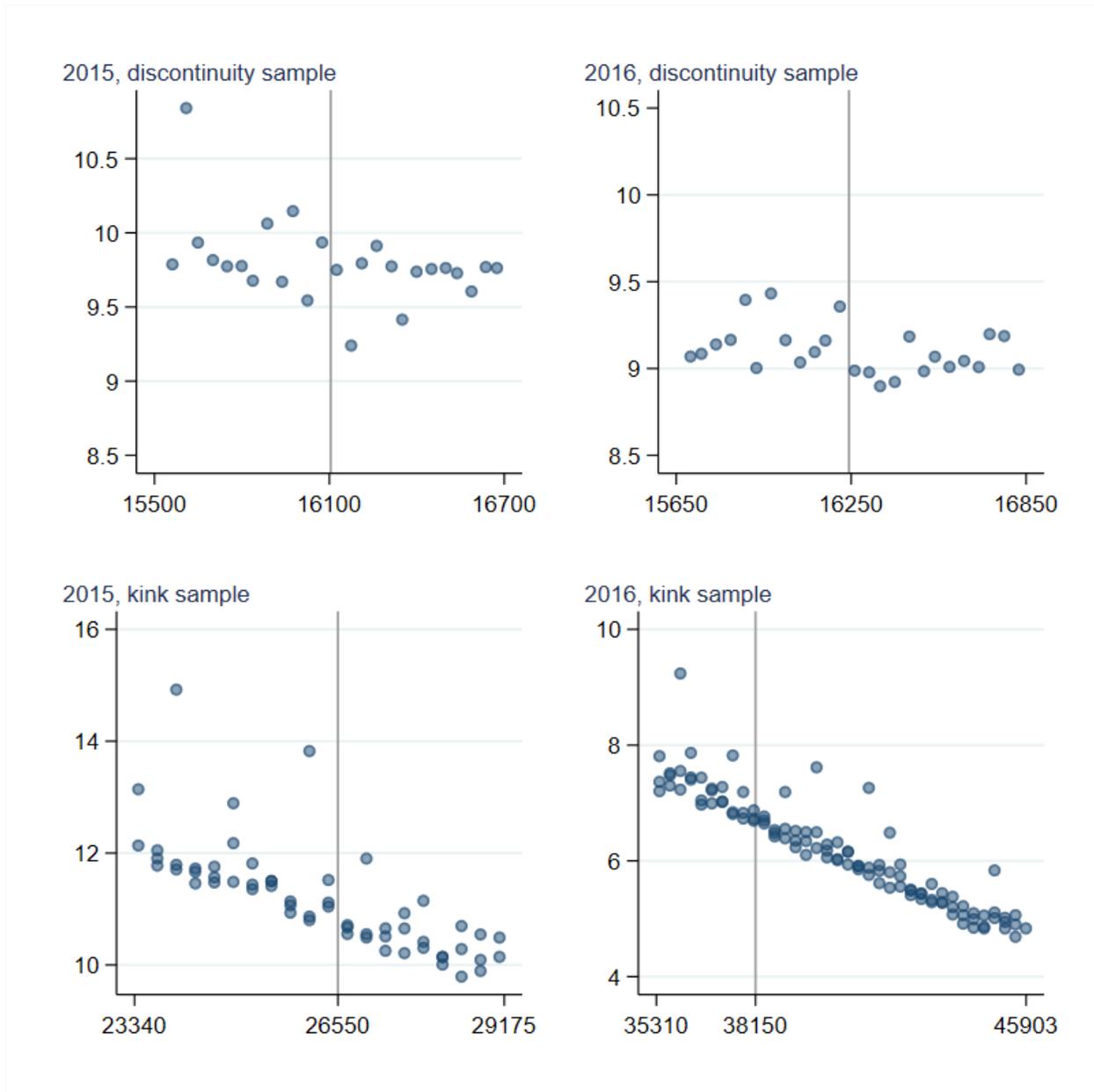
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Figure 1: Mandate penalty and as a function of income



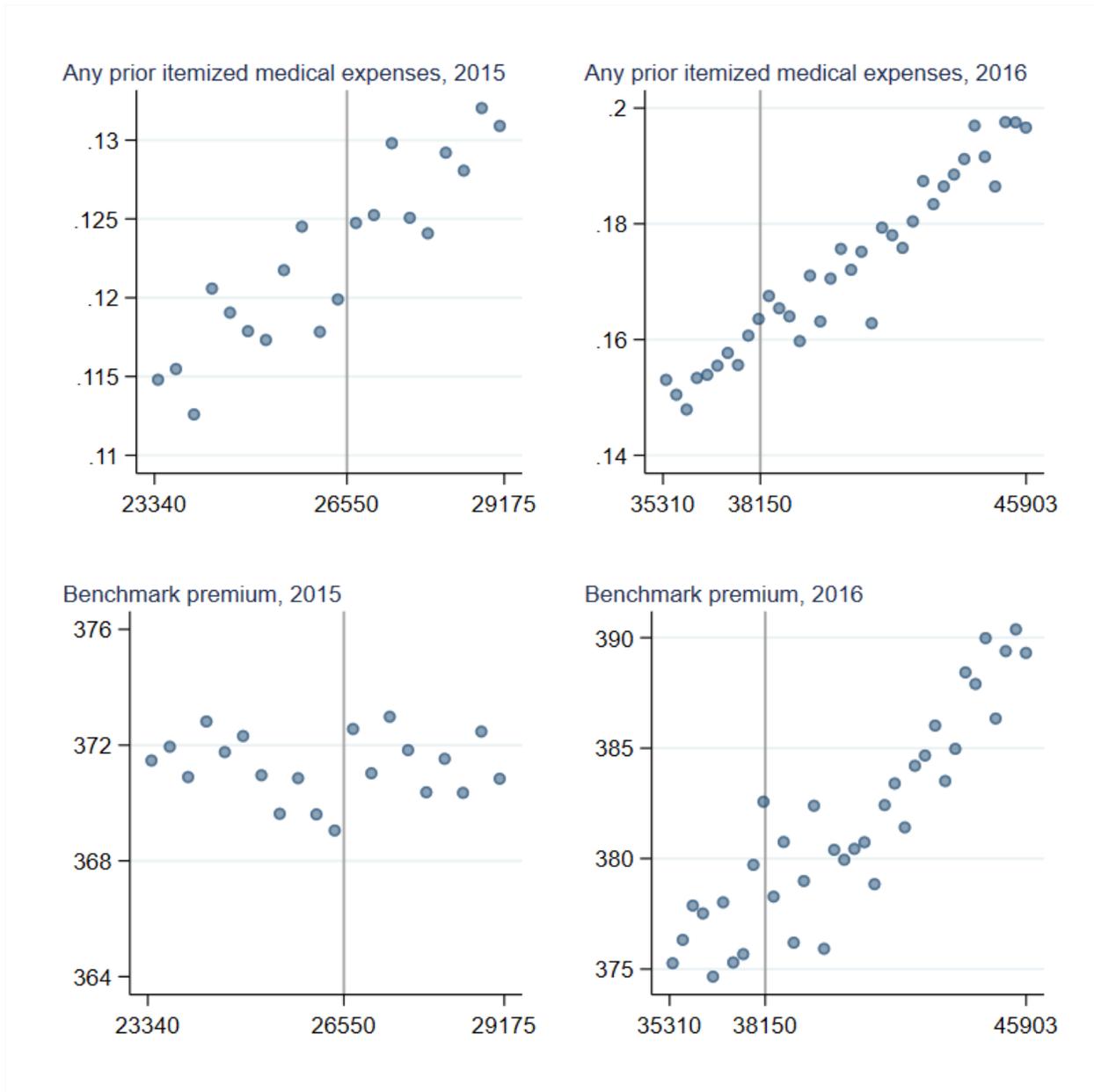
Notes: Figure shows the 2015 and 2016 mandate penalty for a single person per 12 months of uninsurance, as a function of income. The light dashed lines indicates thresholds for cost-sharing reductions and premium tax credits (given the 2016 poverty line of \$11,770).

Figure 2: Income distribution



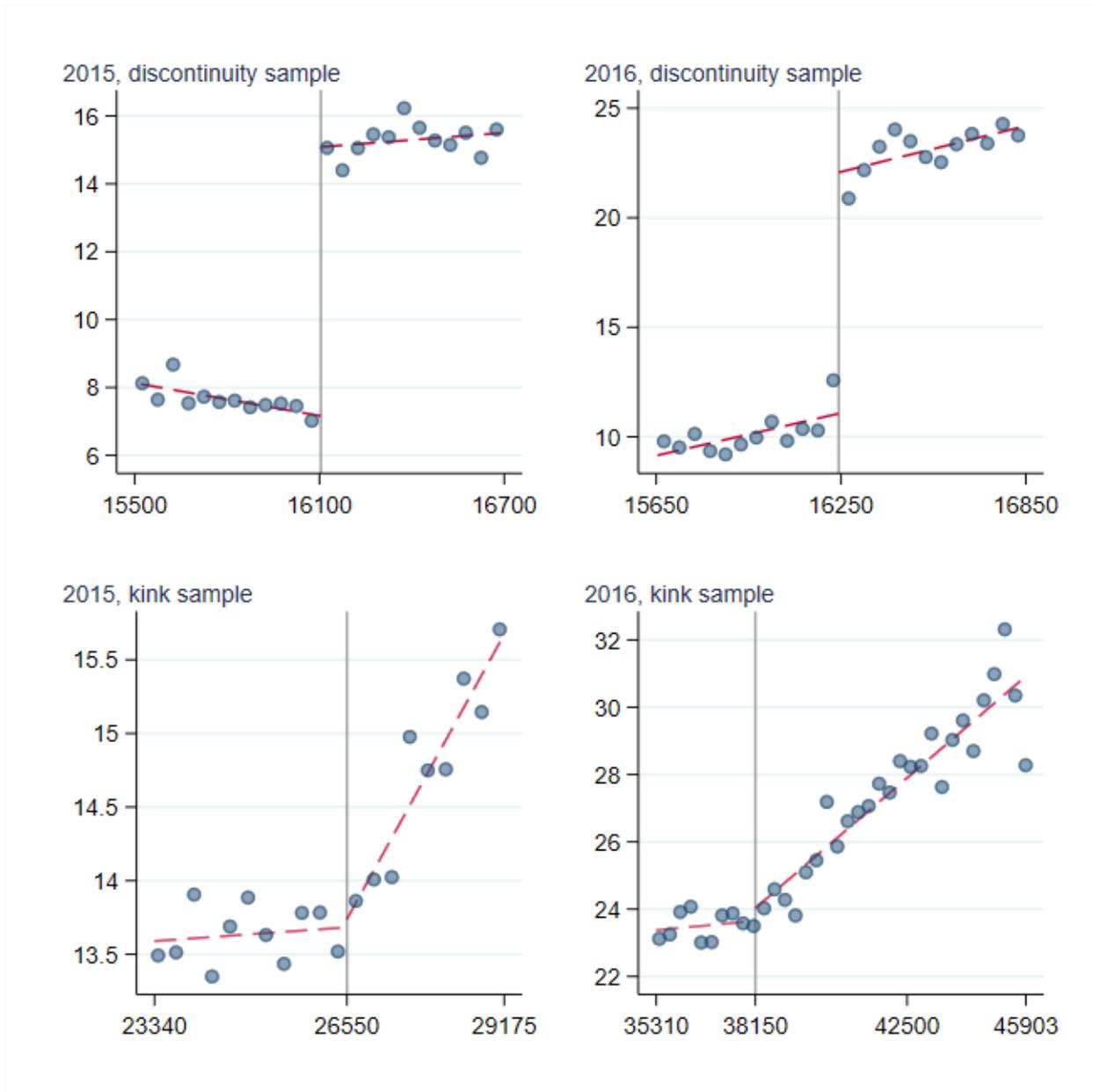
Notes: Each figure shows the number of observations (in 1000s) in each bin of FPL (\$50 bins for the discontinuity sample and \$300 bins for the kink sample). The sample consists of people aged 27-64 in the indicated year, who filed single tax returns with one exemption. The kink sample is further limited to people with no signs of ESI offers. The vertical line shows the mandate discontinuity or kink point.

Figure 3: Smoothness tests, kink sample



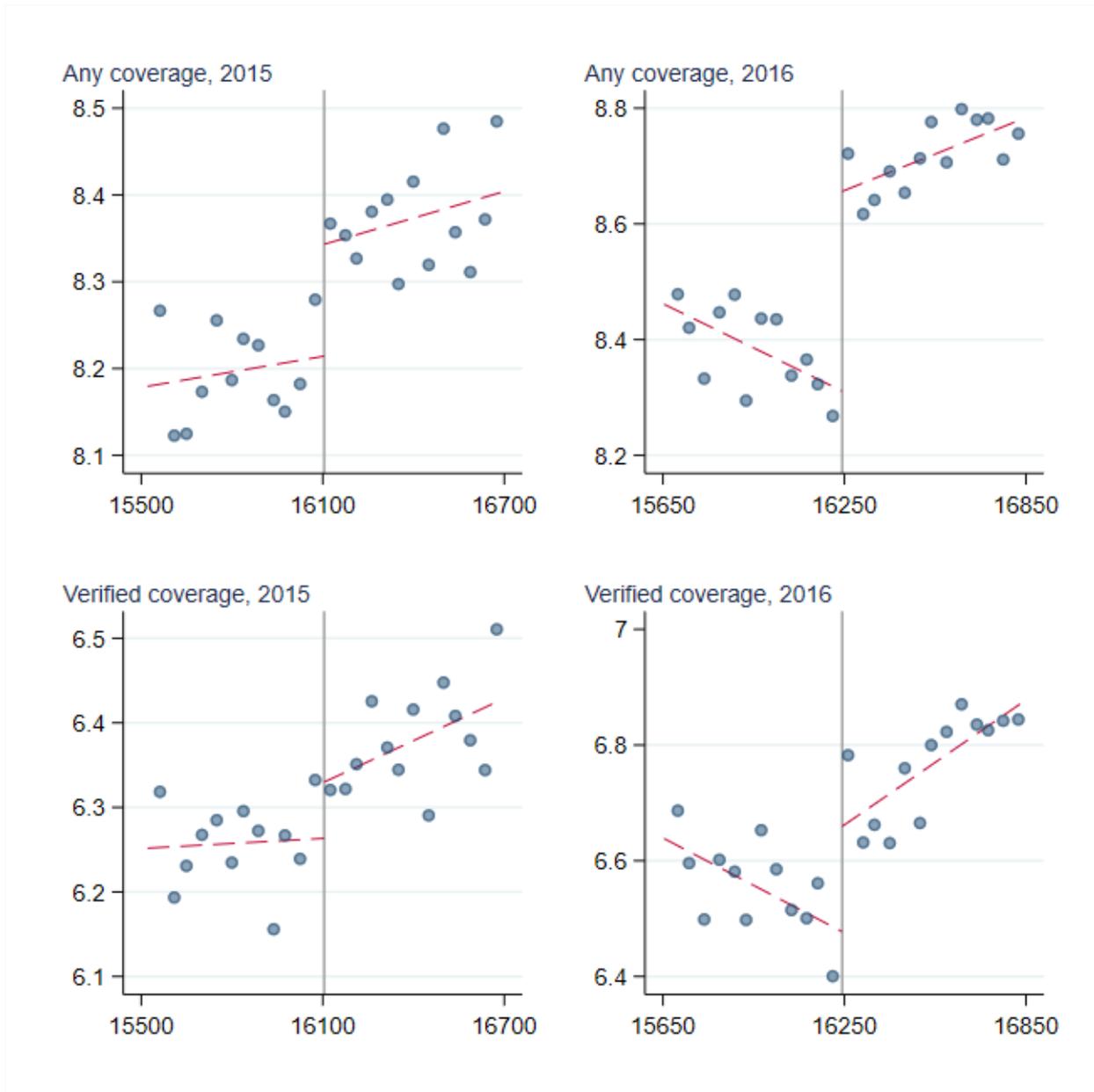
Notes: The top row shows the number of observations in each \$300 bin of MAGI; subsequent remaining rows show bin-level averages of the indicated outcome. The sample consists of people aged 27-64 in the indicated year, who filed single tax returns with one exemption and no signs of ESI offers. The dashed line shows the mandate kink point.

Figure 4: Penalty paid per uninsured month, given income



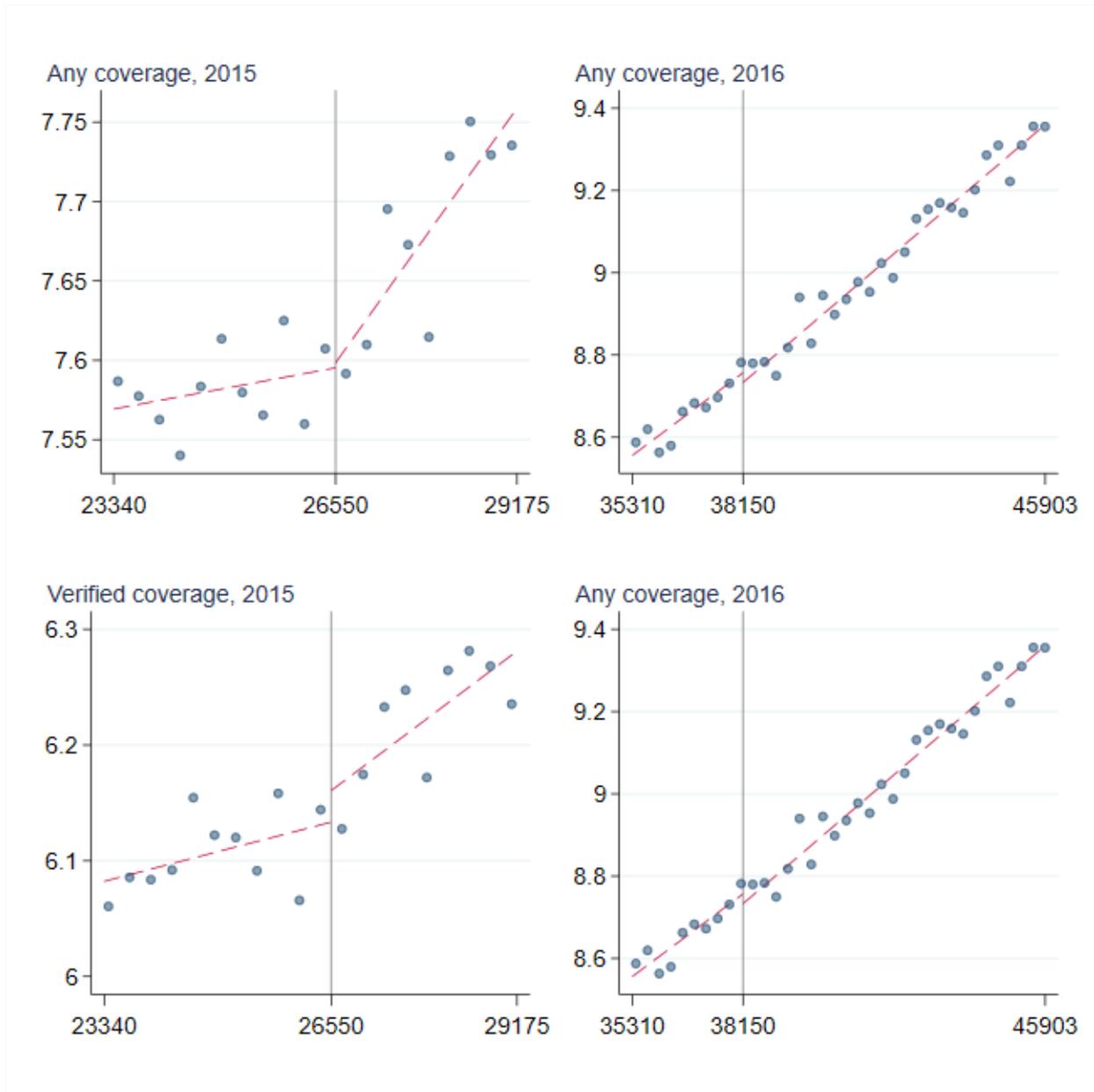
Notes: Figures plots the observed penalty paid per month of uninsurance in each bin of income, in 2015 and 2016, averaging over people with at least one month of insurance. The sample consists of people aged 27-64 in the indicated year, who filed single tax returns with one exemption. The kink sample is further limited to people with no signs of ESI offers.

Figure 5: Months insured, given income, discontinuity sample



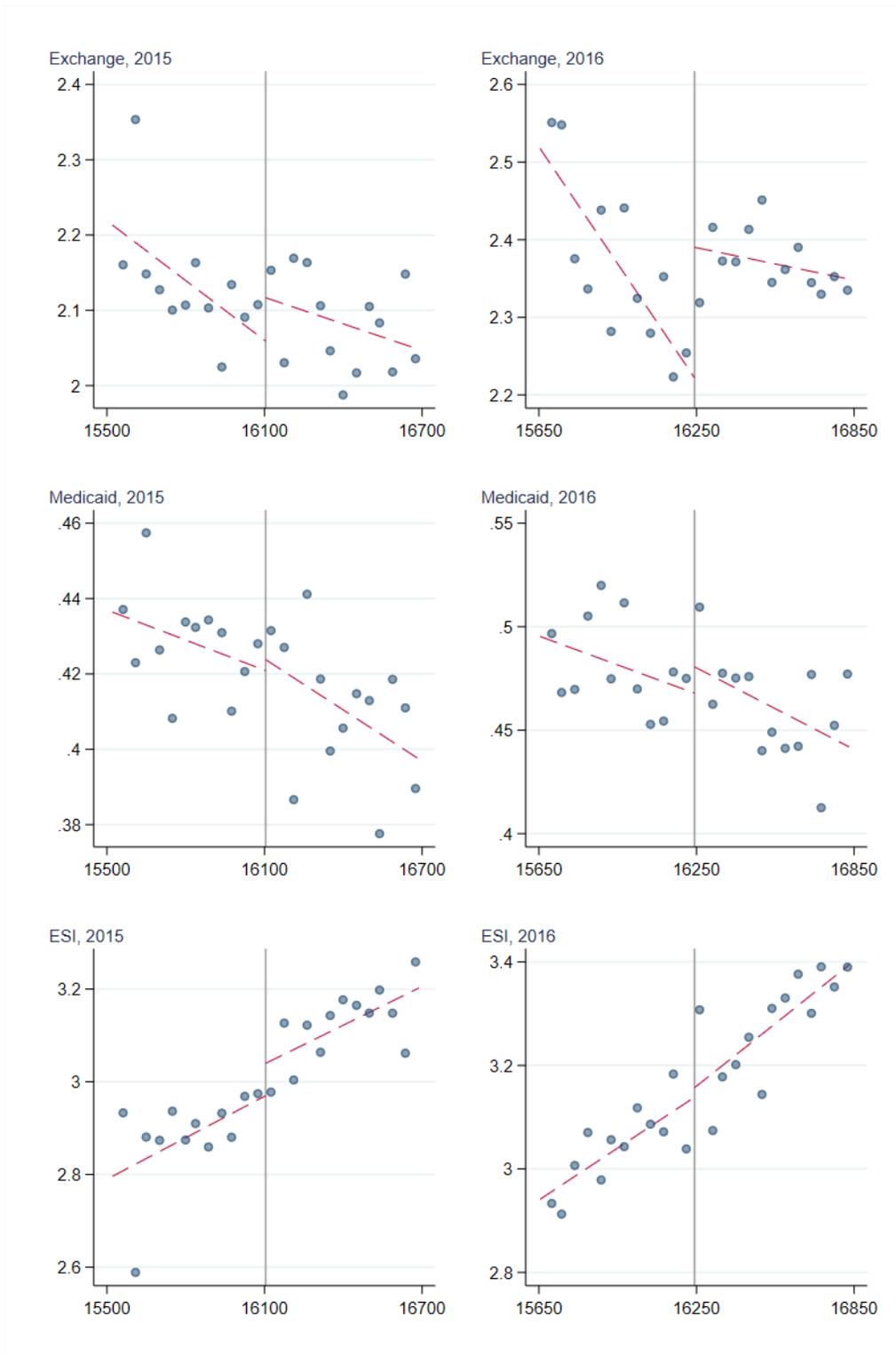
Notes: Figure shows the average number of months insured, in each \$50 bin of modified adjusted gross income. Sample consists of people aged 27-64 in the indicated year, who filed single tax returns with no dependents, in states that did not expand Medicaid. The top row shows months insured using self-reports as well as third-party verified reports. The bottom row shows third-party verified reports only.

Figure 6: Months insured given income, kink sample



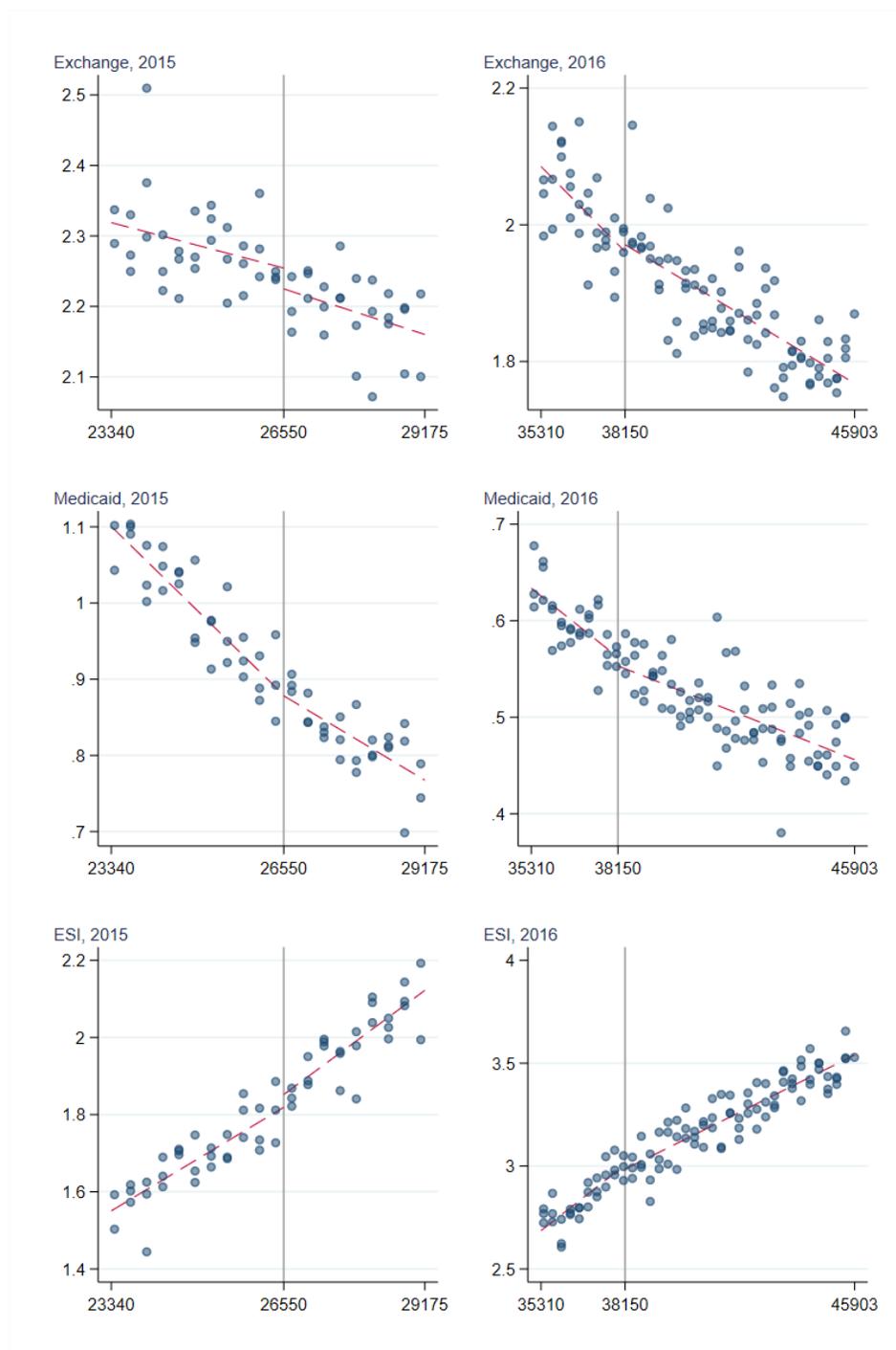
Notes: Figure shows the average number of months insured, in each \$300 bin of modified adjusted gross income. Sample consists of people aged 27-64 in the indicated year, who filed single tax returns with no dependents and no signs of ESI offers. The top row shows months insured using self-reports as well as third-party verified reports. The bottom row shows third-party verified reports only.

Figure 7: Months insured given by type of coverage, discontinuity sample



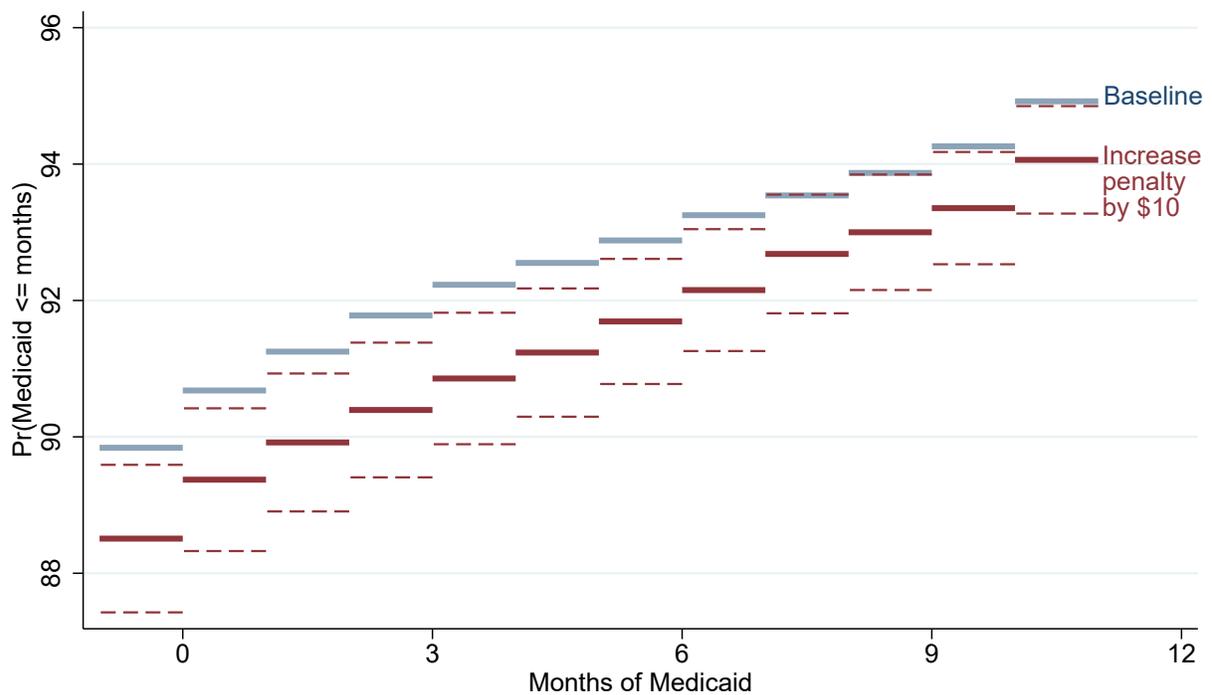
Notes: Figure shows the average number of months insured with the indicated type of insurance, in each \$50 bin of modified adjusted gross income. Sample consists of people aged 27-64 in the indicated year, who filed single tax returns with no dependents, in states that did not expand Medicaid.

Figure 8: Months insured given income, by type of coverage, kink sample



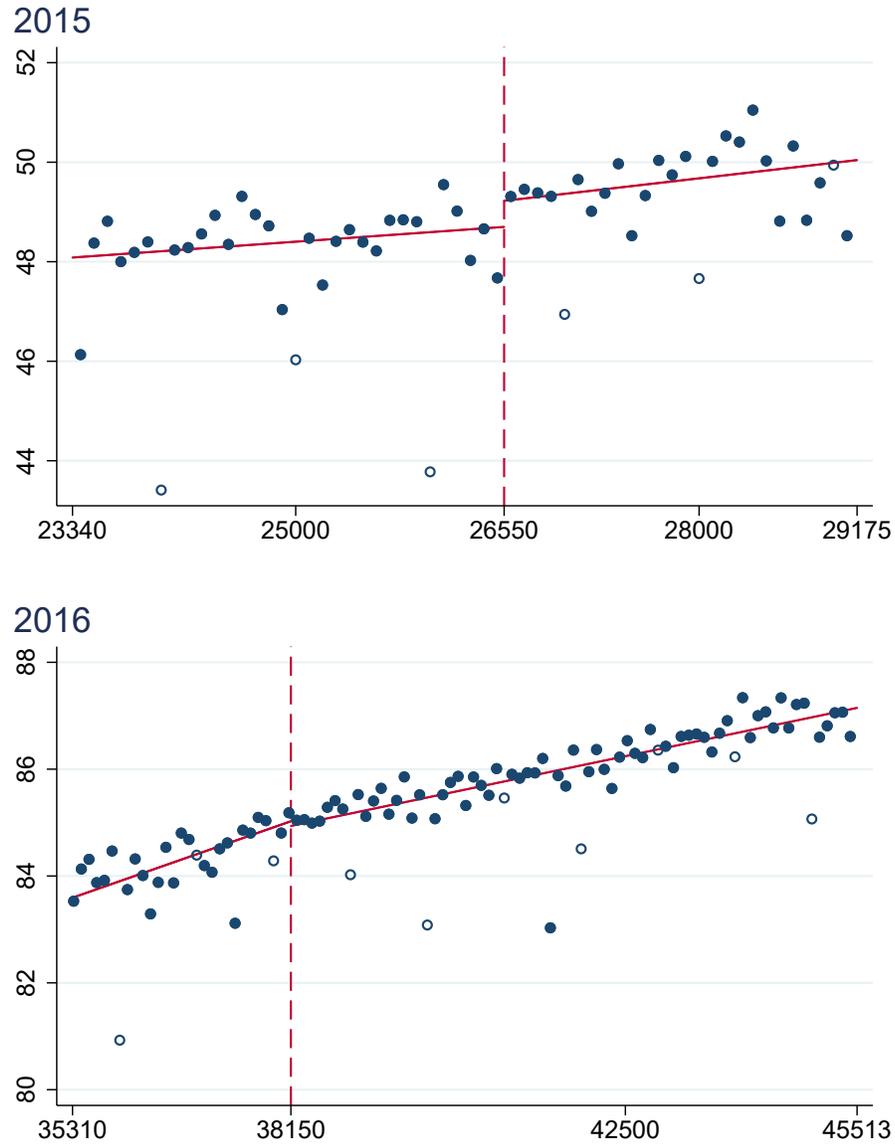
Notes: Figure shows the average number of months insured with the indicated type of insurance, in each \$300 bin of modified adjusted gross income. Sample consists of people aged 27-64 in the indicated year, who filed single tax returns with no dependents no signs of ESI offers.

Figure 9: CDF of months of Medicaid coverage, 2015



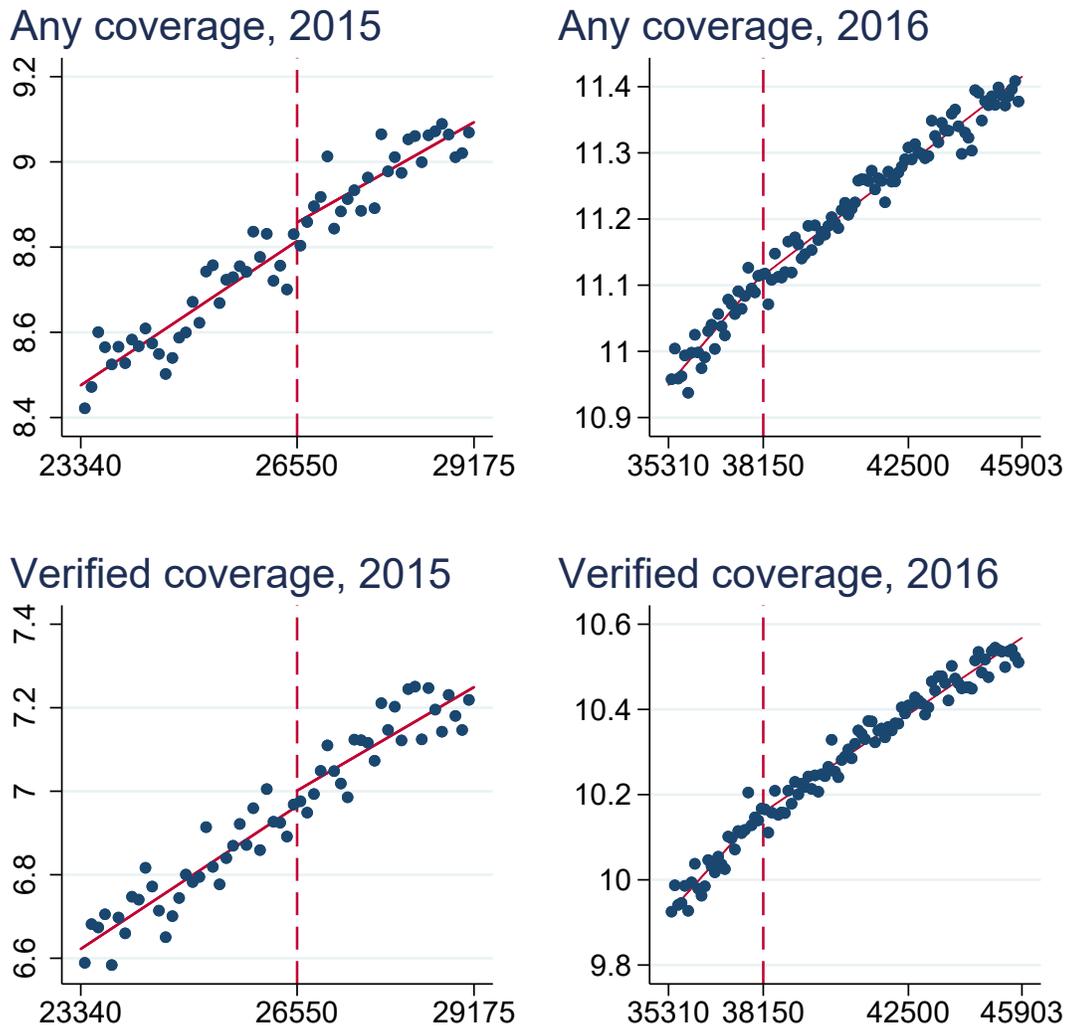
Notes: Source: Figure shows the CDF of months of Medicaid coverage at the 2015 mandate kink point (“baseline”) and the counterfactual CDF induced by a \$10 increase in the monthly mandate penalty, along with the 95% confidence interval.

Figure 10: Probability of sign of ESI offer as a function of income



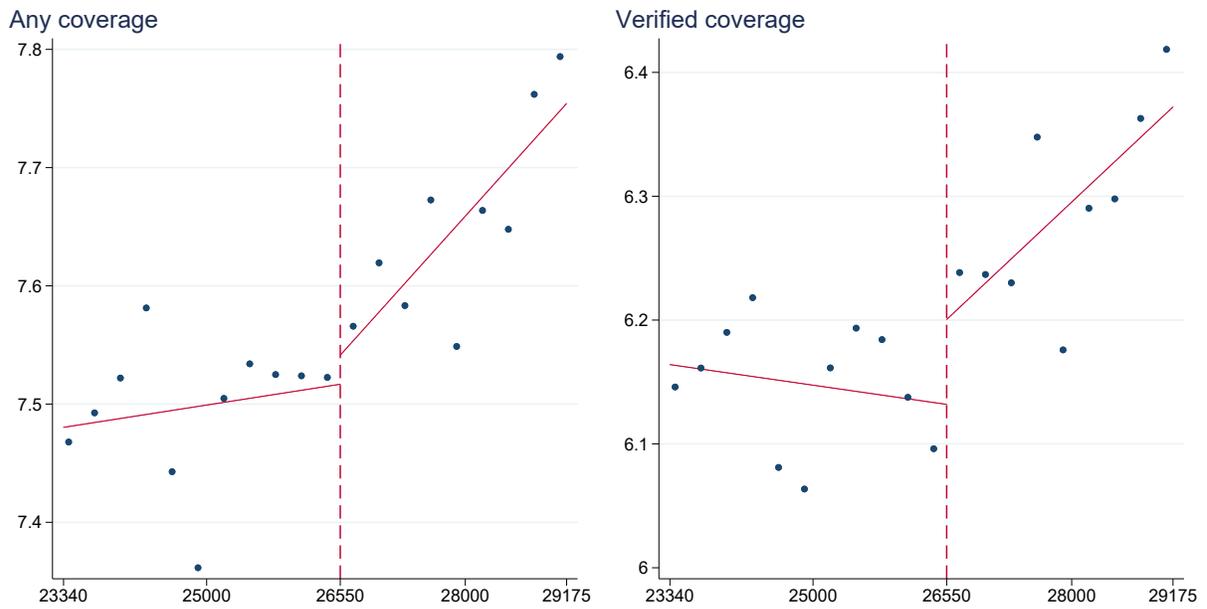
Notes: Figure shows the fraction of people with a sign of ESI offer, in each \$100 bin of modified adjusted gross income. Sample consists of people aged 27-64 in the indicated year, who filed single tax returns with no dependents. The hollow circles indicate round number incomes (\$1000 multiples); such incomes are much more common among the self-employed, who lack signs of an ESI offer. We include dummies for “round number incomes” when estimate RKD models for signs of an ESI offer.

Figure 11: Months insured as a function of income, signs-of-ESI-offer sample



Notes: Figure shows the average number of months insured, in each \$100 bin of modified adjusted gross income. Sample consists of people aged 27-64 in the indicated year, who filed single tax returns with no dependents and no signs of ESI offers. The top row shows months insured using self-reports as well as third-party verified reports. The bottom row shows third-party verified reports only.

Figure 12: Months insured in 2015 as a function of 2014 income



Notes: Figure shows the average number of months insured in 2015, in each \$300 bin of modified adjusted gross income in 2014. Sample consists of people aged 27-64 who filed single tax returns with no dependents and no signs of ESI offers, in 2014.

Table 1: Summary statistics

Sample	Discontinuity		Kink	
	2015 (1)	2016 (2)	2015 (3)	2016 (4)
A. Coverage information				
Months of...				
Any coverage	8.24	8.52	7.62	8.53
Verified coverage	6.28	6.63	6.15	7.06
Exchange	2.10	2.38	2.25	1.93
Off-Exchange	0.44	0.39	0.89	1.29
ESI	2.91	3.07	1.80	3.11
Medicaid	0.44	0.49	0.92	0.54
Medicare	0.36	0.37	0.36	0.28
VA	0.42	0.39	0.33	0.54
Paid penalty	0.15	0.10	0.21	0.13
Penalty per uninsured month	11.01	16.03	14.59	25.92
B. Statistics of income				
10th Percentile	13,400	13,500	23,800	36,125
25th percentile	14,300	14,400	24,600	37,425
Median	15,800	15,900	26,000	39,925
75th percentile	17,200	17,400	27,500	42,625
90th percentile	18,100	18,300	28,500	44,525
Mean	15,773	15,908	26,102	40,135
C. Covariates				
Female	0.46	0.46	0.38	0.36
Age	43.59	43.63	44.73	45.42
Benchmark premium	366.7	363.99	371.21	380.88
Any prior itemized medical expenses			0.12	0.17
Disabled			0.06	0.04
Income prior three years			23,304	35,135
# Observations	1,116,840	1,041,625	653,891	656,064

Notes: Panels A and C report averages of the indicated variables. Panel B reports the indicated statistics of modified adjusted gross income. The sample consists of people who filed single tax returns with one exemption, aged 27-64 in the reference year. The discontinuity sample is limited to people with income between 110-160 percent of FPL in the reference year. The kink sample is further limited to people with no with no signs of ESI offers and income between 200-250 percent of FPL in 2015 or 300-390 percent of FPL in 2016. Income percentiles are percentiles of income in \$100 bins, to avoid disclosing individual taxpayers' income.

Table 2: Smoothness tests

Dep. var.	Female (x100)	Age	Prior income	Any prior itemized expenses	Benchmark Premium	Disabled (x100)
	(1)	(2)	(3)	(4)	(5)	(6)
A. Smoothness tests for 2015 discontinuity sample						
Discontinuity	-0.041 (0.108)	0.005 (0.504)	0.012 (1.388)			
B. Smoothness tests for 2016 discontinuity sample						
Discontinuity	0.006 (0.103)	0.765 (0.470)	1.908 (1.388)			
C. Smoothness tests for 2015 kink sample						
Kink	0.270 (0.148)	0.058 (0.037)	28.1 (74.1)	0.001 (0.001)	0.154 (0.460)	0.028 (0.070)
D. Smoothness tests for 2016 kink sample						
Kink	0.14 (0.13)	0.02 (0.03)	60.5 (71.3)	0.000 (0.001)	0.40 (0.47)	-0.02 (0.06)

The sample consists of single tax returns , with one exemption claimed, aged 27-64. The discontinuity sample is limited to the Calonico et al. (2014) bandwidth, about 5 FPL points around 138 percent of FPL. The kink samples are limited to people without signs of an ESI offer, The 2015 sample is restricted to returns with income between 200 and 250 percent of FPL, and the 2016 sample is restricted to 300 to 390 percent of FPL. Table shows the estimated kink in the indicated outcome, obtained from a regression of the outcome on income, allowing for a kink and discontinuity at the mandate kink point. Robust standard errors in parentheses.

Table 3: Estimated discontinuities and kinks in penalty paid per month of uninsurance

Sample	2015 discontinuity (1)	2016 discontinuity (2)	2015 kink (3)	2016 kink (4)
Discontinuity	7.51 (0.28)	11.85 (0.56)		
Kink			0.75 (0.11)	0.85 (0.19)

The table reports the estimated discontinuity or kink in penalty paid per month of uninsurance, among people with at least 1 month of uninsurance. The sample always consists of single tax returns with one exemption claimed, aged 27-64. The 2015 discontinuity sample is limited to people with income within 4.61 FPL points of 138 percent of FPL (the MSE-optimal bandwidth); the 2016 discontinuity sample is limited to 4.67 FPL points of 138 percent of FPL. The kink samples are limited to people without signs of ESI offer, and further limited to 200-250 percent of FPL (in 2015) or 300-390 percent of FPL (in 2016). Robust standard errors in parentheses.

Table 4: Estimated discontinuities and kinks in months of insurance coverage

Coverage type	Any (1)	Verified (2)	Medicaid (3)	Exchange (4)	Off-Ex (5)	ESI (6)	VA (7)	Medicare (8)
A. 2015 discontinuity sample								
Discontinuity	0.147 (0.059)	0.062 (0.058)	0.003 (0.016)	0.038 (0.041)	-0.066 (0.021)	0.066 (0.049)	0.016 (0.020)	0.013 (0.015)
Statutory semi-elasticity	0.066	0.036	0.029	0.066	-0.557	0.081	0.137	0.151
Observed semi-elasticity	0.236	0.131	0.101	0.227	-1.905	0.279	0.492	0.517
B. 2016 discontinuity sample								
Discontinuity	0.332 (0.062)	0.179 (0.081)	0.018 (0.020)	0.128 (0.047)	-0.021 (0.018)	0.030 (0.071)	-0.031 (0.021)	0.022 (0.020)
Statutory semi-elasticity	0.067	0.047	0.065	0.094	-0.096	0.016	-0.138	0.119
Observed semi-elasticity	0.329	0.228	0.318	0.464	-0.470	0.081	-0.673	0.578
C. 2015 kink sample								
Kink	0.051 (0.017)	0.029 (0.017)	0.023 (0.009)	-0.004 (0.013)	-0.003 (0.010)	0.016 (0.013)	0.010 (0.006)	0.002 (0.006)
Statutory semi-elasticity	0.406	0.279	1.554	-0.111	-0.189	0.528	1.859	0.379
Observed semi-elasticity	0.902	0.620	3.453	-0.247	-0.420	1.173	4.131	0.842
D. 2016 kink sample								
Kink	0.020 (0.015)	-0.002 (0.016)	0.012 (0.007)	0.021 (0.012)	0.007 (0.010)	-0.046 (0.014)	0.001 (0.005)	0.000 (0.005)
Statutory semi-elasticity	0.143	-0.015	1.314	0.639	0.328	-0.923	0.191	0.069
Observed semi-elasticity	0.350	-0.037	3.221	1.566	0.804	-2.262	0.468	0.169

Table reports the estimated kink obtained from a regression of the indicated coverage type on income, allowing for a kink and discontinuity at the mandate kink point. The sample consists of single tax returns with one exemption claimed, aged 27-64. The 2015 discontinuity sample is limited to people with income within 4.61 FPL points of 138 percent of FPL (the MSE-optimal bandwidth); the 2016 discontinuity sample is limited to 4.67 FPL points of 138 percent of FPL. The kink samples are limited to people without signs of ESI offer, and further limited to 200-250 percent of FPL (in 2015) or 300-390 percent of FPL (in 2016). Robust standard errors in parentheses.

Table 5: Kinks in months of insurance coverage, all categories, by Medicaid expansion status

Coverage type	Any		Verified		Medicaid		Exchange		Off-Exchange	
	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Expanded Medicaid?	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
A. Coverage year 2015										
Kink	0.047 (0.028)	0.044 (0.022)	0.012 (0.028)	0.029 (0.022)	0.002 (0.006)	0.033 (0.014)	-0.028 (0.022)	0.008 (0.017)	0.018 (0.015)	-0.018 (0.012)
P-value (difference)	0.934		0.643		0.048		0.193		0.074	
B. Coverage year 2016										
Kink	0.020 (0.026)	0.025 (0.018)	-0.006 (0.027)	0.005 (0.020)	0.006 (0.006)	0.017 (0.010)	0.047 (0.020)	0.006 (0.015)	0.018 (0.016)	-0.001 (0.012)
P-value (difference)	0.889		0.746		0.339		0.107		0.349	

Table reports the estimated kink obtained from a regression of the indicated coverage type on income, allowing for a kink and discontinuity at the mandate kink point, estimated separately by whether the state of residence has expanded Medicaid by the indicated coverage year. The sample consists of single tax returns in 2015 (Panel A) or 2016 (Panel B) with one exemption claimed and no signs of ESI offers, aged 27-64, with income between 200 and 250 percent of FPL (in 2015), or 300 and 390 percent of FPL (in 2016). Robust standard errors in parentheses. The reported p-value is the p-value of the hypothesis that the kinks are the same for the expansion and non-expansion states.

Table 6: Kinks in months of insurance coverage, all categories, signs-of-ESI offer sample

Coverage type	Any (1)	Verified (2)	Medicaid (3)	Exchange (4)	Off-Ex (5)	ESI (6)	VA (7)	Medicare (8)
A. 2015								
Kink	-0.020 (0.007)	-0.028 (0.008)	0.028 (0.004)	-0.000 (0.004)	-0.000 (0.003)	-0.059 (0.009)	0.000 (0.003)	0.003 (0.002)
Semi el	-0.117	-0.193	2.056	-0.035	-0.022	-0.508	0.050	2.410
# Observations	2,301,925	2,301,925	2,301,925	2,301,925	2,301,925	2,301,925	2,301,925	2,301,925
B. 2016								
Kink	-0.022 (0.004)	-0.036 (0.005)	0.012 (0.002)	0.005 (0.002)	0.005 (0.002)	-0.055 (0.006)	-0.000 (0.002)	0.001 (0.001)
Semi el	-0.121	-0.212	1.926	0.807	1.262	-0.358	-0.080	0.659
# Observations	3,841,362	3,841,362	3,841,362	3,841,362	3,841,362	3,841,362	3,841,362	3,841,362

Table reports the estimated kink obtained from a regression of the indicated coverage type on income, allowing for a kink and discontinuity at the mandate kink point. The sample consists of single tax returns in 2015 (Panel A) or 2016 (Panel B) with one exemption claimed and a sign of an ESI offer, aged 27-64, with income between 200 and 250 percent of FPL (in 2015), or 300 and 390 percent of FPL (in 2016). Robust standard errors in parentheses.

Table 7: Heterogeneous responses to the mandate penalty, discontinuity sample

	# in group (1000s) (1)	Months of coverage (2)	Coverage discontinuity (3)
<u>A. Split on sex</u>			
Male	392	7.94	0.304 (0.035)
Female	339	9.16	0.161 (0.036)
p-value of diff			0.005
<u>B. Split on age</u>			
Below median	356	7.87	0.310 (0.041)
Above median	376	9.10	0.174 (0.039)
p-value of diff			0.017

Notes: Table reports the estimated discontinuity in months of coverage of the indicated type, for the indicated groups, as well as the group size and average coverage rate (for months of any coverage). Sample consists of people aged 27-64 in 2015 or 2016, with income between 133 and 150 percent of FPL, who filed single tax returns with one exemption in non-expansion states. Standard error clustered on income-by-type (e.g. male/female) in parentheses.

Table 8: Heterogeneous responses to the mandate penalty, 2015 kink sample

	# in group (1000s) (1)	Months of coverage (2)	Coverage kink (3)
<u>A. Split on sex</u>			
Male	407	7.02	0.063 (0.022)
Female	247	8.61	0.021 (0.026)
p-value of difference			0.211
<u>B. Split on age</u>			
Below median	318	6.49	0.068 (0.025)
Above median	336	8.69	0.027 (0.022)
p-value of difference			0.228
<u>C. Split on prior itemized medical expenses</u>			
None	574	7.26	0.066 (0.019)
Some	80	10.22	-0.066 (0.036)
p-value of difference			0.001
<u>D. Split on disability insurance income</u>			
None	617	7.48	0.054 (0.018)
Some	37	10.05	-0.009 (0.056)
p-value of difference			0.291
<u>E. Split on prior income</u>			
Below median	325	7.01	0.121 (0.026)
Above median	329	8.23	0.028 (0.023)
p-value of difference			0.007

Notes: Table reports the estimated kink in months of coverage of the indicated type, for the indicated groups, as well as the group size and average coverage rate (for months of any coverage). Robust standard errors in parentheses. Sample consists of people aged 27-64 in 2015, with income between 200 and 250 percent of FPL, who filed single tax returns with one exemption and no signs of ESI offers.

Table 9: Kinks in 2015 penalty and coverage as a function of 2014 income

Dep. Var.	Penalty (\$1000s) (1)	Months insured by coverage type						
		Any (2)	Verified (3)	Medicaid (4)	Exchange (5)	Off-Ex (6)	ESI (7)	VA (8)
Kink	0.009 (0.002)	0.056 (0.018)	0.056 (0.008)	0.013 (0.080)	0.009 (0.014)	0.021 (0.010)	0.009 (0.014)	0.006 (0.006)
Statutory semi-elasticity:								
Naive forecast		0.448	0.548	1.032	0.244	1.364	0.278	1.157
Rational expectations		0.996	1.218	2.294	0.542	3.031	0.618	2.571

Table reports the estimated kink obtained from a regression of the indicated dependent variable, measured in 2015, on 2014 income, allowing for a kink and discontinuity at the mandate kink point. The sample consists of single tax returns in 2014, with 2014 taxable income between 200 and 250 percent of FPL, and with one exemption claimed in 2015 and no signs of ESI offers 2014, aged 27-64. Robust standard errors in parentheses. The naive forecast semi-elasticity is the mandate kink scaled up by 1/.02, and divided by average months of coverage at the mandate kink point. The rational expectations semi-elasticity instead scaled by 1/.009, the kink in 2015 penalty given 2014 income.

Table 10: Simulated direct effect of setting mandate penalty to zero

	Coverage Rate	Penalty (\$/month)	Percentage point change in coverage	Percent change in coverage
	(1)	(2)	(3)	(4)
A. Overall coverage response (discontinuity sample)				
2015 estimates	0.69	29.2	-0.01	-0.01
2016 sample	0.71	29.0	-0.01	-0.02
B. Exchange coverage response (discontinuity sample)				
2015 sample	0.17	29.2	-0.00	-0.01
2016 sample	0.20	29.0	-0.01	-0.03
C. Overall coverage response (kink sample)				
2015 sample	0.64	57.9	-0.15	-0.24
2016 sample	0.71	63.1	-0.05	-0.07
Overall average	0.67	60.5	-0.10	-0.15
Density-weighted average	0.66	59.8	-0.11	-0.17
D. Exchange coverage response (kink sample)				
2015 sample	0.19	57.9	0.01	0.05
2016 sample	0.16	63.1	-0.05	-0.31
Overall average	0.17	60.5	-0.02	-0.12
Density-weighted	0.18	59.8	-0.01	-0.06

Table reports the coverage rate (defined as average months of any coverage/12 in Panels A and B, and average months of Exchange coverage/12 in Panel C and D), the average penalty amount, and the implied percentage point drop and percent drop in coverage, if the mandate penalty were set to zero. We report estimates for our 2015 and 2016 kink and discontinuity samples, using the estimated penalty sensitivity for each sample. We also report overall average across the two samples, and a “density-weighted average.” This weighted average weights by the density of the distribution (653,891 / 50 FPL points in 2015 and 656,064 / 90 FPL points in 2016) rather than the sample size.

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A Further details on the mandate penalty

We restrict our sample to single, childless adults because the mandate penalty is more complicated for larger families. Here we describe these complications. Recall from Section 1 that the monthly mandate penalty is

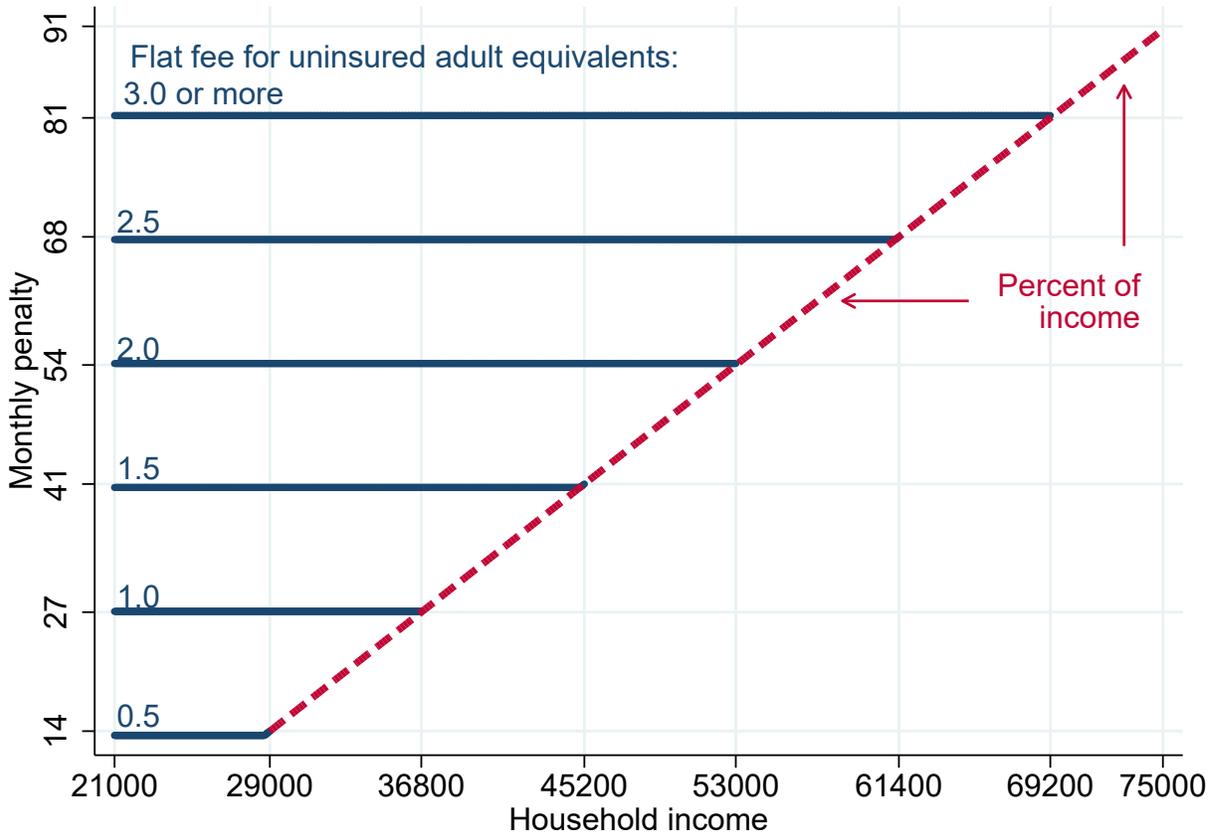
$$Penalty = 1/12 \max \{ \min \{ [A + .5C]F, 3F \}, S(\text{MAGI} - \text{tax filing threshold}) \},$$

where F is the flat fee, A the number of uninsured adults that month, C the number of uninsured children, and S the percent of income. In 2016, F was \$325 and S was 0.02. For a given filing threshold, the penalty is therefore a function of income and the number of uninsured adults and children. Each child counts as half an adult for the purposes of determining the mandate penalty so we refer to the number of uninsured “adult equivalents”, equal to $A + .5C$. Note that the number of uninsured adult equivalents affects the flat fee (the first term in brackets) but not the percent of income (the second term).

Appendix Figure A.1 plots the monthly mandate penalty in 2015 as a function of income and the number of uninsured adult equivalents, for a married, filing jointly tax return (which has a filing threshold of \$20,600). There are six kink points, equal to each intersection of $F(A + .5C)$ with the percent of income, for $A + .5C \in \{0.5, 1, 1.5, 2, 2.5\}$. At each of the kink points, the mandate penalty increases for some margins of coverage, but not for all margins. For example, consider a household with two adults and two children, and income of \$68,000. For this household, the percent of income payment is \$79. If there were one uninsured household member, the penalty would be \$79, because the percent of income exceeds the flat fee. A second uninsured household member would not increase the penalty because the percent of income exceeds the flat fee for two uninsured members. Only if the entire household were uninsured would the flat fee, \$81.25, exceed the percent of income. However, if the household’s income were \$70,000, the percent of income would always exceed the flat fee, regardless of the number of uninsured adult equivalents. Thus there is a kink in the incentive to have the entire family covered, but no kink in the marginal incentive to cover the first, second, or third family member.

This example shows that, for multiperson households, the mandate penalty creates complex and fairly subtle incentives to increase coverage. In principle it is possible to examine coverage responses at each of the six coverage kinks, focusing on the relevant margin of coverage (e.g. a kink in the probability of having three or more uninsured adult equivalents at \$69,200). In practice we are concerned that households may not understand the specific incentives for monthly coverage generated by the individual mandate. We therefore focus on single households. For these households the penalty is relatively simple—it is linear in their number of uninsured months.

Figure A.1: Monthly mandate penalty for multi-person households, 2015



Notes: Source: Figure shows the monthly mandate penalty in 2015 as a function of income and the number of uninsured adult equivalents, for a household with married filing jointly tax return. The number of uninsured adult equivalents is the number of uninsured adults plus half the number of uninsured children. For some income levels and numbers of uninsured adult equivalents, insuring an additional adult equivalent does not change the mandate penalty, because the percent of income penalty is the same for any number of uninsured adult equivalents.

B Monte Carlo Study of RKD Estimators

We conducted a Monte Carlo simulation study to assess the performance of alternative RKD estimators. The canonical RKD specification is

$$y_i = \sum_{d=0}^D \alpha_d (v_i - c)^d + \sum_{d=0}^D \beta_d (v_i - c)^d \times D_i + \varepsilon_i,$$

where v_i is the running variable, D_i is indicator for the running variable exceeding the cutoff, and $\hat{\beta}_1$ is the kink estimate (Card et al., 2015). Our estimating equation is identical to this, with $D = 1$. In estimating a regression kink design, researchers must make several specification choices: the choice of degree D , the bandwidth h , the kernel, and whether to allow for a discontinuity.

The theoretical econometric literature recommends using a triangular kernel for boundary estimation problems such as this one. For estimating a kink, the theoretical literature also recommends $D \geq 2$, and it has developed plug-in estimators for bandwidth choice based on minimizing asymptotic mean squared error of the kink estimate (Imbens and Kalyanaraman, 2012; Calonico et al., 2014). However applied researchers have favored the uniform kernel—as the regression can then be estimated with OLS—and have found that high degree terms and asymptotically optimal bandwidths do not necessarily perform well in finite samples. Applied researchers also sometimes impose continuity (i.e. dropping the $(v_i - c)^0 D_i$ term).

To determine our baseline specification choices, we conducted a Monte Carlo following the suggestions in Card et al. (2017). The overall idea is to simulate many data sets using a data generating process that closely resembles our data, and then compare the performance of alternative RKD estimators across the data sets. Our data generating process is based on a high-order polynomial approximation to the data, with a true kink imposed. To do so, we first “dekink” the data by estimating the following regression, separately for 2015 and 2016:

$$y_i - \hat{\tau}_t D_i v_i = \sum_{d=0}^5 \beta_d v_i^d + \sum_{d \neq 1}^5 \theta_d v_i^d D_i + \epsilon_i. \quad (7)$$

where $\hat{\tau}_t$ is the estimated kink in year t , 0.05 in 2015 and 0.02 in 2016. Let $\hat{y}(v)$ be the predicted value from this regression when the running variable is v .

We simulate data with a known kink τ . We consider two cases: τ corresponding to a semi-elasticity of 0.5, which we consider to be the middle of past estimates, and τ corresponding to a semi elasticity of 0.2, which is low but consistent with the Massachusetts evidence. For each year and each of 1000 simulation data sets, we sampling with replacement from the empirical distribution of v and ϵ in that year. Each simulation dataset is the same size as our estimation dataset. Given the draw of v and ϵ , we form the outcome y as $\hat{y}(v) + \epsilon + D\tau v$, where τ is the assumed kink. We then estimate several different RKD specifications on the simulated data. For each simulated data set, we considered the power set of the following specification choices: bandwidth equal to the full range of in-

come, the Fan-Gijbels bandwidth selector (as proposed by Card et al. (2015)), or Calonico et al. (2014) bandwidth selector (without scale regularization); polynomial degree $D = 1$ or $D = 2$; and imposing continuity or not. Throughout we use a uniform kernel, for consistency with the applied literature. We do not consider the bias-corrected estimator of Calonico et al. (2014) because it is computationally costly and initial simulations suggested that it lead to dramatically higher variance without large reductions in bias or improvements in coverage rates (a result also reported by Card et al. (2017)).

Appendix Table B.1 summarizes the performance of the various estimators in the 2015 sample. The linear estimator performs well: it achieves its nominal coverage rate, and rejects a false null 96-97 percent of the time. The Fan-Gijbels and CCT bandwidth selectors choose fairly small bandwidths, \$1,248 to \$1,806, relative to a maximal bandwidth of about \$2,900. Relative to using the full range of the data, they have a higher RMSE and a lower rejection rate; the coverage rate is slightly higher for the discontinuous estimator and slightly lower for the continuous estimator. The linear estimator using the full range of the data has the lowest RMSE. The estimators that use only relatively local information give up some power without reducing bias. Allowing for a discontinuity results in slightly higher bias and variance. The quadratic estimators perform substantially worse than the linear estimators: they have higher (absolute) bias, much higher variance, and worse coverage.²³ We conclude that the linear estimator using the full range of data is likely to perform better than the alternatives, although none of the estimators achieves the nominal coverage rate, and this estimator has the worst coverage.

Appendix Table B.2 summarizes the performance of the estimators in the 2016 sample. Here too we find that the linear specification using the full range of the data has the lowest mean squared error, again with somewhat higher confidence intervals. In this case the coverage rate of the linear estimator is below the nominal rate when we impose continuity.

Because our 2016 estimates were statistically insignificant, we also investigated the power of our estimator to detect small kinks. Specifically, we re-ran our Monte Carlo simulations, but assuming a semi-elasticity of 0.2 instead of 0.5, and assuming a semi-elasticity of 0.14. The 0.2 semi-elasticity corresponds to the estimate that Hackmann et al. (2015) find using the Massachusetts mandate. They look at a sample of relatively high income adults, with income above 300 percent of FPL, so we believe this is a useful benchmark. We report the results of this simulation in Appendix Tables B.3 and B.4. The semi-elasticity of 0.14 corresponds to our main estimate. Consistent with our other simulation results, we find that the linear estimator using the full range of data outperforms estimators with higher order terms or tighter bandwidths. However, even for this estimator, we find somewhat limited power. When we do not impose continuity, we reject a false null in only 73 percent of iterations. Imposing continuity improves power. At the smallest semi-elasticity we considered, 0.14, we find limited power even when imposing continuity; we reject the false null in 74 percent of iterations. Without continuity we reject in less than half of all iterations.

²³The FG bandwidth usually ends up exceeding the range of data in the quadratic case, so its performance is the same as the estimator using the full range of data.

Table B.1: Summary of performance of RKD estimators in Monte Carlo Simulation, 2015

Estimator	Median Bandwidth (1)	$\frac{RMSE}{\tau}$ (2)	Coverage Rate (3)	$\frac{Bias}{\tau}$ (4)	$\frac{Variance}{\tau^2}$ (5)	Rejection Rate (6)
A. Linear estimators						
BW = full, continuous	–	0.261	0.958	0.001	0.261	0.971
BW = FG, continuous	1806	0.339	0.953	0.005	0.339	0.857
BW = CCT, continuous	1248	0.428	0.939	0.012	0.428	0.697
BW = full, discontinuous	1	0.267	0.951	0.005	0.267	0.963
BW = FG, discontinuous	1806	0.347	0.956	0.009	0.347	0.844
BW = CCT, discontinuous	1248	0.436	0.936	0.019	0.436	0.686
B. Quadratic estimators						
BW = full, continuous	–	1.043	0.900	0.780	1.043	0.393
BW = FG, continuous	4787	1.043	0.899	0.779	1.043	0.393
BW = CCT, continuous	1462	1.519	0.922	0.802	1.519	0.233
BW = full, discontinuous	–	1.071	0.891	0.839	1.071	0.408
BW = FG, discontinuous	4787	1.071	0.891	0.839	1.071	0.408
BW = CCT, discontinuous	1462	1.560	0.916	0.870	1.560	0.247

Table summarizes the performance of 12 RKD estimators, which differ in the degree of the underlying polynomial (linear or quadratic), bandwidth selector (full range of data, Fan-Gijbels, or Calonico et al. (2014)), and whether a discontinuity is imposed. The data are generated using a true kink of $\tau = 6.33 \times 10^{-5}$, corresponding to a semi-elasticity of 0.5 at the mandate kink point. The coverage rate is the fraction of confidence intervals containing this kink.

Table B.2: Summary of performance of RKD estimators in Monte Carlo Simulation, 2016

Estimator	Median Bandwidth (1)	$\frac{RMSE}{\tau}$ (2)	Coverage Rate (3)	$\frac{Bias}{\tau}$ (4)	$\frac{Variance}{\tau^2}$ (5)	Rejection Rate (6)
A. Linear estimators						
BW = full, continuous	–	0.133	0.921	0.068	0.133	1.000
BW = FG, continuous	2308	0.184	0.949	0.066	0.184	1.000
BW = CCT, continuous	1304	0.255	0.945	0.066	0.255	0.963
BW = full, discontinuous	–	0.164	0.950	0.035	0.164	0.999
BW = FG, discontinuous	2308	0.234	0.947	0.044	0.234	0.987
BW = CCT, discontinuous	1304	0.329	0.947	0.043	0.329	0.881
B. Quadratic estimators						
BW = full, continuous	–	0.544	0.778	-0.625	0.544	0.109
BW = FG, continuous	7598	0.557	0.797	-0.624	0.557	0.109
BW = CCT, continuous	1649	0.964	0.890	-0.632	0.964	0.066
BW = full, discontinuous	–	0.666	0.721	-0.929	0.666	0.039
BW = FG, discontinuous	7598	0.681	0.754	-0.929	0.681	0.037
BW = CCT, discontinuous	1649	1.186	0.866	-0.892	1.186	0.030

Table summarizes the performance of 12 RKD estimators, which differ in the degree of the underlying polynomial (linear or quadratic), bandwidth selector (full range of data, largest symmetric band, Fan-Gijbels, or Calonico et al. (2014)), and whether a discontinuity is imposed. The data are generated using a true kink of $\tau = 8.75 \times 10^{-5}$, corresponding to a semi-elasticity of 0.5 at the mandate kink point. The coverage rate is the fraction of confidence intervals containing this kink.

Table B.3: Summary of performance of RKD estimators in Monte Carlo Simulation, 2016, assuming low semi-elasticity

Estimator	Median Bandwidth (1)	$\frac{RMSE}{\tau}$ (2)	Coverage Rate (3)	$\frac{Bias}{\tau}$ (4)	$\frac{Variance}{\tau^2}$ (5)	Rejection Rate (6)
A. Linear estimators						
BW = full, continuous	–	0.332	0.921	0.171	0.332	0.946
BW = FG, continuous	2308	0.461	0.949	0.166	0.461	0.698
BW = CCT, continuous	1304	0.639	0.945	0.166	0.639	0.445
BW = full, discontinuous	–	0.410	0.950	0.088	0.410	0.731
BW = FG, discontinuous	2308	0.584	0.947	0.109	0.584	0.467
BW = CCT, discontinuous	1304	0.822	0.947	0.107	0.822	0.288
B. Quadratic estimators						
BW = full, continuous	–	1.361	0.778	-1.562	1.361	0.008
BW = FG, continuous	7598	1.392	0.797	-1.559	1.392	0.008
BW = CCT, continuous	1649	2.410	0.890	-1.580	2.410	0.019
BW = full, discontinuous	–	1.665	0.721	-2.324	1.665	0.002
BW = FG, discontinuous	7598	1.703	0.754	-2.322	1.703	0.001
BW = CCT, discontinuous	1649	2.965	0.866	-2.229	2.965	0.011

Table summarizes the performance of 12 RKD estimators, which differ in the degree of the underlying polynomial (linear or quadratic), bandwidth selector (full range of data, largest symmetric band, Fan-Gjebels, or Calonico et al. (2014)), and whether a discontinuity is imposed. The data are generated using a true kink of $\tau = 3.5 \times 10^{-5}$, corresponding to a semi-elasticity of 0.2 at the mandate kink point. The coverage rate is the fraction of confidence intervals containing this kink, and the rejection rate is the fraction of confidence intervals that exclude zero.

Table B.4: Summary of performance of RKD estimators in Monte Carlo Simulation, 2016, assuming very low semi-elasticity

Estimator	Median Bandwidth (1)	$\frac{RMSE}{\tau}$ (2)	Coverage Rate (3)	$\frac{Bias}{\tau}$ (4)	$\frac{Variance}{\tau^2}$ (5)	Rejection Rate (6)
A. Linear estimators						
BW = full, continuous	–	0.474	0.921	0.244	0.474	0.731
BW = FG, continuous	2308	0.658	0.949	0.237	0.658	0.448
BW = CCT, continuous	1304	0.912	0.945	0.237	0.912	0.283
BW = full, discontinuous	–	0.586	0.950	0.126	0.586	0.475
BW = FG, discontinuous	2308	0.834	0.947	0.156	0.834	0.281
BW = CCT, discontinuous	1304	1.174	0.947	0.153	1.174	0.183
B. Quadratic estimators						
BW = full, continuous	–	1.944	0.778	-2.231	1.944	0.006
BW = FG, continuous	7598	1.988	0.797	-2.227	1.988	0.007
BW = CCT, continuous	1649	3.443	0.890	-2.257	3.443	0.014
BW = full, discontinuous	–	2.379	0.721	-3.320	2.379	0.000
BW = FG, discontinuous	7598	2.432	0.754	-3.317	2.432	0.000
BW = CCT, discontinuous	1649	4.235	0.866	-3.184	4.235	0.006

Table summarizes the performance of 12 RKD estimators, which differ in the degree of the underlying polynomial (linear or quadratic), bandwidth selector (full range of data, largest symmetric band, Fan-Gjebels, or Calonico et al. (2014)), and whether a discontinuity is imposed. The data are generated using a true kink of $\tau = 3.5 \times 10^{-5}$, corresponding to a semi-elasticity of 0.2 at the mandate kink point. The coverage rate is the fraction of confidence intervals containing this kink, and the rejection rate is the fraction of confidence intervals that exclude zero.

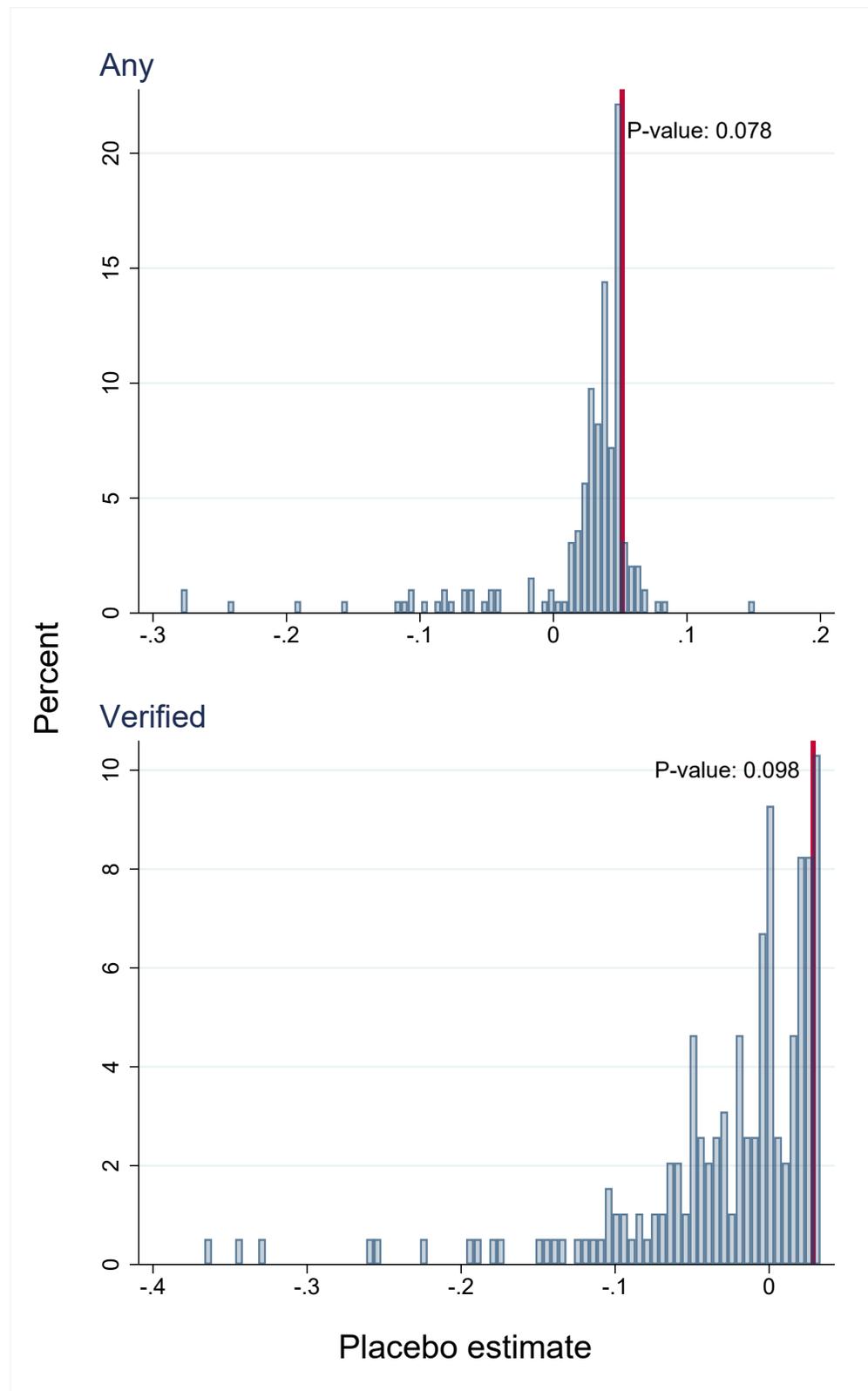
C Permutation tests

We have found clear kinks in months of any insurance coverage and months of verified coverage in 2015. One concern with the regression kink approach, however, is that it may detect spurious kinks, simply due to curvature in the relationship between the outcome and running variable (Ganong and Jäger, 2017). We assess this concern by re-estimating our RKD models, but varying the kink point across a fine grid of placebo locations. If the kink is spurious, then we expect that our estimate is unexceptional in the distribution of placebo estimates.

Figure C.1 shows the distribution of placebo kink estimates, for any coverage and for verified coverage. We consider permutation kinks every \$25, starting from \$500 above 200 percent of FPL, and ending at \$500 below 250 percent of FPL. We look in this range because we do not believe looking elsewhere in the income distribution would be informative about the possibility of a false positive at our income level. There are likely to be other policy-induced kinks elsewhere in the income distribution (for example, because of the PTC). We exclude kink points near the boundaries because estimating a kink near the boundary produces very large, very noisy estimates, because there is very little data with which to estimate a slope on one side of the kink.

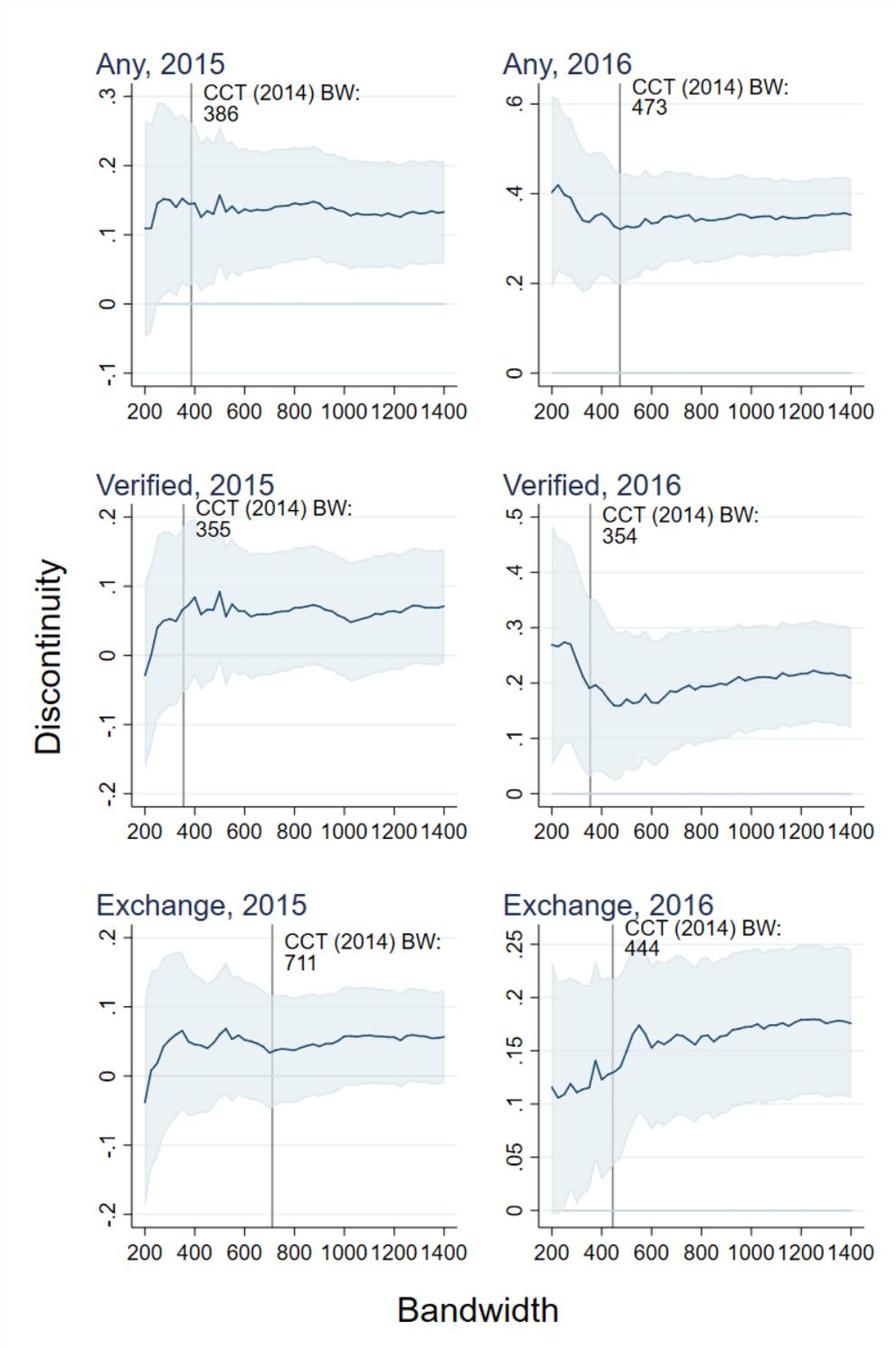
The histograms show, first, a long left tail of placebo kink points. This is generated by the fact that placebo kink locations near the boundaries tend to produce large, negative placebo estimates. Second, the estimated kink, shown with the vertical line, is larger than all but a handful of the placebo kinks. The implied p-value—the fraction of placebo point estimates that exceed the true point estimate—is 0.078 for any coverage and 0.098 for verified coverage. The reader may worry that these p-values are small in part because of the inclusion of the many very negative placebo kink points estimated near the boundary. If we instead estimate p-values, but excluding placebo kink points within \$1000 of the boundary, we obtain p-values of 0.058 for any coverage and 0.118 for verified coverage.

Figure C.1: Estimated kink in months insured at placebo kink points, 2015



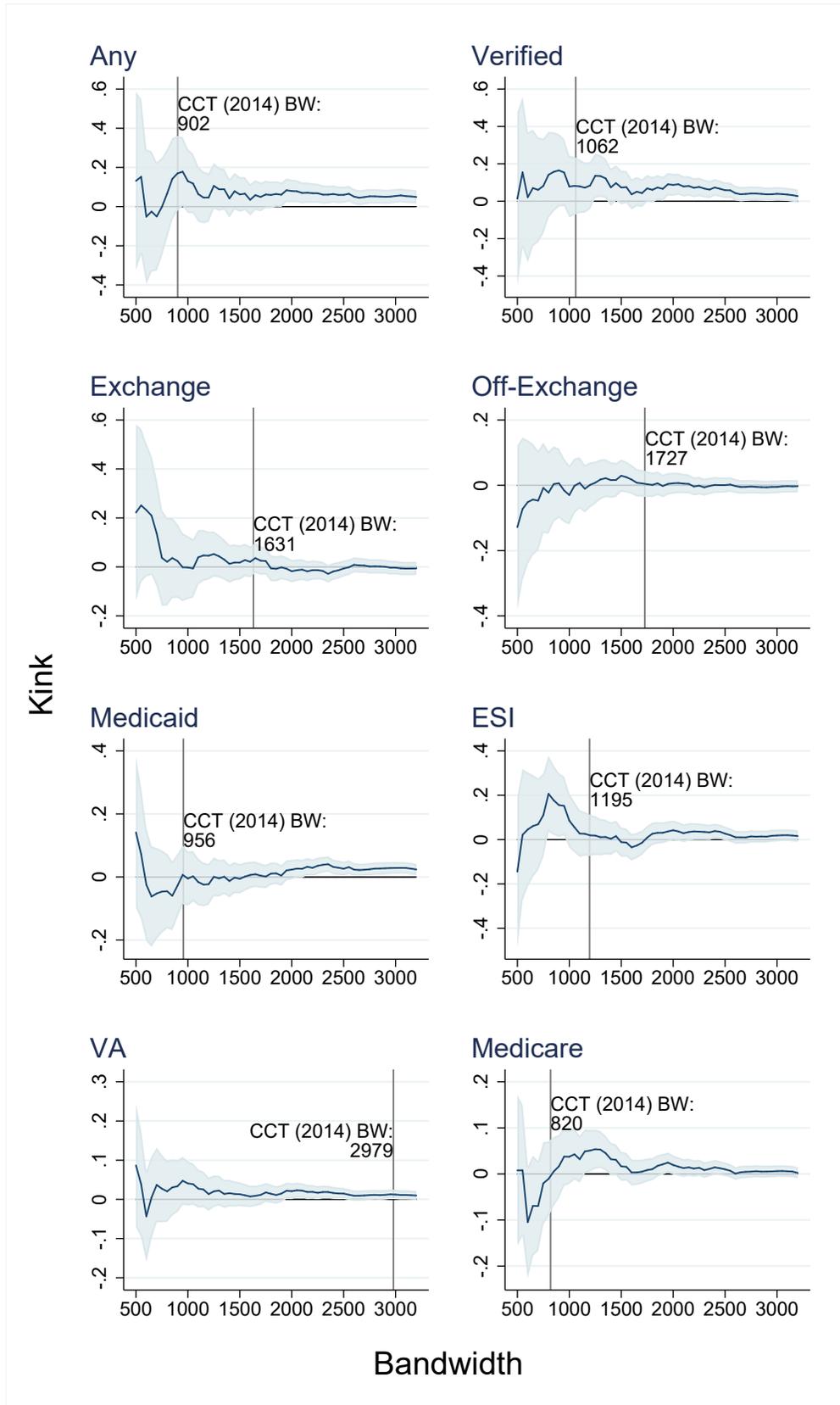
Notes: Figure shows estimated kink in 2015 months insured at placebo kink points. The p-value is the fraction of placebo kinks that exceed the true estimate.

Figure C.2: Estimated discontinuity as a function of bandwidth,



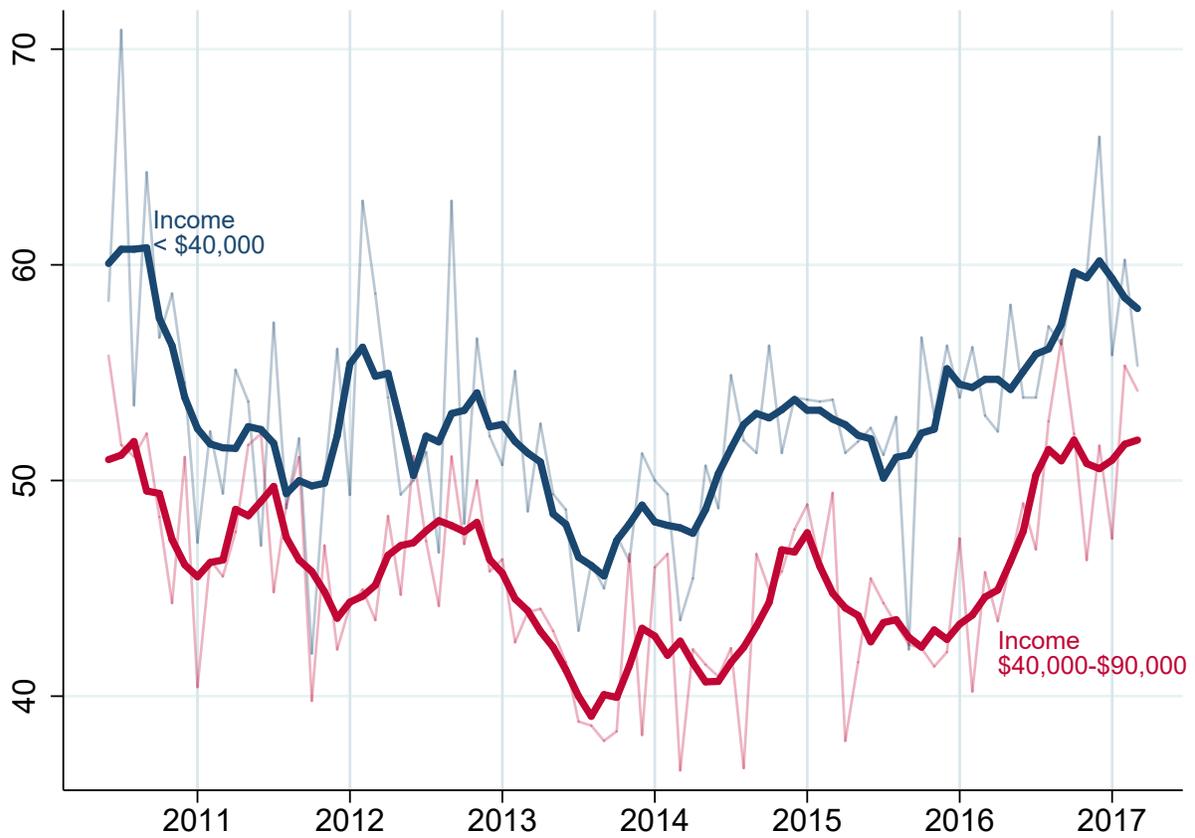
Notes: Figure shows estimated kink (and 95% confidence interval) in 2015 months covered insured as a function of bandwidth, for the indicated types of coverage. The CCT (2014) bandwidth is the MSE-optimal bandwidth of Calonico et al. (2014).

Figure C.3: Estimated kink as a function of bandwidth, 2015



Notes: Figure shows estimated kink (and 95% confidence interval) in 2015 months covered insured as a function of bandwidth, for the indicated types of coverage. The CCT (2014) bandwidth is the MSE-optimal bandwidth of Calonico et al. (2014).

Figure C.4: Percent favorably rating the ACA, by income group



Notes: Source: Figure shows the fraction of people in the indicated group reporting a favorable view of the ACA, among those not expressing “don’t know.” The thick lines are a 5-month rolling average; the thin lines are the monthly data. Data are from the Kaiser Family Foundation’s Health Tracking Poll, a nationally representative sample, Henry J. Kaiser Foundation (2018).

Table C.1: RKD estimates for CDF of Medicaid months, 2015

	Kink (1)	Standard error (2)	Penalty effect (3)
Pr(Medicaid Months ≤ 0)	-0.222	(-0.092)	-0.133
Pr(Medicaid Months ≤ 1)	-0.218	(-0.089)	-0.131
Pr(Medicaid Months ≤ 2)	-0.222	(-0.086)	-0.133
Pr(Medicaid Months ≤ 3)	-0.231	(-0.084)	-0.139
Pr(Medicaid Months ≤ 4)	-0.229	(-0.082)	-0.137
Pr(Medicaid Months ≤ 5)	-0.219	(-0.080)	-0.131
Pr(Medicaid Months ≤ 6)	-0.198	(-0.078)	-0.119
Pr(Medicaid Months ≤ 7)	-0.183	(-0.076)	-0.110
Pr(Medicaid Months ≤ 8)	-0.143	(-0.074)	-0.086
Pr(Medicaid Months ≤ 9)	-0.145	(-0.072)	-0.087
Pr(Medicaid Months ≤ 10)	-0.151	(-0.070)	-0.091
Pr(Medicaid Months ≤ 11)	-0.143	(-0.067)	-0.086

The sample consists of single tax returns with 2015 income between 200 and 250 percent of FPL, with one exemption claimed, no signs of ESI offers, aged 27-64. Each row is a separate regression; the outcome is an indicator for having at most the indicated number of months of Medicaid coverage on income (multiplied by 100). The independent variable is 2015 income (in thousands), allowing for a kink and discontinuity at the 2015 kink point. Column (1) shows the estimated kink, column (2) shows the standard error, and column (3) shows the implied effect of an extra dollar of penalty per month, which is $kink/20 * 12$.

Table C.2: Robustness, RD estimates

Year	2015			2016		
	Linear (1)	Quadratic (2)	Cubic (3)	Linear (4)	Quadratic (5)	Cubic (6)
A. Y = Months of any coverage						
Discontinuity	0.147 (0.059)	0.123 (0.060)	0.125 (0.074)	0.332 (0.062)	0.363 (0.076)	0.402 (0.102)
Observed semi-elasticity	0.236	0.193	0.198	0.329	0.365	0.399
B. Y = Months of verified coverage						
Discontinuity	0.062 (0.058)	0.048 (0.064)	-0.022 (0.071)	0.179 (0.081)	0.189 (0.089)	0.235 (0.105)
Observed semi-elasticity	0.131	0.100	-0.046	0.228	0.246	0.293
C. Y = Months of medicaid coverage						
Discontinuity	0.003 (0.016)	0.002 (0.025)	0.003 (0.032)	0.018 (0.020)	0.022 (0.028)	0.015 (0.038)
Observed semi-elasticity	0.101	0.051	0.098	0.318	0.403	0.270
D. Y = Months of Exchange coverage						
Discontinuity	0.038 (0.041)	0.048 (0.062)	0.003 (0.075)	0.128 (0.047)	0.123 (0.055)	0.137 (0.056)
Observed semi-elasticity	0.227	0.306	0.022	0.464	0.453	0.497
E. Y = Months of Off-Exchange coverage						
Discontinuity	-0.038 (0.023)	-0.039 (0.025)	-0.020 (0.027)	-0.022 (0.017)	-0.001 (0.018)	0.018 (0.026)
Observed semi-elasticity	-1.213	-1.216	-0.583	-0.568	-0.016	0.483
F. Y = Months of ESI coverage						
Discontinuity	0.066 (0.049)	0.038 (0.066)	0.057 (0.070)	0.030 (0.071)	0.055 (0.086)	0.079 (0.105)
Observed semi-elasticity	0.279	0.168	0.241	0.081	0.146	0.202
G. Y = Months of VA coverage						
Discontinuity	0.016 (0.020)	0.009 (0.023)	-0.015 (0.027)	-0.031 (0.021)	-0.051 (0.028)	-0.005 (0.035)
Observed semi-elasticity	0.492	0.295	-0.460	-0.673	-1.119	-0.099
H. Y = Months of Medicare coverage						
Discontinuity	0.013 (0.015)	-0.008 (0.023)	0.005 (0.027)	0.022 (0.020)	0.031 (0.025)	0.046 (0.030)
Observed semi-elasticity	0.517	-0.322	0.210	0.578	0.844	1.249

The sample consists of single tax returns, with one exemption claimed, aged 27-64, income between 133 and 150 percent of FPL. We estimate fuzzy RD models using the MSE optimal bandwidth of Calonico et al. (2014). Robust standard errors in parentheses.

Table C.3: Robustness, months any coverage

Specification	Base (1)	Quadratic (2)	Cubic (3)	No discontinuity (4)	Demographics (5)	No ESI (6)
A. 2015						
Kink	0.051 (0.017)	0.084 (0.063)	0.110 (0.071)	0.050 (0.017)	0.046 (0.017)	0.056 (0.019)
Semi-elasticity	0.406	0.663	0.871	0.392	0.263	0.487
# Observations	653,891	653,891	653,891	653,891	653,891	549,329
B. 2016						
Kink	0.020 (0.015)	0.006 (0.024)	-0.059 (0.059)	0.026 (0.012)	0.016 (0.014)	0.051 (0.018)
Semi-elasticity	0.143	0.046	-0.423	0.184	0.078	0.419
# Observations	656,064	656,064	656,064	656,064	656,064	478,869

The sample consists of single tax returns, with one exemption claimed, no signs of ESI offers, aged 27-64. The 2015 sample is limited to tax returns with income between 200 and 250 percent of FPL, and the 2016 sample is limited to 300 to 390 percent of FPL. Table shows the result of regressing months of any coverage on income, allowing for a kink and discontinuity at the mandate kink point. of FPL. In column (2) we add a quadratic in income, and in column (3) we add a cubic. In column (4) we impose continuity. In column (5) we control for female dummy and a quadratic in age. In column (6) we drop people with ESI coverage. Robust standard errors in parentheses.

Table C.4: Robustness, months verified coverage

Specification	Base (1)	Quadratic (2)	Cubic (3)	No discontinuity (4)	Demographics (5)	No ESI (6)
A. 2015						
Kink	0.029 (0.017)	0.172 (0.063)	0.198 (0.072)	0.030 (0.017)	0.023 (0.017)	0.026 (0.019)
Semi-elasticity	0.279	1.693	1.951	0.290	0.131	0.297
# Observations	653,891	653,891	653,891	653,891	653,891	549,329
B. 2016						
Kink	-0.002 (0.016)	-0.042 (0.025)	-0.128 (0.063)	0.008 (0.013)	-0.007 (0.015)	0.030 (0.018)
Semi-elasticity	-0.015	-0.366	-1.106	0.071	-0.037	0.333
# Observations	656,064	656,064	656,064	656,064	656,064	478,869

The sample consists of single tax returns, with one exemption claimed, no signs of ESI offers, aged 27-64. The 2015 sample is limited to tax returns with income between 200 and 250 percent of FPL, and the 2016 sample is limited to 300 to 390 percent of FPL. Table shows the result of regressing months of verified coverage insured on income, allowing for a kink and discontinuity at the mandate kink point. of FPL. In column (2) we add a quadratic in income, and in column (3) we add a cubic. In column (4) we impose continuity. In column (5) we control for female dummy and a quadratic in age. In column (6) we drop people with ESI coverage. Robust standard errors in parentheses.

Table C.5: Robustness, months Medicaid

Specification	Base (1)	Quadratic (2)	Cubic (3)	No discontinuity (4)	Demographics (5)	No ESI (6)
A. 2015						
Kink	0.023 (0.009)	0.031 (0.034)	0.023 (0.037)	0.021 (0.009)	0.023 (0.009)	0.020 (0.010)
Semi-elasticity	1.554	2.093	1.588	1.452	1.703	1.201
# Observations	653,891	653,891	653,891	653,891	653,891	549,329
B. 2016						
Kink	0.012 (0.007)	0.000 (0.010)	-0.021 (0.026)	0.014 (0.005)	0.012 (0.007)	0.015 (0.009)
Semi-elasticity	1.314	0.004	-2.209	1.556	1.055	1.298
# Observations	656,064	656,064	656,064	656,064	656,064	478,869

The sample consists of single tax returns, with one exemption claimed, no signs of ESI offers, aged 27-64. The 2015 sample is limited to tax returns with income between 200 and 250 percent of FPL, and the 2016 sample is limited to 300 to 390 percent of FPL. Table shows the result of regressing months of Medicaid coverage on income, allowing for a kink and discontinuity at the mandate kink point. of FPL. In column (2) we add a quadratic in income, and in column (3) we add a cubic. In column (4) we impose continuity. In column (5) we control for female dummy and a quadratic in age. In column (6) we drop people with ESI coverage. Robust standard errors in parentheses.

Table C.6: Robustness, months Exchange

Specification	Base (1)	Quadratic (2)	Cubic (3)	No discontinuity (4)	Demographics (5)	No ESI (6)
A. 2015						
Kink	-0.004 (0.013)	0.014 (0.049)	0.023 (0.056)	-0.007 (0.013)	-0.008 (0.013)	-0.001 (0.016)
Semi-elasticity	-0.111	0.373	0.605	-0.189	-0.228	-0.021
# Observations	653,891	653,891	653,891	653,891	653,891	549,329
B. 2016						
Kink	0.021 (0.012)	-0.010 (0.019)	-0.004 (0.047)	0.014 (0.009)	0.019 (0.012)	0.013 (0.015)
Semi-elasticity	0.639	-0.299	-0.115	0.435	0.684	0.295
# Observations	656,064	656,064	656,064	656,064	656,064	478,869

The sample consists of single tax returns, with one exemption claimed, no signs of ESI offers, aged 27-64. The 2015 sample is limited to tax returns with income between 200 and 250 percent of FPL, and the 2016 sample is limited to 300 to 390 percent of FPL. Table shows the result of regressing months of Exchange coverage on income, allowing for a kink and discontinuity at the mandate kink point. of FPL. In column (2) we add a quadratic in income, and in column (3) we add a cubic. In column (4) we impose continuity. In column (5) we control for female dummy and a quadratic in age. In column (6) we drop people with ESI coverage. Robust standard errors in parentheses.

Table C.7: Robustness, months Off-Exchange

Specification	Base (1)	Quadratic (2)	Cubic (3)	No discontinuity (4)	Demographics (5)	No ESI (6)
A. 2015						
Kink	-0.003 (0.010)	0.004 (0.034)	0.029 (0.040)	-0.001 (0.009)	-0.003 (0.010)	0.003 (0.011)
Semi-elasticity	-0.189	0.239	1.938	-0.071	-0.203	0.160
# Observations	653,891	653,891	653,891	653,891	653,891	549,329
B. 2016						
Kink	0.007 (0.010)	-0.001 (0.016)	-0.006 (0.040)	0.012 (0.008)	0.006 (0.010)	0.008 (0.013)
Semi-elasticity	0.328	-0.059	-0.317	0.611	0.383	0.317
# Observations	656,064	656,064	656,064	656,064	656,064	478,869

The sample consists of single tax returns, with one exemption claimed, no signs of ESI offers, aged 27-64. The 2015 sample is limited to tax returns with income between 200 and 250 percent of FPL, and the 2016 sample is limited to 300 to 390 percent of FPL. Table shows the result of regressing months of off-Exchange coverage on income, allowing for a kink and discontinuity at the mandate kink point. of FPL. In column (2) we add a quadratic in income, and in column (3) we add a cubic. In column (4) we impose continuity. In column (5) we control for female dummy and a quadratic in age. In column (6) we drop people with ESI coverage. Robust standard errors in parentheses.

Table C.8: Robustness, months ESI

Specification	Base (1)	Quadratic (2)	Cubic (3)	No discontinuity (4)	Demographics (5)
A. 2015					
Kink	0.016 (0.013)	0.040 (0.046)	0.045 (0.054)	0.020 (0.013)	0.015 (0.013)
Semi-elasticity	0.528	1.317	1.492	0.641	0.189
# Observations	653,891	653,891	653,891	653,891	653,891
B. 2016					
Kink	-0.046 (0.014)	-0.037 (0.023)	-0.036 (0.011)	-0.125 (0.056)	-0.048 (0.014)
Semi-elasticity	-0.923	-0.732	-0.726	-2.471	-0.419
# Observations	656,064	656,064	656,064	656,064	656,064

The sample consists of single tax returns, with one exemption claimed, no signs of ESI offers, aged 27-64. The 2015 sample is limited to tax returns with income between 200 and 250 percent of FPL, and the 2016 sample is limited to 300 to 390 percent of FPL. Table shows the result of regressing months of ESI coverage in 2015 on income, allowing for a kink and discontinuity at the mandate kink point. of FPL. In column (2) we add a quadratic in income, and in column (3) we add a cubic. In column (4) we impose continuity. In column (5) we control for female dummy and a quadratic in age. In column (6) we drop people with ESI coverage. Robust standard errors in parentheses.