

Does the individual mandate affect insurance coverage? Regression kink 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 a Regression Kink Design and tax return data for the population of single, childless tax filers, without observed offers of employer sponsored insurance, with income near the mandate kink point. We find visually clear and economically substantial responses to a greater mandate penalty. Responses are larger among lower income people and people without markers of serious health problems. Despite increasing coverage among healthy people, the mandate may not have improved individual market adverse selection, as the largest responses to the mandate penalty are in public coverage.

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One of the most contentious features of the Affordable Care Act 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, potentially, 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 The individual mandate was effectively repealed by the Tax Cuts and Jobs Act of 2017, which set the penalty for non-coverage to zero.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 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 among on insurance coverage using administrative data.

Our empirical approach exploits the fact that the penalty amount is a kinked function of income. The intuition for our approach is that if the mandate penalty affects coverage, then we should see a kink in the probability of having insurance coverage at the mandate kink point. The magnitude of this kink reflects the effect of a greater mandate penalty on coverage, assuming that absent the mandate kink, there would be a smooth relationship between income and insurance coverage (Card et al., 2015).

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 coverage. These comprehensive administrative data offer a clear advantage over previously used data sources. They are free from the measurement error that plagues survey

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

data, and they record all sources of coverage, unlike administrative data from a single employer or a Health Insurance Marketplace. We focus on a sample of about 1.3 million people with income close to the mandate kink point (\$26,550 in 2015 and \$38,150 in 2016), filing single-person tax returns without observed offers of employer-sponsored insurance (an imperfect proxy for actual offers).

We find visually clear, statistically significant, and quantitatively meaningful evidence that the individual mandate increases insurance coverage. At the 2015 kink point (\$26,550), we find large overall gains. Each dollar of mandate penalty per month raises coverage by about 0.03 months, or 0.41 percent. We argue that this estimate is likely a lower bound on the total response to the mandate. About 60 percent of the response comes from third-party verified coverage. The bulk of the verified response is from Medicaid coverage. We find essentially no coverage increase in the individual insurance market. At the 2016 kink point, we find smaller overall coverage gains, but a larger response in the individual market. Our 2016 point estimates imply that each additional dollar of mandate penalty per month increases the probability of having individual insurance market coverage by about 0.14 percent. These different responses likely reflect the heterogeneous effect of the individual mandate at different income levels, as the 2015 kink point occurs at a lower income level than the 2016 kink point. Taken together, our estimates imply a quantitatively meaningful response to the individual mandate. Extrapolating far beyond the range of identifying variation, our estimates imply that repealing the individual mandate would reduce insurance coverage by about 10 percentage points in our sample.

Our estimated kinks reflect the causal effect of a greater mandate penalty under the identification assumption that the counterfactual relationship between income and insurance coverage would be smooth, absent any 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 near the mandate kink point. We find no evidence for income manipulation: the income distribution is smooth around the mandate kink point. We also find that observed correlates of insurance coverage are not kinked around the mandate kink point.

We believe this is the first quasi-experimental evidence that the ACA's individual

mandate increased insurance coverage. 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). 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.

We document important heterogeneity in who responds to the mandate. People with lower income over the past three years are much more responsive to the mandate penalty, as are young people and men. On the other hand, people with indicators of poor health—receiving Social Security Disability Income, or itemizing medical expenditures on prior tax returns—do not respond to the mandate penalty. This evidence suggests that the individual mandate induces low-spending people to obtain insurance coverage, and therefore may help ease adverse selection, although we find mixed evidence that it raises coverage in the individual market.

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.² 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.³ 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. 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.⁴

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 2016.⁵ We plot vertical lines at 200, 250, 300, and 400 percent of FPL because, as we explain in more detail in Appendix A.2, there are other relevant policy nonlinearities at these thresholds. The figure shows that the mandate kink

²See Lurie and McCubbin (2016) for more detail.

³Modified adjusted gross income is adjusted gross income (AGI) plus tax exempt interest and foreign earned income and housing excluded from AGI.

⁴The penalty is calculated in the instructions to Form 8965, and is paid when the taxpayer files their annual tax return.

⁵For multiperson households, the penalty creates more complex incentives, as we explain in Appendix A.1.

point occurs between these other policy kinks.

2 Empirical Approach

2.1 Econometric specification

We estimate the effect of the mandate penalty on coverage as the ratio of the kink in coverage at the mandate kink point to the kink in the penalty (Card et al., 2015). The kink in the penalty is known. We estimate the kink in coverage with the following 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 (\$26,550 in 2015 and \$38,150 in 2016). Our estimate of the kink in coverage at the mandate kink point is β_3 .

We estimate Equation 2 using people with income between 200 and 250 percent of FPL in 2015, 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. The only other nearby nonlinearities to our knowledge arise from the premium tax credit and cost-sharing reductions, which are kinked or discontinuous at 200, 250, 300, and 400 percent of FPL.⁶

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 lower mean squared error, but slightly worse coverage rates. See Appendix

⁶See Appendix A.2 for details. We 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.

B for more details.⁷

2.2 Identifying assumption and tests of validity

Our key identifying assumption is that in the absence of a kink 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; Appendix A.2 provides more detail about policies affecting insurance coverage, and shows that there are no other policy kinks in our estimation window. In 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 relationship by examining the smoothness of correlates of insurance coverage around the mandate kink point.

2.3 What do people have to know to respond to the kink?

A possible concern with our approach is that even if people are generally aware of the existence and size of the individual mandate, they may not know their exact penalty at the time they make their coverage choices. For example, open enrollment period in 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 2016.⁸ 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

⁷Our specification choices are generally similar to those used in the empirical RKD literature, e.g. Gelber et al. (2017b); Landais (2015).

⁸See <https://www.kff.org/health-reform/issue-brief/explaining-the-2015-open-enrollment-period/>.

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 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. 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). It is plausible that the regression kink design could detect a response to the mandate kink, and if anything our estimates represent a lower bound on the response to the mandate penalty.

2.4 Interpretation of the estimand

We translate our estimated kink β_3 into an economically meaningful object by scaling it by the kink in the monthly mandate penalty, \$20/12 in 2015 and \$25/12 in 2016; the scaled coefficient is the effect of a \$1 increase in the monthly penalty. We also report semi-elasticities, the percentage point increase in insurance coverage caused by a \$1 per month increase in the penalty.

Several caveats govern the interpretation of our estimate. First, it reflects the average response to a \$1 increase in the mandate penalty, averaging over people who are and are not subject to the mandate, and accounting for evasion and avoidance. We believe that this parameter is policy relevant, as policy has largely considered changing the penalty rather than changing who is subject to it.

Second, our estimate is a local average marginal effect of the mandate penalty, specific to people with income near the mandate kink point, and it reflects 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).

Finally, we condition our estimation sample to people without an ESI offer. ESI offers may 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.⁹ However it does change the interpretation: our estimates 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

We extract information maintained by the Internal Revenue Service (IRS) for the full population of U.S. individual income tax returns in 2015 and 2016.¹⁰

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 C, and all other health insurance providers file Form B, which measures coverage in small employer plans, public plans, 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.

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

⁹We test and fail to reject this condition below, by examining the smoothness of the income distribution in our sample.

¹⁰We do not use 2014 because third-party verification did not begin until tax year 2015, and we do not use 2017 because the data are not complete yet.

otherwise. We also define “verified coverage” as months with 1095-reported coverage. We disaggregate coverage into different types using Form 1095. These coverage types include: Medicaid, Veteran’s Affairs (VA), non-VA ESI (including military/Tricare), Exchange, and off-Exchange. Exchange coverage is always recorded on Form 1095-A, and self-insured employer coverage is recorded on Form 1095-C. Other forms of coverage are identified using insurer codes on Form 1095-B. Our definition of “Medicaid” coverage pools together all non-Medicare, non-VA governmental insurance plans. Lurie and Pearce (2018) provide more discussion of these data, and show their validity: tax-based coverage measures closely track survey based measures such as the those in the ACS.

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 as it is calculated for determining the mandate penalty. We obtain our income measure by taking adjusted gross income reported and adding tax exempt interest, foreign earned income, and housing income. All of these fields are reported on Form 1040.

Covariates We observe age and gender, obtained from the Death Master File of Social Security and merge them into our population. We define lagged income as average taxable income in the prior three years. We observe two variables closely related to health: an indicator for any itemized medical expenses in the past three years, and a proxy for disability, which equals 1 if the taxpayer received Social Security Disability Income in any year since 1999. We impute the benchmark Exchange premiums using tax return zipcodes and premium data from the HIXCompare database.¹¹ This is the premium an individual would pay for purchasing the second-lowest cost silver plan available to her on the Exchange, in the absence of any PTC. This premium determines PTC amounts. We think of this variable as a proxy for the local cost of obtaining insurance.¹²

¹¹Available at hixcompare.org.

¹²We lack premium data for about two percent of tax returns with missing zip code information.

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 are 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.1.) 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 mandate kink point: 200 to 250 percent of FPL in 2015, and 300 to 390 percent of FPL in 2016.¹³

We further limit the sample in several ways to exclude people with offers of ESI. We want to exclude people with ESI offers because most people offered ESI have access to highly subsidized insurance and had high coverage rates even prior to the ACA (see, e.g. Kaiser Family Foundation and Health Research and Educational Trust (2012)), making it difficult to detect coverage effects of the mandate. Unfortunately ESI offers from small employers are not reported to IRS. We instead exclude people with observed offers. We observe offers in two ways. First, we drop people employed by large employers, as nearly all of them are offered ESI (96 percent in 2016). For people not working for large employers, we observe several correlates of ESI offers. Specifically, for each employer, we construct flags indicating whether any of the W2s for its employees contain HSA contributions, employee retirement contributions, or health insurance premium payments. We then drop all tax returns with W2s coming from flagged employers.¹⁴ After these exclusions, our analysis sample consists of 653,891 observations in 2015 and 656,056 observations in 2016. We present summary statistics for our analysis sample in Appendix Table C.1.

On average people had about 8 months of coverage. The modal coverage source is individual market coverage, including both Exchange and off-Exchange coverage. Despite our efforts to exclude people with ESI offers, on average people have 2-3 months of ESI

¹³We 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.

¹⁴Among people with W2s from flagged employers and not 1095-Cs, 51 percent have ESI coverage, while people without such W2s or 1095-Cs, the 24 percent have ESI.

coverage. Medicaid is a non-trivial coverage source.¹⁵

Twenty-one percent of our sample paid the mandate penalty in 2015 and 13 percent in 2016. Of those reporting not having year-round coverage, 46 percent paid a penalty in 2015 and 38 percent in 2016. From this we conclude first, that the mandate has bite, as some people paid it; but, second, its teeth are not very sharp, as many avoided it in their initial filing.

4 Results

4.1 Smoothness tests

The top row of Figure 2 shows the income distribution for 2015 and 2016. There is no obvious kink or discontinuity. We test for smoothness by regressing the bin count on income, allowing for a kink and discontinuity at the mandate kink point. The estimated kink is small and statistically insignificant. 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 premium. The remaining panels of the figure show binned scatter plots of these variables. They are clearly correlated with income, but again there is no obvious kink or discontinuity at the mandate kink point. The estimated kinks are statistically insignificant.¹⁶ We conclude that there is no evidence of income manipulation, and the predictors of insurance demand are smoothly distributed around the mandate kink point.¹⁷

¹⁵Most 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.

¹⁶We report smoothness tests for a larger set of outcomes in Appendix table C.2.

¹⁷It is not surprising that we find no evidence of income manipulation at the mandate kink point, despite bunching at PTC notches (Heim et al., 2017; Kucko et al., 2017). The mandate kink is small and even a modest adjustment cost would be enough to prevent bunching (Gelber et al., 2017a).

4.2 Main results

Figure 3 shows months of insurance coverage as a function of income, by year, and looking separately at overall coverage and third-party verified coverage. In 2015, there is a clear upward kink in months of overall insured at the mandate kink point. The estimated kink 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. 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.

The 2015 estimate is roughly in the middle of the range of past estimates of the semi-elasticity of demand for individual insurance. On the low end, Hackmann et al. (2015)'s estimates imply a semi-elasticity of about 0.2, looking at the introduction of the mandate in Massachusetts, and 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. The 2016 estimate is smaller than past estimates, although we cannot reject the hypothesis that the semi-elasticity in 2016 is 0.20, Hackmann et al. (2015)'s estimate for relatively high income households.

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 coverage in our sample be if we set the mandate penalty to zero? Repeal would reduce the average penalty by about \$58/month in the 2015 sample, and \$63/month in the 2016 sample. Given our sensitivity—each dollar of penalty increases coverage by 0.031 months in 2015 and 0.01 months in 2016—this implies a coverage reduction of about 15

percentage points in the 2015 sample and 5 percentage points in the 2016 sample, about 10 percentage points overall. We caution that this calculation 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.

The overall response masks important heterogeneity across different categories of coverage. In Figure 4, we show kinks in Medicaid, Exchange, and ESI coverage. In 2015, Medicaid coverage responds most sharply to the mandate penalty, accounting for 45 percent of the overall response.¹⁸ We also find a modest (but statistically insignificant) response for ESI in 2015. The point estimates for Exchange coverage in 2015 is small, negative, and statistically insignificant. In 2016, we find a marginally significant kink in Medicaid ($p = .09$) and Exchange coverage ($p = .08$), and a very negative kink in ESI coverage, leading to the overall lack of a kink.

4.3 Heterogeneous responses

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 2015 RKD models, stratifying on covariates related to health and spending: sex, age, prior itemized medical expenses, and disability insurance income. The results are in Panels A-E of Table 1.¹⁹ 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 man-

¹⁸The large Medicaid response was initially surprising to us. We have suggested above that much of the Medicaid coverage we observe is possible because Medicaid income eligibility is assessed on a continuous, not annual basis. This suggests that the coverage gains we see in Medicaid should be partial year rather than full year coverage. We find exactly this; see Appendix C.3.

¹⁹We do not examine 2016 because we lack the power to detect cross-group differential responses.

date penalty.

4.4 Reconciling the 2015 and 2016 estimates

We find a substantially larger—but not statistically significantly different—response to the mandate 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 Table 1, which show that people with lower prior income have substantially lower 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. But we 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.

4.5 Further validity and robustness tests

Using lagged income as the running variable Our analysis 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. We describe this approach in more detail in Appendix C.4, and present the estimation results in Appendix Table C.5. The estimated coverage kink using lagged income, 0.056, is quite similar to our main estimate.

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.5 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.3 shows the point estimate for any 2015 coverage and its 95% confidence interval as a function of the bandwidth. The 2015 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 We consider robustness to alternative specification choices: controlling for a global quadratic or cubic in income, controlling for demographics (female dummy and a quadratic in age), and excluding people with ESI. The results are in Appendix Tables C.6 - C.11. In general the results change little when we control for demographics, and the point estimates grow when we exclude people with ESI. The estimates can be sensitive to the inclusion of quadratic or cubic controls. The estimated kinks in 2015 are larger with these controls, sometimes substantially so. However the standard errors are also much larger. We focus on the linear results because the Monte Carlo simulations indicated that controlling for higher order terms is likely to yield uninformative estimates.

5 Conclusions

The individual mandate has generated considerable attention and controversy, with protests, legal challenges, and numerous repeal efforts ultimately culminating in its elimination in the Tax Cuts and Jobs Act of 2017. Despite all this attention, relatively little is known about whether the individual mandate actually causes people to obtain cover-

age. We provide new evidence on this question using a regression kink design and data derived from the population of tax returns.

The individual mandate is important for insurance coverage. Our point estimates indicate that each additional dollar of mandate penalty per month raises insurance by about 0.4 percent at the 2015 mandate kink point, and by about 0.14 percent at the 2016 mandate kink point. Lower income people are more responsive, as are young people, men, and people without markers of poor health. Our estimates imply economically meaningful effects. Extrapolating far beyond the range of identifying variation, they suggest that mandate repeal would reduce coverage by about 10 percentage points in our sample of single-person tax returns with income near the mandate kink points.

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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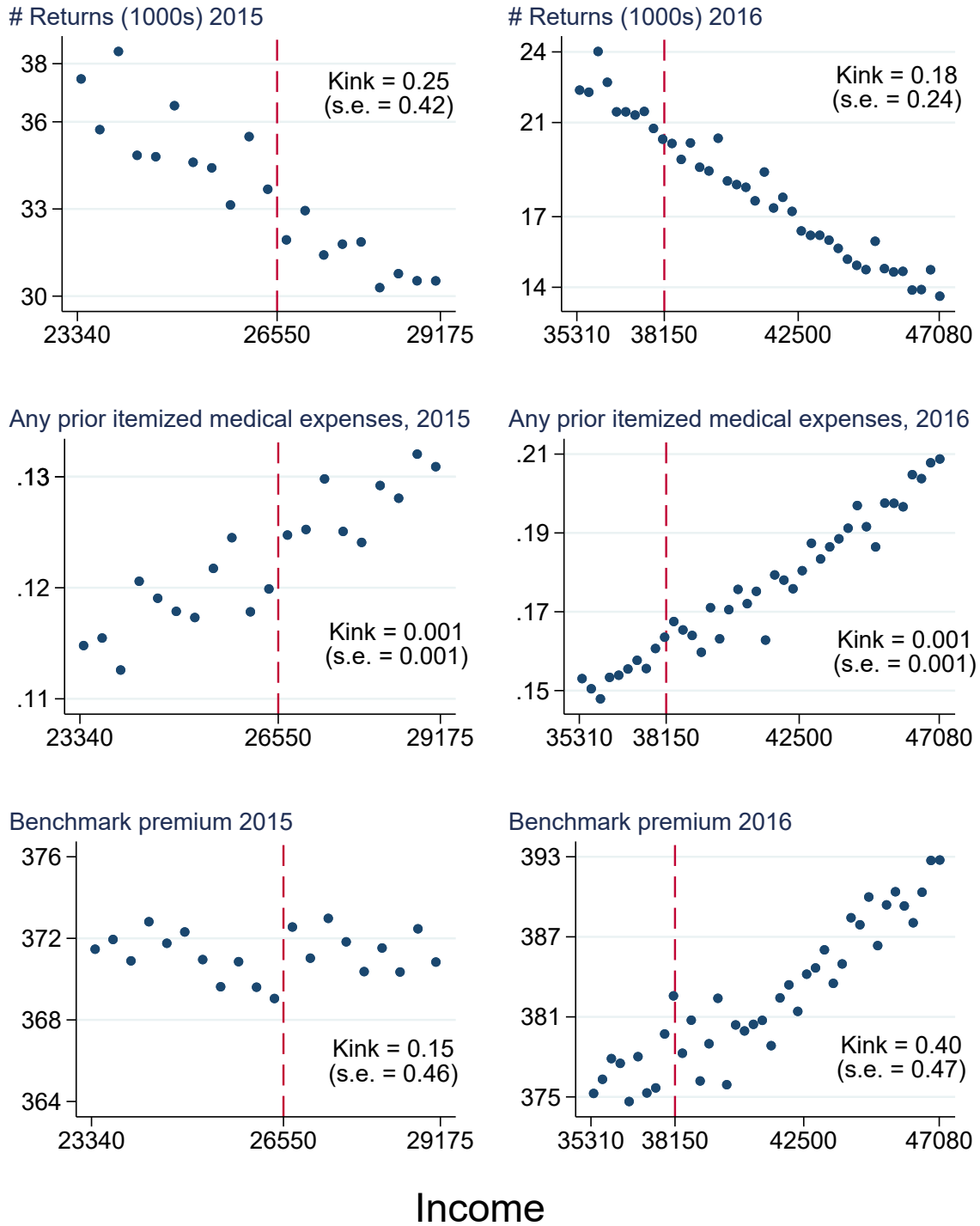
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Figure 1: Mandate penalty and as a function of income



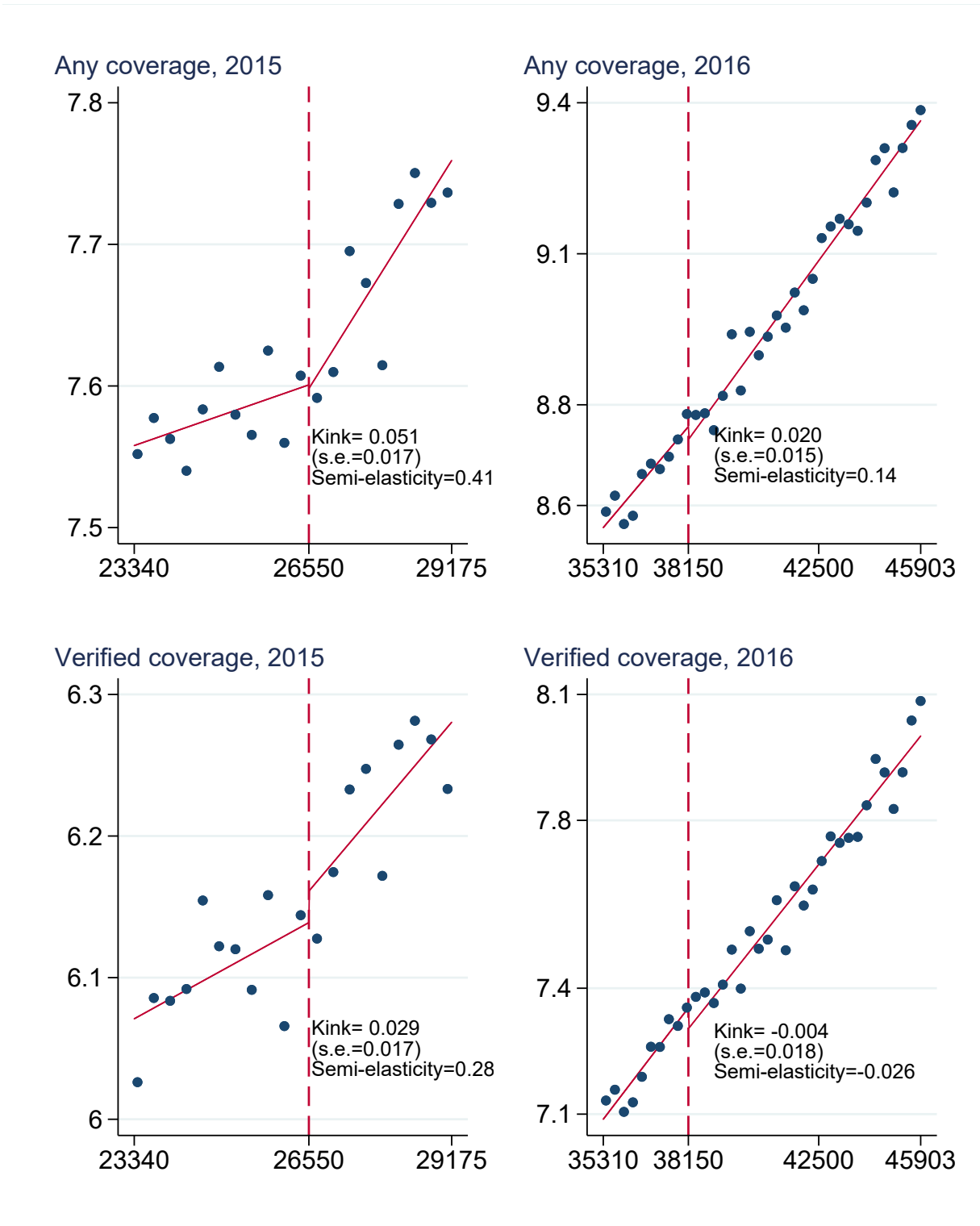
Notes: Figure shows the 2015 and 2016 mandate penalty for a single person per 12 months of uninsurance, as a function of income. The 2015 kink point lies between 200 and 250 percent of FPL (for a single person household) and the 2016 kink point lies between 300 and 400 percent.

Figure 2: Smoothness of income distribution and predetermined covariates



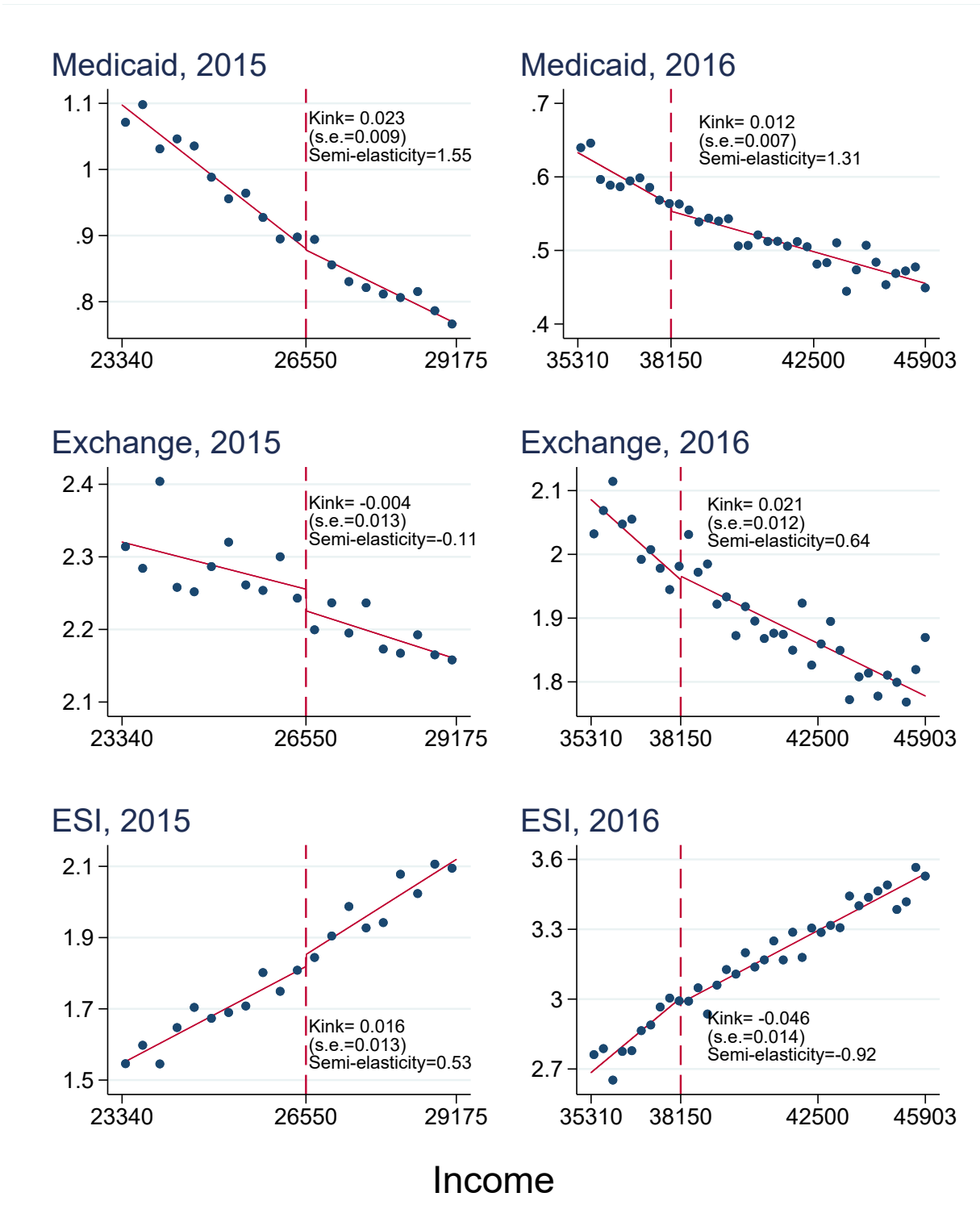
Notes: The top row shows the number of observations in each \$300 bin of MAGI; subsequent remaining rows show bin-level averages of the indicated outcome. The sample consists of people aged 27-64 in the indicated year, who filed single tax returns with one exemption and no observed offers ESI. The dashed line shows the mandate kink point. The reported kink is β_3 in Equation 2, estimated on the bin counts for the top panels, and the microdata for the remainder.²⁰

Figure 3: Months insured as a function of income



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 observed offers ESI. 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. The reported kink is β_3 in Equation 2, estimated using the microdata. 21

Figure 4: Months insured as a function of income, by type of coverage



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 observed offers ESI. The reported kink is β_3 in Equation 2, estimated using the microdata.

Table 1: Heterogeneous responses to the mandate penalty in 2015

	# 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 observed offers ESI.

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A Further policy details

A.1 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.

A.2 Other income-based incentives to obtain coverage

Here we describe other policies that create income-dependent incentives to obtain insurance coverage, with a focus on policies with nonlinearities near 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 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.²⁰ 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.²¹

Cost-sharing reductions Whereas the PTC helps pay for premiums, cost-sharing reductions (CSRs) are subsidies for out-of-pocket 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.

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

²⁰The 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.

²¹Heim et al. (2017) provide more detail on the PTC, APTC, and repayment requirements.

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. Washington, D.C., is the exception, with a limit of 215 percent of FPL.²²

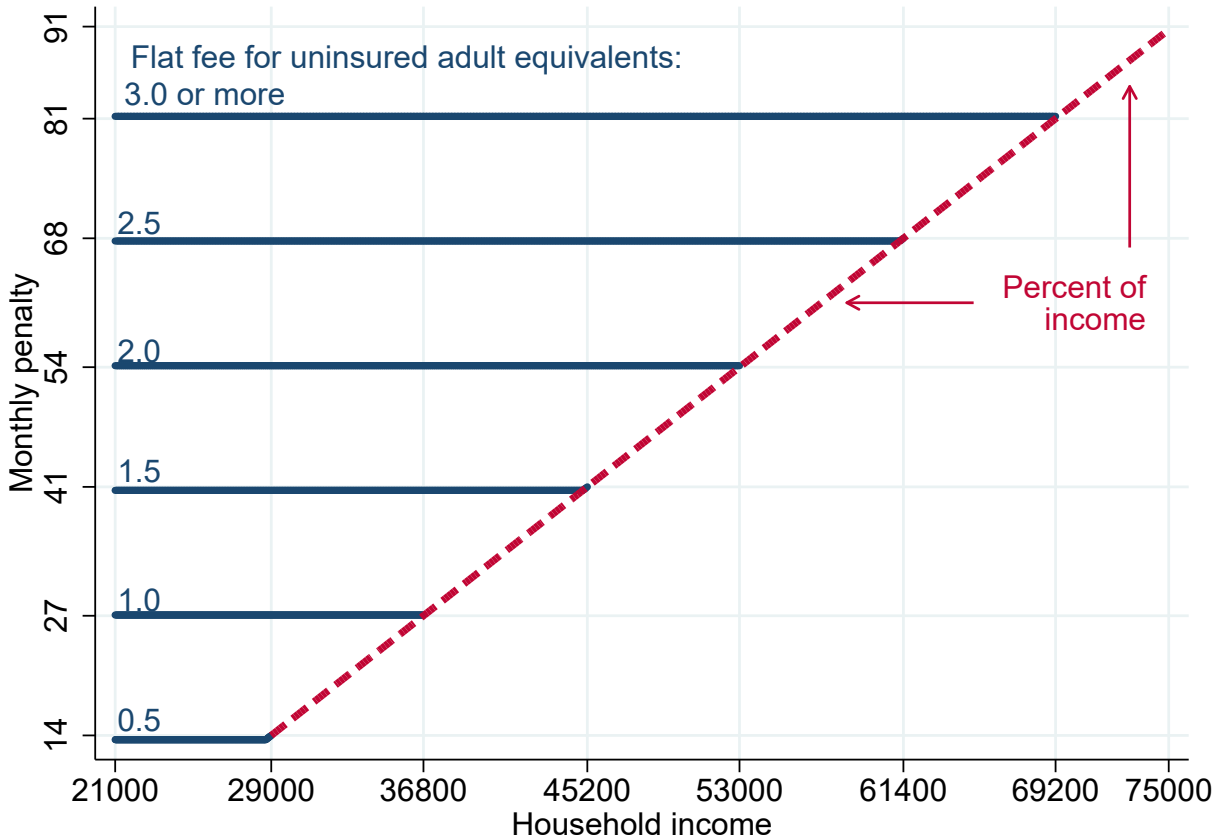
Another kink in the incentive to obtain insurance comes from the tax deductibility of employer sponsored insurance, which creates a kink in the incentive to obtain ESI at each kink point in the income tax code. However, income tax applies to a different income base than the mandate penalty, taxable income rather than MAGI. Taxable income is equal to income minus the personal exemption and standard deduction or any itemized deductions. These deductions and exemptions mean that the kinks in the income tax code generally occur far away from the kink in the mandate kink point. 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.²³ These nonlinearities occur at even increments of the FPL: 200, 250, 300, and 400 percent (as well as at lower income levels that are not relevant for us). The mandate kink point occurs between these critical values. In 2015 it is at roughly 225 percent of FPL, and in 2016 it is at roughly 325 percent of FPL. It is therefore possible to separably identify the coverage effect of the individual mandate from the coverage effects of these policies by looking within the 200-250 percent FPL window in 2015, and the 300-400 percent FPL window in 2016.

²²See <https://www.kff.org/health-reform/state-indicator/medicaid-income-eligibility-limits-for-adults-as-a-percent-of-the-federal-poverty-level/>

²³Tebaldi (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.

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:

$$y_i - \hat{\tau}_t D_i v_i = \sum_{d=0}^5 \beta_d v_i^d + \sum_{d \neq 1}^5 \theta_d v_i^d D_i + \epsilon_i. \quad (3)$$

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.²⁴ 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.

²⁴The 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-Gjebels, 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 Additional empirical results

C.1 Additional smoothness tests

To test for smoothness of the counterfactual (non-kinked mandate penalty) relationship between insurance coverage and income, we re-estimate our main RKD specification, but putting covariates on the left hand side. We believe these variables are not causally related to the mandate penalty, so they should not be kinked, in the absence of any underlying kink in the counterfactual relationship between insurance coverage and income.

In the main text we focus on two covariates, an indicator for prior itemized medical expenses and the benchmark premium, as we believe these are our two variables most closely related to insurance demand. In Appendix Table C.2 we test for smoothness of a wider set of variables: female indicator, age, prior income, and disability status. For none of these do we estimate a statistically significant kink at the mandate kink point.

C.2 Kinks in all coverage categories

We report kinks in all coverage categories in Appendix Table C.3. The first two columns report the kink in any coverage and in verified coverage. In the remaining columns we divide verified coverage into six categories that are exhaustive and nearly mutually exclusive: Medicaid, Exchange, Off-Exchange, ESI, VA, and Medicare. These categories are mutually exclusive in the sense that each 1095 is categorized into exactly one category. However in a given month a person can have coverage from multiple sources.

C.3 Partial year Medicaid coverage

We find a substantial kink in months of Medicaid coverage at the 2015 kink point. We investigate this kink further by looking at where this kink comes from—from increases in partial year coverage, or from people switching from no Medicaid to full year coverage. We estimate regressions of the form

$$Pr(\text{Medicaid Months}_i \leq m) = \beta_0^m + \beta_1^m v_i + \beta_2^m 1\{v_i \geq 0\} + \beta_3^m v_i 1\{v_i \geq 0\} + \varepsilon_i^m. \quad (4)$$

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.

Appendix Table C.4 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. It is easier to understand these estimates graphically, so in Appendix Figure C.1 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 4. 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 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.

C.4 Kink in 2015 coverage given 2014 income

Our analysis uses 2015 income as the running variable, because 2015 income determines the 2015 mandate penalty. However, it is possible that people imperfectly forecast their 2015 income and mandate penalty at the time of enrollment. In an attempt to account for these forecast errors, we re-ran our analysis of 2015 coverage using 2014 income as the running variable, under the hypothesis that people use their 2014 income to forecast their 2015 income and mandate penalty.

Table C.5 shows the results. We begin in column (1) by showing the kink in the 2015 mandate penalty as a function of 2014 income. It is about 0.01, 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 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 estimates are fairly similar to those using 2015 income as the running variable; the Medicaid and ESI kinks are a bit smaller, and the individual market kinks a bit larger, with a very similar kink in overall coverage.

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 $1/0.02$ (since there is a kink of 0.02 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 $1/.01$. 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.

C.5 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.2 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

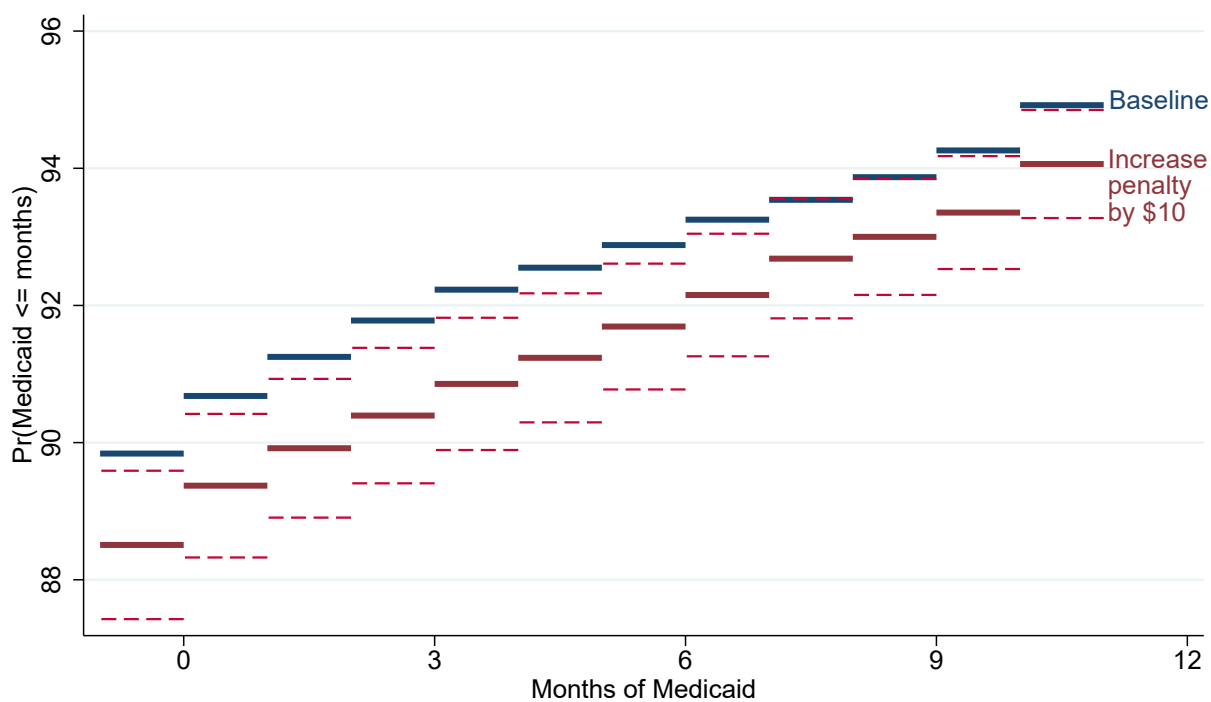
because we do not believe looking elsewhere in the income distribution would be informative about the possibility of a false positive at our income level. There are likely to be other policy-induced kinks elsewhere in the income distribution (for example, because of the PTC). We exclude kink points near 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.

C.6 Robustness tests

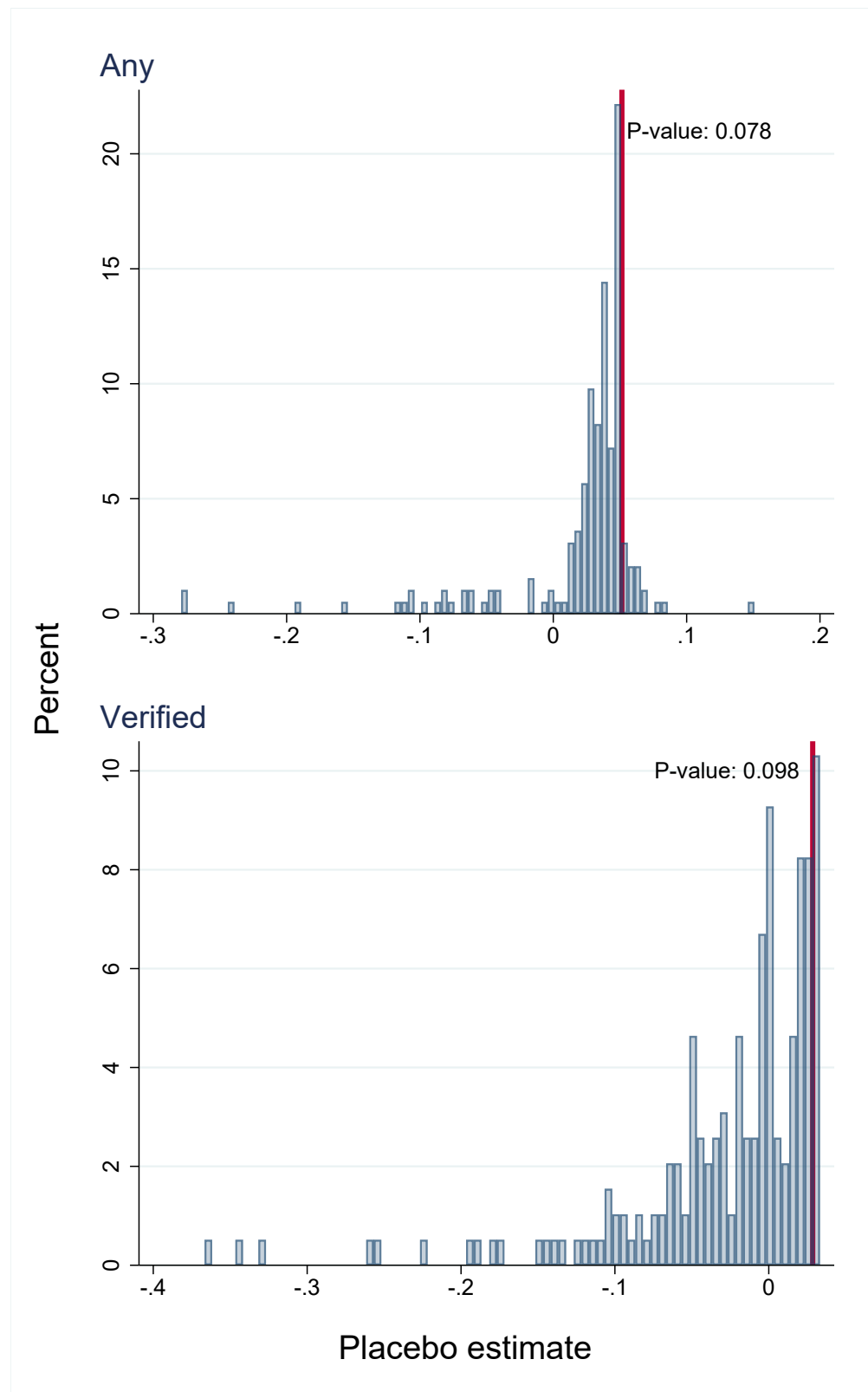
We consider robustness to alternative specification choices: controlling for a global quadratic or cubic in income, controlling for demographics (female dummy and a quadratic in age), and excluding people with ESI. The results are in Appendix Tables C.6 - C.11; each table robustness of results for each insurance measure for 2015. In general the results change little when we control for demographics, and the point estimates grow when we exclude people with ESI. The results are sensitive to the inclusion of quadratic or cubic controls. The estimated kinks are always larger with these controls, sometimes substantially so. However the standard errors are also much larger. We focus on the linear results because the Monte Carlo simulations indicated that controlling for higher order terms is likely to yield uninformative estimates.

Figure C.1: CDF of months of Medicaid coverage, 2015



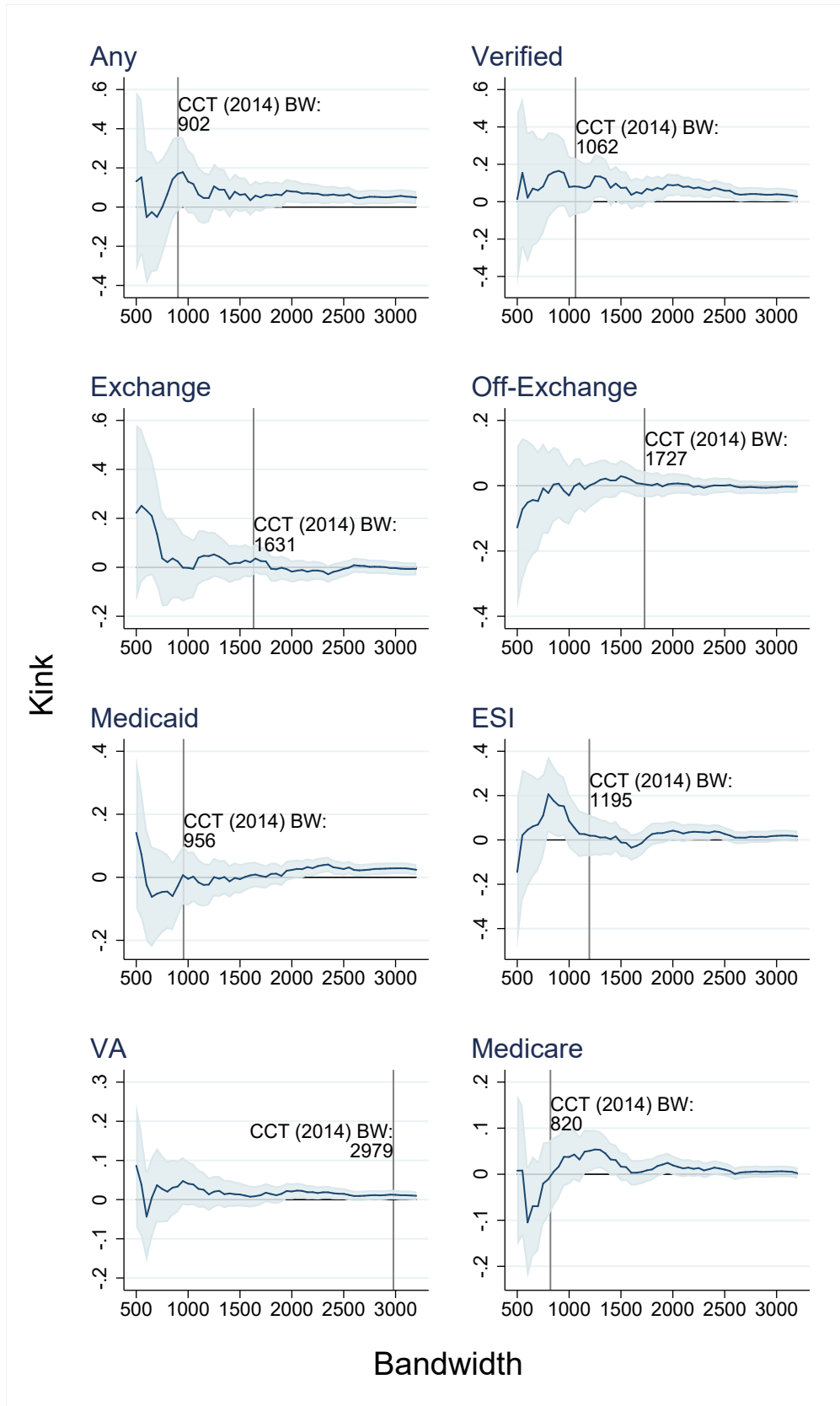
Notes: Source: Figure shows the CDF of months of Medicaid coverage at the 2015 mandate kink point (“baseline”) and the counterfactual CDF induced by a \$10 increase in the monthly mandate penalty, along with the 95% confidence interval. See Appendix C.3 for details on the construction of the figure.

Figure C.2: Estimated kink in months insured at placebo kink points, 2015



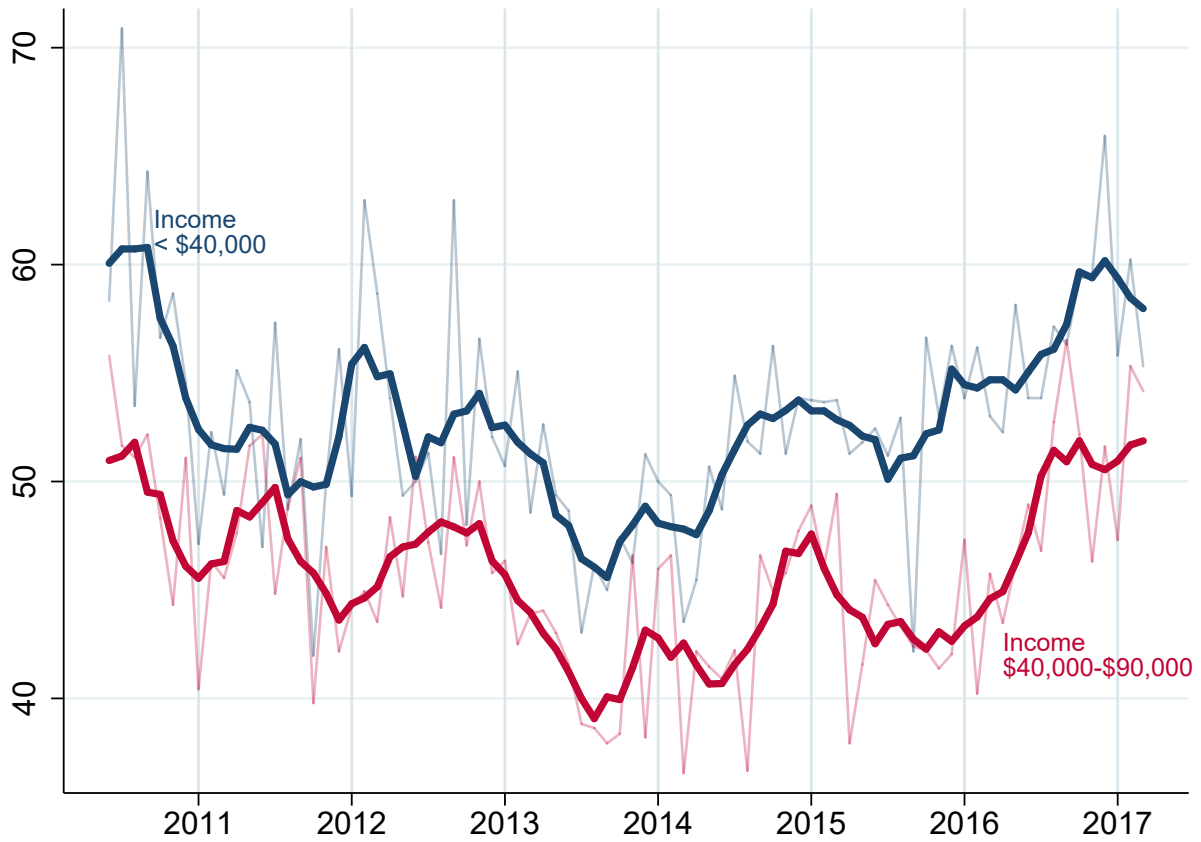
Notes: Figure shows estimated kink in 2015 months insured at placebo kink points. The p-value is the fraction of placebo kinks that exceed the true estimate.

Figure C.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: Summary statistics

Year	2015 (1)	2016 (2)
A. Coverage information		
Months of...		
Any coverage	7.62	8.53
Verified coverage	6.15	
Exchange	2.25	1.93
Off-Exchange	0.89	1.29
ESI	1.80	3.11
Medicaid	0.92	0.54
Medicare	0.36	0.28
VA	0.33	0.54
Insured at least 1 month	0.67	0.74
Paid penalty	0.21	0.13
B. Statistics of income		
10th Percentile	23,800	36,125
25th percentile	24,600	37,425
Median	26,000	39,925
75th percentile	27,500	42,625
90th percentile	28,500	44,525
Mean	26,102	40,135
C. Covariates		
Female	0.38	0.36
Age	44.73	45.42
Benchmark premium	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	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, without offers of ESI as defined in the text, aged 27-64 in the reference year. In 2015 the sample is further limited to people with income between 200 and 250 percent of FPL, and in 2016 it is limited to people with income between 300 and 390 percent of FPL. Income percentiles are percentiles of income in \$100 bins, to avoid disclosing individual taxpayers' income.

Table C.2: Additional 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						
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)
# Obs.	653,891	653,891	653,891	653,891	644,136	653,891
B. Smoothness tests for 2016						
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)
# Obs.	656,064	656,064	6506,064	656,064	642,017	656,064

The sample consists of single tax returns in 2015 (Panel A) and 2016 (Panel B), with one exemption claimed, no observed offer of ESI, aged 27-64. 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 C.3: Kinks in months of insurance coverage, all categories

Coverage type	Any (1)	Verified (2)	Medicaid (3)	Exchange (4)	Off-Ex (5)	ESI (6)	VA (7)	Medicare (8)
A. 2015								
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)
Semi-elasticity	0.406	0.279	1.554	-0.111	-0.189	0.528	1.859	0.379
# Observations	653,891	653,891	653,891	653,891	653,891	653,891	653,891	653,891
B. 2016								
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)
Semi-elasticity	0.143	-0.015	1.314	0.639	0.328	-0.923	0.191	0.069
# Observations	656,064	656,064	656,064	656,064	656,064	656,064	656,064	656,064

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 no observed offer of ESI, 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 C.4: 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 observed offer of ESI, 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.5: Kinks in 2015 penalty and coverage as a function of 2014 income

Dep. Var.	Penalty (\$1000s) (1)	Months insured by coverage type					
		Any (2)	Medicaid (3)	Exchange (4)	Off-Ex (5)	ESI (6)	VA (7)
Kink	0.009 (0.002)	0.056 (0.018)	0.013 (0.008)	0.009 (0.014)	0.021 (0.010)	0.009 (0.014)	0.006 (0.006)
Semi-elasticity:							
Naive forecast		0.448	1.032	0.244	1.364	0.278	1.157
Rational expectations		0.996	2.293	0.542	3.031	0.618	2.571
# Observations	607,030	607,030	607,030	607,030	607,030	607,030	607,030

Table reports the estimated kink obtained from a regression of the indicated outcome, 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 observed offer of ESI in 2015, 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 C.6: Robustness, months any coverage

Specification	Base (1)	Quadratic (2)	Cubic (3)	Demographics (4)	No ESI (5)
A. 2015					
Kink	0.051 (0.017)	0.084 (0.063)	0.110 (0.071)	0.046 (0.017)	0.056 (0.019)
Semi-elasticity	0.406	0.663	0.871	0.263	0.487
# Observations	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)	0.016 (0.014)	0.051 (0.018)
Semi-elasticity	0.143	0.046	-0.423	0.078	0.419
# Observations	656,064	656,064	656,064	656,064	478,869

The sample consists of single tax returns, with one exemption claimed, no observed offer of ESI, 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 control for female dummy and a quadratic in age. In column (5) we drop people with ESI coverage. Robust standard errors in parentheses.

Table C.7: Robustness, months verified coverage

Specification	Base (1)	Quadratic (2)	Cubic (3)	Demographics (4)	No ESI (5)
A. 2015					
Kink	0.029 (0.017)	0.172 (0.063)	0.198 (0.072)	0.023 (0.017)	0.026 (0.019)
Semi-elasticity	0.279	1.693	1.951	0.131	0.297
# Observations	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)	-0.007 (0.015)	0.030 (0.018)
Semi-elasticity	-0.015	-0.366	-1.106	-0.037	0.333
# Observations	656,064	656,064	656,064	656,064	478,869

The sample consists of single tax returns, with one exemption claimed, no observed offer of ESI, 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 control for female dummy and a quadratic in age. In column (5) we drop people with ESI coverage. Robust standard errors in parentheses.

Table C.8: Robustness, months Medicaid

Specification	Base (1)	Quadratic (2)	Cubic (3)	Demographics (4)	No ESI (5)
A. 2015					
Mandate Kink	0.023 (0.009)	0.031 (0.034)	0.023 (0.037)	0.023 (0.009)	0.020 (0.010)
Semi-elasticity	1.554	2.093	1.588	1.703	1.201
# Observations	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)	0.012 (0.007)	0.015 (0.009)
Semi-elasticity	1.314	0.004	-2.209	1.055	1.298
# Observations	656,064	656,064	656,064	656,064	478,869

The sample consists of single tax returns, with one exemption claimed, no observed offer of ESI, 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 control for female dummy and a quadratic in age. In column (5) we drop people with ESI coverage. Robust standard errors in parentheses.

Table C.9: Robustness, months Exchange

Specification	Base (1)	Quadratic (2)	Cubic (3)	Demographics (4)	No ESI (5)
A. 2015					
Kink	-0.004 (0.013)	0.014 (0.049)	0.023 (0.056)	-0.008 (0.013)	-0.001 (0.016)
Semi-elasticity	-0.111	0.373	0.605	-0.228	-0.021
# Observations	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)	0.019 (0.012)	0.013 (0.015)
Semi-elasticity	0.639	-0.299	-0.115	0.684	0.295
# Observations	656,064	656,064	656,064	656,064	478,869

The sample consists of single tax returns, with one exemption claimed, no observed offer of ESI, 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 control for female dummy and a quadratic in age. In column (5) we drop people with ESI coverage. Robust standard errors in parentheses.

Table C.10: Robustness, months Off-Exchange

Specification	Base (1)	Quadratic (2)	Cubic (3)	Demographics (4)	No ESI (5)
A. 2015					
Kink	-0.003 (0.010)	0.004 (0.034)	0.029 (0.040)	-0.003 (0.010)	0.003 (0.011)
Semi-elasticity	-0.189	0.239	1.938	-0.203	0.160
# Observations	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)	0.006 (0.010)	0.008 (0.013)
Semi-elasticity	0.328	-0.059	-0.317	0.383	0.317
# Observations	656,064	656,064	656,064	656,064	478,869

The sample consists of single tax returns, with one exemption claimed, no observed offer of ESI, 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 control for female dummy and a quadratic in age. In column (5) we drop people with ESI coverage. Robust standard errors in parentheses.

Table C.11: Robustness, months ESI

Specification	Base (1)	Quadratic (2)	Cubic (3)	Demographics (4)
A. 2015				
Kink	0.016 (0.013)	0.040 (0.046)	0.045 (0.054)	0.015 (0.013)
Semi-elasticity	0.528	1.317	1.492	0.189
# Observations	653,891	653,891	653,891	653,891
B. 2016				
Kink	-0.046 (0.014)	-0.037 (0.023)	-0.125 (0.056)	-0.048 (0.014)
Semi-elasticity	-0.923	-0.732	-2.471	-0.419
# Observations	656,064	656,064	656,064	656,064

The sample consists of single tax returns, with one exemption claimed, no observed offer of ESI, 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 control for female dummy and a quadratic in age. In column (5) we drop people with ESI coverage. Robust standard errors in parentheses.