A Alternate Specifications

As described in the text, roughly twenty percent of the sample was dropped because of a discrepancy between eligibility as determined by the AHRQ, and eligibility according to the thresholds collected from the TRIM3 database and Kaiser Family Foundation reports. We will document here that this makes no material difference to the analysis above, and in some cases, makes it stronger.

The first concern about dropping these observations is that they might invalidate the RD design. If these dropped observations are disproportionately from either one side of the other of the discontinuity, and the discrepancy between the eligibility rules is tied to outcomes of interest, then the estimates could be an artifact of the data management.

As is evident in Figures 8(a) and 8(b), this first condition does not hold. These figures plot the weights of the observations by distance from the imputed eligibility threshold. The first graph includes the observations with inconsistent measures of eligibility, while the second excludes them. Both graphs demonstrate a smooth distribution through the discontinuity, mitigating concerns about the validity of the RD. This is the same conclusion of the estimation and graphing suggested by McCrary (2008) used in the text.

I also repeat the RD estimates of Equation (2) including the observations with inconsistent measure of eligibility. These point estimates can be found in Table 4, which also reports the point estimates for the preferred (baseline) specifications in Table 2. In the interest of brevity, I only report the point estimates for the regressions with a fifth-order polynomial in family income and distance from the eligibility threshold. In nearly all cases, the point estimates from the preferred specification are smaller in magnitude. This may reflect the misspecification of the running variable. When the running variable is more noisily measured, the estimated control function G may be attenuated. This would leave more of the level differences in outcome between the eligible and ineligible due to differences in the running variable (e.g., the normality of demand for health care) to be assigned to the RD estimate.

B Robustness Exercises

A Higher-order Polynomial Fits and Clustering

The regression coefficients presented above relied on first-order polynomials in the running variable. Table 5 reports the point estimates and the standard errors for key outcome variables using third- and fifth-order polynomials, as well as the preferred specification (first-order) and no controls for the running variable. All specifications include demographic controls. The general patterns hold. The insurance specifications provide further consistent evidence of parental crowd-out at the margin, with only a statistically-significant fraction of uptake of public insurance. The various polynomial specifications support the mixed results on spending by source and use, and the possible conseqeunces for preventive care. The t-statistics presented in the main tables of the paper accounted for the sampling methods used in collecting the MEPS household data. These were calculated using svy prefix commands in Stata. The data in the MEPS are also collected over a short, two-year panel, and I do not account for the potential dependence of errors within an individual over time. The svy prefix in Stata is not compatible with clustering beyond the survey's design. For the preferred specifications, I chose to focus on the inference using the survey design. Each individual is only observed twice, and the variation exploited here is cross-sectional. Not accounting for the survey design inflates the t-statistics by at least a factor of 10. Given the geographic patterns reguarly observed in health-care data, this should not be surprising. Clustering by individual in lieu of accounting for the survey design is likewise problematic: the survey design induces correlation between individuals, and clustering by individual ignores that.

To simulate clustering by individual, while still accounting for the survey design, I drop the second observation of an individual in the data, and re-run the regression specifications. The point-estimates and t-statistics, presented in Table 5 are similar, though the latter are typically smaller to their full-sample counterpart. The sample falls by a little less than half, as the AHRQ did not calculate eligibility measurements for one of the panel-years, and the panel sizes vary, as well.

B Measurement Error of Eligibility

As described in the text, roughly twenty percent of the sample was dropped because of a discrepancy between eligibility as determined by the AHRQ, and eligibility according to the thresholds collected from the TRIM3 database and Kaiser Family Foundation reports. We will document here that this makes no material difference to the analysis above, and in some cases, makes it stronger.

The first concern about dropping these observations is that they might invalidate the RD design. If these dropped observations are disproportionately from either one side of the other of the discontinuity, and the discrepancy between the eligibility rules is tied to outcomes of interest, then the estimates could be an artifact of the data management.

As is evident in Figures 8(a) and 8(b), this first condition does not hold. These figures plot the weights of the observations by distance from the imputed eligibility threshold. The first graph includes the observations with inconsistent measures of eligibility, while the second excludes them. Both graphs demonstrate a smooth distribution through the discontinuity, mitigating concerns about the validity of the RD. This is the same conclusion of the estimation and graphing suggested by McCrary (2008) used in the text.

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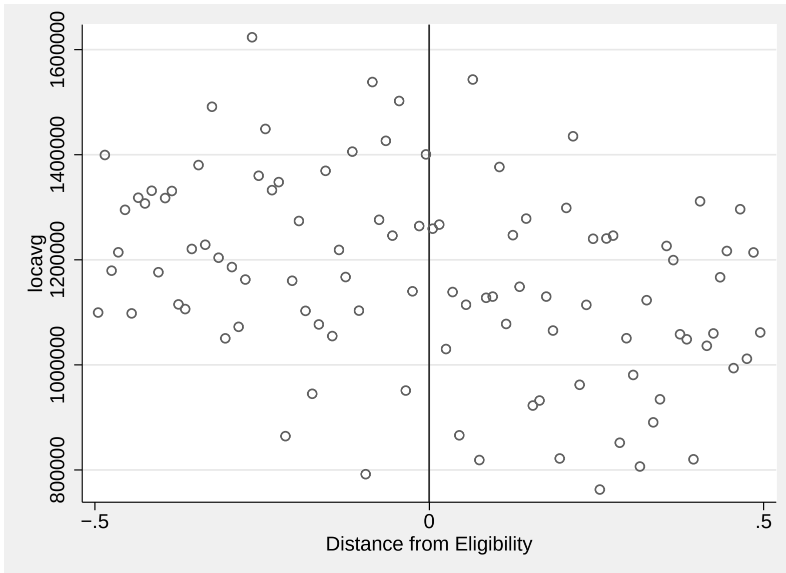
C Predetermined characteristics and Eligibility

Beyond the bin size calculations already presented, I am able to provide further tests of eligibility manipulation. If there is precise manipulation of the forcing variable, and the forcing variable is correlated with a pre-determined characteristic, such as race, then there will be a discontinuity in the racial composition of the bins near the eligibility threshold. The relationship between the forcing variable and the fraction of the bin that is African-American can be found in Figure 9. The fraction black is decreasing at an even rate as the forcing variable increases. The fitted estimate on the left-hand side of the threshold is less than that on the right-hand side. This would be consistent with manipulation of the forcing variable, though the fitted value at the threshold from either side is within the range of the bin averages on the other side of the threshold. The point estimates tend to be robust when controls for demographic information are included in main specifications above. Similarly, I construct two measures of expected out-of-pocket spending for a child if uninsured, as a function of age, race, state of residence and year of observation. I include age in months in a third-order polynomial; the other child information is indicated with dummy variables. Observed out-of-pocket spending is correlated with insurance status, which itself may be correlated with demographic measures. To the extent that those demographic measures are correlated with eligibility, expectations based upon those demographics may be biased. Thus, I attempt to mitigate the bias in two ways, both similar to the methods employed by Fang et al. (2008). First, I include an indicator variable for whether the child has any health insurance. When constructing the fitted variables, I set the insurance indicator to zero for all observations, to measure the expected out-of-pocket spending if uninsured. Alternatively, I regress out-of-pocket spending on the demographic variables only for children without insurance, and construct fitted values of that regression for the entire sample. Both methods attempt to resolve the same underlying problem in different ways; neither provides evidence of manipulation. The bin averages for these expected value, and the local linear fit, can also be seen in Figure 9. The local linear fits of each method predict a discontinuity at the eligibility threshold of a different sign, and both are well within the usual ranges for bin averages on either side of the eligibility threshold. This is, again, consistent with the bin size calculations provided above.

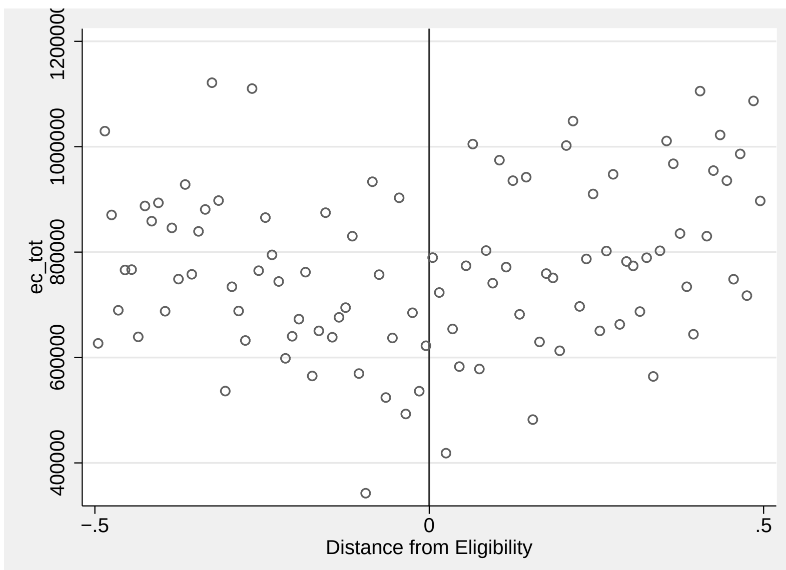
D Selection on Observables

The identification strategy employed here can also be interpreted as selection on observables. The natural way to implement such an estimator is to include controls for the factors determining the child's eligibility—family income as a fraction of Federal poverty guideline and state-year-child's age dummies. The support condition for the selection on observables estimation is related to the smoothness assumptions for the RD estimator. The distance controls are not required, because the state-year-child's age dummies control for the year-state-age variation in eligibility threshold.

Table 4 presents the selection on observables estimates. There are 51 states $\times$ 10 years $\times$ 19 age groups $= 9,690$ state-year-age dummies. This full model was not estimated, due to its computational burden. However, the point estimates for two alternate specifications are presented. The second includes child age (in years), state, and year fixed effects, but not their interactions. The second includes the state and year interactions. Both specifications yield point estimates that are larger than the preferred specification. This may not be surprising, as many states vastly increased the eligibility thresholds for public health insurance. Less stringent controls in the selection on observables can shed light on the validity of other identification techniques common in this literature. Researchers typically exploit across-state over-time variation in eligibility changes to estimate difference-in-difference to identify the causal impact of eligibility. Modifications to the selection on observables test the validity of this strategy. For example, simulated eligibility estimates typically include year- and state-fixed effects, but not their interaction, as the state-year interactions are providing the source of the policy variation. If this variation is correlated with eligibility and health care outcomes, then excluding the state-year interactions will vary the point estimate of the eligibility indicator. A related test would exclude the state-year-age interactions, controlling only for age and state-year interactions. This would test the validity of exploiting within-state, across age group variations in policy. Both appear to overstate the impact of public health insurance eligibility.

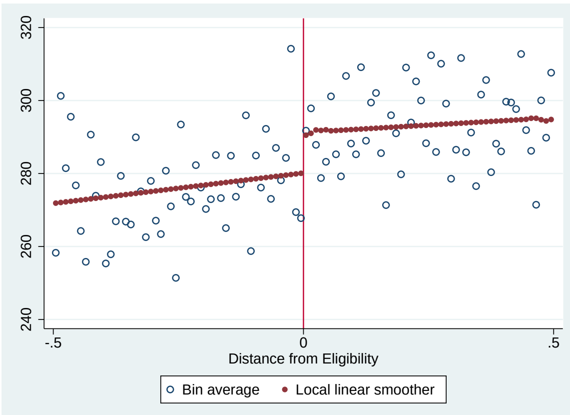


(a) All observations

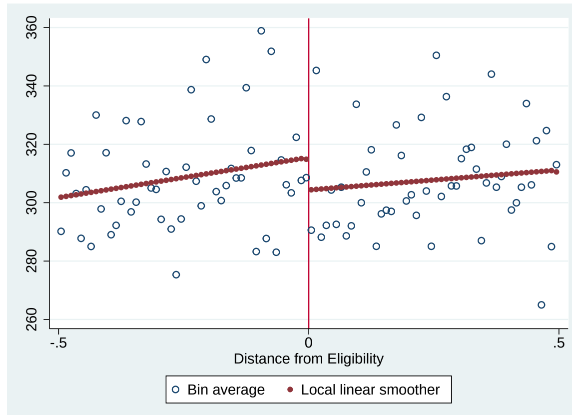


(b) Consistent eligibility measures

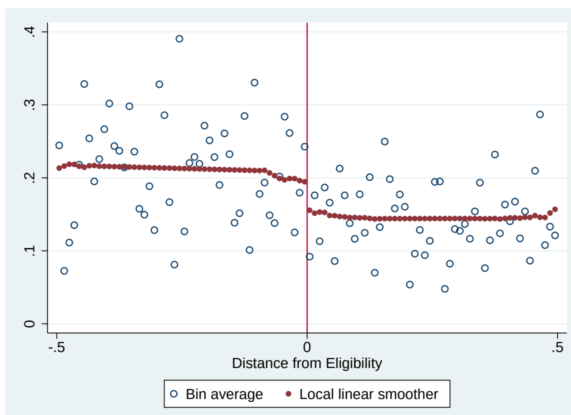
Figure 8: APPENDIX: Public health insurance eligibility and bin size (using weights). The first graph provides the number of observations in each bin for all observations, while the second graph does the same for only observations with a consistent measure of eligibility.



(a) Expected out-of-pocket spending, method 1

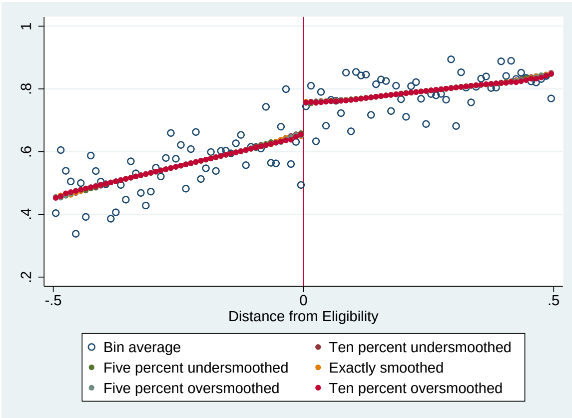


(b) Expected out-of-pocket spending, method 2



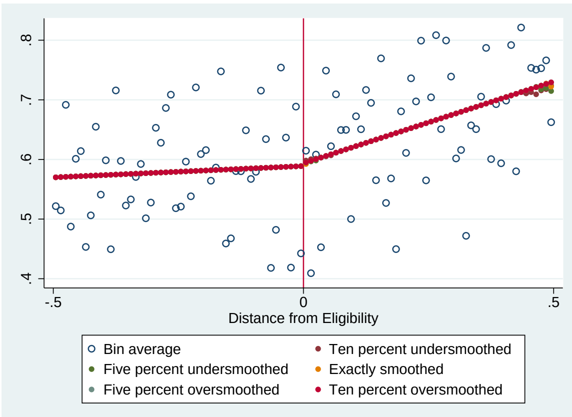
(c) Fraction African American

Figure 9: APPENDIX: Public health insurance eligibility for a child and the child's pre-determined characteristics. The bin average in the first graph expected out-of-pocket spending graph rely on a regression of out-of-pocket spending on demographics and an indicator variable for having any insurance. The second estimates the regression of out-of-pocket spending on demographics on the uninsured only, and then calculates the predicted values for the entire population.

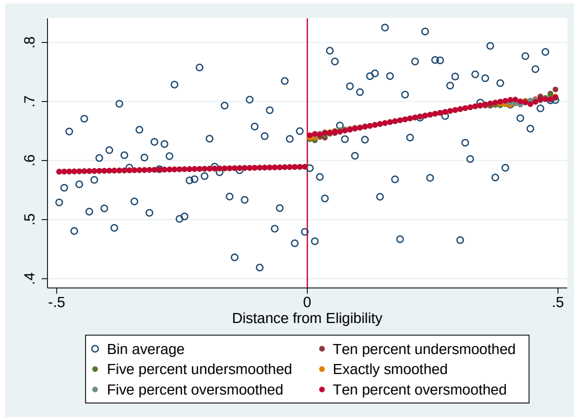


(a) Private insurance

(b) Any insurance



(c) Pap smears



(d) Breast exam

Figure 10: APPENDIX: Public health insurance eligibility for a child and health insurance and health care outcome for the adult; with multiple adjustments to bandwith.

	Polynomial order				
	0	1	3	5	First observation
			Insurance, by source or use		
Private	-0.368***	-0.15***	-0.124***	-0.115***	-0.158***
	(-43.07)	(-8.42)	(-4.64)	(-3.81)	(-10.37)
Any	-0.215***	-0.124***	-0.105***	-0.085***	-0.135***
	(-28.16)	(-7.59)	(-4.19)	(-2.94)	(-9.04)
Public	0.173***	0.042***	0.028***	0.034**	0.036***
	(21.75)	(3.46)	(2.11)	(2.23)	(3.51)
			Spending, by source or use		
Total	-422.06*	-292.73*	-151.90	-181.44	-325.10***
	(-1.75)	(-1.91)	(-1.07)	(-1.12)	(-2.09)
Office-based	-84.30***	-64.33**	-83.72**	-56.18	-59.53*
	(-4.89)	(-2.32)	(-2.33)	(-1.56)	(-1.78)
Out-of-pocket	-42.78***	-41.22*	-52.60*	-39.63	-49.21*
	(-3.92)	(-1.92)	(-1.89)	(-1.11)	(-1.70)
			Preventive care		
Flu shot	-0.036	-0.011	-0.003	0.001	-0.004
	(-4.96)	(-0.72)	(-0.14)	(0.03)	(-0.24)
BP check	-0.074***	-0.046***	-0.029	-0.016	-0.029
	(-7.62)	(-2.96)	(-1.33)	(-0.58)	(-1.46)
Cholesterol check	-0.079***	-0.043***	-0.006	0.027	-0.018
	(-8.51)	(-2.51)	(-0.26)	(0.88)	(-0.84)
Pap smear	-0.111***	-0.078***	-0.062**	-0.053	-0.08***
	(-8.75)	(-3.64)	(-2.01)	(-1.43)	(-2.76)
Breast exam	-0.122***	-0.093***	-0.069**	-0.072**	-0.086***
	(-9.93)	(-4.33)	(-2.35)	(-2.04)	(-3.29)
Mammogram	-0.075***	-0.057**	-0.032	-0.014	-0.057*
	(-5.22)	(-2.19)	(-0.88)	(-0.35)	(-1.67)
Annual exam	-0.085***	-0.060***	-0.04*2	0.001	-0.050**
	(-8.59)	(-3.40)	(-1.86)	(0.02)	(-2.19)
			Self-reported health		
Excellent or	-0.136***	-0.078***	-0.064***	-0.087***	-0.058***
very good	(-14.57)	(-5.46)	(-3.09)	(-3.33)	(-3.07)

Table 4: APPENDIX: The effects (and t-statistics) of a child's public health insurance eligibility on adult insurance outcomes, for alternate specifications, using linear probability models. The first four columns vary the order of the polynomial of the forcing variable. The final column reports the coefficients and t-statistics for the first observations of each adult. All specifications include demographic, state and year fixed effects.
* significant at 10%; ** significant at 5%; *** significant at 1%

	All observations		Baseline		Selection on Observables			
	Elig. Effect	N	Elig. Effect	N	Elig. Effect	N	Elig. Effect	N
					Insurance, by source or use			
Private	-0.208***	50,121	-0.114***	37,029	-0.223***	50,123	-0.219***	50,122
	(-23.93)		(-3.80)		(-26.24)		(-25.81)	
Any	-0.112***	50,121	-0.085***	37,029	-0.123***	50,123	-0.119***	50,122
	(-15.25)		(-2.93)		(-16.06)		(-16.11)	
Public	0.103***	50,121	0.034**	37,029	0.116***	50,123	0.114***	50,122
	(-14.54)		(2.23)		(15.49)		(15.47)	
					Spending, by source or use			
Total	-240.73**	50,121	-181.37	37,029				
	(-2.18)		(-1.12)					
Office-based	-56.18***	50,121	-56.71	37,029				
	(-3.30)		(-1.57)					
Out of pocket	-60.15***	50,121	-39.34	37,029				
	(-6.56)		(-1.10)					
					Preventive care			
Flu shot	-0.019***	34,036	0.001	25,714	-0.017**	34,037	-0.017**	34,039
	(-3.03)		(0.03)		(-2.68)		(-2.62)	
BP check	-0.052***	33,660	-0.016	25,406	-0.054***	33,661	-0.05***	33,663
	(-6.22)		(-0.58)		(-6.49)		(-5.97)	
Cholesterol check	-0.06***	32,545	0.027	24,596	-0.063***	32,546	-0.055***	32,548
	(-6.72)		(0.87)		(-6.91)		(-6.07)	
Pap smear	-0.087***	18,654	-0.053	14,215	-0.097***	18,653	-0.098***	18,653
	(-7.69)		(-1.41)		(-8.59)		(-8.70)	
Breast exam	-0.09***	18,651	-0.071**	14,223	-0.096***	18,650	-0.095***	18,650
	(-7.88)		(-2.02)		(-8.65)		(-8.47)	
Mammogram	-0.065***	12,288	-0.013	9,275	-0.046***	12,290	-0.047***	12,289
	(-4.75)		(-0.31)		(-3.06)		(-3.08)	
Annual exam	-0.069***	33,683	0.001	25,457	-0.065***	33,684	-0.063***	33,686
	(-7.87)		(0.02)		(-7.63)		(-7.29)	
					Self-reported health			
Excellent or Very good	-0.112***	50,121	-0.087***	37,024	-0.115***	50,123	-0.117***	50,122
	(-13.58)		(-3.33)		(-13.98)		(-14.58)	
Age FE	x		x		x		x	
State, year FE	x		x		x		x	
State x year FE							x	

Table 5: APPENDIX: The effects (and t-statistics) of a child's public health insurance eligibility on adult insurance outcomes, for alternate specifications, using linear probability models. The first two columns of point estimates are for the entire sample, including observations with inconsistently measured eligibility, and the preferred sample, excluding such observations. The second pair of columns reports the coefficients and t-statistics for the selection on observables specifications, with varying controls. Sample size for each alternate specification is reported. The estimates are based on a regression with a fifth-order polynomial in family wage income as a fraction of the poverty guideline and, for the first two columns, distance to the eligibility threshold (denominated the same), along with demographic, state and year effects. The final column includes year-state fixed effects.
* significant at 10%; ** significant at 5%; *** significant at 1%