



The effect of Medicaid premiums on enrollment: A regression discontinuity approach



Laura Dague*

Texas A&M University, United States

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ABSTRACT

This paper estimates the effect that premiums in Medicaid have on the length of enrollment of program beneficiaries. Whether and how low income-families will participate in the exchanges and in states' Medicaid programs depends crucially on the structure and amounts of the premiums they will face. I take advantage of discontinuities in the structure of Wisconsin's Medicaid program to identify the effects of premiums on enrollment for low-income families. I use a 3-year administrative panel of enrollment data to estimate these effects. I find an increase in the premium from 0 to 10 dollars per month results in 1.4 fewer months enrolled and reduces the probability of remaining enrolled for a full year by 12 percentage points, but other discrete changes in premium amounts do not affect enrollment or have a much smaller effect. I find no evidence of program enrollees intentionally decreasing labor supply in order to avoid the premiums.

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1. Introduction

Understanding price responsiveness is important for the design of health insurance. The 2010 Patient Protection and Affordable Care Act increases insurance coverage among low-income populations through an expansion of Medicaid and premium subsidies for the purchase of private insurance via health insurance “exchanges”. Whether and how low income families will participate in the exchanges and in states' Medicaid programs depends crucially on the structure and amounts of these premiums, but current knowledge of the price responsiveness of low-income families to health insurance premiums is very limited.

Those at or near Medicaid income eligibility thresholds are less likely than higher income people to be employed at a job providing insurance and less likely to take up employer-provided insurance, suggesting estimates obtained from firm-specific studies may not apply to them. States have historically been restricted

from imposing cost-sharing among Medicaid enrollees, resulting in very limited research on price responsiveness for Medicaid-eligible adults and children. The RAND Health Insurance Experiment found higher coinsurance did not result in poor health except among the poorest (and sickest) sample members (Newhouse, 1993), supporting the idea that the low-income may respond differently to price incentives than the higher-income.

In this paper, I take advantage of the structure of Wisconsin's combined Medicaid/Children's Health Insurance Program to identify the effects of small monthly premiums on the continuity and length of insurance coverage for low-income families enrolled in public insurance using a regression discontinuity design. The program, called BadgerCare Plus, features breaks in premiums by family income level of enrollees, creating groups of families with very similar incomes but different required premiums. I use a 3-year administrative panel of monthly enrollment data for the universe of enrollees for the analysis.

A few studies have considered the impacts of cost-sharing in low income populations by looking at premiums and enrollment in the Children's Health Insurance Program (CHIP). This literature uses quasi-experimental variation in state policies (Marton, 2007; Herndon et al., 2007; Kenney et al., 2007; Marton and Talbert,

* Correspondence to: Bush School of Government & Public Service, 4220 TAMU, College Station, TX 77845, United States. Tel.: +1 979 845 6591.

E-mail address: dague@tamu.edu

2010). These studies have tended to find negative responses to premiums. To the best of my knowledge, no studies have yet considered the effects of premiums on the length of adult enrollment, although Chandra et al. (2010, 2012) study the effects of copayments on demand for health care using a similar design to that used in this paper.

Length of continuous enrollment in Medicaid is important because even though Medicaid coverage is sometimes thought of as implicit, numerous studies have shown that continuous Medicaid coverage is associated with better health outcomes. Bindman et al. (2008) show ambulatory care sensitive hospitalizations are more likely among those with discontinuous Medicaid spells, and Hall et al. (2008) show diabetics with continuous Medicaid coverage have lower health care costs than those with discontinuous coverage. While it is possible those who leave Medicaid switch to employer-sponsored insurance or the individual market (rather than to being uninsured), Lavarreda et al. (2008) find those who switch insurance types are less likely to report a usual source of care. The Oregon Health Insurance Experiment team has shown that Medicaid increases use of preventive care, self-reported health, mental health, and financial well-being, although 2 year clinical outcomes were mixed (Finkelstein et al., 2012; Baicker et al., 2013a). DeLeire et al., 2013 show that for a relatively sick population, Medicaid can decrease hospitalization rates.

I find that an increase in the monthly premium from zero to 10 dollars results in 1.4 fewer months of continuous enrollment for both adults and children. These effects are concentrated in the first few months of coverage: enrollees are 12 percentage points more likely to leave the program within 12 months, and 13–15 percentage points more likely to leave within 6 months. Other discrete changes in premium amounts (for example, increasing the premium from \$10 to \$29) do not affect enrollment.

A second issue with premiums is that they could cause a decline in labor supply in order to avoid having to pay. I also check whether program enrollees appear to be purposefully decreasing their labor supply in order to avoid the required premiums. I use matched data reported by firms for the unemployment insurance program in order to test for this. I find no evidence of such a moral hazard response as a result of the premium requirements.

2. Method

This paper focuses on Wisconsin's joint Medicaid and CHIP program for the non-elderly and non-disabled. Medicaid and CHIP are jointly financed by the federal and state governments. States administer the programs and are required to cover certain groups at specified benefit levels. However, states are allowed flexibility in covering optional groups. Prior to the enactment of the Affordable Care Act, states were required to cover pregnant women and young children up to 133% of the federal poverty level (FPL), older children up to 100% FPL, and parents up to 1996 welfare eligibility levels (below 50% FPL in almost all states). States were not required to provide any benefits to adults without children. While a full discussion of Medicaid is outside the scope of the paper, Gruber (2003) provides background as well as a discussion of the evolution of eligibility rules.

The result of this flexibility has been that considerable state variation exists in income eligibility rules. All states had higher than required income eligibility limits for children and almost all for pregnant women, but most states have a low threshold for parents, with a median limit of 64% FPL (Kaiser, 2011). Wisconsin's income limits were more generous than most states, covering children of all income levels, pregnant women up to 300% FPL, parents and caretaker relatives up to 200% FPL, and childless adults up to 200% FPL.

Prior to 2005, premiums were highly restricted in Medicaid, although CHIP programs had more flexibility. However, because states could obtain waivers for some requirements, especially following the federal Health Insurance Flexibility and Accountability initiative of 2001, exceptions existed. In 2005, the passage of the Deficit Reduction Act allowed states to charge premiums for children and adults with family incomes above 150% FPL. States have some discretion regarding the levels of premiums, but aggregate costs to individuals are capped at 5% of family income. The Deficit Reduction Act further allowed states to disenroll people from coverage due to unpaid premiums. The Kaiser Family Foundation's 50-state survey for fiscal year 2008 indicates 34 states required some premium payment or enrollment fee in their Medicaid or CHIP programs for children, and three for parents (including Wisconsin), either under waiver programs or Deficit Reduction Act provisions (KCMU, 2008).

In February 2008, the state of Wisconsin implemented a major reform in its Medicaid/CHIP programs. The reform included an extension of the income eligibility maximum for parents to 200% FPL and removed the income eligibility cap for children. Promotion and outreach efforts were associated with large increases in enrollment, including among the already income-eligible (Leininger et al., 2011).

With the implementation of reform, newly enrolled adults in families with family income of 150–200% FPL (\$2200 per month for a family of three in 2008) were required to pay a monthly sliding-scale premium beginning at \$10 per person per month, while adults in families income of less than or equal to 150% FPL were not required to pay premiums. For children, this break occurs at 200% FPL, and children also face small copayments for certain health care services beginning at 200% FPL. These sliding scale premiums are described in detail in Table 1. The table shows the maximum per person allowable premium by income level. It also shows average effective per-family premiums, which can be lower because of the 5% cap or higher because more than one person in the family is enrolled and required to pay a premium. For both children and adults, the monthly premium begins at \$10 per person and scales up every 10 percentage points of FPL.¹

2.1. Wisconsin administrative data

I use a set of linked administrative data sets from Wisconsin. Administrative data are well-suited to answer the questions posed here for several reasons. First, respondents to survey data who are enrolled in public insurance may misreport their health insurance enrollment status, called the 'Medicaid undercount' (for a discussion, see Call et al., 2008). Administrative data yield an exact count of enrollees and their enrollment status. Second, I observe exactly the same variables the state uses to determine program eligibility, which is especially important for the regression discontinuity design. In survey data, particularly with respect to income, responses can be imprecise and are often grouped at rounded numbers. Finally, sample sizes in the administrative data are large even though I consider only one state, which allows me to use narrow bandwidths in estimation. A limitation is the inability to observe outcomes for individuals who are not enrolled, the consequences of which are discussed below.

Eligibility records are from Wisconsin's CARES database and are monthly from February 2008 to December 2010. The eligibility data have numerous measures of individual and household

¹ Nominal changes in the premiums occurred in March 2009, with premiums increasing by \$1–6 per month for children and decreasing by \$2–13 per month for adults, with the first required premium remaining \$10. The thresholds did not change.

Table 1
BadgerCare plus premiums effective February 2008.

FPL	Per person maximum		Average effective premium
	Child	Parent	
<150%	0	0	0.00
150–160%	0	10	13.70
160–170%	0	29	40.40
170–180%	0	73	99.00
180–190%	0	130	135.10
190–200%	0	201	148.70
200–230%	10	n/a	25.00
230–240%	15	n/a	30.60
240–250%	23	n/a	43.10
250–260%	31	n/a	54.00
260–270%	41	n/a	70.40
270–280%	52	n/a	88.30
280–290%	63	n/a	101.50
290–300%	76	n/a	121.80
300%+	\$90.74	n/a	148.00

Notes. A family's monthly premium obligation is calculated by multiplying the number of enrollees by their respective premiums and summing, subject to a cap of 5% of family income. Only family members who owe a premium are disenrolled because of lack of payment. Average effective premium is calculated at the family level only for families who owe premiums and includes unpaid premium obligations. Premiums listed as n/a indicates a lack of income eligibility for a category.

characteristics, including gender, age, race, household composition, and employment status in addition to monthly enrollment and premium levels. A key feature of these data is observation of the state's exact determination of family income, both in dollars and as %FPL. Wisconsin assigns FPL based on gross income and family size. While income is initially self-reported by applicants, accuracy of reported income is verified through documentation such as paycheck stubs or direct employer verification.

I also observe quarterly wage income both pre- and post-enrollment by merging to a third party data source, the state's mandatory wage reporting system for unemployment insurance. It contains wages for all employees whose employers are subject to unemployment insurance laws. In Wisconsin, this represents 94% of all employed workers.²

Total premiums due at household and individual level are recorded in the data. Enrollees are able to pay premiums through wage withholding, monthly bank transfers, or direct payment with a check or money order. Failure to pay premiums within 2 months results in disenrollment. If disenrollment occurs as a result of nonpayment of premium, beneficiaries are subject to a 6 month restrictive re-enrollment period and must pay any past due premiums at the time of re-enrollment unless family income has dropped to the point where a premium would not be required. This requirement is meant to prevent the possibility of beneficiaries paying premiums only at times they need to use services, and I discuss this possibility further below.

For the analysis, I focus on a population of new child and parent or caretaker enrollees who enrolled between March 2008 and September 2009. I consider only new enrollees for several reasons. Some existing enrollees were grandfathered in for some provisions post reform, and the set of existing enrollees as of February 2008 is likely to exclude the most price sensitive enrollees, biasing against finding effects of premiums and violating the regression discontinuity assumption.³ In addition, I cannot observe start dates

for spells that begin prior to January 2006. I end the sample in September 2009 so that all enrollees can be observed for at least 1 year following enrollment. I do not use enrollees from the first month of the new program (February 2008) because they are different on several dimensions; most importantly, many of them were automatically enrolled into the program. Those that were automatically enrolled have different average observable characteristics than enrollees in other months, and automatic enrollment is not directly observable in the data.⁴ New enrollees may be more price sensitive than existing enrollees, but are also likely to be particularly policy relevant since they are responding to the insurance expansion and/or shocks to their income or health status.

After making these restrictions, the sample consists of 295,498 new child enrollees and 162,296 new parent or caretaker enrollees. Table 2 summarizes key covariates for the adult and child new enrollee samples. The average age for adults is 34. Most adult enrollees are white females in rural counties who have no more than a high school education. The average number of children in a household with an adult enrollee is just over two, and the average number of adults is just under two. The average adult enrollee has a family income of 90% FPL. Adult enrollees are overwhelmingly citizens who speak English as their main language in the home. The average length of enrollment for an adult is just over 10 months when capped at 14 months to deal with censoring and differing enrollment dates (the outcome used below); uncapped, it is just over 13 months. Of adult enrollees, 77% had at least one wage worker in their case at the time of enrollment.

The average age of children in the enrollment sample is eight. They are evenly split between girls and boys. Relatively more children are reported to be of Hispanic origin than among adults. They are more likely to be citizens but less likely to have English as a main language. Households with child enrollees have more children than households with adult enrollees on average, but fewer adults. Average family income is roughly the same as in the sample of adults, but children stay enrolled for a longer time, averaging more than 11 months when capped and 16 months when uncapped. The proportion of households with a wage worker is lower at 70%.

Public insurance enrollment status is constructed from the eligibility data. As outcomes, I use length of enrollment spell, a dummy variable for whether or not a spell lasted longer than 6 months, and a dummy variable for whether a spell lasted longer than 12 months. In the sample, spells can last up to a maximum of 2.5 years, depending on when the beneficiary enrolled, and 30% of the sample has a spell enduring for the entire period.

2.2. Regression discontinuity design

The design of BadgerCare Plus creates programmatic breaks in premiums by income level, as described above. This suggests an appropriate application for a sharp regression discontinuity design, with cutoff points at each of the income thresholds where changes in premiums occur.

I follow Lee and Lemieux (2010) in using a local linear regression estimation approach. The exact specification of the RD estimator is

$$Y_i = \alpha + \beta(X_i - x_0) + \tau W_i + \gamma(X_i - x_0)W_i + \varepsilon_i \quad (1)$$

² Calculated from 2,772,889 workers covered by unemployment insurance in 2008 in Wisconsin (Bureau of Labor Statistics, 2009) out of 2,937,903 employed members of labor force (Wisconsin Department of Workforce Development, 2008).

³ Some parents and children with incomes above 150% under the pre-reform policy had been required to pay a premium. As a result, through the differential attrition among premium payers and non-premium payers would result in a break in the

types of existing enrollees at the 150% threshold. I do not use any pre-post analysis in this paper since many elements of the program changed in February 2008, but Leininger et al. (2011) considers the effects of the reform itself on program exit.

⁴ See Leininger et al. (2011) for further discussion and a comparison of February enrollees to others.

Table 2
Sample statistics for administrative data.

	Adults				Children			
	Below 150% FPL		Above 150% FPL		Below 200% FPL		Above 200% FPL	
	Mean	SD	Mean	SD	Mean	SD	Mean	SD
Age	33.51	9.34	36.31	9.42	7.78	5.65	8.67	5.37
Female	0.59	0.49	0.58	0.49	0.50	0.50	0.48	0.50
Non-Hispanic White	0.65	0.48	0.80	0.40	0.52	0.50	0.78	0.41
Black	0.15	0.36	0.06	0.24	0.15	0.36	0.04	0.19
Hispanic	0.09	0.28	0.06	0.23	0.16	0.37	0.07	0.25
Other/unreported race	0.07	0.26	0.05	0.21	0.09	0.28	0.05	0.22
Citizen	0.96	0.20	0.97	0.18	0.99	0.10	1.00	0.06
English main language	0.96	0.20	0.97	0.18	0.92	0.27	0.97	0.17
More than high school education	0.18	0.39	0.24	0.43	0.17	0.38	0.23	0.42
Resident of urban county	0.33	0.47	0.39	0.49	0.32	0.46	0.40	0.49
Number of children in household	2.13	1.21	2.01	1.04	2.66	1.43	2.52	1.21
Number of adults in household	1.69	0.61	1.84	0.55	1.57	0.66	1.86	0.51
Family income %FPL	71.60	49.95	172.30	14.28	78.33	59.48	287.49	319.21
Length of enrollment spell	10.69	3.81	7.88	4.89	11.33	3.32	9.87	4.25
Wage worker in household	0.75	0.43	0.88	0.33	0.70	0.46	0.84	0.37
Number of enrollees	132,044		30,252		187,997		16,958	

Source. Author's calculations from Wisconsin administrative data.

Here, Y_i is the outcome under consideration, X_i is family income as a percent of the federal poverty level, x_0 is the FPL threshold at which the premium changes, W_i is an indicator for treatment, and ε_i is a random error term. Treatment is defined as either whether the individual was at an income level required to pay a premium, or whether the individual was required to pay a higher premium,

depending on the threshold. The treatment effect of interest is τ . The coefficients β and γ allow the slope of the regression to differ on either side of the cutoff x_0 . I implement all estimates using a local linear regression approach with triangular kernel weights. I include robustness checks to various bandwidths as part of the analysis.

While the method and data provide strong support for a causal interpretation of the estimates, there is one important caveat for the analysis. Individuals self-select into the program, and I have data only on people who actually enroll in the program. I therefore provide a theoretical framework in the Appendix for understanding the potential consequences of this issue, and show that any bias is against finding an effect.

Intuitively, what matters for this application is differential take-up rates across the threshold for treatment status, which seems possible: if premiums discourage continued enrollment, they may also discourage take up. Nationally, the Medicaid and Children's Health Insurance Program take up rates are less than 100% for both populations eligible without premiums and those required to pay premiums (Currie, 2006).⁵

The selection bias will be positive if the outcome for those not required to pay premiums is larger than the outcome for those who are not enrolled but would have to pay premiums if enrolled in the program. This is consistent with those who do not enroll having a willingness to pay less than those who choose to enroll resulting in shorter enrollment spells. Combined with a negative treatment effect as found below, a positive selection bias indicates the true effect would be even more negative.

3. Results

3.1. The effect of premiums on enrollment

I find premiums reduce the length of enrollment in Medicaid for both adults and children, with the largest effects at the margin of no

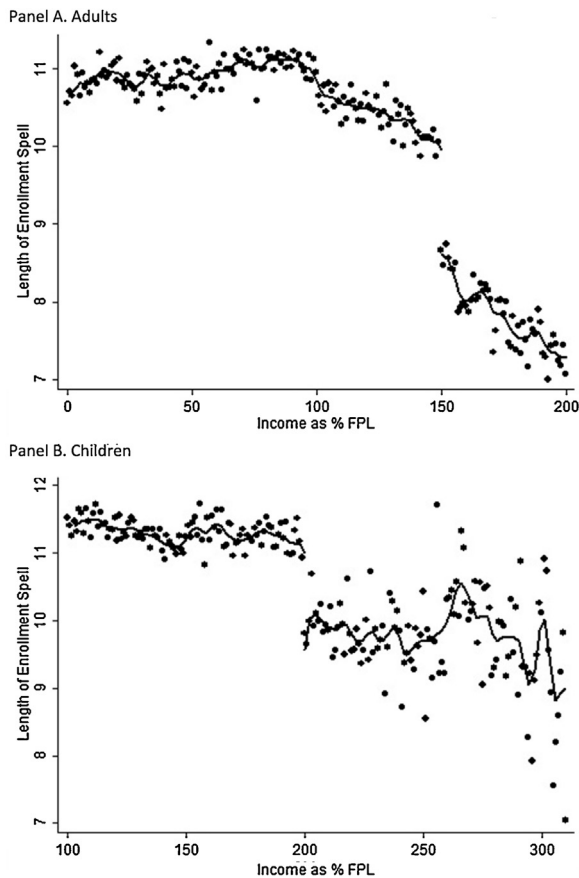


Fig. 1. Effect of premium requirement on length of enrollment spell. Notes. Outcomes calculated in bins of 1% FPL, estimated local linear functions at bandwidth of 5% superimposed. Discontinuity estimate for Panel A is -1.3 with standard error (0.21); for Panel B, -1.4 with standard error (0.32).

⁵ The reasons eligible individuals might choose not to participate in programs that benefit them are not well understood. Recent experimental evidence from Oregon suggests that Medicaid is superior to being uninsured on several measures from the perspective of the beneficiary, including financial well-being (Finkelstein et al., 2012). Moffitt (1983) models non-participation using stigma costs; other types of costs such as transaction costs or informational costs have also been proposed. Time-inconsistent preferences could also explain the take up problem (Currie, 2006).

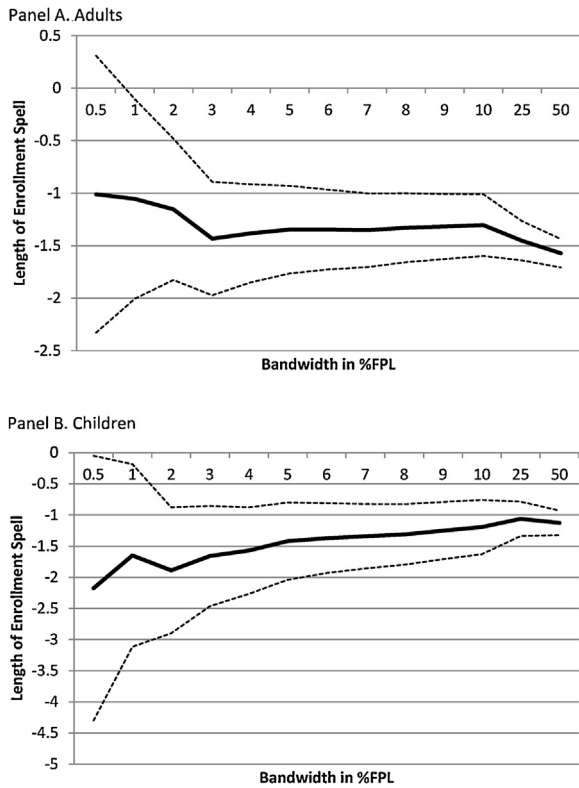


Fig. 2. Point estimates and standard errors as a function of bandwidth, enrollment. Notes: Figures show regression discontinuity estimates as a function of bandwidths. Dotted lines represent 95% confidence intervals.

premium payment to a \$10 monthly premium. As discussed above (and shown in Table 1), adults in families with incomes greater than or equal to 150% FPL are required to pay a monthly premium beginning at \$10. Children in families with incomes greater than or equal to 200% FPL have a required premium also beginning at \$10 per month.⁶ When averaged by income group, those below the premium thresholds have much longer average spell lengths than those above the premium thresholds (see Table 2). Much of this difference in enrollment occurs in the first few months of the spell. For this reason, I consider the probability of an enrollment spell lasting longer than 6 months in addition to length of spell. I also look at the probability of a spell lasting longer than 12 months, which is when eligibility status is re-examined.

Panel A of Fig. 1 illustrates the discontinuity in the outcome defined as total months continuously enrolled, considering the break in premium requirement status at 150% FPL for adults. In the graph, the x-axis shows the assignment variable with the cutoff point at 150% FPL, and the y-axis shows the outcome variable. I plot the average value of the outcome in bins of one percentage point FPL, with the estimated outcome functions superimposed on either side of the cutoff point. There is an obvious break in the number of months enrolled at 150% FPL. Estimation of the local linear regression described above results in an estimated difference of outcomes of -1.3 months, with a heteroskedasticity-robust standard error of 0.21 months at a bandwidth of five percentage points FPL. As Panel A of Fig. 2 illustrates, the estimate is robust to alternative bandwidth choices.

Panel B of Fig. 1 illustrates the discontinuity in the same outcome focusing on the children and the change in premium

⁶ Since small copayments are also required for children starting at 200% FPL, this effect is the joint effect of requiring the \$10 premium and the copayments.

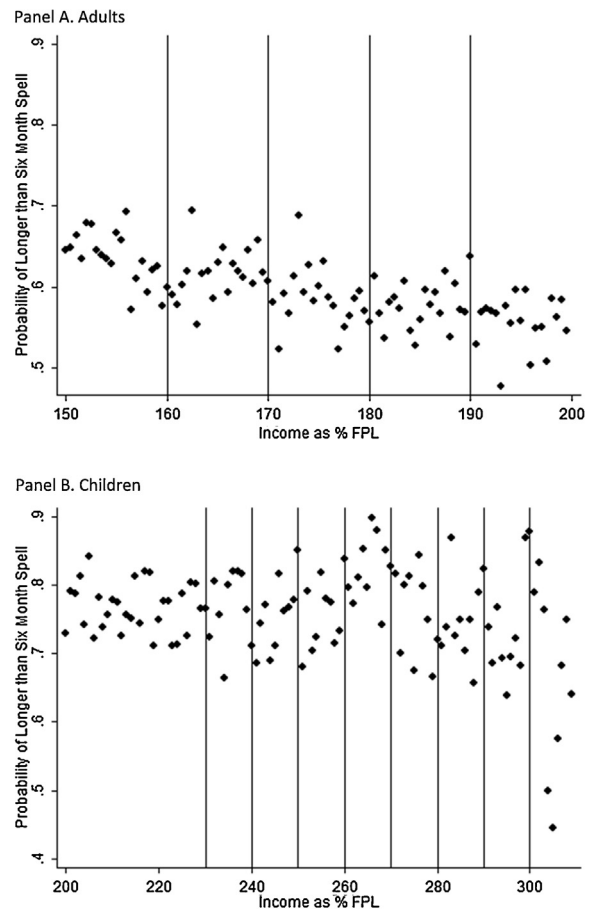


Fig. 3. Scatter plot, other premium cutoff points. Notes: Outcomes calculated in bins of 1% FPL. Vertical lines represent discontinuities in premium amounts. Discontinuity estimates available in Table 3.

requirement status at 200% FPL. In the graph, the x-axis represents the assignment variable with the cutoff point at 200% FPL, and the y-axis shows the outcome variable. I plot the average value of the outcome in bins of one percentage point FPL, with the estimated outcome functions superimposed on either side of the cutoff point. The estimated difference in outcomes is -1.4 months, with a heteroskedasticity-robust standard error of 0.32 at a bandwidth of five percentage points. As Panel B of Fig. 2 illustrates, this estimate is also robust to alternative bandwidth choices.

Breaks in the premium schedule also occur every 10 percentage points above 150% for adults and 200% for children, as indicated in Table 1. Panel A of Fig. 3 is a scatter plot of the average length of enrollment spell with bins of one percentage point FPL, focused on just those observations above 150% FPL for adults. Grid lines are drawn where breaks in the premium amount occur. While a downward trend is certainly evident, breaks at the cutoff points are not obvious. I test each of these discontinuities for all three enrollment outcomes in separate regressions, with results reported in Table 3. The only statistically significant results are found at the 170% cutoff point, where premiums change from \$29 to \$71 per person. This point represents the largest difference in average effective premiums. While these results are only reported for a bandwidth of 9.9 percentage points (using almost all of the data between breaks), the basic conclusions are unchanged for smaller bandwidths.

For the sample of children above the 200% discontinuity and below 320% FPL, Panel B of Fig. 3 plots the average length of an enrollment spell in bins of one percentage point FPL. Grid lines are drawn where breaks in the premium amount occur. No downward

Table 3
Summary of regression discontinuity results.

	Outcome			Observations
	Probability of >6 month spell	Probability of >12 month spell	Length of spell	
Adults				
150%	-0.129 <i>0.021</i>	-0.118 <i>0.023</i>	-1.347 <i>0.213</i>	9035
160%	0.000 <i>0.018</i>	0.029 <i>0.018</i>	0.163 <i>0.179</i>	14,636
170%	-0.039 <i>0.019</i>	-0.038 <i>0.019</i>	-0.496 <i>0.187</i>	12,840
180%	0.005 <i>0.021</i>	-0.007 <i>0.021</i>	-0.068 <i>0.211</i>	10,685
190%	0.002 <i>0.022</i>	-0.035 <i>0.022</i>	-0.172 <i>0.221</i>	9581
Children				
200%	-0.149 <i>0.031</i>	-0.116 <i>0.037</i>	-1.419 <i>0.317</i>	3174
230%	-0.032 <i>0.031</i>	-0.030 <i>0.036</i>	-0.227 <i>0.312</i>	3615
240%	-0.082 <i>0.036</i>	-0.059 <i>0.040</i>	-0.721 <i>0.351</i>	2965
250%	-0.025 <i>0.037</i>	0.021 <i>0.044</i>	-0.147 <i>0.377</i>	2469
260%	0.060 <i>0.040</i>	0.085 <i>0.046</i>	0.719 <i>0.398</i>	2003
270%	-0.040 <i>0.042</i>	-0.020 <i>0.051</i>	-0.346 <i>0.437</i>	1648
280%	0.006 <i>0.056</i>	-0.039 <i>0.062</i>	-0.162 <i>0.543</i>	1361
290%	0.090 <i>0.059</i>	0.011 <i>0.066</i>	0.335 <i>0.600</i>	1018
300%	0.097 <i>0.058</i>	0.110 <i>0.072</i>	0.873 <i>0.598</i>	766

Source. Author's calculations from Wisconsin administrative data.

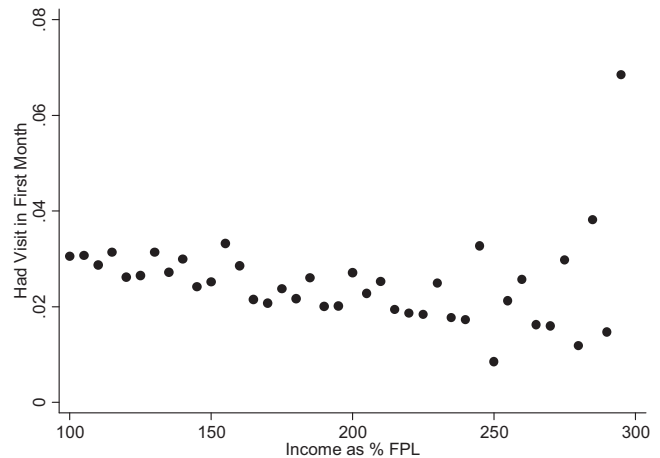
Notes. Reported results at 150% and 200% use a bandwidth of 5; all others use a bandwidth of 9.9 percentage points FPL. Robust standard errors in italics. Results in bold statistically significant at 5% level.

trend in enrollment spells is evident in this population, but the variance in spell length is much higher than for adults. Table 3 summarizes the results for all three enrollment outcomes at these cutoff points. The only statistically significant results are at the 240% cutoff, and they are not robust to alternative bandwidth choices.

Together, these results indicate the premium requirement results in shorter enrollment spells for both adults and children. There is a negative correlation between the amount of the premium and enrollment (which is not separately identified from the negative correlation between income and enrollment). However, since discontinuities in enrollment outcomes are smaller or zero at cutoff point other than the zero to \$10 margin, the existence of the premium requirement may be more important than the dollar amount itself. Consumers appear more responsive to a change in the premium from 0 to \$10 than to larger relative changes in the dollar amount of the premium. The premium response at the zero to \$10 margin is consistent with an arc elasticity of 0.06–0.07.⁷

This result could mean several things. First, fixed costs could be associated with paying the premium. The state allows automatic deductions and payment by mail in the interest of making it easier for families to meet their monthly premium obligations, but it is possible even a small cost associated with paying premiums is

Panel A. Proportion of Kids With An Emergency Visit in First Month



Panel B. Point Estimates and Standard Errors as a Function of Bandwidth

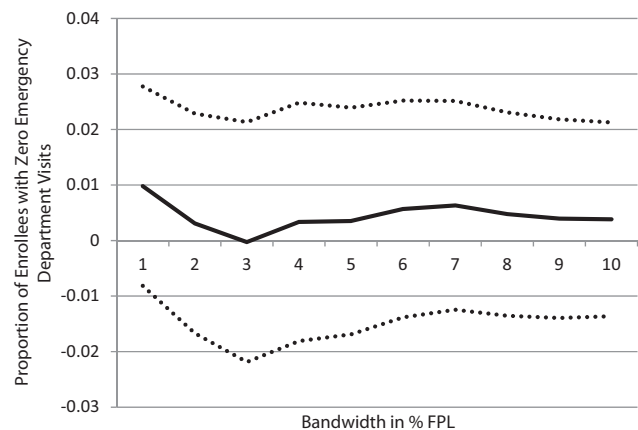


Fig. 4. Premium avoidance and selective enrollment, children. Notes. The outcome in Panel A is calculated in bins of 5% FPL. The discontinuity estimate at that bandwidth is 0.003 with standard error (0.014). Panel B shows regression discontinuity estimates as a function of bandwidths. Dotted lines represent 95% confidence intervals.

sufficient to discourage them from making the payments.⁸ Second, there may be something special about the price of zero resulting in non-linear demand.⁹ Finally, it could be premium payers enroll only when they are sick and drop out once they have received care. I provide a test of this possibility in the following section.

3.2. Premium avoidance and moral hazard

One possibility suggested by the premium results is that premium payers only sign up for the program when they need care and dropping out after receiving it. To check if the results are driven by this type of enrollment pattern, I treat whether or not the enrollee had an emergency department visit in the first month of their enrollment spell as the outcome variable. If those who are required to pay premiums are differentially likely to wait to enroll until they

⁸ Unfortunately I am unable to observe the method of payment, which would provide a clear test of this possibility.

⁹ The literature from development economics has suggested that zero prices are different (Ashraf et al., 2010; Cohen and Dupas, 2010) as has behavioral economics (Shampanier et al., 2007).

⁷ Calculated for adults as $(1.347/10.69)/(10/0.5(10))$ and for children as $(1.419/11.33)/(10/0.5(10))$, using the coefficients from the RD as the change in months enrolled and the average months enrolled for those below the threshold as the baseline quantity.

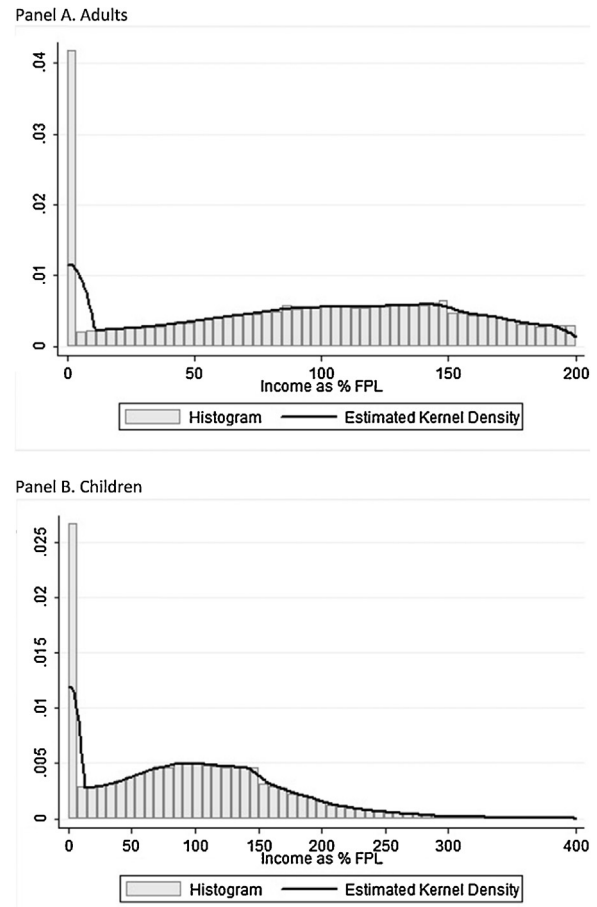
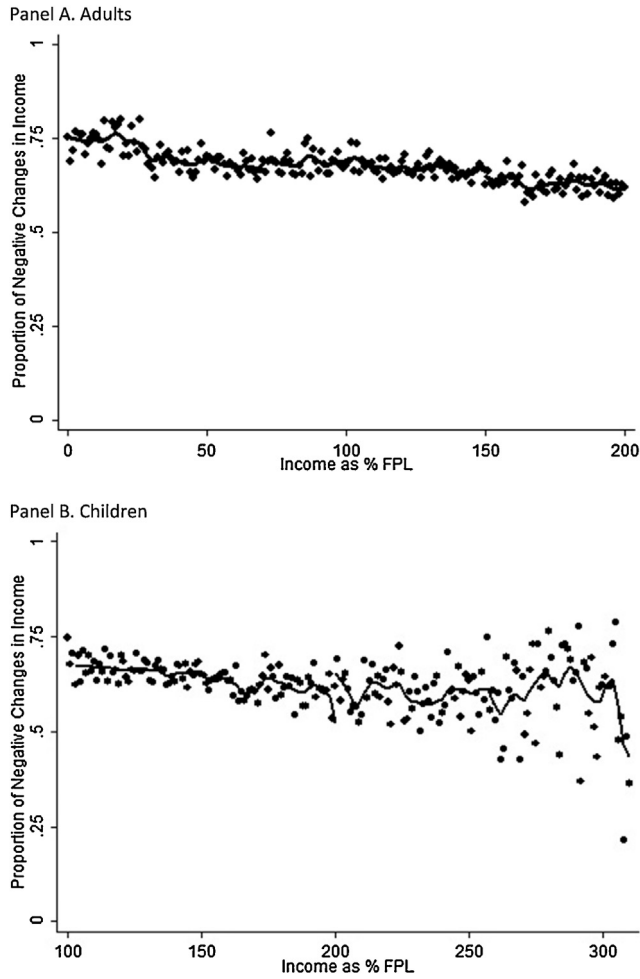


Fig. 6. Histogram and Kernel density, number of enrollees.

Fig. 5. Discontinuity estimates of proportion of negative income changes, cases with wage workers. Notes. Proportion of negative income changes calculated in bins of 1% FPL, estimated local linear functions at bandwidth of 5% superimposed. Discontinuity estimate for Panel A is -0.016 with standard error (0.02); for Panel B, 0.13 with standard error (0.04).

need a high-cost service, we should observe a discontinuity in this outcome at the cutoff point due to premium requirements.¹⁰

Panel A of Fig. 4 illustrates the data on children with the proportion of enrollees with emergency visits in the first month on the y-axis and %FPL as the x-axis, in bins of five percentage points FPL. As indicated in Panel B, I find no evidence of any effect at any bandwidth within 10 percentage points FPL of the cutoff point, indicating enrollees required to pay premiums are equally likely to enroll at the time of a health shock as those who are not required to pay premiums. While not displayed, I consider the same outcome for adults, finding again no differences across the threshold (point estimate 0.008, standard error 0.009).

This paper uses the assignment to treatment by income level for the basis of a regression discontinuity analysis. The fundamental question for identification is whether those just above and just below the cutoff point are truly comparable, and it hinges on both the ability of the individuals to control their assignment to treatment and the benefit to them from doing so. Clearly, individuals are

¹⁰ This is not necessarily a definitive test of differences in usage behavior; in particular, if gaining insurance is associated with an increase in usage this may show instead the relative strength of such an “access” effect. Regardless, finding a difference would be suggestive of an invalid identification strategy.

able to control their general level of income; it is not exogenously determined.¹¹ If they are able to precisely sort around the discontinuity, then the continuity assumption would be violated. The key to this is precisely, as discussed by Lee and Lemieux (2010) in detail. In essence, they show if agents have only imprecise control over the assignment variable (so it contains stochastic error), then variation in treatment is as good as randomized close to the cutoff point. I perform several tests in order to check whether income appears to be a good assignment variable in this case.

One possibility is that enrollees underreport income in order to avoid the premium. To check whether individuals appear to be reporting their income correctly, I compare wage income reported by individuals (measured in the state eligibility data) to their reported wage income to the state by firms (measured in the unemployment insurance data). The average difference between total wages as measured in the average monthly unemployment insurance data and total monthly wages as measured by the

¹¹ Use of an assignment variable agents may have some control over is not unprecedented in the literature. Card et al. (2007) study the effect of unemployment insurance benefits on unemployment duration using months employed as an assignment variable, but firms have an incentive to manipulate employment at the cutoff. They use several specification tests to look for evidence of non-random selection at the discontinuity, a strategy I also follow here. With respect to income in particular, Chandra et al. (2010) use %FPL as an assignment variable and Lalive et al. (2006) use partly income-based eligibility rules to look at the effects of wage replacement rates on unemployment durations; neither paper considers the possibility of sorting on income.

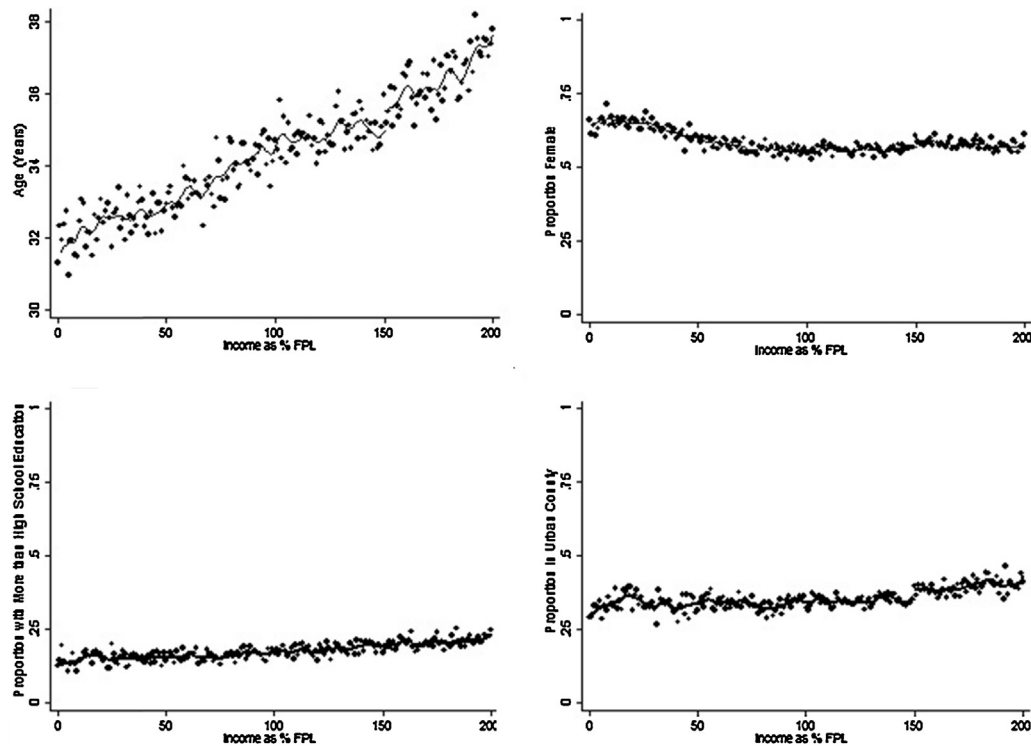


Fig. 7. Regression discontinuity plots for pre-treatment covariates, adults. Notes. Outcomes calculated in bins of 1% FPL, estimated local linear functions at bandwidth of 5% superimposed.

eligibility data is \$28 with a standard deviation of \$1158. No statistically significant differences in reporting behavior exist across the thresholds.

A second possibility is that enrollees adjust income prior to enrolling to avoid paying a premium, either by reducing hours at a current job or switching from the formal to informal sector. I consider whether the proportion of positive or negative changes in income varies discretely at the cutoff points. If enrollees are sorting, we would expect to see more frequent downward changes in income from those just above the cutoff than from those just below. To test this, I restrict attention to a subsample of enrollees who have at least one household member with wage income in the quarter prior to enrolling in the program. I test for differences across the threshold using two different outcomes: the dollar amount of changes in income and a dummy variable equaling one if the enrollee had a negative change in income.

For adults, the subsample of 125,697 enrollees with a wage worker in their household is just over 77% of the full sample and is almost 90% of the sample within a bandwidth of five percentage points FPL of the cutoff point. I compute the difference in total household earnings in the quarter prior to enrollment and the quarter of enrollment. Less than 1% of the sample has no change in total quarterly household earnings. The majority of changes are decreases, with 68% having decreases in income. The average change in quarterly earnings is a decrease of \$1865 (standard deviation \$4006).

There is no evidence of statistically significant differences exist in the dollar amount of any change in income at most bandwidths. In the test of the proportion of changes in income that are negative around the threshold, only the smallest bandwidths tested show any differences. Panel A of Fig. 5 plots the proportion of negative changes in bins of size 1% FPL. Local linear functions of the predicted

proportion of negative changes are superimposed on the plot. At the displayed bandwidth of five percentage points FPL, the estimated difference is -0.016 with a standard error of 0.023 .

To focus in particular on intensive margin changes, which are the main concern as they would imply precise sorting, I restrict attention to the subsample of cases in which no one had a change in job status, eliminating all cases in which one or more members were not working at the same firm in both quarters. This leaves me with 68,513 enrollees in the sample of adults. The average change becomes a decrease of \$786 and 57% of changes were negative. In this subsample, no statistically significant differences exist in the proportion of negative changes around the cutoff point at most bandwidths, and when there appears to be one, it is positive. This result is not driven by a lack of observations; even in the subsample nearly 5000 enrollees remain within the 5% bandwidth. At a bandwidth of 5% FPL, the estimated difference is -0.037 with a standard error of 0.031 .

Performing the same analysis for the sample of children and the 200% cutoff, in the subsample of 74,958 enrollees with a wage worker in their household, the average decrease in income is \$1831 (standard deviation \$4277) between the quarter prior to enrollment and the quarter of enrollment. In the amount of change, I do find statistically significant differences across the cutoff point. However, treatment effects are negative, indicating those just below had smaller decreases in income than those just above the cutoff, which is inconsistent with a hypothesis of manipulation. Of changes in income, 64% were negative, and the difference in the proportion of negative changes is not statistically significant at most bandwidths. Panel B of Fig. 5 plots this outcome. In the subsample with no changes in job status, of the 46,438 child enrollees 55% had a negative income change in their case. Very similar results hold for this subsample.

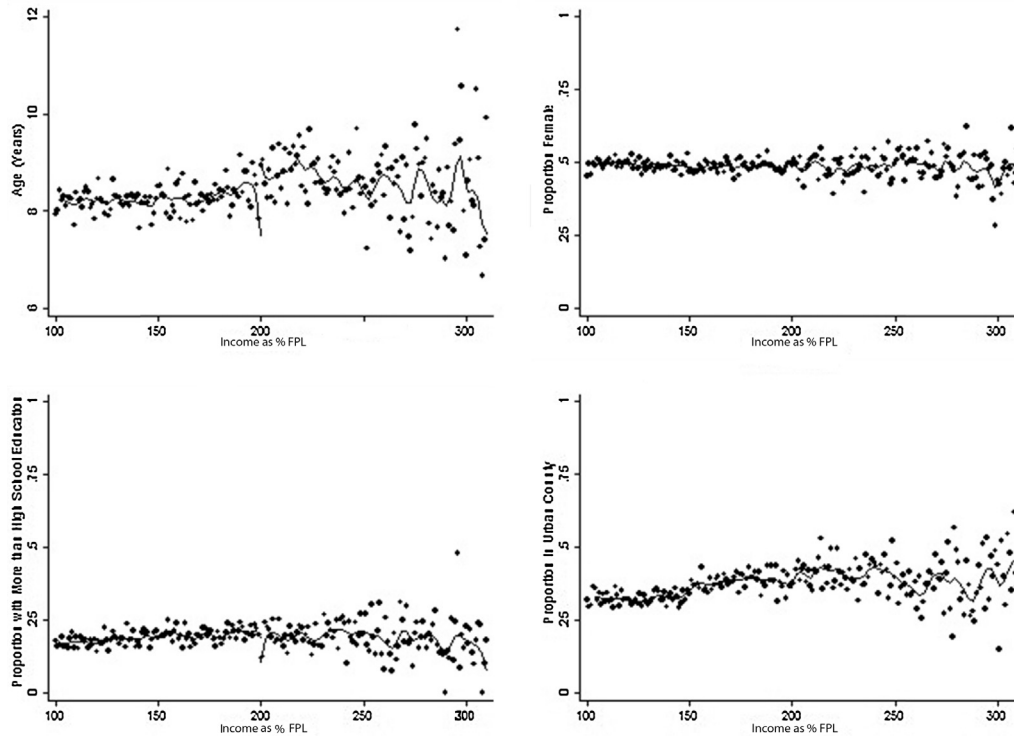


Fig. 8. Regression discontinuity plots for pre-treatment covariates, children. *Notes.* Outcomes calculated in bins of 1% FPL, estimated local linear functions at bandwidth of 5% superimposed.

The issue of sorting could potentially be resolved by examining the distribution of the assignment variable.¹² However, in this application we might ex-ante expect to see a discontinuity in the density because eligible individuals are not required to enroll in the program. Those below the cutoff point have no monetary incentive to prevent enrollment although those above the cutoff point do since a monthly premium is a necessary condition for continuous enrollment. Those whose willingness to pay for insurance is below the amount of the monthly premium would not be expected to enroll.

I address the potential bias from this compositional change above and in the Appendix. However, I also include the density of enrollees for completeness. Panel A of Fig. 6 shows the density of enrolled adults and Panel B of the same figure shows the density of enrolled children. There are indeed fewer enrollees among the premium-payers just above the thresholds.

3.3. Other validity checks

I check whether discontinuities exist in the densities of other covariates at the cutoff point, which would indicate the continuity assumption is violated. I consider age, sex, geography, and education level. Figs. 7 and 8 display corresponding plots of the average values of each of the covariates in one percentage point bins along with the estimated regression lines in the sample of adults and the sample of children respectively. In estimates of Eq. (1) using these covariates as the outcome variable and a bandwidth of five percentage points FPL, no statistically significant differences (at $p < 10$) are

evident, with one exception: age of children. However, this result is not robust to other choices of bandwidth. These patterns in the data are consistent with the conclusion that treatment status is unrelated to sample composition and supports identification.

As a final check, I perform a set of placebo tests. At points in the conditional distribution where program status is unchanged, no discontinuities should exist in the outcome variables. While I do not show graphical representations of these results, Table 4 reports

Table 4
Tests for discontinuities at alternative cutoff points.

Cutoff point in %FPL	Probability of >6 month spell	Probability of >12 month spell	Length of spell
<i>Adults</i>			
100	-0.019 <i>0.017</i>	-0.038 <i>0.021</i>	-0.264 <i>0.173</i>
130	-0.029 <i>0.017</i>	-0.009 <i>0.021</i>	-0.188 <i>0.178</i>
140	0.001 <i>0.018</i>	0.013 <i>0.022</i>	0.052 <i>0.184</i>
145	0.004 <i>0.017</i>	-0.007 <i>0.021</i>	-0.031 <i>0.177</i>
155	0.040 <i>0.024</i>	0.025 <i>0.025</i>	0.197 <i>0.243</i>
<i>Children</i>			
150	0.031 <i>0.016</i>	0.050 <i>0.020</i>	0.452 <i>0.169</i>
180	-0.001 <i>0.020</i>	0.019 <i>0.026</i>	-0.054 <i>0.210</i>
190	0.007 <i>0.021</i>	-0.011 <i>0.029</i>	0.212 <i>0.218</i>
195	0.072 <i>0.023</i>	0.082 <i>0.030</i>	0.836 <i>0.241</i>
205	0.050 <i>0.033</i>	0.019 <i>0.039</i>	0.248 <i>0.355</i>

Source. Author's calculations from Wisconsin administrative data.
Notes. Table reports results of treating alternative %FPL as cutoff point in regression discontinuity design. Robust standard errors in italics.

¹² McCrary (2008) proposes a nonparametric test for determining whether or not the density of the assignment variable is continuous at the cutoff point. However, he makes the important point that continuity of this density is neither a necessary nor a sufficient condition for identification. This is because of the possibility of selective attrition, which implies a discontinuity in the density but not necessarily bias.

the tested discontinuities and the resulting estimates and standard errors at a bandwidth of five percentage points FPL for each of the main premium results. I find no statistically significant discontinuities in the outcome variables at these false discontinuities in the sample of adults, and none of the falsified treatment effects are anywhere close to the magnitudes of those found at the true discontinuity. This is consistent with the interpretation of the 150% discontinuity in the outcomes as a relevant and non-anomalous result.

For the sample of children, I find statistically significant enrollment effects at the 195% cutoff and at the 150% cutoff in some of the enrollment outcomes. Further exploration of the 195% cutoff indicates it is extremely sensitive to bandwidth choice and therefore not a robust effect. The effects at the 150% cutoff are very small, but merit further discussion because of the importance of the 150% cutoff for adults. A possible explanation for this difference is family-level enrollment spillover effects as found in [Sommers \(2006\)](#).

4. Conclusions

In this paper, I find large behavioral responses to a relatively small premium requirement for Medicaid enrollees in Wisconsin. A \$10 premium requirement makes enrollees 12–15 percentage points more likely to exit the program, but no or relatively small effects are found for other large discrete changes in premiums. The implication of these findings is that the premium requirement itself, more so than the specific dollar amount, discourages enrollment. These results are consistent for both adult (parent) and child enrollees.

These results are not driven by moral hazard in enrollment. Enrollees who are required to pay premiums are equally likely to have visits in the first month of enrollment as those who do not, and there is no evidence that enrollees are manipulating income either by misreporting or by altering labor supply in order to avoid the premium payment.

The results are broadly consistent with work on the State Children's Health Insurance Program which has used hazard models to look at changes in premiums. [Herndon et al., 2007](#) study changes in premiums in Florida's SCIP program among children in families from 100% to 200% FPL and find an increase from \$15 to \$20 per month resulted in a 55–61% decrease in the length of enrollment. [Kenney et al. \(2007\)](#) find mixed results across three states: in Kentucky, where a \$20 premium was introduced for kids in families from 150% to 200% FPL and resulted in a 30% increase in the rate of exit; in New Hampshire, where premiums increased by \$5 per month for children 185–300% FPL and resulted in an 11% increase in the rate of exit; and in Kansas, where premiums increased by between \$20 and \$30 per month for children 151–200% FPL resulted in no change in the rate of exit. [Marton \(2007\)](#) and [Marton and Talbert \(2010\)](#) study a newly introduced \$20 premium for Kentucky children in families 150–200% FPL and find a 92% increase in the short run exit rate, but no additional effect in the long run. None were able to consider differential premium amounts in the same state or moral hazard in enrollment.

The finding that the existence of a premium discourages enrollment in such a discontinuous way is especially important because continuous Medicaid coverage is associated with better health outcomes. In particular, if the administrative costs of collecting premiums are high relative to revenue collected, small premiums seem difficult to justify as anything other than a measure to discourage enrollment. If Medicaid coverage interacts with other government assistance programs, as [Baicker et al., \(2013b\)](#) suggest may be true for the Supplemental Nutritional Assistance Program, these effects may be exacerbated.

An important caveat to the findings in this paper is regarding external validity. First, the period of time of the study coincides with a period of poor economic performance in Wisconsin. One would expect this to result in a larger absolute number of new enrollees during this time (due to the large number of employment shocks that likely occurred) relative to the average. I am unable to observe the exact reasons for enrollment. This could matter for external validity if new enrollees during this time are more likely to enroll because of an employment shock and those enrollees are particularly responsive or unresponsive to the premiums relative to the average. Second, a general concern with regression discontinuity designs is that they identify a local effect by definition: the average treatment effect at the cutoff point. If the treatment effect is heterogeneous, it may not be applicable to those away from the threshold. However, the similarity of enrollment results for children and adults at different income thresholds suggests that these effects are robust across at least part of the income distribution.

As of January 2013, 33 states required premiums or enrollment fees for children at some income level, and 19 out of 34 waiver programs for adults required premiums ([KCMU, 2013](#)). Maintenance of effort requirements currently limits the ability of states to increase or require new premiums for existing beneficiaries. In the ACA exchanges, premium subsidies limit the maximum premium to 4–6.3% of income for those in families with incomes 150–200% of the FPL, with the exact amount depending on the lowest cost Silver plan. These premiums will most likely be higher than those in effect in Wisconsin at the time of this study, although they are a smooth function of income rather than a discontinuous one.¹³

Given the results of this paper, which shows even a small premium can have an important effect on enrollment choices for low-income parents, premiums are likely to remain a barrier to coverage for many low-income families, both in Wisconsin and nationally. Effective July 2012, Wisconsin changed its premium threshold for adults on BadgerCare Plus from 150% FPL to 133% FPL, and effective April 1, 2014, all adults in BadgerCare Plus with incomes above the poverty level and children in families with incomes over 300% FPL are no longer be eligible for the program with the expectation that they will seek coverage in the Marketplace. Even in the presence of a penalty for non-compliance with the ACA coverage mandate, which changes the relative trade-off, complete take-up of coverage and continuous enrollment are unlikely to occur.

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Appendix.

[Battistin and Rettore \(2008\)](#) show the average effect of treatment on treated is still identified under self-selective enrollment

¹³ According to the Kaiser Family Foundation's subsidy calculator, on average a family of three with one adult age 35 enrolling in Marketplace coverage would face a \$98/month premium for a Silver plan or \$53/month for Bronze. Retrieved from: <http://kff.org/interactive/subsidy-calculator/> on 3/21/2014.

when data are available on those who do and do not select in to the program. In this ‘partially fuzzy’ case, the object of interest is still the mean impact of treatment W on the outcome Y :

$$\lim_{x \downarrow x_0} E[Y_1 | W = 1] - \lim_{x \downarrow x_0} E[Y_0 | W = 1]$$

which is the difference between the observable outcome and the counterfactual outcome for the treated. The counterfactual outcome is a linear combination of mean outcomes for the marginal untreated and non-enrollees: $\lim_{x \downarrow x_0} E[Y_0 | W = 1] =$

$\lim_{x \uparrow x_0} E[Y_0] \frac{1}{\theta} - \lim_{x \downarrow x_0} E[Y_0 | W = 0] \frac{1-\theta}{\theta}$ where $\theta = \lim_{x \downarrow x_0} E[W]$ and we have used that $\lim_{x \uparrow x_0} E[Y_0] = \lim_{x \downarrow x_0} E[Y_0]$. This is equivalent to Eq. (10) in Battistin and Rettore (2008). I apply their idea under the assumption that those who do not take up the program when it is free would never take up the program when required to pay for it, so the only problem at the threshold comes from those who would take up the program if it were free and they were eligible. While $\lim_{x \uparrow x_0} E[Y_0]$ is observed, θ and $\lim_{x \downarrow x_0} E[Y_0 | W = 0]$ would require data on non-enrollees. I therefore derive the selection bias, defined as sb , as the difference between the effect I measure and the true treatment effect:

$$sb = \lim_{x \uparrow x_0} E[Y_0] \frac{1-\theta}{\theta} - \lim_{x \downarrow x_0} E[Y_0 | W = 0] \frac{1-\theta}{\theta}$$

The selection bias depends on three things; the take up rate θ , the outcome for untreated enrollees, $\lim_{x \uparrow x_0} E[Y_0]$, and the outcome for enrollees who would be treated but did not enroll, $\lim_{x \downarrow x_0} E[Y_0 | W = 0]$. The size of the selection bias is proportional to the take up rate θ , which is between zero and one; the higher the take up rate, the smaller any selection bias.

Since the treatment effect is negative and the selection bias is most likely positive, the bias is against finding a result. By making different assumptions about the take up rate and the difference between those enrollees who are not required to pay premiums (and who are in my data) relative to those who are not enrolled but would have to pay premiums if enrolled in the program (and are not in my data), one can calculate the size of the selection bias.

Since those who do not enroll but would have to pay a premium if they were enrolled are not in the data, they are essentially observed to be enrolled for 0 months, and we could assume that the difference $\lim_{x \uparrow x_0} E[Y_0] - \lim_{x \downarrow x_0} E[Y_0 | W = 0]$ is just $\lim_{x \uparrow x_0} E[Y_0]$. The only bias then comes from $\lim_{x \uparrow x_0} E[Y_0] \frac{(1-\theta)}{\theta}$ (from including some enrollees below the threshold who would never enroll if required to pay a premium). Intuitively, the bias comes from having some enrollees just below the threshold who would never have enrolled if they had to pay a premium. I take $\lim_{x \uparrow x_0} E[Y_0]$ to be the average outcome for enrollees at the premium threshold and multiply it by $(1-\theta)/\theta$, allowing θ to vary.

Medicaid take up is difficult to estimate, and estimates vary depending on many factors. One commonly cited take up rate is 62% (Sommers and Epstein, 2010) which is consistent with what some researchers have shown is likely what the Congressional Budget Office is currently using for Affordable Care Act projections (Sommers et al., 2012). At a take up rate of 62% and an average months enrolled of 10, this would suggest a selection bias of approximately 6.1 months, meaning that the true effect is a decline of $1.4 + 6.1 = 7.5$ months. However, this calculation is very sensitive to the take up rate. The implication is that premiums may discourage enrollment by an even larger factor, magnifying the importance of the result.

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