Using RD Design to Understand Heterogeneity in Health Insurance Crowd-Out

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Abstract

In studies of Medicaid, crowd-out, the switching from private to public insurance, is often found, but estimates are rarely consistent with prior measurements. For example, Cutler and Gruber (1996) found crowd-out in up to half of the newly eligible children, while Card and Shore-Sheppard (2004) found almost none. This study exploits many regression discontinuity (RD) designs to understand the potentially heterogeneous effects of public insurance eligibility. Family income is the key factor in determining the number of children shifted from private to public insurance, and its consequences for spending. Differences in family income correspond to different changes in insurance quality—eligibility leads to larger decreases in out-of-pocket spending when the eligibility threshold is lower. Differences due to monthly vs. annualized insurance measures are minimal. These differing effects correspond to state-specific effects, as well as state-by-state changes in the prices paid to doctors by state Medicaid plans.

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1 Introduction

In studies of Medicaid, crowd-out, the switching from private to public insurance, is often found. The amount of crowd-out, however, is widely disputed. For example, Cutler and Gruber (1996) found crowd-out in up to half of the newly eligible children, while Card and Shore-Sheppard (2004) found almost none. These two studies are emblematic of a larger disagreement within the literature about when and how much crowd-out can be expected.

Because economists and policy experts can only describe, but not understand, the varied amounts of past crowd-out, we are severely limited in advising when crowd-out might occur. If Cutler and Gruber (1996) is correct, then public offerings undermine the private market. If Card and Shore-Sheppard (2004) is correct, then expansions only shore up the missing parts of the private insurance market, instead of working against it.

This paper exploits a series of policy experiments to understand why, and when, there might be differing crowd-out effects. Using the State Children’s Health Insurance Program of the late 1990s and early 2000s, I implement many regression discontinuity (RD) designs. These expansions increased the income thresholds used to determine a child’s eligibility. Because each state’s policy for a given year provides a different local random assignment of eligibility near the eligibility threshold, I can compare the crowd-out, and related effects, in each setting.

With few notable exceptions (Card and Shore-Sheppard (2004), for example), the previous literature uses “simulated eligibility” instruments to estimate the average treatment effect of public health insurance expansions. This produces a simple, if opaque, average treatment effect for the policy change based upon a “difference-in-difference” (DD). These average effects, found in work such as Lo Sasso and Buchmueller (2004), which looks at more recent policy changes, cannot be broken down into differences across income levels. In contrast, the narrowly focused nature of RD estimates limits their potential external validity. Here, however, the narrow nature of the estimate, combined with the rich heterogeneity of the policy changes undertaken, allows for direct comparison of estimated effects across many dimensions.

The rich family structure and earnings data of the MEPS, along with the discontinuous
nature of eligibility, creates many RD estimates. The state-by-state administration of the
the expansions produced eligibility thresholds that varied across the child’s age, family in-
come level and year. Moreover, the monthly nature of the insurance variable of the MEPS
allows comparison of effects on monthly insurance status, and coarser, annualized (Current
Population Survey-style) versions of the same. Thus, differences across survey instruments
can be compared to differences across natural experiments.

I find that potential differences due to survey instruments are not statistically significant,
and are overwhelmed by the differences associated with family income. These differences can
be statistically significant, and indicate that families with larger incomes are less likely to be
crowded out of private insurance. Families near the top of the eligible income distribution
demonstrate a fraction of crowd-out compared to lower-income households. This relationship
suggests that insurance quality is a more normal good than other goods. This is confirmed by
patterns of crowd-out and changes in out-of-pocket spending due to public health insurance.
I find evidence that the quality of care received by children decreases as they become eligible
for public health insurance. Quality of insurance can also be tied to the reimbursement rates
paid by public insurance programs. Running each regression by state, I find that states vary
in the amount and consequences of crowd-out from public health insurance. Using a short
time series of reimbursement rates, changes in reimbursement rates are found to be associated
with different effects on public health insurance on office-based visits and spending.

2 A Simple Model of Crowd-Out

Consider the comparative statics of the simple model in Cutler and Gruber (1996), itself
adapted from Peltzman (1973). Two individuals facing a budget constraint between health
insurance (continuous in quality) and all other goods. The government offers free insurance
of low quality, which interrupts the original budget line. This corresponds to a budget
constraint ABMC in Figure 1. When the cost of the publicly provided good is a large
fraction of the household’s income (as with the lower budget constraint), it can be difficult
to draw indifference curves that do not lead to public insurance take-up.

However, as the budget grows, the challenge to draw such indifference curves lessens. The
impact in-kind benefit of public insurance shrinks; ceteris paribus, higher income should lead to less crowd-out. At the same time, if health insurance is a more normal good than other goods, then the indifference curves should flatten out as we move away from the origin. Public insurance becomes a less acceptable substitute for (higher quality) private insurance.

Non-pecuniary costs may also play a similar role in this setting. There may be stigma associated with holding public health insurance, and this stigma might grow as the family’s income grows. (I.e., it is less becoming for higher-earning families to be on the public dole.) Increasing stigma may also coexist with a decreasing knowledge of institutions—do families know whether their child is eligible for public health insurance? They may be willing to bear the stigma of public health insurance, but not know where their local welfare or public assistance office is located.

What is potential evidence of these effects? First, many public insurance plans are designed to have lower co-pays and deductibles than private plans. A relationship between out-of-pocket spending and crowd-out would be evidence of these effects. There may also be supply-side effects, as well. As has been documented in Decker (2007), low reimbursement rates paid to care providers can lead to less and a lower quality of care afforded by public health insurance.

The prevalence (or lack thereof) of private health insurance might cap the possibility crowd-out. As will be shown below, the incidence of private health insurance grows with income. Higher income households would otherwise be less likely to be crowded out into public health insurance because of preferences. However, their lower-income counterparts are much less likely to have private health insurance, and may not be able to be crowded out.

3 The Many Experiments of SCHIP

The State Children’s Health Insurance Program increased the availability of public health insurance in the US. These changes can be measured using the Medical Expenditure Panel Survey (MEPS). The MEPS is a short-panel data set that measures an individual’s health, health care utilization, insurance status, and related information, such as labor market out-
comes and family structure. Five interviews are conducted over two years for each panel. Monthly insurance variables and annual utilization and expenditure data are among the publicly available data. State of residence is not publicly available. Eligibility variables for the first and third round (the first and second year, respectively), which depend in part on state of residence, were constructed by the Agency for Healthcare Research and Quality (AHRQ), and were made available under a confidentiality agreement.

SCHIP was created in 1997, and it provided money to states to expand the number of children eligible for public health insurance. As documented in Figure 2, once fully implemented in the early 2000s, SCHIP doubled the number of children eligible for public health insurance. Its consequences for the number of children who actually had insurance were not as impressive. As demonstrated in Figure 3, this large expansion of public health insurance coincided with similarly-sized decreases in the number of children privately insured. Accounting for the number of children who were eligible for insurance, but did not take it up, the size of SCHIP did not lead to a similar change in the number of insured children.

The simple differences implied in these two figures do not reliably capture the causal effects of the SCHIP expansions. The nature of eligibility rules allows for a regression discontinuity (RD) design estimation of such a causal impact. SCHIP expansions were either literal expansions of Medicaid, or new programs whose eligibility rules were similar. In either case, a child’s eligibility for public insurance is determined according to family income as a fraction of the federal poverty guideline. If this fraction were lower than a particular ratio, which itself depends upon the age of the child, state of residence and year, then the child passed a key test in determining eligibility. The implementation of SCHIP plans lead to dramatic increases in the eligibility thresholds across states. Figure 4 plots the average eligibility threshold for children from 1996 to 2002.

These changes in eligibility over time have been exploited to identify the causal impact of public health insurance. Using simulated eligibility in an IV regression should provide the average treatment effect of the public health expansion, so long as there are no state-year specific effects that coincide with the expansion. Identification is drawn from the fact that some states happen to expand insurance more rapidly than others, and there are no potentially confounding circumstances for the children in the expanding states. Alternatively,
state-year fixed effects can be included, and identification is found by comparing children in
different ages within the same state. Because the relevant threshold can depend upon the
child's age, there may be enough variation for the instrument to work with some power.

These procedures provide simple average treatment effects that are essentially opaque.
Different treatment effects cannot be estimated, because all potential variation is used in
what is essentially a differences-in-differences approach. Estimates for the amount of crowd
out have varied since the seminal work of Cutler and Gruber (1996), and many have relied on
the simulated eligibility instrument. These differences have been tied to the different policy
changes exploited, but the opacity of the estimation techniques makes it hard to verify these
claims.

Regression discontinuity design is an alternative approach for identification, and is used
in this paper. The narrow focus of RD techniques allows for the comparison of these effects
across different dimensions, because the thresholds varied across age, state and year. As is
evident in Figures 4 and 5, which plot the average threshold by year and child age, state
policy provides a great deal of variation. The thresholds used by states tended to focus on
particular values—twice the poverty guideline, 2.5 times the poverty guideline, and so forth.

4 Measuring Insurance and Utilization

Does public insurance alter the incidence and treatment of disease? Suppose I attempt to
measure the effect of public medical insurance on health-care demand using the equation
below:

\[ c_i = L_i \gamma_c + X_i \beta_c + \epsilon_{c,i}, \]

(1)

where \( c_i \) is child \( i \)'s health-care outcome in study, such as having asthma or body mass
index (BMI); \( L_i \) is equal to one if the child is eligible for public insurance; \( X_i \) is a set of
other relevant information, such as race, ethnicity and state of residence; and \( \epsilon_{c,i} \) is the usual
residual. What does \( \gamma_c \) measure? First, since this is a measurement of eligibility and not of
take-up, there is no take-up endogeneity bias. In part, \( \gamma_c \) measures the causal effect of public
insurance eligibility on the health-care outcome. However, since eligibility is determined by family income and the child’s age, it also picks up the outstanding correlation between income and demand for health care. For example, if health is a normal good, without controls for income, $\gamma_c$ would be biased down.

In order to overcome this, I apply a regression discontinuity method, similar to Card and Shore-Sheppard (2004) in their study of Medicaid expansion. Now, suppose I estimate the following equation:

$$c_i = L_i \gamma_c + X_i \beta_c + G(Age_i, \text{Family Income}_i) + \epsilon_{c,i},$$

where $G(Age_i, \text{Family Income}_i)$ is a non-linear function of family income and age. The effects picked up by $G$ need not be causal—it just needs to pick up all of the covariation between the outcome variable and the variables that determine eligibility, beyond the causal effects of eligibility itself. Adding these controls leaves $\gamma_c$ to measure the effect of public insurance itself, pace concerns to be addressed below. Since there are many different cut-off points, in different states at different times for children of different ages, this is the local average treatment effect (LATE) for the average marginal child. Since some states doubled their cut-off levels for some age groups, this is a non-trivial concern. Here, the averaging is taking place over the discontinuities. These are still the average effects for the marginal individuals—those who are just at the cut-off line for eligibility.

The main specification of Equation 2 includes dummy variables for race, Hispanic ethnicity and whether the child has a single mother. Year- and state-fixed effects are also included. $G(Age_i, \text{Family Income}_i)$ is a third- or fifth-order polynomial in the child’s age (measured in months) and family income as a fraction of the poverty guideline, both measured at the time of eligibility measurement. Family income includes all non-self-employed wage income within the CPS-type family, and poverty guidelines are calculated accordingly. The public insurance variable, $L_i$, is equal to one if eligible for Medicaid or SCHIP, zero otherwise. Since the MEPS is collected using a complex survey design, all estimates and standard errors are calculated using appropriate weights, strata and PSU. The estimates provided below are primarily pooled across the short (twice observed) panel.
Table 1 reports the estimated means of a restricted sample—children whose families earn between half and four times the poverty guideline. As noted above, I observe eligibility at a point in time, not a time-series of eligibility information. There is a difference between the fraction uninsured at all (26 percent) and uninsured the entire year (10). There are children who are observed in the third round (i.e., with eligibility information collected for them), but are not included in the sample because they were not present in the household later that year. Because of this, the sample and all effects are related to those children who do not routinely drop in and out of their household, or display other irregular patterns of household membership.

This empirical specification is complemented by graphical analysis of the data. The data present several complications to this strategy. The eligibility thresholds were constructed by AHRQ, but were not made available to this researcher. In lieu of this, I merged the MEPS data with eligibility rules from the TRIM3 database, which is a collection of welfare rules constructed by the Urban Institute. I supplemented this information with the eligibility rules published in a series of reports by the Kaiser Family Foundation (Ross and Cox (2000) and Ross and Cox (2002)). These rules include the state-year-age specific eligibility thresholds for SCHIP, Medicaid and Medicaid extension programs, as well as the rules governing income disregards.

Eighty percent of the time, these rules match the eligibility as constructed by the AHRQ. Five percent of the time, these rules grant eligibility, while AHRQ does not. Citizenship status was used in the AHRQ’s eligibility variables, but not in the rules constructed from the TRIM3 database. Non-citizens are not eligible for these public insurance programs, so it is appropriate to drop them from the sample. The remaining fifteen percent were ruled ineligible by the rules used here, but eligible by the AHRQ simulation. These discrepancies may occur for a variety of reasons: the date of eligibility rule (TRIM3 provides rules effective by July 1 of each year; in many cases, AHRQ researchers investigated the actual date of policy change); the use of imputed unearned income in the AHRQ simulation; other policy rules enforced by the AHRQ simulation that are not available in the two sources used here. In the graphs that follow, I drop all observations where the AHRQ eligibility differs from those using the TRIM3 and Kaiser Family Foundation rules. Evidence suggests that this is not
a problem: when the marginal effects are consistent in spite of heterogeneous treatment
effects (e.g., linear probability models), the estimated effects match the effects implied by
the graphs.

In general, graphical analysis can be carried out with the average value in “bins” according
to the forcing variable, as advised by Imbens and Lemieux (2007). Here, the forcing variable
is difference (or distance in the graphs) between the ratio of family income to Federal poverty
guideline and the relevant cut-off. This corresponds to a $156 wide 1% bin for a family of
four in 1996. The width of all graphs is half an eligibility threshold. A flexible fitting of
the bin averages is also provided, using local linear regressions. The weighting function of
local linear regressions is based on the rectangular kernel, using the bandwidth selection

Table 2 present the average marginal treatment effect—the treatment effect of the last
eligible child, averaged across the various thresholds. There are three kinds of estimates—
linear probability models (for insurance variables), Poisson count regressions (for the annual
number of office based visits), and Tobit procedures for variables likely censored at zero (all
spending variables). For the count and censored data, the use of those estimators with a
global polynomial has been advised by Imbens and Wooldridge (2009). These procedures
were run with no controls for income and child age; and with polynomials of the third and
fifth order in family income-to-poverty guideline and age.

In all cases, the tables report the average marginal effect of the estimated marginal
treatment effect, as well as the t-statistic for the latter. For the linear probability models,
its linearity implies that the average marginal effect is the same as the marginal effect of
the average effect. However, these two effects may be different for the non-linear models.
The difference between the two depends critically upon the potential heterogeneity of the
effect(s) of the treatment. The marginal effects here are the estimate of the uncensored
equation times the average probability of being uncensored (i.e., $\gamma_c \cdot \Pr(c_i > 0)$).

The estimates, along with Figures 6 through 8 demonstrate complete crowd-out on the
margin. There is a decrease in the private insurance rate by twenty-five percent, while
the increase in public insurance take-up approximately equals it. On net, there is a slight
decrease in the number of insured children on the margin of the eligibility threshold. These
effects are larger than is typically estimated. There are two potential reasons for this. First, the more recent expansions of public insurance seem to have induced more crowd-out—the rates found here are similar to some of the estimates of Gruber and Simon (2007), which studies the SCHIP-era expansions using different data. Using the same data and policy changes (though different identification strategy), Hudson et al. (2005) found large amounts of crowd-out in some of their specifications.

Finally, the effect estimated here is different than is typically found. Identification here finds the marginal treatment effect (“What if we made one more child eligible?”), whereas estimates that depend upon simulated eligibility instruments find an average treatment effect (“What was the average effect on the children now made eligible?”). For example, children closer to the eligibility threshold are less likely to take-up public health insurance, as seen in Figure 8. The average treatment effect is estimated from a fraction of these children, the size of which depends upon the size of the expansion. These average children are more likely to take up insurance than the marginal child, driving down the amount of crowd-out.

This crowd-out has several effects on utilization outcomes for the calendar year: a decrease in the annual number of office-based visits; a larger, in percentage terms, decrease in office-base visit spending; a decrease in out-of-pocket spending; and a decrease in total spending. These effects suggest that, in switching out of private, and into public insurance, they are shifting from high- to low-quality insurance. The estimates found in Table 2 correspond to the graphical evidence in Figures 9, 10 and 11, for spending, and 12 for office-based visits.

The Tobit procedures for censored observations is complemented by a two-stage GLM procedure, as suggested by Manning et al. (2005) and Manning (2010). In particular, I employ a first stage logit estimate for whether the individual has positive expenditures, and a second-stage generalized linear model with log link and Gamma distribution family for those with positive expenditures. The standard errors are derived from a bootstrap procedure, resampling 1,000 times, stratified on the complex survey design, and clustered on the individual (because the data is a short panel). Table 3 presents the estimates by Tobit, Logit-GLM, and the estimate implied by the local linear regressions on the bin averages. The first two estimates may be biased due to the interaction of heterogeneous treatment effects.
with the non-linear representation of expected values. The binned values demonstrate no censoring, because the averages are all greater than zero. This provides a test for the bias due the non-linear nature of the estimators.

The measured effects tend to be disparate, especially in the case for total spending. There are myriad reasons for these differences, given the differences in the non-linear procedures and potential for heterogeneous effects. In two of the three cases (total spending and office-based visit spending), the Tobit marginal effect is closer to the bin-based estimate than the two-stage procedure. The third outcome, out-of-pocket spending, the two-stage estimator is closer. This third case is actually more important than the other two, because out-of-pocket spending is where will see the strongest evidence of heterogeneous effects.

One final unconditional graph is provided as evidence of the validity of the RD design. As suggested by McCrary (2008), Figure 13 provides the size of each bin, with each observation within the bin weighted according to the sampling weights. There does not appear to be evidence of parents curtailing their earnings to duck under eligibility thresholds.

I run each of the above regressions separately according to the size of the eligibility threshold. Eligibility thresholds were gathered into five separate bins—the poverty guideline or below, up to 1.33 times the poverty guideline, 1.85, 2 and 2.5. In all cases, the named threshold represents a large majority of the observations. These threshold categories were designed to spread the number of observations throughout the bins as evenly as possible. These regressions can be run with year and state fixed effects, as well as the third-order polynomial in child’s age in months.

Different amounts of crowding out by eligibility threshold can be found in Figures 14 through 16. These graphs provide the estimated effects of and 95 percent confidence intervals for eligibility, run separately for each group along the horizontal axis. The crowding out of private health insurance by public insurance eligibility decreases as income increases. Private insurance decreases by about thirty percent for those with the lowest thresholds; it is only one third of that for the highest-threshold children. Take-up of public insurance follows a corresponding pattern—those most likely to be crowded out are also those more likely to take up public insurance. These two effects counteract in such a way that the effect on any (public or private) insurance rate is relatively steady across threshold levels. This emphasizes
the returns to this rich data—courser measures of insurance that ignore the private or public distinction would miss this heterogeneity. The pattern demonstrated here is qualitatively similar to the findings of Gruber and Simon (2007), which finds diminishing, though still large, crowd-out as income thresholds grow.

Does this heterogeneity matter? This discussion has focused on the mechanisms behind crowd-out: can differences in crowding out be explained by differences in the population afforded the opportunity to switch? As mentioned before, one quality of insurance is to consider the access to care, and the spending on the care accessed. Figures 17 and 18 plot the estimated effects of eligibility on annual office-based visits and total expenditures, by income threshold. These figures do not demonstrate a discernable pattern, either economically or statistically, the latter evidenced by overlapping confidence intervals. Because there is shrinking crowd-out against a constant decrease in spending, this suggests that the higher-income households that do switch are subject to larger average decreases in medical spending.

These difference in insurance status do correspond to changes in out-of-pocket spending. Figure 19 demonstrates that the crowded-out low-threshold children also have large decreases in out-of-pocket spending. As the threshold increases, the changes in out-of-pocket spending induced by eligibility are diminished. This dimension of insurance quality appears to have particular salience for almost-eligible households.

These findings demonstrate that income is an important correlate with crowding out behavior. It is also well established that the prevalence of private health insurance grows with family income. Figure 20 presents the incidence of private health insurance among those children above the threshold by the relevant threshold level. This measures the availability of children to be crowded out. From the lowest threshold of the poverty guideline to the upper thresholds of twice to three times it, the incidence of private health insurance grows from the low 80s to the low 90s. The larger amount of crowd-out at the lower income thresholds previously demonstrated exist in spite of the lower incidence of private insurance to be crowded out.

This suggests that differences in the quality of insurance plays a larger role in the demand for insurance than pure preference effects. Pure preference effects would work in the opposite way with theoretically-preferred decreasing absolute risk aversion (such as constant relative
risk aversion (CRRA)). The main alternative to CRRA, constant absolute risk aversion (CARA), leaves demand for insurance independent of income or wealth. Another potential factor is the survey instrument used—does the time frame of the question asked alter the estimated effect? To assess this, annualized insurance variables were constructed from the monthly (January to December) variables.

Do these answers vary when we change the insurance question? These regressions were rerun instead using an annualized (from January to December) measure of insurance, instead of just the month of interview. These differences are broken down by income threshold groups, which were also constructed to find the largest five groups. (The main change from the previous by-threshold estimates is that the thresholds at 3 and 3.5 times the poverty guideline are included with those at 2.5.) In some groups, the different measures can mean the difference between a positive and negative estimate effect, though neither are different from zero with statistical significance. These breakdowns can be seen in Figures 21 to 23.

The salience (or lack thereof) is tied to the annual nature of many employer-provided insurance contracts. The more flexibility individuals have in changing insurance during the year, the larger the discrepancies between annualized and monthly insurance measures. For, employees or firms may only alter their insurance during “open enrollment” periods that cover the middle of the calendar year. If these open enrollment periods do not coincide with the calendar year, there will be a discrepancy between annualized changes and monthly changes.

5 Evidence in Quality of Care

The annual spending and visit data provide some insight into the quality of care received by children. While the number of visits measures the extensive margin, the intensive margin is incompletely described by focusing on the price of care. To focus on the intensive margin, I consider a series of questions asked about the quality of care received by children.

The MEPS asks survey participants a variety of questions regarding their access to care in the second and fourth rounds of the MEPS. Of primary interest here are two questions: first, does the child have access to a usual source of care (USC), and if so, what are the
characteristics of that provider? These questions were asked in the round subsequent to the interview during which eligibility for public insurance was determined. That is, if eligibility was determined at a point in time in March (the end of the first round for some survey participants), the USC questions cover the period of time up until the next interview (a random number of months determined by the survey methods of the MEPS). Due to changes in survey design, these questions were only asked for a subset of years (1996 and from 1999 to 2002). Graphs were produced for these variables. Because of the reduced sample, the bins are twice as wide (2 percentage points of the federal poverty guideline instead of one), and the regression results are not broken down by threshold. However, they should provide more evidence as to the differences in quality associated with the changes in insurance at the eligibility threshold.

Table 4 presents the results of a linear probability model of the usual sources of care questions, identifying the causal effects of public health insurance using the RD strategy of Equation 2. Becoming eligible for public health insurance makes a child four percentage points less likely to have a usual source of care, while increasing the likelihood of going without care an equal amount. Both estimates are statistically significantly different from zero at the 99 percent confidence level. Because of the smaller sample size, the bins themselves are noisier, but the local linear smoother fits a discontinuity in Figure 24.

If a respondent says that a child has a usual source of care, the respondent is asked questions about the provider. The respondent can classify that USC as either a hospital or an emergency room. Neither estimate is statistically significant, but the effect on ERs as a USC is particularly small.

5.1 BMI

The body mass index (BMI) for each child was constructed for children between the ages of 3 and 17, based upon parents’ reports of the children’s heights and weights. The responding parent was also asked questions about their children’s interactions with health care providers—has the child been given advice about eating healthfully or exercising by a medical professional? This set of obesity-related questions is asked in the second and fourth round interviews, when the USC questions are also asked. These questions were asked only
for a subset of the years (from 2001 on), so the sample sizes are smaller than they are for the earlier analysis.

As reported in Table 4, eligibility for public health insurance increases a child’s BMI by two percentage points. This effect is confirmed by the local linear fit of Figure 25. Because BMIs (the simple ratio of weight to height squared in SI units) vary greatly for children due to their rapid growth, log-BMIs were used as the dependent variable. Questions in other components of the MEPS ask about the medical advice received by doctors for children. (Because they are from different components, the sample sizes vary due to age-specific skip patterns.) Parents of children who are just eligible for public health insurance are five percentage points less likely to be given advice about their child’s eating healthfully, an estimate that is statistically significantly different from zero at the 99 percent confidence level. These parents are also less likely to be told that their children need to exercise, but the effect is smaller (two percentage points) and not statistically significant.

5.2 Asthma

Questions about asthma were also asked. Asthma was identified as one of several priority conditions for the MEPS, due to its prevalence and the fact that basic standards of care have been established. The asthma-related questions are asked in the third and fifth rounds. Due to the retrospective nature of the questions described below, the third-round observations on asthma incidence are tied to the first-round construction of eligibility (and the fifth to the third). Questions on asthma were asked starting with the 2000 survey. The results can be found in Table 4.

The first question is: has the child ever been diagnosed with asthma? If the answer is yes, the parents is then asked a series of questions regarding the incidence and treatment of asthma. First, the parent is asked whether the child had an asthma attack in the previous year. (Thus, the third-round asthma questions are tied to the first-round eligibility variables.) Parents were also asked if the child took prescription medications and had a peak-flow meter at home. Peak-flow meters measure the flow of air out of the lungs, and are often used for patients with moderate to severe asthma. For an asthmatic, a peak flow meter is used much in the same way that a glucose meter is used by diabetics. Because they just measure the
severity of the asthma, instead of actually treating it, they should be considered complements with prescription drug treatments, instead of substitutes.

There is no statistically significant effect of public health insurance eligibility on the diagnosis of asthma. This is confirmed with visual inspection of Figure 26. Given the potentially extreme nature of asthma, this is a sensible result. Among those with asthma, there is no statistically significant effect on the likelihood of having an asthma attack in the preceding year (during the year for which eligibility was computed; see Figure 27). Becoming eligible for public medical insurance makes a child diagnosed with asthma less likely to take prescription medication or use a peak flow meter. However, conditional on a child taking prescription medications, becoming eligible for public insurance increased the likelihood of taking inhaled steroids, the “gold standard” of asthma medication. Since the condition here makes crowding down less likely, this is evidence that public health insurance can increase the use of care when access to care is not diminished. This can be seen in Figures 28 and 29, respectively. This last graph, on steroid use, casts some doubt on the estimated effect of public insurance eligibility with no effect.

5.3 Sore Throats

Upper-respiratory problems are a frequent problem for children. In such cases, children can be prescribed antibiotics, and there has been concern about the over-prescription of antibiotics. In response to concerns about over-prescription, the Michigan Quality Improvement Consortium set the following guidelines regarding sore-throat treatment:

- High risk children (those with past history or household exposure of rheumatic fever) and those whose symptoms indicate a high probability of streptococcus pharyngitis should start antibiotics immediately, but if a throat culture is obtained and the result is negative the antibiotics should be stopped.

- Antibiotics should not be prescribed for children whose symptoms indicate a low probability of streptococcus infection.

- A throat culture (or rapid screen test) should be obtained for children with an intermediate probability of streptococcus and, if the result is positive, antibiotics should be
The MEPS asked a series of questions regarding sore throats and related treatment in the 2000-2 samples.\footnote{1} The first question asked if the child had seen a doctor regarding a sore throat in the past year. The course of treatment was identified for those who saw a doctor primarily for the sore throat. Respondents were asked if antibiotics were prescribed for the patient. Per the concern about the over-utilization of antibiotics and guidelines above, it was then asked if the child was first given a swab test for streptococcus pharyngitis (strep throat).

The results can be found in Table 4. There is a slight, though imprecisely measured, decrease in the number of children who go to the doctor with sore throats as the child becomes eligible for public insurance. This is confirmed visually in Figure 30. If the child did go to the doctor for a sore throat, they are more likely to be prescribed antibiotics, though the difference is not statistically significant. The relationship can be see in Figure 31. However, doctors are seven percentage points less likely to have the child undergo a throat swab test for strep throat as the child becomes eligible for public health insurance. This effect is statistically significant, and suggests a lower-quality of care provided to the publicly or uninsured. This finding is confirmed in Figure 32. These effects may be larger than the previous effects for the simple fact that the proverbial victim is not in the room—lower quality of care costs are suffered by society through the excess use of antibiotics.

\section{Do Prices Matter?}

As the previous section suggests, the inferiority of public health insurance is associated with crowd-out. This inferiority is often tied to its low reimbursement rates paid to health care providers, which has been linked to some measures of quality of care, such as visit duration, by Decker (2007).

The first test of this is to run the above regressions separately for each state. Figure 33 provides the estimates and 95\% confidence intervals of crowd-out when the ten-most sampled

\footnote{1See Machlin and Carper (2006) for a discussion of medical issues and (non-causal) patterns of care across a variety of descriptives.}
states are estimated separately. The figure demonstrates that the crowding out of private insurance is consistent across states, though there is some (not statistically significant) variation across states. However, the extent to which public insurance programs replace lost private insurance varies across states.

Figure 34 presents the RD effects and 95% confidence intervals on total and office-based expenditures. This figure demonstrates that these effects vary by state, with statistically significant differences in some cases. (In other cases, the confidence intervals can be large due to limited sample size.)

To further test the hypothesis, I use the public insurance reimbursement rate data collected by the American Academy of Pediatrics (AAP) for the years 1998-9 (combined) and 2001. The AAP surveyed the public health administrators for each state for each period. Reimbursement rates are set by the states, and can vary across time and medical procedure or visit type. For example, in 1998-9 California reimbursed doctors $29.04 for a preventative medicine visit for a new patient under one year old; if the new patient were between the ages of 12 and 17, the doctor would receive $58.08. Two years later, the same services were reimbursed at $45.33 and $65.78, respectively. Meanwhile, a doctor in New Hampshire would receive $40 or $42 to see those patients in the earlier period; two years later, the reimbursements remained the same.

As the example above demonstrates, reimbursement rates varied within states across time. Moreover, they varied across different age groups within states across time. This will provide variation to see if changes in reimbursement rates led to changes in treatment effects—did states that increased their reimbursement to doctors mitigate the crowding-down of health care due to supply-side effects.

To assess this mechanism, I estimate:

\[ c_i = L_i \gamma_c + L_i \cdot P_{s,y} \cdot \gamma_p + P_{s,y} \cdot \beta_p + X_{i,s,y} \beta + G(Age, Family Income) + \epsilon_{c,i}, \tag{3} \]

where \( P_{s,y} \) is a state-year specific reimbursement rate for some medical good or service; \( G(Age, Family Income) \) is a non-linear function of family income and age; and \( X_{i,s,y} \) includes the demographic variables as before, as well as state- and year-fixed effects. Thus, the
estimated effect $\gamma_P$—do reimbursement rates alter the effect of public insurance eligibility—is being identified off of the changes in reimbursement rates within states over time. This leads to some sample attrition, as some states (such as New Hampshire) did not vary some of their reimbursement rates. However, the limited sample does provide some interesting and statistically significant effects.

Table 5 presents these estimates. The estimated equations include two reimbursement rates—the amount paid by states for an evaluation appointment of a new patient and an established patient. There are statistically significant interactions for the established patient evaluations. Translated into marginal effects, a four dollar increase in the amount paid for an established patient visit (the average change among states that did change their rates) is associated with a five percentage point increase in the number of visits, and seven dollars more worth of office-based visit expenditures, and nearly ten dollars more of total expenditures. The first two are statistically significant at the 99- and 90-percent levels, respectively. While not statistically significant, the effect of new patient reimbursement changes are negative.

These results reinforce the use of regression-discontinuity approaches. Through the discontinuity, the effects of eligibility for public health insurance are being isolated. They are a mixture of effects on demand (co-payments for visits may differ between private and public insurance) and supply (the publicly insured may not be able to get a visit). In essence, this measures the effect of making the marginal child eligible for a public health insurance card. Attempts to measure the average effect of public health insurance expansions have used a difference-in-difference approach. Difference-in-difference or triple difference estimates, such as those provided by simulated eligibility instruments, include the RD effects. They are the average effect for the newly eligible, not just the marginally eligible.

However, if changes in eligibility are correlated with changes in reimbursement rates, then the differences approaches include a second effect—first, the RD-measured effect of making more children eligible for public insurance, plus the fact that public insurance opens more or fewer provider doors as states adjusted their reimbursement rates along with their eligibility criteria. This second effect is zero if either of two cases hold: first, if changes in eligibility are uncorrelated with changes in reimbursement rates; and second, even if
those two are correlated, if medical goods and services are provided inelastically—changes in reimbursement do not matter to the supply of health care. The second condition is shown to be false in Table 5. The first condition can be tested by the correlation between changes in reimbursement rates and the eligibility of the children in the sample. These correlations are often positive and statistically significant—states that made more children eligible also increased the amount they paid to medical professionals for their services.

It should also be noted that these reimbursement rates were not randomly assigned to different states in different years. Thus, the estimates of $\gamma_P$ do not reflect the casual consequences of changes in reimbursement rates. This is clear from the negative association between new patient reimbursement rates and the effects of eligibility. To the extent that a state's reimbursement rate was endogenously determined in response to state-specific factors (such as the prevailing private insurance reimbursement rates), or induced state-specific factors (private insurers responded to the reimbursement rates), we cannot cleanly identify the causal effects of changing reimbursement rates.

7 Conclusion

The crowding out of private health insurance by public health insurance plans has been widely studied since the seminal work of Cutler and Gruber (1996). Since that work, there has been little consensus as to the average amount of crowd out, or what could potentially be mechanisms behind different amounts of crowd out.

Using many different, but sufficiently similar, policy changes in the US, this paper used many different localized regression discontinuity estimates to understand different treatment effects across different locations. First, a basic model is employed to understand the importance of income or wealth and preferences over insurance quality to the incidence of crowd-out. These theoretical results are reinforced by a series of empirical results—the shift out of private health insurance due to public health insurance eligibility is greatest when a family’s income is the least. There is a corresponding and opposite effect on the take-up of public health insurance, leaving a relatively constant amount decrease in the overall insurance rate over differing eligibility thresholds. These effects overcome issues of prevalence—dropping
out of private insurance is largest where the rate of private insurance are the smallest. These trends point suggest that preferences over insurance quality—ostensibly a more normal good than other goods—drive differing amounts of crowd-out.

What are the sources of insurance quality? These differing effects also correspond to different effects on out-of-pocket expenditures—the decreases in out-of-pocket spending cause by eligibility were largest when the swapping from private to public insurance were the largest. There is less evidence of an association with office-based visits—though public insurance may be criticized for the quality and quantity of care it affords, there are no systematic trends of office-based visit expenditure effects against the falling shifts in insurance source. That said, when states increased their reimbursement rates for basic office-based visits, the effects of eligibility do change. This corresponds to decreases in the health outcomes and quality of care due to eligibility measured here.

References


Figure 1: A simple model of crowd out.
Figure 2: Public insurance eligibility.

Figure 3: Insurance rates.
Figure 4: Average public insurance eligibility thresholds over time.

Figure 5: Average public insurance eligibility thresholds over age.
Figure 6: Public health insurance eligibility and private health insurance during the month of interview.

Figure 7: Public health insurance eligibility and any health insurance during the month of interview.
Figure 8: Public health insurance eligibility and public health insurance during the month of interview.

Figure 9: Public health insurance eligibility and total expenditures.
Figure 10: Public health insurance eligibility and office-based visit expenditures.

Figure 11: Public health insurance eligibility and out-of-pocket expenditures.
Figure 12: Public health insurance eligibility and number of office-based visits.

Figure 13: Public health insurance eligibility and bin size (using weights).
Figure 14: Public health insurance eligibility effects on having private health insurance, by income threshold.
Figure 15: Public health insurance eligibility effects on having public health insurance, by income threshold.

Figure 16: Public health insurance eligibility effects on having health insurance, by income threshold.
Figure 17: Public health insurance eligibility effects on office-based visit expenditures, by income threshold.

Figure 18: Public health insurance eligibility effects on total expenditures, by income threshold.
Figure 19: Public health insurance eligibility effects on having out-of-pocket expenditures, by income threshold.

Figure 20: Incidence of private health insurance among the non-eligible by eligibility threshold, by income threshold.
Figure 21: Public health insurance eligibility effects on having private health insurance, by monthly or annualized question.
Figure 22: Public health insurance eligibility effects on having public health insurance, by monthly or annualized question.

Figure 23: Public health insurance eligibility effects on having any health insurance, by monthly or annualized question.
Figure 24: Public health insurance eligibility and having a usual source of care.

Figure 25: Public health insurance eligibility and natural log of BMI.
Figure 26: Public health insurance eligibility and asthma diagnosis.

Figure 27: Public health insurance eligibility and asthma attack in the previous year.
Figure 28: Public health insurance eligibility and asthma prescription drugs.

Figure 29: Public health insurance eligibility and asthma steroid treatment.
Figure 30: Public health insurance eligibility and sore throat.

Figure 31: Public health insurance eligibility and receiving antibiotics from a doctor.
Figure 32: Public health insurance eligibility and throat swab to test for strep throat before receiving antibiotics.

Figure 33: Public health insurance eligibility and crowd-out by state.
Figure 34: Estimated effects of public health insurance estimated state-by-state.
<table>
<thead>
<tr>
<th>Variable</th>
<th>Overall Mean</th>
<th>Restricted sample mean</th>
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<td>0.51</td>
<td>0.51</td>
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<tr>
<td>1=White</td>
<td>0.78</td>
<td>0.78</td>
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<td>1=Black</td>
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<td>1=Hispanic</td>
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<td>0.74</td>
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<td>N</td>
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<td>32,609</td>
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Table 1: Means of the sample. Restricted sample is observations with measured income between half and 400% of poverty guideline.
### Table 2: Sample means of and marginal effects with coefficient t-statistics for public insurance on month-of-interview insurance status, office-based visits and different uses of expenditures. Insurance outcome effects were estimated in a linear probability model. Visits are count, and the reported estimates are incidence-rate ratios from Poisson regressions. The spending effects are calculated in a Tobit regression. These regressions were run three times—the first, without family wage income as a fraction of the poverty guideline and age; the second and third, with those effects in polynomials of the third and fifth order.

<table>
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<th>Restricted Sample Mean &gt; 0</th>
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<th>Polynomial Order 0</th>
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<th>5</th>
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<td>Any insurance now</td>
<td>.793</td>
<td>-.071</td>
<td>-.048</td>
<td>-.048</td>
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<td></td>
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<td>(-8.77)</td>
<td>(-5.05)</td>
<td>(-5.05)</td>
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<tr>
<td>Private insurance now</td>
<td>.592</td>
<td>-.364</td>
<td>-.236</td>
<td>-.235</td>
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<tr>
<td></td>
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<td>(-30.51)</td>
<td>(-17.87)</td>
<td>(-17.91)</td>
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<tr>
<td>Public insurance now</td>
<td>.282</td>
<td>.319</td>
<td>.212</td>
<td>.211</td>
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<tr>
<td></td>
<td></td>
<td>(26.77)</td>
<td>(17.83)</td>
<td>(17.88)</td>
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<td>Office Based Visits</td>
<td>3.99</td>
<td>67</td>
<td>0.90</td>
<td>0.88</td>
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<td>(-2.76)</td>
<td>(-2.75)</td>
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<td>Total Expenditures</td>
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<td>-260.91</td>
<td>-213.90</td>
<td></td>
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<tr>
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<tr>
<td></td>
<td></td>
<td>(-4.46)</td>
<td>(-3.30)</td>
<td>(-3.33)</td>
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<tr>
<td>Out-of-pocket Expenditures</td>
<td>265.41</td>
<td>70</td>
<td>-137.87</td>
<td>-103.70</td>
<td></td>
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<tr>
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<td></td>
<td>(-7.07)</td>
<td>(-4.93)</td>
<td>(-4.88)</td>
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</table>

N=32,609
Table 3: The estimated marginal effects of eligibility for public health insurance on spending outcomes, using three different estimation procedures. The first is a two stage procedure: first, a logit on whether or not spending is greater than zero; and, second, generalized linear model with log link and Gamma family. The second is a standard Tobit. The third estimate is the estimated effect using the local linear regressions on the bin averages, as in the graphs. Confidence intervals (95 percent) via bootstrapping are in parentheses for the two-stage procedure.

<table>
<thead>
<tr>
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<th>Logit+GLM</th>
<th>Tobit</th>
<th>Local linear on bins</th>
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<td>Total expenditures</td>
<td>-149.78</td>
<td>-213.90</td>
<td>-333.67</td>
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<tr>
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<td>(-255.45, -43.08)</td>
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<td>-59.94</td>
<td>-58.10</td>
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<td>(-61.24, -20.55)</td>
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<tr>
<td>Out-of-pocket expenditures</td>
<td>-75.04</td>
<td>-103.70</td>
<td>-63.86</td>
</tr>
<tr>
<td></td>
<td>(-93.71, -55.72)</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>Sample Mean</td>
<td>Public Eligibility Effect</td>
<td>N=</td>
</tr>
<tr>
<td>--------------------------------</td>
<td>-------------</td>
<td>---------------------------</td>
<td>-----</td>
</tr>
<tr>
<td></td>
<td></td>
<td>Polynomial Order</td>
<td></td>
</tr>
<tr>
<td></td>
<td></td>
<td>3</td>
<td>5</td>
</tr>
<tr>
<td>Go w/o care</td>
<td>0.096</td>
<td>0.038</td>
<td>0.039</td>
</tr>
<tr>
<td></td>
<td>(3.77)</td>
<td>(3.81)</td>
<td></td>
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<tr>
<td>Have USC</td>
<td>0.883</td>
<td>-0.038</td>
<td>-0.039</td>
</tr>
<tr>
<td></td>
<td>(-3.56)</td>
<td>(-3.73)</td>
<td></td>
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<tr>
<td>ln(BMI)</td>
<td>—</td>
<td>0.023</td>
<td>0.022</td>
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<tr>
<td></td>
<td></td>
<td>(2.09)</td>
<td>(2.07)</td>
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<td>Eat right?</td>
<td>0.473</td>
<td>-0.053</td>
<td>-0.054</td>
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<td></td>
<td>(-2.74)</td>
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<td>Exercise?</td>
<td>0.276</td>
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<tr>
<td></td>
<td>(-1.39)</td>
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<tr>
<td>Diagnosed</td>
<td>0.106</td>
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</tr>
<tr>
<td></td>
<td>(1.70)</td>
<td>(1.71)</td>
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<tr>
<td>Asthma attack?</td>
<td>0.398</td>
<td>0.036</td>
<td>0.031</td>
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<td></td>
<td>(0.94)</td>
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<td>Asthma RX?</td>
<td>0.565</td>
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<td>-0.060</td>
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<tr>
<td></td>
<td>(-1.59)</td>
<td>(-1.54)</td>
<td></td>
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<tr>
<td>RX Steroid?</td>
<td>0.509</td>
<td>0.157</td>
<td>0.163</td>
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<tr>
<td></td>
<td>(2.86)</td>
<td>(2.98)</td>
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<td>Flow Meter?</td>
<td>0.234</td>
<td>-0.099</td>
<td>-0.105</td>
</tr>
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<td></td>
<td>(-3.06)</td>
<td>(-3.21)</td>
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<tr>
<td>Sore throat</td>
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<td>-0.020</td>
<td>-0.019</td>
</tr>
<tr>
<td></td>
<td>(-1.18)</td>
<td>(-1.12)</td>
<td></td>
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<tr>
<td>See Dr.</td>
<td>0.868</td>
<td>-0.005</td>
<td>-0.006</td>
</tr>
<tr>
<td></td>
<td>(0.208)</td>
<td>(0.25)</td>
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</tr>
<tr>
<td>Got Antibiotics</td>
<td>0.699</td>
<td>0.043</td>
<td>0.041</td>
</tr>
<tr>
<td></td>
<td>(1.13)</td>
<td>(1.11)</td>
<td></td>
</tr>
<tr>
<td>Throat swab</td>
<td>0.722</td>
<td>-0.078</td>
<td>-0.076</td>
</tr>
<tr>
<td></td>
<td>(-2.11)</td>
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</table>

Table 4: Sample means of having a sore throat and subsequent treatment and estimated effects of public eligibility (with t-statistics) on those variables. All regressions are linear probability models, with state- and year-fixed effects, along with demographics as described in the text.
New Evaluation | Established Evaluation
---|---
Office-based visits | -.011 | .016***
                  | (.007) | (.005)
Office-based Expenditures | -3.89 | 4.64*
                        | (3.08) | (2.37)
Total Expenditure | -3.78 | 5.90
                  | (13.0) | (9.54)

Table 5: Coefficients (standard errors) for the effect of public insurance eligibility, interacted with the state- and year-specific reimbursement rates of public health insurance plans. The reimbursement rates for an evaluation of a new patient and an established patient. N=11,622