Estimating Heterogeneous Treatment Effects of Medicaid Expansions on Take-up and Crowd-out*

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Abstract

Economists have devoted considerable resources to estimating local average treatment effects of expansions in Medicaid eligibility for children. In this paper we use random coefficients linear probability models and switching probit models to estimate a more complete range of effects of Medicaid expansion on Medicaid take-up and crowd-out of private insurance. We demonstrate how to estimate, for Medicaid expansions, the average effect among all of those eligible, the average effect for a randomly chosen person, the effect for a marginally eligible child, and the average effect for those affected by a nonmarginal counterfactual policy change. We then estimate the average effect of Medicaid expansions among all eligible children and the average effect for those affected by a nonmarginal counterfactual Medicaid expansion since these are likely to be the most useful for policy analysis. Estimated take-up rates among average eligible children are substantially larger than take-up rates for those made eligible by a counterfactual Medicaid expansion, moreover both of these effects vary widely across demographic groups. In terms of crowd-out, we find statistically significant, though small, effects for all eligible children, but not for those affected by a counterfactual policy change.

Keywords: Medicaid expansions, take-up, crowd-out, treatment effects, switching probit model, random coefficient models, linear probability models, counterfactual policy analysis.

1. Introduction

In recent years, eligibility for public health insurance among children has expanded substantially, leading to a burgeoning of research on the implications of such expansions for public insurance participation, private insurance crowd-out, and overall levels of health insurance coverage. A common approach to these questions is to estimate a linear probability model of participation (or private or overall insurance coverage) where a dummy variable for eligibility for the program is an endogenous explanatory variable (see Cutler and Gruber (1996), Currie and Gruber (1996a, 1996b), LoSasso and Buchmueller (2004), Hudson, Selden, and Banthin (2005), Ham and Shore-Sheppard (2005), and Shore-Sheppard (2008), among others.) This model permits the estimation of a local average treatment effect—the average effect of eligibility on insurance coverage for (unobserved) families who are responsive to changes in the instrument used for identification. This approach, while quite useful, has three drawbacks. First, it provides an estimate of the policy effect applying to a group of families where the composition of the group is unobserved by the researcher. Second, it provides no information about which groups have low take-up rates. As we demonstrate below, observable groups vary dramatically in their take-up response to Medicaid expansions, and it would be very helpful for policy makers to know how responses differ when designing outreach programs. Third, it may lead to misleading predictions of the effects of non-marginal (large) expansions if non-marginal newly eligible families have different characteristics, and thus different take-up rates, from families at the margin.

Recently the empirical public economics literature has seen a number of papers that explicitly estimate observable heterogeneous treatment effects for different groups, and thus have the potential to be much more informative for policy analysis. The majority of these effects have been estimated within the education and training literature (see, e.g., Aakvik, Heckman, and Vytlacil (2005), Blundell, Diarden, and Sianesi (2005), Card and Payne (2003), Heckman, Smith, and Clemens (1997), and Moffitt (2007)), although there are other applications (see, e.g., Angrist (2004) for treatment effect heterogeneity in the effects of childbearing on marital dissolutions, poverty status, and welfare participation). One popular approach in this literature is to use a random coefficients framework (where the coefficients are a function of demographic variables) to estimate marginal effects for different demographic groups. Another approach is to use a switching probit framework and to predict the effect of changing eligibility on the basis of observables and unobservables. [1]

In this paper we first show that the random coefficients framework, as well as the switching probit framework, can be used to estimate a number of important effects for Medicaid take-up that are not available in the current Medicaid literature. Specifically, we first demonstrate how to estimate, for Medicaid expansions and by demographic group, marginal effects (i.e. the effect of an expansion that makes a relatively small number of individuals eligible), average effects for a randomly chosen child, the effect of eligibility on the eligible, and the effect of a policy expansion that makes a large number of children eligible for Medicaid (i.e. a non-marginal change) on the take-up of public insurance, participation in private insurance, and crowd-out (defined as the change in the probability of private coverage due to a change in eligibility status). We then estimate the average effect of Medicaid expansions among all eligible children and the average effect for those affected by a nonmarginal counterfactual Medicaid expansion, since these are likely to be the most useful for policy analysis. Estimated take-up rates among average eligible children are substantially larger than take-up rates for those made eligible by a counterfactual Medicaid expansion, moreover both of these effects vary widely across demographic groups. In terms of crowd-out, we find statistically significant, though small, effects for all eligible children, but not for those affected by a counterfactual policy change. These policy effects of Medicaid expansions have not previously been estimated using either the random coefficients framework or the switching probit framework. (The latter also permits the estimation of these policy effects while also allowing for differences in unobservables across families.) We compare the estimates from these two frameworks. Finally, to maximize both the usefulness of our approach to other researchers and the ease of replicating our estimates, we restrict ourselves to estimators that can be implemented using preprogrammed commands in Stata, an econometric package commonly used by researchers in this area.

[1] The first approach is used by Blundell et al. (2005) and Card and Payne (2003) while the second is used by Aakvik et al. (2005). The approach introduced in Moffitt (2007) can be considered a combination of the two, since he allows treatment effects to depend on both observables and unobservables that are correlated with the error in the outcome of interest within a random coefficients framework. Heckman et al. (1997) use propensity score matching while allowing for treatment heterogeneity, while Angrist (2004) uses an IV framework to show the relationship between a local average treatment effect and an average treatment effect under certain assumptions.

In the next section we review the Medicaid program and some previous studies that have used a two-equation linear probability model to estimate the insurance responses to the Medicaid expansions. In Section 3, we outline this standard approach to public health insurance take-up and private insurance crowd-out. We then discuss our use of random coefficient models and switching regression models, as well as the policy effects we estimate. After discussing our data briefly in Section 4, in Section 5 we use these two methods to present new estimates of (heterogeneous) treatment effects of the actual Medicaid expansions and a counterfactual expansion. With one exception noted below, these effects are precisely estimated and statistically significant. The take-up rate for the newly eligible is much lower than for all of the previously eligible, and, for the sample as a whole, is comparable to the (local average treatment effect) estimates from a standard linear probability model. Further, we find that groups vary dramatically in terms of their take-up behavior for Medicaid; our estimated take-up rates for the counterfactual Medicaid expansion range from 0.06 to 0.63. In terms of crowd-out, when we look at demographic groups within the sample of previously eligible children we find a positive and statistically significant crowd-out effect, with significant differences across groups. However, we find very noisy estimates of crowd-out among those made newly eligible in our counterfactual experiment. Section 6 concludes the paper.

2. Medicaid Expansions and Previous Literature

Medicaid was first established as a public health insurance program for welfare recipients and low-income aged and disabled individuals. This focus largely remained until the late 1980s, when expansions in eligibility first permitted, and then required, states to cover pregnant women and children with family incomes that made them ineligible for cash welfare. Following the federally mandated eligibility expansions of 1989 and 1990, states were required to cover children age 6 or younger with family incomes up to 133% of the poverty line and children born after September 30, 1983 with family income up to 100% of the poverty line. States were also given the option to increase their eligibility thresholds up to 185% of the poverty line. As these eligibility limits were far more generous than the eligibility limits applying to cash welfare (at the time, Aid to Families with Dependent Children, or AFDC), the link between Medicaid eligibility and AFDC eligibility greatly diminished for young, low-income children. By 1996, of the approximately 30% of children age 19 and younger who were eligible for Medicaid, only about half came from typically welfare-enrolled families (Selden, Banthin, and Cohen 1998). While families who enrolled in cash welfare programs were also automatically enrolled in Medicaid, newly eligible children were not. Consequently the establishment of a new route to Medicaid eligibility raised an important policy question: to what extent did expanded eligibility lead to increased health insurance coverage for the targeted population of children? Moreover, since newly eligible children were less poor than previously eligible children were and hence more likely to have access to private insurance, another important policy question was whether and to what extent expanded eligibility led to "crowding out" of private health insurance by public insurance availability.

There has been a substantial amount of research on these questions, and a non-exhaustive list includes Currie and Gruber (1996a, 1996b), Cutler and Gruber (1996), Dubay and Kenney (1996), Thorpe and Florence (1998), Yazici and Kaestner (1999), Shore-Sheppard (2000), Blumberg, Dubay, and Norton (2000), Ham and Shore-Sheppard (2005), and Shore-Sheppard (2008). Further, there is also research on the related question of how the further public health insurance expansions of the State Children's Health Insurance Program (SCHIP) affected coverage and crowd-out (LoSasso and Buchmueller (2004), Hudson, Selden, and Banthin (2005), Gruber and Simon (2008)). The papers in this literature have provided estimates of a variety of behavioral parameters related to the responsiveness of children's insurance coverage to expanded eligibility. The most common approach used for estimating effects of expanded eligibility, the instrumental variable linear probability model that we describe in more detail below, produces estimates of the average take-up and private coverage for a group of families (unknown to the researcher) who are responsive to changes in the instrument used for identification, and these are often referred to as "marginal" effects. They are not the same as the average take-up rate or private coverage loss among all eligible children (that is, including those children who were eligible prior to the expansion being studied). Moreover, they do not necessarily reflect the change that would occur in take-up or private coverage from a medium sized or large change in eligibility. These distinctions have often been missed in the literature. Definitions of "crowd-out"—loosely, the effect of public insurance availability on the propensity to have private coverage—are particularly diverse in the literature. For expositional purposes we will focus on one definition, but our methodology is easily generalized to other definitions. In this paper, we estimate a host of additional effects for take-up and crowd-out.

Since our aim in this paper is to extend previous results rather than summarizing the literature, here we focus on two of the studies that use the now standard approach. An important study using this approach is the seminal paper of Cutler and Gruber (1996) (CG hereafter). CG use the method outlined below in Section 3.1 and data on children from the March Current Population Survey (CPS) from 1988 to 1993 to estimate the effect of imputed Medicaid eligibility on insurance status, controlling for demographics and state and year effects. They use an instrumental variables approach since eligibility is likely to be endogenous. This potential endogeneity arises for several reasons. First, unobservable factors affecting eligibility are likely to be correlated with unobservable individual and family characteristics that determine the response to eligibility. Second, eligibility may serve as a proxy for family income if income, which is also likely to be endogenous, is not included as an independent variable. Finally, parental wages, which in turn determine eligibility, are likely to be correlated with fringe benefits (including private health insurance) of the parent. Since these benefits are unobserved, they are part of the error term, thus providing an additional factor necessitating treating eligibility as endogenous. To address the endogeneity of the eligibility variable, CG suggest an instrument (which we denote *FRACELIG* hereafter) that is the fraction of a random sample of 300 children of each age imputed to be eligible according to the rules in each state in each year. This instrument, which is essentially an index of the expansiveness of Medicaid eligibility for each age group in each state and year, is correlated with individual eligibility for Medicaid but not otherwise correlated with the demand for insurance, assuming that changes in a state's Medicaid provisions are not correlated with changes in the state's availability of private insurance, which are unobservable to the researcher.[2] CG estimate that the average take-up rate for those affected by the expansions was 23.5% and that the average reduction in private coverage for those affected by the expansions was about 7%. These IV estimates should be thought of as local average treatment effects, i.e. the average effect of eligibility on families whose eligibility status is sensitive to small changes in *FRACELIG*.

Ham and Shore-Sheppard (2005) use data from Survey of Income and Program Participation (SIPP) covering the period from October 1985 to August 1995 to replicate CG's analysis. Their estimates are marginal

[2] One attractive feature of this approach is that *FRACELIG* is an extremely strong instrument. This is very helpful since when we obtain precisely estimated marginal treatment effects for many demographic groups using *FRACELIG* times the demographic variables as instruments; this is not always the case in estimating such a model, see, e.g. Blundell et al (2005).

effects in the same sense that CG's are, and thus the results of both papers are comparable. They find a smaller average take-up rate of 11.8% for those affected by the expansions, as well as a smaller level of private coverage reduction or crowd-out, than CG. Ham and Shore-Sheppard attribute some of the differences between their results and CG's to different samples and recall periods in SIPP and the CPS. Ham and Shore-Sheppard also modify the CG instrument by using all sample observations of children of a given age in a SIPP wave except for those from the state for which the instrument is being calculated. Since this instrument is created using a larger sample, it is theoretically superior to the version using a random sample, but in practice it makes very little difference to the results. We use the data and instrument of Ham and Shore-Sheppard in our estimation of heterogeneous treatment effects and predicted effects from a counterfactual nonmarginal policy experiment below.

3. Approaches for Estimating Heterogeneous Treatment Effects

3.1. Allowing Treatment Effects to Depend on Observable Demographic Variables

As noted above, the approach to evaluating the effect of public health insurance eligibility changes that has become standard in the literature involves estimating simultaneous equation linear probability models (LPMs). The LPM for participation in a public insurance program is given by

$$pub_i = X_i\beta_1 + \gamma_1 elig_i + u_{1i}, \tag{1a}$$

where X_i is a vector of demographic variables for child *i*, $elig_i$ is a dummy variable coded one if the child is eligible for public insurance and zero otherwise, u_{1i} is an error term, and $pub_i = 1$ if child *i* participates in a public insurance program while $pub_i = 0$ otherwise. The LPM for private insurance coverage is given by

$$priv_i = X_i\beta_2 + \gamma_2 elig_i + u_{2i}, \tag{1b}$$

where $priv_i = 1$ if child *i* has private insurance coverage and $priv_i = 0$ otherwise.

The LPM determining public insurance eligibility is

$$elig_i = Z_i\delta + e_i, \tag{2}$$

where $Z_i = (X_i, FRACELIG_i)$, and e_i is an error term. From the discussion in CG, it is clear that they interpret the coefficients γ as average treatment effects for families whose eligibility is affected by marginal changes in $FRACELIG_i$. While this estimated average treatment effect parameter provides a useful summary statistic, it may be quite a bit less useful for predicting the effect of an eligibility change that affects children with different characteristics or for estimating average take-up rates among the currently eligible.

To address these limitations, we first consider a random coefficient model which consists of a linear probability model with interactions (RCLPM) for participation in public insurance (see, e.g., Blundell et al. (2005)). Specifically, we interact $elig_i$ with each of the K elements of X_i to obtain[3]

$$pub_i = X_i\beta_1 + (elig_i X_i)\theta_1 + u_{1i} = \sum_{k=1}^{K} X_{ik}(\beta_{1k} + \theta_{1k} elig_i) + u_{1i}. \tag{3a}$$

We follow the same approach to obtain the following RCLPM for private insurance participation

$$priv_i = X_i\beta_2 + (elig_i X_i)\theta_2 + u_{2i} = \sum_{k=1}^{K} X_{ik}(\beta_{2k} + \theta_{2k} elig_i) + u_{2i}. \tag{3b}$$

The natural vector of excluded instruments in (3a) and (3b) for the K by 1 vector of endogenous variables $(elig_i X_i)$ is the K by 1 vector $(FRACELIG_i * X_i)$.[4]

We can use this model to approximate the average take-up rate among the eligible, i.e. the average treatment effect on the treated (ATET hereafter) as

$$ATRE^{lpm} = \sum_{elig_i=1} [X_i\hat{\beta}_1 + X_i\hat{\theta}_1]/N_e, \tag{4a}$$

where $\hat{\beta}_1$ and $\hat{\theta}_1$ are parameter estimates from (3a) and N_e is the number of eligible children. Note that this is an approximation since it takes into account the distribution of the independent variables among the eligible, but does not take into account the fact that those not on the margin of eligibility will necessarily have the same

[3] An alternative approach would be to use $(X_i * (Z_i\hat{\delta}))$ as our vector of instruments , where $\hat{\delta}$ is estimated by (2) – see Amemiya (1985). However, for comparability with other studies, we follow the procedure in the text.

[4] If one had a very large data set, one could parameterize the model in terms of mutually exclusive demographic cells, but our data set is not large enough for us to do this.

response as those on the margin. We denote the set of children eligible for Medicaid in demographic group *j* by G_j. Then we approximate the average take-up rate among the eligible for group *j* as

$$ATRE_j^{lpm} = \sum_{i \in G_j} [X_i \hat{\beta}_1 + X_i \hat{\theta}_1] / N_{ej}, \tag{4b}$$

where N_{ej} is the number of eligible children in group *j*. In this calculation we exploit the fact that a child cannot participate in public insurance if ineligible. We can measure the average private insurance coverage rate among those who are eligible for Medicaid as

$$PITE^{lpm} = \sum_{elig_i = 1} [X_i \hat{\beta}_2 + X_i \hat{\theta}_2] / N_e, \tag{4c}$$

and the counterfactual average private insurance coverage rate among those eligible if they were not eligible as

$$PITNE^{lpm} = \sum_{elig_i = 1} [X_i \hat{\beta}_2] / N_e, \tag{4d}$$

where $\hat{\beta}_2$ and $\hat{\theta}_2$ are the estimated values of β_2 and θ_2 respectively from (3b). We define crowd-out for a child as the probability that the child has private insurance when not eligible for Medicaid minus the probability the child has private insurance when eligible for Medicaid. Thus we measure average crowd-out among all those eligible as

$$COE^{lpm} = PITNE^{lpm} - PITE^{lpm} = -\sum_{elig_i = 1} [X_i \hat{\theta}_2] / N_e. \tag{5a}$$

To measure crowd-out among those eligible in group j we use

$$COE_j^{lpm} = -\sum_{i \in G_j} [X_i \hat{\theta}_2] / N_{ej}. \tag{5b}$$

Since the measures in (4a)-(5b) are linear functions of regression coefficients, one can calculate their standard errors using preprogrammed commands in Stata. (Details on this calculation and our Stata programs to implement all approaches used in the paper are given in our Online Appendix A.[5])

[5] Readers who are interested in the standard error calculations can access to our Online Appendix at http://econ.williams.edu/people/lshore.

Take-up for a randomly chosen person, i.e. the average treatment effect (ATE hereafter), can be estimated using

$$ATR^{lpm} = \sum_{i=1}^{N} [X_i \hat{\beta}_1 + X_i \hat{\theta}_1]/N, \tag{5c}$$

where the summation is over the whole sample and N is the sample size. As with the ATET above, and for the same reason, this is an approximation of the ATE. Similarly, the approximate crowd-out for a randomly chosen person can be estimated using

$$CO^{lpm} = -\sum_{i=1} [X_i \hat{\theta}_2]/N_e. \tag{5d}$$

The extension of (5c) and (5d) to different demographic groups is straightforward and omitted to save space.

In addition, it is possible to estimate the take-up response and crowd-out effect of further marginal and non-marginal expansions for both all of the newly eligible and the newly eligible in different demographic groups. To calculate the effect of a marginal expansion, we can increase the income limits a relatively small amount (e.g. 1%-2%). Next, we determine which children would become newly eligible under the expansion, denoting this group as New. Then a natural means of measuring the marginal take-up effect when considering all of the newly eligible is

$$TNEW^{lpm} = \sum_{i \in New} [X_i \hat{\beta}_1 + X_i \hat{\theta}_1]/N_{new}, \tag{6a}$$

where N_{new} is the number of newly eligible. Next, determine which children become newly eligible in demographic group *j* under the expansion, and denote this group as $Newj$. (One may need a larger expansion in the income limits to generate a positive number of observations in each cell.)

Then we can estimate the marginal take-up in group *j* as

$$TNEW_j^{lpm} = \sum_{i \in Newj} [X_i \hat{\beta}_1 + X_i \hat{\theta}_1]/N_{newj}, \tag{6b}$$

where N_{newj} is the number of newly eligible in group *j*. We can estimate the marginal crowd-out effect among all the newly eligible as

$$CNEW^{lpm} = -\sum_{i \in New} [X_i \hat{\theta}_2] / N_{new} , \tag{7a}$$

and the marginal crowd-out effect among the newly eligible in demographic group *j* as

$$CNEW_j^{lpm} = -\sum_{i \in Newj} [X_i \hat{\theta}_2] / N_{newj} . \tag{7b}$$

The marginal effects for the whole sample obtained using (6a) and (7a) are comparable to the estimates of γ_1 and γ_2 from the LPM estimates of (1a) and (1b).

Finally, note that (6a)-(7b) can be used in an analogous fashion to calculate the effects of non-marginal increases in the income limits that result in a large number of newly eligible children, and indeed we carry out this calculation for a counterfactual 10% increase in Medicaid income limits below. Compared to the standard approach, our approach also has the advantage that it is clear who the marginal families are.

3.3 Estimating Heterogeneous Treatment Effects Using Switching Probit Models

Our second approach uses the switching probit model (SPM) developed by Quandt (1958, 1960, 1972) and Heckman (1979), which has been applied in the bivariate probit with selection case by van de Ven and van Praag (1981), and in the program evaluation context by Aakvik, Heckman, and Vytlacil (2005) (hereafter AHV). This model allows us to estimate policy effects based on observables and unobservables once we make a distributional assumption.[6] A joint switching probit model for participation in public insurance, participation in private insurance, and public insurance eligibility is quite complicated to estimate since it requires trivariate integration, and there is no preprogrammed routine in Stata to estimate this model.[7] Fortunately, it is straightforward to show that all parameters can be estimated consistently by separately considering two likelihood functions: one containing the parameters for eligibility and for public insurance participation and one containing the parameters for eligibility and for private insurance participation. This allows us to estimate all necessary parameters using preprogrammed routines in Stata.

[6] The SPM also has the advantages that it explicitly precludes anyone from obtaining Medicaid if they are ineligible for it and, of course, that estimated probabilities are always in the unit interval.
[7] The computation is simplified if one uses the factor structure in AHV.

We first consider the likelihood function containing the parameters for eligibility and for public insurance participation. We assume that the index function for eligibility is

$$Elig_i^* = Z_i\delta + e_i . \tag{9}$$

Next, we assume that for a randomly chosen (in terms of unobservables) individual, the index function for participation in public insurance once being made eligible is

$$PPub_i^* = X_i\mu + \varepsilon_i , \tag{10}$$

where $(\varepsilon_i, e_i) \sim iid\ N(0, V_{pub})$ and $V_{pub} = \begin{bmatrix} 1 & \rho_{\varepsilon,e} \\ \rho_{\varepsilon,e} & 1 \end{bmatrix}$. Following the econometrics literature, it is natural to write the index function for take-up for a randomly chosen (in terms of ε_i) child—this index function models how a randomly chosen individual would behave after being exogenously assigned Medicaid eligibility; otherwise one cannot do counterfactual policy analysis. However, it is worth emphasizing that unless $\rho_{\varepsilon,e} = 0$, those actually eligible will not be a randomly chosen subgroup of the population. Below we take this into account when we calculate take-up rates.

The appropriate log likelihood for eligibility and participation in public insurance is

$$L = \sum_{elig=1\, pub=1} \log \Phi_2[(X_i\mu, Z_i\delta, \rho_{\varepsilon,e}] + \sum_{elig=1,\, pub=0} \log \Phi_2[-X_i\mu, Z_i\delta, -\rho_{\varepsilon,e}] + \sum_{elig=0} \log \Phi_1[-Z_i\delta], \tag{11}$$

where $\Phi_2[\bullet,\bullet,\rho_{\varepsilon,e}]$ is the bivariate standard normal distribution function and $\Phi_1(\bullet)$ is the univariate standard normal distribution function. For Medicaid take-up calculations we will only need the parameters estimated by maximizing (11).

For crowd-out calculations, we also need to consider participation in private insurance, so we now consider the likelihood function containing the parameters for eligibility and for private insurance participation. We assume that for a randomly chosen individual, the index function for participation in private insurance given eligibility for public insurance is

$$Priv\_elig_i^* = X_i\gamma_e + u_{ei} . \tag{12a}$$

We define the observed outcome variable $priv\_elig_i = 1$ if $Priv\_elig_i^* > 0$ and zero otherwise, since this will be useful below. (Note that we use upper case (first letters) and stars to denote index functions and lower case to denote observed (realized) outcomes.) Further, we assume that for a randomly chosen individual the index function for participation in private insurance given ineligibility for public insurance is

$$Priv\_nelig_i^* = X_i\gamma_{ne} + u_{nei}, \tag{12b}$$

and we define $priv\_nelig_i = 1$ if $Priv\_nelig_i^* > 0$ and zero otherwise. Finally we assume

$$(u_{ei}, u_{nei}, e_i) \sim iidN(0, V_{priv}) \quad \text{and} \quad V_{priv} = \begin{bmatrix} 1 & \rho_{12} & \rho_{13} \\ \rho_{12} & 1 & \rho_{23} \\ \rho_{13} & \rho_{23} & 1 \end{bmatrix}. \tag{12c}$$

The appropriate log likelihood is

$$\begin{aligned} L = & \sum_{elig=1,\, priv=1} \log \Phi_2[(X_i\gamma_e, Z_i\delta, \rho_{n13}] \quad + \sum_{elig=1,\, priv=0} \log \Phi_2[-X_i\gamma_e, Z_i\delta, -\rho_{n13}] + \\ & \sum_{elig=0,\, priv=1} \log \Phi_2[(X_i\gamma_{ne}, -Z_i\delta, -\rho_{n23}] \quad + \sum_{elig=0,\, priv=0} \log \Phi_2[-X_i\gamma_{ne}, -Z_i\delta, -\rho_{n23}]. \end{aligned} \tag{13}$$

Unfortunately, even though this likelihood function involves only bivariate integration, there is no preprogrammed routine in Stata to estimate this model. Instead, we randomly divide the sample into two subsamples. On the first subsample we maximize

$$L_1 = \sum_{elig=1,\, priv=1} \log \Phi_2[(X_i\gamma_e, Z_i\delta, \rho_{n12}] + \sum_{elig=1,\, priv=0} \log \Phi_2[-X_i\gamma_e, Z_i\delta, -\rho_{n12}] + \sum_{elig=0} \log \Phi_1[-Z_i\delta]. \tag{14}$$

Next, on the other subsample maximize

$$L_2 = \sum_{elig=0,\, priv=1} \log \Phi_2[(X_i\gamma_{ne}, -Z_i\delta, -\rho_{n13}] + \sum_{elig=0,\, priv=0} \log \Phi_2[-X_i\gamma_e, -Z_i\delta, \rho_{n13}] + \sum_{elig=1} \log \Phi_1[Z_i\delta]. \tag{15}$$

Note that since we randomly chose the subsamples, parameter estimates from (14) and (15) are independent. Also note that as is common in this type of problem (e.g. estimation of the union and nonunion wage structures), ρ_{n23} is not identified even if we were to use (13) for estimation, since we never see anyone eligible and not eligible for public insurance at the same time.

The cost of the SPM versus the LPM would seem to be the need for a normality assumption, but interestingly this is not what available Monte Carlo evidence suggests. Angrist (2001) conducts Monte Carlo experiments for a linear probability model with a discrete endogenous regressor estimated by instrumental variables. He finds estimated treatment effects that are very close to those from a properly specified simultaneous equation bivariate probit model. In other words, the simultaneous equation LPM does well with normally distributed errors. However, Bhattacharya et al. (2006) show that the simultaneous equation LPM does not do well when the error terms are *not* normally distributed. In fact, based on their Monte Carlo evidence, Bhattacharya et al. (2006) argue in favor of using simultaneous equation bivariate probit, rather than the linear probability model, when the error terms are *not* normally distributed. Thus it appears that the LPM estimates are actually more dependent on the normality assumption than are estimates from the simultaneous equation bivariate probit model. Of course it would be ideal to eliminate the need to make any distributional assumptions, but unfortunately Shaikh and Vytlacil (2005) show that, in general, only bounds on the average effect of treatment can be identified in this case. Further, AHV stress the need for making a distributional assumption for the treatment effects considered below once the relevant parameters have been estimated. Of course, researchers are free to estimate the model with a different distributional assumption, e.g. assume that the error terms are drawn from a mixture of normals (see e.g., Carneiro, Hansen, and Heckman (2003)). We do not adopt this approach since it would force us to violate our commitment to using prewritten Stata modules

As in the case of the linear probability model with interactions, for the SPM we can look at the predicted average take-up rates among all eligible individuals (the ATET) by calculating

$$ATRE^{spm} = \sum_{elig=1} [1 - \Phi_1(-X_i\hat{\mu})] / N_e, \tag{16a}$$

where $\hat{\mu}$ is obtained by maximizing (11) and again N_e is the number of eligible children. This is comparable to the effect estimated by the RCLPM in (4a). The delta method can be used to calculate the standard errors for these predicted take-up rates in Stata, and details of this calculation are given in our Online Appendix A.[8] One can also calculate take-up for a randomly chosen person over the whole sample (the ATE) as

$$ATR^{spm} = \sum_{i=1}^{N}[1 - \Phi_1(-X_i\hat{\mu})] / N, \tag{16b}$$

where N is the overall sample size. To compute the average take-up among the eligible in different demographic groups, the process can simply be repeated for each group of interest *j*

$$ATRE_j^{spm} = \sum_{i \in G_j}[1 - \Phi_1(-X_i\hat{\mu})] / N_{ej}, \tag{16c}$$

where again G_j denotes the set of individuals eligible for Medicaid in group *j* and N_{ej} is the number of eligible children in group *j*. These estimates are comparable to the effects estimated by the RCLPM in (4b). Calculating these average take-up rates for a randomly chosen child within demographic groups is analogous to (16c).

The above calculations do not take into account the fact that the eligible children will not be a random sample in terms of the error term in the eligibility index (9). To account for this when calculating the average take-up among the eligible (ATET) we use

$$\begin{aligned} ATRSELE^{spm} &= \frac{1}{N_e}\sum_{elig_i=1}\Pr(pub_i = 1 \mid elig_i = 1) = \frac{1}{N_e}\sum_{elig_i=1}\left[\frac{\Pr(pub_i = 1, elig_i = 1)}{\Pr(elig_i = 1)}\right] \\ &= \frac{1}{N_e}\sum_{elig_i=1}\frac{\Phi_2(X_i\hat{\mu}, Z_i\hat{\delta}_e, \hat{\rho}_{\varepsilon,e})}{\Phi_1(Z_i\hat{\delta}_e)}, \end{aligned} \tag{17}$$

where $(\hat{\mu}, \hat{\delta}_e, \hat{\rho}_{\varepsilon,e})$ are obtained by maximizing (11). Intuitively, this equation accounts for selection by treating take-up as a conditional probability, while (16a) ignores selection by using an unconditional probability. We estimate take-up for a specific demographic group *j* using an analogous calculation to (17) just for the subset of children in group *j*.

We can calculate the effects of marginal and non-marginal increases in the income limits in an analogous manner to that used with the RCLPM. For example, consider the non-marginal counterfactual policy change of an

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[8] One does need to program the derivatives that the delta method uses, but one can easily calculate numerical derivatives using the formula in our Online Appendix.

increase in the Medicaid income limits by 10% in 1995 as described in section 3.2 above. To calculate the average take-up rate among the newly eligible across all demographic groups using the SPM when we ignore the fact that they will not be a random sample of the population (in terms of e_i and ε_i), we use

$$TNEWE^{spm} = \sum_{i \in New} [1 - \Phi_1(-X_i \hat{\mu})] / N_{new}. \tag{18}$$

where again N_{new} is the number of newly eligible children. Again the calculation for the newly eligible in demographic group *j* is analogous to (18).

However, one can also take into account the fact that the newly eligible will not be a random sample in terms of the unobservables e_i and ε_i. For each individual let Z_{0i} denote the value of the previous explanatory variables (including the value of *FRACELIG* under the previous income limits) and Z_{1i} denote the value of the new explanatory variables under the new income limits (including the new value of *FRACELIG).* Then the probability that a child is newly eligible is given by $\Pr(newelig_i) = \Pr(-Z_{1i}\delta < e_i \leq -Z_{0i}\delta)$. To consider average take-up among the newly eligible which incorporates selection we use

$$\begin{aligned} TNEWSEL^{spm} &= \sum_{i \in New} \Pr(pub_i = 1 \mid newelig_i = 1) / N_{new} = \sum_{i \in New} \left[\frac{\Pr(pub_i = 1, newelig_i = 1)}{\Pr(newelig_i = 1)} \right] / N_{\text{new}} \\ &= \sum_{i \in New} \frac{\Phi_2(X_i\hat{\mu}, -Z_{0i}\hat{\delta}_e, \hat{\rho}_{\varepsilon,e}) - \Phi_2(X_i\hat{\mu}, -Z_{1i}\hat{\delta}_e, \hat{\rho}_{\varepsilon,e})}{\Phi_1(-Z_{0i}\hat{\delta}_e) - \Phi_1(-Z_{1i}\hat{\delta}_e)} / N_{new}. \end{aligned} \tag{19}$$

As before, the calculation for average take-up among the newly eligible in group *j* is analogous to (19). Note that one can again use the delta method to get a standard error for (19) using Stata. To calculate a marginal effect, increase the income limits by 1%-2%, and use (19). For the case where we do not separate children by demographic group, these effects are comparable to the estimates of γ_1 from (1a) that the standard LPM estimates.

We again define crowd-out for a child as the probability the child has private insurance when not eligible for Medicaid minus the probability the child has private insurance when eligible for Medicaid. Thus we measure average crowd-out among all those eligible when we do not take into account the fact that the eligible are a nonrandom sample in terms of $(e_{i,} u_{ei}, u_{nei})$ using

$$CO^{spm} = \sum_{elig=1} [\Pr(X_i\gamma_{ne} + u_{nei} > 0) - \Pr(X_i\gamma_e + u_{ei} > 0)] / N_e$$
$$= \sum_{elig=1} ([(1 - \Phi_1(-X_i\hat{\gamma}_{ne})] - [(1 - \Phi_1(-X_i\hat{\gamma}_e)]) / N_e. \tag{20}$$

Note that we obtain $\hat{\gamma}_e$ and $\hat{\gamma}_{ne}$ from maximizing (14) and (15) respectively; since we use separate random samples the estimates are independent and thus we do not need to calculate their covariance in calculating a standard error for (20). If we take into account the fact that those currently eligible are a nonrandom sample in terms of $(e_{i,}u_{ei}, u_{nei})$, we estimate average crowd-out among the eligible by

$$COSEL^{spm} = \sum_{elig=1} [\Pr(priv\_nelig_i = 1 \mid elig_i = 1) - \Pr(priv\_elig_i = 1 \mid elig_i = 1)] / N_e$$
$$= \sum_{elig=1} \left[\frac{\Pr(priv\_nelig_i = 1, elig_i = 1)}{\Pr(elig_i = 1)} - \frac{\Pr(priv\_elig_i = 1, elig_i = 1)}{\Pr(elig_i = 1)} \right] / N_e \tag{21}$$
$$= \left[\sum_{elig=1} \frac{\Phi_2(X_i\hat{\gamma}_{ne}, Z_i\hat{\delta}_2, \hat{\rho}_{23})}{\Phi_1(Z_i\breve{\delta}_2)} - \frac{\Phi_2(X_i\hat{\gamma}_e, Z_i\hat{\delta}_1, \hat{\rho}_{13})}{\Phi_1(Z_i\tilde{\delta}_1)} \right] / N_e,$$

where $priv\_elig_i$ and $priv\_nelig_i$ are defined below (11) and (12) respectively. Moreover, since $(\hat{\gamma}_{ne}, \hat{\delta}_2, \hat{\rho}_{23})$ are obtained by maximizing (15) on the first random subsample and $(\hat{\gamma}_e, \hat{\delta}_1, \hat{\rho}_{13})$ are obtained by maximizing (14) on the second random subsample, the estimates are independent and thus the two terms in (21) are independent.

Now consider an expansion of the Medicaid limits and continue to let Z_{0i} denote the value of the previous explanatory variables (including the previous value of *FRACELIG*) under the previous income limits and Z_{1i} denote the value of the new explanatory variables under the new income limits. (The only explanatory variable that changes under the expansion is *FRACELIG,* since as the income limits rise more children will be eligible in each state.) If we ignore the fact that the newly eligible are not a random sample, we estimate average crowd-out among the newly eligible by

$$CNEW^{spm} = [\sum_{i \in New} (\Phi_1(X_i\hat{\gamma}_{ne}) - \sum_{i \in New} (\Phi_1(X_i\hat{\gamma}_e)] / N_{new}, \tag{22}$$

where the two sums in (22) are also independent since, as noted above, they are based on separate estimation based on two random subsamples of independent data. To account for the fact that the newly eligible are not a random sample, we now calculate average crowd-out for the newly eligible using

$$
\begin{aligned}
CNEWSEL^{spm} &= \left[\sum_{i \in New} \Pr(priv\_nelig_i = 1 \mid newelig_i = 1) - \sum_{i \in New} \Pr(priv\_elig_i = 1 \mid newelig_i = 1) \right] / N_{new} \\
&= \left[\sum_{i \in New} \frac{\Pr(priv\_nelig_i = 1, newelig_i = 1)}{\Pr(newelig_i = 1)} - \sum_{i \in New} \frac{\Pr(priv\_elig_i = 1, newelig_i = 1)}{\Pr(newelig_i = 1)} \right] / N_{new} \\
&= \left[\sum_{i \in New} \frac{\Phi_2(X_i \hat{\gamma}_{ne}, Z_i \hat{\delta}_2, \hat{\rho}_{23})}{\Phi_1(-Z_{1i}\delta_2) - \Phi_1(-Z_{0i}\delta_2)} - \sum_{i \in New} \frac{\Phi_2(X_i \hat{\gamma}_{e}, Z_i \hat{\delta}_1, \hat{\rho}_{13})}{\Phi_1(-Z_{1i}\delta_1) - \Phi_1(-Z_{0i}\delta_1)} \right] / N_{new},
\end{aligned}
\tag{23}
$$

where again the two sums in the last line of (23) are independent. Finally, one again can estimate marginal crowd-out effects for the SPM using (23) once for a 1%-2% increase in income limits. For the case where we do not separate children by demographic group, these effects are comparable to the estimates of γ_2 from (1b) that the standard LPM estimates.

3. Comparison to Previous Work Estimating Heterogeneous Treatment Effects

Here we consider how our approach relates to previous work estimating heterogeneous treatment effects. It is worth noting that our approach based on the SPM builds on the important work of AHV and the papers they cite, but also differs in several important ways. First, we analyze a program, Medicaid, where a child cannot participate if ineligible, and we exploit this feature in estimation and in calculating treatment effects. Second, Medicaid eligibility is observable given family income, state of residence, year and child's age. We use this in calculating policy effects, and it allows us to consider non-marginal counterfactual Medicaid expansions. (Note that both of these features of Medicaid apply to other programs, such as Food Stamps.) Third, we assume an unrestricted trivariate normal distribution for our error terms whereas AHV use a factor structure to allow correlation across the errors.[9] Fourth, we show that policy effects based on the SPM can be precisely estimated, at least for the case of Medicaid; this was not the case for AHV's work on Norwegian training programs. Fifth, we show researchers how pre-programmed commands in Stata can be used to estimate all relevant effects and most standard errors.

Compared to the previous work with random coefficients, our first contribution is to analyze the take-up and crowd-out of Medicaid expansions. Secondly, we show how to obtain many interesting policy effects from the

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[9] Note, however, that Carneiro, Hansen, and Heckman (2003) use a mixture of normals.

RCLPM that are comparable to those from the SPM, and then empirically compare these effects below. Finally, we find that our instruments ($(FRACELIG_i * X_i)$ are much stronger (in the Imbens and Wooldridge 2008 sense of using the size of the standard errors on the endogenous variables as an indicator of instrument strength) than those used in an education application by Blundell et al. (2005).

4. Data

We use data from the Survey of Income and Program Participation (SIPP) 1986, 1987, 1988, 1990, 1991, 1992, and 1993 panels, which cover the period 1986-1995. The SIPP is a nationally representative longitudinal household survey which is specifically designed to collect detailed income and program participation information. The recall period between each interview is four months for every individual, and for our panels the panel length ranges from 24 months for the 1988 panel to 40 months for the 1992 panel. Although the sample universe is the entire U.S., the Census Bureau did not separately identify state of residence for residents of nine low population states in those panels. Since state of residence information is critical for us to impute Medicaid eligibility, we drop all individuals whose state of residence is not identified. We also restrict our sample to children living in households that are part of the original sample and who are younger than 16 years old at the first time they are observed. Finally, for comparability with earlier studies we drop children who are observed only once, children who leave the sample and then return, and children who move between states during the sample period. In total, these omitted observations constitute less than 8% of the sample.

Although the four-month period increases the probability of accurate reporting, particularly relative to the fifteen-month recall period of the March Current Population Survey (Bennefield 1996), the SIPP suffers from the problem of "seam bias." Specifically, Census Bureau researchers have shown that there are a disproportionate number of transitions between the last month of the current wave and the first month of the next wave (see, e.g., Young 1989, Marquis and Moore 1990). We use data for all interview months and account for seam bias by including a dummy variable for the fourth month of each interview wave. When we calculate predicted take-up and crowd-out probabilities, we follow Ham, Li, and Shore-Sheppard (2009) and adjust our parameter estimates by dropping the coefficient on the fourth month dummy and adding one-quarter of this coefficient to the intercept.

In their study of accounting for seam bias in a multi-state, multi-spell duration model, Ham, Li and Shore-Sheppard (2009) find that this is preferable to using data only from the fourth month of each wave.

We need to impute Medicaid eligibility and use four steps to do so. First, we construct the family unit relevant for Medicaid program participation and determine family income. Second, we assign family-specific poverty thresholds based on the size of the family and the year. Since Medicaid eligibility resulted from AFDC eligibility over this period, we then use information on the family income and family structure, along with the AFDC parameters in effect in the state and year, to impute eligibility for AFDC. Finally, we assign Medicaid eligibility if any of the following conditions hold: the child is in an AFDC-eligible family; the child is income eligible for AFDC and either lives in a state without a family structure requirement or lives in a state with an AFDC-unemployed parent program and has an unemployed parent; or the child's family income as a percent of the relevant poverty line is below the Medicaid expansion income eligibility cutoff in effect for that age child in his or her state of residence at that time.

In Table 1 we present the (unweighted) sample means for the variables used in our regressions. Both Medicaid participation and Medicaid eligibility rose over the course of the sample, while private insurance coverage fell. The rise in eligibility was particularly dramatic between the 1988 and 1990 panels, when federally mandated expansions took effect. Compared with the changes in insurance eligibility and coverage, the demographic variables are fairly stable across panels.

5. Results

5.1 Medicaid Parameter Estimates, Predicted Rates of Take-up and Responses to a Policy Experiment

In this section, we first briefly discuss our results from the three models we estimate. With the exception of the standard model previously estimated in the literature, our coefficient estimates are not as informative as the predicted rates from our models. Consequently, to save space we do not present the coefficients in the main text but make them available for the reader in Tables B1 and B2 in the Online Appendix. Moreover, we focus on the predicted Medicaid take up rates for the eligible (the ATET) and the response to our counterfactual policy experiment since these are likely to be most useful for policy.[10]

In all our regressions we include demographic variables as well as state, year and age dummies for each child to control for state-specific, age-specific and year-specific unobservables. Also, since we use longitudinal data, we cluster the standard errors to account for dependence across person-specific observations. For the standard LPM, the estimated take-up rate averaged across marginal families is a very statistically significant 0.127. (See column (1) of Table B1 in the Online Appendix.) Since these are IV estimates we need to consider the issue of weak IV. We cannot investigate whether our instruments are weak using the rule of thumb for the F-test being greater than 10 suggested by Staiger and Stock (1997), or the refinements of their rule in Stock and Yogo (2005), since the F-test is not appropriate if the observations are dependent across the same child or if heteroskedasticity is present. Since the first stage equation is a LPM estimated on panel data, both problems will occur in our application. Instead we use the rule-of-thumb from Hansen, Hausman, and Newey (2008) that the Wald statistic for the coefficient on the excluded instrument *FRACELIG* in the first stage equation should be greater than 33 (for one excluded instrument). Since we find that the Wald statistic for the coefficient on *FRACELIG* is approximately 3,500, we conclude that weak instruments are not an issue here.

Using this basic specification, we estimate the simultaneous equation RCLPM discussed in section 3, allowing the eligibility effect to depend on demographic variables as well as on the state, year and age dummies for each child. (The eligibility coefficients for the Medicaid equation are presented in column (3) of Table B1 in the Online Appendix.) It is important to note that since the effect of eligibility is interacted with demographics (including continuous variables) in these models the eligibility main effect cannot be interpreted in isolation. In the Medicaid equation the demographic interaction coefficients vary dramatically across demographic groups and are individually significant (except for race and gender of the child). Of course these interaction effects are relative to the omitted category, and will change if we change the omitted category. On the issue of weak instruments for

[10] We do not feel that an experiment which makes every American child eligible for Medicaid is particularly interesting from a policy perspective since we believe there is little chance that the US will move to universal government sponsored health care, let alone one based on the current Medicaid system. (We include the relevant expressions for the ATEs since they may be useful in other applications.) We do not consider marginal effects since to be able to estimate these effects precisely across many demographic groups, we will need more than a one or two percent increase in income limits, i.e. something closer to our nonmarginal change.

the RCLPM model, we do not know of a well-developed rule of thumb or test for weak IV in an exactly identified model with K endogenous variables and error terms that are correlated over time (for the same individual) and heteroskedastic. However our problem has quite a bit of structure, since we are essentially asking if $FRACELIG_i * X_{ik}$ is a strong instrument for $elig_i X_{ik}$ over $k = 1,...,K$. Since we know from immediately above that $FRACELIG_i$ is an excellent instrument for $elig_i$, we also would expect $FRACELIG_i * X_{ik}$ to be an excellent instrument for $elig_i X_{ik}$.[11] Finally, if one takes the approach that the standard errors for the second stage equation are a good indicator of whether one has a weak IV problem when the number of instruments does not grow large with the sample size (see Imbens and Wooldridge 2008), then it is clear that weak IV are not a problem in our application.

We also estimate the SPM for Medicaid participation (see columns (1) and (2) of Table B2 in the Online Appendix). In all index functions we continue to include age, time and state dummies and account for dependence across observations for the same child. As expected, the coefficients in the eligibility index function are jointly significant, and all the coefficients except that for gender are very significant in the expected direction. While to the best of our knowledge there is not a rule of thumb for the coefficient on *FRACELIG* to assess the quality of empirical identification in the SPM, the Wald statistic for this variable is approximately 3600, suggesting that the participation equations for public private insurance are well identified in the SPM. Again all coefficients are relative to the omitted category.

Since, as noted above, the parameter estimates in the Online Appendix Tables B1 and B2 (for the RCLPM and SPM respectively) are not invariant to the choice of omitted category, it is much more useful to calculate the Medicaid take-up rates that are implied by the models. In column (1) of Table 2 we use the RCLPM to predict the

[11] When we test the null hypothesis $\lambda_k = 0$ in the quasi first stage regressions

$$elig_i X_{ik} = \kappa_{1k} X_i + \lambda_k (FRACELIG_i * X_{ik}) + u_{2i}, k = 1,...,K,$$

it is reassuring to note that the minimum Chi-Square statistic is 1920, 960, 1280 and 78 for the eligibility * demographics , the eligibility * age dummies,, the eligibility * year dummies, and the eligibility*state dummies in the first stage equation, respectively. Of course this is an informal testing procedure, since the tests are not independent. One could avoid this latter problem by testing the null hypothesis $\lambda_1 = \lambda_2 = ... = \lambda_K = 0$. Alternatively, if one followed Amemiya (1985) and used $(X_i * (Z_i \hat{\delta}))$ as the vector of instruments for $X_i * elig_i$ (where $\hat{\delta}$ is estimated by (2)), the test for weak IV would reduce to testing whether $FRACELIG_i$ is a weak IV for $elig_i$, and we have already seen that this null hypothesis is decisively rejected by the data.

average Medicaid take-up rates among eligible children (the ATET) from different demographic groups. In column (2) we present the corresponding estimates using the SPM. (Note that in each case we do not take into account the fact that those eligible for Medicaid are a non-random sample in terms of unobservables and thus the results in columns (1) and (2) are comparable.) The estimates in columns (1) and (2) can be compared to the take-up rate estimated for marginal families in the entire sample (using the standard LPM) of 0.12. We use the data from those eligible in the 1995 SIPP panel to calculate these take-up rates to make them comparable to results from our counterfactual policy experiment, in which we examine the impact of increasing the 1995 eligibility income limits by 10%.

In column (3) of Table 2 we present Medicaid take-up rates from the SPM using (17), where we take into account the fact that those who are eligible for Medicaid are not randomly selected from the population. Note that the results in this column are therefore not directly comparable with the estimated take-up rates in columns (1) and (2). The point estimates of these ATET for a given demographic group are very similar quantitatively in models (1) and (2), except for children from families with two or more earners. In columns (1) - (3) all estimates are easily statistically distinguishable from zero, although the estimates in column (2) are more precise than the estimates in column (1). Not surprisingly, the SPM produces more sensible results than does the RCLPM when the predicted probabilities are near zero or one. The take-up rates in column (3) are somewhat larger than those in columns (1) and (2) because they take into account the fact (as estimated by the data) that those who are eligible for Medicaid have unobservable characteristics that make them more likely to take up Medicaid.

Focusing on the estimates in column (2) for the SPM without accounting for unobservables for currently eligible children, we see a wide variety of average take-up rates among the eligible across groups, ranging from 0.08 for children from families with more than two earners to 0.77 for children from families with no earners. White children have a take-up rate of 0.40 while the take-up rate for non-white children is 0.64. The estimated take-up rate for children from families in which the family head has less than a high school degree is 0.60, while it is 0.22 for children in families in which the family head has a college degree or more. Moreover, the take-up rate for a child from a family in which a female is a single head is 0.68, while it is only 0.27 for a child from a two parent family. These findings indicate that the traditionally welfare-ineligible population has lower response to eligibility than the traditionally welfare-eligible. While this has been suspected in the literature previously, these are the first quantitative estimates of the differences in take-up across groups, and these differences are substantial. Our estimates suggest that improving take-up among the eligible requires efforts to promote public coverage primarily among populations that had not previously been eligible for coverage.

Table 3 shows the predicted take-up rates for children in the 1995 SIPP made newly eligible under a counterfactual nonmarginal policy experiment where we increase the 1995 income limits by 10%. In column (1) we make this calculation for the RCLPM estimates using (6a) and (6b), while in column (2) we make this calculation using the SPM while not accounting for unobservable differences using estimates using (16a) and (16b). The average take-up rates for all the newly eligible in columns (1) and (2) of Table 3 are much smaller than the average take-up rates for all eligible children in the respective columns in Table 2, and for the demographic groups combined, much closer to the estimate from the standard LPM approach. These results are to be expected given that the families with newly eligible children come from further up the income distribution than the previously eligible, and they might consider Medicaid less substitutable for private insurance as they might have such insurance through their employers, a possibility we investigate further below. [12]

Column (3) of Table 3 uses (19) to calculate the take-up rates in newly eligible families using the SPM when we allow selection. As expected, the estimates in column (3) are again larger than those in column (2) because of the positive estimated correlation between unobservables in eligibility and unobservables in take-up. There are also wide differences across groups of the newly eligible in their take-up response to the policy expansion, with the observably less disadvantaged children having substantially lower estimated rates of enrolling in the Medicaid program for which they are eligible. Interestingly, the average take-up rate for marginal families from the standard model is relatively close to the average take-up rates for newly eligible two earner or two parent families (see Table 3), suggesting that these latter groups may each approximate the marginal families in the standard estimates. This interpretation seems especially plausible given that the expansions focused on extending benefits to two-parent families.

[12] Moreover the newly eligible may be unaware of their eligibility for some period of time - see Ham and Shore-Sheppard (2005) for evidence on this issue.

Tables 2 and 3 indicate a significant advantage of the two approaches we explore here over the standard LPM approach. Using the latter all one can do is use the estimated average take-up rate for all (unobserved) marginal families to predict the effect of an expansion, while our approach permits us to estimate effects among observable groups of children actually affected by the expansion. Further, our estimates in column (3) of each table allow us to account for the fact that currently eligible children or those made eligible by our policy experiment will have different unobservables.

5.2 Private Coverage Parameter Estimates, Predicted Crowd-out Rates and Private Insurance Responses to a Policy Experiment

Here we repeat the analysis of Section 5.1 for private insurance coverage. In the standard model, the estimated private insurance response to eligibility is essentially zero for marginal families affected by Medicaid expansions. (Again the parameter estimates are affected by the normalization (omitted category) so we place these estimates in Tables B1 and B2 of the Online Appendix.) In the RCLPM estimates, the only statistically significant coefficients are on the eligibility main effect and on the interactions of eligibility with gender, male head and the age of the highest earner (see column (4) of Table B1 in the Online Appendix), although the number of significant coefficients will be affected by the normalization. However, when we estimate the SPM, we find that all demographic variables, except for the gender of the child, are statistically significantly related to private insurance participation both when a randomly chosen child is and is not eligible for Medicaid (see columns (3) and (4) in Table B2 in the Online Appendix). The signs of the variables are the same in the index functions for private participation when eligible and not eligible for Medicaid, indicating that the effect of demographic variables on the index functions for private insurance coverage is in the same direction when a child is eligible for Medicaid as when the child is not eligible.

In Table 4 we present our estimated crowd-out rates. We use children eligible for Medicaid in the 1995 SIPP panel to calculate these take-up rates; thus they are directly comparable to the results in Table 2. We present the private participation rates when eligible and not eligible in Table B3 of the Online Appendix to save space. However, it is worth noting i) that there is a very wide variation in predicted participation across groups when eligible and when ineligible, with the highest rates of private insurance participation for children whose families have the greatest likelihood of connection to the labor market and ii) these private insurance participation rates are precisely estimated. Column (1) shows crowd-out rates for currently eligible children (again this is an ATET) using the RCLPM estimates, which have been placed in columns (1) and (2) of Table B3 in the Online Appendix. Note that the standard LPM constrains crowd-out to be constant across demographic groups, but this constraint does not hold in the data. The estimated crowd-out rates vary from 0.03 to 0.16 (ignoring the estimated negative effects for some of the more advantaged groups) and are generally statistically significant. The estimated average crowd-out among all eligible children from the 1995 SIPP panel is a statistically significant 0.05, in contrast to the standard LPM estimate for all marginal families of essentially zero; note that there is no way to know how crowd-out affects the eligible population on average from the LPM estimate for marginal families.. This result makes sense in that the average eligible family is likely to find public insurance to be more substitutable for private insurance than will a marginally eligible family, since on average eligible families will have lower incomes, be less likely to receive employer provided coverage, have access to poorer private coverage, and face higher out of pocket costs for insurance as a fraction of their incomes than the marginal families.

Column (2) in Table 4 shows crowd-out rates for eligible children using the SPM and not accounting for unobservable heterogeneity between eligible and non-eligible children. The estimated average crowd-out rate across all eligible children is again statistically significant, but is somewhat smaller than the respective estimate for the RCLPM.[13] Also, crowd-out rates across the different groups range only from 0.004 to 0.04 (again ignoring the negative estimates); thus there is a considerably less disparity in crowd-out rates across groups than predicted by the RCLPM.

Finally, the results when we use the SPM, and allow for the fact that those eligible and those ineligible for Medicaid are nonrandom samples, are in column (3) of Table 4. The private participation rates from this model (which have been placed in columns (5) and (6) of Table B3 in the Online Appendix) show considerable variance across groups, and they again are precisely estimated. Further, as one would expect, they are lower than the respective estimates in columns (3) and (4) of Table B3, since unobservables in eligibility are likely to be negatively

[13] While we have not explored this difference in depth, we suspect that it may have to do with problems in the RCLPM when participation rates approach zero or one.

correlated with the unobservables in the index function for participation in private insurance. In other words, the Medicaid eligible population has unobservable characteristics which make them less likely to get private insurance coverage, *ceteris paribus*. However, all of the implied crowd-out rates (but one) are insignificant, suggesting that allowing for differences in unobservables is asking too much of the data when investigating crowd-out.

Finally in columns (1)-(3) of Table 5 we report our estimates of crowd-out among the newly Medicaid eligible in the 1995 SIPP data under our counterfactual expansion of the Medicaid income limits; again the participation rates among the newly eligible are presented in Table B4 of the Online Appendix. As expected, for all three estimation approaches (i.e. RCLPM, SPM not accounting for unobservables, and SPM accounting for unobservables) the predicted participation rates are smaller than those for all children and continue to be precisely estimated. For the RCLPM the crowd-out rates are generally statistically insignificant but quite small (and many of the point estimates are actually negative). For the SPM not accounting for unobservables, the crowd-out rates are estimated to be positive (as expected), though again most of the estimates are quite small and statistically insignificant. There are four groups of the newly eligible for whom crowd-out rates are statistically distinguishable from zero, although the estimates are still economically small, at 0.03—children in families where the highest earner has a high school degree or some college, children in families with a female head, and children in families with one earner Interestingly the standard errors in column (2) are quite small, indicating that for this model we are estimating relatively precise 'zero' crowd-out rates among the newly eligible; these results are not surprising considering that public insurance is likely to be less substitutable for private insurance for newly eligible families than for previously eligible families. Note that the standard LPM results indicate that crowd-out is likely to be small or zero for the average marginal families, while our results suggest this is true for all demographic groups within the marginal families. Finally, the size of the standard errors in column (3) of Table 5 suggest that we cannot obtain precise crowd-out effects for the newly eligible using the SPM while allowing for selection.

6. Conclusions

In this paper we demonstrate how two approaches—a random coefficients linear probability model (RCLPM) and a switching probit model (SPM)—can be used to estimate a variety of effects of Medicaid eligibility on take-up and private insurance crowd-out. Specifically, they can be used to estimate effects across different demographic groups for an average eligible child, a randomly chosen child, a child from a marginally eligible family, and a child made newly eligible by a counterfactual non-marginal expansion of eligibility. In our empirical work we focus on the effects on eligible children and those made newly eligible by a counterfactual policy experiment, since these are likely to be of most interest to policymakers. Neither of these effects are currently available in the literature, nor is a comparison of their estimates from the RCLPM and the SPM. The RCLPM has the advantage that it is easier to estimate, although this is not a huge advantage given the SPM is also easily estimated in Stata if one follows our approach for obtaining standard errors. The SPM has the advantage that it also allows us to control for changes in unobservables (conditional on making a distributional assumption), although we find that such a model is too rich for the data when calculating crowd-out effects among the newly eligible. Both the RCLPM and the SPM can be implemented using preexisting commands in Stata, and both produce precisely estimated effects for participation in private and public insurance. Interestingly, the RCLPM and SPM generally produce similar policy effects when we do not account for unobserved heterogeneity using the SPM.

We find that average take-up rates are much higher for eligible children than for newly eligible children. Our conclusions about take-up are similar across all three sets of estimates, with each model producing a wide range of take-up rates across demographic groups. Average take-up rates are around 0.5 for all three approaches. Take-up rates among the newly eligible following a counterfactual policy simulation are also fairly similar across estimates, ranging from 0.21 for the RCLPM and the SPM not accounting for selection to 0.27 for the SPM accounting for selection. These take-up rates are substantially larger than the average take-up rate for marginal families of 0.13 obtained from the standard LPM model. In addition, we find that participation rates for the eligible vary dramatically across groups, with observably less disadvantaged children having lower take-up of the public insurance for which they are eligible. In particular, children in families where the highest earner has a high school degree or some college have take-up rates roughly two-thirds as large as the take-up rates among children in families where the highest earner does not have a high school degree, while children in male-headed or dual-headed families have take-up rates only half as large as those among children in female-headed families. Thus our results should prove quite useful in helping policy makers identify groups that are eligible but underserved by Medicaid.

We estimate precise estimates of private insurance participation across demographic groups for the eligible and the newly eligible in our counterfactual experiment. When examining average crowd-out among all eligible children, we find somewhat less heterogeneity across demographic groups and a somewhat lower degree of similarity across estimation methods. We find the largest estimates of average crowd-out using the RCLPM, with estimates ranging from 0 to 0.16 and an overall average of 0.05. (More disadvantaged groups show evidence of higher crowd-out, as well as higher take-up.) The estimates from either version of the SPM range from 0 to 0.05, with larger standard errors in the model accounting for selection. We find relatively precise 'zero' effects of eligibility on crowd-out among newly eligible children on average and across demographic groups using the SPM without selection, but not using the RCLPM or the SPM with selection.

Overall, our results provide important information for policymakers about differences in response to eligibility across observable demographic groups, as well as information about how responses to eligibility differ between individuals who were previously eligible and those made newly eligible by a counterfactual Medicaid expansion. For example, while crowd-out may be an issue for many demographic groups among the eligible, there are (precisely estimated) negligible effects for those affected by nonmarginal expansions, presumably because Medicaid is less of a substitute for private insurance for the newly eligible. This suggests that crowd-out does not need to be considered in any demographic groups when considering Medicaid expansions. While we have demonstrated the usefulness of our approaches in the context of the Medicaid program, they can also be used in evaluating the effects of many other social programs, such as Food Stamps.

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Table 1: Means of the Variables Used in Estimation

SIPP Panel	1986	1987	1988	1990	1991	1992	1993
Medicaid	0.12	0.12	0.11	0.16	0.17	0.18	0.20
Private Coverage	0.73	0.75	0.75	0.71	0.73	0.71	0.69
Imputed Eligibility	0.19	0.18	0.18	0.27	0.29	0.31	0.34
Size of Health Insurance Unit	4.22	4.16	4.18	4.16	4.22	4.17	4.22
White	0.83	0.83	0.82	0.78	0.82	0.80	0.81
Male	0.51	0.51	0.51	0.51	0.51	0.52	0.51
Two Parents	0.76	0.77	0.77	0.71	0.74	0.73	0.73
Male Head Only	0.02	0.02	0.02	0.03	0.03	0.03	0.02
No Earners	0.14	0.13	0.13	0.16	0.15	0.16	0.16
One Earner	0.41	0.42	0.42	0.43	0.42	0.41	0.41
Two Earners	0.38	0.39	0.40	0.37	0.38	0.38	0.38
Age of Highest Earner	36.49	36.47	36.49	36.67	36.90	36.89	37.10
Education of Highest Earner	12.68	12.69	12.84	12.74	12.92	12.95	12.95
FRACELIG	0.20	0.19	0.19	0.28	0.30	0.32	0.34
Years Covered	86-88	87-89	88-89	90-92	91-93	92-95	93-95
Number of Observations	175709	182307	163165	397187	267589	433545	406833

Notes: Shown are unweighted means from the respective SIPP panels noted above. See the text for a description of the sample construction.

Table 2
Estimated Medicaid Average Take-up Rates for Eligible Children
From the Random Coefficient Linear Probability Model and the Switching Probit Model

Group	RCLPM (1)	SPM Selection Ignored (2)	SPM Selection Accounted for (3)
All Population	0.47***	0.47***	0.51***
	(0.03)	(0.01)	(0.02)
Race			
White	0.41***	0.40***	0.44***
	(0.03)	(0.01)	(0.02)
Non-White	0.62***	0.64***	0.68***
	(0.03)	(0.01)	(0.02)
Education of Highest Earner			
High School Drop-out	0.61***	0.60***	0.62***
	(0.04)	(0.01)	(0.02)
High School Graduate	0.44***	0.44***	0.48***
	(0.03)	(0.01)	(0.02)
Some College	0.38***	0.40***	0.44***
	(0.03)	(0.01)	(0.02)
College Graduate	0.19***	0.22***	0.27***
	(0.03)	(0.01)	(0.01)
Family Structure			
Female Head	0.68***	0.69***	0.72***
	(0.04)	(0.01)	(0.02)
Male Head	0.31***	0.28***	0.32***
	(0.04)	(0.02)	(0.03)
Two parents	0.27***	0.26***	0.30***
	(0.03)	(0.01)	(0.02)
Number of Earners			
No earner	0.78***	0.77***	0.79***
	(0.04)	(0.005)	(0.02)
One earner	0.28***	0.28***	0.32***
	(0.03)	(0.01)	(0.03)
Two earners	0.09**	0.12***	0.18***
	(0.04)	(0.01)	(0.02)
More than two earners	-0.03	0.08***	0.12***
	(0.05)	(0.01)	(0.02)

Notes: All regressions/index functions include demographic main effects, year, age, state dummies, eligibility-year, eligibility-age and eligibility-state interactions and a fourth month dummy. Standard errors have been corrected for repeated observations across the same children. Estimates are based on coefficients from all SIPP panels and data on Medicaid eligible children in the 1995 SIPP panel.

*** denotes significantly different from zero at the 1% level of significance; ** denotes significantly different from zero at the 5% level of significance; * denotes significantly different from zero at the 10% level of significance.

Table 3
Counterfactual Policy Analysis: Estimated Medicaid Take-up Rates for the Newly Eligible after Raising the 1995 Income Limits by 10%

Group	RCLPM (1)	SPM Selection Ignored (2)	SPM Selection Accounted for (3)
All Newly Eligible Population	0.21***	0.21***	0.27***
	(0.05)	(0.01)	(0.01)
Race			
White	0.18***	0.18***	0.24***
	(0.05)	(0.01)	(0.01)
Non-White	0.31***	0.32***	0.38***
	(0.05)	(0.01)	(0.01)
Education of Highest Earner			
High School Drop-out	0.30***	0.28***	0.33***
	(0.05)	(0.01)	(0.01)
High School Graduate	0.21***	0.21***	0.26***
	(0.05)	(0.01)	(0.01)
Some College	0.20***	0.21***	0.27***
	(0.05)	(0.01)	(0.01)
College Graduate	0.07	0.11***	0.17***
	(0.06)	(0.01)	(0.01)
Family Structure			
Female Head	0.38***	0.39***	0.46***
	(0.05)	(0.01)	(0.01)
Male Head	0.15***	0.16***	0.21***
	(0.06)	(0.02)	(0.02)
Two parents	0.15***	0.15***	0.20***
	(0.05)	(0.01)	(0.01)
Number of Earners			
No earner	0.63***	0.63***	0.66***
	(0.05)	(0.01)	(0.01)
One earner	0.23***	0.22***	0.28***
	(0.05)	(0.01)	(0.01)
Two earners	0.09	0.11***	0.17***
	(0.06)	(0.01)	(0.01)
More than two earners	-0.07	0.06***	0.11***
	(0.07)	(0.01)	(0.02)

Notes: See notes to Table 2. Estimates are based on coefficients from all SIPP panels and the data on those made newly eligible by the increase in the Medicaid income limits in the 1995 SIPP panel.

Table 4
Estimated Crowd-out Rates for Eligible Children

Group	RCLPM (1)	SPM Selection Ignored (2)	SPM Selection Accounted for (3)
All Population	0.05**	0.02*	0.05
	(0.02)	(0.01)	(0.03)
Race			
White	0.05**	0.01	0.05
	(0.02)	(0.01)	(0.04)
Non-White	0.06*	0.02*	0.04
	(0.04)	(0.01)	(0.03)
Education of Highest Earner			
High School Drop-out	0.08**	0.005*	0.02
	(0.03)	(0.01)	(0.03)
High School Graduate	0.05**	0.03***	0.06
	(0.02)	(0.01)	(0.04)
Some College	0.03	0.04***	0.07
	(0.02)	(0.01)	(0.04)
College Graduate	-0.03	0.02	0.06
	(0.08)	(0.02)	(0.05)
Family Structure			
Female Head	0.10**	0.04***	0.05**
	(0.04)	(0.01)	(0.03)
Male Head	0.16**	0.03	0.06
	(0.04)	(0.02)	(0.05)
Two parents	0.005	0.004	0.04
	(0.02)	(0.01)	(0.04)
Number of Earners			
No earner	0.11**	0.03*	0.03
	(0.05)	(0.01)	(0.02)
One earner	0.03**	0.03*	0.06
	(0.02)	(0.01)	(0.05)
Two earners	-0.08**	-0.002	0.04
	(0.04)	(0.01)	(0.05)
More than two earners	-0.10	-0.03	-0.01
	(0.07)	(0.02)	(0.06)

Notes: See notes to Table 2. Recall that the SPM estimates separate private insurance participation equations when eligible for Medicaid and when ineligible. Thus, estimates in columns (2) and (3) are calculated using coefficients from columns (3) and (4) of Table B2 in the Online Appendix, respectively.

Table 5
Counterfactual Policy Analysis: Crowd-out Rates for the Newly Eligible Population after Raising Medicaid Income Limits by 10%

Group	RCLPM (1)	SPM Selection Ignored (2)	SPM Selection Accounted for (3)
All Population	-0.03	0.02	0.07
	(0.05)	(0.01)	(0.06)
Race			
White	-0.03	0.02	0.07
	(0.05)	(0.01)	(0.06)
Non-White	-0.04	0.01	0.06
	(0.05)	(0.02)	(0.06)
Education of Highest Earner			
High School Drop-out	-0.02	-0.02	0.01
	(0.05)	(0.02)	(0.05)
High School Graduate	-0.02	0.03*	0.07
	(0.05)	(0.02)	(0.06)
Some College	-0.03	0.03**	0.09
	(0.05)	(0.01)	(0.06)
College Graduate	-0.10	0.02	0.09
	(0.05)	(0.01)	(0.06)
Family Structure			
Female Head	0.01	0.03*	0.09
	(0.05)	(0.02)	(0.05)
Male Head	0.10	0.02	0.07
	(0.07)	(0.03)	(0.07)
Two parents	-0.06	0.01	0.06
	(0.05)	(0.01)	(0.06)
Number of Earners			
No earner	0.01	0.00	0.02
	(0.07)	(0.02)	(0.03)
One earner	-0.01	0.03*	0.08
	(0.05)	(0.01)	(0.06)
Two earners	-0.09	0.01	0.06
	(0.06)	(0.01)	(0.06)
More than two earners	-0.13	-0.01	0.03
	(0.09)	(0.02)	(0.07)

Notes: See notes to Tables 3 and 4.