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The Effect of Medicaid Expansions for Low-Income Children on Medicaid Participation and Private Insurance Coverage: Evidence from the SIPP

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Abstract

We examine Medicaid enrollment and private coverage loss following expansions of Medicaid eligibility. We attempt to replicate Cutler and Gruber's (1996) results using the Survey of Income and Program Participation, and find smaller rates of take-up and little evidence of crowding out. We find that some of the difference in results can be attributed to different samples and recall periods in the data sets used. Extending the previous literature, we find that take-up is slightly increased if a child's siblings are eligible and with time spent eligible. Focusing on children whose eligibility status changes during the sample, we estimate smaller take-up effects. We find little evidence of crowding out in any of our extensions.

1. Introduction

In recent years, public commitment to health insurance coverage for children has increased dramatically. Beginning in the mid-1980s, a series of federal laws uncoupled Medicaid eligibility from eligibility for cash assistance (then Aid to Families with Dependent Children, or AFDC), substantially expanding the population eligible for Medicaid. The expansions raised the child eligibility threshold from the AFDC level to at least 100 percent of the poverty line and possibly higher, depending on the age of the child. These expansions in public health insurance for children have led to two potentially contradictory concerns for public policy. On the one hand, the availability of public insurance may lead families to enroll their children in Medicaid rather than obtaining private coverage ("crowding out"). This may occur if the cost of public insurance for an eligible child is less for the family than the cost of employer-sponsored health insurance, or if employers change their dependent health insurance provisions in response to the expansions. On the other hand, research has found that many Medicaid-eligible children still do not have health insurance, with most of these children being eligible under Medicaid expansion programs (Selden, Banthin, and Cohen 1998). While lack of health insurance may not seem to be an important issue when children who need care can receive it in emergency room settings, research has shown that children who do not have health insurance often do not get preventive care (see, for example, Marquis and Long 1994, Currie and Gruber 1996, and McNeil 1995).

The question of whether crowding out occurred as a result of the expansions has received substantial attention from economists, and this literature has influenced public policy. Lawmakers wrote explicit anti-crowd-out provisions into the law creating the new State Children's Health Insurance Program (SCHIP), an action which can plausibly be attributed to the attention drawn to the issue by economists. The paper by Cutler and Gruber (1996) has been particularly influential, since it was the first to be published and since it shows evidence of a substantial negative relationship between eligibility for Medicaid and private coverage. The question of the extent of crowding out has been controversial, however, with the literature producing a range of estimates from considerable (49 percent of new Medicaid enrollees came from private insurance) to negligible (2 percent).

In this paper we use panel data from the Survey of Income and Program Participation (SIPP) to revisit the issue of crowding out, while also examining the question of Medicaid take-up behavior. The SIPP offers several advantages for studying Medicaid participation and private insurance coverage. First, data collection occurs three times per year, rather than annually, as in many data sets. Second, the survey was designed to collect income and program participation information and thus provides more detailed data on these variables. Third, the panel nature of the data allows us to examine whether the response to eligibility varies with time and to relax some of the assumptions made in the previous literature by estimating fixed effect and lagged dependent variable models.

Our goals in this paper are twofold. Our first goal is to attempt to replicate, in the SIPP, the results obtained by Cutler and Gruber (1996) using data from the Current Population Survey (CPS) and to examine possible reasons for the differences in the results across the two data sets. Our second goal is to extend the previous literature on Medicaid take-up and crowding out in several directions. First, we examine the impact of having Medicaid-eligible siblings on public and private coverage. Second, we allow the effects of eligibility to differ with time spent eligible. Third, we examine the effect of eligibility on the response of children in marginal families, i.e. children whose eligibility changes over the sample period. Fourth, we estimate simple dynamic models which allow the short-run and long-run effects of eligibility to differ.

Our results from the SIPP using the method of Cutler and Gruber (1996) differ from those obtained from the CPS, particularly in showing little evidence of crowding out. While the difference in the estimated Medicaid take-up coefficients appears to be due to the omission of small states in the SIPP, the source of differences in private coverage results is less clear. At least some, though not all, of the difference appears to be due to the annual nature of the CPS data collection versus the tri-annual interviews of the SIPP.

Our results from the extensions of previous work on the Medicaid expansions lead to four main conclusions. First, while previous researchers have found relatively weak take-up responses to the expansions, our results indicate that the effect of the expansions on the enrollment of children may be even smaller than previously suspected. Second, take-up of Medicaid is increased slightly if a larger fraction of a child's siblings are eligible. Third, we find that the longer a child has been eligible for Medicaid, the more likely he or she is to be enrolled in Medicaid. Finally, the immediate impact of eligibility on take-up estimated using a lagged dependent variable model is smaller than static models indicate, while the long-run impact is larger. In addition, the dynamic model provides some of the only evidence of crowding out in the SIPP, showing a negative (though statistically insignificant) relationship between eligibility and private coverage.

2. Background

2.1. Expansions in Public Health Insurance

Medicaid is a joint state-federal program financed by state contributions and federal matching funds.[1] Eligibility for the program is limited to essentially three low-income groups: the aged, the disabled, and families with dependent children. Members of the third group were the main focus of the legislative changes, and in this paper we concentrate exclusively on them. Historically, this group was comprised of families receiving cash assistance through the AFDC program. Thus, Medicaid eligibility and participation were directly linked to the eligibility standards for AFDC.

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[1]See U.S. Committee on Ways and Means (1984-1993), Congressional Research Service (1988, 1993), Health Care Financing Administration (1988, 1990), and National Governors' Association Center for Policy Research (1988-1996) for more detailed descriptions of the Medicaid program and the expansions.

Generally, to qualify for AFDC a family must have had either a single parent or an unemployed primary earner. The family's income and resources also had to be less than state-established standards, most of which were well below the federal poverty line.

Starting in the mid-1980s, a series of federal law changes substantially diminished the link between Medicaid eligibility and AFDC eligibility by relaxing the restrictions on two-parent families and those with earned income, extending Medicaid coverage to families with incomes above the AFDC thresholds.[2] Beginning with the Omnibus Budget Reconciliation Acts (OBRA) of 1986 and 1987, Congress gave states the authority to raise the income limits for Medicaid coverage of certain groups (such as infants and very young children) above the AFDC level. Congressionally mandated increases in state eligibility limits followed, most notably with the passage of OBRA 1989 and OBRA 1990. OBRA 1989 required coverage of pregnant women and children up to age 6 with family incomes up to 133 percent of the federal poverty level, and OBRA 1990 required states to cover children born after September 30, 1983 with family incomes below 100 percent of the federal poverty level. Further expansions (within certain guidelines for age and family income) were permitted at state option. In total, the expansions raised the eligibility threshold from the AFDC level to at least 100 percent of the poverty line and possibly higher, depending on age and state of residence. Age plays a role because eligibility standards for younger children were generally less restrictive, while state of residence is important because states had the option of exceeding the federal minimum eligibility limits.

2.2. Trends in Health Insurance Coverage

Between 1986 and 1993, health insurance coverage among children changed substantially.

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[2]Prior to the expansions studied here, there had been minor expansions in Medicaid eligibility (such as the Ribicoff program) which allowed states (at their option) to cover children or pregnant women who met AFDC income standards but did not qualify due to family structure. The Deficit Reduction Act of 1984 began the process of expanding eligibility by requiring states to cover children who lived in families that were income-eligible for AFDC, regardless of family structure.

At the beginning of the period Medicaid coverage was essentially constant (covering approximately 11 percent of children nationally each month, according to weighted estimates from the SIPP), as was private coverage (covering approximately 73 percent). Around 1990, levels of private coverage began to fall and Medicaid coverage began to rise. By 1993 Medicaid covered approximately 19 percent of children, and approximately 69 percent had private coverage. Since the Medicaid expansions began to take effect in the early 1990s, it is plausible that the fall in private coverage was linked to the rise in Medicaid through crowding out. However the economy was in a recession during this period, so another (not mutually exclusive) explanation is that the rise in Medicaid was linked to the fall in private coverage through job losses and other reductions in the availability of employer-sponsored dependent health insurance.

2.3. Previous Literature

A number of studies have examined the impact of the Medicaid expansions on insurance coverage, focusing primarily on whether and to what extent crowding out occurred. In these studies the degree of crowd-out is usually measured as the percent of new Medicaid enrollment estimated to come from private coverage. "New Medicaid enrollment" is defined differently by different studies–either as the entire increase in Medicaid enrollment over a time period, or as the increase in enrollment directly attributable to the expansions. It is important to note that neither of these measures of crowding out are the same as the share of the decline in private coverage attributable to the Medicaid expansions. While this distinction is often lost in public policy discussions, differences in these measures can be substantial, as they share the same numerator but have denominators that can differ widely. For example, worsening economic conditions and increasing health insurance coverage costs occurring concurrently with Medicaid expansions may result in a large loss in private coverage of which expansions in public coverage explain relatively little.

Because other factors in the economy affecting insurance coverage could have changed at the same time as the expansions, research on crowding out has used various empirical strategies to disentangle the effect of the Medicaid expansions from other factors. All of these empirical strategies utilize the form or timing of the expansions and the fact that some groups were affected by the expansions while others were not to identify the Medicaid effect.[3]

The largest estimate of substitution between private and public insurance comes from Cutler and Gruber (1996). Using March Current Population Survey (CPS) data on children from 1988 to 1993, they use two-stage least squares to estimate the effect of imputed Medicaid eligibility on insurance status (Medicaid, private, or uninsured), controlling for demographics and state and year effects. Medicaid eligibility is likely to be endogenous since parental wages (determining eligibility) are likely to be correlated with benefits (including whether private insurance is available to the family), and benefits are unobserved and thus part of the error term. To address the endogeneity problem, they create an instrument which uses the exogenous variation in the Medicaid expansions by year, state, and by age within state, since variation in the expansions is correlated with a child's eligibility but is not otherwise correlated with the availability of private coverage or the demand for insurance.[4] They estimate that a ten percentage-point increase in Medicaid eligibility increased Medicaid coverage by 2.35 percentage points and reduced private coverage by 0.74 percentage points. They measure crowding out as the ratio of these two coefficients, which implies that 31 percent of the rise in Medicaid coverage due to the expansions came from private coverage.[5]

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[3]Several of the summarized papers and a few additional papers have examined the expansions for pregnant women as well as children. We focus only on the children's results, as they are the most relevant for our research.

[4]We discuss their instrument in greater detail in Section III, where we attempt to replicate their results.

[5]When they use an alternative specification in which they attempt to account for the approximate percentage of a family's medical spending covered by Medicaid, the estimate increases to the 49 percent figure often cited in discussion of their work.

Dubay and Kenney (1996) use data from the March 1989 and 1994 CPS. They compare the change in the fraction of children with private coverage in various income groups to the change in the fraction of men with private coverage in those income groups, as men were not directly affected by the Medicaid expansion legislation. However, men do not provide an ideal comparison group. First, reported Medicaid coverage for men in the CPS did rise over this period. Second, to the extent that men dropped coverage when their wives or children gained Medicaid, using men as a comparison group will understate the impact of the expansions on private coverage. Third, this comparison assumes that changes in the availability of private coverage over this period were similar for men as for children. If men were less likely to lose their private coverage, this measure will overstate the degree of crowding out.

Dubay and Kenney estimate that there was an excess decline in private coverage (relative to that of men) of 1 percentage point among poor children (those with family incomes below 100 percent of poverty) and 6 percentage points among near-poor children (those with family incomes between 100 and 133 percent of poverty). They calculate the extent of crowding out by dividing the estimated excess declines among poor and near-poor children by the total increase in Medicaid coverage for these groups (10 and 27 percentage points, respectively), without netting out the increase in coverage for men. Their estimates of crowding out are 10 percent for poor children and 22 percent for near-poor children. Due to the difference in the denominators used in the two studies (new Medicaid enrollment attributable to the expansions in Cutler and Gruber's study, and all new Medicaid enrollment in Dubay and Kenney's study), these crowd-out estimates are not directly comparable.

In two papers, Shore-Sheppard (1997, 2000) also uses data from the CPS. Aggregating the individual data to the state-age-income quartile level for 1988, 1993, and 1996, Shore-Sheppard (1997) regresses the change in coverage rates (private or Medicaid) on the change in eligibility rates for these cells, treating eligibility as endogenous. (The change in eligibility incorporates not only the changes in the Medicaid laws but also changes in population characteristics.) Shore-Sheppard uses the difference between the fraction of each cell eligible in 1988 and the fraction of that cell that would be eligible under the expanded rules as an instrument. Thus she uses variation in the impact of the legislation by state, age, and income to identify the effect of the expansions. Her estimate of the percent of children newly eligible through the expansions who came from private coverage, calculated as a ratio of the coefficients from the private and Medicaid regressions, is 15 percent for 1988-1993 and 41 percent for 1988-1996. This empirical strategy differs from that of Cutler and Gruber in using only the first and last years of the relevant period and not the year-to-year changes in eligibility and coverage. This use of long differences may eliminate some fluctuations resulting from short-run adjustment effects, but it has the disadvantage of not using all of the possible variation, and the magnitude of the estimate is dependent on the endpoints chosen.

Shore-Sheppard (2000) uses 1988, 1989, 1994, and 1995 CPS data to conduct a cell-level analysis similar to that described above, but using region-income decile cells instead of age-state-income quartiles. To attempt to control for the possibility that shocks to coverage may be correlated with the expansions (if region-decile cells that were strongly affected by the expansions were also particularly affected by the recession, for example), she uses single men ages 20 to 45 as a comparison group. Although it is possible that the expansions affected single men if crowding out occurred through employer actions, thus far there is no evidence that employers responded to the expansions by reducing offers of employee coverage (Cutler and Gruber 1996, Shore-Sheppard, Buchmueller, and Jensen 2000). The estimates of crowding out in this study are measured as the ratio of the private coverage coefficient to the Medicaid coverage coefficient (as in Cutler and Gruber (1996)), and range from 7.6 percent when single men are used as a control group, to 37.4 percent when no control group is used. Unfortunately, the standard errors on these estimates are relatively high, so the confidence intervals contain most prior estimates.[6]

The panel data approach of Blumberg, Dubay, and Norton (2000) encounters a similar problem of imprecise estimates. Using data from the 1990 SIPP, Blumberg, Dubay, and Norton examine whether low-income children whose age made them eligible for a Medicaid expansion over the course of the panel were more likely than older low-income children to lose private coverage between the first and last interviews of the SIPP panel. They also examine whether younger low-income children who were uninsured at the beginning of the panel were less likely than older low-income children to gain private coverage. They estimate linear probability models of the probability that a child with private coverage at the first interview had private, Medicaid, or no insurance at the last interview, and similar models for children with no coverage at the first interview. Using the ratio of their coefficients from these models, they estimate the extent of substitution of public for private coverage to be 23 percent for children who already had private coverage and 0 percent for children who began the panel uninsured. However, these estimates are calculated using statistically insignificant regression coefficients, and thus are also likely to be quite imprecise.

Yazici and Kaestner (2000) use data from the National Longitudinal Survey of Youth (NLSY) to compare changes in public and private coverage rates between 1988 and 1992 for children who became eligible and those who did not, distinguishing between eligibility onset based on income loss and eligibility onset due to the expansions. They use a difference-in-differences methodology, with children who were eligible in both years, children who gained eligibility in the second year and did not experience a reduction in family income, and children who gained eligibility but did experience a reduction in family income as the "treatment groups", and children who were

[6]As no other study reports standard errors on the crowding out estimates, it is not possible to say whether the measures of crowding out from the various studies are significantly different from one another.

never eligible and either did or did not experience a reduction in family income as the "comparison groups." Their estimates of the percent of Medicaid enrollment that came from private insurance range from 5 percent to 37 percent, depending on which treatment and comparison group is used. However the study design does not account for the possible endogeneity of selection into the comparison group, i.e. the endogeneity of income which several studies discussed above address. In addition, since children who are never eligible have higher income than children who are eligible, they may be subject to different trends in private coverage.

Finally, Thorpe and Florence (1998) use the NLSY to estimate the fraction of children newly enrolled in Medicaid in a year who had private coverage in the previous year. They measure crowding out as the fraction of children who move from private coverage to Medicaid but whose parents retain private coverage. Using this measure, they find that between 2 and 23 percent of previously privately insured children who enrolled in Medicaid had parents who retained private coverage, depending on the year considered and the income level of the family. This may be an underestimate of crowding out, however, as it is possible that parents may drop their own coverage when their child enrolls in Medicaid; in fact, Cutler and Gruber (1996) find some evidence of this.[7]

3. Data

The data used in the empirical analysis are from the Surveys of Income and Program Participation (SIPP). Individuals in the SIPP are interviewed every four months about employment and program participation during the previous four months (each four-month period is a "wave").

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[7] Since the NLSY is composed of one cohort of mothers who are aging over the time period of the expansions, trends in insurance coverage for children in the NLSY are different from the trends in the general population. Insurance coverage rates among children in the NLSY increased over this time period, while insurance coverage in the population of children more generally was declining. Consequently, estimated effects of the expansions from the NLSY may not be generalizable to the entire population of children.

The lengths of the panels vary from 24 months for the 1988 panel to 40 months for the 1992 panel. A new panel is introduced each year, which yields more than one panel with data covering a particular point in time. We use the 1986, 1987, 1988, 1990, 1991, 1992, and 1993 panels, which cover the period from October 1985 to August 1995, the period most relevant for the Medicaid expansions (the 1989 panel is not used because it was ended after only three waves). Although the sample universe is the entire United States, the Census Bureau does not separately identify state of residence for residents of nine low-population states. Since we need information on state of residence to impute Medicaid eligibility, our analysis sample includes only children whose state of residence is identified. We also restrict our sample to children living in original sample households (that is, households interviewed in the first wave) who are younger than 16 years old at the first time they are observed. We drop children who are observed only once (<1 percent of the sample), children who leave the sample and then return (<3 percent of the sample) and children who move between states during the sample period (approximately 4 percent of the sample).[8]

Although the four-month recall period increases the probability of accurate reporting, particularly relative to the fifteen-month recall period of the March Current Population Surveys,[9] the SIPP suffers from the problem of "seam bias." Census Bureau researchers have shown that there are a disproportionate number of transitions between the last month of a wave and the first month of the next wave (see, e.g., Young 1989, Marquis and Moore 1990). Because of this seam bias problem,

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[8]Children with breaks in their data are dropped because their insurance status while out of the sample is unknown, and this creates difficulties in the dynamic models. We drop children who move between states during the sample because the relatively small number of such children made estimating fixed effects models with state dummies included difficult. Results from models without fixed effects including these children in the sample are essentially the same as those reported here.

[9]Bennefield (1996) finds that health insurance coverage in the early 1990s is measured more accurately in the SIPP than in the CPS, due in part to the shorter recall period.

we estimate our models using only the fourth month of each wave (dropping the first three months).[10] While this approach has the disadvantage that information on the timing of transitions reported to occur between months other than at the seam is lost, the advantage is that the data in the fourth month of each wave are the most likely to be accurate since it is closest to the time of interview.

In Table 1 we present the sample means for the variables used in our regressions.[11] The insurance variables are private insurance and Medicaid, where we define private coverage to include CHAMPUS (military) coverage. A child may report both private and public coverage, although this is relatively uncommon (only 1.8 percent of the total months). Consistent with national trends, Medicaid coverage is higher in our sample in later panels, while private coverage is lower.

Imputation of eligibility is done in four steps. First, we construct the family unit relevant for private insurance and Medicaid program participation–the "health insurance unit"–that is, the head, spouse, and any minor children (or older children who are full-time students) and determine family income. Second, we assign family-specific poverty thresholds based on the size of the family and the year. Since Medicaid eligibility results from AFDC eligibility, 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.[12] Finally, we assign Medicaid eligibility if any of the following

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[10]We estimated all of our models using the monthly data, and found that the results were overall quite similar, with no consistent pattern in the differences between coefficients. It was not the case that using waves or months produced consistently larger or smaller coefficients, for example.

[11]These sample means have not been weighted, so they should not be considered to be representative of the nation.

[12]Families must pass two income tests to receive AFDC, the "gross test", which requires that a family's gross income be less than 1.85 times the state's need standard, and the "net test", which requires that a family's income after disregards be less than the state's payment standard. In determining AFDC eligibility, families are permitted to disregard actual child care expenses up to a maximum. Since we do not know actual child care expenses, we assume that families can deduct the full disregard for all children under age 6, and no disregard for older children. This assumption overstates the amount of the disregard for families that use informal or low cost care.

conditions hold: the child is in an AFDC-eligible family; the child is income-eligible for AFDC and either lives in a state with a "Ribicoff program" 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 addition to the imputed eligibility variable (ELIG), in some specifications we use a measure of the fraction of a child's siblings who are eligible. While the mean of this variable is fairly constant around 0.15 in the first few panels, it is over a quarter in the later panels. In the dynamic specifications we use a variable coded one if an expansion affecting that age child has been passed (AGEELIG) as an additional instrument. The age-eligible variable starts the sample period below the imputed eligibility variable (0.3 percent of person-waves in the 1986 SIPP have AGEELIG=1 while 18.7 percent have ELIG=1) but rises quickly, and by the end of the sample period over three-quarters of person-waves have AGEELIG=1. Characteristics of the child and the family are also included in the regressions.[13]

4. Static Models of Insurance Coverage in SIPP

4.1. Replication of the Cutler and Gruber Results

Using data from the CPS, Cutler and Gruber estimate a static model of the effect of Medicaid eligibility on insurance coverage choice. Using a linear probability model and two-stage methods, they estimate the equations

$$I_{kit} = Z_{kit}\gamma_k + v_{ki} + \varepsilon_{kit}, \quad k=p,s. \tag{1}$$

where I_{kit} is the insurance type–p denotes private and s denotes Medicaid insurance.[14] The vector Z_{kit}

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[13]As can be seen from the fraction white and the fraction in two-parent families, the 1990 panel had a low-income oversample.

[14]Since we have panel data, we incorporate a subscript t denoting the wave.

contains the child's imputed eligibility status (the variable ELIG) and various characteristics of the child and the family that are expected to affect insurance coverage (age, sex, and race, family size and composition). Year and state dummies are also included in Z_{kit} to pick up unobserved differences over time and across states such as the effects of macroeconomic shocks, differences in the cost of private insurance, and the difficulty of the enrollment process for public insurance.

As Cutler and Gruber note, ELIG is likely to be endogenous. There are several reasons for this endogeneity: because parental wages and benefits such as health insurance are likely to be correlated (for example, low-skill household heads may both receive low wages and be less likely to be offered dependent health insurance coverage); because eligibility is a function of (potentially unobserved) individual and family characteristics that may be correlated with the demand for insurance; because a transitory shock such as a job loss affects both eligibility and coverage; and because it may proxy family income if income is not included as a regressor, perhaps because it too is likely to be endogenous. The Medicaid expansions provide a source of exogenous variation in eligibility, since children of different ages and in different states are made eligible while others remain ineligible. For example, at the end of 1991 the mandatory rules meant that a child younger than 6 years old would be eligible if his or her family income was less than 133 percent of the poverty line, children between ages 7 and 9 would be eligible if their family incomes were less than 100 percent of the poverty line, and older children had to have family incomes that met AFDC eligibility criteria. In addition, there were state-implemented rules that expanded the income limits further for some children.

To take advantage of this variation, Cutler and Gruber create an instrument for eligibility by drawing a random sample from the CPS, imputing eligibility to the sample according to the rules in each state, and calculating the fraction eligible of each state-year-age cell. This instrument, which is essentially an index of the expansiveness of Medicaid eligibility for each age group in each state and year, varies only with the legislative environment towards Medicaid for that state-year-age group and is thus uncorrelated with the error in (1), assuming that changes in a state's Medicaid eligibility standards are not correlated with changes in the availability or cost of private insurance in the state or changes in state macroeconomic conditions.[15]

In estimating Cutler and Gruber's model in the SIPP, we change their instrument slightly. Rather than drawing a random sample of children of each age from the SIPP, to calculate our instrument ($FRACELIG_{it}$) we use all SIPP observations of children of a given age in a wave except for those from the state for which the simulation is being performed. This "leave-out" sample will produce an instrument that is free from the potential bias arising from using an average incorporating the individual for whom the average serves as an instrument. In addition, using a larger sample in the calculation of FRACELIG should yield an instrument that is less noisy and presumably more powerful.[16] As with Cutler and Gruber's version of the instrument, FRACELIG does not depend on any individual or family characteristics except age, state of residence, and time. Thus by using FRACELIG as our instrument, we use only the variation in state rules, time, and age-eligibility to identify our models.

Following Cutler and Gruber we use a linear probability model in our estimation.[17] The

[15] State dummy variables are included in each regression and thus correlation between the levels of Medicaid generosity and the cost of private insurance in the state is not a problem.

[16] While this calculation of FRACELIG is theoretically superior to the version using a random sample, in practice FRACELIG is only affected at the second or third decimal place, and the results are essentially the same as when a random sample is used in its construction.

[17] We use the linear probability model since it corresponds to the approach used by Cutler and Gruber and because it makes the models in Table 6 and following much easier to estimate. While Angrist (2001) has argued that this model has some advantages in addition to computational simplicity, one problem with the linear probability model is that predicted values can lie outside the unit interval. When we checked this issue for the CPS replication, only 4% of the private enrollment equation predicted values had this problem. On the other hand, 22% of the predicted values for the Medicaid enrollment equation were not between zero and one. To

results for our SIPP sample are presented in Table 2 (standard errors are corrected for the use of repeated observations within individuals). Looking first at the Medicaid participation equation, the coefficients on the individual and family demographic variables enter as expected. Children who are white, have two parents or only a male head (relative to being in a female-headed family), smaller families, or who have at least one earner in their family are significantly less likely to be enrolled in Medicaid. The eligibility variable is positive and significant, as expected, and implies that the take-up rate among newly eligible children is 11.8 percent. The estimate is smaller than the corresponding coefficient in either the 1988-1993 CPS data, which is 0.235 (Cutler and Gruber 1996, p. 408) or the coefficient in the 1988-1996 CPS data, which is 0.197 (Shore-Sheppard 1997). The difference between the SIPP and CPS estimates is statistically significant. We explore possible reasons for this difference below.

The private insurance equation results are in the second column of Table 2. Again the demographic and family variables have the signs expected—generally the opposite of the signs in the Medicaid regression with the exception of the variables for the number of earners in the family, which indicate that the more earners a family has, the more likely the children are to have private coverage. The coefficient on eligible is statistically insignificant and is positive, unlike the estimate from the CPS, which showed a statistically significant negative relationship between eligibility and private coverage (−0.074 in 1988-1993 (Cutler and Gruber 1996) or −0.091 in 1988-1996 (Shore-Sheppard 1997)). Again the difference in the coefficients is statistically significant.

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address this, we estimated the Medicaid enrollment and eligibility equations jointly under the assumption of bivariate normality. We obtained a coefficient on eligibility of 0.431 with a standard error of 0.044. (We corrected the standard errors for the fact that we have panel data.) The estimated treatment effect is 0.070 at the means of the explanatory variables, which is reasonably close to the linear probability model coefficient of 0.115. Given this result, the previous literature and the more demanding models we estimate below, we use the linear probability model in the remainder of the paper.

4.2 *Exploration of the Differences Between CPS and SIPP Results*

There are several possible reasons why the CPS and SIPP results differ. First, the CPS identifies all states while the SIPP does not, so individuals living in the smallest states are not in our SIPP sample. To check the importance of this explanation, we estimated the model using the CPS data and omitting states and ages not represented in the SIPP sample (children older than 15). This yielded estimates of the coefficient on eligibility in the CPS of 0.136 (0.013) for the Medicaid equation and -0.070 (0.016) for the private equation. The Medicaid coefficient is thus quite close in the CPS and SIPP when equivalent samples are compared, and the difference is no longer statistically significant. The private coefficient remains statistically different in the two data sets, however.

Another possible reason for the remaining difference in private coefficients is the composition of the SIPP sample: if through attrition the SIPP sample has become selected in some way, the results may not be comparable to the CPS results. We explore this issue by running the models using only data from the first year of each panel, since such data should suffer less from attrition. We find that the estimates of the effect of eligibility are similar although somewhat smaller in absolute value than when the whole sample is used, indicating that the difference does not appear to be due to attrition in the SIPP.

A third possible explanation for the CPS-SIPP discrepancy is that it arises from the different methods of data collection in the CPS and SIPP. One primary difference between the CPS and SIPP is the reference period of each survey: annual for the CPS, and monthly for the SIPP. In order to explore the impact of the reference period on the estimates, we create a CPS "look-alike" from the SIPP data. That is, we use the tri-annual data in SIPP to create an annual observation for each child. There are several issues which arise when creating this look-alike sample. First, attrition is likely to be more severe in a longitudinal survey such as the SIPP. Second, it is not clear whether CPS respondents answer the questions about health insurance coverage in the previous year with information about their entire previous year's coverage (as the question is posed) or about their coverage at a particular point in time (as many respondents appear to do–see Swartz 1986 and Shore-Sheppard 1996 for discussions of this issue.)

To address these issues, we create annual data from the SIPP using several alternative hypotheses about the sample to use and the way respondents might answer the CPS. We try three alternative samples of SIPP data: children who have 12 months of data from the first full year of the panel; children who have 12 months of data early in the panel (although not necessarily from the first full year); and children who have at least 6 months of data from the first year of the panel. We combine these three samples with five possible assumptions about how respondents might answer an annual (CPS) insurance question: as posed (had insurance at any time in the previous year); at a point in time (had insurance the last month of the year); over a shorter reference period (had insurance at any time in the last 3 months or alternatively the last 6 months); and for the majority of the year (had insurance over half of the time). For each of the three samples, variables other than the insurance status are summed over the months in the SIPP to create annual data. In particular, to create eligibility, family income is added over all of the months and eligibility is imputed using the annual data. Characteristics such as family size are obtained from the last month of each sample (corresponding to the use of March data on such variables in the CPS).

Health insurance coverage rates in the CPS and SIPP look-alike data match most closely when the sample used is children who had at least 6 months of data. Mean coverage rates for the CPS and for this sample under the various assumptions are given in Table 3. For Medicaid, the CPS coverage rates appear to match most closely the rates under the hypothesis that respondents are answering the question as posed. For private coverage, however, the CPS appears to be eliciting a lower level of coverage, with the rates matching most closely the rates arising from the hypotheses that respondents are answering as of a point in time (the last month of the year) or for the majority of the year.[18]

The coefficients on eligibility from regressions using the various look-alike samples are presented in Table 4. Comparing the coefficients from the SIPP results in Table 4 to those in Table 2 and to the CPS results (in the top row of Table 4), it appears that annualizing the SIPP data gives results that are closer to the CPS results, with larger coefficients on Medicaid and negative coefficients on private coverage. Further, the differences between the SIPP and CPS private coverage results are no longer statistically significant, although this is partly because of the larger standard errors resulting from the annualized data. On the other hand, the coefficients on private coverage remain smaller than in the CPS results, and are not statistically different from zero. We conclude from this exercise that part, but not all, of the difference between the SIPP and the CPS arises because of the fact that SIPP is based on monthly data while the CPS is based on annual data.

Since the Cutler-Gruber methodology shows crowding out in the CPS but little evidence of crowding out in the SIPP unannualized data, it is worthwhile to consider the relative merits of the two data sets. The major advantages of the CPS are that it is nationally representative, the sample size is large, and it does not suffer from problems peculiar to panel data such as attrition. The SIPP has several advantages over the CPS in addition to the more accurate measurement of health insurance coverage discussed earlier. Children's eligibility for Medicaid can be imputed more accurately in the SIPP, since the income data are gathered over a shorter recall period and more detailed information about birth dates is provided. As eligibility status may change over the year,

[18]The apparent difference in response behavior may be due in part to the fact that the Census Bureau imputes Medicaid coverage to children in families receiving AFDC, reducing the likelihood that a child is incorrectly coded as unenrolled (though increasing the likelihood of incorrectly coding a child as enrolled). Thus it is possible that in the CPS measurement of Medicaid coverage is more accurate than measurement of private coverage.

imputed eligibility in the SIPP is more clearly defined than imputed eligibility in the CPS, which is based on annual income and which cannot account for midyear rule changes. We see from above that annual measures of insurance and eligibility do produce different estimates than tri-annual measures, implying that the within-year distinctions that the SIPP provides are valuable. In summary we conclude, perhaps unsurprisingly, that there are benefits and costs to using each data set.

4.3. Robustness of the Static Model

Having taken as our starting point the specification of Cutler and Gruber (1996), we now examine the robustness of our results to changes in specification. Table 5 presents these specification checks, beginning with a model including only eligibility and ending with our preferred specification that we will use for the remainder of the paper. From the first three columns, it is clear that while there is a negative relationship between eligibility and private coverage in the regression with no controls, this estimate of the effect of eligibility on private coverage is biased downward by the omission of age and year effects. This is not surprising, as younger children are more likely to be eligible, and may also be less likely to have private insurance since their parents are on average younger and less likely to be working at a job offering health insurance. Omitting year effects also biases the coefficient in the private insurance regression downwards and the Medicaid regression coefficient upwards, as expected since the Medicaid expansions occurred concurrently with a recession.

Once age, year, and state dummies and basic demographic variables have been included in the regressions, columns (4)-(10) show that including or omitting other control variables makes little difference for the results. In column (6) we add additional family variables to the CPS specification – the age and education of the head – in order to control for additional determinants of income using plausibly exogenous factors. We check whether changes in family structure over the panel affect our results by conditioning on the initial values only in column (7). Column (8) examines whether additional state-level controls (the unemployment rate, the minimum wage, and the AFDC need standard) affect the estimates, while in column (9) we include a full set of month-year dummies. Column (10) is our preferred specification, including demographics, family control variables, age, year, and state dummies, and the unemployment rate (since the unemployment rate affects the employment status of the family head, and thus the constraints facing the family). Throughout these changes to the specification, the coefficient on eligibility remains remarkably robust at around 0.12 and statistically significant in the Medicaid equation and 0.01 and statistically insignificant in the private equation.

5. Extensions

5.1. Including Sibling Eligibility

Although eligibility is an individual characteristic, insurance coverage is usually a family decision, since private insurance plans typically cover all dependents or none in a family. This feature of private insurance may be a factor in the relatively low take-up of Medicaid if families are unwilling to pay the time costs of enrolling only some of their children while others remain ineligible and uninsured. Similarly, our finding of little crowd-out may be explained by families deciding not to drop their private coverage as long as some of their children remain ineligible for Medicaid. We examine these hypotheses by interacting eligibility with the fraction of a child's siblings who are eligible. Instruments for eligibility and its interaction with the fraction of eligible siblings are FRACELIG and the mean of FRACELIG for siblings in the family interacted with FRACELIG. We also include a dummy variable for having no siblings to differentiate between children who have no siblings and children who have no eligible siblings. Results from this analysis are presented in Table 6. We find that an eligible child is more likely to be enrolled in Medicaid if his or her siblings are eligible, however the increase in the estimated take-up rate is relatively small in absolute terms. The take-up rate is 2.6 percentage points higher for eligible children who have all of their siblings eligible, while children with no siblings have a 0.7 percentage point increase in the take-up rate. For private insurance, the coefficients on eligibility and the fraction of siblings eligible continue to show no evidence of crowding out. Somewhat surprisingly, having no siblings reduces the probability that a child has private insurance. Because including siblings complicates the dynamic models considerably and the estimated effects are modest, in the remainder of the paper we focus on the effects of a child's own eligibility.

5.2. Allowing for Different Effects of Eligibility Over Time

In the final sections of the paper, we continue our examination of take-up (and crowding out, though we have found little evidence of it thus far), taking advantage of some of the unique features of the SIPP. In the models above, we have implicitly assumed that the effect of eligibility is constant over time. Using panel data we can relax this assumption to allow the effect of eligibility to differ between children who are observed to gain eligibility and children who have been eligible for several months. Since parents may not be immediately aware of their child's eligibility, or they may not enroll the child until the child needs medical care (for example, a child may be enrolled at a hospital emergency room while being treated for an injury or illness), one would expect the effect of eligibility on insurance coverage to be smaller in the first months of eligibility. To allow for this possibility, we redefine Z_{kit} to contain two endogenous variables: a dummy variable indicating the first wave of eligibility observed following a period of non-eligibility and a dummy variable indicating that an individual has been eligible for two or more consecutive waves.[19] We also define

[19]While we observe the spell of eligibility beginning in the data, unfortunately we are unable to observe whether this is the first spell of eligibility an individual has ever had. Such an eligibility history would be useful in determining the role of learning about the program, however even without this information we are able to examine whether time since the beginning of an eligible spell affects the probability of take-up.

two alternative variables–a dummy variable for the first two waves of eligibility and a dummy variable indicating that an individual has been eligible for three or more consecutive waves–to examine whether our results are sensitive to the specific length of eligibility chosen. In both of these specifications we use current and lagged values of FRACELIG as instruments, since FRACELIG will be correlated with the dummy variable indicating whether the current time period is the first wave of eligibility, and lags of FRACELIG will be correlated with the dummy variable depending on past eligibility.

The results of this specification are presented in Table 7. From these results it is clear that children are not always immediately enrolled upon becoming eligible, lending support to the concern that parents are either not aware of their child's eligibility or are aware but do not enroll their child until urgent care is needed. Indeed, in the Medicaid regression the coefficient on the first four months of eligibility is actually negative, while the coefficient on eligibility after five or more months is positive and larger in magnitude than the effect of eligibility in Table 2. As would be expected if learning about eligibility occurs or if parents delay enrollment until care is needed, enrollment in response to eligibility is higher after three waves of eligibility (column (2)) than it is after two waves. However even among children who have been eligible for at least three waves (one year), the effect of eligibility is only slightly higher than it is for all children (the coefficient is 0.155 as compared with 0.118 in Table 2). In the private regression, the effects of eligibility are similar in magnitude in both specifications (both are close to zero, positive, and statistically insignificant), and it is not possible to reject that the effects are equal. Thus children appear to be somewhat more likely to be enrolled in Medicaid, but no more likely to have lost private coverage, after having spent some time eligible for Medicaid.

5.3. Models Including Individual Fixed Effects

Since we have panel data, we can estimate a fixed-effects model. Estimating such a model has advantages and disadvantages. The first advantage is that if eligibility has different effects for children eligible under different routes (eligibility via AFDC versus eligibility through the expansions, for example), by using a fixed effect model we focus on the treatment effect for children whose Medicaid eligibility changes over the panel–that is, children more likely affected by the expansions–rather than children who are always eligible through AFDC or children who are always ineligible. A second possible advantage is that fixed effect estimation will eliminate any inconsistency in the parameter estimates arising from any potentially endogenous explanatory variable that is correlated with the variables used to impute eligibility.[20] A disadvantage is that it is possible that fixed effects models can accentuate measurement error, although the direction of the bias is not clear given that measurement error in a dichotomous variable such as eligibility cannot be classical.[21] Finally, use of a fixed effect model requires that the explanatory variables be strictly exogenous, i.e. that future values of the explanatory variables are not affected by current insurance status.[22]

Since we are using a linear probability model, the extension to fixed effects estimation is

[20]Another advantage of the fixed effect specification is that it may help control for panel attrition. This would be the case if an individual has permanent characteristics that affect his or her likelihood of leaving the sample (for example, if less-educated family heads are more likely to leave).

[21]It is difficult to determine how measurement error in eligibility affects the fixed effect and first difference estimates, since to the best of our knowledge the effect of measurement error in a dichotomous variable (such as eligibility) has not been analyzed in such a model. Measurement error in a dichotomous variable cannot be classical in the sense of being uncorrelated with the true value. Bound, Brown and Mathiowetz (2000, section 2.6), citing work of Aigner (1973), show that under simplifying assumptions, we would expect measurement errors in a dichotomous variable to bias the coefficient of such a variable (and not its absolute value) in a negative direction. It seems plausible to argue that measurement error is accentuated in fixed effect estimation (but see Wooldridge 2002, chapter 10) and thus we might expect a stronger bias in fixed effect estimation.

[22]The fixed effect may proxy family permanent income, which affects the interpretation of the eligibility coefficient with and without the fixed effects. In this case, the coefficient from the fixed effect regression represents the effect of eligibility holding family permanent income constant (see, e.g., Browning, Deaton and Irish 1985 or MaCurdy 1981).

straightforward. We first estimate the fixed effects model by taking deviations from the individual means in (1). As a specification check on our results, we also estimate the model by first differencing (1) to obtain

$$\Delta I_{kit} = \Delta\tilde{Z}_{kit}\tilde{\gamma}_k + \Delta\varepsilon_{kit}, \quad \text{k=p,s,} \tag{2}$$

where $\tilde{Z}_{kit}$ refers to the time-changing explanatory variables. If the two sets of estimates differ, this would suggest that the model is misspecified.[23]

Table 8 presents the results. The coefficients from the fixed effect and first difference models are quite similar, which is reassuring from the perspective of model specification. However they are smaller in absolute value than the coefficients from the simple static models. In particular, the coefficient on eligibility in the Medicaid equation is reduced to 0.027 (0.019 in the first-differenced model) from 0.118. Note that introducing fixed effects does not affect the pattern of no evidence of significant crowd-out.

There are several possible explanations for the reduction in the estimated coefficients when fixed effects are used. First, as discussed above, the difference may reflect measurement error in eligibility. Second, the smaller size of the eligibility coefficient may indicate that once the between-child variation in eligibility is removed, the probability of taking up coverage among the newly eligible is smaller. This would be the case if the propensity to take up public coverage differs across children. As the eligibility effects are identified from children who become eligible over the course of the sample (and consequently are likely to be further up the income distribution and have greater access to private coverage), we find this explanation for the smaller coefficients when fixed effects are included plausible. To shed further light on this explanation, we reestimated the static model only

[23]89% of observations have greater than 2 waves of data, with 77% having greater than 5 waves and about half of the observations having between 8 and 10 waves.

for families whose income lay between 100% and 350% of the poverty line. The coefficient on eligibility in the Medicaid equation was 0.058 (0.008) which is also smaller than the static model for the entire sample, indicating that more marginal families (with respect to Medicaid eligibility) are indeed less likely to take up Medicaid in response to eligibility.

5.4. Simple Dynamic Models

Our final extension is to consider simple dynamic models of insurance determination. One potential drawback of the static model is that it implicitly assumes that families make a new decision each period about whether or not to obtain public or private insurance for their children, and that this decision is independent of last period's decision. However insurance outcomes are closely related to job outcomes—families often gain access to private insurance when members find a job, and can lose private insurance when they are laid off. Since there is substantial persistence in labor market histories of disadvantaged women (see, e.g., Chay and Hyslop 1998), if we do not control for labor market histories in estimation, we would expect this persistence to carry over into insurance determination. Also, the static model does not incorporate the notion of fixed costs: a family with a child on Medicaid has already paid the fixed costs of enrolling the child and is more likely to have the child on Medicaid next month. Thus short-run and long-run effects of Medicaid eligibility may differ substantially.

A simple dynamic model is obtained by adding a lagged dependent variable to (1)

$$I_{kit} = Z_{kit}\gamma_k + \delta I_{kit-1} + \nu_{ki} + \varepsilon_{kit}, \text{ k=p,s,} \quad (3)$$

where I_{pit-1} (I_{sit-1}) equals one if the individual had private (public) insurance last wave and zero otherwise. We would expect the lagged dependent variable to be correlated with the error term given the substantial persistence in panel data. Thus we must treat eligibility and the lagged dependent variable as endogenous. To account for this, we include FRACELIG, AGEELIG, and lags of those variables and the family characteristics in the first stage regressions for the lagged dependent variable and eligibility but exclude these variables from the second stage equation.

As a summary statistic we first estimate an autoregressive model with no explanatory variables. Not surprisingly, there is a very high degree of persistence in the data–the coefficients (standard errors) on lagged coverage are 0.847 (0.001) and 0.848 (0.001) in the Medicaid and private regressions, respectively. Including covariates and treating both eligibility and the lagged dependent variable as endogenous reduces the coefficient on the lagged dependent variables only slightly (see the results in Table 9). The immediate impact of Medicaid eligibility in the public insurance equation is estimated to be 0.035. To obtain the long-run effect of eligibility, we divide its coefficient by one minus the coefficient on the lagged dependent variable. The estimated long-run effect is thus 0.035/(1-0.800) = 0.175 with a standard error of 0.019, which is larger than the static estimate in Table 2. For private insurance, the immediate effect of eligibility is –0.007 (though imprecisely estimated). The long-run effect is -0.032 with a standard error of 0.021, which unlike the result in Table 2 is negative (and almost statistically significant, with a t-statistic of 1.5). Taking the estimates at face value, we would estimate the long-run crowding out effect as –0.032/0.175 or 18 percent, an estimate in the middle of the estimates from the existing static literature. Thus the results from the dynamic models suggest both that eligibility effects are bigger in the long run, and that some crowding out may have occurred.[24]

6. Conclusions

In this paper we use data from the SIPP to examine the impact of expansions in Medicaid eligibility on Medicaid and private insurance coverage. We attempt to replicate the results obtained

[24]Since we have multiple instruments in this model, we carried out a test of the overidentifying restrictions. It was rejected in each equation, but it is difficult to know whether the rejections reflect an economically significant problem or simply a statistical one. They may indicate a need for a richer dynamic specification, but this extension was beyond the scope of the paper.

by Cutler and Gruber (1996) using the CPS, and find smaller but still statistically significant estimates of Medicaid take-up, but no evidence of crowding out. We investigate the possible sources of this difference, and are able to obtain the same results in the CPS as we find in the SIPP for Medicaid take-up when we eliminate small states and older children from the CPS sample. Even after eliminating these observations from the CPS, however, the SIPP and CPS results for private coverage continue to differ.

Hypothesizing that the remaining difference between the results in SIPP and CPS for private coverage may be due to the annual nature of the CPS data collection, we "annualize" the SIPP under various assumptions about response in the CPS. Using the "annualized" SIPP data, the eligibility effects from the private coverage regressions are larger than when we use the actual data and are no longer significantly different from those of the CPS. However, the estimated coefficients are still uniformly smaller than the CPS estimates and are statistically insignificant. Thus part, but not all, of the difference in the results from the SIPP and CPS data appears to arise from the difference in the reporting periods used by each data set.

We extend the previous literature in several directions. We examine whether having eligible siblings affects the probability a child is enrolled in Medicaid. We find that children with a larger fraction of their siblings eligible are more likely to be enrolled, but the increase in estimated take-up is slight. Similarly, when we allow the effect of eligibility on Medicaid enrollment to differ for children who have just become eligible and children who have been eligible for several months, we find that children are more likely to be enrolled after having spent some time eligible, but again the estimated take-up rate is relatively small. We use a fixed effects model to estimate eligibility effects for children whose eligibility status changes over the sample period. The estimates from this model are smaller than our cross-section estimates, which suggests that it may be fruitful to consider a broader range of variable treatment models in future work. We continue to find no evidence of crowding out in any of these extensions.

Finally, we examine a simple dynamic model of insurance choice, relaxing the assumption that insurance choice in each period is independent of the previous period's choice. We find that insurance choice is quite persistent. When dynamics are accounted for, the estimated long-run impact of eligibility on take-up is larger than the (constant) effect estimated from the static model, while the short-run impact of expanded Medicaid eligibility is smaller. Unlike the static model, the dynamic model shows some evidence of crowding out, though the standard errors are relatively large. These results indicate that further analysis of insurance dynamics among children would be useful in gaining a more complete picture of coverage following the Medicaid expansions.

In general, our results show smaller rates of take-up and less evidence of crowding out than do previous estimates in the literature. In particular, our results indicate that there is a significant delay in enrollment following eligibility onset. From a policy perspective, these results indicate that informing parents both about the possibility that their child is eligible and about the benefits of enrolling their child in Medicaid may be a useful strategy in reducing the apparent gap between eligibility onset and enrollment. Our results also indicate that states which used their SCHIP funds to finance equal income eligibility standards across all ages (so that all children in a given family would be eligible) are likely to experience greater take-up than states with eligibility standards that continue to differ by age. Overall, given the relative magnitudes of take-up and crowding out observed in the SIPP, it appears that focusing on encouraging take-up, even if some crowding out also occurs, is a worthwhile strategy. This may be especially true in the case of the SCHIP program, since the anti-crowd-out measures required by the legislation are likely to decrease any crowding out that might occur from expanded public coverage.

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Table 1: Summary Statistics of Variables Used in Regressions

SIPP Panel:	1986	1987	1988	1990	1991	1992	1993
Insurance Variables:							
Medicaid	0.120	0.116	0.114	0.162	0.167	0.181	0.201
	(0.002)	(0.001)	(0.002)	(0.001)	(0.001)	(0.001)	(0.001)
Private insurance	0.733	0.752	0.749	0.710	0.726	0.710	0.694
	(0.002)	(0.002)	(0.002)	(0.001)	(0.002)	(0.001)	(0.001)
Eligibility variables:							
Imputed eligible	0.187	0.176	0.179	0.276	0.295	0.312	0.334
	(0.002)	(0.002)	(0.002)	(0.001)	(0.002)	(0.001)	(0.001)
Age-eligible (=1 if	0.003	0.027	0.078	0.406	0.534	0.669	0.790
expansion for child's age)	(0.0003)	(0.001)	(0.001)	(0.002)	(0.002)	(0.001)	(0.001)
Fraction of siblings eligible	0.151	0.140	0.144	0.223	0.244	0.252	0.272
	(0.002)	(0.002)	(0.002)	(0.001)	(0.002)	(0.001)	(0.001)
Demographic variables:							
Male	0.508	0.515	0.509	0.512	0.513	0.520	0.515
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
White	0.828	0.830	0.825	0.782	0.815	0.805	0.811
	(0.002)	(0.002)	(0.002)	(0.001)	(0.001)	(0.001)	(0.001)
Age	8.538	8.536	8.326	8.332	8.380	8.360	8.431
	(0.025)	(0.024)	(0.025)	(0.016)	(0.019)	(0.015)	(0.015)
Family characteristics:							
Age of highest earner	36.562	36.563	36.575	36.748	36.983	36.976	37.181
in HIU	(0.040)	(0.038)	(0.041)	(0.026)	(0.031)	(0.024)	(0.025)
Education of highest	12.678	12.693	12.848	12.739	12.919	12.954	12.950
earner in HIU	(0.014)	(0.014)	(0.015)	(0.010)	(0.011)	(0.009)	(0.009)
Size of HIU	4.220	4.160	4.183	4.159	4.216	4.172	4.221
	(0.007)	(0.006)	(0.006)	(0.004)	(0.005)	(0.004)	(0.004)
Two parents	0.758	0.768	0.768	0.710	0.744	0.728	0.733
	(0.002)	(0.002)	(0.002)	(0.001)	(0.002)	(0.001)	(0.001)
Only a male head	0.022	0.023	0.019	0.027	0.029	0.026	0.021
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.0005)	(0.0005)
No earners	0.140	0.131	0.123	0.159	0.154	0.156	0.162
	(0.002)	(0.002)	(0.002)	(0.001)	(0.001)	(0.001)	(0.001)
One earner	0.411	0.416	0.419	0.423	0.420	0.413	0.407
	(0.002)	(0.002)	(0.002)	(0.001)	(0.002)	(0.001)	(0.001)
Two earners	0.382	0.390	0.399	0.368	0.377	0.382	0.384
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.001)	(0.002)
Family income as percent	256.936	267.336	270.839	250.532	257.950	252.493	254.128
of poverty level	(1.033)	(1.072)	(1.118)	(0.677)	(0.844)	(0.646)	(0.666)
State unemployment rate	6.707	5.924	5.507	6.649	7.275	6.883	6.351
	(0.009)	(0.009)	(0.008)	(0.005)	(0.006)	(0.005)	(0.005)
Minimum wage effective	3.359	3.413	3.476	4.053	4.239	4.290	4.299
in state	(0.0003)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
State monthly AFDC need	645.42	670.44	687.09	723.28	751.76	777.67	806.52
standard	(1.009)	(1.002)	(1.051)	(0.772)	(0.999)	(0.856)	(0.909)
Years covered	86-88	87-89	88-89	90-92	91-93	92-95	93-95
Person-waves available	44016	45691	40895	99446	66991	108572	101967

Notes: Summary statistics calculated for sample of children from listed SIPP panels. Standard errors in parentheses.

Table 2: Replication of CPS Results Using SIPP

	Medicaid	Private Insurance
Eligible	0.118	0.006
	(0.010)	(0.014)
Male	0.001	0.002
	(0.002)	(0.002)
White	-0.065	0.067
	(0.003)	(0.003)
Size of HIU	0.027	-0.041
	(0.001)	(0.001)
Two parents	-0.174	0.180
	(0.004)	(0.005)
Only a male head	-0.158	0.083
	(0.006)	(0.009)
No earners	0.430	-0.617
	(0.007)	(0.010)
One earner	0.018	-0.152
	(0.003)	(0.004)
Two earners	0.0003	-0.034
	(0.002)	(0.003)
R^2	0.462	0.334
Coeff. on FRACELIG in first stage	0.888 (0.012)	
$N_{\text{person-waves}}$, $N_{\text{individuals}}$	507578 person-waves, 75139 individuals	

Notes: Estimated using children from the 1986-1993 SIPP panels, as described in the text. All regressions include age, year, and state dummy variables. Standard errors (in parentheses) have been corrected for repeated observations within individuals and heteroscedasticity.

Table 3: Mean Insurance Coverage of CPS and CPS Look-Alike Data from SIPP

Year:	1987	1988	1990	1991	1992	1993
Medicaid:						
CPS	0.159	0.154	0.187	0.210	0.225	0.247
SIPP						
Using assumption:						
Any time last year	0.158	0.154	0.203	0.221	0.236	0.267
Last month	0.121	0.116	0.165	0.173	0.184	0.213
Last 3 months	0.130	0.126	0.175	0.185	0.198	0.228
Last 6 months	0.140	0.138	0.187	0.202	0.218	0.247
Most of year	0.121	0.119	0.153	0.172	0.178	0.211
Private:						
CPS	0.731	0.738	0.707	0.687	0.678	0.666
SIPP						
Using assumption:						
Any time last year	0.812	0.804	0.769	0.775	0.767	0.739
Last month	0.746	0.743	0.700	0.717	0.703	0.679
Last 3 months	0.764	0.759	0.721	0.734	0.718	0.694
Last 6 months	0.785	0.779	0.742	0.755	0.738	0.714
Most of year	0.743	0.743	0.706	0.719	0.708	0.679

Notes: Entries in the table are insurance coverage rates in the 1988-1989 and 1991-1994 CPS and coverage rates from children who provide at least six months of data within the first year of each SIPP panel, aggregated to the annual level under the listed assumptions. In order to ensure comparability, infants are omitted from both samples. Since we do not use the 1989 panel, 1989 is omitted.

Table 4: Comparing Eligibility Coefficients from CPS Data and SIPP CPS Look-Alike Data

	Medicaid	Private
Results from CPS data	0.136	-0.070
	(0.013)	(0.016)
Results from SIPP data:		
I. First year of panel		
Annual	0.170	-0.029
	(0.048)	(0.038)
Last month	0.098	-0.024
	(0.045)	(0.046)
Last 3 months	0.148	-0.012
	(0.047)	(0.043)
Last 6 months	0.147	-0.015
	(0.049)	(0.040)
Majority of year	0.122	-0.030
	(0.040)	(0.043)
II. First year, or surrounding 12 months		
Annual	0.175	-0.040
	(0.050)	(0.036)
Last month	0.109	-0.031
	(0.046)	(0.040)
Last 3 months	0.129	-0.020
	(0.046)	(0.038)
Last 6 months	0.156	-0.025
	(0.049)	(0.037)
Majority of year	0.131	-0.038
	(0.045)	(0.039)
III. First year, or 6 months of data		
Annual	0.170	-0.043
	(0.048)	(0.033)
Last month	0.102	-0.032
	(0.044)	(0.040)
Last 3 months	0.126	-0.028
	(0.046)	(0.037)
Last 6 months	0.150	-0.028
	(0.048)	(0.036)
Majority of year	0.122	-0.055
	(0.038)	(0.036)

Notes: The CPS estimates are from the sample omitting states and ages not represented in the SIPP sample (children older than 15 and children in small states). Each entry in the SIPP portion of the table is the coefficient on eligibility from a regression on the look-alike sample created using the specified reference period assumption and data. "Annual" assumes the respondents answer the CPS insurance questions as posed, "last month" assumes the respondents' reference period is the last month of the period, "last 3 months" and "last 6 months" assume the respondents use a reference period of the previous 3 months and 6 months, respectively, and "majority of year" assumes the respondents answer the insurance questions according to the type of insurance they had for the most time in the previous year.

Table 5: Robustness Checks

	No controls	Age dummies only	Age and year dummies	(3) + indiv. controls, state dummies	CPS spec.	(5) + add'l. family vars.	Initial values of family vars.	(6) + add'l. state vars.	Month-year dummies	Base spec.
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Coefficient on eligible:										
Medicaid	0.351 (0.007)	0.313 (0.009)	0.236 (0.011)	0.133 (0.013)	0.118 (0.010)	0.119 (0.010)	0.112 (0.011)	0.117 (0.010)	0.113 (0.010)	0.121 (0.010)
Private	-0.181 (0.009)	-0.078 (0.013)	-0.005 (0.016)	-0.007 (0.017)	0.006 (0.014)	0.010 (0.013)	0.020 (0.014)	0.009 (0.014)	0.011 (0.014)	0.009 (0.013)
Indiv. controls?[1]	No	No	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Family controls?[2]	No	No	No	No	Yes	Yes	No	Yes	Yes	Yes
Age dummies?	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year dummies?	No	No	Yes	Yes	Yes	Yes	Yes	Yes	No (month x year)	Yes
State dummies?	No	No	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State-level controls?[3]	No	No	No	No	No	No	No	Yes	Yes	Unempl. rate only
Additional controls:	None	None	None	None	None	Head's age and educ.	Initial val. of fam. var.	Head's age and educ.	Head's age and educ.	Head's age and educ.

Notes: Each entry in the table is the coefficient on eligibility from a regression estimated using children from the 1986-1993 SIPP panels, as described in the text. Standard errors (in parentheses) have been corrected for repeated observations within individuals and heteroscedasticity.

[1]Individual controls: male, white.

[2]Family controls: family size, family type (two parent, single male head, single female head), number of earners.

[3]State-level controls: unemployment rate, minimum wage, AFDC need standard. Since year dummies are included, this is approximately equivalent to using the real value of the minimum wage.

Table 6: Effect of Sibling Eligibility

	Medicaid	Private Insurance
Eligible	0.104	-0.005
	(0.014)	(0.018)
Fraction of siblings eligible*Eligible	0.026	0.021
	(0.013)	(0.016)
No siblings	0.007	-0.028
	(0.003)	(0.005)
Male	0.001	0.001
	(0.002)	(0.002)
White	-0.068	0.072
	(0.003)	(0.003)
Age of highest earner in HIU	-0.002	0.003
	(0.0002)	(0.0002)
Educ. of highest earner in HIU	-0.011	0.036
	(0.0004)	(0.001)
State unemployment rate	-0.002	0.001
	(0.001)	(0.001)
Number of children in HIU	0.023	-0.038
	(0.001)	(0.002)
Two parents	-0.132	0.105
	(0.003)	(0.004)
Only a male head	-0.149	0.071
	(0.006)	(0.008)
No earners	0.390	-0.519
	(0.007)	(0.008)
One earner	-0.004	-0.108
	(0.002)	(0.004)
Two earners	-0.016	-0.006
	(0.002)	(0.004)
R^2	0.473	0.387
$N_{\text{person-waves}}$, $N_{\text{individuals}}$	507578, 75139	

Notes: Estimated using children from the 1986-1993 SIPP panels, as described in the text. Instruments for eligibility and its interaction with fraction of eligible siblings are FRACELIG and the mean of FRACELIG for siblings in the family interacted with FRACELIG. Both first stage equations are well identified. For example, the coefficient on FRACELIG has a coefficient (standard error) of 0.819 (0.014) in the first stage equation for ELIG. Further, the mean of FRACELIG for siblings in the family interacted with FRACELIG has a coefficient (standard error of 1.379 (0.019) in the first stage equation for the fraction of siblings eligible interacted with eligibility. All regressions include age, year, and state dummy variables. Standard errors (in parentheses) have been corrected for repeated observations within individuals and heteroscedasticity.

Table 7: Regressions Allowing Effect of Eligibility to Differ

	Medicaid		Private Insurance	
	(1)	(2)	(3)	(4)
Eligible: 2 waves or more	0.140		0.011	
	(0.012)		(0.016)	
Eligible: first wave	-0.085		0.009	
	(0.022)		(0.026)	
Eligible: 3 waves or more		0.155		0.008
		(0.014)		(0.019)
Eligible: first 2 waves		-0.054		0.034
		(0.019)		(0.024)
Male	0.001	0.001	0.001	0.0004
	(0.002)	(0.002)	(0.002)	(0.002)
White	-0.067	-0.068	0.071	0.071
	(0.003)	(0.003)	(0.003)	(0.004)
Age of highest earner in HIU	-0.002	-0.002	0.003	0.003
	(0.0002)	(0.0002)	(0.0002)	(0.0002)
Educ. of highest earner in HIU	-0.011	-0.011	0.036	0.036
	(0.0004)	(0.0004)	(0.001)	(0.001)
State unemployment rate	-0.003	-0.003	0.002	0.002
	(0.001)	(0.001)	(0.001)	(0.001)
Size of HIU	0.023	0.022	-0.033	-0.032
	(0.001)	(0.001)	(0.001)	(0.001)
Two parents	-0.147	-0.144	0.140	0.137
	(0.004)	(0.005)	(0.005)	(0.006)
Only a male head	-0.140	-0.134	0.068	0.068
	(0.006)	(0.006)	(0.009)	(0.009)
No earners	0.408	0.405	-0.530	-0.535
	(0.007)	(0.007)	(0.009)	(0.010)
One earner	0.014	0.016	-0.119	-0.121
	(0.003)	(0.003)	(0.004)	(0.004)
Two earners	-0.006	-0.006	-0.016	-0.018
	(0.002)	(0.002)	(0.003)	(0.003)
R^2	0.485	0.489	0.386	0.387
Reject that effects same?	yes	yes	no	no
(p-value)	(0.000)	(0.000)	(0.955)	(0.406)
$N_{\text{person-waves}}$,	432439,	361681,	432439,	361681,
$N_{\text{individuals}}$	70758	67181	70758	67181

Notes: Estimated from a sample of children from the 1986-1993 SIPP panels, as described in the text. Variables included in first stage and omitted from second stage are FRACELIG and lags of FRACELIG. All regressions include age, year, and state dummy variables. Standard errors (in parentheses) have been corrected for repeated observations within individuals and heteroscedasticity.

Table 8: Fixed Effects Regressions

	Medicaid		Private	
Individual effect removed using:	Diff. from means	First differences	Diff. from means	First differences
	(1)	(2)	(3)	(4)
Eligible	0.027	0.019	0.004	-0.013
	(0.006)	(0.009)	(0.007)	(0.011)
Age of highest earner in HIU	-0.001	-0.001	0.0004	-0.0002
	(0.0002)	(0.0002)	(0.0002)	(0.0002)
Education of highest earner in HIU	-0.002	-0.001	0.002	-0.0003
	(0.0004)	(0.0004)	(0.001)	(0.001)
State unemployment rate	-0.0001	0.001	-0.0002	0.0001
	(0.0004)	(0.0005)	(0.0005)	(0.0001)
Size of HIU	0.014	0.006	-0.006	-0.004
	(0.001)	(0.001)	(0.001)	(0.001)
Two parents	-0.081	-0.057	0.074	0.054
	(0.003)	(0.004)	(0.003)	(0.004)
Only a male head	-0.097	-0.078	0.052	0.037
	(0.005)	(0.006)	(0.006)	(0.007)
Number of earners	-0.030	-0.018	0.046	0.030
	(0.001)	(0.001)	(0.001)	(0.001)
Age	-0.001	-0.002	0.003	-0.001
	(0.001)	(0.001)	(0.001)	(0.001)
Age squared/1000	0.0002	0.00003	-0.0002	0.00001
	(0.00003)	(0.00006)	(0.00004)	(0.0001)
p-value of χ^2 -test on inst. in first stage	0.000	0.000	0.000	0.000
$N_{person-waves}$, $N_{indiv.}$	507578, 75139	432439, 70758	507578, 75139	432439, 70758

Notes: Estimated from a sample of children from the 1986-1993 SIPP panels, as described in the text. All regressions include year effects. Standard errors (in parentheses) have been corrected for heteroscedasticity. Instruments used to identify ELIG are FRACELIG and AGEELIG.

Table 9: Regressions Including Lagged Insurance Status

	Medicaid	Private
Lagged insurance status	0.800	0.789
	(0.007)	(0.007)
Eligible	0.035	-0.007
	(0.004)	(0.005)
Age of highest earner in HIU	-0.0003	0.0004
	(0.0001)	(0.0001)
Education of highest earner in HIU	-0.002	0.007
	(0.0002)	(0.0003)
State unemployment rate	-0.0005	0.0003
	(0.0003)	(0.0003)
Size of HIU	0.006	-0.008
	(0.0004)	(0.0005)
Two parents	-0.035	0.032
	(0.002)	(0.002)
Only a male head	-0.039	0.023
	(0.003)	(0.003)
Number of earners	-0.029	0.046
	(0.001)	(0.001)
Age	-0.001	0.002
	(0.0003)	(0.0003)
Age squared/1000	0.0001	-0.0002
	(0.00001)	(0.00002)
Long-run impact of eligibility	0.175	-0.032
	(0.019)	(0.021)
p-value from F-stat. for omitted inst. for lagged dep. var.	0.000	0.000
$N_{person\text{-}waves}$, $N_{individuals}$	361681, 67181	

Notes: Estimated from a sample of children from the 1986-1993 SIPP panels, as described in the text. The overall amount of first-order serial correlation in Medicaid participation is 0.847 (0.001) and in private insurance participation is 0.848 (0.001). All regressions include year and state effects.