Did the Medicaid expansions for children displace private insurance? An analysis using the SIPP

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Abstract

Using data from the 1990 panel of the Survey of Income and Program Participation (SIPP), we address the question: Did the Medicaid expansions for children cause declines in private coverage? We use a multivariate approach that attributes a displacement effect to declines in private coverage for children targeted by the Medicaid expansions exceeding declines for a comparison group of older low-income children. We find that 23\% of the movement from private coverage to Medicaid due to the expansions was attributable to displacement. There is no evidence of displacement among those starting uninsured, leading to an overall displacement effect of 4\%. © 2000 Elsevier Science B.V. All rights reserved.

Keywords: Medicaid; Private health insurance; Crowd-out

1. Introduction

Until the late 1980s, Medicaid eligibility for children had been limited to children in welfare families and later extended to children in two-parent families with incomes below welfare eligibility thresholds. In response to declining health status indicators for low-income children and growing disparities in health status and utilization between the insured and the uninsured (Rosenbach, 1985; Kasper, 1987; Leftkowitz and Short, 1989; Short and Leftkowitz, 1991), beginning in...
1988, Congress permitted and eventually mandated states to provide Medicaid coverage for children in higher-income families. As of April 1990, coverage became mandatory for children up to age 6 in families with incomes up to 133% of the federal poverty level. As of July 1991, coverage became mandatory for children born after September 30, 1983 with family incomes up to 100% of poverty. Federal legislation also gave states the option (starting in 1988) of covering infants with family incomes up to 185% of poverty. These expansions were intended to reduce the number of uninsured children, improve children’s access to health care, and, thus, improve their health.

Between 1988 and 1993, the number of children receiving Medicaid-covered services grew by 53%. Over the same period, however, employer-sponsored insurance coverage declined (Peat Marwick, 1994; Holahan et al., 1995), and the number of uninsured children grew (Dubay and Kenney, 1996). This combination of trends has led some to question whether the Medicaid expansions for children and pregnant women “crowded-out” employer-sponsored coverage (Cutler and Gruber, 1996a,b; Dubay and Kenney, 1996, 1997; Yazici and Kaestner, forthcoming) rather than expanded coverage for those who would have continued to be uninsured in its absence.

Crowd-out is a term that covers two potential unintended consequences of the Medicaid eligibility expansion: (1) persons with private coverage drop it in order to take advantage of the public subsidy being offered; and (2) some who are uninsured enroll in Medicaid rather than obtain private coverage (as they would have under the more stringent Medicaid eligibility conditions). A related possibility is that employers use the availability of publicly sponsored plans to discontinue (or not begin) offering group coverage to their employees. Crowd-out can have two major policy implications. The substitution of Medicaid for private coverage may lead to fewer improvements in access to care and health status because the change from being uninsured to insured is more limited than expected. Crowd-out may also lead to greater increases in Medicaid expenditures than expected as

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Unpublished Urban Institute tabulations of HCFA Form 2082 data.

Insurance of any kind (private or public) is thought to have positive ramifications for health status relative to uninsurance. If new Medicaid participants are, in significant part, individuals who were previously privately insured instead of previously uninsured, the magnitude of improvements in children’s health status as a result of the Medicaid expansions would be less than expected. Secondarily, there are varying opinions as to the relative health status implications of private vs. public health insurance. On one hand, Medicaid covers all preventive care visits and ‘wrap-around’ services and does not require deductibles and co-payments. Cost-sharing may be required with private insurance coverage, potentially creating barriers to obtaining timely preventive or illness-related care for some low-income individuals. On the other hand, the well-documented access and quality problems within the Medicaid program may present significant barriers to obtaining timely and appropriate care for some enrollees. Therefore, substituting Medicaid for employer-sponsored coverage could lead to better or worse access and outcomes, depending on the characteristics of both the employer-sponsored coverage, the Medicaid program, and the particular group of beneficiaries.
individuals who previously had private insurance drop it to enroll in the subsidized public program. However, providing Medicaid coverage, an income transfer to the low-income population, may be considered desirable regardless of previous insurance status.

The essential policy question is: Did the Medicaid expansions cause the private coverage declines or were the two contemporaneous trends actually independent? This is a difficult question to answer because of the many factors that have to be disentangled to establish causation. In this paper, we use data from the 1990 panel of the Survey of Income and Program Participation (SIPP) to address this policy question. The SIPP is a nationally representative longitudinal database that allows us to track insurance coverage transitions of specific children over the 32-month period during which the mandated expansions were implemented. We compare health insurance coverage transitions of one group of low-income children affected and another group unaffected by the expansions. Our analysis extends the previous literature by using nationally representative longitudinal data to calculate separate effects for children who were privately insured prior to the mandated increases in Medicaid eligibility thresholds and for children who were uninsured before the mandatory Medicaid expansions. We do not address the issue of whether employers are making decisions to offer insurance coverage to their employees or not. Nor do we assess whether the expansions led some families to remain in the Medicaid program for longer periods before obtaining private coverage than they would have without the expansions. We find that 23% of the movement from private coverage to the Medicaid program as a result of the expansions was due to the displacement of private coverage. We find no evidence that the expansions discouraged families with uninsured children from taking up private coverage.

Section 2 summarizes the literature on the subject of crowd-out. Section 3 presents our methodological approach and Section 4 describes the data used. Section 5 presents our empirical results. Section 6 summarizes our findings and conclusions.

2. Previous literature

A number of studies have assessed the extent of crowding-out of private insurance by Medicaid expansions to poor children and pregnant women. Any analysis of crowd-out is inevitably complicated because health insurance transitions take place regardless of policy change. In addition, over the period of interest, there was at least some decline in employer coverage for groups unaffected by the Medicaid expansions, and a recession that increased the number of people in poverty. To put our own analysis in perspective, we review briefly how various studies account for non-expansion-related trends in coverage and how they define the populations of interest. It is important to recognize, however, that while each of the studies uses a difference-in-difference approach to estimate the effects
of the expansions, each poses different questions and measures the extent of 
crowd-out in different ways, making it difficult to compare their results. ³ (For a 
full discussion of the relevant literature, see Dubay, 1999.)

Cutler and Gruber (1996a; b) were the first to assess the extent of crowd-out 
under the Medicaid expansions and used individual level data from the 1987 
through 1993 CPS. The work by Cutler and Gruber makes a number of different 
crowd-out estimates. In contrast to the other studies that use different low-income 
populations to account for what would have happened in the absence of the 
expansions, Cutler and Gruber use the experience of non-eligible populations, 
regardless of income, to control for trends in insurance coverage. Their estimates 
are identified from cross-state differences in changes in eligibility. For children, 
they find that between 31 and 41% of the increase in Medicaid coverage, that 
occurred as a result of the expansion in eligibility, was offset by declines in private 
insurance coverage resulting from the expansions. Accounting for conditional coverage and 
spillover effects, ⁴ Cutler and Gruber estimate that for every two people who 
enrolled in Medicaid as a result of the expansions, one person dropped his private 
insurance, a 50% effect. However, they note that "80% of the increase in 
Medicaid [as a result of the expansions], or 2.8 million people, was from those 
who were formerly uninsured" an alternatively measured crowd-out rate of 20% 
(Cutler and Gruber, 1996a,b). The difference in their two estimates is due to the 
fact that not everyone they identified as dropping private insurance as a result of 
the expansions was eligible for Medicaid; many were non-eligible family members 
of eligible individuals (i.e., a spillover effect).

Dubay and Kenney (1996; 1997) also used the CPS to examine the extent of 
crowd-out that occurred with the Medicaid expansions. Dubay and Kenney limit 
their analysis to low-income young children and pregnant women and use low-in-
come men to control for the secular trend in insurance coverage. They limit their 
sample in this way on the grounds that the expansions affected only a small 
portion of the entire population, and the secular declines in employer-sponsored 
insurance that were occurring for the group targeted by the expansions were likely 
to have been dissimilar to the declines for the whole population. They find that 
overall 14% of the total increase in Medicaid enrollment of pregnant women, and 
17% of the total increase in enrollment of young children that occurred over the 
expansion period were attributable to crowd-out.

³ Shore-Sheppard (1997) has also looked at this issue.

⁴ To account for conditional coverage, Cutler and Gruber treat as Medicaid-covered those children 
and pregnant women who do not enroll in Medicaid but who implicitly have coverage if they become 
pregnant or sick. Spillover effects come about when a family drops employer-sponsored coverage 
because some of the family members are eligible for and choose to take advantage of, Medicaid, 
leaving the ineligible family members without insurance.
Yazici and Kaestner (forthcoming) use the 1988 and 1992 waves of the NLSY, a longitudinal database, and analyze low-income children who were under age 9. They use two groups of low-income children who were never eligible for Medicaid to account for the secular trend in coverage (those whose families had a loss of income and those whose families had no loss of income). Yazici and Kaestner make separate crowd-out estimates for children who were eligible for Medicaid in 1988, and two groups of children who became eligible for Medicaid by 1992 (those whose families did and did not experience a loss of income). Similar to Dubay and Kenney, Yazici and Kaestner estimate that overall 19% of the increase in Medicaid coverage that occurred over this period for their cohort of low-income young children was attributable to crowd-out.

Finally, Thorpe and Florence (1998) take a different approach to measuring crowd-out using data from the 1989 through 1994 waves of the NLSY. They define as crowd-out the movement of a child into Medicaid when his/her parent has private coverage (i.e., they use the children’s parents as a control for what would have happened in the absence of the expansions). They find that for children living in poverty who moved into the Medicaid program from private insurance, between 1 and 14% had parents who maintained private coverage, depending on the year being examined. For children with incomes above poverty, this percentage ranged from 15 to 20%. When all entrants into the Medicaid program with incomes below 200% of poverty are considered, i.e., those entering from uninsurance and from private coverage, they estimate that 16% of all children who entered the Medicaid program in 1990 and 1994 did so as the result of the crowding-out of private coverage.

Our analysis extends the previous literature by examining actual insurance coverage transitions among children affected and unaffected by the expansions and using multivariate analysis to estimate the share of “expansion-related” movement into the Medicaid program that was due to crowd-out. We estimate separate crowd-out effects for children who had private coverage prior to the expansions, for children who were uninsured prior to the expansions, and an overall effect. We use the experience of a group of slightly older children who have similar family incomes to those children targeted by the expansions to control for other changes occurring over the expansion period.

3. Methods

We assess whether and to what extent the Medicaid expansions for children displaced private coverage by examining the health insurance coverage of low-income children at the first interview of the 1990 SIPP panel (prior to the mandated Medicaid expansions) and its last interview (after the mandates are implemented) to see how their insurance coverage changed over time. In order to net out changes in insurance coverage resulting not from the expansions but from other factors that were changing over the period, we contrast observed changes in coverage for
children affected by the expansions in eligibility with changes for a comparison group of children who were unaffected by the expansions. Applying multivariate regression models to this quasi-experimental design, we produce three estimates that together indicate the extent of crowd-out overall and among the two component populations: the percentage of the movement from private coverage into the Medicaid program due to crowd-out; the percentage of the movement from uninsurance into the Medicaid program due to crowd-out (i.e., families choosing not to obtain private coverage); and the overall percentage of the movement into the Medicaid program due to crowd-out.

3.1. Analytic approach

Even with longitudinal data, as noted, determining causation, (i.e., whether the Medicaid expansions actually displaced private insurance) is complicated. In order to isolate displacement from other factors, we need to disentangle the impacts of the expansions from the impacts of these other factors. Otherwise, we risk overestimating (underestimating) the extent to which the expansions displaced private insurance by falsely attributing to (neglecting to attribute to) the expansions movement out of private coverage into Medicaid which was actually unrelated (related).

Our method of controlling for the impacts of these other factors is to subtract the insurance changes of a comparison group of children from the insurance coverage changes of the target group of children. This difference-in-differences approach only attributes to the expansions any decline in private coverage for the target group that is greater than that for the comparison group. This approach assumes that the impact of any other factor that might affect these patterns, beyond those that we control for explicitly, was the same for the two groups.

The 1990 panel of the SIPP collects information over a 32-month period beginning in the last quarter of 1989. Our target group consists of children born after September 30, 1983 — essentially children under age 7 at the beginning of the panel — with incomes below 185% of poverty at the first interview. Depending on family income, some of these children became eligible for Medicaid over the course of the panel as a result of the mandated expansions, while others were eligible through the traditional eligibility routes or optional expansion routes available from the beginning of the panel. The comparison group consists of slightly older children in families with incomes below 185% of poverty at the first interview of the panel. These children were 7 to 11 years old at the first interview. While some of these children were eligible for Medicaid under the traditional eligibility routes at the beginning of the panel, those with higher incomes never became eligible under the expansions due solely to their age. We include children

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5 Income as a percentage of poverty is calculated based on income net of standard work deductions (when appropriate) and simulated child care deductions.
Table 1
Target vs. comparison group children — characteristics of those included in the analysis files

<table>
<thead>
<tr>
<th>Variable means</th>
<th>Children in the target group</th>
<th>Children in the comparison group</th>
</tr>
</thead>
<tbody>
<tr>
<td>Health insurance coverage at 1st interview</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Share with private coverage</td>
<td>0.652</td>
<td>0.639</td>
</tr>
<tr>
<td>Share uninsured</td>
<td>0.348</td>
<td>0.361</td>
</tr>
<tr>
<td>Average income relative to poverty at 1st interview</td>
<td>1.044</td>
<td>1.070</td>
</tr>
<tr>
<td>Average change in income relative to poverty over panel (a)</td>
<td>0.432</td>
<td>0.416</td>
</tr>
<tr>
<td>Average number of children in the family</td>
<td>2.585</td>
<td>2.973***</td>
</tr>
<tr>
<td>Share with full-time adult worker in the family</td>
<td>0.877</td>
<td>0.910*</td>
</tr>
<tr>
<td>Average age of high-earning adult</td>
<td>30.634</td>
<td>35.004***</td>
</tr>
<tr>
<td>Share with high-earner having high school diploma only</td>
<td>0.423</td>
<td>0.438</td>
</tr>
<tr>
<td>Share with high-earner having some college education</td>
<td>0.354</td>
<td>0.322</td>
</tr>
<tr>
<td>Share with two-parent family</td>
<td>0.770</td>
<td>0.730</td>
</tr>
<tr>
<td>Share with white high-earner</td>
<td>0.798</td>
<td>0.768</td>
</tr>
<tr>
<td>Share living in the midwest</td>
<td>0.258</td>
<td>0.226</td>
</tr>
<tr>
<td>Share living in the south</td>
<td>0.398</td>
<td>0.437</td>
</tr>
<tr>
<td>Share living in the west</td>
<td>0.200</td>
<td>0.192</td>
</tr>
<tr>
<td>Sample size</td>
<td>1477</td>
<td>1110</td>
</tr>
</tbody>
</table>

*Expressed as income relative to poverty at last interview minus income relative to poverty at first interview.
* Difference between target and comparison means is significant at the 0.10 level.
** Difference between target and comparison means is significant at the 0.05 level.
*** Difference between target and comparison means is significant at the 0.01 level.

with incomes up to 185% of poverty rather than limiting the analysis to those with incomes up to 133% of poverty because some of these higher-income children may have become eligible for Medicaid due to declines in family income as a result of the recession.\(^6\) We included children in the target and comparison groups based on family income at the first interview.

Table 1 compares initial health insurance coverage and other socio-economic characteristics for the target- and comparison-group children used in our analyses. In most respects, the target group children and the comparison group children appear quite similar. Two statistically significant differences are that the target group children have younger parents on average than their comparison group counterparts and tend to have fewer numbers of children in their families as well.

\(^6\) This approach also allows us to increase our sample size relative to including only those children with incomes below 133% of poverty.
3.1.1. Estimation strategy

Since the ability of our approach to isolate the effects of the expansions from the effects of other factors depends critically on whether the target group would have had the same health insurance transitions in the absence of the expansions that the comparison group had, we use multivariate methods to hold constant differences in measurable family characteristics potentially associated with the ability of families to obtain and maintain private insurance coverage.7

In order to assess the extent of crowd-out, we estimate three sets of linear probability models.8 In the first set, we include only children in the target and comparison groups who had private coverage at the first interview of the panel. For these children, we estimate three separate models: the probability of having private coverage at the last interview of the survey; the probability of having Medicaid coverage at the last interview of the survey; and the probability of being uninsured at the last interview of the survey. In the second set, we include only children in the target and comparison groups who were uninsured at the first interview of the survey. For these children, we also estimate three separate models: the probability of being uninsured at the last interview of the survey; the probability of having Medicaid coverage in the last interview of the survey; and the probability of having private coverage at the last interview of the survey. Our third set of models includes all children in the target and comparison groups, regardless of initial insurance status. Again, three models are estimated: the probability of having private coverage in the last interview; the probability of having Medicaid in the last interview; and the probability of being uninsured in the last period.

We model the probability of having a given type of coverage at the last interview of the panel given the type of coverage at the first interview of the panel as:

\[
\text{Probability}(coverage_{il} = \mu|coverage_{if} = \eta) = a_1 + a_2 \text{target}_i + a_3 \text{family}_i + a_4 \text{region}_i,
\]

where: \(coverage_{il}\) is child \(i\)’s insurance coverage at the last interview of the survey and \(\mu\) can be private coverage, Medicaid coverage, or uninsurance; \(coverage_{if}\) is child \(i\)’s insurance coverage in the first interview of the survey and \(\eta\) is either private coverage or uninsurance depending upon the set of models; target\(_i\) is a binary variable indicating that child \(i\) is in the target group (as defined
previously); family is a vector of explanatory variables depicting characteristics of child \( i \)’s family; and region is a vector of binary variables indicating the region of the country in which child \( i \) resides.

Each independent variable is defined based on information collected at the first panel interview. Family characteristics included in the model are: the number of children in the family, the educational attainment of the high-earner (less than high school, high school, and at least some college), income as a percentage of poverty, presence of a full-time worker in family, having a two-parent family, and race of the high-earner. Our standard errors are based on a robust estimator of the covariance matrix (Huber, 1967; White, 1980) which allows the error terms of observations from children in the same family to be correlated.

In the context of these regressions, the coefficient on the variable ‘target’ in each of these regressions represents the difference between the target and comparison groups in the probability of being in the initial insurance state at the last interview or of having a different (specific) type of insurance in the last interview. As mentioned earlier, our approach assumes that any difference between the target and comparison groups, controlling for measurable factors, is due to the expansions. Therefore, a lower probability of having private coverage at both the first and last interviews for children in the target group relative to children in the comparison group leads to the conclusion that the expansions did, in fact, displace private insurance coverage. No differences between the target and comparison groups in the probability of having private coverage at these two points lead to the conclusion that there was no crowd-out. Similarly, a lower probability of having private coverage in the last interview, for children in the target group who began the panel uninsured relative to children in the comparison group, leads to the conclusion that there was crowd-out among the uninsured.

If we observe some crowd-out among children who started either with private coverage or who were uninsured, we can then estimate the proportion of the expansion-related movement into Medicaid that was due to private coverage displacement. To calculate the percentage of the expansion-related movement from private coverage into Medicaid that was attributable to crowd-out, we divide the coefficient on ‘target’ in the equation predicting the probability of having private coverage at both the first and last interviews by the coefficient on ‘target’ in the equation predicting the probability of movement from private coverage to Medicaid coverage. Similarly, we calculate the proportion of expansion-related movement into Medicaid from uninsurance that is attributable to crowd-out by dividing the coefficient on ‘target’ in the equation predicting the probability of movement from uninsurance to private coverage by the coefficient on ‘target’ in the equation predicting the probability of moving from uninsurance to Medicaid. 9

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9 This approach implicitly assumes that any difference between the target and comparison groups in each of the equations is due to family choices made as a result of the expansions.
We also estimate a combined model to assess the overall extent of crowd-out for children moving into the Medicaid program from either private or uninsurance. This model takes a form similar to the equations described above. Specifically, we estimate:

\[
\text{Probability}(\text{coverage}_i = \mu) = \beta_1 + \beta_2 \text{target}_i + \beta_3 \text{uninsurance}_i + \beta_4 \text{family}_i + \beta_5 \text{region}_i,
\]

where \text{uninsurance}_i is an indicator that child \(i\) was uninsured in the first period, and the other variables are as noted previously.

3.1.2. Estimation issues

Four estimation issues deserve mention. First, while there are important advantages to using longitudinal data such as the SIPP to examine crowd-out rather than cross-sectional data such as the CPS, the relatively small sample size of the SIPP and its complex sampling design reduce our power to detect small differences in outcomes and raise the chance that differences we estimate not to be statistically significant are, in fact, true differences (a Type II error). Since the small sample size of the SIPP should not lead to bias, the point estimates of the coefficients, regardless of their statistical significance, can provide insight into the direction and magnitude of any effect.

Second, we are concerned about two types of measurement error that could affect the composition of our target and comparison groups. One is error in family income measurement. Children are included in our sample based on their parent’s reported monthly income at the first interview of the SIPP. If respondents report their income with error, we may include in our target group children who were never affected by the expansions and/or exclude children who were affected. To the degree that income is generally underreported on surveys, the likely bias would be to include in our target group individuals who are not actually eligible for the expansions. This type of measurement error would bias our coefficients on ‘target’ downwards. We can net out this bias by calculating crowd-out estimates based on the ratio of the coefficients on ‘target’ for two equations (Yazici and Kaestner, forthcoming). However, we need to remember that the point estimates on target in each of the equations may be biased downwards.

The other measurement error we are concerned about stems from the fact that children who were eligible for Medicaid under pre-expansion rules are included in both our target and comparison groups. These children may have been affected by outreach efforts and improvements in the eligibility determination process that occurred as part of the expansions in coverage but were not themselves eligible for the expansions (Yazici and Kaestner, forthcoming). To the degree that these children are represented in similar proportions in both target and comparison groups and that outreach affected children of all ages similarly, the ratio of the coefficients is not biased although, again, the individual coefficients may be biased downwards. To assess the sensitivity of our results to the inclusion of children who were eligible for Medicaid under traditional eligibility routes at the first
interview in both the target and comparison groups, we estimate models that
excluded children who, in the first interview, were eligible for Medicaid due to
traditional (non-expansion) eligibility routes. In our models that sample children
who had private coverage in the first interview, both the estimated coefficients and
the estimate of crowd-out are virtually identical. In the models that sample
children who were uninsured at the first interview, the estimated coefficients and
their significance are somewhat different. We present both sets of results.

Third, while the mandates for the expansions in coverage for young children
did not begin until April 1990, many states took advantage of options to make
such children eligible for Medicaid prior to this period (Hill, 1992). Consequently,
our estimates could underestimate crowd-out due to the expansions if some
children were crowded-out prior to the beginning of the SIPP panel. We do not
think that this is a significant problem for two reasons. First, there is some
evidence that efforts to inform parents about the expansions in Medicaid coverage
for pregnant women and children lagged significantly behind expanded eligibility
(Hill, 1990; Dubay et al., 1995). Consequently, many parents whose children
were made eligible in earlier years may not have known about the expanded
program and therefore, could not have been crowded-out. Second, evidence from
Medicaid enrollment data suggests that there was in fact little expansion in
coverage of children prior to 1990. According to Health Care Financing Adminis-
tration 2082 data reported in Ellwood and Herz (1999), the total number of
children enrolled in Medicaid was only 2% higher in 1989 than in 1987. In
contrast, enrollment of children in 1990 was 7% above the 1989 enrollment level
and even larger percentage increases occurred in 1991 (14%) and in 1992 (13%).
Thus, of the 41% increase in Medicaid enrollment of children that occurred
between 1987 and 1992, all but 2% occurred during the period we analyze.
Consequently, we feel that our panel captures the vast majority of Medicaid
enrollment increases for children that occurred as a result of the expansions and
that our crowd-out estimates are unlikely to be importantly biased downwards.

The final estimation issue is the possibility that spillover effects could bias our
estimates of crowd-out downwards. If non-expansion-eligible children have expan-
sion-eligible siblings, in other words, a family could make a decision to drop
private insurance coverage for all the children, insuring those eligible through
Medicaid and leaving the others uninsured. If such a pattern were to hold,
comparing children in our target group to children in a comparison group that

10 Part of this lag was due to the fact that at that time, prominent publications by the Institute of
Medicine and the National Governors’ Association cautioned states from conducting broad-based
outreach campaigns until improvements in the eligibility determination process were made and that
there were enough providers willing to serve additional Medicaid-covered children and pregnant
women (Hill, 1990).
11 Spillover effects could also result in parents becoming uninsured; however, this effect is not
included in our estimates of crowd-out.
includes children with siblings that are eligible for the expansions might lead to contaminated estimates of the true policy effect. For example, if comparison group children with expansion-eligible siblings were more likely to move from private insurance to uninsurance than comparison group children without such siblings as a result of the expansions, the estimated difference between the target and comparison groups in the probability of having private coverage at the first and last interviews would be too low. This downward bias would occur because any decline in the probability of having private coverage at both interviews due to crowd-out for children in the target group would be offset by reductions in this probability due to spillover for children in the comparison group. Since our crowd-out estimate is derived from coefficients in this regression equation, we could underestimate the crowd-out effect if spillover were occurring.

The same type of bias could be expected in the equations modeling movement from private coverage to uninsurance at the last interview. In such a case, we would estimate a difference between the target and comparison groups that was in part attributable to the target group’s eligibility for the program and only partly attributable to the expansions’ detrimental effect upon the comparison group siblings.

In order to assess whether our estimates were, in fact, contaminated by such a spillover effect, we examined each case in our sample in which a target group child had at least one sibling in the comparison group. Of the 112 families in our sample for which there were children in the target group with siblings in the comparison group, only two families exhibited a pattern similar to the one detailed above and only one of these had their children enrolled in Medicaid due to the expansions per se. As a result of this assessment, we concluded that the presence of target-child siblings in the comparison group does not significantly contaminate our results.

4. Data sources

The SIPP’s core questionnaire (questions repeated in each wave of the interviewing process) is built around labor force participation, public program partici-
participation (e.g., Medicaid and AFDC), and income questions. It also includes information about the health insurance coverage of each person in each sample household.

The SIPP survey design is a continuous series of nationally representative panels and uses a 4-month recall period; individuals answer questions about the preceding 4 months. The 1990 panel follows individuals in 26,000 households for a period of 32 months (eight interviews). The actual initial interviews of the 1990 SIPP panel are staggered over the period February through May 1990, with one-fourth of the panel interviewed each month. Rather than use data from each month, we chose to use the data from the month immediately preceding actual interviews because analysts (Young, 1989) have found that individuals tend to report transitions as occurring during that month even if they actually occurred during an earlier month in the recall period. This phenomenon, known as seam bias, makes data from the month immediately preceding the interview more reliable than data from the other months of the recall period.

4.1. Data preparation and modeling

In order to use the SIPP data for this analysis, we create a number of new variables. First, we use data on the relationships within a household to create and characterize household units not defined on the SIPP. In particular, we created filing units (a subset of the family) for Medicaid, and health insurance units (also a subset of the family) for private insurance. We then created variables that characterize these units along a number of dimensions including family size, family type (two-parent, single-parent, child living with related family members, etc.), family income, and labor force participation of the high-earning parent. In cases where both parents have exactly the same earnings, a random parent is assigned high-earner status.

Second, since individuals can report multiple types of health insurance coverage, we instituted a hierarchy to identify the primary source of coverage for people reporting more than one type.14 We then grouped the different health insurance coverage types into four groups: private coverage (both employer-sponsored and non-group); Medicaid (including those who report both private coverage and Medicaid); other public coverage (CHAMPUS and Medicare); and uninsured. It is important to note that uninsured children are defined as those who do not report any other type of health insurance coverage.

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13 Those individuals interviewed in May 1990 regarding insurance coverage in April of 1990 could, in fact, have been eligible for the expansions in the month of April (their first interview). Because it generally takes 2 months to verify eligibility and become enrolled in the Medicaid program, we are not concerned that this issue seriously compromises our analysis.

14 This hierarchy was Medicaid and any other; employer-sponsored coverage and any other; other private coverage and any other; other public coverage; and uninsured.
4.2. Analysis file

In developing our analysis file, we first identify children born after September 30, 1983 and children born between September 30, 1978 and September 30, 1983. We then identify children living on their own and children living with unrelated persons and exclude them from the analysis. We do this in order to identify appropriate family level characteristics (e.g., family income and structure). After these exclusions and after excluding children in families with incomes above 185% of the federal poverty level and those with Medicaid coverage at the first interview, the analysis file contains 2587 children with observations at both first and last interviews of the panel. Finally, we use the longitudinal weights developed by the Census Bureau to account for any SIPP attrition bias.\textsuperscript{15}

5. Results

Table 2 provides the mean values for the dependent and independent variables used in the six models. Each dependent variable is an indicator for a child’s health insurance status at the last interview of the 1990 SIPP panel given their health insurance status at the first interview of the panel. We focus only on those children who were not enrolled in the Medicaid program (i.e., were privately insured or uninsured) prior to the time the children’s expansions were mandated, as these are the children who may have been influenced to enroll in Medicaid due to the expansions.

Seventy-eight percent of the target group children and 80% of the comparison group children who began the panel with private insurance also had private coverage at the last interview. Thirteen percent of the target group children starting in private had Medicaid coverage at the last interview while 9% was uninsured. This dynamics was reversed for the comparison group children, with 6% of those starting with private coverage having Medicaid in the last period and 14% being uninsured. Of those beginning the panel uninsured, 42% of the target children was also uninsured at the end as was 58% of the comparison group children. The remaining target group children were fairly evenly divided between those with

\textsuperscript{15}To account for the complex sampling design of the SIPP, we inflate our standard errors by a factor of 1.32. According to the SIPP documentation, estimates using these data must be adjusted by a design effect. While precise design effects for particular samples can be calculated using the SIPP cross-sectional files, the necessary information to do so is not available on the longitudinal files. The 1.32 adjustment was calculated by SIPP statisticians after personal communication. This figure takes into account the fact that our estimation was done correcting for correlations across children in the same families. This adjustment, based upon design effects for the full population, is likely high, given that other subsamples of the population for which specific design effects have been calculated are smaller than that for the full population and because we are conducting a multivariate analysis that controls for all but one of the factors used to draw the sample.
Table 2
Sample means of dependent and independent variables
Independent variables measured at first interview.

<table>
<thead>
<tr>
<th>Variable means</th>
<th>Children with private insurance at first interview</th>
<th>Children uninsured at first interview</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Target group</td>
<td>Comparison group</td>
</tr>
<tr>
<td><strong>Dependent variables</strong></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Private insurance in 1st period and private in last (0,1)</td>
<td>0.778</td>
<td>0.803</td>
</tr>
<tr>
<td>Private insurance in 1st period and Medicaid in last (0,1)</td>
<td>0.134</td>
<td>0.055</td>
</tr>
<tr>
<td>Private insurance in 1st period and uninsured in last (0,1)</td>
<td>0.088</td>
<td>0.141</td>
</tr>
<tr>
<td>Uninsured in 1st period and private insurance in last (0,1)</td>
<td></td>
<td>0.284 0.252</td>
</tr>
<tr>
<td>Uninsured in 1st period and Medicaid in last (0,1)</td>
<td></td>
<td>0.292 0.169</td>
</tr>
<tr>
<td>Uninsured in 1st period and uninsured in last (0,1)</td>
<td></td>
<td>0.424 0.576</td>
</tr>
<tr>
<td><strong>Independent variables</strong></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Income relative to poverty</td>
<td>1.177</td>
<td>1.195</td>
</tr>
<tr>
<td>Number of children in family</td>
<td>2.628</td>
<td>3.029</td>
</tr>
<tr>
<td>There is a full-time adult worker in the family (0,1)</td>
<td>0.932</td>
<td>0.936</td>
</tr>
<tr>
<td>Age of high-earning adult</td>
<td>30.873</td>
<td>35.077</td>
</tr>
<tr>
<td>High-earner has high school diploma only (0,1)</td>
<td>0.440</td>
<td>0.444</td>
</tr>
<tr>
<td>High-earner has some college education (0,1)</td>
<td>0.400</td>
<td>0.373</td>
</tr>
<tr>
<td>Child is in a two-parent family (0,1)</td>
<td>0.796</td>
<td>0.735</td>
</tr>
<tr>
<td>High-earner’s race is white (0,1)</td>
<td>0.798</td>
<td>0.774</td>
</tr>
<tr>
<td>Child lives in the midwest (0,1)</td>
<td>0.280</td>
<td>0.250</td>
</tr>
<tr>
<td>Child lives in the south (0,1)</td>
<td>0.365</td>
<td>0.387</td>
</tr>
<tr>
<td>Child lives in the west (0,1)</td>
<td>0.178</td>
<td>0.195</td>
</tr>
<tr>
<td>Sample size</td>
<td>968</td>
<td>717</td>
</tr>
</tbody>
</table>

private insurance and those with Medicaid at the last interview. Of the comparison group children, 25% who started the panel uninsured had private coverage at the end and 17% had Medicaid coverage.

Of the group starting with private insurance, 57% was in the target group. Of the group starting out uninsured, 56% was in the target group. The private group had somewhat higher incomes, more children on average, and more highly educated heads of household. Children beginning the panel uninsured were more likely to live in the south than children with private insurance.
5.1. Insurance status at last interview conditional on having private insurance coverage at first interview

Table 3 provides the results for the set of models that includes only those children who had private insurance at the first interview. Being in the target group (children within the age and income constraints for Medicaid expansion eligibility) is negatively associated with having private insurance in both the first and last interviews of the panel ($\beta = -0.0122$). This implies that there is a difference in probability of $-1.2$ percentage points between the target and comparison groups. The negative coefficient on the target variable implies that some children may have left private coverage as a simple result of the expansions. The estimated difference was not, however, statistically significant, which may be an artifact of the low statistical power noted earlier.  

Being in the target group increases the probability that a child will make a private-to-Medicaid transition by 5.3 percentage points, an effect that is statistically significant at the 0.01 level. Being in the target group lowers the probability of a transition from private insurance to uninsurance at the end of the panel by 3.9 percentage points, a difference that is not statistically significant.

To summarize the results of the first set of multivariate models, being a child in the age and income groups targeted by the Medicaid expansions:
- lowers the probability that a child with private coverage at the first interview also has private coverage at the last interview;
- increases the probability that a child with private coverage at the first interview has Medicaid coverage at the last interview; and
- decreases the probability that a child with private coverage at the first interview is uninsured at the last interview.

The negative coefficient on ‘target’ in the equation predicting whether a child would have private coverage in the first and last interviews of the survey indicates that some displacement of private coverage may have occurred for those children who had private coverage at the beginning of the panel. How important is this displacement? Clearly, not all transitions from private coverage to Medicaid coverage during this period were attributable to ‘crowd-out’. Some children who

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16 Given our sample size, in order to have an 80% probability of detecting a significant difference between the target and comparison groups in the probability of being in private in the last period, the actual difference between the two populations would have to be at least 5.1 percentage points in comparison to the current 1.2 percentage points. In order to have an 80% probability of detecting a difference of 1.2 percentage points as being statistically significant, the sample size for the target and comparison groups would have to be approximately 17,000.

17 The 3 percentage point differences across the three models do not sum exactly to zero because there were a small number of private coverage to non-Medicaid public coverage transitions (e.g., Medicare, CHAMPUS). A regression for private to other public coverage transitions was not included here due to the very small number of such transitions.
Table 3
Results of linear probability models including children with private insurance at first interview

Number of observations is 1685.
Sample used in estimation includes those children in the target and comparison groups who had private insurance at the first interview.
Standard errors adjusted for clustering within family units and SIPP complex sampling design.
Independent variables are measured at first interview.
Income relative to poverty reflects definition of income used to determine Medicaid eligibility (i.e., income less disregards).

<table>
<thead>
<tr>
<th>Independent variables</th>
<th>Did child have private insurance in first and last periods* (0,1)</th>
<th>Did child have private insurance in first period and Medicaid (any kind) in last period* (0,1)</th>
<th>Did child have private insurance in first period and no insurance (measured) in last period* (0,1)</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Coefficient</td>
<td>Standard error</td>
<td>Coefficient</td>
</tr>
<tr>
<td>Child is a member of the target group (0,1)</td>
<td>-0.0122</td>
<td>0.0293</td>
<td>0.0531***</td>
</tr>
<tr>
<td>Income relative to poverty</td>
<td>0.1333***</td>
<td>0.0450</td>
<td>-0.0858**</td>
</tr>
<tr>
<td>Number of children in family</td>
<td>0.0036</td>
<td>0.0164</td>
<td>0.0116</td>
</tr>
<tr>
<td>There is a full-time adult worker in the family (0,1)</td>
<td>0.0095</td>
<td>0.0751</td>
<td>0.0074</td>
</tr>
<tr>
<td>Age of high-earning adult</td>
<td>0.0048</td>
<td>0.0034</td>
<td>-0.0078***</td>
</tr>
<tr>
<td>High-earner has high school diploma only (0,1)</td>
<td>0.0536</td>
<td>0.0674</td>
<td>-0.0551</td>
</tr>
<tr>
<td>High-earner has some college education (0,1)</td>
<td>0.1403***</td>
<td>0.0656</td>
<td>-0.0840</td>
</tr>
<tr>
<td>Child is in a two-parent family (0,1)</td>
<td>0.0445</td>
<td>0.0322</td>
<td>0.0055</td>
</tr>
<tr>
<td>High-earner’s race is white (0,1)</td>
<td>0.0624</td>
<td>0.0586</td>
<td>-0.0309</td>
</tr>
<tr>
<td>Child lives in the midwest (0,1)</td>
<td>-0.0234</td>
<td>0.0464</td>
<td>0.0139</td>
</tr>
<tr>
<td>Child lives in the south (0,1)</td>
<td>-0.1451***</td>
<td>0.0407</td>
<td>0.0472</td>
</tr>
<tr>
<td>Child lives in the west (0,1)</td>
<td>-0.0593</td>
<td>0.0329</td>
<td>0.0250</td>
</tr>
<tr>
<td>Constant</td>
<td>0.3716***</td>
<td>0.1876</td>
<td>0.4364***</td>
</tr>
</tbody>
</table>

* Coefficient to the right is significant at the 0.10 level.
** Coefficient to the right is significant at the 0.05 level.
*** Coefficient to the right is significant at the 0.01 level.
moved into Medicaid from private coverage would have been uninsured in the absence of the expansions, as the recession and other unrelated trends in health insurance coverage which occurred over the same period decreased the overall level of private health insurance coverage.

To answer this question, we divide the decreased probability attributable to the expansions that a child would have private coverage in both the first and last periods by the increased probability (also attributable to the expansions) that a child would have private coverage in the first period but Medicaid coverage in the last period. The resulting calculation, which uses the probabilities shown in Table 3 (–0.0122/0.0531 = –0.2298), implies that 23% of these private-insurance-to-Medicaid transitions attributable to the expansions was made by children who otherwise would have had private insurance coverage (standard error = 0.55). This estimate focuses exclusively on change over a 28-month period, ignoring short-term transitions that may have occurred in the interim or following the end of the panel. Thus, even if the estimated coefficients on the ‘target’ variable reflect a real crowd-out effect that the small sample size is preventing from being statistically significant, the magnitude of the point estimate is relatively small (i.e., 77% of the private coverage to Medicaid transitions was not attributable to those who would have been privately insured in the absence of the expansions).

5.2. Insurance status at last interview conditional on being uninsured at first interview

Table 4 provides the results for children who were uninsured at the first interview. Being in the target group implies a higher probability of moving from uninsurance to private coverage (β = 0.0108), a relationship that is not statistically significant but certainly gives no sign of any crowd-out. The fact that target children were not less likely to move out of uninsurance and into private coverage (and might be more likely to do so) indicates that uninsured children were not opting for Medicaid coverage as opposed to moving into private insurance. The probability of moving from being uninsured to Medicaid coverage was also higher for those in the target group, as one would expect, with a difference of 7.9 percentage points between the target and comparison groups which is statistically significant at the 0.05 level. Being in the target group lowers the probability that a child uninsured at the first interview was uninsured at the last (β = −0.0861), a modestly significant result.

To summarize the results of the second set of multivariate models, being a child in the age and income groups targeted by the Medicaid expansions:
- increases the probability that a child who was uninsured at the first interview had private coverage at the last interview;
- increases the probability that a child who was uninsured at the first interview had Medicaid at the last interview; and
- decreases the probability that a child who was uninsured at the first interview was also uninsured at the last interview.
<table>
<thead>
<tr>
<th>Independent variables</th>
<th>Coefficient Uninsured in First Period</th>
<th>Coefficient Uninsured in First Period</th>
<th>Coefficient Uninsured in the Last Period</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Standard error</td>
<td>Standard error</td>
<td>Standard error</td>
</tr>
<tr>
<td>Child is a member of the target group (0,1)</td>
<td>0.0108</td>
<td>0.0411</td>
<td></td>
</tr>
<tr>
<td>Income relative to poverty (0,1)</td>
<td>0.128**</td>
<td>0.0763</td>
<td>-0.0108</td>
</tr>
<tr>
<td>Number of children in family</td>
<td>-0.0298</td>
<td>0.0283</td>
<td>0.0366</td>
</tr>
<tr>
<td>There is a full-time adult worker in the family (0,1)</td>
<td>-0.0085</td>
<td>0.0835</td>
<td>-0.0675</td>
</tr>
<tr>
<td>Age of high-earning adult</td>
<td>-0.0017</td>
<td>0.0041</td>
<td>-0.0096**</td>
</tr>
<tr>
<td>High-earner has high school diploma only (0,1)</td>
<td>0.1315**</td>
<td>0.0620</td>
<td>0.0102</td>
</tr>
<tr>
<td>High-earner has some college education (0,1)</td>
<td>0.2696***</td>
<td>0.0940</td>
<td>-0.1408*</td>
</tr>
<tr>
<td>Child is in a two-parent family (0,1)</td>
<td>0.0072</td>
<td>0.0750</td>
<td>-0.1036</td>
</tr>
<tr>
<td>Child lives in the midwest (0,1)</td>
<td>0.1462</td>
<td>0.1000</td>
<td>-0.0200</td>
</tr>
<tr>
<td>Child lives in the south (0,1)</td>
<td>0.1593*</td>
<td>0.0824</td>
<td>-0.1290</td>
</tr>
<tr>
<td>Child lives in the west (0,1)</td>
<td>0.0927</td>
<td>0.1029</td>
<td>-0.0806</td>
</tr>
<tr>
<td>Constant</td>
<td>-0.0184</td>
<td>0.1762</td>
<td>0.0186**</td>
</tr>
</tbody>
</table>

* Coefficient to the right is significant at the 0.10 level.
** Coefficient to the right is significant at the 0.05 level.
*** Coefficient to the right is significant at the 0.01 level.
These estimates provide no support for the conclusion that the Medicaid expansions displaced private coverage for those children who were uninsured prior to the implementation of the program mandates. In fact, there is some weak evidence that those initially uninsured in the target group transitioned to private coverage in greater proportions than their comparison group counterparts.

5.3. Combined effect for children in private coverage or uninsured at first interview

We next estimated a set of models which would generate a combined estimate of the effects of the expansions on private coverage for children who had either private coverage or were uninsured the first interview of the SIPP panel. The results of these models are presented in Table 5. Being in the target group has a negative but statistically insignificant effect on the probability that a child had private insurance in the last period. The difference in probability between the target and comparison groups was approximately 0.3 percentage points. Being in the target group had a positive and highly significant effect on the probability that a child would have Medicaid coverage in the last period of the panel, with the difference in probability equal to 6.3 percentage points. In addition, being in the target group had a negative and significant effect on the probability that a child would be uninsured at the end of the SIPP panel, with target children being 5.6 percentage points less likely to be uninsured than the comparison children.

Taking the ratio of the coefficients for the target variables in the first two equations, as we have done in the previous set of models, yields an overall estimate of the Medicaid expansion displacement effect. In this way, we estimate that 4.4% (−0.0028/0.0633) of the children who moved into Medicaid from either private coverage or uninsurance over the course of the SIPP panel would have had private coverage in the absence of the expansions (standard error is 0.38). This result, in effect, averages the two independent results calculated...

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18 In order to increase our sample size, we considered including children up to age 13 in our analysis. However, there are differences in health care utilization patterns and the consequent demand for health insurance between younger and older children. Therefore, we would expect that the older the children included in the comparison group, the less comparable the two groups would be. In addition, we want to avoid including pregnant teenagers in our comparison group because they were affected by the Medicaid expansions for pregnant women. The older the child, the higher the probability of pregnancy. We did, however, test the sensitivity of our results to the inclusion of children who were 13 and younger at the start of the panel. This change leads to an overall crowd-out estimate of 14.9% as compared to 4.4%. The crowd-out effect for the group beginning the panel in private coverage was −40.7% (−0.024/0.0589, as compared to 23%); the effect for the group beginning the panel uninsured was +14.7% (0.014/0.092, as compared to 13.7%). Neither of the coefficients constituting the numerator of these two ratios was statistically significant. The sample size of the comparison group beginning the panel with private coverage was 319 observations larger than for the main results presented in this paper. The comparison group beginning the panel uninsured increased by 168 relative to the main results presented.
Table 5
Results of linear probability models including children with private insurance or uninsured at first interview
Number of observations is 2587.
Sample used in estimation includes those children in the target and comparison groups who had private insurance or were uninsured at the first interview.
Standard errors adjusted for clustering within family units and SIPP complex sampling design.
Independent variables are measured at first interview.
Income relative to poverty reflects definition of income used to determine Medicaid eligibility (i.e., income less disregards).

<table>
<thead>
<tr>
<th>Independent variables</th>
<th>Did child have private insurance in the last period? (0,1)</th>
<th>Did child have Medicaid (any kind) in last period? (0,1)</th>
<th>Was child uninsured in the last period? (0,1)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Child is a member of the target group (0,1)</td>
<td>-0.0028 0.0246</td>
<td>0.063*** 0.0197</td>
<td>-0.057*** 0.0250</td>
</tr>
<tr>
<td>Child was uninsured at first interview (0,1)</td>
<td>-0.4312*** 0.0445</td>
<td>0.0803* 0.0421</td>
<td>0.3513*** 0.0425</td>
</tr>
<tr>
<td>Income relative to poverty</td>
<td>0.1308*** 0.0410</td>
<td>-0.0907*** 0.0386</td>
<td>-0.0389 0.0363</td>
</tr>
<tr>
<td>Number of children in family</td>
<td>-0.0047 0.0142</td>
<td>0.0197 0.035</td>
<td>0.0359 0.0148</td>
</tr>
<tr>
<td>There is a full-time adult worker in the family (0,1)</td>
<td>-0.0195 0.0537</td>
<td>-0.0434 0.0509</td>
<td>0.0664 0.0544</td>
</tr>
<tr>
<td>Age of high-earner adult</td>
<td>0.0025 0.0027</td>
<td>-0.0088** 0.0023</td>
<td>0.0665*** 0.0024</td>
</tr>
<tr>
<td>High-earner has high school diploma only (0,1)</td>
<td>0.0920* 0.0480</td>
<td>-0.0822 0.0517</td>
<td>-0.0114 0.0464</td>
</tr>
<tr>
<td>High-earner has some college education (0,1)</td>
<td>0.1986*** 0.0521</td>
<td>-0.119*** 0.0522</td>
<td>-0.0816* 0.0476</td>
</tr>
<tr>
<td>Child is in a two-parent family (0,1)</td>
<td>0.0225 0.0440</td>
<td>-0.0384 0.0367</td>
<td>0.0036 0.0395</td>
</tr>
<tr>
<td>High-earner’s race is white (0,1)</td>
<td>0.0664 0.0464</td>
<td>-0.0334 0.0396</td>
<td>-0.0347 0.0412</td>
</tr>
<tr>
<td>Child lives in the midwest (0,1)</td>
<td>0.0152 0.0462</td>
<td>0.0127 0.0401</td>
<td>-0.0276 0.0400</td>
</tr>
<tr>
<td>Child lives in the south (0,1)</td>
<td>-0.0383 0.0456</td>
<td>-0.0082 0.0392</td>
<td>0.0661 0.0421</td>
</tr>
<tr>
<td>Child lives in the west (0,1)</td>
<td>-0.0358 0.0523</td>
<td>-0.0019 0.0512</td>
<td>0.0373 0.0493</td>
</tr>
<tr>
<td>Constant</td>
<td>0.4156*** 0.1219</td>
<td>0.5839** 0.1137</td>
<td>-0.0044 0.1113</td>
</tr>
</tbody>
</table>

*Coefficient to the right is significant at the 0.10 level.
**Coefficient to the right is significant at the 0.05 level.
***Coefficient to the right is significant at the 0.01 level.
earlier from the two separate samples conditional on initial insurance coverage. Consistent with those results, this overall estimate takes into account both the negative and insignificant effect seen from the sample of children beginning the panel in private coverage and the positive and insignificant effect seen from the sample of children beginning the panel uninsured.

5.4. Children with Medicaid eligibility under non-expansion rules at first interview

Some children in our sample would have been eligible for Medicaid at the first interview even in the absence of the expansions through the Ribicoff, DEFRA or medically needy provisions.\textsuperscript{19} To the extent that these children behave differently from the (higher-income) children eligible for Medicaid due only to the expansions — and to the extent that they are differentially represented among the target and comparison groups — their inclusion could affect the direct applicability of our results to the Medicaid expansions themselves. For example, the fact that these children were eligible under previous Medicaid rules but did not participate in the program may simply indicate that their families do not have strong preferences regarding insurance coverage. We tested the sensitivity of our results to the inclusion of these children by performing a re-estimation without them.

This re-estimation had virtually no effect on the probability of any particular last period status for those with private insurance at the first interview (see Table 6), nor did it affect the probability that a child would move from being uninsured to having private insurance over the 28-month period. However, the re-estimation did make a difference in the estimated probabilities of having Medicaid or being uninsured in the last period for those who were uninsured at the first interview (see Table 7).

The probability that a child in the target group would move from being uninsured at the first interview to having Medicaid at the last period increased from a $\beta = 0.0791$ (significant at the 0.05 level) for the broader sample of children to $\beta = 0.1190$ (significant at the 0.01 level) for the smaller sample of children. This implies that children who were uninsured and non-Medicaid-eligible at the first interview and became eligible through the Medicaid expansions were 12 percentage points more likely to have Medicaid at the last interview than similar children in the comparison group.

Similarly, the smaller sample estimates imply that children in the expansion-eligible group were 14 percentage points less likely than the comparison group children to be uninsured at both the first and the last interviews, a result which was

\textsuperscript{19} For a full discussion of the legislative history and details regarding these other eligibility routes, see Congressional Research Service (1993).
Table 6
Results of linear probability models including children who had private coverage at first interview: sample excludes children eligible for Medicaid through non-expansion routes of eligibility at first interview.
Number of observations is 1502.
Sample used in estimation includes those children in the target and comparison groups who had private coverage at the first interview.
Standard errors adjusted for clustering within family units and SIPP complex sampling design.
Independent variables are measured at first interview.
Income relative to poverty reflects definition of income used to determine Medicaid eligibility (i.e., income less disregards).

<table>
<thead>
<tr>
<th>Independent variables</th>
<th>Coefficient</th>
<th>Standard error</th>
<th>Coefficient</th>
<th>Standard error</th>
<th>Coefficient</th>
<th>Standard error</th>
</tr>
</thead>
<tbody>
<tr>
<td>Child is a member of the target group (0, 1)</td>
<td>-0.0117</td>
<td>0.0313</td>
<td>0.0517**</td>
<td>0.0216</td>
<td>-0.0374</td>
<td>0.0274</td>
</tr>
<tr>
<td>Income relative to poverty</td>
<td>0.1688***</td>
<td>0.0591</td>
<td>-0.0858*</td>
<td>0.0477</td>
<td>-0.0786*</td>
<td>0.0452</td>
</tr>
<tr>
<td>Number of children in family</td>
<td>0.0108</td>
<td>0.0167</td>
<td>0.0086</td>
<td>0.0164</td>
<td>-0.0196*</td>
<td>0.0105</td>
</tr>
<tr>
<td>There is a full-time adult worker in the family (0, 1)</td>
<td>-0.0682</td>
<td>0.0737</td>
<td>0.0348</td>
<td>0.0450</td>
<td>0.0488</td>
<td>0.0610</td>
</tr>
<tr>
<td>Age of high-earning adult</td>
<td>0.0029</td>
<td>0.0036</td>
<td>-0.0065*</td>
<td>0.0027</td>
<td>0.0037</td>
<td>0.0027</td>
</tr>
<tr>
<td>High-earner has high school diploma only (0, 1)</td>
<td>0.0463</td>
<td>0.0740</td>
<td>-0.0709</td>
<td>0.0646</td>
<td>0.0244</td>
<td>0.0549</td>
</tr>
<tr>
<td>High-earner has some college education (0, 1)</td>
<td>0.1422**</td>
<td>0.0715</td>
<td>-0.1017*</td>
<td>0.0626</td>
<td>-0.0368</td>
<td>0.0504</td>
</tr>
<tr>
<td>Child is in a two-parent family (0, 1)</td>
<td>0.0324</td>
<td>0.0557</td>
<td>-0.0027</td>
<td>0.0428</td>
<td>-0.0309</td>
<td>0.0462</td>
</tr>
<tr>
<td>High-earner’s race is white (0, 1)</td>
<td>0.0461</td>
<td>0.0619</td>
<td>-0.0104</td>
<td>0.0511</td>
<td>-0.0374</td>
<td>0.0490</td>
</tr>
<tr>
<td>Child lives in the Midwest (0, 1)</td>
<td>-0.0328</td>
<td>0.0467</td>
<td>0.0372</td>
<td>0.0319</td>
<td>-0.0040</td>
<td>0.0344</td>
</tr>
<tr>
<td>Child lives in the South (0, 1)</td>
<td>-0.1459**</td>
<td>0.0520</td>
<td>0.0385*</td>
<td>0.0349</td>
<td>0.0842**</td>
<td>0.0419</td>
</tr>
<tr>
<td>Child lives in the West (0, 1)</td>
<td>-0.0555</td>
<td>0.0545</td>
<td>0.0320</td>
<td>0.0379</td>
<td>0.0237</td>
<td>0.0404</td>
</tr>
<tr>
<td>Constant</td>
<td>0.4977**</td>
<td>0.1863</td>
<td>0.3663**</td>
<td>0.1729</td>
<td>0.1342</td>
<td>0.1314</td>
</tr>
</tbody>
</table>

*Coefficient to the right is significant at the 0.10 level.
**Coefficient to the right is significant at the 0.05 level.
***Coefficient to the right is significant at the 0.01 level.
Table 7
Results of linear probability models including children who were uninsured at first interview: sample excludes children eligible for Medicaid through non-expansion routes of eligibility at first interview

Number of observations is 650.
Sample used in estimation includes those children in the target and comparison groups who were uninsured at the first interview.
Standard errors adjusted for clustering within family units and SIPP complex sampling design.
Independent variables are measured at first interview.
Income relative to poverty reflects definition of income used to determine Medicaid eligibility (i.e., income less disregards).

<table>
<thead>
<tr>
<th>Independent variables</th>
<th>Coefficient</th>
<th>Standard error</th>
<th>Coefficient</th>
<th>Standard error</th>
<th>Coefficient</th>
<th>Standard error</th>
</tr>
</thead>
<tbody>
<tr>
<td>Child is a number of the target group (0,1)</td>
<td>0.0194</td>
<td>0.0519</td>
<td>0.1190**</td>
<td>0.0396</td>
<td>-0.1384**</td>
<td>0.0565</td>
</tr>
<tr>
<td>Income relative to poverty</td>
<td>0.0624</td>
<td>0.1086</td>
<td>-0.0377</td>
<td>0.0905</td>
<td>-0.0130</td>
<td>0.0337</td>
</tr>
<tr>
<td>Number of children in family</td>
<td>-0.0288</td>
<td>0.0386</td>
<td>0.0418*</td>
<td>0.0243</td>
<td>-0.0246</td>
<td>0.1148</td>
</tr>
<tr>
<td>There is a full-time adult worker in the family (0,1)</td>
<td>-0.0263</td>
<td>0.1416</td>
<td>-0.0672</td>
<td>0.1051</td>
<td>0.0933</td>
<td>0.1267</td>
</tr>
<tr>
<td>Age of high-earning adult</td>
<td>-0.0042</td>
<td>0.0064</td>
<td>-0.0057</td>
<td>0.0046</td>
<td>0.0999</td>
<td>0.0661</td>
</tr>
<tr>
<td>High-earner has high school diploma only (0,1)</td>
<td>0.1066</td>
<td>0.0932</td>
<td>-0.0194</td>
<td>0.0924</td>
<td>-0.0272</td>
<td>0.1028</td>
</tr>
<tr>
<td>High-earner has some college education (0,1)</td>
<td>0.2580**</td>
<td>0.1170</td>
<td>-0.1075</td>
<td>0.1173</td>
<td>-0.1506</td>
<td>0.1302</td>
</tr>
<tr>
<td>Child is in a two-parent family (0,1)</td>
<td>-0.0108</td>
<td>0.1027</td>
<td>-0.0692</td>
<td>0.0780</td>
<td>0.0800</td>
<td>0.0904</td>
</tr>
<tr>
<td>High-earner’s race is white (0,1)</td>
<td>0.0929</td>
<td>0.0960</td>
<td>-0.0427</td>
<td>0.0717</td>
<td>-0.0502</td>
<td>0.1017</td>
</tr>
<tr>
<td>Child lives in the midwest (0,1)</td>
<td>0.1375</td>
<td>0.1208</td>
<td>-0.0317</td>
<td>0.1329</td>
<td>-0.1058</td>
<td>0.1463</td>
</tr>
<tr>
<td>Child lives in the south (0,1)</td>
<td>0.2057**</td>
<td>0.1022</td>
<td>-0.1476</td>
<td>0.1174</td>
<td>-0.0581</td>
<td>0.1318</td>
</tr>
<tr>
<td>Child lives in the west (0,1)</td>
<td>0.0949</td>
<td>0.1318</td>
<td>-0.0740</td>
<td>0.1768</td>
<td>-0.0209</td>
<td>0.1770</td>
</tr>
<tr>
<td>Constant</td>
<td>0.1575</td>
<td>0.2989</td>
<td>0.5058**</td>
<td>0.2504</td>
<td>0.3367</td>
<td>0.3051</td>
</tr>
</tbody>
</table>

*Coefficient to the right is significant at the 0.10 level.
**Coefficient to the right is significant at the 0.05 level.
***Coefficient to the right is significant at the 0.01 level.
significant (at the 0.05 level). The magnitude of this effect was roughly 60% higher than that of the effect estimated with the larger sample.

To summarize, limiting the sample to those children who were either not eligible for Medicaid at the first interview or who were eligible only as a result of the expansions strengthens two of our major conclusions. First, it increases the probability of expansion-eligible children who were uninsured at the first interview moving to Medicaid. Second, it decreases the probability that expansion-eligible children who were uninsured at the beginning were still uninsured at the end of the 28-month period.

6. Conclusions

In this analysis, we use longitudinal data from the 1990 panel of the SIPP to estimate the extent of crowd-out resulting from the Medicaid expansions for children implemented during the early 1990s. We find that 23% of the expansion-related movement from private coverage to the Medicaid program was attributable to families choosing Medicaid coverage who would have been covered by private insurance in the absence of the expansions. We find no evidence that the Medicaid expansions encouraged families with uninsured children to enroll their children in Medicaid rather than take up private coverage. These results imply that the primary impact of the Medicaid expansions was to prevent low-income children from becoming or remaining uninsured, not to crowd-out private insurance.

This paper raises important questions about the feasibility of using currently available longitudinal data to assess the extent to which expansions of public programs that affect only a small portion of the population displace private insurance coverage. While the SIPP is the largest nationally representative longitudinal dataset that can be used to study this issue either under the Medicaid expansions or under the Children’s Health Insurance Program (CHIP), as we have shown, the sample sizes are small relative to cross-sectional datasets such as the CPS. However, the longitudinal nature of the SIPP provides an important advantage in examining the issue of crowd-out relative to the CPS because movement between different insurance states can be observed. Consequently, there are certain questions that cannot be answered at all using cross-sectional data. The small sample size and complex sampling design of the SIPP reduce our statistical power to identify what may be true effects in our analysis. However, we were able to detect statistically significant results in a number of the models presented here, making clear that the dataset is useful in addressing at least some of the issues raised with regard to crowd-out. Plus, there are no obvious reasons why we should believe that the smaller SIPP sample is biased with regard to the transitions of interest. In addition, if similar analyses using different longitudinal databases yield consistent results, we can have more confidence in the robustness of the estimates.
we present here. Consequently, while we do not consider these results to be definitive, we feel they are a relevant contribution to the ongoing debate regarding the interplay between public and private insurance coverages.

6.1. Relevance for CHIP

With regard to current policy issues and debates, it is difficult to extrapolate the results of this analysis to the new CHIP due to three programmatic differences. First, the fiscal implications of crowd-out under the CHIP program are likely to be greater than under the Medicaid expansions. This is because children eligible for CHIP will, by definition, have higher family incomes than children eligible for the Medicaid expansions in their state. As income increases, the probability that a child has private insurance increases and thus, the probability that a child is uninsured decreases. Therefore, even if children eligible under CHIP leave private coverage at the same rate as children under the expansions did, and even if uninsured children participate at the same rate as uninsured children eligible under the expansions, the proportion of new entrants into the program who previously had private coverage is likely to be higher than under the expansions.

The second potential difference is the structure of CHIP programs. States have the option to implement their CHIP coverage expansions through Medicaid or through other state-designed plans. Depending upon the structural program choices that states make, the participation rates among both the previously privately insured and the uninsured could vary substantially from those under the expansions, with important implications for the magnitude of displacement. The third difference is that states are required to develop strategies for reducing crowd-out under the CHIP program. The effectiveness of these preventive approaches is, of course, still unknown.

The crowd-out issue has the opportunity to focus the debate over how society will evaluate the success of public insurance programs. On one hand is the goal of minimizing the public cost per newly insured individual (target efficiency). On the other hand is the goal of providing public income support in such a way that people in similar economic circumstances receive similar levels of assistance (horizontal equity). The concern with crowd-out, per se, touches only upon the

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20 In fact, our results are consistent with those produced by Thorpe and Florence using the NLSY, although not directly comparable since Thorpe and Florence examine all movements into Medicaid while we examine expansion-related movement into Medicaid. A rough check on the consistency of the two studies can be made by focusing on movements from private coverage to Medicaid coverage for children with incomes above 100% of poverty (who would have only been eligible due to the expansions) during the 1990 to 1992 period in the Thorpe and Florence study. Thorpe and Florence estimate that 19% of the movement into Medicaid from private coverage was attributable to crowd-out. This is quite similar to our estimate that 23% of the expansion-related movement from private coverage to Medicaid was attributable to the displacement of private coverage.
efficiency with which a program targets public dollars to the previously uninsured. While target efficiency is a relevant and important component of judging the cost-effectiveness of particular programs, it is not the only criterion against which new programs need to be evaluated.

The attention that the crowd-out debate has engendered at both the state and federal levels highlights the need for explicit prioritization of public insurance program goals and the design of program evaluations consistent with those priorities. Unless the often-competing goals of target efficiency of public dollars, equity of income redistribution, high participation rates, access to high quality medical care, and continuity of insurance coverage are considered relative to one another, not in isolation, we cannot hope to design public programs to meet the health care needs society has identified.

Acknowledgements

This work was funded by a grant from the Robert Wood Johnson Foundation. The authors are grateful for the helpful advice of John Holahan, Robert Kaestner, Genevieve Kenney, John Marcotte, Len Nichols, Doug Wissoker, and Steve Zuckerman. In addition, we appreciate the suggestions and guidance of David Cutler, Jonathon Gruber, Joseph Newhouse, and two anonymous referees during the review process.

References


