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# Public program crowd-out of private coverage: What are the issues?

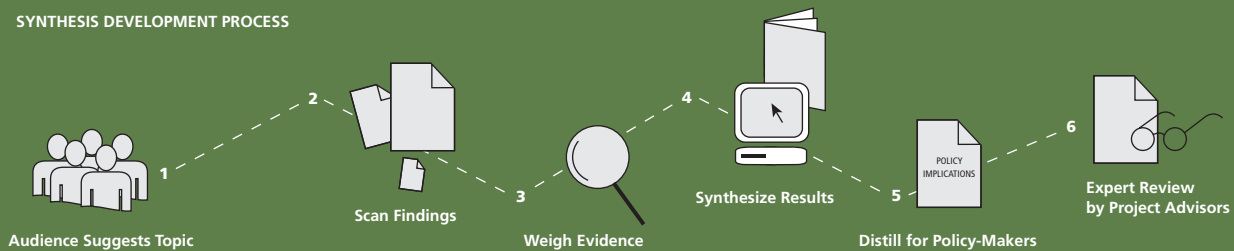
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### SYNTHESIS DEVELOPMENT PROCESS



# Introduction

The Medicaid, State Children’s Health Insurance Program (SCHIP), and state-initiated coverage expansions of the 1980s and 1990s were all undertaken to advance the basic policy goals of increasing health coverage among low-income children and their families and improving their access to health care. As a result of these initiatives, enrollment in public programs has grown significantly, raising concerns among some policy-makers about the extent to which public coverage is “crowding out” private health insurance. Although low-income families may benefit from such substitution, the cost of achieving a reduction in the number of uninsured rises, as both those previously uninsured and those who would otherwise be privately insured receive subsidies. Thus, the existence of “crowd-out” has important implications for the cost, efficiency and efficacy of public coverage initiatives.

Crowd-out is difficult to identify because not all substitution of public for private coverage constitutes crowd-out. The phenomenon arises only if the actions taken—people substituting public for private coverage, or employers changing their insurance offerings—would not have occurred in the absence of the public program. If people would otherwise have become uninsured, enrolling in a public program does not constitute crowd-out.

This synthesis examines the evidence on crowd-out and its implications for future policies to expand coverage. The substantial literature on the subject presents confusing and conflicting estimates for policy-makers seeking a clear answer to the question of how much crowd-out exists. That question, we conclude, has no single answer. Crowd-out rates vary depending on the characteristics of the program, changing economic conditions and differences in the way researchers measure the phenomenon—although some program features and individual characteristics appear to increase the likelihood of crowd-out occurring. Moreover, policy-makers face important trade-offs between the goals of limiting crowd-out and expanding coverage.

## The synthesis addresses the following key questions:

- How does crowd-out occur?
- Why is measuring crowd-out so difficult?
- How prevalent is crowd-out?
- What factors influence the amount of crowd-out?
- How effective are policies aimed at limiting crowd-out?
- What are the policy trade-offs between expanding coverage and reducing crowd-out?

# Findings

## How does crowd-out occur?

**Crowd-out has been examined for three major public program expansions occurring over the past 25 years.**

- The Medicaid program expansions of the late 1980s and early 1990s that targeted young children and pregnant women in poor and other low-income households.<sup>1</sup>
- State-initiated programs implemented in a small number of states in the early- to mid-1990s that targeted children and adults in low-income families and, in some cases, those at somewhat higher income levels.
- The State Children’s Health Insurance Program (SCHIP) initiatives of the late 1990s that targeted children in families with household incomes between 100 and 200 percent of the federal poverty level (FPL), and above 200 percent in some states.

**For any of these expansions, three major crowd-out pathways are possible.**

- *An individual or family drops private coverage for public coverage.* In this situation, a family or an individual drops private health insurance coverage—or does not purchase private health insurance—in order to enroll in a public program. Without the availability of the public program the family or individual would have kept or taken up that private coverage.
- *An enrollee in a public program refuses an offer of private coverage.* This type of crowd-out occurs if a public program enrollee opts to stay in that program, despite having an offer of private coverage that they would have accepted in the absence of the program.
- *Employers induce crowd-out.* Crowd-out can also occur if employers take actions—which would not have been taken in the absence of a public program expansion—that either force or encourage their employees to drop their coverage in favor of the public program. Examples include no longer offering coverage to their employees, declining to begin offering coverage, or increasing employee premium contributions or cost-sharing requirements.

As any of those actions can be undertaken for reasons that are independent of public programs, researchers cannot easily identify individual cases of crowd-out. Accordingly, they use statistical means, and aggregate data, to identify how much of the observed change in public and private coverage rates can be directly attributable to public program expansions.

## Why is measuring crowd-out so difficult?

**Determining whether observed changes in private coverage are directly related to program expansions is inherently difficult.**

- The majority of private health insurance is obtained through employment and, over any given time period, rates of private coverage will change due to the effects of business cycles, changes in employers’ decisions to offer coverage, changes in the affordability of that coverage and shifts in the importance that employees place on having health insurance. The acceptability of public coverage also changes over time, which may make enrollment in public programs a more attractive option. Thus, rates of substitution—both crowd-out and otherwise—will inevitably vary in different periods.
- No ideal approach exists for estimating how much crowd-out may have occurred. Although many statistical tools have been used on a variety of data sources, none is optimal.

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- A large portion of substitution is not crowd-out. Crowd-out occurs only if a person has public coverage when they would otherwise be privately insured. But distinguishing crowd-out from substitution that occurs for other reasons is not possible on an individual-case basis using the existing data sources. Even though we may know why a specific person moved from private coverage to public (e.g., loss of employer-sponsored insurance following a divorce), we cannot say even from this information whether that person would have obtained some form of private insurance or become uninsured if the public program were not an option.

Given both temporal variation and the difficulties of measurement, one should not expect researchers to arrive at a single estimate that is applicable to all programs and all periods. At best, policy-makers may obtain an understanding of the potential range of crowd-out effects.

### How prevalent is crowd-out?

#### Researchers use a variety of techniques to measure crowd-out.

Researchers observe the total change in health insurance coverage occurring over a period of time due to all possible causes—including those independent of the expansion of a public program and those directly related to it. (See Appendix II for a detailed discussion of methodological issues.) Using sophisticated statistical models, they construct likely scenarios, enabling them to estimate how much private and public insurance coverage would have changed in the absence of the expansion. The estimate of crowd-out is the difference between these model-based estimates of the changes that would have occurred without the program and the actual changes that occurred when a new public program was introduced.

#### Different ways of defining crowd-out yield different results.

Researchers define crowd-out in multiple ways, reflecting both their own perspectives and the idiosyncrasies of their data. These differences contribute to confusion when estimates are compared. All crowd-out estimates are expressed as ratios, but both the numerators and denominators of these ratios may measure different concepts. (Appendix II provides a fuller discussion of each of the major methodological approaches. Appendix III identifies the perspective adopted by each of the studies in this synthesis.)

- The most common definition compares the reduction in the share of the population with private coverage to the increase in the share of the population with public coverage due to the expansion. Researchers using this definition attempt to estimate the changes due solely to the expanded eligibility over the period of years included in the study (9).<sup>2</sup>
- A less restrictive definition focuses on the amount of crowd-out that occurs throughout the public program following an expansion, not just among the newly eligible population. This definition generally produces lower estimates of crowd-out as new enrollees in the pre-existing eligibility groups are likely to be poorer and to have less access to employer-sponsored insurance (8).
- Some studies compare the decline in private coverage associated with program expansions to the overall decline in private coverage during the period, rather than to the increase in public coverage. This approach also tends to produce lower estimates of crowd-out (10).

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- Some estimates focus on the extent to which program expansions reduce the number of uninsured. This “broad” definition can and often does produce a larger crowd-out estimate than the “narrow” definition, which focuses on how much private coverage fell as a result of the expansions (see Appendix II). These two approaches can yield quite different estimates of public program crowd-out<sup>3</sup> and it is important to know which perspective was taken (9).

The “narrow” definition compares—in ratio form—the estimated reduction in private coverage due to the expansion of the public program to the estimated gain in public enrollment resulting from that expansion. The “broad” definition asks what percentage of the gain in public enrollment due to the expansion was not the result of a decrease in the number of uninsured. When there is an increase in the number of individuals reporting both private and public insurance, the reduction in the number of uninsured is less than the sum of the increases in public and private coverage. In this case, the broad definition shows greater crowd-out. Appendix II gives a detailed explanation of the differences between these two approaches.

### Estimates are sensitive to the data used.

Crowd-out analyses use data from a variety of surveys—none of them ideal—with different strengths and limitations (Figure 1). Most of the surveys were not originally designed to collect health insurance information and all have features that may affect the accuracy of the crowd-out estimates (e.g., sample design and size, how insurance status information is collected, whether information is gathered from respondents at only one point in time or whether the same respondents are followed over time). Key distinctions among them include the period of observation and the recall period.

Figure 1. Design features, strengths and limitations of survey data used to examine crowd-out

Survey	Design	Strengths	Limitations
Current Population Survey (CPS)	Cross-sectional	Large sample sizes; annual survey; representative at national and state levels	Complex sample design; ambiguous measurement of insurance status
Survey of Income and Program Participation (SIPP)	Longitudinal	Tracks change over time; straightforward measure of insurance; monthly income and program participation reported	Complex sample design; moderate sample size; sample attrition; not all states identified
National Longitudinal Survey of Youth (NLSY)	Longitudinal	Tracks change over time	Small sample size; nonrepresentative sample; sample attrition
National Health Interview Survey (NHIS)	Cross-sectional	Moderately large sample size; health insurance and health care utilization data	Respondent state not identified; all income provided only in ranges
Community Tracking Study (CTS)	Longitudinal	Tracks change over time	Small sample size; limited duration of panel

Source: Authors

- In longitudinal surveys such as the Survey of Income and Program Participation (SIPP), the Community Tracking Study (CTS) and the National Longitudinal Survey of Youth (NLSY), respondents are followed over time and changes in insurance status are directly observed. By contrast, in cross-sectional surveys such as the Current Population Survey (CPS) or the National Health Interview Survey (NHIS), the sample of respondents changes each year. In

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those cases, researchers pool samples over a number of years to estimate changes in population rates. There are no inherent technical reasons why these two types of data should produce systematic differences in the estimated extent of crowd-out, although longitudinal data facilitate the tracking of life events that may precipitate changes in coverage. But because they are costlier, longitudinal surveys generally have smaller sample sizes, which restrict the statistical precision of the estimates. The much smaller sample sizes observed in some longitudinal surveys may also restrict the number of distinct pathways to crowd-out that can be studied, limiting the generalizability of the estimates and their comparability to other studies.

- Surveys also differ in whether they ask respondents to provide their insurance status at the time of the interview (point-in-time responses) or during an earlier period. The CPS, for example, asks for coverage status during the prior calendar year. (Researchers disagree, however, on whether responses elicited from year-long coverage questions actually reflect that time period or reflect coverage at the time of the survey.) In general, respondents reporting both private and public coverage are more likely to appear in surveys with year-long coverage questions, often reflecting part-year nonoverlapping coverage of different types. In these circumstances, use of the broad or narrow definitions of crowd-out may produce different estimates.
- Researchers have most frequently used the CPS and the SIPP to estimate crowd-out rates (see Appendix III). A recent study compared estimates based on those two data sets, employing the same modeling strategies and assumptions (19). Estimates of crowd-out based on the CPS show both higher Medicaid enrollment and larger declines in private coverage than those based on the SIPP. The researchers concluded that distinct differences in the data probably account for some—but not all—of the differences in the estimates. In particular: the CPS includes ten smaller states that the SIPP excludes; and the SIPP is generally used to arrive at point-of-survey estimates, not full-year estimates like the CPS.

### **Policy analysts should understand what different estimates mean and how different approaches can change results.**

The use of different ratios may convey quite different impressions of the extent of crowd-out that has taken place. Thus, for example, the often cited Cutler and Gruber (10) crowd-out estimate of 49 percent—attributable to the coverage expansions for children and pregnant women during the 1987–1992 period—changes dramatically, depending on the denominator that is used (Figure 2).<sup>4</sup> The 49 percent estimate reflects the percentage of the Medicaid enrollment increase among the expansion population attributed to crowd-out. But the estimate drops to 22 percent if all Medicaid enrollees are considered, including those who were eligible under the previous rules as well as those made eligible under the expansion. Moreover, the crowd-out attributable shift from private insurance accounted for only 15 percent of the overall decline in private coverage that occurred. Other factors, such as the 1991 recession and reductions in access to employer-sponsored coverage also contributed to declines in private coverage and public program enrollment growth during this period (10). The lesson here is that the same term—crowd-out—can be used for quite different concepts, and to achieve valid comparisons among studies it is necessary to identify each specific concept that is employed.

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Figure 2. Alternative estimates of public program crowd-out for coverage expansions (children and pregnant women), 1987–1992

Estimated private insurance lost due to crowd-out expressed with alternative denominators	Percentage
As a percentage of total Medicaid enrollment increase specifically due to Medicaid expansion	49
As a percentage of total Medicaid enrollment increase from all causes	22
As a percentage of total decline in private insurance over period 1987–1992	15

Source: Adapted from Cutler and Gruber, 1997

### The range of estimates of crowd-out is large, and the individual estimates are imprecise.

Research to date has achieved no consensus on the magnitude of crowd-out. For each of the three expansions, the range of crowd-out estimates spans findings from no empirical support for crowd-out to high rates of crowd-out (Figure 3).<sup>5</sup> In addition to these very large ranges of estimated amounts of crowd-out, the individual estimates in general have wide margins of error. Thus, no definitive conclusions can be drawn about the extent of crowd-out.

Even studies examining the same expansion, in the same time period and with the same data source can produce quite different results. For instance, both Dubay and Kenney (14) and Cutler and Gruber (9) studied the crowd-out effects of Medicaid expansions for pregnant women in the late 1980s and early 1990s, using the CPS. Their findings are strikingly different. Dubay and Kenney found no empirical support for crowd-out among women with income under 100 percent of FPL and 45 percent for women with income between 100 and 185 percent of FPL. Cutler and Gruber’s study indicated a range of 50 to 500 percent for women among all eligible groups. These discrepancies reflect the substantial differences in the methodologies used by these two research teams.

Figure 3. Crowd-out estimates by type of expansion and target population\*

Public program expansion	Target population	Range of public program crowd-out estimates
Medicaid expansion	Children	No empirical support to 100 percent or more
Medicaid expansion	Pregnant women	No empirical support to 100 percent or more
State-initiated program	Adults	No empirical support to 100 percent
State-initiated program	Children	No empirical support to 60 percent
SCHIP	Children	No empirical support to 77 percent

\*Appendix III provides detailed information on the studies summarized here.

Source: Authors

### Estimates are sensitive to assumptions about what would have occurred in the absence of the program expansion.

To estimate the extent of reductions in private coverage resulting from public coverage expansions, researchers must account for the concurrent economic and policy changes that also affected both public and private coverage. The 1991 recession, for example, occurred in the early period of the Medicaid expansions, and the economic expansion of the late 1990s coincided with the establishment of the various SCHIP programs. Welfare reform legislation in 1996 also affected participation in Medicaid.



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All studies, therefore, make assumptions about what would have happened to enrollment in private and public plans without the program expansion. The estimates of crowd-out are highly sensitive to those assumptions, which vary widely among studies—reflecting both the views of the researchers and the databases that they are using. As demonstrated by the analyses of Card and Shore-Sheppard (5), changing a single assumption can have a major impact on the results. Specifically, Cutler and Gruber (9) assumed that in the absence of the expansion, children of all ages would have had parallel trends in coverage—that is, equal percentage points of change—over the period of years studied. When Card and Shore-Sheppard relaxed that assumption to allow children of differing ages to have different trends in coverage, a drop in the estimated amount of crowd-out resulted and the estimate became insignificant.

### What factors influence the amount of crowd-out?

#### **Crowd-out is more likely at higher levels of income.**

The evidence, albeit relatively weak, suggests that more crowd-out is likely in public programs that have higher income eligibility thresholds—above 200 percent of FPL, for example. Findings from most studies lean in this direction, although their estimates tend to be statistically insignificant. Such findings make intuitive sense as private insurance is more prevalent at higher income levels, providing more opportunities for displacement (Figure 4).

In support of such a conclusion, studies spanning the three major public program expansions found little evidence of crowd-out among enrollees with incomes below 100 percent of poverty (14, 22, 25). Nonetheless, no consensus exists on the magnitude of the difference.

*Figure 4. Rates of employer-sponsored insurance (ESI) among nonelderly by federal poverty level (FPL), 2000–2001*

Percentage of FPL	Percentage with ESI
Under 100	18
100-199	43
200-299	67
300+	84

*Definition: Federal poverty level (FPL) for a family of three was \$13,738 in 2000 and \$14,128 in 2001.  
Source: SHADAC analysis of pooled March 2001 and March 2002 CPS data*

#### **Higher rates of substitution—but not necessarily more crowd-out—are likely among families who experience a large drop in income.**

At least one study (35) that used longitudinal data to track children’s insurance coverage over time found that children who became eligible for Medicaid through a significant drop in family income had the highest rate of public program substitution among the three groups of children studied. At issue is whether such substitution should be considered crowd-out. Declines in income large enough to qualify these children for Medicaid often occurred in families experiencing a recent divorce. Such an event may lead to loss of private insurance coverage through the loss of access to employer-sponsored insurance (ESI), or from the inability to afford private coverage. If, in the absence of the public program, the children would have been uninsured, they should not be included in the crowd-out estimates. The complex dynamics in cases involving divorce and other major life events highlight

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the difficulty of identifying crowd-out, even when following people over time. To do so, researchers need a sample of comparison families without access to the public program but with starting income levels, and declines in income during the period, that are similar to those of the families who became eligible for the public program. A comparison group with income too high could provide a distorted picture of what would have happened because their higher income might protect them from losing private insurance. Yazici and Kaestner (35) used two comparison groups for children whose families experienced a drop in income. They found a smaller estimated rate of crowd-out when using the comparison families with lower initial income as compared to the comparison families with higher initial income, which suggested this difficulty.

### **The state of the economy affects both employer and employee decisions on health insurance coverage.**

In order to attract workers and control their labor costs, employers make a variety of decisions affecting the health insurance coverage of their employees. Those decisions—whether to offer health insurance at all, the share of the premium that employees must contribute and the cost-sharing amounts they must pay—reflect the economic climate in which firms are operating. In periods of economic expansion with increased competition for labor, employers may begin offering coverage or provide more generous benefits. Recessions may result in pressures to cut costs and hence ESI benefits. Because of higher unemployment and lower incomes, recessions also affect families' ability to afford private coverage, even if they still have access to it. As private coverage rates fluctuate with the business cycle, crowd-out estimates from one period may not generalize to another.

### **Some employers may have dropped coverage in response to expansions of public programs.**

Actions that cause employees to lose or drop their ESI coverage would be considered crowd-out only if they were a direct response to an expansion of public coverage. Such actions would be more likely among small and low-wage firms, but determining which actions constitute direct responses is difficult. Three studies have attempted to explore this issue.

Neither Cutler and Gruber (9) nor Shore-Sheppard, Buchmueller and Jensen (30) found evidence that employers are less likely to offer employee-only coverage when a higher proportion of employees or their dependents are eligible for Medicaid. But the latter study, which focused on relatively small firms (those with under 100 employees), found that the Medicaid expansions reduced the likelihood that an employer would offer dependent coverage. Specifically, a 10 percent increase in the proportion of employees eligible for Medicaid reduced the likelihood of offering dependent coverage by six percent. The study found no indication that employers reduced the share of the premium they paid for either employee-only or dependent coverage in response to Medicaid expansions. The estimates in these studies may reflect the time periods over which they were made. Both studies compare employers' offer decisions in the late 1980s—before the expansions—to comparable decisions made in the early- to mid-1990s.

Marquis and Long (26) studied the effects of state-initiated coverage programs and found no overall difference in offer rates for employee-only or dependent coverage in states with such programs compared to others. But small, low-wage firms (those with fewer than 50 employees and more than 75 percent of workers earning less than \$15 an hour) in states with state-initiated programs were around three percentage points less likely to offer employee-only coverage compared to similar firms in other states. (The decisions of these small, low-wage

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firms concerning family coverage were not reported.) In this study, the data used came from 1993 and 1997 surveys.

### **Allowing whole families to enroll in public programs may increase crowd-out.**

A key question for policy-makers is whether allowing whole families to enroll in public coverage—as several state-initiated programs do—produces more crowd-out than allowing only children to enroll, as is generally the case with SCHIP. Two studies provide indirect tests of this hypothesis and reach different conclusions.

Cutler and Gruber (9) found that a higher proportion of a family’s expected health costs being eligible for Medicaid significantly increased the likelihood of a child or a pregnant woman being enrolled in Medicaid, and was associated with a decreased likelihood of having private insurance. Those findings suggest that more substitution would occur when entire families are allowed to enroll. Ham and Shore-Sheppard (19), however, found no evidence of greater rates of crowd-out when higher proportions of siblings were eligible for Medicaid.

### **Dual coverage may reflect a desire for benefits that private insurance does not cover, or covers less generously.**

Many studies have observed that the proportion of the population with both public and private coverage increases following an expansion of public coverage. For studies based on the CPS, such dual enrollment may reflect nonoverlapping spells of private and public coverage in the previous year. But Yazici and Kaestner (35), using the NLSY—a longitudinal survey—found that the percentage of children with both public and private insurance at the same point in time increased between 1988 and 1992. Assuming no reporting errors (which is a major assumption), their findings suggest that some low-income families augmented their private coverage with public benefits.

### **How effective are policies aimed at limiting crowd-out?**

#### **States have employed a variety of policies to limit crowd-out, but these measures have not been formally evaluated.**

Policies to limit crowd-out—specifically in state programs initiated in the early- to mid-1990s—have been in effect for some time, but their effectiveness has not been formally evaluated, largely because only seven states have such programs and they all have different anti-crowd-out policies. Although many states subsequently adopted anti-crowd-out policies as part of their SCHIP programs (Figure 5), no one has yet undertaken a formal, rigorous evaluation of their impact. Those measures included the following:

- Obtaining information from applicants on current and recent health insurance status and monitoring crowd-out.
- Imposing cost-sharing requirements.
- Subsidizing ESI.
- Prohibiting employers from changing their ESI contributions, based on wage level or job classification.
- Imposing waiting period requirements.
- Verifying whether applicants are uninsured by accessing private insurance databases.

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Figure 5. Policies used by states to limit public program crowd-out in their early experience under SCHIP

	Application questions/monitor	Cost-sharing requirements	Subsidies for ESI	Restrictions on employer actions	Waiting periods	Verification of insurance status
Number of states	24	4*	2	2	34	8

\* Only four states specifically identified cost sharing as an anti-crowd-out instrument at this relatively preliminary period; however, all the SCHIP programs that had separate, new programs required some form of cost sharing.

Source: Adapted from Lutzky-Westpfahl & Hill, 2001

### No evidence exists on the effectiveness of waiting periods.

Many states have used waiting periods as a strategy to limit crowd-out, beginning in the mid-1990s with the state-initiated coverage programs. States such as Minnesota, Tennessee, and Vermont, for example, all introduced waiting-period requirements. Such requirements stipulate the length of time after an individual or family has dropped private coverage during which they are ineligible for the public program.

Concerns about potential crowd-out were also a major issue when Congress enacted SCHIP in 1997, and participating states were initially required to monitor crowd-out in various ways and impose waiting periods if the rate of crowd-out—as measured by their own monitoring systems—exceeded eight percent in a nine-month period (34). That regulation was later modified to preclude states from imposing waiting periods if their SCHIP programs were expansions of Medicaid. (Many states have both Medicaid expansion and separate SCHIP programs. The Medicaid expansions, which target lower-income children, must comply with federal restrictions on the types and extent of waiting periods and cost sharing that they may impose.) Some states have applied for waivers in order to implement specific policies to curb crowd-out.

As of early 2001, 34 states had adopted some form of waiting period to limit crowd-out in SCHIP, with half imposing a waiting period of six months or longer. No evidence exists on the effectiveness of such requirements, but waiting periods alone would seem unlikely to have a major impact on crowd-out for the following reasons:

- Enforcement of waiting periods may be difficult and costly.
- Some families may choose to drop their private coverage and wait out the required period. (The waiting periods for some public programs are no longer than those required to qualify for some types of ESI.)
- Waiting periods have no impact on the second pathway to crowd-out—i.e., “within” enrollment crowd-out, which occurs when public program enrollees do not take up private insurance when it becomes available, although they would have done so in the absence of the public program.
- Most states with waiting periods for SCHIP enrollment permit exemptions for certain categories of children who lose their insurance involuntarily. Some of these exemptions may, in effect, allow crowd-out to occur. Examples of such exemptions include a parent’s loss of employment; an employer’s decision to stop offering dependent coverage (regardless of whether that decision was a direct response to the existence of the public program); a parent’s job change to an employer who does not offer dependent coverage; or the expiration of COBRA coverage (23). Some states also grant exemptions if the family

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switches from private nongroup coverage; if the family is contributing more than 50 percent of an ESI premium; or if premium contributions exceed specified percentages of family income (34).

### **Cost-sharing requirements may limit crowd-out.**

States with free-standing SCHIP programs frequently use cost sharing—in the form of monthly premiums, annual enrollment fees, copayments or deductibles—to curb crowd-out, as do most states with state-initiated coverage programs. Originally, few states considered cost sharing to be a strategy to limit crowd-out (23), but most states now do (34). Income thresholds at which states begin to apply cost sharing vary, as do the amounts imposed and the administrative rules, including policies concerning nonpayment (34).

Studies of the demand for health insurance among lower-income families and workers suggest that certain cost-sharing features could reduce crowd-out (27). In particular, sliding-scale premium contributions that rise to significant dollar amounts at the upper limit of income eligibility narrow the gap between out-of-pocket costs in private and public coverage, making public coverage relatively less attractive. States can structure such contributions so that eligible families at lower income levels face no or only nominal contributions, in order to avoid discouraging them from applying.

Qualitative studies of the early phases of SCHIP, however, tell a somewhat different story about the potential effects of cost sharing (23, 34). Key-informant interviews with enrollees and focus groups of families with enrolled children suggest that efforts to make SCHIP programs more like private coverage with premium contributions and other cost sharing may attract rather than discourage enrollees. Again, however, researchers have not tested those hypotheses.

### **What are the policy trade-offs between expanding coverage and reducing crowd-out?**

It is virtually impossible to design public programs that succeed in enrolling substantial numbers of the uninsured and avoid crowding out private insurance. Hence, policy-makers should weigh several trade-offs in planning future initiatives.

### **Crowd-out raises the costs of expanding coverage.**

Inability to target a coverage expansion exclusively to the uninsured raises the cost of expanding coverage. Thus, for a given level of public expenditure on a coverage expansion, enrollment by those who would otherwise have private coverage reduces the potential number of uninsured people who can be covered.

### **Monitoring crowd-out provides imprecise information and implementing deterrents may be costly.**

States that monitor crowd-out identify individual enrollees with specific characteristics—e.g., the percentage of children who had private health insurance within the past six months—but cannot assess what proportion of these enrollees would have dropped their private insurance regardless of the availability of public coverage. Hence, how far such characteristics may serve as reliable indicators of crowd-out cannot be determined with any useful precision. Additionally, other pathways to crowd-out are not included in such monitoring. Moreover, implementing deterrents to crowd-out may require substantial resources, without any guarantee that they will be effective.

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### **Attempts to reduce crowd-out can limit participation by other groups.**

Efforts to reduce crowd-out—for instance, by imposing waiting periods or cost sharing—can reduce program participation for all families, not just those seeking to substitute public for private coverage.

### **People who shift from private to public coverage may obtain better benefits.**

Crowd-out raises public expenditures but it may also translate into better benefits for low-income families. Some individuals who drop private coverage to enroll in a public program may gain better access to care, especially if they were facing high deductibles or copayments in their private plans. Lower health care costs may also improve their standard of living considerably. And the coverage benefits available under the public program may be superior to those available under private coverage.

### **Effective targeting raises equity concerns.**

Public program expansions often provide a much less expensive coverage option for low-income families who drop private coverage in order to enroll in a public program. If no such substitution is permitted, the uninsured will benefit from the subsidy, while families in the same economic circumstances who have spent substantial amounts for private coverage will not.

Of particular concern are families incurring a significant financial burden under their private insurance coverage. Allowing them to enroll in a public program establishes an important safety net (8), which is the rationale for some waiting-period exemptions in SCHIP.

# Implications for Policy-Makers

This synthesis of research on public program crowd-out highlights some key issues for policy-makers.

- **Despite all the research on this topic, there is no bottom-line answer to the question of how much crowd-out exists.** This conclusion reflects both the probable underlying differences in the rate of crowd-out among programs (based on programmatic and enrollee characteristics and environmental factors) and because measuring crowd-out is difficult. Policy-makers should, therefore, be skeptical of definitive and broad statements about how much crowd-out exists. Measurement problems include the following:
  - We can observe only overall trends in private and public coverage, and many factors affect these trends that are independent of public program expansions.
  - Crowd-out estimates are sensitive to the data used and the assumptions underlying different research approaches. Thus, wide variation in estimates is likely.
  - Identifying crowd-out at the individual level is not possible.
- **Crowd-out is more likely to occur among people with moderate incomes who have greater access to employer coverage and ability to pay for it.**
- **There are many substitution pathways—people might drop private coverage, keep public coverage when they have access to private coverage, or work for employers who change their offerings—but crowd-out occurs only if those decisions or changes are driven specifically by the existence of the public program.** We do not know the relative contribution of these different pathways to crowd-out. If policy-makers focus on a single pathway, however, they may miss a large part of the crowd-out that ultimately results from the expansion of a public program.
- **Despite limited evidence on their effectiveness, some strategies may curb the amount of crowd-out, but there are unavoidable trade-offs in adopting them.** Participation in public programs by the uninsured will probably be lower, and the costs of implementing such policies and monitoring their effects are significant.
- **Crowd-out increases the cost of a given expansion, but it may also make lower-income families better off.** Families who substitute public for private coverage may be gaining a more stable and affordable source of insurance.
- **To achieve meaningful reductions in the number of uninsured, some amount of crowd-out seems inevitable.** This dilemma arises in all initiatives to expand coverage, and becomes more prominent as policy-makers seek ways to assist moderate-income uninsured families.
  - Some states, for example, are using SCHIP funds to subsidize ESI premiums for eligible families. Although these initiatives are small as yet—in part because of the restrictions that Congress had placed on them in order to avoid crowd-out—the potential for substituting public for private dollars could be considerable.

## Implications for Policy-Makers

- Interest is growing at the federal level in the possibility of using refundable tax credits to subsidize premiums for private coverage, typically in the nongroup market. Although such initiatives could reduce the number of uninsured significantly, they could also be costly, if large numbers of those who are eligible for tax credits avail themselves of them, regardless of their current insurance status (28).

These implications suggest that federal and state policy-makers considering ways to expand coverage need to define their goals clearly, acknowledging explicitly the inherent uncertainty in measuring crowd-out and the trade-offs that exist between curbing crowd-out and the attainment of other policy goals.



# The Need for Additional Information

The various public program expansions were natural experiments that allowed examination of patterns of private and public coverage among low-income populations before and after the policy change. Unless other expansions are introduced in the future, research on public program crowd-out is limited to these prior expansions and to the survey data sets that were collected over the relevant periods of time before and after their implementation.

While this constraint limits opportunities to engage in further research, there are still several additional avenues that research could explore.

- SCHIP provides continuing opportunities for research since survey data for the relevant period after implementation are still becoming available. Analyzing these additional years of data for the impact of SCHIP could provide important new insights for public program crowd-out.
- The two recently completed studies on the Medicaid expansions (Card, Shore-Shepperd, 2003 and Ham, Shore-Shepperd, 2003) have demonstrated the sensitivity of crowd-out estimates to various modeling assumptions, entire modeling approaches and differing data sets. By the same token, using different approaches and databases to analyze the impact of SCHIP on the private coverage of children might throw further light on the sensitivity of crowd-out estimates to alternative methodologies.
- For the relatively small number of state-initiated programs, there are opportunities to examine the impact of changing economic conditions—from using different post-implementation years—on crowd-out estimates, and to assess the sensitivity of estimates to various modeling assumptions more systematically.
- The recently enacted Medicare Modernization Act, which provides subsidized prescription drug coverage for Medicare beneficiaries, will offer another opportunity to explore whether and the extent to which an expanded public program displaces private coverage.
- Finally, and as was noted, survey households that report having both some kind of private insurance and some type of public insurance—referred to earlier as “duals”—can have important impacts on estimates of public program substitution. Future research that attempts to assess how much these “duals” represent either reporting error or the nature of the private coverage that is held can provide important insights for the interpretation of crowd-out estimates.

## Endnotes

1. We use the convention of referring to families as “low-income” if their family income falls below 200 percent of the federal poverty level (FPL); “poor” families designate those with family income below 100 percent of the FPL; and “other low-income” families specifically designate those families between 100 percent and 200 percent of the FPL. Where greater specificity is desired, we give an explicit range of FPL.
2. Numbers in parentheses refer to reference articles from Appendix I.
3. As discussed in Appendix II, the narrow and broad definitions account for the group reporting both private and public coverage (“duals”) differently.
4. The alternative estimates also use different numerators.
5. “No empirical support for crowd-out” means that point estimates of the change in private coverage attributable to the expansion were not negative—i.e., private coverage was not found to decline specifically due to the expansion—a requirement for crowd-out to have taken place.

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## Appendix II Technical and Methodological Issues

This technical appendix addresses the definitional issues associated with crowd-out, alternative ways that crowd-out has been conceptualized, methodological issues in analysis of this topic and the approach taken by this synthesis to filter the literature.

### Definitional issues in measuring crowd-out

Some household survey respondents report having both private insurance and public coverage. In surveys that collect only point-in-time insurance status, such a dual response indicates that respondents have both types of coverage concurrently. By contrast, in a survey such as the CPS, which collects information on any coverage the respondent held during the prior year, dual public/private responses can reflect changes in insurance status during the year with no overlapping spells. How researchers treat these dual responses gives rise to the two main definitions of crowd-out:

- The *broad* definition focuses on how far increases in public coverage have reduced the uninsured rate in the target population.
- The *narrow* definition focuses on whether private insurance rates have dropped in the target population as a result of the expansion of the public program.

If only three coverage states were possible—private (ESI and nongroup), public (of all kinds), and no insurance—those two perspectives would provide identical estimates. The total net gain in coverage resulting from the expansion of a public program would simply be the increase in public enrollment less any reduction in private coverage resulting from crowd-out. In this case, the increase in total coverage is the same as the decrease in the uninsured.

When survey respondents report having both private and public coverage, however, the broad and the narrow definitions produce different estimates. In this case, the total coverage gain is not simply the sum of the two inclusive concepts of public and private coverage, as the sum would include the dual responses twice. The narrow definition compares the total percentage change in private coverage associated with the expansion (including the duals as well as those with private coverage only) to the overall percentage increase in public coverage, also including the duals. The broad definition, by contrast, compares the change in the percentage of uninsured to the overall percentage increase in public coverage, again including the duals. Specifically, it asks the question: By how much did the drop in the uninsured rate fall short of the percentage increase in enrollment in the public program?

The hypothetical example below illustrates the variation in estimates that can result from these different definitions. The estimates should be understood to represent the changes in coverage that actually occurred net of those that would have occurred in the absence of the public program.

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### Hypothetical example: Differences in the narrow and broad definitions of crowd-out

	Change in percentage with public coverage only	Change in percentage with private coverage only	Change in percentage with public and private coverage	Change in percentage of uninsured	Net change
The full set of differences attributable to expansion	+ 5%	- 2%	+ 1%	- 4%	—
Narrow definition					
Numerator		- 2%	+ 1%		- 1%
Denominator	+ 5%		+ 1%		+ 6%
Broad definition					
Numerator	+ 5%		+ 1%	- 4%	+ 2%
Denominator	+ 5%		+ 1%		+ 6%

Crowd-out according to the narrow definition =

$$\frac{(\text{change in \% with private coverage only}) + (\text{change in \% with private and public coverage})}{(\text{change in \% with public coverage only}) + (\text{change in \% with private and public coverage})}$$

$$= \frac{(-2\% + 1\%)}{(+5\% + 1\%)} = -1/6 = 16.7\% \text{ [Note, by convention, this negative ratio value is expressed as a positive percentage.]}$$

Crowd-out according to the broad definition =

$$\frac{(\text{change in \% with public coverage only}) + (\text{change in \% with private and public coverage}) + (\text{change in \% uninsured})}{(\text{change in \% with public coverage only}) + (\text{change in \% with private and public coverage})}$$

$$= \frac{(+5\% + 1\% - 4\%)}{(+5\% + 1\%)} = 2/6 = 33.3\%$$

Thus, how researchers treat duals can make a significant difference to crowd-out estimates, and understanding how surveys might identify these cases is important.

- With surveys such as the NHIS and the NLSY that collect information on coverage status at a point in time, dual status logically indicates concurrent coverage (if it is not a response error). If we observe an increase in people with such coverage as a result of a program expansion, reasonable assumptions are that: (1) those individuals have the same private coverage that they had previously; and (2) they would have continued to have that coverage in the absence of the public program expansion. Those inferences suggest that Medicaid and SCHIP are augmenting private coverage through wraparound benefits. Thus, if all duals represented concurrent coverage, policy-makers—depending on their view of what constitutes crowd-out—might prefer to use the narrow definition, which takes account of that additional role of public programs.
- In surveys like the CPS, however, duals can also represent part-year, nonoverlapping coverage of both types (assuming that the CPS does indeed measure coverage at any time during the previous year). Analysts might therefore consider some type of partial allocation of them to the public and private coverage counts, but such an approach would be arbitrary. An unknown number of cases would represent concurrent coverage, and some of those with nonoverlapping spells of private and public coverage might also have had one or more uninsured spells during the year. Although we might expect rates of concurrent coverage to be quite low, the point-in-time data available from the study by Yazici and Kaestner (2000) suggest considerable growth in concurrent coverage from 1988 to 1992 among the several categories of children followed, reaching 15 to 35 percent of

## Appendix II Technical and Methodological Issues

total Medicaid enrollees by the end of the period. It should be emphasized that response error may have an important role in reported cases of public and private coverage, and could even account for increases in these reported cases of public and private coverage as families become more aware of the public program over the course of the expansion. No research has been conducted on the potential role of response error, however.

Because of the nontrivial difference in the crowd-out estimates that can result from using these two definitions, we indicate in Appendix III whether the reported estimates use the narrow definition (N), the broad definition (B) or both (N, B).

### Alternative measures of crowd-out

Crowd-out measures differ not only because of the treatment of people with dual coverage, but also because researchers, policy-makers and advocates have used different concepts of crowd-out. The following discussion explains the different measures and clarifies their interrelationships.

- **Marginal measure derived from large-scale research studies.** This measure is the one most commonly encountered in the literature. In the case of the Medicaid expansions of the early 1990s, which broadened eligibility for existing programs, the marginal measure looks at the decline in private coverage among those newly eligible relative to the increase in public coverage among the newly eligible.
- **Full measure derived from large-scale research studies.** A less commonly encountered measure captures the total change in enrollment in public programs, whether related to the expansion or not. Studies using this measure also estimate the reduction in private coverage resulting from enrollment in either the existing or the expanded public program. We would expect this full measure to yield lower estimates of crowd-out because enrollment into already existing programs probably consists of very-low-income individuals who are less likely to have access to employer-sponsored coverage.
- **Measures based on total change in private coverage.** In order to provide a wider perspective on crowd-out, researchers sometimes compare the estimated reduction in private coverage (the numerator of either the marginal or the full measure) to the amount by which private coverage fell for all reasons over the study years. During periods of economic decline, this total drop in private coverage can be quite large. In such periods, the share of the decline in private coverage that is due specifically to the expanded public program can be relatively small—even when a marginal or full estimate of crowd-out is large. Especially if the increase in public enrollment is modest, marginal or full crowd-out estimates may mislead policy-makers about the relative role of the public program expansion in the decline of private coverage.
- **Measures focusing on specific pathways.** In longitudinal studies that follow individual survey respondents over time, researchers sometimes estimate crowd-out rates attributable to one or a limited number of pathways. A study might, for example, follow a group of children who had private coverage before an eligibility expansion and whose family income would qualify them subsequently to enroll in the expanded program. By identifying a similar group of children in states without coverage expansions, the researchers can compare the percentage of privately insured children in this income group who enroll in a public program. The difference in enrollment between children in the expansion and nonexpansion states provides an estimate of crowd-out only among those with prior private coverage. Such pathway-specific measures may under- or over-estimate the marginal or full measures, which are, in effect, weighted averages of all the specific crowd-out rates. A partial estimate, therefore, may not necessarily be an underestimate.
- **Measures based on overall trends in private and public coverage.** Published reports on changes or trends in the proportion of families or children with private and public coverage are common—often for selected income ranges. The ratio of any overall decline in private coverage to any overall increase in public coverage over the time period provides a gross measure of potential crowd-out. Although this ratio informs us about overall trends in coverage patterns, it has no real policy utility as a reliable measure of crowd-out. We cannot safely draw inferences from such overall trend data because we are unable to know what portion, if any, represents crowd-out rather than secular trends or short-term economic effects.

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### Other methodological issues

There is no single correct way of assessing how much of the observed changes in public and private insurance coverage over time are due to macroeconomic changes, long-term secular changes in ESI and in employee decisions about ESI, long-term secular changes in individuals' demand for private, nongroup insurance coverage, and the impact of public program expansions themselves (Cutler & Gruber, 1997). Researchers studying the question of crowd-out have used a variety of approaches to construct the "counterfactuals" to these public program expansions to allow them to identify econometrically the impact due specifically to the expansion.

We have identified from the literature and discuss below several methodological issues that we believe have relevance for this body of research in general. This discussion also serves as a summary of the methodological issue codes that we employ in Appendix III to provide a brief summary of the findings and pertinent characteristics of the studies that are included in this synthesis report. We emphasize that we provide these assessments as guides for interpreting the findings from these studies, and not as criticisms. Virtually all are issues that *may* have impacted the estimates of public program substitution (or their significance) provided by the studies, but neither the existence nor the magnitude of such impacts are knowable a priori.

- **Inclusiveness of the crowd-out measure.** As indicated in the discussion of alternative measures, some longitudinal studies track different cohorts of children to produce partial crowd-out measures that focus on specific pathways. Despite the potential policy interest in the estimates for specific cohorts, in this synthesis—to facilitate comparability with other studies—we report only the overall estimates derived from combining those of the specific cohorts by weighting the rates appropriately. Sometimes, however, the specific cohorts studied may not include all of the possible pathways through which crowd-out can occur, and the weighted estimate still may not be comparable to other, fuller estimates. Because of this lack of comparability, we indicate such studies with an "A" in Appendix III.
- **Specification and design issues.** To estimate crowd-out rates, researchers have to estimate the changes in coverage among the target population that would have occurred in the absence of the expansion of the public program. As no ideal comparison population exists, there is, correspondingly, no perfect way of measuring the true impact of any expansion. The following specification and design issues are particularly important for understanding differences in estimates:
  - Studies differ in how broadly they define their comparison groups in terms of the range of income levels they include. In the absence of the public program expansion, patterns of change in insurance coverage might be expected to vary according to income. Thus, the range of income used can have important implications for the estimates obtained, as LoSasso and Buchmueller (2002) demonstrate in their comparisons of the impact of SCHIP by the range of income included, and as Yazici and Kaestner (2000) illustrate with one of their explicit comparison groups.
  - In addition to differences in the range of income included in the comparison group, the econometric models that are used to control for other differences that may have affected take-up and crowd-out rates also differ implicitly in how they define this comparison group. The results by Card and Shore-Sheppard (2003) demonstrate the importance this consideration can have. Specifically, Cutler and Gruber (1996) used the assumption that in the absence of the expansion, children of all ages would have had parallel trends—that is, equal percentage points of change—in coverage over the period of years studied. When Card and Shore-Sheppard relaxed this assumption to allow children of differing ages to have the possibility of different trends in percentage points of change in coverage (interacting age and year dummy variables), this change resulted in substantial drops in the estimated amount of crowd-out and reduced these estimates to insignificance.
  - The state-initiated public programs differed from the Medicaid expansions in a way that probably affected estimates of crowd-out. The Medicaid expansions occurred over a number of years, following several changes in federal law mandating or permitting changes in income eligibility. States differed both in the income eligibility limits established prior to these changes as well as in the eligibility limits that they subsequently adopted. As a consequence, considerable change in eligibility levels occurred both within states over time and among states. This natural variation facilitated identification of the impact of the Medicaid expansions—i.e., distinguishing



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it from the variation one might expect to see over states and over time in insurance coverage for many other reasons. The state-initiated programs, by contrast, were one-time changes. Studies evaluating their impact often include a considerable number of pre-program years when no one in the program-states was eligible and a number of program-operation years when all members of the target population were eligible—with those living in comparison states being ineligible throughout. Some recent theoretical research has suggested that standard errors derived from this evaluation design can be substantially less useful as guides to assessing how much estimated impacts of program changes might be reflecting random variation than designs such as those used in the Medicaid expansions, which are able to exploit substantial interyear and interstate variation.<sup>1</sup> Indeed, empirical findings from the Kronick and Gilmer (2001) study—and some inconsistencies between their findings and those of the Marquis and Long (2003) study—suggest that much less certainty attaches to these findings.

We indicate with a “B” in Appendix III when the reported crowd-out estimate of public program substitution is derived from a study that includes one or more of these specification and design issues.

- **Duration of eligibility.** After a Medicaid expansion, as families learn of their eligibility and act on that knowledge perhaps gradually over time, estimates of Medicaid take-up rates and crowd-out may differ by virtue of the length of the time captured by the available survey data. Ham and Shore-Sheppard (2003), for example, use the SIPP longitudinal data to split the impact of eligibility for Medicaid into a short-term exposure (less than eight months) and a longer-term exposure (more than eight months). They find no impact on Medicaid take-up for the short-term eligibility exposure—indeed, the coefficient is negative (–.054) and significant—but a large and significant impact for the longer-term exposure (0.155). Moreover, we know that crowd-out can arise *within* an enrollment spell although it was not a factor at the time of initial enrollment. How much a given crowd-out estimate fully represents the within-spell component is thus likely to be determined by how fully the data capture longer-term enrollment patterns. We specifically note with a “C” in Appendix III those studies for which the possible duration of enrollment may be especially relevant.
- **Sample sizes.** Although we note the statistical significance of estimates in Appendix III, we also specifically identify studies where the sample sizes are critically small. Those studies, which we indicate with a “D” in the table, use longitudinal data sets with limited waves of data.
- **Clustering in surveys and standard errors.** The complex, multistage designs of many of the health insurance surveys used in this literature require estimators that fully take account of those design features—to avoid biasing standard errors downward and significance levels upward (thus increasing the chances of Type I errors). While this risk is generally well known in survey statistics, a recent unpublished study (Davern et al., 2003) suggests that it is important for these surveys of health insurance as well. Hence, some of the results included in this synthesis may indicate more significance than would result from estimators that more fully accounted for the effect of clustering on standard errors. Where we believe this problem is especially likely, we denote it with an “E” in Appendix III.

### Approach to filtering the literature

We included in this synthesis any study with estimates of crowd-out that was published in a peer-reviewed journal or that was accepted for publication. We also included one working paper that had been presented at several university seminars and is under review at a peer-reviewed journal (Card & Shore-Sheppard, 2003). Other material was consulted and included in Appendix I if it made an important contribution to a methodological issue, was useful for its general background material or provided earlier reviews of the literature.

The study by Shore-Sheppard (1997/2003), although often cited in the literature from its earlier version, was not included because it was under revision and at the time this synthesis was completed the author had not yet finished these revisions.

<sup>1</sup> Marianne Bertrand, Esther Duflo and Sendhil Mullainathan. *How Much Should We Trust Differences-in-Differences Estimates?* NBER Working Paper #8841, March 2002.

## Appendix III Methods Issues<sup>1</sup>

Study authors	Data source	Years	Target population	Public program	Public program crowd-out estimate	Definition used	Methods issues
Cutler & Gruber (1996) model (a)	CPS	1987–1992	Children	Medicaid expansion	From 31% <sup>***</sup> to 49% <sup>***</sup>	N, B	B, C, E
Card & Shore-Sheppard (2003) model (a)	CPS	1987–1992	Children	Medicaid expansion	From 16% <sup>ns</sup> to 42% <sup>ns</sup>	N, B	C, E
Card & Shore-Sheppard (2003) model (b)	SIPP	1991–1993	Children	Medicaid “100%” & “133% expansion” only	From no empirical support for any public program crowd-out in either expansion to 17% <sup>ns</sup> to 48% <sup>ns</sup>	N, B	C
Card & Shore-Sheppard (2003) model (c)	CPS	1991–1996	Children	Medicaid “133% expansion” only	From 150% <sup>**</sup> to 177% <sup>**ns</sup>	N, B	
Card & Shore-Sheppard (2003) model (d)	NHIS	1992–1996	Children	Medicaid “100% expansion” & “133% expansion” only	From no empirical support for any public program crowd-out to 53% <sup>**</sup>	B	
Ham & Shore-Sheppard (2003) model (a)	SIPP	1986–1993 (omitting 1989)	Children	Medicaid expansion	No empirical support for any public program crowd-out in either expansion	N	
Ham & Shore-Sheppard (2003) model (b)	SIPP	1986–1993 (omitting 1989)	Children	Medicaid expansion	From 8% <sup>ns</sup> to 45% <sup>ns</sup>	N	
Ham & Shore-Sheppard (2003) model (c)	SIPP	1986–1993 (omitting 1989)	Children	Medicaid expansion	From no empirical support for any public program crowd-out to 68% <sup>ns</sup>	N	
Ham & Shore-Sheppard (2003) model (d)	SIPP	1986–1993 (omitting 1989)	Children	Medicaid expansion	18% <sup>ns</sup>	N	
Thorpe & Florence (1998)	NLSY	1989–1994	Children	Medicaid expansion	16%	N	A, B #
Yazici & Kaestner (2000)	NLSY	1988–1992	Children	Medicaid expansion	19% <sup>ns</sup>	N	A, C, D
Dubay & Kenney (1996)	CPS	1989, 1994	Children	Medicaid expansion	10.3% < 100% FPL 21.7% 100–133% FPL	N	A, B ##
Blumberg, Dubay & Norton (2000)	SIPP	1990	Children	Medicaid expansion	4.4% <sup>ns</sup>	B	A, C
Shore-Sheppard (1997, 2003)	CPS	1988, 1993, 1996	Children	Medicaid expansion	Under revision, not available from author	n.a.	n.a.
Shore-Sheppard (2000)	CPS	1988, 1989, 1994, 1995	Children	Medicaid expansion	8% <sup>ns</sup> to 16% <sup>ns</sup>	N	
Dubay & Kenney (1997)	CPS	1989, 1994	Pregnant women	Medicaid expansion < 100% and 100–185%	From no empirical support for any public program crowd-out (for < 100% FPL) to 45% <sup>ns</sup> (for 100–185% FPL)	N	B, E
Cutler & Gruber (1996) model (b)	CPS	1987–1992	Pregnant women	Medicaid expansion	From ~ 500% <sup>ns</sup> to 50% <sup>ns</sup>	N, B	B, E
Kronick & Gilmer (2002)	CPS	1988–1999	Adults	State-initiated programs	From no empirical support for any public program crowd-out (1 state) to 100% <sup>**</sup> (1 state)	B	B, E
Marquis & Long (2003)	CPS	1990–1992, 1996–1997	Adults	State-initiated programs	From no empirical support for any public program crowd-out to 75% <sup>*</sup>	N	B
Marquis & Long (2003)	CPS	1990–1992, 1996–1997	Children	State-initiated programs	From no empirical support for any public program crowd-out to 60% <sup>**</sup>	N	B

## Appendix III Methods Issues<sup>1</sup>

Study authors	Data source	Years	Target population	Public program	Public program crowd-out estimate	Definition used	Methods issues
Shore-Sheppard, Burgmueller & Jensen (2000)	HIAA & KPMG/Peat Marwick, RWJF, KFF	1989–1991 & 1993, 1995	Employer effects	Medicaid expansion	No evidence of any impact for employee-coverage offer rates; but significantly less likely to offer family-coverage; no impact on employer “generosity”		
Marquis & Long (2003)	NEHIS & RWJF-EHIS	1993, 1997	Employer effects	State-initiated programs	Offer Rates down from -1.1% ns (all sized firms) to -3.1%* (small, low wage firms)		
Cutler & Gruber (1996) model (c)	CPS- EBS	May 1988 & April 1993	Employer effects	Medicaid expansion	No evidence of any impact for employee-coverage offer rates		
Cunningham, Hadley & Reschovsky (2002)	Community Tracking Study	1996–1997 & 1998–1999	Children	SCHIP	38% to 77% (significance unknown)	N	C
LoSasso & Buchmueller (2002)	CPS	1996–2000	Children	SCHIP	From no empirical support for any public program crowd-out to 50%*** (for 100-200% FPL & assuming CPS measurement error)	N, B	B, E

<sup>1</sup> See Appendix II for full description of methods issues codes.

Definition used: N=Narrow definition of crowd-out  
B=Broad definition of crowd-out

Methods issues: A=Do not include all crowd-out pathways  
B=Study design and specification issues  
C=Did not examine longer-term impact of expansion  
D=Sample sizes are small  
E=Did not take into account the effect of clustering on standard errors

\*\*\* indicates  $p < .01$

\*\* indicates  $.01 < p < .05$

\* indicates  $.05 < p < .1$

<sup>ns</sup> indicates  $p > .10$

# does not use appropriate comparison group; nongeneralizable

## does not use conventional difference-in-difference approach; nongeneralizable

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# THE SYNTHESIS PROJECT

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