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U.S. Department of Labor
U.S. Bureau of Labor Statistics
Office of Compensation and Working Conditions

Medicaid and Wealth: An Examination Using the NLSY79

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Working Paper 448
August 2011

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July 2011

I thank Jay Zagorsky for making available the wealth data used in the analysis, Leighton Ku for providing parameters from state Medicaid programs used in the Urban Institute's TRIM model, Jon Gruber, Robin Mcknight and Aaron Yelowitz for supplying programs and datasets, Alison Aughinbaugh and Steve McClaskie for answering questions about NLSY79 and Maureen Conway, Keenan Dworak-Fisher and especially Brooks Pierce for many helpful discussions. An earlier version of this paper was presented at the annual meeting of the European Association of Labor Economists. The views expressed here are those of the author and do not necessarily reflect the views or policies of the Bureau of Labor Statistics or any other agency of the U.S. Department of Labor.

Abstract

Do public insurance programs crowd out private savings? I examine the relationship between Medicaid and wealth and make a contribution to the literature on this issue in three primary ways. First, I apply the instrumental-variables approach developed by Gruber and Yelowitz (1999) to a different dataset, the National Longitudinal Survey of Youth, 1979 (NLSY79), while at the same time examining an alternative instrument. The results turn out to differ depending on the instrument and, for one of the instruments, to be sensitive to assumptions needed to identify Medicaid's effects. Second, using the longitudinal data in the NLSY79, I am able to observe families before and after becoming eligible for Medicaid, and use fixed-effects to control for family-specific unobservable factors that are correlated with both Medicaid eligibility and wealth accumulation. It turns out, however, that assessment of the impact of Medicaid by means of fixed effects has its limitations as well. Third, I make use of the SIPP data used by Gruber and Yelowitz themselves, and examine the sensitivity of their conclusions to omitted factors that may be related to both Medicaid eligibility and to wealth accumulation. While more robust than the results using the NLSY79, the SIPP estimates are found to depend on the sample used and on certain specification restrictions. Taken together, the results suggest caution in making inferences about the impact of Medicaid on wealth.

I. Introduction

Do public insurance programs crowd out private savings? Hubbard, Skinner and Zeldes (1995) demonstrated theoretically that social insurance programs can lead to lower saving rates on the part of low-income families, by providing a consumption floor and thereby reducing income uncertainty and thus the need for precautionary savings, and by imposing assets limits that, in some circumstances, effectively tax assets at a rate of 100 percent for those who are income-eligible for a program but have savings above the threshold. Working in the opposite direction are the redistributive effects of these programs, which, all else equal, will increase resources and thus savings.

Gruber and Yelowitz (1999), henceforth G-Y, provide a test of the net effect of these three influences, making use of what they argue is exogenous variation in eligibility for public health insurance that came about as a result of the expansions of eligibility for Medicaid in the late 1980s and early 1990s (Currie and Gruber, 1996). Their results using the Survey of Income and Program Participation (SIPP) provide support for the view that asset-based, means-tested social insurance programs can reduce savings among those eligible for it, as they estimate that Medicaid reduced the net worth of this group by 16 percent. While the authors make clear there is still "considerable skewness" in the wealth distribution that is not explained by their findings, they view these effects as being "sizeable".

In this paper, I re-examine the relationship between Medicaid and wealth and make a contribution to the literature in three primary ways. First, I apply the instrumental-variables approach employed by G-Y to a different dataset, the National Longitudinal Survey of Youth, 1979 (NLSY79) for the period 1987-96, while at the same time making use of an alternative instrument. The results turn out to differ depending on the instrument and, for the one used by G-Y, to be sensitive to assumptions needed to identify Medicaid's effects. Second, and this a key reason for using the longitudinal data in the NLSY79, I observe families before and after becoming eligible for Medicaid, and use fixed-effects to control for family-specific unobservable factors that are correlated with both Medicaid eligibility and wealth accumulation. It turns out, however, that assessment of the impact of Medicaid by means of fixed effects has its limitations as well. Third, I make use of the SIPP data employed by G-Y themselves, and examine the sensitivity of their conclusions to omitted factors that may be correlated with both Medicaid eligibility and savings behavior. While more robust than the results using the NLSY79, the SIPP estimates are found to depend on the sample used and on certain specification restrictions.

Taken together, the results suggest caution in making inferences about the impact of Medicaid on wealth. The findings are, however, consistent with the results in G-Y and elsewhere (Powers, 1998; Ziliak, 2003; Hurst and Ziliak 2006; Maynard and Qiu, 2009) that any effect poverty programs may have on wealth accumulation is quite small relative to the gap between the rich and the poor.

II. Background

A. Savings and the Poor

It has been amply documented that a large share of the U.S. population has little or no wealth. For instance, using the Survey of Consumer Finances (SCF), Wolff (1998) calculates that 18.5 percent of households had zero or negative net worth for 1995 (near the end of the period of examination in this paper), a proportion that rises to 28.7 percent if one excludes housing wealth. Put in terms of the number of months financial reserves can be used to sustain normal consumption, for the bottom quintile of the wealth distribution it was essentially zero, while for the quintile next to the bottom it was only 1.1 months.

The question of why so many families accumulate so little wealth has been an active area of research. Of particular interest here is why a key subset of this group – those with low income – has so little in savings. A number of explanations have been offered for this phenomenon (Beverly 1997; Carney and Gale 2001; Dynan, Skinner and Zeldes, 2004). Among these – in addition to an array of psychological and sociological theories -- are a correlation of low income with other determinants of savings such as age or the degree of financial education, and that – relative to the rest of the population – low-income households are more impatient (Lawrance, 1991), have a higher Social Security replacement rate, have more limited access to institutionalized saving mechanisms such as those embodied in 401(k) plans, and, as mentioned, are more affected by the presence of asset-based, means-tested social insurance programs.

B. Medicaid Eligibility

Historically, Medicaid has provided medical coverage for three broad groups of low-income persons: families, children and pregnant women; the aged; and the disabled. Initially, eligibility for the first category, the group that is at the focus of this paper, was tied to the actual or potential receipt of Aid to Families with Dependent Children (AFDC), implying that those eligible were almost exclusively members of very low-income, single-parent families.[1] Beginning in the 1980s, the connection between AFDC

[1] In 1987, the average income eligibility level, based on income cutoffs for AFDC eligibility, was around 60 percent of the poverty line (Cutler and Gruber, 1996).

and Medicaid was gradually loosened by a series of reforms intended to enable pregnant women and children to have better access to health care. These reforms first made eligibility criteria based on family structure less restrictive and then over time either gave states the option or required them to cover children and pregnant women, and at increasingly high income levels.[2] By 1992, according to one estimate, about one-third of the children in the U.S. were eligible for Medicaid coverage of their total medical expenses, and nearly half of all women qualified for coverage of the costs of pregnancy (Gruber and Yelowitz, 1999).

Though these expansions occurred as a result of Federal legislation, the fact that they affected states differently -- because of the pre-existing variation across states in AFDC income-eligibility thresholds and in the extent to which optional provisions of Medicaid had been put in place -- has provided researchers with a source of exogenous variation to study the effect of the Medicaid program on insurance coverage, health care utilization, labor supply, among other outcomes, and for G-Y to study the effect on savings (Gruber, 2003).

The introduction of the State Children's Health Insurance Program (SCHIP) in 1998 and its subsequent expansion has continued the extension of public health insurance eligibility, but for comparability with G-Y, I do not include SCHIP in the analysis. Interestingly, the literature on the effect of public health insurance expansions in the U.S. and the crowd-out of insurance coverage (see Gruber and Simon, 2008 and the references therein) is many times larger than that for savings which mainly consists of G-Y and the

[2] Gruber (2003), Currie and Gruber (1996) and Congressional Research Service (1993) provide more details on the Medicaid program and the series of reforms.

paper by Maynard and Qiu (2009), which applies instrumental quantile regression techniques to G-Y's SIPP data.

III. Data

The primary data source used in this analysis is the National Longitudinal Survey of Youth, 1979 (NLSY79). The NLSY79 is a nationally representative sample of young men and women who were between the ages of 14 and 22 at the time of their first interview in 1979. Individuals have been surveyed annually beginning in 1979 and biennially beginning with 1994. The NLSY79 has been widely used in studies estimating the impacts of poverty programs in the U.S. Less well known is the fact that the NLSY79 is also a source of data on the wealth of the respondent and spouse or partner.[3] While this information on assets has not been as widely used (see Rendon 2007 for one example), an evaluation by Zagorsky (1999) reveals that, once the data are cleaned, the NLSY79 provides data on wealth that are comparable to that from more frequently used databases such as the SIPP, SCF and the Panel Survey of Income Dynamics. This paper makes use of the cleaned net asset series created for this evaluation.[4]

Given that more detailed questions and the probing that accompanies them are thought to lead to a more accurate reporting of wealth, an important limitation of the NLSY79 wealth data is that respondents are asked about only a small number of assets and liabilities, particularly when compared to the number of instruments covered in the SCF. Offsetting this disadvantage is that the NLSY79 has had a high response rate over

[3] Prior to 1994, questions referred to assets and liabilities of the respondent and spouse. In 1994, references to partner were added.

[4] As detailed in Zagorsky (1999), a cleaned net asset series was created by replacing top coded values for the individual assets and liabilities with the original values, removing out-of-range values, imputing for missing values and then summing the components of net wealth. The wealth dataset used in this study was kindly made available by Jay Zagorsky and is the same as that which he used in his assessment of data quality except that net worth has been top-coded to protect the identity of the respondents.

time, which may be indicative of a good rapport between the interviewers and respondents, something that is of obvious benefit in the collection of sensitive information about respondents' financial positions.

From the round of interviews in 1985 through that in 1996, the NLSY79 collected wealth data in every year except 1991, when there was no wealth module, and 1995, when there was no interview because of the switch to a biennial survey schedule. In the statistical work that follows, I make use of the eight years' worth of wealth data for the 1987-96 span, a period that straddles several major Medicaid reforms.[5] Except in cases where I trim the distribution to avoid the undue influence of outliers, I impose no sample restrictions other than that the respondent has valid wealth data for a given year.[6] While analysts, depending on their focus, sometimes exclude the military sample or the supplemental sample of economically disadvantaged whites from their examinations of the NLSY79 (MaCurdy, Mroz and Gritz 1998), I find that my estimates of the impact of Medicaid on savings are not sensitive to these restrictions. To take account of the variation in the probability of being selected for the sample, I use NLSY79 weights in all calculations and regressions.

As background for the analysis that follows, Table 1 presents summary statistics on net worth.[7] Not surprisingly, given that the respondents are still early in their life

[5]While wealth questions have always excluded the assets of the respondent's parents, it is advantageous to start the analysis using data from the 1987 wave because, until that year, if the respondent was living with his/her parents, income from the parents was included in reported family income. The inclusion of parental income complicates attempts to impute Medicaid eligibility consistently over time for the respondent, his/her spouse and children.

[6] For the regression analysis, valid wealth data must be available for at least two years, in order for the respondent to be included in specifications with fixed effects.

[7] In Table 1 and throughout the paper unless otherwise noted, the years given refer to the year of the survey. The NLSY79 asks for respondents to give their wealth at the time of the survey. As will be discussed below, Medicaid eligibility is then imputed based on the characteristics of the household at the time of the survey and on income in the previous year. G-Y measure eligibility for the family in the SIPP using family membership in the wave preceding and including the wealth wave, in order to "smooth any noise in the

cycles, average net worth shows a steady climb during the period, from $27,603 in 1996 dollars in 1987 when the respondents were aged 22 to 30 to $69,382 in 1996.[8] It is immediately apparent from the summary statistics – and no surprise -- that the wealth distribution is highly skewed. Some 18 percent of the sample had a net worth of zero or less in 1987, with about 12 percent of the sample still being in this position in 1996. Those at the 25th percentile, moreover, hold small amounts both in absolute terms and relative to the median or the mean.

The degree of inequality does decline over the period, at least as measured by the ratio of the 75th percentile to the 25th percentile. This trend may seem surprising, as surveys of the whole population do not show any narrowing of the wealth distribution over this span.[9] Further examination (not shown here) reveals, however, that the source of the inequality reduction is attributable to an age rather than time effect: for instance, those who were recent college graduates in 1987 became settled into their careers and thus began to catch up to their older counterparts, and the proportion of those married climbed above one-half, making the presence of a spouse an equalizing rather than disequalizing force.

Despite the overall narrowing of wealth inequality, there is some evidence of growing wealth gaps by education, as shown in Table 2. While families headed by high school dropouts were able to keep pace with those headed by high school graduates, both groups lost substantial ground to other families, particularly those headed by college

measurement of family structure." While the waves are of shorter duration in the SIPP, I experimented with averaging family characteristics over the year of the wealth survey and the preceding year. As the results were quite similar, and as averaging implies a substantial loss of data, I report results only from the first approach.

[8] Unless noted otherwise, all mean wealth values given in the text are calculated after excluding the top and bottom percent of the distribution, in order to avoid the undue influence of extreme outliers.

[9] For instance, Wolff (1998) finds that wealth inequality changed little between 1989 and 1995, after a steep rise between 1983 and 1989.

graduates. Once again, at least part of the gains by college graduates is attributable to a steeper age-wealth profile early in the life cycle, as educational loans are repaid and incomes begin to reflect returns to human capital investments.

Table 3 summarizes trends in the proportion of families eligible for Medicaid in this time period, decomposing this rate into one for families who are eligible for AFDC and one for those who are not. For each year, using the procedure outlined in the appendix, eligibility for Medicaid – either for coverage for all medical expenses or for those related to pregnancy -- was imputed to each family member, based on annual income, state of residence, and family structure. If the family is eligible for AFDC, all family members qualify for Medicaid. In addition, children and pregnant women may be eligible for coverage via the aforementioned eligibility expansions.[10] I refer to the eligibility thus imputed, as "actual eligibility" to distinguish it from the concept of "simulated eligibility" that will be introduced below.

For the NLSY79 cohort, eligibility – defined as one or more family members being eligible -- rose from 6.4 percent in 1987 to a peak of 12.7 percent in 1992, before falling to 10.5 percent in 1996, as shown in the first column of Table 3. While the expansions of Medicaid made qualification steadily easier, the trend in actual eligibility for this cohort is not monotonic because of the growth in family income during the period, itself resulting from this cohort's movement along its age-income profile and the economic expansion during most of the period. It is clear that the rise through 1992 is driven exclusively by eligibility that is not connected with AFDC, as the proportion of

[10] In addition, children living in so-called "Ribicoff states" may also qualify under an optional program that allowed states to cover children in families who are eligible for AFDC on the basis of income, but not family structure.

families who were eligible for Medicaid but not eligible for AFDC climbed from 1.0 percent in 1987 to a high during the period of 8.0 percent in 1994.

IV. Medicaid and Savings: Econometric Issues

The formidable econometric challenges involved in estimating the effect of Medicaid availability on a variety of outcomes have been discussed extensively elsewhere, so I will highlight only the key issues (Gruber, 2003). While eligibility for Medicaid is the key independent variable in research on the crowding out of private health insurance, G-Y argue that what is relevant for household saving decisions are Medicaid-eligible *expenditures*, which are defined as the forecast amount of medical spending that is eligible for the Medicaid program. A measure of a family's Medicaid-eligible expenditures is endogenous, however, as many of the routes to Medicaid eligibility – for example, loss of a job, the birth of a child or the onset of a serious illness -- will affect both Medicaid eligibility and wealth accumulation. Income, which is the key variable in determining Medicaid eligibility, is clearly endogenous to the saving decision, given that: 1) it depends on savings through asset income; 2) labor supply and thus labor income may be influenced by efforts to qualify for Medicaid; and 3) changes in private insurance coverage that result from becoming eligible may have an impact on savings and, to the extent that workers bear the cost for health insurance, on wages (Gruber and Yelowitz, 1999).

When using the NLSY79, I employ two different approaches to address these econometric problems: instrumental variables (IV) and fixed effects (FE). The first employs what G-Y term "simulated eligibility" to construct two family-level variables for Medicaid-eligible expenditures that will be used, separately, as instruments for two different expenditure variables based on actual eligibility. G-Y work only with *total* Medicaid-eligible expenditures, which is the forecast discounted present value of all current and future Medicaid-eligible expenses. While this is the correct construct theoretically, one needs to know how families are forecasting future Medicaid rules, their income situation and their family structure in order to accurately estimate this variable (see the Appendix for details on the assumptions used). Given the amount of information needed and that forecast errors will tend to be non-classical because they will interact with household characteristics in complicated ways that may be correlated with savings behavior, I also use variables representing actual and simulated *current* Medicaid-eligible expenditures.

The key assumption behind both instruments – current and total -- is that the legislated changes in Medicaid policy parameters are exogenous to saving behavior. [11] To calculate the instruments, I first use Current Population Surveys spanning the period and the procedure outlined in the appendix to impute Medicaid eligibility to all members of a national sample, based on the rules in place for each state and each year of analysis. These measures of eligibility are then used to calculate average eligibility by age of child for cells defined by year, state, and the education of the household head. Age of child, state and year are exogenous variables directly linked to Medicaid rules, while education is an exogenous proxy for income. Eligibility for an adult male, though infrequent, is done in the same way. For a woman who is a respondent or spouse, the likelihood of eligibility for medical coverage and for coverage for pregnancy expenses is imputed from average eligibility for cells defined by her education, age, state and year. The probability

[11] "Legislative endogeneity" is a potential source of bias, but concerns here are lessened by the fact that most of the changes came as a result of a federal legislation (Gruber, 2003).

of being eligible for full medical coverage for each family member is then multiplied by an estimate of medical expenditures (conditional on age and sex) derived from the 1996 Medical Expenditure Panel Survey (MEPS, see Appendix Table 1), and then summed over all family members. In households where a respondent or spouse is pregnant, the final step to arrive at the instrument that G-Y call simulated Medicaid-eligible dollars is to add an amount equal to the probability of being eligible for pregnancy coverage times an estimate for pregnancy-related expenditures.[12]

The method of construction implies that the instrumented regressor varies by education (generally of the head, but for medical and pregnancy-related expenditures for a female respondent or spouse, it will vary by her education), state, year and family structure, that is, the ages and number of children and the age and sex of the respondent and spouse, if one exists. As each of these dimensions may be related to savings directly, it is necessary to include controls for each of them. G-Y also include dummy variables for state x year interactions, arguing that these are necessary (and sufficient) to take account of differences in AFDC programs across states and over time. Thus, identification of the effect of Medicaid is achieved through the remaining second- and higher-order interactions. In particular, there is variation across states and over time in the age ranges of children covered. These age "notches" came about because of the way the Medicaid eligibility expansions occurred. For instance, the Omnibus Budget Reconciliation Act (OBRA), 1987 permitted but did not require states to cover children under the age of eight. Later changes required states to extend eligibility to children up to age six with family incomes up to 133 percent of the poverty line (OBRA 1989) and

[12] As discussed in the appendix, expenditures are also adjusted to take into account state-specific medical costs.

required eligibility for all children under age 19 who were born after September 30, 1983 and whose families were below the poverty line (OBRA 1990). Thus, OBRA 1989 and 1990 affected states differently depending, in part, on how they had responded to the optional provisions of OBRA 1987.

The second approach that will be employed with the NLSY79 to identify the effects of Medicaid will be fixed effects, a technique that was not possible using the datasets employed by G-Y.[13] The key variable of interest in this approach is actual rather than simulated Medicaid-eligible dollars, and fixed family effects will be used.[14] These estimates will be consistent under the assumption that any correlation between Medicaid dollars and the error term comes from unmeasured family characteristics that are stable over time.[15]

Each of these approaches has limitations. The IV approach has great intuitive appeal: given that the instrument is formed by holding population characteristics constant and then estimating eligibility according to the rules for each state at each point in time, variation in simulated Medicaid eligibility arguably arises solely from the exogenous source of legislative change. Even so, it is an open question whether the instrument is correlated with omitted variables that influence wealth accumulation, such as unmeasured attributes that may be related to the propensity to save or the likelihood of

[13] While the SIPP and the Consumer Expenditure Survey (CEX), the other dataset used by G-Y, both have longitudinal components, both must be, for different reasons, used only as repeated cross-sections in the study of the impact of public health insurance on saving behavior.
[14] Currie and Thomas (1995) used fixed effects in their assessment of the impact of Medicaid and other forms of health insurance on medical care utilization.
[15] As eligibility for Medicaid is based on family characteristics, for ease of exposition, I will sometimes refer to the fixed effects as pertaining to the family. Technically, however, they are associated with the respondent.

having private health insurance available.[16] In light of differences by state in returns to education (Acemoglu and Pischke 2000) and in the probability of having private insurance (Branscome *et al.* 2000), it is possible to spin stories, some more compelling than others, why omitted interactions may be correlated with legislated changes in Medicaid policy at the state level.

Another omitted interaction that turns out, as we shall see, to be relevant for the NLSY79 results is that between education and number of children at each age. While the relationships between wealth and fertility have not been studied extensively – Browning and Crossley (2001), in a survey of the savings literature, maintain it would be fruitful for consumption to be modeled jointly with fertility and education – it is possible to think of a number of reasons why education groups might differ in terms of the relationship of wealth to the age and presence of children in the household.[17] Those families where parents have invested heavily in their own education may be more likely to put aside money to invest in the human capital of their children. In addition, the consumption needs of children may be proportionately greater in lower-income households than in higher-income ones. Given that the age of parents at the birth of a first child rises with the education level, another possibility is that those with higher education levels are more likely to wait until their finances are in good shape before deciding to have a child. Those who have children at a young age, moreover, may be prevented from making

[16] One cannot include controls for income, normally present in saving and wealth equations, given that it is endogenous with respect to Medicaid. In addition, it is difficult to find an instrument that is correlated with the probability of having private insurance coverage but does not affect savings directly (Starr-McCluer 1996).
[17] Consistent with this idea, Scholz and Seshadri (2009) find that the higher fertility rates of low-earnings households play an important role in accounting for their very low asset holdings.

investments in their own human capital, and may face a period of reduced earnings as a result.[18]

G-Y's approach, however, assumes that all interactions of education, family structure, state and year other than state x year do not affect savings decisions except through Medicaid eligibility. As they acknowledge in the working paper version of their study (Gruber and Yelowitz 1997), a finding that the results are driven by second- as opposed to higher-order interactions would undermine confidence in their estimates, in part because it is easier to come up with plausible scenarios where second-order interactions may influence saving in ways unrelated to Medicaid. To foreshadow later results, the coefficient for *current* Medicaid-eligible expenditures is never significant. For *total* expenditures, much of the variation that is left in the corresponding instrument -- after controlling for main effects and state x year interactions – is that from education interacted with the presence of child at each age. Attempts to test to see if the results are sensitive to controls for this second-order interaction run into difficulties, however, presumably because of the weakness in the instrument that results after taking into account these omitted interactions.

For the fixed-effects models, to take the second approach used on the NLSY79 data, the ability to account for unmeasured characteristics correlated with Medicaid eligibility is a strength, provided these characteristics are stable over time. It is clear, however, that when using actual eligibility, changes in eligibility may be the result of events such as a loss of job, change in family structure, or occurrence of health problems,

[18] Caucutt, Guner and Knowles (2001) discuss some of the links among marriage, fertility and labor market decisions.

for which there are not adequate statistical controls; these events will affect both Medicaid eligibility and saving behavior.

It is well known, moreover, that the use of fixed-effects tends to make the existence of measurement error more problematic. There are, however, reasons to be concerned about measurement error even under the IV approach. Given that the key independent variable is a linear combination of a binary variable indicating eligibility for each family member, it is apparent that errors in the measurement of actual Medicaid-eligible dollars -- arising in part because of the use of annual survey data to impute eligibility for what is a monthly program – will not be classical.

For each family, the upper bound for Medicaid eligible-dollars will be the sum of medical expenses for all family members, while the lower bound will be zero. In such a situation, there is likely to be a negative correlation between the true value of Medicaid eligible-dollars and measurement error, as some families are deemed ineligible when they are, in fact, eligible and vice versa. Except for the simple bivariate case, the sign of the bias of this type of measurement error is unknown (Aigner, 1973). In his discussion of the instrumental variables technique, Gruber (2003) notes that, to the extent that measurement error in the instrument is uncorrelated with measurement error in the measurement of individual eligibility, any biases from measurement error will be avoided by IV. Implicit in this statement seems to be an assumption that measurement error in imputing eligibility is classical, because, as Kane, Rouse and Staiger (1999) reiterate in the case of measuring returns to schooling, IV estimates will generally have a bias of unknown sign when the error is non-random. In addition, as Yazici and Kaestner (2000) have discussed in the case of using simulated eligibility as an instrument for studying the effect of Medicaid expansions on the utilization of private health insurance, expected errors in imputing simulated eligibility are likely to follow a similar pattern as those for actual eligibility. That is, errors will be greater in those cells where the state/education/year indicate a high proportion of families are near the threshold relative to others where that is not the case. As noted earlier, the problem of measurement error is magnified by forecast error in the case of *total* Medicaid-eligible expenditures.

V. Results Using the NLSY79

A. Medicaid-Eligible Dollars

To recap, there are two key independent variables – actual current and total Medicaid-eligible dollars. Using the approach outlined above and detailed in the appendix, eligibility for Medicaid is imputed for each family member in each year, both for full medical coverage and for the coverage of pregnancy expenses. Estimates of annual medical expenses, including those that are pregnancy-related, are then summed over each family member who is deemed eligible for Medicaid, to arrive at an estimate of *current* Medicaid-eligible dollars. But as G-Y note, if families are forward looking, it is the entire future path of Medicaid eligibility that will affect saving decisions. Following their approach, I trace out eligibility of each family member as s/he ages, assuming that the current law remains in place for the indefinite future. *Total* Medicaid-eligible dollars are then computed by summing Medicaid-eligible dollars over each year, discounting future expenditures using an interest rate of 6 percent per annum.

In addition to these two variables calculated on the basis of *actual* eligibility, I also create two variables on the basis of *simulated* eligibility, for use as instruments. Thus, each member of the family receives a probability of being eligible for Medicaid --

instead of being assigned a 1-0 variable. From the eligibility probabilities and the estimates of medical expenses, the expected value of simulated Medicaid eligible dollars is then computed both for the current year and for all years into the future.

As noted, the variable for *total* Medicaid eligible-dollars, though theoretically preferred has stiff information requirements, as does its counterpart instrument. Thus, as a robustness test, I estimate specifications with both current and total dollars.

An indication of the magnitude of the four Medicaid-eligible expenditure variables and their evolution over time is provided in Table 4. Though expenditures covered by Medicaid move in rough accordance with eligibility (shown in Table 3), the relationship between the two Medicaid concepts is complicated by the number of family members who are covered, whether they are covered for all medical expenses or just those related to pregnancy, and, particularly for expenses in future years, the ages of those covered, as the older a child is the less likely that s/he will be covered in future years.

B. Regression Results

For the purposes of regression analysis, a convenient way to handle the fact that wealth is extremely skewed is to use the natural logarithm as the dependent variable.[19] To take into account the fact that those with zero or negative net worth are excluded from the sample when using the log of wealth, I also run a set of regressions where a discrete dependent variable indicates whether the family has positive net worth. A linear probability model (LPM) is used in this case, following G-Y, in order to facilitate the calculation of standard errors in the IV estimation.[20]

[19] See Carroll, Dynan and Krane (2003) for an alternative approach.
[20] In single-equation cases, results from probits were found to be quite similar to those from LPMs.

The basic regression specification is:

(1)

where j and t index family and year, respectively, A is the measure of net worth, MED the Medicaid-eligible dollars variable, ε is the error term and the set of X's includes a constant and the following: age categories for the head of family and for the wife (if one is present), education categories for the head and wife (if one is present), dummy variables for whether the respondent is Black or Hispanic, an indicator for marital status, an indicator for whether the head is female, a set of dummy variables for size of the family, an indicator for a pregnancy that year, a full set of variables indicating the number of children from age 0 to age 18, an interaction between the female's education categories and age categories, a set of year dummies, a set of state dummies, and a set of year x state dummies. As noted, A either measures whether net worth is positive or not, or is the log of net worth, while MED either measures current or total Medicaid-eligible dollars. When an IV approach is used, the corresponding simulated Medicaid-eligible dollars variable is used as an instrument. For fixed effects analysis, , that is, the error term is divided into a fixed respondent-specific component and an idiosyncratic component. Thus, this specification, the IV version of which follows G-Y closely, has first-order controls for education, family structure, state and year, the interactions among which form the MED variables. Except for state x year, however, it is largely devoid of second order interactions.[21]

Table 5 summarizes the results for the two approaches. In addition to the coefficients for models using both current and total Medicaid eligible variables, I also

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[21] For example, education is interacted with the age of the head and wife, but is not interacted, with state, year or the main family structure variables, the age of children variables. The age of children variables are also not interacted with state or year.

present estimates of the total effect of Medicaid on the wealth of the eligibles and this effect's components. Following G-Y, the total effect is calculated under the assumption that the counterfactual to having non-positive wealth as a result of the presence of Medicaid is having the median wealth of those who are both eligible for Medicaid and have positive wealth.

1. Fixed Effects

Identification for fixed effects relies on within-family variation in actual Medicaid-eligible dollars. If this variation arises purely from exogenous changes in legislation, the resulting coefficient estimates will be consistent. As noted above, though, it seems likely that many of the families undergoing an increase in Medicaid eligibility will be experiencing a change in circumstance – such as a job loss or the occurrence of a health problem – that will simultaneously affect Medicaid eligibility and saving behavior. The failure to control for such events will tend toward an overstatement of Medicaid's impact. Thus, in the absence of measurement error, the FE results would provide an upper bound on the magnitude of the effect of Medicaid on savings.

The fixed-effect results are quite similar across the specifications for the current and total Medicaid-eligible dollar variables. For the two Medicaid coverage variables, there is a negative relationship between Medicaid eligibility and both the probability of holding positive wealth and the amount of wealth held, conditional on net assets being greater than zero. Multiplying average Medicaid-eligible dollars among the eligibles by the coefficients from the linear probability models implies that Medicaid reduces the probability of having a positive net worth by about 2 percent. Among those with positive wealth, the coefficients translate into a 15-16 percent reduction of net worth. Combining the two effects and using the assumption indicated above -- that the counterfactual to having non-positive wealth as a result of Medicaid is having the median wealth of those who are both eligible and have positive wealth – one arrives at a reduction in wealth holdings of the eligibles of 15.0 percent using the current variable and 16.3 percent using the total one. As will be the case throughout, most of the total effect of Medicaid on wealth is attributable to a reduction in the wealth of those with a positive net worth, rather than to a change in the proportion of those with non-positive wealth. Given that the eligibles held on average about 3 percent of the wealth of the total population, these estimates imply that the availability of Medicaid is associated with a decrease of under 0.5 percent in aggregate wealth.

2. Instrumental Variables

Before presenting the IV results, it is useful to consider the fit in the first-stage equations, in light of the pitfalls of instrumental-variables estimation when the correlation between the instruments and the endogenous explanatory variables is weak (Bound, Jaeger and Baker 1995; Staiger and Stock 1997). The expectation, in a finite sample, of coefficients generated by instrumental variables does not exist when the system is just identified, as is the case here, which suggests that the finite-sample properties may be poor (Davidson and MacKinnon 1993). In cases where the mean exists -- that is, when the model is overidentified -- F-statistics can be used in combination with the number of instruments to gauge the degree of finite-sample bias (Bound, Jaeger and Baker 1995). The just-mentioned caveat notwithstanding, the F-statistics shown in Table 5 are high enough not to suggest any obvious problem. A second potential problem that may arise with weak instruments is inconsistency in the estimates. As Bound, Baker and Jaeger

(1995) show, the inconsistency of IV relative to OLS is decreasing in the correlation between the instrument and the endogenous regressor (taken after the common exogenous variables have been partialed out). When this correlation is low – and the partial R^2's shown in Table 5 are 0.05 or lower for *total* Medicaid-eligible dollars – then any inconsistency arising from a correlation between the instrument and the error term in the second equation will be exacerbated. The upshot is that even a weak relationship between the instrument and the error term can be problematic in terms of consistency with low partial R^2's, and the partial R^2's fall even further after the inclusion of omitted interactions.

The IV results for the *current* Medicaid-eligible dollar variable indicate, surprisingly, a *positive* and significant relationship between Medicaid eligibility and positive wealth holdings. This relationship is explained, in part, by the fact that male-headed households have, on average, lower amounts of Medicaid-eligible dollars than do two-parent families and also have a lower likelihood of having positive net worth. There is a negative association between Medicaid eligibility and net worth conditional on positive wealth, but the coefficient is not statistically significant. The point estimate for the total effect is –0.074, but it is not possible to reject the hypothesis that this impact is equal to zero.

The IV results using *total* Medicaid-eligible dollars -- the same econometric approach and variable for Medicaid-eligible dollars as in G-Y – suggest a stronger negative impact of Medicaid on wealth. While there is, once again, a positive association between Medicaid and the probability of positive net worth, the coefficient gauging the effect on log wealth is negative and just on the borderline of being significant at the 10 percent level (p-value 0.101). The magnitude of this coefficient implies a reduction of wealth (among those with a positive net worth and eligible for Medicaid) of about 28.9 percent, which is significant at the 5 percent level.[22] The total effect of Medicaid, a 28.0 percent decrease in wealth holdings among all eligibles, is significant at the 10 percent level. This estimate implies that Medicaid is responsible for a 0.8 percent reduction in aggregate wealth.

As noted above, it is possible to make direct comparisons with G-Y only with the *total* variable. In contrast to the results just discussed, G-Y estimate a significant negative relationship between the total variable and both dependent variables. Their coefficients imply a 4.2 percent reduction in the odds of having positive net worth, and one of 12.8 percent in net worth holdings. The finding of a negative relationship between Medicaid and having positive net worth may be attributable in part to the fact that G-Y's data contains families headed by individuals older than those in the NLSY79, as such families are likely to have both a relatively low probability of being eligible for Medicaid and a high probability of having a positive net worth.[23] The total effect calculated by G-Y is a wealth reduction of 16.3 percent, which translates into a 1.3 percent decline in aggregate net worth holdings.[24]

[22] A value of $19,352 for average Medicaid eligible dollars is used to compute this effect. This value is much larger than any of the numbers in Table 4 because it is conditional on Medicaid eligible dollars being positive.

[23] G-Y include in their samples households where the head is between the ages of 18 and 64 and no family member is above the age of 64.

[24] While the estimate of the reduction in holdings among the eligibles attributable to Medicaid in G-Y is smaller than the comparable one here, their estimate for Medicaid's impact on aggregate holdings is larger. G-Y estimate that Medicaid eligibles hold about 8 percent of total wealth, versus my estimate of 3 percent. While our estimates of the ratio of mean wealth of the eligibles to the non-eligibles are about the same, G-Y estimate a larger share of eligibles than I do. As discussed in the appendix, the larger share of eligibles in G-Y appears to stem from differences in the method of imputing pregnancy coverage.

Thus, while my estimate using an instrument for *total* Medicaid-eligible dollars of the effect of Medicaid on the wealth of those eligible is statistically significant and nearly twice that in G-Y, the fact that the estimate of Medicaid's impact using an instrument for *current* Medicaid-eligible dollars is much smaller and not near significance at conventional levels raises a puzzle. Given that the correlation between the two instruments is in the neighborhood of 0.9 and that the correlation between the two endogenous regressors is about the same, and given that the fixed-effects results did not vary much between the two Medicaid-eligible dollar variables, why would the results be so different across these two variables when using IV methods? This puzzle will be considered further in the next section.

VI. Sensitivity Analyses Using G-Y's SIPP Sample and the NLSY79

The results of section V raise some questions that warrant further investigation. First, why do the IV results using the NLSY79 differ from those in G-Y using the SIPP? Second, in light of those differences, how robust are the G-Y results? Third, why do the IV results using the NLSY79 vary by choice of the variable measuring Medicaid-eligible dollars? To address these issues, I first re-analyze the SIPP sample and then I return to the NLSY79 sample.

A. G-Y's SIPP Sample

With the datasets and programs provided by G-Y, I first reproduce their results, as shown in Table 6. The coefficients for total Medicaid-eligible dollars, -0.0081 for positive wealth holdings and -0.0251 for the log of wealth, are significant at the 1 percent level and, as noted above, imply a 4.2 percent reduction in the odds of having positive net worth, and one of 12.8 percent in net worth holdings. As a first step toward reconciling the results of the two datasets, I restrict their sample from having household heads aged 18 to 64 to include only those households where the head was aged between 14 and 22 in 1979, as were the respondents to the NLSY79. The results of this sample restriction are quite striking: neither coefficient is statistically significant, and their magnitudes, particularly the one for the log of wealth, drop dramatically. Thus, at least in the SIPP data, G-Y's overall findings do not apply to the NLSY79 cohort.

It is difficult to pinpoint why effects might differ by cohort. Maynard and Qiu (2009) found that estimates differ by wealth and income quantile for G-Y's full sample, with the effects being strongest at the middle of the distributions. Thus, depending on how any particular cohort is distributed, one would expect a different average effect from the full sample. In addition, the various Medicaid eligibility expansion themselves had varying effects (Card and Shore-Sheppard, 2004), and cohorts will differ to the extent to which they had children of the right age and were in the income range to be affected by any particular expansion.

Besides testing for robustness with respect to age of household head, it is also of interest to assess the sensitivity of G-Y's findings to their assumption that the interactions of education, family structure, state and year other than state x year do not affect savings decisions except through their influence on Medicaid eligible-dollars. A similar exercise was undertaken first by Shore-Sheppard (2008) and then by Gruber and Simon (2008) in the context of assessing the effect of the expansion of Medicaid eligibility on health insurance coverage. In Table 6, I redo the SIPP analysis, including second-order interactions for education x state, education x year, education x age of children, age of children x state, age of children x year, first individually then altogether.[25] The results are generally robust, but there is an important exception. When age x state interactions are included, the coefficient goes down by more than 50 percent, which, with a large increase in the standard error, makes it no longer statistically significant. Thus, it may be the case that state fixed factors have different effects at different ages (Gruber and Simon, 2008). When all the second order interactions are put into the specification, the coefficient for the LPM model becomes positive and marginally significant, while the coefficient in the log wealth model becomes too negative – at -0.4648 it is more than 18 times the magnitude of the baseline coefficient – to be believed. It appears that the inclusion of all these interactions makes the instrument so weak as to render the results unreliable.

B. NLSY79

1. Including Second-Order Interactions

In Section V, a puzzle arose in that the results for the current and total Medicaid-eligible dollars variables were quite different from each other, even though the fixed effects were quite similar and even though the two actual and two simulated variables have very high correlations with each other. Further, when biased downward OLS is used, the two variables yield almost identical results. Using the *current* variable, one obtains an estimate that Medicaid eligibility reduces wealth holdings among those with positive wealth and eligible for Medicaid by 48 percent, compared to the 46 percent estimate for the *total* variable. This similarity suggests that the choice of instrument is

[25] Education refers to four categories for the education level of the household head, state and year are represented by indicator variables, and age of children refer to a series of variables referring to the number of children ages 0 to 18.

driving the results, raising the possibility that there is a direct relationship between the instrument for *total* Medicaid-eligible dollars and wealth.

The main difference between the two instruments is that the *total* variable is disproportionately larger for those with young children, particularly if the family head does not have a high level of education.[26] Though it is conceivable that the variation in simulated total Medicaid-eligible dollars that comes about from interactions between the education of the head and the presence of children at each age 18 or under is related to wealth through its correlation with actual Medicaid eligibility, I discussed in section IV plausible scenarios for such interactions affecting wealth directly.

To investigate this issue further, I reran the IV regressions including the education x age of children interactions, the results of which are shown in Table 7. The coefficient in the positive wealth equation becomes negative, though, not significant, while that in the log wealth equation is negative and significant. Closer inspection, however, reveals that results are not what they first appear. The coefficient has more than tripled in magnitude, implying that Medicaid eligibility reduces wealth holdings among the eligibles by a whopping 66 percent. This figure is simply too large to be believed, as the OLS results, which should be biased downward severely, suggest a 45 percent reduction. Another sign that there is a problem with this estimate is that neither the OLS or fixed-effects results are much affected by the inclusion of the education x age of children interactions, suggesting that actual eligibility is nearly orthogonal to these interactions, while simulated eligibility is not. The inclusion of the additional controls substantially

[26] It may be worth noting that this difference between the current and total variables comes about from my inclusion in Medicaid-eligible dollars of non-pregnancy adult medical expenditures, in contrast to G-Y. When I do not include such expenses, neither variable is significant. See Appendix, particularly its Table 2, for details.

weakens the relationship between the endogenous regressor and instrument (the common exogenous variables explain more than 90 percent of the variation of the instrument, helping to lower to 0.005 the partial R^2 between the total instrument and endogenous regressor), exacerbating any problems arising from a direct relationship between the instrument and wealth, finite-sample bias and measurement error.

In addition to the inclusion of education x age interactions, I also subjected the NLSY79 data to the same sensitivity checks as the SIPP data and these are also presented in Table 7. The somewhat peculiar positive and significant relationship between Medicaid eligibility and positive wealth, found for both variables, is fairly robust. Of greater interest, however, is what happens in the log wealth equations. For the *current* variable, the results are completely consistent in that there continues to be no suggestion of a significant negative relationship between Medicaid eligibility and wealth holdings. The coefficient of the *total* variable, which was nearly significant at the 10 percent level in the baseline results, becomes significant at the 5 percent level when two second order interactions are added (not including the education x age of children interaction already discussed) and at the 1 percent level when all second order interactions are included. The all interactions coefficient is also too large in magnitude to be credible.

2. By Sub-Period

As a final step toward understanding the difference between the SIPP and NLSY79 estimates, I re-estimate the latter by sub-period. Implicitly I have been assuming that all Medicaid eligibility expansions have the same impact, but, as Shore-Sheppard (2008) notes, this does not appear to have been the case, as different expansions have had different take up-rates (Card and Shore-Sheppard, 2004). To allow for this, I follow Shore-Sheppard and divide the sample into two sub-periods, 1987-92 and 1993-96 and re-estimate the IV models on the NLSY79 data. I do not re-estimate the models on the SIPP data, as those data run from 1984-93, so that is there is only one year of data in the second sub- period.

In the first sub-period, changes in Federal requirements led to the eligibility expansions, while, in the second, States were given the option to extend eligibility. In the process, families with relatively higher incomes became eligible. The expected effects of dividing the sample are not clear. Better-off families may have been less aware of their Medicaid options or may face greater stigma from using such a program and thus would be less likely to have their behavior affected, pointing to weaker effects in the second period. Maynard and Qiu (2009), in their re-examination of G-Y's data, find that effects are strongest toward the middle of the wealth and income distributions and weak or nonexistent at the top and bottom, so the mean effect would depend on exactly which types of families are affected by the change in eligibility.

As it turns out, there are stark differences between the two sub-periods, as shown in the bottom panel of Table 7. For 1993-95, there is no sign that Medicaid deters savings. In fact, there is a positive, though not significant, relationship between Medicaid-eligible dollars and wealth even among those with positive wealth. The situation is different in 1987-92, in that the coefficient on *total* Medicaid-eligible dollars is negative and significant, and, at -0.0257, fairly sizeable. Nonetheless, the puzzle remains that the *current* Medicaid-eligible variable is not significant. Thus, restricting the analysis to 1987-92 has helped to reconcile the SIPP and NLSY79 results for the *total* variable, but even for this period, the results are not robust to the choice of variable.

VI. Conclusions

Does public health insurance reduce wealth holding? This paper has taken a fresh look at this important question. First, I applied the instrumental-variables approach employed by G-Y to a different dataset, the National Longitudinal Survey of Youth, 1979 (NLSY79) for the period 1987-96, while at the same time examining an alternative instrument. Second, and this was a key reason for using the longitudinal data in the NLSY79, I observed families before and after becoming eligible for Medicaid, and used fixed-effects to control for family-specific unobservable factors that are correlated with both Medicaid eligibility and wealth accumulation. Third, I made use of the SIPP data used by G-Y themselves, and examined the sensitivity of their conclusions to omitted factors, as well as to the restriction of their sample to household heads of the same age as those in the NLSY79 cohort.

The NLSY79 results turn out to differ depending on the choice of instrument. Using *current* Medicaid-eligible dollars, there is no evidence that Medicaid deters saving. For the theoretically preferred instrument, *total* Medicaid-eligible dollars, the instrument used by G-Y, the evidence is more ambiguous, as there sometimes is a negative relationship and sometimes not, depending on specification and time period. The difference between the results for the two variables raised questions, however, about the validity of the second instrument – whether it included education x age of children interactions that might be related directly to wealth – but testing that suspicion proved difficult. It turns out, moreover, that assessment of the impact of Medicaid using fixed effects had its limitations as well. Given the likelihood that changes in Medicaid eligibility are correlated with unobservables that are not constant over time, one would have reason to believe that these estimates would provide an upper bound. Any measurement error would, however, bias the estimates in the opposite direction. Finally, while results using SIPP data were certainly more robust than those using the NLSY79, the estimates even with this dataset were sensitive to age of head restrictions and to the inclusion of age of children x state interactions.

In sum, then, it seems one must be cautious about inferring the effects of Medicaid on wealth. It is the case, however, that, despite ambiguity about the effect of Medicaid on the wealth of those eligible for it, the credible estimates of the impact of Medicaid on aggregate wealth holdings were consistently small. Thus, this paper adds to a growing body of literature (Gruber and Yelowitz 1999; Powers 1998; Ziliak 2003; Hurst and Ziliak 2006; Maynard and Qiu 2009) that suggests that public assistance programs explain little in terms of the overall skewness of the distribution of wealth, implying that researchers will have to look elsewhere for explanations of the low saving rate among the income-poor.

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Table 1
Summary Statistics on Wealth Holdings
NLSY79, 1987-1996

Survey Year	Mean	Fraction with Positive Net Worth	First Quartile	Median	Third Quartile	First Quartile/ Median	Third Quartile/ Median
1987	27,603	0.82	1,381	9,668	33,148	0.14	3.43
1988	38,174	0.84	1,857	12,202	43,768	0.15	3.59
1989	42,378	0.84	2,157	14,551	52,511	0.15	3.61
1990	45,315	0.85	2,407	16,806	59,183	0.14	3.52
1992	50,496	0.86	3,243	21,472	67,211	0.15	3.13
1993	53,437	0.86	3,259	25,245	74,813	0.13	2.96
1994	60,276	0.88	4,976	30,702	86,108	0.16	2.80
1996	69,382	0.88	6,600	40,200	106,000	0.16	2.64

Notes: All values converted to 1996 dollars. Cross-sectional sample weights are used. Total sample size over all years is 60,689. Top and bottom 1 percent of wealth distribution in each year excluded in calculations of mean wealth.

Table 2
Wealth Holdings by Education of Head
NLSY79, 1987-1996

Survey Year	Mean Wealth				Ratio of Mean Wealth to that for High School Graduates		
	Less than High School	High School Graduate	Some College	College Graduate	Less than High School	Some College	College Graduate
1987	12,315	27,682	26,361	37,633	0.44	0.95	1.36
1988	17,321	36,840	40,399	50,873	0.47	1.10	1.38
1989	15,350	37,969	45,296	62,457	0.40	1.19	1.64
1990	16,910	40,362	47,563	67,044	0.42	1.18	1.66
1992	19,074	40,779	52,634	81,900	0.47	1.29	2.01
1993	20,105	42,454	55,070	86,608	0.47	1.30	2.04
1994	22,441	46,658	60,393	100,351	0.48	1.29	2.15
1996	26,267	55,014	65,910	116,239	0.48	1.20	2.11

Notes: All values converted to 1996 dollars. Cross-sectional weights are used. Total sample size over all years is 60,689. Top and bottom 1 percent of the sample in each year excluded in calculations of mean values, as are 413 observations with missing education data. If respondent is male, he is considered head. If respondent is female she is considered head if not married, but her spouse is considered head if she is married.

Table 3
Annual Eligibility for Medicaid
NLSY79, 1987-1996

	Percentage of Families Eligible for Medicaid		
Survey Year	Total	Eligible via AFDC	Not Eligible via AFDC
1987	6.4	5.4	1.0
1988	7.0	5.1	1.9
1989	7.7	5.1	2.6
1990	8.6	4.9	3.7
1992	12.7	5.2	7.5
1993	12.6	5.1	7.5
1994	12.1	4.1	8.0
1996	10.5	3.7	6.8
All years	9.6	4.8	4.8

Notes: Those eligible via AFDC may also be eligible through other means. Imputed based on Medicaid rules. Respondent's family considered eligible if imputation indicates coverage for medical expenses of any family member or for expenses of an actual pregnancy. See appendix for details. Total sample size is 60,689. Cross-sectional weights are used.

Table 4
Medicaid Eligible Dollars
NLSY79, 1987-1996

	Actual			Simulated		
Survey Year	Current Year	Future Years	Total	Current Year	Future Years	Total
1987	210	1729	1940	268	1588	1856
1988	235	1786	2021	311	1694	2005
1989	263	1878	2141	369	1851	2220
1990	279	1729	2008	407	1824	2231
1992	381	2093	2474	516	2146	2662
1993	391	2018	2409	503	2148	2650
1994	342	1677	2018	496	2207	2702
1996	283	1538	1821	456	2127	2583
All years	298	1807	2105	416	1947	2363

Notes: All values converted to 1996 dollars. Cross-sectional weights are used. Sample size is 60,689 for measures of actual Medicaid-eligible dollars and 60,374 for those of simulated Medicaid-eligible dollars. See appendix for details on calculations.

Table 5
Regression Analysis: The Impact of Medicaid on Wealth of the Eligibles
NLSY79, 1987-1996

Medicaid-Eligible Dollars:	Fixed Effects: Current	Fixed Effects: Total	Instrumental Variables: Current	Instrumental Variables: Total
Dependent Variable				
Wealth > 0				
	-0.0068***	-0.0011***	0.0190**	0.0036**
	(0.0015)	(0.0002)	(0.0086)	(0.0016)
F-statistic			345.99	257.74
Partial R^2			0.0676	0.0455
ln (wealth)				
	-0.0532***	-0.0089***	-0.0302	-0.0176
	(0.0062)	(0.0009)	(0.0537)	(0.0107)
F-statistic			206.31	162.39
Partial R^2			0.0512	0.0299
Impact of Medicaid on the Wealth of Eligibles				
P(Wealth>0)	-0.021***	-0.024***	0.059**	0.079**
	(0.005)	(0.005)	(0.027)	(0.035)
Wealth| Wealth > 0	-0.146***	-0.159***	-0.086	-0.289**
	(0.016)	(0.015)	(0.146)	(0.147)
Total Effect	-0.150***	-0.163***	-0.074	-0.280*
	(0.017)	(0.018)	(0.144)	(0.147)

Notes: First two panels show coefficients and standard errors for Medicaid eligibility variables measured in 1000s of 1996 dollars, along with the F statistic on the excluded instrument in the first stage and partial R^2 between the instrument and the endogenous regressor after the common exogenous variables have been partialed out of both. Specifications also include controls for the age, education, and sex of head, age and education of spouse (if present), race and ethnicity of respondent, marital status, size of family, the number of children aged 0-18, whether the respondent or spouse is pregnant, interactions between education and age for female respondent or spouse, state, year, and state*year interactions. Regressions with (wealth > 0) as dependent variable are estimated using linear probability models. The impacts are computed by using the mean value of Medicaid-eligible dollars for all with dollars > 0 (for computing impact on probability of having positive wealth) and that for all with dollars > 0 and positive net worth (for computing impact on wealth holdings, conditional on having positive wealth). For families for whom it is estimated that they would have positive wealth in the absence of Medicaid, it is assumed that their wealth would move from 0 to the median wealth of those families who have both positive wealth and positive Medicaid-eligible dollars. Sample sizes are 59,284 for linear probability models and 47,514 for log wealth models. Weight at beginning of the period is used. Standard errors for instrumental-variable models take into account the presence of multiple observations per respondent. Standard errors for impact on wealth conditional on positive holdings calculated via delta method, while standard errors for total effect calculated by bootstrapping.

* significant at 10 percent level, ** significant at 5 percent level, *** significant at 1 percent level

Table 6
Re-analysis of Gruber-Yelowitz (1999) IV Models Using SIPP Data
Restricting to NLSY79 Cohort and Including Second-Order Interactions
Coefficients and Standard Errors for Total Medicaid-Eligible Dollars

Dependent Variable:	Wealth>0	ln(wealth)
Gruber-Yelowitz (1999)	-0.0081***	-0.0251***
	(0.0009)	(0.0054)
NLSY79 Cohort Only	-0.0034	-0.0031
	(0.0022)	(0.0124)
Full sample		
Interactions included:		
Education x age of children	-0.0097***	-0.0303***
	(0.0013)	(0.0082)
Education x state	-0.0054***	-0.0385***
	(0.0010)	(0.0066)
Education x year	-0.0083***	-0.0217***
	(0.0009)	(0.0055)
Age of children x state	-0.0076***	-0.0115
	(0.0010)	(0.0345)
Age of children x year	-0.0078***	-0.0331***
	(0.0009)	(0.0061)
All second order	0.0084*	-0.4648***
	(0.0048)	(0.0890)

Notes: Specification the same as in Gruber and Yelowitz (1999), Table 5 except as noted. For row labeled "NLSY79 Cohort Only", sample is restricted to households where head was aged between 14 and 22 in 1979.

* significant at 10 percent level, ** significant at 5 percent level, *** significant at 1 percent level

Table 7
Sensitivity Analysis of IV Models Using NLSY79, Including Second Order Interactions and By Sub-Period

Dependent Variable:	Wealth>0		ln(wealth)	
Medicaid-Eligible Dollars:	Current	Total	Current	Total
Baseline	0.0190**	0.0036**	-0.0302	-0.0176
	(0.0086)	(0.0016)	(0.0537)	(0.0107)
Interactions included				
Education x age of children	0.0084	0.0003	-0.0376	-0.0562**
	(0.0124)	(0.0035)	(0.0815)	(0.0274)
Education x state	0.0207**	0.0039**	-0.0386	-0.0206**
	(0.0084)	(0.0016)	(0.0519)	(0.105)
Education x year	0.0225***	0.0041**	-0.0055	-0.0132
	(0.0088)	(0.0016)	(0.0552)	(0.0109)
Age of children x state	0.0194**	0.0038**	-0.0578	-0.0199**
	(0.0087)	(0.0016)	(0.0524)	(0.0102)
Age of children x year	0.0139	0.0034**	-0.0271	-0.0173
	(0.0090)	(0.0016)	(0.0585)	(0.0106)
All second order	-0.0102	-0.0022	-0.0961	-0.1106***
	(0.0195)	(0.0054)	(0.1398)	(0.0463)
By sub-period				
1987-92 sample	0.0203	0.0035**	-0.0377	-0.0257**
	(0.0108)	(0.0017)	(0.0638)	(0.0110)
1993-95 sample	0.0168	0.0044*	0.0659	0.0138
	(0.0113)	(0.0022)	(0.0812)	(0.0175)

Notes: Estimates are from regressions of the specification detailed in the notes to Table 5 using different instruments except as noted. Coefficients and standard errors from Medicaid-eligible dollar variables measured in 1000s of 1996 dollars.

* significant at 10 percent level, ** significant at 5 percent level, *** significant at 1 percent level

Appendix

Imputing Medicaid Eligibility

Focusing exclusively on eligibility for low-income families, children and pregnant women (and thus ignoring eligibility for the aged and the disabled), there are essentially three routes to Medicaid eligibility during the span examined: 1) a family that qualified for AFDC is automatically eligible for Medicaid; 2) states may also cover so-called Ribicoff children, that is, children whose families are income-eligible for AFDC, but do not meet family structure criteria; 3) children and pregnant women may be eligible as part of expansions of Medicaid eligibility that began in the mid-1980s.

Using a family's annual income less any means-tested components, rules for each of these routes for a given state and year are applied to each family member. The approach is essentially the same as that discussed in Currie and Gruber (1996) – henceforth C-G. For a single-parent family with a child 18 or under to qualify for AFDC, the family must meet three income tests: 1) gross income must be below 1.85 times the state's need standard for that family size; 2) gross income minus disregards for child care (for children under 6) and work expenses must be below that need standard; 3) gross income minus the disregards minus a portion of earnings must be below the payment standard.

During most of the period, the portion of earnings that was disregarded in calculating AFDC payments was regulated by a so-called "30 and 1/3 rule", allowing AFDC families to keep $30 and 1/3 of their earnings in each of the first four months of recipiency and $30 per month for the next 8 months (U.S. House of Representatives 1994). From 1985 onward, families who would have become ineligible for AFDC and

Medicaid after four months as a result of increased earnings were permitted to remain eligible for Medicaid for nine to 15 months depending on the state; in modeling this provision, I follow C-G and use a $30 and 1/3 rule for the entire year.

In states where AFDC was available for two-parent families via the Unemployment Parent Program[1], I use the approach of C-G and deem eligible for the program families who meet the above tests and have a spouse working fewer than 40 weeks a year. In addition, children whose families meet the income tests for AFDC but not that for family structure could qualify if their state has a Ribicoff program in place and they are under the age limit for coverage. For Medicaid expansions, pregnant women and children under certain ages could qualify if their family's income was below a cut-off, expressed as a proportion of the poverty line. C-G note that there was some uncertainty about which AFDC disregards applied in making this eligibility determination. I follow the approach of computer programs generously provided by Jonathan Gruber and apply the disregards for child care and work expenses but not those embodied in the 30 and 1/3 rule.

Monthly AFDC benefits and needs standards were taken from unpublished U.S. Department of Health and Human Services data. Information on the availability of AFDC-UP was taken from Franco and Solomon (1987) and U.S. DHHS (1990-91). Parameters for eligibility for Ribicoff children and expansions of eligibility for pregnant woman and children were taken from reports of the National Governors' Association (various issues) and unpublished worksheets from the Urban Institute's TRIM model.

[1] All states were required by the Family Support Act (FSA) of 1988 to have such a program in place by October 1, 1990, but only about half did prior to the passage of FSA.

The imputations for actual eligibility were done using the NLSY79. For simulated eligibility, the March supplements of the Current Population Survey (CPS) for the all even years from 1988 to 1996 were used. Each state's rules for each year in the period were applied to the entire combined CPS sample. The proportions of children and adult males who were imputed as eligible for coverage of medical expenses were then calculated by age for cells defined by year, state, and education of the head[2]. For adult women, the proportions who were imputed as eligible for coverage of medical expenses and for pregnancy-related expenses were calculated by age group for cells defined by year, state, and education of the women.[3] These cell averages were then matched to the NLSY79 according to these characteristics.

Measuring Medical Expenditures

With (actual or simulated) eligibility for Medicaid available, the final requirement for calculating Medicaid eligible dollars is to measure medical expenses. Using the 1996 Medical Expenditure Panel Survey, average total medical expenditures were calculated by age and sex. This measure of expenditures includes all costs, that is, those borne by the family, private insurance carriers, the government through Medicaid and Medicare programs, and any other parties. In addition, a measure of the expenses of pregnancy was calculated by subtracting the average medical expenses of women aged 15-44 who were not pregnant anytime during the year from their counterparts who were. The resulting estimates for the nation are presented in Appendix Table 1. To take into account, differences across states in medical costs, I follow G-Y and normalize these by a state

[2] Four groups are used for education: less than high school, high school graduate, some college, and college graduate.

[3] From a single cross-section of the March CPS, it is not possible to know who is pregnant and who is not. As a result, I follow the approach of Gruber and his colleagues, and impute whether or not women would be eligible for pregnancy coverage if they were pregnant.

index. This index is computed by first calculating the average Medicaid expenditures for one adult and two children over FY 1990 through FY1996 in 1996 dollars using data from unpublished Health Care and Financing Administration worksheets. Second, I divide by the average for the median state.

Calculating Medicaid Eligible Dollars

After imputing eligibility and calculating medical expenditures by age and sex, it is straightforward to compute *current* Medicaid eligible dollars for each family. When using actual eligibility, one sums the medical expenditures for each eligible family member and then adds to it the estimate for pregnancy expenses in cases where a woman in the household is found to have been both eligible for pregnancy coverage and pregnant during the year. Using simulated eligibility, the calculations are only slightly different in that eligibility is now a probability rather than a 1-0 variable.

Calculating *total* Medicaid eligible dollars is, however, more complicated. To simplify matters, I follow the approach of G-Y and assume a static environment. First, families are assumed to believe that the current program rules will stay in place indefinitely. Second, it is assumed that the economic situation of the family stays constant, that is, that the relationship of its income to AFDC and Medicaid thresholds does not change over time. Third, the size of the family is assumed to not change over time, except in the case where a woman is known to have been pregnant during the year. Thus, besides the expenses for pregnancy being counted in the current year and those for the newborn child being counted in the future years, the only reason why Medicaid eligible dollars vary in future years is the aging of family members.

Using actual eligibility, therefore, a family that qualifies for AFDC in the current year is assumed to be eligible indefinitely, as long as a child aged 18 or under is present. In each future year, medical expenditures are summed up over all children of eligible ages, and for the parent (or parents), as long as a minor is present. Children may also continue to qualify for the Ribicoff program until their age exceeds the cut-off. Finally, future eligibility under the Medicaid expansions can be determined by "aging" the children and applying the current rules.

The situation using simulated eligibility is somewhat different. As the child ages, s/he is assigned an age-appropriate probability of being eligible for Medicaid. Any adults in the household are also assigned an eligibility probability, provided a minor is present in the household. For both actual and simulated total Medicaid eligible dollars, future expenditures are discounted to the present using a 6 percent annual discount rate.

<u>Differences Vis-à-vis G-Y</u>

A comparison of results generated by my programs using the CPS and those from the programs currently in use by Jonathan Gruber indicates that the two arrive at similar time series for Medicaid eligibility for children and pregnant woman. In calculating Medicaid eligible dollars, however, G-Y do not include medical expenses for adult women, other than those who are pregnant, nor any for adult men, who qualify for Medicaid in a small proportion of the cases. As I see no reason why expenses for adults should be excluded, I include them.

In addition, G-Y do not use actual information of pregnancies, but instead use an age-specific fertility rate. Thus, for actual Medicaid-eligible dollars, all women of child-bearing age who would be eligible for Medicaid in the event of a pregnancy are assigned an amount equal to the product of the fertility rate and an estimate of pregnancy-related expenses. Similarly, for simulated Medicaid-eligible dollars, all women of child-bearing age will be assigned an amount equal to the product of the likelihood of being eligible if pregnant (taken from the appropriate cell), the fertility rate and the estimate for pregnancy-related expenses. As I have information on whether or not a woman actually gave birth, I only assign Medicaid eligible dollars to women that I know were actually pregnant.

To see if these differences affected the findings, I recalculated Medicaid eligible dollars, with the following changes: 1) no expenses for medical coverage were included for adult men and women (other than for pregnancy); 2) all women of child-bearing age who would be eligible for Medicaid in the event of a pregnancy were assigned Medicaid eligible dollars based on multiplying expenses for pregnancy by an age-specific fertility rate; and 3) I trace out expected medical expenditures for any births with a positive probability of having occurred, rather than including expenses only for an actual newborn as s/he ages.

Regression results using recalculated Medicaid eligible dollar variables are shown in Appendix Table 2. For the fixed-effects results, the patterns of signs and significance of coefficients are almost identical to those of Table 5. The coefficients are somewhat larger in magnitude with the recalculated variables. But offsetting this in terms of estimates of the impact of Medicaid on the wealth of the eligibles is the fact that the average for Medicaid eligible dollars among the eligibles is now lower, given that, as a result of the treatment of pregnancy eligibility, they are spread over a larger portion of the population. On net, the total impact is about 13 percent, slightly lower than the 16 percent in Table 5.

There is a bigger contrast for the IV results, particularly those for the total Medicaid-eligible dollars variable. The estimated total impact is that Medicaid reduces wealth among the eligibles by 7 percent, but this estimate is not close to being significant at conventional levels. In contrast, in Table 5, the estimated impact is a reduction of 28 percent, significant at the 10 percent level.

To see where these differences were coming from, I ran regressions where adult expenditures were included and pregnancies were treated as in G-Y, and where adult expenditures were excluded as in G-Y but pregnancies were not treated probabilistically. Most of the difference appears to come from the exclusion of adult expenditures. In the results in the text, adult expenditures – primarily for women, as men are rarely eligible – are highest in families where the adult female is less educated and, particularly for the total variable, has young children. Given that such families also tend to have lower wealth, Medicaid's measured impact is stronger when such expenditures are included.

Appendix Table 1
Medical Expenditures

Annual Medical Expenditures		
Age	Male	Female
0	2275	2464
1	2738	1806
2-5	546	403
6-9	670	392
10-14	718	636
15-19	783	1990
20-29	1281	1263
30-39	980	1967
40-49	1577	1913
50-59	2773	2670
60-69	3921	3766
70-79	5212	4174
80-89	6995	6836
Pregnancy-Related Medical Expenditures		
15-44		3175

Notes:

Expressed in 1996 dollars. Calculated from 1996 Medical Expenditure Panel Survey (MEPS) using variable for total health care expenditures. Pregnancy related expenditures calculated as difference in total expenditures between women aged 15-44 who were pregnant during the year, and those who were not.

Appendix Table 2
Regression Analysis: The Impact of Medicaid on Wealth
Using Recalculated Medicaid Eligible Dollar Variables
NLSY79, 1987-1996

Medicaid-Eligible Dollars:	Fixed Effects: Current	Fixed Effects: Total	Instrumental Variables: Current	Instrumental Variables: Total
Dependent Variable				
Wealth > 0				
	-0.0139***	-0.0025***	0.0304**	0.0063**
	(0.0024)	(0.0004)	(0.0130)	(0.0026)
F-statistic			5178.1	3981.2
Partial R^2			0.0803	0.0629
ln (wealth)				
	-0.0104***	-0.0203***	-0.0374	-0.0116
	(0.0101)	(0.0018)	(0.0862)	(0.0190)
F-statistic			2921.4	1941.8
Partial R^2			0.0579	0.0393
Impact of Medicaid on the Wealth of Eligibles				
P(Wealth>0)	-0.019***	-0.019***	0.041**	0.048**
	(0.003)	(0.003)	(0.018)	(0.020)
Wealth| Wealth > 0	-0.126***	-0.129***	-0.047	-0.075
	(0.011)	(0.011)	(0.106)	(0.119)
Total Effect	-0.129***	-0.132***	-0.038	-0.065
	(0.013)	(0.012)	(0.078)	(0.078)

Notes: First two panels show coefficients and standard errors for *recalculated* Medicaid eligibility variables measured in 1000s of 1996 dollars, along with the F statistic on the excluded instrument in the first stage and partial R^2 between the instrument and the endogenous regressor after the common exogenous variables have been partialed out of both. Specifications also include controls for the age, education, and sex of head, age and education of spouse (if present), race and ethnicity of respondent, marital status, size of family, the number of children aged 0-18, interactions between education and age for female respondent or spouse, state, year, and state*year interactions. Regressions with (wealth > 0) as dependent variable are estimated using linear probability models. The impacts are computed by using the mean value of Medicaid-eligible dollars for all with dollars > 0 (for computing impact on probability of having positive wealth) and that for all with dollars > 0 and positive net worth (for computing impact on wealth holdings, conditional on having positive wealth). For families for whom it is estimated that they would have positive wealth in the absence of Medicaid, it is assumed that their wealth would move from 0 to the median wealth of those families who have both positive wealth and positive Medicaid-eligible dollars. Sample sizes are 59,284 for linear probability models and 47,514 for log wealth models. Weight at beginning of the period is used. Standard errors for instrumental-variable models take into account the presence of multiple observations per respondent.

* significant at 10 percent level, ** significant at 5 percent level, *** significant at 1 percent level