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Does a Higher Income Have Positive Health Effects? Using the Earned Income Tax Credit to Explore the Income-Health Gradient

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Context: The existence of a positive relationship between income and morbidity has been well documented in the literature. But it is unclear whether the relationship is positive because increased income allows individuals to purchase more health inputs that improve their health, because healthy individuals are more productive and thus can earn higher wages in the labor market, or because a third factor is improving health and increasing income. This article explores whether increases in income improve the health of the low-income population.

Methods: Because health status may affect income, this article uses an "instrumental variable" strategy that considers income variations over seventeen years of changes in the generosity of state and federal Earned Income Tax Credits (EITC, a measure that should be exogenous to health status). I measured health status using both the self-reported health status and the functional limitations indicated on the Survey of Income and Program Participation (SIPP), as well as the self-reported health status indicated on the March Current Population Survey (CPS).

Findings: I found only limited support for the theory that the relationship between income and morbidity is derived from shifts in income. Although I did observe a correlation between income and self-reported health, I found no evidence that increases in income significantly improve self-reported health statuses. In addition, while increases in income appear to reduce the prevalence of hearing limitations when using corrective measures, these increases did not have a significant effect on most of the other functional limitations considered here.

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Conclusions: These findings suggest that the ability to improve short-term health outcomes through public transfer payments may be limited. However, the lifetime effects on the health of people with higher incomes would still be a valuable avenue for future research.

Keywords: Health status, low-income population, income-health gradient, EITC.

The evidence of a positive correlation between individuals' socioeconomic status and their health outcome is extensive, with high-income individuals tending to be in better health than low-income persons. This relationship has been observed for a wide range of health measures, including mortality (Backlund, Sorlie, and Johnson 1996; McDonough et al. 1997), chronic conditions (Case, Lubotsky, and Paxson 2002), obesity (Schmeiser 2009), functional limitations (Zimmer and House 2003), and self-reported health status (Deaton and Paxson 1998). Even though the existence of this positive correlation is generally accepted, there is no clear consensus on the direction or the pathway of the relationship. For example, Case, Lubotsky, and Paxson (2002) and Lindahl (2005) suggest that a higher income causes improvements in health outcomes, and Arno and colleagues (2009) present preliminary evidence that income-support programs increase access to health insurance and improve certain health outcomes. Bound (1989), Haveman and colleagues (1995), and Smith (1999, 2004), however, maintain that lower incomes are due to the decline in productivity that results from poor health and disabilities rather than the reverse. Still others, like Fuchs (1982), propose that an outside factor, such as a high discount rate, could lead to poor health and low income, thereby creating the observed correlation.

Distinguishing which pathway or pathways drive this relationship is extremely valuable to understanding the costs and benefits of both public health policies and public transfer programs. For example, increasing the number of income-support programs may be warranted if a higher income is demonstrated to have a positive health effect (Arno et al. 2009; Deaton 2002; Herd, Schoeni, and House 2008; Lindahl 2005). One of the primary income-support programs in the United States is the Earned Income Tax Credit (EITC), which provides a tax credit that is calculated as a percentage of a family's labor earnings. Unlike traditional welfare programs targeted at nonworkers, the EITC is available only to low-income individuals with labor earnings, thereby giving them an incentive to work. Accordingly, if hypotheses that income-support programs improve low-income individuals' health outcomes are correct, we might question the wisdom of proposals to eliminate EITC benefits or other income-support programs, as recommended by the President's Commission on Deficit Reduction (National Commission on Fiscal Responsibility and Reform 2010).

Thoroughly exploring the direction of the income-health relationship requires using exogenous variations in either health or income, which should not be affected by reverse causality, and then evaluating the resulting impact of this variation on the other variable. Smith (1999, 2004) did this by using exogenous variations in health from the unanticipated onset of a chronic condition. He determined that health status had a causal impact on working-age adults' income and wealth. The existence of a causal link in one direction, however, does not preclude the existence of a link in the other direction as well.

Several earlier studies have used two approaches to discover whether exogenous variations in income influence health outcomes. The first method uses random variation in income, such as lottery winnings, to compare the health outcomes of individuals receiving the extra income with those of persons not receiving extra income. The second method is an instrumental variable approach that uses policy variations, such as state-level EITC benefits, to separate the change in income that is *not* affected by health status from the change in income that *is* affected by health status. Such studies have had mixed findings pertaining to the strength of income shocks' causal impact on health outcomes. For example, when comparing the health of midsize lottery winners in Sweden with the health of nonwinners, Lindahl (2005) found evidence that health status improves with a positive income shock. Similarly, Balan-Cohen (2009) observed that the mortality rate of elderly people dropped when old age assistance rose. In contrast, in an instrumental variable approach, Schmeiser (2009) used variation in EITC benefits to discover that women's obesity rates rose along with their income. Snyder and Evans (2006) also found that health declined as a result of exogenous increases in income. In particular, lower Social Security benefits owing to the Social Security Notch, which decreased benefits for individuals born between 1917 and 1921, resulted in lower mortality rates. But several researchers have noted that, based on the diminishing marginal returns for health spending, the gradient between income and health should be strongest for low incomes (Rodgers 1979). As a result, Herd, Schoeni, and House (2008) criticized Snyder and Evans's findings, stating that the main effect of the Social Security Notch is greater in the income distribution in which the income-health gradient is weaker.

In this article I explore the effects of a variation in income on individuals' morbidity status by using an instrumental variable (IV) approach. This looks at seventeen years of changes in state and federal Earned Income Tax Credit (EITC) benefits to estimate how increases in income influenced the health of low-income individuals. During the 1990s, state and federal guidelines for EITC benefits changed dramatically, and these changes in benefits led to substantial shifts in the incomes of poor families. As a result, these variations provide a natural experiment for exploring how changes in income influence the health of low-income individuals, since the changes in state and federal EITC benefits should be exogenous to individuals' health statuses.

Using variations in EITC benefits to address the direction of the income-health gradient has three advantages. First, earlier research indicated that the income-health gradient is strongest in the low-income population (Backlund, Sorlie, and Johnson 1996; Herd, Schoeni, and House 2008; McDonough et al. 1997). Therefore, since the EITC is targeted at the low-income population, it can be used to analyze this group of particular relevance to the gradient. Second, because the EITC-eligible population consists of working-age adults, it can better show the impact of an exogenous shift in income on a younger population than did the earlier studies that looked at changing benefits for retirees. Third, because I am examining the morbidity of working-age individuals, I can use the large sample sizes of the March Current Population Survey (March CPS) data and the Survey of Income and Program Participation (SIPP). These two large, nationally representative data sets strengthen the estimates of observed effects over those found using smaller samples. This in turn allows for more precise estimates of the impact of income on morbidity than would otherwise be possible. Of course, studying the EITC is limited in that recent research has found that unlike income from other sources, many people spend their Earned Income Tax Credit on consumer durable goods (Barrow and McGranahan 2000; Goodman-Bacon and McGranahan 2008). This difference in types of spending accordingly may affect my ability to generalize results to other income shocks. Nevertheless, since income from raising the EITC comes through both the direct benefits and the shifts in work effort or wages, this limitation is controlled by the fact that only the direct benefits have been shown to be spent differently from the income from other sources.

Background of the Earned Income Tax Credit

The Earned Income Tax Credit was first enacted in 1975 as a relatively small credit capped at $400 per family to offset payroll tax payments by families with children (Ventry 2001). Since then, Congress has altered the EITC program several times, especially between 1993 and 1996 when the government introduced benefits for individuals without children and expanded the program's scope and generosity for those with children. These expansions of the program increased the number of people claiming the EITC from 6.2 million families in 1975 to 24.6 million families in 2007. Likewise, the total annual value of EITC benefits rose from $4.4 billion (2007 dollars) in 1975 to $48.5 billion in 2007 (Tax Policy Center 2009b).

A family's eligibility for federal EITC benefits depends mainly on the number of children in the family and the total labor earnings of all family members. As of 2008, a single person with two children and no labor earnings income receives no EITC benefits. Benefits are then phased in at a rate of 40 percent of labor earnings for the first $12,060 of earnings, providing a maximum possible credit of $4,829. This maximum benefit is maintained until the family reaches $15,740 in labor earnings. For each dollar of labor earnings beyond $15,740, the EITC benefits are phased out at a rate of 21.06 percent. When the family reaches $37,783 in labor earnings, the benefits are completely phased out, and the family is no longer eligible for any EITC benefits. The EITC benefits system is similar for families with one child or no children, but with lower maximum benefits and different thresholds for the phasing in and phasing out of benefits. In earlier years, the structure of the EITC benefits was similar to that for 2008, except that the benefit thresholds and rates of phasing in and out benefits varied by year. Table 1 lists the maximum benefits and thresholds for obtaining these benefits in each year since 1992.

TABLE 1
Maximum Federal EITC Benefits and Thresholds by Year and Number of Children (in 2008 dollars)

	No Children		One Child		Two or More Children	
	Earnings Range for Maximum Benefits	Maximum Federal Benefits	Earnings Range for Maximum Benefits	Maximum Federal Benefits	Earnings Range for Maximum Benefits	Maximum Federal Benefits
1992			11,307–17,802	1,991	11,307–17,802	2,081
1993			11,371–17,901	2,104	11,371–17,901	2,217
1994	5,746–7,183	440	11,134–15,803	2,928	12,104–15,803	3,632
1995	5,752–7,197	440	8,641–15,838	2,938	12,121–15,838	4,363
1996	5,766–7,215	441	8,650–15,865	2,941	12,148–15,865	4,859
1997	5,805–7,263	444	8,694–15,957	2,956	12,225–15,957	4,890
1998	5,883–7,348	450	8,812–16,173	2,996	12,387–16,173	4,955
1999	5,854–7,327	448	8,787–16,101	2,988	12,328–16,101	4,931
2000	5,764–7,214	441	8,652–15,866	2,942	12,153–15,866	4,861
2001	5,789–7,236	443	8,683–15,919	2,953	12,186–15,919	4,874
2002	5,876–7,360	450	8,821–16,181	2,999	12,387–16,181	4,955
2003	5,842–7,305	447	8,768–16,073	2,982	12,304–16,073	4,922
2004	5,813–7,284	445	8,731–16,004	2,968	12,254–16,004	4,901
2005	5,757–7,202	440	8,636–15,849	2,936	12,132–15,849	4,853
2006	5,745–7,198	440	8,628–15,815	2,934	12,110–15,815	4,844
2007	5,805–7,269	444	8,712–15,981	2,962	12,243–15,981	4,897
2008	5,720–7,160	438	8,580–15,740	2,917	12,060–15,740	4,824

Source: Author's calculations based on Tax Policy Center 2009a.

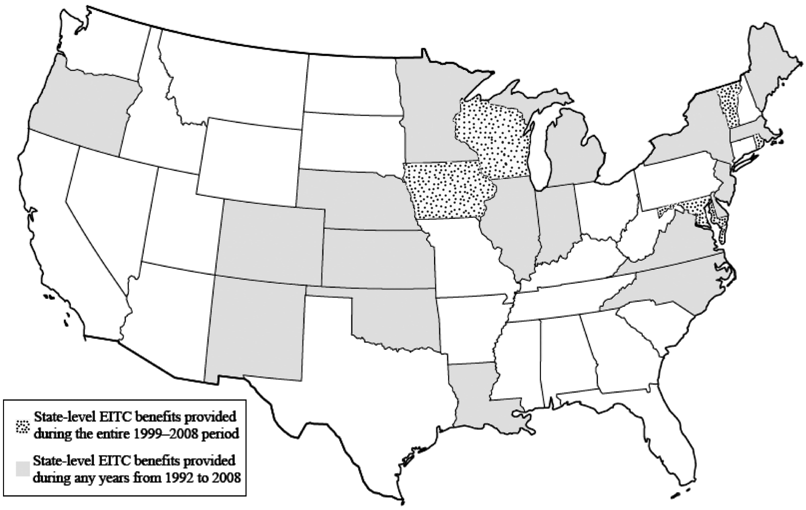

FIGURE 1. State-Level Supplemental EITC Benefits (1992–2008)
Source: Author's compilation from Feenberg 2007, Leigh 2010, and Tax Policy Center 2009c.

In addition to the federal EITC benefits, some of the states supplement federal benefits with additional state-level EITC benefits. The first state to offer supplemental EITC benefits was Rhode Island in 1986. Then in 1992, the first year of my study, five states offered supplemental benefits, and by 2008, twenty-three states plus the District of Columbia had enacted state-level supplements to the federal EITC benefits (for a map of states with state-level supplemental benefits, see figure 1). These state-level benefits are typically calculated as a fixed percentage of a family's federal benefits. In 2008, the states' supplemental EITC benefits varied widely, from 5 percent of federal benefits in Louisiana to 40 percent in the District of Columbia. For low-income families, this combination of state and federal benefits can be quite large, representing between 7.65 and 56 percent of labor earnings for families below the disregard threshold.

Data

The principal source of data for my article is the seven panels of SIPP data from 1992 through 2005. I omitted 1995, as well as the years since 2006, because no health questions were asked in those years. The SIPP is a nationally representative survey conducted by the U.S. Bureau of the Census that follows at least 44,000 individuals for two- to four-year panels (starting in 1996, this number rose to approximately 110,000 persons in each panel). Every three months, the respondents are questioned about their income over the previous quarter, and approximately once a year, they are given a topical question module asking about their health status and functional limitations. In addition to the SIPP data from 1992 through 2005, I calculated my estimates using the March CPS from 1996 through 2009. The March CPS interviews at least 130,000 individuals each year (and at least 200,000 each year since 2002), asking about both their income over the previous calendar year and their current health status. However, unlike the SIPP, the March CPS does not include questions about functional limitations, so I was able to use it only to consider the relationship between income and morbidity after 1996 when health status questions were added to the survey.

Sample Selection

In this article, I included all working-age (22 through 62) individuals in the SIPP and CPS data sets, although the primary estimates, which focus on the low-income population, are only of working-age individuals with an income below 200 percent of the federal poverty line. This threshold roughly coincides with the maximum earnings that families with one or two children can receive before their EITC benefits are completely phased out. Since the potential eligibility for EITC benefits is an important element of the empirical strategy, this criterion is restricted to those individuals whose income may be affected by changes in EITC eligibility rules. Although not all persons in the sample receive EITC benefits, they are the population that is most likely to base their employment and earnings decisions on the EITC benefit formula.

For two reasons, my sample was not limited to people who actually received EITC benefits or by factors that determine EITC benefits such as family size. The first reason is that the changes in EITC benefits are different for families of various sizes, so including families of all sizes adds more variations to use when estimating the effects of income changes. The second reason is that if eligibility for inclusion in the sample were conditioned on the actual receipt of EITC benefits, high-income individuals would be added in those years when the income limits for receiving EITC benefits rose. This increase would, in turn, artificially strengthen the relationship between income and maximum potential EITC benefits and could bias the results of the instrumental variables regressions discussed here.

Measuring Morbidity

To obtain a measure of self-reported health status, respondents in the SIPP are asked in the summer or fall of each year, “Would you say your health in general is excellent, very good, good, fair, or poor?” The March CPS asks the same question, and the wording of this question is the same or very similar to that used in numerous other surveys with self-reported health components, including the Health and Retirement Study and the Behavioral Risk Factor Surveillance System. Indeed, self-reported health status has been widely used in the income-health gradient literature (see, e.g., Case, Lubotsky, and Paxson 2002; Ettner 1996; Lindahl 2005) and has been shown to be a good predictor of functional limitations (Idler and Kasl 1995; Idler, Russell, and Davis 2000), health care utilization (DeSalvo et al. 2005), and future mortality (DeSalvo et al. 2005; Idler and Kasl 1995; Idler, Russell, and Davis 2000; Wannamethee and Shaper 1991).

Nonetheless, self-reported health status is a subjective measure of health, as it may vary in accordance with the respondent’s own assessment of the scale. Therefore, two people with the same health status could report different health ratings simply because of differences in their perceptions of good health. Consequently, to test the results’ sensitivity to this measure of health status, I also measured health based on the eight functional limitations explored in several years of the SIPP panel. These functional limitations are having difficulty (1) seeing and reading newspaper print even with glasses or contact lenses, (2) hearing normal conversation even when wearing a hearing aid, (3) lifting and carrying ten pounds, (4) walking a quarter mile, (5) climbing a flight of ten stairs, (6) getting in and out of bed or a chair, (7) doing light housework, and (8) using an ordinary telephone.

The answers are evaluated both separately and on a single binomial scale on which individuals are considered to have a functional limitation if they say they have one or more of the eight limitations. Since functional limitations are not part of the CPS questionnaire, I analyzed them only when using the SIPP data set.

Measuring Income

Both the SIPP and March CPS contain extensive income questionnaires that are intended to capture most sources of a family's cash income. When measuring income, I defined a family as a *census subfamily*, which is a nuclear family, rather than a *census family*, which consists of all related individuals in a household. The income questions pertain to labor earnings such as wages and salaries or self-employment income; nonlabor income like interest, dividends, or rental income; private transfer income such as alimony or workers' compensation; and public transfer income like unemployment, welfare, Social Security, or SSI benefits. These questionnaires are intended to capture all of a household's pretax cash income except for irregularly received income like stock options and capital gains. Furthermore, the income questionnaire does not ask about tax liabilities or tax credits such as the EITC (for a description of all the income sources included and excluded in the March CPS, see Weinberg 2006).

Because most of the research on the income-health gradient focuses on pretax income, excluding both tax credits and tax liabilities, I initially did the same. But since the relationship between health and income is more likely to depend on income available to individuals for consumption, which is more closely captured by posttax income, it also is valuable to consider the relationship between posttax income and morbidity. To explore posttax income, I calculated the taxes using NBER TAXSIM, version 9, based on the income information provided by the SIPP and CPS respondents. I then added these taxes to each family's income to determine the posttax income for use in this analysis.

In addition, the resources available to any family member depend on both the family's income and the number of family members sharing that income. I accordingly adjusted the income for family size by dividing by the square root of the number of family members. This adjustment closely matches the adjustment for family size implied by the U.S. Census Bureau's poverty thresholds (Ruggles 1990). For single-parent families, I followed the CPS's and SIPP's family definitions, which assume that the children are part of whichever parent's household they reside in.

One limitation of using the SIPP data in conjunction with the EITC and tax information is that the SIPP interviews are staggered so that health questions are asked of each respondent at different times in the calendar year. In contrast, taxes and EITC benefits are based on calendar-year incomes. To ensure that each person's income is considered for the same time span before asking about their health status, I calculated their taxes and EITC benefits using calendar years and assuming that they were paid or received benefits equally in each month of the year. This allowed me to analyze income over consistent twelve-month periods before asking health questions, regardless of whether that twelve-month period was aligned with a calendar year.

This approach reflected actual receipts if individuals obtained credits throughout the year rather than waiting until filing taxes. While this is an option through the EITC Advance program, most eligible individuals chose to receive their benefits in a lump sum as an annual tax refund, and less than 2 percent opted to receive their benefits through the EITC Advance program (Holt 2008). Nonetheless, because most people probably anticipate receiving the refund even if they have not applied for the EITC Advance benefits, the change in income may still be reflected in their spending decisions. But to the extent that they neither receive nor anticipate collecting EITC benefits, this may result in an overstatement of the EITC's immediate effect on posttax income when using the SIPP data and could subsequently result in a downward bias of the effect of EITC benefits on health outcomes.

In contrast to the SIPP, each March the CPS always asks about current health and income for the previous calendar year. Thus, the observation period for income in the CPS is a calendar tax year for both the calculation of EITC benefits and the annual tax liabilities. Furthermore, since most individuals who do not opt for Advanced EITC payments file their taxes early and receive their refund in February or March (Barrow and McGranahan 2000; Goodman-Bacon and McGranahan 2008), the March CPS survey comes after they receive their refund and after any resulting increase in spending based on receiving the refund. Therefore, the concern regarding the SIPP that individuals are asked about their health status before receiving and spending these benefits is not a concern regarding the CPS. The largely consistent results between the two data sets should help alleviate concerns regarding the timing of receiving EITC benefits.

Empirical Strategy

To confirm a positive relationship between income and health in the March CPS and SIPP data, I initially used a standard ordered probit regression. Similar to an ordinary least squares (OLS) regression, the ordered probit regression can observe the relationship between income and morbidity. It differs, however, in that it reflects the ordinal rather than the cardinal relationship between health statuses. This initial analysis regresses an individual's self-reported health status on his or her size-adjusted family income over the previous twelve months (previous calendar year in the CPS), the state dummy variables to account for time-invariant differences between states that influence the health of its residents, the year of observation, and individual demographic variables. These demographic variables are the individual's age, race, ethnicity, gender, education, marital status, and health insurance status; the number of children in his or her family; and whether he or she lives in a metropolitan area.

This simple ordered probit regression can demonstrate the existence of a relationship between income and morbidity. But given the multiple pathways that can affect this relationship, it would be misleading to use these results to interpret the effect as causal. Therefore, to explore causality, I instead used an "instrumental variable" approach requiring a variable that strongly influences family income but does not directly affect the individual's health status except through its impact on his or her income. The approach then uses a two-stage process in which first this variable and the demographic/state control variables are used as explanatory variables in a regression to estimate family income. In a subsequent regression, health status is regressed on this estimated family income and the demographic, state, and time control variables, which yields the impact of an exogenous increase in income on health, devoid of the reverse causality effects.

This instrumental variable approach requires finding a variable that causes variation in income but is not influenced by the individual's health. The changes in state and federal EITC benefits since 1992 fulfill these requirements. Schmeiser (2009) exploited these variations to consider how an exogenous increase in income affects obesity rates among low-income individuals, and I used a similar strategy to consider the impact on their morbidity. This approach is able to identify only short-run effects, however, which may be smaller than those in the long run. Nonetheless, earlier research, such as that done by Schmeiser (2009), found short-run effects of income on specific health outcomes including obesity, so it still is valuable to consider effects in this time frame.

To identify the causal influence of changes in income on morbidity, I used the maximum combined state and federal EITC benefits that the family could receive as an instrument for family income. This maximum potential benefit is a state-level variable reflecting the credits that a family in the state could receive if their labor earnings maximized their benefits. This benefit depends only on the state of residence and number of children in the family and not on the family members' labor earnings. If the state-level benefit is not refundable, the maximum potential EITC benefits will be the same as if it were fully refundable, although fewer individuals will be eligible to receive these full benefits. All federal benefits are fully refundable, however, so the lack of refundability never limits the federal EITC benefits received. I compiled the data on state and federal benefits based on the findings of Leigh (2010), Feenberg (2007), and the Tax Policy Center (2009a, 2009c).

The maximum combined state and federal EITC benefits are a valid instrument if these benefits are not correlated with the unobserved determinants of morbidity status but are correlated with family income. Evaluating the first of these requirements—that the maximum EITC benefits are not correlated with the unobserved determinants of morbidity—requires exploring why governments changed their EITC benefits during this period. Johnson (2001) and Leigh (2010) stated that the three primary motivations for changes in EITC benefits are (1) the federal government's allowing states to use block grants for the Temporary Assistance for Needy Families (TANF) program to partially fund EITC programs after 1996, (2) the strong welfare reform lobbying groups' pushing for EITC programs, and (3) the state budgets' surpluses. Leigh (2010) noted, too, that some political differences also influence whether states change their EITC benefits, because states that are more Democratic are more likely to enact new state-level benefits. Since the health of low-income individuals has traditionally not been a motivation for changing EITC benefits, it is unlikely that the unobserved determinants of an individual's health will be correlated with the state-level

EITC benefits in a given year, unless other state economic conditions or state spending that influences health outcomes vary along with the EITC benefits.

One such possibility is if health insurance status is not observed and Medicaid is changed in conjunction with shifts in EITC benefits. This problem is mitigated, however, since individuals are asked about the status of their health insurance so that it can be included in all regressions. A second possibility is a correlation between state unemployment rates or minimum wages and their EITC benefits. Leigh (2010), however, found no significant relationship between either a state's unemployment rate or its minimum wage rate and its maximum EITC benefits. He also found only a weak negative relationship between a state's welfare benefits, such as those from the Aid to Families with Dependent Children (AFDC) and Temporary Assistance for Needy Families (TANF) programs, and their EITC benefits. Because AFDC/TANF benefits are included in individuals' incomes, this negative relationship may reduce the impact of EITC benefits on income—but it should not bias the second-stage results regressing morbidity on income.

Nevertheless, because the welfare reforms in the 1996 Personal Responsibility and Work Opportunity Reconciliation Act dramatically changed the benefits system for those with low incomes, it is appropriate to include a control for this act to reduce the possibility that unobserved policy changes will affect results. Thus, all instrumental variable regressions include a dummy variable for whether the observation is from after 1996 when welfare reform was enacted.

Despite the evidence that EITC benefits are not closely tied to state unemployment or other unobserved policy changes, the exclusion restriction for an instrumental variables regression cannot, unfortunately, be tested directly. Thus the possibility that concurrent changes in other public spending programs may influence the results cannot be completely discounted. If, for example, the expansion of EITC benefits reduces participation in noncash transfer programs such as food stamps (Mikelson and Lerman 2004), then the effects of income increases on morbidity may be smaller than those observed here.

A related issue that also may influence the magnitude of the results is the possibility of spillover effects from the policy. According to earlier research, local economies improve and mobility increases after EITC benefits are granted (Cohen et al. 2008). If a person's health improves because of an increase in his neighbor's income, the infusion of cash into specific neighborhoods may have a greater impact on the individual's health than would be observed if he received the additional income in isolation. Thus, the presence of these spillover effects also may overstate the impact that an isolated individual's increased income would have on his or her morbidity.

The second requirement for using the maximum potential EITC benefits as an instrument for family income is a correlation between these benefits and poor families' income. The reason for this requirement is that both the large direct supplement to income from the increase in benefits and the increases in the EITC benefits also have been found to encourage labor force participation and subsequently raise labor earnings (Meyer 2002). As I will demonstrate, the generosity of state-level benefits is a powerful predictor of poor families' income. This finding matches earlier findings regarding the importance of EITC benefits to family income, which used the maximum potential EITC benefits as an instrument to calculate income (Schmeiser 2009).

To estimate how the health of low-income individuals is affected by income under this instrumental variable framework, I first estimated size-adjusted family income using an ordinary least squares regression. Here, size-adjusted family income is regressed on the maximum EITC benefits for which an individual could be eligible based on his or her state and number of children, state dummy variables, a dummy variable that is equal to one for observations from the years after 1996 when federal welfare reform was enacted, the year of observation, and the individual demographic variables described at the beginning of this section for the initial ordered probit regression.

Using the results of this first-stage regression, I calculated the predicted values for the size-adjusted family income for each observation. These predicted values for size-adjusted family income then became an explanatory variable in a second-stage regression of health status. Here, self-reported health was regressed on the predicted values of size-adjusted family income from the first-stage regression, state dummy variables, a dummy variable that is equal to one for observations from years after 1996 when federal welfare reform was enacted, the year of observation, and all the individual demographic variables included in the first-stage regression. When the dependent variable is self-reported health, this regression is an ordered probit regression reflecting the ordinal rather than cardinal relationship of health statuses. When the dependent variable is the presence of functional limitations, a probit regression is used to reflect the binary nature of functional limitations. In each case, the coefficient for the predicted size-adjusted family income represents the impact on health outcomes of an exogenous change in poor persons' income.

Results

The positive relationship between pretax income and self-reported health, which has been well documented in the literature, can easily be observed in both the SIPP and the CPS data. Table 2 shows this by providing the fraction of the working-age population reporting each health status by decile of the pretax, size-adjusted, family income. In both data sets, individuals in the bottom decile of income reported being in poor health at more than three times the rate of the total population. Similarly, the fraction of individuals in the bottom decile reporting being in excellent health is less than two-thirds that of the total population in both the SIPP and the CPS data.

When using the initial standard ordered probit regression to regress self-reported health on pretax income for the entire working-age population, including controls for demographic characteristics such as age, race, education, and marital status, this significant positive relationship is still observed (columns 1 and 3 of panel A of table 3). In the SIPP data, an increase in income of $1,000 is associated with an average marginal effect of a 0.154 percentage point increase in the likelihood of being in excellent health and a 0.025 percentage point decline in the probability of being in poor health. This relationship between pretax income and self-reported health is even stronger when the sample is restricted to working-age individuals making less than twice the federal poverty line (columns 2 and 4 of panel A of table 3). For this population, an increase in income of $1,000 in the SIPP is associated with a 0.247 percentage point increase in the probability of being in excellent health and a 0.098 percentage point decline in the probability of being in poor health.

If income influences health by allowing for different consumption patterns, we should expect the relationship to be stronger when using posttax income. As panel B of table 3 shows, this is generally the case. Using the SIPP data for both all individuals and low-income individuals, the coefficient for the relationship between income and health is greater

TABLE 2
Frequency of Self-Reported Health Statuses by Decile of Pretax, Size-Adjusted Family Income

Panel A: Survey of Income and Program Participation					
	Self-Reported Health Status				
Income Decile	Poor	Fair	Good	Very Good	Excellent
1	9.13	17.91	30.41	25.58	16.97
2	5.75	13.26	29.85	30.32	20.82
3	3.56	10.04	28.62	33.30	24.48
4	2.71	7.93	28.08	35.39	25.90
5	2.12	6.73	26.22	36.31	28.62
6	1.74	5.97	24.69	37.40	30.19
7	1.52	5.24	23.15	38.35	31.73
8	1.16	4.61	21.90	38.69	33.64
9	0.84	3.85	20.65	38.79	35.86
10	0.59	2.94	17.48	37.17	41.82
All	2.89	7.80	25.06	35.17	29.08

Panel B: March Current Population Survey					
	Self-Reported Health Status				
Income Decile	Poor	Fair	Good	Very Good	Excellent
1	10.25	16.51	29.29	24.76	19.20
2	6.97	13.03	29.28	28.82	21.89
3	4.08	9.71	28.33	32.78	25.10
4	2.89	7.79	26.94	34.41	27.97
5	2.23	6.54	25.43	35.61	30.19
6	1.73	5.72	24.27	36.26	32.02
7	1.41	4.99	22.66	37.07	33.87
8	1.15	4.44	21.93	36.76	35.71
9	1.00	3.82	19.65	36.86	38.67
10	0.77	3.23	16.66	35.22	44.11
All	3.25	7.58	24.44	33.85	30.87

Source: Author's calculations based on SIPP and March CPS data files.

using posttax income than it was using pretax income. For the CPS data, this is true for all working-age individuals, although not when the sample is restricted to those with low incomes.

In addition, similar to when using pretax income, the relationship between income and health is stronger for low-income individuals than it is for the population as a whole. The strength of the relationship at

TABLE 3
Ordered Probit Results Regressing Self-Reported Health on Size-Adjusted Family Income and Demographic Controls for Working-Age Individuals

	Panel A: Pretax Income			
	(1) SIPP All Working Age[a]	(2) SIPP Low Income[b]	(3) CPS All Working Age[a]	(4) CPS Low Income[b]
Pretax Income ($1,000s)	0.00493***	0.00985***	0.00297***	0.00853***
	(0.000123)	(0.000905)	(0.000036)	(0.000373)
Average Marginal Effect of a $1,000 Increase in Income on Probability of Each Health Status				
Poor	−0.00025***	−0.00098***	−0.00016***	−0.00096***
	(0.000007)	(0.000092)	(0.000002)	(0.000043)
Fair	−0.00051***	−0.00137***	−0.00030***	−0.00111***
	(0.000013)	(0.000127)	(0.000004)	(0.000049)
Good	−0.00087***	−0.00106***	−0.00052***	−0.00088***
	(0.000021)	(0.000096)	(0.000006)	(0.000039)
Very Good	0.00010***	0.00094***	0.00003***	0.00074***
	(0.000005)	(0.000088)	(0.000001)	(0.000033)
Excellent	0.00154***	0.00247***	0.00095***	0.00222***
	(0.000038)	(0.000226)	(0.000011)	(0.000097)
Observations	307,585	85,397	1,325,941	356,427

Continued

TABLE 3—*Continued*

Panel B: Posttax Income				
	(1) SIPP All Working Age[a]	(2) SIPP Low Income[b]	(3) CPS All Working Age[a]	(4) CPS Low Income[b]
Posttax Income ($1,000s)	0.00886***	0.01160***	0.00504***	0.00582***
	(0.000181)	(0.001020)	(0.000061)	(0.000459)
Average Marginal Effect of a $1,000 Increase in Income on Probability of Each Health Status				
Poor	−0.00045***	−0.00116***	−0.00028***	−0.00066***
	(0.000011)	(0.000104)	(0.000004)	(0.000052)
Fair	−0.00092***	−0.00162***	−0.00050***	−0.00076***
	(0.000020)	(0.000143)	(0.000006)	(0.000060)
Good	−0.00157***	−0.00125***	−0.00088***	−0.00060***
	(0.000032)	(0.000109)	(0.000011)	(0.000048)
Very Good	0.00018***	0.00111***	0.00005***	0.00050***
	(0.000008)	(0.000099)	(0.000002)	(0.000040)
Excellent	0.00276***	0.00292***	0.00161***	0.00151***
	(0.000056)	(0.000256)	(0.000019)	(0.000119)
Observations	307,585	85,397	1,325,941	356,427

Notes: (1) Additional covariates are gender, age, age-squared, race, ethnicity, education, year, state of residence, residence in an MSA, number of children in the family, marital status, and health insurance status. (2) Standard errors in SIPP data are clustered standard errors by person to account for the stacked-panel design of the data set.

[a]Working-age population is all individuals aged twenty-two to sixty-two.

[b]Low-income population is all individuals of working age with a pretax, size-adjusted family income less than twice the federal poverty level for a single individual.

*significant at 10% level, **significant at 5% level, ***significant at 1% level.

Source: Author's calculations based on SIPP and March CPS data.

the lower tail of the income distribution shows that the low-income population is particularly relevant to understanding the income-health gradient. Accordingly, this low-income population will be the primary focus for the remainder of this article.

IV Regression: Pretax Income and Self-Reported Health

Despite the significant positive relationship between pretax income and self-reported health for low-income individuals in the ordered probit regression, it does not provide insight into the direction of causation, for the reasons discussed earlier. To consider the extent to which health status is influenced by changes in pretax income, I used an instrumental variable approach in which the generosity of state and federal EITC benefits is an instrument to calculate income. As I mentioned earlier, in order to use the maximum potential EITC benefits as an instrument, it must be a strong predictor of family income. The guideline of Stock, Wright, and Yogo (2002) states that this requirement is satisfied by a first-stage F-statistic over ten when testing the hypothesis that the coefficient on the instrument (maximum EITC benefits) equals zero. An F-statistic above this threshold signifies a strong relationship between maximum potential EITC benefits and family income. As columns 1 and 3 of table 4 show, this first-stage F-statistic exceeds this threshold in both data sets, so it is reasonable to proceed using the maximum potential EITC benefits as an instrument in the instrumental variable regression.

The significant positive relationship observed in the initial ordered probit regression largely disappeared when I used the instrumental variable approach. When I used the generosity of state and federal EITC benefits as an instrument to determine income in the SIPP data, a higher pretax income did not significantly change individuals' self-reported health status (column 1 of table 4). The point estimates for the effect of income on morbidity are positive and larger than those in the initial ordered probit regression, but because of the substantial increase in standard errors from the IV approach, this estimated effect is not significantly different from zero.

To further test the results from the SIPP data showing that changes in pretax income for poor individuals have no statistically significant

TABLE 4
Instrumental Variable Results Regressing Self-Reported Health on Size-Adjusted Family Income and Demographic Controls for Low-Income Working-Age Individuals

	(1) SIPP Pretax	(2) SIPP Posttax	(3) CPS Pretax	(4) CPS Posttax
Predicted Income ($1,000s)	0.0467	0.0129	0.0439	0.0169
	(0.04020)	(0.01110)	(0.02770)	(0.01070)
Average Marginal Effect of a $1,000 Increase in Income on Probability of Each Health Status				
Poor	−0.0047	−0.0013	−0.0050	−0.0019
	(0.00402)	(0.0011)	(0.00313)	(0.0012)
Fair	−0.0065	−0.0018	−0.0057	−0.0022
	(0.00559)	(0.0015)	(0.00360)	(0.0014)
Good	−0.0050	−0.0014	−0.0046	−0.0018
	(0.00433)	(0.0012)	(0.00289)	(0.0011)
Very Good	0.0045	0.0012	0.0038	0.0015
	(0.00384)	(0.0011)	(0.00240)	(0.0009)
Excellent	0.0117	0.0032	0.0114	0.0044
	(0.01009)	(0.0028)	(0.00722)	(0.0028)
First-Stage Results: Dependent Variable Income ($1,000s)				
Maximum EITC Benefits	0.1680***	0.6080***	0.1460***	0.3790***
	(0.03170)	(0.02840)	(0.02110)	(0.01710)
F-statistic	28.15	458.97	47.91	493.51
Observations	85,397	85,397	356,427	356,427

Notes: (1) Additional covariates are gender, age, age-squared, race, ethnicity, education, year, state of residence, residence in an MSA, number of children in the family, marital status, health insurance status, and whether the observation is from after the implementation of the 1996 federal welfare reform act. (2) Standard errors in SIPP data are clustered standard errors by person to account for the stacked-panel design of the data set. (3) The sample population is all individuals of working age with a pretax, size-adjusted family income less than twice the federal poverty level for a single individual.
*significant at 10% level, **significant at 5% level, ***significant at 1% level.
Source: Author's calculations based on SIPP and March CPS data.

effect on morbidity, I replicated the previous IV regression using the March CPS (column 3 of table 4). The point estimates for the effect of changes in income on morbidity were consistent with those found in the SIPP. Once again, given the large standard errors of the IV approach, these estimates are not statistically significant. Therefore, while the point estimates provide some evidence that pretax income positively influences self-reported health, the evidence supporting this theory is weak, given the lack of any significant effects in both data sets.

IV Regression: Posttax Income and Self-Reported Health

Because posttax income more closely approximates the disposable income available to individuals for health-related consumption, I explored whether focusing on posttax rather than pretax income would change the results. Although the first-stage regression using the maximum potential EITC benefits to predict pretax income satisfied the standard requirement of an F-statistic greater than ten, the strength of the first-stage relationship increased dramatically when using posttax income instead. This is because the actual EITC benefits received are observed only when using posttax income. This is in contrast to pretax income, which captures the additional income from shifts in behavior only after a change in EITC benefits and not after the direct income from the benefits themselves. However, even though focusing on posttax income makes the maximum potential EITC benefits a stronger predictor of income, the results for how income affects self-reported health are largely unchanged.

The results of the IV regression estimating the effect of changes in posttax income on self-reported health are provided in columns 2 and 4 of table 4. When using posttax income, there is no change in the direction and significance of the key results, although the point estimates for the effect of income on self-reported health declines in both the SIPP and the CPS data. The point estimates are, however, smaller than those observed when using pretax income (see table 5). Similar to the findings using pretax income, the estimates derived from the SIPP data find that the changes in income resulting from shifts in the generosity of EITC benefits have no significant impact on health status—partly because of the large standard errors associated with the IV regression. Therefore, the positive point estimates provide limited evidence that posttax income positively influences self-reported health, mirroring the results using pretax income. But because they are not statistically significant in either data set, the evidence supporting a positive income effect on health is extremely limited.

TABLE 5
Frequency of Self-Reported Functional Limitations by Decile of Pretax, Size-Adjusted Family Income

Income Decile	Reading Newsprint	Hearing Conversation	Lifting Ten Pounds	Climbing Ten Stairs	Walking Quarter Mile	Using a Telephone	Getting out of Bed or a Chair	Doing Light Housework	Any Functional Limitation
1	6.30	3.85	13.04	15.13	15.04	1.58	4.48	5.21	24.65
2	4.54	3.08	8.46	10.37	10.37	1.26	3.04	3.79	18.11
3	3.00	2.46	5.76	7.01	7.24	0.74	2.10	2.29	13.29
4	2.34	2.60	4.54	5.61	5.48	0.65	1.48	1.64	11.25
5	1.93	2.13	3.56	4.60	4.65	0.45	1.17	1.30	9.49
6	1.70	2.28	3.22	3.94	4.11	0.51	1.11	1.14	9.07
7	1.37	1.97	2.70	3.49	3.52	0.39	0.90	0.97	7.96
8	1.06	1.63	2.29	3.12	2.98	0.27	0.76	0.91	6.67
9	1.21	1.90	2.05	2.30	2.51	0.21	0.57	0.68	6.52
10	0.96	1.51	1.47	2.02	2.04	0.34	0.57	0.48	5.24
All	2.44	2.34	4.71	5.76	5.79	0.64	1.62	1.84	11.22

Source: Author's calculations based on the SIPP (1992–2005) data files.

Sensitivity Analysis of Self-Reported Health Results

Using other sets of covariates and other income thresholds, I also tested the results for the self-reported health instrumental variable regressions. In contrast to the regressions in table 4 that restricted the sample to persons with income below 200 percent of the federal poverty line in the past year, the first "alternative" income thresholds restricted the sample to those people with an income below 100 percent of the federal poverty line in the past year. The second "alternative" income threshold expanded the sample to include all individuals in the SIPP with an annual income below 200 percent of the poverty line at any time during the survey period (rather than only in the past year). I also tested the results' sensitivity to using year fixed-effect dummy variables rather than a single linear-year variable. Because several researchers have also suggested that employment is either positively (Gallo et al. 2000; Snyder and Evans 2006) or negatively (Ruhm 2000) correlated with health, I tested as well the results controlling for employment or limiting the sample to those who were employed. To avoid bias from selection effects, the main analysis included all individuals in the sample, regardless of their employment status, which is consistent with the approach of previous research considering the effects of welfare programs (for further discussion of this issue, see Herd, Schoeni, and House 2008). Nevertheless, I also estimated the results with employment controls—a sample limited to employed individuals—or with year fixed-effects. In general, the results of each of these sensitivity analyses were consistent with the results presented here. The main exceptions were in the CPS data when the sample was restricted to those who were employed or when the sample was restricted to those whose income was below 100 percent of the federal poverty level. In both instances, the relationship between income and health was weakly significant (at the 10% level) in the IV regression, rather than not statistically significant. In addition, when the sample in the SIPP data was restricted to those below 100 percent of the federal poverty line, the sign of the point estimate indicated a negative relationship between income and health, although the estimate remained statistically insignificant. (The results of the sensitivity analyses are not presented here but are available on request from the author).

IV Regression: Measuring Morbidity Using Functional Limitations

While self-reported health status is commonly used to measure morbidity in survey data, an alternative and somewhat less subjective approach is to measure morbidity using self-reported functional limitations. I consider here the eight functional limitations included in each SIPP panel from 1992 through 2005. As was the case for self-reported health, when examining the prevalence of functional limitations among working-age individuals by decile of the pretax, size-adjusted, family income distribution, each of the limitations was most prevalent among individuals in the bottom income decile (table 5). The probability that a working-age individual in the lowest decile of the income distribution would report having at least one of these limitations was 2.2 times greater than that of the general working-age population.

Table 6 uses a probit regression to estimate the relationship between pretax income and functional limitations among low-income, working-age individuals. The outcome variable is having the specified functional limitation, so a negative coefficient signifies that increases in income reduce the probability that an individual would report having the limitation, and thus a higher income is associated with better health. For seven of the eight functional limitations and for the aggregated functional limitations variable, a higher income was associated with lower rates of the limitation. This was true using both pretax (panel A) and posttax (panel B) income. The one exception was having difficulty using a telephone, for which the effect was reversed and higher income was associated with a higher prevalence of the functional limitation.

To see the direction of these effects in the low-income population, I used the same IV approach to reestimate the effect of higher incomes on functional limitations. As with measuring morbidity using self-reported health, the effects are estimated using both pretax and posttax income. Panel A of table 7 shows the results using pretax income, and panel B of table 7 shows those using posttax income.

Similar to the results found when measuring morbidity using self-reported health, a higher income from increased EITC benefit generosity generally had no statistically significant effects on the prevalence of functional limitations. This was true for six of the eight functional limitations using both pretax and posttax income—and two of the eight

TABLE 6
Ordered Probit Results Regressing Functional Limitations on Size-Adjusted Family Income and Demographic Controls for Low-Income Working-Age Individuals

Panel A: Pretax Income	Reading Newsprint	Hearing Conversation	Lifting Ten Pounds	Climbing Ten Stairs	Walking Quarter Mile	Using a Telephone	Getting out of Bed or a Chair	Doing Light Housework	Any Functional Limitation
Pretax Income ($1,000s)	−0.0099***	−0.006***	−0.0167***	−0.0167***	−0.0145***	0.0065*	−0.0143***	−0.0097***	−0.0129***
	(0.00196)	(0.00218)	(0.00163)	(0.00158)	(0.00158)	(0.00371)	(0.00227)	(0.0022)	(0.00136)
Average Marginal Effect of an Increase in Income on Probability of Reporting Limitation									
	−0.0009***	−0.0004***	−0.0023***	−0.0026***	−0.0022***	0.0002*	−0.0010***	−0.0007***	−0.0028***
	(0.00017)	(0.00015)	(0.00023)	(0.00024)	(0.00025)	(0.00010)	(0.00015)	(0.00016)	(0.00030)
Observations	58,943	58,943	58,943	58,943	58,943	58,943	58,943	58,943	58,943

Panel B: Posttax Income	Reading Newsprint	Hearing Conversation	Lifting Ten Pounds	Climbing Ten Stairs	Walking Quarter Mile	Using a Telephone	Getting out of Bed or a Chair	Doing Light Housework	Any Functional Limitation
Posttax Income ($1,000s)	−0.0129***	−.0075***	−0.0213***	−0.0209***	−0.019***	0.0065	−0.0198***	−0.0147***	−0.0198***
	(0.00221)	(0.00249)	(0.00183)	(0.00177)	(0.00178)	(0.00425)	(0.00252)	(0.00245)	(0.00252)
Average Marginal Effect of an Increase in Income on Probability of Reporting Limitation									
	−0.0011***	−0.0005***	−0.0030***	−0.0032***	−0.0029***	0.0002	−0.0013***	−0.0011***	−0.0036***
	(0.00020)	(0.00017)	(0.00026)	(0.00027)	(0.00027)	(0.00011)	(0.00017)	(0.00018)	(0.00033)
Observations	58,943	58,943	58,943	58,943	58,943	58,943	58,943	58,943	58,943

Notes: (1) See table 3. (2) The sample population is all individuals of working age with a pretax, size-adjusted family income less than twice the federal poverty level for a single individual.
*significant at the 10% level, **significant at the 5% level, ***significant at the 1% level.
Source: Author's calculations based on the SIPP (1992–2005) data files.

TABLE 7
Instrumental Variable Results Regressing Self-Reported Health on Size-Adjusted Family Income and Demographic Controls for Low-Income Working-Age Individuals

	Panel A: Pretax Income								
	Reading Newsprint	Hearing Conversation	Lifting Ten Pounds	Climbing Ten Stairs	Walking Quarter Mile	Using a Telephone	Getting out of Bed or a Chair	Doing Light Housework	Any Functional Limitation
Predicted Income ($1,000s)	−0.1803	−0.4801***	−0.0591	−0.1395	−0.1572	0.0336	−0.2983*	0.0000	−0.2003**
	(0.16316)	(0.17069)	(0.12470)	(0.12246)	(0.11954)	(0.30168)	(0.17068)	(0.16962)	(0.10131)
Average Marginal Effect	−0.0160	−0.0322***	−0.0083	−0.0214	−0.0243	0.0009	−0.0200*	0.0000	−0.0433**
	(0.01445)	(0.01148)	(0.01756)	(0.01881)	(0.01850)	(0.00795)	(0.01143)	(0.01250)	(0.02192)
First-Stage Results: Dependent Variable Is Pretax Income ($1,000s)									
Maximum EITC Benefits	0.1162***								
	(0.03257)								
First-Stage F-statistic	12.73								
Observations	58,943								

Continued

TABLE 7—*Continued*

Panel B: Posttax Income									
	Reading Newsprint	Hearing Conversation	Lifting Ten Pounds	Climbing Ten Stairs	Walking Quarter Mile	Using a Telephone	Getting out of Bed or a Chair	Doing Light Housework	Any Functional Limitation
Predicted Income ($1,000s)	−0.0295	−0.0786***	−0.0097	−0.0228	−0.0257	0.0055	−0.0489*	0.0000	−0.0328**
	(0.02672)	(0.02796)	(0.02042)	(0.02006)	(0.01958)	(0.04941)	(0.02795)	(0.02778)	(0.01659)
Average Marginal Effect	−0.0026	−0.0053***	−0.0014	−0.0035	−0.0040	0.0001	−0.0033*	0.0000	−0.0071**
	(0.00237)	(0.00188)	(0.00288)	(0.00308)	(0.00303)	(0.00130)	(0.00187)	(0.00205)	(0.00359)
First-Stage Results: Dependent Variable Is Pretax Income ($1,000s)									
Maximum EITC Benefits	0.7096***								
	(0.02951)								
First-Stage F-statistic	578.09								
Observations	58,943								

Notes: (1) Additional covariates are gender, age, age-squared, race, ethnicity, education, year, state of residence, residence in an MSA, number of children in the family, marital status, health insurance status, and whether the observation is from after the implementation of the 1996 federal welfare reform act. (2) Standard errors in SIPP data are clustered standard errors by person to account for the stacked-panel design of the data set. (3) The sample population is all individuals of working age with a pretax, size-adjusted family income less than twice the federal poverty level for a single individual.

*significant at the 10% level, **significant at the 5% level, ***significant at the 1% level.

Source: Author's calculations based on the SIPP (1992–2005) data files.

even had point estimates suggesting an insignificant negative effect of income on health. Nevertheless, for two of the eight limitations, there was evidence that an increase in income significantly improved health outcomes. These two limitations are difficulty hearing even when wearing a hearing aid and difficulty getting out of bed or a chair. For each of these functional limitations, the increase in either pretax or posttax income significantly reduced the prevalence of the limitation. For example, a $1,000 increase in posttax income brought an average decline of 0.53 percentage points in the probability of a hearing limitation. The effect of a higher income on reducing these limitations was substantial enough that increases in income also significantly reduced the probability of reporting at least one of the eight limitations.

One explanation for the more significant effect of increases in income on the hearing limitation is the availability and relatively low cost of hearing aids, which are referenced in the question and can easily mitigate the limitation. The vast majority of individuals with this limitation do not report being completely deaf but, rather, have limited hearing. Thus, their functional limitation may be the result of not having a hearing aid or having an outdated one. But with the purchase of a hearing aid, the hearing limitation can be corrected in the short run more easily than the other limitations can. Thus this finding may be reflecting the direct health spending in the short run that results from income shocks.

Conclusions

Numerous researchers have previously documented the positive relationship between income and morbidity. Here I considered whether the relationship was derived from changes in income that influenced morbidity rates, but I found only limited evidence that it did.

When I used both pretax and posttax income, I found that shifts in income in the poor population had only a statistically insignificant effect on self-reported health in both the SIPP and the CPS data. Similarly, when measuring morbidity using functional limitations in the SIPP, I found that changes in income resulting from EITC generosity did not have a significant effect on the prevalence of six of the eight functional limitations. But the increases in income did appear to reduce the probability that an individual had some functional limitations, the most notable being an inability to hear normal conversation even when wearing a hearing aid. The positive significant effect of increases in income on this particular limitation was consistent with individuals using additional income from their EITC benefits to remedy a health ailment that is easily observable and relatively inexpensive to correct through the purchase of a hearing aid.

Although this study found only limited support for the theory that higher incomes significantly reduce morbidity rates in the short run, we should recognize that this does not rule out the possibility of larger long-term effects. Because both the SIPP and the CPS follow people for only a relatively short time, we cannot observe the effects of higher income over a lifetime. Thus, over an extended period there may be long-term health effects from a permanent shift in income that exceed those observed just a year after the income shock. It would therefore be valuable to further explore the long-run effects of income shocks to understand how they compare with these short-run effects and to find out how these effects differ in accordance with the nature of the income shock.

The health of other members of the family besides the working-age adults also may be affected by these changes in income. In particular, increases in income could affect both the short-term and the long-term health of children in the family. Finally, individuals may spend their EITC benefits in different ways than they do other income. Barrow and McGranahan (2000) and Goodman-Bacon and McGranahan (2008) found that much of EITC income is spent on consumer durable goods. While several researchers have considered the consumption patterns of the EITC or of income received from tax cuts (Souleles 1999, 2002), to my knowledge no research has specifically analyzed the health effects of consumption from tax refunds and compared them with the health effects of other consumption. Therefore, to the extent that individuals spend income from EITC benefits in different ways than they do income from other sources, it may affect our ability to generalize the results to all income shocks. This limitation is reduced to some degree because the increase in income from greater EITC benefit generosity comes both through the benefits directly, which may be spent differently than other income, and through shifts in work effort or wages, which may not. Nevertheless, future research on the health effects of other public transfer programs and income shocks would help determine whether the results are limited to the short-term effects of this particular program or whether they can be generalized to incomes that individuals spend in different ways than they do the EITC.

Despite these limitations, it is useful to understand the short-term impacts on health of from shifts in income. Many of the pathways through which we might envision income's influencing health—including greater medical compliance, health-related behavioral changes, and reduced stress—can have an impact in the relatively short term even if these effects grow over time. In that regard, this study found only limited support for the theory that increases in income improve health in the short run.

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