MEDICAL CARE RESEARCH AND REVIEW

MEDICAL CARE RESEARCH AND REVIEW

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Special Supplemental Issue: **The Consequences of Being Uninsured**

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Guest Editor: **Thomas Rice**

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Sicker and Poorer—The Consequences of Being Uninsured: A Review of the Research on the Relationship between Health Insurance, Medical Care Use, Health, Work, and Income

Jack Hadley
The Urban Institute

Health services research conducted over the past 25 years makes a compelling case that having health insurance or using more medical care would improve the health of the uninsured. The literature's broad range of conditions, populations, and methods makes it difficult to derive a precise quantitative estimate of the effect of having health insurance on the uninsured's health. Some mortality studies imply that a 4% to 5% reduction in the uninsured's mortality is a lower bound; other studies suggest that the reductions could be as high as 20% to 25%. Although all of the studies reviewed suffer from methodological flaws of varying degrees, there is substantial qualitative consistency across studies of different medical conditions conducted at different times and using different data sets and statistical methods. Corroborating process studies find that the uninsured receive fewer preventive and diagnostic services, tend to be more severely ill when diagnosed, and receive less therapeutic care. Other literature suggests that improving health status from fair or poor to very good or excellent would increase both work effort and annual earnings by approximately 15% to 20%.

Keywords: *health insurance; medical care use; health; earnings*

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BACKGROUND

Much of the current debate over providing health insurance for the uninsured revolves around questions of cost and strategy (Glied 2001; Kahn and Pollack 2001; Feder et al. 2001; Pauly and Herring 2001). How much will it cost? Who will pay? Should it be public or private insurance? Should employers or workers be subsidized? Should subsidies be provided through tax credits or vouchers?

While these are obviously important questions, the emphasis on costs and policy mechanisms tends to push into the background another important question: does having health insurance improve health? Presumably, people value improved health, longer life and a reduction in pain and discomfort, as a good in and of itself. From a more pragmatic perspective, good health is an important element of human capital, leading to improved educational attainment, improved productivity, and greater labor force participation. As such, improved health can potentially increase incomes, increase tax revenues, and reduce government spending for disability and other health-related transfer programs. If having health insurance does not lead to better health, then the public policy case for expanding insurance coverage would be much weaker.

Does having health insurance improve health? Although this is a deceptively simple question, there is no definitive research that unambiguously provides an answer one way or the other. In the absence of a definitive study, one must draw conclusions based on the weight of the available evidence.

NEW CONTRIBUTION

Levy and Meltzer (2001) distinguished between "observational, quasi-experimental (natural experiments), and experimental" studies. They argued that observational studies should be completely ignored because they are hopelessly confounded by the methodological problems of (1) identifying the direction of causation between health insurance and health and (2) controlling for unobserved factors that might simultaneously determine both health

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insurance coverage and health. However, observational studies make up the vast majority of the available research. Ignoring them leaves only a handful of studies from which to draw inferences about the impact of health insurance on health.

This review takes a broad rather than narrow approach to considering research that might help identify the effect on health of having health insurance. Rather than ignoring observational studies, they are included with annotations as to their methodological strengths and weaknesses. Some observational studies use statistical methods designed to correct for potential sources of bias. Others involve research designs that potentially mitigate underlying problems of the direction of causation or the effects of unobserved factors.

Considering a broad set of studies makes it possible to compare the results of the observational studies with the implications of the quasi-experimental research. How consistent are the results of studies of different populations, with different diseases, in different times and places? To what extent do the studies that fail to find positive health effects of health insurance or medical care use employ methods or research designs that correct for potential underlying bias? While circumstantial evidence is not as clear-cut as experimental evidence, it is not inherently wrong per se. Judgments about whether having health insurance improves health should be based on the weight of all the evidence, discounting findings for methodological reasons when appropriate, but also noting similarities across studies of different populations in different circumstances.

The Institute of Medicine (IOM) (2002) also took a broad perspective in its assessment of the effects of health insurance on access, use, and health. This review extends that analysis in several ways. First, it includes several econometric studies that use instrumental variable (IV) estimation as an approach to addressing problems inherent in analyses of observational data. Second, it provides a more comprehensive assessment of the quantitative estimates of the effect of health insurance on health. Third, it explicitly considers potential problems in making inferences about the relationship between health insurance and health from studies of people covered by Medicaid. Last, if lack of insurance is associated with poorer health, it extends the possible consequences of being uninsured to examine the implications of reduced health status on work and earnings.

CONCEPTUAL FRAMEWORK

The review uses a model of the determinants of health to both organize the literature and suggest interpretations of empirical findings. In its simplest

form, the model hypothesizes that health insurance influences the quantity (and quality) of medical care used; medical care use influences health; and health affects educational attainment, work effort, productivity, and ultimately income through these pathways (see Figure 1). This review seeks to evaluate what the evidence tells us about the directions and magnitudes of the main effects:

- Does lack of insurance make it harder for people to obtain clinically effective acute medical care and preventive medical services on a timely and efficient basis?
- Does lower medical care use (in terms of either quantity, quality, or timeliness) reduce health status?
- Does poor health reduce productivity and/or the ability to work and consequently lead to lower incomes?

As simple as this framework appears, the underlying reality is much more complex, and as a result, empirical analysis of these questions is not straightforward. One major problem confounding empirical analysis is that health itself influences both insurance coverage and medical care use, as shown by the dashed arrows in Figure 1. A second major problem is controlling for the effects of other factors, represented by the box (environment, culture, attitudes, preferences, and health behaviors). These factors, which are often difficult to measure or are completely unobserved in empirical studies, can influence each of the elements along the hypothesized causal chain from health insurance to medical care use to health. Failure to control for their effects, either by direct measurement or by research design, can bias the results of empirical studies of the effects of insurance or medical care use on health. Finally, income and education also loop back as factors influencing both health and health insurance coverage, further complicating statistical analyses of the effects of health insurance on health and income.

Empirical studies can also be confounded by difficulties in measuring health, health insurance, and medical care use. All three are multidimensional constructs with important temporal components. Surveys typically measure health and health insurance status at a point in time but measure medical care use over a fixed time period, such as a year or the preceding three months. Similarly, health insurance can cover a broad or narrow set of services, can offer generous or stingy payment rates to providers, and can have small or large patient cost-sharing obligations. Moreover, the positive effects on health of increased medical care use flowing from continuous and reliable insurance coverage may take several years to manifest themselves. All of these factors

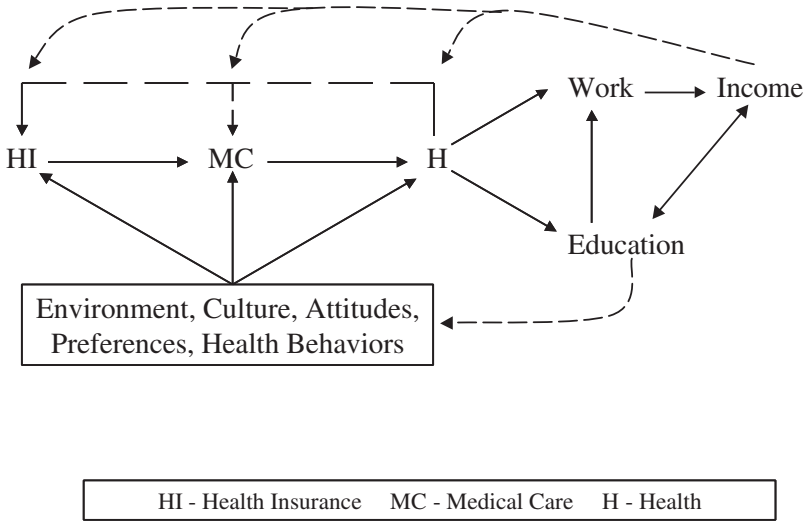

FIGURE 1 Conceptual Model of the Relationships between Health Insurance, Medical Care Use, Health, Education, and Income

Note: HI = health insurance; MC = medical care; H = health.

influence the quantity and timing of medical care use and the subsequent relationship between insurance status and health outcome.

These underlying complexities raise a series of troubling questions about the findings of any single study. The uninsured and the insured differ in many ways. If a study finds a difference in health between the uninsured and the insured, how can one be sure that the explanation is the difference in insurance coverage rather than, or in addition to, differences in other characteristics, some of which may not be observable in the data analyzed? How does one know that the direction of causation does not go in the other direction—that is, Does poor health cause lack of insurance because people cannot get jobs that offer insurance, or does poor underlying health increase both medical care use and poor health outcomes, creating the appearance that greater medical care use results in worse health or no better health? If the evidence suggests that greater medical care use has little effect on health at a point in time, then is it possible for increased medical consumption over time to improve health? Can apparently contradictory findings be reconciled in any way?

While theory cannot answer these questions directly, another representation of the health production process may help guide interpretations and

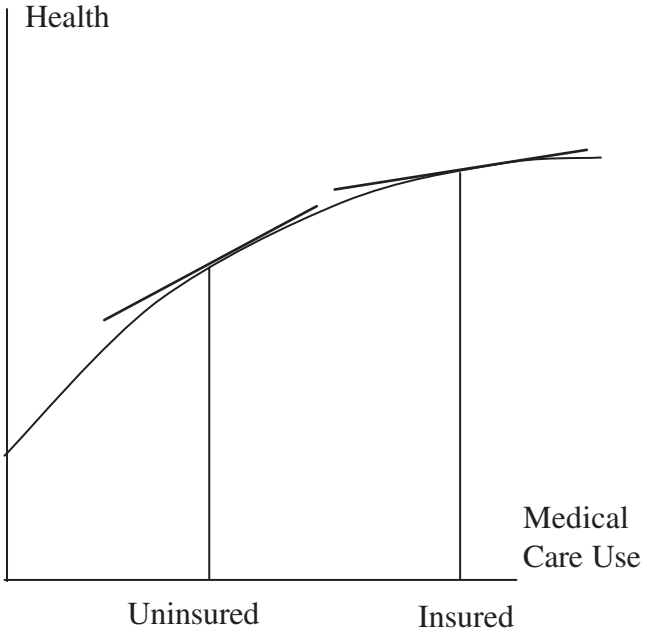

FIGURE 2 Same Health Production Function at a Point in Time

reconciliation of apparently contradictory evidence. Figure 2 shows a health production function, a hypothetical relationship between medical care use and health. It assumes that at low levels of medical consumption, increased use improves health. At some point, however, the curve flattens out, and more use does not improve health. If this figure reasonably represents the true relationship, then it is entirely possible that the uninsured can have poorer health than the insured because of lower medical care use, while increased medical care use by the insured, who are on the flat of the curve, can have no significant effect on health.

The two curves in Figure 3 suggest how it might be possible for additional medical care use to have little impact at a point in time but to improve health over time if the health production relationship shifts up over time, presumably due to technological advances. Note also that in both time periods represented by the two curves, the uninsured would still benefit from additional medical care use, and the change in health over time may be similar for both the uninsured and the insured, even though the marginal impacts of additional medical care use at a point in time are very different.

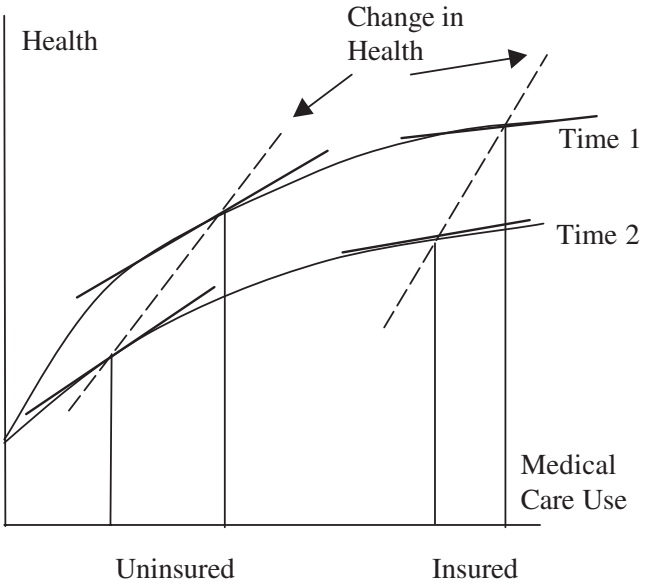

FIGURE 3 Health Production Function Shifts Up over Time

In Figures 2 and 3, the insured and the uninsured are on the same function but at different points. Suppose, however, that the relationship between medical care use and health is fundamentally different for the uninsured. In particular, suppose that it is relatively flat over its entire range, as in Figure 4, perhaps because differences in education, income, and/or social structure make low-income or uninsured people "inefficient producers" of health. In this case, an empirical analysis might still find the uninsured in worse health than the insured, but insurance-induced increases in medical care use would do little to improve health. Rather, broader social policies such as improving education or increasing income transfers might be called for to shift up the uninsured's health production function.

While these conceptual models cannot answer the empirical questions posed by the review, they do suggest the importance of statistical methods that try to sort through the complexities inherent in analyses of observational data. Studies that take advantage of exogenous (to the individual) changes in either health insurance or health status should be given greater weight than studies that simply compare insured and uninsured people. The extent of a study's ability to control for the effects of other factors is also important. In

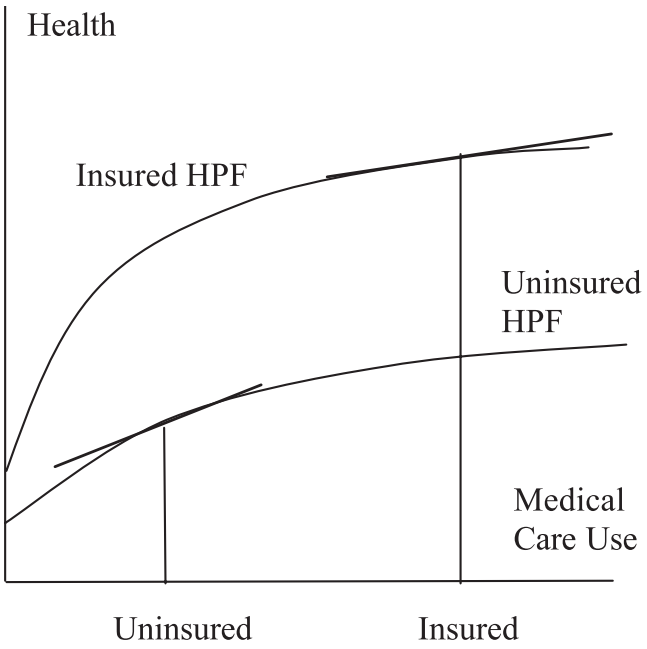

FIGURE 4 Different Health Production Functions (HPF) for Uninsured and Insured

particular, studies of people with a specific health problem or disease have the potential analytic advantage that all of the study cases, both insured and uninsured, have that condition or disease in common. While this does not eliminate all differences in underlying health, it can provide important corroborating or contradictory evidence relative to analyses of general mortality or health status. Studies that recognize and use statistical methods to adjust for the effects of reverse causation from health status to health insurance coverage or medical care use should be given greater weight than studies that do not. Finally, longitudinal studies that follow people over time have the potential advantage that unobservable differences between insured and uninsured people may be less likely to change over time and, therefore, have less impact than unobserved differences between the insured and uninsured at a point in time.

REVIEW STRATEGY AND STRUCTURE

An initial list of studies was selected by searching other literature reviews and bibliographies (American College of Physicians 2000; IOM 2001, 2002; Howell 2001; Currie and Madrian 2000; Office of Technology Assessment 1992). A series of keyword searches was also conducted using the National Library of Medicine's Medline database and the *Journal of Economic Literature*'s EconLit database. Various combinations of keywords were used in the searches: health status and health insurance (or payer source), health outcomes and health insurance, health insurance and mortality, and health insurance and specific diseases (cancer, diabetes, heart disease, etc.). The time period for the keyword searches was limited to 1991 through 2001.

The individual searches identified from as few as 9 to as many as 1,826 citations, depending on the specificity of the key words. More than 9,000 citations (many of which appeared in more than one search) were screened. Bibliographies of selected studies were reviewed to identify other studies either missed by the keyword search or from earlier years. Both published and unpublished work was considered. The review by Currie and Madrian (2000) was the primary source for identifying studies of the effects of health on labor supply and income, supplemented by other studies that are either more recent or were identified through the keyword searches.

Screening the citations produced by the keyword searches, past literature reviews, and individual studies' bibliographies identified 285 distinct, potentially relevant articles for more detailed evaluation. The final criteria for inclusion in this review are

- either explicit comparisons of the uninsured (or self-pay) and the privately insured, or the specification of medical care use as opposed to resource availability, as the key independent variable;
- sample size of at least 500 cases (although exceptions were made for studies with strong research designs); and
- multivariate statistical analysis of the relationships between health insurance, medical care use, and health.

Studies were excluded if they compared medical care use only among the insured, such as studies of the Medicare population only or of privately insured people with either HMO or indemnity coverage. Studies of intermediate birth outcomes, gestation and birthweight, were also excluded because the clinical literature regarding the impact of prenatal care per se on these birth outcomes is controversial and ambiguous and does not provide strong evidence for the efficacy of prenatal care (Fiscella 1995). Although some studies

may have been omitted inadvertently, I believe that the great majority of relevant research was identified and considered.

The final set of studies of health outcomes were then organized into three major groups:

- studies of the relationship between insurance status and the outcomes of specific diseases or conditions,
- studies of the relationship between insurance status and either general mortality or morbidity/health status, and
- studies of the relationship between medical care use and mortality.

Studies of specific diseases or conditions have the analytic advantage that the insured and uninsured are similar along one key dimension, the presence of the particular condition or disease. In some studies, the condition can be considered an exogenous health shock that is unlikely to be strongly related to insurance status. For example, cancer incidence, trauma, and appendicitis are arguably relatively independent of insurance status. They may be related to low income, which is clearly related to insurance status but which can also be observed and controlled for in analysis. In addition, studies of the effects of insurance on the use of clinically relevant services can provide either corroborating or contradictory evidence of the links in the causal chain represented by Figure 1. For example, if studies find differences in outcomes but no differences in related service use by the insured and the uninsured, then that would weaken the inference that insurance coverage is a significant causal factor contributing to the differences in outcomes.

Within these groupings, studies are ranked by their basic methodological approach. As noted above, the primary threats to the validity of observational studies are (1) potential reverse causality between health and either health insurance coverage and/or medical care use and (2) unobserved heterogeneity, that is, the effects of unobserved differences between insured and uninsured people that may simultaneously affect whether they have insurance and their health outcome. Randomized trials and natural experiments generally provide the strongest research designs. However, only one randomized trial, the RAND Health Insurance Experiment conducted in the mid-1970s, even indirectly addressed the question raised by this review (Newhouse et al. 1993). Natural experiments rely primarily on changes in health insurance coverage brought about by government actions. Observational studies, which can be either longitudinal or cross-sectional, are subject to greater methodological uncertainty. However, these effects can sometimes be mitigated by the use of appropriate statistical methods, primarily instrumental variable estimation (Newhouse and McClellan 2000) and by the availability of

extensive and detailed data on people's health and sociodemographic characteristics.

Both observational studies and natural experiments may be subject to other threats to validity, which are also noted. For example, as sample size decreases, the ability to detect significant differences in health between insurance groups declines. An analysis based on data from a single geographic area or a small number of institutions may be affected by unique characteristics of those institutions or geographic areas and, therefore, may have limited applicability to more general populations or settings. Incomplete or inadequate measurement of other control variables may bias or distort the effects of insurance coverage. Including the elderly, who have near universal coverage through Medicare, or nonelderly Medicare beneficiaries, who qualify for Medicare for health reasons (end-stage renal disease or long-term disability), may distort the comparison of the insured and uninsured nonelderly in an analysis.

Another key question in assessing the literature is whether observational studies of health insurance and general health produce results consistent with studies of medical care use and health. In general, one would not necessarily expect the same bias from unobservable factors in the two types of studies. For example, good underlying health may lead one to observe a positive relationship between health insurance and health if healthy people are more likely to be covered by insurance. However, healthy people should also use less medical care, which might lead to the conclusion that using less medical care produces better health. Thus, the extent of agreement between the disease-specific and general health outcome studies, and between studies of the effect of health insurance or medical care use on health, will help gauge the amount of confidence one can place on the observational literature.

The first section of the review considers studies that look directly at the effects of health insurance or medical care use on health. Since several previous reviews have provided extensive summaries of the effects of insurance on medical care use (IOM 2001, 2002; American College of Physicians 2000; Marquis and Long 1994; Office of Technology Assessment 1992), the second section concentrates on studies of the relationship between insurance coverage and services directly related to the disease-specific studies of health outcomes. The third section addresses the conundrum of why many studies have failed to find a positive association between Medicaid coverage and better health outcomes, since expanding Medicaid is often viewed as a primary option for reducing uninsurance. The fourth section examines the evidence that poor health reduces labor supply, productivity (wage rates), and earnings, looking primarily at studies of annual earnings, which incorporates the effects of

health on both labor supply (hours or weeks worked, labor force participation) and productivity or wage rate. The fifth section concludes the review.

STUDIES OF INSURANCE, MEDICAL CARE USE, AND OUTCOMES

Applying the criteria described above resulted in identifying 54 analyses (in 51 distinct studies) for detailed review. Table 1 shows the distribution of the studies by their basic finding (either consistent with or not supporting the hypothesis that having health insurance or greater medical care use improve health) and research designs (outcome measure and basic methodological approach). Overall, 43 analyses report statistically significant and positive relationship, and 11 have results that are not statistically significant. However, of those 11, 4 have quantitative estimates that are similar to those of comparable studies with statistically significant results, and 4 provide partial results supporting a positive relationship between health insurance or medical care use and health.

When one looks at the studies grouped by the strength of their methodological design, the one randomized trial offers only partial evidence consistent with a positive effect of medical care on health. However, the dominance of studies with positive findings persists for each of the other basic methodological approaches: Seven of the 10 natural experiments, 6 of the 7 longitudinal studies, and 5 of the 6 observational studies with instrumental variable estimation have statistically significant results consistent with a positive relationship between health insurance or medical care use and health. Finally, 24 of the 29 observational studies have statistically significant and positive findings.

Table 2 provides a more detailed summary of the specific analyses and their findings. The first column identifies the study, describes the data analyzed, and specifies the study's basic research design: randomized trial, natural experiment, longitudinal, observational, or IV estimation (some studies incorporate combinations of approaches). The second column summarizes the study's main qualitative findings. The third column reports the magnitude of the estimated relationship between insurance or medical care use and the health outcome. Many of the studies report relative odds or relative risks comparing the uninsured to the insured. Some report only regression coefficients. To the extent permitted by information provided by the study, these different approaches to reporting results are converted to estimates of the percentages or probabilities of insured and uninsured people experiencing a particular outcome (approximate relative risk is the ratio of these percentages or probabilities). These estimates emphasize the impact on mortality, both because it is the worst health outcome and because it is unambiguously and consistently

TABLE 1 Numbers of Studies by Methodology and Basic Findings

Type of Study and Basic Research Approach	*Relationship between Health Insurance or Medical Care Use and Health Outcome*	
	Statistically Significant and Positive (Having Health Insurance or Greater Medical Care Use Associated with Better Health)	*Statistically Insignificant and/or Negative (Having Health Insurance or Greater Medical Care Use Associated with Same or Worse Health)*
Insurance and outcomes of specific diseases		
Randomized trial	1	0
Natural experiment	2	0
Longitudinal with observational data	1	0
Observational	15	5
Insurance and general mortality or health status		
Randomized trial	0	1
Natural experiment with instrumental variable (IV) estimation	2	1
Natural experiment with observational data	2	2
Longitudinal with IV estimation	1	0
Longitudinal with observational data	4	1
Observational	9	0
Medical care use and mortality		
Natural experiment	1	0
Observational data with IV estimation	5	1
Total studies	43	11

defined across studies. The final column describes the statistical approach, noting the control variables used and particular strengths or weaknesses of the analysis.

Table 2 is divided into four major sections:

- insurance and the outcomes of specific diseases, by disease group,
- insurance and general mortality,

(text continues on p. 36)

TABLE 2 Summary of Studies

Study, Data, and Method	*Results*	*Magnitude of Effect*	*Comment*
Insurance and Outcomes of Specific Diseases			
Hypertension and high blood pressure			
Manning et al. (1987) approximately 500 low-income people with high blood pressure; randomized trial	People assigned to cost-sharing plan had less reduction in blood pressure than people on free plan	Difference in blood pressure (3 mm HG) equivalent to 10% higher mortality risk	May understate difference due to lack of insurance since many people on cost-sharing plan had less than 100% cost sharing
Fihn and Wicher (1988); 157 adults who lost coverage at VA medical center because of budget reductions; comparison sample of 74 similar adults; natural experiment, longitudinal	Significant differences in health status after 17 months of follow-up, and in uncontrolled high blood pressure after 13 months	41% of those who lost coverage reported health as much worse and had uncontrolled high blood pressure, compared to 8% and 13% of control group; 9.1 mm HG difference in change in blood pressure; discharged people had higher death rate (5.7% compared to 3.5%)	Small sample size; not true randomization

Lurie et al. (1984, 1986); 186 adults who lost Medicaid coverage in California in 1982 because of budget cuts, and a comparison group of 109 similar adults who retained Medicaid coverage; natural experiment, longitudinal	Hypertensives experienced significant decrease in general health status and significant increase in blood pressure relative to controls at 6 and 12 months after losing coverage	After 1 year, 6 mm HG higher blood pressure; death rate was 4 times higher among people who lost coverage (3.9% compared to 0.9%), but not statistically significant	Small sample; control group not truly identical because maintained eligibility due to disability, blindness
Acute myocardial infarction			
Blustein, Arons, and Shea (1995); hospital discharges for 5,857 people admitted for acute myocardial infarction to California hospitals in 1991; observational	Uninsured significantly more likely to die compared to privately insured during initial hospital stay or after short-term transfer	RO = 1.77 without control for revascularization; RO = 1.51 with control for revascularization (implies mortality rate of 8.9% to 10.3% for the uninsured compared to average mortality rate of 6.1%)	Were able to follow hospital stays both before and after index admission for AMI to track follow-up care after index admission; logistic regression controlling for initial AMI severity, age, sex, race, and payor class
Canto et al. (2000); 332,221 people from national registry of heart attack patients, 1994-96; observational	Uninsured had significantly higher in-hospital mortality compared to privately insured	RO = 1.29 (implies mortality rate of 5.1% for the uninsured compared to 4% for the privately insured)	Logistic regression controlling for age, race, sex, prior coronary history, comorbidities, smoking, presentation symptoms, treatment, time to arrival, location, and severity of infarction; Medicare patients included in analysis

(continued)

TABLE 2 (continued)

Study, Data, and Method	*Results*	*Magnitude of Effect*	*Comment*
Kreindel et al. (1997); 3,735 residents of Worcester, Massachusetts, hospitalized for heart attack, 1986-93; observational	Uninsured had higher but statistically insignificant in-hospital mortality risk compared to private FFS	RO = 1.21.	Only 191 uninsured cases; included Medicare patients in analysis; limited to single community; logistic regression controlling for clinical and demographic factors
Sada et al. (1998); 17,600 nonelderly heart attack patients from national registry, 1994-95; observational	Uninsured had higher but statistically insignificant in-hospital mortality risk compared to private FFS	RO approximately 1.2 (implies mortality rate of 4.5% for uninsured compared to 3.8% for private FFS patients)	Limited to patients admitted to hospitals with invasive cardiac procedure capacity; stepwise logistic regression with controls for clinical condition at admission, patient demographics, and hospital characteristics
Young and Cohen (1991); 4,972 emergency nonelderly heart attack patients in Massachusetts in 1987; observational	Uninsured had significantly higher 30-day mortality compared to privately insured FFS	RO = 1.57 (implies mortality rate of 12.4% for uninsured compared to 8.3% for private FFS)	Logistic regression controlling for location and type of myocardial infarction; comorbidities, age, sex, race, income (based on patients' zip code areas), and hospital characteristics

Cancer			
Ayanian et al. (1993); 4,675 nonelderly women with new cases of breast cancer in New Jersey, 1985-87; observational	Uninsured significantly more likely to be diagnosed at late stage; uninsured had significantly higher mortality 54 to 89 months after diagnosis for local and regional disease stages, but not for distant	RO = 1.89 for later-stage diagnosis; survival RR = 1.49 (implies mortality rate of 22.4% for uninsured compared to 15% for privately insured)	Proportional hazard model controlling for age, race, marital status, comorbidity, and community income; survival model includes disease stage
Ferrante et al. (2000), Roetzheim et al. (1999); 29,237 incident cancer cases in Florida in 1994; observational	Uninsured significantly more likely than privately insured to be diagnosed with late-stage disease	RO of late-stage disease: 2.59 for melanoma, 1.67 for colorectal, 1.60 for cervical, 1.47 for cervical, and 1.43 for breast	Logistic regression controlling for age, sex, marital status, education, income, comorbidity
Penson et al. (2001); 860 men diagnosed with prostate cancer in 1995; observational	Uninsured had same health-related quality of life as privately insured at diagnosis, but significantly lower quality of life over 3-year follow-up observation period	Uninsured had more than 10-point reductions in SF-12 scores for physical function, role limitations due to emotional problems, and emotional well-being; people with HMO coverage had increases in these scores	Used validated quality-of-life instrument; mixed model analysis of covariance controlling for education, race, income, marital status, clinical characteristics, and treatment; included Medicare in analysis

(continued)

TABLE 2 (continued)

Study, Data, and Method	*Results*	*Magnitude of Effect*	*Comment*
Roetzheim, Gonzales, et al. (2000); 11,113 incident cases of breast cancer in Florida in 1994; observational	Uninsured had significantly higher 3-year relative risk of death compared to private FFS-insured; higher risk due to greater likelihood of diagnosis at later disease stage	RR = 1.31 (implies mortality rate of 16.1% for uninsured compared to 12.3% for private FFS patients)	Logistic regression controlling for age, race/ethnicity, sex, comorbidities, disease stage, treatment, marital status, smoking status, and zip code measures of income and education.
Roetzheim, Pal, et al. (2000); 9,551 incident cases of colorectal cancer in Florida in 1994; observational	Uninsured had significantly higher 3-year relative risk of death compared to private FFS insured; higher risk persisted even after controlling for stage and treatment	RR = 1.4 to 1.64 (depending on whether disease stage and treatment included in model; RR of 1.5 implies mortality rate of 47.9% compared to 31.9% for private FFS)	Logistic regression controlling for age, race/ethnicity, sex, comorbidities, disease stage, treatment, marital status, smoking status, and zip code measures of income and education
Trauma and acute appendicitis			
Doyle (2001); 10,962 auto trauma cases in Wisconsin in 1992 to 1997; observational	Uninsured were significantly less likely to get treatment and were significantly more likely to die in the hospital	Mortality rate of 5.2% for uninsured compared to 3.8% for privately insured	Exogenous health shock; controls for personal, crash, and hospital characteristics

Haas and Goldman (1994); all (15,008) nonelderly adults hospitalized in Massachusetts in 1990 for emergency acute trauma; observational	Uninsured were significantly more likely than privately insured to die in the hospital; uninsured significantly less likely to receive operative care	RO = 2.15 (implies mortality rate of 2.5% for uninsured compared to 1.2% for privately insured)	Health shock can be treated as exogenous; controlled for injury severity and comorbidities using logistic regression; other control variables were age, sex, and race; results persisted for severe injuries and for different types of hospitals
Tilford et al. (2001); 477 cases of head trauma admitted to 3 pediatric intensive care units; observational	Difference in RO of mortality of uninsured compared to privately insured not statistically significant	RO = 1.69 (implies mortality rate of 12.4% for uninsured compared to 7.8% for privately insured)	Logistic regression controlling for race and intensive care unit; small sample; limited control variables; small number of sites
Braveman et al. (1994); 96,587 nonelderly adults hospitalized for acute appendicitis in California, 1984-89; observational	Uninsured were significantly more likely to have a ruptured appendix compared to privately insured	RO approximately 1.34 compared to all privately insured (implies rupture rate of 33% for uninsured compared to 27% for privately insured)	Exogenous health shock; logistic regression controlling for age, sex, race/ethnicity, substance abuse, diabetes, community poverty rate, hospital characteristics, emergency room, and weekend admission

(continued)

TABLE 2 (continued)

Study, Data, and Method	*Results*	*Magnitude of Effect*	*Comment*
Gadomski and Jenkins (2001); 5,141 kids under age 18 with acute appendicitis in Maryland, 1989-93; observational	Uninsured were not significantly more likely to have a ruptured appendix compared to privately insured	RO = 1.11	Exogenous health shock; this result could be due to the effects of Maryland's hospital rate setting system, which implicitly subsidized hospitals for costs of care to uninsured people; logistic regression controlling for age, sex, race, urban residence; Maryland has a rate-setting system that compensates hospitals for care to uninsured
Hadley and Steinberg (1996); hospital discharge abstracts for 13,885 children (6-18) and 3,907 women (19-50) with acute appendicitis; 11 states between 1989 and 1993; observational	Uninsured were significantly more likely to have a ruptured appendix compared to privately insured	RO = 1.45 (kids), RO = 1.78 (adults)	Exogenous health shock; logistic regression controlling for age, sex, and community measures of demographic characteristics

Other diseases			
Kim et al. (2001); 1995 hospital discharge abstracts for 26,700 cases of HC and 101,200 cases of ALD; observational	Uninsured had significantly higher in-hospital mortality	RO = 1.33 for HC; RO = 1.18 for ALD; approximate mortality rate of 20% for these diseases imply rates for uninsured of 25% for HC and 22.8% for ALD	Logistic regression controlling for age, sex, related comorbidities, and type of hospital; no controls for income or education; lack of insurance and outcomes may be related to unobserved characteristics; includes Medicare patients in analysis
Obrador et al. (1999); 155,067 patients who qualified for ESRD-Medicare between 1995 and 1997; observational	Uninsured had significantly worse blood condition compared to privately insured at entry to dialysis, suggesting less adequate care	Hypoalbuminemia (RO = 1.37); low hematocrit (RO = 1.34); no EPO use (RO = 2.04)	Logistic regression controlling for age, sex, race, employment status, diabetic status, and functional status; includes Medicare patients in analysis
Schnitzler et al. (1998); hospital discharge abstracts for 21,149 nonelderly adults on ventilator support, 1989-92; observational	Uninsured had significantly lower odds of inpatient mortality compared to privately insured	RO = 0.87 (implies mortality rate of 25.3% for the uninsured compared to 28% for the privately insured)	Logistic regression controlling for age, sex, race, zip code income, and hospital characteristics; may be subject to selection bias if uninsured less likely to receive ventilator support; includes Medicare patients in analysis

(continued)

TABLE 2 (continued)

Study, Data, and Method	*Results*	*Magnitude of Effect*	*Comment*
Yergan et al. (1988); 4,369 patients hospitalized for pneumonia between 1970 and 1973 in 17 randomly selected hospitals with discharge abstract data; observational	Self-pay patients had a significantly higher ratio of actual to expected in-hospital mortality	Ratio of actual-to-expected mortality = 1.38	Expected mortality estimated from regression models based on age, sex, height, weight, admission test results, comorbidities, and hospital; includes Medicare patients in analysis
	Insurance and General Mortality		
Currie and Gruber (1996a); infant mortality rates across states between 1979 and 1992; natural experiment–IV	Significant reduction in infant mortality associated with expansion of Medicaid eligibility to low-income women	30 percentage point expansion in eligibility associated with 8.5% decrease in infant mortality	Multivariate regression with IV for Medicaid eligibility, state- and year-specific fixed effects, and measures of time-varying factors within states; analyzes mortality rate of all infants, not just those affected by eligibility expansions

Currie and Gruber (1996b); childhood (ages 1 to 14) mortality rates across states between 1984 and 1992; natural experiment–IV	Significant reduction in childhood mortality associated with expansion of Medicaid eligibility to low-income children	Doubling of Medicaid eligibility associated with 5% to 9% reduction in childhood mortality	Multivariate regression with IV for Medicaid eligibility, state- and year-specific fixed effects, and measures of time-varying factors within states; analyzes mortality rate of all children, not just those affected by eligibility expansions
Hanratty (1996); annual infant mortality in 204 counties across 10 Canadian provinces from 1960 to 1975; natural experiment	Implementation of Canadian national health insurance increased prenatal care use and reduced county-level infant mortality	Universal coverage associated with 4.0% reduction in infant mortality	Multivariate regression model controlling for marital status, area income, parents' ages, prior birth history, and province and year fixed effects; does not analyze data for individual births
Lichtenberg (2002a); age-specific mortality after qualifying for Medicare; natural experiment	Compared to expected mortality based on pre-age-65 projections, actual mortality significantly lower when people qualify for Medicare by turning 65; also significant reduction in bed-days	Medicare coverage associated with 13% reduction in mortality and annual bed-days relative to expected	Uses highly aggregated data; also finds significant and discontinuous increases in medical care use for ages 65 and older compared to trend from ages 50 to 64

(continued)

TABLE 2 (continued)

Study, Data, and Method	*Results*	*Magnitude of Effect*	*Comment*
Franks, Clancy, and Gold (1993); 4,694 nonelderly adults who participated in 1971 to 1975 National Health and Nutrition Examination Survey; longitudinal, observational	Uninsured at baseline had significantly higher relative risk of death	After 17 years of follow-up, uninsured mortality was 18.4% compared to 9.6% for privately insured; adjusted RR = 1.25 implies average mortality rate of 12% for uninsured relative to average mortality of 9.6% for the insured	Cox proportional hazard model with detailed controls for baseline health, health behaviors, and socioeconomic characteristics; may be biased toward 0 because insurance status measured only at baseline; excluded people with public insurance coverage
Sorlie et al. (1995); 147,779 nonelderly adults surveyed by the Current Population Survey in 1982 to 1985 and linked to National Longitudinal Mortality Study through 1987; longitudinal, observational	Uninsured had significantly higher relative risk of death after 5 years compared to privately insured, except for African American women	RR = 1.2 for white men and 1.5 for white women and African American men; RR = 1.2 to 1.3 conditional on working (relative to 2% average annual mortality, implies rates of 2.4% to 3% for uninsured)	Cox proportional hazard model controlling for age, income; may be biased toward 0 because insurance measured only at baseline; no controls for baseline health other than stratification by employment status

Baker et al. (2001); 7,577 adults aged 51 to 61 in 1992 from National Health and Retirement Survey; longitudinal, observational	After 4 years, continuously and intermittently uninsured were significantly more likely to have a major health decline (death or disability) than continuously insured	RO = 1.63 for continuously uninsured and RO = 1.41 for intermittently uninsured; imply rates of major health decline of 12.9% and 11.3% compared to 8.3% for continuously insured	Logistic regression controlling for extensive measures of baseline health and socioeconomic characteristics; measures changes in insurance over time; relatively short follow-up period; excluded people with public insurance at baseline
Baker et al. (2002); 6,072 adults aged 51 to 61 in 1992 from National Health and Retirement Survey with private health insurance at baseline; longitudinal, observational	People who lost insurance coverage between 1992 and 1994 were significantly more likely to experience a major health decline between 1994 and 1996 compared to those with continuous private coverage	RO = 1.82; implies rate of major health decline of 12.4% for people who lost coverage compared to 7.2% for people with continuous coverage	Logistic regression controlling for baseline sociodemographics, health behaviors, and health status; measures change in insurance over time

(continued)

TABLE 2 (continued)

Study, Data, and Method	*Results*	*Magnitude of Effect*	*Comment*
Hadley and Waidmann (2003); 3,951 adults aged 58 to 61 in 1992 from the National Health and Retirement Survey, who turned 65 before 2001; longitudinal-IV	Statistically significant IV estimates of the effects of insurance coverage on death and self-reported poor health at age 65	Continuous insurance coverage reduces predicted probability of death from 6.4% to 3.3% and of poor health from 5.1% to 1.7%; probability of excellent or very good health without disability increases from 42.7% to 52.2%	Validated instrument for extent of insurance coverage over observation period; IV estimate larger in magnitude than observational estimate, consistent with suggestion of downward bias in other longitudinal studies; multinomial logistic model controlling for baseline health, education, health behaviors, and demographics
Hadley, Steinberg, and Feder (1991); national sample of 510,436 hospital discharge abstracts in 1987 for sex-race (white, black) specific cohorts in 4 age groups: 1-17, 18-34, 35-49, and 50-64; observational	Uninsured had significantly higher regression-adjusted probabilities of in-hospital mortality compared to privately insured in 12 of 16 cohorts; relative probability was less than 1 in only 1 cohort (white females, 35-49)	8% to 220% higher in-hospital mortality, depending on cohort; average relative risk of 1.66	Multiple regression controlling for admission severity, Medicare case-mix score, hospital characteristics and community type; limited to in-hospital mortality; no controls for patients' incomes or education

Bradbury, Golec, and Steen (2001); hospital discharge abstracts for 29,237 nonelderly adults admitted for 19 common diagnoses to national sample of 100 hospitals in 1993 and 1994; observational	Compared to privately insured, uninsured significantly more likely to be admitted for more severe condition, more likely to leave against medical advice, and more likely to die in-hospital	RO = 1.37 for in-hospital death; RO = 4.74 for leaving against medical advice	Logistic regression controlling for predicted mortality, diagnosis, condition severity, age, sex, and hospital characteristics; no controls for income or education
Braveman et al. (1989); 146,468 California births, 1982-86; observational	Compared to privately insured, uninsured newborns had significantly higher relative risk of adverse outcome (long stay, transfer, death)	RR = 1.11 (not significant) in 1982, 1.19 in 1984, and 1.31 in 1986 (implies average rate of adverse outcome of 8.5% for uninsured compared to 7.2% for privately insured	Logistic regression controlling for race, ethnicity, gestation, multiple births, and congenital anomalies
Moss and Carver (1998); 9,953 live births and 5,332 infant deaths from the 1988 National Maternal and Infant Health Survey; observational	Compared to privately insured, uninsured babies and babies with uninsured mothers had significantly higher relative odds of death	RO = 1.39-1.46, depending on measure of infant mortality (implies infant mortality rate of 1.1% for uninsured compared to 0.8% for privately insured)	Logistic regression controlling for mother's age, education, race, ethnicity, birth history, income, WIC use, breastfeeding, birthweight, and gestation; limited to low-income (up to 185% poverty) women

(continued)

TABLE 2 (continued)

Study, Data, and Method	*Results*	*Magnitude of Effect*	*Comment*
Insurance and Morbidity or General Health Status			
Newhouse et al. (1993); about 5,800 people in 2,000 nonelderly families randomly assigned to either free care or 13 insurance plans with varying amounts of cost sharing in 1974 to 1977; 3 to 5 years of follow-up; randomized trial	Medical care use increased as cost-sharing decreased, but few differences in health	For adults, no significant differences in general health index and several specific physiological measures (respiratory, muskuloskeletal, gastrointestinal); cost-sharing group had poorer vision outcomes; no difference risk of dying for average person; risk of dying about 10% higher for elevated-risk people on cost-sharing plan (attributable primarily to poorer blood pressure control—see Manning et al. 1987, above). No significant differences in general health and most physiological measures for children; higher rate of anemia among poor children on cost-sharing plan	Not designed to test for differences between people without any insurance (100% cost sharing) and people with insurance; all plans had caps on maximum dollar expenditures and participation incentives to ensure that no family worse off financially as a result of participation

Kaestner, Joyce, and Racine (1999); 12,467 children (aged 2 to 9) with low family incomes interviewed by National Health Interview Survey in 1989 or 1992; natural experiment–IV	Weak evidence that insurance coverage associated with good to excellent health status or number of bed-days	In IV models, privately insured white children and Medicaid covered black/Hispanic children had 13% higher probability of good or excellent health status relative to uninsured; no significant effects on bed days or of Medicaid coverage for white children or private insurance for black/Hispanic children	Multivariate regression analysis controlling for age and sex of child, family income, mother's age, marital status, health, education, and state and year effects; IV estimation of insurance coverage, but IV did not find significant effects of insurance on doctor visits
Haas, Udvarhelyi, and Epstein (1993); all singleton live births in Massachusetts in 1984 (57,257) and 1987 (64,346); natural experiment	Massachusetts's Healthy Start program did not affect difference in rate of adverse maternal health outcomes between uninsured and insured women	Difference in rate of adverse outcomes between uninsured and insured mothers increased from 0.4% to 1.1% after start of program	Single state; not possible to identify who was affected by the program. Related study (Haas et al. 1993) found no differences in quality of prenatal care, suggesting that program was ineffective

(continued)

TABLE 2 (continued)

Study, Data, and Method	*Results*	*Magnitude of Effect*	*Comment*
Racine et al. (2001); 42,140 children interviewed by the National Health Interview Survey in either 1989 or 1995; natural experiment	Expanded Medicaid eligibility for children reduced uninsurance but had very limited effect on medical care use or health, measured by restricted-activity days in past 2 weeks; also, no significant differences in percentages of children rated in excellent or very good health	No significant differences in the change in restricted-activity days in the past 2 weeks by race and ethnicity except for poor Hispanic children in excellent or very good health, who experienced a 48% reduction compared to a 157% increase for nonpoor Hispanic children in similar health	Difference-in-differences approach using nonpoor children in the same race/ethnicity group as controls; assumes that effects of unobservables and secular trends same for nonpoor and poor children
Ross and Mirowsky (2000); 2,592 respondents aged 18 to 95 to 1995 Survey of Aging, Status, and Sense of Control; longitudinal, observational	No difference in health status between uninsured and privately insured at end of 3-year observation period, controlling for baseline health	Private insurance coverage had small and statistically insignificant coefficients relative to uninsured in models of self-reported health status, physical functioning, and presence of chronic conditions	Multivariate regression controlling for education, race, age, gender, marital status, changes in economic status, baseline health; 44% of sample lost to follow-up; analysis included Medicaid and Medicare beneficiaries; people with Medicare and private insurance coded as privately insured

Fronstin and Holtmann (2000); 53,948 workers from the 1999 Current Population Survey; observational	Uninsured workers significantly less likely to report themselves in good health compared to insured workers	3% to 8% lower likelihood of reporting good health status, depending on gender, employment status, and firm size	Multivariate regression model controlling for firm size, marital status, family income, education, race/ethnicity and occupation; no correction for the possibility that good health increases the likelihood of having a job that offers insurance; possible selection bias in limiting to wage and salary workers
Feinberg et al. (2002); 996 children enrolled in Massachusetts's children's health insurance program in 1998 and 1999; observational	Children uninsured for more than 6 months at enrollment had significantly higher reported need for service and higher (not significant) unmet need or delayed care; no difference in need for services after enrollment	At baseline, RO = 3.58 for need for service and RO = 1.29 (not significant) for unmet need or delayed care (implies children uninsured > 6 months had rates or reported need and unmet need/delay of 92% and 15%, compared to average rates of 77% and 12%)	Logistic regression controlling for income, language, race, family size, marital status, employment status, health status, and usual source of care

(continued)

TABLE 2 (continued)

Study, Data, and Method	*Results*	*Magnitude of Effect*	*Comment*
Keane et al. (1999); 750 newly enrolled children in expanded children's health insurance program in western Pennsylvania in 1995; observational	Children uninsured at baseline were significantly more likely to report unmet needs or delays in obtaining care and to have activities limited because of lack of insurance; at 1-year follow-up unmet needs/delays and activity limitations virtually eliminated	Proportion with unmet need/delay in obtaining MD care fell from 25.4% to 3.3%; proportion with activities limited by parent because of lack of insurance fell from 12% to 0.8%	Logistic regression controlling for race, family size, maternal education, parents' employment status, and health indicators
	Medical Care Use and Mortality		
Crémieux, Oullette, and Pilon (1999); infant mortality rates in Canadian provinces, 1978-92; natural experiment	Higher per capita medical spending associated with introduction of Canadian national health insurance significantly related to lower infant mortality rates	10% higher level of spending associated with 0.4% to 0.5% lower infant mortality	Multivariate regression controlling for province and year fixed effects, income, education, population density, lifestyle, and nutrition indicators; very accurate data on medical spending

Auster, Leveson, and Sarachek (1969); age-sex-race adjusted mortality rates across states in 1960; observational-IV	Statistically significant IV estimate of effect of per capita expenditures on mortality	10% increase in per capita spending associated with 1.2% decrease in mortality	Small sample; crude measures of control variables; IV not validated
Hadley (1982); cancer mortality by race (whites, blacks) and gender for 45- to 64-year-olds across U.S. county groups in 1970 (796 county groups for whites and 412 to 428 county groups for blacks); observational-IV	Except for white males, medical care use was not significantly related to cancer mortality rates	10% increase in medical care spending per capita reduces cancer mortality for white males by 2%; estimates for other cohorts statistically insignificant and some negative	IV for medical care expenditure based on Medicare spending per enrollee; other control variables include census based data on education, income, marital status, migration rates, veteran status, and indirect measures of cigarette and alcohol consumption
Hadley (1982); cardiovascular mortality by race (whites, blacks) and gender for 45- to 64-year-olds across U.S. county groups in 1970 (796 county groups for whites and 412 to 428 county groups for blacks); observational-IV	Medical care use significantly related to cardiovascular mortality rates for all four race-gender-age cohorts	10% increase in medical care spending per capita reduces cardiovascular mortality 4.2% for white males, 3.8% for white females, 2.4% for black females, and 1.5% for black males	IV for medical care expenditure based on Medicare spending per enrollee; other control variables include census based data on education, income, marital status, migration rates, veteran status, and indirect measures of cigarette and alcohol consumption

(continued)

TABLE 2 (continued)

Study, Data, and Method	*Results*	*Magnitude of Effect*	*Comment*
Hadley (1982); age-sex-race specific mortality rates across U.S. county groups in 1970 for infants and 45- to 64-year-olds (number of county groups varied from 113 to 790, depending on minimum population requirement in county group); observational-IV	Higher medical spending per capita associated with significantly lower mortality rates	10% higher spending associated with 1.5% to 2.0% lower infant mortality; 0.7% to 3.2% lower mortality for middle-aged men; and 1.3% to 1.7% lower mortality for middle-aged women; average elasticities of –1.6 for adult cohorts and –1.5 for infant cohorts	IV estimation of health production function with census-based controls for cohort-specific population characteristics; IV for medical spending based on Medicare expenditures per beneficiary; IV not validated; in analysis without IV, estimated effects small and insignificant
Lichtenberg (2002b); time series of annual U.S. longevity at birth, 1960-97; observational-IV	Higher medical spending associated with significant increase in longevity.	10% higher spending associated with 0.7% to 0.9% increase in longevity	IV estimation of health production function models, adjusting for effects of possible serial correlation; highly aggregated with few control variables; IV not validated

Rosenzweig and Schultz (1983); 8,119 live births from the 1967 to 1969 National Natality Followback Survey; observational-IV	Longer delay in initiating prenatal care has a statistically significant and positive effect on infant mortality	Regression coefficient for effect of months of prenatal care delay on infant mortality goes from statistically insignificant and negative (–.00145) in observational analysis to statistically significant and positive (.0332) in IV analysis	IV estimation of health production model controlling for smoking, mother's age, race, mother's work status, breastfeeding, and sex of infant; IV not validated; in analysis without IV, prenatal care delay associated with lower infant mortality

Note: Studies with statistically insignificant or negative fundings are noted in italic. Study methods are noted in boldface. RO = relative odds; AMI = acute myocardial infarction; IV = instrumental variable; VA = Veterans Administration; FFS = fee-for-service; RR = relative risk; HC = hepatitis C; ALD = alcohol related liver disease; ESRD = end stage renal disease; EPO = erythropoietin; WIC = Special Supplement Nutrition Program for Women, Infants and Children.

- insurance and morbidity or general health status, and
- medical care use and mortality.

Within each of these major subdivisions, studies are ordered by the strength of their basic research design, then alphabetically by first author.

INSURANCE AND OUTCOMES OF SPECIFIC DISEASES

Hypertension and Heart Attacks

The first three studies listed in Table 2 are a randomized trial (Manning et al. 1987) and natural experiments (Lurie et al. 1984, 1986; Fihn and Wicher 1988) that focused primarily on the effects of high cost sharing or losing free medical care, from either Medicaid or the Veterans Administration (VA), on high blood pressure and hypertension. All three studies are also longitudinal in that they follow cohorts of people over time and also have the advantage of analyzing relatively homogeneous populations: low-income people with high blood pressure, people covered by Medicaid, and people receiving free care from the VA. Thus, the concern that the uninsured are significantly different from the insured in unobservable characteristics is substantially mitigated.

The randomized trial found that among low-income people who began the experiment with high blood pressure, those assigned to insurance plans with cost sharing had a significantly smaller reduction in blood pressure over the study period than those on the free care plan. The difference in blood pressure control was calculated to be the equivalent of a 10% higher mortality risk. More than half the people on the cost-sharing plans had cost-sharing rates of 25% or 50%, and all received lump-sum payments to compensate for financial losses. Thus, this finding may understate the effect on blood pressure control of being uninsured, which is equivalent to 100% cost-sharing with no financial compensation.

In the other two studies, people either lost their Medicaid coverage (Lurie et al. 1984, 1986) or access to free care from the VA because of budget cuts (Fihn and Wicher 1988). In both studies, which followed samples of people for 6 to 17 months, populations with comparable characteristics who did not lose coverage were used as controls. Although the samples were small, those who lost coverage and were hypertensive at baseline experienced significant increases in blood pressure relative to the controls.

Lurie et al. (1984, 1986) followed 186 medically indigent adult patients from a Los Angeles clinic who lost their Medi-Cal (California's Medicaid program)

coverage because they did not qualify under federal criteria for blindness, disability, or Aid to Families with Dependent Children. Their comparison group consisted of 109 adults who did qualify under one of these criteria and, as a result, were in somewhat worse health and had poorer control of their hypertension and diabetes at baseline. In spite of this difference, the adults who lost coverage experienced a significant increase in diastolic blood pressure at both 6 and 12 months after losing benefits, while the comparison group had no significant change in blood pressure.

Fihn and Wicher (1988) followed 157 people who met carefully assessed medical criteria for being in stable medical condition at the time of discharge and 74 comparison subjects who were in similar health and retained coverage. After 17 months of follow-up, 41% of the discharged patients reported their health to be "much worse," compared to 8% of the comparison group ($p < .001$). Almost twice as many of the discharged patients had reduced use of prescribed medications (47% vs. 25%), and among those who had a blood pressure check at 13 months of follow-up, 41% of the discharged patients had uncontrolled high blood pressure compared to 17% of the comparison group. None of the these three studies had sufficiently large samples to detect significant differences in mortality rates, although all three implied higher mortality among those who lost coverage or did not have access to free care.

Five observational studies have analyzed acute myocardial infarction (AMI) patients' in-hospital mortality rates, comparing uninsured and privately insured patients. Two (Blustein, Arons, and Shea 1995; Young and Cohen 1991) also examined mortality after discharge (within 30 days or after short-term transfer to another hospital). All used similar statistical methods, logistic regression controlling for clinical characteristics at admission, demographic characteristics, and treatments received. Three studies (Blustein, Arons, and Shea 1995; Canto et al. 2000; Young and Cohen 1991) found that the uninsured were significantly more likely to die either in the hospital or within 30 days of discharge, with relative odds (RO) ranging from 1.29 to 1.77.

The other two studies did not find significant differences in mortality, although the estimated RO was about 1.2, only somewhat smaller than the low end of the significant findings. However, one (Sada et al. 1998) was limited to people admitted to hospitals with invasive cardiac procedure capacity. To the extent that the uninsured are less likely to be admitted to such hospitals (Blustein, Arons, and Shea 1995), this result could be affected by selection bias. The other study with a finding of no difference was limited to a single community, included a small number of uninsured cases (191 out of a total sample of 3,735), and also included Medicare patients, who made up 53% of the sample.

Cancer

Several studies have examined differences by insurance status in disease stage at diagnosis and survival from various cancers in Florida (Ferrante et al. 2000; Roetzheim et al. 1999; Roetzheim, Gonzales, et al. 2000; Roetzheim, Pal, et al. 2000) and New Jersey (Ayanian et al. 1993). In general, being diagnosed with late-stage disease (stages III and IV) has a highly significant and negative effect on survival. All of these studies employed similar statistical approaches, either logistic or proportional hazard models applied to observational data on nonelderly cases of new cancers (breast, colorectal, cervical, and melanoma) with controls for the age, race, gender, comorbidities, and other selected personal characteristics. None observed income directly but used community-level measures of income. Each study found a statistically significant difference between the uninsured and privately insured people in either the odds of late-stage diagnosis and/or 3- to 6-year survival. The relative mortality risk ranged from 1.31 to 1.64.

These results could reflect "lead-time" bias, that is, the privately insured are diagnosed earlier even within disease stage, which creates the appearance of longer survival. However, the studies by Roetzheim, Pal, et al. (2000) of colorectal cancer treatment and outcome in Florida and by Ayanian et al. (1993) of breast cancer outcomes in New Jersey also suggest that the uninsured were treated less aggressively than the privately insured. One longitudinal study (Penson et al. 2001) used a validated instrument to measure changes in quality of life over a 3-year period for 860 men diagnosed with prostate cancer. It found that the uninsured experienced significant reductions in physical function, role limitations because of emotional problems, and emotional well-being. This result should not be affected by lead-time bias, since it measured changes in quality of life between baseline and follow-up.

Trauma Care and Ruptured Appendix

Trauma care and appendicitis outcomes may be especially good indicators of the effect of insurance status on health because the incidence of trauma or appendicitis should be relatively unrelated to insurance status and, as such, considered an exogenous shock to current health. At the same time, however, it may still be the case that the uninsured have poorer unobserved health prior to the trauma or appendicitis and that differences in outcomes are due to these factors rather than insurance-induced differences in treatment. Haas and Goldman (1994) analyzed 15,008 hospital records of all acute trauma cases between the ages of 15 and 64 in Massachusetts in 1990. Controlling for age, race, sex, severity of injury, and comorbidity, uninsured patients were as likely

as privately insured patients to receive intensive care but significantly less likely to have an operative procedure (odds ratio = 0.68). The uninsured were 2.15 times more likely to die in the hospital. Doyle (2001) analyzed care received by nearly 11,000 people injured in automobile accidents in Wisconsin between 1992 and 1997. People without insurance in severe accidents received about 20% less care and had 37% higher mortality (5.2% compared to 3.8% for people with private insurance). One study of pediatric head trauma (Tilford et al. 2001) estimated a relative odds of mortality of 1.69 for uninsured relative to privately insured children. However, this odds ratio was not statistically significant, in part because of the small sample of 477 cases.

Braveman et al. (1994) assessed hospital discharge data for 96,587 nonelderly adults who were hospitalized for acute appendicitis in California between 1984 and 1989. Controlling for demographic, health, and hospital characteristics, they found that uninsured patients were almost 50% more likely to experience a ruptured appendix compared to cases with private insurance coverage. Hadley and Steinberg (1996) analyzed hospital discharge data from 12 states for years between 1988 and 1991 and obtained a similar result for uninsured children between the ages of 6 and 18 and uninsured adult women between the ages of 19 and 50. Gadomski and Jenkins (2001) also found that self-pay children in Maryland had a greater, though statistically insignificant, relative odds (1.11) of ruptured appendix compared to privately insured children between 1989 and 1992. This result may be due to a combination of Maryland's hospital rate-setting system, which implicitly subsidized hospitals for costs of care to uninsured people, and a relatively strong commitment to community-based health care.

Other Diseases

The final set of four observational studies compared in-hospital mortality for people with hepatitis C, alcohol related liver disease, or pneumonia; or on ventilator support with a diagnosis of DRG 475, a respiratory system diagnosis inclusive of intubation and continuous ventilator support; or blood condition at entry to Medicare's End Stage Renal Disease (ESRD) Program. All four are subject to potential bias because of an arguably stronger association than for some other conditions between unobserved socioeconomic characteristics and disease incidence and because of possible selection effects of insurance on being hospitalized and receiving treatment. Three of the four studies found poorer outcomes for the uninsured compared to the privately insured.

INSURANCE AND GENERAL MORTALITY

Four natural experiments have analyzed the effects of governmental expansions of insurance coverage on mortality. Two (Currie and Gruber 1996a, 1996b) looked at the effects of the expansions of Medicaid eligibility to low-income pregnant women and children that began in the late 1970s; one (Hanratty 1996) examined the impact on infant mortality of Canada's transition to a universal health insurance system in the 1960s and 1970s; and the last (Lichtenberg forthcoming) treated Medicare eligibility and near universal coverage at age 65 as an exogenous change in insurance status that is independent of one's health. While all four are subject to methodological concerns, all four found a statistically significant relationship between insurance expansion and mortality rates across three age groups: infants, children between the ages of 1 and 14, and the elderly. Moreover, the studies by Hanratty (1996) and Lichtenberg (forthcoming) also identified corroborating increases in medical care use associated with insurance expansion.

The studies by Currie and Gruber (1996a, 1996b) have been criticized because they analyzed changes in Medicaid eligibility rather than changes in actual coverage and looked at mortality changes for all infants and children, not just those actually or potentially affected by the expansions (Kaestner 1999). Thus, the reductions in mortality among those actually affected would have to be much larger to produce the predicted overall effect. However, the magnitudes of the estimates from some of the disease-specific mortality studies reported above imply that such an interpretation may be plausible.

Five longitudinal studies using three different data sources and different analytic methods have all found that over time, people who are uninsured, either at baseline or for periods of time during the observation period, have a significantly higher mortality rate than people with private insurance. Franks, Clancy, and Gold (1993) analyzed data for 4,694 adults who participated in the National Health and Nutrition Examination Survey in the early 1970s. Thus, while they only measured insurance status at baseline, they also had the advantage of very detailed information on baseline health characteristics, which they used as control variables in their analysis. Sorlie et al. (1994) used a much larger sample of almost 150,000 nonelderly adults who had responded to the Current Population Survey between 1982 and 1985. This study also measured insurance status only at baseline, followed people for a shorter time period, and did not have as extensive controls for baseline health. Nevertheless, both studies found that the uninsured were 20% to 30% more likely to have died at the end of the observation period in spite of the fact that observing insurance status only at baseline is likely to bias its effect toward zero,

since the insured and uninsured groups presumably become "contaminated" over time by changes in insurance status.

The other three longitudinal studies analyzed data from the National Health and Retirement Survey (HRS), which follows a sample of people between the ages of 51 and 61 starting in 1992. This survey measures insurance status and health status, including mortality, every two years. Baker et al. (2001, 2002) found that those who were either continuously uninsured, intermittently uninsured, or lost insurance coverage over time were significantly more likely than those with continuous coverage to experience a major health decline (disability or mortality) over a 4-year observation period.

Hadley and Waidmann (2003) used HRS data to examine the relationship between coverage and health at age 65, when people qualify for Medicare. Unlike the other longitudinal studies, they treated insurance coverage over time as endogenous and used IV estimation to adjust for the possible effects of health on insurance coverage. They found that continuous insurance coverage has a significant impact on reducing the probabilities of either death or poor self-reported health status. Moreover, the IV estimate, which satisfies standard statistical test for IV validity, is considerably larger in magnitude than the "observational" estimate. This suggests both that older people in good health may be more likely to forgo insurance as they approach age 65 and that the other observational estimates based on longitudinal data may be biased downwards.

The final four studies in this section are all cross-sectional observational studies. Two look at in-hospital mortality rates, controlling for admission severity and diagnosis (Hadley, Steinberg, and Feder 1991; Bradbury, Golec, and Steen 2001), and two examine infant mortality rates (Braveman et al. 1989; Moss and Carver 1998). All four generally find significantly higher mortality rates associated with lack of insurance. In particular, the two studies of infant mortality rates suggest that the uninsured's mortality risk may be almost 40% higher. Depending on the share of the population that is uninsured as insurance coverage expands, this difference is roughly consistent with the 4% to 8% improvement in overall infant mortality found by Currie and Gruber (1996b) and Hanratty (1996) in their analyses of population-wide insurance expansions.

INSURANCE AND MORBIDITY OR GENERAL HEALTH STATUS

The RAND Health Insurance Experiment is the only randomized trial to look at the question of whether type of insurance coverage affects health. As noted above, it compared a variety of health outcomes between families

randomly assigned either to receive free medical care or to health insurance plans with varying degrees of cost sharing. With few exceptions, the study found that people receiving free care used more services but did not have better health outcomes among a broad array of health measures than did people assigned to the plans with cost-sharing requirements. The exceptions were low-income adults with elevated blood pressure, who experienced less blood pressure control over time relative to free care, and poor children, who had a higher incidence of anemia.

This evidence strongly suggests that the health production function, as represented by Figures 2 through 4, does in fact flatten out and that after some level of medical care use, additional care provides little additional benefit. However, in interpreting this evidence in the context of the issues raised by this review, it is essential to understand that the Health Insurance Experiment was not a comparison between uninsured people, who face 100% cost sharing with no offsetting lump-sum payments to compensate them for potential financial losses, and relatively well-insured people, as represented by those with private insurance coverage in the great majority of observational studies. It is also important to underscore that the exceptions to the general finding occurred among low-income people, who are arguably more representative of the population that lacks insurance coverage. While the results of the Health Insurance Experiment clearly support the conclusion that some cost sharing does not reduce health for most Americans, it is not at all clear that they also imply that being uninsured has no effect on the health of lower-income people, where most of the uninsurance occurs.

Analyses of two other natural experiments examine the effects on health status of two government health program expansions, increased coverage of low-income children by Medicaid and Massachusetts's Healthy Start program, which provided care for low-income pregnant women. Neither study found much evidence that either program significantly improved health. Using IV estimation to adjust for potential bias arising from people self-selecting into Medicaid coverage, Kaestner, Joyce, and Racine (1999) found only weak evidence that insured children (between the ages of 2 and 9) were more likely to have good or excellent general health status or fewer bed-days. Among African American and Hispanic children, however, Medicaid coverage was associated with a 13% higher probability of good to excellent general health status.

Racine et al. (2001) also analyzed individual data on children between the ages of 1 and 12 from the 1993-94 National Health Interview Survey (NHIS) for 1989 and 1995. Using a difference-in-differences approach that compared changes for nonpoor children to those for poor children as a way of controlling for unobserved confounding factors, they concluded that the Medicaid

expansions reduced uninsurance among low-income children but had minimal effect on either health services use or health status. Poor African American children in good, fair, or poor health experienced a significant increase in hospitalizations, while poor Hispanic children in excellent or very good health had a significant decrease in restricted-activity days. However, a possible limitation of the difference-in-differences approach is that nonpoor and poor children may not have been subject to identical effects of unobserved trends, which would limit the validity of using nonpoor children as a control group without more detailed controls for differences and changes in circumstances. In addition, restricted-activity days in the past 2 weeks is a fairly subjective and narrow measure of health status.

The remaining four studies of insurance and general health status and morbidity are all observational analyses. Ross and Mirowsky (2000) used longitudinal data on almost 2,600 adults (aged 18 to 95) to look at the effect of baseline health insurance on health status after 3 years. They found no difference between the uninsured and the privately insured. However, more than 40% of their baseline sample was lost to follow-up, and they included Medicare beneficiaries with private supplementary insurance as part of the privately insured group. Even controlling for age and observed baseline health characteristics, combining Medicare beneficiaries with the nonelderly privately insured limits the ability to interpret this study's findings, since the uninsured comparison group is systematically younger than the privately insured group.

The other three observational studies found that the uninsured are in poorer health than the insured. However, the study by Fronstin and Holtmann (2000) was limited to workers and may be subject to both endogeneity bias and selection bias, since health status presumably affects both the ability to work as well as the cost of insurance for people who work for very small employers. The other two studies (Feinberg et al. 2002; Keane et al. 1999) compared health at enrollment to health after enrollment for children who took advantage of expanded health insurance opportunities supported by the State Children's Health Insurance Program. While both found significant improvements in health and health-related activity limitations, the fact that families voluntarily enroll their children raises the possibility that those with a particular need for care were more likely to enroll.

MEDICAL CARE USE AND MORTALITY

The final body of research considered in this section consists of studies of the relationship between medical care use and mortality. This research addresses the second link of the conceptual model represented by Figure 1. One study assesses a natural experiment, the effect on infant mortality of

increases in per capita medical spending associated with Canada's adoption of national health insurance (Crémieux, Oullette, and Pilon 1999). Its primary result—a 10% increase in per capita spending is associated with a 0.4% to 0.5% decrease in infant mortality—is consistent with the analysis by Hanratty (1996), who used variations in provincial implementation of the national health insurance system to identify the impact of expanded insurance coverage.

The other six studies are attempts to estimate the health production function represented in Figures 2 through 4. This set of studies excludes analyses limited to fully insured populations, such as Medicare beneficiaries. As such, the estimates obtained are relevant to levels of medical care use associated with pooled insured and uninsured populations and represent the effects of marginal increases around this mean. These health production function models cover a range of time periods, populations (infant mortality, all people, age-sex-race-specific general mortality, and disease-specific mortality for non-elderly adults), and units of observation (individual births, states, county groups, and a national time series). The one common element of these studies' methodologies is that they all use instrumental variable estimation to adjust for the fact that poor underlying health arguably increases both medical care use and mortality rates.

In spite of the differences in the studies, all but one, which looked at cross-sectional cancer mortality rates in 1970, found that increasing medical care use reduced mortality rates. Although the magnitudes of the estimates vary with the particular population and mortality measures, they tend to fall in the range of a 1% to 2% decrease in mortality associated with a 10% increase in per capita medical care use. All but one of these studies are more than 20 years old and were conducted before the development of formal tests for the validity and quality of IV estimates. Thus, it is not possible to assess their methodologies directly. However, in each analysis, estimation of the health production model without the IV approach resulted in statistically insignificant and sometimes positive estimates, which would be consistent with the effects of unobserved poor health on both use and mortality.

INSURANCE AND THE USE OF CLINICALLY EFFECTIVE SERVICES FOR SPECIFIC DISEASES OR CONDITIONS

Does research on people with particular health conditions or diseases show that uninsured people use fewer diagnostic services, are more severely ill when diagnosed, and receive fewer therapeutic services as well as have poorer outcomes? If the answer is yes, then one can be more confident that differences in insurance-related access to and use of medical care (less preventive

care, later diagnosis and greater severity at diagnosis, and less therapeutic care) are meaningful mechanisms underlying differences between the insured and uninsured in both disease-specific health outcomes and overall mortality rates or general health status. (See Bunker, Frazier, and Mosteller [1994] for an assessment of the clinical links between specific health services and health benefits.)

At the same time, however, finding corroborating evidence does not mean that differences in medical care access and use are the only or the most important factors causing differences in outcomes. Socioeconomic and environmental differences between uninsured and insured people are undoubtedly part of the story as well. Which factors are the most important, which are the most cost-effective strategies, and which are politically feasible to implement are also key social and policy issues but are beyond the scope of this review.

CANCER SCREENING AND DETECTION

Studies reviewed above documented that the uninsured with cancer are more likely to be diagnosed at a later disease stage than the insured. Analyzing large national surveys of nonelderly adults conducted in 1997 and 1998, Ayanian et al. (2000) and Breen et al. (2001) found significantly lower odds of recent cancer screening among the uninsured for tests such as colorectal screening, Pap smears, and mammography. Hsia et al. (2000) and Faulkner and Schauffler (1997) examined data from large national samples of adult women who responded to the Women's Health Initiative surveys between 1994 and 1997 or to the 1991 Behavioral Risk Factor Surveillance System. Both studies found that lack of insurance significantly reduced the odds of having had a mammogram, a clinical breast examination, a Pap smear, or a stool guaiac or a flexible sigmoidoscopy compared to insured women.

Two studies with small, unrepresentative samples reported no differences in cancer screening. Valdez et al (2001) analyzed a small sample of 583 Latina women who were recruited through three community health centers, two HMO clinics, and a breast cancer outreach program. Eisen et al. (1999) failed to a find a significant insurance effect on having had a prostate screening or digital rectal exam within the past 5 years in a sample of 2,652 military veterans drawn from the Vietnam Era Twins registry database.

DIAGNOSIS AND TREATMENT OF CARDIOVASCULAR DISEASE

Several studies have examined service use by people with hypertension or high cholesterol levels, which are risk factors for cardiovascular disease. The

Families USA Foundation (2001) and Huttin, Moeller, and Stafford (2000) both used data from the 1996 Medical Expenditure Panel Survey (MEPS). The former found that the uninsured were significantly less likely than the insured to have been screened or checked within the past year (58.1% of uninsured compared to 75.2% of insured with hypertension and 50% of uninsured compared to 65% of insured with high blood pressure). The latter study found that the uninsured were 59% less likely than the privately insured to have received anti-hypertensive drug therapy. Sudano and Baker (2001) analyzed the use of anti-hypertensive medication by race/ethnicity in a sample of 3,734 people between the ages of 51 and 61 who participated in the national Health and Retirement Survey and also found that the uninsured were less likely to report taking anti-hypertensive medication. Finally, Moy, Bartman, and Weir (1995) investigated a sample of 6,158 adults who reported having hypertension on the 1987 National Medical Expenditure Survey. They found that the uninsured were about 50% less likely than the privately insured to have had a blood pressure check in the past year, to have had more than one doctor visit in the past year, and to be taking any anti-hypertensive medications.

Two analyses of cardiovascular screening and risk-reduction services in the general population using data from the 1997-1999 Behavioral Risk Factor Surveillance System surveys reported significantly lower use by the uninsured (Ayanian et al. 2000; Brown et al. 2001). Faulkner and Schauffler (1997), who analyzed the use of blood pressure and cholesterol screening among almost 30,000 men from the 1991 Behavioral Risk Factor Surveillance System, found that compared to men with insurance coverage for these services, uninsured men were from 7% to 10% less likely to have been screened. Ford et al. (1998) studied 1,724 women between the ages of 50 and 64 who participated in the Third National Health and Nutrition Examination Survey (1988-94). Compared to insured women, uninsured women had worse cardiovascular risk profiles, but were significantly less likely to have had their blood pressure checked in the previous 6 months and to have had their cholesterol level checked.

Several studies have found that uninsured people admitted to the hospital with a heart attack are less likely to receive major therapeutic procedures. Young and Cohen (1991) found that relative to the privately insured, the uninsured were 14% to 43% less likely to receive arteriography, coronary bypass, or angiography. Wenneker, Weissman, and Epstein (1990) found similar results for nearly 38,000 potential cardiac patients in Massachusetts in 1985. Similar results with very different databases were found by Hadley, Steinberg, and Feder (1991), who analyzed a large national database of hospital discharges; Kuykendall, Johnson, and Geraci (1995), who analyzed 24,424 hospital discharges for people with coronary artherosclerosis in California in 1989; and

Carlisle, Leake, and Shapiro (1997), who reviewed hospital discharges for African Americans, Latinos, and Asians in Los Angeles County from 1986 to 1988. Two more recent studies (Canto et al. 2000; Sada et al. 1998) used data drawn from the National Registry of Myocardial Infarction for more than 332,000 people who had heart attacks between 1994 and 1996. Both found that the uninsured used fewer hospital resources and were significantly less likely to have received coronary bypass, angiography, or angioplasty.

Daumit, Hermann, and Powe (2000) and Daumit and Powe (2001) analyzed data for almost 5,000 people who were being treated for ESRD in 1986 and 1987 and had symptoms of cardiovascular disease. Those who had been uninsured at baseline were 24% to 30% less likely to have had a cardiac procedure compared to the privately insured; at follow-up, when all were covered by Medicare, the previously uninsured had a slightly higher rate of cardiac procedure use. These two studies imply that when the previously uninsured obtained both insurance coverage and a regular system of care through the treatment of their ESRD, their use of needed cardiovascular care was similar to those who had been insured prior to qualifying for Medicare coverage.

Three studies suggest some of the ways in which lack of insurance can influence cardiovascular treatment and outcomes. Bluestein, Arons, and Shea (1995) showed that the uninsured were less likely to be admitted to a hospital with revascularization capacity. In a study of 544 children with congenital heart disease, Perlstein et al. (1997) found that referral delay to a pediatric cardiologist was about twice as long for uninsured children compared to children with commercial insurance. Although limited to a small sample of 448 African Americans admitted to two hospitals for chest pain, Ell et al. (1994) also found that the uninsured delayed significantly longer in deciding to seek care for their symptoms, 11.2 hours compared to 7.8 hours for insured patients.

In contrast to these studies, one analysis of 3,006 possible heart attack patients seen in the emergency department of a single hospital found that insurance status had no effect on the admission of high-risk cases (Pearson et al. 1994). However, uninsured medium- and low-risk cases were less likely to be admitted. While these results may reflect hospital policy rather than the general effects of insurance coverage, they also raise the possibility that the privately insured receive too many cardiovascular procedures and the uninsured are treated appropriately; that is, the uninsured receive only necessary care while the privately insured receive unnecessary care.

Two studies have attempted to answer this question by reviewing medical records using accepted and validated criteria to judge whether the use of coronary procedures was medically necessary and appropriate. One study of 631 medically appropriate cases treated in 13 New York City hospitals found that there were no differences by insurance status in recommendations for

coronary revascularization in the hospitals that had revascularization capacity, but in the hospitals that did not have this capacity, uninsured patients were significantly less likely to have had coronary bypass or angioplasty recommended (Leape et al. 1999). The other study sought to determine the extent of overuse/underuse of coronary testing, again using explicit criteria to determine when such testing was appropriate (Carlisle et al. 1999). Using data for 356 people presenting with new cases of chest pain not due to myocardial infarction in 5 Los Angeles hospitals between 1994 and 1996, Carlisle et al. (1999) found that coronary testing was more likely to be underused than overused and that underuse was significantly higher among uninsured patients than insured patients (34% vs. 15%, $p = .01$, but not significant in multivariate regression models of the odds of underuse). Although these studies involved relatively small samples in relatively few institutions, they imply that the uninsured underuse necessary services.

Other research has provided evidence of the contribution of medical care to the dramatic reduction over time in mortality from heart disease. From an extensive review of the literature, Cutler, McClellan, and Newhouse (1998) concluded that "changes in acute treatments such as use of aspirin, beta blockers, thrombolytic drugs, and (to a limited extent) invasive procedures account for a substantial part of the improvement in mortality" (p. 3). Similarly, Cutler and Kadiyala (1999) estimated that about one third of the reduction in cardiovascular mortality over the past 50 years is due to changes in medical treatment, that is, "technological change in treatment of acute episodes and in pharmaceuticals to limit risk factors" (p. 2).

DIABETES

Using data from more than 2,000 adults identified as having diabetes in 1994 on the Behavioral Risk Factor Surveillance System, Beckles et al. (1998) found that the uninsured were less likely to use preventive services (dilated eye examination, self-monitoring of blood glucose, or professional foot examination) than people with any type of insurance coverage. Ayanian et al. (2000) updated this analysis using the 1997 and 1998 Behavioral Risk surveys and confirmed significantly lower rates of dilated eye examination, professional foot examination, and cholesterol measurement among nonelderly, uninsured adults with diabetes.

Other smaller, single-site studies (Songer et al. 1997; Schiff et al. 1998) also tended to find deficiencies in screening and treatment for uninsured or poor diabetic patients. Wilson and Sharma (1995) reported that among a small sample (247) of diabetics hospitalized in Clark County (Las Vegas), Nevada, in 1992 for acute emergencies associated with complications of diabetes, those

without insurance were much more likely to have their admission associated with lack of medication.

BIRTH-RELATED MEDICAL CARE USE

Although the role of timely and high-quality prenatal care is somewhat ambiguous, numerous studies have found that uninsured pregnant women receive less prenatal care than privately insured pregnant women (see American College of Physicians [2001a, 2001b] and Office of Technology Assessment [1992] for earlier summaries of this literature). For example, Braveman et al. (1993) analyzed 593,510 singleton live births in California in 1990 and found that compared to the privately insured, uninsured mothers were significantly more likely to have had late initiation of prenatal care (odds ratio = 2.54), too few prenatal visits (odds ratio = 2.49), or no prenatal care at all (odds ratio = 6.7).

Since studies of administrative records do not have any information about mothers' attitudes, the research by Kalmuss and Fennelly (1990) provides valuable confirmatory evidence of the importance of insurance coverage. They interviewed 496 African American and Hispanic women who delivered babies in six New York City hospitals in 1985 and 1986 and found that lack of health insurance was still a significant predictor of late initiation or no prenatal care, even after controlling for differences in motivation and attitudes about prenatal care, substance abuse, and other sociodemographic factors.

While there are fewer studies of variations in care received by newborns, the evidence also suggests that uninsured newborns receive less care than the privately insured. Braveman et al. (1991) analyzed resources received by sick newborns (N = 29,751), defined as newborns who were discharged with evidence of serious problems from California hospitals in 1987. Uninsured newborns had more severe medical problems than privately insured newborns but received significantly less care, measured by either length of stay (16%), total charges (28%), or charges per day (10%).

Other research has focused on the relationship between high-risk births and the method of delivery, C-section versus vaginal delivery.[1] Aron et al. (2000) applied 39 risk factors to 25,697 women who gave birth at 21 Ohio hospitals between 1993 and 1995 to divide women into risk-factor quintiles. Uninsured women in the two most risky quintiles were 20% to 30% less likely to have had C-section deliveries compared to privately insured women. Stafford (1990) examined more than 460,000 deliveries in California in 1986 and found a significantly lower C-section rate for self-pay and indigent care patients relative to the privately insured, even for women with breech presentations: Ninety percent of the privately insured with a breech presentation delivered

by C-section, compared to 82% of self-pay and 79% of indigent care women. Haas, Udvarhelyi, and Epstein (1993) analyzed singleton births in Massachusetts in 1984 and found that fewer uninsured women had C-section deliveries, 17.2% versus 23.0%.

Given that uninsured women are less likely to have C-section deliveries, even in high-risk situations, Lee et al. (1998) analyzed 371,692 singleton live births with breech presentation in the United States between 1989 and 1991. C-section deliveries had lower neonatal mortality rates compared to vaginal deliveries for all birthweights; for example, for babies weighing 2,500 grams or more, C-section deliveries had a significantly lower ($p < .001$) neonatal mortality rate of 3.2 per 1,000 births compared to 5.3 per 1,000 vaginal deliveries.

Uninsured women's hospital choices may be another factor underlying insurance-related differences in infant mortality. Studies by Bronstein et al. (1995) and Schwartz et al. (2000) suggest that the link between insurance-induced increases in the adequacy of prenatal care and better birth outcomes is not the content of prenatal care per se but better access to high-technology services for high-risk newborns.

As noted earlier, the effects of prenatal care per se on birth outcomes (gestation, low birthweight, or survival) has not been clearly demonstrated (Fiscella 1995). However, method of delivery and access to (or delivery in) hospitals with neonatal intensive care units and greater medical care spending appear to be more strongly associated with better birth outcomes (higher birthweight and greater survival). More generally, Cutler and Meara (1999) asked whether the value of increased life expectancy for low-birthweight infants has been worth the cost of the investment in expensive birth-related medical care. Using annual data from 1950 through 1995, they estimated that increased spending for the care of low-birthweight infants, roughly $39,000 more per birth in 1990 than in 1960, resulted in the survival of an additional 12% of low-birthweight infants, "at what will likely be a reasonable—if not disability free—life." In their analysis, they cited research suggesting that most of the improvement was due to medical care in the immediate postbirth period (Paneth 1995; Williams and Chen 1982). Using relatively conservative estimates of the value of an additional year of life, they concluded that the benefits have substantially exceeded the costs.

Cutler and Meara (2001) presented further evidence suggesting that most of the reduction in infant mortality over the second half of the 20th century was due to reductions in neonatal mortality (within the first 28 days of life), which can be attributed to substantial medical improvements in the care of low-birthweight and premature infants rather than to significant gains in birthweight or gestation. Thus, insurance coverage appears to influence infant

survival by improving access to and use of advanced neonatal care medical services.

CHILDREN'S MEDICAL CARE USE

Newacheck et al. (1998) reported differences in the use of care for insured and uninsured children with data from the NHIS. Uninsured children were less likely to have a regular source of care, less likely to have seen a physician in the past year, and more likely to have gone without needed medical care. McCormick et al. (2001) used data from the 1996 MEPS to show that uninsured children were about two thirds as likely as privately insured children to use any prescription medicines. Research also suggests that the children of uninsured parents are less likely to see a physician than children of insured parents (Hanson 1998) and less likely to have any visit or a well-child visit, even if the child is insured (Davidoff et al. 2002).

In studies of children enrolling in expanded state health insurance programs in western Pennsylvania and western New York, researchers found that uninsured children had considerable unmet need and delayed care, were less likely to have had any prescriptions in the past 12 months, were less likely to have received recommended care, and were more likely to never have had routine care and to not be up to date with well-child care (Lave et al. 1998a; Holl et al. 1995). Stoddard, St. Peter, and Newacheck (1994) found similar differences in a study limited to children with specific health conditions (pharyngitis, acute earache, recurrent ear infections, or asthma): Uninsured children were significantly more likely than insured children to go without any physician care for each of the conditions (odds ratios across the conditions ranged from 1.72 to 2.12). Overpeck and Kotch (1995) and Overpeck et al. (1997) analyzed data from the child health supplement of the 1988 NHIS and found that uninsured children with injuries were significantly less likely to have received medical attention (odds ratios = 0.73 to 0.76 for all injuries and 0.71 for serious injuries).

In a study that addressed the question of whether longer postpartum stays for infants affect their health, Malkin, Broder, and Keeler (2000) analyzed data from more than 108,000 births in Washington state in 1989 and 1990. Infant health was measured by the probability of being readmitted to a hospital between 14 and 60 days after initial discharge, which occurred for between 2% and 5% of the births in their data. Using instrumental variable analysis to adjust for the fact that the infant's health affects postpartum length of stay, they estimated that a 12-hour increase in postpartum length of stay would reduce the probability of readmission by 0.6 percentage points. This study is pertinent because they used infants' insurance coverage to create their

instrumental variable for length of stay and found that Medicaid infants stayed 4 hours less and uninsured infants stayed 6 hours less than privately insured infants, controlling for an extensive set of infants' health characteristics.

A CLOSER LOOK AT MEDICAID

Many of the studies of both disease-specific and general health outcomes discussed in previous sections have reported worse outcomes for people covered by Medicaid compared to those with private insurance (Ayanian et al. 1993; Blustein, Arons, and Shea 1995; Braveman et al. 1993, 1994; Canto et al. 2000; Ferrante et al. 2000; Hadley and Steinberg 1996; Moss and Carver 1998; Obrador et al. 1999; Roetzheim, Gonzales, et al. 2000; Roetzheim, Pal, et al. 2000; Roetzheim et al. 1999; Ross and Mirowsky 2000; Sada et al. 1998; Sorlie et al. 1994) or outcomes that are no better than those of the uninsured (Kaestner, Joyce, and Racine 1999; Racine et al. 2001; Schnitzler et al. 1998; Tilford et al. 2001). Several evaluations of Medicaid eligibility expansions to pregnant women failed to find positive effects on health outcomes (Howell 2001).

Do these results imply that having Medicaid causes poor health or that health insurance has no effect on health? Can the decidedly mixed results of the studies of Medicaid's association with health outcomes be reconciled with the majority of research that compared the uninsured to the privately insured? Three factors that potentially differentiate Medicaid studies from studies of private insurance are the characteristics of the Medicaid population, the process of obtaining Medicaid coverage, and the structure of Medicaid as an insurance program.

One possible explanation of the Medicaid conundrum is that people covered by Medicaid, who voluntarily choose to enroll in the program, are systematically different from both the general population of uninsured people and the population of privately insured. By design, people covered by Medicaid are much more likely to have the lowest incomes and education levels, to be members of single-parent families, and to not be in the labor force. For the most socially and financially disadvantaged people, health insurance alone may not be sufficient to overcome barriers to the timely and efficient use of medical care created by low educational attainment, unstable family and living arrangements, and very low income.

In contrast, most of the uninsured are low-income workers or their dependents and are more likely to be part of two-parent households. As an example of the effects of potentially significant differences in the characteristics of people covered by Medicaid, Krug et al. (1997), in a study of 4,318 admissions of children through the emergency department, found that children on Medicaid

were more than twice as likely as either uninsured or privately insured children to be admitted for "nonmedical" reasons, that is, for reasons having to do with the child's social-economic situation rather than clinical condition.

Moreover, since people voluntarily choose to enroll, those who seek Medicaid coverage may be in poorer health to begin with or may anticipate medical problems. In other words, the endogeneity problem of poor health leading to Medicaid coverage may be more significant in studies of Medicaid and the uninsured. It is estimated that between 40% and 50% of nonelderly adults covered by Medicaid obtain their coverage for reasons not related to income or welfare status.[2] In other words, they are most likely eligible because of poor health, either disability or a major health expenditure that causes income to fall below the ceiling for coverage through medical spend-down provisions. This phenomenon creates a form of selection bias that distorts the true effect of extending insurance coverage to an uninsured population. As a counterexample highlighting the importance of having an appropriate control or comparison population, recall the natural experiment that compared people who were administratively removed from Medicaid to those who retained Medicaid coverage and that did in fact find significant health deterioration associated with the loss of Medicaid (Lurie et al. 1984, 1986).

A second factor possibly contributing to the weak effects on health associated with Medicaid is that the structure of Medicaid itself varies dramatically from state to state in ways that are very difficult to control for statistically. While Medicaid generally provides medical care at no cost to the recipient, the amount, quality, and timeliness of that care can vary widely because of substantial differences in how much Medicaid pays providers and their willingness to treat Medicaid beneficiaries. In some locations, care paid for by Medicaid may not be very different from or better quality than care provided at no cost to the uninsured in public clinics and hospitals or that the uninsured pay for themselves.

For example, Currie, Gruber, and Fischer (1995) found that variations across states in the ratio of Medicaid to private fees for obstetricians-gynecologists were significantly related to infant mortality rates. They estimated that a 10% higher average Medicaid fee was associated with a 0.5% to 0.9% lower infant mortality rate. Gray (2001) investigated the relationship between Medicaid fees and birth outcome using data from the 1988 National Maternal and Infant Health Survey. Using a difference-in-differences approach to control for the effects of unobservable factors, he concluded that higher Medicaid fees were significantly related to a lower risk of having a low-birthweight infant. Thus, unmeasured differences in Medicaid programs' structure and generosity make it difficult to attribute differences in health outcomes

between people covered by Medicaid and private insurance to a simple measure of having a particular type of insurance coverage.

A third problem research studies face is that it is often difficult to determine exactly when Medicaid coverage began. For example, many pregnant women or low-income sick people may not have been enrolled in Medicaid until their conditions were sufficiently advanced to warrant a visit to a hospital. (This is another variant of the basic endogeneity problem.) Although their sample was small ($N = 149$), Oberg et al. (1990) found that 28% of the women who were uninsured at the start of their pregnancy were covered by Medicaid at the end of pregnancy. In an analysis of Medicaid-covered women who had babies in Washington state in 1988 and 1989, Katz, Armstrong, and LoGergo (1994) found that 28% enrolled in the third trimester and 19% enrolled in the second trimester. Women who enrolled in the third trimester were 6.3 times more likely than privately insured women to have had inadequate prenatal care.

Moreover, once contact with a provider has been made, it is in the provider's interest to seek Medicaid coverage ex post to obtain reimbursement for services that would otherwise be written off as charity care or bad debt. Thus, at the time care is sought or obtained, people who appear to be Medicaid recipients on survey or administrative data are in fact more similar to uninsured people than they are to people with continuous private insurance coverage. Similarly, Medicaid coverage is often short-term or transitory, while the positive health effects of increased medical care use by a population due to expanded insurance coverage may take several years to reveal themselves.

Finally, most of the pre-/post-Medicaid expansion studies have been unable to identify and analyze the specific people who shifted from uninsurance to Medicaid. Research suggests that 15% to 50% of people who enrolled in Medicaid in response to the expansions may have switched from private insurance (Shore-Sheppard 2000). Szilagyi, Holl, et al. (2000) and Szilagyi, Shone, et al. (2000) reported that 38% were fully insured prior to obtaining Medicaid, 27% were uninsured for 1 to 5 months, and 35% were uninsured for 6 to 12 months. These types of shifts would tend to bias results toward a finding of no difference by insurance status. In fact, Currie and Gruber (2001), who analyzed medical care received at childbirth, found that women who probably shifted from private insurance to Medicaid coverage might have experienced a reduction in childbirth-related procedures.

Another form of substitution, reported by Epstein and Newhouse (1998), was California's reduction in funding for public clinics that accompanied its expansion of Medicaid coverage for pregnant women. Uninsured pregnant women who may have received free care from public clinics prior to the expansions may have received the same care, now paid for by Medicaid, after the expansions. This may be one factor explaining why other studies (Haas et

al. 1993; Piper, Ray, and Griffen 1990; Dubay et al. 1995; Braveman et al. 1993) have failed to find increases in prenatal care use that accompanied expansions of Medicaid coverage or differences in service use associated with Medicaid coverage of children (Racine et al. 2001; Kuhlthau et al. 2001).

Taking a closer look at Medicaid—at the characteristics of people covered by Medicaid, at differences across-Medicaid programs and between Medicaid and private insurance, at possible substitutions between Medicaid and either private insurance or free medical care from the safety net, and at the statistical treatment of Medicaid coverage in the great majority of observational studies—suggests a number of explanations for the findings of some studies that people covered by Medicaid do not have better health outcomes than the uninsured. For some subpopulations, the most socially and economically disadvantaged, insurance coverage alone may not be sufficient to overcome significant barriers to obtaining timely medical care. In other cases, problems with the structure of Medicaid itself may weaken its effects relative to private insurance. If these explanations are valid, then the much more mixed findings of the studies of Medicaid's effect on health do not necessarily contradict the studies that find that the uninsured have poorer health outcomes than the privately insured. Rather, they highlight the importance of accounting for both the characteristics of disadvantaged subpopulations and the detailed structure of the insurance plan.

HEALTH AND ANNUAL INCOME (WORK AND/OR WAGE RATES)

If lack of health insurance reduces health status, then what are the consequences for work and annual income? Studies have looked at various components of work effort—labor force participation, amount of work (hours per week), wage rates, and annual income. Although results vary, in part because of variations in how health is measured, the research generally concludes that poor health reduces annual earnings from work, primarily through reduced labor force participation and work effort in conjunction with a small effect on productivity, as measured by wage rates. The studies summarized below focus primarily on the relationship between health and annual income, which is essentially the product of hours of work (labor force participation and work effort) and income per hour (productivity).[3]

Some simple tabulations illustrate the complex relationships between health, work, and income. The National Academy on an Aging Society (October 2000) presented data from national surveys conducted in the early 1990s on health and income characteristics of early retirees (aged 51 to 59) and older workers (60 and older). Among the 51-to-59-year-old cohort, a much higher

proportion of early retirees reported that they were in fair/poor health, 46% compared to 12% of workers of the same age. However, among older people, a much higher proportion of workers report themselves to be in excellent or very good health, 48% compared to 26% of nonworkers. Moreover, young retirees in fair/poor health report substantially lower median incomes ($15,000 compared to $41,000) and median wealth ($34,000 compared to $200,000) than young retirees in excellent or very good health. Thus, high income and wealth appear to induce healthy people in this age group to retire early but, simultaneously, poor health in this same age group reduces income and wealth and forces early retirement.

More sophisticated analyses of labor force transitions for older workers (aged 50 and above) confirm the effects of both poor current health and health deteriorations over time (Bound et al., 1999; Blau, Gilleskie, and Slusher 1999). Controlling for prior health, poor current health is strongly associated with both labor force exit in general and application for disability insurance benefits. Smith (1999) exploited the longitudinal information in the Health and Retirement Survey to look at the effects of the onset of a major illness on hours worked per week and the probability of working, controlling for demographic factors, health risk behavior, and preexisting health conditions. He found that the onset of a new major illness resulted in statistically significant decreases between survey rounds of about 4 hours of work per week and of 15% in the probability of working at all.

As straightforward as these inferences may appear, studies of the relationship between health and work and annual income can be confounded by two problems. The first is that people who choose not to work or to work less may be motivated to report poor health to qualify for health-related income payments (disability income or Supplemental Security Income, for example) or because "poor health" is a socially acceptable reason for not working. Second, if people in poor health are less likely to work, estimating the relationship between health and earnings (or wage rates) from data on workers probably understates the total effect of health, since the working population is a selected sample based in part on the effect of health on labor force participation.

Given these difficulties, two studies of men with arthritis (Mitchell and Burkhauser 1990; Mitchell and Butler 1986) and one study that used a measure of general health over an extended time period (Chirikos and Nestel 1985) were able to adjust for the potential selection bias associated with labor force participation. These studies generally found that poor health (having arthritis or poor general health) reduced annual earnings by 15% to 30%.

Three other studies did not make the econometric corrections but still found significant negative effects of poor health on annual earnings. Luft (1975) found that people with activity limitations had annual earnings 30% to

40% lower than people without limitations. Bartel and Taubman (1975) estimated that earnings were 8.5% lower for people with heart disease or hypertension, 22.4% lower for people with arthritis, and 28.7% lower for people with bronchitis or asthma. Finally, Mullahy and Sindelar (1993), who analyzed survey data collected in New Haven, Connecticut, found that people in good physical health had annual earnings 37.7% higher than people with health problems.

Several studies have focused on the effects of mental illness, alcoholism, and drug addiction (Bartel and Taubman 1986; Ettner, Frank, and Kessner 1997; Mullahy and Sindelar 1993, 1995). Although the effects of health insurance on health status related to these conditions are not well established, these studies also found significant negative effects on income associated with neuroses, psychoses, and both recent and long-term alcoholism. The size of the estimates ranges from –10% to –47%.

Fronstin and Holtmann (2000), who did not adjust for possible bias due to the effect of poor health on labor force participation, estimated that workers in good health earned 13.3% to 20.5% more than workers in poor health, depending on the industry and the size of their employer. Hadley and Reschovsky (2002) estimated the effect of health status on hourly wage using a Heckman selection model to adjust for bias due to the effect of health on labor force participation. Estimated with data on nonelderly adults from two large nationally representative surveys conducted in 1996 and 1998, the results suggest strong negative effects of fair or poor health status on both labor force participation and hourly wages. Those in poor health were less than half as likely to work compared to someone in excellent health, and if they did work, their hourly wage was about 23% lower.

There is also evidence that poor health of a family member reduces the caregiver's work and earnings. Wolfe and Hill (1995) found that single mothers are less likely to work if they have a child with a disability. This same study estimated that providing health insurance for children not tied to Aid to Families With Dependent Children/Medicaid prohibitions against work would increase single mothers' labor force participation. Similarly, several studies (Ettner 1995; Boaz and Muller 1992; Stern 1996) have found that women are more likely to work less or not at all if they have a disabled or very ill parent, with one study (Stern 1996) estimating an approximately 20% reduction in labor force participation. Berger (1983) studied the effects of a spouse's illness, disability, or death on labor supply and found that both husbands and wives are affected, but in opposite direction, with husbands decreasing labor supply in response to a wife's health decline and wives increasing their labor supply in response to a husband's health decline. Muurinen (1986) analyzed data on 1,445 family caregivers from the National Hospice Study. Just under half were

in the labor force with annual incomes of approximately $17,000 at the onset of informal caregiving for a dying person. About 30% left the labor force, and almost all of the others reduced their hours worked. Lower annual earnings were associated with a higher probability of the caregiver leaving the labor force.

Overall, these studies suggest that "fair or poor" health, due to either a disability, a serious chronic condition, or general self-assessment, is associated with a 15% to 20% reduction in annual earnings. Most of the reduction appears to come from lower labor force participation and work effort.

SUMMARY AND DISCUSSION

This review finds that there is a substantial body of research supporting the hypotheses that having health insurance improves health and that better health leads to higher labor force participation and higher income. However, none of these studies are definitive; nor are their findings universally consistent. While all of the studies reviewed, including those whose findings are consistent with the above hypotheses, suffer from methodological flaws of varying degrees, one general observation emerges: there is a substantial degree of qualitative consistency across the studies that support the underlying conceptual model of the relationship between health insurance and health.

Studies of different medical conditions, conducted at different times, using different data sets and statistical methods, have produced qualitatively similar estimates of the effects of having health insurance or using more medical care on health outcomes. These studies include observational analyses of cross-sectional and longitudinal data, some of which had extensive and detailed observations on underlying health characteristics, analyses that make statistical adjustments for possible biases from reverse causation or unobserved differences in insured and uninsured populations, as well as several natural experiments.

Every one of these studies could be biased, and it is not difficult to identify potential sources of bias. For example, some, but not all, of the observational studies did not have direct controls for family income. The uninsured in the disease-specific studies may have had poorer general health, which may have contributed to their being uninsured and to their poorer health outcome. In the absence of a true experimental design, it is not difficult to speculate about possible design flaws that could lead to biased results.

But how likely is it that the large number of studies considered would all be biased in the same way, given that they are so different in data and research designs? In fact, the instrumental variable studies and the longitudinal studies

that measured insurance status only at baseline suggest that if there is bias in the observational analyses, it may be toward a finding of no difference in health outcomes. The fact that the estimated effect of insurance or medical care use increases when an instrumental variable approach is used suggests that better unobservable health may be a factor leading to a greater likelihood of being uninsured, of using less medical care, and of having better health outcomes, even though the uninsured appear to be somewhat worse off than the privately insured in observable health characteristics. The remarkable degree of consistency across so many studies in the estimated effects of health insurance on health outcomes and on the intervening mechanisms—use of preventive services, timely diagnosis, adequate therapeutic care—makes a compelling, albeit circumstantial, case for the importance of health insurance coverage to the nation's health and wealth.

How big is the effect of insurance on health? Although one would like to combine the results of the studies of morbidity and mortality, since poor health encompasses both, there is no acceptable metric for combining differences in outcomes as diverse as general health status, rates of ruptured appendices, and differences in blood pressure. Therefore, to combine the information available from multiple studies, it is necessary to limit consideration only to the studies of disease-specific and general mortality.[4]

Thirteen studies analyzed disease- or condition-specific mortality rates for adults.[5] All but one, including two with statistically insignificant findings, implied adjusted relative risks in excess of 1.0, with specific estimates ranging from 1.14 to 2.08. The simple average of all of these relative risk measures is 1.37—on average across these studies of people with particular illnesses or conditions, the mortality rate for the uninsured was 37% higher than for the insured.

Two longitudinal studies of general mortality among all nonelderly adults found relative risks of about 1.25 (Franks, Clancy, and Gold 1993; Sorlie et al. 1994). Given the presumably higher risk of death associated with having any of the conditions analyzed in the disease-specific studies, the somewhat lower relative risk for the uninsured in general mortality studies is a consistent finding. At the same time, three longitudinal studies of major health declines (disability or death) among older middle-aged adults (roughly ages 51 to 67) estimated relative risks ranging from about 1.5 to 3.0 (Baker et al. 2001, 2002; Hadley and Waidmann 2003). This estimate also appears consistent with the inference that lack of health insurance has more severe consequences in an older population that is more prone to major health shocks relative to the population of all nonelderly adults.

Complementing these studies were several analyses of medical care spending or insurance expansions that used aggregate data to examine the impact of

increasing insurance coverage in a population. These studies suggest reductions in infant and childhood mortality of 4% to 8% associated with expanding insurance coverage to infants, pregnant women, and children and of 13% associated with the natural experiment of aging into universal coverage under Medicare. The studies of medical care spending and mortality generally reported elasticities, the percentage change in mortality associated with a 10% change in per capita medical care use, with estimates ranging from –0.7% for the entire population to about –1.5% for specific infant and adult cohorts. Combining these estimates with the finding from other research suggesting that health insurance increases medical care use by 50% (Marquis and Long 1994) implies that expanding insurance coverage to the uninsured would reduce their mortality rate by 3.5% to 7.5%.

Overall, the range of estimated magnitudes is fairly large, making it difficult to derive a precise estimate of how much the uninsured's mortality rate might fall if they had coverage. The lower end of the range suggests reductions of 4% to 5%, while the upper end may be 5 times higher for a general adult population. In 1999, the age-adjusted mortality rate for people younger than 65 was 220 deaths per 100,000, and 16.2% of the nonelderly population was uninsured (National Center for Health Statistics 2001, 2002). Assuming a relative mortality risk of 1.2 for the uninsured compared to the insured implies mortality rates of 213 per 100,000 for the insured and 256 per 100,000 for the uninsured. If this difference were eliminated, there would be 17,200 fewer deaths (assuming about 40 million uninsured, as in 2002). If the relative mortality risk were 1.05, then there would be 4,300 fewer deaths. Given the broad range of data, populations, time periods, statistical methods, and measures in the underlying studies, this range of quantitative estimates should not be surprising. Nevertheless, the qualitative consistency of the results argues against concluding that they are spurious or due to underlying bias from reverse causality or unobservable variations in underlying characteristics.

Several early studies (such as Fuchs 1974; Glazer 1971; McKinlay and McKinlay 1977; Illich 1976; and McDermott 1981) argued that medical care has little impact on health. However, this general conclusion, which may be true for the average insured person in the short run or cross-sectionally, does not necessarily apply to the average uninsured person, who may be sicker than the average insured person and consuming significantly less medical care. In other words, even if the marginal benefit of additional medical care to the average, relatively healthy, privately insured person is close to zero (with a static medical technology), it does not follow that the benefit is also zero for an inframarginal person, that is, someone without insurance. (See Brooks, McClellan, and Wong 2000, who developed explicit estimates of the marginal

benefits of heart attack treatments for different payer groups and found that the uninsured had the largest expected benefit from additional treatment.)

It is also questionable whether one can extrapolate from the time periods covered by these earlier studies to the present. Most used data from roughly 1970 or earlier, either predating or including only the first few years of Medicare and Medicaid, before these programs had substantial and sustained impacts on medical care use by their beneficiaries. Perhaps even more important, these time periods precede the undoubtedly substantial effects of expanded public insurance coverage on the rate of innovation in medical technology. In an analysis of the changing age distribution of mortality over the 20th century, Cutler and Meara (2001) showed that the rate of improvement in longevity at birth did indeed slow down substantially over the period from 1945 to 1965, as observed by Fuchs (1974), Glazer (1971), and others writing in the early 1970s. Cutler and Meara went on to show that that rate of improvement in longevity at birth accelerated after roughly 1965, due primarily to significant improvements in the medical treatment of low-birthweight infants and older adults with cardiovascular disease. Their analysis suggests that simple extrapolation of the longevity trend observed for 1945 to 1965 significantly understates the actual increase in longevity.

Over time, improvements in technology have had an enormous impact on improved longevity and health status. Cutler and Richardson (1999) suggested that even if the marginal value of additional consumption is very low at a particular point in time, upward shifts of the entire health "production function" over time can lead to significant health improvements as medical care use and spending increase. Skinner, Fisher, and Wennberg (2001), who argued that additional medical care use in the Medicare population has a low marginal impact on the mortality of the elderly, made the same point. However, even if one accepts as valid the findings of the more methodologically sound studies that suggest little or no health benefit from additional medical care use by well-insured populations (Newhouse et al. 1993; Skinner, Fisher, and Wennberg 2001), it does not follow that the uninsured would not benefit both from health insurance coverage and from greater medical care use. In fact, it seems both inappropriate and unfair to argue on the basis of these studies that the uninsured should be penalized, that is, denied help in obtaining insurance coverage, because of the inefficient or excessive use of medical care by the well insured.

This review also gave explicit consideration to what one might call the Medicaid conundrum: why do so many studies find that people covered by Medicaid have worse health outcomes than the privately insured? To a large extent, these anomalous findings can be attributed to a combination of factors: (1) the endogeneity problem, that is, reverse causation from poor health to

insurance coverage, may be greater in studies of Medicaid coverage; (2) significant differences across states' Medicaid programs in their generosity of payment to providers may weaken Medicaid's insurance effect relative to private coverage; (3) substitution of both other public programs and private insurance for Medicaid financing may confound pre/post studies of Medicaid expansions; and (4) the fact that health insurance by itself may not be enough to overcome the effects of the substantial socioeconomic deficits of many Medicaid recipients.

Skeptics may still argue that the evidence is suspect, since it is not based on true experiments that randomly assign people to either having or not having insurance. If political action continues to stall because of doubts about the health benefits of health insurance coverage, then it may be time to consider a new health insurance experiment. The IOM (2002) recently called for such an experiment as part of a broad range of demonstration projects. Rather than focusing on the effects of cost sharing on medical care use and outcomes in a general population, as did the original Health Insurance Experiment (Newhouse et al. 1993), the population for the new experiment should be drawn from those who are currently uninsured. Participating families would be randomly assigned to a treatment group that receives insurance coverage or to a control group that remains uninsured (at least initially) but is compensated for continued participation in the study. In this way, no participant in the study, whether in the experimental or control group, would be made worse off because of participation.

Moreover, the experiment should not limit people's health insurance coverage decisions over time, since one of the key areas that needs further analysis is the dynamics of health insurance coverage and the relationship to labor market, educational, health status, and family transitions. Families in the uninsured control group should be allowed to obtain coverage if they can and those in the experimental group to accept "better deals" if available.

Following both groups over a sufficiently long time would provide important information for policy. To what extent is lack of insurance a short-term rather than a long-term problem for specific families? How many of the uninsured eventually gain insurance? What factors determine health insurance coverage transitions? How much medical care do they receive? Where do they get it and how do they pay for it? Most important, periodic health status comparisons between the continuously insured treatment group and the control group should settle the question of whether having health insurance improves health.

With or without new experimental evidence, there are still many other significant issues for research to consider. One important next step should be to develop better estimates of the size of the potential benefits that would accrue

from expanded insurance coverage, taking into account the current range of estimates of the impact of having insurance on the uninsured's mortality. How much would labor force participation increase? How much would incomes and tax revenues increase? What might be the effects on disability transfer payments? What are the implications for both Medicare and Medicaid spending of having a healthier population? Estimates of the size of the potential benefits should become a prominent part of the policy debate over expanding health insurance coverage.

Another area for research to explore is the cost-effectiveness of insurance as a health-improving intervention, particularly with regard to the structure of alternative health insurance plans. In particular, how might various cost-sharing provisions (coinsurance and deductibles) and managed care mechanisms affect health outcomes for a previously uninsured and presumably low-income population? Are there situations or specific populations for whom direct care programs might be both more effective and more efficient?

Overall, then, the research suggests that the uninsured have a significantly higher relative risk of death than the privately insured, although there is greater uncertainty about the exact magnitude of the difference. Both the extra years of life and presumably more healthy years of life would add to individuals' and families' earnings. Depending on the measure of health used, improving a person's health to good or excellent from fair or poor, or reducing the prevalence of a particular health condition, could increase annual earnings by 15% to 20%.

NOTES

1. C-section delivery is generally indicated for breech presentation, which is considered an important risk factor for an adverse birth outcome.
2. Derived from calculations using Centers for Medicare and Medicaid Services (CMS) 2082 data on Medicaid enrollment in 1998. Personal communication from Brian Bruen, the Urban Institute, March 27, 2002.
3. Much of the information reported in this section is from an extensive critical review of the effects of health on work by Currie and Madrian (2000).
4. While mortality rates omit an important dimension of health, they do have the advantage of being unambiguously and consistently defined across studies. Subjective measures of health status, on the other hand, may be less comparable because of differences between the insured and uninsured in frames of reference. Do the uninsured compare themselves to other uninsured people or to the insured? Do they have the same implicit health scales, that is, do they define poor health in the same way? If uninsured people feel either defensive or angry about their lack of insurance, might they be prone to minimize or exaggerate their health status?

5. Blustein, Arons, and Shea (1995); Canto et al. (2000); Kreindel et al. (1997); Sada et al. (1998); Young and Cohen (1991); Ayanian et al. (1993); Roetzheim, Gonzales, et al. (2000); Roetzheim, Pal, et al. (2000); Doyle (2001); Haas and Goldman (1994); Kim et al. (2001); Schnitzler et al. (1998); Yergan et al. (1988).

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