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The Effects of Being Uninsured on Health Care Service Use: Estimates From the Survey of Income And Program Participation

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There is growing concern about the number of people in the United States who have no health insurance. For some, the concern stems from the lack of access to health care for the uninsured and its ultimate effect on their health status and productivity. For others the concern arises from a sense that the present system of financing what care the uninsured receive involves cross subsidies and taxation that are inequitable. The rising number of uninsured in this decade -- from about 30 million people in 1980 to 37 million people in 1987 -- coupled with the increasing pressures on providers from private and public sector cost containment are forces that have served to sharpen concern recently.1/

A number of policy responses to the problem of the uninsured are being considered by the Congress and are being tried in some states. Policymakers, when confronted with such sweeping proposals, seek information on a variety of effects, including those on government budgets, private spending, the people affected, and the economy. One important need is for information about the current use of health care services by the uninsured, both in absolute terms and relative to that of the insured. Relative use information -- often expressed as the percent by which use by the uninsured falls short of that by the insured -- serves two important needs:

- It helps policymakers understand the extent of deprivation of the uninsured, and

- It is a key parameter in estimating the incremental social cost of

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1. These estimates are based on tabulations of the Current Population Survey. Estimates of the absolute number of the uninsured vary from survey to survey, although there is no dispute about the upward trend during the decade.

expanding insurance -- that is, the total cost of expanded insurance overstates its incremental cost to society to the extent that the new insurance covers services that otherwise would have been provided and consumed anyway.

Several estimates of this access gap have been made, for various years and employing a variety of data sources and methods (see Table 1). Some estimates are based on lengthy periods of recall for use of health services, but are limited by their data to a measure of insurance status at the time of the interview. Other estimates are based on data that allow for recall of congruent periods of use and insurance coverage. The estimated gaps in access for the uninsured, taken as a percent of the use by the insured, range from 25 percent to 36 percent for physicians' services and from 19 percent to 67 percent for inpatient hospital services. In addition to the congruency (or lack thereof) in the use and insurance periods, the estimates may vary because they are based on different types of insurance as their point of reference, they may or may not control for other determinants of use that may be correlated with insurance status, and they apply to different population groups.

TWO PROBLEMS IN ESTIMATING RELATIVE USE

This paper is concerned with two specific problems that may affect estimates of the access gap for the uninsured:

- Does pooling people whose insurance status is in transition with people whose insurance status is stable lead to biased estimates of the "steady-state" behavior of the insured and uninsured? and

- Does employing measures of use and insurance that are for different time periods lead to biased estimates of the "steady-state" behavior of the insured and uninsured?

Transitory Insurance Status

The potential bias of pooling people with transitory insurance status with people who have stable insurance coverage is illustrated in Figure 1. The two horizontal lines depict the steady-state levels of use for the continuously insured and the continuously uninsured.

Consider first the possible behavior of someone in transition between the uninsured and the insured state -- that is, an "insurance gainer" (characterized by the dashed line in Figure 1). If the event leading to new coverage -- say new employment -- can be foreseen, then it makes sense to postpone some use while uninsured until the time when it will be at least partially paid by a third party. Under this scenario, prior to the acquisition of insurance we would observe lower use than that of the continuously uninsured. This tendency will be stronger the more the acquisition of insurance can be foreseen and the more the timing of health services use is discretionary. After gaining insurance, whether or not postponement took place, consumers seem likely to "catch up" on the use of services that were forgone when they were uninsured. This phase is represented by the use level temporarily rising above the rate for the continuously insured.

Alternatively, consider the case of transition from being insured to becoming uninsured -- that is, an "insurance loser" (see the dotted line in Figure 1). If the loss can be foreseen, we expect "stocking up" behavior on discretionary services since their price is lower when insured than when uninsured. After the loss of coverage there would be a corresponding sag in consumption, to a level below that of the continuously uninsured, reflecting the shift in timing of consumption to the previously insured period.

To summarize, the strength of these hypothesized effects of insurance transitions will be greater the more discretionary the services in question -- that is, the more the timing of their consumption can be shifted. The strength of the hypothesized postponement and stocking up effects also depends on foresight. If all insurance transition were unforseen, only catching up behavior would be observed. If the hypothesized behaviors prevail, they serve to raise the estimated use of the insured and to lower the estimated use of the uninsured. In other words, pooling people in transition with people in steady state will lead to overestimates of the access gap. This potential bias is difficult to correct with nearly all data sources, because they do not measure insurance for periods before or after their periods of use.

Lack of Congruency Between Use and Insurance Periods

Many surveys only ask about insurance coverage at the time of the interview, although they collect information on the use of health services for a period extending for up to a year prior to the interview. Some people who are currently insured have episodes of no insurance in the months prior to the interview. To

classify them as insured biases estimated use downwards. Likewise, some of the currently uninsured have episodes of insurance coverage in the months prior to the interview, thereby imparting an upward bias to the estimated use of the uninsured. For these reasons a lack of congruency between data on use and data on insurance status is a source of measurement error that biases the estimated access gap of the uninsured toward zero.

The net effect on the estimates of the two problems we have discussed is not clear, because they work in opposite directions. A lack of data on insurance coverage surrounding the period of use prevents correction for transition use, which tends to overstate the "true" access gap. On the other hand, a lack of data on insurance coverage during the entire period of use results in estimates that tend to understate the steady-state relationship. The next two sections provide evidence on the quantitative importance of these two estimation problems.

TESTS OF THE TRANSITORY USE HYPOTHESES

Methods

To test the hypotheses about temporary changes in the use of health care services resulting from transitions in health insurance coverage, we used data from the Survey of Income and Program Participation (SIPP). The core questions, which are asked every four months, provide a continuing source of health insurance information on participants in the survey (Nelson, et al, 1984). A special topical module on health and disability, administered in 1984, provided both 4-month and annual measures on the use of physicians' and inpatient hospital services, as well as information on the respondent's health status (U.S. Bureau of the Census, 1986).

These special health questions were only asked of people 15 years of age or older, however, so our estimates are restricted to adults. Because the elderly are nearly universally and continuously covered by Medicare, we excluded respondents age 65 or over.

Because of the timing of the topical module, we were able to observe either 8 months or 12 months of prior insurance coverage for continuing SIPP participants (depending on the date of their initial interview), although there was no constraint on observing insurance coverage following the special interview. In response to this limited information on prior insurance, we restricted our attention to the 4-month use measures. Insurance measures were created to characterize coverage during three time periods:

- the 4 months immediately prior to the period of reported use,
- the 4 months during the period of reported use, and
- the 4 months immediately following the period of reported use.

Some sample selections for particular patterns of insurance were made. The analysis sample was restricted to respondents who reported either private insurance or no insurance -- the effect of this restriction was to exclude those reporting Medicaid, Medicare, or another source of public insurance. Only cases reporting the same insurance status within a particular 4 month period were included.2/ This group included both people with stable coverage throughout the

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2. Most changes in insurance status, as reported by SIPP respondents, apply to the entire 4-month recall period. As a result, the sample restriction of continuous coverage within each 4-month period only leads to a small reduction in the sample. On the other hand, actual coverage within each

12 months and those who changed coverage from one 4 months interval to another.

Because the SIPP topical module only asked about use in a single 4-month period, we were not able to observe the relationship between a particular respondent's changing insurance status and changing use levels. Instead, our estimates are derived from observing different individuals at different stages of the insurance-gaining and insurance-losing cycles, as depicted in Figure 1, as well as individuals who were continuously insured throughout the 12-month period. Specifically, the sampled observations comprise the following six groups:

	Insurance Status		
Insurance Pattern	4 Months Before Use Period	4 Months During Use Period	4 Months Following Use Period
Continuously Insured	Insured	Insured	Insured
About to Lose	Insured	Insured	Uninsured
Just Gained	Uninsured	Insured	Insured
Continuously Uninsured	Uninsured	Uninsured	Uninsured
Just Lost	Insured	Uninsured	Uninsured
About to Gain	Uninsured	Uninsured	Insured

The estimation method used is conventional and will only be described briefly here (see Manning, et al, 1981). For each type of service, physician and inpatient hospital, one equation was estimated for a binary dependent variable indicating

period may not be as stable as it would appear from the reported data. As a result, our estimates suffer from some measurement error.

whether or not any service was used during the 4-month period.3/ Estimated with the probit technique, this multivariate equation controlled for a number of other determinants of use that might be correlated with insurance, in order to isolate the partial effect of insurance on use. The controls include measures of health status, age, sex, race, marital status, family size, income, education, employment status, and residence.

Findings

Table 2 reports the findings of our investigation of the transitory use hypotheses. For each of the six insurance patterns described above, the table shows a predicted use level, where all other characteristics are held at the sample means. None of the groups undergoing an insurance transition has use that differs significantly from its respective continuously insured or continuously uninsured counterpart. Notably, in every case but one the difference in predicted values is (but often trivially) in the opposite direction from that suggested by the corresponding hypothesis!

These findings are consistent with those for physicians' visits and hospital services in the RAND health insurance experiment. In that research, where the subjects had full knowledge of insurance transitions, the only service showing significant transitory use patterns was dental services (Manning, et al, 1987). In the nonexperimental world surveyed by the SIPP, where many insurance transitions are unexpected, it is perhaps reasonable to find no evidence of transitory changes in use.

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3. Total use can be divided into the probability of any use and the level of use by users. With a relatively short time period, most of the variation in use takes place in the probability stage. Among users, there is little variation in the amount of use, particularly for hospital services.

As a result of these findings (and despite strong personal experience to the contrary), we conclude that there are no significant biases introduced by pooling people whose insurance status is in transition with people whose insurance status is stable.

EFFECTS OF MEASURING INSURANCE CONGRUENTLY WITH USE

To investigate the extent of bias imparted by measuring insurance status for a period shorter than the use period, as contrasted with congruent measures, we simulated each approach using the same sample of SIPP respondents.

Methods

With three exceptions, the methods used for this part of the study were similar to those described above. First, having found no bias imparted from pooling cases with stable and transitory health insurance status, we were able to employ the 12-month use variable from the topical module, rather than the 4-month one. The advantages of an annual variable include greater variation to facilitate more precise estimates and comparability with the period of use most common in the literature. A disadvantage, however, is a small reduction in sample size, because one wave of SIPP respondents only had eight months of insurance data prior to the date of the special module.

Another difference between the present section and the previous one is that we present results from two equations rather than one. The first equation, for the probability of any use, is estimated in the same way. The second equation, estimated only for individuals who had some use of the particular service during the

year, employs ordinary least squares techniques for a dependent variable that is the natural logarithm of the level of use. This transformation reduces the skewness in the dependent variable, making it "more normal." (Manning, et al, 1981) The control variables for both equations are identical to those described above.

The final exception arises in the coding of the insurance variables. For the equations relating use for the past 12 months to current insurance status (biased by design), the comparison is for two groups:

- o currently insured, and
- o currently uninsured.

Using the same sample to estimate equations relating use for the past 12 months to insurance for the past 12 months (the unbiased case), the comparison uses three groups:

- o continuously insured for 12 months,
- o continuously uninsured for 12 months, and
- o a mixture of insured and uninsured months.

As in the earlier investigation, the sample was restricted to nonelderly adults having private insurance or being uninsured.4/ The health services studied are physicians' and inpatient hospital care.

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4. This restriction literally applies to insurance coverage in the current month. In order to replicate the case of measuring insurance with error, individuals with public insurance at any time during the rest of the year were included in the sample.

Findings

Table 3 reports the findings from our simulation using current insurance measures and continuous insurance measures. In addition, estimates based on the 4-month use and 4-month insurance measures from the previous section are reported. The entries in the table show the percent by which the use of the uninsured falls below that of the insured, and they are analogous to the entries in Table 1.

In the first column the shortfall in having any use is displayed, along with the corresponding t-statistics. For example, in the current insurance equation for physicians' services, the predicted likelihood of annual contact for the uninsured fell 18 percent below that for the insured. The second column shows the shortfall in the amount of use by those individuals having one or more contacts during the year. Again, for the current insurance measure the shortfall in physicians' service use was 12 percent. After accounting for the interaction of the separate elements, total use by the currently uninsured is estimated to be 28 percent lower for physicians' services.

Comparing the current insurance to the continuous insurance estimates, the effect of the measurement error is clear. The estimated access gap for physicians' services is about one-third larger using the continuous insurance measure compared with the current insurance measure (37 percent instead of 28 percent), and for hospital services it is over half again as large (69 percent instead of 44 percent). The 4-month and 12-month estimates from SIPP are quite similar. Of course, this is in part a reflection of the substantial overlap of the samples and of the recall periods.

One final comparison allows a check on the validity of our simulation. We made similar estimates of the access gap using the 1986 Health Interview Survey (HIS) for annual use and current insurance, which are reported in Table 1. For physicians' services the HIS estimate is 25 percent, compared to 28 percent from SIPP. The corresponding figures for hospital services are 37 percent for HIS and 44 percent for SIPP respectively.

The magnitude of the access gap for hospital services is suspiciously large compared with that for physician services. A possible selectivity bias may be at work. In those states whose Medicaid program covers the "medically needy," uninsured individuals with large medical bills may be eligible for Medicaid while those with moderate expenses may not be eligible. Thus, this public program may selectively remove many of the individuals with the highest medical expenses from the uninsured group. Since high medical expenses are usually associated with hospital care, this bias would affect the access gap for hospital services more than the one for physician services. Until we investigate this possible source of bias, the size of the hospital gap should be interpreted with care.

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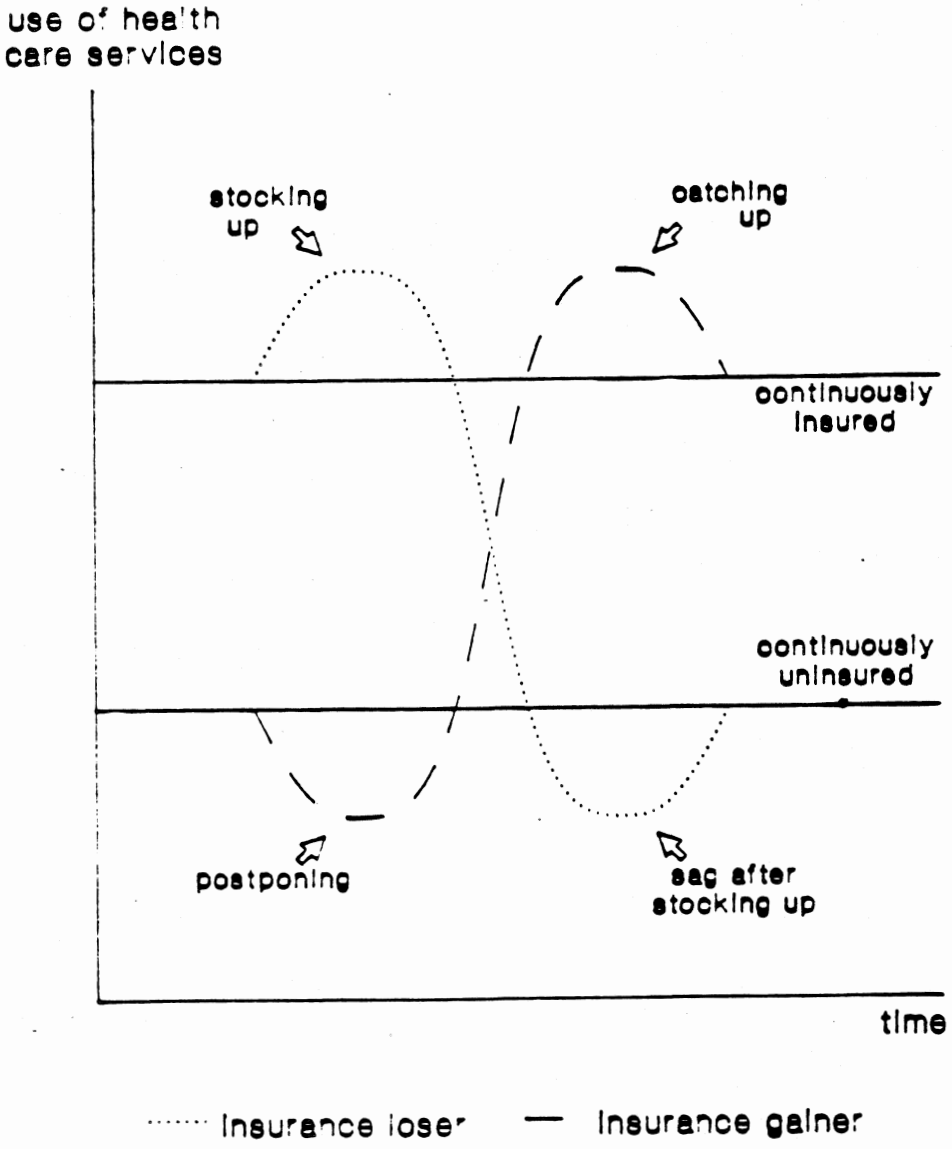
Figure 1. Hypothesized Patterns of Transitory Change in Health Services Use for Insurance Gainers and Losers



SOURCE: Authors' calculations based on hypothetical cases.

TABLE 1. ESTIMATED USE OF HEALTH SERVICES BY THE UNINSURED RELATIVE TO USE BY THE INSURED; VARIOUS SOURCES, METHODS, AND YEARS

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Source	Data	Relative Use (Measure of Use)		Types of Insurance	Controls for Other Determinants of Use	Population
		Physicians' Services	Inpatient Hospital Services			
Use in Last 12 Months by Current Insurance Status						
Freeman, Blendon, Aiken, et al (1987)	1986 Robert Wood Johnson Access Survey	-27% (Total visits)	-19% (Any admissions)	Private and Public	No	Adults and Children
Congressional Research Service (1988)	1986 Health Interview Survey	-36% (Total visits)	-24% (Total hospital days)	Private and Public	No	Nonelderly Adults and Children
Authors' Calculations	1986 Health Interview Survey	-25% (Total visits)	-37% (Total hospital days)	Private	Yes	Nonelderly Adults
Use in Last 12 Months by Insurance Status in Last 12 Months						
Davis and Rowland (1983)	1977 National Medical Care Expenditure Survey	-35% (Total visits)	-48% (Total hospital days)	Private and Public	No	Nonelderly Adults and Children
Use in Current Month by Current Insurance Status						
Long and Settle (1985)	1980 National Medical Care Utilization and Expenditure Survey	-32% (Any visits)	-67% (Any admissions)	Private	Yes	Low-income Adults and Children

Table 1. Estimated Use of Health Services by the Uninsured Relative to Use By the Insured; Various Sources, Methods, and Years

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