INSURANCE BENEFITS, OUT-OF-POCKET PAYMENTS, AND THE DEMAND FOR MEDICAL CARE: A REVIEW OF THE LITERATURE

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Abstract

Numerous studies are reviewed that relate the demand for medical care services to variation in out-of-pocket payments. Medical care services include physician, hospital, dentist, and drugs. For all these services demand increases as out-of-pocket payments fall, but the exact magnitude of the response is somewhat uncertain. Although some believe that eliminating out-of-pocket payments for ambulatory services decreases hospitalization and decreases overall costs, the preponderance of evidence suggests the contrary. Evidence from the Medicare and Medicaid programs and Canada supports the hypothesis that demand responds to variation in out-of-pocket payments.

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The Congress is currently considering national health insurance plans with varying levels of out-of-pocket payments. Any health insurance plan affects out-of-pocket payments in two ways: (1) a given service can be included or excluded from coverage; inclusion clearly lowers the out-of-pocket price; and (2) included services can be subsidized to varying degrees.

The time seems propitious to review the literature on how out-of-pocket payments (or their corollary, insurance benefits) affect the demand for personal medical care services. By out-of-pocket payments I mean such phenomena as coinsurance, copayments, and deductibles;[1] by demand for medical care I mean how much medical care a patient seeks and a physician (or other provider) gives a patient as a function of that patient's insurance coverage.

Despite the apparent resemblance, demand and utilization are not synonyms. If the insurance of others did not change, one may assume that a change in demand (brought about by a change in the individual's insurance) would cause an equivalent change in utilization. If a national health insurance plan were adopted, however, the insurance of many people could change at the same time. In that case the supply system's capacity might not suffice to meet all the demand, and so not all demand would be realized as utilization. Little is known about the nature of any unsatisfied demand; consequently, I do not address that issue, but seek to establish only how much demand responds to alternative levels of out-of-pocket payments.

One school of thought objects to out-of-pocket payments, arguing that they attempt to influence the patient, while the true decisionmaker is the physician. I do not attempt to evaluate to what degree the "true" decisionmaker is the physician and to what degree it is the patient. Rather, I ask about how out-of-pocket payments affect demand, whoever the decisionmaker may be. As will become clear, however, a wide variety of data reject the overly simple view that the physician not only makes all decisions but also, in so doing, ignores the patient's insurance coverage.

If out-of-pocket payments affect demand, one may logically ask whether they impair health status. In the words of some, is the additional care demanded when the price of medical care falls (or the care not demanded when it rises) necessary or unnecessary? Because the lack of an agreed upon definition for necessary and unnecessary precludes an answer to the question when posed in this manner, I will have little to say about this issue, although I will return to it at the end of the paper.

Because of space limitations, I have exercised some selectivity in choosing what to review. In particular, I do not review a great deal of early literature that ignored variation among insurance policies and simply measured differences in utilization between insured and uninsured populations. Eichhorn and Aday (1972) and Donabedian (1976) have reviewed this literature. Others reviewing the literature on the relationship between demand and out-of-pocket payments include: M. Feldstein (1974), P. Feldstein (1966), Frech (1976), Ginsburg and Manheim (1973),

Hall (1974), Joseph (1971), Kimbell and Yett (n.d.), Newhouse, Phelps, and Schwartz (1974), Pauly (1974), and Phelps and Newhouse (1974a).

Variation in coinsurance on copayment rates is simpler to understand and has been much more widely analyzed than variation in a deductible. Hence, most of this review focuses on coinsurance and copayment. Analytically, both a coinsurance rate and a copayment rate reflect a fixed subsidy to unit price. For example, if the physician's charge for an office visit is $10, a coinsurance rate or copayment rate lowers this to some other value (e.g., a 25 percent coinsurance rate lowers the price to $2.50, in effect a subsidy of $7.50). In addition, the premium or taxes paid to finance the insurance lowers the consumer's income, and so decreases the purchase of all goods and services. This reduction in income affects the demand for medical care negligibly, however, and will be ignored in what follows (Phelps & Newhouse, 1974a).

I first review how out-of-pocket payments affect the demand for physician, hospital, and dental services and drugs. Then I review studies of their effect on total medical expenditure, and experience in Medicare, Medicaid, and Canada. I conclude with some thoughts about future progress.

<u>Out-of-Pocket Payments and the Demand for Physician Services</u>

One cannot give a precise answer to the question: How much does reducing the unit price of physician service stimulate demand? That is, what is the responsiveness of demand to price? Piecing data together from a variety of sources, however, one might estimate that a fully-insured consumer would demand about twice as many services as a con-

sumer with no insurance at all, other things equal (or, alternatively, a physician would demand that many more services on behalf of the patient). For example, the uninsured consumer might visit the physician three times per year, while the fully-insured consumer visits six times per year. The factor of two is at best a crude estimate, and is presented to give the reader some feel for the likely magnitudes involved before pursuing the details of various studies.

Perhaps the cleanest study in the literature was done by Scitovsky and Snyder (1972), with a follow-up by them five years later (Scitovsky & McCall, 1977). (Phelps & Newhouse [1972] also analyzed their earlier data and reached similar conclusions.) Scitovsky and Snyder studied utilization of physician services by 2,567 Stanford University employees in 1966 and in 1968. In 1966 physician services were free to the employees, but the insurance plan was running a deficit. Rather than raise premiums, the managers of the plan elected to impose a 25 percent coinsurance rate (roughly $3 per visit). Scitovsky and Snyder present data on both physician visits and use of ancillary services in 1966 (prior to the imposition of coinsurance) and in 1968 (after coinsurance). The office visit rate in 1966 was 33 percent above the rate in 1968, and use of ancillary services was 15 percent higher. Subsequently, data were collected on utilization by the same individuals in 1972 (the coinsurance continued in effect); the 1972 rates were almost identical to the 1968 rates. Thus, one infers that a fall in coinsurance from 25 percent to zero would raise demand for visits 33 percent, and for ancillary services about half that.

After the imposition of coinsurance, demand changed rather uniformly among various groups of individuals. Only female dependents showed a significantly greater-than-average responsiveness to the price change. No statistically significant differences were observed between faculty members and support personnel in the (absolute) amount of the change, although the faculty had greater income, and thus might have been expected to show less responsiveness.

The strengths of these data are several:

1. The change in price (zero to 25 percent coinsurance) is well defined; as will be seen, it is generally more difficult in other studies to specify the magnitude of the coinsurance rate change.

2. The same group of individuals was studied before and after the change. In many other studies the researcher compares individuals with different insurance plans and attempts, typically through analysis-of-covariance techniques, to control for other differences among the individuals. But individuals who have better insurance may systematically differ in ways the researcher fails to control. If the unmeasured differences affect demand, biased estimates of demand's responsiveness to out-of-pocket payments will result. Such bias is discussed more fully below.

3. The Stanford employees made up a relatively small portion of the patient population at the Palo Alto Clinic, where they received care. Thus, one can interpret these results as a change in demand; they should not reflect the influence of supply.

Offsetting these strengths are certain problems:

1. The results attribute the entire difference in utilization between the two years to the change in insurance. Although there is no other obvious explanation (e.g., no known epidemic in 1966), circumstances may have been different in the two years. The stability of utilization rates between 1968 and 1972 is reassuring (suggesting 1968 was not an abnormally low year), but some unidentified factor may have nonetheless artificially increased the 1966 utilization rates.

2. The data cover only the range of 25 percent to zero coinsurance, and apply only to physician and ancillary services.

3. The data come from one local area and one employee group and may not be representative.

For these reasons one wishes for some corroboration. A second study (Straight, 1962) examined the effect of copayment for office visits using data from Saskatchewan. In 1946 the Swift Current district of Saskatchewan instituted a prepaid medical plan providing full coverage for all enrollees. Approximately 90 percent of the district's population enrolled. Physicians were paid on a fee-for-service basis. In August 1953, because of high utilization, a $1 copayment charge was introduced. Calculating an equivalent coinsurance rate is difficult, but the plan paid physicians approximately $2.40 per visit (for the details of this calculation, see Phelps & Newhouse, 1974a), and so $1/2.40 or 42 percent appears to approximate the coinsurance rate.

Office visits per person fell 17 percent between 1952 and 1954. The 17 percent value is a lower bound on the change in demand, be-

cause virtually everyone's coverage changed. As a result, queues may have fallen (and physicians may have induced more demand). This would cause demand to increase, offsetting some of the fall due to the copayment. Seemingly, this study shows demand to respond less to out-of-pocket payments than the Palo Alto study showed (because a larger change in coinsurance caused a smaller change in utilization), but adjusting for other changes that may have occurred (e.g., shorter queues) would make the two estimates more comparable. In other words, the change in utilization in Saskatchewan did not necessarily equal the change in demand.

Household surveys are another source of data describing the utilization of physician services. Newhouse and Phelps (1976) analyzed the data from a 1963 survey that used a national probability sample; the Center for Health Administration Studies (CHAS) of the University of Chicago conducted it. Newhouse and Phelps studied variation in physician visits among individuals as a function of the estimated coinsurance rate, adjusting for their age, sex, race, income, education, family size, self-reported health status, and physician/population and hospital bed/population ratios in their area of residence.

The demand for physician visits was estimated to be roughly one-quarter higher with no out-of-pocket payments than at full price (no insurance).[2] Income groups differ slightly (but statistically significantly) in their response to variation in the coinsurance rate. Lower-income groups showed marginally greater responsiveness.

The 24 percent difference between no coverage and full coverage is much less of a difference than one would expect using the two previous studies (which dealt with changes in the 25 percent to zero and 42 percent to zero ranges of coinsurance). It is probably too low because of errors in measuring the insurance variable.

Random errors in measuring the coinsurance rate will bias the estimated responsiveness of demand toward zero; such errors are likely when using survey data. Any analysis of demand using data from household surveys must represent parsimoniously the terms of the insurance policy. The analyst typically specifies the terms as an average coinsurance rate, that is, the fraction of the bill paid by the policy.

Unfortunately, the complexities of actual policies make such a specification error prone. For example, some policies may require a copayment; to convert a copayment into a coinsurance rate requires the charge for a service, but different physicians may charge different rates. For example, a $5 copayment for a $10 visit charge would equal a 50 percent coinsurance rate, but if the same patient went to a physician charging $20, the coinsurance rate would fall to 25 percent. If the patient visits physicians who charge different prices, the implied coinsurance rate will differ by physician. Simply averaging these rates--dividing total out-of-pocket expenditure by gross expenditure--could introduce error.[3] A somewhat similar complication occurs if coinsurance rates differ across services or if the policy reimburse according to a fee schedule, leaving the patient responsible for any charge above the schedule.

More complicated still is a deductible where additional visits cost less than the average visit or its analytical opposite, a limit on the number of visits the policy reimburses. Because the price of an additional visit (the marginal price) is usually the relevant price for decisions, use of an average price will introduce an error. If the patient exceeds a deductible, the average price is too high, and if the patient uses more than an upper limit, the average price is too low. Even more vexing, one cannot observe the theoretically correct price below the deductible (or below the limit) for reasons explained below (Keeler, Newhouse, & Phelps, 1977). For all these reasons measurement of the price variable when using probability sample survey data will probably contain considerable measurement error, and so the responsiveness of demand will be understated. Such a phenomenon may well have occurred in the Newhouse and Phelps (1976) study.

A bias in the opposite direction occurs if sicker individuals have better insurance. For many individuals, perhaps, the type or amount of insurance does not depend upon their health status. For example, an employer may offer only one health insurance plan and subsidize it sufficiently (by paying a portion or all of the premium) that virtually all employees purchase the insurance.[4]

But some individuals can exert choice over the amount of insurance they have. Certain employees (e.g., federal employees) have "high" and "low" options, and those who consume more medical care disproportionately elect the high-option plan.[5] In some cases the employer does not subsidize the premium (e.g., dependent coverage may be optional), in which case those who are more likely to use medical services are more likely

to buy the insurance. An analogous situation obtains for the part of the population that does not have access to group insurance. Finally, there may be some selection of workplace on the basis of health insurance plan. For example, it will pay individuals who want long-term psychotherapy to find a group health insurance plan that covers that service. For all these reasons one would not expect a random distribution of health insurance; for some empirical confirmation, see Phelps (1973).

To see the effect of a nonrandom distribution of insurance, imagine a plot of individuals' utilization as a function of their coinsurance rates. The resulting estimates of responsiveness would show greater responsiveness than would truly exist. In particular, the less well insured would not consume as much as the well insured, if they had better insurance. A certain econometric technique, simultaneous equation methods, can address this problem and produce a consistent estimate of responsiveness (an estimate that converges toward the true responsiveness as the number of observations increases). This technique requires identification of one or more variables that do not affect the demand for medical care but do affect the demand for health insurance.

Success at identifying such variables has come slowly. The best known variable is work group size, because the fee the insurance company charges falls as the size of the group increases. Such a reduction should not (directly) affect the demand for medical care, but will increase the demand for insurance (Phelps, 1973). Unfortunately, using work group size in this manner produces ambiguous results; estimated changes in demand as coinsurance varies do not differ significantly from zero. Although this

could occur because the true responsiveness were zero, it more likely reflects a lack of statistical power to detect a true nonzero response.

A more recent CHAS survey (1970) has been analyzed by Phelps (1975). His estimates of demand responsiveness using these data are considerably higher than the estimates using the 1963 survey data, and in fact are very close to Scitovsky and Snyder's estimates.

Unfortunately, two problems afflict Phelps' estimates. First, the 1970 sample is mostly uninsured (the mean of coinsurance rates in the sample is 98 percent). Thus, a few insured individuals could dominate his results. Second, Phelps includes individuals with deductibles in their policies in his analysis. This causes an overestimate of responsiveness, because a low price is attributed to individuals who exceed their deductibles (if they bought one more visit, their insurance would pay), whereas the full price is attributed to those who did not exceed their deductible. Thus, even if the true responsiveness were zero, individuals who use more will be attributed a lower price, and so the estimated responsiveness will be nonzero.

Newhouse, Phelps, and Schwartz (1974) also used data from the 1970 CHAS survey to quantify demand response in the 100 percent to 20 percent coinsurance range. They compared utilization by insured and uninsured groups under 65 and presented data to show that coinsurance among insured groups averaged around 20 percent. Comparison of utilization between the two groups should show how demand differs between 100 and 20 percent coinsurance. The insured group used 66 percent more services than the uninsured group; together with the Scitovsky-Snyder estimates, their

results suggest that demand for outpatient physician services more than doubles as the coinsurance rate drops from 100 percent (no insurance) to zero (full insurance).

Some studies of the demand for physician services have used data aggregated across individuals (Davis & Russell, 1972; Fuchs & Kramer, 1973), but these analyses raise additional methodological uncertainties. These studies compute a mean coinsurance rate for a state or region, and then compare the mean utilization of various areas with their mean coinsurance rates. All the ambiguities described above in computing coinsurance rates are present in these studies. In addition, the authors must make rather strong assumptions to use their equations to predict how demand would change if national health insurance were enacted. They must either assume that everyone's insurance will simply increase proportionately or that everyone's response to insurance is similar. (Otherwise, those who are more responsive may have their insurance changed more or less than the average.) Obviously, some individuals' insurance will change more than others' if national health insurance is enacted. Moreover, individuals' responses to insurance changes probably differ. Hence, the predictions derived from these studies may miss rather badly. Other problems with these studies are discussed in Newhouse and Phelps (1974).

Brian and Gibbens (1974) reported that requiring certain Medicaid beneficiaries to pay $1 per visit decreased utilization. Unfortunately, lack of comparability between the copayment and no-copayment groups may account for a substantial portion of the variation they observed, because only the wealthier Medi-Cal beneficiaries were required to copay.

No one has systematically studied how demand for inpatient physician services varies as a function of coinsurance. One would anticipate less responsiveness than for outpatient services, but such a conjecture cannot be supported at this time.

Out-of-Pocket Payments and the Demand for Hospital Services

Insurance benefits typically cover most hospital expenditure. As a result, one observes less natural variation in insurance for hospital services than for physician services. Moreover, the studies in the literature pose a challenge in estimating the variation in price.

One study (Heaney & Riedel, 1970) gathered data before and after a change in Connecticut Blue Cross insurance plans. Prior to the change, Blue Cross paid $15 per day toward room and board charges. Ancillary services were fully covered. Beginning in 1966 Blue Cross offered groups that it insured an option of adopting a semiprivate plan that paid the room and board charge in full, which about half the groups elected.

In comparing data from the six months before and after the change,[6] Heaney and Riedel reported that the admission rate per 100 insurers rose 12 percent, the average length of stay 13 percent, and the rate of patient days per 100 insurers 26 percent.

Heaney and Riedel also presented utilization data on groups that did not change coverage. Although they referred to these groups as controls, such a designation is inaccurate because the "controls" were self-selected (i.e., they represent groups that chose not to change). Additionally, the groups that changed coverage did so at various times between September 1966 to June 1968 period; whereas the control group data compared

utilization in the first and last six months of 1967, not the same period.

Nonetheless, the control group data do rule out the possibility that admissions and length of stay in the general population greatly increased during this period. Such an increase, if it existed, could obviously account for the 25 percent increase among the groups that opted for the more generous coverage, but admissions decreased 5 percent and length of stay 11 percent among the "controls." If anything, therefore, the control data suggest that the 26 percent figure may be too low an estimate of the effect of the insurance change.[7]

The difficulty in interpreting these data comes from translating the insurance policies into appropriate coinsurance rates. For the full coverage plan, such translation is straightforward--the coinsurance rate is zero. For the plan that paid $15 per day, an estimate of the equivalent coinsurance rate may be derived as follows. Average revenue per patient day at Connecticut hospitals in 1967 was $65.89 (Hospitals, 1968, p. 442), and room and board charges were on average 53.3 percent of total hospital charges (Health Insurance Association of America, 1968, Table 3). The average room and board charge was therefore $35.20 (.535 x 65.89), and a $15 payment by the insurance company would leave the patient responsible for $20.20 per day. $20.20 is 31 percent of the total charge of $65.89; hence, 31 percent is an estimate of the average coinsurance rate faced by those contemplating a hospital admission.

Given an admission, a decision on length of stay remains. Here the calculation of coinsurance changes, because ancillary services are

concentrated in the first few days of the admission. At the extreme, no ancillary services occur on the last day; then the proportion of that day's charges that the patient must pay rises to $7 percent ($20.20/$35.20). The true coinsurance rate would be less to the extent of the last day's ancillary charges. On the basis of these data, one can infer that changing the coinsurance rate from 31 percent to zero increased hospital admissions by 12 percent and a change in the coinsurance rate from 57 percent (or somewhat less) to zero increased length of stay by 13 percent.

Williams (1966) also demonstrated responsiveness of patient days to variation in copayment, using data from one Blue Cross plan that offered hospital coverages with $4 per day copayment and no copayment.[8] Patient days were 16 percent higher in the free plan. The $4 copayment approximated a 12-percent coinsurance rate. Thus, a change in the coinsurance rate from 12 percent to zero caused a 16 percent rise in patient days.[9] Virtually all of this effect occurred from a differential admission rate; length of stay was essentially the same between the two plans.

Medical resources per subscriber, however, only rose by 6 percent in the free plan relative to the copayment plan, because the intensity per case was somewhat higher in the copayment plan. But one may question whether this greater intensity per case in the copay plan would have been found in a plan with 12 percent coinsurance (as opposed to a $4 copayment). Any increased intensity in the copay plan that served to reduce the stay would unambiguously benefit the patient financially, whereas this would not be the case in a 12 percent coinsurance plan (because the patient would have to pay 12 percent of any increased intensity).

A third study also supports the notion that hospital utilization responds to out-of-pocket payments. Freiberg and Scutchfield (1976) obtained 1972 data from 13 Kentucky Blue Cross plans that varied the cost of hospital care to the patient from $2.53 per day to $44.18 per day (average within-plan cost). They then regressed the admission rate per thousand contracts for each plan on the cost per day; they also regressed the average length of stay in each plan on both the cost per day and the admission rate. Unfortunately, they did not convert the cost per day figure to a coinsurance rate; I have estimated the $44.18 per day figure to be around a 50 percent coinsurance rate.[10]

Freiberg and Scutchfield's results concur with Williams' that the out-of-pocket payments principally affect admissions. According to their estimated equations (and assuming $44.18 corresponds to a 50 percent coinsurance rate), a decline in the coinsurance rate from 50 percent to zero would raise admissions 53 percent. Length of stay would hardly be affected at all, allowing for the increase in admissions caused by the insurance change.[11]

Although the three studies differ somewhat in how much out-of-pocket payments affect admissions relative to length of stay, they all estimate a roughly similar response of patient days to variation in out-of-pocket payments. One might argue that individuals who planned to be hospitalized more would self-select plans with lower coinsurance, so that all these studies overestimate the responsiveness to coinsurance. But these studies all used data from <u>groups</u>, suggesting that any self-selection, and hence any overestimate, was probably small.

Other researchers have reported results that I consider less reliable. Rosenthal (1970) collected data on 15,685 hospital admissions and analyzed the responsiveness of length of stay to price, which he defined as the proportion of the bill paid out-of-pocket.[12] He grouped admissions into 28 disease categories, so his results could potentially tell us whether certain disease classes respond more to out-of-pocket payment than others. In fact, Rosenthal found little responsiveness among any disease class; the largest estimated responsiveness indicated that a 10 percent increase in out-of-pocket price (for example, from 20 to 22 percent coinsurance) would decrease length of stay only 0.8 percent.

Unfortunately, Rosenthal's price variable contains two types of errors that are likely to bias the observed responsiveness toward zero. The first error was defining price as the average coinsurance rate. As discussed above, the price relevant to length of stay decisions is the price of an additional day in the hospital (the marginal price). If ancillary procedures are concentrated in the first few days of admission, as is likely, the average price of a longer stay will be closer to the marginal price than that of a shorter stay, and this systematic error in measuring price will result in a bias toward zero in the estimated coefficient (Newhouse & Phelps, 1974).

The second source of error arose because Rosenthal's data came from hospital records. If the patient had commercial insurance that reimbursed the patient (i.e., benefits were not assigned to the hospital), the hospital would not know about the patient's insurance benefits, and the average coinsurance rate would be measured with error

for that patient. Such error would be random, and random error in measuring the explanatory variable causes an underestimate of the responsiveness.

Joseph (1972) purports to estimate the responsiveness of length of stay to out-of-pocket payments by disease category, but in fact his estimates represent differences in length of stay between those with and without insurance. Although age, sex, and type of hospital accommodation were controlled for, other differences between uninsured and insured populations were not. Not surprisingly, for 7 of the 22 diagnoses the mean length of stay among the uninsured exceeded that of the insured.

Hardwick, Shuman, and Barnoon (1972) come to a quite different conclusion--that out-of-pocket payments do not matter. They compared utilization in 1969 among one group that had a \$5 per day copayment provision with utilization among five groups that had full service benefits. All groups were insured with Blue Cross of Western Pennsylvania. I estimate the \$5 copayment was roughly 8 percent of average daily patient revenue in Pennsylvania in 1969,[13] so the data could potentially show the effect of a change in coinsurance from 8 percent to zero. The authors found that those in the copayment group had a higher admission rate and longer length of stay than those with no copayment, although differences were generally not statistically significant.

All other factors, however, were not similar between the copay and no-copay groups. In 23 of 25 age/sex classes, the cost per hospital day for the full service group exceeded that in the \$5 per day copay group. (Half of these differences were statistically significant.)

Theory suggests that costs should not differ between the two groups, for if the copay group member went to a more expensive hospital, the additional costs are fully covered, just as in the full service plan.

The authors ascribed the observed discrepancy between the copay and free groups to differences in their location; the copay group resided disproportionately outside the Pittsburgh metropolitan area and so used lower-cost hospitals. But their comment suggests that the authors failed to control for important differences between the two groups. For example, residents living outside metropolitan areas in the Northeast had a 23 percent higher rate of discharge from hospitals than residents of metropolitan areas (National Center for Health Statistics, 1974, Table 11), a difference quite consistent with higher utilization among the copay group. Furthermore, the change in coinsurance (8 percent to zero) was so small that one would not expect much true difference in utilization between the two groups. Based on the differences found in the Heaney-Riedel, Williams, and Freiberg-Scutchfield studies, the true difference in demand should be 5 to 10 percent or even less. Unmeasured demographic differences between the two populations studied could easily overwhelm such a small difference. On balance, Hardwick et al.'s (1972) data are not well suited to estimating the effect of copayment.

M. Feldstein (1971, 1977) reached the conclusion that the effect of out-of-pocket payments on hospital use was quite large, although he presented his numerical results in a form that hampers comparison with the estimates presented above. For each of several years, Feldstein estimated the average coinsurance rate for various states by computing

the ratio of out-of-pocket expenditure to total expenditure. He then used this coinsurance rate to explain the average length of stay and admission rate in the state. Use of state averages to predict changes in demand rather than person-specific data requires additional assumptions to generate valid predictions, as described in the previous section. Because these assumptions are not likely to be satisfied if a general change in insurance occurs, Feldstein's estimates may be unreliable. Other methodological problems in his studies were noted by Newhouse and Phelps (1974).

Insurance for Outpatient Services and The Demand for Inpatient Care

Because much existing insurance pays for procedures only if done in the hospital, some observers conclude that insuring outpatient services would rationalize the use of resources.

Despite its importance, few have attempted to test this hypothesis. Kansas Blue Cross conducted the cleanest test in 1968 (Hill & Veney, 1970; Lewis & Keairnes, 1970). Approximately 5,000 covered persons and their families, who had no prior coverage for office visits, were given such coverage for eight months. The researchers also selected two different control groups of 5,000 covered persons and their families (although they treated them as one group of 10,000). Hospital utilization during the eight-month experimental period was compared to utilization during the corresponding eight-month period in 1967 for both experimentals and controls. At issue was whether admissions, length of stay, or benefit payments would decline among the experimentals relative to the controls.

No statistically significant decrease in hospital utilization among the experimentals relative to the controls occurred. For one subgroup

of the experimentals, hospitalization even increased. Hill and Veney speculated that outpatient services did substitute for some inpatient services, but that the reduced price of outpatient services induced additional visits, during which physicians discovered pathology that led to hospitalization. (The studies do not present data on how much outpatient visit rates changed.) Thus, "demand creation" fought "demand reduction" to a draw. That short-stay (less than 20 days) medical admissions declined among the experimental group, whereas longer stays did not supports this interpretation, because procedures that could be done on an outpatient basis would almost always result in short-stay admissions. By contrast with medical admissions, short-stay surgical admissions did not show a differential decline, and so any explanations of why hospitalization failed to drop overall remained highly tentative.

One could put a particularly favorable interpretation on the Kansas results, arguing that the cases where pathology was discovered should have been hospitalized (i.e., a medical need existed), whereas some savings were achieved in cases where outpatient care substituted for an inpatient episode. In other words, adding coverage of outpatient services did serve to rationalize the use of the medical care delivery system. Additionally, demand for hospital services might fall in the long run, if there were catch-up demand in the initial eight-month period. (That is, over time the stock of individuals whose health problems required hospitalization would be reduced, leaving only the new flow to be treated.) In this case potential savings are masked.

Unfortunately, this interpretation, while not refutable from existing data, really assumes what it wishes to prove. Nothing definite is known about how the mix of hospitalized cases changed, so no firm conclusion can be drawn as to whether hospital use was more rational. Similarly, we have no evidence that the rate of hospitalization would in fact fall in the long run with free outpatient care. All we know with relative certainty is that no striking evidence of overall cost savings from free outpatient care appeared in this experiment.

Roemer, Hopkins, Carr, and Gartside (1975) obtained a result that seemed to differ from the Kansas results. They studied the effect of California Medicaid enrollees' making a $1 copayment for an office visit. They concluded that the copayment had indeed reduced ambulatory visits but had raised hospitalization sufficiently that costs to the state <u>rose</u> almost $1 per eligible per year. Roemer et al. used graphical methods and provided no statistical tests of significance. Those deficiencies in their methods promptly led to questions regarding their conclusions (Dyckman, 1976; Chen, 1976, Dyckman & McMenamin, 1976).

A subsequent reanalysis of Roemer et al.'s data by Helms, Newhouse, and Phelps (1978) generally supported Roemer's conclusions; in particular, they found that the copayment decreased ambulatory visits and increased hospitalization (both effects significant at 5 percent), but the estimated cost increase did not differ significantly from zero at conventional levels.

As Roemer et al. pointed out originally, however, these data are not well suited to answer the question addressed. In particular, indi-

viduals required to copay were the relatively wealthier Medicaid eligibles; the "controls" (who were not required to copay) were the poorer Medicaid eligibles. As a result, the free and copay groups differed substantially. The free care group was younger (average age 15.8 years versus 18.2 years) and used the hospital markedly more than the copay group, both before and after instituting the copayment (around a 60 percent greater rate).

To analyze these data, one must make an assumption about how unmeasured variables affected the two groups during the eighteen-month data collection period (six months before the copayment and twelve months after). The simplest assumption, which Helms, Newhouse, and Phelps made, is that omitted variables affected the groups equally, but with groups as different as those here, such an assumption raises serious questions. For example, if a disease requiring hospitalization differentially affected the young in the six months before copayment, the data would show copayment increased hospitalization, even if it had not. Moreover, any individual whose income fell because of hospitalization in the six months before copayment would have had a greater likelihood of assignment to the no-copay group. For this reason also the data could show that copayment increased hospitalization, even if it had not. Thus, the discrepancy between these results and those observed in the Kansas experiment could be a statistical artifact. Additionally, these data came from a welfare population, whereas the Kansas data did not; among a welfare population the effects of copayment could differ.

Freiberg and Scutchfield (1976) also shed light on this question. They studied insurance plans that covered outpatient services equally

(the authors do not say how well such services are covered), but the price of inpatient services varied among the plans. The authors examined whether utilization of inpatient services relative to outpatient services increased when inpatient services were relatively less costly (and inferentially whether making outpatient services cheaper might decrease the relative use of inpatient services). They found no evidence of substitution of outpatient care as its price fell; in fact, inpatient usage rose as the relative price of outpatient services fell, but the rise was not statistically significant.

Thus, the evidence is not compelling that lowering the price of outpatient care reduces the demand for inpatient care, although such an effect may be at work in a welfare population. Indeed, lowering the price of outpatient care could well raise inpatient demand.

Other studies of this question used less satisfactory methods, although they also came to the conclusion that lowering the price of outpatient care will not reduce inpatient demand. Steep and Tilley (1965) compared hospitalization rates among groups with and without outpatient coverage, but their study suffered from lack of comparability between the groups. A similar problem occurred in Kelley (1965), who examined the effect of adding benefits for ancillary services on an outpatient basis. Davis and Russell (1972) found that hospital admissions were higher where hospital outpatient prices were higher (contrary to the above conclusion), but also higher where private physician fees were lower (supporting the above conclusion). Their estimates were based on state averages, however, and for reasons explained above probably not too reliable.

Out-of-Pocket Payments and the Demand for Dental Services

A number of studies on the demand for dental services show a substantial demand response to insurance benefits. Phelps and Newhouse (1974a) collected data on dental insurance premiums for two policies, one with zero coinsurance for dental services and one with 20 percent coinsurance. Benefits were limited to examination, prophylaxis, and X-rays not more frequently than every six months. If demand did not respond to the coinsurance change, the premium for the no-coinsurance policy should be 25 percent higher than the premium for the policy with 20 percent coinsurance. (In one case the insurer would pay 80 percent of a given total, in the other 100 percent; 1.0/.8=1.25.) In fact, however, the premium was 63 percent higher for the zero coinsurance plan, suggesting that demand was 30 percent higher with no coinsurance (1.63/1.25= 1.3).

This figure should represent a reliable estimate of demand responsiveness in this range; however, one cannot be certain that it derives from the insurance company's experience rather than an actuary's guess at demand responsiveness. Partly for this reason, and partly because the benefits covered are a relatively small set of all dental demands, one would like additional confirmation.[14] Fortunately, a number of other studies' results are consistent with these premium data.

Morehead, Donaldson, and Zanes (1971) measured how visits changed among Teamsters and their families in New York City when a free comprehensive dental care benefit was added. No coverage previously existed. For those who joined, visits rose from 2.5 per person per year in the

year before the plan to 4.9 in the year after introduction. Not all the eligible families joined the plan, but high utilizers of dental services did not appear disproportionately likely to join; the median family dental expenditure in the previous year for those who joined was $78, compared to $105 among the families for those who did not. Thus, moving from no coverage to full coverage caused demand to nearly double.

A study by Avnet and Kisias (1967) supported this estimate. They presented data on utilization of four common dental services (fillings, extractions, cleaning, and examination); the data came from the Group Health Dental Insurance (GHDI) plan in New York City during the period 1958-1964. These four services accounted for two-thirds of the services rendered in the plan (as weighted by the American Dental Association's Relative Value Scale). Although there is no matched group's utilization to compare to GHDI, one can form some impression of how insurance affected demand by comparing utilization at GHDI to the US population.

Phelps and Newhouse (1974a) made such a comparison for both the GHDI "basic" group and the GHDI "voluntary" group. The basic group consisted primarily of persons automatically enrolled at GHDI through their place of employment. The voluntary group had the option to enroll, paying part or all of their premium. One would therefore expect considerably more self-selection among the voluntary group.

For the four dental services age-adjusted demand was 80 percent higher in the basic group than in the United States population in 1964. Because the US population had almost no dental insurance at this time, one can use the 80 percent value as another estimate of how demand changes

as one moves from no coverage to full coverage. In the voluntary group, demand was 180 percent higher than in the general population, indicating that self-selection was certainly present. For both the basic and the voluntary groups, demand was much more responsive for children under six (approximately twice as responsive as at other ages).

Grubb (1964) presented data on how employees of a dental supply firm in York, Pennsylvania, used the same four dental services. These employees had an insurance plan with a $10 deductible and 20 percent coinsurance above the deductible; the deductible was waived, however, if the employee obtained a routine oral examination. Phelps and Newhouse (1974a) also compared this group's utilization to the US population's; the insured employees used slightly more than twice as many services as the population at large. Hence, these employees appear to show slightly greater responsiveness to insurance than did the two New York City groups, but demographic differences between the groups or differences in supply conditions between New York City and York could easily account for these relatively small discrepancies. In general, Grubb's data support the general magnitude of demand response found in the Avnet-Kisias and Morehead-Donaldson-Zanes data.

Manning and Phelps (1978) analyzed dental demand among respondents to the 1970 CHAS survey. They restricted their analysis to whites, because the nonwhite group appeared to exhibit markedly different behavior. The price variation in their sample came from differing relative prices of dental care across various areas of the country rather than from variation in insurance. They did not include a measure of dental

insurance per se (almost no one in the sample had such insurance in 1970), but they projected their estimates of demand responsiveness to full insurance (a zero price). Interestingly, they found that a change from no insurance to full insurance would cause demand among adults to double and among children to triple. Although these extrapolations to a zero price lie far outside the range of price variation that Manning and Phelps observed, their magnitude agrees with the estimates presented above. Additionally, like Avnet and Kisias, Manning and Phelps found demand more responsive among children.

Other estimates use less satisfactory methods. Holtmann and Olsen (1976) collected data from 923 households in five contiguous southern-tier New York and Pennsylvania counties. They found demand considerably less responsive to price than the above estimates, but one must question how much real price variation there was in this small area. Errors in expenditure reporting would tend to conceal the effect of what little true price variation might exist. Moreover, Holtmann and Olsen seek to explain family rather than individual demand, but incorrectly assume that persons within the family respond similarly to insurance (Manning & Phelps, 1978).

P. Feldstein (1973) and Maurizi (1975) also presented estimates of dental demand showing substantial responsiveness. Feldstein used a time series of cross-sections from seven geographic regions showing price and quantity in those regions. Because his observations may reflect the influence of both demand and supply, they cannot be taken as a reliable estimate of demand responsiveness (Manning & Phelps, 1978).[15] Maurizi's data came from observations on individual dental practices, and they too

could reflect both demand and supply. Moreover, he confused the demand curve facing an individual dentist with the consumer's demand curve, so at best he did not measure the demand response from fully or partially insuring dental services.

Out-of-Pocket Payments and the Demand for Prescription Drugs

One can use three sources of data to estimate the responsiveness of prescription drug demand to price. All are roughly consistent in showing that demand approximately doubles when moving from no coverage to full coverage.

Greenlick and Darsky (1968) studied individuals in Windsor, Ontario, who were insured for all drug costs above 35 cents per prescription. The average price per prescription at that time was $3.78, so the 35 cents was on average a 9 percent coinsurance rate (more for drugs costing less than $3.78 and conversely). Greenlick and Darsky compared drug utilization among individuals in this insurance plan during a one-year period with utilization among a random sample of the community. Expenditures per person among the insured group were double those among the noninsured group; thus, one infers that moving from no coverage to nearly full coverage would cause demand to double.

Self-selection could cause part of this responsiveness, because those using drugs frequently had an incentive to join the plan. Their utilization when insured would overstate utilization by those not now in the plan if the latter were to have joined. Fortunately, two other studies that do not involve self-selection suggest that self-selection may not have importantly influenced Greenlick and Darsky's results.

Smith and Garner (1974) studied behavior when the Mississippi Medicaid program instituted coverage for prescription drugs. They collected data from all the pharmacies in one town on drug purchases by a sample of Medicaid beneficiaries during the three months before Medicaid covered drugs (April-June 1970), and compared these data to drug purchases by this sample during the three months after the program. The change from no coverage to full coverage led to an increase in prescriptions per person of 75 percent and expense per person of 124 percent. Some of the increase may reflect postponement of drug purchases in the period immediately before instituting the program, but it is doubtful that many drug purchases could have been postponed. A seasonal falloff in demand during the summer may cause Smith and Garner's estimate to be somewhat understated.

Phelps and Newhouse (1974a) analyzed prescription drug expenditure in the British National Health Service during periods of varying drug charges. From 1955 to 1969 the price of a prescription varied from nothing to 2.5 shillings. Phelps and Newhouse computed average coinsurance rates for each year (out-of-pocket payments/total expenditure) and used that rate (along with a time trend) to explain annual real expenditure on drugs. The average coinsurance rate varied from zero to 23 percent. Estimated expenditure on drugs at zero coinsurance was 16 percent higher than at 25 percent coinsurance at the midpoint of the period, supporting the conclusion that demand for prescription drugs is quite responsive to insurance coverage.

Out-of-Pocket Payments and Total Expenditures on Medical Care

We have now seen how demand for several procedures responds to insurance. Many studies have been discussed, most of which show that demand changes as the level of out-of-pocket payments changes. We can now bring the overall picture into better focus by examinating variation in total expenditure as the level of out-of-pocket payments vary.

Overall demand and coinsurance. Insurance premiums for policies with different coinsurance rates reveal how overall demand responds to coinsurance. Phelps and Newhouse (1974b) ascertained premiums for policies with varying coinsurance rates, netted out that portion of the premium change due to the insurer's covering a bigger portion of a constant size pie, and attributed the residual to differences in demand.[16]

Phelps and Newhouse obtained premium quotations from four insurance companies for four different coinsurance rates (above a $50 deductible)--10, 15, 20, and 25 percent. Their analysis indicated that the insurance companies agreed almost perfectly on how demand changed as the coinsurance rate changed and that between 10 and 25 percent coinsurance the demand curve was linear.

The premium quotations imply that as one moves from 25 percent to 10 percent coinsurance, demand increases 6 percent. Although somewhat lower than the overall estimates one obtains from aggregating the service-by-service values discussed in the preceding sections, the discrepancy is not large (Phelps & Newhouse, 1974a). The service-by-service estimates could be somewhat high because of greater self-selection,

or demand responsiveness might be larger in other ranges of coinsurance. (None of the service-by-service results came exactly from the 25 to 10 percent range.)

Rosett and Huang's (1971) estimates of how demand responded to cost sharing are higher than almost all other estimates reviewed. They examined the relationship between individuals' expenditure and their coinsurance rate, but because they defined coinsurance as out-of-pocket expenditure divided by total expenditure for all services, expenditure appeared greatly influenced by coinsurance. Those with large expenditures tended to be hospitalized. Because insurance typically covered their hospitalization expenditure, their estimated coinsurance rate was well below one. By contrast, those with small expenditures, such as an occasional office visit or prescription, typically had no insurance and so had an estimated coinsurance rate of one. Even if the true response of demand to coinsurance had been zero, therefore, this method would have shown (falsely) that demand responds to coinsurance. Other problems with this study are discussed in Newhouse and Phelps (1974).

Overall demand and deductibles. To this point I have ignored the relationship between deductibles and demand. Deductibles are much harder to analyze than coinsurance or copayments for both theoretical and empirical reasons. Theoretically, it is difficult to define the appropriate price facing the patient below the deductible. One should not use the full price, because of the probability that the patient may later exceed the deductible, in which case any additional expenditure incurred now will effectively be subsidized by insurance benefits. In other words, if

the individual seeks services now, his wealth position at the end of the year may not be reduced by the full amount of the expenditure (Keeler, Newhouse, & Phelps, 1977).

Theory does give some help by suggesting that the responsiveness of demand to deductibles is nonlinear. At very high levels of the deductible, most individuals expect not to exceed the deductible and so act as if uninsured. Consequently, small changes in a very large deductible will not much affect behavior. To take an extreme example, demand will not change much if the deductible decreases from \$10,000 to \$9,000. At the other end of the spectrum, most individuals expect to exceed very small deductibles, so small variation in them will also not much affect behavior. Given the substantial difference in demand between uninsured and insured populations, it follows that demand must be quite sensitive to variation in an intermediate range of deductibles.

Testing propositions about deductibles empirically is difficult because individuals who do not exceed the deductible usually do not file claims. The insurer thus does not know what demand is below the deductible. For example, some studies of hospitalization use data from plans with a deductible (Ackart, 1961; Williams, 1966). One-day stays may be underreported in those plans, thus biasing upward the estimated average length of stay in the deductible plans.

Newhouse, Rolph, Mori, and Murphy (forthcoming) estimated how demand changes as the deductible varies from \$50 per person per year to \$1,000 per person per year. Their estimates used data from two insurers that showed the premiums for six different levels of deductibles (\$50, 75,

100, 200, 500, 1,000). To infer how demand changes as the deductible changes, one must distinguish that part of the premium change due to demand changes and that part due to the insurer's changed portion of total expenditure. Newhouse et al. (forthcoming) did this by using the distribution of expenditures in the federal employees high and low option plans.

Their results showed that demand at a deductible of $1,000 per person per year was roughly two-thirds of demand at a deductible of $50 per person per year. Moreover, the hypothesized nonlinear pattern of demand was present; the responsiveness of demand to changes in the deductible fell steadily as the deductible rose. Derivation of these results required a number of restrictive assumptions, however, and so the results must be considered highly preliminary.

The Medicare, Medicaid, and Canadian Experience

Studies of programs that have changed cost sharing arrangements for large groups of consumers corroborate that out-of-pocket payments affect demand. For example, Davis and Reynolds (1976) used data from the National Health Survey to compare physician visits and hospital days among public assistance recipients (who were eligible for Medicaid) with other low-income persons (family income below $5,000). They controlled for age, sex, race, employment status, family size, education, restricted activity days, chronic conditions, and physician and hospital supply in the respondent's local area. Among individuals of "average" health status (those with the mean level of restricted activity days and chronic conditions), those eligible for Medicaid made nearly 50 percent

more physician visits and had nearly twice as many hospital days than the non-Medicaid-eligible poor. This size difference is similar to the differences found in the studies reviewed above that utilized data from nonpoor populations. Because of the similarity, one could tentatively infer that the poor are not notably more responsive to price than the nonpoor.

Data from before and after the Medicare program demonstrate that it affected demand, but do not indicate the magnitude of the demand change, because the change in utilization was probably less than the change in demand. When Medicare was introduced, insurance coverage for hospital services probably changed most. Prior to the program only about half of the over-65 population had any coverage for hospital services.[17] The Medicare program provided full coverage up to 60 days of care after a one-day deductible. (Because most stays exceeded one day, the deductible was irrelevant to most decisions on length of stay, but it presumably affected admissions.) Physician services were covered with a 20 percent coinsurance rate above a $50 deductible. The number of hospital days of care among the aged increased around 20 percent from the year before to the year after the program (Pettingill, 1972, Table 2). The increase cannot be accounted for by changes in age, sex, race, marital status, education, or labor force participation (Newhouse, 1975). But for supply constraints the increase might have been even greater; one infers the existence of such constraints because patient days among the nonaged actually fell. Physician visit rates changed much less, probably because of the program's $50 deductible; less than half the over-65 population met the Medicare deductible (computed from Peel & Scharff, 1973, Table 2).

The incidence of surgical operations among the aged population also increased (Bombardier, Fuchs, Lillard, & Warner, 1977). The increase was especially marked among those with little education, probably because that group had the least insurance coverage prior to Medicare.

During the 1950s Canada introduced insurance coverage for hospital services and during the late 1960s and early 1970s coverage for physician office services. Unfortunately, lack of data on insurance coverage and utilization prior to the program's effective date make interpretation of the Canadian data difficult. Two studies using Canadian data, however, bear on our subject.

Enterline, Salter, McDonald, and McDonald (1973) estimated physician visit rates in Montreal before and after the introduction of office visit coverage. The extent of the change in insurance coverage remains unclear, but the program plainly had some effects. Office visits rose 17 percent, but home visits fell 59 percent, suggesting that the change led to stress on the ambulatory care delivery system, which rationed services accordingly. The waiting time for an appointment, another indicator of stress, increased from 6 days to 11 days. Although we cannot know the magnitude of the demand change, these data suggest that demand indeed increased.

In the same study visit rates rose among low-income families, but fell among high-income families. Unfortunately, one cannot infer that the poor respond more to variation in out-of-pocket payments. The higher-income groups could have had better insurance initially, in which case their demand may have changed less than the low-income group. The reason why utilization among high-income groups fell when their insurance cover-

age improved probably involved the rationing mechanisms that came into play (e.g., delays in the office increased) to which higher-income groups are more sensitive.

A second Canadian study, however, suggested that the poor may be somewhat more responsive to out-of-pocket payments (Beck, 1974). When a \$1.50 charge for office visits was introduced in Saskatchewan in 1968 (they had previously been free), utilization of physician services declined 18 percent among the poor, but only 6 to 7 percent in the general population.

In sum, these studies corroborate that variation in out-of-pocket payments affects demand, although they are not well suited to establishing the magnitude of the effect.

The Effect of Out-of-Pocket Payments on Provider Prices

Although this review is primarily concerned with how demand responds to out-of-pocket payments, a brief comment on the relationship between such payments and provider prices is in order. In a typical market consumers have an incentive to search for a low-priced supplier of a given quality good or service; any money saved can be used to purchase other valued goods or services. Search behavior by consumers gives suppliers an incentive to keep prices at competitive levels, because any supplier raising his price will receive less business.

What does this discussion have to do with medical care? Simply that insurance tends to subvert the mechanism just described. Consider a hospital facing a market of fully-insured consumers. If the hospital increases its rates (costs), it will collect more from insurance companies

(or the government), but the higher rates will not deter patients from being admitted to that hospital, or from staying longer once admitted, or from more intensive care once admitted. In particular, the physician has no motivation to search out another, potentially more efficient hospital for patients.

Coinsurance, of course, gives an incentive to search, but low coinsurance rates give correspondingly little incentive. Deductibles that are exceeded and copayments give no incentive to search; by contrast, deductibles that are not exceeded give maximal incentive to search, because any dollar saved accrues entirely to the patient (i.e., is not shared with the insurance company).

Newhouse (1977) provided some empirical evidence that prices increase faster in markets with more extensive insurance. Hospital services, which are nearly fully insured, have a significantly higher rate of price increase than other, less well insured services.

Complete or nearly complete coverage of any service, therefore, with simple reimbursement of costs or charges will probably lead to substantial price increases. Newhouse outlined three possible methods for avoiding this:

1. For nonhospital services, insurance policies with deductibles such that a substantial portion of the population does not exceed the deductible. In effect, enough individuals must be motivated to search for efficient providers to give providers an incentive to price competitively. Such policies will not work well for hospital services, because any deductible sufficiently large that most hospitalized patients will

not meet it imposes a greater risk of loss than most persons wish to bear.

2. For hospital services or for medical services generally, insurance premiums that vary as the costliness of the provider varies. This happens automatically in Health Maintenance Organizations (if the consumer reaps the savings from selecting a cheaper HMO); proposals such as Health Care Alliances (Ellwood & McClure, 1976) or Variable Cost Insurance (Newhouse & Taylor, 1971) can achieve this end without imposing the organizational requirements of an HMO.

3. Public sector determination of budgets. Certificate-of-need legislation exemplifies a step in this direction; proposed cost containment legislation would be a further step.

The strengths and weaknesses of these alternative approaches were discussed by the National Commission on the Cost of Medical Care (1978) and Enthoven (1978).

The Future

In a few years we should know much more about out-of-pocket payments and the demand for medical care. A large-scale social experiment in health care financing is presently in progress (Newhouse, 1974). Around 2,000 families have been assigned to insurance plans with varying coinsurance rates. About 30 percent of the families have zero coinsurance; that is, all medical care is free. The remaining families have 25, 50, or 95 percent coinsurance. All these families have an upper limit on their out-of-pocket expenditure in one year. For most of the families the limit is either 5, 10, or 15 percent of income (to a maximum of $1,000);

in one plan it is $150 per person or $450 per family. Virtually all medical and dental services are covered at the specified coinsurance rate, although in one plan the cost sharing applies only to outpatient services, and in another a higher coinsurance rate applies to dental and outpatient mental health services. The experiment samples from the nonaged, civilian, non-institutionalized population, and the poor are somewhat oversampled.

The data from this experiment, when analyzed, should resolve most of the uncertainties that presently surround the subject of this paper. No question of self-selection should arise because the families have been assigned to plans so as to maximize balance across a number of health and demographic characteristics; that is, the group of families on each plan looks like the group of families on every other plan. The coinsurance rate is well specified, so no ambiguity will arise from converting a co-payment amount to a coinsurance rate. Information on the consequences of varying deductibles should greatly improve, because the 95 percent coinsurance rate plans, along with their upper limit on out-of-pocket spending, approximate different levels of deductibles. Families, however, file claims for every use of the medical care system, so good information should be available about demand by those who do not satisfy the deductible. Considerable effort has been made to minimize measurement error, a problem that frequently plagues studies based upon survey data. How coinsurance affects demand for medical and dental services by various subgroups should also be known. Are the poor more responsive to coinsurance than the middle class, and if so, how much more responsive?

At this writing, experimental field work is about half complete. Field work is scheduled to end in January 1982, although preliminary results are likely to be published before then.

Although the data from the experiment will markedly advance knowledge about the subject of this literature review, a look back in time will show that even over the past few years knowledge has advanced considerably. In 1971 Rashi Fein told Senator Kennedy: "I don't think we have any data that demonstrates that deductibles and coinsurance lead to lower utilization" (U.S. Congress, 1971, p. 146). One could not make such a statement today. Although considerable doubt remains about how much deductibles and coinsurance affect demand for different services and how they affect persons who differ by income, age, or other characteristics, the evidence of some systematic effect is compelling.

A logical next question is whether, or to what degree, the additional medical services demanded when the out-of-pocket price is lower contribute to improved health status. Unfortunately, one can only give the frustrating response that very little is known (although many have opinions). Here too the experiment just described should markedly advance knowledge because it undertakes comprehensive measurement of health status (Brook, Ware, Davies-Avery, Stewart, Johnston, Donald, Rogers, & Williams, 1978). Thus, in the early 1980s we should be much better placed to understand the relationship between out-of-pocket payments and health status, a subject that, if not now in darkness, stands in the dimmest of lights.

Footnotes

1. Coinsurance is the percentage of the bill that the patient must pay. For example, a 25 percent coinsurance rate means the patient pays 25 percent of the total charge. Copayment establishes a fixed dollar amount per unit of service that the patient pays, for example, $3 per visit. For a given price, there is a coinsurance rate that corresponds to every copayment level, and the two are analytically equivalent. The difference between them occurs if prices vary according to provider. Then coinsurance leaves some incentive to visit a lower-priced provider, whereas copayment does not. A deductible requires the patient to pay the amount of the deductible before any insurance benefits are paid. Many policies also contain upper limits on what they will pay (per year or per lifetime). I ignore such limits because they are not part of proposed national health insurance plans, and because they are only rarely binding.

2. The one-quarter figure is computed from data in Table 4, and adjusts for the differential probability of any visit to a physician shown in Table 2.

3. Suppose, for example, that price variation reflects quality variation; e.g., a pediatrician may charge $20, a general practitioner $10. Suppose, for the sake of argument, that the pediatrician is twice as good. (I beg the question of how to measure quality.) In effect, then, a "quality-adjusted" price is the same for both physicians. Coinsurance leaves the quality-adjusted price equal; copayment does not. Averaging, as described in the text, fails to account for this difference.

4. The employees may prefer to take compensation in the form of employer-paid premiums because such premiums are not taxable income, whereas if the money were paid in wages or salaries (which the employee could then use to purchase health insurance), the money would be taxable income.

5. See Josephson (1966). The differences in mean expenditure rates between high and low option plans are much too great to be explained by the terms of the insurance policy alone, if the other estimates presented in this paper are to be believed.

6. For any one group a comparison of the six months before and after the change could be dominated by seasonal effects. The groups changed coverage at various times during the year, however, and I have assumed that when comparisons are made across all groups these seasonal effects will cancel out. Heaney and Riedel present no data either to support or refute this assumption.

7. Heaney and Riedel provided statistical tests of significance of the change in the experimental group. For the change in the admission rate they estimate a Z value of 1.70, which is significant at the 5 percent level using a one-tail test. (Heaney and Riedel use a two-tail test, although a one-tail test seems more appropriate.) The t-statistic on the length of stay change was 1.48 with 10 degrees of freedom; this is not significant at conventional levels, although 10 degrees of freedom appears to be too few.

8. These data are from Williams' Plan D. Williams presents data from four other plans, but in three of them (A, B, and C) it is impossible

to calculate an effective coinsurance rate (Williams also indicates that length of stay figures may not be reliable in these plans) and in the fourth (E) it is impossible to adjust the data for age and sex differences between copay and no-copay groups. Hence, data from these four groups are not shown.

9. For details of this calculation, see Phelps and Newhouse, 1974a, Table 5.

10. I was unable to find a revenue-per-day figure for 1972 for Kentucky, so I have used the 1969 figure for Kentucky of $58.13 (Hospitals, August 1, 1970, Part 2, p. 502) and inflated it by the increase in total expense per patient day from 1969 to 1972 of 50.2 percent (Social Security Administration, 1975, p. 37).

11. This can be seen by substituting their estimated equation (1) into their estimated equation (2) and solving for the coefficient of the variable OPC.

12. Rosenthal also gives results using the hospital's charge as an explanatory variable, but I do not present these results because they may confound effects of hospital quality and complexity of case within disease category with price effects.

13. Average daily patient revenue for inpatients in community hospitals in Pennsylvania in 1969 was $65.82 (Hospitals, August 1, 1970, Part 2, p. 502).

14. One must also make the assumption that the loading charge--the insurer's fee--is a constant percentage of the premium in the two policies. This was stated to be the case.

15. Feldstein's only identifying variable is dentists per capita, which cannot be assumed exogenous.

16. Let the total premium be $TP = (1+L)D(1-c)$, where L is the insurer's loading charge or fee, D is demand, and c is the coinsurance rate. Given TP and c, and assuming L is a constant, this equation may be used to find the ratio of demands at different coinsurance rates. See also the description in the dental demand section.

17. Computed from National Center for Health Statistics, 1965, Table 1, and 1964, Table 14.

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A REVIEW OF THE LITERATURE

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