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5 The Availability Effect: Empirical Results

This chapter has two purposes: first, to compare the "information manipulation" version of the availability effect argument that was developed in chapter 4 with alternative explanations that have been or might be developed to explain the effect. The intent here is to develop suggestions for distinguishing empirically between the models, and to spell out the different implications of accepting any particular model. Second, this chapter will develop and use an empirical technique to test for the presence and causes of the availability effect.

Medical Need, Availability Effect, and the Economic Concept of Demand

The discussion of the availability effect in almost all of the noneconomic literature has implicitly been based on the principle of "medical need" or what Fuchs has called the "monotechnic" point of view.[1] In its strongest form this principle asserts that, for any set of preexisting symptoms or complaints, there is a unique, appropriate, and necessary course of treatment, which has unique resource or input requirements. Neither individual preferences nor costs are relevant. For example, there is a specific set of indications for appendectomy or hernia repair. If the indications are present, the procedure "ought" to be demanded and performed. If they are absent, it ought not to be performed. It follows that, for a population with a given distribution of symptoms, there should be a unique number of procedures which ought to be performed, and a unique set of inputs which ought to be used to perform those procedures. While adherents of this view recognize that there may sometimes be vagueness about the necessity of a particular procedure, they do not usually discuss what will or should then determine what is to be done, other than to suggest that additional clinical trials would settle the matter.[2]

The critical feature of the medical need approach is that what should be done is held to depend only upon the patient's physical and mental condition. It follows therefore that if the presence of inputs, of surgical specialists for example, is correlated with use when the prevalence of conditions is known or assumed to be constant, there is prima facie evidence of "demand" or use creation. If surgical rates vary when surgical manpower varies but surgically treatable pathology does not, excess resources must be creating "unnecessary" use. Note that this approach blends or combines normative and positive aspects: demand creation occurs when use varies for reasons for which it ought not.

The economic approaches to be discussed below differ from the medical need approach in that they permit use to be determined by more than just the patient's physical condition and possibly some socioeconomic characteristics. In particular, his tastes or desires, the price (real and monetary) that he pays, and the resources at his disposal are held to be things which affect his use. More than this, there is often the implicit normative judgment that, at least in principle and in some situations, these variables *ought* to affect what he gets. In this approach, then, demand creation requires one to look at *both* health or condition indicators and economic variables before attributing any residual influence to an availability effect.

Viewed as a positive theory of behavior, the economic approach is much less likely to label a given act of behavior as demand creation. Suppose, for instance, a patient has insurance which covers all hospital costs. Suppose there is some newly developed, exotic, and expensive procedure for which facilities have just been installed and which promises him a positive but slight improvement in his expected health. If the patient uses that procedure, this would not be regarded as demand creation; it should only be regarded as satisfaction of his (large) demand at a zero price; supply is responding to demand. The installation of the equipment does not create the demand for improvement in health; it is only the supply response to previously unsatisfied demand. A physician would not be acting in the role of agent for his patient if he did not recommend the procedure. The physician might, especially if he is somewhat unorthodox, recognize the true waste involved in this transaction, and so label it the "technological imperative," but it would not be a manifestation of demand creation. If the patient, supposing he were truly informed, chose *not* to have the procedure done, but the physician manipulated information to get the patient's approval, *that* would be regarded as demand creation.

Economic Theories of the Availability Effect

In what follows I will concentrate on availability effects for physicians' services. Hospital care will be considered in a later chapter. The data will not permit empirical analysis of other types of medical care.

There are four kinds of theoretical explanations of the availability effect to be investigated:

1. The availability effect arises from statistical properties of the estimation procedure.
2. The availability effect represents the response of use to changes in the time or convenience cost of care.
3. Medical services are subject to chronic excess demand, and care is rationed by physicians on the basis of the interest or severity of cases, but excess demand is reduced when the availability of physicians is increased.
4. Physicians create demand by manipulating the information they provide to consumers; when more physicians are present, they alter information to induce consumers to use more care.

These explanations are obviously not mutually exclusive. Consequently, a test which supports one theory does not necessarily disprove another. Moreover, because a number of natural theoretical variables will not be measured (and may not be measurable) directly, proxy variables will have to be selected. As a result, the failure of an empirical test to confirm a theory may only indicate that inappropriate proxy variables were chosen, not that the theory itself is incorrect.

Data Selection and Statistical Properties

It is clear that trying to estimate a separate availability effect from aggregate data will involve a severe identification problem. The demand equation of interest is of the general form

$$Q_D = D(P, X, Z) \tag{1}$$

where Q_D is quantity of some medical input demanded, P is its (user) price, X is a vector of other demand parameters, and Z is a vector of measures of input availabilities. In most demand studies, observations on use per capita and availability have been taken from geographic aggregates.

The problem with such an approach is that not all of the right-hand side variables are exogenous, nor is (1) the sole equation determining observed use Q. One of the other equations would, for example, be a production function:

$$Q = Q(Z) \tag{2}$$

If some demand variables are omitted, if supply responds to demand, and if more inputs are needed to supply this output, there will necessarily be more use where there are more inputs. But there is no independent causal effect. Input availability does not cause more use; rather, when people desire more use, more inputs become available.

It has been noted less frequently that ostensible demand creation could also arise because of measurement error in demand variables. Suppose, as is very common, user price is measured only imperfectly. But suppose that supply responds well to an increase in quantity demanded induced by a decline in user price. In effect, supply availability may be a better proxy measure of true user price than is measured price. In such a case, price elasticity would be underestimated (because of errors-in-variables bias) and the demand creation effect would be overestimated. These problems will tend to be most severe when observations consist of aggregates over the size of a market area (e.g., a state or SMSA).

In such cases there is likely to be still another source of bias if population is measured accurately, and data on use and input availability are taken from the same source. For instance, hospital admissions and hospital beds are usually drawn from the same American Hospital Association survey. But such surveys may omit some possible observations—e.g., the AHA survey omits both Veterans Administration hospital beds and admissions to such hospitals. Then the coefficient on the independent variable will again be biased upward as a measure of the effects of inputs on *total* use.

In general, if equation (1) were estimated using actual values of Z, it is clear that the coefficient on Z might be biased upward. This bias might be avoided by using predicted values of the Z variables in a two-stage procedure. But there may still be problems: there may not be an exogenous variable in the supply equation with which to identify the demand equation. To see this, consider the most fully specified model, that of Fuchs and Kramer.[3] In a technical sense, their approach is free of the omitted demand variable problem because they treat physicians per capita (MD^*) as an endogenous variable, and so estimate the demand equation in which MD^* enters by $2SLS$. They therefore use a predicted value for MD^* rather than the actual value, and this predicted value should be free from correlation with any omitted demand variables. However, the exogenous variables which are used in the first stage to predict MD^* are either explicitly demand variables, or, what is more serious, they are variables which might not really be exogenous, but rather would themselves be correlated with the omitted demand variables.

For example, one of the most important variables in the MD^* equation is hospital beds per capita. But as was argued above, and as will be developed in more detail in chapter 6, it seems more reasonable to suppose that beds and physician time are jointly demanded to produce hospital output. Neither physicians nor beds are exogenous. This effect is even stronger in the Fuchs-Kramer study, since interstate variation in their measure of physician visits per capita (the dependent variable in the demand equation) depends strongly on variation in hospital bed-days per capita, because physician hospital visits are estimated by using the number of bed-days. For both of these reasons, it is likely that much of the positive relationship between MD^* and beds arises from their mutual dependence on omitted demand variables. The other important predictor of MD^* is the number of medical schools in the state (*not* medical schools per capita, or per physician; apparently medical schools are a public good as far as physicians are concerned). It would again appear to be a reasonable conjecture that, if more medical schools in a state do mean more physicians, those states with unusually high demands for physicians' services would be expected to have more medical schools. Again, this variable could be correlated with omitted demand variables.

This is a specific illustration of the general difficulty of finding "truly exogenous" supply variables for a product whose output does not depend on weather or geography. A better choice would have been some determinants of real physician income, such as temperature, extent of urban amenities, or golf courses per capita, or possibly some measures of governmental restrictions on physician flow. Fuchs and Kramer did try some of these variables, but apparently they were not strongly related to MD. In a more recent study of the demand for surgical services, Fuchs was able to relate the surgeon stock to hotel expenditures per capita as a proxy for locational desirability.[4]

One way to mitigate these problems is to avoid aggregated data. That is, instead of using per capita demand in a market area, which is surely going to be related to area-wide levels of omitted variables, one could use as observations the quantities demanded by individuals. It is less likely that the number of physicians will respond appreciably to unusually high or low demands by a single individual. One can only say less likely, not unlikely, since it is possible that any omitted variables might affect all persons in an area in a similar way. If, for instance, all or almost all people in South Dakota have unusually low demands for physicians' services, perhaps because of a common ethnic background, then the omitted variable problem returns. If however, the variance within areas of some characteristic is sufficiently large, and especially if it is large relative to the variance across areas, input availability can safely be treated as exogenous to a given individual. For these reasons, and also because of other desirable aspects of the data, individual observations will be used in the empirical analysis which follows.

Nonmoney Price and the Availability Effect

There is one omitted demand variable which should be given special attention. Suppose the number of physicians in an area is increased exogenously. The money price of their services may fall. But, in addition, the time and inconvenience cost of seeing a physician is likely to be reduced. Distances will be shortened, the possibility of waiting in queues reduced, and so on. As Acton[5] has shown, the time cost of ambulatory physicians' services may be large relative to the money price or cost. Consequently, even with money price held constant, an alteration in input availability may have a substantial effect on use if it affects the time cost of care. (Quality might also be affected.)

It may be possible to construct a test to suggest whether the availability effect arises from a change in the time cost of care. Let the total price of a unit of care be given by

$$P = P_M + Wt \tag{3}$$

where P_M is the (user) money price, t is the time spent per unit of care obtained, and W is a measure of the opportunity cost of time.

Suppose all buyers wait approximately the same length of time. Suppose that increases in physicians per capita reduce waiting time by the same amount for all persons. Newhouse and Phelps[6] have shown that

$$\eta_{qt} = \frac{Wt}{P_M + Wt}\,\epsilon \tag{4}$$

where η_{qt} is the elasticity of demand with respect to time and ϵ is the elasticity of demand with respect to total (money plus time) price. It follows directly that changes in physician stock will reduce time cost by a larger amount for high wage persons. Since time cost is a larger fraction of total cost for high wage persons, equation (4) implies that, if ϵ is the same for all persons, the elasticity of use with respect to physician stock η_{qm} will vary positively with W. While it is likely that higher wage persons will seek medical care which has a higher money cost and a lower time cost, it seems reasonable to suppose that money cannot be substituted for time to such an extent that total time cost will be less. (If P_M equalled zero for a low-income person, then η_{qt} would equal ϵ, and would be greater than η_{qt} for other persons. However, persons in households with incomes low enough to make them eligible for Medicaid are omitted from the sample that will be used.)

Since in practice W and income often vary together, and ϵ may decline with income, availability elasticities may not differ significantly. In the empirical work, I shall make some attempt to control for income while W varies. Note finally that if, instead of being constant elasticity, the demand curve is assumed to be approximately linear in P and q, then ϵ will *increase* as W (and P) increase, making the predicted variation in η_{qm} with W even more likely.

Time cost may also be relevant for understanding differences in the availability effect between urban and rural areas. It is often alleged that there is a relative "shortage" of physicians in rural areas. Indeed, the per capita number of physicians is substantially less in such areas. Time cost may therefore be high in such areas. If time cost is higher in rural areas and if relative money prices are the same in rural and urban areas, a given percentage change in physician stock should produce a larger percentage change in total price in rural areas. To the extent that the availability effect operates through a change in time cost, the elasticity of use of care with respect to physician stock would therefore surely be positive and probably greater in rural than in urban areas. While nominal money prices are lower in rural areas, so is the cost of living, so that relative prices may be sufficiently uniform to permit this test.

Excess Demand

Excess demand exists when the quantity demanded of some service exceeds the quantity supplied at a given price. If excess demand for physicians' services exists at current prices, it is possible that an increase in supply of physicians may cause the observed quantity used to increase with no change in money prices. The case is clearest and neatest when excess demand occurs because of price controls imposed on exogenously determined input supplies. If price is below the market-clearing price, quantity supplied will fall short of quantity demanded. Now let there be an exogenous increase in inputs available to produce care (physicians, beds). The quantity used at the fixed price will increase: this might be called an availability effect.

Of course, if price controls exist everywhere (though at different prices below the equilibrium one), the "demand" elasticity that will be observed empirically will have the wrong sign, since the estimated relationship will actually represent points on a supply curve. If price controls exist in some places from which observations are taken but not others, one may observe *both* a negative demand elasticity and an "availability effect," neither of which will be accurately measured.

The price control model is not very realistic, since, except for the Economic Stabilization Program and some kinds of Blue Shield reimbursement mechanisms (now largely disused), prices are not fixed. One can argue that if prices respond slowly to excess supply or demand, the actual situation may approximate price controls. But this leaves the uncomfortable questions of why prices respond so slowly, and whether one observes equilibrium or disequilibrium in a cross section.[7]

A more complete answer is provided by arguing that there are *reasons* why the price might stay below the market-clearing level. All of these reasons involve entering either the magnitude of excess demand or the price itself in the provider's utility function. In what follows I will first consider alternative theories of physician behavior that might be constructed to explain the existence of permanent excess demand.

Feldstein[8] was the first economist to suggest that physicians may get utility from excess demand; this was one of his explanations for the upward slope of his estimated "demand" curve for physicians' services. (He also argued that lower prices give physicians utility.) The notion is that physicians maintain a queue in order to be able to select "interesting" or urgent cases.

Feldstein appealed to the existence of a chronic doctor shortage since 1946 as evidence for the existence of such utility functions. Exactly what the evidence is for this shortage, and its dimensions, was nowhere stated. However, it will still be useful to present a model of equilibrium nonprice rating.

I assume that each physician has a utility function of the form $U = U(Y, \alpha)$ where Y is money income and α is the fraction of total cases seen that is regarded as interesting. I assume that hours of work are fixed at L, and that output (number of cases seen) depends only on hours of work. Each physician is assumed to be confronted with a prorata share of the market demand curve. Of the total demand (or per physician demand) at each price, the fraction α_D of cases is interesting. However, the physician is assumed to be unable to charge different prices for interesting and uninteresting cases. For example, it may not be possible for the patient (or physician) to identify beforehand which initial visits will be for interesting conditions. Thus, if each physician sets price at the level which clears the market at $Q_D = Q_S = Q(L)$, then $\alpha = \alpha_D$. However, if physicians value interesting cases, they may be willing to reduce prices below the market clearing level. That is, the only way to induce demand for initial and subsequent visits for interesting cases may be to set an initial price below the market-clearing level, and then select interesting cases from the queue thus generated. The marginal equilibrium condition here (holding quantity supplied Q_S constant) is:

$$\frac{U_\alpha}{U_Y}\frac{\alpha_D \partial Q_D/\partial P}{Q_S} = Q_S$$

where U_α is the marginal utility of the proportion of interesting cases, and U_Y is the marginal utility of income. It is easiest to explain this condition by considering a small change in price. The benefit from reducing price by one dollar is a gain in the fraction of interesting cases of $\alpha_D \frac{\partial Q / \partial P}{Q_S}$. But the cost is a reduction in income of Q_S. The physician will create excess demand for his services as long as the benefit from doing so exceeds the cost.

Since the cost of cutting price by one dollar, or Q_S, is positive for even the initial price reduction from the market-clearing income-maximizing level, it is possible that a physician who values interesting cases may still decide *not* to cut the price. The cost may just be too high. But if he does decide to cut price, the effects of such behavior may be to increase his utility, and price may settle to some value below the market-clearing price. Now let the number of physicians be increased. If price is held constant and if α is held constant, income will decline. If α is a normal good, each physician will reduce his desired level of α by drawing down his queue. (The initial price is likely to no longer be the equilibrium price.) Total output will increase, but the *increase in output will consist entirely of uninteresting cases.* This discussion shows a way of determining whether any observed availability effect may be attributed to this cause. In particular, the elasticity of use with respect to the number of physicians per capita should be less for cases labeled as interesting, or for persons with such cases, than for uninteresting cases.

The "interest" or "severity" on the basis of which rationing occurs is, of course, that which is perceived by the physician. Insofar as there are a priori reasons why physicians and patients might differ in their estimates of severity for particular kinds of cases, then another test of this kind of theory would involve determining whether the response for those kinds of cases which physicians do not deem to be severe is larger than for those cases which physicians do regard as severe. Such a test would require proxying severity by symptoms or by diagnosis, or by physician and patient attitudes toward symptoms and diagnosis.

Alternative bases for rationing other than severity yield alternative predictions about differences in the availability effect. It is possible, for example (as also suggested by Feldstein), that excess demand may arise if price enters the physician's utility function. Suppose demand increases for a type of care for which a critical physician input is in perfectly inelastic supply. A price rise could reequate demand and supply, but physicians may recognize that its only function is to transfer income from patients to physicians. Accordingly, they may neglect to raise prices, and ration on the basis of severity or interest. But the higher patients' incomes are relative to physicians' incomes, the less this kind of benevolent rationing is likely to occur. Thus one would predict that the response of use to availability should differ by severity, but that these differences should be greater the lower the relative incomes of patients. If severity itself enters the physician's utility function, there would be no such difference.

The precise determination of whether excess demand exists or not is difficult because queues do not necessarily mean that price is too low, any more than the existence of inventories means that the price is too high. When demand is stochastic, a profit-maximizing firm may choose to hold inventories, or to permit queues to develop; either way the production process is smoothed out. Which strategy is chosen depends, roughly speaking, on which is cheaper. Having groceries wait for people in the supermarket rather than vice versa makes sense if the costs of maintaining inventories are less than the costs of delay. Having people wait for doctors rather than vice versa makes sense if the cost of doctors' time exceeds that of the cost of patients' waiting time. What is actually chosen is some mix of queues and inventories: there are occasional delays at the supermarket, and doctors sometimes have time for a cup of coffee. *If* prices could be adjusted with this variation in demand, some (but probably not all) queues would disappear; but such adjustment is probably itself too costly. One can be certain that excess demand exists only if there is *always* a queue, and it is not clear that this obtains even for "busy" doctors.

One final question is whether the behavior predicted by these theories might also be predicted by, or at least consistent with, long-run profit-maximizing behavior on the parts of physicians. Unfortunately for the purposes of hypothesis testing, the answer seems to be yes.

It may be, for instance, that most people have a kind of implicit contract with their physician. Because it is too costly to vary prices with urgency and demand, the physician agrees to let the patient jump the queue at no increase in price for serious illness if the patient will wait (patiently?) in the queue when the complaint is nonurgent. This permits the physician to even out his patient flow, thus increasing his productivity and lowering the price he needs to charge. Thus it is possible to concoct a profit-maximizing explanation of both (1) the regular doctor and (2) taking the most serious cases first. Hospitals will do the same if they do what physicians want.

Measuring Information Manipulation

In the information manipulation theory, the demand equation of interest has the general form

$$Q^{ij}{}_D = Q(\tilde{P}^{ij}, X^i, A^j)$$

where $Q^{ij}{}_D$ is the quantity of a given medical service demanded by person i in area j, $\tilde{P}^{ij}$ is the user price (net of insurance) paid by person i, X^i is a vector of person i's demand characteristics (including his state of health, and A^j is the level of accuracy he experiences. It is assumed that A^j is the same for all persons in a given area.

It will be possible to obtain reasonable approximations of Q_D, $\tilde{P}$, and X. But A cannot be measured directly. Instead, as suggested by the earlier discussion, the level of accuracy chosen by the representative physician will depend upon the gross price level, the quantity that each physician can sell at that price, and the level of the physician's subjective marginal opportunity cost. I will assume that the marginal cost curve is the same for all providers. With gross price P^j held constant, total quantity demanded in an area depends on the mean area-wide values $\bar{X}^j$ of the demand parameters (including others' insurance coverage). The demand per physician then depends on the number of physicians. Thus we can write:

$$(2) \qquad A^j = A(P^j, MD^{*j}, \bar{X}^j)$$

where MD^{*j} is the stock of physicians per capita in area j and P^j is the gross price received by physicians in area j. Note that A is greater the larger the demand per physician. So while an individual's demand for physicians' services would be greater the sicker he is, it would be smaller the sicker everyone else in his community is, because in the latter case the physician would have less incentive to create demand.

Substituting the equation for A into the demand function gives

$$(3) \qquad Q_D{}^{ij} = Q(\tilde{P}^{ij}, X^i, \bar{X}^j, MD^{*j}, P^j)$$

Since $\tilde{P}^{ij} = INS^i P^j$, where INS^i is the fraction of i's expenditure not covered by insurance, the actual estimating equation will be

$$(3') \qquad Q^{ij}{}_D = Q(INS^i, P^j, X^i, \bar{X}^j, MD^{*j})$$

Since P^j has effects on consumer demand both directly through the user price and indirectly through incentives for demand creation, it is not desirable to impose the constraint that changes in INS^i and P^j which have the same influence on $\tilde{P}^{ij}$ should have the same influence on demand. In particular, changing $\tilde{P}^{ij}$ by changing INS^i should have a larger effect on quantity demanded than that from changing P^j, since changes in the prices paid to physicians provide offsetting incentives to create additional demand. In the data to be used there is no measure of the marginal or average fraction of expense covered by insurance, only an indication of whether or not the individual is covered by hospital and/or medical-surgical insurance. A dummy variable for INS is therefore used in the regressions. This procedure is equivalent to assuming that all persons who have insurance have the same coverage.

Target Income

A final kind of theory that contains elements of all of the preceding ones is the so-called "target income" theory. This theory was first suggested by Newhouse and Sloan,[9] but has received its most extensive development by Evans.[10] In its simplest and most naive form, the theory assumes that physicians have target money incomes and target workloads they wish to achieve. Both are chosen with reference to what is acceptable or usual in the community and in the profession. Productivity and the level of other inputs held constant, the target workload determines output Q. Given input prices and Q, the physician then chooses the gross output price that achieves the target income. If the Q chosen in this way exactly equals the Q corresponding to this gross price on some market demand curve, then demand is just satisfied. If Q is less than the quantity demanded, excess demand prevails. If Q is greater than the quantity demanded, actual Q will be increased as physicians "order extra work for patients, perform unnecessary or marginally necessary operations, or recall patients for extra visits."[11] Increases in the number of physicians will in either case result in an increase in actual Q at any price—an availability effect will be observed.

In its simplest version, the target income theory is consistent with an availability effect which arises *either* from the drawing down of excess demand or from demand creation. It simply makes observed demand depend on the number of physicians. If a distinction is to be made between a target income theory and any other theory, that distinction must be based on a presumed fixity in the incomes, real or money, that physicians desire.

If desired or target income is assumed to be fixed, the theory makes a definite qualitative empirical prediction which permits it to be distinguished from maximizing models. It predicts that an exogenous increase in gross price will lead to a *reduction* in the quantity of services per capita supplied and used. For if quantity were held constant, then income would rise. To keep it at the target level, quantity must fall.[12] The information-manipulation theory, however, is consistent with the finding of a positive or zero effect if the substitution effect of a price increase (which represents a greater reward for more business) offsets the income effect.

While Evans seemed initially to support the naive target income theory,[13] his "more extended model" allowed actual workload to depart from desired workload. More importantly, he allowed desired workloads to be a positive function of price, which makes it possible for price and quantity used to be positively related. He used casual empiricism on the Canadian experience to suggest that the relationship will still usually be negative, but the "extended" theory as such cannot be refuted. Indeed, as Sloan and Feldman have shown,[14] *any* outcome with respect to the relationship between physician stock and price or aggregate use would be possible under the "extended" theory.

These alterations make Evans' models indistinguishable from the demand creation model discussed in chapter 4; they rob the target income approach of any distinctive qualitative empirical predictions. In order to preserve this distinction, in what follows I shall regard the target income approach as the initial, naive model. Evans' "more extended model" will instead be identified with the information manipulation theory.

Data Sources and Variable Measurement

In this section some empirical tests of alternate theories of the availability effect will be provided. The basic source of data on individual use and individual characteristics is the 1970 Health Interview Survey of the National Center for Health Statistics.[15] This survey consists of the results of a questionnaire administered to about 110,000 persons in 39,000 families on their health, use of medical care during the past year, and socioeconomic characteristics, including insurance coverage. From A.M.A. and A.H.A. data sources, I obtained information on the number of physicians of various types, hospital beds, and gross prices of hospital and physician services in the "primary sampling units" (PSUs) from which the sample was drawn. PSUs are counties, groups of counties, or SMSAs. For 22 large SMSAs that are identified on the tape, the actual values of the PSU-level variables could be used. For the other PSUs the National Center for Health Statistics attached a set of decile rank orders of each of the variables to each of the PSU codes. I then substituted for the decile rank orders the population-weighted mean values of each of the variables for each combination of decile rank orders. In most cases, this procedure will associate a unique set of values for the area variables with each PSU. The variables used in the empirical work are as follows:

Dependent Variables

Three measures of utilization:

1. Physician visits, last 12 months. This includes telephone calls but excludes visits to hospital inpatients. Because of the recall period, there may be some error in measurement.

2. Hospital episodes, last 12 months, in nonfederal, short term hospitals.

3. Average stay per episode (equals hospital days divided by hospital episodes).

Independent Variables and Abbreviations

Personal Variables:

1. Restricted activity days, last two weeks (RAD).

2. Number of chronic conditions (CONDS).
3. Age (AGE).
4. Sex, 1 if female (SEX).
5. Female of childbearing age dummy (F15–44).
6. Family size (FAMSZ).
7. Work status: whether individual is currently employed or not (WORKING).
8. No insurance coverage (NOINS): indicates whether the individual is covered by hospitalization or surgical insurance or Medicare or not. About half of the surveyed families were asked this question, so in most of the analysis only observations on persons who were asked this question were used. However, restricting the data set to families who answered the question would lead to small cell size for the high education group in all but the large metropolitan areas. Accordingly, an insurance coverage variable is not included for high education families in other metropolitan or rural areas.
9. Family income in 100's (FAMINC).

PSU Variables:

10. Office based M.D.'s per 100 persons (OBMD*).
11. Surgical specialists as a fraction of office-based M.D.'s (SURG/MD).
12. General practitioners as a fraction of office-based M.D.'s (GP/MD). The variables GP/OBMD and SURG/OBMD measure the proportion of total office-based physicians who are general practitioners and surgeons respectively. They capture any differences in the quality or characteristics of a typical patient visit, as well as differential specialty effects, if any, on demand creation.
13. Hospital based M.D.'s per 100 persons (HBMD*)—mainly residents, interns, and other hospital-salaried physicians.
14. Nonfederal short-term hospital beds per 100 persons (BEDS*).
15. Hospital cost per patient-day for nonfederal short-term hospitals in the PSU (HOSCOST).
16. Population density (POPDENS).

17. Physician office visit fee (MDFEE). Two measures are used. The fee measure used for all areas is the fee screen for Medicare patients for followup office visits. This screen is supposed to represent the seventy-fifth percentile of the prevailing distribution of fees in various geographic areas. However, since different procedures are used by different carriers to set up the fee screen, the accuracy of this measure is unknown. When there were different fees for general practitioners and specialists, a weighted average was used. For the 100 largest SMSAs, Mathematica, Inc. conducted a telephone survey in 1973 to ascertain

the fee for a routine office visit to a primary care practitioner,[16] and this measure should be an accurate index of fee levels for such services. Accordingly, results for office visits for the 22 largest cities are presented using the Mathematica prices as well as using the Medicare fee screens.

In principle, cost-of-living differences should be taken into account in estimating monetary variables (income and fees). A complete set of such indexes is available from the Bureau of Labor Statistics only for the large-city subsample, not for all other cities or for rural areas. A comparison of regressions using undeflated and deflated data for the large city subsample will suggest whether the absence of deflation biases coefficients on the variables of interest.

Classification of Results for Physician Visits

Results for physician visits are presented for several different data sets. First, persons are classified by the number of physicians in the market area in which they live: the 22 largest SMSAs, in which it would probably be very difficult for consumers to get reliable information on any individual physician; nonmetropolitan areas, in which such information is presumably easier to obtain; and other, smaller metropolitan areas, which might be expected to be intermediate.

The information manipulation theory would suggest that availability might have a greater influence in those large metropolitan areas in which individuals have difficulty determining the quality or accuracy of the advice which any individual physician provides. An excess demand theory, on the other hand, would predict that availability effects would be most likely to be observed in rural areas, where physician stock is lowest and excess demand presumably the greatest.

Second, results are presented for persons in households in which the heads have different levels of education. Education level is intended to be a proxy for the prior stock of information. In most of the analysis, results will be presented for two extremes of the distribution of educational attainment—persons in households in which heads are not high school graduates (low head education) and persons in households with heads with college degrees or better (high head education)—for the reasons discussed in chapter 4.

Where an availability effect is detected, the sample is further subdivided in ways which, it is hoped, correspond to differences suggested by other theories of the availability effect. The survey did not ask about wage income or hours, but it did ask whether or not the person was employed. It would seem reasonable to suppose that, money income held constant, adults who are working have a higher time cost than adults who are not. Another method of division is by number of chronic conditions, which may serve as proxy for illness severity or physician interest in the case.

Two alternative methods of estimation were used. Since there is a concentration of observations at zero, especially for the low education group, ordinary least squares regression on all observations would yield incorrect estimates. Tobit regression is usually the appropriate technique to be used.[17] However, there are reasons to fear that tobit may not be theoretically appropriate for the question being investigated here, as has been suggested by Newhouse and Phelps.[18] Physician advice can only influence use if the consumer first contacts the physician to seek his advice. The initial decision to seek advice should not be influenced by physician information; although physician availability may influence this decision, it will do so through changes in nonmoney price. Once a person has made at least one visit, then he will potentially have received some physician advice and be subject to an information effect. Accordingly, analysis of the subsample of persons with positive doctor visits is more likely to display an information effect than is analysis of the total sample. Of course, not all of the persons with positive numbers of visits will have been subject to information manipulation; there may be several initial visits, for a series of illnesses, included in the data. Moreover, the censoring issue discussed earlier suggests that those with positive visits may be more susceptible to information manipulation than those with no visits. Nevertheless, the set of those with positive visits may still be more appropriate for investigation of an availability effect than the full sample. Such analysis of nonzero observation can be done using OLS, since there is less of a concentration of observations at a lower limit.

Two kinds of person are omitted from the sample. Persons with household incomes below a poverty line are dropped because their use is likely to be covered by Medicaid and so not responsive to changes in fees. Those with more than 52 physician visits in a year are also dropped in order to avoid bias from extreme values.

The full specification described in the section "Measuring Information Manipulation" indicates that area demand variables as well as individual demand characteristics should be included in the equations. Several such area demand variables—percentage of population covered by Medicare, percentage of under-65 population with private health insurance (for the state in which the SMSA is located) and percentage of population under age 5—are available for the 22 large cities. Accordingly, results with regressions which include these variables are also presented.

Empirical Results: Physician Ambulatory Visits

The primary prediction of the information-manipulation theory is that alterations in accuracy should have less effect on those with sufficiently larger prior stocks of information. There are two primary proxies for incentives for alterations in accuracy suggested by the theory: the number of physicians per capita, and the gross price of a unit of output.

The two major questions of interest are: (1) whether availability effects are shown and (2) if effects are shown, whether they differ across educational groups. Table 5.1 shows the coefficients for OLS regressions of physician visits during the preceding 12 months for persons with positive physician visits. Different regressions are presented for persons in the two separate education groups and in three different geographic areas; means and standard deviations are shown in appendix table 1. Table 5.2 presents similar tobit regressions on the full sample of persons. For the reasons discussed above, the availability effect is most likely to be observed in the positive visits sample, and so these results will be discussed first.

Availability variables

The three availability variables are OBMD*, HBMD* and BEDS*. OBMD* has a significant and positive coefficient for the low education group in two out of three geographic areas. The size of the coefficient (and the elasticity) is much larger in the large urban areas than in the rural areas. For high education families, however, the coefficient on OBMD* is *negative* and significant for the large urban areas, and insignificant elsewhere. HBMD* tends to have the same sign as OBMD*, but to have a lower significance level. BEDS* is not usually significant. Application of an F-test for differences in the sets of coefficients across education groups finds that the set of coefficients on the two education groups is significantly different in all three area subsamples. In addition, the coefficients on OBMD* and HBMD* do differ significantly between the low and high education regressions in two of the three areas.

The remainder of the area variables are occasionally significant, but there is no consistent pattern. The proportion of physicians who are G.P.'s does not affect the ambulatory visit rate, while the proportion who are surgical specialists tends to depress the rate, but to a significant extent only for high education families. While none of the coefficients differs significantly across education groups, there is some weak evidence that surgeons may substitute away from ambulatory care toward hospital care. However, the results for the effect of surgeons on hospital use, to be presented later, do not confirm this suggestion. Another plausible explanation is that surgical specialists treat conditions with fewer ambulatory visits even when they do not substitute inpatient hospital stays. The physician's fee is not significant for high education families, but does have a significant and positive effect for the low education families in smaller metropolitan areas.

Other demand variables have generally expected coefficients. Both restricted activity days and number of chronic conditions are strongly related to the doctor visit rate. Older persons tend to make more visits as do females in general (in low education families) and females of childbearing age. Family size has a slight tendency to reduce the visit rate, while workers make fewer visits than nonworkers.

These results are consistent with the information manipulation theory of the availability effect. When an availability effect is observed, it is positive and significant for the low education families in two out of three areas. (The negative and significant effect of OBMD* for high education families in large cities may reflect a change in the quality or character of a visit when physicians are more plentiful.) There is also a *positive* and significant effect of price on use for low education families in the other

Table 5.1 Physician Visits Regressions: Persons with Some Physician Visits (ordinary least squares)

Independent Variable or Statistic	Regression Coefficients (*t* statistics in parentheses): 22 Largest SMSAs, Low Head Education	22 Largest SMSAs, High Head Education	Other SMSAs, Low Head Education	Other SMSAs, High Head Education	Nonmetropolitan Areas, Low Head Education	Nonmetropolitan Areas, High Head Education
RAD	.176	.280	.067	.212	.263	.370
	(4.15)	(4.69)	(1.59)	(3.90)	(6.93)	(6.54)
CONDS	2.21	1.81	1.60	1.63	1.60	1.50
	(17.4)	(12.3)	(13.8)	(13.8)	(15.7)	(12.83)
AGE LT 15	−.304	−.429	−.140	1.18	−.376	.304
	(−0.73)	(−0.97)	(−0.35)	(3.45)	(−1.07)	(0.83)
AGE 45–64	.986	.061	1.00	.172	1.06	.254
	(2.55)	(0.14)	(2.72)	(0.42)	(3.19)	(0.70)
AGE 65+	2.45	1.78	.722	1.83	.901	1.00
	(5.01)	(2.68)	(1.53)	(3.65)	(2.09)	(1.88)
SEX	.599	.229	.510	−.101	.451	−.455
	(2.16)	(0.70)	(1.93)	(−0.40)	(1.91)	(−1.74)
F 15–44	.636	.919	.849	1.51	1.20	1.38
	(1.39)	(1.90)	(1.97)	(4.02)	(3.10)	(3.48)
FAMSZ	−.066	−.064	−.052	−.190	−.071	−.230
	(−0.97)	(−0.88)	(−0.89)	(−3.39)	(−1.23)	(−3.66)
WORKING	−.712	−1.33	−.965	−.095	−.620	−1.05
	(−2.54)	(−4.14)	(−3.67)	(−0.60)	(−2.63)	(−3.89)
NOINS	.027	−.994	−.350	——	.517	——
	(0.08)	(−2.02)	(−1.26)	——	(2.19)	——
FAMINC	.001	−.004	.002	−.0001	.0003	−.001
	(0.30)	(−2.55)	(0.95)	(−0.03)	(0.27)	(−0.76)
GP/MD	3.03	−7.14	2.40	−.281	1.54	−1.89
	(0.56)	(−1.60)	(1.09)	(−0.12)	(1.37)	(−1.63)
SURG/MD	−7.00	−16.6	−5.89	−1.21	−.551	−4.63
	(−0.64)	(−1.66)	(−1.35)	(−0.28)	(−0.28)	(−2.09)
POPDENS	.019	.007	−.029	−.052	.173	−.053
	(1.09)	(0.38)	(−0.95)	(−2.12)	(2.62)	(−0.76)
MDFEE	−.125	.126	.178	.009	−.043	.010
	(−0.80)	(0.73)	(2.59)	(0.15)	(−0.28)	(1.14)

Table 5.1—*continued*

Independent Variable or Statistic	22 Largest SMSAs: Low Head Education	22 Largest SMSAs: High Head Education	Other SMSAs: Low Head Education	Other SMSAs: High Head Education	Nonmetropolitan Areas: Low Head Education	Nonmetropolitan Areas: High Head Education
	Regression Coefficients (*t* statistics in parentheses)					
HOSCOST	−.025	.019	−.019	−.003	.025	.045
	(1.79)	(1.28)	(−1.74)	(−0.42)	(2.56)	(0.78)
BEDS*	−2.29	−.370	−1.13	−2.13	−.968	.611
	(−0.87)	(−0.15)	(−0.95)	(−2.12)	(2.62)	(1.04)
HBMD*	14.0	−18.3	4.89	−.945	3.59	−7.93
	(1.59)	(−1.93)	(0.88)	(−.197)	(1.16)	(−2.88)
OBMD*	32.0	−25.8	−.902	3.55	5.79	.771
	(3.02)	(−2.44)	(−.134)	(0.56)	(2.53)	(0.62)
Constant	4.38	12.18	4.96	4.59	.523	5.31
	(0.65)	(2.22)	(1.73)	(1.78)	(0.13)	(3.92)
$\bar{R}^2$	.169	.140	.123	.101	.173	.131
n	3461	2189	2545	3184	3128	2562
F statistic for hypothesis of inequality of regression coefficients. ($F_{.01}$ [19, large *n*] = 1.88.)	2.38		2.46		2.85	

NOTE: Means and standard deviations are shown in appendix table 2.

Table 5.2 **Physician Visits Regressions: All Persons (Tobit Regressions)**

Independent Variable or Statistic	22 Largest SMSAs: Low Head Education	22 Largest SMSAs: High Head Education	Other SMSAs: Low Head Education	Other SMSAs: High Head Education	Nonmetropolitan Areas: Low Head Education	Nonmetropolitan Areas: High Head Education
	Regression Coefficients (*t* statistics in parentheses)					
RAD	.506	.488	.349	.404	.566	.566
	(8.51)	(6.45)	(7.49)	(4.15)	(9.02)	(7.27)
CONDS	4.05	3.32	2.82	2.76	3.44	2.81
	(21.6)	(15.4)	(19.5)	(13.4)	(19.0)	(15.5)
AGE LT 15	1.59	1.37	.082	1.76	.954	1.69
	(2.90)	(2.50)	(0.19)	(3.06)	(1.73)	(3.48)
AGE 45–64	0.55	0.66	.091	.015	.129	.263
	(1.10)	(1.26)	(0.23)	(0.03)	(0.25)	(0.55)
AGE 65+	1.72	.825	−.464	1.16	−.072	.052
	(2.70)	(1.00)	(−0.92)	(1.39)	(−0.11)	(0.07)
SEX	.748	.093	1.12	.197	.770	−.777
	(2.05)	(0.23)	(3.94)	(0.46)	(2.07)	(−2.21)
F 15–44	2.05	2.70	.395	1.93	2.67	2.60
	(3.43)	(4.47)	(0.85)	(3.05)	(4.39)	(4.88)

Table 5.2—*continued*

Independent Variable or Statistic	22 Largest SMSAs		Other SMSAs		Nonmetropolitan Areas	
	Low Head Education	High Head Education	Low Head Education	High Head Education	Low Head Education	High Head Education
	Regression Coefficients (*t* statistics in parentheses)					
FAMSZ	−.332	−.262	−.209	−.261	−.257	−.331
	(−3.62)	(−2.74)	(−2.84)	(−2.76)	(−2.89)	(−3.76)
WORKING	−.173	−1.58	−.811	−.213	−.304	−1.04
	(−0.47)	(−3.83)	(−2.83)	(−0.49)	(−0.81)	(−2.85)
NOINS	−.219	−1.76	−.121	——	−1.59	.549
	(−0.55)	(−3.19)	(−0.39)	——	(−4.31)	(2.05)
FAMINC	.011	.000	.009	.003	.002	−.002
	(3.44)	(0.04)	(3.71)	(1.15)	(0.50)	(−0.98)
GP/MD	.800	−7.78	1.60	−2.89	−1.09	−.481
	(0.12)	(−1.40)	(0.66)	(−0.80)	(−0.60)	(−0.30)
SURG/MD	−6.09	−8.81	−3.60	−3.51	−5.71	−.612
	(−0.43)	(−0.71)	(−0.75)	(−0.49)	(−1.85)	(−0.20)
POPDENS	.042	.023	.011	−.067	−.008	−.119
	(1.83)	(0.87)	(0.35)	(−1.70)	(−0.94)	(−1.69)
MDFEE	−.363	−.327	.047	−.116	−.004	.143
	(−1.80)	(−1.51)	(0.53)	(−1.16)	(−0.31)	(1.95)
HOSCOST	−.066	.018	−.010	.016	.005	.006
	(−3.57)	(0.97)	(−0.93)	(1.08)	(2.82)	(0.51)
BEDS*	−8.87	−5.98	−.059	−1.34	−.525	.705
	(2.63)	(−2.07)	(−0.05)	(0.78)	(−0.45)	(0.90)
HBMD*	30.7	10.15	2.40	2.73	−3.20	−9.04
	(2.62)	(0.85)	(0.39)	(0.14)	(−0.65)	(−2.38)
OBMD*	45.7	−3.79	6.78	−.023	2.21	−.754
	(3.31)	(0.28)	(0.95)	(−0.00)	(0.26)	(−0.46)
Constant	6.06	8.15	−.225	2.81	−2.56	1.49
	(0.70)	(1.18)	(−0.07)	(0.67)	(−1.14)	(0.50)
Significance of chi-square statistic	.000	.000	.000	.000	.000	.000
n	5005	2693	3864	3950	4927	3323

NOTE: Means and standard deviations are shown in appendix table 1.

metropolitan areas, again offering evidence for demand creation (but against the naive target income theory).[19]

It is also worth noting that, although there is an availability effect for low education families in rural areas, that effect is much smaller than in large urban areas. Since rural areas are supposed to be areas of greatest physician "shortage," one could conclude either that the shortage is relatively mild, or that the potential for demand manipulation in such areas is limited by better consumer information on the accuracy of information provided by individual physicians.

The results of tobit analysis are shown in table 5.2. While these results are not conclusive, and while there is no precise way to test the hypothesis, the results are consistent with the notion that the effect of physician availability on whether or not people ever see a physician in a year can be different from the effect on the number of visits they make, given that they make at least one visit. While many of the coefficients are similar in sign, the tobit analysis does yield results for the high education/large urban group which are quite different from those for the persons in the same group with positive doctor visits. The insignificant coefficient of OBMD* in the tobit regressions may be a composite of a significant negative coefficient for those with positive visits and a positive effect on the probability of seeing a physician at all. (It might be possible to develop a statistical test, analogous to an F-test, to determine whether the structures of the OLS and tobit samples are different.) The availability variables in the other regressions tend to have similar signs in OLS and tobit specifications, as do the personal characteristic variables.

Because a large availability effect was detected for the large urban subsample, additional analysis was performed on it. First, variables measured in money were deflated by a cost-of-living index, with little change in results (table 5.3). Absence of such deflators for the other two area subsamples is therefore unlikely to affect results. Next, area-wide demand variables were added to the regressions. The variables added were (1) income per capita, (2) percentage of population over

Table 5.3 **Physician Visit Regressions Using Deflated Money Variables, Area Demand Variables, and Alternative Price Measures: 22 Largest SMSAs, Persons with Some Visits**

Independent Variable or Statistic	Money Variables Deflated by Cost-of-living Index		Money Variables Deflated and Area Demand Variables Added		Mathematica Fee Estimates Used	
	Low Head Education	High Head Education	Low Head Education	High Head Education	Low Head Education	High Head Education
	Regression Coefficients (t statistics in parentheses)					
RAD	.257	.407	.253	.412	.256	.407
	(6.32)	(7.21)	(6.13)	(7.20)	(6.29)	(7.20)
CONDS	2.36	2.15	2.39	2.18	2.36	2.15
	(17.7)	(13.1)	(17.6)	(13.1)	(17.7)	(13.1)
AGE LT 15	−.159	−.163	−.343	−.069	−.163	−.174
	(−0.39)	(−0.37)	(−0.82)	(−0.16)	(−0.40)	(−0.40)
AGE 45–64	.807	−.007	.787	.065	.816	−.011
	(2.09)	(−0.02)	(2.01)	(0.15)	(2.11)	(−0.02)
AGE 65+	2.11	1.41	2.05	1.53	2.12	1.39
	(4.33)	(2.14)	(4.13)	(2.26)	(4.35)	(2.11)

Table 5.3—*continued*

Independent Variable or Statistic	Money Variables Deflated by Cost-of-living Index		Money Variables Deflated and Area Demand Variables Added		Mathematica Fee Estimates Used	
	Low Head Education	High Head Education	Low Head Education	High Head Education	Low Head Education	High Head Education
	Regression Coefficients (t statistics in parentheses)					
SEX	.593	.259	.659	.272	.592	.263
	(2.15)	(0.80)	(2.35)	(0.82)	(2.14)	(0.80)
F 15–44	.671	.889	.607	.873	.672	.880
	(1.46)	(1.84)	(1.32)	(1.78)	(1.47)	(1.82)
FAMSZ	−.100	−.090	−.080	−.079	−.098	−.087
	(−1.14)	(−1.17)	(−1.11)	(−1.00)	(−1.37)	(−1.13)
WORKING	−.848	−1.30	−.964	−1.17	−.857	−1.30
	(−3.02)	(−3.43)	(−3.39)	(−3.49)	(−3.06)	(−3.98)
NOINS	.028	−1.05	.037	−1.04	.014	−1.03
	(0.09)	(−2.32)	(0.12)	(−2.23)	(0.05)	(−2.29)
FAMINC	.002	−.004	.001	−.005	.002	−.004
	(0.70)	(−2.17)	(0.49)	(−2.37)	(0.20)	(−2.21)
GP/MD	7.30	−8.99	7.89	−14.3	5.98	−8.30
	(1.63)	(−2.16)	(1.43)	(−2.74)	(1.38)	(−1.95)
SURG/MD	3.20	−19.6	−1.22	−33.4	−1.87	−17.8
	(0.34)	(−0.78)	(−0.08)	(−2.36)	(−0.18)	(−1.79)
POPDENS	.009	−.006	.054	−.063	−.002	−.002
	(0.50)	(−0.31)	(1.61)	(−1.60)	(−0.10)	(−0.10)
MDFEE	−.040	.171	−.118	.445	.154	.103
	(−0.20)	(0.78)	(−0.49)	(1.65)	(0.54)	(0.34)
HOSCOST	−.024	.028	−.056	.034	−.030	.019
	(−1.46)	(1.49)	(−2.24)	(1.40)	(−1.83)	(1.20)
BEDS*	−1.60	.638	−1.69	1.79	−.215	−.241
	(−0.56)	(0.24)	(−0.43)	(0.50)	(−0.06)	(−0.08)
HBMD*	10.9	−14.9	8.80	−18.2	16.7	−15.4
	(1.23)	(−1.65)	(0.80)	(−1.56)	(1.65)	(−1.46)
OBMD*	34.6	−26.0	28.6	−29.2	30.5	−25.5
	(3.18)	(−2.13)	(2.08)	(−2.23)	(2.51)	(−1.96)
Inc. Per Cap.			−.001	−.001		
			(−1.02)	(−0.84)		
% over 65			−.163	−.256		
			(−0.75)	(−1.07)		
% with insurance (in state)			−.067	.044		
			(−1.69)	(1.03)		
Constant	−1.00	11.1	14.4	16.6	−.334	11.8
	(−0.17)	(2.13)	(1.06)	(1.23)	(−0.17)	(2.13)
$\bar{R}^2$	.173	.146	.177	.147	.173	.146
n*	3461	2189	3311	2126	3461	2189

*Because of the absence of data for some areas in which the SMSA crosses state boundaries, the sample size is slightly reduced for the regressions with area demand variables.

65, and (3) percentage of population with some health insurance. As shown in table 5.3, this addition decreased the estimated magnitude of the availability effect, but the changes were small. Coefficients on all of the areawide variables were negative, as expected, although the only significant coefficient was for areawide family income in the high education subsample. It does not appear that omission of such variables makes a substantial difference, and an F-test indicates that the set of area demand variables does not significantly increase the explanatory power of the regression. Finally, use of the possibly more accurate prices from the Mathematica survey yielded approximately the same coefficients as the use of the other price measure.

Because of the large positive availability effect for the low education/large urban subsample, that subsample was further disaggregated to examine alternative theories of the availability effect. The information-manipulation theory is not refuted by the chronic/nonchronic illness distinction. As shown in table 5.4, the increase in use in response to more physicians is obviously not confined to, nor is it larger for, those with zero chronic conditions. Of course, we cannot be sure that those with chronic conditions are really more severely ill, or that their visits are more desired by physicians, although Friedman offers evidence to suggest that they are.[20] It is also possible, as suggested by Grossman and Rand, that time costs are greater for the chronically ill because they are more severely disabled.[21]

The comparison between the effect on working and nonworking adults fails to support the time cost theory (table 5.4). The coefficients and elasticities on OBMD* are not statistically different between those who work and those who do not. There is no evidence that the response of working adults to a reduction in the availability of physicians (as a proxy for time cost) is greater than that of nonworking adults.

Is the Availability Effect Important for Ambulatory Care?

Determining the reason for any observed availability effect was the primary purpose of this study. However, the results appear to be consistent not only with the hypothesis that the response of use to physician availability differs across education groups, but even with the hypothesis that the effect is important only for low education families.

In order to get an idea of the overall importance of the availability effect for ambulatory care, persons with other levels of education were added to the sample and the regressions rerun. In principle, it would have been preferable to permit interactions between education or location and all of the independent variables, but the large sample size made

Table 5.4 **Physician Visits Regressions, Persons with Some Visits in Households with Low Head Education: 22 Largest SMSAs, Selected Subsamples**

Independent Variable or Statistic	Regression Coefficients (t statistics in parentheses)			
	Number of Chronic Conditions		Employment Status	
	Zero Chronic Conditions	Some Chronic Conditions	Working Adults	Nonworking Adults
RAD	.143	.415	.393	.301
	(2.34)	(4.12)	(2.75)	(4.08)
CONDS	—	2.82	2.51	3.16
	—	(6.03)	(7.36)	(11.26)
AGE LT 15	.473	1.06	—	—
	(1.18)	(0.64)	—	—
AGE 45–64	.801	2.05	.546	1.71
	(2.07)	(1.52)	(0.73)	(1.42)
AGE 65+	2.62	3.33	3.32	2.37
	(4.91)	(2.10)	(2.53)	(1.95)
SEX	.243	.587	.581	1.176
	(0.85)	(0.65)	(0.77)	(1.46)
F 15–44	1.44	1.89	1.48	.821
	(3.17)	(1.18)	(1.39)	(0.66)
FAMSZ	−.064	.059	−.053	.195
	(0.82)	(0.20)	(−0.08)	(0.87)
WORKING	−.153	−1.080	—	—
	(0.52)	(−1.20)	—	—
NOINS	−.429	−1.94	.360	−.869
	(−1.39)	(1.76)	(0.44)	(−1.06)
FAMINC	−.00002	.00023	.0001	−.00001
	(1.07)	(2.70)	(1.81)	(−0.17)
GP/MD	−4.41	−2.61	−3.21	−3.21
	(−0.86)	(−0.15)	(−0.26)	(−0.24)
SURG/MD	−12.40	−10.51	8.23	−29.11
	(−1.17)	(−0.30)	(−0.33)	(−1.07)
POPDENS	.016	.018	.014	.019
	(0.93)	(0.97)	(0.81)	(1.14)
MDFEE	−.329	−1.35	−1.05	−.889
	(1.36)	(−1.54)	(−1.62)	(−1.38)
HOSCOST	.007	−.105	−.057	−.006
	(0.47)	(−2.22)	(−1.62)	(−0.18)
BEDS*	.775	−19.45	−16.29	−2.95
	(0.26)	(−2.11)	(−2.37)	(−0.41)
HBMD*	−13.78	31.09	−9.66	24.31
	(−1.49)	(1.01)	(−0.43)	(1.05)
OBMD*	21.48	91.89	43.83	52.82
	(1.92)	(2.53)	(1.68)	(1.82)
Constant	7.31	21.33	15.20	13.00
	(0.21)	(1.67)	(1.42)	(1.30)
$\bar{R}^2$	.041	.088	.269	.187
n	2096	1365	1359	1171
Visits Per Capita	3.67	8.97	5.00	7.55

Table 5.5 **Physician Visits Regressions, Aggregated Samples: Persons with Some Physician Visits (OLS)**

Independent Statistic Variable or	Regression Coefficients (t statistics in parentheses) 22 Largest SMSAs	Other SMSAs	Nonmetropolitan Areas	All Areas
RAD	.196	.219	.274	.231
	(7.36)	(8.95)	(11.1)	(16.0)
CONDS	1.58	2.01	1.47	1.70
	(24.9)	(30.4)	(24.1)	(46.0)
AGE LT 15	.505	−.033	−.057	.128
	(2.64)	(−0.16)	(−0.29)	(1.11)
AGE 45–64	.704	.786	.737	.729
	(3.75)	(4.01)	(2.89)	(6.55)
AGE 65+	1.05	1.73	1.03	1.30
	(3.89)	(6.18)	(3.89)	(8.23)
SEX	.068	.172	−.004	.085
	(0.49)	(1.16)	(−0.03)	(1.07)
F 15–44	1.29	1.35	1.10	1.25
	(6.17)	(6.00)	(5.14)	(9.94)
FAMSZ	−.163	−.089	−.125	−.128
	(−5.73)	(−2.69)	(−3.93)	(−6.82)
WORKING	−.601	−.724	−.763	−.711
	(−4.36)	(−4.92)	(−5.49)	(−8.64)
NOINS	.269	−.166	−.096	.002
	(1.45)	(−0.87)	(−0.59)	(0.02)
FAMINC	.0006	−.002	−.0003	−.0007
	(0.63)	(−2.17)	(−0.26)	(−1.18)
GP/MD	.164	−2.74	.301	−.049
	(0.15)	(−1.19)	(0.49)	(−0.09)
SURG/MD	−4.05	−14.13	−1.50	−2.34
	(−1.82)	(−2.08)	(−1.39)	(−2.41)
POPDENS	−.033	.013	.100	.0005
	(−2.37)	(1.50)	(2.60)	(0.11)
MDFEE	.101	−.051	−.019	.063
	(2.90)	(−0.67)	(−0.50)	(2.63)
HOSCOST	−.004	−.004	.016	.007
	(−0.86)	(−0.63)	(3.17)	(2.27)
BEDS*	−1.37	−2.66	.128	−.267
	(−2.25)	(−2.31)	(0.39)	(−0.97)
HBMD*	−.540	−3.20	−.275	−1.73
	(−0.19)	(−0.71)	(−0.16)	(−1.33)
OBMD*	.700	.293	.981	1.42
	(0.22)	(0.05)	(1.10)	(1.66)
MIDED	−.027	.010	.008	−.018
	(−0.20)	(0.06)	(0.05)	(−0.21)
LOED	.088	.263	.035	.120
	(0.57)	(1.47)	(0.22)	(1.27)
SMMET	—	—	—	−.081
	—	—	—	(−0.20)

Table 5.5—*continued*

Independent Statistic Variable or	Regression Coefficients (t statistics in parentheses) 22 Largest SMSAs	Other SMSAs	Nonmetropolitan Areas	All Areas
RURAL	—	—	—	−.112
	—	—	—	(−0.67)
Constant	4.68	10.39	2.79	3.11
	(3.36)	(3.53)	(3.65)	(4.92)
$\bar{R}^2$	.113	.147	.129	.131
n	9736	11194	9427	30357

such a procedure prohibitively costly. Dummy variables for education of head and/or location were included in the regressions.

As indicated in table 5.5, physician stock has a positive but statistically insignificant effect in each of the three location subsamples even when all education groups are combined. When data are combined for all variables and all education levels, one does find that the coefficient on OBMD* is positive and significant at the 90% level. Even here the coefficient is numerically quite small. With such a large sample, statistical significance is usually to be expected. Moreover, the F-tests described above suggest that it is not proper to combine the subsamples and constrain coefficients to be equal across subsamples. Accordingly, it seems appropriate to conclude that a positive availability effect for persons with positive physician visits is difficult to detect, quite small in magnitude if it is found, and may be due only to specification error.[22]

For ambulatory visits, then, there appears to be little or no availability effect when other variables (including health measures) are properly controlled. It seems safe to conclude that, for the general population, the availability effect in ambulatory care can safely be ignored.