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UNCONDITIONAL DEMAND FOR CURATIVE HEALTH INPUTS: DOES SELECTION ON HEALTH STATUS MATTER IN THE LONG RUN?

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Abstract

Healthy people are routinely ignored when analyzing curative health inputs. This practice overlooks people's long-term ability to affect their chances of falling sick, and may have perverse effects on welfare analyses. A dynamic model implies that input demand estimates conditioned on current illness can only be interpreted as *short run* effects, in contrast to the long-run nature of unconditional estimates. In addition, conditional estimates may be biased from both sample-selection, and self-reporting of health status. By jointly modeling discrete choices for health inputs and health outcomes, a test for selection bias is derived using the multinomial probit. In data from Cote d'Ivoire, it is found that the usual short-run demand estimates do not suffer from selection bias. However, these conditional estimates differ from the easily estimated long-run unconditional effects, which are often the more relevant policy parameters.

Key words: Demand for Health Inputs, Sample Selection on Health Status

SECTION 1. INTRODUCTION

Curative health care is only demanded by people when they are sick. This simple observation has led many empirical workers and policy analysts to ignore "healthy" people when evaluating the determinants of demand for health care and the effects of health care policy changes. Individuals, however, can in the long run adjust health input mixes to affect the probability of becoming "sick." This indicates that commonly estimated quantities such as price elasticities may be biased, or else only valid in the "short run," if estimation is conditioned on health status. This paper fills a gap in the health care demand literature by analyzing the economic and empirical implications of such conditional[1] estimation.

Economically, Ellis and Mwabu (1991) have shown that demographic characteristics may affect both demand once sick, and who becomes sick. Section 2 presents a simple dynamic economic model to formalize this dual effect of user fees, and it implies that unconditional estimation better captures the long-run effects of policy reform. The relationship between short-run and long-run elasticities is then discussed. It is shown that the difference between the conditional and unconditional demand elasticities is exactly equal to the elasticity of sickness.
Section 3 analyzes the possible statistical bias (hypothesized by Schultz and Tansel, 1993) in conditioning on health to obtain the usual "short-run" elasticities. This bias is illustrated using discrete choice modeling concepts, and economic explanations of

[1]"Unconditional" and "Conditional" are used throughout this paper to refer to whether or not the sample has been truncated to exclude healthy people.

its existence are discussed. Unbiased estimation strategies are proposed and evaluated, including unconditional methods which combine the "sick but zero demand" option with a "not sick" alternative, to create a single non-demander's category. By not jointly estimating demand with health, the resulting unconditional estimates are free of selection bias problems. Finally, a formal test of selection bias is derived using the multinomial probit.

Empirically, Section 4 finds that in Cote d'Ivoire data the conditional estimates do differ from the unconditional population effects. Short-run price elasticities are approximately 25% larger than for the unconditional sample, and the signs and significance of gender, age, and wage variables also differ between the approaches. However, as estimates of the short-run elasticities, the usual conditional results do not appear to be affected by selection bias in this sample.

The implications of the findings are discussed in Section 5. For short-run analysis, if selection bias is also absent in other datasets, then collecting less-expensive choice-based samples appears to be a valid option. Interpretation may still be difficult, though, when self-reported health indicators are used to select the sample. This must be balanced against the fact that estimated long-run effects may also be difficult to interpret when changes over time in health care access are unobserved. However, unconditional estimation requires little extra computational effort when data is collected for the healthy also. Attention should be paid in the future to whether it is the short-run or long-run effects that are actually desired in a particular application.

SECTION 2: ECONOMIC INTERPRETATION OF (UN)CONDITIONAL ESTIMATES

2.1 Literature Review

Most empirical studies of the discrete demand for curative medical care condition estimation on individuals having reported themselves sick, especially in studies of low income populations. Recent examples include Akin et al. (1985), Gertler and van der Gaag (1990), Lavy and Quigley (1993), and Mwabu, Ainsworth, and Nyamete (1993). Akin et al. justify this by arguing that healthy people will not demand curative care. It is argued here, however, that this is only true in the short run. The possibility of selectivity bias was mentioned by Dor and van der Gaag (1987,1993), who report testing to see if selection on sickness was a severe problem. To do this they used a Heckman-type selectivity correction; Section 3, however, discusses why this will not necessarily be the preferred test.

Not all health care demand studies condition in this way. In studies of the United States, for example, Manning et al. (1987) retain healthy people in the analysis, although a different type of selection issue arises in their study. In general, however, consideration of the role of healthy people in curative care demand has received little attention.

Ellis and Mwabu (1991) provide a notable exception to this literature, as they focus attention on the effects of demographic[2] covariates on the probability of falling ill, as well as on demand behavior once ill. However, they do not go on to calculate the net combined ("unconditional") effects. Furthermore, they do not discuss or allow for in their estimation any possible correlation between unobservables in the health demand and the health care demand equations. As Schultz and Tansel (1993) point out in a footnote, if the unobservables in these two equations are correlated, then the conditional estimates will be biased.

2.2: A Simple Dynamic Model for Health Inputs

This section provides a simple behavioral model of health care demand decisions. It emphasizes the idea that people *choose* their expected health level, and in the process, future health

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[2]Strauss et al. (1993) also provide a detailed analysis of how reported illness differs across age and gender.

outcomes are affected by current health care prices. Furthermore, dynamic feedbacks through health status imply potential differences between short-run and long-run health care demand responses to price changes.

Since Grossman's (1972) contribution, numerous models have been formulated to emphasize different aspects of the health production process. These have been reviewed in the developing country context by Akin (1985), Behrman and Deolalikar (1988), and Strauss and Thomas (1994). The distinctive feature of this model is the focus on the tradeoffs between curative and preventive inputs, in addition to the usual tradeoffs between health and other consumption. This highlights different pathways through which health care prices may effect health care consumption. The main implication for empirically estimating input demands is that health itself is endogenous, and the level of healthiness in the population will change when health care prices change. This in turn implies different temporal interpretations of different estimation strategies, as discussed in Section 2.3.

In the model, individuals at the beginning of period t choose consumption C_t and health H_t (through curative I_t^{cur} and preventive I_t^{pre} health inputs) to maximize the discounted (by ρ) sum of expected current and future utilities. Maximization is subject to each period's budget constraint, which depends on income Y_t and prices p_t^{cur} and p_t^{pre} (the price of other consumption may be normalized to one):

$$\max \sum_t \rho^t E[U(C_t, H_t)]$$

$$s.t. \quad Y_t = C_t + I_t^{cur} p_t^{cur} + I_t^{pre} p_t^{pre}$$

Maximization is also constrained by the production technology for health at time t. The time subscript on health denotes the health status at the end of each period. At the beginning of each period health is altered by a stochastic shock λ_t, before health inputs are chosen. The affect of past health on current health is also altered by depreciation δ of the health stock (H increases with good health):

$$H_t = \delta H_{t-1} + \beta_1 I_t^{cur} + \beta_2 I_{t-1}^{pre} + \lambda_t$$

In this system, health at period T may depend on all previous shocks and inputs. This can be seen by assuming $H_0 = \beta_1 I_0^{cur} + \lambda_0$ and solving forward:

$$H_T = \sum_{t=0}^{T} \delta^{T-t}\beta_1 I_t^{cur} + \sum_{t=0}^{T-1} \delta^{T-t-1}\beta_2 I_t^{pre} + \sum_{t=0}^{T} \delta^{T-t}\lambda_t$$

Prices in all past periods may thus also affect health at T, through the input demand equations. To explicitly analyze the price effects, specify simple linear input demand equations:

$$I_t^{cur} = \gamma_1^c p_t^{cur} + \gamma_2^c p_t^{pre} + \gamma_3^c(\delta H_{t-1} + \lambda_t)$$

$$I_t^{pre} = \gamma_1^p p_t^{cur} + \gamma_2^p p_t^{pre} + \gamma_3^p(\delta H_{t-1} + \lambda_t)$$

Then substitute them into the health production function, yielding

$$H_t = \alpha_0 H_{t-1} + \alpha_1 p_t^{cur} + \alpha_2 p_t^{pre} + \alpha_3 \lambda_t$$

where $\alpha_1 = (\beta_1\gamma_1^c + \beta_2\gamma_1^p)$, etc.

The effect of a permanent change at time T in p^{cur} can now be written as:

$$\frac{dH_T}{dp_T^{cur}} = \alpha_1$$

By period T+s this differs from the initial effect in period T, since there is a cumulative effect of the new H_T affecting H_{T+1} (which then affects I_{T+1}^{cur}, etc.):

$$\frac{dH_{T+s}}{dp_T^{cur}} = \sum_{t=0}^{s} \alpha_0^t \alpha_1$$

Next, see how this price change affects the demand equations. In the first period there is an immediate impact effect through the budget constraint:

$$\frac{dI_T^{cur}}{dp_T^{cur}} = \gamma_1^c$$

This is expected to be negative. However, again there is a dynamic feedback effect by period T+1, through health:

$$\frac{dI_{T+1}^{cur}}{dp_T^{cur}} = \gamma_1^c + \delta\gamma_0^c(\beta_1\gamma_1^c + \beta_2\gamma_1^p)$$

This effect can be decomposed into several components. The first term is the impact effect γ_1^c as seen in period T. The second is the feedback through H_t, from T's price change affecting both curative and preventive inputs in time T, and thus changing the health status at the beginning of the period when I_{t+1} is chosen. The total effect, taking into account the feedback, is analogous to dynamic multipliers in macroeconomic models.

Whether the dynamic feedback amplifies or dampens the impact (short-run) effect is ambiguous. Of the dynamic term in parentheses, the first part ($\beta_1\gamma_1^c$) is positive, since curative care decreased last period, meaning health is worse now, thus more curative care is demanded (assuming curative care is a normal good). The second effect cannot be signed, however, because it depends on the cross-price substitution effect of curative price on preventive care.

For example, people might spend more resources on preventing illness when curative costs are higher. Even if period T's ill health is not as well cured, new illnesses could then be prevented so much so as to improve health. In that case, curative care demand would decrease even more over time than it had in the first period, in response to the price change. However, if the increased preventive care is not sufficient to offset the effect of decreased curative care in the previous period, implying worse health at the beginning of period T+1, then the long-run elasticity will be smaller than the short-run elasticity.

One case where the long-run elasticity may be more likely to be smaller than the short-run is if preventive prices increase along with curative prices. This may occur when the "price" referred to is actually travel time to a clinic which provides both types of care. Then preventive care is not as attractive a substitute for curative care, making it more likely that health will worsen over time and thus drive demand levels back towards their original levels.

The relationship between the long-run and short-run elasticities is further complicated by the fact that it may change as the price change becomes more distant in the past. This is seen by the effect on demand in period T+s of a permanent price change in period T:

$$\frac{dI_{T+s}}{dp_T^{cur}} = \gamma_1^c + \delta\gamma_0^c \sum_{t=0}^{s-1} \alpha_0^t \alpha_1$$

For many goods, demand is thought to be more elastic in the long run, as people are better able to substitute to alternative goods such as preventive care. In the present model the effect of the preventive feedback may be stronger in the long run, due to substitution, but it cannot be determined a priori whether the long-run health care demand elasticity will also be larger than in the short-run.

To summarize the implications of this model, write the long-run demand effect of a curative care price change more generally:

$$\frac{dI_{T+s}^{cur}}{dp_T^{cur}} = \frac{dI_{T+s}^{cur}}{dp_{T+s}^{cur}} + \frac{dI_{T+s}^{cur}}{dH_{T+s-1}} \frac{dH_{T+s-1}}{dp_T^{cur}}$$

It is clearly seen here to equal the sum of the short-run negative demand effect (holding health constant) plus the a priori unsignable feedback through health.

2.3: Relating the Model to Conditional and Unconditional Demand

To understand the implications of the previous model for interpreting conditional and unconditional discrete choice demand, consider the following simple econometric equations. Let S_t be a sickness indicator variable equal to one when an individual reports their health to have fallen below a threshold in the current period, such that they are "sick":

S_t = 1 if report sick
S_t = 0 otherwise

Similarly, let M_t be a medical care indicator equal to one if curative care was demanded in the current period:

$M_t = 1$ if $I_t^{cur}>0$
M_t = 0 otherwise

Let X represent a vector of observables assumed exogenous[3], p represent the price of curative medical care, and υ_S and υ_M represent unobserved residuals. Then

(1) $S_t = S(p,X) + \upsilon_S$
(2) $M_t = M(p,X) + \upsilon_M$
(3) $[M_t | (S_t=1)] = M^S(p,X) + \upsilon_{M|S}$

Assume for this section that $Cov(\upsilon_S,\upsilon_M)=0$, or else a non-zero

[3]Exogeneity will be difficult to determine for variables such as income. Health is another variable which is often included as an exogenous covariate. While health indicators may vastly improve predictive power for medical care demand, the model in Section 2.2 should caution against including them due to potential endogeneity.

correlation has been taken into account in the conditional estimates, methods for which are described in Section 3.

Clearly, the population probability of seeking medical care $E[M_t]$ differs from the conditional expectation $E[M_t|S_t=1]$. To see this, re-write $E[M_t]$ as:

$$(4) \quad E[M_t] = E[M_t|S_t=1]*E[S_t=1] + E[M_t|S_t=0]*E[S_t=0]$$
$$= E[M_t|S_t=1]*E[S_t=1]$$

The last equality relies on the fact that if M is defined as curative care, then $E[M_t|S_t=0]=0$. This also shows that the effects of the covariates on conditional medical care demand will differ from those on the unconditional. By substituting the second line of (4) into the elasticity formula for prices, the effects can be separated into distinct components:

$$(5) \quad \frac{dE[M_t]}{dp}\frac{p}{E[M_t]} = \frac{dE[M_t|S_t=1]}{dp}\frac{p}{E[M_t|S_t=1]} + \frac{dE[S_t]}{dp}\frac{p}{E[S_t]}$$

This indicates that the unconditional elasticity of curative care demand equals the sum of the conditional elasticity of curative care demand plus the elasticity of health with respect to the covariate, or $\mathfrak{E}^M = \mathfrak{E}^{M|S} + \mathfrak{E}^S$.

This allows easy interpretation of the different estimates. The model in the previous section highlights that current health is affected by past inputs, which in turn will be affected by covariates such as prices. Equation (1) can thus be interpreted as a reduced form health demand equation. By taking into account this feedback effect through the probability of sickness, the unconditional estimates from equation (2) can then be interpreted as measuring *long-run* effects of changes in covariates. In contrast, the conditional estimates from (3) give only the *short-run* impacts, before healthiness has been affected by price changes.

SECTION 3: ECONOMETRIC IMPLEMENTATION AND SELECTION BIAS

The analysis thus far has assumed that the short-run and long-run responses were both correctly estimated. As alluded to above, however, direct estimates from the conditional sample may suffer from selection bias if health is correlated with the error term of the health care demand equation. This section first discusses intuitively why such a correlation may arise. It then reviews the assumptions about unobserved correlations that are implicit in the nested multinomial logit which is typically used. Next, it outlines methods for using discrete choice models for unbiased estimates of conditional and unconditional demand. Finally, tests for selection bias in conditional estimates are discussed, including the use of the multinomial probit for a nested testing strategy.

A key idea relied on in the econometric modeling below is that in addition to the usual discrete alternatives when sick, the state of "not sick" can be considered as another discrete choice. This interpretation arises from the model in Section 2.2, where people choose their probability of sickness in a given period by adjusting prior inputs. Because people optimize dynamically, this sickness probability is chosen jointly with expected curative care demand levels when sickness does occur. Thus it is analytically reasonable to define joint health and health care demand error distributions on the entire population. This avoids the difficulties of sequential selection rules discussed by Lee and Maddala (1985), and simplifies unconditional estimation, as shown below.

3.1: Intuition Behind the Bias of the Conditional Approach

One reason for concern over conditional estimates is that often health indicators used to condition the sample are *self-reported*. Sindelar and Thomas (1992) find that such self-reported morbidity may only be tenuously related to objective measures. In the Cote d'Ivoire sample used in Section 4, the probability of reporting a sickness actually *increases* with income. It is speculated that this phenomenon is largely a result of lower income persons tolerating higher levels of discomfort before declaring themselves "sick." Anecdotal accounts depict chronically ill poor people in developing countries who often suffer from malaria while attempting to continue with their work. If this is perceived as normal, then they may not report malaria as an illness. Wealthier people, on the other hand, may be able to purchase antibiotics to treat the malaria, and would then classify themselves as sick, given the same objective illness level.

If such reporting biases were solely correlated with an observable such as income, then that by itself would not lead to biased estimates of *short-run* price effects. However, more generally the problem is one of unobserved attitudes towards both illness and care-seeking behavior. This hypothesis is supported by Wolfe and Behrman (1984), who find that "women's childhood backgrounds affect both their adult health and health-care utilization." Such attitudes may be rooted in one's personality, and be unlikely to change as income changes. If so, then income marginal effects estimated from conditional cross-sections will be biased.

Furthermore, it may be that those who tend to under-report illness also tend to avoid modern health care even when sick. Or, a person with an unobservably poor health endowment may become accustomed to the medical care system and be more likely to demand care when sick. Conversely, those who tend to avoid health care (due to unobserved preferences) may be more likely to be sick currently as a result of neglecting to get care in the past.

There are numerous potential reasons to suspect correlation between the health and health care demand residuals[4].

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[4]Correlation may also arise if the data on morbidity and care demand are contemporaneous. Often people are asked how many days they were sick in a recall period, and then if they sought care in that same recall period. However, this should not induce an

Unfortunately, because several effects may work in opposite directions, no clear prediction can be made for the net sign of the correlation.

3.2: Unobserved Correlations in the Nested Multinomial Logit

The treatment of correlation between sickness and demand unobservables in nested multinomial logit models is reviewed here, based on established results in the literature (see eg. McFadden, 1984). For this analysis, assume that three mutually exclusive choices are available, which we may refer to as well w, self care[5] s, and curative modern medical care m. Let V represent the observed portion of the utility index. The unobserved portion of utility contains a random term ϵ for each alternative. The options when sick may have correlated unobserved elements ϵ_{sm}, and the non-demanders may have unobservables ϵ_{ws} common to their sub-groups, or "nests."

$$
\begin{aligned}
U_{well}(Sick=0, Med=0) &= V_w + \epsilon_w + \epsilon_{ws} \\
U_{self}(Sick=1, Med=0) &= V_s + \epsilon_s + \epsilon_{ws} + \epsilon_{sm} \\
U_{med}(Sick=1, Med=1) &= V_m + \epsilon_m \qquad + \epsilon_{sm}
\end{aligned}
$$

Assume first that the true specification of these probabilities is multinomial logit (MNL), and thus Independence of Irrelevant Alternatives (IIA) holds. Then McFadden (1981) shows that when the joint probability distribution

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unobserved correlation between care demand and the *probability* of reporting any sickness. Nevertheless, the fact that the length of illness should be shorter if care was demanded, is strong evidence that sickness intensity should *not* be used as an exogenous variable, as has been done in many past studies.

[5]The term self care is often used for this category, although it is somewhat of a misnomer. Actually, in all cases individuals are likely to provide some care at home. When no formal care is demanded, this is merely likely to be more time-consuming (for a given illness level). It is more appropriately thought of as a residual category for those who are sick.

f(Sick,Med)=f(Med|Sick)f(Sick) is decomposed into its marginal and conditional probabilities, each of these two component probabilities in turn has an MNL form. This is convenient, allowing each to be separately estimated. This will also hold if the true model is a nested MNL (NMNL), allowing for correlations between choices within each of these separate components ($\epsilon_{ws}=0$, $\epsilon_{sm}\neq 0$). Thus for example, if the choices for sick people all have common unobserved elements, then it is still possible to estimate f(Med|Sick) as a distinct NMNL.

These probabilities can be expressed as follows. Let j=1,..,J index a branch of a choice model, for example the alternatives available when Sick=1. Let k=1,..,K index the sub-choices of that branch, for example to seek curative care or not. Define an "inclusive value" (or "dissimilarity") parameter which summarizes the observed information of a particular branch:

$$D_j = \ln \sum_{k=1}^{K_j} \exp(V_{jk})$$

Choice probabilities can then be written as $P_{jk} = P_{k|j}P_j$, where:

$$P_{k|j} = \frac{\exp(V_{jk})}{\exp(D_j)}$$

(6)

$$P_j = \frac{\exp[V_j + (1-\sigma_j)D_j]}{\sum_{n=1}^{J} \exp[V_n + (1-\sigma_n)D_n]}$$

The parameter σ_j on the inclusive value is the correlation between the unobservables of the choices within a sub-group, and can be estimated up to a scale normalization.

Unfortunately, decomposing P_{jk} into two parts each of which has the logit form is only valid when there is correlation between unobservables *within* branches. If instead there is correlation between unobservables in *different* branches, then the resulting conditional probability f(Med|Sick) will no longer have an NMNL form. This would arise for example if there is correlation between the alternatives in which care is not demanded (ϵ_{ws} is non-zero). Thus when demand is estimated as an NMNL conditional on sickness, the implicit assumption is that ϵ_{ws} is zero. Ignoring this correlation between sickness and demand is analogous to estimating OLS on continuous data with sample selection, instead of using a selection model.

3.3: Unbiased Estimation of Conditional Demand

One estimation option to avoid selection bias when ϵ_{ws} is non-zero is a two-step method similar to that proposed in Heckman (1976). Dor and van der Gaag (1993) report having found no selection bias in conditional health care demand estimation, using a discrete choice version (due to Van de Ven and Van Praag, 1981a) of this procedure. However, they do not report how identification was achieved. Relying on functional form has been found unreliable in many applications (eg. Manning, Duan, and Rogers, 1987), and in most datasets there are no obvious identifying variables which affect health but not health care demand.

An alternative is to instead estimate an unconditional model, without constraining $\epsilon_{ws}=0$. If desired, conditional elasticities can then be calculated either directly from the model, or indirectly by subtracting the health elasticity from the unconditional elasticities, as shown in Equation (5) of Section 2.3.

3.4: Unconditional Demand Estimation

Several options are discussed here for unbiased estimation of unconditional demand, ranging from Ordinary Least Squares (OLS) to the Multinomial Probit (MNP). First, it is shown that these fall into two categories, based on whether they estimate health status *jointly* with health care demand, or only estimate the *marginal* probability of health care demand.

As argued earlier, the dynamic model in Section 2 implies that the health care demand decision can be modeled for the entire unconditional population. This implies that the choice set can be redefined to consist of hospital h, clinic c, and none n, where now no distinction is made between whether non-demanders consider themselves as sick or not. This marginal procedure still yields consistent estimates of unconditional health care demand, assuming that the error distributions are appropriately specified (as discussed below). It may be less efficient than the joint estimation, but the marginal procedure may be preferred when self-reported health indicators are suspect, or when identification of the σ_{ws} covariance is problematic.

In discussing the following unconditional estimation strategies, it is useful to sub-divide the medical care choice, since often elasticities are desired for particular care types. In accord with the Cote d'Ivoire data used in Section 4, medical care options considered here are hospital h and clinic c, in addition to the non-care options of self s and well w.

I. Marginal Demand Estimators (Without Health Information)

A) *Binary Regression*

The simplest unconditional demand estimates are obtained from binary techniques, for example regressing hospital choice against all other choices, as depicted in Figure 1. The estimator could be the binary logit, or even the Linear Probability Model (LPM). However, assuming that the non-hospital choice aggregation has a type I extreme value distributed error may contradict the implicit assumption that the non-clinic choice aggregation also has this distribution. Coherently estimating both the hospital and clinic demands in this way requires IIA to hold across the underlying options, and thus may be biased when $\epsilon_{hc} \neq 0$.

B) *3-Choice Multinomial Logit*

Three-choice MNL's can be estimated between hospital, clinic, and no care (n), as in Figure 2. This assumes that $(\epsilon_h, \epsilon_c, \epsilon_n)$ are jointly distributed type I extreme value, instead of specifying the distribution over $(\epsilon_h, \epsilon_c, \epsilon_w, \epsilon_s)$. Notice that this specification does not preclude correlation from non-zero ϵ_{ws}, and thus is unaffected by any possible selection bias.

C) *3-Choice Nested Multinomial Logit*

A reasonable generalization of the MNL in I.B would be to allow correlation between the unobserved components of hospital and clinic utilities in an NMNL, as in Figure 3.

D) *3-Choice Multinomial Probit*

Finally, a general pattern of unobserved correlations could be allowed for by estimating a 3-choice multinomial probit (MNP) for hospital, clinic, and no care, as in Figure 4. This would allow for the possibility that no care unobservables are correlated with hospital and/or clinic unobservables. While this estimator is not biased by the correlations which could bias conditional regressions, it does present a practical barrier in that MNP's are still very costly to estimate (estimates in this paper took 6-12 hours of workstation time each).

Furthermore, as with continuous selection correction techniques such as Heckman's 2-step method, identification of the unobserved correlations does rely on certain assumptions. Keane (1992) argues that even though in theory the joint normality assumption formally identifies the model, in practice estimates may be fragile in the absence of exclusion restrictions. For robustness, he recommends dropping one variable from each utility index V_j, and notes that such exclusion restrictions arise naturally when choice-specific attributes such as prices are available. These cross-price restrictions may not always be valid, however, when a dynamic model implies that past and future prices for goods not chosen today may still affect today's utility. Such exclusions would instead be more justifiable in short-run estimates conditional on health. Fortunately, Keane showed that unidentified multinomial probit models were characterized by larger standard errors than the same models estimated with covariance restrictions, which provides an informal identification test.

II. Estimating Demand Jointly with Health

A) *4-Choice Multinomial Logit*

A direct extension of the usual conditional 3-choice MNL's is to add a "well" alternative, to assume that the population chooses between hospital, clinic, self, and well, as in Figure 5. While this provides unconditional (long-run) demand estimates, they may be biased by the same unobserved correlations which may bias conditional estimates.

B) *4-Choice NMNL, with unobserved correlations among the sick*

An extension of II.A is to allow the self, hospital, and clinic options to make up a nest separate from the well option, as in Figure 6. This makes the reasonable assumption of some common unobserved utility element to all of these sick categories. This could be further refined with a separate nest within the sick category, separating self from hospital and clinic. However, this still does not allow non-zero ϵ_{ws}.

C) *4-Choice NMNL, nesting non-demanders*

To allow ϵ_{ws} to differ from zero, a different nesting structure could be assumed which nests demanders separately from non-demanders, as in Figure 7. This remedies the selection bias problem, but as discussed in Section 3.2, this nesting pattern cannot simultaneously allow II.B's general correlations among the sick options.

D) *4-Choice Multinomial Probit*

The most general estimator for this problem is the 4-choice multinomial probit depicted in Figure 8. Although an order of magnitude more computationally burdensome than the 3-choice MNP, it does allow for a general pattern of correlation between unobserved utility elements.

3.5: Testing for Selection Bias in Conditional Estimates

It is useful to test for when it is inappropriate statistically to condition the sample on sickness. As Duan et al. (1983,1984) show, it is always possible to obtain consistent estimates of a conditional mean without explicitly modeling the correlations between the equation of interest and a "selection" equation. The question here, however, is when the conditional probability will have a logit specification, given that the population probability does. In other words, when can the marginal and conditional components of the population probability be estimated separately within the logit framework. If it is simply assumed that the conditional probabilities are logistic, their relationship to the population effects will be very difficult to interpret, unless it is known that the transition probability of falling sick is also logistic.

A first test for the relationship of the conditional and unconditional effects can be obtained by estimating the bivariate model E(S=1). This will reveal any selection effects based on observables. Next, the elasticity of health can be added to the conditional elasticity with respect to the variables of interest, and a rough test for consistency will be whether that equals the unconditional elasticity (obtained from a method which does not assume $\epsilon_{ws}=0$, such as I.C). If the two methods of recovering the unconditional elasticity are approximately equal, then this application of Equation 5 may be a sufficient test for "rough and ready" policy purposes.

A second rough test can be carried out when Model II.B indicates $\alpha_{shc}=0$. As discussed above, Model II.C can then be estimated, and this will allow a t-test or Likelihood Ratio or other classical tests of whether $\alpha_{ws}=0$. This is the test of whether the coefficient on the inclusive value for the "no medical care" option equals one. This is not an exact nested test, though, since it could yield different results when α_{shc} is simultaneously allowed to differ from zero.

Finally, exact nested tests can be derived from the multinomial probit (MNP) model. To clearly understand this test, assume again that the choices are restricted to well (w), self (s), and medical care (m). The probability that (w) is chosen is:

$$P(w) = P(\epsilon_s - \epsilon_w < V_w - V_s,\ \epsilon_m - \epsilon_w < V_w - V_m)$$

McFadden has shown that the logit models can be derived from stochastic utility maximization assuming the J unobservables are distributed iid type I extreme-value. The MNP, however, assumes that the ϵ_j are distributed multivariate normal. Thus the differences $\epsilon_s-\epsilon_w, \epsilon_m-\epsilon_w$ will also have a normal distribution, with a J-1 dimensional covariance matrix (superscripted by P for Probit):

$$\Omega_w^P = \begin{Bmatrix} \sigma_w^2+\sigma_s^2-2\sigma_{ws} & \\ \sigma_w^2-\sigma_{ws}-\sigma_{wm}+\sigma_{sm} & \sigma_w^2+\sigma_m^2-2\sigma_{wm} \end{Bmatrix}$$

Because the scale of the latent utility is ambiguous, a normalization must be made in Ω, which only leaves two free elements to be estimated. This implies that identification of σ_{ws} would require several restrictions on the other parameters of Ω. However, this does not interfere with an omnibus test against the null of the NMNL-type covariance restrictions. This is seen more clearly by writing the Ω^L analogous to that assumed by the logit, which allows only one free parameter (assumed here to be σ_{sm}). Notice that the logit also implicitly restricts the variances of the unobservables to be equivalent. The combination of these restrictions results in the two diagonal terms of Ω_w^L being constrained to equivalence:

$$\Omega_w^L = \begin{Bmatrix} 2\sigma^2 & \\ \sigma^2+\sigma_{sm} & 2\sigma^2 \end{Bmatrix}$$

It can be seen here that if either of the logit assumptions of equal variances, or of $\sigma_{wm}=0$, is incorrect, then a nested test of Ω^P against a restricted version of the MNP with covariance Ω^L will reveal this. By next comparing the Ω's for the probability that (m) is chosen, it is seen that σ_{ws} not equal to zero would also result in a classical test rejecting the logit covariance structure. This occurs through the nesting restriction here that two times the off-diagonal equals the lower diagonal term:

$$\Omega_m^P = \begin{Bmatrix} \sigma_w^2+\sigma_m^2-2\sigma_{wm} & \\ \sigma_m^2-\sigma_{wm}-\sigma_{sm}+\sigma_{ws} & \sigma_s^2+\sigma_m^2-2\sigma_{sm} \end{Bmatrix}$$

$$\Omega_m^L = \begin{Bmatrix} 2\sigma^2 & \\ \sigma^2-\sigma_{sm} & 2\sigma^2-2\sigma_{sm} \end{Bmatrix}$$

Because Ω^P only has one more free parameter than Ω^L, the overall NMNL type structure is rejected if only just one of these restrictions is rejected:
Without further identifying assumptions, however, it will not be

$$\sigma_w^2=\sigma_s^2=\sigma_m^2, \quad \sigma_{ws}=0, \quad \sigma_{wm}=0$$

known which restriction was violated. For testing purposes, it is irrelevant which of these restrictions is relaxed; they will all yield identical results. A Likelihood Ratio test of the MNP under these two nested alternative covariance structures then serves as an omnibus discrete choice selection bias test.

To summarize this discussion, the simplest (though non-nested) test of $\sigma_{ws}=0$ is through an NMNL such as Model II.C, in the fortuitous case where Model II.B reveals $\sigma_{shc}=0$. A second test estimates Model I.C, and uses Equation 5 in Section 2.3 to compare it to unconditional estimates. If the elasticities are "similar enough" for the application at hand, then selection bias is not a practical problem. When a more precise test is needed, however, the MNP may be used.

SECTION 4: SHORT-RUN AND LONG-RUN ESTIMATES IN COTE D'IVOIRE

The previous sections have outlined many theoretical differences between estimation strategies. This section empirically explores the magnitude of these differences for a widely used dataset from Cote d'Ivoire, and tests for the importance of selection bias. The data and methods are discussed first. Next, the effects of covariates on the probability of sickness are examined, as a simple way to determine how covariates will impact health care demand in the short versus long run. Sections 4.4-4.6 estimate conditional and unconditional demand models with a variety of covariance assumptions, and Section 4.7 presents results of selection bias tests.

4.1: Cote d'Ivoire Living Standards Survey

The data used are the 1985 round of the Living Standards Measurement Survey (LSMS) collected by the World Bank and the Ivorian government (see Ainsworth and Munoz (1986) for a more detailed description of the data). This same data set has been previously used by others for health care demand estimation; see for example Gertler and van der Gaag (1990), and Dor and van der Gaag (1993). Means for the conditional and unconditional samples are reported in Table 1.

The most commonly demanded health care options in the data can be divided into hospitals and clinics. The analysis will be confined to the choice of the first provider visited in the reference period. In 1985, monetary fees for curative services were virtually zero, but there is considerable variation in the distance and time to travel to the facilities, thus time-price elasticities will be estimated below. One implication of this is that preventive and curative care (time) prices are highly correlated, implying that it is possible that the long run elasticities could be either smaller or larger than the short run responses[6].

The health indicator typically used to identify the "sick" sample is whether individuals report themselves to have been sick in the past four weeks. As mentioned earlier, this is *positively* correlated with income in this sample, which may partly be due to reporting bias. This may make the long-run elasticities easier to interpret, and thus preferable a priori. This is supported by the fact that distances to clinics have not dramatically changed in the recent past for rural areas. The estimation is confined to an adult (age>15) rural sample, in order to minimize confounding factors while highlighting the issue of the conditional versus unconditional differences.

The regressor set includes individual gender, age, and education, and uses household consumption per adult as a proxy for income. Also included are community-level wages, hospital travel-times and clinic travel-time (to the nearest facility). The community-level character of the data is intended to reduce the possible endogeneity problems from self-reported data. There is still a danger that endogenous migration (Rosenzweig and Wolpin, 1988) or program placement (Rosenzweig and Wolpin, 1986) will introduce correlation between health care prices and the demand residual, but this possibility will be ignored for the present purposes.

The data come from the 57 rural communities surveyed in 1985, and the community reported variables are correlated as expected. The hospital and clinic travel times have a correlation of 0.47; the wage correlations with these are -.20 and -.27, respectively.

Each of the regressors is specified through interactions with alternative-specific dummies for the hospital and clinic choices. The structural justification for such a specification is presented in Dow (1995). The flexible non-linear terms discussed in that

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[6]The high correlation between curative and preventive prices also implies that cross-elasticities between these cannot be obtained from this data. This is unfortunate, because many user fee proposals in Africa focus on raising curative fees only.

paper are not included, however, in order to ease generalization of results to past studies which mostly used linear specifications of observables.

4.2: Estimation Procedures

The models estimated below begin with the simplest linear probability models, and gradually relax assumptions. The more complex non-linear models include binomial logits, multinomial logits (MNL's), nested multinomial logits (NMNL's), and simulated multinomial probits (MNP's).

The logits were estimated using the "hlogit" routine by Axel Boersch-Supan, and the probits using the "ssmlmnp" smoothly simulated maximum-likelihood routine by Boersch-Supan and Vassilis Hajivassiliou. Davidson-Fletcher-Powell (dfp) was the principal non-linear search algorithm employed, with a covariance correction through recalculating the exact hessian.

Because the coefficients do not directly give the marginal effects, Table 3 presents price elasticities for each of the models estimated. Although classical Likelihood Ratio tests indicate whether the logit models are statistically different from each other, this of course depends on sample size, and the probit cannot be compared using the classical nested tests. The elasticity table will also aid in determining whether the estimates differ much for policy-makers.

Elasticities were obtained from the calculus derivations of the marginal effects, by taking the average of the elasticities evaluated at each observation. Simulations through sample enumeration (Train, 1985) generally yielded very similar results.

The four potential discrete choices are denoted as hospital h, clinic c, self s, and well w. The conditional MNL's allow choice between h,c, and s. The unconditionals allow choices between h,c,s and w, although Table 6 also gives estimates with s and w aggregated into a none (n) category.

For identification, each observable covariate is interacted in the likelihood function with an alternative specific dummy.

Because a constant could be added to the coefficient for each choice on a specific variable without changing the probabilities in the likelihood function, the coefficient vector for one alternative is normalized to zero in the estimation. For ease of interpretation, this reference category is "well" in the 3-choice unconditional estimates, and "self" in the other cases.

4.3: Probability of Self-Reporting Sick

The first step in empirically evaluating the effects of conditioning is to examine the logit results in Table 2 for the probability that sickness is reported in the past 28 days. The farther away are clinics, the higher is the probability of reporting sick, while the effect for hospitals is negative, but tiny and statistically not different from zero. Income and wages are both *positively* correlated with the probability of reporting sick. This could perhaps be due to higher risk activities by the wealthier, or represent differences in the level of discomfort that defines sickness for people in different circumstances. Finally, females and the more highly educated also report more sickness. The price elasticity of health is given in Table 2, and is relatively inelastic.[7] This suggests that empirically for this data set, the difference between long-run and short-run price elasticities is not large.[8] The significant effects for other covariates, however, are seen below to lead to different inferences depending on whether conditional or unconditional estimation is used. Simply by virtue of the significant coefficients on these observables, it can be seen that the conditional estimates are not equal to the unconditional

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[7]A rough test of the effects of multicollinearity when both prices are included as regressors was performed by dropping each in turn. The results were virtually identical.

[8]Virtually the same information is revealed by simple linear probability estimates. They yield health elasticities of -.015 with respect to hospital prices, and -.071 for clinics--versus the logit estimates of -.015 and -.079.

population effects.

4.4: Simple Binomial Specifications

Because the non-linear estimates reported below are expensive to calculate and difficult to interpret, Linear Probability (LPM) estimates are first reported, in Table 4, with each alternative estimated separately as in Model I.A. Columns 2 and 4 give the conditional estimates (i.e. omitting healthy people, thus the non-hospital choice aggregates only self and clinic). These yield price elasticities (all evaluated at sample means) of $\mathcal{E}_h = -0.10$ and $\mathcal{E}_c = -0.36$. Adding these to the health elasticities from the earlier logit of P(S=1) gives the simplest estimate of the population elasticities:

$$\mathcal{E}_h = -0.10 + (-0.02) = -0.12$$

$$\mathcal{E}_c = -0.36 + (0.08) = -0.28$$

These can be compared to the unconditional elasticities from columns 1 and 3 which were also LPM's, but used the entire sample (thus the non-hospital category includes well, in addition to self and clin):

$$\mathcal{E}_h = -0.10$$

$$\mathcal{E}_c = -0.26$$

These two methods of recovering the population elasticities yield remarkably similar results. As discussed in Section 3.5, this gives indirect evidence that conditioning on sickness yields "not too inconsistent" estimates of the *short-run* effects.

Also notice from these LPM's that while clinic prices are statistically significant, the hospital prices are not. Furthermore, while wages positively effect sickness, they negatively impact conditional hospital demand, possibly since they represent the opportunity cost of travel time. In the unconditional estimates, however, the wage effects cancel for hospitals, and appear positive for clinics.

Income is also a positive and significant determinant of medical care demand unconditionally, although for clinics this statistically disappears in the conditional estimates. Also notice that while females in the unconditional sample are more likely to demand clinic services, in the conditional sample the only statistical effect is that males are more likely to demand hospitals. Notice further that unconditionally, older persons demand more care, while in the samples truncated to contain only the sick, age appears negatively correlated with demand.

Both the directions and magnitudes of these effects are very similar in the more complex non-linear estimates, though with a few exceptions. It is noteworthy, however, that even these simple Ordinary Least Squares estimates clearly indicate the importance of distinguishing between conditional and unconditional estimation.

4.5: 3-Choice Unconditional and Conditional Estimates

The binary results in the previous section can be generalized by estimating the hospital and clinic equations jointly in a multinomial model. Table 5 directly compares the unconditional models choosing between none, hospital, and clinic (models I.B,I.C), to the conditional models between self, hospital, and clinic. The first two columns give the MNL versions, while the third and fourth report nested results, where correlation is allowed between hospital and clinic unobservables. For the unconditional sample the MNL is rejected, but not for the conditional sample. As in the binomial estimates, there are differences between the signs and significance patterns of the gender, age, and wage variables.

The price elasticities shown in Table 3 indicate that while nesting of hospital and clinic choices does not affect elasticities, the effects of conditioning remain in these (N)MNL models. The conditional clinic elasticity of -.49 is still over 25% higher than the unconditional of -.37.

4.6: Unconditional 4-Choice (Nested) Multinomial Logits

Next, the well (w) alternative is explicitly considered in a 4-choice model, jointly with the self, hospital, and clinic choices, still assuming zero correlation between error terms, as in Model II.A. These estimates are reported in Table 6, column 1. The price elasticities exhibit the same general pattern as in the LPM, although again the (unconditional) clinic elasticity of .35 is higher than the LPM estimate.

In column 2, a version of Model II.B is estimated, allowing for non-zero covariance σ_{shc}. Using the Likelihood Ratio (LR) test, the nested logit clearly rejects the MNL. The clinic elasticity is relatively unchanged at .36; the hospital elasticity remains insignificantly different from zero.

Although the significant σ_{shc} in Table 6.2 indicates that Model II.C may be inappropriate, it is nevertheless interesting to estimate the model as part of a non-nested testing strategy. This will help show whether a general pattern exists, or whether Model II.C in turn indicates that σ_{ws} is non-zero. Table 6 Column 3 reports this regression which estimates σ_{hc} and σ_{ws} simultaneously, and it can be seen that the LR test again rejects the MNL against this specification. Furthermore, the coefficient on the inclusive value for the "no medical care" sub-branch is significantly larger than one, indicating that this particular model is not consistent with utility maximization (McFadden, 1981). This provides ambiguous evidence concerning the statistical bias of conditioning. However, the fact that the elasticities again do not change much indicates that any statistical selection bias may be unimportant economically. This is corroborated by the fact that the 4-choice clinic elasticity is virtually identical to the 3-choice unconditional elasticity in the previous section; the 4-choice model may be affected by selection bias, whereas the 3-choice model is not.

4.7: MNP Test of Selection Bias in the Conditional Approach

The informal selection tests above provided evidence that conditioning on sickness did not appear to inappropriately restrict short-run estimates. In Section 4.4, the unconditional price elasticity was roughly equal to the sum of the conditional price elasticity plus the elasticity of sickness. In Section 4.6, elasticities appeared robust to non-nested comparisons of covariance assumptions. Finally, the more formal test using simulated MNP's is reported in Table 7.

Four-choice models were estimated between well, self, hospital, and clinic. In Column 1, the MNP was estimated with no free covariance parameters, which is similar to the non-nested MNL; this model has been termed the Independent Probit (IMNP) by Hausman and Wise (1978). In Column 2, the "covariance" MNP was estimated with five free covariance parameters, which is the most that can be identified given that four choices are estimated. The latter nests the IMNP, providing for an omnibus nested test for selection bias.

The hospital and clinic prices were only allowed to enter their own utility indices, providing additional identification restrictions of the covariance parameters. As mentioned in Section 3.5, Keane (1992) shows that "fragilely" identified models are characterized by large standard errors when the covariance terms are freely estimated. The fact that the standard errors are very similar across the restricted and unrestricted regressions in Table 7 is evidence that the model is appropriately identified by functional form[9].

From these two regressions, selection bias does not appear to be a problem in this dataset. The t-tests indicate that the estimated covariance parameters are all individually insignificant. Furthermore, the Likelihood Ratio test reveals them to be jointly insignificant: the chi-squared value of 4.71 is less than the 75% critical value of 6.63 with five degrees of freedom. Finally, the elasticity estimates between the models are

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[9]Functional form was also found to provide reasonable identification of unobserved correlation in continuous selection models by Duan et al. (1984), when full information maximum likelihood was used (as it is here). That contrasts with the fragility of estimates which they found when the same model was estimated with the 2-step Heckman procedure, in the absence of exclusion restrictions.

quite similar, varying only from -.07 to -.11 (both insignificantly different from zero) for hospitals, and -.25 to -.27 for clinics.

SECTION 5: IMPLICATIONS AND DISCUSSION

Evidence in the previous section suggested that in Cote d'Ivoire, health care demand estimates conditioned on health status do not suffer from statistical selection bias. If this is corroborated in other data, it would imply that less expensive data collection efforts which interview randomly sampled sick people may be acceptable for short-run analysis. This would be useful for planners who need to allocate supplies in the short-run, such as medications.

However, the Cote d'Ivoire evidence also suggested that price elasticity magnitudes, as well as the signs and significance of gender, age, and wage covariates, may not be the same in unconditional samples as in such choice-based samples. For example, the price elasticities are about 25% larger in conditional samples than in unconditional ones. The finding that long-run unconditional elasticities are smaller than the short-run elasticities may be due to the fact that travel distances to clinics affect both curative and preventive health inputs, leading to a cumulative worsening of health for those living farther from clinics.

It is important to consider more generally in what scenarios the long-run price effects are likely to differ substantially from the short-run. One factor is whether the health dimension being conditioned on is of a durable nature. Childhood diarrhea, for example, may persist over time if high prices cause it to have gone untreated in the past. Furthermore, it has been shown to worsen the health effects of other childhood diseases in developing countries, thus magnifying health feedbacks on demand. A second factor is whether health input prices are a large portion of the budget, which would cause past inputs to have wealth effects through the budget constraint, thus affecting current health. This is postulated to occur, for example, when families sell their goats to finance health care, but then children are deprived of nutrition and become sicker. Substitution effects may also play a role: If substitute inputs are readily available at similar prices, then while price responses may be large, the response of health status to price may be small, and thus little difference will be apparent between the long-run and short-run. As mentioned earlier, however, when preventive prices are highly correlated with curative prices, then substitution is less likely, meaning that there may be larger health feedbacks.

Differences in long-run and short-run estimates may also arise due to data problems. As discussed earlier, short-run estimates may be biased from the self-reporting of health indicators used to condition the sample. The long-run estimates, however, also suffer from potential problems. First, health status is a very significant determinant of curative care demand, and if health information is not used, the already noisy estimation becomes even less precise. Second, the price effect on health may be spurious due to correlations with other unobserved inputs. Third, the "long-run" interpretation becomes clouded when input access prices are not constant during the past long-run period which affects current health. For adults, the prices of vaccinations as a child are virtually always unobserved by the researcher. For children, however, this has become less of a problem as better data is collected. Also, in rural areas (such as the Cote d'Ivoire sample used in this paper), distances to clinics may be reasonably constant over a decade or two, and inputs before that may not have much effect on current health status.

All of these problems make the demand estimates difficult to interpret. How important these problems are, and whether the conditional or unconditional estimates are more precise, will depend on the application. As discussed in Section 2.3, at least a general idea of the sensitivity of estimates to these problems can be obtained by estimating the elasticity of sickness, which is equal to the difference between the unconditional and conditional elasticities. Furthermore, more precise analysis of the effects of conditioning was shown to be feasible, by comparisons with easily estimated unconditional effects.

SECTION 6: CONCLUSION

It appears that focusing attention on the effects of health care for the healthy in addition to the sick may have important payoffs. Theoretically, a distinction can be drawn between long-run and short-run effects of policies, based on whether the reaction is taken into account of how people change their probability of falling sick. Econometrically, short-run conditional estimates may be biased. Conditioning on sickness may also be undesirable when health indicators suffer from self-reporting bias.

Empirically, it was found that the selection bias does not appear to affect conditional estimates of short-run elasticities in this Cote d'Ivoire sample. This is important, as it enhances the robustness of the many previous studies having conditioned on illness. However, as in Ellis and Mwabu (1991), the conditional demand determinants were found to influence the probability of sickness. As pointed out in Section 2, this implies that the conditional effects do not provide accurate estimates of the long-run unconditional effects, which is what health planners often need to know.

For gender, age, and wage covariates, the signs and significance of effects differ between the conditional and unconditional estimates. For prices, the short-run elasticities were 25% larger than the long-run effects. This difference could be important economically, as user fee policies typically involve price changes of several hundred percent. It is especially noteworthy, also, that it cannot be assumed that the short-run elasticity is a lower-bound for the long-run response after people have had time to adjust behavior. As hypothesized in Section 3, the lower long-run price response found in this sample may be due to the high correlation between curative and preventive health input prices.

It would be important to know if the absence of selection bias in the conditional estimates is robust in other samples. Section 3.5 outlines methods for testing for selection bias in conditional estimates for the many datasets which have information on the healthy. When conditioning is not inappropriate, this also allows Equation 5 in Section 2.3 to be invoked to obtain estimates of the difference between the long-run and short-run elasticities. This difference will be the easily estimated elasticity of the probability of falling sick. When unconditional estimates are desired, however, they can be simply obtained from a model which ignores health information, and estimates the choice between hospitals, clinics, and neither.

The effects of truncating healthy people from samples is not merely a statistical issue. Whether or not conditional techniques are consistent in other data sets, it is important that health care demand studies pay more attention to the fact that the conditional estimates can only be interpreted as short-run effects. As shown, unconditional estimates require little extra computational burden, and may possibly yield significantly different policy implications for forward-looking planners.

Appendix A

Variable Definitions

$E[.]$ = expected value

t = time index

j,k = choice indexes

U = utility function

V = observable portion of utility

H_t = Health status at time t (before period t's inputs)

I = health Input, where:
I^{pre} = health Input, preventive
I^{cur} = health Input, curative

C = Consumption of non-health goods

p = price

Y = income

ρ = discount factor

λ_t = health innovation at start of period t

δ = depreciation of health stock

S = Sickness indicator

M = Medical care utilization indicator

X = observables in a particular model

$\mathcal{E}^{M|S}$ = Elasticity of medical care conditional on sickness

ϵ, υ = unobserved residuals

σ_{jk} = covariance between unobservables in choices j,k

D_j = inclusive value variable for nested multinomial logits

P = Probability

h,c = hospital, clinic

s,w = self care, well

n = no formal care (neither hospital nor clinic)

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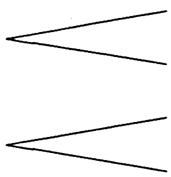
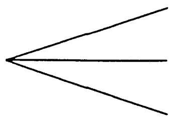
<u>Choice Trees</u> <u>Covariance Structures</u>



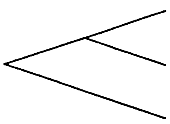
$$\begin{array}{l} \\ hosp \\ none + clin \\ clin \\ none + hosp \end{array} \begin{array}{c} \begin{array}{ccc} h & c & n \end{array} \\ \\ \begin{bmatrix} \sigma_h^2 & & \\ 0 & \sigma_c^2 & \\ 0 & 0 & \sigma^2 \end{bmatrix} \end{array}$$

Figure 1: **Binary Models**



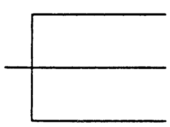
$$\begin{array}{l} hosp \\ clin \\ none \end{array} \begin{bmatrix} \sigma^2 & & \\ 0 & \sigma^2 & \\ 0 & 0 & \sigma^2 \end{bmatrix}$$

Figure 2: **3-Choice MNL**



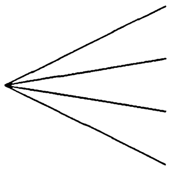
$$\begin{array}{l} hosp \\ clin \\ none \end{array} \begin{bmatrix} \sigma^2 & & \\ \sigma_{hc} & \sigma^2 & \\ 0 & 0 & \sigma^2 \end{bmatrix}$$

Figure 3: **3-Choice NMNL**



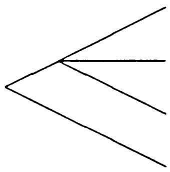
$$\begin{array}{l} hosp \\ clin \\ none \end{array} \begin{bmatrix} \sigma_h^2 & & \\ \sigma_{hc} & \sigma_c^2 & \\ \sigma_{hn} & \sigma_{cn} & \sigma_n^2 \end{bmatrix}$$

Figure 4: **3-Choice MNP, free covariance structure**



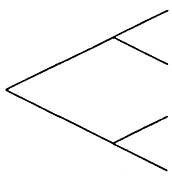
$$\begin{array}{l} hosp \\ clin \\ self \\ well \end{array} \begin{bmatrix} \sigma^2 & & & \\ 0 & \sigma^2 & & \\ 0 & 0 & \sigma^2 & \\ 0 & 0 & 0 & \sigma^2 \end{bmatrix} \quad \Big\} \begin{array}{l} \text{usual} \\ \text{conditional} \\ \text{MNL} \end{array}$$

Figure 5: **4-Choice MNL**



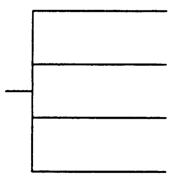
$$\begin{array}{l} hosp \\ clin \\ self \\ well \end{array} \begin{bmatrix} \sigma^2 & & & \\ \sigma_{shc} & \sigma^2 & & \\ \sigma_{shc} & \sigma_{shc} & \sigma^2 & \\ 0 & 0 & 0 & \sigma^2 \end{bmatrix}$$

Figure 6: **4-Choice NMNL, sick nest**



$$\begin{array}{l} hosp \\ clin \\ self \\ well \end{array} \begin{bmatrix} \sigma^2 & & & \\ \sigma_{hc} & \sigma^2 & & \\ 0 & 0 & \sigma^2 & \\ 0 & 0 & \sigma_{sw} & \sigma^2 \end{bmatrix}$$

Figure 7: **4-Choice NMNL, non-demanders' nest**



$$\begin{array}{l} hosp \\ clin \\ self \\ well \end{array} \begin{bmatrix} \sigma_h^2 & & & \\ \sigma_{hc} & \sigma_c^2 & & \\ \sigma_{hs} & \sigma_{cs} & \sigma_s^2 & \\ \sigma_{hw} & \sigma_{cw} & \sigma_{sw} & \sigma_w^2 \end{bmatrix}$$

Figure 8: **4-Choice MNP, free covariance structure**

Table 1: Descriptive Statistics, Unconditional and Conditional Samples

	Unconditional		Conditional on Sick=1	
	Mean	Standard Deviation	Mean	Standard Deviation
Probability Sick (=1 if self reported sick in last 4 weeks)	.34	.47	1	0
Probability visit Hospital (last 4 weeks)	.048	.22	.144	.35
Probability visit Clinic (last 4 weeks)	.078	.27	.231	.42
Travel Time: Hospital (hours)[1]	.95	1.00	.93	.88
Travel Time: Clinic (hours)[1]	.54	.61	.56	.62
Income (annual, 1985 CFA)[2]	2.47	2.13	2.62	2.19
Wage (daily, 1985 CFA)[3,4]	.51	.19	.53	.20
Male	.43	.50	.42	.49
Education (years)	1.14	2.62	.87	2.23
Age[4]	37	16.9	44	17
Sample size		4042		1359

[1]To closest facility reported by community elders.

[2]Permanent income is proxied by total household consumption, normalized by number of adults in household. Divided by 10,000 for estimation.

[3]Community reported agricultural wage, by gender.

[4]Divided by 100 for estimation.

Table 2: **Binary Logit of Probability of Reporting Sick**

Time Hosp	-.02
	(0.55)[1]
Time Clin	.23
	(3.37)
Income	.11
	(5.26)
Wage	.83
	(4.22)
Male	-.27
	(3.55)
Education	.04
	(2.23)
Age	4.20
	(18.16)
Intercept	-3.01
	(18.29)
lnL	-2334
lnL at zero	-2771

Elasticities of Prob(Sick=1) with respect to:

Time Hosp: $E_h = -.015$
Time Clin: $E_c = .079$
Income: $E_Y = .18$

Elasticities from Linear Probability Model (not reported here) of Prob(Sick=1), evaluated at sample means:

$E_h = -.014$
$E_c = .071$
$E_Y = .09$

[1]Absolute values of t-statistics in parentheses.

Table 3: Time-Price (own) Elasticities[1] of Demand
(leftmost column gives Table # and column of regression results)

Regression	Elasticities	
	Hospital	Clinic
4.1 LPM Prob(Hosp)	-.10	--
4.2 LPM Prob(Hosp|Sick=1)	-.10	--
4.3 LPM Prob(Clin)	--	-.26
4.4 LPM Prob(Clin|Sick=1)	--	-.36
5.1 MNL nhc	-.05	-.35
5.2 MNL shc |Sick=1	-.15	-.49
5.3 NMNL nhc (nest σ_{hc})	-.15	-.37
5.4 NMNL shc (nest σ_{hc}) |Sick=1	-.11	-.49
6.1 MNL wshc	-.05	-.35
6.2 NMNL wshc (nest σ_{shc})	-.14	-.37
6.3 NMNL wshc (nest σ_{ws}, σ_{hc})	+.02	-.36
7.1 MNP (MNL type covariances)	-.07	-.25
7.2 MNP (unrestricted cov)	-.11	-.27

[1] Elasticities reported are averages of those evaluated at each individual observation.

Table 4: Linear Probability Estimates of Demand Equations

	P(hosp)	P(hosp|Sick=1)	P(clin)	P(clin|Sick=1)
Time Hosp	-.005	-.02	.002	.01
	(0.17)[1]	(1.33)	(0.51)	(0.98)
Time Clin	.02	.03	-.04	-.15
	(2.44)	(1.37)	(4.85)	(6.72)
Income	.008	.01	.004	.004
	(4.65)	(3.36)	(2.15)	(0.81)
Wage	-.004	-.09	.03	-.02
	(0.19)	(1.71)	(1.48)	(0.40)
Male	.005	.04	-.02	-.02
	(0.66)	(1.90)	(2.58)	(0.95)
Education	.001	.002	-.00	-.01
	(0.74)	(0.39)	(0.04)	(1.15)
Age	.05	-.20	.16	-.16
	(2.56)	(3.30)	(6.03)	(2.23)
Intercept	.005	.22	.02	.39
	(0.34)	(4.99)	(1.02)	(7.29)

[1] t-statistics have not been corrected for heteroscedasticity

Table 5: 3-Choice (Nested) Multinomial Logits
(NMNL's allow correlation between hospital and clinic unobservables)

	MNL[1]	MNL |Sick=1[2]	NMNL[1]	NMNL|Sick=1[2]
Time Hosp_h	-.06	-.18	-.10	-.15
	(0.68)	(1.67)	(1.67)	(1.07)
Time Clin_c	-.68	-1.01	-.46	-1.11
	(5.17)	(6.95)	(2.86)	(4.72)
Income_h	.12	.11	.10	.12
	(4.62)	(3.45)	(4.90)	(3.31)
Income_c	.06	.05	.07	.05
	(2.54)	(1.69)	(3.34)	(1.50)
Wage_h	-.02	-.82	.01	-.91
	(0.40)	(1.84)	(0.29)	(1.76)
Wage_c	.04	-.32	.04	-.31
	(1.28)	(0.87)	(1.38)	(0.80)
Male_h	.07	.30	-.06	.35
	(0.48)	(1.75)	(0.43)	(1.69)
Male_c	-.33	-.07	-.25	-.09
	(2.54)	(0.45)	(2.19)	(0.56)
Education_h	.02	1.002	-.02	.0001
	(0.67)	(0.05)	(0.77)	(0.00)
Education_c	-.00	-.04	-.00	-.05
	(0.07)	(1.24)	(0.13)	(1.25)
Age_h	1.35	-2.12	1.60	-2.22
	(3.00)	(3.93)	(4.43)	(3.60)
Age_c	2.15	-1.40	2.00	-1.37
	(6.08)	(3.14)	(6.34)	(2.88)
Intercept_h	-3.64	-.41	-3.28	-.54
	(11.43)	(1.10)	(12.46)	(1.11)
Intercept_c	-3.21	.19	-3.04	.15
	(12.8)	(0.61)	(14.28)	(0.43)
Inclusive value (h,c nest)			.50	1.18
			(-2.49)[3]	(0.50)
log-Likelihood	-1813	-1185	-1811	-1185

---

[1]The alternatives in the (3-choice) unconditional models are hospital, clinic, and none (the aggregation of well and self).

[2]The alternatives in the (3-choice) conditional models are hospital, clinic, and self.

[3]t-test for null hypothesis that coefficient equals one.

Table 6: 4-choice Unconditional (Nested) Multinomial Logits

	MNL	NMNL[1]	NMNL[2]
Time Hosp_h[3]	-.06	.02	-.10
	(0.66)	(0.31)	(1.57)
Time Clin_c	-.68	-.44	-.48
	(5.16)	(3.10)	(2.86)
Income_h	.12	.09	.13
	(4.62)	(3.78)	(3.64)
Income_c	.07	.05	.10
	(2.60)	(2.56)	(2.73)
Income_s	.02	.04	.12
	(0.85)	(2.14)	(1.20)
Wage_h	.04	.14	.86
	(0.10)	(0.37)	(1.93)
Wage_c	.63	.76	1.18
	(1.93)	(2.77)	(2.93)
Wage_s	.71	.71	2.64
	(3.27)	(3.72)	(2.73)
Male_h	.01	-.01	-.39
	(0.04)	(0.09)	(2.14)
Male_c	-.40	-.39	-.60
	(3.04)	(3.60)	(3.53)
Male_s	-.26	-.27	-1.26
	(2.94)	(3.44)	(2.87)
Education_h	.03	.03	.06
	(0.99)	(1.12)	(1.97)
Education_c	.01	.01	.04
	(0.36)	(0.60)	(1.53)
Education_s	.03	.03	.10
	(1.56)	(1.87)	(1.20)
Age_h	2.76	3.28	7.80
	(5.82)	(7.69)	(6.28)
Age_c	3.66	4.10	8.33
	(9.62)	(12.55)	(6.69)
Age_s	4.63	4.37	20.48
	(9.62)	(18.01)	(5.09)
Intercept_h	-3.96	-3.57	-4.60
	(12.10)	(12.51)	(12.12)
Intercept_c	-3.57	-3.30	-4.41
	(13.71)	(15.67)	(12.37)
Intercept_s	-3.29	-2.94	-14.57
	(19.87)	(18.01)	(2.95)

---

[1]This model nests the hospital, clinic, and self alternatives in a separate branch from the well category.

[2]This model nests hospital and clinic together, and then self and well in a separate nest.

[3]_h, _c, and _w suffixes indicate that the coefficient measures the effect of the variable on the hospital, clinic, and well options, respectively, through interactions with alternative-specific dummies.

Table 6 continued

Inclusive value (shc nest)		0.38 (-5.07)[4]	
Inclusive value (hc nest)			.53 (-2.14)[4]
Inclusive value (sw nest)			4.62 (3.57)[4]
log-Likelihood	-3571	-3566	-3760

[4]t-test for null hypothesis that coefficient equals one.

Table 7: 4-Choice[1] Multinomial Probits

	IMNP (MNL-type covariances)[2]	MNP (unrestricted covariances)[3]
Time Hosp_h	-.04	-.04
	(0.89)	(1.24)
Time Clin_c	-.39	-.22
	(4.49)	(4.08)
Income_h	.08	.06
	(4.98)	(2.91)
Income_c	.05	.04
	(2.84)	(3.02)
Income_s	.02	.01
	(1.01)	(0.95)
Wage_h	.10	.14
	(0.42)	(0.64)
Wage_c	.46	.36
	(2.03)	(2.59)
Wage_s	.56	.58
	(3.26)	(3.48)
Male_h	-.03	-.06
	(.35)	(0.69)
Male_c	-.27	-.18
	(3.13)	(3.19)
Male_s	-.20	-.21
	(2.98)	(3.10)
Education_h	.02	.02
	(1.26)	(1.24)
Education_c	.01	.01
	(0.59)	(0.97)
Education_s	.03	.03
	(1.92)	(1.84)
Age_h	2.18	1.76
	(7.80)	(4.79)
Age_c	2.76	2.28
	(11.77)	(13.32)
Age_s	3.72	3.68
	(18.07)	(18.42)
Intercept_h	-2.92	-2.14
	(16.32)	(3.88)
Intercept_c	-2.72	-1.87
	(16.09)	(15.73)
Intercept_s	-2.66	-2.67
	(21.36)	(21.22)

---

[1]The four choices in these MNP models are well, self, hospital, clinic.

[2]This model mimics the covariance structure of the MNL, restricting all covariances between unobservables to zero.

[3]This model nests the NMNL-type covariance structure, by allowing an unrestricted pattern of covariances between unobservables.

Table 7 continued

sd_h	1.00	.55 (0.44)[4]
sd_c	1.00	.10 (-0.98)[4]
corr_h,c	0.00	-.85 (0.08)
corr_h,s	0.00	-.52 (.51)
corr_c,s	0.00	.18 (0.46)
log-Likelihood	-3570	-3565

[4]t-test for null hypothesis that coefficient equals one.

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