A RAND NOTE

Beneficiary Incentives to Participate in Alternative Health Plans: A Research Design

M. Susan Marquis, Naihua Duan, Ker-Chau Li,
Sandra H. Berry, John G. Haaga, Kathleen N. Lohr

March 1988

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PREFACE

This Note describes the design for a survey of Medicare beneficiaries that will obtain data about preferences for alternative health plans. This work was conducted at the RAND/UCLA Center for Policy Research in Health Care Financing, which is supported through a cooperative agreement with the Health Care Financing Administration. The Note should interest health services researchers and those who design consumer survey instruments and procedures.

SUMMARY

BACKGROUND

To control program costs, Medicare is trying new ways to pay for services. One method is to pay alternative health plans (AHPs) a fixed, per-capita amount to provide Medicare-covered services to beneficiaries who enroll in the plan. For the capitation strategy to fulfill Medicare's cost containment objectives, AHPs must attract sufficient numbers of Medicare beneficiaries including both high and low users of services. To expand enrollment, policymakers are considering options to alter the financial incentives for beneficiaries to join AHPs. Unfortunately, only scant empirical research guides the formulation of policies on the design of optimal incentives.

STUDY OBJECTIVES

Our objectives for this research are to provide Health Care Financing Administration (HCFA) with the information needed to evaluate alternative Medicare capitation options. Specifically, we want to investigate the following issues:

- What plan benefits do Medicare beneficiaries value most?
- What restrictions on provider choice are most influential in the decision to join AHPs?
- What is the effect of price on the decision?
- What is the expected degree of biased selection in new systems?

To address these issues, we propose a method of data collection and analysis that will allow us to estimate a model that relates a beneficiary's decision to enroll in an AHP to the attributes of the AHP.

An alternative to our model-based approach would be a demonstration project to compare who enrolls in several different AHPs. The demonstration approach, however, assumes that policymakers are

interested in only a few particular types of plans, namely those included in the demonstration. The model approach, by providing estimates of how specific attributes influence beneficiaries' choice, allows us to predict how a wide variety of plans might fare. It also will allow us to predict what package of features attract different risk groups. This information can guide policymakers in formulating capitation policy to draw both high and low users of Medicare services to AHPs and in formulating Medicare reimbursement to AHPs to correct for over- or underpayment stemming from selection.

STUDY DESIGN

Approach

The data to estimate the model come from a mail survey of Medicare beneficiaries. Respondents are presented with hypothetical AHP options in the survey and asked the likelihood that they would choose the option in favor of traditional Medicare coverage. We propose to collect data on these hypothetical plans rather than to use data on the choices made by Medicare beneficiaries who currently have the option of enrolling in an AHP for two reasons. First, it would be difficult, if not impossible, to isolate the influence of specific plan attributes using extant data because available AHPs differ from Medicare and from each other in many ways. Second, existing data restrict the analysis to the range of plans currently available; we would learn little about beneficiary acceptance of new forms of AHPs.

The validity of the conclusions from this research depends upon (a) whether responses to hypothetical questions indicate how consumers would actually behave and (b) whether assumptions underlying the form of the decisionmaking model that we fit are valid. A review of several studies that have used hypothetical examples to assess consumers' preferences among health plans supports the validity of our approach. Furthermore, we will validate our model by comparing its predictions of enrollment in markets that have a Medicare AHP option with observed enrollments.

Plan Attributes

Table S.2 describes the 13 AHP attributes that we will study. The attributes include provider choice restrictions (attributes A through D), the plan premium (attribute E), and plan benefits (attributes F through M).

Several considerations guided our choice of attributes. We wish to study attributes that policymakers might influence. Thus we focus on attributes that are, or might be, used to qualify eligible plans and financial aspects of the options rather than on attributes such as wait times to appointment or perceived quality of care. Attributes characterizing the degree of provider choice restriction reflect the variety of AHP structures that are, or might in the future be, offered to Medicare beneficiaries and restrictions that Medicare or health plans might impose to make costs more controllable and predictable.

Each hypothetical AHP presented to the survey respondents is described in terms of all 13 attributes. The combinations of attribute values do not necessarily depict available options for Medicare beneficiaries. Rather, the combinations of attribute values are chosen using experimental design principles in such a way that we can estimate the influence of each of the attributes on beneficiaries' preferences. We can then *predict* enrollment in an AHP characterized by any combination of the attributes.

Measurement Design

The survey instrument will contain several modules. Those concerning sociodemographic characteristics, health status, patient satisfaction, prior and anticipated use of medical care, and current health insurance coverage will be identical for all respondents. The module concerning the AHP options will vary from individual to individual as explained below.

Table S.1

ATTRIBUTES OF PROTOTYPICAL ALTERNATIVE HEALTH PLANS

Attribute	Alternative Levels
Provider Choice Restrictions	Assume usual provider not a member and choose from:
(A) Physician Choice	(1) list that includes 50% of area providers (2) list that includes 10% of area providers (3) providers practicing at the plan facility/facilities (about 10% of area providers participate) Assume usual provider is a member and choose from: (4) providers practicing at the plan facility or facilities (about 10% of area providers participate)
(B) Hospital choice	(1) Any hospital patient and doctor choose (2) Those hospitals associated with plan
(C) Enrollment	(1) Can disenroll with 30-days'notice (2) Enrollment for 1 year
(D) Case management	(1) Patient can self-refer to specialists (2) Personal physician is gatekeeper to specialty care
Premium	
(E) Monthly premium	Quoted premium is -A + B*(Actuarial value of additional benefits G-L) + C*(Actuarial value of M) where ranges are: A from 0 to $50, B from .5 to 1.5, C from .05 to 1.5, and A, B, and C are generated randomly.
Plan Benefits	
(F) Reimbursement for out-of-plan use	(1) 50% cost to maximum of $500 per year (2) No reimbursement
Hospital benefits	
(G) Deductible	(1) Current Medicare deductibles (2) $100 per admission (3) No deductible
(H) Copayment	(1) Current Medicare copayment (2) No copayment
Physician benefit	
(I) Deductible	(1) Current Medicare deductible (2) No deductible
(J) Copayment	(1) Current 20% (2) $5 per visit (3) No copayment
(K) Drug benefit	(1) No coverage (2) All prescribed drugs with $5 copayment per refill (3) All prescribed drugs, no charge
(L) Preventive visits	(1) No coverage (2) $5 per visit copayment (3) Covered in full
(M) Long-term nursing home care	(1) No coverage (2) $25 per day copayment

Pretest Results

We conducted pretests among senior citizen groups in Southern California with draft versions of the questionnaires that would collect the information needed for our model. In general, these pretests confirm that Medicare beneficiaries give realistic answers to hypothetical questions about whether they would enroll in AHPs specified by the characteristics we plan to include. The yes-no question format worked better in the pretests than a probability-scale format. The order of questions did not seem to affect data quality. The number of hypothetical plans presented did not affect the proportion of incomplete forms or the quality of the responses.

Experimental Design

The experimental design concerns (a) how we combine values of the attributes to describe alternative plans and (b) how we assign the resultant combinations to the survey respondents. Our strategy involves two steps: First, we stratify participants into subgroups based on demographic and health status characteristics. Second, within each stratum, we pose combinations of AHP attributes that will allow us to estimate the main effect of each attribute and the effect of selected interactions on the likelihood of joining the plan. Choosing a design strategy that allows us to estimate the effects separately for each stratum preserves a great deal of flexibility for examining how the effect of plan attributes on the decision to join an AHP varies with beneficiary characteristics.

The AHP attributes include 12 nonprice characteristics that take on a discrete number of values; the thirteenth attribute, the plan premium, can assume any value over the range shown in Table S.1. The first 12 characteristics we call "factors." A design that included hypothetical AHPs described by all possible combinations of the factors (a full factorial design) would enable us to estimate the main effects of the factors and all interactions among the factors, but it would require 41,472 different hypothetical plan descriptions to be posed in each stratum. Clearly, this is not feasible. Moreover, we are primarily

interested in the main effects of the factors and in interactions by variables that describe the structure of the AHP (e.g., attributes A and B in Table S.1) with other characteristics of the AHP. A *fractional factorial* design requires only a subset of combinations in the full factorial design and yet enables us to estimate the desired effects.

The basic strategy is to construct a set of plans for each stratum using a fractional factorial design for the 12 nonprice factors and to assign a premium for each hypothetical plan using randomization. Different combinations of the attributes (different AHP descriptions) are presented to different individuals in a stratum; this yields responses to all combinations of factors in the stratum that are necessary to estimate the specified effects. The plan descriptions may also differ among strata. For example, we can construct different AHP descriptions for two strata in a way that will allow us (a) to estimate the desired effects for each stratum separately and (b) to test for additional and higher-order interactions if the two groups respond in the same way (and can be combined).

Sample Design

Our sampling frame is the Health Insurance Master File (HIM); it identifies each person entitled to Medicare benefits and provides some basic information about the individual. We want to sample n individuals partitioned into m strata of k each (n = mk). We draw the sample in m strata of size k so that all k individuals in a stratum will be similar in terms of their demographic and health status characteristics.

The strata should define groups that may respond differently to the "treatments," or variations in AHP attribute values. Health status and residence are especially important. Health status, measured by prior use data from the HIM, is important for studying biased selection. Residence information is a proxy measure for beneficiary awareness and understanding of the AHP option, because in some areas of the country no AHP is yet available to beneficiaries. Other measures available from the HIM that can be used to define strata include age, sex, and welfare status.

The number of beneficiaries to be sampled depends on two factors: (a) how small an effect we wish to detect, where effect is the change in the likelihood of joining an AHP associated with a change in an AHP attribute and (b) how many different plans we present to each beneficiary. Based on our initial pretest, we plan to pose 9 hypothetical plans to each individual. Table S.2 shows the number of sample individuals needed to detect alternative effect sizes, assuming each individual responds to 9 plans.

Analysis Plan

Our analysis will be carried out in two phases. In Phase 1, we will fit a model that describes the relationship between the likelihood of joining an AHP and the characteristics of the plan and of the beneficiary. This model will allow us to *predict* the likelihood of joining a plan with any combination of attribute values, even if that

Table S.2

SAMPLE SIZE TO ESTIMATE VARIOUS EFFECT SIZES

	Effect Size (%)	Individuals* (N)
A.	Main Effect	
	4.2	500
	3.0	1000
	2.4	1500
	2.1	2000
	1.9	2500
B.	Interaction Effect	
	8.4	500
	6.0	1000
	4.8	1500
	4.2	2000
	3.9	2500

*Assumes 9 responses per beneficiary, with power of .8 and significance level of .05.

combination was not included in the options we presented. With the model, we can estimate the demand curve for AHPs and investigate biased selection problems. For example, to study biased selection, we will use the model to simulate who will choose to join a plan with specified characteristics.

The second phase of the analysis will draw on theoretical models of decisionmaking under uncertainty and estimate the parameters of the underlying decisionmaking model. Such a model characterizes how individuals integrate the specific details about an AHP into a set of outcomes and provides a decision rule for choosing among plans. It allows us to extrapolate to any set of options that can be characterized with the same outcomes and provides a potentially more powerful analytic tool than the descriptive model fit in Phase 1. For example, with the Phase-2 model we can generalize beyond a pairwise choice and simulate choices among any number of health plans, including choices among various indemnity plans.

ACKNOWLEDGMENTS

A number of individuals have provided assistance to us in developing the study design. Sally Carson developed the software to generate the experimental design points and she, along with Carol Edwards, developed software to produce each resultant questionnaire. We are indebted to Julie Brown for carefully overseeing implementation of the pretests. We have benefited from comments from Joseph Newhouse of RAND and from Tom Noplock and Mike Hupfer at the Health Care Financing Administration. Bob Bell gave us a careful and constructive review of an earlier draft of this Note. We are also grateful to Eve Kamhi for preparation of several versions of this manuscript.

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I. INTRODUCTION

BACKGROUND

Medicare is trying new ways to pay for services in an effort to control program costs. One method pays alternative health plans (AHPs) a fixed, per-capita amount to provide Medicare-covered services to beneficiaries who enroll in the plan. By making capitation payments, the government shifts financial risk and the responsibility for cost containment to private health plans.

The enthusiasm for encouraging Medicare beneficiaries to enroll in prepaid plans stems from evidence that health maintenance organizations (HMOs), historically the major example of prepaid systems, do deliver less costly care than the fee-for-service sector, primarily by reducing hospital admissions (Luft, 1981; Manning et al., 1984). Although this evidence is based on the experience of prepaid plans in serving those under 65, HMOs participating in the HCFA demonstration projects that preceded the Tax Equity and Fiscal Responsibility Act of 1982 (TEFRA) also reported dramatic reductions in the hospitalization rate of the elderly relative to rates in the fee-for-service system (Greenlick et al., 1983).

Current Medicare Capitation Program

TEFRA authorized Medicare to sign at-risk contracts with HMOs and other AHPs. As of March 1986, about 114 AHPs had signed TEFRA risk contracts (Ellwood, 1986). Alternative health plans share a feature that is not part of the current Medicare system, namely, they restrict the providers from whom patients can receive care. In fact, the TEFRA legislation defines plans that are qualified to negotiate risk contracts with Medicare to be those that provide services primarily through employees of the organization or through physicians under contract. By channeling patients to contract providers, the plan can control use and costs through utilization review, negotiated discounts, and other means.

Plans restrict the patient's choice by refusing to pay benefits, or at least full benefits, for services received from providers who are not part of the panel. If beneficiaries value freedom of choice of provider, then they face a cost when joining an alternative health plan. Hence, the plan must offer some inducement to attract beneficiaries.

The TEFRA legislation provides for such incentives through the capitation formula and regulations that limit the plan's premiums and profits. Currently, Medicare pays participating AHPs 95 percent of the adjusted average per capita cost (AAPCC). AAPCC is an estimate of the cost that would have been incurred by Medicare on behalf of enrollees in the prepaid plan had the care been received from fee-for-service providers in the county of their residence. The premium and actuarial value of any copayment that plans charge for Medicare-covered services cannot exceed the actuarial equivalent of the coinsurance and deductible amount for which beneficiaries who stay with fee-for-service are responsible. And plans that deliver care at a cost below the capitation amount must return the difference to beneficiaries in reduced cost sharing or additional benefits.[1]

Most experts believe AHPs deliver care at a cost below the capitated amount; savings of 15 to 25 percent are predicted. Thus, the requirement that AHPs return the savings to beneficiaries provides a strong financial incentive for beneficiaries to enroll in AHPs (Ginsburg and Hackbarth, 1987).

Options for Reform

If prepaid plans are to fulfill the promise of containing Medicare costs, a sufficient number of beneficiaries will have to enroll. Only about 2 percent of the total Medicare population are currently enrolled in AHPs under TEFRA risk contracts; however, HCFA estimates that

[1]The cost of care includes normal profits earned in delivering care to non-Medicare enrollees. The legislation defines this cost to be the adjusted community rate (ACR), which is the amount the plan would charge its general membership for the Medicare package of services, adjusted for differences between the utilization characteristics of the privately insured and the Medicare population.

enrollment will triple by the end of 1987 (Ellwood, 1986). In an effort to expand enrollments in prepaid plans even further, policymakers are considering options for changing current policies regarding capitation payments (for an in-depth discussion of policy options see Ginsburg and Hackbarth, 1986). Among the options are proposals to eliminate the current premium and profit regulations, thus allowing AHPs to retain part of their savings. This change is proposed to make it more attractive for new AHPs and new forms of AHPs to contract with Medicare. If plans retain higher profits rather than return the savings from practice efficiencies to beneficiaries, however, the incentive for beneficiaries to enroll in AHPs might be reduced.

Others argue that the potential budget savings from the AHP option are larger than what currently accrues to Medicare; to realize these savings, the capitation amount would be reduced. However, doing so would cut into the savings that AHPs return to beneficiaries and hence lower the incentive to enroll in an AHP. Assessing the impact on AHP enrollment of these various changes requires knowledge about how beneficiaries respond to financial incentives—knowledge that is unfortunately quite limited. (We review the existing evidence in Sec. II.)

Biased Selection

If cost-containment objectives are to be realized, the characteristics of beneficiaries enrolling in alternative health plans are as important as the number of beneficiaries who enroll. The AAPCC calculation adjusts the payments to an AHP to reflect differences between the plans' enrollees and the Medicare-eligible population of the county in terms of age, sex, welfare status, and institutional status. But evidence suggests that these adjustments account for very little of the variations among individuals in their utilization (Newhouse, 1986). Medicare will pay more than intended to plans that attract beneficiaries who use fewer services than average (favorable selection), and less than intended to plans that attract high utilizers (adverse selection). Either outcome is detrimental to the success of capitation as a cost-containment policy. If favorable selection occurs, the capitation

strategy could increase, rather than reduce, Medicare outlays. If adverse selection occurs, plans that lose money on their Medicare contracts might drop out of the program.

Most of the evidence on biased selection comes from the choices made by employees who are offered traditional fee-for-service coverage or enrollment in an HMO; less is known about biased selection in other types of AHPs (see Sec. II). Some evidence on biased selection among Medicare beneficiaries is available from the capitation demonstration project that began in early 1980, and experience with enrollment of Medicare beneficiaries in the second set of HCFA demonstrations (known as the Medicare Competition Demonstrations) and under the TEFRA system will add to this knowledge base. However, as plans change the package of benefits offered to beneficiaries, in response to selection or changes in the regulations covering Medicare risk contracts, the pattern of selection is likely to change. In addition, new types of AHPs, such as preferred provider arrangements, are being discussed for Medicare and selection effects in these arrangements are unknown.

Research Objectives

Our objectives for this research are to provide HCFA with the information needed to evaluate alternative Medicare capitation options. Specifically, we want to investigate the following issues:

- What plan benefits do Medicare beneficiaries value most?
- What restrictions on provider choice are most influential in the decision to join AHPs?
- What is the effect of price on the decision?
- What is the expected degree of biased selection in new systems?

To address these issues, we propose a method of data collection and analysis that will allow us to estimate a model that relates a beneficiary's decision to enroll in an AHP to attributes of the AHP.

An alternative to our model-based approach would be a demonstration project to compare who enrolls in several different AHPs. The demonstration approach, however, assumes that policymakers are interested in only a few particular types of plans, namely those included in the demonstration. The model approach, by providing estimates of how specific attributes influence choice, allows us to study a wide variety of plans. Furthermore, demonstration projects are costly, and results take several years to obtain.

The TEFRA experience will be an important source of information about the effectiveness of a number of participation incentives on beneficiary choice. However, existing plans typically differ from Medicare and among each other in many ways. Consequently, it is difficult, if not impossible, to isolate how component factors influence choice and predict how a different mix of features might fare. Nor can extant data inform policymakers about beneficiary acceptance of new forms of AHPs. To develop this analytic capability, therefore, requires new data.

APPROACH: "HYPOTHETICAL" OPTIONS

Presenting beneficiaries with hypothetical options in a survey and assessing their interest in the options is a procedure to generate such data. Marketing researchers typically use this technique to evaluate the market for new products, and there are several applications of the technique in assessing preferences among health plans. Results from these studies tend to support the validity of the approach (see Sec. III).

In this project, the hypothetical options will specify attributes of an alternative health plan, and survey respondents will be asked to evaluate whether they would choose the plan in preference to traditional Medicare. The attributes of the system will include: the degree to which freedom of choice of provider is restricted; the degree of case management that plans exercise to control cost; services covered by the benefit package; copayment provisions; and the cost (if any) to the beneficiary. The attributes will be selected to represent the variety

of existing and emerging AHP structures including closed-panel HMOs, Independent Practice Associations (IPAs), and Preferred Provider Organizations (PPOs).

Data collected from the survey will be used to investigate a number of important questions HCFA will face in considering various Medicare payment options. The data will be used to estimate models of how the decision to enroll in an alternative payment system varies with the structure of the AHP, the benefit package, premiums, and other dimensions of plan attributes that are varied in the hypothetical offers. These models, in turn, will be used to simulate plan enrollment rates under any set of plan offerings. The models will also be used to simulate the pattern of biased selection that occurs and to predict what package of features attract specific risk groups. This information can guide policymakers in formulating capitation policy to draw both high and low users of Medicare services to AHPs and in formulating Medicare reimbursement to AHPs to correct for over- or underpayment stemming from selection.

OUTLINE OF THE REPORT

This document describes the design for the project. Section II of this report briefly reviews what is currently known about the sensitivity of AHP enrollment to differences between AHPs and fee-for-service coverage in costs and benefits and about biased selection into AHPs. Section III presents the proposed project design. It describes the attributes of the AHPs to be varied in the hypothetical options, the rationale for selecting the attributes to vary, the experimental design procedures to achieve variation in the attributes, the sample design, the other measures to be collected in the survey, and results from our pretest of the survey instrument. Section IV describes a plan of analysis for the data.

II. WHO CHOOSES AHPS–EXISTING EVIDENCE

CHOICE BETWEEN AHPS AND TRADITIONAL INSURANCE

In deciding whether to join an alternative delivery system, Medicare beneficiaries must weigh financial consequences of the decision against restrictions on freedom to choose providers and other nonmonetary attributes of the plan. The perceived financial consequences depend on the individual's expectations concerning future illness, the scope and breadth of benefits offered by the AHP versus Medicare (and the individual's supplementary coverage), and the premium differentials.

Most of the research evidence on factors affecting the decision to enroll in AHPs pertains to the employed population, because until recently the opportunity for Medicare beneficiaries to enroll in AHPs was limited. Many of these studies used demographic, economic, and health characteristics of individuals and families to explain the choice, under the hypothesis that persons who are most vulnerable to high out-of-pocket costs of medical care will be more likely than those at less risk to select an AHP (see Luft, 1981, for a review of many of these studies). This is because AHPs typically offer benefit packages that require lower out-of-pocket costs for medical care but higher premiums than policies covering care received in the fee-for-service system. Few studies, however, have explicitly modeled how relative differences in premiums and benefits affect the choice.

In a study of employees in one firm, Berki et al. (1978) did try to quantify the role of financial consequences in AHP choice. They concluded that the ability to maintain an established physician relationship is worth at least $60 to $120 per year (in 1974 dollars; about $120 to $240 in current dollars). Thus, the financial benefits of joining an AHP must exceed this threshold to induce individuals to enroll in an AHP that would require changing physicians. However, this study did not investigate how choices are affected by cost differentials above this threshold.

Empirical estimates of the response of enrollment decisions to price are summarized in Table 1. The estimates suggest that a 10-percent decrease in the premium of an AHP, other things constant, increases enrollment by about 2 to 5 percentage points if the plan is an open-panel AHP, and about 1 to 2 percentage points for closed-panel AHPs. Based on these few available studies, enrollment in open-panel AHPs (which offer greater latitude in selecting a physician) appears to be more responsive to price than enrollment in more restrictive, closed-panel HMOs. However, Welch (1986) found that only about one-third of the enrollment response to changes in premiums of closed-panel AHPs occurs instantaneously; over the longer run, Welch's data indicate that a 10-percent decrease in the closed-panel AHP premium will induce a 5-percentage-point increase in enrollments. Whether the instantaneous response to price changes in other forms of AHPs similarly measures only a part of the full response is unknown.

These results are all from studies of employee groups. A review of several studies of the demand for traditional insurance suggests that the price elasticity of demand among Medicare beneficiaries is similar to that among the employed population (Marquis et al., 1985). This similarity in price response may not carry over to enrollment in AHPs; some believe the elderly will be more reluctant than younger people to break existing physician ties and enter AHPs (Bonanno and Wetle, 1984).

The HCFA capitation demonstrations, however, suggest that beneficiaries will be attracted to AHPs given strong enough financial incentives. Galblum and Trieger (1982) conclude that financial considerations are important in Medicare beneficiaries' AHP enrollment decisions. This conclusion is based on the high rate of enrollment in two AHPs participating in the Medicare capitation demonstration project. These AHPs offered beneficiaries a generous benefit package in contrast with much lower enrollment rates in other AHPs that provided much less generous benefits. Further, financial aspects, including benefits and costs, are reasons most often cited by beneficiaries for joining an AHP (Friedlob and Hadley, 1985).

Table 1

ENROLLMENT RESPONSE TO CHANGE IN AHP PREMIUM

Source	Type of AHP	Change in probability of enrolling in AHP given 10% decrease in AHP premium
Ashcraft et al. (1978)[a]	open panel	.02
Jurgovan and Carpenter (1974)[a]	open panel	.05
Sorenson and Wersinger (1981)[a]	closed panel	.02
Piontkowski and Butler (1980)[a]	closed panel	.01
Welch (1986)	closed panel (short-run response)	.02
Welch (1986)	closed panel (long-run response)	.06

[a] Estimate derived in Marquis et al. (1985).

Nonetheless, no quantitative estimates exist of the price sensitivity of Medicare beneficiaries' decisions to join AHPs. A major objective of the proposed study is to collect data that will enable us to estimate the response.

BIASED SELECTION

The AAPCC, which is the per capita payment rate for AHPs, is intended to represent the amount that Medicare would pay for an AHP enrollee had he or she received care in the fee-for-service system. The AAPCC takes into account differences in utilization patterns among beneficiaries who differ in age, sex, institutional status, eligibility for Medicaid, and reason for entitlement. If those choosing to join the AHP are sicker or healthier than other persons in their group, biased selection occurs.

Biased selection is of concern to Medicare and to the AHPs that sign TEFRA contracts. If favorable selection occurs and the AHPs draw healthier than average beneficiaries, then Medicare outlays may actually increase. If adverse selection occurs and the AHPs draw sicker than average beneficiaries, then the AHP may lose money on the TEFRA contract.

Studies of biased selection in the choice between conventional coverage and a prepaid group practice have found that, prior to joining the group, those who select the group had lower utilization than those who remained in the fee-for-service system. This result is found in studies of employees (Buchanan and Cretin, 1986; Merrill et al., 1985; Jackson-Beeck and Kleinman, 1983) and of Medicare beneficiaries (Eggers, 1980; Eggers and Prihoda, 1982). In the case of Medicare beneficiaries, however, other research has produced some, though inconclusive, evidence that a smaller proportion of persons joining an AHP hold private insurance to supplement their Medicare benefits (Friedlob and Hadley, 1985). Thus, the difference in prior use between those joining an AHP and those remaining in the fee-for-service system may arise from differences in insurance coverage rather than from differences in health risk.[1] Comparisons of health status measures of beneficiaries who join an HMO with measures for those who do not join indicate favorable selection in some HMOs but no biased selection in others (Garfinkel et al., 1986; Friedlob and Hadley, 1985). Moreover, the favorable selection that occurred could be due to the health screening program that demonstration HMOs were permitted (Garfinkel et al., 1986). Now participating HMOs must hold an annual open enrollment period during which Medicare beneficiaries must be accepted regardless of health status.

In addition, the lower prior use among persons joining AHPs appears to apply only to closed-panel HMOs and not to IPAs, in which patients have greater latitude in choosing their provider of care and often do not have to break established ties to a physician in order to join the

[1] Those with greater health risk, however, may also be those who are more likely to purchase supplementary insurance.

plan. Studies of persons with the option of joining IPAs have found no difference in the prior use between those joining and those remaining in the fee-for-service system (Merrill et al., 1985; Eggers and Prihoda, 1982). IPAs represent a growing share of AHPs that are negotiating risk contracts with Medicare. As of January 1986, half of Medicare risk contacts were held by IPAs, but three-quarters of pending contracts were with IPAs (Ellwood, 1986). This trend suggests that the threat that favorable selection in AHPs poses for the Medicare budget may diminish in the future. However, some IPAs are seeking to be released from their Medicare risk contracts because they are suffering economic losses (Ginsburg and Hackbarth, 1987). If this reflects adverse selection,[2] it poses a threat to the sustainability of the Medicare capitation option.

Because biased selection is a potential obstacle to the long-run success of the Medicare capitation strategy, research has begun to investigate ways of modifying the AAPCC formula to solve the problem. These studies have focused on identifying additional adjustment factors that improve the ability of the AAPCC to explain variations in use in the fee-for-service system (Beebe et al., 1985; Thomas and Lichtenstein, 1986). However, that research alone cannot inform us about the reduction in biased selection that new formulas might achieve. That assessment requires monitoring or simulating a policy change. The models of AHP choice that we will develop will permit us to simulate the consequences of biased selection using the current payment formula and alternative pricing policies.

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[2] The finding that some IPAs are withdrawing because of economic losses, however, does not necessarily imply that adverse selection has occurred. Even without adverse selection, some TEFRA contract holders will lose by chance.

III. STUDY DESIGN

THE APPROACH

Data to estimate the demand for AHPs will come from a mail survey of Medicare beneficiaries (with telephone follow-up as necessary). The survey will ask the beneficiaries to choose between traditional Medicare coverage and a number of prototypical AHPs. The AHPs will differ in a variety of ways including the degree of restriction on choice of provider, the scope and breadth of benefits, and the premium or cost to the beneficiary. The choice of AHP features to examine is discussed later in this section.

We prefer a mail survey to other modes for three reasons. First, the presentation of the AHP features requires the use of visual aids that cannot be used in a telephone interview. Second, we want to allow beneficiaries time to think about their preference, consult others for advice, and provide a reasoned response. Finally, it is less expensive than an in-person interview.

The advantages of posing a hypothetical AHP option are twofold. First, we can separate the influence on beneficiaries' preferences of the disparate factors that are usually confounded in observational studies. Second, we can investigate preferences for new forms of AHPs that are not currently available to Medicare beneficiaries.

Because the estimation data will consist of responses to hypothetical offers, one might question how well the results from the study would predict beneficiaries' behavior if they really faced the choices outlined in our survey instrument. Fortunately, several previous studies suggest that carefully designed hypothetical questions about health care and health insurance purchases will provide data of sufficient accuracy for the analyses we propose. The rest of this introductory section briefly reviews what is known about the approach.

Participants in the RAND Health Insurance Experiment (HIE) were asked if they would purchase supplemental insurance that would cover all or some fraction of the maximum yearly amount they would have to pay

under the basic health insurance plan to which they had been randomly assigned. Premiums for this hypothetical supplementary insurance were varied to allow calculation of demand curves for supplementary insurance (Marquis and Phelps, 1985). Reported willingness to purchase the supplementary insurance varied with the price of the supplementary insurance; the implied price elasticity corresponded fairly closely to price elasticities estimated with actual data on purchase of supplementary health insurance by federal employees (Holmer, 1984) and by Medicare beneficiaries (Long and Settle, 1982). The HIE results also showed that respondents who expected to have higher medical expenses in the coming year were more likely to want to purchase the supplementary insurance. These results suggest that the answers of HIE participants to the hypothetical question conformed to the purchase behavior that would be expected if they had actually faced the decision whether to purchase supplementary insurance.

Marquis and Holmer (1986) have further analyzed these data, showing that models of risk aversion derived from economic and psychological models of decisionmaking under uncertainty fit these hypothetical data quite well; this evidence adds further support to the idea that such data accurately describe respondents' stable preferences. Finally, and perhaps most significantly, predictions from the Marquis and Holmer model of the amount of employee contributions to flexible spending accounts (FSA) corresponded with independent observations on actual employee FSA contributions (U.S. Department of Health and Human Services, 1985).

The HIE hypothetical questions concerned variation in the breadth of insurance benefits and the size of the premium. HMOs and traditional forms of health insurance, however, differ on many other dimensions. Several studies have obtained apparently reliable results using hypothetical questions about preferences for health plans characterized along these other dimensions. LaTour et al. (1983) used a questionnaire sent to a mail panel of Medicare-eligible respondents, in which health plans were defined in terms of selected combinations of the following attributes: the sponsoring organization, the degree of restriction on participating physicians and hospitals, physicians and hospital staff

friendliness and reputation for quality of care, deductibles, coinsurance and stop-loss provisions, and additional coverage for long-term care and mental health care. The effect of price on reported intentions to purchase was large and statistically significant in the expected direction. Furthermore, this study shows that Medicare beneficiaries who are also the subject of our research are willing to participate in the type of survey we propose; LaTour et al. obtained response rates of 57 percent from their survey that was mailed to Medicare beneficiaries in a consumer panel.

Rosko et al. (1985) used observed behavior, in addition to the internal consistency of results with what would be expected from economic theory or marketing experience, to assess the validity of results of a survey of intentions to join hypothetical health plans. Their sample consisted of just under 100 persons eligible for Medicare, about half of whom had already joined an HMO. (The sample was not selected randomly but rather consisted of people who had already expressed an interest in joining an HMO, so it is difficult to extrapolate the substantive results to the general population of Medicare beneficiaries.) Based on preference rankings of hypothetical plans varying in monthly premiums, office hours, group or solo practice, typical waiting time, travel time to physicians' offices, and coverage of eye care and prescription drugs, Rosko et al. were able to predict correctly for about three-quarters of their sample whether or not they had joined an HMO.

A similar validation technique was used for a study of consumers' choice among hypothetical sources of primary health care in rural New York state (Parker and Srinivasan, 1976). Again, the alternatives that consumers were asked to rank differed in numerous attributes: type of facility (physicians' office, health center, or nurse-practitioners' office), travel time, typical office waiting time, hours of operation, etc. (Price was not varied in this experiment.) Parker and Srinivasan then used fitted equations, in which rankings of the hypothetical plans were a function of plan and respondent characteristics, to predict the ranking each family would assign to each of the 16 actual providers in the area. The model predicted actual choices much better than would be

expected by chance and much better than simple predictions based on assuming that each family goes to the nearest provider or that each family ranked each attribute equally. These results are even more respectable if we take into account possible supply constraints; some of the consumers in the Rosko et al. and Parker and Srinivasan studies may not yet have found or been accepted by the plan or health care provider that they most preferred.

One other health care marketing study (Rosko and McKenna, 1983) applied a split-sample technique quite common in the market research literature. College students were asked to rank 20 hypothetical ambulatory care services that varied in numerous dimensions. The model relating these rankings to plan and respondent characteristics was then used to calculate *predicted rankings* for seven more hypothetical plans; the predicted rankings accorded well with the rankings independently assigned by the students to these seven new hypothetical plans. Split-sample comparisons of models of choice among traditional health insurance plans that were derived from hypothetical preference data support the reliability of responses to hypothetical choices (Hershey et al., 1985).

Outside the fields of health care and health insurance, thousands of published market research studies have used data elicited by asking consumers about their likelihood of purchasing hypothetical products or services defined by several attributes. One recent comparison of hypothetical responses and actual market transactions concluded that both data sources yield the same demand relations (Dickie, Fisher, and Gerking, 1987). However, other studies have reported some problems in relating reported intentions to actual purchases. The marketing literature is reviewed by Belk (1985), who concludes that reported intentions are most useful as predictors when "involvement is high, intentions are strong, and the impacts of unrecognized situational, habitual, affective, and curiosity effects are comparatively weak" (Belk, 1985, p. 27). Thus, we expect reported intentions to be better predictors of choosing health plans than of demand for more trivial products typically purchased without much forethought (*low involvement*), or products purchased as a matter of habit at every trip to the grocery

store, or products that consumers will try in different brands out of curiosity.

The studies summarized above give us reasonable assurance that the hypothetical AHP approach proposed is feasible. Studies of health services have shown that consumers can assess the relative attractiveness of hypothetical health plans. Therefore, we tested the approach in several pilot studies. The findings from those studies, which will be discussed later in this section, also suggested that the responses to hypothetical questions will produce valuable data. Furthermore, we will use the Medicare experience under TEFRA to validate our approach. We will use the model estimated on the hypothetical responses to predict enrollment shares in markets that have a Medicare AHP option. A comparison between the predicted and actual enrollments in these markets tests both (a) the validity of the hypothetical responses and (b) the validity of the assumptions underlying the form of the model we fit.

PLAN ATTRIBUTES

Table 2 outlines the attributes of AHPs that we will study and gives the alternative values for each attribute. Attributes are divided into three main groups: those related to provider choice restrictions (attributes A through D in Table 2), the plan premium (attribute E), and the plan benefits (attributes F through M).

Several considerations guided our choice of attributes to study. We wish to study attributes of AHP options available to Medicare beneficiaries that policymakers might influence. Thus we focus on the types of plans that qualify and the financial aspects of the options rather than attributes, such as wait times to appointment or travel time.[1]

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[1]This is not to say that we believe that these other attributes have no effect on the decision to join an AHP; research indeed suggests that these factors do matter. However, it is unlikely that policymakers can influence these attributes. In terms of our estimation, these other attributes are unmeasured characteristics and their influence on individuals' choices are part of the residual error term.

Choice of Provider

A key characteristic of an AHP, as defined by TEFRA, is that it provides services through health care providers under contract. Therefore, enrollees must restrict their choice of provider or suffer some adverse financial consequence. We wish to vary the degree of choice restriction to reflect the variety of AHP structures that are, or might in the future be, offered to Medicare beneficiaries. For example, restricting coverage to care obtained from physicians practicing at plan facilities and at the participating hospital is characteristic of closed-panel HMOs. Providing reduced benefits unless the patient seeks care from a restricted list of providers is characteristic of preferred provider organizations (PPOs), whereas providing benefits only if care is obtained from a restricted list of providers reflects an independent practitioner association (IPA). Attributes A and B attempt to capture these characteristics of capitation plans.

For most scenarios, we will tell beneficiaries to assume that their usual physician is not a member of the AHP (see attribute A). We assume that if the usual physician is a member, then the beneficiary will join the plan because he or she stands to gain from so doing; in simulating plan enrollments, we will adopt this assumption in most instances (see Sec. IV for a discussion of the simulation analysis). However, in the most restrictive choice situation, which is characteristic of an HMO, beneficiaries must also obtain their care at plan facilities rather than the physician's fee-for-service practice location. In this situation, beneficiaries may require more than a token financial incentive to join the plan, even if the usual provider is a member of the plan. Therefore, we include variation in whether the usual physician is a member of the group (see attribute A).

Attributes were also selected to capture additional restrictions that Medicare or health plans might impose in order to make costs more controllable and predictable. Thus, we vary the length of the enrollment period and whether the primary care provider serves as a "gatekeeper" who must provide referrals for all specialty care. These are seen in attributes C and D, respectively.

Table 2

ATTRIBUTES OF PROTOTYPICAL ALTERNATIVE HEALTH PLANS

Attribute	Alternative Levels
Provider Choice Restrictions	Assume usual provider not a member and choose from:
(A) Physician Choice	(1) list that includes 50% of area providers (2) list that includes 10% of area providers (3) providers practicing at the plan facility/facilities (about 10% of area providers participate) Assume usual provider is a member and choose from: (4) providers practicing at the plan facility or facilities (about 10% of area providers participate)
(B) Hospital choice	(1) Any hospital patient and doctor choose (2) Those hospitals associated with plan
(C) Enrollment	(1) Can disenroll with 30-days' notice (2) Enrollment for 1 year
(D) Case management	(1) Patient can self-refer to specialists (2) Personal physician is gatekeeper to specialty care
Premium	
(E) Monthly premium	Quoted premium is -A + B*(Actuarial value of additional benefits G-L) + C*(Actuarial value of M) where ranges are: A from 0 to $50, B from .5 to 1.5, C from .05 to 1.5, and A, B, and C are generated randomly.
Plan Benefits	
(F) Reimbursement for out-of-plan use	(1) 50% cost to maximum of $500 per year (2) No reimbursement
Hospital benefits	
(G) Deductible	(1) Current Medicare deductibles (2) $100 per admission (3) No deductible
(H) Copayment	(1) Current Medicare copayment (2) No copayment
Physician benefit	
(I) Deductible	(1) Current Medicare deductible (2) No deductible
(J) Copayment	(1) Current 20% (2) $5 per visit (3) No copayment
(K) Drug benefit	(1) No coverage (2) All prescribed drugs with $5 copayment per refill (3) All prescribed drugs, no charge
(L) Preventive visits	(1) No coverage (2) $5 per visit copayment (3) Covered in full
(M) Long-term nursing home care	(1) No coverage (2) $25 per day copayment

Premiums and Rebates

A primary objective of this research is to determine the financial incentive necessary to induce beneficiaries to join an AHP and to estimate how the size of this incentive varies among AHPs of various types and between beneficiaries of differing characteristics. Under current TEFRA regulations, these incentives take the form of reduced cost-sharing for Medicare-covered services or in coverage for additional services at a cost that is presumably below that of policies that cover these services in the fee-for-service sector. Some advocates of capitation have argued that plans also should be permitted to return savings to beneficiaries in the form of cash rebates.

The incentives offered in the hypothetical plans in the survey thus will include both cash rebates and expanded coverage. If the AHP offers no additional coverage, a rebate ranging from $0 to $50 a month will be quoted for joining the plan (this is the A in the formula for attribute E). We restrict the maximum rebate per year to $600 because this is in line with proposals that have been made to allow AHPs to offer rebates to Medicare beneficiaries.

If the AHP provides benefits beyond the standard Medicare package, we will compute a quote for the package according to the formula shown in Table 2. If the term A in the formula exceeds the second and third terms in the formula, we quote a rebate; otherwise we quote a premium. The quantities B and C in the premium formula can be thought of as a price per additional unit of benefit; C is the price for the long-term care benefit, and B is the price for a unit of benefit for all other services.

Plan Benefits

Estimates of the amount of cash rebate associated with each type and level of restriction on beneficiaries' freedom of choice would enable us to determine the size of the financial incentive required for alternative AHPs. However, in order to estimate whether an expanded benefit package yields sufficient financial incentive, we also need to measure how beneficiaries value additional insurance benefits.

The benefit variations were chosen for assessing how beneficiaries value coverage for services that have a low probability of occurring but are costly—such as the catastrophic hospital benefit (attribute H) and the long-term care benefit (attribute M)—and for services that have a high probability of occurrence but are less costly—such as the coverage of the physician deductible (attribute I) and the drug benefit (attribute K). By estimating how beneficiaries value different types of risk, we may be able to generalize to packages that provide expanded benefits for health services that are not among those varied in the hypothetical options (see the analysis plan in Sec. V).

The benefits we chose to vary were also picked because of considerable interest in how beneficiaries value coverage for these services beyond their role as inducements to join AHPs. Consumer groups have long advocated extending Medicare coverage to prescription drugs. Provision of catastrophic hospital coverage in Medicare has often surfaced in the health policy debate. Recently, the problem of underinsurance for long-term care has attracted a great deal of attention, and some analysts believe that prepaid capitated plans are the most logical vehicle for insuring long-term care (Knickman and McCall, 1986). The simplified form of the long-term care provision under attribute M is based on a comparison of the most common policies now on the market or soon to be offered by private insurers. We include preventive care because HMOs generally emphasize health promotion rather than just care for the ill, and the Department of Health and Human Services recently announced a number of health promotion efforts for the elderly (Heckler, 1985).

Variation in the benefits provided for out-of-plan use (attribute F) is included to assess the potential for PPOs to attract Medicare beneficiaries. AHPs that have signed TEFRA at-risk contracts to date include HMOs and IPAs that provide benefits only if services are received from plan providers. However, the TEFRA definition of an AHP appears to permit participation of PPOs, which typically offer financial incentives to use contract providers but pay some benefits for out-of-plan use. In addition, a Medicare-sponsored PPO is one option for

further reform of the way Medicare pays for physician services. Consequently, efforts to establish the size and nature of PPO selection are important to evaluate this reform.

EXPERIMENTAL DESIGN

Overview

The experimental design concerns (1) how we combine values of attributes to describe the alternative plans and (2) how we assign the resultant combinations to the survey respondents. Our strategy involves two steps: First, we stratify participants into very small subgroups based on demographic and health status characteristics. Second, within each stratum, we pose combinations of AHP attributes that will allow us to estimate the main effect of each attribute and the effect of selected interactions on the likelihood of joining the plan. By choosing a design strategy that allows us to estimate the effects separately for each stratum, we preserve a great deal of flexibility for examining how the effect of plan attributes on the decision to join an AHP varies with beneficiary characteristics.

The AHP attributes include 12 nonprice characteristics that take on a discrete number of values; we will refer to these attributes as "factors." The thirteenth attribute, the plan premium, can assume any value over the range shown in Table 2. Our method for choosing the combination of characteristics that define any hypothetical plan treats the factors and the premium separately.

A design that included hypothetical AHPs described by all possible combinations of the nonprice factors (a full factorial design) would enable us to estimate the main effects and all interactions among the factors. However, for our 12 nonprice factors this would yield 41,472 possible combinations. Clearly, we cannot pose this many scenarios to an individual respondent, or even within each stratum. Moreover, we are primarily interested in the main effects of the factors and in a subset of interactions among the factors. A "fractional factorial design" requires only a subset of combinations in the full factorial design and yet enables us to estimate the desired effects.

The basic strategy is to construct a set of plans for each stratum using a fractional factorial design for the 12 nonprice factors and then randomly assign a premium to the plan. The methods are discussed further below.

Assignment of Factors

Since we cannot pose all possible combinations of attributes to an individual respondent, we need to determine who will receive what plan descriptions. Here we discuss the principles we used in determining our experimental design. The technical details are given in Appendix 1.

The appropriate design depends on how the data will be analyzed. In our case, we want to estimate all the main effects of the 12 nonprice factors as well as certain interactions among them. We are particularly interested in interactions among the restrictions on consumer choice; for example, whether the effect of restricting physician choice on the likelihood of enrolling in an AHP depends on whether the beneficiary must enroll for 12 months. Specifically, we want to estimate the following interactions:

A*B, A*C, A*D, A*F, B*C, B*D, B*F, C*D, C*F, D*F,

where "A*B" stands for the interaction between factor A (physician choice) and factor B (hospital care), etc. We assume that all other interactions are negligible. (The experimental design that we propose, however, identifies many additional interaction terms so that this assumption can be tested empirically.)

Our objective is then to choose a design that will allow us to estimate the specified main effects and interactions and to do so as efficiently (that is, with as much precision) as possible. To meet this objective, we wish to choose a design in which the main effects and interactions of interest are orthogonal in the scenarios posed to each respondent, or at least are orthogonal within a group of very similar respondents.

One experimental design that would be fairly easy to implement is complete randomization: We would simply generate each individual's scenarios randomly. While simplicity is an obvious advantage of complete randomization, there are several important disadvantages. First, complete randomization might be very ineffective for estimating the effects of interest.[2] Second, complete randomization might require that the analysis rely heavily on between-individual comparisons to estimate the factor effects, rather than within-individual comparisons. Within-individual comparisons are usually preferable because the individual serves as his or her own control; any difference in an individual's responses to different hypothetical AHPs is due to differences in the AHP characteristics. With between-individual comparisons, however, responses to the different hypothetical AHPs come from different respondents. Thus, differences in responses might be due to a difference between AHP characteristics, or to a difference between the individuals. We cannot distinguish the cause unless we make further assumptions, for example, that factor effects are the same from individual to individual.

The difference between within-individual comparisons and between-individual comparisons would not be very crucial if we were solely interested in the factor effects and not interested in the interactions between factor effects and the demographic variables. However, this is not the case in our study. To assess the nature and degree of adverse selection with alternative health plans, one of our goals is to determine whether different subpopulations respond differently to the plan attributes. Therefore, we need to emphasize within-individual comparisons for estimating factor effects so that we can examine whether factor effects do differ among individuals with different characteristics.

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[2]Our calculation shows that the design we propose is more than twice as efficient as complete randomization for estimating the main factor effects.

Since each respondent can only respond to a few scenarios, it is unlikely that complete randomization will result in a set of plan scenarios that allows us to identify and estimate the factor effects of interest for each respondent. An alternative to randomization is to use a fractional factorial design, in which we determine a priori the set of scenarios for each respondent. If we choose the set of scenarios appropriately, we can identify at least some of the factor effects of interest using the responses from only one individual.

While it would be desirable to use only within-individual comparisons, it is not feasible to identify all factor effects of interest using only within-individual comparisons, given the number of factors to study. For example, ignoring the interactions of interest, we have 1 + 7 + 4*2 + 1*3 = 19 main effects coefficients to be identified, including the grand mean coefficient. That is, we would have to pose 19 AHPs to each correspondent to identify all main effects coefficients from within-individual comparisons. This is a response task that we deem too burdensome. Therefore, some between-individual comparison cannot be avoided.

In order to estimate the interaction between factor effects and individual characteristics, however, we will restrict our between-individual comparisons to individuals who are matched in terms of observable demographic and health characteristics. We do this by forming very small clusters of matched individuals and selecting a design that enables us to estimate all factor effects of interest from the responses from individuals within each cluster. Since individuals in the same cluster are matched in terms of observed demographic variables, we expect them to respond similarly to plan attributes, even though individuals with other characteristics may respond differently.

The fractional factorial design we propose (see Appendix 1) requires only four matched individuals to identify all main factor effects. We can identify all factor effects of interest (including specified interactions) with 16 matched individuals.

In summary, our experimental design consists of a collection of fractional factorial designs, one for each cluster of matched individuals. All main effects and specified interactions can be estimated for each cluster. Differences between subpopulation in the response to factors can be estimated by comparing the responses of different clusters.

Premium Assignment

We will generate the monthly premium for each hypothetical plan as the random variable, -A + B*(Actuarial Value of Benefits G-L) + C*(Actuarial Value of Benefit M). If the plan offers no additional benefits, there will be a rebate equal to A, where A is a uniform random variable over the range ($0, $50). The terms B and C can be thought of as a price per additional unit of benefit; C is the price for the long-term care benefit, and B is the price for a unit of benefit for all other services.

The factors B and C are randomly selected so that log(B) is uniform over the range (log(0.5), log(1.5)), and log(C) is uniform over the range (log(0.05), and log(1.5)). Because we do not have data to compute the actuarial value of benefits for each individual respondent, we use the actuarial value for the average Medicare beneficiary to generate premium quotes. Because health expenditures are skewed, the individual price for a benefit—the price per dollar of coverage given the individual's health risk—will be higher than the multiplicative factor used to generate the quote for a large fraction of beneficiaries. A price that is actuarially fair (price equal 1) based on the expenditures for an average beneficiary will therefore exceed an actuarially fair price for the majority of beneficiaries. Therefore, we choose the factor B to be uniform on the log scale rather than the actual dollar scale so that the average multiplicative factor is less than 1. We apply a different multiplicative factor to the long-term care benefit than to other benefits, because expenditures for long-term care are even more highly skewed than other health expenditures; the vast majority of individuals incur no long-term care expenditures in a given year, and a

very small number incur very high expenditures. Therefore, we allow C to fall as low as .05 so that the resultant individual prices span the range of low and high prices.

The premium quoted for each plan depends on the value of the other attributes, because the actuarial value depends on the factors. However, the price of the plan--the cost per dollar of additional benefit--is generated randomly and is independent of the assignment of factors. Because they are independent, we can estimate interactions between price and the other factors. For example, we can estimate whether there is an interaction between price and the degree of restriction on provider choice, or equivalently whether the demand curves differ depending on the type of AHP.

SAMPLE DESIGN

Sample Selection

In many experimental studies, there are a limited number of experimental units (individuals) who can be assigned experimental treatments; therefore, it is difficult to construct clusters of matched individuals. In our study, there is a very large sampling frame of *potential* experimental units, namely, all Medicare beneficiaries. Our *actual* experimental units are a sample from the frame, and we can select the units so that a specified number are closely matched in terms of demographic characteristics.

One way to do this is as follows: Suppose we want to draw a total of n = mk individuals, partitioned into m strata of k each. Instead of drawing n individuals directly, we draw them in m clusters of k each. For example, we might draw m "seed" individuals randomly,[3] and then for each seed individual we draw k-1 individuals whose covariates match those of the seed individual. Each cluster of k individuals is one

[3]As an alternative to drawing the seed individuals randomly, we might instead use a stratified random sample based on Neyman allocation: we partition the sampling frame into R superstrata, R being substantially smaller than m. Within, say, the r-th superstratum, we draw $m_r = m^*N_r/N$ seed individuals, where N_r denotes the total number of individuals in the r-th superstrata, and N denotes the total number of individuals in the sampling frame.

stratum. We impose a fractional factorial design (described above) on each cluster or stratum. Since the individuals in a cluster are taken to match the seed individual, we can expect their characteristics (covariates) to be close to each other.

Our proposed design requires clusters of individuals of size 16 to estimate all main effects and the interactions between factors that we have specified to be important (see section on assignments of factors above), that is, in our study k is 16. However, we can estimate all main effects with clusters of size 4. Therefore, we might draw a hierarchical sample such that we first choose clusters of size 4, on whom we can estimate the main effect, using a stringent matching criterion, and meta-clusters consisting of four clusters (or 16 individuals) that are matched using a looser criterion.

Our design was chosen so that all factor effects of interest can be estimated for each cluster. However, if some members of a cluster do not respond to the survey, we will need to combine two or more clusters to identify the factor effects. In combining clusters, we must assume that individuals in the different clusters respond to factors in the same way—a much stronger assumption than is necessary when the analysis is based on only a single cluster. Because our study is a survey, we of course anticipate some nonresponse. (See the section below on response rates in our pretests.) Therefore, we plan to conduct the full data collection in two waves. In a two-wave strategy, a sample is selected and surveys administered to this sample. We will implement procedures to provide timely feedback on the design points for which we did not obtain complete interviews. A second set of individuals would be selected as replacements for the initial nonrespondents. (In order to implement the strategy, we will select several "reserve" individuals in each cluster to use in the second wave.)

Our information for selecting the sample will come from the Health Insurance Master File that identifies each person entitled to Medicare benefits. In choosing the covariates to be used in the matching, we at a minimum want to use characteristics that define groups that we hypothesize will respond differently to the various treatments. Three characteristics are of particular importance in this respect: age of the beneficiary, health status, and residence.

If the choices of newly entitled beneficiaries differ from the choices of beneficiaries who have been in the system for some time, then AHP enrollments may change over time simply because of the aging of the population. To simulate the long-run rates, then, we want to test for interactions between age of the beneficiary and AHP attributes in the response.[4]

Studying biased selection in AHPs is an important aspect of this study, and so we want to test for differences in the response to plan attributes among beneficiaries with low health risk and those with high health risk. Although the Health Insurance Master File does not provide measures of health status, the file does include some data that can be used as proxy measures, namely, recent hospitalizations and Part B payments.

In some areas of the country, several AHPs compete with each other to attract Medicare beneficiaries; in other areas, no participating AHPs are in operation. As a result, there are probably large differences in beneficiary awareness and understanding of the AHP option. Because familiarity with the AHP option may affect responses, we will use residence information and information about the availability of the AHP option in the area in the selection and matching of the sample.

In addition to age, residence, and prior use, other measures available from the Health Insurance Master File can be used for matching, such as sex and welfare status. The final set of variables to be used in matching and the matching criterion will be determined once we have received more detail about the data items available and have examined the distribution of and relationships among the characteristics.

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[4] We plan to exclude persons under age 65 who are entitled to Medicare benefits because of disability or End Stage Renal Disease. The responses of these groups may differ from the responses of the elderly, and attempting to estimate the responses for both groups would strain the data collection budget.

Sample Size and Precision

We consider the precision in estimating two quantities of interest: the main effect of a two-level factor, and the interaction between a two-level factor and a covariate. First we consider the main effect.

An example of a two-level factor is whether the primary provider is a gatekeeper. We want to estimate the effect of this factor. Assume that the experimental design successfully balances all covariates and other factors, so that whether the primary provider is a gatekeeper is orthogonal to (uncorrelated with) all other explanatory variables. Also assume that the responses we observe are the dichotomy "join" and "not join" the AHP. Let P0 denote the average probability that an individual joins an AHP when there is no gatekeeper (the value of the factor is set to zero), and P1 the probability when the factor is set to one.

We will test the hypothesis H_0:P0 = P1 using a two-sided test with significance level 0.05 (a 5 percent probability of rejecting the null hypothesis when it is true). It can be shown that the smallest effect δ = P1 − P0 that can be detected with 80 percent power (we can expect to reject the null hypothesis when it is false with probability 0.8) is given as follows:[5]

$$\delta \approx 2.80/\mathrm{sqrt}(n*g),$$

where n is the total number of individuals in the sample, and g is the number of scenarios per person.[6] The number of individuals sampled depends on the number of trials per questionnaire. We plan to present 9 scenarios to each individual, so the smallest effect size can be written as:

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[5]The probability of joining the plan depends on individual characteristics as well as other plan factors and the premium. We assume for now that the effect of the factor being considered is additive and has no interactions with other variables. It follows from the balanced nature of the fractional factorial design that the effect of the factor, P1 − P0, does not depend on the other variables, and can be estimated as if the other variables are not present.

[6] See Appendix 2; the calculation neglects the intraperson correlation.

$$\delta \simeq 2.80/\text{sqrt}(n*9).$$

The following table gives the minimum effect we can detect for various sample sizes.

Number of Individuals	Main Effect (P1 − P0)
500	.042
1000	.030
1500	.024
2000	.021
2500	.019

We now consider the precision for detecting an interaction between a two-level factor and a dichotomous covariate, such as sex. There are four possible combinations of the factor and the covariate. We will assume that each combination is equally likely, and thus a quarter of the sample is allocated to each combination.

We assume that the main effects for these two variables and their interaction are orthogonal to all other variables. Let P00 denote the probability that an individual with factor level zero and covariate level zero would join the plan, and P10 denote the probability that an individual with factor level one and covariate level zero would join, etc. The factor effect for individuals with covariate level zero is therefore P10 − P00, and it is P11 − P00 for individuals with covariate level one. If the effects of the factor and the covariate are additive, i.e., there is no interaction between the two, then we should have H_0: P10 − P00 = P11 − P01. If there is an interaction, the two factor effects are not the same, and the difference δ = (P10 − P00) − (P11 − P01) is the interaction effect.

Again using 0.05 significance and 80 percent power, it can be shown that the smallest interaction effect that we can detect, δ, is approximately 5.60/sqrt(n*9). The following table gives the smallest effect for various sample sizes.

Number of Individuals	Interaction Effect (P10 - P00) - (P11 - P01)
500	.084
1000	.060
1500	.048
2000	.042
2500	.038

The estimates of the interaction effect that we can detect assume that we are contrasting responses of two equal-sized subsamples; for example, that we are comparing the response of beneficiaries with health status scores above the median and the response of beneficiaries with scores below the median. We cannot expect to detect effects this small when comparing the response of population subgroups of unequal size--for example, a comparison of those under age 85 with those over 95--unless we oversample the small subpopulation group (and consequently undersample the larger group).

Although sampling groups with unequal probabilities does offer some advantages, we do not currently plan to do so. One reason to oversample some groups would be to increase the precision of estimated responses for certain subgroups, such as those over age 85. Another would be to increase the precision for estimating the parameters of the model relating individual characteristics to the likelihood of joining an AHP. For example, if we firmly believed in the linear model, we would attempt to select individuals whose covariate values are extreme. There are certain disadvantages of oversampling, however. First, analysis is complicated by the need to weight observations. Second, although oversampling may improve the precision for estimating some parameters and for making some population contrasts, it may reduce the precision for other contrasts. As a result, our recommendation is to select a self-weighting sample.[7]

[7] However, if HCFA identifies certain parameter estimates that are of highest priority for policy formulation, it may be desirable to oversample to obtain precise estimates of these parameters, even at a sacrifice in precision of other parameters.

MEASUREMENT DESIGN

In addition to beneficiaries' preferences about the hypothetical scenarios, we will collect information about beneficiaries that has theoretically or empirically been related to choice among health plans in previous studies. The information will come from three sources: (1) Data about the individual characteristics used to form the sampling strata will come from the Health Insurance Master File, as described in the preceding discussion on sample design. (2) Use of Medicare-covered services for the calendar year prior to the survey will be obtained from Medicare claims data. These data will enable us to address issues of adverse selection and also help us to evaluate, and correct for, any nonresponse bias. (3) The mail survey will gather additional information about past and anticipated health services use, measure beneficiaries' health status and satisfaction with their medical care, and obtain data about demographic and economic characteristics.

The key variables we will measure are discussed next.

Measures of Biased Selection

The nature and extent of biased selection into AHPs is a critical issue in Medicare capitation policy. At issue is whether those who select AHPs would use more or fewer services in the fee-for-service system than others in the same AAPCC pricing group who remain in the fee-for-service system. To answer this directly, we would want to compare subsequent medical care use of those who report they would join an AHP with the care used by those who would not. However, because we do not know what individuals' subsequent use patterns will be, we will use measures of prior medical care use, anticipated health expenditures, and health status to investigate biased selection.[8]

[8] A follow-up phase, however, could acquire information about subsequent use from the Medicare claims database. Unlike observational studies, we can compare the subsequent fee-for-service use of people who would prefer to join an AHP with those who would remain in the fee-for-service system because the AHP option is not available to beneficiaries in all areas.

Prior Use of Health Services. Most studies assess the degree of biased selection by comparing use rates of AHP enrollees and nonenrollees at a time prior to AHP enrollment (see Sec. II), because other studies have shown that an individual's past use is positively correlated with subsequent use (Eggers, 1980; Eggers and Prihoda, 1982; McCall and Wai, 1983; Anderson and Knickman, 1984).

We will obtain data about the use of Medicare-covered services for the year prior to the survey from the Medicare claims database. This will include hospitalizations from the Part A Stay Record and physician visits, outpatient laboratory use, and other medical services from the Part B files. These data will be extracted for all beneficiaries in the sample, including beneficiaries who do not respond to the survey, allowing us to assess potential nonresponse bias.

We will supplement the claims data with information collected in the survey in order to have measurements for newly entitled beneficiaries, for whom Medicare claims data for the previous year will not exist. We also wish to measure use of services that are not covered by Medicare. The noncovered services we will ask about will include those that are covered in one or more of the AHP hypothetical plans.

Anticipated Use of Care. Individuals can to some degree predict their future health care needs (Marquis and Holmer, 1986). If they use this information in selecting a health plan, and if Medicare or the insurer does not have (or use) the same information in setting prices, then biased selection will occur.

The survey will include questions to measure what individuals anticipate they will spend in the next year for various health services. The anticipated expense questions will cover expenditures for hospital care, physician care, and drugs. It will also cover expenditures for services that are not mentioned in the scenarios, such as mental health, hearing and vision, and dental care. By measuring beneficiaries' anticipated risk for a wide range of services, we can use the analytic estimates of how individuals value risk reduction (see analysis plan in Sec. IV) to simulate demand for AHP options that provide benefit packages that include services that are not covered in our hypothetical plans.

Health Status. To measure health status, we will use an adaptation of a short-form health survey recently developed by our RAND colleague John Ware and others. This instrument was developed from extensive evaluation of longer measures collected during the RAND Health Insurance Experiment (Brook et al., 1979). The new instrument was tested using data collected in a telephone survey of a national sample of adults and is currently being used in a large national longitudinal study of medical outcomes. The internal consistency for each scale was satisfactory for group comparisons; the validity of the scales was supported by the pattern of correlation among the scales and with other health and demographic measures. The items we will use measure physical functioning, role functioning, general health perceptions, and expected future health.

Patient Satisfaction

Beneficiaries' satisfaction with their current health care is likely to influence the decision to select an alternative health plan. Previous research has demonstrated that patient satisfaction is strongly related to subsequent doctor shopping (Marquis et al., 1983). Patients who are considering changing their usual provider may be quite willing to consider joining an AHP.

We plan to adapt the Patient Satisfaction Questionnaire (PSQ) developed by John Ware and his colleagues (Ware et al., 1976) to assess beneficiaries' satisfaction with their current care. The scaling properties, reliability, and validity of the PSQ have been widely studied and extensively documented.

For this research, we will assess two aspects of patients' attitudes that the PSQ was designed to tap: general satisfaction with care and attitudes about financial aspects. We include the general satisfaction measure because it predicts subsequent provider change. We include satisfaction about financial aspects of care because beneficiaries who are dissatisfied with the cost of traditional fee-for-service coverage may be more attracted by the financial benefits of joining an AHP than beneficiaries who are satisfied with this aspect of care.

Other Beneficiary Characteristics

We will also collect measures of age, sex, race, education, family income, whether the individual has a usual provider, eligibility for Medicaid, the extent and cost of Medigap coverage, whether the individual has ever enrolled in an HMO, and place of residence, to use as explanatory variables in our models of health plan choice.

Survey Questionnaire

Our questionnaire is reproduced in Appendix 3. The modules concerning sociodemographic characteristics, health status, patient satisfaction, prior and anticipated use of medical care, and current health insurance coverage will be identical for all respondents. The module concerning the AHP options, however, will vary from individual to individual as discussed in the section on experimental design. The questionnaire shown in Appendix 3 is illustrative of the way that the AHP scenarios are presented to respondents; it does not actually represent one of the design points.

DESIGN PRETEST RESULTS

We have conducted several pretests of the measurement design. These pretests were designed to answer a number of issues concerning the measurement design including: (1) the best response format for the hypothetical scenarios; (2) the number of scenarios a single respondent can reliably answer; and (3) the effect of question order on responses. In addition, because each survey respondent will receive a tailored set of hypothetical scenarios, special software that produces the plan descriptions and produces each questionnaire is needed. The pretest phase gave us the opportunity to develop and test this software.

The pretests were conducted in several group administrations using senior citizen groups in the Southern California area. The pretests were conducted in two phases. In the first phase, 35 pretests were completed using questionnaires that differed in the response format, number of scenarios (8, 16, and 24), and ordering of the health status, patient satisfaction, and scenario sections. Because the effect of

question format and questionnaire length on the reliability of responses to the hypothetical scenarios was of paramount interest, we included two "ringers" among the set of plans offered to every beneficiary in the pretest. One posed restrictions on the beneficiary's freedom of choice of provider but offered no incentive to join; we expect most beneficiaries to turn this offer down. The other offered expanded benefits at no premium and with no restriction on freedom of choice; we expect most beneficiaries to accept this offer.

On the basis of the results from the first pretests, we revised the survey and conducted a second round of 36 pretests, designed to further resolve the question about the number of scenarios to ask.

Pretest Results: Answer Format

One issue for pretest was whether to ask respondents to make a dichotomous choice ("Yes, I would choose the plan" versus "No, I would not") or to ask them to rank the likelihood that they would join a hypothetical AHP on a probability scale. The latter approach has been used in the marketing literature (e.g., Rosko et al., 1985). However, evidence from laboratory studies suggests that individuals have difficulty assessing probabilities. In the first phase of pretests, we systematically varied the form of the response choice.

The pretest results clearly indicated that beneficiaries had difficulty with the probability scale. Over half of those presented with the probability scale format failed to provide an answer to a single scenario. All respondents presented with the dichotomous choice answered some of the hypothetical offers, and most answered all of them. In addition, among those who did respond using the probability scale, there was a strong tendency to check the lowest scale point for all plans, including the plan designed to elicit a "buy" response. The yes/no answer format did not evidence this "nay-saying" bias.

Based on the initial pretest results, therefore, we decided to use the dichotomous response choice for the second round pretest and for the final survey instrument.

Pretest Results: Order of Questionnaire Components

The initial pretest also included questionnaires that differed as to the order in which different components of the questionnaire were presented. We varied order to investigate whether patient satisfaction and health status assessment appear to be affected if these items are asked before or after the plan scenarios. Order did not appear to matter. Missing data rates on the health status and patient satisfaction items were the same whether they preceded or followed the scenarios, and scale scores did not differ.

Pretest Results: Number of Scenarios

The marketing literature does not appear to provide information about the relationship between data quality (reliability and validity) and respondent burden (number of attribute packages presented), though reasonable results have been obtained when consumers have ranked as many as 20 attribute packages (Rosko and McKenna, 1983). Consequently, we designed our pretest to address this. The first phase of pretests included questionnaires that posed 8, 16, and 24 AHPs (initial work on the experimental design had indicated that multiples of 8 were desirable). Stopping work is a sign of fatigue; however, among those presented with the dichotomous answer format, most completed all of the scenarios regardless of the number posed. In addition, the missing data rate for items that were asked after the scenarios did not increase as the number of AHP scenarios posed increased. These measures, therefore, did not indicate that responses deteriorated with the increasing number of attribute packages.

Other measures, however, suggest a potential effect of length. There appeared to be a slight "nay-saying" bias among respondents who were given 16 or 24 scenarios, and these respondents were also more likely to say "no" to the scenario intended to elicit a "buy" response than respondents receiving only 8 AHP options. Therefore, in the second round of pretests, we continued to test for burden effects on response quality. In the second phase, we included instruments that posed 3, 6, and 9 plans (further design work indicated that we could achieve high precision with 9 scenarios).

As in the first pretest, neither stopping work in the scenario section of the questionnaire nor missing data in subsequent sections seemed to be related to the number of AHPs that were presented. The distribution of completed scenarios was distinctly bimodal for each variant: Most respondents completed all scenarios. Those who did not tended to complete one or two, perhaps indicating that these individuals did not fully understand the task.

Beneficiaries' responses about the AHPs did relate to the price of the alternative and to the degree of restriction. The likelihood that a beneficiary would indicate a preference for the AHP increased as the price (the premium divided by the actuarial value of the additional benefits) decreased and as an index of freedom of choice restriction decreased. This finding is based on an examination of the data from pooling the two pretests, yes/no preferences for 399 scenarios (ignoring the designed "ringer" plans). The finding that changes in AHP characteristics affect the likelihood that a beneficiary would (hypothetically) purchase the plan in the expected direction tends to support the proposed approach.

Controlling for price and the restriction index, the number of scenarios posed did not affect the likelihood that a beneficiary would indicate the preference for the AHP. Neither the response to price nor the response to the degree of restriction appeared to vary with the number of scenarios presented--indicating the absence of systematic effects of length on data quality.[9] Consequently, we conclude that we can pose 9 hypothetical scenarios as called for in our experimental design.

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[9] There did appear to be a greater effect of variations in the degree of restriction on the likelihood of preferring the AHP among respondents in the first pretest than in the second pretest; because respondents were presented with more scenarios in the first pretest this did result in a small decrease in the response to the restriction index as the number of scenarios falls. However, we attribute the difference in response to the fact that the majority of respondents to the second pretest had prior AHP experience, whereas few of those in the first pretest had this experience and not to an effect of length.

Pretest Results: Response Rates

The early pretests we conducted suggested that we can obtain information about beneficiaries' preferences for AHPs by presenting them with hypothetical options. However, these pretests were administered in groups rather than using a mail survey, which is the mode of administration we propose for the full data collection. Therefore, we carried out a third pretest to investigate whether we would encounter response problems or data quality problems using the mailed questionnaire.

Our procedures called for a phone contact at the time of the initial mail-out to inform respondents about the purpose of the study, a post-card reminder mailed to nonresponders after the first two weeks, and a second questionnaire mailing and phone call at four weeks. However, we were unable to find telephone numbers for about half of the selected sample; consequently, we were unable to make initial or follow-up telephone calls to these individuals.

Respondents were to be paid a token amount for participation in the study. Half of the sample was mailed the check with the initial mailing; the other half was promised payment for return of the questionnaire.

Response rates were much higher among those for whom we were able to contact by phone than among those whom we could not. Some of this difference is probably because we are unable to identify all selected names who are not part of the sampling frame (namely, deceased individuals and those in institutions) when we have no contact. That is, the response rate among eligible sample individuals is probably higher than the return rate for the group that we were unable to phone. However, because the response rates differ so markedly, we are investigating additional sources to convert information obtainable from the Medicare Beneficiary File to telephone numbers in an effort to reduce the proportion of individuals we cannot contact by phone.

We also found that mailing the check with the questionnaire was more effective in eliciting returns than the promise of payment; within both the group we contacted by phone and within the group we did not

phone, response rates were about 10 percentage points higher for the subsample that received payment with the mailing.

Based on this pretest, we can expect response rates of about 60 percent among beneficiaries whom we are able to reach by phone and response rates of about 30 percent among others. While these response rates are somewhat lower than we hoped to obtain, they do compare favorably with other studies of Medicare beneficiaries that posed hypothetical insurance offers. Our 60 percent response rate among beneficiaries we contacted by phone is similar to the 57 percent response rate reported by LaTour et al. (1986) from a consumer panel. We are, however, continuing to explore the effectiveness of alternative follow-up procedures, such as administration by phone to mail nonrespondents, in improving the response rate.

As with the group administrations, the data that we did obtain from the mailed pretest indicated that the nine scenarios called for in our design do not present an undue response burden; over three-quarters of respondents completed all nine of the hypothetical options. Again, beneficiaries were more likely to express interest in options as the price fell and less likely to express interest as the restrictions increased.

Mail administration therefore seems feasible; however, fairly high nonresponse rates will result in missing design points. Therefore, to minimize the number of missing design points, we plan to administer the survey in two waves and to mail replicates of each design point. In the first wave mailing, we will send r replicates of each design point. In the second wave mailing, we will mail s replicates for each design point that remains empty, that is if none of the r replicates is received. Given the number of design points, n, a total budget of C, and the objective of minimizing the number of empty design points, it can be shown that the optimal number of replicates in wave one, r, satisfies,

$$q^r + \ln q^r = 1 + \ln q^{(C/n)}$$

where q is the nonresponse rate; and the optimum number of replicates in wave two, s, is

$$s = ((C/n)-r/q^{r}.$$

Because we have two groups with very differing response rates, those we phone versus those we cannot, the optimum number of replicates for each group will differ and will depend on the allocation of the total budget to each group. Final decisions on the allocation of sample between the two groups and on the number of replicates for each group will be made once we complete our investigation of alternative sources of telephone numbers and of the effectiveness of some new nonresponse follow-up procedures.

IV. PLAN OF ANALYSIS

Our analysis will be carried out in two phases. In Phase 1, we will fit a model that describes the relationship between the likelihood of joining an AHP and the characteristics of the plan and of the beneficiary. This model will allow us to *predict* the likelihood of joining a plan with any combination of attribute values, even if that combination was not included in the options we presented. With the model we can estimate the demand curve for AHPs and investigate biased selection problems. For example, to study biased selection, we will use the model to simulate who will choose to join a plan with specified characteristics.

In the second phase of the analysis, we will draw on theoretical models of decisionmaking under uncertainty and estimate the parameters of the underlying decisionmaking model. Such a model characterizes how individuals integrate the specific details about an AHP into a set of outcomes and provides a decision rule for choosing among plans. It allows us to extrapolate to any set of options that can be characterized with the same outcomes and provides a potentially more powerful analytic tool than the descriptive model fit in Phase 1. For example, with the Phase-2 model we will be able to generalize beyond a pairwise choice and simulate choices among any number of health plans, including choices among various indemnity plans.

PHASE 1

Our experimental design was chosen to allow us to estimate all the factor effects of interest on small clusters of individuals who are selected to be similar in their demographic characteristics and health status. That is, we can think of carrying out the analysis in two-steps. First, we estimate the treatment effects—that is, the way changes in the attributes affect the likelihood of joining an AHP—for each cluster. Second, we compare the estimated effects across different clusters to determine whether clusters differ in the way they respond.

If they do, we can then relate the differential responses to observed demographic variables. As an alternative to this nonparametric analysis, we might pool the data from all clusters and specify a parametric model relating the demographic variables, the treatment variables, and the decision about joining the AHP.

Each approach has advantages and disadvantages. As we noted under Sample Selection in Sec. III, the nonparametric approach requires that we have little or no missing data. With nonresponse, we will need to pool several clusters in order to identify the factor effects of interest, and this requires that we assume that individuals in the several clusters all respond in the same way. On the other hand, the nonparametric approach allows us great flexibility in comparing responses across clusters because it provides separate estimates of the response for each cluster.

The parametric approach can be carried out even in the presence of large amounts of missing data; the parametric function that relates the factor effects and the demographic variables to the buy decision determines how to combine clusters to estimate the treatment effects. On the other hand, we might misspecify the parametric function, and different parametric specification might lead to different combinations of the clusters. In the absence of strong prior information about the parametric function, we need to explore different models and compare them.

We expect that our analysis will combine both the parametric and nonparametric approach. Because nonresponse is unavoidable in a sample survey, most of the analysis will pool the responses from all clusters and use parametric models that allow interactions between the factors and demographic characteristics. For clusters in which there is little or no missing data, we can estimate the factor effects using within-cluster comparisons. We can then compare the cluster-specific treatment effect with predicted effects from the parametric model. The comparison of the nonparametric analysis and the parametric analysis will serve as a useful goodness-of-fit test to evaluate the appropriateness of the parametric model.

In fitting either model, our data include multiple observations for a single individual, each observation being the individual's response to one of the hypothetical options presented. We expect there to be some correlation among observations from the same individual due to individual specific preferences. For example, some individuals may particularly value freedom of choice and be unwilling to join any AHP while others may be willing to accept any AHP if it is less costly than Medicare. Analysis of the pretest data show the intra-individual correlation is about .25. Failure to account for this correlation would yield inefficient estimates of the coefficients and incorrect estimates of the standard errors. Our design allows us to use a fixed-effects model to account for individual specific effects; that is, we will fit a model that includes an individual specific term to measure individual propensities to join an AHP that are due to unmeasured characteristics of the individual.

The rest of this section focuses on the uses to which we will put the fitted model.

Demand Curves

Demand curves for AHPs can be obtained by using the fitted model to simulate the changes in enrollment probabilities that are associated with a change in the price. For each respondent, we predict the probability that he or she would choose an AHP given the characteristics of the AHP, the offered benefit package, and the premium. Averaging the predicted value over all respondents gives the enrollment rate at that premium. Holding all else constant, we vary the premium, predict enrollment rates at each new premium, and determine how the enrollment rate varies with changes in the price.

We will also examine whether the enrollment response to the price of an AHP differs among different AHP structures. That is, we examine whether the price elasticity of demand for AHPs varies between options that have attributes characteristic of HMOs and those that are more characteristic of PPOs or IPAs.

Biased Selection

The other detailed investigation that will be carried out using the fitted model is an analysis of biased selection. Here the issues include: What is the nature and extent of biased selection? How does it differ among different AHP structures? What are the implications for Medicare? What are the implications for the survivability of plans?

For example, for an AHP of given attributes we can use our plan choice model to simulate who will choose to join the plan. Once the beneficiaries are divided into joiners and nonjoiners, we can compare beneficiaries' use of medical care in the two groups. Such data would then permit us to evaluate whether the current AAPCC payment formula tends to be overly generous or overly punitive given this pattern, and to examine whether other adjusters proposed for the AAPCC (such as prior use) correct the problem.[1]

Over time, AHPs will respond to biased selection by adjusting premiums and benefits, or by withdrawing if they suffer serious adverse selection problems. These responses will change the pattern of choice. By making assumptions about how AHPs will respond, we can simulate plan choices over time to investigate stable market shares. For this analysis, we wish to investigate the consequences of biased selection given the current payment formula and restrictions on the premiums that AHPs can charge beneficiaries, and how the consequences would differ if Medicare used additional information about beneficiaries' health risk in setting capitation rates or if regulations controlling the premiums that AHPs can charge were modified.

PHASE 2

The model that we will fit in Phase 1 is primarily a descriptive one--it describes the relationship between choice and characteristics of the plan and of the beneficiary. It does not derive from theoretical models of decisionmaking.

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[1] Our dataset will include information about expected use in the next year as well as both self-report and claims data on use in the year preceding the interview.

Estimating the parameters of an underlying decisionmaking model, however, would provide us with a potentially more powerful analytic tool. For example, the Phase-1 model enables us to predict who will choose an AHP and who will choose traditional Medicare when faced with a pairwise choice. The decisionmaking model lets us simulate market shares when beneficiaries face a number of options to traditional Medicare.

The decision of what health plan to join is a decision made under uncertainty. At the time of the choice, beneficiaries are uncertain as to whether they will be sick or well, and so they are uncertain about the financial consequences of choosing between health plans. The most widely used model of how people make decisions under uncertainty is the expected utility model. According to the expected utility theory, individuals know the probability distribution of possible outcomes, they evaluate these outcomes according to a utility function, and they evaluate each choice or option by its expected utility, which is the sum of the utility of each outcome times its corresponding probability. The decision rule is to choose the option that maximizes expected utility. Appendix 4 discusses how we will use the data generated in this project to test alternate formulations of utility functions and estimate the model's parameters.

The expected utility model has the advantage over the Phase-1 model in that it reduces the dozens of features distinguishing different modes of health care delivery and payment to a few basic summary measures of risk and financial loss and restrictions on provider. If such a model successfully captures the process by which Medicare beneficiaries choose among alternatives, we will have a more powerful tool with which to analyze future changes in policy and the medical market place.

What is important to note at this stage is that the analysis of Phase 2 draws on theoretical models of how individuals process and integrate information and how they use the information to make a choice among alternatives. To the extent that our empirical models adequately capture the decisions that beneficiaries voice in the survey, we can use the model to simulate choices among a wider variety and number of

options than those posed in the survey. Because the model characterizes how individuals integrate the specific details about the plan into a set of outcomes (e.g., into a distribution of expected financial gains and losses and a set of provider restrictions) and provides a decision rule (e.g., maximizes expected utility), we can extrapolate to any set of options that can be characterized with the same outcomes.

For example, the fitted models indicate what individuals are willing to pay to reduce the financial risk of illness. Therefore, we can infer whether plans will be attractive if they offer expanded or new benefits for services other than those posed in the survey, as long as we know the distribution of risk. By providing a decision rule expressed in terms of a few basic characteristics (rather than all the many features that can vary among specific insurance and payment plans), the models can be used to simulate beneficiary choice and adverse selection when there are a number of alternatives to traditional Medicare, including indemnity plans as well as AHPs, rather than just the pairwise choice offered in the hypotheticals.[2] Obviously, there would still be limits on the degree to which we could extrapolate. We could not, for example, simulate the effect on choice of features that could not be characterized in terms of financial consequences or the specific provider restrictions we pose in our hypothetical offers. But a very wide variety of innovations in health care delivery and financing could be adequately characterized in terms of these summary measures. It would thus be worthwhile to extend the empirical analysis, investigating whether the results for AHP versus Medicare choices lead us to a more general and basic model of how older consumers make decisions about health care services and financing.

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[2] For an empirical investigation of plan choice using stochastic simulation methods and estimated parameters of the utility function based on respondents preferences among hypothetical plans see Marquis and Holmer (1986).

Appendix A

EXPERIMENTAL DESIGN

This appendix provides the technical detail about how we construct the experimental design. The design depends critically on the number of scenarios that each individual will be asked to consider. Based on the results from the pretest, we plan to pose nine scenarios to each individual.

We have 12 factors (see Table 2), one of which has four levels, four of which have three levels, and seven of which have two levels. This gives 19 main effect parameters to be estimated (including the intercept). We will reformat the four-level factor as two factors with two levels each. In doing so, the third main effect parameter for this factor becomes the interaction between the new two-level factors. We can now regard the design problem as consisting of 13 factors, nine of which have two levels, and four of which have three levels.

Since we have some two-level factors and some three-level factors, this is a mixed design. For the number of factors we need to consider, there does not exist a readily available fractional factorial design that can be used. (The largest design given in the National Bureau of Standards tabulation[1] allows 6 two-level factors and 4 three-level factors, and identifies all second-order interactions.) Therefore we developed a design that accommodates the number of factors required in our study and is specifically tailored to identify the important interactions discussed in Sec. III. Since we do not need to identify all second-order interactions, our design requires fewer individuals than a design aimed at identifying all second-order interactions. On the other hand, we are interested in some second-order interactions between the four-level factor (A) and other two-level factors, thus in the reformatted problem, we have some third-order interactions among the

[1]National Bureau of Standards, *Fractional Factorial Designs for Experiments with Factors at Two and Three Levels*, Applied Mathematics Series #58, National Bureau of Standards, 1961.

new two-level factors derived from the four-level factor and other factors. As it turns out, our design requires the same number of individuals (36) as the National Bureau of Standards design with fewer factors, referenced above.

Our design uses the method of cyclic generation (Dean and John, 1975, Patterson, 1976, and Bailey, 1977). First we choose a set of nine AHP scenarios to pose to the first individual in a cluster. We then choose suitable "translation vectors" to generate the scenarios for other individuals in the same cluster. The design consists of two parts: for each individual, we choose a suitable subdesign for the three-level factors for the nine AHP scenarios and choose another suitable subdesign for the two-level factors based on only eight of the nine scenarios available. This procedure makes use of the fact the number of scenarios per individual, (9), is an exact power of three, and is nearly an exact power of two (differs from $2^3 = 8$ by one). We append a ninth scenario for the two-level factors somewhat arbitrarily. When the subdesign is extended using the method of cyclic generation, the three-level factors are orthogonal among themselves and are also orthogonal to the two-level factors. Some of the two-level factors are only nearly orthogonal because of the arbitrariness of the ninth scenario. However, if we neglect the ninth scenario from each individual, the two-level factors are orthogonal to each other.

For each scenario, we denote the nine two-level factors as a vector [f1, . . . f9], where the first two entries correspond to the two two-level factors derived from the four-level factor A (see Table 2 for the plan attributes). The two-level factors that are involved in the interactions that we wish to test for (factors B, C, D, and F) are assigned to the 3rd, 5th, 6th, and 7th entry in the vector. The exact order is arbitrary, however. The remaining two-level factors (H, I, and M) are assigned to the 4th, 8th, and 9th entries and again the order is arbitrary. For each two-level factor, we denote the possible levels as zero or one. We denote the three-level factors as [g1, . . . g4], where the levels are zero, one, or two.

The cyclic generation is based on finite group arithmetics. For the two-level factors, we use modulo two addition: 0 + 0 = 0, 0 + 1 = 1, 1 + 0 = 1, and 1 + 1 = 0. Note that the addition of zero leaves the original level unchanged, while the addition of one interchanges the levels zero and one. (In other words, we relabel the two levels for the factor.) For three-level factors, we use modulo three addition: 0 + 0 = 0, 0 + 1 = 1, 0 + 2 = 2, 1 + 0 = 1, 1 + 1 = 2, 1 + 2 = 0, 2 + 0 = 2, 2 + 1 = 0, and 2 + 2 = 1. Thus the addition of one relabels the levels as follows: 0 ==> 1, 1 ==> 2, 2 ==> 0, while the addition of two relabels the levels as follows: 0 ==> 2, 1 ==> 0, 2 ==> 1.

For the first individual in a cluster, we use a subdesign consisting of nine scenarios that combine a subdesign for the two-level factors and another subdesign for the three-level factors. The latter subdesign is given as follows:[2]

g1	g2	g3	g4
1	0	1	1
0	1	1	2
1	1	2	0
2	0	2	2
0	2	2	1
1	2	0	2
2	1	0	1
2	2	1	0
0	0	0	0

We will refer to the above subdesign as Q1. The main effects in Q1 are orthogonal to each other. (For any pair of three-level factors, all combinations of factor levels appear the same number of times, namely, once.)

[2]The first two rows in Q1 are the generators for the entire subdesign: all the other seven scenarios can be expressed as sums of the two generators based on the modulo three addition. For example, the third row is the sum of the two generators. The fourth row is the sum of the first generator and itself.

For the two-level factors, the first eight scenarios in the subdesign are given as:[3]

f1	f2	f3	f4	f5	f6	f7	f8	f9
1	0	0	1	1	0	1	0	1
0	1	0	1	0	1	1	1	1
0	0	1	0	1	1	1	1	1
1	1	0	0	1	1	0	1	0
1	0	1	1	0	1	0	1	0
0	1	1	1	1	0	0	0	0
1	1	1	0	0	0	1	0	1
0	0	0	0	0	0	0	0	0

In this subdesign, the first seven factors (f1 . . . f7) are orthogonal to each other. The last two factors (f8, f9) are replicates of f6 and f7, thus the main effect for f6 is confounded with f8, and the main effect for f7 is confounded with f9. (We cannot identify nine main effects with eight scenarios; seven is the maximum.) Remember also that the third main effect parameter for the four-level factor is given as the interaction between the first two two-level factors. This interaction, which is represented as the column-wise modulo two addition of f1 and f2, is identical to f4, thus this factor effect is confounded with the main effect for f4. With this first individual, we obviously cannot identify all main factor effects.

In order to complete the subdesign, we append a ninth row to the above subdesign, given as follows:

f1	f2	f3	f4	f5	f6	f7	f8	f9
0	0	0	0	0	0	1	0	0

The choice of this ninth scenario is somewhat arbitrary; we will give the reason for this specific choice later.

[3]The first three rows in this subdesign are the generators for the other five rows, based on the modulo two addition.

We will refer to the subdesign consisting of the first eight scenarios and this ninth scenario as R1. Because of the arbitrariness of the ninth scenario, the two-level factors are not orthogonal to each other. (For any two factors, the combinations of factor levels do not appear the same number of times.) Therefore we might not achieve the best precision that is possible for nine combinations of scenarios. However, the first eight scenarios in R1 are chosen to be optimal for eight combinations of nine factors. If the ninth scenario does not contribute any information, i.e., if we neglect the ninth scenario from all analysis for two-level factors, we would at most lose one ninth of the achievable efficiency. However, even though the ninth scenario is chosen rather arbitrarily, it does contribute some information, therefore we can achieve an efficiency higher than 8/9 = 89 percent.[4] Therefore the efficiency lost is fairly small.

The scenarios assigned to the first individual combine Q1 and R1. In other words, the subdesign for this individual is

[Q1 R1].

Since the subdesign R1 does not identify all main effects for the two-level factors and the four-level factor, we need to modify the subdesign for the second individual so that we can identify all the main effects by comparing responses between the two individuals. This is done using cyclic generation with the following translation vector:

f1	f2	f3	f4	f5	f6	f7	f8	f9
1	1	1	1	1	1	1	0	0 .

The translation vector is added to each of the nine rows in R1 (modulo two). For example, the first scenario[5] for the second individual is

[4]For the main effect parameters, the actual precision for the complete design achieved is about 95 percent of the optimal precision based on exact orthogonality.

[5] We will reorder all scenarios randomly for each individual to avoid possible effects due to a scenario's position in the sequence.

f1 f2 f3 f4 f5 f6 f7 f8 f9
0 1 1 0 0 1 0 0 1 .

(All factor levels for the first seven factors are interchanged, while those for the last two factors remain the same.) We denote the subdesign for the two-level factors for the second individual as R2. We use the subdesign Q1 for the three-level factors for the second individual so the full subdesign for the second individual is

[Q1 R2]

Combining the two individuals, we have the subdesign:

$$\begin{bmatrix} Q1 & R1 \\ Q1 & R2 \end{bmatrix}.$$

Based on the subdesign for the two-level factors, the main effects for all nine two-level factors are now identified. Furthermore, the interaction between f1 and f2 is no longer confounded with f4. If we omit the ninth scenario for both individuals, the main effects for the two-level factors, including the third main effect parameter for the four-level factor, are orthogonal to each other.

For the third individual, we use the translation vector

f1 f2 f3 f4 f5 f6 f7 f8 f9
1 0 0 0 0 0 0 1 1

and add this row vector (modulo two) to each row for R1 to obtain subdesign R3. For the fourth individual, we use this same translation vector added to each row of R2 to obtain subdesign R4. We use the subdesign Q1 for the three-level factors for the two new individuals. Combining the four individuals, we now have the subdesign

$$\begin{bmatrix} Q1\ R1 \\ Q1\ R2 \\ Q1\ R3 \\ Q1\ R4 \end{bmatrix} .$$

In this subdesign, the two-level factors are orthogonal to the three-level factors. If we wish to estimate only main effects, this is a very good design using only four individuals and 36 scenarios. We can identify all main effects, and they are either exactly or nearly orthogonal to each other.

Since we believe there may be interactions among the factors, we need to expand the design using cyclic generation to identify the interactions assumed to be important. For the fifth through the eighth individual, we use the translation vector

f1	f2	f3	f4	f5	f6	f7	f8	f9
0	1	0	0	0	0	0	1	1

and we add (modulo two) this row vector to all row vectors in R1, R2, R3, and R4, and denote the resulting subdesigns as R5, . . . R8. For the ninth through the sixteenth individuals, we use the translation vector

f1	f2	f3	f4	f5	f6	f7	f8	f9
0	0	1	0	0	0	0	0	0,

to generate R9, . . . R16.

The design for the 16 individuals identifies all the important interactions (see Sec. III). Therefore, we could stop here with our design and use 16 individuals per cluster. However, in order for the design to allow flexibility, we will expand this design to 36 individuals so as to obtain a complete factorial design for the three-level factors.

For the two-level factors, we generate the subdesigns for the seventeenth through the thirty-second individual using the translation vector.

f1 f2 f3 f4 f5 f6 f7 f8 f9

0 0 0 0 1 0 0 0 0 .

For the thirty-third through the thirty-sixth individual, we use either the same subdesigns for the first four individuals, or the next four individuals, etc. (We will cycle through the eight available choices in different clusters.)[6]

We have so far used three generators for the initial subdesign R1, and five additional generators as translation vectors. There are nine linearly independent generators for nine factors. We have used the ninth generator as the ninth row in the initial subdesign R1 so that the ninth row will never be the same as the other eight rows in any individual's subdesign. (This choice is not crucial, though.)

Turning to the subdesigns for the three-level factors, the first four individuals receive the subdesign Q1 described above. The next four individuals receive subdesign Q2 obtained by adding the row vector (modulo three)

g1 g2 g3 g4

1 1 1 1

to Q1. Individuals nine through 12 receive the subdesign Q3 obtained by adding the above translation vector to Q2. For individual 13 through 25, we use subdesigns Q4, Q5, and Q6 obtained by adding translation vector

[6]If we neglect the ninth row for each individual and the last four individuals, we have a 1/2 factorial design for the two-level factors. We can obtain a complete factorial design in two clusters if we use the ninth generator as the translation vector for the next cluster.

g1	g2	g3	g4
1	1	0	0

to subdesigns Q1, Q2, and Q3. Adding this same translation vector to subdesigns Q4, Q5, and Q6 gives subdesigns Q7 through Q9 that are used for individual 25 through 36. We now have a complete factorial design for the four three-level factors: All possible combinations of factor levels for the four three-level factors appear the same number of times.

The design we have obtained has the following features:

- We can identify all main effects with only four individuals.
- All main effects are exactly or nearly orthogonal to each other. The precision of the estimated main effects is very near the optimal precision that can be achieved with a design with exact orthogonality.
- We can identify all important interactions specified in Sec. III with 16 individuals per cluster.

We have described a design for one cluster. Although we can use the same scenarios from cluster to cluster, if we vary the hypothetical offers across clusters, we can then pool data across clusters to test whether interactions that we have assumed to be negligible really are important. We will vary the offers from cluster to cluster in the following way: The levels of the attributes for the first scenario for the first or seed individual in a cluster are arbitrary; the translation vectors are then applied to yield the remaining scenario for the individual and others in a cluster. For the seed individual in the first cluster, we randomly pick the levels of the attributes for the first scenario. We randomly permute the levels of this scenario to obtain the first scenario for the seed individual for each subsequent cluster and then apply the translation vectors to complete the design for the cluster.

Appendix B

SAMPLE SIZE CALCULATIONS

We give the derivation for the smallest main effect that can be detected with a sample size of n below. Here n refers to the number of scenarios for which we have responses, not the number of individuals. The derivation for the interaction is analogous and is omitted.

Based on the additivity and orthogonality assumption, we can analyze the data as if we had a contrast between two groups:

$y_{0i} = P0 + S0*e_{0i}$, $i = 1, \ldots n/2$,

$y_{1i} = P1 + S1*e_{1i}$, $i = 1, \ldots n/2$,

where the error terms e have mean zero and variance one. Since the outcome is dichotomous, we have

$Sj = \text{sqrt}(Pj*(1-Pj)) \simeq 0.5$,

where the approximation is exact if $Pj = 0.5$, but remains fairly accurate for P in the range from 0.2 to 0.8.

The test for the null hypothesis H_0:P0 = P1 is based on the sample difference D = R1 − R0, where Rj is the average of y_{ji}'s. Under the null hypothesis, the distribution of D is approximately normal with mean zero and variance

$S0^2/(n/2) + S1^2/(n/2) \simeq 1/n$.

It follows that the two-sided 5 percent test reject the null hypothesis if D is larger than 1.96/sqrt(n) in absolute magnitude.

Under the alternative hypothesis H_A: P0 ≠ P1, the distribution for D is approximately normal with mean δ = P1 − P0 and variance given above. It follows that the power of rejecting the null hypothesis is

$$\begin{aligned} \text{power} &= P(\text{reject } H_0) \\ &= P(D > 1.96/\text{sqrt}(n)) + P(D < -1.96/\text{sqrt}(n)) \\ &= P((D - \delta)*\text{sqrt}(n) > 1.96 - \delta*\text{sqrt}(n)) + \ldots . \\ &= \Phi(-1.96 + \delta*\text{sqrt}(n)) + \ldots . \end{aligned}$$

For δ > 0, the second term in the third line above is negligible. (For

$\delta < 0$, the first term is negligible, and the derivation based on the second term gives the same result as below.) In order to obtain 80 percent power, we should have

$$-1.96 + \delta * \text{sqrt}(n) > 0.84,$$

where .84 is the point at which the area under the normal curve to the left of the point equals .8, the desired power ($\Phi(0.84) = 0.8$ where Φ denotes the standard normal distribution function). Hence,

$$\delta \geq 2.80/\text{sqrt}(n).$$

Therefore the smallest main effect we can detect with sample size n at 80 percent power with .05 significance is $\delta = 2.80/\text{sqrt}(n)$.

Appendix C

QUESTIONNAIRE

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Version PT

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MEDICARE QUESTIONNAIRE

Thank you for filling out this questionnaire. Your responses will be very important in helping us understand what improvements could be made in the Medicare program.

THANK YOU FOR TAKING PART IN THIS STUDY.

STATEMENT OF CONFIDENTIALITY

All information that would permit identification of any person who completes this questionnaire will be regarded as strictly confidential. Such information will be used only for the purposes of this study and will not be disclosed or released for any other purposes without prior consent, except as required by law.

INSTRUCTIONS FOR COMPLETING THE QUESTIONNAIRE

- **Please read each question carefully before you give your answer.**

- **Answer the questions by CIRCLING the appropriate number or numbers, or by WRITING in the answer as requested.**

  EXAMPLES: 1. Do you carry any health insurance other than Medicare?

  (Circle One)

  Yes ..... (1)
  No 2

  2. How much do you pay per year for your health insurance? (If none, write in "0.")

  Write in Dollars Per Year: $ 264.00

- **Please answer every question, unless you are asked to SKIP questions that don't apply to you. In that case, follow the instructions NEXT to the answer you have circled.**

  EXAMPLES: 3. Do you belong to a Health Maintenance Organization or HMO?

  Yes ..... 1 - - -> **Continue with QUESTION 4**
  No ..... 2 - - -> **GO TO NEXT PAGE - - ->**

- **If you are unsure about how to answer a question, give the best answer you can and make a comment in the LEFT margin.**

What time did you <u>begin</u> this page? ☐☐ : ☐☐ 6-9/

YOUR HEALTH

1. **In general, would you say your health is:**

(Please Circle One Number)

Excellent	1	10/
Very good	2	
Good	3	
Fair	4	
Poor	5	

2. **<u>Does your health limit you</u> in any of the following activities?**

(Please Circle One Number On Each Line)

	Not Limited At All	Limited	
a. <u>Vigorous</u> activities you can do, like lifting heavy objects, running, or participating in strenuous sports	1	2	11/
b. <u>Moderate</u> activities you can do, like moving a table, carrying groceries, or bowling	1	2	12/
c. Walking uphill or climbing a few flights of stairs	1	2	13/
d. Bending, lifting, or stooping	1	2	14/
e. Walking one block	1	2	15/
f. Eating, dressing, bathing or using the toilet	1	2	16/

3. **Does your health keep you from working at a job or doing work around the house?**

(Circle One)

Yes	1	17/
No	2	

4. Have you been unable to do certain kinds or amounts of work or housework because of your health?

(Circle One)

Yes 1 *18/*

No 2

5. Please circle the number below that best describes whether each of the following statements is true or false for you.

Please circle one number on each line.

	Definitely True	Mostly True	Not Sure	Mostly False	Definitely False	
a. I am somewhat ill.....	1	2	3	4	5	*19/*
b. I think my health will be worse in the future than it is now.....	1	2	3	4	5	*20/*
c. I feel about as good now as I ever have	1	2	3	4	5	*21/*
d. I have been feeling very bad lately.....	1	2	3	4	5	*22/*
e. In the future I expect to have better health than other people I know	1	2	3	4	5	*23/*
f. I expect to have a very healthy life.....	1	2	3	4	5	*24/*
g. I am in poor health.....	1	2	3	4	5	*25/*
h. I expect my health to get worse.....	1	2	3	4	5	*26/*
i. I am as healthy as anybody I know	1	2	3	4	5	*27/*
j. My future will be unhealthly.....	1	2	3	4	5	*28/*
k. My health is excellent.....	1	2	3	4	5	*29/*
l. Good health is in my future.....	1	2	3	4	5	*30/*

YOUR SATISFACTION WITH YOUR MEDICAL CARE

This section contains some statements about medical care. Please read each one carefully, keeping in mind the medical care you are receiving now. If you have not received any medical care recently, please think about what you would expect if you needed care today.

1. **On the line next to each statement, please circle the one number for the category that comes closest to your opinion.**

	Strongly Agree	Agree	Not Sure	Disagree	Strongly Disagree	
a. I am very satisfied with the medical care I receive.....	1	2	3	4	5	31/
b. I worry sometimes about having to pay large medical bills.....	1	2	3	4	5	32/
c. Sometimes it is a problem to cover my share of the cost for medical care visit.....	1	2	3	4	5	33/
d. The medical care I have been receiving is just about perfect.....	1	2	3	4	5	34/
e. I feel confident that I can get the medical care I need without being set back financially.....	1	2	3	4	5	35/
f. Sometimes I go without the medical care I need because it is too expensive.....	1	2	3	4	5	36/
g. There are things about the medical system I receive my care from that need to be improved.....	1	2	3	4	5	37/
h. I have to pay for more of my medical care than I can afford.....	1	2	3	4	5	38/
i. Regardless of the health problems I have now or develop later, I feel protected from financial hardship.....	1	2	3	4	5	39/

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	Strongly Agree	Agree	Not Sure	Disagree	Strongly Disagree	
j. The amount I have to pay to cover or insure my medical care needs is reasonable.....	1	2	3	4	5	40/
k. There are some things about the medical care I receive that could be better.....	1	2	3	4	5	41/
l. All things considered, the medical care I receive is excellent.....	1	2	3	4	5	42/
m. I feel insured and protected financially against all possible medical problems.....	1	2	3	4	5	43/

2. Do you have a usual doctor or doctor's office, clinic, health center, or other place or provider that you go to if you are sick or need advice about your health?

(Circle One)

Yes 1 44/
No 2
Don't Know..... 9

3. Have you ever belonged to a prepaid health plan, sometimes called a Health Maintenance Organization, or HMO?

(Circle One)

Belong to one now 1 45/
Have belonged in the past 2
Have never belonged to one 3

MEDICARE AND INSURANCE COVERAGE

For <u>each</u> question, please circle a number for the <u>one</u> answer that comes closest to your understanding of any "extra" insurance coverage (Medi-gap) you have or might be eligible for.

1. Are you eligible for any <u>Medicaid</u> benefits (or other welfare medical care) in addition to your Medicare coverage?

(Circle One)

Yes 1 *46/*
No 2
Don't know 9

2. Do you currently have any private health insurance policies that pay hospital, doctor, or other medical bills that Medicare <u>does not</u> pay for?

(Circle One)

Yes 1 *47/*
No ..... 2 ---> Please skip the rest of this section and start the next section.
Don't know 9

Now, think only about your <u>extra</u> insurance, not Medicare.

3. Does this <u>private supplementary policy</u> pay for:

3a. Hospital stays?

(Circle One)

Yes 1 *48/*
No 2
Don't Know 9

3. (Continued) Does your <u>private supplementary policy</u> pay for:

3b. Your doctor's care while you are in the hospital?

(Circle One)

Yes	1	49/
No	2	
Don't Know	9	

3c. Office visits to your regular doctor or to a specialist?

(Circle One)

Yes	1	50/
No	2	
Don't Know	9	

3d. Office visits to a doctor or other professional for emotional or mental health problems?

(Circle One)

Yes	1	51/
No	2	
Don't Know	9	

3e. Prescribed medicines (but not including those you get while you are in a hospital)?

(Circle One)

Yes	1	52/
No	2	
Don't Know	9	

3f. Short-term nursing home care in a specialized facility following a stay in a hospital. (These are sometimes called "skilled nursing facilities".)

(Circle One)

Yes	1	53/
No	2	
Don't Know	9	

3. (Continued) Does your <u>private supplementary policy</u> pay for:

3g. Nursing home or custodial care (for example, for as long as a year or two)?

(Circle One)

Yes	1	54/
No	2	
Don't Know	9	

3h. Visits to your doctor for preventive or well-care services, such as annual check-up or shots to protect you from getting the flu during the winter?

(Circle One)

Yes	1	55/
No	2	
Don't Know	9	

3i. Visits to a doctor or other professional for hearing problems or hearing aids?

(Circle One)

Yes	1	56/
No	2	
Don't Know	9	

3j. Dental services?

(Circle One)

Yes	1	57/
No	2	
Don't Know	9	

The next question concerns **what you have to pay** for this private, supplementary insurance. If you have more than one such policy, please think of them all together in answering this question. Count total cost, even if it covers more than one person.

Amount

4. What is the premium or amount you have to pay per year for this policy? ---> $_____ per year 58-61/

OR

If it is more convenient to fill in a monthly cost, please answer here ------> $_____ per month 62-64/

5. Does this policy cover:

(Circle One)

You, alone 1 65/
You and your husband or wife 2

WHAT YOU SPENT FOR HEALTH CARE IN THE LAST 12 MONTHS

For each of the types of health care services listed below, please circle the number for the range that comes closest to your total expenditures in the last 12 months.

TOTAL COSTS (including costs paid by Medicare or other insurers)

(Circle One)

1. Admission to a hospital for at least one overnight stay? *66/*

No hospital stay	1
$0 - $1000	2
$1001 - $2500	3
$2501 - $5000	4
More than $5000	5

(Circle One)

2. Any stays in a nursing home? *67/*

No nursing home stay	1
$0 - $1000	2
$1001 - $2500	3
$2501 - $5000	4
More than $5000	5

(Circle One)

3. Doctor's visits or other services while you were in the hospital? *68/*

No physician care in hospital	1
$0 - $100	2
$101 - $250	3
$251 - $500	4
More than $500	5

(Circle One)

4. Visits to a doctor's office or a clinic for preventive care, such as an annual checkup or flu shot *69/*

No preventive visits	1
$0 - $100	2
$101 - $250	3
$251 - $500	4
More than $500	5

TOTAL COSTS (including costs paid by Medicare or other insurers)

(Circle One)

5. Visits to a doctor's office or a clinic for any illness or medical problems? 70/

No visits for illness	1
$0 - $100	2
$101- $250	3
$251 - $500	4
$501 - $1000	5
$1001 - $2500	6
More than $2500	7

(Circle One)

6. Prescription medications that were prescribed for you by a physician or dentist? 71/

No medications	1
$0 - $100	2
$101 - $250	3
$251 - $500	4
More than $500	5

WHAT YOU EXPECT TO SPEND FOR HEALTH CARE IN THE NEXT 12 MONTHS

For *each* of the types of health care services listed below, please circle the number for the range that comes *closest* to your expected total expenditures in the next 12 months. Of course, you won't know exactly what you will spend in the next 12 months, but give the best answer you can, based on what you expect.

TOTAL EXPECTED COSTS (including costs paid by Medicare or other insurers)

(Circle One)

1. Admission to a hospital for at least one overnight stay?

No hospital stay expected 1 72/
$0 - $1000 2
$1001 - $2500 3
$2501 - $5000 4
More than $5000 5

(Circle One)

2. Any stays in a nursing home?

No nursing home stays expected 1 73/
$0 - $1000 2
$1001 - $2500 3
$2501 - $5000 4
More than $5000 5

(Circle One)

3. Doctor's visits or other services while you are in a hospital?

No physician care in hospital expected 1 74/
$0 - $1000 2
$1001 - $2500 3
$2501 - $5000 4
More than $5000 5

(Circle One)

4. Visits to a doctor's office or a clinic for preventive care, such as an annual checkup or flu shot?

No preventive visits expected 1 75/
$0 - $100 2
$101 - $250 3
$251 - $500 4
More than $500 5

CARD 02 4-5/

1-3/

TOTAL EXPECTED COSTS (including costs paid by Medicare or other insurers)

(Circle One)

5. Visits to a doctor's office, a clinic for any illness or medical problems? 6/

No visits for illness expected	1
$0 - $100	2
$101 - $250	3
$251 - $500	4
$501 - $1000	5
$1001 - $2500	6
More than $2500	7

(Circle One)

6. Prescription medications prescribed for you by a physician or dentist? 7/

No medication use expected	1
$0 - $100	2
$101 - $250	3
$251 - $500	4
More than $500	5

(Circle One)

7. Visits to a doctor, optometrist, or optician for vision problems? 8/

Do not expect to use	1
$0 - $100	2
$101 - $250	3
More than $250	4

(Circle One)

8. Visits to a doctor or audiologist for hearing problems? 9/

Do not expect to use	1
$0 - $100	2
$101 - $250	3
More than $250	4

(Circle One)

9. Visits to a doctor or to any other professional for any emotional distress or mental health problems? 10/

Do not expect to use	1
$0 - $100	2
$101 - $500	3
$501 - $1000	4
$1001 - $2500	5
$2501 - $5000	6
More than $5000	7

NEW KINDS OF MEDICARE INSURANCE PLANS

Now, we want you to imagine that there was another, new kind of Medicare insurance plan and that you had to choose whether you wanted to have the same kind of Medicare you have now or the new plan.

Answering this questionnaire will not affect your current Medicare. This questionnaire is only for research purposes and does not describe actual plans that exist now.

Of course, you would have to compare the old and new plans to decide which was better for you. That's what we want you to do now: Compare Medicare as it is now with several new plans and tell us whether or not you would change. To make it easier we show you what Medicare now provides on the yellow card. You can put the card next to each new plan and compare them.

For each of these new plans, we will ask you to tell us whether you would join this new plan or stay with your current Medicare plan by answering YES or NO.

DESCRIPTION OF YOUR CURRENT MEDICARE

SELECTING YOUR HOSPITAL AND DOCTOR

CHOOSING A HOSPITAL	You can go to any hospital that you and you doctor choose
CHOOSING A PERSONAL PHYSICIAN	You can go to any doctor you choose
RECEIVING CARE FROM SPECIALISTS	You can visit any specialist whenever you wish
JOINING AND LEAVING	Part A: (Hospital coverage) automatically enrolled Part B: (Physician coverage) voluntary enrollment and you can leave anytime

SERVICES AND BENEFITS

HOSPITAL CARE	Days 1--60	You pay $520 total
	Days 61--90	You pay $130 a day
	Days 91--150 (reserve days)	You pay $260 a day
	After 150 days	You pay all costs
LONG TERM NURSING (CUSTODAL CARE)	You pay all costs	
DOCTOR VISITS	You pay the first $75 per year and 20% of approved amounts after that, plus all costs above approved amounts	
PREVENTIVE SERVICES (i.e. check-ups and flu shots)	You pay all costs	
PRESCRIPTION DRUGS	You pay all costs	
USING DOCTORS OR HOSPITALS OUTSIDE THE PLAN	Not applicable for Medicare	

PREMIUM — You pay $17.90 per month for Part B

It may help you in your evaluation of these plans to record the amount you pay for any extra health insurance here:

$ _____

0076-M9-03 01-08/

DESCRIPTION OF NEW MEDICARE PLAN #1

SELECTING YOUR HOSPITAL AND DOCTOR

CHOOSING A HOSPITAL	You can only go to hospitals associated with the plan
CHOOSING A PERSONAL PHYSICIAN	You can visit doctors at the plan facilities. About 10% of doctors in your area belong to the plan. Your usual doctor **IS NOT** a member of the plan.
RECEIVING CARE FROM SPECIALISTS	You can visit a specialist who belongs to the plan whenever you wish
JOINING AND LEAVING	You can leave the plan with 30 days notice

SERVICES AND BENEFITS

HOSPITAL CARE	Days 1 — 60	You pay $520 total
	Days 61 — 90	You pay $130 a day
	Days 91 — 150 (reserve days)	You pay $260 a day
	After 150 days	You pay all costs
LONG TERM NURSING HOME (CUSTODIAL CARE)	You pay all costs	
DOCTOR VISITS	You pay $5 per visit	
PREVENTIVE SERVICES (I.E. CHECK-UPS AND FLU SHOTS)	You pay all costs	
PRESCRIPTION DRUGS	You pay $5 per visit	
USING DOCTORS OR HOSPITALS OUTSIDE THE PLAN	You pay all costs	

PREMIUM

Your total cost is **$2.40** per month.
You pay $17.90 per month to Medicare, but you receive $15.50 **rebate** per month from the plan.

Would you join this plan instead of your current Medicare?

YES.....1 09/

NO..... 2

2311-11122-121 10-21/

-15.50 22-28/

0076-M9

DESCRIPTION OF NEW MEDICARE PLAN #2

SELECTING YOUR HOSPITAL AND DOCTOR

CHOOSING A HOSPITAL	You can only go to hospitals associated with the plan
CHOOSING A PERSONAL PHYSICIAN	You can only go to doctors on the plan list. About 10% of doctors in your area are on the list. Your usual doctor **IS NOT** on the list.
RECEIVING CARE FROM SPECIALISTS	You can visit a specialist who belongs to the plan only when your personal physician refers you
JOINING AND LEAVING	You can leave the plan only at the end of a full year of enrollment

SERVICES AND BENEFITS

HOSPITAL CARE	Days 1 — 60	You pay $100 total
	Days 61 — 90	You pay $130 a day
	Days 91 — 150 (reserve days)	You pay $260 a day
	After 150 days	You pay all costs
LONG TERM NURSING HOME (CUSTODIAL CARE)	You pay all costs	
DOCTOR VISITS	You pay the first $75 per year and $5 per visit after that	
PREVENTIVE SERVICES (I.E. CHECK-UPS AND FLU SHOTS)	You pay $5 per visit	
PRESCRIPTION DRUGS	You pay nothing	
USING DOCTORS OR HOSPITALS OUTSIDE THE PLAN	You pay all costs	

PREMIUM

Your total cost is **$27.60** per month.
You pay $17.90 per month to Medicare, plus
$9.70 **extra** per month to the plan.

Would you join this plan instead of your current Medicare?

YES.....1 29/

NO..... 2

2222-21112-231 30-41/

+9.70 42-48/

0076-M9

DESCRIPTION OF NEW MEDICARE PLAN #3

SELECTING YOUR HOSPITAL AND DOCTOR

CHOOSING A HOSPITAL	You can only go to hospitals associated with the plan
CHOOSING A PERSONAL PHYSICIAN	You can visit doctors at the plan facilities. About 10% of doctors in your area belong to the plan. Your usual doctor **IS** a member of the plan.
RECEIVING CARE FROM SPECIALISTS	You can visit a specialist who belongs to the plan only when your personal physician refers you
JOINING AND LEAVING	You can leave the plan with 30 days notice

SERVICES AND BENEFITS

HOSPITAL CARE	Days 1 — 60	You pay $520 total
	Days 61 — 90	You pay nothing
	Days 91 — 150 (reserve days)	You pay nothing
	After 150 days	You pay nothing
LONG TERM NURSING HOME (CUSTODIAL CARE)	You pay $25 per day	
DOCTOR VISITS	You pay the first $75 per year and nothing after that	
PREVENTIVE SERVICES (I.E. CHECK-UPS AND FLU SHOTS)	You pay nothing	
PRESCRIPTION DRUGS	You pay nothing	
USING DOCTORS OR HOSPITALS OUTSIDE THE PLAN	You pay 50% of the first $1000 charges per year, and all costs after that	

PREMIUM

Your total cost is **$26.70** per month.
You pay $17.90 per month to Medicare, plus
$8.80 **extra** per month to the plan.

Would you join this plan instead of your current Medicare?

YES.....1 49/

NO..... 2

2421-12213-332 50-61/

+8.80 62-68/

0076-M9-04 01-08/

DESCRIPTION OF NEW MEDICARE PLAN #4

SELECTING YOUR HOSPITAL AND DOCTOR

CHOOSING A HOSPITAL	You can only go to hospitals associated with the plan
CHOOSING A PERSONAL PHYSICIAN	You can only go to doctors on the plan list. About 50% of doctors in your area are on the list. Your usual doctor **IS NOT** on the list.
RECEIVING CARE FROM SPECIALISTS	You can visit a specialist who belongs to the plan whenever you wish
JOINING AND LEAVING	You can leave the plan only at the end of a full year of enrollment

SERVICES AND BENEFITS

HOSPITAL CARE	Days 1 — 60	You pay nothing
	Days 61 — 90	You pay nothing
	Days 91 — 150 (reserve days)	You pay nothing
	After 150 days	You pay nothing
LONG TERM NURSING HOME (CUSTODIAL CARE)	You pay $25 per day	
DOCTOR VISITS	You pay nothing	
PREVENTIVE SERVICES (I.E. CHECK-UPS AND FLU SHOTS)	You pay $5 per visit	
PRESCRIPTION DRUGS	You pay $5 per visit	
USING DOCTORS OR HOSPITALS OUTSIDE THE PLAN	You pay 50% of the first $1000 charges per year, and all costs after that	

PREMIUM

Your total cost is **$54.20** per month.
You pay $17.90 per month to Medicare, plus
$36.30 **extra** per month to the plan.

Would you join this plan instead of your current Medicare?

YES.....1 09/

NO..... 2

2112-32223-222 10-21/

+36.30 22-28/

DESCRIPTION OF NEW MEDICARE PLAN #5

SELECTING YOUR HOSPITAL AND DOCTOR

CHOOSING A HOSPITAL	You can go to any hospital that you and your doctor choose
CHOOSING A PERSONAL PHYSICIAN	You can only go to doctors on the plan list. About 50% of doctors in your area are on the list. Your usual doctor **IS NOT** on the list.
RECEIVING CARE FROM SPECIALISTS	You can visit a specialist who belongs to the plan only when your personal physician refers you
JOINING AND LEAVING	You can leave the plan with 30 days notice

SERVICES AND BENEFITS

HOSPITAL CARE	Days 1 — 60	You pay $520 total
	Days 61 — 90	You pay nothing
	Days 91 — 150 (reserve days)	You pay nothing
	After 150 days	You pay nothing
LONG TERM NURSING HOME (CUSTODIAL CARE)	You pay all costs	
DOCTOR VISITS	You pay the first $75 per year and 20% of all costs after that	
PREVENTIVE SERVICES (I.E. CHECK-UPS AND FLU SHOTS)	You pay $5 per visit	
PRESCRIPTION DRUGS	You pay all costs	
USING DOCTORS OR HOSPITALS OUTSIDE THE PLAN	You pay all costs	

PREMIUM

You receive a net rebate of **$15.50** per month. You pay $17.90 per month to Medicare, but you receive $33.40 **rebate** per month from the plan.

Would you join this plan instead of your current Medicare?

YES.....1 29/

NO..... 2

1121-12111-211 30-41/

-33.40 42-48/

0076-M9

DESCRIPTION OF NEW MEDICARE PLAN #6

SELECTING YOUR HOSPITAL AND DOCTOR

CHOOSING A HOSPITAL	You can go to any hospital that you and your doctor choose
CHOOSING A PERSONAL PHYSICIAN	You can visit doctors at the plan facilities. About 10% of doctors in your area belong to the plan. Your usual doctor **IS** a member of the plan.
RECEIVING CARE FROM SPECIALISTS	You can visit a specialist who belongs to the plan whenever you wish
JOINING AND LEAVING	You can leave the plan only at the end of a full year of enrollment

SERVICES AND BENEFITS

HOSPITAL CARE	Days 1 — 60	You pay nothing
	Days 61 — 90	You pay nothing
	Days 91 — 150 (reserve days)	You pay nothing
	After 150 days	You pay nothing
LONG TERM NURSING HOME (CUSTODIAL CARE)	You pay all costs	
DOCTOR VISITS	You pay $5 per visit	
PREVENTIVE SERVICES (I.E. CHECK-UPS AND FLU SHOTS)	You pay nothing	
PRESCRIPTION DRUGS	You pay all costs	
USING DOCTORS OR HOSPITALS OUTSIDE THE PLAN	You pay all costs	

PREMIUM

Your total cost is **$15.40** per month.
You pay $17.90 per month to Medicare, but you receive $2.50 **rebate** per month from the plan.

Would you join this plan instead of your current Medicare?

YES.....1 49/

NO..... 2

1412-32122-311 50-61/

-2.50 62-68/

0076-M9-05 01-08/

DESCRIPTION OF NEW MEDICARE PLAN #7

SELECTING YOUR HOSPITAL AND DOCTOR

CHOOSING A HOSPITAL	You can go to any hospital that you and your doctor choose
CHOOSING A PERSONAL PHYSICIAN	You can visit doctors at the plan facilities. About 10% of doctors in your area belong to the plan. Your usual doctor **IS NOT** a member of the plan.
RECEIVING CARE FROM SPECIALISTS	You can visit a specialist who belongs to the plan only when your personal physician refers you
JOINING AND LEAVING	You can leave the plan only at the end of a full year of enrollment

SERVICES AND BENEFITS

HOSPITAL CARE	Days 1 — 60	You pay nothing
	Days 61 — 90	You pay nothing
	Days 91 — 150 (reserve days)	You pay nothing
	After 150 days	You pay nothing
LONG TERM NURSING HOME (CUSTODIAL CARE)	You pay $25 per day	
DOCTOR VISITS	You pay the first $75 per year and 20% of all costs after that	
PREVENTIVE SERVICES (I.E. CHECK-UPS AND FLU SHOTS)	You pay all costs	
PRESCRIPTION DRUGS	You pay nothing	
USING DOCTORS OR HOSPITALS OUTSIDE THE PLAN	You pay 50% of the first $1000 charges per year, and all costs after that	

PREMIUM

You receive a net rebate of **$7.10** per month. You pay $17.90 per month to Medicare, but you receive $25.00 **rebate** per month from the plan.

Would you join this plan instead of your current Medicare?

YES.....1 09/

NO..... 2

1322-32211-132 10-21/

-25.00 22-28/

DESCRIPTION OF NEW MEDICARE PLAN #8

SELECTING YOUR HOSPITAL AND DOCTOR

CHOOSING A HOSPITAL	You can go to any hospital that you and your doctor choose
CHOOSING A PERSONAL PHYSICIAN	You can only go to doctors on the plan list. About 10% of doctors in your area are on the list. Your usual doctor **IS NOT** on the list.
RECEIVING CARE FROM SPECIALISTS	You can visit a specialist who belongs to the plan whenever you wish
JOINING AND LEAVING	You can leave the plan with 30 days notice

SERVICES AND BENEFITS

HOSPITAL CARE	Days 1 — 60	You pay $100 total
	Days 61 — 90	You pay $130 a day
	Days 91 — 150 (reserve days)	You pay $260 a day
	After 150 days	You pay all costs
LONG TERM NURSING HOME (CUSTODIAL CARE)	You pay $25 per day	
DOCTOR VISITS	You pay 20% of all costs	
PREVENTIVE SERVICES (I.E. CHECK-UPS AND FLU SHOTS)	You pay nothing	
PRESCRIPTION DRUGS	You pay $5 per visit	
USING DOCTORS OR HOSPITALS OUTSIDE THE PLAN	You pay 50% of the first $1000 charges per year, and all costs after that	

PREMIUM

Your total cost is **$45.10** per month.
You pay $17.90 per month to Medicare, plus
$27.20 **extra** per month to the plan.

Would you join this plan instead of your current Medicare?

YES.....1 29/

NO..... 2

1211-21221-322 30-41/

+27.20 42-48/

0076-M9

DESCRIPTION OF NEW MEDICARE PLAN #9

SELECTING YOUR HOSPITAL AND DOCTOR

CHOOSING A HOSPITAL	You can go to any hospital that you and your doctor choose
CHOOSING A PERSONAL PHYSICIAN	You can visit doctors at the plan facilities. About 10% of doctors in your area belong to the plan. Your usual doctor **IS NOT** a member of the plan.
RECEIVING CARE FROM SPECIALISTS	You can visit a specialist who belongs to the plan only when your personal physician refers you
JOINING AND LEAVING	You can leave the plan only at the end of a full year of enrollment

SERVICES AND BENEFITS

HOSPITAL CARE	Days 1 — 60	You pay $100 total
	Days 61 — 90	You pay $130 a day
	Days 91 — 150 (reserve days)	You pay $260 a day
	After 150 days	You pay all costs
LONG TERM NURSING HOME (CUSTODIAL CARE)	You pay $25 per day	
DOCTOR VISITS	You pay the first $75 per year and nothing after that	
PREVENTIVE SERVICES (I.E. CHECK-UPS AND FLU SHOTS)	You pay all costs	
PRESCRIPTION DRUGS	You pay all costs	
USING DOCTORS OR HOSPITALS OUTSIDE THE PLAN	You pay 50% of the first $1000 charges per year, and all costs after that	

PREMIUM

Your total cost is **$49.60** per month.
You pay $17.90 per month to Medicare, plus
$31.70 **extra** per month to the plan.

Would you join this plan instead of your current Medicare?

YES.....1 49/

NO..... 2

1322-21213-112 50-61/

+31.70 62-68/

CARD 06 4-5/
1-3/

GENERAL INFORMATION ABOUT YOU

We need some basic information about you.

1. **How old were you on your last birthday?** Age: _____ 6-7/

2. **What is your sex?**

(Circle One)

Female 1 8/
Male 2

3. **What is your main racial or ethnic group?**

(Circle One)

American Indian or Alaskan Native ... 1 9/
Asian or Pacific Islander 2
Black (and not of Hispanic origin) 3
White (and not of Hispanic origin) 4
Hispanic 5

4. **What is the highest grade or year of regular school that you completed?**

(Circle One)

No formal schooling 1
Some elementary school 2 10/
Completed elementary school (up through grade 8) 3
Some high school (up through grade 12) ... 4
High school diploma or G.E. Degree 5
Some college (up through four years) 6
College degree 7
Graduate or professional school 8
No school 9

(Circle One)

5. Are you now:

Married and living with your husband or wife	1	11/
Widowed	2	
Divorced	3	
Separated, or	4	
Never married	5	

6. What is the zip code where you live all or most of the year?

Please fill in the five digits of your zip code: ☐☐☐☐☐ 12-16/

7. Think about the total income that you (and your husband or wife) had in 1986.

Did you have any income from:

(Circle One For Each Source)

	Yes	No	
a. Wages or salaries from a job	1	2	17/
b. Interest or dividends from investments	1	2	18/
c. Social security payments	1	2	19/
d. Pension or retirement payments	1	2	20/
e. Income from rental properties	1	2	21/
f. Payments from county, state or federal agencies (food stamps or other assistance)	1	2	22/

8. What was your <u>total</u> income for 1986? Include income from all sources you circled above. If it is more convenient for you, please fill in your monthly income.

Total yearly income in 1986 _____ 23-27/

OR

Monthly income in 1986 _____ 28-31/

What time did you finish this page? ☐☐ : ☐☐ 32-35/

Appendix D

PHASE-2 ANALYSIS PLAN: ESTIMATION OF UNDERLYING DECISION MODE

For an assumed form of the utility function specifying how demographic characteristics and plan attributes affect utility, we will pool the responses to the hypothetical scenarios from the clusters to estimate the parameters of the utility function in the following way:

We follow McFadden (1981) and consider a simple random utility formulation of choice. Let the expected utility from joining AHP option j for beneficiary m be $E(U_{jm})$. Let $E(U_{jm}) = E(U_j) + v_{jm}$, where $E(U_j)$ is the average expected utility assigned to AHP option j by all beneficiaries similar to individual m, and where v_{jm} is a stochastic error term that represents the influence of unobserved attributes on a beneficiary's assessment of the expected utility of the option. If individuals choose the plan option that yields the highest expected utility, then the probability that a random beneficiary m will choose plan j over the traditional Medicare plan (j = 0) can be expressed as

$$\begin{aligned} &\text{prob\{individual m chooses plan j\}} = \text{prob}\{E(U_{jm}) > E(U_{0m})\} \\ &= \text{prob}\{E(U_j) + v_{jm} > E(U_0) + v_{0m}\} \qquad (1) \\ &= \text{prob}\{E(U_j) - E(U_0) > v_{0m} - v_{jm}\}. \end{aligned}$$

Assuming that $v_{0m} - v_{jm}$ is drawn from a normal distribution with mean 0 and variance σ^2, the choice probability becomes:

$$\text{prob\{individual m chooses plan j\}} = \Phi\{\beta[E(U_j) - E(U_0)]\}, \qquad (2)$$

where Φ is the standard normal cumulative distribution function and $\beta = 1/\sigma$.

The $E(U_j)$ depends on the assumed form of the utility function and probability distribution of outcomes. Let

$$U_{ij} = U(n_{ij}, R_j), \qquad (3)$$

be the utility that is realized if plan j is chosen and illness state i occurs; n_{ij} is the nonmedical consumption that is possible in state i with coverage provision of plan j, and R_j represents the restrictions on choice imposed by plan j. Let e_{ij} denote the out-of-pocket expenditures the beneficiary would be responsible for if illness state i occurs and he or she chose health plan j; further let z_j be the plan premium. Then nonmedical consumption in state i is:

$$n_{ij} = D - z_j - e_{ij},$$

where D denotes disposable income. That is, nonmedical consumption and hence utility in any illness state will depend on the cost and benefits of the health plan option selected.

If we let p_{ij} be the probability that state i occurs, then

$$E(U_j) = \Sigma_j p_{ij} U_{ij}. \qquad (4)$$

Data about a beneficiary's expected expenses in the next year can be used to infer the probabilities p_{ij}. Then, once we choose a specific form for the utility function in (3), we can substitute the righthand side of equation (4) into equation (2). Given our assumption that the stochastic component of utility is drawn from a normal distribution, equation 2 is similar to a standard probit model except that the $E(U_j)$ may also be a nonlinear function. We can use maximum likelihood methods to solve for the unknown parameters of the utility function and the β in equation (2).

To arrive at a specific form for the utility function we assume:

$$U_{ij} = f(n_{ij}) + g(R_j), \qquad (5)$$

that is, that utility is additive in nonmedical consumption and plan restrictions. During analysis we will try various specifications for

the function f and the function g in equation 5. For a recent empirical study that used consumer preferences among hypothetical insurance plans to compare various forms of the utility function in nonmedical consumption, see Marquis and Holmer (1986).

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