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THE IMF'ACT OF THE MEDICARE RISK PROGRAM ON THE USE OF SERVICES. AND COSTS TO MEDICARE

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Authors:

Jerrold Hill
Randall Brown
Dexter Chu
Jeanette Bergeron

Submitted to:

Health Care Financing Administration
Office of Research and Development
Room 2306, Oak Meadows Building
6325 Security Boulevard
Baltimore, MD 21207

Attention: James Hadley

Submitted by:

Mathematica Policy Research, Inc.
P.O. Box 2393
Princeton, N.J. 08543-2393
($09) 799-353s

Project Director: Randall S. Brown

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EXECUTIVE SUMMARY

The Medicare risk contracting program, in operation since 1985, currently enrolls nearly 1.4 million Medicare beneficiaries in 83 plans. The program was designed to reduce the cost to Medicare for enrolled beneficiaries, encourage more efficient use of health care resources and provide beneficiaries with the option of receiving care from Health Maintenance Organizations (HMOs). The mechanism for achieving cost-savings to HCFA and a more efficient use of resources is the program's system of capitated reimbursements. Participating HMOs are paid 95 percent of the estimated cost to Medicare for providing Medicare-covered benefits to beneficiaries in the same geographic area and with the same actuarial risk factors (age, sex, Medicaid eligibility, whether disabled, and whether in nursing home). If the costs that would have been incurred for enrollees had they remained in the fee-for-service (FFS) sector are predicted accurately by the payment rate methodology, then paying 95 percent of this rate will reduce the costs to Medicare for the enrolled population by 5 percent. Since HMOs must hold their costs below their capitation payments in order to realize a profit, they have an incentive to provide care more efficiently than FFS providers.

In this study we evaluate whether HCFA is realizing the anticipated 5 percent costs savings for enrollees and whether participating HMOs are providing care more efficiently than the FFS sector. The primary results summarized here are based on a stratified random sample of nearly 6,500 beneficiaries in Medicare risk plans (enrollees) and approximately the same number of beneficiaries in the FFS sector (nonenrollees). Data on health care utilization, insurance, health and functional status, history of serious illness, and demographic characteristics were obtained from a survey of this sample. These data were augmented by data on Medicare reimbursements, HMO characteristics, market area characteristics, and mortality. Together, these data sources enabled us to control statistically for differences between enrollees and nonenrollees on many factors thought to influence health care utilization. Thus, we are able to obtain unbiased estimates of the impact that the Medicare risk program has on HCFA's costs and of HMOs' ability to provide Medicare-covered services more efficiently than the FFS sector.

A. PROGRAM IMPACT ON COST TO THE MEDICARE PROGRAM

The first question that we addressed in the study was whether Medicare is realizing the anticipated 5 percent savings in costs for enrollees. We found that Medicare risk plans are experiencing favorable selection, and as a result, HCFA is spending more than they would have under FFS care for the beneficiaries enrolled in the program, rather than saving the anticipated 5 percent.

1. HCFA Pays 5.7 Percent More For Enrollees Than Would Have Been Spent on Them Under Fee-For-Service

A number of characteristics associated with high service use were less prevalent among enrollees than nonenrollees. Enrollees in Medicare risk plans were less likely to report poor health, to report functional impairments, to have a history of serious illness (cancer, heart disease, or stroke), and less likely to die in the **9** month period after the survey interview. Thus, all of the measures of health and functional status examined suggest that enrollees should use fewer services than nonenrollees. Compared with nonenrollees, enrollees also had a lower propensity to use services, as measured by the higher fraction of enrollees who said they avoid seeing a physician when a health problem arises, and the lower proportions of enrollees who say they worry more about their health than their contemporaries do. Thus, based on all available measures of health and attitudes toward health care, enrollees were expected to use less services than nonenrollees.

As a result of these differences, HCFA paid HMOs 5.7 percent more for coverage of Medicare beneficiaries than would have been paid under fee-for-service, with most of the overpayment being for Part A services. The estimated effect is significantly different from zero at the .01 level, and the 95 percent confidence interval surrounding our estimate of the cost increase ranges from 2.4 percent to 9.1 percent. Payments exceeded projected costs by 8.5 percent for Medicare Part A coverage (for hospital, skilled, nursing and home health care), compared to 2.7 percent for Part B (physician services, office procedures). Our regression analysis shows that the measures of health status and propensity to seek care have sizable effects on the Medicare reimbursements of nonenrollees. Thus, when the estimated models were used with the personal characteristics of enrollees to predict what Medicare FFS costs would have been for enrollees had they not enrolled, we find that the average actual FFS costs for nonenrollees are about 20 percent higher than the average projected Medicare FFS costs for enrollees (compare first and last bars in Figure 1). After adjusting nonenrollee costs to eliminate any cost differences that could be accounted for by differences between nonenrollees and enrollees on factors controlled for by the AAPCC we find that even the adjusted nonenrollee costs are 11.3 percent higher than predicted costs for enrollees (compare second and last bars in Figure 1). This difference means that the actuarial risk factors used to determine AAPCC rates failed to account for the better health status and, hence, lower costs of enrollees, and as a result AAPCC rates exceed by 11.3 percent the FFS costs that Medicare would have incurred for enrollees. Since HCFA pays 9.5 percent of the AAPCC, the 11.3 percent over-prediction of costs by the AAPCC methodology translates into the 5.7 percent loss reported above (1.1134 x .95 = 1.057).

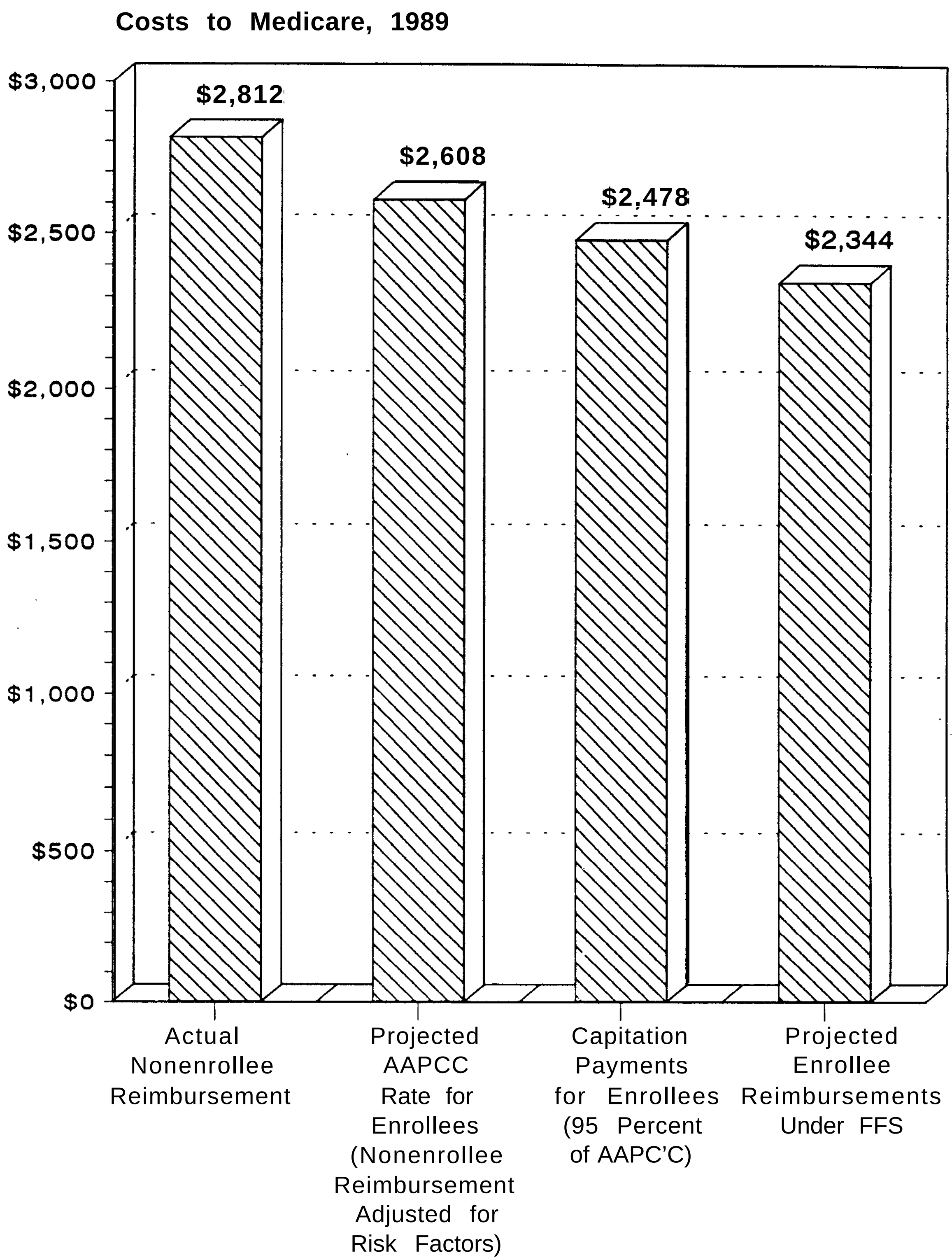
Disaggregation of our impact estimates showed that 83 percent of the overestimate of cost that is implicit in the AAPCC is due to enrollee-nonenrollee differences on health status measures. The difference in the proportion of beneficiaries with a history of cancer, heart disease or stroke was the single most important source of the AAPCC overestimate of costs for enrollees, accounting for 38 percent of the total. Attitudinal differences toward health care account for about 14 percent of the overestimate. Differences on socioeconomic characteristics, including income and (predicted) Medigap coverage, account for the remaining 3 percent of the difference between AAPCC rates and the projected FFS costs that would have been incurred.

2. Costs Increases Generated by the Program Are Greater for Staff Model Plans, High-AAPCC Areas, and Plans Charging No Premium

The extent to which AAPCC payments exceeded projected FFS costs varied with some key plan and market area characteristics. Staff model plans experienced the most favorable selection, increasing cost to HCFA by 7.8 percent versus cost increases of 4.4 percent for group model plans and IPAs. The finding that IPAs have less favorable selection than staff model plans is consistent with previous studies, and with the fact that a high proportion of enrollees in IPAs are patients who were seeing an IPA physician prior to enrolling. Cost increases are strongly and inversely related to the premium charged by risk plans. Enrollees in plans charging zero premium for supplemental services cost HCFA 8.3 percent more than they would have under fee-for-service, compared to cost increases, of only 2 percent for enrollees in plans charging over $50 per month and 4.5 percent for those charging $1 to **$50.** This result is not surprising, since more favorable selection enables plans to offer supplemental coverage for lower or zero premium. Similarly, plans in market areas with high AAPCC rates experience more favorable- selection and generated cost increases of 7.6 percent in

FIGURE 1

AVERAGE COSTS TO HCFA FOR
ENROLLEES AND NONENROLLEES



1989, twice the 3.8 percent loss incurred by HCFA for enrollees in HMOs with AAPCC rates of $275 to $325 per month. Apparently, HMOs in areas with high AAPCC rates are just as able to enroll a disproportionately high number of beneficiaries with low demand for health care.

B. PROGRAM IMPACT ON THE USE OF SERVICES

Despite the fact that HCFA does not save money on the Medicare risk program, we found that the potential for savings exists, because HMOs do reduce utilization of some costly services. However, some of these findings were somewhat unexpected.

1. Medicare Risk Plans Reduce Hospital Length of Stay, but not Admissions

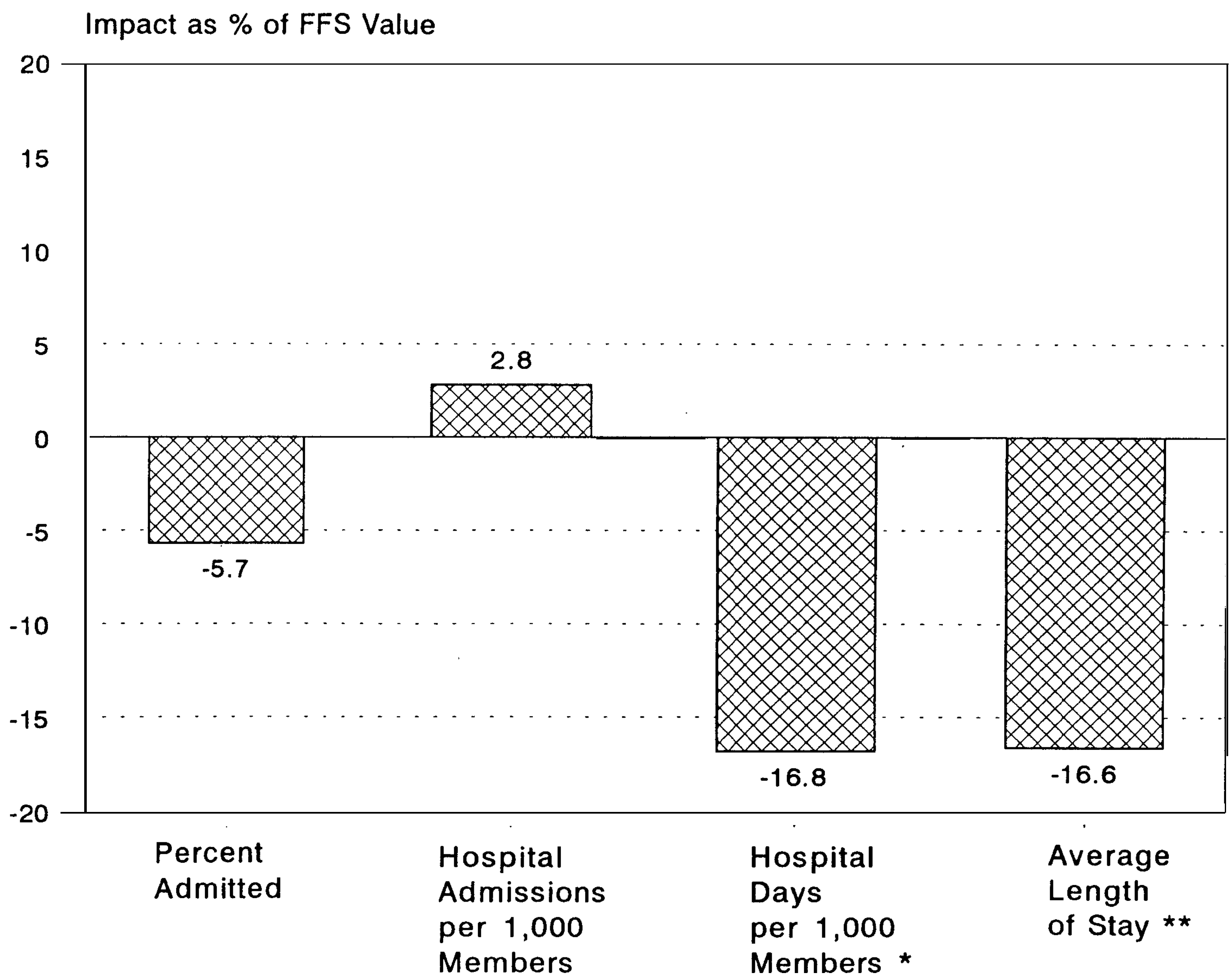
We found HMOs did not reduce the hospitalization rate, but did reduce the number of hospital days by about 17 percent (see Figure 2). Our finding that HMOs have no impact on the hospitalization rate is in contrast to the sizable reductions found in previous studies of HMO impacts on use among the non-Medicare population. However, hospitalizations among the Medicare population may be less discretionary in nature, reducing the ability of HMOs to lower the admission rate. More importantly, pre-admissions screening by indemnity insurers is now a common practice for non-Medicare patients in indemnity plans, and hospitalization rates in both the Medicare and non-Medicare populations have declined since 1980. (Admission rates per 1,000 Medicare beneficiaries nationally dropped by 25 percent between 1985 and 1989.) Hence, general medical practice has responded to financial incentives to eliminate discretionary hospital admissions, which is not reflected in earlier studies of HMO impacts.

The 17 percent reduction in hospital days and average length of stay are consistent with recent findings in the literature for the non-Medicare population and with our findings for Medicare patients with two specific conditions, based on an independent source of data used in the quality of care study conducted as part of the evaluation of the Medicare risk program (Retchen, et al., 1992). The reduction in hospital length of stay is particularly impressive, given the incentives under the Prospective Payment System (PPS) to reduce length of stay in the FFS sector.

The finding that HMOs do not reduce hospital admissions does *not* imply that HCFA cannot save money by enrolling beneficiaries in HMOs. This erroneous inference appears plausible because HCFA pays hospitals a predetermined amount for each non-HMO Medicare patient based on diagnosis, regardless of the length of stay. However, savings to HCFA depend only upon whether the AAPCC payment rates are an accurate estimate of what FFS reimbursements would have been for enrollees. If AAPCC rates were accurate predictors (that is, if there were no unaccounted for biased selection), then paying HMOs 95 percent of the AAPCC would *guarantee* savings of 5 percent to HCFA, even though admission rates would be unaffected. HMOs, in turn, offset their lower revenue relative to FFS providers by shortening the average length of hospital stays, since most HMOs pay negotiated per diem rates to hospitals. Thus, even though HMOs do not reduce hospital admission rates, savings to HCFA on Part A Medicare costs can occur because payments to HMOs will be 5 percent less than FFS costs would have been.

FIGURE 2

HMO IMPACTS ON THE USE OF INPATIENT SERVICES



* Significantly different from zero at the .10 level, 2-tailed test.
** Significantly different from zero at the .05 level, 2-tailed test.

2. **Enrollees in Medicare Risk Plans Are More Likely to Have Some Physician Visits but Are Less Likely to Have Frequent Visits**

We found that Medicare risk plans increased by about 5 percentage points the likelihood of receiving at least one visit to a physician during the year (from 84 to 89 percent). Enrollees were also 6 percentage points more likely to have received a physical exam in the past year than comparable nonenrollees (see Figure 3). However, enrollees were significantly less likely to report frequent visits to a physician **(12 or more a year), and risk plans had no** effect on the number of visits during the past month. The results are consistent with the financial incentives facing both enrollees and HMO physicians. In most Medicare risk plans beneficiaries face little or no copayments for primary care visits, and are typically offered preventive care as part of their benefits package. Thus, because enrollees face little or no financial barriers to receiving care from their primary care physicians, we would expect a higher likelihood of some physician use compared to nonenrollees. However, HMO physicians--in particular, those under capitation or profit sharing--have a financial incentive to reduce the number of visits and reduce referrals to specialists. Thus, we find enrollees to be less likely to have many visits. These results are confirmed by a companion study in this evaluation (Clement et al, 1992), which finds less use of specialists, fewer followup visits, and less monitoring for patients with three separate chronic conditions.

3. **Medicare Risk Plans Increase the Likelihood of Receiving Care in A SNF, But Not SNF Days**

We found that Medicare risk plans increased the likelihood of receiving care in a skilled nursing facility (SNF) by 0.3 percentage points (see Figure 4). This increase is statistically significant and large in percentage terms, but small in absolute magnitude since by comparison only 0.8 percent of nonenrollees in the sample received care in a SNF over the past year. The estimate is consistent with the expectation that reduced length of stay in an acute care hospital may be achieved by substituting SNF care for acute hospital care. However, the estimated effect on total SNF *days* was-not statistically significant (see Figure 5). This result, like those for physician visits, suggests that HMOs increase the frequency of SNF use but decrease the intensity of use.

4. **Enrollees Are Equally Likely to Receive Home Health Care As Comparable Nonenrollees, but Have Only Half as Many Visits**

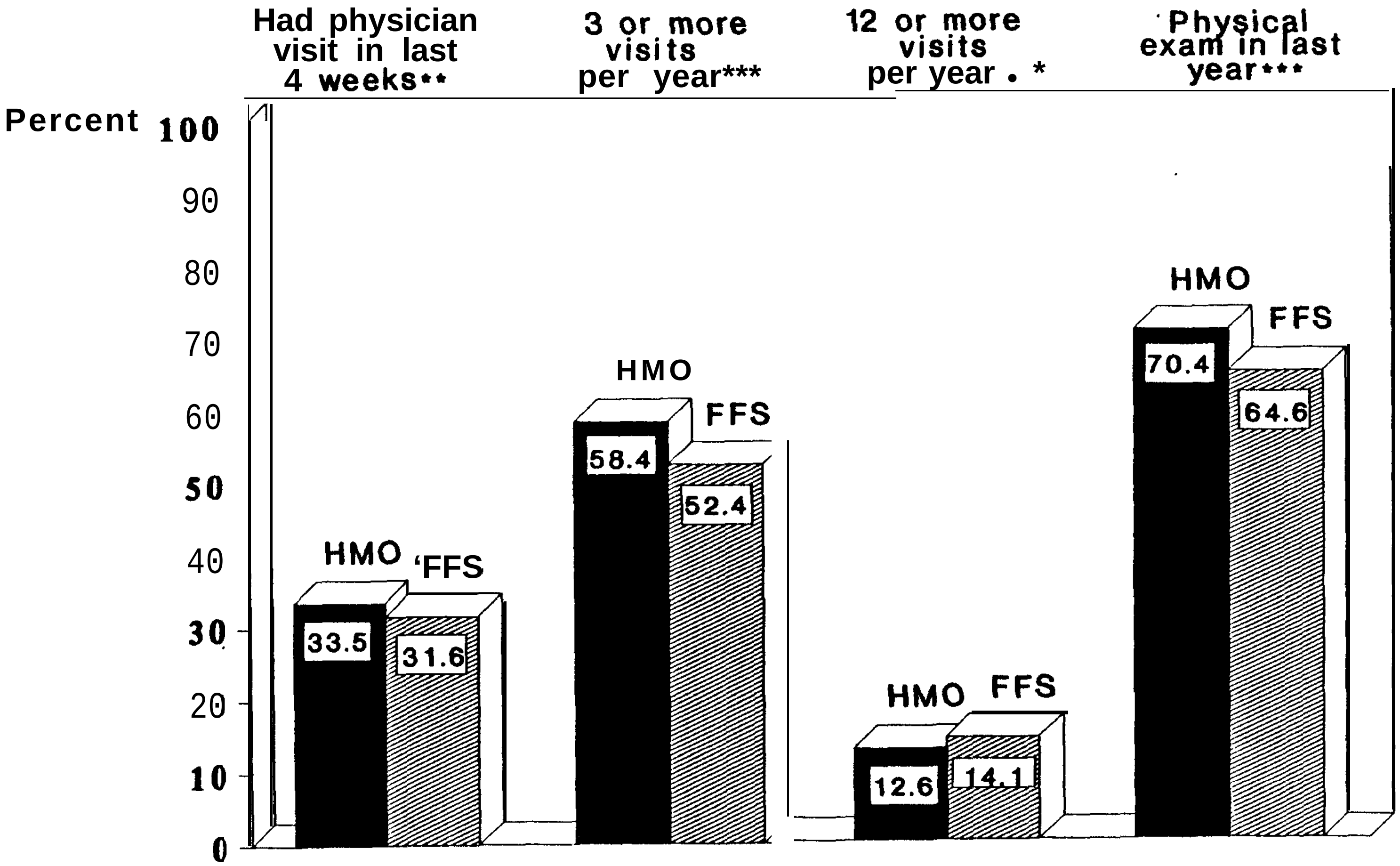
We found that HMOs have no impact on the likelihood that HMO enrollees receive home care by a skilled nurse, therapist, or home health aide (see Figure 4). However, we do find that enrollees receive 50 percent fewer home health visits than comparable nonenrollees (see Figure 5). The results suggest that Medicare risk plans are not substituting home health care visits for acute care hospital days and have been able to reduce the number of visits. This reduction occurs during a time period in which the rate of growth in home health visits in the FFS sector greatly accelerated.

5. **HMO Impacts on Service Use Vary by Health Status**

We found that HMOs' increase the use of some services for the beneficiaries in the poorest health, but for other services the ***reduction in service use is*** greater for those in the poorest health. For example, the estimated HMO impact on hospital admission was small and not significantly different from zero overall, but positive and significant for enrollees in poor health or with functional impairments. On the other hand, reductions in hospital days and home health visits were observed

FIGURE 3

USE OF PHYSICIAN SERVICES

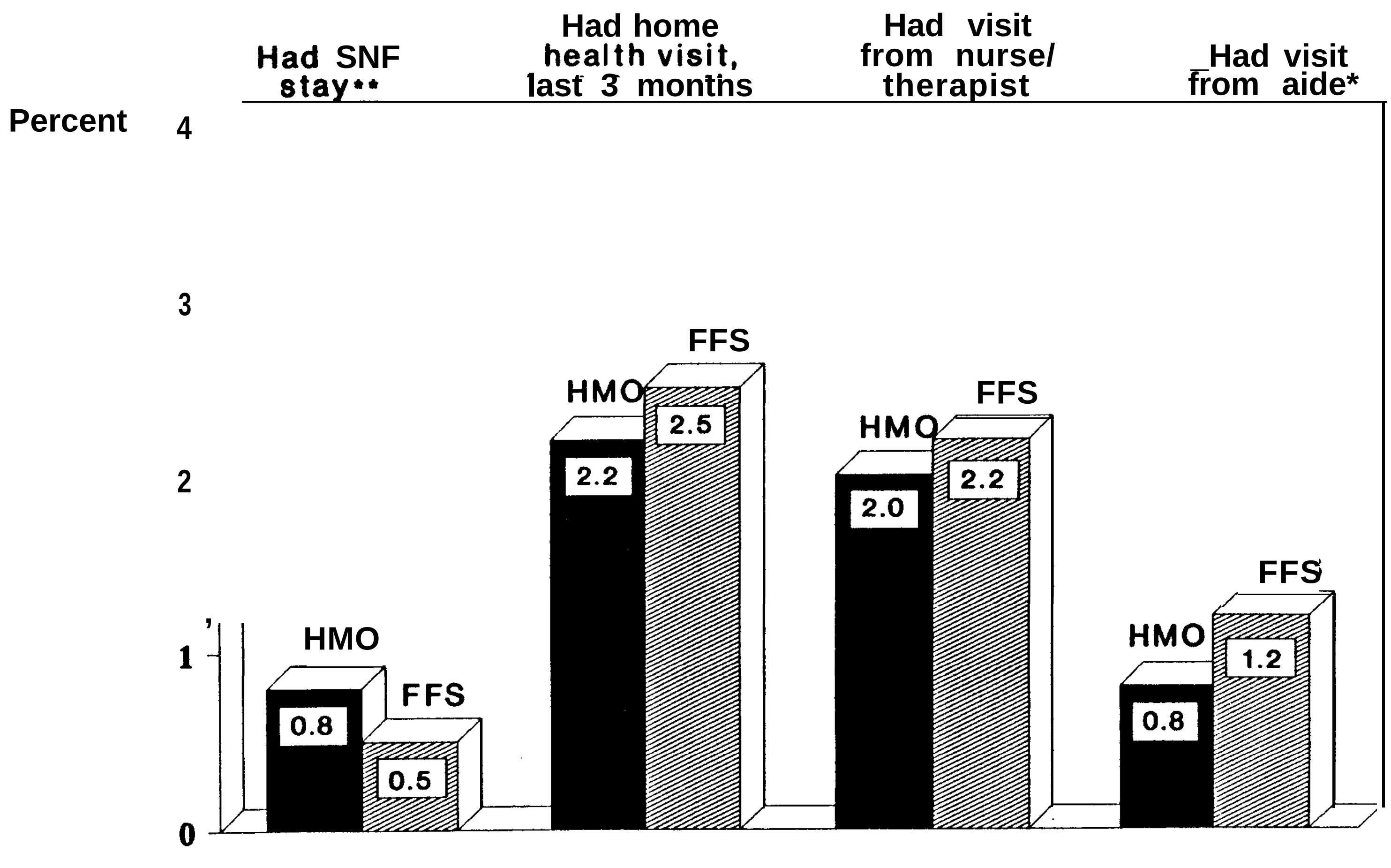


. . Statistically significant difference at the .05 level, 2-tailed tort.

. ** Statistically significant difference at the .01 level, 2-tolled tent.

FIGURE 4

PERCENT RECEIVING SNF AND HOME HEALTH SERVICES

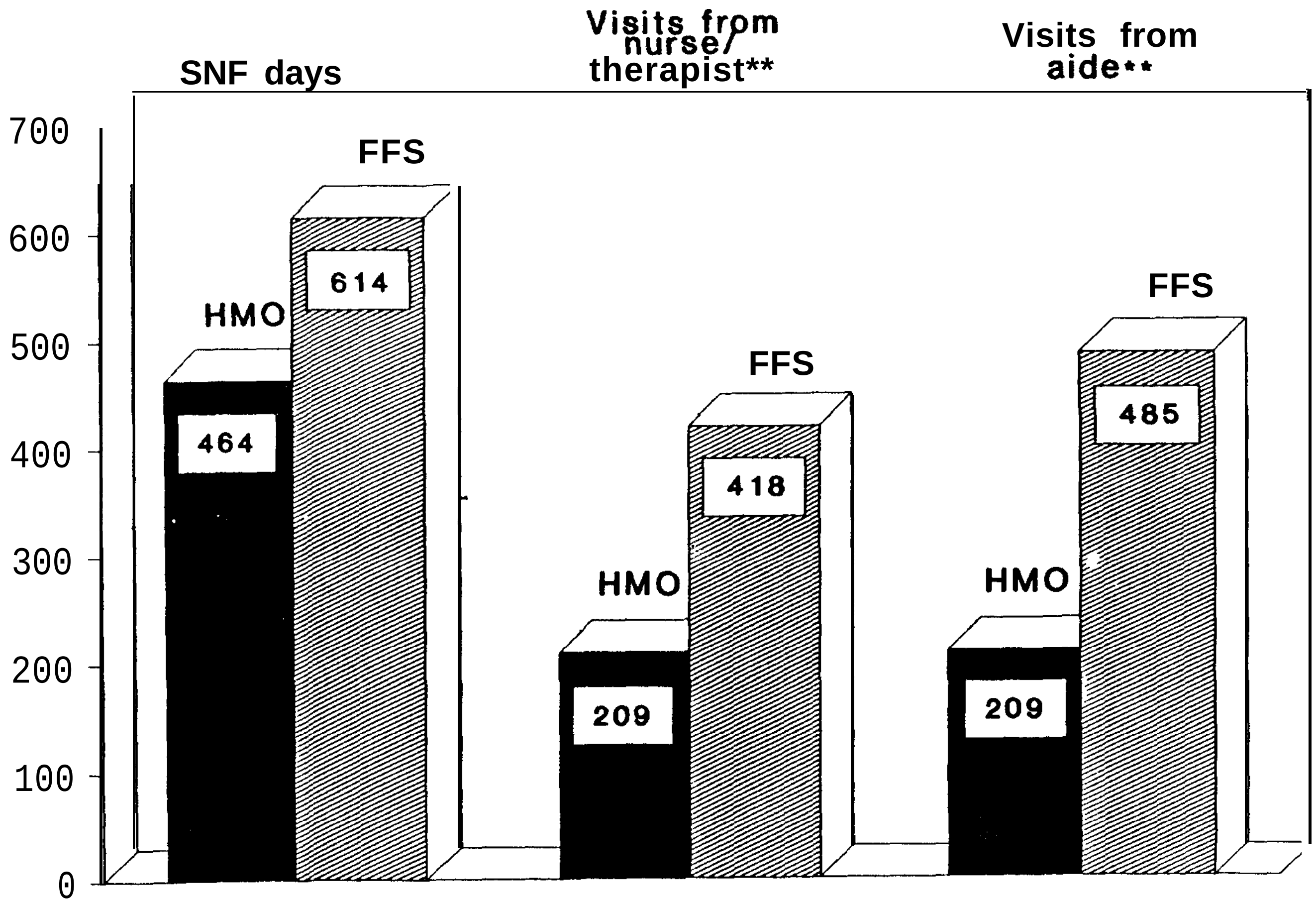


. Statistically significant difference at the .10 level, 2-tailed test.

. = Statistically significant difference at the .05 level, 2-tailed test.

FIGURE 5

SNF DAYS AND HOME HEALTH VISITS PER 1,000 MEMBERS



. . Statistically significant difference at the .05 level, 2-tailed test.

for all groups and were greatest for beneficiaries who were in poor health, had ADL impairments, or died within 9 months after the interview.

The increased rate of hospital admissions and physician visits suggest that enrollees in poor health may have greater access to initial care for a problem than comparable nonenrollees. The reduction in hospital days and home health days have three possible explanations. One interpretation 'is that HMOs are able to achieve reductions in use for those most in need of care without sacrificing quality of care. Under this interpretation, HMOs are able to provide care more efficiently to beneficiaries who are high users of resources. A more negative interpretation is that enrollees with health problems are denied the level of care that is needed to deal with their condition appropriately. The third interpretation is that our measures of health may not be fine-grained enough to control for enrollee-nonenrollee differences in health status that influence use. That is, the subgroup of enrollees reporting poor health or functional impairments may be healthier or less impaired than the subgroup of nonenrollees reporting the same problems. If so, the fewer number of hospital days for enrollees in poor health compared to nonenrollees in poor health may reflect, in part, the somewhat better health for enrollees, which we can not observe. Under this third scenario, our estimates would overstate HMO impacts since they would be due to favorable selection that is not controlled for by our measures of health status. Given the detailed set of control variables, we believe that this last explanation is less likely than the others. The issues of whether HMOs deliver adequate care or restrict access to care are addressed in other project reports (Clement et al, 1992, and Retchin et al, 1992), which find no evidence of poorer outcomes for HMO members. Thus, HMO impacts on service use appear to be due to elimination of unnecessary services.

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6. HMOs Spend Less in Total for Medicare-covered Medical Services than FFS Medicare Would

The reductions in service use that HMOs achieve have no effect on costs or savings to HCFA--the net effect on HCFA costs from the risk program depends only upon whether the risk-adjusted AAPCC rates accurately reflect the costs that Medicare would have incurred in FFS reimbursements had the enrolled beneficiaries not joined an HMO. Any "savings" due to HMO reductions in service use relative to FFS use rates (or to more favorable prices for services) are realized only by the HMO and will go to (1) help HMOs cover the 5 percent reduction in payments that HCFA imposes by paying risk plans only 95 percent of the AAPCC; (2) cover costs that risk plans incur but FFS providers do not (e.g., administrative expenses for utilization review, recruiting and educating physicians, marketing costs, etc.); (3) HMO profits; and (4) reduced or zero premiums for additional benefits for enrollees. Thus, although reductions in utilization of medical services do not directly lead to savings to HCFA, the total value of these reductions provides an indication of whether the HMOs are likely to be able to prosper if favorable selection were eliminated (or the payment method fully captured it) and whether enrollees are likely to benefit from the risk plans' efficiency.

Unless the value of these "savings" to HMOs due to reductions in resources are equal to about 15 percent of costs (5 percent to cover the fact that payments are 95 percent of FFS costs and about 10 percent on average to cover administrative costs), HMOs will find it difficult to profit on their Medicare plan unless they have favorable selection. The finding that HMOs reduce hospital days by 17 percent and home health visits by 54 percent suggest that HMOs ***may*** be able to break even or profit if selection were neutral or accounted for. However, HMOs do not reduce the use of physician services at all (our point estimate suggests that visits increase slightly, but the estimate is not significant). Thus, HMOs save nothing on physician services, which account for about 25 percent of the total costs that Medicare would have incurred for enrollees. We estimate, conservatively, that HMOs spend about 10.5 percent less in total on services covered by Medicare than Medicare would have spent in reimbursements for the enrollees. However, these rough calculations do not take into account possible additional savings that HMOs may achieve by using less expensive providers (e.g., fewer specialists), using resources less intensively during encounters (e.g., by performing fewer tests, using intensive care units less often, etc.), or negotiating prices for provider services that are below the rates HCFA pays to FFS providers. While there is evidence of such economies from other components of the evaluation, we have no estimate of the dollar magnitude of the savings that they generate. Nonetheless, without the favorable selection experienced, HMOs would have to rely fairly heavily on these additional sources of savings in order to break even, unless the HMO has a large Medicare enrollment (enabling it to cover fixed administrative costs more easily) or reduces service use by more than the average estimated for all HMOs.

C. QUALIFICATIONS

Although we believe the methodology, sample, and data to be sound and the best available for this analysis, and we have confidence in the validity of the estimates, there are three qualifications that should be kept in mind. First, the data on utilization are self-reported, which could lead to inaccuracies. Second, the estimates of costs to HCFA are based on our projections of the AAPCC rates rather than actual AAPCC payments. And finally, our estimates of program effects on costs reflect only costs to HCFA, not total health resource costs.

We believe that none of these qualifications casts serious doubt on our overall findings, but the last two qualifications do affect the interpretation of some estimates. While self-reported data may be less accurate than records data due to recall errors, the data are collected in exactly the same way over the same time period for enrollees and nonenrollees, so any reporting errors are equally likely for both groups and should not influence the findings. The use of projected AAPCC rates rather than actual payment rates was necessary because all nonenrollees were alive over the time period for which costs were measured. While this approach should lead to more reliable estimates of the likely systematic, overall program impact on costs to HCFA across areas and across years, it will yield inaccurate estimates of actual costs (or savings) to HCFA for subgroups of HMOs defined by market areas or HMO characteristics because it does not reflect errors in the geographic adjustors to the AAPCC. Furthermore, impacts on costs and utilization might have been somewhat different if those who died during the year prior to interview could have been included. Finally, it must be remembered that the estimates of impacts on costs reflect only costs to the Medicare program, not total costs to all payors (beneficiaries and Medicare) nor total resource costs. These are different questions that go beyond the scope of this study.

D. IMPLICATIONS FROM THE STUDY

One clear message from our analysis of impacts of the Medicare risk program on costs to HCFA is that the current payment rate methodology overstates what the enrolled beneficiaries would cost HCFA if they were receiving care in the FFS sector. HCFA is spending more money on the currently enrolled population than it would have spent had they remained in FFS, because enrollees are healthier than nonenrollees on most of the available measures. The results indicate that it may be necessary to incorporate some type of health status adjuster in the payment methodology. Since over one-third of the AAPCC overestimate of cost is attributable to the below-average proportion of enrollees with a history of cancer, heart disease or stroke, a measure such as this might be used as an additional factor in the AAPCC. The results also suggest that proposals to raise capitation to 100 percent of the AAPCC to encourage greater HMO participation in the program should be carefully evaluated. With the current payment methodology, such proposals are likely to generate much greater costs for the Medicare program, at least in the short run.

The results from our analysis of HMO impact on service use provide an equally clear message: HMOs are reducing service use, and they are doing so apparently without limiting beneficiaries' initial access to either inpatient or primary care. Indeed, we find that enrollees are *more* likely to receive primary care. A qualification to this assessment is the evidence that reductions in the amount of services used are greatest among those in poor health, with functional impairments, or in the last year of life. Evidence from the study of quality of care in Medicare risk plans (Retchin et al, 1992) indicates that the amount of services used by enrollees hospitalized for stroke and cancer were indeed less than use by nonenrollees, and that enrollee and nonenrollee outcomes (mortality rates and hospital readmission rates) were the same. Furthermore, there is little or no differences in patient-reported outcomes for chronic conditions examined (Clement et al, 1992). This suggests that HMOs can reduce service use for those most in need without compromising patient outcomes.

These findings might suggest that payments to HMOs should be reduced to reflect the favorable selection experienced by HMOs, thereby lessening the net cost to HCFA. AAPCC payments exceed by 5 to 6 percent the cost that HCFA would have incurred had enrollees not joined the HMO, and HMOs reduce utilization such that direct total expenditures by HMOs for Medicare-covered services is less than what Medicare would have spent in FFS reimbursements for these individuals. Thus, it would appear that lower payments to HMOs (implemented by taking into account enrollees' better health status) could reduce the magnitude of cost increases to HCFA while still enabling HMOs to prosper.

Other evidence from the evaluation, however, suggests that cutting payments to all HMOs may drive many plans out of the risk program. Nearly half of the HMOs having 1,000 or more Medicare risk enrollees at some time between 1987 and 1990 had discontinued their risk contract by 1991, typically citing financial losses as the principal reason (McGee and Brown, 1992). And there have been very few new entrants to replace these exiting plans. Furthermore, a number of still active HMOs, including some with sizeable enrollments, are complaining about losing money and considering dropping their contracts. While the rate of contract nonrenewals slowed in 1991 and 1992, reducing payments to all HMOs is likely to kindle a new round of nonrenewals and further stifle interest in participation among HMOs not currently holding a risk contract.

If plans are being paid more than FFS providers would have been paid for services to these individuals, and plans are successfully cutting utilization, why should they be losing money? A number of explanations for this anomaly have been proposed by HMOs and others: (1) nonmedical costs associated with Medicare risk contracting, such as the costs of monitoring and managing utilization and the cost of marketing the plan to individual beneficiaries, may offset much of the savings from reduced utilization, especially in smaller plans; (2) Medicare capitation payments are based on Medicare FFS payment rates for services (e.g., physician visits), which may be lower than the rates HMOs are accustomed to paying for providing the same services to commercial clients; (3) the greater access to and emphasis on preventive care and early detection of serious illness in HMOs may actually increase costs; (4) because they must bear the full cost of any increase in service use arising from increasing beneficiaries' access to care, HMOs are at a competitive disadvantage relative to medigap policies; (5) enrollment may be too low in many HMOs to spread fixed costs adequately.

Whatever the reason for the incongruity of increased costs to HCFA and a high rate of financial failure among risk plans, four factors should be kept in mind in seeking ways to help the risk program achieve the goal of saving money for the Medicare program:

- Although a number of risk plans have financial problems, many others are prospering.

- Much of the increase in costs to HCFA is passed on to enrollees in the form of additional benefits and lower premiums.

- Even if Medicare is currently spending more on risk contracting than it would for FFS care, longer term benefits of risk contracting may outweigh these costs.

- Favorable selection is the reason that payments are higher for risk contracting than for FFS care.

The first point is that AAPCC payments may need to be lowered in some areas or for some plans but increased in others. Some risk plans, most located in areas with high AAPCC rates, charge no premium for covering the Medicare deductibles and copayments, and also cover at no charge other services not covered by Medicare. These plans are obviously prospering, and the beneficiaries who enroll in them are receiving greatly enhanced benefits. At the other extreme, some plans receive payment rates that are over $100 less per member month than the plans that charge no premium. Many of these plans are losing money and leaving the risk program. Our estimates indicate that the increases in costs to HCFA are much greater in high AAPCC areas as a result of more favorable selection in these areas. Modifying the AAPCC risk adjustor by including a history of serious illness as an additional risk factor would essentially eliminate the overpayment and would reduce AAPCC payments the most for HMOs with the most favorable selection. It is also likely that the geographic adjustors do not accurately represent- the relative cost of providing Medicare-covered services in different market areas and should be calculated differently.

The second point--that much of the excess payments from HCFA result in increased benefits for enrollees--suggests that HMOs may deliver the total package of services provided for less than the FFS sector. About half of Medicare risk enrollees are in risk plans that charge no premium, and many plans offer benefits for far lower premiums than the beneficiary would have to pay for comparable Medigap or indemnity coverage. Nonetheless, it was not HCFA's intent for the program to result in taxpayers everywhere subsidizing enrollees in a few HMOs. Thus, to achieve the original goal of reducing costs to the Medicare program, both the excess payments due to favorable selection and the geographic disparities should be eliminated. Perhaps one partial solution would be to require that risk plans projecting a surplus share some of the expected excess with HCFA, rather than being allowed to pass it all along to enrollees in lower premiums and greater benefits.

The third point is that incentives to control utilization and HMOs' demonstrated success in doing so suggest that, over the longer term, risk contracting may be a more efficient delivery system that could ultimately yield savings to HCFA Substitution of one type of service for another by HMOs results in real resource savings rather than simply shifting costs, as sometimes occurs in the FFS sector. HMOs are also able to use their market power to negotiate favorable rates from hospitals and other providers. Furthermore, HMOs' emphasis on preventive care could result in long-term savings, or at least better long-term outcomes for enrollees.

Perhaps the best solution for HMOs, Medicare, and beneficiaries is to focus efforts on obtaining **a more neutral self-selection of enrollees into HMOs. Payments are too high currently because those** who enroll are healthier on average and less prone to use services than the general Medicare population, even after controlling for age and other AAPCC risk indicators. However, HMOs have proven that they can cut costs by providing care more efficiently than the FFS sector, and they do so by managing the care of the sickest patients most efficiently. Thus, if methods can be devised to increase enrollment among enrollees with more serious health problems, the payment would no longer exceed the projected FFS cost, HMOs would still prosper, and total resource use and costs would decline. Such a change, coupled with payment reform to reduce AAPCC inequities within and across market areas, could enable the program to achieve its original objectives of cost savings through greater choice for Medicare beneficiaries.

I. INTRODUCTION

As of March 1992, HCFA's Medicare risk contracting program included 83 HMOs providing care to nearly 1.4 million Medicare beneficiaries. The program was initiated to achieve two primary objectives: to provide Medicare-coverage at lower cost than the fee-for-service (FFS) sector, and to provide Medicare beneficiaries with a range of health care choices similar to that which employed individuals face. In this study, we analyze the program's ability to provide coverage for health care services covered by Medicare at lower cost than FFS providers in the same market areas. We assess this ability by estimating the impact of the risk contracting program on the costs to the Medicare program, and on the services used by beneficiaries enrolled in Medicare risk plans. As we discuss below, the impact of the program on costs to Medicare depends largely on two factors that are not related to the efficiency of Medicare risk plans: how accurately capitation payment rates reflect FFS costs, and the degree of biased selection in enrollment. However, by estimating the impact that Medicare risk plans have on the volume of services used by enrolled beneficiaries, we provide evidence on whether risk plans operate more efficiently than FFS Medicare, and by how much. Such evidence is important in evaluating alternative proposals for health care reform, since HMOs are envisioned as an important element in containing health care costs under some proposals.

A. EVALUATING PROGRAM IMPACT ON MEDICARE COSTS

A principal reason for instituting the Medicare risk contracting program was to slow the increase in rising Medicare costs. Indeed, increased use of HMOs and other forms of managed care is still viewed as a principal vehicle for cost-containment in proposals for health care reform (see, for example, The President's Comprehensive Health Reform Program, 1992, pp. 36-42). The principal element in the Medicare risk contracting program for achieving cost savings is capitation. In exchange for providing all Medicare-covered services for enrolled beneficiaries, HMOs under contract receive a monthly capitation payment that is 95 percent of the projected costs to Medicare for these beneficiaries had they not enrolled in the HMO. By design this system of capitation payments should achieve two objectives:

1. By the 95 percent formula, it should save the Medicare program five percent for the beneficiaries enrolled, if projected FFS costs are an accurate estimate of what Medicare costs would have been had enrollees remained in the FFS sector.

2. Capitation also provides an incentive for Medicare risk plans to provide health care coverage at lower cost than the FFS sector, since plans can realize a profit on the difference between their costs and the capitation they receive for the beneficiary.[1]

In analyzing the program's impact on the costs to Medicare, we are evaluating whether the first objective is achieved. As we discuss below, this is principally an evaluation of how payments to the HMOs under the AAPCC methodology compare to the costs that Medicare would have incurred for enrollees had they remained in the FFS sector. In analyzing the program's impact on service use, we are evaluating the second objective, the effectiveness of capitation as an incentive to contain costs.

1. Reasons Why the Program May Not Achieve a Five Percent Savings

There are two fundamental principles in the methodology for determining the AAPCC payment rates: the rates should reflect (1) average Medicare FFS costs in the enrolled beneficiary's geographic area, and (2) the expected cost of providing Medicare-covered services to the enrolled beneficiary, relative to the area average, based on his or her age, sex, reason for entitlement to Medicare, place of residence (nursing home or other), and Medicaid status. If the payment rate methodology does not accurately estimate on average what enrollees' costs would have been had they remained in the FFS sector, savings to the program will depart from the expected 5 percent.

In setting capitation rates, three types of inaccuracies could arise: (1) random forecast errors in projecting FFS costs for any particular year or site, (2) systematic forecast errors in projecting FFS

[1]However there is an implicit limit on the HMO's rate of profit on the Medicare portion of its enrollment: it is not to exceed the HMO's profit rates on its commercial enrollment. Projected profits exceeding the rate implicit in the HMO's commercial rates for services must be returned to HCFA, or to beneficiaries in the form of more benefits or lower premiums.

costs, and (3) biased selection. Any payment system based on projected costs will have forecast errors, and there are two reasons why errors may be especially large for the Medicare risk program. First, FFS costs for the payment rate year are projected from data that are several years old. Second, the Medicare program frequently experiences changes in benefits covered and provisions for reimbursement, making future costs more uncertain and, therefore, difficult to predict. If the errors are random with a mean of zero, they cause little problem, unless they are especially large. If the errors are systematic-estimates persistently too high or too low for some area, some type of beneficiary, or overall--the consequences are more severe.

Forecast errors have different implications for the Medicare program and Medicare risk plans. From the plan's perspective, forecast errors as they affect its payment rates are relevant, since they affect the financial risk and profitability of program participation. An analysis of forecast errors from the perspective of the plan, while important, is beyond the scope of this study. From the perspective of the Medicare program, the average forecast error program-wide is the relevant measure for determining the impact of the program on costs to Medicare. Evidence of greater forecast errors facing plans in some geographic regions or during some time periods is not the main concern in determining cost impacts, if the errors do not persist over long periods of time and the average error over time is zero. Since our main purpose is to evaluate the impact of the program on the costs to Medicare, this is the perspective relevant for our analysis.

The other possible source of inaccuracy in setting rates, biased selection, is the difference between the average FFS costs of nonenrollees (with the same distribution as enrollees on AAPCC factors)_ and the average FFS costs for enrollees had they remained in the FFS sector. Biased selection thus reflects enrollee-nonenrollee differences in health status or other personal characteristics that influence demand for care but are not accounted for in the payment rate methodology.

2. Evidence from the Literature On the Accuracy of Payments and Biased Selection

A previous study on the accuracy of the payment rate methodology (Gruenberg, Pomeranz, and Porell, 1988) suggests that the projection errors expected program-wide are relatively small and provide no evidence that payment rates evaluated program-wide would systematically overpay or underpay HMOs over time. Milliman and Robertson (1987) note that projections of the USPCC were relatively accurate in 1985 and 1986, but note (based on the estimates for 1987) the trend toward underprediction in 1986 and 1987. Evidence released from the Office of the Actuary (1990) indicates that projections of the USPCC subsequently showed a trend toward overprediction between 1988 and 1990, and that the average prediction error between 1985 and 1990 was less than 1 percent. Hence, there is not compelling evidence that projected FFS costs are systematically under or over-predicted for the Medicare program as a whole.

There is a considerable body of literature providing evidence of favorable selection in the Medicare risk program. Eggers and Prihoda (1982), Brown (1988), Nelson and Brown (1989), Hill and Brown (1990), and Hill and Brown (1991), compare enrollees' Medicare FFS costs prior to enrolling to the FFS costs of nonenrollees with the same distribution on AAPCC risk factors, In general the studies found that pre-enrollment FFS reimbursements for enrollees were 20-25 percent less than the payment rate methodology would predict, implying a potential loss to Medicare of 20 to 28 percent rather than a five percent savings. Brown et al. (1986) report that at the time of enrollment, enrollees in the Medicare competition demonstrations had better health and fewer functional impairments than nonenrollees. Lichtenstein et al. (1989) performed a similar comparison for 22 risk plans and reported that nine plans experienced favorable selection and the remainder neutral selection. Hill and Brown (1991) did a similar analysis of a random sample of all enrollees in the Medicare risk program, and found that enrollees were healthier, had fewer functional impairments, and a lower incidence of high cost illnesses, even after controlling for AAPCC risk factors. Brown (1988) and Riley, Rabey, and Lubitz (1991) both report a 20 percent lower rate of mortality for enrollees than for Medicare beneficiaries in the same area with the same AAPCC risk factors.

There are a number of reasons why the impact on HCFA's costs from favorable selection, as measured by the FFS costs of enrollees prior to enrollment, may be less than the 20-28 percent implied in previous studies. First, enrollee FFS costs would probably have regressed toward the mean of the Medicare population over time, leading to a smaller differential than that observed prior to enrollment. Second, the lower FFS costs for enrollees may reflect the smaller fraction of enrollees with Medigap coverage prior to enrolling. (Brown and Langwell, 1988, show that prior to enrolling, enrollees were 7 percentage points less likely than non-enrollees to have Medigap coverage.) The higher out-of-pocket costs for those without Medigap coverage would have reduced enrollees' demand for care and their Medicare reimbursements relative to nonenrollees in the preenrollment period. However, it is likely that some of these enrollees would have purchased Medigap coverage in the absence of the Medicare risk program.[2] If the HMO program had not existed, the ratio of FFS reimbursements for those who did enroll to reimbursements for nonenrollees would then have been higher than the ratios presented in the studies based on the Medicare reimbursements for enrollees prior to enrollment.

The studies of biased selection comparing the FFS reimbursements of enrollees prior to enrollment to reimbursements for nonenrollees do not provide supporting evidence on health status or demand for care that would explain the favorable selection. An exception is the study by Hill and Brown (1991), in which data on beneficiary health status, and other personal characteristics were available. Controlling for the actuarial risks used to determine payments, they estimate that enrollee

[2]This expectation follows from two observations. First, by joining an HMO, enrollees have expressed their demand for supplemental coverage to Medicare. Second, we found that nearly one-fourth of current enrollees are beneficiaries who became entitled to Medicare after the beginning of the risk program, and that among this group, two-thirds joined an HMO within the first year of their entitlement. This suggests that enrollees are shopping for supplemental coverage--either a Medigap policy or an HMO--in their first year of entitlement. Thus, in the absence of the Medicare risk program, it seems likely that a substantial fraction of enrollees would have purchased some type of coverage.

FFS costs for enrollees would have been 25 percent lower than FFS costs for nonenrollees--an estimate of biased selection that is consistent with the others just cited. They then show that enrollee-nonenrollee differences in health status, financial access to care, and preferences for seeking medical care explain over one-half of the estimated difference.

3. Research Agenda For Analyzing Impacts on Costs to the Medicare Program

Errors in projecting FFS reimbursements will result in the Medicare program realizing cost savings that differ from the expected five percent. However, there is no evidence of systematic error in the current methodology for predicting the FFS reimbursements of nonenrollees. There is evidence, however, that because of biased selection in enrollment, the payment rate methodology may incorrectly predict what Medicare would have paid for enrollees had they remained in the FFS sector. Indeed, the studies reviewed above suggest that the Medicare program may be paying as much as 28 percent more for enrollees in the risk program than they would have if enrollees had remained in the FFS sector. However, the following shortcomings have been identified with many of the previous studies:

1. They fail to account for the possibility of regression to the mean, the statistical phenomenon under which the costs of beneficiaries with very high or very low costs in a particular year tend to approach average costs for beneficiaries over time.

2. They fail to account for the possibility that enrollees may have purchased Medigap coverage in the absence of the Medicare risk program.

3. They do not explain the relationship between favorable selection and observable measures of health status and other personal characteristics that influence the demand for medical care.

The magnitude of favorable selection reported in the studies will be overstated if the first two shortcomings are true, and our confidence in the validity of the estimates is undermined because of the third.

In Chapter III of this report, we address each of these potential shortcomings in estimating the impact that the Medicare risk program has had on Medicare's costs.

B. EVALUATING THE PROGRAM'S IMPACT ON SERVICE USE

Capitation provides Medicare risk plans with an incentive to reduce their costs, since they will realize a profit on the difference between capitation and their costs. In setting capitation at 95 percent of expected F'FS costs, it is expected that Medicare risk plans will reduce their costs by at least five percent. Indeed, for the program to be profitable to HMOs, a five percent reduction from the level of costs in the FFS sector is necessary if payment rates are accurately set at the intended 95 percent of what enrollees would have cost the Medicare program under FFS. There are two ways that HMOs can reduce their costs: by negotiating lower rates for providers under contract, or by controlling the volume and mix of services that Medicare beneficiaries use.

By negotiating rates lower than those charged by the FFS sector, HMOs will achieve lower operating costs and higher profits, increasing the likelihood that they will remain in the program. If providers do not increase rates for non-HMO patients in response to lower rates negotiated with the HMO, then a real reduction in health care costs is achieved by the lower rates. We know from case studies that some HMOs are able to negotiate favorable rates for various services. However, measuring this effect would be a difficult task--a given HMO may negotiate different reimbursement rates for each of the providers under contract. In addition, the rates may not correspond with the service categories (for example, DRG codes) used for Medicare reimbursements, making direct comparison of unit costs in the HMO and FFS sectors difficult. Hence, we do not consider this potential source of cost savings in our analysis.

We focus our analysis of cost-efficiency on the impact of HMOs on the use of services. If Medicare risk plans reduce the volume of expensive services used by beneficiaries relative to what their use would have been in the FFS sector by substituting less costly services, or eliminating unnecessary services, they will achieve lower costs and higher profits, increasing the likelihood that they will continue to participate in the program. Moreover, if HMOs can reduce service volume--without reducing the quality of care--then a real gain in efficiency is demonstrated; HMOs are achieving the same health outcomes with less resources.

1. Evidence from the Literature

A number of studies have examined the impact of HMOs on the use and cost of medical care. Table I.1 summarizes some major studies. Luft (1981) reviews a number of studies of HMO impacts on utilization conducted between 1950 - 1978, and reports that HMOs reduce hospital days by 10-40 percent for the nonaged population. This reduction was accomplished mainly through reducing the hospitalization rate and not length of stay, though some HMOs were successful at reducing average length of stay. Use of outpatient services by HMO patients varied considerably, with some HMOs--especially IPAs--experiencing higher use than FFS patients. Higher use of outpatient services is consistent with the notion that HMOs have a financial incentive to substitute less costly outpatient procedures for inpatient care. However, the concentration of higher use among IPAs may reflect a higher incidence of adverse selection (or lower incidence of favorable selection) among this model type. Luft cautions that many of the studies reviewed have a limited number of control variables (often only age and sex) for health status and health risks and, hence, the HMO impact on service use may reflect favorable selection rather than a true HMO impact.

Subsequent studies which use more complete controls for health risks also show reduced hospital use for HMO members. Manning et al. (1984) report that the HMO participating in the Health Insurance Experiment (HIE) reduced hospitalizations by 40 percent compared with FFS plans. Since individuals in the study were randomly assigned to the HMO or FFS plans, the study is often quoted as evidence of an HMO impact that does not suffer from selection bias. However, Welch et al. (1987) contest the study's claim of random assignment by noting that 29 percent of those contacted for participation in the study refused to participate. Dowd et al. (1991) note that the results in Manning et al. (1984) may be biased as a result. In their own study Dowd et al. (1991) extend the

TABLE I.1

SUMMARY OF STUDIES ANALYZING HMO IMPACTS ON UTILIZATION

Study Authors	Study Sample/Population	Principal Results
Luft (1981)	Review of studies conducted between 1950-1978.	• HMOs reduce hospital days by 10 percent - 40 percent. • Reduction mostly attributable to lower admission rate. • Outpatient use for HMO members can be higher than FFS.
Welch (1984)	1 HMO plan and 1 FFS indemnity plan. (1971-1975)	• HMO reduced costs by 31-32 percent.
Manning et al. (1984)	1 HMO and several FFS plans.	• HMO reduced hospital admissions by 40 percent.
Nelson and Brown (1989)	Medicare beneficiaries in 9 demonstration HMOs and FFS Medicare beneficiaries in the same market area.	• HMOs reduced hospital admissions by 8 percent in first 2 years of enrollment. • Impact was bigger in 2nd year (14-28 percent) • Hospital days reduced by same percentage as admissions.
Stem et al. (1989)	617 hospital patients from 1 HMO or 1 BC/BS plan.	• HMO reduced length of stay by 14 percent, controlling for risks and severity of illness index. • HMO costs were 4 percent less than FFS.
Bradbury, Colec, and Stearns (1991)	9,100 hospital patients from 10 IPAS and BC/BS plans.	• HMO reduced length of stay by 14 percent, controlling for risks and severity of illness (Medis Groups).

TABLE I.1 (continued)

Study Authors	Study Sample/Population	Principal Results
McCombs, Kasper & Riley (1990)	Medicare beneficiaries in 2 demonstration HMOs and FFS Medicare beneficiaries in the same market area.	• One HMO reduced costs and one HMO incurred losses. • Both experienced "start-up" effect. • The HMO with reduced costs realized a 38 percent cost reduction in 2nd year. • The HMO with increased costs realized an 11 percent loss in 2nd year.
Dowd et al. (1991)	Employees (and family members) enrolled in HMOs or FFS plans in the Minneapolis area.	• HMOs reduce hospital days by 30 percent. • Physician contacts are about the same for HMO and FFS.

econometric model of Lee (1983) to address biased selection, and report that HMOs in the Minneapolis area reduce hospital days by about 30 percent, but have little effect on physician visits.

Several recent studies investigating the impact of HMOs on hospital length of stay control for health risks, DRG, and commonly used measures of severity of illness (e.g., MedisGroups). Both the study of Stem et al. (1989) and Bradbury, Golec and Stearns (1991) report a 14 reduction in the average length of stay for non-Medicare HMO members. The studies are noteworthy in that they show an HMO impact on length of stay after controlling for diagnosis and severity of illness. Since HMOs supposedly reduce the hospitalization rate by certifying admissions only for those who cannot be treated in other ways, we might expect more severe cases admitted on average for the HMO population. It would not be unusual, therefore, to see longer hospital stays for HMO members. By controlling for severity of illness, the two studies just noted are able to estimate HMO impact conditional on this factor.

Relatively few studies have investigated the impact of HMOs on the service use of the Medicare population. Nelson and Brown (1989) evaluated the impact that 9 HMOs participating in the Medicare Competition Demonstrations had on hospital use. Hospitalizations of HMO enrollees in their first two years of enrollment were 8 percent lower than those of n&enrolled beneficiaries in the same market' areas. The authors report evidence of a start-up effect, i.e., the higher use of services by enrollees in their first year on enrollment. This effect is reflected in the greater HMO reductions in the hospitalization rate in the second year, which ranged from 14 to 28 percent. However, this study also had limited data to use as control variables.

McCombs, Rasper, and Riley (1990) examine the impact that two Medicare Demonstration plans had on beneficiary costs in the first two years of enrollment, and report that one plan reduced costs and the other experienced higher costs compared to FFS Medicare. Once again there is evidence of a start-up effect, since costs in the second year were lower relative to FFS for both plans.

2. Expectations for the Medicare Risk Program

The studies reviewed show that HMOs reduce costs by reducing the rate of hospitalization, and according to two recent studies, average length of stay. There are several reasons why the sizable HMO impacts on the rate of hospitalization reported in these studies may not be realized in the Medicare risk program. First, rates of hospitalization and average length of stay for Medicare beneficiaries in FFS have been trending downward over the 1980's (ProPAC, June 1991, pp. 87-88). The reasons for the decline in lengths of stay may be attributable to the introduction of the prospective payment system (PPS), which provides hospitals with an incentive to reduce length of stay, and the introduction of new technologies, procedures, and medications. New technologies may also reduce admissions by allowing more procedures to be performed on an outpatient basis. Furthermore, growing pressure from commercial insurers to reduce the hospital use of the nonelderly (e.g., by requiring pre-certification for admissions) may have contributed indirectly to the decline in use among Medicare beneficiaries, as physicians learn to use other modes of care. The decline in hospitalization rates and length of stay is evidence of efficiency gains in FFS medicine, and suggest that HMOs may not be able to achieve the same percentage reductions relative to the FFS sector that are reported in earlier studies, even if those earlier estimates were accurate.

A second reason for expecting smaller HMO impacts in this study than those cited in the literature is that the major body of empirical evidence of HMO impacts is for the non-Medicare population. HMO impacts for the Medicare population (Nelson and Brown, 1989, and McCombs, Kasper, and Riley, 1990) are not as large according to the limited evidence available.

C. OVERVIEW OF THE STUDY

In Chapter II, we describe the samples selected, and sources of data used in the study. The primary sample used for the study is a stratified random sample of approximately 6400 beneficiaries enrolled in the Medicare risk plans, and an approximately equal number of beneficiaries in the FFS sector of market areas served by the risk plans. Data on the use of Medicare-covered services, health status, insurance coverage, and personal characteristics were obtained by surveying the samples of enrollees and nonenrollees. These data were supplemented by data on Medicare reimbursements and mortality provided by HCFA.

In Chapter III, we present estimates of the impact of Medicare risk contracting on the costs to Medicare. A variety of models are used to predict what Medicare reimbursements would have been for enrollees, had they not enrolled. These estimates are compared to estimated capitation payments for sample members to determine the cost savings (or added costs) attributable to the program.

In Chapter IV we present estimates of the impact of Medicare risk contracting on the use of services. A variety of models are used to predict what enrollee use of hospital, SNF, physician, and home health services would have been in the FFS sector.

In our analysis, we examine the HMO impact on the likelihood that the service was used and the total volume used. This enables us to examine, for example, whether reduced use of hospital services is accompanied by a greater likelihood of receiving care in a skilled nursing facility (SNF) or home health services. Since HMOs can reduce the duration of use for these substitutes as well, the probability of any use as well as mean use is important for analyzing patterns of substitution.

We estimate the impact of the program on the number of hospital days, physician visits, nursing home days, visits by a home health aide, and home visits by a nurse. We then use estimates of unit costs reimbursed by Medicare for each service and our estimated HMO impacts to determine the impact of the program on medical resource costs. We also determine whether HMO effects on service use differ with the characteristics of the patient, such as frailty or presence of chronic problems, or with the characteristics of the HMO, such as model type and number of years in operation.

Our conclusions are stated in Chapter V. We also indicate what the implications of the results might be for HMOs and the program.

II. SAMPLES AND SOURCES OF DATA

The primary data source for the analyses was our survey of nearly 13,000 Medicare beneficiaries, which collected data both for this study and for studies of the effects of Medicare risk plans on satisfaction with care, access to care, and beneficiary choice. Survey data used in this study include measures of the amount of various types of services used (hospital admissions and days, nursing home admissions and days, physician visits, home health visits), health status measures, attitudinal variables that may affect service use, income and other measures of access to care, and socioeconomic characteristics. In Section A of this chapter, we explain how the sample of enrollees in Medicare risk plans and the geographically matched sample of nonenrolled beneficiaries were selected. Since the sample of enrollees was not a simple random sample, it was weighted to reflect the actual distribution of TEFRA enrollees across the 75 plans included in the analysis. Section A also discusses the effect of this weighting scheme and others used in this report on the efficiency of the estimates. In Section B, we document the completion rates for beneficiaries contacted for interviews and the percentage of interviews completed by proxy respondents. In Section C, we describe the various other sources of data that were assembled for the beneficiary survey sample.

For a limited number of analyses in this study, we used samples of Medicare beneficiaries drawn for the analysis of biased selection conducted as part of the evaluation of Medicare risk program (Hill and Brown, 1990). The method of sample selection, and sources of data used in these analyses is explained in Section D.

A. SAMPLE SELECTION

The analyses in this report were based primarily on survey data collected from a stratified random sample of 6,476 beneficiaries who were enrolled in a Medicare risk plan as of April 1, 1990 (the "enrollee sample") and a stratified random sample of 6,381 beneficiaries who did not enroll but resided in one of the 44 market areas where these risk plans were operating (the "nonenrollee sample"). The nonenrollee sample was selected to match the distribution of enrollees across ZIP codes, to ensure that service environment and regional variations in practice patterns were the same for both groups. Enrollees and nonenrollees were required to be eligible for both Part A and Part B of Medicare coverage as of March 31, 1989 or earlier, to ensure that data on hospital stays in the past year reflected the beneficiary's Medicare experience. Telephone interviews, requiring approximately 25 minutes to complete on average, were conducted between May and October 1990.

1. The Enrollee Sample

The enrollee sample was restricted to individuals who had been enrolled at least since January 1, 1990, in order to increase the likelihood that interviewees would have had some exposure to the HMO by the time of the interview. Some exposure was necessary in order to obtain valid answers to key questions about service use, satisfaction with care, and access to care, which are required for the other studies that will evaluate the Medicare risk program. The enrollee sample was also restricted to beneficiaries who were members of one of the 75 plans that contained at least 1,000 enrollees as of February 1, 1990, according to the February status report issued by HCFA's Office of Prepaid Health Care (OPHC). This restriction was imposed so that conclusions about differences across types of plans would not be distorted by the inclusion of plans that were very new or that participated at a very limited level in the risk program.

These eligibility criteria encompassed about 88 percent of the total number of beneficiaries enrolled in Medicare risk plans as of April 1, 1990, according to HCFA's Group Health Plan Organization (GHPO) file of all beneficiaries ever enrolled in Medicare plans. Thus, the sample should be representative of the great majority of enrollees for that time period. As Table II.1 shows, less than 1 percent of enrollees belonged to one of the 20 active Medicare risk plans that contained less than 1,000 members as of the preceding month; hence, this restriction had virtually no effect on our estimates. A surprisingly high proportion, 7.3 percent, of those enrolled as of April 1, 1990 had been enrolled for less than three months; that is, their coverage in the HMO became effective the

TABLE II.1

ELIGIBILITY CRITERIA FOR SAMPLE SELECTION

	Enrollees in Medicare Risk Plans as of April 1, 1990	
Eligibility for Interview	Number	Percent
Eligible	1,001,407	87.6
Ineligible	141,482	12.4
Enrolled in plan with less than 1,000 members	9,400	0.8
Enrolled after January 1990	82,914	7.3
Not continuously entitled to Medicare coverage throughout 12 months prior to survey	49,168	4.3
Total[a]	1.142889	100.0

NOTE: Sample members were selected from HCFA's April 1990 GHPO file--that is, from the set of beneficiaries who were enrolled in a Medicare risk plan as of April 1, 1990. Enrollees were classified as ineligible if they (1) were enrolled at that time in a risk plan with under 1,000 enrollees according to the February 1990 report from the OPHC or (2) had been a member of the risk plan for less than three months (that is, enrolled after January 1, 1990); or (3) had not continuously been entitled to both Part A and Part B Medicare coverage for the 12 months preceding the sample selection (that is, since March 31, 1989).

[a]The total number of enrolled beneficiaries on the GHPO file as of Aprill, 1990 differs slightly from the number recorded on the monthly reports prepared by the OPHC for this date, due to later adjustments made to the file.

first day of either February, March, or April 1990. About 4.3 percent of enrollees had been enrolled long enough in an established plan but had not been entitled to full Medicare benefits for the 12-month period preceding the start of the survey (that is, since March 31, 1989).

Eligibility rates vary substantially across HMOs (not shown), from about 54 percent to over 99 percent. Three plans had eligibility rates of 54 to 60 percent; all of the remaining plans had eligibility rates of over 77 percent. Each of these three HMOs enrolled over one-third of its members in the three months preceding the sample selection. The proportion of HMO enrollees who were not eligible for the survey because they were not covered under Medicare throughout the preceding year ranges from less than 1 percent to nearly 11 percent in one plan.

We used data from the February 1990 report prepared by the OPHC to calculate overall sample sizes and to stratify across HMOs and market areas. The samples were selected from the enrollment file maintained by HCFA's GHPO office for enrollees as of April 1, 1990, since that was the most current file that we could obtain.

Overall, 1,142,889 beneficiaries were enrolled in Medicare HMOs as of April 1, 1990, according to the GHPO file (HCFA's OPHC reports indicate a slightly smaller number). Hence, the sample of 6,476 enrollees represents approximately 0.6 percent of all enrollees at that time. Because response rates were slightly higher than anticipated, the sample size exceeded the target sample size of 6,281, which is the number of observations required to detect a 10 percent difference in the probability of hospitalization with 80 percent power.[1]

We stratified the sample by HMO in order to obtain the maximum representation of the enrollee population. In general, the target number of interviews per HMO was set equal to .00565 times the number of enrollees in the HMO as of February 1, 1990, according to the OPHC report for that date. However, in order to increase our flexibility to give equal weight either to each enrollee or to each

[1]The sample size calculations were based on the assumption that 20 percent of beneficiaries are admitted to a hospital in any given year. Detecting an effect of 2 percentage points on a binary variable with a mean of .20 in a two-tailed test at the 5 percent significance level requires samples of 6,281 cases in each group in order for the test to have 80 percent power.

HMO without major sacrifices in the statistical precision of either type of estimate, we set a minimum sample size of 40 for each HMO, and proportionately reduced the number of cases selected from the four largest HMOs in order to maintain the same total number of interviews. Thus, for half (38) of the HMOs (all those with less than about 7,100 enrollees), our target number of completed interviews was 40. For the four largest HMOs (with enrollments of about 80,000 to over 150,000), we reduced the sampling rate to about .004, yielding target sample sixes of 325 to 627 per plan (still far more than the target sample size of 237 for the fifth largest risk plan). For the 33 plans whose enrollments ranged between 7,100 and 42,000, we set the target sample size equal to 0.565 percent of the total enrollment on February 1, 1990.

In order to ensure that the desired sample sizes were achieved, we selected samples of twice the target sample size from each HMO. We then divided cases randomly into groups of 500 cases, which were released for interviewing as required until the overall target sample size was reached.

2. The Nonenrollee Sample

In selecting the nonenrollee sample, our goal was to match the distribution of the enrollee sample across market areas.. We selected somewhat larger samples of nonenrollees than enrollees in each of the 44 areas, to ensure that the desired number of completed cases were obtained; previous experience indicated that the response rate for nonenrollees was likely to be somewhat lower than for enrollees. We computed sample sixes by ascertaining the number of enrollee cases actually selected in each ZIP code (to ensure a close geographic match between enrollees and nonenrollees within market areas), and multiplying these counts by the expected response rates for enrollees and dividing by the expected response rate for nonenrollees. This procedure yielded the number of nonenrollee cases to be selected from each ZIP code.[2]

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[2]Some enrollees had address information that was clearly incorrect (for example, an address in an entirely different part of the country from where the HMO was located). Hence, we computed the percentage of enrollees in a given ZIP code by using only the set of enrollees whose listed place of residence was in one of the counties served by the Medicare plan to which the enrollee belonged. The number of sample enrollees from a given plan who resided in a given ZIP

We used HCFA's Health Insurance Master (HIMRS) file, which contains the names and addresses of beneficiaries, to select the nonenrollee sample. The HIMRS file comprises a 5 percent sample of beneficiaries. The nonenrollee interview sample was selected at random from this 5 percent file, subject to the following restrictions: (1) the beneficiary had to have been entitled continuously to Medicare Parts A and B since March 31, 1989 or earlier, and (2) the beneficiary must not have been enrolled in a Medicare risk plan at any time since April 1, 1989. These restrictions ensured that reported utilization reflected the beneficiaries' experience in the Medicare FFS sector. After we selected the nonenrollee sample, we divided it randomly into groups of 500 cases, which were then released for interviewing as required.

With this sampling plan, weighting enrollee observations to reflect their probabilities of selection (so that the sample reflected the enrollee population) or to give each Medicare plan equal representation led only to a modest loss in statistical precision. With a minimum sample size of 40, the weights required for analyses for which each plan received an equal weight were closer to 1.0 than those that would be required with simple random sampling. (The maximum weight expected from random sampling would be 12.01, but was only 2.89 in our sample.) Similarly, for the four largest plans, which were undersampled, the weights were also closer to 1.0. (The minimum weight expected from random sampling would be .10, but was .14 in our sample.)

For most analyses in this report, observations from each plan are weighted to reflect the plan's proportion of the program population. Thus, weights for enrollee observations from the oversampled plans are less than 1.0. Weights greater than 1.0 are required for the observations for the four largest plans, which were under-sampled. Once again, the loss in efficiency is very modest, since the largest value for the weights is 1.61.

As noted earlier, the number of nonenrollees selected randomly for interviews was determined so that the expected number of completed interviews of nonenrollees in any market area would be

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code was then estimated as the total number of plan enrollees in the sample multiplied by the estimated percent who resided. in that ZIP code.

equal to the expected number of completed interviews of enrollees who resided in that area. For all analyses in this report, nonenrollees were weighted to match the distribution of weighted enrollee observations by geographic area. (The proportion of weighted observations for nonenrollees from a given county matched the proportion of weighted enrollee observations from the same county.) Thus, the weight for nonenrollees in any area is approximately equal to the average weight for enrollees in that area. A full description of all the weights is presented in Appendix A.

B. SURVEY COMPLETION RATES

Between April and November 1990, MPR interviewed 6,476 enrollees in Medicare risk plans and a geographically matched sample of 6,381 nonenrolled beneficiaries. Table II.2 lists the target number of enrollee and nonenrollee interviews by plan and market area. The number of completed enrollee interviews by plan membership and the number of completed nonenrollee interviews by the geographic location of the plan are shown in the second and third columns, respectively. In general, the number of enrollees and nonenrollees who were interviewed are close to their target levels.

The total number of interviews per market area ranged from 80 to 2,641. About 20 percent of the interviewees were from the Los Angeles area, in which a total of six risk plans were in operation, including three of the four largest plans and the eleventh largest plan. Miami, Minneapolis, and Portland also had a large number of interviewees; each of these areas had four or five risk plans.

Overall, 81.6 percent of the enrollees and 72.6 percent of the nonenrollees completed the interview. The lower completion rate for nonenrollees can be attributed primarily to the larger proportion of nonenrollees who could not be contacted because they had an unlisted telephone number, no telephone, or an incorrect address; 16.6 percent of nonenrollees identified for interviews could not be contacted, compared with 12.1 percent of enrollees (see Table II.3). Thus, this factor alone accounts for about one-half of the difference in response rates. Few beneficiaries in either

TABLE II.2

THE TARGET AND ACTUAL NUMBER OF OBSERVATIONS, BY MARKET AREA AND HMO

Market Area/HMO	Target Sample Size for Each Group/I-MO	Enrollee Observations	Nonenrollee Observations	Total Observations
Los Angeles, CA	1,408	1,330	1,311	2,641
FHP	472	421		
PacifiCare of CA	367	349		
Kaiser Southern	325	317		
Partners Health Plan	135	142		
Intervalley Health Plan	48	51		
United Health Plan	61	50		
Miami, FL	905	a74	864	1,738
Humana Medical	627	617		
CAC	94	95		
AvMed Health Plan	84	70		
Health Options	52	51		
Heritage Health Plan	48	41		
Minneapolis, MN	442	506	516	1,022
Share	237	274		
Group Health	94	115		
Med Centers	71	79		
HMO Minnesota	40	38		
Portland, OR	353	386	345	731
Kaiser NW	181	192		
Secure Horizons	92	90		
Good Health Plan	40	55		
HMO of Oregon	40	49		
Seattle, WA	276	308	288	5%
Group Health Cooperative	235	266		
Network Health Plan	41	42		
Chicago, IL	291	292	260	552
Share	153	157		
Michael Reese	98	98		
Health Chicago	40	37		
Denver, CO	193	194	222	416
Kaiser CO	113	112		
HMO of Colorado	40	43		
CompreCare	40	39		
Boston, MA	171	185	178	363
Harvard Community Health Plan	51	56’		
Lahey clinic	40	47		
Bay State Health Plan	40	41		
Medical East	40	41		
New York, NY	197	177	181	358
HIP of NY	157	138		
Total Health Plan	40	39		
Phoenix, AZ/FHP of AZ	173	166	181	347

TABLE II.2 (continued)

Market Area/HMO	Target Sample Size for Each Group/HMO	Enrollee Observations	Nonenrollee Observations	Total Observations
Rochester, NY	134	148	134	282
Blue Choice	51	57		
Genesee Valley	43	48		
Preferred Care	40	43		
Philadelphia, PA	121	135	129	264
HMO PA	40	51		
Premier	81	84		
San Francisco, CA	116	108	119	227
Bay Pacific	72	74		
Bridgeway Plan for Health	44	34		
Kansas City, MO	88	99	106	205
Total Health Care of Kansas City	48	55		
Prime Health of Kansas City	40	44		
Wichita, KS	80	%	85	181
Equicore Health Can	40	53		
HMO Kansas of Wichita	40	43		
Worcester, MA/Fallon Community Health Plan	86	92	84	176
San Antonio, TX	80	80	75	155
PacificCare of TX	40	50		
Humana of TX	40	30		
Albuquerque, NM/FHP of NM	61	71	69	140
Cleveland, OH/Kaiser OH	53	60	61	121
Honolulu, ED/Kaiser HI	65	54	54	108
DesMoines, IO/Share Iowa	40	47	60	107
Dallas, TX/Kaiser of TX	40	54	51	105
Duluth, MN/HMO Minnesota	48	53	49	102
Hampshire County, MA/Kaiser	40	45	57	102
Milwaukee, WI/FHP Cooperative	40	47	53	100
Louisville, KY/Humana Health Plan	40	46	53	99
Las Vegas, NV/Health Plan Nevada	54	49	49	98
Detroit, MI/Health Alliance	46	48	49	97
Providence, RI/Ocean State Physicians	40	49	48	97
Sacramento, CA/PCA Health Plans	40	49	44	93
Bridgeport, CT/Physicians Health Plan	40	45	48	93
Corpus Christi, TX/Humana of TX	40	46	45	91
Atlanta, GA/Kaiser Foundation	40	51	39	90

TABLE II.2 (continued)

Market Area/HMO	Target Sample Size for Each Group/HMO	Enrollee Observations	Nonenrollee Observations	Total Observations
Lansing, MI/Health Central	40	51	39	90
Washington, DC/M.D.IPA	40	43	47	90
Buffalo, NY/Health Care Plan	40	43	46	89
Paramus, NJ/HIP of NJ	40	49	40	89
Pueblo, CO/Peak Health Plan	40	45	43	88
Omaha, NB/Share Health Plan	40	40	48	88
Flint, MI/Health Plus of MI	40	48	39	87
Indianapolis, IN/Metro Health Plan	40	40	47	87
Tulsa, OK/Prudential Health Care Plan	40	43	43	86
Daytona, FL/Florida Health Plan	40	44	42	86
Vineland, NJ/Omnicare	40	40	40	80
Total	6,281	6,476	6,381	12,857

TABLE It.3

RESPONSE RATES AND REASONS FOR NONRESPONSE
(Percent)

	Enrollees	Nonenrollees
Complete	81.6 %	72.6 %
Incomplete	18.4	27.4
Telephone number unavailable	12.1	16.6
Refused	4.2	7.2
Unable to respond	1.7	2.5
Never answered/telephone problems	0.4	1.1
Total Number of Interviews Attempted	7,937	8,798

NOTE: The table excludes 96 enrollees and 202 nonenrollees for whom a telephone contact was made but the individual was determined to be ineligible for interview. Individuals were ineligible if they (1) died 'prior to the sampling date (April 1, 1990), or (2) were in the enrollee sample but asserted that they were never a member of the HMO, or (3) were in the nonenrollee sample but enrolled in an HMO between the date of sample selection and the date of the interview.

sample (4.2 and 7.2 percent) refused to complete the telephone interview, perhaps because each prospective interviewee had received a letter that detailed the purpose of the survey.

Interviews were completed by either the beneficiary or a proxy respondent. Table II.4 lists the percentage of enrollee and nonenrollee interviews completed by the beneficiary or proxy. The percentage of proxy respondents was considerably lower for enrollees than nonenrollees, 9.5 versus 17.2 percent. As Table II.4 shows, about one percentage point of this difference is attributable to the higher mortality rate for nonenrollees.

Survey nonresponse is a potential source of bias, since nonresponders may systematically differ from responders in their Medicare costs and use of Medicare-covered services. To assess the degree to which our key impacts estimates may be affected by nonresponse bias, we assembled data on hospital utilization and a limited number of demographic characteristics for enrollees from several HMOs and for nonenrollees from Medicare (MADRS). The HMO data were for all enrollees--survey responders and nonresponders--selected for interview from the several HMOs. Similarly, the MADRS data were assembled for all nonenrollees selected for interview--survey responders and nonresponders. From these data were able to assess whether key impact estimates were significantly different when the estimation sample included and excluded nonresponders. The analyses, which are presented in detail in Appendix C, find no significant difference in impacts generated from samples including and excluding nonrespondents. The results indicate that the analyses conducted on the survey data (i.e., on responders) should not be biased from nonresponse.

Item nonresponse was generally low for the numerous variables we incorporated into the analysis. For some variables, such as income, nonresponse rates were much higher (e.g., approximately 10 percent for income). In order to maintain sample sizes in the analyses, we used sample means for missing values for the small number of variables where item nonresponse exceeded 3 percent. In regression analyses a dummy variable was used to denote whether a sample mean replaced a missing

TABLE II.4

PERCENTAGE OF SURVEY INTERVIEWS COMPLETED BY
SAMPLE MEMBER AND PROXY RESPONDENTS

Person Responding on the Survey	Percentage of: Enrollee Sample Members	Percentage of: Nonenrollee Sample Members
Sample Member	90.4	82.8
Proxy, Sample Member Living	8.9	15.6
Proxy, Sample Member Deceased	0.6	1.6
Total	100	100
Sample Size	6,476	6,381

value for an observation, relaxing somewhat the assumption that those who fail to respond to a particular question had the same value for that variable as did the responders.

C. OTHER SOURCES OF DATA ASSEMBLED FOR THE SURVEY SAMPLE

To complete our analysis of the survey sample we required data from several sources in addition to the beneficiary survey, which provided measures of health status, medical conditions, functional status, service use in the year prior to the interview, insurance coverage, and income. We assembled individual level data from a number of sources maintained by HCFA. The Medicare IDs of survey sample members were submitted to HCFA in data requests, and the data received were merged by Medicare ID to our survey data. Information on the use of Medicare-covered services and reimbursements in the FFS sector for calendar years 1985-1990 was obtained from the Medicare Automated Data Retrieval System (MADRS). For enrollees, data on age, sex, race, welfare status, disability status, and dates of entitlement and enrollment were obtained from the GHPO file. For nonenrollees, these same data items were obtained from the Medicare master beneficiary file (URHIMRS). Data on dates of death for beneficiaries through July 1, 1991 were obtained from the Health Insurance Printout (HIPO) file.

In addition to individual-level data, we assembled data on characteristics of the beneficiary's county from the Area Resource File (ARF, March 1989). These data included measures of population size, number of physicians, number of surgeons, and number of community hospital beds. For enrollees in our survey sample we assembled plan characteristics (premiums, benefits, and type of model) from monthly OPHC reports. The numbers of enrollees and nonenrollees in the Medicare population of the market areas included in our analyses were obtained from the AAPCC master file. These data were used in the construction of weights, discussed in Appendix A..

D. THE BIASED SELECTION SAMPLE AND DATA

For a limited number of analyses, we use samples of enrollees and nonenrollees selected for the MPR study of biased selection in the Medicare risk program. The full details of sample selection for that study are presented in Hill and Brown (1990, pp.22-29). Approximately 2,000 nonenrollees were selected for each of 48 market areas with Medicare risk plans in 1988, and data on their reimbursements for 1985 through 1987 were gathered for the biased selection study. These data are used in this study to illustrate the robustness of various statistical results, based on the samples drawn for four large market areas (Miami, Los Angeles, Minneapolis, and Chicago). Of the original samples of enrollees and nonenrollees drawn for these market areas, we selected all beneficiaries alive as of January 1, 1988 for the analyses. Medicare reimbursements for 1988 and 1989 were assembled for all nonenrollees in these samples from MADRS. Data on date of death were assembled from the HIPO file for enrollees and nonenrollees in the samples. These data were then combined with the data on 1985 and 1986 Medicare reimbursements, age, sex, disability, and Medicaid status that were assembled for the analysis conducted in Hill and Brown (1990).

III. THE IMPACT OF THE MEDICARE RISK PROGRAM ON COSTS TO MEDICARE

Medicare risk plans receive monthly capitation payments in exchange for providing all Medicare-covered services for each beneficiary enrolled. The payments are 95 percent of the estimated per capita costs for a Medicare beneficiary in the FFS sector with the same age, sex, disability status, welfare status, institutional status, and county of residence. By design, reimbursements for enrollees under this system of capitation payments should be five percent less than what they would have been had enrollees remained in the FFS sector. In this chapter we examine whether the Medicare system is realizing the anticipated five percent savings.

There are essentially only two reasons why the Medicare risk program may not save the intended five percent. The foremost is biased selection. Enrollees are a self-selected group; hence, per capita costs for beneficiaries' in the FFS sector may well differ substantially from the costs that Medicare would have incurred for enrollees had they remained in the FFS sector. The other reason for departures from the expected five percent savings is the possible inaccuracies resulting from the current method for prospectively determining rates. That is, the prospectively set rates may not accurately reflect the FFS reimbursements of even those who do not enroll. We do not assess the accuracy of the payment rates in the time periods considered in our analysis. Such an analysis would offer little insight into the program's prospects for achieving cost savings over time. Our focus in this chapter is to assess the degree of biased selection under the current system of setting payment rates. To do so, we estimate a regression model to predict what reimbursements for enrollees would have been in the FFS sector in 1989. We then compare predicted FFS costs for enrollees to their implicit payment rates (which must be estimated for our sample of beneficiaries, since our survey sample contains only individuals who were alive throughout the period) to assess the program's impact on costs.

In Section A, we present background information on the payment methodology, and the relationship between biased selection, payment rate inaccuracies and program cost savings. In Section B, we present estimates of what Medicare reimbursements would have been had enrollees remained in the FFS sector. In Section C we review estimates from alternative models for predicting FFS reimbursements. In Section D, we review the estimated payment rates for enrollees, and present the estimated impact of the program on Medicare costs. In Section E, we examine cost impacts for various subgroups of enrollees.

A. OVERVIEW OF THE PAYMENT METHODOLOGY AND DEFINITIONS OF COST IMPACTS

Medicare risk plans are paid monthly capitation payments that in principle are 95 percent of what Medicare would have paid for enrollees in the FFS sector. In designing the program, the five percent discount from FFS costs was viewed as a level of compensation that would be profitable to HMOs, given available evidence of cost savings achieved by HMOs, but would yield some'savings to HCFA after the program was established. (HCFA actuaries predicted that the program would actually increase costs in 1985 and 1986. See Federal Register, January 10, 1985, pp. 1340.)

Separate capitation payments are computed for Part A and Part B services for the aged, disabled, and end stage renal disease (ESRD) populations. The number of enrollees with ESRD is very low, and only one enrollee classified as ESRD is in our beneficiary survey sample. Hence, this group is excluded from our analysis. For aged and disabled beneficiaries, Part A and Part B capitation payments are the product of two factors:

1. The adjusted average per capita cost (AAPCC) for the county, which is 95 percent of the estimated per capita cost for beneficiaries in the enrollee's county of residence, adjusted for differences across counties in the distribution of beneficiaries across AAPCC risk cells.

2. A demographic cost factor, expressing the enrollee's expected FFS costs as a multiple or fraction of the county AAPCC based on the enrollee's age, sex, Medicaid eligibility, disability, and institutional status.

The county per capita cost is derived from the projected U.S. per capita cost (USPCC) for all Medicare beneficiaries. The county's per capita cost relative to the USPCC is calculated as the average ratio of per capita costs in the county to the average for the U.S. calculated over the five most recent years of data. This relative cost factor, the Area Geographic Adjuster (AGA) is used to inflate (deflate) the USPCC to reflect the county's costs relative to the USPCC. Medicare reimbursements to HMO enrollees in the county are netted out of per capita costs to arrive at per capita FFS costs for the county. Costs are normalized by dividing the product of the AGA and USPCC by the average risk factor for the county, so that the county payment rates do not reflect differences that will be captured by the individual-level demographic factors.

The demographic cost factors are based on beneficiary characteristics that are easily measured, and are correlated with Medicare costs. The demographic cost factor for each of the 60 actuarial categories (2 sex, by 10 age/disability categories, by 3 welfare-institutional classifications) is computed as the ratio of the average reimbursement for beneficiaries in that category to the average for all Medicare beneficiaries. Table III.1 illustrates, for Part A payments, the. 60 actuarial categories and the demographic cost factors used for the 1991 payments to Medicare risk plans. Henceforth, we refer to the 60 categories as the AAPCC risk classifications and the demographic characteristics used to define those categories (age, sex, welfare status, disabled, and institutional status) as the AAPCC risk indicators. The capitation payment for a beneficiary's Part A services is computed as the product of the beneficiary's demographic cost factor for Part A services and the Part A AAPCC for the beneficiary% county of residence. Capitation payment for Part B services are computed similarly as the product of the Part B demographic cost factor and Part B AAPCC rate.

If payment rates accurately predict 95 percent of the FFS costs for nonenrollees, and there is no biased selection, then by design, capitation payments should produce a five percent savings to Medicare. For later discussions, it is useful to express this in equation form. Let $C_f =$ the average cost to Medicare for enrollees in a particular rate cell, had they remained in the FFS sector, $C_a =$

TABLE III.1

DEMOGRAPHIC COST FACTORS FOR MEDICARE, PART A, 1991

Age Category	Male			Female		
	Institutionalized	Non-Institutionalized, Medicaid	Non-Institutionalized, Non-Medicaid	Institutionalized	Non-Institutionalized, Medicaid	Non-Institutional, Non-Medicaid
Aged						
85 and over	2.40	250	1.30	1.95	1.90	1.10
80 - 84	2.40	235	1.20	1.95	1.60	1.00
75 - 79	240	210	1.10	1.95	1.40	.80
70 - 74	2.40	1.75	.90	1.80	1.10	.70
65 - 69	1.95	1.30	.70	1.55	.85	.55
Disabled						
60-64	.60	1.85	1.00	.65	1.55	1.25
55 - 59	.90	1.55	.85	1.00	1.40	1.00
45 - 54	1.15	1.30	.70	1.20	1.20	.80
35 - 44	1.20	1.05	.55	1.35	1.20	.60
Under 35	1.60	1.00	.55	1.75	1.20	.55

SOURCE: Office of the Actuary, HCFA

the average projected cost to Medicare for beneficiaries in the FFS sector in that rate cell, and $C_a x(.95)$ = the capitation payment for the rate cell. The percentage cost savings to Medicare for enrollees in the rate cell is simply the percentage difference between C_f, what the enrollees' costs would have been in the FFS sector, and $C_a x(.95)$, the capitation payment to the enrollees' risk plan, or:

(1) Cost savings $= [C_f - C_a x(.95)]/C_f,$

$= 1 - [C_a/C_f]x(.95),$

(2) Cost savings $= 1 - (.95) = .05$, if $C_a = C_f$.

Thus, if average FFS cost (as predicted by the payment methodology), C_a, is equal to what FFS cost would have been for enrollees, C_f, then cost savings will be five percent for this category of enrollees.

Inaccuracies in projected FFS costs (C, not equal to actual average reimbursements for nonenrollees) are relatively small when averaged over time and across areas. These inaccuracies arise from errors in the components of the estimates: the USPCC, the AGA, or the demographic cost factors. Some projection error is expected in any given year, and larger errors can be expected in years in which fundamental changes in coverage or reimbursement are first implemented. Thus, an audit of the accuracy of payment rates which compares actual FFS costs for nonenrollees to projected FFS costs used for the AAPCC rates will always reveal inaccuracies. However, in assessing the impact of the program on Medicare costs, the key question is whether the errors are systematic.- In an evaluation of the payment rate methodology, Gruenberg, Pomeranz, and Porell (1988) found the basic methodology for predicting FFS costs and hence payment rates was unbiased and that the average projection errors for the USPCC were small. This is supported by estimates released by the Office of the Actuary, HCFA, showing that the average difference between the projected USPCC and actual average costs was less than 1 percent for the 6 year period from 1985 to 1990. This suggests that in any given year or area, cost savings could be greater or smaller than five percent due to inaccuracies in correctly projecting FFS costs of nonenrollees, but over time the average overall cost savings will be about 5 percent, provided that nonenrollee average reimbursements in any given rate cell are an accurate reflection of what ***enrollee*** reimbursements would have been.

If there is biased selection, however, this last condition will not be met. As we documented in Chapter I, there is considerable empirical evidence suggesting that Medicare risk plans are experiencing favorable selection. Thus, in our formulation, C_a, the FFS costs as predicted by the AAPCC risk indicators, is greater than the cost (C_f) that Medicare would have incurred had the enrollee remained in the FFS sector. As an example, Hill and Brown (1990) estimate that favorable selection resulted in FFS costs for enrollees that were 75 percent of the FFS costs of nonenrollees with the same AAPCC risks. That is, $C_f = .75$ C,, and $C_a/C_f = 1.33$. From Equation (1) cost savings as a proportion of C_f would then be -.267, or a loss of 26.7 percent to the Medicare program according to the results of Hill and Brown (1992). If $C_a/C_f = 1.00$, selection is neutral and the program saves the expected five percent. If $C_a/C_f < 1$, selection is adverse and the program saves in excess of five percent, but HMOs will have difficulty covering their costs. Thus, if projected per capita FFS costs for the county are accurate, biased selection is the sole factor determining the program's impact on costs to HCFA.

In Section B, we describe the methodology for estimating C,, the cost to Medicare for enrollees had they remained in the FFS sector, and present the results. In Section D, we present the methodology for estimating C_a, the FFS costs for enrollees as predicted by the AAPCC risk indicators, and present our estimates of the program's impact on Medicare costs, using Equation (1).

B. PREDICTING WHAT FFS COSTS WOULD HAVE BEEN FOR ENROLLEES

Previous studies of biased selection in the Medicare risk program may have overestimated the degree of favorable selection by using the ratio of enrollees' average Medicare reimbursements prior to enrollment to the average reimbursements over the same time period for nonenrollees, with the latter adjusted to account for differences between the groups on AAPCC risk factors. In Chapter

I, we noted that these estimates may overstate favorable selection because (1) enrollee FFS costs would regress toward the mean of the Medicare population, and (2) the fraction of enrollees who had Medigap probably would have increased had the HMO option not been available, resulting in somewhat higher service use for the group on average. In addition, the studies of biased selection based on prior reimbursements provide no other evidence that enrollees are healthier or would demand less care than nonenrollees with the same AAPCC risk indicators. In this study, we have a unique opportunity to address these problems because of the detailed characteristics on health status and preferences for care assembled on the beneficiary survey.

To obtain our estimates of what enrollees would have cost Medicare had they not enrolled in an HMO, we first estimate on the nonenrollee sample the relationship between Medicare costs and all observed characteristics thought to influence costs, using a regression model of the following form:

(3) $Y = b_s X_s + X_a B_a + X_o b_o$

where

Y = Medicare FFS costs for nonenrollees for 1989, including pass-through[1] and administrative[2] costs as well as reimbursements

X_s = a matrix of binary site variables indicating the site in which the sample member resides

X_a = a matrix of binary variables indicating the AAPCC risk cell into which the beneficiary falls

X_o = a matrix of other independent variables from the survey that may affect costs

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'Pass-through costs were calculated from MADRS records, and include Medicare reimbursement to hospitals for capital costs, direct and indirect medical education costs, and acquisition costs for transplanted organs.

[2]We use the same administrative loading factors as HCFA reports for processing claims. For 1989, the principal time period for our analysis, the factors were .005178 for Part A and .026494 for Part B.

b_s, b_a, b_o = vectors of regression coefficients for X_s, X_A, and X_o

e = a regression error term.

We then predict C, the FFS cost for enrollees, using the parameters (b,, b,, b,) and the observed characteristics (X_s, X_a, X_o) for ***enrollees. C,,*** the FFS costs that the AAPCC implicitly predicts for enrollees based on their AAPCC risk indicators, is estimated (as described in Section D) and cost savings are computed by Equation (1). Based on these computations, any deviation of C_a/C_f from 1.00 will reflect observed characteristics of enrollees that cause their predicted FFS costs to differ from what the AAPCC predicts their FFS costs to be. Thus, unlike biased selection based on prior FFS costs, our measure reflects observed measures of health status and other demand characteristics that influence enrollee FFS costs but are not explained by the AAPCC risks. Thus, our measure is not a "residual," with any difference not explainable by AAPCC factors being attributed to biased selection. Only enrollee-nonenrollee differences in reimbursement which can be linked to specific characteristics not captured by the AAPCC factors are considered to be due to biased selection. In addition, our results should not be distorted by regression to the mean, because health status and other characteristics are measured while the beneficiary is enrolled. That is, most of any regression toward the mean should have occurred by the time of our survey for sample members, since over 70 percent had been enrolled for 2 years or more by the time of the survey.[3] Beebe (1988) estimates that most of the regression toward the mean for a group will take place within the first two years.

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[3]Indeed, the majority of enrollees surveyed were enrolled more than 3 years at the time of interview, as the following distribution by time of enrollment shows:

Length of Enrollment:	Percent of Enrollees:
1 year or less	11.1
≥ 1 to 2 years	19.6
≥ 2 to 3 years	17.3
≥ 3 to 4 years	24.1
≥ 4 to 5 years	11.9
≥ 5 to 6 years	7.3
≥ 6 years	8.6

The independent variables in the model capture demand and supply factors influencing the level of Medicare reimbursements. Factors hypothesized to influence demand were the AAPCC risk indicators, health status, functional limitations, history of high cost illness, preferences for seeking care, income, and Medigap coverage. A number of studies have reported a significant relationship between health care expenditures and measures of self-reported health status and functional limitations (Newhouse et al., 1989; Thomas and Lichtenstein, 1986; Christensen, Long, and Rodgers, 1987; Whitmore et al., 1989; and Davies, 1989). Riley and Lubitz (1988) report that cancer, heart disease, and stroke account for nearly 70 percent of deaths among the Medicare population and are associated with reimbursements 3-6 times the Medicare average in the last year of life. From our beneficiary survey, we are able to identify whether the beneficiary had ever had at least one of these conditions. The role of income and health insurance coverage in health care demand has been documented in the Health Insurance Experiment (Manning et al., 1984), and the relationship between Medigap coverage and Medicare reimbursements have been documented by Christensen, Long, and Rogers (1987). The relationship between preferences for seeking medical care and Medicare reimbursements was recently reported by Hill and Brown (1992).

To control for geographic variations in factors that influence the level of reimbursements, such as factor input prices and prevailing patterns of medical practice, we included dummy variables identifying the beneficiary's market area.

The means and standard deviations for the independent variables (except the market area dummy variables) are listed in Table 111.2, and include the AAPCC risk indicators;[4] self-reported health

[4]Because of the small numbers of beneficiaries in some of the 60 AAPCC risk classifications, we consolidated some classifications in X_a. All institutionalized beneficiaries, who constitute five percent of the nonenrollee sample, were lumped together into a single cell. This aggregation reduced the number of AAPCC risk classification cells from 60 to 41, requiring 40 dummy variables in X_a. Disabled beneficiaries, who constitute 8 percent of the nonenrollee sample, were consolidated into one age group, versus the five groups used for the payment rates. This further reduces the number of dummy variables in X_a from 40 to 24. Similarly, we consolidated counties into market areas to define the dummy variables denoting geographic area (X_s). We found that our results were robust to changes in this specification. A simple OLS model with dummy variables (for age category, sex, reason for entitlement, institutionalization, and medicaid status) but without interaction terms generated predicted values that were almost identical to those reported in the text.

TABLE III.2

INDEPENDENT VARIABLES INCLUDED IN MODELS PREDICTING FFS COSTS
(All variables are binary except where indicated)

	Enrollees		Nonenrollees		
	Mean	Standard Deviation	Mean	Standard Deviation	Enrollee-Nonenrollee Difference
AAPCC Risk Indicators					
Disabled (under age 65)	0.028	0.164	0.077	0.267	-0.050 • **
Ages 65-69	0.227	0.419	0.217	0.412	0.010
Age IO-74	0.309	0.462	0.270	0.444	0.039 • **
Age 75-80	0.223	0.416	0.188	0.391	0.034 • **
Age 80-84	0.129	0.335	0.134	0.341	-0.005
Age ≥ 85	0.085	0.279	0.114	0.318	-0.028 • **
Male	0.442	0.497	0.417	0.493	0.025 • **
Medicaid	0.024	0.152	0.093	0.291	-0.070 • **
Nursing home resident	0.018	0.134	0.058	0.235	-0.040 • **
Health Status					
Poor health	0.058	0.233	0.095	0.289	-0.037 ***
Number of impairments on activities of daily living	0.130	0.604	0.307	0.977	-0.178 • **
Number of impairments on instrumental activities of daily living	0.673	1.379	1.101	1.811	-0.428 • **
History of cancer, heart disease, or stroke	0.216	0.445	0.329	0.466	-0.053 • **
Died within 9 months of interview date	0.046	0.209	0.054	0.226	-0.008 • *
Preferences for Seeking Care					
Worry about personal health more than others	0.178	0.378	0.211	0.397	-0.033 • **
Avoid doctor if a problem arises	0.274	0.442	0.256	0.429	0.018 • **
Have a usual place of care (prior to enrollment for enrollees)	0.850	0.354	0.914	0.280	-0.063 • **
Medigap Coverage	b	b	0.723	0.441	b
Other Personal Characteristics					
Race (percent not white)	0.079	0.268	0.069	0.250	0.010 • *
Income[a]	$17,679	$19,279	$20,148	$30,897	($2,469)***
Education					
College degree	0.120	0.322	0.155	0.355	-0.035 • **
High school graduate, no college degree	0.557	0.492	0.561	0.487	-0.004

TABLE III.2 (continued)

	Enrollees		Nonenrollees		
	Mean	standard Deviation	Mean	Standard Deviation	Enrollee-Nonenrollee Difference
Market Area Characteristics[c]					
Live in a large MSA (population of 250,000 or more)	0.810	0.393	0.810	0.393	0.000
Doctors per 1,000 people"	2.179	0.824	2182	0.832	-0.003
Surgeons per 1,000 people[a]	0.580	0.215	0.580	0.217	-0.001
Acute care hospital beds per 1,000 people[a]	4.058	1.472	4.069	1.478	-0.010
County AAPCC rate, Part A, 1989[a]	$176.46	524.38	$177.68	$24.63	($1.22)**
County AAPCC rate, Part B, 19898	$135.54	$36.03	$135.77	$35.64	($0.23)
Sample Size		6,475		6,107	

NOTE: All variables except the AAPCC risk indicators and market characteristics were obtained from the survey. With the exception of nursing home residence, the AAPCC risk indicators were obtained from the Medicare master beneficiary file (nonenrollees) and the GHPO file. Data from the survey identified nursing home residents. All market area characteristics, except county AAPCC rates, were obtained from the Area Resource File. The county AAPCC rates were obtained from the AAPCC master file.

[a]These variables are continuous; all other variables are binary measures.

[b]Individuals enrolled in an HMO do not need Medigap coverage because there are no deductibles or coinsurance for Medicare-covered services in Medicare risk plans. The likeiihood that enrollees would have bad Medigap coverage had they not been enrolled was imputed, as explained later in this chapter. About 12 percent of enrollees did report having an individual Medigap policy or similar coverage from a former employer despite belonging to an HMO.

[c]Binary variables for site, rather than these site characteristics, were included in the FFS cost model. The characteristics are presented here simply to provide some description of the market areas in which risk plans were operating and to illustrate the similarity of the means for enrollees and nonenrollees that is produced by the weighting scheme. The site characteristic means are not quite identical due to rounding and to the loss of a few observations as a result of item nonresponse.

- Significantly different from zero at the .10 level, two-tailed test.
- * Significant at the .05 level, two-tailed test.
- ** Significant at the .01 level, two-tailed test.

status, functional impairments and medical conditions; income and Medigap coverage; preferences for seeking medical care; and demographic characteristics. Note that there are statistically significant differences between enrollees and nonenrollees on virtually every variable. Enrollees are significantly less likely to report poor health, ADL or IADL impairments, and a history of cancer, heart disease or stroke; they are also less likely to worry about their health and more likely to avoid seeing a doctor if a problem arises. Hill and Brown (1992) show that these variables--in particular, measures of health status and insurance coverage--explain nearly one-half of the enrollee-nonenrollee difference in pre-enrollment FFS reimbursements. Enrollees also have less income and are less educated. We also include for illustrative purposes on Table III.2 various measures of the supply of health care services in the beneficiary's county: number of physicians per capita, surgeons per capita, and general hospital beds per capita. By design, these market area characteristics for the two samples are essentially identical.

The regression results for Equation (4) for Part A reimbursements (55 percent of total reimbursements) and Part B reimbursements (45 percent) for calendar year 1989 are listed in Table III.3. We also estimated impacts for the 12 month interval prior to the beneficiaries' interview date, and obtained very similar results. Hence, the results for that time period are not displayed here.[5]

The mean of the dependent variable for 1989 was \$1,558 for Part A costs and \$1,254 for Part B, or \$2,812 in total. The number of observations included in the regression was 6,107, with missing data on 34 cases being the primary reason for the loss of observations (recall that the sample size for nonenrollees was 6,141, after 240 ESRD cases were dropped from the analysis). Although a number of characteristics were found to have statistically significant effects on reimbursements, the overall fit of the model was fairly weak (R^2 = .06 for Part A, .11 for Part B), as is typical for models of Medicare reimbursements. The results for the 12 months preceding interview were similar.

[5]Our original intent was to estimate reimbursement models for 1990 as well. However, examination of the data for calendar year 1990 suggests that the claims data for the last quarter of 1990 may not have been complete by the date of our data request, August 1991.

TABLE III.3

REGRESSION MODEL FOR 1989 MEDICARE COSTS,
NONENROLLEES IN SURVEY SAMPLE

	Part A	Part B
Intercept	-242 (.762)	-280 (.289)
AAPCC Risk **Indicators**[a]		
Non-Institutionalized, Non-Medicaid		
Male, **disabled** (under 65)	-347 (.564)	-568 • ** (.004)
Male 65-69	a	a
Male, 70-74	221 (.575)	-45 (.727)
Male, **75-79**	258 (.562)	320 • * (.029)
Male, 80-84	930 * (.066)	654 • ** (.000)
Male, ≥ 85	685 (.283)	4 (.984)
Female, **disabled** (under 65)	8 (.990)	-15 (.946)
Female, **65-69**	-104 (.793)	-8 (.951)
Female, 70-74	163 (.663)	227 * (.067)
Female, **75-79**	-46 (.909)	157 (.239)
Female, 80-84	879 • (.051)	239 (. 107)
Female, ≥ 85	285 (.569)	-6 (.971)
Non-Institutionalized, Medicaid		
Male, disabled (under 65)	1,689 • (.063)	29 (.923)
Male, **65-69**	1,444 (.446)	2,390 • ** (.000)
Male, 70-74	658 (.602)	336 (.420)
Male, 75-79	-2,123 (.261)	406 (.514)
Male, **80-84**	4,534 ** (.048)	1,161 (.123)
Male, ≥ 85	-2,112 (.278)	-22.5 (.726)

TABLE III.3 (continued)

	Part A	Part B
Female, disabled (under 65)	607 (.568)	206 (.555)
Female, 65-69	(.423)	37 (.917)
Female, 70-74	406 (.668)	678 •* (.030)
Female, 75-79	87 (.934)	57 (.868)
Female, **80-84**	-237 (.830)	196 (.589)
Female, ≥ 85	71 (.949)	113 (.759)
Nursing Home Resident	1,168 •* (.043)	-156 (.412)
Socioeconomic/Demographic Variable		
Minority race (not white)	223 (.557)	-114 (.359)
Race information Missing	1,830 • (.070)	149 (.653)
Education		
College	-84 (.783)	153 (.128)
High school graduate, no college degree	-76 (.731)	75 (.299)
Education data missing	504 (.390)	-216 (.263)
Access to Care		
Income ($1,000)	-1 (.659)	-1 (.214)
Income data missing	-120 (.658)	-34 (.705)
Medigap coverage	123 (.594)	169 • * (.027)
Health Status		
Number of ADL impairments	430 • .* (.002)	253 • ** (.000)
Number of IADL impairments	105 (.187)	96 • ** (.000)
Poor health	774 • * (.024)	351 ••• (.002)
Ever had cancer, heart disease, or stroke	1,183 • *' (.000)	527 • *' (.000)
Died during 9 months following **interview**	1,532 • ** (.001)	648 • '* (.000)

TABLE III.3 (continued)

	Part A	Part B
Missing data poor health	1,376 (.122)	1,704 • ** (.000)
Missing data, cancer, heart disease, stroke	2,216 * (.051)	-338 (.364)
Preferences for Seeking Care		
Avoid doctors	-300 (.158)	-295 • ** (.000)
Missing data, avoid doctors	146 (.825)	-87 (.687)
Worry about health	840 *** (.000)	561 • '* (.000)
Missing data, worry about health	365 (.465)	-224 (.174)
usual place of care	621 * (.062)	591 • ** (.000)
Market Area Dummy Variables[b]		
Worchester, MA	-320 (.745)	-6 (.985)
Hampshire City, MA	-829 (.715)	-380 (.612)
Rochester, NY	-7.54 (.401)	-327 (.268)
Washington, DC	272 (.916)	277 (.745)
Philadelphia, PA	352 (.716)	433 (.173)
Miami, FL	-441 (.515)	606 *** (.007)
Chicago, IL	-29 (.969)	169 (.505)
Indianapolis, IN	1,298 (.326)	290 (.506)
Flint, MI .	-254 (.862)	-109 (.821)
Lansing, MI	577 (.735)	236 (.674)
Minneapolis, MN	-169 (.816)	-232 (332)
Cleveland, OH	-94 (.933)	86 (.818)
Duluth, M-N	404 (.745)	-515 (.208)
Albuquerque, NM	-450 (.686)	118 (.748)
Wichita, KS	-649 (.649)	34 (.943)

TABLE III.3 (continued)

	Part A	Part B
Denver, CO	-456 (.595)	-130 (.645)
San Francisco, CA	184 (.841)	34 (.911)
Honolulu, HI	145 (.898)	10 (.979)
Los Angeles, CA	136 (.838)	491 • * (.025)
Portland, OR	-49 (.948)	-104 (.674)
Bridgeport, CT	-216 (.867)	-128 (.763)
Vineland, NJ	-997 (.625)	-248 (.711)
Paramus, NJ	-931 (.676)	-203 (.782)
New York, NY	90 (.913)	277 (.310)
Buffalo, NY	260 (.867)	-139 (.784)
Daytona, FL	-556 (.661)	255 (.541)
Detroit, MI	-314 (.795)	-205 (.605)
Milwaukee, WI	-433 (.772)	-2 (.997)
Corpus Christi, TX	464 (.766)	86 (.866)
Dallas, TX	-488 (.817)	246 (.723)
Des Moines, IA	-573 (.758)	-328 (.592)
Omaha, NE	-303 (.851)	-135 (.800)
Pueblo, CO	562 (.668)	-202 (.640)
Phoenix, AZ	-142 (.866)	375 (.175)
Las Vegas, NV	-273 (.813)	387 (.308)
Seattle, WA	191 (.805)	119 (.640)
Atlanta, GA	-1,185 (.669)	-539 (.555)
Louisville, KY	-352 (.885)	-159 (.843)

TABLE III.3 (continued)

	Part A	Part B
Kansas City, MO	-110 (.919)	-76 (832)
Tulsa, OK	-748 (.782)	-397 (.655)
Providence, RI	-788 (.743)	-287 (.717)
San Antonio, TX	-1,490 (.203)	-374 (333)
Sacramento, CA	-371 (.864)	186 (.795)
Mean of Dependent Variable	$1,558	$1,254
R^2	0.058	0.110
N	6,107	6,107

NOTE: Number in parentheses are p-values for 2-tailed tests of the hypothesis that the coefficient is zero for the population. Thus, values below .05 indicate that the effect of the variable on reimbursements is significantly different from zero at the .05 level.

[a]The reference category for the set of bii variables indicating the AAPCC risk classification categories in males age 6569. Thus, all of the coefficients on AAPCC risk indicators are expected costs relative to this reference group.

‘The excluded site is Boston, MA. Hence, the coefficient on a particular binary site variable indicates the expected difference in reimbursements between that site and Boston.

- Significantly different from zero at the .10 level, two-tailed teat.
- *Significantly different from zero at the .05 level, two-tailed teat.
- **Significantly different from zero at the .01 level, two-tailed teat.

Because health status, other indicators of frailty, and mortality are controlled for, we find that the AAPCC risk indicators do not have the anticipated effect on nonenrollee reimbursement in some instances. For example, very few of the coefficients on age are statistically significant and they do not have the expected monotonically increasing patterns (the reference category is males age 65-69). Such anomalies are not without precedent in the literature. For example, Christensen, Long, and Rodgers (1987), using a model similar to ours to control for self reported health status and functional limitations, find that females 85 and older are less likely to be hospitalized than females 75-84 years of age. They also find no significant difference between females age 85 and older and females age 66-69 in the likelihood of hospitalization. Whitmore et al., 1989 also report a negative relationship between age and Medicare reimbursements, after controlling for health status and functional limitations. Nursing home residents (our measure of institutional status) have significantly higher Part A reimbursements in 1989 than the reference group (males age 65-69), but do not differ on Part B reimbursements. Nursing home residents are typically at high risk of death and as a group have average reimbursements about 2.5 times greater than the average for the Medicare population. However, since we control for mortality, as well as health and functional status, the estimated effect of nursing home residence is somewhat smaller (though still sizable).

The several measures of health status in the model each have large and significant effects in the expected direction on both Part A and Part B reimbursements. These measures include number of ADL impairments, number of IADL impairments, poor health status, history of serious illness (cancer, heart disease, or stroke), and whether the beneficiary died in the nine month period after interview.

Preferences for seeking medical care also have a significant effect on reimbursements. For example, other things being equal, beneficiaries who avoid seeing a doctor when a health problem arises had 19 percent lower Part A reimbursements and 24 percent lower Part B reimbursements than the overall averages for the sample. Having a tendency to worry about one's health more than most people their age increased Part A reimbursements in 1989 by 54 percent relative to the overall average and Part B reimbursements by 45 percent. Having a usual place of care--which reflects the desire to seek care as well as the need for care--increased Part A reimbursements by 40 percent and Part B reimbursements by 47 percent. Thus, preferences for seeking care, a factor not available in most analyses of health care utilization and cost, have a large effect on FFS reimbursements.

As expected, Medigap coverage has a positive effect on the level of reimbursements. The effect is significant only for Part B reimbursements, but the estimate is in the range reported in other studies. This differential impact of Medigap on Part A and Part B reimbursements has been noted by Christensen, Long, and Rodgers (1987). Beyond the payment of the hospital deductible, most beneficiaries hospitalized will face no Part A coinsurance, and hence, Medigap coverage will have little effect on Part A services used. And since most Medicare hospital admissions are not discretionary, few beneficiaries will fail to enter a hospital because of inability to pay the deductible. Most beneficiaries will face Part B copayments; hence, Medigap coverage is likely to induce higher demand for Part B services. According to our model, that higher demand increases Part B reimbursements by 13 percent relative to the overall mean in 1989. The coefficients on the other measure of financial access to care, income, are very small and not statistically significant in our models.

The market area dummy variables were not significantly related to the level of reimbursements with two exceptions: Part B reimbursements were significantly greater in Miami and Los Angeles than in the Boston area (our excluded market area in the regression). This result is due to the much larger sample size-s in these two areas, which have much larger Medicare risk enrollment than other areas. The coefficients on the site dummies are not important for our analysis, but it is important that these variables be retained in the model since we wish to include as regressors all variables that determine the AAPCC rate.

Three major conclusions can be drawn from the regression results. First, measures of health status, functional impairments, medical conditions, and mortality have consistently large and significant effects on FFS costs. Second, Medigap coverage has a sizable and significant positive effect on Part B Medicare costs. Third, personal preferences for seeking care--characteristics not considered in most analyses of health care use and cost--have a major influence on costs.

1. Predicting FFS Reimbursements

To predict what enrollees in the survey sample would have cost Medicare had they remained in the FFS sector, we use the parameter estimates from Table III.3 and, with two exceptions, enrollee values for the independent variables in the model. The two exceptions are Medigap coverage and having a usual source of care. Most enrollees do not have a Medigap policy[6], since the coverage would be redundant. However, a sizable fraction of enrollees (about 32 percent) have had coverage in the past, and the decision to enroll in a Medicare risk plan indicates the beneficiaries' desire to extend coverage beyond that provided in FFS Medicare. What we would like to know when predicting FFS costs for enrollees is the coverage they would elect if they were receiving care in the FFS sector. To answer this question, we used the nonenrollee sample to estimate a probit model of insurance coverage, and then predicted the probability that each enrollee would purchase a Medigap policy, given his/her personal characteristics. Similarly, enrollees were asked whether they had a usual source of care before they joined the HMO, which was over three years prior to the date of interview for over half the enrollees. Since we are interested in whether enrollees would have had a usual source of care had they been in FFS, we estimate a model on nonenrollees for the probability of having a usual place of care and then use this to impute a probability for each enrollee.

Given that the factors that influence health care needs and use are likely to also influence the demand for supplemental coverage and attachment to a particular provider, we use the same set of

[6]"Medigap policies" refers both to individual policies officially classified as Medigap and to employer-provided Medicare supplemental coverage.

characteristics from Table III.3 to predict Medigap coverage and usual source of care.[1] Table III.4 reports the results of the two probit models. The likelihood of having Medigap coverage is affected most heavily by beneficiaries' reason for entitlement and Medicaid eligibility, but is also strongly influenced by beneficiaries' age, sex, health status, preferences for care, race, and education. Most of the AAPCC risk factors have a significant effect on Medigap coverage--beneficiaries age 70 to 84 are more likely to have coverage than those over 85 or those 65 to 69 years old. Although this pattern seems odd, it is consistent with the findings of Christensen, Long, and Rodgers (1987). Being disabled or on Medicaid have the largest effects of any characteristic on Medigap coverage; beneficiaries with either condition are far less likely to have Medigap coverage than those not disabled or on Medicaid.

The relationship between health/functional status and Medigap coverage is mixed. On the one hand, those with more IADL impairments are *less* likely to have Medigap coverage than those with fewer impairments. However, those with a history of serious illness are significantly ***more*** likely to have Medigap. It may be the case that those with impairments sought a Medigap policy but were denied coverage. One would expect the same denial to have occurred for those with serious illness in their medical history, but they may have purchased the policy prior to the illness.

Preferences for seeking care have the expected relationships to Medigap. Those who say they avoid seeing physicians whenever possible are less likely to have Medigap coverage, whereas those with a usual place they go for care are much *more* likely to have Medigap than those with weaker ties to a physician. However, worrying more than others about health does not seem to lead to higher rates of Medigap coverage (although the worry about health for some could be due, in part, to the fact that they do not have coverage).

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[1] However, characteristics that determine the AAPCC--age, sex, reason for entitlement, nursing home residence and Medicaid coverage--are entered as separate variables in the model to facilitate interpretation.

TABLE III.4

PROBIT RESULTS: USUAL, PLACE OF CARE AND MEDIGAP COVERAGE

Independent Variables	Medigap Coverage Coefficient[a]	Medigap Coverage Effect of unit Change on Probability[b]	Usual Place of care Coefficient[a]	Usual Place of care Effect of Unit Change on Probability[c]
Intercept	-0.0113 (.944)		1.5054 (.000)	--
MPCC Risk Indicators				
Disabled (under 65)	-0.8340 • ** (.000)	-0.279	-0.0051 (.962)	-0.001
Age 70 - 74	0.1794 • ** (.001)	0.060	0.1475 • * (.042)	0.023
Age 75 - 79	0.1885 • ** (.002)	0.063	0.0475 (.540)	0.007
Age 80 -84	0.1561 • * (.022)	0.052	0.2035 • ' (.025)	0.032
Age 85 and above	0.0406 (.594)	0.014	-0.0656 (.486)	-0.010
Medicaid By-In	-1.2959 • ** (.000)	-0.434	-0.0231 (.795)	-0.004
Institutionalized	-0.1345 (.212)	-0.045	-0.9858 • ** (.000)	-0.155
Sex (Male)	-0.1244 • ** (.002)	-0.042	-0.1536 • ** (.003)	-0.024
Socioeconomic/Demographic				
Minority race (not white)	-0.4497 • ** (.000)	-0.151	0.0240 (.820)	0.004
Race information missing	-0.0521 (.824)	-0.017	0.2595 (.394)	0.041
Education:				
College	0.2746 • *' (.000)	0.092	0.0763 (374)	0.012
High school graduate, no college degree	0.2563 • '* (.000)	0.086	0.0033 (.956)	0.001
Education data missing	-0.1543 (.222)	-0.052	-0.0420 (.770)	-0.007
Access to Care				
Income ($1,000)	0.0096 • ** (.000)	0.003	-0.0004 (.613)	0.000
Income data missing	-0.0041 (.945)	-0.001	-0.2051 • ** (.004)	-0.032

TABLE III.4 (continued)

Independent Variables	Medigap Coverage Coefficient[a]	Medigap Coverage Effect of unit Change on Probability[b]	Usual Place of Care Coefficient[a]	Usual Place of Care Effect of unit Change on Probability[c]
Health Status				
ADL impairments	-0.0326 (.271)	-0.011	-0.1821 • ** (.000)	-0.029
IADL impairments	-0.0646 • ** (.000)	-0.022	0.0483 • * (.032)	0.008
Poor health	0.0524 (467)	0.018	-0.0626 (.505)	-0.010
Cancer, heart disease., or stroke	0.1538 • ** (.000)	0.052	0.3541 *** (.000)	0.056
Died during 9 months following interview	-0.0624 (.526)	-0.021	0.0994 (.437)	0.016
Missing data, poor health	-0.0952 (649)	-0.032	-0.0183 (.940)	-0.003
Missing data, cancer, heart disease, or stroke	0.0218 (.941)	0.007	0.5704 (.107)	0.090
Preferences for Seeking Care				
Avoid doctors	-0.1133 • * (.012)	-0.038	-0.4556 • ** (.000)	-0.072
Missing data, avoids doctors	-0.2715 • (.060)	-0.091	-0.2727 • (.072)	-0.043
Worry about health	-0.0045 (.930)	-0.001	0.3595 *** (.000)	0.057
Missing data, worry about health	-0.0458 (.671)	-0.015	-0.1054 (.403)	-0.017
Usual Place. of Care	0.3750 • ** (.000)	0.126	--	--
Market Area Dummy Variables				
Worchester, MA	0.6932 • ** (.002)	0.232	0.1288 (642)	0.020
Hampshire City, MA	0.1855 (.707)	0.062	-0.2748 (.625)	-0.043
Rochester, NY	0.0959 (.612)	0.032	0.3330 (245)	0.052
Washington, DC	0.0997 (.862)	0.033	0.2490 (.760)	0.039
Philadelphia, PA	0.0891 (.661)	0.030	-0.1800 (485)	-0.028
Miami, FL	0.2085 (.144)	0.070	0.0953 (.619)	0.015
Chicago, IL	0.4166 ** (.011)	0.140	0.0271 (.900)	0.004

TABLE III.4 (continued)

Independent Variables	Medigap Coverage Coefficient[a]	Medigap Coverage Effect of Unit Change on Probability[b]	Usual Place of Care Coefficient[a]	Usual Place of Care Effect of Unit Change on Probability[b]
Indianapolis, IN	0.3120 (.277)	0.104	-0.1872 (.594)	-0.029
Flint, MI	0.6003 • (.064)	0.201	0.0736 (865)	0.012
Lansing, MI	0.4283 (.278)	0.141	0.0811 (865)	0.013
Minneapolis, MN	0.0674 (.659)	0.023	0.0828 (.687)	0.013
Cleveland, OH	0.1505 (.525)	0.050	-0.1631 (.583)	-0.026
Duluth, MN	-0.0649 (-7%)	-0.022	0.1699 (.629)	0.027
Albuquerque, NM	0.1681 (488)	0.056	-0.2019 (.491)	-0.032
Wichita, KS	0.8546 • * (.017)	0.286	0.8845 (.168)	0.139
Denver, CO	0.2652 (.152)	0.089	0.0689 (.775)	0.011
San Francisco, CA	-0.0094 (.961)	-0.003	-0.1963 (.428)	-0.031
Honolulu, HI	0.4129 • (.091)	0.138	-0.2201 (.453)	-0.035
Los Angeles, CA	0.1455 (.299)	0.049	-0.0585 (.754)	-0.009
Portland, OR	0.3115 • (.052)	0.104	-0.0987 (.635)	-0.016
Bridgeport, CT	0.3163 (.258)	0.106	0.1822 (.630)	0.029
Viieland, NJ	0.0205 (.961)	0.007	-0.4906 (-293)	-0.077
Paramus, NJ	-0.0542 (.904)	-0.018	-0.0277 (.964)	-0.004
New York, NY	-0.0126 (.941)	-0.004	0.0255 (.912)	0.004
Buffalo, NY	0.5280 (.139)	0.177	-0.0319 (.941)	-0.005
Daytona, FL	0.3583 (.201)	0.120	-0.0025 (994)	0.000
Detroit, MI	0.0790 (.751)	0.026	0.5864 (.205)	0.092
Milwaukee, WI	0.2043 (.520)	0.068	-0.1849 (.623)	-0.029
Corpus Christi, TX	0.0238 (.945)	0.008	-0.0018 (997)	0.000

TABLE III.4 (continued)

	Medigap Coverage		Usual Place of Care	
Independent Variables	Coefficient[a]	Effect of Unit Change on Probability[b]	Coefficient[a]	Effect of Unit Change on Probability[c]
Dallas, TX	0.1771 (.707)	0.059	-0.2253 (.662)	-0.035
Des Moines, IA	0.3382 (.425)	0.113	0.0387 (.941)	0.006
Omaha, NE	0.5391 (.158)	0.181	0.2678 (.610)	0.042
Pueblo, CO	-0.0430 (.872)	-0.014	03755 (.391)	0.059
Phoenix, AZ	0.1145 (.516)	0.038	-0.0151 (.949)	-0.002
Los Vegas, NV	0.0967 (.693)	0.032	-0.2116 (.473)	-0.033
Seattle, WA	0.3589 • * (.032)	0.120	-0.0234 (.914)	-0.004
Atlanta, GA	0.3601 (.575)	0.121	-0.3827 (.574)	-0.060
Louisville, KY	0.4947 (.371)	0.166	0.7037 (.515)	0.111
Kansas City, MO	0.3756 (.122)	0.126	0.0705 (.821)	0.011
Tulsa, OK	0.3575 (.555)	0.120	-0.0556 (.940)	-0.009
Providence, RI	-0.2905 (537)	-0.097	-0.0447 (.946)	-0.007
San Antonio, TX	-0.2999 (.215)	-0.100	-0.3182 (.286)	-0.050
Sacramento, CA	03830 (.458)	0.128	1.0261 (.330)	0.161
Mean of Dependent Variable	0 . 7 2 2 9		0.9137	
N	5,923		6,087	

‘Estimated by maximum likelihood probit. Probit coefficients and their p-values (in parentheses) are reported here.

[b]Effects of a one-unit change in an independent variable on the probability of having Medigap coverage are approximately one-third the size of the coefficient on average for a dependent variable with a mean of .72.

[c]Effects of a one-unit change in independent variable on the probability of having a usual place of care are approximately one-sixth the size of the coefficient on average for a dependent variable with a mean of .91.

- • Significant at .10 level, two-tailed test.
- • * Significant at .05 level, two-tailed teat.
- • ** Significant at .01 level, two-tailed teat.

Having Medigap coverage is more common among those with higher incomes, among whites and among those with at least a high school education than among beneficiaries without these traits. These effects are sizeable as well as being statistically significant, and are consistent with expectations and prior research.

The parameter estimates from Table III.4 were used to predict each enrollee's probability of Medigap coverage. The mean of the predicted probabilities, .76, is slightly higher than the proportion of nonenrollees with coverage, .72. This result is intuitively appealing. All enrollees have expressed a desire for more extensive coverage by joining an HMO. Hence, we should not be surprised that the predicted fraction of enrollees with coverage is greater than the proportion of nonenrollees with coverage.

The principal reason that the predicted proportion of Medigap coverage for enrollees is higher than the observed rate for nonenrollees, however, is that much lower proportions of enrollees are disabled or on Medicaid. As noted, these characteristics have the biggest negative effects on Medigap coverage. Thus, because the proportion on welfare (Medicaid) is 7 percentage points lower for enrollees than nonenrollees (see Table III.2), and the proportion disabled is 5 percentage points lower, the probit index will be higher by about .13 for enrollees than for otherwise similar nonenrollees. This difference in the probit index implies that the predicted probability of having Medigap will be about 4 percentage points higher for the enrollees (for those with probabilities near the overall mean), exactly equal to the difference between the predicted mean probability of coverage for enrollees and the actual proportion for nonenrollees.[8]

Using the same approach to predict the proportion of enrollees who would have had a usual place that they went for care had they been in the FFS sector yields an estimate (.92) that is

[8]The higher predicted probability of Medicare coverage for enrollees than nonenrollees does not imply that the risk program fails to increase access to care. About 22 percent of the enrollees would have had neither Medigap nor Medicaid coverage for Medicare's deductibles and coinsurance had they not enrolled in a risk plan. For these individuals the risk program substantially improved access to care. The proportion of nonenrollees lacking either Medigap or Medicaid coverage was about 19 percent.

substantially larger than the proportion who had a usual source of care prior to enrollment (.85), and slightly larger than the proportion of nonenrollees with such coverage (.91). It is not surprising that the proportion of enrollees predicted to have a usual source of care increases over the pre-enrollment level, because some who had no regular source of care prior to enrollment would have established one in the FFS sector during the past several years had they not joined the HMO. Most of the enrollees had been in the HMO for 3 years or more by the time of the interview, and it is likely that during that time many would have established a relationship with a particular FFS physician had they not been in the HMO. Furthermore, the fact that they joined the HMO is some indication that they wanted or needed a regular source of care. On the other hand, it is perhaps somewhat surprising that the proportion predicted to have a usual source of care is higher (though only slightly) than the proportion of nonenrollees with a usual source. Enrollees are healthier, less worried about their health, and more likely to avoid going to the doctor than nonenrollees--all characteristics that one would associated with being less likely to have established a usual place of care.

Examination of the coefficients in Table III.4 shows that although our expectations are borne out by the model in most cases, these negative influences on the probability of having a usual source of care are outweighed by the statistically significant positive effects of two indicators of frailty--number of impairment on activities of daily living and being in a nursing home. Enrollees are more likely to avoid doctors, which lowers the probability of having a usual source of care. Worry about health, a history of serious illness, and impairments on IADL activities all significantly increase the likelihood that a beneficiary will have a usual place to which they go for care on a FFS basis, and enrollees are less likely to exhibit these traits. However, being in a nursing home and having impairments on ADL activities significantly decrease the likelihood that the beneficiary has a usual place of care. Since enrollees are less likely to exhibit these characteristics, they are projected to be more likely than nonenrollees to have had a usual source of care.

While these findings appear to be somewhat anomalous, examination of the coefficients in the Part A and Part B reimbursement equations and further inspection of the survey data suggest a partial explanation. We find that although nursing home residents have very high Part A reimbursements, their Part B reimbursements are not significantly higher than those who are not in nursing homes (the coefficient is negative, in fact). Thus, the results suggest that nursing home residents are not especially likely to use physician services; furthermore, nursing home residents may be unlikely to specify a usual place they go for care because they receive much of their care from the nursing home itself. The finding for ADL is still counterintuitive, however, since having more ADL impairments significantly increases both Part A and Part B reimbursements. Thus, these beneficiaries are seeing physicians more often than those without ADL impairments, but claim not to have a usual place that they visit for their care. Since one-fourth of those without a usual place indicated that they were bedbound and did not go out for medical care, these individuals may have received care in their homes or received physicians' services only when they went to the hospital.'

Using alternative estimates for the proportion of enrollees that would have had Medigap coverage and the proportion that would have had a usual source of care produces a range of estimates of what FFS costs would have been for enrollees. Table III.5 displays the estimated FFS reimbursements using our best estimates of these proportions, along with estimated reimbursements for lower bound and upper bound estimates of the proportions. The lower bound estimate for Medigap coverage for enrollees is 64 percent. This estimate was obtained by Brown et al. (1986) for

[9]It is possible that these beneficiaries have so many problems that they see many different physicians and therefore responded "no" when asked whether there was "a doctor's office, clinic, or health center where they *usually* went for health care." However, when those who reported that they had no usual places for care were asked why that was so, very few (less than half of one percent of the sample) indicated that seeing many doctors was the reason for not having a usual source.

TABLE III.5

PREDICTED MEDICARE COSTS FOR ENROLLEES HAD THEY BEEN IN THE FFS SECTOR, 1989

	Assumed Proportion of Enrollees with Usual Place of Care		
Assumed Proportion with Medigap Coverage	Lower Bound[a] (85.0%)	Predicted Value[b] (92.4%)	Upper Bound (100%)
Lower Bound[c] (64.0%)	$2,219 (.79)	$2,308 (.82)	$2,400 (.85)
Predicted Value[d] (76.1%)	$2,254 (.80)	$2,344 (.83)	$2,436 (.87)
Upper Bound (100%)	$2,324 (.83)	$2,414 (.86)	$2,506 (.89)

NOTE: Predicted Medicare costs are obtained by inserting enrollee mean values for the explanatory variables (X_s, X_a, X_o) into the regression models for Part A and Part B that were estimated on the nonenrollee sample (see Table III.3). For Medigap coverage and usual place of care, the three values shown in the table were inserted in place of the means for these variables to obtain the predicted Medicare cost. Numbers in parentheses below the costs are the ratio of predicted cost to average (unadjusted) Medicare costs for the nonenrollee sample, $2,812.

[a]The lower bound is the proportion of enrollees who said they had a usual source of care prior to enrolling.

[b]The predicted value was estimated by using the probit model of usual source of care estimated on nonenrollees (Table III.4) to predict the probability that each enrollee would have had a usual source of care. The mean of these predicted probabilities was then used as the predicted proportion.

[c]The lower bound was obtained from Brown (1986), based on a survey of enrollees in Medicare demonstration risk plans who had all been covered under FFS Medicare for at least two years prior to enrolling. Enrollees were asked soon after joining the HMO whether they had had Medigap coverage prior to enrolling in the HMO.

[d]As described in Footnote b, predicted values were obtained for enrollees from the estimated model for Medigap coverage, presented in Table III.4.

a sample of enrollees who joined Medicare demonstration risk plans but had been covered under FFS Medicare for at least 2 years prior to enrolling. The upper bound is that all enrollees would have purchased Medigap. For usual source of care the lower bound is the proportion that had a usual source prior to enrolling (85 percent); the upper bound is that everyone would have had a usual source.

Using our best estimates of the two proportions we find that reimbursements for enrollees would have been $2,344, which is about 17 percent less than the actual (unadjusted) mean reimbursement for nonenrollees ($2,812). However, the estimates range from $2,219, to $2,506, or 79 to 89 percent of the nonenrollee mean. The assumption about usual source of care makes somewhat more difference than the Medigap variable because it has such a large effect on costs (those with a usual source of care in the FFS sector have reimbursements that are 43 percent ($1,212) above the overall mean reimbursements for nonenrollees (40 percent for Part A, 47 percent for Part B). The effects of Medigap coverage on reimbursements are much more modest. Note that even if all enrollees would have had Medigap coverage and a regular source of care, however, reimbursements for enrollees would still have been 11 percent below the mean for nonenrollees because of the enrollee-nonenrollee differences on other characteristics.

The ratio of .83 for the predicted value for enrollees relative to the unadjusted mean for nonenrollees is somewhat greater than earlier estimates of the ratio of enrollee to nonenrollee means from biased selection studies. Prior to adjusting for differences between the groups in AAPCC risk factors, Brown (1988) finds a value of .69 for the ratio of reimbursements prior to enrollment for enrollees in 17 demonstration risk plans to the mean reimbursement for nonenrollees in the same market areas. Hill and Brown (1990) find a ratio of .75 using the same procedures on samples of recent enrollees from 98 Medicare risk plans in 1988.

There are three reasons for the higher estimate in our current analysis. First, the sample we are currently using is a cross section of all enrollees, not first-year enrollees. Thus, due to regression toward the mean, differences between enrollees and nonenrollees on health status and other characteristics have narrowed relative to the differences observed between new enrollees and the stock of nonenrollees. Second, the age distribution of the stock of enrollees more closely resembles that of the stock of nonenrollees than does the age distribution of enrollees at the time of enrollment. Third, the proportion of enrollees estimated to have Medigap coverage had they not enrolled (76 percent) is somewhat greater than the proportion of new entrants that have Medigap, even among those who spent at least two years in the fee-for-service sector prior to joining the HMO.

The ratio that we wish to estimate, as indicated in Section A, is the ratio of what enrollees would have cost Medicare to what ***nonenrollees with the same distribution on* AAPCC *risk indicators actually cost.*** Before estimating the denominator of this fraction, we first explore alternative methods of estimating the numerator, what the enrollees would have cost.

C. ALTERNATIVE MODELS TO PREDICT FFS COSTS

In this section, we explore the efficacy of two alternative models for predicting costs: the sample-selection bias model of Heckman (1978, 1979), and the two-part model of Duan et al. (1983). In simulations testing model accuracy, we find that neither alternative performs as well as the basic OLS model.

1. The Sample Selection Bias Model of Heckman

Under certain conditions, estimating Equation (4)--our ordinary least squares (OLS) regression model to predict enrollee FFS costs--will produce biased estimates of. the regression parameters, b,, b,, and b_o, which for convenience we will refer to jointly as the vector B in this section. To understand the potential for bias, note that since we can only estimate Equation (3) on the nonenrollee sample (Y_i is unobserved for enrollees), we are estimating the following conditional expectation function:

(4) $E(Y_i|I_i = 0) = \mathbf{B'X_i} + E(e_i|I_i = 0)$,

where $I_i = 1$ if the beneficiary is enrolled and $I_i = 0$ if the beneficiary is in the FFS sector. If the error term, e_i, does not vary systematically by enrollment status then $E(e_i|I_i = 0) = 0$. In this instance, regressing observed Y_i on $\mathbf{X_i}$ produces unbiased estimates for B. However, if unobserved characteristics influencing the beneficiary's choice of HMO versus FFS sector also influence Y_i, then $E(e_i|I_i = 0) \neq 0$. In this instance, regressing Y_i on $\mathbf{X_i}$ will produce biased estimates of the B's for enrollees, which in turn will yield misleading estimates of what FFS costs would have been for enrollees.

Heckman (1978, 1979) proposes a two-step estimator, which we use to test and correct for the bias when $E(e_i|I_i = 0) \neq 0$. In the first step, the probability that a beneficiary chooses the FFS sector, given observed characteristics $\mathbf{Z_i}$, is estimated by maximum likelihood probit. The list of variables that we include in Z, are the same as those reported in Hill and Brown (1992): the AAPCC risk indicators, measures of health and functional status, income, the lowest premium for a Medicare risk plan in the market area, and the number of Medicare risk plans in the market area. Under the assumption that the error terms from Equation (3) and the probit equation are correlated (with covariance = σ_{12}) and distributed bivariate normal, then

(5) $E(e_i|I_i = 0) = -(\sigma_{12}/\sigma_2)f(\mathbf{D'Z_i})/(1-F(\mathbf{D'Z_i}))$,

$= -(\sigma_{12}/\sigma_2)\,\lambda_i$.

where $f(\mathbf{D'Z_i})$ is the standard normal density, and $F(\mathbf{D'Z_i})$ is the cumulative standard normal density (the predicted probability that I = 1), D is the vector of probit coefficients from the first step and σ_2 is the standard error of the disturbance term from the probit equation. Inserting Equation (5) into Equation (4) yields the following equation, which can be estimated by OLS and will eliminate the asymptotic bias in the estimates of B that is caused by the nonzero covariance (σ_{12}) of the two disturbance terms:

$$(6)\ Y_i = \mathbf{B'X_i} - (\sigma_{12}/\sigma_2)\ \lambda_i$$

To implement this procedure, we calculate λ_i for each sample member from the estimated probit enrollment model and then estimate Equation (6) on the nonenrollees to obtain unbiased estimates of B and (σ_{12}/σ_2). To test for sample selection bias, we test the null hypothesis, $(\sigma_{12}/\sigma_2) = 0$. If we reject this hypothesis, then (by the assumptions of the model) sample selection bias is present, and we cannot justify the use of Equation 3 to estimate the B's required to predict FFS costs for enrollees. In this case (σ_{12} not equal to zero), the appropriate method to predict FFS costs for enrollees is to substitute the estimates of B obtained from Equation (6) and the estimates of D from the probit models, together with $\mathbf{Z_i}$ and $\mathbf{X_i}$'s for enrollees, into the following equation:

$$(7)\ E(Y_i|I_i = 1) = \mathbf{B'X_i} + (\sigma_{12}/\sigma_2)\ \lambda_i^*,$$

where $\lambda_i^* = f(\mathbf{D'Z_i})/F(\mathbf{D'Z_i})$.

Table III.6 reports the results for the enrollment model used in the calculation of Heckman's lambda. Since λ will be constructed from the set of variables that predict enrollment and will be included as a regressor in Equation (6), the enrollment equation must contain one or more variables not included in $\mathbf{X_i}$ (i.e., one or more variables that do not influence Medicare costs), to identify the parameters in Equation (3). Our enrollment equation has two identifying variables: the lowest premium charged by a TEFRA risk plan in the beneficiary's market area and the number of TEFRA risk plans per 1,000 beneficiaries in the market area. Both variables have a significant effect on enrollment status. The coefficient on lowest premium offered implies that a \$10 per month reduction in the lowest premium offered would increase the probability of enrollment by about 3 percentage points above the sample mean of 8.8 percent. Thus, while the coefficient is significant, only large changes in premiums would substantially change the probability of being enrolled. In general, the results conform to our expectations. Beneficiaries younger than 85 are more likely to join, though

TABLE III.6

PROBIT RESULTS: PROBABILITY OF BEING ENROLLED IN A MEDICARE RISK PLAN

Independent Variable	Probit Coefficient	Effect of a Unit Change on Probability[a]
Intercept	-.767 *** (.000)	---
Age		
65-69	.088 .(195)	.014
70-74	.122 * (.060)	.019
75-79	.117 * (.080)	.019
80-84	.072 (-322)	.012
Sex (Male)	.065 * (.062)	.010
Medicaid Buy-in	-.559 *** (.000)	-.084
Disabled (< age 65)	-.321 *** (.001)	-.051
Institutionalized	-321 *** (.004)	-.051
Usual Place of Care	-.393 *** (.000)	-.063
Any ADL Impairments or Poor Health	-.243 *** (.000)	-.039
Income	-.004 *** (.001)	-.001
Missing Income	-.228 *** (.000)	-.036
College, Highest Degree	-.071 (.235)	-.011
High School, Highest Degree	-.016 (.684)	-.003

TABLE III.6 (continued)

Independent Variable	Probit Coefficient	Effect of a Unit Change on Probability[a]
Minority Race	.001 (.983)	-.000
Missing Race Data	-.070 (.650)	-.01
Lowest Premium in Market Area	-.016 *** (.000)	-.003
HMOs per 1,000 Beneficiaries	26.6 *** (.000)	4.25
N	12,582	
Mean of Dependent Variable	.088	

Nom: Numbers in parentheses are p-values.

[a] Effects of a unit change in the independent variable on the probability of enrollment are approximately one-sixth the size of the probit coefficient for a dependent variable with a mean of .088.

* Significant at .10 level, two-tailed test.
** Significant at .05 level, two-tailed test.
*** Significant at .01 level, two-tailed test.

this effect is only significant for those 70-79 years of age. Medicaid recipients are less likely to be enrolled, perhaps indicating their lesser need for supplemental coverage under state buy-in arrangements, which cover Medicare deductibles and coinsurance.

Disabled and institutionalized beneficiaries are also less likely to be enrolled. This is consistent with the argument that beneficiaries with existing health problems tend to have ties to physicians in the FFS sector, and are unlikely to break them to enroll in an HMO. Further evidence that existing physician ties discourage enrollment are the negative and significant effects of having a usual place (physician's office or clinic) for receiving care,[10] and of having ADL impairments or poor health. As income rises, the likelihood of being enrolled falls. Recall that income has a positive effect on the likelihood of purchasing Medigap coverage. The results of both models then suggest that holding other factors constant (including the lowest premium charged in the market area), beneficiaries are more likely to purchase Medigap coverage and less likely to enroll as their incomes increase.

When the lambda variable is constructed from the estimated participation model and included in the participation equation, we find no evidence of selection bias. Table III.7 reports the results with total Medicare reimbursements (Part A plus Part B) for 1989 as the dependent variable. (Models estimated on Part A and Part B separately had the same qualitative results as those reported here.) The coefficient on lambda has a very large standard error and is not statistically significant. Since we cannot reject the null hypothesis that the coefficient on lambda is zero, Heckman's two-stage method thus provides no evidence that Equation (3) suffers from sample selection bias. This result should be interpreted with caution, since it is possible that the participation model is simply too weak to provide a reliable estimate of lambda. Examination of predicted probabilities from the

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[10]For enrollees, this variable measures whether they had a usual place (physician is office or clinic) for receiving health care prior to enrollment. For nonenrollees, it measures whether they currently have a usual place of care.

TABLE III.7

REGRESSION RESULTS FOR TOTAL MEDICARE REIMBURSEMENTS:
SAMPLE SELECTION BIAS AND OLS MODELS

	Reimbursements Calendar Year 1989	
	Sample Selection Bias Model	OLS
Lambda	-1,513 (.674)	
Intercept	-1,218 (.309)	-920 (.343)
AAPCC Risk Indicators		
Disabled (under 65)	222 (.708)	110 (.836)
Age 65-69	376 (.390)	416 (.331)
Age 70-74	902 • * (.040)	958 • * (.021)
Age 75-79	613 (.167)	668 (.116)
Age 80-84	1,547 • ** (.001)	1,581 • ** (.000)
Sex (Male)	457 • * (.047)	483 • * (.030)
Welfare	173 (.778)	-16 (.969)
Nursing home resident	788 (.206)	694 (.235)
Socioeconomic/Demographic Variables		
Minority race (not white)	41 (.927)	39 (.932)
Race information missing	1,910 (.119)	1,880 (.126)
Education		
College	10 (.979)	-30 (.934)
High school graduate, no college degree	-73 (.781)	-82 (.754)
Access to Care		
Income ($1,000)	3 (.463)	-4 (.288)
Income data missing	-56 (.888)	-153 (.640)
Medigap coverage	162 (556)	162 (.558)
Missing data, Medigap	-77 (.927)	-30 (.971)

TABLE III.7 (continued)

	Reimbursements Calendar Year 1989	
	Sample Selection Bias Model	OLS
Health Status		
ADL **impairments**	690 • *' (.000)	675 • ** (.000)
IADL **impairments**	289 • ** (.003)	285 • *' (.003)
Poor health	1,036 • * (.018)	969 ** (.019)
Ever had cancer, heart **disease,** or stroke	1,673 • ** (.000)	1,671 • ** (.000)
Died during 9 months following interview	2,216 • ** (.000)	2,218 • ** (.000)
Missing data poor **health**	3,124 • ** (.004)	3,020 • ** (.005)
Missing data, cancer, heart disease, stroke	2739 • * (.046)	2,186 (.112)
Preferences for **Seeking** Care		
Avoid doctors	-607 • * (.017)	-602 • * (.018)
Missing data, avoid doctors	-134 (865)	18 (.982)
Worry about health	1,328 *** (.000)	1,326 • ** (.000)
Missing data. worry about health	477 (.427)	175 (.770)
Usual place of care	1,426 • * (.018)	1,237 *** (.002)
Missing data, usual plane	-3,797 • * (.041)	-3,684 • * (.048)
Market Area Dummy Variables		
Worchester, MA	-320 (.787)	-338 (.775)
Hampshire City, **MA**	-1,258 (645)	-1,281 (.640)
Rochester, NY	-1,283 (.287)	-1,055 (.329)
Washington, DC	423 (.891)	388 (.901)
Philadelphia, PA	564 (.634)	674 (.561)
Miami, FL	-132 (.902)	164 (.841)
Chicago, IL	-32 (973)	66 (.943)

TABLE III.7 (continued)

	Reimbursements Calendar Year 1989	
	Sample Selection Bias Model	OLS
Indianapolis, IN	1,499 (.344)	1,508 (343)
Flint, MI	-608 (.729)	-553 (.753)
Lansing, MI	543 (.793)	688 (.737)
Minneapolis, MN	-672 (.494)	-481 (.583)
Cleveland, OH	-138 (.919)	-122 (.928)
Duluth, MN	-557 (.739)	-233 (.876)
Albuquerque, NM	-1,095 (.575)	-494 (.712)
Wichita, KS	-1,544 (.585)	-593 (.730)
Denver, CO	-763 (.492)	-583 (.571)
San Francisco, CA	157 (.887)	227 (.837)
Honolulu, HI	-108 (.939)	81 (.953)
Los Angela, CA	347 (.740)	629 (.432)
Portland, OR	-539 (.653)	-205 (.821)
Bridgeport, CT	-202 (.897)	-288 (.853)
Vineland, NJ	-1337 (-585)	-1,281 (.601)
Paramus, NJ	-1,274 (.633)	-1,245 (.642)
New York, NY	115 (.916)	303 (.761)
Buffalo, NY	45 (.980)	101 (.957)
Daytona, FL	-750 (.668)	-384 (.801)
Detroit, MI	658 (.650)	-605 (.676)
Milwaukee, WI	-542 (.762)	-531 (.768)

TABLE III.7 (continued)

	Reimbursements Calendar Year 1989	
	Sample Selection Bias Model	OLS
Corpus Christi, TX	-442 (.871)	393 (.634)
Dallas, TX	-282 (.911)	-270 (.915)
Des Moines, IA	-1,392 (.567)	-974 (.663)
Omaha, NE	-571 (.775)	-347 (.858)
Pueblo, CO	-64 (.969)	197 (.901)
Phoenix, AZ	-71 (.953)	210 (.835)
Las Vegas, NV	-506 (.785)	15 (.992)
Seattle, WA	125 (.899)	257 (.783)
Atlanta, GA	-1,736 (.602)	-1,664 (.618)
Louisville, KY	-912 (.765)	-535 (.856)
Kansas City, MO	-278 (.832)	-195 (.881)
Tulsa, OK	-1,406 (.668)	-1,170 (.719)
Providence., RI	-1,240 (.669)	-1,087 (.707)
San Antonio, TX	-2,514 (.196)	-1,946 (.167)
Sacramento, CA	-95 (.971)	-6 (.998)
R^2	.075	.075
Mean of Dependent Variable	$2,811	$2,811
N	6,107	6,107

NOTE: Number in parentheses are p-values for 2-tailed tests of the hypothesis that the coefficient is zero for the population. Thus, values below .05 indicate that the effect of the variable on reimbursements is significantly different from zero at the .05 level.

- Significantly different from zero at the .10 level, two-tailed test.
- * Significantly different from zero at the .05 level, two-tailed test.
- ** Significantly different from zero at the .01 level, two-tailed test.

model shows that very few enrollees have predicted probabilities of enrollment that exceed .5. Nonetheless, the sizeable number of statistically significant coefficients, including those on the two identifying variables, suggests that the model is adequate.

Also noteworthy is the fact that the coefficients on most variables in Equation 3 change very little when the sample selection term λ is added to the model. Column 2 of Table III.7 reports the results for the corresponding OLS model. In particular, the coefficients on the health status variables are quite similar for the two specifications. Two notable exceptions are the coeffkients on Medicaid coverage and disability status, both of which are larger in the sample selection bias model compared to OLS. However, the change in predicted FFS costs for enrollees from these higher coeffkient values would be minimal given the small fractions of enrollees classified as Medicaid and disabled.

Despite the statistical insignificance of the effect of λ on FFS costs, we find that the addition of the λ term and use of Equation 7 rather than Equation 3 to predicted FFS cost would alter the predicted cost for enrollees substantially. This change results because the estimated coefficient on λ, though statistically insignificant, is large, and when multiplied by the average value of λ_i^* for enrollees (1.65) yields a sizeable change to BX_i. Indeed, average total FFS cost for enrollees in 1989 as predicted by Equation 7 is actually a negative number, -$642 (results using average total FFS costs for the 12 months prior to interview as the dependent variable were equally implausible).

The results of the sample selection bias model suggests that the OLS model (Equation 3) is preferred over Heckman's sample selection bias model for several reasons:

1. The coefficient on lambda, which measures the degree of biased selection, is not statistically significant in the Part A, Part B, or total reimbursement regressions, using either 1989 or 12 months preceding interview as the time period over which the dependent variable was measured. In one of the 6 applications, the coefficient on lambda is negative and in another instance implies that the correlation between error terms exceeds 1.0.

2. The value for FFS cost for enrollees as predicted by the sample selection bias model is implausible.

3. The coefficients obtained when controlling for possible sample selection bias are very similar to those obtained from the straightforward OLS model without this term, providing further evidence that the **OLS** estimates of Equation (3) are robust and do not suffer from selection bias as defined in the Heckman model.

It appears then that the extensive set of control variables on health status and attitudes succeed in capturing the major sources of correlation between service use and the likelihood of enrollment in an HMO.

2. The Two-Part **Model**

The distribution of health care expenditures across individuals is characterized by (1) a large fraction of individuals with zero expenditures (about 17 percent of nonenrollees in our sample) and (2) a small fraction of individuals with extremely high expenditures (over $300,000 in our sample). Duan et al. (1983) argue that OLS will not generate precise forecasts of expenditures compared to alternative models that consider the truncated and highly skewed distribution of expenditures. As an alternative they propose a "two-part model" of health care costs which explicitly models (1) the likelihood that any health care services are used, and (2) the level of expenditures for those with some use. The probability that any services are used (i.e., expenditures are greater than zero) is given by:

(8) $\text{Prob}(Y_i > 0) = F(\mathbf{D'X_i})$,

where, $F(\mathbf{D'X_i})$ is the cumulative probability density function for the standard normal, D is a vector of parameters, and $\mathbf{X_i}$ is the same vector of explanatory variables as used in Equation (3). We estimated Equation (8) on the entire nonenrollee sample using maximum likelihood probit. The expenditures for those with some service use is given by the following equation:

(9) $\ln Y_i = \mathbf{B'X_i} + e_i$,

where $\ln Y_i$ is the natural log of expenditures for $Y_i > 0$, and B, X_i, and e_i have the same meanings as before. We estimated (9) on the portion of the nonenrollee sample with $Y_i > 0$. Expected reimbursements for the i^{th} beneficiary are then given by:

(10) $E(Y_i) = F(D'X_i) \cdot \exp\,[B'X_i] \cdot \phi,$

where the "smearing factor," ϕ, is equal to the expected value of the exponentiated disturbance term, $E(\exp(e_i))$.[11]

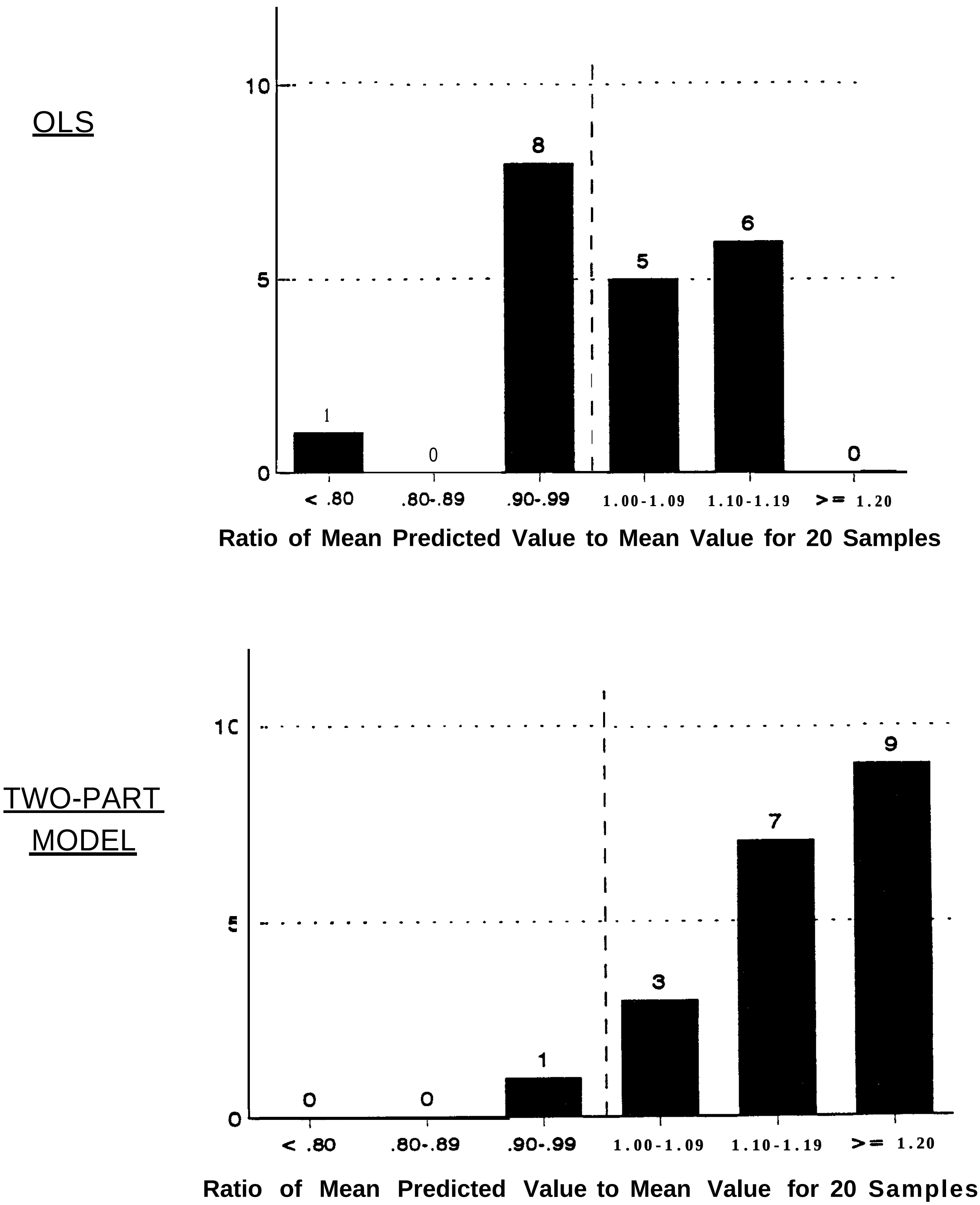
The primary motivation for estimating the two-part model is the possible improvement in the accuracy of FFS predictions for enrollees. To test the accuracy of the two-part model compared to the OLS model, we conducted simulations similar to those presented in Duan et al. (1984). The authors compare the predictive accuracy of the two-part model with a two-stage tobit estimator by splitting their sample into an estimation sample and prediction sample. At each iteration of the simulation, the model parameters are fit to the estimation sample. The dependent variable is predicted using the parameters and independent variables in the prediction sample. Predicted values are compared with the actual values for the dependent variable in the prediction sample to assess the performance of the model.

We conducted 20 such iterations on the nonenrollee survey sample to assess the predictive accuracy of the OLS model and 2-part models. The two attributes we were most interested in were bias and mean squared error. To measure bias we computed for the prediction sample at each iteration the ratio of the mean predicted value to the actual mean value of the dependent variable. We then plotted the distribution of the 20 ratios for each of the models. Figure III.1 displays the results.

"If the distribution of e_i is normal with variance σ^2, then the smearing factor equals exp $(\sigma^2/2)$. A nonparametric estimate of ϕ is simply the sample mean of $\exp(\hat{e}_i)$. Separate estimates were made for each smearing estimate, but only the results for the nonparametric smearing factor--which we found to be more reasonable--are reported here.

FIGURE III.1

COMPARISON OF PREDICTED TO ACTUAL VALUES FOR TWO MODELS OF FFS REIMBURSEMENTS



Note that the two-part model tends to substantially over-predict, by 19 percent on average for the 20 simulations. In only one iteration is the mean predicted value less that the actual mean; and in 9 of the 20 iterations, the mean predicted value exceeds the actual by 20 percent or more. The OLS model over-predicts in about half of the iterations, as expected, and the average ratio across our 20 simulations was quite close to 1.0 (1.015). The two-part model is thus clearly inferior to the OLS model in terms of bias. Comparisons of root means squared errors for the alternative estimates yields similar conclusions. The root mean squared errors for the two part model were 12 percent higher on average than those for OLS.

We obtain similar results in simulations conducted on samples of nonenrollees that were used in the analysis of biased selection (Hill and Brown, 1990). Table III.8 reports the results for nonenrollees from four of the largest market areas. For each market area, we selected an estimation and prediction sample from the nonenrollee samples, and estimated the OLS and two-part models. The independent variables were age, sex, disability status, welfare status, Medicare reimbursements in 1986, and dummy variables indicating whether the beneficiary died. The results indicate, once again, that OLS is a reasonably accurate predictor of FFS costs on average (average ratio of predicted to actual values equal to 1.025), while the two-part model systematically over-predicts by a large percentage (average ratio equal to 1.42).

3. Reasons for Not Adopting the Alternative Models

The simulations above confirm that the basic OLS model was more accurate than the sample selection bias and two-part models in predicting FFS reimbursements for nonenrollees. Furthermore, the results from the sample selection bias models reported on Table III.7 provide evidence that the estimates are not distorted by sample selection bias. Thus, we are reasonably confident that the model estimated on the nonenrollee sample yields reliable estimates of what FFS reimbursements would have been for the enrollee sample, had they remained in FFS.

TABLE III.8

A COMPARISON OF THE TWO-PART MODEL WITH OLS, USING SAMPLES FROM THE BIASED SELECTION STUDY

			Ratio of Predicted to Actual Reimbursements for Nonenrollees[a]	
Market Area	Sample Size	Year	OLS Model[b]	Two-Part Model[c]
Miami	5,896	1988	1.09	1.26
		1989	.97	1.15
Los Angeles	5,850	1988	1.06	1.40
		1989	1.02	1.26
Minnesota	1,896	1988	.97	1.30
		1989	1.00	1.47
Chicago	1,998	1988	1.02	1.73
		1989	1.07	1.82

[a]For each market area, the models were estimated on one-half of the nonenrollee sample;' predicted values were obtained from the other half and then compared to actual values.

[b]The dependent variable is Medicare reimbursement. The independent variables are age, sex, welfare status, disability status, 1986 Medicare reimbursement, and mortality (whether and when the beneficiary died.).

[c]See the text for the details of the model. The independent variables are the same as the OLS model.

D. ESTIMATING CAPITATION PAYMENTS

The most straightforward method for determining the impact of the Medicare risk program on costs to Medicare is to compare the predicted FFS costs of enrollees with the actual capitation payments made to the HMO for these individuals during the same time period. However, this approach would yield misleading estimates of the program's long-run impacts on costs, for the following reasons:

1. Our estimates of what enrollees would have cost had they remained in FFS are for the year preceding interview, during which time all were alive. That is, the regression model on which the estimates are based are for a set of nonenrollees who were alive throughout the period over which reimbursements were measured. The AAPCC, however, is an estimate of the average reimbursements for *all* beneficiaries, including the 6 percent who die during the year (and incur much larger costs).

2. Differences between payment rates and predicted FFS costs in 1989 will reflect inaccuracies in projecting the USPCC and the AGA, which may be especially large for this period given the advent of the Medicare Catastrophic Coverage Act.

3. Sampling error--the sample of nonenrollees used to generate our estimates of what enrollees would have cost the Medicare program may not be an especially representative sample of a given market area, simply due to sampling variance. Thus, there will be some differences (perhaps sizeable) between actual AAPCC payments and projected FFS costs, which are due solely to the fact that the two estimates were computed on different samples. Such differences limit our ability to assess the effect that observable beneficiary characteristics have on the costs or savings to HCFA

The most compelling reason for not using actual payment rates in our assessment of cost impacts is that it would yield seriously biased estimates, due to the fact that our sample includes no one who died. The beneficiary samples selected for interview were drawn from the set of beneficiaries who were alive as of April 1, 1990. For 1989, the average FFS costs of nonenrollees in our survey sample will be considerably less than the average for the Medicare population in the same market areas, because unlike the Medicare population, all survey nonenrollees were alive throughout the year. Indeed, since Medicare beneficiaries in their last year of life account for 28 percent of Medicare costs by one estimate, and the mortality rate is approximately 6 percent of the Medicare population (Lubitz and Prihoda, 1984), we would expect that FFS cost for our sample would be about 23 percent less that the average for the Medicare population.[12] Indeed, if we compare average 1989 FFS costs (reimbursements plus pass-through and administrative costs) of our survey sample of nonenrollees with average capitation payments adjusted to reflect 100 percent rather than 95 percent of expected FFS costs, we find a difference of 21 percent. A direct comparison of 1990 payment rates with 1990 predicted FFS costs would suffer to some extent from the same problem, since the 1990 mortality rate in our nonenrollee sample is still lower than the rate for the Medicare population (since all beneficiaries were known to be alive as of April 1, 1990 and since we are often not successful in obtaining proxy interviews for sample members who died between April 1 and the date of the attempted interview).

The second problem with using actual payment rates for a specific calendar year in our cost estimates is that it reflects transitory (year-specific) as well as systematic errors in projecting the FFS costs of nonenrollees. In 1989, the transitory error is likely to be large since Medicare Catastrophic coverage began in 1989 and HCFA's estimates of its impact on Medicare costs may not be very accurate. The change in coverage for skilled nursing facilities provides an excellent example. Under Medicare Catastrophic, the previous requirement that a beneficiary be in the hospital for at least 3 days before being eligible for SNF coverage was dropped, copayments for SNF days exceeding the first eight days were dropped, and maximum coverage was changed to 150 days per year (formerly 100 days per spell of illness). As a result, Medicare reimbursements nationally for SNF care increased

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[12] The 23 percent estimate is derived as follows. First, express average Medicare costs, R, as a weighted average of the average cost for the 94 percent of beneficiaries alive throughout the year, R_a, and the average cost of the 6 percent of beneficiaries who die during the course of the year, R_d. More succinctly, $\bar{R} = .94R_a + .06R_d$. Since decedents account for 28 percent of total Medicare costs, $.06\ R_d = .28R$. Substituting this relationship into our formula for the weighted average, and solving for R_a, we get the following:

$$R_a = (.72/.94)R = .766R$$

Thus, average cost for survivors is 76.6 percent of the average for all beneficiaries, or about 23.4 percent less than the average for all beneficiaries.

from $97 million for the month of January 1989 to $280 million for November 1989. Predicting this nearly three-fold increase in SNF spending is obviously difficult, and points out the difficulty in assessing the program's impact on costs based on the payment rates in any given year.[13]

Finally, the third major problem with using actual capitation rates as the measure of the cost to HCFA is that sampling error will distort our comparisons. For example, even if the AAPCC predicted perfectly the average FFS cost for the population of nonenrollees residing in a given county, our small sample of nonenrollees from that county may have a very different mean cost simply by chance. Such chance differences due to our relatively small sample could make it more difficult to identify the reasons for any differences between AAPCC payments and what reimbursements for enrollees would have been in the FFS sector.

1. Predicting Payment Rates Using the Payment Rate Methodology

The problems above can all be avoided if we use our nonenrollee sample to estimate a payment rate formula for enrollees that incorporates the two basic principles of the current AAPCC methodology: (1) payment rates should reflect the per capita FFS costs in the enrollee's geographic area, and (2) payment rates should reflect the different relative costs of beneficiaries in the 60 AAPCC classifications, (5 age categories x 2 sex categories x 2 eligibility categories x 3 welfare/institutional categories). To do so, we estimated the following regression model on the nonenrollee sample:

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[13]A related problem encountered in assessing the program's impact on costs for the 12 months preceding interview using actual payment rates is that the benefits covered under FFS in 1990 differed from the benefits covered by the AAPCC. The Medicare Catastrophic Coverage Act (MCCA) was repealed in 1989, but payment rates for Medicare risk plans had already been established based on the expectation that catastrophic coverage would continue in 1990. Medicare risk plans received the established rate and were required to provide coverage according to the MCCA In this instance, the payment rates reflect an estimate of FFS cost for nonenrollees if catastrophic coverage was in effect, while actual FFS costs for nonenrollees in 1990 reflects costs after repeal of the MCCA It is possible to net out the component of 1990 capitation payments attributable to catastrophic coverage, since HCFA actuaries have computed this figure, and then compare the adjusted rates for enrollees with their predicted FFS costs. However, one could argue that the true cost impact in 1990 is still the difference between actual payments and what costs for enrollees would have been in the FFS sector.

$$(11) \quad Y. = X_s c_s + X_a c_a + u,$$

where Y, X_s, and X_a are described as before (see Equation 3), c_s and c_a are vectors of regression coefficients, and u is the regression error term.

Note that except for the exclusion of the survey variables, X_o, this model is exactly the same as Equation (3), which was used to estimate the costs that HCFA would have incurred had enrollees not joined the HMO. (The coefficients and error term are renamed because they will have different values here, due to the exclusion of X_o from the model.) This nesting of the models ensures that any difference between predicted costs and predicted AAPCC payments is due entirely to observed characteristics of the enrollees that affect costs but are not captured by the AAPCC risk indicators, as we shall see later.

Equation (11) was estimated on the nonenrollee sample and then used as our "AAPCC formula," to project what risk program payments would be in 1989 if the AAPCC were accurate for our sample of nonenrollees and based on only the AAPCC risk and market area indicators. Separate models were estimated for Part A and Part B. We obtain AAPCC payments for each enrollee by inserting their AAPCC risk and market area indicators into the estimated equations.

The estimates of Equation (11) for Part A and Part B provided in Table III.9 conform to our general expectations. The excluded (reference) category for the risk cells is males age 65 to 69; hence, the coefficient for a given risk cell indicates that expected difference in cost between those in the risk cell and those in the reference group. For example, we see the expected monotonic relationship between age and Part A payment rates for men who are not institutionalized and not on Medicaid. Females over 80 also have significantly higher AAPCC rates. Both Part A and Part B reimbursements are significantly and substantially greater for residents of nursing homes than for the

TABLE III.9

REGRESSION RESULTS: MODEL TO PREDICT AAPCC PAYMENTS RATES

Independent Variables	Part A	Part B
Intercept	979 (.169)	760 • ** (.002)
AAPCC **Risk Indicators**[a]		
Non-Institutionalized, Non-Medicaid		
Male, disabled (under 65)	132 (.824)	-348 * (.081)
Male, 70-74	234 (.558)	-22 (.873)
Male, 75-79	488 (.280)	3% • ** (.009)
Male, 80-84	1,315 • ** (.010)	789 • ** (.000)
Male, ≥ 85	1,592 • * (.012)	383 * (.072)
Female, disabled (under 65)	841 (.224)	371 (.110)
Female, 65-69	-310 (.439)	-122 (.365)
Female. 70-74	79 (.834)	196 (.123)
Female, 75-79	-85 (.834)	158 (.248)
Female, 80-84	1,038 • * (.021)	349 • * (.021)
Female, ≥ 85	1,033 • * (.035)	325 • * (.048)
Non-Institutionalized, Medicaid		
Male, disabled (under 65)	1,927 (.032) • *	98 (.746)
Male. 65-69	2,344 (.221)	2,557 *** (.000)
Male, 70-74	1,115 (.382)	295 (.491)
Male, 75-79	99 (.959)	1,174 • (.067)
Male, 80-84	5,761 • * (.013)	1,478 * (.058)
Male, ≥ 85	170 (.931)	352 (.591)
Female, disabled (under 6.5)	1,240 (.242)	551 (.121)

TABLE III.9 (continued)

Independent Variables	Pan A	Part B
Female, 65-69	1,248 (.243)	141 (.695)
Female, 70-74	640 (.500)	743 • * (.020)
Female, 75-79	451 (.667)	180 (.609)
Female, 80-84	574 (.604)	442 (.234)
Female, Age ≥ 85	1,336 (.229)	547 (.143)
Nursing Home Resident	3,407 • ** (.000)	861 • ** (.000)
Market Area Dummy Variables[b]		
Worchester, MA	-167 (.867)	50 (.882)
Hampshire City, MA	-597 (.796)	-354 (.649)
Rochester, NY	-680 (.457)	-289 (.345)
Washington, DC	197 (.940)	1 6 2 (.855)
Philadelphia, PA	425 (.665)	393 (.233)
Miami, FL	-262 (.704)	650 • ** (.005)
Chicago, IL	-18 (.982)	110 (.673)
Indianapolis, IN	1,766 (.188)	515 (.253)
Flint, MI	155 (.917)	27 (.957)
Lansing, MI	517 (.766)	219 (.707)
Minneapolis, MN	-144 (.846)	-237 (.341)
Cleveland, OH	. 61 (.958)	102 (.791)
Duluth. MN	468 (.711)	-506 (.233)
Albuquerque, NM	-327 (.773)	40 (.917)
Wichita. KS	-490 (.736)	106 (.827)
Denver. CO	-439 (.614)	-182 (.534)

TABLE III.9 (continued)

Independent Variables	Part A	Part B
San Francisco, CA	265 (.776)	-12 (.970)
Honolulu, HI	493 (.659)	-34 (.928)
Los Angeles, CA	268 (.693)	508 . * (.025)
Portland, OR	104 (.892)	-50 (.845)
Bridgeport, CT	-607 (.644)	-288 (.514)
Vineland, NJ	-1,270 (.540)	-515 (.460)
Paramus, NJ	-633 (.780)	-99 (.897)
New York, NY	168 (.842)	265 (.348)
Buffalo, NY	704 (.654)	36 (.945)
Daytona, FL	629 (.625)	207 (.632)
Detroit, MI	-290 (.813)	-184 (.654)
Mihvaukee, WI	-413 (.786)	-39 (.940)
Corpus Christi, TX	1,112 (.483)	280 (.599)
Dallas, TX	-340 (.874)	285 (.693)
Des Moines, IA	-112 (.953)	-275 (.664)
Omaha, NE	-133 (.935)	-90 (.871)
Pueblo, CO	615 (.645)	-186 (.678)
Phoenix, AZ	-61 (.943)	357 (.213)
Las Vegas, NV	-264 (.821)	250 (.524)
Seattle, WA	256 (.745)	84 (.751)
Atlanta, GA	-1,088 (.700)	-511 (.589)
Louisville, KY	-302 (.903)	-280 (.737)

TABLE III.9 (continued)

Independent Variables	Part A	Part B
Kansas City, MO	-8 (.994)	-61 (.868)
Tulsa, OK	-626 (.820)	-278 (.764)
Providence, RI	-978 (.690)	-415 (.614)
San Antonio, TX	-1,247 (.295)	-316 (.430)
Sacramento, CA	-153 (.945)	214 (.774)
Mean of Dependent Variable	$1,558	$1,254
R^2	0.028	0.037
N	6,107	6,107

NOTE: Number in parentheses are p-values for 2-tailed tests of the hypothesis tbat the coefficient is zero for the population. Thus, values below .05 indicate that the effect of the variable on reimbursements is significantly different from zero at the .05 level.

[a]The excluded AAPCC category is males, age 65-69. Thus, the coefficients on the AAPCC risk indicators represent the difference in projected AAPCC rates between that risk cell and males age 65-69.

[b]The excluded site is Boston, MA Thus, the coefficients for a particular site indicates the difference in projected AAPCC rates between that site and Boston.

*Significantly different from zero at the .10 level, two-tailed teat.
**Significantly different from zero at the .05 level, two-tailed test.
● **Signibcantiy different from zero at the .01 level, two-tailed teat.

reference group. Expected Part A costs for this group are over three times the overall average Part A costs, and Part B costs are about 70 percent above the overall mean. In general, however, few of the coefficients are statistically significant, and the model does not explain much of the variance in the Medicare costs. The low value for R^2, about .02, is consistent with previous studies (e.g., Ellis and Ash, 1988) investigating the predictive power of the AAPCC risk indicators.

Inserting enrollee sample means for the AAPCC variables into the estimated models yields a projected total AAPCC rate of $2,607 for enrollees for 1989, about 7.3 percent below the average actual reimbursements ($2,812) for nonenrollees. The difference is due to the previously noted fact that enrollees are younger, much less likely to be in nursing homes or on Medicaid, and less likely to be disabled. The predicted average AAPCC rates for enrollees are $1,385 for Part A (11.1 percent below the nonenrollee mean) and $1,223 for Part B (2.5 percent below the nonenrollee mean). The payment to the HMOs is set at 95 percent of the AAPCC; hence, the average estimated capitation payment per enrollee is $2,478, or about $207 per month.

2. **Estimates of the Savings or Cost of the Program to HCFA**

Our estimates suggest that the AAPCC methodology overestimates the costs that would have been incurred for enrollees, had they remained in F'FS, by 11.3 percent for 1989, as a result of favorable selection. Dividing our estimate of the implicit AAPCC rate for our sample of enrollees ($2,608) by the average estimated cost (see Section B) that would have been incurred for these enrollees had they remained in fee-for-service ($2,344), we obtain a ratio of 1.113 for 1989. Very similar estimates were obtained in preliminary analyses using the year prior to interview as the time period of interest.

The inverse of the above ratio, C_f/C_a, provides an estimate of biased selection that can be compared directly with those reported in previous studies. As noted earlier, in studies of biased selection using prior use, C_f is estimated by the actual FFS cost of enrollees prior to enrolling. C_a in these studies is estimated by nonenrollee FFS cost during the same calendar period, adjusted to reflect the different distribution of enrollees on AAPCC risk indicators, which is analogous to our estimate of C_a obtained from Equation (11). Thus, C_f/C_a from our study is analogous to the measures of biased selection reported in previous studies. The comparisons are presented in Table III.10. Note that magnitude of favorable selection is considerably smaller in this study (.90) than previous estimates (.75 - .80). This reflects, in part, our adjustment for the higher fraction of enrollees that would purchase Medigap insurance if the HMO option were not available. More importantly, it reflects regression toward the mean, since enrollees will have developed more health and functioning problems by the time of the survey than they had at enrollment, making them resemble nonenrollees more closely than they would have in the year prior to joining the HMO. (Recall that over half the enrollees in our sample had been enrolled for 3 years or longer at the time of interview, and only 11 percent had been enrolled for less than one year.)

Our estimates suggest that, due to favorable selection, HMOs are paid 5.7 percent more than it would have cost the Medicare program for enrollees had they remained in the FFS sector, or $134 per enrollee-year in 1989. Table III. 11 compares the predicted FFS costs for enrollees from Equation (3) with predicted capitation costs ($.95C_a$), for Part A and Part B separately. For 1989, total predicted capitation payments for enrollees were $2,478 compared with predicted FFS costs of $2,344. Since payments exceed expected FFS costs, the program results in an increase in costs to HCFA instead of the expected five percent cost savings.

The bulk of the increase in costs is for Part A services. Our estimates indicate that AAPCC payments exceed what FFS costs would have been by 8.5 percent for Part A, but only 2.7 percent for Part B. The Part A payments account for just over half (53 percent) of the total payments, but over three-fourths of the increase in costs is due to Part A Thus, implicit AAPCC monthly payments for the enrollees in the sample, which averaged $206.50 in 1989 according to our estimates, were about $11 per month higher than what average FFS costs would have been.

TABLE III.10

COMPARISON OF BIASED SELECTION, THIS STUDY VS. PREVIOUS STUDIES USING PRIOR USE

	Measure of Biased Selection:	
	C_f/C_a: Ratio of Expected FFS Costs to FFS Costs Predicted by the AAPCC Methodology	C_a/C_f: Measure of Biased Selection in Cost Savings Calculations
This Study, 1989 Reimbursements	.90	1.11
Hill and Brown (1991)	.75	1.33
Hill and Brown (1990)	.77	1.30
Brown (1988)	.80	1.25

TABLE III.11

COMPARISON OF AVERAGE ANNUAL PREDICTED CAPITATION PAYMENTS AND PREDICTED FFS COSTS PER ENROLLEE, 1989

	Part A	Part B	Total
Predicted AAPCC, AAPCC Model[a]	$1,385	$1,223	$2,608
Predicted Capitation Payments, 95 Percent of Predicted AAPCC	$1,316	$1,162	$2,478
Predicted FFS Costs[b]	$1,213	$1,131	$2,344
Difference between Predicted Payments and Predicted FFS Costs	$103	$31	$134
Percent Difference Between Predicted Payments and Predicted FFS Costs	8.5 %	2.7 %	5.7 %

NOTE: Predicted costs and capitation payments were obtained for the sample of 6,475 enrollees.

[a]Capitation payments were imputed for enrollees from a regression model estimated on nonenrollees, with Medicare cost (reimbursements plus pass-through and administrative costs) as the dependent variable and AAPCC risk classifications and binary site variables as the independent variables. Capitation was computed for each enrollee as 95 percent of the predicted AAPCC rate from this model.

[b]FFS costs were imputed for enrollees from a regression model estimated on nonenrollees with Medicare cost as the dependent variable and the set of variables in Table III.3 as the independent variables. FFS costs were. predicted for each enrollee in the sample, based on the enrollee's predicted probabilities of having Medigap coverage and having a usual source of care had they not joined an HMO, and on the enrollee's actual values for all other variables.

The estimated impact on program costs to HCFA is significantly different from zero at the one percent level (t-statistic = 3.35). Based on the estimated standard error of $40, the 95 percent confidence interval around our point estimate of $134 is $56 to $212, or about 2.4 to 9.1 percent of the cost that HCFA would have incurred for enrollees under FFS arrangements. Because of the way that the estimated effect on costs is constructed, the standard error was not straightforward to compute. The derivation is given in Appendix D.

Our estimate that favorable selection generates a loss to the Medicare program of 5.7 percent is somewhat smaller than earlier estimates. The loss is less than the 19 to 27 percent losses implied by the previous estimates of biased selection showing that enrollee costs in the FFS would be between 20 and 25 percent lower than the FFS costs of nonenrollees with the same actuarial risks (although these studies have always acknowledged that regression toward the mean would reduce these differences somewhat). The estimated losses in this study are also less than the 15-33 percent losses to HCFA estimated by Nelson and Brown (1989) in their evaluation of the impact of the Medicare Demonstration HMOs on costs to Medicare. The use of a random sample of all enrollees rather than of new enrollees, along with better data, explain the more moderate losses estimated in this study.

3. Sources of the Increase in Costs to HCFA

To identify the characteristics of enrollees that lead to the increased cost to HCFA, we take advantage of the simple nested models that we use and a basic but useful feature of regression analysis. Essentially, we show how each of the new survey variables (X_o) that are included in the regression model of FFS costs contributes to the difference between the AAPCC payments and the FFS costs that would have been incurred for enrollees.

The contribution of each of the survey variables that are not incorporated into the AAPCC--that is, everything in the FFS cost model except age, sex, nursing home residence, Medicaid coverage and site--is the product of two components. The first component is the effect that the characteristic has on FFS costs, as estimated in Table 111.3. The second component is the degree to which the characteristics that ***are*** included in the AAPCC mechanism fail to account for these characteristics that are ***not*** taken into account. If the characteristic (e.g., impairments on ADL tasks) has no effect on cost, then enrollee-nonenrollee differences on this factor do not account for any of the observed effect of risk contracting on costs to HCFA Even if the characteristic is important, the enrollee-nonenrollee difference will have no effect on costs to HCFA if these differences are fully accounted for by the characteristics that are included in the AAPCC.

The following derivation shows this relationship more clearly. First, rewrite the two equations as follows:

$$\text{(12)} \qquad \textit{FFS Costs:} \qquad Y = X_a b_a + X_o b_o + e$$

$$\text{(13)} \qquad \textit{AAPCC Rate:} \; Y = X_a c_a + u,$$

where X_a now includes both the demographic risk factors included in the AAPCC and the binary variables for sites (X_s in our earlier notation). These equations are estimated on nonenrollees, then enrollee mean values for X_a and X_o ($\bar{X}_a^e$ and $\bar{X}_o^e$) and are inserted into the two equations to obtain the estimated FFS costs and AAPCC rates for the enrollee sample. Hence, the difference between the AAPCC and the projected FFS costs for enrollees, which is the reason for the cost increases to HCFA, is:

$$\text{(14)} \qquad \hat{Y}^e_{AAPCC} - \hat{Y}^e_{FFS} = \bar{X}^e_A \hat{c}_a - (\bar{X}^e_a \hat{b}_a + \bar{X}^e_o \hat{b}_o)$$

$$= \bar{X}^e_a(\hat{c}_a - \hat{b}_a) - \bar{X}^e_o \hat{b}_o$$

A convenient property of regression analysis enables us to convert this expression into one that is solely a function of the more detailed survey characteristics (X_o) that are not part of the AAPCC formula. Because the AAPCC rate equation is a “shortened” version of the “full” FFS cost model, the coefficients $\hat{c}_a$ from the short regression can be shown to be exactly equal to the coefficients on these same variables in the full regression plus an additional term that is a function of the coefficients on the X_o variables that appear only in the full regression:

$$
\begin{aligned}
c_a &= (X_a'X_a)^{-1}X_a'Y \\
&= (X_a'X_a)^{-1}X_a'(X_a\hat{b}_a + X_o\hat{b}_o + \hat{u}) \\
&= \hat{b}_a + (X_a'X_a)^{-1}X_a'X_o\hat{b}_o \\
&= \hat{b}_a + P_{ao}\hat{b}_o
\end{aligned}
\tag{15}
$$

Each column of matrix P_{ao} is a vector of regression coefficients from the “auxiliary” regression of the corresponding new survey variables (X_o) on the set of characteristics used to risk-adjust the AAPCC (X_a). Since the models are estimated on the nonenrollees only, this relationship applies only if the auxiliary regressions are estimated on the nonenrollee sample.

If we insert the expression for c_a in (15) into Equation (14) we find:

$$
\begin{aligned}
\hat{Y}^e_{AAPCC} - \hat{Y}^e_{FFS} &= \bar{X}^e_a(\hat{c}_a - \hat{b}_a) - \bar{X}^e_o\hat{b}_o \\
&= \bar{X}^e_a(\hat{b}_a + P_{ao}\hat{b}_o - \hat{b}_a) - \bar{X}^e_o\hat{b}_o \\
&= \bar{X}^e_a P_{ao}b_o - \bar{X}^e_o b_o \\
&= (\bar{X}^e_a P_{ao} - \bar{X}^e_o)\hat{b}_o \\
&= (\hat{X}^e_o - \bar{X}^e_o)\hat{b}_o
\end{aligned}
\tag{16}
$$

That is, the difference between the average AAPCC rate and the average projected FFS costs that would have been incurred for enrollees is equal to the product of the error in predicting the mean of the X_o variables using the auxiliary regressions and the coefficients on these X_o's from the full FFS cost regression (Equation 12). The predicted value of the X_o variables for enrollees are obtained by inserting mean values for X_a for enrollees into the auxiliary regressions, which were estimated on the nonenrollees.

The interpretation of these results is straightforward and appealing, and enables us to identify exactly which characteristics of enrollees account for the cost increases to HCFA. If $\hat{b}_o$ for some characteristic (e.g., income) is zero, that is, the characteristic has no effect on FFS costs, then enrollee values on this characteristic, which is not included in the AAPCC formula, have no effect on costs to HCFA. Conversely, even if the characteristic does affect FFS costs, if the AAPCC factors are able to predict this excluded characteristic reasonably well on average, having excluded it from the payment determination method will not result in AAPCC rates that are too high or too low. This feature of our estimates is consistent with the concept behind the AAPCC. If the characteristics that are included in the AAPCC rate structure are good proxies on average for other characteristics that are different to measure, the AAPCC will be an accurate projection of the costs that would have been incurred for enrollees under FFS care, and HCFA will save the intended 5 percent. The poorer the ability of AAPCC risk indicators to capture the effects of excluded characteristics, and the more important these characteristics are for determining Medicare costs, the further the gap is likely to be between the AAPCC and the costs that would have been incurred under FFS coverage.

There are actually two other ways that the impact of risk contracting on costs to HCFA would be equal to zero. If enrollees had the same unadjusted mean values for X_a and X_o as nonenrollees, or if the ***auxiliary relationships*** (the P_{ao}) between X_a and X_o (including intercept terms) were identical for enrollees and nonenrollees, we would observe no effect on costs to HCFA for enrollees. These conditions are also appealing--if enrollees looked like nonenrollees on average, there would be no biased selection and we would expect HCFA to save 5 percent as intended (assuming the AAPCC accurately projects cost for nonenrollees). Alternatively, if the relationship between AAPCC characteristics and other personal characteristics were identical for enrollees and nonenrollees, the

AAPCC characteristics would be fully accounting for the observed differences in means in these other characteristics.[14]

Obviously, since we find that the AAPCC for enrollees exceeds what their FFS costs would have been by 11 percent, none of the four conditions that would lead to no effect on costs to HCFA are satisfied:

- Many of the coefficients (b,) on survey variables that are not used in the risk adjustor to the AAPCC rate are large and significantly different from zero (see Table III.3).
- The AAPCC factors fail to predict accurately the values of the other survey variables
- The unadjusted means for enrollees on X_o and X_a are quite different from the unadjusted means for nonenrollees (see Table III.2)
- The relationship of AAPCC factors to beneficiary characteristics from the survey is quite different for enrollees and nonenrollees (see Hill and Brown, 1992, and the discussion concerning that study in Chapter I of this report)

The estimates we obtain from the decomposition given in Equation (16) show that 83 percent of the observed difference between the average AAPCC rate projected for enrollees ($2,608) and

[14]These relationships can be seen by adding to Equation (16) a term that is equal to zero and then rearranging:

$$\begin{aligned}\hat{Y}^e_{AAPCC} - \hat{Y}^e_{FFS} &= \bar{X}^e_a P_{ao}\hat{b}_o - \bar{X}^e_o\hat{b}_o \\ &= \bar{X}^e_a P_{ao}\hat{b}_o - \bar{X}^e_o\hat{b}_o + (\bar{X}^n_o - \bar{X}^n_a P_{ao}) \\ &= [(\bar{X}^e_a - \bar{X}^n_a)P_{ao} - (\bar{X}^e_o - \bar{X}^n_o)]\hat{b}_o\end{aligned}$$

The term in parentheses in the second line is equal to zero because the mean predicted value for X_o for nonenrollees from the auxiliary regressions, $\bar{X}^n_a P_{ao}$, is required to equal the mean actual value for nonenrollees, $\bar{X}^n_o$, since the auxiliary regressions were estimated on this sample of nonenrollees. Thus, if the difference in unadjusted means between enrollees and nonenrollees on X_a and X_o were equal to zero, or if the unadjusted difference in means between enrollees and nonenrollees on the excluded survey characteristics (X_o) are fully explained by the difference in means on the AAPCC factors (X_a), the AAPCC would be accurate and HCFA would have saved 5 percent according to our estimates. Alternatively, if P_{ao} were estimated on enrollees and found to be identical to P_{ao} estimated on nonenrollees, then $\bar{X}^e_a P_{ao} - \bar{X}^e_o$ would equal zero, due to the same econometric requirement of regression models described above. Again, our estimates in this case would show that HCFA saved the intended 5 percent.

the average FFS cost that would have been incurred for them ($2,344) is due to the effects of health status variables that are not captured by the **AAPCC** risk indicators. As Table III.12 shows, the number of ADL impairments, numbers of IADL impairments, a history of heart disease, cancer or stroke, and a self-rating of health as "poor" all contributed substantially to the increase in costs to HCFA. Having a history of serious illness is the most important single factor, accounting for about 38 percent of the cost increase, nearly half of the effect of the set of health measures. The importance of this measure is due to the large effect that it has on Medicare costs, and to the inability of the auxiliary regression to explain the large unadjusted difference between enrollees and nonenrollees on this measure. Interestingly, the variable indicating death within the g-month period following the interview had no effect on the cost increase to HCFA Although those who died had much higher Medicare costs (the coefficients in the FFS cost model are very large for both Part A and Part B), the mortality rate for enrollees (4.6 percent) was predicted very accurately by the auxiliary regression on AAPCC factors (the predicted mean was 4.7 percent).

Attitudes toward health care also contributed somewhat to the increased costs to HCFA, but socioeconomic characteristics had little effect. Beneficiaries who worry less about their health than others and those who avoid going to the physician generate significantly lower Medicare costs than other beneficiaries and there are higher proportions of enrollees than nonenrollees with these characteristics. The auxiliary regression underpredicts slightly the proportions of enrollees who would have these characteristics, leading to AAPCC rates that are too high. These differences account for 14 percent of the cost increase to HCFA. Differences on socioeconomic factors (education, race, income, Medigap coverage) account for only 3 percent of the difference, since none of these factors has much effect on Medicare costs. Beneficiary income is estimated to have a ***negative*** effect on both Part A and Part B reimbursements, although the coefficients are very small and not significantly

TABLE III.12

EFFECTS OF ENROLLEE CHARACTERISTICS ON DIFFERENCE BETWEEN AAPCC RATE AND PROJECTED FFS COSTS FOR ENROLLEES

	Effect on AAPCC Rate - FFS Cost	Percent of Total Difference
Health Status Indicators		
ADL impairments	$ 40.29	15.3 %
IADL impairments	$31.66	12.0 %
History of cancer, heart disease, stroke	$100.61	38.2 %
Poor health	$44.08	16.7 %
Died during g-month post-interview period	$ 2.47	0.9 %
Total effect of health status measures	**$219.11**	**83.1 %**
Attitudes Toward Health Care		
Worries about health more	$19.20	7.3 %
Avoids seeing physicians	$15.15	5.7 %
Has usual source of care	$ 2.23	0.9 %
Total effect of attitudinal variables	$36.58	**13.9 %**
Socioeconomic/Ethnic Characteristics		
Income	-$17.58	-6.7 %
Whether nonwhite	$13.23	5.0
Education	$6.16	2.3
Has Medigap coverage	$6.19	2.3
Total effect of socioeconomic variables	**$8.00**	**3.0 %**
Difference Between AAPCC and Projected FFS Cost	$263.70	100.0 %

different from zero (a $10,000 increase in income decreases predicted total reimbursements by about $26, or about 1 percent). Medigap coverage has a moderate impact on reimbursements ($292 higher for those with Medigap), but the predicted proportion of enrollees with Medigap coverage is actually over-estimated by the auxiliary regressions. Thus, differences between enrollees and nonenrollees in access to care would have been small if enrollees had not joined the HMO and these differences would have had little effect on the estimated increase in costs to HCFA.

E. IMPACTS ON COSTS FOR DIFFERENT TYPES OF HMOS AND MARKET AREAS

In addition to determining how the risk program affects costs overall, we are also interested in whether cost increases or decreases from risk contracting are different for enrollees in certain types of HMOs or market areas. Table III.13 reports the results based on the models for 1989 reimbursements.

Staff model plans, in general, have more favorable selection than IPAs and group practice model HMOs, and as a result, cost increases due to risk contracting are greater for staff plans. The increase in cost to HCFA is 4.4 percent for IPAs and Group plans, which is considerably less than the 7.8 percent cost increase for staff plans. This similar degree of favorable selection for IPAs and group plans was also found by Hill and Brown (1990).

Cost increases to HCFA for enrollees in for-profit plans (6.5 percent) are somewhat greater than the 4.5 percent increase found for not-for-profits. This difference is not sufficiently large to explain the much stronger financial performance found among for-profit plans (Shin and Brown, 1992), which suggests that the difference in financial performance is attributable to factors other than favorable selection.

Cost increases to HCFA are much greater for enrollees in HMOs charging the lowest premium. Plans charging zero premiums are paid 8.3 percent more than predicted FFS costs for their enrollees, while plans charging over $50 per month are paid only 2 percent more than the expected FFS costs

TABLE III.13

AVERAGE COSTS TO HCFA FOR ENROLLEES IN PLANS WITH DIFFERENT CHARACTERISTICS

Plan Characteristics	Sample Size	Predicted AAPCC Payment[a] (1989)	Predicted FFS Cost[b] (1989)	Cost (Savings) to HCFA	Percentage Cost (Savings) to HCFA
Overall	6,475	$2,477	$2,344	$133	5.7 %
Model Type					
IPA	2,624	$2,455	$2952	$103	4.4 %
Group	1,873	$2,306	$2,207	$98	4.4 %
Staff	1,978	$2,626	$2,435	$191	7.8 %
Tax Status					
Not-for-profit	3,030	$2,267	$2,169	$97	4.5 %
For-profit	3,445	$2,631	$2,471	$160	6.5 %
Premium (1989)					
0	1,947	$2,720	$2511	$208	a.3 %
$1 - $50	3,330	$2,399	$2,296	$103	4.5 %
> $50	1,198	$2,107	$2,067	$40	2.0 %
Enrollment Size (1/89)					
< 10,000	2353	$2.121	$2,024	$97	4.8 %
10 - 20,000	1,084	$2,209	$2,091	$118	5.6 %
> 20,000	3,038	$2,689	$2,538	$152	6.0 %
County AAPCC Rate (1989)					
c $275	1,837	$2,137	$2,010	$127	6.3 %
$275 - $325	3,008	$2,424	$2,335	$90	3.8 %
> $325	1,630	$2,787	$2,591	$196	7.6 %

‘Predicted payments are 95 percent of FFS costs as predicted by equation 11, the AAPCC model. Predicted payments are estimated for each enrollee in the sample, and arc then averaged across enrollees in plans with specific characteristics.

[b]FFS casts were predicted using the coefficient estimates presented in Table III.3 and enrollee values for the independent variables.

for their enrollees. The estimates suggest that enrolled beneficiaries gain when the payments to HMOs exceed what FFS costs would have been, in the form of lower monthly premiums.

There is essentially no relationship between enrollment size and the cost increases to HCFA. Plans with fewer than 10,000 Medicare beneficiaries are paid about 5 percent more than the expected FFS costs for their enrollees, and plans with more than 20,000 enrollees are paid about 6 percent more than the expected FFS costs for their enrollees.

Cost increases are substantially greater in counties with the highest AAPCC rates (actual rates, not our predicted payment rates), although the relationship between AAPCC rates and cost increases is not monotonic. Enrollees in counties with monthly rates exceeding $325 generated an average cost increase to HCFA of 7.6 percent relative to what HCFA would have paid under FFS. This cost increase is double the average increase of 3.8 percent observed for enrollees in counties with rates between $275$325, but only slightly greater than the average increase of 6.3 percent for counties with AAPCC rates below $275. Despite the non-monotonic relationship between the AAPCC and the increased costs to HCFA, both the predicted AAPCC payments and predicted FFS cost increase monotonically with the actual AAPCC rate.

The results from Table III.13 illustrate that the size of the impact of the risk program on costs to HCFA varies with the characteristics of the HMO. However, the table also illustrates that the program not only fails to generate the anticipated 5 percent *savings* for any group of HMOs, it increases costs to HCFA for every subgroup examined.

IV. THE IMPACT OF MEDICARE RISK PLANS ON THE USE OF SERVICES

In this chapter, we present estimates of the impact that Medicare risk plans have had on the use of hospital, primary care, skilled nursing facility, and home health services. Capitation provides Medicare risk plans with an incentive to provide care at the lowest cost possible. Reductions in cost can be achieved by reducing the units of services provided, substituting less costly services for more costly care, and by negotiating lower rates of compensation for providers. Here we explore the impact that Medicare risk plans (henceforth, HMOs) have on the units of service provided, and assess whether substitution of less costly for more costly care is occurring.

In Section A, we provide a brief discussion of HMOs' incentives and ability to reduce service use, and the implications for HCFA of their success or failure in doing so. In Section B, we describe the methodology used to estimate HMO impacts. In Section C, we present the basic results for hospital use, physician visits, and home health visits. In Section D, we present impact estimates based on alternative model specifications to illustrate the robustness of our results. In Section E, we present analyses of impacts for subgroups defined by plan characteristics, and in Section F for subgroups defined by health status. In Section G, we translate service use impacts into their likely impact on HMO expenditures for Medicare covered services.

A. INCENTIVES FOR AND IMPLICATIONS OF REDUCTIONS IN SERVICE UTILIZATION BY HMOs

The impact of Medicare risk plans on the use of services is defined as the difference between the amount of services used by HMO enrollees and what their use of services would have been had they remained in the fee-for-service (FFS) sector. We measure the HMO impact by comparing the use of services by beneficiaries enrolled in Medicare risk plans with the use of services by beneficiaries in the FFS sector, controlling for observed differences in the characteristics of beneficiaries in the two sectors.

The impact that Medicare risk plans have on service use will reflect how the financial incentives facing enrollees and HMO providers differ from the incentives faced by their counterparts in the FFS sector. On the demand side of the market, enrollees face incentives (out-of-pocket costs) that are similar to beneficiaries with relatively comprehensive Medigap coverage. That is, they face no deductibles and little or no coinsurance for Medicare-covered services. Thus, enrollees are not likely to have financial barriers constraining their demand for care. Indeed, since nearly all Medicare risk plans offer more extensive coverage than the typical Medigap policy, we might expect greater demand for some types of services by enrollees. On the supply side of the market, capitation provides HMOs with an incentive to provide care at the lowest cost that will achieve accepted standards for quality of care. Costs can be reduced by several methods, including (1) a reduction in the volume of services, (2) negotiating lower rates of compensation for providers under contract with the HMO (e.g., lower per diem hospital rates), and (3) substituting less costly for more costly services.

The combination of few constraints on the demand side and strong financial incentives 'to cut costs on the supply side is likely to result in quite different patterns of utilization than exhibited in the FFS sector. For example, the HMO's ability to reduce the number of physician visits is limited to some extent by the lack of financial barriers to obtaining care from a primary care physician and the coverage of preventive care. However, unlike their counterparts in the FFS sector, HMO physicians under capitation have an incentive to limit the number of follow-up visits for a specific health problem.

Similarly, enrollees face no financial barriers to inpatient care. The HMO has a strong incentive to reduce the number of hospitalizations and the length of stay for those hospitalized. However, reducing the hospitalization rate below the rate for the FFS sector will depend on the ability of HMOs to reduce the rate of discretionary hospitalizations and to substitute outpatient care for inpatient care more effectively than the FFS sector does. Reducing the average length of stay for patients admitted will depend on the ability of HMOs to manage the hospital stay. For example, a recent case study (Huriey, 1992) shows that many HMOs rely heavily on intensive case management of hospitalized Medicare risk enrollees to reduce the average length of stay, and believe this to be highly effective.

Thus, the impact of HMOs on the use of service will reflect both the greater access to care by enrollees compared with nonenrollees and the greater incentive to cut costs that capitation provides Medicare risk plans. Greater access to care may increase the likelihood that an enrollee seeks care, even though HMOs may reduce the total volume of services received. Similarly, by substituting less costly procedures for more expensive ones, the HMO may increase the likelihood that certain procedures are used by enrollees, even though HMOs reduce the total volume of services that enrollees receive. In estimating HMO impacts on' the use of services, it is useful, therefore, to ask the following questions:

1. What is the impact of HMOs on the probability that enrollees receive a particular service?
2. What is the impact of HMOs on the volume of specific services received by users?
3. What is the impact of HMOs on the overall volume of services used?
4. Are HMO impacts on service use consistent with expected patterns of substitution (e.g., the substitution of outpatient for inpatient care?

If Medicare risk plans are using medical resources more efficiently by reducing volume or by substituting services, the result will be a reduction in expenditures for resources devoted to the care of HMO enrollees. However, reduced HMO expenditures do not translate into direct cost-savings to the Medicare program. Indeed, under the current system of risk contracting, it is possible for Medicare risk plans to realize increased profits from this reduction in expenditures while the Medicare program actually loses money. As Chapter III described in detail, the cost-savings or losses to Medicare generated by the program are purely a function of biased selection in the program and the accuracy of the AAPCC. The impact that HMOs have on the resources used to care for its enrollees has no direct bearing on costs to HCFA in the short-run. However, the financial performance of Medicare risk plans--and therefore their willingness to continue participating in the risk program--will depend on both their ability to cut resource use and the degree of biased selection they experience. It is instructive, therefore, to translate HMO impacts on service use into likely impacts on HMO expenditures for Medicare-covered services.

B. METHODOLOGY FOR ESTIMATING PROGRAM IMPACTS

We use weighted regression analysis to estimate the impacts that Medicare risk plans have on service use. The dependent variables in the analysis are measures of hospital use, physician visits, skilled nursing days, and home health visits taken from the beneficiary survey. The independent variables thought to influence health service use are enrollment status, plus the same characteristics assumed to affect costs in Chapter III--the risk factors accounted for in the AAPCC payment methodology, financial barriers, health and functional status, medical conditions, attitudes toward health care, market area characteristics, and whether the beneficiary died in the nine month period after interview. The rationale for including this particular set of variables in the regression model, and precedents for their use found in other health services research was discussed in Section B of Chapter III. Enrollee observations are weighted to reflect their probability of selection. Nonenrollee observations are weighted to match the weighted distribution of enrollees across counties. The full list of explanatory variables, their weighted means and standard deviations are given in Table IV.l.

The general form of the regression model for estimating HMO impacts is the following:

(1) $Y_i = B'X_i + cI_i + e_i,$

where

$\mathbf{Y_i}$ = Service use (e.g., number of hospital days) for the i^{th} beneficiary

$\mathbf{X_i}$ = A vector of explanatory variables listed in Table IV.l.

TABLE IV.9 (continued)

Independent Variables	Probability of Any SNF Days[a]	Number of SNF Days[b]	Probability of One or More Home Health Visits[a]	Number of Home Health Visits[a]	Probability of One or More Visits by Nurse or Therapist[a]	Number d Visits, Nurse or Therapist[b]	Probability d One or More Visits by a Home Health Aide	Number of Visits by a Home Health Aide
Market Area Characteristics								
Metro Statistical Area >250,000	.064	-.294	.095	-.101	.122	.941	-.336	-.224
	(.632)	(.390)	(.249)	(.614)	(.155)	(.437)	(.770)	(.107)
Physicians per Capita	-.476 *	-.581	.104	.084	-.040	-.196	.151	.249
	(.066)	(.413)	(.521)	(.817)	(.808)	(.435)	(.579)	(.359)
Surgeons per Capita	2.08 **	3.66	-.518	-.401	.048	.541	-.738	-.919
	(.039)	(.183)	(.413)	(.790)	(.941)	(.568)	(.423)	(.428)
Hospital Beds per Capita	-.114 **	-242 • *	.021	.135 • *	.010	-.196	.008	.098 **
	(.011)	(.022)	(.374)	(.026)	(.648)	(.435)	(.815)	(.024)
County AAPCC Rate, Part A	.004 *	.013 **	.001	-.002	.005	-.001	.003	.008
	(.097)	(.043)	(.485)	(.645)	(.730)	(.665)	(.195)	(.751)
County AAPCC Rate, Part B	-.002	-.006	-.002	-.002	-.002	-.002	-.001	.002
	(.142)	(.155)	(.981)	(.896)	(.012)	(.161)	(.335)	(.241)
Inco[redacted]								
Race, Other Than White	-.217	.130	-.102	-.058	-.097	-.057	-.085	-.052
	(.281)	(.767)	(.355)	(.810)	(.401)	(.720)	(.571)	(.775)
Missing Race	.511	.082	.?1o ***	1.79 • *	.641 ***	.287	.643 **	1.08 *
	(.138)	(.910)	(.002)	(.028)	(.008)	(.573)	(.036)	(.060)
Income	-.007	.003	.002 • *	.002	.002 **	.003	-.005	.0001
	(.146)	(.901)	(.016)	(.428)	(.012)	(.207)	(.154)	(.900)
Missing Income	.033	-.009	-.005	.130	-.096	.003	.095	.144
	(.820)	(.929)	(.961)	(.562)	(.341)	(.931)	(.444)	(.360)
Highest Degree, College	.145	.013	-.070	.049	.008	-.109	.149	.178
	(.366)	(.924)	(.483)	(.817)	(.932)	(.451)	(.398)	(.287)
Highest Degree, High School	.082	.472 •	-.028	-.030	.017	-.051	.149	.055
	(.434)	(.083)	(.657)	(.814)	(.796)	(.607)	(.101)	(.634)
Missing Education	.025	266 **	. . 187	-.371	-.083	.095	-3.17	-.857 • *
	(.909)	(.004)	(.271)	(.496)	(.623)	(.763)	(.130)	(.023)
Enrollee, Medicare Risk Plan	.196 • *	-.150	-.053	-.471 **	-.031	-.209 **	-.136	-.276 • -•
	(.036)	(.538)	(.360)	(.001)	(.607)	(.013)	(.102)	(.005)
Mean of Dependent Variable	.0081	.653	.030	.845	.026	.408	.475	.475
R^2	--	.025	--	.107	--	.064	--	.079
N	12,077	12,077	12,215	12,215	12,251	12,251	12,257	12,257

[a]Estimated by maximum likelihood probit. Probit coefficients and their p-values (in parentheses) are reported here.

[b]OLS regression, p-values are reported in parentheses.

* Significant at .10 level, two-tailed test.
** Significant at .05 level, two-tailed test.
*** Significant at .01 level, two-tailed test.

TABLE IV.1

INDEPENDENT VARIABLES INCLUDED IN MODELS OF SERVICE UTILIZATION
(All variables are binary except where indicated)

	Enrollees		Nonenrollees		
	Mean	Standard Deviation	Mean	standard Deviation	Enrollee-Nonenrollee Difference
AAPCC Risk Factors					
Disabled, under age 65	.028	(.165)	.077	(267)	-.049 . **
Ages 6569	,227	(.419)	.217	(.412)	.010
Age 70-74	309	(.462)	.270	(.444)	.039 . **
Age 75-80	.222	(.416)	.188	(.391)	.034 . **
Age 80-84	.129	(335)	.134	(.341)	-.005
Age ≤85	.085	(.278)	.114	(.318)	-.029 . **
Male	.442	(.497)	.417	(.493)	.025 . **
Medicaid, state buy-in	.023	(.150)	.093	(.290)	-.070 . **
Nursing home resident	.018	(.133)	.058	(.234)	-.040 . **
Health Status, Medical Conditions					
In poor health	.056	(.23)	.092	(.289)	-.036 . **
Number of ADL impairments	.128	(.601)	303	(.968)	-.175 . **
Number of IADL impairments	.668	(1372)	1.093	(1.803)	-.425 . **
History of cancer, heart disease, or stroke	.274	(.446)	322	(.467)	-.048 . **
Died in follow-up period	.046	(.209)	.053	(224)	-.007 .
Preferences for Receiving Cars					
Worry about personal health more than others	.173	(378)	.200	(.400)	-.027 . **
Avoid doctor if a problem arises	.270	(.444)	.247	(.431)	-.023 . **
Have a usual place of care	.853	(.354)	.914	(.280)	-.061 . **
Other Personal Characteristics					
Minority race	.078	(268)	.067	(.250)	.011 . *
Income (dollars)	$17,689	(19296)	$20,157	(30,875)	-$2,468 . **
Education					
College degree	.118	(.323)	.149	(.356)	-.031 . **
High school, no college degree	.566	(.496)	.579	(244)	-.013 .
Less than high school (reference category)	.316	(.465)	.272	(.445)	.044 . **

TABLE IV.1 (continued)

	Enrollees		Nonenrollees		
	Mean	Standard Deviation	Mean	Standard Deviation	Enrollee-Nonenrollee Difference
Market Area Characteristics					
Live in a large MSA (population or 100,000 or more)	.809	(.437)	.810	(.392)	-.001
Number of doctors per 1,000 area residents	2.180	(.825)	2.184	(.833)	-.004
Number of surgeons per 1,000 area residents	.580	(.215)	581	(.217)	-.001
Number of acute care hospital beds per 1,000 area residents	4.059	(1.472)	4.067	(1.478)	-.006
County AAPCC rate, Part A, 1989 (dollars)	$176.44	(24.39)	3177.70	(24.63)	-$1.26 • *
County AAPCC rate, Part B, 1989 (dollars)	$135.55	(36.02)	$135.79	(35.67)	-$.24
Sample Size		6,458		6,071	

NOTE: AU of these variables were obtained from our survey except for age, disability, and sex, which were obtained from HCFA's Master Beneficiary File (for nonenrollees) or Group Health Plan Operations (GHPO) file (for enrollees), and the market area characteristics, which were obtained from the area resource file. AAPCC rates were obtained from the AAPCC master files, HCFA.

• Significant at the .10 level, two-tailed tat.
•* Significant at the .05 level, two-tailed test.
• •• Significant at the .01 level, two-tailed test.

I_i = An indicator of enrollment status equal to 1 for enrollees and zero for nonenrollees.

e_i = The error term.

B' = The vector of regression coefficients corresponding to X_i.

c = The coefficient on enrollment status.

The coefficient, c, is the measure of program impact. That is, it gives us the difference in the service use of HMO enrollees and a group of nonenrollees with the same AAPCC risk factors, health status, medical conditions, demographic characteristics, and preferences for receiving care. For each category of service use, we also estimate the beneficiary's probability of using the service; e.g., the probability of any hospital stays. For the probability models Y, is a binary variable (i.e., equal to one if hospitalized, zero otherwise) and the probability of use is estimated by maximum likelihood probit. The HMO impact is then measured as the difference between (1) the mean probability of service use and the mean predicted probability assuming beneficiaries in the sample are not enrolled ($I_i = 0$).

There are several conditions that must be met for equation (1) to be a valid estimate of HMO impacts. First, I_i (and variables in X) must be exogenous. That is, unmeasured characteristics influencing the enrollment decision cannot be related to service use. If they are, c may be a biased estimate of program impact. Second, the coefficient vector **B** must be the same for enrollees and nonenrollees. If we have a strong reason to suspect that a characteristic (e.g., poor health) effects HMO service use and FFS service use differently, then equation (1) is not the preferred model. The preferred model would allow the coefficients in **B** to differ for the HMO and FFS sectors. Service use in the HMO sector for a group of beneficiaries with the same X_i's could then be predicted by the HMO sector model and service use for the same group in FFS sector could then be predicted by the FFS model. The difference in predicted use in the HMO versus FFS sector would then measure HMO impact.

We investigate, in Section D, alternatives to the model in equation (1) which relax our assumptions on the exogeneity of I, and the equality of B for the HMO and FFS sectors. We find that in most instances, the impact estimates are quite close to those generated by equation (1).

C. RESULTS BASED ON THE BASIC MODEL

HMO impacts may well differ for the three major groups of services: (1) acute care hospitalizations; (2) physician office visits; and (3) skilled nursing facility (SNF) care and home health visits.. The previous studies reviewed in Chapter I found evidence that HMOs reduce the rate of hospitalizations and hospital length of stay (LOS). HMOs may achieve a lower rate of acute hospitalizations by substituting outpatient care for a hospital stay. Similarly, length of stay may be reduced by substituting SNF days or home health visits for days otherwise spent in a hospital. Thus, while capitation provides an incentive to reduce the use of all services, cost savings may be best-achieved by increasing the use of less costly SNF or home health care as a substitute for inpatient days.

The expected effects of HMOs on use of physician services are ambiguous. HMOs have traditionally emphasized the use of primary care as a means of reducing inpatient care through prevention or early detection and treatment of medical conditions. Typically, HMO members face lower fees for office visits than do FFS beneficiaries who lack comprehensive Medigap coverage. Thus, HMOs encourage the use of primary care with this fee policy and with their orientation to preventive care and early detection of serious illness. However, HMOs discourage high use of primary care and specialist physicians by capitating their physicians or physician groups (for IPA and group model HMOs) or by monitoring the physicians' productivity (for staff model HMOs). Given these two HMO policies with opposing effects, we have no strong expectations for the impact that HMOs may have on physician office visits.

1. HMO Impacts on Acute Care Hospital Use

Because we are interested in not only whether HMOs reduce hospital use but in how they achieve these reductions, we estimate **HMO effects** on both the frequency of hospitalizations and the intensity or duration of this use. Thus, we examine four measures of hospital use based on beneficiary survey responses:

1. The probability of any hospital use in the 12 months prior to survey interview.
2. The number of hospitalizations, during the 12 months prior to interview.
3. The number of hospital days, during the 12 months prior to interview.
4. The average length of stay (LOS) for hospitalizations, during the 12 months prior to interview.

Impacts on the probability of any hospitalizations and the number of hospitalizations should capture the effects of HMO efforts to avoid hospital stays, such as requiring. pre-admission approval of hospitalizations and substitution of other types of care. Impacts on length of stay (LOS) should capture the effects of the HMO's utilization management procedures for inpatients (e.g., discharge planning, case management) and any financial incentives the admitting physicians face to shorten stays. Impacts on hospital days should reflect the composite of HMO effects on the hospitalization rate and LOS.

Descriptive statistics computed from our survey (Table IV.2) on the frequency of hospital stays (admissions) and hospital days show that enrollees clearly made less use of hospital care than nonenrollees. Enrollees are less likely than nonenrollees to have had one or more hospital stay, 15 percent compared to 18.6 percent, a difference of about 20 percent. Among those with at least one stay, enrollees have slightly fewer stays, and spend fewer days in the hospital compared with nonenrollees. For example, only 10 percent of enrollees who were hospitalized spent more than 3 weeks in the hospital, compared with 14 percent of nonenrollees. These descriptive data suggest that HMOs may reduce the hospitalization rate and length of stay, although the differences may be due

TABLE IV.2

DESCRIPTIVE STATISTICS ON HOSPITAL USE

	Enrollee Frequencies		Nonenrollee Frequencies	
	Au Enrollees	Enrollees with One or More Hospital Stays	All Nonenrollees	Nonenrollees with One or More Hospital Stays
Number of Hospital Stays				
0	**85.0 %**	--	**81.4**	--
1	11.3	75.3 %	13.6	73.1 %
2	2.2	14.7	3.2	17.2
3	1.0	6.7	1.0	5.3
4	0.3	2.0	0.4	2.2
≥5	0.2	1.3	0.4	2.2
	100.0	100.0	100.0	100.0
Sample Size	6,459	969	6,088	1,132
Number of Hospital Days				
0	**85.0**	--	81.6	--
1-7	**9.2**	61.3	9.6	52.2
8-14	**3.1**	20.7	4.4	23.9
15-21	**1.2**	8.0	1.8	9.8
22-28	**0.5**	3.3	0.7	3.8
29-60	**0.8**	5.3	1.5	8.2
>60	**0.2**	1.4	0.4	2.1
	100.0	100.0	100.0	100.0
Sample Size	6,459	969	6,073	1,117

in whole or in part to HMOs' favorable selection. Hence, we must control for other differences between the two groups.

Our estimates yield the somewhat surprising finding that HMOs have no effect on the number or probability of hospital stays, but they do reduce the average length of stay for beneficiaries (and therefore the total number of hospital days). Table IV.3 presents the estimated effects from the regression model (column 5), along with the unadjusted mean values for enrollees and nonenrollees and the difference in means (column 3). For all of the measures, the estimated effect of HMOs is smaller (closer to zero) than the difference in means, suggesting that much of the unadjusted difference in utilization between enrollees and nonenrollees is due to favorable selection rather than to HMOs being much more efficient in delivering care. Nonetheless, the significant differences for number of hospital days and average length of stay clearly indicate that HMOs do have some effect on utilization by reducing the average length of hospital stays.

The lack of a significant HMO impact on the probability of being hospitalized and on the number of hospital stays is not consistent with previous estimates of HMO impacts showing about a 25 percent reduction in hospitalization for HMO enrollees. However, the previous studies are largely based on data for non-aged populations from the 1970's. Since that time, the rate of hospitalization has declined for both the non-Medicare and Medicare populations. For the non-Medicare population, this decline is due in part to indemnity insurers and HMOs requiring prior authorization of hospital admissions for their members and in part to technology changes affecting both sectors. For the Medicare population, the decline in admissions may reflect, in part, a substitution of outpatient for inpatient care since the advent of PPS, although the decline in admission rates began before 1983. The effect of greater scrutiny by insurers (indemnity and HMO) has probably made physicians better able to discriminate between medically necessary and unnecessary hospitalizations. Thus, the admitting practices of FFS physicians and HMO physicians are probably more similar than they were in the 1970's.

TABLE IV.3

ESTIMATED IMPACTS ON HOSPITAL USE

	Sample Size (Enrollees/ Nonenrollees)	(1) Nonenrollee Mean	(2) Enrollee Mean	(3) Enrollee-Nonenrollee Difference	(4) Enrollee-Nonenrollee Difference AAPCC Model[a]	(5) HMO Impact OLS, Basic Model[b]	(6) 95 Percent Confidence Interval[c]
Probability of One or Mote Hospitalizations	6,457/6,071	.186	.150	-.036 • ** (19.4)	-.027 • **	-.009 (-5.7)	[-.025, .003]
Hospital Stays/1,000 Beneficiaries	6,457/6,071	269	218	-51 • * (19.0)	-29 • *	6 (2.8)	[-19, 31)
Hospital Days per 1,000 Beneficiaries	6,457/6,071	2,490	1,530	-960 • ** (38.6)	-703 • **	-389 • (-16.8)	[-650, 32)
Average Length of Stay	969/1,117	9.46	7.25	-221 • ** (23.4)	-1.93 • **	-1.44 • (-16.6)	[-2.92, .04]

NOTE: The numbers in parentheses are HMO impacts expressed as a percent of the expected service use for enrollees had they remained in the FFS sector, which we estimate here by the enrollee mean for service use in the HMO sector minus the HMO impact. Thus, for hospital days per 1,000 beneficiaries, we have -309/(1,530-(-309))=-.168, or -16.8 percent. Impacts on the probability of a hospital admission were estimated with a ptobit model. Impacts on other outcome measures were estimated using ordinary least squares.

[a]Enrollee-nonenrollee differences were estimated from ptobit or least squares (OLS) regression models with enrollment status and the AAPCC risk indicators as independent variables.

[b]HMO impacts were estimated from probit or least squares (OLS) regression models with enrollment status and the full aet of variables in Table IV.1 as independent variables. Impacts from the OLS models ate measured by the coefficient on the enrollment status variable. Impacts from the ptobit models are measured as the difference between the mean probability of service use for enrollees and the mean predicted probability for enrollees, assuming they were not enrolled.

[c]The 95 percent confidence interval was calculated as the coefficient on enrollment status ± 1.96 standard errors for the coefficient.

For hospital days (per 1,000 member years) the difference between enrollees and nonenrollees drops from -960 days (nearly 39 percent of the nonenrollee mean) to -309 days--a 16.8 percent impact--after controlling for beneficiary characteristics.[1] However, the differences are still fairly sizeable (309 days per 1,000 beneficiaries) and statistically significant. The difference in average length of stay (9.5 days for nonenrollees, 7.3 for enrollees) declines from -2.2 days to -1.4 days--a 16.6 percent impact after characteristics are controlled for. Again, this is a sizeable difference, but very consistent with recent estimates from the literature (Stem et al., 1984, and Bradbury, Colec, and Stearns, 1991).

The significant HMO impact on average LOS is somewhat surprising, given that Medicare's Prospective Payment System (PPS) has created the incentive for hospitals serving non-HMO Medicare patients to reduce their LOS. Indeed, we might argue that LOS should be similar for enrollees and nonenrollees with comparable characteristics: DRG payments are, in effect, a capitation payment for the stay, and hospitals realize higher margins if they reduce length of stay. However, FFS *physicians* are not capitated and have no financial incentive to reduce LOS. Since the admitting physician has ultimate control over when the patient is discharged, this may mitigate the strong hospital incentive provided by PPS to reduce LOS. HMO physicians on the other hand are more likely to face capitation or profit-sharing and, hence, have an incentive to reduce hospital. length of stay. Thus, the financial incentives to reduce length of stay are present for both the physician under contract to the HMO and the HMO. This may account for the ability of Medicare risk plans to realize a 16.6 percent reduction in LOS compared to FFS providers.

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[1]The reader should note that the base of comparison for impacts expressed in percentage terms is the actual enrollee mean minus the HMO impact estimate. This base, since it nets out the HMO impact from the observed enrollee mean, yields an estimate of what enrollee use would have been in the FFS sector. Hence, the percentage HMO impact tells us the HMO impact on enrollee use compared with what enrollee use would have been in the FFS--the exact measure we desire.

2. The Contribution of Biased Selection to Enrollee-Nonenrollee Differences in Hospital Use

Because the AAPCC methodology adjusts payments to HMOs for differences between enrollees and area nonenrollees on some characteristics, it is not appropriate to infer that the difference between the ***unadjusted*** enrollee-nonenrollee difference and the estimated impact of HMOs is due solely to biased selection. That is, to the extent that the enrollee-nonenrollee difference in utilization can be explained by enrollee-nonenrollee differences in the risk factors incorporated in the AAPCC payment (e.g., age, sex, disability status), the payments to HMOs will be lower, reflecting these differences. Therefore, a useful exercise for assessing the implications of the estimated HMO impacts for HCFA and for HMOs is to determine what proportion of the unadjusted difference in means can be attributed to each of three factors:

1. The difference in the distribution of enrollees and nonenrollees across AAPCC risk classifications.

2. Other enrollee-nonenrollee differences in health status, medical conditions, and preferences for care, which influence service use, but which are not captured by differences on AAPCC risk indicators (favorable selection).

3. The impact of HMOs on service use.

To determine how much of the unadjusted difference between enrollee and nonenrollee use can be explained by differences in AAPCC risk indicators alone, we estimate the following model:

$$(2)\quad Y_i = A'X_i^* + dI_i + e_i,$$

where, $\mathbf{X_i^*}$ is a vector of the AAPCC risk characteristics listed in Table IV.1 (a subset of the variables in X_i), and **A is** the vector of regression coefficients for these variables. $\mathbf{Y_i}$, I,, and $\mathbf{e_i}$ have the same meanings as in equation (1). The coefficient on enrollment status, **d, is** the enrollee-nonenrollee difference in service use, controlling for enrollee-nonenrollee differences in the AAPCC risk factors. Equation (1), our impact model with the full set of independent variables from Table IV.1, controls for both the set of risk factors and other characteristics that are likely to influence utilization and are known to differ for enrollees and nonenrollees. The remaining difference in service use of enrollees and nonenrollees--the coefficient c in equation (1)--is attributed to the impact of HMOs.

We expect that as we control for more characteristics that affect utilization, the enrollee-nonenrollee differences in hospital use will diminish. This expectation follows from enrollee-nonenrollee differences in AAPCC risk indicators and other measures of health and medical conditions listed in Table IV.l. Those differences suggest that enrollees should use fewer services than nonenrollees because they are at lower risk or are less likely to seek care. For example, enrollee-nonenrollee differences in AAPCC risk indicators suggest a lower level of use because enrollees tend to be younger, and are less likely to be disabled, a nursing home resident, or a Medicaid recipient. Thus, compared to the unadjusted enrollee-nonenrollee difference in hospital use, the enrollee-nonenrollee difference should decline in our AAPCC model (equation 2). However, we do not expect the estimated difference from the AAPCC model to decline as much as the full model estimates, because enrollees are also less likely to report poor health, impairments on independent activities of daily living (IADLs), impairments on activities of daily living (ADLs), and a history of heart disease, cancer, or stroke. Enrollees are also less likely to show a preference for seeking care. That is, they are less likely to worry about their health, they are more likely to avoid seeing a doctor if a health problem arises, and they are less likely to have had a regular health care provider in the past. All of these differences imply lower service utilization (see Table IV.4), leading to larger estimated differences between enrollees and nonenrollees when these factors are not controlled for. The difference **d-c** between the coefficients on enrollment status in equation 2 (the AAPCC model) and equation 1 (the impact model), therefore reflects biased selection; i.e., the additional portion of the enrollee-nonenrollee difference in service use that cannot be explained by enrollee-nonenrollee differences on AAPCC risk factors alone, but is not attributable to the effects

TABLE IV.4

REGRESSION RESULTS: HOSPITAL USE

Independent Variables	Probability of One or More **Hospitalizations**[a]	Number of Hospital **Stays**[b]	Number of Hospital **Days**[b]	Average Length of **Stay**[b]
Intercept	-1.78 • ** (.000)	-.142 ** (.012)	-221 • ** (.004)	4.46 (.176)
AAPCC Risks				
Age 65 - 69	.127 • * (.026)	.120 • ** (.000)	1.21 • ** (.001)	.134 (.886)
Age 70 - 74	.108 • * (.049)	.115 • ** (.000)	1.36 • ** (.000)	1.70 (.194)
Age 75 - 79	.151 • ** (.007)	.118 • ** (.000)	1.21 • ** (.001)	.572 (.671)
Age 80 - 84	.098 (.110)	.081 • ** (.002)	.877 • * (.015)	.532 (.706)
Medicaid Buy-in	-.036 (.558)	-.009 • (.746)	-1.08 ••• (.006)	-2.99 •• (.037)
Disabled	.161 • * (.031)	.226 • ** (.000)	3.03 • ** (.000)	4.67 • ** (.008)
Institutionalized	-.078 (.334)	-.087 • * (.026)	.046 (.890)	1.28 (.465)
Sex (Male)	.159 • " (.000)	.044 • ** (.001)	.527 • '* (.003)	1.10 (.123)
Health/Functional Status				
ADL Impairments	-.005 (.834)	.027 • * (.033)	391 • ** (.010)	.704 (.139)
IADL Impairments	.123 • ** (.000)	.052 • ** (.000)	.610 • ** (.000)	.890 • ** (.002)
Poor Health	.300 · · · (.000)	.298 • ** (.000)	1.92 • ** (.001)	-.460 (.681)
Missing Value, Poor Health	.499 ••• (.000)	.279 • ** (.000)	3.35 • ** (.000)	-.231 (.899)
History of Heart Disease., Cancer, Stroke	.449 • ** (.000)	.182 • ** (.000)	1.57 • ** (.000)	.893 • (.226)
Missing Value, Heart Disease, Cancer, Stroke	.703 • ** (.000)	.432 • ** (.000)	6.61 • ** (.000)	7.98 • (.061)
Died Within 9 Months of Interview	.378 ••• (.000)	.333 • ** (.000)	3.07 ••• (.000)	-.640 (.630)
Preferences for Seeking Care				
Worry About Health	.136 • ** (.000)	.065 • ** (.000)	.847 • ** (.000)	.757 (.388)
Missing, Worry About Health	-.156 • (.084)	-.084 • ' (.031)	-.673 (.705)	.377 (.852)

TABLE IV.4 (continued)

Independent Variables	Probability of One or More Hospitalizations[a]	Number of Hospital Stays[b]	Number of Hospital Days[b]	Average Length of Stay[b]
Avoid Seeing Doctor, if Problem	-.059 • (.075)	-.021 (.142)	-.229 (.243)	-.240 (.772)
Missing, Avoid Doctor	.020 (.862)	-.023 (.654)	.080 (.875)	-.234 (.452)
Usual Place of Care	.150 • ** (.002)	.051 • ** (.009)	.160 (.556)	-205 (.106)
Missing, Usual Place of Care	.113 (.431)	-.086 (.216)	3.72 • ** (.000)	-4.46 (.114)
Market Area Characteristics				
Metro Statistical Area >250,000	-.059 (.178)	-.005 (.758)	-.128 (.607)	.076 (.900)
Physicians per Capita	-.029 (.758)	-.034 (.360)	.485 (.340)	3.33 (.105)
Surgeons per Capita	-.015 (.963)	.081 (.588)	-215 (.282)	-11.2 (.169)
Hospital Beds per Capita	.027 (.033)	.007 (.192)	.093 (.225)	-.072 (.807)
County AAPCC Rate, Part A	.001 (.136)	.0006 (.074)	.007 (.167)	.007 (.712)
County AAPCC Rate, Part B	-.0004 (.382)	-.004 (.081)	-.0008 (.782)	.0003 (.930)
Income/Education/Race				
Race, Other Than White	.001 (.980)	-.021 (.396)	.252 (.454)	270 • (.053)
Missing Race	.535 (.001)	.391 (.000)	5.66 (.000)	4.72 (.202)
Income	-.0006 (.335)	-.0003 (.242)	-.006 (.070)	-.018 (.128)
Missing Income	-.089 (.065)	-.044 (.029)	-.416 (.127)	-.353 (.775)
Highest Degree, College	.086 (.076)	.049 (.022)	.766 (.009)	2.48 * (.040)
Highest Degree, High School	.029 (.390)	.006 (.685)	.239 (.228)	1.08 (.200)
Missing Education	-.164 (.111)	-.637 (.169)	-.367 (.571)	.126 (.914)
Enrollee, Medicare Risk Plan	.044 (.131)	.006 (.631)	-.309 * (.072)	-1.44 • (.053)
Mean of Dependent Variable	.167	.243	1.99	8.44
R^2		.11	.07	.05
N	12.528	12.528	12.528	2.086

TABLE IV.4 (continued)

"Estimated by maximum likelihood probit. Probit coefficients and their p-values (in parentheses) are reported here.

[b]OLS regression, p-values are reported in parentheses.

* Significant at .10 level, two-tailed test.
** Significant at .05 level, two-tailed test.
*** Significant at .01 level, two-tailed test.

of the HMO. The closer the estimates of **d** and c, the less the extent of biased selection and the fairer the payment methodology.

The results in Table IV.3 bear out our expectations. For each of the outcome measures, the enrollee-nonenrollee differences when only the AAPCC risk factors are controlled for (Column 4) are smaller than the unadjusted differences (column 3), but the differences remain statistically significant and sizeable. For the probability of hospital admission the estimated enrollee-nonenrollee difference drops from -.036 to -.027, a decline of one-fourth, when AAPCC factors are controlled for. But when the full set of characteristics is controlled for, the difference drops to only about -.01 and is not significantly different from zero. Thus, the AAPCC factors account for only one-fourth of the enrollee-nonenrollee difference in the probability of a hospital stay, and HMO effects account for only about one-fourth. Favorable selection therefore accounts for the lion's share of the unadjusted difference in the probability of a hospital admission.

The results for number of hospital stays closely parallels the findings for the probability of any admission, not surprisingly. The AAPCC factors account for about 43 percent of the difference observed, but again favorable selection accounts for 45 percent. There is no evidence that HMOs reduce the number of admissions. The estimated effect is very small and statistically insignificant.

For hospital days we again see that AAPCC factors can explain about one-fourth of the enrollee-nonenrollee difference in means. Adding the other characteristics to the regression model accounts for another 40 percent of the enrollee-nonenrollee differences. The remaining unexplained difference between enrollees and nonenrollees, 309 days per 1,000 members, is our estimate of HMOs' effect on hospital days--about one-third of the raw difference in means between the two groups. Again, biased selection--the proportion of the difference between the two groups that is explained by characteristics beyond those accounted for by the AAPCC--accounts for over 40 percent of the observed difference in means between enrollees and nonenrollees.

The results for length of stay suggest that the enrollees who were hospitalized were less seriously ill than hospitalized nonenrollees on average, but that HMO effects account for most of the observed enrollee-nonenrollee difference in mean length of stay. AAPCC factors explain about 13 percent of the observed differences. Favorable selection (differences on other characteristics) accounts for 22 percent, and the HMO effect accounts for the remaining 65 percent of the 2.2 day unadjusted difference in mean length of stay.

3. Impacts on Physician Use

Although hospital use accounts for the majority of the costs to Medicare and to HMOs, costs of physician services are also sizeable. We examine several measures of physician use in our analysis: (1) the number of physician visits in the past 4 weeks,[2] (2) whether a physical exam was received in the past year, and (3) whether the beneficiary usually receives: one or more physician visits a year, three or more physician visits a year, or twelve or more physician visits a year.

HMOs encourage preventive care through the primary care physician by charging lower fees (or no fees) for office visits, but discourage frequent office visits by capitating their physicians, withholding a part of their compensation subject to satisfying utilization targets, or profit-sharing. Hence, we might expect to see a higher proportion of HMO enrollees with physical exams but a lower proportion of enrollees with frequent office visits when compared to nonenrollees. Indeed, the unadjusted means for enrollees and nonenrollees reported on Table IV.5 reveal this pattern. About the same percentage of enrollees and nonenrollees report some physician visits in the past 4 weeks (33.5 percent of nonenrollees) and report seeing a doctor at least once a year (88.9 percent of enrollees versus 87.6 percent of nonenrollees). Enrollees were (slightly) more likely to report receiving a physical exam last year (70.4 percent of enrollees versus 68.2 percent of nonenrollees).

[2] Survey respondents were only expected to have accurate recall of the number of physician visits for the time period immediately before the interview. Hence, the survey asked the frequency of visits in the past 4 weeks.

TABLE IV.5

DESCRIPTIVE STATISTICS, PHYSICIAN VISITS

	Percent of:	
	Enrollees	Nonenrollees
Number of Visits, Past 4 Weeks		
0	66.6	65.8
1	22.8	22.7
2	6.3	6.7
>2	4.3	4.8
	100.0	100.0
Usual Frequency of Visits, Each Year		
0	11.1	12.3
1 or 2	30.5	29.5
3-11	45.8	40.0
≥12	12.6	18.2
	100.0	100.0
Physical Exam, Past Year		
Yes	70.4	68.2
No	29.6	31.8
	100.0	100.0

NOTE: N = 6,448 for enrollee
N = $336 for nonenrollees

However, enrollees were substantially less likely to see a physician 12 or more times a year (12.6 percent of enrollees versus 18.2 percent of nonenrollees).

The impact estimates, obtained by controlling for differences in patient characteristics, reveal a pattern very consistent with expectations. Impacts are given in Table IV.6, column 5; coefficients on all of the variables in the models are presented in Table IV.7. HMOs increase slightly the likelihood that beneficiaries had a recent visit and the likelihood that beneficiaries have at least occasional or periodic visits, but they decrease (slightly) the likelihood that the beneficiaries have had frequent visits (an average of one or more per month). The differences are not large, but they are statistically significant and suggest that HMOs increase access to care (by eliminating or reducing the out-of-pocket cost for visits and by encouraging preventive care, such as free annual checkups), but they also decrease the proportion of patients making frequent visits to their doctor by discouraging such behavior. HMOs also increase the likelihood (by about 6 percentage points on average) that a beneficiary will have had a physical exam in the past year (just over two-thirds of nonenrollees reported having exams). Note that the estimated effect on number of visits in the past 4 weeks was small and not statistically significant.

As indicated above, the estimated effects are small. The probability of having one or more visits in the past week, about 35 percent for nonenrollees, was increased by 2 percentage points on average by the HMO. Similarly, the probability of having at least periodic visits (3 or more a year) was increased by 6 percentage points for HMO members, a modest increase relative to the 58 percent probability for nonenrollees of having at least periodic visits. The proportion of beneficiaries who "almost never" visit the doctor (the complement of whether they have at least an occasional visit) is 12.4 percent. HMOs reduce this probability by 5.2 percentage points, a decease of over one-third in percentage terms.

TABLE IV.6

ESTIMATED IMPACTS ON PHYSICIAN VISITS

		(1)	(2)	(3)	(4)	(5)	(6)
	Sample Size (Enrollee/ Nonenrollee)	Nonenrollee Mean	Enrollee Mean	Enrollee-Nonenrollee Difference	Enrollee-Nonenrollee Difference AAPCC Model[a]	HMO Impact[b]	95 Percent Confidence Interval[c]
Whether Had One or More Visits, Past 4 Weeks	(6,427/6,013)	.345	.335	-.010	-.008	.019 ** (6.2)	[.002, .037]
Number of Visits, Past 4 Weeks	(6,427/6,013)	.690	.596	-.106 ***	-.054	.026 (4.6)	[-.049, .101]
Whether Have at Least Occasional Doctor Visits (1 or More a Year)	(6,384/6,028)	.876	.889	.013	.016 ***	.052 *** (6.2)	[.036, .069]
Whether Have at Least Periodic Doctor Visits (3 or More a Year)	(6,384/6,028)	.582	.584	.002	.022 **	.060 *** (11.5)	[.040, .060]
Whether Have Frequent Doctor Visits (12 or Mote a Year)	(6,384/6,028)	.181	.126	-.055 ***	-.029 ***	-.016 ** (-11.4)	[-.030, -.003]
Whether Had Physical Exam, Last year	(6,399/6,037)	.682	.704	.022 ***	.026 ***	.058 *** (9.0)	[.040, .077]

NOTE: The numbers in parentheses are HMO impacts expressed as a percent of the expected service use for enrollees had they remained in the FFS sector, which we estimate here by the enrollee mean for service use in the HMO sector minus the HMO impact.

[a] Enrollee-nonenrollee differences were computed fmm pmbit models (regression models for the number of visits variable), with enrollment status and the AAPCC risk as independent variables.

[b] HMO impacts were computed from pmbit models (regression models for the number of visits measure), with enrollment status and the full list of independent variables on Table IV.1 discussed in the text. Impacts from the OLS models ate measured by the coefficient on the enrollment status variable. Impacts fmm the pmbit models ate measured as the difference between the mean probability of service use for enrollees and the mean predicted probability for enrollees, assuming they were not enrolled.

[c] The 95 percent confidence interval was calculated as the coefficient on enrollment status ±1.96 standard errors for the coefficient.

TABLE IV.7

REGRESSION RESULTS: PHYSICIAN USE

Independent Variables	Probability of Any Visits, Past 4 Weeks	Number of Visits, Past 4 Weeks	Probability of:[a] One or More Visits Per Year	Three or Morn Visits Per Year	Twelve or More Visits Per Year	Physical Exam Last Year
Intercept	-1.04*** (.000)	.011 (.903)	082 (.574)	-.942*** (.000)	208*** (.000)	,912*** (.000)
AAPCC Risks						
Age 65 - 69	.058 (.286)	206 (.775)	.026 (.697)	-.033 (.510)	.117* (.051)	113** (.024)
Age 70 - 74	.050 (.346)	.002 (.929)	.053 (.422)	.011 (.819)	.160*** (.005)	.131*** (.007)
Age 75 - 79	.050 (.312)	.0003 (.944)	.060 (.376)	.088* (.073)	.168*** (.004)	.095* (.055)
Age 80 - 84	.069 (.182)	-.103 (.183)	.033 (642)	.059 (.255)	.164*** (.007)	.036 (.489)
Medicaid Buy-m	.136*** (.013)	.115 (.169)	.231*** (.006)	.208*** (.000)	.305*** (.000)	.069 (.223)
Disabled	.177*** (.000)	.247** (.016)	.066 (.504)	.123* (.081)	.343*** (.000)	.109 (.119)
Institutionalized	-.393*** (.000)	.015 (.867)	.230** (.045)	.170** (.039)	.496*** (.000)	.237*** (.004)
Sex (Male)	-.048** (.050)	-.030 (.423)	-.078** (.019)	-.054** (.027)	-.014 (.636)	-.027 (.276)
Health/Functional Status						
ADL Impairments	-.089** (.000)	.106*** (.002)	-.156*** (.000)	-.083*** (.000)	-.031 (.170)	.018 (.415)
IADL Impainnents	.061***(.000)	.035** (.043)	.097*** (.000)	.109*** (.000)	.091*** (.000)	.054*** (.000)
Poor Health	.286*** (.000)	.437*** (.000)	.094 (.251)	,364*** (.000)	.489*** (.000)	-.078 (.149)
Missing Value, Poor Health	.082 (.527)	.815*** (.000)	-.249 (.141)	.018 (.894)	.261* (.069)	.054 (.688)

TABLE IV.7 (continued)

Independent Variables	Probability of Any Visits, Past 4 Weeks	Number of Visits, Past 4 Weeks	Probability of:[a] One or Mote **Visits** Per Year	**Three** or More Visits Per Year	Twelve or More Visits Per Year	Physical **Exam** Last Year
History of Heart Disease, Cancer, stroke	.309 • ** (.000)	.298 • ** (.000)	.497 • ** (.000)	.454 • ** (.000)	.246 *** (.000)	.206 • ** (.000)
Missing Value, Heart Disease, Cancer, **Stroke**	.803 • ” (.000)	.922 • *’ (.000)	.860 *** (.002)	.780 • ** (.000)	.221 (244)	.545 • ** (.004)
Died Within 9 Months of **Interview**	.073 (236)	.188 ** (.043)	.104 (.269)	.283 • ** (.000)	.319 • ** (.000)	-.032 (.617)
Preferences for **Seeking** Care						
Worry About Health	.198 • ** (.000)	.141 • ‘* (.004)	.475 • ** (.000)	.420 • ** (.000)	.389 • ** (.000)	.279 • ** (.000)
Missing, Worry About Health	.163 • ** (.030)	-.008 (.942)	.390 *** (.001)	.331 • ** (.000)	.364 • ** (.000)	.033 (.665)
Avoid Seeing Doctor, if Problem	-.224 *** (.000)	-.121 • ** (.004)	-.556 • ** (.000)	-.395 • ** (.000)	-.170 • ** (.000)	-0.351 *** (.000)
Missing, Avoid Doctor	-.305 • ** (.005)	-510 • ** (.001)	-.431 • ** (.001)	-.239 • * (.020)	-.121 (.301)	-.326 • ** (.001)
Usual Place of **Care**	.378 • ** (.000)	.218 • ** (.000)	1.06 • ** (.000)	.600 • * (.000)	.237 • ** (.000)	.540 • ** (.000)
Missing, Usual Place of Care	-.084 (549)	.688 • ** (.001)	-.516 • * (.011)	-.181 (.290)	-.418 • (.065)	-.122 (.399)
Market **Area Characteristics**						
Metro Statistical **Area > 250,000**	-.037 (288)	-.048 (.368)	-.360 (.447)	-.009 (.805)	.029 (.506)	-.071 • * (.043)
Physicians per Capita	.050 (.484)	-.024 (.809)	.054 (.580)	-.027 (.709)	-.037 (682)	.061 (.401)
Surgeons per Capita	-.209 (.458)	.075 (.839)	-.184 (.629)	.030 (.903)	.030 (.933)	-.402 (.159)
Hospital Beds per Capita	.010 (.349)	-.021 (.209)	.010 (.500)	.012 (.260)	.010 (.481)	.050 • ** (.000)

TABLE IV.7 **(continued)**

Independent Variables	Probability of Any Visits, Past 4 Weeks	Number of Visits, Past 4 Weeks	Probability of:[a] One or More Visits Per Year	Three or More Visits Per Year	Twelve or More Visits Per Year	Physical Exam Last Year
County AAPCC Rate, Part A	-.0005 (.457)	.002 • (.086)	.0004 (.660)	.0005 (.508)	.0003 (.713)	.0002 • ** (.001)
County AAPCC Rate, Part B	.0007 (.101)	-.0002 (.755)	.0005 (.408)	.0002 • ** (.000)	.030 • ** (.000)	.001 • ** (.001)
Income/Education/Race						
Race, Other Than White	-.006 (.89)	-.115 (. 102)	.069 (.295)	.1689 *** (.000)	.143 •** (.007)	.047 (.322)
Missing Race	.016 (.912)	.152 (.500)	.161 (.433)	-.084 (.581)	.107 (.545)	-.028 (.797)
Income	.0007 (.125)	.004 *** (.000)	-.008 (.139)	-.002 •** *.005)	-.003 *** (.003)	.003 (.529)
Missing Income	-.034 (.381)	.051 (.393)	-.177 *** (.000)	-.206 • ** (.000)	-.031 (.523)	.010 (.797)
Highest Degree, College	.187 •** (.000)	.106 • (.089)	.122 • * (.027)	-.027 (.517)	-.032 (.542)	.174 •** (.000)
Highest Degree, Hi School	.108 • ** (.000)	.011 (.791)	.128 • ** (.001)	.008 (.787)	-.077 • * (.024)	.107 • ** (.000)
Missing Education	.403 • ** (.000)	-.301 • * (.028)	-.372 • ** (.001)	-.158 • (.087)	-.105 (.315)	.158 • (.100)
Enrollee, Medicare Risk Plan	.054 ** (.027)	.026 (.500)	.240 *** (.000)	.152 *** (.000)	-.075 ** (.014)	.162 • ** (.000)
Mean of Dependent Variable	.340	.641	.883	.583	.153	.694
N	12,441	12,441	12,441	15413	15413	15436

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TABLE IV.7 (continued)

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[a] Estimated by maximum likelihood probit. Probit coefficients and their p-values (in parentheses) are reported here.

[b] OLS regression, p-values are reported in parentheses.

* Significant at .10 level, two-tailed test.
** Significant at .05 level, two-tailed test.
***Significant at .01 level, two-tailed test.

At the other extreme of the utilization scale, it is less likely that beneficiaries will have frequent visits if they belong to an HMO. HMOs reduce the probability of having an average of one or more visits per month by about 1.6 percentage points. For enrollees this translates into an 11.4 percent reduction in the proportion with frequent use, compared to the proportion who would have had frequent use had they been in the FFS sector.

Part of the small *unadjusted* difference between enrollees and nonenrollees in the proportion of heavy users of service is attributable to favorable selection. Controlling only for AAPCC factors, the difference between enrollees and nonenrollees shrinks to nearly half its size, from -5.5 percentage points to -2.9 percentage points. Since the impact (column 5), based on controlling for all of the available beneficiary characteristics, is -1.6 percentage points, we infer that about half of the difference in the proportion of heavy users not reflected in the payment mechanism is due to favorable selection and half is due to the cost-cutting influence of the HMO.

Enrollees are healthier and have other characteristics associated with lower use of physician services, but are just as likely as nonenrollees to use physician services regularly. Thus, the impact estimates, which control for these differences, are substantially larger than the unadjusted differences. The importance of controlling for characteristics beyond those available from the Medicare system is evident from the difference between column 4 and column 5. Controlling only for AAPCC factors, for example, would yield an estimate that enrollees were no more likely than nonenrollees to have had a physician visit in the past 4 weeks. However, controlling for other factors as well, we find a statistically significant effect indicating enrollees are 2 percentage points more likely than nonenrollees to have had a visit.

4. **Impacts on the Use of Home Health and SNF Services**

The reduction in hospital length of stay for enrollees may be achieved, in part, by substituting care in a skilled nursing facility or care provided in home health visits for care otherwise received in an inpatient setting. If this substitution is occurring, enrollee use of a SNF or home health services should increase relative to nonenrollees. However, unlike FFS providers HMOs have a financial incentive to limit the number of SNF days and home health visits received.[3] Thus, enrollees may be more likely to receive some SNF care or home health care as a substitute for acute care but may receive fewer services overall when compared to nonenrollees with similar characteristics.

We estimate SNF days from the survey data as all reported nursing home days for all beneficiaries not identifying their place of residence as "nursing home." By eliminating nursing home residents from the sample, we delete almost all beneficiaries reporting more than 180 nursing home days. The resulting mean number of SNF days per 1,000 beneficiaries from the survey data for nonenrollees in our sample is close in value to the mean for SNF days computed from MADRS data: 895 for MADRS versus 863 from the survey. Table IV.8 summarizes the unadjusted and regression-adjusted enrollee-nonenrollee differences in SNF days and home health visits. Detailed regression results are presented in Table IV.9.

Our estimates (column 5, Table IV.8) indicate that HMOs reduce SNF use by 24.4 percent. While this impact is not statistically significant, it is a substantial reduction considering the incentive for HMOs to substitute SNF days for hospital days and the estimated reduction in average length of hospital days. The unadjusted difference in means between the two groups is - 399 days per 1,000 beneficiaries, a difference of 46 percent. Controlling for AAPCC factors alone reduces the difference only slightly; controlling for the full set of characteristics yields the impact of 150 fewer SNF days per 1,000 beneficiaries.

The large fraction of beneficiaries with no SNF use--about 99 percent--suggests that two alternative estimators, tobit and the two-part model (Duan et al., 1983), may be more appropriate than OLS for estimating impacts on SNF use. Of the two alternatives, we prefer the two part model, since it imposes no constraints on the relationship between estimates of the HMO impact on the

[3]FFS providers are compensated on a per diem basis for SNF days and on a per visit basis for home health care and, hence, have no incentive to limit services.

TABLE IV.8

ESTIMATED IMPACTS ON SNF DAYS AND HOME HEALTH VISITS

	(1) Nonenrollee Mean	(2) Enrollee Mean	(3) Enrollee-Nonenrollee Difference	(4) Enrollee-Nonenrollee Differences, AAPCC Model[a]	(5) HMO Impact, OLS, Basic Model[b]	(6) 95 Percent Confidence Interval[d]	(7) Sample Size (Enrollee/ Nonenrollee)
Percent with One or Mote SNF Stay Last Year	0.8	0.8	0.0	0.06	0.3 • * (74.0) [c]	[0.03, 0.54]	N = 12,077 (6,350/5,727)
Number of SNF Days per 1,000 Beneficiaries	863	464	-399 • **	-351	-150 (-24.4)	[-620, 320]	N = 12,077 (6,350/5,727)
Percent with One or More Home Health Visit, Past 3 Months	3.8	2.2	-1.6 • **	-1.0 • **	-0.3 (-11.8)	[-1.0, 0.3]	N = 12,215 (6,392/5,823)
Total Number of Home Health Visits per 1,000 Beneficiaries, Past 3 Months	1,324	408	-916 • **	-680 • **	-471 •** (-536)	I-743, -199]	N = 12,215 (6,392/5,823)
Percent with One or More Home Visit by a Nurse or Therapist, Past 3 Months	3.2	20	-1.2 •••	-1.0 • **	-0.2 (-7.2)	[-0.8, 0.4]	N = 12,251 (6,407/5,844)
Total Number of Home Visits, by Nurse or Therapist per 1,000 Beneficiaries, Past 3 Months	626	209	-417 • **	-277 • '*	-209 •* (-50.0)	[-375, -43]	N = 12,251 (6,407/5,844)
Percent with One or Mote Home Visit by An Aide, Past 3 Months	1.9	0.8	-1.1 • **	-0.7 •••	-0.4 • (-30.5)	[-0.9, 0.1]	N = 12,256 (6,408/5,848)
Number of Home Visits by Aide, per 1,000 Beneficiaries, Past 3 Months	767	209	-558 • **	-431 • **	-276 • ** (-56.9)	[-470, -82]	N = 12,256 (6,408/5,848)

'Enrollee-nonenrollee differences were estimated from probit or OLS regression models with enrollment status and the AAPCC risks as independent variables.

[b]HMO impacts were estimated from probit or OLS regression models with enrollment status and the full set of variables on Table IV.1 as independent variables Impacts from the OLS models ate measured by the coefficient on the enrollment status variable. Impacts from the probit models ate measured as the difference between the mean probability of service use for enrollees and the mean predicted probability for enrollees, assuming they were not entolled.

[c]The numbers in parentheses ate HMO impacts expressed as a percent of expected service use for enrollees in the FFS sector. This is the enrollee mean for service use in the HMO sector minus the HMO impact.

[d]The 95 percent confidence interval for all estimates was calculated as the coefficient on enrollment status ± 1.96 standard errors for the coefficient.

'Sample Sizes in parentheses are for enrollees/nonenrollees.

* Significant at the .10 level, two-tailed lest.
** Significant at the .05 level, two-tailed test.
• ** Significant at the .01 level, two-tailed test.

probability of any SNF use and the HMO impact on the level of service use. (In the tobit model, only one set of coefficients is estimated to predict both the probability that use is greater than zero, and the level of use for users.) The estimated impact from the two-part model, a 1.3 percent reduction in SNF use, is substantially less than the 24.4 percent reduction from the OLS model.[4] The virtually zero HMO impact on SNF days from the two-part model, along with the statistically insignificant (though large) reduction in SNF days found in the OLS model, suggest that an HMO impact on SNF days cannot be demonstrated.

While HMOs do not have a significant impact on SNF days, they do have a positive and significant impact on the probability of using a SNF. As Table IV.8 illustrates, the unadjusted proportions of enrollees and nonenrollee with some SNF days are about equal. After adjusting for AAPCC risk factors, the proportion of enrollees with some SNF days is still about the same as nonenrollees. However, after controlling for all of the characteristics in our impact model, we find that enrollees are considerably more likely to have a SNF stay. Unlike the negative insignificant impact on SNF days from the basic model, this positive impact is statistically significant at the .05 level. The greater incidence of SNF use among enrollees is consistent with HMOs reducing hospital LOS by substituting less costly SNF care for acute care. The effect is quite large in percentage terms (because so few enrollees use SNF care) but is small in absolute terms (3 tenths of one percentage point).

For the 3 month interval prior to the survey interview survey respondents were asked the number of home health visits received from a nurse, therapist, or aide. Survey respondents were also asked what specific tasks were performed by each home care provider (e.g., assistance with medical care, therapy, meal preparation, and household chores). We found that almost all personnel identified as nurses or therapists performed medical assistance or therapy. Similarly, less than 35 percent of home

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[4]The two-part model and results from the model are explained in greater detail in Section D, where we compare for all measures of service use the OLS estimates with estimates from alternative models.

health aides were cited as performing medical assistance or therapy. This pattern of responses for services provided by type of home health workers suggests that respondents were able to discriminate between nurses, therapists, and aides. From the survey responses, we estimated the following 6 measures of home health use from the survey data:

1. Whether the beneficiary received home health care by a skilled nurse or therapist.
2. Whether the beneficiary received care from a home health aide.
3. Whether the beneficiary received care from either nurse, therapist, or aide.
4. The number of visits by nurses and/or therapists.
5. The number of visits by home health aides.
6. The total number of visits received from nurses, therapists, and aides.

We find that HMOs have no effect on the likelihood that beneficiaries receive any home health visits. Nonenrollees were significantly more likely than enrollees to report one or more home health visits (3.8 percent versus 2.2 percent), but this 1.6 percentage point difference declines to -1.0 percent after controlling for AAPCC risks (column 4, Table IV.8) and further declines to -0.3 percentage points after controlling for all variables in our impact model. This impact is small and not significantly different from zero, suggesting that enrollees are just as likely to have home health visits as nonenrollees with the same characteristics.

On the other hand, HMOs substantially reduce the amount of home health care provided. The enrollee-nonenrollee difference in the mean number of home health visits per 1,000 beneficiaries is substantial: -916 per 1,000 beneficiaries. Controlling for enrollee-nonenrollee differences in AAPCC risk indicators reduces this difference to -680 visits per 1,000 (Table IV.8, column 4), and this difference is further reduced to -471 per 1,000 in our impact model. This impact is statistically significant and suggests that Medicare risk plans reduce home health visits by 53.6 percent relative to what they would have been in the fee-for-service sector.

The estimated impacts are generally similar, regardless of whether we are examining effects on **"skilled** care" (visits by a nurse or therapist) or "semi-skilled" care (visits by a home health aide). HMOs reduce the number of home health visits by a nurse or therapist by 50 percent and visits by an aide by 56.9 percent. Again, enrollees and nonenrollees are equally likely to receive ***any*** visits by a nurse or therapist. The proportion of enrollees who received a home health aide visit (.8 percent) is one-third less than it would have been had they remained in the FFS sector. This impact is statistically significant at the .10 level.

As with SNF use, the large fraction of beneficiaries in the sample with zero home health use (about 97 percent) suggests that the two-part model may be more appropriate than OLS for estimating impacts. Again, we find the impact from the two-part model, a 48.3 percent reduction in total home health visits is close to the 53.6 percent reduction from the OLS model.[5]

The results for home health use suggest that Medicare risk plans are reducing the use of home health care, and not using it as a substitute for inpatient care. Enrollees are just as likely to receive some home care from a nurse or therapist, but are less likely to receive home care from a personal aide. Furthermore, enrollees receive substantially fewer home visits of all types when compared to nonenrollees with comparable characteristics. These results are particularly interesting in light of recently released information suggesting that the number of home health visits per episode of home care has increased dramatically between 1987 and 1990, from 23 visits per episode to 40 visits per episode (see ***Home Care News,*** March 1992). The increase has been attributed in part to relaxation of restrictive payment policies between 1986 and 1988 and in part to the Staggers decision, a court case resulting in Medicare beneficiaries being entitled to ongoing home health care to maintain in their health as well as rehabilitative care. If HMOs have continued to provide home health visits at

[5]A more detailed discussion of the two-part model and estimation results is presented in Section D.

the 1987 rate, the increase in visits per episode in the fee-for-se&e sector could fully account for the estimated difference that we obtain.

D. EXTENSIONS OF THE BASIC MODEL

In this section, we consider two alternative models which relax some of the assumptions implicit in our basic model. The first is a model suggested by Maddala for estimating impacts of programs with self-selected participants. The second is an application of the two-part model similar to that used by Christensen, Long, and Rodgers (1987). We find no strong evidence for preferring these models to our basic model.

1. The Maddala Model

Maddala (1983) considers two possible problems with using our basic model, equation (1), to estimate program impacts when program participants are self-selected. First, with self-selection of program participants, the indicator of enrollment status (Ii) may not be exogenous. Second, the independent variables in the model may have different effects on the outcomes of program participants and nonparticipants. That is, **B** may differ for enrollees and nonenrollees. He offers, as a more general model, a three equation model to characterize (1) outcomes for program participants, (2) outcomes for non-participants, and (3) the decision to participate in the program. In our context, the outcome relevant for participants is the service use of enrollees. Using the same notation as equation (1), this is given by the following equation:

(3) $\quad Y_{i1} = \mathbf{B}'_1 \mathbf{X}_{i1} + e_{i1},$

Similarly, service use for non-enrollees is characterized by the following equation:

(4) $\quad Y_{i2} = \mathbf{B}'_2 \mathbf{X}_{i2} + e_{i2},$

Finally, the participation decision is characterized by the following:

(5) $\quad I_i^* = \mathbf{D'Z}_i + e_{i3},$

where, I_i^* is an unobserved variable measuring' propensity to enroll, Z is a vector of independent variables explaining the enrollment decision (which may include some or all of the variables in X), D is the vector of coefficients on $\mathbf{Z}_i$, and e_{i3} is the error term. The beneficiary is enrolled ($I_i = 1$) if $I_i^* \rangle 0$, and the beneficiary is in the FFS sector if I_i^* (0. By assumption, e_{i1}, e_{i2}, and e_{i3} are correlated, with $\mathrm{cov}(e_{i1}, e_{i3}) = \sigma_{13}$ and $\mathrm{cov}(e_{i2}, e_{i3}) = \sigma_{23}$.

Estimating equation (3) on the enrollee sample is equivalent to estimating the following conditional expectation function:

(6) $\quad E(Y_{i1} | I_i = 1) = \mathbf{B}_1\mathbf{X}_{i1} + E(e_{i1} | I_i = 1)$

Similarly, estimating equation (4) on the nonenrollee sample is equivalent to estimating the following conditional expectation function:

(7) $\quad E(Y_{i2} | I_i = \mathbf{0}) = \mathbf{B}_2\mathbf{X}_{i2} + E(e_{i2} | I_i = 0)$

Since neither $E(e_{i1} | I_i = 1)$ or $E(e_{i1} | I_i = 0)$ may be equal to zero, estimating (6) by regressing Y_i on $\mathbf{X}_{i1}$ or (7) by regressing Y_{i2} on $\mathbf{X}_{i2}$ will yield biased estimates of $\mathbf{B}_1$ and $\mathbf{B}_2$ if the expected values are correlated with the variables in X_{i1} or X_{i2}. If e_{i3} is distributed standard normal with f(D' $\mathbf{Z}_i$) denoting the probability density function and $\mathbf{F(D'Z}_i)$ denoting the cumulative density, then $E(e_{i1} | I_i = 1)$ and $E(e_{i2} | I_i = 0)$ can be written as:

(8) $\quad E(e_{i1} | I_i = 1) = \sigma_{13}\, f(\mathbf{D'Z}_i)/F\,(\mathbf{D'Z}_i) = \sigma_{13}\lambda_{i1}$

(9) $\quad E(e_{i2} | I_i = 0) = -\sigma_{23}\, f(\mathbf{D'Z}_i)/(1-F(\mathbf{D'Z}_i)) = -\sigma_{23}\lambda_{i2}$

To avoid the bias of omitting (8) and (9) from the regressions, Maddala suggests estimating λ_{i1} and λ_{i2} and entering them as regressors in (6) and (7). This two-step estimation procedure is the same as proposed by Heckman (1978) and Lee (1978). To implement it, we estimated the probability of being enrolled, $F(D'Z_i)$, using maximum likelihood probit, and constructed λ_{i1} for enrollees and λ_{i2} for nonenrollees. (Full details on the probit model of enrollment are presented in Chapter III.) For each measure of service use, we then regressed Y_{i1} on X_{i1} and λ_{i1} to estimate B_1 and σ_{13}, and regressed Y_{i2} on X_{i2} and λ_{i2} to estimate B_2 and σ_{23}

Our results, which are presented in detail in Appendix B, suggest that our impact estimates are not distorted by sample selection bias; i.e., that the control variables in the model are adequate to capture the common factors influencing both utilization and the probability of enrollment. This conclusion was based on a test for sample selection bias; i.e., on tests of whether $\sigma_{13} = 0$ and $\sigma_{23} = 0$, which are conducted by testing the null hypothesis that the coefficients on λ_{il} and λ_{il} are zero. For each of the utilization measures examined, we could not reject the null hypothesis. Thus, under the assumptions of the model, sample selection bias is not a problem, and ordinary least squares regression can be used to estimate equations (3) and **(4).**

On the other hand, we do find that using separate regression models for enrollees and nonenrollees provides a significantly better fit to the data. Program impact for a group of beneficiaries with the same characteristics (X_i) is the difference in the mean predicted valuesfrom equations (3) and (4). This measure of program impact differs from our basic model in that the slopes as well as the intercepts of the service use equations differ for enrollees and nonenrollees; i.e., the relationship between personal characteristics and utilization is no longer assumed to be identical for enrollees and nonenrollees. To test whether the slopes differ for enrollees and nonenrollees, we interacted enrollment status with all the variables in X_i, and estimated the model with the interaction terms added. (This is the same as separately estimating equation (3) on the enrollee sample and equation (4) on the nonenrollee sample). We then used the standard F-statistic to test whether $B_1 = B_2$ **The** p-values for the test statistics, reported in Table IV.10, indicate that the null hypothesis $B_1 = B_2$ **is** rejected for every measure of service use.

While these differences suggest that HMO impacts should be estimated using separate models for the two groups rather than the single equation approach used to estimate impacts, earlier in this chapter, the most critical factor is whether the differences between impacts estimated from the basic model, equation (1), and impacts from the model with $B_1 \neq B_2$ are meaningful. Finding statistically significant differences in coefficients is not a surprising result given the relative large sample sixes--even small differences in some of the coefficients in B_1 and B_2 could cause us to reject the null hypothesis.

Comparison of impacts from the basic model and the model with the full set of interactions ($B_1 \neq B_2$) shows that the two sets of estimates are quite similar (see Table IV.10). Again, the results show that Medicare risk plans have little impact on the rate of hospitalization (a 3.2 percent reduction versus 5.7 percent for the basic model), but reduce length of stay by an amount similar to that found in recent studies (15.6 percent versus 16.8 percent for the basic model). Medicare risk plans have little effect on the number of visits over the four weeks prior to the survey (a 3.7 percent increase versus 4.6 for the basic model), but reduce the likelihood of frequent visits. Medicare risk plans reduce the number of SNF days (19.4 percent versus 24.4 percent for the basic model), and reduce the number of home health visits substantially (58.0 percent versus 53.6 percent for the basic model). Table IV.10 illustrates that in most instances the impact estimates from the model with a full set of interaction terms are of similar magnitude to the impacts derived from the basic model. Also note that the impacts expressed as a percentage changes in service use are similar in magnitude.

The findings from the model with the full set of interaction terms enabled us to identify important subgroups for which separate impacts should be estimated. In particular, we find that health status has a different effect on the use of enrollees and nonenrollees. In almost all categories of services, we find that enrollees use less services than nonenrollees with the same health or functional status. The two exceptions are the number of hospitalizations and physician visits. In both instances we find that enrollees reporting functional impairments or poor health had more

TABLE IV.10

COMPARISON OF BASIC MODEL AND MODEL WITH FULL INTERACTIONS

Dependent Variable	p-Values for Test statistic, Equality of HMO, FFS Coefficients[a]	HMO Impact, Basic Model[b]	HMO Impact, Model with Interactions[c]
Probability of Having One or More Hospitalizations	.085	-.009 (-5.7)	-.005 (-3.2)
Hospital Stays per 1,000 Members	.017	6 (5.7)	7 (3.2)
Hospital Days per 1,000 Members	.010	-309 * (-16.8)	-220 (-12.6)
Average Length of Stay	.013	-1.44 • * (-16.6)	-1.34 (-15.6)
Number of Physician Visits, Past 4 Weeks	.010	.026 (4.6)	.021 (3.7)
Probability of Having Frequent Doctor Visits (12 or More in the Past Year)	.022	-.016 ** (-11.4)	-.028 (-18.2)
Estimated SNF Days per 1,000 Members	.010	-150 (-24.4)	-112 (-19.4)
Total Home Health Visits per 1,000 Members	.010	471 • ** (-53.6)	-564 (-58.0)

[a]p-values for the statistic testing the hypothesis that the coefficients on the interaction terms are zero.

[b]HMO impacts are the same as those reported on Tables IV.3, N.6, and N.8.

[c]HMO impacts were estimated from OLS models (or probit for binary variables) with the full set of independent variables on Table IV.1, and a full set of variables interacting enrollment status with these variables. Mean service use was predicted assuming all enrollees were enrolled, and then predicted assuming all enrollees were in the FFS sector. HMO impact is the difference in the mean predicted values. Percentage impacts are the impacts divided by the predicted mean obtained by assuming enrollees were in the fee-for-service sector. Statistical significance tests were not conducted on the estimated impacts from the fully interactive model.

hospitalizations and physician visits. We explore how HMO impacts vary by health status in greater detail in Section F below.

Given the similarity of the alternative impact estimates on Table TV.10 we prefer the results from the basic model for the overall findings for two reasons. First, it is much easier to interpret the respective effects that enrollment status and other variables have on service use by examining the regression coefficients from the basic model. Second, the estimate of program impact from the basic model is more efficient (i.e., has a smaller standard error) since it is a function of the variance of only one parameter--the coefficient on the enrollment status variable. The standard errors of impacts as measured from the model with interactions with enrollment status are functions of variances and covariances of the coefficients on enrollment status and all interactions terms (Kmenta, 1971, p. 444). In general, this increases the standard errors of the impact estimates relative to those from the basic model.

2. The Two-Part Model

The distribution of the dependent variable in models of health care expenditures or service use is often characterized by (1) a large fraction of sample members with zero use, and (2) a small fraction of sample members with very high levels of use. A common criticism of using OLS to estimate models of health service use is the possible bias resulting from fitting a linear function to what appears to be a non-linear relationship. To address this problem the two-part model of Duan et al. (1983) models separately the probability that any services are used, and for users, the level of services used. The probability that any services are used, $\mathrm{Prob}(Y_i > 0|X_i,I_i,B,c)$ is estimated using probit. To model the service use for users, Duan et al. (1983) suggest transforming the dependent variable to the natural log form, 'or

$$(10) \quad \ln Y_i = B_iX_i + cI_i + e_i.$$

The expected value of equation (10) is then given by the following:

(11) $E(Y_i|Y_i > 0) = \exp(B\, X_i + cI_i)\, \phi,$

where $\phi = E(\exp(e_i))$.

The expected level of service use is then

(12) $E(Y_i) = \mathrm{Prob}(Y_i > 0\, |X_i, Ii) \bullet E(Y_i|Y_i > 0).$

Our objective is to estimate HMO impact on service, or the percentage difference in the expected service use of enrollees and nonenrollees. In estimating the impact that Medigap coverage has on service use, Christensen, Long, and Rodgers (1987) use the following formula to disaggregate the overall effect:

$$(13)\quad \%\Delta Y_i = \%A\ \mathrm{Prob}(Y_i > 0) + \%A\ E(Y_i|Y_i > 0) + [\%A\ \mathrm{Prob}(Y_i > 0)]\,[\%A\ E(Y_i|Y_i > 0)]$$

To calculate equation (13) we simply estimate the three terms on the right hand side of the equation. The terms, from left to right, are: (1) HMO percentage impact on the probability of any service use, (2) HMO percentage impact on the amount of service use by users, and (3) the product of the two percentage impacts. We have already reported HMO impacts on the probability on any use above. To estimate HMO impact on the service use of users, we estimate equation (10) for each service. For enrollees using services, we use equation (11) to predict what service use would have been in the FFS and HMO sectors. The difference between mean predicted use in the HMO sector and the FFS sector is the HMO impact on users. This impact, expressed as a percent of mean predicted use for users in the FFS sector, is then used in equation (13).

Our results suggest that except for SNF use the two-part model yields estimates of impacts that are quite'similar to the basic models. Table IV.11 reports HMO impacts calculated from equation (13), along with the HMO impacts from the basic model and model with the full set of interaction terms. HMOs reduce hospital stays by 2.9 percent and hospital days by 18.7 percent according to the two-part model. The latter is slightly larger than the 16.8 percent reduction estimated from the basic model, but similar.

According to the two-part model, HMOs increase the number of physician visits in the last 4 weeks by 5.8 percent, compared to the estimate of 4.1 percent from the basic model. For home health visits, the impact estimate of -48.3 percent from the two-part model is about the same as the 53.6 percent estimate from the basic model. However, the impacts are somewhat different for SNF days: an estimated reduction of 1.3 percent from the two-part model versus a 24.4 percent reduction from the basic model. Note from Table IV.ll, that the impact from the basic model is not significantly different from zero. While the point estimates differ for the HMO impacts on SNF days, from both models we can conclude that there is no evidence of a significant HMO impact on SNF days overall. Thus, with the exception of the impact on SNF days, the magnitudes of the impacts are quite similar for the two-part model and the basic models. And for SNF days, we have no reason to change our findings from the basic model that HMOs have no significant impact on SNF days overall.

E. THE RELATIONSHIP BETWEEN HMO CHARACTERISTICS AND IMPACTS

HMO impacts on service use may be related to a number of characteristics that vary across HMOs, such as financial incentives facing HMO physicians and the HMO's experience with serving the Medicare population. In this section we examine the variation in HMO impacts by plan characteristics. This investigation may be useful, in light of the insignificant impact found for the rate of hospitalizations and the number of physician visits. Analysis of subgroups of HMOs may reveal significant impacts for some, even though the overall impact of Me&care risk plans on service use may be insignificant.

To estimate impacts by plan characteristic, we modified the basic impact model slightly. In examining the subgroups defined by each of the plan characteristics of interest, the dummy variable

TABLE IV.11

COMPARISON OF BASIC MODEL WITH TWO PART MODEL

	Percentage Impacts, Two-Part Model					
	(1)	(2)	(3)	(4)	(5)	(6)
Dependent Variable	HMO Impact on Probability of Use[a]	HMO Impact on Level of use per User[b]	Interaction Term[c]	HMO Impact on Total Use[d]	HMO Impact, Basic Model'	HMO Impact, Model with Full Set of Interaction Terms'
Hospital Stays	-5.1	29	-0.2	-29	28	3.2
Hospital Days	-5.7	-13.8 • *'	0.8	-18.7	-16.8 *	-126
Physician Visits, Past 4 Weeks	6.2	-0.4	0.0	5.8	4.6	3.7
Estimated SNF Days	74.0 **	-43.3 •	-320	-1.3	-24.4	-19.4
Total Home Visits	-11.8	-41.4 ***	4.9	-48.3	-53.6 •*	-58.0

[a] HMO impacts on probability of nonzero use, are expressed as a percentage of the expected proportion of enrollees who have had nonzero use if they remained in the FFS sector. Values are the same as those reported on Tables IV.3, IV.6, and IV.8.

[b] HMO impacts on the level of use for users are estimated from OLS models with the natural log of service use as the dependent variable. Impacts are expressed as a percentage of the mean predicted use for users.

[c] The interaction term is the product of the impacts in columns 1 and 2

[d] HMO impact from the two-part model is the sum of columns 1, 2, and 3. Standard errors of these estimates were not calculated, so no significance tests have *been* conducted.

'These estimates were obtained from Table IV.10.

[f] These estimates were obtained from Table IV.10. Standard errors of these estimates were not calculated, so no significance tests have been conducted.

- Significant at the .10 level, two-tailed test.
- * Significant at the .05 level, two-tailed test
- ** Significant at the .01 level, two-tailed test.

indicating enrollment status in the basic model is replaced with a set of dummy variables indicating enrollment in a plan with a specific characteristic. For example, to estimate separate impact estimates by HMO model type, we created three dummy variables indicating whether the beneficiary is enrolled in (1) an IPA, (2) a staff plan, or (3) a group plan (nonenrollees are the excluded group, once again). Since impacts are not estimated for all characteristics simultaneously, differences on one dimension are not necessarily caused by that characteristic; for example, if IPAs are found to have significantly greater effects than other model types, it may be due to the fact that IPAs are larger (for example). We also change the weighting scheme, so that each plan has equal weight in this analysis (see Appendix A for a detailed discussion of weights. The results by plan characteristic are presented in Table IV. 12.

1. Impacts by Model Type

Across all measures of service use except physician visits, group model plans reduce utilization more than the other model types in use. The group model impacts are substantially greater than impacts for IPAs or staff plans, and except for hospitalizations and physician visits, are significantly different from zero. IPAs rank second to group model plans in reductions achieved for hospital days and home health visits by an aide, and they reduce impacts on visits by a nurse or therapist by about the same amount as group plans. Only IPA and group models affect hospital days.

The smaller impacts found for staff plan enrollees probably reflect the weaker financial incentives to reduce service use for this model type. Compared to FFS physicians, salaried physicians have no financial incentive to increase use for their patients. However, unless they have some form of profit sharing, they have little incentive to explore new ways to reduce service use. Physicians facing some form of capitation can increase their profits by further reducing service use. Since many group and IPA physicians are under some form of capitation, it is not surprising to find stronger impacts for these types of plans than for staff model plans. This finding is consistent with the findings from

TABLE IV.12

HMO IMPACTS BY PLAN CHARACTERISTICS

Measure of Impact for Subgroups of Enrollees by Plan Characteristic	Percent of Plans	Percent of Beneficiaries	HMO Impacts by Type of Service Use: Hospitalizations/ 1,000, Past Year	Hospital Days/ 1,000, Past Year	Physician Visits, Last Four Weeks	Estimated SNF Days/1,000, Past Year	Home Health Visits By Nurse or Therapist/1,000 Past 3 Months	Home Health Visits by Aide/1,000, Past Three Months
All Plans	100.0		6	**-309 ***	**.026**	-150	**-209** • *	-276 • **
Model Type								
IPA	53.3	31.6	10	-328 •	**-.01**	23	**-306** • **	-219 • *
Staff	18.6	26.4	-2	-28	**.15** • '	-477	-55	-78
Group	28.1	**36.0**	**-17**	-465 • *	**.07**	-651 •	**-280** • **	-292 **
Medicare Enrollment (January 1989)								
1 ,oOo-5,000	38.6	7.8	12	-1%	**-.01**	-187	-245 **	-199 •
5,001-10,000	29.3	15.2	-8	-282	**.10** • *	-15	-283 • *	**-204 ***
10,001-20,000	17.5	17.0	**-20**	**-668 ****	**-.05**	-183 *	-275 *	-277 •
>20,000	14.1	60.0	8	-249	**.09**	-303	-180	-193
AAPCC Rate, (1989)								
≤ $275	37.3	23.2	-9	-192	**.02**	-187 • *	-469 • **	-326 • *
$275-$325	46.7	43.9	13	-113	**.06**	-77	-184	-199
>$335	19.1	329	-12	-1,022 • *	**.06**	-816	-30	-203
Monthly Premium (1989)								
Zero	20.0	37.7	5	-626 • *	**.004**	-372	-395 • *	-328 • *
$1-$25	13.4	14.9	-11	-471	**.04**	187	-283	**-228**
$26-$50	427	328	4	-408 • *	**.04**	-318	-222 •	**-208 ***
>$50	24.0	14.7	9	**204**	**.09** •	-296	-170	-119
Mean of Dependent Variable			243	1,990	**.641**	653	408	475
Sample Size (Enrollee/Nonenrollee)			**(6,457/6,071)**	**(6,457/6,071)**	**(6,427/6,013)**	**(6,350/5,727)**	**(6,407/5,844)**	**(6,408/5,848)**

NOTE: HMO impacts by plan characteristics were estimated with OLS regression models with service use as the dependent variable and the full list of variables on Table IV.1 as independent variables. The binary enrollment status variable was replaced by a set of binary variables for the plan characteristic of **interest.** For example, to determine HMO impacts by model type, we included 3 binary variables indicating whether the sample member was an **IPA** enrollee, staff HMO enrollee, or group HMO enrollee. Impacts by model type are the coefficients on these variables. Observations were weighted in this analysis so that each **plan received** equal weight. This was done to ensure that no single HMO dominated the result of a particular category. **The** "All plans" results are the program as a whole and are taken **from** earlier tables; each beneficiary received equal weight in their calculations.

Nelson et al. (1989) that a combination of financial incentives and cohesiveness of providers yields the most successful HMOs.

2. Impacts by Enrollment Size

Plans with large Medicare enrollments are more likely to be more effective in controlling service use. The number of Medicare beneficiaries enrolled will reflect the length of time the plan has participated in the Medicare risk program, its success in attracting beneficiaries, and the importance of the risk plan to the HMO. The larger the plan, the more effort it may put into adapting its utilization management practices to deal specifically with the problems of the elderly.

The results on Table **IV.12** are interesting in that plans with enrollment between 10,000-20,000 have substantially larger impacts on hospital use, SNF days, and home health aide visits than plans with less than 10,000 enrollees and plans with more than 20,000 enrollees. For example, the reduction of 668 hospital days per 1,000 members is about two and one-third times the size of the estimated impact for plans of 5,000 to 10,000 members and over three times the size of the impact estimate for plans with fewer than 5,000 members. Plans with 10,001 to 20,000 enrollees also reduced SNF days per 1,000 members and home health aide visits per 1,000 members by greater margins than other plans. Surprisingly, plans with the largest Medicare enrollments (over 20,000 members) experienced smaller reductions in hospital days compared with all but the smallest plans, although the impacts are not significantly different.

3. Impacts by AAPCC Rate

AAPCC rates vary across geographic area due to regional variations in factor input prices and medical practice patterns. High AAPCC rates may reflect less efficient use of services by FFS providers as well as higher costs for labor, materials, and equipment. HMOs operating in market areas with high AAPCC rates and identifiable inefficiencies (e.g., high hospitalization rates and longer lengths of stay) may find Medicare risk contracting more attractive than HMOs in other market areas. The profits that can be gained from efficient operation in these market areas are obviously greater.

Thus, controlling for the AAPCC rate, which we do in our models, we might expect higher impacts for plans operating in market areas with high AAPCC rates.

Table IV.12 shows that indeed this is the case for hospital days and SNF days; reductions in use due to HMO membership are substantially greater for beneficiaries in counties with rates above $335. In fact, the results suggest that *only* plans in areas with AAPCC rates above $335 per month were able to reduce hospital days. For home health visits and physician visits no such pattern exists; in fact, reductions in home health visits are larger in areas with the lowest AAPCCs.

These results have implications for the financial performance of Medicare risk plans and for possible revisions to the AAPCC. Plans operating in areas with high AAPCC rates should be the most profitable, and generate the greatest resource savings. These findings are consistent with our results from several other components of the evaluation of Medicare risk plans. They suggest that paying HMOs in all areas the same fixed 95 percent of the local AAPCC may unfairly penalize plans in areas with low AAPCCs but may result in sizeable overpayments (relative to actual costs of caring for patients) in areas with high AAPCC rates.

4. **Premium Rates**

The maximum amount that Medicare risk plans can charge beneficiaries for supplemental coverage is the actuarial value of Medicare's coinsurance and deductibles for the Medicare population, plus the cost of any benefits covered beyond those offered by Medicare. Data released from OPHC show the median premium charged by risk plans is close to the actuarial equivalent of Medicare's coinsurance and deductibles and that premiums are quite competitive with those charged by Medigap insurers.[6] Although a plan may wish to set its rate above the prevailing rate for the market area in response to lower than expected profits, its ability to do so over the long-run is

[6]The average Medigap premium paid by our sample of nonenrollees was about $60 per month in 1990. The median premium for the 94 risk plans in 1990 was $39.14 (Bergeron and Brown, 1992) which is higher than the average premium paid by enrollees since a large percentage of enrollees were concentrated in plans charging little or no premium.

limited, given. competition from Medigap insurers and, in many cases, other HMOs in the market area.

If lower rates reflect more competition, plans operating in areas with low rates may face greater pressure to reduce service use to remain competitive. The results in Table IV.12 show that this may be the case.

The differences in impacts are fairly striking: impacts are largest for plans charging no premium and smallest for 'plans charging $50 or more per month (about one-fourth of risk plans), for virtually every category of utilization examined (though not all of these differences were statistically significant). Most striking is the finding that enrollees in plans charging $50 or more used *more* hospital days than nonenrollees (though the estimate is not statistically significant), whereas each of the other three categories of plans defined by premiums exhibited reductions of 408 to 626 hospital days per 1,000 enrollees. Impacts on home health visits for plans charging $50 or more were one-half to one-third the size of impacts for plans charging lower premiums.

Although a high premium may be indicative of a lack of competition and, therefore, lack of effort to be efficient, another possible explanation for these results is that plans charging higher premiums tend to be ones in market areas with lower AAPCC rates. Since lower AAPCC rates may reflect a more efficient FFS sector relative to markets with higher rates, plans operating in areas with lower AAPCC rates (1) may be less able to subsidize the premiums charged to beneficiaries with their AAPCC payments, and (2) may find it more difficult to reduce utilization to a lower level than FFS providers in their area in order to reduce costs below 95 percent of the AAPCC. Thus, the possible inability of HMOs to achieve greater efficiencies in market areas characterized by greater efficiency and lower costs in the FFS sector will be reflected in HMO premiums that are higher compared with those in market areas with less efficient FFS providers, and HMO reductions in utilization that are smaller.

F. IMPACTS BY HEALTH STATUS OF THE BENEFICIARY

When analyzing HMO impacts on service use, a key question asked in previous studies was whether HMOs achieve reductions in utilization by eliminating discretionary hospitalizations and care, or by cutting use "across the board." At least one study (Manning et al., 1984) argues that HMOs reduce the hospitalization rate by eliminating unnecessary, or discretionary, hospital admissions. Evidence that HMOs reduce use for all patients, those with severe conditions as well as those with minor conditions, could be viewed unfavorably. A key concern is that financial incentives may reduce service use for those most in need, thereby compromising their quality of care. On the other hand, reduced service use for those with severe conditions may be viewed as a sign that HMO are able to manage care even for those who would be high users in the FFS sector, or that it is these patients who are most likely to receive excess services in the FFS sector.

Our data set does not contain specific measures of disease conditions or patient severity of illness at admission. Thus, there is no way that we can assess, for example, HMO impacts on length of stay for patients with the same admitting diagnosis or same severity of illness at admission. (However, such an analysis is being conducted on a different data set, as part of the quality of care analysis.) We are able, however, to estimate HMO impacts on use for beneficiaries with poor health, functional impairments, medical conditions, or who died within 9 months of their interview date. If reductions in service use--in particular, reductions in hospital days--are achieved largely through reducing discretionary use, then HMO impacts on the service use of enrollees with poor health or medical conditions should be less. If, however, HMOs are able to manage the care of those most likely to use services--those with poor health or medical conditions, HMO impacts on the service use of this group may be greater than impacts for those in good health.

Whether the reductions observed are attributable to greater efficiently by the HMO or to inappropriately restricted access to care has been assessed by Retchin et al., 1992. They report that Medicare HMO enrollees hospitalized for cardiovascular accidents (CVA) and colon cancer had shorter lengths-of-stay and were less likely to receive discretionary tests or procedures, with no significant differences in post-operative outcomes. Moreover, HMO patients with low severity CVA s spend fewer days in an ICU compared to FFS beneficiary with low-severity CVAS. However, the number of days spent in ICU was the same for HMO and FFS patients with high severity CVAs. This suggests that HMOs can reduce utilization for those with greater need for care without compromising care. HMO patients with less severe illness may be targeted for the reductions in resource utilization (e.g., fewer ICU days for less severe CVA patients). However, the authors caution that HMO CVA patients are discharged with more neurological deficits, and that HMO patients receive fewer services after discharge from home health agencies or rehabilitation hospitals.

1. HMO Impacts on Hospital Use

For most indicators of poor health status, HMOs ***increase*** hospital admissions among beneficiaries in poor health, despite the overall lack of impact on admissions, and the ***decrease in*** number of days. On Table IV.13, impacts by health status are reported for six measure of service use. HMO enrollees with poor health have significantly more hospitalizations than nonenrollees in poor health, whereas there is no difference for those not in poor health. The same is true for enrollees with IADL impairments, and ADL impairments. Thus, this appears to be one instance in which the lack of an overall impact masks an effect for a particular small subgroup of beneficiaries. A history of cancer, heart disease, or stroke, or death within 9 months of interview, appear to have no discemable influence on HMO impacts on hospital admissions.

Despite the higher rate of admission for enrollees with poor health or functioning problems than for nonenrollees, these same enrollees have substantially *fewer* hospital days than nonenrollees with comparable conditions. Enrollees without health or medical problems also have fewer days than nonenrollees with the same characteristics. However, the magnitude of these impacts are substantially less than the impact for those with health or functioning problems. Especially striking is the large

TABLE IV.13

SERVICE USE IMPACTS BY HEALTH STATUS AND MEDICAL CONDITIONS

Measure of Impact for Subgroups by Health or Medical Conditions	Percent of Beneficiaries	HMO Impacts,[a] by Type of Service Use: Hospitalizations/ 1,000, Past Year	Hospital Days/ 1,000, Past Year	Physician Visits, Past Four Weeks	Estimated SNF Days/1,000, Past Year	Home Visits by a Nurse or Therapist/1,000, Past Three Months	Home Visits by Aide/1,000, Past Three Months'
All Beneficiaries	100.0	6	-309 • *	.026	-150	-209 • *	-276 • +*
Self-Rating of **Health**							
Poor Health	5.7	130 • **	-878	.297 • +	-2,296 • *	-1,149 • **	-5355 • **
Other than Poor Health	94.3	2	-271	-.005	-5	-134	-101
IADL Impairments							
One or more IADL impairments	29.3	11 • **	-343	.028	-296	-293 • **	- 4 1 1 ***
No IADL impairment	70.7	-15	-164	.021	214	46	128
ADL Impairments							
One or more ADL impairment	6.3	58 ***	-711 **	-.117 ***	-2,341 • **	-3,306 ***	-1,238 • **
No ADL impairments	92.7	-5	-223	.055	150	-31	-121
Serious Illness							
History of Cancer, Heart Disease, Stroke	21.4	-6	-808 • *	.096 *	-380	-296	-934 • **
No History of Cancer, Heart Disease, or Stroke	726	12	-95	-.004	-54	-172 •	2
Mortality							
Died Within Nine Months of Interview	4.6	17	-5237 • *	.561 • **	-15	-2,341 • **	-5495 ***
Did not Die	95.4	6	-223	.003	-157	-121	-185 •
Mean of the Dependent Variable		243	1,990	.641	653	408	475
N		(6,457/6,071)	(6,457/6,071)	(6,427/6,013)	(6,350/5,727)	(6,407/5,844)	(6,408/5,848)

[a] HMO impacts by health and medical condition were estimated with OLS regression models with service use as the dependent variable and the full list of variables on Table IV.1 as independent variables. Also in the list of independent variables were enrollment status and enrollment status interacted with the health status or medical condition of interest. For example, to estimate the HMO impact for persons with poor health status, we interacted poor health with enrollment status. HMO impact for persons with poor health is then the sum of the coefficients on enrollment status and enrollment status interacted with poor health.

impact for beneficiaries who die within 9 months of the interview. Though this includes only about 4 percent of the sample, the differences in use are substantial. Thus, a disproportionate amount of the reduction in hospital days comes from enrollees with poor health or medical conditions--those most likely to be high users in the FFS sector. However, these individuals actually have greater initial access to the hospital than fee-for-service cases.

2. HMO Impact on Physician Use

Except for those with ADL impairments, enrollees with a health or medical condition visit the physician more frequently than comparable nonenrollees. Except for IADL impairments these positive impacts are sizable and significant. The impacts are essentially zero for enrollees without health problems or medical conditions. The results suggest that enrollees without health problems or medical conditions visit the physician as frequently as comparable nonenrollees, but that enrollees with health problems have substantially more visits than comparable nonenrollees. This finding is consistent with the claim that HMOs increase access to primary care, and perhaps substitute primary care for inpatient care. The significant reduction in visits for those with ADL impairments suggest that perhaps HMOs reduce visits for members with chronic health conditions, but increase visits for those with acute problems, since many ADL limitations are due to chronic problems like arthritis.

3. HMO Impacts on SNF Days and Home Health Visits

HMO impacts on SNF and home health visits are much larger for beneficiaries with health problems. With only one exception, estimated reductions in use of these services are larger for those beneficiaries exhibiting the trait associated with greater need for health care. For beneficiaries in poor health, HMOs significantly reduce SNF days, home health visits by nurses/therapists, and home health aide visits. The estimated reductions are very large in magnitude and fully account for the overall estimated effect. Subgroups defined by ADL impairments exhibit the same pattern of very large significant impacts on SNF use and home health care for those with impairments and small, statistically insignificant estimates for those with no impairments. Subgroups defined by other indicators of health--IADL impairments, history of serious **illness,** or death within 9 months of interview--exhibit the same pattern of larger effects for the most seriously ill, but the differences are somewhat less dramatic in most cases. An exception is the reductions in use of home health care, which are very large for beneficiaries who died shortly after interview.

4. **Interpretation of Impacts by Health and Medical Condition**

Impacts by health or medical condition reveal an interesting pattern. Enrollees in poorer health are more likely to be hospitalized compared to nonenrollees in poorer health, but spend less time in the hospital. Enrollees in poorer health visit the physician more frequently than nonenrollees in poorer health. However, enrollees in poorer health have fewer home health visits.

The results show that enrollees with health or medical problems have similar or better initial access to hospital and primary care when compared to nonenrollees with the same conditions. A remaining question is the reason for the substantially lower number of hospital days and home health visits for enrollees with health problems compared to nonenrollees with the same problems. We offer three possible explanations. First, HMOs may be able to manage the care of those with health problems more efficiently than the fee-for-service providers and, thus, obtain substantial reductions in use. Second, HMOs may overly restrict use for those with health problems in which case the quality of care may be compromised. Third, there may be unobserved heterogeneity for enrollees and nonenrollees with the same medical condition. That is, enrollees reporting poor health may have less severe conditions than nonenrollees reporting poor health, so that independent of the HMO's effect on utilization, service use of enrollees reporting poor health would be less than use for nonenrollees reporting poor health. Under this scenario, the larger impacts for enrollees in poorer health would reflect, in part, biased selection. A definitive interpretation cannot be derived from our data, since they lack sufficient detail on the severity of conditions for those hospitalized. However, as we noted in Section C above, results from the quality of care study (Retchin et al., forthcoming) suggest that

Medicare risk plans are able to reduce hospital length of stay for patients with the same conditions without seriously compromising quality of care. This evidence from the quality of care study suggests that HMOs can successfully manage the care of patients with serious medical conditions.

G. UTILIZATION IMPACTS AND HMO EXPENDITURES ON MEDICAL SERVICES

The utilization impacts reported above suggest that with the exception of physician services, enrollees use fewer services than they otherwise would in the FFS sector. As we argue below, the likely effect of these changes in utilization is a reduction in expenditures by HMOs for medical services covered under Medicare. While such reductions do not affect costs to HCFA, lower total demand for medical services could affect prices for these services. More importantly, the size of HMOs' savings through reductions in service use will affect both their willingness to continue with risk contracting and the likelihood that HMOs will continue to offer enrollees lower premiums for additional benefits.

1. Rationale for Translating Utilization Impacts into Impacts on HMO Expenditures for Medical Services

In addition to program effects on costs to HCFA, we are also interested in how HMO impacts on service use affect *HMO* expenditures. Since HCFA pays 95 percent of the AAPCC for enrollees, Medicare risk plans must reduce their expenditures for Medicare-covered services by at least 5% to cover costs if selection is neutral. Furthermore, HMOs must be able to cover their administrative costs, which averaged about 10 percent of total costs for Medicare members for a group of 33 HMOs in 1988 (see Palsbo and Cold, 1990, p. 66). Even with favorable selection as reported in Chapter III, it is useful to know whether plans are reducing their expenditures 'for Medicare-covered services, since it may be possible to modify the payment formula to reduce favorable selection. That is, if the AAPCC were modified to reduce favorable selection we are interested in knowing whether plans could reduce expenditures sufficiently to operate on 95 percent of this amount.

2. Estimated Impact of the Program on HMO Expenditures for Medicare-Covered Medical Services

Our service use impacts directly correspond to four aggregations of expenditures for Medicare-covered services by place of service: inpatient hospital, physician's office, skilled nursing facility and home health visits. Weighting the utilization impacts for each of these aggregates by the dollar expenditures expected for each aggregate under FFS Medicare is the most straightforward method for estimating the likely impact on HMO expenditures for changing utilization in these four aggregates. Equivalently, to arrive at the percentage change in HMO expenditures, we can weight each of the four aggregations by their respective percentages of total Medicare reimbursements for medical services. That is, we can weight the 16.8 percent reduction in hospital days by the percentage of total dollar expenditures devoted to inpatient care (e.g., 58.7 percent of total expected expenditures), weight the 4.6 increase in physician visits among enrollees by the 27.3 percent of total expenditures devoted to physician visits and so on. This method is intuitively appealing since the various impacts are weighted by their relative importance in total Medicare expenditures for medical services. This allows us to estimate the likely percentage change in expenditures for Medicare-covered services that HMOs will experience given the changes in service use for their enrollees. However, this estimate is based on the assumption that HMO expenditures for each aggregate changes in direct proportion to the estimated HMO impact for service use corresponding to that aggregate (e.g., inpatient expenditures will decrease by 16.8 percent given our estimated 16.8 percent reduction in hospital days). We explore more critically the implications of this assumption in Section 3, below.

To estimate what Medicare costs would have been for enrollees in the FFS for each of the cost aggregates, we used the same methodology as Chapter III. We regressed nonenrollees' FFS costs for each aggregate[7] on the full list of variables explaining costs and then using the regression models and

[7] We estimated each cost aggregate for nonenrollees from 1989 Medicare reimbursements as follows. For inpatient hospital days, we calculated the Part A and Part B reimbursements identified

enrollees' values for the independent variables, predicted enrollee FFS costs. The results are presented in column 2 of Table IV.14. Following the weighting scheme detailed in the previous paragraph, HMO impact on expenditures for each aggregate was computed as the product of predicted FFS cost for enrollees and the HMO percentage impact on service use for the aggregate. For example, on Table IV.14, line one, we predict average inpatient costs to be $1,363 for enrollees if they had not joined an HMO. Since HMOs reduce hospital days by 16.8 percent, we estimate a reduction in expenditures of $229 per enrollee (.168 x $1,363) compared to their predicted inpatient Medicare costs in the FFS sector. Doing the same calculations for each cost aggregate and summing the results yields an estimate of the effect that HMO utilization impacts had on HMO expenditure for Medicare-covered services.

As Table IV.14 illustrates, the likely effect--valued at Medicare payment rates--is -10.5 percent. That is, we estimate that HMOs used 10.5 percent fewer medical resources than Medicare would have expended on enrollees if they had not joined the HMO. As the first entry in column 4 of Table IV.14 shows, over 80 percent of this reduction is attributable to the reduction in hospital days. Reductions in SNF days and home health visits also contribute to the savings, but these gains are offset slightly by higher use of office visits for ambulatory care. This finding, that HMOs achieve rcduced expenditures on medical services by reducing hospital use and substituting more ambulatory care, is consistent with conventional wisdom and HMO behavior. Both SNF days and home health

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as inpatient. For physician visits, we calculated the Part B reimbursements identified as medical care and non-medical care received in an office. For SNF days, we identified all Part A reimbursements for days spent in a SNF. For home health visits, we calculated all Part A and Part B reimbursements made to home health agencies. As Table IV.14, column 1 shows, the four aggregates cover 92 percent of total nonenrollee reimbursements. Only one category of service use and costs, those received as an outpatient in a hospital setting are excluded from the analysis, because we have no survey data on such services with which to estimate HMO impacts. Given the lack of effect of HMOs on hospital admissions, it seems highly unlikely that HMOs would have increased use of hospital outpatient care. However, if HMOs reduce the use of care, we will be underestimating the HMO effect on resource costs.

TABLE IV.14

HMO IMPACT ON RESOURCE COSTS, VALUED AT MEDICARE PRICES

	Average 1989 FFS Reimbursements			
Cost Aggregates for:	Actual Nonenrollee	Predicted Enrollee[a]	HMO Impact on Service Use, Basic Model	Implied Impact on Costs, at Medicare Prices[b]
Inpatient Hospital Days	$1,657 (58.9)[c]	$1,363 (58.1)[c]	-16.8 %	-$229
Outpatient Hospital Visits	225 (8.0)	206 (8.8)	–	–
Visits to Physician's Office	719 (25.6)	656 (28.0)	4.6	+30
SNF Days	125 (3.0)	(G)	-24.4	-14
Home Health Visits	85 (4.4)	59 (2.5)	-53.6	-32
Total	$2,811	$2,344		-$245 (-10.5 %)

[a]For each cost aggregate, enrollee Medicare FFS reimbursements were predicted from regression models estimated on nonenrollee Medicare reimbursements as the dependent variable. The independent variables in the model are the same as those used to predict FFS cost:. in Chapter III.

[b]Implied impact is calculated as the product of predicted enrollee reimbursements and HMO impact on service use. For example, for hospital days impact is calculated as -.168 x $1,363 = - $229.

[c]Numbers in parentheses are percentages of total actual or predicted Medicare reimbursement.

care account for small fractions of total expenditures in the FFS, so even large percentage reductions in these services by HMOs contribute only moderately to their overall savings.

3. **Limitations of the Estimation Method**

The estimated 10.5% reduction in HMO expenditures for Medicare-covered services is based on the assumption that expenditures within each of the four aggregates change in proportion to the corresponding service-use impact for the aggregate (or equivalently, that the average amounts paid by HMOs per unit of service are equal to the average amounts paid to FFS providers by Medicare). There are two major reasons why this simplifying assumption may not hold:

1. Our units of service use (hospital days and physician visits) do not capture the intensity of resources used by different beneficiaries during the hospital stay or physician visit. For example, our estimated 16.8% reduction in hospital length of stay may reflect reductions among the less costly diagnoses or for the less costly period of a hospital stay (e.g., just prior to discharge).

2. We do not know what HMOs are paying hospitals, physicians, and other providers. Fees may exceed or be less than those paid by Medicare, though we find it improbable that plans with significant Medicare enrollments would pay more than Medicare would pay FFS providers. Thus, compared to expenditures under Medicare FFS, HMO expenditures will reflect both their impact on service use and their negotiated payment rates.

We discuss each of these reasons in turn.

Using survey data, we have no way of determining the HMO's impact on the intensity of services used during the hospital stay, other than their impact on length of stay. However, evidence from Retchin et al. (1992) on the use of services by enrollees and nonenrollees hospitalized for stroke and colon cancer, indicate that HMO enrollees spend less time in an ICU, receive fewer discretionary tests, receive less physical therapy, and receive less medication for management of pain. This evidence suggests that HMOs reduce the intensity of services used during the stay in addition to reducing the length of stay. Clement et al. (1992) find that for 3 chronic conditions (joint pain, chest pain, and urinary incontinence), HMO enrollees are less likely to have follow-up visits and are less likely to receive care from a specialist. These findings are consistent with our impact estimates for physician use, in that HMO enrollees were less likely to visit a physician frequently (12 or more times a year). The lesser use of specialists among enrollees also suggests that while enrollees had about 4.6 percent more physician visits in the past month than they would in FFS, costs may be somewhat less than 4.6 percent higher than they would be in FFS.[8] On the other hand, the percentage reduction in costs may be less that the percentage reduction in hospital days, because the days eliminated by HMOs are likely to be less costly than the first few days of the stay.

We do not have data on fees paid by HMOs for specific services. Hence, we do not know how payments under these methods would compare with reimbursements that would have been made by Medicare to FFS providers for the same services. However, it would seem irrational for HMOs to pay more than Medicare reimburses for the same care.[9] For example, HMOs that pay physicians a capitation rate will base this rate on the average AAPCC payment received from HCFA, while HMOs paying discounted fee-for-service rates are likely to base these rates on Medicare's prevailing charges. For hospital services, most HMOs pay negotiated per diem rates and prefer this method, but they have the option of having hospitals bill Medicare (which will pay DRG rates) directly for all hospital services (these payments would then be deducted from the next monthly AAPCC payment to the HMO). This option ensures that HMOs should not pay more than DRG rates for hospital care. Hence, it seems reasonable to assume that HMO payment rates are not higher than those paid by Medicare to FFS providers, but may be lower if HMOs are successful at negotiating lower rates, as they often are for their non-Medicare enrollees.

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[8]This estimated effect was not statistically significant, Hence, the number of physician visits may not be greater for enrollees. However, the point estimate is our best estimate, so it was used in the calculations instead of using zero.

[9]Even if HMOs pay rates by DRG for the hospital stay, in light of our evidence that risk plans reduce hospital length of stay, one might expect HMOs to negotiate lower DRG payments than are paid under Medicare. However, some states mandate that all payors, including HMOs, pay state or Medicare DRG rates for hospital stays.

The reductions in intensity of services and the ability of HMOs to negotiate favorable prices for services suggest that for any percentage reduction in services used by enrollees compared to their use in the Medicare sector, HMO expenditures for the service are likely to be reduced by **at least** the same percentage. If HMOs can secure discounts from FFS rates, their percentage reduction in expenditures for the services will be greater than the percentage reduction in units of service used. Conversely, for any percentage increase in services used (e.g., physician visits) in comparison to the FFS sector, HMO expenditures for the service will **at most** increase by the same percentage over what expenditures would have been for enrollees in the FFS sector. If the HMO negotiates discounts from FFS, the percentage increase in HMO expenditures for the service will be less than the percentage increase in units of service used.

To conclude, the estimated 10.5 percent reduction in HMO expenditures for Medicare-covered services is imprecise, given our current lack of knowledge of the intensity of services used during hospital stays or visits to a physician. As a result, HMO expenditures compared to expenditures in the FFS sector, may not change in the same proportions as estimated HMO impacts for service use, as our estimate of 10.5 percent implicitly assumes. However, supplementary evidence from Retchin et al. (1992) and Clement et al. (1992) indicates that enrollees use fewer services throughout the hospital stay and less care in an ambulatory setting from specialists. Furthermore, it would be difficult to argue that Medicare risk plans would pay higher rates to its providers than Medicare pays to FFS providers. Thus, we find no strong evidence that the reduction in expenditures for Medicare-covered services by HMOs would be substantially less than our estimate of 10.5 percent.

V. CONCLUSIONS

The purpose of this study was to evaluate the impact of the Medicare risk program on the costs to HCFA and on the use of services by Medicare beneficiaries. Costs to HCFA, at least in the short-run, are chiefly determined by the accuracy of prospectively determined payment rates and the degree of biased selection in the program. The program's impact on the use of services will reflect the effect of capitation and HMOs' structural features on the efficiency with which health care is delivered to Medicare beneficiaries. The degree to which HMOs can provide care more efficiently than FFS Medicare is of great interest in the current debate on health care reform, not only for the Medicare program, but for overall national health policy. In particular, an effective program of cost-containment is seen as an essential element in any proposal to expand access to care. Programs of coordinated care such as the Medicare risk program are seen by some as an important component of any cost-containment strategy.

A. IMPACT ON COSTS TO HCFA

Although capitation payments to Medicare risk plans are set prospectively at 95 percent of the expected FFS costs for beneficiaries in the same geographic area and with the same actuarial risk factors, the program may not realize the expected 5 percent savings. There are two reasons why the 5 percent savings may not be realized First, if the prospectively set rates over or under-predict the actual FFS costs for beneficiaries who remain in ***the FFS sector,*** then cost savings will be less or more than five percent. Second, if the actuarial risk factors used to construct the AAPCC payment rates fail to account fully for differences between the costs that Medicare would have incurred for enrollees had they remained in FFS and the costs actually incurred for beneficiaries who do remain in FFS, i.e., if there is biased selection, then cost-savings will differ from five percent.

We do not address the first of these sources of costs or savings--the accuracy of the AAPCC forecast of F'FS costs. Indeed it is difficult to assess forecast accuracy in a meaningful way, given the changes in recent history to the provisions of the Medicare Catastrophic Coverage Act of 1988 and the subsequent repeal of those provisions in 1989. Any forecast errors in setting the 1989 and 1990 rates, the time period evaluated in our study, would not necessarily persist in the future. In addition, a previous study of the accuracy of the payment rates for the FFS sector and the accumulated evidence on the accuracy of payment rates since the advent of the program do not indicate a systematic bias in prospectively setting the overall per capita cost for the United States (USPCC) on which, the county AAPCC rates are based.

Our estimates suggest that due to favorable selection, HCFA is spending about 5.7 percent more for enrollees than they would have cost had they remained in FFS--about 8.5 percent more for Part A services, and about 2.7 percent more for Part B services. Previous evidence of favorable selection has cast doubts on the program's ability to achieve cost savings, and our beneficiary survey provides further evidence of favorable selection. The enrollees interviewed were less likely than nonenrollees to report poor health, functional impairments, a history of serious illnesses, and were less likely to die in the 9 months following the survey interview. They were also less likely to say they worried about their health more than others their age, and were more likely to say that they avoid doctors when a health problem arises. In regression analyses all of these factors had significant and large effects on the FFS Medicare costs of nonenrollees. Predictions of average FFS costs for enrollees generated from these regression models were 17 percent lower than the unadjusted average FFS costs for nonenrollees, and indicate the magnitude of cost differences that are generated by these observed characteristics. We then computed an adjusted average nonenrollee cost to eliminate any cost differences that could be accounted for by differences between nonenrollees and enrollees on factors controlled for by the AAPCC--the geographic location of the beneficiary and the actuarial risk indicators. We found that the predicted average FFS costs for enrollees are 10 percent ($264) lower than adjusted average of nonenrollee costs. This means that the costs of enrollees in the FFS sector, had they not enrolled, would have been 10 percent lower than the AAPCC rate on average, if the

AAPCC perfectly predicted average FFS costs for nonenrolled beneficiaries in each market area. We estimate that this favorable selection implies that HCFA is losing 5.7 percent on the currently enrolled population (since HCFA only pays HMOs 95 percent of the AAPCC).

Disaggregation of our impact estimates showed that 83 percent of the overestimate of cost that is implicit in the AAPCC is due to enrollee-nonenrollee differences on health status measures (impairments on functioning, self-rating of health, and a history of serious illness) that are not accounted for by the AAPCC risk classification. The difference in the proportion of beneficiaries with a history of cancer, heart disease or stroke was the single most important source of the AAPCC overestimate of costs for nonenrollees, accounting for 38 percent of the total. Attitudinal differences toward health care (avoidance of doctors, worry about health) account for about 14 percent of the overestimate. Differences on socioeconomic characteristics, including income and (predicted) Medigap coverage, account for the remaining 3 percent of the difference between AAPCC rates and the projected FFS costs that would have been incurred.

Our estimate of favorable selection and its impact on costs to HCFA is more moderate than those implied by previous studies of biased selection. This finding was expected, since previous studies were not designed to take account of possible regression toward the mean and therefore overstated potential losses to the Medicare program. Regression toward the mean is a much less critical issue in our analyses--we do not use reimbursement prior to enrollment as our measure, and we measure enrollee-nonenrollee differences in health, functional status, and preferences for seeking medical care in the post-enrollment period, for the current stock of enrollees. Since 70 percent of our sample had been enrolled for at least 2 years by the time of interview, it is likely that enrollee values for characteristics influencing costs will have already regressed toward the mean nearly as much as they are likely to; thus, remaining differences are due to ongoing differences in health status. We also note that our sample frame excluded 12 percent of enrollees--primarily those who joined the HMO within 3 months of the start of the survey and those who had been eligible for Medicare for less than 1 year. Previous studies suggest that selection is likely to have been even more favorable for these individuals than for those in our sample.

B. HMO IMPACTS ON THE USE OF SERVICES

There are two primary reasons for investigating the impact of the Medicare risk program on the use of services. First, by setting capitation payments at 95 percent of expected FFS costs of enrollees, the Medicare risk program anticipated that HMOs would be able to reduce their expenditures for Medicare-covered services by at least 5 percent. This expectation was reasonable in light of empirical evidence on HMO impacts through the late 1970's, indicating that HMOs achieved considerable cost savings through reduced hospital days. In the 1980s, hospital use in the FFS sector for Medicare dropped, in part due to the introduction of PPS, and to the advent of medical technologies that allowed procedures to be moved from an inpatient to outpatient setting. The ability of HMOs to reduce costs relative to FFS providers now appears to be more limited, given cost-containment initiatives instituted in the FFS sector. Thus, it is important to assess whether HMOs are currently achieving cost savings through reductions in service use, as was initially anticipated at the program's inception. We also are interested in determining the size of the reductions and how they differ across types of services. A number of studies suggest that HMOs may actually increase the use of ambulatory services and preventive care in order to reduce the need for more costly institutional care. Unless HMOs reduce at least some services by enough to outweigh any increases in other services, HMOs would not be able to continue providing coverage to Medicare members if capitation payments were accurately set at 95 percent of what enrollees' FFS costs would have been.

The second consideration is the general interest in the efficacy of capitation as a method for cost-containment. Evidence on the ability of HMOs to provide care more efficiently to the Medicare population (especially in light of PPS) is useful for assessing the efficacy of capitation as a cost containment strategy in proposals for health care reform currently under consideration. Unless HMOs can create real resource savings, there will be no long-run effects of HMOs on costs.

In assessing HMO impacts on service use we examined hospital use, use of skilled nursing facilities (SNF), physician use, and home health care. Our expectation was that HMOs would reduce the use of hospital care, but that use of other services might actually increase, despite the incentives of capitation to reduce all services, as HMOs strive to substitute less costly for more costly services.

We were surprised to find that HMOs had no effect on hospital admissions, but shortened lengths of stay substantially. This pattern was the reverse of what we had expected, based on earlier studies and on the incentives in the FFS sector to shorten hospital stays. Enrollees had about the same rate of hospitalization as nonenrollees after controlling for differences on observable characteristics that influence hospital use. However, enrollees had about 17 percent fewer hospital days than nonenrollees, due to a 17 percent lower average length of stay. This estimated reduction in length of stay is consistent with other recent studies of HMO effects, with our own findings for colon cancer and stroke patients (see Retchin, et al, 1992), and with information gathered in case studies of 5 successful risk plans (see Hurley and Bannick, 1992).

Enrollees were significantly more likely to have some contact with a physician over the course of the year, as measured by the beneficiary's usual frequency of physician visits each year and the likelihood that the beneficiary had a routine physical exam in the past year (controlling as always for differences on beneficiary characteristics). However, enrollees were less likely to have frequent physician visits (more than 12 a year). We found no effect, of HMOs on the number of visits over the 4 weeks before interview.

Enrollees were significantly more likely than nonenrollees to have been admitted to a SNF, an indication that HMOs may substitute SNF days for acute care hospital days. HMOs increased SNF use by 74 percent; however, the absolute size of the increase was quite small (0.3 percentage points), because so few beneficiaries received SNF care (about .8 percent of enrollees). The estimated effect on SNF days was negative, but not statistically significant. HMOs had no discernable effect on the likelihood of use of home health care services, but reduced the number of home health visits by about 50 percent.

The higher likelihood that enrollees saw a physician at some time during the year is consistent with the absence of beneficiary copayments under most HMO plans, and the commitment by HMOs in general to providing preventive care. The lower percentage of enrollees with frequent physician visits is consistent with the incentive of physicians under capitation to limit the number of follow-up visits for a specific illness. The patterns of service use also show that enrollees are in no sense denied initial access to care: they were just as likely to be hospitalized or receive home health services, and were more likely to visit a physician or receive SNF care during the course of the year.

Assuming that services used by enrollees are purchased at the average prices paid by Medicare, we estimate that Medicare HMOs spent 10.5 percent less on Medicare-covered services than HCFA would have spent, as a result of the combined HMO impacts on service use. While this estimate is rough, it is clear that the value of the resources saved by reducing utilization should be more than enough to cover the 5 percent lower reimbursements that HMOs would receive compared to FFS providers if the AAPCC were accurate.

C. ACCOUNTING FOR THE ESTIMATED SURPLUS FROM FAVORABLE SELECTION AND HMO EFFICIENCIES

If HCFA overpays HMOs by 5.7%, and the greater efficiency of HMOs reduces their expenditures for Medicare-covered services by 10.5%, what happens to this surplus of capitation payments by HCFA for Medicare-covered services over expenditures by HMOs for Medicare-covered services? Enrollees receive part of it in the form of lower premiums and/or more benefits than nonenrollees receive. Some of it goes to cover HMOs' higher administrative costs, which have been estimated by HMOs to comprise 10 percent of their risk plans' total costs; thus, the overall efficiency of HMOs is somewhat less than is implied by our utilization impacts. Finally, some of the surplus becomes HMO profits.

To examine this question more fully it is useful to define two distinct surpluses (losses), which we outline in Table V.l. The first surplus is the sum of overpayments by HCFA (favorable selection) and reduced expenditures by HMOs for Medicare-covered services. (This is the surplus of revenues paid by HCFA over dollars spent by HMOs for Medicare-covered services, or the difference between HMO expenditures and revenues for the shaded boxes in Table V.1.) This first surplus does not characterize the actual operating surplus or profits that Medicare risk plans realize on the Medicare portion of their business. As Table V.l illustrates, in addition to expenditures for Medicare-covered services, risk plans typically cover the deductibles and co-insurance for Medicare-covered services, and cover services in addition to those covered under Medicare. HMOs also incur administrative costs in addition to those incurred by providers of medical services or Medigap insurers. These include the costs of recruiting and negotiating contracts with providers (for aspects of compensation and service provision unique to Medicare), utilization review and management, quality assurance reviews, and the annual cost of maintaining their risk contract with HCFA (e.g., preparing the adjusted community rate calculations annually, complying with PRO reviews). Thus, on the expenditure side, these additional costs imply that total health care expenditures for enrollees (for Medicare-covered services, other services, and administrative costs) will not be 10.5 percent less than total HMO expenditures for enrollees in the FFS sector.

On the revenue side, Medicare risk plans can charge premiums in addition to capitation payments from HCFA to meet the additional expense from covering Medicare deductibles, coinsurance, and services in addition to those covered by Medicare. While the actuarial value of these benefits is substantial (for 1989, the annual value of coverage for deductibles and copayments alone was estimated at about $474), over one-third of the beneficiaries in our analysis belonged to plans that charged no premium. In addition, Clement, Gleason, and Brown (1992) report that average Medigap premiums were greater than Medicare risk plan premiums for 43 of the 44 market areas examined. Thus, while our estimates from Chapter III suggest that HMOs receive 5.7% more

TABLE V.l

MAJOR CATEGORIES OF EXPENDITURES AND REVENUES FOR MEDICARE RISK PLANS

HMO Expenditures	HMO Revenues
1. Fees paid for services covered under Medicare	1. AAPCC Payments
2. Coinsurance and deductibles for services covered Medicare	2. Premiums from Enrollees
3. Fees paid for benefits in addition to those covered under Medicare	
4. Administrative costs: (paying claims, negotiating contracts with providers, utilization review, marketing, quality assurance, complying with HCFA requirements).	

in revenues from HCFA than FFS providers would receive for Medicare-covered services, they receive far less revenues from premiums than Medigap insurers do for covering coinsurance and deductibles, and they typically cover more services.

The higher expenditures from additional administrative costs and enrollees benefits and lower revenues from premiums set substantially below the actuarial value of benefits covered (deductibles, coinsurance, and additional services) mean that Medicare risk plans are not realizing as large an operating surplus (profit) overall as indicated by our first surplus--the sum of favorable selection and reduced HMO expenditures for Medicare-covered services. Indeed, given current regulations under the risk program, plans **cannot** have expected profits on the Medicare portion of their business that exceed the rate of profit on their commercial business. Projected surpluses greater than their commercial rate of profit must either be returned to HCFA or transferred to beneficiaries in the form of lower premiums or more services. Almost without exception, HMOs choose the latter option, improving their competitive position by charging lower premiums or offering more generous coverage. (Stated alternatively, they will be compelled through competition to share the surplus with beneficiaries, since their competitors or potential competitors will most likely do so.).

It is difficult and beyond the scope of this evaluation to quantify precisely how much of the projected difference between HMO revenues and HMO costs for medical services are devoted to additional benefits for beneficiaries, coverage of administrative costs, and HMO profits. Indeed, the size of the surplus to be distributed is not readily estimable due to unmeasured HMO-FFS differences in the intensity of services and differences in prices paid to providers, and because errors in geographic adjustors are not taken into account. Furthermore, the amount of "surplus" and the distribution of it will vary widely across risk plans. However, over one-third of beneficiaries receive benefits valued by actuaries at well over $500 per year, which alone would account for over $167 of the crude estimate of $380 for the surplus (10.5 percent savings on the $2,344 projected FFS costs for enrollees, due to reduced service-use, plus an excess of $134 in payments from HCFA over this projected cost). Other plans also subsidize enrollee benefits by charging premiums below the market cost. On the other hand, administrative costs have been estimated by HMOs themselves to comprise 10 percent of their total cost (or equivalently, about 11 percent of medical costs). Our admittedly crude estimates suggest that these costs might equal $231 per member per year (11 percent of projected costs for medical services, which are assumed to be 10.5 percent less than the $2,344 estimated FFS cost). The estimates of administrative cost plus the lower bound estimate of savings to beneficiaries more than exhausts the projected surplus, suggesting that HMO profits overall are not likely to account for much of the "surplus." This is consistent with the findings of Shin and Brown (1992) and Palsbo and Gold (1990), which both suggest that on average risk plans break even.

While these estimates are very imprecise, they suggest that as a very rough rule of thumb the excess of HMO revenues over HMO costs for medical services may be split fairly evenly between increased benefits for enrollees and coverage of HMOs' administrative costs, with very little going to HMO profits on average. However, a number of plans, especially the largest plans, do earn profits, while others lose money. These estimates are intended only to indicate the sizeable magnitude of both administrative costs and additional benefits to beneficiaries that are being funded by the excess payments from HCFA arising from Favorable selection and by HMOs' greater efficiency in the use of health care resources.

D. DIFFERENCES IN IMPACTS ACROSS SUBGROUPS OF HMOS AND BENEFICIARIES

In general, we find that cost to HCFA varies considerably with some plan characteristics, but the relationship between plan characteristics and service use impacts is much weaker. For example, we find that staff plans are experiencing more favorable selection than the other model types and, hence, are generating larger percentage losses to HCFA. We also find that plans with higher monthly capitation payments (greater than $325 per enrollee month in 1990) and plans charging zero premiums have more favorable selection than other plans, and hence generate greater losses to HCFA. The results are sensible and expected because selection previously has been found to be most favorable in areas with high payment rates, and favorable selection allows HMOs to offer supplemental coverage at low or zero premiums. This finding is consistent with the argument just put forth in Section C, and is encouraging in that it suggests that much of the increase in cost to HCFA is not enriching HMOs, but providing greater benefits to Medicare beneficiaries. Thus, the safeguard built into the risk program for preventing excess profits appears to be at least somewhat effective.

For service use impacts, we find that group model plans have a substantially greater impact on reducing utilization than other model types. We also find that plans in market areas with the highest AAPCC rates and plans charging zero premiums are much more successful at reducing hospital days than other plans. The relationship between zero premiums and larger reductions in hospital use may indicate that plans are sharing part of their cost savings with enrollees by offering lower premiums, as required by law. Once again, this finding is consistent with the argument put forth in Section C. The relationship between higher AAPCC rates and larger HMO reductions in service use may reflect greater inefficiencies in the FFS sector in certain market areas and, hence, greater opportunities for HMOs to achieve cost savings.

We also find that HMO reductions in hospital days and home health visits are achieved chiefly among the subgroups of enrollees reporting functional impairments or poor health, or who died in the nine month period after interview. That is, HMO impacts for these services were significant for those with health problems but were insignificant and of smaller magnitude for those without these problems. However, the results for the hospitalization rate and use of physicians services were quite different. We find that HMOs had a positive and significant impact on the rate of hospitalization for enrollees with poor health or functional impairments. HMOs also had a positive and significant impact on the number of physician visits for enrollees with poor health or who died in the nine month period following the survey interview. Thus, for beneficiaries with health problems, HMOs appear to provide equal or better initial access to hospital and physician care than the FFS sector provides, but the number of days of hospital care or visits to a physician may be fewer for these individuals as HMOs provide their care more efficiently.

E. LIMITATIONS OF THE STUDY

As in any study, there are limitations with the data and methods used for this study which must be considered in evaluating the results. We have attempted to enumerate them throughout this report, but review them here.

1. Use of Self-Reported Utilization

Our telephone survey of beneficiaries was the source of data on use of hospital, physician, SNF, and home health care, as well as other dependent variables in the evaluation of the Medicare risk program. Self-reported use of health care services introduces the possibility of measurement error from inaccurate recall (by the beneficiary or proxy respondent). Furthermore, the reliance on self-reported use limits the level of detail on specific services used, diagnoses of the patient, and types of physicians (by specialty) used.

Inaccurate recall is not likely to differ by enrollment status, so our estimates of HMO impacts on service use should not be biased; they are simply less precise than they would be with accurate recall. Furthermore, the alternative to survey data--HMO and HCFA records--is not attractive. In particular, the possibility of **bias** from comparing HMO records for enrollees with the HCFA records for nonenrollees is quite high. Previous experience from the Medicare Competition Demonstrations (Nelson and Brown, 1989) and our own analysis of HMO records done for this study (documented in Appendix C), has revealed significant problems with estimates derived from administrative records of HMOs. These. problems resulted in the exclusion of many HMOs from the evaluation of the Medicare Competition Demonstrations and from the analysis conducted in Appendix C, which introduces the possibility of bias due to this nonrandom exclusion of plans from the sample. Thus, while the level of detail on service use obtained from the beneficiary survey is by necessity limited in its detail, HMO impacts based on survey measures are less likely to suffer from bias than would impacts estimated from the administrative records of HMOs and HCFA.

Survey data may also suffer from nonresponse bias, in that beneficiaries who are do not respond may be substantively different from those that do respond. Our analysis in Appendix C suggests that estimated impacts were not biased by the loss of observations due to nonresponse. Two-thirds of nonresponse was due to our inability to obtain phone numbers for some sample members. Response rates were quite high (82 percent for enrollees, 73 percent for nonenrollees), which further reduces concerns about possible bias, as does the availability of detailed data on beneficiary characteristics.

2. **Using Projected AAPCC Payments Rather than Actual Payments**

Our analysis of program impacts on costs to HCFA was based on 1989 Medicare reimbursements for nonenrollees. Although other time periods were considered (the 12-month interval prior to telephone interview and calendar year 1990), 1989 was the time period over which Medicare reimbursement records were most complete. Because all members of our nonenrollee sample were alive as of April 1, 1990, the dependent variable--l989 Medicare reimbursements--for this sample does not reflect the costs for individuals who died during the year. As noted in Chapter III, the most important implication of excluding decedents from the sample is that actual AAPCC payment rates will systematically overstate FFS costs for members of the sample. This is because payment rates are based on reimbursements for **all** beneficiaries, including decedents, whose reimbursements account for approximately 28% of total Medicare reimbursements. Comparing the actual payment rates for our sample for 1989 to predicted FFS costs for 1989 would show a large loss to HCFA--even with neutral selection--because these high-cost enrollees (decedents) are excluded from the analysis.

To resolve this problem, we predicted what payment rates would be for enrollees if average per capita Medicare costs by AAPCC risk classification were computed for this subset of beneficiaries. This procedure is appropriate since we are using the same actuarial. factors used in the payment rates to determine average per-capita costs for this subset of the Medicare population. However, unlike the Office of the Actuary, we did not have to predict what average per-capita costs would be two years hence, since we had actual reimbursements data for 1989. Thus, our estimate of costs to HCFA does not reflect any effect on HCFA's costs arising from incorrect projections of the USPCC. Actual cost impacts to the Medicare program and Medicare risk plans in 1989 may have differed from our estimate if the USPCC for 1989 misestimated actual average Medicare FFS costs per beneficiary for 1989. By including binary site variables, we also assume that the AAPCC correctly estimates the average FFS cost for nonenrollees in each market area.

The fact that we used predicted rather than actual payment rates does not change the interpretation of our overall costs impacts, but does affect the interpretation of results for subgroups defined by plan or market area characteristics. The objective in our analysis of costs to HCFA was not to determine the accuracy of the AAPCC in a specific year or market area, but rather to determine whether the current method of setting capitation payments is likely to generate systematic savings or losses to Medicare. Our chosen methodology for answering this question, using predicted payment rates, is preferable to using actual payment rates, since inaccuracies in projecting the USPCC or in estimating costs for an area relative to the national average may be transient in nature. Basing the cost estimates on payment rates which reflect inaccuracies specific to one year thus may yield misleading evidence on the systematic impact of the risk program. Since the average annual error in the USPCC is quite small, it is likely that our conclusions for the program as a whole are an accurate reflection of systematic costs to the program overall. However, since our methodology implicitly assumes that the AAPCC is correct on average for nonenrollees in each market area, any systematic errors in the area geographic adjustor are not reflected in our estimates. Our estimates reflect only costs to HCFA arising from biased selection that is not captured by the AAPCC demographic risk adjustor. Hence, differences in estimated effects on costs to HCFA enrollees in high AAPCC areas versus other areas or for enrollees in HMOs charging zero premiums versus those in HMOs with higher premiums reflect only differences in the degree of favorable selection in these different subgroups. They do not reflect differences across areas or HMOs in the accuracy with which county AAPCC rates estimate the average FFS costs for *nonenrollees.*

F. IMPLICATIONS

HMOs in the Medicare risk program are experiencing favorable selection, which results in capitation payments to the Medicare program that are 5.7 percent greater than payments to FFS providers would have been for the enrolled population, despite the fact that capitation rates are set at 95 percent of projected costs. This difference suggests that some alternative method for establishing payment rates may be appropriate.

The finding that over one-third of the AAPCC overestimate of costs is attributable to the lower proportion of enrollees who have a history of cancer, heart disease, or stroke suggests that this may be a useful type of variable to consider for inclusion in the payment system. Another possibility, based on our finding that cost increases are greatest for enrollees in plans charging zero premiums, would be to require that risk plans share some of their projected surpluses with HCFA, rather than being allowed to pass it all along to Medicare enrollees in the form of enhanced benefits and lower premiums.

The projected loss should also be noted in light of recent recommendations to raise payment rates to 100 percent of the AAPCC, or to reduce Part B premiums for beneficiaries enrolling in Medicare risk plans (see The President's Comprehensive Health Reform Program, p. 41). Such proposals may have been made under the assumption that the current program is saving 5 percent, as originally intended (e.g., see the interview with a HCFA official in Business and Health, April 1992, pp. 58-62). Our results suggest that if new enrollees are like the current stock of enrollees, the program will continue to lose money as enrollment expands. Increasing payment rates (or lowering Part B premiums) will only increase the costs to HCFA.

On the other hand, capitation does appear to encourage a more efficient use of all resources. Although the reductions in the amount of each of the services used among those with poor health might be cause for concern, the HMO savings in resources used are not accomplished by denying enrollees initial access to services. Indeed, controlling for characteristics, enrollees are just as likely as nonenrollees to be hospitalized and receive home health visits, and they are more likely to receive SNF care and some primary care. The results are thus encouraging for those who advocate capitation as a method for accomplishing cost-containment, since Medicare risk plans appear to reduce resource use while providing adequate access to health care providers.

Enrollees in Medicare risk plans appear to be the principal beneficiaries of favorable selection and HMO efficiency. Over one-third of beneficiaries in our sample were in plans charging zero premium. In addition, benefits covered by risk plans tend to be more generous than those covered by Medigap insurers. Sharing the benefits of HMO efficiency with enrollees in the form of lower premiums and more benefits was intended under the program, under the assumption that once the program become established, the Medicare program would save money or at least pay no more than it would have paid for enrollees if they had been in the FFS sector. However, favorable selection suggests that these additional benefits to enrollees are being paid for, in part, by the Medicare program.

The challenge then is to identify a payment methodology that accurately reflects what enrollees would have cost HCFA had they remained in the FFS sector, and to encourage a more neutral selection of enrollees. With these changes, the real cost-savings generated by Medicare risk plans would also be shared with the Medicare program, as envisioned at the program's inception.

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APPENDIX A

WEIGHTS USED IN THE ANALYSIS

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WEIGHTS USED IN THE ANALYSIS

A. WEIGHTING OBSERVATIONS TO REFLECT THE ENROLLED POPULATION

Unless indicated otherwise in the text, all regression models and means that compare the attributes of enrollees with those of nonenrollees were estimated with enrollee observations weighted so that the proportion of the weighted observations from a given Medicare risk plan in the sample reflected the plan's proportion of enrollees in the program population. As described in Chapter II, the sample drawn for the survey under-represents enrollees from the four largest TEFRA plans and over-represents enrollees from small plans, so that a minimum of approximately 40 enrollees were interviewed. Thus, to reflect their proportions in the program population, the enrollee observations from the four largest plans received weights of greater than 1.0, and the observations from the smaller plans that were over-sampled receivedweights of less than 1.0. The weights also must account for differential rates of nonresponse to the survey. To obtain the weights for enrollees in any given plan in the analysis, we took the ratio of the plan's proportion of enrollees in the program population to that plan's proportion of enrollees in the survey sample. Thus, for the i^{th} enrollee in the sample from the jth plan, we had the following weight:

(1) $W_{ij}^{e} = (N_j^{e}/N^{e})/(n_j^{e}/n^{e})$,

where:

N_j^e = the number of enrollees in plan j, as of 3/1/90 according to the OPHC report.

N^e = the number of enrollees in the Medicare risk plans with 1,000 or more enrollees as of 3/1/90.

n_j^e = the number of enrollees in the sample from plan j.

n^e = the total number of enrollees in the sample.

By construction, the sum of weights across the enrollee sample equals the total number of enrollees in the sample.

The number of nonenrollees selected for interviews equals the number of enrollees chosen for each market area. (In fact, the nonenrollee sample was selected to match the distribution of enrollees across ZIP codes within each market area.) Thus, our only requirement for weighting nonenrollee observations was that the proportion of weighted nonenrollee observations from any market area equal the proportion of weighted enrollee observations from that market area. This requirement ensured that the enrollee and nonenrollee samples were matched by geographic area. Furthermore, to ensure that the distribution of nonenrollee observations reflected the distribution of enrollee observations by county, we weighted nonenrollee observations by the ratio of (1) the proportion of weighted enrollee observations from county k to (2) the proportion of unweighted nonenrollee observations from county k. Thus, the weight for the i^{th} nonenrollee observation from county k is the following:

$$(2) \quad W_{ik}^{n} = (S_k/n^e)/(n_k^n/n^n),$$

where:

S_k = the sum of weighted observations for enrollees in county k.

n^e = the total number of enrollee observations, which by construction equals the sum of weighted enrollee observations.

n_k^n = the number of nonenrollee observations from county k.

n^n = the total number of nonenrollee observations.

Like the enrollee weights, the sum of weighted nonenrollee observations by construction equals the nonenrollee sample size.

B. WEIGHTING OBSERVATIONS SO THAT EACH PLAN HAS EQUAL WEIGHT

Attributes specific to plans may have influenced the outcomes examined in the analysis. For example, HMO impact on use of services may vary by model type (IPA, staff, or group). In analyzing whether plan attributes influenced impacts on service use, it was useful to conduct the analysis with enrollee observations weighted so that every plan in the sample has an equal weight. Doing so precludes the possibility that the large plans with the attribute would dominate the analysis. As an example impacts for IPAs may not differ from other model types as a general rule. However, one large IPA with an especially larger impact may yield the incorrect conclusion that IPAs overall have larger impacts. By weighting enrollee observations such that the weighted observations for each plan are equal, we precluded this possibility.

Since there were 75 plans in the analysis, the weights were constructed so that the sum of the weights for the observations from each plan summed to 1/75 of the total sum of weights. Thus, for the i^{th} enrollee from the j^{th} plan, we used the following weight:

$$(3)\quad W_{ij}^{e*} = (1/J)/(n_j^e/n^e),$$

where:

J = the number of plans in the analysis.

n_j^e and n^e are the same as above.

By construction, the sum of weights equals the sample size for enrollees.

The method for weighting nonenrollees was identical to the method used above in equation 2. After the enrollee weights were constructed by equation 3, the sum, S_k, was calculated for each county. The weights for nonenrollees were then constructed following equation 2.

C. WEIGHTS FOR THE ENROLLMENT MODELS

Two models presented in chapters III and IV, the sample selection bias model of Heckman (1979) and the switching regression model of Lee (1978), have an enrollment equation in the model's system of equations. When we estimated the probability that a Medicare beneficiary was enrolled (the enrollment equation), we had to weight enrollee and nonenrollee observations so that the proportion of weighted enrollee observations in the beneficiary sample reflected the proportion of Medicare beneficiaries enrolled in TEFRA plans in the market areas included in the analysis. Enrollees constitute only 8 percent of the beneficiary population in the 44 sites from which one sample was drawn, but, as noted in Chapter II, enrollee observations were chosen to constitute 50 percent of the sample used to analyze enrollee-nonenrollees differences in use and cost. (This larger percentage of enrollees compared with a random sample increased the statistical power of hypothesis tests and increased the precision of any point estimates of impacts in the analysis.)

The rationale and method for the weights is documented fully in Manski and Lerman (1977). For each enrollee, the weight is the following:

$$(4) \quad V_{ij}^{e} = (N^{e}/N)/(n^{e}/n) \; * \; W_{ij}^{e} = (N_j^{e}/N)/(n_j^{e}/n)$$

where:

N = the total number of beneficiaries in the market areas included in analysis (from the 1989 AAPCC master file).

n = the total number of observations from the survey (enrollees and nonenrollees).

For each nonenrollee in site j, the weight is the following:

$$(5) \quad V_{ij}^{n} = (N_j^{n}/N)/(n_j^{n}/n),$$

where:

N_j^n = the total number of nonenrollees in market area j included in the analysis (from the 1989 AAPCC master file).

n_j^n = the total number of nonenrollee observations from market area j.

N and n have the same meaning as before.

APPENDIX B

'REGRESSION RESULTS FOR SERVICE USE VARIABLES: A COMPARISON OF THE MODEL OF LEE (1978) WITH OLS

TABLE IV.9

REGRESSION RESULTS: HOME HEALTH VISITS

Independent Variables	Probability of Any SNP Days[a]	Number of SNF Days[b]	Probability of one or More Home Health Visits[a]	Number of Home Health Visits[a]	Probability of one or More Visits by Nurse or Therapist[a]	Number of Visits, Nurse or Therapist[b]	Probability of one or More Visits by a Home Health Aide	Number of Visits by a Home Health Aide
Intercept	-3.02 *** (.000)	-.916 (.399)	-2.29 *** (.000)	.176 (.769)	-2.42 *** (.000)	.404 (.287)	-2.82 *** (.000)	.586 (.184)
AAPCC Risks								
Age 65 - 69	-4.37 ** (.012)	-.443 (.372)	-.093 (.364)	.565 ** (.043)	.011 (.916)	.481 (.005)	-.177 (.215)	.148 (.468)
Age 70 - 74	-.282 * (.050)	-.466 (.328)	-.079 (.932)	310 (.253)	.052 (.604)	.255 (.121)	-.126 (.324)	.119 (.548)
Age 75 - 79	-.116 (.391)	-.349 (.481)	-.059 (.532)	.633 ** (.022)	.030 (.762)	.430 (.011)	-.026 (.831)	.260 (.189)
Age 80 - 84	-.097 (.482)	.414 (.429)	-.019 (.846)	-.046 (.849)	.047 (.647)	.033 (.832)	-.051 (.686)	.205 (.885)
Medicaid Buy-In	.095 (.562)	-.158 (.772)	.366 *** (.000)	1.28 *** (.000)	.169 * (.096)	.282 (.149)	.495 *** (.000)	.976 *** (.000)
Disabled	-.273 (.174)	-1.53 ** (.024)	-.020 (.871)	-.083 (.815)	.152 (.223)	.641 (.007)	-.303 * (.077)	-.400 (.147)
Institutionalized			-.419 *** (.003)	.322 (.563)	-.100 (.456)	2.92 (0.00)	-.801 *** (.000)	-1.89 *** (.000)
Sex (Male)	.050 (.580)	.285 (.231)	-.118 ** (.039)	-.091 (.518)	-.028 (.634)	-.018 (.812)	-.205 ** (.015)	-.022 (.810)
Health/Functional Status								
ADL Impairments	.090 ** (.048)	225 *** (.000)	.179 ** (.024)	2% *** (.000)	.126 *** (.000)	1.07 *** (.000)	.190 *** (.000)	1.73 *** (.000)
IADL Impairments	.156 *** (.000)	.224 ** (.049)	.206 *** (.000)	.437 *** (.000)	.200 *** (.000)	.152 *** (.000)	.211 *** (.000)	.318 *** (.000)
Poor Health	.069 (.596)	.190 (.717)	.179 ** (.024)	.980 *** (.001)	.132 (.111)	.310 (0.80)	.284 *** (.005)	.648 *** (.002)
Missing Value, Poor Health	.334 (.297)	.646 (.622)	.291 (.214)	.695 (.356)	.190 (.450)	-.284 (.541)	.289 (.354)	.463 ** (.016)
History of Heart Disease, Cancer, Stroke	.377 *** (.000)	.390 (.134)	.265 *** (.000)	.187 (.217)	.262 *** (.000)	.076 (.419)	.191 ** (.020)	.105 (.336)
Missing Value, Heart Disease, Cancer, Stroke	.439 (.266)	1.44 (.401)	.903 *** (.001)	2.85 *** (.004)	.843 *** (.001)	266 *** (.000)	.903 *** (.005)	229 *** (.001)
Died Within 9 Months of Interview	.185 (.208)	289 *** (.000)	.435 *** (.000)	1.31 *** (.000)	.474 *** (.000)	.888 *** (.000)	.256 ** (.040)	.366 (.151)
Preferences for Seeking Care								
Worry About Health	.087 (.416)	.583 * (.060)	.003 (.968)	.114 (.536)	.038 (.591)	.141 (1.99)	-.066 (.488)	.005 (.922)
Missing, worry About Health	.068 (.772)	-.891 (.240)	.004 (.981)	-.849 ** (.050)	-.028 (.868)	-.495 (.060)	-.050 (.818)	-.539 * (.081)
Avoid Seeing Doctor, if Problem	-.015 (.885)	-.393 (.143)	-.166 ** (.013)	-.397 ** (.011)	-1.82 ** (.010)	-.149 (.199)	-.097 (.294)	-.196 * (.077)
Missing, Avoid Doctor	.273 (.337)	-.977 (.361)	-.440 * (.061)	-1.67 *** (.005)	.278 (.226)	-.807 ** (.024)	-.507 (.106)	-1.23 *** (.004)
usual Place of Care	-.086 (.553)	-.417 (.265)	-.031 (.743)	-.495 ** (.021)	.030 (.769)	-.263 ** (.044)	-.121 (-347)	-.184 (.232)
Missing, Usual Place of Care	.510 (.143)	-1.44 (.298)	-.031 (.981)	.645 (.428)	.027 (.898)	.457 (.351)	-.139 (.656)	.248 (.668)

In the second step, the FFS equation was estimated by regressing service use for nonenrollees on λ_{i1} and the other variables included in our basic impact model. Similarly, the HMO equation was estimated by regressing service use for enrollees on λ_{i2} and the other explanatory variables. The coefficient on the λ terms are used to test for sample selection bias, i.e., for whether estimating the equation on the nonenrollee sample using OLS produces biased estimates. If so, then predicted use for enrollees had they remained in the FFS sector, based on the OLS estimates, will be biased. In turn, the HMO impact (the difference between actual enrollee use and predicted use for enrollees under FFS) will also be biased.

As in the model of Heckman (1979), a significant coefficient on lambda indicates selections bias. For each category of service use--hospital, physician visits, home health, and SNF days--we estimated the FFS and HMO models with lambda included in the equation, and tested for selection bias. In all instances, lambda was not significant, indicating that we have no evidence that OLS produces biased estimates of the FFS and HMO equations.

Tables B.l and B.2 report the results of the FFS equations estimated by OLS, with and without lambda included in the regressions. (The pattern of results is similar for the HMO equations which are not reported). Note that the coefficients are quite similar for the two specifications. This provides further evidence that including lambda in the model does not alter the results obtained from OLS.

TABLE! B.1

REGRESSION RESULTS: HOSPITAL AND PHYSICIAN USE
(p-values are given in parentheses below coefficients)

	Number of Hospital Days		Number of Physician Visits, Past 4 weeks	
Independent Variables	OLS	Selection Bias Model	OLS	Selection Bias Model
Intercept	-5.56 • *' (.000)	-4.71 • ** (.002)	-.071 (.717)	-.192 (.407)
AAPCC Risks				
Age 65 - 69	1.58 • ** (.005)	1.65 • ** (.004)	.140 (.131)	.145 (.122)
Age 70 - 74	2.19 • ** (.000)	2.28 • ** (.000)	.208 • * (.020)	.215 • * (.017)
Age 75 - 79	1.40 • * (.012)	1.48 *** (.009)	.222 ** (.015)	.229 • * (.013)
Age 80 - 84	1.37 • * (.018)	1.42 • * (.015)	.055 (.580)	.059 (.547)
Medicaid Buy-In	-1.16 ** (.026)	-1.40 • * (.014)	-.140 (.349)	-.186 (.235)
Disabled	260 • ** (.000)	249 • * (.023)	.371 • * (.016)	.346 • * (.027)
Institutionalized	-1.64 • * (.032)	-1.78 • * (.023)	-.324 • (.083)	-.352 • (.067)
Sex (Male)	.619 • * (.032)	.651 • * (.026)	-.008 (.832)	-.003 (.948)
Health/Functional Status				
ADL Impairments	.710 *** (.002)	.688 • ** (.002)	-.010 • * (.043)	-.014 • * (.039)
IADL Impairments	.760 • '* (.000)	.754 • ** (.000)	.086 • * (.000)	.086 • ** (.000)
Poor Health	1.85 • ** (.001)	1.75 • ** (.001)	.652 • ** (.000)	.637 • ** (.000)
Missing Value, Poor Health	5.43 *** (.001)	5.44 • ** (.000)	1.29 *** (.000)	1.29 • ** (.000)
History of Heart Disease, Cancer, Stroke	1.84 • ** (.000)	1.84 • ** (.000)	.326 • *' (.000)	.326 *** (.000)
Missing Value, Heart Disease, Cancer, Stroke	7.27 • ** (.000)	7.26 • ** (.000)	.361 (.307)	.359 (.308)
Died Within 9 Months of Interview	3.09 *** (.000)	3.10 • ** (.000)	.568 • '* (.000)	.569 • ** (.000)
Preferences for Seeking Care				
Worry About Health	1.05 *** (.005)	1.04 • ** (.005)	.176 • ** (.004)	.175 • ** (.004)
Missing, Wony About Health	-1.26 (.110)	-1.26 (.113)	.211 (.185)	.214 (.182)
Avoid Seeing Doctor, if Problem	-.233 (.494)	-.226 (.499)	-.128 • ' (.011)	-.127 • * (.011)

TABLE **B.1** (continued)

Independent Variables	Number of Hospital Days: OLS	Number of Hospital Days: Selection Bias Model	Number of Physician Visits, Past 4 Weeks: OLS	Number of Physician Visits, Past 4 Weeks: Selection Bias Model
Missing, Avoid Doctor	.280 (.780)	280 .790)	.232 (.272)	-.232 (.271)
Usual **Place** of Care	1.18 • * (.023)	.913 (.112)	.134 • * (.029)	.102 (.142)
Missing, Usual Place. of Care	-2.98 (.226)	-2.98 (.228)	-.232 (.317)	-.230 (.318)
Market Area Characteristics				
Metro Statistical **Area >250,000**	-.264 (53)	-.320 (.440)	-.107 • (.088)	-.107 • (.092)
Physicians per Capita	.826 (.328)	.484 (.587)	-.009 (.902)	-.060 (.673)
Surgeons per Capita	-4.25 (.197)	-3.09 (.372)	.162 (.751)	.331 (.547)
Hospital Beds per Capita	.261 • * (.038)	.273 • * (.032)	-.022 (.278)	-.020 (.316)
County AAPCC Rate, Part A	.011 (.150)	.010 (.219)	.002 (.169)	.001 (247)
County AAPCC Rate, Part B	.003 (.567)	.005 (.376)	-.0003 (.655)	-.0001 (.904)
Income/Education/Race				
Minority Race	663 (247)	.653 (256)	-.131 (.111)	-.130 (.118)
Missing **Race** Data	7.01 • ** (.000)	6.97 • ** (.000)	-.134 (647)	-.142 (.623)
Income, **$1,000's**	-.058 (.219)	-.068 (.157)	.022 • (.066)	.019 (.131)
Missing Income Data	-.428 (321)	-.557 (.211)	.049 (533)	.029 (.713)
Highest Degree, College	.874 • (.065)	.819 • (.089)	.037 (646)	.033 (.671)
Highest Degree, High School	.533 (.116)	.517 (.132)	.021 (.683)	.020 (.693)
Missing **Education** Data	-.523 (.590)	-.511 (.590)	-.247 (.150)	-.274 (.153)
λ (Lambda)		2.09 (.253)	--	.100 (.323)
Mean of Dependent Variable	249	249	.690	.690
R^2	.091	.092	.045	.045
N	6.071	6.071	6.013	6,013

• Significant at **.10** level, two-tailed test.
• * Significant at **.05** level, two-tailed test.
***Significant at **.01** level, two-tailed test.

TABLE B.2

REGRESSION **RESULTS:** HOME **HEALTH** AND SNF DAYS
(p-values **given in** parentheses)

	Home Health **Visits**		Estimated SNF Days	
Independent Variables	**OLS**	Selection Bias Model	**OLS**	Selection Bias Model
Intercept	**.188** (.765)	**.057** (.940)	1.53 (.338)	**.783** (.716)
AAPCC Risks				
Age 65 - 69	.668 • * (.018)	.658 • * (.071)	-1.38 ** (.046)	-1.45 • (.078)
Age **70** - 74	.447 (.101)	.434 (.118)	-1.46 ** (.030)	-1.54 • (.054)
Age 75 - 79	.727 • ** (.010)	.715 • * (.012)	-2.03 • ** (.004)	-2.10 • * (.011)
Age 80 - 84	.134 (.657)	.126 (.673)	-.820 (.262)	-.868 (.310)
Medicaid Buy-In	.374 (.165)	.410 (.159)	-.005 (.942)	.197 (.874)
Disabled	.492 (.163)	.509 (.155)	-3.21 • ** (.000)	-3.12 *** (.003)
Institutionalized	3.87 • ** (.000)	3.88 • '* (.000)		
Sex (Male)	.036 (.790)	.031 (.828)	.215 (.545)	.186 (.649)
Health/Functional Status				
ADL Impairments	1.31 • ** (.000)	1.31 *** (.000)	3.27 • ** (.000)	3.29 *** (.000)
IADL Impairments	.119 • (.058)	.119 • (.060)	.127 (.428)	.129 (.483)
Poor Health	-.018 (.903)	-.004 (.988)	.681 (.318)	.764 (.340)
Missing Value, Poor Health	-.513 (.495)	-.514 (.486)	1.41 (.434)	**1.40** (.497)
History of Heart Disease, Cancer, Stroke	.134 (389)	.134 (.384)	.441 (.237)	.439 (.312)
Missing Value, Heart Disease, Cancer, Stroke	4.15 • *' (.000)	4.15 • ** (.000)	1.10 (.637)	1.11 (.678)
Died Within 9 Months of Interview	1.63 *** (.000)	1.63 • ** (.000)	232 • * (.011)	2.31 ** (.031)
Preferences for **Seeking** Care				
Worry About Health	.338 • (.063)	.339 • (.065)	1.10 • * (.014)	1.11 • * (.034)
Missing, Wony About Health	-.886 • * (.027)	-.885 • * (.028)	-1.17 (.238)	-1.18 (.311)
Avoid Seeing Doctor, if Problem	-.117 (.485)	-.118 (.474)	-.441 (.270)	-.445 (.339)

TABLE B.2 (continued)

Independent Variables	Home Health Visits OLS	Home Health Visits Selection Bias Model	Estimated SNF Days OLS	Estimated SNF Days Selection Bias Model
Missing, Avoid Doctor	-1.48 • ** (.009)	-1.48 • ** (.009)	-.700 (.630)	-.697 (.674)
Usual Place of Care	-.230 (.405)	-.184 (.546)	-1.88 • ** (.005)	-1.61 • (.067)
Missing, Usual Place of **Care**	6.11 • ** (.000)	6.11 • '* (.000)	.674 (.832)	.665 (.876)
Market Area **Characteristics**				
Metro Statistical Area **>250,000**	.060 (.763)	.068 (.740)	.018 (. 922)	.065 (.910)
Physicians per Capita	-.298 (.482)	-.247 (.577)	-.116 (.875)	-.170 (.892)
Surgeons per Capita	.864 (.607)	.690 (.688)	3.07 (.450)	2.09 (.668)
Hospital Beds per Capita	.060 (.344)	.058 (.357)	-.405 •• (.008)	-.415 • * (.021)
County **AAPCC** Rate, Part A	-.001 (.721)	-.001 (.768)	.013 (.428)	.014 (.227)
County **AAPCC** Rate, Part B	-.026 (.288)	-.003 (.263)	-.010 • (.087)	-.011 (.113)
Income/Education/Race				
Minority Race	-.466 • (.094)	-.464 • (.099)	.505 (.469)	.516 (.520)
Missing Race Data	-.426 (.600)	-.421 (.597)	2.53 (.181)	2.56 (.249)
Income, **$1,000's**	.001 (.673)	.001 (.627)	.0004 (.901)	.001 (.849)
Missing Income Data	.045 (.814)	.064 (.768)	-.633 (.215)	-.524 (.398)
Highest Degree, **College**	-.178 (.457)	-.169 (.474)	.303 (.604)	.356 (.560)
Highest Degree, High School	-.147 (.392)	-.145 (.394)	1.05 •• (.011)	1.07 • * (.027)
Missing Education Data	.389 (.773)	.388 (.439)	.290 (.802)	.280 (.848)
A (Lambda)		-.309 (.732)	--	-1.72 (.499)
Mean of Dependent Variable	.539	.539	.863	.863
R^2	.088	.088	.031	.037
N	5.849	5,849	5,727	5,727

• **Significant** at **.10** level, two-tailed test.
• * Significant at **.05** level, two-tailed test.
• ** Significant at **.01** level, two-tailed test.

APPENDIX C

EVIDENCE OF WHETHER ESTIMATES OF HMO IMPACTS ARE BIASED DUE TO SURVEY NON-RESPONSE

APPENDIX c

EVIDENCE OF WHETHER ESTIMATES OF HMO IMPACTS ARE BIASED DUE TO 'SURVEY NONRESPONSE

One concern with using survey data is that nonresponse to the survey may lead to a nonrepresentative sample and biased estimates of program impacts. This problem was of special concern for analyses of HMO impacts on the use and cost of services because beneficiaries who were in the poorest health (and therefore using a higher than average level of services) may be the least likely to respond to the survey. In this appendix we present evidence that indicates that our estimates are not biased by nonresponse to our survey.

A. INTRODUCTION

Although nonrespondents have higher average hospital utilization rates than responders in the records data that we examine, the difference is not a major concern to the evaluation for two reasons. First, the survey collected information on an extensive set of control variables that, when used in regressions, should eliminate any differences between enrollees and nonenrollees that is due to differential nonresponse. Second, the difference between respondents and the full sample (respondents and nonrespondents) when controlling for just a few demographic characteristics that are available from records data is too small (0.4 percent for 1989 and 0.2 percent for 1990) to have much influence on the estimated effects.

Two conditions must be met for nonresponse to yield biased estimates of HMO effects: (1) the expected value of the dependent variable (health care use in our case), given a set of explanatory variables, must be different for nonrespondents than for respondents, and (2) response rates must differ for enrollees and nonenrollees. Table C.l shows that the second condition is met: response rates are high for both enrollees and nonenrollees (81.6 percent, and 72.6 percent, respectively), but

TABLE C.1

RESPONSE RATES AND REASONS FOR NONRESPONSE
(Percent)

	Enrollees	Nonenrollees
Complete	81.6 %	72.6 %
Incomplete	18.4	27.4
Telephone number unavailable	12.1	16.6
Refused	4.2	7.2
Unable to respond	1.7	2.5
Never answered/telephone problems	0.4	1.1
Total Number of Interviews Attempted	7,937	8,798

NOTE: The table excludes 96 enrollees and 202 nonenrollees for whom a telephone contact was made but the individual was determined to be ineligible for interview. Individuals were ineligible if they (1) died prior to the sampling date (April 1, 1990), or (2) were in the enrollee sample but asserted that they were never a member of the HMO, or (3) were in the nonenrollee sample but enrolled in an HMO between the date of sample selection and the date of the interview.

the rates differ by nine percent. However, sample members fail to complete interviews for a variety of reasons, only some of which are likely to be correlated with health care use. The nonresponse categories in Table C.l that are likely to be correlated with service use variables include "refused," "unable to respond," and "never answered;" beneficiaries who were not interviewed for these reasons may well have been ill and may be especially likely to have had a stay in a hospital or nursing home. In other words, sick or institutionalized sample members are much less likely to complete an interview than sample members who are well. The nonresponse categories that are ***not likely*** to affect use variables include "telephone problems," and "telephone number unavailable;" nonresponse in these two categories probably has no relationship with use variables. The difference between the proportions of enrollees and nonenrollees in the latter categories is about four and one-half percent, accounting for about half of the overall difference in response rates for enrollees and nonenrollees of nine percent.[1] Thus, the relevant difference in nonresponse rates is about 4.5 percentage points.

We also suspect that the first condition required for bias (as noted above) is met--a different conditional expected value (regression-adjusted mean) of the dependent variables for respondents and nonrespondents. However, this is not so easy to prove, and the magnitude of the difference determines the size of the bias. Fortunately, we have other sources of data on utilization, HMO and HCFA records, that are available for beneficiaries regardless of whether they completed the interview and which can be used to compare the respondents to the nonrespondents. This analysis will provide measures of the likely bias.

'The reasons for nonresponse that are grouped in the category "telephone number unavailable" include unlisted telephone numbers, beneficiaries who are without telephones, and telephone numbers that cannot be located.. The reasons for nonresponse that are grouped in the category "telephone problem" include connection problems. The reasons for nonresponse that are grouped in the category "refused" include refusals by sample members, by proxy respondents, and by other people who answer the telephone for the respondent. The reasons for nonresponse that are grouped in the category "unable to respond" include language barriers, incapable sample members with no proxy available, and sample members that are not at home or who are dead with no proxy respondent available. The reasons for nonresponse that are grouped in the category "never answered" include unanswered rings, busy signals, and answering machines.

Our expectation is that if heavy users of services are less likely to respond than other beneficiaries, and response rates are lower for nonenrollees, the mean for nonenrollees is biased downward to a greater degree than the mean for enrollees when the sample is limited to responders. That is, our concern is that we may be losing a larger proportion of the high use cases in the nonenrollee sample than in the enrollee sample, lowering the mean for nonenrollees by a greater amount than the reduction in the mean for enrollees.

B. DATA AND METHODOLOGY

1. Data

Because the effects of nonresponse cannot be examined directly by using survey data on dependent variables, we look for other sources of data on our dependent variables that would be available for the full sample. However, such sources are not readily available; if another source could provide the survey data elements, we would have no reason to perform the survey. Thus, sources that reflect the nature of the survey data on use and costs must be identified.

HCFA data on utilization of hospital services, skilled nursing services, and Part B physician expenses are readily available for all nonenrollees from the Medicare Automated Data Retrieval System (MADRS) files. While these -data allow us to compare respondents to nonrespondents for the nonenrollees, they do not enable us to assess whether the enrollee-nonenrollee *difference* in utilization is affected by the differential nonresponse. HCFA does not maintain data on the service use of enrollees in HMOs.

To obtain data for enrollees, we asked 25 Medicare risk plans (selected according to size) to provide admission and discharge dates for all hospital and skilled nursing facility stays occurring in 1989 and 1990 for Medicare members. We received machine-readable data from 16 of these plans.

Unfortunately, the data that we received from risk plans varied in quality and quantity. Many of the Medicare risk plans did not provide Medicare health insurance claim numbers, and some could provide data for only a particular time period or data on paid claims only. Data on the use of skilled nursing facilities were maintained well by so few plans that we could not use the data in the analysis of nonresponse.

When the survey sample cases were matched to the HMO data, we found very low rates of hospital use for many of the HMOs, rates too low to be plausible (e.g., less than 10 percent). There were two explanations for this--incomplete data on the hospital stays provided by the HMO, and poor data on identification numbers (despite our request for Medicare identification numbers), inhibiting our ability to match the hospital data to the beneficiaries in the survey sample.

To investigate these problems further and obtain some insight into the nature of the data problems, we constructed two admission rate measures for each HMO. An "implied" admission rate was obtained by dividing the total number of beneficiaries for whom a hospital admission was recorded in the HMO data for 1989 by the number of beneficiaries enrolled in the HMO at the midpoint of the year (July 1, 1989). The same procedure was used to get an implied admission rate for 1990. The second procedure was to match the "test sample" of approximately 1,000 enrollees from each plan that was drawn for our biased selection analysis to the HMO data on hospital stays and compute a hospitalization rate. The first measure provides an indication of the completeness of the HMO data; the second measure captures problems in the quality of the identification numbers. The admission rates and data on which they were based are contained in Table C.2.[2]

The implied admission rates showed 5 of the 15 plans to have rates less than 10 percent for 1989, far too low to be plausible, since 15 percent of enrollee respondents said they had been hospitalized in the past year. Admission rates for another 5 plans fell between 11 and 14 percent. The remaining 5 plans admitted 14 to 21 percent of their enrollees in 1989, using this crude measure. Rates for

[2]Further evidence that admission data received from some HMOs were incomplete was obtained by comparing the HMO data to dummy claims (no-pay bills) submitted to HCFA for HMO members. In many cases, a high proportion of admissions recorded in HCFA's files were not recorded in the HMO data, even though the dummy claims are known to be incomplete.

TABLE C2

IMPLIED ADMISSION RATES USING DATA PROVIDED BY 16 HMOs

	1989				1990			
Plan	Number of Enrollees with a Hospital Admission	Enrollment as of July 1	Implied Admission Rates	Admission Rate for Test Sample[b]	Number of Enrollees with a Hospital Admission	Enrollment as of July 1	Implied Admission Rate[a]	Admission Rate for Test Sample[b]
A	4,979	22948	21.7	14.9	4,653	24,799	18.8	11.8
B	10,054	74,749	13.5	126	15326	83,966	14.7	14.2
C	11,007	81,383	13.5	5.2	13,843	100,006	13.8	5.3
D	2676	19,542.	13.7	8.0	2,855	20,479	13.9	8.9
E	15,895	142052	11.2	6.4	17,024	169,633	10.0	5.1
F[c]	126	1,414	8.9	5.2	188	1,428	13.2	9.0
G	282	3,934	1.2	6.0	455	3,118	14.6	6.7
H	1,556	9,592	16.2	13.5	1,778	11,718	15.2	12.2
I	1,488	8,595	17.3	13.2	1,617	9,265	17.5	125
J[d]	55	8,351	6.6	0.4	1,725	10,017	17.2	16.0
K	908	14,311	6.4	0.0	912	16,999	5.4	0.0
L	7,136	41,291	17.3	29.6	7,317	41,840	17.5	25.1
M	1,052	7,438	14.1	9.9	801	7,399	10.8	7.0
N	2,504	26,595	9.4	8.2	2,763	27,275	10.1	7.0
O[e]	838	6,061	13.8	128	1,139	6,800	16.8	16.2
P	•	•	.	•	.	.	•	•

[a]The implied admission rate was obtained by dividing the total number of enrollees with a hospital admission, as determined from the data supplied by the HMOs for the year in question, by the HMO's Medicate risk plan enrollment as of July 1 of that year (obtained from the OPHC monthly report). Although not a highly accurate measure of the admission rate, it provides a basis for assessing the completeness of the admission data supplied by HMOs, without regard to the reliability of the Medicate ID numbers.

[b]"Test sample" refers to the sample of enrollees that was used in another analysis in the TEFRA evaluation where we selected at least 1,000 enrollees from each plan.

TABLE C.2

[c]Includes hospital claims paid from July 1, 1989 through December 31, 1990. Thus, many claims are likely to be missing for both 1989 and 1990.

[d]Data were provided only for 1990.

[e]Admissions during the first three months of 1989 were not included in the data from this plan.

*One of the 16 plans provided data for members of the survey sample only.

1990 were generally higher (only 1 below 10 percent, 6 between 10 and 14 percent, 8 between 14 and 19 percent).

Even among the plans with rates in the plausible range, however, a substantial number had very low rates when matched to specific individuals in our test sample. For 1989, 9 of the 15 plans had rates below 10 percent; in 1990, 8 plans had rates this low. In virtually every case where rates for the test sample were very low, data on Medicare ID numbers was not provided and matching was done on the basis of social security numbers, which often differ from the numeric portion of the beneficiary's Medicare ID number. Furthermore, we suspect that data on Medicare ID numbers and social security numbers are often erroneous, since HMOs have little reason to ensure that they are accurate.

Of the 7 plans with test sample admission rates exceeding 10 percent for 1990 (all but one also had rates exceeding 10 percent for 1989), we included 5 in our analysis--plans B, H, J, L, and 0. The two plans that had fairly plausible rates but were not used, plans A and I, were dropped because each plan had a large proportion of cases for which there were no-pay bills (dummy claims) in the MADRS data indicating a hospital stay, but no matched record in the HMO hospital data. Although the dummy claims are themselves known to be missing for many hospital admissions, of the Plan A cases for which there were dummy claims indicating a hospital stay, no match was found on the HMO data tape for 70 percent. For plan I the rate was 38 percent. These estimates, implying substantial underreporting by the HMOs, led us to exclude these plans from our nonresponse analysis. The five included plans had much smaller proportions of cases with dummy claims but no matched HMO data on hospital use.

2. Methodology

Our analysis of survey nonresponse proceeds according to the following plan.

a) Compare the demographic characteristics of respondents and nonrespondents to determine how nonrespondents differ and if the differences apply to both enrollees and nonenrollees

b) Compare the hospital utilization (a proxy dependent variable for measures of health care utilization from the survey) of respondents and nonrespondents and determine whether any differences apply to both enrollees and nonenrollees

c) Determine whether any differences in hospital use between respondents and nonrespondents can be "explained" or accounted for by differences in demographic characteristics, using a regression model.

d) Determine whether the effect of enrollment status on hospital utilization, when estimated on only the respondents, differs from the estimate obtained for the full sample (respondents and nonrespondents).

A comparison of the demographic characteristics of respondents to that of nonrespondents will indicate whether we should expect differences between respondents and nonrespondents on service use. Data on demographic characteristics were obtained from HCFA for both enrollees and nonenrollees and therefore are of comparable quality for the two groups. Table C.3 shows differences between respondents and nonrespondents on demographic characteristics from HCFA data for both enrollees and nonenrollees. For both groups, nonrespondents are more likely to be age 75 or older, welfare recipients, disabled, and of nonwhite race, with some of the differences being fairly sizeable. These differences portend higher rates of utilization for nonrespondents, because Medicare beneficiaries who are older, welfare recipients, disabled, or nonwhite tend to use more health care services than other beneficiaries. The respondent-nonrespondent differences for enrollees tend to be somewhat smaller than the differences for nonenrollees, and not statistically significant. However, the lack of significance is due in part to the smaller sample size for enrollees. (When comparing respondents to nonrespondents for enrollees from all 16 HMOs, the same pattern of differences was found, and the differences were statistically significant for welfare and disability status, and nonwhite race.)

TABLE C.3

'DISTRIEKJTIONS OF RESPONDENTS AND NONRESPONDENTS ON PATIENT CHARACTERISTIC VARIABLES FOR ENROLLEES AND NONENROLLEES
(Percent)

	Enrollees		Nonenrollees		
	Respondents	Non-Respondents	Respondents		Non-Respondents
	(N = 799)	(N = 171)	(N = 6,110)		(N = 2,235)
Age As of January 1990 for Those 65 or Older					
65-69	32.7	36.0	29.2		24.8
70-74	31.1	18.3	27.9		22.9
75-79	17.7	23.8	19.0		20.3
80-84	12.3	13.4	13.2		16.1
85 or more	6.2	8.5	10.7		15.9
Mean	73.6	74.3	74.6	*	76.2
Percent Male	45.1	41.5	41.5		39.0
Percent on Welfare	1.4	2.9	8.6	*	16.5
Percent Whose Current Reason for Entitlement to Medicare is Disability	2.4	4.1	8.3	*	12.0
Percent Whose Original Reason for Entitlement to Medicare Was Disability	9.0	11.1	14.1	*	18.2
Percent Nonwhite (race)	9.9	14.0	9.8	*	15.1

NOTE: Data on beneficiary characteristics were obtained from HCFA's Master Beneficiary file.

*Denotes that difference of proportions between respondents and nonrespondents is significantly different from zero at the .01 level, using a two-tailed test.

Table C.4 supports the hypothesis that nonrespondents use more hospital and skilled nursing services than respondents among nonenrollees, but shows less definitive results for enrollees. Nonenrollee nonrespondents have significantly higher utilization than nonenrollee respondents for several measures of utilization in the table (number of hospital and skilled nursing admissions, and the number of hospital and skilled nursing days for both 1989 and 1990). The differences in means range from 30 to 100 percent. However, while nonenrollee respondents and nonrespondents sometimes differ markedly from each other, limiting the sample to respondents does not greatly distort the results from the full sample of nonenrollees, since the response rate is high:

Use Measure	Respondents	Full Sample
Number of Admissions		
1989	0.22	0.24
1990	0.21	0.23
Hospital Days		
1989	1.82	2.02
1990	1.84	2.03
SNF Stays		
1989	0.03	0.03
1990	0.02	0.03
SNF Days		
1989	1.00	1.22
1990	0.72	0.92

For enrollees, the differences between respondents and nonrespondents tend to go in the same direction as for nonenrollees, but are smaller in magnitude, and none of the differences are statistically significant.

Some of the differences in the utilization of nonenrollee respondents and nonrespondents can be explained by the differences in demographic characteristics that we found in Table C.3. The regression estimates in Table C.5 show that when controlling for measures of demographic

TABLE C.4

DISTRIBUTIONS OF RESPONDENTS AND NONRESPONDENTS ON SERVICE UTILIZATION VARIABLES FOR ENROLLEES FROM FIVE HMOs AND NONENROLLEES
(Percent)

	Enrollees		Nonenrollees	
	Respondents	Non-Respondents	Respondents	Non-Respondents
	(N = 799)	(N = 171)	(N = 6,110)	(N = 2,235)
Hospital Use Variables				
Number of 1989 Admissions				
Zero	88.5 %	83.9 %	85.0 %	81.5 %
One	8.8	11.8	10.7	11.8
Two	1.9	3.1	2.7	4.1
Three or more	0.8	1.2	1.6	2.4
Mean	0.15	0.22	0.22 *	0.30
Number of 1990 Admissions				
Zero	91.0	91.2	84.9	81.7
One	7.1	7.0	10.9	12.7
Two	1.4	1.2	2.9	3.4
Three or more	0.5	0.6	1.3	2.2
Mean	0.12	0.11	0.21 *	0.28
Number of Hospital Days in 1989				
Zero	88.1	83.9	84.7	81.2.
One to three	5.0	4.4	3.5	3.5
Four to seven	2.2	6.2	4.3	5.1
Eight or fourteen	3.1	3.0	4.0	4.8
Fifteen or more	1.6	2.4	3.5	5.3
Mean	0.92	1.13	1.82 *	2.60
Number of Hospital Days in 1990				
Zero	90.9	91.2	84.7	81.3
One to three	2.5	2.9	2.9	3.8
Four to seven	3.1	2.3	4.6	4.8
Eight or fourteen	2.5	1.8	4.1	4.3
Fifteen or more	1.0	1.8	3.8	5.8
Mean	0.75	0.80	1.84 *	2.56
Length of Stay in 1989				
One to three	30.6	24.0	25.8	22.5
Four to seven	35.7	48.0	36.7	38.4
Eight to fourteen	22.5	20.0	27.3	27.3
Fifteen or more	11.2	8.0	10.2	11.8
Mean	6.16	5.97	7.60	8.40

TABLE C.4 (continued)

	Enrollees		Nonenrollees	
	Respondents	Non-Respondents	Respondents	Non-Respondents
	(N = 799)	(N = 171)	(N = 6,110)	(N = 2,235)
Length of Stay in 1990				
One to three	23.9	23.3	22.0	23.2
Four to seven	44.9	30.0	37.2	36.4
Eight to fourteen	26.8	40.0	29.3	24.5
Fifteen or more	4.4	6.7	11.4	15.9
Mean	6.36	6.21	8.42	9.07
Skilled Nursing Facility				
Number of 1989 Admissions	N / A	N/A		
Zero			98.2	97.2
One or more			1.8	2.8
Mean			0.03 *	0.05
Number of 1990 Admissions	N/A	N/A		
Zero			98.3	96.8
One or more			1.7	3.3
Mean			0.02 *	0.04
Number of 1989 Days	NA/	N/A		
Zero			98.1	97.1
One to thirty			1.0	1.0
Thirty one or more			0.9	1.9
Mean			1.00 *	1.82
Number of 1990 Days	N/A	N/A		
Zero			98.1	96.3
One to thirty			1.1	2.2
Thirty one or more			0.8	1.5
Mean			0.72 *	1.48

SOURCE: Data on utilization of nonenrollees were obtained from the Medicare Automated Data Retrieval System (MADRS) files. Data on utilization of enrollees were obtained from the five HMOs supplying useable data. The total number of enrollee cases for 1989 data drops to 739 respondents and 161 nonrespondents because the data for one HMO could not be used for 1989.

N/A: Denotes that data on the utilization of skilled nursing facilities for enrollees were not of sufficient quality to include.

*Denotes that difference of means is significantly different from zero at the .01 significance level, using a two-tailed test.

TABLE C.5

REGRESSION ESTIMATES OF EFFECTS OF PATIENT CHARACTERISTICS AND RESPONSE TO SURVEY ON HOSPITAL UTILIZATION FOR NONENROLLEES

	Number of Hospital Stays		Number of Hospital Days		Whether Hospitalized[a]	
	1989	1990	1989	1990	1989	1990
Variable						
Responded to Survey	-0.05 • * (-3.16)	-0.05 ** (-3.39)	-0.57 • * (-2.66)	-0.53 • * (-2.87)	-0.09 • * (-2.65)	-0.09 * (-2.56)
Age 70-74	0.06 ** (293)	0.34 (1.76)	0.50 (1.90)	0.48 * (2.08)	0.16 • * (334)	0.11 * (2.24)
Age 75-79	0.08 • * (338)	0.05 • (234)	0.59 * (2.04)	0.66 • * (261)	0.20 • * (3.90)	0.20 • * (4.01)
Age 80-84	0.16 • * (6.22)	0.14 • * (6.01)	1.38 • * (4.25)	1.91 • * (6.79)	0.40 ** (7.64)	0.38 • * (7.32)
Age 85 or More	0.16 • * (5.88)	0.17 • * (6.76)	1.31 • * (3.80)	1.90 • * (634)	0.40 • * (7.28)	0.46 ** (8.79)
Disabled	-0.07 (-1.64)	-0.09 * (-2.41)	-0.42 (-0.83)	-0.35 (-0.79)	-0.08 (-1.05)	-0.16 • (-2.03)
Originally Entitled Due to Disability	0.17 ** (5.16)	0.10 • * (334)	1.29 ** (3.19)	0.95 • * (2.70)	0.26 • * (4.30)	0.24 • * (3.88)
Welfare Recipient	0.11 • * (4.40)	0.10 • * (4.15)	1.31 • * (4.13)	1.24 • * (4.50)	0.22 • * (4.87)	0.19 • * (4.04)
Male	0.05 • * (3.25)	0.05 • * (3.16)	0.55 • * (2.82)	0.29 (1.72)	0.13 • * (4.00)	0.07 • (2.23)
Nonwhite	0.02 (0.81)	-0.05 * (-245)	0.48 (1.61)	-0.33 (-1.29)	-0.03 (-0.54)	-0.16 • * (-3.11)
Dependent Variable Mean	0.24	0.23	2.03	203	0.16	0.16
R^2	0.02	0.01	0.01	0.01		
Sample Size	8,345	8,345	8,345	8,345	8,345	8,345

NOTE: All explanatory variables are binary. Data were obtained from the Medicare Automated Data Retrieval System (MADRS) files. t-statistics are given in parentheses.

[a]Logit coefficients are not directly interpretable as the effect of the independent variable on the probability of being hospitalized. An estimate of this effect can be obtained be evaluating the derivative of the logit with respect to the characteristic of interest: effect = p • (l-p) • coefficient, where p is the mean of the dependent variable. For example, the effect of responding to the survey on the probability of being hospitalized in 1989 is .16 (.84) • (-.09) = -.012.

• Denotes that coefficient is significantly different from zero at the .05 level, using a two-tailed test.
• * Denotes that coefficient is significantly different from zero at the .01 level, using a two-tailed test.

characteristics that may influence utilization (age, disability status, welfare status, sex, and race), the differences in predicted utilization between respondents and nonrespondents for nonenrollees decrease in magnitude (only the first row of the table is of interest here). For example, we found a difference in the average number of hospital admissions of 0.08 and 0.07 for 1989 and 1990 in Table C.4; this difference decreases to 0.05 for both years when controlling for age, disability, welfare status, sex, and race in Table C.5. Likewise, we found that the differences of 0.78 and 0.72 for average number of hospital days in Table C.4 dropped by one-quarter, to 0.57 and 0.53, when controlling for measures of demographic characteristics that influence utilization (age, disability, welfare status, sex, and race). In addition, the likelihood of hospitalization is only 1.2 percentage points greater for nonenrollee nonrespondents than the rate for respondents when controlling for the measures of demographic characteristics in Table C.5 using a logistic regression, compared to the difference of 3.5 percentage points found for 1989 in Table C.4. The results for 1990 show a similar pattern.

Although the available measures of demographic characteristics do not account for all of the differences in utilization between nonenrollee respondents and nonrespondents, the data collected in the survey provide much richer and better measures of demographic characteristics. Regressions of utilization on survey control variables explain between seven to eleven percent of the variance in utilization, compared to the one to two percent explained by the regression on only age, disability, welfare status, sex, and race. The types of rich data that the survey collected include measures of activities of daily living, self-reported health status, propensity to seek medical care and an established place for care, the presence of medical conditions such as stroke, cancer, and coronary heart disease, and whether the respondent died within a certain period after being interviewed, all of which have statistically significant effects on utilization. Thus, we expect that the survey control variables would account for all or a large portion of any differences between enrollees and nonenrollees on utilization measures that result from the exclusion of the nonrespondents.

When we perform the same type of regression analysis for enrollees, we find that response to the survey is not a significant factor in hospital utilization. The first row of Table C.6 shows very small coefficient estimates and insignificant t-statistics for coefficients on the survey response indicator variable in the regressions that model the effects of demographic characteristics and response to the survey on the number of hospital admissions and days during 1989 and 1990. In addition, the probabilities of hospitalization for enrollee nonrespondents are found to be only 1.1 percentage pointsgreater than for respondents when we control for demographic characteristics using a logistic regression, compared to the difference of 4.6 percentage points found in Table C.4 for 1989. The respondent-nonrespondent difference for 1990 was not significant in Table C.4 and remained insignificant in the regression.

These results suggest that there is only a small likelihood that nonresponse will create bias in our estimates of HMO impacts, which are based solely on the responding portion of the sample. To further test this inference we use the hospital data for our five HMOs and the HCFA data for the nonenrollees in the market areas where these HMOs were located to estimate HMO impacts on hospital stays, days, and admissions, controlling for demographic characteristics. We estimate each equation twice; once on the full sample of cases and once on only the sample members who responded to our survey.

Examination of the coefficients on the first row of Table C.7 shows that there is very little difference between HMO impacts estimated on respondents only and impacts estimated on the full sample (respondents plus nonrespondents) when controlling for demographic characteristics that are available from HCFA data (age, disability, welfare status, sex, and race). For 1989, the estimated effect of HMOs on both admissions and days is statistically significant for both the full sample and respondents only, and, for both utilization measures, the estimates from the full and respondent-only portion of the sample are very similar in magnitude. For 1990, the estimated effect on neither measure is statistically significant, and again, we obtain results that are comparable in size for the full

TABLE C.6

REGRESSION ESTIMATES OF EFFECTS OF PATIENT CHARACTERISTICS
AND RESPONSE TO SURVEY ON HOSPITAL UTILIZATION
FOR ENROLLEES IN FIVE HMOs

	Number of Hospital Stays		Number of Hospital Days		Whether Hospitalized[a]	
	1989	1990	1989	1990	1989	1990
Variable						
Responded to Survey	-0.06 (-1.43)	0.02 (0.42)	-0.24 (-0.74)	0.03 (0.09)	-0.09 (0.71)	-0.01 (-0.00)
Age 70-74	0.03 (0.81)	-0.01 (-0.16)	0.58 (1.81)	0.03 (0.08)	0.13 (0.94)	-0.03 (-0.22)
Age 75-79	0.04 (0.88)	-0.01 (-0.17)	0.25 (0.69)	-0.09 (-0.26)	0.19 (133)	0.07 (0.54)
Age 80-84	0.07 (1.26)	0.05 (0.97)	0.51 (1.23)	0.28 (0.70)	0.37 • (2.36)	0.40 ** (2.82)
Age 85 or More	0.20 ** (2.87)	0.13 • (2.14)	1.08 * (2.08)	0.89 (1.74)	0.40 * (2.18)	0.70 • * (4.39)
Disabled	-0.25 * (-2.05)	-0.12 (-1.18)	-0.74 (-0.81)	-0.35 (-0.40)	-7.90 (-0.00)	0.11 (0.37)
Originally Disabled	0.30 • * (4.63)	0.19 • * (3.28)	0.97 * (1.96)	1.32 • * (2.76)	0.31 (1.85)	0.27 (1.62)
Welfare Recipient	-0.08 (-0.61)	0.24 * (2.12)	0.82 (0.87)	218 • (2.32)	0.46 (1.48)	-0.21 (-0.54)
Male	0.05 (1.48)	0.10 • * (333)	0.36 (1.42)	0.70 • * (2.91)	0.10 (0.97)	0.15 (1.65)
Nonwhite	-0.04 (-0.65)	0.06 (1.18)	0.07 (0.16)	0.64 (1.64)	-0.02 (-0.14)	-0.17 (-1.07)
Dependent Variable Mean	0.16	0.12	0.95	0.76	0.14	0.16
R^2	0.03	0.03	0.00	0.02		
Sample Size[b]	900	970	900	970	900	970

NOTE: Data on patient characteristics were obtained from the Medicare automated Data Retrieval System (MADRS) files; data on hospital utilization were obtained from the five HMOs supplying reliable data. T-statistics are given in parentheses.

[a] Logit coefficients are not directly interpretable as the effect of the independent variable on the probability of king hospitalized. An estimate of this effect can be obtained by evaluating the derivative of the logit with respect to the characteristic of interest: effect = p • (l-p) • coefficient, where p is the mean of the dependent variable. For example, the effect of responding to the survey on the probability of being hospitahi in 1989 is .14 • (.86) • (-.09) = -0.011.

[b] The total number of enrollee cases for 1989 data drops to 900 because the data for one HMO could not be used for 1989.

• Denotes that coefficient is significantly different from zero at the .05 level, using a two-tailed test.
• * Denotes that coefficient is significantly different from zero at the .01 level, using a two-tailed test.

TABLE C.7

EFFECTS OF PATIENT CHARACTERISTICS AND ENROLLMENT STATUS ON HOSPITAL UTILIZATION FOR ENROLLEES FROM FIVE? HMOs AND CORRESPONDING NONENROLLEES, ESTIMATED ON RESPONDENTS ONLY AND FULL SAMPLE

	Number of Hospital Stays				Number of Hospital Days				Whether Hospitalized'			
	1989		1990		1989		1990		1989		1990	
Variable	Resp.	Full	Resp.	Full	Resp.	Full	Resp.	Full	Resp.	Full	Resp.	Full
Enrolled in HMO	-0.09 • •	-0.10 • •	0.01	-0.01	-0.75 •	-0.74	-0.02	-0.27	-0.15	-0.17 •	0.04	0.02
	(-278)	(-3.42)	(0.35)	(-0.23)	(-239)	(-1.84)	(-0.06)	(-1.00)	(-1.88)	(-252)	(0.47)	(0.32)
Age 70-74	0.07	0.05	0.01	-0.00	0.92 •	0.67	0.07	-0.08	0.20	0.14	0.11	0.07
	(1.80)	(1.33)	(0.18)	(-0.05)	(2.34)	(1.28)	(0.21)	(-0.24)	(1.92)	(1.92)	(1.11)	(0.78)
Age 75-79	0.09	0.11 •	0.03	0.04	1.01 •	0.90	-0.21	-0.03	0.14	0.23 •	0.19	0.17
	(1.94)	(2.52)	(0.64)	(0.83)	(2.17)	(1.53)	(-0.49)	(-0.07)	(1.11)	(2.21)	(1.67)	(1.72)
Age 8084	0.13 •	0.16 ••	0.18 • *	0.18 • *	0.98	0.92	1.07 •	1.12 •	0.40 • *	0.45 ••	0.50 • *	0.49 • *
	(239)	(3.38)	(3.15)	(3.59)	(1.87)	(1.39)	(2.25)	(2.49)	(3.21)	(4.29)	(4.25)	(4.92)
Age 85 or More	0.20 ••	0.24 ••	0.18 • *	0.15 •	1.35 •	1.89 •	1.34 •	0.96	0.52 • *	0.54 • *	0.62 • *	0.56 • *
	4 ' 0)	(4.43)	(2.61)	(2.45)	(2.16)	(2.45)	(2.35)	(1.82)	(3.77)	(4.63)	(4.69)	(4.97)
Disabled	-0.09	-0.23 • *	-0.05	-0.09	-0.56	0.49	-0.62	-1.07	-0.27	-0.48	-0.10	-0.11
	(-0.92)	(-2.80)	(-0.51)	(-1.02)	(-0.59)	(0.42)	(-0.72)	(-1.34)	(-1.11)	(-227)	(-0.44)	(-0.58)
Originally Disabled	0.17 •	0.22 • *	0.06	0.04	0.72	0.77	0.83	0.64	0.20	0.24	0.24	0.13
	(2.48)	(3.75)	(0.78)	(0.57)	(1.08)	(0.91)	(1.38)	(1.12)	(1.31)	(1.87)	(1.62)	(1.00)
Welfare Recipient	0.06	0.11 •	-0.01	0.12 •	0.82	253 • *	-0.25	1.16 •	0.22	0.22 •	-0.12	0.22
	(0.84)	(2.03)	(-0.14)	(1.98)	(1.23)	(3.35)	(-0.41)	(2.22)	(1.49)	(1.98)	(-0.70)	(1.93)
Male	0.03	0.02	0.06	0.08 • '	0.29	0.41	0.32	0.49	0.05	0.06	0.04	0.56
	(1.08)	(0.84)	(1.85)	(2.67)	(0.92)	(1.02)	(1.14)	(1.80)	(0.69)	(0.95)	(0.57)	(0.86)
Nonwhite	-0.03	-0.02	0.01	-0.01	0.34	0.06	0.59	0.31	0.07	0.07	-0.14	-0.17
	(-0.61)	(-0.53)	(0.10)	(-0.23)	(0.63)	(0.09)	(1.22)	(0.70)	(0.50)	(0.63)	(-1.00)	(-1.50)
Mean of Dependent Variable	0.21	0.22	0.22	0.23	1.54	1.76	1.46	1.67	0.15	0.16	0.15	0.16
R^2	0.02	0.03	0.01	0.01	0.01	0.01	0.00	0.01				
Sample Size	1,400	1,784	1,517	1,925	1,400	1,784	1,517	1,925	1,400	1,784	1,517	1,925

TABLE C7 (continued)

NOTE: Data were obtained from five HMOs for enrollees and from the 1989 and 1990 Medicate Automated Data Retrieval System (MADRS) files for nonenrollees who were matched to enrollees by site. T-statistics are given in parentheses below the coefficients

[a] Logit coefficients are not directly interpretable as the effect of the independent variable on the probability of being hospitalized. An estimate of this effect can be obtained by evaluating the derivative of the logit with respect to the characteristic of interest: effect = p • (l-p) • coefficient, where p is the mean of the dependent variable. For example, the effect of enrollment status on the probability of being hospitalized in 1989 estimated on only the respondents is .15 • (.85) • (-.15) = -0.019. Likewise, the effect of enrollment status on the probability of beiig hospitalized in 1989 for the full sample is .16 • (.84) • (-.17) ≈ 4.023. Thus, the difference in the two estimates of the HMO impact on the probability of being hospitalized in 1989 is only .004, less than half of one percentage point.

• Denotes that coefficient is significantly different from zero at the .05 level, using a two-tailed test.
• * Denotes that coefficient is significantly different from zero at the .01 level, using a two-tailed test.

and respondent-only samples (although the two estimates for impacts on hospital stay for 1990 are opposite in sign, both are very small). In addition, the logistic regression coefficients obtained on enrollment status in the models of the probability of having a hospital stay are very similar for the respondent sample and the full sample for both the 1989 and 1990 measures. Again the two 1989 estimates are both statistically significant[3] and very similar in size; the 1990 estimates are very small, statistically insignificant, and similar to each other. Converting the logit estimates to estimated impacts on the probability of having a hospital stay shows that the estimated effects for the full and respondent-only samples differ by only .004 for 1989 and by .002 for 1990. The similarity in magnitudes of coefficient estimates and the similarity in statistical significance between the estimates based on respondents and those based on the full sample confirm our expectations about the lack of bias due to nonresponse. Furthermore, when the rich data on beneficiary characteristics from the survey are used to further control for differences between enrollees and nonenrollees, even small difference-s between the two groups that are due to nonresponse are likely to disappear.

The estimated effect in Table C.7 suggesting that HMOs reduced hospital admissions (by about 50 percent) is contrary to the results obtained in our analysis presented in the text. This difference is due to the much weaker set of control variables available for analysis on the results presented in this appendix. Including measures of health status and functional status, on which enrollees differ substantially, eliminates the differences between enrollees and nonenrollees in the number of admissions and probability of admission. The estimated effect on hospital days remains statistically significant in our results in the text because HMOs reduce the average length of stay. The fact that some of the estimates presented here are different from those in the text is irrelevant--the key to the results here is that limiting the sample to only the respondents does not change the estimates of HMO impacts on utilization is any meaningful way.

[3]The estimates for the respondents-only portion of the sample for 1989 is statistically significant at only the .10 level, not the .05 level (or equivalently, at the 5 percent level using a one-tailed test). However, the estimates are very similar in magnitude.

C. DISCUSSION

Although the conditions necessary to create bias due to nonresponse are present in our data set, we find convincing evidence that no such bias occurred. The nonresponse rate is greater for nonenrollees, and nonrespondents do appear to be heavier users of health care services on average than respondents, especially among the nonenrollees. However, the response rate difference is small, and only half of the difference is likely to be related to factors that influence utilization. Furthermore, the differences between respondents and nonrespondents appear to be explainable in part by their observable characteristics, even when only crude data on such characteristics are used. The survey provides a rich array of variables to control for differences between enrollees and nonenrollees, whether due to inherent differences or to differences in response. The estimated effects of HMOs on hospital use, based on the full sample, are very similar in size and statistical significance to estimates based on survey respondents only.

We also believe that analyses of HMO effects on other variables obtained from our survey (such as satisfaction with care) will not be biased by nonresponse. While we cannot be as certain of this, given that most of our analysis of nonresponse is based on utilization measures obtained from records, we believe that the same arguments made above apply to other measures as well. If there are no unobserved variables that affect both the probability of nonresponse and service utilization, it is relatively unlikely that there are unobservable factors affecting both nonresponse and satisfaction or other outcome measures.

APPENDIX D

VARIANCE OF THE ESTIMATED EFFECT OF THE RISK PROGRAM ON COSTS TO HCFA

APPENDIX D

VARIANCE OF THE ESTIMATED EFFECT OF THE RISK PROGRAM ON COSTS TO HCFA

To obtain somc feel for the precision of the estimated effect of the risk program on costs to HCFA we have also estimated the standard error of the estimated impact. The estimated effect of the risk program on the cost to HCFA is

(A.1) $\textit{Effect} = .95\ \hat{Y}_A - \hat{Y}_F\ ,\ \textit{where}$

(A.2) $\hat{Y}_A = \bar{X}_A^{E\prime}\hat{a},$

and

(A.3) $\hat{Y}_F = \bar{X}_A^{E\prime}\hat{b}_A + \bar{X}_O^{E\prime}\hat{b}_O = \bar{Z}^{E\prime}\hat{b}\ .$

These predicted values were obtained from the following regressions estimated on nonenrollees:

(A.4) $Y_A = X_A a + u$

(A.5) $Y_F = X_A b_A + X_O b_O + \mathbf{e} = Zb + \mathbf{e}\ .$

In equations A2 and A.3, $\hat{a}$ and $\hat{b}$ are vectors of least squares regression estimates of parameters a and b in the estimated AAPCC payment equation (k4) and fee-for-service cost equation (AS); $\bar{X}_A^E$ is a Kx1 vector of mean values for enrollees for the variables included in the AAPCC risk adjustor (age, sex, reason for entitlement, welfare, nursing home residence and site); and $\bar{X}_O^E$ is an Lx1 vector of enrollee mean values for the other survey variables that are not in the AAPCC but are expected to affect cost (e.g., health status indicators, income, attitudes toward health care). Matrices X_A and

X_o in equations A.4 and A.5 contain the data on these characteristics for the nonenrollees, and u and e are random disturbance terms with mean zero,. The variance of the expression in A.1 is:

$$\text{(A.6)} \qquad V(\text{effect}) = .95^2 V(\hat{Y}_A) + V(\hat{Y}_F) - 2(.95) \text{ cov } (\hat{Y}_A, \hat{Y}_F)$$

There are two components of the variance in A.6, one component due to the variance in the estimates of the $\hat{a}$ and $\hat{b}$, and one component due to the fact that the sample means $\bar{X}_A^E$ and $\bar{X}_O^E$ for enrollees are estimates of the population means for enrollees in the risk program'. Using the "delta" method we can show that the overall variance in A.6 is equal to the sum of (1) the variance due to the imprecision in the coefficients, and (2) the variance due to the imprecision in the sample means. These terms are derived separately below.

1. Variance Due to Imprecision in Coefficients

The variance due to the coefficient estimates is comprised of the following terms (where σ^2 is the variance of e in equation AS):

$$\text{(A.7)} \qquad \begin{aligned} V(\hat{Y}_A) &= V(\bar{X}_A^{E'}\hat{a}) \\ &= \bar{X}_A^{E'} V(\hat{a}) \bar{X}_A^E \\ &= \sigma^2 \bar{X}_A^{E'} (X_A' X_A)^{-1} \bar{X}_A^E \end{aligned}$$

$$\text{(A.8)} \qquad \begin{aligned} V(\hat{Y}_F) &= V(\bar{Z}^{E'}\hat{b}) \\ &= \bar{Z}^{E'} V(\hat{b}) \bar{Z}^E \\ &= \sigma^2 \bar{Z}^{E'} (Z'Z)^{-1} \bar{Z}^E \end{aligned}$$

$$\text{(A.9)} \qquad \text{cov}(\hat{Y}_A, \hat{Y}_F) = \bar{X}_A^{E'} \text{cov}(\hat{a}, \hat{b}) \bar{Z}^E$$

'The variances of $\hat{Y}_A$ and $\hat{Y}_F$ and the covariance each include an additional term for the "forecast error," but these terms cancel out and therefore need not be considered here.

where

(A.10)
$$\begin{aligned}\mathrm{cov}(\hat{a}, \hat{b}) &= E[(\hat{a} - E(\hat{a}))\,(\hat{b} - E(\hat{b}))'] \\ &= E[(X_A'X_A)^{-1}\, X_A'e\, e'Z(Z'Z)^{-1}] \\ &= \sigma^2(X_A'X_A)^{-1}\, X_A'Z(Z'Z)^{-1} \\ &= \sigma^2(X_A'X_A)^{-1}\ (I\ \ 0)\end{aligned}$$

where I is a KxK identity matrix and O is a KxL matrix of zeros.

The results for $var(\hat{a})$ and $cov(\hat{a},\hat{b})$ require further explanation. The term $\hat{a}$ - E($\hat{a}$) is obtained as follows:

$$\begin{aligned}\hat{a} &= (X_A'X_A)^{-1}\ X_A'y \\ &= (X_A'X_A)^{-1}\ X_A^1\,(X_Ab_A + X_ob_o + e) \qquad \text{(from equation AS)} \\ &= b_A + P_{Ao}b_o + (X_A'X_A)^{-1}\, X_A'e\ ,\end{aligned}$$

where P_{AO} is the matrix of auxiliary regression coefficients from regressing the X_O variables on X_A, and the "true" equation for y is the cost equation (AS). Then $\hat{a}$ - E($\hat{a}$) = $(X_A'X_A)^{-1}\, X_A'e$, since X_A is assumed to be independent of e and E(e) = 0. This expression is inserted into line 2 of A.10 and also yields the result used in A.7 that $var(\hat{a}) = \sigma^2\,(X'X)^{-1}$. The last line of equation A.10 is derived from the fact that $Z = (X_A\, X_O)$ and from the property of linear algebra requiring that multiplying the first n rows of a t x t matrix D by the inverse of D (D^{-1}) yields an n x t matrix comprised of an n x n identity matrix and an adjacent n x (t-n) matrix of zeros $(I\ 0)$.

Substituting A.10 into A.9 yields

(A.11)
$$\begin{aligned}cov(\hat{Y}_A,\hat{Y}_F) &= \bar{X}_A^{E'}\ cov(\hat{a},\hat{b})\bar{Z}^E \\ &= \sigma^2\bar{X}_A^{E'}\ (X_A'X_A)^{-1}\ \bar{X}_A^E\end{aligned}$$

If we then substitute A.7, A.8, and A.11 into **A.6 we find:**

$$var(effect) = (.95^2)\,\sigma^2\,\bar{X}_A^{E'}\,(X_A'X_A)^{-1}\,\bar{X}_A^E + \sigma^2\bar{Z}^{E'}(Z'Z)^{-1}\,\bar{Z}^E - 2(.95)\sigma^2\,\bar{X}_A^{E'}(X_A'X_A)\,\bar{X}_A^E$$

(A.12) $$= \sigma^2[\bar{Z}^{E'}(Z'Z)^{-1}\,\bar{Z}^E - .9975\,\bar{X}_A^{E'}(X_A'X_A)^{-1}\,\bar{X}_A^E]$$

This result is quite surprising at first glance because (ignoring the .95 factor) it implies that the variance of the difference between $\hat{Y}_A$ and $\hat{Y}_F$ is essentially equal to the difference in their variances. However, we know that the variance of the difference of two random variables is the sum of the variances of the two variables, minus twice the covariance.

The reason for this somewhat surprising result is the fact that the covariance between the two equations is exactly equal to the variance of the AAPCC equation, which occurs because the disturbance terms u and e both have the same variance. The variances are the same because the AAPCC equation (A.4) is simply a misspecified version of the true cost equation A.5 (since only a few of the variables that affect costs are available to be used in the AAPCC). Thus, subtracting twice the covariance of $\hat{Y}_A$ and $\hat{Y}_F$ is equivalent to subtracting twice the variance of $\hat{Y}_A$.

This result is confirmed by Hausman's (1978) paper on specification tests, which shows that under certain conditions the variance of the difference between coefficient vectors for two alternative specifications of a model is equal to the difference in their variances. The conditions required are that:

(1) The first estimator is consistent (unbiased) ***and*** efficient if the model specification from which it was derived is the true specification, but this estimator is inconsistent if the alternative model specification is the correct one, and

(2) The second estimator is consistent regardless of which model specification is the correct one, but if the first specification is correct, then the second estimator is less efficient.

These are precisely the conditions for our estimators. If the "true" model of Medicare costs were our AAPCC payment equation, the coefficient estimates $\hat{a}$ would be consistent and more efficient that the $\hat{b}$'s, because fewer coefficients are being estimated. However, if the true model is our cost equation model, then the coefficients $\hat{a}$ are biased and inconsistent. The coefficient $\hat{b}$, on the other hand, is a consistent estimator of b regardless of which model is correct, since adding unnecessary variables to a regression model does not introduce bias, but does increase the variance of estimates. While we are not comparing the coefficients themselves, we are comparing the variances of a linear combination of the $\hat{a}$'s to a linear combination of the $\hat{b}$'s (that is, the two predicted values), so the same principle applies. Hence, the variance of the difference between the two linear combinations is equal to the variance of the linear combination of the always-consistent estimator 8 minus the variance of the linear combination of the more efficient but possibly biased estimator $\hat{a}$. [Note, however, that the variance for $\hat{Y}_A$ depends on σ^2, the variance of the disturbance term from the FFS cost equation (A.5), because that is our best estimate of the "true" variance of the dependent variable.]

2. **Component of Variance Due to Variance of Sample Means**

In addition to the variance due to uncertainty in our estimates of the parameters a and b there is also variance due to the use of sample means rather than population means for enrollees. That is, our sample of enrollees may have mean values for enrollee characteristics that differ from the population means for enrollees. The means are unbiased, because the sample has been selected at random from the population of enrollees, but the use of sample means introduces additional uncertainty into our estimate. There is no covariance between the coefficient estimates and the sample means because the coefficients are estimated on the nonenrollee sample while the means are estimated on the enrollees.

The variance associated with the sample means can be expressed most easily by first collecting terms. Thus:

$$
\begin{aligned}
\textbf{effect} &= .95\bar{X}_A^{E\prime}\hat{a} - (\bar{X}_A^{E\prime}\hat{b}_A + \bar{X}_O^{E\prime}\hat{b}_O) \\
&= \bar{X}_A^{E\prime}(.95\hat{a} - \hat{b}_A) - \bar{X}_O^{E\prime}\hat{b}_O, \\
\text{(A.13)} \qquad &= \bar{Z}^{E}\hat{c}, \\
\text{where } \hat{c} &= \begin{pmatrix} .95\hat{a} - \hat{b}_A \\ -\hat{b}_O \end{pmatrix}
\end{aligned}
$$

Then the component of the variance due to use of sample means is:

(A.14) $var = \hat{c}' V(\bar{Z}^E)\hat{c},$

where $V(\bar{Z}^E)$ is the variance-covariance matrix of the sample means for characteristics Z for enrollees.

Thus, the total variance of the estimated effect is the sum of A.12 and A.14:

(A.15) $var(overall) = \sigma^2\left[\bar{Z}^{E\prime}(Z'Z)^{-1}\,\bar{Z}^E - .9975\bar{X}_A^{E\prime}(X_A'X_A)^{-1}\,\bar{X}_A^E\right] + \hat{c}'V(\bar{Z}^E)\hat{c}$

3. **Estimates of the Variance of the Program Effect on Costs to HCFA**

Estimating the components of the variance from our sample yields the following results:

$$
\begin{aligned}
\sigma^2 &= 68{,}565{,}458 \\
\bar{Z}^{E\prime}(Z'Z)^{-1}\,\bar{Z}^E &= .00019866 \\
\bar{X}_A^{E\prime}(X_A'X_A)^{-1}\,\bar{X}_A^E &= .00018218 \\
\hat{c}'V(\bar{Z})\hat{c} &= 483.89
\end{aligned}
$$

Inserting these estimates into equation A.15 yields an estiniated variance of 1,614 and standard error of 40.18. Thus, our estimated impact on cost of $134 per enrollee is significantly different from zero statistically at the .01 level (t = 134/40.18 = 3.33). The 95 percent confidence interval for our estimated effect on cost is $56 to $212, or 2.4 to 9.1 percent of what costs would have been. That is, we are 95 percent certain that this interval contains the true effect of the risk program on costs to HCFA (assuming that the AAPCC provides an accurate estimate of average costs to HCFA per beneficiary in the FFS sector). It is unlikely therefore that the true program effect on cost is less than 2 percent or more than 9 percent.