COPAYMENTS AND DEMAND FOR MEDICAL CARE: THE CALIFORNIA MEDICAID EXPERIENCE

PREPARED UNDER A GRANT FROM THE DEPARTMENT
OF HEALTH, EDUCATION, AND WELFARE

L. JAY HELMS, JOSEPH P. NEWHOUSE, CHARLES E. PHELPS

R-2167-HEW
FEBRUARY 1978

Rand
SANTA MONICA, CA. 90406

The research reported herein was performed pursuant to Grant No. 016B-7601-P2021 from the U.S. Department of Health, Education, and Welfare, Washington, D.C. The opinions and conclusions expressed herein are solely those of the authors, and should not be construed as representing the opinions or policy of any agency of the United States Government.

Library of Congress Cataloging in Publication Data

Helms, L. Jay, 19[illegible]2-
Copayments and demand for medical care.

[Report] - Rand Corporation ; R-2167-HEW
1. Medical care, Cost of--California. 2. Medical care--California--Utilization. 3. Medicaid--California. I. Newhouse, Joseph P., joint author. II. Phelps, Charles E., joint author. III. Title. IV. Series: Rand Corporation. Rand report ; R-2167-HEW. [DNLM: 1. Fees, Medical--California. 2. Hospitalization--California. 3. Medical assistance, Title 19--Economics--California. W275 AC2 H4[illegible]]
AS36.R3 R-2167 [RA410.54.C2] 081s [338.4'3]
ISBN 0-8330-0005-[illegible]

Published by The Rand Corporation

COPAYMENTS AND DEMAND FOR MEDICAL CARE: THE CALIFORNIA MEDICAID EXPERIENCE

PREPARED UNDER A GRANT FROM THE DEPARTMENT
OF HEALTH, EDUCATION, AND WELFARE

L. JAY HELMS, JOSEPH P. NEWHOUSE, CHARLES E. PHELPS

R-2167-HEW
FEBRUARY 1978

Rand
SANTA MONICA, CA. 90406

PREFACE

For many years the role played by price in determining demand in the health care sector has been a controversial one. Of particular interest is the effect of an increase in out-of-pocket payments for ambulatory medical services on the demand for hospital care.

The incorporation, in 1972, of an experimental copayment provision into the California Medicaid program provided an opportunity to explore these issues. A 1975 paper by Milton Roemer, Carl Hopkins, and others presented graphical displays of the aggregated quarterly data from this experiment. The paper came to the widely disputed conclusion that higher prices for ambulatory services might so significantly increase hospitalization that the total cost of medical care would rise.

In view of the importance of these questions, the authors of the present report requested and were given the original data by Professors Roemer and Hopkins. The report--prepared under a grant from the U.S. Department of Health, Education, and Welfare--contains the results of an econometric analysis of these data.

The text of this report was published in the Spring 1978 issue of the *Bell Journal of Economics*.

SUMMARY

In 1972, the State of California required selected Medicaid beneficiaries to make a small copayment for (previously free) out-of-hospital services. Inpatient care continued to be provided to all beneficiaries without charge. Data are available on the utilization of hospital and ambulatory care services by approximately 40,000 persons in the Aid to Families with Dependent Children program in three counties for 6 months before and 12 months after the imposition of the copayment requirement. Some of these persons paid the copayment, others were exempt from it.

We have used an efficient, indirect, multiple regression technique to analyze the disaggregated time-series (six quarters) of cross-sections, controlling additively for between-group differences, seasonal variation, and such data on personal characteristics as were available, as well as for a major structural change during the sample period. In addition, we have introduced a previously unavailable measure of utilization--hospital inpatient days--to estimate substitution effects. We have also used data on amounts actually paid by Medi-Cal for hospital services, ambulatory care, and prescription drugs to yield estimates of the net cost of the copayment program.

Of necessity we assume that the two groups are equally affected by omitted variables, and that the differences between the control and experimental groups can be fully accounted for by the included variables and an additive shift term. Unfortunately, these groups exhibited substantially different propensities to utilize medical services in the period when neither group was required to make copayments, and therefore the assumption that an additive shift term captures the differences between the groups could be questioned.

Conditional upon the validity of our assumptions, our results indicate that strong price effects may be at work in a welfare population. Requiring a $1 copayment for physician visits decreased the demand for physician services by 8 percent, but increased the demand for hospital inpatient services by 17 percent. And while the confidence

intervals are large, including negative as well as positive values, point estimates indicate that there was a 3 to 8 percent *increase* in overall program cost. Thus, copayments for ambulatory services in a welfare population as a method of controlling costs may well be ineffectual or even self-defeating.

ACKNOWLEDGMENTS

The authors wish to thank Paul Joskow and Stephen Carroll for comments on an earlier draft, and Bryant Mori for highly valued computational assistance.

CONTENTS

I. INTRODUCTION

The growth of the Medicaid program and the rapidly increasing cost of hospitalization have heightened interest in various methods of controlling the demand for medical care. Considerable controversy exists about the role played by "price"--i.e., out-of-pocket payments by patients--in determining demand. But, especially with regard to the poor, there is little evidence on which to make a judgment.

In this report we attempt to answer three questions regarding health care services for the poor: (1) How strong are the inhibiting effects of an increase in the out-of-pocket cost of physician office visits on the demand for ambulatory care? (2) Does increasing the relative price of ambulatory care increase or decrease the demand for hospitalization? (3) How does increasing the price of office visits affect the total resource cost of the health care services provided both in and out of hospitals?

Although the existing findings on these questions--particularly the sign of the cross-price elasticity of demand for hospital services--are inconclusive, the literature favors the conclusion that hospital and ambulatory care services are not substitutes for one another, and may well be complements in the population at large.[1] However, our focus is on the welfare population. This group is of special interest both because it is a current target of national health insurance programs and because evidence suggests that its response patterns may differ from those of the general population.[2]

Our data are from the 1972 California Copayment Experiment in which a group of Medi-Cal[3] beneficiaries was required to make small payments for visits to a doctor and for prescription drugs--services that had previously been rendered free of charge. A control group was exempted from the charge. Roemer et al. (1975) used aggregated quarterly data from this experiment to construct graphical displays of index figures for hospital and ambulatory care provided to the experimental and control groups. Visual inspection of the data led Roemer and his coauthors to conclude that although the copayment scheme did

indeed lower demand for ambulatory care, there was an increase in the demand for hospital services and an overall *increase* in the program's cost. Such methods are, of course, unreliable for testing hypotheses,[4] especially when there is relatively little power in the data.[5] Moreover, the methods do not provide quantitative estimates of the effects of the program. Because these questions are of considerable policy significance, we requested and were given the original data used by Roemer and his colleagues.

We have used an efficient, indirect, multiple regression technique to analyze the disaggregated time-series of cross-sections, controlling additively for between-group differences, seasonal variation, and personal characteristics as data were available, as well as for a major structural change during the sample period. In addition, we have defined and used a previously unavailable dependent variable--hospital-days--to estimate substitution effects between inpatient and outpatient care. We have also used amounts actually paid by Medi-Cal for hospital services, ambulatory care, and prescription drugs to estimate the cost of the copayment program. Thus, our methods allow formal testing of hypotheses about the implications of copayment.

II. THE EXPERIMENTAL DESIGN AND THE DATA

Data are available for six quarters, from July 1971 through December 1972. The sample includes 40,662 individuals from San Francisco, Tulare, and Ventura counties who were Medi-Cal beneficiaries in the Aid to Families with Dependent Children program for all six quarters. The characteristics of the sample differ markedly from those of the general population. In addition to being entirely a welfare population, 70 percent of the individuals in the sample were under 18 years of age (compared with 36 percent in the general population in 1970) and 60 percent of the sample were female.

Starting in October 1971 (the beginning of the second quarter of the sample period), and for the remainder of the sample period, the state required a state-employed Medi-Cal consultant to "prior authorize" (a) a third or subsequent visit to a doctor's office or prescription in any month, (b) many outpatient services other than simple office visits, and (c) all nonemergency hospital care. The imposition of the prior authorization requirement caused a large and immediate decline in the number of health services provided.

From January 1, 1972 (the beginning of the third quarter) to December 31, 1972, the state imposed copayment charges on 26 percent of the individuals in the sample; the remainder of the sample continued to receive free medical care services. The copayment was $1 for each of the first two office visits in a month, and $.50 for each of the first two prescriptions in a month. Additional services, if authorized, were free to the patient. The 26 percent who were required to copay did *not* constitute a random sample of the Medi-Cal population; rather, beneficiaries were selected to make copayments on the basis of their incomes and holdings of real and personal property (the poorer beneficiaries were not required to copay). There was a much smaller proportion of young children in the copayment group than in the noncopayment group. And, as can be seen in Table 1, the two groups differed markedly in their tendencies to use hospital care.[6]

Data on certain personal characteristics were available for each Medi-Cal beneficiary who was continuously eligible for the six quarters.

Table 1

DESCRIPTIVE STATISTICS ON SAMPLE POPULATION

Statistic	Copayment Group		Noncopayment Group	
	Before Copayment Required	After Copayment Required	Before Copayment Required	After Copayment Required
Average number of physician visits per person per quarter	.6772	.6494	.7316	.7274
Average number of hospital inpatient-days per person per quarter	.0915	.0921	.1585	.1309
Average expenditure ($) on prescription drugs reimbursed by Medi-Cal per person per quarter	3.72	3.16	4.03	4.16
Number of individuals in each group, by county:				
San Francisco	5,592		23,569	
Tulare	3,985		4,689	
Ventura	1,110		1,717	
Total number of individuals	10,687		29,975	
Average age in years	18.2		15.8	
Percent females	59.7		59.8	

In addition, some 800,000 records of paid claims for ambulatory and hospital care for these individuals were aggregated to the individual level and merged with eligibility lists (to avoid truncation bias). The resulting data base consisted of six quarters of observations on the 40,662 individuals. Every effort was made to exclude erroneous claims; in particular, a large number of redundant claims for hospital stays were detected in and eliminated from the records of the fifth and sixth quarters.[7] Although the possibility of error in the claims file remains, the modified records are as accurate as we can make them.

III. MODELS AND METHODOLOGY

Because the standard approach to estimating the effects of the copayment is an Analysis of Covariance (ANOCOVA) technique, we begin with such a model. Unfortunately, the ANOCOVA model is a misspecification and leads to inconsistent estimates of the parameters. Even with proper specification of the explanatory variables, a more general stochastic specification is required to obtain efficient estimates of the parameters and consistent estimates of their standard errors. To achieve the required generality, we estimate three successively more general models with appropriate stochastic specifications.

Of our sample, $n_C = 10{,}687$ individuals were in the copayment group and $n_N = 29{,}975$ were not. We therefore "stack" the observations into six blocks of $n = n_C + n_N$ observations, one block per quarter, with those in the copayment group positioned first within each block. We define the following variables:

Variable	Definition
$\text{PHYSICIAN-VISITS}_t$	Visits to physicians in quarter t ($t = 1,\ldots,6$)
HOSPITAL-DAYS_t	Days spent in the hospital (as an inpatient) in quarter t
SEX	1 for males
AGE	Natural logarithm of age
TULARE	1 for residents of Tulare County
VENTURA	1 for residents of Ventura County
PRIOR-AUTHORIZATION	Prior authorization dummy: 1 for all individuals in quarters 2 through 6
COPAY-GROUP	1 for those in the copayment group (invariant over time)
COPAY-REQUIRED	Copayment required dummy: during quarters 3 through 6 only, 1 for those in the copayment group
SUMMER, AUTUMN, WINTER	Seasonal dummies

The coefficient of COPAY-GROUP measures systematic differences between the two groups, whereas the coefficient of COPAY-REQUIRED measures the effect of the copayment program.

Let the dependent variable under consideration (either PHYSICIAN-VISITS or HOSPITAL-DAYS) be y. Then we can write the ANOCOVA model (which we shall refer to as Model I) as

$$\begin{aligned} y = {} & \alpha_1 + \alpha_2 \text{ PRIOR-AUTHORIZATION} + \alpha_3 \text{ SUMMER} + \alpha_4 \text{ AUTUMN} \\ & + \alpha_5 \text{ WINTER} + \alpha_6 \text{ COPAY-GROUP} + \alpha_7 \text{ COPAY-REQUIRED} \qquad (1) \\ & + \alpha_8 \text{ SEX} + \alpha_9 \text{ AGE} + \alpha_{10} \text{ TULARE} + \alpha_{11} \text{ VENTURA} + \varepsilon^{I} . \end{aligned}$$

This specification reflects three implicit constraints: (1) Seasonal effects are constrained to be the same in both years. (2) The coefficients of SEX, AGE, VENTURA, and TULARE are invariant over time. (3) COPAY-GROUP and COPAY-REQUIRED have constant coefficients over time. In generalizing Model I, we relax these three sets of constraints successively.

In Model II, we eliminate the seasonality constraint. The constant and three seasonal dummies in Eq. (1) must then be replaced by six time dummies (one for each quarter). Note that when we drop the seasonality constraint, the effect of prior authorization is no longer identified. (The PRIOR-AUTHORIZATION variable is the sum of the time dummies for quarters two through six; thus, including it in the revised model would result in perfect multicollinearity.)

In Model III we drop the second as well as the first set of restrictions. Thus, separate coefficients of the sex, age, and county dummies are estimated for each quarter of the sample period.

Finally, we introduce a fully unconstrained model (Model IV) in which the first and second sets of restrictions are relaxed and in which, additionally, the coefficients of COPAY-GROUP and of COPAY-REQUIRED may vary from quarter to quarter. If both the between-group difference and the experimental effect vary across time, the former cannot be controlled for in the estimation of the latter, and hence

neither is identified. Thus, in the fully general context of Model IV, we cannot estimate the coefficient of the crucial variable--COPAY-REQUIRED.

Note that these four models, being successively more general, allow for the empirical determination of the most highly constrained model within which the implicit restrictions are not rejected by the data. This determination is important because the less-constrained models are less likely to lead to specification bias, but the more highly restricted models allow us to identify more of the coefficients of the ANOCOVA model.

Setting up the models in this nested fashion also permits us to estimate all four models with a very general stochastic specification. Note that the assumption that the error terms in Model I or any of its generalizations are independently and identically distributed is not likely to be satisfied. In particular, we would expect the error terms for a given individual to be correlated, especially between adjacent quarters. For example, an individual who was undergoing treatment in quarter three could be expected to have a higher-than-average probability of also undergoing treatment for the same or a related condition in quarter four. Estimation of the person-specific, residual, variance-covariance matrix reveals that this is indeed the case. Accordingly, we develop a method of estimating the four models with a stochastic specification that allows for an unconstrained matrix of person-specific covariances. The series of nested models, this general covariance structure, and the computational technique that links the two are explained in detail in the appendix.

IV. ESTIMATION AND DISCUSSION

Estimated coefficients for Model I, with PHYSICIAN-VISITS and HOSPITAL-DAYS as dependent variables, are shown in Table 2. We construct a test of the null hypothesis that Model I is valid given that Model IV is valid. Recalling that Model IV is a strict generalization

Table 2

MODEL I: ESTIMATED EQUATIONS FOR PHYSICIAN-VISITS AND HOSPITAL-DAYS

(Standard errors are shown in parentheses)

Explanatory Variables	Dependent Variable: PHYSICIAN-VISITS	Dependent Variable: HOSPITAL-DAYS
Constant	.8169 (.0170)	.0301 (.0110)
PRIOR-AUTHORIZATION	-.1496 (.0078)	-.0237 (.0073)
SUMMER	-.0478 (.0062)	.0096 (.0063)
AUTUMN	-.0111 (.0057)	.0076 (.0059)
WINTER	.0747 (.0063)	.0072 (.0063)
COPAY-GROUP	-.0786 (.0124)	-.0626 (.0090)
COPAY-REQUIRED	.0156 (.0096)	.0124 (.0096)
SEX	-.2076 (.0094)	-.0375 (.0047)
AGE	.0637 (.0050)	.0557 (.0025)
TULARE	-.0618 (.0113)	-.0321 (.0057)
VENTURA	-.1399 (.0177)	-.0065 (.0090)
F-statistic to test restrictions of Model I (d.f. = $(25,\infty)$)	26.22 $p < .00001$	1.58 $p = .033$

of Model I, and that our estimation procedure for Model I was to estimate Model IV subject to certain constraints, the desired test is an F-test of those constraints. For the PHYSICIAN-VISITS equation, the test statistic is $F(25,\infty) = 26.22$ ($p < .00001$), which clearly warrants the rejection of the validity of Model I.[8] The statistic for the HOSPITAL-DAYS equation is $F(25,\infty) = 1.58$ ($p = .033$). Thus, the ANOCOVA model is almost certainly not valid either for physician visits or for hospital-days.

Table 3 gives the estimated coefficients for Model II. These coefficients suggest that significant bias is present in the Model I estimates. In particular, the coefficient of COPAY-REQUIRED is positively (negatively) biased in the PHYSICIAN-VISITS (HOSPITAL-DAYS) equation, because of the implicit omission of a variable that is positively correlated with COPAY-REQUIRED (see the discussion of α_{NULL} in the appendix). Turning to similar tests of the validity of Model II, we see from Table 3 that the appropriate F-statistics are $F(24,\infty) = 23.56$ ($p < .00001$) for the PHYSICIAN-VISITS equation, and $F(24,\infty) = 1.36$ ($p = .109$) for the HOSPITAL-DAYS equation. Although we may confidently reject Model II for the PHYSICIAN-VISITS equation, rejection of Model II for the HOSPITAL-DAYS equation is not decisively called for. At least for the ambulatory care sector, we must move to Model III in our search for a valid model. Fortunately, the F-statistics for the test of the null hypothesis of the validity of Model III (the most general model in which we can estimate a time-invariant coefficient of COPAY-REQUIRED) are well below the $F_{.05}(4,\infty) = 2.37$ critical value, as can be seen in Table 4.

The rejection of Models I and II and the failure to reject Model III imply that, particularly for physician office visits, there were significant local patterns in medical care usage over time. For example, although San Francisco usually has higher utilization rates than the other counties, these rates were even higher in the winter of 1971. These differences may arise from changes in the supply of medical care at the different sites (Holahan, 1976). By contrast, the effects of SEX and AGE were nearly constant over time. Because differences among the three counties are not our primary concern, we do not pursue these results.

Table 3

MODEL II: ESTIMATED EQUATIONS FOR PHYSICIAN-VISITS AND HOSPITAL-DAYS

(Standard errors are shown in parentheses)

Explanatory Variables	Dependent Variable: PHYSICIAN-VISITS	Dependent Variable: HOSPITAL-DAYS
$QUARTER_1$	.7561 (.0158)	.0426 (.0091)
$QUARTER_2$	.6224 (.0151)	.0262 (.0092)
$QUARTER_3$	.7422 (.0153)	.0136 (.0084)
$QUARTER_4$	.6715 (.0152)	.0057 (.0085)
$QUARTER_5$	.6278 (.0150)	.0151 (.0085)
$QUARTER_6$	.6913 (.0152)	.0058 (.0085)
COPAY-GROUP	-.0533 (.0127)	-.0697 (.0094)
COPAY-REQUIRED	-.0215 (.0103)	.0219 (.0103)
SEX	-.2076 (.0094)	-.0375 (.0047)
AGE	.0637 (.0050)	.0557 (.0025)
TULARE	-.0618 (.0113)	-.0321 (.0057)
VENTURA	-.1399 (.0177)	-.0065 (.0090)
F-statistic to test restrictions of Model II (d.f. = $(24,\infty)$)	23.56 $p < .00001$	1.36 $p = .109$

Table 4

MODEL III: ESTIMATED EQUATIONS FOR PHYSICIAN-VISITS AND HOSPITAL-DAYS

(Standard errors are shown in parentheses)

Explanatory Variables[a]	Dependent Variable: PHYSICIAN-VISITS	Dependent Variable: HOSPITAL-DAYS
$QUARTER_1$	.7603 (.0242)	.0182 (.0176)
$QUARTER_2$	.7990 (.0190)	.0255 (.0181)
$QUARTER_3$	.8018 (.0210)	.0284 (.0150)
$QUARTER_4$	.6100 (.0201)	.0183 (.0152)
$QUARTER_5$	.5202 (.0191)	.0089 (.0153)
$QUARTER_6$	.5999 (.0201)	.0012 (.0156)
COPAY-GROUP	-.0282 (.0128)	-.0714 (.0096)
COPAY-REQUIRED	-.0581 (.0107)	.0242 (.0107)
F-statistic to test restrictions of Model III (d.f. = $(4,\infty)$)	1.71 $p < .145$	0.29 $p = .885$

[a]Except SEX_t, AGE_t, $TULARE_t$, and $VENTURA_t$.

The magnitude of the estimated Model III coefficients of COPAY-GROUP emphasizes how much the copayment group differed from the non-copayment group. The grand mean of HOSPITAL-DAYS is .127 hospital-days per quarter. Thus, the between-group difference represented by the -.0714 coefficient of COPAY-GROUP implies that, other things being equal, the wealthier copayment group would utilize the hospital about half as much as the poorer noncopayment group.

Our specification implies (a) that this substantial difference between the experimental and control groups can be fully accounted

for by the additive shift term in the equations and (b) that the two groups are equally affected by omitted variables. With groups as different as those considered here, we regard these assumptions as restrictive, albeit necessary. For example, if the incidence of a disease affecting primarily the young (and requiring hospitalization) was relatively high (low) in the first two quarters, the results would be biased toward showing that copayments increased (decreased) hospitalization. Unfortunately, we do not have data on diagnosis to control for this possibility. Also, hospitalization in quarter 2 could lower income and increase the likelihood of assignment to the noncopay group. A part of the drop in hospitalization, following quarter 2, among the noncopay group would then be spurious. Such unmeasured control-experimental differences may dominate our results concerning copayment.

Assuming the correctness of our model, we now turn to the questions posed in the introduction. In answer to our first question, the significant negative coefficient of COPAY-REQUIRED in the PHYSICIAN-VISITS equation implies that the copayment requirement did indeed reduce the demand for ambulatory care. The seemingly modest charge of $1 per visit resulted in a decrease of .058 office visit per person per quarter--an 8 percent decline. Thus, it is clear that the own-price effect for ambulatory services is important, as others have found (Scitovsky and McCall, 1977).

The major finding of this study is the answer to our second question. The coefficient of COPAY-REQUIRED in the HOSPITAL-DAYS equation implies that imposing a charge of $1 per office visit increased *hospital* care provided per person by .024 day per quarter--a 17 percent jump relative to the precopayment average in the entire sample. Although this coefficient has a wide confidence interval, it is significantly positive and decidedly large. This result differs markedly from previously reported studies (see footnote 1).

There are at least three possible explanations for the increased rate of hospitalization in the experimental group: (1) It is a statistical artifact, arising from unmeasured differential effects on the copayment and noncopayment groups, as discussed above. (2) The locus of treatment for a given mix of medical problems shifted in response to a change in relative prices. (3) The mix of medical problems changed

because those of whom copayment was required were deterred from seeking preventive or early diagnostic treatment in a doctor's office. As a result, they were later hospitalized for diseases that could have been avoided or treated at an earlier stage on an outpatient basis. Unfortunately, the data are of little help in distinguishing among the hypotheses.[9,10]

Now we turn to the third question posed in the introduction, the effect of copayment on overall Medi-Cal resource costs. Did increasing the price of ambulatory care save health care resources on balance, or did the resulting increase in hospital expenses dominate the savings in ambulatory care? Roemer et al. simply compared average usage by the copayment group in periods before and after the copayment requirement was imposed and concluded that the program increased total Medi-Cal costs by a small percentage. Statistical analysis of the data provides a sounder basis for estimation, as well as confidence intervals for the effect of copayment on both dollar expenditures and actual resource costs.

We begin by reestimating Model III, using costs reimbursed by Medi-Cal as the dependent variable instead of utilization measured in physical units. In addition to the change in physician and hospital costs, we have also estimated the change in costs for prescription drugs. Unfortunately, in the case of physician costs, Model III was rejected. To derive the estimated effect on cost, we modified Model III to allow the coefficients of COPAY-REQUIRED to differ in the last four quarters; this modified model was not rejected. We then took a weighted average of the coefficients for COPAY-REQUIRED for the last four quarters (using shares of expenditure as weights) and used this average as an estimate of the program's effect on physician costs. (For predictive purposes, this requires an assumption that the mean of the four quarters is an unbiased estimate of the mean from an extended time series.) In the case of prescription drugs, Model III, even as modified for physician costs, was rejected. To derive an estimate of the effect on drug costs, we used the $\left({}^{i}_{0}C_{N}\right)$ coefficients from Model IV, and took the difference between a weighted average of the coefficients for the first two quarters and the coefficients for the last four quarters. (Imposing Model III would yield similar estimates.)

The estimated changes in program cost and their standard errors are shown in Table 5. The total estimated program cost increased by \$.93, with a standard error of approximately \$1.46.[11] The average expenditure per Medi-Cal eligible person per quarter was approximately \$28, so the point estimate of \$.93 represents a 3 percent increase in program costs (the 95 percent confidence bounds are (-7 percent, +14 percent)).

Table 5

ESTIMATED EFFECTS OF THE COPAYMENT REQUIREMENT ON COSTS FOR HOSPITAL SERVICES, AMBULATORY CARE, AND PRESCRIPTION DRUGS

(Per person per quarter)

	Hospital Expense	Physician Expense	Drug Expense	Total
Expenditure change	+\$2.64	-\$1.04	-\$.67	+\$.93
Standard error	(1.45)	(.12)	(.08)	(1.46)[a]

[a]See footnote 11 in the text.

Resource costs exceed program costs, because the copayments themselves represent true resource expenditures. To estimate resource costs precisely, one must know the distribution of physician visits and numbers of prescriptions per person per month. (Then one would know the fraction of doctor visits and prescriptions for which copayments were made.) For computational reasons, we have not estimated this distribution. An additional problem in estimating resource cost is that the copayment was never collected from 30 percent of those in the copayment group (see Brian and Gibbens, 1974).

Nevertheless, we have calculated bounds on the true resource expenditure. The data show an average of approximately .7 visit per person per quarter. The average number of prescriptions per person per quarter is unknown to us, but the average expense of \$4 per quarter almost exactly matches the average cost of a single prescription for the U.S. population as a whole (Firestone, 1970). Thus, it

appears that there was approximately one prescription filled per person per quarter. At one extreme, the distribution of services would be highly concentrated, and the resource cost charge would be negligibly above the 3 percent program cost charge. At the other extreme, a $1 copayment would be made for each .7 physician visit per person per quarter, and a $.50 copayment would be made for 1.0 prescription per person per quarter. This would add $1.20 per person per quarter to the resource use of persons with copayments. In this case, the copayment program would increase resource use by a maximum of $.93 + $1.20 = $2.13 (8 percent) per person per quarter, and the 95 percent confidence interval would shift upward (-2 percent, +18 percent). The logical bounds on the point estimate of the change in resource utilization are thus 3 and 8 percent.

V. SUMMARY

We have employed a very general Zellner-type indirect regression technique to assess the impact of a copayment requirement on the utilization of health care resources by the poor. The data come from the California Copayment Experiment, the major shortcoming of which is that the experimental and control groups exhibited very different propensities to use the hospital in the period when neither had to copay.[12] To analyze the data, we have made the assumptions that the two groups were equally affected by the incidence of disease and other omitted variables, and that the difference between them is captured by an additive constant.

Our results indicate that strong price effects may be at work in a welfare population. Requiring a $1 copayment for physician visits appears to decrease the demand for ambulatory care by 8 percent and increase the demand for hospital inpatient services by 17 percent. Although the 95 percent confidence intervals for our estimates of the total resource cost of the program are large, including negative as well as positive values, point estimates indicate that there was a 3 to 8 percent *increase* in overall program cost. Thus, out-of-pocket payments for ambulatory services in a welfare population could be self-defeating as a method of controlling costs.

FOOTNOTES TO TEXT

1. See, for example, Davis and Russell (1972), Freiberg and Scutchfield (1976), Hill and Veney (1970), and Lewis and Keairnes (1970).

2. See Beck (1974).

3. The Medicaid program is called Medi-Cal in California.

4. For example, Chen (1976) presents a graph of the physician visit data for which he used an alternative base quarter for the index; Chen's graph does not suggest any obvious difference in the behavior of the two groups.

5. We will show that even when efficient statistical techniques are used, and despite the large sample size, confidence intervals on our parameter estimates are surprisingly large.

6. For a more detailed description of the program, see Brian and Gibbens (1974).

7. This enabled us to use a "hospital-days per quarter" variable in the analysis, rather than the less-reliable "unduplicated count of hospital patients during a quarter" variable that Roemer et al. used in their visual analysis of the data. The former measure provides a natural severity weighting, which is important because total length of stay is not a constant among those admitted during a quarter or between quarters.

8. It would have been preferable to calculate F-statistics for the test of the validity of each successive set of constraints rather than to compare Models I, II, and III with Model IV. We were unable to perform such tests because of software limitations.

9. The hypothesis that copayment degrades health status has the implication that hospital admissions should be rising through the experimental period for the copayment group, ceteris paribus. We tested the null hypothesis that there was no rising trend in the effect of the copayment requirement on hospitalization by reestimating Model III, allowing the COPAY-REQUIRED coefficient to vary linearly across quarters three through six. The null hypothesis could not be rejected; however, calculations (available from the authors) showed that the test had very little power, even if there were, in fact, a dramatic increase in the effect over time. (A doubling of the effect between quarters three and six would be detected by a one-tailed test of size α = .10 only with probability 0.33.)

10. Another method of partially disentangling the effect of a substitution from that of worsened health is the following. Let X = the matrix of exogenous variables in Eq. (A.6) in the appendix. Then suppose we interpret the equations for Model III,

$$y^1 = X\gamma^1 + \varepsilon^1 ,$$
$$y^2 = X\gamma^2 + \varepsilon^2 ,$$

where y^1 = PHYSICIAN-VISITS and y^2 = HOSPITAL-DAYS, as the reduced form of a recursive system:

$$y^1 = Xc^1 + u^1 ,$$

$$y^2 = c_o^2 y^1 + Xc^2 + u^2 .$$

Then we can decompose the total effect of a shift of an exogenous variable on HOSPITAL-DAYS into its component parts:

$$\gamma^2 = c_o^2 c^1 + c^2 .$$

Thus c^2 is the direct effect and $c_o^2 c^1$ is the indirect effect, working through changes in PHYSICIAN-VISITS. Unfortunately, there were no instruments available for the estimation of the second set of equations above, and the GLS estimation of these structural equations will result in inconsistent estimates of the parameters unless u^1 and u^2 are uncorrelated. Although estimation of the structural equations by GLS attributed the increased hospitalization resulting from the copayment requirement almost completely to a cross-price effect, there are too many caveats to justify reporting these results in detail or placing much reliance on them.

11. Because it would involve excessive computational costs, we have not derived the estimates of the cost effects of the program in a larger simultaneous model allowing calculation of the covariances between the cost estimates for each sector. These covariance estimates are necessary to derive an estimate of the variance of the sum of the component costs. However, because the magnitude of the covariance of two random variables is bounded by the geometric mean of their variances, we can bound the estimated standard error of the sum of the costs between \$1.33 and \$1.58.

12. In a few years, we will have much better data with which to address the issue of cross-price elasticities for hospital and physician services in both the poor and general populations. A social experiment is underway in which a random sample of the nonaged population has been assigned to various health insurance plans. Families with incomes of less than 150 percent of the poverty line have been oversampled. The experimental plans include a plan in which all care is free, several that require a percentage of the bill to be paid by the patient (the percentage is the same for inpatient and outpatient services), and one that approximates a \$150 per-person-per-year deductible that applies to outpatient services only (inpatient services are fully reimbursed). The collection and analysis of these data should be complete by the early 1980s (see Newhouse, 1974).

APPENDIX

In this appendix we present the series of four nested models in matrix notation and develop explicitly the computational technique used to estimate these models with a stochastic specification of the desired level of generality.

MODEL I

The simplest ANOCOVA model (Model I) can be written in matrix notation as

$$
\begin{bmatrix} Y_1 \\ Y_2 \\ Y_3 \\ Y_4 \\ Y_5 \\ Y_6 \end{bmatrix}
=
\begin{array}{c}
\begin{array}{cccccccc} \text{constant} & \text{PRIOR-AUTHORIZATION} & \text{SUMMER} & \text{AUTUMN} & \text{WINTER} & \text{COPAY-GROUP} & \text{COPAY-REQUIRED} & \end{array} \\
\begin{bmatrix}
i & 0 & i & 0 & 0 & \begin{smallmatrix} i_C \\ 0_N \end{smallmatrix} & 0 & Z \\
i & i & 0 & i & 0 & \begin{smallmatrix} i_C \\ 0_N \end{smallmatrix} & 0 & Z \\
i & i & 0 & 0 & i & \begin{smallmatrix} i_C \\ 0_N \end{smallmatrix} & \begin{smallmatrix} i_C \\ 0_N \end{smallmatrix} & Z \\
i & i & 0 & 0 & 0 & \begin{smallmatrix} i_C \\ 0_N \end{smallmatrix} & \begin{smallmatrix} i_C \\ 0_N \end{smallmatrix} & Z \\
i & i & i & 0 & 0 & \begin{smallmatrix} i_C \\ 0_N \end{smallmatrix} & \begin{smallmatrix} i_C \\ 0_N \end{smallmatrix} & Z \\
i & i & 0 & i & 0 & \begin{smallmatrix} i_C \\ 0_N \end{smallmatrix} & \begin{smallmatrix} i_C \\ 0_N \end{smallmatrix} & Z
\end{bmatrix}
\end{array}
\alpha +
\begin{bmatrix} \varepsilon_1^I \\ \varepsilon_2^I \\ \varepsilon_3^I \\ \varepsilon_4^I \\ \varepsilon_5^I \\ \varepsilon_6^I \end{bmatrix}
\qquad (A.1)
$$

where the Y_t are $n \times 1$ vectors of observations on the dependent variables during quarter t; Z is an $n \times 4$ matrix of observations on the variables SEX, AGE, VENTURA, and TULARE; the ε_t^I are $n \times 1$ vectors of error terms; i and 0 are $n \times 1$ vectors of units and zeros, respectively; i_C and 0_N are column vectors of units and zeros containing n_C and n_N elements, respectively; and α is a column vector with 11 elements. As we noted in the text, this specification implies three sets of constraints that will be successively relaxed in the models

introduced below. Before doing so, however, we turn to the issue of the stochastic specification of Model I.

The ANOCOVA model is based on the assumption that the error terms are independently and identically distributed. It is unlikely, however, that the error terms are independent in this model; in particular, we would expect the error term for a certain individual in a given quarter to be correlated with the error term for the same individual in another quarter. One possible parameterization of this relationship is the error components model (see Wallace and Hussain, 1969):

$$\varepsilon_{it} = \eta_i + \nu_{it} , \tag{A.2}$$

where the error term ε_{it} for the ith individual in the tth quarter is the sum of an individual effect η_i and a particular effect ν_{it}.[1]

Unfortunately, neither the deterministic nor the stochastic portions of the modified ANOCOVA model as defined by Eqs. (A.1) and (A.2) are fully adequate to answer questions of interest about the copayment experiment.[2]

MODEL II

The first generalization of Model I relaxes the constraint that seasonal effects be the same each year. We replace the first five columns in the matrix of exogenous variables with seven columns: a constant, PRIOR-AUTHORIZATION, SUMMER_{71}, AUTUMN_{71}, WINTER_{72}, SUMMER_{72}, and AUTUMN_{72}. (In addition, α is replaced by a column vector with 13 elements.) The resulting matrix, however, is not of full column rank because of the linear dependence among SUMMER_{71}, PRIOR-AUTHORIZATION, and the constant columns. Accordingly, we must replace the seven time dummies (a constant, PRIOR-AUTHORIZATION, and five quarter dummies), with six time dummies that span the same space, giving

$$\begin{bmatrix} Y_1 \\ Y_2 \\ Y_3 \\ Y_4 \\ Y_5 \\ Y_6 \end{bmatrix} = \begin{array}{c} \begin{array}{ccccccccc} \text{QUARTER}_1 & \text{QUARTER}_2 & \text{QUARTER}_3 & \text{QUARTER}_4 & \text{QUARTER}_5 & \text{QUARTER}_6 & \text{COPAY-GROUP} & \text{COPAY-REQUIRED} & \end{array} \\ \begin{bmatrix} i & 0 & 0 & 0 & 0 & 0 & \begin{smallmatrix} i_C \\ 0_N \end{smallmatrix} & 0 & Z \\ 0 & i & 0 & 0 & 0 & 0 & \begin{smallmatrix} i_C \\ 0_N \end{smallmatrix} & 0 & Z \\ 0 & 0 & i & 0 & 0 & 0 & \begin{smallmatrix} i_C \\ 0_N \end{smallmatrix} & \begin{smallmatrix} i_C \\ 0_N \end{smallmatrix} & Z \\ 0 & 0 & 0 & i & 0 & 0 & \begin{smallmatrix} i_C \\ 0_N \end{smallmatrix} & \begin{smallmatrix} i_C \\ 0_N \end{smallmatrix} & Z \\ 0 & 0 & 0 & 0 & i & 0 & \begin{smallmatrix} i_C \\ 0_N \end{smallmatrix} & \begin{smallmatrix} i_C \\ 0_N \end{smallmatrix} & Z \\ 0 & 0 & 0 & 0 & 0 & i & \begin{smallmatrix} i_C \\ 0_N \end{smallmatrix} & \begin{smallmatrix} i_C \\ 0_N \end{smallmatrix} & Z \end{bmatrix} \end{array} \beta + \begin{bmatrix} \varepsilon_1^{II} \\ \varepsilon_2^{II} \\ \varepsilon_3^{II} \\ \varepsilon_4^{II} \\ \varepsilon_5^{II} \\ \varepsilon_6^{II} \end{bmatrix} \qquad \text{(A.3)}$$

where β is a column vector with 12 elements, and the new time dummies are QUARTER_t, $t = 1,\ldots,6$. It is not possible to recover the coefficients of the seven time dummies from estimates of β. Therefore, we cannot determine the effect of prior authorization using Model II. However, the uniformity of seasonal effects can be reimposed, and the Model I parameters (α) can be recovered from the estimates of the Model II parameters (β), by imposing a restriction on the coefficients in the estimation of Model II. Specifically, we can estimate Model II subject to $\beta_{\text{QUARTER}_2} = \beta_{\text{QUARTER}_6}$ (since only the second and sixth quarters hold both season and prior-authorization constant) and then use

$$\begin{bmatrix} \alpha_{\text{CONSTANT}} \\ \alpha_{\text{PRIOR-AUTHORIZATION}} \\ \alpha_{\text{SUMMER}} \\ \alpha_{\text{AUTUMN}} \\ \alpha_{\text{WINTER}} \end{bmatrix} = \begin{bmatrix} 1 & 0 & 0 & 1 & -1 & 0 \\ -1 & 1 & 0 & 0 & 1 & -1 \\ 0 & 0 & 0 & -1 & 1 & 0 \\ 0 & 0 & 0 & -1 & 0 & 1 \\ 0 & 0 & 1 & -1 & 0 & 0 \end{bmatrix} \begin{bmatrix} \beta_{\text{QUARTER}_1} \\ \beta_{\text{QUARTER}_2} \\ \beta_{\text{QUARTER}_3} \\ \beta_{\text{QUARTER}_4} \\ \beta_{\text{QUARTER}_5} \\ \beta_{\text{QUARTER}_6} \end{bmatrix} \qquad \text{(A.4)}$$

to estimate the Model I parameters.[3] The importance of this indirect method of estimating α will be explained below.

MODEL III

If we also relax the assumption that the coefficients of SEX, AGE, TULARE, and VENTURA are constant over time, we can rewrite the model as

$$
\begin{bmatrix} Y_1 \\ Y_2 \\ Y_3 \\ Y_4 \\ Y_5 \\ Y_6 \end{bmatrix}
=
\begin{array}{c}
\begin{array}{cccccccc} \text{COPAY-GROUP} & \text{COPAY-REQUIRED} & & & & & & \end{array} \\
\begin{bmatrix}
\begin{matrix} i_C \\ 0_N \end{matrix} & 0 & i\ Z & 0 & 0 & 0 & 0 & 0 \\
\begin{matrix} i_C \\ 0_N \end{matrix} & 0 & 0 & i\ Z & 0 & 0 & 0 & 0 \\
\begin{matrix} i_C \\ 0_N \end{matrix} & \begin{matrix} i_C \\ 0_N \end{matrix} & 0 & 0 & i\ Z & 0 & 0 & 0 \\
\begin{matrix} i_C \\ 0_N \end{matrix} & \begin{matrix} i_C \\ 0_N \end{matrix} & 0 & 0 & 0 & i\ Z & 0 & 0 \\
\begin{matrix} i_C \\ 0_N \end{matrix} & \begin{matrix} i_C \\ 0_N \end{matrix} & 0 & 0 & 0 & 0 & i\ Z & 0 \\
\begin{matrix} i_C \\ 0_N \end{matrix} & \begin{matrix} i_C \\ 0_N \end{matrix} & 0 & 0 & 0 & 0 & 0 & i\ Z
\end{bmatrix}
\end{array}
\gamma
+
\begin{bmatrix} \varepsilon_1^{III} \\ \varepsilon_2^{III} \\ \varepsilon_3^{III} \\ \varepsilon_4^{III} \\ \varepsilon_5^{III} \\ \varepsilon_6^{III} \end{bmatrix}
\qquad (A.5)
$$

The crucial parameter, $\gamma_{COPAY\text{-}REQUIRED}$, can be estimated directly in this model. By using a procedure analogous to that developed for Model II, α can then be recovered by imposing the restrictions that $\gamma_{QUARTER_2} = \gamma_{QUARTER_6}$ and that the coefficients of Z are constant over time.

MODEL IV

The final model in this series of nested models also relaxes the assumption that COPAY-GROUP and COPAY-REQUIRED have constant coefficients over time. Because fully expanding the first two columns of the exogenous variable matrix in Eq. (A.5) would result in a matrix not of full column rank, we must collapse such a matrix and write Model IV as

R-2167

$$\begin{bmatrix} Y_1 \\ Y_2 \\ Y_3 \\ Y_4 \\ Y_5 \\ Y_6 \end{bmatrix} = \begin{bmatrix} i\,{}^{i}_{0}C_{N}\,Z & & & & & \\ & i\,{}^{i}_{0}C_{N}\,Z & & & 0 & \\ & & i\,{}^{i}_{0}C_{N}\,Z & & & \\ & 0 & & i\,{}^{i}_{0}C_{N}\,Z & & \\ & & & & i\,{}^{i}_{0}C_{N}\,Z & \\ & & & & & i\,{}^{i}_{0}C_{N}\,Z \end{bmatrix} \delta + \begin{bmatrix} \varepsilon_1^{IV} \\ \varepsilon_2^{IV} \\ \varepsilon_3^{IV} \\ \varepsilon_4^{IV} \\ \varepsilon_5^{IV} \\ \varepsilon_6^{IV} \end{bmatrix} \qquad (A.6)$$

Equation (A.6) can be viewed as a set of six seemingly unrelated equations and suggests the stochastic specification $\varepsilon \sim N(0, \Sigma \otimes I)$, where Σ is a 6×6 general matrix of person-specific correlations and I is the identity matrix of order n. An important question is whether Σ can be reasonably well approximated by

$$\Sigma = \sigma_\nu^2 I_6 + \sigma_\eta^2 J_6 , \qquad (A.7)$$

where I_6 is a 6×6 identity matrix and J_6 is a 6×6 matrix of unit elements as implied by the error components model in Eq. (A.2). Although we would expect an important "individual component" in the error term, the covariance terms close to the diagonal should be larger than those farther away from it because a person might be receiving treatment for the same or a related illness in an adjacent quarter.

Examination of the estimated residual variance-covariance matrices $\hat{\Sigma}$ for hospital-days and office visits reveals such a pattern.[4] For the hospital-days equation in particular, there were pronounced differences in the off-diagonal elements of $\hat{\Sigma}$, the largest term being four times greater than the smallest term. There was also significant heteroscedasticity across quarters, especially for the physician visits equation. (Variance fell by 40 percent after prior authorization was introduced, implying that prior authorization had a marked effect on

use by outliers.) For these reasons, we do not impose the variance components structure on Σ, but, instead, leave Σ unconstrained.

The estimation procedure is as follows: (1) We estimate Model IV directly as a system of seemingly unrelated regressions. It is not possible, however, to estimate Models I, II, or III directly with a stochastic specification of $\varepsilon \sim N(0, \Sigma \otimes I)$ by estimating six constrained seemingly unrelated regressions (one for each quarter), because perfect multicollinearity would result. (For example, in quarters three through six, COPAY-GROUP and COPAY-REQUIRED are identical.) Instead, we employ the following indirect method: (2) We estimate Model III indirectly by estimating Model IV subject to the constraint that the coefficients of $\begin{pmatrix} i_C \\ 0_N \end{pmatrix}$ in the first two equations (corresponding to the first two quarters) must be equal and also that the coefficients of $\begin{pmatrix} i_C \\ 0_N \end{pmatrix}$ in the third, fourth, fifth, and sixth equations must all be equal. The coefficient of COPAY-GROUP in Model III is given by the estimated coefficient of $\begin{pmatrix} i_C \\ 0_N \end{pmatrix}$ in the first equation of Model IV, and the coefficient of COPAY-REQUIRED is given by the difference of the coefficients of $\begin{pmatrix} i_C \\ 0_N \end{pmatrix}$ in the first and third equations of Model IV. (3) We estimate Model II by using a seemingly unrelated regression method on Model IV constrained as in Model III, but with the further constraint that the coefficients of Z must be constant over time. (4) Finally, we estimate Model I by using a seemingly unrelated regression method on Model IV constrained as in Model II but with the additional constraint that the constants in the second and sixth equations (β_{QUARTER_2} and β_{QUARTER_6}) must be the same. The parameters of COPAY-GROUP and COPAY-REQUIRED can be recovered as for Model III; the remaining Model I parameters are given by Eq. (A.4).

To summarize, we estimate the model of interest indirectly by using a very general, seemingly unrelated, regression model (Model IV) and then imposing constraints on the coefficients. The desired parameter values can be recovered by exploiting their relationship to the constrained Model IV coefficients. This procedure permits a general stochastic specification and offers the opportunity to test hypotheses about a series of nested models. If the validity of Models I and II can be rejected, the ANOCOVA model will lead to inconsistent estimates

of the parameters, because inappropriate constraints have been imposed. Conditional upon the validity of Model III, we can estimate the coefficient of COPAY-REQUIRED, the variable of greatest policy interest.

R-2167

FOOTNOTES TO APPENDIX

1. Note that if the individual effects η_i cannot be regarded as independent of the exogenous variables in Eq. (A.2), a variance components estimator will be biased and inconsistent. This would not seem to be a problem here; however, a test of independence (see Hausman, 1976, pp. 16-21) was not performed.

2. The gains from using a more general stochastic specification than Eq. (A.2) are likely to be small. Generalization will, however, be pursued because there is a simple computational technique available to deal with a more complex error structure, and because it is important to use powerful test statistics in this model. The computational technique will be developed below. We have not incorporated the nonnegativity constraint on the dependent variable through a Tobit estimator because of computational costs.

3. To verify this, write Model I as

$$\begin{bmatrix} y_1 \\ y_2 \\ y_3 \\ y_4 \\ y_5 \\ y_6 \end{bmatrix} = \left[\begin{array}{cccccc|ccc} 0 & i & 0 & i & 0 & 0 & \begin{smallmatrix} i_C \\ 0_N \end{smallmatrix} & 0 & Z \\ 0 & i & i & 0 & i & 0 & \begin{smallmatrix} i_C \\ 0_N \end{smallmatrix} & 0 & Z \\ i & i & i & 0 & 0 & i & \begin{smallmatrix} i_C \\ 0_N \end{smallmatrix} & \begin{smallmatrix} i_C \\ 0_N \end{smallmatrix} & Z \\ i & i & i & 0 & 0 & 0 & \begin{smallmatrix} i_C \\ 0_N \end{smallmatrix} & \begin{smallmatrix} i_C \\ 0_N \end{smallmatrix} & Z \\ i & i & i & i & 0 & 0 & \begin{smallmatrix} i_C \\ 0_N \end{smallmatrix} & \begin{smallmatrix} i_C \\ 0_N \end{smallmatrix} & Z \\ i & i & i & 0 & i & 0 & \begin{smallmatrix} i_C \\ 0_N \end{smallmatrix} & \begin{smallmatrix} i_C \\ 0_N \end{smallmatrix} & Z \end{array}\right] \left[\begin{array}{l} \alpha_{NULL} \\ \alpha_{CONSTANT} \\ \alpha_{PRIOR\text{-}AUTHORIZATION} \\ \alpha_{SUMMER} \\ \alpha_{AUTUMN} \\ \alpha_{WINTER} \\ \hline \alpha_{COPAY\text{-}GROUP} \\ \alpha_{COPAY\text{-}REQUIRED} \\ \alpha_Z \end{array}\right] + \begin{bmatrix} \varepsilon_1^I \\ \varepsilon_2^I \\ \varepsilon_3^I \\ \varepsilon_4^I \\ \varepsilon_5^I \\ \varepsilon_6^I \end{bmatrix} \qquad \text{(nA.1)}$$

where an additional column has been added on the left-hand side of the matrix of exogenous variables and its coefficient is α_{NULL}. More compactly,

$$y = (X_A^I \mid X_B) \begin{pmatrix} \alpha_A \\ \hline \alpha_B \end{pmatrix} + \varepsilon^I = X_A^I \alpha_A + X_B \alpha_B + \varepsilon^I \ . \qquad \text{(nA.2)}$$

R-2167

Similarly, but without the addition of the extra column and parameter, we can write Model II as

$$y = (X_A^{II} \mid X_B)\begin{pmatrix}\beta_A \\ \hline \beta_B\end{pmatrix} + \varepsilon^{II} = X_A^{II}\beta_A + X_B\beta_B + \varepsilon^{II} . \qquad \text{(nA.3)}$$

Now let

$$F = \begin{bmatrix} 0 & 1 & 0 & 1 & 0 & 0 \\ 0 & 1 & 1 & 0 & 1 & 0 \\ 1 & 1 & 1 & 0 & 0 & 1 \\ 1 & 1 & 1 & 0 & 0 & 0 \\ 1 & 1 & 1 & 1 & 0 & 0 \\ 1 & 1 & 1 & 0 & 1 & 0 \end{bmatrix}$$

Then $X_A^I = X_A^{II}F$. Thus $X_A^{II}\beta_A = X_A^{II}FF^{-1}\beta_A = X_A^I F^{-1}\beta_A$. Note that the error terms and the coefficients of X_B in Eqs. (nA.2) and (nA.3) are identical, since the columns of X_A^I and X_A^{II} span the same space. And, since the equality of Eqs. (nA.2) and (nA.3) implies that $X_A^I\alpha_A = X_A^{II}\beta_A$, we have $\alpha_A = F^{-1}\beta_A$, or

$$\begin{bmatrix} \alpha_{NULL} \\ \alpha_{CONSTANT} \\ \alpha_{PRIOR\text{-}AUTHORIZATION} \\ \alpha_{SUMMER} \\ \alpha_{AUTUMN} \\ \alpha_{WINTER} \end{bmatrix} = \begin{bmatrix} 0 & -1 & 0 & 0 & 0 & 1 \\ 1 & 0 & 0 & 1 & -1 & 0 \\ -1 & 1 & 0 & 0 & 1 & -1 \\ 0 & 0 & 0 & -1 & 1 & 0 \\ 0 & 0 & 0 & -1 & 0 & 1 \\ 0 & 0 & 1 & -1 & 0 & 0 \end{bmatrix} \begin{bmatrix} \beta_{QUARTER_1} \\ \beta_{QUARTER_2} \\ \beta_{QUARTER_3} \\ \beta_{QUARTER_4} \\ \beta_{QUARTER_5} \\ \beta_{QUARTER_6} \end{bmatrix} \qquad \text{(nA.4)}$$

Finally, note that if $\alpha_{NULL} = 0$, Eq. (nA.4) reduces to Eq. (5) in the text plus the constraint that $\beta_{QUARTER_2} = \beta_{QUARTER_6}$.

4. An OLS estimation of Model IV provides a consistent estimate of δ and hence of ε. Since Model IV is a strict generalization of Models I, II, and III, $(\hat{\Sigma} \otimes I)$ is a consistent estimate of the residual variance-covariance matrix for each model, where $\hat{\Sigma} = (\hat{\sigma}_{ij}) = (\hat{\varepsilon}_i'\hat{\varepsilon}_j)$.

R-2161

REFERENCES

Beck, R. G., "The Effects of Co-Payment on the Poor," *Journal of Human Resources*, Vol. 9, No. 1, Winter 1974, pp. 129-142.

Brian, Earl W., and Stephen F. Gibbens, "California's Medi-Cal Copayment Experiment," *Medical Care*, Vol. 12, December 1974, pp. 1-56.

Chen, Martin, "Letter to the Editor," *Medical Care*, Vol. 14, November 1976, p. 958.

Davis, Karen, and Louise B. Russell, "The Substitution of Hospital Outpatient Care for Inpatient Care," *Review of Economics and Statistics*, Vol. 54, May 1972, pp. 109-120.

Firestone, John M., *Trends in Prescription Drug Prices*, American Enterprise Institute, Washington, D.C., 1970.

Freiberg, Lewis, and F. Douglas Scutchfield, "Insurance and the Demand for Hospital Care: An Examination of the Moral Hazard," *Inquiry*, Vol. 13, No. 1, March 1976, pp. 54-60.

Hausman, Jerry A., "Specification Tests in Econometrics," Massachusetts Institute of Technology, Department of Economics Working Paper Number 185, August 1976.

Hill, Daniel B., and James E. Veney, "Kansas Blue Cross/Blue Shield Outpatient Benefits Experiment," *Medical Care*, Vol. 8, May 1970, pp. 143-158.

Holahan, John, *Physician Supply, Peer Review and Use of Health Services in Medicaid*, The Urban Institute, Washington, D.C., 1976.

Lewis, Charles E., and Harold Keairnes, "Controlling Costs of Medical Care by Expanding Insurance Coverage," *New England Journal of Medicine*, Vol. 282, June 18, 1970, pp. 1405-1412.

Newhouse, Joseph P., "A Design for a Health Insurance Experiment," *Inquiry*, Vol. 11, March 1974, pp. 5-27.

Roemer, Milton I., Carl E. Hopkins, Lockwood Carr, and Foline Gartside, "Copayments for Ambulatory Care: Penny Wise and Pound Foolish," *Medical Care*, Vol. 13, June 1975, pp. 457-466.

Scitovsky, Anne A., and Nelda McCall, "Coinsurance and the Demand for Physician Services: Four Years Later," *Social Security Bulletin*, Vol. 40, No. 5, May 1977, pp. 19-27.

Wallace, T. D., and Ashiq Hussain, "The Use of Error Components Models in Combining Cross Section with Time Series Data," *Econometrica*, Vol. 37, January 1969, pp. 55-72.