FAX COVER SHEET

OFFICE OF LEGISLATION

Number of Pages: Ctr 7 Date: 11/2/99

To:	From:
Chris Jennings	Peter Hickman
Fax: 456-5557	Fax: 202-690-8168 or 205-5157
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REMARKS: Preliminary cuts for on Ever Care

HEALTH CARE FINANCING ADMINISTRATION
200 Independence Ave., SW
Room 341-H, Humphrey Building
Washington, DC 20201

EVERCARE
Managing Medical Care for Nursing Home Residents

Under a cooperative agreement with the Health Care Financing Administration (HCFA), the EverCare Demonstration utilizes geriatric nurse practitioners as care providers and care managers in nursing facilities. EverCare determines the most appropriate interventions for permanent long-term facility residents who are enrolled in the program and assures follow-up care. The geriatric nurse practitioner authorizes hospitalizations, schedules clinic and outpatient appointments, and consults with the nursing staffs at each facility to assist in problem solving and in coordinating the most effective and efficient care pattern for each enrolled resident. By increasing the availability and intensity of medical services in the nursing home setting, EverCare believes that unnecessary hospitalizations can be avoided and, therefore, care will be less costly.

A physician incentive plan provides a higher payment rate for a nursing home visit than for an office visit and a less than favorable rate for services furnished in the physician's office or in other settings. To facilitate communication between caregivers, the patient, and the patient's family, EverCare pays physicians for attending family conferences. Except for short periods following hospitalization, which may be covered by Medicare (depending on level of need, benefit period, etc.), the room and board and custodial long-term care costs of the nursing home stay are paid by the enrollee and/or the Medicaid program.

The demonstration is expected to reflect lower total medical costs for enrolled nursing home residents, comparable or improved quality of care and health outcomes for these residents, and increased resident and family satisfaction. EverCare has developed specific practice guidelines and care management protocols for the demonstration so that this model can be successfully replicated.

The demonstration is being conducted in six sites: Atlanta, Baltimore, Boston, Denver, Phoenix, and Tampa. No additional sites are anticipated. Each of the sites will operate for 5 years. The demonstration is scheduled to December 31, 2000.

The demonstration sites were initially paid a capitation rate equivalent to the institutional rate category under the Adjusted Average Per Capital Cost (AAPCC) paid to Medicare TEFRA HMO risk contractors for enrolles residing in institutional settings. The capitation rate was 100 percent of the cost for the first year of operation at each site, 95 percent of the AAPCC for the second year of operation, and then 93 percent of the AAPCC for the third through fifth years of the project. Starting January 1998, the sites were paid 93 percent of the Medicare + Choice rates instead of the AAPCC rates.

An evaluation contract was awarded to the Division of Health Services Research and Policy, School of Public Health, University of Minnesota. The Principal Investigator is Robert L. Kane, M.D.

Site enrollment as of September 1998

Atlanta	1402
Baltimore	1901
Boston	2087
Denver	513
Phoenix	266
Tampa	1090

Under the proposed PIP/DCG payment model, HCFA has determined that the EverCare demonstration sites will be underpaid by approximately 15%. A letter was recently sent to EverCare indicating that HCFA is willing to extend the demonstration for an additional year and defer the imposition of the proposed risk-adjusted payment methodology until January 2001.

Comments on Provisions Related to EverCare's
Medicare Demonstration and Non-Demonstration Activities

1. **If the EverCare demonstration had been paid in January 1999 using updated institutionalized factors, payments would have been about *22% less* than under the institutionalized factors now in use.** The demographic-based institutional factors now in use are based on survey data from the mid-1970s. In 1997, prior to enactment of the BBA, HCFA published proposed institutionalized factors for use in 1998 based on up-to-date survey data on Medicare spending for beneficiaries in institutions. (Data had not been available for many years because the survey had been discontinued for funding reasons.) These updated factors were lower, indicating overpayments for institutionalized beneficiaries. Subsequently, pursuant to the M+C payment provisions of the BBA, HCFA had to rescind the updated institutionalized factors and, instead, continued to use factors based on data that was twenty years out of date.

2. **If the EverCare demonstration had been paid in January 1999 using the PIP-DCG method, its payments would have been *15% less* than they were under the payment method now being used.** This is within the range of PIP-DCG impacts for Medicare+Choice plans and it is a substantially smaller impact than the 42% reduction EverCare has reported. Selectively protecting EverCare from risk adjustment while other plans are facing similar impacts established a bad precedent.

3. **When points 1 and 2 above are taken together, they show that EverCare's payments under risk adjustment would be higher than under the current method with the more accurate, updated institutionalized factors. That is, instead of a 22% reduction there would be a 15% reduction.**

4. **Payments to the EverCare demonstration and to M+C plans that contract with EverCare should not be unfairly affected by the risk adjustment system because the system does not pay more or less for avoiding the types of hospital admissions that EverCare focuses on preventing.** In the current demographic system, all payment amounts are based on demographic factors, e.g., age, sex, welfare and institutional status. Under the PIP-DCG risk adjustment method, most payment amounts continue to be based on demographic factors, but a portion (about 20% of spending on fee-for-service beneficiaries) is based on prior year hospital admissions. However, not all admissions generate higher payments. Only hospital admissions that are unavoidable and that are also predictive of future medical costs receive an additional payment. The costs of other admissions, i.e., those that are avoidable or that are not predictive of future costs, are factored into payments based on demographic factors. Thus, when enrollees are admitted for avoidable or non-predictive hospitalizations, there is no additional payment to a M+C plan. Many of the types of types of preventable hospitalizations EverCare works to avoid, e.g., bacterial pneumonia, infections, therefore would not generate an additional payment under the PIP-DCG risk adjustment method. Thus, EverCare would not be disadvantaged since spending for those admissions is factored in to payments

based on demographic factors.

5. **Enrollees in EverCare demonstration sites do have a substantial number of hospitalizations so they would receive the higher payments associated with those hospitalizations that are considered unavoidable and predictive of future costs.** HCFA assumes that EverCare is not avoiding hospitalizations of the type that generate higher payments under the PIP-DCG method; for example, those associated with serious conditions such as stroke.

6. **The average institutional enrollment for M+C plans is about 1.5% and only one plan (that is not an EverCare demonstration) has over 5% institutional enrollees. Thus, the impact of the 10% phase-in of risk adjustment would be modest for most plans. And the portion of plan payments for the institutionalized under the old system (i.e., 90% of payments in 2000) would continue to benefit for the excessive payments generated by using outdated demographic factors (see point 2.)**

   o In addition, if the phase-in of the PIP-DCG part of risk adjustment is slowed significantly, for example, as proposed by both the House and the Senate, then the impact on payments for the institutionalized will be correspondingly reduced.

7. **In a preliminary finding from the data being gathered for an evaluation of the EverCare demonstration, it appears that EverCare demonstration enrollees have lower than average scores on 3 ADLs (activities of daily living) relative to other Medicare institutionalized beneficiaries.** This may point to EverCare enrollees being somewhat more highly functional than the comparison group of beneficiaries residing in nursing homes. It also suggests that a special "frailty" or functional status adjuster developed for the institutionalized population, such as the one called for in HR 1998, might not result in favorable payments to EverCare relative to other programs for the institutionalized.

1998 Demographic Cost Factors for the Institutionalized Aged

	OLD FACTOR* (still in use because of BBA)	UPDATED FACTOR* (never implemented because of BBA)	CURRENT PERCENT OVERPAYMENT
Part A - Female			
65-69	0.55	1.45	164%
70-74	0.75	1.8	140%
75-79	1.05	2.1	100%
80-84	1.3	2.1	62%
85+	1.6	2.1	31%
Part B - Female			
65-69	1.05	1.5	43%
70-74	1.3	1.65	27%
75-79	1.45	1.65	14%
80-84	1.6	1.65	3%
85+	1.65	1.65	0%
Part A - Male			
65-69	0.8	1.75	119%
70-74	1.05	2.25	114%
75-79	1.4	2.25	61%
80-84	1.75	2.25	29%
85+	2.15	2.25	5%
Part B - Male			
65-69	1.1	1.6	45%
70-74	1.4	1.8	29%
75-79	1.65	1.95	18%
80-84	1.8	1.95	8%
85+	1.85	1.95	5%

* The demographic factors for the institutionalized for 1997 and prior years were based on Current Medicare Survey data from the mid-1970s. The survey was discontinued for funding reasons and data were not available to

update the factors for the institutionalized until 1997. The 45-day notice for the 1998 rates, published July 24, 1997, provided revised institutional factors, many of which were lower than the factors based on 20-year-old data. With enactment of the BBA, after the publications of the 45-day notice, HCFA concluded that under the BBA, using the updated factors for the institutionalized would not be permissible.

DRAFT

Provisions of H.R. 1998

1. The risk adjustment methodology mandated in the BBA and used to determine payment for Medicare+Choice (M+C) enrollees can not be used to determine payment for a frail elderly M+C beneficiary (as defined in 4 below) enrolled in a M+C plan under a specialized program (described in 3 below) until the Secretary certifies that a "comprehensive risk adjustment methodology" that takes account of the factors (described in 2) below is being fully implemented.

2. The Secretary shall develop a payment methodology for frail elderly M+C enrollees "under a specialized program for the frail elderly" (as defined in 3 below). It must account for the "prevalence, mix, and severity of chronic conditions among such beneficiaries" and "include medical diagnostic factors from all provider setting (including hospital and nursing facility settings)." It must include functional indicators of health status and other appropriate factors.

3. A specialized program for the frail elderly is a program determined by the Secretary to:

   - be offered "as a distinct part of a M+C plan;
   - be primarily enrolling frail elderly M+C beneficiaries; and
   - have a clinical delivery system specifically designed to serve the special needs of the frail elderly and coordinate care through --
     - the use of a team that includes a physician, a nurse practitioner or geriatric care manager, and has members who specialize in the care and management of the frail elderly beneficiaries; and
     - through the provision of primary care services to such beneficiaries by means of such a team at the nursing facility.

4. A frail elderly M+C beneficiary is someone who is *eligible* to enroll in an M+C plan and who is (1) residing in a SNF or a nursing facility for "an indefinite period and without any intention of residing outside the facility" and (2) has a condition the severity of which the Secretary determines, under guidelines, "makes the individual frail."

5. There will be continuous open enrollment for individuals described in (4) above who is "seeking to enroll in a M+C plan under a specialized program for the frail elderly" (as defined in 3 above).

6. The Secretary must develop and implement a program to measure the quality for care provided in specialized programs for the frail elderly that reflects the unique needs of frail elderly M+C beneficiaries not later than July 1, 2000.

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Community Nursing Organization Demonstration

Background

In 1987, Congress passed Community Nursing Organization (CNO) legislation directing the Health Care Financing Administration to design and test a new model of community-based care for Medicare beneficiaries. The current CNO demonstration program, which was extended in the Balanced Budget Act of 1997, provides services to more than 10,000 Medicare beneficiaries. The four participating CNO sites vary in structure and setting.

1. Carle Clinic, Urbana, IL, is a large medical group practice serving a primarily rural population.
2. Carondolet Health Services, Inc., Tucson, AZ, is a hospital-based organization.
3. Visiting Nurse Service of New York, NY, NY, is a non-profit certified home health agency.
4. Living at Home/Block Nurse Program, Minneapolis, MN, is a joint venture between a community-based non-profit nursing program and an accredited home health agency.

Over the past year, the CNOs have worked together to analyze their experience and refine the CNO model for the future. The intent of this document is to share our findings and insights with members of Congress and other key stakeholders in this Medicare program.

Analysis of CNO Data

The analysis reported in this paper, sponsored by the ANA, utilized data collected by Abt Associates, the evaluation contractors for the CNO demonstration. The HCFA evaluation, conducted by Abt Associates, used an experimental design in which program applicants were randomly assigned to treatment and control groups. Costs and use of Medicare-covered services were compared for CNO and control group members. The secondary analysis, conducted by the CNO's and described in this paper, focused on high-cost and high volume services, including hospitalization, emergency room utilization, skilled nursing facility stays, and outpatient/physician visits with one goal of identifying predictors of high-cost utilization.

Results: Medicare Service Use and Costs for CNO Members

Positive results have been obtained across sites despite varying markets reflecting high to low penetration of managed care and diverse settings operating in rural to urban environments. A nurse partner who works with the member to resolve problems and coordinate appropriate services and care across settings has been the primary intervention. Between 1994 and 1996, one

or more of the CNO sites showed reduced costs and/or reduced use of the higher cost services impacted by the CNO (e.g. hospitalizations, emergency room use, outpatient/physician visits.)

Arizona showed their treatment group, compared to their control group, had:
1. Fewer hospital admissions in 1994, 1995 and 1996
2. Shorter hospital length of stay in 1994, 1995 and 1996
3. Fewer emergency room events and less emergency room expenditure in 1996
4. Less outpatient expenditure in 1994, 1995 and 1996
5. Less hospital "short stay" expenditure in 1994, 1995 and 1996
6. Less skilled nursing facility expenditure in 1994, 1995 and 1996
7. Equivalent non CNO covered services* expenditure in 1996

Illinois showed their treatment group, compared to their control group, had:
1. Fewer hospital admissions in 1994, 1995 and 1996
2. Shorter hospital length of stay in 1995 and 1996
3. Less hospital "short" and rehabilitation/ psychiatric "long" stays expenditure in 1996
4. Less skilled nursing facility expenditure in 1994, 1995 and 1996
5. Equivalent "non CNO covered services" expenditures in 1995 and 1996

Minnesota showed their treatment group, compared to their control group, had:
1. Fewer hospital admissions in 1996
2. Shorter hospital length of stay in 1996
3. Less outpatient expenditure in 1996
4. Less emergency room expenditure in 1996
5. Less short and long stays expenditure in 1996
6. Less "non CNO covered services" expenditure in 1996

New York showed their treatment group, compared to their control group, had:
1. Fewer hospital admissions in 1996
2. Shorter hospital length of stay in 1996
3. Less outpatient and ER expenditure in 1996
4. Less short and long stay expenditure in 1996
5. Less skilled nursing facility expenditure in 1996
6. Less "non CNO covered services" expenditure in 1996

Targeting Members at Risk

Through the analysis, predictors of high Medicare costs were identified. These will be utilized by the CNO sites to allow for improved and efficient screening and allocation of resources for CNO enrollees during a continuation of the program. Predictors of high Medicare costs were identified using regression analysis. The HCFA evaluation of the CNO demonstration included extensive data about member characteristics, their health status and chronic illnesses, and their pattern of using health care services. Various predictive models were tested to find the set of factors that would most accurately identify "true" high risk individuals.

*non CNO covered services include hospital, emergency room, physician services

Members who are more likely to use high-cost Medicare services can be prospectively identified by a small set of characteristics. Perceived health status, the number of hospitalizations and emergency room visits within the previous six months, and the presence of specific chronic health care conditions are predictors of increased utilization of high-cost Medicare services.

The analysis demonstrated that a number of predictive models are successful in identifying true high-risk individuals. Each high-risk member, by definition, has a greater probability of spending significantly more Medicare dollars. The ability to accurately target these members and link them with a nurse partner to coordinate their care suggests the potential for considerable cost savings.

Goals for the CNO Extension

The goal for the extension of the CNOs is a refined model for home and community-based care utilizing nurses as care managers.

Objectives for the extension:

1. Continue the existing four demonstration sites using the current risk adjustment for three years
2. Determine, refine and transfer "best practices" from sites that have reduced cost and use of high-cost Medicare services.
3. Utilize predictors of high service use and costs to target selective allocation of nurse case management services and application of other care coordination best practices.
4. Assign highest priority to members predicted to be among those in the highest 25% of expenditures.
5. Test whether the targeting of CNO resources to high-risk members will reduce costs associated with expensive service use.
6. Refine procedures for coordinating CNO services with primary care providers.
7. Develop and refine processes for managing chronic illness.
8. Refine and test a new payment model.

DEPARTMENT OF HEALTH & HUMAN SERVICES Health Care Financing Administration

7500 SECURITY BOULEVARD
BALTIMORE MD 21244-1850

JUL 19 1999

Dear CNO Site Director:

Thank you for submitting Community Nursing Organization (CNO) program phase down plans to us. This letter is to inform you of some activities, time lines, and dates which will be necessary to adhere to as we proceed through the phase down process of the CNO Demonstration. These dates and timelines will be consistent across sites.

Marketing

All marketing activities should be curtailed by September 15, 1999.

Beneficiary and CNO Contract Provider Notifications

The Medicare beneficiaries and providers under contract with the CNO should be given written notification no later than October 1, 1999, that the CNO demonstration ends on December 31, 1999. This is necessary to provide adequate time to prepare and assist beneficiaries in transitioning to other providers. Also, all CNO contract providers should be reminded to submit their bills to the CNO no later than February 15, 2000.

Enrollments

The last date for any new (first time) enrollments will be October 1, 1999. Reenrollments will be accepted up to and including December 1 as long as they are informed that the demonstration ends December 31, 1999.

Reassessments

Reassessments can be conducted up to December 31, 1999, to avoid HCFA setting cells for missed assessments.

Disenrollments

Continue to submit disenrollments as usual using reason codes 01 through 11. After HCFA processes the sites' last diskette, HCFA will globally assign a disenrollment date of 12/31/99 and a reason code designated for the demonstration end for those beneficiaries who have not been disenrolled for another reason.

Receipt of Final Data

March 15, 2000, will be the last day for HCFA to receive data (enrollments, reassessments, disenrollments, cell changes).

Receipt of Final Cost Report

A final cost report is due in HCFA by June 1, 2000.

Payments

Since October 1 is the last day we will accept new enrollments, October will also be the last month we will pay on an estimate of your enrollments. We need to hold back some payments until the monthly reconciliations and 1998/1999 Out-of-Plan adjustments are made. We suggest that your enrollment figures from now until October be as accurate as possible to avoid overpayments. We hope to complete the monthly reconciliations and the 1999 Out-of-Plan Adjustments by May 2000.

Personnel

Since we estimate May 2000 for HCFA to complete final processing of data and payments, we hope that appropriate staff will be retained to assist us until the end.

Recap of Timetable

September 15, 1999	Curtail marketing activities.
October 1, 1999	Last date to notify beneficiaries/contract providers of demonstration end date. Last day to accept new enrollments. Last month for receiving enrollment payments.
November 1999	No payment for enrollments; June 1999 reconciled. 1998 OOPS adjustment.
December 1999	No payment for enrollments; July 1999 reconciled.
December 1, 1999	Last day to accept reenrollments.
December 31, 1999	Last day for reassessments.
January 2000	August 1999 reconciled.
February 2000	September 1999 reconciled.
February 15, 2000	Last day for CNO contracted providers to submit bills to CNO.
March 2000	October 1999 reconciled.
March 15, 2000	Final receipt of data submitted to HCFA. HCFA globally assigns 12/31/99 disenrollment date and reason

code for beneficiaries not otherwise disenrolled.

April 2000	November 1999 reconciled.
May 2000	December 1999 reconciled. 1999 OOPS adjustment.
June 1, 2000	Cost Report due.

Please stick to the above dates and timelines as indicated. These timelines cannot be extended but tasks under them can be completed earlier. We appreciate all your hard work and effort in the CNO Demonstration over the last six years. Should you have questions, please contact me at (410) - 786-6654 or Sharon Norman at (410) - 786-6553.

Sincerely,

Thomas Theis
Project Officer

JIM RAMSTAD
THIRD DISTRICT, MINNESOTA

WAYS AND MEANS COMMITTEE

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Congress of the United States
House of Representatives
Washington, DC 20515–2303

August 31, 1999

Mr. Mike Hash
Deputy Administrator
Health Care Financing Administration
Department of Health and Human Services
Hubert H. Humphrey Building, Room 309-C
200 Independence Avenue, SW
Washington, D.C. 20201

Dear Mr. Hash:

I recently introduced H.R. 1999, a bill to extend the Medicare Community Nursing Organization demonstrations for another three years. It builds on the language in the Balanced Budget Act, which I championed, to continue this effective and popular Medicare option. Just like last Congress, H.R. 1999 has garnered bipartisan support and I am optimistic about passing the bill this year.

As you know, all of the CNO sites received a letter dated July 19, 1999 outlining the time table for terminating the program. This current phase-down schedule will require current beneficiaries to voluntarily leave the program or not be reassessed by the four sites. This will also negatively impact the sites' ability to maintain their current nursing staff – the very nurses who have developed the key relationships with patients that make the program such a success.

That's why I am respectfully requesting HCFA to issue a revised disenrollment date of November 1, 1999. This date will allow Congress to complete its deliberations and reach a consensus on extending the CNO demonstrations. Should Congress not extend the demonstration authority, HCFA would still be able to terminate the programs by the scheduled date.

Thank you for your attention and consideration of this request. I look forward to your timely response.

Sincerely,

JIM RAMSTAD
Member of Congress

PAUL D. WELLSTONE
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COMMITTEES:
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FOREIGN RELATIONS

September 2, 1999

Nancy-Ann Min DeParle, Administrator
Health Care Financing Administration
200 Independence Avenue, SW
Room 314-G
Washington, DC 20201

Dear Ms. Min DeParle:

As you may know, I recently introduced S. 1550, a bill to extend certain Medicare community nursing organization demonstration projects for an additional three years.

While there is significant bi-partisan support in Congress for extending the Community Nursing Organization (CNO) program beyond the current expiration date of December 31, 1999, I am concerned that the uncertainties about the continuation of the demonstration will soon affect day-to-day management and services provided to the Medicare beneficiaries enrolled in the four CNO programs.

It is quite possible that the CNO demonstration projects will not be extended until the final days of the first session of the 106th Congress, in late October or early November. Nonetheless, because of HCFA's proposed phase-down timetable, a significant number of Medicare beneficiaries will have either voluntarily left the program or not have been reassessed at the four sites as part of the phase-down process. Such uncertainty will negatively impact the sites' ability to maintain their current nursing staff, who have developed long-standing relationships with program participants.

Consequently, I am requesting that any notice of phase-down be delayed until the outcome of the legislative deliberations is known. Each CNO site received the enclosed communication from HCFA dated July 19, 1999, which outlines the timetable to be followed in the absence of enactment of legislation to extend the CNO programs. A revised disenrollment date of November 15, 1999, would allow Congress to complete deliberations and reach consensus on extending this important demonstration project.

I appreciate your prompt consideration and response to my concerns about the Medicare Community Nursing Organization demonstration projects.

Sincerely,

Paul D. Wellstone
United States Senator

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FAX COVER SHEET

OFFICE OF LEGISLATION

Number of Pages: 07 Date: 11/2/99

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REMARKS: Preliminary info on

SHMO demos

HEALTH CARE FINANCING ADMINISTRATION
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SOCIAL HEALTH MAINTENANCE ORGANIZATION DEMONSTRATION

HCFA's Social Health Maintenance Organization (Social HMO) demonstration was established by Congress in 1984 to test whether investing in some long-term care benefits for Medicare HMO enrollees could save money through coordinating care and providing services that might prevent more costly medical complications. The Social HMO demonstration has provided standard HMO benefits, such as hospital, physician, skilled nursing home, and home health services, together with limited long-term care benefits to Medicare beneficiaries who voluntarily enroll. In addition, expanded benefits, such as eye glasses and prescription drugs, are available. Social HMOs enroll a cross-section of the elderly living in the community. Social HMOs' long term care services range from community-based care to institutional nursing home care. Long term care services provided include personal care aides, homemakers, medical transportation, adult day health care, respite care, and case management in a community setting.

The components of a social HMO are an integrated service delivery system, a coordinated case management system, an enrollment of a cross-section of the elderly population, provision of a long term care benefit, and a fixed, prepaid capitation payment for each enrolled beneficiary. The authorizing legislation for the demonstration stipulated that this annual capitation for Medicare be 100 percent of the AAPCC, with the additional funds intended to fund long term care services.

In 1985, Social HMO projects became operational at the following four sites:

- Kaiser Permanente Northwest established Medicare Plus II in Portland, Oregon
- Group Health and Ebenezer Society established Seniors Plus in Minneapolis-St. Paul, Minnesota
- Metropolitan Jewish Geriatric Center established Elderplan in Brooklyn, New York
- Senior Health Action Network established SCAN Health Plan in Long Beach, California

However, in January 1995, the Minneapolis site withdrew its participation because it believed the S/HMO was too costly to administer. Currently, over 20,000 Medicare beneficiaries are enrolled in the three remaining demonstration sites.

PERFORMANCE OF SOCIAL HMO I

In 1996, HCFA prepared a status report on the implementation and evaluation of the Social HMO demonstration. This report found that the Social HMO I sites had lower levels of disenrollment than Medicare's risk contract HMOs. Healthy Social HMO enrollees also expressed overall satisfaction with their participation in the program. Case managers were able to effectively manage the long term care benefit, and the provision of formal long term care services through Medicare dollars did not supplant informal caregiving.

The satisfaction of frail Social HMO enrollees was compared to frail fee-for-service enrollees, in areas of access to care, interpersonal relationships with their physician, cost and benefits of care, quality or competence of care, and an overall measure of satisfaction. Frail Social HMO enrollees

were more satisfied than their fee-for-service counterparts in only one of these areas -- costs and benefits of care.

There was no evidence that the Social HMOs were less costly than fee-for-service (although utilization and expenditure data were limited to 1987 and 1988), and no improvements in mortality or active life expectancy were demonstrated.

Sites were evaluated for the period 1985-1989; since that time, their care model has evolved and matured. We plan to re-evaluate the effectiveness of the initial sites, given this maturation.

DEVELOPMENT OF SOCIAL HMO II

In 1990, Congress authorized an extension of the demonstration and established the second generation Social HMO. One purpose of Social HMO II is to test the effects of more closely linking long term care case management services to acute care providers who are more actively intervening and managing the chronic care needs of at-risk and frail enrollees.

In January 1995, HCFA awarded developmental grants to the following six Social HMO II project sites:

- CAC-United HealthCare Plans in Coral Gables, Florida
- Contra Costa Health Plan in Martinez, California
- Fallon Community Health Plan in Worcester, Massachusetts
- Health Plan of Nevada, Inc. in Las Vegas, Nevada
- Richland Memorial Hospital in Columbia, South Carolina
- Rocky Mountain HMO in Grand Junction, Colorado

Nevada implemented the Social HMO II project in November 1996; other sites are expected to become operational in 1997.

SIMILARITIES AND DIFFERENCES

- Social HMO I and II both enroll a cross-section of the elderly population.
- Social HMO I and II offer a limited long term care benefit consisting of similar community-based and institutional services and of a similar dollar value.
- Both Social HMO I and II provide case management. In Social HMO I, this has tended to be management of the long term care benefit, although more recently Social HMO I sites have expanded the role of case managers. In Social HMO II, case managers provide more active case management around common geriatric problems (e.g., depression, polypharmacy, social issues such as a change in family structure), intervene at more points such as hospital discharge, and work more closely with the primary care physician and other professionals.
- In Social HMO II there is a focused physician intervention whereby physicians are trained in the use of practice protocols around common geriatric conditions and problems such as diabetes, COPD, incontinence, and several other conditions as well as their management.

- In Social HMO I, the risk adjustment involves creating a nursing home certifiable (NHC) or frailty factor for frail community-dwelling enrollees, with other non-institutional rate cells adjusted downward. In Social HMO II, the risk-adjustment is a regression-base factor that relies on functional information, presence of certain medical conditions, self-reported health status, and gender to set a rate for each enrollee.

SOCIAL HEALTH MAINTENANCE ORGANIZATION DEMONSTRATION

PHASE I PROJECT DESCRIPTION

BACKGROUND

In accordance with the congressional mandate in section 2355 of P.L. 98-369, this project developed and implemented the concept of a social health maintenance organization (S/HMO) for acute and long term care. Section 4018(b) of P.L. 100-203, the Omnibus Budget Reconciliation Act (OBRA) of 1987, extended the demonstration through 1992, and section 4207(b)(4) of P.L. 101-508, OBRA-90, further extends the demonstration through 1995, and section 5079 of P.L. 103-66 extends the demonstration through 1997.

PROJECT DESIGN

The S/HMO model includes four basic organizational and financing features. A single organizational structure provides a full range of acute and long term care services to voluntarily enrolled Medicare beneficiaries. Beneficiary premium payments vary by site. In addition to the basic Medicare benefits, long term care services include additional nursing home and home health services, homemaker, transportation, drugs, and similar services. Second, a coordinated case management system is used to authorize long term care services for those members who meet specific disability criteria within a fixed limit of about $6,000 - 12,000 per year. Third, the S/HMOs are designed to serve a cross-section of the elderly population, including both the functionally-impaired and unimpaired elderly. Fourth, financing is accomplished through prepaid capitation by pooling funds from Medicare, Medicaid, and member premiums and copayments. The benefits and capitation vary by state and are negotiated independently between the specific site and the respective Medicaid State Agency. The number of dually eligible served by each site has to date remained limited. The initial financing risk was shared by the S/HMOs and HCFA; after 30 months of the demonstration the S/HMO sites assumed full financial risk for service costs.

Monitoring of these sites is performed by the HCFA Office of Research and Demonstrations, the Office of Managed Care, and the regional office.

CURRENT STATUS

The three S/HMO demonstration sites include one HMO that has added a long term care service package (Kaiser Permanente in Portland, Oregon) and two long term care providers that have added acute care services to their service packages (Metropolitan Jewish Geriatric Center, Inc. in Brooklyn, New York and Senior Care Action Network in Long Beach, California). HealthPartners (formerly Group Health in Minneapolis-St.Paul, Minnesota) previously was a participating site. This site discontinued participation on January 1, 1995.

FACT SHEET
Social Health Maintenance Organization (HMO) II Demonstration

Background

The Health Care Financing Administration (HCFA) selected six applicant organizations to participate in the congressionally-mandated Social HMO-II demonstration. The second generation Social HMO will focus on refining the targeting and financing methodologies and benefit design of a Social HMO, with an emphasis on geriatric care and the expansion of the model to special populations.

The Social HMO offers Medicare beneficiaries the opportunity to receive a wide range of services to meet both acute and long term care needs. The Social HMO demonstration includes several unique organizational and financing features, including a fully integrated structure to provide a range of services to enrollees, a coordinated case management system, enrollment of a cross section of the elderly population including the functionally impaired and the well elderly, and financing methodology comprised of prepaid capitation by pooled funds from Medicare, Medicaid, and member premiums.

The purpose of the Social HMO-II demonstration is to refine the targeting and financing methodologies and benefit design of a Social HMO. In addition, the projects should also provide an opportunity to test more geriatrically oriented models of care, as well as to test expansion to special populations, such as beneficiaries living in rural settings and beneficiaries who are dually entitled to Medicare and Medicaid.

Award of the Social HMO II sites was through a competitive process. The following six sites receiving awards were selected from 19 applicant organizations.

- CAC-United Health Plans, Inc., Coral Gables, Florida
- Contra Costa Health Plan, Martinez, California
- Fallon Community Health Plan, Worcester, Massachusetts
- Health Plan of Nevada, Inc., Las Vegas, Nevada
- Richland Memorial Hospital and its consortium, Columbia, South Carolina
- Rocky Mountain HMO, Grand Junction, Colorado

The six sites are currently undergoing a development period funded by a HCFA grant, during which time HCFA's technical assistance contractor, the University of Minnesota and the University of California, San Francisco, will assist the sites in development of the demonstration project. Sites are not projected to begin enrollment until at least July, 1996, and will serve approximately 85,000 Medicare beneficiaries under the demonstration.

Social HMO-I sites have been operational since 1985 and include Medicare Plus II, sponsored by Kaiser Permanente Northwest, Portland, Oregon; SCAN Health Plan, Long Beach California; and Elderplan, Inc., Brooklyn, New York. The participation of the Social HMO-I sites in the Social HMO-II demonstration has not yet been determined.

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Payment Methodology

- In the Request for Proposals for the Social HMO-II sites, an AAPCC-based payment methodology was proposed with risk-adjustment for various limitations in Activities of Daily Living (ADLs). However, at the initial all-site meeting, the participating sites expressed great interest in further refining the payment methodology and including factors other than ADLs in the risk-adjusted payment methodology.

- Liz Mauser from the Office of Research and Demonstrations, Mike Finch from the University of Minnesota, and Lenny Gruenberg from the Long-Term Care Data Institute have further refined the Social HMO-II payment methodology. It was decided that the following variables would be included in the new risk-adjusted payment model:

  - ADLs, such as bathing, dressing, and walking, etc.;
  - IADLs, such as shopping, etc.;
  - self-reported health status (excellent, very good, good, fair, poor);
  - presence of various medical conditions; and
  - sex;

- Using data from the Medicare Current Beneficiary Survey, we have been estimating total cost functions including the variables described above. Approximately, 6 percent of the variance in costs is explained by including a subset of the statistically significant variables in the above list (approximately 3 percent of the variance in cost is explained by an ADL only model).

- We intend to use a regression-based model in which each enrollee will be assigned a rate based on the characteristics that are included in the regression model. Although we will not be using a rate cell approach due to the number of variables in our model, the regression model will capture differences in per capita costs for beneficiaries with a given set of characteristics relative to the average beneficiary. The purpose of designing this new rate methodology is to encourage sites to bring in frailer beneficiaries, while reducing the reimbursement rates for healthier enrollees.

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Enrollment and Eligibility

All beneficiaries currently eligible for Medicare A and B will be eligible to enroll in Social HMO-II. Unlike the first generation Social-HMO, the under 65 will also be included in Social HMO-II. At this point, the only exclusions on enrollment are ESRD beneficiaries and hospice beneficiaries. Under Social HMO-I, the sites were able to pre-screen beneficiaries and queue to limit their number of severely and moderately impaired enrollees to approximately 5 percent of the their total enrollment. Queuing will not be an option for sites under Social HMO-II.

The Social HMO-II sites are planning to rollover large numbers of their current Medicare enrollees into the Social-HMO demonstration. Each site will be expected to also maintain a comparison group. The sites are also planning to market to new enrollees, however we expect the number of new enrollees to be limited.

During the developmental period the six Social HMO-II sites are expected to conduct negotiations with their respective State Medicaid agencies to develop a Medicaid capitation rate for dual eligible beneficiaries. Enrollment of dual eligible beneficiaries was limited in Social HMO-I, primarily because of the difficulty in determining an appropriate capitation rate for Medicaid covered services. Enrollment of dual eligibles is of primary importance under Social HMO-II and we are encouraging the sites to include institutional long-term care in their capitation methodology.

EverCare

- EverCare has a HCFA demonstration project (see attached overview). The demonstration had been scheduled to end December 31, 2000. EverCare also contracts with M+C plans to care for their institutionalized enrollees outside of the demonstration.

- Earlier this year, we informed EverCare that payments under the demonstration would not be subject to risk adjustment in 2000.

- EverCare worked with members of Congress to draft a bill (H.R. 1998) with provisions that include: (1) exempting payment for all institutionalized M+C enrollees who are also in EverCare (and similar programs) from risk adjustment until a risk adjustment method meeting criteria specified in the bill had been developed and (2)overriding the limitation on enrollment and disenrollment opportunities that the BBA mandated beginning in 2002 to allow for continuous enrollment in EverCare type programs.

- EverCare contacted the Secretary and OS (Jane Horvath). OS and HCFA met with EverCare on October 13 to discuss the bill. EverCare's letter to Jane Horvath states that risk adjustment would reduce their payment by 42%. (We believe EverCare has used this figure in other communications, e.g., with Members of Congress.) This figure is a significant overstatement of the impact of risk adjustment, in part because they do have complete data on enrollees, e.g., hospital data prior to enrollment in EverCare. In preparation for this meeting, HCFA computed EverCare's payments (1) under the PIP-DCG risk adjustment method and (2) under the current method but using the updated demographic factors for the institutionalized. These updated factors are lower than the ones now in use. One effect of the BBA was to require continued use of the factors in use in 1997, which are based on data from the mid-1970s. A comparison of the older and updated demographic factors for the institutionalized is attached. **HCFA's computations showed that if the updated demographic factors for the institutionalized could have been used in the demonstration, EverCare payments would be 22% less than they are today. They also showed that EverCare's demonstration payments under PIP-DCG risk adjustment method would be 15% less than they are today. Thus, EverCare's payments under risk adjustment are actually higher than they would be if use of the more accurate factors for the institutionalized had not been precluded by the BBA, i.e., a 15% revenue loss under risk adjustment compared to a 22% loss under the demographic system.**

- A summary of the comments on the bill and EverCare payment levels, prepared after the October 13 meeting, is attached.

- HCFA sent a letter on October 22 to EverCare informing it of HCFA's decision to extend the demonstration, which had been scheduled to end in December 2000, through December 2001. The letter also informed EverCare that the exemption from risk adjustment will apply in 2001.

- The House "giveback" bill includes language to allow continuous enrollment for all institutionalized M+C enrollees, not just those in EverCare and similar "specialized programs." We understand that the Senate "giveback" bill may include an enrollment provision but may limit it to those in "specialized programs." The Senate bill also includes language extending the demonstration 2 years beyond the current termination date, i.e., through 2003, exempting the demonstration from risk adjustment in 2000, and exempting demonstration enrollees from the lock-in provisions for the duration of the demonstration. The Senate language requires MedPAC to do a report on the criteria for a risk adjustment methodology in the bill. Neither bill excludes EverCare enrollees outside the demonstration from risk adjustment.

Social HMO Demonstration

- The BBA extended the demonstration through December 2000 and mandated a report on transition of SHMOs to the M+C program by January 1, 1999. This timeframe would have given the Congress 2 years to decide how to transition SHMOs into M+C. HCFA advised congressional staff long before the transition report was due that it would not be submitted on or near the due date. The report is now in HCFA clearance and HCFA hopes the report will be cleared through all necessary levels and submitted to Congress in the Spring of 2000.

- Preliminary analysis findings suggested that while Medicare pays SHMOs more than it would pay them if they were M+C plans, their enrollment in most plans is similar to that of local risk plans.

- The Senate and House "giveback" bills both extend the termination date of the demonstration. The Senate extends it to 12 months after the Secretary issues the transition report; the House Ways and Means language extends it to 18 months after the report is submitted and requires MedPAC to issue recommendation 6 months after the report is submitted.

- A long-standing contentious issue in the SHMO demonstration is a per site cap on enrollment. Prior to BBA, the statute said HCFA could not set a cap below 12,000 per site. At the time of the BBA, enrollment exceeded that level at one site; other sites were not close to that level of enrollment. The BBA revised the 12,000 figure to 36,000. In response, HCFA set the limit at 36,000 per site. Since 2 of the 4 sites have significantly less than 12,000 enrollees, a 36,000 cap is not an issue for them. However, 2 sites, SCAN in Los Angeles and Health Plan of Nevada, are at or near the cap. SCAN has argued that HCFA should raise the cap because the BBA cost estimate assumed 9 sites with 36,000 enrollees and total SHMO demonstration enrollment is substantially less.

- The House Ways and Means giveback bill modifies the cap language, and mandates a minimum aggregate cap of 364,000 (9 x 36,000). HCFA believes the 36,000 per site cap language in the BBA should be maintained because the higher payments given to SHMOs provide them with a competitive advantage relative to M+C plans. As long as the SHMOs are operating under a demonstration, it is desirable to limit the impact of that competitive advantage.

CNO Demonstration

- The BBA extended the CNO demonstration 2 years, through 1999. It mandated a final report by June 30, 1999.

- We have just received the independent evaluation of the project, which we can make public. We will attach this evaluation to a letter report to Congress that summarizes the findings in the independent evaluation. The executive summary, table of contents, and selections from the full report (from the results, discussion, and conclusions text in sections 5, 6, 7, and 8) are attached.

- The report indicates that while HCFA pays CNOs more ($25-$75 per person per month, depending on the site) there are not statistically significant differences between control and treatment groups with regard to health and functional status as measured by the physical component summary (PCS) of the SF-36 nor are there any statistically significant effect of CNO enrollment on health behaviors such as change in smoking habits, seat belt use, influenza vaccination or exercise habits. There was a small but statistically significant increase as measured by the mental component summary (MCS) of the SF-36 when data from all 4 sites was combined. The evaluation noted that enrollees might have found the contact with the CNOs reassuring, producing the small increment in the MCS.

- The ANA has prepared its own analysis, which highlights selected findings that reflect more favorably on the results of the demonstration. Comments on some of the methodological issues with their presentation are attached.

Attachments mentioned in cover document on EverCare in this package. There are 4 in all. They were all faxed to Chris Jennings on November 2.

EVERCARE
Managing Medical Care for Nursing Home Residents

Under a cooperative agreement with the Health Care Financing Administration (HCFA), the EverCare Demonstration utilizes geriatric nurse practitioners as care providers and care managers in nursing facilities. EverCare determines the most appropriate interventions for permanent long-term facility residents who are enrolled in the program and assures follow-up care. The geriatric nurse practitioner authorizes hospitalizations, schedules clinic and outpatient appointments, and consults with the nursing staffs at each facility to assist in problem solving and in coordinating the most effective and efficient care pattern for each enrolled resident. By increasing the availability and intensity of medical services in the nursing home setting, EverCare believes that unnecessary hospitalizations can be avoided and, therefore, care will be less costly.

A physician incentive plan provides a higher payment rate for a nursing home visit than for an office visit and a less than favorable rate for services furnished in the physician's office or in other settings. To facilitate communication between caregivers, the patient, and the patient's family, EverCare pays physicians for attending family conferences. Except for short periods following hospitalization, which may be covered by Medicare (depending on level of need, benefit period, etc.), the room and board and custodial long-term care costs of the nursing home stay are paid by the enrollee and/or the Medicaid program.

The demonstration is expected to reflect lower total medical costs for enrolled nursing home residents, comparable or improved quality of care and health outcomes for these residents, and increased resident and family satisfaction. EverCare has developed specific practice guidelines and care management protocols for the demonstration so that this model can be successfully replicated.

The demonstration is being conducted in six sites: Atlanta, Baltimore, Boston, Denver, Phoenix, and Tampa. No additional sites are anticipated. Each of the sites will operate for 5 years. The demonstration is scheduled to December 31, 2000.

The demonstration sites were initially paid a capitation rate equivalent to the institutional rate category under the Adjusted Average Per Capital Cost (AAPCC) paid to Medicare TEFRA HMO risk contractors for enrolles residing in institutional settings. The capitation rate was 100 percent of the cost for the first year of operation at each site, 95 percent of the AAPCC for the second year of operation, and then 93 percent of the AAPCC for the third through fifth years of the project. Starting January 1998, the sites were paid 93 percent of the Medicare + Choice rates instead of the AAPCC rates.

An evaluation contract was awarded to the Division of Health Services Research and Policy, School of Public Health, University of Minnesota. The Principal Investigator is Robert L. Kane, M.D.

Site enrollment as of September 1998

Atlanta	1402
Baltimore	1901
Boston	2087
Denver	513
Phoenix	266
Tampa	1090

Under the proposed PIP/DCG payment model, HCFA has determined that the EverCare demonstration sites will be underpaid by approximately 15%. A letter was recently sent to EverCare indicating that HCFA is willing to extend the demonstration for an additional year and defer the imposition of the proposed risk-adjusted payment methodology until January 2001.

~~Attachment 1~~

Provisions of H.R. 1998

1. The risk adjustment methodology mandated in the BBA and used to determine payment for Medicare+Choice (M+C) enrollees can not be used to determine payment for a frail elderly M+C beneficiary (as defined in 4 below) enrolled in a M+C plan under a specialized program (described in 3 below) until the Secretary certifies that a "comprehensive risk adjustment methodology" that takes account of the factors (described in 2) below is being fully implemented.

2. The Secretary shall develop a payment methodology for frail elderly M+C enrollees "under a specialized program for the frail elderly" (as defined in 3 below). It must account for the "prevalence, mix, and severity of chronic conditions among such beneficiaries" and "include medical diagnostic factors from all provider setting (including hospital and nursing facility settings)." It must include functional indicators of health status and other appropriate factors.

3. A specialized program for the frail elderly is a program determined by the Secretary to:

   - be offered "as a distinct part of a M+C plan;
   - be primarily enrolling frail elderly M+C beneficiaries; and
   - have a clinical delivery system specifically designed to serve the special needs of the frail elderly and coordinate care through --
     - the use of a team that includes a physician, a nurse practitioner or geriatric care manager, and has members who specialize in the care and management of the frail elderly beneficiaries; and
     - through the provision of primary care services to such beneficiaries by means of such a team at the nursing facility.

4. A frail elderly M+C beneficiary is someone who is *eligible* to enroll in an M+C plan and who is (1) residing in a SNF or a nursing facility for "an indefinite period and without any intention of residing outside the facility" and (2) has a condition the severity of which the Secretary determines, under guidelines, "makes the individual frail."

5. There will be continuous open enrollment for individuals described in (4) above who is "seeking to enroll in a M+C plan under a specialized program for the frail elderly" (as defined in 3 above).

6. The Secretary must develop and implement a program to measure the quality for care provided in specialized programs for the frail elderly that reflects the unique needs of frail elderly M+C beneficiaries not later than July 1, 2000.

~~Attachment 2~~

1998 Demographic Cost Factors for the Institutionalized Aged

	OLD FACTOR* (still in use because of BBA)	UPDATED FACTOR* (never implemented because of BBA)	CURRENT PERCENT OVERPAYMENT
Part A - Female			
65-69	0.55	1.45	164%
70-74	0.75	1.8	140%
75-79	1.05	2.1	100%
80-84	1.3	2.1	62%
85+	1.6	2.1	31%
Part B - Female			
65-69	1.05	1.5	43%
70-74	1.3	1.65	27%
75-79	1.45	1.65	14%
80-84	1.6	1.65	3%
85+	1.65	1.65	0%
Part A - Male			
65-69	0.8	1.75	119%
70-74	1.05	2.25	114%
75-79	1.4	2.25	61%
80-84	1.75	2.25	29%
85+	2.15	2.25	5%
Part B - Male			
65-69	1.1	1.6	45%
70-74	1.4	1.8	29%
75-79	1.65	1.95	18%
80-84	1.8	1.95	8%
85+	1.85	1.95	5%

* The demographic factors for the institutionalized for 1997 and prior years were based on Current Medicare Survey data from the mid-1970s. The survey was discontinued for funding reasons and data were not available to

update the factors for the institutionalized until 1997. The 45-day notice for the 1998 rates, published July 24, 1997, provided revised institutional factors, many of which were lower than the factors based on 20-year-old data. With enactment of the BBA, after the publications of the 45-day notice, HCFA concluded that under the BBA, using the updated factors for the institutionalized would not be permissible.

Comments on Provisions Related to EverCare's Medicare Demonstration and Non-Demonstration Activities

1. **If the EverCare demonstration had been paid in January 1999 using updated institutionalized factors, payments would have been about *22% less* than under the institutionalized factors now in use.** The demographic-based institutional factors now in use are based on survey data from the mid-1970s. In 1997, prior to enactment of the BBA, HCFA published proposed institutionalized factors for use in 1998 based on up-to-date survey data on Medicare spending for beneficiaries in institutions. (Data had not been available for many years because the survey had been discontinued for funding reasons.) These updated factors were lower, indicating overpayments for institutionalized beneficiaries. Subsequently, pursuant to the M+C payment provisions of the BBA, HCFA had to rescind the updated institutionalized factors and, instead, continued to use factors based on data that was twenty years out of date.

2. **If the EverCare demonstration had been paid in January 1999 using the PIP-DCG method, its payments would have been *15% less* than they were under the payment method now being used.** This is within the range of PIP-DCG impacts for Medicare+Choice plans and it is a substantially smaller impact than the 42% reduction EverCare has reported. Selectively protecting EverCare from risk adjustment while other plans are facing similar impacts established a bad precedent.

3. **When points 1 and 2 above are taken together, they show that EverCare's payments under risk adjustment would be higher than under the current method with the more accurate, updated institutionalized factors. That is, instead of a 22% reduction there would be a 15% reduction.**

4. **Payments to the EverCare demonstration and to M+C plans that contract with EverCare should not be unfairly affected by the risk adjustment system because the system does not pay more or less for avoiding the types of hospital admissions that EverCare focuses on preventing.** In the current demographic system, all payment amounts are based on demographic factors, e.g., age, sex, welfare and institutional status. Under the PIP-DCG risk adjustment method, most payment amounts continue to be based on demographic factors, but a portion (about 20% of spending on fee-for-service beneficiaries) is based on prior year hospital admissions. However, not all admissions generate higher payments. Only hospital admissions that are unavoidable and that are also predictive of future medical costs receive an additional payment. The costs of other admissions, i.e., those that are avoidable or that are not predictive of future costs, are factored into payments based on demographic factors. Thus, when enrollees are admitted for avoidable or non-predictive hospitalizations, there is no additional payment to a M+C plan. Many of the types of types of preventable hospitalizations EverCare works to avoid, e.g., bacterial pneumonia, infections, therefore would not generate an additional payment under the PIP-DCG risk adjustment method. Thus, EverCare would not be disadvantaged since spending for those admissions is factored in to payments

based on demographic factors.

5.. **Enrollees in EverCare demonstration sites do have a substantial number of hospitalizations so they would receive the higher payments associated with those hospitalizations that are considered unavoidable and predictive of future costs.** HCFA assumes that EverCare is not avoiding hospitalizations of the type that generate higher payments under the PIP-DCG method; for example, those associated with serious conditions such as stroke.

6. **The average institutional enrollment for M+C plans is about 1.5% and only one plan (that is not an EverCare demonstration) has over 5% institutional enrollees. Thus, the impact of the 10% phase-in of risk adjustment would be modest for most plans. And the portion of plan payments for the institutionalized under the old system (i.e., 90% of payments in 2000) would continue to benefit for the excessive payments generated by using outdated demographic factors (see point 2.)**

   o In addition, if the phase-in of the PIP-DCG part of risk adjustment is slowed significantly, for example, as proposed by both the House and the Senate, then the impact on payments for the institutionalized will be correspondingly reduced.

7. **In a preliminary finding from the data being gathered for an evaluation of the EverCare demonstration, it appears that EverCare demonstration enrollees have lower than average scores on 3 ADLs (activities of daily living) relative to other Medicare institutionalized beneficiaries.** This may point to EverCare enrollees being somewhat more highly functional than the comparison group of beneficiaries residing in nursing homes. It also suggests that a special "frailty" or functional status adjuster developed for the institutionalized population, such as the one called for in HR 1998, might not result in favorable payments to EverCare relative to other programs for the institutionalized.

Attachments mentioned in cover document on CNO demonstration in this package. The 2-pager marked "draft" and titled "Executive Summary" was faxed to Chris Jennings on November 2. The other materials were not. They are:

- The Table of Contents and pages from Chapters 5, 6, 7, and 8 on "Results", "Discussion", and "Conclusions". These are from the final evaluation, which we will provide to interested congressional staff.

- Comments on the ANA's material on CNO results.

(Also, note that while the Executive Summary faxed on November 2 is marked "draft", we understand it is the version in the final report. However, we do not yet have the final report.)

DRAFT

Executive Summary

The Community Nursing Organization (CNO) Demonstration was implemented on January 1 1994. It was designed to test a system of capitated payment for specified community nursing services covered by Medicare. The demonstration operated in four sites: Carle Clinic (Urbana IL), Carondelet Health Care (Tucson AZ), the Living at Home/Block Nurse Program (Minneapolis MN), and the Visiting Nurse Service (New York NY). The CNOs are at full financial risk for all care provided under the service package.

The demonstration was implemented as a social experiment. Applicants were informed during the introductory information session that there was a one-third probability that they would not be allowed to enroll in the CNO but would instead be assigned to a control group. Members of the control group received care in whatever way they would have had there been no demonstration. All beneficiaries who wished to apply to a CNO after being informed of the requirements of participation and the of the process of randomization were interviewed prior to randomization. The interview elicited information about the applicant's background, health and functional status, behaviors, recent use of health care, and overall satisfaction with care. The interview was repeated by telephone 15 months after the time of randomization and again 27 and 39 months after randomization.

Interim Evaluation Reports were issued in May 1996 and April 1998. They described program outcomes at earlier stages of the demonstration. Those reports found no statistically significant differences in health and functioning between the treatment and control groups. Medicare expenditures for the treatment group, however, were found to be uniformly greater than for the control group.

This final report analyzes follow-up interview data and Medicare claims and payment information for 10,632 beneficiaries randomized between January 1994 and September 1995. Utilization and expenditure data were collected for up to 42 months following randomization..

Effects of the CNO on individual outcomes were assessed using 27-month and 39-month changes in well-known measures of health and functional status, including the physical component summary (PCS) and mental component summary (MCS) of the SF-36, Activities of Daily Living, and questions regarding satisfaction with care. No statistically significant differences in the PCS between the treatment and the control groups emerged, whether at 27 months or 39 months after random assignment. Over the longest possible follow-up period (39 months), assignment to the treatment group was associated with a small but statistically significant increase in the MCS when data from all four CNO sites were combined. It is possible that the periodic assessments and the continuing availability of contact with the CNOs, by telephone or at some other location, is a source of reassurance for many enrollees, producing a small increment to the MCS. At one site, VNS, functional ability, as measured by a combined ADL/IADL scale, appeared to be enhanced by the CNO, although it was impossible to state definitively that this was a direct result of the intervention.

There were no statistically significant effects of CNO enrollment on measured health behaviors. The proportions of individuals who changed smoking habits, wore automobile seat belts, received an annual vaccination for influenza, and who exercised each week were the same for the treatment and control groups. There was a tendency to members of the treatment group to be somewhat more knowledgeable about their blood pressure, serum cholesterol and medications, though these differences were not always statistically significant.

In all four sites, Medicare payments for the treatment group (including capitation payments) exceeded those for the control group. Virtually all of the discrepancy was accounted for by an excess of capitation and case management payments over the expected payments for CNO-type services provided to the control group. Demonstration rates, computed using the *average* utilization of CNO-type services, were probably set at a level above the expected Medicare expenditure for the typically healthy individuals who applied to the CNO. To achieve budget neutrality, CNO capitation payments would need to decline by an average of $25 to $75 per person per month.

evaluation - from cochlear for
(ISOPP)
we will do a white report
- shorter, more concise -
only famous people who enrolled
early years - for 3 yrs

Set up low - CNOS would not play
then raised so they would participate

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this survey. The scales were recalculated using followup data secured during subsequent interviews conducted 15 months, 27 months, and 39 months after randomization. Dates of death were drawn from the Medicare Enrollment Database. All measures were computed for individuals randomized to treatment or control groups prior to October 1 1995.

5.5 Results

SF-36 Scales

Table 5.2 displays the change in the SF-36 Physical Score, measured from baseline to successive follow-up interviews conducted 15, 27, and 39 months later. Most figures in the table are negative, indicating that physical scores declined on average over the intervening periods. Because all interviewing ceased in December 1997, 39-month followup interviews were conducted only for those randomized before September 1994; 27-month interviews were conducted only with for those randomized before September 1995. Consequently the sample sizes decline for later interviews. The table shows little association between assignment to the treatment group and mean improvement or decline at subsequent points. In the three instances in which treatment/control differences were significant at the 0.10 level, all showed superior outcomes for the treatment group.[1] Nevertheless, thirty-nine months after random assignment, no differences between the treatment and control group are statistically significant, even at the ten percent level.

Table 5.2: Mean Change in SF-36 Physical Health Summary Between Baseline and Three Follow-up Interview Points for Treatment and Control Groups, by CNO Site

	Months after randomization						
	15 months		27 months			39 months	
	T	C	T	C		T	C
Carle	-0.88	-0.73	-1.85	-1.63		-2.67	-2.26
n	1,622	746	1,166	556		988	448
Carondelet	-0.42	-0.94	-0.60	-0.31		-2.09	-1.84
n	1,731	794	561	252		370	166
LAH	0.10	0.20	0.22	-0.75	*	-1.12	-1.25
n	1,372	616	591	300		422	194
VNS	-0.38	-0.54	-0.99	-2.11	*	-2.35	-2.01
n	708	241	439	141		326	103

** Statistically significant at p=0.05 level using t test.
* Statistically significant at p=0.10 level using t test.

Note: Changes are computed as followup value - baseline value. Positive numbers indicate improvement in score; negative numbers indicate decline.

Sources: Baseline and followup assessments of randomized CNO applicants.

1 Nearly identical results were obtained when tests were conducted using the two-sample Wilcoxen rank-sum test rather than the t-test. Using the Wilcoxen test, the 15-month difference at Carondelet is not significant at the 0.10 level. The 27-month difference at LAH is significant at the 0.05 level. Other results are unchanged.

Change scores for the mental health summary (MCS) over the same three followup periods are shown in Table 5.3. Although the change scores are consistently higher for the treatment group, the differences are never significant at the 0.05 level and only once are significant at the 0.10 level.

Least-squares regression estimates of the regression of 39-month change scores for the PCS and MCS scales are shown in Table 5.4. The absence of a statistically significant effect of assignment to the treatment group on change in physical health as measured by the PCS is unsurprising in light of the earlier results seen in Table 5.2. Nor perhaps is the detection of a statistically *significant* effect of treatment on mental health. The emergence of this effect is clearly the result of aggregation across sites and the consequent increase in total sample size.

Table 5.3: Changes in Sf-36 Mental Health Summary (Mcs) Between Baseline and Three Followup Interview Points for Treatment and Control Groups, by CNO Site

	Months after randomization						
	15 months		27 months		39 months		
	T	C	T	C	T	C	
Carle	0.08	-0.21	(0.11)	0.08	-0.52	-1.11	
n	1,622	746	1,166	556	988	448	
Carondelet	0.53	0.29	0.56	-0.56	0.43	-0.63	
n	1,731	794	561	252	370	166	
LAH	0.33	-0.03	-0.48	-0.83	-0.47	-1.60	*
n	1,372	616	591	300	422	194	
VNS	0.57	-0.33	0.02	-1.54	-1.33	-1.65	
n	708	241	439	141	326	103	

** Statistically significant at p=0.05 level.
* Statistically significant at p=0.10 level.

Note: Changes are computed as followup value - baseline value. Positive numbers indicate improvement in score; negative numbers indicate decline.

Sources: Baseline and followup assessments of randomized CNO applicants.

Table 5.4: OLS Regression Estimates for 39-Month Change in PCS and MCS Scores

	PCS		MCS	
	Coefficient	Std error	Coefficient	Std error
Intercept	-2.84	0.35	-1.01	0.30
TREAT	-0.26	0.36	0.77	0.32
Carondelet	0.64	0.46	0.75	0.40
LAH	1.56	0.44	-0.15	0.38
VNS	0.28	0.50	-0.72	0.44
R2	0.01		0.01	
n	3017		3017	

Sources: Regression analysis of baseline and followup survey data.

The ADL/IADL Scale

The effects of the CNO on functional ability as measured by changes in the ADL/IADL scale are shown in Table 5.5. In three of the four sites, there is virtually no difference between treatment and control groups in ADL/IADL change over time. None of the observed differences for these sites is significant even at the 0.20 level. For VNS, however, the picture is quite different. Not only is the mean level of decline much greater than in the other sites, it is also substantially greater for the control than for the treatment group. This treatment/control difference is statistically significant at the 0.05 level for the 27-month and 39-month followup periods.

Table 5.5: Mean Change in Adl/iadl Total Score Between Baseline and Three Followup Interview Points for Treatment and Control Groups, by CNO Site

	Months after randomization					
	15 months		27 months		39 months	
	T	C	T	C	T	C
Carle	0.01	0.06	-0.13	-0.06	-0.34	-0.31
n	1,735	792	1,252	591	1,058	477
Carondelet	-0.07	-0.12	-0.20	-0.24	-0.48	-0.59
n	1,885	877	628	282	399	178
LAH	-0.04	-0.03	-0.24	-0.26	-0.50	-0.50
n	1,490	671	652	329	466	212
VNS	-0.15	-0.43 *	-0.24	-0.67 **	-0.79	-1.39 **
n	831	311	499	171	381	140

** Statistically significant at p=0.05 level using t test.
* Statistically significant at p=0.10 level using t test.

Note: Changes are computed as followup value - baseline value. Positive numbers indicate improvement in score; negative numbers indicate decline.

Sources: Baseline and followup assessments of randomized CNO applicants.

Mortality

The incidence of mortality among the treatment and control groups for each of the CNO sites is shown in Table 5.6. Mortality was similar, in general, for the treatment and control groups. The clear exception was at VNS, where deaths per person-month were 22 percent higher among the treatment group, although this difference was not statistically significant.[2] As we shall see, the pattern of mortality across the four CNO sites creates a difficulty in interpreting the ADL/IADL results presented above.

2 The confidence interval for the rate ratio (RR) is computed as $EXP\{\ln(\hat{RR} \pm 1.96\,SE)\}$ where $\hat{RR}$ is the estimated rate ratio and SE is its standard error. See Rothman (1986) which also provides the formula for the pooled Mantel-Haenszel estimate.

Table 5.6: Mortality in the Four CNO Sites

	Treatment	Control
Carle		
Deaths	145	76
Person-months at risk	75,479	35,578
Deaths per 10^3 person months	1.92	2.14
Carondelet		
Deaths	247	136
Person-months at risk	75,153	38,032
Deaths per 10^3 person months	3.29	3.58
Living at Home		
Deaths	164	81
Person-months at risk	58,506	28,899
Deaths per 10^3 person months	2.80	2.80
VNS		
Deaths	172	71
Person-months at risk	40,951	20,689
Deaths per 10^3 person months	4.20	3.43

Rate ratios (treatment/control) for beneficiary mortality

	Rate Ratio	Standard error	95%Confidence interval	
Carle	0.899	0.142	(0.681,	1.187)
Carondelet	0.919	0.107	(0.746,	1.133)
Living at Home	1.000	0.136	(0.766,	1.305)
VNS	1.224	0.141	(0.928,	1.614)
Pooled (Mantel-Haenszel)	0.992	0.064	(0.875,	1.125)

Sources: Medicare Enrollment Database

5.6 Discussion

Three years after random assignment, the physical health of Medicare beneficiaries assigned to the treatment (CNO) group, as measured by the physical component summary (PCS) of the SF-36 was not different from that of the control group. There is no reason to believe that nurse case management, the practice of periodic in-person assessment, or other interventions carried out by the CNOs led to any improvement in physical health. Nor is there any evidence that physical health of the treatment group deteriorated relative to that of the control group.

Over the same period, assignment to the treatment group was associated with consistent and statistically significant improvements in mental health, as measured by the mental component summary (MCS) of the SF-36. The overall decline in mental health scores for both treatment and control groups combined was quite small, averaging about one point, or two percent of the typical baseline score of 50 or so. Hence the treatment effect, equal to about three-quarters of a point, may not be clinically meaningful. Nevertheless, the issue bears further investigation. It is possible that the periodic assessments and the continuing availability of contact with the CNOs, by telephone or at some other location, is a source of reassurance for many enrollees, producing a small increment to the MCS.

The issue of ADL/IADL performance is the most difficult matter to address here. In three of the four sites — Carle, Carondelet, and LAH — changes over time in ADL/IADL performance were practically identical for the treatment and control groups. At VNS, however, a marked and statistically significant positive effect of treatment was identified. A straightforward conjecture is that something about the activities carried out by VNS produced a beneficial effect on functional independence or that the population applying to the CNO in New York was especially well-suited to benefit from the CNO. The pattern of mortality among treatment and control groups across the four sites suggests a second hypothesis. Recall from Table 5.6 that mortality for the treatment group at VNS was some 22 percent higher than for the control group. Because this difference was not statistically significant, one cannot conclude that a population of individuals enrolled in CNO at VNS would experience higher mortality than would an identical group who was not enrolled. But the higher mortality *in the sample* raises the possibility of so-called informative censoring.

If the death rate is the same for treatment and control groups or if the probability of death is unrelated to outcome measures of interest, then loss of followup data (censoring) due to death will not create analytic difficulties. However, if the death rate is higher for one group and if individuals who die, would, had they lived, have exhibited greater-than-average declines in ADL/IADL performance, then treatment/control differences in outcome measures may be biased despite random assignment. In this instance, it is reasonable at least, to suggest that the observed decline in ADL/IADL scores for the treatment group at VNS is not as great as it would have been if the death rate had been the same for the treatment and control group. It is possible, then, that the observed positive CNO effect at VNS is an artifact of higher mortality in the treatment sample. Whether the observed result is genuine or the result of censoring cannot be ascertained at this point.

Broadly speaking, then, our conclusions regarding the effect of the CNO on individual outcomes echo those of the authors cited in the introduction to this chapter. There is no solid evidence, positive or negative, of an effect of the CNO on physical health, measured by the PCS scale or by observed mor-

tality. At one site, VNS, functional ability, as measured by a combined ADL/IADL scale, appeared to be enhanced by the CNO, although it was impossible to state definitively that this was a direct result of the intervention. Finally, there was a small but consistent beneficial effect of the CNO on mental health. This latter effect is believed to be genuine because of its uniformity across all four CNO sites.

5.7 References

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treatment effect if one exists. However, if an effect is detected, it cannot stated with confidence whether it was a result of the treatment or of selection bias.

6.5 Results

Preventive Care and Health Promotion Activities

Each CNO had its own strategy in disseminating information on health promotional activities and instructing their enrollees in preventive care. Below is an overview of each sites' methods:

Carle

In addition to encouraging preventive care and providing health information at the time of assessments, Carle implemented monthly and quarterly newsletters which featured health education topics and activities available in the community. An annual survey was sent to enrollees to determine which education topics most interested them and to help in planning future health promotion events. In addition, the CNO worked with local colleges and community organizations to hold an annual Health Fair and several health education sessions on arthritis, hospice care, managing chronic pain, and physician/patient relationships.

Carondelet

Carondelet CNO, facing stiff competition from local HMOs, developed a wide range of health promotion activities as one means of differentiating itself from look-alike competitors. The CNO's Health Education Coordinator, along with CNO nurses, helped organize several monthly presentations focusing on topics including exercising safely, healthy eating, and stress management. Special classes, seminars with guest speakers, and social events were held throughout each year. Videos were made of the presentations so that housebound enrollees could benefit as well. Each year, health screenings were held at various community sites. In addition, a monthly newsletter and an annual survey of health education topics of interest were sent out to all enrollees. Volunteers assisted in the organization of these activities.

LAH

At the LAH/BNP CNO, each subsite organized several monthly educational and social events, inviting guest speakers and featuring topics such as T'ai Chi, eye care, and personal safety. Community coordinators organized walking clubs and support groups for weight loss and other concerns. These activities were very popular because they became opportunities for enrollees to socialize with other seniors. Additionally, several health screenings and flu clinics were held throughout the year. As at the other sites, an annual survey was sent to enrollees about which health education topics most interested them. A quarterly newsletter tried to convey the impression of a social, club-like atmosphere at the CNO, in order to provide appealing incentives for low-risk seniors to remain involved in the organization. Volunteers played a major role in organizing and implementing these activities.

VNSNY

As noted in the 1995 Annual Report (Teitelbaum & Thomas), VNSNY enrollees were less interested in prevention, health promotion, and wellness when compared to the enrollees at the other sites. Still, VNSNY CNO nurses attempted to reach out to enrollees by giving frequent

presentations at community sites on topics ranging from memory loss to insomnia. Flu clinics were held in the fall of each year. An annual survey elicited enrollee input on health topics of interest, and the monthly newsletter listed health and wellness activities sponsored by local organizations as well as the CNO nurses' presentations.

Preventive health care behaviors of the treatment and control groups were surveyed at the 27 and 39 month follow-up interviews. Behaviors analyzed in this chapter are:

- The amount of information applicants recalled receiving on nutrition and eating healthier,
- The reported change in applicants' smoking habits,
- The amount of time applicants wear seat belts,
- The receipt of a flu shot in the past year, and
- The amount of exercise done each week.

As seen in Table 6.1, the proportion of individuals who reported receiving information on nutrition and healthier eating is significantly higher for the treatment group than the control group at both the 27 and the 39 month survey. None of the other available survey items yielded significant differences between treatment and control groups. In the cases of smoking behavior and receipt of a flu shot, the treatment group showed slight gains over time; still, none of these differences were significant, even at the 0.15 level.

Treatment and control groups were surveyed regarding their health knowledge at the 27 and 39 month follow-up interviews in terms of:

- The applicants' knowledge of their own blood pressure,
- The applicants' knowledge of their own cholesterol level,
- The applicants' overall assessment of 'health symptoms and conditions,' and
- The applicants' assessment of their understanding of medication prescribed to them.

The results of this analysis are detailed in Table 6.2. The most dramatic of the differences between the control and the treatment group is in the knowledge of blood pressure. A much greater proportion of respondents in the CNO treatment group than in the control group said they knew their blood pressure at both 27 and 39 month follow-up surveys. The proportion of treatment group members reporting 1) knowledge of cholesterol levels, 2) their health conditions and symptoms, and 3) an understanding of medications prescribed are slightly higher than the proportion of the control group, but none of these results are significantly different.

Satisfaction

Individual satisfaction with the health care received from providers was gauged by an examination of respondent agreement with several statements about their care at the 27 and 39 month assessment. The statements concern seven dimensions of satisfaction with health care and providers:

- Time spent by providers,
- Satisfaction with care received from home nursing care,
- Opportunity to participate in decision making,
- Courtesy and respect shown by providers,
- Satisfaction with care and assistance from nurses,
- Overall satisfaction with health care services, and
- Confidence that needed care is available.

CNO applicants' responses to these survey statements concerning the perceived quality of health care and health care providers are shown in Table 6.3 and 6.4. While it is clear that the opinions elicited 27 months after random assignment show slight increases in satisfaction for the treatment groups, none of the difference are statistically significant. After 39 months of enrollment, there is the same general trend where the proportion of applicants in the treatment group demonstrate higher satisfaction levels than the control group. Differences in satisfaction with home health care and with the opportunity to participate in health care decisions between the treatment and control group are statistically significant. These results must be interpreted with caution; while it is tempting to state that the increasing levels of satisfaction for the treatment group is due to a longer exposure to CNO services, we cannot be sure that these results are caused by eliminating the disenrollees, who may be less interested in health education.

6.6 Discussion

Twenty-seven months after random assignment, there is no firm evidence to suggest that health education or satisfaction outcomes associated with the CNO are superior to those experienced by the control group. This result, while perhaps discouraging for the CNOs, was not surprising. The primary interventions employed by the CNO, case management and periodic assessment, have not been shown to improve outcomes in the general elderly population. As we have seen, CNO applicants are probably healthier than most Medicare beneficiaries. Few interventions of any sort have been found to markedly improve the health outcomes for elderly individuals. To our knowledge organizational interventions not targeted to specific groups of individuals have not been shown to enhance outcomes for the over-65 population.

Individuals enrolled at the CNO sites 39 months following their assignment to the treatment group reported significantly greater satisfaction on two measures: nursing care and participation in decisions regarding their health care. These changes may be attributable to CNO performance, however, those that were not satisfied may have disenrolled by the 39 month follow up, skewing the data.

Managed care plans in general, whether Medicare risk plans or private HMOs, are not and have never been judged by their ability to produce outcomes that are superior to those observed under fee-for-service care. The standard for care has been to produce outcomes that are *not* demonstrably worse than generated under fee-for-service. Analyses including the final follow-up data, almost three years after enrollment, prove that CNOs have met this standard.

this result and the fact that no other results in the table were significant, we conclude that enrollment in the CNO did not have systematic effects on utilization of non-CNO services, and that the single observed lower mean utilization rate of CNO enrollees relative to the control group was likely to be the result of nonrandom selection.

7.5 Conclusion

This chapter analyzed data from three different sources to determine whether enrollment in the CNO resulted in changes in patterns of utilization of health services. If CNO enrollment had improved health status, we would have expected utilization of both CNO-covered and non-CNO services to be less for the treatment groups than for the control groups. If the sites engaged in cost shifting to maximize their net revenues, we would have expected higher non-CNO utilization for the treatment groups. None of these patterns were observed in the data.

While survey responses indicated that members of the treatment group were more likely to receive a variety of services than were members of the control group, the survey data also revealed that the amounts of service received tended to be less. Medicare claims data and records kept by the sites showed that overall average levels of utilization for a wide variety of services were the same for treatment and control groups. This fundamental finding was not affected by including variables to adjust for differences in individual health risk.

Many of the analyses in this chapter also were conducted under a more restrictive definition of the treatment group, excluding those person-months when the beneficiary was not actually enrolled in the CNO. While this definition resulted in several significant differences between treatment and control groups, most disappeared when the analysis was risk-adjusted, suggesting that they were the products of selection bias.

Finally, timesheet and enrollment records indicated that expected efficiency gains from substitution of telephone for in-person contact were not realized. On the contrary, hours per enrollee were relatively constant after enrollment stabilized about 15 months after start-up, and the sites continued to commit growing resources to home visits years later.

The data, therefore, do not support the hypothesis that enrollment in the CNO had appreciable effects on the utilization of health services. While it is likely that some effects did occur, they were too small to be detectable.

Threats to the Analysis

The analyses undertaken in this chapter are confronted by two problems that cannot be wholly resolved. The first of these is the appropriate treatment of individuals who are randomized to the treatment group but who either fail to enroll in the CNO or who drop out of the CNO. Following the principle of "intent to treat," we have retained all such persons in the treatment group throughout the period after randomization. This procedure will tend to bias estimates of the treatment effect, positive or negative, toward zero. A natural alternative procedure, that of removing individuals from the treatment group when they disenroll, also leads to biased estimates due to the process of self selection. If, for example, those individuals who are least healthy and most likely to use health care services are most likely to drop out of the CNO, then mean expenditures, calculated for those who remain, would tend to fall. This decline in mean expenditures would have nothing to do with actions of the CNO, however. Rather it would simply reflect the sorting of high-expenditure individuals out of the treatment group. The computation of expenditure per member per month reported in the next section used both definitions of the treatment group.

The second analytic problem concerns the nature of the expenditure comparisons used to evaluate the net Medicare cost or saving associated with the CNO intervention. These comparisons are based on actual payments, including CNO capitation and case management payments, and thus are measures of the net cost or saving given the payment structure used for the demonstration. Whether the programs could exist at other, lower payment rates is more difficult to assess. Later in this chapter we compute the mean change in capitation and case-management payments necessary to achieve cost neutrality for the program.

8.3 Data

Part A expenditures were drawn from the National Claims History file via the HCFA Decision Support Access Facility (DSAF). All Part B data were extracted by HCFA staff using a finder file of Medicare numbers supplied by Abt Associates. Dollar values were aggregated to the person-month prior to estimation. Expenditures were declared to be missing values for those person-months after death, entry into a Medicare risk HMO, or loss of Medicare eligibility. Expenditures are expressed in 1997 dollars using the Consumer Price Index for discounting.

8.4 Results

Over the first 42 months of operation of the demonstration, total monthly Medicare expenditures per person were higher for the treatment group in all of the four sites. The difference in expenditure per month between the treatment and control groups was only eight percent at Carondelet, but as high as 18 percent at LAH. The dollar value of such expenditures for the first 36 months of operation are shown in Table 8.1 below. The discrepancy is not particularly sensitive to the method of defining the treatment group — all those assigned to treatment or only those enrolled in a given month. That is, total Medicare expenditure per person per month for all randomized beneficiaries assigned to the treatment group exceeded expenditures for currently enrolled CNO members by less than five percent in each of the CNO sites.

Trimmed expenditures, as described in Section 8.2, are also shown in the table. Although trimmed expenditures for CNO services are 8-15 percent lower than total expenditures for these services, they are nevertheless greater than expenditures for the control group in every instance.

Table 8.1: Medicare Expenditure per Person per Month, 36 Months after Random Assignment

Total Medicare expenditure per month

		Carle-IL		*Carondelet-AZ*		*LAH-MN*		*VNS-NY*	
All randomized beneficiaries	T	$365	[$355]	$474	[$462]	$429	[$423]	$806	[$787]
	C	$315		$437		$363		$712	
Randomized controls and currently-enrolled treatments	T	$357	[$345]	$474	[$459]	$411	[$405]	$805	[$782]
	C	$315		$437		$363		$712	

Services not covered by CNO

		Carle-IL	*Carondelet-AZ*	*LAH-MN*	*VNS-NY*
All randomized beneficiaries	T	$285	$379	$345	$654
	C	$280	$385	$332	$619
Randomized controls and currently-enrolled treatments	T	$272	$375	$325	$639
	C	$280	$385	$332	$619

CNO-covered Services

		Carle-IL		*Carondelet-AZ*		*LAH-MN*		*VNS-NY*	
All randomized beneficiaries	T	$80	[$70]	$95	[$83]	$84	[$78]	$152	[$132]
	C	$35		$52		$31		$93	
Randomized controls and currently-enrolled treatments	T	$78	[$59]	$106	[$88]	$67	[60]	$137	[$117]
	C	$35		$52		$31		$93	

Note: Trimmed expenditures for treatment group appear in brackets.
All figures are in 1997 dollars.

Figure 8.1 shows the time path of cumulative Medicare expenditure per-person per-month for individuals assigned to the treatment and control groups for each site. At each site, the discrepancy between the treatment and control groups is quite small in the initial months following randomization.

At Carondelet, total expenditures per member per month were actually smaller for the treatment group for about 10 months following randomization. Nevertheless, total Medicare outlays PMPM for the treatment group exceeded outlays for the control group by a more-or-less stable amount in every site by the time 25-30 months had elapsed from the date of randomization.

Figure 8.2 displays expenditure for non-CNO services. At Carle and LAH, expenditures for the treatment and control groups are nearly identical in this category. However at Carondelet and VNS, the expenditure for treatment and control groups differed for the first 24 months or so after random assignment. At Carondelet, non-CNO expenditures for the control group exceeded those for the treatment group by as much as $100 over this period. At VNS, the treatment group expenditures were higher during the same period. In both cases, non-CNO expenditures eventually converged to near equality after about two years.

Figures 8.3 and 8.4 show the six-month moving average for the same two categories of expenditure. As expected, the paths exhibit greater month-to-month movement, although as in the previous figures, the path for each control group lies generally below that for the treatment group. At two of the four sites the discrepancy between treatment and control in expenditure per month tends to increase in the final few months of the series. It should be noted that the moving average expenditure, unlike the cumulative expenditure shown in Figures 8.1 and 8.2, are based on a progressively smaller number of individuals as the number of months increases. In order to contribute to the calculation for month 24, for example, an individual must have been randomized prior to June 1994.

Risk-adjusted expenditures for the treatment and control groups are shown in Table 8.2 for the P_{ra} risk measure and in Table 8.3 for the prior-year risk measure. With the P_{ra} used as a risk adjuster, mean expenditure for the treatment group is typically higher than for the control group in each of the four CNO sites. Although the treatment group exhibited lower total Medicare expenditure for one risk category in each of the four sites, the specific category of risk for which this was achieved showed no regularity across sites. At Carle, the treatment group exhibited slightly lower cost in the highest risk category, at Carondelet in the next-to-highest, at VNS in the next-to-lowest, and at LAH in the lowest.

When pre-randomization Medicare expenditure is used as a risk adjuster, treatment expenditure exceeds that for controls in nearly every instance. Expenditures for controls are larger in only two cases – for Carondelet in the middle quintile and for VNS in the next-to-lowest quintile. There is a barely discernable tendency for the relative discrepancies to be larger in the lower two quintiles than in the upper two. The expenditure for the treatment group exceeds that of the control group by more than 10 percent in six of the eight comparisons in the lower two quintiles, but only in three of the same eight comparisons in the upper two quintiles.

8.5 Discussion

In all of the four CNO demonstration sites, overall Medicare expenditures per person per month were higher (by 8 to 18 percent) for the treatment than for the control group. Mean monthly expenditures for non-CNO services were comparable for the two groups in all sites. Hence the main impediment to achieving cost effectiveness for the CNOs is probably capitation and case management payments that were set too high. This probably occurred because the rates were set based on the average Medicare use (under fee-for-service) of services covered by the CNO plan. Our best estimates, based on data from previous reports, is that CNO applicants have turned out to be healthier than the average beneficiary. Thus members of the control group have tended to consume fewer services than those used to price the CNO bundle.

A rough approximation of the reduction in monthly CNO payments necessary to achieve budget neutrality is given by the difference between the trimmed monthly expenditure for the treatment group and the corresponding monthly mean expenditure for the control group in the top panel of Table 8.1. An alternative estimate results from comparing trimmed payments for CNO services to control group expenditures in this category as seen in the third panel of Table 8.1. These calculations suggest that budget neutrality payment reductions of $35-$40 per month at Carle, $25-$31 at Carondelet, $47-$60 at LAH, and $39-$75 at VNS.

8.6 References

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Comments on the ANA Report on the CNO Demonstration

(1) In the absence of more details about the analytical methods used, it is not possible to assess the validity of the findings presented by the ANA. Further, there is no indication that the differences between treatments and controls cited in the ANA report were statistically significant.

The fact that the independent Abt evaluation found no significant overall differences in non-CNO expenditures for the treatment and control groups suggests that the ANA analysis has been selective in reporting comparisons that favored the treatment group. It stands to reason that there were counter-balancing instances in which the control groups had lower utilization rates than the treatment rate. Otherwise, Abt's finding of no true cost differences between the two groups would not make sense. For example, the ANA report shows that the treatment groups had lower rates of utilization than the control groups in Minnesota and New York sites for 1996. Logically, it would follow that the control groups at these sites had lower rates of utilization in 1994 and 1995. In short, it only a selected number of the many possible treatment/control comparisons that could have been made were actually reported - those that favored the treatment group. Many other specific treatment/control comparisons could have been made but were not reported. This leads one to question whether the comparisons that were not reported favored the control group. presumably because they favored the control group.

(2) It is difficult to assess the statistical validity of the ANA analyses because there is insufficient information about how they dealt with attrition from the treatment group. For example, if disenrollees from the treatment group were sicker than those who remained, any analysis that failed to include the utilization data for these disenrollees would be biased in favor of the treatment group.

(3) Similarly, the validity of the multiple findings of lower inpatient expenditures for the treatment groups is difficult assess in the absence of more information. For example, it is not clear whether these comparisons included all DRG's or was limited to sub-sets of DRG's. If the analyses were limited to specific sub-sets of DRG's it is likely that some of these would show lower utilization by the treatment group.

EverCare

- EverCare has a HCFA demonstration project (see attached overview). The demonstration had been scheduled to end December 31, 2000. EverCare also contracts with M+C plans to care for their institutionalized enrollees outside of the demonstration.

- Earlier this year, we informed EverCare that payments under the demonstration would not be subject to risk adjustment in 2000.

- EverCare worked with members of Congress to draft a bill (H.R. 1998) with provisions that include: (1) exempting payment for all institutionalized M+C enrollees who are also in EverCare (and similar programs) from risk adjustment until a risk adjustment method meeting criteria specified in the bill had been developed and (2)overriding the limitation on enrollment and disenrollment opportunities that the BBA mandated beginning in 2002 to allow for continuous enrollment in EverCare type programs.

- EverCare contacted the Secretary and OS (Jane Horvath). OS and HCFA met with EverCare on October 13 to discuss the bill. EverCare's letter to Jane Horvath states that risk adjustment would reduce their payment by 42%. (We believe EverCare has used this figure in other communications, e.g., with Members of Congress.) This figure is a significant overstatement of the impact of risk adjustment, in part because they do have complete data on enrollees, e.g., hospital data prior to enrollment in EverCare. In preparation for this meeting, HCFA computed EverCare's payments (1) under the PIP-DCG risk adjustment method and (2) under the current method but using the updated demographic factors for the institutionalized. These updated factors are lower than the ones now in use. One effect of the BBA was to require continued use of the factors in use in 1997, which are based on data from the mid-1970s. A comparison of the older and updated demographic factors for the institutionalized is attached. **HCFA's computations showed that if the updated demographic factors for the institutionalized could have been used in the demonstration, EverCare payments would be 22% less than they are today. They also showed that EverCare's demonstration payments under PIP-DCG risk adjustment method would be 15% less than they are today. Thus, EverCare's payments under risk adjustment are actually higher than they would be if use of the more accurate factors for the institutionalized had not been precluded by the BBA, i.e., a 15% revenue loss under risk adjustment compared to a 22% loss under the demographic system.**

- A summary of the comments on the bill and EverCare payment levels, prepared after the October 13 meeting, is attached.

- HCFA sent a letter on October 22 to EverCare informing it of HCFA's decision to extend the demonstration, which had been scheduled to end in December 2000, through December 2001. The letter also informed EverCare that the exemption from risk adjustment will apply in 2001.

- The House "giveback" bill includes language to allow continuous enrollment for all institutionalized M+C enrollees, not just those in EverCare and similar "specialized programs." We understand that the Senate "giveback" bill may include an enrollment provision but may limit it to those in "specialized programs." The Senate bill also includes language extending the demonstration 2 years beyond the current termination date, i.e., through 2003, exempting the demonstration from risk adjustment in 2000, and exempting demonstration enrollees from the lock-in provisions for the duration of the demonstration. The Senate language requires MedPAC to do a report on the criteria for a risk adjustment methodology in the bill. Neither bill excludes EverCare enrollees outside the demonstration from risk adjustment.

Social HMO Demonstration

- The BBA extended the demonstration through December 2000 and mandated a report on transition of SHMOs to the M+C program by January 1, 1999. This timeframe would have given the Congress 2 years to decide how to transition SHMOs into M+C. HCFA advised congressional staff long before the transition report was due that it would not be submitted on or near the due date. The report is now in HCFA clearance and HCFA hopes the report will be cleared through all necessary levels and submitted to Congress in the Spring of 2000.

- Preliminary analysis findings suggested that while Medicare pays SHMOs more than it would pay them if they were M+C plans, their enrollment in most plans is similar to that of local risk plans.

- The Senate and House "giveback" bills both extend the termination date of the demonstration. The Senate extends it to 12 months after the Secretary issues the transition report; the House Ways and Means language extends it to 18 months after the report is submitted and requires MedPAC to issue recommendation 6 months after the report is submitted.

- A long-standing contentious issue in the SHMO demonstration is a per site cap on enrollment. Prior to BBA, the statute said HCFA could not set a cap below 12,000 per site. At the time of the BBA, enrollment exceeded that level at one site; other sites were not close to that level of enrollment. The BBA revised the 12,000 figure to 36,000. In response, HCFA set the limit at 36,000 per site. Since 2 of the 4 sites have significantly less than 12,000 enrollees, a 36,000 cap is not an issue for them. However, 2 sites, SCAN in Los Angeles and Health Plan of Nevada, are at or near the cap. SCAN has argued that HCFA should raise the cap because the BBA cost estimate assumed 9 sites with 36,000 enrollees and total SHMO demonstration enrollment is substantially less.

- The House Ways and Means giveback bill modifies the cap language, and mandates a minimum aggregate cap of 364,000 (9 x 36,000). HCFA believes the 36,000 per site cap language in the BBA should be maintained because the higher payments given to SHMOs provide them with a competitive advantage relative to M+C plans. As long as the SHMOs are operating under a demonstration, it is desirable to limit the impact of that competitive advantage.

CNO Demonstration

- The BBA extended the CNO demonstration 2 years, through 1999. It mandated a final report by June 30, 1999.

- We have just received the independent evaluation of the project, which we can make public. We will attach this evaluation to a letter report to Congress that summarizes the findings in the independent evaluation. The executive summary, table of contents, and selections from the full report (from the results, discussion, and conclusions text in sections 5, 6, 7, and 8) are attached.

- The report indicates that while HCFA pays CNOs more ($25-$75 per person per month, depending on the site) there are not statistically significant differences between control and treatment groups with regard to health and functional status as measured by the physical component summary (PCS) of the SF-36 nor are there any statistically significant effect of CNO enrollment on health behaviors such as change in smoking habits, seat belt use, influenza vaccination or exercise habits. There was a small but statistically significant increase as measured by the mental component summary (MCS) of the SF-36 when data from all 4 sites was combined. The evaluation noted that enrollees might have found the contact with the CNOs reassuring, producing the small increment in the MCS.

- The ANA has prepared its own analysis, which highlights selected findings that reflect more favorably on the results of the demonstration. Comments on some of the methodological issues with their presentation are attached.

Attachments mentioned in cover document on EverCare in this package. There are 4 in all. They were all faxed to Chris Jennings on November 2.

EVERCARE
Managing Medical Care for Nursing Home Residents

Under a cooperative agreement with the Health Care Financing Administration (HCFA), the EverCare Demonstration utilizes geriatric nurse practitioners as care providers and care managers in nursing facilities. EverCare determines the most appropriate interventions for permanent long-term facility residents who are enrolled in the program and assures follow-up care. The geriatric nurse practitioner authorizes hospitalizations, schedules clinic and outpatient appointments, and consults with the nursing staffs at each facility to assist in problem solving and in coordinating the most effective and efficient care pattern for each enrolled resident. By increasing the availability and intensity of medical services in the nursing home setting, EverCare believes that unnecessary hospitalizations can be avoided and, therefore, care will be less costly.

A physician incentive plan provides a higher payment rate for a nursing home visit than for an office visit and a less than favorable rate for services furnished in the physician's office or in other settings. To facilitate communication between caregivers, the patient, and the patient's family, EverCare pays physicians for attending family conferences. Except for short periods following hospitalization, which may be covered by Medicare (depending on level of need, benefit period, etc.), the room and board and custodial long-term care costs of the nursing home stay are paid by the enrollee and/or the Medicaid program.

The demonstration is expected to reflect lower total medical costs for enrolled nursing home residents, comparable or improved quality of care and health outcomes for these residents, and increased resident and family satisfaction. EverCare has developed specific practice guidelines and care management protocols for the demonstration so that this model can be successfully replicated.

The demonstration is being conducted in six sites: Atlanta, Baltimore, Boston, Denver, Phoenix, and Tampa. No additional sites are anticipated. Each of the sites will operate for 5 years. The demonstration is scheduled to December 31, 2000.

The demonstration sites were initially paid a capitation rate equivalent to the institutional rate category under the Adjusted Average Per Capital Cost (AAPCC) paid to Medicare TEFRA HMO risk contractors for enrolles residing in institutional settings. The capitation rate was 100 percent of the cost for the first year of operation at each site, 95 percent of the AAPCC for the second year of operation, and then 93 percent of the AAPCC for the third through fifth years of the project. Starting January 1998, the sites were paid 93 percent of the Medicare + Choice rates instead of the AAPCC rates.

An evaluation contract was awarded to the Division of Health Services Research and Policy, School of Public Health, University of Minnesota. The Principal Investigator is Robert L. Kane, M.D.

Site enrollment as of September 1998

Atlanta	1402
Baltimore	1901
Boston	2087
Denver	513
Phoenix	266
Tampa	1090

Under the proposed PIP/DCG payment model, HCFA has determined that the EverCare demonstration sites will be underpaid by approximately 15%. A letter was recently sent to EverCare indicating that HCFA is willing to extend the demonstration for an additional year and defer the imposition of the proposed risk-adjusted payment methodology until January 2001.

~~Attachment 1~~

Provisions of H.R. 1998

1. The risk adjustment methodology mandated in the BBA and used to determine payment for Medicare+Choice (M+C) enrollees can not be used to determine payment for a frail elderly M+C beneficiary (as defined in 4 below) enrolled in a M+C plan under a specialized program (described in 3 below) until the Secretary certifies that a "comprehensive risk adjustment methodology" that takes account of the factors (described in 2) below is being fully implemented.

2. The Secretary shall develop a payment methodology for frail elderly M+C enrollees "under a specialized program for the frail elderly" (as defined in 3 below). It must account for the "prevalence, mix, and severity of chronic conditions among such beneficiaries" and "include medical diagnostic factors from all provider setting (including hospital and nursing facility settings)." It must include functional indicators of health status and other appropriate factors.

3. A specialized program for the frail elderly is a program determined by the Secretary to:

   - be offered "as a distinct part of a M+C plan;
   - be primarily enrolling frail elderly M+C beneficiaries; and
   - have a clinical delivery system specifically designed to serve the special needs of the frail elderly and coordinate care through --
     - the use of a team that includes a physician, a nurse practitioner or geriatric care manager, and has members who specialize in the care and management of the frail elderly beneficiaries; and
     - through the provision of primary care services to such beneficiaries by means of such a team at the nursing facility.

4. A frail elderly M+C beneficiary is someone who is *eligible* to enroll in an M+C plan and who is (1) residing in a SNF or a nursing facility for "an indefinite period and without any intention of residing outside the facility" and (2) has a condition the severity of which the Secretary determines, under guidelines, "makes the individual frail."

5. There will be continuous open enrollment for individuals described in (4) above who is "seeking to enroll in a M+C plan under a specialized program for the frail elderly" (as defined in 3 above).

6. The Secretary must develop and implement a program to measure the quality for care provided in specialized programs for the frail elderly that reflects the unique needs of frail elderly M+C beneficiaries not later than July 1, 2000.

~~Attachment 2~~

1998 Demographic Cost Factors for the Institutionalized Aged

	OLD FACTOR* (still in use because of BBA)	UPDATED FACTOR* (never implemented because of BBA)	CURRENT PERCENT OVERPAYMENT
Part A - Female			
65-69	0.55	1.45	164%
70-74	0.75	1.8	140%
75-79	1.05	2.1	100%
80-84	1.3	2.1	62%
85+	1.6	2.1	31%
Part B - Female			
65-69	1.05	1.5	43%
70-74	1.3	1.65	27%
75-79	1.45	1.65	14%
80-84	1.6	1.65	3%
85+	1.65	1.65	0%
Part A - Male			
65-69	0.8	1.75	119%
70-74	1.05	2.25	114%
75-79	1.4	2.25	61%
80-84	1.75	2.25	29%
85+	2.15	2.25	5%
Part B - Male			
65-69	1.1	1.6	45%
70-74	1.4	1.8	29%
75-79	1.65	1.95	18%
80-84	1.8	1.95	8%
85+	1.85	1.95	5%

* The demographic factors for the institutionalized for 1997 and prior years were based on Current Medicare Survey data from the mid-1970s. The survey was discontinued for funding reasons and data were not available to

update the factors for the institutionalized until 1997. The 45-day notice for the 1998 rates, published July 24, 1997, provided revised institutional factors, many of which were lower than the factors based on 20-year-old data. With enactment of the BBA, after the publications of the 45-day notice, HCFA concluded that under the BBA, using the updated factors for the institutionalized would not be permissible.

Comments on Provisions Related to EverCare's
Medicare Demonstration and Non-Demonstration Activities

1. **If the EverCare demonstration had been paid in January 1999 using updated institutionalized factors, payments would have been about *22% less* than under the institutionalized factors now in use.** The demographic-based institutional factors now in use are based on survey data from the mid-1970s. In 1997, prior to enactment of the BBA, HCFA published proposed institutionalized factors for use in 1998 based on up-to-date survey data on Medicare spending for beneficiaries in institutions. (Data had not been available for many years because the survey had been discontinued for funding reasons.) These updated factors were lower, indicating overpayments for institutionalized beneficiaries. Subsequently, pursuant to the M+C payment provisions of the BBA, HCFA had to rescind the updated institutionalized factors and, instead, continued to use factors based on data that was twenty years out of date.

2. **If the EverCare demonstration had been paid in January 1999 using the PIP-DCG method, its payments would have been *15% less* than they were under the payment method now being used.** This is within the range of PIP-DCG impacts for Medicare+Choice plans and it is a substantially smaller impact than the 42% reduction EverCare has reported. Selectively protecting EverCare from risk adjustment while other plans are facing similar impacts established a bad precedent.

3. **When points 1 and 2 above are taken together, they show that EverCare's payments under risk adjustment would be higher than under the current method with the more accurate, updated institutionalized factors. That is, instead of a 22% reduction there would be a 15% reduction.**

4. **Payments to the EverCare demonstration and to M+C plans that contract with EverCare should not be unfairly affected by the risk adjustment system because the system does not pay more or less for avoiding the types of hospital admissions that EverCare focuses on preventing.** In the current demographic system, all payment amounts are based on demographic factors, e.g., age, sex, welfare and institutional status. Under the PIP-DCG risk adjustment method, most payment amounts continue to be based on demographic factors, but a portion (about 20% of spending on fee-for-service beneficiaries) is based on prior year hospital admissions. However, not all admissions generate higher payments. Only hospital admissions that are unavoidable and that are also predictive of future medical costs receive an additional payment. The costs of other admissions, i.e., those that are avoidable or that are not predictive of future costs, are factored into payments based on demographic factors. Thus, when enrollees are admitted for avoidable or non-predictive hospitalizations, there is no additional payment to a M+C plan. Many of the types of types of preventable hospitalizations EverCare works to avoid, e.g., bacterial pneumonia, infections, therefore would not generate an additional payment under the PIP-DCG risk adjustment method. Thus, EverCare would not be disadvantaged since spending for those admissions is factored in to payments

based on demographic factors.

5.. **Enrollees in EverCare demonstration sites do have a substantial number of hospitalizations so they would receive the higher payments associated with those hospitalizations that are considered unavoidable and predictive of future costs.** HCFA assumes that EverCare is not avoiding hospitalizations of the type that generate higher payments under the PIP-DCG method; for example, those associated with serious conditions such as stroke.

6. **The average institutional enrollment for M+C plans is about 1.5% and only one plan (that is not an EverCare demonstration) has over 5% institutional enrollees. Thus, the impact of the 10% phase-in of risk adjustment would be modest for most plans. And the portion of plan payments for the institutionalized under the old system (i.e., 90% of payments in 2000) would continue to benefit for the excessive payments generated by using outdated demographic factors (see point 2.)**

   o In addition, if the phase-in of the PIP-DCG part of risk adjustment is slowed significantly, for example, as proposed by both the House and the Senate, then the impact on payments for the institutionalized will be correspondingly reduced.

7. **In a preliminary finding from the data being gathered for an evaluation of the EverCare demonstration, it appears that EverCare demonstration enrollees have lower than average scores on 3 ADLs (activities of daily living) relative to other Medicare institutionalized beneficiaries.** This may point to EverCare enrollees being somewhat more highly functional than the comparison group of beneficiaries residing in nursing homes. It also suggests that a special "frailty" or functional status adjuster developed for the institutionalized population, such as the one called for in HR 1998, might not result in favorable payments to EverCare relative to other programs for the institutionalized.

Attachments mentioned in cover document on CNO demonstration in this package. The 2-pager marked "draft" and titled "Executive Summary" was faxed to Chris Jennings on November 2. The other materials were not. They are:

- The Table of Contents and pages from Chapters 5, 6, 7, and 8 on "Results", "Discussion", and "Conclusions". These are from the final evaluation, which we will provide to interested congressional staff.

- Comments on the ANA's material on CNO results.

(Also, note that while the Executive Summary faxed on November 2 is marked "draft", we understand it is the version in the final report. However, we do not yet have the final report.)

DRAFT

Executive Summary

The Community Nursing Organization (CNO) Demonstration was implemented on January 1 1994. It was designed to test a system of capitated payment for specified community nursing services covered by Medicare. The demonstration operated in four sites: Carle Clinic (Urbana IL), Carondelet Health Care (Tucson AZ), the Living at Home/Block Nurse Program (Minneapolis MN), and the Visiting Nurse Service (New York NY). The CNOs are at full financial risk for all care provided under the service package.

The demonstration was implemented as a social experiment. Applicants were informed during the introductory information session that there was a one-third probability that they would not be allowed to enroll in the CNO but would instead be assigned to a control group. Members of the control group received care in whatever way they would have had there been no demonstration. All beneficiaries who wished to apply to a CNO after being informed of the requirements of participation and the of the process of randomization were interviewed prior to randomization. The interview elicited information about the applicant's background, health and functional status, behaviors, recent use of health care, and overall satisfaction with care. The interview was repeated by telephone 15 months after the time of randomization and again 27 and 39 months after randomization.

Interim Evaluation Reports were issued in May 1996 and April 1998. They described program outcomes at earlier stages of the demonstration. Those reports found no statistically significant differences in health and functioning between the treatment and control groups. Medicare expenditures for the treatment group, however, were found to be uniformly greater than for the control group.

This final report analyzes follow-up interview data and Medicare claims and payment information for 10,632 beneficiaries randomized between January 1994 and September 1995. Utilization and expenditure data were collected for up to 42 months following randomization..

Effects of the CNO on individual outcomes were assessed using 27-month and 39-month changes in well-known measures of health and functional status, including the physical component summary (PCS) and mental component summary (MCS) of the SF-36, Activities of Daily Living, and questions regarding satisfaction with care. No statistically significant differences in the PCS between the treatment and the control groups emerged, whether at 27 months or 39 months after random assignment. Over the longest possible follow-up period (39 months), assignment to the treatment group was associated with a small but statistically significant increase in the MCS when data from all four CNO sites were combined. It is possible that the periodic assessments and the continuing availability of contact with the CNOs, by telephone or at some other location, is a source of reassurance for many enrollees, producing a small increment to the MCS. At one site, VNS, functional ability, as measured by a combined ADL/IADL scale, appeared to be enhanced by the CNO, although it was impossible to state definitively that this was a direct result of the intervention.

There were no statistically significant effects of CNO enrollment on measured health behaviors. The proportions of individuals who changed smoking habits, wore automobile seat belts, received an annual vaccination for influenza, and who exercised each week were the same for the treatment and control groups. There was a tendency to members of the treatment group to be somewhat more knowledgeable about their blood pressure, serum cholesterol and medications, though these differences were not always statistically significant.

In all four sites, Medicare payments for the treatment group (including capitation payments) exceeded those for the control group. Virtually all of the discrepancy was accounted for by an excess of capitation and case management payments over the expected payments for CNO-type services provided to the control group. Demonstration rates, computed using the *average* utilization of CNO-type services, were probably set at a level above the expected Medicare expenditure for the typically healthy individuals who applied to the CNO. To achieve budget neutrality, CNO capitation payments would need to decline by an average of $25 to $75 per person per month.

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this survey. The scales were recalculated using followup data secured during subsequent interviews conducted 15 months, 27 months, and 39 months after randomization. Dates of death were drawn from the Medicare Enrollment Database. All measures were computed for individuals randomized to treatment or control groups prior to October 1 1995.

5.5 Results

SF-36 Scales

Table 5.2 displays the change in the SF-36 Physical Score, measured from baseline to successive follow-up interviews conducted 15, 27, and 39 months later. Most figures in the table are negative, indicating that physical scores declined on average over the intervening periods. Because all interviewing ceased in December 1997, 39-month followup interviews were conducted only for those randomized before September 1994; 27-month interviews were conducted only with for those randomized before September 1995. Consequently the sample sizes decline for later interviews. The table shows little association between assignment to the treatment group and mean improvement or decline at subsequent points. In the three instances in which treatment/control differences were significant at the 0.10 level, all showed superior outcomes for the treatment group.[1] Nevertheless, thirty-nine months after random assignment, no differences between the treatment and control group are statistically significant, even at the ten percent level.

Table 5.2: Mean Change in SF-36 Physical Health Summary Between Baseline and Three Follow-up Interview Points for Treatment and Control Groups, by CNO Site

	Months after randomization						
	15 months		27 months			39 months	
	T	C	T	C		T	C
Carle	-0.88	-0.73	-1.85	-1.63		-2.67	-2.26
n	1,622	746	1,166	556		988	448
Carondelet	-0.42	-0.94	-0.60	-0.31		-2.09	-1.84
n	1,731	794	561	252		370	166
LAH	0.10	0.20	0.22	-0.75	*	-1.12	-1.25
n	1,372	616	591	300		422	194
VNS	-0.38	-0.54	-0.99	-2.11	*	-2.35	-2.01
n	708	241	439	141		326	103

** Statistically significant at p=0.05 level using t test.
* Statistically significant at p=0.10 level using t test.

Note: Changes are computed as followup value - baseline value. Positive numbers indicate improvement in score; negative numbers indicate decline.

Sources: Baseline and followup assessments of randomized CNO applicants.

1 Nearly identical results were obtained when tests were conducted using the two-sample Wilcoxen rank-sum test rather than the t-test. Using the Wilcoxen test, the 15-month difference at Carondelet is not significant at the 0.10 level. The 27-month difference at LAH is significant at the 0.05 level. Other results are unchanged.

Change scores for the mental health summary (MCS) over the same three followup periods are shown in Table 5.3. Although the change scores are consistently higher for the treatment group, the differences are never significant at the 0.05 level and only once are significant at the 0.10 level.

Least-squares regression estimates of the regression of 39-month change scores for the PCS and MCS scales are shown in Table 5.4. The absence of a statistically significant effect of assignment to the treatment group on change in physical health as measured by the PCS is unsurprising in light of the earlier results seen in Table 5.2. Nor perhaps is the detection of a statistically *significant* effect of treatment on mental health. The emergence of this effect is clearly the result of aggregation across sites and the consequent increase in total sample size.

Table 5.3: Changes in Sf-36 Mental Health Summary (Mcs) Between Baseline and Three Followup Interview Points for Treatment and Control Groups, by CNO Site

	Months after randomization					
	15 months		27 months		39 months	
	T	C	T	C	T	C
Carle	0.08	-0.21	(0.11)	0.08	-0.52	-1.11
n	1,622	746	1,166	556	988	448
Carondelet	0.53	0.29	0.56	-0.56	0.43	-0.63
n	1,731	794	561	252	370	166
LAH	0.33	-0.03	-0.48	-0.83	-0.47	-1.60 *
n	1,372	616	591	300	422	194
VNS	0.57	-0.33	0.02	-1.54	-1.33	-1.65
n	708	241	439	141	326	103

** Statistically significant at p=0.05 level.
* Statistically significant at p=0.10 level.

Note: Changes are computed as followup value - baseline value. Positive numbers indicate improvement in score; negative numbers indicate decline.

Sources: Baseline and followup assessments of randomized CNO applicants.

Table 5.4: OLS Regression Estimates for 39-Month Change in PCS and MCS Scores

	PCS		MCS	
	Coefficient	Std error	Coefficient	Std error
Intercept	-2.84	0.35	-1.01	0.30
TREAT	-0.26	0.36	0.77	0.32
Carondelet	0.64	0.46	0.75	0.40
LAH	1.56	0.44	-0.15	0.38
VNS	0.28	0.50	-0.72	0.44
R2	0.01		0.01	
n	3017		3017	

Sources: Regression analysis of baseline and followup survey data.

The ADL/IADL Scale

The effects of the CNO on functional ability as measured by changes in the ADL/IADL scale are shown in Table 5.5. In three of the four sites, there is virtually no difference between treatment and control groups in ADL/IADL change over time. None of the observed differences for these sites is significant even at the 0.20 level. For VNS, however, the picture is quite different. Not only is the mean level of decline much greater than in the other sites, it is also substantially greater for the control than for the treatment group. This treatment/control difference is statistically significant at the 0.05 level for the 27-month and 39-month followup periods.

Table 5.5: Mean Change in Adl/iadl Total Score Between Baseline and Three Followup Interview Points for Treatment and Control Groups, by CNO Site

	Months after randomization					
	15 months		27 months		39 months	
	T	C	T	C	T	C
Carle	0.01	0.06	-0.13	-0.06	-0.34	-0.31
n	1,735	792	1,252	591	1,058	477
Carondelet	-0.07	-0.12	-0.20	-0.24	-0.48	-0.59
n	1,885	877	628	282	399	178
LAH	-0.04	-0.03	-0.24	-0.26	-0.50	-0.50
n	1,490	671	652	329	466	212
VNS	-0.15	-0.43 *	-0.24	-0.67 **	-0.79	-1.39 **
n	831	311	499	171	381	140

** Statistically significant at p=0.05 level using t test.
* Statistically significant at p=0.10 level using t test.

Note: Changes are computed as followup value - baseline value. Positive numbers indicate improvement in score, negative numbers indicate decline.

Sources: Baseline and followup assessments of randomized CNO applicants.

Mortality

The incidence of mortality among the treatment and control groups for each of the CNO sites is shown in Table 5.6. Mortality was similar, in general, for the treatment and control groups. The clear exception was at VNS, where deaths per person-month were 22 percent higher among the treatment group, although this difference was not statistically significant.[2] As we shall see, the pattern of mortality across the four CNO sites creates a difficulty in interpreting the ADL/IADL results presented above.

2 The confidence interval for the rate ratio (RR) is computed as $EXP[\ln(\hat{RR} \pm 1.96\,SE)]$ where $\hat{RR}$ is the estimated rate ratio and SE is its standard error. See Rothman (1986) which also provides the formula for the pooled Mantel-Haenszel estimate.

Table 5.6: Mortality in the Four CNO Sites

	Treatment	Control
Carle		
Deaths	145	76
Person-months at risk	75,479	35,578
Deaths per 10^3 person months	1.92	2.14
Carondelet		
Deaths	247	136
Person-months at risk	75,153	38,032
Deaths per 10^3 person months	3.29	3.58
Living at Home		
Deaths	164	81
Person-months at risk	58,506	28,899
Deaths per 10^3 person months	2.80	2.80
VNS		
Deaths	172	71
Person-months at risk	40,951	20,689
Deaths per 10^3 person months	4.20	3.43

Rate ratios (treatment/control) for beneficiary mortality

	Rate Ratio	Standard error	95%Confidence interval	
Carle	0.899	0.142	(0.681,	1.187)
Carondelet	0.919	0.107	(0.746,	1.133)
Living at Home	1.000	0.136	(0.766,	1.305)
VNS	1.224	0.141	(0.928,	1.614)
Pooled (Mantel-Haenszel)	0.992	0.064	(0.875,	1.125)

Sources: Medicare Enrollment Database

5.6 Discussion

Three years after random assignment, the physical health of Medicare beneficiaries assigned to the treatment (CNO) group, as measured by the physical component summary (PCS) of the SF-36 was not different from that of the control group. There is no reason to believe that nurse case management, the practice of periodic in-person assessment, or other interventions carried out by the CNOs led to any improvement in physical health. Nor is there any evidence that physical health of the treatment group deteriorated relative to that of the control group.

Over the same period, assignment to the treatment group was associated with consistent and statistically significant improvements in mental health, as measured by the mental component summary (MCS) of the SF-36. The overall decline in mental health scores for both treatment and control groups combined was quite small, averaging about one point, or two percent of the typical baseline score of 50 or so. Hence the treatment effect, equal to about three-quarters of a point, may not be clinically meaningful. Nevertheless, the issue bears further investigation. It is possible that the periodic assessments and the continuing availability of contact with the CNOs, by telephone or at some other location, is a source of reassurance for many enrollees, producing a small increment to the MCS.

The issue of ADL/IADL performance is the most difficult matter to address here. In three of the four sites — Carle, Carondelet, and LAH — changes over time in ADL/IADL performance were practically identical for the treatment and control groups. At VNS, however, a marked and statistically significant positive effect of treatment was identified. A straightforward conjecture is that something about the activities carried out by VNS produced a beneficial effect on functional independence or that the population applying to the CNO in New York was especially well-suited to benefit from the CNO. The pattern of mortality among treatment and control groups across the four sites suggests a second hypothesis. Recall from Table 5.6 that mortality for the treatment group at VNS was some 22 percent higher than for the control group. Because this difference was not statistically significant, one cannot conclude that a population of individuals enrolled in CNO at VNS would experience higher mortality than would an identical group who was not enrolled. But the higher mortality *in the sample* raises the possibility of so-called informative censoring.

If the death rate is the same for treatment and control groups or if the probability of death is unrelated to outcome measures of interest, then loss of followup data (censoring) due to death will not create analytic difficulties. However, if the death rate is higher for one group and if individuals who die, would, had they lived, have exhibited greater-than-average declines in ADL/IADL performance, then treatment/control differences in outcome measures may be biased despite random assignment. In this instance, it is reasonable at least, to suggest that the observed decline in ADL/IADL scores for the treatment group at VNS is not as great as it would have been if the death rate had been the same for the treatment and control group. It is possible, then, that the observed positive CNO effect at VNS is an artifact of higher mortality in the treatment sample. Whether the observed result is genuine or the result of censoring cannot be ascertained at this point.

Broadly speaking, then, our conclusions regarding the effect of the CNO on individual outcomes echo those of the authors cited in the introduction to this chapter. There is no solid evidence, positive or negative, of an effect of the CNO on physical health, measured by the PCS scale or by observed mor-

tality. At one site, VNS, functional ability, as measured by a combined ADL/IADL scale, appeared to be enhanced by the CNO, although it was impossible to state definitively that this was a direct result of the intervention. Finally, there was a small but consistent beneficial effect of the CNO on mental health. This latter effect is believed to be genuine because of its uniformity across all four CNO sites.

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treatment effect if one exists. However, if an effect is detected, it cannot stated with confidence whether it was a result of the treatment or of selection bias.

6.5 Results

Preventive Care and Health Promotion Activities

Each CNO had its own strategy in disseminating information on health promotional activities and instructing their enrollees in preventive care. Below is an overview of each sites' methods:

Carle

In addition to encouraging preventive care and providing health information at the time of assessments, Carle implemented monthly and quarterly newsletters which featured health education topics and activities available in the community. An annual survey was sent to enrollees to determine which education topics most interested them and to help in planning future health promotion events. In addition, the CNO worked with local colleges and community organizations to hold an annual Health Fair and several health education sessions on arthritis, hospice care, managing chronic pain, and physician/patient relationships.

Carondelet

Carondelet CNO, facing stiff competition from local HMOs, developed a wide range of health promotion activities as one means of differentiating itself from look-alike competitors. The CNO's Health Education Coordinator, along with CNO nurses, helped organize several monthly presentations focusing on topics including exercising safely, healthy eating, and stress management. Special classes, seminars with guest speakers, and social events were held throughout each year. Videos were made of the presentations so that housebound enrollees could benefit as well. Each year, health screenings were held at various community sites. In addition, a monthly newsletter and an annual survey of health education topics of interest were sent out to all enrollees. Volunteers assisted in the organization of these activities.

LAH

At the LAH/BNP CNO, each subsite organized several monthly educational and social events, inviting guest speakers and featuring topics such as T'ai Chi, eye care, and personal safety. Community coordinators organized walking clubs and support groups for weight loss and other concerns. These activities were very popular because they became opportunities for enrollees to socialize with other seniors. Additionally, several health screenings and flu clinics were held throughout the year. As at the other sites, an annual survey was sent to enrollees about which health education topics most interested them. A quarterly newsletter tried to convey the impression of a social, club-like atmosphere at the CNO, in order to provide appealing incentives for low-risk seniors to remain involved in the organization. Volunteers played a major role in organizing and implementing these activities.

VNSNY

As noted in the 1995 Annual Report (Teitelbaum & Thomas), VNSNY enrollees were less interested in prevention, health promotion, and wellness when compared to the enrollees at the other sites. Still, VNSNY CNO nurses attempted to reach out to enrollees by giving frequent

presentations at community sites on topics ranging from memory loss to insomnia. Flu clinics were held in the fall of each year. An annual survey elicited enrollee input on health topics of interest, and the monthly newsletter listed health and wellness activities sponsored by local organizations as well as the CNO nurses' presentations.

Preventive health care behaviors of the treatment and control groups were surveyed at the 27 and 39 month follow-up interviews. Behaviors analyzed in this chapter are:

- The amount of information applicants recalled receiving on nutrition and eating healthier,
- The reported change in applicants' smoking habits,
- The amount of time applicants wear seat belts,
- The receipt of a flu shot in the past year, and
- The amount of exercise done each week.

As seen in Table 6.1, the proportion of individuals who reported receiving information on nutrition and healthier eating is significantly higher for the treatment group than the control group at both the 27 and the 39 month survey. None of the other available survey items yielded significant differences between treatment and control groups. In the cases of smoking behavior and receipt of a flu shot, the treatment group showed slight gains over time; still, none of these differences were significant, even at the 0.15 level.

Treatment and control groups were surveyed regarding their health knowledge at the 27 and 39 month follow-up interviews in terms of:

- The applicants' knowledge of their own blood pressure,
- The applicants' knowledge of their own cholesterol level,
- The applicants' overall assessment of 'health symptoms and conditions,' and
- The applicants' assessment of their understanding of medication prescribed to them.

The results of this analysis are detailed in Table 6.2. The most dramatic of the differences between the control and the treatment group is in the knowledge of blood pressure. A much greater proportion of respondents in the CNO treatment group than in the control group said they knew their blood pressure at both 27 and 39 month follow-up surveys. The proportion of treatment group members reporting 1) knowledge of cholesterol levels, 2) their health conditions and symptoms, and 3) an understanding of medications prescribed are slightly higher than the proportion of the control group, but none of these results are significantly different.

Satisfaction

Individual satisfaction with the health care received from providers was gauged by an examination of respondent agreement with several statements about their care at the 27 and 39 month assessment. The statements concern seven dimensions of satisfaction with health care and providers:

- Time spent by providers,
- Satisfaction with care received from home nursing care,
- Opportunity to participate in decision making,
- Courtesy and respect shown by providers,
- Satisfaction with care and assistance from nurses,
- Overall satisfaction with health care services, and
- Confidence that needed care is available.

CNO applicants' responses to these survey statements concerning the perceived quality of health care and health care providers are shown in Table 6.3 and 6.4. While it is clear that the opinions elicited 27 months after random assignment show slight increases in satisfaction for the treatment groups, none of the difference are statistically significant. After 39 months of enrollment, there is the same general trend where the proportion of applicants in the treatment group demonstrate higher satisfaction levels than the control group. Differences in satisfaction with home health care and with the opportunity to participate in health care decisions between the treatment and control group are statistically significant. These results must be interpreted with caution; while it is tempting to state that the increasing levels of satisfaction for the treatment group is due to a longer exposure to CNO services, we cannot be sure that these results are caused by eliminating the disenrollees, who may be less interested in health education.

6.6 Discussion

Twenty-seven months after random assignment, there is no firm evidence to suggest that health education or satisfaction outcomes associated with the CNO are superior to those experienced by the control group. This result, while perhaps discouraging for the CNOs, was not surprising. The primary interventions employed by the CNO, case management and periodic assessment, have not been shown to improve outcomes in the general elderly population. As we have seen, CNO applicants are probably healthier than most Medicare beneficiaries. Few interventions of any sort have been found to markedly improve the health outcomes for elderly individuals. To our knowledge organizational interventions not targeted to specific groups of individuals have not been shown to enhance outcomes for the over-65 population.

Individuals enrolled at the CNO sites 39 months following their assignment to the treatment group reported significantly greater satisfaction on two measures: nursing care and participation in decisions regarding their health care. These changes may be attributable to CNO performance, however, those that were not satisfied may have disenrolled by the 39 month follow up, skewing the data.

Managed care plans in general, whether Medicare risk plans or private HMOs, are not and have never been judged by their ability to produce outcomes that are superior to those observed under fee-for-service care. The standard for care has been to produce outcomes that are *not* demonstrably worse than generated under fee-for-service. Analyses including the final follow-up data, almost three years after enrollment, prove that CNOs have met this standard.

this result and the fact that no other results in the table were significant, we conclude that enrollment in the CNO did not have systematic effects on utilization of non-CNO services, and that the single observed lower mean utilization rate of CNO enrollees relative to the control group was likely to be the result of nonrandom selection.

7.5 Conclusion

This chapter analyzed data from three different sources to determine whether enrollment in the CNO resulted in changes in patterns of utilization of health services. If CNO enrollment had improved health status, we would have expected utilization of both CNO-covered and non-CNO services to be less for the treatment groups than for the control groups. If the sites engaged in cost shifting to maximize their net revenues, we would have expected higher non-CNO utilization for the treatment groups. None of these patterns were observed in the data.

While survey responses indicated that members of the treatment group were more likely to receive a variety of services than were members of the control group, the survey data also revealed that the amounts of service received tended to be less. Medicare claims data and records kept by the sites showed that overall average levels of utilization for a wide variety of services were the same for treatment and control groups. This fundamental finding was not affected by including variables to adjust for differences in individual health risk.

Many of the analyses in this chapter also were conducted under a more restrictive definition of the treatment group, excluding those person-months when the beneficiary was not actually enrolled in the CNO. While this definition resulted in several significant differences between treatment and control groups, most disappeared when the analysis was risk-adjusted, suggesting that they were the products of selection bias.

Finally, timesheet and enrollment records indicated that expected efficiency gains from substitution of telephone for in-person contact were not realized. On the contrary, hours per enrollee were relatively constant after enrollment stabilized about 15 months after start-up, and the sites continued to commit growing resources to home visits years later.

The data, therefore, do not support the hypothesis that enrollment in the CNO had appreciable effects on the utilization of health services. While it is likely that some effects did occur, they were too small to be detectable.

Threats to the Analysis

The analyses undertaken in this chapter are confronted by two problems that cannot be wholly resolved. The first of these is the appropriate treatment of individuals who are randomized to the treatment group but who either fail to enroll in the CNO or who drop out of the CNO. Following the principle of "intent to treat," we have retained all such persons in the treatment group throughout the period after randomization. This procedure will tend to bias estimates of the treatment effect, positive or negative, toward zero. A natural alternative procedure, that of removing individuals from the treatment group when they disenroll, also leads to biased estimates due to the process of self selection. If, for example, those individuals who are least healthy and most likely to use health care services are most likely to drop out of the CNO, then mean expenditures, calculated for those who remain, would tend to fall. This decline in mean expenditures would have nothing to do with actions of the CNO, however. Rather it would simply reflect the sorting of high-expenditure individuals out of the treatment group. The computation of expenditure per member per month reported in the next section used both definitions of the treatment group.

The second analytic problem concerns the nature of the expenditure comparisons used to evaluate the net Medicare cost or saving associated with the CNO intervention. These comparisons are based on actual payments, including CNO capitation and case management payments, and thus are measures of the net cost or saving given the payment structure used for the demonstration. Whether the programs could exist at other, lower payment rates is more difficult to assess. Later in this chapter we compute the mean change in capitation and case-management payments necessary to achieve cost neutrality for the program.

8.3 Data

Part A expenditures were drawn from the National Claims History file via the HCFA Decision Support Access Facility (DSAF). All Part B data were extracted by HCFA staff using a finder file of Medicare numbers supplied by Abt Associates. Dollar values were aggregated to the person-month prior to estimation. Expenditures were declared to be missing values for those person-months after death, entry into a Medicare risk HMO, or loss of Medicare eligibility. Expenditures are expressed in 1997 dollars using the Consumer Price Index for discounting.

8.4 Results

Over the first 42 months of operation of the demonstration, total monthly Medicare expenditures per person were higher for the treatment group in all of the four sites. The difference in expenditure per month between the treatment and control groups was only eight percent at Carondelet, but as high as 18 percent at LAH. The dollar value of such expenditures for the first 36 months of operation are shown in Table 8.1 below. The discrepancy is not particularly sensitive to the method of defining the treatment group — all those assigned to treatment or only those enrolled in a given month. That is, total Medicare expenditure per person per month for all randomized beneficiaries assigned to the treatment group exceeded expenditures for currently enrolled CNO members by less than five percent in each of the CNO sites.

Trimmed expenditures, as described in Section 8.2, are also shown in the table. Although trimmed expenditures for CNO services are 8-15 percent lower than total expenditures for these services, they are nevertheless greater than expenditures for the control group in every instance.

Table 8.1: Medicare Expenditure per Person per Month, 36 Months after Random Assignment

Total Medicare expenditure per month

		Carle-IL		*Carondelet-AZ*		*LAH-MN*		*VNS-NY*	
All randomized beneficiaries	T	$365	[$355]	$474	[$462]	$429	[$423]	$806	[$787]
	C	$315		$437		$363		$712	
Randomized controls and currently-enrolled treatments	T	$357	[$345]	$474	[$459]	$411	[$405]	$805	[$782]
	C	$315		$437		$363		$712	

Services not covered by CNO

		Carle-IL	*Carondelet-AZ*	*LAH-MN*	*VNS-NY*
All randomized beneficiaries	T	$285	$379	$345	$654
	C	$280	$385	$332	$619
Randomized controls and currently-enrolled treatments	T	$272	$375	$325	$639
	C	$280	$385	$332	$619

CNO-covered Services

		Carle-IL		*Carondelet-AZ*		*LAH-MN*		*VNS-NY*	
All randomized beneficiaries	T	$80	[$70]	$95	[$83]	$84	[$78]	$152	[$132]
	C	$35		$52		$31		$93	
Randomized controls and currently-enrolled treatments	T	$78	[$59]	$106	[$88]	$67	[60]	$137	[$117]
	C	$35		$52		$31		$93	

Note: Trimmed expenditures for treatment group appear in brackets.
All figures are in 1997 dollars.

Figure 8.1 shows the time path of cumulative Medicare expenditure per-person per-month for individuals assigned to the treatment and control groups for each site. At each site, the discrepancy between the treatment and control groups is quite small in the initial months following randomization.

At Carondelet, total expenditures per member per month were actually smaller for the treatment group for about 10 months following randomization. Nevertheless, total Medicare outlays PMPM for the treatment group exceeded outlays for the control group by a more-or-less stable amount in every site by the time 25-30 months had elapsed from the date of randomization.

Figure 8.2 displays expenditure for non-CNO services. At Carle and LAH, expenditures for the treatment and control groups are nearly identical in this category. However at Carondelet and VNS, the expenditure for treatment and control groups differed for the first 24 months or so after random assignment. At Carondelet, non-CNO expenditures for the control group exceeded those for the treatment group by as much as $100 over this period. At VNS, the treatment group expenditures were higher during the same period. In both cases, non-CNO expenditures eventually converged to near equality after about two years.

Figures 8.3 and 8.4 show the six-month moving average for the same two categories of expenditure. As expected, the paths exhibit greater month-to-month movement, although as in the previous figures, the path for each control group lies generally below that for the treatment group. At two of the four sites the discrepancy between treatment and control in expenditure per month tends to increase in the final few months of the series. It should be noted that the moving average expenditure, unlike the cumulative expenditure shown in Figures 8.1 and 8.2, are based on a progressively smaller number of individuals as the number of months increases. In order to contribute to the calculation for month 24, for example, an individual must have been randomized prior to June 1994.

Risk-adjusted expenditures for the treatment and control groups are shown in Table 8.2 for the P_{ra} risk measure and in Table 8.3 for the prior-year risk measure. With the P_{ra} used as a risk adjuster, mean expenditure for the treatment group is typically higher than for the control group in each of the four CNO sites. Although the treatment group exhibited lower total Medicare expenditure for one risk category in each of the four sites, the specific category of risk for which this was achieved showed no regularity across sites. At Carle, the treatment group exhibited slightly lower cost in the highest risk category, at Carondelet in the next-to-highest, at VNS in the next-to-lowest, and at LAH in the lowest.

When pre-randomization Medicare expenditure is used as a risk adjuster, treatment expenditure exceeds that for controls in nearly every instance. Expenditures for controls are larger in only two cases – for Carondelet in the middle quintile and for VNS in the next-to-lowest quintile. There is a barely discernable tendency for the relative discrepancies to be larger in the lower two quintiles than in the upper two. The expenditure for the treatment group exceeds that of the control group by more than 10 percent in six of the eight comparisons in the lower two quintiles, but only in three of the same eight comparisons in the upper two quintiles.

8.5 Discussion

In all of the four CNO demonstration sites, overall Medicare expenditures per person per month were higher (by 8 to 18 percent) for the treatment than for the control group. Mean monthly expenditures for non-CNO services were comparable for the two groups in all sites. Hence the main impediment to achieving cost effectiveness for the CNOs is probably capitation and case management payments that were set too high. This probably occurred because the rates were set based on the average Medicare use (under fee-for-service) of services covered by the CNO plan. Our best estimates, based on data from previous reports, is that CNO applicants have turned out to be healthier than the average beneficiary. Thus members of the control group have tended to consume fewer services than those used to price the CNO bundle.

A rough approximation of the reduction in monthly CNO payments necessary to achieve budget neutrality is given by the difference between the trimmed monthly expenditure for the treatment group and the corresponding monthly mean expenditure for the control group in the top panel of Table 8.1. An alternative estimate results from comparing trimmed payments for CNO services to control group expenditures in this category as seen in the third panel of Table 8.1. These calculations suggest that budget neutrality payment reductions of $35-$40 per month at Carle, $25-$31 at Carondelet, $47-$60 at LAH, and $39-$75 at VNS.

8.6 References

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Comments on the ANA Report on the CNO Demonstration

(1) In the absence of more details about the analytical methods used, it is not possible to assess the validity of the findings presented by the ANA. Further, there is no indication that the differences between treatments and controls cited in the ANA report were statistically significant.

The fact that the independent Abt evaluation found no significant overall differences in non-CNO expenditures for the treatment and control groups suggests that the ANA analysis has been selective in reporting comparisons that favored the treatment group. It stands to reason that there were counter-balancing instances in which the control groups had lower utilization rates than the treatment rate. Otherwise, Abt's finding of no true cost differences between the two groups would not make sense. For example, the ANA report shows that the treatment groups had lower rates of utilization than the control groups in Minnesota and New York sites for 1996. Logically, it would follow that the control groups at these sites had lower rates of utilization in 1994 and 1995. In short, it only a selected number of the many possible treatment/control comparisons that could have been made were actually reported - those that favored the treatment group. Many other specific treatment/control comparisons could have been made but were not reported. This leads one to question whether the comparisons that were not reported favored the control group. presumably because they favored the control group.

(2) It is difficult to assess the statistical validity of the ANA analyses because there is insufficient information about how they dealt with attrition from the treatment group. For example, if disenrollees from the treatment group were sicker than those who remained, any analysis that failed to include the utilization data for these disenrollees would be biased in favor of the treatment group.

(3) Similarly, the validity of the multiple findings of lower inpatient expenditures for the treatment groups is difficult assess in the absence of more information. For example, it is not clear whether these comparisons included all DRG's or was limited to sub-sets of DRG's. If the analyses were limited to specific sub-sets of DRG's it is likely that some of these would show lower utilization by the treatment group.

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DRAFT, 1999
~~Nov~~ Oct.

Executive Summary

The Community Nursing Organization (CNO) Demonstration was implemented on January 1 1994. It was designed to test a system of capitated payment for specified community nursing services covered by Medicare. The demonstration operated in four sites: Carle Clinic (Urbana IL), Carondelet Health Care (Tucson AZ), the Living at Home/Block Nurse Program (Minneapolis MN), and the Visiting Nurse Service (New York NY). The CNOs are at full financial risk for all care provided under the service package.

The demonstration was implemented as a social experiment. Applicants were informed during the introductory information session that there was a one-third probability that they would not be allowed to enroll in the CNO but would instead be assigned to a control group. Members of the control group received care in whatever way they would have had there been no demonstration. All beneficiaries who wished to apply to a CNO after being informed of the requirements of participation and the of the process of randomization were interviewed prior to randomization. The interview elicited information about the applicant's background, health and functional status, behaviors, recent use of health care, and overall satisfaction with care. The interview was repeated by telephone 15 months after the time of randomization and again 27 and 39 months after randomization.

Interim Evaluation Reports were issued in May 1996 and April 1998. They described program outcomes at earlier stages of the demonstration. Those reports found no statistically significant differences in health and functioning between the treatment and control groups. Medicare expenditures for the treatment group, however, were found to be uniformly greater than for the control group.

This final report analyzes follow-up interview data and Medicare claims and payment information for 10,632 beneficiaries randomized between January 1994 and September 1995. Utilization and expenditure data were collected for up to 42 months following randomization..

Effects of the CNO on individual outcomes were assessed using 27-month and 39-month changes in well-known measures of health and functional status, including the physical component summary (PCS) and mental component summary (MCS) of the SF-36, Activities of Daily Living, and questions regarding satisfaction with care. No statistically significant differences in the PCS between the treatment and the control groups emerged, whether at 27 months or 39 months after random assignment. Over the longest possible follow-up period (39 months), assignment to the treatment group was associated with a small but statistically significant increase in the MCS when data from all four CNO sites were combined. It is possible that the periodic assessments and the continuing availability of contact with the CNOs, by telephone or at some other location, is a source of reassurance for many enrollees, producing a small increment to the MCS. At one site, VNS, functional ability, as measured by a combined ADL/IADL scale, appeared to be enhanced by the CNO, although it was impossible to state definitively that this was a direct result of the intervention.

There were no statistically significant effects of CNO enrollment on measured health behaviors. The proportions of individuals who changed smoking habits, wore automobile seat belts, received an annual vaccination for influenza, and who exercised each week were the same for the treatment and control groups. There was a tendency to members of the treatment group to be somewhat more knowledgeable about their blood pressure, serum cholesterol and medications, though these differences were not always statistically significant.

In all four sites, Medicare payments for the treatment group (including capitation payments) exceeded those for the control group. Virtually all of the discrepancy was accounted for by an excess of capitation and case management payments over the expected payments for CNO-type services provided to the control group. Demonstration rates, computed using the *average* utilization of CNO-type services, were probably set at a level above the expected Medicare expenditure for the typically healthy individuals who applied to the CNO. To achieve budget neutrality, CNO capitation payments would need to decline by an average of $25 to $75 per person per month.

Evaluation of the Community Nursing Organization Demonstration Second Interim Evaluation Report

Contract No. 500-92-0050

Robert J. Schmitz, PhD
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Executive Summary

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The demonstration was implemented as a social experiment. Applicants were informed during the introductory information session that there was a one-third probability that they would not be allowed to enroll in the CNO but would instead be assigned to a control group. Members of the control group received care in whatever way they would have had there been no demonstration. All beneficiaries who wished to apply to a CNO after being informed of the requirements of participation and of the process of randomization were interviewed prior to randomization. The interview elicited information about the applicant's background, health and functional status, behaviors, recent use of health care, and overall satisfaction with care. The interview was repeated by telephone 15 months after the time of randomization and again 27 months after randomization.

The First Interim Evaluation Report was issued in May 1996. It described program outcomes and operation through the first 21 months of operation. This Second Interim Report is more limited in its aims than the first. It analyzes CNO enrollment, individual outcomes, and Medicare utilization and expenditures for the treatment and control groups. It does not, however, treat the processes of marketing, decision making, assessment, and nurse intervention that characterize the CNO as implemented in each of the four sites. The Final Evaluation Report, to be completed later this year, will address these concerns in detail. Issues to be addressed include:

- clinical interventions and outcomes,
- nurse activity and clinical practice, and
- management, organization, and service coordination.

Over 11,178 beneficiaries were randomized by June 1996. This report analyzes follow-up interview data and Medicare claims and payment information through the first 30 months of CNO operation. Once again, CNO applicants have been found to be generally healthier and more independent than the general population of Medicare beneficiaries in each of the sites.

The First Interim Report found no statistically significant differences between the CNO treatment and control groups in well known measures of health and functional status, including the SF-36, Activities of Daily Living, Instrumental Activities of Daily Living, and questions regarding satisfaction with care. However, in the First Interim Report there were at least hints that members of the treatment group with functional limitations at baseline experienced slightly better outcomes than their counterparts in the control group. At that time, it appeared that subsequent analyses of a larger data set covering a longer period of CNO operations might furnish more precise estimates of CNO effects. However, these hints did not materialize for the Second Interim Report. While site-specific analyses found improvements in a few cases, these positive findings were scattered across sites and various measures and in most cases not

significant at the standard 0.05 level. Under commonly-accepted standards of statistical inference, we cannot assert that the overall outcomes of the treatment group are superior to those of the control group.

In three of the four sites, Medicare payments for the treatment group (including capitation payments) exceeded those for the control group. Virtually all of the discrepancy was accounted for by an excess of capitation and case management payments over the expected payments for CNO-type services provided to the control group. Demonstration rates, computed using the *average* utilization of CNO-type services, were probably set at a level above the expected Medicare expenditure for the typically healthy individuals who applied to the CNO. To achieve budget neutrality, CNO capitation payments would need to decline by an average of $30 to $55 per person per month.