Abt Associates Inc.

Phase II Evaluation of CNO Demonstration

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Executive Summary

The Community Nursing Organization (CNO) Demonstration is an innovative approach to the provision of community nursing and ambulatory care services for Medicare beneficiaries. Structured around the two fundamental concepts of nurse case management and capitated payment, CNOs attempt to promote the timely and appropriate use of community health services and to reduce the use of costly acute-care services.

In order to explore the impact of this model of care delivery on cost and outcomes, the CNO Demonstration was created by the Omnibus Budget Reconciliation Act (OBRA) of 1987. To carry out this demonstration, the Centers for Medicare & Medicaid Services (CMS) entered into cooperative agreements with the following four eligible organizations to serve as demonstration providers in 1993:

- **Carle Clinic, Urbana, IL,**
- **Carondelet Health Care, Tucson, AZ,**
- **Living at Home/Block Nurse Program (LAH/BNP), Minneapolis, MN, and**
- **Visiting Nurse Service, New York, NY (VNSNY).**

OBRA, 1987 also mandated an evaluation of the CNO demonstration. Abt Associates Inc. was awarded a contract to provide technical assistance to the sites and to evaluate the effects of the demonstration from January, 1994 until July, 1997.

The evaluation design permitted especially strong results because applicants to the CNOs were randomized to treatment (CNO) or control (traditional Medicare) groups. Abt Associates Inc. reported the results of the CNO Evaluation in a Second Interim Report on April 6, 1998 and in a Final Report on April 13, 2000. The Medicare, Medicaid and SCHIP Benefits Improvement and Protection Act (BIPA) of 2000 directed the Secretary of Health and Human Services to investigate whether results from the CNO demonstration might change in the long run, since gains from the preventive efforts of the CNOs might take time to materialize. At the same time, BIPA reduced the inflation-adjusted capitation rates to be paid to the sites (by 15 percent in New York and 10 percent at the other sites) and replaced the previously mandated evaluation with a Preliminary Report due no later than July 1, 2001 and a Final Report no later than July 1, 2002. The Preliminary Report was prepared by Abt Associates Inc. and summarized the evaluation of the extension of the CNO Demonstration (known as Phase II) from January, 1994 through December, 1999. This Final Phase II Evaluation Report adds seven months to the study, concluding in July, 2000.[1]

The evaluation of Phase II faced a major problem that was avoided by the experimental design of Phase I. Because the randomization requirement was maintained only intermittently after October 1995, and control group members were permitted to enroll in the CNO after October 1997, analysis of demonstration enrollees during these periods must address the possible effects

[1] **The original plan was to add an additional year of data. However, at the time of preparation of this report, Medicare outpatient data for August through December, 2000 were unavailable due to the implementation of a new prospective payment system for outpatient services.**

of selection bias. Selection bias arises when individuals decide (self-select) whether to participate in a program and when some of the factors that influence their decision also influence the outcomes to be evaluated. In the CNO Demonstration, it appeared that beneficiaries who chose to participate consistently used more services than the average beneficiary in the CNO service area. Failure to account for this difference would confuse the selection effect with the treatment effect.

Recognizing the selection bias problem, the Phase II evaluation employed two complementary approaches:

1. **The comparison of average utilization and expenditures exclusively for randomized beneficiaries (these results are described in detail in Chapter 3 of the Phase II Preliminary Report and summarized in Chapter 2 of this report), and**
2. **The analysis of utilization and expenditures for all treatment group beneficiaries as compared to a reference group drawn from the Medicare population living in the same geographic area (first presented in Chapter 4 of the Phase II Preliminary Report and updated in Chapter 4 of this report).**

This second approach required more complex statistical models to adjust as much as possible for the effects of selection bias (see Appendix A for methodological details).

Preliminary Report Results

With respect to the most important question, whether the CNO sites achieved budget neutrality for the Medicare program, these two analyses reached the same conclusion: they did not. When analysis was restricted to randomized beneficiaries, Medicare spending per person per month was higher for members of the treatment group than for members of the control group and these differences were statistically significant at three of the four sites. When all treatment group members were compared to the population-based reference group, treatment group spending per person per month was statistically significantly higher at all four sites, and these differences tended to become larger and more significant over time.

These results have proven to be robust across a variety of different definitions of the treatment group. The project team restricted the sample and repeated both approaches. For the first approach, the first six months after randomization and all months in 1994 were excluded, but the results were very similar. For the second approach, all beneficiaries enrolled less than six months were excluded from the treatment group, producing similar, though in some site-years smaller, estimated differences between groups. This slight change in results could be traced to the disproportionate exclusion of high-cost beneficiaries from the sample, who were more likely to drop out of the CNO, perhaps because they became ineligible due to institutionalization or because they found case management to be too restrictive.

Both analytic approaches found that capitation rates for CNO-covered services resulted in payments for the treatment group that were higher than payments for the control or population reference groups for nearly all site-years. The analysis of randomized beneficiaries indicated that this was the only source of elevated expenditures for the treatment group, implying that a reduction in capitation rates might make the CNO budget neutral. By contrast, comparison of the treatment group to the population reference group suggested that expenditures for non-

CNO services, particularly inpatient hospitalization, were also higher for the treatment group, implying that CNO participation induced higher utilization of non-CNO services.

Final Report Results

The results presented in this report differ from those of the Preliminary Report (and summarized in Chapter 2) in that the sample in this report includes data through July 2000 whereas the sample for the Preliminary Report included data only through 1999. Second, we do not present any results that compare treatment to controls and restrict attention to treatment vs. population comparisons. This is because in 2000 the number of control group members that had not enrolled in treatment had grown sufficiently small that any meaningful comparison between treatment and control groups is not possible (see Section 3.2 for further discussion of this point).

The results presented in this Final Report clearly demonstrate that average monthly Medicare spending increased much faster in the treatment group than in the population. In two sites (LAH/BNP and VNSNY), the difference in changes was already statistically significant in 1995, whereas it became significant only later at the Carle Clinic and Carondelet sites. All four sites showed a steady increase in estimated differences through time, indicating that average spending in the treatment group kept increasing relative to the population over the course of the demonstration. Not only are the estimates statistically significant, but also of a substantial magnitude: by 2000, average spending increased by $358 more per CNO participant per month in the New York site, and $63 more in the site with the smallest differences, Carle Clinic.

The project team repeated the comparison of CNO treatment beneficiaries to a population reference group two more times, defining the treatment group as described in BIPA. The first alternative definition included anyone enrolled in a CNO as of July 1, 1997 and enrolled for at least six months thereafter. The second alternative definition included anyone enrolled in a CNO as of January, 2000 and enrolled for at least six months thereafter. Results based on these alternative definitions were similar to those described above, providing further evidence of the robustness of the findings.

In addition to the analysis described above, this report also includes results based on the BIPA mandated beneficiary satisfaction survey. Data collected from CNO enrollees by two sites—the VNSNY and Carle Clinic in January, 2001 and May, 2001, respectively—were provided to Abt Associates Inc. and are summarized in Appendix D. An overwhelming majority of enrollees at both sites were satisfied with the care received, thought that the services helped with health needs and problems, felt that their nurse consultant was available when needed, responded that participation was worth their time, and would recommend the program to others.[2] However, results were only obtained for CNO enrollees so no analysis comparing them to those of a control or reference group can be conducted.

[2] **At both sites, at least 77% of respondents agreed or strongly agreed with each statement.**

1. Introduction to the Community Nursing Organization Demonstration and Evaluation

The Community Nursing Organization (CNO) Demonstration is an innovative approach to the provision of community nursing and ambulatory care services for Medicare beneficiaries. Structured around the two fundamental concepts of nurse case management and capitated payment, CNOs attempt to promote the timely and appropriate use of community health services and to reduce the use of costly acute-care services.

The impetus for developing the CNO model stemmed from limitations in traditional fee-for-service Medicare. Parts A and B of Medicare only reimburse care that is ordered by a physician and supplied by certain providers under certain specified conditions. The Medicare program generally has no provision for reimbursing preventive care, health promotion, or care not authorized by a physician, services that might lead to lower medical costs and improved health outcomes for Medicare beneficiaries. Since 1985, many Medicare HMOs have aimed to compensate for these limitations by providing a broader and more flexible array of services, in return for a fixed monthly payment for each subscriber. However, many Medicare beneficiaries are reluctant to join HMOs, since the organizations typically restrict members' choice of providers.

The CNO concept thus provides an alternative to both traditional fee-for-service Medicare and Medicare HMOs. Like HMOs, CNOs are funded by flat monthly Medicare payments for each enrolled member and are responsible for operating within that budget, but can exercise substantial discretion in organizing care in the most efficient and productive way. Since only a limited range of services is covered by the capitation payment, beneficiaries are still able to choose their providers, notably physicians, hospitals and other facilities in the same manner as all other Medicare fee-for-service beneficiaries. However, CNO nurses coordinate the provision of health care services for each enrollee with a strong focus on prevention and disease management, thus attempting to avoid higher future health care costs (Storfjell, 1997; Schraeder, 1997; Ethridge, 1997).

1.1. Background on the CNO Demonstration

In order to explore the impact of this model of care delivery on cost and outcomes, the CNO Demonstration was created by the Omnibus Budget Reconciliation Act (OBRA) of 1987. To carry out this demonstration, the Centers for Medicare and Medicaid Services (CMS) entered into cooperative agreements in 1993 with the following four eligible organizations to serve as demonstration providers:

- **Carle Clinic, Urbana, IL,**
- **Carondelet Health Care, Tucson, AZ,**
- **Living at Home/Block Nurse Program (LAH/BNP), Minneapolis, MN, and**
- **Visiting Nurse Service, New York, NY (VNSNY).**

OBRA, 1987 also mandated an evaluation of the CNO demonstration. Abt Associates Inc. was awarded a contract to provide technical assistance to the sites and to evaluate the effects of the demonstration from January, 1994 until July, 1997. The evaluation, based on randomized assignment of applicants to either CNO participation or traditional Medicare coverage, was aimed at addressing two fundamental questions:

1. **Were beneficiary outcomes such as health status, physical functioning, and satisfaction with health care improved as a result of enrollment in the CNO?**
2. **What were the implications of the CNO demonstration on Medicare program costs?**

The evaluation design permitted especially strong answers to these questions because applicants to the CNOs were randomized to treatment (CNO) or control (traditional Medicare) groups. Abt Associates Inc. reported the results of the CNO Evaluation in a Second Interim Report on April 6, 1998 and in a Final Report on April 13, 2000.

Telephone survey responses to questions designed to estimate the impact of the intervention on overall physical and social functioning found only small and generally insignificant differences in functional status between treatment and control groups at 15, 27, and 39 months after randomization. Total Medicare expenditures were found to be significantly higher among treatment group beneficiaries than among those assigned to the control group. This result held regardless of whether the treatment group was defined as all beneficiaries randomly assigned to treatment (the "intent-to-treat" model) or was defined as beneficiaries assigned to treatment and actually enrolled in the CNO. These results were in close agreement with those of other studies of Medicare risk HMOs carried out during the 1980s and 1990s (e.g. Brown et al., 1993) and were consistent with favorable selection into the CNOs.[3]

Questions regarding beneficiary satisfaction and health education also were analyzed in the Final Report. Survey responses at 27 and 39 months following random assignment were examined, indicating no superior outcomes at 27 months and some improved satisfaction associated with assignment to the CNO at 39 months. Treatment group respondents reported 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 who were not satisfied may have disenrolled by the 39-month follow-up, skewing the data.

The Medicare, Medicaid and SCHIP Benefits Improvement and Protection Act (BIPA) of 2000 directed the Secretary of Health and Human Services to investigate whether results from the CNO demonstration might change in the long run, since gains from the preventive efforts of the CNOs might take time to materialize. BIPA also mandated that CNO sites conduct a beneficiary satisfaction survey. At the same time, BIPA reduced the inflation-adjusted capitation rates to be paid to the sites (by 15 percent in New York and 10 percent at the other sites) and replaced the previously mandated evaluation with a Preliminary Report due no later than July 1, 2001 and a Final Report no later than July 1, 2002. The Preliminary Report was prepared by Abt Associates Inc. and summarized the evaluation of the extension of the CNO

[3] **Age-adjusted mortality rates were lower for CNO applicants than for eligible non-applicants residing in the same localities.**

Demonstration (known as Phase II) from January, 1994 through December, 1999. This Final Phase II Evaluation Report adds seven more months to the study, concluding in July, 2000.[4]

1.1.1. Participating Sites

Through a competitive selection process, CMS chose four diverse sites to set up CNOs for the demonstration:

- **Carle Clinic in Urbana, IL, a for-profit private physician group practice;**
- **Carondelet Health Care in Tucson, AZ, a Catholic, non-profit, full-service health care corporation;**
- **Living At Home/Block Nurse Program (LAH/BNP) in Minneapolis, MN, a community-based nursing program for the elderly, in partnership with HealthSpan, the largest home health agency in the state; and**
- **Visiting Nurse Service of New York (VNSNY) in New York City, the largest non-profit Medicare certified home health agency in the United States.**

Each site had considerable freedom in how it chose to organize itself. As long as the mandatory services were provided and the basic OBRA, 1987 guidelines were followed, sites could individually determine the most efficient and productive ways to serve their members. Considerations addressed by each CNO included:

- **Their relationship with the sponsoring organization, e.g. how would the sponsor benefit from the CNO demonstration;**
- **The optimal location for their sites;**
- **How to recruit members (what would appeal most to applicants in the local community?);**
- **How to maintain the financial viability of the project;**
- **How to define the roles of the Primary Nurse Providers (PNP) and other staff members;**
- **How to coordinate the provision of services through physicians and contracted providers (since the CNO itself did not provide physical therapy, home health care, durable medical equipment, etc.);**
- **How to connect enrollees to available community services;**
- **How to standardize the authorization of services for enrollees; and**
- **How to encourage the continued participation of enrollees in the CNO.**

Since the sites represent diverse locations and clienteles, they have responded in a variety of ways to these considerations. Below each CNO site is briefly discussed, highlighting the manner in which it chose to fulfill the OBRA, 1987 mandate.

Carle Clinic CNO

Carle Clinic, the sponsoring organization for the Carle Clinic CNO, is a for-profit, private physician group practice with a large ambulatory nursing component. Serving nearly 2,500

[4] **Medicare outpatient data for August through December, 2000 were unavailable at the time of preparation of this report due to the implementation of a new prospective payment system for outpatient services. Thus, the inclusion of a full year of additional data was not possible.**

patients daily, the Carle Clinic organizations act as the regional medical center for the primarily rural population of Central Illinois and Western Indiana. The Carle Clinic system is designed to provide primary care through a network of clinics, each using local community services and networking with local providers.

By mid-demonstration, the Carle Clinic CNO was operating 7 sites that served predominantly rural areas, with health services provided by 13 PNPs. Carle Clinic PNPs provided direct care and case management, and they tended to be paired with physicians or assigned to groups of physicians who provided a wide range of services, including services to non-CNO enrollees. PNPs who served higher-risk enrollees had smaller, more specialized caseloads than PNPs serving low-risk clients. By mid-demonstration, seven case assistants (CAs) supported the PNPs by doing administrative work and monitoring low-risk enrollees by telephone. Because of the rural clientele served by this CNO, the Carle Clinic PNPs relied more heavily on telephone monitoring of their patients than on in-person visits; there were also fewer opportunities to "drop in" here than at the other CNOs. Some of the contracted providers for the demonstration were affiliated with Carle Clinic while others were not.[5] Finally, as in other rural areas, managed care penetration in rural Illinois was low during the demonstration, making beneficiaries less familiar with managed care practices than they were at the other demonstration sites.

Carondelet Health Care CNO

Carondelet Health Care (CHC), the sponsoring organization for the Carondelet CNO, is a Catholic, non-profit, full-service health care corporation that has operated in southern Arizona for more than 100 years. By mid-demonstration, the Carondelet CNO had 21 community sites at a variety of locations including senior centers, clinics, mobile home parks, and housing units. All of these sites were accessible to both CNO and non-CNO enrollees. The CNO utilized some of CHC's nurse case managers, community health centers, outpatient services, and its home health agency. Most of the contracted providers for the demonstration were affiliated with CHC, although there were no formal relationships between PNPs and CHC physicians.

Two distinct types of nurses worked as PNPs: 1) nurse case managers, usually nurse practitioners, who traditionally worked with higher risk individuals who were hospitalized or home-bound; and 2) nurse partners, usually RNs, who worked in the community with lower risk individuals. If the low-risk clients moved into a higher risk category, they were assigned to a nurse case manager.

The Tucson area in which the Carondelet CNO operated had the most competitive managed care environment of the four sites. Several managed care programs competed directly with the CNO. The area was also characterized by populations of retirees who, because of seasonal migration out of the service area, periodically enrolled and disenrolled, according to the rules of the CNO. In the latter part of the demonstration, the CNO expanded to include the largely Hispanic populations in southern Arizona.

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[5] **"Contracted providers" refers to any agencies authorized by the CNO to provide direct health services to CNO enrollees, such as physical therapy, durable medical equipment, home health care, etc.**

Living at Home/Block Nurse Program CNO

The Living At Home/Block Nurse Program Inc. (LAH/BNP) is a community-based initiative that was first piloted in St. Paul, MN in 1982 and has grown to have thirteen programs across Minnesota. The first program was started when community residents organized to care for the elderly in the community, implementing case management services for which there was no Medicare reimbursement. To set up the CNO, LAH/BNP formed a contractual relationship with HealthSpan, the largest Medicare certified home care agency in the state. HealthSpan provided the CNO with nursing staff, financial services, and home care services, as well as durable medical equipment. PNPs had to forge their own relationships with physicians in the community.

The CNO opened two rural and two urban sites, all of which served CNO enrollees exclusively. By mid-demonstration, eight PNPs were each assigned to one of the sites to provide direct care and case management services. Each nurse worked with a mixture of high- and low-risk individuals. The CNO incorporated the LAH/BNP principles of self-governance by community members, including an advisory committee and an emphasis on volunteers. Each CNO site employed a community coordinator to assist with non-health services and coordinate the volunteers. There were over 200 volunteers working for the sites, and more than 10 percent of them are CNO enrollees.

The Minneapolis/St. Paul area had higher managed care penetration than the rest of Minnesota. However, the HMOs in this area tended to be non-profit entities and not as competitive as those in the Tucson area. HealthSpan was an experienced player in this particular market.

Visiting Nurse Service of New York (VNSNY) CNO

Visiting Nurse Service of New York is the largest non-profit Medicare-Certified Home Health Care Agency in the nation, providing more than 1.2 million professional visits annually to residents of New York City. By mid-demonstration, the VNSNY CNO had 28 urban sites, all in Queens, NY. Sites were located in various organizations, such as senior centers or housing units, that were accessible to both CNO and non-CNO enrollees. Each enrollee was assigned to one of the ten PNPs during the initial assessment, and for many enrollees, the PNP served as their main primary care provider. The PNPs carried a mixed caseload of high- and low-risk patients, and had "office hours" at the different sites during which enrollees could easily drop in.

VNSNY CNO enrollees tended to be older and sicker than enrollees at other sites. Many of them lived alone, and some had psychological problems, as in the case of enrollees who were Holocaust survivors. This CNO therefore had a heavier emphasis on psychological services than other sites. A member services assistant at the central office would identify community resources for enrollees, but PNPs had the main responsibility for referring enrollees to community services. The VNSNY CNO enrollees were reluctant to relinquish access to services that they believed they deserved or could obtain elsewhere. Physicians and other contracted providers, such as physical therapists, tended to respond to this environment by being independent and competitive, presenting some challenges for the VNSNY CNO.

The New York City area has traditionally been resistant to managed care, in comparison to other parts of the state. During the demonstration, HMOs in the New York City area became

increasingly interested in the use of mid-level and non-traditional providers that might appeal to a managed care-resistant population, but most of these initiatives appear to have been terminated for financial reasons.

Although fully operational during the time period covered by this study, all four CNO sites shut down by the end of 2001.

1.1.2. Eligibility and Enrollment

All Medicare beneficiaries residing in catchment areas close to the CNOs, who were entitled to benefits under Part A and who were enrolled in Part B of Medicare were eligible to enroll in the CNO, with the following exceptions:

- **beneficiaries enrolled in Medicare risk HMOs,**
- **beneficiaries receiving care under the Medicare hospice benefit, and**
- **beneficiaries entitled to Medicare under the End Stage Renal Disease (ESRD) benefit.**

Each CNO site was required to hold at least one open enrollment period during the operational phase of the demonstration and to accept any eligible beneficiary who applied for membership. Initially, those accepted into the demonstration were randomly assigned to treatment and control groups for the evaluation. In later phases of the demonstration, direct enrollment into the treatment group and switching from control to treatment group became possible.

CNO members were allowed to disenroll at the end of a calendar month for any reason. No enrollee could be forced to leave the CNO due to high service use. However, under the following conditions, a CNO was required to disenroll a member:

- **failure to maintain enrollment in Parts A and B of Medicare,**
- **institutionalization for 60 or more consecutive days (changed to 30 days in 1998),**
- **enrollment in a Medicare risk HMO,**
- **use of the Medicare hospice benefit,**
- **residence outside of the CNO service area for more than 30 consecutive days,**
- **persistent use of out-of-plan care for CNO mandatory services while enrolled in the CNO, or**
- **refusal of mandatory six-month assessment.[6]**

Sites began randomization and enrollment on January 1, 1994 with the expectation that the demonstration would last for three years. In 1996, CMS extended the CNO demonstration and evaluation for an additional year. The Balanced Budget Act of 1997 subsequently granted a further two-year extension for the project. Most recently, the Balanced Budget Refinement Act of 1999 granted an additional two-year extension, authorizing the sites to continue operating until December 31, 2001.

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[6] **In some cases beginning in 1998, payment rates were set to the lowest value instead of disenrolling the beneficiary.**

1.1.3. Covered Services

OBRA, 1987 required that certain services be provided as part of the CNO service package. These services were further clarified by contracts between CMS and the four CNO sites to include:

- ***Home health services*** **as defined in 42 CFR 409.40-409.42, provided by qualified personnel who meet the qualifications specified in 42 CFR 484.4. Home health services are traditional Medicare covered home health agency services or comparable level CNO services, which may be authorized by either a physician or a CNO nurse, furnished to home-bound patients. These services include:**

  - **part-time or intermittent nursing care provided by or under the supervision of a registered professional nurse;**
  - **physical, speech, and occupational therapy;**
  - **medical social services supportive plan of care; and**
  - **part-time or intermittent services of a home health aide furnished under the supervision of a registered nurse.**

- ***Medical supplies, appliances, and devices*** **as defined in 42 CFR 410.36, including:**

  - **surgical dressings, and splints, casts, and other devices used for reduction of fractures and dislocations;**
  - **prosthetic devices, other than dental, that replace all or part of an internal body organ, including colostomy bags and supplies directly related to colostomy care; and**
  - **leg, arm, back, and neck braces and artificial legs, arms, and eyes.**

- ***Durable medical equipment*** **as defined in 42 CFR 410.38, to be used in the patient's home.**

- ***Ambulance services*** **as defined in 42 CFR 410.40, when:**

  - **medically necessary because other means of transportation would endanger the beneficiary's health;**
  - **the enrollee is not a hospital inpatient; and**
  - **the transportation is not by air or water.**

  (Ambulance service was removed from the CNO package on February 1, 1997.)

- ***Outpatient physical therapy services*** **as defined in 42 CFR 410.60.**

- ***Outpatient speech pathology services*** **as defined in 42 CFR 410.62.**

- ***Medical supplies*** **(other than drugs and biologicals) furnished while an enrollee is under a plan of care, if the supplies are of the type that are commonly furnished in a physician's office or clinic and are commonly furnished either without charge or included in the physician's or clinic's bill.**

- **Services furnished by a *clinical psychologist* who meets the qualifications specified in 42 CFR 410.71(d), or a *clinical social worker* as defined in section 1861 (hh) of the Social Security Act, as well as services and supplies furnished as an incident to their services.**

- **Part-time or intermittent *nursing care* and related medical supplies (other than drugs and biologicals) furnished by a registered professional or licensed practical nurse employed or under arrangement with a Medicare certified *rural health clinic*.**

- ***Case management services* defined as services which assist enrollees in gaining access to and coordinating/approving utilization of needed medical, social, educational and other services. In the CNO, this service must include providing an in-person assessment and updating the patient's care plan every six months. This service also includes coordinating these services with other providers and monitoring the enrollee's progress towards the achievement of objectives specified in the patient's CNO plan.**

Thus, expenditures were classified as CNO or non-CNO expenditures for the purposes of the evaluation, as described in Table 1.3.1.

Table 1.3.1

Allocation of Medicare expenditures to CNO and non-CNO services

CNO Service Package ("in-bundle")	*Non-CNO services ("out-of-bundle")*
CNO capitation payments	**Inpatient hospital (short and long stay)**
CNO case management payments	**Hospital outpatient**
Home health care (6 disciplines)	**Skilled nursing facility**
Outpatient physical therapy	**Hospice**
Durable medical equipment	**Physician office visits**
Prosthetics/orthotics	**Physician other**
Supplies	**Part B other (lab, ancillary, other)**

Note that enrolled members of the treatment group should have little or no in-bundle costs except for capitation and case management payments.

Source: Abt Associates coding algorithms

1.1.4. Capitation and Case Mix Adjustment

Each CNO received a monthly payment for each enrolled member. Payment amounts were based on the local average annual per capita cost for Medicare-covered services that were part of the CNO's package. These rates in turn were adjusted for case mix as directed by OBRA. In all sites, payments were adjusted for age, sex, and number of Medicare-covered home health

visits in the previous six months. In three of the sites (AZ, MN and NY) payments were further adjusted for the number of limitations in activities of daily living (ADLs) experienced by the enrollee. This resulted in a total of 39 different payment levels, also called payment cells, for those three sites. Payments to the Carle Clinic (IL) site were not adjusted for ADL limitations and were based on 13 payment cells. Following each 6-month reassessment, enrollees were reassigned to the payment cell appropriate for their current age, home health utilization, and (in three sites) number of ADL limitations. The total payment that a CNO site received per member per month consisted of this risk adjusted capitation fee plus a case management fee that was the same for each enrollee and each site.

1.1.5. Case Management

Aside from the requirement that every CNO member be evaluated in person at six-month intervals, each of the CNO sites was free to define and configure the process of case management in the way it judged to be most beneficial to the member and efficient for the organization. Methods of assessment, resources devoted to planning and monitoring, as well as the number of members whose care was actively managed, therefore, differ from site to site. Although the benefits and cost effectiveness of case management for the frail or chronically ill are fairly well established (Cohen, 1991), the value of case management in the broader population of the "generally well elderly" remains unknown. Because the demonstration has only four sites and because the case management intervention was not experimentally varied across sites or individuals, the evaluation was unable to distinguish the distinct effects of capitation and case management on beneficiary outcomes, utilization, or cost.

1.1.6. Recruitment and Intake

Each site developed its own strategy for marketing and recruitment of eligible beneficiaries. All sites relied on physician referrals, direct mail, and word of mouth. Some sites also used brochures, fliers, group presentations, television and newspaper advertising, and telemarketing efforts. Because the demonstration was conducted as an experiment, with random assignment to treatment or control groups, it was important that beneficiaries who expressed interest in the program understood that there was a certain probability that they would be assigned to a control group and not be enrolled in the CNO. Sites were therefore required to secure informed consent from each applicant. The consent document informed applicants

1. **that the CNO was a temporary demonstration project,**
2. **that, if enrolled, they must agree to receive all care in the CNO service package *only* from the CNO,**
3. **that they would be enrolled in the CNO only if assigned to the treatment group, and**
4. **that they would be contacted by Abt Associates Inc. for telephone interviews at one-year intervals.**

After securing informed consent from the applicant, a CNO staff person conducted a baseline interview with the applicant. The interview elicited information on health, mental status, functional limitations, health risk, demographic characteristics, and attitudes toward health providers and satisfaction with care. Applicants were randomized after the interview. Applicants assigned to the control group were thanked for their participation and informed that they could not receive services from the CNO. If necessary, applicants assigned to the

treatment group were further assessed to facilitate care planning and case management, and were enrolled in the CNO.

1.2. Design of the CNO Evaluation

The evaluation of the CNO Demonstration was designed so that the impact of the intervention could be readily measured. Implementation of any novel approach to health care delivery is, however, a dynamic process where theoretical design concepts may sometimes be altered to accommodate operational constraints. CMS, the sites, and the evaluation contractor collaborated in an effort to balance issues related to implementation with issues related to evaluating the effect of CNOs on outcomes and cost. The compromises that were necessary and their implications for the evaluation are discussed below.

1.2.1. Experimental Design

In order to develop the most precise estimates possible of the impacts of the CNO intervention, the demonstration was structured as a social experiment in which individuals were randomized to either CNO participation (the treatment group) or to continue to receive their traditional Medicare benefits (the control group). However, given that participation in the CNO was voluntary, the decision to apply was likely to be influenced by hard-to-measure factors that also influence health outcomes and cost. The subset of the Medicare population that wished to participate in the CNO was likely to differ from those who had no interest in joining the CNO. Thus, the only way to create a valid comparison strategy was to do so *after* the decision to participate in the evaluation had been made so that only those who wished to participate could be compared. Applicants were randomly assigned to treatment or control status after the decision to apply had been made, a consent statement had been signed, and collection of baseline data had occurred.

1.2.2. Implementation of Random Assignment

To accommodate the program's need to build up enrollment quickly, two applicants were assigned to the treatment group for every applicant assigned to the control group. The fact that the control group was smaller than the treatment group reduced the statistical power of the evaluation, increasing the size of the minimum impact that could be detected reliably. In determining what proportion of applicants to enroll in the CNOs, the size of the impact that could be detected (and therefore the threshold for being considered a *significant* impact) was balanced against the sites' need to recruit more participants.[7]

To avoid potential bias on the part of the CNO site staff, who conducted the baseline assessments, baseline data on the CNO applicants were collected *before* the applicants were

[7] **For example, it was estimated that an assignment ratio of 2:1 meant that an 8 percent reduction in the rate of inpatient admissions could be detected with statistical power of .71 (at a .10 significance level), assuming total enrollment of 4,800 (3,200 in the treatment group and 1,600 in the control group). Allocating to treatment and control groups using a 1:1 ratio would have allowed a smaller impact to be detected with comparable power, but would have resulted in only 2,400 treatment participants, unless sites recruited a larger total number of applicants to yield the same number of enrollees (3,200).**

randomized. Thus while they were performing the baseline assessment, the assessors did not know whether the applicant would in fact be able to enroll in the CNO. In order to facilitate and control the randomization process, Abt Associates Inc. developed a centralized CNO Random Assignment System (CNORAS) maintained at Abt Associates' offices in Cambridge, MA. After the baseline was performed, site staff were able to dial into this database via laptop computer and modem and to enter basic data on each new applicant. The system then assigned each applicant to the treatment or control group and gave them a unique identifier. Site staff could copy down the identifier and the assignment and enter it in site records. If an enrollee was already in the database, the system indicated his or her existing identifier and treatment/control assignment status.

Members of the same household who applied to the CNO were automatically assigned to the same treatment/control status. This was done to avoid problems in service delivery within the household and the likelihood of control group members benefiting from CNO services provided to treatment group members in the same household. To facilitate this assignment, site staff identified the potential eligible members of each applicant's household; these were termed Qualified Household Members (QHMs). Data on all QHMs were entered into the CNORAS, even if they were not applying to the CNO. QHMs who later decided to apply would hence automatically be assigned to the proper group.[8] This led to a slight increase in the ratio of treatments to controls, since QHMs of control group members generally did not apply to the CNO.

1.2.3. Special Situations

The original specifications for the implementation of random assignment called for the following sequence. First, the beneficiary would be recruited by the site and sign an informed consent form accepting participation in random assignment. Then collection of baseline assessment and other data for the evaluation would occur. Once baseline data collection was complete, the randomization assignment would be requested from Abt Associates Inc. Control group members would be informed of their status and have no further contact with the CNO. Treatment group members would be enrolled, receive a clinical assessment, and begin to receive CNO services. Unfortunately for the evaluation, this sequence frequently had to be altered in practice. The most common exceptions are described below.

Randomization Before Baseline Assessment

Three of the four sites lacked laptop computers that would allow staff to call in to CNORAS from applicants' homes. At the outset, site staff protested that it was awkward and inefficient to conduct a baseline assessment, leave and obtain the random assignment, and return at some later date to perform a clinical assessment and develop a care plan for treatment group members. Eventually, it was agreed that CNO office staff could call in to CNORAS for cases that were to be assessed that day, obtain the assignments, conceal them in an envelope, and provide them to the assessment nurse. Once the baseline assessment was completed, the nurse could reveal the random assignment. If the applicant were assigned to the control group, the

[8] **The system allowed site staff to link each applicant with one QHM, which covered the vast majority of situations encountered. Occasionally, an applicant had multiple QHMs. These were reported to Abt Associates Inc. on a case-by-case basis, and Abt Associates Inc. staff established the link in the CNORAS manually.**

nurse would thank them and leave; if assigned to the treatment group, she could continue with the enrollment and care planning process.

Randomization without Baseline Assessment or Enrollment

There were some situations where beneficiaries were randomly assigned but never received a baseline assessment, or were assigned to treatment status but never enrolled in the CNO. This included cases where the beneficiary changed his/her mind about participation after being randomized; where the site assigned the beneficiary to treatment or control status before s/he had agreed to participate; where the beneficiary was determined to be ineligible for the CNO after being randomized; and where the beneficiary died before baseline assessment or enrollment. These cases were relatively rare, but they do occupy "slots" in the CNORAS, and may therefore cause the analysis samples to depart from the 2:1 ratio. Treatments and controls who received no baseline assessment did not receive follow-up assessments from Abt Associates Inc.

Contaminated Controls

In several instances, beneficiaries who were randomly assigned to the control group were inadvertently enrolled in the CNO and received the same services as a member of the treatment group. The intent-to-treat design of the evaluation mandated that these cases were nonetheless analyzed as controls. This was a relatively minor problem before October, 1997. After this date, however, the sites enrolled control group members in substantial numbers.

Hiatus in Randomization to the Control Group

The CNO Evaluation was originally scheduled to end on December 31, 1995. Starting October 1, 1995, all new applicants were "randomized" to the treatment group, since no follow-up assessments allowing comparisons between treatments and controls would have been performed on applicants randomized after that date. In early 1996 CMS modified the original contract allowing the evaluation to continue for another year. At that point, it was decided that the pool of control group members was already sufficiently large and that randomizing a small number of new controls would contribute little to the analysis. Throughout 1996, all applicants were assigned to the treatment group. However, when the demonstration was again extended for two more years, the randomization of new applicants to both treatment and control groups in a 2:1 ratio was resumed. Overall, the hiatus in randomization to the control group lasted from October, 1995 through December, 1996. As a result, all 1,144 CNO applicants during that time period were enrolled as treatments, and the overall ratio of treatments to controls became greater than 2:1. This period is sometimes referred to as Wave 2 of the evaluation, with the initial phase with intact randomization being labeled Wave 1. When it had been decided to continue the demonstration for two additional years, randomization in a 2:1 ratio was resumed from January 1, 1997 until October 2, 1997 (Wave 3). After October 3, 1997, sites again were allowed to enroll applicants without randomization (Wave 4).

1.2.4. Comparison Strategies

This evaluation employs three comparison strategies. The first two are comparisons of mean utilization and expenditures that rely on the experimental design of the evaluation. These strategies are directly comparable to analyses performed for the Phase I Evaluation. Results of these analyses are described in detail in Chapter 3 of the Phase II Preliminary Report and summarized in Chapter 2 of this report. The third strategy was added to address the fact that

significant numbers of beneficiaries enrolled in the CNOs during periods when randomization did not occur. These enrollees have no appropriate control group, so an alternative reference group had to be constructed and comparisons had to be adjusted for known differences between the treatment group and the reference group.

The primary analytic strategy for evaluation of CNO effects has been the "intent to treat" approach commonly employed in the analysis of clinical trials (Lachin, 2000). This method compares Medicare expenditures for beneficiaries assigned to the treatment group with expenditures for those assigned to the control group regardless of whether or not those assigned to treatment remained in the CNO for the entire follow-up period. This comparison strategy is typically selected for randomized studies in order to emulate real world conditions and to thus make results generalizable. Were a CNO program to become part of the Medicare benefit, it is most unlikely that beneficiaries would be compelled to remain in the CNO once they had joined. Rather, beneficiaries would be permitted to leave the program and some would do so, just as they did in the demonstration. Given this expectation, it would be unrealistic to compare only months in which beneficiaries were actually enrolled. The treatment/control difference thus answers the question, "For beneficiaries likely to enroll in a CNO program, what is the average monthly saving to Medicare of giving them the option to do so?" In other words, the treatment/control contrast is meant to estimate the effect of assignment to the treatment (CNO) group on the trajectory of Medicare expenditures regardless of future events that might lead beneficiaries to leave or become ineligible for the CNO.

A second comparison strategy was added upon request by the CNO sites, which contrasts the control group with only the months in which a beneficiary was actually enrolled in a CNO. The rationale behind this enrollee/comparison group contrast was to provide an upper bound of what effect CNO enrollment could have had, if all treatment group members had remained enrolled in a CNO. However, the risk of differential attrition causes this contrast to be problematic. Participants randomized to treatment who experience deteriorating health during the study might choose to disenroll disproportionately because they felt constrained in their choice of providers. Under this assumption, only a relatively healthy subgroup remains enrolled in the CNO, whereas the control group's average health status does not change. The remaining enrollees will be on average healthier than the control group members, and will therefore have lower expenditures and better outcomes. As one cannot adequately account for those disenrollment decisions, it is not possible to quantify whether differences between the treatment and control groups are caused by a treatment effect or by differential attrition. Thus, if this contrast is reported, it will overstate the beneficial effect of the CNOs on cost and outcomes.

A final comparison strategy contrasted CNO enrollees to Medicare beneficiaries who lived in the same geographic area but never applied to a CNO. Since the initial evaluation focused on enrollees from Wave 1 of the demonstration, during which the randomization design was fully intact, a simple comparison of means was sufficient to identify the effect of the intervention. However, this project analyzes enrollees from Waves 2 and 4, during which study participants were enrolled without a corresponding control group. In addition, as all participants randomized to control status were given the option to switch to treatment after October 3, 1997, some of the original controls are now "contaminated" by virtue of having later joined the CNO. Hence estimates computed from post-1997 data will inevitably be less protected against bias due to self-selection than will estimates computed with 1994-1997 data. Our tabulations of the CNO Enrollment File indicate that approximately 18 percent of applicants originally assigned to the

control group subsequently enrolled in the CNO. This share is low enough that the original gains from randomization might not have been fully lost through its subsequent abandonment. However, the need arises to test this hypothesis by constructing a reference group from Medicare beneficiaries who never applied to a CNO. The selection of this reference group is discussed in Chapter 3, and details on our non-experimental comparison strategy are presented in Appendix A.

1.3. Data Sources

For this report the project team analyzed data from the following sources:

- **Medicare enrollment and claims files from CMS,**
- **CNO Enrollment and Payment Files maintained by CMS staff overseeing the demonstration, and**
- **Hierarchical Coexisting Conditions (HCC) files constructed by Fu Associates,**

1.3.1. Medicare Enrollment and Claims Files

Medicare service utilization and eligibility information was obtained from databases maintained by CMS. The National Claims History Database (NCH) contains Part A and Part B claims records, including line item information on all services provided, for all claims since October 1, 1990. To analyze each beneficiary's utilization of Medicare services, we collected the following information from the Inpatient, Skilled Nursing Facility (SNF), Outpatient, Home Health Agency (HHA), Hospice, and Physician/Supplier claims files for all randomized beneficiaries:

- **Beneficiary identification numbers (Medicare health insurance claim numbers (HICN)),**
- **Provider identification numbers,**
- **Dates of service,**
- **Type of claims (inpatient, SNF, outpatient, HHA, hospice, physician/supplier, etc.),**
- **Units of service,**
- **Submitted charges,**
- **Allowed charges,**
- **Reimbursement amount,**
- **Coinsurance and deductible amounts,**
- **Type of service codes,**
- **Place of service codes,**
- **Diagnosis codes,**
- **Procedure codes.**

Claims records were collected for the years 1993 to 2000 for all demonstration participants and the population-based reference group.

For members of the control and population reference groups, Medicare claims files provided information on cost of all services, regardless of whether or not they were part of the CNO package. Since the CNO package services were paid prospectively by the capitation fee for the

treatment group, the CNOs assumed full financial risk and did not bill Medicare for these services. The Medicare claims files, therefore, should only have contained information on cost of services that were not part of the CNO package, such as physician or hospital services. However, we discovered that some members of the treatment group had obtained CNO package services outside of their CNO. This was possible because of the absence of a lockout mechanism that would have prevented payments from out of plan use of services. Instead, CMS staff followed such out-of-plan use and recouped the capitation payment for every month in which out-of-plan reimbursements exceeded $120 ($100 in 1995).

The CMS Enrollment Database (EDB) contains demographic data elements as well as the entitlement status of all Medicare beneficiaries. The project team used this information to determine beneficiaries' eligibility for this demonstration. The EDB provided the following types of information about Medicare enrollees:

- **Identification numbers (Medicare HICN),**
- **Demographic information (date of birth, sex, race, state, county, zip code),**
- **Date of death,**
- **Medicare Part A entitlement and/or Part B enrollment and termination dates,**
- **End Stage Renal Disease (ESRD) entitlement,**
- **Disability entitlement,**
- **Group Health Organization start dates, end dates, and lock-in codes,**
- **Hospice start and end dates.**

1.3.2. CNO Enrollment and Payment Files

These files were maintained by CMS to determine CNO eligibility both at the time of enrollment and on a continuous basis during the demonstration. They also provided information on how many individuals were actually enrolled at each CNO site, by month. In addition, these files contained capitation rates, group cell categories to which enrollees were assigned, and corresponding assessment dates. The enrollment files allowed CMS to keep accurate eligibility records, and were necessary for CMS to determine capitation payments for the CNOs each month. The same CMS project staff maintained files reflecting adjustments to the capitation payments resulting from out-of-plan use as discussed above.

1.3.3. HCC Files

As mentioned earlier, because of the departure from a strict randomized design there is no control group for enrollees from Waves 2 and 4 of the demonstration. Thus, to be able to analyze the effect of the intervention on all participants, a reference group had to be constructed. Because of potential differences in average health status between the treatment group and the reference group, a risk adjustment technique was needed to separate the potential demonstration effect from these background differences. Hierarchical Coexisting Conditions (HCCs) are a prominent method for this kind of risk adjustment, as they are groups based on ICD-9 diagnoses that are predictive of future utilization of health care (Ellis et al., 1996). HCC scores for both demonstration participants and the population-based reference group were calculated by Fu Associates, under a direct contract with CMS, and made available to the project team.

1.4. Incentives and Expectations

Experiments with delegation of healthcare decision-making and authority delivered under a capitation arrangement usually aim at familiar goals—either enhancing health and well-being without substantially increasing cost, or reducing cost with no measurable sacrifice in health, functioning, or satisfaction. This naturally leads us to ask what scope of action was available to the CNOs to effect improvements in cost and outcomes.

The CNO demonstration altered the provision of ambulatory care to the treatment group in two ways. First, the CNOs assumed full financial risk for all care in the CNO service package, in return for a monthly capitation payment for each enrollee. Second, the CNOs provided nurse case management to all enrollees, including in-person assessments for all members at six-month intervals. These alterations gave rise to three mechanisms by which CNOs could alter directly the manner in which resources were used to maintain and improve the health and functioning of enrollees.

- **The CNO was accorded much greater discretion in the provision of Medicare-covered services. Hence, in principle, the individual needs of an enrollee could be accorded greater importance than under fee-for-service Medicare, which requires determinations of coverage and medical necessity.**

- **The CNO could choose to provide additional services not traditionally covered by Medicare, such as prevention and health promotion, if these were judged to be effective for the enrolled population.**

- **More frequent screening (via the six-month reassessment) could identify some conditions at an earlier point than in its absence.**

Because the literature is a poor guide to the effects of these mechanisms on health outcomes, few clear hypotheses emerge. Two themes, however, stand out from a review of the literature: capitation payments are thought to provide an incentive to reduce the cost of care, and evidence is mixed on whether case management and periodic assessment are likely to improve outcomes.

1.4.1. Capitation and Financial Incentives

CNO services were financed through capitation payments, an arrangement that removes the link between service provision and payment and also affords the CNOs increased discretion in matching services to enrollee needs. CNOs have a financial incentive to provide fewer services than they would if they were paid separately for each service. In the only study to date comparing Medicare home health care under HMO and fee-for-service (FFS) arrangements, Schlenker, Shaughnessy, and Hittle (1995) found evidence that providers responded to these incentives. Among Medicare beneficiaries who received home health care, those who were enrolled in Medicare risk HMOs received fewer home health visits on average than beneficiaries who remained under fee-for-service Medicare, even after adjustment for casemix, location, and demographic characteristics. In a separate article, Shaughnessy, Schlenker, and Hittle (1994) found that these same beneficiaries experienced somewhat better outcomes under

fee-for-service, leading them to argue that "most HMO patients are underserved in terms of the number of home health visits."

It should be noted that the service package and payment structure faced by the CNOs could produce stronger financial incentives to restrict services than those faced by the HMOs studied by Shaughnessy, Schlenker, and Hittle. Most acute care services covered by Medicare (in particular hospital and physician services) were *outside* the CNO service package. Hence in contrast to Medicare risk HMOs, at least some portion of any financial consequences of adverse outcomes resulting from a reduction in services (relative to FFS) would not be borne by the CNO. Consider for example a CNO and a Medicare risk HMO each contemplating the provision of home care costing $200 to a member. Suppose that both providers believe that this care will reduce the probability that the member is hospitalized in the current month from 0.3 to 0.2. Both providers will incur a cost of $200 by providing the care. The expected financial benefit from providing the care is 0.1 times the cost of the hospitalization for the HMO. The expected financial benefit to the CNO is zero. This argument does not imply that the CNO would fail to provide the care in question—only that the *financial* incentives to provide the care are weaker for the CNO than for the HMO.

Although capitation does reduce the incentive to provide services, it also permits greater flexibility for the provision of services that the CNO case manager considers most appropriate, even if the services are not covered by the Medicare fee-for-service program. These may include homemaker services, preventive care, health promotion classes (e.g., smoking cessation, cholesterol and weight control, exercise classes, etc.) or telephone consultations. Therefore while we may hypothesize that the number of Medicare-covered home health visits per month or the proportion of individuals receiving durable medical equipment (DME) will be lower among CNO enrollees than among the control group, this does not imply that enrollees necessarily received fewer total services or that these services are of lesser value or effectiveness than those received by the control group.

1.4.2. Case Management and Periodic Assessment

Whether nurse case management can be expected to markedly improve the health of CNO members or the cost-effectiveness of their care is difficult to predict. The relevant literature provides little guidance on the issue. The benefits claimed for case management are typically rooted in the assertion that health services to a substantial portion of the elderly are heavily fragmented. But evidence that such fragmentation seriously compromises care has been difficult to find because of the paucity of studies directly comparing case-managed and non-case managed elderly populations. There have been several studies comparing alternative approaches to case management (Eggert et al., 1991) or evaluating the internal efficiency of resource use by case managers (Davidson, Muscovice, and McCaffrey, 1989). However, most studies that compare the effect of case management on a treatment group against a control group without case management were limited to psychiatric populations and have thus limited generalizability (e.g., Jerrell and Hu, 1989).

More recently, Burns, Lamb, and Wholey (1996) found that provision of nurse case management services during and after hospitalization to certain high-risk members of a senior risk plan resulted in a significant reduction in subsequent hospitalizations and outpatient visits. A critical feature of the case management system studied by the authors was targeting of

individuals believed to be at high risk. In contrast, Gagnon et al. (1999) recently reported on a randomized trial of case management versus usual care. They found that frail older people receiving nurse case management were more likely to use emergency health services without a concomitant increase in health benefits.

While there is little direct evidence on the subject, a consensus appears to have formed that effective case management requires successful targeting. Eggert et al. (1991) argued that the success of the team model of case management relied in part on targeting a "high use/high cost group." And Kemper (1988) among others, argued that failure to target services properly contributed to the absence of significant results in the Channeling demonstration.

For the most part, the individual CNO sites were free to develop nurse case management and tailor it to the needs of the enrolled population. One element of case management under the CNO, a health assessment, conducted in person every six months, was required for all members. Periodic assessment of the elderly has been examined in several studies with conflicting results. Tulloch and Moore (1979) reported that after two years, a randomly chosen group of patients aged 70 and over showed no significant change in functional or medical disorders relative to a control group. Nevertheless, the authors reported that "there was some evidence to suggest that they were kept independent for longer and when admitted to hospital, their duration of stay was significantly shorter than control group patients." Hendriksen, Lund, and Strømgård (1984) found stronger evidence for beneficial effects of screening in a randomized trial conducted among individuals aged 75 and over in a suburb of Copenhagen, Denmark. Members of the treatment group were visited in their homes every three months. After three years, the treatment group was found to have experienced lower mortality, lower probability of hospital admission, and a strong suggestion of reduced use of emergency medical service. No differences were found in the number of physician visits or home nursing visits. In a similarly designed three-year study, van Rossum et al. (1993) found no effect of home visits four times per year on the health of study subjects aged 75-84. Further analysis of the data, however, identified dramatic treatment effects among those who had initially rated their health as poor. The treatment group averaged 20 hospital days per person over the three-year period versus 39 for the control group.

The aforementioned studies, while suggestive, need not bear directly on expectations for the current CNO demonstration since they described interventions that were more rigid than the current one. To the extent that CNO sites effectively targeted and individualized their prevention and health promotion activities to their served populations, their outcomes and cost-effectiveness could turn out to be superior to those observed in earlier studies. Of particular interest is the fact that each site had substantial discretion about its model of care delivery so that differential effects across sites might be identifiable.

2. Summary of Results from the Preliminary Report

The Preliminary Report to Congress (Abt Associates Inc., 2001) contained two analyses of the impact of the CNO on Medicare expenditure and utilization: one which contrasted CNO applicants randomly assigned to treatment and control groups, and one which contrasted CNO enrollees with the general Medicare population residing in the CNO catchment areas. This chapter reviews results of these analyses, originally presented in Chapters 3 and 4 of the Preliminary Report (and based on data through December, 1999). Chapter 4 of this report provides updated versions (based on data through July, 2000) of the Preliminary Report results that are summarized in this chapter.[9]

2.1. Analysis of Randomized Beneficiaries (Treatment to Control Contrasts)

Between January 1, 1994 and September 30, 1995 and again between January 1 and October 2, 1997, applicants to the four CNO programs were randomized to treatment or control status. The randomized design implemented during these periods afforded an especially accurate estimation of CNO effects. The Preliminary Report used the experience of randomized beneficiaries to estimate the effect of the CNO on total Medicare outlays per month and on utilization of selected categories of service.

The net saving (positive or negative) of the CNO intervention for the Medicare program was estimated by contrasting total Medicare expenditures for the treatment and control groups from the time of randomization through December, 1999. If the CNO capitation rate and case management fee were set to be no greater than the expected value of monthly Medicare outlays for CNO-covered services in the fee-for-service sector and if enrollment in the CNO did not lead to an increase in use of non-CNO (in particular hospital and physician) services, then total Medicare expenditures per-person per-month for the treatment group should be no greater than that of the control group.

Medicare claims for every randomized beneficiary were assembled from the month of randomization until December, 1999 or the month of death, whichever came earlier. Hence a maximum of 72 months of expenditure data for each randomized person were available for analysis. All person-months were deleted from the analysis in which a beneficiary a) was not enrolled in Part A and B of Medicare, b) was enrolled in a Medicare HMO (cost or risk) or Health Care Prepayment Plan (HCPP), c) was resident in a hospice, or d) resided in a state other than Arizona, Illinois, Minnesota, and New York. Expenditures were classified as CNO-covered or non-CNO-covered services, as shown in Table 1.3.1. The treatment and control groups were also compared in terms of a) total Medicare expenditures, b) hospital utilization

[9] **The results presented in Chapter 4 do not coincide exactly with those presented in this chapter in years prior to 2000 due to random sampling of the Medicare population reference group.**

and expenditures, c) emergency room utilization and expenditures, and d) physician office visit utilization and expenditures.

Two separate definitions of the treatment group were used. The first defined the treatment group as all individuals randomized to the treatment group, even if they later disenrolled from the CNO. In so doing, it followed the principle of "intent to treat" under which study subjects are analyzed according to their initial treatment/control assignment regardless of compliance or noncompliance with the experimental intervention. The second defined the treatment group to consist of person-months during which beneficiaries were actually enrolled in the CNO, regardless of the randomization status of the beneficiary. Under this second definition, individuals were retained in the treatment group only for those months during which they were actually enrolled in the CNO. Cumulative expenditures per person per month (PPPM) were computed for both groups by month of enrollment. All dollar amounts were expressed in 1999 dollars using the Consumer Price Index for discounting.

In order to best estimate "mature" CNO effects and to eliminate any downward bias resulting from a preponderance of "early CNO person-months" in the data, expenditures per-person per-month were computed after deleting all data for the first six months after randomization for all beneficiaries and after deleting data for 1994, the first year of CNO operation. If "startup effects" on either beneficiaries or the CNOs themselves were of substantial magnitude, then treatment/control differences computed on this pared-down sample may be a more accurate estimate of long-term CNO effects.[10]

Results are shown in Table 2.1 (reproduced from Table 3.3.2 of the Preliminary Report). Over the first 72 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 relative difference in total expenditures per month between the treatment and control groups varied from seven percent at Carondelet, to over 13 percent at the Illinois and Minnesota sites. Although expenditures for non-CNO services were comparable for treatment and control groups at three of the sites and $7 per-person per-month *lower* for treatments than controls at the Carondelet site, mean expenditures for CNO-covered services were greater for treatments than for controls at every site by amounts ranging from $38 to $55 per-person per-month. These amounts represented relative differences of between 50 and 177 percent. Hence the main impediment to achieving Medicare budget neutrality for the CNOs was the high level of capitation and case management payments. These are evident in the bottom panel of Table 2.1.

In most cases, the discrepancy in total Medicare expenditures between treatment and control groups was substantially smaller when the treatment group was defined as beneficiaries enrolled in the CNO than it was when the treatment group was defined as those randomly assigned to the treatment group. This may have been the result of a tendency for beneficiaries to drop out of the CNO in periods when their Medicare expenditures were especially high. When monthly Medicare expenditures in the six months prior to leaving the CNO by beneficiaries who voluntarily disenrolled were compared with monthly expenditures for CNO enrollees who remained in the CNO, they were found to be two to five times higher.

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[10] **Results were nearly identical when all data were used. See Table 3.3.1 of the Preliminary Report (Abt Associates Inc., 2001).**

Further analysis found no statistically significant differences between the treatment and control groups (or between currently enrolled CNO participants and the control group) in the monthly mean number of hospital admissions, emergency room visits, or physician visits.

Table 2.1

Medicare Expenditure Per Person Per Month, 72 Months After Random Assignment
(first six months after random assignment and months in 1994 excluded)

		Carle	Carondelet	LAH/BNP	VNSNY
Total Medicare Expenditures Per Month					
All randomized beneficiaries	**Treatment**	**$361***	**$487 ~**	**$424***	**$852**
	Control	**$318**	**$456**	**$369**	**$799**
CNO enrollees vs. controls	**Treatment**	**$355**	**$467**	**$398**	**$805**
	Control	**$318**	**$456**	**$369**	**$799**
Services Not Covered by CNO					
All randomized beneficiaries	**Treatment**	**$278**	**$387**	**$338**	**$695**
	Control	**$281**	**$394**	**$338**	**$694**
CNO enrollees vs. controls	**Treatment**	**$264**	**$367**	**$309**	**$634**
	Control	**$281**	**$394**	**$338**	**$694**
CNO-Covered Services					
All randomized beneficiaries	**Treatment**	**$84**	**$100**	**$86**	**$158**
	Control	**$37**	**$62**	**$31**	**$105**
CNO enrollees vs. controls	**Treatment**	**$90**	**$100**	**$89**	**$171**
	Control	**$37**	**$62**	**$31**	**$105**

All figures are in 1999 dollars. Means describe beneficiaries randomized between January, 1994 and September, 1995 and between January, 1997 and October, 1997. Total beneficiary-months used for these computations are shown in Appendix Table B.1 of the Preliminary Report (Abt Associates, Inc., 2001). ~ denotes significance at p<0.1; * p<0.05.

Sources: Abt Associates Inc. CNO Evaluation Analytic Files.

2.2. Analysis of All Demonstration Participants (Treatment to Population Contrasts)

No randomization of CNO applicants occurred between October 1, 1995 and December 31, 1996. During this period, all applicants to the CNO were accepted, as they were after October 2, 1997. Therefore a quasi-experimental approach was adopted to measure CNO effects over the entire period from January, 1994 through December, 1999. A comparison group was drawn from the fee-for-service Medicare population in the CNO catchment area. Regression techniques were used to adjust for differences between the CNO enrollee population and the comparison group. To account for pre-existing differences between the treatment and comparison group that might not be captured by the regression model, the increase in monthly Medicare expenditure for the treatment and comparison groups between 1994 and each successive year from 1995 through 1999 were computed at each CNO site. Table 2.2 shows the difference in growth both for services covered by the CNO (in-bundle services) and for services not covered by the CNO (out-of-bundle services). Positive numbers indicate greater expenditure by CNO members; negative numbers indicate greater spending by the comparison group.

Table 2.2

Estimated Differences between the CNO Treatment Group and the Population Reference Group: Changes in Per Month Spending for In-bundle and Out-of-bundle Services, Relative to 1994

	Carle	Carondelet	LAH/BNP	VNSNY
In-Bundle				
1995	$1	$17 **	$31 ***	$21 *
1996	$15 ***	-$1	$18 *	$17 ~
1997	$14 **	$11	$33 ***	$55 ***
1998	$12 *	$22 **	$36 ***	$105 ***
1999	$0	$14 ~	$52 ***	$96 ***
Out-of-Bundle				
1995	-$1	$3	$87 **	$128 *
1996	$3	-$9	$79 *	$128 *
1997	$22	$22	$67 *	$201 ***
1998	$11	$12	$62	$267 ***
1999	$60 *	$42	$142 **	$255 ***

All dollar amounts are expressed in 1999 constant dollars. All differences are adjusted for beneficiary risk and year effects. The CNO treatment group consists of all those randomized to treatment or directly enrolled in the CNO. Sample sizes are described in Appendix Table C.1 of the Preliminary Report (Abt Associates Inc., 2001). ~ denotes significance at p<0.1; * p<0.05; ** p<0.01; ***p<0.001.

Sources: Abt Associates Inc. CNO Evaluation Analytic Files

For CNO-covered services, the estimates show that average monthly cost in the treatment group increased substantially faster than in the population, and did so steadily over time. The effect is more marked at the LAH/BNP and VNSNY sites, where this difference was statistically significant in every single year and of much higher magnitude than at the two other sites.

However, even at Carle and Carondelet, demonstration costs increased significantly faster than population costs in three of the five years. These results tended to support the conclusion that payment rates for the CNO sites were set too high to achieve budget neutrality.

By contrast, the results for non-CNO services do not match up with the findings from the analysis of the randomized portion of the demonstration. When comparing CNO applicants randomly assigned to treatment or control status, we found that average spending for those services was similar in both groups. In contrast, the results from the entire demonstration suggest that non-CNO spending increased disproportionately for CNO participants over time. To shed further light on this discrepancy, Medicare utilization of three categories of service not covered by the CNO were examined: inpatient care, ER visits, and physician office visits. These results are summarized in Table 2.3.

Table 2.3

Estimated Differences between the CNO Treatment Group and the Population Reference Group: Changes in Per Month Utilization of Inpatient Hospital Admissions, ER Visits and Physician Office Visits, Relative to 1994

	Carle	Carondelet	LAH/BNP	VNSNY
Hospital Admissions				
1995	-0.03	-0.13	0.20 ~	0.10
1996	0.16 ~	-0.14	0.22 *	0.19 *
1997	0.15 ~	-0.05	0.18 ~	0.27 **
1998	0.07	-0.06	0.16	0.41 ***
1999	0.10	0.06	0.20 ~	0.39 ***
ER Visits				
1995	-0.03	0.00	0.33 **	-0.03
1996	0.04	-0.05	0.24 *	0.07
1997	-0.01	0.01	0.28 *	0.09
1998	-0.05	-0.12	0.20 ~	0.17
1999	0.01	-0.03	0.14	0.07
Physician Office Visits				
1995	-0.01	0.01	0.02	-0.03
1996	0.03 **	-0.03	0.01	0.01
1997	0.02	0.01	0.03 *	0.02
1998	0.02	-0.01	0.01	0.00
1999	0.00	0.01	-0.01	-0.02

All differences are adjusted for beneficiary risk and year effects. The CNO treatment group consists of all those randomized to treatment or directly enrolled in the CNO. ~ denotes significance at p<0.1; * p<0.05; ** p<0.01; ***p<0.001. Sample sizes are described in Appendix Table C.1 of the Preliminary Report (Abt Associates Inc., 2001).

Sources: Abt Associates Inc. CNO Evaluation Analytic Files

The probability of a hospital admission increased more for beneficiaries enrolled in the LAH/BNP and VNSNY sites than for the fee-for-service population in the corresponding

catchment areas. There is also evidence of higher hospital utilization for the Carle Clinic treatment group, whereas there was no difference in hospital utilization for Carondelet enrollees, the site that also had the smallest differentials in total non-CNO spending. With the exception of elevated ER utilization at LAH/BNP, the results for physician and ER visits were generally not significant and showed no clear pattern.

The general conclusion that enrollment in the CNO was associated with higher Medicare expenditure than would have occurred in the absence of CNO enrollment is thus supported by both the experimental and the quasi-experimental designs. The experimental design, however, ascribes the increase entirely to higher payments for CNO-covered services, while the quasi-experimental design found the largest share of the increase occurring in non-CNO-covered services. The difference in results may result from systematic differences in the providers serving CNO applicants (in both treatment and control groups) and the general Medicare population in areas served by the CNOs.

3. Description of the Phase II Sample

3.1. Identifying Eligible Beneficiaries

For members of the treatment, control, and population-based reference groups, demonstration eligibility had to be assessed every month over the course of the observation period. Though not essential for the evaluation, monthly eligibility assessment was necessary for operational reasons: since CMS was paying CNO sites monthly capitation and management fees for enrolled participants, it was important to ascertain how many beneficiaries were actually enrolled and eligible to remain enrolled each month. Since CMS staff performed this screening only on CNO enrollees, we set out to maintain comparability by excluding from our analysis all claims and utilization data from months during which the beneficiary would have been deemed ineligible had they been enrolled. However, identifying and excluding such ineligible months presented a challenge because CMS staff decisions for enrollees were based on direct exchanges with site staff so the underlying information was quite accurate. Since the sites did not follow control group participants or treatment group members who were not currently enrolled, let alone the population reference group, the same information was not available for them.

To address this problem when it first arose, the initial evaluation project team constructed a computerized algorithm to screen the control group in a way that emulated as much as possible CMS staff's eligibility decisions for the treatment group. The algorithm had a separate component that attempted to identify months potentially ineligible for each of the following reasons:

- **Medicare entitlement based on end-stage renal disease only,**
- **Ineligibility for either Part A or Part B,**
- **Enrollment in a Medicare risk HMO,**
- **Enrollment in a hospice,**
- **Residency in a nursing home for more than 60 days (30 days beginning in 1998), and**
- **Residency outside the catchment area for the CNO.**

For many of these criteria, however, the algorithm could produce only an imprecise estimate of the eligibility as determined by CMS staff. For example, it proved extremely difficult to emulate residency in a nursing home on the basis of Medicare administrative data, since there is no explicit variable for nursing home residency available. Furthermore, since Medicare does not pay for lengthy stays in a nursing home but only for post-acute stays in a skilled nursing facility, there are no Medicare claims for long-term nursing home care. The only proxy for nursing home residence of more than 60 days that could be constructed from Medicare data was based on at least one claim for the beneficiary that detailed a skilled nursing facility as place of service. In contrast, CMS staff were able to ask each beneficiary's caseworker directly and get more accurate information on actual nursing home stays. Given the differences between the effects of the algorithm and of CMS staff decisions, the eligibility screen had to be applied to the treatment group as well as the control group for consistency. Consequently, some treatment group months that had been deemed eligible by CMS staff were excluded by the algorithm, but comparable months were also excluded from the control group. In order to define the best

feasible eligibility screen, we evaluated the performance of each of its components on two criteria, using CMS staff assessments as reference:

1. **False Positive Rate: The component should eliminate as few as possible of the enrolled treatment group months that CMS staff have classified as eligible.**
2. **Sensitivity: For a component to be effective, it should remove considerably more months from the control group than from the enrolled months group.**

The results of applying each screen component to a subsample[11] of the data are summarized in Table 3.1.1. The table contrasts the effect of each screen component on control group months with its effect on actually enrolled months because these were the only months during which CMS staff and the sites actually made a determination of eligibility.

The result for both Part A and Part B ineligibility was unambiguous: Neither component of the screen eliminated any enrolled months, i.e. they had a zero false positive rate, but a substantial number of control group months were excluded, i.e. their sensitivity was sufficient. Similarly, the Medicare HMO component eliminated almost no enrolled months but a high number of control group months. This result was not surprising since those three criteria are unambiguously defined in the Medicare Enrollment Database. We conclude that these three components should clearly be applied. It should be noted that since a beneficiary would not generate any Medicare claims in these months, failure to exclude such months would have led us to code expenditures incorrectly as zero rather than as missing or censored,[12] resulting in too low an estimate of average monthly outlays. We also decided to retain the hospice residency component since it excluded more control group months than eligible months both in absolute terms and as a percentage.

The "out-of-area" residency screen component did not perform as well as hoped, probably because we used the official address of the beneficiary. This might not correspond to the actual place of residency, in particular for beneficiaries who migrate seasonally ("snowbirds" and "sunbirds"). However, we had to retain this criterion, because the Medicare Part B claims data for the population reference group had been retrieved through the Medicare Part B State Files rather than the National Claims History 100% File. Thus, without the residency exclusion, we would have missing data for Part B claims for beneficiaries who moved into a state without a CNO site.

The remaining two components did not seem useful, since they eliminated similar shares of enrolled and control group months. Thus, they appeared not to emulate CMS staff decisions but to impose a different decision rule. This was not a surprising outcome for the nursing home residency screen. As outlined above, our ability to operationalize this criterion on the basis of administrative data was quite limited. By contrast, the fact that the ESRD screen eliminated a

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[11] **This analysis was performed under a previous contract that only examined data from Waves 1 and 2 of the demonstration.**

[12] **Obviously, the Medicare program would still incur costs for a beneficiary enrolled in a Medicare HMO that should be included in a comparison of expenditures. However, as the present evaluation had no access to data on payments to Medicare HMOs, we were operationally unable to account for these costs.**

similar number of enrolled and control group months was more of a surprise because both our algorithm and CMS staff decisions were based on information in the Medicare EDB. However, since our algorithms were applied months or years after CMS staff decisions, changes in the EDB over time probably explain this observation. Since we were unable to reproduce CMS staff decisions with respect to ESRD, this component of the eligibility screen could not be retained.

Table 3.1.1

Examination of the effect of eligibility screening algorithms on exclusion of analyzed months

	Enrolled Months Excluded		Control Group Months Excluded	
Eligibility Screen	**Number**	**Percent**	**Number**	**Percent**
Part A Ineligibility	**0**	**0.00%**	**45**	**0.04%**
Part B Ineligibility	**0**	**0.00%**	**164**	**0.14%**
ESRD Eligibility	**182**	**0.09%**	**138**	**0.12%**
Medicare HMO	**52**	**0.03%**	**4,013**	**3.47%**
Hospice Enrollment	**221**	**0.11%**	**347**	**0.30%**
Nursing Home Residency	**1,272**	**0.65%**	**991**	**0.86%**
Residency out of Area	**6,494**	**3.34%**	**6,478**	**5.60%**

The denominators are total numbers of non-missing months (Enrolled Months n=194,496, Control Group Months n=115,577). ESRD denotes end stage renal disease, HMO health maintenance organization.

Sources: CNO Enrollment File, Medicare Enrollment Database, January 1994-December 1996

These results suggested that the two components to add to the eligibility screen beyond those which remove months without Medicare claims (Part A and Part B eligibility and enrollment in a Medicare HMO) were hospice residency and out-of-area residency. Given that the available data did not permit us to emulate CMS staff assessments on ESRD eligibility and residency in a nursing home, those components were removed from the eligibility screen.

3.2. Enrollment

As described in Section 1.2.2, CNO applicants were randomized to treatment or control status in a 2:1 ratio during Wave 1 and Wave 3 of the demonstration. This arrangement allowed sites to build up enrollee numbers more quickly in order to be able to provide their full range of services. In addition, all applicants were assigned to the treatment group during Wave 2 and Wave 4. Thus, three-quarters of the 15,061 demonstration participants were assigned to CNO treatment. The number of participants by site and treatment/control status is summarized in Table 3.2.1.

The intermittent nature of randomization can be seen in Table 3.2.2, which shows enrollment in treatment and control groups by wave. As indicated by the table, enrollees from Waves 2 and 4 had no control group, making a comparison of means between treatment and control potentially misleading. To see why this might be the case, consider that new CNO enrollees were generally younger than those who had enrolled earlier. Consequently, if a mean calculated from the entire treatment group were compared to a mean calculated from the entire control group, the control group would be older on average and therefore would be expected to have higher per person expenditures.

Table 3.2.1

Number and percentage of treatment and control group participants by site

Site	Treatment Group		Control Group	
	Number	Percent	Number	Percent
Carle Clinic	3,321	77%	1,000	23%
Carondelet	3,739	74%	1,322	26%
LAH/BNP	2,218	72%	868	28%
VNSNY	1,999	77%	594	23%
Total	11,277	75%	3,784	25%

Sources: CNO Enrollment File, January, 1994-July,2000

Table 3.2.2

Number and percentage of treatment and control group participants by wave

Wave	Treatment Group		Control Group	
	Number	Percent	Number	Percent
1	7,138	67%	3,508	33%
2	1,016	100%	4[a]	0%
3	885	76%	272	24%
4	2,238	100%	0	0%
Total	11,277	75%	3,784	25%

[a] The four beneficiaries assigned to the control group during Wave 2 may have been Qualified Household Members who were unaware that their spouse had previously been assigned to control status.

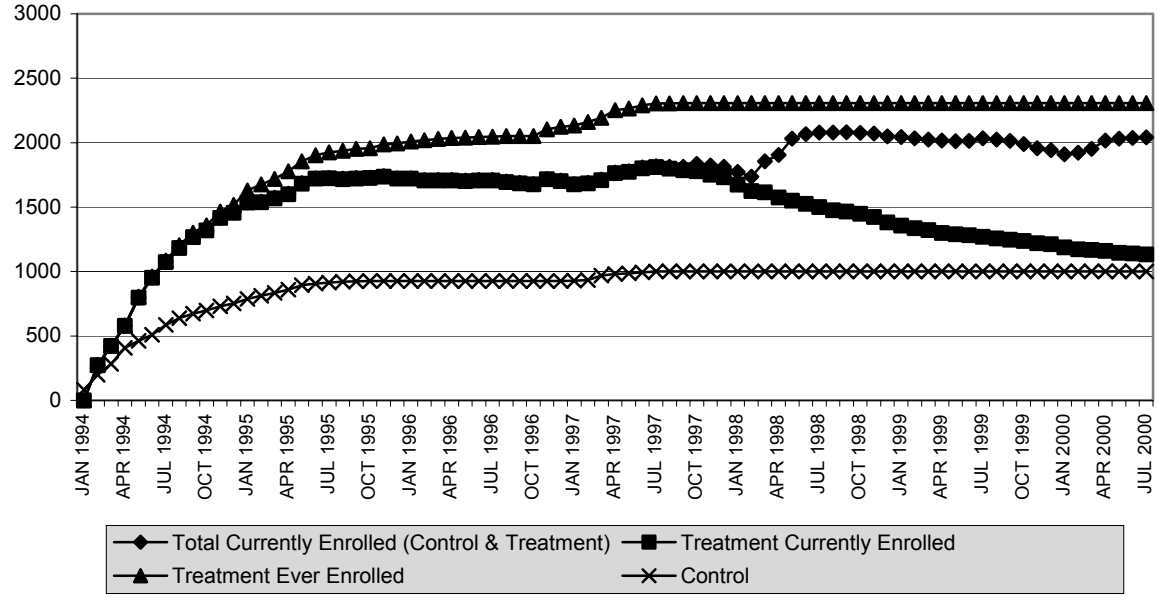
Sources: CNO Enrollment File, January, 1994-July,2000

When initial assignment to the treatment and control groups is plotted against time the assignment patterns of the four waves are clear. Figures 3.1 through 3.4 illustrate assignment over time for each of the four sites. One can see that the treatment group grows much faster than the control group, and that the control group has two long periods without any new assignments (Waves 2 and 4), interrupted by a short period of additional assignment (Wave 3). Thus, the treatment group is almost two-and-one-half times as large as the control group by the end of the observation period (compare the top and bottom lines).

Also shown is actual enrollment to the treatment group and to treatment and control groups combined. It is clear from these figures that the sites maintained relatively steady enrollment through July of 2000, replenishing the treatment group as members disenrolled. Since the control group was not replenished in this way during Waves 2 and 4, the resulting compositional change in the treatment group is a potential source of bias when unadjusted means are compared. Finally, the contamination of the control group is evident in the number of controls who enrolled (difference between middle two lines). By June of 2000, more than half of the control group at every site had been enrolled, substantially compromising the value of randomization. Specifically, at Carle Clinic, 91 percent of the control group had enrolled by June of 2000; at Carondelet, LAH/BNP, and VNSNY the proportion of controls enrolled by June of 2000 is 57 percent, 52 percent, and 54 percent, respectively. This contamination of

controls compromises treatment-to-control comparison using year 2000 data. Hence, no such comparison is made in this report (with year 2000 data).

Figure 3.1: Treatment and control groups by month: Carle Clinic



Control group contains both enrolled and unenrolled beneficiaries.

Sources: CNO Enrollment File

Figure 3.2: Treatment and control groups by month: Carondelet

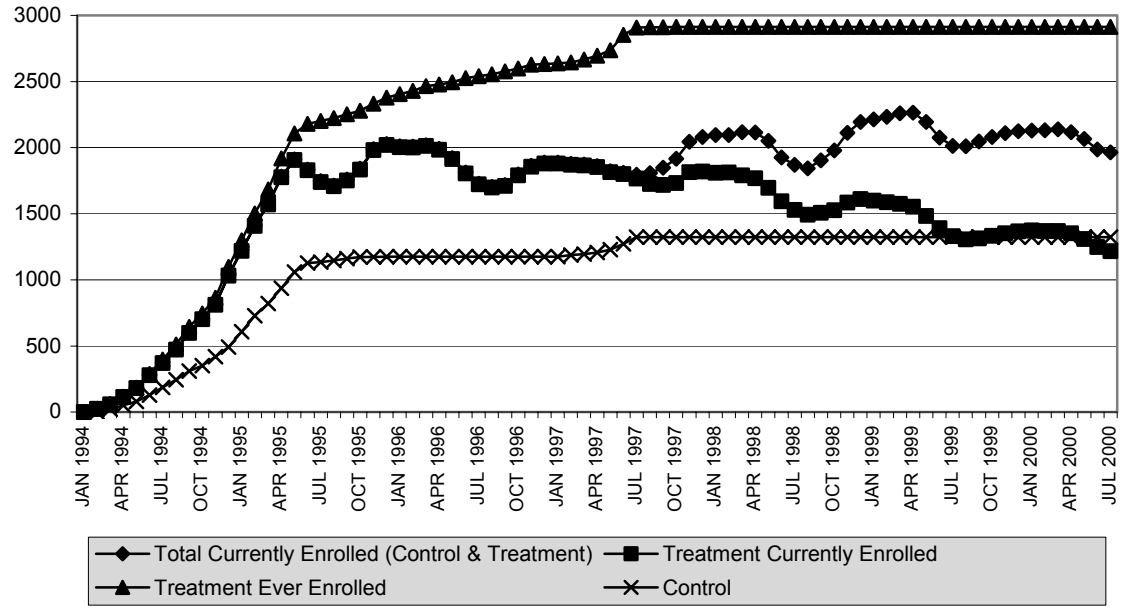
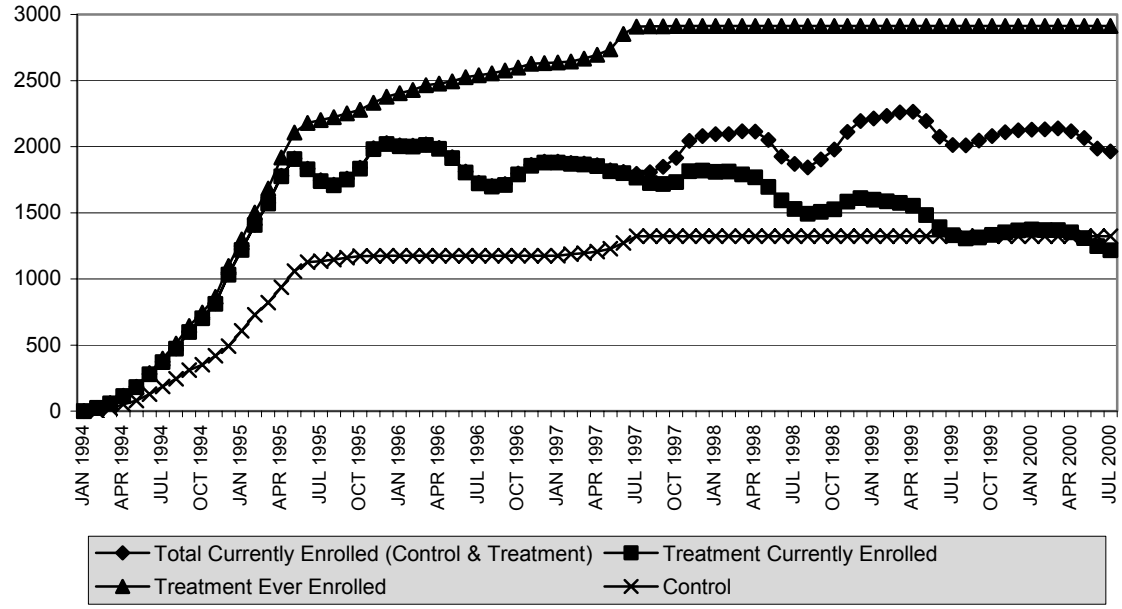


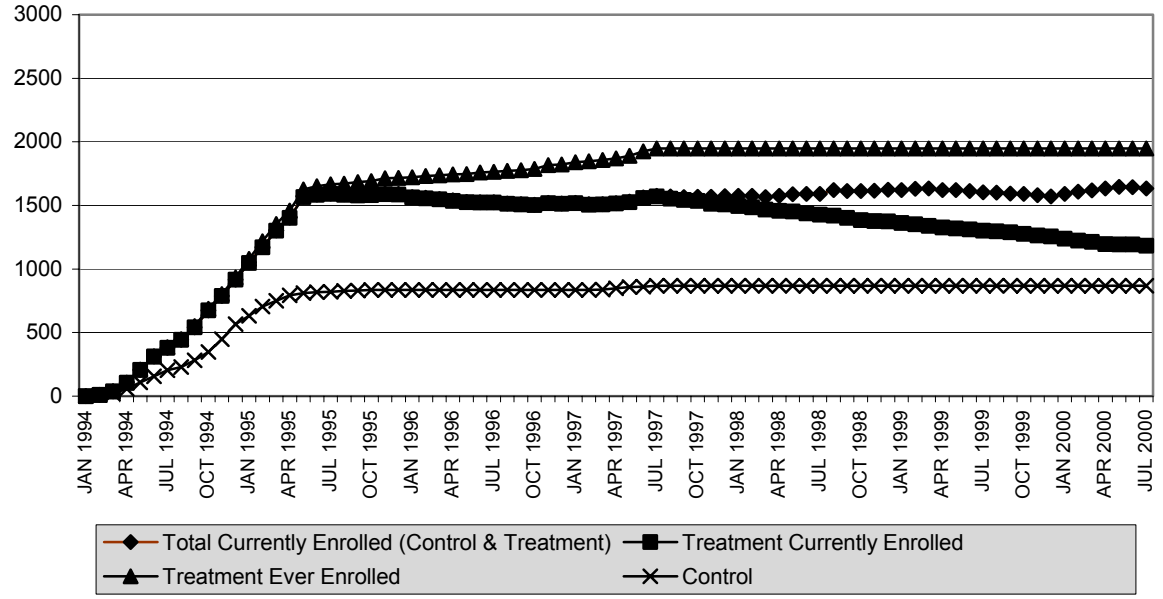
Figure 3.2: Treatment and control groups by month: Carondelet



Control group contains both enrolled and unenrolled beneficiaries.

Sources: CNO Enrollment File

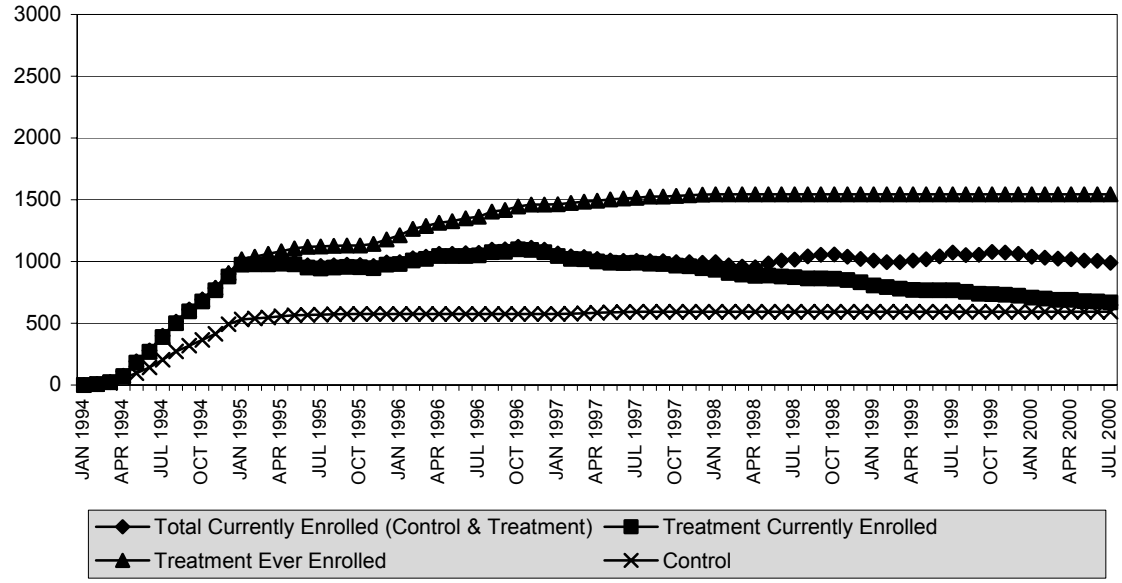
Figure 3.3: Treatment and control groups by month: LAH/BNP



Control group contains both enrolled and unenrolled beneficiaries.

Sources: CNO Enrollment File

Figure 3.4: Treatment and control groups by month: VNSNY



Control group contains both enrolled and unenrolled beneficiaries.

Sources: CNO Enrollment File

The cyclical pattern of actual enrollment at Carondelet reflects seasonal migration by beneficiaries ("snowbirds" and "sunbirds"), a phenomenon that was particularly pronounced at the Arizona site where enrollment declined every summer.

3.3. Comparison of Reference Group to Participants

As discussed in Section 1.2.4, the randomized design of the Phase I Evaluation permitted a simple comparison of means as a measure of the treatment effect, whereas nonrandom selection in the Phase II Evaluation necessitated adjustment for differences between treatment and reference groups. Since the adjustment process was based on complex statistical models that are explained in detail in Appendix A, a much larger sample size than the number of demonstration participants was required. Therefore, a population-based reference group was created consisting of all eligible Medicare beneficiaries who lived in a CNO catchment area. The characteristics of this reference group are described in the following tables and compared to the demonstration participants for each site separately. Note that in order to complete the analysis for this study in a reasonable amount of time, some calculations were done on a random sample of this reference group (a 2.5% sample in the case of VNSNY and a 10% sample for all other sites).

In 1994, the first year of the CNO demonstration, the treatment and control groups were similar with respect to age, sex and race (Table 3.3.1). This is as expected, since a substantial number of them had been assigned randomly. Demonstration participants appeared to be older on average than the population due to the lack of non-elderly disabled Medicare beneficiaries in the demonstration.

When the comparison is restricted to Medicare beneficiaries over the age of 65 (Mean Age of Elderly column), participants and the population were similar with respect to age. In addition to the disabled, Table 3.3.1 indicates that men and nonwhites were underrepresented in all four demonstration sites relative to the population.

To compare utilization and spending, we tabulated Medicare claims from 1993, the year before the inception of the demonstration, to eliminate any possible demonstration effect. With the exception of treatment group members in LAH/BNP, participants had higher spending on average than the Medicare population, both for CNO-provided and for non-CNO services (Table 3.3.2).[13] Participants also had more physician office visits, emergency room days and hospital admissions. This more intense use of medical resources is to some degree surprising, since previous analyses had shown that participants had lower age-adjusted mortality rates, indicating better health status (Abt Associates Inc., 1998).

[13] **CNO-provided, or "in-bundle" services under the demonstration are described in Section 1.1.3.**

Table 3.3.1

Demographic characteristics of the treatment, control and population reference groups, first year of CNO demonstration (1994)

Site	Status	N	Mean Age	Elderly %	Mean Age of Elderly	Female %	Non-white %
Carle	Treatment	3,191	72.21	88%	73.84	62%	3%
Carle	Control	1,000	72.23	94%	73.01	60%	2%
Carle	Population	86,407	70.81	82%	75.08	58%	6%
Carondelet	Treatment	3,691	72.60	90%	74.01	62%	3%
Carondelet	Control	1,322	73.55	93%	74.50	60%	3%
Carondelet	Population	124,594	70.35	82%	74.29	55%	10%
LAH/BNP	Treatment	2,196	74.62	92%	75.74	68%	1%
LAH/BNP	Control	868	74.96	97%	75.33	66%	0%
LAH/BNP	Population	164,029	70.92	83%	75.24	59%	5%
VNSNY	Treatment	1,988	76.35	95%	77.18	77%	6%
VNSNY	Control	594	76.93	98%	77.21	78%	7%
VNSNY	Population	643,856	71.50	84%	75.20	60%	29%

Elderly refers to those over the age of 65.

Sources: CNO Enrollment File, Medicare Enrollment Database, January 1994-December 1999.

Table 3.3.2

Spending and utilization characteristics of the treatment, control and population reference groups, first year prior to CNO demonstration (1993)

Site	Status	N	Expenditures: Average Medicare Spending			Utilization: Average Number of		
			Total	In-Bundle	Out-of-Bundle	Admis-sions	ER Days	MD Visits
Carle	Treatment	3,191	$2,033	$230	$1,803	0.18	0.20	5.32
Carle	Control	1,000	$2,316	$208	$2,108	0.21	0.15	5.11
Carle	Population	86,407	$1,865	$182	$1,683	0.20	0.18	3.61
Carondelet	Treatment	3,691	$3,070	$253	$2,817	0.23	0.21	7.41
Carondelet	Control	1,322	$3,205	$274	$2,931	0.22	0.21	7.69
Carondelet	Population	124,594	$2,224	$252	$1,973	0.15	0.17	3.97
LAH/BNP	Treatment	2,196	$1,860	$105	$1,754	0.19	0.15	4.97
LAH/BNP	Control	868	$2,372	$160	$2,212	0.22	0.20	4.80
LAH/BNP	Population	164,029	$2,147	$192	$1,954	0.17	0.20	3.07
VNSNY	Treatment	1,988	$4,151	$500	$3,651	0.23	0.13	8.44
VNSNY	Control	594	$4,362	$561	$3,800	0.23	0.16	8.64
VNSNY	Population	643,856	$3,586	$337	$3,249	0.20	0.13	4.79

In-bundle and out-of-bundle spending are as defined in Section 1.1.3; admissions denotes inpatient hospital admissions; ER days refers to the number of days during which there was an emergency room claim; and MD visits refers to the number of physician visits.

Sources: CNO Enrollment File, Medicare Enrollment Database, National Claims History File

One reason why average expenditures would be higher for CNO demonstration participants is the fact that participants were more likely than the population to have nonzero Medicare claims in a given year. As illustrated in Table 3.3.3, only about one percent of the treatment group had zero claims. By contrast, between 11 and 14 percent of the population had zero Medicare utilization. These figures show that beneficiaries with minimal utilization were unlikely to apply to the demonstration, leading to adverse selection. This phenomenon was mitigated by the tendency of beneficiaries with high utilization to drop out of the demonstration (or not apply in the first place), as was shown in the Preliminary Report (see Table 3.3.5 of Abt Associates Inc., 2001). Note also that the percentage of beneficiaries in the population group with no Medicare claims is higher in year 2000 than in any prior year. This is most likely due to the fact that only data through July of 2000 were available for this study. So, any beneficiaries with claims only in August-December, 2000 can not be distinguished from those with no claims in the entire year.

Table 3.3.3

Percentage of Medicare Beneficiaries without any Medicare Claims in Each Year of the Demonstration

Year	Status	Carle	Carondelet	LAH/BNP	VNSNY
1994	Treat	2%	2%	5%	2%
	Pop	12%	14%	15%	15%
1995	Treat	1%	2%	1%	2%
	Pop	11%	13%	14%	14%
1996	Treat	1%	1%	1%	1%
	Pop	10%	12%	12%	14%
1997	Treat	1%	1%	1%	1%
	Pop	9%	12%	11%	13%
1998	Treat	1%	1%	1%	1%
	Pop	9%	13%	11%	14%
1999	Treat	1%	1%	1%	0.5%
	Pop	9%	11%	11%	13%
2000	Treat	1%	1%	1%	1%
	Pop	16%	17%	19%	16%
Average	Treat	1%	1%	2%	1%
	Pop	11%	13%	13%	14%

Treat denotes treatment group; Pop denotes population reference group. Figures for the control group were similar to those for the treatment group. Results for year 2000 are based on data through July, 2000 (data beyond July, 2000 were not available for this study).

Sources: Abt Associates Inc. CNO Evaluation Analytic Files

Any two groups not generated by random assignment are likely to be different, so the fact that the population reference group systematically differed from the treatment group should come as no surprise. It is precisely these differences that can lead to selection bias, requiring the application of statistical techniques, discussed in detail in Appendix A.

4. CNO Effects on Medicare Expenditures

The randomized design of Waves 1 and 3 of the CNO demonstration (see Section 1.2.4 for a description of demonstration Waves) permitted a simple comparison of means between treatment and control groups as a measure of the treatment effect. The results of these comparisons, carried out for the Preliminary Report to Congress, were summarized in Chapter 2. During Waves 2 and 4, however, applicants could enroll directly into the treatment group and no control group members were assigned. Consequently, there is no randomized control group for those enrollees and different methods are required to measure the treatment effect. We chose a quasi-experimental approach whereby we constructed a comparison group based on the fee-for-service Medicare population in the CNO catchment area. However, we have presented evidence in Section 3.3 that CNO applicants differed from the population in the CNO catchment areas substantially. The proportion of disabled beneficiaries and of minorities and men was lower in the participant group, and participants had higher prior utilization of and spending for medical services. In addition, there was a higher proportion of beneficiaries without any Medicare claims in the population reference group. While this does not necessarily mean that participants were in worse health, one can at least state that the two groups are not comparable. A straight comparison of means would therefore reflect both a treatment effect and a selection effect and would provide a biased estimate of the effect of CNO participation on our spending and utilization measures. Thus, adjustment techniques that account for observable and unobservable differences between the treatment group and the population reference group were necessary.

In this chapter, we first show results for the *biased* comparison of means, and then introduce in a stepwise fashion a series of statistical strategies that allow us to disentangle the treatment and selection effects. Since the effect of CNO participation on total cost per participant per month is the key research question for the evaluation, we present results for each statistical step for this measure. For brevity and to avoid possible confusion, we limit the presentation of results for the remaining cost components (e.g., CNO-services, inpatient care) and the utilization measures (physician visits, ER days, hospital admissions) to the most reliable selection-corrected model.

Before presenting the results, there are two points worth emphasizing. First, the results presented in this report differ from those of the Preliminary Report (and summarized in Chapter 2) in that the sample in this chapter includes data through July 2000 whereas the sample for the Preliminary Report included data only through 1999. The results prior to 2000 presented here also differ from those in the Preliminary Report because a different random sample of population reference beneficiaries has been used.[14] Second, we do not present any results that compare treatment to controls and restrict attention to treatment vs. population comparisons. This is because in 2000 the number of control group members that had not enrolled in treatment had grown sufficiently small that any meaningful comparison between treatment and control groups is not possible (see Section 3.2 for further discussion of this point).

[14] **A 2.5% random sample is used for the VNSNY population reference group and a 10% sample is used at other sites.**

4.1. Differences in Total Spending

Differences in average total Medicare spending per participant per month (PPPM) are reported for all four sites in Table 4.1.1. The first row displays unadjusted estimates, which, as discussed above, reflect both a treatment and a selection effect. In all four sites, average spending was significantly higher in the treatment group than in the population. The difference ranged from $221 at the VNSNY site to $37 at Carle Clinic.[15]

However, given that CNO participants had higher baseline spending (except those at LAH/BNP; see Table 3.3.2), not all of these differences should be attributed to CNO participation. Adjusted differences in average monthly spending, accounting for observable beneficiary risk factors, are presented in the second row of the table. The risk adjustment method, explained in detail in Appendix A, adjusted the spending estimates for differences in age, sex, race, prior diagnoses and prior expenditures. Although the treatment group still had higher expenditures, these adjusted differences were considerably lower and were not statistically significant for Carle Clinic. Between 50 percent (LAH/BNP) and 83 percent (Carle Clinic) of the unadjusted differences could be explained by differences in casemix, demonstrating the power and importance of the risk adjustment model.

In addition to observable factors of beneficiary risk, we also adjusted for the impact of random events that occurred in one or more years of the observation period, such as a flu epidemic, by using so-called year fixed effects. The third row of Table 4.1.1 illustrates that adjusting for year fixed effects had almost no effect on the estimated differences between spending for treatment group members and spending for the population. This is not surprising, since it is unlikely that such events would have affected one of the two groups more than the other.

Table 4.1.1

Estimated Differences in Per Month Spending between CNO Treatment Group and the Population Reference Group for All Services in All Years (January, 1994-July, 2000)

	Carle	Carondelet	LAH/BNP	VNSNY
Unadjusted	$37 ***	$73 ***	$46 **	$221 ***
Adjusted for Risk	$6	$26 *	$23 ~	$48 *
Adjusted for Risk and Year Effects	$6	$26 *	$22 ~	$49 *

All dollar amounts were converted to 1999 constant dollars. The CNO treatment group consists of all those randomized to treatment or directly enrolled in the CNO. Sample sizes are described in Appendix Table B.1. Note that a random sample of the population reference group was used to produce these figures (a 2.5% sample in the case of VNSNY and 10% for other sites). ~ denotes significance at p < 0.1; * p<0.05; ** p<0.01; ***p<0.001.

Sources: Abt Associates Inc. CNO Evaluation Analytic Files

[15] **Results presented in this chapter differ slightly from those of the Preliminary Report (and those presented in Chapter 2 of this report) due to random sampling of the population reference group.**

4.2. Differences in Spending Changes Over Time

Despite the predictive power of our risk adjustment method, it is possible that part of the remaining spending differences might be traceable to unaccounted-for baseline differences between CNO participants and the population. The risk adjustment models can only correct for differences that are observable in the administrative data available to the study, such as demographic characteristics or prior diagnoses. Information was not available on other characteristics, such as beneficiary preferences or current health status, and it is reasonable to think that those factors may have influenced both the decision to join a CNO and the level of Medicare spending.

To shed further light on the question of whether CNO participation had an effect on Medicare spending, we examined differences in spending changes over time. In other words, starting from the baseline year of 1994 (the first year of the demonstration) did average spending change differently for participants and the population? For example, assume that average spending in 1994 was $60 per treatment group member and $45 in the population, and that spending in 1995 rose to $90 in the treatment and $65 in the reference group. Thus, the change in spending was $30 per month in the treatment group compared to $20 in the population, and the differential change was $10. This would indicate that cost increased by $10 more in the treatment group in that year, suggesting that the demonstration caused costs to increase faster than they would have otherwise.

This approach is referred to as the difference-in-differences method, since it investigates differences in changes over time for two groups to be compared. The advantage of this approach is that baseline differences, which may be a consequence of non-random selection, are not considered and only differences in the change from that baseline are analyzed. Thus, unobservable differences between the two groups that are present at baseline do not bias estimated effects. By analyzing changes over the years of the demonstration, we can also address the question of whether the sites became more efficient over time. The underlying hypothesis to be tested is that CNO sites might not have been cost-effective in the early years since enrollees had not had time to benefit from preventive services and the sites were still improving their model of care, but that later years would show more positive health effects. The results of this analysis are summarized in Table 4.2.1.

Table 4.2.1

Estimated Differences between the CNO Treatment Group and the Population Reference Group: Changes in Per Month Spending for All Services, Relative to 1994

Year	Carle	Carondelet	LAH/BNP	VNSNY
1995	$8	$57	$74 *	$105 ~
1996	$0	$83 *	$71 ~	$162 **
1997	$36	$97 **	$49	$276 ***
1998	$62 *	$77 *	$84 *	$403 ***
1999	$71 *	$120 ***	$184 ***	$236 **
2000	$63 ~	$111 **	$153 ***	$358 ***

Table 4.2.1

Estimated Differences between the CNO Treatment Group and the Population Reference Group: Changes in Per Month Spending for All Services, Relative to 1994

All dollar amounts were converted to 1999 constant dollars. All differences are adjusted for beneficiary risk and year effects. The CNO treatment group consists of all those randomized to treatment or directly enrolled in the CNO. Results for year 2000 are based on data through July, 2000 (data beyond July, 2000 were not available for this study). Sample sizes are described in Appendix Table B.1. Note that a random sample of the population reference group was used to produce these figures (a 2.5% sample in the case of VNSNY and 10% for other sites). ~ denotes significance at p<0.1; * p<0.05; ** p<0.01; ***p<0.001.

Sources: Abt Associates Inc. CNO Evaluation Analytic Files

The results clearly demonstrate that average monthly Medicare spending increased much faster in the treatment group than in the population. In two sites (LAH/BNP and VNSNY), the difference in changes was already statistically significant in 1995, whereas it became significant only later at the Carle Clinic and Carondelet sites. All four sites showed a steady increase in estimated differences through time, indicating that average spending in the treatment group kept increasing relative to the population over the course of the demonstration. Not only are the estimates statistically significant, but also of a substantial magnitude: by 2000, average spending increased by $358 more per CNO participant per month in the New York site, and $63 more in the site with the smallest differences, Carle Clinic.

The same methodology was used to assess the hypothesis that a beneficiary had to be enrolled in a CNO over a certain period for the positive effects of case management on cost to materialize. For this analysis, we removed those treatment group members who had less than six months of enrollment. As shown in Appendix Table B.1, this definition of CNO treatment selectively removes about a third of the person-years from the treatment group, while the population reference group remains unchanged. The results are summarized in Table 4.2.2. Although different in detail, the estimates show a similar pattern as the ones presented above: differences in monthly spending between CNO participants and the reference group tended to grow over time, with the exception that the differences at Carle Clinic were not statistically significant in any year.

Table 4.2.2

Estimated Differences between the CNO Treatment Group Enrolled at Least Six Months and the Population Reference Group: Changes in Per Month Spending for All Services, Relative to 1994

Year	Carle	Carondelet	LAH/BNP	VNSNY
1995	-$9	$107 *	$41	$135 ~
1996	-$23	$166 ***	$38	$181 *
1997	$7	$172 ***	$3	$248 **
1998	$19	$120 **	$35	$277 ***
1999	$35	$167 ***	$137 ~	$178 ~
2000	$35	$169 ***	$84	$342 ***

Table 4.2.2

Estimated Differences between the CNO Treatment Group Enrolled at Least Six Months and the Population Reference Group: Changes in Per Month Spending for All Services, Relative to 1994

All dollar amounts were converted to 1999 constant dollars. All differences are adjusted for beneficiary risk and year effects. The CNO treatment group consists of all those randomized to treatment or directly enrolled in the CNO provided they were enrolled for at least six months. Sample sizes are described in Appendix Table B.1. Note that a random sample of the population reference group was used to produce these figures (a 2.5% sample in the case of VNSNY and 10% for other sites). Results for year 2000 are based on data through July, 2000 (data beyond July, 2000 were not available for this study). ~ denotes significance at p<0.1; * p<0.05; ** p<0.01; ***p<0.001.

Sources: Abt Associates Inc. CNO Evaluation Analytic Files

Appendix C includes two additional sets of results based on definitions of the treatment sample similar to that used in Table 4.2.2. In particular, the law that mandates this CNO evaluation stipulates that it also include spending comparisons based on the following two definitions of the treatment group:

- **Individuals enrolled in a CNO as of July 1, 1997 and for six months thereafter and**
- **Individuals enrolled in a CNO as of January 1, 2000 and for six months thereafter.**

The results based on these definitions of treatment are qualitatively similar to those presented above and lead to the same conclusions (see Appendix C for details).

In summary, these results suggest that the CNO as a model of care provision was associated with increased cost to the Medicare program compared to traditional fee-for-service payment. This conclusion is based on very robust findings that were consistent across several analytic approaches. The differences persisted after the application of increasingly complex risk adjustment methods so one can be confident that they were not due to baseline differences between the treatment group and the fee-for-service population. In addition, these differences were robust to changes in the way CNO participation was defined. While the cost differential was consistently smallest at the Carle Clinic site and largest at the VNSNY site, it increased over time at all four sites, with only occasional interruptions in this trend over the period.

4.3. Differences in Spending Changes Over Time Controlling for Unobservable Selection

There remains the possibility that beneficiaries who enrolled in the CNO may have been at greater risk for increased Medicare spending as a result of characteristics not captured by the risk adjustment model underlying Tables 4.2.1 and 4.2.2. We investigated this possibility by constructing a statistical model of self selection into the CNO. This two-part statistical model, known as a "switching regression model," uses site-and-time-specific enrollment rates as a so-called instrumental variable. Details are presented in Appendix A. Results of the switching regression model are presented in Table 4.3.1.

Table 4.3.1

Estimated Differences between the CNO Treatment Group and the Population Reference Group, Controlling for Unobservable Selection: Changes in Per Month Spending for All Services, Relative to 1994

Year	Carle	Carondelet	LAH/BNP	VNSNY
1995	$7	$58	$72 *	$105 ~
1996	$0	$84 *	$69 ~	$161 *
1997	$36	$95 **	$50	$273 ***
1998	$62 *	$77 *	$83 *	$401 ***
1999	$71 *	$118 ***	$183 ***	$234 **
2000	$67 ~	$112 *	$156 ***	$362 ***

All dollar amounts were converted to 1999 constant dollars. All differences are adjusted for beneficiary risk, year, and unobservable selection effects. The CNO treatment group consists of all those randomized to treatment or directly enrolled in the CNO. Results for year 2000 are based on data through July, 2000 (data beyond July, 2000 were not available for this study). Sample sizes are described in Appendix Table B.1. Note that a random sample of the population reference group was used to produce these figures (a 2.5% sample in the case of VNSNY and 10% for other sites). ~ denotes significance at p<0.1; * p<0.05; ** p<0.01; ***p<0.001.

Sources: Abt Associates Inc. CNO Evaluation Analytic Files

The results are nearly identical to those seen in Table 4.2.1, further suggesting that the increased Medicare spending seen here is the result of the CNO intervention, together with the capitation rates used for the CNO sites and not a consequence of systematically biased selection of high-cost beneficiaries into the CNO.

4.4. Decomposition of Differences in Total Spending

Having established that CNO participation was associated with higher average monthly Medicare expenditures, we analyzed which components of spending were responsible for this difference. We again used the risk-adjusted difference-in-differences methodology, as described above, to account for observable and unobservable characteristics other than CNO participation that might have contributed to differences in spending. The first step was to decompose total Medicare spending into spending for services provided under the CNO arrangements ("in-bundle services," see Section 1.1.3 for a detailed definition), which corresponds to the capitation and case management fees for the treatment group, and all other services ("out-of-bundle services"). These results are presented in Table 4.4.1.

Table 4.4.1

Estimated Differences between the CNO Treatment Group and the Population Reference Group: Changes in Per Month Spending for In-Bundle and Out-of-Bundle Services, Relative to 1994

	Carle	Carondelet	LAH/BNP	VNSNY
In-Bundle				
1995	-$1	$20 ***	$30 ***	$21 *

Table 4.4.1

Estimated Differences between the CNO Treatment Group and the Population Reference Group: Changes in Per Month Spending for In-Bundle and Out-of-Bundle Services, Relative to 1994

1996	**$17 *****	**$5**	**$13 ****	**$17**
1997	**$13 ***	**$14 ***	**$32 *****	**$49 *****
1998	**$15 ***	**$15 ***	**$41 *****	**$93 *****
1999	**$9 ***	**$15 ***	**$36 *****	**$94 *****
2000	**-$8**	**$6**	**$19 ****	**-$7**
Out-of-Bundle				
1995	**-$2**	**$29**	**$42**	**$78**
1996	**-$14**	**$62 ***	**$40**	**$139 ***
1997	**$9**	**$67 ***	**$10**	**$199 *****
1998	**$26**	**$47**	**$45**	**$286 *****
1999	**$45**	**$85 ****	**$137 ****	**$139 ~**
2000	**$56 ~**	**$72 ***	**$107 ****	**$316 *****

All dollar amounts were converted to 1999 constant dollars. All differences are adjusted for beneficiary risk and year effects. The CNO treatment group consists of all those randomized to treatment or directly enrolled in the CNO. Sample sizes are described in Appendix Table B.1. Note that a random sample of the population reference group was used to produce these figures (a 2.5% sample in the case of VNSNY and 10% for other sites). Results for year 2000 are based on data through July, 2000 (data beyond July, 2000 were not available for this study). Note that, since in-bundle and out-of-bundle components were estimated in separate statistical models, they do not necessarily add up to the differences in total spending presented above. ~ denotes significance at p<0.1; * p<0.05; ** p<0.01; ***p<0.001.

Sources: Abt Associates Inc. CNO Evaluation Analytic Files

For CNO-covered services, the estimates show that average monthly cost in the treatment group increased substantially faster than in the population. This result is especially marked at the LAH/BNP and VNSNY sites, where the difference was statistically significant in nearly every year. While these results tend to confirm our previous conclusion that CNO payment rates were set too high to achieve budget neutrality, the table shows a sharp decline in this difference for every site in 2000, though the year 2000 results are only statistically significant at LAH/BNP. Several factors may contribute to this year 2000 result. Capitation rates were lower in 2000 as compared to prior years. Also, the year 2000 results are based only on data through July of 2000 so certain seasonally dependent phenomena may not be captured in available study data. Lastly, changes in home health prospective payment and the implementation of the outpatient prospective payment system may have had confounding influences on claims data.

The results for non-CNO (out-of-bundle) services are not consistent with findings from the analysis of the randomized portion of the demonstration described in Chapter 2 (see Table 2.1). When comparing CNO applicants randomly assigned to treatment or control status, we found that average spending for those services was similar in both groups. In contrast, the results from the entire demonstration suggest that non-CNO spending increased disproportionately for CNO participants over time. To shed further light on this discrepancy, we examined spending for three major components of non-CNO spending: inpatient care, ER visits, and physician office visits. These results are summarized in Table 4.4.2.

Table 4.4.2

Estimated Differences between the CNO Treatment Group and the Population Reference Group: Changes in Per Month Spending for Inpatient Hospital Services, ER Visits and Physician Office Visits, Relative to 1994

	Carle	Carondelet	LAH/BNP	VNSNY
Hospital Services				
1995	-$5	$12	$18	$36
1996	-$13	$34	$23	$110 *
1997	$3	$39 ~	-$5	$132 **
1998	$11	$32	$34	$193 ***
1999	$24	$50 *	$106 **	$86
2000	$34	$57 ~	$80 **	$230 **
ER Visits				
1995	$0.22	$0.46 *	$0.41 *	$0.40
1996	$0.40 ~	$0.53 *	$0.17	$0.44
1997	$0.19	$0.43 *	$0.07	$0.38 ~
1998	$0.16	$0.45 ~	$0.09	$0.31
1999	$0.11	$0.48 *	$0.21	$0.32
2000	$0.11 *	$0.53 *	-$0.06	$0.36
Physician Office Visits				
1995	-$0.36	$1.04	$0.38	-$0.40
1996	$0.71 *	$0.44	$0.07	$0.97
1997	$0.46	$1.09 ~	$0.12	$0.28
1998	$0.60	$0.44	$0.07	-$1.46
1999	$0.03	$0.93	-$0.85 ~	-$2.01 ~
2000	$0.96 ~	-$0.34	-$0.05	-$2.31 ~

All dollar amounts were converted to 1999 constant dollars. All differences are adjusted for beneficiary risk and year effects. The CNO treatment group consists of all those randomized to treatment or directly enrolled in the CNO. Sample sizes are described in Appendix Table B.1. Note that a random sample of the population reference group was used to produce these figures (a 2.5% sample in the case of VNSNY and 10% for other sites). Results for year 2000 are based on data through July, 2000 (data beyond July, 2000 were not available for this study). Note that these spending components are not exhaustive and were estimated in separate statistical models; they do not necessarily add up to the differences in total out-of-bundle spending presented above. ~ denotes significance at p<0.1; * p<0.05; ** p<0.01; ***p<0.001.

Sources: Abt Associates Inc. CNO Evaluation Analytic Files

These results suggest that the more rapid cost increase in the treatment group can mainly be attributed to hospital services. Across all four sites and particularly for LAH/BNP and VNSNY, average spending for hospital services increased substantially more in the treatment group. In addition, cost for ER visits increased disproportionately for all sites (ignoring the insignificant results). Changes in spending for physician office visits showed no overall differential pattern. Further support for these findings can be derived from the utilization analysis in which we compared changes in the number of physician office visits, as well as in the probability of an ER visit and of a hospital admission, as displayed in Table 4.4.3.

Table 4.4.3

Estimated Differences between the CNO Treatment Group and the Population Reference Group: Changes in Per Month Utilization of Inpatient Hospital Admissions, ER Visits and Physician Office Visits, Relative to 1994

	Carle	Carondelet	LAH/BNP	VNSNY
Hospital Admissions				
1995	0.00	-0.05	0.09	0.05
1996	0.06	-0.06	0.11	0.13
1997	0.12	0.06	0.06	0.25 **
1998	0.04	0.08	0.03	0.33 ***
1999	0.11	0.12	0.17	0.30 **
2000	0.13	0.11	0.21 ~	0.30 **
ER Visits				
1995	-0.11	0.04	0.33 **	-0.06
1996	-0.05	-0.03	0.21 ~	0.15
1997	-0.16 ~	0.00	0.29 *	0.12
1998	-0.23 **	0.03	0.22 ~	0.23 *
1999	-0.11	-0.01	0.18	0.09
2000	-0.23 *	-0.03	0.12	0.10
Physician Office Visits				
1995	0.00	0.02	0.01	0.00
1996	0.03 **	0.02	0.00	0.02
1997	0.19	0.03	0.01	0.01
1998	0.03 **	0.01	0.01	-0.02
1999	0.00	0.00	-0.01	-0.04 ~
2000	0.03 *	0.01	0.00	-0.02

All differences are adjusted for beneficiary risk and year effects. The CNO treatment group consists of all those randomized to treatment or directly enrolled in the CNO. Sample sizes are described in Appendix Table B.1. Note that a random sample of the population reference group was used to produce these figures (a 2.5% sample in the case of VNSNY and 10% for other sites). Results for year 2000 are based on data through July, 2000 (data beyond July, 2000 were not available for this study). ~ denotes significance at p<0.1; * p<0.05; ** p<0.01; ***p<0.001.

Sources: Abt Associates Inc. CNO Evaluation Analytic Files

The probability of a hospitalization increased much more rapidly for CNO enrollees relative to the population reference group at the VNSNY site, and also at the LAH/BNP site as well. There is no statistically significant evidence that the probability of hospitalization increased disproportionately for CNO enrollees at the Carle or Carondelet sites. With the exception of elevated ER utilization among CNO enrollees at the LAH/BNP site prior to 1999, the results for ER and physician visits were generally insignificant and showed no clear pattern.

If one were to interpret these results without the context provided by the findings from the randomized portion of the demonstration, the conclusion would be unambiguous: care for

VNSNY enrollees was shifted to the inpatient setting, and the magnitude of this shift increased over time. This is consistent with the incentive structure that the CNO demonstration created: In any payment arrangement in which the provider bears the burden of the marginal cost of selected areas of care there is a strong incentive to avoid cost by shifting the provision of care away from those areas. Since the CNOs would not have to provide any care to a beneficiary while hospitalized, but would still collect the monthly payments, hospital care appears to have been a particularly appealing option for cost shifting.

The main challenge to this interpretation is the fact that a comparison of average spending of demonstration applicants randomized to treatment or control status did not show any meaningful differences in non-CNO or hospital spending. An explanation for this discrepancy is that no risk adjustment method can be as powerful as true random assignment. To be able to extend the evaluation to the later, non-randomized portion of the demonstration, we had to use a quasi-experimental design in which the fee-for-service Medicare population living in a CNO catchment area was used as comparison group. As demonstrated in Section 3.3, this group differs in many important respects from the demonstration participants. While we have tried to account for those differences with well-established and powerful methods, there will always be a residual possibility that self-selection into the demonstration has biased the results. However, given the risk-adjusted difference-in-differences approach, one would have to postulate a selection process that had a differential impact over the course of the demonstration, and we have not identified a plausible hypothesis for such a process.

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Appendix A: Detailed Methodology

This technical appendix provides additional detail on the analytic methods employed to produce the results in this report. In past CNO evaluation reports, we compared mean (average) values for members of the treatment group to mean values for members of the control group, where such a comparison was possible (Chapter 2 of this report reviews these results). However, since randomization to treatment and control groups was not consistently implemented in the latter years of the demonstration (see Section 1.2.3), more complex methods became necessary. We implemented three such approaches: comparison of risk-adjusted means, calculation of difference-in-differences estimators, and calculation of difference-in-differences within a switching model framework.

Although for ease of exposition the following discussion generally refers to total Medicare expenditures as the outcome of interest, this report considers a series of variables (results are provided in Chapters 4 and 5). The next section defines these variables in detail.

Description of Outcome Variables to be Analyzed

The project team evaluated the effect of CNO participation on Medicare expenditures, utilization of care. The following expenditure variables were used for the treatment, control, and population reference groups. All costs were expressed in payments per person per month (PPPM).

- ***Adjusted CNO-covered expenditures*: This variable accounts for claims for services covered according to the definitions in Section 1.1.3 as well as CNO-capitation and case management payments. As there was no lockout mechanism in place, treatment group members could still obtain services on a fee-for-service basis. CMS managed this situation by recovering from the CNO the capitation payment for each CNO member for those months in which he/she received more than $120 in CNO services outside the CNO, i.e. under fee-for-service ($100 in 1995). The adjusted expenditure variables reflect this payment correction.**
- ***Non-CNO expenditures*: This variable accounts for claims for all services not included in the CNO-package, as defined in Section 1.1.3. Beyond the overall total, subtotals of claims for *inpatient hospital services*, *emergency room services,* and *physician office visits* were also analyzed.**
- ***Total expenditures*: This variable contains the sum of all costs to the Medicare program, including claims, capitation payments, and case management fees. This variable is the sum of adjusted CNO-covered expenditures and non-CNO expenditures.**

Utilization for each beneficiary was measured in three categories:

- ***Inpatient admissions:* Number of hospital admissions.**
- ***Emergency room visits*: Number of days with a claim for ER visits.**
- ***Physician visits*: Number of claims for outpatient physician office visits or office consultations.**

These categories were chosen to correspond with the subcategories of non-CNO expenditures to facilitate the investigation of whether and to what degree CNO-enrollment resulted in cost-neutral changes in utilization patterns.

Analysis Methods

When the above analyses were restricted to comparisons of "intent to treat" treatments and controls through October, 1997, the randomized design of the demonstration ensured a fair comparison. Unfortunately (for the evaluation), the periods of open enrollment and the decision to allow controls to switch to the treatment group after October, 1997 meant that substantial periods of time and numbers of participants were without a properly randomized control group. Evaluating the effects of the demonstration during these periods and on these participants required addressing the problem of selection bias.

In what follows, we discuss estimation techniques that were applied to the problem of self-selection or selection bias that arose in the CNO demonstration. To simplify the discussion we mostly neglect the existence of the control group, focusing attention on the contrast between the treatment group (participants) and the population reference group (non-participants).

Selection bias arises when individuals decide (self-select) whether to participate in a program and when some of the factors that influence their decision also influence the outcomes to be evaluated. In the CNO, it appeared that beneficiaries who chose to participate consistently used more services than the average beneficiary in the CNO service area. Failure to account for this difference would confuse the selection effect with the treatment effect, overstating the latter.

Selection bias can arise because of observable factors (like age, sex, and prior diagnoses) and unobservable factors (like attitudes and lifestyle). Equations [3] and [4] illustrate this distinction:

[3] $P^*_{it} = X_{it}\gamma + \varepsilon_{it}$ **(P=1 if P*>0, P=0 otherwise)**

[4] $Y_{it} = \alpha + X_{it}\beta_1 + P_{it}\beta_2 + \nu_{it}$

where P indicates participation in the demonstration, Y denotes some outcome of interest, and i and t index individuals and time, respectively. X represents observable factors that influence both the decision to participate and the outcome; ε_{it} and ν_{it} denote unobservable factors that influence participation and the outcome, respectively; and α, β, and γ are parameters to be estimated.

If there are neither observable nor unobservable factors that influence both selection and the outcome ($\beta_1 = 0$), then a direct comparison of mean values for Y will produce an unbiased estimate of the effect of the demonstration (β_2). This is the case with randomized design, since the random allocation of applicants to either treatment or control ensures that both groups are equal with respect to both unobservable and observable characteristics.

Comparisons of Risk-Adjusted Means

If there are observable factors but not unobservable factors that influence both selection and the outcome, ε_{it} and ν_{it} will be uncorrelated and a fair comparison between participants and non-participants can be achieved by controlling for observable factors. This can be done by estimating Equation [4] alone, with the estimated coefficient on the program participation variable (P_{it}) providing an unbiased measure of the treatment effect. Our first set of models was based on this assumption. In these models, X_{it} was a risk score, defined as total expenditures predicted by demographic characteristics, prior expenditures, and prior diagnoses.

These types of models, commonly referred to as risk adjustment models, use information about individual beneficiaries from basic demographics like age and sex to complex data on prior service utilization and diagnoses to adjust for differences in their baseline health status. It should be kept in mind, however, that it is almost impossible to comprehensively account for differences in overall health status by a statistical model. Risk adjustment models can therefore only serve the narrow purpose of explaining differential risk for a particular outcome of interest. Against this background, we explain the construction of our risk adjustment model here using total expenditures as an example. The described procedure was used in an analogous fashion for all other outcomes analyzed in this report.

The overarching strategy for risk adjustment was to predict total expenditures with a very rich model separately and then use the predicted value from this first model as a single risk score variable in the regression models that tested the treatment effect (Needleman et al., 2001). In other words, the treatment effect models investigated to what degree CNO-participation could explain the deviation of the actually observed expenditures from the estimated expenditures, which were predicted on the basis of beneficiary characteristics. Separately estimating these models streamlined the computations for this project, since a large number of risk adjustment variables could be narrowed down to one single score, increasing both speed of estimation and interpretability of output. In addition, the risk adjustment model had to be run only once per outcome, not repeated for the different tested specifications of the treatment effect. Separate risk adjustment models were run for each CNO site and each year.

The goal for the risk adjustment model was to predict as much variance in expenditures as possible. We therefore estimated a very comprehensive model with information on demographic characteristics, past utilization and past diagnoses. Demographic characteristics were captured by a variable for sex and four variables for age groups (<65, 65-74, 75-84, >85 years of age). Past utilization was captured by including the expenditures incurred during the previous year. It is both plausible and well-established by prior research that such prior utilization measures are highly predictive of future resource use (van Vliet, 1993; Pope, 1998), rendering them ideal candidates for the proposed adjustment model. They are, however, frequently not used in a regulatory or rate-setting context, since basing future payment rates on past utilization creates incentives for maintaining high utilization levels. In addition, we included squared prior expenditures to account for possible non-linear relationships between past and current spending.

To capture past diagnoses, we included 118 indicator variables for the prospective Hierarchical Co-existing Conditions (HCCs). HCCs have been created to calculate risk adjusted capitation

payments for Medicare beneficiaries enrolling in HMOs (Ellis, 1996). They are groupings of ICD-9-CM diagnostic codes into non-mutually exclusive categories based on clinical logic that have been shown to be good predictors of utilization of medical care. This holds particularly true for the so-called concurrent models in which clinical diagnoses in a given time period are used to predict spending in the same time period. Obviously, it is much more difficult to use HCC models to predict future resource use, since acute care episodes account for a large part of medical spending.[16]

Despite the superior predictive power of concurrent HCC models, however, the prospective HCC model appeared more appropriate for this evaluation. Consider the following example: if enrollment in the CNO were so effective that typical enrollee health status were improved (i.e. they had less severe diagnoses), their actual expenditures would be reduced, and the concurrent HCC model would correctly predict those lower expenditures. Consequently, the difference between their actual and predicted expenditures would be small. As our test for the treatment effect relies on a comparison between actual and predicted expenditures, we would incorrectly conclude little or no effect of the demonstration. Thus, the use of concurrent HCC models might unfairly bias the analysis against finding an effect of CNO participation.

In addition to the main effects of the described variables, we included interaction terms between the demographic variables (age, race, and sex) and the HCCs variables. Those interaction terms represent the assumption that the effect of prior illnesses on total expenditures differs by age and sex. Thus, the risk adjustment model takes advantage of the full complexity of beneficiary-level information that was available to the project team.

The resulting model is depicted in Equation [5]:

[5] $$Y_{it} = \alpha_0 + Age_{it}\alpha_1 + Sex_i\alpha_2 + Race_i\alpha_3 + HCC_{it-1}\alpha_4 + Interactions\alpha_5 + Y_{it-1}\alpha_6 + Y_{it-1}^2\alpha_7 + \varepsilon$$

where the subscripts *i* and *t* denote the beneficiary and the year, respectively. Age denotes the age group vector, Sex the sex variable, Race the white/nonwhite indicator, HCC the vector of HCC variables, Interactions the vector of the interaction terms, and Y the expenditures; α_0 through α_7 represent vectors of parameters to be estimated. This model was estimated separately for each site and year, then predicted values ($\hat{Y}$) were saved and used as the risk score in our treatment models (X_{it} in Equation [4]).

The predictive power of the risk adjustment model as applied to the expenditure and utilization variables for Carle Clinic in 1994-1999 is illustrated in Table A.1 (results were similar for the other sites). As expected, the complex model performed better than a model based on past diagnoses alone, as it was able to explain, on average, 11% of the variance in total spending, whereas the prospective HCC models explain only around 8%. The explanatory power varied considerably for the different measures. Items that tend to be recurring, such as home health

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[16] **Ellis et al. (1996) found that concurrent HCC models could predict between 41 and 55 percent of the variance in total expenditures, depending on which HCC variant is used. Prospective models, however, were much less powerful, predicting approximately 8 percent of variance.**

expenditures or physician office visits, can be predicted much more precisely than items that tend to have a strong random component, such as hospital care or ER visits.

In summary, our approach to risk adjustment consisted of the estimation of predicted expenditures on the basis of beneficiary-level information condensed into a risk score. While the approach could be called agnostic, since it did not try to uncover the contribution of each risk score component, the method was highly economical and suitable for the particular purpose of this project. In addition, since the large sample size of the population reference group was already fully exploited when the risk score was created, we were able to estimate the treatment effect by comparing the full treatment group to a 10% random sample of the population reference group, substantially reducing computational demands.

Table A.1

Predictive Power of the Risk Adjustment Model for Expenditure and Utilization Measures: Carle Clinic

	1994	1995	1996	1997	1998	1999	Average
Total Expenditures	12%	12%	11%	9%	13%	11%	11%
In-bundle Expenditures	36%	38%	40%	55%	44%	47%	43%
Out-of-bundle Expenditures	11%	10%	10%	8%	11%	10%	10%
Hospital Expenditures	8%	8%	7%	5%	7%	7%	7%
ER Expenditures	8%	7%	6%	6%	8%	10%	7%
Office Visit Expenditures	34%	34%	34%	34%	37%	38%	35%
Admissions [probability]	11%	10%	10%	10%	10%	10%	10%
ER Visits [probability]	8%	8%	9%	8%	8%	9%	8%
Office Visits [count]	41%	42%	43%	42%	44%	45%	43%

Numbers express percent variance explained by the model, measured by the Pseudo-R^2 for predictions of probability of admission or ER visit, and from the R^2 for all other measures.

Sources: Abt Associates Inc. CNO Evaluation Analytic Files, January, 1994-December, 1999.

Difference-in-Differences

The risk adjustment models described above are sufficient if selection is influenced by purely observable factors. However, if there are both observable and unobservable factors that influence both selection and the outcome, then estimating Equation [4] alone will produce biased estimates of the demonstration effect (β_2) because some of the unobservable factors that drive selection will also cause Y to be systematically high (or low) for participants. For example, if individuals with particularly active lifestyles were recruited to the CNO, then participants will tend to have lower Medicare expenditures than non-participants, even if the CNO itself had no effect on their expenditures.

If these unobservable characteristics of beneficiaries that influence their decisions to participate in the demonstration and also affect their outcomes (expenditures and utilization) are invariant over time, then the difference-in-differences approach can remove the resulting selection bias.

To illustrate, we can rewrite Equations [4] and [5] to decompose the unobservable terms into permanent and transitory components:

[6] $P^*_{it} = X_{it}\gamma + \delta_i + \lambda_t + \varepsilon_{it}$ **(P=1 if P*>0, P=0 otherwise)**

[7] $Y_{it} = \alpha + X_{it}\beta_1 + P_{it}\beta_2 + \delta_i + \lambda_t + \nu_{it}$

where δ_i denotes permanent, unobservable characteristics of beneficiaries (like family history) and λ_t denotes unobservable factors that affect all beneficiaries in the same month (like the weather). Since δ_i and λ_t are unobservable and influence both selection and the outcome, they will result in selection bias unless they can be removed from the estimating equation through algebraic manipulation. The difference in differences estimator achieves this by comparing the change in average Y over time for one group with the change in average Y over time for another. The first difference (the change in average Y for one group) can be written as follows, since the permanent components cancel out:

[8] $$\overline{Y}_t - \overline{Y}_{t-1} = (\overline{X}_t - \overline{X}_{t-1})\beta_1 + (\overline{P}_t - \overline{P}_{t-1})\beta_2 + (\lambda_t - \lambda_{t-1}) + (\overline{\nu}_t - \overline{\nu}_{t-1})$$

where P_{it}-P_{it-1} is equal to zero for the population reference group and one for the treatment group (provided t-1 refers to a period before the treatment effect is expected to be observed). If the expected values of ν_t are always zero, then the second difference (between groups) will cause these terms as well as (λ_t -λ_{t-1}) to cancel out. What remains indicates that the difference in differences for the outcome is the sum of the difference in differences for the predicted outcome (based on the estimated risk score and denoted by $X\beta_1$) and the demonstration effect (β_2). A comparison of differences in differences for actual and predicted outcomes therefore isolates the demonstration effect in an unbiased fashion.

Switching Model

If, even after removing δ_i and λ_t with difference in differences, the unobservable term in the selection equation (ε_{it}) and the unobservable term in the outcomes equation (ν_{it}) are still correlated, then estimation of Equation [8] alone will produce biased estimates. This could be the case if beneficiaries systematically enrolled and disenrolled from month to month based in part on their expected utilization of particular services.

A switching model (see Heckman, 1974 and Maddala, 1983 for background and derivations of this technique) jointly estimates Equations [6] and [7], using the parameters of Equation [6] to calculate the expected values of ε_{it} for participants and non-participants. These quantities, denoted $E(\varepsilon_{it}|P=1)$ and $E(\varepsilon_{it}|P=0)$ are the unobservable factors that cause the difference in differences estimates to be biased. Thus, the switching framework estimates specifications given by Equation [9], based on results from estimating Equation [6].

[9] $Y_{it} = (\alpha + \beta_2) + X_{it}\beta_1 + \delta_i + \lambda_t + E(\varepsilon_{it}|P=1) + \nu_{it}$ **for participants**

$Y_{it} = \alpha + X_{it}\beta_1 + \delta_i + \lambda_t + E(\varepsilon_{it}|P=0) + \nu_{it}$ **for non-participants**

In this context, the demonstration effect may be observed in the difference in constant terms ($(\alpha + \beta_2) - \alpha = \beta_2$) and in any differences between participants and non-participants in the effects of risk adjusters ($X_{it}\beta_1$). In principle, this additional information could identify sub-groups of participants for whom the demonstration effect was most pronounced.

An important consideration when specifying such a model concerns the identification of Equation [6] as distinct from Equation [9]. Since these equations contain many of the same right-hand side variables, the distinction between the two equations can be difficult to defend unless factors are identified that can reasonably be expected to influence the decision to participate (Equation [6]) but not the levels of utilization and expenditure (Equation [9]). Fortunately for our purposes, the demonstration sites did not enroll applicants in a uniform fashion over time. To the contrary, new enrollment was most intense at the outset of the demonstration and subsequently fluctuated depending on site-specific factors and the policy environment. The probability that a beneficiary will apply to the demonstration is therefore affected by the overall enrollment rate at their site in that month. To reflect the inclusion of this variable in the selection equation we have rewritten Equation [6] as Equation [6'] below

[6'] $P^*_{it} = X_{it}\gamma_1 + S_{it}\gamma_2 + \delta_i + \lambda_t + \varepsilon_{it}$ **(P=1 if P*>0, P=0 otherwise)**

where S_{it} denotes the site- and time-specific sign-up rate. Equation [6'] is estimated using probit techniques, Then, the estimated parameters are used to construct estimated values of $E(\varepsilon_{it}|P=1)$ and $E(\varepsilon_{it}|P=0)$ which are included in Equation [9]. Equation [9] is then estimated by ordinary least squares.

Finally, there remains a rather technical issue that can be resolved easily. Because this is a two-step procedure, the standard errors for the second step should be estimated in a way that takes into account the randomness of the first step. This is done by bootstrapping the second-step standard errors (see Efron, 1993 for background on bootstrapping).

Appendix B: Sample Sizes for Chapters 4

Table B.1

Description of the Samples for the Regression Based Analyses: Number of Person-Years Analyzed for Different Definitions of the Treatment Group

	Carle	Carondelet	LAH/BNP	VNSNY	Total
Randomized to treatment (Tables 4.1.1, 4.2.1, 4.3.1, 4.4.1, 4.4.2, 4.4.3)					
T	18,781	19,706	10,200	11,012	59,699
P	44,847	39,085	52,093	60,823	196,848
Total	63,628	58,791	62,293	71,835	256,547
Randomized to treatment and enrolled for at least six months (Table 4.2.2)					
T	13,395	13,396	7,930	7,406	42,127
P	44,847	39,085	52,093	60,823	196,848
Total	58,242	52,481	60,023	68,229	238,975

T denotes treatment group, P population reference group. Note that the sample sizes for the population reference group listed here represent random samples of the full population reference group (a 2.5% sample in the case of VNSNY and 10% for other sites).

Sources: Abt Associates Inc. CNO Evaluation Analytic Files, January, 1994-July, 2000.

Appendix C: Results for Other Samples

Table C.1

Estimated Differences between the CNO Treatment Group Enrolled on July 1, 1997 and for at Least Six Months Thereafter and the Population Reference Group: Changes in Per Month Spending for All Services, Relative to 1994

Year	Carle	Carondelet	LAH/BNP	VNSNY
1997	$22	$98 **	$47	$74
1998	$66 *	$120 **	$101 *	$285 ***
1999	$80 *	$158 ***	$195 ***	$135
2000	$106 *	$107 *	$137 **	$361 **

All dollar amounts were converted to 1999 constant dollars. All differences are adjusted for beneficiary risk and year effects. The CNO treatment group consists of all those randomized to treatment or directly enrolled in the CNO provided they were enrolled on July 1, 1997 and enrolled for at least six months thereafter. Results for year 2000 are based on data through July, 2000 (data beyond July, 2000 were not available for this study). ~ denotes significance at p<0.1; * p<0.05; ** p<0.01; ***p<0.001.

Sources: ***Abt Associates Inc. CNO Evaluation Analytic Files***

Table C.2

Estimated Differences between the CNO Treatment Group Enrolled on January 1, 2000 and for at Least Six Months Thereafter and the Population Reference Group: Changes in Per Month Spending in Year 2000 for All Services, Relative to 1994

Year	Carle	Carondelet	LAH/BNP	VNSNY
2000	$5	$50	$37	$150

All dollar amounts were converted to 1999 constant dollars. All differences are adjusted for beneficiary risk and year effects. The CNO treatment group consists of all those randomized to treatment or directly enrolled in the CNO provided they were enrolled on January 1, 2000 and enrolled for at least six months thereafter. Results for year 2000 are based on data through July, 2000 (data beyond July, 2000 were not available for this study). ~ denotes significance at p<0.1; * p<0.05; ** p<0.01; ***p<0.001.

Sources: ***Abt Associates Inc. CNO Evaluation Analytic Files***

Appendix D: 2001 CNO Satisfaction Survey Results

Visiting Nurse Service of New York

Community Nursing Organization

Enrollee Satisfaction Survey

850 Surveys were mailed to patients currently enrolled as of January 1st 2001 and 611, 72% were returned within 3 weeks.

The questions and the responses were as follows:

Question/Response	# of Responses	Percentage
I am satisfied with the care I am receiving from VNSNY's CNO program:		
Strongly Agree	386	63.2%
Agree	180	29.5%
Disagree	27	4.4%
Strongly Disagree	2	0.3%
No Response	14	2.3%
The services provided by my CNO Nurse Consultant have helped me with my health needs and problems:		
Strongly Agree	368	60.2%
Agree	186	30.4%
Disagree	37	6.1%
Strongly Disagree	3	0.5%
No Response	15	2.5%
My Nurse Consultant has been available when I needed her:		
Strongly Agree	372	60.8%
Agree	186	30.4%
Disagree	37	6.1%
Strongly Disagree	0	0%
No Response	14	2.3%
Participating in VNSNY's CNO program is worth my time:		
Strongly Agree	386	63.2%
Agree	173	28.3%
Disagree	35	5.7%
Strongly Disagree	3	0.5%
No Response	12	2%
I would recommend the CNO program to others:		
Strongly Agree	411	67.3%
Agree	162	26.5%
Disagree	24	3.9%
Strongly Disagree	2	0.3%
No Response	9	1.5%

Comments:

Comments were added to 253, or 41%, of the surveys. Of the 253 comments 96% were positive.

The positive comments can be divided into several broad categories. Many comments addressed more than one of the categories and in such cases comments were attributed to multiple categories as follows:

79 responses included positive comments about specifically named nurse consultants
77 expressed strong satisfaction with or appreciation for CNO services
53 specifically said "thank you" for access to the nurse consultant/CNO services
30 referenced the caring attitude and skills of the nurse
29 expressed the CNO "should always continue" and/or the respondent "couldn't do without" the CNO
12 commented that the availability of the CNO was a comfort
10 noted that senior centers/all senior centers needed CNO services
3 explained how the CNO made things financially easier
2 noted the nurse consultant was more satisfactory than their MDs

Seven of the surveys included follow-up information for the nurse or suggestions. For example, requesting information for someone else to join the program; senior center scheduling changes; requesting bereavement service information and requesting eggplant instead of meat from MOW.

Specific suggestions for CNO improvement were included on twelve of the 611 returned surveys. There were two complaints about the style of their nurse consultant versus the style of a previous nurse consultant and three had specific home health aide complaints. These were not signed, nor did they have return addresses on the envelope and so couldn't be addressed. Seven of the comments were wishes for increased home health aide service. When identification was present, there was follow-up to address these concerns.

Conclusion

A review of the comments provides colorful, enthusiastic and detailed support for the responses to the survey questions. Each question received a "strongly agree" by

60% or more of the respondents and together the "strongly agree" and "agree" responses totaled over 90% for all questions. 92% of the respondents either agreed or strongly agreed that they were satisfied with the CNO. And nearly 94% would recommend the program to others.

The current average length of enrollment is more than three years. Contact with enrollees varies according to their clinical stability and so varies dramatically from visits several times per week for some enrollees to those who have contact with the nurse only several times per year. The survey results are testimony to the staff for being able to maintain satisfactory long term relationships with enrollees under varying circumstances in this unique program.

An extensive sampling of the comments follows. All the comments are available upon request.

Comments
• I get more satisfaction and care from my nurse than from my doctor. Thank you.
• Thank you for the wonderful job you do. I am glad that I am a member.
• My nurse Claire has been of infinite help to me to in a very difficult physical condition.
• This service has been extremely important to me. I hope it will last for a long time.
• It is such a comfort to know there is someone with whom I can talk to when the need is there. My children are all far away and most of my friends have ills of their own. I thank God so far I am in good health, but when I did need to talk to someone was there to help.
• Ms. Colleen O'Hara has been a Godsend. I greatly appreciate her wonderful care of my mother. It is wonderful to know that I can rely on her when I need it to care for my mother. Thank you very much.
• This is one of the best programs any senior could have. You can't imagine how much you've helped us!
• My reply is not confidential, for I have spread my respect and thanks for the CNO nurses and services since 1994. I have recommended a few friends also.
• My nurse is a loving angel. Her name is Anne.
• No matter the cost this service is well worth it.
• My nurse's name is Luisa. She is doing a fine job taking care of my badly ulcerated legs. She comes to see me faithfully, checks my heart, pulse, and BP, offers me many words of encouragement when I am down and depressed.
• I am more than pleased with the fine attention my sister and I receive from our visiting nurse Eugenie.
• Please keep this program going. Thank you.
• I have known Martha Fortune for the past two years and she has been very helpful in many situations for myself and my husband Edward. She is what I call a "Pro" in her field, with understanding, compassion, and great medical knowledge.
• I can only hope this service will continue – it gives me peace of mind to know that I have a caring nurse – and available service at a stone's throw. Keep it up!!
• Rita is a very fine person, nice to talk to and confide in. I am sure she is helpful to many people. She comes to ROAC on Tuesday and the people look forward to her. Thank you!
• The visiting nurse along with the staff at the CNO are absolutely wonderful. They are hardworking individuals and should be commended for all that they do.
• My nurse, Anne Muenzinger, has taken excellent care of me. She has worked with me and for me, to this point of my health level. I don't know what would have become of me without her help.
• This was one of the best things I ever did when I joined VNS. You've always been there for me when I

needed help which I'm very grateful for.

- Yours is the greatest! Your nurses have been of tremendous help to us so many times – I don't know how we could have managed so much trouble without your help.
- Our nurse, Phyllis, is superb. She is very knowledgeable and has helped us (my husband and me) a great deal. We are absolutely delighted that we joined your organization. Thank you!
- All Senior Centers need this type of service (Mary Jean Brennan is A1, she came to my house after my operation to check me out and fixed my bandage).
- The Community Nursing Services is a family taking care of seniors as if they were their own family. – Our grateful thanks to all.
- Very caring informed nurses who give time and understanding to one's situations along with very sensible advice.
- Please thank Rosemarie for the "cozy coverlet". It keeps me warm as I relax on the sofa.
- I don't know what I would have done if it weren't for Michele Freeman at a time of grief. She was wonderful. God bless her and your organization.
- The VNS of NY has been of great help to my wife and me. It's a source of comfort to know that in time of need that we can count on such a competent and caring organization.
- Having VNS has been extremely helpful, helping me to understand my condition, informative and very understanding. She (in my opinion) is better than the doctors. Mary Jean Brennan has been a God-send to me. Jennie too! M. Diercks is an excellent therapist. Thank you!
- I am very grateful to VNS. It makes it a little easier financially. A million thanks.
- Jean Arnold has been a lifesaver at my house at my time of need and most patient and helpful at Sheriden. So glad to have this opportunity to express myself.
- God bless all of you. I am very pleased and my visiting nurse, Ms. Bony, is so efficient, understanding and helpful.
- My visiting nurse is Jane Kizner, the most wonderful lady and a very caring lady. I love her, I look forward to her visits.
- I hope the funding for this very valuable program continues indefinitely.
- As a retired RN I feel this is a good program and I hope it will continue.
- It is such a wonderful gift to feel I can call the CNO with any health problems and be certain that I will get caring responses to my questions. It's like having someone knowledgeable to hold your hand. Thank you from the bottom of my heart.
- It is comforting to have someone to call on.
- I am very fortunate to have a VNS program in my community. The nurse is always ready to help when needed. Please continue the program. Thanks.
- I would be very helpless without your services and thank you very much. I am 97 years old.
- I am not only satisfied that the nurses who have helped me, since I had a stroke 8 years ago. They helped me recover and regain my life and I thank them from the bottom of my heart, They are kind, well informed, and intelligent.
- I hope they will always continue. Ruth is excellent.
- Without their help I don't know how I could cope. They are always here for me. God bless all of them.
- Thanks you so much for caring for us, your nurses are so kind and very helpful to me, it is a wonderful organized gift to all of us senior citizens. Thank you again.

Suggestions/Indications for improvement included the following:

- It would be more convenient if the Visiting Nurse were able to come twice a month instead of once a month to the senior centers.
- When a client has a 6-mo interview why can't an appointment be arranged so that other people do not have to wait a half-hour. We should be informed when such an interview is to be made sometimes we have to wait a very long time.
- I was advised to notify VNS if I needed surgery. Why? A newsletter could give this info.
- The HHA's provided by VNS/Personal Touch have been a source of problems.
- I find my nurse very rigid, not like the one I had before who showed a lot more compassion.
- The nurse assigned to me seems to want to be in charge & tries to tell my family what to do. She upsets me & subsequently upsets my family. I would think you would have staff members who were more caring.

- The home care person who provides direct services consistently give very limited services. Once these limited services have been completed the person simply sits. Also, even though the discharge planning is done before the hospital discharge the service is often slow to get to the home.

Carle Clinic Association
Community Nursing Organization

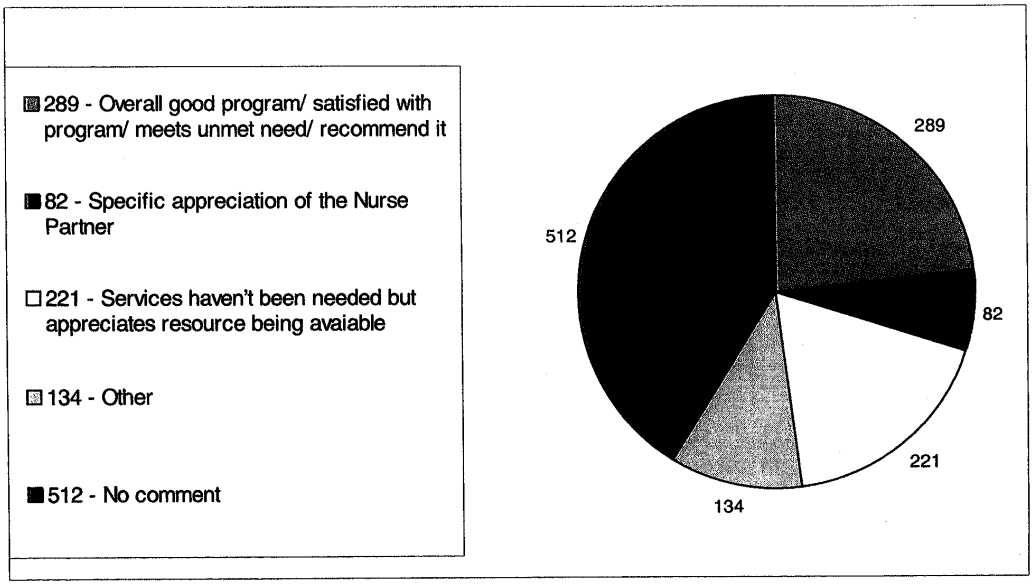
Enrollee Satisfaction Survey

In April 2001, 1950 surveys were mailed to CNO members who were enrolled as of March 1, 2001. As of May 21, 2001, 1235 completed surveys were returned, for a response rate of 63%. The questions and response frequencies are listed below:

Question/Response	# of Responses	Percentage
I am satisfied with the care I am receiving from Carle's CNO program:		
Strongly Agree	585	47.4%
Agree	425	34.4%
Combined response Strongly Agree and Agree	***1010***	***81.8%***
Disagree	24	1.9%
Strongly Disagree	20	1.6%
No Response	181	14.7%
The services provided by my CNO Nurse Partner have helped me with my health needs and problems:		
Strongly Agree	518	41.9%
Agree	454	30.8%
Combined response Strongly Agree and Agree	***972***	***78.7%***
Disagree	26	2.1%
Strongly Disagree	23	1.9%
No Response	214	17.3%
My Nurse Partner has been available when I needed her:		
Strongly Agree	482	39%
Agree	477	38.6%
Combined response Strongly Agree and Agree	***959***	***77.7%***
Disagree	22	1.8%
Strongly Disagree	18	1.5%
No Response	236	19.1%
Participating in Carle's CNO program is worth my time:		
Strongly Agree	600	48.6%
Agree	432	35.0%
Combined response Strongly Agree and Agree	***1032***	***83.6%***
Disagree	15	1.2%
Strongly Disagree	22	1.8%
No Response	166	13.4%
I would recommend Carle's CNO program to others:		
Strongly Agree	632	51.2%
Agree	399	32.3%
Combined response Strongly Agree and Agree	***1031***	***83.5%***
Disagree	15	1.2%
Strongly Disagree	21	1.7%
No Response	168	13.6%

Carle CNO Survey Comments (2001):
The Carle CNO Satisfaction survey allowed for member's open-ended comments. These comments were categorized into themes. Theme summaries are listed below. Note that each response was categorized only in one area, even though, at times the response reflected numerous pieces of information. The strongest statement was used as the guide. The number of responses for each category is listed below and a percentage comparing the number of responses to the total number of responses made. A full listing of comments is available.

General Category	Total # of Comments	% of Total Comments
No Comments written on survey	512	
Overall good program/ satisfied with program/ meets unmet need/ recommend it	289	40%
Services haven't been needed but appreciates resource being available	221	11%
Specific appreciation of the Nurse Partner	82	16%
Other	134	33%



Samples of Comments by Category:

Overall good program/ satisfied with program/ meets unmet need/ recommend it

- I would feel lost without it! Please continue it.
- Good program. CNO nurses always nice people.
- CNO has been a great help to me. I am not sure who my nurse partner is but I call Jacque at Carle on College in Bloomington.
- It is the best thing since Mother's apple pie.
- The services we have received have been a real blessing. I hope it will always be available.
- They have been there when we needed them!
- We have had very good luck with CNO for several years.
- I appreciate the time the CNO nurse takes coming to my home for the talk, discussion on my health or any problems and the exam.
- This program has been very helpful.
- All the CNO nurses were interested in my well being and kept in touch with my Carle doctor and offered suggestions on care.
- This is a service which could make problems treatable which might otherwise be passed by.
- Our CNO nurse has been a good friend and help. We appreciate the CNO program.
- It is a good and worthwhile program.
- The CNO had always been there for me. In fact if I don't contact them, they call me to see if all is well. They have been wonderful!
- This is a fine program and gives me peace of mind that I can talk to my CNO nurse whenever I have a questions regarding any health problems, before making an unnecessary Dr's appt
- A valuable service
- They have been very helpful to me in every way. Thanks.
- Have picked up some good health tips. Thank you!
- With all the problems I've had in the last year, CNO has saved me quite a lot of money, for which I am very thankful.
- Thank you - you were very helpful when my husband needed you--"you were there"
- My CNO nurse has always answered all my questions advised me and has always helped me through all my difficult times and stress. I don't think I would have made it with out her.
- Since it is very difficult for my mother to get ready and get to the Doctor's office this is very helpful for us. Thank you so much!
- I have not needed your services but it is helpful to know you are available if needed.
- I've appreciate help with therapy, my cane and check-ups, conversations with my CNO contacts.
- It is very helpful. My husband has Alzheimer's and I had a stroke in Jan. I am thankful to be a member of CNO.
- The CNO nurse is the only individual who telephones regularly to ask how I am getting along physically and who is really interested in my response
- I am fortunate to have this service I thank you for it.

Specific appreciation of the Nurse Partner

- Phyllis is very helpful.
- I was in her office just before my heart attack she took me up to see the doctor, from there I was admitted to the hospital. I am forever in her debt. She is Mrs. Latoz.
- We always looked forward to our monthly visit with our CNO nurse. Sharon Latoz, until the winter of 2000-01, when it was stopped almost without warning. We had expressed by letter how important the visits were to us. She kept us informed about our vital signs and always checked on

anything unusual in our daily living, that might require a visit to our physician. She has called us since with questions and information about the fact that we may call at any time, but it is not as handy for us to visit with her at Carle, since my husband is handicapped.

- We do appreciate the fact that we are being helped with medical supplies through the CNO. Thank you. A great staff, received great service from Barbara Long, outstanding personality. Thanks very much.
- I have been very pleased with the care and concern shown me by CNO personnel with calls, with some donated supplies, actual taken care of a wound which has taken so long to heal. Thank you!
- Heart attack 2 surgery of the stomach and the CNO nurses were there for us- to help in any way and doing a very good job many helpful things at times of need. And I appreciate all the nurses' help.
- My CNO nurse has always answered all my questions advised me and has always helped me through all my difficult times and stress. I don't think I would have made it with out her.
- I have not had need of the CNO nurse of late but Chris Kaufman was the person who helped me get the testing kit and supplies after I was diagnosed with diabetes. It has been very helpful to have a nurse available when I need answers that my doctor is too busy to ask him.
- I have enjoyed all my nurses - I've had - through your program. Jacquie Helphinstine is my present nurse. She is efficient, kind and always helpful - I'm always pleased with her help - and think she is a wonderful person - as well as a very good nurse. Thank you for all the help I have received throughout this wonderful program.

Services haven't been needed but appreciates resource being available

- Thus far I have received no tangible or direct benefit from participation, but nevertheless still believe it is a worthwhile program and provides some assurance that there would be real benefit in possible future circumstances.
- Feel ace in the hole for later use.
- I have not needed the services of this organization as yet.
- So far I haven't had to use the services but it is nice to know that I can.
- Have not needed the program thus far but feel it is a good program.
- I haven't had to use CNO much-thank God! But I feel good knowing it's an option for me if I do need it.
- I have never needed to use this program. I'm not sure if I should have joined until I need it. At age 82 I may need it someday. If Medicare or Heath Org. is paying for this maybe it's not fair for me to have joined. It's a comfort to me to know that if I need this help someday it is there.
- I'm very healthy and do not need help but if I ever do I will call the CNO.
- Fortunately I have not needed and called on the services of CNO, but feel confident that if I needed to they would be helpful. Basically I have gone in when called and answered questions and the time spent was worthwhile. Thank-you.
- It gives me a sense of security.
- We have just recently applied for CNO it case we need it sometime soon. We have heard good things about CNO from relatives that have it.

Summary

In summary, the CNO satisfaction survey, completed by 63% of enrolled members indicated that:

- 81.8% responded that they were satisfied with the care received through the CNO ("agree" and/or "strongly agree").
- 83.5% indicated they would recommend the CNO to others ("agree" and/or "strongly agree").
- Comments supported their appreciation of the services and the nurse resource.