Effects of the Medicare Alzheimer's Disease Demonstration on Medicare Expenditures

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Applicants were randomized either into a group with a limited Medicare community care service benefit and case management or into a control group receiving their regular medical care. Analyses assess whether or not community care management affected health care use. A tendency toward reduced expenditures was observed for the treatment group, combining all demonstration sites, and when observing each separately. These differences were or approached statistical significance in two sites for Medicare Part A and Parts A and B expenditures averaged over 3 years. Expenditure reductions approached budget neutrality with program costs in two sites.

INTRODUCTION

Dementia is a highly disabling and destructive condition that has high social and political visibility due to its prevalence (11.3 percent among those age 65 years or over, 47.6 percent among those 85 years or over) (Evans et al., 1990) and its effects on family and community support systems. Much of the analysis of cost for this population has been concerned with imputing the economic value of care provided by family members, documenting acute and long-term care costs (such as payments for community-based services and nursing home care), and estimating the indirect costs of caregiving such as from stress and burden of family members and earnings foregone from employment (e.g., Ernst and Hay, 1997; Max, Webber, and Fox, 1995; Rice et al., 1993; Scharlach and Boyd, 1989; Stommel, Collins, and Given, 1994; Weinberger et al., 1993; Wimo, Ljunggren, and Winblad, 1997). Some recent work has examined the effects of dementia on annual Medicare expenditures (Gruenberg, Kaganova, and Hornbrook, 1996; Pope et al., 1996; Weiner et al., 1998).

This article considers whether participation in a case management program by persons with dementia affected their total Medicare expenditures. The case management program's involvement with the families was expected to have an indirect effect on such use and expenditures, even though the program was not designed or implemented so as to expressly coordinate with primary care physicians. Case manager involvement may have affected the quality of primary care through improved patient advocacy, monitoring of health status among clients and caregivers, and provision of instrumental support to caregivers.

Demonstration Program

The Medicare Alzheimer's Disease Demonstration and Evaluation consisted of two basic components. One was the assignment of demonstration treatment subjects to a case manager (either a social worker or nurse) who worked with family caregivers to plan and coordinate

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community services, and when needed, helped train caregivers about disease progression and the evolving commensurate functional assistance tasks that caregivers would have to perform. The second component was access to a special Medicare benefit that allowed for the reimbursement of community care services.

Two case management models were implemented that differed by case manager-to-client ratios and per-month service expenditure ceilings for each client. Model A sites operated with a target case manager-to-client ratio of 1:100 and had a monthly community service reimbursement limit or cap of $290 to $489 per month per client. Model B sites operated with a target case manager-to-client ratio of 1:30 and had a slightly higher reimbursement limit of $430 to $699 per month per client. The per month reimbursement caps in each model varied by site and over time due to regional cost variations and inflation adjustments.

Regular Medicare services—hospital, skilled nursing, home health, durable medical equipment (DME), physician services, and all other services normally reimbursed under Medicare Parts A and B—continued to be available. Services reimbursed under the demonstration's special benefits included: adult day care; homemaker/personal care; housekeeping; general chore (i.e., heavy cleaning); companion (e.g., friendly visiting, caretaking while caregiver attended educational and/or support groups); non-emergency transportation for client; adaptive and assistive equipment (not otherwise reimbursed as DME under Part B); medical supplies used in conjunction with skilled and unskilled home care (not otherwise reimbursed under Medicare skilled home health care); consumable goods (such as incontinence supplies); home repairs and safety modifications to the home.[1] Clients and families, other than Medicaid participants, paid a 20-percent copayment for demonstration services.

Case management and caregiver support services were reimbursed separately from the capped demonstration benefit. These services included caregiver education and training, caregiver support groups, and caregiver transportation to education and support groups. The costs for these services were included in the administrative budgets of the demonstration programs.

Eight sites, four in each model, participated in the demonstration.[2] The sites became operational in December 1989 and served clients and their families until November 30, 1994. Eligibility for the demonstration required a physician-certified diagnosis of an irreversible dementia (e.g., Alzheimer's disease or vascular dementia), client enrollment in (or eligibility for) both Parts A and B of the Medicare program, and residence in a community setting in the demonstration site's catchment area. Nursing home residents at the time of enrollment were ineligible. Although no restrictions were placed on a participant's ability to use community services, persons receiving Medicaid case management services at the time of application (for example, through Medicaid home and community-based care programs) were generally not accepted into the demonstration. These individuals were excluded because their case management and benefits were equal to or greater than

[1] Additional, but seldom used, services included skilled nursing and rehabiitation nursing therapies (i.e., speech, occupational, physical; home health aide) home health aide, and home-delivered meals.

[2] Model A (lower reimbursement-higher caseload) sites were located in Champaign/Urbana, IL; Memphis, TN; Portland, OR; and Rochester, NY. Model B (higher reimbursement-lower caseload) sites were located in Cincinnati, OH; Miami, FL; Minneapolis, MN; and Parkersburg, WV.

that available through the demonstration. This left the demonstration little opportunity to affect client outcomes. All exclusions occurred prior to randomization. Treatment or control group members becoming eligible for such programs after enrollment remained in the demonstration.

Enrollment into the demonstration was voluntary. Recruitment included information about the program being a demonstration and the fact that some applicants would be assigned to a control group. The demonstration was designed to maximize the project's ability to test the effectiveness of the intervention, not specifically to test its generalizability to the full eligible population. How the requirements of a demonstration may have influenced the decision of the dementia population and their caregivers about whether to apply for the program is unknown. Records were not maintained on individuals who contacted the program, unless they submitted a completed application. Randomized assignment of cases was used to minimize any influence of choice among such applicants. This design precluded testing what the treatment effect would have been in a probability sample of the eligible dementia population.

Study subjects were recruited using both physician referral and self-referral. Data on applicants obtained from a screening questionnaire and physician referral form were the basis for establishing eligibility for the program. Those found to be eligible were then randomly assigned by the evaluators to either the treatment group (where they were eligible for case management and service coverage), or into a control group (where they continued to receive their usual care). Once enrolled in the demonstration (either as treatment or control), participants were given a semiannual reassessment throughout their participation (i.e., to death, to 12 months following a permanent nursing home placement, or to the end of study at 36 months). Clients lost their eligibility for community demonstration services and case management once they had been permanently placed into a nursing home. The intake assessments were conducted via in-person interviews and the reassessments were conducted predominantly via telephone. Medicare claims data for each study subject also were obtained for the period starting 12 months prior to enrollment, through the 36 months of participation, death, or health maintenance organization (HMO) enrollment A total of 4,151 participants enrolled into the treatment group and 3,944 into the control group.[3]

Hypotheses

The intervention of case management and community service reimbursement had a strong, consistent, and positive impact on the likelihood of using such community-based, long-term care services as homemaker/chore, personal care, companion services, and adult day care.[4] Treatment group clients were at least twice as likely as control group members to use one or more of these four services (Newcomer et al., 1999b). These outcomes are consistent with the primary objectives of the demonstration.

The analyses reported in the current article examine whether increased access to community care and case manager oversight had a secondary consequence related to health care use and expenditures. While the demonstration was not explicitly directed to affecting health care use, reductions

[3] Applicants during the first 24 months of operation constituted a closed panel of cases for the evaluation. The programs continued to enroll members for 2 years after that so they could maintain a steady-state client-to-case-manager ratio. Evaluation data were not collected on these later enrollee cohorts.

[4] Together these services accounted for more than 80 percent of the services used by demonstration participants (consumable supplies being the other most commonly used benefit).

in use are necessary for the program to approach budget neutrality for Medicare. Several hypotheses are tested related to total Medicare health care expenditures for participants, all of whom were in the Medicare fee-for-service (FFS) system:

- Medicare Part A (largely hospital and skilled nursing home inpatient care) expenditures will be lower in the treatment group compared with controls.
- Medicare Part B expenditures (inclusive of physician and outpatient care, some home health, and DME) will be higher in the treatment group compared with controls.
- Total expenditures combining Part A and Part B will be lower in the treatment group compared with controls.
- The treatment effect on Medicare Part A and Parts A and B expenditures will be greater in Model B than in Model A.
- Medicare Part A and Parts A and B expenditures will be lower in the site that used predominantly nurse case managers than in sites with social worker case managers.

Treatment effects were hypothesized to occur, in part, because of home care worker contacts with the client with dementia and their caregivers and regular contacts between case managers and caregivers. Such contacts potentially gave case managers ongoing information about: (a) the ability of the informal caregiver to provide care and the adequacy of the care they provided; and (b) changes in the health and functional status of the client. Such information, shared with the client's health care providers, may have had a preventive effect in that it could facilitate interventions that reduced avoidable physical deterioration, hospitalization, and emergency room use. Case manager involvement also had an assumed potential (given requests by the family or health care providers) to be responsive to the care and support needs of clients and caregivers after critical events such as hospital stays, possibly reducing home health costs and nursing home stays.

The case manager's oversight and patient advocacy are hypothesized to decrease use of hospital-related (mostly Part A) services, and increase use of ambulatory care (mostly Part B) services. Combining Part A and Part B expenditures permits evaluation of whether increased ambulatory care expenditures associated with greater medical management were offset by decreased acute care expenditures for hospital and nursing home care.

Demonstration treatment group participants in the Model B (i.e., higher resource) program sites are hypothesized to have lower health care expenditures than were treatment group participants in Model A (i.e., lower resource) sites due to the greater availability of care manager resources. The Illinois program, which featured only nurse care managers (and not social workers), is hypothesized to show lower total health care costs than the other sites, due to the greater involvement of demonstration nurses in clinical care. Other sites generally used social workers as case managers, although they had access to nurse consultants. (Refer to Newcomer, Arnsberger, and Zhang, 1997 for a discussion of operational differences in case management among demonstration sites as measured by client chart reviews.)

These hypotheses were tested under varying period and geographic aggregations. Models were estimated for the entire period that participants resided in the community (the "entire exposure period," and for each of up to 3 separate years (year 1, year 2, and year 3) that they resided in the community. These period analyses were conducted combining subjects in all study sites. Only the entire exposure period was used in analyses for each of the eight demonstration sites (com-

munities). The focus on study subjects only during their residence in the community (i.e., not in a nursing home) recognizes that being in the community was a demonstration criterion for receiving case management and home care services. Determining the effect on total expenditures regardless of location (i.e., community versus nursing home) would be confounded by the fact that a case would not be affected by the demonstration's resources after entering a nursing home. Site-specific replications of the analysis recognize that demonstration effects may vary among communities due to several factors, including the two different program models, within-model operational differences, and/or practice variations among the communities.

Methodology

The analyses were conducted using data from caregiver assessment interviews linked to Medicare claims records. Claims records were available only for beneficiaries who received their health care through Medicare FFS reimbursement (n = 5,649), as service encounters within managed care systems (e.g., HMOs) are paid on a capitation basis and HCFA does not receive service claims records for those encounters.

The study sample entered the demonstration from December 1989 through November 1991. Assessment data were collected at baseline and semi-annually on each case that continued to reside in the community.[5] The baseline instrument was used for year 1 analyses. The 12- or 24-month assessments (if the instruments were available or the immediately prior one otherwise) were used for year 2 and 3 analyses, respectively. Analyses based on aggregations of cost over more than 1 year used baseline instruments for client and caregiver covariates.

Randomization produced highly comparable groups of treatments and controls at baseline, but covariates were used in the analysis to increase the precision of the estimates of the effect of the demonstration on Medicare expenditures (by reducing unexplained variance in the outcome variable). There were no differences in nursing home placement or sample attrition (due to death or other reasons) between treatment and control groups (Miller et al., 1999).

The assessment instruments include data on: client and caregiver functional status, as measured by a version of the Katz activities of daily living (ADL) scale (Katz et al., 1963) and Lawton and Brody's instrumental activities of daily living (IADL) scale (Lawton and Brody, 1969); client cognitive status, as measured by the mini-mental status examination (Folstein, Folstein, and McHugh, 1975); client behavioral problems, as measured by an adaptation of the index developed by Zarit, Todd, and Zarit (1986); caregiver burden, as measured by a revised version of Zarit's caregiver burden scale (Zarit, Reever, and Bach-Peterson, 1980); caregiver depression, as measured by the brief version of the Geriatric Depression Scale (Yesavage et al., 1983); and service utilization. The scale ranges are shown in Table 1.

Additional client and caregiver data included: age; insurance; educational level; living arrangement; ethnicity; marital status; sex; relationship between client and caregiver; and income.

The client measures generally follow the Anderson framework of predisposing, enabling, and need characteristics of clients (Anderson and Newman, 1973). Client measures were expanded to include cognitive impairment (Bass, Looman, and

[5] Patients enrolling between May and November 1991 constituted a supplemental sample (n=2,310 cases, selected to assure the evaluation of an adequate sample size for the full 36 months of exposure in the demonstration. Assessment data were collected at baseline, 24 and 36 months for this group.

Table 1
Characteristics of the Analysis Sample

Characteristic	Treatment Group		Control Group	
	Count	Percent	Count	Percent
Client Age				
20-64 Years	45	1.6	48	1.7
65-69 Years	280	9.7	253	9.1
70-79 Years	1,219	42.4	1,160	41.8
80-84 Years	693	24.1	700	25.2
85-90 Years	452	15.7	451	16.3
90 Years or Over	186	6.5	163	5.9
Client Sex (Female)	1,770	61.6	1,650	59.5
Client Race				
White	2,471	85.9	2,379	85.7
Black	297	10.3	288	10.4
Hispanic	92	3.2	95	3.4
Other	15	0.5	13	0.5
Client Lives with Relatives	1,024	35.6	1,007	36.3
Client Lives in Own Home	1,851	64.4	1,768	63.7
Client Income				
Less than $5,000	242	8.4	212	7.6
$5,000 - $9,999	743	25.8	780	28.1
$10,000-$19,999	973	33.8	949	34.2
$20,000-$30,000	425	14.8	379	13.7
$30,000 or More	354	12.3	328	11.8
Unknown	138	4.8	127	4.6
Medicaid Recipient in Any Period	216	7.5	213	7.7
Medicare Supplemental Insurance	1,333	46.4	1,250	45.0
Entered Health Maintenance Organization (After Baseline)	65	2.3	64	2.3
Client Activities of Daily Living Limitations[1]				
0-1	543	18.9	554	20.0
1.5-2	459	16.0	450	16.2
2.5-3	390	13.6	362	13.0
3.5-5	647	22.5	595	21.4
5.5-8	486	16.9	476	17.2
8.5-10	350	12.2	337	12.1
Client Instrumental Activities of Daily Living Limitations[2]				
0-3	171	5.9	177	6.4
3.5-5.5	515	17.9	534	19.2
6-6.5	456	15.9	412	14.8
7-7.5	787	27.4	766	27.6
8	946	32.9	885	31.9
Client Mini-Mental Status Examination Score[3]				
0	306	10.6	278	10.0
1-5	244	8.5	226	8.1
6-10	292	10.2	292	10.5
11-15	448	15.6	469	16.9
16-20	617	21.5	573	20.6
21-25	543	18.9	500	18.0
25-30	256	8.9	261	9.4
Missing	169	5.9	176	6.3
Client Behavior Problems[4]				
0-4	439	15.3	385	13.9
5-7	625	21.7	577	20.8
8-10	717	24.9	723	26.1
11-13	649	22.6	609	21.9
14-19	430	15.0	471	17.0
Missing	15	0.5	10	0.4

See footnotes at end of table.

Table 1—Continued
Characteristics of the Analysis Sample

Characteristic	Treatment Group Count	Treatment Group Percent	Control Group Count	Control Group Percent
Caregiver Relationship to Client and Sex				
Husband	465	16.2	410	14.8
Wife	839	29.2	827	29.8
Daughter	855	29.7	787	28.4
Son	237	8.2	242	8.7
Daughter-in-Law	105	3.7	119	4.3
Female Relative	179	6.2	198	7.1
Other	185	6.4	168	6.1
Missing	10	0.3	24	0.9
Caregiver Age				
Under 70 Years	1,707	59.4	1,673	60.3
70-74 Years	350	12.2	335	12.1
75-79 Years	336	11.7	329	11.9
80-84 Years	236	8.2	223	8.0
85 Years or Over	111	3.9	103	3.7
Missing	135	4.7	112	4.0
Caregiver Income				
Less than $10,000	228	7.9	250	9.0
$10,000- $40,000	721	25.1	667	24.0
$40,000 or More	407	14.2	412	14.8
Income Joint with that of Client	1,304	45.4	1.237	44.6
Missing	215	7.4	209	7.6
Caregiver Educational Attainment				
Less than High School	603	21.0	572	20.6
High School Graduate	844	29.4	820	29.5
Some College	659	22.9	669	24.1
College Graduate	681	23.7	648	23.4
Missing	88	3.1	66	2.4
Caregiver Health Status (Fair/Poor)	660	23.0	641	23.1
Caregiver Activities of Daily Living Limitations[1]				
0	2,376	82.6	2,288	82.5
1 or More	413	14.4	422	15.2
Missing	86	3.0	65	2.3
Caregiver Instrumental Activities of Daily Living Limitations[2]				
0	1,932	67.2	1,898	68.4
1	269	9.4	246	8.9
1	586	20.4	562	20.3
Missing	88	3.1	69	2.5
Caregiver Stress and Burden Scale[5]				
0-6	559	19.4	589	21.2
7-10	544	18.9	487	17.5
11-14	602	20.9	603	21.7
15-18	558	19.4	524	18.9
19-28	529	18.4	506	18.2
Missing	83	2.9	66	2.4
Caregiver Depression Scale[6]				
0	228	7.9	238	8.6
1-2	766	26.6	747	26.9
3-4	648	22.5	632	22.8
5-6	456	15.9	448	16.1
7-8	302	10.5	305	11.0
9-15	383	13.3	329	11.9
Missing	92	3.2	76	2.7

[1] ADL score is based on needing none, some or maximum assistance. Scores range from 0 (low) to 10 (high) impairment.

[2] IADL score is based on needing none, some or maximum assistance. Scores range from 0 (low) to 16 (high) impairment.

[3] Mini-mental status examination values can range from 1 to 30. Lower scores are indicative of greater impairment.

[4] The behavioral problems index ranges from 0 to 19. The higher the score, the greater the number of behavior problems.

[5] Scores range from 0-36. Lower scores equal lower stress and burden

[6] Scores range from 0-15. Lower scores reflect a greater likelihood of depression.

NOTES: Chi-square analyses were used to test significant differences between groups. No such differences were found.

SOURCE: Unpublished data from the Medicare Alzheimer's Disease Demonstration and Evaluation, 1989-1994.

Ehrlich, 1992), behavior problems, and type of housing. Caregiver attributes were added to the model and grouped into parallel predisposing, enabling, and need dimensions. Caregiver attributes were selected from those found in other studies to influence the likelihood of service use.

Medical diagnoses and Medicare expenditures were obtained from Medicare claims history files.[6] A subset of 16 major diagnostic groups were selected from the 4,000 plus *International Classification of Diseases, 9th Revision, Clinical Modification* (ICD-9-CM) codes used on Medicare claims. These aggregations are shown in Table 2 and follow grouping procedures developed by the Center for Health Economics Research (Pope et al., 1996). Cases were classified as having the listed chronic condition if there was a qualifying diagnosis on any available record.

A number of analyses were conducted testing sensitivity of the prevalence counts to the claim source (i.e., Part A versus Part B, versus Parts A and B), and the number of months used in classifying a case as having the condition. These analyses, for the study's chronic conditions, show that prevalence rates increased by a multiple of 2 or even 3, comparing all period counts relative to single 6-month or 12-month periods. Similarly, prevalence counts generally doubled when Parts A and B counts were compared with Part A only reported diagnoses. An abridged version of this analysis was published (Newcomer et al., 1999b).

The period used to identify these diagnoses was inclusive of the 12 months prior to the client's enrollment into the demonstration through their completion of the study or earlier exit due to HMO enrollment (n=129) or death (n=1998). A small group of clients (n=2) also had reduced exposure months because they went into HMOs and subsequently went back to regular Medicare FFS prior to or during their demonstration enrollment.

The operating assumption when using this extended exposure period for diagnostic classification was that any mention of a chronic condition implied that the existence of the condition preceded demonstration enrollment. Acute conditions (such as injuries, fractures, or nutritional problems) were not used as control variables as these are associated with incidents or care outcomes. Prevalence rates of chronic conditions treated as risk factors are shown in Table 3. These data affirm the effectiveness of the randomization in achieving comparable groups, and they provide descriptive information about the differences in prevalence among the various conditions.

In analyses with all sites combined, a set of dummy variables representing the demonstration sites was included, omitting a reference site whose expenditures were closest to the median expenditures among all sites. This facilitates comparison of any one site to the median performance of all sites in the demonstration. Table 4 shows the sample size by site.

Medicare expenditures, the dependent variable in the analysis, were based on the allowed amounts indicated on each claim record. For most claim files this included the amount paid by Medicare, plus deductibles and coinsurance, and any additional amount paid by the recipient. For each subject the exposure within a given period was limited by their eligibility for Medicare, the date of permanent nursing home placement, mortality, and loss to followup. Claims spanning periods were prorated. All claims were summed within

[6] For Part A, these included hospital inpatient, nursing facility inpatient, hospital outpatient, home health, and hospice claims files. For Part B, there were physician and home health, and a DME file.

Table 2
Chronic Conditions and Their Defining Diagnostic Codes

Cancer (Except Minor Skin Cancer)	
Any Diagnosis:	14, 15, 16, 170-1729, 174-179, 18, 1800, 1801, 1808, 1809, 181, 1820, 1821-1849, 185, 19, 190, 1940-1991, 2, 20, 23, 230, 239, 7950, 7951
Cerebrovascular Aneurysm	
Any Diagnosis:	436, 4373
Cerebrovascular Disease Events, Other	
Any Diagnosis:	438, 439
Congestive Heart Failure	
Any Diagnosis:	39891, 40403, 40411, 40491, 40493, 428, 4280, 4281, 4289
Coronary Artery Disease	
Any Diagnosis:	4110, 4111, 4118, 41181, 41189, 412, 4130, 4131, 4139, 4140, 41400, 41401, 4148, 4149, 4297
Diabetes (with and Without Complications)	
Any Diagnosis:	25, 250, 25000, 25001-3, 25010-13, 25020-2323, 25030-33, 25040-43, 25050-53, 25060-63, 25070-73, 25080-83, 25090-93, 2510-13, 3620, 36641, 7902, 7915, 7916
Liver/Gall Bladder/Pancreas Disease	
Any Diagnosis:	070, 1514, 251, 2518, 570, 571, 5710-19, 572, 5720-28, 573, 5730, 5734, 5738, 5739, 574, 57400, 57401, 57410, 57411, 57420, 57421, 57430, 57431, 57440, 57441, 57450, 57451, 575, 5750, 5751-59, 576, 5760-69, 577, 7824, 7891, 7895, 7904, 7905, 7933, 7948
Acute Myocardial Infarction	
Any Diagnosis:	41, 410, 4100, 41000, 41001-2, 4101, 41010-12, 4102, 41020-22, 4103, 41030-32 ,4104, 41040-42, 4105, 41050-52, 4106, 41060-62, 4107, 41070-72, 4108, 41080-82, 4109, 41090-92, 412
Nerve Problems, Paralysis (Excludes Coma or Injuries)	
Any Diagnosis:	342, 3430-34, 3438, 3439, 344, 3440, 3441, 3443, 3444, 3446, 34460, 3448, 3449, 7814
Nerve Problems, Peripheral (Excludes Paralysis)	
Any Diagnosis:	350-7, 7307, 9529, 953-56
Nerve Problems, Other Degenerative Nervous System Conditions	
Any Diagnosis:	013, 014, 015, 332, 3320, 335, 336, 337, 340, 341, 342, 343, 344
Other Arthropathies Including Rheumatoid Arthritis	
Any Diagnosis:	7110-16, 7193-99, 720-21
Pulmonary Disease, Chronic Obstructive Pulmonary Disease/Asthma/Emphysema	
Any Diagnosis:	01140, 1370, 491, 4910-12, 49120, 49121, 4918, 4919, 492, 4920, 4928, 493, 49300, 49301, 49310, 49311, 49320, 49321, 49390, 49391, 494, 496, 5081, 5100, 5109, 515, 5183
Renal Disease	
Any Diagnosis:	0160, 27410, 27411, 403, 404, 58, 590, 591, 592, 593, 7880, 7885, 7944, 866
Substance Abuse/Alcoholism	
Any Diagnosis:	291, 292, 303-5, 3050, 3052-59, 4255, 571, 5710, 5711-13, 5715, 5353, 7903
Vascular Disease, Peripheral	
Any Diagnosis:	440, 4400, 4401, 4402, 44020, 44021, 44022, 44023, 44029, 4408, 4409, 443, 4430, 4431, 44381, 44389, 4439, 4460-62, 44620-21, 44629, 4463, 4464-67, 4470-81, 4489, 5570, 5571, 5579, 7854

SOURCE: Unpublished data from the Medicare Alzheimer's Disease Demonstration and Evaluation, 1989-1994.

each period and an annualized total was calculated by summing expenditures in the period of interest (i.e., year 1, year 2, year 3, or the entire exposure period from their date of randomization up to 3 years) dividing the result by the number of exposure days in the period to get a daily rate, and then multiplying by 365 to create an annual rate. In statistical calculations (including group means and regression analyses), exposure days was included as a covariate to ensure that cases with few days and

Table 3

Prevalence of Selected Chronic Health Conditions Found on Any Medicare Claims Record

Conditions	Treatment Group		Control Group	
	N with Condition	Percent	*N* with Condition	Percent
Cancer (Except Minor Skin Cancer)	546	19.0	542	19.5
Diabetes (with and without Complications)	614	21.4	621	22.4
Eyes/Ears				
Blindness or Substantial Vision Defect	52	1.8	44	1.6
Hearing Loss	132	4.6	122	4.4
Vertigo/Dizziness	263	9.1	282	10.2
Heart and Vascular Systems				
Aneurysms (Other than CVA)	45	1.6	45	1.6
Cerebrovascular Aneurysm	693	24.1	647	23.3
Cerebrovascular (Other) Disease Events	1,159	40.3	1,096	39.5
Congestive Heart Failure	811	28.2	760	27.4
Coronary Artery Disease	923	32.1	919	33.1
Hypertension (Any)	1,335	46.4	1,211	*43.6
Myocardial Infarction	257	8.9	272	9.8
Peripheral Vascular Disease	745	25.9	708	25.5
Liver/Gall Bladder/Pancreas Diseases	256	8.9	261	9.4
Musculo-Skeletal System				
Intervertebral Disorders	282	9.8	259	9.3
Osteoarthritis Related Disorders	700	24.3	735	26.5
Other Arthropathies	889	30.9	908	32.7
Other Connective Tissue Disorders	85	3.0	89	3.2
Nervous System				
Degenerative Nervous System Conditions	484	16.8	464	16.7
Paralysis (No Coma or Head Injuries)	208	7.2	221	8.0
Peripheral Nerve Problems	135	4.7	146	5.3
Pulmonary System				
COPD/Asthma/Emphysema	769	26.7	726	26.2
Tuberculosis (Pulmonary Only)	57	2.0	51	1.8
Renal System Diseases	439	15.3	409	14.7

*$p<0.05$ using Fisher's statistical test, 2-tailed.

NOTES: COPD is chronic obstructive pulmonary disease. CVA is cerebrovascular aneurysm. The diagnostic categories included in each of the listed conditions are shown in Table 2.

SOURCE: Unpublished data from the Medicare Alzheimer's Disease Demonstration and Evaluation, 1989-1994.

large expenditures did not disproportionately influence results by inflating the mean daily expenditure rate.

Table 5 shows the annualized means and standard deviations for each year and entire exposure period in the analysis for Part A, Part B, and the combined Parts A and B expenditures. Annualized expenditures for yearly periods and the entire exposure period were used in the analysis to assess the sensitivity of the findings to different time intervals. Expenditures during the entire exposure period in the demonstration were specifically used for two reasons: (1) whereas single year period expenditures are commonly used as dependent variables in risk adjustment methodologies, the resulting estimates generally have wider variance than those for multiple year period expenditures; and (2) in a cohort study (such as this one), there are increasingly fewer cases available in each period after baseline. The combination of more variance and fewer cases results in analyses with lower statistical power than analyses that use data for the full exposure period

The observed effect sizes (i.e., the difference between treatment and control group means divided by the pooled stan-

Table 4

Number in Analysis Sample, by Site and Available Claims Data

Sites	Total Enrolled[1]	Part A Enrolled[2]	Parts A and B Enrolled
Model A Total	3,965	2,995	2,883
Treatment	2,029	1,520	1,467
Control	1,936	1,475	1,416
Illinois	999	920	881
Treatment	515	475	454
Control	484	445	427
New York	1,048	761	732
Treatment	530	383	374
Control	518	378	358
Portland	932	430	414
Treatment	485	212	207
Control	447	218	207
Tennessee	986	884	856
Treatment	499	450	432
Control	487	434	424
Model B Total	4,130	2,655	2,518
Treatment	2,122	1,355	1,288
Control	2,008	1,300	1,230
Florida	1,157	726	675
Treatment	593	371	346
Control	564	355	329
Minnesota	1,417	604	585
Treatment	738	314	305
Control	679	290	280
Ohio	927	825	779
Treatment	481	423	401
Control	446	402	378
West Virginia	629	500	479
Treatment	310	247	236
Control	319	253	243
Total All Sites	8,095	5,650	5,401
Treatment	4,151	2,875	2,755
Control	3,944	2,775	2,646

[1] The total sample excludes persons enrolled into the demonstration who died or entered a nursing home within the first 30 days after enrollment, and persons who were living in nursing homes at the time of enrollment and who failed to return to the community. It includes persons who were enrolled in an HMO.

[2] The difference in cases between the total sample and those eligible for Part A reflects the number of enrollees who were members of an HMO at baseline. The difference between the Part A sample size and the Parts A and B sample size occurs because Part B claims were unavailable until January 1, 1991. Any demonstration plan member dying or joining an HMO prior to that date did not have Part B claims records available, consequently Parts A and B expenditures could not be calculated prior to that date.

NOTE: HMO is health maintenance organization.

SOURCE: Unpublished data from the Medicare Alzheimer's Disease Demonstration and Evaluation, 1989-1994.

dard deviation of the two groups) are .06, .03, and .06 respectively comparing Part A, Part B, and Parts A and B treatment and control case expenditures in year 1 for all cases combined; and 0.08, 0.05, and 0.08 respectively comparing Part A, Part B, and Parts A and B treatment and control case expenditures over all periods. An effect size of 0.06, assuming a standard deviation of $11,800 translates into a difference of $709; 0.06 of a standard deviation of $14,500 equals $870. Approximately 5,300

Table 5

Annualized Mean Medicare Expenditures, by Period

Period	Part A			Part B			Parts A and B		
	Total	Treatment	Control	Total	Treatment	Control	Total	Treatment	Control
Year 1									
Total Cases	5,648	2,874	2,774	5,151	2,641	2,510	5,151	2,641	2,510
Mean Expenditures	5,284	4,983	5,596	1,791	1,759	1,825	7,526	7,169	7,898
Standard Deviation	9,695	9,626	9,760	2,484	2,572	2,387	11,544	11,751	11,316
Year 2									
Total Cases	3,665	1,870	1,795	3,665	1,870	1,795	3,665	1,870	1,795
Mean Expenditures	6,023	5,743	6,317	1,673	1,634	1,714	7,697	7,378	8,032
Standard Deviation	10,497	9,948	11,036	2,687	2,638	2,739	12,463	11,812	13,105
Year 3									
Total Cases	2,255	1,184	1,071	2,255	1,184	1,071	2,255	1,184	1,071
Mean Expenditures	7,067	6,791	7,367	1,833	1,736	1,939	8,901	8,526	9,305
Standard Deviation	12,364	12,218	12,524	3,006	2,931	3,084	14,458	14,127	14,813
All Periods									
Total Cases	5,649	2,875	2,774	5,170	2,652	2,518	5,170	2,652	2,518
Mean Expenditures	5,862	5,578	6,161	1,758	1,709	1,810	7,898	7,555	8,260
Standard Deviation	6,922	6,822	7,020	1,911	1,930	1,890	8,533	8,486	8,576

NOTES: Means are based on an annualization of expenditures for each case alive and living in the community at the start of the period shown. Counts differ in year 1 between Part A and Part B due to unavailable physician payment records prior to 1991. Partial use in a period was annualized by dividing total expenditures in the period by the number of exposure days and multiplying by 365. Statistical calculations further weighted each case by the proportion of days in the exposure period.

SOURCE: Unpublished data from the Medicare Alzheimer's Disease Demonstration and Evaluation, 1989-1994.

cases are needed to have a statistical power of 0.75 (assuming an alpha of 0.05 and a one-tailed test) to observe a difference of this magnitude given the variance.

Model development consisted of several steps. The initial step used the client and caregiver attribute interview items shown in Table 1. Using total Medicare expenditures as the dependent variable these were reduced to a set of 15 covariates using backward stepwise regression analysis. Interaction effects between treatment and these covariates were tested with none found to be consistently significant. The second step added the 26 chronic condition variables shown in Table 3 and used a backward stepwise methodology to adjust for collinearity among chronic conditions that were added at this step. The statistically significant covariates from step 1 and treatment group were forced into this analysis. Both untransformed and log-transformed dependent variables were analyzed, and both were found to yield residuals which did not fit a normal distribution.

To respond to the non-normality issue, a two-stage analysis was conducted. It consisted of a multivariate logistic regression, a bootstrap regression, and further methods to combine the results of the two-stage model. The dependent variable for the logistic model was a binary indicator variable that was 1 when there was any Part A expense during the period, and zero if there was no expense. (A variation on this threshold was tested: expenses of less than $500. It produced similar results.) The dependent variable in the bootstrap regression was the log of the positive expenses, and was performed only on the subjects who had an expense. The independent variables in both models were the same, except that certain low-incidence dummy variables were eliminated from the logistic models because small cell size (fewer than five subjects) caused convergence problems. Each model also included a dummy variable for those cases having fewer than 20 percent exposure days in the period, relative to the maximum possible days. This controlled for the

fact that fewer exposure days would cause less likelihood of any expense, but given that there was an expense, would cause greater average expenses.

From the regression results, the estimated expense for each subject was calculated as the estimated probability of an expense (from the logistic model), times the anti-log of the estimated log expense (from the bootstrap regression), times a "smear factor." The smear factor is the mean anti-log of the residuals from the regression (Duan, 1983). Following a suggestion from Manning (1998), smearing factors were calculated for the treatment and control groups separately. These produced results similar to the smearing factor with all cases combined. The reported results reflect the combined factor.

The estimated treatment effect was calculated for each subject based on the difference between their estimated expense assuming they had been in the treatment group, minus their estimated expense assuming they had been in the control group. In this way, the simultaneous effects of the covariates could be applied to both the logistic and regular regression models (Haber, 1996). The weighted mean of the individual treatment effects was used to estimate the overall treatment effect.

A series of 200 bootstrap replications were performed to obtain a simulated distribution of each point estimate of the overall treatment effect described previously. Each replication consisted of a sample of cases taken with replacement from the original sample. A 95-percent confidence interval was calculated consisting of the 5th and 196th order statistic for the distribution obtained from the resampling. A non-parametric *p*-value testing the null hypothesis that the treatment effect was zero was then calculated.

Because of the presence of statistically significant interaction terms for treatment and site, and regional differences in prevailing service unit prices, separate analyses were also conducted for each site. This afforded the opportunity to more clearly assess the performance of each program at the level of each community, where randomization produced equivalence between those in treatment and control groups.

Findings

Table 6 compares the logistical regression results using two alternative threshold values for the dependent variable. Separate models were estimated for each row in the table. All models used the covariates shown in Tables 1 and 3. Table 6 shows the percent of the sample having Part A expenditures of the qualifying level. The immediately following column shows the odds ratio or likelihood that those in the treatment group had this level of expenditures in the period. These analyses might be seen as a non-conservative test of the treatment effect because they require that there be only a reduced likelihood of expenditures. The preponderance of period and site models show a pattern of a lower likelihood for Medicare Part A expenditures among those in the treatment group, but only one of these odds ratios is statistically significant.

Table 7 summarizes the results of the bootstrap regressions, comparing the demonstration's estimated treatment effect on annualized expenditures. Several methods of aggregating the dependent variable data are used. Table 7 shows estimated treatment effect results from the regression models in which all sites are included together. Annualized mean expenditures are shown by year and for the entire expo-

Table 6

Comparing the Likelihood of Medicare Part A Expenditures Among Treatment and Control Groups

Period	Expenses Less than $500			Any Expenses		
	Percent >0	Odds Ratio	Confidence Interval	Percent >0	Odds Ratio	Confidence Interval
All Sites						
All Periods	69.3	0.99	0.87 to 1.13	86.9	0.96	0.87 to 1.13
Year 1	54.4	0.96	0.86 to 1.08	75	0.93	0.82 to 1.06
Year 2	53.6	0.96	0.82 to 1.12	72	0.9	0.77 to 1.06
Year 3	55.8	1.03	0.85 to 1.26	74.2	0.88	0.71 to 1.09
Model A Sites						
Illinois, All Periods	69.7	0.84	0.60 to 1.17	86.3	0.96	0.62 to 1.50
New York, All Periods	60.6	0.94	0.66 to 1.34	88.8	1.22	0.70 to 2.17
Oregon, All Periods	62.3	1.01	0.61 to 1.66	85.3	0.91	0.45 to 1.84
Tennessee, All Periods	76.9	1.38	0.92 to 2.09	88.2	0.92	0.54 to 1.56
Model B Sites						
Florida, All Periods	74.3	1.39	0.90 to 1.17	85.1	1.43	0.84 to 2.46
Minnesota, All Periods	59.3	0.89	0.59 to 1.34	81.3	0.91	0.54 to 1.51
Ohio, All Periods	70.2	0.84	0.58 to 1.21	86.9	0.59	0.36 to 0.95
West Virginia, All Periods	78.2	0.84	0.52 to 1.36	93.2	0.78	0.34 to 1.74

NOTE: Confidence interval is 95 percent likelihood that true odds ratio is within this range.

SOURCE: Unpublished data from the Medicare Alzheimer's Disease Demonstration and Evaluation, 1989-1994.

sure period. Also shown here are the treatment effect results based on similar bootstrap regression models estimated for each demonstration site (community) considered separately. The site level regression models report annualized expenditures for the entire exposure period. Three sets of columns are used to show the estimated treatment effect on various groups of Medicare annualized expenditures. Separate regression models were estimated for each cell in this table, using the client, caregiver, and health condition covariates shown previously in Tables 1 and 3. In the interest of presentation parsimony, only parameter estimates for treatment dummy variables are shown. The results shown use the likelihoods derived from the (any expenditures) dummy variable in the stage-one model (Table 6). Similar analyses were conducted using the less than $500 level. These produced findings comparable those in Table 7, and have not been shown.

Several patterns emerge from these results. For all sites combined, there is a tendency for a negative treatment effect on Medicare Part A and Parts A and B expenditures when expenditures are annualized over all periods, but these results are not statistically significant. For each year considered separately, there is a similar tendency for a negative treatment effect (i.e., reduced expenditures) on Medicare Part A and Parts A and B expenditures, however, again these results do not obtain statistically significance at a 0.05 level. These results do not support the hypothesis that the demonstration intervention would lower Medicare Parts A and A and B expenditures.

Again, for all sites combined, the estimates of the treatment effect on Part B expenditures are also not statistically significant. These findings do not support the hypothesis that the demonstration would lead to higher Part B expenditures for treatments compared with controls, and suggest that the demonstration program, on average, did not stimulate or facilitate more access to ambulatory health care than was obtained by those in the control group.

Five of the individual sites showed differences in expenditures for treatments and controls, relative to at least one type of

Table 7

Demonstration Treatment Effects on Medicare Expenditures, by Location, Period, and Type of Medicare Coverage

Sample	Medicare Part A			Medicare Part B			Medicare Part A and B		
	N	Medicare Part A Treatment Effect	95 Percent Confidence Interval	*N*	Medicare Part B Treatment Effect	95 Percent Confidence Interval	*N*	Medicare Part A+B Treatment Effect	95 Percent Confidence Interval
All Sites									
All Periods	5,649	-435	-1,047 to 133	5,170	38	-83 to 137	5,170	-167	-822 to 486
Year 1	5,648	-473	-1,247 to 342	5,151	27	-105 to 136	5,151	-229	-1,062 to 436
Year 2	3,665	-102	-934 to 772	3,665	47	-89 to 185	3,665	17	-691 to 922
Year 3	2,255	-529	-2,005 to 735	2,255	-1	-193 to 161	2,254	-325	-1,536 to 747
Model A Sites									
Illinois, All Periods	920	-363	-1,608 to 1,038	825	97	-204 to 337	825	-436	-2,321 to 1,049
New York, All Periods	761	51	-2,072 to 1,985	707	64	-227 to 360	707	-267	-2,040 to 1,694
Oregon, All Periods	430	761	-1,960 to 4,199	403	237	-213 to 689	403	1,935	-1,242 to 5,064
Tennessee, All Periods	884	*-2,100	-4,862 to -174	826	-260	-655 to 156	826	-2,843	-5758 to 95
Model B Sites									
Florida, All Periods	725	*-2,169	-4,474 to -370	657	-575	-1519 to 159	657	*-4,558	-8,871 to -1,420
Minnesota, All Periods	604	152	-1,848 to 2,116	554	-63	-378 to 240	554	17	-3,002 to 2,418
Ohio, All Periods	825	-656	-2,409 to 614	734	101	-292 to 499	734	578	-1,572 to 3,111
West Virgninia, All Periods	500	705	-2,778 to 3,958	464	*483	-2 to 917	464	1,884	-943 to 5,101

*Probability value <0.5

NOTES: Sample size for Medicare Part A only analyses is larger than for analyses combining Medicare Parts A and B because Part B records were unavailable before 1991. All expenditure estimates are on an annual basis rounded to whole numbers. These analyses included a covariate that adjusted for exposure days in the period. All used the covariates shown in Tables 1 and 3.

SOURCE: Unpublished data from the Medicare Alzheimer's Disease Demonstration and Evaluation, 1989-1994.

Medicare coverage. These results were statistically significant in two sites. Whether for Part A, Part A and B, or merely Part B expenditures in at least three the sites showed increased cost (albeit statistically non-significant). Illinois (a Model A site with nurses as care managers) did not show the expected estimates of expenditures that were significantly lower for the treatment compared with the control group. Tennessee (also a Model A site) and Florida (a Model B site) showed estimates of Medicare Part A expenditures that were significantly lower for treatments compared with controls. This relationship also held for Part A and B expenditures, although the coefficient was just over the 0.05 level in Tennessee (p >0.06). West Virginia (a rural Model B site) showed statistically significant higher Part B expenditures for treatments compared with controls. These results, at the site level, offer limited support for three of four study hypotheses.

The study's final hypothesis was that participants in the higher resource demonstration programs (Model B) would have lower Medicare expenditures than participants in the lower resource programs (Model A). This hypothesis was tested both using the results of the individual site regression models discussed previously and through a *t*-test of the mean effects among the group of sites in each model. The *t*-test results showed no difference between models, and as seen from Tables 6 and 7, neither Model produced a consistent pattern of lower Medicare Part A and Medicare Parts A and B expenditure estimates for treatments compared with controls.

An alternative set of bootstrap regressions was estimated using a continuous measure of exposure days instead of the binary measure. These models generally produced point estimates of the treatment effect—approaching those in Table 5, but with wider confidence intervals than those shown in Table 7. The loss of precision dropped Florida from having a statistically significant difference in Part A expenditures and Tennessee from the combined Parts A and B expenditures. The all period/all sites model did become statistically significant. The results reported in Table 7 have been used because they have more precision.

CONCLUSIONS

The Medicare Alzheimer's Disease Demonstration and Evaluation randomly assigned about one-half of a large group of voluntary applicants into a treatment group that had access to a limited Medicare community care service benefit and routine monitoring and other assistance from a case manager. Those randomly assigned to the control group continued to receive standard care. The demonstration was not expressly designed to affect medical care, but the increased access to community care obtained by those in the treatment group suggested the possibility of a secondary effect on health care. Five hypotheses were tested controlling for a variety of attributes of persons with dementia, including functional, cognitive, and physical health conditions; and of caregivers, including relationship to client, and physical and mental health status. These hypotheses were tested for participants that obtained services from the regular Medicare FFS system, and thus, for whom there were health care service claim records.

Overall, these findings provide only limited support for the hypothesis that the demonstration (which did not expressly coordinate with primary care physicians) reduced hospital utilization and other Part A expenditures among treatment group members using Medicare FFS. Five sites showed reduced treatment group expenditures for Part A, and/or the combination of

Part A and B expenditures. These differences reached or approached statistical significance in two sites. Treatment group members, when considered as a single group were not found to have lower Medicare Part A expenditures than control group members with comparable health status. This finding was consistent in models using annualized expenditures with all sites combined for the clients' full exposure period of up to 36 months, and for each year in the demonstration.

The third hypothesis, that the demonstration program would increase access and use of Part B services, was not generally supported. Only one site had treatment group members with higher predicted expenditures than those in the control group. A related hypothesis, that the one site using only nurses as case managers would have more effect on health care expenditures than the sites using social workers (usually with nurses available as consultants), also was not supported.

The hypothesis that Model B would have a stronger treatment effect on Medicare expenditures relative to Model A was not supported. There was no demonstrable advantage relative to lowering Medicare expenditures for the high resource program (Model B) compared with the low resource program (Model A). One of the Model B sites achieved Medicare (Part A and Parts A and B) expenditure savings for the treatment group that were statistically different from their respective control groups, and one site even produced higher costs, although this finding was non-significant. On the other hand, one Model A site also showed treatment groups with lower expenditures than their controls.

All of this supports a conclusion that, while site differences undoubtedly have some importance in the relative success of the intervention, there is no evidence that the high resource program (at least within the range of resources made available) produced more expenditure savings relative to those in usual FFS Medicare within those communities than did the lower resource program.

Comparisons of model effects are complicated by the fact that clients were randomized within communities, not between models. Thus, the demonstration reflects a four-site replication of each model. Whatever practice pattern and other situational factors that operated locally to affect Medicare use were not randomly distributed among the models. A larger sample of communities or another mix of communities within each model may produce a different finding. Perhaps the more germane conclusion to be drawn is that the demonstration program (ignoring model) did achieve some savings in at least two communities, and that there was a tendency toward reduced expenditures in the majority of the sites. The failure of the treatment program to achieve reduced Medicare expenditures consistently among all sites and for all periods suggests the conservative conclusion that the mere presence of a case manager does not assure increased access to health care or to the prevention of conditions that may otherwise increase Medicare expenditures.

One reason for a possible absence of a consistent effect is suggested by the wide variation in expenditures (especially within single years). This raises the possibility that case manager involvement may produce at least a short-term increase in expenditures for some cases. Further, estimates of the demonstration program effectiveness are most favorable when multiyear expenses were averaged together. This suggests that steady-state program effects on Medicare expenditures may be relatively long term and require a sustained intervention. A third qualification is

that the program was targeted to caregiver support and community service provision, not to coordination with primary care physicians or health care risk management processes. The tendency for some cost reductions among the treatment group, in spite of this, suggests the potential for further cost reductions in situations where more attention is directed to the health care management of this population. Of particular note in this regard, is the high proportion of cases with one or more significant chronic health condition. For example, as shown by Medicare claims records, more than 20 percent had diabetes, more than 25 percent had congestive heart failure, almost one-third had coronary artery disease, and more than 25 percent had some form of pulmonary disease.

A more cautionary viewpoint arises from the possibility that the demonstration may have attracted a population with higher than average cost among those with dementia. Among the factors possibly influencing this were the availability of care coordination and community services. These could be expected to be more sought after by those needing this type of assistance. Such selection or choice factors have been mitigated in the demonstration using randomization, but a fully operational program or a program more oriented to the time of one's initial dementia diagnosis could be expected to have a broader cross section of enrollees. Under such circumstances, Medicare savings relative to program costs could be less than those observed in the demonstration, unless there is an effective targeting of care manager resources and benefits.

Accepting the evidence (or potential) of lower Medicare expenditures for treatment cases relative to controls in some communities and circumstance, there is still the issue of whether the case management and community service intervention, as implemented, was cost effective in lowering those expenditures. In other words, did the cost of the intervention, even in the most favorable sites, exceed the cost savings resulting from reductions in Medicare expenditures? The following illustrates the demonstration performance that would be needed to show cost effectiveness of the demonstration in lowering Medicare expenditures.

A first assumption is that the treatment effect parameter estimates shown in Table 7 are indicators of the true average treatment effects among a cohort of clients over time (ignoring whether or not the coefficients are statistically significant). A second assumption concerns the average cost of the case management intervention itself. For the sake of simplicity, the average annual salary for a full time equivalent case manager is set at $30,000 and the administrative overhead, home visit and other operating expenses are set equal to the annual salary. For a Model A site, with a ratio of 1 case manager per 100 clients, this produces an annual cost per client of $600 or $50 per month. For the Model B sites with a 1:30 case manager to client ratio the annual cost per client is $2,000 or $167 per month.

Based on these rates, two Model A sites with a combination of lower staffing costs and somewhat larger treatment effects showed (or approached) a net savings relative to additional case-management costs for Part A and Parts A and B expenditures. One of the Model B sites reduced average Medicare expenditures enough to offset their estimated staff and operational costs. This is true using both the actual treatment parameter estimates for each site, and the average treatment parameter estimates among all Model B sites combined. If the Model B per client cost were set at the Model A rate, two sites would show net savings for Part A, and one for Part A and B combined.

The preceding comparisons do not include the cost of the additional Medicare benefits obtained by treatment group participants under the demonstration waiver for community care services. These averaged more than $200 a month in Model A and $300 per month in Model B. When an average monthly benefit of $200 is added to the $600 cost for the case manager and other operational costs, one Model A and one Model B site reached or approached budget neutrality (defined by combining Medicare Part A and B expenditures). These calculations do not include or consider any offsetting savings from nursing home care, because analysis reported elsewhere found no demonstration linked reductions in nursing home care (Miller et al., 1999). However, it is possible that reductions in hospitalization could also reduce nursing home placements.

As with many other national demonstrations, a number of questions are raised that highlight either: (a) limitations of the design of the particular project intervention; or (b) methodological issues that might inform further work in this area. One limitation of this study is that results pertain only to beneficiaries who use the regular Medicare FFS system. In that system, it may be difficult or impossible to create appropriate financial incentives to reduce overall costs, unlike the Medicare HMO sector, where there is an incentive to reduce costs.

A second limitation is that the program was not expressly designed for case managers to work with health care providers in the identification and management of high-risk cases. Although this occurred to varying degrees within each site, including in response to the request of family members and providers, it was not explicit or common in any program. Whether more attention to this, especially among cases with complex chronic conditions, would be effective has not been fully tested in the demonstration program design. A further refinement in case targeting may be an important continuing step to reducing per client cost, and an essential step in beginning to provide a cost effective program. Examining caregiver attributes and their relationship to preventable health care episodes (such as injuries, medication mismanagement, inadequate nutrition, or skin care), issues not examined in these analyses, may also be a productive area for further research into case targeting risk factors.

Another needed refinement is in the clinical efficacy of treatment for high-risk cases, whether these result from chronic conditions or other factors. Finding a high-risk case does not by itself assure that an effective care plan can be designed or implemented. Exploration of possible modes of communication and collaboration between caregivers (whether family members or paid providers) and care managers, and between care managers and health care providers was not a subject tested in depth in the demonstration. Further work in this area may be productive. Also important is gaining a better understanding of the health care use of those with dementia and other chronic conditions. What portion of the expenses incurred by this population are resultant from poorly managed chronic conditions? What practice interventions may be helpful in improving this care? Can coordination among community and health care providers be achieved? Are there any chronic conditions where such coordination is particularly helpful?

Finally, relative to the question of quantitative evaluation of the cost effectiveness of programs such as this demonstration, there is a need to be cognizant of the differential findings that arise among individual sites, and over single versus multiple years. Sampling theory speaks to concerns like homogeneity bias in cluster samples and the issue of within-group versus

between-group variation. Sites and time periods are clusters, and expenditures by an individual within the same period often are not independent events and they may be affected by community-specific practice patterns. While this demonstration had sufficient power to detect an effect size of 0.15 in seven of the eight sites, the total number of sites (four in each model) was insufficient to address the within-and-between group problem. More fixable, from a research cost standpoint, are the time aggregations that are used. The findings from this article suggest that treatment effects may be more discernible when expenditures are averaged over multiple years rather than using only the events of a single year. Average expenditures over time also are likely to be a better reflection of the performance of a steady-state program. Such a finding may have implications for risk adjustment methods based on a single year of expenditure data, as well as for the future evaluation of interventions.

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