Continuity of Care and Client Outcomes in the Robert Wood Johnson Foundation Program on Chronic Mental Illness

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UNDER THE ROBERT WOOD JOHNSON FOUNDATION (RWJF) Program on Chronic Mental Illness (PCMI), cities extensively reorganized their services for the target population. The logic of this intervention derived from the premise that fragmentation of administrative, fiscal, and clinical responsibilities is a major impediment to effective services for persons with CMI. Model clinical programs addressing this fragmentation have been found to improve continuity of care and outcomes (Aiken, Somers, and Shore 1986; Shore and Cohen 1990; see also the article by Goldman, Morrissey, and Ridgely in this issue). The RWJF intervention itself occurred at a high administrative level by establishing citywide local mental health authorities. These authorities sought to improve services and continuity of care at the provider level for all persons with CMI by centralizing administrative, fiscal, and clinical responsibility. This improvement in services at the system/provider level was expected to translate into improvements in services at the client level, leading in turn to better client outcomes.

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Based upon this program logic, the following evaluation questions were addressed in this client outcome study:

1. Did continuity of care and case management improve during the course of the program?
2. Did client outcomes improve during the course of the program?

Because persons with CMI are highly diverse and because, inevitably, client subgroups vary in their exposure to aspects of the program, a study of its impact on treatment and outcome at the client level can be carried out more efficiently by focusing on the targeted subgroups most likely to be affected by the program. This focus, analogous to a "tracer condition" in medical studies, will not provide information on other subgroups of clients; however, it will tell us whether or not groups of clients viewed as highly likely to be affected by the program indeed experienced improvements in services and outcomes. The observation of program effects would lend support to the hypothesis that the program influenced the system of services for persons with CMI, and at the very least would inform us of such effects among high-priority subgroups of clients. The absence of program effects among highly targeted subgroups of clients with CMI would raise questions about whether the program had any significant impact at the client level regardless of effects observed at the system level.

A variety of subgroups of high-priority clients with CMI can be identified: those who are homeless, unserved, in crisis, or in some critical transition (such as hospital discharge or move to a new residence). As a general strategy, we chose to evaluate the program impact among persons with CMI who were at the point of discharge from intensive 24-hour care (short-term hospitalization) because they are at a point of high need and vulnerability to system failures, are likely to be affected by the types of system changes foreseen under the RWJF program, and do not present the extreme methodological problems of case identification and tracking posed by certain other high-priority CMI groups.

In addition, we decided to focus on continuity of care and case management as the key client-level service outputs because of the central role these concepts played in the development of the RWJF program (Shore and Cohen 1990) and their prominence in current views of what constitutes good care for persons with CMI and what typically is lacking in existing service systems (Bachrach 1981; Aiken, Somers, and Shore 1986; Lehman 1988).

Methods

Overview

This client study utilized a quasi-experimental design with replications in four of the nine RWJF demonstration cities: Baltimore, Cincinnati, Columbus, and Toledo. Two separate cohorts of individuals with CMI from each city participated in the study. Both sets of cohorts were interviewed at baseline (hospital discharge) and at two follow-up points: 2 and 12 months after discharge. Data collection from the first cohort began in November 1988 and ended in June 1990. For the second cohort, interviews were conducted from September 1990 to February 1992. The RWJF awards to initiate the local mental health authorities in the nine cities began in November 1986. Therefore, the first cohorts reflect service experiences early in the development of the authorities and are not true preprogram baselines.

To obtain a reliably homogeneous sample of individuals with CMI, our screening process specified the criteria for selecting study participants: subjects who had been hospitalized because of a mental disorder; who met the Ohio criteria for severe mental disability (based upon diagnosis, duration of treatment, and disability)(available upon request from the authors); whose current hospitalization lasted no more than 120 days; who were between the ages of 18 and 64; who spoke English, resided in the study area, and were legally competent. A liaison assigned to each hospital identified the potential subjects and obtained the consent of eligible subjects to participate in the study. Trained interviewers contacted these clients and interviewed them initially within a defined time frame spanning from four days prior to hospital release to seven days following discharge.

A structured interview was used to elicit respondents' sociodemographic characteristics, current clinical status, level of functioning, quality-of-life judgments, mental health services received in the past, perceived psychosocial and clinical service needs, and information on whether these needs were being addressed.

Subjects

Among the large pool of clients experiencing acute 24-hour-care episodes who were screened in the four cities (2,438 for Cohort 1 and 3,417 for Cohort 2), 46.3 percent (1,127) in Cohort 1 and 36.3 percent (1,042)

in Cohort 2 met the study eligibility criteria. Among the eligible clients, 359 (42 percent) agreed to participate in Cohort 1 and 302 (31 percent) agreed to participate in Cohort 2. Compared with Cohort 1, Cohort 2 refusal rates were lower (37 percent versus 19 percent, respectively), but the rates of loss due to discharge prior to study recruitment attempts were higher (22 percent versus 50 percent). Although consent rates were consistently higher for the cohorts in the three Ohio cities (CIN = 40 percent, COL = 40 percent, TOL = 42 percent) than for those in Baltimore (28 percent), consent rates did not differ significantly by diagnosis for both cohorts across cities (schizophrenia = 39 percent, other = 43 percent). Consenters did not differ from the entire pool of eligible subjects on gender, age, or race.

Comparison of the characteristics of subjects recruited into the two sets of cohorts revealed that the two groups had similar demographic characteristics, levels of functioning, and clinical status at baseline. The mean ages of the subjects were 35.1 for Cohort 1 and 36.4 for Cohort 2 (n.s.). Nearly half of the subjects from both cohorts were women (47.9 percent and 44.7 percent, respectively, n.s.). Almost half of each group was Caucasian (47.1 percent and 44.7 percent, respectively, n.s.), and the majority of both had never been married (58.6 percent and 52.8 percent, respectively, n.s.). In terms of self-rated level of functioning, the two cohorts were equivalent (scale: 1 = excellent, 4 = poor, both cohort means = 2.6). A higher proportion of Cohort 1 was employed prior to admission (21.8 percent versus 16.3 percent), but this difference was not statistically significant.

More than three-fifths of participants from both groups (63.1 percent of Cohort 1 and 61.9 percent of Cohort 2) had a primary diagnosis of schizophrenia. Both groups had spent on average over 50 days in the hospital during the past year before the first interview (51.3 days for Cohort 1 and 54.6 for Cohort 2). The mean number of hospital admissions during the preceding year for the two cohorts also did not differ (Cohort 1 = 2.1, Cohort 2 = 2.3, n.s.). However, statistically, although probably not clinically, significant differences in baseline symptomatology were found between members of the two cohorts (symptom checklist [SCL] scales:1 = none, 5 = extreme; Cohort 1 mean = 2.3 [s.d. = 0.89], Cohort 2 mean = 2.2 [s.d. = 0.84], $P < .04$).

Very high retention rates for both cohorts were attained at the 2-month and 12-month follow-up points, with completion rates ranging from 86 percent to 97 percent.

Method of Analysis

The objective of the analysis was to test whether differences existed between the two cohorts on continuity of care, case management, and outcomes at the two follow-up points after hospital discharge. In order to relate any observed between-cohort differences in the impact of the RWJF program, it was necessary to control for potentially confounding differences between cohorts at baseline, such as the baseline value for the dependent measures of interest, demographic characteristics (age, gender, education, and marital status), diagnosis, and city. Analysis of covariance (ANCOVA) was conducted to partial out the confounding variables by treating them as covariates. Although comparison of the patients from the two cohorts on baseline demographic characteristics and diagnosis upon entry to the hospital did not show significant differences, partialing out their effects reduces error variance. In addition, a city-by-cohort interaction was included in the analysis to detect and control for any treatment-by-site variations. An alternative series of ANCOVAs was conducted using a composite baseline covariate derived from a combination of variables (demographics, diagnosis, prior hospitalizations, symptoms, and objective and subjective quality of life) that best predicted cohort membership in a logistic regression. Results from these analyses yielded the same results that we have presented here.

After partialing out these confounding variables, the main effect of cohort could be more accurately assessed. It was then possible to consider each test as a one-factor (cohort) analysis of variance on a dependent variable adjusted for the covariates. Cohort differences were assessed separately at the 2- and 12-month follow-up points to determine if there were significant cohort effects at either one or both time points.

The first evaluation question asked whether case management and continuity of care changed over time (between cohorts). Five variables reflecting certain dimensions of continuity of care and case management were analyzed, and included the following:

1. whether the respondent had a case manager during the interim assessment period
2. whether clients with case managers had experienced a change in their managers during the interim period
3. whether the respondent experienced any service needs during the interim period

4. how many of these needs were addressed by services during the interim
5. the respondent's rating of the helpfulness of the services

We expected that if the RWJF program was effective in improving continuity of care, Cohort 2 participants would be more likely to have case managers, to have greater continuity in the identities of these case managers, to identify fewer service needs, to report a greater proportion of service needs addressed, and to rate the helpfulness of their services more highly than individuals in Cohort 1.

The second question concerned differences in outcomes between cohorts. A large number of outcome variables were assessed using the Lehman Quality of Life Interview (Lehman 1988), adaptations of subscales from the Uniform Client Data Instrument (Goldstrom and Manderscheid 1986), and the Tessler Continuity of Care Provider Questionnaire (Tessler, Willis, and Gulman 1986), the Denver Consumer Questionnaire (Ciarlo and Reihman 1977), and the SCL-90 (Derogatis 1983). Because the findings were highly consistent across all of these measures, we will present the results from only a few key summary measures. Results from two subjective quality-of-life variables—general life satisfaction and satisfaction with living situation (Lehman, Ward, and Linn 1982)—and three objective outcome indicators—number of nights hospitalized per month during the study period, symptomatology (as rated by three subscales of the SCL-90) (Derogatis 1983), and self-reported level of functioning (Goldstrom and Manderscheid 1986)—will be reported. In our design, significant improvements in outcomes related to the program would be evidenced by Cohort 2 achieving better scores on these variables compared with Cohort 1.

For variables that obtained a significant ($P < .05$) main or interaction effect, adjusted means were examined to determine the direction of the effect.

Results

Case Management and Continuity of Care

Results of the ANCOVA performed on whether the subjects had a case manager indicated a significant cohort main effect at 2 months' follow-

up ($F = 12.16$, $df = 1, 563$; $P < .001$). By 12 months' follow-up, this significant cohort main effect disappeared, but a significant city × cohort interaction effect emerged ($F = 6.67$, $df = 3, 537$; $P < .001$). Further analysis on the cohort-adjusted means (table 1) revealed that a significantly higher proportion of Cohort 2 subjects received case management services (mean [M] = 0.76 versus $M = 0.64$; $t = 3.30$; $P < .001$) during the 2-month period after hospitalization. This trend was significant in Cincinnati, Toledo, and Columbus. There was no such trend in Baltimore. At the 12-month follow-up point, the significant difference between the two cohorts had disappeared in the overall com-

TABLE 1
Case Management Services Received: City × Cohort-adjusted Means

	Mean[a] differences and probabilities					
	Two months after baseline			Twelve months after baseline		
City	Cohort 1	Cohort 2	Difference	Cohort 1	Cohort 2	Difference
Have a case manager (%)						
Baltimore	42	40	−0.02	52	38	−0.14*
Cincinnati	70	83	0.13**	84	80	−0.04
Columbus	74	92	0.18*	64	92	0.28***
Toledo	70	88	0.18*	80	78	−0.02
All cities	64	76	0.12***	70	72	0.02
Changed case managers (%)						
Baltimore	24	27	0.03	55	17	−0.38*
Cincinnati	15	5	−0.10	49	31	−0.18*
Columbus	7	12	0.05	37	29	−0.08
Toledo	25	10	−0.15	48	33	−0.15
All cities	18	14	−0.04	47	27	−0.20**
Number of non-case-management services received						
Baltimore	3.73	3.27	−0.46	4.21	3.36	−0.85**
Cincinnati	3.28	3.54	0.26	4.05	3.66	−0.39
Columbus	3.43	3.27	−0.16	3.57	3.79	0.22
Toledo	3.58	3.31	−0.27	3.05	3.46	0.41
All cities	3.51	3.35	−0.16	3.72	3.57	−0.15

[a] Adjusted for baseline lagged variable and subject characteristics.
* $P < .05$, ** $P < .01$, *** $P < .001$.

parison, but remained significant in the positive direction in Columbus. Baltimore, on the other hand, demonstrated a significant decrease in the percentage of clients who had a case manager over the 12 months.

Whether respondents had changed case managers—an indicator of continuity of care—was examined next. A lower propensity to change case managers indicates better continuity of care among these subjects. The ANCOVA yielded a significant cohort main effect ($F = 7.96$, $df = 1$, 288; $P < .01$) 12 months after hospitalization. Table 2 shows the direction of this significant main effect: a greater percentage of Cohort 1 ($M = 0.47$) changed case managers, compared with Cohort 2 ($M = 0.27$) during the one-year period. Although all four cities exhibited this pattern,

TABLE 2
Needs Addressed and Helpfulness of Services: City × Cohort-adjusted Means

	Mean[a] differences and probabilities					
	Two months after baseline			Twelve months after baseline		
City	Cohort 1	Cohort 2	Difference	Cohort 1	Cohort 2	Difference
Needs addressed/needs identified						
Baltimore	.79	.89	0.10	.85	.85	0.00
Cincinnati	.74	.73	−0.01	.82	.80	−0.02
Columbus	.85	.74	−0.11	.88	.82	−0.06
Toledo	.81	.82	0.01	.91	.93	0.02
All cities	.80	.79	−0.01	.87	.85	−0.02
Have no needs (%)						
Baltimore	17	46	0.29***	26	37	0.11
Cincinnati	39	22	−0.17**	36	34	−0.02
Columbus	19	27	0.08	26	25	−0.01
Toledo	28	22	−0.06	69	26	−0.43***
All cities	26	29	0.03	39	30	−0.09*
Helpfulness of services[b]						
Baltimore	1.73	1.75	0.02	1.71	1.62	−0.09
Cincinnati	1.85	1.83	−0.02	1.90	1.87	−0.03
Columbus	1.57	1.79	0.22	1.81	1.80	−0.01
Toledo	1.68	1.72	0.04	1.72	1.72	0.00
All cities	1.71	1.77	0.06	1.78	1.75	−0.03

[a] Adjusted for baseline lagged variable and subject characteristics.
[b] 1 = very helpful, 4 = not at all.
* $P < .05$, ** $P < .01$, *** $P < .001$.

statistically significant results were noted only in Baltimore and Cincinnati.

Results of the number of types of mental health care services, other than case management, received by the two cohorts suggested a nonsignificant cohort difference across cities for both study periods. However, there was a modest and significant city × cohort effect at the 12-month follow-up ($F = 3.74$, $df = 3, 547$; $P < .05$). Further analysis conducted on the city × cohort adjusted means (table 1) revealed a significant difference in favor of Cohort 1 in Baltimore; that is, clients in Cohort 1 received a broader variety of service categories compared with Cohort 2. Such a finding indicates a decrease in the breadth of aftercare services obtained by the Cohort 2 subjects in Baltimore one year after discharge from the hospital. A similar but nonsignificant pattern was observed in Cincinnati for the same time period.

It is beyond the scope of this article to offer detailed descriptions of changes observed in service categories other than case management. However, a brief summary of changes in each city at the 12-month follow-up point is instructive because of the cross-city variations. The categories of services assessed included case management, psychosocial rehabilitation, housing, vocational rehabilitation, psychiatrist, and therapist. For each category, the dichotomous response was whether or not the client had received the service during the preceding 10-month period.

In Baltimore there was a decrease in the proportion of clients reporting receipt of case management services (53 percent in Cohort 1 versus 37 percent in Cohort 2, $P < .05$) and housing services (76 percent versus 42 percent, $P < .001$) 12 months after hospital discharge, but no significant changes in other service categories. In Cincinnati, housing services (70 percent versus 55 percent, $P < .05$) decreased from Cohort 1 to Cohort 2 at 12 months. In contrast, Columbus clients reported increases in case management (64 percent versus 91 percent, $P < .001$) and psychiatrist services (68 percent versus 90 percent, $P < .01$) between Cohorts 1 and 2. Finally, in Toledo, a dramatic increase in psychosocial services was noted (13 percent for Cohort 1 versus 61 percent for Cohort 2, $P < .001$). Clearly service changes varied considerably across time in the four cities.

Needs Addressed and Helpfulness of Services

To determine how well the needs of clients were being addressed, the interviewers enumerated psychosocial and medical/clinical needs for the

respondents. The respondents were asked (a) what needs they had; and (b) if they had a need, whether they had received help for it. A ratio of the total needs addressed to the total needs identified (based upon measures determined by Tessler, Willis, and Gubman 1986) was computed for each respondent. Results showed nonsignificant cohort and cohort × city effects. Similarly, a comparison of the needs-addressed to needs-identified ratio (table 2) revealed no significant overall difference between the two cohorts at either study follow-up point. This overall trend of no difference held across all of the cities.

A substantial number of respondents from each cohort did not have a need for the services enumerated in the interviews. Thus, a dichotomous variable that indicated whether a respondent had any needs was derived. ANCOVA was conducted using this variable as a dependent measure, and the results yielded a significant city × cohort interaction effect ($F = 8.38$, $df = 1, 576$; $P < .001$) in the 2-month follow-up period (table 2). The simple effects t-tests conducted on covariate-adjusted means in each city revealed that, in Baltimore, a larger percentage of Cohort 2 reported having no needs, compared with Cohort 1 (table 2). In Cincinnati, the reverse pattern was observed. During the second follow-up period (12 months), significant cohort and city × cohort effects emerged. T-tests conducted on cohort-adjusted means indicated that a significant and lower overall percentage of respondents in Cohort 2, compared with Cohort 1, claimed to have no needs ($M = 0.30$ versus $M = 0.39$, $t = -1.73$; $P < .05$). A closer examination, however, revealed that this finding held only for Toledo (table 2).

The analysis on the helpfulness of services revealed no significant differences between the two cohorts during follow-up (table 2).

Changes in Outcome

Of all five outcome variables, the only significant difference in outcomes between cohorts was observed at the 12-month follow-up on the symptom scores ($F = 7.06$, $df = 1, 524$; $P < .01$). A simple effects t-test revealed that Cohort 2 respondents reported significantly more psychiatric symptoms one year after the initial interview than did respondents in Cohort 1 ($t = 2.40$, $df = 524$; $P < .01$), primarily in Cincinnati and Toledo. The 2-month follow-up symptom levels did not differ significantly between the cohorts (table 3).

Although none of the city-by-cohort interactions at the 12-month fol-

TABLE 3
Client Clinical Outcomes: City × Cohort-adjusted Means

	Mean[a] differences and probabilities					
	Two months after baseline			Twelve months after baseline		
City	Cohort 1	Cohort 2	Difference	Cohort 1	Cohort 2	Difference
Average number of nights hospitalized per month						
Baltimore	.56	.70	0.14	3.03	2.78	−0.25
Cincinnati	.40	.41	0.01	1.78	2.56	0.78
Columbus	.39	.29	−0.10	1.74	1.25	−0.49
Toledo	.27	.18	−0.09	2.56	1.31	−1.25
All cities	.41	.39	−0.02	2.28	1.97	−0.31
SCL-90[b]						
Baltimore	2.16	2.04	−0.12	2.03	2.17	0.14
Cincinnati	2.04	2.23	0.19*	1.97	2.22	0.25**
Columbus	2.04	2.05	0.01	2.09	2.00	−0.09
Toledo	2.03	1.89	−0.14	1.31	1.69	0.38*
All cities	2.07	2.05	−0.02	1.85	2.02	0.17**

[a] Adjusted for baseline lagged variable and subject characteristics.
[b] 1 = not at all, 4 = quite a lot.
* $P < .05$, ** $P < .01$.

low-up achieved significance, modest interaction effects were observed for general life satisfaction and satisfaction with living situation 2 months after discharge. However, an inspection of the adjusted means from both cohorts in each city revealed that Toledo produced significant differences favoring Cohort 2 for both of these variables (see table 4). A significant city × cohort interaction was observed for respondents' self-rated level of functioning ($F = 2.86$, $df = 3, 566$; $P < .05$) 2 months after discharge. This finding is apparently due to respondents in Cincinnati Cohort 1 rating their own functioning better than did those in Cincinnati Cohort 2 ($t = 2.17$; $P < .05$).

Discussion

The first major finding of this study is that improvements in services of the types targeted by the RWJF program—specifically, enhancing the

TABLE 4
Client Outcomes: City × Cohort-adjusted Means

City	Mean[a] differences and probabilities					
	Two months after baseline			Twelve months after baseline		
	Cohort 1	Cohort 2	Difference	Cohort 1	Cohort 2	Difference
General life satisfaction[b]						
Baltimore	4.64	4.35	−0.29	4.44	4.22	−0.22
Cincinnati	4.63	4.43	−0.20	4.70	4.61	−0.09
Columbus	4.33	4.39	0.06	4.55	4.62	0.07
Toledo	4.82	5.46	0.64*	5.52	5.73	0.21
All cities	4.61	4.66	0.05	4.80	4.79	−0.01
Satisfaction with living situation[b]						
Baltimore	4.96	4.93	−0.03	5.10	4.89	−0.21
Cincinnati	5.19	4.85	−0.34	5.04	5.12	0.08
Columbus	4.92	5.13	0.21	5.13	5.17	0.04
Toledo	5.41	6.01	0.60*	6.70	5.09	−1.61*
All cities	5.12	5.23	0.11	5.49	5.27	−0.22
Self-rated level of functioning[c]						
Baltimore	2.56	2.34	−0.22	2.27	2.72	0.45**
Cincinnati	2.31	2.62	0.31*	2.34	2.39	0.05
Columbus	2.47	2.31	−0.16	2.28	2.45	0.17
Toledo	2.32	2.33	0.01	2.08	1.99	−0.09
All cities	2.41	2.40	−0.01	2.24	2.39	0.15

[a] Adjusted for baseline lagged variable and subject characteristics.
[b] 1 = terrible, 7 = delighted.
[c] 1 = excellent, 4 = poor.
* $P < .05$, ** $P < .01$.

provision and continuity of case management—were found for individual clients in three out of the four cities studied. The second, and less encouraging, major finding is that we were unable to detect improvements in client outcomes related to these enhancements in case management or to the program as a whole. In fact, psychiatric symptoms were statistically, although probably not clinically, significantly worse in Cohort 2 compared with Cohort 1 in two of the cities. (The overall between-cohort difference on symptoms is 0.20 standard deviation, a small effect.) Each of the findings deserves further consideration to understand its significance.

The finding for expansion of case management services reflects the success of the local mental health authorities in the three Ohio cities in carrying out this targeted system change. These three local authorities believed that the route to improving continuity of care for clients was through case management, and the data indicate that they achieved this goal. Their success suggests that the authorities were able to direct resources to effect desired changes (Roth et al. 1993). Clearly, such changes indicate the intended capacity of a local mental health authority to assume a systemwide planning role and to implement change through control of financial streams (Aiken, Somers, and Shore 1986; Lehman 1989; see also the article by Morrissey et al. in this issue).

The finding of no increase in case management services in Baltimore illustrates both some of the pitfalls in conducting this type of evaluation and the variability in the process of implementing change through local mental health authorities. As Morrissey et al. discuss elsewhere in this issue, Baltimore was slower than the Ohio cities in implementing the activities of the local authority, but authority functioning also improved rapidly there during the last year of the project. The timing of this client outcome study was such that any changes in the quantity and continuity of case management services in Baltimore may have occurred after the client data were collected. A further complication in Baltimore, unforeseen at the time this study was being conducted, was that the areas of the city from which the majority of the client sample was drawn experienced the greatest delay in implementating new case management services. This delay stemmed largely from the politics of establishing in these areas the "lead agency" through which the case management services were performed. This unfortunate convergence of delayed fulfillment of services in the very areas of the city where the evaluation was conducted arose partly from happenstance and partly from the fact that the politically most complicated area of the city also housed the major state psychiatric facility from which clients were recruited. It should be noted, however, that the continuity of case management, as measured by the lack of change in case managers, improved significantly in Baltimore in Cohort 2.

Despite this variability in achieving case management across the cities, it is nonetheless impressive that the service effects of the program carried out at the level of a citywide authority were clearly detectable in a predictable manner at the client level. Such service penetration indicates a definite impact of the program on services throughout the system.

The positive findings regarding case management are offset by the negative findings about outcomes; that is, there was no detectable improvement in outcomes between the two cohorts in any of the cities across multiple domains. This finding requires consideration of both the methodological limitations of the study and the realities of changing outcomes for persons with CMI.

One evaluation challenge was the fact that the "program"—the creation of the local mental health authority—assumed different forms and was instituted with different degrees of success and rapidity across the cities (see Morrissey et al. in this issue). Thus, the intervention being evaluated was heterogeneous across the sites, and even across areas within sites, as we have discussed with reference to Baltimore. This heterogeneity complicated detection of service impact and would tend to obscure more specific effects. Much more detail than our evaluation supplied regarding the nature and intensity of services received by individual clients would be needed to investigate fully the relation of service heterogeneity to client outcomes.

A second methodological issue concerns the timing and length of the follow-ups. We chose the 2-month and 12-month follow-up points in order to capture any short-term postdischarge effects as well as longer-term effects. We did, in fact, note considerable postdischarge improvement in clients between baseline and the 2-month follow-up (not reported in this article). What we failed to find was differential improvement between the cohorts, that is, a program effect. Clients showed the most dramatic improvements during these first 2 months after discharge and then leveled off between 2 and 12 months. A major criticism could be made that the follow-up period was not long enough. It is frequently noted that outcome improvements for persons with CMI often require considerable time—more than a year. Hence, it can be argued that the follow-up time was insufficient to detect improvements. We were restricted to the one-year follow-up because of the pre–post cohort design and budget. The companion study by Shern et al. in this issue is helpful because they were able to follow cohorts in the two cities over a five-year period, using their comparison-site design. The similarity of their findings to those in this study tends to argue against an inadequate follow-up period as the major reason for failure to find outcome effects.

Third, the timing of the overall evaluation may have been problematic. Funding for establishing the local mental health authorities began in November 1986, a full year before recruitment of the "baseline" co-

horts (Cohort 1) began. Therefore, it is possible that the evaluation missed early improvements in the treatment system related to the RWJF program. The site-level evaluation conducted by Morrissey and colleagues indicates that this may have been the case in the Ohio cities. Conversely, as previously noted, in Baltimore, where implementation was slow, the timing of Cohort 2 may have occurred too early to detect effects of the program at the client level.

Even with these caveats about the evaluation design, the finding of an overall increase in case management without improvements in outcomes is disappointing. The research results elsewhere on the outcomes of case management are variable (Attkisson et al. 1992). Some studies report convincing benefits of case management (Goering et al. 1988; Borland, McRae, and Lyca 1989; Olfson 1990), whereas others report no benefits or even disadvantages, such as increased service consumption without outcome improvement (Franklin et al. 1987). The major problem with this body of research, of which this article can be viewed as another example, is that "case management" differs considerably across studies and is often poorly defined.

A second problem is that case management is often applied indiscriminately to all clients with the expectation that it is a panacea. Such indiscriminate application of the intervention will obscure its differential impact for client subgroups. In addition, the effectiveness of case management within a system of care is undoubtedly affected by the quality of other available services. An excellent case management service may be able to accomplish little if the rest of the service system is grossly inadequate. In this study we have essentially no information on the quality of the case management offered across the sites or differentially to clients within sites. Reflecting the logic of the program itself, the method of analysis here assumes that case management in itself is an appropriate intervention for all persons with CMI. Our analytic approach ignores issues of appropriate matching of clients to case management.

Finally, in the RWJF cities studied, the major service change was more case management, a premise supported by data from the Ohio Department of Mental Health, although in some cities there were increases in certain other services (e.g., medical services in Cincinnati and outpatient counseling services in Columbus) (Roth et al. 1993). It may be that although increased case management was a necessary first step toward improving outcomes, it was not sufficient. Perhaps other community services should be developed or enhanced to realize the benefits of better

case management. As we report, the provision of certain other categories of services actually decreased in some cities over the course of the demonstration.

Clearly the relation between change in services and outcomes is complex. The most judicious interpretation of the findings in this client study may be that the steps taken to create local mental health authorities and to improve case management and continuity of care in the cities were necessary, but not sufficient, steps toward improving client outcomes. More attention should be given to the quality and quantity of available services other than case management. There is substantial evidence from well-controlled efficacy studies that high-quality clinical, rehabilitative, and supportive services (Attkisson et al. 1992; Hargreaves and Shumway 1989), including pharmacotherapies (Kane and Lieberman 1987), family educational programs (McFarlane 1983; Anderson, Reiss, and Hogarty 1986), skills training (Liberman, Mueser, and Wallace 1986; Benton and Harold 1990), and assertive community treatment teams (Stein and Test 1980; Olfson 1990), among others, can improve client outcomes. The challenge remains to disseminate these approaches and technologies effectively so that all persons with CMI can benefit from them.

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