A Randomized Controlled Trial of Academic Group Practice

Improving the Operation of the Medicine Clinic

Harold I. Goldberg, MD; David I. Cohen, MD; Charles O. Hershey, MD;
I-Ling Hsiue, MS; Dan K. Porter, MSSA; Christine E. McLaren, PhD

We conducted a controlled trial of the adoption of a group-practice model within an academic department of medicine. Ongoing randomization yielded similar groups of patients and residents. To determine the effect of the intervention on medicine-clinic operation, we monitored the hospital outpatient activity of 28 residents and 2299 patients during an 11-month study period. The group-practice clinics generated 20% more patient encounters per month than did the traditional, control clinics (328 vs 273 encounters), primarily because twice as many voluntary, overflow clinic sessions were scheduled (20.2 vs 9.7 sessions). Yet, because group-practice registration was decentralized, patients spent 15% less time in completing scheduled visits (93.2 vs 109.9 minutes). Regular utilizers of the group practices made 7% more scheduled clinic visits on average (3.27 vs 3.05 visits), but 39% fewer walk-in visits (0.14 vs 0.23 visits). Hospital-wide, continuity of care was not affected. We conclude that adoption of a group-practice model at our institution improved clinic productivity, enhanced patient flow, and decreased unscheduled clinic visits.

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THE SHIFT toward outpatient care engendered by prospective payment has caused a resurgence of interest in teaching in ambulatory settings.[1,2] This in turn has served to refocus attention on the inefficiencies inherent in how outpatient teaching facilities are traditionally organized. Because academic clinics have been characterized as operating around the schedules of harried house officers who provide care for only a few hours each week, we again see calls for fundamental changes in how these facilities are run.[3]

During the past decade, several academic centers have sought to improve the operation of their general medical clinics by reorganizing them along the lines of a private group practice. Subsequently published reports noted increases in access to health services and decreases in the fragmentation of care provided.[4-8] In 1980, the Robert Wood Johnson Foundation funded the development of group practices involving residents at an additional 15 university hospitals. A cross-sectional study of these practices was performed to assess the primary care services and education they provided. However, because the evaluation did not involve an experimental design, it was not intended to determine the degree to which adoption of a group-practice model improved clinic efficiency at the participating institutions.[9]

Accordingly, we conducted a randomized, controlled trial of the implementation of a resident group practice at Cleveland Metropolitan General Hospital, a major teaching unit of Case Western Reserve University School of Medicine. We have previously reported that this intervention was associated with a 27% reduction in the inpatient charges incurred by patients cared for under a group practice vs a more conventional clinic model. We hypothesized that this was attributable to a shifting of inpatient workup to the outpatient setting made possible by an increase in the efficiency of the medicine clinic.[10] Other reports have documented increased patient satisfaction and show rates.[11,12]

Given the growing interest in academic ambulatory care, we thought it important to determine directly if the operation of the medicine clinic had in fact been affected positively by the group-practice reorganization. This report, therefore, addresses four principal questions. First, did clinic productivity improve, as evidenced by an increase in the number of patients seen or sessions scheduled? Was patient flow enhanced, as indicated by a decrease in processing times? If access was thus improved, was fragmentation of care reduced, ie, did utilization patterns shift away from the unscheduled use of outpatient settings in the hospital? Finally, if patterns of utilization did change, was continuity of care affected?

METHODS

The Firm System

The Department of Medicine at Cleveland Metropolitan General Hospital is organized into four general medical units, called *firms*. Each firm, consisting of a 28-bed inpatient ward and an outpatient service, is staffed by 12 to 14 residents. Residents are randomly assigned to one of the firms when they enter the training program as interns and remain with that firm for the duration of their training. Patients are also randomly assigned to one of the four firms on first contact with the department, either in the outpatient clinic or on admission to the hospital. They are then assigned on the basis of availability and are linked by computerized record to a single resident within that firm who serves as their primary care physician. When that house officer completes the program, the patient is assigned to another resident on the same firm. Although faculty members are present during all medicine-clinic sessions to serve as teachers and supervisors, they do not directly participate as providers in the outpatient care offered within the firm system. This ongoing randomization into the firms has created four similar groups of patients and resi-

From the Department of Medicine, Cleveland Metropolitan General Hospital (Drs Goldberg, Cohen, and Hershey, Ms Hsiue, and Mr Porter), and the Department of Epidemiology and Biostatistics, Case Western Reserve University, Cleveland (Dr McLaren). Dr Goldberg is now with the Department of Medicine, University of Washington, Seattle; Dr Cohen, with the Departments of Medicine and Community Medicine, Mount Sinai School of Medicine, New York; Dr Hershey, with the Department of Medicine, State University of New York, Buffalo; and Dr McLaren, with the Department of Mathematics, Moorhead State University, Moorhead, Minn.

Reprint requests to Harborview Medical Center, ZA-60, 325 Ninth Ave, Seattle, WA 98104 (Dr Goldberg).

dents, enabling us to study the effects of an intervention on one or two firms while using the others as controls.[13] For this study, two firms were randomly assigned to receive the experimental intervention, and the other two served as controls.

The Intervention

The intervention, the reorganization of two firms into group practices, consisted of a number of changes that were intended to improve resident and patient access to the medicine clinic and to improve coordination of care between the inpatient and outpatient services. We believed that these changes were interdependent and jointly necessary to provide for the practice of high-quality primary care. The organizational differences between the traditional control firms and the experimental group practices are summarized as follows:

1. The group-practice clinics were in session all ten half-days per week as opposed to only two afternoons for each of the control firms. This was generally accomplished by having residents who were not on ward rotations attend clinic in the morning. Because space requirements were more evenly distributed over the entire week, it was easier to offer experimental residents the use of more than a single examining room. Residents on both control and experimental firms were required to attend clinic once each week. The number of appointments per required clinic session scheduled by clinic receptionists remained unchanged during the study period. To accommodate additional patients, both control and experimental residents could either overbook required sessions or schedule extra "overflow" clinic sessions, if they so chose, during any of the ten medicine-clinic sessions each week. Although they did not serve as formal primary providers, nurse practitioners who staffed the walk-in component of the medicine clinic were available to see both control and experimental patients on a scheduled appointment basis during all ten sessions each week.

2. On the experimental firms, one or two house officers were assigned to be in clinic during each session. They worked with a specifically designated nurse practitioner, staff nurse, receptionist, and attending physician. The small size of this team was intended to encourage one-on-one interactions between group members. In contrast, the control clinics were staffed by six to ten house officers at a time, working with groups of practitioners, nurses, and receptionists who were not linked to specific firms. Interactions among clinic providers were minimal.

3. Resident and nurse-practitioner members of the group practices were encouraged to cross-cover one another in clinic. If a primary physician was not available, timely clinic appointments could be scheduled with another group member. A telephone answering service and on-call schedule were implemented to provide 24-hour coverage for group-practice patients. On the control firms, residents were individually responsible for the care of their own panel of patients, and there were no formal provisions for cross-coverage. Off-hour coverage (ie, nonclinic days, nights, and weekends) was generally available only in the walk-in clinic or in the hospital's emergency department. In these settings, patients were seen by providers less likely to be familiar with their medical problems. In general, both control and experimental patients could self-refer into other hospital clinics (eg, cardiology, dermatology, or surgery) where they had been seen in the past.

4. Control patients continued to register at a central clinic registration area remote from the medicine clinic. Because this area served all hospital patients attending all clinics, patients often encountered long lines. In the group practices, registration was performed in the medicine clinic by each group's receptionist. The time spent waiting to register at the clinic desk was negligible, because only a small number of group-practice patients were appointed at any given time. This change was meant to reduce the time and complexity of the registration process and to eliminate whatever additional time was needed to walk between the registration area and the clinic. Each group-practice receptionist also served as a single identifiable person for that group's residents and patients to call with requests or problems. She could be reached through a direct telephone number that bypassed the hospital's central switchboard.

5. Group meetings were held on the experimental firms so that members of the inpatient and outpatient staffs could discuss patients and develop mechanisms to coordinate patient flow between the inpatient and outpatient areas. On the control firms, no formal communication mechanisms existed between the inpatient and outpatient staffs.

Data Collection

The trial employed a pretest-posttest, hierarchical design. Patients were nested within primary residents, residents were nested within firms, and firms were nested within intervention groups. Data were collected for the study period that ran from Aug 1, 1983, to June 30, 1984 (time 2), as well as for the corresponding time period during the prior year, the preintervention period (time 1).

To measure overall medicine-clinic productivity, the number of patient encounters and the number and type of clinic sessions scheduled each month were abstracted from appointment records maintained by the clinic receptionists. Because of the normal annual turnover of house staff, we restricted this analysis to the activity of the 14 control and 14 experimental residents who attended clinic during both times 1 and 2. As shown in Table 1, the two house-staff groups were similar along demographic characteristics gathered by self-administered questionnaires completed by 27 (96%) of the residents.

Patient-related outcomes, ie, patient flow, utilization pattern, and continuity data, were recorded on all regular users of the medicine clinic who made medicine-clinic visits during the first three months of the study period. Regular users were defined as those patients who had at least two clinic encounters during the prior academic year. Of the 4190 patients seen in clinic during the first three months, 2299 (55%) were regular users. This panel, the study patient sample, was assembled in this manner to ensure that outcomes were examined for a group of stable primary care patients who would have had contact with their respective firms both before and after the intervention's implementation. Patient demographic and utilization data were obtained from the hospital's computerized billing tapes. As shown in Table 2, the patient groups did differ slightly in age and racial composition, but not in their overall use of hospital resources during the preintervention period.

To evaluate flow through the system, we used three measures of the time spent by study patients in making scheduled medicine-clinic visits. During the preintervention period, and for the control group during the study, the hospital's central registration clerks recorded the time at which each patient initiated the registration process. During the study period, experimental patients went directly to the clinic to register. For all patients during both periods, the time of arrival at and discharge from the clinic itself was recorded by the clinic receptionists. Subtraction of registration from arrival time yielded the number of minutes spent in the registration area; subtraction of arrival from discharge time yielded clinic area time. Addition of the

Table 1.—Resident Characteristics

Characteristic	Control (n=14)	Experimental (n=13)	P
Year of training			
% Year 2	57.1	61.5	.816
% Year 3	42.9	38.5	...
Sex (% male)	71.4	84.6	.410
Race (% white)	78.6	69.2	.580
Marital status (% married)	53.9	53.9	1.000
Have children (% yes)	30.8	41.7	.571
Living arrangement (% alone)	23.1	33.3	.568
Have published (% yes)	30.8	41.7	.571

Table 2.—Patient Characteristics

Characteristic	Control (n=1016)	Experimental (n=1283)	P
Demographic			
Age, mean y	62.9	64.7	.002
Sex (% male)	30.8	27.1	.053
Race (% white)	50.7	44.9	.006
Religion			
% Catholic	28.3	24.7	.172
% Protestant	62.0	65.0	...
% Other	9.7	10.3	...
Marital status (% married)	43.5	43.6	.975
Payer			
% Medicare	38.4	42.6	.141
% Medicaid	16.8	16.9	...
% County welfare	11.3	9.5	...
% Other	33.5	31.0	...
Utilization*			
Outpatient charges, mean $	940.3	893.3	.140
Inpatient charges, mean $	8225.2	7659.2	.537
Hospitalization, mean d	15.4	15.0	.859
Length of stay, mean d/admission	10.2	10.6	.809

*Resources used during the preintervention period. Inpatient comparisons were based only on control (n = 244) and experimental (n = 262) patients admitted to the hospital.

two area times yielded the total time of each patient visit. Total times did not include additional minutes spent waiting in central registration lines prior to initiating the registration process with hospital clerks. Because patients made multiple visits each year, we used each patient's mean values in our comparisons.

Utilization patterns were assessed by comparing the number and type of all ambulatory visits made by the study patient sample to hospital physician or nurse-practitioner providers. Visits were categorized as occurring in one of four settings: the medicine clinic, the walk-in clinic, other hospital clinics, or the emergency department. Medicine-clinic visits were further disaggregated by provider type (primary physician, other physician, or nurse practitioner) to allow for detection of coverage patterns. Medicine-clinic and other clinic visits were scheduled, as opposed to walk-in and emergency encounters, which were unscheduled.

To measure continuity, we calculated both a hospital-wide and a medicine-clinic usual provider continuity (UPC) score for each study patient.[14] These scores respectively represented the simple percentage of all hospital ambulatory visits and the simple percentage of all medicine-clinic visits that each patient made to his or her primary care resident. Because UPC scores are independent from the total number of different providers seen per unit time, as additional indexes of continuity, we also recorded the total number of hospital and the total number of medicine-clinic physician or nurse-practitioner providers seen by each patient.[15]

Statistical Analysis

To compare resident and patient characteristics, we used t tests for continuous variables and contingency table analysis for categorical variables. The t tests were also used for cross-sectional comparisons of all outcome variables at both times 1 and 2. In those instances where a significant difference was found at time 1, we controlled for that difference with an analysis of covariance. Time 1 values were used as a covariate to compare adjusted time 2 means. For all patient-related outcomes that then differed at time 2, we again employed analysis of covariance to correct further for patient age and race, the two prognostic characteristics that differed between the groups at baseline. Although two of the outcome differences were slightly accentuated, the analyses did not meaningfully alter our findings. Therefore, we did not include the adjustment for age and race in the results reported. Finally, we performed nested analyses of variance that confirmed that differences in patient-related outcomes during the study period were due to significant effects at the group level rather than lower levels in the hierarchy. All statistical tests employed an α level of .05.

RESULTS

The comparison of control vs experimental monthly clinic productivity for both the study and preintervention periods is shown in Table 3. The group-practice clinics generated 20% more patient encounters per month during the study period than did the traditional clinics (328 vs 273 encounters, $P = .003$). This difference was largely attributable to the fact that more than twice as many overflow clinics were scheduled to accommodate patients who needed to be seen at times other than when primary care residents were attending required clinic sessions (20.2 vs 9.7 overflow clinics, $P = .001$). Because it was also possible that increases in the number of patients seen per clinic session contributed to the difference in productivity, we calculated the mean number of patients seen for both required and overflow sessions during the study period. Differences were not found between the control and experimental clinics for either required (5.2 vs 5.3 patients, $P = .432$) or overflow sessions (1.6 vs 1.7 patients, $P = .507$).

The comparison of the time spent by patients in completing scheduled appointments to medicine clinic is shown in Table 4. Visits by group-practice patients took 15% fewer minutes on average than did control patient encounters (93.2 vs 109.9 minutes, $P < .001$). The fact that no difference was found in clinic area times indicates that total time savings were mainly due to decentralization of registration. However, experimental clinic area times included the registration process, while control times did not. Thus, the equality of time spent in the clinic area also suggests that group-practice patients may have been handled more efficiently once they had arrived in clinic as well.

Given that more overflow sessions were scheduled in the group practices and that experimental patients could be

processed more quickly, there remained the question of whether such improvements in access prompted regular patients to make increased use of the medicine clinic in lieu of unscheduled walk-in or emergency department visits. As shown in Table 5, group-practice patients did make 7% more medicine-clinic visits than did traditional clinic patients during the study period (3.27 vs 3.05 visits, $P = .003$). This difference was partly due to greater use of visits to the primary physician (2.94 vs 2.80 visits, $P = .048$) and partly attributable to the fact that experimental patients made nearly twice as many scheduled visits to covering nurse practitioners (0.15 vs 0.08 visits, $P = .001$).

Concomitantly, group-practice patients made 39% fewer visits to walk-in clinic (0.14 vs 0.23 visits, $P < .001$). However, experimental and control patients did not differ in their average number of emergency department visits (0.64 vs 0.72 visits, $P = .302$). This may have been due in part to the fact that during normal business hours, the majority of patients seen in the walk-in clinic are sent from a nurse-staffed triage area located outside of the emergency department near the central registration area. It was this population at which the intervention was targeted. A small number of walk-in clinic patients are routinely triaged directly from the emergency department. However, because our data source captured where patients were actually seen, as opposed to where they initially presented, even a decline in "urgent" use of the emergency department by these patients would have contributed to the observed decrease in walk-in utilization, rather than being counted as a decrease in emergency department visits.

The comparison of the continuity of care afforded patients is also shown in Table 5. Hospital-wide, the intervention was not associated with differences in either experimental vs control UPC scores (0.55 vs 0.55, $P = .678$) or in the number of different providers seen (3.80 vs 3.79 providers, $P = .940$). This finding parallels utilization pattern results in suggesting that the intervention, which did not attempt to limit referrals by imposing a gatekeeping function, was unable to deter a chronically ill, high-utilizing patient sample from continuing to make liberal use of hospital clinics other than the general medicine clinic. Within the medicine clinic itself, group-practice patients experienced slightly lower UPC scores (0.90 vs 0.82, $P < .001$) and saw a higher number of different providers (1.12 vs 1.41 providers, $P < .001$) during the study period. Resulting in large part from the higher number of nurse-practitioner visits, this decrease in continuity was a corollary of the increased access to scheduled medicine-clinic visits that cross-coverage offered.

Table 3.—Medicine-Clinic Productivity per Month, Mean ± SE

	Preintervention Period*			Study Period		
Productivity Measure	Control (n = 11)	Experimental (n = 11)	P	Control (n = 11)	Experimental (n = 11)	P
Total No. of patient visits†	263.4 ± 11.1	300.5 ± 9.7	.021	273.3 ± 10.9	328.2 ± 10.9	.003
No. of required clinic sessions	47.0 ± 1.9	51.0 ± 2.0	.161	50.5 ± 1.2	54.4 ± 1.6	.068
No. of overflow clinic sessions	7.8 ± 1.4	9.6 ± 1.9	.459	9.7 ± 1.2	20.2 ± 2.3	.001

*The preintervention and study periods were 11 months in length.
†Study period means were adjusted to control for the significant difference during the preintervention period.

Table 4.—Time in Minutes Spent by Patients During Scheduled Medicine-Clinic Visits, Mean ± SE

	Preintervention Period			Study Period		
Time Measure	Control (n = 1016)	Experimental (n = 1283)	P	Control (n = 1016)	Experimental (n = 1283)	P
Total visit	110.8 ± 1.6	113.5 ± 1.5	.184	109.9 ± 1.5	93.2 ± 1.0	<.001
Registration area	15.1 ± 0.8	15.4 ± 0.7	.764	16.7 ± 1.0	0.0 ± 0.0	<.001*
Clinic area	95.7 ± 1.2	98.1 ± 1.2	.137	93.2 ± 1.1	93.2 ± 1.0	.964

*Because experimental patients registered in the medicine clinic during the study period, we used a one-sample *t* test to determine if the control group mean was significantly different from zero.

Table 5.—Patient Ambulatory Utilization, Mean ± SE

	Preintervention Period			Study Period		
Utilization Measure	Control (n = 1016)	Experimental (n = 1283)	P	Control (n = 1016)	Experimental (n = 1283)	P
No. of ambulatory visits						
Medicine Clinic	2.92 ± 0.05	3.00 ± 0.05	.215	3.05 ± 0.05	3.27 ± 0.05	.003
Primary physician	2.53 ± 0.05	2.61 ± 0.05	.218	2.80 ± 0.05	2.94 ± 0.04	.048
Other physician	0.25 ± 0.02	0.30 ± 0.02	.075	0.18 ± 0.02	0.18 ± 0.01	.904
Nurse practitioner*	0.14 ± 0.02	0.09 ± 0.01	.020	0.08 ± 0.02	0.15 ± 0.01	.001
Other clinics	3.51 ± 0.15	3.43 ± 0.13	.697	3.35 ± 0.15	3.54 ± 0.14	.339
Walk-in clinic*	0.32 ± 0.02	0.24 ± 0.02	.010	0.23 ± 0.01	0.14 ± 0.01	<.001
Emergency department	0.95 ± 0.05	0.84 ± 0.05	.102	0.72 ± 0.06	0.64 ± 0.03	.302
Total Visits	7.70 ± 0.19	7.52 ± 0.18	.477	7.36 ± 0.19	7.59 ± 0.16	.371
Continuity indexes						
Hospital-wide usual provider continuity	0.48 ± 0.01	0.49 ± 0.01	.246	0.55 ± 0.01	0.55 ± 0.01	.678
Hospital-wide providers	4.28 ± 0.10	4.06 ± 0.09	.102	3.79 ± 0.10	3.80 ± 0.08	.940
Medicine-clinic usual provider continuity	0.87 ± 0.01	0.86 ± 0.01	.524	0.90 ± 0.01	0.82 ± 0.01	<.001
Medicine-clinic providers	1.13 ± 0.01	1.11 ± 0.01	.428	1.12 ± 0.01	1.41 ± 0.02	<.001

*Study period means were adjusted to control for the significant difference during the preintervention period.

COMMENT

We found that an attempt to improve the quality of the primary care provided, and simultaneously alter the character of the training experienced, in an academic medicine clinic could proceed without necessarily sacrificing efficiency of clinic operation. Indeed, the adoption of a group-practice model was associated with modest but meaningful improvement in many of the measurements we used to assess clinic efficiency.

By expanding clinic operation beyond the afternoon hours and by making even use of clinic facilities during the week, residents were accorded increased flexibility in scheduling their clinic time. The presence of a single receptionist working full-time with each group also facilitated the task of coordinating house-staff and patient schedules. Instead of feeling compelled to overbook an arbitrarily chosen weekly clinic session, it appears that the intervention made it easier for experimental residents to exercise the option of seeing many patients in overflow clinics at times more convenient for both parties. Thus, the increase in the number of patients seen was accomplished largely through a voluntary increment in resident work load, as opposed to an increase in resident productivity. It does not appear that the additional work load occurred at the expense of lost opportunity for work elsewhere in the hospital.

Overflow clinics were generally scheduled either during down time on subspecialty rotations or when temporary ward coverage could be arranged with fellow group members. In addition, by decentralizing registration to the clinic site, patient flow was enhanced. The decreased total visit times we observed, in fact, underestimated the actual time savings of experimental patients, who were also spared long waits in line prior to initiating the central registration process.

The end result of these changes was a significant improvement in clinic productivity that involved few additional costs other than those associated with the evaluation. Although an increased resident time cost was involved in providing 24-hour coverage and in attending extra clinic sessions, the intervention was accomplished through the reconfiguration of existing personnel and without increasing the total number of hours that the medicine clinic was open and staffed.

If experimental residents could not come to clinic, they had the option of asking a nurse practitioner with whom they closely worked to assist them in handling intercurrent patient problems. The resulting increase in nurse-practitioner visits caused individual primary provider continuity in the medicine clinic to decline slightly from its high baseline level. However, we believe that such an arrangement is decidedly preferable to the alternative of promoting higher walk-in utilization rates. Because walk-in patients arrive with little advance notice and are seen on a first-come, first-served basis without regard to firm assignment, their medical charts are often unavailable and they are less likely to be seen by a provider with whom they have established a longitudinal relationship. At peak periods, patients often experience long waits prior to being seen. Across both patient groups, total times for walk-in visits were 27% longer on average than those seen for scheduled medicine-clinic appointments during the study period (128 vs 101 minutes, $P<.001$).

The issue of the generalizability of our experience to other academic centers warrants comment. Most centers should be able to expand clinic operation into the morning or other times during which outpatient facilities have been chronically underused. This may require some revamping of long-standing ward coverage arrangements or subspecialty rotation schedules. Hence, the support of department chairmen committed to improving medicine-clinic performance will be crucial.[16] Changes in the registration process or in how receptionists and other ancillary personnel function will require the cooperative support of both medical and administrative staff members.

In contrast to many training programs, our residency is conducted at a single medical center. Even with expanded clinic hours, multihospital programs may not be able to use overflow clinics as effectively to increase the time spent in clinic by residents rotating through distant hospitals. Such programs may need to rely more heavily on block clinic time or, alternatively, on increases in the number of required clinic sessions.[1] If house staff cannot spend more time in clinic, more use will have to be made of midlevel practitioners or full-time staff physicians to provide necessary access and shared continuity. This in turn will emphasize the importance of establishing effective means of communication between residents and other providers, such as the group meetings we conducted.

In conclusion, it should be reiterated that current trends in how medical care is financed will continue to place increasing importance on the development of innovative approaches to ambulatory training. Therefore, although solutions to the problems of providing primary care in the academic setting will undoubtedly vary from institution to institution, many of these solutions will involve elements of team or group practice. We found that the adoption of a group-practice model at our institution was successful in improving clinic productivity, enhancing patient flow, and decreasing unscheduled clinic visits.

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