Increased access to medical care: the impact on health

Paula Diehr, University of Washington

Available at: https://works.bepress.com/paula_diehr/30/
Increased Access to Medical Care

The Impact on Health

PAULA K. DIEHR, PH.D., WILLIAM C. RICHARDSON, PH.D.,
STEPHEN M. SHORTELL, PH.D., AND JAMES P. LOGEROFO, M.D., M.P.H.

Many federally financed programs have been launched to improve the access of the poor to medical care, under the assumption that this will improve their health. The effectiveness of these programs, however, has generally been measured by increased utilization rather than by improved health. The few studies which have considered health status have shown small or negative effects. Here, data are presented from a project which provided fully prepaid care to near poor families through existing sources in the community. A group of 748 enrollees was found to report worse health on four of five health indicators after one year of enrollment in the program; further, they appeared sicker on all five measures than a group without free medical care. It is suggested: 1) that the impact of health programs on the health of a population is a complex and poorly understood issue; and 2) that increasing access to health care may not be an effective way to improve health.

WITH THE ADVENT of rapidly increasing medical care costs and the preponderance of studies which show that health problems most affect the less educated, poorer and older members of society, many programs such as Medicaid, Neighborhood Health Centers and Rural Health Programs have been planned to expand government services in the financing and provision of health care. An assumption implicit in this expansion is that, since the availability and accessibility to some (minimum) level of health services is clearly necessary for maintenance of health, provision of more health services will provide a corresponding increase in health of target groups.

The effect of this assumption is evident in the multiplicity of research studies which measure the effectiveness of health programs not by their impact on health status, but on their ability to increase utilization. This might be appropriate if the relationship between utilization and health status were known and strong; however, this is not the case.

Measures of health status, if used at all, are usually incorporated into evaluative research only as independent variables, explaining increases in utilization, rather than as measures of outcome. One exception is in quality of care studies, which frequently examine the effect of organizational or procedural factors on patient outcomes. Such studies, however, measure the impact of medical care on persons who have sought care, or on patients, which is not the same as the effect of increased ac-
cess to care on a population. The question of the impact on the health status of a population due to expanded health services remains unanswered by these quality findings.

Several studies have addressed this question with data collected for other purposes. Findings have generally been inconclusive or negative, but the nature of the data available or design problems inherent in the studies have precluded strong conclusions. For example, Wennburg and Gittelsohn found no relationship between health service input variables and age-adjusted mortality rates, but this might be due to the insensitivity of mortality rates to small variations in health services inputs, as well as to problems inherent in defining the catchment areas for the small geographical areas analyzed. Aggregated data are generally not sensitive indicators.

Benham and Benham used person-based data from national surveys in 1963 and 1970, on three health status indicators. The people surveyed were not enrolled in a demonstration project, but since government support for medical services increased rapidly during this period, it was assumed that increases in utilization across this period would result from the increased access to medical care. This, in turn, should lead to improved health status. The change in age-education-adjusted utilization rates was thus compared to mean health status in 1979, and a negative relationship was found; i.e., groups which increased utilization over this period were sicker than those which did not, providing no evidence that increased access to care in this period had improved the individual’s health. However, it seems more likely that illness caused increased utilization than that increased access to care caused worse health.

The evaluation of Canadian medicare dealt with a specific health care program, which began in Saskatchewan in 1962. When survey results for the years 1960 and 1965 were compared, the per cent of people with certain symptoms decreased from 68 per cent to 33 per cent for the high socioeconomic status group, but decreased only slightly, from 77 per cent to 75 per cent, for the low SES group. About half of the people interviewed felt that health in the province had improved, but only 20 per cent felt that their own health had improved. These data suggest some improvement in health status associated with (Saskatchewan) medicare, but improvements for the socially disadvantaged group were small.

The Many Farms experiment measured effects of introducing a system for primary health care into a Navajo community. Despite large increases in the availability of health care services, the health of the community remained essentially unchanged, with the incidence of four of the five major acute microbial diseases unchanged throughout the study while a fifth, otitis media, decreased only after five years of the program. This very limited success in a setting where there was a large opportunity for improvement of health status, although hardly generalizable to most areas of this country, has been seen as an indication of the limitations of comprehensive health care in improving the health status of recipients.

Gordis studied the effectiveness of comprehensive care programs in preventing rheumatic fever by examining the admission rate for rheumatic fever before and after the establishment of a number of comprehensive care programs in Baltimore. A statistically significant decline was noted in the census tracts where children were eligible for the programs. However, a control set of adjacent census tracts also showed a significant decline; further, the rate for the eligible tracts was extremely high initially, and may simply
have declined to a more stable rate (regressed to the mean) by the time of the second measurement. The author suggested that a more general health status measure should be used for evaluation of this type of program.

A function status indicator was developed for use as a community measurement of health in the Alabama Health Care Evaluation project, to evaluate improved public health services. The initial impact of the program was not assessed, but subsequent measures showed a slight decrease in function status over time, so that continuation of the project was associated with worse health.

In another study, Gordis and Markowitz randomly assigned 200 infants born to young mothers to either a treatment group which received comprehensive care, or to a control group which had to seek services through standard channels. The health variables studied were mortality, height and weight at one year. No difference was found between the treatment and control groups, although this may be due to the relatively small numbers of babies involved.

In a final program evaluation using health status indicators, Moore and Frank sought to evaluate a children's health center by comparing the number of days absent from school for users of the clinic in the year before the clinic opened and one year afterward. There was a general increase in absenteeism over this time, from 10.4 to 13.7 days per school year. Thus, the program was not associated with "improved" health on this measure. The authors suggested that absenteeism may not be related to health status, or that perhaps the health center had not improved people's health.

Thus, the evidence to date for the effectiveness of improved health services is weak or negative, with only three of the eight studies showing even limited success. However, these studies share a number of problems. In some, there was no specific health program, which make the attribution of findings somewhat nebulous. Some studies had no well-defined (enrolled) population, and had to use geographical definitions of eligibility. Only one study used a true control group. The others used "before-after" designs which are subject to many sources of invalidity. In addition, the same people were usually not measured at both times, so that the comparisons were fairly insensitive to changes which may have occurred.

In this paper, data are presented from the Seattle Prepaid Health Care Project for further study of the impact of improved access on the health status of an enrolled population. The underlying hypothesis is, of course, that this impact is positive, and that the studies cited above failed to detect the benefits because of problems with their data or design. The study reported here avoids many of these problems, but still raise fundamental issues in the interpretation of the results. These issues are discussed at length in the final section.

Study Background and Methods

In 1971, as part of the Model Cities Program, Seattle initiated a comprehensive health care program with the goal of raising the health status of residents of a low-income geographic area. Two specific components were developed: prepaid medical and dental services for eligible residents of the Model Neighborhood, and support services including transportation, child care, and an outreach program.

Basically, the prepaid health insurance program involved voluntary choice on the

* Eligibility for the coverage included residence in the Model Neighborhood, income within $2,000 of the Federal poverty guidelines, and priority for families with children. Welfare or medicare recipients were ineligible.
part of families between a well-established 200,000 member prepaid group practice and an independent practice plan with services available through virtually all non-federal solo and group practitioners, clinics and hospitals in the Seattle area. Both systems provided the same package of medical benefits to participants with no direct or indirect payment on the part of the enrollees.

Recipients made substantial use of the services. After six months of enrollment, approximately 65 per cent had made one or more visits, and at the end of two years 90 per cent had used services. The proportion of persons reporting they had a usual source of care rose from 66 to 88 per cent in the first year of the plan, and the per cent with a personal physician increased from 56 to 82. Finally, the mean number of medical care contacts rose from about 4.8 per year before enrollment to 7.6 in the first year of enrollment.

Each enrollee was interviewed at enrollment and annually thereafter. Enrollment was a continuous process, so that the surveys were conducted throughout the year. Five health indicators which were measured at each time are shown in Table 1, with differences in the wording between the baseline and subsequent interviews shown. These questions are similar to those of other household interview surveys.

The Seattle Prepaid Health Care Project thus provides person-based longitudinal data on the reported health status of low-income persons enrolled in a program designed to improve their access to mainstream sources of medical care. These data are used to address the effectiveness of increased access to comprehensive health services.

Study Design

To study the question of the effect of the program on health, project enrollees were divided into three groups. The treatment group consists of 748 people who were enrolled and interviewed in 1973 and interviewed in 1974. The comparison group consists of 941 individuals who were not enrolled until 1974, and thus did not have services in 1973. The main programmatic difference between the 1973 and 1974 enrollees is geographic, since recruitment for the program proceeded throughout the city, block by block. The longitudinal group consists of 1,649 different people who were enrolled in 1971, and remained on the program for three years, but did not have a baseline interview.

Three comparisons are made. The before/after (B/A) study compares the health measures of the treatment group before and after one year of enrollment in the project. The treatment/comparison (T/C) study uses the 1974 measures of both the treatment and the comparison groups, since at the time the treatment group had a year of program enrollment and the comparison group did not. The design of the B/A and the T/C studies is shown in Table 2. The third part of this study involves the longitudinal group, whose health measures are available at the end of one, two and three years of enrollment, but for which no baseline data are available. These data may be used to assess the stability of each health variable over time, to see if there are changes which occur after the second or third year of enrollment. To the extent that the longitudinal study and treatment groups are similar, the longitudinal study may provide longer term information on any findings of the B/A study.

This quasi-experimental design provides considerable protection against the two largest sources of internal validity: selection biases and trends over time which occur independent of the program. Although there is no a priori reason for the treatment and comparison groups to differ, selection bias is possible since the groups were not assigned at random.
**TABLE 1. Definition of Health Status Indicators**

1. **Perceived Health**
   - In general, would you say your health is excellent, good, fair, or poor?

2. **Limitations on Activities**
   - **Baseline:** Does your health keep you from doing any of these things? Stop me if I name one you don't do because of your health. Working, going to school, participating in sports, working around the house, doing other things you want to, no limitation.
   - **Resurvey:** In general these days, does your health limit the kind or amount of any of these things? Working, going to school, participating in sports, working around the house, doing other things you want to do. (A "doesn't apply" category was added here)
   - Coded 1 for limitations, 2 for no limitation.

3. **Number of symptoms**
   - In the last year, that's since __________, did you have these symptoms? (List of 22 symptoms including headache, unexplained weight loss, and toothache. The complete list is in Taylor et al.\(^\text{14}\))

4. **Limited Activity Disability Days**
   - How many days in the last month were you kept from (working, keeping house, going to school, doing what you usually do) because of being sick?

5. **Bed Disability Days**
   - **Baseline:** Now I am going to ask a question about the last year. Altogether during the last twelve months, how many days did you have to stay in bed all or most of the day because of illness or injury?
   - **Resurvey:** Since you joined the Prepaid Project, how many days altogether did you have to stay in bed all or most of the day because of illness or injury? (Including days in the hospital but not including days associated with a normal pregnancy—in or out of the hospital.)

* Coded 0, 1, 8, 20, based on preliminary analysis.\(^\text{14}\)
† Square root used for significance testing.
1 Mean time "since you joined the project" 13.5 months.

---

Statistical methods are used to adjust as well as possible for such differences. There can be, however, no selection bias in the B/A or the longitudinal comparisons, since each person serves as his own control. Thus, changes observed in all of the comparisons should not be vulnerable to selection bias.

Similarly, the B/A study might show spurious results because of changes over time (epidemics, aging of the subjects) which are not related to the program. This is true to a lesser extent in the longitudinal study which should be insensitive to short-term effects since a longer time period is examined. The T/C study, however, compares two groups at the same point in time, so that secular trends cannot be a cause for differences. Again, any results detected in the B/A, longitudinal and

**TABLE 2. Design of Study**

<table>
<thead>
<tr>
<th>Group</th>
<th>1973</th>
<th>1974</th>
</tr>
</thead>
<tbody>
<tr>
<td>Treatment group</td>
<td>&quot;Before&quot;</td>
<td>&quot;After&quot; = &quot;Treatment&quot;</td>
</tr>
<tr>
<td>(n = 748)</td>
<td>Enrolled, had baseline interview</td>
<td>After 1 year of service, received resurvey interview</td>
</tr>
<tr>
<td>Comparison group</td>
<td>No information</td>
<td>&quot;Comparison&quot; enrolled, had baseline interview</td>
</tr>
<tr>
<td>(n = 951)</td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

993
the T/C studies are robust to problems of secular trends.

Findings which occur consistently in the three studies may be considered relatively valid. The only sources of internal invalidity which are not controlled in at least one of the comparisons are "pretesting" and "instrumentation." The pretesting effect would occur if the baseline interview caused people to alter their health perceptions. Since the questions asked were fairly innocuous, and the post-test occurred one year later, this should not be a serious problem. The slight differences in the wording of some of the questions on the two survey instruments may have elicited different responses on the two interviews; again, this does not appear to be a serious consideration. The same group of interviewers was used for both surveys, and consistency was stressed.

This design is externally valid if enrollment in the Prepaid Health Care Project may be considered equivalent to increased access to health care. Since the program made free health care available to a near-poor population, and increased the quantity and changed the patterns of utilization, it seems fairly safe to make this assertion. It is, however, possible that the enrollees already had adequate access to necessary health care, as evidenced by the 4.8 physician contacts per year prior to joining the program, and that this project may have increased access to unnecessary medical care rather than to all medical care. This is discussed in the final section of the paper.

Analytical methods used included transformations of the health variables to improve their statistical properties, both parametric and non-parametric testing, and multiple regression to hold characteristics of the treatment and comparison groups more nearly constant. In addition, the data were analyzed in "split halves," where the data set was divided in half at random and findings for the two halves compared, to guard against spurious findings. Since these approaches provided similar results, however, the findings are presented as differences in mean values, with the paired or unpaired t-test used to test for significant differences. Given the existing literature there is no reason to expect an a priori positive or negative impact; therefore, two-tailed tests are used.

Findings

Some baseline characteristics of the treatment and comparison groups are shown in Table 3. The age and sex distributions are quite similar, but the comparison group has significantly more whites. The choice of plan, number of physician contacts, and number of chronic conditions were similar for the two groups, but significantly more people in the treatment group had an episode of illness during the three months prior to joining the program. The high proportion of blacks as well as the relatively young age of both groups may affect the generalizability of the results.

The longitudinal group was very similar to the other groups on age and sex, but had significantly more blacks than either group, and significantly more independent practice enrollees than the comparison group. This probably reflects geographical patterns in the racial composition of the Model Neighborhood, since the longitudinal group was the first enrolled and the comparison group the last enrolled. Although the longitudinal results will be consistent, since each person is his own control, the differences in the racial composition may lessen the comparability of the B/A and the longitudinal results.

Table 4 shows mean values for the five study variables: for the treatment group in 1973 and 1974, and for the comparison group in 1974. Significant and "nearly" significant differences are presented for the B/A comparison, the T/C differences,
TABLE 3. Baseline Characteristics of Treatment and Comparison Groups

<table>
<thead>
<tr>
<th></th>
<th>Treatment</th>
<th>Comparison</th>
</tr>
</thead>
<tbody>
<tr>
<td>N</td>
<td>748</td>
<td>951</td>
</tr>
<tr>
<td>Mean Age</td>
<td>25.2</td>
<td>25.0</td>
</tr>
<tr>
<td>Age Range</td>
<td>1-65</td>
<td>1-65</td>
</tr>
<tr>
<td>Sex</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Per cent male</td>
<td>43</td>
<td>45</td>
</tr>
<tr>
<td>Per cent female</td>
<td>57</td>
<td>55</td>
</tr>
<tr>
<td>Race</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Per cent white</td>
<td>20</td>
<td>28*</td>
</tr>
<tr>
<td>Per cent black</td>
<td>63</td>
<td>60</td>
</tr>
<tr>
<td>Per cent other</td>
<td>17</td>
<td>12</td>
</tr>
<tr>
<td>Plan</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Per cent prepaid group</td>
<td>29</td>
<td>28</td>
</tr>
<tr>
<td>Per cent independent practice plan</td>
<td>71</td>
<td>72</td>
</tr>
<tr>
<td>Physician Contacts in Month Prior</td>
<td></td>
<td></td>
</tr>
<tr>
<td>To enrollment</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Per cent no</td>
<td>74</td>
<td>71</td>
</tr>
<tr>
<td>Per cent yes</td>
<td>26</td>
<td>29</td>
</tr>
<tr>
<td>Chronic Conditions</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Per cent none</td>
<td>56</td>
<td>53</td>
</tr>
<tr>
<td>Per cent one</td>
<td>21</td>
<td>20</td>
</tr>
<tr>
<td>Per cent &gt; one</td>
<td>23</td>
<td>27</td>
</tr>
<tr>
<td>Two-day Episode of Illness in Three Months Prior to Enrollment</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Per cent no</td>
<td>69</td>
<td>75*</td>
</tr>
<tr>
<td>Per cent yes</td>
<td>31</td>
<td>25</td>
</tr>
</tbody>
</table>

* p < .005.

...and the "comparison"-"before" differences which tests whether the treatment and comparison groups where comparable when they entered the program.

Note that the 1974 treatment scores are higher (sicker) for four of the five measures in the B/A comparison and for all five measures of the T/C comparison. These

TABLE 4. Mean Values for Health Indicators

<table>
<thead>
<tr>
<th>Group</th>
<th>Treatment 1973 (Before)</th>
<th>Treatment 1974 (After)</th>
<th>Comparison 1974 (Comparison)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Perceived Health Status*</td>
<td>2.56</td>
<td>2.86</td>
<td>2.45</td>
</tr>
<tr>
<td>Proportion with Limitation†</td>
<td>.12</td>
<td>.19</td>
<td>.12</td>
</tr>
<tr>
<td>Number of Symptoms‡</td>
<td>2.14</td>
<td>2.60</td>
<td>2.22</td>
</tr>
<tr>
<td>Limited Activity Disability§</td>
<td>1.82</td>
<td>1.62</td>
<td>1.21</td>
</tr>
<tr>
<td>Bed Disability Days§</td>
<td>7.41</td>
<td>8.33</td>
<td>5.09</td>
</tr>
</tbody>
</table>

* Treatment group sicker for B/A and T/C comparisons (p < .05, one-tailed test)
† Treatment group sicker for B/A and T/C comparisons (p < .01, two-tailed test)
‡ Treatment group improved in B/A comparison (p < .05) but sicker than comparison group (p < .05, one-tailed test)
§ Treatment group (before) sicker than comparison group at enrollment (p < .01, two-tailed test)
When bed disability days were changed to ranks or square roots, a decrease was seen in the B/A comparison.
differences are highly significant for limitations and symptoms, and marginally significant for perceived health status (p < .05 for a one-tailed test). The results for the two disability days measures are inconclusive, since the T/C comparison shows the treatment group significantly higher but the B/A comparisons (after transformation of the data) show that there was a decrease over time in the number of disability days.

Examination of the 1973 (before) and 1974 (comparison) values show that the treatment and comparison groups were comparable at enrollment for the first three health measures. However, the treatment group had significantly more disability days and bed disability days than the comparison group at enrollment. The treatment group was also significantly higher at enrollment than the age/sex/race adjusted national average for these two measures. The high number of disability days in the treatment group before joining the project suggests that regression to the mean could have caused the drop in the B/A study after one year. Multiple regression analysis of T/C data to adjust for other demographic and health characteristics did not change any of the results found above. Further analysis of the B/A data showed that the decline in health status was significantly larger for older persons.

Examination of the longitudinal trends for 1,649 people followed for three years showed that the levels of perceived health status, symptoms, and limitations were quite stable, with the first two showing a slight increase of year 2 over year 1, and the third showing a slight decline over three years on the order of one-tenth of a symptom. The two disability measures appeared quite unstable over time, with limited activity disability days decreasing an average of .4 days in year 2, but returning to previous levels in year three; bed disability days showed an increase of .3 days from year 1 to year 2, with the same level at year 3. Thus, the three health measures for which the B/A analysis showed significant decline in health appear to be quite stable even after three years of enrollment in the program; the two measures where the results were equivocal showed more variability over time.

Summary and Discussion

This study has followed a "treatment" group of people with improved access to medical care in the form of completely free care from the provider of their choice, together with improved community services such as outreach, child care and transportation. After one year, the treatment group reported worse health on four of five health measures examined. When compared to a similar group with no increased access, the treatment group appeared less healthy on all five of the measures, even after adjustment for demographic and health variables. The differences in perceived health, number of symptoms, and limitations on activities were consistent between the studies, statistically significant, and stable over time, while the differences in disability days were inconsistent, and were not stable. On the average the treatment group reported .5 additional symptoms, 5 to 9 percentage points more people with limitations, and one to three more bed disability days per year. Thus, the group with increased access to medical care apparently became "sicker."

Does this imply that increased access to medical care makes people less well? Before dealing with this question, one must consider the nature of the health status measures used and the problems of interpreting any findings of program impact on health. These measures are discussed in the general framework of validity, reliability, sensitivity, and specificity.

The issue of validity is mainly concerned with whether the indicators used actually measure health. Although the
questions seem to measure health, they might not elicit health-related responses. This problem is often resolved by noting positive correlations between various health measures used, showing that a common construct is being measured by the different measures. For our data, these correlations were about .4 and highly significant, lending modest support for the validity of the measures used.

The indicators used in this study deal with how a person feels in general, how he thinks his health limits his activities, and how he actually deals with this in terms of restricted activity or bed disability days. These are clearly a part of health, but certainly do not cover the entire domain, which must include measured or observed symptoms and conditions, morbidity and mortality, as well as function and prognosis. However, the studies mentioned in the literature review used other measures such as mortality, disease rates, function status, growth, and absenteeism and also failed to find positive benefits.

Reliability is essentially a function of the variability of an indicator, showing how much the measure might vary in short time periods due to chance alone. Low variability permits the detection of smaller differences, and so is desirable. Ware found that self-reported measures were reliable in other studies. This study shows some statistically significant differences, between and within persons, suggesting that the reliability of the data we used was adequate for our purposes. In addition, the correlation structures of the health variables in the two “split halves” of the data were very similar, providing further support for the reliability of the measures used.

The sensitivity of health status measures to program effects is not known, but they might be insensitive either because they do not address areas where major program benefits occurred or because the health changes are too small to be detected with the samples available for study.

As an example of the first lack of sensitivity we note that improvement in vision (via prepaid eye examinations and glasses) was probably a major health benefit of this program since refraction was by far the most common outpatient procedure. However, the effect of less than optimal vision on disability days, symptoms, limitations, or even on perceived health status is probably negligible. A future study might look for perceived improvement in vision in order to better document program benefits.

It is possible that all of the expected benefits are very small, and so would require very sensitive measures or large study populations to show the differences. One might wonder about the value of a program which provided such small benefits. In this study, however, statistically significant “negative” health benefits were found. Thus, the small size of any expected benefit is not an issue.

Finally, we deal with the specificity of health measures: will they change only as a response to the project impact on health, or are there other “false positive” factors? A problem with the broad spectrum of items labeled “health” is that programs which impact on some aspects of health may not affect all aspects of it, and may in fact affect different areas in the opposite direction. For instance, one could divide health into “objective” and “subjectively perceived” components, then construct examples where the intervention of a program such as this would have different impacts on those two areas. A “worried sick” person might improve in both objective and perceived health; a “worried well” person might have improved perceptions, although his objective health would not change; an “unworried well” would show no change on either measure; an “unworried sick,” on the other hand, might be found to have hyperten-
sion, and although his prognosis would be for improved physical health his perceptions might be permanently lowered; and finally, there are the "unworried sick" with a condition which cannot be treated, such as "prediabetes," whose long-term prognosis is not changed, but who must now adjust their perceived health downward. Without a model of the impact of increased access to care on different types of people and different aspects of health, it is difficult to draw definitive or consistent conclusions about changes in health.

With respect to the current study, increased access to care (as provided by this project) was associated with lower perceived health status, more symptoms, and more perceived limitations on activities. These findings are consistent with a number of other studies in the literature and suggest several possible implications. The first is that the inverse relationship between improved access and lower perceived health status is desirable because people's more realistic knowledge of their health status may, in the long run, lead to improved health. The second implication, following Illich, is to suggest that unrestricted access to medical care actually causes more sickness and is another example of social iatrogenesis. The third implication is that the results support what many groups are coming to believe; namely, that above a certain level, medical care itself bears little relationship to general health and that what really influences health status is the quality of the physical and social environment and one's personal health habits and lifestyle. This is not to suggest that medical care cannot be beneficial to a sick person but, rather, to suggest the instances in which it is beneficial may carry less weight than originally believed and are not likely to influence such population based measures as disability days or symptom reporting.

Put another way, there is considerable danger in generalizing from the known fact that medical care can improve health in the face of illness to the supposition that improving a population's access to medical care of all kinds will improve its health. The problem is that sick people may already be obtaining medical care for their problems, and that enormous expenditures on improving access through comprehensive programs may achieve improved access to medical care only for apparently unimportant problems.

The lack of support for a positive relationship between access and improved health does not, of course mean that equity of access to health services is necessarily any less important as a social goal. The basic issue of what minimum level of health services should be available to all Americans remains, but the findings from this and other studies suggest that this decision needs to be tempered with the knowledge that increasing access is not likely to result, on average, in improved health status for the population, and may lead to greater behavioral disability.

More sophisticated policies which attempt to take into account those covered services which result in improved health rather than those which do not, need to be developed. In addition, such policies would consider the benefits from alternative programs to improve physical and social environments and encourage more healthy lifestyles and health practices. To develop such policies, future evaluations of health programs should increase their emphasis on health measures, improve the study designs by including true control groups, and use a larger number of different health measures which will cover the domain of health adequately.

References


17. Ware, J.: The Reliability and Validity of General Health Measures. Presented at the Health Status Index Conference held in Phoenix, Arizona (October), 1976.


---

Erratum

"Access to Medical Care in the U.S.: Realized and Potential." July 1978, p. 543. Table 5: The first four values in the column "Indirect Effects—Through Enabling Variables" should be .024, .005, .009 and —.043. The sixth value in the column "Indirect Effects—Through Illness Variables" should be .000. These changes do not affect any substantive comments in the text.