

And a Response

WHEN I STARTED DEVELOPING PLANS to study the library as an open system, I found no awareness of the concept in library literature, and my discussion of it with other librarians indicated a general skepticism regarding certain key aspects. But library administration generally was (and is) so obviously in need of revamping that I decided to commit myself to its study and improvement and to use the open system theory for direction. But, of course, one must start such a study by cutting out a manageable area for research. I chose participative management because I recognized in Likert's writings, and the generalized theory drawn from it by Katz and Kahn, answers to behavior under stress which I had observed and experienced in my own library staff prior to beginning my doctoral study.¹

But even participative management was too broad a study for a dissertation, so I limited it more specifically to the area of decision making and determined to be satisfied with manifestations of such generalized effect as a horizontal study might indicate within the parameters between input into the library and its outputs. This meant that no in-depth, step-by-step causal ladder would be likely to emerge. But it appeared proper to establish the general relationships initially before filling in the specifics. I established other constraints, such as type and size of library, to try to control some external variables. Yet, despite this, many variables remained

which might confound the relationships to be studied.

In ideal experiments, variables are either brought under control, which calls for satisfactorily accurate measurement, or are randomized. In nonexperimental surveys and investigations, uncontrolled variables are only assumed to be randomized.

I attempted to identify such variables as appeared to me likely to affect library outputs. In doing so, I paid particular attention to the standard statistics that are compiled, since their general acceptance insinuates belief that they are important and since action has been taken to attempt to standardize their collection and definitions. Other variables were also identified and added as appeared appropriate and as means of measuring them, presumably accurately, were found or devised. However, concern for the accuracy of measurement of many variables often nags at one, especially as one finds cases in which the data collection has been incomplete or not conforming to a standard definition. In such cases, there are only a few practical alternatives. One might reject the variable from the study, assigning it to the group of uncontrolled variables which might act to confound the study. One can attempt to improve the measurement, although this option is sometimes not open. Or one can use the values available if the inaccuracy is sufficiently minor.

Nonexperimental research generally requires sampling a total population rather than drawing data from every population member, and this leads to concern over the extent to which the sample typifies the entire population. I made choices by which I tried to assure

Dr. Marchant is associate professor, Graduate Department of Library and Information Sciences, Brigham Young University, Provo, Utah.

representation of various characteristics, but only future application of the research results to other libraries can determine their application beyond the sample studied.

In my research, the number of libraries studied was twenty-two, which was smaller than might be hoped for. It meant that a fairly high measurement of relationship had to exist in the sample in order to be significant for the total population at any magnitude.

The basic statistical procedure which I used to test the relationships being studied was linear multiple regression analysis, a process which also provides multiple and partial correlation coefficients. It allows for predicting a dependent variable by the joint variance of two or more independent variables. It does not demonstrate causality, however, which must rely on a theoretical or logical basis. But, given such a basis, it can test the theorized relationship and confirm its likelihood and magnitude within stated statistically significant levels. However, care needs to be taken against misuse of this procedure to assure the quality of the results.

Ms. Lynch's complaints concern several matters, a few of which are major. Some, especially minor ones, are not well expressed and lead to the impression of generalized errors whereas, in my attempt to identify their referral points, I have found them to deal with single and minor matters.

One of Ms. Lynch's major complaints was really directed at Rensis Likert's theory of participative management. She went to considerable pains to point toward publications which disagreed with Likert. This is all very admirable. I chose to emphasize McGregor, Argyris, Maslow, Marrow, Bowers, Seashore, D. Katz, R. L. Kahn, Herzberg, Blake, and Mouton instead.² I mentioned a small number of demurrals to bring attention to their existence; but, since I was testing the Likert theory, it seemed

proper to me to discuss the theories and research which generally support Likert. We also shared some discussion, since we both mentioned Pelz' work. And the relationship between participative management and variable work was also emphasized by Likert. I find it difficult, considering the magnitude of literature related to the subject, to understand why her choice of readings should be considered preferable to those included in my study. Mine are certainly as up-to-date as hers. Except to rebut the insinuation that I am ignorant of the field, I feel that this part of the discussion is irrelevant. However, I appreciate her references and will review those with which I am unfamiliar for future interest.

Ms. Lynch also accused me of having neglected the entire corpus of literature regarding job satisfaction. Ms. Lynch is simply wrong, and I refer her to the dissertation regarding this matter.³ Mentioned there are the writings and research of Herzberg, Argyris, Maslow, Ash, McGregor, Likert, Mayo, Myers, Paul, Roche, Pffner, and Etzioni. Some of these describe specific research projects and results dealing with motivation and satisfaction, and the last two include short surveys of the literature. This subject certainly could have been expanded; but with thousands of papers to draw upon, there must be a point of diminishing returns. Dissertations are notorious for their lack of readability and I am sure mine is no exception. I chose to sample important sources rather than exhaust them (and the reader in the process) because the statistical data and analyses made the text as formidable as most librarians are likely to be able to stand anyway.

More central to the issue was her criticism of my definition of participative decision making. Actually, I felt I had described it quite well. In the *Library Trends* article, I stated that it "was an index of the extent to which the profes-

sional library staff perceives of itself as involved in the 'decision making process' and referred to Likert's questionnaire for specifics.⁴ The dissertation described the three factors involved and included the precise wording of the questions.⁵ Because Ms. Lynch doesn't like the definition is hardly to say it is vague. Her statement that I am unclear regarding whether I mean "actual decision making, perceived decision making," and so forth seems to me due to her imprecise reading. I knew what I meant and said so.

I added a second independent variable, which I called the Profile. This was not, as Ms. Lynch suggested, intended to be a measure of decision making. It was intended to be a measure of managerial style, and I said so in both documents.⁶ I accurately described Likert's concept of managerial style as composed of seven interrelated factors, one of which is decision making.⁷ The high correlation between the two independent variables and among pairs of the eighteen items making up the questionnaire are not indications that they are measuring managerial style. The correlations simply demonstrate that they vary together. Since decision making is part of managerial style, one should expect a significant correlation, although theirs is surprisingly high. That they are not the same measurement is demonstrated both by the questions asked and by comparison of their means, which differ at the .05 level of significance.

Ms. Lynch acknowledged that the Likert instrument has been validated "to some extent." But why did she insert the phrase "to some extent"? All validations can be so described. I think she intended the phrase to suggest a low magnitude of validation. And even if she didn't, that is likely to be the impression projected to most readers. Yet I suspect Ms. Lynch knows little more about that instrument than she acquired from examining it. Those who have used it ex-

tensively can describe its shortcomings, but are also strong in praising it. After my use of it, I sent Dr. Likert an appraisal and suggested an area in which it might be improved. His response was that he intended to revise it soon and would try to rectify the weakness. It has been revised several times and even has variant forms for use with particular groups. At least one form has more than one hundred items. This growth and diversity are the result of highly competent study based on many research projects. The eighteen items in the short form I used were carefully chosen from larger instruments to represent the important dimensions of managerial style and of each of the seven processes of which it is composed. Ms. Lynch's complaint of the low reliability possible with three-item scales is not applicable here because they were carefully chosen from among a larger group to include those most important in describing and measuring the decision making process. In fact, use of this instrument was determined in part by the reliance that could be placed upon it and the extent to which it has been validated by use.

Beyond that was another consideration. I had initially intended to develop my own instrument for measuring decision making and was, in fact, in process of doing so. Learning of my study, Dr. Likert suggested that I use his. I decided to do so because of its high quality and because it would allow expansion of the study into the other six organizational processes. The dissertation, utilizing only two somewhat generalized indexes, has become phase one of my study. Further work has included consideration of each of the eighteen items separately and more in-depth consideration of other variables. The total pattern has been to confirm the dissertation findings generally and to define the interrelationships more specifically.

There seems little purpose in expending great energy validating an instru-

ment already known to be satisfactory for the purpose to which it is to be put. I proposed to test Likert's theory on libraries. What could be more satisfactory than to use his measuring device?

Ms. Lynch complained that I changed Likert's independent, intervening, and dependent variables and added my own control variables and she reproduces part of page 137 of his *The Human Organization* to indicate what she thought I should have tested. Except as regards the independent variables already discussed, I acknowledge her statement. But then I never aspired to test that specific model. It is appropriate to profit-making organizations but not to such nonprofit organizations as academic libraries. She is correct in assuming that my intention was to apply Likert's theory to the library setting rather than to reevaluate the theory. So why should she complain when I do what I propose? Considering the difficulty of measuring library performances, I felt my limited endeavor was quite sufficient for one dissertation.

I fail to understand the complaint regarding my use of control variables. Their use in multiple regression analysis is well understood, and they offer data by which the true relationships between the independent variables and the performance measurements might be better approached. The simple correlations as manifestations of those relationships are certainly less acceptable. To the extent that such influences are considered and, when appropriate, partialled out, the values of the primary relationships are improved.

However, Ms. Lynch manifested a lack of understanding regarding the proper discipline to be placed on multiple regression analysis, its interpretation, and my use of it. She also misstated the total number of variables involved in the research.

Listed in my research model are ten groups of control variables which final-

ly accounted for twenty variables which were used in the dissertation phase of the research.⁸ This was more than could enter the regression analysis along with the independent variables because of the limitation placed by the number of libraries in the study, so three variables which were found to be largely repetitious were deleted.

It would be most surprising to find more than three or four variables enter significantly into a multiple regression analysis with such a small sample. Moreover, in order to enter, a variable must explain a fairly large percent of the variance remaining at that point in the dependent variable. The partial correlations I reported between the independent and dependent variables partialled out only the variance attributable to control variables which had entered at the .05 level of significance. And the levels of significance which I reported are as accurate as possible considering the limits placed by the small sample size.

Ms. Lynch's complaint that I have padded the proportionate variance (coefficient of determination was her term) by including all the control variables is without substance. While I presented tables including as many as seventeen control variables and showing the cumulative proportionate variance, I also showed the significance levels related at each step as well as the significance level of each variable at entrance into the analyses. This was a preliminary step. Then I reanalyzed the problem allowing entrance only to those control variables which could enter significantly at the .05 level plus the independent variables separately. I even went to the trouble of correcting cumulative proportionate variance for bias to assure as much as possible against overstating it. My procedures were particularly conservative and my evaluation cautious. I suspect some significant relationships were missed thereby, and it is important

not to assume that relationships do not exist simply because they are not demonstrated.

The fact that more than one variable entered a regression analysis significantly indicated that they independently explained significant amounts of variance in the dependent variable. This does not mean that they are independent of each other and I did not so state nor imply. It is even possible that some of the control variables cause libraries to move toward participative management or toward that part dealing with decision making. But that was not what I was studying. I assume such causal variables exist and might be identifiable, and I think it would be worth studying. But it wasn't part of my study.

It is also possible that the independent variables cause some of the control variables or that both result from some unidentified variable. In either of the last two cases or if spurious correlation coefficients result from sampling bias, inclusion of a control variable can mask the true relationship being searched. The proper way to deal with such an eventuality is to delete the confounding variable. I have done so, but only when there was real evidence in the specific case of likely confounding. When doing so, one ought to be aware of the possibility that the deleted variable rather than the independent variable might belong in the analysis. However, the primary purpose of introducing control variables into the study was to partial out their effect on the dependent variables in order to determine more accurately the true relationships between the independent and dependent variables. The independent variable measurements were assumed to be accurate, and partialling out of variance in the independent variables tended to confound rather than clarify the relationships sought. It would be best if such a problem did not arise. It did so only once; and in that case, the resulting partial

correlation was of questionable significance (.10) even with its use and was so identified. In other words, while a strategy for handling such cases was developed, within the study it was little used. Ms. Lynch has assumed erroneously that it was used routinely. Had she checked the analyses she would have known better.

Ms. Lynch complained of my procedure for studying the relations between the independent variables and the control variables wherein I inserted the control variables as predictors of the independent variables and then deleted several of them in sequence to determine the decrease in cumulative proportionate variance that resulted. This was simply a practical procedure for studying interrelationships and for providing insight into their potential confounding effect. It was not part and had no direct bearing on the regression analyses which were intended to clarify the relationships between the independent and performance variables at all, as the reading of her paper suggests. When dealing with as many variables and as few libraries as are involved in this study, one needs as much insight as possible regarding their interrelationships. Ms. Lynch's statement that in doing so I violated the assumption that error terms are randomly distributed is absurd. She might be reminded that random distribution does not mean equal distribution.

Ms. Lynch accused me of implying a lack of relationship when variables are not controlled in computing partial correlation coefficients. I did no such thing. The partial correlations are meant to clarify relationships to the extent possible considering the complications involved and limitations imposed by such realities as size of the sample. Libraries contain complex interactions, and anyone who believes they can be sorted out so as to describe accurately all their true and independent causal relationships is deluding himself. Ms.

Lynch's accusation that I implied non-relationship suggests that she needs more experience with multiple regression analysis.

Another example of her statistical in-expertness was demonstrated by her labeling as a flaw in regression analyses the long quotation from my dissertation regarding the meaning of the individual variables as predictors of staff satisfaction. Regression analyses predict best around the mean values of the variables involved and become less successful as the values move toward the limits of those sampled. Predicting beyond the limits or outside the boundaries of the relationships existing within the libraries involved in the study is speculative. Ms. Lynch needs to discipline herself to this reality.

Regarding her accusation that I confused partial correlations with causality, I refer her to page eighty-one of the dissertation, part of which reads, "Statistical relationships are inadequate for the purpose of proving causality but are useful in verifying causal relationships empirically which have been otherwise inferred by logical or theoretical procedures." I laid out the logical and theoretical undergirdings. Ms. Lynch's complaint appears to emanate from her disagreement with the theory, as previously discussed. But that is her problem. The results tended to support my position, especially from managerial style to staff satisfaction to faculty evaluation.

Again, regarding my preliminary model of causality, Ms. Lynch accused me of assuming that no relationships exist that are not stated. There is a great difference between stating that a relationship does not exist and that it has not been demonstrated. I presented some interrelationships that appeared to have been demonstrated within reasonable bounds of logic and statistical significance. I was even careful to state that it is incomplete and has weaknesses. That Ms. Lynch continued to assume

that a lack of demonstrated statistical significance carried the intention of no significance indicates a gross misunderstanding of the meaning of correlation coefficients.

As a matter of fact, considerable attention has been given since completion of the dissertation to filling out and improving the model. A few errors have been identified, corrected, and used in reevaluations. I hope by this time next year the improved model will be available (ALA is the publisher). I have no illusions, however, regarding its completeness even then.

I did not assume and do not believe that managerial style is a static variable. I speculated on, but did not study, the possible effect of historical factors on the current state of a library's managerial style. Inertia is generally thought to influence behavior, but change occurs despite it.

I am fascinated by Ms. Lynch's conflicting tendency to complain that my research is too broad and not adequately refined on the one hand and, on the other, that I have failed to study various related problems.

While I was still analyzing some of my data, I appeared on a discussion panel regarding participative management at which Dr. Stanley E. Seashore, then associate director under Dr. Likert at the Institute for Social Research, was the main speaker. Afterwards, while we traveled together to Ann Arbor, he asked me to describe my research. I did so, emphasizing some of the problems that bothered me. I was particularly concerned because of the small sample size, and I asked him for his evaluation. He agreed that more libraries would have been better, but he also said it was the largest study of its kind that he knew of. He suggested it was an important study and made no reference to the possibility that it was outdated by recent research. And he brought to my attention my responsibility to distribute the

results once they were complete. Given the comparison between Dr. Seashore's and Ms. Lynch's evaluation of the importance and timeliness of my research, I have little difficulty choosing.

I expect criticism, especially from those who feel threatened by suggestions of administrative change. I even solicit criticism from knowledgeable people who can offer suggestions for improvement. But I wonder about the value of a paper which starts by saying the research is "better than many recent studies," claims to review the study, finds thereafter not one good thing to report, and then deals with it ineptly.

It might be that future research will better explain the relationships I have studied. I hope so. Man is not likely to do better than approach truth, particularly in the study of human behavior, so there is always room for improvement. The study of libraries as dynamic operations has been largely ignored, and mine is only a pioneering study. Ms. Lynch has demonstrated that she is acquiring the competency to aid in that endeavor despite some present inadequacies. I will look forward to reviewing her contributions as she completes her dissertation.

REFERENCES

1. Rensis Likert, *New Patterns of Management* (New York: McGraw-Hill, 1961), and *The Human Organization* (New York: McGraw-Hill, 1967); Daniel Katz and Robert L. Kahn, *The Social Psychology of Organizations* (New York: Wiley, 1966).
2. "The Effects of the Decision Making Process and Related Organizational Factors on Alternative Measures of Performance in University Libraries" (unpublished Ph.D. dissertation, Univ. of Michigan, 1970). See the bibliography, p. 300-5. Also Chapter II, Survey of Literature, p. 49-66, especially the footnotes.
3. See the dissertation bibliography, Chapter II, and p. 161-3 of Chapter VI, "Staff Satisfaction."
4. "Participative Management as Related to Personnel Development," *Library Trends* (20 July, 1971), p.52.
5. Dissertation, p. 8-9, 127-9, and 276-8.
6. *Library Trends* article, p.52; dissertation, p.8.
7. Dissertation, p. 127-8.
8. Chapter IV of the dissertation; p.84-126 describes them. Table 4.16, p.120 lists them with their intercorrelations.