Correspondence

Response to Adams and Bumb, "The Economic, Political, and Social Dimensions of an Indian State: A Factor Analysis of District Data for Rajasthan," in *Journal of Asian Studies* XXXIII (November 1973), pp. 5–23.

The article, "The Economic, Political and Social Dimensions of an Indian State," by John Adams and Balu Bumb in the November 1973 issue attempts to describe several important aspects of the social patterns of an Indian state based on measures of economic, geographical, political and social factors. The authors' aim is to identify ways in which districts vary among themselves, to rank them according to several underlying factors, and to suggest tentatively that these factors might serve as the basis for a revised public policy of development, especially in the rural areas. They above all wish to show that merely grouping districts by geographic propinquity, as is often done, will result in poorly applied policies. Four factors were identified by the authors: urbanization, agricultural development, political participation and ecological characteristics. Each factor is taken to represent a different aspect of economic development. The twenty-six districts of the north Indian state of Rajasthan are used, and twenty-five variables are analyzed.

The article is open to question both on methodological and on substantive grounds. Both of these are important, for the work presented by Adams and Bumb is among the few using such macro-data to analyze the changing social situation in India. At its best this sort of macroanalysis should be applicable to other parts of India and the non-specialist reader should be aware of some of the questionable assumptions made by the authors. Since the substantive errors may derive from the methodological problems, the latter will be treated first.[1]

Adams and Bumb do not give the reader information crucial to interpretation of their data. We do not know, for example, which basic solution was employed, the type of rotation used, how factor scores were computed in light of the ill-conditioned matrix (defined below),[2] nor the sizes of the eigenvalues. Without the eigenvalues, the reader is not able to satisfy himself that the authors' findings are being properly interpreted. We assume that Adams and Bumb have employed the most common type of factor analysis, the principal components solution and varimax rotation, but this should be stated. (In trying to replicate and verify the results published, we used this method most often.)

The chief methodological problem in the factor analysis is Adams and Bumb's use of twenty-five variables and only twenty-six districts. Factor analysis is based on a correlation matrix (presented in Appendix A of the article), which is then analyzed to show the underlying "factors" that help explain the intercorrelations

[1] Conklin is entirely responsible for the methodological critique, which he prepared in greater detail than is outlined here. Adams and Bumb's matrix was reanalyzed, using a variety of factor techniques, and several analogous data sets were substituted to see how unique the published solution was.

[2] All attempts by Conklin to recalculate factor scores were unsuccessful due to the ill-conditioned matrix. Available programs showed factor scores that were indeterminant. Even the factor solution had to be computed using default routines.

of clusters of the variables. Unfortunately, when the number of cases exceeds the number of variables by only a few (and in this case by only one), the result is an ill-conditioned matrix which is close to being a singular matrix. In a severely ill-conditioned, or "too few cases", matrix, computational errors (among other things) will result in the discovery of four or so dominant factors almost in spite of the data being used. Indeed, when a matrix for Mysore State was created using Mysore's nineteen districts and eighteen variables similar to those used by the authors, four factors also emerged which accounted for the identical amount of explained variance as did Adams and Bumb's solution for Rajasthan. Even when a matrix of completely random numbers was used in place of "real" data, four factors emerged which explained forty percent of the variance. Thus the continued reappearance of four dominant factors which Adams and Bumb believe to be a major research finding is more an artifact of the method than of the real-world situation which they have set out to clarify. Researchers who attempt to replicate the findings of Adams and Bumb in other sections of India should be aware that the continued emergence of four dominant factors in other states could represent errors of methodology and not uniformities in various sections of India. Even the calculation of factor scores, if possible at all, would be subject to extreme errors due to the impossibly small determinant of the correlation matrix.

These methodological limitations may indeed go far to explain a number of problems of interpretation in the article. In several instances the authors have offered rather far-fetched explanations rather than questioning the serious problems with their data. Instead of detailing large numbers of small points at which the interpretation may be questioned, we will make three major points and illustrate each with an example.

First, some of the apparent anomalies in the results may be due to the data itself. Although all readers will sympathize with the difficulties of gathering accurate, comparable data in India, some problems could surely have been avoided with a bit more awareness of the situation actually prevailing. For example, the authors attach considerable importance to the number of workers employed in various categories—especially agriculture, and household industry—taken from the Census of India, 1961. Previous work with the census has indicated serious problems with the occupational data, both in definition and in data collection.[3] Yet this data forms the basis of six of the twenty-five indicators.

The authors also fail to take account of important changes that have occurred between 1961, the year of the census, and 1966–1967, the base year for much of their other data. An important example is the rapid growth in population in Ganganagar District between 1961, when whole square miles were uninhabited, and the middle of the decade; the 1971 Census of India shows a growth rate of over thirty-seven percent for the period 1961–1971. With only twenty-six cases, distortion of data from even one case can result in spurious correlations. Similarly, to associate pump use in 1965 with the level of agricultural credit in 1966 is problematic when 3102 electric pumps alone were installed during that year, an increase of over eighty percent.

[3] See, for earlier censuses, Daniel and Alice Thorner, "Economic Concepts in the Census of India 1951," in *Land and Labor in India* (London, 1962), the comments in which also apply to 1961. Also, Ashish Bose, "Six Decades of Urbanization in India," *The Indian Economic and Social History Review*, Vol. II, No. 1, January, 1965.

The second general problem of the analysis lies in the curious distinction propounded between two types of agriculture: one characterized by high production and the use of irrigation, fertilizer, and tractors; the other a rainfed agriculture using pumps, with relatively large supplies of credit and a concentration on commercial crops. The separation of fertilizer from pumps and commercial crops is very strange indeed, because fertilizer is of course part of the whole package of new agricultural inputs that includes pumps and improved seeds as well. In fact, the installation of a pump *without* the concomitant use of fertilizer would almost negate a peasant's investment: similarly, most commercial crops require the entire package to be profitable. Even minimal field experience suggests that the inputs are closely linked rather than divided into two distinct usage patterns. In addition, it should be noted that because of better communications and the demonstration effect, advanced agricultural usages occur frequently, although not exclusively, near urban areas. This is not indicated in any of the factors.

This leads to the last point, which pertains to the relationship between economic and political variables. The authors are rightly pleased that each of their four factors contains economic, political, and demographic variables, suggesting that none of these is independent of the others. However, their factors do not do justice to the complex interactions of all these variables and are therefore misleading in some important aspects. This is at least partly due to the use of percentage of electorate voting as the exclusive measure of political participation. For example, Adams and Bumb find no association between fertilizer-based agriculture and political participation, but the nexus between the two is very close, as Blue and others have shown. Peasants simply participate in ways unrelated to voting.[4] On the other hand, the authors do find a high degree of association between political participation and percentage of agricultural employment, which is interpreted as showing that farmers do use the political process for acquiring benefits. This is but one of numerous instances in which characteristics of the whole are imputed to individuals—an interpretive problem known as the ecological fallacy.

The distributive aspects of politics have also escaped the authors' attention. For example, Udaipur district ranks very high on factor 4, ecological base, which includes roads per unit area. Roads are of course a crucial component of development, especially of commercial agriculture which needs a means for shipping its products to market. However, most roads in Rajasthan are constructed as part of the famine relief program, and famine relief is distributed on an almost purely political basis. Thus Udaipur, the home district of Rajasthan's Chief Minister for eighteen years, received more famine relief projects than the rest of the state during the 1960's.[5] Similarly, agricultural credit is not distributed according to the number of cultivated hectares in a district, or even its population as the authors suggest, but on other criteria that include the power of the district's politicians.

[4] Richard Blue and Yashwant Junghare, "Political and Social Factors Associated with the Public Allocation of Agricultural Inputs in a Green Revolution Area: The Case of Rajasthan," Monograph, Center for Comparative Studies in Technological Development and Social Change, University of Minnesota, Minneapolis, 1973. See also other work of Blue, and Brian W. Coyer, "The Politics of Agricultural Policy Distribution in Rajasthan, 1961–1971: The Socio-Economic Contexts of Party Strategy and Development," unpublished Ph.D. thesis, Michigan State University, 1974.

[5] See data in Susan G. Hadden, "The Political Economy of Agricultural Policy: Rural Electrification in Rajasthan, India," unpublished Ph.D. dissertation, University of Chicago, 1972, page 121.

Thus Adams and Bumb's analysis falters because of their use of an ill-conditioned matrix, inexact data, and some misleading indicators. These problems then lead to some curious findings which Adams and Bumb have trouble explaining. Herbert Blalock, a prominent writer on social statistics, has said that factor analysis "should be used as a tool which may possibly contribute to the clarification of theory, but it cannot be expected to serve as a substitute for sound theoretical thinking".[6] Adams and Bumb have no real theoretical framework, except perhaps the negative one of criticizing the grouping of districts on a purely geographical basis. The factors were the result of a fishing expedition, and would probably have been more helpful had they been chosen in order to demonstrate, for example, that agricultural development policy has been directed towards the already advanced areas.[7] This approach would also have had the advantage of minimizing the numbers of variables used, thus reducing the matrix problem discussed above. While the type of analysis used by Adams and Bumb can prove a helpful tool in the understanding of patterns of development, problems of data collection and interpretation suggest that either smaller-scale or larger-scale studies may be more helpful for the present. There are many ways to analyze "small" samples of the type discussed here. Factor analysis is not one of the best.

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[6] Herbert Blalock, Jr., *Social Statistics* (New Jersey: McGraw-Hill Book Co., Inc., 1972) page 389.

[7] For example, family size is notorious for correlating only with the sex ratio. Instead of using the crude measure of family size, Adams and Bumb should have used the household composition data which is available for 1961. The key to understanding family or household variations by district lies not in Moslem–Hindu differences offered by the authors, but in migration patterns into the irrigated areas of the state. Migration is not mentioned by the authors.

REPLY

In writing our article on Rajasthan[1] we were deliberately conservative and cautious in our choice of statistical procedures, data base, and interpretative argument. It was, after all, our primary intention to demonstrate the utility of subjecting Indian microdata to orthodox multivariate techniques, not to probe the frontiers of statistical research. In consequence, we are somewhat bemused to find that our paper has prompted a wide-ranging critique, but are happy to clarify its method and content. After examining the many objections Conklin and Hadden (hereinafter C-H) make, we have concluded that whatever the faults of our study, they are not those identified by C-H.

The first criticism that C-H attempt to establish concerns statistical procedures. They argue that when the number of variables (twenty-five in our case) is permitted to approach the number of observations (twenty-six, one for each of Rajasthan's districts) several bad things happen: 1) an "ill-conditioned," nearly "singular" matrix will result, 2) the number of factors identified in a factor analysis will inevitably tend towards four, and 3) errors of interpretation become common. They provide no rigorous proofs or references to proofs of these propositions.[2] If accurate, these sweeping suggestions would have a major impact upon the core structure of factor analysis and cast a shadow over many applied studies other than our own.

We could find no definitive support for the first proposition in any standard work on factor analysis with which we are familiar.[3] It is, of course, well-known that computational problems (which are not unavoidable) arise when the number of observations used in a multivariate analysis equals the number of variables.[4] It is far less certain what takes place in solving linear systems, which must be done as part of factor analysis, when the number of cases and the number of variables are merely close to each other. The condition of the matrix and its effects on the derivation of the factor matrix and factor scores are dependent upon the character-

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[1] John Adams and Balu Bumb, "The Economic, Political, and Social Dimensions of an Indian State: A Factor Analysis of District Data for Rajasthan," *Journal of Asian Studies,* XXXIII (November, 1973), pp. 5–23.

We wish to thank Professor William Schafer of the Department of Measurement and Statistics, College of Education, University of Maryland, for his helpful discussions with us about factor analytic procedures.

[2] We wrote to Professor Conklin asking for the elaboration of his statistical argument mentioned in his first footnote, but it was not provided.

[3] Major texts include: Harry H. Harman, *Modern Factor Analysis,* second edition, revised (Chicago: University of Chicago Press, 1967); Paul Horst, *Factor Analysis of Data Matrices* (New York: Holt, Rinehart and Winston, 1965); D. N. Lawley and A. E. Maxwell, *Factor Analysis as a Statistical Method* (New York: American Elsevier, 1971); and, John P. Van de Geer, *Introduction to Multivariate Analysis for the Social Sciences* (San Francisco: W. H. Freeman, 1971).

[4] The practical difficulties of dealing with experiments where the number of cases and number of variables are similar have not been widely discussed. The problem does not usually arise in psychological studies since the number of observations can easily be expanded: Cross-section studies of nations, regions, or Indian districts will usually have to cope with small populations and large numbers of relevant variables. R. B. Cattell, a psychologist and authority on factor analysis, suggests that in such research the ratio of observations to variables may be allowed to approach 1:1 (in contrast to the ratio of 2:1 or more psychologists use as a lower bound), where the resulting factors have an intuitive or theoretical plausibility. See R. B. Cattell, ed., *Handbook of Multivariate Experimental Psychology* (Chicago: Rand McNally, 1966), pp. 237, 783.

istic values of the particular matrix, the method chosen to reduce the correlation matrix to the smaller factor matrix,[5] and the computational procedures built into the computer program being employed.[6] It is impossible to allege, as C-H do, that problems will necessarily arise in the course of a particular factor analysis experiment without knowing more about each of these matters.

C-H commit a common error when they suppose that the size of the determinant affects the condition of a matrix in a direct way. Forsythe and Moler say, "A popular misconception is that the smallness of [the determinant of a matrix] causes the ill-condition of [the matrix]. Our considerations show that this is not so."[7] In any event, our matrix was not singular since the number of variables was less than the number of observations. We applied the criterion for conditioning suggested by Forsythe and Moler to our original 25x25 correlation matrix and on different-sized variations of that matrix. We detected no problem. In practice, our computational procedure at no time threatened to break down and our factor composition and factor score groupings remained stable, allowing for the effects of the deleted or added variables.[8] It is not necessary to undertake a matrix inversion to compute factor scores, nor does the generation of the factor matrix, which is really the key result, depend upon inverting the correlation matrix.[9] In short, C-H commit a serious error by relying exclusively upon a simplistic rule of thumb based on a comparison of the number of variables and observations. They advance an injudicious assessment of the condition of our matrix and make some unsupported conjectures about difficulties in the computation of our results. The handling of small samples (in our case, a small population, the number of districts) does require care, but familiarity with the appropriate danger signals will provide a basis for determining the existence of genuine problems and ensuring the generation of meaningful results.[10]

[5] There are more than a half dozen common techniques for resolving a correlation matrix into factors and obtaining an acceptable rotated solution. See Harman, *op. cit.*, for a thorough discussion of their characteristics.

[6] It is likely that the difficulty C-H experienced in working with our correlation matrix (see their second footnote) should have been attributed to a problem intrinsic to their computer program rather than to some intractable feature of the matrix. It would be necessary to work through their computational routine in order to determine exactly why it experienced a breakdown.

[7] G. E. Forsythe and C. B. Moler, *Computed Solution of Linear Algebraic Systems* (Englewood Cliffs, N.J.: Prentice-Hall, 1967), p. 22. Generally, see Chs. 8 and 18. Also, see J. H. Wilkinson, *The Algebraic Eigenvalue Problem* (Oxford: Clarendon Press, 1965), pp. 196–7.

[8] We designed and executed a type of sensitivity test to see whether C-H's first proposition held in our case, for whatever cause. Beginning with a subset of fifteen variables, we added one variable at a time until we reconstructed the original matrix of twenty-five variables. At each step we conducted a new factor analysis and monitored the eigenvalues, the factor loadings, and the factor scores—in fact, all the output of each trial. These indicators would have revealed symptoms of distress or breakdown. None appeared. To place even greater weight on our computational technique we added sequentially five new variables so that the number of variables exceeded the number of observations by a margin of four. Even this extreme and theoretically unsound manipulation did not lead to a breakdown in our practical procedures.

[9] For a discussion of alternative means of obtaining factor scores, see Harman, *op cit.*, Ch. 16, and Lawley and Maxwell, *op. cit.*, Ch. 8.

[10] Once the number of variables equals or exceeds the number of cases (or if the correlation matrix contains a row or column that is a linear combination of another row or column, or of other rows and columns) the result is a singular matrix with a zero determinant. Depending on the choice of procedures, we believe, although we have no formal, general proof, that it will often prove feasible to work with the first few characteristic roots and the resulting factors and factor scores even when the numbers of variables and cases are close. We repeat that our original matrix was not singular or ill-conditioned and we mention this conjecture merely to suggest that one need not rule out working with such cases, although a great deal of caution and discretion should be used.

C-H next assert that there is an innate tendency for a four factor solution to result as the number of variables approaches the number of observations, apparently independently of the exact number of the latter. This point is completely lacking in theoretical or practical support.[11] In the original study we never claimed that the four factor matrix was unique or that other trials always yielded four factors. The critical point, with which anyone who uses factor analysis is familiar, is that there is no single accepted criterion for determining the proper number of factors. Several decision rules have been suggested, but none has found wide acceptance, leaving the ultimate decision in the hands of the individual experimenter. We could certainly have designed three or five factor resolutions of our total variable set that would have been statistically acceptable, but we chose the published version because it was the most interesting.[12]

When C-H speak of four factors "emerging" from their correlation matrix for Mysore (now Karnataka) or from a set of random numbers, they can do so only because they themselves predetermined that four and only four factors would be used in those trials. We will not discuss the various procedures that have been proposed to control the number of factors extracted from a correlation matrix. But, if the rules we adopted in the original paper are used, the C-H nonsense matrix of random numbers remains just that: nonsense. The proportion of variance explained is only forty percent, far too low for acceptability. What is more important, the ultimate test of the adequacy of a set of factors is their meaningfulness, individually and collectively, and random number factors would hardly satisfy this criterion.[13] We would be very surprised if parallel studies of other Indian states resulted in four factor resolutions, particularly ones identical with ours. C-H misrepresent what we said, for we argued only that our four factors appeared appropriate for Rajasthan in the mid-1960s, when the selected variables were used to characterize the state. There remains enormous scope for comparative work; and, indeed, we are preparing similar studies of Maharashtra and Karnataka.

In sum, the wide-ranging statistical principles C-H strive to establish are invalid because they are based upon a superficial appreciation of the techniques of factor analysis. We followed secure procedures that successfully reduce a large correlation matrix to a meaningful set of factors. Naturally, C-H were not able to show how "methodological errors" led to the interpretative "problems" they discuss in the second half of their comment.

The first substantive point C-H raise concerns our use of employment data from

[11] For clarification and elaboration, see Harman, *op. cit.*, pp. 94–109 and other discussion identified in his Contents and Index. In fact, presentations of factor analysis techniques always make exactly the opposite point. Any number of factors greater than zero and less than the number of variables could represent a satisfactory reduction of the correlation matrix.

[12] Applying a rule of stringent parsimony, we could have identified three or four factors with as few as eight to twelve selected variables. We instead retained as many social, political, and economic indicators as possible because we wanted to enrich the description of Rajasthan. We were interested not only in identifying factors, but in the complex relationships between indicators and factors and in factor scores.

[13] C-H also note that four factors accounted for seventy percent of the variance in their 18 variable, 19 observation Karnataka matrix, the same proportion of the variance our four factors explained. This appears to be sheer coincidence. When using the sixty-five percent of variance explained criterion as one control over the process of factor selection, many final outcomes will fall in the 65–75 percent range. That two separate analyses of different data sets explain the same amount of variance is hardly rare or even interesting.

the Census of India, 1961. Having worked with Indian research materials for many years, we were aware of the problems we faced in assembling our data base. We followed conservative procedures and used no variables that were presumptively invalid. We reread the article by the Thorners that C-H mention to see if we had overlooked or forgotten some relevant aspect of their discussion of census definitions and collection practices.[14] They were chiefly critical of the differentiation of workers within the agricultural sector, arguing that various types of subinfeudation were not reducible to the census categories. We were conscious of this problem and had responded to it by lumping everyone deriving his primary income from agriculture into the "agricultural work force," the concept we employed. We agree, too, that the distinctions among the agricultural work force, household industry workers, and those in industry and service categories may be slightly blurred, but we believe that variations in the data are adequate to capture broad occupational differences across districts. Furthermore, C-H apparently are not aware that a consistent error of, say, ten to twenty percent will not affect one variable's correlations with the other variables in a set. In other words, the correlation coefficients that are the basic input for factor analysis may not be biased even if the original raw data are. Naturally, one has to be judicious in taking advantage of this feature, since irregularities in estimation procedures across districts will render data useless for this purpose.

C-H practice a bit of deception when they next suggest that we ignored "important changes," across the years 1961 through 1966/67, the span of our data. They never specify exactly what these might be, but we had deliberately avoided this sort of difficulty by using no 1961 data that were likely to be substantially distorted five years later. The example they use, rapid population growth in Ganganagar district, gives a misleading impression because they refer to a change of thirty-seven percent in the *decade,* 1961–1971. From 1961 to 1966, the change was probably roughly half that, or about three and one-half percent per year. Since other districts were simultaneously experiencing population growth, the relative position of each district would change very little in just five years. As we just commented, such small effects are very unlikely to make a significant difference in a correlation matrix. Much the same point can be made about the credit-pump association.

The second objection to our interpretation concerns our treatment of agriculture. Much of what C-H say about the acceptance of packages of technical changes would probably have more relevance to the late 1960's and early 1970's than to the mid-1960's, when Rajasthan's farmers had not yet really had the opportunity to respond to new cultivation practices. The very positive and general assertions C-H advance, however, are unlikely to hold true at any time on a uniform basis for all farmers or regions, since allowance must be made, as is well known, for size of holding, for access to capital, and for ecological, political, administrative, and other factors in studying the diffusion of innovations. We were simply describing the broad patterns we found, and it seems plausible to us (however "curious" to C-H) that a state as big and complex as Rajasthan will have several recognizable types of

[14] The reference to Ashish Bose's article in the same footnote is apparently gratuitous, since he does not discuss the issue of using employment data. It is possible that C-H meant to cite the article by J. Krishnamurthy which follows immediately in the same issue of the *Indian Economic and Social History Review,* entitled "Secular Changes in the Occupational Structure of the Indian Union, 1901–1961," where census occupational data are used—but even more aggressively than we did.

agriculture. C-H misrepresent our position when they speak of "distinct usage patterns" in agriculture, since we stated expressly that the secondary loadings of several of our variables across factors two and four, explicitly irrigation and, more modestly, fertilizer use, meant that there was an observed *overlap* of practices, rather than a rigid demarcation of zones.

Finally, C-H object to our handling of the relationships between economic and political variables. We have no quarrel with the banal assertion that "peasants simply participate in ways unrelated to voting." But we were careful, within the limitations of our twenty-five variables, to assess the behavior of six (seven, with cooperative credit) political-administrative indicators (see Table 2 of the original paper). Our original argument is complex enough not to be easily summarized here, but in discussing the distribution of the three components of development spending, for example, we showed how different groups and areas received differential benefits. We related our findings on landownership and political participation and rural Congress Party support to the major published studies of Myron Weiner and Richard Sisson. Yet, C-H complain that we ignored two unpublished pieces written after our paper was completed! We commented in our original paper on the need for more comprehensive treatment of political-economic relationships in Rajasthan. The fairly low correlation coefficients and the low R^2s in the factor matrix leave a great deal of room for tighter framing of hypotheses, the introduction of new explanatory variables, such as "the power of the district's politicians" (if quantifiable), and more careful testing of relationships, the stimulation of which was the chief purpose of our exploratory effort. If new studies fill these gaps, we will be delighted to yield to their analyses.

Although we used three explicit political distributive variables (nos. 18, 19, and 20), C-H state that this aspect of government "escaped our attention." From this unfair beginning, they embark upon a plausible, but untested and essentially irrelevant argument that Udaipur district benefited extraordinarily, in the form of roads, during the tenure of Shri M.L. Sukhadia as Chief Minister. We are uncertain how much famine relief roadwork was completed in Udaipur district prior to 1966 (when Shri Sukhadia had been in office only twelve years, not eighteen as C-H imply). It does seem improbable that Udaipur received more roads "than the rest of the state" in the 1960s (Do they mean more "than any other district in the state?"). Fortunately, their proposition concerning ministerial intervention can be tested to determine its impact upon our analysis. In 1966, Udaipur, far from being an extreme observation, ranked with Bharatpur district behind Ajmer and Jaipur in roads per 100 square kilometers. When we dropped the transportation variable entirely from our data matrix, no change occurred in Udaipur's rank on factor four.[15] Udaipur's high factor score can clearly be attributed to the district's values on the other seven variables contained in that factor. There is nothing in our study that is sensitive to the participation of Chief Minister Shri Sukhadia in Udaipur's road development, however importantly his intervention might be weighted by other modes of testing the hypothesis.

[15] As a further test, we arbitrarily added 40 percent to Udaipur's road network in 1966 to see what would happen if the Chief Minister had been as effective in skewing the distribution of roads in Udaipur's favor as C-H claim. Again we recomputed factor scores and again we found that Udaipur remained in the same range as the other districts with which it was previously ranked.

C-H failed to recognize the tentative, pre-theoretical nature of our discussion, but even while treating our interpretation more rigidly than it deserved they were unable to challenge or extend it in useful ways. The critical quotation from Blalock in their final paragraph could not be traced to its original context because it was mis-cited.[16] We can do no better in justifying our use of factor analysis than to quote Thurstone:

> The exploratory nature of factor analysis is often not understood. Factor analysis has its principal usefulness at the border line of science. It is naturally superseded by rational formulations in terms of the science involved. Factor analysis is useful, especially in those domains where basic and fruitful concepts are essentially lacking and where crucial experiments have been difficult to conceive. The new methods have a humble role. They enable us to make only the crudest first map of a new domain. But if we have scientific intuition and sufficient ingenuity, the rough factorial map of a new domain will enable us to proceed beyond the exploratory factorial state. . . .[17]

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[16] Neither on page 389 or elsewhere in his *Social Statistics* could we find a discussion of factor analysis. We expect McGraw-Hill is confounded to find itself in New Jersey. And, Mrs. Blalock will no doubt be dismayed to learn her little boy has been rechristened Herbert.

[17] L. L. Thurstone, *Multiple Factor Analysis* (Chicago: University of Chicago Press, 1947), p. 56.