high. As soon as risk is lower, it moves to take whatever advantages are offered by the then not quite new ideas or practices.

Wealth and Adoption of Agricultural Innovations. The results suggest that while wealth may remain a very rough predictor of tendency to adopt new agricultural practices, its influence on adoption operates through a complex of intervening variables that are sometimes at cross purposes. Moreover, in the early stages of the introduction of a new practice, the results show that inclination to risk, which appears to be inversely related to wealth (for the middle ranks at least), may be as important a component in the decision to adopt as knowledge and wealth itself. This suggests that theories of risk-taking may be more important than theories of information diffusion for the study of the earliest stages of the process of adoption of new agricultural practices.

ISSUES IN THE ECOLOGICAL STUDY OF DELINQUENCY *

ROBERT A. GORDON

Johns Hopkins University

Starting with Lander, in 1954, several studies have debated whether delinquency is fundamentally more related to census tract variables indicative of socioeconomic status or to those indicative of anomie. All of these studies misused such multivariate procedures as partial correlation, multiple regression and factor analysis. In addition, these studies and others of a similar nature have been affected by serious artifacts stemming from the accepted practice of using indexes with mixed cutting-points, some of which are much more sensitive to the tails of their distributions than others. When all of these errors are taken into account, it turns out that the association between delinquency and socioeconomic status is quite unambiguously very strong.

EVER since its appearance in 1954, Lander's *Towards an Understanding of Juvenile Delinquency* has drawn much attention.[1] The major thesis of Lander's study, based upon multivariate analyses of ecological data, was that juvenile delinquency rates over a four-year period in the city of Baltimore were related in only a superficial sense to census tract variables indicative of socioeconomic status. Lander claimed to show that, in actuality, the juvenile delinquency rates in question were not related to socioeconomic status at all, but rather to the variables: percentage of homes owner-occupied, and percentage nonwhite. Since these latter variables seemed to him to be more identifiable with degrees of social integration than with degrees of socioeconomic status, Lander was led to conclude that his data favored an "anomie theory" explanation of delinquency rather than one based upon some kind of economic determinism.

Both types of theory have a long tradition in sociology, and both have their special adherents. It was only natural that the overall reaction to Lander's study be one of ambivalence. On the one hand, the study appeared to support the existence of the more elusive and therefore more glamorous variable, anomie. On the other hand, it denied a relation with the most concrete and most solidly established of all sociological variables, namely, socioeconomic status. This denial ran coun-

* Work on this paper was supported by Research Grant MH 10698–01, from the National Institute of Mental Health. The present version was part of a longer paper, "Issues in Multiple Regression and the Ecological Study of Delinquency" (Department of Social Relations, Johns Hopkins University, 1966), the remainder of which will appear elsewhere under the title "Issues in Multiple Regression." Readers who are interested in a more detailed discussion of the methodological problems of regression analysis touched upon here are referred to this other paper.

[1] Bernard Lander, *Towards an Understanding of Juvenile Delinquency,* New York: Columbia University Press, 1954.

ter both to much statistical evidence and to intuition, and thus placed in doubt one of the few strong relations with delinquency that sociologists had been able to identify. As a result, sociologists have been at once fascinated with and suspicious of Lander's conclusion.

The two most ambitious re-examinations of Lander's findings to appear thus far have been by Bordua and by Chilton.[2] Bordua, employing data for Detroit, raised a number of questions concerning the original study, in the course of attempting to replicate some of Lander's analyses. Somewhat cautiously, he concluded that the Lander interpretations were essentially confirmed for Detroit. Chilton incorporated both Lander's and Bordua's data into an almost total replication, adding data for a third city, Indianapolis. On the basis of his analysis, he severely questioned the utility of Lander's anomie explanation. His main criticism, and potentially the most damaging one, was that Lander had confused the signs of the factor loadings of four of his variables, and that, as a result, his factor analysis could no longer support the interpretation that his variables gave rise to an anomie factor and a socioeconomic factor, with delinquency being more closely related to the anomie factor. This criticism alone would probably have been sufficient to discredit Lander's theory and to remove whatever doubt that theory had raised concerning the proposition that delinquency and socioeconomic status were related. At a time when resources are being committed on an unprecedented scale against poverty, partly on the justification that social ills such as crime and delinquency have a socioeconomic basis, it is certainly important that sociologists be correct about the facts of this particular relationship. Unfortunately Chilton's criticism of Lander on this point, and on other points as well, is mistaken. However, there are other important faults in Lander's procedures that completely invalidate his conclusions. The purpose of this paper, therefore, is to describe these mistakes, and others appearing in the studies by Bordua and Chilton, so that this particular erroneous challenge to the hypothesis of a relationship between delinquency and socioeconomic status may finally be laid to rest.

[2] David J. Bordua, "Juvenile Delinquency and 'Anomie': An Attempt at Replication," *Social Problems,* 6 (1958–1959), pp. 230–238; Roland J. Chilton, "Continuity in Delinquency Area Research: A Comparison of Studies for Baltimore, Detroit, and Indianapolis," *American Sociological Review,* 29 (February, 1964), pp. 71–83. Somewhat related papers are those by Kenneth Polk, "Juvenile Delinquency and Social Areas," *Social Problems,* 5 (1957–58), pp. 214–217; Bernard L. Bloom, "A Census Tract Analysis of Socially Deviant Behaviors," *Multivariate Behavioral Research,* 1 (1966), pp. 307–320; and Desmond S. Cartwright and Kenneth I. Howard, "Multivariate Analysis of Gang Delinquency: I. Ecologic Influences," *Multivariate Behavioral Research,* 1 (1966), pp. 321–371.

LANDER'S FACTOR ANALYSIS

Although not first in order of appearance in Lander's study, it is convenient to discuss his factor analysis before the other analyses. He performed a centroid factor analysis of seven census variables and the juvenile delinquency rate for census tracts in Baltimore. The census data were obtained from the 1940 census, and the delinquency data were for the years 1939 to 1942. From the resulting correlation matrix, he extracted two factors by the centroid method. These two factors, at this point, were unrotated and orthogonal. Lander gave no indication that he even considered an orthogonal rotation; instead, he apparently proceeded directly to an oblique solution that was no doubt intended to pass one of the two factor axes through the center of each of the two major clusters of variables in his plot. However, this is not what actually occurred. To understand what went wrong, we must refer to Table 1, which is adapted from Lander's Table XII, on page 53 of his book, and to Figure 1, which is equivalent (except for certain additions) to his Graph I, on his page 54.

Since Lander did not indicate the oblique factors on his plot, we must conjecture to some degree how he intended to rotate his factors. If one were of the opinion that an oblique solution was desirable for these data, it seems logical that one would aim to pass vectors near or through the two major clusters in Figure 1. In attempting to understand Lander's results, we ourselves drew such lines on his plot, and then later found each of their angles of rotation to be within one degree of one or the other of the angles of rotation indicated by the sine and cosine

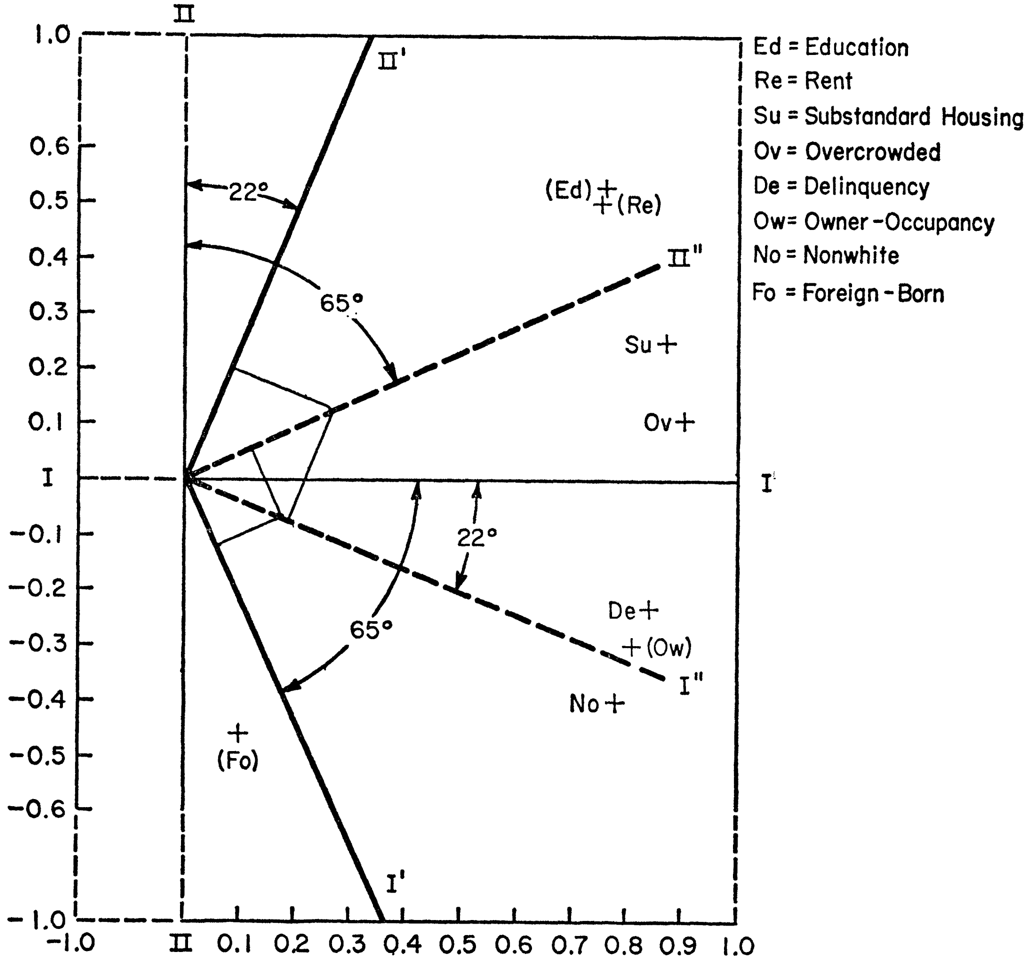

FIGURE 1. LANDER'S ACTUAL OBLIQUE FACTORS (SOLID OBLIQUE LINES I′ AND II′) COMPARED WITH HIS INTENDED OBLIQUE FACTORS (BROKEN OBLIQUE LINES I″ AND II″) (Variables In Parentheses Have Been Reflected.)

TABLE 1. LANDER'S FACTOR ANALYSIS, WITH CORRECT OBLIQUE SOLUTION

	Lander's Centroid Solution		Lander's Oblique Solution		Correct Oblique Solution	
Variable	I	II	"Anomie"	"SES"	I	II
Delinquency rate	.84	—.23	.56	.11	.87	.67
Low education	.76	.53	—.16	.78	.51	.92
Low rent	.75	.50	—.14	.75	.51	.90
Overcrowding	.90	.11	.28	.44	.80	.86
Substandard housing	.87	.25	.14	.56	.71	.90
Low owner occup.	.81	—.30	.62	.03	.86	.61
Nonwhite	.78	—.40	.70	—.08	.88	.54
Few foreign-born	.10	—.47	.47	—.40	.27	—.10
Transformation						
matrices for each	...	...	.423	.375	.927	.906
oblique solution	...	...	—.906	.927	—.375	.423

values of his transformation matrix. This suggests that his choice of oblique solution was also ours. To further confirm that this choice was also Lander's natural choice, we submitted unidentified plots, on irregularly trimmed paper, to two experienced factor analysts, with the request that they draw the oblique solutions they would recommend.[3] The plots they were shown displayed no axes at all, merely eight unnamed variables located with respect to each other and to an origin, with the length of a unit vector indicated. Both offered oblique solutions that came within a few degrees of our reconstruction of Lander's intentions, thus upholding the reasonableness of our conjecture.

It is important to establish what must have been his intention here, because his transformation matrix does not yield the solution toward which we surmise that he was aiming. To obtain that solution, it would have been necessary to rotate his centroid factor I 65 degrees, and his centroid factor II 22 degrees. Unfortunately, the transformation matrix he employed—which does yield the values of his oblique factor matrix—accomplished the wrong rotation; the angles of rotation were the same, but they applied to the wrong factors. (Note the interchange of rows and columns in the two transformation matrices in Table 1.) It was a peculiar property of this mistake that gave rise to the erroneous interpretation Lander placed upon the resultant oblique factor structure. Because he wanted to rotate centroid I 22 degrees in order to maximize the loadings of one cluster on I, but instead rotated centroid II 22 degrees in the same direction, and because centroid II was orthogonal to (90 degrees from) centroid I to begin with, he inadvertently created an oblique factor II that was exactly orthogonal to (90 degrees from) his target factor or, in other words, one that would *minimize* the loadings of that factor's cluster of variables on oblique factor II. For the same reasons, an identical outcome was obtained for oblique factor I, with the result that rather than maximizing the loadings of its target cluster of variables, each oblique factor *minimizes* the loadings of the *other* cluster. This accounts for the fact that the socioeconomic variables of median rent and median education show negligible loadings on the oblique anomie factor and, especially, that the delinquency rate has a negligible loading on the oblique socioeconomic factor. In the case of a rotational solution with these properties, only variables that lie distant from the center of their cluster (and at the same distance from the origin as the cluster itself) can possibly have substantial loadings on both factors simultaneously (if we may be permitted an overly simple but essentially correct characterization). In Lander's data, there were no such variables.

However unlikely, it is still possible that Lander actually intended the oblique solution be achieved. In this event, we would have to challenge the principles underlying such a solution. This is quite easy to do. The rotational criterion implicit in his actual procedure amounts to determining a rotation by setting in advance what a given factor *is not* (what variables should not load on it); what it is would then turn out to be a composite of whatever variables remain. The sterility of this procedure may be grasped immediately by imaging what the outcome would be if it were applied to the case in which there was but a single cluster. In this event the factor would be aimed at nothing. That such a criterion would represent a radical departure from accepted principles is witnessed by the consensus among the three factor analysts (including ourselves) who independently rotated Lander's factors, and by authoritative texts.[4] Furthermore, Lander appears to have understood that the oblique solution he was aiming at would produce two positively correlated oblique factors, because he twice represents this correlation—correct in absolute value—as being positive.[5] In actuality, the two oblique factors of his solution are negatively correlated, with a value of —0.68.

Thinking that the correlation between his oblique factors was strongly positive in combination with the factor structure derived from a solution in which their correlation was instead strongly negative, Lander was

[3] We are grateful for the help of Kenneth I. Howard and Jack Sawyer in this task.

[4] See, for example, Harry H. Harman, *Modern Factor Analysis,* Chicago: University of Chicago Press, 1960, p. 265.

[5] Lander, *op. cit.*, pp. 53 and 59.

TABLE 2. ZERO-ORDER AND SIXTH-ORDER PARTIAL CORRELATIONS BETWEEN DELINQUENCY AND INDEPENDENT VARIABLES, AND MULTIPLE CORRELATIONS WITH OTHER INDEPENDENT VARIABLES

Variable	Zero-Order	Lander's Sixth-Order	Correct Sixth-Order	Multiple Correlation With Other Six	Squared Multiple Correlation
Median education	−.51	.0055	−.06	.93	.87
Median rent	−.53	.0003	−.02	.92	.84
Overcrowded	.73	.0079	.10	.90	.81
Substandard housing	.69	.0052	.08	.90	.81
Owner occupancy	−.80	.1764	−.43[a]	.87	.75
Nonwhite	.70	.0086	.08	.86	.73
Foreign born	−.16	.0213	−.15	.47	.22

[a] Of the partial correlations, only the one for owner occupancy is significant: $p<.001$.

led to conclude that the factor loadings represented a more fundamental aspect of his data than the strong zero-order correlations observed between delinquency and the socioeconomic variables, and that these more superficial zero-order relations were produced, and thereby accounted for, by the presumed positive correlation between the factors. Thus Lander stated:

> The factor analysis clearly demonstrates that delinquency in Baltimore is fundamentally related to the *stability* or *anomie* of an area and is not a function of nor is it basically associated with the economic characteristics of an area.
>
> . . . The correlation between the *anomic* and the *socioeconomic* factors is, as one would expect from the inspection of graph I, high (.684). It provides an explanation of the fact that delinquency is so highly correlated with the socioeconomic properties of a tract. The association between the factors however is statistical.[6]

This interpretation is, of course, completely erroneous. Had he employed the correct transformation matrix, he would have obtained the loadings of the variables on two oblique factors that were substantially positively correlated in fact and, as is usually the case in such circumstances, his variables, including the juvenile delinquency rate, would have displayed high loadings on both factors. In addition, it would have been obvious that both factors were highly saturated with SES, and his anomie explanation would not have arisen. This can be seen in Table 1, which presents the correct transformation matrix and the oblique factor structure it produces, and in Figure 1, where the correct oblique factors are indicated by broken lines.

[6] *Ibid.*, p. 59.

In our opinion, because of the high correlation between its factors, the oblique solution adds little to our understanding of this particular set of data, but that is beside the point—had he obtained this solution, Lander would not have been misled.

We postpone consideration of a possible orthogonal rotation until after our discussion of Chilton's article.

THE PARTIALLING FALLACY

Another of Lander's analyses consisted of obtaining the sixth-order partial correlation coefficients between the delinquency rate and each census variable in turn, holding the remaining census variables "constant."[7] Table 2 presents Lander's sixth-order partials, together with the zero-order correlations, of each variable with delinquency. His partials seem to have been subject to computational errors; Table 2 also provides recalculated partials, derived from his matrix of correlations.[8] It is clear at a glance that

[7] *Ibid.*, p. 46.

[8] With the exception of those involving delinquency, these zero-order correlations were themselves checked, starting with the original census data. Only minor discrepancies were found; for the sake of comparability, Lander's values were retained throughout this paper, unless otherwise stated. The delinquency rate data could be only approximated, however, by four class intervals, read from Map I in Lander's Appendix D with the aid of a magnifying glass. The resulting correlations with delinquency were close enough to Lander's to permit using the values that he calculated with reasonable security, especially since the remaining correlations had all been verified.

One strong indication that the partial correlations were in error was the fact that four of them display reversals of sign. Although it is possible to reverse the sign of a zero-order relationship in even the corrected partials are almost all much lower than the zero-order correlations, so low in fact as to be not significantly different from zero in all but one case. Lander went on to introduce curvilinear components for four of the variables, in effect creating four new variables, and when these four were also incorporated into the partialling operation, he obtained a new set of correlations that were now tenth-order partials; however, out of respect for their violation of the assumption of linearity of regression, these new correlations were called "indices of partial correlation." Among these tenth-order partials, the two highest appear for the variables percentage nonwhite and percentage of homes owner-occupied. Lander drew the following conclusion from these results:

> In the zero order correlation table, the juvenile delinquency rate is highly correlated with substandard housing and with residential overcrowding. In the partial correlation analysis, when the influence of other vairables studied is eliminated, instead of positive correlations between these variables and delinquency of r=+.69 and +.73, we have derived coefficients of partial correlations of .0052 and .0079 as describing the *real* relationship between these variables and delinquency; and when adjustment is made for the curvilinearity of the data the partial correlations are reduced in both instances to .0000. [Actually, in his table, the correlation for homes overcrowded rises to .0090, but the point is trivial]. This indicates that, despite the high correlation coefficients, there is no substantive relationship between these two variables and delinquency when all other factors are held constant and their influence eliminated. We also cite the presented data to emphasize the danger of attaching great importance to interpretations based on zero order correlation analysis.[9]

We have here one of the clearer examples of an error that is not rare in sociology. The introduction of a control variable into a relationship implies the existence of a theoretical context, although in practice the context itself is often left unspecified. When experienced researchers fail to state the theoretical context explicitly, it is because they feel that it is sufficiently obvious to the reader. Often they are right. Some researchers, however, have been misled by this silence, and they are unaware of how necessary it is to be conscious of the theoretical implications underlying any partialling operation. As though to emphasize this aspect of the process, Kendall and Lazarsfeld, their classic exposition of the logic of partialling operations with categoric data, referred to the control variable as the "test" variable—clearly, a hypothesis is to be tested, and a hypothesis implies the existence of a theoretical context.[10] Some researchers, however, engage in what is actually atheoretical partialling, as though the only hypothesis to be tested were the purely statistical one of whether the zero-order relationship could survive the application of any conceivable control. The object, of course, is not simply to destroy an observed relationship, but rather to see whether it can be destroyed using as a control a variable that has been hypothesized to be potentially relevant and conceptually distinct within the theoretical context in which one is operating. However, without a theory, there is no way of telling what is conceptually distinct and what is not. Consequently, variables are often introduced as controls that are not meaningfully different in terms of what would constitute an appropriate theory. These variables so closely approach being identical with one of the variables already in the zero-order relationship that controlling for them becomes tantamount to partialling that relationship out of itself. This is exactly what Lander has done by taking a series of variables, many of which are important indicators of SES, and partialling all combinations of n-1 of them out of the relationship of the nth with delinquency. Under the circumstances, controlling for any

partialling, this is extremely rare in practice, since it requires an unlikely combination of correlations. For this to have happened not once but four times, as his figures seem to indicate, is most improbable. By the same reasoning, it is unlikely that all of the unreversed partials would be as close to zero as his figures show, because this places them on the threshold of a reversal of sign. Just as it is difficult to find data that would produce a sign reversal, it is difficult to find data that would entirely obliterate a strong zero-order relationship.

[9] Lander, *op. cit.*, pp. 46–47. We have not checked the values of his tenth-order partial correlations.

[10] Patricia L. Kendall and Paul F. Lazarsfeld, "Problems of Survey Analysis," Robert K. Merton and Paul F. Lazarsfeld, eds., *Continuities in Social Research; Studies in the Scope and Method of "The American Soldier,"* Glencoe, Ill.: Free Press, 1950, 133–196.

one of them would be a mistake; controlling for them all approximates—in view of the high multiple correlation that must obtain between any one of them and all of the rest—using as a control a variable that is almost perfectly correlated with at least one of the two in the zero-order relationship. (For the multiple correlation of each of his independent variables with the rest, see Table 2.) In view of this, it is not surprising that the partial correlation coefficients in Lander's analysis turn out to be so small.

In reports of sociological research, it is not uncommon to find presented all of the possible highest-order partial correlations between each of a set of independent variables and the same dependent variable. Apparently, this practice also draws its inspiration from the Kendall and Lazarsfeld paper. However, the procedures advocated there are quite different in their logic. All of them *assume* knowledge concerning the presumed causal pirority of the variables; they were never intended to provide that knowledge. Roughly, they ask, "Is variable A causally prior to B, or is it irrevelant?" and not, "Is variable A causally prior to B or is B causally prior to A?" Yet it appears to be the latter question that researchers are addressing when they calculate all possible partials to see which variable will emerge with the largest partial. There is nothing in the Kendall and Lazarsfeld paper to justify using each independent variable in turn as the test variable for each other independent variable. For one thing, the outcome of such a procedure is strongly influenced by small sampling or measurement errors when the independent variables are themselves highly correlated.[11] Moreover, not all covariation is necessarily spurious. Controlling for valid covariation makes as much sense as controlling for a parallel form of the same measuring instrument. Presenting all possible highest-order partials is a sure indication that the researcher has not thought through the theoretical connections among his variables. Once committed to such a mechanical procedure, he is quite apt to control for variables whose covariation is largely valid. Finally, there is no rule for attributing controlled covariation to the influence of one rather than another of the independent variables, regardless of the disparity in size between their partial correlations. Although the temptation to commit the partialling fallacy is greater in the case of continuous data—where it is more convenient to obtain partials of a high order—it must be emphasized that all control procedures are equally susceptible, including those for categoric data and for experiments.

An important property of this procedure of obtaining all possible highest-order partials is that the variables emerging with the largest partials will be those that are least redundantly represented in the set. Conceivably, these could even be the variables that show the poorest zero-order associations (although this happens not to be the case in Lander's analysis). In Lander's case, racial composition and owner occupancy were less redundantly represented than variables that were good indicators of SES and, as a result, the better indicators of SES wiped each other out. Inasmuch as racial composition and owner occupancy were also the highest-loading variables, along with delinquency, on his supposed anomie factor, their higher partials in the present analysis must have strongly reinforced Lander's interpretation of his mistaken oblique rotation.

One could argue, of course, that median education *is* different from median rent and that it is reasonable to examine the relationship between either variable and delinquency free of the effects of the other variable. This is true as far as it goes, but it implies a theoretical focus that is much narrower and much more highly specialized than the one with which Lander was properly concerned. This smaller question should not be confused with a hypothesis concerning the relationship between SES and delinquency when one has two or more equally valid indicators of SES. For example, one might wish to inspect for some reason the partial correlations between quantitative ability and verbal ability on the one hand and academic achievement on the other, but this would be a poor way to test whether ability in general is related to academic achievement. Clearly, the partialling operation implies distinctness between the

[11] For an excellent discussion of this point, see H. M. Blalock, Jr., "Correlated Independent Variables: The Problem of Multicollinearity," *Social Forces*, 42 (December, 1963), pp. 233–237.

control and zero-order variables, although there are different levels of distinctness. If the theoretical context is left implicit, the investigator may find that he has committed himself to a theory—or a level of distinctness—that he did not intend and that he would not support upon deeper consideration.

It is important to note that it is only the unusual explicitness with which Lander declared his intentions that enables us to criticize his methods so confidently. All too often, investigators are so unclear in their own minds as to why they are partialling that it is impossible to determine their intended level of distinctness. In this way they enjoy the methodological security of the microscopic level of distinctness, in that one is always entitled to examine a partial if he wishes, while leaving their readers with impressions concerning the macroscopic level. Should one call attention to their indiscriminate partialling, they are apt to find themselves suddenly convinced that they had intended the narrower focus all along. Potential critics are naturally unwilling to take a stand when the question of whether there is even an issue is itself so slippery. As a result, sociology that is conceptually blurred accumulates, unchallenged, in the literature.

The reasoning underlying the partialling fallacy is reduced to the absurd, incidentally, when we realize that one could calculate all possible highest-order partials between the variables of a highly interrelated set and erroneously conclude, when the low partials fail to be significantly different from zero, that none of them was related to any other one.

LANDER'S MULTIPLE REGRESSION ANALYSIS

Lander also performed several multiple regression analyses. The first of these employed all seven census variables as linear predictors of delinquency. This analysis was immediately rejected, however, in favor of a second that incorporated, in addition, curvilinear (quadratic) components for four of the variables.[12] His purpose in these analyses was not simply to see how well he could predict the dependent variable, but also to examine the standardized regression coefficients as indicators of the relative importance of the variables. He next tested the second set of regression coefficients for statistical significance, and found that only four were significant. These four were the coefficients of the linear and quadratic components of the same two variables that had stood out from the rest in the partial correlation study: percentage nonwhite and percentage of homes owner-occupied. At this point Lander concluded, "Of all the variables studied, only the percentage of homes owner-occupied and percentage of Negroes in an area are fundamentally related to the delinquency rate and can be characterized as statistically significant predicting variables" [13] He then introduced just these two predictors into a third, multiple curvilinear, analysis, in which all of the coefficients were statistically significant.[14] Thus, yet a third time, his analysis conveyed the impression that the delinquency rate was associated with these particular two variables rather than with the variables that were more obviously socioeconomic in nature.

This type of analysis can lead easily to other, perhaps more insidious, versions of the partialling fallacy. We have indicated that to the degree the variables of a set are highly interrelated or numerous and conceptually similar, we approach being able to produce a partial correlation coefficient of zero between any two of them by controlling for the rest. Similar circumstances affect the partial regression coefficient in nearly the same way. This comes about in the following manner.

As redundant independent variables are successively introduced into a regression problem, their common predictive value gets averaged, in a weighted manner, over all of their regression coefficients. As a result, all of their regression coefficients decline in absolute value. At the same time, the multiple correlation increases only a trivial amount with each new variable, reflecting the fact that little new information is being added. That the multiple correlation cannot decrease indicates that the common predictive value is conserved, although it does get

[12] Lander, *op. cit.*, p. 48.

[13] *Ibid.*, p. 62.

[14] *Ibid.*

spread out over more and more regression coefficients, each becoming smaller and smaller as new redundant variables are fed into the problem.

Continuing with our examination of the regression coefficient, we note that if at any point a new variable is added that is uncorrelated with previous independent variables, then the regression coefficients of the previous variables will be unaffected. Of course, it would be possible then to add more variables that are redundant with respect to this new variable, but not redundant with respect to the earlier set, so that the regression coefficient of the new variable is reduced, but not those of the earlier variables.

The argument developed above helps us to realize that among the independent variables there could occur two or more subsets of variables, the members of which were redundant (strongly correlated) with variables in the same subset, but relatively independent (weakly correlated) with respect to variables in other subsets. It becomes immediately apparent that, under these circumstances, the relative size of a variable's regression coefficient depends to a considerable extent upon the *number* of other variables in its subset. If all variables were redundant to the same degree with others in their subset, unrelated to the same degree with variables in other subsets, and all were equally related to the dependent variable, then differences in the sizes of the regression coefficients between the variables of one subset and those of another subset would depend entirely upon the relative numbers of variables in the two subsets. These conditions are, of course, quite special, but they serve to bring into sharp relief processes that operate as well in the analysis of real data.

A subset, for example, can be thought of as representing a particular domain of content (or an underlying factor). In the case of Lander's data, the two domains of content can be partitioned into two major subsets, one containing the four SES variables and the other the two anomie variables. (The remaining variable, foreign born, falls outside of both clusters, and it may be ignored for our purposes, since its correlation with delinquency is too low for it to act as an important subset composed of but a single variable.) Within the SES subset, the average absolute correlation is .77; within the anomie subset it is .76. Although the four variables of the SES subset have lower correlations with delinquency, on the average, than the two anomie variables, thus departing slightly from our ideal example, it is nevertheless true that the potential importance of the SES variables is completely obscured by the fact that there are so many of them. This is borne out by the fact that each of the SES variables, when included by itself in a regression analysis with either just the linear or both the linear and quadratic components of the two anomie variables, yields a regression coefficient that is significant at the .001 level. Thus, it is the *number* of SES variables, rather than the superiority of the anomie variables, that causes the regression coefficients of the former not to be significant in Lander's analysis. The supposed "importance" of variables thus turns out to be inversely related to the frequency with which their domain has been sampled.

By the same logic, we can understand what is wrong with Bordua's attempt (repeated by Chilton) to test Lander's hypothesis by adding to the regression two new variables, median income and an index for unrelated individuals, deemed to be representative of each type of factor, SES and anomie, respectively.[15] Bordua reasoned that if Lander were correct, the regression coefficient of the new anomie variable would be larger than the regression coefficient of the new SES variable or, possibly, the first would be significant and the second not. However, if these new variables were truly typical of their respective domains, the outcome could not possibly be otherwise. If one pie is to be divided among a larger number and another pie among a smaller number, no matter how often we add one to the number for each pie, it will never alter the fact that the portions from the first pie will be smaller than those from the second.

THE CONSTRUCT VALIDITY OF ANOMIE

Campbell and Fiske have proposed certain criteria to be satisfied whenever it is claimed that a particular set of measurements repre-

---

[15] Bordua, *op. cit.*, pp. 232–235; Chilton, *op. cit.*, pp. 74–75.

sents a particular theoretical construct.[16] Among these is the simple but powerful requirement that different measurements of the same construct correlate more highly with one another than with measurements of alternative constructs.

If one studies the correlation matrices for all three sets of data—Lander's, Bordua's and Chilton's—it can be seen that the putative anomie variable (nonwhite and homes owner-occupied) do not constitute a genuine construct in terms of this criterion, although the SES variables do.[17] For Detroit and Indianapolis, the anomie variables split apart, in that their highest correlations are not with each other. For Detroit, the variable most correlated with nonwhite is foreign born (—0.73), and with homes owner-occupied it is substandard housing (—0.64); for Indianapolis, nonwhite correlates most with overcrowded (+0.46), and homes owner-occupied with overcrowded (—0.56). Thus, in not a single instance out of four possible ones does an anomie variable have its highest correlation with another anomie variable in the other two cities. However, in all three replications each SES variable always has its highest correlation with another SES variable.

Furthermore, the correlation between the two anomie variables declines drastically. Whereas it was —0.76 for Baltimore, it drops to —0.43 for Detroit and to —0.26 for Indianapolis. In contrast, the mean absolute correlations between the four SES variables are 0.77, 0.60, and 0.79 for the three cities, respectively. We see, therefore, that in two of the cities the anomie variables are substantially more highly correlated with other variables than they are with each other. Thus, quite aside from the question of whether nonwhite and homes owner-occupied approximate our intuitive conception of what is meant by anomie, there is no evidence whatsoever that these two variables jointly define any theoretical construct at all that is uniquely different from what is measured by other variables in the analysis.

[16] Donald T. Campbell and Donald W. Fiske, "Convergent and Discriminant Validation by the Multitrait-Multimethod Matrix," *Psychological Bulletin,* 56 (1959), pp. 81–105.

[17] All three correlation matrices appear in Chilton, *op. cit.,* p. 73.

CHILTON'S STUDY

In an attempt to reduce the data for all three cities to some common basis, Chilton performed new factor analyses for each city. In each of these analyses, he retained and rotated four factors—far too many for only eight variables. This is reflected in the appearance of unmistakable specific factors in several of the solutions, and corroborated by the fact that eigenvalues drop below 1.0 beyond the second factor in all three analyses.[18]

Chilton then noted that, in his factor analysis for Baltimore, the variables were grouped differently from their arrangement in Lander's original two-factor, unrotated centroid analysis. Thinking that the discrepancy might have resulted from his own use of a principal-axis solution, Chilton refactored the Baltimore data by the centroid method. Again, four factors were extracted, and these were presented both in rotated and unrotated form. Again seeming discrepancies were noted between Lander's solution and his own. In an effort to check these last results, he reconstructed the correlation matrix from both sets of factor loadings, his own and Lander's, and found that his solution led to a better approximation of the original correlations than did Lander's. He concluded that this was because Lander had erroneously reversed the signs of the loadings of four variables.[19]

However, Lander was right and Chilton wrong. First of all, there is never that much difference between centroid and principal-axis solutions. All of the discrepancies between factor solutions noted by Chilton are due in part to the natural differences between rotated and unrotated solutions, and in the main to the difference between the number of factors in Chilton's rotated analyses and the number employed by Lander. Because he extracted so many factors be-

[18] On 1.0 as the criterion for stopping, see Harman, *op. cit.,* p. 363. On the appearance of specific factors, see the discussion and citations in Kenneth I. Howard and Robert A. Gordon, "Empirical Note on the 'Number of Factors' Problem in Factor Analysis," *Psychological Reports,* 12, No. 1 (1963), pp. 247–250, and the erratum, *loc. cit.,* No. 2 (1963). When eigenvalues drop below 1.0, factors cease to account for as much variance as a single variable, and so no data reduction is achieved.

[19] Chilton, *op. cit.,* p. 76.

yond the two that the data can barely support, Chilton's common factors show no stability and decompose rapidly under rotation. Furthermore, to the extent that the original correlations were more nearly reproduced using his own factor loadings rather than Lander's, it is entirely due to the contribution of the additional variance accounted for by the two extra factors in Chilton's four-factor solution. Possibly there was also some confusion of signs at this point because of Lander's having reversed four of his variables.

Lander reflected these four variables in order to save space in plotting them in his Graph I. The reflection is indicated, somewhat obscurely to be sure, by the minus signs to the left of his variable numbers in his centroid solution, and by actual changes in the names of his variables in his oblique solution.[20] In view of these changes, the signs of his loadings are all quite correct. Chilton's statement to the contrary notwithstanding, one can reverse any number of variables at any time, so long as the appropriate sign changes are carried through all of the factors. This Lander did.

The first two factors of Chilton's unrotated centroid solution should correspond exactly with Lander's centroid analysis, and they do—when certain facts are taken into account. One of these is that Chilton's factor II is a total reflection of Lander's factor II. (It is always permisisble to reflect an entire factor.) In order to see that the two solutions are identical, it is necessary therefore to reflect Chilton's factor II, and then to make all of the sign changes in both factors as dictated by Lander's reversal of four variables. It was probably this fortuitous reflection of factor II between the two analyses that led Chilton to name as the confused variables not the four that Lander reversed, but the four that he did not reverse. Ideally, Chilton should have considered whether any reflections were required to facilitate comparisons between the analyses before attempting to interpret the results. It should be emphasized that there is no substantive issue involved in any of these points of dispute between Lander and Chilton, with the possible exception of how many factors are appropriate.

[20] Lander, *op. cit.*, p. 53.

One final point concerning the factor analyses remains to be corrected. Chilton, like Lander, wished to present a picture of two of his factors in as little space as possible (see his Graph 1). Therefore he treated negative signs on the abscissa as though they were positive, and included a brief explanation of what he had done. This is an extremely undesirable solution to the space problem. In effect, it folds over some of the variables, and moves them into an adjacent quadrant. Their presence there has no substantive or mathematical significance, and it leads to confused interpretations, as witnessed by the fact that Chilton himself was misled by his own device into stating that "the graphic plot upon which part of the original interpretation was based now presents a very different picture. Delinquency may be said to cluster with rent, education, and percent nonwhite, two of which were interpreted as indicators of an economic factor in the original Baltimore analysis."[21] Actually, rent and education belong either one quadrant to the left or one quadrant down, depending on how they were reflected. Other variables in his diagram, similarly treated, should also be relocated. As we indicated before, this analysis is actually identical with Lander's, and no interpretations not common to both are justified. If one wishes to save space in plotting factors, the appropriate variables should be reflected 180 degrees, which moves them through two quadrants (for two factors) and preserves their mathematical and substantive meaning.

INTERPRETATION OF FACTORS AND THE PROBLEM OF MIXED CUTTING POINTS

We have already indicated why we feel that no more than two factors can be supported by the three sets of census tract variables. It is also our opinion that the two factors in Lander's unrotated orthogonal centroid solution are best interpreted as two aspects of socioeconomic status, one giving more emphasis to economics and the other to race (just as verbal and quantitative skills might define two aspects of intellectual ability.) This organization of the variables emerges even more sharply when the first

[21] Chilton, *op. cit.*, p. 76.

TABLE 3. ORTHOGONAL VARIMAX ROTATIONS FROM PRINCIPAL AXES SOLUTIONS; BALTIMORE, DETROIT, AND INDIANAPOLIS

	Baltimore		Detroit		Indianapolis	
Variable	I	II	I	II	I	II
Delinquency rate	−.58	−.67	−.49	−.64	−.91	−.15
Median education	.91	.06	.86	.19	.71	.51
Median rent	.90	.06	.89	−.02	.60	.59
Overcrowded	−.79	−.47	−.55	−.67	−.88	−.32
Substandard housing	−.86	−.35	−.63	−.58	−.86	−.42
Owner occupancy	.52	.71	.62	.42	.76	−.19
Nonwhite	−.38	−.82	−.26	−.81	−.32	−.64
Foreign born	−.38	.70	−.10	.90	−.09	.81

two principal components for each of the three cities are rotated to an orthogonal varimax solution. In all three cases, foreign born and nonwhite define the race factor, with strong help in Baltimore from owner occupancy.[22] These factor loadings appear in Table 3. Coefficients of factorial similarity, in Table 4, show that the economic factor is highly invariant for all three cities, and that the race factor is highly invariant for all but Indianapolis, where it is nevertheless easily recognizable.[23] Except for Indianapolis, where it fails to load on race, delinquency loads strongly on both factors.

Despite the effort by Lander and others to see in some of these variables something other than socioeconomic status, it is obvious that—with the possible exception of foreign born—they all share heavily in that concept. This viewpoint is supported by the very high loadings that all of the variables except foreign born received on their first principal components. Excluding foreign born, the *smallest* such loading was 0.76 for Baltimore, 0.62 for Detroit, and 0.59 for Indianapolis. On this basis alone, a strong argument could be made for applying a general factor interpretation to all three sets of data. The general factor interpretation, furthermore, is also consistent with the fact that the coefficients of similarity (Table 4) between all of the factors are never small.[24] Thus, even the interpretation of the rotated two-factor solutions would have to be tempered by this consideration and, consequently, regardless of their preferences among these equally tenable alternative solutions, the factor analysts should all have arrived at pretty much the same substantive conclusions concerning the particular sets of correlations under study. As it happens, a somewhat different set of correlations would have been more appropriate in each case, but before showing this let us first note certain characteristics of the present analyses.

It is true that plots of the three analyses show owner occupancy—one of Lander's anomie variables—consistently diametrical to delinquency. However, Lander gave far too much emphasis to the possibility that the stabilizing influence of home ownership prevents delinquency, and not enough to the

[22] The Detroit and Indianapolis data are for 1950. Therefore, the shift of owner occupancy away from the race factor for those cities probably reflects the improvement in the economic position of Negroes during the intervening decade. For urban Maryland only 10 percent of the nonwhite dwelling units were owner-occupied in 1940. This was less than half of the nonwhite rates of either urban Michigan or Indiana at *that* time. Furthermore, the owner-occupancy rates in general increased markedly in all three places during the next decade, more so for nonwhites than whites. This indicates that the 1940 Baltimore nonwhites were just much poorer than the 1950 nonwhites of the other two cities. On the economic position of nonwhites at the two points in time, see *Statistical Abstract of the United States, 1952,* p. 270. On owner occupancy, see U.S. Department of Commerce, Bureau of the Census, *Census of Housing: 1950, Vol. I: General Characteristics,* Washington, D.C.: U.S. Government Printing Office, 1953, Table 3.

[23] On the use of this coefficient, see Harman, *op. cit.,* pp. 257–260. Values as low as 0.94 previously have been accepted as indicating factors that are congruent, while one of 0.46 has been rejected as being too low for congruence.

[24] In performing the rotation of Lander's centroid factors without knowing the substance of the problem, Sawyer commented that a general factor would do very well, except for foreign born. The general factor interpretation looks even better, as would be expected, for the principal axes solutions (which were not available to him).

TABLE 4. COEFFICIENTS OF FACTORIAL SIMILARITY

	Baltimore		Detroit		Indianapolis	
Factors	I	II	I	II	I	II
Baltimore I	..	.51	.98	.52	.95	.55
Baltimore II	..	..	.56	.96	.69	.64
Detroit I	..	..	..	.54	.94	.60
Detroit II	..	..	..	..	.68	.75
Indianapolis I	..	..	..	..	..	.53
Indianapolis II	..	..	..	..	..	..

opposite interpretation, according to which a low owner occupancy rate may be merely an index of delinquency itself. Those who can afford to own property can generally afford to choose where they will live—and they will probably choose to live outside of areas with high delinquency rates, if only in the interests of their children.

Not easily seen from the table of loadings, but obvious when the variables are plotted, is the fact that, besides owner occupancy (when reflected), overcrowding and substandard housing are most consistently in the greatest proximity to delinquency.[25] This results from the high zero-order correlations that these variables, in comparison to the other variables, particularly the more obviously socioeconomic ones of education and rent, have with delinquency. Anyone seeking a theoretical construct different from SES in these data might be inclined to see evidence for it in this somewhat poorer showing made by the education and rent measures in all three cities. Moreover, similar findings exist for other cities.

For San Diego, Polk found that a socioeconomic index based on occupation and education was correlated more weakly with delinquency than was a measure of ethnic status.[26] Bloom has presented correlations for an unnamed city that show median school years completed and median family income less related to delinquency than was the percentage of the white population with Spanish surnames.[27] And, in their examination of census tract data for the neighborhoods of sixteen gangs in Chicago, Cartwright and Howard report:

> . . . whereas it has previously been found that higher delinquency rates are associated with lower rent, lesser educational attainment, more residential mobility and more overcrowding, such associations are not found with gang neighborhoods in the present study.[28]

Paradoxically, overcrowding now makes its appearance in this list as one of the variables failing to correlate with delinquency.

To say the least, these repeated failures of the quintessential indexes of socioeconomic status to correlate as well with delinquency as other variables is inconvenient to the argument that the other variables are also essentially themselves measures of socioeconomic status. Fortunately, it can be demonstrated that the inconsistencies between the two kinds of variable for the case of Baltimore, for which we possess the required data, are completely artifactual. There is no reason to think that the same explanation would not apply as well to the data from the remaining cities.

Even among high-delinquency census tracts, one rarely encounters delinquency rates much greater than 20 percent.[29] There-

[25] The less preferred centroid solutions show overcrowding and substandard housing farther from delinquency than they really are. As a result, our Figure 1 gives a somewhat misleading picture of the locations of these variables in comparison to the solutions based on principal axes.

[26] Polk, *op. cit.*

[27] Bloom, *op. cit.*, p. 316.

[28] *Op. cit.*, p. 358.

[29] To some degree these rates depend on the age range under consideration. Rates based on the narrower 12–16-year-old range, for example, which is weighted more heavily by peak rate ages of 14, 15, and 16, can reach 30 percent. For typical figures in the wider age ranges, for example 7–20, see the data for Negro Harlem in Harlem Youth Opportunities Unlimited, Inc., *Youth in the Ghetto,* New York: Harlem Youth Opportunities Unlimited, Inc., 1964, pp. 36 and 140. A prevalence rate of 20.7 percent for boys has been estimated for the Lexington, Kentucky area by John C. Ball, Alan Ross, and Alice Simpson. See their "Incidence and Estimated Prevalence of Recorded Delinquency in a Metro-

fore, if delinquency were perfectly correlated with socioeconomic status, at most only the lowest 20 percent of the households on any index of SES would be implicated. An index that is expressed as a percentage would be sensitive to the proportion of persons of critically low SES (low enough to be delinquent) in a tract, so long as the index was dichotomized at a point in its range close to the delinquent-nondelinquent boundary. If the point at which it was dichotomized is remote from that boundary, e.g., the proportion with incomes under a million dollars, it will, of course, be insensitive. More generally, if the information concerning a dependent variable is concentrated in one tail of the distribution of an independent variable, the full strength of the association will not be revealed unless the independent variable is dichotomized at the optimal point, in the tail.

These principles are so well-known that considerable care is exercised, usually, in choosing an at least intuitively appropriate cutting point for categoric census data that must be dichotomized. Indeed, census data are rarely dichotomized except with an end in mind which implicitly governs the choice of cutting point. In some cases there is no choice of cutting point, e.g., percentage nonwhite; in others one has been established by the Census Bureau, e.g., percentage overcrowded; whereas in others a good precedent is lacking and the investigator must do what he thinks is best with the available categories, e.g., Lander's substandard housing, apparently.

With respect to truly continuous variables, e.g., income, rent, or education, the situation is somewhat different. In their case, the purposes to which the information may be put are so various that the Census Bureau provides a measure of central tendency—which best summarizes the entire distribution. Because of open-ended categories at the extremes of these distributions, the measure chosen is usually the median rather than the mean. As a result, it has become common practice in the case of these variables to employ the conveniently available median.

Unlike percentages based upon appropriate cutting points, measures of central tendency, and especially the median, are insensitive to conditions in the tails of their distributions. Consequently, by employing both percentages and medians in multivariate studies of a phenomenon, such as delinquency, that is concentrated in the lower tail of the distribution of any index of SES, investigators have been using indexes with mixed cutting points for their independent variables, some of which carry much more information about the lower tail of their distribution than others.

To some degree, this adherence to medians when they are available may be motivated by the feeling that it would be improper to exploit the vagaries of data by searching for cutting points that would yield stronger associations. However, the chance inflation of a correlation by this means is probably trivial in magnitude when the question is one of choosing between adjacent cutting points that are equally appropriate theoretically. In contrast, when the cutting point is arbitrarily assigned, it is much more apt to be remote from the one that is both theoretically appropriate and statistically optimal, and hence the effect on the measure of association is more likely to be drastic. Furthermore, in the absence of information that would enable one to narrow down the range of choices to those cutting points that would be theoretically appropriate, more or less, the best estimate of an appropriate cutting point is probably the one that is statistically optimal.

In the studies by Lander and his followers, both education and rent are median-based indexes, whereas the other variables—that we contend are also measures of SES—derive from percentages based on dichotomization close to the lower tails of their distributions. This accounts for the lower correlations of education and rent with delinquency. According to Lander's median-based indexes, education and rent are corre-

politan Area," *American Sociological Review*, 29 (February, 1964), pp. 90–93. They also cite a number of observed rates, some of which are much higher than 20 percent. However, our thesis depends mainly on the typical delinquency rate for an area, not on the maximum rate, and it can accommodate rates much higher than the 20 percent level we employ for the sake of argument.

TABLE 5. FACTOR ANALYSIS OF BALTIMORE DATA WITH EDUCATION AND RENT REVISED, AND REVISED CORRELATIONS

	Orthogonal Varimax			Revised Correlations With Remaining Variables	
Variable	SES	Race	h^2	Educ.	Rent
Delinquency rate	—.86	—.23	.79	.72	.73
Low education	—.94	.19	.91	..	.87
Low rent	—.91	.18	.87	.87	..
Overcrowded	—.92	—.10	.85	.84	.79
Substandard housing	—.93	.05	.87	.88	.90
Owner occupancy	.80	.36	.77	—.62	—.62
Nonwhite	—.69	—.56	.79	.58	.46
Foreign born	—.11	.91	.85	.26	.18

lated —0.54 and —0.55 with delinquency.[30] However, by redefining education as "the proportion with less than five years of grade school," and rent as "the proportion paying less than $15.00 monthly rent," these correlations undergo spectacular rises to 0.72 and 0.73, respectively—each accounting for an additional 23 percent of the variance of delinquency. Their new absolute values are now approximately of the same order of magnitude as the correlations with delinquency of overcrowding (+0.75), substandard housing (+0.74), and owner occupancy (—0.78), and they are higher than that of nonwhite (+0.67), which was one of Lander's anomie variables.[31]

The correlations between both education and rent and the remaining variables also increase in absolute value, so that when the new correlation matrix is factor analyzed, factor I, even after rotation, contains the highest loadings of *all* of the variables except foreign born.[32] This rotated factor differs very little from the first principal component in this analysis, so that it continues to support the interpretation of a general factor that is unmistakably based upon socio-economic status. Instead of having its highest loading on the second factor—as it did even in our revision of Lander's analysis—delinquency now loads practically exclusively on this SES factor. (See Table 5 and compare it with Table 2.) Factor II, which is brought out somewhat more clearly by the rotation, remains a race factor (or, if you will, a native-born factor).

The question naturally arises as to whether cutting points that are more nearly optimal could be found for the other variables, so that their correlations with delinquency would increase too. Of the other variables, only overcrowding and substandard housing offer the conceptual freedom for possible redefinition. However, none of the other logical cutting points for these variables increased their correlations with delinquency. They only decreased them. Given the available categories, their definitions were already optimal.

Although Polk's SES variables were not based on medians, the evidence that this same explanation applies to his San Diego data is strong. The operational definitions he employed were those introduced by Shevky and Bell in another study.[33] They divided education at "completion of the eighth grade," so that the cutting point used by Polk was four grades higher than the one we found optimal for Baltimore. Their second SES variable was based on the proportion of persons employed as "craftsmen, foremen, and kindred workers," "op-

[30] To make the comparisons fairer, the correlations given here are based on our raw data, rather than Lander's published values, from which they differ slightly. See fn. 8.

[31] The correlation of owner occupancy with delinquency (reversed in sign) is significantly greater than that for education at the 0.05 level, using a one-tailed test. The difference between the owner occupancy and rent correlations with delinquency is not significant.

[32] Again, two factors were justified. One principal component accounted for 67 percent of the variance, two for 84 percent.

[33] Eshref Shevky and Wendell Bell, *Social Area Analysis* Stanford: Stanford University Press, 1955.

eratives and kindred workers," or "laborers, except mine." This formulation excludes service workers and private household workers, occupational categories that are lower in status than many of those included and which contain 11.8 percent of the San Diego civilian labor force. Even more importantly, it excludes the unemployed, who constituted an additional 7.9 percent of the 1950 San Diego civilian labor force. Needless to say, the presence of this last group in particular is critical to any delineation of a segment of population of extremely low socioeconomic status. It appears that Polk's categories did not embrace the full lower range of the SES distribution.

Furthermore, the optimal cutting points for education and rent for Baltimore in 1940 included 16 and 14 percent of the reported population, respectively, whereas Polk's categories for education and occupation, applied to San Diego in 1950, included 28 and 34 percent of the population at risk, respectively. These percentages alone indicate that his categories were quite broad, and therefore that he was operating much more closely to the center of the SES distribution than may be warranted for predicting delinquency.

Finally, his actual SES index represented a composite of his occupation and education variables, so that the greater disadvantages of the occupation variable were visited upon the education variable. Although it cannot be assumed that the cutting points that prove optimal for one time and place will be the same as those for another time and place, it does seem likely, in view of the breadth of his categories, that Polk's ethnic status index, which correlated more highly with delinquency than his SES index, was actually the more valid indicator of extremely low socioeconomic status. Only 8.9 percent of San Diego's population fell into the ethnic category, as it was defined by Shevky and Bell. Negroes and Mexicans, whom it contains, comprised 4.5 and 1.2 percent, respectively, of the population, for a total of 5.7 percent. The much narrower ethnic category probably focused on persons of much lower status than those singled out by the SES index; the latter was not only broader in range, but was also shifted toward the center of the distribution by the exclusion of groups at the lower end of its continuum.

In Bloom's study median-based measures of SES were again presented along with a percentage-based ethnic variable, and no further comment is necessary. The Cartwright and Howard study, it should be emphasized, was not intended as an exact replication of Lander's work, and any implications common to both types of study must be cautiously drawn. In their investigation, they were comparing certain gang neighborhoods with the entire city, controlling only for race. Consequently, their associations are apt to appear weaker than those from correlational studies of delinquency rates over all census tracts; any interpretation of their null findings especially must carry this qualification. Over and above this, however, we strongly suspect that it was mainly their use of medians in measuring rent, education, and overcrowding that accounts for the failure of these variables to produce differences. It is instructive to note that they shifted away from the percentage-based definition of overcrowding employed by Lander in favor of median persons per room and per dwelling unit. This shift coincides with the paradoxical first appearance of overcrowding among those variables showing weaker associations. Despite these ambiguities, it should be added, Cartwright and Howard were quite definite in concluding that their gang neighborhoods were of lower socioeconomic status.

We admit that we too were surprised to find that the effect of different cutting points on Lander's data was so strong. Clearly, the importance of choosing appropriate cutting points deserves great emphasis. In the future, it is recommended that investigators working with data like these select only the optimal cutting points for each of their independent variables with respect to a given dependent variable. This will necessitate a rather laborious searching procedure that is not now a standard step in data analysis. We suspect that it will prove especially crucial in the case of ecological correlations, where a badly chosen cutting point can cost information in every observation.

Our own experience has shown that it is better to pick a point between existing categories of a variable, and to calculate the percentage to one side of that, than to employ the value of the category that a particular percentile falls in. Because category boundaries are so broad, the location of a given percentile cannot be determined precisely enough; accordingly this solution yields lower correlations than the other. We call attention to this because it would naturally occur to investigators to assign a uniform percentile to all of their independent variables in the hope of making all of their cutting points comparable, both within a study and across different studies.

Some variables are, of course, defined intrinsically, and must be used as they stand: nonwhite is a good example. Whether to accept a given definition or to operationally redefine a variable will in most cases be quite clear, but there will be some occasions that require a judicious decision. Variables expressed in categories not all of which are distinctly ordered with respect to each other also pose a problem that can be resolved perhaps only somewhat arbitrarily.

CONCLUSION

Barring the appearance of surprising new data, there should no longer be any question about the ecological relations among these variables—particularly the one between SES and official delinquency rates. We have seen, for Baltimore, that when the optimal cutting points are used, the more traditional SES indexes of education and rent approach within a few points the correlations of the other indexes with delinquency. It is not unlikely that other data will continue to show this separation of a few points and perhaps, as a result, generate speculation as to its cause. In closing, therefore, it is worthwhile to call attention to possible reasons why the correlations observed between education and rent, on the one hand, and delinquency on the other, might be depressed slightly below those of the other variables that have been studied. Probably the most important of these reasons is the failure of measures like education and rent to be calibrated in the same way for Negroes and for whites.

In the case of education, the gap that widens between the performances of Negro and of white children as they progress through public school means that formally equivalent amounts of schooling, in years, do not imply equal competence in the competition for socioeconomic status.[34] This would be especially true for Negroes whose education was received in the rural South prior to their moving to those cities in which they were found at the times of the 1940 and 1950 censuses.[35] On top of this, the validity of formal education as an index of SES is further undermined by discrimination in employment. Thus Levenson and McDill, whose data are recent, report that even when there is good reason to believe that education is constant in quality, Negro high school graduates trained for a given vocation have substantially lower earnings than their white counterparts, although employment rates for the two groups are practically the same.[36]

Indexes based on rent present similar problems. In their analysis of 1950 Chicago data, the Duncans report that artificial restrictions upon his access to the housing market force the Negro to pay more than the white for housing of a given quality. They state:

> One thing seems quite clear: non-whites get less desirable housing for a given rent than do whites. . . . a much larger proportion of non-whites than of whites occupy dwelling units that either lack a private bath, are in a dilapidated structure, or fail to meet acknowledged housing standards in both these respects. In 1950, over half, or 53 per cent, of non-white households, as against 15 per cent of white households, lived in units with no private bath or which were dilapidated. This difference prevailed despite the fact that non-white median rental was only slightly below white median rental. . . .
>
> . . . Partly in order to pool incomes and partly because of the limited housing supply, Negroes resort to doubling-up of families and incorporation of non-family members into their households. . . . and the Negro household must more often endure a crowding of

---

[34] This trend is documented for Harlem in *Youth in the Ghetto, op. cit.*, pp. 168–195, and for the country at large in James S. Coleman *et al.*, *Equality of Educational Opportunity*, Washington: U.S. Government Printing Office, 1966, pp. 220–275.

[35] Coleman *et al.*, *op. cit.*, pp. 219–220.

[36] Bernard Levenson and Mary S. McDill, "Vocational Graduates in Auto Mechanics: A Follow-up Study of Negro and White Youth," *Phylon*, 27, (1966), pp. 347–357.

> the dwelling unit to a degree that is generally recognized as undesirable.[37]

The import of these passages concerning the validity of rent as opposed to either substandard housing, overcrowding, or by implication owner-occupancy, as an index of socioeconomic status is clear. Shevky and Bell have also pointed to problems associated with rent as an index, the most important of which is probably the existence of rent controls; at the time of the 1950 census these were still in force, and somewhat spottily at that, thus compounding the difficulty.[38]

We see then that there are reasons for expecting a bit more error in predicting a style-of-life variable that is correlated with race, such as delinquency, from SES indexes based upon formalistic criteria, such as education measured in years, or rent, than from indexes in which present life-style is immanent, such as overcrowding and substandard housing. Until such a time as these reasons can be safely discounted, it would be unwise to conclude on the basis of small differences in correlation that the independent variables in question differ from each other in any fundamental sense.

Finally, it should be emphasized that this paper has been concerned with the empirical issue of whether delinquency is related to socioeconomic status or not, and not with the mechanisms of that relationship. The effect of the revised cutting points on education and rent, and the nature of the variables, such as overcrowding, that already possessed optimal cutting points, indicate that it is the extremely low end of the SES range that is most relevant. The advantage of having this established is that the many known concomitants of low SES become more worthy of investigation in the search for mechanisms.

This finding also contains a warning concerning the conduct of antipoverty programs. It suggests that, in order to decrease delinquency, for example, it is necessary to reach the very bottom-most stratum in every census tract. Simply pumping money into low-income areas may result in helping needy people, but they may not be the ones chiefly responsible for the high social pathology indexes from which intervention against poverty now derives its main political justification. To the extent that programs fail to reach this lowest stratum—however successful they are at assisting the more accessible higher-stratum poor—they will fail to alleviate the more intractable and socially visible consequences of poverty. Certainly there is much to be said, on humanitarian grounds alone, for directing limited resources toward the people best able to take advantage of them. Undoubtedly, this serves to prevent even higher pathology rates in the future. Nonetheless, there remains the possibility that the failure of programs to materially reduce delinquency and eliminate hard-core poverty will trigger political reactions that make it impossible to gain support for efforts that would benefit the very poorest. For these people's own misery to be used to legitimate help for someone else, and in a manner that diminishes their own chances of eventually receiving help themselves, would be the ultimate exploitation.

---

[37] Otis Dudley Duncan and Beverly Duncan, *The Negro Population of Chicago: A Study of Residential Succession,* Chicago: University of Chicago Press, 1957, pp. 81–84.

[38] *Op. cit.,* pp. 23–24.