MODELS OF STATUS INCONSISTENCY AND SOCIAL MOBILITY EFFECTS*

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American Sociological Review 1975, Vol. 40 (June): 322–343

The author draws out the conceptual implications of technical procedures which he employed in an analysis published in this journal (Hope, 1971). He defends an earlier statement of status-inconsistency theory (Lenski, 1954; 1956) against attacks on its coherence, and he provides a model which accurately represents the theory (and an equivalent theory of mobility effects) and may be used to test it. By contrast with this "diamond" model, he shows that the usual "square-additive" model cannot test for the presence of status inconsistency or mobility effects, whether these are defined additively or interactively. The technique of "design matrix regression analysis" is introduced as a means for exploring the relations between models in order to grasp their structure and implications before they are fitted to data. The square-additive model is criticized for its conceptual vagueness, but a theoretical position is suggested which incorporates the main positive feature of the model while constituting a worthy rival to inconsistency theory.

Status Inconsistency Theory

The purpose of this paper is to reopen an issue which many sociologists regard as settled, by questioning a model which has been generally accepted and widely used. In this undertaking we are faced with three serious problems. The first is the obvious difficulty of persuading readers to reconsider a position which has, in many cases, become part of their standard intellectual equipment. The second is the existence of alleged ambiguities in statements of the theory which the model is supposed to test. And the third is the absence of any detailed explication of the model itself which would make clear which of its features are essential, and which are adventitious, to its claim to test the theory which it purports to test.

The theory to which we address ourselves is the theory of status inconsistency as set out by Lenski in his paper "Status crystallization: a non-vertical dimension of social status" (Lenski, 1954; "consistency" has now replaced "crystallization" as the usual term for the phenomenon). Lenski says:

> The basic hypothesis tested in this study is as follows: individuals characterized by a low degree of status crystallization differ significantly in their political attitudes and behavior from individuals characterized by a high degree of status crystallization, when status differences in the vertical dimensions are controlled.

The argument of the present paper turns on the appropriate interpretation of the phrase "when status differences in the vertical dimensions are controlled."

Having stated a general theory, Lenski distinguished one possible way in which such a difference might manifest itself. Observing that "without common scales [on his four status hierarchies or axes], a measure of status crystallization would be impossible" he establishes his "quantitative measure of status crystallization . . . by taking the square root of the sum of the squared deviations from the mean of the four hierarchy scores of the individual and subtracting the resulting figure from one hundred."

As he makes clear in this and in a subsequent paper (Lenski, 1956), the sug-

*The earliest version of this paper was cited in a note published in this Journal (Hope, 1973). Successive drafts benefited from detailed critical scrutiny by Professor O. D. Duncan, Professor R. W. Hodge and the editor. The author is also indebted to Mr. J. S. Flemming, Miss A-M. Skrimshire and Miss P. Thorburn for comments. The penultimate draft was delivered to the World Congress of Sociology meeting in Toronto, August, 1974.

gested quantitative measure of sheer extent of inconsistency is not the only possible representation of inconsistency; it is also possible to distinguish different patterns of inconsistency, and these might be represented by contrasts between hierarchies in which the signs of differences are retained. I refer to such signed contrasts as "status discrepancies."

Although Lenski later endorsed the attempt to represent his theory which I am about to criticize (Lenski, 1964), I can see no significant incoherence or ambiguity in his two original papers. It is true that he sets out at least two ways in which inconsistency effects might occur, and it is also true that, at first sight, there might appear to be some difficulty in reconciling his verbal formulations of the theory; but both these difficulties vanish if we concentrate on his fundamental point, which is that previous workers, such as Warner, have ignored possibly important aspects of their data by aggregating the positions of persons across different status dimensions. Lenski is simply saying that the singular "vertical" dimension studied by Warner[1] may not be the only one to affect political action (or other dependent variables), and in order to investigate this we must define non-vertical dimensions of one sort or another. It is true that he speaks of "vertical dimensions" in the plural, and he refers to status axes as "parallel vertical hierarchies which usually are imperfectly correlated with one another." But the apparent ambiguity is immediately resolved if we consider and compare the two most common diagrammatic ways of representing the profile of a person across a set of axes. One method is to draw a set of parallel vertical lines such as those on a personality profile chart and to represent the individual by a stylized graph. The second method is to draw a vector diagram in which axes appear like the arrows under the American eagle; the cosine of an angle between vectors being set equal to the correlation between the variables which they represent. The position of an individual is then represented by a point in the vector space. There is no obvious way of representing a "non-vertical dimension of status" on the "parallel vertical hierarchies" of the profile chart, whereas in the vector space a non-vertical dimension is any dimension orthogonal to the singular dimension which we choose to stand for the vertical dimension. The vector representation of status axes has been lucidly discussed by Jackson and Curtis (1968) and it is an essential element in Hope's (1972a) concept of a "stratification space."

Lenski's theoretical statement has been discussed at some length because comments by sociologists on earlier drafts of this paper revealed that the 1954 and 1956 papers were commonly believed to be confused and incoherent, and the quotations given above were cited as evidence of this.[2] In fact, however, all his statements can be interpreted in the light of one or other of the two diagrams which have been described, and these are merely alternative ways of representing the same situation. The appropriate criticism which could be advanced is not that the theory is confused, but that the means suggested for testing it are not quite strong enough for their purpose. The logic of the theory is that an effect of status inconsistency *per se* has been established only if the inconsistent person differs from the consistent person who lies at the same level of the vertical dimension. Lenski, however, confined himself to checking that the mean of all inconsistent persons on the dependent variable does not differ from the mean of all consistent persons. Brandmeyer (1965) recognized the inadequacy of this control but did not succeed in producing a better one.

Lenski's proposal for preserving information omitted by earlier workers is quite

[1] Cf. also Sorokin's (1927) emphasis on the vertical dimension in *Social Mobility*. Sampson (1963) gives a clear statement of the historical background of Lenski's work.

[2] This generally-held opinion may well have been important in persuading subsequent writers that they need not take the theory too seriously and that they were at liberty to redefine it in terms of their own models. Whatever Lenski may have said elsewhere, I can find no justification for the view that he was confused in the 1954 and 1956 papers. On the contrary, the confusion occurred with the introduction of the additive model (which is discussed below), and with the failure of its proponents to explain why they thought it adequately represented the theory. The crucial point to grasp is that Lenski was starting from the current practice of examining only a single vertical dimension, and he (like Benoit-Smullyan, 1944) was asking us to consider the possibility that in doing this we are ignoring potentially important aspects of the data, which might take a variety of forms.

straightforward: granted that we wish to work with a vertical dimension, somehow defined, then we can preserve all the available linear information by retaining as many dimensions at right-angles to the vertical (and to one another) as there are degrees of freedom left when the space is collapsed across the vertical dimension. If we wish also to look at certain interactions among these dimensions, then we can do so by defining appropriate contrasts such as absolute deviation from the vertical dimension. An alternative way of preserving all the linear information present in the data is to retain all the original status variables without defining a vertical dimension and *a fortiori* without defining any non-vertical dimensions perpendicular to it. The critical purpose of the first part of the present paper is to point out that subsequent empirical work on status inconsistency has fallen into conceptual confusion by representing the second alternative as a model of the theory which was stated in terms of the first. The paper develops themes which were introduced in an earlier paper published in this journal under the title "Social mobility and fertility" (Hope, 1971).

In 1964 Mitchell published a discussion of status inconsistency theory as it had been formulated by Lenski and by Jackson (1962). The important point to note about Mitchell's paper is that it was quite explicitly a proposal to supplant status inconsistency theory by the alternative theory formulated in terms of the original status axes. But it was subsequently misinterpreted as a statement, not about the theoretical priority but about the conceptual coherence, of status inconsistency theory.[3] Mitchell's position is made quite clear when he writes, with reference to Lenski and Jackson:

> However, rather than first allowing the traditional methods and variables to account for as much of the variation as possible and relating crystallization to the residuals, the authors begin with their crystallization measures and then work backwards.

By contrast, Lenski (1954:405), had applied the adjective "traditional" to the concept of a single unidimensional hierarchy. To put the point crudely, Lenski found himself with a vertical dimension and only a vertical dimension, and he set out to elaborate his theoretical baggage by the addition of non-vertical dimensions. Mitchell found himself equipped with "traditional" variables from which no vertical dimension had been derived and he therefore had no need to specify non-vertical dimensions. Both authors outlined self-consistent theoretical positions, but subsequent writers confused the picture by representing the latter as a means for testing the former. There is in fact no empirical way of deciding between the two theories because one is merely an orthogonal rotation of the other.[4] If, for example, one and only one of Mitchell's original status axes[5] is related to the dependent variable, and if Lenski's vertical dimension is not identical with that axis, then a status inconsistency effect is present in the data.

There is a further respect in which Mitchell's arguments constitute an attempt to bypass, rather than to overthrow Lenski's theory, and it may be mentioned here in order to avoid further confusion. Mitchell criticized Lenski for concentrating on status inconsistency rather than on variables which might be supposed to intervene between inconsistency

[3] Blalock (1966:59) says "Hyman and Mitchell argue in effect that the identification problem (though they do not use this term) has in the past . . . ." Mitchell's paper has nothing to say, either explicitly or implicitly, on the concept of identification. Hyman's (1966) paper points out that there is no way of choosing empirically between Lenski's and Mitchell's formulations and inclines to the latter. The problem of identification does not arise until we attempt to use Mitchell's model as a *means for testing* the existence of Lenski's status inconsistency effects. So long as they are merely alternative ways of looking at the same data (i.e., alternative sets of axes spanning the same stratification space), and we do not introduce aspects of one into a model based on the other, then identification is not an issue.

[4] They are special cases, respectively, of models three and two in my paper "Social mobility and fertility" (Hope, 1971) where I give the rotation matrix explicitly.

[5] This is clearly the alternative which Mitchell thinks is most likely to explain Lenski's findings, his preferred variable being ethnicity. It is true that he also speaks of combining the status variables, but in referring to this he seems to use "interaction" and "addition" interchangeably, and he rather mysteriously suggests that we add dimensions together "one at a time." The fully-fledged model involving recourse to additive combinations of *all* the status axes is not really explicit in Mitchell's paper.

and the dependent variable. In particular, he reserved the term "status incongruence" to refer to reversals in relations of dominance and subordination. The distinction between inconsistency and incongruity is quite straightforward. Incongruity is a relational phenomenon which takes the form of saying that (individual or group) A prevails over B in one situation, but not in another. Inconsistency, on the other hand, is a distributional phenomenon which differs from incongruity in that one can speak of the inconsistency of a single individual or group, without explicit reference to a second individual or group, simply by specifying its position on a set of axes. Mitchell's proposal was that incongruity, being an observable phenomenon of social interaction, is more worthy of study than the remote structural condition of inconsistency. It may be replied that while incongruity is undoubtedly a legitimate object of investigation there is no reason to suppose that it is invariably pathological–it may simply reflect a division of social labour–or that it is an invariable concomitant of inconsistency, or that it is the only possible mechanism transmitting the effects of inconsistency to a dependent variable. As Mitchell himself says (1964:318n), "A concern with single individuals [i.e., with inconsistency] is, of course, just as legitimate as a concern with social relations. However, the investigator should be clear about the distinction between the two."

The Linear Additive Model

Two models have been advanced as ways of testing for the existence of status inconsistency effects. The first is the linear additive model (Lenski, 1964; Blalock, 1967). In his paper, Lenski acknowledges his indebtedness to Blalock and frankly admits that his knowledge of the statistical properties of the model is limited. He was clearly not aware that it misrepresented his original theory. Since his initial presentation of the theory is sound and he admits his uncertainty over the statistics, the most reasonable interpretation of his position is to concentrate on the theory and to ignore his endorsement of the model. The second model to be suggested as a test for the presence of status inconsistency effects is the non-linear additive model which is discussed below.

Whatever these two models do, they cannot test for the existence of status discrepancy effects (correlates of signed contrasts between the status axes) even though a status discrepancy effect was taken by one of their authors[6] to be the most obvious example of a status inconsistency effect (Blalock, 1966b; 1967; cf. also Hodge, 1970). This point was, of course, made by Blalock when the linear additive model was first introduced, and Hodge gives a very clear algebraic account of the reasons why it is so. The question one must ask, therefore, is why research workers persevered with the new model when it proved to be incapable of testing for the existence of status inconsistency effects in the most straightforward sense of that term. No explicit defence of this perseverance appears to have been made, but it may be surmised that the identification problems faced by the linear additive model were taken to be evidence of obscurity or error in the original theory.

The essence of the difficulty may be grasped by considering the case of just two status axes x_1 and x_2. The linear additive model purports to test for the presence of a status discrepancy effect by first of all computing the regression of a dependent variable y on x_1 and x_2,

$$\hat{y} = b_1 x_1 + b_2 x_2$$

and then adding a discrepancy term to the equation,

$$\hat{y} = b_1 x_1 + b_2 x_2 + b_3 (x_1 - x_2) .$$

The problem is that the estimates $\hat{y}$ yielded by the second equation[7] are identical with those yielded by the first. The covariance matrix for the three terms x_1, x_2 and $(x_1 - x_2)$ is singular and hence no unique set of regression coefficients b_1, b_2 and b_3 is specified by the second model. We may employ any one of an infinite number of sets

[6] And also by others who did not advocate the linear additive model (Zelditch and Anderson, 1966).

[7] Note that this formulation differs from the usual one in that it focuses primarily on the identity of the estimated values of the dependent variable under the two equations, rather than on the indeterminateness of the estimates of the regression coefficients b_i in the second equation. This shift in emphasis was introduced in my previous paper (Hope, 1971).

of coefficients, all of which yield the same estimates as one another and the same estimates as the first equation.

When a model proposed as a test of a theoretical position runs into immediate mathematical difficulties, we may either question the coherence of the theory or impugn the claim of the model to represent the theory. It seems that no writer has taken the latter course. In fact, however, the model misrepresents the theory, and in strict logic sociologists were faced with a choice between, on the one hand, retaining the model and dropping the pretension to test for the presence of status inconsistency effects, and, on the other, adhering to the original theory and finding a better model for it. In fact they tried to ride both horses at once, persevering with the model and purporting to investigate status inconsistency theory.

In order to salvage what they could of the theory, they redefined status inconsistency, identifying it with the occurrence of interaction between status axes. While the original formulation had certainly allowed that some elements of interaction may represent status inconsistencies, it had not claimed that any and every interaction may be so identified. To put the point specifically, Lenski's non-linear definition of a status inconsistency effect is stated in terms of an interaction between his vertical and his non-vertical dimension. It takes the form of saying, for example, that, at a given level of the vertical dimension, political discontent is greater in those at the extremes of the non-vertical dimension than it is in those who lie at the middle. But the additive model defines status inconsistency as all and every interaction between the two status axes. It might be inferred therefore, that although it completely fails to test for the presence of signed status inconsistencies, the linear additive model is generous in its definition of other types of inconsistency, since it subsumes in its concept of interaction all the variance explicitly indicated by status inconsistency theory plus other variance as well. We shall see below, however, that the procedures actually employed to test for the presence of interaction are not equivalent to the simple linear additive model and that they are much more restrictive in their definition of interaction.

Thus the linear additive model fails completely to test for the presence of signed status discrepancies, and it tests for absolute inconsistency effects only to an uncertain degree and in an ambiguous sense which has never been explained. The model is erroneous in that it fails to represent status inconsistency theory while being confounded with a perfectly straightforward model which does represent it.

The starting and finishing points of the reformulation of the theory which is necessitated by a mistaken choice of model were succinctly stated by Blalock (1967) when he wrote (a) "In both the status inconsistency formulations of Lenski and Jackson, and discussions of the effects of social mobility, these strains are conceptualized as being produced by marked *differences* in hierarchical statuses or in rapid changes of status" (p. 791, my italics) and (b) "Duncan notes that the mobility thesis depends, for verification, on the existence of an interaction effect in exactly the same way that tests of status inconsistency have relied on showing an interaction effect. If one assumes that the main effects of father's and son's occupations are additive, then there is no need to introduce the 'strains-due-to-mobility' argument unless the *interaction* term is significant. Exactly the same applies to the inconsistency factor, as Lenski has noted." (p. 795; I have omitted some italics and inserted others.)

It is not reasonable to conclude without further ado that, because a mathematical model has run into immediate difficulties, the theory which it is intended to express must be defective. Direct conceptual analysis of Lenski's verbal statement of the theory shows that it is coherent. The fault, therefore, must lie in the model, and one need not seek far to find it. The crucial misrepresentation is contained in a statement by Blalock (and echoed by many users of the model since) to the effect that "an underlying thesis of all these approaches is that dependent variables may be influenced not only by the separate effects of several independent variables but . . . ." (p. 790). In fact Lenski did not, in his original paper, define inconsistency by contrast to the *separate* effects of the status variables. Rather he defined it as deviations from a vertical dimension which specifies the *shared* effects of the status variables. The logic of the geometric structure he describes is that, in controlling for the vertical dimension of status, *some* aspect of *each* of the status axes should be controlled for, but not that *all* (linear) aspects of *all* axes should be controlled for. Thus a status discrepancy is present to the degree that the estimates obtained from the regression of y on the set of independent variables (each contributing its separate effect) fit the data better than estimates obtained by regressing y on the vertical dimension, however defined. Putting the matter at its simplest and considering only signed differences, we may say that the existence of a status inconsistency effect has been disproved if the equation

$$\hat{y} = b_1 (x_1 + x_2) + b_2 (x_1 - x_2)$$

yields the same estimates as

$$\hat{y} = b_1 (x_1 + x_2).$$

This is not to deny that the vertical dimension of status might be defined as a weighted sum of x_1 and x_2. The point is that neither a simple sum nor an *a priori* given weighted sum should be confused with an empirically derived sum of weighted x_1 and weighted x_2. The weights appropriate to the definition of the vertical dimension need not be the same as the weights which optimally predict the dependent variable. Insofar as they differ, a status inconsistency effect is present. The crucial step which invalidates the model as a bearer of the theory is the assumption that the parameters of χ_1 and χ_2 are to be separately estimated from the data.

The Distinction Between Construction and Application of Variables

At its root, the confusion engendered by the linear additive model stems from a failure to distinguish two quite separate activities of empirical inquiry, namely measurement of a characteristic or dimension and testing for its association with a dependent variable. The logic of status inconsistency theory demands that we first define status, so that we can define status inconsistency as orthogonal to it (or as an interaction between the vertical dimension and dimensions orthogonal to it), and then that we test whether the inconsistency dimensions or contrasts are related to the dependent variable. This two-stage nature of the model may be indicated by using parentheses to distinguish between two orders of weighting coefficient. In the two-axis case for the investigation of status discrepancy effects, we first find weights b_i such that

$$(b_1 x + b_2 x_2)$$

is a measure of status and

$$(b_3 x_1 - b_4 x_2)$$

is orthogonal to it. We then examine the coefficients c_i in the equation

$$\hat{y} = c_1 (b_1 x_1 + b_2 x_2) + c_2 (b_3 x_1 - b_4 x_2)$$

to see whether the difference term is contributing anything over and above the sum term which represents the vertical axis of general status. In the case of three or more status axes, more than one set of contrasts will, in general, be required if all linear deviations from the vertical axis (or interactions between the vertical axis and axes orthogonal to it) are to be explored.

Our first object, then, in testing for the presence of status inconsistency effects on a dependent variable y should be to ensure that we have a measure of status which is uncontaminated by our knowledge of how y is related to the status axes.[8] Such a measure may be arrived at by a variety of routes, which may be enumerated under the following heads (cf. Jackson and Curtis, 1968):

(1) A given set of weights is derived from *a priori* considerations. At its simplest, this might involve standardizing the axes and summing them, perhaps scaling them all to some common distribution before standardizing.
(2) The first principal component of the status axes is taken.
(3) Some criterion measure of status z is observed, and an equation is computed for the regression of z on the status axes x_i. It is of course desirable that errors and biases in the measurement of z should be independent of errors and biases in the measurement of the

[8] This point should not be confused with the suggestion that we should employ an independent measure of the intervening variable (e.g., psychological stress) by which inconsistency is supposed to exert its effects (Blalock, 1966a).

variable y which is to feature in our search for status inconsistency effects.

(4) A large number of dependent variables, perhaps including y, is measured and the first explanatory variate provides the weights for the x_i in the definition of status. Explanatory variate analysis was introduced by the author for the analysis of contingency tables but it was pointed out that it applies equally to the analysis of continuous variables (Hope, 1972b). The first explanatory variate is defined as that weighted linear combination of the x_i variables which predicts as much as possible of the variance of a set of variables y_j. It must be sharply distinguished from the first canonical variate which maximizes correlation rather than explained variance. Unlike a canonical variate, an explanatory variate does not remain unchanged under alteration of the relative variances of the variables y_j. Although including our dependent variable of primary interest y among the y_j of an explanatory variate analysis introduces an element of contamination between the definition of status and its application to the investigation of status inconsistency, this will not be a serious drawback if there are many variables on the right hand side of the analysis.

It is important to note that the explanatory analysis must, for present purposes, be abandoned after the extraction of the first variate because subsequent variates, although orthogonal to the first within the y-space, are not orthogonal to it within the x-space. Having employed the method to find that status axis which has maximum explanatory force over a whole range of dependent variables, the analyst may define the remaining, inconsistency, dimensions as principal components of the x-space after collapsing it across the status axis.

The logic of this operation is that we are not sure how to define overall status, but we wish to define it so as to minimize the chances of finding an inconsistency effect. If a dimension orthogonal to the first explanatory variate is significantly associated with variance in the y_j variables over and above the variance accounted for by the explanatory variate, then the presence of a status inconsistency effect has been established.[9]

As with all techniques of social measurement, the procedures enumerated suffer from a variety of defects, but they do not all suffer from the same defects, and we might be advised to employ more than one method. But the crucial point is that our method of synthesizing an axis should be independent of our analytical application of the resulting vector.

Status and status inconsistency are concepts which stand or fall together.[10] Either it makes sense to aggregate status axes by some means into an overall measure of general status, in which case dimensions orthogonal to general status (and some interactions between the vertical and the non-vertical) can be plausibly said to represent status inconsistency, or we must give up the attempt to investigate status inconsistency because it cannot be defined as a non-vertical dimension where no vertical dimension exists. It is a paradox of empirical research that investi-

[9] If confusion is to be avoided, it must be pointed out that the coefficient of multiple determination R^2 may vary from dependent variable to dependent variable even though only one explanatory variate exists; what is at issue is the *collinearity* of the projections of the dependent variables in the space of the status axes, not the strength of the relations.

If sociologists are willing to specify corresponding points on the various status axes but are unwilling to confine themselves to a linear model (perhaps because they are not confident that the axes are properly scaled), then the model of the halfway hypothesis (Hope, 1971; this generalizes very easily to three or more variables) can be employed on the left-hand side of the equations. If corresponding points on the axes cannot be identified with certainty, then the square-additive model may be employed. It should be noted that introducing the *design matrices* for these models into an explanatory variate analysis is not the same thing as imposing the models *in toto* and estimating the sums of squares associated with all their degrees of freedom. On the contrary, far from aggregating degrees of freedom, in explanatory variate analysis we are forcing the data to yield up one shared dimension which we propose to take as our best estimate of social status.

[10] This is a statement in conceptual analysis. It is not a claim that status effects and status inconsistency effects both exist.

gators have purported to test for the existence of status inconsistency effects while refusing to identify an overall dimension of status. If, in a particular analysis, we do not recognize the existence of such a dimension because we regard the axes as incommensurable, then we cannot meaningfully say that a person is consistent or inconsistent. As Lenski (1954:407) said:

> Having established the structure of these vertical hierarchies, the next problem was to establish common scales for all of them, so that the relative position of respondents in the several hierarchies might be compared. Without common scales, a measure of status crystallization would be impossible.

The converse of this point is that if we are in principle willing to make a judgment of relative status *tout court* for all pairs of persons, then we are committed to the existence of a general factor and have reserved for ourselves at least the logical possibility of making judgments of relative position on different axes.

Because it is possible to think of relations of distribution as being expressed in distances and directions in a continuous "stratification space" (Hope, 1972a) and because we often wish to think in terms of three or more stratification axes (in which case we can no longer comprise all additive deviations from consistency in a simple difference between variables), it is convenient to introduce a term which allows us to quantify extent and direction of inconsistency. I propose that we should employ the theory-neutral term *status polarity* to stand for perpendicular distance from the general status axis. Status consistency, or perfect crystallization, then becomes *zero status polarity*. Direction of deviation may be indicated by reference to the principal components of the residuals remaining after the extraction of the general status axis, however that has been defined.[11]

Although the present paper is mainly concerned with conceptual questions rather than with the question of empirical fit which has dominated much of the literature on the additive hypothesis, it is perhaps not out of place to point out that empirical investigators, by overlooking considerations of measurement theory, have in consequence failed to appreciate the fact that inconsistency effects will always be attenuated by measurement error to a greater degree than status effects. Hence the chances of demonstrating the presence of inconsistency effects are relatively low. When we add or average two equally-unreliable, standardized and positively correlated, status axes to construct a measure of status, the latter has a higher reliability than its two constituent axes. When we take the difference between the two axes as a measure of inconsistency, this has twice the error variance of the individual axes. Thus correlations with the dependent variable suffer greater attenuation in the horizontal than in the vertical dimension.[12] It is quite possible that some of the difference variables which have been investigated have reliabilities not appreciably greater than zero. The implication of these remarks is that a dependent variable which happened to correlate equally with status and with status-discrepancy, measured by means of perfectly reliable variables, would correlate more highly with the former than with the latter if the status axes were less than perfectly reliable. The difference between the two observed correlations would be exaggerated if partial regression coefficients were computed.

The Square-Additive Model

The upshot of the introduction of the linear additive model was the identification of

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[11] Dr. D. R. Ploch has drawn my attention to his unpublished Ph.D. thesis (University of North Carolina, 1968) in which he employs spherical coordinates to distinguish types of inconsistency. Like Jackson and Curtis, whose paper appeared in the same year, he adopts Lenski's approach of defining status inconsistency as deviation from an overall status axis. All subsequent work seems to have adopted the additive model.

It is notable that Ploch and Jackson and Curtis use geometrical models just as I have done in introducing the concept of a "stratification space" (Hope, 1972a). It is therefore no accident that, working in complete ignorance of one another, Ploch has employed polar projection while I have proposed the term "status polarity." The difficulties of the additive model became really clear only when a geometrical interpretation of it is essayed.

[12] A further consequence is that selection of inconsistent (or mobile) people for special investigation is more fraught with regression artifacts than is selection of people in terms of status. It is probably necessary to take at least two determinations of each axis before estimating a difference.

status inconsistency with all and only the variance representing interaction among the status axes x_i. There are three observations to be made about this redefinition. The first is that the definition appears to extend the concept of inconsistency to include all deviations from the additive model and not only those explicitly specified by inconsistency theorists. Such an extension might, however, be defended. The second is that, as users of the model often point out, the definition fails entirely to cover an obvious and important type of status inconsistency, viz. signed deviations from the vertical dimension. The third is that the definition contains a fundamental ambiguity, in that it does not make clear whether inconsistency is identified positively with interaction variance or negatively with residual variance. In practice, as we show in this section, inquiries purporting to test for the presence of inconsistency have defined it narrowly as interaction rather than broadly as deviation from the linear additive model. Furthermore, it is shown that the model which is commonly employed deprives inconsistency of some of the interactive elements which rightly belong to it.

If our aim is to explore interactions among continuous axes, then a natural first step is to chop each axis into segments or levels in order to study multiplicative relations between segments belonging to different variables. Having segmentalized our variables in this way, there is in fact more than one way to partition the model, but as a matter of historical fact only one way has been employed. The basic additive model against which to test for the presence of interaction was set out by Duncan (1964; 1966; see also Treiman, 1966). It has been used in many subsequent investigations which will be found listed in recent papers (Vorwaller, 1970; Laumann and Segal, 1971; Jackson and Curtis, 1972; Jackman, 1972). Because occupational mobility may be regarded as inconsistency between status assessed at two points in time (and indeed as a special case of Jackson's (1962) category of inconsistency between an ascribed and an achieved status), the model has been applied in tests for the occurrence of mobility effects, as well as in tests of inconsistency theory. For reasons which will become apparent, I propose to refer to the model as the square-additive model.

Many of the research reports which employ the model begin by pointing out that inconsistency or mobility is confounded with the basic linear additive model, and they present the square-additive model as a route by which the analyst may escape from this impasse. It is also sometimes claimed that the square-additive model is more parsimonious than the status inconsistency model because it does not incorporate a redundant term (see, for example, Hodge and Siegel, 1970).[13] In my earlier paper on the topic (Hope, 1971), I presented a technical re-examination of fertility data reported by Berent (1952) which were reanalysed by Duncan (1966) to illustrate the square-additive model. The conceptual analysis of the present paper has its roots in the technical innovations of the earlier paper and it is therefore convenient to employ the same data for purpose of illustration.

Duncan's analysis took the form of a 4 x 4 non-orthogonal analysis of variance (also known as a multiple classification analysis) in which the cells of the design were formed by cross-classifying a man's origin status and his final status (each being categorized into four levels). The observation recorded in a cell is the mean number of children born to men in that cell (Fig. 1a).[14] Duncan used a model

[13] The argument of the section on the linear additive model has shown that the identification problem is a burden which users of the additive model have imposed upon themselves. Like Bishop Berkeley's contemporaries, "we have first raised a dust and then complain we cannot see." The argument from parsimony may be ignored insofar as it alludes only to redundancy introduced by the analyst himself. However parsimony or simplicity is an important criterion of choice between theories. Following Wrinch and Jeffreys (1921) and Popper (1959, section 42), I regard a function as simple insofar as it has few freely adjustable parameters, which means, in its present application, that the degree of simplicity of a model is given by its degrees of freedom. The two concepts of degrees of freedom and of rotation may be regarded as the technical principles guiding the theoretical analysis of this paper. They inform the maxim which was expressed in the earlier paper as follows, "Many of the difficulties which research workers find themselves in when handling difference concepts such as mobility or inconsistency would dissolve if they would learn to ask not 'are these two sets of equations identical?' but 'are the *spaces* mapped by the two sets of equations identical?' " (Hope, 1971:1028n).

[14] In a personal communication, Professor Duncan has emphasized the tenuousness of the theoretical link between the occupational position

(a) Conventional Mobility Table

Husband's Origin	Present Social Class I	II	III	IV	All
I	1.74	1.79	1.96	2.00	1.81
II	2.05	2.14	2.51	2.97	2.38
III	1.87	2.01	2.67	3.69	2.81
IV	2.40	3.20	3.22	3.68	3.44
All	1.88	2.17	2.73	3.56	2.77

(b) Mobility Diamond

								Means
				2.40				2.40
			3.20		1.87			2.60
		3.22		2.01		2.05		2.57
Mobility	3.68		2.67		2.14		1.74	2.73
		3.69		2.51		1.79		3.10
			2.97		1.96			2.71
				2.00				2.00
Means	3.68	3.50	2.75	2.25	2.09	1.91	1.74	2.77
	Mean Level							

Figure 1. Conventional Square Mobility Table Rotated to Form a Mobility Diamond. The Elements of the Table Are Mean Numbers of Live Births to Married Couples (Berent, 1952).

which is additive in the rows and columns of the table to test for the existence of mobility effects on fertility. In this, and in most of the many other analyses which have been carried out using the same model, the interactions have been either non-significant or negligible in degree, and so it has been concluded that mobility or status inconsistency (the interpretation varies according to the nature of the marginal variables) effects are either non-existent or unimportant.

The first point to be noted about this square-additive model is that it is not the linear additive model. In the two-axis case the linear model has two degrees of freedom, one for each axis. But the 4 x 4 square-additive model has six degrees of freedom, one for each of the three independent constants fitted to the rows, and one for each of the three independent constants fitted to the columns.[15] In the earlier paper, I defined linear effects for the table of Fig. 1a by means of the linear polynomial for four points: −3, −1, +1 and +3. It is an easy matter to show (using the techniques described in the section on exploring a model, see below) that the two linear polynomials in the rows and columns of the square table are wholly subsumed in the square-additive model. Since these two linear terms are equivalent to the linear additive model for two four-valued status variables, we see at once that the six degrees of freedom of the square-additive model incorporate the two degrees of freedom of the linear additive model. But we also see that the square-additive model is augmented by a further four degrees of freedom, which are, in fact, the quadratic and cubic terms for the rows and columns of the square. Thus the square-additive model is doing something over and above the mere fitting of the linear additive model.

Concentrating for a moment on the strict linear model, we find that this is an orthogonal rotation of the vertical and non-vertical dimensions in the table, where the vertical dimension is the sum of a man's two statuses and the non-vertical dimension is the (signed) difference. The two pairs of axes are completely equivalent to one another; each has two degrees of freedom and each yields the same estimates of cell means. Thus the square-additive model has the same property as the linear additive model: it cannot test for the presence of a mobility (status-discrepancy) effect because it incorporates such an effect within its own variance. It may or may not be defensible as a distinct theoretical position, but it cannot be a *test* of mobility or status inconsistency theory. It cannot even claim the virtue of parsimony because (in the present example) it fits six independent constants as against the two fitted by a simple linear mobility analysis.

The relations between the linear terms of the mobility or status inconsistency model and the linear terms of the square-additive model have the merit of being clear and comprehensive; the two pairs of terms are exactly equivalent to one another. The importance of this equivalence is two-fold: (a) the linear additive model incorporates the status-discrepancy term in its entirety, leaving no untidy remainder, and (b) deviations from the linear additive model are identical in all respects with deviations from the mobility or status inconsistency model.

When, however, we move on to the quadratic and cubic terms which take up the remaining four degrees of freedom of the square-additive model, the situation becomes much more obscure because these terms are partially confounded with both additions and interactions between the vertical and non-vertical dimensions of the inconsistency or mobility model, and the degree of confounding varies with the distribution of observations across the cells of the table. The nature and extent of these relations varies from table to table and may be teased out by the technique

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and mobility of men on the one hand and the fertility of their wives on the other. However, the theoretical deficiencies of this particular application of the square-additive model are irrelevant to the conceptual analysis of the present paper. The reader is at liberty to rename the independent and dependent variables in Fig. 1 to achieve a more defensible analysis.

[15] It is worth noting that if we fit a constant to every row and column the model suffers from an identification problem in that the covariance matrix is singular (it has two zero roots, Hope, 1971:1030f) and so there is no unique set of coefficients for the eight axes of the model. This does not matter of course (indeed it is a universal feature of the analysis of variance) because we are interested, not in the regression coefficients, but in the estimates of cell means, and these are invariant under choice of one of the sets of coefficients which maximize the proportion of variance explained. Identification is a problem only when it is a sign that our conceptualization has fallen into confusion or that our techniques of empirical discrimination have failed.

described below in the section on exploring a model. We shall not pursue the analysis of the relations in detail here because users of the square-additive model have not provided a rationale for the inclusion of quadratic and cubic terms in the model. They did not, of course, conceive the model in terms of polynomials and so the need for an explanation of non-linear terms was not apparent to them. An obvious justification which they might have presented is that non-linear terms are required to compensate for irregularities in the scaling of the row and column axes. But this explanation does not get away from the facts, first, that the basic linear additive model has been complicated to improve its chances of fitting the data, and second, that the more complex model is wholly or partially confounded with important elements of the theoretical position which it is alleged to be testing. (The quadratic term is in fact correlated with a row term in the diamond model which is described in the following section.)[16]

When the six independent row and column terms (whether these are sets of polynomials or the more usual binary constants, which are entirely equivalent to the polynomials), have been fitted to a 4 x 4 table we are left with nine degrees of freedom which are lumped together and treated as a residual. The failure of users of the additive model to explain the nature of this residual variance might be regarded as the basic conceptual weakness of their claim to test for the presence of inconsistency or mobility effects, since we cannot know what they have disproved unless we are told the theoretical nature of the variance which they have found to be non-significant. If certain cells were to deviate sufficiently from the additive model to yield a significant interaction term, what name would we give to this effect, other than purely technical appellations such as "interaction" or "non-additivity?"

If a proponent of the square-additive model ventures to construct a conceptual interpretation of its interaction term, he may find it useful to note that each degree of freedom for interaction can be uniquely assigned (apart from the covariance introduced by the non-orthogonality of the design, i.e., the unequal numbers of observations in the cells) to the product of one of the three row polynomials and one of the three column polynomials.

The Diamond Model

In order to pursue the analysis further, we introduce here a substantive interpretation of a technical stage in the analysis of the previous paper. It was pointed out there that the basic conceptual axis of a table such as Fig. 1a is the diagonal containing the cells of non-mobile men. Taking the square table of the fertility analysis and rotating it through 135° to the position of Fig. 1b gives us a diamond-shaped table, and I propose to refer to the model whose structure is represented by that table as the *diamond-additive* model. In Fig. 1a the rows and columns of the table represent class of origin and destination class respectively. In the mobility diamond, the rows represent mobility (equivalent to status inconsistency if the two measures of status are simultaneous rather than successive) and the columns represent a mean of origin and destination class. The column effect in the diamond has more levels than there are classes because the model takes account of the fact that (if class-intervals are equal) a man who has crossed an odd number of class boundaries, let us say from I to II or from IV to I, must, in the absence of a mobility effect, be halfway between stayers in one class and stayers in the other. In an inconsistency diamond, a column effect is a defensible specification of Lenski's vertical dimension of status and row effects represent status inconsistency. (The precise interpretation of non-linear terms in the columns, i.e., higher-order polynomials, depends on the degree of our confidence in the scaling of the axes.) It is perhaps unfortunate that a vertical dimension should be represented by a column rather than a row effect, and analysts may prefer to

[16] In the earlier paper (Hope, 1971), I introduced a model called the halfway hypothesis which was designed to correspond more accurately than does the square-additive model to a feature of the verbal formulation of the latter, in that the estimated mean for movers between class i and class j lies exactly at the unweighted mean of the estimated mean of stayers in i and that of stayers in j. The halfway model differs from the square-additive model in that it is not perfectly confounded with a simple mobility effect (indeed I showed that such an effect is required to supplement the variance explained by the halfway model in Fig. 1a) but it shares most of the properties of the additive model and is open to the same objections.

reverse this situation. Such a reversal is not carried out here because mobility is obviously best represented in an up-down orientation and inconsistency is logically equivalent to mobility.

As with the linear model, the introduction of a mean or sum variable clears up some of the puzzles which beset analyses of difference variables (Duncan and Hodge, 1963; Hawkes, 1972). More important than that, however, is the conceptual clarification which it introduces into the discussion of status inconsistency and mobility. In the diamond model a status inconsistency effect is positively identifiable as an effect in the rows of the diamond. When the diamond model is brought into conjunction with Lenski's analysis it makes obvious sense to claim that differences among row means are a consequence or concomitant of status inconsistency or mobility. The row differences may take the form of a simple linear ordering, as in my earlier paper, or they may be a non-linear contrast between the extremely mobile on the one hand and the immobile on the other, that is they may be signed or unsigned mobility effects (cf. Blau, 1956); or they may be associated with some higher-ordered polynomial. The important consideration, however, is that it is possible to specify terms in the columns which have the effect of indicating where the mobile or inconsistent would have projected on to the dependent variable if they had been non-mobile or consistent. It is then a simple matter to test for the presence of inconsistency or mobility effects by examining the upshot of adding constants or polynomials for the rows to a model made up of constants or polynomials for the column effects. Unless we are prepared to entertain a subjunctive or counterfactual conditional proposition of this sort, we cannot conceptualize a mobility or inconsistency effect.

At first sight it may seem very odd to analyze a diamond, which looks like an enlarged square from which the corners have been removed, other than as the usual textbook square. But there is no intrinsic difficulty about carrying out the analysis of variance. A square with side k and degrees of freedom $k-1$ for each side rotates into a diamond with side $2k-1$ and degrees of freedom $2(k-1)$ for each side. The analysis may be performed by fitting binary constants to rows and columns or, equivalently, by fitting sets of polynomials (see appendix to Hope, 1971).[17]

As with the linear additive model, the dimension to be partialled out may be identified by specifying effects in a number of ways. General effects may be indicated by the use of polynomials. But if the crudity of the data is such that the analyst is willing to do no more than make pairwise contrasts between classes or levels of status axes (Simpson, 1973; Hope, 1973), then each such analysis may be made in diamond form. The 2 x 2 table for origin and destination class i and j rotates into a diamond with three rows and columns. Sets of constants may be simultaneously fitted to the columns of all the 3 x 3 diamonds derivable from a table such as that in Fig. 1 in order to test for the presence of row effects.

Four methods of partialling out the overall dimension may be distinguished, and these are listed here in ascending order of stringency and decreasing order of falsifiability. In describing the methods, terms appropriate to inconsistency analysis and terms appropriate to mobility analysis are used interchangeably.

(a) a linear ordering of the column means in the diamond, with one degree of freedom;

(b) a polynomial of order k, the rationale for which is that only k items of direct

[17] It has been objected that it is nonsense to talk of the degrees of freedom of the rows or columns of a diamond. In fact it is not nonsense. The mean of a row or column is the mean of the persons in that row or column, irrespective of the number of cells among which they are distributed. Any set of numbers may be conceived as lying in a space of a certain dimensionality, and it is nowadays a simple matter to compute the maximum possible dimensionality (if we cannot work it out analytically) as the rank of a covariance matrix calculated in the course of a design matrix regression analysis (see below). The dimensionality of a set of means is its degrees of freedom.

It is perhaps significant that Lenski stated his model in geometrical terms whereas subsequent writers have tended to work with algebra. This in itself need not have led to confusion because there is a complete logical equivalence between corresponding geometrical and algebraic formulations. However it is often very much more difficult to "see" the "structure" of a "model" presented in purely algebraic terms, whereas geometrical representation gives the analyst a clearer intuition of the general form of his model, an appreciation of the transformations to which it might be subjected, insight into its relations with other models and a grasp of its order of complexity.

information are available about the dimension to be partialled out, these being the means of the k groups of stayers;

(c) all $2(k-1)$ degrees of freedom for columns. These may incorporate some mobility effects but it may be claimed that this is inevitable because we have no empirical basis for estimating those effects: we have no theoretically-supplied criteria of interpolation which would enable us to decide where certain groups of movers ought to project on the overall dimension. It may be noted that the column constants necessarily yield a perfect fit to the means of consistent (immobile) people in the top and bottom class. Clearly no inconsistent person can project on to the general status axis at its extremes and so no comparison can be made between inconsistent people and consistent people at the extremes.

(d) the $\frac{1}{2} k (k-1)$ degrees of freedom for each pairing of cells i and j which partial out any tendency for the mean of movers c_{ij} plus c_{ji} to differ from the mean of the relevant stayers c_{ii} plus c_{jj}. In the design matrix c_{ij} and c_{ji} are allotted a +1, c_{ii} and c_{jj} are allotted a −1, and all other cells have a zero. If we at the same time fit the further $k-1$ degrees of freedom necessary to account perfectly for the means of the stayers, then the model simply tests whether the mean of the movers from i to j is the same as the mean of the movers from j to i, irrespective of where the movers lie in relation to the stayers. This is the weakest of the four models in that it makes no attempt to surmise where a group of movers would have lain on the dependent variable if they had not been mobile. Its explanatory power is limited because it contents itself with drawing very specific contrasts between groups of movers. The model does not really count as a test of the existence of mobility effects because it involves no counterfactual proposition which would indicate the non-mobile group or groups appropriate to the estimation of an effect for a particular mobile group.

Unless the data are very crude, a partialling of type (c) will probably satisfy our requirements in most analyses. In fitting $2(k-1)$ independent terms to the columns of a diamond, we are allowing for the possibility of finding both additive and interactive mobility or inconsistency effects in the diamond. The interaction term in the diamond model has an identifiable interpretation in that it reflects the presence of inconsistent inconsistency or mobility effects, that is the occurrence of different inconsistency or mobility effects at different levels of the general status dimension. An interaction term manifests itself in the diamond model only if $k \geqslant 4$. This is a consequence of the fact that, in fitting all $2(k-1)$ column constants, we are implicitly assuming that some mobile cells cannot be compared with any non-mobile cell.

Deficiencies of the Square-Additive Model

The diamond model not only serves to represent status inconsistency theory as it was originally formulated by Lenski, but its success in this respect helps us to see precisely where the square-additive model fails. The latter incorporates inconsistency (or mobility) effects in that the two linear terms of the diamond model (a linear ordering of the row means and another linear ordering of the column means) are perfectly and reciprocally related to the row and column linear terms of the square model – that is, the two submodels made up of just the linear orderings in their respective figures fit the data to exactly the same degree and yield exactly the same estimates. This explains why the square-additive model, which subsumes the linear term in the rows of the square and the linear term in the columns, cannot be used to test for a straightforward mobility effect, defined as a linear effect in the rows of the diamond. There are also other, more complicated and partial, relations between terms of the two models, which may be explored by the methods described in the following section.

It may be allowed that the square-additive model fails as a test of the existence of status inconsistency or mobility effects but it may nevertheless be claimed that it should be retained because it models some other defensible sociological theory. After all, it may be said, the model does work, in the sense that it displays an excellent fit to many different dependent variables. There are two distinct points here and each must be dealt with on its own terms. The difficulty of assessing the force of the first claim is that no user of the model has ventured an explicit statement of either the theory which it is supposed to represent or the theory which deviations from it would represent. It is thus not clear which of its several properties are essential, and which are adventitious, to its interpretation. I have elsewhere alluded to the difficulty of interpreting the "effects" which the model fits to the rows and columns of a square mobility table such as Fig. 1a (Hope, 1971). There is something intrinsically odd about saying, for example, that the effect of origin class I may be computed from all those who started in that class, whether they stayed in it or dropped out of it, while the effect of origin class IV may be computed from all those who started in IV, whether they stayed in it or rose out of it. Part of the difficulty resides in the systematically different weightings allotted to the destination classes because of the non-orthogonality of the typical "design." But there is the further consideration that, as Sorokin (1959:509f) said, "If we want to know the characteristic attitudes of a farmer, we do not go to a man who has been a farmer for a few months, we go to one who is a farmer for life." If, as Durkheim (1970:317) alleged, it is a profound mistake to confuse the collective type of a society with the average type of its individual members, how much more mistaken is it to confuse the collective type of a class with the average type of all those who have at some time been members of that class? Since the analysis of variance has its paradigmatic application in the area of biological experiment, it would be a salutary exercise for a proponent of the square-additive model to try to describe a biometric experimental design which incorporated all the essential structural characteristics of the usual sociological applications of the model. It may be suspected that the experiment would be disowned by biologists.

Turning to the second defence of the model, namely its empirical omnicompetence, we may note that this has two aspects, one conceptual and the other an artifact of the typical distribution of cell numbers in a table such as that in Fig. 1. A biometrical analogy may suffice to illustrate the conceptual problem inherent in the interpretation of the model. When a biometrician studies consequences or concomitants of weight gain, he typically partials out initial weight and analyses the residual deviations from the regression line. With straight regression lines and homoscedasticity this means that "At each level of initial weight, the 'gainers' and 'losers' should be defined, so that the proportion of gainers at that level is equal to the proportion of losers at that level, and also equal to the proportion of gainers and losers at every other level . . . . When extreme groups are chosen in this way, then the embarrassing relationship between gain and initial weight disappears; in this method we are investigating the relation of other variables to gain with initial weight effectively partialled out – in other words held constant statistically." (Lord, 1962). The square-additive model is equivalent to partialling out *both* initial weight and final weight *and also* their non-linear as well as their linear components. When data are thus ground between an upper and a nether millstone, it is not surprising if little but chaff remains. Partialling out a (linear) column effect in a diamond is equivalent to partialling out the mean of initial and final weight, such as one might do if one were investigating natural, rather than experimentally-induced growth.

The second characteristic of the square-additive model which helps to explain its excellent empirical performance is that, as well as incorporating several sources of variance, it also contrives to subsume those aspects of the sources which relate to the most populous cells of the typical table, i.e., the cells on and near the principal diagonal. This may be illustrated by a comparison between two analyses which employ as "data" only the design matrices (a) of the square – additive model and (b) of the complete model which accounts for all k^2-1 degrees of freedom in the square. In the first analysis, (b) is predicted from (a) on the assumption that each cell contains the same number m observations, and the proportion of the variance of (b) which is predicted by (a) is given by the ratio of their degrees of freedom. Thus in a 4 x 4 table (a) has d.f. 6 and (b) has d.f. 15 so the proportion of variance predicted is 0.4. In the second analysis, the cell sizes are replaced by typical values n_{ij} which tend to crowd round the principal diagonal. When a comparative analysis of this sort is carried out using the cell sizes of the fertility analysis of

Fig. 1 (given in Duncan, 1966 and Hope, 1971), the percentage of total variance explained by the constants of the square-additive model increases from 40% to 53% with the introduction of weighting. I refer to an analysis of this sort as a *design matrix regression* analysis and I use it to distinguish between the *ostensible* degrees of freedom (6 out of 15 in this case) and the *effective* degrees of freedom (8.1 out of 15). Computation of the effective degrees of freedom involves weighting the R^2 for a column of the design matrix (b) by the non-standardized variance of that column. In carrying out a design matrix regression analysis, allowance must of course be made for the total loss of one or more dimensions owing to redundancy among the columns.

Although the discrepancy between ostensible and effective degrees of freedom plays a part in the empirical success of the square-additive model, it is not a sufficient condition of that success. When constants are fitted to the columns of the diamond in order to account for linear and non-linear status effects, the ostensible degrees of freedom are 6 out of 15 and the effective degrees of freedom are 8.7. The difference between the two models does not therefore reside in the extent to which they account for populous cells (the status model necessarily effects a good fit to diagonal and near-diagonal cells), but in the nature of the fit achieved.

Exploring a Model by Means of Design Matrix Regression Analysis

Schopenhauer remarked that "the solution of a problem often first looks like a paradox and later like a truism," and the critical aspects of this paper may well suffer that fate. Accordingly, we introduce now some positive recommendations for the guidance of workers in this and other fields. And we begin by asking what, in general, a model is for, what sort of task it is supposed to perform and how we may know that it is performing it properly. The grounds of these recommendations are to be found in a remark in Kant's *Critique of Judgement* (paragraph 68) to the effect that we have complete insight only into that which we have produced ourselves. It is because a model is an artifact whose principles of production and operation can be explicitly formulated that models are useful in natural science and social studies.

It is however a mistake to suppose that because the structure of a model is knowable in principle that we do in fact know it. There is more to understanding a model than seeing whether it fits. In order to gain a real acquaintance with it and in order to reap the benefits of having constructed it, we must play with it, seeing how it behaves in differing, often counterfactual circumstances.[18] If two or more models have been constructed, then it becomes important to know how they are related. In comparing the fit of different models to the data, we must be aware of the possibility that one model is more elaborate than another or that one subsumes some or all of the features of the other.

Considered from this point of view, the square-additive model possesses the desirable features of a profitable model in that it is perfectly precise, its properties are explicable without reference to its author's opinions and it is therefore corrigible. As so often happens, a meaningful debate is possible because the critic shares the basic tenets of the position he is criticizing.[19] The debate is conceptually fruitful because the proposed model has properties which become apparent only upon close

[18] The essential role of counterfactual conditionals in historical and sociological explanation is a central theme in a monograph on social mobility on which the author is now engaged.

[19] This position is totally opposed to the psychologism of critics such as Doreian and Stockman (1969). By all means let us construct means for obtaining culturally-enshrined ostensive definitions of inconsistency, whether these are normative or attributive, consensual or dissensual, conscious or unconscious (cf. the techniques employed for the investigation of shared meanings in Appendix A of Goldthorpe and Hope, 1974). But let us not employ these measures as intervening variables until we have first established that distributional inconsistency has some effects which call for explanation. Reductionism is frequently a barren creed because the reductionist, in his eagerness to degrade the higher-level analysis, deprives it of concepts and generalizations to such a degree that it is impossible to establish those isomorphisms between the two levels which would constitute a reduction. My experience as a clinical psychologist leads me to doubt whether any single psychological reaction will be found to mediate between status inconsistency and even one of its effects, if it has any. A certain school of sociology calls persistently for neglect of the structural study of attributes in favour of the investigation of perceptions and sentiments. This represents a sustained attempt to stand dialectics on its Hegelian head again.

examination, and yet these properties cannot be disputed because they are analytically present. The square-additive model has been faulted on the grounds that it is a conceptual *omnium gatherum*, but it is only because the model is formally precise that it becomes possible to argue this case by the use of the analytical techniques which will now be described.

In footnote 10 of my earlier paper, I invited data analysts to ask themselves whether the *spaces* mapped by two sets of equations are identical. The easy way to answer this question is to perform a canonical analysis on the two design matrices. If all possible canonical correlations are perfect, then one set must lie wholly within the space of the other. The technique may be extended to the case of partially overlapping design matrices by computing the proportion of the variance of each column of a design matrix which is predictable from the other design matrix. (This calculation is also necessary in the case of perfect canonical correlations since only by means of it is it possible to establish the strict equivalence of two design matrices.)

The present paper may be regarded as an invitation to sociologists to begin an analysis by ignoring their dependent variable and addressing themselves to an investigation of the formal properties of different functions of the independent variables (models). When offering one model as a means for testing another, it is desirable to show, by analysis of design matrices, that the two models are formally independent, or at least that any overlap between them may rightly be attributed to the former rather than to the latter. At the same time discrepancies between ostensible and effective degrees of freedom may be examined. Although it seems paradoxical to speak of a fraction of a dimension, there is no intrinsic oddity about a non-integral set of degrees of freedom. The ostensible number of degrees of freedom of a model is the number of dimensions into which it projects, and its effective degrees of freedom are the sum of dimensions with each dimension weighted by its variance.

It is not enough to write down a set of equations with perhaps five or ten degrees of freedom and to leave the reader to intuit both the internal properties of the model and the relations which it has to other possible models. There is no point in supplying a model if neither the writer nor the reader is aware of its formal properties. In determining its degree of fit to the dependent variable, we need to know what it is that is fitting, or failing to fit, the data. Furthermore, if the model purports to test a theory we need to be told the meaning of the terms which have not been included in the model.

I refer to this recommended task of formal analysis as a process of *exploring the model(s)*. The obvious technique to employ in accomplishing the task is that of *design matrix regression analysis. Venn diagrams* might be used to illustrate relations between models in simple cases. My preliminary work in this field suggests that it is useful to carry out the exploration both with observed frequencies in the cells and with equal frequencies. No doubt in some cases, particularly where comparative analyses are being performed, it will be desirable to employ other hypothetical patterns of frequencies. It should be noted that if sets of orthogonal polynomials are employed to specify terms in a dimension, pairs of polynomials will often be correlated even when cell frequencies are all equal, because the number of cells projecting on to each polynomial is different at different points along its length.

A design matrix regression analysis of the extent to which (a) the square-additive model overlaps with (c) the six polynomials defining all possible mobility effects (row effects in the diamond—see the table on page 1031 of Hope, 1971) illustrates the logical problem inherent in the proposition that the square-additive model may be used to test for the presence of mobility effects. Using observed frequencies in the cells, we find that 21% of the variance of (c) is accounted for by (a). This may not seem a very serious degree of overlap until we look at the distribution of explained variance among the dimensions or axes specifying mobility effects. 100% of the variance of the linear polynomial and 76% of the variance of the cubic polynomial are accounted for by the square-additive model. These two axes, which

number of ranks. All that is required is the identification of homologous points in the distribution. This may be accomplished by simple techniques such as standardization of variances (cf. Goldthorpe and Hope, 1974: Appendix A) or, if the data are adequate, by more powerful scaling procedures.

correlate 0.87 with one another, taken together are practically equivalent to a simple difference between the upwardly mobile and the downwardly mobile. It is just these two terms which account for the bulk of the variance of the dependent variable over and above that accounted for by the model which I called the halfway hypothesis (Hope, 1971). Hence, in this particular analysis, the argument for or against the existence of mobility effects turns on our interpretation of the difference between the halfway hypothesis and the square-additive hypothesis. The former ascribes one and the same effect to class i considered as origin and to class i considered as destination, whereas the latter allows i to have different effects according to whether it is an origin or a destination class. The halfway hypothesis may be regarded as a general factor of father-son status which is defined *a priori* by specifying that comparable points on the two axes shall have equal weights. It has three degrees of freedom because it is a non-linear rather than a linear general factor.

A set of equations is nothing in itself, even if it fits the data. In order to schematise it as something deserving the name model—that is, as a man-made analogue of social reality which we can comprehend because we know the principles of its production—we need to have insight into its properties. In the earlier paper, I argued that one means of gaining insight is to examine and compare the estimates of the dependent variable which it yields for different categories of person. The present paper has indicated how a model may be formally explored by discrete analysis of its distinct axes and by relating it to the axes of other models. The two approaches might be combined by partialling one model out of another and testing the fit of the elements which are unique to the latter.

Reconstruction of the Additive Model

Perhaps by analogy with Voltaire's famous argument "the French admiral was as far from the English admiral as the latter was from the former" (so why not hang both?), it has been objected that the confounding between the diamond model and the square-additive model is an impartial criticism of both their respective theories. This however is to misunderstand the nature of explanation and the relation between theory and empirical testing. It is by no means uncommon in the physical sciences to have two theories which are equally compatible with a wide array of phenomena because they make identical predictions in those areas. Choice between the theories then rests partly on considerations of conceptual clarity, simplicity and elegance, partly on grounds of consonance with accepted general principles of the science and partly on the search for phenomena which support one theory at the expense of the other. In sociology the appeal to general principles is indistinguishable from dogmatism and may be ruled out. The *experimentum crucis* cannot be as clear-cut in its results as one would like because relative degree of fit is often a shifting and equivocal criterion of discrimination between models, especially when much of the variance lies within cells. I have explained why clarity and simplicity appear to lie with the diamond model rather than with the square model. However, it would be desirable to have a conceptually defensible alternative to the theory represented by the diamond model which embodied the strong points of the square model. The elaboration of such an alternative will demonstrate at once the formal corrigibility and the conceptual deficiencies of the square model. It will, of course, still be a rival to the diamond model, not a test of it (the column effects in a diamond are the best possible test of the row and interaction terms which represent inconsistency and mobility effects) because there will be considerable overlap in the variance of the dependent variable explained by the two models.

The square-additive model may be criticized (a) because it has high effective dimensionality leading to empirical overkill even though the formal structure has no clear sociological interpretation and (b) because it does not adequately specify class effects. The appropriate cure for the former defect is, in Popper's (1959: section 40) terms a "formal reduction," i.e., simplification leading to greater conceptual clarity and a higher degree of falsifiability, while the second defect calls for a "material reduction," i.e., a precise identification of the bearers of class effects.

The strong point of the square model is its suggestion that movers lie halfway between those who best exemplify the effect of their origin class or aggregate and those who best exemplify the effect of their destination class. (The proposition may be restated in terms of consistent and inconsistent aggregates. In this case, one must be careful not to imply that consistent aggregates form classes or that all classes are status consistent.) In order to advance this theory to the status of a worthy opponent of inconsistency theory, we must incorporate the latter's two-stage property, namely the distinction which it draws between the specification of a function and its empirical application. If we are to construct a theory based on class effects, this implies that we should first identify the characteristic bearers of those effects and then test the deviations of movers from the estimated halfway points. In the case of many dependent variables this means computing discriminant functions for the classes without reference to movers and then examining the deviations of the movers from the halfway positions in the discriminant space.

There is however one weakness of the square model which such a proposal would not be able to remedy, in that it does not offer a general explanation of what deviations from the model would signify. Finding evidence for an inconsistency or mobility effect, which is a function which spans several cells of the table, gives us guidance on how we should set about explaining it (by looking for characteristics which vary with strength or direction of mobility or inconsistency). Finding evidence of significant deviations from the class effects model, on the other hand, leaves us at a loose end.

An analogy may make this point clear. In the nature-nurture controversy, a strong theory–Mendelian theory–is pitted against a weak one–environmentalism. The former predicts precise values for observable statistics such as intrasibship correlation. The latter is compatible with the values predicted by Mendelian theory and also with a wide range of other values. Similarly, the idea that a person combines the influences of his class of origin with those of his destination class has little explanatory power because it is compatible with a wide range of outcomes, including significant deviations from a halfway model. There is nothing intrinsic to the basic idea of combining origin and destination effects which restricts the results to the halfway mark.

One way of making the thoery less vacuous would be to specify mechanisms which determine the relative strength of origin and destination classes. Another would be to distinguish between dependent variables which are more influenced by one class and variables which are more influenced by another. The former suggestion would give the theory an advantage over inconsistency theory in that it would enable it to explain within-cell variance by taking account of differing strengths of the mechanisms in individual cases.

It is arguable that the whole apparatus of the additive model misses the point of the basic theoretical position which it tries to formalise, namely that mobile people combine or average the characteristics of people in their class of origin and the characteristics of people in the class in which they find themselves. Conformity with the model is quite consistent with a situation in which no such combining or averaging occurs. If, for example, roughly half the people in cell c_{ij} retain the level of the dependent variable which they would have had if they had stayed in i and if the other half switch over completely to the value which they would have had if they had always been in j, then a mobility effect manifests itself, not as a mean deviation from the additive model, but by inflation of the within-cell variance of c_{ij}. Since 90% of the variance often lies within cells, any model which can explain an appreciable proportion of this variance has a head start over models which do not make the attempt. To forfend an error which only too readily insinuates itself into discussions of summed error distributions it should be pointed out that inflation of within-cell variance will typically not result in bimodality of within-cell distributions. It can be shown that bimodality manifests itself only if the distance between the means of two summed distributions exceeds two standard deviations (Hope, 1969). This is mentioned here because Berger and Fisek (1970) have proposed to use bimodality as a criterion in an experimental investigation of status inconsistency.

Do Status Inconsistency Effects Occur?

The inability of the linear additive model to test for the presence of simple status-difference effects is well-documented. This paper has demonstrated that the linear-additive model is subsumed in the square-additive model which therefore has the same incapacity. The paper has also shown that there are other, more occult, sources of confounding between the square-additive model and the theory it purports to test. And it has been pointed out that, from the fact that status inconsistency can be defined as interaction between certain axes, we are not justified in inferring that it is correctly represented by interactions between very different axes. It has also been pointed out that measurement theory indicates that the dice are loaded against the detection of difference effects, as contrasted with sum effects.

No argument has yet been advanced for the actual occurrence of status inconsistency or mobility effects. Some evidence with this import was presented in the earlier paper (Hope, 1971). However, it is of interest to note that the literature on the square-additive model contains an empirical analysis which, if it is interpreted as a test of correctly-formulated inconsistency theory, confirms the existence of inconsistency effects. Hodge (1970) computed severally the regressions of a number of dependent variables on one and the same set of status variables and he claimed to show that the regressions differ not only in strength (measured in terms of R^2), but also in direction, that is in the relative weights allotted to the status axes. As the discussion above shows, differences of the latter kind demonstrate that no single dimension of status can be responsible for predicting position on all the dependent variables, and so it must be the case that at least one dimension orthogonal to a status dimension, however the latter is defined, is at work. Hence status inconsistency effects exist, though the analysis does not tell us how important they are.

Hodge's own interpretation of the analysis is that it shows that there cannot be such a thing as a single overall dimension of status. But this is to beg the question, since the interpretation can be sustained only if it is assumed that an overall status dimension, if it exists, must necessarily be the only source of explanatory variance. In other words, the interpretation assumes that status inconsistency effects cannot occur. This criticism is a mirror image of that which has been advanced against employment of the additive model to test for the presence of status inconsistency effects. There is a reciprocal relation between general status and status inconsistency in that any endeavour to find evidence for (or against) either one must involve measurement of the other. It is open to us to deny the usefulness or meaningfulness of both concepts, but we cannot supply evidence against either one without admitting the meaningfulness of both.

The dispute between the additive hypothesis and status inconsistency theory resembles that between general factor and group-factor theory in the study of intelligence, in that protagonist and antagonist are not always aware of the distinction between theory and model or of the distinction between empirical fit and conceptual interpretation. For example, Spearman and Garnett claimed to interpret a table of correlations as proof of Spearman's two-factor theory; Thomson (1920) pointed out that his sampling theory is equally consistent with the observed correlations and that his opponents were in the position of a man who argues that a quadrilateral must be a square because its angles sum to 360° (cf. Spearman, 1927, Appendix I.5). Garnett's (1920) claim that the two-factor theory is simpler than Thomson's no longer rings true because the latter is clearly more consonant with polygenic theories of inheritance. Nevertheless for practical purposes, we do in fact find it simpler to think in terms of a single general factor. The analogy has further relevance because it can be shown (Hope, 1972c; 1974; 1975) that a regression equation becomes immediately interpretable if it can be identified with a principal component of the set of variables to which it applies.

Status and status inconsistency, like general and bipolar factors, are concepts which sink or swim together. The sociologist may decide to investigate them jointly – let us say in the study of prestige in its strict sense of deference-entitlement – or to ignore them both – let us say in the study of variables such as ethnicity and education – but he cannot identify effects of inconsistency without first looking at effects of status. Similarly, it is not possible to conceptualise mobility effects except by formulating a counterfactual proposition specifying where a particular group of mobile people would have lain on the dependent variable if they had not been mobile.

The argument of this paper exemplifies the general proposition that an analytical concept can be deployed only in a theoretical medium, since only thus is it possible to specify the value of Y corresponding to a value of X, other things being kept constant. We are on fairly strong ground in the theoretical analysis of status inconsistency or mobility because here it is possible to employ observed values of Y for the consistent (non-mobile) in estimating what the values of Y for the inconsistent (mobile) would be if no inconsistency (mobility) effect were present. But even in this case an explicitly analytical concept (of general status) is required to direct our choice of comparison group appropriate to a particular category of inconsistency or mobility.

Sociologists who take fright at the prospect of venturing beyond the confines set by observed, descriptive, variables should, logically, forswear the use of many of the commonest concepts of their discipline, because those concepts are intrinsically analytic. In fact the typical response of sociologists who have accepted the argument of this paper has been to eschew a concept of general status. If this response were to generalize to all areas of enquiry, the sociologist would be left with descriptive concepts only and would be deprived of any pretension to explain, since explanation requires the deployment of analytic concepts. Such concepts are essential to the specification of the *ceteris paribus* clause of a counterfactual conditional proposition.

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