

COMMENT ON "DISCRIMINANT FUNCTION ANALYSIS  
OF REGIONAL ECONOMIC DEVELOPMENT"

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The technique of "discriminant function" analysis is one which has been used to a great extent in sociology and psychology. The reason for this use in the "softer" social sciences is twofold: first, the lack of hard quantitative data; and, second, the relative obscurity, or total lack of any mathematically specifiable model of the phenomena to be explained or predicted. Recently, it has become more clearly evident that the entire area of economic development suffers from both of these shortcomings.

Perhaps an example would be of use here. Let us assume that our desire is to determine the effect of education on income. We can then specify a function  $Y = f(E)$  and proceed to gather data on each person's income and educational level. We can then use linear (or non-linear) regression techniques to determine if there is any causality between these two variables. Regression can be used to explain income level in terms of education levels.

However, let us suppose that our desire were to find those factors which were important in determining the level of income. Further suppose that these factors are such things as social strata of origin, ethnic group, type of education (technical or liberal), institution attended (Harvard or Chicago), etc. It is clear that many of these items are not readily quantifiable. This problem could easily be dealt with by aggregating to the regression equation a series of dummy variables which indicate inclusion or exclusion in each of the above groups. Then the regression equation will indicate the importance of each variable in determining the level of income. It can also be determined which of the variables are important and their order of importance by utilizing a stepwise regression technique. This technique includes the important variables in the regression equation in their order of importance and rejects any variables which fail to make a significant contribution to the equation.

In this particular paper, neither of those techniques was applicable. The reason is that the desire was to determine, not a level of income, but whether or not an individual item belonged to a given group. Suppose we wish to determine if people with high incomes have anything in common and what are these common things. In this case, the technique used in this paper is appropriate. Obtaining information on those people with high incomes and then utilizing discriminant function analysis will indicate what factors are held in common. The actual process, which is quite involved and lengthy is described very well in Chapters 6 and 7 of Cooley and Lohnes, Multivariate Procedures for the Behavioral Sciences, published by Wiley and Sons, in 1962. In addition, there are several computer programs available for this technique from the UCLA Bio-Med series.

In evaluating the "economic content" of the paper, there are several areas of concern. First of all, it needs to be said that very probably no economists will agree on the criteria for the original grouping process. Since the process involved a normative judgment on the relative weights to be assigned to the per capita income, unemployment, and the amount of poverty, and since the actual criteria used is not specified, it would be difficult indeed to argue or agree with the criteria selected. However, the general concept of tempering the per capita income with the level of poverty and un-

employment is certainly appropriate. It should be pointed out, however, that these criteria imply a welfare judgement in that a given level of per capita income with less poverty and unemployment is preferable to the same per capita income (or one higher) with more poverty and unemployment.

It appears that two important considerations have been omitted. First, using simple averages of data over a five or six year period ignores the effects of the turning points of the business cycle which occurred during the 1960-66 period. Second, there is no mention of migratory workers or their effect. It appears that some measure of this must be included. It is well known that this form of labor is very important in certain Florida counties.

Professor Benjamin Higgins, in his review article of the Adelman-Morris book, Society, Politics, and Economic Development, Johns Hopkins, 1967, published in the Journal of Economic Literature, Vol. VII (June, 1969), pp. 436-43, indicates several areas of concern with the technique and methodology used by Adelman and Morris. Much of what he says is clearly applicable since the technique used in this paper is similar, if not identical, to that used by Adelman and Morris.

The first area of concern is with variables 11, transportation adequacy, 13, index of natural amenities, and 16, government outlay per capita. These indices require a scaling procedure which could very easily result in different indices depending upon who was doing the scaling and how much he knew concerning the county in question. For example, some secondary roads are very highly traveled while some are really hardly used at all. Rivers should be included in the natural amenities index as well as the level and nature of pollution in the waterways. It appears that state and local government expenditures ought to be included directly. Since, as Higgins points out, "the output of the factor analysis depends on the input," much care should be exercised in ensuring that the qualitative variables are properly scaled.

The danger of implicit theorizing which Higgins points out Adelman and Morris fell into is present in this paper. However, it does not appear to be in error. The implicit theorizing that does occur is in keeping with currently accepted economic doctrine.

The problem of cross-correlation among the variables is handled very well by the authors, who very correctly distinguish between prediction and explanation as having a very different set of desirable characteristics. For prediction, simultaneity is almost desirable, while for explanation, the same characteristic becomes highly undesirable.

Finally, the danger that preconception of causal relationship may be allowed to overrule the statistical analysis in interpreting the results has been avoided in this paper, at least to my mind. The analysis has been careful and thorough, and at least as far as I am able to determine, free from conceptual error.

In summarizing this critique, there are four things which need attention:

- (1) make all welfare judgements explicit;
- (2) explain and describe the technique utilized more carefully and clearly;
- (3) make the criteria used for grouping the counties more explicit, including the weights used to include unemployment and poverty; and
- (4) explain the tables more carefully.

Any overall evaluation would have to include the comment that the paper makes a significant contribution to the literature in economic development but needs some work on the specifics. The technique has been demonstrated to be of great value in analyzing the process of economic development. It would have been interesting to include in the analysis variables of a socio-political nature in order to test if there are any cross county differences in this area and their effect, if any.

The really important substantive issue raised by this paper is a methodological one concerning the whole area of model building in economics. The very use of discriminant function analysis, or its close kin, factor analysis and canonical analysis imply, indeed define, that there is not a known model to explain this observed phenomena. This in and of itself is enough to give the model-building economists fits of fever. The temperature level generated by this issue can be readily gauged by reading the discussion by Arthur Skein and others and rebuttal by Adelman and Morris in the March 1970 issue of the AER. I have only glanced at that piece, but even from that casual perusal it is clear that this issue has been joined. This paper presents additional evidence, strong evidence I might add, on the side of Morris and Adelman.