



## On "A Reconceptualization of School Effects"

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## COMMENTARY AND DEBATE

### ON "A RECONCEPTUALIZATION OF SCHOOL EFFECTS"\*

(COMMENT ON SØRENSEN AND HALLINAN, SOE, OCTOBER, 1977)

With the forbearance of my colleagues and friends Sørensen and Hallinan, I will argue that their reconceptualization of school effects is neither novel nor plausible. Lack of novelty is no sin, but it is easier to understand and to criticize their linear differential equation when we view it as a rearrangement of the parameters of a familiar structural equation (path) model. Viewed in this way the model is unconvincing. I question whether the metric properties of data on family, person, and school are strong enough to sustain the theoretical demands of the Sørensen-Hallinan model. I question the interpretation of autoregression in the endogenous (dependent) variable as an index of opportunities for learning. I question the implicit specification that exogenous variables have constant lagged effects. From these observations I am led to question the empirical directive of the Sørensen-Hallinan paper, that studies of school effectiveness should focus analytic attention on school-to-school variations in the autoregression of achievement variables. Also, I am even less impressed than the authors by the evidence they have adduced to bear on the validity of the model, and I point to types of evidence which would bear on the validity of their specification.

Consider the following structural equation model:

$$(1) \quad Y_t = a^* + b^* Y_{t-1} + \sum_{i=1}^n c_i^* x_i + \epsilon_t.$$

For the case  $n = 2$ ,  $t = 0, \dots, T$ , the path diagram looks like Figure 1.

\* This comment was prepared while the author was at the Center for Advanced Study in the Behavioral Sciences with support from the National Institute of Mental Health, the Spencer Foundation, and the Graduate School of the University of Wisconsin-Madison. An antecedent memorandum was presented to a seminar at the University of Wisconsin. I thank Aage Sørensen for friendly advice and Michael Hannan for his helpful reading of the manuscript. The opinions expressed herein are those of the author.

We assume  $0 < b^* < 1$ . Then the reduced-form equation for  $Y_T$  is

$$(2) \quad Y_T = a^* \left( \sum_{t=0}^T b^{*t} \right) + \sum_{i=1}^n \left( \sum_{t=0}^T b^{*t} \right) c_i^* x_i + \epsilon'_T$$

$$(3) \quad \approx \frac{a^*}{1-b^*} + \sum_{i=1}^n \frac{c_i^*}{1-b^*} x_i + \epsilon'_T \text{ as } T \rightarrow \infty.$$

Suppose our model is

$$(4) \quad \frac{dY}{dt} = a + bY_t + \sum_{i=1}^n c_i x_i + e,$$

whose solution is

$$(5) \quad Y_t = \frac{a}{b} (e^{bt} - 1) + e^{bt} Y_0 + \sum_{i=1}^n \frac{c_i}{b} (e^{bt} - 1) x_i + \epsilon_t.$$

Obviously, this is identical to equation 1 where  $Y_0 = Y_{t-1}$ , so  $\Delta t = 1$ , and

$$a^* = \frac{a}{b} (e^b - 1), \quad b^* = e^b, \quad c_i^* = \frac{c_i}{b} (e^b - 1).$$

The parameters of the differential equation may be obtained as

$$a = \frac{a^* b}{e^b - 1} = \frac{a^* \ln b^*}{b^* - 1}$$

$$(6) \quad b = \ln b^*$$

$$\text{and } c_i = \frac{c_i^* b}{e^b - 1} = \frac{c_i^* \ln b^*}{b^* - 1}.$$

In the model of equation 4 the equilibrium value of  $Y$  is

$$(7) \quad Y_e = -\frac{a}{b} - \sum_{i=1}^n \frac{c_i}{b} x_i + \epsilon.$$

Substituting the results of equations 6, we may rewrite equation 7 as

$$Y_e = -\frac{a^* \ln b^*}{b^* - 1} \Big/ \ln b^* - \sum_{i=1}^n \frac{c_i^* \ln b^*}{b^* - 1} x_i \Big/ \ln b^* + \epsilon.$$

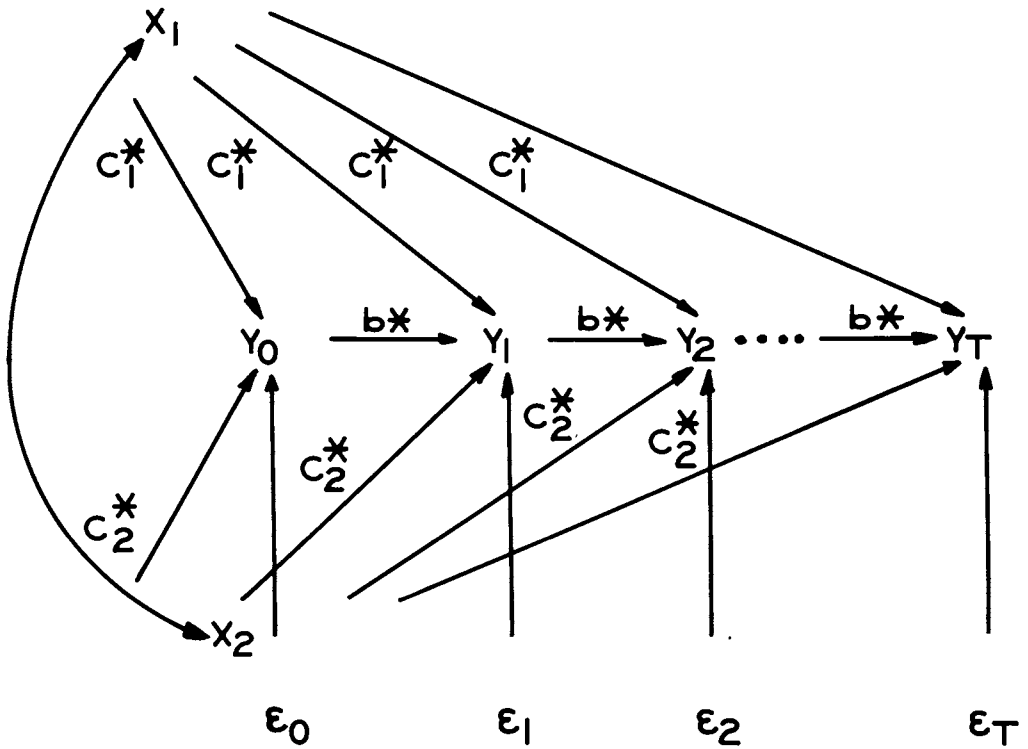


Figure 1. A Path Model of a First-Order Autoregressive Process with Constant Lagged Effects of Two Exogenous Variables

$$(8) = \frac{a^*}{1-b^*} + \sum_{i=1}^n \frac{c^*}{1-b^*} x_i + \epsilon,$$

but equation 8 is identical to equation 3. This says that the differential equation model of equation 4 is indistinguishable from the structural equation model of equation 1, so long as the system works until equilibrium is reached, or one is willing to estimate the parameters of equation 4 from estimates for equation 1 at any value of t.

Similarly, the simultaneous differential equation model developed by Sørensen and Hallinan in their equations 14 to 17 may be rewritten as a structural equation model:

$$(9) \begin{aligned} Y_{1,t} &= a^*_{11} + b^*_{11} Y_{1,t-1} \\ &+ b^*_{12} Y_{2,t-1} + \sum_{i=1}^n c^*_{1i} x_i + \epsilon_{1t} \\ Y_{2,t} &= a^*_{21} + b^*_{21} Y_{1,t-1} \\ &+ b^*_{22} Y_{2,t-1} + \sum_{i=1}^n c^*_{2i} x_i + \epsilon_{2t}. \end{aligned}$$

The model of equation 9 again has temporally constant lagged effects of the exogenous variables and temporally constant (and lag-1) autoregression in each of the endogenous variables, but it adds temporally constant (and lag-1) effects of each endogenous variable on the other. In its treatment of the endogenous variables the model resembles the well-known scheme of cross-lagged causation. While several of the following remarks pertain to the model of equation 9 as well as to that of equation 1, I shall follow Sørensen and Hallinan in focusing on equation 1.

To cut through the algebra, thus far I have said that the essential features of Sørensen and Hallinan's (continuous-time) model are conveyed by the (discrete-time) model of Figure 1. What does that imply about the fruitfulness of their reformulation? Given just two measurements of the endogenous variable, their model says to do just what many investigators would do (anyway) in an analysis of change: regress later on earlier values of the endogenous variable plus other variables thought to effect change. That is what Sørensen and Hallinan do in their reanalysis of the Project Talent data; it is also what Shaycoft (1967) did and

what Jencks and Brown (1975) have done (using a larger set of regressors) with the same data.<sup>1</sup> Thus, while the reconceptualization by Sørensen and Hallinan does affect how one motivates, interprets, and extends the usual analysis of change data, it does not affect what one does initially with the data. Moreover, while Sørensen and Hallinan's argument provides one motivation for the conventional analysis of change, the converse does not follow. Contrary to Sørensen and Hallinan's statement (p. 281), "Cross-sectional studies using a linear model therefore implicitly assume equilibrium," that is simply not the case.

The learning mechanism proposed by Sørensen and Hallinan does motivate the usual linear model for change within a constant learning environment, but the proposal ignores fundamental issues in measurement and functional form which do not arise in a less theoretically loaded context. That is, the theory does not specify the metrics of learning or of its causes. Rather, it moves effortlessly from contentless symbols for variables to their concrete expressions in the usual metrics of psychological and sociological measurement. How does one know that a theory is confirmed by the shape of a relationship between variables when the metrics of those variables may be stretched or compressed at will in any segment of their range? In economics, where the basic theoretical units are time and money, it is feasible to weigh theoretical alternatives in terms of functional form, but can one do as well in the present context? If the equations estimated by Sørensen and Hallinan were monotonic, but nonlinear in the variables measured in Project Talent, would that say their theory is wrong? Or would it say that the metrics are wrong? I think the history of social measurement may in part be read as an effort to find metrics in which relations between variables will be linear, just because linear relations are simpler to analyze than nonlinear relations. But how germane is the outcome of that historical process to the units of variables in the theory proposed by Sørensen and Hallinan? Is the history of measurement read by them as a search for the proper units for variables in their theory? The paper is silent on these questions, but they arise again in pressing fashion in Sørensen and Hallinan's search for heterogeneity in the autoregressions ( $b$ 's) of achievement variables. If differing (autoregressive) slopes are induced by differing environments for learning and rates of

learning, presumably they are also associated with differing joint means of the achievement variables. How—lacking a theoretically specified metric—do we know that the (purportedly) heterogeneous slopes in Sørensen and Hallinan's Table 2 confirm the empirical suggestion of their theory, rather than a misspecification of metrics within some alternative theory? Again, the history of data analysis is filled with instances in which functional forms may be altered by transformations of metric; for example, see the empirical literature of economics and sociology on the specification of individual earnings functions.

Sørensen and Hallinan emphasize the theoretical importance of the parameter  $b$ , the scalar coefficient of the endogenous variable in their differential equation. Under the heroic assumption (fn. 4), "the more that must be taught, . . . the faster the teaching process," it is defined by them as the negative of the reciprocal of the total amount of knowledge communicated on a given subject. That assumption is neither defended nor elaborated in the paper, yet its influence is pervasive in the interpretations offered by Sørensen and Hallinan. The parameter  $b$  is described as a direct index of "opportunities for learning," which is nominally consistent with the notion that one can learn more where there is more to be learned, but it turns out in the regression analysis that  $b$  is just (a scalar transformation of) the log of the autoregression coefficient of the endogenous variable (see equation 9 of Sørensen and Hallinan or my equation 6). Simply stated, Sørensen and Hallinan's definition says that opportunities for learning are greater where achievement is more persistent. That may or may not be true, but in my view it is not true by definition. Imagine a perfunctorily led high school English class and a Marine boot camp. In the former case we might expect relative levels of achievement to persist, but little learning to take place in the aggregate. In the latter we might expect relative levels of performance to be in flux while the aggregate rate of learning is rapid. Yet these examples violate the definition by Sørensen and Hallinan of "opportunities for learning." Alternatively, imagine defining economic opportunity in parallel fashion as the interannual autoregression of personal earnings.

Under highly restrictive conditions Sørensen and Hallinan's definition of opportunities for learning does mesh in a satisfying way with the pattern of growth in achievement produced by their model. *Ceteris paribus*, steeper autoregressions do imply higher mean levels of achievement because they increase the reduced form coefficients of the exogenous vari-

<sup>1</sup> The reader should note that Jencks and Brown (1975) appeared after Sørensen and Hallinan's analysis was complete, so I do not fault the authors in citing its relevant findings below.

ables (see equation 3). That is, the model dictates a time path of learning if we regard the playing out of initial levels of achievement and of other exogenous variables as the only processes affecting learning. Yet, as I have tried to illustrate above, this need not be the case. There may in general be an aggregate shift in achievement in each successive period without regard to the parameters of the learning process which are incorporated in Sørensen and Hallinan's model. Note in Figure 1 that we have entered (unstandardized) regression coefficients on the paths, but the model does not include an intercept. The time path of achievement is determined jointly by initial values of the exogenous and endogenous variables, by the structural coefficients, and also by environmental or intrapersonal factors which shift the aggregate level of learning. Those factors, as well as other parameters of Sørensen and Hallinan's model, might appropriately be included in a definition of opportunities for learning.

The inappropriateness of Sørensen and Hallinan's definition of opportunity as autoregression is further illustrated by one of their deductions from the model. Again, because autoregression inflates the reduced form coefficients of their model, *ceteris paribus*, greater "opportunities" imply that exogenous variables produce more variance in achievement. Sørensen and Hallinan conclude (p. 280), "good schools . . . will increase inequality in achievement . . . inequality of educational opportunity will be increased." While non-obvious consequences are sometimes counted as virtues among model-builders, I would take this one as reason to look again at the definition of opportunity, the specification of the model, or both.

Sørensen and Hallinan find scant data with which to evaluate the specification and the implications of their model, and they are appropriately modest in the conclusions they draw from published work and from their reanalyses of Project Talent data. In light of their model, Sørensen and Hallinan argue, one should not expect school characteristics to have substantial effects in the usual (linear, additive) educational production function (like their equation 10). Indeed, they say, "variables measuring school characteristics that determine opportunities for learning should not be included in an additive specification at all" (p. 282). Thus are avoided the numerous negative findings with respect to the influence of school characteristics on academic achievement. It is doubtful to me that this is an implication of the Sørensen-Hallinan model. Even if variables in the school environment affect learning primarily through exogenous variables or through var-

iation in the autoregression of achievement, the latter effects should ultimately be reflected in significant inter-environmental variation in achievement. To quote Sørensen and Hallinan, "the question that ultimately generates the interest in school effects is whether it makes a difference to a child's future attainment which school he or she attends" (p. 275). Their model specifies a unique mechanism by which school effects may occur, but it does not tell us that such effects should go undetected in the usual linear model.

Following up their argument that the autoregression of academic achievement variables will vary directly with opportunities for learning, Sørensen and Hallinan review several bits of evidence on between-school interaction effects in structural models of educational performance. No published studies test directly for between-school interaction effects in the autoregression coefficient, so they rely on weaker tests involving other coefficients in reduced-form equations. Aside from the research they cite, Hauser (1971:54-56) found small and inconsistent interactions between Nashville secondary schools in regressions of reading achievement and English grades on measures of ability and of social background. More recently, however, Jencks and Brown (1975:286) have reported negligible interaction effects between lagged test scores and environmental variables including curriculum, region, urbanism, SES, and college plans in their reanalyses of the Project Talent data.

Sørensen and Hallinan's own analyses of the Project Talent data offer no more than equivocal support for the hypothesis that school characteristics interact with the persistence of academic achievement. As they describe the pattern of *b*'s in Table 2, the findings are less than convincing. Moreover, it is not clear that the reported differences in slopes are statistically significant. No statistical tests for interaction are reported, but I am willing to extrapolate from the results of Table 1. Based on about 700 observations, Sørensen and Hallinan report an *F*-statistic of 98.8 for the regression of 12th grade on 9th grade mathematics achievement. That implies a *t*-statistic of  $\sqrt{98.8} = 9.94$  which says the standard error of the autoregression coefficient in the full sample is  $.634/9.94 = .064$ . Thus, a 95 percent confidence interval for the autoregression coefficient is approximately  $.634 \pm 1.96(.064)$  or  $.509 \leq b^* \leq .759$ . Translating this result into the metric of Table 2 (by taking natural logs), I obtain the approximate 95 percent confidence interval  $-.675 \leq b \leq -.276$ . Considering the larger number of cases in the full sample, this confidence band seems large relative to the range of

autoregression coefficients reported in the first column of Table 2 (for the same mathematics information test).

There are other respects in which Sørensen and Hallinan overlook tests of their model which might be carried out with the Project Talent data. The model implies temporal increases in the effects of exogenous variables in reduced-form equations (excluding lagged achievement). While they report analogous findings in another body of data, they do not report reduced form coefficients in the Project Talent data at the 9th or the 12th grades. However, the model carries an even stronger implication for the reduced form coefficients, namely, that the coefficients of the several exogenous variables increase over time in the same proportion (see my equation 2 or Sørensen and Hallinan's equation 11). This type of cross-equation constraint may readily be tested (Hauser and Goldberger, 1971). Also, the model implies that the intercept will be zero in Sørensen and Hallinan's equation 8. That is, conditional on the lagged value of the endogenous variable, growth in achievement is predicted by the exogenous variables. Again, there is a straightforward statistical test of this hypothesis.

Sørensen and Hallinan note correctly that their results may be particularly vulnerable to errors of measurement in achievement, and they further mention the possibility of estimating the effects of random measurement error if there be three waves of measurement of the endogenous variable. That possibility, however, depends on one's willingness to accept the specification of no delayed effects of the endogenous variable; there may be better ways to use the information in a third wave than to estimate components of random measurement error. In any event one need not wait for third measurements to estimate the influence of measurement error; for example, Jencks and Brown (1975) make some use of estimated test reliabilities in their analyses of the 9th to 12th grade Project Talent panel.

Finally, in their agenda for testing and extending their model Sørensen and Hallinan give short shrift to some of its most salient features. (Again, it may be helpful to look at Figure 1.) Is the learning process lag-one in the endogenous variable, or can much earlier achievements affect contemporaneous learning directly? My guess is that might depend on the heterogeneity and on the cumulative character of the subject matter to be learned. Is the autoregression of achievement ( $b^*$ ) constant over time? That is, does learning persist to the same degree among novices and near-experts? Similarly, why do exogenous variables like ability and social

background have temporally constant lagged effects? If the learning process is lag-one in the endogenous variable, what are the mechanisms by which the effects of less proximate variables persist? This last assumption (temporally constant  $c^*$ ) is very important, as we have seen, because the  $c^*$ , as well as  $b^*$ , affect the ultimate level of achievement. Each of these features of Sørensen and Hallinan's model strikes me as more central to its validity than, say, the possibility that autoregressions of achievement variables shift from school to school. Further, I believe each of these features of the model may be tested if achievements be measured at three or more times. I will be the first to admit that the proof of the pudding is in the eating.

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#### REPLY TO STEINBERG

To restate it, the issue is simply this: the assertion that religious groups are over- or under-represented in higher education *necessarily* implies a comparison between the proportions of religious groups holding faculty positions and the proportions of these religious groups in the general population. By this criterion, Catholics are under-represented and Jews are over-represented. The question then becomes whether or not this situation is changing. Since longitudinal data have not been available (until very recently), Steinberg (1977) rightly chose to examine cohort differences. But, inexplicably, he chose to look only at cohort differences among faculty, ignoring cohort differences in the general population. To ignore these differences is indefensible