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A SIMULATION STUDY OF THE USE OF CHANGE MEASURES TO COMPARE EDUCATIONAL PROGRAMS

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James M. Richards, Jr.

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Introductory Statement

The Center for Social Organization of Schools has two primary objectives: to develop a scientific knowledge of how schools affect their students, and to use this knowledge to develop better school practices and organization.

The Center works through three programs to achieve its objectives. The Schools and Maturity program is studying the effects of school, family, and peer group experiences on the development of attitudes consistent with psychosocial maturity. The objectives are to formulate, assess, and research important educational goals other than traditional academic achievement. The School Organization program is currently concerned with authority-control structures, task structures, reward systems, and peer group processes in schools. The Careers program (formerly Careers and Curricula) bases its work upon a theory of career development. It has developed a self-administered vocational guidance device and a self-directed career program to promote vocational development and to foster satisfying curricular decisions for high school, college, and adult populations.

This report, prepared by the School Organization program, examines methods of assessing the effectiveness of schools and educational programs in promoting educational growth of students.

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Abstract

Artificial data were used to assess the correlation between several estimates of average student change in various schools and the "true" impact of those schools. Results indicate that all estimates involving pretest-posttest differences measure school impact with reasonable accuracy. It is important to measure change over the entire course of learning, however, and not just over the later stages of learning. The correlations between change scores and other school characteristics reflect with reasonable accuracy the relationships between those characteristics and impact, but will be large only when the underlying relationships are substantial. Simple gain scores measure the true situation about as accurately as other change estimates, are easier to compute, and probably are more meaningful to non-researchers.

Introduction

A basic purpose of education is to promote desirable change or growth in the educational attainment of students. It follows that schools or other educational programs should be evaluated largely on their effectiveness in promoting such change. There are many theoretical problems in estimating student change from scores on standard tests of educational attainment, however, and these problems are heightened in the typical situation where the students entering various schools differ systematically (Astin and Panos, 1971; Cronbach and Furby, 1970; Harris, 1963; Herriott and Muse, 1973; Klittgard and Hall, 1973; O'Connor, 1972).

It has been difficult to assess the practical importance of these theoretical problems because true change scores are unknown in most longitudinal research. Recently, a computer procedure was developed to provide artificial data in which these true change scores are known (Richards, Karweit, and Prevatt, in press). When such artificial data were used to compare several statistical techniques for assessing change in individual students (Richards, 1974), the results indicated that individual change is measured with reasonable accuracy by all techniques that involve the difference between the pretest and the posttest. In particular, the simple difference between the pretest and the posttest is about as accurate as other change estimates, such as regressed gain scores, and is much easier to compute than other estimates. These trends hold even when students are assigned nonrandomly to schools that differ in their impact on students.

These results strongly suggest that the theoretical problems of change measures have limited practical significance for measuring individual growth, and it is important to determine whether this is also the case for measuring school impact. Accordingly, in this study artificial data were used to assess the correlation between several estimates of average student change in various schools and the "true" impact of the same schools. This study is stated in the context of education, but the procedures for generating data and measuring change are abstract. Therefore, the results should generalize to many situations where one wishes to compare the impact of varying social interventions.

Method

Simulation Procedure. Because it seems desirable for artificial data to resemble real data as closely as possible, the computer procedure was designed (Richards, et al., in press) to reproduce selected aspects of the ETS Growth Study (Hilton, Beaton, and Bower, 1971) and of the Project TALENT study of high schools in the United States (Flanagan, et al., 1962). In the ETS Growth Study, students were assessed initially with a measure of academic potential (SCAT) and a measure of educational attainment (STEP). Subject to the usual attrition in longitudinal research, the educational attainment of these students was reassessed on three subsequent occasions. Project TALENT provided intercorrelations among a variety of community, school, and student characteristics for a representative sample of U. S. high schools.

The computer procedure generates scores for individual students that strive to reproduce the means, standard deviations, and intercorrelations obtained in the ETS Growth Study. The student's score on academic potential is generated first and used to derive that student's score on initial academic attainment. Then gain scores are generated and added to yield subsequent attainment scores. True standard scores are generated initially, then the appropriate amount of random error is added to each score and the scores are transformed to the metric of the ETS Growth Study observed scores. This simulation procedure closely reproduces the ETS Growth Study results (Richards, 1974).

The simulation procedure permits the investigator to assign students to schools either randomly or nonrandomly. When students are assigned nonrandomly, the program strives to reproduce the average correlation between community per capita income and average academic potential of students estimated from Project TALENT results ($\rho = .54$). The ratio of between schools variance to total variance also simulates the Project TALENT ratio.

The simulation procedure assumes that community per capita income determines school resources, and that school resources in turn determine school impact. A review of Project TALENT results suggested an average correlation of approximately .25 between community income and those school resources commonly assumed to facilitate student growth, so the simulation procedure strives to reproduce this relationship between income and resources. Community income is drawn randomly from a normal distribution, and it is assumed that school resources and school impact also are normally distributed.

There is little empirical basis for estimating either the correlation between resources and impact or the extent to which schools vary in impact. Therefore, the simulation procedure allows the investigator to specify both the correlation between resources and impact and the standard deviation of the impact variable. This standard deviation is specified in the form of a number between _0_ and _1_. When the standard deviation is .10, the average growth values used in generating scores are equal to the average growth scores obtained in the ETS study for a school with average impact, and are 10% higher than the ETS averages for a school one standard deviation above the mean on impact. (The simulated data appear to meet the assumptions for this manipulation even if the ETS data do not.)

Gain scores for individuals are generated according to the following principle:

$$G_t = G_m + G_d$$

where G_t is total (true) growth, G_t is average (or mean) growth (i.e., the parameter estimated from the ERS data) and G_d is a deviation from this average that represents individual differences in true growth. The total gain score is added to the pretest score to yield the posttest score, and the posttest score then becomes the pretest for the next growth interval. For each growth interval, the pretest is one of the elements entering a multiple regression formula used to generate the

G_d values. The correlations between pretest and growth become increasingly negative for successive intervals (Richards, 1974).

In generating scores, the mean growth parameters for the three intervals are adjusted for school impact, and no other changes are made. Consequently, the adjusted mean growth parameters frequently will not be equal to the obtained average true growth scores for a given school. A school with above average impact will have higher than average mean growth parameters and therefore higher than average true posttest scores. These become higher than average true pretest scores for subsequent learning intervals, and these higher pretest scores make an increasingly negative contribution in the computation of subsequent true growth scores. The averages of the obtained true growth scores for that school will tend to be lower than the adjusted mean growth parameters. Similarly, the averages of the obtained true growth scores will tend to be higher than the adjusted mean growth parameters for a school with below average impact. Table 1 presents a simplified illustration of these trends for five hypothetical schools that are average in every respect except for differing in impact. Because other parameters besides pretest score are involved

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Insert Table 1 About Here

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in generating scores (Richards, 1974), it is conceivable that a school with below average impact (and therefore below average adjusted mean growth parameters) will have higher average obtained true growth scores than a school with above average impact. This is especially true when students are assigned to schools nonrandomly.

Data Sets. Six independent sets of simulated data were generated for the present study. In each set students were assigned to 100 schools or treatments. The number of students per school varied randomly with mean = 150 and standard deviation = 15. Therefore, the total number of students in each of these sex sets was approximately 15,000.

In three of these sets students were assigned randomly to schools or treatments, and in the other three sets students were assigned nonrandomly. Under each type of assignment, simulated data were generated for three different assumptions about the relationship between school resources and school impact. Specifically, it was assumed that school resources account for 5%, 20%, or 80% of the variance in school impact (corresponding to correlations of .2236, .4472, or .8944).

Finally, in all six sets the standard deviation of the impact variable was set at .10. At approximately this magnitude two simulated schools one standard deviation apart on impact (with N's = 150) will differ at the .05 level when compared with respect to educational growth between successive occasions.

Change Measures. A wide variety of change measures have been proposed (Cronbach and Furby, 1970), but recent results suggest that most of these measures yield essentially equivalent results (Richards, 1974). Accordingly, this study used only four measures of change, each representing a different approach to estimating change. These change estimates included:

1. Posttest score.
2. Posttest score adjusted for initial academic potential. This change estimate is the difference between posttest score and

predicted posttest score, using initial academic potential as the predictor. (The prediction equation for each data set was based on the observed relationships in that set.) Thus, this technique resembles analysis of covariance with academic potential treated as the covariate.

3. Raw gain. This change score is the simple difference between pretest score and posttest score.
4. Raw residual gain. This estimate is the difference between posttest score and predicted posttest score, using pretest score as the predictor.

Results

To facilitate comparison with the earlier study of individual change estimates (Richards, 1974) the first step in the data analysis was to compute the correlations between average estimated change scores for various schools and average true change scores for the same schools. An unresolved question is whether it is better to compute change scores for individual students and then average within schools or to compute change scores from school means (Dyer, Linn, and Patton, 1969), so both procedures were used to estimate change in this analysis. Table 2 summarizes the results.

Insert Table 2 About Here

These results seem quite consistent with the results of the earlier study of individual change estimates (Richards, 1974). Change is estimated most accurately by techniques that involve the difference between the pretest and the posttest, and these techniques seem equally accurate (i.e., raw gain is just as accurate as residual gain). For the most part, there is little difference between change estimates based on individual students and change estimates based on school means. In a few cases estimates based on school means have a clear advantage and these estimates are also easier to compute, so subsequent analyses in this paper involve only estimates based on school means.

The next analysis evaluated the accuracy of these change estimates as measures of school impact. Table 3 summarizes the correlations between impact and various change estimates. For comparative purposes, this table also summarizes the correlations between impact and average true growth scores.

Insert Table 3 About Here

These results indicate that change estimates can be quite effective in rank ordering schools with respect to their impact even when students are assigned to schools nonrandomly. The simple gain scores again were just as accurate as the residual gain scores and, as Cronbach and Furby (1970) point out. posttest score measures impact adequately when students are assigned to treatments randomly.

The results also indicate that it is important to measure change over an appropriate interval. Adjusted posttest scores, simple gain scores, and regressed gain scores all rank ordered schools accurately when they involved change from initial status, but none of the measures were particularly effective in rank ordering schools when they involved growth in the later stages of the learning process. This ineffectiveness reflected the true situation, because it is also characteristic of the true growth scores. The ETS data resemble other longitudinal or learning data in a number of respects (Richards, 1974), so these findings about <u>when</u> to measure change should have considerable generalizability.

The final question examined in this study involves the relationships among these change measures and the school characteristics that cause variations in impact. Such results are more typical of what would be obtained in a "real" longitudinal study. Table 4 summarizes the relevant correlations between resources and change. The magnitudes of these correlations clearly follow the underlying relationship between resources and impact, but are somewhat lower. The smaller magnitude of these

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Insert Table 4 About Here

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correlations perhaps is partly the consequence of unreliability of the change scores, but also appears to reflect the imperfect correspondence between school impact and average true change. The results again indicate that raw gain is about as accurate as any other change estimate, reemphasize the importance of measuring change over an appropriate interval, and suggest that the correlation between a school characteristic and school impact must be reasonably substantial before <u>any</u> change score will reveal the relationship.

Discussion

Theoretical treatments of the issues considered in this paper have emphasized the theoretical difficulties of using change scores in general and of using simple gain scores in particular. The results of this study, like those of the earlier study of individual change (Richards, 1974), suggest that the practical importance of these theoretical difficulties may have been exaggerated. It appears that change estimates over an appropriate interval (e.g., the entire course of learning, not just the later stages) do measure school impact with reasonable accuracy. The correlations between change scores and other school characteristics reflect with reasonable accuracy the relationships between the same characteristics and school impact, but consequently will be large (or "significant") only when the underlying relationship is fairly substantial. These conclusions appear relatively unaffected by random vs. nonrandom assignment of students (although this finding could change for more severe nonrandomness), or by whether change measures involve individual scores, or school means.[1]

Insensitivity to weak relationships almost certainly is characteristic not just of change scores, but of all statistical procedures that might be applied to these data, and simple gain scores appear to reflect the true situation about as accurately as any other estimate of change or impact. Simple gain scores also are easier to compute than most other estimates and probably are more meaningful to non-researchers. Therefore, the results of this study suggest that it often may be quite appropriate

[1]It should be emphasized that these conclusions apply to true longitudinal designs and this study should not be used to justify such procedures as measuring impact by educational attainment adjusted for a test of academic potential administered at the same time.

to compare educational programs on the basis of simple pretest-posttest differences.

The discrepancy between this study and earlier theoretical treatments may perhaps best be resolved in terms of degree of concern about "Type I" errors. That is, theoretical treatments usually seem to assume that educational treatments do not differ on impact and emphasize the possibility that use of change scores, particularly simple gain scores, will lead to the false conclusion that they do differ. Certainly this possibility cannot be ignored, especially when the students assigned to various treatments differ considerably (Astin and Panos, 1971; Cronbach and Furby, 1970), and certainly it is possible to propose hypothetical situations where change scores could be misleading or confusing, especially if one has a taste for paradoxes (Lord, 1967). This study, on the other hand, assumed that schools do differ on impact and asked how accurately change scores describe these differences. The answer to this question appears much more favorable to change scores. Indeed, the results suggest that when one uses change scores over an inappropriate interval in a correlational study there may be a greater danger of the false conclusion that schools do not differ with respect to impact than of the false conclusion that schools do differ.

Cronbach and Furby (1970) correctly point out that some of the questions to which change scores might be applied could be answered more directly with such techniques as partial correlation. The advantages of such techniques are that they are more direct than change scores, however, not that they are more accurate, nor that they require less statistical sophistication. The results of this study lend support to the investigator who prefers to use change scores for reasons of convenience or ease of understanding.

Finally, the results of this study again illustrate the usefulness of simulation techniques for investigations of longitudinal methodology. It would be impossible to investigate the questions considered in this study with "real" longitudinal data because the investigator would have no way of knowing either the true individual growth scores or the true school impact scores. At best one could compute the intercorrelations among different estimates of change (Dyer, et al., 1969). With simulated data it was easy to compute the correlations between true scores and the different estimated scores. It would also be easy to extend the simulation procedures to the situation where considerable attrition of subjects occurs, to the situation where one has only pseudo-longitudinal data (e.g., test scores for Occasions 1 and 2 obtained from different groups of students in the same school), or to different models for growth. Thus, simulation techniques offer considerable promise for refining our knowledge about when various procedures for analyzing longitudinal data are appropriate.

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Table 1

Comparison of Adjusted Mean Gain Parameters With Average of Obtained True Gain Scores for Hypothetical Schools Varying in Impact

School	School Impact	Adjusted Mean Gain Parameter For Computing True Gain Scores in Interval:			Average of Obtained True Gain Scores in Interval:		
		Occasions 1 to 2	Occasions 2 to 3	Occasions 3 to 4	Occasions 1 to 2	Occasions 2 to 3	Occasions 3 to 4
1	1.20	1.0392	.9852	.7812	1.0392	.9277	.6834
2	1.10	.9526	.9031	.7161	.9526	.8744	.6672
3	1.00	.8660	.8210	.6510	.8660	.8210	.6510
4	.90	.7794	.7389	.5859	.7794	.7676	.6348
5	.80	.6928	.6568	.5208	.6928	.7143	.6186

NOTE: Gain scores are expressed in units of the true score standard deviation for Occasion 1. It is assumed these schools are average in every respect except for differing in impact. Values for school 3 are identical with those estimated from the ETS Growth Study.

Table 2

Correlations Between Average True Gain in Different Schools

and Various Estimates of Average Change

(N's = 100)

		Assignment of Students											
		Random						Nonrandom					
		Average True Gain in Interval:						Average True Gain in Interval:					
		1 to 2		2 to 3		3 to 4		1 to 2		2 to 3		3 to 4	
Change Estimate	Correlation Between Resources and Impact	I	M	I	M	I	M	I	M	I	M	I	M
Posttest Score	.2236	72	72	69	69	42	42	47	47	45	45	00	00
	.4472	77	77	80	80	36	36	54	54	43	43	11	11
	.8944	70	70	75	75	42	42	50	50	38	38	-03	-03
Posttest Score Adjusted For Initial Academic Potential	.2236	81	80	72	72	46	46	80	78	73	69	33	47
	.4472	86	87	86	85	37	37	88	82	82	81	41	49
	.8944	85	85	80	80	49	49	81	76	75	76	26	38
Raw Gain	.2236	90	90	76	76	71	71	90	90	77	77	65	65
	.4472	89	89	87	87	67	67	91	91	80	80	70	70
	.8944	88	88	77	77	73	73	86	86	79	79	61	61
Residual Gain	.2236	90	90	80	63	70	73	79	84	69	70	22	65
	.4472	90	90	88	71	65	68	85	83	70	75	35	70
	.8944	88	88	82	65	73	75	80	80	65	74	19	61

NOTE: Numbers in columns labelled I show correlations between average true gain and estimates based on individual students. Numbers in columns labelled M show correlations for estimates based on school means. Decimals are omitted. For this and subsequent tables r_{es} = .14 and r_{oi} = .18.

Table 3

Correlations of School Impact with Change Measures and Estimates
(N's = 100)

	Correlations Between Resources and Impact	Assignment of Students: Random — Change to Occasion 2	Random 3	Random 4	Nonrandom — Change to Occasion 2	Nonrandom 3	Nonrandom 4
Change Measured By:							
Average True Gain From Occasion 1	.2236	.87	.92	.90	.86	.92	.94
	.4472	.90	.95	.94	.88	.93	.94
	.8944	.89	.94	.95	.83	.90	.92
Average True Gain From Just Previous Occasion	.2236	.87	.71	.18	.86	.71	.35
	.4472	.90	.82	.21	.88	.82	.46
	.8944	.89	.76	.35	.83	.75	.33
Change Estimated By:							
Posttest Score	.2236	.64	.77	.77	.27	.34	.37
	.4472	.71	.83	.84	.31	.39	.43
	.8944	.63	.74	.80	.22	.31	.35
Posttest Score Adjusted For Initial Academic Potential	.2236	.69	.83	.80	.73	.84	.87
	.4472	.80	.90	.90	.80	.87	.91
	.8944	.78	.85	.88	.71	.83	.87
Raw Gain From Occasion 1	.2236	.77	.85	.85	.79	.85	.89
	.4472	.78	.89	.88	.82	.89	.92
	.8944	.81	.86	.90	.71	.85	.89
Raw Gain From Just Previous Occasion	.2236	.75	.53	.12	.79	.54	.26
	.4472	.78	.70	.14	.82	.66	.36
	.8944	.81	.57	.18	.71	.66	.30
Residual Gain From Occasion 1	.2236	.77	.84	.85	.78	.85	.89
	.4472	.79	.88	.87	.82	.89	.92
	.8944	.80	.85	.90	.70	.86	.88
Residual Gain From Just Previous Occasion	.2236	.77	.31	.30	.78	.48	.25
	.4472	.79	.43	.21	.82	.61	.34
	.8944	.80	.40	.30	.70	.63	.39

Table 4

Correlations of School Resources with Change Measures and Estimates

(N's = 100)

		Assignment of Students					
		Random			Nonrandom		
	Correlations Between	Change to Occasion			Change to Occasion		
Change Measured By:	Resources and Impact	2	3	4	2	3	4
Average True Gain From Occasion 1	.2236	.20	.15	.13	.22	.21	.18
	.4472	.38	.42	.42	.50	.52	.50
	.8944	.78	.83	.83	.74	.82	.85
Average True Gain From Just Previous Occasion	.2236	.20	.03	-.04	.22	.12	-.01
	.4472	.38	.39	.09	.50	.45	.16
	.8944	.78	.69	.29	.74	.72	.31
Change Estimated By:							
Posttest Score	.2236	.11	.06	.08	.22	.23	.22
	.4472	.23	.33	.34	.36	.39	.41
	.8944	.53	.66	.69	.21	.29	.33
Posttest Score Adjusted For Initial Academic Potential	.2236	.07	.15	.08	.15	.14	.11
	.4472	.28	.37	.38	.42	.42	.45
	.8944	.65	.75	.75	.66	.78	.80
Raw Gain From Occasion 1	.2236	.21	.12	.14	.21	.20	.17
	.4472	.31	.39	.39	.48	.47	.50
	.8944	.68	.76	.78	.66	.78	.80
Raw Gain From Just Previous Occasion	.2236	.21	-.06	.05	.21	.10	-.02
	.4472	.31	.34	.07	.48	.28	.22
	.8944	.68	.54	.12	.62	.63	.24
Residual Gaim From Occasion 1	.2236	.05	.21	.12	.17	.15	.12
	.4472	.31	.40	.39	.41	.41	.44
	.8944	.68	.76	.78	.62	.78	.79
Residual Gain From Just Previous Occasion	.2236	.05	.13	-.02	.17	.04	-.02
	.4472	.31	.26	.09	.41	.21	.20
	.8944	.68	.40	.23	.62	.59	.32