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ABSTRACT

In the most narrow sense, compensatory education has failed; most Title I programs which seek to improve achievement have no discernible effect. This is apparently a result of the fact that the programs seek to provide for disadvantaged students more of the school resources which have never been found to affect the achievement of advantaged students. It is, on the other hand, clear that compensatory programs need not fail; highly structured preschool programs aimed at language development produce impressive short run IQ gains. The most important difference between these experiments and most other preschool programs is that the former were aimed narrowly at skills clearly related to what tests of IQ and verbal ability happen to measure. In addition, there are other models of what might be called "compensatory" education--at least their object is to redistribute educational outcomes, and to improve life chances of children from disadvantaged circumstances. These programs however, focus on more distant outcomes of schooling--college entrance and high school graduation. The difficulty with both approaches is that they rest on an extremely limited evidence about the process of schooling. For example, we have no evidence that IQ affects life chances though if it does cause longer school retention, it may. While most standard compensatory programs have failed to affect achievement, there was never any evidence that much else could have been expected. (Author/JM)

THE SCHOOLS AND SOCIAL REFORM: THE CASE OF COMPENSATORY EDUCATION

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I. INTRODUCTION

For the last two or three years public education has been mired in disaster, conflict, and failure. The passage of the 1964 Civil Rights Act lent renewed force to the drive for school desegregation, but within a few years many black leaders were questioning integration. The inception of Project Headstart and Title I of the 1965 ESEA stimulated efforts to improve education for poor children, but by 1969 it had been officially announced that both programs were failures. While the early and middle 1960's were spent searching for ways in which schools could remedy discrimination and poverty, the decade's last years were spent searching for ways to preserve some minimal semblance of order in the schools.

The reasons for this extend far beyond the failures--real or imagined --of public education, but the crisis is due in no small part to the idea that the schools have been unable to improve education for poor children. Is this true?

In one sense, of course, it is. The "failure" is by now not only comfortably ensconced in the public mind, but it is firmly bound up with other current notions, including the "necessity" for decentralization and the "impotence" of social programs. This is a situation which contrary facts--if indeed they exist--would be unlikely to alter. Nonetheless, the facts would be useful. At least they could help us to see clearly just what the schools have done or might do, and to decide whether the many inferences from their "failure" are warranted.

II. PROGRAM AIMS

The place to begin is where the programs do, with some idea of their objectives, and what these imply about schools and children. This is less easy than it seems. Improving school performance, after all, has not been the sole aim of such endeavors as Title I, and--if we think of Congressional intent--perhaps not even their main purpose. In addition, the programs have often been carried out with astonishing clumsiness: typically the amount of money spent on individual children is trivial, and in many cases the funds have simply been misspent entirely.[1] Despite all this, better education was an important aim of these efforts, and not all the funds were badly used.

What do these programs seek, in the way of improved school performance? For the most part, local Title I projects announce improved reading or mathematics achievement as their principal goal, but this is far from the only program aim. Many of the reading or mathematics programs also seek to improve students' self-concept, and a small proportion focus entirely on this problem. Others claim to be concerned mostly with truancy or school retention, and still others aim at improved school-community relations, or greater parent involvement. In fact, one really remarkable aspect of local Title I programs is the diversity of aims most projects choose to announce. They seek nothing less than to eliminate the educational problems of disadvantaged children, and thus it is not surprising that they are characterized by a multiplicity of aims, many of them global.

But this diversity is only skin deep. Whether it is test scores or truancy, the real (and avowed) aim of Title I programs is to eliminate inequalities in educational opportunity. Almost uniformly this is taken to

imply eliminating race or class disparities in the outcomes of schooling. What is more, the program materials envision this not as an end in itself, but as a means to another more important end--the elimination of poverty. Thus the programs differ only in their conception of the mechanisms which will lead to better school performance. They are as one in the view that improved performance is important solely because it will lead to more education, better jobs, more income, and less poverty.

Title I programs, then, make several important assumptions about schooling and its situation in the social order. One is that school performance has a direct causal impact on adult social and economic status. Another is that the chief obstacle to eliminating poverty is the inadequate training, or bad manners of the poor. The first point rests on well-established ideas about education; ever since the turn of the century, when Americans began to see their society as one built on technology, schooling has been regarded as the crucial determinant of adult success. And while there is less agreement on the second point, the main drift of opinion has been that stupidity and bad manners were the consequences of poverty, rather than the results of selection of the fittest. It follows from both assumptions that if deprivation and discrimination were eliminated, those with ability could rise as far as their talents would permit. The assumption was that there has been plenty of room at the top, and that schooling was the appropriate escalator.

The programs also assume, of course, that the schools can affect students' performance. As I indicated a moment ago, there are different views about <u>how</u> this occurs. Some programs assume that the main problem is students' discouragement over their poor chances in life; most hold that

the problem arises from cognitive deficits due to environmental deprivation, but still others assume that the problem lies in children's poor self-evaluations. Whatever the view, however, the educators who design and implement Title I programs assume that these difficulties can be repaired by the schools.

It will surprise no one to find that these ideas were not probed before Title I was launched. Had they been, the result would have been surprising, for all the available evidence runs contrary to the assumptions. While this would have had no effect whatever on the Congress or the Executive Branch (other, perhaps, than to provoke irritation at the perversity of social research), the evidence is well worth reviewing.

The availability of room at the top is doubtless the most important issue, but it is also the most difficult. There is, for example, plenty of evidence that occupations at the top of the social structure have been expanding steadily since the turn of the century. Not only are there more white-collar jobs, they have been a growing proportion of the labor force. But this only proves that there are more good jobs--the question is who gets them? If the patterns of social inheritance are such that they are virtually closed to all but the children of professional and technical workers, the abundance of jobs means absolutely nothing. Though there is little research on this point, the one extant study concludes that there is much greater recruitment to white-collar jobs from the ranks of white-collar than blue-collar workers:

> ...the movements of men starting careers on blue-collar levels effect little change in their ultimate occupational distributions compared to those of their fathers, whereas the movement of men starting on higher white-collar and farm levels achieve considerable improvements in their positions over those of their fathers.[2]

During the last two or three generations, then, recruitment to occupations at the top has taken place disproportionately from families at or near the top already. But this only proves that those at the bottom of the occupational ladder tend to stay there. It doesn't show that this occurred despite superior education. Does schooling tend to equalize life chances among broad social class groupings?

Unfortunately, there is no evidence directly on this point. Blau and Duncan present extensive evidence on the impact of schooling on mobility among the nearly one hundred occupations which sociologists survey in such studies. They find that educational attainment (years of school completed) has a stronger impact on occupational achievement than several measures of social inheritance (fathers' occupation and education).[3] The problem is that most of this movement is short-range, and may have little relation to movement across major class and occupational barriers. The question raised by the compensatory programs, after all, is whether education plays an important role in promoting movement among these major groupings, not whether it is associated with movement within social or occupational classes.

Thus, there is no way of knowing the relative importance of education and social inheritance in determining membership in social or occupational classes. There is some evidence, however, on the role of intelligence in occupational mobility. Duncan found that early intelligence was moderately

related to educational attainment, but that it had no direct impact whatever on occupational attainment. More precisely, IQ in the elementary school years accounts for roughly fifteen to twenty percent of the variation in educational attainment, but none of the variation in adults' occupational status.[4/] This means (1) that brightness is a relatively minor cause of how long people stay in school, and (2) that differences in the occupational status of people who stay in school equally long are not at all affected by differences in their IQ. This does not suggest that compensatory programs which seek to change adult status by changing IQ are on the right track.

The other point to bear in mind is that all this research and argumentation concerns the entire American labor force; compensatory programs, however, are aimed at children in segments of the society which either do not participate in the labor force or do so in a marginal way. Even if we had shown that there was room at the top and that education was the high road to mobility, this would not hold automatically for the poor or for blacks. The problem is particularly acute for Negro Americans. They are the principal target group of Title I programs in the larger urban areas, and they are identified nationally as the main object of compensatory treatment in the schools and elsewhere. Is there room at the top for Negroes? Does education "work" for them in the same fashion as it does for whites?

Merely to put the questions suggests the answer. Although summary measures of discrimination are not easy to come by, there is an enormous difference in the distribution of occupations for black and white Americans. Most studies suggest that roughly two-thirds of these gross differences cannot be accounted for by such "objective" measures of qualification as education

or training. The remaining differences are presumed to be due to discrimination. Moreover, the most sophisticated study on this point found that even when whites and Negroes in the same regions and with similar social and economic backgrounds were compared, blacks attained much lower levels in the occupational structure.[5] The differences are due chiefly to discrimination. Most important, however, is whether schooling is the solution to this problem--on this point, the study concludes that:

> ...Negroes have less incentive than whites to acquire an education and to make the serious sacrifices that doing so entails for persons from the underprivileged socio-economic classes...approximately the same amount of educational investment yields considerably less return in the form of superior occupational status or mobility to non-whites than to whites.[6]

Under these circumstances it is hard to see the special relevance of compensatory education programs for blacks; attacks on job discrimination, and programs of income maintenance, work training, and job creation would seem to be more appropriate.

It is important to point out, of course, that I have not provided an exhaustive account of the relation of schooling and adult achievement, nor have I shown that there is no room at the top. All that this brief review of the research suggests is that all the existing evidence is either ambiguous or contrary to the implicit assumptions of compensatory programs. Let me summarize:

> --IQ is apparently unrelated to occupational attainment, once variations in years of school completed are accounted for. This suggests that improving IQ or achievement scores is probably not an efficient way to eliminate disparities in occupational achievement.
>
> --There is no evidence that the main obstacle to adult achievement for black Americans is the lack of education; the real problem is discrimination, that is, the absence of room at the top.

> --There is no direct evidence about the role of schooling in promoting mobility across broad occupational and class boundaries; the notion that education is the chief prerequisite for such movement is nowhere confirmed by research on the subject.

It would be a mistake to conclude from this that schooling bears no relation to mobility among social and occupational classes. We do not know. We do know, however, that schooling is probably not related to adult status in the ways most compensatory programs assume--through improved performance on standardized tests of IQ of achievement.

What does one make of this conclusion? The fact remains that there is a rather strong association between IQ and years of school completed, even after the effects of inherited status have been removed. Does this mean that the people who complete more school do so <u>because</u> they are brighter? That is the easiest answer, but things are not all that clear. Perhaps the smarter people who complete more school do so because they also have been better socialized to expected school behavior, or because they are more obedient or better able to control their boredom. There also is a rather strong association between years of school completed and adult occupational status, and again it persists even after the effects of social inheritance have been removed. How is this explained? One line of argument is that there are other, nonacademic factors which cause this association: perhaps the people who stay in school longer are more diligent, more docile, more driven by a need to achieve, or in some other way better suited socially and emotionally to the entry requirements for "good" jobs. In some respects this is an attractive view. Almost everyone has known dull people who by dint of perseverance and good manners wound up higher in the occupational structure than others with bad manners, erratic temperaments, and twenty-five more IQ points. And even a passing acquaintance with the public schools suggests that they are inclined

to reward diligence, docility, and good manners more than ill-mannered and inconsistent intelligence. If jobs are handed out on the same lines as rewards in schools, the diligent and docile children who please their teachers will turn out to be the ones who please employers, and will be rewarded accordingly with the high-status jobs.

There are, however, several difficulties with this account, not the least of which is that docility and good manners are not the exclusive property of the professional classes. Another is that the attitudes one might expect to be important--such as need-for-achievement--seem to have little impact on adult status.[7] The major problem, however, is that all the evidence on these points is fragmentary, inconsistent, and incomplete. While non-academic factors may cause the observed relationship, we are far from any convincing evidence about it.[8] For the time being, then, we are left with the association between length of schooling and adult status, the evidence that it is not conditioned by IQ, and the ensuing perplexity.

It goes almost without saying that this offers little support for the ideas underlying compensatory programs. But the problem does not end here. As I mentioned earlier, all the projects supported by Title I assume that the schools can affect those attributes typically thought to be the agents of later achievement: test scores, self-concept, school retention, etc. Yet these assumptions were sharply challenged shortly after Title I and Headstart had been set in motion. In 1966 the U. S. Office of Education published the Equality of Educational Opportunity report which announced that differences among schools in the resources and practices they applied to children were related hardly at all to differences among schools in the children's performance on standardized tests of achievement or verbal ability. The only differences among schools which had an important impact on test

performance involved the students' social inheritance; children from advantaged homes entered school with an enormous lead on disadvantaged students (in terms of test scores), and they maintained or increased this lead through the twelve years of school.[9/]

This news was not received with wild enthusiasm either by the Office of Education or the various professions it represents. The report was treated as though it were some species of important leper; it could hardly be ignored, but then neither could it be welcomed with open arms. Educators and bureaucrats alike contented themselves with either attacking it (on the grounds that it was methodologically faulty, or that it measured the wrong things), or suggesting that since it was concerned only with the existing differences among schools, one could hardly make inferences about the effect of new remedial programs which went beyond these differences.

The really remarkable element in the report's reception, however, was neither the pain nor the anger with which it was greeted--it was the surprise. The entire affair would have given an innocent observer the impression that the Survey's findings ran counter to the results of decades of research on the effects of schooling. While decades of research did exist, the fact is that the EEOS report's conclusions were almost perfectly consistent with the results of this earlier work. Indeed, if one had gone through the previous research in order to estimate the probability that Coleman would have turned up important effects of schools on achievement, the resulting number would have approached zero.

The previous studies, of course, were hardly exhaustive. They consist mainly of small studies involving a few classrooms, several schools, or at

most a few school systems. But they did focus on many of the gross relationships usually assumed to make a difference to students' school performance; per pupil expenditures, class and school size, teachers' qualifications and the content of curricula. With very few exceptions, the results showed that the school-to-school differences in these characteristics were unrelated to differences in students' test performance. As J. M. Stephans has pointed out in a review of these studies, the consistently negative results became so well known among researchers that it was commonplace to refer to the findings without even so much as a cursory citation.[10/]

One could, of course, always argue that small studies of a few classrooms or schools would inevitably turn up small differences, since the schools and classrooms were almost always adjacent, and therefore probably quite similar. This objection, however, cannot be sustained in the case of results from Project Talent, a massive national study of American high schools underwritten by the U.S. Office of Education in the early 1960's. The Talent survey collected a great deal of information on student background and achievement, as well as schools' characteristics, programs, and facilities.[11/] Students were tested at grade nine and then again when they reached grade twelve, so that it was possible to ascertain the impact of differences in school resources on differences in what students learned during the four years of high school. Shaycoft undertook just such an investigation, with two principal results.[12/]

First, she found that the simple correlations (corrected for attenuation) between students' grade nine and twelve performance on tests of such

basic skills as reading comprehension and abstract reasoning ranged between .85 and .95. This means that between seventy and ninety percent of the variation of students' grade twelve performance was explained by variations in their grade nine performance.[13] As things stood in the early 1960's, then, the student "output" of high schools was little different from their student "input." In four years American high schools did almost nothing to change the relative ordering of achievement with which they began. Such findings provide little encouragement for the notion that schools affect achievement.

The second part of Shaycoft's investigation only confirmed this dismal result. Having shown that for the most part student performance on tests of basic skills was quite invariate over the high school years, she sought to determine if school characteristics had any impact on the remaining ten or twenty percent of the variance. Her conclusion was that the results "were inconclusive;"[14] apparently none of the school characteristics measured in Project Talent had any consistent association with the residual achievement differences. This result (which was published at about the same time as the EEOS) accorded perfectly with an earlier cross-sectional analysis of the grade nine scores.[15]

While these results would have discouraged most advocates of compensatory schooling, they would have come as no surprise to students of human development. In a study published in 1964, Benjamin Bloom reviewed the results of all extant research on the stability of individual intelligence over time. The simple correlation between IQ at age eleven and age seventeen (for the same individuals) was always found to be on the order of .90.[16]

This is perfectly consistent with the results from Project Talent. In fact, had any other result appeared in the Talent data it would have been astonishingly inconsistent with everything previously known about the relative stability of IQ.

Thus, were one to have formulated reasonable inferences from the pre-EEOS research on the effects of schooling, the two chief results would have been as follows:

> --The differences among schools which people usually suppose to be important--size of school, amount of budget, teachers' qualifications, and curriculum content--are not related to students' test performances;
>
> --Achievement and IQ test scores are a relatively invariate human characteristic (at least when the tests are well made), and they are highly related to indices of social inheritance and family background.

Now, none of this meant that schools couldn't "make a difference" to student achievement. All it meant was that the gross differences among schools usually thought to be important had no educationally significant impact on test scores. There may well have been other factors, more subtle than those measured in these studies, which did influence student achievement. Indeed, most of these unsuccessful studies argued that this was the case. (To date, however, no one has produced any such evidence.) The fact is, however, that the studies we have reviewed here measured those attributes of schools which usually figure prominently in debates over educational quality--money, qualifications, curriculum, and size. They also happen to be the school attributes which can be manipulated by public agencies. Indeed, money, qualifications, and size have been the subject of several decades' close attention by state and local education agencies. Enormous amounts of time and energy have been spent to eliminate small schools, to reduce class size, to increase

teachers' educational and professional qualifications and to upgrade curriculum. In every instance, the advocates of change argued that these innovations would benefit students. These debates have been the stuff of conflict over local tax levies, state equalization formulae, local hiring standards and state certification. Thus, if the pre-EEOS research did not prove that schools are incapable of making a difference (what empirical inquiry could?), it did suggest that those things typically thought to distinguish good from bad schools did not make good students out of bad ones.

The analyses reported in the EEOS--and the results of subsequent reanalysis of the data at Harvard--only confirmed this conclusion. Although the Survey measured school resources and practices in much more detailed fashion than either Project Talent or the earlier studies, there was no effort to go much beyond the sorts of gross characteristics measured in previous work. Chiefly as a result of the great haste with which the original report was prepared, its analyses were flawed in several respects. But reanalyses carried out at Harvard during the last three years--which corrected some of the methodological flaws--only strengthened its original findings.[17/] These results show, for example, no relationship between teachers' qualifications or experience and student achievement. Nor are there achievement differences associated with variations in per-pupil expenditures, class or school size, or curriculum or grouping policies. Indeed, none of these school attributes had more than a small zero-order correlation with achievement scores, and once school-to-school variations in students' social inheritance were removed, the associations vanished completely.[18/]

Much the same results can be produced with no reference to the school

resource measures, but simply by examining the relation between school "input" and "output" in the EEOS. Among the Northern urban schools, the correlation between school average achievement at grade one and grade six is in excess of .8; the same correlations between grade nine and grade twelve achievement are in excess of .95. These confirm Shaycoft's work: most of the differences among the "output" of elementary or senior high schools are accounted for by differences in their "inputs." Of course this relationship is not carved in stone; if schools were different the correlations might be smaller. But the important point is that as things presently stand, schools' output is very little different from their input. They do little to change the relative ordering of ability with which they begin.

Thus, the EEOS results reinforce and solidify the pattern apparent in earlier research. The Survey confirms the similarity between schools' input and output, and the fact that gross differences among school resources and practices have no impact upon student achievement. As the evidence stood in the mid 1960's, there was no empirical encouragement for the notion that schools could have much effect on students' achievement.

III. PROGRAM CONTENT

The obvious--and by 1966 well-worn--reply was that the existing differences among schools were either too small or too crude to affect achievement. Although this argument has the advantage of being applicable to negative findings of any sort or size (and is therefore indefinitely useful), it is not entirely persuasive. After all, the existing differences among schools cannot simply be brushed aside; they are differences among schools attended

by the non-poor--the differences that most Americans have been concerned about during the past four or five decades. If they are too small or too crude to affect achievement, what have the arguments been about for all these years? To argue that the existing differences among school resources and practices are too gross or crude to affect achievement is to implicitly agree that most of the educational "process" of the last forty or fifty years is a hoax.

Although most advocates of compensatory schooling probably wouldn't agree to that formulation, the programs do assume that the existing differences between education for poor and advantaged children are too small to affect achievement, and their results might reasonably be taken as a rough test of this notion. The overwhelming majority of them, for example, seem to assume that the main difference between the educational needs of advantaged and disadvantaged children is quantitative--the latter group is presumed to need more. These programs are all aimed at achievement (mostly reading), and they regard compensatory schooling as a substitute for cognitive stimulation presumed to be absent in deprived home and neighborhood environments.

The results of these efforts, however, are hugely unconvincing. Part of the problem is that most of the evaluations obscure any clear idea of what happened. (A recent review of exemplary programs, for example, found that the overwhelming majority of programs were evaluated by comparing test scores to national norms, rather than to the scores of a comparison or control group. Consequently it is impossible to tell if the program had any impact.)[19/] Another difficulty is that many programs diffuse funds so widely

as to add little to expenditures on any given pupil. But even if we confine ourselves to projects which concentrate substantial amounts of money on disadvantaged children most of the results are discouraging. This was demonstrated on a grand scale by the More Effective Schools Program in New York City. Per pupil expenditures were doubled or tripled, the ratio of teachers to students at least doubled, but there was no clear or consistent gain (over expectations) in students' achievement. The evaluation attributed this to the fact that while the conditions of instruction had changed--each student was exposed to much more educational resources--the character of the resources had not changed.[20/] Teachers were still doing the same things with fifteen children that they previously had done with thirty.

A few programs funded by Title I do seem to reveal achievement gains, although the evaluations are all questionable.[21/] Efforts to discern similarities among them, in order to determine if promising programs have common characteristics, turned up some similarities (continuity in staff and program over several years, for example), but none of them lay at the curricular or instructional level. The most important problem, however, is that most of the gains appear to have been clustered in the lowest quartile of the test distribution; this suggests that the gains may be nothing more than regression toward the mean.

Thus, while the reviews of Title I programs do not produce a totally bleak picture, they reveal no consistent evidence that the mere concentration of standard educational resources improves achievement. Most of the programs which take this approach produce no discernable gains, and among those that display gains there is serious doubt that the effects are genuine. If we

simply took the universe of resource-concentration programs and calculated the changes that any given program would succeed on the basis of the percentage that already have, the resulting probability would be extremely low. The results from these programs, then, are consistent with earlier research: gross differences among schools have little impact on achievement.

IV. ALTERNATIVE APPROACHES

But achievement is hardly the only possible outcome of schooling, and resource concentration is not the only imaginable way to affect it. Although Title I programs present several alternative outcomes (absenteeism, vandalism, self-concept, etc.) they are either unmeasureble, trivial, or unmeasured. Title I program evaluations are not the best place to search for alternative models of successful strategies.

How might such approaches be identified?

One useful way is to begin with the unsuccessful programs. Their most impressive and consistent characteristic is the global quality of both the program aims and the diagnosis of children's problems. Most assume an undifferentiated lack of cognitive stimulation in children and propose enrichment with standard (or slightly modified) curriculum materials and teaching techniques. Yet there is little evidence that the tests usually employed to measure IQ assess an undifferentiated range of cognitive skills, nor is there any evidence that undifferentiated enrichment (in the typical classroom sense of the phrase) is related to cognitive growth. Indeed, all the experimental evidence runs in just the other direction. The only preschool programs which present unimpeachable evidence of success in raising IQ for

disadvantaged children in classroom settings are those which focus narrowly on the verbal and linguistic abilities. The aims of these programs are narrow, and their techniques are also carefully fitted to the aims. They involved rigid classroom drill and specially designed curriculum materials, and they require fairly intensive teacher retraining.

Joan Bissell has surveyed the available evidence on these programs and reanalyzed the evaluation results.[22] She contrasted the effects of highly structured programs concentrating on language development, with the more permissive programs which focused on the traditional range of goals, especially enrichment. Her conclusions are worth quoting:

> Pre-school programs with general objectives of fostering cognitive growth, with specific emphasis on language development, and with teacher-directed strategies that provide highly structured experiences for disadvantaged children are more effective in producing cognitive gains than programs lacking these characteristics. Pre-school programs high on the dimension of quality control, having well trained staff, a high degree of supervision, and a low pupil-teacher ratio, are the most effective programs in producing cognitive gains.[23]

The results from this research clearly demonstrate that substantial IQ gains are possible with classroom programs. Although the results are ambiguous in some respects--the effects of the programs declined over the succeeding two years--they do show that schools can improve test performance. There is no reason to believe that these results could not be replicated in the primary grades.

These findings suggest that specific attacks on learning differences associated with variations in class culture and child-rearing can succeed. That notion is confirmed by the results of other experiments, in which aspects of parents' behavior were changed. In one case parents of preschool children

were trained to be more effective in a range of school-related activities, including teaching their children to read, helping with school work, etc. In another, this was augmented with more general competency training, which involved home management and related skills.[24/] In both cases the children showed marked improvement on IQ tests, over otherwise similar non-experimental pre-schoolers.

The underlying difficulty with these programs, however, is the same as the compensatory programs; they assume that IQ influences the probabilities of success after schooling is completed, and this notion rests on an extraordinarily insubstantial empirical foundation. It may be that raising IQ in the early school years increases the probability of completing more years of school, either because there is some intrinsic relationship between the two or because the students' successful participation in the program changes the expectations of all parties concerned. It also may be that nothing will change but the students' IQ's. Unfortunately we will never know the answers from the existing experiments. They are too small, and the conditions of the students' subsequent experiences too diverse in place and quality for follow-up studies to be helpful. Experimentation on a much larger and longer scale would be needed to find out if changes in early IQ have any social significance.

Another approach to finding promising strategies is to identify potentially important educational outcomes, and try to devise ways to redistribute them. College matriculation is one obvious example. Although in one sense most standard remedial programs are aimed in this direction, they do so by applying undifferentiated enrichment in an undifferentiated fashion to an un-

differentiated group of students; increasing their achievement is presumed to increase their chances of matriculation. Another reasonable approach would be to select the most promising students in high school, or late in elementary school, and encourage them to prepare for college simply by increasing the objective probability that they will attend. Although there are probably several ways to do this, the Upward Bound program seems to have hit on one of them. In these projects the most promising (and motivated) students are identified fairly late in high school, it is made clear to them that completing the program is nearly a guarantee of college matriculation (nearly eight out of ten who complete these programs gain admission to college). The students are provided with a variety of formal instruction: some is remedial, some is designed to familiarize them with university work, and some seems to be aimed simply at their morale and expectations. Upward Bound graduates have a much higher rate of college entrance than would otherwise be expected, and they seem to drop out less frequently than average.

Another educational outcome of possible importance is high school completion; it is not hard to imagine analogous ways to increase its likelihood for disadvantaged students, and in fact there appear to be several types of successful dropout recovery programs.[25/] Some are aimed at bright but disaffected students, and seem to consist of nothing more than providing a more interesting and unusual academic fare than can be found at the typical high school. Others are aimed at students with minimal academic skills, in which case the crucial element appears to be a remedial program which is integrated with a job.[26/] While these programs come nowhere near the scale of Upward Bound, they do seem to succeed.

These programs do not present precisely the same problems as the preschool projects. It is true almost by definition that a work-study program for dropouts which provides both a high school diploma and a job has a positive effect on the adult outcomes of schooling. The same thing can be said of the Upward Bound programs. If it is likely that the long-range effects will be less impressive than the immediate ones, it is hard to imagine that students' life chances will not have been substantially improved.

The really important question is why. Is it because changes in the objective probabilities of success alter expectations, and therefore behavior? If this were the case, presumably a program like Upward Bound would have roughly the same effect on less able students. An analogous experiment would be to identify promising students at the end of elementary school and enter them in the academic curriculum of high school, rather than allowing them to slip into some other track. Or is it because both types of programs are highly selective, and simply offer an opportunity to students who--were all else equal--would achieve the same end through the usual channels? The only way this idea could be tested (and then only in part) would again be to offer an opportunity like Upward Bound to students of a wide variety of ability. In either event, the questions underscore our ignorance about the process of schooling. We simply do not know whether educational attainment (clearing the certification hurdles) is chiefly a function of inherited status and the attendant differences in both objective assistance and expectations it generates, or whether the selection system does involve intellectual and psychological criteria which are at least in

part independent of social inheritance, and which do have some genuine relationship to performance later in life. Some of these questions could be explored if a somewhat more experimental approach to a program such as Upward Bound were adopted, but there is little evidence that this will occur.

V. CONCLUSIONS

Has compensatory education failed?

In the most narrow sense, the answer is affirmative. Most Title I programs which seek to improve achievement have no discernible effect. This is apparently a result of the fact that the programs seek to provide for disadvantaged students more of the school resources which have never been found to affect the achievement of advantaged students. It is, on the other hand, clear that compensatory programs need not fail; highly structured pre-school programs aimed at language development produce impressive short run IQ gains. The most important difference between these experiments and most Title I programs (or, for that matter, most other preschool programs), is that the successful experiments were aimed narrowly at skills clearly related to what tests of IQ and verbal ability happen to measure. That is, the programs rest on a body of knowledge which at least approximates a model of the learning measured by the tests. Given the relative plasticity of IQ prior to age ten or twelve, there is every reason to believe that similar programs at the primary level would yield similar results.

In addition, there are other models of what might be called "compensatory" education--at least their object is to redistribute educational outcomes, and to improve the life chances of children from disadvantaged circumstances. These programs, however, focus on more distant outcomes of schooling--

college entrance and high school graduation. They differ from the preschool and compensatory programs because they seek to affect these outcomes directly, by visibly altering the objective probabilities of success, rather than by affecting them indirectly by raising achievement or IQ.

The difficulty with both approaches is that they rest on an extremely limited evidence about the process of schooling. We have no evidence that IQ affects life chances, though if it does cause longer school retention, it may. Nor do we understand why programs like Upward Bound succeed. Is it because they simply select students who would have done well anyway (were it not for the consequences of discrimination), or because within the "normal" range of human ability, anyone who has the opportunity can succeed? If the first is true, it might be argued that the school selection system rests on criteria which bear some intrinsic relation to later achievement. If the second is true, it could be argued that the school selection system rests on a series of arbitrary conventions which only work to the advantage of those who could expect success anyway. Without a good deal more evidence, it is difficult to know which account is more likely, and for that reason, impossible to arrive at firmly grounded compensatory strategies.

Thus, while most standard compensatory programs have failed to affect achievement, there was never any evidence that much else could have been expected. Nor is there any evidence that the life chances of poor children would be different if they had succeeded. Although it is easy to imagine the experimentation and research on the process of schooling which would illuminate these issues, there is little evidence that either will be undertaken. Instead, the majority of compensatory programs will probably continue to "fail" at a task whose significance is unknown, and other programs, like Upward Bound, will succeed for reasons no one understands.

FOOTNOTES

1. A full discussion of the diversity of aims and problems of execution in these programs can be found in Cohen, D., "Politics and Research: The Evaluation of Social Action Programs in Education," Review of Educational Research, April, 1970.
2. Blau, P., and Duncan, O. D., The American Occupational Structure, New York, Wiley, 1967, p. 78.
3. Ibid., p. 202.
4. Duncan, O. D., "Ability and Achievement," Eugenics Quarterly, Vol. 15, No. 1, March 1968, pp. 1-11.
5. Blau and Duncan, pp. 208-227.
6. Ibid., p. 212.
7. Duncan, O. D., Socioeconomic Status and Occupational Achievement, University of Michigan Population Study Center, 1968.
8. Gintis, H., Alienation and Power: Towards A Radical Welfare Economics, Unpublished Ph.D. dissertation, Harvard University, 1969, esp. Ch. VI.
9. Coleman, J., et al., Equality of Educational Opportunity, Washington, 1966.
10. Stephens, J. M., The Process of Schooling, New York, 1968, Ch. VII.
11. Project Talent was the first large-scale survey in the U. S. which allowed any association of school resources and achievement.
12. Shaycoft, M., The High School Years: Growth in Cognitive Skills, Pittsburgh (American Institute for Research), 1967.
13. Ibid., Table 5.4, pp. 5-10.
14. Ibid., pp. 7-24.
15. Thomas, J. A., Efficiency in Education: A Study of the Relationship Between Selected Inputs and Test Scores in a Sample of Senior High Schools, unpublished Ph.D. dissertation, School of Education, Stanford University, 1962.

16. Bloom, B., Stability and Change in Human Characteristics, New York, 1964, pp. 52-68.

17. Jencks, C. S., "The Coleman Report and the Conventional Wisdom," and Smith, M. S., Equality of Educational Opportunity: The Basic Findings Reconsidered, both forthcoming in Mosteller, F., and Moynihan, D. P. Equality of Educational Opportunity, New York, 1970-71.

18. Jencks. This is not a result of ordering in the introduction of variables.

19. The following comments are based on an unpublished review of Title I programs, available from CEPR upon request; the review was undertaken under contract with the U. S. Department of HEW.

20. Fox, D. Expansion of the More Effective Schools Program, New York (Center for Urban Education), p. 63.

21. These were turned up in our review; see note 19.

22. Bissell, J. S., "The Cognitive Effects of Pre-School Programs for Disadvantaged Children," unpublished paper, June, 1970, Washington (National Institute of Child Health and Development).

23. Bissell, p. 28.

24. Bissell, J., "The Cognitive Effects of Pre-School Programs for Disadvantaged Children," unpublished Ed.D. dissertation, Harvard Graduate School of Education, 1970.

25. Millsap, M. A.,"An Evaluation of Dropout Prevention and Dropout Recovery Programs," unpublished paper, Office of Program Planning and Evaluation, USOE, 1969.

26. Ibid.