

Formal Education and Its Effect on Academic Achievement

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Ce document examine le mythe selon lequel l'éducation de type formel ne change en rien la formation de l'enfant. Il considère le mythe selon deux points de vue: l'éducation ou l'instruction change-t-elle en fait l'enfant? différents types d'instruction ont-ils des effets différents? Il commence par la source du mythe: le rapport Coleman. L'auteur nous laisse sans preuve; trois raisons à cela. D'abord, il mesure des résultats scolaires très généralisés — les plus petits dénominateurs communs les plus contaminés par les facteurs hors de l'école. Deuxièmement, il définit les facteurs relatifs à la famille de façon si large qu'il ne s'agit plus fort probablement de facteurs relatifs à la famille mais partiellement en fait d'un résultat de l'école. Troisièmement, Coleman utilise, pour mesurer la qualité de l'école, des éléments matériels qui ne sont plus probablement considérés maintenant comme les facteurs les plus importants relativement à cette qualité.

On examine alors la preuve que la formation a, de fait, un effet appréciable — les personnes éduquées diffèrent des personnes non éduquées. La formation affecte la culture d'un peuple, elle affecte la croissance économique d'une nation et elle affecte également les chances de vie individuelles.

Finalement, on considère la recherche insuffisante relative aux facteurs qui rendent certaines formes d'instruction plus effectives que d'autres. Bien que la plupart de la recherche soit américaine, celle menée au Canada concernant l'immersion en français permet, comme effet indirect, la pénétration dans quelques facteurs qui influencent l'efficacité de l'instruction. La conclusion montre l'utilité probable de cinq facteurs dans la mesure comparative de l'efficacité des différentes formes d'instruction: le contexte social du corps étudiant, l'atmosphère d'étude de l'école, le temps passé à l'instruction d'objectifs particuliers, le degré selon lequel l'instruction est formellement dirigée vers les objectifs, et la qualité des professeurs.

BACKGROUND

A currently fashionable myth is widely bruited that formal education doesn't make any difference. The myth had its origin in the United States, where so much faith has been placed in education and, ironically, where education has had such a demonstrably important effect on society.

We should not be surprised by this turn of events — there is no vengeance like that of a spurned lover. Education's suitors have claimed that she is a maid for all seasons — she will bring succor to the poor and disadvantaged, permit the ascent of the talented, and bring justice and peace to all peoples. When the fair maiden turns out to be an old whore who can be abused for the clandestine satisfaction of special interest groups in just the same way as society's other institutions, idealists renounce their

tryst and move on in their quest for a Venus who will help them realize utopian satisfaction. Various aspirants try out for the vacant role — participatory democracy and the fulfilment of human potential have been recent strong contenders.

Liberal ideologists, confident of the inherent goodness and perfectibility of mankind, created the illusion that education would eliminate social disadvantage, give intelligence to those who lack it, and generally erase the inequity and wickedness of western society. To their horror and amazement, compensatory education failed to compensate. Special coaching helped bright and advantaged children improve their IQ scores on “culture fair” tests more than it helped slow and disadvantaged children. Worst of all, Coleman (1966) made the remarkable discovery that differences in schools have far less effect on young people’s achievement than do individual differences among the children themselves. (Coleman, good liberal that he is, did not venture so far into apostasy as to name those differences intelligence and culture, but that is what they are.)

Now of course Coleman never researched the question as to whether education actually changes children. But enough was enough for the credulous and disenchanted faithful — if education does not fulfil the fantasy of social and intellectual equality, then of what use could it possibly be? To ask if education trains young people for future vocations, permits the continuity of our western culture, and helps intelligent, ambitious people to improve their social status would be like asking John Kennedy, after his inaugural address promising a brave new world, whether he did not think it would be rather nice if government could help people live together in peace and safety. Similarly, here in Canada, some of us, far from worrying about whether Canada will be multicultural or bicultural, participatory or tri-modal (big government, big business, and big labor), would be quite happy to be assured that there will be a Canada at all in five years’ time. Having scented the heavy bouquet of champagne, it is hard to be back to beer again.

This paper suggests that beer may not be a bad starting point. The paper is concerned with two topics — the actual effect of schooling, and the different effects of different kinds of schooling. Although in hindsight Coleman is seen to have been wrong in many ways — wrong to expect that schools would be able to compensate for intellectual and social differences, wrong to select crude, materialistic measures of school “quality,” and wrong to measure outcomes of schooling in terms of generalized attributes to which schooling only vaguely contributes — he was right in one important way. We do need to know what the different factors are that affect children’s learning.

This paper begins by examining evidence for the belief that schools really don’t make any difference. It finds that evidence to be of only very limited legitimacy. It then looks at the positive evidence that schools do in fact have an overall effect on society, and that different types of schooling

have different types of effect. No startling new evidence is introduced but the cumulative evidence suggests very strongly that educators are foolish indeed if they believe that what they do makes no difference to what children learn.

“SCHOOLS DON’T MAKE ANY DIFFERENCE TO STUDENT ACHIEVEMENT”

As is often the case, there is a grain of truth to the myth; and the educators themselves have perpetuated the confusion of fact and fiction.

The two most frequently quoted sources are Coleman (1966) and Jencks (1972). Coleman’s study is probably the most extensive piece of research ever carried out in education, and is without doubt an important contribution to the literature. However, it should not be forgotten that any large-scale research on important issues in education is almost certainly “dirty research,” for two reasons: it is impossible to control all the variables, and it is impossible to create true experimental situations. This does not mean that the research is worthless, but that one must be ever mindful of the uncontrolled or unsatisfactorily controlled variables.

Coleman’s research was intended primarily to measure differences between educational opportunities for whites and blacks in the U.S.A. Coleman set out to explain the discrepancies between white and black achievement in terms of school factors. With the advantage of hindsight, we can now say it was naive to expect factors such as per pupil expenditure and teacher experience to explain the disparity between levels of achievement for blacks and whites. Of more interest to Canadians in 1978 is the generalized conclusion that most of the inter-school variance in achievement (which according to Coleman is rather small anyway) can be attributed to the following factors: (1) student attitudes to education (including interest in school, self-concept, and control of environment); and (2) the quality of the student body in terms of the average home background. These factors, Coleman claims, were significant in explaining the variance in his measure of verbal achievement, while variables such as school facilities, curriculum, teacher quality, and teacher attitudes were of almost no account. In contrast to the rather low level of explanatory power of school characteristics, home background characteristics, according to Coleman, explained much of the overall variance in student achievement. This is the stuff of which legends are made.

There are, in my view, three major objections to basing conclusions on Coleman’s research.¹ The first objection relates to the control of variables. One should look closely at Coleman’s so-called home or external variables. These variables include parents’ education and a number of other objective items that precede the child’s education. They also include the number of books in the home and the parents’ educational expectations of children (clearly not prior variables). The measures were taken 6, 9, or 12 years after the child had started school. It can be hypothesized

that the number of books in the home and, particularly, parental expectations of children relate closely to children's academic performance not only because high expectations lead to high performance (Coleman's explanation, which I do not deny) but also because good performance in school leads to high expectations. That same problem is even more serious for the student variables (interest in school, self-concept, control of environment). Surely it is not unreasonable to suggest that the relationship between these factors and student achievement is spurious. In other words, success and self-respect go hand in hand. There is no more reason for saying that self-respect causes high achievement than for saying that high achievement causes self-respect. The same thing can be said of control of the environment. Surely it is not surprising that the student who achieves well thinks he has control of his environment — but which came first? If Coleman's use of those two variables (self-concept, control of environment) is confused, his use of the other, interest in school, is opaque. It is quite impossible to discern why Coleman should think that children's interest in school and enjoyment of reading are entirely independent of what the school does to them. In general, schools which do a good job of encouraging parents to take an interest in children's education, that provide children with opportunities for success, that encourage children and parents to develop home libraries, are classified by Coleman as schools with a high quality of student body. By some perverse logic, Coleman then says schools don't make any difference, it is the student body that counts.

The second objection to Coleman's finding that schools contribute little to the differential achievement of students relates to the nature of the criteria used to measure achievement. Most of the conclusions are based on a measure of verbal ability (sometimes described by Coleman as verbal achievement), although tests were administered in mathematics, non-verbal ability (sic), reading, and general knowledge. It is only fair to point out that Coleman selected verbal ability as the independent variable most often used because the other tests showed even lower percentages of variance explicable through inter-school differences as distinct from individual within-school differences. Nevertheless, one has to question whether any of the generalized tests do a good job of testing inter-school differences. Surely it is reasonable to assert that verbal and non-verbal ability, general knowledge, and a generalized understanding of everyday mathematics are not the best measure of differences in the effectiveness of various schools — particularly at the ninth- and twelfth-grade levels, where few if any courses are directed to those objectives. Coleman's criteria are good generalized measures of the outcomes of education (broadly defined) but they are not recommended for identifying school differences. Considerable portions of those criteria, one might expect, are learned in the home, from magazines, from daily life, and from television. To test school differences, particularly at the high school level, one would have to look

at more specific objectives, in creative writing, in physics, biology, and French, for example. But this objection, though substantial, should not be overemphasized. Given the large scale of Coleman's investigation, it was probably inevitable that he would choose generalized measures. Furthermore, the public has a legitimate interest in such generalized measures.

An analogy may clarify the problem. Height at maturity is a good general indicator of nutritional health in a homogeneous society. If the average height of Italian males increased by 4 cm between 1900 and 1950 then one might assume that nutritional standards had improved. But height would not be a good indicator of inter-family nutritional standards in a heterogeneous society. For example, one would not conclude that Jamaican immigrants in Canada are healthier than Italian immigrants because they are taller. Obviously a more specific measure than height must be used if we are to check nutritional health of different families in Canada. In the same way, vocabulary development is a reasonable measure of the educational level of a society, but it can hardly be expected to differentiate the educational effectiveness of different schools.

The third objection to Coleman's conclusions is concerned less with the report itself than with inferences that others have made from it. Coleman did not report that schools do not have an effect on their pupils. He did conclude (wrongly in my view) that there is very little difference in the effects on pupils of very different schools. His research was not designed in any way to examine the overall effects of schooling.

No attempt is being made to discredit this important research in its entirety — the negative findings are of significance. The finding that the school characteristics measured by Coleman had no effect on generalized measures of achievement is an important contribution to the literature.

Less time need be spent on Jencks' study (1972) as many of the points are repetitive. Once again the criteria of achievement tend to be highly generalized measures of achievement. Jencks reports that ninth-grade scores on such measures predict twelfth-grade scores very well, and that inter-school differences explain practically none of the remaining variance (1972, p. 90). However, if there is a relationship between the quality of the ninth-grade school and the quality of the twelfth-grade school, it would wash out the school effect completely. It is not unreasonable to expect that districts with good junior high schools have good high schools. But if there is a relationship between the quality of the junior high school attended by youngsters and the quality of the senior high school, Jencks' conclusion loses its force. The point also bears repeating that high schools (perhaps regrettably) tend not to emphasize the generalized factors that the researchers measured. It is particularly unfortunate that written English, the one universal objective that may be comparatively little influenced by external learning, is never included in the measured outcomes.

Systematic student evaluation is needed so that we can measure exactly what effects different processes have on students. Jencks argues that nothing we do will make much difference anyway. I would be the first to agree that schools cannot solve major social and political problems, such as racial and social discrimination; but there is evidence, I believe, not only that schools do have considerable effect on their students but also that there is a difference on the effects that different schools have. Jencks comes perilously close to confusing these two. He claims to prove that schools have little differential effect and then goes on to conclude that there is no point in putting resources into education and expecting them to have any effect. Even if one accepts his evidence (which I do not) his conclusion does not follow.

THE EFFECTS OF FORMAL EDUCATION

Let us turn now to the evidence that schools do have an effect. It is not as easy as it might seem to prove that schools influence students. After all, comparable societies usually have comparable education systems. The problem is analogous to the problem of the effect of television on children. We know that we are all affected by our environment, and it is axiomatic that the time we spend — in school, watching television, or with friends — affects our beliefs, attitudes, and behavior. It is nonetheless difficult or impossible to sort out precisely the effects of each variable. If, for example, one were to find that children who do not watch television are as violent as children who do watch television, surely that does not prove that television does not make children more violent. Presumably the main effect of television would be to raise the norm of tolerable violence in a society, and society's members would be affected by that norm indirectly as well as directly. Similarly, if one were to find that children who do not participate in formal education learn as much as those who are enrolled in school, it would be naive to conclude that schools don't make any difference. Schools establish a norm of learning which has an influence even on those who do not participate in the process. Indeed, the overall effect of schooling is generally accepted, whether welcomed or bemoaned. The circumstantial evidence is strong. A case can be made even in terms of economic development. No one would maintain that there is a simple linear relationship between development of education in a country and the growth of the national per capita income. (Naturally, there is a relationship between an individual's share of the national income and the individual's education, but that is another question.) Similarly, no educational economist would claim that economic development can take place independently of the educational level of the population. Carnoy (1969) studied a sample of Latin American countries and concluded that "rates of overall economic growth are most closely related (positively) to rates of return to primary and university levels (especially the last two years of those levels)" (p. 154). In developed countries, the highly educated

populace is different (in terms of literacy and numeracy for example) from the populace in less developed countries. Differences are not only a result of formal education; but formal education is generally recognized as being one important component of the processes that make, say, a citizen of Denmark different from a citizen of, say, Afghanistan. Some developing countries provide considerable education for some and no education for others. There are marked differences, in literacy and other measures, between the educated and uneducated portions of the population of Pakistan, for example.

Macro-social differences resulting from education are difficult to quantify because the factors involved are so complex. Most people would readily concede that education plays a part in making Danes different from Afghans, but it would indeed require wisdom to say how large that part is — in view of all the other differences between the two societies. Most comparative research has been done by economists — it is easier to quantify the economic effects of education than the social, aesthetic, or cultural effects.

At the micro-social level, it is fairly generally conceded, even by education's enemies, that formal education has an effect. Friedenberg (1974), on the right, and Wasserman (1971), on the left, both concede that education is having an effect, even as they deplore the effect.

The best study of the effects of education, intelligence, and family background characteristics on males within a western setting is that carried out by Sewell and Hauser (1975) in Wisconsin. Theirs is a carefully documented longitudinal study, a model of its kind. They note previous findings that education is more influential than first job in determining later occupational status, when social background is held constant (p. 4). The writers comment that "the effects on earnings of socio-economic background, represented by father's education and occupation, are almost entirely mediated by son's ability and by intervening educational and occupational achievements of the son" (p. 70). They note that although mental ability has an important effect on future earnings (net of socio-economic background), a large component (38%) of the effect of ability on earnings is mediated by educational attainment. Thus, while the net effect of education on future earnings and job is comparatively small, the effects of family background and mental ability will be very small without education.

Another important study is that by Hyman, Wright, and Reed (1975). This study, less sophisticated than the Sewell-Hauser longitudinal report, uses secondary data to establish relationships between education and knowledge that last even into middle and old age. The investigators use Canadian data, for example, to show the relationship between education and understanding of contemporary current affairs (pp. 198–200). What is more interesting is that the relationships with education hold, even when social class origin is controlled, particularly in the case of adults

of low social class (pp. 276–289). This finding complements those of Sewell and Hauser. Education is an important mediating variable between intelligence and social background on the one hand, and achievement and earnings on the other. The effects of education are particularly noticeable when education takes place despite an unfavorable social background. The sceptic may ask what would happen to Hyman's data if both intelligence and social background were controlled. It is reasonable to infer from the Sewell study that comparatively few of the enduring effects of education would be independent of both those background characteristics — but it would be foolish to assert that the effects would result without education.

International comparisons illustrate that without education there is little literacy, and without literacy knowledge cannot be disseminated efficiently or effectively.

The effects of formal education are obvious. We are all affected by our environment. Children brought up in the African Kalahari are different from those brought up in a Swiss village. Even within a single society, children brought up in the inner city are different from those raised in the suburbs. If we send all children to school for 12 years they will be different from children in a society providing no school at all, and even from one in which children finish school at 14. Not only does education have an overall societal effect; it has an effect on individuals within a society. It permits some children having other basic determinants of educational growth — intelligence and cultural background — to advance more than others, both as a result of their intelligence and culture and as a result of individual choices and personal characteristics.

THE DIFFERENTIAL EFFECT OF SCHOOLS

Most readers will probably accept the thesis thus far — that there is some overall effect of formal education; also that students benefit (or suffer) in different ways. The major problem area is that studied by Coleman — the differential effect of schools.

Let us first accept without qualification that there is consistent evidence that the background characteristics of children (their intelligence and their social and home environment) influence their achievement far more than do school factors that can be changed by teachers and administrators. That fact, however true and however obvious, should not be magnified out of significance. Of course, children's intelligence and environment influence their future. A useful analogy can be made with physical health. Inherited characteristics, nutritional standards, and social factors probably influence life expectancy more than the standard of medical treatment — but that hardly prevents us from caring about the last.

What is the evidence that schools do in fact have some differential effect on students? That question can best be answered by looking at the various factors that may have a differential effect. But first, a caveat — the research described here is generally tentative rather than conclusive.

One of the most disputed factors is the so-called contextual effect. Does the academic performance of children vary not only with their own social background but also with the composite social background of their fellow pupils? Research into this complicated question is by no means consistent. There is a considerable body of evidence suggesting that the contextual effect is a real one. Coleman, for example, concluded that the effects of other students on an individual are far greater than the effects of school facilities or attributes of staff (1966, table 3.23.1, p. 303). A more recent Philadelphia study (Summers and Wolfe, 1975) affirmed that racial balance in the school (as distinct from racial imbalance) improves the achievement of black students. Research in New Brunswick (Holmes, 1971) suggests that student-body characteristics have far more influence on the achievement of grade 9 students than teachers' attitudes and training. On the other hand, work with Wisconsin data has led Hauser, Sewell, and Alwin (1976) to conclude that there is no single, clear, unilinear effect of social context on achievement.

The problem with social context is that, like social status itself, it is a proxy variable representing various factors we do not fully understand. Presumably the social context of the school, if it affects students at all, affects them through such intervening variables as the expectations and aspirations of students, community (i.e. the school's catchment area) expectations and aspirations, and teachers' expectations and aspirations. What further complicates the problem is that as soon as one leaves aside the proxy variable (school social status) and measures what one thinks is the real variable (expectations, perhaps) one has to be careful to measure not effects but causes of achievement. It may well be that some variables for which contextual status is proxy have different effects from others. For example, Hauser, Sewell, and Alwin (1976, p. 337) suggest that aspiration levels may be reduced when the ability level of the student body is high. If that is so, it would mitigate contextual effects of high aspirations resulting from high social status. It is possible that the contextual effect works more within schools than among schools, in cases where schools are streamed. For example, the contextual effect of placement in a high social status secondary school would presumably not be very great (or might even be negative) if the individual were placed in a four-year non-university-bound stream, as distinct from a five-year college preparatory stream. There would not be much point in drawing school district lines to ensure comparable, heterogeneous student bodies in every school if the principals of the schools then turned around and streamed their students by social class and ethnic origin.

Attempts have been made to analyze the contextual status. McDill (1967), for example, found that the academic atmosphere of a school influences pupils' achievement. Academic atmosphere is not a measure of the future aspirations of parents or students, but rather of the expectations that pupils perceive that teachers and other pupils have. Do teachers

and students expect, demand, and respect a high level of achievement? The New Brunswick research (Holmes, 1971, p. 74) suggests that pupils' expectations (i.e. the expectations of the student body, not the individual's expectations) may be potent explanations of the different achievement levels of schools.

The problem of cause and effect is immensely difficult. The point was made earlier that high self-respect is as likely to be the effect of high achievement as the cause. Are high academic expectations the result rather than the cause of high achievement? The possibility cannot be ruled out, but it seems unlikely. In the case of self-concept, the relationship is at the individual level; the individual's achievement is being compared with the same individual's self-concept. In the case of academic atmosphere, the comparison is between the perception by students of other students and teachers, on the one hand, and mean school achievement on the other. The school, not the individual, is the unit of analysis. One interesting sidelight on this question is that the New Brunswick study also finds a negative relationship between the level of marks awarded in the school and the level of achievement (with social status controlled) (Holmes, 1969). Perhaps giving low marks is a way of expressing high expectations! Precisely the opposite relationship had been hypothesized.

Hargreaves (1976) graphically explains how the McDill hypothesis can work in practice. He describes a streamed secondary school where each class adopts a normative attitude towards school work — ranging from the A class's very positive attitude to the D class whose attitude is totally negative. Hargreaves relates (but does not quantify) the effects of the different norms on student achievement.

Although there is evidence that streaming affects the expectations and academic achievement of elementary students placed in specific streams (Jackson, 1964), there is no conclusive evidence that streaming (before the age of 13 or 14) has any overall positive or negative effect. It is quite possible that, in some circumstances, superior students achieve better when they are segregated, but, correspondingly, poor students do worse.

The questions raised by streaming are analogous in some ways to those raised by social context. Over a large geographical area, the total effect on achievement resulting from different enrolment or streaming patterns is probably zero. But that does not eliminate the possibility that by streaming young people we are in fact predicting future success by use of a self-fulfilling prophecy. In exactly the same way, by setting school boundaries so that children of low social status are segregated from those of high social status we may also be helping increase the relative advantage of the high-status pupil. Thus, when we look at the differential effects of schooling we must not only think in terms of factors that may prove generally positive. We must also consider the possible inequality that results from our applying organizational systems that may have different effects on different classes of students.

One other factor that may help explain the differential achievement of schools is time. Wiley (1976) argues felicitously that a child who does not go to school will not obtain any of the benefits of school. Similarly the student who attends for the maximum length of time will receive the maximum benefit. Therefore, he argues, an intermediate amount of time will provide an intermediate benefit. What Wiley overlooks is the possibility of the social norm of learning — that a certain level is the norm and that there is a tendency for teachers and children to work towards the norm irrespective of the time available. All teachers can think of students who could miss half the school year and keep at precisely the same level. They can probably think of others who would miss half the year and omit three quarters of the work. Wiley re-analyzes Coleman's data (1966) and suggests that the amount of time is a powerful explicator of inter-school differences when race, number of children in family, and possessions in the home are controlled. He suggests that improvement in reading comprehension (as distinct from verbal ability and mathematics) is particularly amenable to improvement given increased instructional time. The IEA study of international achievement found instructional time to be generally a poor predictor of differences in achievement on an international basis, with science proving an important exception (Platt, 1975, p. 41). Wiley's work is still in an experimental stage and must be treated with caution. He has applied his ideas to Illinois achievement data where he also claims success (Wiley & Harnischfeger, 1974). The weakness of Coleman's measure of home background has already been explored; and in addition there is no control for intelligence, which could quite possibly be related to school attendance. Husén (1972) surveyed the evidence on time and concluded that there is little or no relationship between time spent in school and achievement, using as examples different starting ages.

There are three components to time in school. The first is the length of the school year; the second is student attendance; and the third is the amount of instructional time devoted to the subject in question. There are problems with all three. If length of school year predicts achievement is it because jurisdictions with longer school years have a greater respect for education? Is it cause and effect or is it that community respect for education determines both the length of the school year and achievement? Student attendance is related to achievement — but does poor attendance cause poor achievement or does poor achievement lead to poor attendance? Or do families that produce poor achievers produce poor attenders? If a school gives more time to mathematics, is it the increased time that improves achievement or is it the school's valuing of mathematics that leads both to the increased time and to the increased performance? A basic problem with the time-in-school variable, quite apart from Wiley's failure to control ability and social status, is the nature of its relationship with achievement. Undoubtedly there is some cause and effect, but we do not know exactly how much.

One of the clearest examples of the effects of schooling comes from Canada's experiments in the teaching of French as a second language. It is rather obvious from Canadian experience that Anglophone children who are not taught French in school rarely learn the language (unless they come from bilingual homes). On the other hand some children, by no means all, who are taught French become quite competent in their second language. This is most obvious in the province of Quebec where Anglophone children, despite the Francophone environment, have only recently come to be effective in French with the development of immersion and bilingual programs. Important research by Lambert and Tucker (1972) established that immersion does have an important effect on the learning of a second language.

More recent research (Stern, 1977) suggests that there are four factors affecting the effectiveness of French instruction. By far the most important is time — the amount of instructional time devoted to the teaching of French. This Canadian experience with various forms of French immersion is borne out by large-scale research in England, which showed a direct relationship between amount of instructional time and effectiveness of instruction. A second factor appears to be the incidence of instructional time. Thus twenty minutes a day over six years may be less effective than one hour a day over two years. The other two factors are difficult to disentangle. French immersion programs characteristically depend on two complementary methodologies — indeed, it is not at all certain that the second can exist without the first. French as a second language is typically taught in a highly structured, teacher-centred style. Courses of study, whether audio-lingual or traditional, are sequentially structured. But French immersion is also characterized by the teaching of other subjects in French — also in highly structured, formal, teacher-centred styles. It is not at all clear the degree to which it is the formal, sequential instruction in French as a discipline, the necessity of understanding and speaking French to survive in classes instructed in French, or the formal methods used in teaching the other subjects that has made French immersion so successful. Clearly, instructional time is a key variable, but we cannot be sure to what extent the instructional techniques characteristic of French immersion also contribute to the success of second-language French instruction.

Bennett (1976), in a recent controversial study published in England, concludes that formal instruction at the elementary level is generally more effective in developing pupils' skills than is informal instruction. The essential finding is supported by Traub, Weiss, and Fisher (1976) in Ontario, but only for disadvantaged children. The Bennett study has been strongly criticized (as one would expect, considering its implications for school reform), notably by Rogers and Baron (1977). Some of the objections to the Bennett study are serious. For example, it has no control of social status or intelligence. While it is reasonable to guess that in a fairly large-scale study these factors would be equalized (or favor informal schools,

which are generally associated with high-status districts), the fact is we cannot be sure. Bennett also used the individual student as the unit of analysis, and a strong case can be made that comparison by class (as treatment group) would have been more appropriate. There is some irony in the fact that one of the objections to Bennett's study (Rogers & Baron, p. 623) is that more of the formal classes were taking external tests than was the case with informal classes. Ironical or not, the point is interesting, as Bennett does suggest that external testing contributes to higher achievement levels. This may relate again to the academic atmosphere of the school, a point discussed earlier. Bennett's findings may not be entirely inconsistent with Bloom's thesis (1976) that definitive, minimal-error instruction towards explicit objectives can produce very effective learning.

Although parents generally believe that teachers influence what children learn in different ways, most attempts to use teacher variables to explain differences in achievement have proved unsuccessful. Experience, qualifications, salary levels, and attitudes (professional versus collegial) have generally not proved of much value. Further, within school systems, the different effects of teachers cancel one another out, so that one system, in terms of teachers, tends to be much like another. Nevertheless, it is nonetheless true that if teachers do in fact make a difference it is hypothetically possible to isolate those factors that make the difference. If the factors can be isolated it is possible that school administrators can make decisions that take advantage of the increased knowledge. The Summers and Wolfe study (1975), for example, found that the quality of college from which the teacher came helped explain teacher effectiveness. That variable, very possibly, is a proxy variable for factors such as teachers' intelligence and teachers' family background. In general, we must admit ignorance about the system-level influence of teachers on student achievement, but it would be premature to say it is, and must remain, insignificant.

Class size is one variable that appears unlikely to play an important part in our discovery of school effects on achievement. Ryan and Greenfield (1975) admirably summarize the data on this question and it appears somewhat improbable that any strong, general relationship between achievement and class size will be found. It is possible, however, that class size may be related to certain very specific kinds of achievement.

CONCLUSION

Coleman has been cast as the villain of the piece. To be fair, he was an unwitting villain starting out with the best of liberal intentions. In any case, education owes a great debt to Coleman for contributing to the annihilation of the grandiose and self-seeking puffery that more resources, more qualified teachers, smaller classes (in short more money) will solve educational and indeed more basic social problems.

Coleman has never claimed that formal education has no effect, only that different schools and different resources fail to have any appreciable effect

compared with students' background characteristics. An analysis of the Coleman research suggests that there are serious shortcomings, particularly in that Coleman failed to adequately distinguish cause from effect. His definition of home variables is so all-embracing, and so strongly related to school variables, that it is of questionable value. His measures of achievement (principally verbal ability) are so generalized that one expects them to be highly contaminated by family, social, and media exposure.

The evidence is reasonably substantial that education does affect learners, not only because it serves as a qualification for later advancement for the individual and as a prerequisite for economic advancement of a state, but because intrinsically it makes a difference. It is very difficult to disentangle the intrinsic value of education from the value it holds as a qualification, as a necessary stepping stone to later achievement. It is also difficult to determine at all precisely the effects of education as distinct from effects of other environmental factors. Nevertheless, it is clear that educated people are different from uneducated people and that, on a worldwide basis, a large part of that difference can be attributed to the formal educational process. This in no way negates the fact that individual factors (social background and intelligence) explain a large proportion of the difference in achievement among individuals. Education affects everybody but it affects people differently. Differences in the individual effects of education depend largely on non-educational factors just as our reaction to different drugs reflects innate and environmental factors in our physical composition.

The second argument must also be made clearly and strongly. That parental background and intelligence (over which educators have comparatively little control) have a greater effect on student achievement than do school factors is no reason at all for failing to use the school factors that demonstrably do have an effect. Do we refuse to irrigate small parts of the earth's surface because man cannot change the earth's climate? That kind of non sequitur diverts us from useful investigation. Questions we should be asking ourselves, or should be investigating, are: Which school factors over which we have some control most affect student achievement? How much change in achievement can be brought about by specified amounts of change? How much will the change cost? Are there other losses from the change that counterbalance the gains? Is the public given the information and the choices so that it can make intelligent decisions?

Research is gradually pinpointing some of the variables that may explain differences in achievement levels among schools and systems. None can be named with certainty as being the single, most critical factor but a field of probable starters would include:

- the social context of the school (probably in terms of attitudes, expectations, and assumptions on the part of the student body at large);
- the academic atmosphere of the school — being the extent to which high achievement is a norm among the student body;

- time — being the total amount of school time, amount of time devoted to a particular topic, and the students' presence for the instruction;
- formal and informal instruction — formal instruction may be related to improved effectiveness in the achievement of clearly stated objectives;
- teacher quality — possibly being some combination of intelligence, verbal ability, and conscientiousness.

Much of the evidence for this paper has been taken from American sources where most of the comprehensive research has taken place. Canada is singularly deficient in student achievement data, particularly data related to school factors.

There is no reason for educators to retreat into their corners and pout. Education is a formidable force in our society and its effects are far reaching and long lasting. We do not know exactly which factors make one school more effective than another but we know that there are significant differences among different schools.

The interest of educational researchers in pursuing the truth about the effectiveness of education seems to have waned. Yet, what could be more important than finding out how children learn more effectively? As the old myths are exploded, many educators are unhappy with the answers they are getting. As they increasingly find their pet theories invalidated, perhaps they decide that it is better to stop asking the questions in the first place.

There are strong indications that some kinds of schooling are more effective than others. Many of the apparently more successful methods run counter to the prevailing ideology in recent years that assumes that children learn best without structure, without direction, and without incentives. There is certainly a need for more investigation of such findings, tentative as they are. What there is no need for is a funeral ceremony for education, condemned because it has no effect.

Education is much like the other social forces in our society — government, medicine, media — its effect is a small fraction of what its enthusiastic practitioners believe — but still several times as much as its detractors claim. Educators have a triple responsibility — to inform the public about what we know concerning the effectiveness of education, so that the public may make intelligent choices; to enhance our crude understanding of the social system within which children learn; and to exercise intelligent judgment in the light of research findings.

NOTES

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1. The best, if somewhat technical, critique of Coleman's research is that of Bowles and Levin (1968).

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