DOCUMENT RESUME

ED 294 914 TM 011 631

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TITLE	Accelerating the Development of Formal Thinking in Middle and High School Students.
PUB DATE	23 Mar 88
NOTE	22p.; Paper presented at the Annual Meeting of the American Educational Research Association (New Orleans, LA, April 5-9, 1988).
PUB TYPE	Speeches/Conference Papers (150) -- Reports - Research/Technical (143)
EDRS PRICE	MF01/PC01 Plus Postage.
DESCRIPTORS	*Adolescents; Age Differences; Cognitive Processes; *Developmental Stages; Elementary Secondary Education; Foreign Countries; *Formal Operations; *High School Students; *Junior High School Students; Middle Schools; *Piagetian Theory; Schemata (Cognition); Science Instruction
IDENTIFIERS	United Kingdom

ABSTRACT

In an attempt to accelerate the development of formal operations in average young adolescents, up to 30 intervention lessons relating to all formal schemata were given by science teachers, over a period of 2 years, to classes in eight British schools. Boys starting the program aged 12 years-plus showed a pre-test, post-test effect size on Piagetian tests of 0.89 standard deviation compared with control classes; this represents a mean change from the 49th to the 26th percentile in terms of British norms for the development of operational thinking. Middle school students and girls of 12 years-plus did not show greater gains than controls. Gains were shown by girls in one 11 years-plus class and two laboratory classes for the same age group. In one school, gains were maintained for one year after the program ended. There were no effects on tests of science achievement during the intervention. It is concluded that in-service training designed to enable teachers to adapt to the students' increased operational thinking capacity should accompany the interventions. Six tables and four figures are included. (SLD)

Presented to AERA Meeting, New Orleans, April 1988

ACCELERATING THE DEVELOPMENT OF FORMAL THINKING IN MIDDLE AND HIGH SCHOOL STUDENTS.

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Manuscript submitted March 27th 1988

Abstract

In an attempt to accelerate the development of formal operations in average young adolescents, intervention lessons relating to all formal schemata were designed in the context of school science courses. Over a period of 2 years, up to 30 intervention lessons were given by science teachers to their classes in 8 schools. Boys who started the program aged 12+ showed a pre-posttest effect size on Piagetian tests of 0.89 SD compared with control classes. In terms of British norms for the development of operational thinking this was a mean change from the 49th to the 26th percentile. Neither the middle school students nor the 12+ girls showed greater gain than the controls. Gains were shown by girls in one 11+ class and in the two 11+ laboratory classes. In the laboratory school students given intervention lessons by the researchers maintained their gains over controls in formal operations at a delayed posttest one year after cessation of the program. There was no effect on tests of science achievement during the intervention. It was argued that the interventions needed to be accomoanied by inservice training designed to enable teachers to change their teaching style in line with their students' increased operational thinking capacity.

ACCELERATING THE DEVELOPMENT OF FORMAL THINKING IN MIDDLE AND HIGH SCHOOL STUDENTS

This paper will describe the results of a research project, Cognitive Acceleration through Science Education (CASE) designed to test whether science education in secondary schools could be a context in which the cognitive development of adolescents can be changed. The profile of cognitive development of British students has been determined by a large-scale representative survey using Piagetian measures (Shayer, Kuchemann & Wylam 1976; Shayer & Wylam 1978) as part of the Concepts in Secondary Mathematics and Science (CSMS) program. Among 12 year-olds the results showed a developmental range of +/- six years, centered around late concrete operational thinking. Among 16 year-olds, only 30% achieved even early formal operations, and only 11% showed the mature formal operations needed, for example, for successful university work in science or mathematics.

It seemed to be of the utmost importance to determine whether schools might alter this dismal picture. The alternative would be that teachers would have to adapt their teaching to the current reality of the developmental profile and the variation in development of their students. If it did prove to be possible to change the current profile, then it would be important also to estimate by how much the professional skills of science teachers needed to be enhanced. A more distant objective was to look at how students' learning in other school subjects might be affected.

Review of Previous Work

There is nothing new about attempts to accelerate cognitive development. Siegler, Liebert, and Liebert (1973) showed that 10 year olds could be trained to solve the pendulum problem, involving control of variables. Each training procedure, however, itself involved the pendulum problem, so this can be seen as direct training with no attempt to test for any generalistion of effect to other contexts or other schemata. Case (1974) also showed that it was possible to train 8 year olds in the control of variables strategy, and further that for field independent subjects there was some transfer to combinatorial thinking - but there is some doubt as to whether combinatorial reasoning is really the formal schema that Piaget believed it to be. Bredderman (1973) used two types of training ('reinforcement' and 'cognitive conflict') in an attempt to train the control of variables schema, but neither trained group showed any significantly greater gain than a control group. Lawson, Blake, and Nordland (1975) were able to train 14-17 year olds (in pairs) to improve their performance in a control of variables task, but with no specific transfer to other tasks in the same schema. On the other hand, Lawson and Wollman (1976) trained 10 and 12 year olds in the bending rods task, and found specific transfer to other control of variables tasks but no general transfer to compensation tasks or other more general measures of formal operational thinking. Similarly, Lawson and

Snitgen (1982) demonstrated significant gains in formal reasoning ability of college freshmen as a result of a special one semester biology programme, but again no transfer was observed to schemata not included in the program.

The reports cited so far employing a strategy of training directly in the use of one or more schemata have failed to produce evidence of general transfer. There are, however, a few intervention studies which do suggest that general cognitive acceleration may be an attainable goal. Kuhn and Angelev (1976) trained some 8 - 11 years olds by setting series of problems on identifying which boxes had 'prizes' inside by the shape, colour, and size of symbols on the boxes. On each presentation the rules were changed, and the child was asked to think aloud to explain his/her strategy in isolating the features that led to the prize. Structurally this is a control of variables problem, but superficially it is quite different from, say, the pendulum problem. Compared to a control group, subjects performed better both in a pendulum posttest, and in the chemical combinations task. Rosenthal (1979) gave two one-hour sessions to groups of 1S girls aged about 1! years. The training focussed the subjects' attention on the dimensions of variables in various situations, without specifically steering them towards a control of variables strategy. In posttests, subjects trained thus performed significantly better than controls both in flexible rods (control of variables) and in proportionality tasks. Here is some evidence for general transfer.

An important feature of these last two studies is that the strategies of formal schemata are not directly trained at all, but rather the subjects are directed to notions of measurement, of dimensions, or given successive problems without solutions which force them to focus on new aspects of the problem. It seems likely that the success of such strategies is that subjects are given the essential mental tools which enable them to construct the formal schemata for themselves. We hypothesise that it is the process of constructing their own meanings which leads students to the cognitive restructuring responsible for increased scores of experimental groups.

A final point about the attempts at cognitive acceleration reported in the literature is that they have generally been limited in extent. Often they consist of one or just a few special 'interventions', never extended beyond two months at one intervention per week. If we are considering the possibility of a significant restructuring of a person's thinking capability, it is reasonable to suppose that this could require interventions over a longer time scale.

Description of the CASE Project

Aims and Constraints.

The program was carried out in British secondary schools between October 1984 and July 1987. Features which distinguish this program from the interventions described in the literature are that it was to be delivered by regular high school teachers in ordinary school settings, over a period of two academic years, and was to be integrated into the schools' regular science curriculum. It was proposed to produce intervention lessons related to ten formal operational schemata in the contexts of Physics, Chemistry and Biology, to take up no more than 25% of the time normally allotted to science. An important spin-off of the work would be the development of the teachers' professional expertise in recognising some sources of learning difficulties amongst their pupils and introducing to them strategies which may overcome such difficulties.

Subjects

The main experiment was conducted in eight schools. In addition, one school was designated a "Laboratory school", for the trial of new intervention material. This Laboratory School was an ordinary London comprehensive secondary school in in which the project staff themselves taught the intervention lessons to two 1st year (11+ years) classes over a period of 18 months. The purposes were:

(a) the formative evaluation of the intervention lessons drafted by the project. The laboratory school provided an opportunity to try out new material and to make some revisions before asking others to teach it;
(b) to investigate what effect could be achieved by teachers who fully comprehended the model underlying the interventions.

The main experiment schools. Thse were chosen on the advice of science advisers from seven different areas (Avon, Bedfordshire, Cheshire, Gloucestershire, Inner London, Surrey, and Wigan). The schools included inner city, suburban, and rural schools. All of the schools were mixed-sex. Two were middle schools (9 - 14 years). The remainder were secondary schools (11 or 12 - 18 years). Experimental classes from the middle schools started at the 11+ age level. Most experimental classes in the secondary schools started at the 12+ age level (second year), although one first year (11+) experimental class was used.

Controls. In each school control classes were chosen that were parallel in age and ability to the experimental classes, and who normally received the same science curriculum. Two types of control class were identified. Control 1 classes were taught science by the same teachers who were to use the intervention material with the experimental classes. Control 2 classes were taught by different teachers. In practice, the distinction between the two types of control became blurred by the second year, with changes of schedules and teachers' responsibilities.

The Interventions

The interventions were special lessons used with experimental classes instead of regular science lessons at the rate of about one 60 to 80 minute lesson every two weeks over a period of two years. No school used more than 30 intervention lessons in all.

Intervention lessons were designed around these schemata of formal operations: Control and exclusion of variables, ratio and proportionality, equilibrium, compensation, combinatorial thinking, correlation, probability, compound variables, and conservation involving formal modelling. The strategy of the intervention lessons was based on the experiences of workers cited above and on a pre-trial feasibility study conducted by one of the authors. Each intervention lesson focusses on one of the schemata, although as the program progresses one lesson may also call up schemata introduced previously. The terminology required is initially introduced in contexts which require concrete modelling only. Once familiar with terms such as "variables", "values of variables", and "relationships between variables", students are given practical problems which require the use of the formal schema for their solution. There is no attempt to teach, for instance, "rules for controlling variables". Rather, the student is put in the position where she has to construct the schema for herself in order to solve a practical problem.

For example, a set of tubes of different length, thickness, and material are provided, and the students have to decide which variables affect the note produced when they blow across the tube. Feedback from the problem, organised by the teacher, shows the student the extent to which she is being successful in reaching a solution. In this example, a student who says "the note depends on length" is asked to show how he knows. He may demonstrate with two tubes of different length and different thickness. The teacher's questioning probes what this says about thickness. Even if the student does not see that variables have been confounded, he does see that there is some problem with his conclusion. This is the cognitive conflict with which he may be left, but which it is supposed "loosens" an existing cognitive structure, making it more amenable to restructuring at a higher level on another occasion.

Another aspect of the intervention strategy is that each schema is related to examples from the regular science curriculum. In the science curriculum, reference is made to experiences from the intervention lessons. This 'bridging' back and forth is hypothesised to be necessary for the consolidation of the development of formal schemata.

Materials and Inservice

Project teachers received the intervention lessons in the form of teachers' notes, students' worksheets and problems. At the start of the project they spent one day at the project centre for an introduction to the nature of the schemata of formal operations, and a review of the main features of a Piagetian developmental perspective on learning. There was also a run-through of the specific activities included in the intervention lessons to be taught in the first term. In each subsequent term during the project a similar one day meeting was held, including a mixture of practical experience with new intervention activities and some theoretical perspective such as bridging, formal modelling, or the possibilites of such a thing as general development. As the project progressed teachers played an increasingly participatory role, providing feedback on the lessons taught and their own suggestions for new activities and for bridging from the interventions to the regular science curriculum. In addition to these meetings, each school was visited by project staff at least once per term. Intervention lessons were observed and discussed with the teacher afterwards.

Tests and the testing program

The main test instruments used were Piagetian Reasoning Tasks, (NFER 1979). These are demonstrated group tests of cognitive development whose development and validation have been reported (Shayer, Adey & Wylam 1981). Recently completed re-analysis of the Tasks using Rasch scaling has enabled the task item level and the person level to be estimated on the same scale. This permits a greater degree of precision on the ascription of a level to a person. From the total number of items correct a score on the following scale is obtained. The scale score may be interpreted in terms of levels of development if required.

early concrete,	2A:	3
mid concrete,	2A+:	4
mature concrete,	2B:	5
concrete generalisation,	2B+:	6
early formal,	3A:	7
mature formal,	3A+:	8
formal generalisation,	3B:	9

Testing occasions for experimental and control groups were planned as follows:

Pretests, before any intervention lessons were given:
'Pendulum' and 'Equilibrium in the Balance' in the lab school,
'Volume and Heaviness' and 'Pendulum' in the main experiment.

Midtests, after approximately one year of intervention:
'Pendulum' and 'Equilibrium in the Balance' in the lab school,
'Flexible Rods' in the main experiment.

Posttests, after approximately two years of intervention:
'Flexible Rods' in the lab school,
'Probability' and 'Pendulum' in the main experiment.

Delayed posttests, one year after the posttests and one year after intervention lessons had ceased (lab school only): 'Pendulum'.

All Piagetian Reasoning Tasks (PRT) were administered either by the project staff or by the project teachers after seeing one demonstrated by project staff. All were scored in the project office, with cross checking of a sample between two markers.

In addition to the PRT testing program, posttest batteries in Main Experiment schools included a common science achievement test designed in cooperation with the project teachers, and agreed by them as representing a fair test of some of their science curriculum objectives.

Results and Discussion

Laboratory school

Mean scores for experimental and control groups on pre-, mid-, post-, and delayed post- Piagetian tests in the Laboratory School are shown in table 1 for all pupils and for boys and girls separately. Figure 1 displays the results for boys and girls combined.

(Table 1 about here)
(Figure 1 about here)

It is clear that by the end of the intervention period the experimental group had achieved a significantly greater gain in levels of cognitive development than had the control group. In the subsequent year during which there was no further intervention, the experimental group continued to develop at the same rate as the control group and gains made during the intervention period were maintained. It appears that having being moved on to a faster developmental track, the experimental group continued on this track without further special treatment. Differences between boys and girls will be discussed later.

Main Experiment

Equivalence of control groups. The gains made by the two control groups differed by only 0.7 standard errors, and the control-1 group gains were not significantly correlated with the gains of the experimental groups taught by the same teacher. The two groups are thus merged in the discussion that follows.

Overall results. Firstly, a global comparison of the experimental and control groups is presented in the top part of table 2 and figure 2.

(Table 2 about here)
(Figure 2 about here)

Overall the experimental group made gains in levels of cognitive development which were statistically greater than those made by the control group over the two year period of the intervention.

Yet the relative gain is only 0.21 levels, or 0.20 standard deviation. As a size of effect this is equivalent to perhaps two or three months differential in development, and were this all that the project had to show, it would be difficult to justify the effort. However, the composite data conceals some interesting differences in gains made by different age groups, by boys and girls, and within individual schools. Data on each of these dimensions will be presented in the following sections.

Age group. The gains made by experimental and control groups starting the two year intervention program at 11+ and at 12+ years are shown in the lower part of table 2. All the extra gain by the experimental group as compared with the control group appears to be concentrated in the 12+ age group. Does this suggest an optimum time at which interventions designed to accelerate cognitive development will be effective? We shall see later that such an interpretation may be over-simple.

Gender. If the results are now broken down within each age group for boys and girls, a further concentration of the effect emerges (table 3).

(Table 3 about here)

Only the boys within the 12+ group appear to have been affected by the intervention program, with an effect size of 0.89 of a level, or 0.91 SD. With none of the other sub-groups does any difference in gain approach significance.

Starting Level. Perhaps a more pertinent factor than chronological age is the level of development of the pupils at which the intervention starts - yet no such differences appeared in the data. This is exemplified by two sub groups, one which did and one which did not did not show a significant difference of experimental over control: 12+ boys and 11+ boys respectively. The data is presented in table 4.

(Table 4 about here)

Overall, the 11+ boys in the experimental group showed no greater gain than those in the control group. The data in table 4 show that this is also true for each subgroup broken down by starting level. A similar effect is found with the two girls' subgroups. Overall, the 12+ boys in the experimental group showed a significantly greater gain than those in the control group. Table 4 shows that for each starting level, the experimental group gain is obviously larger than the control group gain, reaching statistical significance at the 5% level in the first three of four subgroups. Thus the difference between the effect of the interventions on 11+ and 12+ starting age groups cannot be attributed to lower levels of cognitive development amongst the younger pupils.

Teacher/ Class. Could the rather dramatic effect noticed with the 12+ boys be accounted for, perhaps, by the exceptional performance of one or two classes / teachers? To answer this, and to reveal more about possible interpretations of the data, we must turn to individual class results.

In table 5 the gain scores for boys and girls are given for each class at mid- and posttests, and contrasted with (a) the mean gains of the relevant controls, and (b) the expected mean gains based on the CSMS survey results for pupils at comparable age ranges (Shayer et al. 1976; Shayer & Wylam 1978; Shayer & Williams, 1984). Mean gains under (b) are added as a check on the representiveness of the control groups (a). They are not (statistically) significantly different. The differential growth rates in the different years of the CASE control groups are consistent with the CSMS survey data.

(Table 5 about here)

In table 6, the gain scores shown in table 5 have been converted to percentile changes in comparison to the CSMS survey data.

(Table 6 about here)

Internalising the Model.

In figure 3 the pre-, mid-, and posttest means for the boys in each of the 12+ classes are plotted in comparison with the mean of all the controls. It can be seen that in the first year three of the seven classes far outperform the controls, but four do not. Yet in the second year all experimental classes show greater slopes than the controls. A possible interpretation is that three of the teachers internalised the intervention model during the first year, but only during the second year did all do so. A comparable figure for the girls shows no differences from the controls in either year.

(Figures 3 snd 4 about here)

Figure 4 displays pre- to posttest mean gains for the 12+ boys and girls. The values shown are the mean gains for each class less the control mean gains (for boys and girls separately), so that differences between experimental classes and controls are shown against a comparable reference point. The gender of the teacher is added at the foot of the diagram. It can be seen that although there is substantial variation in the class means (the standard error of each mean is around 0.35 levels), it is reasonable to treat them as small-sample variations around a mean of 0.89 levels (0.91 SD) for boys and around 0 for girls. No evidence from the diagram supports the notion that the gender of the teacher is responsible for the non-effect with the girls.

We have no explanation to offer for this difference between boys and girls. Two weaker lines of evidence caution against a belief either that girls' development cannot be accelerated, or that ages 11 to 12 are unfavourable to intervention both for boys and girls. The pre- and delayed posttest differences between experimental and controls for the laboratory school were 0.77 levels for girls and 0.46 levels for boys, and this was a first-year 11+ group. And one of the four 11+ experimentals was a secondary school class in which, although there was no difference from the control mean for the boys, the girls showed an effect-size of 0.76 levels, which was significant just at the

0.05 level in comparison with the controls. We believe that the most likely explanation of the general non-effect for the 11+ classes was that the three that showed no effect were all Middle school classes. It is possible that the teachers concerned had more difficulty with the intervention model than those with experience of teaching science requiring formal operational thinking to the older pupils in secondary schools.

Transfer?

There is some evidence for general transfer from one school which was asked specifically not to use any of the 4 intervention lessons concerned with the proability schema. The school used the time available to teach either alternative interventions based on other schemata, or their regular science curriculum. The pre- to posttest gains to the Pendulum PRT were 1.07 levels ($N = 39$, $\underline{SD} = 1.28$) and to the Probability PRT were 1.01 levels ($N = 38$, $\underline{SD} = 1.31$). Both of these gains were significantly greater than the control groups gains at $p < .01$. With this sample size the power of the test comparison is low, but it does suggest that the gains made are not the result of direct teaching of the subject of the tests but reflect deeper changes in cognitive structures, since the same effect size shows for the probability schema which was not included in the intervention program of this school as for the control of variables schema which was.

Science Achievement Tests.

Few schools found time to give the common science achievement test as well as the main PRT test battery, but data is available from classes 0301, 0302, 0902, and the corresponding control classes. It can be seen from figure 4 that these are classes where, if any exists, one would expect to find enhanced science achievement in the boys. Yet for matched pairs from the experimental and control group boys with the same mean on Piagetian pre-test, the control mean on science achievement was 44.5% ($N = 19$, $SD = 21.0$) as compared with the experimental mean of 42.4% ($SD = 16.8$), giving $t = -0.4$. The effect of the intervention is not yet showing in the students' science achievement: it is neither improved nor, although the experimental groups lost up to 25% of their science curriculum time to the intervention program, has it been adversely affected.

Conclusion: The Educational Meaning of the Research

The size of the experimental effect for the boys in the intervention classes starting at 12+ - about 0.9 Piagetian level - suggests to us very strongly that the CSMS British population survey data do not represent an unalterable feature of human development. A gain of 23 percentile points (table 6) achieved in 20 months should almost double the numbers of boys developing the formal operational thinking needed for success in traditional school courses in science and mathematics. Control class results indicate that current teaching routines do not change the rate of development any more than was apparent from the CSMS survey data obtained 10 years ago. Thus a major decision point reached by the project has been resolved in support of using schooling to alter cognitive development, and against the notion of merely adapting to existing norms. However, while we now believe that schooling can alter development, it is clear that this project is at best a first step to enabling schooling to change to do it. Some important issues remaining to be resolved are discussed in the concluding sections.

Training?

A believer in a skill-training model of development might assert that we nave but tested the skills we have trained. Our first reply would be that the indirect training methods adopted would have been a very foolish strategy to adopt if a skill-training model were true.

The second reply is related to the more general issue of whether the developmental spectrum can be altered. It is that effect sizes of comparable magnitude with students initially 12+ years old have been reported in another study where the intervention model did not involve training of Piagetian operations. Shayer and Beasley (1987) reported using Feuerstein's Instrumental Enrichment (IE) course for two years with students in the bottom 5% of the performance range in a special school. As measures of fluid intelligence, Raven's Matrices gave an effect size of 1.1 SD and a Piagetian interview battery gave 1.2 SD compared with controls.

Paradoxically, only if one views the Piagetian account of cognitive development to be false is the testing strategy we used the wrong one. If it is believed to be true, then the Piagetian tests are essential to answer the research question: Were the effects of the intervention due to the model used? Having said which, we must face squarely the fact that in this study, no effects were found on measures of school achievement.

Transfer to Science Achievement

There are two distinct issues about the relation between an intervention program and school achievement.

When should the effect show? If one regards an intervention as a process exhibiting an sigmoid learning curve, it will only be about two-thirds of the way through that there will be enough difference between the thinking of experimentals and controls to make any substantial difference. The right way to test the effect of an intervention on achievement is to wait until it is over, give the controls and experimentals the same fresh learning experience, and then test them on the content, concepts, and processes of the new learning. In this way the test will be of the mature effect of the intervention on subsequent learning ability. On such a view this and all previous intervention studies must be regarded as exhibiting non-statistical type 2 error. It is as though one had found a way of increasing adolescents' strength and reaction speed, and then expected an instantaneous improvement in their tennis performance. Certainly some improvement should be noticed straight away but any good coach will know that only a replanned training program will deliver an increased performance that the underlying increase in competence makes potential. We believe that this applies quite generally to all attempts to improve thinking skills.

A Pygmalion effect? The second issue is more subtle. If one accepts the reasonableness of Rosenthal's (1987) review of the Pygmalion effec literature, then it seems that teachers and students enter into a process of mutual adapatation related to the learning process. The nature of the learning experiences offered by the teacher will depend on her or his view of the classes' capabilities, and the students likewise become accustomed to responding to the teachers' expectations. Following an intervention which affects students' fluid intelligence, it may be quite a long time before teacher and students re-adapt to the new quality of learning that is available. If we develop fluid intelligence without at the same time helping students to develop their learning habits, an important element of "natural" development may be missed. Perhaps one of the reasons that cognitive development is so slow in childhood is that here-and-now thinking is not the whole story. The unseen element is the process of re-adapting their whole spectrum of reality-processing habits to a level which a change in fluid intelligence has made potential.

Inservice for Teachers

From the start of the project it was believed that unless teachers' professional skills are developed appropriately, pupils' development will not be changed. But we were not certain about the quality and quantity of inservice training that would be required for teachers to make the methods their own. In effect we wrote the scripts of the lessons which the teachers gave to their pupils, but cannot yet say that we have shown teachers how to write those scripts for themselves, although some start has been made on that task.

A major project is required to investigate the nature of the inservice training which will enable teachers to generalise the methodology of the intervention lessons, and to review their everyday teaching strategies so that each new topic is approached so as to enable students to apply their developed operational thinking in a more advanced learning style. In this way, perhaps, teachers could learn to exercise their professional skills reliably so that what has been achieved for the boys in these project schools can become a feature of all secondary schooling both for girls and boys. Given this as a major objective, it would be sensible also to assess fresh learning experiences after the intervention, as argued above, firstly in science but also in school subjects completely outside the context of the intervention.

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Table 1

Laboratory school scores on Piagetian Reasoning Tasks

Group	N	Period of test				Gain score	
		Pre-	Mid-	Post-	Delayed post-	Pre-post	Pre-delayed
All							
Experimental	29						
M		5.89	6.46	6.35	7.01	0.46**	1.13*
SD		0.55	0.72	1.00	1.21	1.05	1.12
Control	19						
M		6.46	6.72	6.26	7.01	-0.20	0.54
SD		0.36	0.56	0.87	1.09	0.70	0.86
Boys							
Experimental	15						
M		5.84	6.52	6.35	7.03	0.51	1.19
SD		0.59	0.88	1.14	1.48	1.17	1.36
Control	11						
M		6.54	6.87	6.64	7.27	0.10	0.73
SD		0.41	0.53	0.89	1.05	0.69	0.71
Girls							
Experimental	14						
M		5.94	6.41	6.35	7.00	0.42**	1.06*
SD		0.52	0.59	0.87	0.86	0.92	0.83
Control	8						
M		6.35	6.53	5.75	6.64	-0.61	0.29
SD		0.26	0.56	0.55	1.09	0.49	1.02

Notes * = significantly greater than control at $p < .05$;
** = significantly greater than control at $p < .01$
Time of tests: Pre-test 1/84; Mid-test 7/85; Post-test 7/86; Delayed 7/87.
Mean age of pupils at pre-test: 12/8.

Table 2

Main experiment school scores on Piagetian Reasoning Tasks

Group	N	Pre-	Mid-	Post-	Gain
		Period of test			
All					
Experimental	190				
M		6.10	6.38	6.97	0.87*
SD		0.81	1.04	1.17	1.03
Control	208				
M		6.08	6.30	6.75	0.66
SD		0.99	1.11	1.24	1.04
11+ start					
Experimental	76				
M		6.12	6.48	6.91	0.80
SD		0.83	1.08	1.23	1.11
Control	88				
M		5.96	6.24	6.90	0.94
SD		0.95	1.02	1.05	0.92
12+ start					
Experimental	114				
M		6.10	6.31	7.00	0.91**
SD		0.80	1.02	1.13	0.97
Control	120				
M		6.17	6.35	6.64	0.46
SD		1.03	0.88	1.36	1.09

Notes * = significantly greater than controls at $p < .05$
** = significantly greater than controls at $p < .01$
Time of tests: Pre-test 9/85; Mid-test 7/86; Post-test 7/87
11+ start: mean age at pre-test 12/4. 12+ start: mean age at pre-test 13/5.

Table 3

Boys and Girls scores on Piagetian Reasoning Tasks in main experiment schools

Group	N	Period of test Pre-	Mid-	Post-	Gain
11+ Boys					
Experimental	39				
M		5.85	6.35	6.65	0.80
SD		0.86	1.03	1.23	1.03
Controls	50				
M		5.95	6.42	7.04	1.09
SD		1.17	1.01	1.16	1.01
11+ Girls					
Experimental	37				
M		6.40	6.61	7.19	0.79
SD		0.72	1.14	1.18	1.19
Controls	38				
M		5.97	5.99	6.73	0.76
SD		0.55	1.00	0.86	0.76
12+ Boys					
Experimental	56				
M		6.20	6.50	7.36	1.16**
SD		0.78	1.09	1.07	0.95
Controls	63				
M		6.24	6.37	6.51	0.27
SD		1.03	0.95	1.39	1.01
12+ Girls					
Experimental	58				
M		6.00	6.13	6.67	0.67
SD		0.81	0.92	1.09	0.94
Controls	57				
M		6.10	6.33	6.77	0.67
SD		1.02	0.80	1.32	1.13

Note ** = significantly greater than controls at $p < .01$

Table 4

Pre-post-test gain scores on Piagetian Reasoning Tasks for 11+ & 12+ boys related to pre-test score grouping

Group	Pre-test score range <5.0	5.0 - 5.9	6.0 - 6.9	>7.0
11+ Boys				
Experimental				
M	1.04 (7)	0.83 (14)	0.77 (15)	0.27 (3)
SD	1.08	1.14	1.00	0.84
Controls				
M	1.51 (11)	1.38 (11)	0.89 (22)	0.45 (6)
SD	1.61	0.64	0.64	0.97
12+ Boys				
Experimental				
M	1.86*(9)	0.99*(12)	1.04*(26)	1.01 (9)
SD	0.79	1.02	0.86	1.08
Controls				
M	0.31 (8)	-0.19 (13)	0.47 (30)	0.26 (12)
SD	1.47	1.27	0.76	0.26

Notes In brackets are given the number of boys in each sub-group.

* = Experimental gain score significantly greater than controls at $p < .05$

Table 5

Mean gain scores of each experimental class on Piagetian Reasoning Tasks compared with (a) CASE control groups and (b) CSMS (1976; 1978) survey data.

	Experimentals				Controls			
Age	class	N	pre-mid	pre-post		N	pre-mid	pre-post
11+	0501 Boys	7	-0.30	-0.21				
	Girls	9	0.15	0.19	CASE Boys	50	0.47	1.09
	0502 Boys	9	-0.01	0.78	Girls	38	0.02	0.76
	Girls	8	-0.26	0.45				
	0601 Boys	12	1.01	1.29	CSMS Boys	>700	0.70	0.90
	Girls	11	0.10	0.84	Girls	>700	0.88	0.92
	0901 Boys	11	0.89	0.92				
	Girls	9	0.83	1.65				
12+	0301 Boys	7	-0.41	1.16				
	Girls	8	0.13	0.59				
	0302 Boys	8	0.78	1.08	CASE Boys	63	0.13	0.27
	Girls	6	-0.10	0.78	Girls	57	0.23	0.67
	0701 Boys	8	0.74	0.91				
	Girls	11	0.57	0.92				
	0801 Boys	7	-0.07	1.25	CSMS Boys	>700	0.15	0.43
	Girls	11	0.41	0.90	Girls	>700	0.39	0.62
	0902 Boys	7	1.03	1.72				
	Girls	7	0.30	1.10				
	1001 Boys	7	-0.22	0.67				
	Girls	8	-0.36	-0.05				
	1101 Boys	12	0.20	1.29				
	Girls	7	-0.37	0.30				

Table 6

Changes in class means for 12+ boys expressed as percentiles in relation to the CSMS survey data

Group	Pre-test at 12+	Post-test at 14+	Mean change
All Controls	48th	51st	-3
Class			
0301	56th	32nd	24
0302	67th	45th	22
0701	38th	23rd	15
0801	63rd	36th	27
0902	52nd	17th	35
1001	33rd	25th	8
1101	42nd	18th	24
All Experimentals	49th	26th	23

Figure 1: Cognitive development of CASE ex rimental group over two and a half years compared with a control group. - Laboratory school.

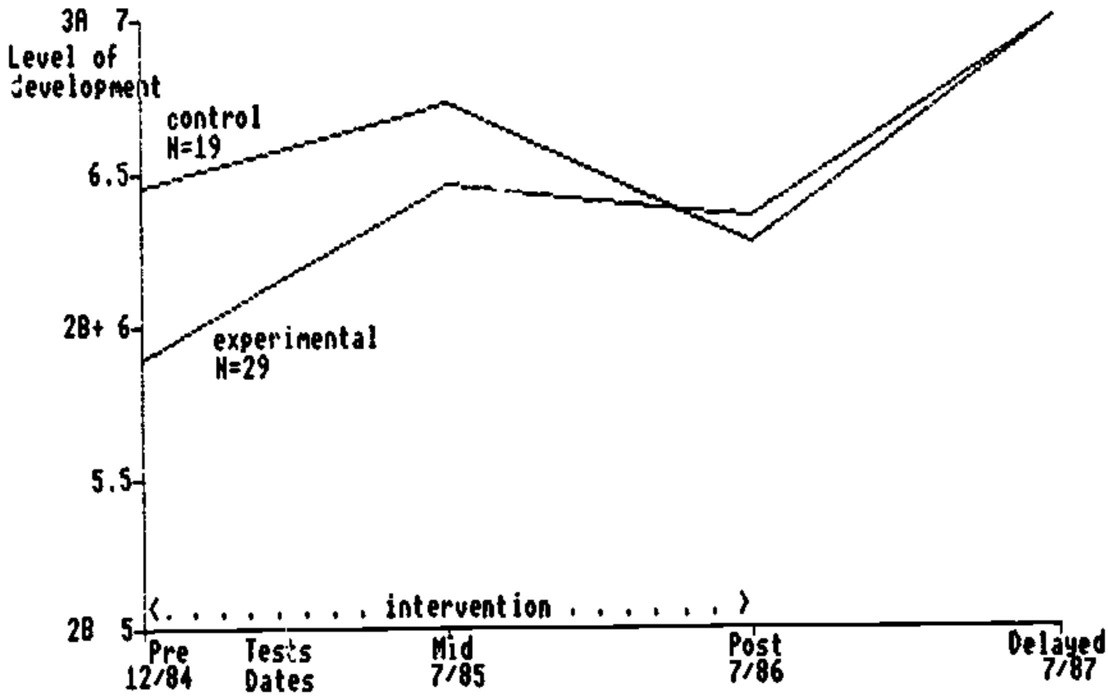

Figure 2: Cognitive development of CASE experimental group over two years compared with a control group - Main Experiment

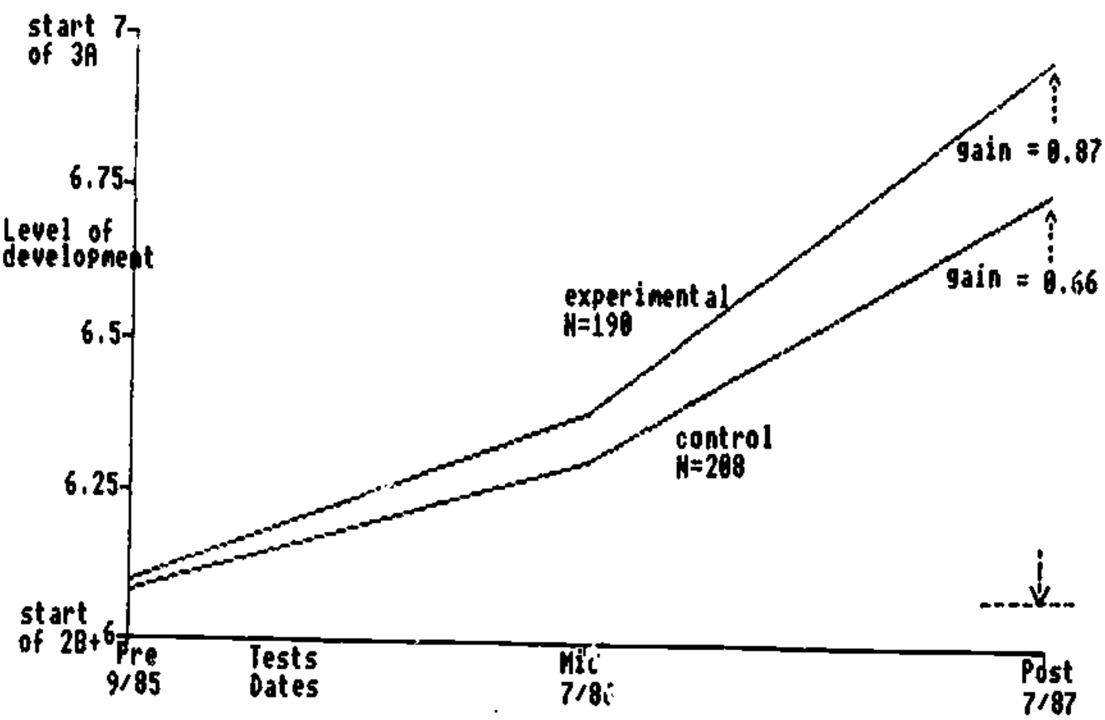

Figure 3: Changes in class means on Piagetian level for boys in the 12+ group compared with changes in the mean for all control classes

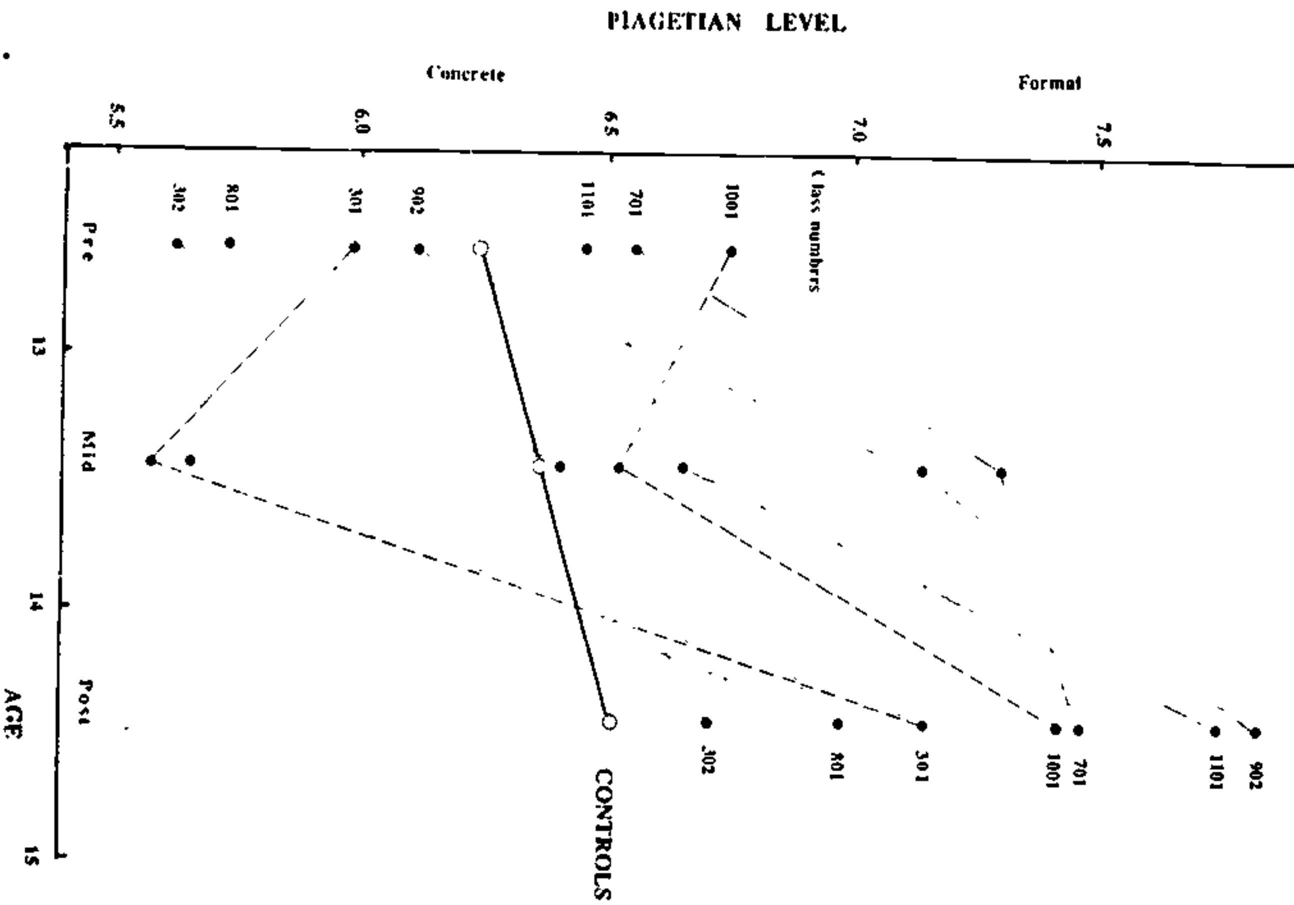

Figure 4: Pre-post test differences for boys and girls in experimental 12+ classes in relation to mean gains of corresponding controls