ABILITY AND INCOME

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I.—INFLUENCE OF INNATE ABILITY AND PARENTAL INCOME ON ENTRANCE TO UNIVERSITIES.

Problems.—In discussions on post-war reconstruction one of the commonest demands is for "equal educational opportunities for all." There is a widespread impression that the children of the poor generally, or (as the more cautious prefer to put it) the brightest children among the poor, are at present prevented by economic handicaps from enjoying the fuller and higher type of education which children from richer homes can secure by simply paying fees. Two questions of fact, therefore, urgently call for investigation: What proportion of the non-fee-paying population are really capable of profiting by higher education? What proportion of these actually fail to obtain it? In a questionnaire recently sent out to about 400 educationists and teachers we found that very few were willing to hazard any precise opinion on these points; and, indeed, what little evidence there is has rarely been subjected to an objective scrutiny.

In London (to take one of the most striking illustrations) a survey[1] of junior county scholarship awards during the years preceding the last war showed that in certain electoral divisions (N. St. Pancras, N. Hackney, Lewisham, Dulwich, and Hampstead) the average number of scholarships annually awarded was about six or seven per 1,000 pupils in attendance; in others (S. St. Pancras, Finsbury, Bethnal Green, S. Islington, W. Southwark, N. Lambeth) it was less than one per 1,000. A study of entrants to the universities reveals a still more startling anomaly. Taking figures for all England and Wales, it appears that, out of a total age-group, comprising something like 700,000 persons, about 660,000 belong to the elementary school or non-fee-paying class, and only 40,000 to the fee-paying class; yet of the former less than 5,000 annually enter the universities, and out of the latter more than 6,000; that is, only 0·7 per cent in the one case, and nearly 15·0 per cent in the other. This means that, if a child's parents can afford fees for his early education, his chances of going to a university are more than twenty times as great as they would be if such fees could not be afforded.

As psychologist to the L.C.C., one of my first tasks was to inquire into the causes for these persistent discrepancies. When from time to time the matter came up for review before the education committee, various explanations were put forward—lack of efficiency in the teachers at certain schools; lack of interest among the parents in their children's educational progress; malnutrition or ill-health in the children themselves; and, most frequently of all, the poverty of the family and all that poverty entails. It may, therefore, be helpful to begin by summarising the more relevant facts, collected at different times during school surveys with the aid of psychological tests and the assistance of the local care committees, and recorded in some of my published or unpublished reports.

Data.—First of all, there can be little question that the intelligence of children, and still more of adults, differs appreciably according to the occupational class to which they belong. Average I.Q.'s are shown in Table I.[2] Within each occupational category, however, the range of variation is enormous. The standard deviations lie between 9 and 14 for adults and 12 and 16 for the children: the range is greatest in the middle categories, and least in the upper.

[1] The data are tabulated in full in the L.C.C.'s annual report on *London Statistics*, Vol. XXIV (1913-14), p. 434. Later figures will be found in *The Backward Child* (1937), Table IV.

[2] These figures were obtained during surveys carried out for the London County Council and the National Institute of Industrial Psychology. The classification follows that which I adopted in our joint *Study of Vocational Guidance* (H.M. Stationery Office, 1926, p. 16).

TABLE I.—INTELLIGENCE OF PARENTS AND CHILDREN CLASSIFIED ACCORDING TO OCCUPATIONS.

Occupational Category.		*Average Intelligence Quotient.*	
		Children.	*Adults.*
Class I.	Higher professional: administrative	120·3	153·2
Class II.	Lower professional; technical, executive	114·6	132·4
Class III.	Highly skilled; clerical	109·7	117·1
Class IV.	Skilled	104·5	108·6
Class V.	Semi-skilled	98·2	97·5
Class VI.	Unskilled	92·0	86·8
Class VII.	Casual	89·1	81·6
Class VIII.	Institutional	67·2	57·3

For the sake of comparison I have expressed the figures for adults, as well as for children, in terms of I.Q.'s. With an adult a statement of the I.Q. is somewhat arbitrary. Roughly, an I.Q. of 100 may roughly be taken as equivalent to a mental age of about 15. Assuming that the step denoted by one mental year after puberty is the same as before puberty, and that the distribution is approximately normal, an s.d. of 16 would mean that the *range of mental age among a hundred adults would be from about* 9 *to* 21 *mental years*. Thus, the dullest would be almost a certifiable defective; the brightest would be as much above the average as the defective is below; and *every intermediate grade between is represented*: there are no gaps.
The correlation between children's intelligence and economic status was found to be approximately ·32.[1] In the L.C.C. elementary schools the children from 'superior' homes were about 10 I.Q. above the average, and those from 'poorer' homes about 10 I.Q. below.[2]

In view of the small differences *between* the groups and the large differences *within* them, it may seem at first difficult to say which line of argument to emphasise. (1) Looking first at the group-averages, it might be argued: if the father is in an occupation where he can earn sufficient income to pay for his child's education, then in all probability his innate ability is above the general average by at least 20 per cent; his child, therefore, inheriting about half that ability, is likely to possess an I.Q. that is higher than the general average by at least 10 per cent. Hence a larger number of scholarship awards and university entrances is only to be expected among children from fee-paying classes. (2) However, on turning to the standard deviations, we observe that, within each economic class, the range of individual differences is far wider than the differences between the average levels of any two classes. Hence, we are tempted to infer that the vastly greater numbers of the non-fee-paying class should more than compensate for their slightly inferior average level; so that, even if geniuses are *relatively* rare among the poor, nevertheless in *absolute* numbers the mute, inglorious Miltons may run to many thousands. Consequently the figures for scholarship awards and university entrances may reveal a gross social injustice. The only way to resolve such a dilemma is to undertake a careful calculation.

[1] This figure is based on a carefully studied composite group of 343 cases, chosen so that the several proportions in each category should correspond with those in the population at large: correlations from larger samples agree, when corrected for homogeneity or selection. Coefficients reported by other investigators appear to be in harmony with the above, after allowing for the differences in heterogeneity that different samples are almost bound to show, e.g., Bryn and Henmon, ·18; Chauncey, ·20; Lawrence, ·22; Gray and Moshinsky, ·25; McDonald, ·26; Duff and Thomson, ·28; Chapman and Wiggins, ·32; Freeman, ·48; Fakuda, ·53; Cattell, ·89 and ·92. (The correlations of Freeman and Fakuda are with cultural status rather than economic; those of R. B. Cattell so exceptional as to indicate some special peculiarity in his data.)

[2] *Mental and Scholastic Tests*, 1921, p. 191. I there further emphasised the eugenic or rather the dysgenic significance of the size of the families (2·9 children in the former group, 5·2 in the latter). This is an important problem to which one of my former research students (Dr. R. B. Cattell) has since devoted special attention.

Potential University Entrants in the Fee-paying and the Elementary School Classes.— The detailed surveys that have been carried out among both school and adult populations enable us to estimate, at least approximately, the number of pupils to be expected within almost any of these social classes and above almost any line of demarcation. In my earlier reports, estimates were made from time to time on this basis for potential entrants to both secondary schools and universities ; and, now that more accurate figures are available for the latter, it seems worth while to review the problem at its final stages once again.[1] The successive steps in the necessary calculations are as follows :

(1) *The University Standard.*—Taking estimates based on the Census of 1931, in the year 1939-40 (the last year for which detailed figures for university entrants are available) the approximate number of persons aged 18 in England and Wales was 709,580. In that year the total number of new full-time entrants to the universities of England and Wales was 10,785, or 1·52 per cent of the total population. Hence, if we assume that the distribution of ability is approximately normal, the borderline for entrance may be assessed as +2·17 s.d. (for other years where similar figures are available it fluctuates between +2·06 and +2·25 s.d.) In terms of the intelligence quotient this may be interpreted as meaning that a student entering the university should have an I.Q. of at least 134·7 ; and this in turn implies that the ability of the university entrant should (roughly speaking) be *at least as far above that of the average person as that of the average person is above that of a borderline defective.*

It would be possible to complete the calculation without embarking on the difficult question : what do this and other borderlines mean, when translated from standard deviations to terms of the I.Q. ? Most readers, however, find it easier to think in terms of an I.Q., so I shall translate my argument accordingly. For this purpose we have first to decide what is the probable standard deviation of the general population in terms of the I.Q.

(2) *The Standard Deviation of the General Population.*—The most reliable figures would seem to be those obtained with ' group tests ' of intelligence similar to those used for junior county scholarship examinations and for the examination of ex-service candidates after the last war.[2] On equating the results with I.Q.'s obtained with the London revision of the Binet scale, I estimate that the standard deviation of the upper half of the curve of distribution is approximately 16 I.Q. This yields the figure for university entrance quoted above, namely, 134·7 I.Q.

[1] In addition to acknowledging my indebtedness to teachers and others who assisted in the earlier surveys, I am particularly grateful to Miss Joan Mawer for compiling much of the data on which the following conclusions are based, and for thus bringing my earlier computations up-to-date. A fuller account of sources and calculations, with detailed tables, will be found in her degree essay on *The Relative Influence of Mental Ability and Economic Class on Entrance to the Universities* (filed at the Psychological Laboratory, University College).

[2] The tests which I drew up for this latter purpose (slightly revised) were subsequently published by the National Institute of Industrial Psychology under the title of ' Group Test No. 33.' They were used regularly for entrants to the London Day Training College, for our own students at University College, for investigations on vocational guidance among adults in various fields of work, and more recently for recruits in the Army. Consequently, a good deal of data is now available. It is advisable, however, to note several complicating difficulties, commonly overlooked in discussions on the general standard deviation. (i) The variability, in terms of the I.Q., is itself bound to vary somewhat with the type of test used : results based on group tests may differ appreciably from those based on individual tests of the Binet-Simon type. (ii) As the efficacy of each type of test is improved, the resulting standard deviation is likely to increase : thus it is generally larger with revised versions of the Binet tests than with the original. (iii) If my own figures can be accepted, it is not the same at every age : in particular it appears to increase towards puberty, and to decline after adolescence is over. (iv) We cannot assume that the amount of variability above the average (or below) can be determined by calculating the amount of variability over the entire sample, i.e., that the curve of distribution is exactly symmetrical, much less exactly normal. In the lower half of the population, disease and other disturbances augment the frequency of the more extreme deviations (as is shown by figures for pathological types of the imbecile grade) ; in the upper half the absence of a definite upper limit to the scale seems (with most tests) to prolong the upper tail still more. There can be no ' mental age ' below zero ; but there is no *à priori* limit to mental ages in the upward direction, so that an I.Q. above 200 is not impossible, while an I.Q. below 0 is out of the question. Accordingly, my use of tables for the normal probability integral to deduce percentages above any given borderline from the s.d. value of that borderline must be regarded as merely a convenient way of smoothing the empirical data. If figures for the higher moments could be more exactly determined, it might be better to work with a hypergeometric curve. Alternatively, we can calculate the numbers above or below specified percentiles directly from the tabulated data. I have tried both these alternatives as checks ; and find little change in the ultimate percentages.

(3) *The Average Intelligence of Fee-Payers.*—We could estimate the expected average for the fee-paying pupils by calculation in much the same way. Thus, with a correlation of ·32 between intelligence and economic status, we should anticipate that the average intelligence of the fee-payers would be about 10 I.Q. above that of the general population. Here, however, it seems better to employ an empirical figure. If I can trust my samples, the average level of the fee-paying pupils is approximately 111·6 I.Q.[1]

(4) *The Standard Deviation of Fee-paying Pupils.*—If intelligence and income are correlated, we cannot assume that the standard deviation of the small group of fee-payers is identical with that of the population at large. However, by using appropriate formulæ to estimate the effects of selection, we can readily deduce the probable size of the standard deviation for the selected sample. The fee-payers amount to between 6 and 7 per cent of the population—say, for purposes of calculation, 6·5 per cent; and the correlation between intelligence, (y, say) and economic status (x, say) is, we have seen, approximately ·32. Hence we have (with the usual notation) $\sigma_y/\Sigma_y = \sqrt{\left\{1 - R^2_{xy} \left(1 - \sigma^2_x/\Sigma^2_x\right)\right\}} = \cdot 955$. Accordingly, if the s.d. in the general population is 16·0 I.Q., then that of the fee-paying section will be about 15·3 I.Q. This estimate agrees with the value arrived at empirically from tests applied direct to a representative sample of fee-payers.

From (3) and (4) it follows that the university entrance standard will differ from the average I.Q. of these fee-payers by 134·7—111·6=23·1 I.Q., that is, by +1·51 s.d. Judging by the curve of normal distribution, therefore, we should expect about 6·55 per cent of the fee-payers to enter the university. As noted above, we should have reached practically the same estimate, had we based our figures directly on the actual distribution of the frequencies at each level, and also kept the whole calculation in terms of the initial s.d.

(5) *Total Numbers at Age of Entering Universities in Fee-paying and non-Fee-paying Classes.*—According to the census there were 725,540 children aged 9 in 1930-31. Of these, as we find from the Board of Education returns, 679,590 were then on the rolls of the public elementary schools, and another 1,793 had been formerly in elementary schools: this makes a total of 681,383 children in the non-fee-paying group. The remaining number, 44,157, can presumably be regarded as members of the fee-paying class. In view of the mortality-rate in children between 9 and 18, we should expect only 97·4 per cent of the elementary and 98·5 of the fee-paying children to survive until 18. The final numbers aged 18 in 1939-40 may therefore be assessed as 663,667 and 43,495 respectively. (The total tallies as well as could be expected with the census estimate, when we allow for migration and other minor factors.)

(6) *Expected and Actual Numbers of Entrants.*—From the report of the University Grants Committee, we learn that, of the entire number of full-time students actually entering the universities during the year in question (viz., 10,785), only 4,531 were ex-pupils of public elementary schools; the remainder, we may presume, namely, 6,254, were drawn from those whose parents had paid for their early education. Now, according to the theoretical proportion as calculated in (4), we should have expected only ·0655×43,495=2,849 to come from the fee-paying classes, and the balance, namely, 10,785—2,849=7,936, to be made up of ex-pupils from elementary schools. The difference between the expected number and the actual number of ex-elementary pupils is 7,936—4,531=3,405. We may, therefore, infer that out of all the ex-elementary pupils who were endowed with sufficient ability to enter a university during the year in question, as many as 42·9 per cent failed to do so.

The figures for the preceding year (1938-9) prove to be much the same: out of an expected number of 7,640 ex-elementary pupils, only 4,341 actually entered, and therefore 3,299—that is, 43·2 per cent—failed to do so. For the earlier years, the post-war fluctuations in the birth-rate, the wide variations in the number of students at different universities, and the inadequate details about entrants, render comparable figures less easy to ascertain. Nevertheless, from 1935 onwards, the proportions seem to have been of much the same order, namely, between 40 and 45 per cent.

[1] This average (like the figure the standard deviation referred to in section 4) is derived from one or two special inquiries described in a *Memorandum on the Influence of Ability and Economic Class on Entrance to Secondary Schools and Universities*: the detailed figures are given in Miss Mawer's thesis cited above. The estimate agrees with what can be inferred from the class-averages shown in Table I, and with estimates obtained from various independent studies. Thus, we found that the average I.Q. of fee-paying children attending secondary schools was 114; of scholarship winners, 133. (Cf. Board of Education, *Report on Tests of Educable Capacity*, pp. 162-4.)

Similar reasons make it difficult to decide whether any progress has been made towards easing the ladder for the poorer child during the last ten or fifteen years. To answer this question it would seem better to estimate what proportion of *all* ex-elementary pupils (i.e., of the total number regardless of ability) have entered the universities. If the calculations can be trusted, it would appear that during 1936-9 the proportion was between 0·6 and 0·7 per cent; during 1925-35 it apparently averaged only 0·4 per cent, or very little more.

There are, no doubt, several questionable assumptions in the foregoing argument. On various grounds, it would seem that the statistical analysis is, on the whole, most likely to have under-estimated both the average and the numbers in the upper tail for the distribution of intelligence among the fee-payers, and to have over-estimated both the standard deviation and the numbers in the upper tail for the distribution of intelligence among the non-fee-payers. On the other hand, early ill-health and lack of intellectual opportunities may (in spite of the most careful precautions and allowances) have tended to reduce both the average and the numbers in the upper tail among the non-fee-payers. After computing the possible effects of such disturbances either way, I think it safe to pronounce that the true proportion cannot be less than 25 per cent nor more than 55 per cent. I conclude, therefore, that in round numbers about 40 per cent, or 2 out of 5, among the pupils from the elementary school, who are capable of a university education, never obtain it. It would, of course, be an error to suppose that every child of sufficient ability —whether girl or boy—either wants to, or ought to, become a student at a university on reaching the age of 18. Yet it seems clear that a considerable fraction, though not (as has sometimes been alleged) "the majority," of those who would and should do so, are nevertheless prevented by purely economic handicaps.

The upshot of the whole analysis may be concisely summarised in a four-fold table. The figures in Table II show the averages for the most recent years for which reliable data are available. They are expressed as percentages of the entire age-group, so as to be independent of any fluctuation in the size of the population from one year to the next.[1] From the figures for the 'expected proportions' we can, if we wish, calculate the tetrachoric correlation between ability and economic status: it proves to be ·341, which accords with the product-moment coefficient as calculated from samples intensively studied, and cited above.

TABLE II.—PERCENTAGES OF TOTAL AGE-GROUP ENTERING OR NOT ENTERING UNIVERSITIES.

Category.	*A.—Expected Proportions.*			*B.—Actual Proportions.*		
	Entering.	*Not Entering.*	*Total.*	*Entering.*	*Not Entering.*	*Total.*
Non-Elementary	0·4	5·8	6·2	0·9	5·3	6·2
Ex-Elementary	1·1	92·7	93·8	0·6	93·2	93·8
TOTAL..........	1·5	98·5	100·0	1·5	98·5	100·0

[1] It will be seen that my figures imply that only 1·20 per cent of the elementary school children (as contrasted with 6·55 per cent of the fee-paying pupils) reach an intelligence level of university standard (roughly 135 I.Q.). The only investigators who have arrived at a conclusion in conflict with this estimate are Gray and Moshinsky. With the help of school teachers they set a group test of intelligence to about 10,000 L.C.C. school children; and, according to their tabulated results, more than 22 per cent were found to have I.Q.'s above 135. The vast majority of these bright pupils, it was contended, were missing the secondary and university education to which they were entitled (*Sociological Review*, 1935, pp. 138 *et seq.*: the results have become widely accepted among social writers owing to the fact that they were reprinted in *A Survey of the Social Structure of England and Wales as Illustrated by Statistics*, by Prof. Carr-Saunders and Dr. Caradog-Jones, 1937, pp. 200 *et seq.*). The investigators, however, were not themselves psychologists; and they employed a test which was not standardised for English school children. Thus, according to their results, over 71 per cent of the children have I.Q.'s above 100, that is, above the average I.Q., which is absurd. From their table we can roughly correct the inappropriate standardisation; and the figures so inferred are consistent with those reported here.

II.—Is ability as assessed by intelligence tests really innate?

The sceptical reader will doubtless question my initial assumption that the higher I.Q.'s found among children of the fee-paying classes really represent inborn differences partly inherited from parents who themselves owe their superior incomes to their superior mental efficiency. Now, as will be obvious from my previous publications, I should be the last to maintain that every child who gets a high (or a low) I.Q. in the Binet tests, or in a written group-test of intelligence, must therefore of necessity be endowed with a high (or low) innate ability: to take only the most conspicuous exceptions, one child may do well in such tests because of his exceptional verbal fluency; another may do badly because he has played truant, and so missed the rudiments or instruction which all such tests presuppose. Nevertheless, the cautious opinions on this matter uttered by psychologists are often, I fancy, misinterpreted by advocates of educational and social reform, owing to the fact that they have so frequently misunderstood the issue in which the psychologist is primarily interested. The psychologist wants, first of all, to know how far the results of each particular test, taken by itself and uncorrected by other information, may be relied upon to reflect the innate abilities of individual children; and he discovers that, with this test or with that, their performances and their scores are, in certain cases at any rate, appreciably affected by environmental advantages or handicaps.[1] But, when he turns from the theoretical question of test-reliability to the practical task of assessing the innate ability of Harry or Tom, he would never rely merely on a single automatic test-measurement, unchecked by any other observations. Yet the social and educational workers who note his careful reservations are apt to infer that all variations in intelligence as such, however carefully they have been measured and checked, must largely depend on environmental conditions.

Much of the controversy has arisen because the terms employed are not always explicitly defined. The definitions now accepted pretty generally in this country have been reached in the following way: (1) The earliest experiments appeared to demonstrate that a general cognitive factor enters into all that we say or do or think, and accounts for quite 50 per cent of the variance displayed in these different processes (practical as well as intellectual) whenever they are quantitatively assessed. This hypothetical general factor, so far regarded simply as an abstract statistical concept, was conveniently designated *g*. (2) Subsequent experiments appeared to indicate that the greater portion of this general factor (possibly the whole of it, could it be measured with precision) is dependent on the individual's innate or hereditary constitution. This innate general cognitive factor is what psychologists understand by the word 'intelligence': indeed, from Binet onwards practically all the investigators who have attempted to construct 'intelligence tests' have been primarily searching for some measure of *inborn* capacity, as distinct from acquired knowledge or skill.

With such an interpretation it obviously becomes foolish to inquire how far 'intelligence' is due to environment and how far it is due to innate constitution: the very definition begs and settles the question. The proper points to ask are really these: First, how far does the innate factor of intelligence determine successful performance in this or that test, or in this or that concrete achievement (e.g., school progress or industrial efficiency)? And, secondly, how far does the innate factor of intelligence differ from one family to another, or from one social or economic class to another? To gain a rough

[1] The more important studies on this problem are admirably summarised in Sandiford's *Foundations of Educational Psychology* (1938, pp. 71-135, which includes a full bibliography). Of the numerous researches the greater part have been carried out in the United States, and the general verdict of American psychologists is perhaps best expressed by Barbara Burke: "Home environment contributes about 17 per cent of the variance in I.Q." (as actually tested); "parental intelligence accounts for about 33 per cent; and the total contribution of innate and heritable factors is probably not far from 75 or 80 per cent": with tests of the Terman-Binet type, uncorrected by any supplementary evidence, about "70 per cent of the children tested obtain an I.Q. within six to nine points of that representing their innate intelligence" (*27th Yearbook*, 1928, p. 309). Sandiford sums up the matter in a sentence: "With intelligence *as measured by intelligence tests*, the contribution of heredity is about four times as potent as that of home environment" (*loc. cit.*, p. 95).

answer we may use the ordinary imperfect tests, and accept the I.Q. (corrected or uncorrected) as the best available measure of the individual's inborn ability ; even if it turned out that an I.Q. obtained with some one particular test was largely dependent on the child's health or educational opportunities, that would not suffice to demonstrate that ' intelligence ' (in the psychologist's sense) was not inborn.

Actually, I imagine, most psychologists believe that differences in intelligence are innate, not merely because of the results obtained with the standard tests of intelligence, but rather because of the vast mass of converging evidence, consisting partly of general inferences, and partly of data procured by various methods of observation, including tests quite different from the standardised scales in practical use and familiar to the educational student. Perhaps, therefore, it will be helpful to summarise quite briefly what appear to be the most convincing lines of argument, and (since some writers have doubted whether it is fair to apply American conclusions to English children) to illustrate those arguments, so far as space allows, from material collected in British schools during inquiries carried out by myself, my colleagues, or my research students.[1]

(1) Social reformers in this country have always been deeply impressed with the powerful influence of education or the lack of it, and, until the days of Darwin, tended to ignore the influence of heredity, at any rate within the human race. Their philosophic affiliations incline them to accept Locke's doctrine of the new-born mind as a *tabula rasa* ;[2] and their more up-to-date adherents think they can discover scientific support for their views in the pronouncements of the American behaviourists. They quote Watson's declaration : " There is no such thing as an inheritance of capacity."[3] Yet even Watson acknowledges hereditary differences in structure ; and ' intelligence,' as the psychologist understands it, must depend essentially on the structural organisation of the brain or central nervous system (and doubtless on its chemistry as well). Since for almost every characteristic that is not directly indispensable for mere survival, innate difference is the rule throughout the animal kingdom, it would be all but inconceivable to the biologist if human intelligence were identical in every normal individual, and if the mental defectives and the geniuses were freaks and exceptions.

(2) These *à priori* inferences, however, call for direct verification by empirical means : and the actual existence, and still more the extent, of such differences can only be determined by statistical surveys based on a properly controlled experimental technique. These reveal that every intermediate grade, from mental deficiency up to the highest genius, is fully represented in the general population. Variety, not uniformity, is everywhere the rule, however uniform the environment.

(3) But here as elsewhere it is exceedingly difficult, if not impossible, to draw a rigid line between what is hereditary and what is environmental. Nevertheless, in many researches an attempt has been made to devise tests (often of the nature of laboratory experiments) on which the superior cultural conditions of the successful child could have had no helpful influence—indeed, if anything, rather the reverse. Thus, in what I believe was one of the earliest studies of the problem, a series of experimental tests of a sensori-motor type, and of varying degrees of complexity, were applied both to children of elementary schools and to children

[1] Some of the inquiries have been published in L.C.C. reports or elsewhere : but the majority remain buried in typed memoranda or degree theses. I should like to repeat my acknowledgments to the many workers who assisted me.

[2] It was the traditional doctrine handed down from Aristotle and the scholastics to Descartes. Descartes opens his *Discourse on Method* by announcing that he is " disposed to adopt the common opinion of philosophers, who say that the difference of greater or less holds good only of *accidental* characteristics," and that, in their " essential form or nature," all individuals of the same species are identical : further, since " it is reason alone that distinguishes us from the animals and constitutes us men," reason must be " complete in each individual " ; it therefore follows that " what is called reason or good sense must be, by nature, *equal in all men*." (Cf. Helvétius : " La grande inégalité d'esprit qu'on apperçoit entre les hommes dépend uniquement de la différente éducation qu'ils recoivent." *De l'esprit*, 1758, III, 26.) Descartes' argument seems to express explicitly the feeling of the modern social reformer. It may be added that, if we re-interpret the scholastic phrase ' essential nature ' to mean those characteristics directly needed for survival, and ' accidental ' to mean, not those due to the accidents of time and place in the individual's life-history, but rather those which are not absolutely indispensable for survival, then the Cartesian premises, but not the conclusion, might still be accepted by any modern biologist.

[3] WATSON : *Behaviourism* (1930), p. 94 ; but cf. *ibid.*, p. 100.

attending a preparatory school, who were sons of Oxford professors and lecturers. In this and several subsequent researches it appeared that, the more the test was saturated with the 'general factor,' the higher were the performances of the children of abler parents; and the more it depended upon educational acquirements, the higher were the performances of the elementary children, who came from somewhat poorer homes, but who at these earlier ages had received a better grounding in the more fundamental school-subjects. Further, it was in the complex tests, i.e., in those depending most on the 'general factor,' that the correlations between parents and children, or between brothers and sisters, were found to be greatest.[1]

(4) The differences between individuals in the same economic class prove to be far wider than the differences between the averages for different economic classes. Thus, numerous children from the poorest homes, brought up under the most unfavourable conditions, achieve I.Q.'s of 130 or above; while others from the most comfortable and cultured homes get I.Q.'s of only 70 or below. If the high I.Q.'s obtained by the average members of the better classes are to be attributed chiefly to their environmental advantages, how can we explain the low I.Q.'s of so many others in those classes, or the high I.Q.'s of poorer children?

(5) Current handicaps, arising from environmental conditions, such as physical ill-health, lack of cultural opportunities, or passing emotional disturbances, as a rule make very little difference to the I.Q. when properly assessed. In following up cases of various types, I have encountered many instances where the child's home conditions have been vastly improved, and still more where they have rapidly deteriorated: yet, even after five or ten years in the changed environment, the I.Q. seldom alters greatly. This conclusion is further confirmed by re-testing evacuated children after two years or more in their new surroundings. Even prolonged disease or malnutrition, as Shepherd Dawson has shown, exerts very little influence, provided the nervous system itself is not directly attacked.

Yet this, to my mind, does not altogether dispose of the possibility that poverty and its concomitants may permanently impair 'intelligence.' If bad feeding, infectious disease, and the like exert any serious influence on mental ability, the damage, I believe, is most likely to be done *during the first few years of life, before ever the child comes to school*: and such impairment, I can readily imagine, might be lasting. The real question, therefore, is—how frequent and how serious are the effects of such pre-school handicaps?

(6) To this question the best reply is to be found in comparative studies of children at residential schools and orphanages, where the inmates are received during early infancy, and where the environment is virtually the same for all. Such data are not easy to procure on any large scale; but the following results may be cited from one of my earlier reports.

In inquiries on children adopted, boarded out, or transferred to residential institutions, an endeavour was made to compare the intelligence of the children with that of their parents. These inquiries differed somewhat from similar researches reported by American investigators. Unlike the theoretical investigator, the school psychologist attached to an education authority is rarely content to assess the I.Q. of a doubtful or special case on the basis of a single test alone; even if he uses the Binet scale as his chief stand-by, he regularly supplements it by others (performance tests, for example, or tests of reasoning); and, before he reaches his final verdict, he will make numerous allowances for disturbances due to shyness, emotional instability, ill-health, reading disability, fatigue, lack of interest, and the like.[2] The I.Q.'s

[1] BURT: "Experimental Tests of General Intelligence."—*Brit. J. Psych.*, III (1909), pp. 175 *et seq.*: "The Inheritance of Mental Characteristics."—*Eugenics Review*, IV (1912), pp. 180 *et seq.*

[2] If these allowances are not made, then improved (or depressed) environmental conditions appear to raise (or depress) the I.Q., as assessed by the Binet scale with younger or duller children or by group-tests with older children, by about five or six points. In exceptional cases (about once in a thousand cases) the distortion may amount to as much as fifteen points. The experienced psychologist, of course, always endeavours to detect and allow for such distortions, before declaring that the child is mentally defective or reporting on his case to the school authority. The need for such corrections was admirably shown by the results obtained by Mr. Hugh Gordon, H.M.I., with canal boat children. He found an average I.Q. with the Binet tests of 69. When, at my suggestion, Dr. Frances Gaw applied performance tests to the same group, she found an average I.Q. of 82 (cf. *The Backward Child*, p. 59, and refs.). I may add that, in my experience, most of the alleged 'cures' of certified mental defectives are usually obtained with children certified by doctors untrained in the pitfalls of psychological testing, who have diagnosed mental deficiency by simply taking at its face value an I.Q. based on the printed version of the Terman-Binet scale (which was not standardised for English children) without any further adjustments.

of the residential pupils were first assessed in this way ; and subsequently the desired information procured about the parents from independent investigators. It was found that, even among children whose mothers belonged to the poorest or most undesirable classes, there were a small proportion having I.Q.'s well over 100. In such cases we commonly learned later on that the child was the illegitimate offspring of a father belonging to a superior social class.

During a period of fifteen years it was possible to accumulate many instances of this kind. Thus, my records included 67 cases[1] where the mother's I.Q. was apparently between 70 and 85, but the father's I.Q. was apparently between 120 and 145 ; the average I.Q. of the children was 103·2. As a control-group I took a second batch of children (105 in number) brought up under the same circumstances, with mothers whose I.Q.'s ranged between the same limits and fathers whose I.Q.'s ranged between 65 and 100 ; for these the average I.Q. was 88·6. The standard deviations were 14·3 and 12·1 respectively. The difference, therefore, was 14·6 I.Q., and its standard error 2·1. The odds are enormously against so large a difference being the result of random sampling ; and, since both the pre-natal and the post-natal conditions of the children must have been much the same, it seems impossible to escape the conclusion that the difference in their I.Q.'s was the effect of a difference in heredity.

Among 157 children boarded out in foster-homes the following correlations were obtained : (i) I.Q.'s of brothers and sisters in the same homes, ·51 ; (ii) of brothers and sisters in different homes, ·42 ; (iii) of foster-children with foster-parents' own children, ·27 ; (iv) economic status of foster-parents and of foster children's own parents, ·24. With coefficients of this size, the p.e. is approximately $\pm$·05. Thus the small correlation between unrelated children in the same home can be almost wholly accounted for by an occasional and very natural tendency to place foster-children in homes resembling those from which they have come.[2]

(7) To obtain cases where the *environment* is practically identical, the psychologist, as we have seen, goes to residential institutions : to obtain cases where the *heredity* is practically identical, he turns to the study of ' identical ' twins. Since the days of Galton and Thorndike, numerous investigations have been made in this very suggestive field, particularly in America. In London, during a survey with the Binet tests covering 3,510 children,[3] we found 68 twins of whom 19 appeared to be ' identical ' (monozygotic). During subsequent years an additional 121 cases have been added to the data. The correlations between the I.Q.'s are as follows : non-identical twins (156 cases), ·54 (little, if at all, higher than for ordinary brothers and sisters) ; twins of like sex and ' identical ' in type so far as could be judged (62 cases), ·86 (almost as high as the correlation between two successive testings of the *same* individuals : in the few cases (15 in number) where the ' identical ' twins had been reared separately the correlation was ·77). And, in general, the remoter the family relationship the smaller the correlation : e.g., between first cousins (167 cases), ·30 ; second cousins (86 cases), ·24.[4]

As regards acquired educational attainments, I will only note one suggestive point. Both for twins and for ordinary brothers and sisters, the average correlations are decidedly higher for brighter children than for duller (with sibs over 100 I.Q. it is ·61 ; with sibs under 100 I.Q., only ·47). Thus, paradoxically enough, the influence of a good environment appears most

[1] There were in addition a few cases in which I learnt that the father had made special arrangements for the mother's care just before or just after the birth of the child. These I have omitted.

[2] For the data relating to these boarded-out children I am indebted to Miss Conway, who was good enough to carry out the inquiry at my suggestion. She reports that, had the I.Q.'s been estimated solely on the Binet scale, the correlation between foster-children and the foster-parents' own children would have risen to ·36.

[3] *Mental and Scholastic Tests*, p. 131.

[4] All the above correlations have been calculated by Fisher's formula for intra-class correlation. American investigators have used either the ordinary product-moment formula or the Otis difference formula (which assumes that the means for the two series are identical). A novel method of analysis was attempted by Miss V. Molteno, who up to the outbreak of the war, was working up data obtained for twins in London. She has applied the alternative technique of ' correlating persons ' to numerous assessments for a variety of mental characteristics (collected by herself and Dr. R. B. Cattell). The research unfortunately remains incomplete, but indicates, so far as it goes, that the qualitative resemblances between twins are even more striking than the quantitative. (For references, cf. Cattell and Molteno, *J. Genetic Psych.*, LVII, 1940, pp. 31-47 ; Herman and Hogben, *Proc. Roy. Soc. Edin.*, LIII, 1933, pp. 105-129.) American investigations on twins are fully summarised by Sandiford (pp. 98-121) ; on comparing the figures it would seem that, with twins, the correction of the I.Q. (as carried out in our own cases) does not, as a rule, greatly alter the results. There is one minor exception. Most observers report that, if anything, the I.Q. tends to diminish with age ; if confirmed, that, of course, militates against the theory that the resemblance is the cumulative effect of similar environments. We ourselves, however, have so far found no significant difference at different ages.

conspicuous where the influence of good heredity is also most conspicuous. There is an obvious practical corollary: it is *far more urgent to provide brighter children with an education appropriate to the ability of each than to do so for the dull, the backward, or the defective.*

Taken together all these items of evidence strongly corroborate Galton's hypothesis that the intellectual achievement of individuals depends largely on a capacity which is inherited or, at any rate, inborn. It is this inborn capacity, as we have seen, which intelligence tests have been constructed to measure and the I.Q. designed to assess. If, as now appears, they test and assess it pretty successfully, it follows that the differences —not very wide, but fully established—between the average intelligence of different social classes are themselves largely innate. The implication seems clear. However much the education and the health of children in the poorer classes are improved, we shall not succeed in raising their average I.Q.'s (when properly assessed) by more than a very few points. It therefore becomes all the more urgent to discover those numerous individuals, in the poorer as well as in the wealthier classes, who are endowed at birth with high native abilities, and to give them the full measure of education which their superior intelligence deserves.

III.—The relation between the distribution of ability and the distribution of income.

So far I have argued that differences in income, and in economic and social advantages generally, cannot form the sole or even the main cause of the observable differences in mental ability. Is it, then, reasonable to conjecture that these differences in innate mental ability may after all form the main cause, though not perhaps the only cause, of the wide differences in income or earnings? If that were so, the first and most obvious consequence would be that the distribution of individual ability would resemble the distribution of private incomes.

Accordingly, in our surveys of mental ability, one of the first questions to decide (if I may quote the terms of my earlier *Report*) was this[1]: "Is intelligence distributed like income, where those who have little are the commonest type and those who have much are few and far between? Or is it distributed like height and other physical characteristics, where the average type is the commonest, and the dwarfs and the weaklings are almost as rare as the giants and the strong?" As we have seen, the results obtained seemed definitely to favour the latter hypothesis; and with this general conclusion most psychologists, I imagine, would now agree. If, however, we accept the theory of a normal (or nearly normal) distribution, how are we to account for an amazing disparity between the ascertainable curve for incomes and the assumed curve for general ability?

From the figures published by the Board of Inland Revenue and other authorities we may calculate that the average income in this country is about £180; the figures for surtax show that more than sixty persons have incomes of above £100,000, and the largest incomes

[1] *Distribution of Educational Abilities* (1917), pp. 34 *f.* and Fig. 6; *Mental and Scholastic Tests* (1921), p. 162 and Fig. 24. My conclusion in these and other cases was that the distributions were "only *approximately* normal": on applying the recognised statistical test for 'goodness of fit,' the departure from normality proved to be significant in every instance (P always less than ·01). Dearborn (*Intelligence Tests*, 1928) reproduces for comparison curves from various investigations in America: "In all," he says, "the distribution is symmetrical and continuous" (and, one might add, approximately normal); "practically the same range and distribution of individual differences in intelligence which were found by Burt in the schools of London are found in the schools of Boston" (p. 85; cf. pp. 150 *et seq.*). In a paper on 'The Mental Differences between Individuals' (*Brit. Ass. Ann. Rep.*, 1923, p. 229), Fig. 1, I later gave results for 8,599 adults. Here the conclusion was the same—approximate normality only. (I may add that data from intelligence tests now being applied in the Army seem in complete conformity with these earlier inferences.) More recently, however, Thorndike has applied the same test of significance to pooled distributions for the sixth, ninth, and twelfth grades in American schools and for freshmen at American colleges: he obtains, in every case, P = ·9999 or more (*Measurement of Intelligence*, 1927, pp. 521-56; cf. pp. 271-87). Here, however, it seems important to recall the criticisms passed by Fisher and others on such high values for P: "extremely close agreement throws as much suspicion on the hypothesis or the technique as extreme disagreement" (cf. *Statistical Methods*, p. 83).

of all run to over half a million.[1] In the graph for the distribution of intelligence (*The Distribution of Abilities*, Fig. 6), the printer has allowed about two inches for the frequencies below the average ; to plot a frequency-curve for incomes on such a scale would require a graph running to over 500 feet in length. To put it another way, if human stature, instead of obeying the normal curve, followed that of incomes, then our richest millionaires would be giants three miles tall, with heads like Mount Blanc capped in perpetual snow.

Prof. Pigou has endeavoured to reconcile the two different distributions in the following way. He agrees that " on the face of things we should expect that, if people's capacities are distributed according to the Gaussian curve of error, their incomes will also be distributed in the same way." But, as he points out, a normal distribution of capacity might easily hold good within the more or less homogeneous groups that have been examined, without holding good of the composite population as a whole. " Brain-workers may constitute one homogeneous group, hand-workers another, but jointly they do not ; thus the normal law would rule in each separately, but not in both together."[2] The wider psychological surveys, however, put this suggestion out of court. Intelligence tests have now been applied to large and comprehensive samples, including school children of every social grade, adults of almost every occupation, and (within the last year or two) thousands of recruits for the Army. The results make it perfectly clear that, although the distribution of ability does not perfectly conform with the normal curve, nevertheless the amount of skewness is much too slight to bear out the explanation Prof. Pigou has suggested. The deviations from normality exhibited by different distributions can be readily compared by computing the appropriate functions of the higher moments (beta-functions) ; for the normal curve $\beta_1 = 0$, $\beta_2 = 3$; for most distributions of intelligence quotients, β_1 lies between 0·0 and 0·2, and β_2 between 2 and 4 ; for curves of income in Great Britain at various dates, $\beta_1 = 1{\cdot}2$ (approximately), $\beta_2 = 50{,}000$ or more.

Of the few other economists who have touched upon the psychological problem, the majority seem disposed to abandon the notion of a normal distribution altogether. In particular, Pareto, and still more Pareto's followers in the United States, have declared that the elongated curves of income-distribution can be no economic accident, but represent an iron law resulting from an " inexorable biological fact."

Carl Snyder, for instance, has recently come to the following conclusion : " Where differences of attainment are concerned, the frequencies do *not* follow the pattern of the normal curve : the number of persons superior to the mode tends to be much smaller than the number inferior. The explanation is obvious. High achievement is always due to a combination of *several* fundamental faculties : hence, the number of persons with exceptional artistic ability (for example) is far less than the number with average talents " ; and, to support this view, he cites Seashore's figures for the distribution of musical ability.[3]

Similarly, Prof. Harold Davies maintains that " the Pareto law is only one example of a much more general law of inequality, which we might refer to as the *law of the distribution of special abilities*. . . . One of the strongest arguments *against* the Binet I.Q. as a measure for the higher levels, is the fact that abilities as measured by it are made to conform to the normal curve." With the Binet scale " the addition of a unit at a high level is considerably *more* difficult than the addition of a unit at a low level." On the other hand, " in playing billiards the addition of one billiard to a run of x is no more difficult than the addition of one billiard to a run of x' " ; similarly, in working for an income, " it is not improbable that to add one dollar to actual income is approximately the same at each level," e.g., whether your income is $100,000 or only $1,000. Hence, he believes, the symmetrical curve of I.Q.'s does a flagrant injustice to the actual spread of high abilities towards the upper end of the scale.[4]

[1] These figures are based on the latest accessible returns. For earlier years, and for a discussion of the sources of information, see Colin Clark, *National Income and Outlay* (1937), p. 109 *et seq.*, and refs.

[2] *Economics of Welfare*, 1924, pp. 608-9. Pigou and Hugh Dalton (*The Inequality of Incomes*, 1920, p. 128) both insist that " the facts of bequest and inheritance of property " must tend to skew the curve of income still further. The same objection was urged against Pareto's claim (that the ' law ' of income-distribution is the direct result of a ' biological fact ') by Benini (*Principii di Statistica Metodologia*, 1906, pp. 310 *et seq.*). However, it now seems generally agreed that, although the inheritance of property must unquestionably magnify the pre-existing asymmetry in the income-curve, it cannot account for that asymmetry entirely, or even to any large extent.

[3] *Capitalism the Creator* (1940), chaps. xiv. and xv.

[4] *The Analysis of Economic Time Series* (1941), p. 427.

It seems, therefore, incumbent on the psychologist to examine more closely this 'general law of inequality,' which these writers propose to substitute for the normal law. Pareto[1] has expressed his 'universal law' for the distribution of earnings by a simple mathematical equation, $N = \frac{C}{x^a}$, where N is the number of persons whose income exceeds x units, and C a constant; the index or exponent, a, measures the inequality of the incomes: according to Pareto, its value cannot vary greatly from 1·5; according to the actual data it appears never to fall below 1 and seldom to be greater than 1·67.[2] Assuming the variables to be continuous, and differentiating Pareto's equation, we can express his formula in terms more familiar to the statistical psychologist. We obtain $y = \frac{aC}{x^{a+1}}$, where y denotes the proportionate number of persons having an income of £$(x \pm \frac{1}{2}dx)$. Such an equation describes, not a symmetrical, but a J-shaped curve, belonging to Pearson's Type XI.[3] In old schemes of marking a J-shaped distribution seems often to have been tacitly assumed: the vast majority of pupils merely 'passed'—i.e., satisfied the minimum requirements; a smaller proportion were awarded a third class; fewer still a second; and fewest of all a first; while one or two individuals, standing out from the rest, achieved a 'mark of distinction.' In the moral sphere, too, as F. H. Allport has noted, what he terms the 'J-curve of conforming behaviour' is apt to "appear in place of the chance-biological (normal) curve."[4] Many of these distributions can be plausibly fitted by means of the foregoing formula.

But I am tempted to simplify Pareto's formula still further, and to suggest that, in the case of income at any rate, the initial value of a is approximately unity and that it is augmented to 1·5, or rather more, by various artificial circumstances, peculiar to the country or the time (e.g., the manner in which property is inherited and taxed). If this were done, the fundamental law would reduce to a simple law of the inverse square, viz., $y = \frac{C}{x^2}$; and therefore $N = \frac{C'}{x}$, or Nx=Constant.

To the psychologist, familiar with the text-book curves for the distributions of mental abilities, all these equations may wear an unaccustomed aspect. Yet analogous laws are by no means difficult to find in the physical world. Thus, with a gas expanding adiabatically, $P = \frac{C}{V^a}$; and the rate of decrease of pressure (P) per unit increase of volume (V) is consequently $\frac{aC}{V^{a+1}}$, where a is never less than 1, and never exceeds 1·67. If we put $a = 1$ (as in isothermal expansion) we have PV=Constant, the equation known to every schoolboy as the formula

[1] *Cours d'economie politique* (1897), II, pp. 299-345. Both Bowley and Stamp have shown that (with certain reservations) the law is applicable to British incomes. Lord Stamp fitted Pareto's formula to the early returns of the British super-tax; and, on the strength of the discrepancies, informed the Inland Revenue authorities that they must have missed over 1,000 payers in certain classes. He adds: "They promptly went and found them!" (*Wealth and Taxable Capacity*, p. 83.)

[2] Most observers, however, seem now agreed that, instead of remaining relatively constant, it has (during the past half century at any rate) shown a discernible tendency to decline: cf. A. L. Bowley, *ap. Select Committee on Income Tax*, 1906; *Evidence*, p. 81.

[3] For the fitting of such a type, see Elderton, *Frequency Curves*, p. 110. Elderton, curiously enough, remarks that he has "not come across a distribution really represented by Type XI."

[4] *J. Soc. Psych.*, V (1934), pp. 141 *et seq.* What about those who do not conform, or who fail in the examination, or have incomes below the mode? These have to be treated as rare exceptions beyond the pale of the J-law: in the same way the initial rise of pressure in experiment on Boyle's law, and the extreme cases in experiments on Weber's law, used to be treated as exceptions to the theoretical curve, not as part of it. It would seem better, however, to meet the difficulty by regarding the Pareto equation as a first approximation to a Type V or VI formula: an instructive modification of this kind has indeed been proposed by one of his Italian followers (Amoroso, 'Ricerche intorno alla curva dei redditi,' *Ann. di Matem.* II, 1925, pp. 123-60). The psychologist would probably think first of rescaling the base line by taking a logarithmic function of income, and then using the ordinary formula for the normal distribution; and, in point of fact, except for the highest incomes of all, this device has been claimed to give a very plausible fit (Gibrat, *Les inégalitiés économiques*, 1931): but the fit is a poor one for British incomes.

for Boyle's law.[1] The non-mathematical reader will perhaps more easily grasp the implication of the simplified expression I have proposed if he recalls the numerous examples of the law of the inverse square occurring in other fields : e.g., its appearance in measuring the attractive force of gravitation, magnetism, electric charges, heat, light, and sound, radiation, and the like, and, indeed, any effect radially and uniformly distributed from some central point. In sound, for instance, the intensity or loudness of a noise diminishes in inverse proportion to the square of the distance of the receiver from the source.

The analogies from physical dynamics are, I venture to think, not so far fetched as they may seem. In estimating the mental output of a human being or a human community, it is natural to begin by imagining a simplified working model, just as in thermodynamics we start from the notion of an ideal machine. And the calculations appropriate to such a model will naturally be expressed in terms of familiar dynamic concepts, whether or not they obey the familiar laws. Unfortunately, in discussions on what may conveniently be termed psycho-dynamics, owing to a confusion between the metaphorical and the strict meanings of the terms, 'capacity for work' has been identified with mental 'energy'; and mental 'energy' in turn has been identified with 'general intelligence' as measured by the usual tests. At the same time, amount of work is measured by actual output; and since, in physics, energy as capacity for work is itself measured by amount of work done on actual trial, psychologists have apparently assumed that the distribution of output (and therefore the distribution of payment for output) should follow the same law as the distribution of mental capacity, whether or not that is expressed by the Gaussian or 'normal' curve. This I hold to be a fallacy.

If I take a large number of my students, I find that, with intelligence-tests or academic examinations, the marks measuring their 'ability' conform pretty closely with the normal curve.[2] Yet, when I collect records of their output as psychologists in later life, I find that the frequency-curve is not even approximately normal, but J-shaped; and this holds good in many other fields of human output for which detailed data are available. May I give one simple illustration of a type that every reader can verify for himself?

Let us take the latest publication of sufficient size on educational psychology—Prof. Valentine's *Psychology of Early Childhood*—and let us study the output records of the chief workers in this sphere as shown in the index of authors. It contains just over 200 names. How great have been the contributions of these writers as assessed by the number of references to the works of each one?

An exponential law (like that of cooling or diminution of pressure with increase of altitude) yields a very poor fit. Let us therefore turn to the figures deducible from the simplified formula suggested above, viz., $y = \frac{1}{x^2} \cdot \frac{1}{\Sigma \frac{1}{x^2}}$ or in percentages, $y = \frac{100}{1 \cdot 645\, x^2} = \frac{60 \cdot 8}{x^2}$, where x is the number of references, and y the number of psychologists whose output has been sufficiently large or important to be referred to x times. The actual and the calculated frequencies are shown in Table III. Now the fit is surprisingly close.

Should frequency of reference be thought to indicate qualitative value rather than quantitative amount, it is quite as easy to procure a direct measure of individual output from the indexes of various psychological journals. In general, the exponent of x, namely $(a+1)$, hovers between 1·5 and 2·6, exactly as the simplified version of Pareto's formula requires.[3]

It appears evident, then, that individual output as thus assessed does not follow the normal curve, although individual ability conceivably may. But I venture to suggest that the apparent inconsistency between the two distributions vanishes directly we recognise that the functional relation between output (as effect) and capacities (as causes)

[1] Other parallels are the law relating rate of working and resistance in an electrical conductor circuit, and the laws of friction in mechanical processes. At the Ministry of Munitions, during the last war, I found that the 'output' of the heavier howitzers (number of rounds fired during its life) and the 'output' of accidents among munition workers both gave frequency-distributions conforming approximately to the formula just cited.

[2] Miss Harwood has recently analysed the marks of many groups of candidates sitting for two or three typical university academic examinations over a period of years; and finds that, even when no instructions are given the examiners about the allotment of such marks, they nevertheless show an approximately normal distribution, i.e., the prior attempt to admit only suitable candidates on entrance has not skewed the distribution so much as might be supposed.

[3] I may add that Miss Stevenson has recently analysed a number of output-curves in this way; and further confirmed this result.

may be of many different kinds, and indeed is more likely to be indirect and complex than immediate or simple. Thus, we may willingly grant, with Snyder, that " achievement of a high sort " is the ultimate resultant of a " *combination* of fundamental faculties " (or abilities). But then we must go on to observe that everything really depends on *how* they are combined.

TABLE III.—FREQUENCY CURVE FOR OUTPUT IN EDUCATIONAL PSYCHOLOGY.

No. of References (x)	1	2	3	4	5	6	7	8
No. of Psychologists (y) : (i) Actual	121	32	12	9	6	2	4	2
(ii) Calculated	122·1	30·0	13·6	7·6	4·9	3·4	2·5	1·9
No. of References (x)	9	10	11	12	13	14	15	16
No. of Psychologists (y) : (i) Actual	3	2	1	2	1	1	0	0
(ii) Calculated	1·5	1·2	1·0	0·9	0·7	0·6	0·5	0·5
No. of References (x)	17	18	19	20-23	..	24	..	27
No. of Psychologists (y) : (i) Actual	0	1	1	0	..	1	..	1
(ii) Calculated	0·4	0·4	0·3	0·3	..	0·2	..	0·2

Ordinarily, having assumed that the measurements for the independent ' factors ' are distributed among the different individuals in accordance with the normal curve, we make the further assumption that these ' factor-measurements ' combine by simple addition. Now I suggest that, where we are dealing, not with a complex mental *ability*, but with a complex mental *output*, it would be quite as reasonable (at least in many instances, though possibly not in all) to *multiply* as to add. It is a simple matter to show how this will lead from a normal curve for the components to a J-shaped curve for the products. Take factor-measurements for two factors only, and imagine that each is distributed into five classes (allotted marks of 0, 1, 2, 3, 4 respectively) and that distribution obeys the binomial law (i.e., the frequencies are proportional to 1, 4, 6, 4, 1). Combine the marks for these two factors by multiplying them instead of summing them ; and then redistribute the final marks into five classes as before. We arrive at the frequencies shown in Table IV (*b*).

TABLE IV.—FREQUENCY DISTRIBUTION OBTAINED BY MULTIPLYING THE COMPONENT FACTOR-MEASUREMENTS.

Measurement.	*Frequencies (in Percentages).*	
	(*a*) *For Each Factor.*	(*b*) *For Two Factors Combined.*
0—1	6·25	49·6
1—2	25·0	36·0
2—3	37·5	10·9
3—4	25·0	3·1
4—5	6·25	0·4
TOTAL	100·0	100·0

What particular function should be chosen in any given case is a point to be determined by the concrete and empirical nature of the processes concerned, not by some abstract *à priori* principle, laid down once and for all. Thus, bodily height, width, and depth are each of them (in the case of most animals) normally distributed, or nearly so : but, since these ' factors ' must be highly correlated (otherwise the individuals could not preserve approximately the same shape) it follows that volume, and therefore weight which depends upon volume, and pressure which depends on weight, will be estimated better by multiplying rather than by adding. This, indeed, is likely to be the case with any varying characteristic which (like measurements involving time, to take one obvious instance) has an absolute zero of its own.[1] If, for example, one of the ' factors ' is speed, industry, or retentiveness, the deviations must tend to augment those due to mere intelligent insight, by a process more akin to multiplication than to addition. Or consider the effect of blindness on the number of runs scored by one cricketer, or of doubling the speed of leg-movement of those of another : the change in score would not be correctly estimated by just *adding* the changing measurements. In short, when it comes to computing actual output, we seem to be faced with something like the converse of Weber's law : so long as we are measuring sensory *capacity* in the laboratory, we proceed from the physical stimulus to the consequent mental change, and, in so doing, we encounter the well-known phenomenon of *diminishing* returns ; but when we are measuring *output* in industry, in commerce, or in any intellectual field, we virtually proceed from mental capacity to a consequent physical change ; and there we meet with the opposite phenomenon of *increasing* returns.

The practical corollary seems plain. The tacit habit of treating the symmetrical curve of mental ability as entailing a corresponding symmetry in the curve of mental output has hitherto led us to underrate, and to underrate very grossly, the extraordinarily high output of which the super-normal child should eventually be capable. It follows that the ultimate return to the community that would be gained by investing public funds in the tasks of discovering and educating those super-normal individuals is far above what we have hitherto been inclined to expect. Every psychologist, therefore, should readily endorse the pronouncements of the few economists who have expressed an opinion on this point : " No extravagance," says Marshall, " is more prejudicial to the growth of national wealth than the wasteful negligence which allows genius that happens to be born of lowly parentage to expend itself in lowly work ; and there is no change that would conduce so much to a rapid increase in that wealth as an improvement in our schools and scholarships such as would enable the clever son of a poor man to rise gradually till he has the best education the age can give."[2]

IV.—SUMMARY.

Since teachers and administrators will be interested solely in the practical inferences, while psychologists will ask rather for the evidence on which those inferences are based, it will perhaps be convenient to summarise the technical arguments first, and then set down the practical outcome in as simple and non-technical language as possible.

The problem with which we have been concerned is the relations between intelligence, on the one hand, and economic conditions, on the other. All who have discussed this issue, no matter which side they take, assume that ' intelligence ' is one of the most important factors both in educational progress and in social and industrial efficiency ; but no final agreement can be reached, unless both parties to the controversy accept the same definition of ' intelligence.' By ' intelligence ' is here understood an innate factor entering in various degrees into every mental process that involves cognition—not (as some writers would suggest) any complex set of performances as measured by a recognised scale of intelligence tests.

A.—*Technical conclusions.*

(1) When this distinction is made, it appears that differences in ' intelligence,' defined as an *innate* factor, can only be assessed *approximately* by the raw measurement

[1] This would seem to be Pareto's own explanation. In his later work he writes : " au-dessus de la moyenne il n'y a pas de limite de hauteur ; il y a une limite au-dessous " ; and he claims that this is so both for income and for ability, as measured, for example, at ordinary scholastic examinations (*Manuel*, 1927, p. 385).

[2] *Principles of Economics*, p. 213. Cf. Pigou, *loc. cit.*, p. 707 : " Stupidly organised investments in children's capacities, like other stupidly organised investments, will yield little return : well-organised investments, especially investments adjusted to the natural abilities of the children affected, hold out large promise."

of ' intelligence,' automatically obtained by applying one of the recognised scales. Hence for the study of theoretical questions like the present, as well as for the practical diagnosis of individual cases, it is necessary to adjust the calculated I.Q. (or whatever mark or score is used) in the light of other relevant information, including supplementary tests of a practical type. Obviously, for research purposes, such adjustments must not be too arbitrary or subjective ; nor must they beg the question at issue in the research.

(2) Measured by these adjusted I.Q.'s intelligence appears to be distributed—approximately, though not exactly—in conformity with the symmetrical ' curve of error.' On the other hand, the distribution of personal income does not present, even approximately, any such symmetrical curve, but rather a highly skewed J-shaped curve, which can be fitted by a law of the inverse square (or some low power of that order) such as could be deduced from what economists know as ' Pareto's equation.'

(3) The discrepancy can best be reconciled, not by substituting a new law of ability for the normal law, but by regarding earned income as depending mainly on output, and output as related to the contributory abilities by some special and possibly complex function. This suggestion is confirmed by observing that, in many intellectual fields at any rate, the distribution of the output itself approaches the J-shaped curve (shown by income) rather than the symmetrical curve (shown by measurements of intelligence).

(4) The particular function relating the output of different individuals to their respective abilities requires to be determined empirically for each important type of work whether scholastic or industrial. There are, however, indications that such functions will be similar to those already encountered in dealing with the work or output of physical machines.

B.—*Practical conclusions.*

(1) The foregoing results support the view that the wide inequality in personal income is largely, though not entirely, an indirect effect of the wide inequality in innate intelligence.

(2) They do not support the view (still held by many educational and social reformers) that the apparent inequality in intelligence of children and adults is in the main an indirect consequence of inequality in economic conditions.

(3) Nevertheless, mental output and achievement, as distinguished from sheer innate capacity, are undoubtedly influenced by differences in social and economic conditions. In particular, the financial disadvantages under which the poorer families labour annually prevent three or four thousand children of superior intelligence from securing the higher education that their intelligence deserves.

(4) The most striking instances of this are to be found at the final stage of education. With the available data a simple calculation shows that about 40 per cent of those whose innate abilities are of university standard are failing to reach the university ; and presumably an equal number from the fee-paying classes receive a university education to which their innate abilities alone would scarcely entitle them.