THE INTELLIGENCE QUOTIENT FROM TWO VIEWPOINTS

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Recent literature has revealed considerable difference of opinion concerning the constancy, value and interpretation of the I Q. Much of the discussion arises from two distinct practical situations which educators and psychologists have to meet. It is the purpose of this paper to analyze these attitudes arising therefrom and apply a little psychology to the psychologist. The writer feels that much of this difference of opinion is largely due to bias arising from the direction of attention to one's limited interests, and is more apparent than real.

The view of the psycho-statistician and the educational administrator. The psychologist hunting for statistical trends and the administrator seeking approximations find themselves in accord in the interpretation of the I Q. They find that for the very large majority of the subjects tested the I. Q. remains constant or nearly so—sufficient for all practical purposes. For example, if a school administrator wishes to reorganize his school, let us say the Junior High, into three sections for each grade, and has the list of pupils below 80 I Q., from 80 I Q. to 100 I Q., and 100 to the upper limit, he can *safely* count on these groups remaining constant for rough administrative purposes. There will be a few shiftings to be sure.

But the very weight of statistical trends and the serviceability of tests in a broad administrative way lead the practical school man who has gone in for tests to view the I Q. as very "reliable," and he feels considerable impatience with any dwelling upon and analysis of the exceptional cases, or anything that detracts from the simplicity of the intellectual and administrative schema. Immediate practical usability in a large way dominates his thinking and action, while his daily duties accentuate this view. Again, teachers in certain sections are divided into testers and antitesters, and one may safely assume that such "all-or-none" conditions of acceptance rest on emotional belief akin to religious fervor which is likely to supplant detached scientific attitude.

Turning now to the psycho-statistician's view and the organization of the Binet tests. The Stanford Revision *is so constructed* that in an unbiased sampling of children the median chronological age equals the median mental age year by year. This unbiased sampling assumes of course that we have "medianed" all factors; physical, health, hereditary, environmental, and so on. Roughly the data make a symmetrical curve, year by year, with an increasing spread of the scatter-distribution of the mental performance as we ascend the chronological age scale. It is interesting and lends

force to the constancy of I. Q. argument, that the increasing scatter fits nicely just what one would have if each variate were plotted as having a constant ratio between mental and chronological age. The data year by year or as a whole may be treated as conforming to the normal curve¹ (although this may not prove to be true,)—but at least it suffices for all immediate practical purposes.

Let us now assume an unbiased sampling of the child population. Will this group from year to year give a distribution similar to that obtained in the Stanford Revision (applying the usual statistical tests of central tendency and variability—median, P. E.'s, etc'?) Will the mental-age rank order and the I. Q. remain relatively constant for each of the variates?

Statistical prediction depends upon the causal factors remaining constant for the mass of the variates in question. Turning now to our group under consideration the factors of hereditary equipment which are basic will in the main remain the same, and while there will be some ups and downs, in the vast majority of cases the environmental setting will continue roughly constant. The mores, culture, ideals, aims and habits of thought will continue relatively unchanged for the different family groups. The dominant cultural change will be provided by the public schools, but considering masses, this element is injected at the same time for the same duration as in the statistical sampling (the Stanford Revision). The burden of proof is with him who would predict that our three year group will depart seriously from the established norms as they advance year by year. Farthermore, the constancy of the factors producing intelligent behavior will maintain for the mass of the subjects considered individually, which in turn would make for progressive constancy of mental age rank-order and constancy of I. Q. It is highly improbable that any pronounced hereditary or environmental change will affect the masses in any brief period of time but it is equally true that conspicuous changes in environment, sensory deprivation or restoration, systemic conditions, emotional complexes, disease, and so on, can and do take place *in the individual case*.

The view of the clinical psychologist So much for the ninety-and-nine that determine the character of distributions, stabilize medians, permit the psycho-statistician to indulge in scientific prophecy, and allow the educational administrator to convince his board that he has unusual clairvoyant powers. The writer confesses that his sympathies are at times with those who must meet the one, two or three that lie outside the fold of statistical con-

¹"Roughly symmetrical" is usually all that can be said of the data

formity Psychological clinics are likely to get this residue of atypical cases, consequently the clinical psychologist is less likely to be impressed by statistical trends than the educational administrator, and gets little solace from the average constancy of the I. Q. when he deals with a peculiarly exasperating subject.

This can best be illustrated with cases. Case M has had successively I. Q.'s of 93, 79, 98, and 89. The details of the case are too long to give here, suffice it to say that the same examiner administered the Stanford Revision at each examination. The subject possesses no marked peculiarities except the inability to learn to read. This inability is in the nature of almost complete failure to acquire an appreciation of phonic values. Practically all words are sight words to this subject. After the drop to an I. Q. of 79, the child was given strenuous individual phonic drill by a skilled primary reading teacher with years of experience. At the next examination the I. Q. jumped to 98. The drill was stopped for the summer vacation. On returning in the Fall the I. Q. had dropped to 89. The only explanation presenting itself at present is that the phonic drill (from which he profited at the time) improved his reading ability and hence his test performance. He has a splendid physique, teaches credit him with common-sense, good judgement, willingness and so on. The I. Q. 79 and 98 do not warrant the same prognosis, and there was nothing in the first I. Q. to lead one to anticipate the second. The environment is median and constant in character. Time and follow-up work, not I. Q., will throw light on this case. Examination of the eyes reveals no defect of sufficient importance to warrant the character of reading. If he had only accommodatively maintained an I. Q. of about 60 all would have been well!

Within the last two years the Psychological Clinic of the University of Pittsburgh has handled a number of cases going into a very superior institutional home on the cottage plan, and a few cases of children going from very abject conditions into superior private homes. In the majority of these cases the institution marks a distinct improvement in intellectual, hygienic, moral and social environment. The accompanying tabulation presents a few such cases from which we have had our first report since the environmental change. It will be noted (comparing columns 4 and 10) that in all the cases except number 2 there is a distinct increase in the rate of gain in mental age following the change in living conditions. The remaining cases hug the lower level of median performance, had poor environmental conditions and are accruing mental months at a much faster rate than before environmental change. There is nothing in the cases just cited which does vio-

lence to any of the known facts concerning the origin or composition of the data from which the I. Q. is derived. Such data depend, in the by and large, upon all of the factors lying back of complex human behavior remaining relatively constant; in the above cases we have introduced marked change, tearing the child away from family habits, mores, and influences *that under ordinary laissez faire conditions might have required three or four generations to effect*. One may thoroughly believe in the constancy of the I. Q. and with perfect consistency refuse to accept statistical trends if there is evidence of marked departure between the first and second test in any one of the factors making up the composite nature of behavior. Any other procedure would be to have the statistical tail wag the causal dog.

Cases transferred from inferior homes to superior institutional life

1	2	3	4	5	6	7	8	9	10
Case	Chronological Age at time of 1st examination	Mental Age 1st examination.	I. Q. 1st examination	Chronological Age at time of *2nd examination	Mental Age 2nd examination	I. Q. 2nd examination	Time—in months between 1st and 2nd examination	Number of months gained in Mental age between 1st and 2nd examination	Column 9, 8 giving I. Q. in terms of rate of mental increase chance of environment
1	4-0	4-8	116	5-7	6-10	123	19	26	186
2	9-9	11-4	116	11-4	12-1	107	19	9	47
3	6-5	5-6	84	7-7	7-0	90	16	18	112
4	7-7	7-0	92	8-10	9-4	106	14	28	200
5	6-2	5-4	86	7-3	7-0	97	14	20	148
6	14-0	12-5	89	15-0	13-11	93	12	18	150

*2nd examination after change of environment in every case

One case more, which the writer had up for consideration recently. The subject, a little girl, was eleven years five months old at the time of testing, I. Q. 67. (Has had simply unbelievable social conditions. Mother and father died of "flu," went with brother and sister, to live with grandfather and grandmother. Grandmother died. Children then alone with grandfather who is eccentric, morose, grossly ignorant and surly. No companions, and school several miles away. . . . No shoes, no underwear during winter months. Only a few months schooling. . . . No toys, no outside contacts. Squalor and poor food.) This child was as timid

as a deer. Although she was almost without vocabulary, and could not read, her ability in the Porteus maze tests, Healy-Fernald construction boards, and her complete lack of the "so-called" stigmata of feeble-mindedness pointed towards a diagnosis and prognosis more favorable than the I Q would suggest. Now the institution having the child examined did not want her if feeble-minded. The matter was finally presented to the institution frankly stating the dilemma, namely the highly atypical character of the environment. The institution accepted her. At the present moment of writing, she has within a few months acquired sufficient skill to read the passage from the Stanford Revision beginning, "New York Sept. 5," without error, but still too slowly to score plus.

What prognosis should be made? To begin with the case is not quite analogous to the ones cited in the accompanying tabulation, for in these the I Q in no case takes the child out of the normal group. But may it not be true that the monstrous conditions under which the child has lived has markedly deflated her I. Q.? Is it not possible that if the institution in which she is placed offers her even median social milieu that the I Q may rise appreciably? The writer would venture the prediction that the subject may attain an adult intelligence of low normal (I Q of 85 or 90).

Certain practical applications and suggestions can be made from these two contending viewpoints, statistical trends on the one hand and analysis of the qualitative factors peculiar to an individual case on the other:—

1. Probably no one not even the most enthusiastic eugenist would deny that in trends and for school administrative purposes in the large, the results of an examination of a school either by the Stanford Revision or a battery of mass tests must be accepted as highly indicative of relative rank-order of the intellectual status of the group studied, and *vastly superior to any impressionistic estimate*.

2. More than this, in the individual case of fluctuation of I. Q. should arouse the attention as to the cause. The first assumption should be that a clerical error in scoring has been made, or age has been reported differently at different times. This "hunch" is often correct. (We are assuming trained examiners throughout.) Up to this point the burden of proof is with the case that departs markedly from statistical trends. A search for causal factors and an analysis of the nature of the response must now be made. But having verified the accuracy of the data no apology needs be offered to statistical methods; neither is it

in order for statistical method to offer an apology for failure in individual conformity.

Statistical method was developed to anticipate trends in data and error therein. At this point in our thinking a quotation from a recent article by Yule¹ is well worth pondering —

“Statistical methods, I say, should be regarded as ancillary, not essential. They are only essential where the subject of investigation is itself an aggregate, as a swarm of atoms, or a crowd. But here (mental measurement) the subject is the individual, not the aggregate of individuals as such. This being the case, statistical methods are only necessary in so far as experiment fails to attain its ideal, the ideal of only permitting one causal circumstance to vary at a time. And it should always be the aim of the experimenter not to revel in statistical methods (when he does not revel and swear) but steadily to diminish, by continual improvement of his experimental methods, the necessity for their use and the influence they have on his conclusions. Statistical methods are not only ancillary; they are, to the experimenter, a warning of failure.

“During the last year or two I have made it a practice to begin a short course of lectures to agricultural students, on some elements of statistical methods, by a lecture devoted entirely to showing how exceedingly bad agricultural experiments are, how this introduces the necessity for statistical methods in discussing the results, but does not obviate the primary necessity for improving the experimental methods. Is the latter warning unnecessary in the case of experimental psychology? I can only speak as a statistician with an exceedingly limited and sporadic knowledge of what is being done, but it seems to me that it is urgently necessary. The experiment (if it can be called an experiment at all) is often not only bad, but its badness seems to be accepted merely as a rather interesting fact, calling perhaps for the exercise of more elaborate statistical methods. . . a few partial coefficients of correlation . . . a few rather elaborate probable errors . . . and that is all. It does not seem to be recognized as primarily a condemnation of the experimenter, perhaps because the experimenter has never been exercised in the sciences where really accurate experiment is possible, so that he has no high ideals before him.”

3 Intellectual or emotional repugnance to statistical fatalism has been assigned as one of the reason for objecting to the constancy of the I. Q. I find no reason to believe this to be true of any trained psychologist. The layman and certain schoolmen may

¹Yule, G. Udny Under, “Critical Notice,” *Br. J. Psych.*, vol. XII, Part 1, June, 1921, page 105 ff

be motivated vaguely by a certain repugnance to anything that undermines the beautiful optimism of,

"I am the captain of my soul,

I am the master of my fate"

but the majority probably do not attempt to think the matter through, or try to orientate intelligence testing in respect to any philosophical basis such as free will or determinism. The practical school man is likely to be an opportunist, and fatalistic finality gives an air of professional expertness, and saves vexatious analysis, trial and error, and suspense.

However, while certainly no psychologist would oppose the constancy of the I. Q. for other than scientific reasons, he is most emphatically opposed to any premature statistical finality regarding the whole matter.

There is nothing gained by dealing with statistical results as causally final or chronologically static. Statistical stability or variability rests upon causes of such a nature that a rational inquiry into the possibility of modification is irksome, undramatic and may become very vexatious by leading to still greater causal complexities. It is also not a simple matter of "either-or" as regards statistical predictability and stability. I read in my morning paper that Los Angeles had over six thousand automobile accidents in 1921. It is safe to predict six thousand for 1922, but it is philosophically bad to resign oneself to the situation. A careful analysis of causes could conceivably reduce the death rate. Likewise statistics could easily have told us that the death rate in building the Panama Canal would have been thus and so, but Goethals did not accept the statistical inference but applied preventive medicine and hygiene.

Now turning to intelligence, it is manifestly a behavior composite. In our carefully non-selected group in the Stanford Revision there is an unbiased sampling of many causal factors of this complex response we call intelligence. Let us name some of the probable ones: heredity, sensory efficiency or deprivation, social milieu, educational opportunities, glandular secretions, general systemic conditions, disease, nutrition, nature of conditioned reflexes, Freudian complexes, emotional make-up, and so on. Immediately it is not likely that any one or a group of these factors will be sufficiently changed, considering the masses, to invalidate the norms of the Stanford Revision. In this sense the age norms are probably temporarily static. It would be bad statistical philosophy, however, acting like an insidious paralysis to all constructive educational advance, if we substituted statistical results for painstaking analysis in a specific case, or for causal analysis for society at large. It is true that there is a wide gulf between

logically possible modifications of causal factors and the surrogation of these factors into the social habituations, which condition gives much greater stability to social action and the complex statistical results than the reformer is likely to appreciate. For example, the means is now probably at hand to wipe out tuberculosis if we had the hygienic habits and hygienic morality to back up the sanitary expert

Aside from changes in social habituations, unforeseen factors may make marked changes through regulation of glandular secretions. The cure of cretins may be followed by the cure of Mongolian idiots. Again, greater scientific precision in the education of the baby's conditioned reflexes could conceivably change the intelligence of any particular child, and if instituted on a large scale appreciably raise the median performance or change the intelligence rank-order of certain classes of cases

In pedagogical and administrative problems it is inexcusable to conceive the intelligence norm (namely, the cross-section of the composite set of causal factors) as static. The administrator is justified in making subdivisions in classes on the basis of testing but broad constructive pedagogical and administrative effort must not cease. The teacher should take philosophically the clearly demonstrated cases of mental deficiency and not attempt to make a silk purse out of a sow's ear, to be sure. On the other hand, she should avoid using the I. Q. as a blanket excuse, allowing it to act like a paralysis to all pedagogical resourcefulness and effort. *The low I. Q. of her class can easily become the perpetual alibi of the poor teacher and a new form of infant damnation for the pupils.*

What the average *does* may be pretty safe betting as to what the average will do under a *laissez faire* policy, but it does not follow at all that it is what the average could or should do. Suppose that by scratching Aladdin's lamp we could reduce the enrollment of every teacher to fifteen pupils, or say correct all glandular difficulties or wipe out all superstition, or secure specifics for several of the serious children's diseases, would it not only change the median, P. E.'s, and lower limit but might not whole groups change in their relative position in the rank-order of intelligence? Preventive medicine, refinement of social and sanitary habituations, improved methods of handling the emotions, more individualized teaching, and constructive eugenics constitute our only magic lamp.

Again, sufficient time has not elapsed since the beginning of testing to warrant much prognosis if we are to judge and profit by the vicissitudes of prognosis and cures in medicine.

4. There is considerable evidence from the pathology of the nervous system that the lower end of the intelligence curve marks the

intrusion of a distinct set of factors not found throughout the curve. From normal to superior is probably bridged without any change in kind, from normal to feeble-minded we have a blending of dull normal with varying degrees of pathological derangement; i. e. the entire curve is distinctly bimodal.

This means that preventive medicine or more healthy conditions of conception may remove our worst cases forever. This would not help the pathological cases of to-day to be sure, but such a possibility should make us cautious of statistical estimates of the number of feeble-minded we must have. Equally true, it would be fatuous to feel secure for the future on the basis of the present percentage of low grade intelligence. Relatively changing birth-rates for different classes (all other factors constant) could conceivably shunt the entire intelligence distribution curve. The point is that cogent reasoning and analysis of present and possible factors and not more abuse of the must over-worked normal distribution curve, will solve our difficulties.

5 As pointed out by Stenquist, quite recently, we do not yet know just what a low I. Q. means in terms of eventual adjustment in the world. And as proof of the tester's diagnosis and prognosis lies far ahead in the future (many times the present age of the Binet Tests, Stanford Revision) it becomes peculiarly easy to make sweeping assertions without any immediate factual check.

6 Finally, there is nothing in the nature of the tests to warrant speaking of them as measuring either heredity or environment separately. Any one is, of course, quite within scientific proprieties, when he offers his opinion as to the dominating factor in any mental test performance. We must regret, however, that the commercializing of the tests has resulted in such unqualified statements as this (appearing in the prospectus of a mass test). " is an instrument for the measurement in groups of the native ability of any individual whether child or adult, who has had the equivalent of three or four years of schooling."