such a "banal" (*sic* Weber) toxic factor as, for example, ammonia.

Hence it is important to distinguish clearly between specific and nonspecific changes that occur during physiologic or pathologic processes (*5*). For example, it can be shown that the "toxin" present in the urine of menstruating women (*8*) is due mainly to increased amounts of ammonia (*9*), a concomitant of menstrual acidosis. Furthermore, acidosis, a banal change, can produce a decrease in cerebral blood flow (*10*).

Summarized: tadpoles, and especially those of *Xenopus levis,* are reliable test animals, for example, for the detection of a toxic factor in the body fluids of schizophrenics. It is self-evident that the foregoing holds only if one is able to control the conditions under which the experiments are reproducible (*1*).

Finally, I wish to thank Georgi *et al.* for having initiated by their remarks a further clarification of some of our divergent views.

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6 August 1954.

More on "Different IQ's for the Same Individual"

In their independent and valid criticisms of Dreger's study (*1*), Stanley (*2*) and Kurtz (*3*) failed to mention four other highly pertinent considerations.

Perhaps some readers will not recall Dreger's paper. Ten children were each tested on four different intelligence tests, with alternate forms somtimes used. Because numerical IQ's on the same individual "do differ from one another," since there was only one statistically significant correlation between the tests, and because of a significant difference in IQ's as determined by an indefensible method (see criticism by Stanley and by Kurtz), Dreger concluded that "individual's IQ's may differ widely and significantly from one another on different tests." My four additional points of criticism are as follows:

1) It has long been known that numerical IQ's for the same person on different tests may differ because of over-all differences in means and standard deviations and because the tests tap somewhat different functions.

2) The six correlations reported by Dreger are actually based on N's of 9, 5, 7, 6, 7, and 4. Such small N's do indeed aid one in accepting the null hypothesis.

3) Pearson (*4*) as long ago as 1903 demonstrated that correlations are reduced by homogeneity or restriction in range, yet 50 years later we find Dreger drawing conclusions from correlations based on scores that are restricted mainly to average and up (only 5 percent are below average, and not much below at that).

4) Why did Dreger ignore the literature? A quarter of a century ago Goodenough (*5*) reported an r of .74 (N = 334) between the 1916 Stanford-Binet and her Draw-a-Man-Test. Wallin (*6*) reported an r of .72 (N = 290 clinic cases) between the 1916 Stanford-Binet and Arthur I, and an r of .53 (N = 172 clinic cases) between the 1937 Stanford-Binet and Arthur I (contrast this with the r of −.41 given by Dreger on five cases!) Cohen and Collier (*7*) found an η of .71 (N = 51) for the 1937 Stanford-Binet versus Arthur II, and for the same tests Hamilton (*8*) found an r of .73 (N = 40), and Manolakes and Sheldon (*9*) found an r of .64 (N = 217 atypical cases: good and poor readers).

We are forced to agree with Kurtz' conclusions that "Dreger's little study has, thus, contributed nothing. . . ." We also agree with Dreger when he concludes that IQ's from different tests are not comparable, but the lack of comparability is not nearly so sad as he would have us believe. We venture the opinion that Dreger, in his reply (*10*) to Stanley and to Kurtz, missed the point of their criticism. We hope that his hope to repeat the experiment is not realized if by repeat he means an exact replication.

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7 June 1954.

McNemar's criticism (like Kurtz') of one portion of my statistical treatment is correct. I called it "the most indefensible statistically," although I tried to give justification both logically and statistically. In respect to McNemar's specific points:

1) Yes. I cited only two references of the vast literature on test comparisons. Certainly, these other studies emphasizing tests serve as background for work like mine on intra-individual comparisons.

2) If Binet Forms M and L are considered comparable, N's are then 10, 6, 8, 6, 8, and 4. I used Kendall's (*1*) rank correlation coefficient τ, not Pearson's r. The experiment obviously was not intended as a mass demonstration among tests but, instead, to see how *indi-*

viduals fared. In making significance tests, I adhered to the probabilities given in Kendall's tables. Small N's do, however, make for easier acceptance of the null hypothesis.

3) The host of correlational studies on mental defectives, superior children and adults, and other groups more or less homogeneous must bear the same criticism. IQ's in my study running from somewhat below average to very superior seem to me to cover a rather wide range. Nevertheless, McNemar is right; restrictions of range may limit correlation.

4) It was a search of the literature that prompted me to make my study and submit it for publication. Among 235 items in one of several bibliographies I have available on the subject, only seven are truly relevant items (*2–8*). In the more extended report from which my *Science* article was abstracted, I stated that the significant correlation between the Binet and Goodenough was to be expected (*9–11*). Wallin (*12*) and Hamilton (*13*) had group comparisons similar to many in the literature. Cohen and Collier (*14*) I overlooked for some reason; I am sorry, for their work is closest to mine in several ways. Manolakes and Sheldon (*5*) likewise used two of the tests I employed, but their restriction of possible IQ's (which they carefully pointed out) tended in the opposite direction from mine, that is, toward maximizing correlation. In my search of the literature, however, I found many studies but few that evidenced interest in the individual *qua* individual.

The differences among an individual's IQ's may be wide; if the correlational results of my study are accepted, such differences may be significant. I did not intend to make a contribution to the problem of the "constancy of the IQ," although I sincerely thought that I was making a contribution in terms of different IQ's for the same person. The lack of comparability of IQ's is sad for any one person. With nearly all of my counselees, however, I employ at least two intelligence tests, primarily as a measure of some of their capacities against the background of the standardizing group in each case.

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16 August 1954.

Serial Cultivation of Normal Human Embryonic Cells Directly on Glass*

Normal human cells from the tissues and organs of a 3½-mo old fetus, obtained at sterilization by total hysterectomy of a psychopathic patient, have been successfully adapted to a glass surface without clot. Three of the cell strains have yielded a tenfold increase in cell count every 5 to 6 days and have undergone 10 transfers.

Clot cultures of skin, muscle, spinal cord, thymus, lung, kidney, spleen, and placenta were prepared in Carrel flasks. The fresh tissue was cut into 1-mm cubes and imbedded in 0.5 ml of chick plasma and 1.0 ml of the culture medium, composed of 40 percent human cord serum, 8 percent chick embryonic extract (diluted 1:1 with Tyrode's solution), 5 percent saline containing 50 units of penicillin and 50 μg of streptomycin per milliliter, and 47 percent Tyrode's solution. An additional milliliter of the culture medium was introduced after clotting had occurred. Proliferation of all cell strains permitted subculturing at 3-wk intervals, and the outgrowth was reimbedded in plasma clots.

Adaptation to a glass surface was accomplished at 9 wk. Two cultures of each of the cell strains were removed from the Carrel flasks, and the dense outgrowth of new cells was dissected from the central tissue mass and from the surrounding clot. These cell fringes (three from each flask) were placed in a 15-ml centrifuge tube with 5 ml of a 0.25 percent trypsin-Tyrode's solution. The tissue was broken up by pipetting and incubating at 37°C for 5 hr. This treatment allowed complete digestion of the clot and liberation of the cells as a suspension. The trypsin-Tyrode's solution was removed after low-speed centrifugation for 5 min. The cells were washed twice with Tyrode's solution, centrifuged again, suspended in 2 ml of the culture medium, and transferred to duplicate Carrel flasks for each cell strain. No cell count was made at this first transfer. The remaining two cultures of each strain were subcultured and imbedded in plasma clots as controls.

The cells adhered to the glass within 24 hr. The original inoculum was heavy, which necessitated the first transfer at 3 days. The trypsin-Tyrode's solution was used to remove the cells from the glass surface of the flask. Digestion of necrotic material and liberation of the cells as a suspension were accomplished after ½ hr of incubation, and the cells were again washed and transferred as described in the preceding paragraph. At this stage, and subsequently, growth was estimated by counting the cells in a hemocytometer chamber. Growth was very rapid in three of the cell strains, and transfers were made every 4 to 5 days with an approximate tenfold increase in cell count each period. The plasma-clot cultures of the same strains doubled in radial outgrowth in 2 to 3 wk. The five remaining cell strains showed a great variation in the rate of growth on the glass surface. Transfers have

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