

STUDIES IN INDIVIDUAL DIFFERENCES IN MAZE ABILITY

III. THE COMMUNITY OF FUNCTION BETWEEN TWO MAZE ABILITIES¹

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I. INTRODUCTION

Evidence presented in an earlier study (18) clearly shows that individual differences in a learning function in a subhuman species (the rat) may be measured to a high degree of accuracy. The following question next naturally arises: When members of a group of animals are measured in a number of *different* learning functions, is there any evidence of a *general ability* to learn? Does the animal who is brightest in one function tend to be brightest in the others, and similarly down the scale of ability? Or is there, on the other hand, a variable degree of correlation between different tasks, indicating that the ability to form diverse habits hinges upon more or less specific factors? The writer will risk being charged as audacious when he states his belief that previous evidence collected on this point has been inconclusive. He must confess that the evidence which he will himself present in this paper is of the most preliminary character as bearing on the problem of *general ability*, for it will show the degree of correspondence of individual differences among rats in the learning of only two T-mazes. For the two abilities measured, however, it will be clear that a high community of function exists between them.

The evidence presented here leads to conclusions in distinct opposition to those formed by previous investigators, the majority

¹ Regarding the previous two studies in this series, see references (18) and (19). In the first study are given the diagrams of the mazes and full experimental details.

of whom have failed to find correlation between animal abilities, and have therefore decided *against* the operation of common factors. But the previous experiments, I submit, did not meet certain quantitative requirements exacted of similar types of investigations in human mental measurement, whereas in the present experiment a deliberate attempt was made to satisfy them.

This paper will attempt to prove the sundry points made above, that is to say, it shall have three objects:

1. The formulation of the quantitative requirements of any research which purports to ascertain the community of function between two or more abilities.

2. A critique of the previous research on the correlation between abilities in animals, made in the light of these requirements, and showing the inconclusiveness of these experiments.

3. The presentation of the results of an experiment which shows the relatively high correlation between the ability to learn two different mazes.

II. QUANTITATIVE REQUIREMENTS OF AN EXPERIMENT ON THE COMMUNITY OF FUNCTION BETWEEN TWO MENTAL ABILITIES

1. *The abilities measured should be clearly defined in terms of the experimental conditions under which they appear.*

Such a definition involves a statement as to the type of measuring instrument employed, the type of score which is used to represent the behavior displayed, and a clear description of the experimental conditions (incentive, technique, etc.). The purpose of this requirement is to facilitate a "psychological analysis" or explanation of the degree of correlation shown between the abilities, and further, to circumvent the affixing of high-sounding psychological faculties to the ability measured.

2. *The "spread of talent" of the individuals measured should be described.*

By "spread of talent" is meant the degree to which the sample is selected on the basis of hereditary or other known factors. The sampling problem is notoriously complex, but is certainly not solved by ignoring it. If the animals measured come from highly inbred stocks, or have been homogeneously selected either con-

sciously or unconsciously on the basis of some performance correlated with either of the abilities measured, the correlation between the abilities will be systematically lowered.

3. *The heterogeneity of irrelevant factors likely to be the "source of correlation" between the abilities should be ascertained by measurement.*

If such an irrelevant factor as sex, age, etc., correlates with the abilities measured, it should be removed as a systematic source of correlation between the abilities either by being experimentally held constant, or by partial correlation.

4. *In order to make use of conventional probable error formulae, the number of individuals entering into any statistic should exceed thirty.*

The writer blushes to mention such an elementary principle, yet the fact must be recorded that most of the researches he is to review have been made upon samples less than thirty. On this account alone they must be condemned as inconclusive. In this connection Holzinger says (3): "The chief danger in using such (sampling) formulas . . . may be avoided by never applying them to a small number of cases (say less than thirty)" (p. 232).

5. *The reliability coefficient of the measure of each ability must be ascertained by the proper methods.*

Since this matter was considered rather extensively in the first paper of this series (*q. v.*), the only thing that need be added here is the point that the magnitude of the reliability coefficient of any measure of an ability *fixes the limits of correlation* of that ability with other measures. It is well known that a measure cannot correlate with another measure in excess of the square root of its reliability coefficient (7, formula 160). Since the experimenter may *arbitrarily* raise or lower the reliability coefficient, as I have previously indicated (18), the experimenter thus arbitrarily fixes the upper limit of the magnitude of the correlation of the given ability with other abilities.

6. *In order to ascertain the true community of function between the two abilities measured, the reliability coefficients should be used to correct for attenuation the raw correlation between the two functions.*

By "true community of function" is meant the correlation between the two abilities when the arbitrary variation due to unsystematic "chance" factors has been removed. It may very well happen that two abilities may be entirely determined by the same systematic factor, such as, for example, a *general ability to learn*, yet the correlation between the two fallible measures of these abilities be far below unity because of adulterating "errors of measurement" which, to repeat, are *arbitrarily* determined by the conditions of the experiment. Though the attenuating effect of such unsystematic factors upon correlation has been known for more than twenty-five years (20), animal psychologists have not taken sufficient cognizance of it, it seems, for of the previous researches on the community function between formation of different habits in animals only *one* investigation has taken cognizance of it.

7. *The true correlation coefficient between the two abilities should be translated, if possible, into values indicating the percentage by which variation in each function is occasioned by variation in the factor which they possess in common.*

The correlation coefficient is a magnitude which *per se* is difficult to interpret in terms of community of function. The writer has elsewhere (16, 17) indicated the methods and assumptions involved in translating the correlation coefficient into a number indicating the degree of determination of each ability by the common factor, that is, the factor which is *general* as far as the two abilities are concerned. The greater the knowledge the experimenter possesses regarding the two abilities under analysis, the more likely will the percentage determinations represent true values, but in the absence of any knowledge regarding the abilities, the *limits* of the percentage determinations may be ascertained.

III. CRITIQUE OF PREVIOUS ANIMAL EXPERIMENTS ON THE COMMUNITY OF FUNCTION BETWEEN DIFFERENT LEARNING ABILITIES

In the light of the above requirements which experimenters in human mental measurement have in the main taken cognizance

of, how do previous researches on animals measure up? The writer wishes here emphatically to state that these researches should *not* be held in a derogatory light. They represent pioneering efforts in virgin territory and have in effect cleared away the underbrush so that the true nature of the terrain may be disclosed. Professor W. S. Hunter was the first to attack the problem in 1922, and though the writer is rather at variance with some of his conclusions, I consider it a tribute to him and to his acumen for early raising the cry that animal behavior measurements were at that time utterly unreliable. Professor E. C. Tolman made several ingenious attempts to improve the reliability of measurements and it is largely due to his efforts in this direction and to those of his students that the principles were disclosed which lay at the base of the unreliability of animal measurements.²

Experiment 1. One of the first attempts to determine the correlation between two ostensibly different functions in sub-human animals was that of Professor Hunter in 1922 (4) on rats. He investigated the relationship between ability to form one habit, and then to "break" it by forming its behavioral opposite. He found that the correlation between making and breaking a visual habit was .226 (trials), a simple kinaesthetic habit was $-.04$ for time and $-.075$ for trials, and a circular maze habit was .36 for time and $-.02$ for distance. The number of individuals was respectively 16, 25, and 25. The calculations were made by means of Spearman's "foot-rule" method of correlation regarding which Kelly (7, p. 193) says that it "does not vary between -1.00 and 1.00 ; is not comparable with a product-moment coefficient." The "spread of talent" was not described, the *N*'s were inadequate, no attempt was made to ascertain reliability coefficients, and of course no corrections for attenuation were made. Hunter implies that the absence of correlation in these experiments may be imputed to the unreliability of the rat, for he says that "if the rat in the maze or discrimination box . . . gives reliable data on learning, one would expect that the animals learning the first habit most readily would also learn the second

² Invaluable aid upon these problems was received from Dr. R. H. Franzen.

habit most readily (p. 48).” Here he does not distinguish “reliability of performance” from “general ability”; it does not follow *necessarily* that high reliability in one function results in a high correlation between it and different functions, indeed, earlier in his paper he makes this very point, for he says (p. 43): “We should not, however, be blinded to the existence of quite highly independent habits.” For 31 human subjects who learned a stylus maze, Hunter found a correlation between making and breaking this habit of .64 (time) and .68 (trials). The difference between the human and rat correlations Hunter decided might be due to either of the following factors: “(1) A better and more uniform control of experimental conditions is possible for human subjects. Or, (2) the rat is intrinsically more unstable than human adults. . . .” Besides the factors already mentioned which may have occasioned the differences in correlation, it may be questioned whether two functions similar in the superficial respect of appearing on “mazes” can reasonably be compared in different species. Even in this superficial respect, the human maze had twice as many cul de sacs as the most complex animal maze, a variation which might well have occasioned a difference in reliability and hence *opportunity* for correlation between the two functions. By virtue of its not satisfying the requirements as we originally formulated them for such experiments, we are forced to conclude from this research that it furnished no conclusive evidence as to the community of function between different abilities in rats.

Experiment 2. In the same year, appeared Heron’s paper (2), the purpose of which was “to determine the degree of reliability of the problem box as a test of the learning ability of the white rat.” Here again we encounter the failure to distinguish between *reliability* and *validity*, pointed out long ago by Spearman (20). Truly, the problem box ability might have high reliability, though still not be a valid measure of “the learning ability” of the rat. By correlating the time scores on odd vs. even trials on the problem box, Heron found that the highest coefficient, .63, appeared after deducting the first eight trials. This means that the reliability coefficient for the total score from trials nine on was,

by the Brown-Spearman formula,³ approximately .8, a "fair" reliability, though not computed by Heron. Heron concludes that "the more chance errors are eliminated by deducting early trials, the greater is the consistency of results." Next, Heron ran a group of rats *only six trials* on the problem box, rested them 30 days, then ran them six more trials on the problem box. That is, after it had already been shown and explicitly stated that individual differences on the first six trials were due almost wholly to "chance errors," these six trials were now used as a criterion of learning on the problem box. The correlation between the first six trials and the six 30 days later was considered a measure of reliability by the "retest method," and the fact of it being zero followed *necessarily* from the reliability coefficient of the first six trials being zero. With a third group of rats, Heron now attempted to measure the validity of problem box ability as a measure of general learning capacity by using "an approximation to the method . . . which has been frequently employed to determine the reliability (he means *validity*) of intelligence tests given to human beings . . . (namely, that of correlating the) standing in the intelligence test with standing in school grades or the estimating of the subject's ability by a competent judge." That is, he set up maze ability as a *criterion* of general ability against which to correlate problem box ability. He ran the animals again *only six trials* on the problem box, rested them thirty days, then ran them six trials on Watson's circular maze. Again the correlation was zero *necessarily*, by virtue simply of the unreliability of his problem box scores. Heron now sought "to justify the use of only six trials in the correlations of results in the problem box obtained at two different applications and in the correlation of the maze and the problem box" by correlating the score on these first six trials with the total score. This correlation was .51, which Heron deemed a "close relationship." Not only is this relationship *not* close, for the first six trials contributes only

³ The odd-even correlation, r , represents the reliability coefficient of only half the measures in the total score and must be substituted into the special case of the Brown-Spearman formula, to wit, $2r/(1+r)$, in order to obtain the reliability coefficient of the total score.

26 per cent of the variance to the total score, but even this magnitude constitutes a *spurious correlation!* The proof of these assertions I have relegated to a footnote,⁴ though it deserves more prominence since this same method of justification was later used by Liggett. Besides these factors which have accounted for the lack of correlation between the abilities, we must record that the number of individuals in each group was rather small ($N = 28, 22, 20$), the selection of individuals was not mentioned, and no true correlations were estimated. Here, too, we must conclude that this experiment sheds no light on the community of function between animal abilities.

Experiment 3. Using a single T-unit, a simple circular maze, and another, more complex, Hunter (5) investigated "the relationship between performance in one part of the learning process and in other parts. . . ." He used three groups of indeterminately selected rats ($N = 25, 36, 24$). The intercorrelations between stages (Vincent tenths) of learning averaged (approximately) for the simple T-unit about .30, and for the two circular mazes about .20.

To explain this lack of correlation, Hunter says: "Progress in the formation of a given habit is determined by: First, *the general adaptive capacity* of the individual, his general intelligence, second, *the special ability* in the field from which the

⁴ Let $x =$ first 6 trials, $y =$ remainder of trials, $z =$ total trials. Then, $z = x + y$ (1)

and

$$r_{xz} = \Sigma x(x + y) / N\sigma_x\sigma_z = (\sigma_x^2 + \sigma_x\sigma_y r_{xy}) / (\sigma_x\sigma_z) = (\sigma_x + \sigma_y r_{xy}) / \sigma_z \quad (2)$$

Since the evidence indicates that the reliability coefficient of x is approximately zero, then r_{xy} is *necessarily* approximately zero, and the square of (2) becomes

$$r_{xz}^2 = \sigma_x^2 / \sigma_z^2 = (.51)^2 = .26 \quad (3)$$

But from (1) it must be apparent (since $r_{xy} = 0$) that $\sigma_z^2 = \sigma_x^2 + \sigma_y^2$, that is, the variance of total scores is the sum of the variances of x and y . Hence, (3) is the percentage determination in terms of variances of total scores, z , by the first 6 trials, x , and amounts to approximately 26 per cent.

But $r_{xz} = .50$ is spurious in the sense that the magnitude arises only because of x being *in* z arithmetically. Had x been excluded from the total score as is the customary procedure in test construction when an item such as x is validated against a criterion such as z , then r_{xz} ($x - z$) would have been *zero*, for $(z - x) = y$, and we have already pointed out that $r_{xy} = 0$. Thus $r_{xz} > 0$ only because x has been spuriously included in the total score.

particular habit is chosen, and third, by various temporary factors. . . .” The third class he terms *chance*. The lack of high correlation between parts of learning Hunter ascribes to the operation of these chance factors. Now, Hunter’s explanation is only *one* of several possible ones, and his results neither prove nor disprove it. (Incidentally, it has a Spearman flavor.) Another theory, equally consistent with the facts, is that the *systematic* factors causing individuals to differ at successive stages of learning are not entirely the *same* and that the absence of correlation between successive stages of learning ensues from a gradual change in systematic factors, more or less independent. Indeed, there is considerable evidence to support this view for, if Hunter’s theory is correct, the intercorrelations between successive stages of learning corrected for attenuation should be unity. That this is far from true is shown by evidence of the writer’s (14) which he hopes shortly to publish. Hunter did not, however, correct his intercorrelations for attenuation (it is doubtful if they warrant correction, the *N*’s being so small, and the sample not adequately described), and hence, here again no conclusions may be drawn as to true community of function.

Experiment 4. Professor Tolman distinguished between the *reliability* of the maze he used in his preliminary experiment on the inheritance of maze ability (11) and its *validity* as a measure of maze learning ability. In his inheritance paper, he found the reliability coefficient to be about .6, inadequate for his purposes. In collaboration with Davis (12), he correlated performance on the inheritance maze with that on another maze of the same internally partitioned Carr type, yet the best raw coefficient was only .389. Though in many ways an advance over previous work on this problem, especially in the distinction made between reliability and validity, in the attempts to “refine” the scores to secure the highest reliability, and as the first instance of the use of “Brown’s formula” (a *necessary* procedure), this experiment none the less, yielded *results* that were inconclusive. The computations were made on only 19 animals, and the nature of the selection of these was not described. Further, no attempt was made to determine whether the absence of correlation between the two

abilities was due to attenuating errors or to the presence of systematic independent special factors. I have therefore corrected for attenuation the correlation between unrefined scores (errors), and the true correlation came out to be *unity!* But the correlation corrected for attenuation in the case of refined scores was only .6. This ambiguity probably derives from fewness of cases, a condition which places uncertainty upon these results.

Experiment 5. "What degree of correlation exists between learning in the sawdust problem box, the T-shaped maze (single unit) and the straight-away maze?" Hunter and Randolph (6) posed this question and sought to answer it by running rats for three trials upon each of the above mentioned devices. But no reliability coefficients were computed upon any of these three trial measures, and the evidence indicates that the reliabilities were insignificantly different from zero. Thus it followed *necessarily* that the correlations between the measures were zero. Since the stage was therefore not set for determining the true community of function between these abilities, the results are again unfortunately inconclusive.

At this point should be mentioned the constructive paper by Tolman and Nyswander (13) on "The reliability and validity of maze-measures for rats." Their use of the term "validity" is here, however, very narrow, and does not denote the community of function between the learning of one problem and that of others, but means the community of function between different measures of scoring the *same* task (errors, time, retracings, perfect runs).

Experiment 6. Liggett, one of Hunter's students, proposed "to determine the consistency of performance of the chick in a simple maze" (10) though actually he estimated (*a*) the reliability of performance on a straight-away and on a single T-unit maze, and (*b*) the correlation between them. He did not, however, distinguish between these two problems. By interspersing suitable intervals, he ran his animals on two widely separated occasions (7 runs each time) on the straight-away and also on two occasions on the maze (3 runs each time). In order to give "satisfactory justification for the use of the first three trials in

the T-maze as indicative of rank in total learning," he ran another group of 20 animals for ten trials on the maze, obtaining a correlation of .98 of the first three trials against all trials. This type of correlation has already been pointed out to be spurious. In the other group ($N = 48$) which formed both habits, Liggett attempted to increase the reliability of his measures by use of two of Tolman's techniques reported in the inheritance paper. But here Liggett ran aground. He says: "Following Tolman's method, each *total score* was divided by the sigma of the totals" (p. 476). This was not Tolman's method (11, p. 7) of dividing each *trial score* by the sigma of that trial, a method which weights each trial equally in the total score, preventing thus the overweight of early unreliable trials. Liggett's correlations after such "correction" were different from those before correction, but I cannot understand how this could be, for r is unaffected by the dividing of each gross *total score* in each variable by a constant, such as sigma. Again, Tolman's method of excluding inconsistent animals who deviated more than 2 sigmas from the mean in any *single trial* was not followed. Though Tolman stated that such discards "were not ones whose *total scores* (italics Tolman's) were necessarily widely variant from the average," nevertheless, Liggett says "In Table 3 the same weighted scores were, the extreme cases having been ruled out by dropping all scores that were more than three sigmas away from the mean. . . ." This cast out not necessarily the inconsistent animals, but the brightest and dullest! Considering "corrected" scores, I find that the reliability coefficients for seven trials on the straight-away and for three trials on the maze were respectively, .304 and .038. The four correlations between straight-away and maze scores were .370, .248, .179 and .002. Now these last four inter- r 's indicate low community of function between the fallible measures of the two abilities. By the use of the Yulean correction for attenuation formula ($r_{\infty \infty} =$ geometric mean of intercorrelations divided by geometric mean of reliability coefficients) (1, p. 158) I have found the r between the true measures of the ability to be around .7. This value *suggests* that in so far as individual differences in learning were measured by Liggett at all, the straight-away and

the maze may have measured somewhat the *same* mental function. But the errors of these coefficients are so large as to make the results inconclusive.

Experiment 7. In the recent report of his researches on the correlation between maze ability and brain injury (9), Lashley deals, in part of his work, with two 8-blind mazes. Normal animals ($N = 59$) gave a correlation of $-.36$ between errors on the two mazes, operated animals ($N = 37$), $.67$. The correlation for normal animals was not significantly different from zero. But the reliability of the measures was not known, the only evidence available on this point being the correlation between the first 10 trials on maze III with total number of trials to meet the learning criterion on III. This correlation, which for normal animals was $.06$, for operated $.62$, does not in any degree satisfy the definition of the reliability coefficient, though it suggests that individual differences in ability were completely unreliable among normal animals, but *were fairly reliable* among operated ones. These data indicate, it seems to me, that the maze problems were probably so simple, there being so few blinds, that individual differences among the normal animals were due to unsystematic factors, but when systematic variations were *introduced* by differential brain destruction then the mazes became more complex problems to those animals who experience the greatest brain destruction. Thus a *general learning ability* was caused to appear by operation, for the amount of destruction was positively correlated with the formation of each of the *different* maze habits (see p. 73). But Lashley's data offer nothing conclusive as to the community of function between maze abilities for normal animals. We do not know whether absence of correlation was due to attenuating errors of measurement (which might have been excluded by increasing the complexity of the mazes) or to independent *special* factors at work in each maze ability.

Experiment 8. Finally, we come to Mrs. Williams experiment (21) in which, as a subsidiary problem, she calculated the correlation between the rats' ability to form a visual discrimination (D) and ability to learn a 14 unit T-maze (M). The reliability

coefficient of the discrimination measure was .96, that of the maze measure .88. For one group of rats the raw r_{dm} was .16, for another group, r_{dm} was .08. The coefficients corrected for attenuation came out respectively, .17 and .09. But even here, we are haunted by the spectre of *inconclusiveness*. For the animals were given varying numbers of trials on the discrimination box, thus rendering a reliability coefficient spuriously high because all animals were not given an equal opportunity to make errors. While this was acceptable for the main purpose in Mrs. Williams' experiment, nevertheless the spuriously high reliability coefficient causes the r_{dm} corrected for attenuation to be *too low*. We have, in fact, no way of estimating its value. Again, the N was only 25 in her groups, and the probable errors of her values are fairly indeterminate.

This resumé of previous experiments on the community of function between divers learning capacities in animals leaves us approximately where we started from. No one of them sufficiently satisfies the quantitative requirements which enable us to say either that there is or is not a general learning capacity, or to what degree common factors operate. We turn now to data, which were collected and analysed with an eye to satisfying *all* of the requirements necessary in such an analysis.⁵

IV. RESULTS: THE COMMUNITY OF FUNCTION BETWEEN TWO MAZE ABILITIES

A. *The community between "total" maze abilities*

In this section, the results will be analyzed systematically under the captions of the quantitative requirements outlined in Section II.

1. *Definition of the abilities here investigated.* The X -ability is defined as the total number of errors (partial or whole entrances

⁵ In such a critique as the above, it is virtually impossible to avoid a critical tone which may sound disparaging. Disparagement is furthest from my intention. If the analysis of the data of the writer is an advance over the previous work, it is only because he has had the later opportunity of studying this previous work and of thus being enabled to avoid features in it which, in his opinion, lead to inconclusiveness.

into blind alleys) made in 18 trials (2-19) on a 17 unit T-maze. The Y -ability is defined as the total errors in 18 trials (2-19) on another 20 unit T-maze. In Section II of the first paper of this series (18) rather complete experimental details were given, so to conserve space they will not be repeated here. Suffice it to say that the mazes differed in many mechanical details. Thus, pattern, number of blinds, experimental setting, and the method of handling and rewarding the animals were different.

2. *The "spread of talent" of the individuals.* Every effort was made to get a "random sample" of adults. This was approximated by choosing the individuals from a large number of litters from stocks which had no history of inbreeding (See (18), Section II, 5).

3. *Heterogeneity of irrelevant factors.* The rats differed in sex, pigmentation, age and weight. But in the second paper of this series (19), it was shown that the correlation of each of these factors with maze performance was insignificantly different from zero. Hence, they may be dismissed as sources of correlation between X and Y .

4. *The number of individuals.* The larger group which we will consider numbered 141, the smaller comprised a sample of 107. Since these N 's are satisfactorily large, the dependability of our results may therefore be accurately estimated.

5. *The reliability coefficient of the correlated measures.* By correlating error on odd and even trials, then estimating the reliability of total trials by the Spearman-Brown formula, the coefficients came out:

$$r_x = .9876 \pm .0014 \quad r_y = .9682 \pm .0018$$

6. *The true community of function between the two abilities.* The raw correlation coefficient was computed from a scatter diagram as shown in figure 1. The entries have been distinguished as to males (squares), females (circles), pigmented (blackened) and albino (white). The purpose was to indicate that the correlation existed for all groups, and that it was not produced by a few extremely bright and extremely dull animals. The computations resulted as follows:

Correlation between X and Y $r_{xy} = .77 \pm .02$
 Correlation between X_{∞} and Y_{∞} $r_{x_{\infty}y_{\infty}} = .79 \pm .02$

The high correlation of about .8, between the true abilities, indicates that of the total systematic factors which cause individuals

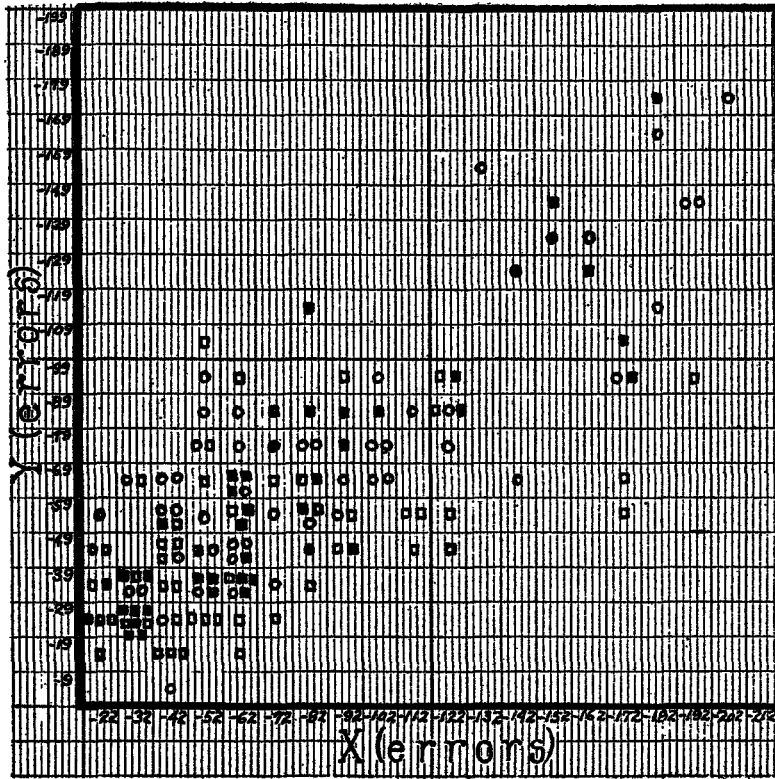


FIG. 1. SCATTER DIAGRAM SHOWING THE CORRELATION BETWEEN ERRORS MADE IN 18 TRIALS ON MAZE X AND ERRORS MADE IN 18 TRIALS ON MAZE Y

The entries have been distinguished as the males (squares), females (circles), pigmented (blacked) and albino (white).

to differ in each of these two maze abilities, a large proportion is common.

Interpretation in terms of percentage determination. Explicitly, what does this coefficient of approximately .8 mean? In terms of predicted overt behavior, the answer is simple enough, for we

know from it that the animal who is bright in X has a distinct tendency to be above the average in Y -ability. But in terms of the proportion to which variation between individuals in X is caused by the common factor that produce variation in Y , its answer is not so definite. This common factor represents the ability to learn which is *general* so far as these two abilities are concerned. To what extent does it operate in X and Y ? There are several equally plausible answers to this question, depending upon the hypothesis one entertains regarding the structure of these abilities. I have elsewhere (16, 17) gone rather extensively into this matter.

We might suppose, first, that the variation in ability X_{∞} represents *itself* the general ability, and that individual differences in Y_{∞} are functions of other factors as well as of this common ability. In such a case the square of the true correlation represents the per cent to which the variance in Y_{∞} is determined by the variance of this general common factor (17, p. 150). That is, the general ability determines 100 per cent of X_{∞} but only 64 per cent of Y_{∞} the remaining 36 per cent deriving from factors special to Y_{∞} alone. Such an hypothesis might be defended on the ground that since every effort was exerted to control environmental variations in the running of maze X , the individual differences resulting there represent genetic variation in T-maze ability, a source of variation also at work in the Y ability. Further, since X preceded the running of Y , certain non-genetic factors occasioning variation in X ability may have readily *transferred* to Y . Thus, all the variation of X may be represented in Y , though other independent sources of variation are at work in Y , these deriving from uncontrolled systematic factors arising late in the experimental period.

Second, we might suppose that the general or common ability determined to approximately an equal degree individual differences in both X_{∞} and Y_{∞} , other residual factors deriving from rather local special features in each maze regimen being at work as differentiae. In such a case the true correlation itself represents the percentage determination of each ability by the common factor (18, p. 151). This would be 80 per cent, leaving for the residual determination by special factors of 20 per cent. Such

an assumption was made by Kelley in his determination of community of function between intelligence and achievements in human beings (8, Chap. 8), and it could be defended on the grounds that the common factor represented genetic variation as a source of ability to learn, whilst a small portion of variation (20 per cent) derives in each ability from uncontrollable environmental systematic causes.

Finally, the assumption is tenable that $X_{..}$ represents a more complexly determined function than $Y_{..}$ which in itself derives from only a portion of those factors working in $X_{..}$. This amounts to hypothesizing that $Y_{..}$ is the common factor, determining a per cent of $X_{..}$ denoted by the square of the true correlation (18, p. 151), namely, 64 per cent of $X_{..}$. This would leave about 36 per cent of $X_{..}$ arising from the operation of other factors not represented in $Y_{..}$.

The net result of these interpretative considerations is to indicate that a true correlation of .8 has somewhat of an elastic meaning in terms of the manifestation of a general or common ability. But from them may be drawn, none the less, a conservative statement, namely, that *at least 64 per cent of the variation in either ability derives from a common or general ability and possibly a good deal more.*

The writer is not convinced that less than a unity correlation between $X_{..}$ and $Y_{..}$ indicates the presence of independent special factors in these two abilities. It seems quite possible that the very same factors may be at work occasioning individual differences in both abilities yet due to a complex interaction between these factors, or to certain arbitrary experimental conditions, the correlation fail of being unity. But this rather complex problem cannot be considered here. At the present time, furthermore, we cannot state what these factors are in terms of genetic or physiological or behavioral units, but must wait upon results of further experiments.

B. The community of function between stages of learning in one maze and "total" ability in the other

We present here only subsidiary evidence to substantiate further the conclusion drawn that there was a high community of

function between performance on maze X and on maze Y. It occurred to the writer that in certain *stages* in the learning of X and Y, common or general factors may have been more potent than at other stages. Data is at hand on 107 rats, a representative sampling of the total 141 of the above major group, and for the smaller group the correlations between the successive stages in X and the total score in Y, and successive stages in Y against the total score in X have been calculated. Each *stage* is defined as the errors made on three trials.⁶ In the following table are given the raw correlations of successive stages in X with the total score on Y. Let the subscripts tell the number of the trials entering into a stage. The number below each coefficient is its P.E.

Raw correlation	$X_{2,3,4}$	$X_{5,6,7}$	$X_{8,9,10}$	$X_{11,12,13}$	$X_{14,15,16}$	$X_{17,18,19}$
with Y	{ .47	.64	.70	.75	.77	.79
	{ ±.05	±.04	±.03	±.03	±.03	±.03

And the raw correlation of successive stages in Y with the total score on X were as follows:

Raw correlation	$Y_{2,3,4}$	$Y_{5,6,7}$	$Y_{8,9,10}$	$Y_{11,12,13}$	$Y_{14,15,16}$	$Y_{17,18,19}$
with X	{ .76	.71	.71	.60	.60	.59
	{ ±.03	±.03	±.03	±.04	±.04	±.04

From these tables it is apparent that even with stages containing errors made on only three runs, which have, of course, considerably less reliability than score on 18 runs, the raw correlation reached almost .8 in some instances. When we estimate the true correlations by correcting the above values for attenuation,⁷ we secure the following values:

True correlation	$X_{\infty 2,3,4}$	$X_{\infty 5,6,7}$	$X_{\infty 8,9,10}$	$X_{\infty 11,12,13}$	$X_{\infty 14,15,16}$	$X_{\infty 17,18,19}$
with Y_{∞}	{ .57	.73	.76	.80	.82	.84
	{ ±.06	±.04	±.03	±.03	±.03	±.03

⁶ The sample of 107 was that upon which the writer made an analysis for a doctor's thesis (14).

⁷ The reliability coefficient of a given stage was estimated as the average correlation between the stage and the one preceding and following it. Such a coefficient is probably too low, and gives corrections for attenuation too high. The corrected values given in the table in the text are therefore only to be considered as approximations. The P.E.'s of the true correlations were calculated by means of the formula printed in Kelley's *Statistical Method* (7, p. 209, formula 161).

	$Y_{\infty 2,3,4}$	$Y_{\infty 5,6,7}$	$Y_{\infty 8,9,10}$	$Y_{\infty 11,12,13}$	$Y_{\infty 14,15,16}$	$Y_{\infty 17,18,19}$
True correlation	.91	.81	.80	.70	.69	.65
with X_{∞}	$\pm .03$	$\pm .03$	$\pm .03$	$\pm .05$	$\pm .05$	$\pm .04$

The evidence from these tables indicates that between certain stages of learning one maze and total performance on the other a high community of function exists. One of the most noticeable things in these tables is the systematic change in the magnitudes of the correlations as learning proceeds. The temporal sequence in learning actually proceeded from $X_{2,3,4}$ to $Y_{17,18,19}$ so that in proportion as each stage is *nearer* temporally the learning of the other maze the correlation is higher. One might immediately interpret this to indicate that correlation arises *because of* temporal proximity and by virtue of greater transfer of training at the most propinquitous points of special factors independent of hereditary factors. But the writer has resisted (14) this interpretation because he sees no reason why just the same systematic absolute change in magnitudes might not have occurred if the learning of Y had preceded that of X . In the absence of such evidence, it seems best for the present to waive any conclusions as to the *nature* of the factors at work and just to note the *presence* of common factors as indicated by the high correlation coefficients.

We find, therefore, that there really exists for the different habits here considered a high community of function, and that for parts of learning it is even higher than for total learning, reaching in some cases a high degree indicated by a true correlation of .91. These common factors may be thought of as a general learning factor (i.e., "general" as far as the learning of these two mazes is concerned), the nature of which is undivulged by these experiments. Doubtless it has many degrees of freedom, being the function of variation in numerous component factors or sub-abilities. The origin of these, the nature of their physiological or behavioral expression may, we hope, be disclosed by future experiments (e.g., see (15)).

V. SUMMARY AND CONCLUSIONS

1. The purpose of this paper is to investigate the problem as to whether individual differences in rats in the learning of one maze

are correlated with individual differences in the learning of another.

2. Certain quantitative requirements of such investigations as this on the community of function between abilities are formulated.

3. Tested by these quantitative requirements, eight previously reported experiments purporting to indicate low community of function between abilities in animals are found to render inconclusive results.

4. Results of the performance of 141 rats who ran two long T-mazes are reported and a high true correlation coefficient of approximately .8 found between the errors made in the learning of one maze with those made in the learning of the other.

5. Furthermore, the true correlation coefficients (107 rats) between errors made on only three trials in the learning of one maze and total errors made on 18 trials on the other maze are found in some instances to be as high as .91.

6. It is concluded, therefore, that relative to the maze abilities dealt with here, the high correlation between them indicates the existence of a fairly general capacity to learn.

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