STUDIES IN INDIVIDUAL DIFFERENCES IN MAZE LEARNING[1]

VI. Disproof of Sensory Components: Experimental Effects of Stimulus Variation

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INTRODUCTION

When an unselected sample of rats is put to the task of learning a complex 17-unit alley maze, marked individual differences in the final plateau appear. What is the psychological nature of the differences between the animals when they reach their several plateaus?

Theory of sign-learning components

One may postulate that the skilled performer follows specific cues in his errorless runs. He is the expert "sign-learner" in the maze situation. Theoretically, one could analyze his behavior into a complex of stimulus-response connections—the differential visual, auditory, tactual, olfactory, kinesthetic, and other sense cues constituting the "signs" to which his "correct" movements have been associated by conditioning. Rats differ widely and consistently in sign-learning, as experiments with discrimination boxes show (13). Hence, one may postulate that individual differences in plateau performance are due to differences in capacities for sign-learning. We may call such determiners "sign-learning components." If one postulates these factors as the *exclusive* determiners of final skill, one would

assume that the stimulus differences upon which the learning depended are above all rats' thresholds.

Theory of sense-acuity components

One may assume, on the other hand, that sense-organ efficiencies are the major components of maze behavior. Rats doubtless differ widely in their acuity thresholds in the various sense-modalities. Though admitting that rats are directed by sense cues, a proponent of the sense-acuity theory would believe that the major determiners of *individual differences* in final skill are acuity differences in the animals. On this theory, one would hold that rats would perform similarly if they could perceive cues equally well.

The above two theories may be termed *theories of sensory components* since both assume that performance is determined by the responses of the animals to specific stimulus features presented to the animals in maze running. Neither theory postulates necessarily the existence of a single component, for there may be multiple sign-learning or sense-acuity components. One may, indeed, postulate the operation of both types of components.

Theory of generalized directional components

One may assume, however, that the final skill is not determined by responses to specific stimulus features. Differences in performance depend on the capacity or capacities of the animals to evolve generalized directional "sets." These abstracted sets determine his movements at choice points. Performance differences do not turn on differences in sense efficiencies or in sign learning. Stimuli serve only as data on the basis of which the animal constructs or abstracts directional sets that determine his final efficiency. Ample stimuli are supplied to all animals for the development of an adequate "conception" of the true maze path. Even from different modalities, sensory data may be supplied, and from these, "correct" directional sets are educed by the animal. Since the generalized postural sets are in the nature of abstractions, after the animal has educed them, one may, within

wide limits, unsystematically vary the stimulus features of the maze, and the animal could continue his correct performance, for it is controlled by directional abstraction and not by sensory signs. Individual differences in performance at the plateau would reduce to differences in capacity to educe or evolve these relational directional abstractions.

It is theoretically possible, of course, for final efficiency to involve the interaction of the three types of components: the bright rat may be the one with the best sense efficiencies and thus perceive more crucial stimulus features, he may be better able to associate specific cues with certain paths and actually do so in parts of the maze, and he may more readily develop generalized directional abstractions regarding the "plan" of the maze. Rats doubtless differ in sense efficiencies, sign-learning capacity, and ability to evolve generalized directional sets. The question as to the extent to which these cognitive components enter into complex maze learning constitutes our problem, which clearly lends itself to experimental investigation.

In a previous paper (12), the writer reported evidence indicating that the visual stimuli of the alley maze were apparently quite unimportant to the rat in the maze studied. The results led the writer to reject the possibility of visual components being the determinant of differences in maze ability and to postulate that the differences resided in "capacity for association." This last phrase was not analysed further, and was simply put forth to indicate that the important differentia constituted some sort of internal integrating capacity. No evidence was available to indicate the rôle of sense acuity in other modes than vision, and no evidence was presented to indicate whether the "capacity for association" referred to simple stimulus-response connectability or to a more generalized mode of association.

The present paper proposes to describe experiments[2] the results of which lead one to reject the possibility of acuity differences in other sense modes than vision determining maze ability, and

[2] The reader will not be burdened with a superficial summary of previous work on the sensory control of the maze habit. A bibliography may be found in the monograph on this subject by Honzik (4).

further to describe the methods and results which have enabled us to decide that differences in maze ability are determined by relational-set formation rather than by simple S-R connectability. The method in these experiments required the rats to run nineteen trials on the 17-unit T-maze. By the nineteenth trial the effects of practice ceased—as a group and as individuals the rats reached their several plateaus. Analysis shows that there are marked differences between rats in final efficiency and that these differences are determined primarily by hereditary causes. If these fundamental hereditary causes primarily operate as determiners of differences in the sense acuities so that differences in maze ability are caused by the animals being differentially capable of discerning cues in the maze, then by the simple procedure of disrupting the cues in the maze, we would disrupt individual differences in efficiency. For example, if a rat is bright because he has inherited a superior olfactory apparatus which enables him to pick up and be guided by distinctive olfactory cues in the maze, then if we disrupt such cues, he should lose his efficiency. If we find by disruption of olfactory cues that the animal still runs the maze perfectly, we know that he is not bright by virtue of olfactory superiority. If, for the full array of rats, the correlation is high between a trial with olfactory cues present and a trial with such cues disrupted we know that the factors causing individual differences in ability are not mediated through olfaction. Further, if no disruption occurs, we have also eliminated the second possibility of efficiency being determined by simple olfactory stimulus-response association ability.

This technique has been used in all of the experiments reported below. In each, the animals were run to a plateau, then they were given several "test trials" in which cues of the various sense modes were disrupted in various ways. In the first experiment, all the visual, olfactory and tactual cues associated with the portable choice points and curtains were disarranged in the test trial by simply interchanging the choice points and curtains. In the second, all visual cues were completely withdrawn by running the animals in complete darkness. In the third, fixed serial kinesthetic cues were disrupted in the test by "short-

cutting" the animals in the maze, that is, by leaving out a section of the maze and permitting the animal to go directly from unit 2 into unit 7. If an animal is bright because of a superior ability to sense proprioceptive stimuli or to form simple serial kinesthetic associations such disruption of the serial order should reduce him to ignorance of the maze pattern after the short-cut. In the fourth and fifth experiments, stimuli of all modalities were obliterated or disarranged on the test trial by running the animals in the dark, short-cutting them, interchanging the units, etc.

If, despite such violence done to the stimulus field, there still remains a high correlation between the period of "fixed stimuli" and the test period of "disarranged stimuli," the factors causing individual differences must not reside in differential sense acuities or capacity to make simple S-R connections, but must be mediated through a higher generalizing, abstracting capacity.

EXPERIMENTAL DETAILS AND ANALYTICAL METHODS

The maze

The maze used consists of seventeen T-units arranged in the pattern shown in figure 1. Each T-unit is like that shown inset in the figure, consisting of a stem, choice-point, blind, true path, and generally an elbow or corner. A one-way door is in the stem, one curtain in the blind, another in the true path. All parts and sections of each T are removable. Except the choice-point which has a stationary floor, the floors of the blind, stem, and true path balance on a central fulcrum so that as the animal steps on them they dip slightly, causing an electrical contact to be made underneath which transmits an electrical impulse to a recorder. The animal thus writes his path on a running tape. By means of a revolving table each animal is debouched into the maze and "picked up" after his run. Full details of the construction and operation of this apparatus are given elsewhere (8).

The animals

These experiments were adjuncts to an inheritance problem (10, 13, 14) and the animals employed were subjects also in the

inheritance problem. The animals were given nineteen trials on the maze, the total number of errors made on these trials, excluding the first, being the score on the basis of which "bright" and "dull" animals were selected for breeding. The subjects reported below were drawn from the P, F_1, F_5, and F_6 generations.
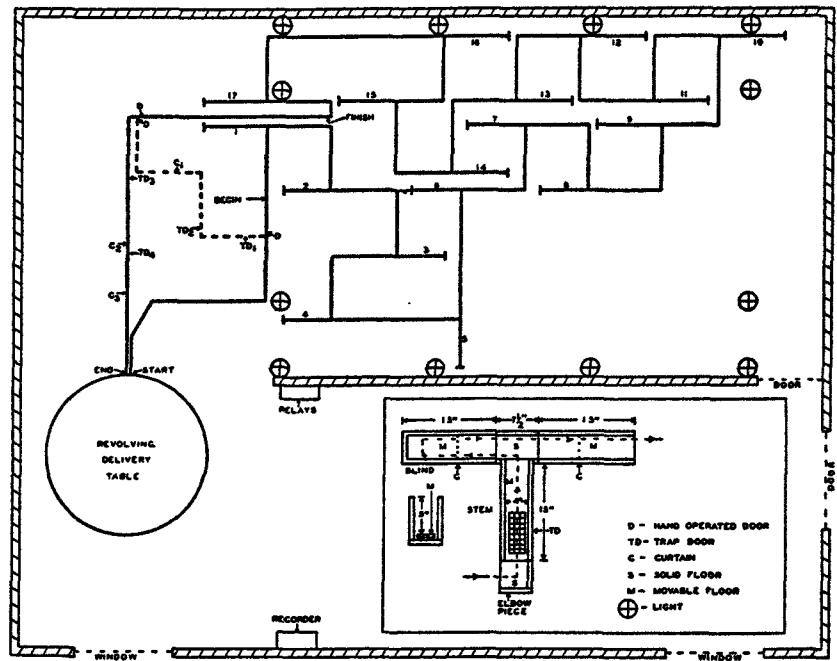
FIG. 1. FLOOR PLAN OF THE 17-UNIT T-MAZE

The dashed line beginning at TD_1 is the preliminary practice path. The inset figure shows the detail of each T-unit.

General procedure

Full statement of the procedure is given in previous papers (8). Briefly, it consists in giving the animals eleven trials on a practice path involving no choices but gradually introducing them to the mechanical features of the maze. Then they run one trial a day for nineteen days. The test trial in most of the experiments reported below followed the nineteenth trial. In several experiments, slight deviations from this procedure were introduced but these will be mentioned in their proper place.

Methods of analysing results

We shall analyse the effects of the sundry variations in the stimulus field in two ways:

Effect on mean performance. In view of the high correlations found between trials on this maze and of the fairly large number of animals employed in each experiment the standard error of the difference between means on consecutive trials is exceedingly small, and the learning curve especially in the plateau period before the test trial is generally smooth. If variation in the stimulus features has any real effect in the test trials, the curve will rise up out of the region of variation of means on the preceding plateau trials. Instead of calculating the standard error of the difference between the means in the test trial and the means in the preceding plateau trials, we shall use the more empirical test of significance in which the actual variation of the means in the preceding plateau period is used as evidence of chance fluctuation of such means, and declare that if the mean performance in the test trials lies outside of this actual preceding variation of means, then the stimulus variation has a true effect on the *particular* sample of rats employed.

In cases where a real loss of efficiency in the test trial is indicated, the next question arises: What is the psychological significance of the *magnitude* of this loss? We need psychological reference points against which to gauge this loss. One such reference point is the final degree of improvement at the plateau period. If this, for example, shows a mean score of zero errors, this fact would indicate that all animals know the true maze path perfectly. Whatever its magnitude, however, the plateau level represents the mean limit of learning in the group. Now, the obvious other psychological reference point is that represented by no knowledge whatsoever of the maze. There are two methods of determining this. The first is *theoretical,* and may be calculated for errors by determining the mean number of errors expected by *chance.* If each animal runs by chance, being equally likely to make a correct as an incorrect choice, then the most probable number of errors for N rats per trial would be for our seventeen blind maze $8.5N$.

A criticism of the view that mean chance performance represents zero knowledge of the maze is that animals ignorant of the maze may never run by chance. Much depends on the pattern of the maze. The writer has analysed the entrances of more than one thousand animals into each blind on the first trial of their maze runs when, if ever, chance behavior should manifest itself, and has found that rarely do blinds get fifty per cent errors. For instance, in our maze, blind 15 earns eighty-five per cent errors and blind 14 only twenty per cent on the first trial. Thus, the theoretical chance performance cannot be used uncritically as a criterion of ignorance. The second method of determining zero knowledge is the *empirical* one of using the mean performance of the animals on their first trial, when they know nothing of the maze.

The importance of knowing the zero point of knowledge of the maze is to discover to what extent a loss of efficiency in the test trials is a return to ignorance. For example, if we blotted out visual cues entirely in the test trials and found a real loss of mean efficiency, it is important to know whether or not that loss represents a return to complete ignorance. If the mean score returned to the magnitude shown on the first trial we should conclude that visual cues are the sole cues used in the maze (neglecting for the moment the possibility of emotional upset or distraction as being a factor). If the mean score showed only a slight loss relative to the reference point of ignorance, then we would know that other factors than visual were at work enabling the animal to find his way correctly through the maze.

Effect on individual differences

One method of analysis consists in discovering whether or not individuals take the same relative position in the plateau as in test periods. For example, in the experiments where, in the test trials, *all* apparently relevant stimuli were varied, if the correlation between the test trials and plateau trials approached unity, we would conclude that the factors causing individual differences were the same in the two periods. But since high efficiency in the *test* trials could not be due to superiority in

discriminating stimuli or in simple S-R connectability, as all the stimuli were translocated in this period, then the factors making for superiority must represent some non-sensory function.

Another method of analysis consists in noting the number of rats making errorless and high speed performance during the test trial. For these rats it would appear unequivocal that their performances were governed by non-sensory components.

<div align="center">THE EVIDENCE</div>

Evidence indicating that trials immediately preceding the test trials represent a plateau in mean performance

Our technique requires that the animals plateau in learning before variations in the stimulus field are introduced. If they do plateau then the group serves as a "control" on itself in the sense that, had the animals been run on into the test period without disruption of stimuli, we know that they would continue with the same statistical constants per trial. We thus may compare the new statistical constants earned under the new test conditions with the previous plateau constants just as if we were actually dealing with two different groups: an experimental and a control group. The two such theoretical groups would be perfectly matched samples and not two random samples.

To establish unequivocally the presence of a plateau, we need a large number of animals so as to smooth out sampling fluctuations. The mean errors (1,170 rats) and mean time (1,160 rats) per trial on successive stages of learning are given in table 1. For errors, each mean value was secured by adding the total errors for a stage of three trials, dividing this total by $3N$ to secure the average error per trial. The mean time was similarly figured for three trial stages for 387 rats from generations F_4 to F_6. In the earlier generations, P to F_3, as the time data were totalled for successive stages of two trials each in our computations, the total time of these 773 rats per stage was divided by $2N$ to give mean time per trial. Note that in errors, from trials 14 to 19 there is no material change in efficiency; in time, the F_4 to F_6 group show a slight loss, the P to F_3 a slight gain during this period, but in general no im-

portant changes over this period appear. We have therefore termed the period from trials 14 to 19 the "plateau period."

We conclude from these data (1) that were the animals to continue for several trials beyond nineteen under unaltered stimulus conditions the statistical constants of these later trials would correspond closely to those of the plateau period, (2) that any material changes of efficiency in the test trials following trial 19 would be due to experimentally produced changes in the stimulus field.

TABLE 1

Mean errors and time (5 second units) on successive stages of learning

Trials included in a stage enclosed in parentheses

	GENERA-TIONS	N	(2+3 +4)	(5+6 +7)	(8+9 +10)	(11+12 +13)	(14+15 +16)	(17+18 +19)
Errors.......	P to F$_8$	1,170	7.0	4.3	3.4	3.0	2.6	2.4
Time........	F$_4$ to F$_8$	387	42.1	27.7	23.7	20.8	19.3	20.5

			(2+ 3)	(4+ 5)	(6+ 7)	(8+ 9)	(10+ 11)	(12+ 13)	(14+ 15)	(16+ 17)	(18+ 19)
Time........	P to F$_2$	773	50.3	31.1	22.6	18.5	17.3	15.7	15.0	15.1	14.4

Evidence as to the generality of the components causing individual differences in capacity in the plateau period

Since our primary objective is inference as to the psychological nature of factors causing individual differences during the plateau, we must establish the fact that there *are* stable components throughout this period. This fact is established if there exists an exceedingly high reliability coefficient of scores in this period. Reliability coefficients and relevant constants are given for stages of three trials and of six trials in table 2.

In view of the fact that the means and sigmas of stages (14 + 15 + 16) and (17 + 18 + 19) are approximately equal, these two measures satisfy the *statistical* requirements of two comparable measures of the same function. Hence the correlation between them represents the reliability coefficient of each. Note that for all three generations considered together the value has the

high magnitude of .918 ± .007 for errors, and .892 ± .010 for time. Due to the excessive skewness of the time scores, a fact which weighted heavily the differences between the slow animals, the calculations were done all over again, this time using the logarithm of the time scores according to scale A (see appendix). But this transformation did not significantly change the correlation, the value turning out to be .900 ± .009. Graphic evidence of the high correlations between the successive stages of the plateau period is shown in table 3.

The criticisms by Leeper (6) and Spence (7) of the method of using the correlation between trials as evidence of the reliability of individual differences at such trials requires comment at this

TABLE 2

Reliability coefficients and other constants of errors and of time on stages (14 + 15 + 16), (17 + 18 + 19), and (14 to 19) for the $F_4 + F_5 + F_6$ population

	N	STAGE (14 + 15 + 16)		STAGE (17 + 18 + 19)		r BETWEEN (14 + 15 + 16) AND (17 + 18 + 19)	RELIABIL- ITY COEFF.[*] OF (14 TO 19)
		M	S.D.	M	S.D.		
Errors.................	400	9.7±.5	9.3±.3	9.4±.4	8.8±.3	.918±.007	.957±.004
Time (raw)............	391	57.2±1.9	38.4±1.4	60.8±2.1	42.0±1.5	.892±.010	.943±.006
Time (log scale A).....	391	6.85±.18	3.62±.13	7.19±.18	3.65±.13	.900±.009	.947±.005

* Reliability coefficients of raw errors and time for F_4, F_5, F_6 separately are respectively, errors: .95, .97, .93; time: .93, .97, .92.

point. Their position is, briefly, that such correlations are too high due to "correlation of errors" between trials. This criticism harks back to Kelley's declaration that securing the reliability of a test by correlation with a "retest" gives too high a value due to "memory transference" (5). However aptly Kelley's point may apply to a single form of a mental test, it seems to the writer that it does *not* apply to a specific *learning* function, for the simple reason that in such a function "memory transference" is not here an error but the very ability which one actually wishes to measure. On trials (17 + 18 + 19) we wish to measure the capacity of the rat to transfer the memory of what he experienced in trials (14 + 15 + 16). The correlation coefficient between these periods tells the degree to which individuals consistently differ in this capacity. There is nothing spuriously high in such

TABLE 3

Correlations between errors (a) and between time (b) on successive stages (14 + 15 + 16) and (17 + 18 + 19) of the plateau period for 400 (errors) and 391 (time) $F_4 + F_5 + F_6$ rats

(a) Errors

Stage (14 + 15 + 16) vertical, Stage (17 + 18 + 19) horizontal. $r = .92$

Stage	0–1	−3	−5	−7	−9	−11	−13	−15	−17	−19	−21	−23	−25	−27	−29	−31	−33	−35	−37
−37														1					
−35																			
−33									1		1		3				1		1
−31													3	1			1	1	
−29							3		1	1	2		1	3	3				
−27										2	1	2	5	1					
−25								1		1	2	3	2		1	1			
−23								1	1	1	3		3	2					
−21							3	2	2	3	2	1	1		1				
−19					2	1	2	2	4	3	2	1	2	1					
−17	1					3		2	3	1		2							
−15						2	3	2	2	3	3								
−13							1	2	1	3	1	1		1					
−11		1	3	6	6	5	2	1	1	2									
−9	1	1	3	6	7	2		2											
−7		9	9	7	3		1	1											
−5	5	15	8	8	4		1												
−3	19	24	17	9	1	2													
0–1	35	28	11	1															

(b) Time (Log scale A)

Stage (14 + 15 + 16) vertical, Stage (17 + 18 + 19) horizontal. $r = .90$

Stage	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17
17																	
16													1		1		
15											1	1		1	3		2
14											1	2	1	4		1	1
13										1	1			2	4	2	
12							1					3	3	6	4	3	
11									1	1	4	8	8	2		1	
10								1	1	3	6	5	6	2	3		
9						1	1	2	2	7	8	1	2	1			
8						1	2	8	7	7	1	1	1	1			
7					1	5	6	5	7	2	1	1					
6					4	8	9	10	1								
5				2	8	12	13	3	2	1							
4			1	7	22	9	5	4									
3	1		6	19	13	9	1					1					
2	1	11	6	6	4			1	1								
1	1	1	1									1					

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a statistic, for it is simply a symbolic statement of what one can actually see in table 3. Knowing what score a rat secures in the first stage of the plateau period, we can predict with small error what he will secure in the second. In other words, this magnitude measures the consistency of individual differences over the period, and from it we can secure the error of an individual's score, which in this case is small. We conclude from these high reliability coefficients that potent systematic causes are at work causing individual differences.

We turn now to discovering the extent to which the factors are the *same* on successive trials. To answer this question, I present the data from a group of 67 rats of the F_s generation, called the 5/22/31 group as designated by the date of their first maze trial, a group used in several of our experiments reported below. In table 4, the raw intercorrelations between trials 14, 15, 16, 17, 18, 19 for time and for errors are presented in regular type respectively above and below the upper-left-to-lower-right diagonal.

We define the reliability coefficient of a trial (in the plateau period) as its correlation with the preceding or with the following trial (or the average of these if both are available). We have considered the validity of this definition above and a further complete vindication of such a definition will appear later. So defined, these coefficients are given in the second column and second row of the table. For errors, they are on trials 14 to 19: .86, .87, .88, .87, .85, .83; for time: .94, .93, .90, .88, .88, .89. It is evident that even for separate trials the reliability coefficients are high, that as between trials they are approximately equal and show little systematic change. We conclude, therefore, that on successive plateau trials true systematic factors are of equal and high potency in determining individual differences.

As to whether these true systematic factors or causes of individual differences on *each* trial are the *same*, or *general* throughout all plateau trials, I present the following evidence:

We have stated above that $r_{14,\ 15}$, for example, is the reliability coefficient of either trial 14 or 15. If 14 and 15 each measures the same components and differ only in "chance" factors, then the reliability coefficient of the sum of these scores, namely, of

(14 + 15) is, by the theoretical Spearman-Brown formula, $2r_{14, 15}/(1 + r_{14, 15})$. This coefficient is by definition the correlation between (14 + 15) and another theoretical score comparably composed and equally saturated with the same common components present in (14 + 15). Is (16 + 17) such another score?

TABLE 4

Intercorrelations between errors and between time* of trials 14, 15, 16, 17, 18, 19 for 67 F_4 (5/22/31) rats

Coefficients below diagonal are for errors, above, for time; regular type, raw; italics, true. Left margin refers to errors; top to time; r refers to reliability coefficients

				14	15	16	17	18	19
r...				.94	.93	.90	.88	.88	.89
M...				6.7 ± .5	7.0 ± .5	6.9 ± .5	6.8 ± .5	7.5 ± .5	7.3 ± .5
SD...				4.1 ± .4	3.8 ± .3	4.1 ± .4	4.4 ± .4	3.9 ± .3	4.2 ± .4
	r								
14	.86	M	3.0±.4		.94	.92	.91	.89	.89
		SD	3.2±.3		*1.01*	*1.00*	*1.00*	*.98*	*.98*
15	.87	M	3.2±.4	.86		.92	.90	.90	.89
		SD	3.4±.3	*1.00*		*1.01*	*1.00*	*1.00*	*.98*
16	.88	M	3.2±.4	.88	.87		.88	.86	.90
		SD	3.4±.3	*1.01*	*1.00*		*.99*	*.97*	*1.01*
17	.87	M	3.0±.4	.85	.82	.88		.87	.87
		SD	2.9±.3	*1.00*	*.94*	*1.01*		*.99*	*.99*
18	.85	M	3.1±.4	.90	.87	.87	.86		.89
		SD	3.4±.3	*1.06*	*1.01*	*1.01*	*1.00*		*1.01*
19	.83	M	2.7±.3	.82	.82	.87	.78	.83	
		SD	3.4±.3	*.98*	*.96*	*1.02*	*.92*	*.99*	

* Log scale D used; time record for one rat not complete, hence excluded.

If so, then the *experimental* coefficient, $r_{(14+15)\ (16+17)}$, should equal the *theoretical* value. Table 5 gives the actual results. The first row gives the values to be compared in the example just given. When $r_{14, 15}$ (column 1) is substituted in the theoretical SB formula, the value for errors is .925 (column 3). The experimental coefficient, $r_{(14+15)\ (16+17)}$ (column 2), is .917 (column 4).

Similar comparisons throughout the plateau period are given in the next four rows. The standard errors of the theoretical coefficients by Shen's formula are for errors of the order .018, for time .013. The experimental values thus fit closely the theoretical value. This evidence leads to the conclusion that any two consecutive trials measure the same factors as any adjacent four consecutive trials.

We can likewise test whether any three consecutive trials measure what all *six* trials do. We do this by calculating the theoretical correlation between (14 + 15 + 16) and another score

TABLE 5

Correspondence between theoretical coefficients (Spearman-Brown) expected if same true factors common to all plateau trials and the actual experimental coefficients

THEORETICAL FORMULA INVOLVING:	EXPERIMENTAL COEFFICIENT	ACTUAL VALUES					
		Errors			Time		
		Theor.	Exp.	Diff.	Theor.	Exp.	Diff.
$r_{14,15}$	$r_{(14+15)(16+17)}$	.925	.917	+.008	.969	.958	+.011
$r_{15,16}$	$r_{(15+16)(17+18)}$	.930	.925	+.005	.958	.937	+.021
$r_{16,17}$	$r_{(16+17)(18+19)}$	.936	.907	+.029	.936	.931	+.005
$r_{17,18}$	$r_{(17+18)(15+16)}$	.925	.925	.000	.930	.937	-.007
$r_{18,19}$	$r_{(18+19)(16+17)}$	907	.907	.000	.942	.931	+.011
$r_{14,15}, r_{14,16}, r_{15,16}$	$r_{(14+15+16)(17+18+19)}$	.953	.953	.000	.964	.954	+.010
$r_{17,18}, r_{17,19}, r_{18,19}$	$r_{(14+15+16)(17+18+19)}$	.949	.953	-.004	.936	.954	-.018

equally saturated by the same systematic common factors. We find the unit or average reliability coefficient, r, per trial in this triplet-set of trials, and then substitute this value into the Spearman-Brown formula, $3r/(1 + 2r)$. This gives the theoretical correlation between the sum of these three trials and that of three other such comparable trials. This value we now compare with the experimental value, $r_{(14+15+16)(17+18+19)}$, and should expect agreement if the same factors are at work in all six trials. The values are given in the last two rows of table 5. The standard errors of the theoretical coefficients are of the order .010. Here again we find striking agreement. We thus conclude

that each of the six plateau trials is approximately equally saturated with the same common factors, and that no residual or "group" factors overlap on any two trials.

It follows necessarily that the intercorrelations between all *plateau* trials however remote, should be unity when corrected for attenuation. The corrected coefficients are presented in table 4 below each of the raw coefficients. They are uniformly approximately unity.

Our problem is to determine the nature of the stable common factors (or factor) at work causing individual differences in these plateau trials. In the next section, we shall briefly present evidence showing that, whatever their nature psychologically, they are definitely inherited. Following this, we shall present the evidence bearing on the psychological nature of these factors.

Evidence as to the hereditary causation of the components determining individual differences in ability at the plateau period

The extent to which these systematic causes are hereditary will be determined by discovering the relationship between the individual's score and that of his parents. If rats who are bright, that is, who make few errors, during the plateau trials, are progeny of bright parents, and conversely if those who make poor scores in this period come from dull parents, then we may conclude that the cause of such a difference is hereditary. Since the animals studied here were subjects of the general inheritance investigation, we are in a position to investigate this matter. Table 6 presents the data on progeny of bright and of dull parents. For each rat the total errors and total time on trials 14 to 19 were summed. The mean (M) and standard deviation (SD) of progeny of bright parents (B) and of dull (D) were figured for the four generations, F_4 to F_7. For errors and time, these values are shown in the table. The difference between means is given in the next to last row, and the critical ratio in the last. For example, the mean errors of 74 offspring of dull F_3 parents, i.e., the M_D of F_4, is 29.4 errors; the difference between them and the bright is 21.3, which is 10.7 times its standard error. In the F_7, the difference was 24.5, which is 17.5 times its standard error.

A negligible amount of overlapping of the two groups of progeny occurs. The least overlap was in the F_7 where 95 per cent of the progeny of bright parents made fewer errors than the very brightest of the progeny of dull parents. Even these data do not give the best case for heredity, for by the F_7 the course of selective breeding for maze-brightness and maze-dullness had not proceeded to a point of complete homozygosity in the genetic factors determining the difference between the strains. The facts of table 6 prove definitely that the common factors occa-

TABLE 6

Constants for errors and for time of progeny of bright (B) and of dull (D) parents for generations F_4, F_5, F_6, and F_7 and only for the plateau period (Trials 14 + 15 + 16 + 17 + 18 + 19)

	(a) ERRORS				(b) TIME (LOG SCALE B)			
	F_4	F_5	F_6	F_7	F_4	F_5	F_6	F_7
N_D	74	67	73	68	72	66	73	68
N_B	92	86	88	85	90	82	88	85
M_D	29.4±1.8	34.6±2.0	33.0±1.9	27.4±1.4	15.17±.44	15.26±.53	16.21±.45	14.8±.43
M_B	8.1±1.0	12.4±1.8	11.2±1.2	2.9±.29	8.36±.29	8.39±.44	9.42±.44	4.8±.27
SD_D	15.5±1.3	16.7±1.4	16.0±1.3	11.1±.95	3.73±.31	4.34±.38	3.80±.31	3.52±.30
SD_B	9.2±.68	16.2±1.2	11.0±.83	2.7±.20	2.80±.21	3.97±.31	4.20±.31	2.44±.19
M_D-M_B	21.3±2.0	22.2±2.7	21.8±2.2	24.5±1.4	6.81±.53	6.87±.69	6.79±.63	10.0±.50
Diff/SD* diff.	10.7	8.2	9.9	17.5	12.8	10.0	11.0	20.0

* These critical ratios are approximate since conditions of random sampling do not strictly hold.

sioning individual differences during the plateau period are not environmental but are largely nereditary.

Evidence as to the effect on performance of variation in visual, olfactory, and tactual cues associated with choice points and curtains

Our first experiment attempted to answer these questions: Is efficiency in traversing the maze controlled by response to stimuli at the choice points and at the curtains to the left and right of these? Are individual differences in efficiency determined by the capacity to sense such stimuli and to make simple S-R connections between such stimuli and correct response?

For a group of 71 (5/22/31) F_s animals which had plateaued in learning, we completely interchanged the choice points and curtains. If the animals were being guided by such cues and if individual differences hinged upon capacity to sense such stimuli and to make specific S-R responses, then the mean performance in the test should revert back toward chance, and the intercorrelations between performance on the plateau and on the test trial should go to zero.
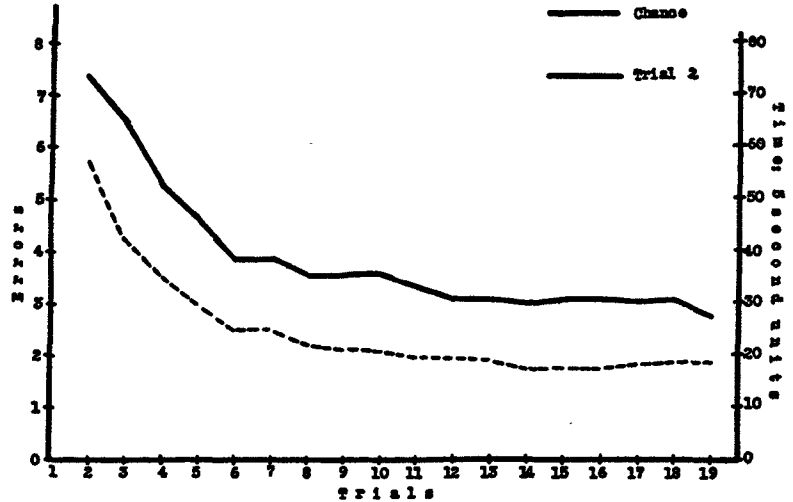

FIG. 2. Learning curves of 71 (F_s) rats; maze unchanged, trials 1–16; all curtains interchanged, trial 17; 8 choice points interchanged, trial 18 and trial 19. Solid line, errors; dashed line, time.

Effect on mean performance. Figure 2 shows the mean error and time curve of the 71 rats, the left ordinate referring to errors, the right to time. Note that by the 16th trial both functions show a plateau. Before the run on trial 17 we introduced the first variation by interchanging the curtain in the blind of each unit with its companion curtain in the opposing true path. Before the daily run on trial 18 was made, half of the choice points were interchanged as follows: Choice point 1 with 17, 3 with 14, 6 with 15, 8 with 12. On trial 19 others were interchanged as follows: 2 with 4, 5 with 7, 9 with 13, and 10 with 11. If specific cues in these choice points controlled the rats' behavior,

then on these trials the animals' performance should have been disrupted. In figure 2, for trials 18 and 19, there appears no disturbance of efficiency whatsoever from these stimulus changes.

Effect on individual differences. As to the intercorrelation between plateau trial 14, 15, 16 and "test trials" 17, 18, 19, the data on 67 rats of this group have already been presented in table 4 above, where it was seen that the crude correlations were exceedingly high, and that the true r's were unity.[3] On the basis of our analysis of table 5, we concluded that exactly the same true systematic causes of individual differences were at work on trials 17, 18, 19 as on 14, 15, 16.

From the data presented in figure 2 and table 4, we conclude that the animals are *not* guided by specific stimuli at the choice points or curtain in the maze and that the causes of individual differences *cannot* be assigned to the capacity to sense stimuli presented by these features of the maze or to the capacity to form S-R bonds between such stimuli and movement.

Evidence as to effect on performance of complete obliteration of visual cues throughout the whole maze

The above evidence does not rule out completely the possibility of the use of visual cues other than those specifically presented by choice points and curtains. Shadows in the paths, lights, and other fixed objects above the maze, the hardware cloth maze-unit covers, and other visual cues from the walls and floors might present stimuli which serve as signs of the correct route.

Our next experiment attempted completely to obliterate such visual stimuli. This was done in the test trials by running the animals in darkness. Since our maze room at the time of this experiment was located in the basement of the old Psychology

[3] In calculating these r's, four animals were dropped from the total of 71 in the error calculation and five in the time. These animals were not excluded because of atypicalness in their performance but because in another experiment involving the run of this same group on trial 20, a complete record was accidentally not secured on these animals, so that in our correlational analyses involving trials 17, 18, 19, 20 (to be presented later) these animals obviously could not be included. They were therefore excluded from all the correlations.

Building (now razed), turning off the light and covering one small window with opaque cardboard sufficed to enshroud the maze in darkness. The rats' progress was automatically written on the recorder in the next room.

I have presented briefly the results of this experiment in the previous paper (12) but did not there show the effects on time, or in certain sections of the maze. The animals used were 46 rats from the P generation and 25 from the F_1. These animals had run 19 trials in the maze, then they were given a long rest (during which time the P rats had run on another maze) and then, they ran 16 *relearning* trials. We shall examine the results on all 71 animals considered as one group. On trials 14, 15, 16 of the relearning series all animals ran for the first time in darkness as above described.

If visual cues completely or partially control maze performance it would appear that such cues would be utilized by the animal throughout the length of the maze. Shutting off the lights would therefore cause approximately an equal loss in efficiency in all sections of the maze. On the other hand, if the animals are not guided *in any extent* by visual cues, some distracting effects of the sudden absence of light might nevertheless still appear. The sudden change in the environmental field would, it is reasonable to suppose, have different effects on successive units of the maze. On entering the first section of the maze, the rats, though not *dependent* in the maze run on visual cues, would notice the absence of light. I am sure that most experienced rat operators would agree that such a sudden new condition would act as a distraction, at least, to some of the animals, who would proceed in a cautious, investigatory fashion in the first few units. As the animal continued through the maze the effect of the dark distraction would diminish as a disturbance and in the last section of the maze affect negligibly the maze run. To investigate these possibilities, I have divided the maze statistically into three sections: Section A consisting of blinds 1, 2, 3, 4, 5, 6; section B of blinds 7, 8, 9, 10; and section C of blinds 11, 12, 13, 14, 15, 16, 17.

Effect on mean performance. The time and error learning

curves per section are presented in figure 3.   The first four trials
are presented in each graph to show the ordinal reference point
of ignorance as denoted by efficiency at the beginning of learning,
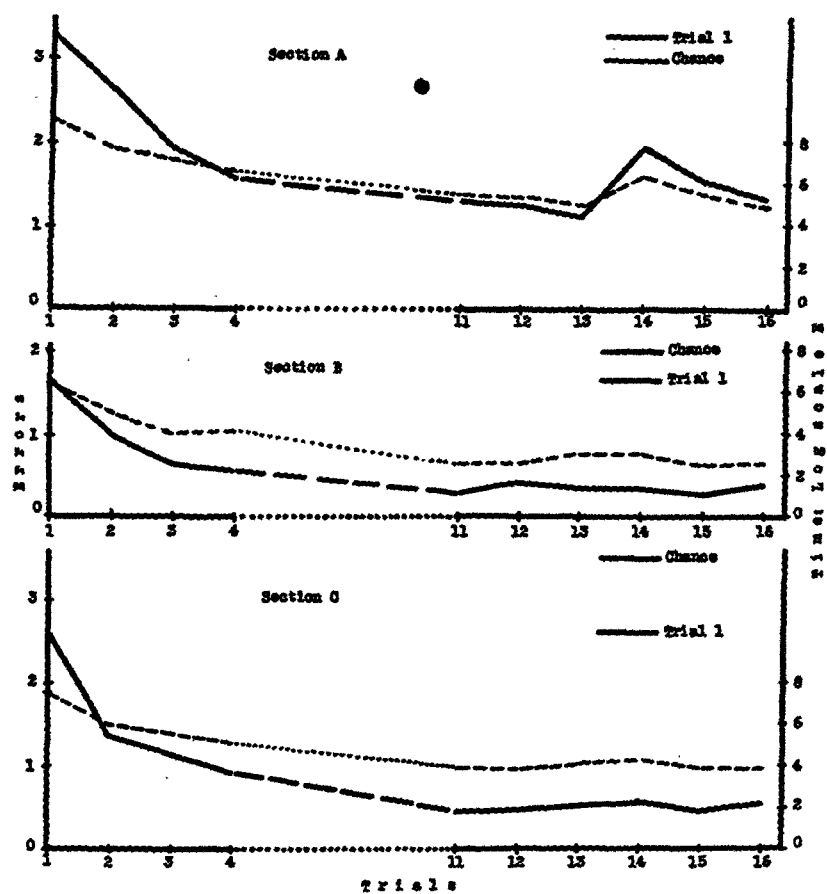

FIG. 3. Learning curves of 71 (P + F$_1$) rats on sections A (blinds 1-6), B
(blinds 7-10) and C (blinds 11-17) of the maze; lights on, trials 1-13; lights off,
trials 14-16.   Solid line, errors; dashed line, time.

then trials 5 to 10 are deleted as not being particularly relevant,
then the three trials, 11, 12, 13, immediately preceding the shut-
ting off of the light are given to show the plateau period, and
finally the trials with lights off, 14, 15, 16, are given.

These curves prove conclusively that removal of visual cues

affects errors and time only in section A consisting of the first 6 blinds. Beyond ,blind 6 the animals were not disturbed by the absence of such cues. These data support the hypothesis that sudden darkness distracts the animal on his first entering the maze, and that such distraction disappears quickly. In the later sections of the maze he ran with the same efficiency and speed as in previous lighted trials, evidently being directed by determinants of a non-visual sort.

TABLE 7

In a group of 71 $(P + F_1)$ rats, the numbers per trial who made no errors, and who ran at high speed in sections A, B, and C during lighted and dark trials

SECTION	CHANCE	TRIALS										
		Lights on								Lights off		
		1*	2	3	4	...	11	12†	13†	14	15	16
Number of rats who made zero errors												
A (1–6)	1.1	1	3	12	11	—	32	29	31	13	19	28
B (7–10)	4.4	12	24	38	43	—	55	52	52	54	58	54
C (11–17)	.6	4	21	32	39	—	56	53	54	48	51	47
Number of rats who ran at the speed of about 1.5 feet per second or better‡												
A (1–6)		0	2	4	6	—	19	21	24	4	19	26
B (7–10)		0	10	17	23	—	40	43	39	40	52	50
C (11–17)		1	8	13	16	—	37	40	33	29	36	38

* Time record for one animal not secured.

† Time record for two animals not secured.

‡ In section A this includes in log scale E steps 1, 2, 3; in section B steps 1, 2; in section C steps 1, 2, 3.

Effect on individual differences. Effect on most skilful performers. Very significant is the fact that some of the most skilful performers were undisturbed by the darkness even in the first section. For these the possibility of their use of visual cues seems completely out of question. In table 7, I present the number of animals who showed perfect scores (zero errors), and exceedingly fast time. These data are presented for trials 1 to 4 and 11 to 16, and for the three sections of the maze. Thus, in the error section of table 7, in the row marked A (1–6), the number 1 under trial 1

means that one animal made 0 errors on trial 1. Notice in this row that on plateau trials 11, 12, 13, the number of animals who made perfect scores were 32, 29, and 31 respectively, but on trials 14, 15, and 16 in the dark the number of perfect scores dropped to 13, 19, and 28 respectively. While this drop is probably significant, the point to be made here is that even on trial 14 and in section A of the maze, *13 animals showed no loss of efficiency* —in fact, ran perfectly. That this loss does not mean a lapse to ignorance of the blinds is clear from a comparison of the number of perfect performers on trial 14 with the number on trial 1, namely 13 as compared to 1. Under "chance" I have placed the theoretical number of rats who would run the section with no errors by chance. This chance frequency is determined as follows: since the probability of a single rat making no errors in one unit by chance is 2^{-1}, for n units it is 2^{-n}. Hence for N rats the number making zero errors in n units is $N2^{-n}$. In section A this number, for 71 rats, is 71/64 or 1.1 rats; in section B is 71/16 or 4.4 rats, in section C is 71/128 or .6 rats. In section B (blinds 7 to 10) on day 14, 54 rats out of 71 made no errors, and in section C, blinds 11 to 17, 48 made no errors.

Table 7 also shows the number of animals who ran at high speed. Time in section A was read off the recording tape from the stem of unit 1 to stem of unit 7. The true path involved traversing 20.3 feet, turning 10 corners, and operating six doors. I have counted the animals who managed this in *15 seconds or less*, that is, those who ran about 1.5 feet in a second or less. Thus, in trial 1, the table shows that none satisfied this criterion, but on trial 13, 24 made this speed. On the first dark trial only 4 made this speed but on the next trial *19* returned to high speed. In section B, time was taken from stem of unit 7 to stem of 11, the true path involving traversing 14.3 feet, turning 8 corners and operating 4 doors. I have here counted the rats who managed this in *10 seconds or less*, that is, those who ran about 1.5 feet in a second or less. Note that on the average about 40 out of 71 could do this in the three lighted plateau trials. Forty kept this speed on the first dark trial, and 52 on the second trial, trial 15. In section C, time was taken from stem of unit 11 to unit 17, the

true path here involving traversing 22.5 feet, turning 12 corners and operating 7 doors. As in section A, I have counted those who managed this in *15 seconds or less*, that is, those who made 1.5 feet in a second or less. On the average about 35 did this in the plateau period and that number was not markedly changed when the lights were turned off. Except for a short section at the beginning of the maze, the evidence from table 7 shows that about half the animals ran at high speed when the lights were turned off, being unaffected by this change. For these brighter animals who ran perfectly and with high speed when visual cues were withdrawn, visual acuity or visual sign-learning played no rôle in their performance.

Correlation between trials with visual cues present and trials with visual cues absent. If highly reliable differences in performance occur in the dark and correlate unity with performance in the lighted maze, we would conclude that individual differences in the normally lighted maze in the plateau period are not due to visual components. The correlation plots showing the relation between total score in the lighted period (trials 11 + 12 + 13) and in the dark period (trials 14 + 15 + 16) are given in table 8. The error plot (a) gives a raw r of .895, the time plot (b), .876. Though these coefficients are not unity, they do indicate a high community of function between fallible performance in the two periods. Does the departure from perfect correlation mean that in the light vs. the dark trials different specific behavior components operate which are elicited by differences in the external stimulus matrices? Not necessarily; it may indicate merely that each measure is somewhat "unreliable." By correcting for attenuation, we may solve this problem. To do this, we need data on the internal consistency of each period.

Table 9 presents the constants of and intercorrelations between the trials in each stage. In section (a) of this table are given the means and standard deviations of each trial, in (b), the correlation between trials. Note that between trials of the lighted stage, where no stimulus changes were introduced, the coefficients are not unity, indicating that some unreliability is present. What we need for correction for attenuation is the reliability coefficient

TABLE 8

Correlations between errors (a) and between time (b) on lighted trials (11 + 12 + 13) and dark trials (14 + 15 + 16) for 71 (P + F₁) rats

(a) Errors

Trials 11+12+13 (Lights on)	0-1	-3	-5	-7	-9	-11	-13	-15	-17	-19	-21	-23	-25	-27	-29
-29															1
-27															
-25															
-23										1		1			
-21															1
-19									1		1				
-17							1	1			1				
-15								2			1				
-13						1	1	1		1					
-11					1		2								
-9				2		1			1				*r* = .895		
-7		1		4		1									
-5			2	1	1		1								
-3	2	2	3	3	1										
0-1	10	11	2	2		1									

Trials 14 + 15 + 16 (Light off)

(b) Time: log scale D

Trials 11+12+13 (Lights on)	2-5	-9	-13	-17	-21	-25	-29	-33	-37	-41	-45	-49
-49												1
-45										1		
-41								1	1			2
-37									1			
-33								1	1	1	1	
-29						2	2		1			
-25			3	2	2			1	1			
-21				4	3		1					
-17		1	6	4	3			*r* = .878				
-13		1	9	2								
-9	1	2	6									
2-5	1		2									

2-5 -9 -13 -17 -21 -25 -29 -33 -37 -41 -45 -49

Trials 14 + 15 + 16 (Light off)

of each period or stage totaling three trials each. To secure this value, we find the reliability coefficient of a single trial in each stage, substitute in the Spearman-Brown formula, setting n equal to 3. On an earlier page, we have shown that this procedure is valid. Thus, the single trial reliability for errors in the lighted stage is .805, this being the mean of the three intercorrelations between single trials in the lighted period, namely, of .871, .707, .839. Substituting this value in the Spearman-Brown formula gives .925, the reliability coefficient of the lighted stage. Calculated in similar fashion, the reliability coefficient of the dark stage of three trials is .957. As these coefficients are not unity, it should be apparent that the raw coefficient *between* the lighted and dark stages could not be unity. The raw coefficient, .895, when corrected for attenuation turns out to be .988. In table 9 the estimated reliabilities and true r's are given in sub-tables (c) and (d) respectively. For time the true r is .928. From these values, we conclude that the factors determining individual differences in performance as measured by errors are in no sensible degree visual components. Some slight specific factor generated by the sudden removal of visual cues affects speed of running. Whether speed of running is to this slight degree *controlled* by specific visual cues, or whether a new but relatively unimportant factor of *distraction* appears in the dark period by the sudden stimulus change is not determined by these data.

Evidence as to the effect on performance of variation in the serial order of kinesthetic cues

There remains the possibility that the rat may be guided in his correct choice of paths in the maze by the specific pattern of his movements made before such choices. The muscle movements involved in the correct right turn of the first unit may provide the stimuli determining the correct left turn in the second, this latter movement providing proprioceptive stimuli eliciting the right turn in the third, and so on. On this theory, the bright rat may possess the most superior proprioceptive acuity, or he may possess the most superior capacity for proprioceptive sign-learning, or he may be the most superior in both regards. Such abili-

ties as these, in so far as they determine individual differences in performance, we may term *proprioceptive sensory components*.

TABLE 9

Constants of and intercorrelations between errors and time* on lighted trials 11, 12, 13 and dark trials 14, 15, 16 for 71 ($P + F_1$) rats

(a) Means and standard deviations

	LIGHTED TRIALS			DARK TRIALS		
	11	12	13	14	15	16
Errors						
M...........	2.0±.3	2.1±.3	2.0±.3	2.9±.3	2.3±.3	2.3±.3
SD..........	2.5±.2	2.5±.2	2.4±.2	2.6±.2	2.5±.2	2.5±.2
Time						
M...........	6.5±.4	6.4±.4	6.2±.4	7.8±.4	6.2±.4	5.7±.4
SD..........	3.5±.3	3.7±.3	3.5±.3	3.2±.3	3.4±.3	3.4±.3

(b) Correlation between trials

	$r_{11, 12}$	$r_{11, 13}$	$r_{12, 13}$	$r_{14, 15}$	$r_{14, 16}$	$r_{15, 16}$
Errors.........	.871±.028	.707±.059	.839±.035	.736±.054	.688±.062	.742±.053
Time..........	.917±.018	.856±.031	.873±.028	.816±.039	.762±.049	.894±.023

(c) Reliability coefficients

	ERRORS	TIME
Stage (11 + 12 + 13):		
Single trial.............................	.805	.882
Three trials.............................	.925	.957
Stage (14 + 15 + 16):		
Single trial.............................	.722	.824
Three trials.............................	.886	.933

(d) Intercorrelations between stage (11 + 12 + 13) and stage (14 + 15 + 16)

	RAW	TRUE
Errors....................................	.895	.988
Time.....................................	.878	.928

* Log scale D used.

Our next experiment constitutes a test of the existence of these components in our maze ability. Clearly, if they operate as the

exclusive determiners of maze efficiency, then any experimental disruption of proprioceptive cues during the plateau period of learning should completely disrupt performance and return all animals to ignorance of the maze pattern. Proprioceptive disturbance was effected on the 20th trial by the simple expedient of short-cutting the animals through an initial part of the maze. This was accomplished by deleting units 3, 4, and 5 (see fig. 1). The rats thus ran directly from the true path of unit 2 into the blind of unit 6 and from there on into unit 7 and the rest of the maze. Mechanically this was effected by the removal of the corner which originally turned the rat unto unit 3 and of the end block of blind 6, and by substitution of a straight piece of alley in the gap. This straight alley was part of the straight path of the deleted section connecting the true path of unit 4 with the stem of unit 5, and thus was adequately "smelled-up." The curtains were left as usual in units 2 and 6, and the door in the stem of unit 6 was wired up to prevent the animal from getting back through into the deleted section.

Were the animals guided by the proprioceptive stimuli occasioned by the maze pattern, they should be completely at sea in the maze beyond the short-cut. This will be clear from a study of the turns in the maze. The correct turns at the 17 successive choice points are as follows:

RL(RLLR)RL*RL*RLR*RR*LL. In parentheses are the choices removed by the deleted section. The first choice point beyond the short-cut is in unit 7 where the correct turn is R. If the rats have learned the maze as a sequence of the correct turns, then in unit 7 they would make an R turn appropriate to unit 3 and here make no error since R is also correct in unit 7; in unit 8 the L turn appropriate to unit 4 would give no error; but in unit 9 the L turn appropriate to unit 5 would send them into the blind. Note in the above series that R for unit 9 has been italicized, meaning by this that the rat would go counter to the R turn here and thus make an error if he persisted in a kinesthetic sequence appropriate to the deleted series. As the italics show, errors would also occur in units 10, 14, and 17, making a total of four errors on the short-cut trial. It is possible that the kinesthetic sequence is even

more refined, bringing in corners as well as choice points.   This
full sequence is as follows:

RRL(RRLLLR)LRRLLRLLLRRLLRLRRRLLLL.   If this
full pattern were persisted in after the short-cut, errors would be
made at choice points in units 10, 11, 12, 13, 15, 16, and 17, a
total of seven errors on the short-cut trial.

Seventy-six rats of the F_{5} generation, group 3/18/31, were sub-
jects in this experiment.   They were given 19 trials, one trial a
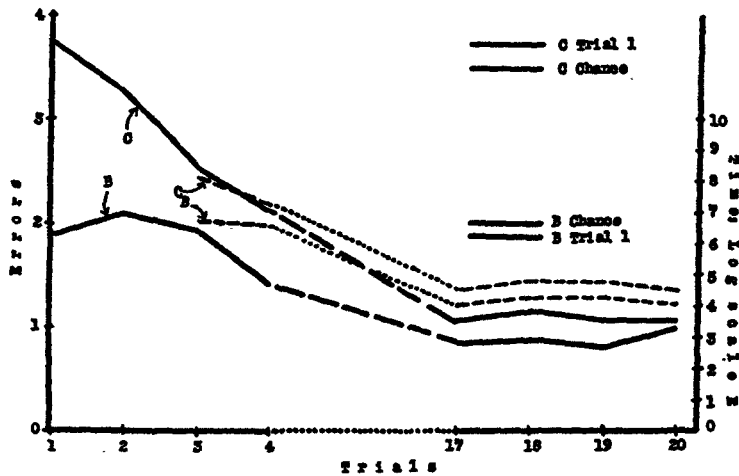day, with the full pattern maze.   On the evening of the 19th day,

Fig. 4. Learning curves of 76 (F_{5}) (3/18/31) rats on section B (blinds 7-10)
and C (blinds 11-17); full maze pattern, trials 1-19; short-cut, trial 20. Solid
line, errors; dashed line, time.

the maze was arranged for the short-cut.   No other features were
altered other than those above mentioned.   On the 20th day,
they were run on the regular time schedule.

Effect on mean performance.   The performance is analysed for
section B (blinds 7-10) and section C (blinds 11-17).   The error
and time learning curves for these two sections are shown in
figure 4.   Mean errors and mean time are shown for the first four
trials and trials 17, 18, and 19.   The curves show that the rats
had plateaued in efficiency on these last trials on the full pattern
maze.   On the 20th short-cut trial when the kinesthetic sequence

was altered, there the curves show no significant loss of efficiency. Even in section B, just following the short-cut, time did not increase and errors show a negligible increase; in section C no loss in time or errors appears. Errors did not increase to chance, or to initial errors,[4] or to the magnitudes of 4 or 7 errors expected if kinesthetic sequences appropriate to the deleted sections were persisted in. We conclude, therefore, that the rats knew precisely where they were in the maze after the short-cut, and that this knowledge was not elicited by the specific sequence of muscular movements made in the traversing of the maze.

Effect on individual differences. Effect on the most skilful performers. As in the previous experiment, we are especially interested in those rats who, before the test trial, ran perfectly and at high speed. What effect does the disruption of the kinesthetic sequence have upon them. Table 10 gives the numbers of rats who made no errors in trials before the 20th and on the test trial itself and who ran at high speed. In section B, consisting of blinds 7 to 10, the table shows that on the 20th trial 36 rats made no errors in these blinds which occur just beyond the short-cut. Quite obviously these rats appeared to be unaffected by the kinesthetic variation. And it is to be noted that on trials 19, 18, and 17

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[4] As in all of our analyses, it is necessary to compare any increase in errors on the test trial with performance on the first trial. This comparison is a bit academic in this analysis since no increase is evident. For completeness, however, I have presented in the curves this first trial record. Unfortunately, I did not have the first trial record for this F_3 group, since after the F_2 this first trial performance was not recorded. The mean errors on the first trial was estimated by the following method, which will here be described, since, though not of great moment in the present analysis, it is employed in the treatment of the results of another experiment reported later. The total error score of these 76 rats for trials 2 to 19 were distributed in a frequency table with a class interval of 10 errors. From the P, F_1, and F_2 groups, on whom there *was* a first trial record, 76 animals were selected who showed the same distribution of total errors on trials 2 to 19 as the 76 F_3 rats employed in this experiment. Because of the large number of animals available from the P, F_1, and F_2 the matching was made nearly perfectly. Then the mean errors on the first trial of the matched group was calculated, and this value considered to be closely similar to that of our F_3 group, had their errors actually been recorded. In figure 4 the mean errors shown on trials 1 and 2 are those of the matched group; errors on all other trials are those of the present experimental group. Time scores were not secured for some of the F_3 rats on trials 1 and 2, and hence the mean time for the group was not calculated.

when the maze was full-patterned not many more than this number ran perfectly. In section C, consisting of the last seven blinds, 41 rats ran perfectly on the short-cut trial, and on no previous intact-maze trial were there more errorless rats than this. That these numbers do not represent chance or the number found when rats run in ignorance, as in the matched sample on the first trial, is evident when one compares them with those in the table under "chance" and under the first trial. The number of high speed performers is not reduced by the short-cut experience as

TABLE 10

In a group of 76 (3/18/31) rats, the numbers per trial who made no errors, and who ran at high speed in sections B and C during full maze pattern trials 1 to 19 and short-cut trial 20

SECTION	CHANCE	TRIALS								
		1*	2†	3	4	···	17†	18	19	20
Number of rats who made zero errors										
B (7–10)	4.8	6	3	6	18	—	40	39	41	36
C (11–17)	.6	0	1	4	10	—	40	41	37	41
Number of rats who ran at the speed of about 1.5 feet per second or less										
B (7–10)		No record	1	7	—	29	24	25	28	
C (11–17)		No record	1	8	—	30	35	31	38	

* Matched group.
† Time records for two animals not secured.

the table shows. In section B 28 rats kept right on going without any measurable hesitancy, this number being exceeded only in trial 17. In section C more rats ran at high speed than on any previous intact-maze trials.

Correlation between trials with full maze pattern and the trial involving kinesthetic variation. If the correlation between the trial where the maze path sequence is so disrupted as to preclude the use of muscle cues as directive stimuli and the trials in which such cues are available is very high, then the capacity to use such cues is not the cause of individual differences in maze performance at the plateau. Table 11 shows the coefficients for time and for

errors.   On trials 18, 19, and 20, errors and time were summed for each rat only on units 7 to 17, i.e., after the short-cut.   The correlation between trial 19 and trial 20 was .820 for errors and .884 for time.   We lack reliability coefficients for trial 20, so corrections for attenuation cannot be made.   But it is to be noted that the correlations between the two trials 18 and 19 *both on the full pattern maze* are .885 for errors, .895 for time, and thus are not significantly higher than those between the disrupted and intact trials.   Hence the latter correlations are evidently about as high as the unreliability of the scores will permit.

TABLE 11

Intercorrelations between errors and between time on full maze pattern trials 2-16, 18, 19 and short cut trial 20 for 76 F_1 (3/18/31) rats

	(a) ERRORS				(b) TIME†			
	2-16	18	19	20	2-16	18	19	20
M	82±5	2.04±.27	1.89±.24	2.04±.28	10.2±.4	6.7±.5	6.9±.5	6.4±.4
SD	46±4	2.37±.19	2.07±.17	2.42±.20	3.8±.3	4.4±.4	4.5±.4	3.8±.3
18	.857±.030				.864±.029			
19	.881±.025	.885±.024			.831±.035	.895±.023		
20	.809±.039	.787±.043	.820±.037		.844±.033	.858±.030	.884±.025	

* Trials 2-16 included performances on all blinds 1 to 17; trials 18, 19, 20 on blinds 7 to 17 only.

† Log scale C used for trials 2-16; log scale D for trials 18, 19, 20.

Another interesting fact concerns the relation of individual differences on the kinesthetically disrupted trial with those occurring on the earlier trials in the learning of the intact maze.   Table 11 presents the correlations between total errors and between total time in all 17 units of trials 2 to 16 and trials 18, 19 and 20.   Between trial 20 and this earlier period the values are .809 for errors and .844 for time.   Trials 18 and 19 show values not much higher than these.   Correction for unreliability would doubtless push all these values over .90.

This experiment appears definitely to establish the fact that maze efficiency in the plateau period is not controlled by proprioceptive sensory components.   Furthermore, the correlational evidence mentioned in the preceding paragraph seems to indicate

that kinesthetic stimulation *per se* is not the important *differential* even in earlier stages of learning.

Evidence as to the effect on performance of variation of cues in all sense modalities

From the foregoing experiments we have concluded that sensory components in each of the modalities do not operate alone as determiners of differences in the plateau performance. This conclusion has not excluded the possibility of components from several modalities operating in combination. It might be supposed that the bright rat, say, uses visual, olfactory, tactual, auditory, and proprioceptive cues to guide him, and that when one or more, but not all, of these types of cues are obliterated or disarranged, he falls back on those remaining. Such a supposition, which we may term *the doctrine of multiple sensory components*, appears untenable in two respects. The first is that one would expect every rat to show *some* disruption of performance by virtue of his having suddenly to respond in the test trial to a reduced cue matrix. But our previous results have proven that many animals were undisturbed by change of stimuli. The second is that the supposition assumes, in view of our findings of high correlation between plateau and test trial, that the sensory components are not independent factors but are highly correlated: the bright rat must be superior in all components, the average rat must have an average capacity in all, and the dull must be inferior throughout. This assumption follows necessarily from the fact that when by change of stimulus we have excluded the operation of each type of component, the rats have nevertheless not changed their relative position in the group. They must be equally facile, therefore, in the components remaining in the test trial and in the components excluded. If one holds for the operation of multiple sensory components, one necessarily espouses the theory of a high inter-r between them—a theory which is not supported by experimental data on rats and human beings that seems to prove the high specificity of the various acuities and sensory sign learnings (13). These criticisms of the multiple sensory component

doctrine, however cogent, are theoretical. We need a definite investigation of the validity of this doctrine. Our remaining experiments were framed to satisfy this need. The procedure was obviously that of disrupting as effectually as possible *all* relevant stimuli on a test trial.

Experiment 1

The sample of 67 (5/22/31) F_s rats was exposed to rather extensive stimulus changes on their 20th trial. Olfactory and tactual cues were disarranged by the interchange of all curtains on trial 17 and all choice points on trials 18 and 19. On trial 20 the following interchanges of blinds (B), true paths (T), stems (S), and corners (C) were made: B6 and B7, T8 and B4, T5 and T9, B10 and B5 and B16, S11 and S4, C4 and C17, C10 and C13. Serial proprioceptive cues were broken by short-cutting the maze as in the preceding experiment.

Auditory cues occasioned by reverberations from the walls of sounds made by the rat's manipulation of the floors and doors were muffled and displaced by the stretching of a black flannel cloth over the top of the maze. This cloth covered units 1, 2, 7, 8, 9, 13, 14, 15, and 17.[5]

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[5] I am indebted to Dr. F. Henry for an investigation by him of the determination of the intensity difference of noises in the open alley and in the alley covered by the double blanket. Determinations were made just at unit 1, at the choice point in unit 6, and at the choice point in unit 10. A crystal microphone connected with an amplifier and output meter which registered zero in a soundproof room was placed in the units and the readings made with the unit open and with it covered. For general room noises the decibel difference between the open and covered unit for the three positions was respectively 1.4, 1.2, and 1.6. Differences of this magnitude approximate a just noticeable difference in human beings. The mean intensity of room noises in the blanketted alley was approximately 18 decibels above the average human threshold for 1,000 cycles. Room noises were therefore very weak. Of greater moment, however, would be a significant difference of noise intensities between the various sections of the blanketted maze. Were these appreciably different, they might provide significant location cues to the rat. The determination of noise intensity difference between unit 1 and unit 6 was found to be 1.0 decibels, between unit 1 and unit 10 it was 1.2 decibels, and between unit 6 and unit 10 it was 2.1 decibels. Such differences are just discernible by human beings, but probably are not by rats (1). We may safely conclude that intensity differences did not provide differential cues to the rats.

To eliminate visual cues, the lights were turned off, as in the earlier experiment. The present experiment was conducted in a new laboratory where several large windows made complete exclusion of light difficult. Though the shades were drawn and black flannel placed over the windows, some leakage of light occurred. As the maze was enclosed on three sides by opaque walls and the flannel placed over it, the units appeared, however, to be pitch black.
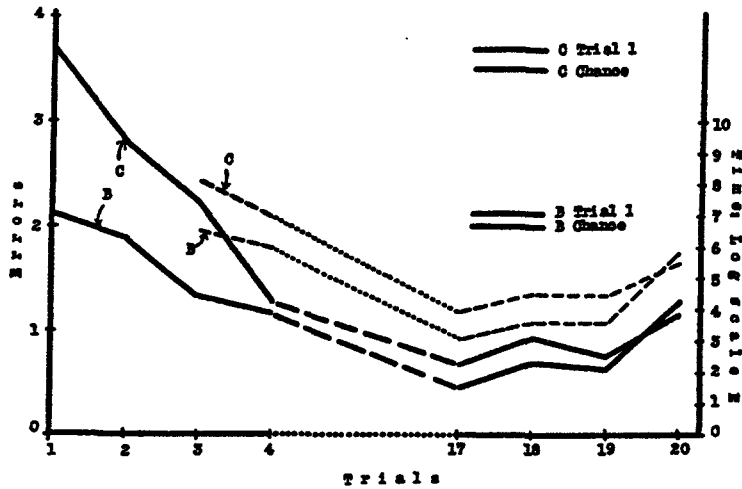

FIG. 5. Learning curves of 67 (errors) and 66 (time) F$_2$ (5/22/31) rats on section B (blinds 7-10) and section C (blinds 11-17); full maze pattern, trials 1-19; shortcut and other changes, trial 20. Solid line, errors; dashed line, time.

Effect on mean performance. The error and time learning curves for sections B and C are shown in figure 5. The error data on trials 1 and 2 were secured, as in the preceding experiment, from a matched sample drawn from the P, F$_1$, and F$_2$ groups on which recordings for these trials had been taken. In section B a rise from about .6 errors in the plateau to 1.3 errors on trial 20 may be noted. This latter value may be compared with 2.1 errors on the first trial and to the chance score of 2 errors. A rise in time is also indicated, but unfortunately no record on the first or second trials was obtained with which to compare this rise. Whether or not this loss of efficiency is attributable to withdrawal of guiding

cues or to distraction occasioned by the sudden introduction into the radically changed maze is not evident. The distraction theory is rather supported by the performance in the last seven units. The section C curves show an insignificant relative loss of efficiency both for errors and for time on the 20th trial when compared to plateau, first trial, or chance performance. By the time the rats reached this section, it would appear that distractive effects had disappeared. Were the performance controlled by cues, it would seem that section C would have suffered to a similar relative degree as B.

TABLE 12

In a group of 67 (errors) and 66 (time) F_1 (5/22/31) rats, the numbers per trial who made no errors and who ran at high speed in sections B and C during full maze pattern trials 1–19 and on the 20th test (short-cut, etc.) trial

SECTION	CHANCE	TRIALS								
		1*	2*	3	4	⋯	17	18	19	20
Number of rats who made zero errors										
B (7–10)	4.2	4	6	19	22	—	48	39	44	15
C (11–17)	.5	1	4	6	28	—	46	38	45	27
Number of rats who ran at speed of 1.5 feet per second or better										
B (7–10)		No record	2	5	—	40	27	27	2
C (11–17)		No record	0	4	—	39	27	26	13

* Matched group.

Effect on individual differences. Effect on the most skilful performers. In table 12 it will be observed that in section B, whereas about 40 rats ran perfectly during the plateau trials 17, 18, and 19, only 15 showed a perfect record on the 20th trial. But this number is more than three times as many as would have done so by chance or who did so in the matched sample on trial 1. Only two rats met the high speed criterion, it being evident that the stimulus changes slowed up even the most speedy rats. In section C, however, 27 rats proceeded without errors, and 13 at high speed. It would appear that the postulation of the determination of the performance of these skilful animals by multiple sensory compo-

nents is untenable provided, of course, that we satisfactorily experimentally disrupted all relevant sensory cues.

Correlation between plateau trials involving no stimulus changes and test trials involving multiple stimulus variation. The intercorrelations between trials 18, 19, 20 and total on trials 2 to 16 are given in table 13. For errors the correlation between trial 20 and 19 is .705. Though this value is not unity, indicating that some changes in relative performance occurred on the 19th and 20th trials, all the displacement cannot be put down to the effects of stimulus change. It must be noted that between trials 18 and 19, during which no stimulus changes occurred, the r is only

TABLE 13

Intercorrelations between errors and between time on full maze pattern trials 2-16, 18, 19 and 20th test (short cut etc.) trial for 67 (errors) and 66 (time) F_1 (5/22/31) rats*

	(a) errors				(b) time†			
	2-16	18	19	20	2-16	18	19	20
M	62±5	1.63±.26	1.37±.25	2.48±.25	9.3±.5	6.0±.4	5.6±.4	8.8±.4
SD	44±4	2.16±.19	2.09±.18	2.01±.17	3.9±.3	3.5±.3	3.6±.3	3.5±.3
18	.855±.032				.828±.038			
19	.806±.043	.795±.044			.872±.029	.805±.043		
20	.687±.064	.670±.067	.705±.061		.752±.053	.695±.063	.817±.040	

* Trials 2-16 included performance on all blinds 1 to 17; trials 18, 19, 20 on blinds 7 to 17 only.

† Log scale C used for trials 2-16; log scale D for trials 18, 19, 20.

.795. If we take the magnitudes at face value, stimulus changes account only for a drop from .795 of .09 in correlation. A comparison for time of the analogous coefficients, .817 as against .805, shows no effects of stimulus change. The comparison of the correlation between trials 18, 19, 20 and the earlier stages of learning, trials 2 to 16, has interest in shedding light on the community of components on these segments of performance. Though trial 20 shares less communality than trials 18 and 19, both for errors and for time, the values remain fairly similar in view of their standard errors. In general, this evidence indicates some disruption of performance arising from the gross changes of multiple stimuli, but the writer believes it can be fairly said

that the disruption is relatively small. The data seem not to lend much support to the doctrine of multiple sensory components.

Experiment 2

The above experiment appeared in principle to be so crucial that it was decided to repeat it and to introduce certain improvements. In this second experiment (a) *all* the stimulus changes were presented on the 20th trial, (b) better disarrangements and obliterations of cues were effected, (c) complete recordings of errors and time were secured on trials 1 and 2, important as criteria of ignorance, and (d) the animals were given three trials after the 19th under changed stimulus conditions.

The purpose of running the rats on the three test trials 20, 21, and 22 was three-fold. First, if the amount of disruption of performance on trial 20 were due to distraction and not to loss of guiding cues, as the evidence on the progressive diminution of disruption from section B to C in the preceding experiment seemed to indicate, then one might anticipate that on the second and third trials after the 20th, the mean performance would return to plateau efficiency, a result not so plausible on the theory of sensory components. Second, the possession of scores on several trials under changed stimulus conditions would provide a means of ascertaining the unreliability of these scores, i.e., of estimating reliability coefficients of these trials. Third, we wished to secure scores on several test trials with which to compare the initial learning of a maze-wise control troup (described in detail below) which ran this maze for the first time under the same stimulus conditions as the experimental group. As the maze, when short-cut, consists only of 13 units, having a rather simple configuration, a quite valid question may be raised: May not the performance of the experimental group on trial 20 merely be that of maze-wise rats running this simple 13-unit maze for the first time? It might be argued that the first trial of the experimental group on sections B and C does not provide a criterion of ignorance, as we have been wont to treat it, for the presence of blinds 3, 4, 5, and 6 may have increased the error score in the end sections of the maze.

For the new experiment, 76 F₄ rats, termed the 11/1/31 group, were subjects. On the 20th trial olfactory and tactual cues were disarranged by interchanging (a) the curtains in the true path with the curtains in the blinds in the following units: 6, 7, 8, 9, 10, 12, 14, 15, (b) the following choice points: 5 with 14, 8 with 15, 7 with 9, and 10 with 12, (c) the following sections: B6 with B7, T8 with B4, T5 with T9, B10 with B5, B16 with B17, S11 with S4, S5 with S8, C10 with C13, and C4 with C17. Vision was obliterated by turning off the lights and pulling down the window
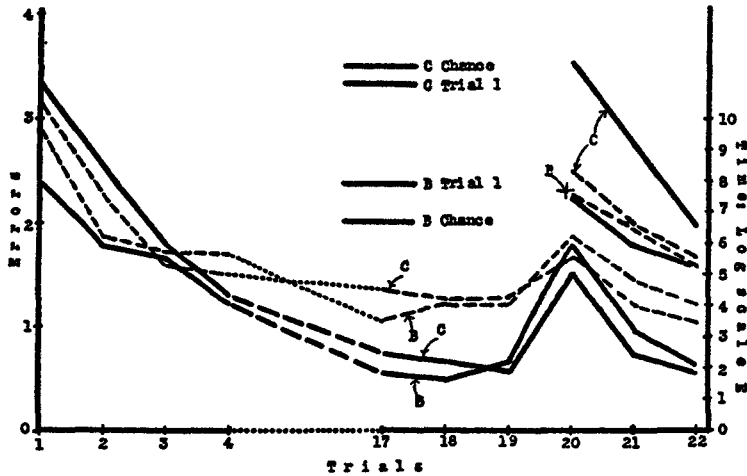

Fig. 6. Learning curves of 76 (errors) and 75 (time) F₄ (11/1/31) rats on section B (blinds 7-10) and section C (blinds 11-17); full maze pattern, trials 1-19; short-cut and other changes, trials 20, 21, 22. The four upper curves beginning trial 20 are of a control group on its first three trials (see text). Solid line, errors; dashed line, time.

shades and covering each window with black flannel cloth, the edges of which were pasted to the sills to prevent leakage of light.

The whole maze top was completely covered with *two* layers of black flannel, this feature not only insuring all elimination of light from the maze, but dislocating auditory cues as well as modifying the temperature and draft conditions in the units. Though a partition separated the maze from the electric recording device, it was thought desirable to eliminate the subdued hum from it by placing it in a sound-proof box. Serial proprioceptive cues were broken by short-cutting the maze as in the preceding experiments.

Effect on mean performance. The error and time learning curves are shown in figure 6. In section B, during plateau trials 17, 18, 19, the mean errors are of the order .5, which on trial 20 increased to 1.8, a rather serious loss, approaching chance and first trial score. But it is to be noted that on trial 21, the rats returned nearly to plateau skill, and did so exactly on trial 22. The performance may be compared with the first three trials of 31 maze-wise control rats (described in detail later) whose section B curve is shown in figure 6 plotted above trial 20, 21 and 22, and whose curve is very like that of the experimental group on *its* first three trials. The disruption in section B on trial 20, con-

TABLE 14

In a group of 76 (errors) and 75 (time) F₆ (11/1/31) rats, the number per trial who made no errors and who ran at high speed in sections B and C during full maze pattern trials 1–19 and on the 20th, 21st, and 22nd trials (short-cut, etc.)

SECTION	CHANCE	TRIALS										
		1	2	3*	4*	...	17	18	19	20	21	22
Number of rats who made zero errors												
B (7–10)	4.8	1	11	12	21	—	45	51	41	12	32	44
C (11–17)	.7	2	2	11	26	—	45	50	51	24	44	48
Number of rats who ran at speed of 1.5 feet per second or better												
B (7–10)		0	4	4	5	—	32	30	22	0	4	10
C (11–17)		0	0	15	22	—	25	36	38	8	40	50

* No time record for one rat.

sidered in view of the rapid return to plateau efficiency on the next two trials may reasonably be put down to distraction. Most illuminating is the behavior in section C, where on trial 20, the disruption in absolute magnitude is not as great as in section B, even though C has more blinds, but relative to the first trial record both of the control group and of the experimental group itself the loss is small. The effects of disruption have nearly disappeared by the time the end section of the maze is reached. On trial 21 the efficiency in absolute errors is nearly that of plateau, and certainly is so on trial 22. The performance as measured by time gives results throughout analogous to those of errors. Our

general interpretation of these results is that, in view of the brief
distractive effects of extensive stimulus changes, the rats are not
guided in the plateau by multiple stimulus features of the maze.
Furthermore, in view of the initial learning performance of the
control group, the rapid return to plateau efficiency of the experi-
mental group on trials 21 and 22 is not to be interpreted as the
initial learning of a simpler 13-unit maze.

*Effect on individual difference. Effect on the most skilful per-
formers.* In table 14, the number of rats making perfect runs in
section B on trial 20 is 12. If the data are taken at face value,
more than twice as many ran errorlessly on trial 20 than were to
be expected by chance, and 12 times as many did so as on the first
trial. In comparing these data with the findings on the control
group, we must determine the per cent of perfect performers in
the groups, since the total N's differ. In the experimental group
on trial 20, 12/76, or 16 per cent ran perfectly; in the control, on
trials 1, 2, and 3, the per cents (not tabled) were respectively 0.
.7, and 1. In section C, 24 rats ran errorlessly, 34 times the chance
number, and 12 times the first trial frequency. In comparison
with controls, whereas the experimental group gave 31 per cent,
the controls gave on trials 1, 2, and 3, respectively 0, 0, and .7.
On trial 21 the number of perfect performers doubled in section B
and reached nearly plateau frequency in C, and on trial 22 both
sections gave plateau frequencies. Reduction of the number of
most speedy runners is striking, especially in section B, where
even on the 22nd trial only 10 rats, or 13 per cent, met the high
speed criterion as compared to nearly 30 in the plateau trials. In
the control group, none met the criterion on trials 1 and 2, and
6 per cent met it on the third trial. But in section C more met
the criterion on trial 21 than during plateau, and on trial 22
appreciably more. In the control group a total of only 6 per cent
met the criterion during the first three trials. We interpret these
data as indicating that the marked stimulus changes were noticed
by the rats and caused even the most speedy rats to proceed with
some caution in section B, this hesitancy being lost by the time
section C was reached. But such hesitancy did not result in loss
of knowledge of the true path in significant proportions of the

TABLE 15

Intercorrelations between errors and between time on trials preceding and on those involving marked changes in stimuli on section B + C (blinds 7-17) for 76 (errors) and 75 (time) F₉ (11/1/31) rats

(1) Intercorrelations between trials*

	ERRORS (N = 76)						TIME (N = 75) LOG SCALE D					
	17	18	19	20	21	22	17	18	19	20	21	22
M	1.26±.18	1.14±.20	1.20±.18	3.33±.25	1.71±.20	1.14±.16	5.8 ±.4	6.0 ±.5	6.1 ±.4	9.4 ±.3	6.5 ±.4	5.4±.4
SD	1.60±.13	1.71±.14	1.55±.13	2.16±.18	1.77±.14	1.38±.11	3.5 ±.3	3.9 ±.3	3.5 ±.3	2.7 ±.2	3.6 ±.3	3.2±.3
18	.68±.06						.68±.06	.61±.07				
19	.64±.07	.69±.06					.61±.07	.61±.07				
20	.40±.10	.35±.10	.56±.08				.54±.08	.54±.08	.54±.08			
21	.42±.09	.59±.08	.57±.08	.56±.08			.50±.09	.67±.06	.72±.05	.72±.06		
22	.58±.08	.60±.07	.64±.07	.60±.07	.65±.07		.71±.06	.68±.06	.74±.05	.60±.07	.79±.04	

(2) Reliability coefficients

	ERRORS	TIME
Stage (17 + 18 + 19):		
Single trial	.67	.63
Three trials	.86	.84
Stage (20 + 21 + 22):		
Single trial	.60	.70
Three trials	.82	.88
Stage (21 + 22):		
Single trial	.65	.79
Two trials	.78	.88
Trial 22	.65	.79

(3) Correlation between stages†

	RAW		TRUE	
	Errors	Time	Errors	Time
(17 + 18 + 19) and (20 + 21 + 22)	.66	.81	.78	.95
(17 + 18 + 19) and (21 + 22)	.69	.81	.85	.94
(17 + 18 + 19) and (22)	.69	.81	.95	1.00

* All r's were calculated to 3 places; tables show them rounded to 2 places.
† Coefficients calculated by formula for sums, trials weighted equally.

animals. In these rats, the performance was evidently not determined by multiple sensory components.

Correlation between plateau trials involving no stimulus changes and trials involving multiple stimulus variation. Table 15, sub-table (1), presents for errors and for time all the intercorrelations between the three plateau trials preceding stimulus change and the three test trials.

For errors, the mean r between the plateau trials 17, 18, 19 is .67, a value which we term the single plateau trial reliability coefficient in sub-table (2). This coefficient gives an index of the inter-trial constancy of individual differences in performance *within* the plateau period of no stimulus change. If correlations *between* trials in the test period and the plateau period approach this magnitude, then stimulus changes cannot be said to have affected individual differences in the test period, and *pari passus* response to stimulus features could not have determined relative efficiency in the plateau period. The mean inter-r between trial 20 and 17, 18, 19 (i.e., mean of .40, .35, .56) is .44, somewhat lower than .67. But trial 21 gives a mean inter-r of .53, and trial 22, of .61. Thus the third trial after multiple-stimulus change gives a value with plateau trials very like any plateau trial itself. A similar analysis of time scores shows the mean plateau trial inter-r to be .63, whereas the mean inter-r between trial 20 and the plateau trials is .54, for trial 21 it is .63, and for 22, .71, this last value being higher than that for *any* plateau trial.

A more illuminating analysis involves combining the scores on the plateau trials into one composite score comprising a more reliable index of individual differences in plateau efficiency. Using the mean inter-r as an index of unit trial reliability, and applying the Spearman-Brown formula, setting n equal to 3, the reliability coefficient of this composite score is, for errors, .86, for time, .84 (sub-table 2). The composite score of the three test trials has analogous reliabilities of .82 for errors and .88 for time. The raw correlation *between* these two composites is given in sub-table (3), namely for errors, .66, and for time, .81, and the correlation plots are given in table 16. Correcting for attenuation, we find the true r between the plateau and test stages to be, for

TABLE 16

Correlations between errors (a) and between time (b) on trials (17 + 18 + 19) preceding and trials (20 + 21 + 22) involving marked changes in stimuli on sections B + C (blinds 7–17) for 76 (errors) and 75 (time) F₈ (11/1/31) rats

(a) Errors

Trials (17 + 18 + 19) [rows] vs Trials (20 + 21 + 22) [columns]

Trials (17+18+19)	0	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20	21
21																						
20																						
19																						
18																						
17										1												
16								1														
15																						
14																						
13												1		1	1							
12											1					1						
11									1			1										
10																						1
9											2											
8																						
7											1		1									
6			1		1						2		1									
5				1	1	1					1											
4	1		1		1	1			1		1											
3		1			3		1				1											
2		3	1		1	3										1						
1	1	1			3	1		1														
0	1	5	2	2	2	3	4	5		1												

Trials (20 + 21 + 22)

(b) Time*: (log scale D)

Trials (17 + 18 + 19) [rows] vs Trials (20 + 21 + 22) [columns]

Trials (17+18+19)	0–2	–5	–8	–11	–14	–17	–20	–23	–26	–29	–32	–35	–38	–41	–44	–47	–50
–50																	
–47															1		
–44																	
–41													1				
–38											1						1
–35										1	1	2					
–32										1	2	1	2				
–29										1							
–26										1		2					
–23						1		1	1	1		1					
–20			1	1	1	2	1	1		1							
–17				3	3	1	2	2			1						
–14			1		5	2		1									
–11			1	4	3	6											
–8			1	2	3												
–5		1	1		1												
0–2																	

Trials (20 + 21 + 22)

* Sum of logs of three trials.

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errors .78, for time, .95.   It is evident that some specific non-chance factors appear in errors but are negligible in time.   It has been postulated that these non-chance factors derive from distractive effects on trial 20.   If this be so, then deleting trial 20 from the test trial composite should show a higher error inter-stage r.   In sub-table (3) this raw r for errors is .69, for time, .81; the true r's respectively are .85 and .94.   The non-chance specifics are thus partly removed from the test trial composite error score.   Finally, the true r between plateau composite and trial 22 is, for errors, .95, and for time, 1.00.   Thus by trial 22 the specific non-chance factors have virtually vanished, leaving the true variance among individuals essentially identical with that present during the plateau trials.   Since this true variance on trial 22 appears under circumstances of marked stimulus obliteration and recent stimulus variation, it may reasonably not be described psychologically as occasioned by multiple sensory components.   The specific non-chance variance which so rapidly disappears from the test trials may be ascribed to susceptibility to distraction.

The control group

We ran a maze-wise control group in order to check whether the initial level and rapid return to plateau efficiency of the experimental group on trials 20, 21, and 22 could be interpreted as the performance of maze-wise rats starting in ignorance of the specific maze plan and running an initial three trials on the 13-blind maze. The results in figure 6 show that by trial 22 the experimental rats far surpassed the controls.   This difference is significant, for the mean total error score of the controls on their first three trials was 13.80, that of the experimental animals was 6.18 on their three test trials.   The difference, 7.72, is 8.4 times its standard error (.92).   The difference in mean time score is 5.8 times its standard error.

One may make the legitimate query: How should one train the control group to be as inured to this type of maze and its mechanical features as was the experimental group at the end of its 19th trial?   The obvious procedure is to chose a comparable sample of rats and to run it 19 trials on a 17-blind maze of equal difficulty to

the present maze, and under the same conditions of procedure as obtained with the experimental group. On the 20th trial this control group would then be presented with the 13-blind test trial maze. The objection to this type of control is that on the 20th trial the control rats would doubtless show marked interference effects of the original learned maze plan. We needed as a control, rats who were completely inured to the maze and its procedure, who had experienced considerable learning in the types of maze units employed, but whose preliminary learning of a maze path was of such character as would not be expected to interfere with initial performance in the 13-blind maze, and who on their first trial would know by unit 7 (where our scoring begins) that they were *in* a new maze. Such a control is difficult to arrange. We attempted to meet these demands by (1) choosing a group of rats who had experienced many trials in some other learning problems and who were therefore highly sophisticated in laboratory handling, (2) running them 19 days under our laboratory experimental procedure. For the first nine of these days they ran through the regular preliminary practice path adjacent to the entrance of the maze proper, a schedule which attempts to familiarize the rat with the mechanical features of the recording maze and its revolving delivery table. On days 10 to 17, they learned four simple two-blind mazes made up out of units actually to be part of the 13-blind maze. These four mazes were placed on the floor beside the recording maze which itself is on an elevated platform. On days 18 and 19 they returned to several trials on the preliminary practice path, then on the 10th day they entered the 13-blind maze for their first trial. By the 20th day they had experienced considerable learning in the recording type of unit, and on the 20th trial as they entered the 13-blind maze as it debouches off the practice path they would reasonably be expected to know at least by unit 7 that they were not in one of the two-blind mazes but in a maze of new plan. Our purpose of giving the *four* preliminary mazes to learn was to adjust them to the presentation suddenly of a new maze. We assume that the previous training of the rats was adequate and that their performance in section B and C was that of a maze-wise group not subject to interference by the learning of the four two-blind mazes.

The control group consisted of 31 rats obtained from other experimenters in the psychological laboratory. All the animals were of ages within the range found not to correlate with this maze ability (11). From C. Honzik and I. Kreshevsky, four groups, whose previous training by these experimenters is given in table 17, were secured. Rats of group A had experienced 753 trials, of B 153 trials, of C 385 trials, and of D 168 trials on the problems shown in the table. Twenty-three of the rats were males drawn from the general colony and were presumably a

TABLE 17

Data on previous experimental training of 31 (12/20/31) rats

GROUP	N	EXPERIMENTER	PROBLEM	TOTAL TRIALS	TRIALS PER DAY	TOTAL DAYS	DATE OF FINISH	DAYS TO 1ST MAZE TRIAL
A	9	Honzik	1. Pattern discrimination	414	10*	40		
			2. Insight	96	8	12		
			3. Pattern discrimination	243	9*	29		
Total...................................				753	27*	81	11/13/31	37
B	4	Honzik	Pattern discrimination	153	9*	17	11/13/31	37
C	8	Honzik and Kreshevsky	Distance discrimination	385	12*	33–43	10/24/31	57
D	10	Kreshevsky	Light-dark discrimination	168	12*	14	11/22/31	28

* Number approximate.

random selection. Six were females derived from the writer's dull strain, and two were females from the bright.

The specific procedure followed during the 19 days is summarized in table 18. The parts of the training labelled "Preliminary" refer to the trials on the preliminary practice path described in detail in a previous paper (8). The four two-blind maze patterns which the rats learned on days 10 to 17 are shown in figure 7. As the figure shows, the establishment of any directional or sequence fixation which might have transferred to the longer maze was avoided by starting the runs from four different geographical

TABLE 18

Experimental procedure for 31 (12/20/31) rats

day		day	
1	Placed in upper compartment of revolving table	12	Pattern 1, four trials
2	Preliminary: trials 1 and 2, day 2	13	Pattern 2, four trials
3	Preliminary: trials 1 and 2, day 3	14	Pattern 2, two trials then pattern 3, two trials
4	Rest	15	Pattern 3, four trials
5	Preliminary: trial 2, day 4; trial 1, day 5	16	Pattern 4, four trials
6	Preliminary: trial 2, day 5	17	Pattern 4, five trials
7	Preliminary: day 7	18	Preliminary: day 8, two trials
8	Preliminary: day 8	19	Preliminary: day 8, one trial (note: after the rats had finished the run, 7 F_4 rats who had 19 trials on the maze ran through it "to smell it up" for the next day's run of the control group)
9	Preliminary: day 8		
10	Pattern 1, (a) half hour in food box—1 teaspoon food; (b) one run into food box from choice point of unit 2; (c) two trials on full pattern	20, 21, etc.	One trial on the maze per day
11	Pattern 1, three trials		

Note on feeding: regular preliminary rations given when rats ran on preliminary trials. When running patterns, rats ate to satiety on last trial of day, and were given several nibbles on trials preceding this.
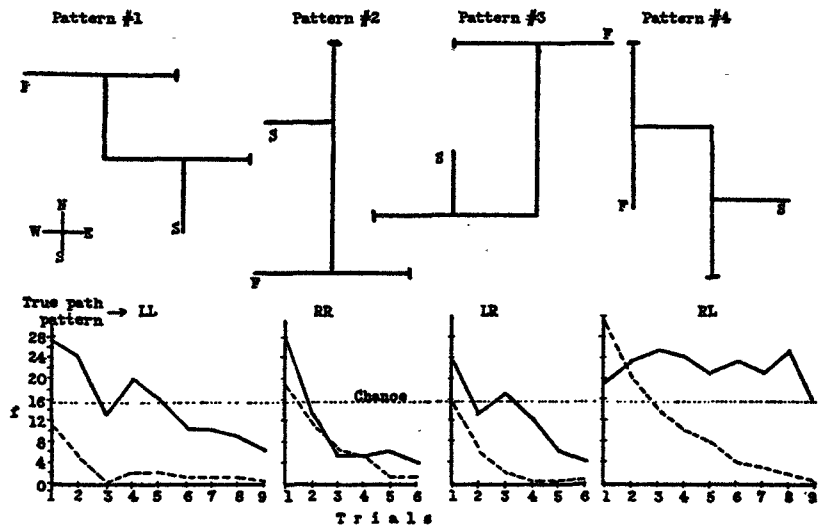
FIG. 7. Pattern of four 2-blind mazes, and learning curves (errors) per blind of 31 (12/20/31) rats. Abscissae are trials; ordinates (f) are number of rats making an error. Solid line, blind 1; dashed line, blind 2.

directions, and arranging the true path turns in the four possible combinations of left and right taken two at a time.

The learning curves of blind 1 and blind 2 of each of the patterns are shown in figure 7 below their respective maze patterns. The ordinates of the curves are the number of rats making an error. It is clear that the rats learned to avoid blind 2 in all patterns and blind 1 in all except pattern 4, in which the effect of the correct turn in unit 2 made this blind difficult to avoid. We may conclude from these data that the rats were maze-wise, not only in the sense of being inured to experimental conditions but of having experienced learning in the actual recording units used in the longer test maze. The marked superiority of the experimental group in their test trials over the control group we do not therefore attribute to the former merely being maze-wise and running a simple maze. Their superiority derives from non-sensory components developed in their preceding 19 trials on the maze.

Addenda on the variability of the various experimental groups

As the correlational evidence obtained in the experiments described in some of the preceding sections was secured from the F_5 and F_6 groups of our major inheritance experiment, one might well ask whether excessive variability of these groups might not account for the high magnitudes of the coefficients evident in and between plateau and test trials. If such excessive variability existed, our conclusions could not be extended to a normal sample of the population but only to the groups actually used. It is to be recalled that the original P generation was an unselected sample of rats (10). The F_1 population, composed of progeny of bright, dull, and median breedings, showed only slight effects of selective breeding, the frequency distribution of total errors on 19 trials closely resembling that of the P group. We have therefore combined the P and F_1 groups into one major statistical sample, whose variability we have considered to be closely similar to that of a random sample drawn from the rat population. The frequency polygons of total errors and total time of 340 P and F_1 on the plateau trials 14 to 19 are given in the dashed line curves

of figure 8. It is to be recalled that one of the experiments above used a sample of these P and F_1 rats, hence the conclusions from that experiment hold for a random sample. The solid line frequency polygons of figure 8 are those of 314 F_5 and F_6 rats from the population of which the samples in the remainder of the experiments were drawn. It is to be noted that the F_5 and F_6 polygons closely resemble those of the P and F_1 group.
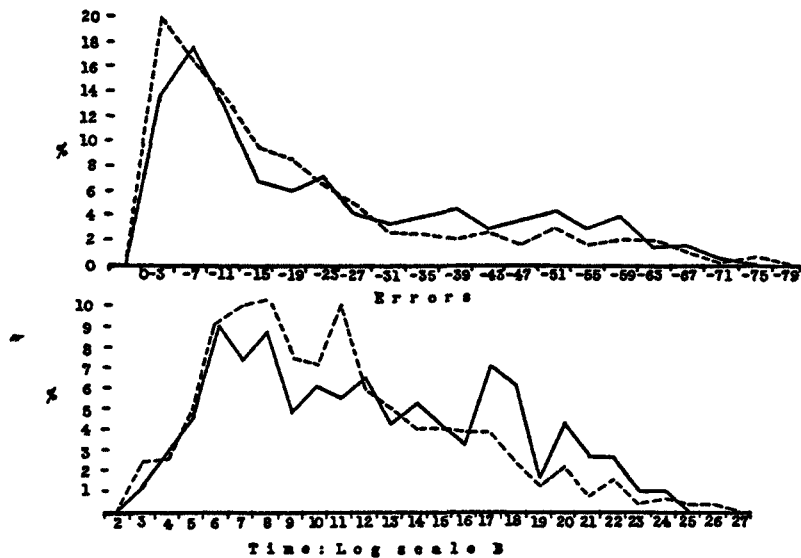

FIG. 8. FREQUENCY POLYGONS OF POPULATIONS FROM WHICH EXPERIMENTAL SAMPLES WERE DRAWN

Abscissae are total scores on plateau trials 14–19; ordinates, per cent of population. Solid line, $F_5 + F_6$; dashed line, P + F_1. N's are: errors, 314 $F_5 + F_6$, 340 P + F_1; time, 309 $F_5 + F_6$, 339 P + F_1.

Even though we know that the left end of the F_5 and F_6 polygons consists largely of rats from the bright strain, the right end, largely of rats from the dull, the variability of the group as a whole resembles closely that of a random sample.

More specification may be given the matter by comparing the various groups with respect to their sigmas. The sigma of total errors on plateau trials 14 to 19 of the P and F_1 group was 16.9, that of a carefully chosen unselected sample of 107 P rats ex-

tensively studied by the writer in a former analysis (9) was 16.4.[⁶]
The sigma of errors of the 400 F_4, F_5, F_6 group considered in
table 3 was 17.7, that of the 314 F_4 and F_5 group shown in figure 8
was 18.6. This latter group, which contributed most of our
experimental rats was only, therefore, 10 per cent more variable
in errors than the P sample. With time scores, it was 14 per
cent more variable.

The crucial question is whether this slight increase of varia-
bility in our experimental groups has radically increased the
magnitudes of their correlations. Though we cannot investigate
this matter for all types of coefficients calculated in our foregoing
analyses, we do possess data by means of which we can compare
the experimental groups with a random sample in respect of one
important type of coefficient which affects all the rest, namely,
the reliability coefficient of plateau trials. For the random
sample of 107 P rats the reliability coefficient for errors of three
plateau trials, namely $r_{(14+15+16)(17+18+19)}$ was .927 ±.014. For
the large sample of 400 F_4, F_5, F_6 rats whose correlation scatter
is given in figure 2, the coefficient is .918. In table 5 the similar
coefficient for 67 F_5 rats was .953. The latter values are not
significantly different from the coefficient of the random sample.

In view of the slightly greater variability of the population
from which our experimental groups were drawn when compared
to the random sample, one might well expect that the sundry
correlations obtained in the experimental groups would run in
general slightly higher in magnitude, but as with the coefficients
just compared, not significantly so. It appears that our con-
clusions based upon the coefficients would therefore approxi-
mately hold for a random sample.

SUMMARY AND CONCLUSIONS

The purpose of our experiments was to investigate the extent
to which individual differences among rats in final plateau effici-
ency in the learning of a 17-unit T-maze were determined by

[⁶] The time scores were not analysed in this P group; hence comparisons of
variability for time cannot be made.

sense-acuity, sign-learning, and generalized directional components. On the basis of the records of more than a thousand rats, it was established that in errors and time the animals plateaued from trials 14 to 19. The reliability coefficient of total scores for this plateau period was for errors .957, for time, .947. An analysis of the communality of the components throughout this period showed that they were identical on all these trials, there being no evidence of specific non-chance components on any trial. Genetic evidence indicated that these common components were largely hereditary in origin. A series of experiments was performed which was designed to investigate the extent to which these components could be described psychologically as *sensory* components. The method employed was to introduce after the plateau trials one or more "test trials," in which various stimulus features were either obliterated or disarranged in radical fashions. The following types of stimulus variations on the test trials were introduced, and for each type a separate experiment on approximately 70 rats was performed: (a) visual, olfactory, and tactual cues associated with choice points and curtains were disarranged; (b) visual cues were obliterated by running the rats in darkness; (c) serial proprioceptive cues were broken by "short-cutting" the rats in the maze; (d) (two experiments) sensory cues of all modalities were changed on test trials by interchanging choice points, curtains, and pathways, by turning off the lights, by "blanketting" the maze, and by short-cutting. the path. A maze-wise group ran several initial trials under conditions (d) to afford a control. On the various test trials the rats manifest slight loss of mean efficiency, a significant number showed no loss whatsoever, and the correlation coefficient between scores on the test trials and plateau trials approached unity when corrected for attenuation. Whatever loss of efficiency occurred was interpreted as due to "distraction" rather than to loss of guiding cues, for the rats rapidly returned to plateau efficiency in the end section of the maze on the test trials. The conclusion is drawn that the components determining individual differences in plateau

efficiency are non-sensory in their psychological nature.[7] This conclusion holds for a random sample, since the variability in plateau performance of the groups employed in these experiments did not greatly differ from that of a known random sample.

The above conclusion is based on the assumption that effective stimulus variation in the test trials was accomplished in our experiments. There can be no doubt that in the crucial experiments involving multiple stimulus variation, visual stimuli were obliterated, and large masses of olfactory, tactual, temperature, draft, and auditory stimuli were disarranged. Doubtless some olfactory and tactual stimuli remained undisturbed such as those associated with some of the maze parts which for mechanical reasons it was undesirable to move. As the vibrissae were not cut, stimulation of these was present, though because of the homogeneous character of the maze T-units it is difficult to see how differential guiding cues from these could control performance. If the performance had been controlled by stimulus signs, the residual fixed stimuli not changed would have served to produce some correct movement, but by the same token *the translocated stimuli would have produced confusion*. Had the performance been conditioned to *serial* order of stimuli, confusion

[7] This conclusion should be evaluated in the light of the findings of Honzik (2, 3, 4), who has performed experiments analogous to some of those reported in this paper. Honzik's animals showed marked upset when some types of changes were introduced. The writer believes that this upset may have been contingent upon the fact that Honzik's mazes were elevated. It seems quite tenable to suppose that rats, especially blind rats, may to some extent use guiding sensory cues in the elevated maze, and do not do so in the alley maze where the rats are enclosed. It would appear also to the writer that with some stimulus changes marked distractive effects and emotional upset would appear on the elevated maze because, as running is somewhat precarious, any changes in the maze would institute investigating and disruptive activities in the rat. Furthermore, Honzik's discovery of disruptive effects from stimulus changes *before* learning has plateaued is not necessarily at variance with our results. For it is quite conceivable that if the stimulus matrix, on the basis of which the rat evolves generalized directional sets, is disturbed during the process of the evolution of them, these sets would be developed with more difficulty, and would be more easily disrupted by stimulus changes than would be the case, as in our experiments, where the matrix is held constant.

would certainly have occurred, since the serial order was manifestly greatly disordered by the short-cut and by the translocations of sections. But the performance of the rats as a group on the first test trial showed relatively small disruption in the last units of the maze and on the second test trial nearly plateau performance. Even on the first test trial a third of the group made no errors in the last seven units of the maze. Such results are difficult to reconcile with the multiple stimulus control doctrine.

Our rejection of sensory components leads us directly to the espousal of the doctrine of generalized directional components. Individual differences in plateau efficiency in this maze are not determined by sensory components, but by the *native* capacity of the animal to evolve directional abstractions regarding the plan of the maze. These abstractions are developed out of sensations *derived from stimuli received from the maze during learning*. Doubtless the more abundant the stimuli and the more fixed from trial to trial *during learning*, the easier it would be for the animal to evolve directional abstractions. Rats differ in performance because of differences in native capacity to utilize the presented stimuli in the evolving of the necessary directional sets. But when a rat has developed these directional sets, they guide his movements in the maze even in the presence of radical stimulus changes—he becomes "free" of the specific stimulus features of the maze.

We need evidence as to the nature of these directional abstractions developed by the rat. In a subsequent paper, the writer will present data on the differential responses to the various units of the maze at different periods of learning. An analysis of these responses will enable us more specifically to delineate the character of these spatial directional sets.

APPENDIX

Transformation of the raw time scales into logarithmic scales. The original raw time scores, taken off the tape in units of 5 seconds each, were thrown into a frequency table, the lower limits of whose steps ascended in approximately equal logarithmic intervals. Simple arbitrary scale values then assigned the mid-

points of these steps. The arbitrary scale values 1, 2, 3, 4, ...
were assigned the steps whose lower limits, expressed in raw time
units were, respectively, for scale A: 17, 20, 24, 28, 33, 39, 46,
54, 63, 74, 87, 102, 120, 141, 166, 195, 229; for scale B: 28, 32, 35,
40, 45, 50, 56, 63, 71, 79, 89, 100, 112, 126, 141, 159, 178, 200,
224, 251, 282, 316, 355, 398; for scale C: 100, 115, 132, 151, 174,
200, 229, 263, 302, 347, 398, 457, 525, 603, 692, 794, 912, 1047,
1202; for scale D: 3, 4, 5, 6, 7, 8, 9, 10, 12, 14, 16, 18, 21, 24, 28,
32, 36, 42, 48, 55; for scale E: 1, 2, 3, 4, 5, 6, 8, 10, 13, 16, 20,
25, 32, 40, 50, 63, 79, 100, 126, 158.

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