Some Issues in the Theory of "g" (including the Law of Diminishing Returns).¹

By Prof. C. SPEARMAN, F.R.S.

THEORY OF "G."

THE following communication treats of certain points in a theory which has become known as that of Two Factors or of *g*. At the present time this theory has undergone very elaborate development. The mental testing from which it originated lay at first as a foreign intruder in the midst of general psychology. Its opponents—and these were not few—regarded it as an excrescence that should be forthwith cast out; and even its best friends wondered how the general psychology was ever going to assimilate it. But, seemingly, neither of these solutions is happening to any great extent. The mental testing has waxed larger and established itself more firmly than ever *without* much assimilation with the current general doctrines; indeed, it seems more likely, cuckoo-wise, to eject *them* from the psychological nest. In particular, the theory of *g*, which arose from the mental tests, has now managed to spread itself over the whole of the cognitive side of psychology, and not impossibly it will soon extend its scope over into the supplementary or conative side.

For the present I do not propose to depict the whole elaborate theory of *g* even in outline. Such an attempt is reserved for a work that will appear shortly. But a very few words may be allowed here to indicate its essential foundation as unwaveringly preserved from the very beginning. This consists in the theorem, that the measure of *every different ability of any person can be resolved into two factors, of which the one is always the same, but the other always independent*. Suppose, for example, that any person undergoes a mental test and obtains seventeen marks for it. The theory asserts that this can actually be divided into two parts, say eleven and six, such that (on reduction to comparable units) the eleven re-occurs for this person in every other test however widely different, whereas the six is each time replaced by some other number independently.

The establishment of this doctrine falls into three distinct phases. The first is to ascertain what are the conditions under which the measures of any ability admit of such division into two factors. We may note that this phase has often been erroneously called an assumption or hypothesis. It is really nothing of the kind, but simply a mathematical demonstration. Given the said conditions, then the divisibility into such two factors must necessarily occur, just as, given that a triangle has all its angles equal, then it must needs also have all its sides equal. The second phase is to find out where, if at all, the conditions are actually fulfilled. Again, no assumption or hypothesis of any kind is involved; the matter is nothing more than observation of fact.

Then comes the third and last phase, that of supplying the factors with some plausible interpretation. Here the most cautious procedure, and one that goes not an inch beyond what has really been proven, is simply to denote these factors by the non-committal letters *g* and *s* respectively. Any interpretation going beyond this can only, in our present state of knowledge, have a provisional value; it can serve to inspire further

investigation, by which it will assuredly suffer much modification itself. With this reservation, then, the hypothesis at present seeming most helpful and suggestive is that the *g* measures something of the nature of an "energy" derived from the whole cortex or wider area of the brain. Correspondingly, the *s*'s measure the respective efficiencies of the different parts of the brain in which this energy can be concentrated; they are, so to speak, its "engines." Whenever the mind turns from one operation to another, the energy is switched off from one engine to another, much as the power supply of a factory can be directed, at one moment to turning a wheel, at the next to heating a furnace, and then to blowing a whistle.

RECENT CONFIRMATION.

So much to serve as a general description of the doctrine. I will now bring to your notice some recent work whereby its foundations have received additional strengthening, both on the side of mathematics and on that of actual observation.

To take the former first, the earlier researches had only shown what conditions are *necessary* for the divisibility into the two factors. Later investigation has proved that these conditions are also *sufficient*. In other words, we now know, not only that under the said conditions the divisibility into two factors may possibly occur, but also that it inevitably must do so. I stress this point because some of the recent writing on the subject appears to make the contrary and mistaken statement that, even when the conditions are satisfied, still the divisibility either may or may not ensue.

As to the precise nature of these conditions, they are based upon the coefficients of correlation. Such coefficients, as is now generally understood, consist in numbers that indicate just how closely any two abilities or other characters tend to vary in proportion to one another. They are usually symbolised by the letter *r*. Thus, r_{12} would denote the degree that any ability 1 tended to vary from individual to individual proportionately to some other ability 2. Now, the conditions for the divisibility into the two factors reduce themselves to the simple equation:

$$r_{12} \cdot r_{34} - r_{13} \cdot r_{24} = 0.$$

Here the value on the left is called the tetrad-difference. When such tetrad-differences remain equal to zero throughout any set of abilities, whichever of them may be taken as 1, 2, 3, and 4 respectively, then, and then only, each of these abilities can be divided up into two factors such as we have described. Should any one ask *why* this particular equation should have the virtue of necessitating such a divisibility, I can only answer that it is but one out of all the miracles of mathematics. I never cease to be astonished at it myself. For further elucidation, reference must be made to the mathematical proof (Proc. Roy. Soc., 1923).

So far we remain in comparatively smooth waters. The chief difficulty arises when we turn to what are known as the errors of sampling. Suppose you wanted to know whether a field of potatoes was bearing a good crop. You walk about, pulling up a plant here and

¹ Presidential address delivered at Southampton on August 27 before Section J (Psychology) of the British Association.

there. This gives you some approximate knowledge, but not an exact one. For all you can tell, you may have been exceptionally lucky or unlucky in your selection. The degree of discrepancy between the average size of the potatoes actually pulled and that of the whole field is your error of sampling. Now, just the same befalls any coefficient of correlation between two abilities. You want to know how closely these two go together with people of some general class. You pick out, say, 100 of these people; but just in this 100 the correspondence between the two abilities may happen to be rather higher or lower than on an average throughout the entire class. Your coefficient of correlation will have an error of sampling; and our preceding tetrad-difference, being made up of correlational coefficients, will have one also.

Now, the latest advance in the theory of *g* consists in showing the general magnitude of the tetrad-differences that will arise from the sampling errors alone, even when the true magnitude is always zero. This value of the tetrad-differences to be expected merely from sampling was published last year (*Brit. J. Psychol.*).

Having got this theoretical value, there remains the momentous step of comparing it with the median value which is actually observed. The theory of *g* stands or falls according as these two values are or are not found to agree.

This step so fraught with fate has now been taken. To avoid all danger of personal bias, no work of my own was chosen for this crucial decision, but that of an investigator who, more than all others, had shown himself unsympathetic with the doctrine of *g*. Here is his table of correlations as published by himself:

Test	1	2	3	4	5	6	7	8	9	10	11	12	13	14
1. Completion		98	94	79	62	91	71	54	78	88	55	42	33	25
2. Hard Opposites	98		84	80	64	81	79	70	73	74	52	43	26	25
3. Memory words	94	84		62	55	82	49	56	73	71	53	40	28	21
4. Easy Opposites	79	80	62		57	52	68	53	42	56	45	29	38	48
5. A Test	62	64	55	57		55	54	73	39	51	39	59	25	22
6. Memory pass.	91	81	82	52	55		53	57	66	54	31	28	19	
7. Adding	71	79	49	68	54	53		45	39	47	51	57	17	25
8. Geomet. forms	54	70	56	53	73	57	45		35	49	34	56	25	25
9. Learn. pairs	78	73	73	42	39	59	39	35		69	36	29	26	09
10. Recog. forms	88	74	71	56	51	66	47	49	69		44	37	34	28
11. Scroll	55	52	53	45	39	54	51	34	36	44		31	19	27
12. Compl. words	42	43	40	29	59	31	57	56	29	37	31		21	07
13. Estim. length	33	26	28	38	25	28	17	25	26	34	19	21		24
14. Drawing length	25	25	21	48	22	19	25	25	09	28	27	07	24	

To work out all the tetrad-differences was no light undertaking, since they run to the number of 3003. The calculation of these was entrusted to the competent hands of Mr. Raper. When all was reported ready we met, I carrying the "probable error" of the tetrad-differences—that is, the median value that should by theory be expected to arise from sampling alone, he bringing the average value of the 3003 tetrad-differences actually derived from the above table. My number was 0.061. His was 0.074; this, in order to get from the average to the median, had to be multiplied by the well-known constant 0.845, whereby it came finally to 0.062.

It may be of interest to survey the entire frequency distribution of the values concerned. The dotted curve (Fig. 1) shows the relative frequencies that should be expected from the sampling errors alone. About half should lie between *a* and *b*; extremely few beyond

c or *d*. The continuous rectangles show the relative frequencies that actually occurred. A better agreement of a theoretical frequency curve with one of actual observations does not, I venture to say, exist throughout psychology, or perhaps even throughout statistics.

The preceding result may be instructively compared with another one. The doctrine of Two Factors, as is only proper, has had to make its way in the face of strong resistance. But the latter has curiously adopted two contrary lines of defence. One is to question whether the mathematical criterion would really be satisfied by actual observation; and this doubt, I hope, has been met by the facts just cited. But the other opposition has instead asserted that *any* ordinary table of positive correlations would naturally satisfy it, so that such satisfaction must be devoid of peculiar significance. Recently this second line of opposition

FIG. 1.—Tetrad-differences of Simpson (37 subjects).

has acquired much greater vitality, in that a table of correlations has now been brought forward as an actual example; it is not derived from mental but from physical traits, and yet, it is said, exhibits a quite similar character. Here is the table:

	1	2	3	4	5	6	7	8
1. Area of ossification		88	60	62	43	31	25	26
2. Ratio of ossification	88		52	58	41	21	24	29
3. Height	60	52		69	44	51	45	11
4. Weight	62	58	69		65	39	40	83
5. Chest girth	43	41	44	65		59	36	69
6. Lung capacity	31	21	51	39	59		46	26
7. Strength of grip	25	24	45	40	36	46		14
8. Nutrition	26	29	11	83	69	26	14	

Let us, then, look at the median tetrad-difference derived from this table and compare it with the probable error to be expected from sampling alone; the respective values are 0.089 and 0.011; that is to say, the actually observed value is no less than eight times greater than what theory demands. Here again we can examine the whole frequency distribution.

The explanation of the figure (Fig. 2) is the same as in the preceding case of Simpson (Fig. 1). Between the curve showing the values to be expected from the errors of sampling and the rectangles showing those actually observed there is this time no agreement whatever.

LAW OF DIMINISHING RETURNS.

So much for the strengthening of the doctrine. I will now proceed to describe a rather curious matter that has arisen in the course of its elaboration.

Since the very beginning it has been known that the two factors, *g* and *s*, the energy and the engines, may have widely different relative importance, according to the particular mental operation involved. With some operations the superiority of one person over another is preponderantly decided by their respective amounts of the energy. With other operations, on the contrary, the dominant factor is the engine.

Subsequent research, moreover, has been gradually outlining the cases which incline in one or the other direction. Thus the energy is in general more important for operations that are composite, the engines for those that are monotonous. This is natural enough. The composite operation really involves several different engines; the superiority that an individual may happen to have in any one of them will tend to be neutralised in the average of them all; but a superiority in the energy will make itself felt in each, and thus obtain cumulative influence. Again, the energy is less and the

FIG. 2.—Tetrad-differences of A. Gates (115 subjects).

engine more influential whenever the operation depends much upon the efficiency of some sensory or motor apparatus. This, too, is natural enough; for such apparatus obviously constitutes a part and parcel of the engine. Yet again the energy has been found to lose in importance as compared with the engine in proportion as the operation tends less to create new mental content and more to reproduce old.

The point which I wish to bring forward in this place is that the relative influence of the energy and the engines changes largely with the class of person at issue. The most drastic example of this is supplied by a comparison between normal children and those who are mentally defective. The work of Abelson may be quoted, where the same tests were applied by the same experimenter to both classes. The correlations obtained for the two respectively are as follows:

NORMAL CHILDREN. (78 CASES.)												
	1	2	3	4	5	6	7	8	9	10	11	12
1. Opposites . . .	75	78	71	62	64	72	78	57	40	46	33	
2. Observation . . .	75	72	58	60	58	67	56	58	56	52	29	
3. Absurdities . . .	78	72	53	41	44	79	68	41	46	34	29	
5. Memory sentences . . .	71	58	53	54	61	54	37	54	55	19	43	
5. Crossing o's . . .	62	60	41	54	73	48	54	30	42	48	35	
6. Geometrical figs. . .	64	58	44	61	73	45	48	30	42	48	35	
7. Discrim. length . . .	72	67	79	54	48	45	56	49	30	31	06	
8. Crossing patterns . . .	78	56	68	37	54	48	56	30	21	27	18	
9. Memory form . . .	57	58	41	54	38	30	49	30	24	31	29	
10. Tapping . . .	40	56	46	55	36	42	30	21	24	29	18	
11. Strength of grip . . .	46	52	34	19	52	48	31	27	31	29	28	
12. Interpret. pictures . . .	33	29	43	35	35	06	18	29	18	28		
Mean = 0.466.												

DEFECTIVE CHILDREN. (22 CASES.)

	1	2	3	4	5	6	7	8	9	10	11	12
1. Absurdities . . .	1.0	1.0	98	97	1.0	1.0	1.0	98	94	94	79	
2. Opposites . . .	1.0	97	95	87	91	85	76	85	87	70	72	
3. Crossing patterns . . .	1.0	97	91	80	88	68	92	74	78	76	67	
4. Crossing o's . . .	98	95	91	85	77	84	67	76	81	73	55	
5. Memory sentences . . .	97	87	80	85	73	90	68	88	65	78	68	
6. Observation . . .	1.0	91	88	77	73	76	83	71	86	59	65	
7. Memory form . . .	1.0	85	68	84	90	76	65	67	70	77	75	
8. Interpret. pictures . . .	1.0	76	92	67	68	83	65	74	80	80	59	
9. Geometrical figs. . .	98	85	74	76	88	71	67	74	65	60	62	
10. Discrim. length . . .	94	87	78	81	65	86	70	80	65	51	45	
11. Tapping . . .	94	70	76	73	78	59	77	80	60	51	45	
12. Strength of grip . . .	79	72	67	55	68	65	75	59	62	45	61	
Mean = 0.782.												

All round, obviously, the correlations are much smaller in the case of the normal children. This indicates that with these the influence of the energy has gone down and that of the engines has correspondingly gone up.

Compare next young children with those that are older. Here I may quote from the admirable work of Prof. Burt. Applying his test of reasoning to numerous children of different ages, he obtained the following correlations with the estimates of the teachers (in general, the older children show smaller correlations):

Ages . . .	10-11	11-12	12-13	13-14
Correlations . . .	78	81	64	59

No less marked is this tendency on comparing children with adults. As examples may be taken the correlations obtained by Otis and Carothers respectively for what appear to have been similar tests in each case.

Test.	Correlations with <i>g</i> .	
	Otis, grades IV.-VIII.	Carothers, students.
Analogies . . .	0.84	0.71
Completion . . .	0.88	0.53
Directions . . .	0.86	0.45
Digits, memory . . .	0.41	0.22

Now these changes obviously follow a general rule. The correlations always become smaller—showing the influence of *g* to grow less—in just the classes of person which, on the whole, possess this *g* more abundantly. The rule is, then, that the more energy available already, the less advantage accrues from further increments of it. This is a well-known property of engines in general. Suppose that a ship at moderate expenditure of coal goes 15 knots an hour. By additional coal the rate can readily be increased another 5 knots. By doubling the addition of coal, however, the additional knots will certainly not be anything like doubled. This relation is observed not only in engines, but also widely elsewhere. In the science of economics, for example, it is expressed in the well-known law of diminishing returns. A moderate amount of capital spent on a given piece of land will produce a certain return; but on adding further doses of capital the returns will not increase proportionately.

In our psychological case of different classes of persons there enter no doubt various complications which render the theoretical interpretation more dubious. Above all, there is the fact that the classes better endowed with *g* have usually undergone more or less selection. For example, the university students of Carothers had been cleared of the weaklings who could not matriculate. This in itself would tend to lower all correlations due to *g*. However, such facts would seem capable of accounting for only part of the phenomenon,

not for the whole. There remains enough over to suggest a genuine law of diminishing returns for mental as for material processes.

COROLLARY OF INDEPENDENCE.

The next and final point to be raised here is a corollary of what has been said. Since a great many abilities depend almost entirely upon the efficiency of the engines involved and this efficiency varies independently from individual to individual, we may conclude that these abilities themselves vary almost independently from individual to individual.

This theorem has, indeed, been called into question. Some authorities have asserted the existence of a general "sensory level" of ability, so that the persons who are successful at one kind of sensory performance will tend to be so at others also. Other writers have adopted a similar position as regards what they call "practical" ability; persons are taken to be either endowed or not endowed with this all round. But no such position would appear to be supported by the available definite evidence. Dr. McCrae, for example, has recently examined the correlations between different tests that have been entitled those of "performance." These, even among persons of comparatively low status, proved to be, in fact, almost independent of each other. Still more striking has been the result of a very valuable investigation by Mr. Philpott. He undertook to test the discrimination of length, a power which obviously possesses great importance in many spheres of industry. He wisely tested this discrimination in two different ways. First, he showed pairs of lines and made the subjects judge which was the longer; and then he gave them single lines and made them, in each case, draw another line as nearly as possible of the same length. As result, these two performances, that seemingly are but manifestations of one and the same power, turned out to be almost entirely independent. Those who were best at judging between the two lines already drawn did not, to any appreciable extent, excel at making a second line equal to a given one. Quite similar results were obtained for the discrimination of angles, as also for perceiving whether a line is straight or not.

Accepting, then, the conclusion that an immense number of abilities vary from one individual to another almost independently of each other, what is the practical result? Let us try to get a notion how such abilities of any person must be distributed in respect of excellence. By all experience, and also by statistical theory into which we cannot enter here, the great bulk of his abilities will tend to be mediocre; that is to say, they will be near the general average of his class. A fair number will be distinctly above this average, and a fair number below. A small number will be much above; and so also below. The whole frequency distribution will, in fact, have a bell-like shape similar to that which was shown by the curves of the tetrad-differences to be expected from sampling errors. At the extreme ends of the distribution will lie a very small number of performances for which the person is, on one side a genius, and on the other an idiot. Every normal man, woman, and child is, then, a genius at something as well as an idiot at something.

It remains to discover *what*—at any rate in respect of the genius. This must be a most difficult matter, owing to the very fact that it occurs in only a minute proportion out of all possible abilities. It certainly cannot be detected by any of the testing procedures at present in current usage, but these procedures are capable, I believe, of vast improvement.

The preceding considerations have often appealed to me on looking at a procession of the unemployed, and hearing some one whisper that they are mostly the unemployable. That they are so actually I cannot help concurring. But need they be so necessarily? Remember that every one of these, too, is a genius at something—if we could only discover what. I cherish no illusion, indeed, that among them may be marching some "mute inglorious Milton, some Cromwell guiltless of his country's blood." For these are walks in life that appear to involve a large amount of *g*. But I am quite confident that every one of them could do something that would make him a treasure in some great industrial concern; and I see no reason why some should not have even become famous, in such occupations, for example, as those of dancers, jockeys, or players of popular games.

Radiometric Measurements of Stellar and Planetary Temperatures.

By DR. W. W. COBLENTZ, Bureau of Standards, Washington, D.C.

THE recent measurements of planetary radiation and planetary temperatures, especially of Mars, had their beginning in the first really successful tests at the Lick Observatory, Mt. Hamilton, California, in July 1914, when thermo-couple measurements were made on 112 celestial objects, including 105 stars, down to magnitude 6.7, and the planets Venus, Mars, Jupiter, and Saturn. (Bureau of Standards Sci. Paper, § 244, 1914.) The experimental procedure then employed, and the results obtained, have foreshadowed, in an unforeseen manner, practically everything that has been accomplished since then.

During the world strife and confusion that occurred in the meantime, nothing further was accomplished until the fall of 1921, when, at the invitation of the Lowell Observatory, I was given a further opportunity to try out a new method of measuring stellar tempera-

tures by means of screens of quartz, water, etc., which separated the incident radiation into components containing radiation of the following wave-lengths: 0.3 to 0.43 μ ; 0.43 to 0.6 μ ; 0.6 to 1.4 μ ; 1.4 to 4.1 μ ; and 4.1 to 12 μ . In this manner the distribution of energy in the spectra of 16 stars was determined, thereby obtaining for the first time an insight into the radiation intensities in the complete spectrum of a star.

From a comparison of the observed stellar spectral radiation components with similar data, calculated for a black body at various temperatures, it was found that the stellar temperatures range from 2500° to 3200° K for red stars, up to 14,000° K or even higher for blue stars, which is in good agreement with other methods of estimating stellar temperatures.

Since then Pettit and Nicholson, using similar thermocouples and a transmission screen of water attached to