of digits will fluctuate about the mean 450 according to a probability-curve whose "probable error" is about 19.

(1) One explanation of the failure of the law is that the requisite plurality of items is wanting. Suppose we had taken sums of *two* (instead of a hundred) digits, the grouping of these sums would be best represented by a right line, or rather two right lines. If we took three digits at a time, the resulting form would be parabolic. A variant of this class of exception is when the larger items are few or unique, while items of an inferior order congregate in great numbers. Suppose each observation to consist *either* of the digits 3 or 6, *plus* ten items taken at random from the series ·1, ·2, ... ·9. There would then be generated a curve like those in Dr. Venn's Fig. 2. If, instead of 3 and 6, we had two digits, 4 and 5, differing by very little from each other, the abnormal uniqueness of the larger items would be disguised. It is upon this principle, doubtless, that the population of a kingdom appears to conform (in respect of height or other attribute) to the law of error, while at the same time each province may present a distinct type. Suppose that the majority of our returns were, as the last-mentioned case, *either* 4 or 5 *plus* an aggregate of smaller items; but that a small proportion of the returns were governed by a widely disparate "large item," *e.g.* 8 or 9; in this case we might have the appearance presented by Dr. Venn's Fig. 1. The body of the curve would seem to be of the probability family; but there would be tacked on a tail appertaining to a different type. Dr. Charles Roberts has adduced some statistics of this species in a paper published in the *Medical Times*, February 7, 1885.

(2) We have hitherto supposed that the constituent items have no bias in one direction. Suppose, however, that instead of the digits 1, 2, ... 8, 9 being each equally eligible, 8 and 9 became inadmissible; and, whenever one of those digits was presented, we had to substitute 6 and 7 respectively. There would thus be two chances in favour of 6 and also of 7. An aggregate of 100 digits each selected according to this unsymmetrical scheme would be grouped about the mean value $10 \times (1 + 2 + 3 + 4 + 5 + 2 \times 6 + 2 \times 7)$, or 410, in a form which as to the body of the curve would be a probability-curve, but which would be unsymmetrical at the extremities. The most familiar example of this case is afforded by games of chance. If black and white balls, in an unequal proportion and immense numbers, are mixed up, then if you take at random batches of 100 (or 1000) balls the percentage of white or black balls will fluctuate in the manner described. It is quite possible that this principle should govern what Dr. Venn calls a "one ended phenomenon," *i.e.* one in which unlimited variation is conceivable in one direction but not in the other. Dr. Venn's Fig. 1 seems fairly well to represent a biased probability-curve.

(3) We have hitherto supposed that the individual observation or return is the *sum* of the variable elements. But it may be a more complicated function. Thus it may be a product. The *logarithm* of the observations may fluctuate according to a probability-curve, while the observations themselves obey a law which has been investigated by Dr. Macalister in the Proceedings of the Philosophical Society (1879); related to the geometrical mean just as the probability curve is to the arithmetic mean. This grouping is to be expected wherever the analogies of *Fechner's law* prevail. This may be the rationale of the fact which I have elsewhere pointed out, that fluctuations of price rise much higher above, than they fall below, the mean. But, where the principle of estimation does not come in, it is not quite clear why the geometrical curve should be more appropriate to a "one-ended phenomenon" than the biased probability-curve which has been described under our heading (2). At any rate, in the case before us, Dr. Venn's Fig. 1, the numerical statistics which he has allowed me to inspect show much too close a correspondence between the body of the figure and the probability-curve to admit of the geometrical explanation. There is also this peculiar difficulty, that the longer limb of the given curve is the lower one. F. Y. EDGEWORTH.

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A Null Method in Electro-calorimetry.

BY reference to the last number of the *Electrical Review* (vol. xxi. p. 262), wherein is printed a short abstract of our paper on "A Null Method of Electro-calorimetry" read before the British Association on September 1, Mr. Huntly will find that the method of measuring specific heats suggested by him is in principle similar to that described by Mr. Gee and myself. The method has been employed for determining specific heats during the last two years, but we have delayed publication till the best working details of the method have been elaborated.

In certain practical details our method differs from Mr. Huntly's suggestion. The mass of liquid in each calorimeter is *not* the same. It is much preferable to have the masses inversely proportional to the specific heats, so that the thermal capacities of the liquids are equal. In this way it will be readily understood that the correction for radiation can be made to disappear altogether. For since the calorimeters are precisely equal, and their temperatures equal, the loss of heat by radiation must be the same from each; further, since the thermal capacities of the liquids are the same, as well as that of vessels and stirrers, it follows from the equality of the resistances that the same current will produce the same rise in temperature in each case, and conversely, since the heat radiated from each calorimeter is the same, and since the thermal capacities of the calorimeters and stirrers are equal, it follows that, if the same current traversing the equal resistances produces the same rise in temperature in each liquid, the thermal capacities of the two liquids are the same, whence the specific heat can at once be determined by determining the masses of the liquids. Virtually, then, the null method of obtaining the same rise of temperature in each calorimeter is attained by varying the mass of liquid in either or both calorimeters. In practice we approximate as nearly as possible to the condition by adding liquid to that calorimeter which rises in temperature most quickly, and then make a final adjustment by shunting a *very small fraction* of the current by means of the high resistance in the box. This is, we believe, the first time that a method for measuring specific heats has been published in which the correction for radiation and for the thermal capacity of calorimeters and stirrers has been entirely eliminated.

With the first apparatus we had made to embody these ideas, viz. that described in the *Electrical Review* (*loc. cit.*), an accuracy of at least one-tenth per cent. could be obtained from a single experiment, thoroughly confirming Mr. Huntly's anticipations as to the delicacy of the method. We have just introduced some considerable improvements in the apparatus which we hope will enable us to insure much greater accuracy than that hitherto obtained.

A few words are required in reply to some observations of Mr. Huntly. First, he suggests a bolometric method of determining the difference of temperature. We have so far preferred a thermo-electric method, which, without a specially constructed galvanometer, enables us to detect with certainty 1/2000 of a degree; the necessary corresponding variation in the resistance of a Pt wire would only be 1·6 parts in a million; besides some difficulties may arise in procuring two pieces of Pt wire which shall have the same temperature coefficient to 1 part in a million, even if they be cut from the same piece originally. Secondly, the time method described by Mr. Huntly at the end of his paper seems to me to have a fatal objection: it would be quite impossible to keep the current constant for a long time to the 1/2000 part which would be requisite to secure such accuracy as we can get with present arrangements.

WILLIAM STROUD.

Mental Development in Children.

I SHOULD like to hear the opinion of psychologists on the following circumstance:—A female child, quick and intelligent, when about fifteen months old, learned to repeat the alphabet, shortly afterwards the numerals, days of the week, month, &c., and, subsequently, scraps of nursery rhymes, English and German; then to spell words of two and three letters. All this was learned readily, eagerly indeed, and for a time she remembered apparently every word acquired, indelibly. At about two years old further teaching was for a time remitted, as she was observed to be repeating audibly in her sleep what she had learned during the day. Subsequently, tuition was resumed, under a governess, but she had not only forgotten much of what she had previously known perfectly, but learns far less readily than formerly. She is now about three and a half years old, in perfectly good health and spirits, quick, and particularly observant, but the capacity for learning by rote is materially diminished; she is remarkably imitative, but shows no faculty whatever for writing, and as little for music.

I should like to hear of any parallel cases, and what the ultimate development has been; with any opinions upon the cause of their appearances. M A.

September 18.