Physiology. — *"A few remarks concerning the method of the true and false cases."* By Prof. J. K. A. WERTHEIM SALOMONSON. (Communicated by Prof. C. WINKLER.)

The method of the true and false cases was indicated by FECHNER and used in his psychophysical investigations. He applied this method in different ways: first to determine the measure of precision (Pracisionsmasz) when observing difference-thresholds, afterwards to determine these difference-thresholds.

Already in the course of his first experiences arose the difficulty that not only correct and incorrect answers were obtained, corresponding with the "true" and "false" cases, but that also dubious cases occurred, in which the observer could not make sure as to the kind of difference existing between two stimuli, or whether there did exist any difference at all. FECHNER himself, and many other investigators after him, have tried in different ways to find a solution to this difficulty. What ought to be done with these dubious cases?

FECHNER has indicated several methods, which he subjected to an elaborate criticism. Finally he concluded that the method to be preferred to all others was that one, in which the dubious cases were distributed equally amongst the false and the true cases. If e. g. he found w true cases, v false cases and t dubious cases, he calculated his measure of precision as if there had been $w + \frac{1}{2} t$ true cases and $\frac{1}{2} t + v$ false cases.

Furthermore he showed that a method, employed especially by American experimental physiologists, in which the reagent is urged always to state a result, even if he remains in doubt, practically means the same thing as an equal distribution of the t cases amongst the true and the false cases.

FECHNER still worked out another method, by means of which the threshold value was first calculated from the true cases, then from both the true and dubious cases, whilst the final result was obtained with the aid of both threshold values.

A most elegant method to calculate the results of the method of the false and true cases has been pointed out by G. E. MÜLLER, starting from this view, that as a matter of necessity the three groups of cases must be present, and that they have equal claims to exist; that the number of cases belonging to each of these groups in any case, are equally governed by the well-known law of errors. From the figures for the true false and dubious cases the thresholdvalue may afterwards be calculated.

I need not mention some other methods, e.g. that of FOUCAULT,

that of JASTROW, because the method of FOUCAULT is certainly incorrect (as has been demonstrated among others by G. E. MULLER), whilst that of JASTROW is not quite free of arbitrariness.

Against all these different ways of using the method of the false and true cases, I must raise a fundamental objection, which I will try to elucidate here.

Whenever two stimuli of different physical intensity are brought to act on one of the organs of the senses, either the reagent will be able to give some information as to the difference between these stimuli, or he will not be able to do so. If he cannot give any information, then we have before us a dubious case, if on the contrary he is able to give some information, this information may either be correct, — this constituting a true case — or it may be incorrect, when we shall have a false case.

If the experiment is repeated a sufficient number of times, we shall have obtained at last a certain number of true cases w, of false cases v and of dubious cases t.

Generally it is admitted that the reagent has indeed perceived correctly w times, that he has been mistaken v times, that he was in doubt t times. If this premiss were correct, FECHNER's or G. E. MULLER's views might be correct too. This however is not the case. An error has already slipped into the premiss, as will become evident furtheron.

No difference of opinion exists as to the dubious cases. To this category belong first those cases, where the reagent got the impression of positive equality, and next those cases, where he did not perceive any difference, and consequently was in doubt. Together they embrace such cases only, in which a greater or lesser or even infinitesimal physical difference was not perceived.

Neither need any difference of opinion exist as regards the false cases. In these cases a stimulus has been acting on the organs of the senses, and information was given about the effect, but on account of a series of circumstances, independent of the will of the reagent, his judgment was not in accordance with the physical cause. The physical cause therefore has not been perceived, but accidental circumstances led the reagent to believe that he was able to emit a judgment, though this judgment, accidentally, was an incorrect one.

And now we are approaching the gist of the argument. If it be possible, that amongst a series of experiments a certain number occur, in which the reagent really does not perceive the physical cause, but is yet induced by chance to emit a judgment which proves to be an *incorrect* one, then there ought to be also a number of

cases, in which likewise the physical cause is not perceived, in which however by chance a judgment is emitted, though this time a *correct* one. These facts being dependent on circumstances beyond our will, the chances are equal that either a wrong or a right judgment may be given. If therefore we had v false cases, we may reasonably admit the existence of v cases, in which practically the physical cause has not been perceived, and where yet a judgment, this time a correct one, has been given. These v cases however have been recorded amongst the true cases, though they cannot be admitted as cases of correct perception: it is only in $w—v$ cases that we may suppose the physical cause to have been really and correctly perceived; in all other cases, in $2v + t$ cases therefore, there has been no perception of the real difference of the stimuli.

In this way we have only to consider two possibilities, constituting the *perceived* and *non-perceived* cases, the number of which I will indicate by ξ and χ. The supposition that we may apply the principles of the calculus of probability to them, is justified a priori.

This supposition is changed into a certainty, if we apply the mathematical relations, stated by FECHNER to exist between the numbers of true and false cases.

As is well known, FECHNER added to the number of true cases, obtained by the experiment, one half of the dubious cases: he used therefore in his calculation a rectified number of true cases $w' = w + \frac{1}{2}t$. In the same manner he corrected the number of false cases by adding to them likewise one half of the dubious cases: $v' = v + \frac{1}{2}t$.

In calculating the number of my perceived cases, I get $\xi = w—v$, whilst the number of non-perceived cases is represented by $\chi = t + 2v$. Evidently I may also express the number of perceived cases by

$$\xi = w^1 - v^1.$$

As FECHNER has given for the relative value of the corrected number of true cases the expression:

$$\frac{w^{11}}{w+t+v} = \frac{w+\frac{1}{2}t}{n} = \frac{1}{2} + \frac{1}{\sqrt{\pi}}\int_0^{Dh} \varepsilon^{-t^2} dt$$

and for the corrected relative number of false cases the expression:

$$\frac{x'}{w+t+v} = \frac{v+\frac{1}{2}t}{n} = \frac{1}{2} - \frac{1}{\sqrt{\pi}}\int_0^{Dh} \varepsilon^{-t^2}\, dt$$

we obtain from these immediately for ξ and χ the two relations:

$$\xi = \frac{2}{\sqrt{\pi}} \int_0^{Dh} \varepsilon^{-t^2} dt$$

and

$$\chi = 1 - \frac{2}{\sqrt{\pi}} \int_0^{Dh} \varepsilon^{-t^2} dt.$$

We find therefore that the way of dealing with the true, dubious and false cases as proposed by me, allows us to use FECHNER's well-known tables.

I wish to lay some stress here on the fact, that G. E. MÜLLER's formulae give the same result, saving only the well-known difference in the integral-limits: these latter being 0 and $(S_u \pm D)\, h_u$.

I need scarcely add that my remarks do not touch in the least the question about "thresholdvalue" between FECHNER and G. E. MÜLLER.

It is evident, that the result of the calculation of a sufficiently extensive series of experiments according to the principles, given in my remarks should give numbers, closely related to those either of FECHNER or of G. E. MÜLLER — depending on the limits of integration. Still I wish to draw special attention to the fact that the formulae of G. E. MÜLLER about the true, false and dubious cases are rather the statistical representation of a series of nearly identical psychological processes, whilst the opinion professed by me on the method of the false and true cases, represents a pure physiological view.

Finally my remarks show, that CATTELL and FULLERTON's way of applying the method of the true and false cases is less arbitrary than it seems to be at first sight. They take for the thresholdvalue the difference of stimuli with which the corrected number of true cases attains 75 %. Such being the case, ξ and χ are both $= 50$ %. They consider therefore the thresholdvalue to be a difference between two stimuli such, that there is an equal chance of this difference being perceived or not.