THE

AMERICAN JOURNAL

OF

PSYCHOLOGY

EDITED BY

G. STANLEY HALL

PROFESSOR OF PSYCHOLOGY AND PEDAGOGICS IN THE JOHNS HOPKINS UNIVERSITY

VOL. I No. 1

BALTIMORE, NOVEMBER, 188

(ISSUED QUARTERLY)

N. Murray, Publisher

THE PSYCHO-PHYSIC LAW AND STAR MAGNITUDES.

BY JOSEPH JASTROW, PH. D.

The application of the psycho-physic law to the relation between the estimated and the photometrically measured brightness of the stars has good claims to rank at once as the most practical, most important, most interesting and historically valuable illustration of the significant natural fact which that law formulates. The magnitudes were assigned to the stars at a time when no objective method of measuring the light emitted by them existed, and the stars were thus graded because that seemed the best way of arriving at a roughly quantitative notion of their relative illuminating powers. The eye was used as a natural (psychical) photometer, and now that artificial (physical) photometers rearrange these magnitudes, it is possible and important to trace the (psychophysical) relation between these two photometric scales. In fact, this very relation (with the exception, perhaps, of the physiological researches of E. H. Weber) did most to suggest to Fechner the formulation of his law. As he pointedly remarks, in this field the psycho-physic problem was solved before it was stated.

It will perhaps be well, before passing to the magnitudes themselves, to illustrate by an analagous instance what the psycho-physic relation in question

really means. For this purpose the historically first suggestion of the psycho-physic law, dating from Daniel Bernoulli (1730 or 1731) and elaborated by Laplace, will be the best. Bernoulli introduced into the calculation of probabilities the distinction between the value and the emolument of money. By the first he meant the buying power of the coin, by the latter the amount of the additional pleasure, comfort, etc., the money could bring in any one case. In other words, while a dollar will buy for A as it will for B, C and D the same amount of sugar, or of bread, yet the real value of that dollar will be much more to B than it will to A if B is a poor man and A a wealthy one. If A were to find a dollar on the street it would produce in him only the very slightest, if any, addition of pleasure or satisfaction, while if B found the dollar it would mean to him a very great happiness-increment indeed. To get a proportionately equal pleasure A would perhaps have to gain ten thousand dollars by a rise in his railroad stocks. The notion that underlies these commonplaces is that the amount of pleasure, the import of an addition of wealth, depends upon the wealth already possessed, being greater when that is less, and less when that is greater; and the most plausible supposition is that, *cœteris paribus*, (for one's liberality or avarice, and a hundred other circumstances, can alter this) the import, that is, the emolument, is inversely as the wealth. If my fortune amounts to $5,000 and my neighbor's to $10,000, an additional $500 is worth *twice* as much to me as it is to him; to have an equal increase he must get $1,000 when I get $500, in which case our fortunes are increased by one-tenth their whole amount. Hence "equal emoluments" means "equal *ratios*" of the original wealth. Finally, suppose A has $1,000 and I give him $100; I now want to again so increase his fortune that he feels himself as much benefited as he did by the first increase; that is, I want to give him an equal ratio of his fortune, or an emolument equal to the first. To do this I must give him $110, and to give him a third "equal emolument" I must give him $121; and for a fourth, $133.10; for a fifth, $146.41, and so on. To produce an arithmetical series of 1, 2, 3, 4, etc., equal emoluments, I need a geometrical series of money-quantities, and the function expressing the relation of an arithmetical and a geometrical progression, that converts multiplying into adding is the *logarithm*. Hence we may say that the emolument is the logarithm of the wealth; and by widening the conception of the wealth to the general one of a physical stimulus of any kind, and similarly putting sensation in general for the particular sensation caused by an increase in money, you have the psycho-physic law. The practical difficulty is to *prove* that an increase of stimulus has always the same effect when it forms an equal part of the stimulus already present, instead of assuming it as was done above.

In the stars we have a large number of stimuli of all variations of intensity, and to introduce order into this series we roughly divide them into classes or magnitudes. This classification dates from Hipparchus, (about 150 B. C.), who happened to choose six such magnitudes, to one or other of which every star visible to the naked eye could be assigned. The stars of the first magnitude, by their preëminent

brightness, probably first attracted the attention, and got to be first enumerated; then in a descending scale the second, third, fourth and fifth, leaving all the faintly visible stars for the sixth. The magnitudes were determined presumably with the intention of making as much apparent difference *in toto* between one magnitude and the next above it, as between it and the next below it. That Hipparchus's catalogue happened to be divided into just six magnitudes we must regard as largely a matter of accident; an accident in the same sense as it is an accident that our foot is just 304.8 mm., and not a little more or a little less. With a more delicate eye Hipparchus might have made twelve magnitudes by making each magnitude half its present compass; and, in fact, he indicated in regard to some stars that they were rather larger or smaller than the average star of the magnitude to which it was assigned by the terms *μείζων* and *'ελάσσων*. The point of interest is to see whether the magnitudes presumably thus of equal compass, forming to the mind a decreasing arithmetical series, will have for the photometric quantities of light emitted by average stars of each magnitude a geometrical series decreasing by a common fractional ratio. If this is found to be true, then the psycho-physic law holds, and astronomers must take it into account.

The first notice of the existence of such a ratio and of its determination that I can find is given by Steinheil (1835). Steinheil's photometer has an object glass divided into two halves, and the light of the two stars to be compared is thrown by prisms, one into one and the other into the other half. Both stars are put out of focus so as to appear as discs by sliding the half objective towards the eye-piece, and the brighter disc is enlarged until the two are equally bright; whereupon the position of the two half objectives, with reference to the focal distance (by the law of inverse squares), shows the relative reductions of the light. For illustration's sake he chose thirty stars whose estimated magnitudes were known, and he expressed the amounts of light emitted by each in terms of one of them. Arranging these in five classes, he finds that there is a ratio by which the amount of light of a star of any magnitude is to be multiplied in order to equal in brightness a star of the next higher magnitude: that this ratio is tolerably constant, and equals on the average 2.831. Fechner's revision of these observations gives 2.702.

At about the same time Sir John Herschel made a similar comparison of stars at the Cape of Good Hope, but concluded that the quantities of light emitted by stars of various magnitudes formed a series of inverse squares, such as 1, $\frac{1}{4}$, $\frac{1}{9}$, $\frac{1}{16}$, $\frac{1}{25}$, etc. But Fechner has shown that Herschel's own observations really correspond more accurately to a geometrical progression with the ratio $\frac{1}{2.241}$ than to the series above proposed; the sum of the deviations by least squares being 2.719 for Herschel's series and only 2.2291 for the geometrical series. As Mr. Peirce says, "So powerful is this natural influence [to make equal ratios correspond to equal intervals] that even Sir John Herschel's scale, which was conceived by its author to conform to a very different photometric law, really does conform to this and not to the one he desired to follow."

Johnson, in 1851, compared the light of two stars by reducing the light of the brighter (by diminishing the aperture of the object glass) until it equalled the latter, and found as the ratio of light between two magnitudes 2.358 from sixty stars of from the 4.1 to the 9.7 magnitude. Johnson also deduces the following ratios from the catalogues of previous astronomers, and assigned to each an appropriate weight to mark its reliability. He makes for Herschel 2.46 (wt. 1), for Struve 2.61 (wt. 2), which would be 2.41 if he had taken 13 instead of 12 magnitudes as limit of telescopic vision; for Otto Struve 2.46 (wt. 1), for Argelander 2.32 (wt. 3), for Groombridge 2.58 (wt. 1.5), for his own 2.36 (wt. 4).

Stampfer (1852), from the observation of 132 stars of fourth to tenth magnitude, fixed the ratio as 2.519, and from the observation of small planets 2.545.

[Dawes (1851), by a peculiar and much discredited method, arrived at a ratio of 4.00. This has been so unfavorably criticised, and so many sources of error in it have been pointed out, that it will not be considered here.]

Pogson (1857) compared the light of stars by finding the size of the aperture of the object glass necessary to extinguish their light, and concluded that the ratio (from observations of thirty-six small planets and stars) is 2.427; but proposes as the ratio to be adopted by astronomers 2.572, whose logarithm is just .4.

Seidel, (1861) who used Argelander's estimations of magnitudes and photometrically measured 175 stars with a Steinheil photometer, deduced 2.8606 as the ratio, mainly from determination of the brighter stars. He mentions, however, that the ratio is subject to many irregularities, and that perhaps it decreases as the stars decrease in brightness. Mr. Peirce deduces 2.754 as Seidel's ratio from stars to the 3.5 magnitude.

Wolff calculates from his observations that the ratio for passing from the 2nd to the 2.5 magnitude is 1.52:1; from 2.5 to 3 is 1.53:1; from 3 to 4 is 1.51:1; but 3.5 to .4; 4 to 4.5 and 4.5 to 5 have smaller ratios, on the average only 1.34:1. This gives for the higher entire magnitudes 2.310, for the lower 1.795.

From Zöllner's observations of forty-two stars (1st to 6th magnitude), the ratio 2.761 was deduced; from 102 stars (2d to 6th magnitude) of the same observer, 2.366.

Dr. Rosen's observations of 100 stars from the 5th to 10th magnitude give, according to Peirce, the ratio 2.339, with an indication of a higher ratio for the brighter stars.

Mr. Peirce, from his own observations, deduces for stars (1.5 to 6.5 magnitude) 2.773, but on throwing out certain stars affected by a constant error 2.449 for stars of 4.5 to 6.5 magnitude. Mr. Peirce gives reasons for believing that the Steinheil photometer is apt to make the ratio in question too large. Steinheil and Seidel, who used this instrument, give by far higher values than other observers, and the determination of the same twenty-seven stars gives for Seidel 2.780, for Zöllner 2.4275.

On the whole, Mr. Peirce prefers to consider the ratio as slightly decreasing with the magnitude, and proposes the formula, log. ρ=0.486—.0162m., which empirically satisfies the observations of Seidel, Rosen

and himself. Here ρ is the ratio in question and m the average magnitude.

One sees from these facts (1) that the existence of a ratio by which the quantity of light emitted by a star of one magnitude is to be multiplied to express the light emitted by a star of the next higher magnitude has been questioned by Herschel alone, whose own observations, however, show that he was wrong; (2) that [with the exception of Dawes] the ratio thus found does not differ very considerably from 2.5 in different observers, and (3) that there are many indications that this ratio is not quite constant, but decreases with the magnitude.

Under these circumstances it seemed to the writer well worth while to reinvestigate this ratio throughout the visible scale of star magnitudes from the valuable photometric comparisons which Prof. Pickering (with the assistance of Mr. Searle and Mr. Wendell) has made at the astronomical observatory of Harvard College. (v. Memoirs of that Obs., vol. XIV).

Their method of observing stars was by means of the meridian photometer. The essential parts of this instrument consist in two right angled prisms to reflect the two stars to be compared into the two similar objectives of a horizontal telescope; of a system of adjusting apparatus by which the stars thus observed could be kept in the centre of the field; of a double-image prism of Iceland spar and glass set in the tube near the focus of the objectives, in order to split the emerging pencil from each objective into two, and so adjusted as to make one pencil from one objective coincide with the opposite pencil of the other objective; of an eye-piece through which the two centrally coinciding pencils pass, in front of which is placed a Nicol with an eye-stop of such an aperture that it will cut off the two outside pencils, allowing only the central one to pass; of a graduated circle attached to the eye-piece and the Nicol. The pole star was always used as the constant star, and an observation consisted in determining the angle through which the Nicol must be rotated from the point where the two lights are equal to the point where the pole star disappears, the relative brightness of the two stars being measured by the square of the sines of these angles. Adopting the proposition of Pogson, that the logarithm of the ratio of light between two successive magnitudes is .4, it is easy to form a table of photometric magnitudes corresponding (to the nearest tenth) to the angles thus determined.

In all, 4,260 stars of from the first to the sixth magnitude were thus observed in 700 series, including 94,476 separate comparisons. The special sources of error avoided by this method are that one star is seen at a time, and contrast with bright neighboring stars is avoided; that the combined light of several stars is never mistaken for one; that errors resulting from the relative position of stars do not occur; that all stars are observed near the meridian, thus facilitating the correction for atmospheric absorption, and so on. (v. original.)

An important part of the work consists in the comparison between these photometric magnitudes and the eye estimates of former observers, with a discussion of their deviations. It is these tables that have been here used.

By a simple formula with which Prof. Pickering, for

whose aid I desire publicly to record my obligations, has kindly furnished me, these tables can be transformed so as to become directly useful for the present purpose. That is, the eye-estimations of magnitude of the several observers can be compared with the Harvard photometric determinations of the same stars (or equivalent stars), and the ratio which each observer more or less unconsciously used for passing from one magnitude to the next may be deduced. It must not, however, be supposed that these estimations are entirely independent of one another. There was almost an unbroken tradition which, to a greater or less extent, either determined the estimation of the magnitudes themselves or influenced the habit of those who made new estimations.[1]

The resulting deviations between observers are

[1] "In Ptolemy's catalogue of stars, which is supposed to date from Hipparchus, we find the stars ranged in six orders of brightness called magnitudes. The earlier observers not only imitated this method of indicating the brightness from Ptolemy, but also, each of them derived immediately from the study of the Almagest and its comparison with the heavens the habit which determined the limit of brightness between stars which he would assign to different classes. This must, at least, have been the case with Sufi and with Tycho Brahe. Ulugh Beg was, no doubt, influenced by Sufi, as well as by Ptolemy directly, and Hevelius was in the same way influenced by Tycho. It appears that down to about 1840, Bayer's Uranometria enjoyed a high reputation. Argelander showed, however, that its magnitudes were simply extracted from Tycho's catalogue [and from the Almagest in most cases, s. Argelander, De fide Uran. Bayeri, p. 15 (E.)], and he himself proceeded to make a Uranometria Nova. It is to be presumed, therefore, that he endeavored to model his scale of magnitudes upon that of Tycho, although he may have sought to improve upon Tycho's scale by making the intervals between the limits of successive magnitudes such as would seem equal. All observers of stars visible to the naked eye since Argelander have sought to conform to his scale. It is, thus, easy to understand how all the observers have, roughly speaking, the same scale of magnitudes. On the other hand the scale of Sir John Herschel, which was based on common English tradition from Flamsted (who perhaps imitated Hevelius, but was a careless observer of magnitudes), is very different." C. S. Peirce, Harv. Annals, vol. IX., p. 1-7, where is also given an ingenious diagram illustrating the differences between various observers.

many, and are, with regard to the completeness of the survey, the total number of magnitudes used, the fineness to which the estimations were made, and the method of making them.

The tables of Prof. Pickering, which are readily serviceable for my purpose, are those comparing the photometric measurements with the estimations of Ptolemy, Sufi, Struve, J. Herschel, the Uranometria Nova of Argelander, the Durchmusterung of Argelander, Behrmann, Heis, Houzeau, the Uranometria Argentina of Gould, Flammarion, the Bonn observations, (Argelander), and of Prof. Pickering himself. Other of the tables there given are also indirectly useful for this purpose.

The total number of estimations thus furnished is very near twenty thousand, all but eighty-five of which fall between the 1st and the 7th magnitude. The estimations of each observer were distributed in a somewhat peculiar manner, there being always an undue number of stars estimated as being just of the 2d, 3d, etc., magnitude than of the 2d to 3d, 3d to 4th, etc., when that mode of estimating magnitudes was used. The rule followed in condensing tables arranged on this plan was to sub-divide them into divisions in which the even magnitudes came at the centre and the intermediate divisions to either side, dividing the exactly intermediate division into two, and counting half for the group above and half for that below, when necessary. Moreover, the average photometric result corresponding to any one magnitude, or sub-division of a magnitude, was weighted by the number of stars observed as of that magnitude; and the stars of intermediate magnitudes were

weighted by half the sum of the number of stars to either side of the even magnitude with which they were grouped, so as to bring the average estimation at exactly an even magnitude. Where the tables were given in 10ths of magnitudes, both the photometric result and the eye-estimates were weighted by the number of stars observed, and the groups formed by taking all the stars from the middle of one magnitude to the middle of the next, counting the number of just 1.5, 2.5 magnitudes, etc., as half for each. The photometric results corresponding to exactly one magnitude of interval were then calculated from the average weighted 10ths of eye-estimation (which seldom differed much from unity in either direction). With the exception of the eye-estimates of Professor Pickering, which were made with reference to the photometric magnitudes as well as with especial care and with the avoidance of many sources of error (and of a few observations by Sir J. Herschel, which have not been here considered), all the tables show one serious and one more or less decided deviation; they estimate stars of the first magnitude too bright. Or perhaps one ought to say that some of the stars of the first magnitude are so intensely bright that they make the average star of the first magnitude much too bright; or again, that the stars enumerated as of the first magnitude really should be sub-divided into two, those of the first magnitude and those few preëminently bright stars which one might term the 0th magnitude. It is also to be remembered that there are fewer of these stars to be observed, and thus greater room for error. A similar but opposite effect is noticed in the fact that in the six cases in which basis is given for calculating the ratio of 7th to 6th magnitude this ratio is too small; these six ratios present great discrepancies, and the result is not of great reliability. My method of correcting for these errors is to calculate the curve which the other four ratios follow and calculate the positions at the "2-1"[1] and the "7-6" ratio from the formula thus obtained.

Another peculiar irregularity is to be found in the two ancient catalogues of Ptolemy and Sufi. The ratio from "3-2" to "4-3" undergoes only a very slight fall or in Sufi's case even a rise, but in passing from "4-3" to "5-4" a sudden and most decided fall. I see no ready way of accounting for this except perhaps that these observers may have had in mind a general comprehensive distinction between bright and faint stars, and that in the desire to separate the two they made a gap between the 1-2-3 and the 4-5-6 magnitudes. No such effect occurs at all in the modern catalogues. On the whole, as the importance of these catalogues for this purpose is slight, it seemed better to omit the ratios in question, and perhaps it might be best to omit Ptolemy's and Sufi's catalogues altogether, the effect of which would be to slightly lower the resulting ratios.[2]

[1] The 2-1 ratio, 7-6 ratio, etc., means the ratio for passing from an average star of the 2nd to an average star of the 1st magnitude; from one of the 7th to one of the 6th, and so on.

[2] It should be added that Houzeau's table gives a value for passing from the 6.7 to the 6th magnitude, which I could not use. Behrmann's ratio for 2-1 from only five stars, and the Bonn observation ratio of 5-6 from twenty-two stars were also not used, for evident reasons.

The general average of all[1] the tables here used gives the following table, including all the above corrections:

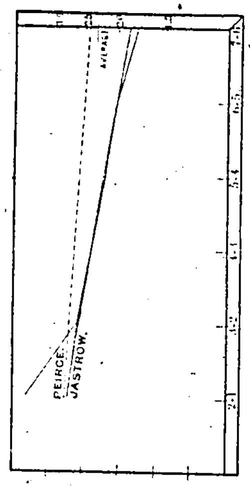
Magnitudes.	2-1.	3-2	4-3	5-4	6-5	7-6	Av. of all.
Logarithms of Ratios.	(.5572) Corrected .4474	.4147	.3780	.3360	.3125	(.2501) Corrected .2732	.3603
Ratios....... ..	(3.607) Corrected 2.802	2.598	2.388	2.1675	2.053	(1.779) Corrected 1.876	2.293
Weight, i. e. Number of observations.	288	818.5	1791.5	3746.5	9772	2428.5	

It is evident that the ratio decreases with the magnitude, and the empirical formula thus calculated by the method of least squares that best satisfies these results is log. $\rho = .49974 - .03486\ m$., or, $\rho = 3.16\ (.9228)\ m$. Mr. Peirce's results by the same method give log. $\rho = .48 - .0151\ m$.

The following table shows the divergence between the observed and calculated ratios by my formula. The logarithms are given. It should be said that the magnitude, *m*, means the mean magnitude, *e. g.*, for passing from 3-2, the value of *m* is 2.5. As will be seen, the result when we plot the logarithms is a straight line, with a more decided inclination than that of Mr. Peirce. The resulting curve

Observed.	Calculated.
.44745	.44745
.4147	.41259
.3780	.37773
.3360	.34287
.3125	.30801
.27315	.27315

[1] The methods of deriving these averages is as follows: A[illegible]r deriving the (logarithm of) the ratio for passing from one magnitude to the next above in the way already described, all these results are grouped with their approximate weights and expressed in logarithms. The weighted average logarithm for each group, "2-1," "3-2," "4-3," etc., is calculated, and these form the uncorrected series of logarithms given in the table. The reasons for plotting the logarithms rather than the ratios themselves is that the logarithms form the simpler mathematical curve, a straight line, for comparison, and in addition obviate the distinction between the ascending ratios and their reciprocals, the descending ratios.



as well as the theoretical curves of Mr. Peirce and myself are given in the figure, q. v.

It should be added that facts favoring this interpretation of Fechner's law exist in other kinds of sensation; but perhaps not in sufficient quantity to allow of a quantitative determination such as is here made. It is universally admitted that the accuracy of Fechner's law (or Weber's law, for from this point of view the two are simply two different modes of experimenting) suffers a deviation both when the stimulus becomes very intense and when it becomes very slight. A more or less extended intermediate region in which the law holds is generally supposed, and the deviations at the extremes, which are admitted to be of opposite natures, would then form a broken line somewhat like this—

. But it is certainly more natural to suppose that the curve is more regular and can be represented by a straight line inclined throughout. Such is the result which the consideration of star magnitude suggests and formulates. Whether and in what way this result is to be taken into account by astronomers must be left to them to decide.[1] To the psychophysicist this method of testing the law is of very especial interest, amounting almost to a new psycho-physic method; even

[1] The astronomers have generally adopted the ratio whose logarith is .4, *i. e.*, 2.512, as proposed by Pogson. The average of the above values is .3603 (or 2.293). If we confine ourselves to stars down to the fifth magnitude the average is .39635 (2.492).

though it be one rough in its nature and limited in its applicability. The psychological processes involved in this kind of experiment differ so much from those employed in the more current experimental methods, that a comparison between the two is extremely valuable; and is made the more so as it is capable of furnishing the grounds of the validity of the inference from Weber's to Fechner's law.

The general conclusion reached by my investigation is that the law regulating the ratio of light between stars of one magnitude and those of the next above or below it, is the psycho-physic law as formulated by Fechner, with the modification, however, that the ratio in question, instead of being perfectly constant, decreases slightly with the brightness of the star, and may provisionally be regarded as following the empirical formula, $\log. \rho = .5102 - .0353\ m.$, where ρ is the ratio of the light of one magnitude to that of the next below it and m is the magnitude intermediate between the two between which the ratio is to hold. All this is claimed for stars down to the sixth or seventh magnitude only; what the law is for fainter stars remains to be determined.