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LETTERS TO THE EDITOR.

* * Correspondents are requested to be as brief as possible. The writer's name is in all cases required as proof of good faith.

The editor will be glad to publish any queries consonant with the character of the journal.

On request, twenty copies of the number containing his communication will be furnished free to any correspondent.

Right-handedness and Effort.

PROFESSOR JAMES replies in *Science* for Nov. 14 to my letter in the issue of Oct. 31, taking exception to my interpretation of my baby's use of her right hand only for strong efforts. Without summarizing the points at issue, I may indicate where it seems to me his explanation lacks force.

In the first place, I agree with him in all that he says about a "natural prepotency in the (brain) paths to discharge into the right arm." This is undoubtedly the explanation of right-handedness, as my observations would indicate as far as they go. I also agree with him in casting out the view that brings in conscious distinct memories and choices. They are a later development. There is nothing in my letter to indicate such a view. On the contrary, I accept the "semi-reflex" theory of the possibility of the use of either hand. But quite apart from these facts of the nervous basis, the question arises: What is the least difference in consciousness required to explain the preferential use of the right hand when effort is involved?

Now, Professor James kindly says that my observations "show how strong stimuli may produce more definitely localized reactions than weaker ones. The baby grasped at bright colors with the right hand almost exclusively." So far clear enough. But whenever the same stimulus, say a piece of common newspaper, was used in two experiments, at ten and at fourteen inches distance respectively, the same "more definitely localized re-action" took place in the second case; but in this latter case the stimulus which produced this "more definitely localized re-action" was fainter, being farther away, and the other conditions being the same in the two experiments. The child always used the right hand for long distances, even when the objective amount of stimulus remained the same. The least inference, I think, is that the intensity of the stimulus is not, at any rate, the exclusive cause of the more definite re-action. Greater intensity might account for the use of the right hand in some cases, but we certainly cannot hold at the same time that lesser intensity accounts for it in others.

The new element must represent the influence of former experience. I see no way to avoid this alternative. This is what I meant by "memories," merely some kind of a conscious modification which alters future re-actions. A purely physical modification would not suffice, for it would have its full force also in cases which involved no effort. Now, we may hold that such "memories" are exclusively of afferent nerve processes, or that they involve also a conscious modification due to efferent nerve processes. If the former, we may attribute them to the greater "promptitude, security, and ease" of right-hand movements, as Professor James suggests, or to former movements of the eyes, involved in the visual estimation of distance (which I am astonished he does not suggest). The first alternative, which Professor James asks my ground for rejecting, is inadequate for the following reasons. If such memories of afferent processes be of movements with effort, they are already right-handed, and the question is only thrown farther back; but, if they be of effortless movements, then their motor influence would be perfectly indifferent, as I said in my former letter. My experiments show this. If there had been differences in "promptitude," etc., the child certainly would have shown preference for the right hand in effortless movements during the latter six months of the first year. But, on the contrary, it was only when making violent effort that there was any preference at all. Even after she developed such preference in cases of effort, the use of her hands when no effort was required continued to be quite indifferent. Does not this indicate that the traces left by former afferent processes of the same sense are not sufficient?

Moreover, in the absence of all feeling of the efferent current, what could sensations of "promptitude," etc., be but the consciousness of better adaptation and co-ordination of movements? But at this stage of life all the child's movements are so ataxic, that there seems to be no practical difference between the two hands in regard to the lack of the tactile delicacy in which pathological cases show motor ataxy to consist.

If we seek for the needed "memory" among the sensations of eye-movements in the case where the stimulus is weaker (more distant), it is possible that we may find an afferent element which brings up the intensity of the hand-memories to the necessary pitch. There may be a connection between the centres for feelings of eye-movement and feelings of hand movement, so that their united "dynamogenic" influence is the same as the high intensity of the color stimulus. But, while freely admitting such a possibility, it only pushes the question farther back again; for how do we know that these eye-memories do not involve consciousness of the efferent process which innervates the eye-centre? And, besides this, there is another element in the hypothesis that afferent elements from other senses may furnish the "kinæsthetic co-efficient" for a given voluntary movement; namely, that such activities of the other senses invoked took place along with movements of the attention, which might, and probably do, contribute an efferent element to consciousness. This possibility I have never seen anywhere recognized.

But in this case my experiments show conclusively that eye-movement memories did not re-enforce the intensity of the arm-movement memories; for, when the distance was more than fourteen

inches, the re-action was inhibited altogether. The distance of the stimulus as apprehended by the eye, therefore, instead of giving the increased motor excitement which we require, rather diminishes it, and makes the need for some other explanation all the more imperative.

It appears, therefore, that the element needed in consciousness to explain the facts cited in my former letter is some kind of a difference in sensation corresponding to the outgo of the nervous current into the right arm, be it as vague, subconscious, and unworthy of the name of "memory" as you please; that is, I still think that my experiments support the traditional doctrine. On any other theory, right-handedness would have been developed independently of effort.

J. MARK BALDWIN.

Toronto, Ont., Nov. 18.

Mount St. Elias.

It is with great reluctance that I return to the subject again, but I beg to be permitted two statements in regard to the matter recently a subject of discussion between myself and Professor Heilprin in your columns.

In the first place, I did not "unfavorably criticise" Professor Heilprin's "work in Mexico." I merely pointed out that he assigned a weight to the observations which his equipment afforded which that class of instrument (viz., a pocket aneroid) is not entitled to, and that the result of such observations (as to the accuracy or inaccuracy of which I raised no question) is not determinative within the limits he assumed.

In the second place, discussion, in order to be profitable, especially in such matters as measurements and methods, must be just and accurate as well in the representation of an adversary's position as in the statement of one's own. In cases where the mutual recognition of this obvious truism is impracticable for any reason, I feel that it is better to cease the discussion, even though it leaves me apparently worsted in the argument. As a matter of fact, Professor Heilprin's understanding of the work printed in the St. Elias report ("Coast Survey Report for 1875") is hopelessly inaccurate and confused; and to that report, therefore, I refer those who are competent to judge of such matters, and may care to possess themselves of the facts in the case.

WM. H. DALL.

Smithsonian Institution, Nov. 22.

Annular Phase of Venus.

An opportunity of observing an unusual, if not remarkable, phenomenon will soon occur; and I wish to call the attention of astronomers to it, as another opportunity will not present itself until after the lapse of eight years. This phenomenon may be conveniently called the annular phase of the planet Venus, though it be produced not by reflected light only, as in the ordinary phases of the moon, but partly also by the refracted light of the sun, which has passed through the planet's atmosphere. This phase I unexpectedly witnessed twenty-four years ago under the following circumstances:—

I desired to observe the prolongations of the cusps of the crescent of light, mentioned by several writers, and which I afterwards found had been observed by Mädler in May, 1849, and used by him to obtain the amount of refraction in the atmosphere of Venus; but I had not then read his paper on the subject and was unacquainted with his formulæ.

It was well known, that, if Venus and the earth at any time occupied certain relative positions in their orbits, they would return very nearly to the same points, after an interval of eight years less two and a half days. It was also well known that Venus would transit the northern part of the sun during the forenoon of the 9th of December, 1874 (civil day at Greenwich), and would transit the southern part eight years less two and a half days later, or during the afternoon hours of the 6th of December, 1882. It was therefore evident that it would pass north of the sun, and very near it, eight years less two and a half days before the first of these transits, and would approach nearest to the sun about 2 P.M. (Greenwich time) on the 11th of December, 1866, least dis-

tance of centres being about 38' of arc. I therefore prepared to observe the planet on the forenoon of that day.

My observations were made in the open air, on the grounds of the College of Charleston, with a telescope presented to the college many years ago by William Lucas, Esq. This telescope is a refractor by Troughton & Simms, 5 feet focal length, 3½ inches aperture, eye pieces used magnifying 70 and 120 diameters. I so placed my telescope that the apex of the north gable of the library building, 23 yards distant, screened its object-glass from the rays of the sun; and the planet was easily found and distinctly seen above the roof of the library, least distance of nearest limbs about 29'. To my surprise, even astonishment, I saw not merely two cusps prolonged, but the whole circumference completely enlightened, the disk of the planet surrounded by a ring of light, broadest on the side nearest to the sun, narrower but quite bright on the opposite side. To have additional testimony to this fact, I immediately called to witness it Messrs. E. T. Frost and W. St. J. Jervy, two students in my astronomical class. They at once recognized the illuminated circumference, and said that it resembled in form the annular eclipse of the sun in October, 1865, which they had seen in this city in the preceding year. As said above, I was at this time unacquainted with Mädler's observations and formulæ, and, not having seen any intimation of the possibility of such a phenomenon, it took me wholly by surprise. I continued to watch the planet from 9 to 11 A.M., when the library building ceased to be available as a screen. This interval includes the instant of nearest approach of centres, which occurred about 9.30 A.M., Charleston mean time.

As far as I can learn, the only other persons who saw the phenomenon at that time were Professor C. S. Lyman of New Haven, Conn., and a few of his friends. In his equatorial of 9-inch aperture he saw the annulus or ring on the 10th completely formed; but the line of light on the side farthest from the sun was slender, faint, and only seen by glimpses. He saw it again on the 12th, but did not attempt to observe it on the 11th, the day of conjunction, when I saw it as a brilliant ring of light. He doubtless would have succeeded perfectly if he had abandoned the equatorial, which could not be screened, and used a more portable telescope, with some building as a screen.

In 1874 I watched the planet at intervals from the 30th of November to the 12th of December, the transit taking place on the night of the 8th and 9th, Charleston civil time. On the 2d of December I saw for the first time during this interval the distinct prolongation of the cusps, and watched their increase from day to day until the 8th, making eye-estimates of the number of degrees in the enlightened portion of the circumference, as I had not efficient means for making micrometer observations. On the 8th and the 9th I fully expected again to see the annular phase, but failed entirely to find the planet on both days. There were no clouds, at least not sufficient to entirely prevent observations, but there was a dense haze, and the region near the sun was strongly illuminated.

At this transit Mr. Lyman was more successful than myself, making good micrometer observations of the enlightened portion of the circumference, and seeing distinctly the illuminated ring on the 8th, the day before the transit. On the 9th he was, like myself, wholly unsuccessful in finding the planet, but on the following days continued his micrometer measures. The results of these observations he published in the *American Journal of Science and Arts* for January, 1875, with the amount of refraction in the atmosphere of Venus deduced from his observations, and also Mädler's formulæ by which it was deduced.

In December, 1882, the weather was so unfavorable on the day of the transit, the 6th, and for several days preceding and following, that I made no attempt to observe it before and after conjunction, and no accounts of the observations of others have reached me; but the scientific periodicals to which I have access are so few, that it would be unwarrantable to say that none have been made.

The next opportunity for observation will occur eight years less two and a half days after the last transit, that is, on the 3d of December next, when the least distance of centres will be about 35', at about 5.30 P.M., Greenwich civil time. As Venus will