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 distance 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, 3¾ 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 22′. 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. Jervey, 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 pass south of the sun, it will not be so easy to use buildings as screens in the northern hemisphere, and special means must be devised. The luminous ring will be as bright and conspicuous as in 1866, and the first appearance of the prolongation of the cusps may be looked for about the 24th of November.

It is now evident that similar opportunities will happen on the 1st of December, 1898, when least distance of centres will be about 1°, and about the 28th of November, 1906, when least distance of centres will be about $1\frac{1}{2}$°, the planet in both cases south of the sun. In each case the least distance of centres will be less than the limit within which the formation of the luminous ring is possible, but the duration of the ring will be successively less as the least distance between centres becomes greater. No other opportunities will present themselves until near the end of the next century, when they will occur in June.

Similar opportunities must have occurred in years preceding 1866; that is, on the 14th of December, 1858, and also on the 16th of December, 1850; but it does not appear that either was used. This last date is only nineteen months after Mädler's observations in May, 1849; and, if any one properly situated as to time had endeavored to repeat Mädler's observation on the day of conjunction, he would almost certainly have seen the luminous ring.

LEWIS R. GIBBES.

Charleston, S.C., Nov. 13.

A Problem in Physics.

AN experiment was tried by Joule nearly fifty years ago which has attained a world-wide reputation, and which has crept into nearly every text-book of physics. The commonly accepted interpretation of it, however, would seem not entirely satisfactory. I will quote from Tait's description of the experiment.

"Joule took a strong vessel containing compressed air, and connected it with another equal vessel which was exhausted of air. These two vessels were immersed each in a tank of water. After the water in the tanks had been stirred carefully, . . . a stop-cock in the pipe connecting the two vessels was suddenly opened. The compressed air immediately began to rush violently into the empty vessel, and continued to do so till the pressure became the same in both; and the result was, as every one might have expected, that the vessel from which the air had been forcibly extruded fell in temperature in consequence of that operation. It had expended some of its energy in forcing the air into the other vessel; but that air, being violently forced into the other vessel, impinged against the sides of that vessel, and thus the energy with which it was forced in through the tap was again converted into heat. On stirring the water round these vessels, after the transmission of air had been completed and the stop-cock closed, Joule found that the number of units of heat lost by the vessel and the water on the one side was almost precisely equal to the quantity of heat which had been gained on the other side." Tyndall gives the following (let B represent the vessel in which the air was compressed to 22 atmospheres, and A the vessel which was exhausted): —

"Now, the air, in driving its own particles out of B, performs work, . . . and the air which remains in B must be chilled. The particles of air enter A with a certain velocity, to generate which the heat of the air in B has been sacrificed; but they immediately strike against the interior surface of A, their motion of translation is annihilated, and the exact quantity of heat lost by B appears in A. The contents of A and B mixed together give air of the original temperature. There is no work performed, and there is no loss of heat." Tyndall gives an illustration of a cylinder having a piston in the centre, and the space above the piston a vacuum. Suppose the air below the piston is heated up from 0° to 273° C. "If the pressure were removed, the air would expand, and fill the cylinder. The lower portion of the column would thereby be chilled, but the upper portion would be heated ; and, mixing both portions together, we should have the whole column at a temperature of 273°. In this case we raise the temperature of the gas from 0° to 273°, and afterward allow it to double its volume. The temperatures of the gas at the beginning and at the end are the same as when the gas expands against a constant pressure, or lifts a constant weight; but the absolute quantity of heat in the latter case is 1,421 times that employed in the former, because, in the one case, the gas performs mechanical work, and in the other not."

The following quotation is from Balfour Stewart, and bears upon this question: —

"The prevalent idea is, that when air expands it becomes colder, and that when condensed it becomes hotter; but Joule, by experiment, has shown that no appreciable change of temperature occurs when air is allowed to expand in such a manner as not to develop mechanical power. It follows as an inference, that, when air is compressed, the rise of temperature is scarcely at all due to the mere diminution of the distance between the particles, but almost entirely to the mechanical effect which must be spent on the air before this condensation can be produced."

A final quotation is taken from Ganot's "Physics:"—

"A strong metal box is taken, provided with a stop-cock, on which can be screwed a small condensing-pump. Having compressed the air by its means as it becomes heated by this process, the box is allowed to stand for some time, until it has acquired the temperature of the surrounding medium. On opening the stop cock, the air rushes out; it is expelled by the expansive force of the internal air : in short, the air drives itself out Work is therefore performed by the air, and there should be a disappearance of heat; and, if the jet of air be allowed to strike against a thermopile, the galvanometer is deflected, and the direction of its deflection indicates a cooling. . . . Joule placed in a calorimeter two equal copper reservoirs, which could be connected by a tube. One of these contained air at 22 atmospheres; the other was exhausted. When they were connected, they came into equilibrium under a pressure of 11 atmospheres; but, as the gas in expanding had done no work, there was no alteration in temperature."

I have given these quotations rather freely from standard authors, in order to present the problem as clearly as possible. In order to arrive at just the action taking place in this experiment, it seems to me a phenomenon first described by Faraday in 1827 should be mentioned. Gas compressed to 30 atmospheres was allowed to suddenly enter a cylinder 30 feet long, in which the gas was at atmospheric pressure presumably. It was found, that, where the gas rushed in, the cylinder was much cooled, while at the other end it was heated. It would seem that in this case the heating was not produced by the particles of gas impinging upon the end of the cylinder. If a piston were placed in front of the expanding gas, the whole of the gas on the other side of the piston would be compressed and heated. If, now, instead of a piston, we open a stop-cock at the end of the cylinder, the gas would stream in and compress that already there, and heat it; but the gas, expanding violently as it enters, would be much cooled, and this would more than counteract the heating where it enters. Thus the farther end would show a heating, while the end at the orifice would show a cooling as observed. Have we not precisely analogous phenomena in Joule's experiment? For a very small fraction of a second (perhaps .0001) after the stop-cock was opened, there would be a partial vacuum in A, into which the air streams; but after that the particles would not impinge upon the sides of A, but would have their velocity diminished and finally overcome by striking other particles. In imparting this velocity, the particles in B would be slightly chilled. The air, in streaming out of B, would be cooled by expansion after an instant, and would serve to cool the end of A near the orifice, as we have just seen; also the chilled particles in B would stream into A, and thus cool it still more. Whatever may be the action in these vessels, it is certain that the final heating in A, and cooling in B, would be exceedingly slight as shown by Joule's experiment, though it does not seem that the popular explanation is entirely correct.

It seems to me this question of the action of air in Joule's two vessels is an intensely interesting one. The conclusion that the chilling of the air in the vessel due to the work of imparting a velocity to its particles is very slight, corroborates in a marked manner the experiments tried by the present writer, in which he found a cooling of four degrees, while the dynamical cooling should have been ten times greater. The quotation from Ganot shows precisely an analogous case.

H. A. HAZEN.

Washington, Nov. 17.