union of these narrowed sepal-leaves in the monopetalous calyx is induced by the highly refined sap circulating in a part of the plant so distal, so remote. "In the calyx, Nature produces nothing new. She simply unites and changes parts already familiar, and so rises by one step nearer her destination."

Now, as the calyx owes its character to the refinements of organs preceding it, so it in turn becomes an organ of filtration; and from the crowded vessels of the sepal comes the pure liquid which makes possible the petal. Colored petals indicate that the nourishing fluid has reached a high degree of refinement, white, of absolute purity. The equivalency of sepal and petal is shown by the usual comparison, Goethe adding only that as the sepal evinces contraction, so does the petal expansion, and we are thus prepared for the last degree of contraction in the formation of the stamen. The foliar nature of the latter organ, as of the carpels, is shown in the familiar way. The petaloid style of the iris is cited, as also the reversion so common among crowfoots, tulips, etc.

But it is to the nectary (a very comprehensive term in Goethe's time) that the poet here gives chief attention. Nectaries seem to occupy an intermediate place between stamens and petals, now partaking of the character of one organ, now of the other. Goethe inclines to the opinion that nectaries are undeveloped stamens. He is certain, at least, that the nectar itself is concerned in the fertilization of the flower; for, "after fertilization, the nectar disappears, and no more is formed." He conceived that the nectar might be an imperfect fertilizing fluid; that the pollen-grains are so many minute vesicles containing an exceedingly delicate matter elaborated by the vessels of the stamen, and destined to be absorbed by the pistil.

Think of all this in view of the modern doctrine of fertilization and cross-fertilization, and you begin to appreciate the inventive genius of the man, hovering about his subject, and almost divining that which he could not clearly see, and then his conclusion: "The forms of plants about us are not originally determined and invariably fixed, but unite with some persistence of generic, specific, and individual character, a fortunate power to vary and to accommodate themselves to circumstances, so as to be able to meet and adapt themselves to the varying conditions which come upon the earth." Darwin might have written it: Darwin could have said no more.

Such is a brief outline of Goethe's contribution to botanical science. The importance of his discovery can scarce be over-estimated, while its beauty can be appreciated only after careful examination, not only of the discovery itself, but likewise of the manner of its attainment. As to the latter, we are happily not left in doubt. The poet has himself given us a concise account of every step of his progress. We may see the impassioned enthusiasm of Linné stimulating society universal, so that Goethe says it fairly 'floated him along.' What an era in which to live, — the very springtime of science, the air redolent of odors of the life to be! Even petulant, passionate Rousseau forgets for the nonce his dreams of social compact, and, charmed with the beauty of the living world, becomes botanist, and even begins a botanical dictionary. The spirit of investigation was abroad, and Goethe entered his morphological studies with all the energy of his nature. His ideas took possession of him. The voice of Nature cried: he could not choose but hear. His work became a passion, a *leidenschaft* he calls it, from which even the marbles of Italy and the palaces of the 'eternal city' could not divert him. He was a poet; but he suddenly found out that Nature, too, is poetic, and that even her most gifted child has nothing that he has not received, nor has so much that he may not crave and receive the more. The facts of Nature were before him: his thought responded to the thought in Nature. It seemed as if it were so. It must be so. It was so.

It is said that each individual lives in epitome the life-history of his race. May it not be said that in this brief sketch of the rise of a single scientific doctrine we have found those agencies which make possible all and every science, — the light of the eye, the light of the intellect, and the vivid brilliancy of the imagination, — Linné, Wolff, Goethe, — the systematist, the student, and the poet?

T. H. McBride.

AN ATTEMPT TO PHOTOGRAPH THE SOLAR CORONA WITHOUT AN ECLIPSE.

Since writing my last communication on this subject, I have made a series of investigations with the object of improving my apparatus, if possible, and of obtaining some quantitative measurements of the light reflected by the atmosphere near the sun. To avoid the reflection of the light from the surfaces of the glass, I removed my achromatic lens, and substituted for it a simple spectacle-glass of one and three-eighths inches diameter, and forty-nine inches focus. As the diameter was relatively so small, the inclination of the surfaces to one another at any point was necessarily very slight; and this fact, combined with the extreme thinness of the glass, rendered the multiple internal reflections almost entirely harmless. Five diaphragms were used between the lens and plate, cutting off totally all internal reflection from the tube. The same drop-shutter was used as before, working just in front of the lens.

Several observers have attempted to compare the brilliancy of the corona as seen during a total solar eclipse with that of the full moon. Thus Belli in 1842, and Wilson in 1860, find the corona brighter than the moon; while Halley in 1715, Billerbeck in 1851, and Curtis in 1869, find the moon brighter than the corona.[1] Other observations by W. O. Ross in 1870, and by J. C. Smith in 1878,[2] would indicate that the corona was somewhat brighter than the full moon.

A photograph of the solar corona in order to be of any use whatever should show something more than a thin uniform ring of light around the sun. It should show some structure, some details of the rays and rifts visible at the time of an eclipse. The only observations which I have been able to find of the intrinsic brightness of different portions of the corona, were those by Professor John W. Langley in 1878.[3] He found that the corona at 1′ distance from the sun had a brilliancy equal to six full moons, while at 4′ distance it was only equal to .1 of a full moon. Unfortunately for our present purpose these observations were visual, and not photographic; but they will give us an idea of the size of the figures with which we are dealing. In order, then, for a photographic plate to show any of the irregularities of detail in the corona, such as the contrast between a ray and one of the neighboring rifts, it must be capable of showing contrasts of light which do not vary from one another by more than about one-tenth the intrinsic brilliancy of the full moon.

A series of investigations was next instituted to determine what excess of brilliancy it was necessary for one surface to have over another, in order that the contrast between them might be rendered perceptible in a photograph. The difference of brilliancy required by an acute eye amounts to between one and two per cent. One sees it sometimes stated that the camera is capable of distinguishing contrasts which are quite invisible to the eye. This, I think, is a serious error. If both sources of light are of great brilliancy, though differing considerably, the eye may not perceive a very great difference between them; while a negative with a very short exposure may show a very considerable difference. If, however, the eye be protected by colored glasses, the contrast will be as great as that presented by the negative.

But the question which occurs at present is not where great differences of light exist, but where the differences are very small, of only a few per cent. The inferiority of the best gelatine plates to the human eye in this respect is very readily shown by an attempt to photograph distant mountains. It will be found that it is perfectly impossible, even in the clearest weather, to photograph at a much greater distance than fifty or sixty miles. I recently, on a very clear morning, made a mountain ascent with a camera. Mountains over ninety miles distant were readily recognized and distinguished; but, on pointing the camera in their direction, nothing over forty miles distant could be photographed. There was no question but that mountains at a much greater distance than ninety miles could have been seen, had there been any high enough to be visible. Every photographer who has visited mountain regions is perfectly aware of the disabilities under which he labors in this respect.

Another illustration of the same thing is the impossibility of photographing the moon in the daytime, when the sun is high above the horizon. Although the moon may be perfectly distinct to the eye, the negative shows no trace of it. This fact of itself, I think, has a direct bearing on the question in point.

But in addition to these general facts it was thought that some quantitative results would be desirable. Besides the chloride plates which I had been using, several well-known kinds of bromide plates were tested at the same time. These were selected with especial regard to the strong contrast qualities which they were supposed to possess. The plates tested were the Anthony chloride, the Carbutt B, the Allen and Rowell, and the Stanley. Different portions of the plate were exposed to a uniform illumination for various times, and it was found that all the plates gave about the same result, and that if the division lines between the areas were very sharp, and over an inch in length, as small a contrast as five per cent could be detected; but if the division lines were not over one-eighth of an inch in length, even if one knew just where to look for them, it was impossible to recognize a difference of less than ten per cent upon the negative. As the coronal rays on the photograph would be less than one-eighth of an inch in length in order to reach out beyond 3′, ten per cent was selected as the limit of contrast necessary to obtain a satisfactory result.

Since the light reflected by the corona at 3′ distance from the sun is only .1 that of the full moon, in order to distinguish between a coronal ray and a neighboring rift at that distance, it is necessary that the light reflected from the earth's atmosphere in that region should not exceed in intrinsic brilliancy that reflected by the moon itself.

A series of observations was next made to determine the relative light of the sun and of the sky in its immediate vicinity. The method employed was as follows: Half of the photographic plate was covered with thick yellow paper; a diaphragm of .016 centimeter in diameter was placed in front of the lens, and four different exposures made to the sun on different parts of the plate, lasting respectively for two, four, eight, and sixteen seconds. The plate was then taken into the dark room, and the exposed portion protected by the yellow paper, which was re-

[1] *Memoirs of the Royal astronomical society*, vol. xli. pp. 243-253.

[2] Washington observations, 1876: Appendix iii., p. 387.

[3] Washington observations, 1876: Appendix iii., p. 211.

moved from the other half of the plate. The telescope was now so placed that the sun should be hidden behind a paper disk, fixed at about twenty feet distant. A diaphragm of one centimeter aperture was placed in front of the lens, and an exposure of four seconds given to the sky. On development, half of the plate, except where cut by the image of the disk, was found uniformly darkened. On the other half were four images of the sun, two of which were lighter, and one darker, than the sky. The third image of eight seconds exposure was of exactly the same darkness as the sky; and it was accordingly shown, that since the diaphragm used with the sky was about four thousand times larger, the sun was about two thousand times as bright, photographically, as the sky in its immediate vicinity. A number of plates were taken on different days, when the sky seemed perfectly clear, and the results indicated that the number varied in general between a thousand and four thousand. Owing to the diffraction produced by the small diaphragm used in photographing the sun, which rendered the image 1.6 times larger than it really should be, all these figures must be multiplied by 1.6.

Comparisons were then made in a similar manner between the sky near the sun, and the full moon, the latter taken with the full aperture of the lens, 3.65 centimeters, and the former with an aperture of .204 centimeter. Under these circumstances, with exposures of fifteen seconds, the moon and sky darkened the plate to about an equal amount. The result of a number of experiments indicated that the sky in the immediate vicinity of the sun was of about four hundred times the intrinsic brilliancy of the full moon. The ratio of the sky to the sun on this same day was fifteen hundred, so that the light of the moon was to that of the sun as one to six hundred thousand. In some experiments which I made in 1879,[1] I found the visual ratio was one to three hundred and fifty thousand. On account of the extreme blueness of the sun, it was to be expected that the photographic ratio should be somewhat higher than the visual one.

I next tried comparing directly the light of the sun and moon on the same plate, in order, if possible, to get a check on my results. The results, however, were unsatisfactory, the ratio coming out as 1 to 300,000, or only one-half the former amount. Owing to the difficulties of the experiment, this discrepancy may very well be referred to inaccuracies of the photographic plate, and changes in the sun's and moon's light during the course of the experiments. In all the results with regard to the sun, it must be remembered that the figures must be multiplied by 1.6, on account of diffraction. The two ratios, then, of the light of the moon to that of the sun, stand as 1 to 960,000, and 1 to 480,000; and of these, I think, in connection with my visual result, the former is the more correct figure. The moon at the time of these observations, June 26, 12 M., had an altitude of 29°, when the atmospheric absorption would amount to about twenty per cent.[1] Making this correction, we have the photographic ratio of the moon to the sun, as 1 to 760,000, or about twice as great as that to the eye. This is, of course, only an approximate result, as only very few observations were made, and as it was entirely outside the course of our inquiry.

[1] *Proceedings of the American academy of arts and sciences*, 1880, p. 246.

Returning, then, to our original subject, we found the sky near the sun 400 times as bright as the full moon. Correcting for atmospheric absorption, this figure becomes 320 times. But we found before, that in order to detect the contrast between a coronal ray and a neighboring rift, the light of the sky must not exceed that of the full moon. It therefore seems that even in the clearest weather the reflected light of the atmosphere is 300 times too strong to obtain the faintest visible image of the true coronal rays.

In connection with these experiments, I took a few photographs of the sun with my improved apparatus. In order to still further diminish the reflection of the light from the surfaces of the lens, I so placed the telescope that the sun was almost completely hidden behind the high steeple of a neighboring church. A vast improvement in the results was at once obtained. The sun stood out sharply defined on a perfectly uniform background of blue sky. There was not the slightest trace of a fringe either where the steeple crossed the disk, or where the sky came in contact with the solar limb. The day was beautifully clear, and at six in the afternoon some more photographs were taken; but now, although the steeple was as clear as ever, all around the limb of the sun appeared the atmospheric halo, extending out in all directions, and gradually growing fainter as it receded from the sun. We may, therefore, in general, say, that with properly constructed apparatus, in perfectly clear weather, no halo whatever appears around the sun. It is only in slightly hazy weather, or as the sun approaches the horizon, that the appearances are produced which have been elsewhere described.

In brief, the result of my researches would seem to indicate, 1°; that without a total eclipse it ought to be impossible to photograph the solar corona, 2°; having tried, I have failed to photograph the corona, but have obtained the result which theory indicated.

WM. H. PICKERING.

[1] *Annals Harvard college observatory*, vol. xiv. p. 62.

STEINEN'S EXPLORATIONS OF THE XINGU.

DR. KARL VON STEINEN has recently made a most interesting report of his explorations in the Matto Grosso,— the immense region, more than four times as large as France, which occupies a large part of central and western Brazil, and is hardly known to geographers except in the most imperfect manner. It is divided by great rivers, of which the Madeira, the Tapajos, the Xingu, the Araguaya, and the Tocantins flow northward, and the Paraguay flows southward. It is watered by innumerable streams which unite with these rivers, along whose banks live thou-