notice of 'Piazzi Smyth,' by Ralph Copeland. Notes as usual are published on variable stars, planets and current spectroscopic work.

DISCUSSION AND CORRESPONDENCE.

NOTE ON THE SILURO-DEVONIC BOUNDARY.

IN the recently published bulletin of the U. S. Geological Survey, No. 165, entitled 'Contributions to the Geology of Maine,' Professor H. S. Williams has again defined his attitude on the question of the Siluro-Devonic boundary in America. Here the critical argument advanced with some emphasis for the construction of the Helderbergian as a Siluric fauna is given in the following words (p. 25):

"The boundary between the Silurian and Devonian systems was first made in the Welsh series, in which the transition was from calcareous sedimentation, with rich and purely marine faunas, into sandstones of great thickness containing land plants and fishes whose habitat was, presumably, fresh or brackish waters.

"The New York section, from the Lower Helderberg limestones through the Oriskany, Cauda-galli, and Schoharie grits back again into limestones, does not pass out of marine conditions. In the Gaspé region, however, there is a complete change (as there was on the other side of the Atlantic Basin) at the point where the Oriskany fauna was evolved. [NOTE A.] In these Silurian faunas of the eastern province there is also much closer resemblance to the Wenlock-Ludlow series than is found in the faunas of the Appalachian province in New York. The correlation of the passage beds at the top of the Silurian of Wales is clearly to be recognized in the passage from the limestones to the Gaspé sandstones of the eastern province of America. This Gaspé transition is also to be traced with precision to the horizon of the introduction of the Oriskany fauna into the basins farther west and southwest, in which no direct passage into Old Red sandstone condition is apparent.

"We have thus in America a means of determining where the Silurian boundary belongs in purely marine series of beds and among marine faunas of unbroken succession. The Lower Helderberg in the interior of the American continent, as the Koniprusien F_2 fauna in the Bohemian Basin of Europe, is closely related in its species to what succeeds, because there was no radical disturbance of the conditions of marine life. Nevertheless, it is not the Lower Helderberg species that mark the conditions corresponding to the beginning of the Old Red sandstone; but the changes which that fauna suffered during the passage into the Oriskany time are evidences of a general disturbance which resulted in the lifting of large areas of marine surface above the level of the sea." [NOTE B.]

Note A.—This statement is wholly inaccurate. In the Gaspé limestones the Oriskany fauna manifests itself pronouncedly with *Hipparionyx proximus*, *Rensselæria ovoides*, *Megalanteris*, *Camarotœchia pliopleura*, *Rhipdomella*, cf. *musculosa*, *Meristella* cf. *lata*, etc., at the base of Logan's limestone No. 8, and above this horizon is the great thickness of 500–600 feet of pure limestone beds with chert bands, surprisingly similar in lithologic aspect to the gray and chocolate-colored Onondaga limestones of New York, and throughout these beds such Oriskany species are found in association with a profusion of others not represented in the interior basin Oriskany and many of them closely comparable to species of the Helderbergian. The plane of reappearance of certain Oriskany species in the Gaspé sandstone above, was shown by Logan to be 1100 feet above the top of these limestones and to be restricted to a comparatively slight vertical range. The fossils of the sandstone are not abundant nor is the fauna diversified. To any one studying these relations on the ground it is clear that they represent a brief return of the fauna of the limestone with evidences of progression and the further intermixture of species from the interior province (*Rensselæria* cf. *ovoides*, *Chonostrophia dawsoni* and *Chonetes melonica* (both of the latter in the New York Oriskany), *Leptostrophia blainvilli*, cf. Oriskany species, *Orthothetes becraftensis*, *Phacops* probably identical with *P. anceps*). The evidence from the Gaspé series is potent and conclusive that the introduction of the Oriskany fauna was accompanied by the deposition of pure calcareous sediments which were continued for a protracted period and nearly equal in actual thickness, the sum total of the Helderbergian and Onondaga limestones in the New York area of the interior basin. The species cited are in themselves evidence of the wide transgression during Oriskany time which is especially noticeable in the distribution of the sediment in New York. No tectonic change disturbs the succession in the 2000 feet of Gaspé limestones.

The fauna alone shows that during the earlier part of the period of their deposition the eastern province was more completely secluded from the interior sea than during its later stages. The Canadian geologists have expressed the probability that the 7000 feet of arenaceous sediments comprising the Gaspé sandstone may represent the major part of all subsequent Devonic deposition in that region.

Note B.—The fauna of the Helderbergian passes upward into the deposits of the Oriskany without abrupt or profound change. Its species perdure, but progress and definition are evinced in the later fauna by the introduction of many distinct types.

The prevailing conception of the Oriskany as a purely arenaceous deposit and which figures largely throughout this argument is one which needs readjustment. The normal fauna of the Oriskany of New York is that of the calcareous beds of the eastern part of the basin. These beds always contain a considerable content of silica in the form of sand, but they are clearly the deeper water deposits of which the sandstone beds of the typical Oriskany section and the intermediate thin bands of altered sandstone (quartzite) are the shallow water shore-line deposits. The sandy layers of the Oriskany only share the fauna of the calcareous beds, and it is quite clear that their species have been derived from the deeper water centers of dispersion largely through mechanical agency. It is therefore not competent to argue a lower calcareous Oriskany and an upper arenaceous Oriskany, as, in New York, at least, there is but one Oriskany fauna, and the formation is not divisible into facies except geographically.

The foregoing notes indicate that the argument cited is built upon the sand. Nevertheless it is throughout that which served de Verneuil and Murchison above fifty years ago and through that agency produced its effect upon Hall's correlation of the Oriskany and Lower Helderberg. It is that argument too with no additions, summed up in the statement that Siluric time was closed with a general world-wide crustal elevation initiating rapid base-leveling and the accumulation of sandy deposits at the opening of the Devonic in all countries. To the recrudescence of this ancient doctrine the labors of Kayser, Frech, Tschernyschew and other European geologists upon the calcareous facies of the earliest Devonian in the Harz, Westphalia, Bohemia and the Urals afford no balm. The old hypothesis of cycles of sedimentation loses force when applied simultaneously to every part of the earth's surface, and cycles of sedimentation are not a basis of geologic classification save as some element therein indicates widespread orographic derangement. The argument as here constructed seems to be as follows: The grand event which terminated the Siluric was the universal elevation of the land, the erosion of which supplied the materials for the sandy sediments of the opening stages of the Devonic. This opening Devonic stage in the marine succession is impregnated with species of Oriskany type; 'the Lower Helderberg is therefore proven to belong to the typical Silurian system of the American Continent' (*op. cit.*, p. 26). Both premises limp and the conclusion falls. The deposition of sandy sediment was not contemporaneous in the early Devonic, but, however widespread, it may have been upon the epicontinental plateau, calcareous sediments of contemporary origin must have been present in the greater and less disturbed depths, retaining some of the pre-existing types, but showing freely the progressed and differentiated types of the new era. These relations of coeval faunas can be determined only upon the most careful analysis of organic content, and such analysis has cogently shown the intimate affinity of the Helderbergian with the calcareous Oriskany of which it is the immediate and purest calcareous predecessor in the vertical series. As in the Gaspé succession, so in New York, the species of Helderbergian time, notably unlike in the two separated provinces, pass, in each, into association with those of the Oriskany when by transgressing sedimentation and freer intercourse between the provinces a consequent commonalty of species was effected.

The succession in the Gaspé peninsula like that of the basins of Bohemia and the Urals again declares the ultimate and final authority of the fauna, its variations, progression and

specialization, in pronouncing upon a critical question in the classification of the fossiliferous rocks.

John M. Clarke.

THE PROBLEM OF COLOR.

Although I don't accept Professor Cattell's contention, in the last number of the *Psychological Review*, that the nugatory process by which two colored lights (if properly chosen in hue and in intensity) disappear for sensation and leave behind a sense of grayness only is due to a cortical and not to a retinal physiological process, I am nevertheless willing (in the interest of fair play) to furnish him with one more reason on his side. When a colored object is mirrored in a piece of colored glass (say red in blue), we get in general a color blend, that is, for consciousness, a reddish-blue sensation. In case the colors chosen are a pair which, on fusing, are transformed into something else (yellow and blue into white, or red and green into yellow), this is, according to all the non-psychical color-theories, because two counteracting color-processes in the retina are exactly balanced, or else because two partial photo-chemical molecular dissociations unite to complete each other and to produce an undifferentiated gray-process,—either of these suppositions being sufficiently plausible in itself. But—and this is the fact, if it is a fact, which works upon Professor Cattell's side—there are occasions upon which, according to Helmholtz and to Wundt, this antagonism, or this completion, fails to take place. One sometimes sees, they say, one color *through* the other; guided by the belief that the red sensation is due to the presence of a red book, *e. g.*, one cannot help but *see* the redness of the book through the sea of blue. They do not dwell upon the colors which they used in making the experiment—so long as these are red and blue there is nothing strange in the differing interpretations; but if, under these circumstances, blue and yellow should not give white (and red and green should not give yellow), then it would seem to follow that the antagonistic or the completing processes are not of the nature of chemical changes in the retina—such could not be so easily undone by the reasoning, or the perceiving, Psyche. Hering denies with great warmth the contention of Helmholtz and of Wundt that these exceptional cases occur; or rather, he says that if they do occur it is owing to spots or unevennesses in one or the other of the two surfaces. But even though she be assisted by any ulterior aids whatever, it would not seem that the Psyche can undo, in the interests of reasonable interpretation, a chemical change that has already taken place. Perhaps she can, however; but in that case her powers must also suffice to undo an *actual* white (or yellow) and separate it into its possible components. If, in the case of a blue book seen in a yellow glass, for a portion in the center of the surface of the book a gray of equal brightness be substituted, and a like gray for an exactly coinciding portion of the yellow reflector, then it is possible that self-deception would go so far as to enable us to see a continuous blue book in a continuous yellow mirror. The experiment is perhaps worth trying.

On the other hand (to be equally fair to my own side, in turn), the fact that *binocular* color mixture does not occur to any great extent—that is, does not occur for colors far apart in the spectrum—is at once destructive to any hypothesis which relegates the fusion of colors to the perception-forming centers of the brain. Whether an overlapping blue and yellow are mediated by one eye or by two can have nothing to do with the case if their mutual quenching is an affair of perception. Helmholtz, after a long series of the most painstaking experiments, declared absolutely that binocular color-fusion does not take place.* This shows, in passing, the unprejudiced character of his work, for the fact, as I have said, is quite destructive to his theory that the mutual suppression of blue and yellow into white is merely a matter of the judgment: it cannot make any difference whether we know that we see blue and yellow at once through one nasal half-retina, or through a nasal and a temporal half-retina together—the more so as we have in general

* Binocular color-fusing of two complementary colors many be obtained with the Hering color-mixer by 'long and steady gazing,' but this is the sufficient condition for turning each color into a dead gray, when looked at by itself.