NOTE ON THE SENSORY TRACT OF THE BRAIN.

By Edward C. Spitzka, M. D., New York City.

It is well known that Meynert (1) and those who followed that distinguished anatomist, believed that the tract through which the conscious sensory impressions reach the cortex, extends from the columns of Goll and Burdach, of the cord and lower oblongata, through the so-called superior or sensory decussation, to the anterior pyramids; that thence the tract runs with the anterior pyramids in their outermost third through the pons and pes pedunculi, courses between the thalamus and lenticular nucleus in the posterior third of the internal capsule, and arching back, terminates in the cortex of the occipital lobe. Flechsig showed that what Meynert interpreted as the sensory pyramidal decussation, has no connection with the anterior pyramids, but, on the contrary, enters the lemniscus layer, or interolivary strand, whose relations to the corpora quadrigemina had been explained by Meynert, although he was befogged as to its lower relations, owing to the aforesaid confounding with the anterior pyramids proper.

Now, Flechsig (2) distinctly states in his work that the explanation he has been able to furnish of the real nature of the superior decussation, demonstrates the non-existence of a direct tract from that decussation to the cortex.

The true tract has, however, been known to exist, although the relations have not been properly interpreted. The lemniscus layer is not only a detachment from the corpora quadrigemina, but also distinctly incorporates a peculiar bundle, described by Henle as a fasciculus, from the pes to the tegmentum(3). This tract continues, in at least a part of the fibres, from the columns of Goll and Burdach to the pes pedunculi and thence, no doubt, to the cortex of the brain. The circuit for the conscious sensory impressions transmitted by the cord, and proposed by Meynert, therefore becomes re-established, with a modification, namely, that the sensory tract does not run through the pyramids and pons, but immediately above them, and after entering the *pes pedunculi* probably takes the course claimed by Meynert.

That there is a close relation between the pyramidal tracts and the by-track from the superior decussation to the *pes pedunculi*, is proven by an interesting observation which I have been able to make on the elephant's brain. In this animal(4) the entire pyramidal tract takes the course of the by-track, that is, there are no vertical fibres in the pons. The crus is continued bodily above the latter (which is composed exclusively of transverse fibres) to take the usual course on the ventral and medial aspect of the olivary nucleus.

This fact strengthens the proposition of Meynert, that there intervenes a third projection series between that of the tegmentum and that of the pes pedunculi, for which he proposes the name of the *stratum intermedium* (5). In man, I believe this stratum intermedium to be the main tract for the conveyance of conscious sensory impressions from the general sensory periphery, while in other animals, at least in the elephant, it is at the same time the voluntary motor tract.

That the sensory fibres occupy the most posterior portion of the internal capsule, while they compose the most dorsal in the pes pedunculi, shows that the fibres of the latter must pursue a spirally twisted course betore entering the brain. Such an arrangement seems to be indicated, indeed, in the outer contours of the crus. In an early human embryo, of about the third month, I find a well marked columnar elevation running from the outer part of the crus through the pons, where it touches its fellow of the opposite side, and then passes between the olives(6). This I regard as the embryonically distinct stratum intermedium.

(1). Das Gehirn der Saügethiere, in Stricker's Histology.
(2). Die Leitungsbahnen des Gehirnes und Rückenmarks. 1875.
(3). Lehrbuch der Anatomie des Menschen. 1872.
(4). "Science," February 7, 1881. (5). Archiv fuer Psychiatrie. 1874.
(6). Demonstrated before the N. Y. Neurological Society, March 1, 1881.

ASTRONOMIAL MEMORANDA.

A small pamphlet containing notes, corrections, etc., to the "Handbook of Double Stars," has been recently prepared by Messrs. Crossly, Gledhill and Wilson. In the introduction, the editors say: "The corrections have been thrown into two classes: the first contains those which from their importance demand immediate attention in order to save waste of time. These the reader is requested to insert at once. In the second list will be found a large number of corrections which may be entered as the stars are observed or read.

A very copious set of additional notes has also been drawn up, embodying, so far as we know them, the most recent and improved orbits, measures and discoveries.

It seems probable that the asteroid, No. 220, discovered by Palisa on the 23d of March, is identical with No. 139, Juewa. Juewa was discovered by the late Prof. Watson while engaged upon one of the transit of Venus parties in 1874 at Pekin. The asteroid was observed by Rümker at Hamburg, on November 8th of the same year, but since that date it has not been seen.

Nature for March 17, contains the following note upon the largest refractor in the world. "A very interesting scientific work, the most important of its kind yet attempted in the kingdom, has just been completed. It is the great refracting telescope, constructed by Mr. Grubb, of Rathmines, Dublin, for the Austro-Hungarian Government, and it is to be placed in the Observatory at Vienna. A commission appointed by the Government to examine the work, transmitted yesterday to the Austro-Hungarian Embassy, in London, a report expressing their full approval of the manner in which the task has been completed. It is a matter of no little pride to Ireland that she has produced the largest refracting as well as the largest reflecting telescope in the world." The object glass of this instrument is 27 inches in diameter or 1 inch larger than that of the Washington Refractor made by Clark.

W. C. W.

Washington, D. C., *April* 21, 1881.

INTRA-MERCURIAL PLANETS.

In "Science" of February 26, appeared an article on the above subject by "W. C. W.," which I have read with considerable personal interest, wherein we are led to infer, from purely negative testimony alone, that no such objects were seen during the total eclipse of Aug. 29, 1878, either by the late Prof. Watson or myself. Unfortunately, Prof. Watson's tongue and pen are now silent, and no one exists to defend his observations. What he has written on the subject the astronomical world is familiar with. It is about my own I wish to speak, and in defending them against the negative testimony which your correspondent brings, I hope to be able to convince the reader that because the observers whom he cites saw no planets, it is very far from proving their non-existence.

If the reader will refer to the article itself, he will find delineated on a chart the ground swept over by six observers, but he fails to tell us how short a time was devoted to a search west of the sun, and especially in the immediate region of the two objects seen by me, and near which one of Watson's objects was, viz., near θ Cancri. As not one in a thousand of your readers will have the privilege of reading the reports of those six observers, just published by the Naval Observatory, and, are therefore incapable of forming a correct conclusion on the subject, I have thought it advisable to quote what they really say, and, to remark, that when negative testimony is arrayed against positive, it is very important that its weight, if it has any, be carefully considered.

First, let the fact be stated, that during the total phase of the eclipse which lasted but 162 seconds, two experienced observers, with telescopes in every way well adapted for the work, state with positiveness that each saw two objects not down on any star chart, and, that they were not there when the sun had sufficiently withdrawn to allow the locality to be re-observed. On the other hand, three observers who searched west of the sun, one in a cloudy sky, and two of the others poorly equipped, and, devoting but a few seconds to the search, saw nothing, not even θ Cancri, a star of the fifth magnitude, near where one of Watson's and both of my objects were seen. The weakness of this negative testimony will be apparent from a few extracts from their reports.

Mr. Wheeler (telescope 5 inch, power 100) says, he observed the second and third contacts (beginning and end of totality), also the Corona on both sides of the sun, saw with the naked eye Venus, Mercury and Regulus, observed carefully the several prominences, etc., and then says, "An unsatisfactory attempt was made to sweep for Vulcan. The time given to it was limited, as I was expected to observe all the contacts, and time was consumed in recording the second, and again in bringing the telescope into position for observing the third contact." Now when it is considered that he undoubtedly occupied several seconds in looking at the grand sight with the naked eye, and, that the power used was altogether too high, and of course, the field very small, the time devoted to the search for Vulcan could have been but a few seconds. Is it therefore surprising that Mr. Wheeler saw nothing of the objects seen by me? Only those familiar with the use of telescopes know how perplexingly difficult it is to bring a well-known object in the field of a telescope, using a power of 100.

Mr. Bowman (telescope 3 inch, power 30) says he searched *north* and west of the sun (my objects, also Watson's, were southwest), and that some time was lost (during totality) in exchanging the diagonal tube for the straight one, swept to the westward 5° or 6° in the declination of the sun, and then returning, shifted the declination just far enough *north* to clear the Corona and swept to the westward again, then returned to the R. A. of the sun and shifted to the proper declination just in time to observe the third contact. When it is considered how much precious time was lost in observing and recording in his note-book the time of second contact, changing tubes, and probably observing the eclipse for several seconds with his naked eye, which he could hardly refrain from doing, is it at all wonderful that Mr. Bowman saw nothing of my objects or Watson's either?

Prof. Todd (telescope 4 inch, power 20) says, "I searched 15° each side of the sun, but the sky was *cloudy*, so much so that I was unable to see Delta Cancri," (a 4th mag. star). He does not say how much time he spent searching west of the sun. It certainly could have been but a moment, and, in the region where my objects were, but a few seconds. He, too, observed the second contact, also the Corona, saw Mercury, Venus, Mars, and Procyon. Again I ask is it at all surprising that Prof. Todd saw nothing of the objects seen by me?

Prof. Pritchett (telescope 3½ inch, power 90) says he first observed the grand scene with a naked eye, then swept along the ecliptic several degrees *each* side of the sun, observed all the phenomena of the eclipse, the second contact, Corona, the prominences, and the question arises how many seconds he searched with a very small field west of the sun for the "Ghost of Vulcan," as he facetiously calls it. Still again I ask is it at all wonderful that Prof. Pritchett saw nothing of the objects seen by me? Would it not, in fact, have been very surprising had he seen them at all?

Your correspondent has given in his diagram the outlines of the regions swept over by the above observers, saying: "The place of one of Watson's stars was covered by Wheeler, Bowman and Pritchett, and the place of Swift's two stars was examined by Bowman and Wheeler, and that one of the stars appears in the corner of Pritchett's sweep." Now all this is calculated to convey a wrong impression, for it is not likely that either of them knew within from 1° to 3° the exact boundaries of their hastily-made sweeps; neither do I pretend to be exact about the location of the stars I saw, although I made three estimates of their deviation and distance from the sun, by sighting along the outside of the telescope tube.

They are wrongly placed in the diagram. They were nearer where Theta is, and probably somewhat west of it, which would place it outside of the sweeps of all the observers. I should strongly suspect that one of them was θ, were it not that Watson, who says he saw that star, says nothing about another equally bright some 7′ from it, both ranging with the sun's centre.

Neither in his published statements, or letters to me, does he allude to this vital point. It was as impossible for him to have seen one and not the other, as for one to see Epsilon 4 Lyræ, without, at the same time, seeing Epsilon 5.

Again, he says, as far as relative position is concerned, my objects resemble closely d^2 Cancri, and B. A. C. 2810, on the *east* side of the sun. I hope he does not mean to be understood as inferring that it was on the east, instead of the west, of the sun I was searching.

Finally, he says, the existence of an intra-mercurial planet is not yet admitted by the majority of astronomers. This may be true, but I hope their opinion is based on stronger evidence than that adduced by "W. C. W."

LEWIS SWIFT.

ROCHESTER, N. Y., *April* 11, 1881.

CORRESPONDENCE.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. No notice is taken of anonymous communications.*]

DISCREPANCIES IN RECENT SCIENCE.

To the Editor of SCIENCE:—

The article on "Discrepancies in Recent Science" in a late number of this journal demands some attention, not because the Nebula Theory is seriously threatened by it, but because it properly calls attentions to some physical inferences that have been drawn from other phenomena and applied to the Nebula Theory, especially in the domain of heat. It is assumed by the writers quoted in that article, that *luminousness implies high temperature* and also that the rarity of the gaseous material of the nebula is the immediate result of the high temperature of the constituent atoms. Neither of these assumptions is correct. The trouble comes chiefly from the writer's failure to make the proper distinction between *energy* and *heat*, and I apprehend, also, in the failure to see clearly what the nature of heat is. Most of the books treat of this in a very loose way, and most of the statements on the subject by Mr. Charles Morris are wrong. How far wrong may be seen by comparing his statements with the following quotation from "The Mechanical Theory of Heat," by Clausius, Chap. 1st, Sec. X, p. 24: "*All heat existing in a body is appreciable by the touch and by the thermometer; the heat which disappears under the above changes of condition (fusion and vaporization) exist no longer as heat, but has been converted into work, and the heat which makes its appearance under the opposite changes (solidification and condensation) does not come from any concealed source, but is newly produced by work done on the body.*" We have all along been familiar with the conception of *heat as a mode of motion*, but not with the character of the motion except as "a brisk agitation of the molecules" or "a rapid vibration of the atoms;" but there are two kinds of vibratory motions possible to atoms, one of the character of pendulous motion or a