

scientific world of some fresh discovery or advance—works of research; to offer a digest, for the information of students, of results already attained—text-books; and to attract to the paths of science the outside public—popular works. The pretty and attractive book before us belongs to the last of these categories, and is, we think, well calculated to gain the end in view. It consists of chromo-lithographs of nearly fifty of our better-known native wild flowers, with two or three pages of gossip talk about each. Of the letter-press not much more can be said than that it is fairly accurate from a botanical point of view, and pleasantly written. The illustrations strike us as unusually good of their kind. They have of course the inherent defects of this mode of illustration, in the absence of half-tones and delicate shades; but the general aspect of the plant is in nearly all cases well and faithfully given, and the drawing is good. The book is a very good one to put in the hands of a child to interest him or her in the wealth of wild flowers which is such a source of delight to all dwellers in the country who have eyes educated to see their beauty.

### LETTERS TO THE EDITOR

[The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.]

[The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.]

#### The Telephone

I HAVE just read an article in *NATURE*, vol. xviii. p. 698, on the history of the speaking telephone, which contains an erroneous statement of facts which happen to be within my own knowledge; so that, in the interest of a truthful history of this discovery, it is incumbent upon me to make a brief statement in regard to it.

I had the honour to be one of the judges at the International Exhibition at Philadelphia, and of the group to whom was confided the examination of instruments of research and precision. In the performance of my official duty I took part in the experiments which first brought the speaking telephone to the notice of the scientific world. Prof. Bell and Mr. Elisha Gray were both exhibitors at that Exhibition. Mr. Gray's apparatus was conspicuously shown near one of the main aisles, with the exhibit of the Western Electric Company, while Prof. Bell's was in a side room in one of the galleries, as a part of the Massachusetts' educational exhibit. About the middle of June, 1876, Prof. Bell came to Philadelphia to give personal explanations in reference to his apparatus, and before any public exhibition was made he stated to me in detail the character of his inventions. He was working at two independent things, the one the multiple telegraph by means of transmitted musical notes, the other the transmission of articulate speech over long distances. I told him that I was present in May, 1874, at the rooms of Prof. Henry, in the Smithsonian Institution, when Mr. Elisha Gray exhibited to us an apparatus for the electric transmission of musical sounds, and I asked him whether his first invention was similar. He said there was some similarity, although each had worked independently, and that there was a dispute as to the priority of invention. While sanguine as to practical results from his multiple telegraph, his great invention was the speaking telephone, which he believed he had discovered, and in respect to which there was no rival claimant. He said the idea came to him from some of the suggestions in respect to sound vibrations made by Helmholtz, and that he had succeeded, after patient research, in constructing an instrument which would transmit articulate speech. To this invention he desired to direct the attention of the judges.

The experiments with the telephones had to be made when the Exhibition was closed to the public, and the first experiments were made by Sir William Thomson and others on Sunday, June 18 or 25—I do not now remember upon which of these two dates. Their Majesties the Emperor and Empress of Brazil were present at these experiments. Attention was first given to Mr.

Gray, and he gave a lengthy account of his experiments, which had resulted in the perfected apparatus which he then exhibited. He gave an explanation of his various instruments in chronological order, and conducted some very entertaining experiments as he proceeded in his discourse. The object which he had in view was to send many messages simultaneously over the same wire by using sending and receiving instruments of different musical notes.

The greater part of the day was given to Mr. Gray, so that insufficient time remained for satisfactory trial of Prof. Bell's apparatus. The judges and the distinguished visitors present did, however, proceed to the Massachusetts gallery, and Prof. Bell explained briefly his two inventions, and some experiments were made with his speaking telephone, enough to excite the curiosity of those present in the highest degree. The results were so at variance with the views hitherto received that it was determined by my distinguished colleague, Sir William Thomson, to make other experiments, in which I took part. These experiments were made two or three days later, in the building known as the Judges' Pavilion, in the evening, after the visitors had left the grounds. Prof. Bell had returned to Boston, and was not present at this trial of his apparatus. It was brought over to the judges' pavilion, at my request, by Mr. Hubbard, one of the officers in charge of the Massachusetts exhibit, and the experiments were made by Sir William Thomson and myself. Every precaution was taken to make an impartial test. I was at the transmitting instrument which was placed out of doors at a distant part of the building, and Sir William Thomson was at the receiving instrument in a distant room in the building. After some experiments to find the pitch of voice which would suit the vibrating membrane then used, I received word by messenger from Sir William that he could then hear distinctly, and accordingly the pitch of voice then used was maintained in the subsequent trials. I held in my hand a copy of the *New York Daily Tribune*, and I began to read to him items from its news summary, and soon the messenger came to tell me that the messages were heard distinctly at the other end. The longest message which I sent was the following from that paper:—"The Americans of London have made arrangements to celebrate the coming Fourth of July," and the messenger brought me back from Sir William Thomson the exact repetition of the message. Thereupon we exchanged places, and I could not only hear distinctly the utterances of my colleague, but I could even distinguish the ictus of his voice. The results convinced both of us that Prof. Bell had made a wonderful discovery, and that its complete development would follow in the near future.

The news of these successful experiments soon circulated freely, and the day following, or possibly two days afterwards, Mr. Gray came to me and inquired whether the reports of our success with Bell's telephone were correct; and upon receiving from me an affirmative reply, he said that it was impossible, that we had been deceived in some way, that the transmission was by actual metallic contact through the wire, and that it was, to use his own words, "nothing more than the old lover's telegraph." In reply I said to him that we had taken every possible precaution against error, that we were both convinced of the reality of Bell's claims, and that Sir William Thomson would report to that effect. He persisted in his statement that the result was impossible, and that we must have been deceived in some way or other.

After having had direct knowledge of Mr. Gray's views at that time, I must confess to some astonishment at his claim now made that he anticipated Mr. Bell in the invention of the speaking telephone. Several months ago I saw an article in *Scribner's Magazine*, by Mr. Prescott, in which, while no direct assertion was made that Mr. Gray was the first inventor, there were illustrations given to show the development of the invention in chronological order, and Mr. Gray's instrument was there given priority. I had it in mind then to write a note to Mr. Prescott upon this subject, but I feared that there might be unpleasant controversies over the patents, and the claim of Mr. Gray being rather indefinitely stated, I held my peace. But now that the error appears to be taking root, I have felt it to be my duty to make the statements above given. I have before me a letter from Mr. Bell, dated at Boston, Wednesday, June 28, 1876, and directed to me at Philadelphia, in which he gives diagrams showing how we might arrange the apparatus to transmit articulate speech, as he believed, from Boston to Philadelphia, and proposing experiments to that end if the judges should so desire.

In conclusion I ought to state further, that after Sir William Thomson's address at Glasgow had brought the telephone into

notoriety, Mr. Gray, whose instruments had also been called telephones, gave a public exhibition, in Chicago, I think, and in the report of his lecture which I read, he never once alluded to Bell's invention. His discourse was then, as at Philadelphia before the judges, solely in reference to the musical telephone. In fact, the newspapers had to take pains to inform the public that Mr. Gray's invention must not be confounded with Mr. Bell's, to which Sir William Thomson had referred. You will imagine, then, the surprise of the judges who examined these inventions particularly at Philadelphia in 1876, and heard the personal explanations made by the inventors, to be told now that Gray had already invented the speaking telephone, when all his statements then made show directly to the contrary.

Ann Arbor, November 18

JAMES C. WATSON

### The Intra-Mercurial Planets

NATURE (vol. xviii, p. 569), in commenting upon my letter published the previous week, regarding the discovery of Vulcan, accused me of being not only "indefinite," but "contradictory." The number containing my letter (p. 539) has, from some unknown cause, not yet reached me, though I am in receipt of four numbers published later.

In the several articles written by me on that subject—to the Chicago Astronomical Society, to the Naval Observatory at Washington, to the Astronomer-Royal, to Admiral Muochez of the Paris Observatory, and to others—I have invariably stated the facts as they occurred under my observation, and as they impressed themselves upon my mind, and have invariably adhered to these statements, viz., that the two stars seen by me were of about the fifth magnitude, about 7' or 8' apart, with large red disks, and pointing towards the sun's centre. It is true my letter did contain an error, but not of observation, nor of estimation. In reducing the 8' of arc (the estimated distance between the stars) to time, I somehow called it 2', when, in reality, it is but 32s., thus not only changing its position in R.A., but also increasing, in this element, the discordance between Prof. Watson and myself. The detection of this error has changed, to me, the whole aspect of the Vulcan question. I had previously written to Prof. Watson that I could not reconcile his observations with my own either in R.A., or in Dec., but did not tell him what changes were necessary in order that they might harmonise. He gave me his corrected positions, which helped matters considerably, but still his R.A. was too great, and Dec. too little, for, from three estimations, the two stars ranged with the sun's centre. Recently I have been experimenting with  $a^1$  and  $a^2$  Capricorni (two stars which, in respect to distance from each other, resemble those I saw during the eclipse), my object being to test the accuracy of estimations made of the directions towards which two stars will range when hastily brought into the estimated centre of the field of a telescope having a diameter of one and a half degrees. I find that unless the objects are brought exactly to the centre, they do not point to the same place. During totality time was, of course, too precious to waste in being precise in this, and yet I endeavoured to be so, and as at each of the three comparisons they seemed to range with the sun's centre, I feel convinced that I was not far out in my estimated Dec.

In order to meet Prof. Watson's excessive R.A., I published (contrary, however, to my better judgment), that the distance between the stars was about 8' instead of 7' (as previously announced). On the assumption, therefore, that (a) one of the objects was  $\theta$  Cancri, and (b) that they were 8' apart, and (c) that the one nearest the sun was the planet, as Watson says, the position of the planet was as follows:—

Washington M. T.			
1878, July 29, 5h. 22 m. R.A., $\theta$ Cancri	...	8h. 24m. 40s.	
Add 8' =			32s.
Planet's R.A., Swift	...	8h. 25m. 12s.	
" " Watson	...	8h. 27m. 35s.	
Difference	...	2m. 23s.	
Dec. Swift	...	18° 30'	
" Watson	...	18° 16'	
Difference	...		14'

It will be seen that there is a discrepancy between us of over a half degree of arc in R.A. If we saw the same objects how can we differ so widely? Could I be in error to the amount of 34' between two stars in the same field? Can two stars be three

and one half times the distance of Mizar from Alcor and an observer of experience estimate them at only 7' or 8'? It will be remembered that I recorded in my note-book at the time the distance as 12', but knowing how liable I might be to error in the valuation of so large a distance (for though, from practice, I can estimate quite closely double stars whose distances are from 2" to 20", I have had no experience in the estimation of those of several minutes separation), I chose to carry it in my mind until I should reach home, when it would be the work of only a few minutes to find two stars of the same apparent distance.

I said to Prof. Hough on our homeward journey, that, from memory, I thought their distance was about equal to that separating  $a^1$  and  $a^2$  Capricorni, and that I could decide when I should observe them. My memory of Mizar and Alcor was quite distinct, and as soon as I thought of those (which I did before my arrival at Kansas City) I mentally said, "A little over half the distance between them equals that between  $\theta$  Cancri and the new object," which I did not doubt was Vulcan. Upon my arrival at home I immediately consulted "Webb's Celestial Objects," and was not a little surprised to find their whole distance to be less than 12'. Thus I know they were not over 8' apart, I believe they were but 7'. I know they pointed to the sun's disk, I believe they did to his centre. I know they did not differ one-fourth of a magnitude in brightness, I believe they were exactly equal. I see them, in my mind's eye, as I then saw them, and, while consciousness endures, their image can never fade from the retina of my memory!

I consider the estimated distance in arc, made in such great haste, as valueless compared with the distance as impressed upon the mind from three comparisons, and verified by observations of a reliable character since arriving at home.

Can any error, then, be ascribed to the measurements of Watson, a skilful observer, with telescope well mounted, and with appliances for measuring, and who not only did measure the position of the new planet, but that of the sun and  $\theta$  and  $\delta$  Cancri (three objects in its immediate neighbourhood) as well.

Have we any right to call in question the accuracy of his circles in giving the position of the new object when they correctly gave the positions of the others?

Wherein, then, lies the discrepancy, and how can it be reconciled? Again, Watson says the planet was much brighter than  $\theta$ , while the stars which I saw were of equal magnitude.

Several times since my return from the eclipse expedition I have, both in darkness and in strong twilight, examined  $\theta$ , and I find no star near it, nor no two stars in its vicinity answering, in any particular, to those seen by me at Denver.

The above facts I submit to the world, and astronomers must deduce therefrom their own conclusions as to what the objects were. My own are reached, and, briefly stated, are as follows:—That the two objects seen by me were both intra-Mercurial planets, and that I did not—as was for a time supposed—see  $\theta$  Cancri. Prof. Watson saw  $\theta$ , and, some 42' of arc south-east of it, another planet, and determined its position, and near to  $\zeta$  Cancri still another, whose position also he fortunately ascertained, making four in all. It will not do to say, as some have intimated, that Watson saw  $\theta$  Cancri, and 42' from it a planet which I did not see, and that I, also, saw  $\theta$ , and, 7' or 8' from it, another planet which he did not see. This reasoning appears to me untenable, for how could he have failed to see mine, when the diameter of his field was over 40', and had  $\theta$  in its centre?

If the above conclusions are true, and that four planets were discovered instead of two (as at first supposed), the question naturally arises, Which, if any one, is Lescarbault's Vulcan?

I estimated, at the time, the objects as being of the fifth magnitude, that is, as bright as a fifth magnitude star would appear in a clear, dark night. How much allowance ought to be made for diminution from atmospheric illumination I know not. I was then of the opinion that it would make a difference of at least one magnitude, but, having examined the region around  $\theta$ , and finding many stars there, and several which are quite bright, not one of which I saw during the eclipse, I think that fully two magnitudes should be allowed.

In what way can these intra-Mercurial planets (of which there are probably many) be detected?

I would suggest that, on July 29 next, a determined and systematic effort be made, with large telescopes equatorially mounted, to observe  $\theta$  Cancri, and, if then successful, there is hope that these planets, or some of the larger ones, may be discovered in the absence of a total eclipse, or while in transit. If