ASTRONOMY.

DOUBLE STARS—A collection of "Observations of Double Stars made at the United States Naval Observatory," by Professor Hall, has just been issued from the Government Printing Office at Washington. The list includes, besides a small number of stars observed in 1863, with the 9.6 in. equatorial, all the observations of double stars made by Prof. Hall with the 26 in. refractor since 1875. The whole number of observations is 1614.

THE TRANSIT OF VENUS, 1882.—At the sitting of the Paris Academy of Sciences, on the end of May, the Minister of Foreign Affairs transmitted a letter from the British Ambassador, on the part of his Government, desiring to be informed with which French authorities the Royal Society of London should communicate with the view of an interchange of opinions relative to the observations of the approaching transit of Venus. The letter was referred to a committee already nominated.—*Nature*.

CORRESPONDENCE.

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

To the Editor of SCIENCE:—

In "SCIENCE" for May 21, Professor Dolbear replies to my criticism of his papers in a manner which, if unanswered by me, is liable to place me in a false light. I, therefore, again request a portion of your valuable space. Professor Dolbear says that, perhaps, he was not guarded enough in some of his statements, and I perfectly agree with him. His reply, too, seems to me to be "not guarded enough." I should be very loth indeed to ascribe to intention what looks very much like an attempt to drag into scientific controversy the legal maxim, "*Falsus in uno, falsus in omnibus.*" When, however, the Professor says that by dealing with the last part of my paper, "it will save saying very much about the first part," it certainly looks as if he tried to apply that maxim. In regard to the quotation from Maxwell's paper published in Vol. XI. of Nature, I frankly own that I never saw it till the Professor's reply drew my attention thereto, and I thus publicly express to him my obligations and thanks for having done so. I cannot, however, admit that this investigation of Maxwell's materially alters the situation. Maxwell treats of molecules and Professor Dolbear speaks of atoms, something altogether different. His results are, furthermore, the mathematical consequences of certain hypotheses in regard to the molecules; but these mathematical deductions do not agree with the results of experiment—the ratio of the specific heats deduced from Maxwell's investigation does not agree with the same ratio experimentally determined. And Maxwell says that in this disagreement the greatest difficulty of the Kinetic Theory of Gases lies. Boltzmann's result applies to a rigid body, and is not general, if I understand Maxwell correctly. Now, it would certainly be more in accord with scientific principles to use an experimental constant rather than a purely theoretical one in such calculations as the Professor's.

Professor Dolbear's reply gives the impression that in $E'-E = \varepsilon$. I regard ε as a ratio when I clearly state that in $E' = \varepsilon\frac{m v^2}{2}$ it must be the ratio $\frac{E'}{E}$ if it is anything; and this is clearly inconsistent with its also being the difference $E'-E$. As, however, the Professor admits the expression $E' = \varepsilon\frac{m v^2}{2}$ to be wrong, we agree on this point, "There is nothing in the first paper that is a deduction from any mathematical work given," says the Professor. In his first paper, Section III., he obtains the equation $\frac{m}{m'} = \frac{v'^2}{v^2}$ and says, "That is, the square of their velocities is inversely as their masses, *so that wave length in the ether will vary as the mass of the atom.*" This looks to me like a deduction from mathematical work, though a strange one. With the second quotation from Maxwell I also perfectly agree, for I did not for a moment think, nor did I say, that ether and ordinary matter are the same. I only maintain that ether *is* matter. To define matter as that which obeys Newton's law of gravitation as Professor Dolbear does, seems to me but little better than to say matter is that which has weight. The defense given for the use of the word density as applied to something which it is claimed is not matter, would, if sanctioned, make sad havoc with the precision of scientific and technical terms. The word density has a definite meaning, and if Professor Dolbear wished to attach to it a new meaning, he should have said so. I confess to no little curiosity to know this new meaning of the word density as applied to ether regarded as non-matter.

I do not by any means wish to restrict Professor Dolbear to one or any other number of planes in arranging his atoms, but I do not see how that improves his position. A radial or triangular prismatic structure is open; and such saturated molecules as HCl, H^2O &c., could not exist. This is, however, but a minor objection to the hypothesis and need not occupy our attention until the more serious ones are removed. It is but a natural and inevitable consequence of this hypothesis to suppose dissociation at absolute zero. And as we do not know anything about matter at absolute zero the necessity of supposing dissociation at that temperature presents to my mind a very great difficulty in applying the hypothesis of synchronous vibrations to explain even the molecular phenomena of adhesion, cohesion, &c. Had I read Professor Dolbear's description of his highly curious and interesting experiments before writing my criticism I should most assuredly have arrayed these very experiments in evidence against his theory, though I feel by no means sure that the formation of one vortex-ring from two may not be due to friction. If it can be shown mathematically that the same results follow in a perfect fluid, I fear the Professor's experiments make an end of the vortex-ring theory.

Now lest any doubt should arise as to the relative position of Professor Dolbear and myself in this controversy, allow me in conclusion to state the case as it appears to me. Professor Dolbear, a well-known scientist, advances new hypotheses; I, an unknown student of science, object to these hypotheses on the ground of insufficient evidence. Then according to the general rules of argumentation the burden of proof rests with Professor Dolbear, and it is for him to remove my objections either by showing that they are not well taken or by overcoming them by further evidence.

WM. H. DOPP.

BUFFALO, N. Y., *May* 24, 1881.

BOOKS RECEIVED.

THE CAT. An Introduction to the Study of Backboned Animals, Especially Mammals. By ST. GEORGE MIVART, Ph. D., F. R. S. 200 Illustrations. Scribner's Sons, New York, 1881.

In this octavo volume of about 600 pages the author has attempted to give what has never been attempted for any other animal in a single volume; viz., a complete account of the domestic cat's anatomy, physiology, embryology and psychology, its place in nature and relations to the external world, its pedigree and origin; in short, its biology.

In the preface the author defines his position, and gives the reason for his book: "The advances of astronomy and geology have produced great changes in men's minds during the last three centuries; biology is pro-