most favourable circumstances. Other Trigoniæ with surfaces precisely similar are *T. elongata*, Sow., from the Cornbrash of the same locality, and from the Oxford Clay of Dorsetshire; *T. costata*, var. *lineolata*, Ag., from the grey limestone of Scarborough; another lunulate and lengthened form from the Upper Trigonia grit, Inferior Oolite near Stroud; *T. monilifera*, Ag., from the Coral Rag of Weymouth; and *T. marginata*, Lyc., from the Kimmeridge Clay of Wiltshire. This granulated surface occurring, as is now ascertained, in so many species of the costated Trigoniæ, whose general forms and other characters are very dissimilar, renders it evident that the whole group of the Costatæ is characterized by its presence, although we may only expect to discover it occasionally in specimens derived from fine argillaceous deposits, and cleared simply by washing, or by using only a light brush. The other sections of Trigonia having tubercles, varices, or serrated ribs upon the sides, appear to have been destitute of this granulated tegument, as are also the recent Trigoniæ. I would also venture to remark that the value of the granulated tegument as a ground of distinction in the groups of testacea, does not appear to be sufficiently appreciated by some palæontologists; that it is of higher value to us than as a separation between species, may be inferred from the fact that in the great family of the fossil Anatinidæ it characterizes all the species of the genera in which it occurs, and that the present appears to be the first known instance in which a well-defined genus can be separated into two sections, the one having the surface granulated, the other smooth: in Trigonia, however, it is found to pervade only a single but large and well-defined group, which in its general characters is as clearly separated from the other fossil groups as from the recent members of the genus.

JOHN LYCETT, M.D.

Scarborough, May 4th, 1864.

On the Nebular Theory.

Salford, May 17, 1864.

Sir,—In No. 75 of your 'Geologist,' I find an article on Planetary Orbits, etc., written with a considerable degree of ingenuity, in which you ask, and I suppose with no objection to a reply, for instances "of the evolution of light and heat by *slow* condensation of gaseous matter." Chemistry supplies us with abundance of proof in this respect. One of the most familiar is shale loaded with iron pyrites, which, when exposed to the influence of the atmosphere, often takes fire from the slow absorption of gaseous matter.

So, on the other hand, excessive heat has greater power than chemical affinity, and will, if supplied in sufficient intensity, release the condensed oxygen again from its compound. Metals, too, have a very great power to condense gaseous matter within their pores, and this power is generally proportionate to their spongy and divided character; but if heated, their affinity for gaseous bodies is likewise proportionately increased. But as I have just stated, heat has a greater power than chemical affinity, and therefore no condensation of gaseous matter could take place until it was sufficiently cooled to be within the range of chemical power.

Now the nebular theory assumes that condensation is the result of slow cooling, and could not have taken place in any other manner; consequently, no universal conflagration and condensation could simultaneously have taken place, as your reasoning supposes. Again, if we closely examine the crust of the earth, especially amongst the igneous rocks, with which we

only profess now to deal, we shall find that they are combinations of the most infusible character, and, *à priori*, bodies which would be the first condensed, especially when their strong affinities are taken into consideration. And those bodies which are easily dissipated by heat are almost wholly excluded, such as water, carbonic acid, etc.

There is another argument I wish to point out, which I think is erroneous. You suppose that the heat of the earth is sufficiently accounted for on the grounds of its retardation through the ether of space. A little reflection will show that a body once heated to its maximum temperature, from whatever cause, whether in passing through air or the ether of space, if the medium be uniform in resistance and the motion constant, the heated body must necessarily diminish in temperature until it is reduced to the same degree of heat as the medium through which it passes. The reasons are obvious. The original cause of heat in the case supposed is ethereal resistance, and the moving body eliminating heat is in consequence of the rearrangement of its constituent particles adjusting themselves to their altered condition; and when this is attained the heated body must sink again to its normal condition of heat, and could not, I think, permanently retain a heat so much superior to the medium through which it moves, simply in consequence of resistance.

I am, Sir, most respectfully yours,

THOS. GALLASPIE.

[I am very glad to have my suggestions—I do not call them "views"—more fully discussed. Mr. Gallaspie however does not give any of the illustrations, with which he says chemistry abounds, of the *permanent* or rather long-continued production of light and heat by the slow condensation of *gaseous* bodies. Take oxygen and hydrogen. They combine with explosion. The heat, I conceive, which drove these particles apart whilst they were gases, has passed off into the air, and become motion acting upon the particles of the atmosphere. The resulting produce is a drop of water, not boiling. Shales and metals are solid substances; and even if we take metallic vapour, what should cause metallic vapour to exist in space? Where is the heat to come from anywhere except within the circuit of our earth's orbit, which should raise gold, iron, or even tin and lead into vapour. There seems to me not a particle of scientific evidence nor of probability in favour of the nebular *hypothesis*—such it was first properly termed, such it still, to my mind, remains.

The other point as to the effect of the resistance of the ether of space—although I do not agree with Mr. Gallaspie—is better put. The point raised by me was this: if the earth's motion in her orbit be due to any original impetus given to our planet, then the resistance of the ether of space to the earth's motion must give rise to friction, and this friction must be, by the laws of the correlation of the physical forces, be changed into some other force than motion. What is lost by friction as motion must become heat, light, electricity, chemical, or molecular action. As to what is the temperature of space, we have yet to learn what that temperature is in the area of the earth's orbit. Mr. Gallaspie should bear in mind that if this heat of our portion of space be due to the heat of the sun, it can be estimated. But certainly friction may raise a body, gaseous or solid, to, and maintain it at a higher temperature than the surrounding air or gaseous medium. The production of fire by a lucifer match, or the rubbing of two sticks together, shows this. If the heat produced by friction cannot be carried off by the conductivity of the atmosphere, it will be accumulated in the object. As the orbital speed of the earth is, on the spiral-orbit hypothesis, slowly and constantly diminishing, there should be thus consequently a slow and constant diminution of the heat acquired by the past accumulation from higher friction—that is practically a slight cooling of the globe throughout past ages, and at present going on. The amount of this would be negatived, outbalanced, or controlled by the inward tendency of the earth to nearer proximity to the sun. I regret much that space does not permit me to say more in this place.—S. J. MACKIE. *27th May*, 1864.]