The Transposition of Zoological Names.

I WISH to say how thoroughly I agree with Mr. Lydekker in his remarks on the unwisdom of transposing zoological names, and on the confusion caused by this objectionable practice. To the instances which he has mentioned I may add the following cases relating to two well known and familiar species of animals. Linnæus called the only European hare known to him *Lepus timidus*, and for many years that name was applied to the common brown hare of Central Europe, while the northern hare, which changes to white in winter, was known by Pallas's appropriate name, *Lepus variabilis*. This was the nomenclature used by Blasius, by Bell in his "British Quadrupeds," and in all the ordinary text-books of zoology. It was, however, pointed out some years ago, first, I believe, by Lilljeborg, that the *Lepus timidus* of Linnæus had been based mainly upon the northern or variable hare, or that at all events Linnæus had confounded the two species together. In these circumstances obviously the best plan was to call the middle-European brown hare by its next given name, *Lepus europeus*, and this course has been adopted by most writers. But the advocates of unrestricted priority are not content with this, and insist upon calling the variable hare *Lepus timidus*, the consequence being that when that name is used it is impossible to know which of two perfectly distinct animals is intended by it.

Another still more objectionable transposition of two well known names has been lately suggested. Linnæus, in the twelfth edition of the "Systema Naturæ," gave the name *Turdus musicus* to the song-thrush and that of *Turdus iliacus* to the redwing, and these familiar terms have been used by all writers for these well known birds respectively ever since. But about a year ago it was discovered by an ardent member of the new school of priority that in his tenth edition of the "Systema" Linnæus had unfortunately (by some error in his MS. or of his printer) attached the diagnosis of *Turdus musicus* to *T. iliacus*; and that of *T. iliacus* to *T. musicus*. It was admitted that Linnæus had corrected the mistake in his later edition of 1760, but even Linnæus could not be allowed to correct his own errors in the face of the inviolable law of "priority." In future, therefore, it was maintained, the song-thrush must be called *T. iliacus* and the redwing *T. musicus*! This course has been actually adopted by a subsequent writer, but we may trust that it will not meet with general approval, and that the song-thrush and redwing will remain under the old names given to them by the father of scientific nomenclature in 1760, and used by every subsequent writer until 1904. P. L. SCLATER.

Modern Algebra.

THE publication of Messrs. Grace and Young's treatise on algebra will direct attention to the importance and difficulty of the theory of the concomitants of ternary and quaternary quantics in connection with plane and solid geometry. There are one or two points on which I propose to make some remarks.

In the first place, canonical forms are sometimes deficient in generality, and this will be the case whenever the form is the analytical expression for some special property of an anautotomic curve. Of this defect the canonical form of a ternary cubic furnishes a striking example, for it is the analytical expression for the theorem that through each of the three real points of inflexion one real straight line can be drawn which passes through one pair of conjugate imaginary points of inflexion on an anautotomic cubic curve; and since autotomic cubics do not possess this property such curves cannot be represented by the canonical form.

In the next place, anautotomic curves are not by any means the most interesting species of curves, and to go through the process of calculating their concomitants, and then specialising them for some particular species of autotomic curves, is often very laborious. In the case of unicursal quartics, many interesting results might be obtained by calculating directly the concomitants of the quantic $(\alpha\beta\gamma, \gamma\alpha, \alpha\beta)^2$, and this would give results applicable to all unicursal quartics, except those which possess

the five compound singularities called the tacnode, the rhamphoid cusp, the oscnode, the tacnode cusp, and the triple point. Also, since an evectant is the tangential equation of a curve which is related in a special manner to the original one, an examination of the evectants of the above quantic would lead to interesting results concerning conics and other curves connected with trinodal quartics.

In this subject geometrical methods are a powerful assistance to pure analysis. For example, let U be a ternary cubic in (α, β, γ) ; eliminate γ by means of the equation $\beta = k\gamma$, and equate to zero the discriminant of the resulting cubic equation in α/β . This will give a sextic equation $\Delta(k) = 0$, which determines the six tangents drawn from A to the curve. The condition that the curve $U = 0$ should have a node is that the equation $\Delta(k) = 0$ should have a double root; hence the discriminant of this binary sextic is the discriminant of the original ternary cubic U .

Many other examples of a similar kind could be mentioned, and we may observe that from the discriminant of a binary duodecimic, all the conditions that a quartic curve should possess point singularities may be obtained.

April 28.

A. B. BASSET.

Current Theories of the Consolidation of the Earth.

IN Lord Kelvin's philosophical and justly celebrated paper on the secular cooling of the earth (Thomson and Tait's "Nat. Phil.," vol. i., part ii., Appendix D), the assumption is made that the earth was once a fiery molten mass, liquid throughout, or melted to a great depth all round. He cites Bischof's experiments showing that "melted granite, slate, and trachyte all contract by something about 20 per cent. in freezing," and continues:—

"Hence, if, according to any relations whatever among the complicated physical circumstances concerned, freezing did really commence at the surface, either all round or in any part, before the whole globe had become solid, the solidified superficial layer must have broken up and sunk to the bottom, or to the centre, before it could have attained a sufficient thickness to rest stably on the lighter liquid below. It is quite clear, indeed, that if at any time the earth were in the condition of a thin shell of, let us suppose, 50 feet or 100 feet thick of granite, enclosing a continuous melted mass of 20 per cent. less specific gravity in its upper parts, where the pressure is small, this condition cannot have lasted many minutes. The rigidity of a solid shell of superficial extent so vast in comparison with its thickness, must be as nothing, and the slightest disturbance would cause some part to bend down, crack, and allow the liquid to run over the whole solid. The crust itself would in consequence become shattered into fragments, which would all sink to the bottom, or meet in the centre and form a nucleus there if there is none to begin with."

In adhering to these views, Lord Kelvin has been followed by Prof. G. H. Darwin (cf. "Tides and Kindred Phenomena of the Solar System," p. 257) and other eminent mathematicians; so that the theory that the earth consolidated by the building up of a solid nucleus through the sinking of portions of the crust of greater specific gravity is no doubt generally accepted by geologists and others interested in the physics of the earth.

Recent researches on the pressures within the planets (cf. *Astronomische Nachrichten*, No. 3992) have thrown great doubt on this mode of consolidation of the globe. The line of argument by which we reach this conclusion is a double one:—

(1) It is shown that the effect of pressure in the highly heated fluid assumed to have constituted the molten earth would have been to dissolve the portions of the sinking crust before they attained any considerable depth.

(2) The increasing density of the fluid itself would have prevented sinking of the crust below one-tenth of the radius, so that a solid central nucleus could not have been built up in this way.

To see this clearly, let us suppose that the earth were a molten mass, and that a crust of rock several kilometres in area, and a considerable fraction of a kilometre in thickness, had formed, and begun to sink in the molten fluid

by its superior gravity. What would happen to it as it descended towards the earth's centre?

The densities and pressures in the outer layers of the earth, found by Laplace's law, are as follows:—

Radius	Depth below the surface km.	Density	Pressure in atmospheres
1'000 ...	...	2'55 ...	1'000
0'995 ...	31'85 ...	2'608 ...	8,610
0'990 ...	63'70 ...	2'667 ...	16,470
0'985 ...	95'55 ...	2'725 ...	25,080
0'980 ...	127'40 ...	2'785 ...	33,690
0'97 ...	191'10 ...	2'904 ...	51,670
0'96 ...	254'80 ...	3'025 ...	70,410
0'95 ...	318'50 ...	3'144 ...	89,400
0'94 ...	382'20 ...	3'265 ...	109,860
0'93 ...	445'90 ...	3'386 ...	130,130
0'92 ...	529'60 ...	3'508 ...	152,940
0'91 ...	593'30 ...	3'629 ...	175,470
0'9 ...	657'00 ...	3'751 ...	198,760

The above table shows that before the mass has descended 31.85 km. ($1/200$ th of the radius) the pressure about it would have become more than 8000 atmospheres, which would force the molten fluid deep into the heated rock. The rising temperature at that depth would also rapidly dissolve the mass, and before the solid has sunk through another equal space in the viscid liquid, and thus reached a depth of 63.7 km., it seems almost certain that it would be completely dissolved.

It must be borne in mind that the solid is not much denser than the liquid; and as the liquid is highly viscous the mass would sink slowly, while the increase of temperature and pressure would conspire together in the most powerful manner to dissolve the mass and reduce it to the same temperature and density as the enclosing liquid, which would be forced into it on all sides by a pressure vastly greater than any known in our laboratories.

Even if we make the violent assumption that the sinking mass is a kilometre, or several kilometres, thick, it is difficult to see how it could continue its downward course, undissolved by temperature and pressure, below a depth approximating one-tenth of the radius, or 637 kilometres. The sinking would be quite slow, owing to stiffness of the fluid, and could hardly be accomplished to this depth inside of several days, or more probably weeks.

Moreover, before the mass reached a depth of 260 kilometres, or less than one-twentieth of the radius, the density of the molten fluid would become 20 per cent. greater than it was at the surface, owing to pressure; and when the solid mass was no denser than the surrounding fluid it would cease to sink. Or, if it had acquired a small velocity downward in the fall from the surface against the viscous resistance of the fluid, which is enormously increased by the eddy arising from the condition of continuity, it might go down a little lower until the motion was overcome by the buoyancy of the denser fluid below. Accordingly, so far as one can see, solidified crust in sinking could by no possibility go lower than one-tenth of the radius, which would hardly accomplish the building up of a solid nucleus.

In considering the effects of pressure in forcing molten fluid into the sinking solid, we have not assumed that the density would thereby be increased; for at the great temperature of the fluid it is obvious that the solid into which the hot liquid entered would be dissolved, and heat from the fluid would be conducted rapidly through the solid mass. Thus no cause seems to be overlooked which could invalidate our conclusion.

It rests primarily upon the enormous pressures known to exist at great depths in the earth, and their undeniable effect in forcing the molten fluid into any possible solid body, so as to prevent it attaining any considerable depth without dissolving; and upon the assumption that even molten rock under such forces would take approximately the density given by Laplace's law, which hardly admits of reasonable doubt.

In considering these questions heretofore, the hypothesis of incompressibility for the molten fluid has been tacitly implied or assumed. Whether such an hypothesis is justified will appear differently to different minds, but for our

part we cannot hesitate in rejecting it on account of the known porosity of all matter, and its observed yielding and condensation under great forces.

On account of the difficulty in handling liquids, especially when at high temperatures, they have not been so carefully investigated in the laboratory as solids; but there remains scarcely any doubt that under planetary pressure they would all yield like sponges.

In indicating his interest in the paper on planetary pressures (*Astronomische Nachrichten*, No. 3992), one of the most eminent British mathematical physicists has pointed out that to his mind the present writer has underestimated the probability that the earth has a metallic nucleus. I have since pointed out in a letter to the editor of *NATURE* (April 13, p. 559) that pressure, and not metallic constitution, is the true physical cause of the earth's rigidity; for under such pressure any kind of matter would assume a hardness greater than that of steel; and as the material is above the critical temperature of every substance it is really gaseous, and would expand with incredible violence if the pressure could only be relieved.

In the *Astronomische Nachrichten*, No. 3992, I have shown that in any mass of considerable size, so condensed that the pressure amounts to millions of atmospheres, circulation at great depth becomes practically impossible, on account of the friction due to the increasing pressure as we descend within the mass. The pressure and friction which prevent circulation also prevent separation of the elements according to their densities. While it may not be possible to say that there is not an increasing amount of metals, such as iron, towards the centre of the earth, it is, I think, clear that there is no distinctively iron nucleus; for the existence of such a nucleus would imply that the earth's mass had unimpeded circulation when in a fluid state, all of which is to the last degree improbable.

When the earth was less condensed it was at lower temperature, and the elements may not have been fused; and as condensation advanced, and the temperature rose, the friction due to pressure operated with increasing intensity to destroy circulation, which would thus be restricted to the subsidence of compact masses decidedly denser than the surrounding fluid. As the fluid was necessarily at high temperature, a compact mass would soon be dissolved, and further circulation of its elements practically cease.

It seems, therefore, very difficult to escape the conclusion that the earth's interior is a magma of all the elements, the increasing density towards the centre being due primarily to pressure. If any separation of the metals from the rocks took place, it could only be near the surface where the pressure is slight; but because the rocks predominate at the surface, we must not conclude the same material does not exist abundantly in the great central nucleus of the globe.

The difference in the point of view here adopted and that held by the older school of physicists is based primarily upon the effects of pressure. While there is a certain disappointment in negative results, they are sometimes useful in leading us to new conceptions, and perhaps we may hope that further study of these difficult questions will produce results admitting of general acceptance. It should be added that the pressures for the interior of the earth, calculated in the *Astronomische Nachrichten*, No. 3992, would not be very greatly modified by any other admissible law of density.

The researches of Radau and Darwin (*cf. Monthly Notices*, Roy. Astron. Soc., December, 1899, pp. 122-3) have shown that, so far as the mathematical conditions are concerned, the law of density within the earth might depart considerably from that of Laplace. But on physical grounds, including the incontestably steady rise of pressure towards the earth's centre, whatever be the exact law of density, and especially the observed yielding and condensation of all matter under such forces, I hold that the true law is essentially that of Laplace and any departure from it in the actual arrangement of the matter of the globe is likely to be extremely small and unimportant.

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March 31.