

points in the reasoning, however, and the proof consists in showing (1) that y satisfies the differential equation, (2) from the second formula that $y=1-\cos t$ when $x=0$, (3) from the first formula that y and dy/dt are both zero when $t=0$, (4) from the first formula that when t is finite y is small for all large values of x . If, now, x is finite and t great, the second formula reduces to $y=-\cos(t+x)$, so that the motion now consists entirely of waves proceeding towards the source of the disturbance—a most remarkable result. If in the formulæ for y we change the sign of x , the J functions are replaced by I functions. The resulting value of y does not satisfy (4), and cannot be accepted as a solution of the problem.

H. C. POCKLINGTON.

The Transposition of Zoological Names.

AMONG the many radical changes in zoological nomenclature proposed of late years, none appear to me more open to objection than those where names which have long been in general use for particular species or groups are transferred to others on the ground that they were originally applied to the latter. One of the earliest of such transpositions was suggested by Prof. Newton, of Cambridge, who urged that *Strix* is not the proper generic designation of the barn-owl, and that while this species should be called *Aluco flammeus*, the tawny owl should take the generic title *Strix*, as *S. aluco*. I find, however, that this emendation is not accepted in the British Museum "Hand-list of Birds," where the barn-owl figures under its familiar title of *Strix flammea*. Uniformity is not, therefore, attained by this proposal.

Another instance occurs in the case of the walrus, which was long known as *Trichechus rosomarus*, until systematists discovered that the generic title refers properly to the manati, to which animal they transferred the name. Again, the *Simia satyrus* of Linnaeus is now stated to be the chimpanzee, and not the orang-utan, and consequently *Simia* is made to stand for the latter instead of for the former. As a fourth example of this transference of a familiar generic name may be cited the case of the marmosets of the genus *Hapale*, to which it is now proposed to apply the title *Chrysotrix*, despite its practically immemorial use as the designation of the titi monkeys.

As an example of the transference of a species name, it will suffice to take the case of the African antelope commonly known as the white oryx (*Oryx leucoryx*). This name, it is stated, properly belongs to the Arabian Beatrix oryx, to which it is accordingly proposed that it should be transferred, after being so long used for the former animal.

Personally, I am very strongly of opinion that such transpositions should not on any account be permitted, and that when a species or genus has been known by a particular name for a period of, say, fifty years, this should, *ipso facto*, give such an indefeasible title to that name (altogether irrespective of its original application) as to bar its transference to any other group or species. It may, indeed, be deemed advisable that, as in the case of the walrus, the old name should not be retained in the generally accepted sense, but, if so, it should be altogether discarded, and not transferred. The practice of transferring names must, if persisted in, inevitably lead to much unnecessary confusion without the slightest compensating advantage. Indeed, it will render such works as Darwin's "Origin of Species" and Wallace's "Geographical Distribution of Animals," which are certain to live as biological classics, absolutely misleading to the next generation unless special explanatory glossaries are supplied.

Advanced systematists urge that those who refuse to follow their lead in this and other kindred emendations in nomenclature are not only old-fashioned and behind the times, but that they are absolutely doing their best to hinder the progress of zoological science. This, however, is but the opinion of a comparatively small (and, shall we say, somewhat prejudiced?) section. What we really want is the opinion of all those interested in zoology and natural history, namely, professional zoologists, palæontologists, geologists, physiologists, anatomists, zoogeographers, amateur naturalists, and sportsmen. If the general consensus of opinion of all these were on the side

of the proposed changes, and of others of a similar type, then, and then only, I venture to think, could they be regarded as obligatory.

It may be added that the use of combinations, which Mr. Stebbing has felicitously designated "comicalities in nomenclature," of the type of *Anser anser* and *asinus asinus* (or, still worse, *Asinus asinus asinus*, which is a possible contingency), is rapidly tending to discredit the common sense of scientific zoologists among matter-of-fact men of the world.

R. LYDEKKER.

A little known Property of the Gyroscope.

TO my surprise I have found that the property of the gyroscope which I am about to describe, although perfectly elementary, appears to be little known to either physicists or astronomers. Neither is it mentioned in the text-books so far as I am aware. That it has a very important bearing on the mechanism of the solar system has been shown in some of my earlier papers, but the laws which govern the rotation and the simple facts themselves seem to be so little understood that I have thought it worth while to explain them more fully in this place.

If a gyroscope is mounted on gimbals so that it may shift its plane of rotation freely about an axis passing through the plane of the revolving disc, we shall find it is possessed of certain curious properties. To most persons the notable characteristic of a gyroscope is the resistance it offers to any force tending to change the plane of its rotation. This is true of it only, however, in case certain conditions are complied with. If these conditions are neglected, it will change its plane with the greatest facility.

If the wheel is properly balanced and mounted as above described, and we set it spinning, it will continue to rotate in one plane without change until it stops. Suppose that while it is spinning we set it upon a table, and cause the stand supporting it to revolve slowly about its vertical axis. Instantly the wheel will adjust itself so as to revolve in a plane parallel to the surface of the table.

Furthermore, the direction of rotation of the wheel upon its axis will be the same as the direction of rotation of the stand. If we turn the stand in the opposite direction the wheel will at once shift its plane, and turn over, so as again to rotate in the same direction as the stand.

Another way of showing the experiment is to hold the stand supporting the gyroscope at arm's length. The observer then slowly revolves upon his heels, first in one direction and then in the other. Each time the observer shifts his own direction of motion the gyroscope will shift its plane, and always in such a manner that its direction of rotation shall be parallel and in the same direction as its revolution in its orbit.

It is a well known fact that according to the nebular hypothesis all the planets should have rotated in a direction opposite to that of their revolution in their orbits, just as Neptune does at the present time. This is because by Kepler's laws the inner edge of a revolving ring must necessarily move faster than the outer edge. The fact that Neptune is the only planet that even approximately fulfils this condition has always been a source of trouble to the adherents of the nebular hypothesis. No one has ever even attempted to explain the anomalous rotation of Uranus, in a plane practically perpendicular to the plane of its orbit.

The interesting property of rotating bodies illustrated above in the case of the gyroscope, and fully explained by its theory, now at once makes the matter perfectly clear. In the case of the planetary bodies, the force rotating the stand of the gyroscope is supplied by the annual tide raised upon the planets by the sun. In former times, when the planets were large diffuse bodies, this tidal force was of considerable importance. Neptune, however, is so remote from the sun that the tidal influence upon it has always been small. The plane of its rotation, therefore, has been but slightly shifted from that of its orbit—about 35° . Uranus being nearer the sun has had its plane shifted nearly half-way over, or through 82° . The plane of rotation of Saturn has been shifted through 153° , while that of Jupiter has suffered a nearly complete reversal, and the planet now revolves approximately in the plane of its

orbit. The deviation amounts to but 3° , and its plane of rotation has therefore shifted through 177° .

The explanation of the retrograde rotation of Phœbe is now also clear. Phœbe, the first-born of Saturn's numerous retinue, came into being while the planet itself still retained its original plane of rotation, that is, while it was still revolving in a retrograde direction. Before Iapetus, Saturn's second satellite, reckoning from without inwards, was created, the mighty tides acting upon the planet in its then diffuse condition had shifted its plane of rotation more than 90° . Two forces then acted on the plane of the orbit of the new satellite, one from the sun tending to bring the orbit into the plane of the orbit of Saturn, the other from Saturn tending to bring the orbit of the satellite into the plane of the equator of its primary. At first both forces tended to produce the same result, namely, to diminish the angle of inclination of the plane of the orbit of the satellite. They are now pulling in opposite directions, as is the case with our own moon, the inclination of the orbit of Iapetus, 10° , being less than that of the equatorial plane of its primary.

The inner satellites of Saturn are more powerfully affected by the equatorial expansion of the planet than by the action of the sun, the planes of their orbits, 27° , coinciding nearly with the plane of the planet's equator.

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Have Chemical Compounds a Definite Critical Temperature and Pressure of Decomposition?

So far nobody seems to have considered the question whether to every chemical compound there exists a definite critical temperature and pressure of decomposition. Yet I think the following considerations show that such constants probably do exist. Suppose we place a given compound (say CaCO_3) in a closed cylinder and subject it to a continually increasing temperature, keeping the pressure constant by means of a weighted piston. Then at a certain definite temperature range the compound will begin to decompose. Suppose, now, we increase the pressure sufficiently; then the decomposition ceases, and the substance can now bear a higher temperature than before without decomposition.

Proceeding in this way, it is, I think, obvious from the finite nature of the mass of the atoms, and from the limited intensity of the forces holding them together in the molecule, that ultimately at some definite finite temperature the external forces tending to drive the atoms apart will become equal to the maximum internal forces that the atoms can exert on each other in the molecule. It therefore follows that above a certain definite temperature, depending upon the nature of the molecule, no pressure, however great, can prevent the substance from completely decomposing. This temperature and pressure, above which a compound is incapable of existing, we will call the critical temperature and pressure of decomposition of the compound. The critical temperature and pressure of decomposition would therefore be completely analogous to the critical temperature of liquefaction of a compound—only in the latter case we are dealing with the temperature whereat a certain molecular condition of existence disappears, and in the former case with the temperature whereat a certain atomic condition of existence disappears.

Since atoms are a very much more finely divided form of matter than molecules, it is clear that the critical temperature of decomposition of a compound must be a very much sharper and clear-cut constant than its critical temperature of liquefaction. The critical temperature and pressure of even very unstable compounds is usually very high, provided there exist but a few atoms in the molecule. For example, AuCl_3 , ozone, and the oxides of nitrogen, although very unstable at ordinary temperatures, seem capable of existing at very high temperatures. In general, the greater the number of atoms contained in the molecule the lower the critical temperature of decomposition, as is evident from the general observation that the more complex a compound is the easier it is to decompose. Many of the very complex carbon compounds—for example, the

proteids—have, on account of their complexity, critical temperatures of decomposition which lie very close to the normal temperature of the earth's surface.

If, now, by some means we proceed to add on atoms to such a molecule so as to make it more and more complex, we would steadily lower its critical temperature of decomposition, and by adding on a suitable kind and number of atoms we could reduce the critical temperature and pressure of the compound until they coincided with the normal temperatures and pressures which hold upon the earth's surface. Such a compound would be possessed of an extraordinary sensitiveness to external influences on account of the sharpness of the constants called above the critical temperature and pressure of the compound. The slightest increase of temperature or decrease of pressure would serve to throw it into a condition of rapid chemical decomposition, whereas a slight increase of pressure and decrease of temperature would cause it to cease to decompose. Even did we maintain the external temperature and pressure exactly at the critical temperature and pressure of the compound, nevertheless the external impulses which are continuously pervading all space in the neighbourhood of the solar system, beating intermittently upon the sensitive substance, would be sufficient to throw it into a series of rapidly alternating states of decomposition and repose.

I suggest that the temperature range of animal life is probably nothing more or less than the range of the critical temperatures of decomposition of a series of certain very complex carbon compounds which are grouped together under the name "protoplasm," the external pressure of the atmosphere coinciding roughly with their critical pressures of decomposition. In fact, I suggest that just as a tuning-fork is set into motion by vibrations of a certain definite frequency and by no others, so living matter is so constructed as to respond continuously to the incessant minute fluctuations in the external conditions which hold upon the earth, the state of response being what is known as life. The temperature of animal life keeps remarkably constant, as it should do on our supposition, a temperature too high exceeding the critical temperature of decomposition of living matter and so destroying its structure, while a temperature too low causes it to cease to decompose, and the living matter becomes inactive.

GEOFFREY MARTIN.

University of Kiel, April 4.

[THE writer of the above will see his "suggestion" discussed in Lockyer's "Inorganic Evolution," book iii.—ED. NATURE.]

Experiment on Pressure due to Waves.

I HAVE seen both in the *Physikalische Zeitschrift* (January) and in the *Physical Review* (February) an account of an experiment by Prof. R. W. Wood to demonstrate the pressure due to waves, and which he suggests as a lecture demonstration of the effect observed by Lebedeff and by Nichols and Hull. The same experiment is quoted by Prof. Poynting in his address on this subject to the Physical Society of London (*Phil. Mag.*, April). I venture to suggest that the experiment, which consists in setting a small windmill in motion by means of Leyden jar discharges maintained by a transformer, will bear a different explanation. It was shown long ago (1793) by Kinnersley, of Philadelphia, in his "Electrical Thermometer," that a jar discharge produces in air a violent explosive effect, which we should now explain by the repulsion between constituents of the current in opposite phase to one another. The repulsive force may be very great. I think it is this explosive effect that Prof. Wood shows in the experiment, and not the pressure due to reflection of a continuous train of waves. I do not think that the suggestion is new, but it appears to me that the same cause may account for the disruption which occurs when lightning strikes a building, an instance of which is recorded in *NATURE* of April 13 (p. 565) in the displacement of some of the blocks of the small pyramid.

SIDNEY SKINNER.

South-Western Polytechnic, Chelsea, April 15.