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ery small portion of a heavy
light gas; and it therefore
ate argon, with the view of
omplex.
had placed at our disposal
ities of liquid air, and it was
n which we had obtained
reduced pressure. By means
was allowed to enter a small
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ler pump, by means of which
thoroughly exhausted. The
the same time a considerable
separate, partially round the
low the surface of the liquid
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erature was kept low for some
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raction consisting of about
should contain the light gas
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l, and was found to have the
the air was now allowed to
ay into a separate gas-holder,
nsed in the upper portion of
ate quickly, and that portion
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connection with the Töpler
tinued until the liquid had
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owered. The solid volatilised
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ised, and the jacketing tube
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r, the solid was seen to melt,

ixed with oxygen, and sparked
e oxygen with phosphorus
e, and the spectrum examined.
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nt, and a brilliant yellow line,
ines were numerous, but com
ave-length of the yellow line
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lentical with those of sodium,
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... ... 5889·0
... ... 5875·9
... ... 5866·5
.. .. 5849·6

we propose to name "neon"
A bulb of 32·35 cubic
ple of neon at 612·4 mm. pr
92° it weighed 0·03184 gram.

... ... 14·67

what we had hoped to obtain
position in the periodic table,
Assuming the density of argon
n 10, the sample contains
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2 to 14·7 shows that there is
gas can be further purified

ctionation, the density has been

That this gas is a new one is sufficiently proved, not merely by the novelty of its spectrum and by its low density, but also by its behaviour in a vacuum-tube. Unlike helium, argon, and krypton, it is rapidly absorbed by the red-hot aluminium electrodes of a vacuum-tube, and the appearance of the tube changes, as pressure falls, from carmine red to a most brilliant orange, which is seen in no other gas.

We now come to the gas obtained by the volatilisation of the white solid which remained after the liquid argon had boiled away.

When introduced into a vacuum-tube it showed a very complex spectrum, totally differing from that of argon, while resembling it in general character. With low dispersion it appeared to be a banded spectrum, but with a grating, single bright lines appear, about equidistant through the spectrum, the intermediate space being filled with many dim, yet well-defined lines. Mr. Baly has measured the bright lines, with the following results. The nearest argon lines, as measured by Sir William Crookes, are placed in brackets:—

Reds very feeble, not measured.		
First green band, first bright line	5632·5	(5651 : 5619)
First green band, second bright line	5583·0	(5619 : 5567)
First green band, third bright line	5537·0	(5557 : 5320)
Second green band, first bright line	5163·0	(5165)
Second green band, second bright line ...	5126·5	(5165 : 5065) brilliant.
First blue band, first bright line	4733·5	(4879)
First blue band, second bright line	4711·5	(4701)
Second blue band, first bright line	4604·5	(4629 : 4594)
Third blue band (first order)	4314·0	(4333 : 4300)
Fourth blue band (second order)	4213·5	(4251 : 4201)
Fifth blue band (first order), about	3878	(3904 : 3835)

The red pair of argon lines was faintly visible in the spectrum. The density of this gas was determined with the following results:—A globe of 32·35 c.c. capacity, filled at a pressure of [illegible] mm., and at the temperature 17·43°, weighed 0·05442 grms. The density is therefore 19·87. A second determination, made after sparking, gave no different result. This density does not sensibly differ from that of argon.

Thinking that the gas might possibly prove to be diatomic, we proceeded to determine the ratio of specific heats:—

Wave-length of sound in air	... ...	34·18
,, ,, gas	... ...	31·68
Ratio for air	... ...	1·408
,, gas	... ...	1·660

The gas is therefore monatomic.

Inasmuch as this gas differs very markedly from argon in its spectrum, and in its behaviour at low temperatures, it must be regarded as a distinct elementary substance, and we therefore propose for it the name "metargon." It would appear to hold the position towards argon that nickel does to cobalt, having approximately the same atomic weight, yet different properties.

It must have been observed that krypton does not appear during the investigation of the higher-boiling fraction of argon. This is probably due to two causes. In the first place, in order to prepare it, the manipulation of air, amounting to no less than 60,000 times the volume of the impure sample which we obtained was required; and in the second place, while metargon is a solid at the temperature of boiling air, krypton is probably a liquid, and therefore more easily volatilised at that temperature. It may also be noted that the air from which krypton has been obtained had been filtered, and so freed from metargon. A full account of the spectra of those gases will be published in due course by Mr. E. C. C. Baly.

University College, London.

ON THE STABILITY OF THE SOLAR SYSTEM.[1]

ALL persons who interest themselves in the progress of celestial mechanics, but can only follow it in a general way, must feel surprised at the number of times demonstrations of the stability of the solar system have been made.

Lagrange was the first to establish it, Poisson then gave a new proof; afterwards other demonstrations came, and others will still come. Were the old demonstrations insufficient, or are the new ones unnecessary?

The astonishment of these persons would doubtless be increased if they were told that perhaps some day a mathematician would show by rigorous reasoning that the planetary system is unstable. This may happen, however; there would be nothing contradictory in it, and the old demonstrations would still retain their value.

The demonstrations are really but successive approximations; they do not pretend to strictly confine the elements of the orbits within narrow limits that they may never exceed, but they at least teach us that certain causes, which seemed at first to compel some of these elements to vary fairly rapidly, only produce in reality much slower variations.

The attraction of Jupiter, at an equal distance, is a thousand times smaller than that of the sun; the disturbing force is therefore small; nevertheless, if it always acted in the same direction, it would not fail to produce appreciable effects. But the direction is not constant, and this is the point that Lagrange established. After a small number of years two planets, which act on each other, have occupied all possible positions in their orbits; in these diverse positions their mutual action is directed sometimes one way, sometimes in the opposite way, and that in such a fashion that after a short time there is almost exact compensation. The major axes of the orbits are not absolutely invariable, but their variations are reduced to oscillations of small amplitude about a mean value.

This mean value, it is true, is not rigorously fixed, but the changes which it undergoes are extremely slow, as if the force which produces them was not a thousand times, but a million times smaller than the solar attraction. One may, therefore, neglect these changes, which are of the order of the square of the masses. As to the other elements of the orbits, such as the eccentricities and the inclinations, these may acquire round their mean value wider and slower oscillations, to which, however, limits may easily be assigned.

This is what Lagrange and Laplace pointed out, but Poisson went further. He wished to study the slow changes experienced by the mean values—changes to which I have already referred, and which his predecessors had at first neglected. He showed that these changes reduced themselves again to periodic oscillations round a mean value which is only liable to variations a thousand times slower.

This was a step further, but it was still only an approximation. Since then further advance has been made, but without arriving at a complete definitive and rigorous demonstration. There is a case which seemed to escape the analysis of Lagrange and Poisson. If the two mean movements are commensurable among themselves, at the end of a certain number of revolutions, the two planets and the sun will be found in the same relative situation, and the disturbing force will act in the same direction as at first. The compensation, to which I have referred, will not any more be produced, and it might be feared that the effects of the disturbing forces will end by accumulating and becoming very considerable. More recent works, amongst others those of Delaunay, Tisserand, and Gyldén, have shown that this accumulation does not actually occur. The amplitude of the oscillations is slightly increased, but remains, nevertheless, very small. This particular case, therefore, does not escape the general rule.

The apparent exceptions have not only been dispensed with, but the real reasons of these compensations, which the founders of celestial mechanics had observed, have been better explained. The approximation has been pushed further than was done by Poisson, but it is still only an approximation.

It can be shown, in certain particular cases, that the elements of the orbit of one planet will return an infinite number of times to very nearly the initial elements, and that is also probably true in the general case; but it does not suffice. It should be shown

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[1] Translation of a paper, by M. H. Poincaré, in the *Annuaire du Bureau des Longitudes*, 1898.

that these elements will not only regain their original values, but that they will never deviate much from them.

This last demonstration has never been given in a definite manner, and it is even probable that the proposition is not strictly true. The statement that is true, is that the elements can only deviate extremely slowly from their original values, and this after a long interval of time. To go further, and affirm that these elements will remain not for *a very long time*, but *always* confined within narrow limits, is what we cannot do.

But the problem does not take this form.

The mathematician only considers fictitious bodies, reduced to simple material points, and subject to the exclusive action of their mutual attractions, which rigorously follows Newton's law. How would such a system behave, would it be stable? This is a problem which is as difficult as it is interesting for an analyst. But it is not one which actually occurs in nature. Real bodies are not material points, and they are subject to other forces than the Newtonian attraction. These complementary forces ought to have the effect of gradually modifying the orbits, even when the fictitious bodies, considered by the mathematician, possess absolute stability.

What we must ask ourselves then is, whether this stability will be more easily destroyed by the simple action of Newtonian attraction or by these complementary forces.

When the approximation shall be pushed so far that we are certain that the very slow variations, which the Newtonian attraction imposes on the orbits of the fictitious bodies, can only be very small during the time that suffices for the complementary forces to destroy the system; when, I say, the approximation shall be pushed as far as that, it will be useless to go further, at least from the point of view of application, and we must consider ourselves satisfied.

But it seems that this point is attained; without quoting figures, I think that the effects of these complementary forces are much greater than those of the terms neglected by the analysts in the most recent demonstrations on stability.

Let us see which are the most important of these complementary forces. The first idea which comes to mind is that Newton's law is, doubtless, not absolutely correct; that the attraction is not rigorously proportional to the inverse square of the distances, but to some other function of them. In this way Prof. Newcomb has recently tried to explain the movement of the perihelion of Mercury. But it is soon seen that this would not influence the stability. It is true, according to a theory of Jacobi, that there would be instability if the attraction were inversely proportionate to the cube of the distance. It is easy by rough reasoning to account for this; with such a law, the attraction would be great for the small distances and extremely feeble for great distances. If therefore, for any reason, the distance of one of the planets from the central body were to increase, the attraction would diminish rapidly until it would not be capable of retaining the planet in its orbit. But that only takes place with laws very different from that of the square of the distances. All laws, near enough to that of Newton's to be acceptable, are equivalent from the stability point of view.

But there is another reason which opposes the theory that bodies move without ever deviating much from their original orbits. According to the second law of thermodynamics, known by the name of Carnot's Principle, there is a continual dissipation of energy, which tends to lose the form of mechanical work and to take the form of heat. There exists a certain function called entropy, which it is unnecessary to define here; entropy, according to this second law, either remains constant or diminishes, but can never increase. When once it has deviated from its original value, which it can only do by diminishing, it can never return again, as it would have to increase. The world consequently could never return to its original state, or to a slightly different state, so soon as its entropy has changed. It is the contrary of stability.

But the entropy diminishes every time that an irreversible phenomenon takes place, such as the friction of two solids, the movement of a viscous liquid, the exchange of heat between two bodies of different temperatures, the heating of a conductor by the passage of a current. If we observe, then, that there is not in reality a reversible phenomenon, that the reversibility is only a limiting case—an ideal case which nature can more or less approach but can never attain—we shall be led to conclude that instability is the law of all natural phenomena.

Are the movements of the heavenly bodies the only ones to escape? One might believe it by seeing that they move in a vacuum, and are thus free from friction. But is the planetary vacuum absolute, or do the bodies move in tremely attenuated medium of which the resistance is extr feeble, but nevertheless is capable of offering resistance?

Astronomers have only been able to explain the movem Encke's comet by supposing the existence of such a medium. the resisting medium which would account for the ano of this comet, if it exists, is confined to the immediate bourhood of the sun. This comet would penetrate it; but distances at which the planets are, the action of this me would cease to make itself felt, or would become much feeble. As an indirect effect, it would accelerate the ments of the planets; losing energy, they would tend to the sun, and by reason of Kepler's third law the duration revolution would diminish at the same time as the dista the central body. But it is impossible to form any idea rapidity with which this effect would be produced, as we no notion of the density of this hypothetical medium.

Another cause to which I am now going to refer must it seems, a more rapid action. It had for some time imagined, but was first more especially brought to light Delaunay, and afterwards by G. Darwin.

The tides, which are direct consequences of celestial ments, could only stop if these movements ceased. B oscillations of the seas are accompanied by friction, and quently produce heat. This heat can only be borrowed fro energy which produces the tides—that is to say, to the of the celestial bodies. We can therefore foresee that, f reason, this *vis viva* is gradually dissipated, and a little tion will enable us to understand by what mechanism surface of the seas, raised by the tides, presents a kind of If high tide took place at the time of the meridian pas the moon, this surface would be that of an ellipsoid, the which would pass through the moon. Everything wo symmetrical in relation to this axis, and the attraction moon on this wave could neither slow down nor acceler terrestrial rotation. This is what would happen if there no friction; but in consequence of this friction, high tide on the moon's meridian passage; symmetry ceases; the tion of the moon on the wave no longer passes throu centre of the earth, and tends to slow down the rotation globe.

Delaunay estimated that, for this cause, the length sidereal day increases by one second in a hundred tho years. It is thus he wished to account for the secular ation of the moon's motion. The lunation would seem to become shorter and shorter, because the unit of time to we ascribed it, the day, would become longer and longer

Whatever we may think of the figures given by De and the explanation which he proposes for the ano of the moon's movement, it is difficult to dispute the produced by the tides.

It is just this that may help us to understand a well- but very surprising fact. It is known that the period of of the moon is exactly equal to that of its revolution; in way that, if there were seas on this body, they would tides—at least, tides due to the attraction of the earth; for an observer situated at a point on the surface of the the earth would be always at the same height above the It is also known that Laplace tried to explain this coincidence. How can the two velocities be *exactly* the It is exceedingly improbable that this strict equality is mere chance. Laplace supposes that the moon has the an elongated ellipsoid; this ellipsoid behaves like a pe which would be in equilibrium when the major axis is along the line joining the centres of the two bodies.

If the *initial* velocity of rotation differs slightly fro of revolution, the ellipsoid will oscillate about its po equilibrium without ever deviating much from it. A p which has received a slight impetus behaves in this way *mean* velocity of rotation is then exactly the same as the position of equilibrium round which the major axis os it is, therefore, the same as that of the straight line whi the centres of the two bodies. It is therefore strictly e the velocity of revolution.

If, on the contrary, the initial velocity differs cons from the velocity of revolution, the major axis will not any more round its position of equilibrium, like a pe which under a strong impulse describes a complete circle.

It suffices, therefore, that the velocity of revolution

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almost equal to the *initial* velocity of rotation, in order that it may be exactly equal to the *mean* velocity of rotation. A strict equality being no longer necessary, the paradox does not exist any more. The explanation is nevertheless incomplete. What is the reason of this approximate equality, of which the probability is no longer zero, it is true, but still very small? And, especially, why does not the moon undergo slight oscillations about its position of equilibrium (if we eliminate, of course, its numerous librations, due to other well-known causes)? These oscillations must originally have existed; they must have become extinct by a kind of friction, and everything tends to make us believe that the mechanism of this friction is that which I have just analysed with respect to the ocean tides.

When the moon was not yet solid, and formed a fluid in the form of a spheroid, this spheroid must have experienced enormous tides, by reason of the proximity of the earth and of its mass. These tides could only have ceased when the oscillations became almost entirely extinct.

It seems that Jupiter's satellites, and the two planets nearest the sun, Mercury and Venus, have also a rotation, the duration of which is the same as that of their revolution; it is doubtless for the same reason.

It might be thought that this tidal action has no connection with our subject. I have as yet only spoken of rotations, and in the studies relative to the stability of the solar system the movements of translation are only dealt with; but a little attention shows that the same action makes itself equally felt on the latter.

We have just seen that the attraction of the moon on the earth does not act exactly through the centre of the earth. The attraction of the earth on the moon, which is equal and exactly opposite, would not pass either through this centre; that is to say, through the focus of the lunar orbit. A disturbing force is the result, very small in reality, but sufficient to make the moon increase in energy. The active force of translation thus gained by the moon is evidently smaller than that of rotation, lost by the earth; because a part of the energy must be transformed into heat in consequence of the friction engendered by the tides. The period of revolution of the moon lasting about twenty-eight sidereal days, a very simple calculation shows that this body gains twenty-eight times less *vis viva* than the earth loses.

I have already explained the action of a resisting medium; I have shown how, by making the planets lose energy, their movements are accelerated; on the contrary the action of the tides, by increasing the energy of the moon, retards its movements; the month lengthens therefore as well as the day. Now if this cause acts alone, what is the final state towards which the system will tend? Obviously this action would only stop when the tides have ceased—that is to say, when the rotation of the earth would have the same duration.

This is not all: in the final state the orbit of the moon must have become circular. If it were otherwise, the variations of the distance of the moon to the earth would suffice to produce tides. As the movement of rotation would not have changed, it would be easy to calculate what angular velocity would be common to the earth and to the moon. One finds that, at the limit, the month, like the day, would last about sixty-five of our actual days.

Such would be the final state if there were no resisting medium, and if the earth and the moon existed alone.

But the sun also produces tides, the attraction of the planets likewise produces them on the sun. The solar system therefore would tend to a condition in which the sun, all the planets and their satellites, would move with the same velocity round the same axis, as if they were parts of one solid invariable body. The final angular velocity would, on the other hand, differ little from the velocity of revolution of Jupiter. This would be the final state of the solar system if there were not a resisting medium; but the action of this medium, if it exists, would not allow such a condition to be assumed, and would end by precipitating all the planets into the sun.

It must not be thought that a solid globe which was not covered by seas would, by the absence of tides, find itself free from actions analogous to those just mentioned, even by admitting that the solidification had reached the centre of the globe. This body, which we suppose solid, would not on that account be an invariable one; such bodies only exist in textbooks on rational "mechanics." It would be elastic and be subject, by the attraction of neighbouring celestial bodies, to deformations analogous to tides and of the same order of magnitude.

If the elasticity were perfect, these deformations would occur without loss of work, and without the production of heat. But perfectly elastic bodies do not exist. There would be in consequence development of heat, which would take place at the expense of the energy of rotation and translation of the bodies, and which will produce absolutely the same effects as the heat engendered by the friction of the tides.

This is not all: the earth is magnetic, and very probably the other planets and the sun are the same. The following well-known experiment is one which we owe to Foucault: a copper disc rotating in the presence of an electromagnet suffers a great resistance, and becomes heated when the electromagnet is brought into action. A moving conductor in a magnetic field is traversed by induction currents which heat it; the produced heat can only be derived from the *vis viva* of the conductor. We can therefore foresee that the electrodynamic actions of the electromagnet on the currents of induction must oppose the movement of the conductor. In this way Foucault's experiment is explained. The celestial bodies must undergo an analogous resistance because they are magnetic and conductors.

The same phenomenon, though much weakened by the distance, will therefore be produced; but the effects, being produced always in the same direction, will end by accumulating: they add themselves, besides, to those of the tides, and tend to bring the system to the same final state.

Thus the celestial bodies do not escape Carnot's law, according to which the world tends to a state of final repose. They would not escape it, even if they were separated by an absolute vacuum. Their energy is dissipated; and although this dissipation only takes place extremely slowly, it is sufficiently rapid that one need not consider terms neglected in the actual demonstrations of the stability of the solar system.

ON THE USE OF METHYLENE BLUE AS A MEANS OF INVESTIGATING RESPIRATION IN PLANTS.

IT has long been known that methylene blue is capable of being decolorised by reducing agents, and the object of the present communication is to point out its use as a means of demonstrating in a striking manner the reducing power possessed either by living protoplasm or at any rate by substances intimately associated with the exercise of its vitality. Its employment is not new to animal physiologists, but botanists appear not to have recognised the possibilities latent in the method, perhaps because some ten years ago Pfeffer ("Oxydationsvorgänge in Lebenden zellen") stated that although fermenting yeast would decolorise the blue solution, green plants would not do so. Doubtless this was true under the conditions of Pfeffer's experiments, but nevertheless many green plants are, as a matter of fact, found to give admirable results.

If germinating seedlings of barley or peas be placed in test tubes filled with a 0·0005 per cent. solution of methylene blue, which has been boiled in order to expel air, it will be found that in the course of a few hours the liquid around them will have lost its colour. The most striking way of performing the experiment is to suspend the peas in the solution, then a decolorised zone is formed between the upper and lower parts of the liquid, each of which still retain its blue colour. Gradually the clear zone extends until the entire mass of the liquid, except just at the surface where it is in contact with the air, becomes decolorised. At first the radicles of the seedlings are strongly stained; they finally again become white.

If some of the decolorised liquid be drawn off by means of a pipette, and shaken up with air, the blue tint speedily returns. If some of the seedlings be removed from the now colourless liquid, and be rinsed in boiled water and then exposed to the air, they soon become blue, and the stain gradually extends into the internal tissues as these become gradually aerated. The "development" of the blue can readily be seen in sections, quickly made, under the microscope.

Cress seedlings are far more active than either barley or peas, just as would have been expected from the relations which they exhibit towards oxygen.

But perhaps the most remarkable results are those obtained from a plant like *Chara*. This alga is suspected to possess peculiar properties in regard to its connection with