OUR ASTRONOMICAL COLUMN.

Comet 1898 VII. (Coddington-Pauly).—Mr. C. J. Merfield, of Sydney Observatory, gives an ephemeris of this comet, in *Astronomische Nachrichten* (Bd. 148, No. 3542), as he considers it likely that it will be possible to observe the comet from northern observatories.

1899.		α (app.) h. m. s.		δ (app.) ° ′ ″
Feb. 10	...	2 18 2	...	−35 36 44
14	...	23 11	...	33 36 25
18	...	28 16	...	31 40 50
22	...	33 17	...	29 50 0
26	...	38 15	...	28 3 48
Mar. 2	...	43 10	...	26 22 9
6	...	48 3	...	24 44 57
10	...	52 52	...	23 12 4
14	..	2 57 40	...	−21 43 25

The comet is rapidly moving northwards, passing from near φ Fornacis into Eridanus. It should be looked for soon after sunset, almost due south from Mira Ceti. It is said to be easily visible with an instrument of six inches aperture.

Eros (433).—In *Harvard College Observatory Circular*, No. 37, Prof. E. C. Pickering describes the finding of trails of the planet on thirteen more plates, in addition to those mentioned in the last *Circular*. These plates were exposed during the period 1893–6, and the estimated photographic magnitude varied from 8·2 to 12·5. He lays great stress on the fact that all the photographs which have been found showing the planet have been taken with *doublet* objectives, giving a large field and large relative aperture. The difficulty is enhanced by the variation in the brightness of the planet, as during the last eleven years it has only been brighter than the ninth magnitude for two months.

In *Popular Astronomy*, January 1899, Mr. W. W. Payne brings together the information given in various disconnected articles by several authors. This will prove convenient for many interested in the planet, and unable to find access to the individual papers.

The Sun's Heat.—In the *Astronomische Nachrichten* (Bd. 148, No 3540), Dr. T. J. J. See introduces a new law bearing on the thermodynamics of a contracting gas, and discusses its bearing on the question of the heat of the sun, and also its application to estimating the relative ages of the stars and nebulæ.

The modern theory of the sun's heat is primarily due to Helmholtz, and its conclusions are based on the supposition that the sun's mass is of *homogeneous* density. This Prof. See doubts, and the result of a series of computations for the heat given out by the contraction of a *heterogeneous* mass was the law of temperature he now brings forward. The effect of this unequal density is to lengthen considerably the period heretofore advanced for the duration of the sun as a light- and heat-giving source. Helmholtz's theory indicates that the output of heat for a given change in radius of the contracting mass increases very rapidly as the radius itself becomes small. From this it appears that the greatest amount of heat is produced when the mass has reached its least dimensions and contraction is about to cease.

Discussing the resulting temperature of a mass contracting under its own gravity, the law deduced is: "The absolute temperature of a gaseous star or nebula condensing under its own gravitation varies inversely as the radius of the contracting mass." The curve representing this condition will be recognised as a rectangular hyperbola referred to its asymptotes.

With this idea it would follow that at first when the nebula is infinitely expanded, its temperature is the absolute zero of space, and that this gradually rises to a maximum when the mass has contracted to the smallest radius consistent with the laws of gases. After liquefaction has set in, free contraction is obstructed and finally ceases; the temperature falls, and the body becomes finally invisible.

This is in accord with the idea of the nebulæ being low temperature bodies. As it contracts the production of heat exceeds the radiation, and the temperature rises inversely as the radius decreases.

In this connection Prof. See mentions the curves of Lane for the laws of internal temperature and density of gaseous masses. (*Am. Jour. Sci.*, July 1870.) He illustrates these by the case of the sun, and infers that it is *increasing* in temperature still. The presence of hydrogen in the white stars he reasons in the same manner. While the nebula is yet considerably extended, gravity is small, and all the elements float in the atmosphere without regard to relative atomic weight, and such produce spectra with many substances, as we see in such solar stars as Capella, Arcturus, &c. When the mass is further condensed, the heavier elements are kept relatively lower by the increased gravitation, and hydrogen, the lightest of the elements, is present as the exterior envelope, and hence the simplified spectrum of the Sirian stars.

The phenomenon of variable stars with dark companions is accounted for on this assumption, the two being of the same age but of greatly different masses. In the case of coloured double stars the companion is generally blue or purple, and the large star yellow or red, which again is in accord with this theory.

Taking the present temperature of the sun to be 8000° C., he calculates that the temperature of the central nebula at the time of formation of the earth was less than 40° C.; the earth beginning at this, contracted until it rose to about 2000° C., which is high enough to account for all known geological phenomena. Jupiter and Saturn are considered, on similar grounds, to be still gaseous and increasing in temperature, and though not now self-luminous, may eventually become so. In conclusion, Prof. See suggests that as the nebulæ are at low temperatures many of them may be invisible, although existent. Many nebulæ have been photographed by the ultra-violet light they emit, which are quite invisible in the most powerful telescopes. If this be true, the numerical predominance of stars over nebulæ, visually, is explained, as according to the nebular hypothesis, the two classes of bodies should exist in approximately equal numbers.

THE CONSTITUTION OF THE ELECTRIC SPARK.[1]

When an electric spark passes between metallic electrodes, the spectrum of the metal appears, not only in immediate contact with the electrodes, but stretches often across, from pole to pole. It follows that during the short time of the duration of the spark, the metal vapours must be able to diffuse through measurable distances.

The following investigation was undertaken primarily to measure this velocity of diffusion with the special view of comparing different metals, and different lines of the same metal.

Feddersen published, in the year 1862, an interesting research, in which photographs of sparks passing between different metal poles are taken after reflection from a rotating mirror. He could from his experiments draw some conclusions which have a bearing on the subject, but it was necessary for our purpose that the light should also be sent through a spectroscope, so as to distinguish between the luminous particles of air and those of the metal poles.

The method of the rotating mirror tried during the course of several years in various forms by one of us, did not prove successful. On the other hand, good results were obtained at once on trying the method used by Prof. Dixon, in his researches on explosive waves. This method consists in fixing a photographic film round the rim of a rotating wheel. All that is necessary for its success is to have sparks so powerful that each single one gives a good impression of its spectrum on the film. Were the sparks absolutely instantaneous, the images taken on the rotating wheel would be identical with those developed on a stationary plate, but on trial this is found not to be the case. The metal lines are found to be inclined and curved when the wheel rotates, and their inclination serves to measure the rate of diffusion of the metallic particles. The air lines, on the other hand, remain straight, though slightly widened.

To avoid the tendency of the film to fly off the wheel when fixed round its rim, as in the original form of the apparatus, a spinning disc was constructed for us by the Cambridge Scientific Instrument Company. The film is placed flat against the disc, and is kept in place by a second smaller disc, which can be screwed lightly to the first. The diameters of the two discs are 33 and 22·2 cm., the photographs being taken in the annular space of 10·8 cm., left uncovered by the smaller disc. An electric motor drives the disc, and we have obtained velocities of 170 turns per second, though in our experiments the number of revolutions was generally about 120, giving a linear velocity

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[1] By Prof. Arthur Schuster, F.R.S., and G. Hemsalech. Read before the Royal Society February 2.

of about 100 metres/second for that part of the film on which the photograph was taken.

The electric discharges were obtained from a battery of six Leyden jars, having a total capacity of 0·033 microfarad, and being charged from an induction machine constructed for us by Mr. H. C. Wimshurst. This machine has twelve plates of 62 cm. diameter, and gives sparks which are 13 inches long. The electrodes were, as a rule, placed 1 cm. apart, and an image of the spark was projected on the slit of the spectroscope, the distance of the slit from the electrodes being equal to four times the focal length of the projecting lens, so that the image was equal in size to the spark. The prism used was made by Steinheil, and had a refracting angle of 60°.

We may now pass to the description of the results obtained when the spectrum of a single spark is taken on a moving film. A preliminary trial with various metallic electrodes had shown us that the sharpest results were obtained with zinc, and we therefore chose that metal for our first investigation. The principal lines of zinc as they appear on our photographs are the double line, the least refrangible of the two having a wave-length 4924·8, and the blue triplet, the wave-length of the leading line being 4810·7. All the lines are curved on the photographs taken with the spinning disc, but the displacements, especially near the poles, are subject to considerable variations. This is probably due to the fact that the path of the metallic particles is not always straight, and, if straight, its image does not necessarily coincide with the slit. A very slight error in measurement will also affect the results considerably when the total displacement measured is small. Our results do not for this reason allow us at present to give any opinion as to the maximum velocity of the particles near the pole; but if these are considerable, they drop down very quickly to speeds which, in the case of zinc, are not far off 500 metres/second.

We have adopted two methods of comparison between different photographs. We have in the first place measured the displacements at a number of nearly equidistant points, and from these measurements we have deduced the time taken for a metallic molecule to pass from the pole to a point 2 mm. away from it. If this method could be applied in every case, it would form a rational and consistent basis of comparison. But the curved lines which are to be measured are often very diffuse near the pole, this, and the continuous spectrum, may render it impossible to obtain satisfactory measurements at that point. In order not to have to reject unnecessarily a large number of measurements because the spectrum near the pole was indistinct, we have adopted another method, which, though less rational than the first, is found to give consistent results. From all our measurements we may deduce certain figures for the molecular velocities at different and generally equidistant points on the photographs, and may take the average of all these figures as the mean velocity of the particle. In the following tables, V_1 will always refer to the mean velocity between the pole, and a point 2 mm. away from it, while V_2 refers to the average velocity taken for different distances, as just explained. The influence of change of capacity and change in the length of the spark was investigated in the case of zinc, and the following tables exhibit the results. As the zinc lines are sharp near the pole, the first of the above methods of measurement could be applied.

TABLE I.—*Average Velocity (V_1) in metres/second of Zinc Molecules.*

Sparking distance.	Wave-length.	Number of jars.		
		2.	4.	6.
cm. 0·51	4925	814	556	416
	4811	1014	668	529
1·03	4925	400	499	415
	4811	501	548	545
1·54	4925	723	1061	435?
	4811	1210	1526	492?

The first striking result to be deduced from the table is the uniformly higher velocity deduced from the double line 4925, as compared with that found when one of the lines of the triplet is measured; for we have ascertained that the two first lines of the triplet are always displaced by the same amount, and the third is so much mixed up with the air lines in its neighbourhood that it cannot be measured. It was one of the objects of the investigation to detect, if possible, differences of this kind, which might be accounted for by the fact that the molecules producing different lines of the same spectrum have not necessarily the same mass. We nevertheless hesitate to ascribe the smaller apparent velocity derived from λ = 4925 to this reason. This line, as has been mentioned, is one component of a double line, and the doublet is not resolved on the photographs taken with the moving film. Near the pole where the light is strong, the edge of the least refrangible component of the doublet would be considered to be the least refrangible edge of the doublet; but near the centre of the spark the light is weaker, and the lines, owing to the motion of the wheel, are drawn out towards the violet. The most intense portion of the image will here be that part where the two lines are superposed, and, in wishing to set the cross wire on the edge of the line, we should be tempted to set it on the edge of the *most* refrangible component. There is reason to believe that this is the cause of the greater deflection of the double line, and the photographs show some signs that if this source of error is eliminated, the molecule giving out the double line moves more quickly than that giving rise to the triplet. We reserve the decision of this point until we have been able to apply greater dispersion.

Comparing the spark obtained with different capacities, it is found that when the spark gap is small, there seems a very curious *diminution* of velocity as the capacity increases; this is not what should have been expected at first sight, as with the large number of jars we should expect higher temperatures, and therefore greater velocity of diffusion. When the spark gap is 1 cm., the experiments do not reveal any marked change due to capacity. When the gap is increased still further the sparks become very irregular and unsteady, and no certain conclusions can be drawn from our measurements; the numbers marked with a query are specially doubtful. When six jars are used practically identical numbers are obtained for all sparking distances, but with small capacity the centimetre spark seems to give a lower result than in the two other cases. While we should not like at present to consider this as an established result, the table serves to show that the centimetre spark and the highest capacity used gives the most consistent numbers, and our experiments with other metals were all made under these conditions, except in the case of bismuth, where clearer spectra were obtained with only two jars.

Comparing different metals with each other, we find in the first place that those having comparatively low atomic weights, viz. aluminium and magnesium, have higher molecular velocities. With magnesium the metal vapour is scattered about to such an extent that no measurements could be made, but the average velocity of the aluminium molecule was found to be over three times as great as that of zinc, the numbers not laying any claim to accuracy. Comparing zinc and cadmium with each other, we obtain almost identical numbers, both for the corresponding doublet and triplets.

Bismuth gave remarkable results. In spite of its high atomic weight some of the lines are but little displaced, indicating an average molecular velocity of 1420 metres/second. For other lines the velocity falls down to that of zinc and cadmium, while one line (λ = 3793) has a still smaller velocity.

We have not obtained satisfactory results with mercury; the best were those in which poles used were of zinc or cadmium, which were covered with amalgam. Differences in molecular velocities were obtained for different lines, but the result here is not so certain as with bismuth. There is obviously no simple law connecting these velocities with the atomic weight.

Dr. Feddersen was led through his researches to the conclusion that the metallic particles after being once torn off from the electrodes by the discharge took no further part in it, were thrown irregularly into the space surrounding the electrodes quite independently of the electric current. Although in some cases, and especially with magnesium poles, there is some evidence that this is partly true, we are led to take the following modified view of the matter.

The initial discharge of the jar takes place through the air; it must do so because there is at first no metallic vapour present. The intense heat generated by the electric current volatilises the metal, which then begins to diffuse away from the poles; the subsequent oscillations of the discharge take place through the metallic vapours, and not through the air. We find confirmation of this view in a striking experiment which is easily repeated. If a coil of wire be inserted in the spark circuit of a Leyden jar, which may be charged either by a Wimshurst machine or an induction coil, the air lines disappear almost completely, the metallic lines alone remaining. According to our view we should explain the experiment by saying that the coil which adds self-induction lengthens the duration of the discharge, and allows time for the metallit molecules to diffuse properly into the spark gap. A great part of the energy of the current may then do useful work by heating up the metallic molecules instead of those of air. Mr. Hemsalech is at present engaged in investigating the changes in the metallic spectra which accompany the insertion of self-induction.

The first spark passing through the air will give rise to a sound wave which, during the complete time of the discharge, will only travel a few millimetres. We may therefore consider that the mass of metallic vapours suddenly set free is driven by its own pressure into the partial vacuum formed by the heated air. It would seem more correct to liken the process to that of a gas under pressure flowing into a vacuum than to that of a pure thermal diffusion. There is not much difference between these views, and we may take it that in our experiment we have approximately measured the velocity of sound in the metallic vapours. This gives a relation between their temperature and density. If we neglect the differences in the ratio of specific heat, we find approximately

$$V = 80 \sqrt{T/\rho},$$

where T is the absolute temperature and ρ the vapour density referred to hydrogen. Thus for cadmium the average molecular velocity found was 560, and substituting $\rho = 56$ we obtain $T = 2700$, which seems a possible value. Hence we conclude that the molecule of cadmium in the spark cannot have a mass which is much smaller than that directly determined near the boiling point of the metal.

In conclusion we have also taken some photographs in which the slit was directly focussed on the sensitive film without the interposition of the film. The photographs show a straight image of the slit followed by a number of curved bands extending from both poles into the spark gap.

The straight image we consider to be the initial discharge through air creating sufficient heat to fill the space with vapour through which the oscillating discharges may then pass. Our experiments point to the fact that the periodic time was rather too small in our experiments to give the best results. The metallic molecule before it has had time to reach through a sufficient distance was possibly affected in its motion by the subsequent oscillation. We hope to remedy this defect by introducing still higher capacities than those used. Our experiments allow us to give the following approximate numerical data. The air rendered luminous by the first discharge remains luminous for a time of about 5×10^{-7} seconds, the metallic vapours then begin to diffuse and reach the centre of the spark (the gap being 1 cm. long) in a time which in the case of cadmium was about 6×10^{-6} seconds. The periodic time of the oscillations with our six jars and a circuit possessing as little self-induction as possible was about 2×10^{-6} seconds. The metallic vapours remain luminous in the centre of the spark for a longer period than near the poles, the duration of the time during which some luminosity can be traced with a discharge from six Leyden jars is about $1{\cdot}5 \times 10^{-5}$ seconds.

MR. BALFOUR AND PROF. JEBB ON TECHNICAL AND SECONDARY EDUCATION.

TWO important speeches on technical and secondary education were made during last week—one by Mr. Balfour in opening a new hall which has been erected in connection with the Battersea Polytechnic, and the other by Prof. Jebb at Cambridge. The *Times* reports of the remarks made on these two occasions are abridged below.

MR. BALFOUR ON TECHNICAL EDUCATION.

Everybody interested in the least in the progress of education must watch with the profoundest interest the great experiment now being carried on in this metropolis, and not the least in the building where I am now addressing you, in connection with technical education. If I understand the matter rightly, the experiment differs from any other efforts in the same direction which have been made, either in this country, in other great centres of population, or on the continent of Europe—in Germany or in Switzerland, or in any other countries which have been pioneers in this matter of scientific and technical education.

Every scheme of education has to be considered from two distinct points of view. We have to consider its effect in qualifying the individual who receives the education for the particular work in life which he has to do. That is the first aspect of it; but there is another aspect not less important, which certainly ought never to be lost sight of, and which is not lost sight of in this institution—namely, the general educational results at which any sound system of education ought to aim. There is the technical side and there is the general side. There is the skill infused in the pupil for following that profession in life which he has selected, or which circumstances have forced upon him; and there is that other and that broader aspect in which all education of every kind is intended to co-operate—namely, the development of the general faculties of mind, eye, and body, and also to make a man or a woman a complete citizen, with all their faculties developed to the highest possible point.

Technical Instruction.

Taking these two aspects in turn, and dealing, in the first instance, with the industrial and technical aspect, I do not feel myself qualified to speak with any authority upon that part of the work of this institution which has to do with handicrafts. I understand that the aim of the institution in this connection is to supply those who are engaged in these handicrafts with more theoretical and general instruction in connection with their special pursuit which is required to enable the people to reach the highest results in that pursuit. I cannot imagine a better object. I am not aware that in other places the same object is pursued systematically and successfully in the same way in which it is pursued in the London polytechnics. After all, it has to be recognised that work is mainly to be learnt in the workshops, and I am convinced that no wise teacher would for a moment attempt to substitute either the lecture-room or experiment-room for that which can be learnt only in the workshop. But unquestionably there are branches of knowledge connected with trades and handicrafts which have a theoretical side which cannot with equal facility be learnt except in a place devoted to that side, and I believe that the work done in this institution in that connection is one of the greatest value, not merely to the pupils, but to the trades and industries which they have elected to follow.

Scientific Education.

But there is another side, and, from a national point of view, perhaps a decidedly more important side than that, and the side I mean is the complete scientific equipment of a student for those professions in which a thorough grounding in science, theoretical and practical, is absolutely necessary if he is to make the most of himself and the most of the profession in which he is engaged. I have always been deeply interested in this aspect of the question, which is the one specially considered in Germany and elsewhere, and the value of which we have perhaps in this country until recent years unduly ignored and neglected. It is an interesting question to ask ourselves how it comes about, and why it comes about, that it is only in the latter half of the nineteenth century that the absolute necessity of this thorough scientific grounding has been recognised in connection with great industrial enterprises. And the real reason I take to be this—that it is only after science has developed to a certain point, and after industry has developed to a certain point, that you can, as it were, successfully and usefully combine the two, and that there is forced upon you the necessity of recognising that almost every advance in theoretical science is reflected in a corresponding advance of industrial enterprise, and in like manner industrial enterprise and the practical application of science is itself from day to day giving birth to new scientific conceptions and new improvements either in the machinery of discovery or in the results of discovery. If anybody wishes to have a concrete illustration of this abstract truth I would ask him to make the following comparison. Take for a moment the career of the greatest man of science that this world has ever seen—I mean Sir Isaac Newson. As far as I know—I speak under correction—neither by Sir Isaac Newton himself, nor by