new movement, young engineers found themselves required to learn for the purpose of examinations "facts" which they were well aware from their own experience to be misrepresentations of the real state of affairs. Among the men who have since helped to put these students into the right path and to help them on the way, none were more prominent or are more deserving of praise than Prof. Perry.

In his laboratories the phenomenon of friction, to take this same instance, is dealt with in such a way as to give the student opportunity to exercise all his knowledge of mechanics. Thus he has to test the effects of friction in every part of a mechanism—he is not allowed to forget its existence or to have his mind taken away from it, as may happen so easily when working among the large and complicated machines of the engineering laboratory. Even in so simple a case as that of the spinning of a horizontal fly-wheel by means of the unwinding from its axis of a rope which passes over a pulley, and carries a weight at its far end, the number of problems that arise is very numerous. To instance the variety of information which can be derived even from such a simple experiment as this, we may quote the following from Prof. Perry's "England's Neglect of Science":—

"Let us take this well-used fly-wheel. The M of a fly-wheel, multiplied by the square of its number of revolutions per minute, gives the kinetic energy stored up in it in foot-pounds. You are asked to measure experimentally the M of this fly-wheel; the loop at the end of a cord goes over the pin A on the spindle, and is wrapped n times round the spindle, then goes over the pulley B, and has a weight W at its end. At time O the wheel is let go; in t_1 minutes—carefully observed—the cord drops off; in t_2 minutes from starting the wheel has been brought to rest again by friction. The weight W lb. multiplied by the height in feet through which it has fallen gives the energy stored up in the wheel at time t_1, so that if the speed were then known M could at once be calculated. But as we have no speed indicator, we take it that the motion is uniformly accelerated till the cord drops off or we take $2\frac{n}{t_1}$ as the revolutions per minute at the time t_1. The corrections are of more interest. We have first to deduct the kinetic energy of W when the cord drops off. Then we must make experiments on the friction of the pulley B, for the pull in the cord at C is less than what it is at D, and these experiments are themselves very interesting, for they are made with the two parts C and D vertical, so that the parallelogram of force principle must be brought in to make them available. Next we correct for the friction at the pivots E and F. And here we observe that the average speed from t_1 to t_2 is the same as from O to t_1, and hence that from t_1 to t_2 the motion is uniformly retarded, and hence that there is as much energy wasted in any one revolution as in any other. If, then, we know the number of revolutions from t_1 to t_2 we know the energy wasted in one revolution, and we can correct for friction before the cord drops off, and so we make one correction after another, and there is hardly any limit to the amount of ingenuity required, as the corrections get less and less important. I remember that four grey-headed men worked together once at this piece of apparatus in the evening for five weeks, and when at length they had satisfied themselves with their corrections they had practically used many times every important principle of mechanics, and they had acquired a handy working knowledge of all these principles."

It is hardly possible to set bounds to the usefulness of such calculations as the above in making students think for themselves and, if they have even a moderate acquaintance with mathematics, in assisting them to find themselves, even unwittingly, engaged on what is really an original piece of research.

A mechanics laboratory is by its constitution less adapted than an engineering laboratory to research work of the usual kind, but during the last few years an important piece of original work on the air friction of rotating paper discs has been carried out. A note on the preliminary experiments was read before the Southport meeting of the British Association in 1904 by Mr. W. Odell. Mr. Odell's experiments consisted in measuring electrically the torque necessary to keep in uniform rotary motion circular discs of paper which were mounted on a horizontal shaft. These discs were 15, 22, 27 and 47 inches in diameter, and the torque was measured for (1) different speeds, (2) different diameters. The very interesting result was found that, once the critical speed was passed, the torque was proportional to the nth power of the speed, and that n was about 2·5. It was further found that the critical speed was roughly proportional to the square of the diameter when different discs were used. For a given speed the torque increased with the 5·5th power of the diameter, and this striking result leads to the deduction that, 6 watts only being required to keep a 27-inch disc moving steadily at 550 r.p.m., no less than 32 h.p. would be necessary to keep a 9-feet disc rotating at the same speed. This deduction has a very important bearing on the design of high-speed generators and other machines in which the rotating parts have considerable diameter. Roughly, a one per cent. increase in diameter would lead to an increase in the necessary torque for the same speed to be obtained of no less than $5\frac{1}{2}$ per cent.

A similar plan is followed in the laboratory with regard to problems connected with the torsion of shafts, the flow of water, the bending of beams, the efficiency of mechanisms, the swinging of pendulums, and others of the same kind. The result of such a training on students is that they acquire a kind of instinct in mechanical matters, one which is difficult to describe, but which develops alertness, and would, for instance, lead such a student to doubt immediately the accuracy of the usual measurement of horse-power by means of the average indicator. Even with such a well-made instrument as the Hopkinson reflecting indicator he would not omit to make calculations as to the effect of inertia lag when rapid explosions were being recorded, the effect of damping, and other points. After doing this he would appreciate the more the modest claim of the inventor of a 2 per cent. accuracy, and contrast it with the far more heavily drawn claims of instruments much less carefully designed. To instil this attitude of mind into young students, that they should "test all things" and take nothing for granted, is to lay the basis of a scientific way of thinking which is of fundamental importance to them in after years.

H. E. W.

THE MILKY WAY.[1]

It may be that the limitations imposed upon us by restrictions in time and space will never allow a complete solution of the problems offered by the study of the sidereal universe. But the effort to comprehend the processes that have contributed to its structure, or

[1] (1) "La Distribution des Étoiles par rapport à la Voie lactée d'après la Carte et le Catalogue photographiques du Ciel." Par Paul Stroobant. Extrait des Annales de l'Observatoire royal de Belgique, Annales astronomiques, Tome xi., Fascicule ii.
(2) "Die Milchstrasse." By Prof. Max Wolf. Pp. 48. (Leipzig: J. A. Barth, 1908.) Price 4 marks.

to penetrate the mystery that conceals its destiny, will not be abandoned on account of the difficulty of the problem or the dearth of pertinent facts. There may be little hope that our observations and those of our predecessors will prove adequate to the task of reading the riddle, but the human mind needs very little information to tempt it to form conjectures concerning the order of creation in its widest extent. In this department of science, history unfortunately bears witness rather to the richness of our imagination than to our skill in securing facts. But in recent times, as the contents of the two works under notice show, the tendency has been to limit our excursions into the unknown, and to substitute exact inquiry directed to a definite end, in place of the loose, but possibly plausible, suggestions that did duty for critical examination. In the first-mentioned work M. Stroobant is content to count the stars the positions of which have been recorded in connection with the scheme for the construction of the photographic chart of the heavens. Such work is no doubt tedious and unheroic, but it is eminently useful, and more welcome than any random speculations, however brilliant or startling they might be. The object the author had in view in undertaking this wearisome task was to determine the law of stellar distribution, both on the chart and in the catalogue, according to variation of galactic latitude. For the present the research is limited to the stars in the zones taken at the observatories of Paris, Bordeaux, Toulouse, Algiers, and San Fernando. Of the star charts 879 have been used, containing the total of 985,430 images; and of the catalogue negatives 535, which show the places of 163,009 stars. The celestial surface scrutinised contains 4126 square degrees, approximately one-tenth of the entire surface of the sphere.

One of the by-products of the research is to indicate that the mean magnitude of the faintest stars recorded on the catalogue plates is 11·5 mag., and that of the faintest stars on the chart 13·5 mag., or taking into account the loss of images, unavoidable in reproduction, 13·7 mag. These figures show, so far as this inquiry is trustworthy, that the original proposals for the construction of the international chart have been adhered to very faithfully. A further conclusion is that the total number of stars we may expect to find in the complete catalogue is 2,676,000, and on the chart 9,854,000. These totals are more modest than early and less informed estimates, and M. Stroobant gives reason to think that these numbers will be exceeded when some of the unpublished charts become available. The data supplied from each observatory are discussed separately and fully, but space will not permit more than the reproduction of the final result, which exhibits the conclusions drawn from the whole material under discussion :—

Galactic Latitude	No. of stars in square degree		Stellar density	
° °	Chart	Catalogue	Chart	Catalogue
+90 to +70	91	30	0·14	0·19
+70 ,, +50	83	29	0·13	0·18
+50 ,, +30	140	48	0·21	0·30
+30 ,, +10	337	90	0·51	0·57
+10 ,, −10	660	159	1·00	1·00
−10 ,, −30	344	83	0·52	0·52
−30 ,, −50	130	39	0·20	0·25
−50 ,, −70	111	24	0·17	0·15

The want of exact regularity in the change of these numbers with the latitude is doubtless due to insufficient data, but considering the number of stars involved and the care taken to secure uniformity, the result is probably more trustworthy than that drawn from Herschel's gauges, which indicate a much more rapid increase in stellar density as the latitude diminishes. Further examination shows that the number of stars on the chart increases uniformly in both hemispheres, but that if the faintest stars be excluded and the research limited to those that appear in the catalogue, the density is more marked in the northern than in the southern hemisphere. It is not possible to make any complete inquiry as to the variation of density depending on galactic longitude, but from a preliminary investigation of those regions of the Milky Way where it cuts the celestial equator, M. Stroobant shows that at the ascending node of the Galaxy, the northern border is richer in stars than the southern, and that at the descending node this relation is reversed. It is further pointed out that the increase in the number of stars on the photographic chart does not correspond with the contour lines drawn in naked-eye representations of the Milky Way, and in conclusion the author directs attention to regions of the sky which are very rich in stars, though fairly remote from the central line of the Milky Way, to the pole of which are assigned the coordinates $\alpha = 12h.\ 46m.$, $\delta \pm 28°$.

Prof. Max Wolf addresses himself to a scientific congress, mainly composed of medical men, and necessarily his paper is of a more popular character. He frankly admits that we have but very little knowledge of the true construction of the Milky Way, and that speculation has supplied the place of exact information. By means of excellent photographs he shows the great variety of structure running throughout the Galactic Belt, and indicates the difficulties which any theory of the Milky Way has to surmount. Prof. Max Wolf is intimately acquainted with the literature of the subject, knows the strength and the weakness of the various hypotheses that have been advanced, and treats the many problems that arise in a luminous and interesting manner.

He glances at the various studies that have been made to solve the problem of the possible geometrical form of the Galaxy, from the time when Herschel began his laborious task of counting the stars visible in the field of his telescope, down to that later period when the resources of photography have supplied more information, but at the same time revealed a more complex structure, offering fresh difficulties for solution. Of the different attempts that have been made to represent its true shape, concealed as it is by the curious bifurcations, rifts, condensations, and lacunæ, that suggested by Dr. Easton, of Amsterdam, meets with the greatest favour. In this scheme the Spiral Nebula in Ursa Major has admittedly supplied the model. A nucleus is placed in the constellation Cygnus, and from this central condensation radiate streamers, which can be arbitrarily arranged so that the combined effect can be made to resemble the general aspect of the Milky Way. The objection the author raises to the scheme is that Cygnus does not present that close agglomeration of stars which such an hypothesis requires. It might further be added that in the sketch given, the sun occupies too much the place of a detached spectator, and is apparently quite disconnected from the system.

A feature of great prominence in many photographs of nebulæ is the comparative scarcity of stars in the immediate neighbourhood of the nebula, and Dr. Max Wolf discusses the probable physical connection between the dark lacunæ and the brilliant condensations adjoining. This effect is real, and not due to contrast, for by counting the stars on a photograph within a definite area, and shading the different parts of that area according to the number of images impressed on the film, it is possible to exhibit statistically the relative density of stars surrounding the different nebulæ. This plan has been adopted with great success in the most important nebulæ and clusters, and the diagrams reproduced show the completeness of the connection. The most conspicuous, as it is one of the most interesting instances, is that known as the "cocoon" nebula, where the complex nebulous structure lies concealed at the end of a long channel, extending more than two degrees into the luminous clouds. The author suggests that we have here to do with an absorptive phenomenon, and certainly the appearance warrants the suggestion. But such instances, if less pronounced, are not rare, and therefore it is legitimate to imagine that the whole heavens are more or less hidden by the results of processes still in progress. In that case the Milky Way itself may be regarded as a remnant of an earlier much more extended universe. This hypothesis, as any other we may form, may be quite misleading, but into whatever errors our assumptions may conduct us, it is certain that the Milky Way offers a grand and sublime problem, indicating the action of processes and forces for the adequate description of which we still lack rudimentary conceptions. We stand face to face with a great mystery without the partial unveiling of which our pictured scheme of the cosmos must remain an imperfect patchwork.

SIR JOHN EVANS, K.C.B., F.R.S.

BY the death of Sir John Evans, British archæology has lost one who was amongst its foremost figures for more than fifty years. The son of the late Rev. A. B. Evans, D.D., he was born at Britwell Court in 1823, educated at Market Bosworth School, and entered the business of his maternal uncle, Mr. John Dickinson, F.R.S., the founder of the famous paper factory at Nash Mills. From school young Evans brought with him a genuine love of classical literature and history, and presently he developed a no less strong taste for science, whilst he at once showed business capacity of no ordinary kind. Very soon he directed his attention to geology. Practical reasons may have hastened a natural tendency, as he was led to this study by a dispute respecting the water rights of his uncle's firm, and in a comparatively short time he mastered the principles of that science. He became an active member of the Geological Society, of which he was elected president in 1874.

Sir John Evans's scientific training had a very important bearing on his work as an archæologist, and in no little degree enabled him to make those great advances in British archæology which that science owes to him. Scientific method, combined with his love of historical literature, gave him an equipment for antiquarian studies possessed by but few of his contemporaries. Evans's interests were of the widest, but in his early years they chiefly lay in the coins of the ancient Britons. Though from Camden onwards much had been written about them, Evans for the first time coordinated the entire mass of material, and worked out systematically the evolution of the British types, as Lelewel had done for the Gaulish series and partially attempted for Britain. When, in 1864, Evans published his "Coins of the Ancient Britons," it was at once recognised, not only as a masterly example of learning and minute accuracy of detail, but also as a model of method. He published a supplement to it in 1890, and though his chronology, based on the time supposed to be necessary for the degradation of the original type of the gold stater of Philip II. of Macedon, may not now commend itself, the book must always remain one of our chief authorities for the early history of this island.

But his attention was not confined to the period between the occupation of south-eastern Britain by the Belgæ and the Roman conquest. He worked incessantly at the remains of prehistoric man both on the Continent and in these islands, following the method of the great Scandinavian archæologists. The first results of these labours were embodied in "The Ancient Stone Implements," &c., of Great Britain, published in 1872 (second edition in 1897). Here, of course, his geological knowledge came into play, more especially in reference to the relics left by Palæolithic man in the fluviatile gravels of our own island. Yet all this time he had been working incessantly at the first beginnings of the use of metal, and the fruits of his work in this all-important field were put forth in "The Bronze Implements of Great Britain and Ireland," in 1881. All his three great works are largely based upon and illustrated from his own magnificent collections in the several departments, though these were but a fraction of his vast treasures, which comprised a great series of Greek, Roman, English, and other coins, medals, rings, enamels, and most other classes of antiquities. At a time when so many objects which form an integral part of our national history are constantly finding their way across the Atlantic, it is pleasant to think that the collections amassed at Nash Mills are not to be dispersed under the auctioneer's hammer.

Besides his three master-works. Evans wrote innumerable papers in *Archaeologia,* the *Numismatic Chronicle,* and various other journals, all of which are distinguished by the same rigorous accuracy and keen insight as his larger publications. It is hard for us in this generation to realise clearly all that he did to advance the study of archæology in this country. Though Worsaae and his school had already firmly laid down the principles of archæology in Scandinavia when Evans began his career, in this country such studies were almost entirely in the hands of the Oldbucks and the Simpkinsons, whose fatuities, credulities and wild speculations were scorned by serious historians and mocked at by the general public. Evans's strong common sense, his scientific training, and his instinctive love of historical records soon made him a powerful steam-hammer which pulverised mercilessly the trivialities and inanities of the old antiquarians. To his influence is due in no small degree the hold which scientific archæology has been able to get on the respect of men of science, historians, and the general public. The fact that he was a most successful man of business, and at the same time took the lead in the public affairs of his county, contributed in no small degree to this result. It was felt by men of the world and men of science alike that if so powerful and practical a mind could find its chief interests in the pursuit of archæology, these studies deserved better than to be the mere pastime of pedants or enthusiasts.

Evans's great characteristics were his strong common sense, his courage, and his extraordinary mastery of details, though the last became sometimes even a weakness. Thus, whilst his books on the Stone and Bronze ages are vast storehouses of facts and minute and accurate details, he sometimes lost sight of the general principles, and did not always lay sufficient stress on the importance of associated groups of objects to which the younger generation attaches so much value. But make what deductions we may, the fact remains that Sir John Evans must always hold his place along with John Kemble, Wollaston Franks, Greenwell, and Boyd Dawkins in the front rank of those who have set British archæology on a scientific basis. Vigorous in defence of his own views, yet from his innate love of truth ready to modify them and accept those of others when his