The films are supplied ready packed and arranged in the order in which they are to be inserted into the magazine.

To understand more clearly the position of the notches, it is best to take the empty magazine in hand, and entirely withdraw the black exposing shutter. It will then be seen that the front of the magazine is provided along its sides with two series of projecting teeth; it is upon these teeth that the films inserted into the holder are supported. At one end of the magazine, which we shall call the top, is a button; if this button be pushed from one side to the other, this movement will shift all the sorting teeth at the same time, so that they will occupy positions a little to one side of their former ones.

A film introduced into the magazine will then be supported by the sorting teeth, when these stand in the original positions; if this film be put into the holder with its notched corners towards the top end of the magazine. It will, however, fall past the sorting teeth, which pass through its notches, when the change button is moved to one side and the sorting teeth stand in the second position mentioned.

The process of filling the magazine is very simple, for the pressure-board has only to be removed, and the films inserted into the holder with the white film downwards, *i.e.* towards the pointers, and eventually towards the lens. Should there remain in the magazine any unexposed films, with their backings, and it is merely required to add to their number, the additional films with their backings may be dropped into the holder by twos or threes, due care being taken that the alternate arrangement be maintained.

The whole process, although somewhat lengthy to describe, is in itself very simple and neat, and can be at once grasped by an examination of the holder itself in daylight.

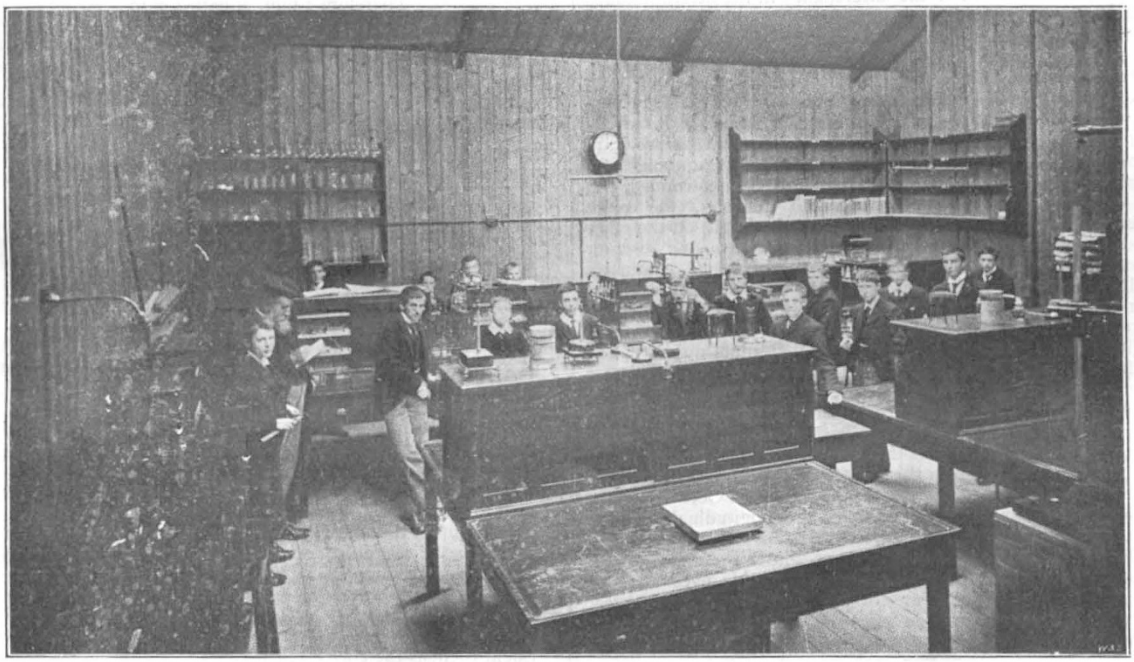
THE NEW NATURAL SCIENCE SCHOOLS AT RUGBY.

THIS new building for the physical part of natural science, which has recently been opened at Rugby School, is well worth a visit from any one engaged in teaching that subject. The building, owing to want of funds, is not at present of a permanent nature, being of the felt and matchboarding type, and in consequence has no pretensions to structural beauty; but when funds are forthcoming, no doubt the whole will be built in brick, and this will enable any alteration or improvement which may then be deemed necessary to be made. The building comprises a lecture room, a large laboratory, two small laboratories, an apparatus room, and workshop. At the back is another large room to be used for a natural history museum.

Every room is fitted with electric light and Ridge ventilation, which keeps the air pure even when filled with workers. The lecture theatre, which is capable of holding from 80 to 100 boys, is fitted with a solid slate table on brick piers, so that work can be done on it with the most delicate instruments without interference from the vibration of the floors. The fact that the rooms are all on the ground floor, gives the opportunity of putting all delicate instruments, such as balances, galvanometers, &c., on brick pillars, and thus to get rid of any vibration whatsoever.

The main laboratory contains ten tables for elementary physical measurements, two for calorimetry, two for magnetism, and two for heat experiments. Each table has a cupboard containing the necessary apparatus, and an electric lamp giving direct illumination on the tables without shadow or glare in the eyes of the worker.

Of the two smaller laboratories, one is an optical room, which can, of course, be completely darkened, and is fitted with two optical tables and a heliostat, so as to use direct sunlight as often as possible.



The other is the electricity room, containing two tables for frictional, and two for voltaic electricity, with cupboards, &c., as in the main laboratory.

Provision has been made for a small engine and dynamo for electrical work, and these will no doubt be added in time.

The whole is under the charge of Mr. L. Cumming, to whom the arrangement is due, and who is certainly to be congratulated on the result.

Every boy who takes up natural science at Rugby not only goes through a course of lectures, but has also to do experimental work himself in the laboratory. This enables him to grasp the subject much more thoroughly, and to remember it much better than if he attended the lectures only. That this method has had excellent results, will be seen by the number of successes in natural science that Rugby has gained of late years in scholarship and other examinations.

EVIDENCE OF A TWILIGHT ARC UPON THE PLANET MARS.

DURING last summer and autumn Mr. Douglass made at this observatory 341 micrometric measures of the diameters of Mars. In addition to their general value as micrometric measurements, these turn out to be of a peculiarly interesting character. For on reducing them I find that beside furnishing, from their great number, relatively accurate values of the equatorial and polar diameters and of the polar flattening, they yield a by-product as unexpected as it is important. Their discussion reveals, in short, what appears to be unmistakable evidence of a twilight upon the planet, sufficiently pronounced to be visible from the earth, and actually to have been measured unconsciously by Mr. Douglass. That Mars possessed an atmosphere, we had what amounted to proof positive before; but that the fact should again be brought to light in this literal manner, as a silver lining to a cloud of figures, is a point of some curiosity. The measures had no such end in view; indeed, to detect the presence of an atmosphere by measures of the diameters had not suggested itself to any of the most adventurous of observers. Yet, as will be seen, the quantities upon which the evidence rests are so large as to be quite without the pale of accidental error, being ten times as great as the probable errors of observation, and twice as large as those that disclose the polar flattening. That they have hitherto escaped detection is due to their having been masked by another factor affecting the size of the polar diameter, as will appear in the course of this paper. To the unsuspected presence of these two causes, at times nearly offsetting each other, so far as relative values go, is attributable in all probability much of the discrepancy in the determinations of the polar flattening hitherto made.

The first measures were made on July 6, and the last on November 21, 1894. From October 12 they were taken nearly every night. Those here given were all made by Mr. Douglass. Later in the paper I shall introduce others by Prof. W. H. Pickering, which confirm the result. But here at the outset it may be well to point out that whether the results of many observers are to be preferred to those of one is, omitting discourteous personalities, a question entirely of what is to be determined. If the determination be one of absolute quantity, the more observers the better, provided they be good; but if, on the other hand, the determination be of relative magnitudes, one observer is better than many, as his personal equation obligingly eliminates itself, whereas two such equations can by no possibility, short of chance, eliminate each other. Now, in the present case, while the determination of the planet's size, and even to some extent of its polar flattening, are matters of absolute quantity, the evidence of a twilight upon it is one which rests upon relative results. The former, therefore, are subject to any systematic errors there may be; the latter, essentially free of them. In consequence, the by-product in this case is actually more trustworthy than the main results themselves.

Much care was taken in the matter of the Martian measures. In the ones I shall first discuss, those made from October 12 and November 21, Mr. Douglass adjusted the longitudinal thread of the micrometer, parallel or perpendicular, as the case might be, to the planet's polar axis, according to Marth's ephemeris, and then placed himself, so that the line joining his eyes was kept parallel to this thread or to the fixed transverse thread at right angles to it, during any one set of observations, the position being then recorded. As measures were taken in both positions for each diameter at various times, we have here a comparison of some eventual value. In eye-estimates such orientation in the position of the observer is absolutely essential in order to correct his possible astigmatism. Into measures, however, astigmatism enters only to cancel out. For if we consider the matter, it is at once evident that the whole field is distorted in the same proportion, the space between one turn of the micrometer and the next being reduced or expanded in the same ratio as the part of the image measured. The astigmatism thus eliminates itself.

From October 12 to November 21, Mr. Douglass made in all 275 measures; 140 of the equatorial, and 135 of the polar diameter. In the reduction of the measures, account has been taken of the place upon the micrometer screw at which the measures were made, and its appropriate value introduced. For by the forethought of Mr. Douglass in suspecting the possibility of variation, we measured the value of a micrometer turn at different points of the scale to confirm his conjecture.

Preliminary to the discussion of the results, it will be well to explain the corrections determined and applied. The first correction is that arising from refraction. This is the correction due to the differential effect of refraction upon the planet's opposite limbs at the extremities of the particular diameter measured. It depends both upon the altitude of the planet at the time of observation, and upon the inclination at that moment, of the particular diameter to the vertical. In many cases it was so small as not to make itself perceptible in the column.

The correction for aberration, similarly a differential effect, was so utterly insignificant throughout as not to appear at all.

The next correction is that due to irradiation. Toward its determination two different tests were made, in each case upon both Prof. W. H. Pickering and myself; in the one the effect should have been less than in the case of Mars, in the other greater. As in both cases the observers substantially agreed, the results may be accepted as having some impersonal value.

The first test was made upon a railroad switch-head, a white circular disc with a smaller black circle painted upon it. The size of these circles was unknown to the observers.

Their estimates were:

(W. H. P.) ... (white rim) ... 1; (diameter black circle) ... 1·3
(P. L.) ... ... ,, ,, ... 1; ,, ,, ,, 1″·265

The discs and their distance were then measured and gave:

For diameter black circle	202 mm.
For radius white rim	126 mm.
For ratio	$1\frac{1}{16}$
For distance from eye	57 yds.

Therefore 1 mm. equalled 3″·9.

For the amount of the irradiation in seconds of arc, x, assume the amount of the irradiation of the white rim against the general background of earth of a brown colour to have been two-thirds that of the rim against the black circle. We have then, for the first observer, the following equation to determine x.

$$\frac{252 \text{ mm. } 10/3\, x}{212 \text{ mm. } 6/3\, x} = \frac{2{\cdot}0}{1{\cdot}3}; \text{ from which } x = 9{\cdot}2 \text{ mm. or } 36'';$$

for the second observer: $x =$ 40″.

The second test was on the moon (November 22), when the old moon was seen in the new moon's arms. In this case the irradiation proved for both observers to be one-seventh of the radius of the old moon, or about 157″.

In the case of Mars, the value for the irradiation probably lies between these two limits. For the contrast between the Martian limb and the sky is pretty certainly greater than that of the white rim and the black circle of the switch-head, and less than that of the moon's bright limb and the sky, to which the contrast between the limbs of the old and of the new moon closely approximates.

It is to be noted that with a given illumination and a given eye, the irradiation correction is a personal constant, not depending upon the size of the disc measured and diminishing inversely as the magnification. In all the measures subsequent to and including October 15, the power used was 860; in those of October 12, it was 617. The correction, therefore, for all except those of October 12 was 0″·10; for those of October 12, 0″·14.

Such, then, is the correction for irradiation upon the planet's limb. The double of it, therefore, would need to be subtracted from the measures of a disc similarly placed to that of Mars when fully illuminated. But the disc of Mars was not fully illuminated even at the moment of opposition, and grew less so as time went on. Now it will be evident on consideration that the irradiation from the terminator must be very different from that upon the limb, inasmuch as the light fades away to nothing at the one, while it has its full value at the other.

To determine the amount of the correction needed at the terminator it is to be observed that if

$\gamma =$ the areocentric angle between the sun and the earth;
$\alpha =$ the angle between the terminator and the point of the illuminated surface of which the irradiation is sought; and
$m =$ the ratio of the irradiation at the limb to the radius of the disc, we have for the extent of the irradiation at the terminator

$$m\left(\frac{\sin\alpha}{\sin(\gamma+\alpha)}\right)^{\frac{1}{n}} - \left(\cos\gamma - \cos(\gamma+\alpha)\right)$$

where n denotes the ratio of the irradiation to the illumination, and is equal to about 2^5; that is, it takes 2^6 times the illumination to produce twice the irradiation effect. This value is got from inter-comparison of the above tests as limiting values, the resulting value for Mars and the known decrease in illumination due to the telescopic magnification employed.

To deduce the resulting irradiation we must find the value of which renders the above equation a maximum, and then substitute this value in the equation. To do so directly leads to an equation of so high an order that approximation will be found the better, if indeed it be not the only, method of solution. By this means it appears that the necessary correction does not become insensible, to three places of decimals, till the phase angle, γ, somewhat exceeds 30°.

The formula must be used within the limits for which $\frac{\sin \alpha}{\sin (\gamma+\alpha)} = 1$; beyond them $\frac{\sin \alpha}{\sin (\gamma+\alpha)}$ must be taken as unity.

If the reflection from the disc followed the law of the cosines—that is, if the apparent illumination were always equal to the true one—we should have

$$m\left(\sin \alpha\right)^{\frac{1}{n}} - \cos \gamma - \cos\Big((\alpha + \gamma)\Big)$$

where α, γ, and n have their previous values, and m = a constant to be determined from the equation, from the value at the limb.

But although this is the formula for the case of a theoretical rough bare globe, it manifestly does not hold in the case of Mars, of which the limbs are not only as bright as the centre of the disc, but much brighter. The previous formula is, therefore, to be preferred to it, although even that formula makes the irradiation correction at the terminator too great as compared with that at the limb.

But it is to be specially noticed that no law of correction for irradiation at the terminator, however big it make that correction to be, is able to do away with the outstanding differences, presently to be noted, of the equatorial diameter at different times upon which the evidence of the twilight arc is based.

There is also the correction for phase. Inasmuch as the phase axis and the polar axis did not in general coincide, there entered into its determination beside the amount of the lacking lune, the angle of inclination of the two axes. So that the amount of the defalcation had to be calculated in accordance for each night. These corrections and their results reduced to distance unity have been calculated and tabulated.

Besides the above there is a fifth correction needed to reduce the diameter measured for the polar one, to the true polar diameter. The diameter measured perpendicular to this, or the apparent equatorial diameter, although not in fact an equatorial diameter, was always exactly equivalent to one, since its extremities were always each 90° distant from the pole. The other, however, was the diameter of the ellipse made by the plane passing through the polar axis, which was inclined to the polar axis by the angle of tilt, and needed, therefore, to be reduced to that ellipse's minor axis. This correction is best applied to the means, and appears in the subjoined table.

Polar Diameters.

	Cor.. measures.	Cor. for inclination.	Further cor. for twilight band.
Oct. 15 to 23 inc.	9″·385	9″·379	9″·356
,, 15 to 1 of 24 ,,	9″·377	9″·371	9″·348
,, 15 to 24 ,,	9″·368	9″·362	9″·339
,, 15 to 29 ,,	9″·375	9″·369	9″·346
,, 12 to 30 ,,	9″·384	9″·378	9″·354
Nov. 2 to 21 ,,	9″·397	9″·390	9″·353

Equatorial Diameters.

Oct. 15 to 23 inc.	9″·420	—	9″·404
,, 15 to 1 of 24 ,,	9″·428	—	9″·402
,, 15 to 24 ,,	9″·424	—	9″·395
,, 12 to 30 ,,	9″·440	—	9″·396
Nov. 2 to 21 ,,	9″·545	—	9″·402

Twilight arc ... ... 10°
Polar flattening ... 1/191 of the equatorial diameter.

As previously explained, no correction is needed for astigmatism, as the measures themselves correct it.

So soon as the measures had been corrected and reduced to distance unity, two things became apparent, both so large as to be almost unmistakable before taking the means. The first was the polar flattening; the other an equally systematic difference in the size of the equatorial diameter according as the measures were made in October or in November. The November measures came out much larger than the October ones; while the corresponding polar measures, on the other hand, showed no corresponding increase. Struck by this fact, and suspecting its cause, instead of taking the mean of all the measures for each diameter, I divided them into sets according to their proximity in date to the time of opposition, and took the mean of these sets.

The means are as follows:—

Polar Diameter.

Mean October 15 to October 23, both dates inc.	9″·379
,, ,, 12 ,, 30, ,, ,,	9″·378
,, Nov. 2 to Nov. 21, ,, ,,	9″·390

Equatorial Diameter.

Mean October 15 to October 23, both dates inc.	9″·420
,, ,, 12 ,, 30 ,, ,,	9″·440
,, Nov. 2 to Nov. 21, ,, ,,	9″·545

Opposition occurred on October 20. The first set in each schedule, therefore, was made within four days of opposition; the second, within eleven days of it; the last, from fourteen to thirty-two days after it. That there is a systematic increase in the equatorial measures is apparent. That it is not paralleled by a corresponding increase in the polar ones shows instantly that it can hardly have been due to systematic error in the observer, since in that case both sets of measures should, in all probability, have been affected.

Now as all the measures had previously been corrected for refraction, irradiation, phase and tilt, the means of each diameter should have agreed with themselves. The polar did so in a very satisfactory manner; the equatorial not only did not, but they differed in proportion to their distance in time from the date of opposition. Now the only factor that increased in proportion to the distance in time from opposition was the phase. The direct effect in the way of decreasing the equatorial diameter had already, as we have seen, been allowed for; what is more, it is a correction susceptible of great accuracy, since it depends upon the motions and relative distances of the earth and Mars, quantities very accurately known. Besides these quantities, there is nothing which enters into the calculation but the position of the pole of Mars, and this would have to be, not only some 35 Martian degrees in error to explain the discrepancy, but would have had to have shifted obligingly to an opposite error during July and August to account for the measures taken then, as we shall see later. In other words, no such discrepancy exists.

In the case of a bare globe this direct effect would be the only effect phase could have upon the equatorial diameter; not so, however, in the case of a body not bare. If a planet possessed an atmosphere, that atmosphere would cause the phenomenon of twilight, and to an observer at a distance the effect of the twilight would be to prolong the terminator beyond what would otherwise be its limits. There would thus result a seeming increase in the equatorial diameter as the disc passed from the full to the gibbous phase. Now this increase is precisely the increase that the measures disclose.

It is furthermore worth noting that in the absence of an atmosphere, the measures of the equatorial diameter as the phase increased would not only have shown no increase, but would actually have shown a decrease, inasmuch as it would be impossible for an observer to see quite out to the edge under the diminishing illumination.

To determine the extent of the twilight thus disclosed by the measures, the angle between the radius to the sunset point and the radius prolonged to the point of the atmosphere last illuminated, had to be found. This enabled an equation to be developed, which gave for the visible twilight fringe an arc of 5°, the double of which, or 10°, is the angle which determines the duration of the twilight, or the twilight arc. On the earth this angle is 18°.

Applying the correction due to the twilight fringe, to the means previously obtained, we find the following close agreement between them:—

Polar Diameter.

October 15 to 22 inc.	9″·356
October 12 to 30 ,, ...	9″·354
November 2 to 21 ,, ...	9″·353

Equatorial Diameter.

October 15 to 23 inc.	9″·404
October 12 to 30 ,, ...	9″·396
November 2 to 21 ,, ...	9″·402

The value for the twilight band, deduced from these observations, does not measure the full breadth of that band. It gives rather a minimal value for it. For although Mars shows us a disc which is always more than half full, in which aspect an illuminated fringe of atmosphere would be more perceptible to an observer placed without than to one placed within it, provided both were at the same distance off, in the case before us the outsider is a great deal farther off. In consequence, what would be quite recognisable to one standing upon the planet's surface would be too faint to be seen by him at a distance of forty millions of miles away. The detection, therefore, of any twilight on Mars hints that the extent of that twilight is greater than appears; how much greater, we cannot at present say. A second possible cause affecting the extent of the twilight is the constitution of the Martian atmosphere. That atmosphere is practically cloudless; if, also, it be clearer than our own, the twilight would be relatively less for equal amounts of atmosphere, for the amount of twilight is, among other things, a question of the clearness of the air. In a perfectly transparent atmosphere there would be much less twilight than in one charged with solid or liquid particles.

It is to be noted that the evidence of a twilight is independent of any possible change in the value of the corrections. The only corrections that admit of uncertainty are those for the irradiation; and on examining them it will be seen that by no possible alteration can they be made equal to account for the observed increase in the equatorial diameter. Whatever alteration in them be assumed only affects somewhat the extent of the increase; it never does away with it. In other words, whatever these corrections, the fact of a twilight remains.

For the determination of the polar flattening, the measures of October 15 to 23 promise the best result, as the measures of the polar diameter on the 24th were so small, compared with those of the equatorial diameter, as to suggest error. Comparing, therefore, the polar and equatorial means of October 15 to 23, we get for the polar flattening 1/196. This, however, is probably too small; for though the polar cap was nearly non-existent during these observations, there were, on occasions, signs of its temporary reappearance, and an almost continuous brightness of the limb where it had previously existed. This by irradiation would increase the apparent polar diameter, and so decrease the resulting value for the polar flattening. If we compare each polar determination with its corresponding equatorial one, deduce the resulting polar flattening, and then take the mean of them all, we have for the polar flattening the value 1/191.

This is probably not far from the truth, although also probably a little too small, as the polar diameter was unquestionably still slightly increased beyond its real extent, by irradiation from the remains or consequences (vapour in the air, &c.) of the polar cap.

This value, 1/191, is also happily accordant with what theory would lead us to expect. Tisserand has found that with the known rotation of Mars and supposing homogeneity, the planet's flattening should be 1/175 of the equatorial diameter, while if the strata varied in density, after the manner of those of the earth, the polar flattening should be 1/227 of it. Now, assuming Mars to have been developed in general accordance with the nebular hypothesis, his strata would be neither homogeneous, on the one hand, nor, on the other, would they vary in density from the surface to the centre so markedly as is the case with those of the earth. For Mars being a smaller body, the pressure due to gravity would be less, somewhere between that of the earth and that of homogeneity, which is nothing, and the polar flattening should be somewhere between 1/227 and 1/175 of the equatorial diameter. 1/191 is, therefore, not far from the value probable *a priori*. It is interesting to have this result agree thus closely with theory, as it furnishes so much more reason for believing in the general evolution of our solar system.

Any value much less than 1/191 would require that Mars should have had at some time a much swifter axial rotation than he has now, which there is not only no ground for thinking, but much reason for thinking could not have been the case. For Mars lacks the tools for tidal friction, possessing insufficient satellites on the one hand and insufficient oceans on the other, so that even solar tides would be out of the question. Even had he possessed both requisites, it is more than doubtful if their slow action would have materially affected his form. For on the earth, which did possess them, we see that they were practically impotent to alter her shape. Any great change in Mars' period of rotation since he cooled must be looked upon, therefore, as unlikely.

For the final values of the diameters we have, allowing for a slight irradiation from the remains of the polar cap:—

True equatorial diameter	9″·40	″·007
True polar diameter	9″·35	″·007

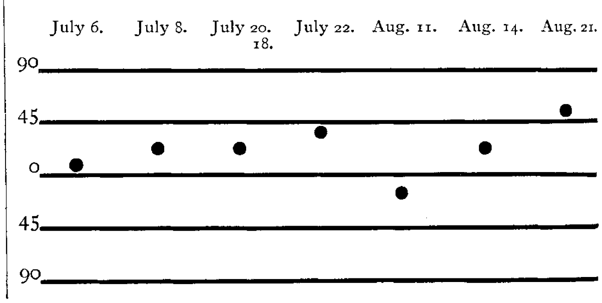
It will be noticed how near these values are to that found by Hartwig from his general discussion several years ago.

We will now consider the September observations and the first of the October ones, those taken upon the fifth of the month. The first thing we notice about them is the abnormal size of the polar measures, so large as to suggest error. On examination, however, we find that instead of mistake they give us our first recognition of the cause that has so long masked the effect of the twilight fringe. The equatorial measures, it will be seen, come out in fairly good accordance with the October and November determinations, being greater than those taken near opposition, although somewhat smaller than the November ones, the discrepancies falling probably within the errors of observation. The polar measure of October 5 is also much what we should expect, but the polar measures of September 20 and 23 are apparently unaccountably larger. If we consider, however, the dates at which they were taken, we shall at once perceive a cause capable of producing the apparent increase. For in September and early in October the polar cap was still in existence. Now the south polar cap is eccentric to the pole, being situated some 5° from it, and from Mr. Douglass's micrometric measures of its position in October, in longitude 59°. As during the observations the south pole was tipped towards the observer, the polar cap was carried, in consequence of the planet's rotation, now in upon the disc, now out upon the limb. Now, if it chanced to be upon the limb at the hour at which the measures were made, its excessive irradiation would produce just such apparent increase in the polar diameter as was observed. On calculating its position for the hours of observation on September 20 and 23, it appears that at those times it was in fact upon the side of the pole toward the limb. Here, then, we have the *deus ex machina* in the matter. To clinch the conclusion, we find on calculating its position for the observation on October 5, when it suddenly measured small again, that at that hour the polar cap was upon the hither side of the pole. Such was also the case on October 12. The discrepancy thus stands accounted for. On October 13, very obligingly, the polar cap practically vanished just in time not to interfere with the most valuable measures at and near opposition.

That such is the explanation of the change in the polar diameter, comes out still more markedly from the July and August measures. Turning to those measures we find that the position of the polar cap is an all-important factor in them. Indeed, it is possible to follow its change of place upon the disc from its effect as reflected in the measures. This will appear at a glance from the accompanying diagram of the July and August measures of Mr. Douglass. A similar sequence of position and effect is apparent in Prof. Pickering's measures made at the same time.

COMPARISON OF POSITION OF POLAR CAP AND MEASURE OF POLAR DIAMETER.

The distance of the point from the medial line shows the angular position of the polar cap from the pole at the times of observation; 90° denoting its lower, and 90° its upper meridian transit. At its lower culmination it was at its nearest to the centre of the disc; at its upper, nearest the limb. The measures show the corresponding effect in irradiation.



			Polar.			
9″·85	10″·29	9″·57	9″.46	9″·41	9″·40	9″·34
			Equatorial.			
9″·67	10″·08	9″·48	9″·33	10″·03	9″·75	9″·41
In relative values						
			Polar.			
1019	1021	1009	1014	938	965	993
			Equatorial.			
1000	1000	1000	1000	1000	1000	1000

At first sight it would seem that the later August measures do not support the rule. Closer consideration will, however, show that they do. For while in July the polar cap was still large, and in consequence reached to the limb, even when its centre was at some distance from it, by August it had dwindled to so small a patch as to be incapable of doing so when at the same angular distance away. Taking account of this fact, it will be seen that the effect is quite in accordance with the position, as comes out clearly in the relative values for the two diameters of August 14 and August 21.

It will now be evident why so large, and intrinsically so unmistakable, an effect as that of the Martian twilight should hitherto have escaped detection; the reason being that the twilight effect and the irradiation from the polar cap each increased their respective diameters to a simultaneous augmentation of both, conspiring each thus to mask the other.

Had measures been continued through a series of months, and been made in sufficient number, both causes must have made themselves evident. For both are periodic, and their periods are not the same. The irradiation from the polar cap has a primary period of thirty-seven days, a secondary one of a Martian year as well as a third depending on the tilt of the pole toward the earth; that of the twilight fringe a varying one of about thirteen months. But as previous measures have been made quite regardless of the twilight effect, and largely regardless of the polar cap, regardless, that is, of its varying position, the results have merely disagreed with each other, and the disagreements been credited to errors of observation. One result of this was discordance in the value of the polar flattening.

When we take both causes into account we find that the means of the July and August observations confirm the October and November ones.

For by comparing the values of the polar diameter when on and away from the limb, it is possible to deduce both the amount of the irradiation from the polar cap and the value of the twilight band from the measures themselves. The results in the case of Mr. Douglass agree with those of his October-November measures. In the case of Prof. Pickering, there is the same relative difference between the determinations, although the absolute values are all smaller.

That in the table the corrections to the July and August measures differ from those applied to the later ones, comes from the different manner of their taking; in the July and August measures the longitudinal thread of the micrometer having been set to the phase axis or perpendicular to it, instead of to the polar one.

In Mr. Douglass' determinations the value for the twilight arc comes out 8°. This is somewhat smaller than the result from the November measures. But a smaller value is precisely what should have been found. For the greater the phase angle, the less the foreshortening, which foreshortening by massing the illumination lets the fringe of light become evident farther out. Now the average phase angle was 43° in July and August, as against 18½° in November.

From Prof. Pickering's measures the twilight arc comes out greater, or 11°, and by inference would have come out greater still in November.

Thus it appears that measures made by separate observers, and measures made before and after opposition, all confirm each other to the existence of a twilight band upon the planet.

PERCIVAL LOWELL.

THE FOUNDATIONS OF ENGINEERING EDUCATION.[1]

LET us consider what is the education which a young man needs to fit him for the profession of engineering, whatever be the special line of engineering which he proposes to follow.

[1] Extracted from a course of lectures delivered in the Lowell Institute, Boston, by Prof. G. Lanza, Professor of Theoretical and Applied Mechanics, Massachusetts Institute of Technology, and published in the *Journal* of the Franklin Institute.

And, before discussing the details of what he ought to study, let us consider what it is that we desire to accomplish by giving him an engineering education. Naturally, we wish, as far as any education can accomplish it, to put him in the best condition to meet and grapple with the duties, the problems, and the responsibilities of his profession, as they arise.

There are two things which are absolutely necessary to make a successful engineer: first, a knowledge of scientific principles and of the experience of the past; and second, his own experience. The last cannot be given in a school, and each one must gain it for himself in his practice.

But the greater his familiarity with scientific principles and the experience of the past, the more able will he be to advance in his profession, and to be trusted to assume responsibility; indeed, if a man is ignorant of certain details and knows he is ignorant, he can—and if he is the right kind of a man, he will—take pains to learn them, if they bear on the work he has in hand; but if he is ignorant of scientific principles, it is very likely that he does not know he is ignorant, or, if by good luck he becomes aware of the fact, it is next to impossible for him to devote the time and study necessary to correct his ignorance while his mind is busy with his daily work.

Moreover, a man who is not familiar with the scientific principles which concern his work is not a safe man to trust with responsibility; for scientific principles are merely the laws of nature, as far as known, as shown by the experience of the past.

Hence it is that the first and most important thing to be done for the student is to give him a thorough drill in the scientific principles which find their application in his profession. It is in the school that this knowledge may best be acquired, since it is only with great difficulty that principles can be mastered after the student begins practice, and then as a rule but very imperfectly; and this view is borne out by those engineers who have been successful, and who have had to acquire their knowledge of scientific principles little by little, and as best they could, during the practice of their profession. Too much cannot be said by way of insisting that a thorough mastery of such scientific principles far outweighs in importance anything else that can be done for the student; and this is so true, that it is a decided mistake to neglect it in order to impart to him greater skill in such processes as will probably engage his attention the first year after he goes to work, as, for instance, to make him a skilful surveyor, a finished machinist, or an elegant draughtsman. Greater skill can far more easily be acquired after he goes to work than can scientific principles, and if this mistake is made the consequences will probably pursue him throughout his professional life.

The two fundamental sciences upon which the scientific principles of engineering are especially dependent are mathematics and physics, and no proper course in engineering can be arranged without insisting upon these fundamentals.

Let us begin with the subject of pure mathematics, and consider what portions should be studied, how they should be studied, or rather how they should be known, and of what service they are to the engineer after they have been mastered; bearing in mind that, in accordance with the opinions already expressed, the course of study should be laid out with direct reference to the needs of the engineer; and that when it is so laid out, it will, by the very fact that it leads to a definite end, subserve best the purpose of true education, and hence of developing the powers of the mind. Probably the best definition of mathematics is that given by Prof. Benjamin Pierce, who defined it as "the science of drawing necessary conclusions." This definition, of course, includes formal logic, and hence embraces more than is ordinarily understood by mathematics. We may assert, however, that the only function of mathematics is to draw necessary conclusions from the assumed data. Mathematics has nothing whatever to do with the correctness or incorrectness of the data. If these are correct, the conclusions deduced by mathematics will also be correct; whereas, if the data are false, the conclusions deduced by mathematics will be false.

Thus, if we require the sum o. a certain set of numbers, the process of addition will give the correct result, provided the numbers added are the right ones; but if the numbers added are not the right ones, the result of the addition will not be the one desired. Indeed, we might compare pure mathematics to a mill—it will only produce good meal when the corn furnished to it to grind is of good quality; and if the corn is poor, the meal pro-