THURSDAY, JANUARY 11, 1883

GEIKIE'S GEOLOGY

Geological Sketches at Home and Abroad. By Archibald Geikie, LL.D., F.R.S. With Illustrations. (London and New York: Macmillan and Co., 1882.)

Text-Book of Geology. By Archibald Geikie, LL.D., F.R.S. With Illustrations. (London: Macmillan and Co., 1882.)

THESE two works, by the same author, are presented to the public at nearly the same time, but there is no other reason why they should be described together. The first is a collection of short papers, each presenting some matter of personal observation or some contribution to geological philosophy. The second exhibits the science of geology in a systematic way, and of necessity deals chiefly with the results of the work of others. The first is addressed to the general reader, and in part to the geologist; the second is addressed specifically to the student.

The sketches of the first volume are not new, but are here collected for the first time. Several of them received their first publication as magazine articles, others have been presented to scientific societies, and a few have taken the form of lectures. They constitute but a small portion of the author's voluminous contributions to scientific literature, and have evidently been selected because of their popular interest. A few are addressed to the popular audience only, and merely present some of the elements of stratigraphical and dynamical geology, with familiar Scottish scenes as texts; but the majority embody original contributions to knowledge, couched in so simple language that the layman reads them without being fully aware that they belong to the frontier of geological thought. Prof. Geikie possesses the happy faculty of addressing himself simultaneously to a professional and an unprofessional audience in such way that the former do not find his science to o dilute nor the latter too condensed.

One of the sketches describes a journey to central France, undertaken for the purpose of studying the extinct volcanoes of that region as an aid to the imagination in restoring the condition of Scotland during the Carboniferous period; and another describes a journey to Norway with the parallel purpose of rendering vivid the mental restoration of Scotland in Glacial times. These two are perhaps the most instructive of the collection, for besides making definite additions to the geological history of Scotland, they present admirable illustrations of one of the most valuable methods of scientific investigation. The principles which distinguish modern scientific research are not easily communicated by precept, and it is by no means certain that they have yet been correctly formulated. However it may be in the future it is certain that in the past they have been imparted, and for the present they must be imparted, from master to pupil chiefly by example; and whoever in publishing the result of a scientific inquiry sets forth at the same time the process by which it was attained, contributes doubly to the cause of science.

Two chapters are devoted to a journey in the United States; a journey undertaken, like the others, for a definite purpose—that of enabling the author to see with his own eyes the monuments of erosion for which the Rocky Mountain region is so illustrious. His account deals also with a variety of geological topics, as well as with the peculiar aspects of American frontier life. He describes the geysers of the Yellowstone country, some of the extinct glaciers of the head-waters of the Missouri, the parallel shore-lines of the great extinct lake of Utah, and the great lava field of the Snake River Plain. In another chapter he appears as the apostle of massive eruptions, first recognised by Richthofen, and afterwards by many American geologists, but so foreign to European experience, that the accounts of them had seemed to many English geologists to border on the marvellous, and had even thrown discredit upon American science.

Perhaps the most important paper of all is that upon geographical evolution. It was originally read to the Royal Geographical Society, and has received in various ways so wide a publication, that it is probably accessible already to the majority of the readers of NATURE. The lecture on the weathering of rocks, as illustrated by tombstones, is also included, and a lecture on the geological influences which have affected the course of British history.

In the whole collection there is nothing polemic, nor anything that could even be called controversial. Attention is never directed to an error, except as the merest incident to pointing out that which is true. No words are given to the censure of others, but many to their praise, and one of the chapters has for its theme a eulogy on the work of the early Scottish school of geology.

The style is peculiarly genial and entertaining—a merit unfortunately rare in the writings of modern geologists; but accuracy of statement is not sacrificed to vivacity. As in all his writings, there is nothing sensational, either in description or in speculation. His inductions are not expanded into brilliant, universal theories, but are modestly advanced with all those limitations which impress themselves on the mind of one who constantly questions nature.

Turning now to the text-book, we come to consider a work of greater importance, and one especially deserving of careful criticism by reason of its relation to education. The text-books of this generation must furnish to the geologists of the next their fundamental principles, so that those who prepare them and those who commend them are responsible, not merely to the youth of to-day, but to the science of the future.

There are four features in regard to which a work designed for geological instruction should be scrutinised: Its scope, the arrangement of its matter, the quality of its matter, the manner of presentation.

In the scope of geological text-books, on the range o subjects considered and the relative space allotted to each, there has been a progressive development, parallel with and dependent upon the evolution of geology and cognate sciences. Our knowledge of the earth's history is so dependent upon and interwoven with other departments of knowledge, that a clear presentation of it cannot be made without either reciting the elements of other sciences or assuming them to be known. In the early history of the subject, when the volume of geological material was small, and when the elements of zoology, botany, and chemistry were not so widely diffused as now, it appeared to most writers necessary to devote some space, either in a prefatory or in an incidental way, to these sciences. Mineralogy and palæontology, growing with the growth of geology, were likewise treated with it. But owing to the rapid development of geology, its own subject matter has now become so voluminous that it can only with difficulty be outlined in the compass of a text-book, and step by step it has displaced everything of which a sufficient knowledge could be assumed.

While mineralogy and palæontology have by their growth become more and more differentiated from geology, astronomy has been affiliated in a degree that was not anticipated. Previous to the revelations of the spectroscope, our earth was regarded indeed in origin, composition, and career as analogous to other planets, but only in a hypothetic and speculative way; but now that there is a large body of evidence pointing to identity of composition throughout the solar system, there is no longer any question of a common history, and every advance in celestial physics is now regarded as a contribution to the early history of the earth. A department of astronomical geology has thus arisen.

In the work under consideration no space whatever is permitted to zoology and botany; chemistry is barely mentioned; mineralogy (chiefly descriptive) is accorded only 25 pages; palæontology proper is omitted, but 28 pages are devoted to the principles of palæontological geology—a department of science clearly distinguishable if not distinct from palæontology, and inseparable from stratigraphy; mythological cosmogony is not even mentioned, but the space it has too often occupied is given to physiographical geology—a discussion of the origin of the physical features of the land. Astronomical geology is accorded 23 pages. The bulk of the volume—570 pages out of 910—is devoted to geognosy, and dynamical and structural geology that is, to rocks and rock structures, and to the physical changes whereby rocks originate. Stratigraphy, which until very recently has arrogated the lion's share of space, is here reduced to less than one-third of the total.

The distribution of space thus outlined is eminently judicious, and it may be doubted whether any could be better adapted to the present status of the science and the present demands of instruction. If it has a fault it is in the amount it concedes to the demands of the geologist in the matter of stratigraphy. The student's text-book has not yet been clearly differentiated from the geologist's handbook, and there is certainly an open field to-day for a manual specially adapted to the use of the working geologist, and not primarily arranged for instruction. All of the larger text-books have been partially adjusted to this need, and Prof. Geikie's is not an exception; but in his work the adjustment appears only in the stratigraphical chapter, which embodies a mass of detail that can serve only to bewilder if the student undertakes to master it. If the 275 pages of descriptive stratigraphy were reduced to 50, and a portion of the space thus saved were devoted to a rapid review of the salient points of the geological history of some limited region, as Great Britain, for example, I am prone to believe that the student would be afforded a better insight into the aims and results of geological inquiry.

In the classification and arrangement of the subject-matter of geological text-books, there has been as marked a development as in the scope. The number of different manners in which a congeries of allied topics can be grouped is practically limitless, for the bases of possible groupings are as numerous as the relations sustained by the topics; but not all classifications are of equal utility, and at each stage in the progress of a science there is usually some one which commends itself as of superior advantage. As, in the progress of knowledge, new relations are discovered, and the importance of relations previously known comes to be differently estimated, new classifications are adopted, in comparison with which the old appear crude. Geology is so young a science, that a single generation has witnessed a complete revolution in this regard. The primary classifications of the modern text-books have nothing in common with the earlier editions of Lyell's manual. In the division and arrangement adopted by Geikie, only a single feature is original, but the order of presentation as a whole is new.

The theme of geology is the history of the earth. In its study there are two lines of inquiry, which are so nearly independent that they form co-ordinate branches of the general theme: the one is cosmic, the other terrestrial.

Cosmically considered, the earth is one of a group of worlds believed to have a common origin, and to be pursuing parallel courses of development, in which they have reached various different stages. Assuming this to be true, the less developed worlds present phases, through which the earth has already passed, and by studying them we may learn something of the youth of our planet.

The terrestrial branch of inquiry is concerned chiefly with rocks. The changes of the crust have led to the formation of rocks, and have given to them great variety of composition and structure. It is known, moreover, that rock formation is still in progress, and that agencies whose operations can be witnessed are now forming many varieties of rocks, and are initiating many peculiarities of rock structure. It is possible, therefore, to associate certain rocks and rock structures with certain processes of change, and by this means to derive from a study of the rocks of the crust a history of the changes which led to their formation. This inquiry is greatly facilitated by the fact that rocks have been partly formed from animal and vegetable remains, and by the additional fact that there has been a progressive development of life; so that, the key once obtained, the chronological order of rocks can be deduced from their organic contents.

In presenting the second line of inquiry as to the earth's history it is therefore proper to treat: of the composition of rocks and other materials of the earth's crust (geognosy); of the forms in which rocks are aggregated, or the structure of rock masses (structural geology); of the agencies which in modern times are observed to produce changes of the earth's crust (dynamical geology); of the relation of organic remains to geological formations (palæontological geology); and finally, of the actual order in which the various kinds and groups of rocks succeed each other, and the deduced series of changes the earth has undergone (stratigraphical or historical geology). The first and last of these categories claim their respective positions without question: geognosy constitutes the alphabet of the subject, and must precede all else, while stratigraphical geology depends upon all the other divisions, and must follow them. Palæontological geology is in some sense co-ordinate with dynamical and structural geology taken together, but finds place after them because its use cannot be explained before their principles are known. Whether dynamical geology should precede or follow structural, is a question admitting of discussion. They are to a large extent correlatives, and either is more intelligible if preceded by the other. To give precedence to structural geology is to describe phenomena in advance of their explanation. If dynamical geology precedes, a variety of natural agents are described which have no apparent connection with the general subject. The majority of writers have selected the former alternative; but a few have preferred the latter, and among them our author. All things considered, he appears to have chosen the lesser evil.

The single new departure of the volume consists in the elevation of physiographical geology to the rank of a major division. The same title it is true has been placed by Dana at the head of a primary division of the subject, but it was used by him in a different sense. With Dana it is a synonym for physical geography; with Geikie it is that "branch of geological inquiry which deals with the evolution of the existing contours of the dry land." So far as the subject has had place in earlier treatises it has been regarded as a subdivision of dynamical geology, and the classification which placed it there was certainly logical. In dynamical geology, as formulated by Geikie, the changes which have their origin beneath the surface of the earth (volcanic action, upheaval, and metamorphism), and the changes which belong exclusively to the surface (denudation and deposition) are separately treated. In physiographical geology the conjoint action of these factors of change is considered with reference to its topographical results. Starting from geological agencies as data we may proceed in one direction to the development of geological history, or in another direction to the explanation of terrestrial scenery and topography, and if the development of the earth's history is the peculiar theme of geology, it follows that the explanation of topography, or physiographical geology, is of the nature of an incidental result—a sort of corollary to dynamical geology. The systematic rank assigned to it by Geikie is an explicit recognition of what has long been implicitly admitted: that geology is concerned quite as really with the explanation of the existing features of the earth as with its past history. The separation initiated by our author is an indication of the growing importance of the subject, and it is safe to predict that in the future it will not merely retain its new position, but will even demand a larger share of space.

The following scheme exhibits the general plan of the volume:—

Book 1.—Cosmical aspects of geology.

Book 2.—Geognosy: an investigation of the materials of the earth's substance.

Book 3.—Dynamical geology.

Book 4.—Geotectonic geology; or the architecture of the earth's crust. (*Geotectonic* is a new term proposed as a substitute for *structural*).

Book 5.—Palæontological geology.

Book 6.—Stratigraphical geology.

Book 7.—Physiographical geology.

Comparing this classification with that of other authors, and viewing it with reference to the present condition of the science, we may say without hesitation that it has no superior, and that it is well adapted to existing needs.

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(*To be continued.*)

OUR BOOK SHELF

Uniplanar Kinematics of Solids and Fluids; with Applications to the Distribution and Flow of Electricity. By George M. Minchin, M.A. Pp. viii. + 266. (Oxford: Clarendon Press, 1882.)

IN subject-matter this book is almost unique among our mathematical manuals. The only fellow to it is Clifford's "Kinematic." It consists of six chapters, the first dealing with Displacement and Velocity, the second with Acceleration, the third with Epicycloidal Motion, the fourth with the Mass-Kinematics of Solids, the fifth with the Analysis of Small Strains, and the sixth almost as long as the others put together, with the Kinematics of Fluids. The subdivisions of the last chapter are headed —General Properties: Multiply Connected Spaces; Motions due to Sources and Vortices, Electrical Flow; Conjugate Functions. There is also a short appendix, with notes on such subjects as Vectors and their Derivatives, Current-Power, and Routh's Use of Conjugate Functions.

It is impossible, without occupying considerable space, to give an adequate idea of the freshness and originality which mark Prof. Minchin's work. These are notable in the exceedingly valuable sixth chapter, but even on such well-worn subjects as velocity and acceleration, he treats us to many pleasant little surprises. Nor is this accomplished at the expense of the student; the clearness, fulness, and good arrangement specially requisite in a college text-book are all of them conspicuous; and valuable collections of exercises, worked and unworked, and given at intervals. The book is altogether one for which success may be cordially wished, not merely as a reward to the author, but in order that the science of which he treats may go on as steadily and rapidly advancing as it has of recent years been doing.

Die Käfer Westfalens. Zusammengestelt von F. Westhoff. Abtheilung ii. (Supplement zu den Verhandlungen des naturhistorischen Vereins der preussischen Rheinlande und Westfalens, Jahrgang 38, pp. 141-323.) (Bonn, 1882.)

WE have already noticed the first part of this work in NATURE. The second and concluding portion is now before us. It forms one of the most useful local Beetle catalogues that we have seen, nicely printed (the names being in bold black type), with copious local and other information. The district comprises about 450 square (German) miles, and is varied in its physical conditions. In all, 3221 species are enumerated, in 59 families. The *Staphylinidæ* comprise 667 species, *Curculionidæ* 471, *Carabidæ* 321, *Chrysomelidæ* 265, and *Dytiscidæ* 115. All the other families have each less than 100 representatives, and 10 of them less than 5. The nomenclature followed is that of the newest "Stein-Weise" German list, which, as is well known, has introduced a great multitude of changes and innovations; but other generally received names are indicated in brackets, thus avoiding confusion. Westhoff describes no new species in Part ii., but indicates and names a good many new (chiefly colour) varieties. Probably the rage for naming colour-varieties, so wide-spread at the present day, should be deprecated. For instance, in this catalogue we find a list of 27 named varieties following the indication of *Coccinella* 10-*punctata*, L., and 6 or 8 analogous varieties are appended to many other species of Ladybirds. Taking it as a whole, this excellent catalogue may serve as a model for compilers of lists of the Beetle (or other entomological) fauna of other districts.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.*]

Equal Temperament of the Scale

In your number of November 8, 1877, p. 34, Mr. Chappell, F.S.A., has intimated that mathematicians who propose to divide the octave into twelve equal semitones instead of "equally tempered semitones," are deficient in musical ear. I have not noticed that any mathematician has replied to him.

Representing (with Mr. Chappell) the number of vibrations in the C of my piano by 1, and the octave *c* therefore by 2, and dividing the octave into 12 equal intervals, I obtain for the vibration-numbers—

C = 1·	G = 1·4983 = $2^{\frac{7}{21}}$
C♯ = 1·0594 = $2^{\frac{1}{12}}$	G♯ = 1·5874 = $2^{\frac{8}{12}}$
D = 1·1224 = $2^{\frac{2}{12}}$	A = 1·6818 = $2^{\frac{9}{12}}$
D♯ = 1·1892 = $2^{\frac{3}{12}}$	B♭ = 1·7818 = $2^{\frac{10}{12}}$
E = 1·2599 = $2^{\frac{4}{12}}$	B = 1·8877 = $2^{\frac{11}{12}}$
F = 1·3348 = $2^{\frac{5}{12}}$	*c* = 2·
F♯ = 1·4142 = $2^{\frac{6}{12}}$	

In these equal semitones each is equidistant from the preceding and following: as F is to F♯, so is F♯ to G, &c. Hence in whatever key I play a passage on my piano, the divergence from harmonic intervals will be alike at every point; the keys on my piano will have no distinctive character, the key of 3 sharps will not be more "brilliant" or less "plaintive" than that of 4 flats.

In the key of C, the harmonic third, fifth, and seventh will be, according to the above notation, 1·25, 1·5, and 1·75 respectively. As regards the fifth G it is a remarkable numerical coincidence that $2^{\frac{7}{12}}$ only differs from 1·5 by $\frac{1}{600}$, *i.e.* the equal temperament G only differs from the harmonic by its $\frac{1}{400}$ part, a difference so slight that it may be neglected. We tune fiddles by fifths therefore. This coincidence is the fundamental fact which enables us to modulate into various keys on a piano, and it is the reason why the scale must be divided into 12 (and not any other number of) semitones; for it will be found that, until you get to the unmanageably high number of 53, no other equal division of the scale has any note so near the harmonic G.

The crucial point of tempering arises on the third. The E of my piano is $2^{\frac{1}{3}} = \frac{126}{100}$, whereas the harmonic E is $= \frac{125}{100}$; my E is therefore by its $\frac{1}{80}$ part too sharp, *in the key of C*, a perceptible degree of error, unpleasant to many musicians. In ordinary pianoforte tuning, the E (by the plan in Hamilton's pianoforte tuner or some similar compromise) is tuned somewhere between $\frac{125}{100}$ and $\frac{126}{100}$, say $\frac{125\frac{1}{2}}{100}$, and the wolf between this E and the upper *c* is distributed.

This is all very simple so long as we remain in the key of C; indeed if we remain there, we want no tempering. But G♯ is the third to E, and *c* is the third to A♭; on the piano G♯ and A♭ are one. On my equal-semitone piano I have

$$c = 1\,;\ E = 2^{\frac{1}{3}}\ (= \tfrac{126}{100} \text{ nearly}\,;$$

$$G\sharp = A\flat = 2^{\frac{2}{3}}\ (= \tfrac{157}{100} \text{ nearly})\,;\ c = 2.$$

I now ask the champion of "equally tempered semitones" what is the numerical value of his E and what of his G♯. If he gives hem any other values than $2^{\frac{1}{3}}$ and $2^{\frac{2}{3}}$ respectively, it is clear hat a *greater* error will be introduced in one part of the scale than is saved in another. Instead of algebraic proof I take an instance—suppose that Mr. Chappell tunes his E at $\frac{125\frac{1}{2}}{100}$; if he equally tempers his G♯ in the scale of E, it will be $\left(\frac{125\frac{1}{2}}{100}\right)^2 = \frac{31}{20}$ very nearly. Then when he puts down the common chord in the key of A♭, his third the *c* will be by its $\frac{1}{32}$ part too sharp, whereas on my equal temperament piano it would only be by its $\frac{1}{80}$ part too sharp. In other words, though the keys of C and E may be somewhat better on Mr. Chappell's piano than on mine, the key of A♭ will be *very much* worse. This is pretty nearly what occurs in practice. The point of my argument is that Mr. Chappell cannot move his E ever so little from the value $2^{\frac{1}{3}}$ without introducing a greater error somewhere else. The term "equally tempered semitone" is inaccurate; the semitones on my piano are all equal; and no one of them can be altered by a disciple of the "equally-tempered semitone" without making them unequal. The "equally-tempered semitones" are *not* equally tempered. Moreover if you "temper" *at all* you lose the effect of the harmonics; by moving E from $\frac{125}{100}$ to $\frac{125\frac{1}{2}}{100}$ you sacrifice harmonic coincidence.

The simple reason that unequal tempering is practised is because all keys are not used equally often. A piano is unequally tempered so that the keys C, G, A, F are fair, E, B♭, E♭ tolerable, the other keys being very much worse than on my equal-semitone piano. On most church organs, being unequally tempered, if you modulate even transiently into 4 or 5 flats, the effect is unendurable.

The crucial question in tuning is the question, if your E is not $2^{\frac{1}{3}}$ and your G♯ $2^{\frac{2}{3}}$, what values do you put them at? The question of the seventh is more complex; I may observe that though my equal-semitone seventh (1·7818) appears far away from the harmonic seventh (1·75), yet that the B♭ of tuners on the "equally-tempered semitone" system is not much nearer it. Their B♭ is $\frac{16}{9}$ or thereabout, or in other words, the sub-subdominant of C. Therefore, on the piano, you have not got the "harmonic-seventh" at all; the note which replaces it is one that suggests overpoweringly the key of F. This is the secret which underlies several of our rules in harmony. It is also the reason why valve-horn players play B♭ (though an open note) with valve *n*.2, or if they play without a key "lip it up" very carefully.

It is often supposed that the "wolf" has been introduced into music by that most useful though imperfect instrument the piano, and that the noble violin or human voice knows it not, except in so far as our natural good ear for harmonic intervals has been debauched by continually hearing tempered intervals. This is not so; the "wolf" is not only in the piano but in the scale. It is true that a violin can play in harmonic tune so long as the melody runs in one key, or if it modulates into a closely allied key, and *back again the same way*. But suppose my violin begins by rising from C to E harmonically, *i.e.* to $\frac{125}{100}$; then after playing awhile there proceeds to G♯ $(\frac{125}{100})^2$ harmonically, being then in 8 sharps; and then, after playing awhile in 8 sharps, proceeds to *c*; the *c* of the fiddle will then be $(\frac{125}{100})^3$ instead of 2, *i.e.* it will be $\frac{3}{128}$ out of tune. In this simple case the fiddle is supposed to play alone, unfettered by any harmonics but its own; in the case of a string-band, the agreeableness of many modulations actually depends upon some chords being harmonically out of tune, the note in the chord which performs the duty of G♯ to its preceding chord, performing the duty of A♭ to its succeeding chord.

The practical conclusion is that the best plan of tuning a piano for vulgar music and vulgar players is that now ordinarily practised by the tuners, and recommended by Mr. Chappell; but if the piano is to be used equally in all keys (or even frequently in 4 or 5 flats, 5 or 6 sharps) the best plan is to tune it in 12 mathematically equal semitones. C. B. Clarke

Animal Intelligence

In an excellent paper on "Animal Intelligence" (Nature, vol. xxvi. p. 523), Mr. C. Lloyd Morgan says that "The brute has to be contented with the experience he inherits or individually acquires. Man, through language spoken or written, profits by the experience of his fellows. Even the most savage tribe has traditions extending back to the father's father. May there not be, in social animals also, traditions from generation to generation, certain habits prevailing in certain communities in consequence neither of inherited instincts nor of individual experience, but simply because the young ones imitate what they see in their elder fellows?

As is well known, the stingless honey-bees (*Melipona* and *Trigona*) build horizontal combs consisting of a single layer of cells, which, if there is plenty of space, are of rather regular shape, the peripheral cells being all at about the same distance from the first built central one. Now, on February 4, 1874, I met with a nest of a small Trigona ("Abelha preguicosa") in a very narrow hole of an old canella-tree, where, from want of space they were obliged to give to their combs a very irregular shape, corresponding to the transversal section of the hole. These bees lived with me, in a spacious box, about a year (till February 10, 1875), when perhaps not a single bee survived of those which had come from the canella-tree; but notwithstanding they yet continued to build irregular combs, while quite regular ones were built by several other communities of the same species, which I have had.

The following case is still more striking. In the construction of the combs for the raising of the young, as well as of the large cells for guarding honey and pollen, our *Meliponæ* and *Trigonæ* do not use pure wax, but mix it with various resinous and other substances, which give to the wax a peculiar colour and smell. Now I had brought home from two different and distant localities two communities of our most common *Melipona* (allied to *M. marginata*), of which one had dark reddish-brown, and the other pale yellowish-brown wax, they evidently employing resin from different trees. They lived with me for many years, and either community continued, in their new home, to gather the same resins as before, though now, when they stood close together, any tree was equally accessible to the bees of either community. This can hardly be attributed to inherited instinct, as both belonged to the same species, nor to individual experience about the usefulness of the several resins (which seemed to serve equally well), but only, as far as I can judge, to tradition, each subsequent generation of young bees following the habits of their elder sisters. FRITZ MUELLER

Blumenau, St. Catharina, Brazil, November 14, 1882

The Inventor of the Incandescent Electric Light

IN the "Notes" of NATURE, vol. xxvii. p. 209, M. de Chagny is described as "the first electrician who attempted to manufacture incandescent lamps *in vacuo* about twenty years ago." This invention and its successful practical application (irrespective of cost) was made by a young American, Mr. Starr, and patented by King in 1845. A short stick of gas-retort carbon was used, and the vacuum obtained by connecting one end of this with a wire sealed through the top of a barometer tube blown out at the upper part, and the other end with a wire dipping into the mercury. The tube was about thirty-six inches long, and thus the enlarged upper portion became a torrecellian vacuum when the tube was filled and inverted. I had a share of one-eighth in the venture, assisted in making the apparatus and some of the experiments, and after the death of Mr. Starr all the apparatus was assigned to me. I showed this light (in the original lamp) publicly many times at the Midland Institute, Birmingham, and on two occasions in the Town Hall, all of them more than twenty years ago. The light was far more brilliant, and the carbon-stick more durable, than the flimsy threads of the incandescent lamps now in use. It was abandoned solely on account of the cost of supplying the power. As a steady, reliable, and beautiful light, its success was complete. In "A Contribution to the History of Electric Lighting," published in the *Journal of Science*, November 5, 1879, and reprinted lately in my "Science in Short Chapters," may be found further particulars concerning this invention and its inventor. W. MATTIEU WILLIAMS

Stonebridge Park, N.W.

The Reversion of Sunflowers at Night

WHILE the fact that sunflowers turn their faces toward the sun in its course during the day is as old as our knowledge of the plant, I am not aware that any record has been made as to the time of night that they turn to the east again after their obeisance to the setting sun.

One evening during a short stay at a village in Colorado, in the summer of 1881, I took a walk along the banks of a large irrigating ditch just as the sun was setting. The wild variety of *Helianthus annuus*, Lin. (= *H. lenticularis*, Douglass) grew abundantly there, and I observed that the broad faces of all the flowers were, as is usual in the clear sunset, turned to the west. Returning by the same path less than an hour afterwards, and immediately after the daylight was gone, I found, to my surprise, that much the greater part of those flowers had already turned their faces full to the east in anticipation, as it were, of the sun's rising.

They had in that short time retraced the semi-circle, in the traversing of which with the sun they had spent the whole day. Both the day and night were cloudless, and apparently no unusual conditions existed that might have exceptionally affected the movements of the flowers.

I doubt not that many persons like myself have supposed that sunflowers remain all night with their faces to the west, as they are when the sunlight leaves them, and until they are constrained by the light of the rising sun, to turn to the east again. It is not my purpose to offer any explanation of the cause of the phenomenon here recorded, but it seems to me improbable that it could have been an exceptional instance; and I only regret that no opportunity has since occurred to me to repeat the observation.

Washington, December 26 C. A. WHITE

Pollution of the Atmosphere

MR. H. A. PHILLIPS, in NATURE, vol. xxvii. p. 127, thinks that the effect of the increasing quantity of hydrocarbons in the air from the combustion of coal will be to make climates more extreme. It seems to me the effect will be the direct contrary. Gaseous and vaporous hydrocarbons absorb heat much more powerfully than air, and whatever makes the atmosphere absorb and retain more solar heat than at present will tend to equalise temperatures between day and night, and also between different latitudes. I think, however, that any possible effect of hydrocarbons will be quite insignificant in comparison with the effect of the watery vapour of the atmosphere, which, as Tyndall has shown, moderates climates by its power of absorbing solar heat. JOSEPH JOHN MURPHY

Old Forge, Dunmurry, Co. Antrim, December 28, 1882

A "Natural" Experiment in Complementary Colours

ON page 79 of vol. i. of the "Life, Letters and Journal of Sir Charles Lyell," his visit to the Fall of the Rhine at Schaffhausen is described, and he notes that "as the sun shone on the foam it took very much the rose-coloured tint so remarkable on the snow in the Alps."

His experience as regards the colour being observed in the full sunlight seems to differ from that of Mr. Chas. T. Whitmell, which you published in NATURE, vol. xxvi. p. 573.

E. J. BLES

Moor End, Kersal, near Manchester, January 8

BAIRDS' HARE AND ITS HABITS

SEVERAL instances have been recorded in which individual male mammals have produced milk from their mammary glands for the nutriment of their young. But that the young of a mammal should be ordinarily suckled by the male parent is such an extraordinary anomaly that it is very hard to believe it. Yet that such is the case in an American species of hare (*Lepus bairdii*) would seem to be highly probable from observations made by Dr. Hayden and his party during one of their expeditions in the Yellowstone Mountains. In the last number of the *American Naturalist*, Mr. Lockwood gives the following details on this curious subject:—

"In the months of May and June, 1860, Prof. F. V. Hayden and his party of United States explorers found themselves up in the Alpine snows of the Wind River Mountains, where they were detained several days in an attempt to feel their way to the Yellowstone. On May 31 Dr. Hayden declared that a new species of hare was around, as he had observed unusually large hare-tracks in the snow. As the Doctor expressed himself to us:—The tracks were very large, the feet being wide-spread, and the hair thick between the toes, thus really furnishing the animal with snow shoes." In June, one was captured, and the Doctor named the species *Lepus bairdii.* The animal seemed limited to that small Alpine territory. But one specimen was secured, and no more was heard of this hare until 1872, when Dr. Hayden and party were in that region in the months of August and September. At this time five specimens of Baird's hare were obtained by Mr. C. Hart Merriam, the naturalist to the Hayden Survey. Of these four were adult males, and all had large teats and udders full of milk. The hair round the nipples was wet, and stuck to them, showing that they had just been suckling their young. To make all certain, resort was had to dissection, when the sex was demonstrated. Not only did Mr. Merriam make dissection, but also Dr. Josiah Curtis, a naturalist of the United States Geological Survey, with the same result. In the face of such testimony disbelief would seem discourtesy."

NOTES FROM THE LETTERS OF CAPTAIN DAWSON, R.A., IN COMMAND OF THE BRITISH CIRCUMPOLAR EXPEDITION[1]

July 30, *Fort Chipewyan, on Lake Athabasca*

AFTER practically incessant travel since leaving England, at last I find myself condemned to a week's rest, as there are no boats going to Port Rae until the Mackenzie River boats return. But here we are in the lap of luxury; we get bread, butter, and milk, which we have not tasted for ages, to say nothing of the novel experience of sleeping under a roof and on a bed. I have had a most delightful journey, but it all seems like a dream to look back to: my memory is a kaleidoscope of pine trees, rapids, lakes, and golden sunrises and sunsets. Down stream we travel day and night. At sunset the boats are lashed together, and then the crew go to sleep. It is very nice drifting down in the silence amongst the pines, but bed-time comes at last. I then roll myself in a blanket, lie down, and look at the stars till I fall asleep. At sunrise I wake to find the crew on the shore, boiling their kettle, and a cup of tea is very refreshing. My blanket and my hair, too, I find dripping wet with dew when I wake.

At noon or so we land, and cook more tea, and make breakfast usually off pemmican, which is composed of buffalo flesh dried and pounded, and put in a leather bag with grease poured over it. It is not nice, but it supports life. When we have such a luxury as flour, it is baked into cakes in a frying-pan. We get into the boat again, and eat our breakfast whilst drifting down stream. Bye-and-bye the current becomes more rapid, and at last we see the river disappearing in a cloud of spray. Here is a Portage, so the boats pull to shore and the cargo is landed. The crew then return. I take my place in the boat, and after each man has laid aside his pipe, settled himself in his seat, and got a good grip of his oar, we shove off and dash into the rapid as fast as twelve oars can take us, with shouts of "Hurrah! boys!" (the only English words the Indians know) and "ekwa," a Cree word, meaning "Come on." The guide or steersman stands on a seat in the stern steering the boat with a long oar—a picturesque figure, with his long black hair waving behind him. In a moment we are among the rapids, and seem to sink into a mass of foam, from which we emerge sideways, and are carried towards a projecting rock. Wild exclamations in French from the guide! the bow oarsman seizes a pole, and sends the boat off, and then we spin down the tail of the rapids, not without one or two bumps that make the whole frame of the boat quiver. The whole distance, a mile or two, is done in two or three minutes, and it is not bad fun. After the boats have run the rapids it is dinner-time, and then the crew set to work to carry the cargo over the portage—a work of two or three hours. In some places the boats themselves have to be hauled across on rollers, which is pretty hard work. We continue our way down stream, stopping about 4 o'clock for tea, and at sunset reach another Portage. Here we camp, and in a very short time the tents are pitched, a tree felled to make a camp-fire, and kettles singing thereon. Supper and bed-time make up the day. Such is a fair specimen of a day's river travelling. With a fair wind we sail, especially on the lakes.

The crews are Chipewyans; their language is chiefly made up of clicks and gurglings in the throat, and differs altogether from Sioux, Cree, and the other languages spoken further south.

A Roman Catholic priest here showed me a Chipewyan grammar and dictionary that they have composed. There are over sixty sounds in the language, so they have to invent additional letters. There is something Asiatic in the appearance of these Indians, with their small moustache and tufts of hair on their chin, quite unlike the Indian of the plain. They are Roman Catholic.

After leaving Portage la Loche, on July 24, the first day's journey took us down to the Terre Blanche falls; here we had to haul the boats over a small hill, as the river is a succession of falls and rapids for about half a mile; a very pretty place, the river runs between limestone cliffs, crowned with pine trees, and all stained bright orange colour with lichen.

On the 28th we reached the Athabasca, a splendid river, usually half a mile in width, sometimes more. Its course is pretty straight to the north, so we often had a view of some fifteen miles or so down the valley.

On the 29th, having a fair wind, we made a hundred miles. We met two lots of Indians; from the first we got some moose, the first fresh meat we had tasted for a long time, and from the others we got some raspberries and asketoon berries, which were very refreshing.

As we drifted down the river, the pines began to give place to poplar, the poplar to willow, and the willow to reeds, till at last we saw Lake Athabasca before us, a rocky coast to the north, and to the east water as far as the eye could reach.

A fresh breeze took us across the lake in two hours, and we received a hospitable welcome at this place, together with all sorts of luxuries that had become quite strange to us.

This is quite a large place; there are about a dozen houses, two churches, two bishops, a sisterhood, and some missionaries. The country is rocky, and most desolate. To the south and west the great lake stretches away to the horizon, and the land view is composed of hills of reddish granite, no soil, plants growing here and there out of occasional crevices, and a few stunted firs scattered about. There are woods in the valleys, but the trees are of no size. No sound breaks the stillness but the weird cry of the loon, a sort of maniacal laugh that is almost a wail; and the solitude is heightened by the reflection, that for 1000 miles north, south, east, and west all is wilderness.

Towards the lake the view is pretty, as there are many islands covered with pines.

The weather is cooler than it has been, I am glad to say. For days we had the thermometer at 85° and 86°, and even higher; but though hot, the summers are short, and I think that of this year is over. The mosquitos, at any rate, are beginning to disappear, and now the climate is nearly perfect, like the best English summer weather.

August 5.—There was a fine aurora last night: a curtain of flame seemed to descend from the sky nearly overhead and right across the sky, and after waving about for a few moments, died away again. Yesterday I went to see the Roman Catholic Mission; they have quite a pretty church, which has been built some thirty years. I was also taken to see the sisters, of whom there are six. They all seemed very flourishing, and have a very nice house.

[1] Continued from p. 105.

August 6, *Sunday.*—I was at the English church this morning. It is a nice little church, and there was a congregation composed of the Hudson's Bay people, twenty or thirty. Most of the Hudson's Bay people are Scotch, many coming from the Orkneys. The Bishop Bompas is very pleasant, he is a great traveller, and has lived amongst the Esquimaux at the mouth of the Mackenzie River, and he works very hard.

August 9.—The weather has been stifling hot, 89° indoors, for the last three days, quite like the West Indies. Yesterday I went over to see a performance at the Roman Catholic Mission of the school children, got up by the sisters in our honour. They sang, and acted, and danced remarkably well. They have very good memories I am told.

It is curious living together without money, as one does in this country. Everything is done by barter, the unit of value being a skin ; the average value of a beaver skin is said to be worth twenty ducks, or forty white fish, or twenty plugs of tobacco, so that for a plug of tobacco (about an ½ oz.) one can get a duck or two white fish, a large fish about two feet long, and very good eating. This place, like all other habitations in the north-west, swarms with large wolf-like dogs. These are used in winter for drawing carrioles, and a team of four dogs will draw 500 lbs. or more. The Indians use them too, in summer, as pack animals.

The boats have just made their appearance, four black specks on the horizon to the north, so we shall be off in a few hours.

THE SWEDISH EXPEDITION TO SPITZBERGEN, 1882

THE results of the researches of the expedition despatched to Spitzbergen last summer by the Swedish Academy of Sciences, under the eminent *savants* Baron G. de Geer and Dr. Nathorst, for the study of the geological and geographical features of the island, are very interesting. In the first instance, these gentlemen have drawn two maps, showing the exact geographical features of the island, as compared with those prepared by two previous expeditions. Of these, one shows the outlines of the fjords and valleys in the southern part of the island, with the boundary of the inland ice, and the other the relative depth of the seas around Spitzbergen and Scandinavia. From the latter it appears, that these two land-formations are really elevated ridges on a comparatively level plateau, which sinks abruptly in the ocean west of Spitzbergen. In the second instance, the expedition has ascertained that the deep fjords and narrow valleys of the island have not been formed by upheaval of the terrestrial crust or by strong water-courses, but are due to the action of glaciers during the Glacial period, while from the marks on the rocks of the Beeren Island, it may be assumed that the Spitzbergen glaciers extended even so far.

At the close of the Glacial period a sudden subsidence, followed by a still greater rising of the shores, both of Spitzbergen and Scandinavia, most probably took place, which is demonstrated by the discovery, in Scandinavia as well as Spitzbergen of old gravel beaches and the shells of salt-water mussels far inland. The existence in Spitzbergen of some of the most characteristic species of the Scandinavian flora and fauna, may perhaps be explicable by migration from Scandinavia, at a period when the plateau between the two ridges was above the level of the sea, we may assume, shortly after the close of the Glacial period. It seems impossible to explain otherwise how, for instance, birds, particularly those living on land, could have found their way to this island, some 700 miles distant from the Scandinavian peninsula.

At the same period, the common Scandinavian "Blaamusling," *Mytilus edulis*, and a few other species have, no doubt, also migrated into the island. This species is now, however, extinct, but the large quantities of shells found on the shores indicate that at one time it must have been common enough. The latter circumstance seems to prove that the climate of Spitzbergen at an earlier period was much milder than at present, and corroborates also the theory of a connection having existed between Spitzbergen and Scandinavia about the Glacial period, as such a land-barrier would have caused the eastern arm of the Gulf Stream, which now flows by the North Cape, to have taken a more northerly direction, and thus carried the softening elements of a southern clime to the now desolate rocks in the Arctic Ocean. C. S.

THE INCREASE IN THE VELOCITY OF THE WIND WITH THE ALTITUDE

THE fact that the upper strata of the atmosphere as a rule move more rapidly than those near the earth's surface, has long been inferred on theoretical grounds, though little direct evidence beyond the marvellous and often unexpected voyages of aëronauts, or casual observation of the clouds, has hitherto been furnished in its favour. The practical value of this fact is beginning to be felt by engineers since the investigations undertaken by Mr. T. Stevenson in 1876, and more recently (see *Journal* of Scottish Meteorological Society, vol. v. pp. 103 and 348), showed that even for moderate heights the old notion of assuming the wind to be of uniform velocity at all altitudes was seriously in error, and that to rely upon it in the case of lofty structures might entail disastrous consequences.

While Mr. Stevenson's experiments have shown that the wind's velocity increases very considerably, especially near the surface, they do not touch the question of the increase noticed at great heights, nor can the formulæ or conclusions derived from them be said to throw any light on a matter which evidently contains the germs of many important truths for the meteorologist.

Where the engineer ends in fact the meteorologist may be said to begin ; but in this case the engineer ends a little too soon, since Mr. Stevenson's latest experiments terminate at the top of a pole only 50 feet high, where he leaves us with a formula "believed to be sufficiently accurate for practical purposes," and which is said to give the velocity for "*great heights* above sea-level." Whence Mr. Stevenson obtains this formula, or on what data he believes it to be approximately correct, we are not told, and here the question is left in a state of uncertainty for greater heights, in which we trust neither engineers nor meteorologists will allow it long to remain. It might even be advantageous to the former, if instead of trusting to a few empirical formulæ, they would ask the meteorologists what they knew about the matter, and joined with them in endeavouring to discover a rational formula which would yield satisfactory results at all elevations.

Theoretically the main factor at small elevations in determining the increase of velocity, would appear to be the diminution of friction as we rise above the surface, and as this must occur most decidedly near the surface, so the velocity must increase in the first few feet "per saltum." Mr. Stevenson's experiments and curves show this very clearly. Indeed up to a height of 15 feet the increase is so sudden, so irregular, and so clearly dependent on the nature of the surface, that no attempt has been made to include this space within a formula.

There is, however, another factor which acts *positively* in the same direction, and which, while operating for the most part at great heights, where its influence ultimately predominates to the exclusion of the friction factor, must be felt to some extent at comparatively moderate elevations.

I allude to the general increase in the barometric gradient with the height above the earth's surface, due to the general temperature gradient between the equator and the poles, in conjunction with the earth's rotation. This fact has been thoroughly investigated by Mr. Ferrel of the U.S. Coast Survey, and the results given in his "Meteorological Researches," vol. i. In this work he has given on p. 45 the mean west-easterly component of the velocity at the surface due to the causes just mentioned, and the term by which this increases with the height (in metres) for every fifth degree of latitude on the mean of all longitudes, for the months of January and July, and for mean annual temperatures, calculated from the observed barometric pressures and temperatures in every part of the world.

For latitude 50° the eastward velocities at the surface and increment terms for the elevation are as follows in two different measures.

	Mean temperatures.	January.	July.
Miles per hour	$3{\cdot}35 + 8{\cdot}6h$	$3{\cdot}97 + 12{\cdot}1h$	$2{\cdot}73 + 5{\cdot}1h$
Feet per second	$4{\cdot}91 + {\cdot}0024h$	$5{\cdot}82 + {\cdot}0033h$	$4{\cdot}00 + {\cdot}0014h$

where h represents the height in miles and feet respectively.[1]

Owing to *this cause alone* therefore the eastward (and therefore in our latitudes the prevailing) motion of the atmosphere will be increased on the mean of the year by $8\frac{1}{2}$ miles per hour at a height of 5280 feet, or by $2\frac{1}{8}$ miles per hour at a height of about 1300 feet.

The increase in the horizontal velocity which results from the joint action of these two factors, is thus probably very different from that which would arise from a mere diminution of friction alone, since at great heights this would theoretically become almost insensible.

For a thoroughly satisfactory solution of the matter, nothing will avail except anemometrical observations made at every possible elevation (preferably, as I lately suggested in a paper read before the Meteorological Society, with instruments attached to kite-strings), but in the absence of these at present, it may be worth while to use some excellent observations on the velocity of different cloud-layers recently communicated to the Austrian *Zeitschrift für Meteorologie*, by Dr. Vettin,[2] for the purpose of showing the complete breakdown of Mr. Stevenson's formula when applied to "great heights above sea-level."

The following table, which is taken from Dr. Vettin's paper, gives the mean velocity of the clouds from all directions, at five altitudes to which they respectively belong, and which, together with their velocities, have been measured by methods described in detail in the paper from which it is extracted :—

TABLE I.

Name of station.	Barometric pressure.[3] in.	Height in feet.	Number of observations.	Mean velocity in feet per second.
Upper cirrus	11·968	23,000	879	59·5
Under cirrus	17·953	12,800	1047	51·8
Cloudlets[4]	22·441	7200	1588	35·0
Cloud	25·733	3800	1871	30·4
Under cloud	28·127	1600	1292	37·4
Wind (sea-level)	29·922	0	4168	19·8

It will be seen from this table that while there is a rapid increase in the velocity of the wind through the first 1600 feet, an abrupt diminution occurs between this height and 3800 feet, after which the motion again increases at a more moderate rate.

Now Mr. Stevenson's formula for heights above 50 feet is $\frac{V}{v} = \frac{H}{h}$, where V, v, H, h, are the velocities and heights at the upper and lower stations respectively. If we apply this formula to the preceding table and calculate the heights at the higher levels from those at the lower ones, we get for the most favourable cases the following values :—

Observed Velocity.	Observed Height.	Calculated Velocity.	Calculated Height.
37·4	1,600	259·2	12,800
51·8	12,800	113·1	23,000

which are so absurdly in excess of those observed at the same levels, and so far beyond what we might reasonably expect as to render it doubtful whether this formula is true for any height above the first 100 feet. Even the formula which is supposed by Mr. Stevenson to fail above 50 feet gives better results than this one at the higher levels.

This formula is $\frac{V}{v} = \sqrt{\frac{H+72}{h+72}}$, and from the same observed values as those used above gives the following calculated values :—

Height.	Velocity.
12,800	103·7
23,000	69·3

but even these are far in excess of those observed. Both formulæ moreover fail lamentably up to 1600 feet, for even if we assume that 19·8 represents the velocity, not at sea-level, but at an elevation of 100 feet above it (an exceedingly favourable assumption since the velocity at this height would considerably exceed that at sea-level), the first formula would make the velocity at 1600 feet *four* times, and the second more than *three* times that actually observed by Dr. Vettin.

It is plain, therefore, that both formulæ must fail considerably below 1600 feet, and until further evidence is furnished it would seem probable that neither of them give correct results much above 100 feet or so.

For practical engineering purposes no doubt they would succeed only too well, since they would probably give a maximum velocity far in excess of the truth, and this, judging from examples such as the Tay Bridge, would be no disadvantage when the force of the wind enters into engineering calculations.

It would surely be better, however, if we could arrive at a somewhat closer approximation to the truth, and better still if we could arrive at the truth itself. This, as I have already pointed out, will be only accomplished for the lower strata by further experiment in the same direction as that already followed by Mr. Stevenson, modified by attaching anemometers to kite strings, and for the upper strata, which chiefly concern the meteorologist by observations of the clouds similar to those made by Dr. Vettin.

Meanwhile, however, I have found a formula which gives very much more satisfactory values at the higher levels than those furnished by Mr. Stevenson. This formula is—

$$\frac{V}{v} = \sqrt[4]{\frac{H}{h}}$$

And although I do not expect it will be found to hold *very near* the surface, it certainly accords, omitting the anomalous case at 3800 feet, from 1600 feet to 23,000 feet, or through a range of 21,400 feet, very closely with the values observed by Vettin.

The figures observed and those calculated from this formula are as follows :—

TABLE II.

Height of lower stratum.	Height of upper stratum.	Velocity Observed.	Velocity Calculated.
3,800	7,200	35	35·6
1,600 / 7,200	12,800	51·8	51·5[1]
12,800	23,000	59·9	59·5
23,000	41,000	67[2]	68·7

[1] The mean of the two. [2] Calculated by Vettin.

[1] It must be noted that the surface velocities given in this table are somewhat in excess of the truth, owing to the neglect of surface friction, but this does not affect the increment terms to any large extent.

[2] *Zeitschrift für Meteorologie*, Band xvii., July and September Heft; "Die Luftströmungen über Berlin."

[3] Reduced from the original figures in millimetres. [4] Wölkchen.

Moreover, if we assume that 19·8 is the velocity at 100 feet, the velocity at 1,600 feet calculated from it by this formula would be almost exactly *equal* to that observed, instead of *four* times as much as it was when calculated from $\frac{V}{v} = \frac{H}{h}$. The empirical formula $v = 28·1 + 0·2 \sqrt{h}$, where v is the velocity in feet per second, and h is the corresponding height in feet, gives for the four elevations above, results in close agreement with those observed, but it would probably fail below 1,600 feet.

Mr. Stevenson in his first paper uses a formula $\frac{F}{f} = \sqrt{\frac{H}{h}}$ where Ff are the forces ("pressures" I suppose is meant by this objectionable word) at the two levels corresponding to H and h.

In his second paper he says he prefers the formula $\frac{V}{v} = \frac{H}{h} = \frac{P}{p}$, where P and p are the pressures at the two levels to the formula $\frac{V}{v} = \frac{\sqrt{H}}{\sqrt{h}}$, from which and some other remarks it would appear that pressure and velocity are considered to vary directly with each other. This is a notion which is certainly at variance not only with the hitherto generally accepted empirical formulæ, but is distinctly contrary to the results lately deduced by Mr. Ferrel from the hydrodynamical theory (see Van Nostrand's *Engineering Magazine*, vol. xxvii. p. 141). The formula usually found in the text-books is $p = ·00492\, v^2$, and the one deduced by Ferrel is

$$p = \frac{·0027}{1 + ·003665\, t}\, \frac{P}{P'}\, v^2,$$

where $P\,P'$ are the barometric pressures at the level under consideration and at sea-level respectively, and t is the temperature in degrees Centigrade; and though from the latter formula it is evident that the pressure at the higher levels will be less for the same velocity than below (at the height of Mont Blanc, for example, it would be reduced by about one-half) there is nothing to lead us to infer that $\frac{V}{v} = \frac{P}{p}$.

If in the formula $\frac{F}{f} = \sqrt{\frac{H}{h}}$ which was discarded by Mr. Stevenson, we make the ordinary assumption that $\frac{F}{f} = \frac{V^2}{v^2}$ we get the formula which I have already shown gives results which agree closely with the values observed by Vettin above 1,600 feet, and which even below this height is much nearer the truth so far as can be inferred from the slender data employed, than the formula preferred by Mr. Stevenson.

If we take the heights as abscissæ, the curve traced out by the velocity-ordinates will be much flatter than the ordinary conical parabola, and at great heights will approximate very nearly to a straight line parallel to the axis. Ferrel's increment-term makes it a straight line all through, but his formula assumes that the temperature gradient between the pole and the equator is the same above as at the earth's surface, it leaves out friction altogether, and also supposes the velocity for the same gradient to be the same at all heights, whereas according to theory it should increase with the height in the ratio $\frac{\cos i}{P}$, where i is the angle the wind makes with the isobar, and P is the barometric pressure at the level under consideration. Notwithstanding these omitted factors, of which the first and last probably tend to destroy each other, it will be found that the addition of the increment corresponding to each altitude to the velocity at the surface observed by Vettin gives us the following fair approximation to the values actually observed, though the calculated values are too small at 1600 feet and too large at 23,000 feet by just about the same amount:—

Height.	Velocity observed.	Calculated from Ferrel's Formula.
23,000	59·5	75·0
12,800	51·8	50 5
7,200	35	37·0
3 800	30·4	28·9
1,600	37·4	23·6
0	19·8	

In conclusion, it is evident that, quite apart from the meteorological side of the question, more investigations like those undertaken by Mr. Stevenson, are urgently required to determine the actual rate of increase of the velocity at moderate heights, from which a formula like the one I have recommended may be deduced, which will yield values more within the range of probability than those furnished by the one which is apparently supposed to suffice for the rest of the atmosphere after we have reached the top of the fifty-foot pole.

E. Douglas Archibald

KRAO, THE "HUMAN MONKEY"

THROUGH the courtesy of Mr. Farini, I have had a private interview with this curious little waif, which he is now exhibiting at the Royal Aquarium, Westminster, and for which he claims the distinction of being the long-sought-for "missing link" between man and the Anthropoid apes. Krao certainly presents some abnormal peculiarities, but they are scarcely of a sufficiently pronounced type to justify the claim. She is, in fact, a distinctly human child, apparently about seven years old, endowed with an average share of intelligence, and possessing the faculty of articulate speech. Since her arrival about ten weeks ago in London, she has acquired several English words, which she uses intelligently, and not merely parrot-fashion, as has been stated. Thus, on my suddenly producing my watch at the interview, she was attracted by the glitter, and cried out *c'ock, c'ock,* that is, *clock, clock!* This showed considerable powers of generalisation, accompanied by a somewhat defective articulation, and it appears that her phonetic system does not yet embrace the liquids *l* and *r*. But in this and other respects her education is progressing favourably, and she has already so far adapted herself to civilised ways, that the mere threat to be sent back to her own people is always sufficient to suppress any symptoms of unruly conduct.

Physically Krao presents several peculiar features. The head and low forehead are covered down to the bushy eyebrows with the deep black, lank, and lustreless hair, characteristic of the Mongoloid races. The whole body is also overgrown with a far less dense coating of soft, black hair about a quarter of an inch long, but nowhere close enough to conceal the colour of the skin, which may be described as of a dark olive-brown shade. The nose is extremely short and low, with excessively broad nostrils, merging in the full, pouched cheeks, into which she appears to have the habit of stuffing her food, monkey-fashion. Like those of the anthropoids her feet are also prehensile, and the hands so flexible that they bend quite back over the wrists. The thumb also doubles completely back, and of the four fingers, all the top joints bend at pleasure independently inwards. Prognathism seems to be very slightly developed, and the beautiful round black eyes are very large and perfectly horizontal. Hence the expression is on the whole far from unpleasing, and not nearly so ape-like as that of many Negritos, and especially of the Javanese "Ardi," figured by me in NATURE, vol. xxiii. p. 200. But it should be mentioned that when in a pet, Krao's lips are said to protrude so far as to give her "quite a chimpanzee look."

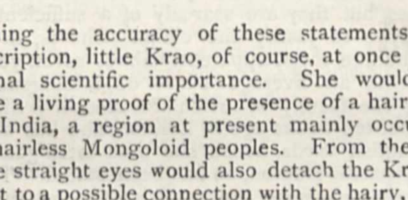
Apart from her history one might feel disposed to regard this specimen merely as a "sport" or *lusus naturæ*, possessed rather of a pathological than of a strictly anthropological interest. Certainly isolated cases of hairy persons, and even of hairy families, are not unknown to science. Several were figured in a recent number of the Berlin *Zeitschrift für Ethnologie*, and, if I remember, both Crawfurd ("Journal of an Embassy to Ava") and Col. Yule ("Mission to the Court of Ava") speak of a hairy family resident for two or three generations at the Burmese capital. This family is reported to have come originally from the interior of the Lao country, and in the same region we are now told that little Krao and her parents, also hairy people, were found last year by the well-known eastern explorer, Mr. Carl Bock. Soon after their capture, the father appears to have died of cholera, while the mother was detained at Bangkok by the Siamese Government, so that Krao alone could be brought to England. But before his death a photograph of the father was taken by Mr. Bock, who describes him as "completely covered with a thick hairy coat, exactly like that of the anthropoid apes. On his face not only had he a heavy, bushy beard and whiskers, similar in every respect to the hairy family at the court of the King of Burmah, who also came from the same region as that in which Krao and her father were found; but every part was thoroughly enveloped in hair. The long arms and the rounded stomach also proclaimed his close alliance to the monkey-form, while his power of speech and his intelligence were so far developed that before his death he was able to utter a few words in Malay."

Assuming the accuracy of these statements, and of this description, little Krao, of course, at once acquires exceptional scientific importance. She would at all events be a living proof of the presence of a hairy race in Further India, a region at present mainly occupied by almost hairless Mongoloid peoples. From these races the large straight eyes would also detach the Krao type, and point to a possible connection with the hairy, straight-eyed Aino tribes still surviving in Yesso and Sakhalin, and formerly widely diffused over Japan and the opposite mainland.[1]

A. H. KEANE

FIGURE OF THE NUCLEUS OF THE BRIGHT COMET OF 1882 (*GOULD*)[2]

ALTHOUGH this comet presented a beautiful spectacle, when seen with the naked eye, I have been disappointed at the small amount of work which I have been able to do in the way of accurate observation. I give herewith the only two good sketches which I have been able to make. The aperture employed was 15 inches, and the power was 145 diameters.



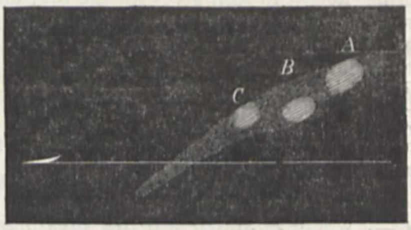
1882, October 13.

1882, *October* 13.—(See the figure.) The nucleus is curved as in the drawing. It consists of three masses. I am sure of a break at *a*; tolerably sure of the break at *b*, and I suspect a break at *c*, but I am not certain of it.

[1] See my paper on "Aino Ethnology" in NATURE, vol. xxvi. p. 524.
[2] Paper by Prof. Edward S. Holden, in the *American Journal of Science and Arts*.

1882, *October* 14.—The night is very poor. (In general the appearances of last night are confirmed.) The nucleus is about 1′ long.

1882, *October* 17.—(See the figure.) There are three masses, plainly separated. *B* is farther north than the line *A—C* by 3–4″. There is a dark division between each pair of masses. *B* and *C* are nearly in the parallel.



1882, October 17.

The brush of light from the mass *A* toward the east, comes from the south side of *A*, as it is drawn. From the W. end of *A* to the E. end of the brush of light, is about 15″.

1882, *October* 18.—The dark space between *A* and *B* is about 10″; it is as wide as *A* itself, and wider than on October 17. *C* is certainly seen as a separate mass; *A* and *B* are bright and stellar in appearance, more so than on October 17. *C* is, however, fainter than then. The dark axis of the tail extends quite up to the coma.

1882, *October* 19.—Cloudy. The nucleus is seen as before. *A* and *B* are seen, as also the dark space between them. *C* is not seen, but this is probably on account of the unsteady air.

I regret that my opportunity did not allow me to make any further sketches of value.

Washburn Observatory, University of Wisconsin,
Madison, November 3, 1882

NOTES

THE office of Director of the Geological Survey of Scotland, vacant by the promotion of Mr. Geikie to be Director-General, has been filled up by the appointment of Mr. H. H. Howell, District Surveyor of the Geological Survey of Scotland during the earlier years of its progress, but who since the separation of the Scottish branch of the establishment in 1867, has been continuously employed in England, where he has personally surveyed large tracts of the northern counties, and where for some years past he has had the direct personal supervision of the whole of the field-work in that district. He will not be able to enter fully on his duties in Scotland until the area now under his charge in the north of England has been completely surveyed. The promotion of Mr. Howell having caused a vacancy in the rank of District Surveyor, Mr. W. Whitaker has been appointed to the post. This geologist is well known for his detailed surveys of the Tertiary deposits of the London basin. He is at present engaged in the survey of Norfolk.

THE United States Transit of Venus expedition, under Prof. Newcomb, arrived at Plymouth on Sunday as passengers by the Union Steamship Company's steamer *Moor*, from Cape Town. They report that their observations were made at Wellington, fifty-eight miles from Cape Town, under extremely favourable conditions, two good observations of internal contact and 236 photographs being obtained, of which more than 200 can be measured.

THE annual general meeting of the Association for the Improvement of Geometrical Teaching will be held, through the kindness of the Council, at University College, Gower Street, on Wednesday, the 17th instant, at 11 a.m. In addition to the usual routine business, the president (R. B. Hayward, F.R.S.)

will propose "that the Committee for Elementary Plane Geometry be instructed to publish Part 1 of the Plane Geometry, and to take such steps as they may deem advisable to secure its recognition as a basis of instruction and examination in geometry." It will be in the recollection of some of our readers that the object of the Association was extended at the last annual meeting, so as to include the effecting of improvements in the teaching of elementary mathematics and mathematical physics. This extension has met with great approval, and the novelty of this year's meeting will be the reading of three papers: (1) "The Teaching of Elementary Mechanics," by W. H. Besant, F.R.S. (2) "The Basis of Statics," by Prof. H. Lamb, of the Adelaide University. (Prof. Lamb is of the opinion that the true and proper basis of statics is to be sought for in the principles of linear and angular momentum). (3) "Notes on the Teachings of Dynamics," by Prof. Minchin. The reading of the papers will be followed by a discussion, in which it is hoped that Prof. G. Carey Foster, F.R.S., Prof. Minchin, and others will take part. The papers will be read at the afternoon meeting which begins at 2 p.m. The two present honorary secretaries will resign office; Mr. Levett, in consequence of the pressing necessity of his other duties, and Mr. Tucker, in consequence of the state of his health, which compels him to withdraw from some of his engagements; both gentlemen, however, hope to remain on the council and act as *amici curiæ* to their successors in office.

THE well-deserved honour of C.I.E. has been conferred upon Surgeon-Major James Edward Tierney Aitchison (Bengal Army), F.L.S., who did such excellent botanical work with Sir Frederick Roberts's force in the Kuram Valley during the Afghan war.

GEOLOGISTS will regret to hear that one of the most promising of the younger members of their number, Mr. E. B. Tawney, of the Woodwardian Museum, Cambridge, died suddenly at Mentone on the 30th ult.

THE death is announced, on December 22 last, of Dr. Carl Hornstein, professor of theoretical and practical astronomy, and director of the observatory in the Carl Ferdinands University, Prag, at the age of fifty-eight years.

THE thirty-sixth annual general meeting of the Institution of Mechanical Engineers will be held on Thursday, January 25, and Friday, January 26, at 25, Great George Street, Westminster. The chair will be taken by the president, Percy G. B. Westmacott, Esq., at half-past seven p.m. on each evening. The following papers will be read and discussed:—Report on the hardening of steel, by Prof. F. A. Abel, C.B., F.R.S., of Woolwich; on the molecular rigidity of tempered steel, by Prof. D. E. Hughes, F.R.S., of London; on the working of blast furnaces, with special reference to the analysis of the escaping gases, by Mr. Charles Cochrane, of Stourbridge, vice-president; on the St. Gothard tunnel, by Herr E. Wendelstein, of Lucerne; on the strength of shafting when exposed both to torsion and end-thrust, by Prof. A. G. Greenhill, of Woolwich.

THE old female Hippopotamus (*Adhela*) presented to the Zoological Society in 1853 by the then Viceroy of Egypt, died in the Gardens on the 16th ult., after having for some time past exhibited manifest signs of old age. Her mate (*Obaysch*) died in 1877, after having lived twenty-seven years in the Gardens. It is thus evident that about thirty years is the extreme limit of Hippopotamine existence, as it is not at all likely (judging from the state of the teeth and bones) that either of these animals would have been able to support existence so long in its native wilds, as under the favourable circumstances in which it lived in the Regent's Park.

DR. BLASIUS of Brunswick has recently shown that the fossil remains of a species of Souslik, found in various parts of Northern Germany, which are usually attributed to *Spermophilus altaicus*, really belong to *S. rufescens*, Keys. et Bl. It is probable that the cave-bones from the Mendip Hills, upon which Dr. Falconer established his *Sp. erythrogonoides* (Pal. Mem., ii., p. 453), are really of the same species, and that this Rodent, now driven far east into the steppes of Orenburgh (like other members of the Steppe-fauna), formerly extended all over Northern Europe, and even into the British Islands.

RECENTLY, our readers may remember, Miss Baxter of Balgavies, sister of Sir David Baxter, and aunt of the Right Hon. W. E. Baxter, and the late Dr. Baxter, Procurator-Fiscal of Dundee, gave jointly 150,000*l.* for the endowment and erection of a college in Dundee. Buildings have been acquired, professors appointed, and the work of the college will soon be begun. Miss Baxter has just given another 10,000*l.* to provide a laboratory, and the trustees of the late Dr. Baxter also 10,000*l.* to found a Chair of Law.

As the late M. Gambetta was a member of the Society of Dissection, an autopsy of his body was made. The weight of his brain was found to be 1100 grams; M. Mathias Duval, Professor in the Faculty of Medicine, found the structure of the brain to be very fine, and the third convolution, which M. Broca associates with the speechifying faculty, to be remarkably developed.

THE project of the United States for establishing an universal meridian has been sent to the Paris Academy of Sciences for approval. It is expected that Great Britain may object to this measure, and it has been proposed that, in consideration of the services rendered to geography by England, that the Greenwich meridian should be selected as the start-point for time and longitude.

OUR Paris Correspondent writes that the second Paris inundation is developing its ravages with peculiarities which prove that modern engineers do not pay sufficient attention to the effects of their works on the *régime* of the stream they profess to regulate. The level of the Seine is just as high at Charenton as it was in 1876, although it is 20 centimetres less elevated at Pont Royal, where it reaches only 7 metres. The reason of this difference is that an ignorant Municipal Council authorised the building of a bridge which crosses the river obliquely, and a new quay at Bercy, where in some places the dimensions of the bed of the stream have been diminished by not less than 55 metres. If the rains continue it is feared that one of the Paris bridges, the Invalides, will be carried away, which will produce real disaster.

PROF. BÖRNSTEIN, of Berlin, has brought out a small work on meteorology under the title of "Regen oder Sonnenschein." In conjunction with Prof. Landolt he has also nearly completed an important work ("Physikalisch-Chemische Tabellen") containing all the most reliable determination of constants required in chemical and physical work, some of which will be published in a collected form for the first time.

MR. J. P. MCEWEN, of Hong Kong, under date November 28, 1882, sends us some observations of the comet, taken on the morning of the 27th:—26d. 15h. 43m. 17s. mean time at place, the distance from Sirius measured by sextant was 34° 32′; 26d. 15h. 50m. 30s. mean time at place, the distance from Procyon was 39° 31′; longitude in time, 7h. 36m. 40s. A line drawn from the small brightest star in the lower part of the sword-scabbard of Orion through Sirius almost exactly passed through the nucleus of the comet; the apparent length of the tail was about twice that of Orion's belt. It was getting very indistinct, and on the 27th, owing to the bright moonlight, was more so than if the night had been dark and clear. Mr. McEwen has seen the comet several times, and when at its greatest brilliancy, stars of the 4th or 5th magnitude could be distinctly seen through the tail. The tail pointed in a direction about midway between Sirius and Procyon. M. Dechevrens, the director of the Zi-ka-Wei Observatory (near Shanghai) has devoted a good deal of attention to this comet, the result of which will directly be published.

Amateur Mechanics is the name of a new illustrated monthly Magazine, conducted by Mr. P. N. Hasluck, and published by Trübner and Co.

We have received from the U.S. Naval Observatory the results of the observations made to determine the longitude of the observatory of the J. C. Green School of Science, Princeton, N.J. The final result is that the latter is 0h. 9m. 34s.·538 east of the central dome of the observatory.

The earthquake in Panama on November 7 was followed by a violent shock on November 13 at 2.30 a.m. It was observed also at Taboga and Colon. It is remarkable that all the Central American earthquakes since August last have occurred between midnight and daybreak. Their general direction was invariably from north to south, and it is supposed that they proceeded from one and the same cause. The West Indian cable broke, at a point about thirty miles from land, during a violent shock. The centre of the disturbance seems to lie near the West Indian Isles. During the second week of December seven shocks were felt in the Spanish province of Almeria. On December 8 at 10.1 p.m. a fearful shock lasting four seconds was felt at Tecuci (Roumania). Its direction was from south-east to north-west. Another earthquake is reported from Hermagor (Carinthia). It occurred on December 10 at 2 a.m., and was preceded by a terrible thunderstorm.

An "Illustrirte Bienenzeitung," organ for the propagation of rational apiculture, will be edited by Prof. Adolphson of Zürich. beginning on the 1st inst.

In the Pelion district a moderately violent earthquake occurred on December 11, but no damage was done. Upon the island of Santorin new volcanic activity has recently been noticed; also in the subterranean volcano which formed near Missolunghi.

The additions to the Zoological Society's Gardens during the past week include a Himalayan Bear (*Ursus tibetanus*) from Burmah, presented by Capt. Connor; two Bronze Fruit Pigeons (*Carpophaga ænea*) from India, presented by Mrs. A. H, Jamrach; four Barred-shouldered Doves (*Geopelia humeralis*) from Australia, presented by Mr. Ernest L. Bentley; a Lesser Sulphur-crested Cockatoo (*Cacatua sulphurea*) from Moluccas, presented by Mr. K. Digby; a Gannet (*Sula bassana*), British, presented by Mr. Thomas Keen; a Cape Bucephalus (*Bucephalus capensis*) from South Africa, presented by Mr. H. Pillans; a White-fronted Lemur (*Lemur albifrons* ♀) from Madagascar, four Wood Thrushes (*Turdus mustelinus*), a Golden-winged Woodpecker (*Colaptes auratus*) from North America, two Cirl Buntings (*Emberiza cirlus*), two Crested Grebes (*Podiceps cristatus*), a Razorbill (*Alca torda*), a Bar-tailed Godwit (*Limosa lapponica*), a Red-throated Diver (*Colymbus septentrionalis*), British, purchased.

OUR ASTRONOMICAL COLUMN

The Total Solar Eclipse on May 6.—The right ascensions and declinations of the moon for 1883, both in the *Nautical Almanac* and the *American Ephemeris*, depend upon Hansen's Tables, with the recent corrections of Prof. Newcomb. They furnish as accurate positions as are obtainable from existing tabular data, and it will be of interest to trace their bearing upon the circumstances of the total eclipse of the sun which crosses the Pacific on May 6. On laying down the belt of totality upon the Admiralty chart of this ocean, it appears that the following islands are included within it, viz.:—Rance, Buffon, Beveridge, Flint, Caroline, and Chanel Island (in the Marquesas); the positions read off from the general chart or for Flint, Caroline, and Chanel Island, from the enlarged Admiralty charts are as follow:—

		° ′		° ′
Rance Island,	Long.	176 22 West.	Lat.	24 20 South.
Buffon ,,	,,	170 0 ,,	,,	20 39 ,,
Beveridge,,	,,	167 50 ,,	,,	20 0 ,,
Flint ,,	,,	151 50 ,,	,,	11 25 ,,
Caroline ,,	,,	150 6 ,,	,,	9 54 ,,
Chanel ,,	,,	140 31 ,,	,,	7 55 ,,

From direct calculation for each of these points the following local mean times of beginning of totality, the duration of the same, and the sun's approximate altitude at the time, result:—

	Totality begins May 6. h. m. s.	Duration. m. s.	Sun's Altitude. °
Rance Island,	8 47 36 a.m.	3 27	29
Buffon ,,	9 22 18 ,,	4 20	38
Beveridge,,	9 34 48 ,,	4 1	41
Flint ,,	11 19 43 ,,	5 26	61
Caroline ,,	11 33 4 ,,	5 7	63
Chanel ,,	0 43 32 p.m.	1 47	63

It should be mentioned that the semi-diameter of the sun has been taken from the *Nautical Almanac*; that of the moon was obtained from her horizontal parallax, using the factor 0·2725. The duration of totality at Sohag in Egypt in the eclipse of last May was exactly given by this arrangement.

The Minor Planets.—The part of the *Berliner Astronomisches Jahrbuch* for 1885, containing ephemerides of the minor planets for 1883, has been issued to the various observatories in advance of the publication of the annual volume. It contains approximate places for every twentieth day of 224 of these bodies, the latest being No. 225, with accurately calculated opposition ephemerides of 43, each extending over about five weeks; this division of the *Jahrbuch* occupies upwards of one hundred pages.

There are six cases during the year where the planets approach the earth about opposition, within her mean distance from the sun. On June 22 *Phocea* is at a distance of 0·93, declination + 16°; on July 12 the distance of *Clio* is 0·96, declination $-35\frac{1}{2}°$; on August 1 that of *Isis* is 0·90, declination − 28°; on October 1 that of *Polyhymnia* is 0·98, declination $+8\frac{1}{2}°$; on October 20 that of *Virginia* is 0·98, declination + 13°, and on December 11 *Flora* in perigee is at a distance of 0·97, with declination + 18°. Galle's method of determining the solar parallax, so strongly advocated and ably applied by Mr. Gill, is not likely to fail for want of opportunities of applying it. As regards the magnitude near opposition we have in the case of *Phocea* 9·0; *Clio*, 10·2; *Isis*, 8·8; *Polyhymnia*, 9·7; *Virginia*, 9·9; and *Flora*, 8·2.

During the year 1883 four of these planets descend below 14m., from coming into opposition not far from aphelion.

Comet 1882 *c*.—Mr. Gill has secured five complete observations of this comet (discovered by Mr. Barnard in September) on the meridian S.P., with the transit-circle at the Cape of Good Hope, between November 11 and 30, so that places for upwards of a fortnight after the perihelion passage will be available for calculation.

THE EDUCATION OF OUR INDUSTRIAL CLASSES[1]

It is, I believe, according to precedent, now that another year's work of the Science Classes here has been crowned by the award of prizes, that I should address you on some topic allied to the matters which have brought us together to-night. I need not search long for a subject, for the scientific education of those engaged in our national industries—upon the success or failure of which, in the struggle for existence, the welfare of our country so largely depends—is now one of the questions of the day. I propose, therefore, to lay before you some facts and figures bearing upon the education of our industrial classes, and I shall attempt to make what I have to say on that special point clearer, by touching upon some preliminary matters, which will show how it is that such a question as this has not been settled long ago; and further, that we can, if we wish, settle it now in

[1] An address delivered in presenting the prizes at the Coventry Science Classes, by J. Norman Lockyer, F.R.S.

the full light of the experience gained elsewhere, instead of wasting let us say a quarter of a century in costly experiments which may perhaps leave us in confusion more confounded. To begin, then, why is this question being discussed now? There is a great fact embodied in the most concrete fashion in the way in which our Government is now compelled to deal with our national education. Side by side of the Education Department by which our Minister controls in the main that book learning which has been given time out of mind, there has sprung up during the last thirty years another department—the Science and Art Department—by which he controls a new kind of national learning altogether. We have added to the old study of books a new study of things. This new learning was, we may say, only introduced in 1852, in which year the Queen in her speech on opening Parliament said: "The advancement of the fine arts and of practical science will be readily recognised by you as worthy the attention of a great and enlightened nation." We have since found out that they are indeed worthy the attention of a great nation, and more than this, that no nation can be called enlightened whose citizens are not skilled in both; in fact, that they are to peace what cannon and swords are to war. But for a nation to foster them is one thing, to include them in a national scheme of education is another. Ought they to be so included? Let us see. What do we mean by education? Roughly speaking, we may say that there are two distinct schools of thought on this subject, although the existence of these two schools is not so generally recognised as it should be. According to one view, the human mind is an elastic bag into which facts are to be crammed for future use. A variation of the view is that the mind is inelastic, and then the stuffing process becomes more serious, and instead of depending upon a natural expansion, a process like that in use by the manufacturers of soda-water is employed. It is not to be wondered at that the youthful mind likes neither of these methods; what ought to be a true delight becomes a real agony, and hence it is, as a Warwickshire man wrote many years ago—

> "Love goes toward love
> As schoolboys from their books;
> But love from love
> Toward school with heavy looks."

—The mind on this view resembles a store where, as our American cousins say, everything, from a frying-pan to a frigate, which shall be useful to the owner in after life, is to be found. Hence such terms as Grammar School, Trade School, Science School, Commercial Academy, and hence I am sorry to say, systems of examination which too often only serve to show what a boy can remember, and little care about either what a boy can do, or whether he can think. So much for one view. Now for the other. It is more difficult to image it, but in the absence of a better illustration, the mind may be likened to the body—a thing to be trained so that its grace, its freedom, its strength, its grasp, indeed all its powers in all directions and in all ways may be brought out by proper training. If the training is one-sided its power cannot be many-sided, but it is most useful when many-sided. Therefore, as each muscle of the body has to be properly trained to make a perfect man, so must the educational system brought into play be such as to train to its uttermost and bring out each quality of the mind. Each faculty of it when called into play becomes as a two-edged sword in the arms of a strong man. In this, or some such way, then, may we picture to ourselves the difference between instruction in its real sense, and education in its real sense. Now, which of these systems is the better one? We shall see at once that the first may give us a mind stored with facts covering a large or a small area; it may be book keeping, or it may be Latin, or anything else. But will the mind be able to use this store in all cases? We grant *knowledge*, but may not *wisdom* linger? Those of us who have got to Voltaire's second stage, and who have studied men, know that this too often happens, and that much knowledge does not prevent the owner from being absolutely unfitted to grapple with the problems which each rising sun brings to him for solution. The other system, on the other hand, if the training is not thoroughly all-round, may give us a man who finds that the questions presented to him on his entrance to active life are precisely those which require the application of that quality of mind, whichever it may be, which was least trained at school. He may find himself face to face with problems of the existence of which he never dreamed, and so far removed from his experience that his mind, however powerful in some directions, fails to grapple with them. We seem, then, on the horns of a dilemma. Instruction may provide us with a store of facts, which the mind does not know how to use. Education may provide us with a mind which has been trained in a world utterly different from the real one. How can we escape from this dilemma. *We must use the materials of that instruction which is most useful to us in our progress through life as a basis for the complete education of the mind.* Which instruction is the most useful to us? The poet tells us, that "the proper study of mankind is man"; but when we come to prose and read the views of those who best know the needs of modern society, and especially industrial society, we read something like this which I quote from the report on elementary and middle class instruction, published by the Royal Commission of the Netherlands: "The idea of *Industrial Society* not limited to agriculture, manufactures, and trade or commerce, but understood in its widest signification, points plainly to the acquiring of the knowledge of the present world, and to its application to economical and technical pursuits." Now, here is a subject on which a volume might be written, but I shall only point out to you the obviousness of the importance of the study, not merely of ourselves, *or* of the world around us, but of ourselves, *and* of the world around us. This lands us in the necessity of training our minds in literature or humanities, and science and art—the study of the humanities enables us to know the best thoughts, and the most stable conclusions on vital questions, arrived at by our forerunners and those who are fighting the same battles in other lands. The study of science enables us, on the other hand, to get a true idea of the beautiful universe around us, of our real work in the world, and of the best manner in which we can do that work in closest harmony with the laws of Nature. Did we study the external world alone we should not profit by the experience of those that preceded us. Did we study humanities alone we should be shorn of half our natural strength in face of many of the problems placed before us by the conditions of modern life; and, more than this, all the glories of the beautiful world on which our lot is cast, and the majesty of the universe of which that world forms part would hardly exist for us, or give rise only to dumb wonder. Here let me tell you a little story. Three years ago when travelling in America, one morning, at a little station—we were approaching the Rocky Mountains—I was astonished to see a very old and venerable French curé in his usual garb enter the car, and as he was evidently in some distress of mind, and as evidently had little command of English, I asked him in his native language if I could be of any service to him. There was a difficulty about a box which I soon settled, and then we sat down and entered into conversation. He soon found out that I was very astonished to see him there and told me so. I acknowledged it. "It is very simple," he said, "I am very old, and six months ago I was like to die and I was doing my best to prepare myself for the long journey. In my fancies I imagined myself already in the presence of *le bon Dieu*, and I fancied this question addressed to me, 'M. le curé, how did you like the beautiful world you have left'? I rose in my bed as this thought came into my head for I—I who—figure to yourself—had dared to preach of a better world for fifty years, was, oh! so ignorant of this. And I registered a vow that if *le bon Dieu* allowed me to rise from that bed of sickness I would spend the rest of my life in admiring his works—*et me voici!* I am only on my journey round the world; I am going now to stop at the Yosemite Valley a few days *en route* for San Francisco and Japan, and the box, Monsieur, which your kindness has rescued for me contains a little scientific library, now my constant companion in my delicious wanderings." Our general scheme of education, therefore, unless it is to be one-sided, must combine science with the humanities. But, so far, I have said nothing about art. Now, from the educational point of view, science and art are very closely connected, inasmuch as in the early stages of both studies the student's powers of observation are brought out and trained in the most perfect way, while in the later stages, to succeed in either, he must have learned that very important thing—how to use his hands—and at whatever age you put it that a boy or a girl should use the hand neatly and skilfully, before that age you should take care that some elementary grounding at all events, in the only training which can do this, shall have been given. No amount of Greek, or of useful or of useless geography, or even of rule of three, can prevent the fingers being all thumbs, unless some such training has been given, and for the very earliest training drawing is undoubtedly the best. But this is by no means the only advantage of the combination. Anyone who has to go over thousands of examination papers finds in nineteen cases out of twenty that an orderly drawing or diagram is generally associated with an orderly mind. In fact, a diagram may be regarded as an index of the amount and accuracy of the knowledge possessed by the student. The text of the student who fails in the diagram is generally a more awkward jumble than the diagram itself. Hence the facts show that this training of the hand is accompanied by much good mental results. This is now so generally recognised, that in a not distant period, no professor of biology, for instance, will attempt to demonstrate practically microscopic structure to students who have had no preliminary training in drawing. This is one example out of many which might be given, for as natural science is the study of nature, and as we can only study her by phenomena, the eye, and the hand, and the mind, must work together to achieve success, and he who attempts to describe the geology of a district, the minute structure of a frog's foot, an eclipse of the sun, or the rings of Saturn, in words, and words only, has only done half his work; to complete it he must appeal to art for aid. Now, many of you may be prepared to concede, without any further insistance on my part, that an elementary acquaintance with art is of great, nay, of even essential importance, not only for its own sake, but because of its aid in natural studies. We must then add art to science and literature in order to form a complete curriculum. Here pardon me one moment's digression from the direct line of my argument. Many will agree that science is aided by art who deny that art is aided by science to the same extent. Indeed, some are prepared to urge that one who proposes to devote himself to art can derive no possible benefit from the study of science. Let us inquire into this a little. If we wish to excel in the art of figure-painting, we must know anatomy, a most important branch of science; and as a matter of fact, many artists study anatomy as minutely as many surgeons do; and in the old days, when the artist and the poet were more saturated with the knowledge of the time than they are now, we find the great Leonardo at once professor of anatomy and founder of a school of painting as yet unsurpassed. If we pass from the figure to ornamental design, or if we wish to show objects in perspective, is not every line, whether straight or curved, dominated by an appeal to geometry? Again, suppose we take landscape. Here we meet with phenomena of colour as much regulated by law as are the phenomena of form, and an anatomy of colour is fast being formulated, which to the artist of the future will be as precious as the anatomy of form has been in the past, and will ever continue to be. Let us take, for instance, an artist who wishes to paint a sunset, one of the most magnificent sights which it is given to man to witness. The sky is covered with clouds here and there, and not only do the colours of the clouds vary, almost from moment to moment, but in all cases they present the strongest contrast to the colour of the sky itself. The artist is bewildered, and finds each effect that he would seize to be so transient that at last he gives up in despair the attempt to note down the various tints. But the possession of a knowledge of the part played by the lower strata of our atmosphere in absorbing now one and now another of the components of the light of the setting sun, would change this despair into a joy almost beyond expression. For the bewildering changes of colour are then discovered to be bound together by a law as beautiful as the effects themselves. There is another point of view. One is frequently pained in seeing in an otherwise noble work of art, evidences that the artist was crassly ignorant of the phenomena he attempted to represent, and in his attempts to transcend nature had only succeeded in caricaturing her, painting, for example, a rainbow in perspective, or a moon with its dark side turned towards the setting sun. Yet these are almost trifles, and, in fact, here we have the excuse of the ignorant artist—now, I am thankful to say, the representative of a class that is fast disappearing—for his defence is, that he has nothing to do with such small matters, and that accuracy of this kind may quite properly be sacrificed to secure the balance of his picture. Now, to return to the main drift of my address, we have seen that in any complete system of education neither science nor art must be neglected by the side of the old humanities—the old more purely literary studies; and it is indeed fortunate for us that we live in an age in which the laws and the phenomena of the external world have been studied and formulated with such diligence and success that it is as easy now to teach science, in the best possible way, as it is to teach classics in the best possible way. It is half a century since the Germans found out the importance of the new studies from a national point of view. We are now finding it out for ourselves, and finding it out not a moment too soon, and it is not needful for me to tell you that the transformation which is going on is acknowledged to be one of the highest national importance. It is no longer an abstract question of a method of education; it is a question of the life or death of many of our national industries, for, in a struggle for existence, how can a man who wins his bread by the application of national laws to some branch of industry, if he be ignorant of those laws, compete with the man who is acquainted with them? If for man we read nation, you see our present position. How far then have we got with our transformation, limiting our inquiry to primary and secondary instruction? First, as to elementary education. The idea of the education—the compulsory education, if necessary, of all the citizens in a state—dates from the time of Luther. It is a horrible thing that we should have had to wait three and a half centuries since his time for such a measure, which is an act of simple justice to each child that is brought into the world. In 1524 Luther addressed a letter to the Councils of all the towns in Germany begging them to vote money, not merely for roads, dykes, guns, and the like, but for schoolmasters, so that the poor children might be taught, on the ground that if it be the duty of a State to compel its able-bodied citizens to take up arms to defend the fatherland, it is *a fortiori* its duty to compel them to send their children to school, and to provide schools for those who, without such aid, would remain uninstructed. Thanks to our present system, now about ten years old, out of an estimated population of 8,000,000 children between the ages of two and fifteen, we had last year nearly four millions at school, and out of an estimated population of 4,700,000 between five and thirteen, we had 3,300,000 at school. Among this school population elementary science is at last to be made a class subject, and we find mechanics, mathematics, animal physiology, and botany among the specific subjects in addition to the three R's. 120,000 children received education in these specific subjects last year, and if we are justified in assuming that as many will learn science when it becomes a class subject as now already learn drawing, we may expect in a year or two to have this 120,000 swelled into three-quarters of a million. I must again insist upon the fact that practical teaching in science is the only thing that can be tolerated. Of course, with a new subject the great difficulty is the difficulty of the teacher. Any system, therefore, of economising teaching power is of the highest importance. I am glad to know that a system suggested by Col. Donnelly, which uses the utmost economy of teaching power, has been carried into admirable practical effect at Birmingham, and I believe also at Liverpool, and other large towns. So that in the most important centres we may be certain that science will be taught in the best manner. It is worth whie to dwell on this system for a moment. Under it practical teaching is given to boys and girls of the fifth and higher standards, and also to the pupil teachers. The subject chosen for the boys is mechanics, that for the girls domestic economy, giving each of these subjects a wide range of meaning. There is a central laboratory in which the experiments are prepared, and from which the apparatus ready for use is conveyed in a light hand-cart to the various schools—twenty-six in number in Birmingham—belonging to the Board. In this way it is possible to give twenty lessons a week, and the circuit of the schools can be made in a fortnight. In the intervals between the visits of the demonstrator the class teachers recapitulate his lessons and give the children written examinations. About 1200 children are now being instructed in this way. To make the instruction as real as possible, children are brought out to aid in performing the experiments, objects are passed round, and questioning at the end of the lecture is encouraged. In the education, then, of our children, from the ages of five to thirteen, we may reasonably expect to find that science teaching will in the future be carefully looked after. We now come to the secondary education. Here, again, great progress has been made during the last few years. The real difficulties against its introduction have been the overcrowded state of the old curriculum, the scarcity of teachers, the want of sympathy with [illegible] and the ignorance of its importance on the part of some h[illegible]masters. But to those headmasters who held the view that no real training could be got out of a subject which boys studied without positive pleasure, parents began to reply that whether the boy liked it or not he must get that knowledge somewhere. But where the experiment was really tried under good conditions it was soon found not only that the boys were willing to give three or four hours a week of their playtime to scientific subjects, but that the one or two hours filched from the curriculum were more than made up for by the greater ease with which the other subjects could be learnt, in consequence of the additional training of the mind which the new subjects gave. We may hope, then, that in the course of time our secondary education may be much improved in the direction indicated. What we may expect, taking the principle of natural selection as our guide will be this. First, the head-masters will themselves be men chosen among other grounds for their knowledge of science, they will become more and more all round men. Next, the curriculum will be arranged not for the few who go to the University, but for the many who do not. We shall have more science and less Greek in the early years of the school course. We shall have laboratories, and drawing rooms, and workshops. In some schools we may find modern living languages taught in a living way replacing the dead languages altogether. Now, here our difficulties begin. We are face to face indeed with the same difficulties which the Continental nations, our precursors in educational matters, have experienced. Our secondary education is at the present moment all but absolutely separated from the primary one. Of the 4,000,000 scholars on the books of elementary schools last year there were only 44,000 over the age of fourteen, and it is to be feared that the remainder left school at that age, most of them, the best as well as the worst of them, to fight the battle of life with such an education as they had got up to that time. Germany, again, was the first to find out that this would never do, even though in that country science and art was taught in the Primary School. And for the reason that though such a meagre education might possibly do for ordinary workers in their hives of industry, it was totally insufficient for the future foremen, overseers, and the like, and special schools were established to carry their education further. Quite of late years this question has been studied in the most interesting way in the Netherlands, under the advice of a wise minister, whose example will be followed some day in our own country. Let me briefly refer to it. This work began in 1863. In that year in Holland there were no middle class or secondary schools for artisans, but there were evening schools for drawing which dated from 1827. "Burgher Schools" were established to provide the secondary instruction still felt to be needed by those who otherwise would have to content themselves with the primary instruction (although in its more extended form it contained natural philosophy, mathematics, and modern languages). In these schools—some day, some night schools (in these the lessons went on from September to May), with a course of two or three years, we find mathematics, theoretical and applied mechanics, and mechanism, physics, chemistry, natural history, either technology or agriculture, drawing, gymnastics, and other subjects among the fixed subjects, modelling and foreign languages being permissive. These burgher schools were compulsory in all parishes of 10,000 inhabitants. The evening burgher schools especially were at once seized on with avidity, chiefly by apprentices and the like. Here let me give you some statistics which will show you how these schools were working even ten years ago. They are much more flourishing now, but I have not the figures. I will show how the Dutch (of whom it cannot be said, to vary an old rhyme,

> In matters of *learning* the fault of the Dutch,
> Is giving too little and asking too much.

for the instruction is practically free), who are already learning a trade or working at one, use the evening hours for the further cultivation of their minds.

	Population.	Number of students in Burgher Schools.
Delft	23,000 ...	171
Utrecht	64,000 ...	283
Deventer	81,000 ...	285
Dordrecht	26,000 ...	146

Among the students at these schools in 1874 were 1582 carpenters and joiners, 472 smiths, &c., 236 plumbers and masons, 170 goldsmiths, engravers, &c., 320 painters, to give examples. Higher burg[illegible] schools were also established in the chief towns. In these scho[illegible] still more advanced instruction was given: and here the course was for five years. In all these schools there was a considerable state endowment, and an endowment on the part of the town, so that the fees were almost nominal, and in some cases even the instruction was gratuitous. When I was inspecting these schools in Holland with an eminent man of science, whose advice had helped largely to make them such a success, and when I expressed to him my astonishment at the smallness of the fees—only a very few shillings a year—he put before me the question of State aid to schools in a way which had never struck me before. He said: "We regard it as a sort of education insurance. A small tax is paid by everybody during the whole of his life, and in this way a man who brings up children for the service of the State is helped by him who shirks that responsibility; and the payment which each citizen is called upon to make towards this instruction is spread over his whole life, and does not come upon him when he is probably most pinched in other ways. Now for one practical result of the establishment of these schools. The year 1863 found Holland full of the notion that every hour a child spent away from the desk or the bench after thirteen was time wasted; but after these burgher schools were instituted a change came over the spirit of that dream, and now no employer of labour except of the lowest and most manual kind in Holland, will look at a boy who cannot produce a certificate from his burgher school. Another very remarkable thing was soon observed, with a most important moral for us. The great difference between their burgher schools and the old gymnasia, the equivalents of our grammar schools, was a greater infusion of science into the teaching, and the introduction of three modern languages in addition to Dutch, Latin and Greek being omitted altogether from the curriculum. After four years of this training, many of the boys showed such high promise that all connected with them thought it a pity that they should not enter a university. They were therefore allowed six months as an experiment to take up Latin and Greek, and the result was that in a great number of cases they beat the gymnasia boys in their own subjects, and passed with flying colours. The Real Schul in Germany and the modern sides of our own secondary schools are almost the exact equivalents of the higher burgher schools to which I have especially called your attention. What, then, is the experience which has been gained in these gigantic educational experiments, experiments by which we may profit, as we are so late in the race, if we care to do so. One point is that if a chance is put before those who have passed through the elementary schools of further culturing their minds, they seize upon it with avidity. Another is that the employers of labour appreciate the value of the greater intelligence thus brought about. It is better to have to instruct in a trade men who have shown themselves anxious to learn, than to have to do with blockheads. Another, I think, is this: Your best secondary school is best for everybody; a secondary school with a properly mixed curriculum of literature, science, and art, is best for him who proceeds either to the University or to the workshop. A second-rate education in a second-rate school, gives us a second rate man, and we do not want our national industries to be worked entirely by second-rate men. On this point I am glad to fortify what I have said by a reference to Dr. Siemens' important address at the Midland Institute the week before last. He says: "It is a significant fact that while the thirty universities of Germany (you see they do not educate by halves in Germany; they have seven times as many universities as we have in England) continued to increase, both as regards number of students and high state of efficiency; the purely technical colleges, almost without exception, have during the last ten years been steadily receding, whereas the provincial Gewerbe Schuls have, under the progressive minister, von Falke, been modified so as to approximate curriculum to that of the gymnasium or grammar school. "As regards middle-class education, it must be borne in mind that at the age of sixteen, the lad is expected to enter upon practical life, and it has been held that under these circumstances at any rate it is best to confine the teaching to as many subjects only as can be followed up to a point of efficiency and have reference to future application. It is thus that the distinction between the German gymnasium or grammar school and the real Schule or technical school has arisen, a distinction which, though sanctioned to some extent in this country, also by the institution of the modern side, I should much like to see abolished." We see then the gradually increasing weight of opinion, and the result of the experiments both in Germany and Holland, and I may add France, point to these conclusions. Some kind of secondary education must be provided for the best students when they leave the elementary school, either before they begin work or while they are at work. Our secondary education should go practically along one line, how far soever the student goes along that line, some, of course, will go further than others; *provided always that our secondary education is the best possible, that is, having the broadest base.* Now, if this be generally conceded our problem in England, at the present moment, is simpler than we thought it. We are face to face with the fact that it is for the good of the nation that those who have passed most successfully through the elementary education must continue that education in a secondary school, whether for two, or for three, or for six years, matters little for the argument. Are we then to build technical schools for such students? Thirty years ago the answer would have been yes. To-day we may say firmly, no. If a town has a grammar-school, let the town see that the curriculum of that school is based upon our best secondary models. If the town has no such school, then let it build one. If one school is not sufficient, then build two. That town will be the best off in the long run which gives the greatest number of free exhibitions from the elementary schools into such a school as this, and that town will be the wisest which holds out such inducements at the earliest possible moment. I have lately read with much interest a copy of resolutions and suggestions, passed at a meeting of an Association of Elementary Teachers in the north of England. From these we may gather that this question is already one of practical politics. It is agreed that the secondary education of the best boys leaving the elementary schools must also on. It is also taken for granted that the question lies between building a technical school or utilising the grammar school. One argument used in favour of the latter cause is, that the grammar school will be strengthened by drawing to itself the best boys from the elementary schools. The present proposals are that a number of free scholarships should be competed for annually, that these free scholarships shall, if need be, be supplemented by exhibitions from the fund at the disposal of the Governors (I should not accept this at once. Why should not the town pay them?), and the length of time for which these scholarships shall be tenable is not to be less than three years. You see, then, that in the north of England, at all events, it is conceded that the best children in our elementary schools should have a three years' course in a school of higher grade in which, of course, all the class subjects in the Elementary Code will be expanded, and all the linguistic studies of the grammar school taken in hand. When this system is at work, as it is bound to be in a few years, two things will happen, and it is as well we should be prepared for them. In the first place, our secondary schools—all of one model, the best model, be it understood—must so arrange its curriculum, that the students can leave after a three years' course, if need be, for the workshop or the office, or after a longer course for the University. That is the first point. The second one is this. The present system of apprenticeship will be called in question. A boy who has been educated to the age of sixteen will learn very much more in three or four years, and will be very much more valuable to his master during that time than he who was formerly bound apprentice at the age of thirteen or fourteen, with his fingers all thumbs, and no mind to speak of. It seems to me as it does to a daily increasing number, that the present mode of dealing with those matters which were formerly regarded as arts and mysteries known only to a few, and carried on on a small scale under the eye of the master, is dead against the system of apprenticeship as it has come down to us. Now the master does not teach, and the boy in nine cases out of ten has no opportunity of grasping the whole of the art or mystery at all. Many of you will begin to think that you are listening to the play of Hamlet with the part of the Prince of Denmark omitted, for so far I have said nothing whatever about technical education. I have said nothing about it for the reason that I believe the less said to a boy about technical education before he is sixteen years old the better. I now proceed to discuss this question, which is far more important, far more a national question, than you would gather from the debates in Parliament. What is technical education? It is the application of the principles of science to the industrial arts. And the rock ahead against which I am anxious to join Dr. Siemens in warning you is this: Under the influence of the present scare—for it is a scare, and a real one—there is a chance that attempts may be made to teach the applications to those who are ignorant of principles, whereas we have to fight those who study applications with a full knowledge of the principles which underlie them. We may congratulate ourselves on the fact that when we have once made up our minds as to the right place of technical instruction in our scheme of education, we have much of the necessary machinery already at our disposal; and the recent action of the City Guilds and of the Government is enormously increasing the quantity and improving the quality of this machinery. Let us first consider the classes now formed all over the country under the auspices of the Science and Art Department. Their development in the last thirty years has been something truly marvellous. When the Queen, in 1852, opened Parliament, there were already 35,000 students of art, but practically no students of science, in this country, amongst the industrial classes. That 35,000 will, if the present progress goes on, give us nearly 1,000,000 students of art at the end of this year; while the science schools have increased from 82 in 1860 to 1400 in 1880, with 69,000 students. The system which has thus developed so enormously has dealt chiefly with pure science, but for the future we shall have side by side with it, and built upon the same lines, a system of teaching the applications of this pure science to each of our national industries. He who wishes in the future to have to do in any way with the manufacture of alkali, gas, iron, paper, or glass, to take instances, or in the dyeing of a piece of silk, or the making of a watch, to take others, will find the teaching brought to his door, and obtainable almost for the asking. Here, again, we may congratulate ourselves, for while those who know most about the subject tell us that the more ambitious attempts at technical instruction in Germany and elsewhere have failed, because the teaching is not in sufficiently close contact with the works in which the processes are actually carried on, the system to which I have drawn your attention will enable the instruction to be given at night to those who have already begun practical work during the day. We have, then, come to this: that putting together what is most desirable in the abstract, and what has been practically proved to be the best, the education of our industrial classes should be, and can easily be, something like this. The boy will go to an elementary school till he is thirteen. He will then pass with an exhibition, if necessary, to a secondary school till he is sixteen. He will there go on with his science—now a class subject in the elementary school—and begin the study of languages. At sixteen he will leave school and begin the battle of life, and can still in the evening proceed further with his studies in pure science, if the secondary education has left him too ill-equipped in that direction. Having thus got the principles of pure science into his mind he will be able to take up the technical instruction in the particular industrial art to which he is devoting himself. But be the number of our future foremen and managers who who have had this extra three years of secondary instruction, large or small, if there be in Coventry let us say out of your population of 45,000, one thousand boys, or girls, or men, who are anxious not only to learn science, but its application to their particular industries, then the Government is ready to endow Coventry with a sum varying from two thousand to six thousand pounds a year, according to the results of the examinations, if two subjects of pure science are taken up, and the students pass. The City Guilds are prepared to endow the town with from 1000*l.* to 2000*l.* a year additional, provided some application of the principles of science to the industrial arts is taken up, and evidence forthcoming that the principles themselves have been studied. Now if among your 45,000 there is not 1000 who care for these things which are vital to your trades, seeing that abroad these things are cared for, how can your trades stand against foreign competition? Let such a system as this go on for twenty years, and we shall hear nothing more of the decay of our national industries. Now here I am bound to point out a distinct gap in the present system. We have classes for art, classes for pure science, classes for applied science, but where are the classes for languages? The modern languages are taught so badly in our secondary schools, that it is hopeless to expect that sufficient knowledge, either of French or German can be acquired in the three years' course to enable the student to find out what his French and German rivals are doing in the branch of industry which he takes up; and we must, moreover, consider those who may wake up to the importance of studying science and its technical applications after the chance of a secondary education is lost. Such classes then are a real want. But I will not end my address by a reference to what I regard as an unfortunate gap, but would rather conclude what I have to say by pointing out that the scheme I have sketched out need be no Utopia, so far, at all events, as a supply of well-trained teachers is concerned. This, up to the present time, has been the real difficulty. But now that the authorities at South Kensington have started summer courses of lectures to teachers, and that they actually pay the teachers for going to learn, the methods of teaching, both in the elementary and secondary schools, and evening classes, cannot fail to improve. Quite recently, too, we have seen the inauguration of a Normal

School, where Royal Exhibitioners and other free students are admitted without payment; where the teacher has the first claim, and where he can attend any single course for a nominal fee. Now every town of importance in the country should associate itself with the Government in this attempt, and should have one, at least, of its citizens always in training there, so that the scientific instruction in that town, whether primary, secondary, or tertiary, should always be at its highest level. On the other side of the road, too, at South Kensington, is rapidly rising another institution where we may hope the teachers of our technical instruction will receive an equally careful training. So that you see, to bring what I have to say to a conclusion, that though we are late in the day, though many people have not yet made up their minds as to what is best to be done—and I acknowledge that the question is hedged in with difficulties on all sides—there is an easy solution of the difficulty based on the experience of other countries, which is at the same time an act of simple justice; that this solution requires no dislocation if we adopt it, but simply a natural growth of our existing means, and that all the newest developments of our educational machinery will all fall naturally into place.

THE TRANSIT OF VENUS [1]

The Observations at the Cape

THE long looked-forward-to transit of Venus occurred yesterday afternoon, causing, we may be sure, a flutter of excitement amongst astronomers throughout the whole of the world. To some the special duty was entrusted of carefully noting everything connected with the ingress of this familiar planet, and after they had concluded their labours at the setting of the sun, it fell to astronomers in other portions of the globe to pay equally minute attention to the planet's egress. By and bye we may expect columns of thoughtfully worked-out details in connection with this peculiar and interesting astronomical event, all of which will tend to still further solve the problem of the exact distance of the sun from the earth. We need not remind our readers that herein consists the whole scientific value of the transit. When crossing the sun's disc the planet is at its nearest distance from the earth—estimated at about 25,000,000 miles—and through the peculiar facilities thus afforded of directly measuring its parallax, observers are enabled to calculate the parallax of the sun, which to astronomers is a matter of very considerable importance. The credit of the suggestion of this particular method of calculation is due to Dr. Halley, and it is still popularly held to be the best for the purpose. But accompanying the rapid strides astronomic science has taken in its development since the days of Halley, instrumental means have been invented and accepted by modern astronomers, which appear to afford methods, perhaps even more exact, of arriving at the desired result. For all this, however, the transit of Venus retains a powerful hold upon the popular mind, and, indeed, upon the minds of many astronomers, as the best method. There is, too, one specially strong argument why a particular interest should be taken in this planet's transit. No one who witnessed the phenomenon yesterday will live to see it again—unless, indeed, he fairly outrivals old Parr and other gentlemen famed for longevity. Occurring as these transits do at the unequal but regular recurring intervals of 8, 122, 8, and 105 years, no one could well expect to see more than two in a lifetime. The last took place in 1874, while the next will occur in December, 2007. It need, therefore, be no longer surprising why, both popularly and scientifically, the event is regarded as one of such special interest, and why the most eminent scientific observers are selected to note everything that takes place.

Before proceeding to refer to the observations which were taken yesterday at the Royal Observatory we may mention that, acting under the advice of the Astronomer-Royal of the Cape of Good Hope (Dr. Gill), the British Transit of Venus Committee decided upon establishing stations at Aberdeen Road and Montagu Road as auxiliary places of observation to the principal station here at the Observatory itself. And before proceeding further it may be added that Natal has come forward very pluckily in this matter, exhibiting an amount of interest in astronomic science which does great credit to that colony. Mr. Escombe himself contributed a sum of between 400*l.* and 500*l.* for the purpose of providing a proper telescope; while two merchants subscribed 50*l.* each, the Corporation of Durban giving 300*l.*, and the Natal Government voting 500*l.* towards founding an observatory for the colony, and the defraying of expenses connected with taking observations of the present transit. As a pleasant sequel to this, we are glad to learn by telegraph, that Mr. Neison, who was in charge of the party of observation there, most successfully observed the internal contact at Durban, the enterprise of Natal thus meeting with a well-merited reward. As announced by us some time since, South Africa was selected by the Americans as a station for one of their photographic transit of Venus expeditions under the charge of Prof. Newcomb, who has the reputation of being one of the most celebrated of living astronomers. On arrival here Prof. Newcomb, after consultation with the best authorities as to atmospheric conditions, &c., finally decided, with the kind consent of the trustees of the Huguenot Seminary to take his observations from the foot of the gardens of that institution at Wellington. We hope to shortly hear of the entire success of the labours of the party, and perhaps to see some specimens of their photographic skill.

At the Observatory itself it need scarcely be said that for some weeks past great preparations had been made for the event. There are few living astronomers who have more carefully studied the subject of the transit of Venus than the present Astronomer-Royal here, Dr. Gill, and few are more thoroughly posted up in all the details of the rare occurrence. In 1874 Dr. Gill was Chief Astronomer to Lord Lindsay's Transit of Venus Expedition to the Mauritius, where he not only took most valuable observations, but evinced a very intimate acquaintance with the entire subject. It was only to be expected, therefore, that in this instance no detail in connection with the arrangements for a proper observation in Cape Colony would be lost sight of by the Astronomer-Royal. The few visitors who received invitations to the Observatory yesterday found Dr. Gill courteous and affable as ever, but wholly absorbed in the important work on hand. "You may go here and go there, look through that glass and have a peep through the other one," were his remarks just before commencing operations, "but whatever you do, please don't speak to me or any of the observers until the internal contact has been made." No injunction not to speak to the "man at the wheel" could have been more respected than this, and from that moment until a couple of hours later Dr. Gill and his assistants became objects of almost reverential awe to those outside the pale of strict astronomic science.

One of the principal instruments employed was a new equatorial telescope by Grubb of Dublin, made and sent out here specially for the transit of Venus, the old wind tower in which it is now mounted having been prepared as an observatory for its reception. There was also a heliometer which had been used at the last transit by Dr. Gill at the Mauritius, and was afterwards borrowed by him from Lord Lindsay for use on the Isle of Ascension, where he made a determination of the sun's distance from the planet Mars. Subsequently this fine instrument was purchased by Dr. Gill and was brought out here as his private property on his being appointed Astronomer-Royal at the Cape. Another noticeable instrument employed yesterday was the great theodolite intended for the trigonometrical survey of India. The designs of Col. Strange, however, from which it was constructed, were so long in being carried out in manufacture that General Walker, the Director of Survey, decided not to bring it into use, especially as it was somewhat too heavy for service in the field. Upon the application of Dr. Gill, it was lent by the Indian Government, for the purpose of some special researches in which that gentleman was engaged at the time, and it was successfully employed the other day in taking observations of the great comet. The other instruments included a small equatorial telescope of 3½ inches aperture, which was used by Mr. Stone on the occasion of the last transit of Venus; an equatorial telescope of 7 inches aperture, which has also been for some time at the Observatory, and a telescope of 2½ inches aperture belonging to Capt. Jurisch, examiner of diagrams in the Surveyor-General's department. Having mentioned the several instruments, we must go on to state by whom they were used. Dr. Gill himself observed the contact of Venus with the sun's limb, with the new 6-inch aperture equatorial, a similar observation being taken by Mr. Maclear with the 7-inch equatorial. Dr. Elkin, a scientific friend and guest of the Astronomer-Royal, took observations with the heliometer; Mr. Freeman, with the great theodolite; Mr. Pillans, with the small equatorial; and Capt. Jurisch with his own equatorial. Several important measures were also taken at the heliometer by Dr. Gill and Dr. Elkin.

[1] From the *Cape Times*, December 7, 1882.

With regard to the weather, which of course was a very important element, the sky was perfectly clear, and altogether suitable for the purpose of observation. There was a light south-east wind blowing, and this prevented the definition being so steady as might have been wished. We are "officially" assured, however, that on the whole the observations made at the Cape of Good Hope may be regarded as perfectly satisfactory, and that they will add considerably towards the solution of the problem of the sun's exact distance from the earth. We have already intimated that at the suggestion of Dr. Gill, other stations than that of the Observatory had been selected. At Aberdeen Road, Mr. Finlay (of comet fame), the first Assistant at the Cape Observatory, and Mr. Pette, third Assistant, were provided with an equatorial of six inches aperture, and the report received last evening by telegraph, was that complete success had attended their labours. Mr. Marth, the well-known astronomer, was detailed at Montagu Road in charge of one of the British Transit of Venus Expeditions, and was provided with a 6-inch aperture equatorial, his assistant, Mr. Stephen, formerly of the Observatory, and now of the Treasury Department, Cape Town, being provided with a 4½-inch equatorial. In his report last evening, Mr. Marth states that the sky was cloudless, but a heavy dust-storm prevailed during the day. He reported, however, that the important internal contact was observed satisfactorily both by himself and Mr. Stephen. A report from Capt. Skead, in conjunction with Mr. Spindler, of Port Elizabeth, states that they also obtained satisfactory observations.

We fear that the courtesy of the General Manager of Telegraphs, Mr. Sivewright, must have been sorely tested by the frequent demand upon his staff for signals for the purpose of determining longitudes, &c. The telegraphic department, we ought to state, has given the utmost facilities in connection with these operations, and thanks to the co-operation of the General Manager, everything connected with his department was accomplished without a hitch. The transit of Venus expedition will indeed be indebted to Mr. Sivewright for his energy and devotion in their interests. This additional work has necessarily fallen heavily upon the shoulders of the staff at the Observatory. Not only has the normal work of that establishment been carried on as diligently as heretofore, but there has been the additional task of taking observations of the great comet, which with other things has told severely upon the endurance of Dr. Gill and his assistants. Judging, though, from what we saw there yesterday, there is no sign of anyone breaking down under the strain of extra work.

The signals for time comparison were sent to the observers engaged in the transit about nine o'clock on Tuesday evening The night is described as having been beautifully clear, and the occultation of the bright star Spica Virginus was observed in the early morning. Signals were also sent to Mr. Eddie, Graham's Town.

We have thus far briefly sketched the manner in which the observations were taken yesterday—excepting the somewhat primitive method of smoked glass adopted by a good many of the general public, to whom the transit of Venus was not quite such a matter of exquisite nicety as to such gentlemen as those to whom we have just alluded. From a non-astronomic point of view there was even with the aid of the proper instruments, only to be seen a dark spot crossing the sun, resembling very much a Wimbledon bull's eye. Roughly speaking, the planet made its external contact at five minutes past three o'clock, when through a proper instrument it might have been seen minutely notching into the sun's edge. At twenty-five minutes past the hour—still roughly speaking, for when the calculations are worked out there might be a fractional part of a second one way or the other—the internal contact occurred.

The sun set long before the transit had been completed. It consequently fell to the lot of other astronomers to observe its egress, which of course was as eagerly watched for as had been that of the ingress. The ingress, it might be interesting to mention, was visible in North and South America; Europe, excepting the west of Russia and the north of Norway and Sweden; the whole of Africa, Madagascar, Seychelles, and the Mauritius. The egress was visible in North and South America, Australia, New Zealand, and nearly the whole of the South Pacific. This egress will have been completed by about eight o'clock this morning, and then all interested in the subject of Venus may look forward to another 122 years before the interesting occurrence again takes place.

ELECTRIC RAILWAYS[1]

WE have grown so accustomed to the regular announcement—"serious—accident on such and such a railway, several passengers injured" that we have almost come to regard railway accidents as inevitable, just as parents mistakingly think the measles and whooping cough necessary accompaniments of childhood. But speed no more means disaster than a densely crowded city means disease. The first effect of overcrowding is undoubtedly to produce fever and other complaints. If, however, the knowledge and practice of the laws of hygiene increase more rapidly than the population of a town, the death-rate, as we have seen, diminishes, instead of augmenting. And so it is with locomotion; the stage-coach journeys of our ancestors were slow enough for the most staunch conservative, and yet the percentage of the passengers injured on their journeys was far greater than even now with our harum-scarum railway travelling. The number of passengers has increased enormously, but the safety has increased in an even greater rate. If then we can devise methods introducing still greater security, a far larger number of passengers may travel at a far greater speed and with less fear of danger than at present.

Accidents constitute one charge against railway conveyance, but there is another, and that is the cost. Cheap as railway travelling now is, compared with the departed stage-coach locomotion, the price of the tickets is still far too high for railways to fulfil, even in a small degree, one of their most important functions, and that is transporting labourers from parts of the country where labour is scarce, to others where it is abundant and labourers in demand.

But how is a happier state of things to be realised? We cannot expect the railway companies to lower their fares merely to benefit humanity. If, however, we can prove to them that the present system of railways is neither the most remunerative to themselves nor the most beneficial to the community at large, we may hope to win the attention of railway directors, whose stock question is, and quite rightly, "Will it pay?"

Those of you who have read the life of Stephenson know what a protracted fight he had to carry one of his most cherished ideas, and that was the employment of a locomotive engine to draw the train, instead of a stationary engine to pull it with ropes or chains. His adversaries saw the disadvantage of adding the weight of the locomotive to the weight of the train, whereas Stephenson was especially struck with the enormous waste of power in the friction of ropes or chains passing over pulleys. [Experiments were then shown proving, *first*, that the mass of the locomotive necessitated the engine having a greater horse-power to get up the speed of the train quickly as well as a greater horse-power to keep up the speed; *secondly*, that the friction and wear and tear of ropes, such as were employed on the London and Blackwall Railway, would have been an insuperable hindrance to the development of railways.] From this was deduced that, since in Stephenson's day the only feasible mode of communicating the power of a stationary engine to a moving train was by means of ropes, his decision to adopt the locomotive was perfectly correct at the time it was made.

Attempts have been made to propel trains by blowing them through tubes, or by blowing a piston attached to the train through a tube, but such attempts at pneumatic railways have nearly all been abandoned. The employment of air compressed into a receiver on the train by fixed pumping engines stationed at various points along the line, and employed to work compressed air engines on the carriages has been effected with considerable success by Col. Beaumont, especially for tram-lines. The weight of the compressed air-engine is, however, still very considerable. Any system of pumping water through a pipe and employing the water to work a hydraulic engine on the train is hardly worth considering, seeing that the mechanical difficulties of keeping up a continuous connection between the moving train and the main through which the water is pumped seem insuperable. Gas-engines worked with ordinary coal-gas, stored perhaps under pressure, might be employed on the moving train, but the advantage arising from the absence of boiler and coal would be more than compensated for by the fact, that the weight of a gas-engine per horse-power developed is so much greater than that of a steam-engine. None of these systems, then, of dispensing with a locomotive is by any means perfect, and the success of the recent experiments on the electric transmission of

[1] Abstract of a lecture at the Royal Institution by Prof. W. E. Ayrton, F.R.S.

power has turned the attention of engineers to the consideration, whether electricity could not successfully supplant steam for the propulsion of trains and tram-cars; whether it could not, in fact, supply an efficient means of transmitting power, the absence of which caused Stephenson to abandon ropes in favour of a heavy locomotive engine.

The whole question, like every similar one, is mainly a question of expense; and what we have to consider is, whether electric transmission on the whole leads to greater economy than can possibly be obtained by the employment of any kind of locomotive. The average weight of a locomotive is about that of six carriages full of people; ten carriages compose an ordinary train, hence the presence of the mass of the locomotive adds at least 50 per cent. to the horse-power absolutely necessary to propel the carriages alone, and therefore at least 50 per cent to the amount of coal burned. But there is another most serious objection to the engines, perhaps even more important than the preceding. The heavy engine passing over every part of the line necessitates the whole line and all the bridges being made many times as strong, and therefore many times as costly, and the expense of maintenance consequently also far greater, than if there were no locomotive. And it is not possible to make the engine much lighter; for it would not have then sufficient adhesion with the rails to be able to draw the train; in fact, you cannot diminish the weight as long as the train is propelled with only one or two pair of driving wheels as at present. The employment of electricity, however, will enable a train to be driven with every pair of wheels, just as the employment of compressed air enables every pair of wheels to brake the train.

To propel a train, we must either utilise the energy of coal by burning it, or use the energy possessed by a mountain stream, or the energy stored up in chemicals, and which is given out when the chemicals are allowed to combine, or we must employ the energy of the wind. Practically we employ at present only the first store for propelling railway trains—the potential energy of coal; and that is to a great extent the store on which we shall still draw, even when we employ electric railways. For experience shows that, with the modern steam-engine and dynamo, at least one-twentieth of the energy in coal can be converted into electric energy; and that this is at least twenty times as economical as the direct conversion of the energy of zinc into electric energy by burning it in a galvanic battery.

But it may be asked, did not Faraday's discovery, in 1831, that a current could be produced by the relative motion of a magnet and a coil of wire, settle this point half a century ago? Theoretically—yes; practically, however, the problem was very far from being solved, because the dynamo machine was very unsatisfactory, and it was not until Pacinotti, in 1860, suggested the solution of the problem of obtaining a practically continuous current from a number of intermittent currents, and until Gramme, about 1870, carried out Pacinotti's suggestion in the actual construction of large working machines, that the mechanical production of currents became commercially possible. [Experiments were then shown illustrating the complete electric transmission of power, a gas-engine on the platform giving rapid motion to a magneto-electric machine, and the current thereby produced sent through an electro-motor at the other end of the room, which worked an ordinary lathe.]

In electric transmission of power there is not only waste of power from mechanical friction, but also from electric friction arising from the electric current heating the wire, through which it passes.

It was then explained and demonstrated experimentally that this latter waste could be made extremely small by placing so light a load on the electro-motor, that it ran nearly as fast as the generator or dynamo, which converted the mechanical energy into electric energy; actual experiments leading to the result that for every foot-pound of work done by the steam-engine on the generator, quite seven-tenths of a foot-pound of work can be done by the distant motor.

One reason why electric transmission of power can be effected with so little waste is because electricity has apparently no mass, and consequently no inertia; there is, therefore, no waste of power in making it go round a corner, as there is with water or with any kind of material fluid. Another reason why electro-motors are so valuable for travelling machinery is on account of the light weight of the motor. Experiment shows that one horse-power can be developed with 56 lbs. of dead weight of electro-motor, and that for large electro-motors of several horse-power the weight per horse is even much less; a result immensely more favourable than can be obtained with steam, gas, or compressed-air-engines.

In addition to the loss of power arising from the heating of the wires by the passage of the current, there is another kind of loss that may be most serious in the case of a long electric railway, viz., that arising from actual leakage of the electricity due to defective insulation. To send an electric current through a distant motor, two wires, a "going" and "return" wire must be employed, insulated from one another by silk, guttapercha, or some insulating substance; and if the motor be on a moving train, there must be some means of keeping up continuous connection between the two ends of the moving electro-motor and the going and return wire. The simplest plan is to use the two rails as the two wires, and make connection with the motor through the wheels of the train; those on one side being well insulated from those of the other, otherwise the current would pass through the axles of the wheels, instead of through the motor. It is this simple plan that is employed in Siemens' Lichterfelde Electric Railway, now running at Berlin; the insulation arising from the rails being merely laid on wooden sleepers having been found sufficient for the short length, 1½ mile. The car is similar to an ordinary tram-car, and holds twenty passengers. [Photographs were then projected on the screen of this and of the original electric railway laid by Siemens in the grouuds of the Berlin Exhibition of 1879, and exhibited in 1881 at the Crystal Palace, Sydenham.] It was explained that on this latter railway, which was 900 yards long, both the ordinary rails were used as the return wire, and that the going wire was a third insnlated rail rubbed by the passing train. [Photographs were then projected on the screen of Siemens' electric tram-car at Paris, used to carry fifty passengers backwards and forwards last year to the Electrical Exhibition.] In this the going and return wires were overhead and insulated, connection being maintained between them and the moving car by two light wires attached to the car, and which pulled along two little carriages running on the overhead insulated wires, and making electric contact with them. [Experiments followed, proving that although two bare wires lying on the ground could be quite efficiently employed as the going and return wire, if the wires were short and the ground dry, the leakage that occurred if the wires were long and the ground moist was so great, as to more than compensate for the absence of the locomotive.] Consequently Prof. Perry and myself have for some time past been working out practical means for overcoming these difficulties, and we have arrived at what we hope is an extremely satisfactory solution. Instead of supplying electricity to one very long, not very well insulated rail, we lay by the side of our railway line a well insulated cable, which conveys the main current. The rail, which is rubbed by the moving train, and which supplies it with electric energy, we subdivide into a number of sections, each fairly well insulated from its neighbour and from the ground; and we arrange that at any moment only that section or sections, which is in the immediate neighbourhood of the train, is connected with the main cable; the connection being of course made automatically by the moving train. As then leakage to the earth of the strong propelling electric current can only take place from that section or sections of the rail, which is in the immediate neighbourhood of the train, the loss of power by leakage is very much less than in the case of a single imperfectly insulated rail such as has been hitherto employed, and which being of great length, with its correspondingly large number of points of support, would offer endless points of escape to the motive current.

Dr. Siemens has experimentally demonstrated that an electric railway can be used for a mile or two; Prof. Perry and myself, by keeping in mind the two essentials of success, viz. attention to both the mechanical and electrical details, have, we venture to think, devised means for reducing the leakage on the longest railway to less than what it would be on the shortest.

For the purpose of automatically making connection between the main well-insulated cable and the rubbed rail in the neighbourhood of the moving train we have devised various means, one of which is seen from the following figures.

A B (Fig. 1) is a copper or other metallic rod resting on the top of and fastened to a corrugated tempered steel disc D D (of the nature of, but of course immensely stronger than the corrugated top of the vacuum box of an aneroid barometer), and which is carried by and fastened to a thick ring E E made of ebonite or other insulating material. The ebonite ring is itself screwed to the circular cast-iron box, which latter is fastened to the ordinary railway sleepers. The auxiliary rail A B and the corrugated steel discs D D have sufficient flexibility that two or more of the latter are simultaneously depressed by an insulating collecting brush or roller carried by one or by all of the carriages. Depressing any of the corrugated steel discs brings the stud F, which is electrically connected with the rod A B, into contact with the stud G electrically connected with the well-insulated cable.

As only a short piece of the auxiliary rail A B is at any moment in connection with the main cable, the insulation of the ebonite ring E E will be sufficient even in wet weather, and the cast-iron box is sufficiently high that the flooding of the line or the deposit of snow does not affect the insulation. The insulation, however, of G, which is permanently in connection with the main cable, must be far better. For this purpose we lead the gutta-percha, or india-rubber, covered wire coming from the main cable through the centre of a specially formed telegraph insulator, and cause it to adhere to the inside of the earthenware tube forming the stalk. And as, in addition, the inside of each contact box is dry, a very perfect insulation is maintained for the lead coming from the main cable. Consequently as all leakage is eliminated except in the immediate neighbourhood of the train, this system can be employed for the very longest electric railways. Fig. 2 shows a modification of the contact box when the insulated rail L, instead of extending all along the line, is quite short and is carried by the train, and by its motion presses forwards and downwards a metallic fork on the contact box, thus making contact between F and G. [Other diagrams were explained, illustrating modifications of the contact-boxes: in one case the well-insulated cable is carried inside the flexible rail, which then takes the form of a tube, shown in Fig. 3; in another case the cable is insulated with paraffin oil instead of with gutta-percha or india-rubber, shown in Fig. 4, &c.]

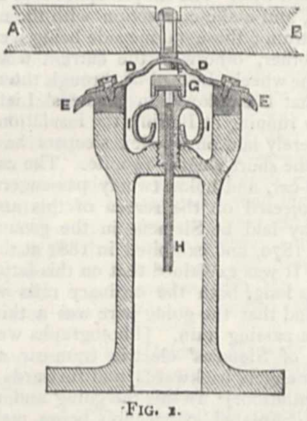


FIG. 1.

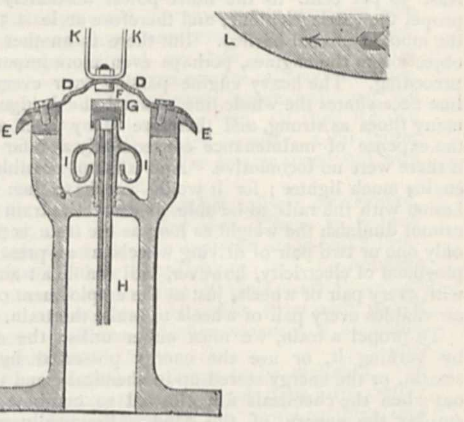


FIG. 2.

The existence of these contact-boxes at every 20 to 50 feet also enables the train to graphically record its position at any moment on a map hanging up at the terminus, or in a signal-box or elsewhere, by a shadow which creeps along the map of the line as the train advances, stops when the train stops, and backs when the train backs. This is effected thus:—As the train passes along, not only is the main contact between F and G automatically made, as already described, but an auxiliary contact is also completed by the depression of the lid of the contact-box, and which has the effect of putting, at each contact-box in succession, an earth fault on an insulated thin auxiliary wire running by the side of the line. And just as the position of an earth fault can be accurately determined by electrical testing at the end of the line, so we arrange that the moving position of the earth fault, that is the position of the train itself, is automatically recorded by the pointer of a galvanometer moving behind a screen or map, in which is cut out a slit representing by its shape and length the section of the line on which the train is, as shown

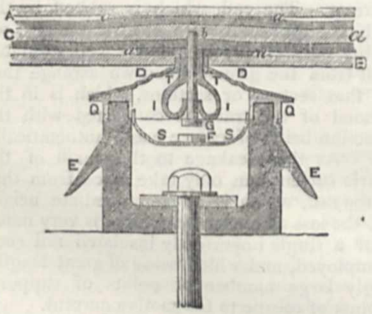


FIG. 3.

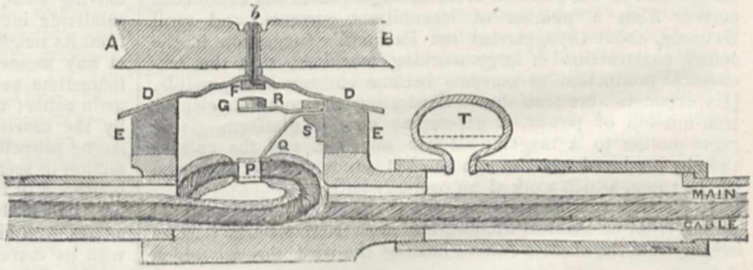


FIG. 4.

in Fig. 5. In addition, then, to the small sections of 20 feet or more into which our auxiliary rubbed rail is electrically divided, there would be certain long blocked sections one mile or several miles in length, for each of which on the map a separate galvanometer and pointer would be provided. [Experiments were shown of the system of graphically automatically recording the progress of a train.]

In the preceding systems there are several contact-boxes in each section of the insulated rubbed rail, and several sections of the insulated rail in each section of the line blocked, but in the next system the rubbed rail is simply divided electrically into long sections each of as great a length as the particular system employed to insulate the rubbed rail will allow. In this case we arrange that the electric connection between the main cable and the rubbed conductor shall be automatically made by the train as it enters a section, and automatically broken as the train leaves a section. The model before you is divided into four sections, each about 11 feet in length, and you see from the current detectors that as the train runs either way, it puts current into the section just entered, and takes off current from the section just left.

[Experiments were then shown of the ease with which an electric train could be made to back instead of going forwards, by reversing the connections between the revolving armatures and the fixed electro-magnets of the motor; also that the accidental reversal of the field magnets of the main stationary generator, although it had the effect of reversing the main current, produced no change in the direction of motion of an electric engine, the direction of motion being solely under the control of the driver.]

But more than this, not only does the train take off current from the section 1 when it is just leaving it, and entering section 2, but no following train entering section 1 can receive current or motive power until the preceding train has entered section 3. [Experiments were then shown proving that with this system a following train could not possibly run into a preceding train even if the preceding train stopped or backed.] Now why does the following train when it runs on to a blocked section pull up so quickly? The reason is because it is not only deprived of all motive power, but is powerfully braked, since when electricity is cut off from a section, the insulated and non-insulated rail of that section are automatically connected together, so that when the train runs on to a blocked section the electromotor becomes a generator short circuited on itself, producing, therefore, a powerful current which rapidly pulls up the engine. [Experiments were then shown of the speed with which an electromotor, which had been set in rapid rotation and then deprived of its motive current, pulled up when its two terminals were short-circuited.]

Whenever, then, a train, it may be even a runaway engine, enters on a blocked section, not only is all motive power withdrawn from it, but it is automatically powerfully braked, quite independently of the action of the engine-driver, guard, or signalman. No fog, nor colour-blindness, nor different codes of signals on different lines, nor mistakes arising from the exhausted nervous condition of overworked signal-men, can with this system

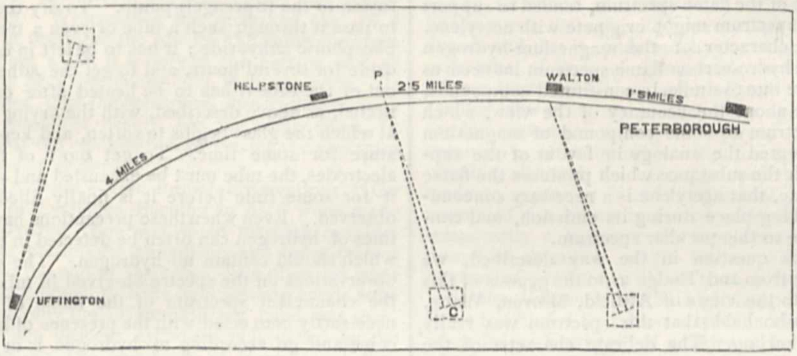


Fig. 5.

produce a collision. The English system of blocking is merely giving an order to stop a train; but whether this is understood or intelligently carried out is only settled by the happening or non-happening of a subsequent collision. Our Absolute Automatic Block acts as if the steam were automatically shut off and the brake put on whenever the train is running into danger; nay, it does more than this—it acts as if the fires were put out and all the coal taken away, since it is quite out of the power of the engine-driver to re-start his train until the one in front is at a safe distance ahead.

But all trains will undoubtedly be lighted with electricity; must, then, the train be plunged into darkness when it runs on to a blocked section to which no electric energy is being supplied? No! If some of the electric energy supplied to a train when it is on an unblocked section be stored up in Faure's accumulators, such as are at present used on the Brighton Pulman train, the lamps will continue burning even when the train has ceased to receive electric energy from the rubbed rail.

When, then, we commit the carrying of our power to that fleet messenger to which we have been accustomed to entrust the carrying of our thoughts, then shall we have railways that will combine speed, economy and safety; and last, but not least to us Londoners, we shall have the entire absence of smoke, the presence of which nearly causes the convenience of the Underground Railway to be balanced by the pernicious character of its atmosphere.

SOCIETIES AND ACADEMIES

LONDON

Royal Society, December 21, 1882.—"On the Origin of the Hydrocarbon Flame Spectrum." By G. D. Liveing, M.A., F.R.S., Prof. of Chemistry, and J. Dewar, M.A., F.R.S., Jacksonian Prof., University of Cambridge.

In previous communications[1] to the Society we have described the spectra of what we believe to be three compound substances, viz., cyanogen, magnesium-hydrogen, and water.

In these investigations our chief aim has been to ascertain facts, and to avoid as far as possible adopting any special theory regarding the genesis of the spectra in question.

Specific spectra have been satisfactorily proved to emanate from the compound molecules of cyanogen, water, and magnesium-hydrogen, so far as we can interpret in the simplest way the many observations previously detailed. The fact that a fluted spectrum is produced under certain conditions, by a substance which does not give such a spectrum under other conditions, is of itself a proof that the body has either passed into an isomeric state or has formed some new compound; but we are not entitled to assert, without investigation, which of these two reasonable explanations of the phenomena is the true one. There is, however, a spectrum to which we have had occasion to refer in the papers on the spectra of the compounds of carbon, which closely resembles that of a compound substance, and which we, in common with some other spectroscopists, have been led to attribute to the hydrocarbon acetylene, without, however, being able to bring forward such rigid experimental proofs of its origin as we have adduced in the case of the three substances above referred to. In other words, the experimental evidence that the hydrocarbon flame spectrum is really due to a hydrocarbon was always indirect. Thus, we showed that many flames containing carbon, such as those of hydrogen mixed with bisulphide of carbon or carbonic oxide, and the flame of cyanogen in air, did not give this spectrum, and these particular flames are known, from the investigations of Berthelot, to be incapable of generating acetylene under conditions producing incomplete combustion. On the other hand, we found that a flame of hydrogen mixed with chloroform, which easily generates acetylene, gives the hydrocarbon flame spectrum in a very marked manner, and it is known that the ordinary blow-pipe flame, in which the same spectrum is well developed, contains this hydrocarbon.

These and other experiments point to the intimate relation of hydrogen and carbon in the combined form of acetylene to the production of this spectrum during combustion. In our various observations on the spectrum of the electric arc taken in different gases, the flame spectrum was always noticed, and seemed to be independent of the surrounding atmosphere. In the mode in which those experiments were conducted, it was easily shown that the carbons were never free from hydrogen, and that the gases always contained traces of aqueous vapour. Under these conditions acetylene is formed synthetically during the electric discharge, the line spectrum of hydrogen being absent; so that we were never convinced that the spectrum was not due to the former substance.

It is well to remark in passing, that our previous work on the spectrum of the carbon compounds was mainly directed to that particular spectrum which is characteristic of the flame of cyanogen, and only indirectly to the flame spectrum of hydrocarbon. We were further supported in connecting the latter

1 "On the spectra of the Compounds of Carbon with Hydrogen and Nitrogen." I and II. *Proc. Roy. Soc.*, vol. 30, pp. 152, 494. "On the Spectrum of Carbon," *ib.*, vol. 33, p. 403. "General Observations on the Spectrum of Carbon and its Compounds," *ib.*, vol. 34, p. 123. "On the Spectrum of Water," *ib.*, vol. 30, p. 480, and vol. 33, p. 274. "Investigations on the Spectrum of Magnesium," *ib.*, vol. 32, p. 189.

spectrum with acetylene, by observing that cyanogen compounds are continously formed when the arc discharge takes place in gases containing nitrogen, and that in all probability their formation is due, as Berthelot has shown, to a reaction taking place between acetylene and nitrogen. Berthelot is positive in his assertions that cyanogen is never formed by a direct combination between carbon and nitrogen, and any such apparent combination is due to impure carbon, and the presence of an imperfectly dried gas; in other words, hydrogen is essential to the production of cyanogen under such conditions according to the views of Berthelot.

The fact that carbonic oxide, which is one of the most stable binary compounds of carbon, forms a distinct spectrum of a character similar to that of the flame spectrum, tended to support the view that the flame spectrum might originate with acetylene. The similarity in the character of the magnesium-hydrogen spectrum to that of the hydro-carbon flame spectrum induced us to believe that they were due to similarly constituted compounds, and seeing we felt sure about the accuracy of the view, which assigns the former spectrum to some compound of magnesium with hydrogen, we accepted the analogy in favour of the supposition that acetylene is the substance which produces the flame spectrum; or, at any rate, that acetylene is a necessary concomitant of the reaction taking place during its emission, and consequently might give rise to this peculiar spectrum.

Having examined this question in the way described, we adopted the view of Angström and Thalén as to the genesis of this spectrum in opposition to the views of Attfield, Morren, Watts, Lockyer, and others, who held that this spectrum was really due to the vapour of carbon. The delicate character of the experiments which were required to discover the origin of the peculiar set of flutings in the more refrangible part of the spectrum of cyanogen made it apparent that, whatever views as to the origin of the hydrocarbon flame spectrum were adopted by different workers, it could not be regarded hitherto as experimentally proved which was the correct one.

With the object of being able to exhaust this question, a special study was subsequently made of the ultra-violet line spectrum of carbon, in order to ascertain whether any of its lines could be found in the spectra of the arc or flame. We have found that the ultra-violet lines of metallic substances have as a rule the greatest emissive power, and are often present when no trace of characteristic lines in the visible part of the spectrum can be detected. If carbon resembled the metals in this respect, then we might hope to find ultra-violet lines belonging to its vapour, thus enabling us to detect the volatilisation of the substance at the relatively low temperatures of the arc and flame. The test experiments made on this hypothesis are recorded in the paper entitled "General Observations on the Spectrum of Carbon and its Compounds." It is there shown that some seven of the marked ultra-violet spark lines of carbon occur in the spectrum of the arc discharge, although one of the strongest lines, situated in the visible portion of the spectrum at wave-length 4266, could not be found. Further, it is proved that the strongest ultra-violet line of carbon does occur in the spectrum of the flame of cyanogen fed with oxygen. Thus it seems probable that the same kind of carbon molecule exists, at least in part, in the arc and flame, as is found to be produced by the most powerful electric sparks, taken between carbon poles or in carbon compounds.

Now the spark gives us the spectrum which is associated with the highest temperatures, and therefore it is assumed that this spectrum is that of the simplest kind of carbon vapour. If that be the case, we cannot avoid inferring that denser forms of carbon vapour must exist in arc and flame, emitting, like other complex bodies, a fluted, in contrast to a line, spectrum; or rather that the two distinct kinds of spectra may be superposed. Such considerations showed that a series of new experiments and observations must be made with the special object of reaching a definite conclusion regarding the origin of the flame spectrum, and the following paper contains a summary of the results of such an inquiry.

Vacuous Tubes.—We have heretofore laid little stress on observations of the spark in vacuous tubes on account of the great uncertainty as to the residual gases which may be left in them. The film of air and moisture adherent to the glass, the gases occluded in the electrodes, and minute quantities of hydrocarbons of high boiling-point introduced in sealing the glass, may easily form a sensible percentage of the residue in the exhausted tube, however pure the gas with which it was originally filled. The excessive difficulty of removing the last traces of moisture we learnt when making observations on the water spectrum, and the almost invariable presence of hydrogen in vacuous tubes is doubtless due in great measure to this cause. Wesendonck (*Proc. Roy. Soc.*, vol. 32, p. 380) has fully confirmed our observations as to this difficulty. By a method similar to that employed by him, we have, however, succeeded in so far drying tubes and the gases introduced into them that the hydrogen lines are not visible in the electric discharge. For this purpose the (Plücker) tube was sealed on one side to a tube filled for some six or eight inches of its length with phosphoric anhydride, through which the gas to be observed was passed, and on the other side to a similar tube full of phosphoric anhydride, which was in turn connected by fusion to the (Sprengel) pump. To dry the gas it is not enough to pass it through such a tube or even a much longer one full of phosphoric anhydride; it has to be left in contact with the anhydride for several hours, and to get the adhering film of moisture out of the tube it has to be heated after exhaustion while connected, as above described, with the drying tubes up to the point at which the glass begins to soften, and kept at near this temperature for some time. To get most of the gases out of the electrodes, the tube must be exhausted and sparks passed through it for some time before it is finally filled with the gas to be observed. Even when these precautions have been observed, the lines of hydrogen can often be detected in tubes filled with gases which should contain no hydrogen. The general result of our observations on the spectra observed in tubes so prepared is that the channelled spectrum of the flame of hydrocarbons is not necessarily connected with the presence of hydrogen; it does not come and go according as hydrogen is or is not present along with carbon in the way that the channelled spectrum of cyanogen comes and goes according as nitrogen is present or absent. Our observations confirm those of Wesendonck on this point. This spectra given by various tubes containing carbon compounds are described in the paper.

Tubes filled with carbonic oxide exhibit in general at different stages of exhaustion the following phenomena. When the exhaustion is commencing and the spark will just pass, the spectrum is usually that of the flame of hydrocarbons and nothing else. As the exhaustion proceeds, the spectrum of carbonic oxide makes its appearance superposed on the former, and gradually increases in brilliance until it overpowers, and at last at a somewhat high degree of exhaustion, entirely supersedes the flame spectrum. This is when no jar is used. In the earlier stages of exhaustion, the effect of the jar is to increase the relative brilliance of the flame spectrum, and diminish that of the carbonic oxide spectrum, and at the same time to bring out strongly the lines of oxygen and carbon; at a certain stage of the exhaustion, when the flame spectrum is very weak without the jar, the effect of the jar is to bring it out again, but without sensibly enfeebling the carbonic oxide spectrum, and without bringing out the carbon lines. At a still higher stage of exhaustion, when the carbonic oxide spectrum is alone seen without the jar, the flame spectrum is sometimes, not always, brought out by putting on the jar, though the carbon lines again show well. At this stage, at which the flame spectrum is not seen at all, the distance between the striæ in the wide part of the tube is considerable, and much metal is thrown off the electrodes, which are rapidly heated by the discharge. In a tube filled with carbonic oxide mixed with a little air imperfectly dried, when not too highly exhausted, the carbonic oxide spectrum, that of the flame of hydrocarbons, and that of cyanogen, may all be seen at once superposed when no jar is used. With a jar and a tolerably high exhaustion the carbonic oxide spectrum, the hydrocarbon flame spectrum, and the carbon line spectrum, may all be seen at the same time.

Spectrum of the Spark in Compounds of Carbon at Higher Pressures.—In the spark taken between poles of purified graphite in hydrogen, the spectrum of hydrocarbon flames is seen, and it increases in brilliance, as the pressure of the gas is increased up to ten atmospheres, and continues bright at still higher pressures so far as we have observed, that is, up to twenty atmospheres. The spark without condenser in carbonic oxide at atmospheric pressure, shows both the spectrum of carbonic oxide and that of the hydrocarbon flame; and as the pressure of the gas is increased, the former spectrum grows fainter, while the latter grows brighter, no jar being used. The line spectrum of carbon is also visible. At the higher pressures the flame spectrum predominates and is very strong. The observations were carried up to a pressure of twenty-two and a half atmospheres. On letting down the pressure, the same phenomena occur in the reverse order. All the parts of the flame spectrum, as seen in a Bunsen burner, are increased in intensity as the pressure is increased. The fact that the effects of high pressure are so similar to those produced by the use of a condenser at lower pressures, seems to point to high temperature as the cause of those effects. But against this, we have the fact that at reduced pressure we get in carbonic oxide, the carbonic oxide spectrum and the line spectra of carbon and oxygen simultaneously, without that of the hydrocarbon flame. As we cannot doubt that a very high temperature is required to give the line spectrum of carbon, we must suppose that reduced pressure is unfavourable to the stability of the molecular combination, whatever it be, which gives the hydrocarbon flame spectrum. Wesendonck has remarked (*loc. cit.*.) that in carbonic acid at pressures too low for the flame spectrum to be developed without a jar, it is only in the narrow part of the tube that the use of a jar brings out that spectrum. It would appear, therefore, that the constraint, due to the confined space in which the discharge occurs, has the same effect, in regard to the stability of the combination producing the spectrum in question, as increase of pressure.

Cyanogen Flame Spectrum.—Our former observations "On the Flame Spectrum of Cyanogen Burning in Air" were made on cyanogen gas, prepared from well-dried mercury cyanide, which was passed over phosphoric anhydride, and burnt from a platinum jet fused into the end of the tube. We observed what Plücker and Hittorf had noted, that the hydrocarbon bands were almost entirely absent, only the brightest green band was seen, and that faintly. When gaseous cyanogen is liquefied by the direct pressure of the gas, the researches of Gore (*Proc. Roy. Soc.*, vol. 20, p. 68) have shown that it is apt to be contaminated with a brownish, treacley liquid, which probably arises from the imperfectly purified or dried cyanide of mercury. In order to obtain pure cyanogen, we have prepared quantities of liquid cyanogen, not by compression, but by passing the already cooled gas into tubes placed in a carbonic acid and ether bath. By this method of condensation any easily liquefiable substances are isolated, and any permanently gaseous substance escaped. The samples were sealed up in glass tubes into which different reagents were inserted. After such treatment the cyanogen was used for the production of the flame in dry air or oxygen. The liquid cyanogen was left in contact with phosphoric anyhdride, Nordhausen sulphuric acid, and ordinary sulphuric acid. By means of a special arrangement of glass tubing surrounding the flame dry oxygen could be supplied, or oxygen made directly from fused chlorate of potash could, by means of a separate nozzle, be directed on to the flame, and thus perfectly dry and pure gases used for combustion. Liquid cyanogen which had remained in presence of the above reagents gave only the single green hydrocarbon flame line faintly in dry air, all the cyanogen violet sets being strong. When oxygen made directly from chlorate of potash was directed on to the flame, all the hydrocarbon flame sets appeared with marked brilliancy. The set of lines which we have formerly referred to as the three set of flutings of the cyanogen spectrum, showed marked alteration of brilliancy with variations in the oxygen supply. Thus, liquid cyanogen purified by the action of the above reagents, does yield the spectrum of hydrocarbon flames on combustion in pure oxygen. From the great precautions we have taken, we feel sure that the amount of combined hydrogen in the form of water or other impurities in the combining substances must have been exceedingly small, and that the marked increase in the intensity of the flame spectrum, when oxygen replaces air is essentially connected with the higher temperature of the flame, and is not directly related to the amount of hydrogen present. This being the case, it must be admitted that the flame spectrum requires a higher temperature for its production during the combustion of cyanogen than that which is sufficient to cause a powerful emission of the special spectra of the molecules of cyanogen. Now, the two compounds of carbon, which give the highest temperature on combustion are cyanogen and acetylene. Both of these compounds decompose with evolution of heat, in fact, they are explosive compounds, and the latent energy in the respective bodies is so great that if thrown into the separated constituents a temperature of near four thousand degrees would be reached. The flames of cyanogen and acetylene are peculiar in this respect, that the temperature of individual decomposing molecules is not dependent entirely on the temperature generated by the combustion, which is a function of the tension of dissociation of the oxidised products, carbonic acid and water. We have no means of defining with any accuracy the temperature which the particles of such a body may reach. We know, however, that the mean temperature of the flames of carbonic oxide and hydrogen lies between two and three thousand degrees, and if this be added to that which can be reached by the substance independently, then we may safely infer that the temperature of individual molecules of carbon, nitrogen, and hydrogen in the respective flames of cyanogen and acetylene may reach a temperture of from six to seven thousand degrees.

A previous estimate of the temperature of the positive pole in the electric arc made by one of us, was something like the same value.

The formation of acetylene in ordinary combustion seems to be the agent, through which a very high local temperature is produced, and this is confirmed by the observations of Gouy on the occurence of lines of the metals in the green cone of the Bunsen burner, which are generally only visible in spark spectra. On this view acetylene is a necessary agent in the production of the flame spectrum during combustion. The fact that the flame spectrum is often invisible when the arc is taken in a magnesia crucible, although the cyanogen spectrum is strong, but may be made to appear by introducing a cool gas or moisture, must be accounted for by an increased resistance in the arc producing temporarily a higher mean temperature. The experiments in course of execution, where the arc will be subject to a sudden increase of pressure, will, we trust, solve this difficulty.

Further evidence of the high temperature of the cyanogen flame is afforded by the occurrence in the spectrum of that flame, when fed with oxygen, of a series of flutings in the ultra-violet, which appear to be due to nitrogen. The series consists of four, or perhaps more, sets, each set consisting of a double series of lines overlapping one another. The lines increase in their distance apart on the more refrangible side, otherwise the flutings have a general resemblance to the B group of the solar spectrum.

The four sets commence approximately at about the wavelengths 2718, 2588, 2479, 2373 respectively. They are frequently present in the spectrum of the arc taken in a magnesia crucible, and show strongly in that of the spark taken without a condenser either in air or nitrogen. As they appear in the spectrum of the spark in nitrogen, whether the electrodes be aluminium or magnesium, and do not appear when the spark is taken in hydrogen or in carbonic acid gas, they are in all probability due to nitrogen. When a large condenser is used they disappear.

Linnean Society, December 21, 1882.—Alfred W. Bennett, M.A., in the chair.—Prof. Adolph. Ernst, of Venezuela, and Dr. W. C. Ondaatje, of Ceylon, were elected Fellows.—Prof. T. S. Cobbold exhibited specimens of Ligules from the Bream, the Minnow, and the Grebe to compare with those from man. The worm from the Bream is called *L. edulis* by Briganti, and is eaten under the name "macaroni piatti."—Mr. T. Christy called attention to experiments lately made, which show that the Kola nut possesses singular properties of clearing fermented liquors.—Mr. Thos. H. Corry read a paper on the development and mode of fertilisation of the flower in *Asclepias Cornuti.* R. Brown, 1809, J. B. Payer, 1857, and thereafter H. Schacht, have made Asclepias the subject of interesting study; Mr. Corry nevertheless has added new observations thereto. He finds that the petals and stamens, which in the early stage originate separately, become afterwards adnate; the stamens, moreover, by their broad filaments form a fleshy pentagonal ring, *i.e.* are monadelphous. The "*stigma-disk*" is not formed by the fusion of two stigmas, for the styles proper remain distinct throughout their entire extent. The greatest analogy of the flower to that of the Apocynaceæ is at this period; thereafter differences ensue. From a careful study of the different stages of the pollen in Asclepias it appears to exhibit a perfectly isolated and peculiar case of formation. The idea that self-fertilisation can take place with the parts *in situ* is shown to be impossible, and the need for insect or artificial aid rendered imperative. Cross-fertilisation is the great law in the Asclepiads. —Dr. F. Day read a paper, "Observations on the Marine Fauna of the East Coast of Scotland," founded on a recent survey by H.M.S. *Triton* off Aberdeen, Kincardine, and Forfar. As regards the herring and its migrations, they shift their locality for breeding purposes or in search of food, occasionally being driven from a spot where extensive netting or other causes disturb them. The herring seems of late years to take to deeper water off shore, but at times they appear to return to their old habitats in the comparatively shallow water. Although it is true that some fisheries—Wick, for instance—have decreased in plenty, at the same time other places, *e.g.* Fraserburgh, have proportionately increased. The fishery records prove that from the beginning of this century onwards there has been a steady annual increase of fish taken, though desponding fishermen aver to the contrary. At Wick, herring of different ages and conditions arrive and depart thrice yearly. Dr. Day recounts the results of his dredgings, and describes the Crustaceans and Mollusks obtained.—An additional report on the Echinoderms collected by Dr. Day, was made by Prof. F. J. Bell, and of the Zoophytes and Sponges by Mr. S. O. Ridley.—Mr. J. G. Baker afterwards read his second contribution on the Flora of Madagascar. In this paper, upwards of 150 new species of monopetalous dicotyledons are characterised. They were gathered chiefly by the Rev. R. Baron, F.L.S., of the London Missionary Society. Among others described are four new genera, one nearly allied to Cinchona, a second of semi-parasitic Scrophulariaceæ, and two of Acanthaceæ; besides these, many representatives of well-known European genera occur.—Prof. T. S. Cobbold read a description of *Ligula Mansoni*, a new human Cestode. He shows it to be extremely probable that the trout's ligule is the sexually immature state of the great broad tapeworm of man. Other interesting genetic relations are established, and several important generalisations discussed.—Additions to the Lichens of the *Challenger* Expedition was a short paper by the Rev. J. M. Crombie.—Mr. J. G. Baker made a second communication, being descriptions of about thirty plants from the Fiji Islands referred to by Mr. J. Horne in his recent work on the economic resources of Fiji.

Victoria Institute, January 1.—A paper upon "Design in Nature," was read by Mr. W. P. James. It was stated that Prof. Stokes, F.R.S., would read a paper at the next meeting.

Paris

Academy of Sciences, January 2.—M. Blanchard in the chair.—M. Rolland was elected vice-president for 1883.—The Academy has lost four Members during 1882, viz MM. Liouville, Decaisne, Bussy (Free Academician), and Wöhler (Foreign Associate); and six Correspondents, viz. MM. Plantamour, Lutke, Billet, Darwin, Cornalia, and Schwann.—Memoir on the vision of material colours, &c. (continued), by M. Chevreul.—Researches on hyponitrites; first part, chemical researches, by MM. Berthelot and Ogier. They study hyponitrite of silver, describing their analyses, and examination of the action of heat and oxidising agents, also calorimetric measurements. The formula $N_2O_5Ag_2$ agrees best with the results.—Ramifications of *Isatis tinctoria*, formation of its inflorescences, by M. Trécul.—It was announced that the U.S. Congress had invited the President of that country to convoke all nations to a conference with a view to adoption of a common initial meridian and an universal hour.—Reply to the objections presented by MM. Faye and Hirn to the theory of solar energy, by Dr. Siemens.—On a method of photographing the corona without an eclipse of the sun, by Dr. Huggins.—On geodetic circles, by M. Darboux.—On algebraic integrals of linear differential equations with rational coefficients, by M. Autonne.—On a communication of M. de Jonquières relative to prime numbers (continued), by M. Lipschitz.—Remarks on the subject of a note of M. Hugoniot, on the development of functions in series from other functions, by M. du Bois-Reymond.—Does oil act on the swell or on the breaker? by M. Van der Mensbrugghe. His theory applied only to two cases: where calm water, covered with oil, came to be acted on by wind, and where waves break. The relative calm of phosphorescent portions of tropical waters, he attributes not to increase of cohesion of the water (Admiral Bourgeois), but to the innumerable floating objects forming an obstacle to the slip of surface-layers over each other.—Decomposition of formic acid by the effluve, by M. Maquenne. The results are the same as those got by M. Berthelot in decomposing gaseous formic acid in a closed vessel, by heat alone, about 260°.—On the chloride of pyrosulphuryl, by M. Ogier.—On a vibrion observed during measles, by M. Le Bel. It is found in the urine in the early stages, and disappears with the fever: is a slightly curved, very refringent rod, moving very slowly; contains oval spores at one-third of its length, in a bag of dead protoplasm, which gradually disappears, the spore showing then a zone of mucilage around it. Another occurrence of spores on the thirty-fifth day was observed in an adult. The vibrion also may be got from the skin at the time of desquamation. M. Le Bel cultivated the vibrion, and injected it into a guinea-pig; which, on the tenth day, showed small vibrions in its urine, but did not seem incommoded. The urine in scarlatina and in diphtheria shows a microbacterium and a micrococcus, respectively, both quite different from the vibrion of measles.—Existence of zinc in the state of complete diffusion in dolomitic strata, by M. Dieulafait.—On the Marine Carboniferous of Haute-Alsace; discovery of culm in the valley of the Bruche, by MM. Bleicher and Mieg.—On the excitant property of oats, by M. Sanson. He has experimented with Du Bois Reymond's electrical apparatus on the neuromuscular excitability of horses, before and after ingestion of oats, or of an excitant substance, which he isolated from oats (from the pericarp of the fruit); this is called *avenine*, is quite unlike vanilline. is uncrystallisable, brown in mass, finely granular, and has the formula $C_{56}H_{21}NO_{18}$. All kinds of cultivated oats elaborate it, but in different quantity; as a rule the white varieties have less than the dark. The quantity seems also to depend on the place of cultivation. Crushing the grain weakens the excitant property. The total duration of the excitation (which grows to a point, then gradually disappears) seemed to be about an hour per kilogramme of oats ingested.

Errata in last week's report.—P. 236, top of second column, 7th line, for "Guimareo" read "Guimaraes" 9th line, for "argotised" read "azotised"; 13th line, for "usteria" read "Asteria"; 16th line, for "*pedunculus*" read "*pedunculatus*"; 16th line, for "suctocitiates" read "suctociliates."

Vienna

Imperial Academy of Sciences, November 9, 1882.—The following papers were read:—K. Laker, studies on the hæmatic discs (Hayem's hæmatoblasts), and on the so-called dissolution of the white blood-corpuscles in the process of the purification of the blood.—E. Ludwig, note relating to the chemical composition of the damburite from the Scopi Mountain (Graubündteng).—T. Herzig, on guaiaconic acid and guiacic acid.—On the action of nitrous acid on guiacol, by the same.—A. Grunow, preliminary communication on the Diatomaceæ collected by the Austro-Hungarian North Polar Expedition.

November 16, 1882.—The following papers were read:—N. Polejæff, on the sperm and spermatogenesis of *Sycandra rajahamus Hæckelii*.—F. v. Hauer, new contributions to the knowledge of the elder tertiary Brachiuara fauna of Vicenza and Verona (Italy).—M. Margules, note on the dynamo-electric process.—A. Tarolimek, contributions to mechanical theory of heat.—K. Zelbr, on the comet Schmidt, October 9, 1882.—A sealed paper dated from November 6, 1882, was opened and read containing a short note by Josef Popper, on the transmission of power and the realisation of unused natural powers by electricity.

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