THURSDAY, AUGUST 11, 1892.

THE BRITISH ASSOCIATION.

EDINBURGH.

THE Edinburgh meeting has not been remarkable for a large turn-out of members. Probably the greatest number of members are present on the Friday, when practically all have come and none have left. At this high-water mark the number of members, associates, and holders of transferable ladies' tickets, was 2009, and although the tickets sold were increased to 2068 by Wednesday, the total attendance probably never quite reached 2000, which, although greater than last year's meeting at Cardiff, is much less than that twenty-one years ago at Edinburgh. Year by year the number of ladies taking part in the proceedings mounts steadily, and on several occasions the "popular" sections of anthropology and geography, which were frequently crowded, showed a great preponderance. Everything has not gone quite smoothly in spite of the efforts of the local secretaries. Edinburgh society is inelastic in its traditions, and Edinburgh institutions are ruled by rigid laws, which even a meeting of the British Association finds difficulty in relaxing. For the first day or two the reading and writing rooms and other apartments were closed at 5 p.m., as they occupy part of the Advocates' Library, while the reception-room being in Parliament House remained open for the usual time. Unqualified praise can be given to the commissariat arrangements. The main luncheon-room in the Students' Union was deservedly busy from 1 to 3. The handsome building containing these commodious rooms was greatly admired, and the enterprise of the students to whose efforts alone its construction is due, and by whom alone it is managed, was the subject of frequent comment. Passing between the section rooms and the Union members availed themselves of frequent opportunities to inspect the great MacEwen Hall of the University, now approaching completion, the prospective use of which, by the way, was one of the considerations that led the deputation from Edinburgh to defer to that from Cardiff in arranging the order of the Association's visits to the respective towns.

Rarely is it the privilege of the mixed multitude who throng the hall on the opening night of the meeting to listen to so comprehensible and attractive an address as the President delivered on this occasion. Sir Archibald Geikie's lucid exposition was crowned by a characteristically happy speech by Lord Kelvin in moving the vote of thanks. Altogether the first gathering dealt a blow at the belief, still amusingly common, that the true scientific man is a being of terms and formulæ, and that true science is colourless and unsympathetic. The other evening discourses were highly appreciated, and maintained the high character which the Association lectures have made for themselves. Prof. Milnes Marshall played upon his vague title of "Pedigrees" until the scintillations lit up a great part of the theory of evolution; while Prof. Ewing on Magnetic Induction threw a flood of light on what has hitherto been to ordinary minds one of the obscurest recesses of physics. The lecture to the working classes, from which members of the Association are excluded—unless they attend on false pretences—turned out a great success. Mr. Vernon Boys showed and explained his wonderful experiments on photographing bullets and the waves they produce in traversing the air with point and brilliancy. The work in some of the Sections has been of a high order of excellence; in Section A especially very few British physicists were absent, and some of the discussions alone justified the existence of the Association as a means of bringing men together from different working centres. The reading of the various reports in the different Sections in some cases gave rise to suggestions of high value for future work. Unfortunately, in consequence of the illness of the President, the address in the Geological Section was not read till Monday. It was not then quite complete, so its publication is postponed for the present.

Edinburgh, if possible, exceeded its old reputation for hospitality, and meetings of a purely social character were unusually numerous, and the two conversaziones proved thoroughly enjoyable.

Excursions practically took up the whole of Saturday, and an unusually large number took advantage of the opportunity for visiting the many scenes of historical, archæological, geological, and engineering interest which lie around Edinburgh. The range was by no means restricted to the immediate vicinity, the excellent railway arrangements permitting of visits to Glasgow, Dundee, and the land of Scott, with no greater expenditure of time than the carriage parties demanded for visiting the Forth Bridge and Roslin, or the pedestrians for geologizing in the Pentlands and on Arthur's Seat. The weather for the first few days was favourable, being dry and free from excessive heat. But Monday was a most unfortunate sample of Edinburgh's weather at its worst, strong east wind and cold continual showers; even this state of matters failed to empty the section-rooms where papers of popular interest were being read. Afternoon receptions both public and private, were particularly well managed, perhaps the most enjoyable being that given by the Royal Scottish Geographical Society in the spacious halls of the National Portrait Gallery, where the Antiquarian Museum is now worthily housed. A special reception of foreign members was also given by the University in the Library Hall. The number of distinguished foreigners present marked out this meeting from most others of recent years. The Prince of Monaco, who, with the princess, lived on board his new yacht the *Princesse Alice*, was perhaps the greatest attraction, and he succeeded in bringing together one of the largest audiences to listen to his papers in Section E. He also showed his yacht to a select party of members specially interested in marine studies, and took endless trouble in explaining the ingenious original devices for deep-sea research with which she is fitted. Profs. von Helmholz, Wiedemann, von Richthofen, Ostwald, and Goebel worthily represented the science of German Universities, and many of their somewhat less distinguished colleagues were also present. Baron de Guerner, MM. de Margine, Demolins, Bertrand, Manouvrier, Guillaume, and Richard came from France, the Abbé Renard and Profs. Errera and Hulin from Belgium, Drs. Arrhenius and Pettersson from Sweden, Prof. Fritsch and others well known in the scientific world from Austria, while the United States, Holland, Russia, and Switzerland were also represented. The brilliant young physicist, Nikola Tesla, appears in the list as a visitor from America.

A small meeting unfortunately means a small sum available for grants to scientific workers, and on account of the large sums asked for by the various Sections, the work of the Committee of Recommendations was no sinecure.

Next year's meeting will be held at Nottingham, and that for 1894 at Oxford.

The list of awards finally arrived at was as follows :—

Investigation of the Eruptive Deposits of Pentland Hills	£10
Isomeric Naphthalene Derivatives ...	20
Index of Plants, &c.	20
Climatology, &c., of Africa	50
Place Names in Scotland	10
Electrical Measurements	25
Observations on Ben Nevis	150
Falmouth Observatory	25
Photography of Meteorological Phenomena	10
Solar Radiation	10
Spectra of Elements	10
Analyses of Iron and Steel	20
Action of Light on Dyed Colours	5
Erratic Blocks of England, Wales, and Ireland	10
Fossil Phyllopoda	5
Geological Intervals	10
Underground Waters...	5
High-level Shell-bearing Deposits	20
Zoological Station at Naples	100
Plymouth Biological Station	30
Sandwich Islands	100
West Indies	50
Irish Sea Exploration	30
Oxygen in Asphyxia	20
Exploration of the Karakorum Mountains	50
Methods of Economic Training	5
Anthropometric Tabulations	5
Exploration of Assam	25
North-West Tribes of Canada	100
Natives of India	10
Corresponding Societies Committee	30
Total ...	£970

SECTION D.

BIOLOGY.

OPENING ADDRESS BY PROF. WILLIAM RUTHERFORD, M.D., F.R.S., PRESIDENT OF THE SECTION.

AT the meeting of this Association held at Birmingham in 1886 I had the honour of delivering a lecture on the Sense of Hearing, in which I criticized the current theory of tone-sensation, and I propose on this occasion to discuss the current theories regarding our sense of colour.

I may premise that our conceptions of the outer world are entirely founded on the experience gathered from our sensory impressions. Through our organs of sensation, mechanical, chemical, and radiant energies impress our consciousness. The manner in which the physical agents stimulate the peripheral sense-organs, the nature of the movement transmitted through our nerves to the centres for sensation in the brain, the manner in which different qualities of sensation are there produced—all these are problems of endless interest to the physiologist and psychologist.

Every psychologist has acknowledged the profound significance of Johannes Müller's law of the specific energies—or, as we should rather say, the specific activities of the sense-organs. To those unfamiliar with it, I may explain it by saying, that if a motor nerve be stimulated, the obvious result is muscular movement ; it matters not by what form of energy the nerve is stimulated—it may be by electricity or heat, by a mechanical pinch or a chemical stimulus, the specific result is muscular contraction. In like manner, when the nerve of sight is stimulated—it may be by light falling on the retina, or by electricity, or mechanical pressure, or by cutting the nerve—the invariable result is a luminous sensation, because the impression is transmitted to cells in the centre for vision in the brain, whose specific function is to produce a sense of light.

The same principle applies to the other sensory centres ; when thrown into activity, they each produce a special kind of sensation. The sun's rays falling on the skin induce a sense of heat, but falling on the eye, they induce a sense of sight. In both cases the physical agent is the same ; the difference of result arises from specific differences of function in the brain centres concerned in thermal and visual sense. We have no conception how it is that different kinds of sensation arise from molecular movements in the different groups of sensory cells ; we are as ignorant of that as we are of the nature of consciousness itself.

The subject I propose to discuss on this occasion is not the cause of the different *kinds* of sensation proper to the different sense-organs, but the causes of some *qualities* of sensation producible through one and the same sense-organ.

The theory of tone-sensation proposed by Helmholz is, that the ear contains an elaborate series of nerve terminals capable of responding to tones varying in pitch from 16 vibrations to upwards of 40,000 vibrations per second, and that at least one different fibre in the auditory nerve, and at least one different cell in the centre for hearing, is affected by every tone of perceptibly different pitch. Although the physical difference between high and low tones is simply a difference in frequency of the sound waves, that is not supposed by Helmholz to be the cause of the different sensations of pitch. According to his theory, the function of frequency of vibration is simply to excite by sympathy different nerve terminals in the ear. The molecular movement in all the nerve fibres is supposed to be identical, and the different sensations of pitch are ascribed to a highly specialized condition of cells in the hearing centre, whereby each cell, so to speak, produces the sensation of a tone of definite pitch, which in no way depends on the frequency of incoming nerve impulses, but simply on the specific activity of the cell concerned.

In my lecture on the Sense of Hearing I pointed out in detail the great anatomical difficulties attending the theory in question. I endeavoured to show the physical defect of a theory which does not suppose that our sensations of harmony and discord must immediately depend upon the numerical ratios of nerve vibrations transmitted from the ear to the central organ, and I offered a new theory of hearing based upon the analogy of the telephone. According to that theory, there is probably no analysis of sound in the ear ; the air-cells at the peripheral ends of the auditory nerve are probably affected by every audible sound of whatever pitch. When stimulated by sound they probably produce nerve vibration, simple or compound, whose frequency, amplitude, and wave-form correspond to those of the sound received. The nerve vibrations arriving in the cells of the auditory centre probably induce simple sensations of tones of different pitch, or compound sensations of harmonies or discords strictly dependent on the relative frequencies of the nerve vibrations coming in through the nerve.

I cannot now recapitulate the evidence derived from anatomical, experimental, and pathological observations that give support to my theory of hearing, but I may briefly say that it is opposed to the theory of specific activities, in so far as it has been applied to explain the different qualities of sound sensation. It is, however, in strict accord with the fundamental proposition stated by Fechner[1] in his great work on Psychophysics in these words : "The first, the fundamental hypothesis is, that the activities in our nervous system on which the sensations of light and sound functionally depend are, not less than the light and sound themselves, to be regarded as dependent on vibratory movements." It is evident that, if we could only comprehend the nature of the molecular movement in the nerve that links the vibration of the physical agent to that in the sensory cell, we could advance towards a true theory of the physiological basis of different qualities of sensation in the different sense-organs. As yet no definite answer can be given to the question, what sort of molecular movement constitutes a nerve impulse, but in recent years our knowledge of the subject has been extended in a direction that opens up a vista of new possibilities.

A nerve impulse travels at a rate not much more than 100 feet per second—an extremely slow speed compared with that of electricity in a wire. It has been thought to be of the nature of a chemical change sweeping along the nerve, but that hypothesis is opposed by the fact that the most delicate thermopile shows no production of heat, even when an impulse is caused to sweep repeatedly along the same nerve. Again, it is far easier to fatigue a muscle than a nerve. A living frog's nerve removed from the animal, and therefore deprived of all nutrition, can retain its excitability for nearly an hour, although subjected all the while to thirty or forty stimulations per second. An excised muscle, when similarly stimulated, is exhausted far sooner, because the mechanical energy entirely

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[1] "Elemente der Psychophysik," 1860 2nd edition, 1889, part ii. p. 282.

springs from chemical change in the muscular substance, and therefore the muscle is more easily fatigued than the nerve. The molecular motion in an excited nerve produces a momentary electric current; but that result is not peculiar to nerve. The same occurs in muscle when stimulated. Possibly the molecular movement is of the nature of a mechanical vibration; at all events, we know now that a nerve can transmit hundreds, even thousands of impulses, or let us simply say vibrations, per second. The fact is so important and significant in relation to the physiology of the sense organs, that I show you an experiment to render it more intelligible. A frog's muscle has been hooked to a light lever to record its movement on a smoked cylinder. The nerve of the muscle has been laid on two electrodes connected with the secondary coil of an induction machine. In the primary circuit a vibrating reed has been introduced to serve as a key for making and breaking the circuit, and so stimulating the nerve with periodic induction shocks. If we make the reed long enough to vibrate ten times per second, ten impulses are sent through the nerve to the muscle and ten distinct contractions produced, as shown by the wavy line upon the cylinder. If we shorten the reed so that it will vibrate, say, fifty times per second, the muscle is thrown into a continuous contraction and traces a smooth line on the cylinder; but if we listen to the muscle we can hear a tone having a pitch of fifty vibrations per second, from which we know that fifty nerve impulses are entering the muscle and inducing fifty shocks of chemical discharge in the muscular substance. If we take a reed that vibrates, say, 500 times per second, we hear, on listening to the muscle, a tone having the pitch of 500 vibrations. Observe, that we are not dealing with the transmission of electrical shocks along the nerve, but with the transmission of nerve impulses. By stimulating the nerve with wires of a telephone it has been shown by D'Arsonval that a nerve can transmit upwards of 5000 vibrations per second, and that the wave-forms may be so perfect that the complex electrical waves produced in the telephone by the vowel sounds can be reproduced in the sound of a muscle after having been translated into nerve vibrations and transmitted along a nerve. Such experiments go far in helping us towards a comprehension of the capabilities of nerves in transmitting nerve vibrations of great frequency and complicated wave form; but although they enable us reasonably to suppose that all the fibres of the auditory nerve can transmit nerve vibrations, simple or complex, and with a frequency similar to that of all audible tones, we encounter superlative difficulty in applying such a theory to the sense of sight. In objective sound we have to deal with a comparatively simple wave motion, whose frequency of vibration is not difficult to grasp even at the highest limit of audible sound—about 40,000 vibrations per second. But in objective light the frequency of vibration is so enormous—amounting to hundreds of billions per second—that every one feels the difficulty of forming any conception of the manner in which different frequencies of ether waves induce differences in colour sensation.

But before passing to colour sense, I wish to allude for a moment to the sense of smell. The terminals of the olfactory nerve in the nose are epithelial cells. It has been recently shown by Von Brunn[1] that in man and other mammals the cells have at their free ends very delicate short hairs, resembling those long known in lower vertebrates. These hairs must be the terminal structures affected by substances that induce smell, and are therefore analogous to the hairs on the terminal cells in our organ of hearing. No one ever suggested that the hairs of the auditory cells can analyze sounds by responding to particular vibrations, and I think it quite as improbable that the hairs on any particular olfactory cell respond to the molecular vibrations of any particular substance. If we follow those who have had recourse to the doctrine of specific activities to explain the production of different smells, we must suppose that at least one special epithelial cell and nerve fibre are affected by each different smelling substance. Considering how great is the variety of smells, and that their number increases with the production of new substances, it would be a somewhat serious stretch of imagination to suppose that for each new smell of a substance yet to emerge from the retort of the chemist there is in waiting a special nerve terminal in the nose. It seems to me far simpler to suppose that all the hairs of the olfactory cells are affected by every smelling substance, and that the different qualities of smell result from difference in the frequency and form of the vibrations initiated by the action of the chemical molecules on the olfactory cells and transmitted to the brain. That hypothesis was, I believe, first suggested by Prof. Ramsay,[1] of Bristol, in 1882, and it seems to me the only intelligible theory of smell yet offered. But it must be admitted that a theory of smell, such as that advanced by Ramsay, involves a more subtle conception of the molecular vibrations in nerve fibrils than is required in the case of hearing. It involves the conception that musk, camphor, and similar substances produce their characteristic qualities of smell by setting up nerve vibrations of different frequencies and probably of different complexities. We shall see what bearing this may have on the theory of colour sense, to which I now pass.

No impressions derived from external Nature yield so much calm joy to the mind as our sensation of colour. Pure tones and perfect harmonies produce delightful sensations, but they are outrivalled by the colour effects of a glorious sunset. Without our sense of colour all Nature would appear dressed in bold black and white, or indifferent grey. We would recognize, as now, the beauty of shapely forms, but they would be as the cold engraving contrasted with the brilliant canvas of Titian. The beautiful tints we so readily associate with natural objects are all of them sensations produced in our brain. Paradox though it appear, all Nature is really in darkness. The radiant energy that streams from a sun is but a subtle wave-motion, which produces the common effects of heat on all bodies, dead or living. It does not dispel the darkness of Nature until it falls on a living eye, and produces the sense of light. Objective light is only a wave motion in an ethereal medium; subjective light is a sensation produced by molecular vibration in our nerve apparatus.

The sensory mechanism concerned in sight consists of the retina, the optic nerve, and the centre for visual sensation in the occipital lobe of the brain. In the vertebrate eye the fibres of the optic nerve spread out in the inner part of the retina, and are connected with several layers of ganglionic cells placed external to them. The light has to stream through the fibres and ganglionic layers to reach the visual cells—that is, the nerve terminals placed in the outer part of the retina. They may be regarded as epithelial cells, whose peripheral ends are developed into peculiar rod- and cone-shaped bodies, while their central ends are in physiological continuity with nerve fibrils. Each rod and cone consists of an inner and an outer segment. The outer segment is a pile of exceedingly thin, transparent, doubly refractive discs, colourless in the cone, but coloured pink or purple in the rod. In man, the inner segment of both rod and cone is colourless and transparent. Its outer part appears to be a compact mass of fine fibrils that pass imperceptibly into the homogeneous-looking protoplasm in the shaft of the cell. Owing to the position of the rods and cones, the light first traverses their inner, then their outer segments, and its unabsorbed portion passes on to the adjacent layer of dark-brown pigment cells by which it is absorbed. It is not necessary for me to discuss the possible difference of function between the rods and cones. I may simply say that in the central part of the yellow spot of the retina, where vision is most acute, and from which we derive most of our impressions of form and colour, the only sensory terminals are the cones. A single cone can enable us to obtain a distinct visual impression. If two small pencils of light fall on the same cone the resulting sensory impression is single. To produce a double impression the luminous pencils must fall on at least two cones. That shows how distinct must be the path pursued by the nerve impulse from a visual cell in the yellow spot of the retina to a sensory cell in the brain. The impulses from adjacent terminals must pursue their own discrete paths through the apparent labyrinth of nerve fibrils and ganglion cells in the retina to the fibres of the optic nerve. How these facts bear on the theory of colour sense will presently be apparent. Meantime I pass to the physical agent that stimulates the retina.

When a beam of white light is dispersed by a prism or diffraction grating, the ether-waves are spread out in the order of their frequency of undulation. The undulations of radiant energy extend through a range of many octaves, but those able to stimulate the retina are comprised within a range of rather less than one octave, extending from a frequency of about 395 billions per second at the extreme red to about 757 billions at the extreme violet end of the visible spectrum. The ultra-violet waves in the spectrum of sunlight extend through rather more than half an octave. Although mainly revealed by their chemical

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[1] Von Brunn, *Archiv für mikroskopische Anatomie*, 1892, Band 39.

[1] Ramsay, NATURE, 1882, vol. xxvi. p. 189.

effects, they are not altogether invisible: their colour is bluish-grey. The only *optical*—that is, strictly *physical*—difference between the several ether-waves in the visible or invisible spectrum is frequency of undulation, or, otherwise expressed, a difference in wave-length. The *chromatic*—that is the colour-producing—effects of the ether-waves depend on their power of exciting sensations of colour, which vary with their frequency of undulation.

Although the retina is extremely sensitive to differences in the frequency of ether-waves, it is not equally so for all parts of the spectrum. In the red and blue portions, the frequency varies considerably without producing marked difference of colour effect, but in the region of yellow and green, comparatively slight variations in frequency produce appreciable differences of colour sensation. One striking difference between the effect of ether-waves on the eye and sound waves on the ear is the absence of anything corresponding to the octave of tone sensation. The ether-waves in the ultra-violet, which have twice the frequency of those of the red end of the spectrum, give rise to no sense of redness, but merely that of a bluish-grey. Even within the octave there are no harmonies or discords of colour sense corresponding to those of tone sensation.

Colours are commonly defined by three qualities or constants,—hue, purity, and brightness. Their hue depends upon the chromatic effect of frequency of undulation or wave length. Their purity or saturation depends on freedom from admixture with sensations produced by other colours or by white light. Their brightness or luminosity depends on the degree to which the sensory mechanism is stimulated. The loudness of sound depends on the amount of excitement produced in the auditory mechanism by the amplitude of sound waves; but a sound with small amplitude of undulation may seem loud when the nerve apparatus is unduly sensitive. The brightest colour of the spectrum is orange-yellow, but it does not follow that the amplitude or energy of the ether-waves is greater than in the region of dull red. There is no physical evidence of greater amplitude in the orange-yellow, and its greater luminosity is no doubt purely subjective, and arises from the greater commotion induced in the sensory mechanism.

The theory of colour sense long ago proposed by Sir Isaac Newton[1] is now commonly treated with what seems to me very undeserved neglect. Newton supposed that the rays of light induce vibrations in the retina which are transmitted by its nerve to the sensorium, and there induce different colour sensations according to the length of the incoming vibrations—the longest producing sensations of red and yellow, the shortest blue and violet, those of medium length a sense of green, and a mixture of them all giving a sense of whiteness. At the beginning of this century Thomas Young proposed a theory which seems to have been intended as a modification of that suggested by Newton rather than as a substitute for it. Young supposed that the ether-waves induce vibrations in the retina "whose frequency must depend on the constitution of its substance; but as it is almost impossible to conceive that each sensitive point of the retina contains an infinite number of particles, each capable of vibrating in unison with every possible undulation, it becomes necessary to suppose the number limited to three primary colours, red, yellow, and blue, and that each sensitive filament of the nerve may consist of three portions, one for each principal colour."[2] Soon afterwards he substituted green for yellow, and violet for blue, so that he came to regard red, green, and violet as the three fundamental colour sensations, by mixture of which in varying proportions, all other colours, including white, are produced. Young believed that his suggestion "simplified the theory of colours, and might therefore be adopted with advantage until found inconsistent with any of the phenomena."

Young's trichromic theory of colour sense was adopted by Clerk-Maxwell and Von Helmholz, and underwent important amplification. Helmholz suggested that the three sets of fibres supposed by Young to exist in the optic nerve are connected with three sets of terminals in the retina; that each terminal contains a different visual substance capable of being decomposed by light; that when the substance in the red nerve terminal undergoes chemical change its nerve fibre is stimulated, and the excitement travels to a cell in the brain by whose specific activity the sensation of red arises. In like manner, when the visual substances in the green and violet terminals are decomposed, nerve impulses travel through different fibres to different cells in the vision centre, by whose specific activities the sensations of green and violet arise. With Helmholz there was no question as to difference in quality of sensation depending on difference in frequency of nerve vibration arriving in the sensorium; no such hypothesis was entertained by him either for tone or for colour sensation. With sight, as with hearing, he supposed that the function of frequency of undulation virtually stops at the nerve terminals in the eye and ear, and that the frequency of undulation of the physical agent has no correlative in the quality of motion passing from the receiving terminal to the sensory cell. He believes that the different frequencies of ether-waves simply excite chemical changes in different nerve terminals. He expressly states[1] that the molecular commotion in the nerve fibres for red, green, and violet is identical in kind, and that its different effects depend on the specific activities of the different cells to which it passes in the sensorium. It is evident that Helmholz entirely dismissed the Newtonian theory of the production of different qualities of colour sense, and substituted for it the doctrine of his own great teacher, Johannes Müller.

The theory of Young and Helmholz offers an explanation of so many facts, and has at the same time provoked so much criticism, that I must enter more fully into some of its details. On this theory, the sense of white or grey is supposed to result from a simultaneous and duly balanced stimulation of the red, green, and violet terminals. The red terminals are supposed to be excited chiefly by the longer waves in the region of the red, orange, and yellow, but also by the shorter undulations extending as far as Fraunhofer's line F at the beginning of the blue. In like manner, the green terminals are excited chiefly by the waves of medium length, and to a less extent by the waves extending to C in the red, and by the shorter waves extending to G in the violet. The violet terminals are stimulated most powerfully by the shorter undulations between F and G, but also by the longer ones reaching as far as D in the yellow; therefore, optically homogeneous light from any part of the spectrum, except its extreme ends, does not usually give rise to a pure colour sensation; all three primary sensations are present, and consequently the colour inclines towards white—the more, the stronger the light.

The experimental facts in support of Young's theory are familiar to all who have studied physics. Compound colour sensations may be produced by causing light of different wave lengths to fall simultaneously or in rapid succession on the same part of the retina. The commonest experimental device is to rapidly whirl discs with sectors of different colours, and observe the results of the mixed sensations; or to cause the images of coloured wafers or papers to fall simultaneously on the retina by Lambert's method; or to transmit light through glass of different colours, and cause the different rays to fall on the same surface; or to mix pure homogeneous light from different parts of the spectrum. For obvious reasons, the last method yields the most trustworthy results. We cannot, by any mixture of homogeneous light from different parts of the spectrum, obtain a pure red or green sensation, and according to Helmholz, the same holds true of violet. On the other hand, a mixture of homogeneous rays from the red and green parts produces orange or yellow, according to the proportions employed. A mixture of rays from the green and violet gives rise to intermediate tints of blue, and a mixture of red and violet light produces purple. Therefore, Young regarded red, green, and violet as primary sensations, and orange, yellow, and blue—just as much as purple—he regarded as secondary or compound sensations. Helmholz discovered that to obtain a sense of white or grey it is not necessary to mingle rays from the red, green, and violet portions of the spectrum. He found that he could obtain a white sensation by mixing only *two* optically homogeneous rays from several parts of the right and left halves of the spectrum. The pairs of spectral colours which he found complementary to each other are, red and greenish-blue, orange and cyan-blue, yellow and ultramarine-blue, greenish-yellow and violet; the complement for pure green being found not in any homogeneous light, but in purple—a mixture of red and violet. The complementary colours may be arranged in a circle, with the complementaries in each pair placed opposite one another. Of course, the circle cannot be completed by the colours of the

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[1] See quotations from Newton made by Young in Reference 2.

[2] Thomas Young, "On the Theory of Light and Colours," *Phil. Trans. Lond.*, 1802, p. 12.

[1] Von Helmholz, *Handbuch der physiologischen Optik*, 2nd edition, 1892, p. 350.

spectrum; purple must be added to fill in the gap between the red and violet. Helmholz found no constant ratios between the wave lengths of homogeneous complementaries; and it is a striking fact that, while a mixture of the green and red, or of the green and violet undulations gives rise to a sensation such as could be produced by rays of intermediate wave length, no such effect follows the mingling of rays from opposite halves of the spectrum. Pure green, with a wave length of 527 millionths of a millimetre, marks the division between the right and left halves. The mixture of blue from the right and yellow from the left side does not produce the intermediate green, but a sensation of white. A mixture of blue or violet and red produces not green, but its complementary—purple. On the trichromic theory, the sense of white produced by the mingling of any of these two colours is simply regarded as the result of a balanced stimulation of the red, green, and violet terminals.

But Young's theory is beset with serious difficulties. It implies the existence of three sets of terminals in the retina, and these must all be found in the central part of the yellow spot where cones alone are present. Three sets of cones there would be necessary to respond to the red, green, and violet light, and a colourless pencil of light could not be seen uncoloured, unless it falls on three cones, which we know from astronomical observations is not the case. Therefore, if there are three different terminals, it seems necessary, in the human retina at all events, that they should be found in every single cone in the yellow spot. But I cannot believe it possible that within a single cone there can be three sets of fibrils capable of simultaneous stimulation in different degrees, and of ultimately transmitting impulses through three different fibres to three different cells in the brain. That would imply a greater number of fibres in the optic nerve, than of terminals in the retina, and we know that precisely the reverse is the case. The anatomical difficulty is therefore great, and I am unable to see how it can be surmounted.

The phenomena of colour-blindness also offer great difficulty. In several cases of apoplectic seizure it has happened that the centre for vision on both sides of the brain has been completely or partially paralyzed by the extravasated blood. In such cases the sense of colour may be entirely lost either for a time or permanently, while the sense of light and form remain—although impaired. The loss of colour sense in some cases has been found complete in both eyes; in most of the recorded cases the loss of colour sense was limited to the right or left halves of both eyes; that is, if the lesion affected the vision centre on the right side of the brain, the right halves of both eyes were blind to all colours. That illustrates the fact that a sense of light does not necessarily imply a sense of colour. The colour sense probably involves a more highly refined action of the sensory cell than the mere sense of light and form, and is on that account more liable to be lost when the nutrition of the sensory cell is interfered with. In the normal eye the peripheral zone of the retina is totally blind to colour. If you turn the right eye outwards, close the left, and then move a strip of coloured paper from the left to the right in front of the nose, the image of the paper will first fall on the peripheral zone of the retina, and its form will be seen, though indistinctly, but not its colour. It is difficult to say in that case whether the colour-blindness is due to the state of the retina or to that portion of the vision centre in the brain associated with it. The absence of cones from the peripheral part of the retina has been assigned as the cause, but it is much more probable that the portion of the vision centre associated with the periphery of the retina, being comparatively little used, is less highly developed for form sensation, and not at all for colour sense. It is evident that the production of a sense of white or grey in the absence of all colour sense is not to be explained on the theory that it results from a balanced stimulation of red, green, and violet nerve terminals.

I need scarcely say that colour-blindness has attracted a large share of attention, not only because of its scientific interest, but still more on account of its practical importance in relation to the correct observation of coloured signals. In 1855 the late Prof. George Wilson,[1] of this city, called attention to the growing importance of the subject. Some years ago Prof. Holmgren made an elaborate statistical inquiry regarding it at the instance of the Swedish Government, and lately it has been investigated by a committee of the Royal Society of London, who have quite recently published their report.[1]

Although colour-blindness occasionally results from disease of the brain, retina, or optic nerve, it is usually congenital. Total colour-blindness is extremely rare, but partial colour-blindness is not uncommon. It occurs in about 4 per cent. of males, but in less than 1 per 1000 of females. Its most common form is termed red-green blindness, in which red and green sensations appear to be absent. So far as I can find, the first full and reliable account of the state of vision in red-green blindness is that given in 1859 by Mr. Pole,[2] of London, from an examination of his own case, which appears to be a typical one. The state of his vision is dichromic; his two-colour sensations are yellow and blue. The red, orange, and yellowish-green parts of the spectrum appear to him yellow of different shades. Greenish-blue and violet appear blue, and between the yellow and blue portions of the spectrum, as it appears to him, there is a colourless grey band in the position of the full green of the ordinary spectrum. This neutral band is seen in the spectrum in all cases of dichromic vision. It may appear white or grey according to the intensity of the light, and it apparently results from an equilibrium of the two sensations; no such band is seen in the spectrum by a normal eye. Mr. Pole, in the account of his case given now three and thirty years ago, considered it impossible to explain his dichromatic vision on the commonly received theory that his sense of red is alone defective, and that his sense of yellow is a compound of blue and green. He believed his green quite as defective as his red sensation, and that yellow and blue are quite as much entitled to be considered fundamental sensations as red and green. He suggested that in normal colour vision there are at least four primary sensations—red and green, yellow and blue. Prof. Hering is commonly accredited with the four-colour theory, but it was previously suggested by Pole.[3]

A year after Pole's paper appeared, Clerk-Maxwell[4] published his celebrated paper on the theory of compound colours, to which he appended an account of his observations on a case of what he believed to be red-blindness, but which we now know must have been red-green blindness. The spectrum appeared dichromic, its only colours being yellow and blue. His description of the case does not materially differ from that given by Pole; but Clerk-Maxwell believed in the trichromic theory of normal vision, and that red-green and blue are the three primary sensations; consequently he supposed that the yellow sensation of a red blind person is not pure yellow, but green.

It is evident that much depends on the question, "Is the yellow sensation of a red-green blind person the same as that of normal vision?" For many years it was impossible to give a definite answer to that question, but the answer can now be given, as we shall immediately see. Colour-blindness is frequently hereditary, and two or three cases are known in which the defective colour sense was limited to one eye, while in the other eye colour vision was normal. In such a case observed by Prof. Hippel, of Giessen, there was red-green blindness in one eye. Holmgren, who examined Hippel's case, has published an account of it.[5] With one eye all the colours of the spectrum were seen, but to the other eye the spectrum had only two colours with a narrow grey band between them at the junction of the blue and yellow. The yellow seen by the eye with the red-green defect had a greenish tinge like that of a lemon, but in other respects the observations confirmed Pole's account of his own case.

Hippel's case seems to me important for another reason. By some it is believed that congenital colour defect is due to the brain. If there had been defective colour sense on one side of the brain, it would not have implicated the whole of one eye, but the half of each eye. Its limitation to one eye, therefore, seems to me to suggest that the fault was in the eye rather than in the brain.

Another interesting fact in this relation is that in every normal eye, just behind the peripheral zone of total colour-blindness, to

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[1] Wilson, "Researches on Colour-Blindness," Edinburgh, 1855.

[1] "Report of the Committee on Colour Vision," Proc. Roy. Soc. Lond., July 1892.

[2] W. Pole, "On Colour-Blindness," Phil. Trans., 1859, vol. cxlix. p. 323.

[3] *Ibid.*, p. 331.

[4] Clerk-Maxwell, "On the Theory of Compound Colours," &c., Phil. Trans., 1860, vol. cl., p. 57.

[5] F. Holmgren, "How do the Colour-Blind see the Different Colours?" Proc. Rov. Soc. Lond., 1881, vol. xxxi. p. 302.

which I have already referred, there is a narrow zone in which red and green sensations are entirely wanting, while blue and yellow sensations are normal. Possibly the red-green defect is due to an imperfectly developed colour sense in the portion of the vision centre connected with that zone of the retina, but Hippel's case seems to me to show that such defect might be on the retina.

It has probably already struck you that red-green blindness is really blindness to red, green, and violet, that Young's three primary sensations appear to be absent, and the two remaining colours are those which he regarded as secondary compounds of his primaries.

That, however, is not all that is revealed by colour-blindness. There is at least another well-known though rare form in which a sense of yellow, blue, and violet is absent, and the only colour sensations present are red and green. The defect is sometimes termed violet blindness, but the term is somewhat misleading. It is much more in accordance with the fact to term it yellow-blue blindness; indeed, we would define it precisely by terming it yellow-blue-violet blindness. Holmgren[1] has recorded a unilateral case of that defect analogous to Hippel's case of unilateral red-green defect; we therefore know definitely how the spectrum appears to such a person. In the case referred to all the colours of the spectrum were seen with the normal eye, but to the other eye the spectrum had only two colours, red and green. The red colour extended over the whole left side of the spectrum to a neutral band in the yellow-green, a little to the right of Fraunhofer's line D. All the right side of the spectrum was green as far as the beginning of the violet, where it "ended with a sharp limit (about the line G)."

If you turn to the Report of the Royal Society's Committee[2] on Colour Vision, you will find the spectrum as it appears to yellow-blue-violet blind persons. The plate agrees with the description of Holmgren's case already given; but you will not find a representation of the spectrum as it appears to those who are red-green blind, and as described by Pole and others. In place of it you will find two dichromic spectra, one with a red and blue half said to be seen by a green blind, the other with a green and a blue half said to be seen by a red blind person. We have copied the spectra for your inspection, and you will observe that yellow does not appear in either of them. I do not for a moment pretend to criticize these spectra from any observations of my own; I am aware Holmgren maintains that red-and-green blindness may occur separately; but, on the other hand, Dr. George Berry, an eminent ophthalmologist, has assured me that he has always found them associated. That statement was originally made by Hering.

Of the various methods of testing colour vision, that suggested by Seebeck is most commonly employed. The individual is mainly tested with regard to his sense of green and red. He is shown skeins of wool, one pale green, another pink or purple, and a third bright red, and he is asked to select from a heap of coloured wools, laid on a white cloth, the colours that appear to him to match those of the several tests. We have arranged such test skeins for your inspection, and have placed beneath each of them the colours which a red-green blind person usually selects as having hues similar to those of the test. It is startling enough to find brown, orange, green, and grey confused with bright red; pale red, orange, yellow, and grey confused with green; blue, violet, and green confused with pink; but these confusions have all their explanation in the fact that the red-green blind have only two colour-sensations—yellow and blue, with a grey band in what should have been the green part of his spectrum.

We have now to show you another and far more beautiful method of ascertaining what fundamental colour sensations are absent in the colour-blind. It is the method of testing them by what Chevreul long ago termed *simultaneous contrast*. If in a semi-darkened room we throw a beam of coloured light on a white screen and interpose an opaque object in its path, the shadow shows the complementary colour. If the light be red, the shadow appears green-blue; if it be green, the shadow appears purple or red according to the nature of the green light employed. If the light is yellow, the shadow is blue; if it is blue, the shadow is yellow. We must remember that the part of the screen on which the shadow falls is not entirely dark; a little diffuse light falls on the retina from the shadowed part, so that the retina and vision centre are slightly stimulated, whereby the image of the shadow.

The experiment can be rendered still more striking, though at the same time a little more complicated, by using two oxy-hydrogen lamps and throwing their light on the same portion of the screen. If a plate of coloured—say ruby—glass is held before one of the lamps, and an opaque object such as the head of a T-square is placed in the path of both lights, the shadow cast by the white light falls on a surface illuminated by a red light, and shows a deep red far more saturated than the surrounding surface of the screen where the red and white lights fall. The shadow cast by the red light shows the complementary bluish green; and the contrast of the two is exceedingly striking.

These experiments we have shown you point to some subtle physiological relations between complementary colours. A colour sensation produced in one part of the vision apparatus forces, so to speak, the neighbouring part, which is relatively quiescent, to produce the complementary colour subjectively. I say *vision-centre* rather than *retina*, because, if one eye is illuminated with coloured light while the other eye is feebly illuminated with white light, the complementary colour appears in the centre belonging to that eye. The sense of white appears to be a mysterious unity; if you *objectively* call up one part of the sensation, you call up its counterpart *subjectively*. If a colour and its complementary counterpart be both displayed objectively at the same time, the action and reaction of effect afford a sensation far more agreeable than is producible by the objective display of only one of them. The agreeableness of the contrast of complementary colours, no doubt, springs from the harmony of effect. There is no harmony of colour effect analogous to that of music, but there is harmony of a different kind, and that harmony is formed by the contrast of complementary colours.

Now I imagine many of you have already anticipated the question, What information can simultaneous contrast give regarding the fundamental sensations of the colour-blind? From an extended series of observations Dr. Stilling,[1] of Cassel, has entertained that if a person cannot distinguish between red and green, no complementary colour appears in the shadow when the inducing light is red or green, but if the inducing light is yellow or blue the proper complementary appears in the shadow. If a person was blind to red he never found the complementary green appear; if he was blind to green, he never found the complementary red appear. When the inducing light appeared colourless, the shadow was also colourless. Stilling therefore concluded that either the sensations of red and green or of blue and yellow were wanting at the same time or all colour sense was absent. It is difficult to see how these results are to be harmonized with the conclusions arrived at by the Committee of the Royal Society.

Facts such as these are regarded by some as lending support to the theory of colour sense proposed by Prof. Hering, of Prague.[2] He supposes that the diversity of our visual perceptions arises from six fundamental sensations constituting three pairs—white and black, red and green, yellow and blue. The three pairs of sensations are supposed to arise from chemical changes in three visual substances not confined to the retina, but contained also in the optic nerve and in the vision centre.[3] He imagines that a sense of white results from *decomposition* induced in a special visual substance by all visible rays, and that the *restitution* of the same substance produces a sense of black. The sensations of the red and green pair are supposed to arise, the one from decomposition, the other from restitution of a second substance; while yellow and blue are supposed to result from decomposition and restitution of a third substance. From our knowledge of photo-chemical processes we can readily suppose that light induces chemical change in the visual apparatus; but that the wave-lengths in the red and yellow parts of the spectrum induce *decomposition*, while the wave-lengths in the green and blue induce *restitution* of substances, it is difficult to believe. How such a visual mechanism could work it would be difficult to comprehend; for example, if we look at a bright red light for a few moments and then close our eyes, the sensation remains for a time, but changes from red to green and then slowly fades away. According to Hering's theory, the green

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[1] F. Holmgren, "How do the Colour-Blind see the Different Colours?" Proc. Roy. Soc. Lond., 1881, vol. xxxi. p. 306.

[2] See Reference 8, Plate I., No. 4.

[1] J. Stilling, "The Present Aspect of the Colour Question," *Archives of Ophthalmology*, 1879, viii. p. 164.

[2] E. Hering, *Zur Lehre vom Lichtsinne*, 2nd ed. Vienna, 1878.

[3] Hering, *ibid.*, p. 75.

after-sensation results from the restitution of a substance decomposed by the red light. But if we reverse the experiment by looking at a bright *green* light and then closing our eyes, the after-sensation changes to *red*. The theory in question would require us to suppose that the green light builds up a visual substance which spontaneously decomposes when the eyes are closed, and so produces the red after-image. I confess that such a hypothesis seems to me incredible. Another remarkable feature of Hering's theory is that colours termed *complementary* ought to be termed *antagonistic*,[1] because they are capable of producing a colourless sensation when mingled in due proportions. If the complementary colours yellow and blue could, when mixed, produce black, they might well be named "antagonistic;" but since their combined effect is a sense of whiteness, and since the addition of them to white light increases its luminosity, it seems very difficult to comprehend on what ground the term *antagonistic* should be substituted for *complementary*. I confess I am quite unable to follow Hering when he supposes that three pairs of mutually antagonistic chemical processes are produced in the retina when white light falls on it, that these processes are all continued on through the optic nerve into the vision centre, and there give rise to our different light and colour sensations.

In 1881 Prof. Preyer[2] advanced a theory of colour sensation, in which he supposes that in the retina there are four sets of cones arranged in pairs—one pair being excitable by the waves in the blue and yellow parts of the spectrum, the other pair being excitable by the red and green. He supposes that each pair of cones is connected with a ganglionic cell in the retina, and through that with one fibre in the optic nerve, which transmits the impulse to at least two cells in the vision centre, in which two different qualities of sensation, red and green, yellow and blue, are severally produced. I confess, however, that I am not able to understand how nerve impulses received, say, from the red terminal of a pair, can specially affect one of the cells in the nerve centre to produce a red sensation. But if the red or green sensation were supposed to arise in the same central cell according to the frequency of the impulses transmitted from either terminal of the pair at the periphery, I should feel that an important difficulty had been removed from Prof. Preyer's theory.

It must be admitted that the production of nerve impulses within the terminals in the retina is almost as obscure as ever. It is still the old question, Does light stimulate the optic terminals by inducing vibration, or by setting up chemical change? Whichever view we adopt, it seems to me necessary to suppose that all the processes for the production of nerve impulses can take place in one and the same visual cell, and are transmitted to the brain through the same nerve fibre; because the image of a coloured star small enough to fall upon only one cone is seen of a fixed and definite colour which does not alter when the position of the eye is changed. It seems to me that if there are special cones for red, green, yellow, and blue, the colour of the star should change when its image falls on different terminals, but I am assured by Mr. Lockyer that such is not the case.

I referred to the sense of smell because it seems to me that we cannot in that case escape from the conclusion that the different sensations arise from different molecular stimulations of the same olfactory terminals.

From Lippmann's recent researches on the photography of colour[3] it appears that all parts of the spectrum can now be photographed on films of albumino-bromide of silver, to which two aniline substances, azaline and cyanine, have been added. It seems, therefore, reasonable to suppose that a relatively small number of substances could enable all the rays of the visible spectrum to affect the retina. Helmholz believes that three visual substances would suffice; but if the primary sensations are to be regarded as four—red, green, yellow, and blue—at least four visual substances appear to be necessary; and I think we must assume that all of them are to be found in the same visual cell in the retina, and that the nerve impulses which their decompositions give rise to are all transmitted through the same optic fibres to the brain cells, there to produce a sense of uncoloured or coloured light. Evidently such a hypothesis is not altogether novel; it is essentially a return to that long ago suggested by Newton. The only difference is that light is supposed to induce photo-chemical changes in the retina, as Von Helmholz suggested, instead of mere mechanical vibration, as Newton supposed. But if in the sense of smell nerve undulations are induced by mechanical vibrations of molecules acting on delicate hairs at the ends of cells, would it, after all, be unreasonable to suppose that within each visual cell there are different kinds of molecules that vibrate in different modes when excited by ether-waves? Four or five sets of such molecules in each terminal element in the retina would probably be sufficient to project successively or simultaneously special forms of undulations through the optic nerve, to induce colour sensations differing according to the wave form of the incoming nerve undulation. It seems to me that the question becomes narrowed down to this: Do the nerve impulses arise from mere vibration or from chemical change in the molecules of the nerve terminal? The photo-chemical hypothesis has much in its favour. We know how rapidly light can induce chemical change in photographic films, and we know that light induces chemical change in the vision-purple in the outer segments of the rod cells in the retina. The fact that the cones contain no vision-purple is no argument against the theory, for the inner segment of both rod and cone is by many regarded as the true nerve terminal, and there is no vision-purple in either of them. The visual substances in the cones, at all events, are colourless, and the existence of them as substances capable of producing nerve impulses by chemical decomposition is as yet only a speculation awaiting proof. The fatigue of the retina produced by bright light is best explained on a chemical theory, but it could also be explained on a mechanical theory, for we must remember that, even if the nerve impulses produced in the visual cells were merely a translation of the energy of light into vibration of nerve molecules, the nerve impulse has to pass through layers of ganglionic cells before reaching the fibres of the optic nerve, and in these cells it probably always induces chemical change. The phenomena of partial colour-blindness could be explained on a photo-chemical theory by supposing that it arises from the absence of the substances required to produce the wave forms necessary for the colour sensation which is defective, but the total colour blindness at the anterior part of the retina is evidently a difficulty. How could we have a sense of light from that portion of the retina if all the visual substances are absent? That is one of the reasons why Hering supposed that a special visual substance is present everywhere in the retina, which by decomposition gives rise to a sense of light as distinguished from colour. But even on the hypothesis we are pursuing, it is not necessary to suppose that all visual substance is absent, for colour-blindness in the front of the retina could be explained by supposing that colour perception has not been developed in the corresponding portion of the vision centre, and consequently all nerve impulses coming from that part of the retina produce scarcely anything more than a sense of light.

If the photo-chemical theory is entertained, it seems necessary to suppose that there is some singular relation between the pairs of substances which respectively give rise to red and green, and yellow and blue, seeing that both members of a pair frequently, if not always, fail together.

It seems to me that the great difficulty arises when we consider the puzzling phenomena of contrast. If light of a particular wave length decomposes a special substance, and gives rise to, say, a sense of red, why does the complementary bluish-green sensation appear in the vision centre around the spot in which the red sensation arises? If the induced colour were a pure green, one might attempt to explain it by supposing that a sympathetic change had been induced in a substance closely related to that suffering decomposition by the objective light, but no such simple explanation is admissible; the complementary contrast of red is not green, but a mixture of green and blue. The inadmissibility of such an explanation becomes still more apparent if we take pure green as the inducing colour—the complementary contrast that appears is purple, which involves a blue or violet, as well as a red sensation. It matters not what inducing colour sensation we adopt, the induced contrast is always the complementary required to make a sense of white. George Wilson[1] long ago suggested that the simultaneous contrast probably arises from a "polar manifestation of force;" indeed, he regarded it as a "true, though unrecognized, manifestation of polarity." It is enough to mention that interesting

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[1] E. Hering, *Zur Lehre vom Lichtsinne*, 2nd ed. p. 121.

[2] W. Preyer, "Über·den Farben und Temperatur Sinn," &c., *Archiv für Physiologie*, 1881, Band xxv., p. 31

[3] G. Lippmann, "On the Photography of Colour," *Comptes Rendus*, 1892, tome 114, p. 961.

[1] Wilson, "Researches on Colour-Blindness," Edinburgh, 1855, p. 179.

suggestion, but I must not pursue it, for we are dealing with a problem that has as yet baffled the wit of man.

I have endeavoured to place before you a subject that involves physical and physiological considerations of extreme difficulty. I have endeavoured to show the nature of these difficulties, and although I have not attempted to solve them, I have at all events tried to show reasons why we should refer our different colour sensations to differences in the incoming nerve impulses rather than to specifically different activities of cells in the visual centre. I have not found it an agreeable task to point out the shortcomings of theories advanced by those for whom I have the deepest regard ; but in the progress of scientific thought it is especially necessary to keep our minds free from the thraldom of established theory, for theories are but the leaves of the tree of science ; they bud and expand, and in time they fade and fall, but they enable the tree to breathe and live. If this address has been full of speculation, I trust you will allow that the scientific use of the imagination is a necessary stimulus to thought, by which alone we can break a path through the dense thicket of the unknown.

SECTION E.

GEOGRAPHY.

OPENING ADDRESS BY PROF. JAMES GEIKIE, LL.D., D.C.L., F.R.SS.L. & E., F.G.S., PRESIDENT OF THE SECTION.

AMONGST the many questions upon which of late years light has been thrown by deep-sea exploration and geological research not the least interesting is that of the geographical development of coast-lines. How is the existing distribution of land and water to be accounted for? Are the revolutions in the relative position of land and sea, to which the geological record bears witness, due to movements of the earth's crust or of the hydrosphere? Why are coast-lines in some regions extremely regular, while elsewhere they are much indented? About 150 years ago the prevalent belief was that ancient sea-margins indicated a formerly higher ocean-level. Such was the view held by Celsius, who, from an examination of the coast-lands of Sweden, attributed the retreat of the sea to a gradual drying up of the latter. But this desiccation hypothesis was not accepted by Playfair, who thought it much more likely that the land had risen. It was not, however, until after Von Buch had visited Sweden (1806–1808), and published the results of his observations, that Playfair's suggestion received much consideration. Von Buch concluded that the apparent retreat of the sea was not due to a general depression of the ocean-level, but to elevation of the land—a conclusion which subsequently obtained the strong support of Lyell. The authority of these celebrated men gained for the elevation theory more or less complete assent, and for many years it has been the orthodox belief of geologists that the ancient sea-margins of Sweden and other lands have resulted from vertical movements of the crust. It has long been admitted, however, that highly flexed and disturbed strata require some other explanation. Obviously such structures are the result of lateral compression and crumpling. Hence geologists have maintained that the mysterious subterranean forces have affected the crust in different ways. Mountain-ranges, they conceive, are ridged up by tangential thrusts and compression, while vast continental areas slowly rise and fall, with little or no disturbance of the strata. From this point of view it is the lithosphere that is unstable, all changes in the relative level of land and sea being due to crustal movements. Of late years, however, Trautschold and others have begun to doubt whether this theory is wholly true, and to maintain that the sea-level may have changed without reference to movements of the lithosphere. Thus Hilber has suggested that sinking of the sea-level may be due, in part at least, to absorption, while Schmick believes that the apparent elevation and depression of continental areas are really the results of grand secular movements of the ocean. The sea, according to him, periodically attains a high level in each hemisphere alternately, the waters being at present heaped up in the southern hemisphere. Prof. Suess, again, believing that in equatorial regions the sea is, upon the whole, gaining on the land, while in other latitudes the reverse would appear to be the case, points out that this is in harmony with his view of a periodical flux and reflux of the ocean between the equator and the poles. He thinks that we have no evidence of any vertical elevation affecting wide areas, and that the only movements of elevation that take place are those by which mountains are upheaved. The broad invasions and transgressions of the continental areas by the sea, which we know have occurred again and again, are attributed by him to secular movements of the hydrosphere itself.

Apart from all hypothesis and theory, we learn that the surface of the sea is not exactly spheroidal. It reaches a higher level on the borders of the continents than in mid-ocean, and it varies likewise in height at different places on the same coast. The attraction of the Himalaya, for example, suffices to cause a difference of 300 feet between the level of the sea at the delta of the Indus and on the coast of Ceylon. The recognition of such facts has led Penck to suggest that the submergence of the maritime regions of North-west Europe and the opposite coasts of North America, which took place at a recent geological date, and from which the lands in question have only partially recovered, may have been brought about by the attraction exerted by the vast ice-sheets of the Glacial Period. But, as Drygalski, Woodward, and others have shown, the heights at which recent marine deposits occur in the regions referred to are much too great to be accounted for by any possible distortion of the hydrosphere. The late James Croll had previously endeavoured to show that the accumulation of ice over northern lands during glacial times would suffice to displace the earth's centre of gravity, and thus cause the sea to rise upon the glaciated tracts. More recently other views have been advanced to explain the apparently causal connection between glaciation and submergence, but these need not be considered here.

Whatever degree of importance may attach to the various hypotheses of secular movements of the sea, it is obvious that the general trends of the world's coast-lines are determined in the first place by the position of the dominant wrinkles of the lithosphere. Even if we concede that all "raised beaches," so called, are not necessarily the result of earth-movements, and that the frequent transgressions of the continental areas by oceanic waters in geological times may possibly have been due to independent movements of the sea, still we must admit that the solid crust of the globe has always been subject to distortion. And this being so, we cannot doubt that the general trends of the world's coast-lines must have been modified from time to time by movements of the lithosphere.

As geographers we are not immediately concerned with the mode of origin of those vast wrinkles, nor need we speculate on the causes which may have determined their direction. It seems, however, to be the general opinion that the configuration of the lithosphere is due simply to the sinking in and crumpling up of the crust on the cooling and contracting nucleus. But it must be admitted that neither physicists nor geologists are prepared with a satisfactory hypothesis to account for the prominent trends of the great world-ridges and troughs. According to the late Prof. Alexander Winchell, these trends may have been the result of primitive tidal action. He was of opinion that the transmeridional progress of the tidal swell in early incrustive times on our planet would give the forming crust structural characteristics and aptitudes trending from north to south. The earliest wrinkles to come into existence, therefore, would be meridional or submeridional, and such, certainly, is the prevalent direction of the most conspicuous earth-features. There are many terrestrial trends, however, as Prof. Winchell knew, which do not conform to the requirements of his hypothesis ; but such transmeridional features, he thought, could generally be shown to be of later origin than the others. This is the only speculation, so far as I know, which attempts, perhaps not altogether unsuccessfully, to explain the origin of the main trends of terrestrial features. According to other authorities, however, the area of the earth's crust occupied by the ocean is denser than that over which the continental regions are spread. The depressed denser part balances the lighter elevated portion. But why these regions of different densities should be so distributed no one has yet told us. Neither does Le Conte's view, that the continental areas and the oceanic depressions owe their origin to unequal radial contraction of the earth in its secular cooling, help us to understand why the larger features of the globe should be disposed as they are.

Geographers must for the present be content to take the world as they find it. What we do know is that our lands are distributed over the surface of a great continental plateau of irregular form, the bounding slopes of which plunge down more or less steeply into a vast oceanic depression. So far as geological research had gone, there is reason to believe that these elevated and depressed areas are of primeval antiquity—that they antedate the very oldest of the sedimentary formations. There is abundant evidence, however, to show that the relatively elevated or continental area has been again and again irregularly submerged under tolerably deep and wide seas. But all historical geology assures us that the continental plateau and the oceanic hollows have never changed places, although from time to time portions of the latter have been ridged up and added to the margins of the former, while ever and anon marginal portions of the plateau have sunk down to very considerable depths. We may thus speak of the great world-ridges as regions of dominant elevation, and of the profound oceanic troughs as areas of more or less persistent depression. From one point of view, it is true, no part of the earth's surface can be looked upon as a region of dominant elevation. Our globe is a cooling and contracting body, and depression must always be the prevailing movement of the lithosphere. The elevation of the continental plateau is thus only relative. Could we conceive the crust throughout the deeper portions of the oceanic depression to subside to still greater depths, while at the same time the continental plateau remained stationary, or subsided more slowly, the sea would necessarily retreat from the land, and the latter would then appear to rise. It is improbable, however, that any extensive subsidence of the crust under the ocean could take place without accompanying disturbance of the continental plateau ; and in this case the latter might experience in places not only negative but positive elevation. During the evolution of our continent, crustal movements have again and again disturbed the relative level of land and sea, but since the general result has been to increase the land surface and to contract the area occupied by the sea, it is convenient to speak of the former as the region of dominant elevation, and of the latter as that of prevalent depression. Properly speaking, both are sinking regions, the rate of subsidence within the oceanic trough being in excess of that experienced over the continental plateau. The question of the geographical development of coast-lines is therefore only that of the dry lands themselves.

The greater land masses are all situated upon, but are nowhere coextensive with, the area of dominant elevation, for very considerable portions of the continental plateau are still covered by the sea. Opinions may differ as to which fathoms line we should take as marking approximately the boundary between that region and the oceanic depression ; and it is obvious, indeed, that any line selected must be arbitrary and more or less misleading, for it is quite certain that the true boundary of the continental plateau cannot lie parallel to the surface of the ocean. In some regions it approaches within a few hundreds of fathoms of the sea-level ; in other places it sinks for considerably more than 1000 fathoms below that level. Thus, while a very moderate elevation would in certain latitudes cause the land to extend to the edge of the plateau, an elevation of at least 10,000 feet would be required in some other places to bring about a similar result.

Although it is true that the land surface is nowhere coextensive with the great plateau, yet the existing coast-lines may be said to trend in the same general direction as its margins. So abruptly does the continental plateau rise from the oceanic trough, that a depression of the sea-level, or an elevation of the plateau, for 10,000 feet, would add only a narrow belt to the Pacific coast between Alaska and Cape Horn, while the gain of land on the Atlantic slope of America between 30° N.L. and 40° S.L. would not be much greater. In the higher latitudes of the Northern Hemisphere, however, very considerable geographical changes would be accomplished by a much less amount of elevation of the plateau. Were the continental plateau to be upheaved for 3000 feet, the major portion of the Arctic Sea would become land. Thus, in general terms, we may say that the coast-lines of Arctic and temperate North America and Eurasia are further withdrawn from the edge of the continental plateau than those of lower latitudes.

In regions where existing coast-lines approach the margin of the plateau, they are apt to run for long distances in one determinate direction, and whether the coastal area be high or not, to show a gentle sinuosity. Their course is seldom interrupted by bold projecting headlands or peninsulas, or by intruding inlets, while fringing or marginal islands rarely occur. To these appearances the northern regions, as every one knows, offer the strongest contrast. Not only do they trend irregularly, but their continuity is constantly interrupted by promontories and peninsulas, by inlets and fords, while fringing islands abound. But an elevation of some 400 or 500 fathoms only would revolutionize the geography of those regions, and confer upon the northern coast-lines of the world the regularity which at present characterizes those of Western Africa.

It is obvious, therefore, that the coast-lines of such lands as Africa owe their regularity primarily to their approximate coincidence with the steep boundary slopes of the continental plateau, while the irregularities characteristic of the coast-line of North-Western Europe and the corresponding latitudes of North America are determined by the superficial configuration of the same plateau, which in those regions is relatively more depressed. I have spoken of the general contrast between high and low northern latitudes, but it is needless to say that in southern regions the coast-lines exhibit similar contrasts. The regular coast-lines of Africa and South America have already been referred to, but we cannot fail to recognize in the much indented sea-board and the numerous coastal island of Southern Chili a complete analogy to the fiord regions of high northern latitudes. Both are areas of comparatively recent depression. Again, the manifold irregularities of the coasts of South-eastern Asia, and the multitudes of islands thai serve to to link that continent to Australia and New Zealand, are all evidence that the surface of the continental plateau in those regions is extensively invaded by the sea.

A word or two now as to the configuration of the oceanic trough. There can be no doubt that this differs very considerably from that of the land surface. It is, upon the whole, flat or gently undulating. Here and there it swells gently upwards into broad elevated banks, some of which have been traced for great distances. In other places narrower ridges and abrupt mountain-like elevations diversify its surface, and project again and again above the level of the sea, to form the numerous islets of Oceania. Once more, the sounding-line has made us acquainted with the notable fact that numerous deep depressions—some long and narrow, others relatively short and broad—stud the floor of the great trough. I shall have occasion to refer again to these remarkable depressions, and need at present only call attention to the fact that they are especially well developed in the region of the Western Pacific, where the floor of the sea, at the base of the bounding slopes of the continental plateau, sinks in places to depths of three and even of five miles below the existing coast-lines. One may further note the fact that the deepest areas of the Atlantic are met with in like manner close to the walls of the plateau—a long ridge, which rises midway between the continents and runs in the same general direction as their coast-lines, serving to divide the trough of the Atlantic into two parallel hollows.

But, to return to our coast-lines and the question of their development, it is obvious that their general trends have been determined by crustal movements. Their regularity is in direct proportion to the closeness of their approach to the margin of the continental plateau. The more nearly they coincide with the edge of that plateau, the fewer irregularities do they present ; the further they recede from it, the more highly are they indented. Various other factors, it is true, have played a more or less important part in their development, but their dominant trends were undoubtedly determined at a very early period in the world's history—their determination necessarily dates back, in short, to the time when the great world-ridges and oceanic troughs came into existence. So far as we can read the story told by the rocks, however, it would seem that in the earliest ages of which geology can speak with any confidence, the coast-lines of the world must have been infinitely more irregular than now. In Palæozoic times, relatively small areas of the continental plateau appeared above the level of the sea. Insular conditions everywhere prevailed. But as ages rolled on wider and wider tracts of the ptateau were exposed, and this notwithstanding many oscillations of level. So that one may say there has been upon the whole a general advance from insular to continental conditions. In other words, the sea has continued to retreat from the surface of the continental plateau. To account for this change we must suppose that depression of the crust has been in excess within the .oceanic area, and that now and again positive elevation of the continental plateau has taken place, more especially along its margins. That movements of elevation, positive or negative, have again and again affected our land areas can be demonstrated, and it seems highly probable, therefore, that similar movements may have been experienced with the oceanic trough.

Two kinds of crustal movement, as we have seen, are recognized by geologists. Sometimes the crust appears to rise, or, as the case may be, to sink over wide regions, without much disturbance or tilting of strata, although these are now and again more or less extensively fractured and displaced. It may conduce to clearness if we speak of these movements as regional. The other kind of crustal disturbance takes place more markedly in linear directions, and is always accompanied by abrupt folding and mashing together of strata, along with more or less fracturing and displacement. The plateau of the Colorado has often been cited as a good example of regional elevation, where we have a wide area of approximately horizontal strata apparently uplifted without much rock-disturbance, while the Alps or any other chain of highly flexed and convoluted strata will serve as an example of what we may term axial or linear uplifts. It must be understood that both regional and axial movements result from the same cause—the adjustment of the solid crust to the contracting nucleus—and that the term *elevation*, therefore, is only relative. Sometimes the sinking crust gets relief from the enormous lateral pressure to which it is subjected by ridging up along lines of weakness, and then mountains of elevation are formed; at other times, the pressure is relieved by the formation of broader swellings, when wide areas become uplifted relatively to surrounding regions. Geologists, however, are beginning to doubt whether upheaval of the latter kind can affect a broad continental area. Probably in most cases, the apparent elevation of continental regions is only negative. The land appears to have risen because the floor of the oceanic basin has become depressed. Even the smaller plateau-like elevations which occur within some continental regions may in a similar way owe their dominance to the sinking of contiguous regions.

In the geographical development of our land, movements of elevation and depression have played an important part. But we cannot ignore the work done by other agents of change. If the orographical features of the land everywhere attest the potency of plutonic agents, they no less forcibly assure us that the inequalities of surface resulting from such movement are universally modified by denudation and sedimentation. Elevated plains and mountains are gradually demolished, and the hollows and depressions of the great continental plateau become slowly filled with their detritus. Thus inland seas tend to vanish, inlets and estuaries are silted up, and the land in places advances seaward. The energies of the sea, again, come in to aid those of rain and river, so that under the combined action of all the superficial agents of change, the irregularities of coast-lines become reduced, and, were no crustal movement to intervene, would eventually disappear. The work accomplished by those agents upon a coast-line is most conspicuous in regions where the surface of the continental plateau is occupied by comparatively shallow seas. Here full play is given to sedimentation and marine erosion, while the latter alone comes into prominence upon shores that are washed by deeper waters. When the coast-lines advance to the edge of the continental plateau, they naturally trend, as we have seen, for great distances in some particular direction. Should they preserve that position, undisturbed by crustal oscillation, for a prolonged period of time, they will eventually be cut back by the sea. In this way a shelf or terrace will be formed, narrow in some places, broader in others, according to the resistance offered by the varying character of the rocks. But no long inlets or fiords can result from such action. At most the harder and less readily demolished rocks will form headlands, while shallow bays will be scooped out of the more yielding masses. In short, between the narrower and broader parts of the eroded shelf or terrace a certain proportion will tend to be preserved. As the shelf is widened, sedimentation will become more and more effective, and in places may come to protect the land from further marine erosion. This action is especially conspicuous in tropical and subtropical regions, which are characterized by well-marked rainy seasons. In such regions immense quantities of sediment are washed down from the land to the sea, and tend to accumulate along shore, forming low alluvial flats. All long-established coast-lines thus acquire a characteristically sinuous form, and perhaps no better examples could be cited than those of Western Africa.

To sum up, then, we may say that the chief agents concerned in the development of coast-lines are crustal movements, sedimentation, and marine erosion. All the main trends are the result of elevation and depression. Considerable geographical changes, however, have been brought about by the silting up of those shallow and sheltered seas which, in certain regions, overflow wide areas of the continental plateau. Throughout all the ages, indeed, epigene agents have striven to reduce the superficial inequalities of that plateau, by levelling heights and filling up depressions, and thus, as it were, flattening out the land surface and causing it to extend. The erosive action of the sea, from our present point of view, is of comparatively little importance. It merely adds a few finishing touches to the work performed by the other agents of change.

A glance at the geographical evolution of our own continent will render this sufficiently evident. Viewed in detail, the structure of Europe is exceedingly complicated, but there are certain leading features in its architecture which no profound analysis is required to detect. We note, in the first place, that highly disturbed rocks of Archæan and Palæozoic age reach their greatest development along the north-western and western borders of our continent, as in Scandinavia, the British Islands, North-west France, and the Iberian peninsula. Another belt of similarly disturbed strata of like age traverses Central Europe from west to east, and is seen in the South of Ireland, Cornwall, North-west France, the Ardennes, the Thüringerwald, the Erzgebirge, the Riesengebirge, the Böhmerwald, and other heights of Middle and Southern Germany. Strata of Mesozoic and Cainozoic age rest upon the older systems in such a way as to show that the latter had been much folded, fractured, and denuded before they came to be covered with younger formations. North and north-east of the central belt of ancient rocks just referred to, the sedimentary strata that extend to the shores of the Baltic and over a vast region in Russia, range in age from Palæozoic down to Cainozoic times, and are disposed for the most part in gentle undulations—they are either approximately horizontal or slightly inclined. Unlike the disturbed rocks of the maritime regions and of Central Europe, they have obviously been subjected to comparatively little folding since the time of their deposition. To the south of the primitive backbone of Central Europe succeeds a region composed superficially of Mesozoic and Cainozoic strata for the most part, which, along with underlying Palæozoic and Archæan rocks, are often highly flexed and ridged up, as in the chains of the Jura, the Alps, the Carpathians, &c. One may say, in general terms, that throughout the whole Mediterranean area Archæan and Palæozoic rocks appear at the surface only when they form the nuclei of mountains of elevation into the composition of which rocks of younger age largely enter.

From this bald and meagre outline of the general geological structure of Europe, we may gather that the leading orographical features of our continent began to be developed at a very early period. Unquestionably the oldest land areas are represented by the disturbed Archæan and Palæozoic rocks of the Atlantic sea-board and Central Europe. Examination of those tracts shows that they have experienced excessive denudation. The Archæan and Palæozoic masses, distributed along the margin of the Atlantic, are the mere wrecks of what, in earlier ages, must have been lofty regions, the mountain-chains of which may well have rivalled or even exceeded in height the Alps of to-day. They, together with the old disturbed rocks of Central Europe, formed for a long time the only land in our area. Between the ancient Scandinavian tract in the north and a narrow interrupted belt in Central Europe stretched a shallow sea, which covered all the regions that now form our Great Plain; while immediately south of the central belt lay the wide depression of the Mediterranean—for as yet the Pyrenees, the Alps, and the Carpathians were not. Both the Mediterranean and the Russo-Germanic sea communicated with the Atlantic. As time went on, land continued to be developed along the same lines, a result due partly to crustal movements, partly to sedimentation. Thus by and by the relatively shallow Russo-Germanic sea became silted up, while the Mediterranean shore-line advanced southwards. It is interesting to note that the latter sea, down to the close of Tertiary times, seems always to have communicated freely with the Atlantic, and to have been relatively deep. The Russo-Germanic sea, on the contrary, while now and again opening widely into the Atlantic, and attaining considerable depths in its western reaches, remained on the whole shallow, and ever and anon vanished from wide areas to contract into a series of inland seas and large salt lakes.

Reduced to its simplest elements, therefore, the structure of Europe shows two primitive ridges—one extending with some interruptions along the Atlantic sea-board, the other traversing Central Europe from west to east, and separating the area of the Great Plain from the Mediterranean basin. The excessive denudation which the more ancient lands have undergone, and great uplifts of Mesozoic and of Cainozoic times, together with the comparatively recent submergence of broad tracts in the north and north-west, have not succeeded in obscuring the dominant features in the architecture of our continent.

I now proceed to trace, as rapidly as I can, the geographical development of the coast-lines of the Atlantic as a whole, and to point out the chief contrasts between them and those of the Pacific. The extreme irregularity of the Arctic and Atlantic shores of Europe at once suggests to a geologist a partially drowned land, the superficial inequalities of which are accountable for the vagaries of the coast-lines. The fiords of Norway and Scotland occupy what were at no distant date land valleys, and the numerous marginal islands of those regions are merely the projecting portions of a recently sunken area. The continental plateau extends up to and a little beyond the one hundred fathoms line, and there are many indications that the land formerly reached as far. Thus the sunken area is traversed by valley-like depressions, which widen as they pass out to the edge of the plateau, and have all the appearance of being hollows of subaerial erosion. I have already mentioned the fact that the Scandinavian uplands and the Scottish Highlands are the relics of what were at one time true mountains of elevation, corresponding in the mode of their formation to those of Switzerland, and, like these, attaining a great elevation. During subsequent stages of Palæozoic time, that highly elevated region was subjected to long-continued and profound erosion—the mountain country was planed down over wide regions to sea-level, and broad stretches of the reduced land surface became submerged. Younger Palæozoic formations now accumulated upon the drowned land, until eventually renewed crustal disturbance supervened, and the marginal areas of the continental plateau again appeared as dry land, but not, as before, in the form of mountains of elevation. Lofty table-lands now took the place of abrupt and serrated ranges and chains—table-lands which, in their turn, were destined in the course of long ages to be deeply sculptured and furrowed by subaerial agents. During this process the European coast-line would seem to have coincided more or less closely with the edge of the continental plateau. Finally, after many subsequent movements of the crust in these latitudes, the land became partially submerged—a condition from which North-western and Northern Europe would appear in recent times to be slowly recovering. Thus the highly indented coast-line of those regions does not coincide with the edge of the plateau, but with those irregularities of its upper surface which are the result of antecedent subaerial erosion.

Mention has been made of the Russo-Germanic plain and the Mediterranean as representing original depressions in the continental plateau, and of the high grounds that extend between them as regions of dominant elevation, which, throughout all the manifold revolutions of the past, would appear to have persisted as a more or less well-marked boundary, separating the northern from the southern basin. During certain periods it was no doubt in some degree submerged, but never apparently to the same extent as the depressed areas it served to separate. From time to time uplifts continued to take place along this central belt, which thus increased in breadth, the younger formations, which were accumulated along the margins of the two basins, being successively ridged up against nuclei of older rocks. The latest great crustal movements in our continent, resulting in the uplift of the Alps and other east and west ranges of similar age, have still further widened that ancient belt of dominant elevation which in our day forms the most marked orographical feature of Europe.

The Russo-Germanic basin is now for the most part land, the Baltic and the North Sea representing its still submerged portions. This basin, as already remarked, was probably never so deep as that of the Mediterranean. We gather as much from the fact that, while mechanical sediments of comparatively shallow-water origin predominate in the former area, limestones are the characteristic features of the southern region. Its relative shallowness helps us to understand why the northern depression should have been silted up more completely than the Mediterranean. We must remember also that for long ages it received the drainage of a much more extensive land surface than the latter—the land that sloped towards the Mediterranean in Palæozoic and Mesozoic times being of relatively little importance. Thus the crustal movements which ever and anon depressed the Russo-Germanic area were, in the long run, counterbalanced by sedimentation. The uplift of the Alps, the Atlas, and other east and west ranges, has greatly contracted the area of the Mediterranean, and sedimentation has also acted in the same direction, but it is highly probable that that sea is now as deep as, or even deeper than, it has ever been. It occupies a primitive depression in which the rate of subsidence has exceeded that of sedimentation. In many respects, indeed, this remarkable transmeridional hollow—continued eastward in the Red Sea, the Black Sea, and the Aralo-Caspian depression—is analogous, as we shall see, to the great oceanic trough itself.

In the earlier geological periods linear or axial uplifts and volcanic action again and again marked the growth of the land on the Atlantic sea-board. But after Palæozoic times, no great mountains of elevation came into existence in that region, while volcanic action almost ceased. In Tertiary times, it is true, there was a remarkable recrudescence of volcanic activity, but the massive eruptions of Antrim and Western Scotland, of the Færöe Islands and Iceland, must be considered apart from the general geology of our continent. From Mesozoic times onwards it was along the borders of the Mediterranean depression that great mountain uplifts and volcanoes chiefly presented themselves. And as the land surface extended southwards from Central Europe, and the area of the Mediterranean was contracted, volcanic action followed the advancing shore-lines. The occurrence of numerous extinct and of still existing volcanoes along the borders of this inland sea, the evidence of recent crustal movements so commonly met with upon its margins, the great irregularities of its depths, the proximity of vast axial uplifts of late geological age, and the frequency of earthquake phenomena, all indicate instability, and remind us strongly of similarly constructed and disturbed regions within the area of the vast Pacific.

Let us now look at the Arctic and Antarctic coast-lines of North America. From the extreme north down to the latitude of New York the shores are obviously those of a partially submerged region. They are of the same type as the coasts of North-western Europe. We have every reason to believe also that the depression of Greenland and North-east America, from which these lands have only partially recovered, dates back to a comparatively recent period. The fiords, and inlets, like those of Europe, are merely half-drowned land valleys, and the continental shelf is crossed by deep hollows which are evidently only the seaward continuations of well-marked terrestrial features. Such, for example, is the case with the valleys of the Hudson and the St. Lawrence, the submerged portions of which can be followed out to the edge of the continental plateau, which is notched by them at depths of 474 and 622 fathoms respectively. There is, in short, a broad resemblance between the coasts of the entire Arctic and North Atlantic regions down to the latitudes already mentioned. Everywhere they are irregular and fringed with islands in less or greater abundance—highly denuded and deeply incised plateaus being penetrated by fiords, while low-lying and undulating lands that shelve gently seaward are invaded by shallow bays and inlets. Comparing the American with the opposite European coasts, one cannot help being struck with certain other resemblances. Thus Hudson Bay at once suggests the Baltic, and the Gulf of Mexico, with the Caribbean Sea, recall the Mediterranean. But the geological structure of the coast-lands of Greenland and North America betrays a much closer resemblance between these and the opposite shores of Europe than appears on a glance at the map. There is something more than a mere superficial similarity. In eastern North America and Greenland, just as in Western Europe, no grand mountain uplifts have taken place for a prodigious time. The latest great upheavals, which were accompanied by much folding and flexing of strata, are those of the Appalachian chain and of the coastal ranges extending through New England, Nova Scotia, and Newfoundland, all of which are of Palæozoic age. Considerable crustal movements affected the American coast-lands in Mesozoic times, and during these uplifts the strata suffered fracture and displacement, but were subjected to comparatively little folding. Again, along the maritime borders of North-east America, as in the corresponding coast-lands of Europe, igneous action, more or less abundant in Palæozoic and early Mesozoic times, has since been quiescent. From the mouth of the Hudson to the Straits of Florida the coast-lands are composed of Tertiary and Quaternary deposits. This shows that the land has continued down to recent times to gain upon the sea—a result brought about partly by quiet crustal movements, but to a large extent by sedimentation, aided, on the coasts of Florida, by the action of reef-building corals.

Although volcanic action has long ceased on the American sea-board, we note that in Greenland, as in the West of Scotland and North of Ireland, there is abundant evidence of volcanic activity at so late a period as the Tertiary. It would appear that the great plateau-basalts of those regions, and of Iceland and the Færöe Islands, were contemporaneous, and possibly connected with an important crustal movement. It has long been suggested that at a very early geological period Europe and North America may have been united. The great thickness attained by the Palæozoic rocks in the eastern areas of the latter implies the existence of a wide land surface from which ancient sediments were derived. That old land must have extended beyond the existing coast-line, but how far we cannot tell. Similarly in North-west Europe, during early Palæozoic times, the land probably stretched further into the Atlantic than at present. But whether, as some think, an actual land connection subsisted between the two continents it is impossible to say. Some such connection was formerly supposed necessary to account for the emigration and immigration of certain marine forms of life which are common to the Palæozoic strata of both continents, and which, as they were probably denizens of comparatively shallow water, could only have crossed from one area to another along a shore-line. It is obvious, indeed, that if the oceanic troughs in those early days were of an abysmal character, a land bridge would be required to explain the geographical distribution of cosmopolitan life-forms. But if it be true that subsidence of the crust has been going on through all geological time, and that the land areas have notwithstanding continued to extend over the continental plateau, then it follows that the oceanic trough must be deeper now than it was in Palæozoic times. There are, moreover, certain geological facts which seem hardly explicable on the assumption that the seas of past ages attained abysmal depths over any extensive areas. The Palæozoic strata which enter so largely into the framework of our lands have much the same appearance all the world over, and were accumulated for the most part in comparatively shallow water. A petrographical description of the Palæozoic mechanical sediments of Europe would serve almost equally well for those of America, of Asia, or of Australia. Take in connection with this the fact that Palæozoic faunas had a very much wider range than those of Mesozoic and later ages, and were characterized above all by the presence of many cosmopolitan species, and we can hardly resist the conclusion that it was the comparative shallowness of the ancient seas that favoured that wide dispersal of species, and enabled currents to distribute sediments the same in kind over such vast regions. As the oceanic area deepened and contracted, and the land surface increased, marine faunas were gradually restricted in their range, and cosmopolitan marine faunas diminished in numbers, while sediments, gathering in separate regions, became more and more differentiated. For these and other reasons, which need not be entered upon here, I see no necessity for supposing that a Palæozoic Atlantis connected Europe with North America. The broad ridge upon which the Færöe Islands and Iceland are founded, seems to pertain as truly to the oceanic depression as the long Dolphin Ridge of the South Atlantic. The trend of the continental plateau in high latitudes is shown, as I think, by the general direction of the coast-lines of North-Western Europe and East Greenland, the continental shelf being submerged in those regions for a few hundred fathoms only. How the Icelandic ridge came into existence, and what its age may be, we can only conjecture. It may be a wrinkle as old as the oceanic trough which it traverses, or its origin may date back to a much more recent period. We may conceive it to be an area which has subsided more slowly than the floor of the ocean to the north and south; or, on the other hand, it may be a belt of positive elevation. Perhaps the latter is the more probable supposition, for it seems very unlikely that crustal disturbances, resulting in axial and regional uplifts, should have been confined to the continental plateau only. Be that as it may, there seems little doubt that land connection did obtain between Greenland and Europe in Cainozoic times, along this Icelandic ridge, for relics of the same Tertiary flora are found in Scotland, the Færöe Islands, Iceland, and Greenland. The deposits in which these plant-remains occur are associated with great sheets of volcanic rocks which in the Færöe Islands and Iceland reach a thickness of many thousand feet. Of the same age are the massive basalts of Jan Mayen, Spitzbergen, Franz Joseph Land, and Greenland. These lavas seem seldom to have issued from isolated foci in the manner of modern eruptions, but rather to have welled up along the lines of rectilineal fissures. From the analogy of similar phenomena in other parts of the world it might be inferred that the volcanic action of these northern regions may have been connected with a movement of elevation, and that the Icelandic ridge, if it did not come into existence during the Tertiary period, was at all events greatly upheaved at that time. It would seem most likely, in short, that the volcanic action in question was connected mainly with crustal movements in the oceanic trough. Similar phenomena, as is well known, are met with further south in the trough of the Atlantic. Thus the volcanic Azores rise like Iceland from the surface of a broad ridge which is separated from the continental plateau by wide and deep depressions. And so again, from the back of the great Dolphin Ridge, spring the volcanic islets of St. Paul's, Ascension, and Tristan d'Acunha.

I have treated of the Icelandic bank at some length for the purpose of showing that its volcanic phenomena do not really form an exception to the rule that such eruptions ceased after Palæozoic or early Mesozoic times to disturb the Atlantic coast-lines of Europe and North America. As the bank in question extends between Greenland and the British Islands, it was only natural that both those regions should be affected by its movements. But its history pertains essentially to that of the Atlantic trough; and it seems to show how transmeridional movements of the crust, accompanied by vast discharges of igneous rock, may come in time to form land connections between what are now widely separated areas.

Let us next turn our attention to the coast-lines of the Gulf of Mexico and the Caribbean Sea. These enclosed seas have frequently been compared to the Mediterranean, and the resemblance is self-evident. Indeed, it is so close that one may say the Mexican-Caribbean Sea and the Mediterranean are rather homologous than simply analogous. The latter, as we have seen, occupies a primitive depression, and formerly covered a much wider area. It extended at one time over much of Southern Europe and Northern Africa, and appears to have had full communication across Asia Minor with the Indian Ocean, and with the Arctic Ocean athwart the low-lying tracts of North-Western Asia. Similarly, it would seem, the Mexican-Caribbean Sea is the remaining portion of an ancient inland sea which formerly stretched north through the heart of North America to the Arctic Ocean. Like its European parallel, it has been diminished by sedimentation and crustal movements. It resembles the latter also in the greatness and irregularity of its depths, and in the evidence which its islands supply of volcanic action as well as of very considerable crustal movements within geological times. Along the whole northern borders of the Gulf of Mexico the coast-lands, like those on the Atlantic seaboard of the Southern States, are composed of Tertiary and recent accumulations, and the same is the case with Yucatan; while similar young formations are met with on the borders of the Caribbean Sea and the Antilles. The Bahamas and the Windward Islands mark out for us the margin of the continental plateau, which here falls away abruptly to profound depths. One feels assured that this portion of the plateau has been ridged up to its present level at no distant geological date. But notwithstanding all the evidence of recent extensive crustal movements in this region, it is obvious that the Mexican-Caribbean depression, however much it may have been subsequently modified, is of primitive origin.[1]

Before we leave the coast-lands of North America, I would again point out their leading geological features. In a word, then, they are composed for the most part of Archæan and Palæozoic rocks; no great linear or axial uplifts marked by much flexure of strata have taken place in those regions since Palæozoic times; while igneous action virtually ceased about the close of the Palæozoic or the commencement of the Mesozoic period. It is not before we reach the shores of the Southern States and the coast-lands of the Mexican-Caribbean Sea that we encounter notable accumulations of Mesozoic, Tertiary, and younger age. These occur in approximately horizontal positions

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[1] Professor Suess thinks it is probable that the Caribbean Sea and the Mediterranean are portions of one and the same primitive depression which traversed the Atlantic area in early Cretaceous times. He further suggests that it may have been through the gradual widening of this central Mediterranean that the Atlantic in later times came into existence.

round the Gulf of Mexico, but in the Sierra Nevada or Northern Colombia and the Cordilleras of Venezuela Tertiary strata are ridged up into true mountains of elevation. Thus the Mexican-Caribbean depression, like that of the Mediterranean, is characterized not only by its irregular depths and its volcanic phenomena, but by the propinquity of recent mountains of upheaval, which bear the same relation to the Caribbean Sea that the mountains of North Africa do to the Mediterranean.

We may now compare the Atlantic coasts of South America with those of Africa. The former coincide in general direction with the edge of the continental plateau, to which they closely approach between Cape St. Roque and Cape Frio. In the north-east, between Cape Paria, opposite Trinidad, and Cape St. Roque, the continental shelf attains a considerably greater breadth, while south of Cape Frio it gradually widens, until, in the extreme south, it runs out towards the east in the form of a narrow ridge, upon the top of which rise the Falkland Islands and South Georgia. Excluding from consideration for the present all recent alluvial and Tertiary deposits, we may say that the coast lands from Venezuela down to the South of Brazil are composed principally of Archæan rocks; the eastern borders of the continent further south being formed of Quaternary and Tertiary accumulations. So far as we know, igneous rocks are of rare occurrence on the Atlantic sea-board. Palæozoic strata approach the coast-lands at various points between the mouths of the Amazons and La Plata, and these, with the underlying and surrounding Archæan rocks, are more or less folded and disturbed, while the younger strata of Mesozoic and Cainozoic age (occupying wide regions in the basin of of the Amazons, and here and there fringing the sea-coast), occur in approximately horizontal positions. It would appear, therefore, that no great axial uplifts have taken place in those regions since Palæozoic times. The crustal movements of later ages were regional rather than axial; the younger rocks are not flexed and mashed together, and their elevation (negative or positive) does not seem to have been accompanied by conspicuous volcanic action.

The varying width of the continental shelf is due to several causes. The Orinoco, the Amazons, and other rivers descending to the north-west coast, carry enormous quantities of sediment, much of which comes to rest on the submerged slopes of the continental plateau, so that the continental shelf tends to extend seawards. The same process takes place on the south-east coast, where the River Plate discharges its muddy waters. South of latitude 40° S., however, another cause has come into play. From the mouth of the Rio Negro to the terminal point of the continent the whole character of the coast betokens a geologically recent emergence, accompanied and followed by considerable marine erosion. So that in this region the continental shelf increases in width by the retreat of the coast-line, while in the north-east it gains by advancing seawards. It is to be noted, however, that even there, in places where the shores are formed of alluvia, the sea tends to encroach upon the land.

The Atlantic coast of Africa resembles that of South America in certain respects, but it also offers some important contrasts. As the northern coasts of Venezuela and Colombia must be considered in relation rather to the Caribbean depression than to the Atlantic, so the African sea-board between Cape Spartel and Cape Nun pertains structurally to the Mediterranean region. From the southern limits of Morocco to Cape Colony the coastal heights are composed chiefly of Archæan and Palæozoic rocks, the low shore-lands showing here and there strata of Mesozoic and Tertiary age together with still more recent deposits. The existing coast-lines everywhere advance close to the edge of the continental plateau, so that the submarine shelf is relatively narrower than that of Eastern South America. The African coast is still further distinguished from that of South America by the presence of several groups of volcanic islands—Fernando Po and others in the Gulf of Guinea, and Cape Verde and Canary Islands. The last-named group, however, notwithstanding its geographical position, is probably related rather to the Mediterranean depression than to the Atlantic trough.

The geological structure of the African coast-lands shows that the earliest to come into existence were those that extend between Cape Nun and the Cape of Good Hope. The coastal ranges of that section are much denuded, for they are of very great antiquity, having been ridged up in Palæozoic times. The later uplifts (negative or positive) of the same region were not attended by tilting and folding of strata, for the Mesozoic and Tertiary deposits, like those of South America, lie in comparatively horizontal positions. Between Cape Nun and Cape Spartel the rocks of the maritime tracts range in age from Palæozoic to Cainozoic, and have been traced across Morocco into Algeria and Tunis. They all belong to the Mediterranean region, and were deposited at a time when the southern shores of that inland sea extended from a point opposite the Canary Islands along the southern margin of Morocco, Algeria, and Tunis. Towards the close of the Tertiary period the final upheaval of the Atlas took place, and the Mediterranean, retreating northwards, became an almost land-locked sea.

I need hardly stop to point out how the African coast-lines have been modified by marine erosion and the accumulation of sediment upon the continental shelf. The extreme regularity of the coasts is due partly to the fact that the land is nearly co-extensive with the continental plateau, but it also results in large measure from the extreme antiquity of the land itself. This has allowed of the cutting-back of headlands and the filling-up of bays and inlets, a process which has been going on between Morocco and Cape Colony with probably little interruption for a very prolonged period of time. We may note also the effect of the heavy rains of the equatorial region in washing down detritus to the shores, and in this way protecting the land to some extent from the erosive action of the sea.

What now, let us ask, are the outstanding features of the coast-lines of the Atlantic Ocean? We have seen that along the margins of each of the bordering continents the last series of great mountain-uplifts took place in Palæozoic times. This is true alike for North and South America, for Europe and Africa. Later movements which have added to the extent of land were not marked by the extreme folding of strata which attended the early upheavals. The Mesozoic and Cainozoic rocks, which now and again form the shore-lands, occur in more or less undisturbed condition. The only great linear uplifts or true mountains of elevation which have come into existence in Western Europe and Northern Africa since the Palæozoic period trend approximately at right angles to the direction of the Atlantic trough, and are obviously related to the primitive depression of the Mediterranean. The Pyrenees and the Atlas, therefore, although their latest elevation took place in Tertiary times, form no exceptions to the rule that the extreme flexing and folding of strata which is so conspicuous a feature in the geological structure of the Atlantic sea-board dates back to the Palæozoic era. And the same holds true of North and South America. There all the coastal ranges of highly flexed and folded strata are of Palæozoic age. The Cordilleras of Venezuela are no doubt a Tertiary uplift, but they are as obviously related to the Caribbean depression as the Atlas ranges are to that of the Mediterranean. Again, we note that volcanic activity along the borders of the Atlantic was much less pronounced during the Mesozoic period than it appears to have been in earlier ages. Indeed, if we except the great Tertiary basalt-flows of the Icelandic ridge and the Arctic regions, we may say that volcanic action almost ceased after the Palæozoic era to manifest itself upon the Atlantic coast-lands of North America and Europe. But while volcanic action has died out upon the Atlantic margins of both continents, it has continued during a prolonged geological period within the area of the Mediterranean depression. And in like manner the corresponding depression between North and South America has been the scene of volcanic disturbances from Mesozoic down to recent times. Along the African coasts the only displays of recent volcanic action that appertain to the continental margin are those of the Gulf of Guinea and the Cape de Verde Islands. The Canary Islands and Madeira may come under the same category, but, as we have seen, they appear to stand in relationship to the Mediterranean depression and the Tertiary uplift of North Africa. Of Iceland and the Azores I have already spoken, and of Ascension and the other volcanic islets of the South Atlantic it is needless to say that they are related to wrinkles in the trough of the ocean, and therefore have no immediate connection with the continental plateau.

Thus in the geographical development of the Atlantic coast-lines we may note the following stages:—*First*, during Palæozoic times a series of great mountain-uplifts, which were frequently accompanied by volcanic action. *Second*, a prolonged stage of comparative coastal tranquillity, during which the maritime ranges referred to were subject to such excessive erosion that they were planed down to low levels, and in certain areas even submerged. *Third*, renewed elevation (negative or positive) whereby considerable portions of the much denuded Archæan and Palæozoic rocks, now largely covered by younger deposits, were converted into high lands. During this stage not much rock-folding took place, nor were any true mountains of elevation formed parallel to the Atlantic margins. It was otherwise, however, in the Mediterranean and Caribbean depressions, where coastal movements resulted in the formation of enormous linear uplifts. Moreover, volcanic action is now and has for a long time been more characteristic of these depressions than of the Atlantic coast-lands.

I must now ask you to take a comprehensive glance at the coast-lines of the Pacific Ocean. In some important respects these offer a striking contrast to those we have been considering. Time will not allow me to enter into detailed description, and I must therefore confine attention to certain salient features. Examining first the shores of the Americas, we find that there are two well-marked regions of fiords and fringing islands—namely, the coasts of Alaska and British Columbia, and of South America from 40° S.L. to Cape Horn. Although these regions may be now extending seawards in places, it is obvious that they have recently been subject to submergence. When the fiords of Alaska and British Columbia existed as land valleys it is probable that a broad land connection obtained between North America and Asia. The whole Pacific coast is margined by mountain ranges, which in elevation and boldness far exceed those of the Atlantic sea-board. The rocks entering into their formation range in age from Archæan and Palæozoic, and they are almost everywhere highly disturbed and flexed. It is not necessary, even if it were possible, to consider the geological history of all those uplifted masses. It is enough for my purpose to note the fact that the coastal ranges of North America and the principal chain of the Andes were all elevated in Tertiary times. It may be remarked further, that from the Mesozoic period down to the present the Pacific borders of America have been the scene of volcanic activity far in excess of what has been experienced on the Atlantic sea-board.

Geographically the Asiatic coasts of the Pacific offer a strong contrast to those of the American borders. The latter, as we have seen, are for the most part not far removed from the edge of the continental plateau. The coasts of the mainland of Asia, on the other hand, retire to a great distance, the true margin of the plateau being marked out by that great chain of islands which extends from Kamchatka south to the Philippines and New Guinea. The seas lying between those islands and the mainland occupy depressions in the continental plateau. Were that plateau to be lifted up for 6,000 or 7,000 feet the seas referred to would be enclosed by continuous land, and all the principal islands of the Indian Archipelago—Sumatra, Java, Celebes, and New Guinea, would become united to themselves as well as to Australia and New Zealand. In short, it is the relatively depressed condition of the continental plateau along the western borders of the Pacific basin that caused the Asiatic coast-lines to differ so strikingly from those of America.

From a geological point of view the differences are less striking than the resemblances. It is true that we have as yet a very imperfect knowledge of the geological structure of Eastern Asia, but we know enough to justify the conclusion that in its main features that region does not differ essentially from Western North America. During Mesozoic and Cainozoic times the sea appears to have overflowed vast tracts of Manchooria and China, and even to have penetrated into what is now the great Desert of Gobi. Subsequent crustal movements revolutionised the geography of all those regions. Great ranges of linear uplifts came into existence, and in these the younger formations, together with the foundations on which they rested, were squeezed into folds and ridged up against the nuclei of Palæozoic and Archæan rocks which had hitherto formed the only dry land. The latest of these grand upheavals are of Tertiary age, and, like those of the Pacific slope of America, they were accompanied by excessive volcanic action. The long chains of islands that flank the shores of Asia we must look upon as a series of partially submerged or partially emerged mountain-ranges, analogous geographically to the coast ranges of North and Central America, and to the youngest Cordilleras of South America. The presence of numerous active and recently extinct volcanoes, taken in connection with the occurrence of many great depressions which furrow the floor of the sea in the East Indian Archipelago, and the profound depths attained by the Pacific trough along the borders of Japan and the Kurile and Aleutian Islands—all indicate conditions of very considerable instability of the lithosphere. We are not surprised, therefore, to meet with much apparently conflicting evidence of elevation and depression in the coast-lands of Eastern Asia, where in some places the sea would seem to be encroaching, while in other regions it is retreating. In all earthquake-ridden and volcanic areas such irregular coastal changes may be looked for. So extreme are the irregularities of the sea-floor in the area lying between Australia, the Solomon Islands, the New Hebrides, and New Zealand, and so great are the depths attained by many of the depressions, that the margins of the continental plateau are harder to trace here than anywhere else in the world. The bottom of the oceanic trough throughout a portion of the Southern and Western Pacific is, in fact, traversed by many great mountain rides, the summits of which approach the surface again and again to form the numerous islets of Polynesia. But notwithstanding the considerable depths that separate Australia and New Zealand there is geological evidence to show that a land connection formerly linked both to Asia. The continental plateau, therefore, must be held to include New Caledonia and New Zealand. Hence the volcanic islets of the Solomon and New Hebrides groups are related to Australia in the same way as the Kiu-kiu, Japanese, and Kurile Islands are to Asia.

Having rapidly sketched the more prominent features of the Pacific coast-lines, we are in a position to realise the remarkable contrast they present to the coast-lines of the Atlantic. The highly folded strata of the Atlantic sea-board are the relics of great mountains of upheaval, the origin of which cannot be assigned to a more recent date than Palæozoic times. During subsequent crustal movements no mountains of corrugated strata were uplifted along the Atlantic margins, the Mesozoic and Cainozoic strata of the coastal regions showing little or no disturbance. It is quite in keeping with all this that volcanic action appears to have been most strongly manifested in Palæozoic times. So many long ages have passed since the upheaval of the Archæan and Palæozoic mountains of the Atlantic sea-board that these heights have everywhere lost the character of true mountains of elevation. Planed down to low levels, partially submerged and covered to some extent by newer formations, they have in many places been again converted into dry lands, forming plateaus—now sorely denuded and cut up into mountains and valleys of erosion. Why the later movements along the borders of the Atlantic basin should not have resulted in the wholesale plication of the younger sedimentary rocks is a question for geologists. It would seem as if the Atlantic margins had reached a stage of comparative stability long before the grand Tertiary uplifts of the Pacific borders had taken place; for, as we have seen, the Mesozoic and Cainozoic strata of the Atlantic coast-lands show little or no trace of having been subjected to tangential thrusting and crushing. Hence one cannot help suspecting that the retreat of the sea during Mesozoic and Cainozoic ages may have been due rather to subsidence of the oceanic trough and to sedimentation within the continental area than to positive elevation of the land.

Over the Pacific trough, likewise, depression has probably been in progress more or less continuously since Palæozoic times, and this movement alone must have tended to withdraw the sea from the surface of the continental plateau in Asia and America. But by far the most important coastal changes in those regions have been brought about by the crumpling up of the plateau, and the formation of gigantic mountains of upheaval along its margins. From remotest geological periods down almost to the present the land area has been increased from time to time by the doubling-up and consequent elevation of coastal accumulations and by the eruption of vast masses of volcanic materials. It is this long-continued activity of the plutonic forces within the Pacific area which has caused the coast-lands of that basin to contrast so strongly with those of the Atlantic. The latter are incomparably older than the former—the heights of the Atlantic borders being mountains of denudation of vast geological antiquity, while the coastal ranges of the Pacific slope are creations but of yesterday as it were. It may well be that those Cordilleras and mountain-chains reach a greater height than was ever attained by any Palæozoic uplifts of the Atlantic borders. But the marked disparity in elevation between the coast-lands of the Pacific and the Atlantic is due chiefly to a profound difference in age. Had the Pacific coast-lands existed for as long a period and suffered as much erosion as the ancient rocks of the Atlantic sea-board, they would now have little elevation to boast of.

The coast-lines of the Indian Ocean are not, upon the whole, far removed from the margin of the continental plateau. The elevation of East Africa for 6000 feet would add only a very narrow belt to the land. This would still leave Madagascar an island, but there are geological reasons for concluding that this island was at a far distant period united to Africa, and it must therefore be considered as forming a portion of the continental plateau. The great depths which now separate it from the mainland are probably due to local subsidence, connected with volcanic action in Madagascar itself and in the Comoro Islands. The southern coasts of Asia, like those of East Africa, approach the edge of the continental plateau, so that an elevation of 6000 feet would make little addition to the land area. With the same amount of upheaval, however, the Malay Peninsula, Sumatra, Java, and West Australia, would become united, but without extending much further seawards. Land connection, as we know, existed in Mesozoic times between Asia, Australia, and New Zealand, but the coast-lines of that distant period must have differed considerably from those that would appear were the regions in question to experience now a general elevation. The Archæan and Palæozoic rocks of the Malay Peninsula and Sumatra are flanked on the side of the Indian Ocean by great volcanic ridges, and by uplifts of Tertiary strata, which continue along the line of the Nicobar and Andaman Islands into Burma. Thus the coast-lines of that section of the Indian Ocean exhibit a geographical development similar to that of the Pacific seaboard. Elsewhere, as in Hindustan, Arabia, and East Africa, the coast-lines appear to have been determined chiefly by regional elevations of the land or subsidence of the oceanic trough in Mesozoic and Cainozoic times, accompanied by the outwelling of enormous floods of lava. Seeing, then, that the Pacific and Indian Oceans are pre-eminently regions which, down to a recent date, have been subject to great crustal movements and to excessive volcanic action, we may infer that in the development of their coast-lines the sea has played a very subordinate part. The shores, indeed, are largely protected from marine erosion by partially emerged volcanic ridges and by coral islands and reefs, and to a considerable extent also by the sediment which in tropical regions especially is swept down to the coast in great abundance by rains and rivers. Moreover, as the geological structure of these regions assures us, the land would appear seldom to have remained sufficiently long at one level to permit of much destruction by waves and tidal currents.

In fine, then, we arrive at the general conclusion that the coast-lines of the globe are of very unequal age. Those of the Atlantic were determined as far back as Palæozoic times by great mountain uplifts along the margin of the continental plateau. Since the close of that period many crustal oscillations have taken place, but no grand mountain ranges have again been ridged up on the Atlantic sea-board. Meanwhile the Palæozoic mountain-chains, as we have seen, have suffered extensive denudation, have been planed down to the sea-level, and even submerged. Subsequently converted into land, wholly or partially as the case may have been, they now present the appearance of plains and plateaus of erosion, often deeply indented by the sea. No true mountains of elevation are met with anywhere in the coast-lands of the Atlantic, while volcanic action has well-nigh ceased. In short, the Atlantic margins have reached a stage of comparative stability. The trough itself, however, is traversed by at least two well-marked banks of upheaval—the great meridional Dolphin Ridge, and the approximately transmeridional Færöe-Icelandic belt—both of them bearing volcanic islands.

But while the coast-lands of the Atlantic proper attained relative stability at an early period, those of the Mediterranean and Caribbean depressions have up to recent times been the scenes of great crustal disturbance. Gigantic mountain-chains were uplifted along their margins at so late a period as the Tertiary, and their shores still witness volcanic activity.

It is upon the margins and within the troughs of the Pacific Ocean, however, that subterranean action is now most remarkably developed. The coast-lines of that great basin are everywhere formed of grand uplifts and volcanic ranges, which, broadly speaking, are comparable in age to those of the Mediterranean and Caribbean depressions. Along the north-east margin of the Indian Ocean the coast-lines resemble those of the Pacific, being of like recent age, and similarly marked by the presence of numerous volcanoes. The northern and western shores, however (as in Hindustan, Arabia, and East Africa), have been determined rather by regional elevation or by subsidence of the ocean-floor than by axial uplifts—the chief crustal disturbances dating back to an earlier period than those of the East Indian Archipelago. It is in keeping with this greater age of the western and northern coast-lands of the Indian Ocean that volcanic action is now less strongly manifested in their vicinity.

I have spoken of the comparative stability of the earth's crust within the Atlantic area as being evidenced by the greater age of its coastal ranges and the declining importance of its volcanic phenomena. This relative stability is further shown by the fact that the Atlantic sea-board is not much disturbed by earthquakes. This, of course, is what might have been expected, for earthquakes are most characteristic of volcanic regions and of those areas in which mountain-uplifts of recent geological age occur. Hence the coast-lands of the Pacific and the East Indies, the borders of the Caribbean Sea, the volcanic ridges of the Atlantic basin, the lands of the Mediterranean, the Black Sea, and the Aralo-Caspian depressions, the shores of the Red Sea, and vast tracts of Southern Asia, are the chief earthquake regions of the globe. It may be noted, further, that shocks are not only most frequent but most intense in the neighbourhood of the sea. They appear to originate sometimes in the volcanic ridges and coastal ranges, sometimes under the floor of the sea itself. Now earthquakes, volcanoes, and uplifts are all expressions of the one great fundamental fact that the earth is a cooling and contracting body, and they indicate the lines of weakness along which the enormous pressures and strains induced by the subsidence of the crust upon its nucleus find relief. We cannot tell why the coast-lands of the Atlantic should have attained at so early a period a stage of relative stability—why no axial uplifts should have been developed along their margins since Palæozoic times. It may be that relief has been found in the wrinkling-up of the floor of the oceanic trough, and consequent formation of the Dolphin Ridge and other great submarine foldings of the crust. And it is possible that the growth of similar great ridges and wrinkles upon the bed of the Pacific may in like manner relieve the coast-lands of that vast ocean, and prevent the formation of younger uplifts along their borders.

I have already remarked that two kinds of elevatory movements of the crust are recognized by geologists—namely, axial and regional uplifts. Some, however, are beginning to doubt, with Professor Suess, whether any vast regional uplifts are possible. Yet the view that would attribute all such apparent elevations of the land to subsidence of the crust under the great oceanic troughs is not without its difficulties. Former sea-margins of very recent geological age occur in all latitudes, and if we are to explain these by sub-oceanic depression, this will compel us to admit, as Suess has remarked, a general lowering of the sea-level of upwards of 1,000 feet. But it is difficult to believe that the sea-floor could have subsided to such an extent in recent times. Suess thinks it is much more probable that the high-level beaches of tropical regions are not contemporaneous with those of higher latitudes, and that the phenomena are best explained by his hypothesis of a secular movement of the ocean—the water being, as he contends, alternately heaped up at the equator and the poles. The strand-lines in high latitudes, however, are certainly connected with glaciation in some way not yet understood. And if it cannot be confidently affirmed that they indicate regional movements of the land, the evidence, nevertheless, seems to point in that direction.

In concluding this imperfect outline-sketch of a large subject, I ought perhaps to apologize for having trespassed so much upon the domains of geology. But in doing so I have only followed the example of geologists themselves, whose divagations in territories adjoining their own are naturally not infrequent. From much that I have said, it will be gathered that with regard to the causes of many coastal changes we are still groping in the dark. It seems not unlikely, however, that as light increases we may be compelled to modify the view that all oscillations of the sea-level are due to movements of the lithosphere alone. That is a very heretical suggestion; but that a great deal can be said for it anyone will admit after a candid perusal of Suess's monumental work, "Das Antlitz der Erde."

SECTION G.

MECHANICAL SCIENCE.

OPENING ADDRESS BY W. CAWTHORNE UNWIN, F.R.S., M.INST.C.E., PRESIDENT OF THE SECTION.

BY what process selection is made of a Sectional President of the British Association is to me unknown. I may confess that it was pleasant to receive the request of the Council to preside at the meetings of Section G, even though much of the pleasure was due to its unexpectedness. I ventured to believe I might accept the honour gratefully, trusting to your kindness to assist me in fulfilling its obligations. Amongst engineers there are many with greater claims than I have to such a position, and who could speak to you from a wider practical experience. Here in Section G, I think it may be claimed that the profession of engineering owes much to some who from circumstances or natural bias have concerned themselves more with those scientific studies and experimental researches which are useful to the engineer, than with the actual carrying on of engineering operations. Here, at so short a distance from the University where Rankine and James Thomson laboured, I may venture to feel proud of being amongst those whose business it has been rather to investigate problems than to execute works.

The year just passed is not one unmemorable in the annals of engineering. By an effort remarkable for its rapidity, and as an example of organization of labour, the broad gauge system has been extinguished. It has disappeared like some prehistoric mammoth, a large-limbed organism, perfect for its purpose and created in a generous mood, but conquered in the struggle for existence by smaller but more active rivals. If we recognize that the great controversy of fifty years ago has at last been decided against Brunel, at least we ought to remember that the broad gauge system was one only of many original experiments due to his genius and courage, experiments in every field of engineering, in bridge building, in locomotive design, in ship construction, the successes and failures of which have alike enlarged the knowledge of engineers and helped the progress of engineering.

The past year has seen the completion of the magnificent scheme of water supply for Liverpool, from the Vyrnwy, carried out from 1879 to 1885 by Mr. Hawksley and Mr. Deacon, and since then completed under the direction of the latter engineer. This is one of the largest and most striking of those works of municipal engineering rendered necessary by the growth of great city communities and made possible by their wealth and public spirit. For the supply of water to Liverpool, the largest artificial lake in Europe has been created in mid-Wales, by the contruction across a mountain valley of a dam of cyclopean masonry, itself one of the most remarkable masonry works in the world. The lake contains an available supply of over 12,000 million gallons, its size having been determined not only to supply forty million gallons daily for the increasing demand of Liverpool, but also to meet the necessity imposed by Parliament that an unprecedentedly large compensation, amounting to ten million gallons daily and fifty million gallons additional on thirty-two days yearly, should be afforded to the Severn. The masonry dam, though a little less in height than some of the French dams, is of greater length. It is nearly double the length of the great dam at Verviers.[1] Athough masonry dams were an old expedient of engineers, it is in quite recent times, and chiefly in consequence of the scientific investigations of French engineers, that they have been revived in engineering practice. Since the completion of the Vyrnwy dam, another very large dam, the Tansa dam, has been completed in Bombay. This dam has a length of two miles and a height of 118 feet, and it is 100 feet thick at the base. The reservoir will supply 100 million gallons per day. In the United States a still greater work of the same kind has been commenced on the Croton river, in connection with the water supply of New York. This dam will have a length of 2000 feet, and a height of 285 feet. Its greatest thickness will be 215 feet. It will be very much the boldest work of its kind.

Returning to the Liverpool supply, the water taken from the lake at the most suitable level into a straining tower provided with very complete hydraulic machinery, passes through the Hirnant tunnel, and thence by an aqueduct, partly consisting of rock tunnels, partly of pipes 39in. to 42in. in diameter, sixty-eight miles in length, being the longest aqueduct yet constructed. The crossing of the Mersey by an aqueduct tunnel has proved the greatest engineering difficulty to be surmounted. The tunnel has been carried through layers of running sand, gravel, and silt. At first slow progress was made, but later, by the adoption of the Greathead system of shield, with air locks and air-compressing machinery, as much as fifty-seven feet of tunnel were driven and lined in one week. The whole work is now complete, and Liverpool has available an extra supply of very pure water, amounting to forty million gallons daily.

A scheme of water supply for Manchester from Lake Thirlemere in Westmoreland, on an equally large scheme, is approaching completion. Birmingham is likely to carry out another work of the same kind. And London, at a greater distance from pure water sources and under greater difficulties from the complexity of existing interests, has come to realize that, within fifty years, a population of 12½ millions will probably have to be provided for. To supply such a population, a volume of water is required ten times as great as the whole available supply from Lake Vyrnwy.

Here in Edinburgh one remembers that the birth-place of the steam-engine is near at hand. A century and a quarter ago James Watt made an invention which has profoundly influenced all the conditions of social, national, commercial, and industrial life. It is due to the steam-engine more than to any other single cause that the population in this country has tripled since the beginning of the century, and that we have become dependent on steam-power for fuel, for transport, for manufactures, in many cases for water supply, for sanitation, and for artificial light. From some German statistics it appears that there are probably now in the world, employed in industry, steam-engines exerting 49 million horse-power, besides locomotives exerting six million horse-power. Engines in steam-ships are not included. The steam-engine has become a potent factor in civilization, because it places at our disposal mechanical energy at a sufficiently low cost, and the efforts of engineers have been steadily directed to diminishing the cost at which steam-power is produced. Members of one great branch of our profession are much concerned in the production of mechanical energy at a sufficiently cheap rate. They require it in very large quantity for transformation into light and for re-transformation into mechanical energy under conditions more convenient than the direct use of steam-power. Perhaps it will not be inappropriate if in Section G I first discuss briefly some of the causes which have made the steam-engine inefficient and the extent to which we are getting to a scientific knowledge of the methods of evading them. I propose then to consider some of the methods of economizing the cost and increasing the convenience of mechanical power by generating it at central stations and distributing it, and lastly, how far means of transporting energy are likely to make available cheaper sources of energy than steam-power.

Let us go back for a moment to James Watt. The most distinct feature about the invention of the steam-engine is that it arose out of studies of such questions as the relation of pressure and temperature of steam, the heat absorbed in producing it, and its volume at different pressures.

Armed with this knowledge, Watt was able to determine that the quantity of steam used in a model atmospheric engine was enormously greater than that due to the volume described by the piston. There was waste or loss. To discover the loss was to get on the path of finding a remedy. The separate condenser, by diminishing cylinder condensation, annulled a great part of the loss. So great was Watt's insight into the action of the engine that he was able to leave it so perfect that, except in one respect, little remained for succeeding engine builders, except to perfect the machines for its manufacture, to improve its details, and to adapt it to new purposes. Now it very early became clear that there were two directions of advance which ought to secure greater economy. Simple mechanical indications showed that increased expansion ought to ensure increased economy. Thermodynamic considerations indicated that higher pressures, involving a greater temperature range of working, ought to secure greater economy. But in attempting to advance in either of these directions, engineers were more or less disappointed. Some of Watt's engines worked with 5 lbs. of coal per indicated horse-power per hour. Many engines with greater pressures and longer expansions have done but little better. The history of steam-engine improvement for a quarter of a century has been an attempt to secure the advantages of high pressures and high ratios of expansion. The difficulty to be overcome has proved to be due to the same cause as the inefficiency of Watt's model engine. The separate condenser diminished, but it did not annul, the action of the cylinder wall. The first experiments which really startled thoughtful steam engineers were those made by Mr. Isherwood,

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[1] The length of the dam from rock to rock is 1172 feet. Height from lowest part of foundation to parapet of carriage way, 161 feet. Height from bed of river to overflow sill, 84 feet. Thickness of masonry at base, 120 feet.

between 1860 and 1865. Mr. Isherwood showed that in engines such as those then in use in the United States Navy, with the large cylinders and low speeds then prevalent, any expansion of the steam beyond three times led, not to an increased economy, but to an increased consumption of steam. Very little later than this M. Hirn undertook, in 1871–5, his classical researches on the action of the steam in an engine of about 150 indicated horse-power. Experiments of greater accuracy or completeness, or of greater insight into the conditions which were important, have never since been made, and Hirn with his assistants, MM. Hallaner and Dwelshauvers Dery, has determined, once for all, the whole method of a perfect steam-engine trial. M. Hirn was the first to clearly realize that the indicator gives the means of determining the steam present in the cylinder during every period of the cycle of the engine. Consequently, superheating in ordinary cases being out of the question, we have the means of determining the heat present and the heat already converted into work. The heat delivered into the engine is known from boiler measurements, combined with calorimetric tests of the quality of the steam, tests which Hirn was the first to undertake. The balance or heat unaccounted for is, then, a waste or loss due to causes which have to be investigated. Hirn originated a complete method of analysis of an engine test, showing at every stage of the operation the heat accounted for and a balance of heat unaccounted for; and the latter proved to be a very considerable quantity.

Meanwhile theoretical writers, especially Rankine and Clausius, had been perfecting a thermodynamic theory of the steam-engine, based primarily on the remarkable and irrefragable principle of Carnot. The result of Hirn's analysis was to show that these theories, applied to the actual steam-engine, were liable to lead to errors of 50 or 60 per cent., the single false assumption made being that the interaction between the walls of the cylinder and the steam was an action small enough to be negligeable.

In this country Mr. Mair Rumley, following Hirn's method, made a series of experiments on actual engines with great care and accuracy and completeness. All these experiments demonstrated the fact of a large initial condensation of steam on the walls of the cylinder, alike in jacketed and unjacketed engines. This condensed steam is re-evaporated partially during expansion, but mainly during exhaust, and serves as a mere carrier of heat from boiler to condenser, in conditions not permitting its utilization in producing work.

It became clear from Hirn's experiments, if not from the earlier experiments of Isherwood, that for each engine there is a particular ratio of expansion for which the steam expenditure per horse-power is least. Professor Dery has since deduced from them that the practical condition of securing the greatest efficiency is that the steam at release should be nearly dry. In producing that dryness the jacket has an important influence. In spite of much controversy amongst practical engineers about the use of the jacket, it does not appear that any trustworthy experiment has yet been adduced in which there was an actual loss of efficiency due to the jacket. In the older type of comparatively slow engines it is a rule that the greater the jacket condensation the greater the economy of steam, even when the jacket condensation approaches 20 per cent. of all the steam used. It appears, however, that as the speed of the engine increases, the influence of the jacket diminishes, so that for any engine there is a limit of speed at which the value of the jacket becomes insignificant.

Among steam-engine experiments directed specially to determine the action of the cylinder walls, those of the late Mr. Willans should be specially mentioned. Mr. Willans' death is to be deplored as a serious loss to the engineering profession. His steam-engine experiments, some of them not yet published, are models of what careful experiments should be. They are graduated experiments designed to indicate the effect of changes in each of the practically variable conditions of working. They showed a much greater variation of steam consumption (from 46 to 18 lbs. per indicated horse-power hour) in different conditions of working than, I think, most practical engineers suspected, and this has been made more significant in later experiments, on engines working with less than full load. The first series showed that in full load trials the compound was superior to the simple engine in practically all the conditions tried, but that the triple was superior to the compound only when certain limits of pressure and speed were passed.

As early as 1878 Prof. Cotterill had shown that the action of a cylinder wall was essentially equivalent to that of a very thin metallic plate, following the temperature of the steam. The exceedingly rapid dissipation of heat from the surface during exhaust especially being due to the evaporation of a film of water initially condensed on its surface. In permanent *régime* the heat received in admission must be equal to that lost after cut off. In certain conditions it appeared that a tendency would arise to accumulate water on the cylinder surfaces, with the effect of increasing in certain cases the energy of heat dissipation. Recently Prof. Cotterill has been able to carry much further the analysis of the complex action of condensation and re-evaporation in the cylinder, and to discriminate in some degree between the action of the metal and the more ambiguous action of the water film. By discarding the less important actions, Prof. Cotterill has found it possible to state a semi-empirical formula for cylinder condensation in certain restricted cases which very closely agrees with experiments on a wide variety of engines. It is to be hoped that, with the data now accumulating, a considerable practical advance may be made in the clearing up of this complex subject. There are, no doubt, some people who are in the habit of depreciating quantitative investigations of this kind. They are as wise as if they recommended a manufacturer to carry on his business without attending to his account books. Further, the attempt to obtain any clear guidance from experiments on steam-engines has proved a hopeless failure without help from the most careful scientific analysis. There is not a fundamental practical question about the thermal action of the steam-engine, neither the action of jackets or of expansion or of multiple cylinders, as to which contradictory results have not been arrived at, by persons attempting to deduce results from the mass of engine tests without any clear scientific knowledge of the conditions which have affected particular results. In complex questions fundamental principles are essential in disentangling the results. Interpreted by what is already known of thermodynamic actions, there are very few trustworthy engine tests which do not fall into a perfectly intelligible order. There is only one known method, not now much used, by which the cylinder condensation can be directly combated. Thirty years ago superheating the steam was adopted with very considerable increase of economy. It is likely that it was thought by the inventor of superheating that an advantage would be gained by increasing the temperature range. If so, his theory was probably a mistaken one. For the cooling action of the cylinder is so great that the steam is reduced to saturation temperature before it has time to do work; but the economy due to superheating was unquestionable, and was very remarkable considering how small a quantity of heat is involved in superheating. The heat appears to diminish the cylinder wall action so much as almost to render a jacket unnecessary. The plan of superheating was abandoned from purely practical objections, the superheaters then constructed being dangerous. Recently superheating has been tried again at Mulhouse by M. Meunier, and his experiments are interesting because they are at higher pressures than in the older trials and with a compound engine. It appears that even when the superheater was heated by a separate fire there was an economy of steam of 25 to 30 per cent. and an economy of fuel of 20 to 25 per cent., and four boilers with superheating were as efficient as five without it.

It may be pointed out as a point of some practical importance that if a trustworthy method of superheating could be found, the advantage of the triple over the compound engine would be much diminished. For marine purposes the triple engine is perfectly adapted. But for other purposes it is more costly than the compound engine, and it is less easily arranged to work efficiently with a varying load.

There does not seem much prospect of exceeding the efficiency attained already in the best engines, though but few engines are really as efficient as they might be, and there are still plenty of engines so designed that they are exceedingly uneconomical. The very best engines use only from 12 to 13 lbs. of steam per indicated horse-power hour, having an absolute efficiency reckoned on the indicated power of 16 per cent., or reckoned on the effective power, 13 per cent. The efficiency, including the loss in the boiler, is only about 9 per cent. But there are internal furnace engines of the gas-engine or oil-engine type in which the thermal efficiency is double this.

In his interesting address to this Section in 1878, Mr. Easton expressed the opinion that the question of water-power was one deserving more consideration than it had lately received, and he pointed to the variation of volume of flow of streams as the principal objection to their larger utilization. Since that time the progress made in systems of transporting and distributing power has given quite a new importance to the question of the utilization of water-power. There seems to be a probability that in many localities water-power will, before long, be used on a quite unprecedented scale, and under conditions involving so great convenience and economy that it may involve a quite sensible movement of manufacturers towards districts where water-power is available.

If we go back to a period not very distant in the history of the world, to the middle of the last century, we reach the time when textile manufactures began to pass from the condition of purely domestic industries to that of a factory system. The fly-shuttle was introduced in 1750, the spinning-jenny was invented in 1767, and Crompton's machine only began to be generally used in 1787. It was soon found that the new machines were most suitably driven by a rotary motion, and after some attempts to drive them by horses, water-power was generally resorted to. In an interesting pamphlet on the Rise of the Cotton Trade, by John Kennedy, of Ardwick Hall, written in 1815, it is pointed out that the necessity of locating the mills where water-power was available, had the disadvantages of taking them away from the places where skilled workmen were found, and from the markets for the manufactured goods. Nevertheless, Mr. Kennedy states that for some time after Arkwright's first mill was built at Cromford, all the principal mills were erected near river falls, no other power than water-power having been found practically useful. "About 1790," says Mr. Kennedy, "Mr. Watt's steam-engine began to be understood, and waterfalls became of less value. Instead of carrying the workpeople to the power, it was found preferable to place the power amongst the people."

The whole tendency of the conditions created by the use of steam-power has been to concentrate the industrial population in large communities, and to restrict manufacturing operations to large factories. Economy in the production of power, economy in superintendence, the convenience of the subdivision of labour, and the costliness of the machines employed, all favoured the growth of large factories. The whole social conditions of manufacturing centres have been profoundly influenced by these two conditions—that coal for raising steam can be easily brought to any place where it is wanted, and that steam-power is more cheaply produced on a large scale than on a small scale. It looks rather, just now, as if facilities for distributing power will to some extent reverse this tendency.

Let me first point out that water-power, where it is available, is so much cheaper and more convenient than steam-power that it has never been quite vanquished by steam-power.

I find, from a report by Mr. Weissenbach, that in 1876 70,000 horse-power derived from waterfalls were used in manufacturing in Switzerland. According to a census in 1880, it appears that the total steam and water-power employed in manufacturing operations in the United States was 3,400,000 horse-power. Of this, 2,185,000 horse-power, or 64 per cent., was derived from steam, and 1,225,000 horse-power, or 36 per cent., from water. In the manufacture of cotton and woollen goods, of paper and of flour, 760,000 horse-power were obtained from water, and 515,000 horse-power from steam. If statistics could be obtained from other countries, I believe it would be found that a very large amount of water-power is actually made available. The firm of Escher Wyss and Company, of Zurich, have constructed more than 1800 turbines of an aggregate power of 111,460 horse-power.

With a very limited exception all the water-power at present used is employed in the neighbourhood of the fall where it is generated. If means were available for transporting the power from the site of the fall to localities more convenient for manufactures, there can be no doubt that a much larger amount of water-power would be used, and the relative importance of water and steam power in some countries would probably be reversed. It is because recent developments seem to make such a transport of power possible without excessive cost and without excessive loss, that a most remarkable interest has been excited in the question of the utilization of water-power. Take the case of Switzerland for instance. At the present time Switzerland is said to pay to other countries £800,000 annually for coal. But the total available water-power of Switzerland is estimated at no less than 582,000 horse-power, of which probably only 80,000 are at present utilized. I found a year ago that nearly every large industrial concern in Switzerland was preparing to make use of water-power, transported a greater or less distance. Besides the great schemes actually carried out at Schaffhausen, Bellegarde, Geneva, and Zurich, where water-power is already utilized on a very large scale, there is a project to develop 10,000 horse-power on the Dranse near Martigny.

Hence it is easy to see that problems of distribution of power—that is, the transformation of energy into forms easily transportable and easily utilizable—have now a great interest for engineers.

Besides the power required for manufacturing operations, there is a steadily increasing demand for easily available mechanical energy in large towns. For tramways, for lifts, for handling goods, for small industries, for electric lighting, and sometimes for sanitation, power is required. Hitherto steam-engines, or more lately gas-engines, have been used, placed near the work to be done. But this sporadic generation of power is uneconomical and costly, especially when the work is intermittent; the cost of superintendence is large, and the risk of accident considerable. Hence attention is being directed to systems in which the mechanical energy of fuel or falling water is first generated in large central stations, transformed into some form in which it is conveniently transportable and capable of being rendered available by simpler motors than steam-engines.

Just as in great towns it has become necessary to supersede private means of water supply by a municipal supply; just as it has proved convenient to distribute coal-gas for lighting and heating, and to provide a common system of sewerage, so it will probably be found convenient to have in all large towns some means of obtaining mechanical power in any desired quantity at a price proportionate to the quantity used, and in a form in which it can be rendered available, either directly or by simple motors requiring but little skilled superintendence.

Telodynamic Transmission.—First, then, let me say a few words as to the modes of distributing power which it is possible to adopt. In 1850, at Logelbach in Alsace, M. Ferdinand Hirn used a flat steel belt to transmit power directly a distance of eighty metres. Subsequently a wire rope was used on grooved pulleys. This worked so well that a second transmission to a distance of 240 metres was erected. The details of the system were worked out with great care with a view to securing the least cost of construction, the least waste of energy, and the greatest durability of the ropes. So successful did this system of telodynamic transmission prove that within ten years M. Martin Stein, of Mulhouse, had erected 400 transmissions, conveying 4200 horse-power, and covering a distance of 72,000 metres.

Just at this time a very able and far-seeing manufacturer at Schaffhausen, Herr Moser, had formed a project for reviving the failing industries of the town by utilizing part of the water-power of the Rhine: Hirn's system of wire-rope transmission rendered this project practicable. The works were commenced in 1863. Three turbines of 750 horse-power were erected on a fall which varies from 12 to 16 feet, created by a weir across the river. From the turbines the power is transmitted by two cables, in one span of 392 feet, across the river. Similar cables distribute the power to factories along the river bank. In 1870 the transmission extended to a distance of 3400 feet. Power is sold at rates varying from £5 to £6 per horse-power per annum. In 1887 there were twenty-three consumers of power paying a rental of £3500 per annum for power. The project has been financially successful, and is still working. At Zurich, Freiberg, and Bellegarde there are similar installations, and a large scheme of the same kind has recently been carried out at Gokak in India. Wire-rope transmissions are of great mechanical simplicity, and the loss of power in transmission is exceedingly small. They are extremely suitable for certain cases where a moderate amount of power has to be transmitted a moderate distance, to one or to a few factories. On the other hand, they become cumbrous if the amount of power transmitted exceeds 600 or 1000 horse-power. The wear of the ropes, which only last a year, has proved greater than was expected, and is a source of considerable expense.

The practical introduction of a system of distributing power by *pressure water* is due to Lord Armstrong. Such a system involves a central pumping station, a series of distributing mains, and suitable working motors. From its first introduction the peculiar advantages of this system for driving intermittently working machines, such as lifts, dock machinery, railway cranes, and hauling gear, became obvious. But, with intermittent working machines, there rose the need of an appliance for storing energy during periods of minimum demand and restoring it in periods of maximum demand. The invention of the accumulator by Lord Armstrong made the system of hydraulic transmission a success, and at the same time fixed its character as a system specially adapted for those cases where intermittent work is required to be done. Lord Armstrong's system of hydraulic distribution by water at a pressure of 700 or 800 lbs. per square inch, with the use of accumulators for equalizing the variations of supply and demand, has now been widely adopted. The most extensive scheme of that kind hitherto executed is the important scheme carried out by the Hydraulic Power Company. Over fifty miles of pressure mains have now been laid in the streets of London. The Falcon Wharf pumping station contains four sets of compound pumping engines, each of 200 horse-power. Two additional pumping stations have now been erected, and 1500 lifts are worked from the pressure mains. The minimum charge for water is 2*s*. per 1000 gallons. This rate of charge is economical for such machines as lifts, but it would be extravagant for machines working continuously. It would be equivalent to a charge of nearly £50 per horse-power per year of 3000 working hours, apart from interest and maintenance of machines.

I shall indicate later on that in some cases where local conditions are favourable, where there is cheap water-power, and the possibility of constructing high-level storage reservoirs, then hydraulic transmission can be adopted with success for distributing power for ordinary manufacturing purposes. But neither telodynamic transmission nor hydraulic transmission have proved suitable as methods for the general distribution of motive power from central stations. Distribution by steam and distribution by heated water have both been tried in the United States, but not with very remarkable success. Only two other methods are available—distribution by compressed air and distribution by electricity.

For many years *compressed air* has been used to distribute power in tunnelling and mining operations to considerable distances. It is only recently that it has been used as a general method of distributing power to many consumers. In many installations the machinery has been rough and unscientific, and the waste of energy very considerable. It is through experience gained and improvements carried out in the remarkable system now at work in Paris, and known as the Popp system, that the great advantages of compressed air distribution have been proved. The Paris system has very gradually developed. About 1870 a small compressing station was erected to actuate public and private clocks by intermittent pulses of air conveyed along pipes chiefly laid in the sewers. In 1889 about 8000 clocks were thus driven. Meanwhile the compressed air had also been applied to drive motors for small industries. The demand for power thus supplied grew so rapidly that a second compressing station was built in the Rue de Saint Fargeau. In 1889 steam air compressors of 2000 horse-power were at work, and additional compressors were under construction. The pressure at that time was five atmospheres, and the largest air mains were 12 inches in diameter. Ingenious and simple rotary machines were used as air motors for small powers, and for larger powers any ordinary steam-engine was converted into an air motor. Prof. Kennedy made tests in 1889, which were communicated to this Association. He found that a motor four miles from the compressing station indicated 10 horse-power for 20 indicated horse power expended at the compressing station, an efficiency of 50 per cent. only. There were then 225 motors worked from the air mains.

Since 1889 more extended investigations have been made by Professor Riedler, of Berlin, and the chief part of the waste of work has been traced to inefficiency of the air compressors. Compound air compressors of much higher efficiency have now been constructed. The plant at the Saint Fargeau station has been increased to 4000 horse-power. A new station has been erected on the Quai de la Gare, intended ultimately to contain compressors of 24,000 horse-power. Compressors of 10,000 horse-power are already under construction.

Compressed air transmission, whether or not it is the most economical system, is undoubtedly applicable for the distribution of power on a very large scale and to very considerable distances. There is nothing in any of the appliances which is novel or imperfectly understood. The air is used in the consumer's premises in machinery of well-understood types, and old steam engines can be converted into air motors without difficulty and without alteration of existing transmissive machinery in the factories. Not least important, the air can be measured with accuracy enough for practical purposes by simple meters, and charged for in proportion to the power consumed. Air compressors and air motors are not as efficient as dynamos and electric motors, but in one respect distribution by air and electricity are similar. For distances which are not more than a few miles the loss of energy in transmission is small enough to be insignificant.

There is yet one other mode of power distribution which promises to become the most important of all, and which, in the case of transmission to very great distances, if such transmission becomes necessary, has undoubtedly great advantages over every other method.

About *electrical distribution* of power I shall not venture to say much, partly because I am not an electrical expert, partly because it has been lately pretty fully discussed. In the United States there has been an enormous development of electric tramways, which are essentially cases of electric power distribution. In this country we have the South London and some other railways worked electrically. There are others also on the Continent. But electrical power distribution to private consumers for industrial purposes has not yet made as much progress as might have been expected. Perhaps electrical engineers have been so busy with problems of electric lighting that they have had no time to settle the corresponding problems of power distribution.

No doubt continuous current distribution presents at the moment the fewest difficulties, or, at any rate, involves the fewest comparatively untried expedients. Several continuous-current plants for distributing power are in operation, of which perhaps the most interesting is that at Oyonaz, which was described in Section G last year by Prof. G. Forbes. There 300 horse-power obtained by turbines is transmitted 8 kilometres at 1800 volts. It is then let down by motor transformers to a voltage suitable for lighting and driving motors. A number of small workshops are driven, the power being supplied at a fixed rent.

At the Calumet and Hecla mines on Lake Superior, at the Dalmatia mines in California, and some other places, energy derived from turbines is transmitted distances of a mile or two by continuous electric currents and used in driving mining machinery, and some cases of the use of electrical distribution in mines in this country were mentioned by my predecessor in his address last year.

At Bradford a few electric motors are being worked from the electric lighting mains. The largest of these is of twenty horse-power. The price at which the electricity is supplied is not given, but I believe the cost is high when reckoned for continuous working. It would seem that it must be so when the electric current is generated by steam power.

At Schaffhausen an electric transmission has now been constructed alongside of the wire-rope transmission. The power is derived from two turbines, and is transmitted across the Rhine, a distance of 750 yards, at 624 volts. The current drives a spinning-mill, in which the largest motor is 380 horse-power. The power is sold, I believe, at £3 per horse-power of the motors per annum.

Many engineers have now apparently come to the conclusion that alternating currents will be better for power transmission to considerable distances than continuous currents. One interesting alternate current transmission, partly for power, partly for lighting purposes, has been for some time in operation at Genoa.

On the line of the aqueduct bringing water from the Gorzente rivulet three electric stations are being established. The reservoirs are 2050 feet above Genoa, and as this is a much greater fall than is required for water-supply purposes, part can be used to generate about 1600 horse-power.

In the first of the power stations erected there are turbines of 450 horse-power driving two dynamos. A second larger station was completed in November. In this there are eight alternate-current dynamos of 70 horse-power each. Six alternators are worked in series, transmitting a current of 6000 volts. The current is transmitted sixteen miles by bare copper wires, 8·5 mm. diameter, placed overhead. The current is used both for lighting and power purposes.

Another method of using alternating currents was adopted in the remarkable experiment at Frankfort last year. In that case energy obtained by turbines at Lauffen was transmitted to

Frankfort, a distance of 108 miles, and used for lighting and driving a motor. The current was obtained at low tension, transformed up to a tension of 18,000 to 27,000 volts for transmission, and then transformed down again for distribution. The loss in the conducting wires ranged from 5 horse-power when the turbines worked at 100 horse-power, to 25 horse-power when the turbines worked at 200 horse-power. The efficiency of dynamo, two transformers, and line ranged from 68 to 75 per cent., a remarkably satisfactory result.

There can be little doubt that if efficient and durable transformers can be constructed, they do give a considerable advantage to an alternate-current system. To an ordinary engineer it appears also that the system of producing current at a low tension in the dynamo, and using it at low tension in the motors, permits the construction of dynamos and motors more mechanically unexceptionable than those worked at high voltage.

I have spoken of the growth of a demand for power distributed in a convenient form in towns. The power distribution in London, Manchester, Birmingham, and Liverpool by pressure water, and that by compressed air in Paris, shows how rapidly, when power is available, a demand for it arises. A striking instance may be found in the small town of Geneva.

In 1871, soon after the completion of the earlier system of low-pressure water supply, Col. Turrettini applied to the municipal council to place a pressure engine on the town mains for driving the factory of the Society for Manufacturing Physical Instruments. The plan proved so convenient that nine years after, in 1880, there were in Geneva 111 water-motors supplied from the low-pressure mains, using 34,000,000 cubic feet of water annually, and paying to the municipality nearly £2000 a year. The cost of the power was not low. It was charged at a rate equivalent to from £36 to £48 per horse-power per year of 3000 working hours. But even the high price did not prevent the use of power so conveniently obtainable.

Since then a high-pressure water service has been established, the water being pumped by turbines in the Rhone. From this high-pressure service power is supplied more cheaply. On the high-pressure system the cost of the power is about 0·7*d.* per horse-power hour, or £8 per horse-power for 3000 working hours.

In 1889 the annual income from water sold for power purposes on the low-pressure system was £2085 and on the high-pressure system £4500. On the high-pressure system the receipts in 1889 were increasing at the rate of £880 per year.

In 1889 the motive power distributed, on the high-pressure system alone, amounted to 1,500,000 horse-power hours, there being seventy-nine motors of an aggregate working power of 1279 horses.

In Zurich there is quite a similar system and power, amounting to 9,000,000 horse-power hours in the year, distributed hydraulically to various consumers, who pay a rental of £1200 per annum. It will be noted that all this power in Geneva and Zurich is obtained from water which has been pumped, and it is the low cost of the water-power which does the pumping which makes this possible.

But, further, in both Geneva and Zurich the whole of the dynamos supplying electric light are also driven by turbines using pumped water. The convenience of this arises in this way. The fall obtainable in the river in both cases is a small one, and varies. Large turbines are required, and these cannot work at a constant speed. Further, it is expensive to use these large low-pressure turbines to drive directly dynamos which only work with a considerable load for a short portion of the day. The low-pressure turbines in the river are therefore used to pump water to a high-level reservoir, and they work with a constant load all the twenty-four hours.

From the high-level reservoir water is taken as power is required to drive the dynamos, and the turbines driving the dynamos are small high-pressure turbines, working always on a constant fall at a regular speed, and easily adjusted by a governor to a varying load. The system seems a roundabout one, but it is perfectly rational, effective, and economical.

Few persons can have seen Niagara Falls without reflecting on the enormous energy which is there continuously expended, and for any useful purpose wasted. The exceptional constancy of the volume of flow, the invariability of the levels, the depth of the plunge over the escarpment, the solid character of the rocks, all mark Niagara as an ideally perfect water-power station; while, on the other hand, the remarkable facilities of transport, both by steam navigation on the lakes and by four systems of railway, afford commercial advantages of the highest importance. From a catchment basin of 240,000 square miles, an area greater than that of France, a volume of water amounting to 265,000 cubic feet per second descends from Lake Erie to Lake Ontario, a vertical distance of 326 feet, in 37½ miles.

Supposing the whole stream could be utilized, it would supply 7,000,000 horse-power. This is more than double the total steam and water-power at present employed in manufacturing industry in the United States.

Immediately below the Falls the river bends at right angles, and flows through a narrow gorge. The town of Niagara Falls on the American side occupies the table-land in this angle.

The earliest traders who settled near the Falls erected stream mills in the Upper River in 1725 for preparing timber. Later, the Porter family erected factories on the islands in the rapids above the falls. It was not, however, till about thirty years ago that any systematic attempt was made to utilize part of the water-power of the Falls. Then a canal was constructed from Port Day, about three-quarters of a mile above the Falls, to a fore-bay or head-race along the cliff overlooking the lower river. In 1874 the Cataract Mill was established, taking power from this canal, and other mills were gradually erected till about 6000 horse-power were utilized. These mills have been exceedingly prosperous, but since the growth of a feeling against the disfigurement of the Falls it has become impossible to extend works of the same kind.

The idea of a method of utilizing the Falls, capable of greater development, and free from the objections to the hydraulic canal with mills discharging tail water on the face of the cliff, is due to the late Mr. Thomas Evershed, Division Engineer of the New York State Canals. He proposed to construct head-race canals on unoccupied land some two miles above the Falls. From these the water was to fall through vertical turbine pits into tail-race tunnels, converging into a great main tunnel, discharging into the lower river. Apart from an inappreciable diminution in the volume of flow over the Falls, this plan avoids any disfigurement of the scenery near the Falls, and permits a head of nearly 200 feet to be made available. It is, however, essential to such a plan that work should be undertaken on a very large scale. In 1886 the Niagara Falls Company was incorporated, and obtained options over a considerable area of land, extending from Port Day for two miles along the Niagara River. In 1889 the Cataract Construction Company was formed to mature and carry out the constructional works required.

The present plans contemplate the utilization of 100,000 effective horse-power. The principal work of construction is a great tunnel 7260 feet long, which is to form a tail-race to the turbines, starting from lands belonging to the Company, and discharging into the lower river. The tunnel is 19 feet by 21 feet, or 386 square feet in area, inside a brickwork lining 16 inches thick.

The base of the tunnel is 205 feet below the sill of the head gate, and permits a fall of 140 to be rendered available at the turbines. The brickwork of the tunnel is lined for 200 feet from the mouth with cast-iron plates.

The tunnel has been excavated with remarkable rapidity with the aid of drills worked by compressed air.

The main head-race, about 200 feet wide, will run for about 5000 feet parallel with the river, having entrances from the river at both ends. Near the lower reach the Soo Paper Company is already arranging to utilize 6000 horse-power, discharging the water from the turbines through a lateral tunnel into the main tunnel. Near this lower reach will also be placed two principal power stations, from which power will be distributed, either electrically or otherwise in ways not yet fully determined. The first turbines to be erected in these power stations will be twin turbines of the outward flow type of 5000 effective horse-power. These turbines have a vertical shaft for driving dynamos or other machinery placed above ground.

According to Mr. Evershed's original plans, it was intended to distribute water by surface canals to different power users, each of whom would sink his own turbine pits, connected below by lateral tunnels to the main discharge tunnel. Some of the power at Niagara will undoubtedly be used in this way, and in the case of industries requiring a large amount of power it will be economical to purchase a site and water rights.

Such a plan is, however, not adapted to smaller factories. Obviously for them it would be more economical to develop the power in one or more central stations by turbines of large size under common management. Further, once given the means of distributing power instead of water, an important extension of the project becomes possible.

Besides supplying power to industries which may locate themselves at Niagara, the power may be transmitted to the existing factories in Buffalo and Tonawanda.

Arrangements are already proceeding to transmit 3000 horsepower to Buffalo, a distance of 18 miles, to work an electric lighting station.

In 1890, Mr. Adams, the President of the Niagara Construction Company, visited Europe to examine systems of power distribution. It was in consequence of this visit that the important modification of the plans of the Company involved in the substitution, to a large extent, of a system of power distribution, for a system of water distribution came to be adopted. The American engineers were anxious to obtain the best European advice as to the methods best suited to the local conditions. A commission was formed, consisting of Lord Kelvin, Dr. Coleman Sellers, Prof. Mascart, and Colonel Turrettini, and an invitation was given to engineers and engineering firms in Europe and America to send in competitive projects for the utilization of the power at Niagara and its distribution to different consumers at Niagara and in Buffalo by electrical or other means. Many of the plans sent in were worked out with great care and completeness. As to the hydraulic part of the projects there was some approach to general consent as to the arrangements to be adopted, but as to the methods of distributing the power there was an extraordinary diversity.

Generally the Commission reported in favour of electrical distribution, with perhaps a partial use of compressed air as an auxiliary method.

Generally also they reported in favour of methods of distribution by continuous currents in preference to alternating currents. Since the date at which the Commission reported, the Frankfort-Lauffen experiment has been made, and in the opinion of some electrical engineers a distinct advance has been achieved in the use of alternating currents at high potential.

The Company has not yet decided to adopt any plan for the central stations except in a tentative way. One or more turbines of 5,000 horse-power are to be erected, and probably at first this power will be distributed to Buffalo by an alternating current system.

The cost of a steam horse-power at Buffalo is reckoned at 35 dollars per annum. I believe the Company will be able to deliver power at from 10 dollars for large amounts, and a greater price for small amounts, this price being reckoned for twenty-four hour days.

The new industry of electric lighting has made necessary the provision of large amounts of motive power. Electric traction similarly depends on the supply of motive power. New chemical and metallurgical processes are being introduced which entirely depend for their commercial success on the supply of motive power at a low price.

Niagara is likely to become not only a seat of large manufacturing operations of familiar types, but also the home of important new industries.

NOTES.

WE regret to have to announce the death of Sir Daniel Wilson, the President of Toronto University.

ALTHOUGH the sixth International Geographical Congress will not assemble in London until June, 1895, arrangements are already being made in connection with it. The organizing committee is not quite completed, and the Royal Geographical Society is still adding to it. Among those already nominated are the President of the Society (Sir Mountstuart Grant Duff), the honorary Secretaries of the Society (Messrs. Douglas Freshfield and Henry Seebohm), Sir George Bowen, Sir Charles Wilson, General J. T. Walker, Major Darwin, M.P., Mr. J. Scott Keltie, Sir Frederick Abel, Sir Henry Barkley, and General J. F. D. Donnelly. This committee is busily engaged in making its arrangements.

THERE has been a recrudescence in the eruption of Etna during the past week. We trust that there is a local successor to the lamented Prof. Silvestri to give us some day a complete history of the phenomena.

THE weather during the past week has been very unsettled, although during the first part the disturbances were mostly confined to the north. The anticyclone which had for some time lain to the westward of our islands moved southwards, and shallow depressions appeared off Scotland. The prevailing winds were consequently westerly or south-westerly, and temperature was rather above the average, except in the north and west, where the daily maxima were frequently below 60°, being some degrees lower than the average. On Sunday a rather deep depression from the Atlantic became central over our islands, accompanied by very heavy rainfall in Ireland and Wales, and rainy weather subsequently spread over the whole of the kingdom; while a considerable fall of temperature and strong northerly winds followed the passage of the depression to the eastwards. The report issued by the Meteorological Council for the week ending the 6th instant shows that the rainfall only exceeded the mean in the north of Scotland; in all other districts there was a deficit. The deficiency was greatest in the south-west of England, where it amounted to eight inches since the beginning of the year.

PROF. LOEFFLER, of the University, Greifswald, has published two articles in the *Centralblatt für Bakteriologie*, on his discovery of, and experiments with, the *Bacillus typhi murium*, and on the result of its application, at the request of the Greek Government, to arrest a plague of field-mice in Thessaly. In view of their scientific interest, these articles have been translated under the direction of Mr. Harting, and will appear in the next number of *The Zoologist*.

VON HELLMUTH PANCKOW contributes an article on the dwarf races in Africa and South India to the recent number of the *Zeitschrift der Gesellschaft für Erdkunde*.

A MOST important report of the sugar-cane borers, which do so much harm in the West Indies, from the pen of Mr. W. F. H. Blandford (Lecturer on Entomology, Cooper's Hill), appears in the *Kew Bulletin* for July and August.

THE *Monthly Weather Review*, of the Dominion of Canada, for April 1892 contains notices of aurora seen on almost every day of the month. The most widely-observed display occurred on the 23rd, 24th, and 25th.

THE *Abhandlungen* of the Royal Prussian Meteorological Institute (Bd. I., No. 5) contains a very elaborate investigation, 154 quarto pages, of the aspiration apparatus invented by Dr. R. Assmann, of Berlin, an instrument intended to determine the true temperature and humidity of the air under any conditions. The first apparatus of this kind was invented by Mr. John Welsh in 1853, and was used by him and also by Mr. Glaisher in their balloon ascents, after which time it appears to have been overlooked, or set aside, until it was again reinvented by Dr. Assmann, in a modified form, in 1889. We cannot enter into the construction of the apparatus here, further than stating that by the rotation of discs, the continual renewal of the air in connection with very sensitive thermometers is ensured, by which means sudden changes of temperature which cannot be followed by an ordinary thermometer are indicated. The apparatus is used at the Prussian Institute and at the German colonies in Africa as a standard instrument for the determination of the true temperature and humidity of the air. For ordinary stations however, or for observations at sea, we presume that it is not likely to come into general use.

THE report of the director of the Hong Kong Observatory for the year 1891 contains a table of the monthly and yearly rainfall value for about forty years. The mean yearly value is

90·17 inches, most of which falls between May and September. Dr. Doberck states that there is apparently a little more rain when there are many spots on the sun, but the difference is too slight to be of any practical importance. The east wind is most prevalent at all seasons, the colony being within the region of the trade wind; about 59 per cent. of all winds blow from this quarter, but from June till September there is also a southerly maximum, caused by the monsoon. In winter the temperature is highest with south, and lowest with north wind, and in summer it is highest with south-west, and lowest with east winds. During the year, 213 ships' log-books have been examined for data relating to typhoons, and registers have been regularly kept at about forty stations.

THE additions to the Zoological Society's Gardens during the past week include two Macaque Monkeys (*Macacus cynomolgus*, ♂ ♂) from India, presented respectively by Lieutenant H. S. Wilson and Mrs. Dunnington Jefferson; a Ring-tailed Coati (*Nasua rufa*) from South America, presented by Mr. C. Carrington; an Angolan Vulture (*Gypohierax angolensis*, juv.), a —— Buzzard (*Buteo* ——) from West Africa, presented by Dr. Ferrier; a Spiny-tailed Mastigure (*Uromastix acanthinurus*) from Algeria, presented by Lady Sebright; a Black-headed Caique (*Caica melanocephala*) from Demerara, two Spiny-tailed Mastigures (*Uromastix acanthinurus*) from Algeria, deposited; three Short-headed Phalangers (*Belidens breviceps*) from Australia, a Hairy Armadillo (*Dasypus villosus*, ♂) from La Plata, a White-throated Capuchin (*Celeus hypoleucus*, ♀) from Central America, four Scarlet Ibises (*Eudocimus ruber*) from Para, purchased; a Testaceous Snake (*Ptyas testacea*) froms California, received in exchange.

OUR ASTRONOMICAL COLUMN.

NATAL OBSERVATORY.—The superintendent of the Nata Observatory, in his report for the year 1890–91, tenders hi obligations to no less than seven ladies, without whose zealou assistance, he says, the greater part of the numerous astronomica computations, &c., would not have been carried out. Although lacking such aid as is consistent with the proper working of an Observatory, a great amount of very useful work has been accomplished. For instance, the entire mass of meridian observations of the moon made at Greenwich during the period 1851–1861 have been reduced and compared with the theoretical basis of Hansen's Lunar Tables, thus completing the whole number of lunar observations up to the year 1890. The work with the transit, magnetic transit, and equatorial have been continued as usual. For the determination of the latitude of the Observatory 1022 observations of thirty-five pairs of stars have been obtained. Owing to the close proximity of the equatorial and transit instruments, we are informed that it is impossible to use them both at the same time; this should be at once remedied, for the Observatory does not seem to be supplied with many surplus instruments.

The meteorological observations have been made regularly throughout the year. We hope, now that provision has been made for supplying a rain gauge and set of thermometers for each of the coast magistracies, that the Observatory will still continue to urge the necessity of maintaining and extending the system of weather reports, in the interests of the Colony, for, as is now well known, the value of such observations is only maintained when the stations are *numerous* and well *distributed*.

GEODETIC SURVEY OF SOUTH AFRICA.—Since the issue of the last (Jan. 1891) report by H.M Astronomer, Dr. Gill, on the Geodetic Survey carried on in South Africa, the work has been progressing very successfully and swiftly, an average of five principal stations being occupied and completed every month by a single observer. On May 31, 1891, the field work as far as Modder River was completed, the site for the base line being reached the following day. Some difficulty was here encountered with regard to the selection of the position for the base, but it was eventually fixed near Kimberley, the permanent camp being fixed about eight miles from this place. The total length of the measured base was 6000 feet, and it was divided into sections of 500 feet, since this seemed "a convenient length for a forward and backward measurement in one day." The figures given in this report, although uncorrected for sea-level, &c., speak well for the accuracy of the undertaking, as will be seen from the following table. Each length of 500 feet was measured both forward and backward, and it is the differences of these measurements that are here shown:—

Section.	F − B in feet.	Section.	F − B in feet.
I.	+ 0·0025	VII.	+ 0·0014
II.	− ·0020	VIII.	+ ·0011
III.	− ·0006	IX.	+ ·0014
IV.	− ·0040	X.	+ ·0009
V.	+ ·0019	XI.	+ ·0028
VI.	− ·0019	XII.	+ ·0015

The probable error of the whole base was ± 0·028 inches. The lengths of the two sections came out as—

$$M_{i} = 2999{\cdot}4445 \text{ feet}$$
$$M_{ii} = 2999{\cdot}7545 \text{ ,,}$$

The differences between the measured and the computed lengths of Section II. through the triangulation were: by the eastern triangles $M - C_1 + 0{\cdot}0035$ feet; by the western triangles $M - C_1 - 0{\cdot}0083$ feet.

During the triangulation work several observations for latitude were made at Tafelberg, Hanover, De Put, and Kimberley Camp, the results showing, as Dr. Gill points out, "that the abnormal deviation of the plumb line found along the coast in the neighbourhood of Port Elizabeth had disappeared." The report concludes with the determinations of the observers' personal equations and two diagrams of the triangulation.

THE INTERNATIONAL CONGRESS OF EXPERIMENTAL PSYCHOLOGY.

WHEN the first Congress on this subject met in Paris in 1889 under the presidency of Prof. Ribot, and with Prof. Charles Richet for its secretary, it proved a vigorous and most successful attempt to gather together from all parts of the world the students of a difficult branch of learning in which some methods of modern physics are being used in psychology, and these methods, or at least their results, are invading the province of what our ancestors would have preferred to call metaphysics. In the opinion of many of the most thoughtful students of the subject it has been considered an important point to keep up the connection between the physiological and the psychological sides of the questions under discussion, and the present Congress under the careful and admirable presidency of Prof. Henry Sidgwick, has proved very successful on this point, and has led to much pleasant acquaintanceship between those whose general work lies in different branches of learning. At Paris the full number at the Congress was about 150, and very little notice was taken of it in England; but at this recent Congress in London there have been nearly twice as many members, and it has received 70 or 80 visitors from all parts of Europe and from the United States and Canada. The vice-presidents have been Prof. A. Bain, Prof. Baldwin, Prof. Bernheim, Prof. Ebbinghaus, Prof. Ferrier, Prof. Preyer, Prof. Delboeuf, Prof. Liégeois, Prof. Preyer, Prof. Richet, and Prof. Schäfer. Among the other well-known names of the visitors there were those of Helmholz, Binet, Ribot, Henschen (Upsala), Münsterburg (Freiburg), and among the English names Herbert Spencer, Francis Galton, Prof. Oliver Lodge, Prof. Victor Horsley, Dr. Lauder Brunton, and Dr. Hughlings Jackson. The honorary secretaries were Prof. James Sully and Mr. F. W. H. Myers. The rooms of University College were kindly lent to the Congress by Mr. Erichsen for its use during the four days of the meeting (Aug. 1-4). Prof. Sidgwick's address attracted a large audience. He expressed himself as feeling it his first duty to apologize for the choice of England as the place of meeting, inasmuch as England could not be said to be the country which had done most for experimental psychology which, in the common meaning of the terms, had been most advanced in German and French laboratories, and was making recent and rapid progress in America. However, in a slightly different sense of the word the English school of psychologists from Locke and Hume down to Bain and Herbert Spencer had been for the most part experimentalists or at least empiricists. They had before them at this Congress a very wide range of subjects, too extensive he thought on the whole to be covered by the term "Psychologie Physiologique," which had been used at Paris as the name of their first Congress, and he thought "Experimental Psychology" more appropriate. In laboratory work the leadership was taken by Germany; in hypnotism France was our master and Germany our colleague. He was glad to see some of the leaders of the Nancy School with them that day, as he thought they were taking the broader lines in the subject, and that Europe was certainly not inclined on the whole to narrow the subject. He would not attempt to discuss the larger questions at that time, but would confine himself to the harmless task of explaining the arrangements that were proposed. In the morning meetings the Congress would be divided into two sections, of which Section A would be devoted to neurology and psycho-physics, and Section B to hypnotism and cognate questions; in the afternoon there would be general meetings.

The address was very warmly received, and Prof. A. Bain, in reading the first paper took the opportunity of expressing his gratitude to Prof. Sidgwick and the secretaries for the energy they had shown in bringing together such a large group of men who were glad to make each other's acquaintance. He went on to read an interesting paper on the advantages in psychology of introspection on the one side and experiment on the other, and the ways in which one could help the other. Prof. Charles Richet went on to discuss some of the possible prospects of psychology, and to express a hope that some of the most difficult subjects, such as thought-transference and clairvoyance, might be helped by the minute study of the process of development of the human mind. Prof. Gruber (of Roumania) then gave a very vivid sketch of the remarkable association of colour with sound, which he had spent many years in observing. To a very small number among his best educated patients the sound of the vowel "e" was accompanied by a sensation of yellow colour, of "i" by blue, of "o" by black, and so on through the long list of the Roumanian vowels and diphthongs, and also to some extent with numbers. The same colour was not always induced by the same sound in different patients, but the observations had been carefully tested. Prof. Pierre Janet related in detail a long case of complete loss of memory for present events and complete incapacity for any decision (*l'aboulie*) which had been suddenly brought about by the foolish jest (on August 28, 1891) of telling her what was not true, viz. that her husband was dead. The most curious points were that the loss of memory extended backwards as far as July 14, 1891, *i.e.* of what had happened during the six weeks before the accident, though the natural memory was complete up to July 14, and the patient's sub-conscious memory of all that had happened after that could be easily demonstrated by her automatic writing and by unconscious speech in a normal or hypnotic sleep. Prof. Ebbinghaus, in criticizing the paper, remarked that the woman's state seemed best explained as a condition of such complete distraction by things without that she had no power to attend to things within. Mr. Myers cited a case described by the elder Despine in 1830, in which there was a description of double memory and double personality such that the woman in the second state could eat and drink like a drayman, but soon reverted with no memory to her first state, and asked pitifully for her usual four teaspoonfuls of arrowroot.

Next day Section A and Section B went to work separately. In Section A Prof. Henschen (Upsala) read a paper which attracted considerable attention and consisted in a very careful examination of the exact tract of the visual path in man through the brain from the eye to the visual centre in the cortex of the calcarine fissure. It was admitted that it was not in accordance with the results of physiological experiments on animals; but the arguments for its proof in man were considered quite sufficient. Prof. Horsley followed with a paper on the degree of Localization of movements and correlative sensations, which roused some discussion; and then Prof. Schäfer brought forward careful experiments to show that there was no valid reason to attribute any intellectual powers to the prefrontal lobes of the brain; and Dr. Waller ended the work of the morning by illustrating the difficulties of accurately defining the functional attributes of the cerebral cortex.

In Section B Prof. Liégeois read a paper which M. Liébeault, of Nancy, had written along with him describing a case of suicidal monomania, which they had succeeded in curing by hypnotic suggestion. The President expressed himself much interested in the paper, and regretted that they could not see Liébeault among them, for he was a man who, after twenty-five years of contempt, had succeeded in making the world realize some new methods. Dr. Frederic van Eeden (Amsterdam) read a careful report of his five years' experience of the medical cases of hypnotism along with Van Renterghem in Amsterdam. He laid stress on the care which should be taken to avoid the distrust and prejudice caused by the abnormal facts of hypnotism in public exhibitions. With the upper classes he thought hypnotism more difficult than with the lower, for they objected, rightly, to a tone of command. Psycho-therapy with them must guide and support, but not command, and that it would do so even to the extent of curing some organic disease he regarded as well proved. Virchow's cellular pathology had neglected the psychical forces of the living cell. Now that these were acknowledged some principles of the old vitalism must revive. Prof. Bernheim read another more technical paper on hysterical amaurosis, explaining it as a purely psychical state brought about by suggestion, with which Dr. Bérillon could not agree, but Prof. Bernheim replied that there was nothing abnormal in hypnotism; there was no difference between normal and hypnotic sleep, though the two states were produced by different means. Further, there was not necessarily any sleep in hypnosis. It was a pity for that reason that the word had been chosen, for hypnotism meant simply suggestibility. Prof. Delboeuf took a similar view; to hypnotize a man was only to persuade him that he could do something that he thought he could not do. Supposing the man thought he had a pain, to hypnotize him was to make him sure he had not. Dr. Bérillon preferred to define hypnotism as the psychical state in which the cerebral control had been taken away artificially, and the patient became an automaton for any use. Such automatism was not in any way necessarily injurious to the subject, and was certainly useful in some diseases.

In the general afternoon meeting there were elaborate theories of colour perception well explained to the Congress both by Prof. Ebbinghaus and by Mrs. C. L. Franklin; and Prof. Lloyd Morgan attempted the difficult task of defining the limits of animal intelligence, chiefly as shown by the dog, whom he was sorry not to be able to credit with as much power of introspection as many of his friends. After some slight discussion on this, Dr. Bramwell (of Goole) brought forward four subjects from Yorkshire, on whom he showed some of the common phenomena of hypnotism and related some of his experiences in recent medical practice, which he had been able to show to doctors in Leeds and elsewhere, *e.g.* that he had been able in a few cases to produce by hypnotism, at a time when the patient seemed fully awake and normal, a state of local anæsthesia to allow a dentist to extract seven double teeth without any pain to the patient.

On Wednesday morning, in Section A, Prof. Heynaus (of Copenhagen) read a paper on the relation of Weber's law to the phenomena of the inhibition of presentations; Dr. Mendelssohn (St. Petersburg) on the parallel law of Fechner; Dr. Verricst (Louvain) on the physiological basis of rhythmic speech; and M. Binet (Paris) on the psychology of insects, showing that in the Coleoptera the dorsal nervous centres were motor and the ventral sensory. In Section B Prof. Delboeuf pointed out the remarkable power of the somnambulist in judging of the length of passing time without any watch or instrument. He had found some simple Belgian countrywomen when hypnotized able to carry out suggestions at any time he liked to name from 300 to 3000 minutes, and he thought the subject deserved further inquiry. Prof. Hitzig (Berlin) brought forward a minute and careful physiological study of some attacks of sleep which had some resemblance to hypnotic conditions.—Mr. F. W. H. Myers showed from the reports drawn up by Mr. Kenlemans, Mrs. Verrall, and two other experimenters of some experience that in some cases, though probably only in a few, it was possible to induce hallucinations by such an experiment as crystal vision, *i.e.* the purely empirical process of looking steadily into a crystal or other clear depth or at a polished surface. These externalized images or quasi-percepts illustrated some little known points in conscious and sub-conscious memory. Prof. Pierre Janet corroborated Mr. Myers's results by some of his own, in which, for instance, dreams which had been manifest to the onlooker but unknown to the sleeper were brought within the sleeper's knowledge by gazing on a bright surface or by the essentially similar process of automatic writing. In the afternoon the President presented a very long report of careful detail of a census of hallucinations which had been agreed upon at the Congress in Paris in 1889, and which had been carried out in England by himself, in America by Professor William James, and in France by M. Marillier. The question asked in England had been, "Have you ever, while in good health, and believing yourself to be awake, seen the figure of a person or heard a voice which was not in your view referable to any external cause?" In England 17,000 answers had been obtained, and about 1 in 10 persons (taken at random) who had answered had had some such hallucination in their lives. The great majority of these hallucinations consisted of realistic appearances of living men, a small minority of dead persons, and a still smaller group of grotesque objects. A remarkable class was that of hallucinations of several persons at one time—collective hallucinations; and a still more remarkable class was of those coincidental with some distant event unknown to the percipient, such as the death of the person whose figure appeared. The President came to the conclusion that after careful allowance for all sources of error, the probability against these coincidences being chance was enormous, and if the hypothesis that they were not casual was to be accepted, the assumption of the inaccuracy of the informants and inquirers must be strained to an extreme pitch. M. Marillier explained that it had been very difficult to get any large number of answers in France because of the dislike shown by the French to answer any psychological questions about themselves.

On Thursday morning, in Section A, Dr. Donaldson gave an interesting account of the minute investigation of the brain of Laura Bridgeman, the well-known blind deaf mute, who died in 1889 in Boston. There was depression of the motor speech centre, with slender sensory nerves and somewhat thin cortex over the areas of the defective senses. In Section B Dr. Bérillon raised a lively debate by describing the good effects he had brought about by hypnotism in the education of about 250 children, who were suffering from many childish discomforts, such as night-terrors, insomnia, somnambulism, or faults, such as kleptomania, idleness, cowardice, &c. After this Mrs. H. Sidgwick gave a summary of some experiments in thought-transference she had made, with the help of Miss A. Johnson and Mr. G. A. Smith as hypnotiser. By thought-transference she meant the communication from one person whom they called the agent to another, whom they called the percipient, otherwise than through the recognized channels of sense. The successful percipients were seven in number, and had generally been hypnotised. They had succeeded in transferring numbers, mental pictures, *i.e.* mental pictures in the agent's mind, and induced hallucinations given by verbal suggestion to one hypnotic subject, and transferred by him to another. In the total number of experiments the number of failures was much larger than of successes, but as the antecedent probability could in most cases be accurately determined, the proportion of successes was amply sufficient to show that the result was not due to chance. The many precautions necessary to such experiments were described in detail. One percipient succeeded in the experiments with numbers when divided from the agent by a closed door at a distance of about 17 feet. Attention was called to the great variability of results with the same percipients and agents for which they had not been able to discover any reason. An account was added of some experiments in producing local anæsthesia under conditions apparently excluding all suggestion other than mental. The President wished to remark that he thought it important in such experiments that all the failures should be recorded as well as the successes. In the afternoon, after papers by Dr. Lightnar Witmer, Dr. Wallaschek, and Prof. von Tschisch, the President put several questions to the vote as to matters of future organization, and it was decided to hold the next international Congress in Munich in 1896, with Prof. Stumpf as President and Baron von Schrenck as secretary. A suggestion was also made that there should be an extraordinary meeting in America next year, and a small American committee was appointed to consider this. After a hearty vote of thanks to the President and Secretaries, and a brief reply, the Congress was dissolved.

SOCIETIES AND ACADEMIES.

PARIS.

Academy of Sciences, August 1.—M. de Lacaze-Duthiers in the chair.—On boron pentasulphide, by M. Moissan. If the tri-iodide of boron, instead of being treated with sulphur in the dry way at a low red heat, as in the preparation of boron trisulphide, be mixed with sulphur and dissolved in carbon bisulphide at the ordinary temperature, boron pentasulphide is obtained. It fuses at 390°, and does not pass through the pasty state. In contact with water it forms boric acid, sulphuretted hydrogen, and a precipitate of sulphur. Mercury and silver reduce it to the trisulphide, forming metallic sulphides. Heated to fusion in a vacuum it decomposes into sulphur and the trisulphide. Its density is 1·85. It is very difficult to obtain free from iodine, but in all the preparations the ratio between the boron and the sulphur has indicated the formula B_2S_5.—On the stripped plants of autumn, and their utilization as green manure, by M. P. P. Dehérain. It has been found that by planting the ground with vetch or mustard, and digging it in during the autumn, the amount of nitrogen retained in the soil was nearly doubled.—Remarks on alimentation in the Ophidia, by M. Léon Vaillant.—A report on the great anaconda of Central America kept in the reptile menagerie. Since 1885 the snake has eaten on the average five times per annum, its nourishment consisting of goats, three rabbits, and one goose. The interval between two meals was in one instance 204 days.—On symmetric tetrahedral curves, by M. Alphonse Dumoulin.—On Stokes' law, its verification, and interpretation, by M. G. Salet.—A spectrum, given by a spectroscope with quartz prisms, is received on the fluorescent substance contained in a Soret eye-piece. It is then projected transversally on to the slit of a second spectroscope. Through this the diagonal spectrum of Stokes' classical experiment is seen with perfect definition, no ray exceeding the theoretical limit. The law thus verified can also be deduced from thermodynamic considerations. According to Stokes' law, "the rays emitted by a fluorescent substance always have a smaller refrangibility than the exciting rays." If it were possible to transform yellow into violet light by fluorescence, many chemical reactions would become possible which only occur at the higher temperature at which violet appears in the spectrum. This would be equivalent to the passage of heat from a colder to a hotter body, in contradiction to the second law of thermodynamics.—Constitution of pyrogallol, by M. de Forcrand.—On Cascarine, by M. Leprince.—Physiological examination of four cyclists after a run of 397 km., by MM. Chibret et Huguet. This distance, which was covered by the youngest of the party, an Englishman of 18, in seventeen hours, was that between Paris and Clermont-Ferrand. It was found that the temperature was at the finish rather below than above the normal; that the coefficient of utilization of urinary nitrogen varied inversely as the degree of fatigue, and that therefore a decided waste of nitrogen is a concomitant of excessive fatigue. The nutriment taken during the course consisted of much alcohol, champagne, beef-tea, and Kola solution in the case of the Englishman. He and the next in speed both took Kola. The winner was extremely fatigued at the finish; the next man, a Frenchman of 28, not at all. His pulse was beating at 60, that of the former at 84. The coefficients of utilization of nitrogen were 76·32 and 58·27 per cent. respectively.—On the properties of the vapours of formol or formic aldehyde, by MM. F. Berlioz and A. Trillat.—Subcutaneous grafting of the pancreas: its importance in the study of pancreatic diabetes, by M. E. Hédon.—On the habits of *Clinus argentatus* Cuv. and Val., by M. Fréderic Guitel.—On a Permian Alga, with its structure preserved, found in the boghead of Autun: *Pila Bibractensis*, by MM. C. Eg. Bertrand and B. Renault.—The chalk of Chartres, by M. A. de Grossouvre.

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