THURSDAY, AUGUST 25, 1887.

## THE HEALTH OF NATIONS.

*The Health of Nations.* A Review of the Works of Edwin Chadwick, with a Biographical Dissertation. By Benjamin Ward Richardson. 2 vols. (London: Longmans, 1887.)

DR. RICHARDSON'S two volumes afford much matter for reflection for all those who have endeavoured to improve the condition of the working classes in England during the last half century. They form a panegyric on Mr. Chadwick, and boldly claim for him the credit of having brought forward the principal social improvements of the Victorian era. We think that these wide claims are somewhat to be regretted, as they compel criticism where we should be anxious to speak only in praise; for we are scarcely prepared to go the length of ascribing almost entirely to Mr. Chadwick's influence the vast improvements in the social condition of the people which have taken place during that period.

Mr. Chadwick's active life commenced at a time when the dawn of a new state of things was appearing in this country, and indeed over the world; when, by the development of new means of communication and intercourse, all society was beginning to be completely revolutionized. He was a deep thinker, and seems to have understood intuitively the social problems which were arising; but he undoubtedly had the despot's view that whatever he thought good ought to be carried out. He may be said to have begun his career as Secretary to the Poor Law Board, and then as Commissioner. He was the member of that Board who most persistently urged the extension of the areas of administration, and the employment of paid officers instead of gratuitous service which rewarded itself by favouritism and jobbery. The Poor Law as amended at that time, and as worked by the then Poor Law Commissioners, was devised to abolish out-door relief to the able-bodied, and to apply a labour test for all able-bodied persons who sought the temporary relief of the workhouse; and Mr. Chadwick's fearless administration of that rule brought upon him much enmity from the supporters of the former system of local jobbery. In the half century which has elapsed since that time, there has undoubtedly been a gradual drifting back to the old methods; and it would certainly be an opportune time to make a new inquiry into the administration of the Poor Law on the lines pursued by Mr. Chadwick in 1832.

The investigations of the Poor Law Commissioners brought to light the vast importance of the sanitary problem, which, of all the social problems of that day, was probably the one that cried most for consideration. The chief advance in medical science during the hundred years previous to the Victorian era seems to have resulted from the discoveries by Jenner in regard to smallpox. Beyond this the art of prevention of disease, at the Queen's accession, rested mainly on the laurels gathered by Lind and Meade in the eighteenth century, and by Pringle during the great war. The principle of prevention enunciated by these early pioneers still remains the foundation of our sanitary system; but the practical application of those doctrines has received an enormous extension during the last fifty years; and the various essays and reports by Mr. Chadwick, collected by Dr. Richardson, show that he was undoubtedly the first person who made it his business to impress the nation with the fact that public health was a public question. From the official position occupied by Mr. Chadwick during the earlier years of the Queen's reign, he had an immense influence, which he exercised with all the energy of his nature, in bringing to the front the question of public health; and it may be safely affirmed that the remarkable Report of the Poor Law Commissioners in 1842, which was drawn up by Mr. Chadwick, laid down almost all the sanitary principles upon which the sanitary legislation of the last forty years has been based.

The Report of 1842 led to the Health of Towns Commission and other inquiries into public health, and paved the way for the Public Health Act of 1848. In one sense Mr. Chadwick was admirably adapted for this service. He was gifted with indomitable perseverance, and with a clear insight into what he wanted to obtain. He sought nothing for himself. His only object was to promote the views which he considered beneficial to the public, and to compel their adoption in whatever way he could. But unfortunately he was not gifted with that most valuable quality which may go a long way to secure results which talent alone may fail to obtain, viz. tact. Through this quality alone many of those changes and improvements, which necessarily injuriously affect some persons or classes of the community, can be brought into operation. Had Mr. Chadwick possessed tact, and been satisfied with obtaining reform in instalments and by slow degrees, he would probably have become one of the greatest powers in the country. But Dr. Richardson's description of the way in which Mr. Chadwick acted at the Poor Law Board shows how impossible it was for a man of his nature to remain long in a public department.

Whilst, however, Mr. Chadwick had the foresight to shadow out, in the Report published by the Poor Law Commissioners in 1842, all the improvements which have taken place up till this time, the working out of the various problems has been due to many others besides himself: and, prepared as we are to award a full meed of praise to Mr. Chadwick for his foresight and energy, by which he, and he alone, made the health of the nation a public question, we regret that the author of these volumes should have somewhat ignored the efforts of many of those who were mainly instrumental in raising the superstructure on the foundations laid by Mr. Chadwick. For it cannot be denied that in the early efforts at sanitation made by the Poor Law Commissioners, and enforced by the Board of Health, many grievous mistakes were committed. For instance, in the Report of 1842 the Poor Law Commissioners recommended, and their recommendation was largely adopted, that all refuse should be at once discharged into the drains and sewers, as the cheapest means of getting rid of it from the houses, although the sewers were avowedly at that time not constructed to remove the fæcal matter, and no provision was made to prevent it from lodging in them as fœtid mud, or passing into and polluting the water-courses: indeed, the Report stated that that danger was a smaller evil than the retention of refuse in the houses. This recommendation entailed a new class of evils, which has resulted in a very large expenditure and loss of life. No doubt the removal of refuse in this way was fairly simple, and certainly economical, until it created new evils whose remedy was very costly; but no one can say that the retention of the refuse in the houses might not have been prohibited, and the removal effected in some other manner, which, although possibly more expensive at the time, would not have been followed by disastrous consequences.

As an instance of the evils which the want of foresight entailed in the earlier introduction of the water-carriage system, the drainage of Croydon may be mentioned. This was executed directly according to the then views of the Board of Health. Soon after its introduction a most virulent fever broke out in Croydon, owing to the fact that the system totally ignored the ventilation of the sewers in any other way excepting into the houses themselves.

The application of sanitary science to practical life has arrived at its present state like most English matters, where action comes first and reflection afterwards; that is to say, in the elaboration of the early ideas at a great expenditure of money and experience, many blunders have been committed and many failures have ensued. The present condition of the practical application of sanitary science to the health of the nation rests upon the labours of many men. But although we may perhaps regret that Dr. Richardson's volumes attribute to Mr. Chadwick a larger share in the social changes which have taken place during the last fifty years than he is actually entitled to, yet all sanitarians are ready and willing to accord to him a very high place as a leader in the sanitary movement during Queen Victoria's reign. So long as he retained his office at the Poor Law Board, or in the General Board of Health, Mr. Chadwick laboured unceasingly to lay the foundations of our present system of public health; but in the erection of the superstructure we owe our gradual approach to practical perfection to many others, of whom it is only necessary to mention two or three.

Dr. Farr placed the vital statistics of the country upon a scientific basis. Mr. Humphry tells us that Dr. Farr received, in 1838, his appointment under the first Registrar-General, in consequence of his papers on benevolent funds, life assurance in health and disease, and various other statistical papers, and on the recommendation of Sir James Clark. Dr. Sutherland was an energetic worker in the Health of Towns Commission, and he, with Miss Nightingale, was the chief adviser of Mr. Sidney Herbert in his efforts to place army sanitation on a sound basis; and he has ever since continued as sanitary adviser of the War Office and India Office. Sir Robert Rawlinson is acknowledged to be the highest authority on modern sewerage. Sir John Simon began his admirable reports with the Public Health Act of 1848, and continued them until soon after the formation of the Local Government Board in 1875.

Having thus briefly mentioned some of those to whom credit should be given as prominent among the originators of the health movement which has prevailed in England during the last fifty years, we may consider what are the broad principles which underlie the reports and papers of Mr. Chadwick, edited by Dr. Richardson, and which, indeed, are the doctrines accepted to-day by most sanitarians. Practically, they advocate State socialism; and it is impossible to maintain large communities in a due state of health and a due condition of morality in any other way than under some form of State socialism. Our population is aggregating more and more into towns; but how little do we attend to the decencies or the amenities of life in the masses of population we allow to assemble! A leading sanitarian some forty years ago wrote:—

"If there be citizens so destitute that they can afford to live only where they must straightway die—renting the twentieth straw-heap in some lightless fever-bin, or squatting amid rotten soakage, or breathing from the cesspool and the sewer; so destitute that they can buy no water—that milk and bread must be impoverished to meet their means of purchase—that the drugs sold them for sickness must be rubbish or poison; surely no civilized community dare avert itself from the care of this abject orphanage.

"It may be that competition has screwed down the rate of wages below what will purchase indispensable food and wholesome lodgment. But all labour below that mark is masked pauperism. Whatever the employer saves is gained at the public expense. When, under such circumstances, the labourer, or his wife or child, spends an occasional month or two in the hospital, that some fever infection may work itself out, or that the impending loss of an eye or a limb may be averted by animal food; or when he gets various aid from his Board of Guardians, in all sorts of preventable illness, and eventually for the expenses of interment, it is the public that, too late for the man's health or independence, pays the arrears of wage which should have hindered this suffering and sorrow.

"Before wages can safely be left to find their own level in the struggles of an unrestricted competition, the law should be rendered absolute and available in safeguards for the ignorant poor—first, against those deteriorations of staple food which enable the retailer to disguise starvation to his customers by apparent cheapenings of bulk; secondly, against those conditions of lodgment which are inconsistent with decency and health."

Since these words were written it has been made the care of the community to remove refuse, to insure a good water-supply, to prevent adulteration of food, and to close unhealthy dwellings; but many wretched dwellings exist, and starvation wages still remain a disgrace to a country which calls itself Christian. The whole of Mr. Chadwick's papers, and indeed the arguments of all the most advanced sanitarians, are a protest against the doctrine of "laissez-faire," which emanated from the school of political economists in the earlier part of the century. And we are daily becoming more and more alive to the fact that this doctrine of "laissez-faire" is incompatible with the healthy existence of large communities. This form of socialism is one that should commend itself to all thinking men, for it is quite certain that in these days of advanced intercourse and universal education a helot class consisting of the many living in misery side by side with the few living in luxury is a condition of things which cannot be permanently maintained. The fact that Mr. Chadwick was the first person to bring this subject prominently forward and to compel Parliament to recognize that the public well-being is a public question, will always cause his name to be remembered with respect.

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## *THE FORESTRY OF WEST AFRICA.*

*Sketch of the Forestry of West Africa, with Particular Reference to its Principal Commercial Products.* By Alfred Moloney, C.M.G., of the Government of the Colony of Lagos. (London: Sampson Low, Marston, Searle, and Rivington, 1887.)

THIS, as its title indicates, is intended to form a handbook to the economic plant-products of Western Africa. Although the author is Governor of a British colony in this region, his remarks are by no means confined to British possessions, but are intended to include all that is at present known of economic interest connected with the plants of Western Tropical Africa.

Following Prof. Oliver, the author deems it expedient to divide Western Tropical Africa into two principal geographical regions. The first, called Upper Guinea, includes the western coast region from the River Senegal on the north to Cape Lopez immediately south of the equator; the interior drained by rivers intermediate between these limits, and the small islands of the Gulf, Fernando Po, Prince's Island, St. Thomas, and Annabon. The second region, called Lower Guinea, includes West Tropical Africa from Cape Lopez southward to the Tropic of Capricorn, including Congo, Angola, Benguela, and Mossamedes. Within the limits here indicated we have British possessions represented by "colonies" and "protected territories," and we have numerous possessions claimed by the French, Portuguese, Spanish, and German Governments, some of which have only lately been acquired in the European scramble for African territory. It is only right to mention that the term "possessions," as here applied, is somewhat a misnomer. There is little practically possessed, even by ourselves, except a slender coast-line: the interior is described as having no "territorial definiteness," and it is politically, no less than scientifically and commercially, unexplored. Capt. Moloney has wisely not attempted to treat separately of the economic products of these possessions. He has taken their present economic botanical productions in order of export value, and we find that these consist chiefly of palm oil, ground nuts, india-rubber, coffee, gum, dye-woods, cacao, cotton, fibres, and timbers. Palm oil, the produce of *Elæis guineensis*, a plant which covers immense tracts of country in Western Africa, is imported to this country to the value of nearly a million and a quarter annually. The yellow palm oil is obtained from the outside fleshy portion (sarcocarp) of the nut, while a white solid oil is obtained from the kernel. India-rubber is another West African product obtained chiefly from climbing vines belonging to the genus *Landolphia*. The author was one of the first to draw attention to the value of *Landolphia owariensis* as a rubber-plant, and it must be gratifying to him to find that the exports of "white African rubber," as the produce is called, have during the last four years risen from almost nothing to a value of nearly £36,000. What is known as "Yoruba" indigo, derived from a large tree, *Lonchocarpus cyanescens*, has evidently a commercial value, but at present it is used to mix with butter or "shea" to make the negroes' hair a fashionable gray!

Numerous West African plants are cited as yielding either gum tragacanth, copal, frankincense, gum-arabic, bdellium, or resin; what is called "ogea" gum, derived from an unknown tree, *Daniellia* sp., is used powdered on the body and as a perfume by women. The true frankincense-tree of Sierra Leone is *Daniellia thurifera*. Camwood, used largely as a dye, is derived from *Baphia nitida*; but although barwood is generally said to be derived from the same source, it fetches only one-sixth the price of the former. The medicinal properties possessed by numerous West African plants is a subject full of interest.

Various species of *Strophanthus*, the active principle of which was formerly used for poisoning arrows and is known to be of incalculable benefit in cardiac diseases, and the merits of the "miraculous berry" (*Sideroxylon dulcificum*) of the Akkrah and Adampe districts, which is credited with rendering the most sour and acid substances "intensely sweet," and of the "oro" plant of Sierra Leone, said to act as an irritant poison cumulative in its effects (which has been ascertained at Kew to be a species of *Euphorbia*), are among the numerous subjects requiring further investigation.

A most cursory glance at this book cannot fail to suggest the wonderful wealth both of botanical and industrial problems which are yet unsolved in connexion with West Tropical Africa. The "Flora of Tropical Africa," by Prof. Oliver, of which three volumes are published (the last in 1877), has made a beginning in the work of elucidating some of these problems; but in recent times few men have systematically pursued West African botany, and the entire absence of a resident botanist or of a properly-equipped botanical establishment in any of our West African colonies has left the plants of a most important region to be known only by the intermittent collections of travellers who have either perished there before their mission has been completed or have hastened home to avoid the effects of the deadly climate.

Nearly 200 pages of Capt. Moloney's book are taken up with condensed notes and references to the economic plants of Western Africa arranged in natural orders according to the "Genera Plantarum" of Bentham and Hooker. To many people both in West Africa and at home these notes, brought together by the assistance of an officer connected with the Kew Museums, will prove of great value. In the appendices are given a copy of the instructions for collecting plants, seeds, and useful plant-products issued by the Royal Gardens, Kew; an ornithology of the Gambia, by Capt. Shelley; a list of Coleoptera and of diurnal Lepidoptera of the Gambia, by the same writer; and a list of reptiles, batrachians, and fishes collected at the Gambia by Capt. Moloney in 1884–85.

The book is well got up and clearly printed, but it has the unpardonable defect of being published without a good alphabetical index. This greatly detracts from its value as a book of reference. It, however, is the chief fault we have to find with a work full of interesting matter for the first time brought together, and evidently prepared with great care.

D. M.

## *OUR BOOK SHELF.*

*Annals of the Astronomical Observatory of Harvard College.* Edward C. Pickering, Director. Vol. xvii. (Cambridge: John Wilson and Son, 1887.)

THIS volume of the Annals of the Harvard College Observatory contains the description and theory of the instrument invented by Mr. S. C. Chandler, and called by him the almucantar, as well as the reduction and discussion of a series of observations made with it at the Observatory in 1884 and 1885. The instrument consists of a telescope mounted upon a base that floats in mercury, and the observation consists in noting the time of transit of a star across an almucantaral (or horizontal) circle, the particular horizontal circle which the inventor has found most convenient being that passing through the Pole, which he has called the "co-latitude" circle. If, therefore, the telescope be clamped at the given altitude, "the sight-line will mark accurately in the heavens a horizontal circle: and the transits of stars, as they rise or fall over this circle in different azimuths, will furnish the means of determining instrumental and clock corrections, the latitude, or right ascensions and declinations." Mr. Chandler believes that an instrument on the almucantar principle is capable of giving results more free from both accidental and systematic errors than those obtained from a meridian circle, and certainly the discussion of his observations contained in the volume before us goes far to justify such a belief. The probable accidental error of a single observation in zenith distance is $\pm 0''.404$, whilst for stars north of 60° declination it is as small as $\pm 0''.379$; the probable accidental errors of the clock corrections from a complete transit (including the residuals for Polar stars) are $\pm$ 0·047s. and $\pm$ 0·043s. for two observers. And these results have been obtained, it must be remembered, with a telescope of only 4 inches aperture and less than 44 inches focus. The chief advantage of the system is, however, that it gives measurements of both co-ordinates of a star which are absolutely free from the effects of flexure, and also of refraction as far as it depends on zenith distance. The almucantar certainly appears to be a valuable addition to our means of attacking difficult problems of practical astronomy.

*The Distribution of Rain over the British Isles during the Year* 1886. Compiled by G. J. Symons, F.R.S. (London: E. Stanford, 1887.)

MR. SYMONS explains that the delay in the appearance of this volume is due chiefly to the exceptional character of many of the phenomena of the year 1886, and partly to some observers not having had sufficient health, or courage, or interest in their records, to induce them to face the snowstorms of March 1 and December 26. The volume contains, besides articles upon various branches of rainfall work, the results of observations made at nearly 2500 stations in Great Britain and Ireland. In the various sections the compiler has brought together an immense mass of information, and he has taken great pains to present his facts clearly. There are several illustrations, in one of which he shows the fluctuations of annual rainfall from the year 1726 to 1886.

## *LETTERS TO THE EDITOR.*

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

### Slate Ripples on Skiddaw High Man.

THE slate ripples on Skiddaw are not, so far as I am aware, mentioned by writers on the Lake District, geological or otherwise. Their peculiar character puzzled me so much, after noticing them on Saturday, July 23, that I visited the spot again on the 30th to see whether the origin which suggested itself to me was probable.

Following the pathway from Keswick, you pass through a small gate a little way up the final ascent, from the dip between Skiddaw Low Man and High Man. Turning to the left along the wire fence, one comes, where it ends, to the best-developed of these peculiar ripple marks; but they extend upwards from here on the left (south) side of the pathway until you are more than half-way up to the first cairn. On the right (north) side the ripples begin later, extend higher, but are less distinct. They cease, apparently, simply from want of the clay foundation, which is an essential feature in their development.

The rippled areas are patches of bare clay or soil, from a few yards to half an acre or so in extent, coated with a thin layer of the slates, which elsewhere form the cap of Skiddaw High Man. The slate fragments, however, instead of being confused, form more or less regular lines, generally running north-west and south-east, but varying towards north and south or east and west, when the patches are small and longest in these directions. The greater the slope the greater appears to be the average size of the slates. The larger fragments average a foot by four or five inches, always lying lengthwise along the lines, which are seven or eight inches apart. The clay is washed out beneath the stones, which therefore do not rise above the general level. The clayey intervals have numerous smaller bits of slate, and are scored at right angles to the lines by the action of rain and wind on these. Of course there are always loose fragments not on the chief lines.

Obviously the slates are arranged by the wind, apparently without much aid from water, as the slopes would not let it collect. But it would be very interesting to have a complete explanation of the lines.

A suggestion, largely confirmed by my second visit, may at any rate help to solve the point, even if it is inadequate by itself. The hurricane force of Skiddaw storms, mostly from the south-west, no doubt drives before it the loose slates, sliding over the surface of the slates below. On reaching a bare patch, the front edges of the slates are stopped by the clay. Finally a sudden gust tilts them over. Thus a first line is formed. More slates slide, or are tilted over, upon the first layer, which have meanwhile worked down to the general level by rain action. The second set slide over the first set and are in their turn tilted over on reaching the far side. Thus a second line is formed, and the rest follow in the same way. On slopes larger fragments are moved than on the level; hence such are there found in the lines. In small areas, with their long axes not perpendicular to the prevailing winds, the general direction is modified by the natural position (according to the explanation here suggested) of the first line.

There was a moderate gale on my first visit, and only a stiff breeze on my second, neither enough to move stones. But on the latter occasion, hearing a strange hissing noise, I looked up and saw a violent eddy, 20 or 30 yards across, whirling small slates 20 to 40 feet into the air. This advanced from the south-west at the rate of 8 or 10 miles an hour, coming so close that some of the fragments fell around and on me.

Probably the lines are stationary, although the stones may pass from one to another. To test this, if possible, I took up on the second occasion seven small Permian sandstone pebbles from the shore and placed them a foot or less apart on the *windward* edge of a conspicuous line, sheltering each behind a narrow slate, hammered firmly into the ground. I am not likely to be up again, but should any of your readers be on the spot a few months hence they might find the line in question by ascending the path until the line of the Helvellyn range is above Skiddaw Low Man by about the breadth of a pencil held at arm's length. The line lies twenty-seven paces to the left of the path.

I might mention that the thermometer was at 45° on the top about half-past one or two, when the sun was clouded. Soon after four, at Crosthwaite, the same thermometer was at 63° in the shade.

J. EDMUND CLARK.

August 2.

### Dr. Klein and "Photography of Bacteria."

ALTHOUGH I feel indebted to Dr. Klein for his appreciation of my work as expressed in his review in NATURE of August 4 (p. 317), still I must ask him to allow me to correct a statement which detracts very much from the credit of photo-micrography. Dr. Klein says :—" In connexion with this it must certainly appear remarkable that in the numerous and important publications on Bacteria by Koch and his pupils since 1877 to the present time we do not find a single illustration represented by micro-photography. All their published illustrations are drawings."

In addition to the works I have quoted (viz. Koch, " Mitth. aus d. K. Gesundheitsamt," Band I, 1881, illustrated with an extensive series of photographs; Hauser, "Ueber Faulnissbakterien," 1885, illustrated with an elaborately reproduced series ; Van Ermengem, " Recherches sur le Microbe du Cholera," 1885, illustrated very successfully with twenty photographs), there are, since my work was published, Riedel, " Die Cholera," 1887, illustrated with most beautifully reproduced plates from negatives of comma-bacilli taken by Plagge and by Koch ; " Zeitschrift für Hygiene," Zweiter Band, May 1887, two publications, illustrated with a series of photographs by the assistants at Koch's laboratory ; Löffler, " Vorlesungen über Bacterien," May 1887, illustrated with reproductions from Koch's negatives.

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## THE LANDSLIP AT ZUG.

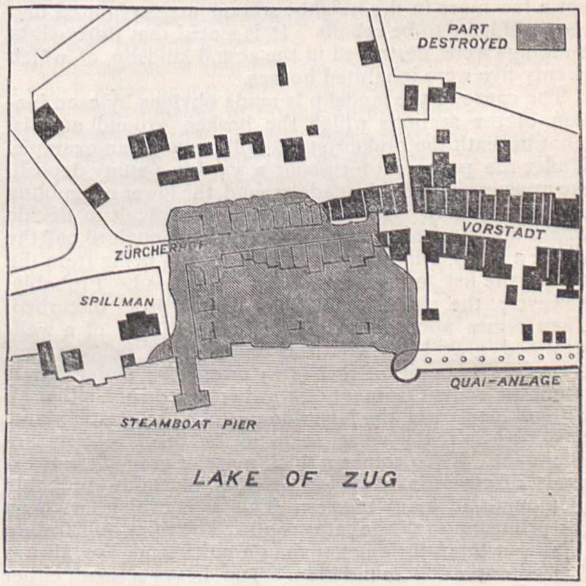
TO judge by the glimpses which I obtained of English newspapers during my late visit to the Alps, considerable misapprehension has prevailed in this country as to the nature of the disastrous landslip at Zug. For instance, one of the most important journals had a leading article on the subject, describing learnedly the fall of the Rossberg, the destruction of Plurs, and other like Alpine instances, with which the late calamity has no more connexion than the slipping of a piece of the Thames Embankment into the river would have with the fall of a peak of Snowdon. Hence, as I had the opportunity a short time since of visiting Zug, and in company with my fellow-traveller, the Rev. E. Hill, forming an opinion as to the cause of the accident, it may be worth while to give a few details. In drawing up this account I have used the abstract of a report by Prof. Heim, which takes the view which I had already adopted from examination of the locality, and has supplied me with a number of important details.

The newer part of the town of Zug stands on a plain which extends back from the lake to a considerable distance inland. Generally almost level, this at last shelves gently down, falling perhaps a dozen feet in the last hundred yards. The older part of the town occupies slightly rising ground between the water and hills which in England we should call mountains. Both parts, however, are not founded upon the rock, but upon a detrital deposit. Where are now the streets of Zug was once the lake : the streams from the adjoining hills have encroached upon its waters, and the town stands upon the delta which they have formed ; the older upon the coarser more pebbly material, the newer upon the finer and sandy, where, in prehistoric times, the piles of lake-dwellings were driven.

A few years since the people of Zug thought to improve and beautify their town by building an esplanade in the place of the old irregular shore of the lake. It is faced by a wall of solid granite, which rests on a foundation of concrete, supported by piles. Outside this the water deepens rather rapidly : still no great depth is reached. Twenty metres from the edge of the quay it is 9 metres ; at a distance of 100 metres it does not exceed 20, and even at a distance of 800 metres from the shore has only attained 45. The portion of the quay completed at the beginning of the present summer terminated for a time with a sort of bastion ; north of that the piles had been driven for some distance, but no masonry had been laid. Rather more than 100 yards in this direction from the end of the new wall was a steam-boat pier, constructed as usual of wood.

Twice already in its history has Zug been the scene of disastrous landslips, once in the year 1435, and again in 1594 ; so that some few months back, when formidable cracks and indications of settlement began to appear in the new quay wall, considerable anxiety was aroused. Prof. Heim, among others, was consulted, and was not able, as a geologist, to offer much consolation, for he could only say that the foundation on which the whole place rested was, as will be seen, naturally defective. Still, as things had on the whole held together in the past, so, after this protest on the part of Nature, they might continue in the future. Certain remedial measures were suggested, and a careful watch was kept upon the new structures.

The catastrophe, however, occurred without further warning on July 5. Suddenly, about four o'clock in the afternoon, a large piece of land, occupied by houses and gardens, between the bastion and the steam-boat pier, seemed to break up, descend almost vertically, and become ingulfed in the lake. It was a scene of wild and awful confusion, unhappily not unaccompanied by loss of life. A steam-boat had just come up to the pier : the waves broke the hawser and drove the vessel more than a hundred yards back into the lake. Here, however, all




escaped unhurt, but the occupant of a small boat was upset and drowned, and the landlord of an adjoining restaurant, who had gone from his garden with some guests to see what was happening (for the ground seems to have gone in a series of quickly successive slips, not in one single fall), when the earth cracked beneath his feet, sprang in the wrong direction and was ingulfed in the muddy whirlpool. Three children also perished in one of the fallen houses.

Again about seven o'clock another and a larger slip took place ; the destruction of property was greater, but this time without loss of life, for the people had taken the alarm and evacuated the houses. The dust from this ruin rose like a cloud, and was seen from the Rigi. Since then there has been no further slip ; indeed, as we read, no further movement ; for the cracks in neighbouring walls have been sealed up in many places, so that even a slight settlement could readily be detected.

The result of the landslip is as follows. A few months since there was a street in Zug running roughly parallel with the shore, terminated by a road leading to the steam-boat pier, and at the end, on the land side, was a good-sized hotel, while between the shed and the lake were gardens with cottages and other buildings. Where once were houses and gardens there is now a kind of bay of the lake. It is as though a pit had been excavated parallel with the shore, which, about 120 metres wide at the water-side, extends inland from 60 to 80 metres, widening as it does so on the eastern side to about 150 metres. This "harbour" is bounded by a low cliff, which rises gradually from a little above the water's edge to a height of about four yards; the surface, however, instead of being occupied by vessels, is a scene of the wildest confusion: slabs of pavement here, a pile of bricks there, the broken framework of a roof with its displaced tiles, a group of beams, some trees yet living, in one place the wooded gable of a house, project from the surface of the water, which is covered thick with timber and floating debris. A sadder scene of ruin it would be difficult to imagine. On the land side, part of the pavement of the street yet crests the little cliff, displaced near its edge by a series of vertical faults, with a throw of a few inches. Below, large slabs, with the squared blocks still in contact, lie at various angles on a slope of rubbish which just rises above the water. Houses, cracked and shattered, with their fronts in some cases partially fallen, look down on the scene of ruin, and not a few more in the neighbourhood are so injured that they will have to be rebuilt. It is stated that thirty-eight buildings were destroyed in the actual landslip, of which twenty-five were inhabited houses.

The cause of the landslip is made obvious by examination of the sections which the broken ground affords. That beneath the broken street will serve as an example. Under the pavement for about a yard is a stony deposit, the upper part probably made ground, the lower resembling a coarse gravel. As is natural, it is difficult to decide where undisturbed ground begins: it is enough to call the whole a stony soil, many of the fragments being from the size of the fist to nearly as big as the head. Probably, however, the lowest foot has been little disturbed. Then comes about fifteen or eighteen inches of a well-stratified gravel—rather iron-stained, the pebbles not exceeding a couple of inches in diameter; under this is about the same thickness of a rather peaty silt—either an old soil, or part of the lake floor, on which aquatic plants have grown; for what seem to be dead rootlets are abundant. Then comes a thick mass of gray silt. It extends downwards below the level of the lake—probably to a depth of many metres. This it is which has been the prime cause of the catastrophe. The thick substratum of silt, at times little better than a quicksand, has always formed an unsafe foundation. Too heavy a load, either locally by building too large a house, or generally by building many smaller dwellings, any weakening of the cohesion of the mass, exceptional seasons,[1] may at any time suffice to pull the trigger of a weapon which, so to say, is always charged. It is doubtful whether this part of the town can ever be regarded as absolutely safe: at the same time there have been but three slips in four centuries and a half, and no doubt precautions will be taken to reduce the danger to a minimum. It is possible that the building of the esplanade has been the immediate cause. Prof. Heim, however, does not so regard it, though I cannot say that his arguments entirely satisfied me. However, this is certain, that of the completed building only a few feet were damaged; the frontage which slipped was that into which piles alone had been driven.

The most remarkable thing about the slip is that the displacement has been nearly vertical. There has been but little outward lateral movement of the ruined buildings. As Prof. Heim words it in the above-named report, "Ground which formerly was from 6 to 2 metres above the water is now from 2 to 6 metres below it." The silty substratum must have flowed outwards into the deeper water, or in some way been displaced laterally to allow of the surface thus sinking. In accordance with this it is stated that the piles driven for the new wall—which were fixed in the silt alone—were thrust outwards for distances of from 100 to 300 metres from the shore, and were pushed up above the level of the water. The catastrophe, then, cannot be numbered with the bergfalls, or even with the ordinary landslips, though perhaps an analogy may be established with some sea-side slipping of cliffs; but it is none the less lamentable, for, in addition to five deaths, many families have lost their all—goods, house, and even the site itself being destroyed; and great additional expenditure will be required before the neighbourhood can be regarded as safe.

T. G. BONNEY.

[1] It is stated that the weather changed on the evening of July 5: storms and rain succeeding to a long period of dry weather. At the time the "ground water" beneath the town was rather above, the lake rather below, its usual level.

## THE NORWEGIAN NORTH ATLANTIC EXPEDITION.

NOT surpassed by the records of the Austrian Novara Reise, nor by those of our own *Challenger* Expedition, is the account of the Norwegian Expedition to the North Atlantic, the latest part of which is a Report on the Alcyonida, by D. C. Danielssen. Like the other parts of this Report the present forms a quarto or rather small folio volume, and contains over 160 pages of text with 23 plates and a map giving the details of the geographical distribution.

The author was one of the staff on board the *Vœringen*, and he now has the pleasure of describing the specimens collected, but he has not had the assistance of that excellent zoologist (Koren) whose able work on the Alcyonids of Norway had been executed in partnership with Danielssen, and whose death all those interested in natural science have to deplore.

The Alcyonids collected during the Norwegian Expedition are almost exclusively deep-sea forms: the depths varying from 38 to 1760 English fathoms. Among them there are no less than nine new genera, which all belong to the sub-family of the Alcyoninæ, with 33 new species, of which two belong to Clavularia, one to Sympodium, one to Nidalia, and the rest to the several new genera. There is also a new sub-family with a new genus and species described.

The author says quite truly, that, of all the large groups of the Alcyonaria, none have been treated more superficially by recent zoologists than that of the Alcyonids. No doubt there are many reasons for this; the delicacy of their structure, combined with the difficulties of their preservation in a state for minute investigation, has to some extent made their study a difficult one; and even the repeated endeavours of Mr. Danielssen to observe them in a recent state were unsuccessful. In regard to classification, the author for the moment follows that of Milne Edwards; in this we think he is correct, and we thoroughly agree with his reasons; for until the present material in the museums of Europe and America has been properly worked out, and much fresh material has been collected, any attempt to give a definite classification of the group will be so much lost labour.

In the diagnosis of the genera and species, especially of the latter, the form of the spicules, as well as their arrangement and position on the polyps, have been found of great value, though minuter histological details have not been used as much as they possibly will be in the near future. One very important and interesting fact is mentioned, viz. the discovery in a species of a new genus Vœringia of a nervous system. On the uppermost part of the ventral surface of the œsophagus there is to be found a group of large ganglion cells containing extremely large nuclei with viscid protoplasm and prolonged filaments. Mention is also made of the grooves lined with long flagelliform cells, which, however, were some time since described by Hickson in a paper published in the Philosophical Transactions under the name of "Siphonoglyphe."

Another novel phenomenon was observed in a species of Nephthya, where several of the polyps seemed to be solely reproductive, and in them as soon as fertilization was effected, the tentacles became incurved over the oral aperture, which then became plugged with a viscid mucous, and apparently during the gravid period these polyps were nourished by the other polyps of the colony.

We must content ourselves with giving but a very brief summary of the forms described. The genus Vœringia is established for a series of branched Alcyonids with retractile polyps, in this differing from those of Duva; eight new species of this genus are described, to which also the *Alcyonium fruticosum*, Sars, is referred. Eight new species of the beautiful genus Duva are recorded. A new genus, Drifa, is established for an arborescent species, the spicules in which differ from those of both Vœringia and Duva; of the two species, one, *D. islandica*, exhibits an interesting structure; around the mouth and between its external opening and the base of the tentacles, there are eight little fringe-like protuberances, which form a ruff. An appearance of the same kind, only outside the circle of the tentacles, we have observed in a Plexaurid, but we are not certain but that it may be due to the sudden immersion of the polyps into strong spirits. For a graceful arborescent form with auto- and siphonozooids, which reminds us of Anthomastus, Verrill, the genus Nannodendron is proposed; the polyps are completely retractile. *Fulla schiertzi* is a new genus and species of another branching form with a somewhat flattened stem, showing a distinct bilateral symmetry, the branches only springing from the opposite sides of the main axis. Three new species of Nephthya are enumerated. For a species in which in addition to a well-marked siphonoglyphe there are also in the first part of the œsophagus two flap-like protuberances, the genus Gersemiopsis is made. The only species, *G. arctica*, was dredged in a depth of 658 fathoms. A new genus, Barathrobius, is made for two new species, in which the basal part of the colony is hard and often dilated, the polyps are retractile, appearing only, when fully withdrawn, as slight elevations above the mass of the branches. *Sarakka crassa* (n. g. et sp.) is a species with a very peculiar structure in its œsophagus, which seems to be constricted laterally into two independent portions; while *Crystallofanes polaris* is a form with few polyps on the stem but with a summit rich in polyps, borne on short branches which are placed in whorls round the stem; the polyps are retractile.

A new sub-family is made for a new genus and species *Organidus nordenskjoldi*; in this species the polyp cells are long, connected together so as to form an axis; these polyp cells are long, cylindrical, calcareous, with both the polyp body and its tentacles well provided with spicules. The author thinks that this sub-family shows some affinity to the Tubiporinæ, but it would appear to us to show more relationship to such forms as Gersemia and Eunephthya. *Clavularia frigida* and *Sympodium abyssorum* are described as new species.

This memoir is published in both Swedish and English, in parallel columns, for which the student cannot be too thankful; true, the English may strike the reader as a little quaint, and in the nomenclature of the spicules it is somewhat novel, but criticism would be out of place in the presence of so great a boon. The day is coming when a new classification of the spicules of the Alcyonaria must be made; at present, while new types are constantly being discovered, any such would be but premature, and we must be content with that laid down for us by Kölliker. Had the value of the labours of Valenciennes been properly appreciated, this might not now be the case. The almost overcrowded plates have been drawn by H. Bucher, Jun., with all that skill which we have before admired, though perhaps the drawings of the spicules convey too much the notion of their being perfectly solid.

We shall wait with great expectancy the publication of future memoirs of the other families of the Alcyonaria.

## THE COLOURS OF THIN PLATES.[1]

THE physical theory, as founded by Young and perfected by his successors, shows how to ascertain the composition of the light reflected from a plate of given material and thickness when the incident light is white; but it does not and cannot tell us, except very roughly, what the *colour* of the light of such composition will be. For this purpose we must call to our aid the theory of compound colours, and such investigations as were made by Maxwell upon the chromatic relations of the spectrum colours themselves. Maxwell found that on Newton's chromatic diagram the curve representative of the spectrum takes approximately the simple form of two sides of a triangle, of which the angular points represent a definite red, a definite green, and a definite violet. The statement implies that yellow is a compound colour, a mixture of red and green.

In illustration of this fact, an experiment was shown in which a compound yellow was produced by absorbing-agents. An infusion of litmus absorbs the yellow and orange rays; a thin layer of bichromate of potash removes the blue. Under the joint operation of these colouring-matters the spectrum is reduced to its red and green elements, as may be proved by prismatic analysis; but, if the proportions are suitably chosen, the colour of the mixed light is yellow or orange. When the slit of the usual arrangement is replaced by a moderately large circular aperture, the prism throws upon the screen two circles of red and green light, which partially overlap. Where the lights are separated, the red and green appear; where they are combined, the resultant colour is yellow.

On the basis of Maxwell's data it is possible to calculate the colours of thin plates and to exhibit the results in the form of a curve upon Newton's diagram. The curve starts at a definite point, corresponding to an infinitely small thickness of the plate. This point is somewhat upon the blue side of white. As the thickness increases, the curve passes very close to white, a little upon the green side. It then approaches the side of the triangle, indicating a full orange; and so on. In this way the colours of the various orders of Newton's scale are exhibited and explained. The principal discrepancy between the curve and the descriptions of previous observers relates to the precedence of the reds of the first and second orders. The latter has usually been considered to be the superior, while the diagram supports the claim of the former. The explanation is to be found in the inferior brightness (as distinguished from purity) of the red of the first order, and its consequent greater liability to suffer by contamination with white light. Such white light, foreign to the true phenomenon, is always present when the thin plate is a plate of air inclosed between glass lenses. To make the comparison fairly, a soap film must be used, or recourse may be had to the almost identical series of colours presented by moderately thin plates of doubly-refracting crystals when traversed by polarized light. Under these circumstances the red of the first order is seen to be equal or superior to that of the second order.

[1] Abstract of Lecture delivered by Lord Rayleigh at the Royal Institution on March 25, 1887.

## FIFTY YEARS' PROGRESS IN CLOCKS AND WATCHES.—I.

HOROLOGY being one of the oldest aits and branches of science, it is almost inevitable that advances in it should be of a mediocre and modest character, and not of a nature to claim great attention in these days of startling and sensational discovery. But nevertheless during the period we refer to much good work has been done. In chronometers, the secondary compensation error has been discovered and means found to rectify it. In clocks, the same has been done for the barometric error. Moreover, the difficulties connected with the correct working of gravity escapements have been overcome; so that scarcely a good turret clock is made without one now. Electricity also has been largely applied for driving or controlling clocks, or for controlling chronometers; and the measurement of minute fractions of a second has been attained by chronographic appliances of extreme accuracy. Articles explanatory of these subjects have appeared in the pages of NATURE [1] from time to time. In addition there has been a mass of subsidiary improvements which it is impossible to classify, and of which we shall have to describe the leading features in a somewhat desultory manner in the succeeding pages.

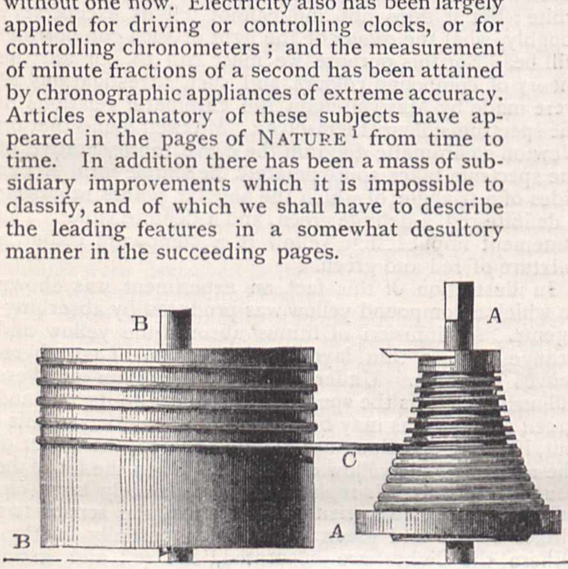



FIG. 1.—Barrel and Fusee.

Naturally, the first subject to claim our attention is that important mechanism which enables us to wind up and

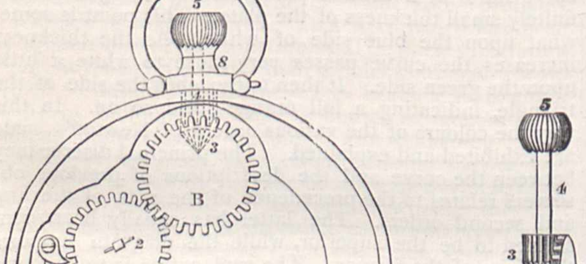


FIG. 2.—Prest's Keyless work.

set the hands of our watches without a key. And it is to be remarked that its introduction has led to the almost general abandonment in watches of the fusee (A A, Fig. 1), a contrivance of considerable antiquity, a picture of which used to appear in nearly every popular book on mechanics

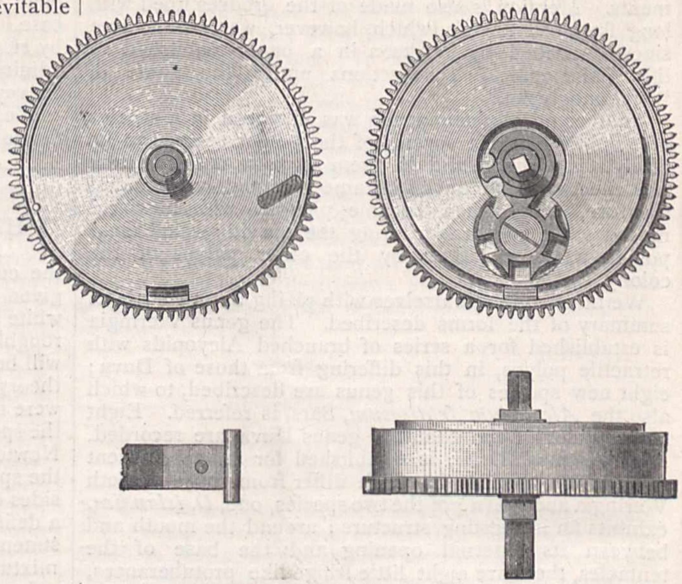


FIG. 3.—Going Barrel and Stop-work.

a few years ago. The discovery of such mechanism was not made all at once; at first it was applied solely for the

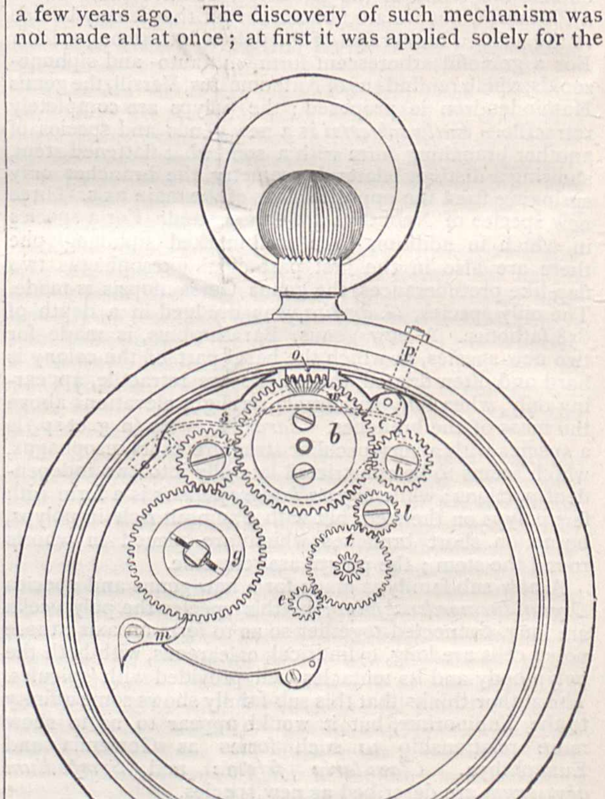


FIG. 4.—Rocking-bar Keyless work.

purpose of winding up the watch. The conception of the present form of winding from the pendant is due to Prest

[1] See vols. xiv. pp. 529, 554, 573; xv. 9; xx. 345; xxiii. 59; xxvi. 107, 369.

of Chigwell, and Fig. 2[1] shows his plan. 2 may be considered the same as the square to which the key would be applied in winding, and the wheel A is fastened to it and is geared to B, which in turn engages the pinion 3, which is part of the stalk 4, passing through the pendant 8, and terminating in the crown-piece 5. On turning 5, A will revolve, winding up the watch in doing so; the clicks 6 6 prevent A from returning. It is now clear why the fusee must be dispensed with. With a fusee (whilst the watch is going) the square which you wind travels backwards, and it would naturally turn the crown-piece in doing so; this latter, meeting with resistance in the pocket, would obviously stop the watch. Fig. 3 shows the mechanism which takes the place of the fusee. It will be seen that the main wheel is attached to the barrel; the shaft (squared at its extremities) which passes through the barrel is connected with the main-spring; when the shaft is turned the main-spring is wound. The shaft, being held by the intervention of the clicks, cannot return, and the outside of the barrel being urged to follow it by the pulling of the main-spring, impels the main wheel and drives the train. Overwinding is prevented by means of the star wheel and finger-piece shown in the diagram. Every turn of the shaft causes the star to move on one division, but on passing the last division the circle, out of which the finger is cut, meets a convex instead of a concave surface, and further movement is arrested. There is much less difference between the pull of the main-spring when the watch is wound up and nearly down than might be expected. To obtain as much uniformity as possible a long thin main-spring is used, it is tapered, and very few turns of it are brought into service.

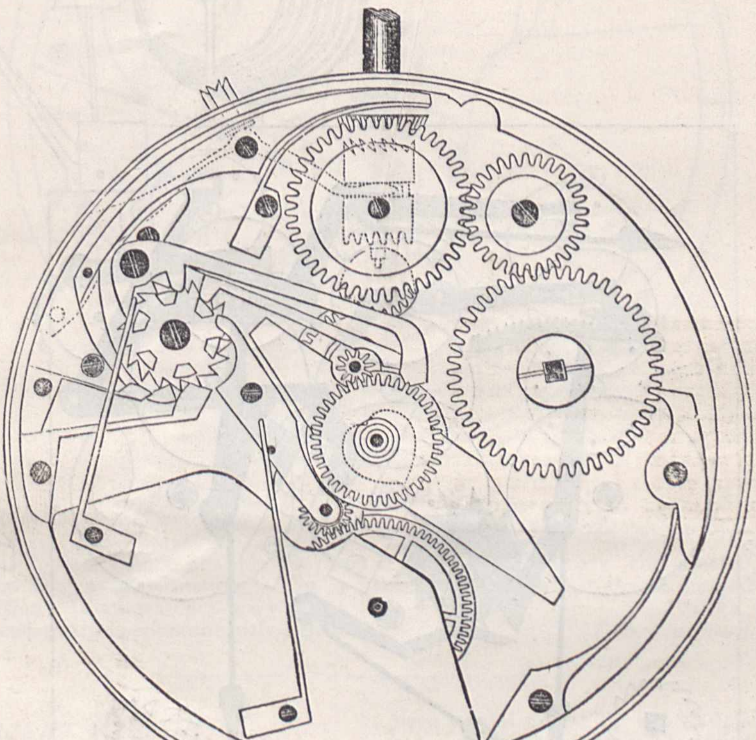


FIG. 5.—Chronograph with Swiss Keyless work.

About twenty-five years elapsed before any satisfactory method was established of causing the keyless mechanism to set the hands of the watch in addition to winding it. The method which was first adopted had the drawback that the hands could not be put backward when the watch was fully wound. At present two systems are principally employed, they are known as the English and Swiss. Fig. 4 shows the English or rocking-bar plan. Wheel *b* is in connexion with the crown-piece, and communicates with the square of the shaft passing through the barrel by means of the wheels *i* and *g*. Wheels *i* and *h* are on a lever, or rocking-bar, pivoted about the centre of *b*. *p* is a push piece acting against *c*, which is a part of the rocking-bar. When *p* is depressed by the finger or thumb, it lifts *i* and forces down *h* into connexion with *l*, which communicates with the pinion of the minute-hand *e*. If the crown-piece now be turned, the hands will follow; no winding is performed, because *i* has been lifted away from *g*. When the pressure is removed from *p*, a spring, *s*, puts the rocking-bar back again into its normal position, *i* engaging *g*, and *h* quitting *l*. The Swiss system is different in this: that connexion with the winding or set-hands wheels is made by a pinion faced with teeth on both sides, sliding up and down the stalk of the crown-piece. The normal position (as in the English system) is engagement with the winding-wheels, but when the push piece is depressed the pinion moves away from its engagement with the winding-wheels, and

[1] We are indebted to Mr. David Glasgow, Vice-President of the British Horological Institute, and the Messrs. Cassell for the use of Figs. 2, 3, and 4, and to Mr. F. J. Britten, Secretary of the British Horological Institute, for the use of Figs. 5, 6, and 7.

takes up with the set-hands wheels. In Fig. 5, which we shall refer to again further on, this arrangement can be readily perceived.

To understand repeating work—in which a good deal of progress has been made—it will be as well at first to refer to Fig. 6, which shows the mechanism of a clock chiming the quarters. On the left will be seen an anchor-shaped piece with teeth in it, called a "rack." At the foot of the rack will be seen a star wheel carrying a piece in form similar to a snail. This piece is called the "snail," and it has twelve gradations corresponding to the twelve hours. On the right will be seen another rack and snail which do duty for the four quarters. Both the quarter and hour racks are at present held free of their respective snails by the hooks shown in the diagram. The method of action is as follows:—At each hour the quarter rack, by means of mechanism connected with the going train of the clock, gets itself liberated from the hook and falls upon its snail. The distance through which it falls is determined by the depth of the depression in the

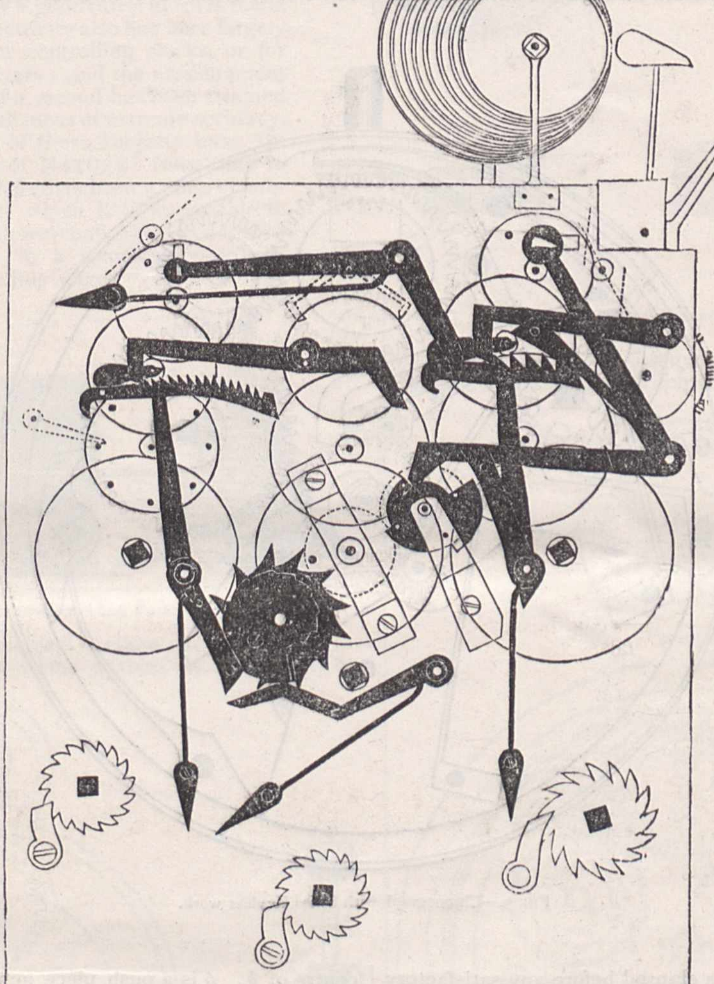


FIG. 6.—Hour and Quarter striking mechanism.

snail which is opposite to it. The quarter train having also got freed at the same time, proceeds to run, and winds up the rack again in doing so. The distance through which the rack has fallen determines the length of time the quarter train runs, and consequently the length of the chime. In falling the quarter rack also discharges the hour rack. The hour train is held until the quarters are finished; at their conclusion the hour rack is wound up by the hour train through the distance it has fallen (which depends upon the depth of depression in its snail opposite to it), and the number of the hour struck is in proportion.

The light which the foregoing throws upon repeating work is with regard to the snail and rack arrangement. When you move the slide of a repeating watch you do two things. You wind up the main-spring, which actuates the repeating train, and the extent to which you are able to do so depends upon the depth of the depression in the snail which is opposite to the piece which you are moving. When you reach the bottom and press against the snail, it is so arranged that the snail shall give a little. The small play the snail has, the distance it can move under pressure, is sufficient to discharge the quarter rack on to its snail. In repeating work the quarter rack is also an "all or nothing piece," for this reason, that until it is discharged the hammer which strikes the hours is hung up, and should you not press down the slide sufficiently to reach the bottom of the depression in the

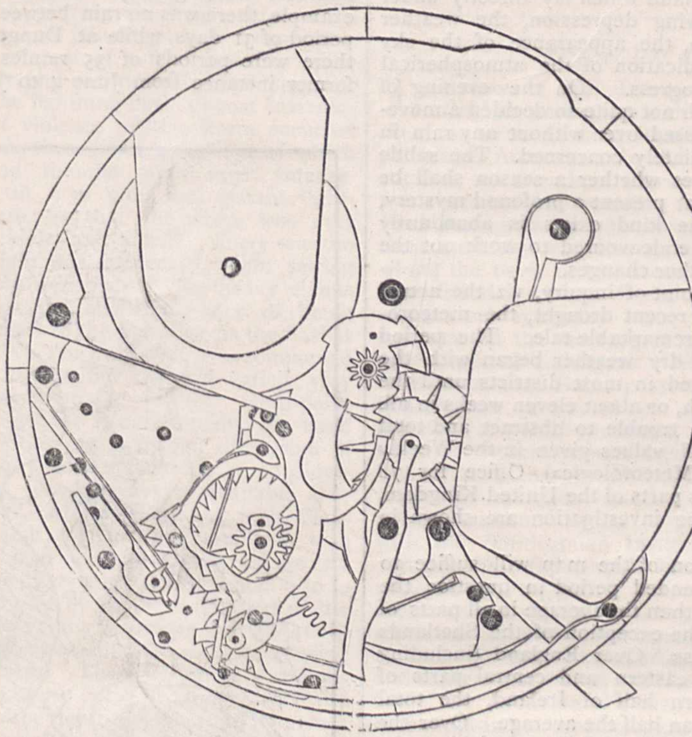


FIG. 7.—Repeating mechanism.

snail no blows are struck, so it is not possible for the repeating work to give you a false answer. Fig. 7 illustrates repeating action. Clock-watches are watches which strike the hours and quarters spontaneously; their action is exceedingly complicated, and, unless their mechanism is seen, is almost incapable of explanation.

HENRY DENT GARDNER.

(*To be continued.*)

## *THE RECENT DROUGHT.*

THE spell of dry weather recently experienced over the United Kingdom has been so unusually prolonged, and its effects have in many instances been so disastrous, that a brief inquiry into its history and general results may not be without interest. In the present article it is therefore proposed to take into consideration,—firstly, the conditions of barometrical pressure under which the drought occurred; and secondly, the actual deficiency of rainfall experienced in various parts of the country.

With respect to the first point it will readily be surmised by those who are in any way acquainted with the subject of our meteorological changes that the general distribution of pressure during the recent dry spell was anticyclonic. At times, and notably during the second half of June, the middle of July, and the early part of August, the anticyclonic conditions ruled supreme over the entire Kingdom. On other occasions, however, the influence of the high-pressure areas was confined to a portion of our islands, the favoured localities being usually those included within the eastern or the southern half of Great Britain. With these latter conditions the extreme western and northern districts were influenced to a very partial extent by the anticyclone, and to a much greater extent by areas of low pressure, the centres of which were, however, in nearly all cases at a considerable distance from our shores. On a few rare occasions the main disturbances were accompanied by shallow subsidiary depressions, which advanced directly over us, and occasioned the temporary bursts of showery weather which occurred from time to time. The most important and general instances of this kind were observed during the second week of July and towards the end of the same month; but in the former case there were isolated portions of our southern and south-eastern counties which remained altogether unaffected by the disturbed weather, while in the latter instance the showers were in many districts far too insignificant to be of any real value.

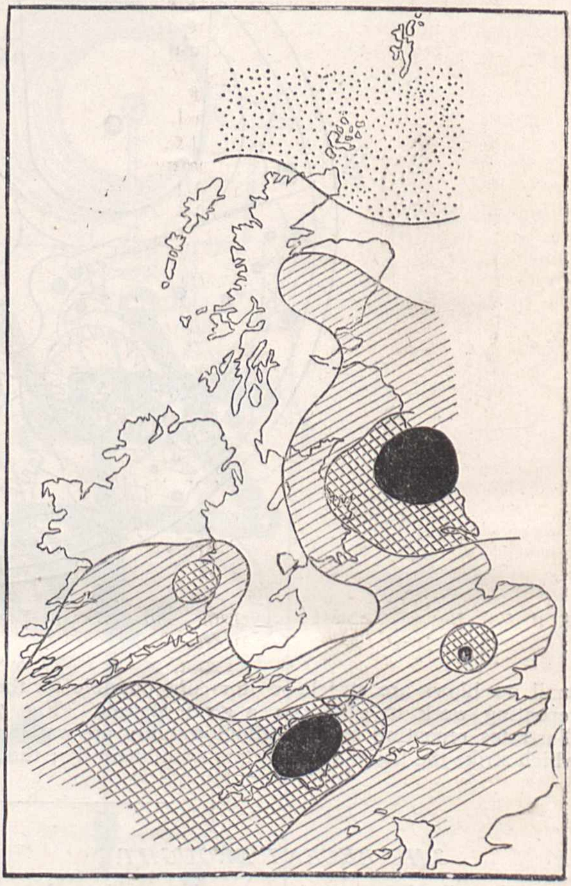
Although an endeavour has thus been made briefly to account for the unusual drought which occurred, one cannot but feel that beyond and irrespective of the various pressure movements which were reported from time to time there was a distinct *tendency* for the weather to remain dry and warm. Instances were not wanting of the prevalence of very disturbed conditions of pressure without any corresponding break up in the atmospherical appearance. Of this, two recent examples may be cited. On the afternoon and evening of August 12, a depression formed directly over England, while another appeared over Ireland. Under such circumstances a good deal of rain might naturally have been anticipated in all districts, and in ordinary seasons there can be no doubt that it would actually have fallen. As a matter of fact, however, in those parts of England which lay directly under the influence of the growing depression, the weather remained persistently fine, the appearance of the sky even giving but little indication of the atmospherical change which was in progress. On the evening of August 11 a similar though not quite so decided a movement in pressure also passed over without any rain in the districts more immediately concerned. The subtle influence which determines whether a season shall be dry or wet, hot or cold, is at present a profound mystery, but that something of the kind exists is abundantly evident to all who have endeavoured to work out the causes of our seasonal weather changes.

As regards our second point of inquiry, viz. the actual nature and extent of the recent drought, the meteorological records tell a most remarkable tale. The period embraced by the spell of dry weather began with the early part of June, and lasted in most districts until the middle of the present month, or about eleven weeks in all. I have therefore taken the trouble to abstract and total for this period the rainfall values given in the Weekly Weather Report of the Meteorological Office for 78 stations situated in various parts of the United Kingdom. The general results of the investigation are shown in the accompanying map.

A very brief examination of the map will suffice to show that during the extended period in question the aggregate rainfall was less than the average in all parts of the British Islands, with the exception of the Shetlands and a portion of Caithness. Over England (including also South Wales), the eastern and central parts of Scotland, and the southern half of Ireland, the total amount of rain was less than half the average. Over the north of England, the county of Hertfordshire, the greater part of the south-western district, comprising the counties of Somerset, Devon, and Cornwall, and a small tract of country surrounding Dublin, the aggregate amount was less than a third of the average; while in the north-east of England, in portions of Devon and Cornwall, and at Rothamsted, the rainfall did not amount to one-fourth of the average. The only exceptions to these general rules occurred over some portions of Eastern England, where, owing to heavy local thunderstorms, the aggregate was much greater than at surrounding, or even at neighbouring, stations. At Attleborough, in Norfolk—a station which is not included in the official report, and which has therefore not been employed in the preparation of the map—as much as 2·03 inches of rain were measured on July 31 in the short space of an hour and a half. A very similar local plump occurred at Ingatestone in Essex, where, during a severe thunderstorm on July 16, a fall amounting to 1·8 inch was recorded in about two hours. So far as I am aware, no local falls of anything like so heavy a nature were experienced either over our midland or our southern couties.

With regard to the frequency, or rather in the present instance to the rarity, of the summer rainfall, it appears that over the greater part of the midland, southern, and south-western districts the number of rainy days was less than 15 out of a total of 77. At Cirencester, Hastings, and Southampton, the number did not amount to more than 12, at Oxford to 11, and at Hurst Castle to 10; while at Dungeness there were only 8 days with rain, or about one-fourth of the average number. In the south-west of England the number of rainy days varied from 13 to 25, in the north-east from 15 to 23, and in the north-western district, including North Wales, from 19 to 26.

One other very important feature in connexion with the drought has been the prevalence of unusually long periods of absolutely rainless weather. Between the early part of June and the beginning of July there were in many of the English districts as many as 25 to 28 consecutive days without rain, and in some localities these numbers were greatly exceeded. At Falmouth, for example, there was no rain between June 7 and July 7, a period of 31 days, while at Dungeness and Cullompton there were periods of 35 rainless days, lasting in the former instance from June 4 to July 8, and in the latter



Districts in which rainfall was in excess of the average... ...

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from June 3 to July 7. As regards the London district it appears that the drought in its absolute sense was longer than any experienced since the year 1865. Between the first week in June and the beginning of July there were in London 25 consecutive days without rain; in June 1865 the number was 26. In the period of 21 years intervening between 1865 and the present year there were only three instances of an absolute drought lasting for as long as three weeks.

FREDK. J. BRODIE.

## THUNDERSTORM IN LONDON.

AN exceptionally severe thunderstorm was experienced in London and the suburbs on the evening of the 17th inst. It commenced with distant thunder at about 5.30 p.m., and by 6 o'clock the storm was fully over the southern suburbs. The lightning was very vivid, and the flashes were very frequent, following each other occasionally with but an interval of a few seconds. The thunder was very heavy, and at times quite deafening, the crash often following the lightning-flash almost instantaneously. The greatest violence of the storm occurred between 6.30 and 8 p.m., throughout the whole of which time the lightning and thunder were most intense. Thunder was heard till 9.30 p.m., and distant lightning seen till 10 p.m., so that the storm was over London for about four hours and a half. There was no evening as far as daylight was concerned, night setting in at the close of the afternoon, and the heavy clouds which covered the sky had the appearance of being doubly massive in contrast to the lightning as the flashes illumined the whole sky. The rain which accompanied the storm was very heavy, but the fall varied very considerably in different parts of the metropolis. Unfortunately at present the measurements at hand are by no means numerous, but a careful discussion of the rainfall of this storm would probably be of considerable scientific interest. The falls as yet available are: Brixton Hill 2·02 inches, Camden Town 1·42 inch, Clapham 0·97 inch, Greenwich 0·54 inch, Westminster 0·50 inch, and East Finchley 0·16 inch. At Brixton Hill the rain was intensely heavy for twenty minutes from about 6.10 to 6.30 p.m., during which time by far the larger part of the fall occurred; the observer not being on the spot until later in the evening, measurements were not made during the progress of the storm. There is ample evidence, however, to confirm the heavy fall at Brixton, as the roads were flooded in parts to the depth of from 12 to 18 inches, and the water rushed down the roadways with such force that it was thought a large reservoir had burst. Mr. Wallis, writing from the head-quarters of the "British Rainfall" at Camden Town, states that the total fall there was 1·42 inch, and heavy rain did not commence till 6.30 p.m. He gives the following rates of fall:—7 to 8 p.m. 1·24 inch, 7 to 7.30 p.m. 0·45 inch, 7.30 to 8 p.m. 0·79 inch; in 22 minutes, from 7.42 to 8.4, the amount measured was 0·66 inch; and in 10 minutes, from 7.45 to 7.55, the heavy fall of 0·50 was measured. The primary cause of the storm was due to a somewhat shallow barometric depression, the mercury at the centre standing at 29·7 inches, which passed completely over London during the evening.

This disturbance was central over the north of Devon at 8 a.m. 17th, and by 8 a.m. 18th was situated over North Germany, but from some cause, not yet understood, its rate of travel when passing over London was very much slower, and its energy more intense, than at any other stage of its existence. The weather had been dry during the first twelve days of August, as well as at the close of July, especially in the southern and eastern districts of England, where, indeed, a second drought, during the present summer, had prevailed, but which was much less marked than the drought of June and the early part of July, but yet severe, following as it did so closely on its predecessor, with so small a fall of rain intervening. After the 12th, however, the weather over England became disturbed, and the anticyclone which had prevailed gave place to cyclonic conditions, and a series of disturbances passed over our islands; it was one of these which resulted in this severe thunderstorm. Very little rain fell over the country generally in connexion with this storm, but other falls of rain occurred in many places about this time. In London, as well as in the Midlands, and the southern and eastern districts of England, a thunderstorm had been experienced in the early morning of the same day; the total fall of rain in London, as the result of the two storms, was 2·62 inches, a fall 0·34 in excess of the total average for August, all of which fell in less than twenty-four hours. CHAS. HARDING.

## SPENCER F. BAIRD.

THE news of Prof. Baird's death will be received by English naturalists with the most profound regret, the more so as no intimation of the indisposition of the celebrated American man of science had been communicated to his friends in this country, and the intelligence was therefore unexpected. By Englishmen who knew Prof. Baird personally the loss must be especially felt, but there are many who had never met him in the flesh, to whom the news of his decease must come as that of a dear friend. As one of the latter class, we venture to express our sympathy with our scientific brethren in America on the decease of one of their most eminent and respected colleagues. As chief of the Smithsonian Institution, Prof. Baird possessed a power of conferring benefits on the world of science exercised by few directors of public museums, and the manner in which he utilized these powers has resulted not only in the wonderful success of the United States National Museum under his direction, but in the enrichment of many other museums which were in friendly intercourse with the Smithsonian Institution. We know by experience that the British Museum is indebted beyond measure to Prof. Baird, and we need only refer to the recent volumes of the "Catalogue of Birds" to show how much our national Museum owes to the sister Museum in America for hearty co-operation. We had only to write and express our wants, and immediately every effort was made, by Prof. Baird's instructions, to supply all the desiderata in our ornithological collection, and this without the slightest demand for an equivalent exchange, though of course in the case of the British Museum every effort was made to reciprocate the good feeling shown towards that institution by the great American Museum. There must be many private collectors in this country who will indorse our acknowledgments to Prof. Baird for the unrivalled liberality which he has always shown in the advancement of the studies of every ornithologist who invoked his aid.

Of the celebrated trio, Baird, Cassin, and Lawrence, who together wrote "The Birds of North America," the last-named naturalist is now the only survivor, but Baird lived long enough to see the results of that great undertaking, which placed American ornithology on a sound working basis, and established an era from which progress has been both sound and rapid, until there is perhaps no country in the world where birds have been so thoroughly and scientifically studied as in America. This result is undoubtedly due to the influence of Prof. Baird in directing the scientific studies of his colleagues in the New World. His "Review of North American Birds" is really a wonderful work, and, though published twenty-five years ago, is of the greatest service to students of Passerine birds at the present day. Our only regret is that it was never completed. The celebrated paper on the distribution of North American birds, published in 1867, laid the foundation of the division of the Nearctic Region into natural sub-regions, which the multitudinous labours of travellers in recent years have tended to elaborate and confirm. Prof. Baird's last great effort in the cause of ornithology was the publication of the "History of North American Birds," in conjunction, this time, with Robert Ridgway and T. M. Brewer.

After the completion of that important work he was occupied chiefly with his duties as head of the Smithsonian Institution, and of the United States National Museum, and with the United States Fish Commission, of which he was also President. In 1884 the *Auk* announced that the bird-registers of the United States National Museum had reached 100,000 specimens in number, this splendid collection having been based on the nucleus of 3696 skins, the private collection of Prof. Baird; and the same journal states:—"As being, more than any other living person, entitled to the privilege, specimens numbered 100,000 and 100,001 are entered as donations from Prof. Baird, to whom they were presented by Mr. Geo. N. Lawrence, the oldest active American ornithologist. One of these, a common Crossbill, was shot by Mr. Lawrence, in New York City in 1850, and the other, a Flicker, on Long Island, in 1862."

We may add that, during an experience of twenty years, we have never heard from any ornithologist, European or American, a single unkind word concerning Prof. Baird, either in his public or private capacity. This is something to say in this age of jealousies and backbitings. R. BOWDLER SHARPE.

## NOTES.

LAST year the New South Wales Government, through their Agent-General, invited the British Association to meet at Sydney in January. The invitation has now been withdrawn. Strangely enough, the matter was treated as a party question in the New South Wales Parliament.

THE American Association for the Advancement of Science met in New York from August 10 to 17. Prof. S. P. Langley, the President, in his opening remarks, congratulated the members on the fact that the meeting promised to be most successful. Prof. E. W. Morse, of Salem, Mass., the retiring President, chose as the subject of his address, "What American Zoologists have done for Evolution." "Eleven years ago," said Prof. Morse, "I had the honour of reading before this Association an address in which an attempt was made to show what American zoologists had done for evolution. My reasons for selecting this subject were, first, that no general review of this nature had been made; and, second, that many of the oft-repeated examples in support of the derivative theory were from European sources, and did not carry the weight of equally important facts the records of which were concealed in our own scientific journals. Darwin was pleased to write to me that most of the facts I had mentioned were familiar to him, but, to use his own words, he was amazed at their number and importance when brought together in this manner. The encouragement of his recognition has led me to select a continuation of this theme as a subject for the customary presidential address—a task which is at best a thankless if not a profitless one. Had I faintly realized, however, the increasing number and importance of the contributions made by our students on this subject, I should certainly have chosen a different theme." Prof. Morse laid much stress upon the fact that "American biological science stands as a unit for evolution."

IN Europe the weather rendered almost useless the elaborate preparations which had been made for observations of the total solar eclipse of August 19. From the German stations the Berlin Observatory received a series of dismal telegrams, such as, "Fog and rain; no observations," "Nothing done; quite cloudy," "Cloudy; observed nothing." Partially successful observations were made in Germany only at Nordhausen and Eisleben. In European Russia observers were almost equally unfortunate. At Klin all attempts to get a glimpse of the eclipse were "completely frustrated by the dull gray sky and thick Scotch mist which quickly damped both one's clothes and one's spirits." At the last moment Prof. Mendeleieff, who was stationed at Klin to observe the form of the corona, its spectrum, and the course of the shadow, went up alone in a balloon, but he was too late to obtain important results. A balloon which went up at Tver was met in its ascent by torrents of rain. A glimpse of the sun was obtained at Tver only twice—at the contact, and when it was about seven-eighths obscured. At Spirovs, nearer St. Petersburg, totality is said to have been visible for twenty seconds. At Petroffsk, in the Government of Jaroslav, Prof. Glasenapp, of St. Petersburg, was lucky enough to be able to make six drawings and to get two photographs, while Prof. Stanoievitsch, of Belgrade, was successful in observing and photographing the spectrum of the corona. Fortunately there was a clear sky at Tomsk and other stations in Siberia.

IT is worth noting that an extraordinary amount of interest was excited on the Continent by the eclipse. It is calculated that in Berlin and the neighbourhood no fewer than 200,000 persons were waiting in the hope of seeing it, and in Russia great numbers of people flocked to many points of observation. This may, we hope, be taken as an indication that both in Russia and Germany there is a growing popular appreciation of some of the more striking truths of physical science.

THE Berlin Correspondent of the *Times* has brought together some interesting reports as to the effect of the eclipse upon the lower animals. Foresters state that the birds, which had already begun to sing before the eclipse took place, became of a sudden quite silent, and showed signs of disquiet when darkness set in. Herds of deer ran about in alarm, as did the small four-footed game. In Berlin a scientific man arranged for observations to be made by bird-dealers of the conduct of their feathered stock, and the results are found to deviate considerably. In some cases the birds showed sudden sleepiness, even though they had sung before the eclipse took place. In other cases great uneasiness and fright were observed. It is noticeable that parrots showed far more susceptibility than canaries, becoming totally silent during the eclipse, and only returning very slowly to their usual state.

IT is greatly to be regretted that the Government has found it necessary to abandon the Technical Education Bill. In announcing to the House of Commons the surrender of the measure, Mr. W. H. Smith said:—"We hoped that the Bill would have been received almost unanimously by the House, but it has met with opposition, and we are threatened with prolonged discussion of the measure, and on August 18 I cannot encounter the difficulties which are likely to be thrown in the way if we persist in the carrying through of that Bill in the course of the present session. It is, however, a measure which we should feel it our duty to introduce in the very earliest days of the next session, and I hope that the consideration which will be given to the subject in the interval will enable us to meet any objections raised by hon. friends on this side of the House, and by hon. gentlemen on the other side, so as to produce a measure which will rapidly obtain the concurrence of the House without exciting any party feeling of any kind whatever, for I should greatly deprecate any party or sectional feeling in a question of this kind."

IN the discussion on Tuesday evening of the vote to complete the sum of £147,385 for the British Museum, Sir J. Lubbock expressed much regret that the amount allotted to purchases for the Museum was £10,000 less than usual. It would be hard to conceive a more striking instance of misplaced economy, for, as Sir J. Lubbock pointed out, there is at the present moment an exceptional number of interesting specimens for sale. Mr. Molloy proposed that the Museum should be opened at night, and maintained that the sum required for the electric light would not exceed £1000 per annum. Mr. W. H. Smith, on behalf of the Government, promised that this question should be most carefully examined during the recess.

On Tuesday evening, in connexion with the vote to complete the sum of £23,900 for learned Societies, &c., several Scottish members complained that science in Scotland receives anything but generous treatment. Sir John Lubbock was able to show that in some of their statements they did not take all the facts into account; but the demands made on behalf of the Ben Nevis Observatory were certainly not unreasonable. If a grant cannot be made to this institution, the Government might at least give up its claim to the sum of £130 paid annually to the Post Office for the use of the telegraph.

Messrs. Macmillan will publish shortly a work on the nervous system and the mind, by Dr. Charles Mercier. It is intended to serve as an introduction to the scientific study of insanity. It will contain an exposition of the new neurology as founded by Herbert Spencer and developed by Hughlings Jackson; an account of the constitution of mind from the evolutionary standpoint, showing the ways in which it is liable to be disordered; and a statement of the connexion between nervous function and mental processes as thus regarded.

Dr. Alfred R. Wallace arrived at Liverpool on Saturday last by the steamship *Vancouver* from Quebec, after his ten months' lecture tour in the United States and Canada. He saw a good deal of the country, and spent two months in California and the Rocky Mountains. During his stay at Boston and Washington he made the acquaintance of most of the American men of science.

A letter on Antarctic exploration has been addressed by Capt. C. Pasco, R.N., to Admiral Sir E. Ommanney, Hon. Secretary to the Antarctic Committee of the Geographical Section of the British Association. Capt. Pasco writes on behalf of the Antarctic Committee appointed by the Royal Society of Victoria and the Victorian branch of the Royal Geographical Society of Australasia. Much of the information contained in the letter has already appeared in Nature (June 30, p. 211, and July 21, p. 277). Having dealt with the question of ways and means, Capt. Pasco says the Victorian Societies feel warranted in recommending the renewal of Antarctic research for the following reasons: (1) that the configuration of the Antarctic continent may be traced further, with the view to extend our acquaintance with the geography of the globe; (2) that a further insight into the geology of these lands may be obtained; (3) that it is desirable to increase the extent of the determined physiography of the world by ascertaining whether the recent volcanic disturbances in New Zealand and in the Sunda Islands—both situated on the line of weak earth crust which is believed to carry the volcanoes of Victoria Land—have produced changes of any kind in the Antarctic Circle; (4) the examination of Mount Erebus would appear to be practicable, as Ross reports that the coast became lower as it trended towards its foot, and the results of a visit to the locality should be of the highest interest; (5) that the question whether any secular climatic change is in progress may be investigated, and that the sea temperatures may be ascertained by means of the most modern appliances; (6) that the magnetic survey of these parts may be resumed, and that new data may be obtained for comparison with the elements recorded by Ross; (7) that the existence of whales or seals, or the occurrence of any commercial products, may be accurately observed. In concluding his letter, Capt. Pasco expresses a hope that the efforts of the British Association in favour of the renewal of Antarctic exploration may speedily receive the reward which they deserve. "We sincerely trust," he says, "the exploratory work in the Antarctic, commenced so well by the illustrious Cook, and continued by other brave seamen, but stayed ever since the return of that successful explorer and navigator Ross, may be resumed at once with energy, intelligence, and ample appliances."

The *Jahrbuch* of the Norwegian Meteorological Institute for 1885 (the last published) shows the part which is taken by Norway in the general system of meteorological organizations. The work contains complete observations for twelve stations, and summaries for sixty-eight others, among which are seven lighthouse stations; for the latter the observations of sea-temperature are also published. Systematic observations were begun in Christiania as early as 1837 by the professors of the University, and were regularly published until the end of 1867. In the meantime (1860) the Director of the Christiania Observatory had commenced the publication of the observations taken at five telegraphic stations on the coast, in addition to those of the Observatory, and this series was continued until the end of 1886, at which time the Meteorological Institute was established under the present Director, Prof. H. Mohn, so that the present year-book forms really the nineteenth of the series. With the year 1874 the work took its present shape, in order to bring it more into conformity with the decisions of the Meteorological Congress at Vienna (1873); yet we observe that the wind-force is estimated according to the old land-scale 0–6, which is now seldom used, while in this country the scale is 0–12, and in other countries more usually 0–10. This diversity of scales leads to great confusion when dealing with wind-observations generally. The Institute publishes somewhat less than several other countries, but its staff is doing good work in connexion with the Reports of the Norwegian North Atlantic Expedition of 1876–78, and the Polar Expedition of 1882–83.

The Imperial Academy of Sciences of St. Petersburg has issued a new edition of its "In tructions for use at Meteorological Stations" (St. Petersburg, 1887, 106 pp. 8vo, and 34 woodcuts) by Dr. H. Wild, Director of the Central Physical Observatory. The first edition appeared in 1869, since which time the number of meteorological stations of the second order in Russia has increased from 33 to 255. The work is written in the German language and is divided into three parts: (1) directions for the erection of the instruments and for taking the observations, (2) a description of the instruments used at the stations of the Russian system, and (3) a description of the instruments required for observations not always taken at ordinary stations. But the work is not accompanied by the tables generally annexed to such instructions. Siphon barometers are mostly employed in Russia, but latterly portable cistern barometers have been adopted for use at distant stations. Several of the instruments, all of which are clearly illustrated, are not in use in this country. Among them may be specially mentioned:—(1) A portable equatorial sun-dial on Fléchet's principle for places where no good time-piece is available, showing the mean time within five minutes. (2) A swinging-plate wind-gauge, which was generally in use in Switzerland while Dr. Wild was at Berne, and now is adopted in Russia. This instrument is described in the Report of the Meteorological Congress held at Vienna in 1873; it consists of a rectangular metal plate suspended like a sign-board, and shows the force of the wind by its displacement from the vertical position. (3) A rain-gauge on Prof. Nipher's principle, with arrangements for protection against the influence of high winds on the rainfall. (4) A nephoscope invented by Dr. Fineman, of Upsala, a model of which was exhibited at the meeting of the International Meteorological Committee at Paris in 1885. In the *Meteorologische Zeitschrift* for August, Dr. Köppen points out a discrepancy in the description of the somewhat rare phenomenon of glazed frost; but, as a whole, the treatise will rank as one of the best extant.

The Report of the Meteorological Reporter for the Punjab for the financial year 1886–87 states that the most important feature in the meteorology during the year has been the failure of the cold weather rains (December to March). In January there was a certain amount of excess in the Delhi division, but in all other parts of the province there was a deficiency, and the following months were practically rainless. The monsoon rains (June to September) were generally good, notwithstanding their early cessation. The Report of the Sanitary Administration for the calendar year 1886, which has arrived simultaneously with that above mentioned, shows that the greatest annual rainfall was 53·3 inches at Abbottabad (Peshawar), and the least 4·3 inches at Muzaffargarh (Deraját). We omit the exceptional amount of 127·5 inches at Dharmsála (Jullundur). The inclusion of this excessive amount vitiates the general mean for the stations for the year, viz. 29·1 inches, which is more than three inches higher than it would be if this exceptional amount were omitted from it. The Meteorological Reporter gives 183° as the highest temperature in the sun's rays, being at Lahore on April 28, and it ranged from 172° to 175° in the five succeeding months. The maximum reading in the shade was 118° at two stations on the 13th, and the lowest maximum was 79° at Sirsa (Delhi), in January. The absolute minimum in the shade was 29° at Ráwalpindi, in February, giving a range of 89° in the shade temperature for the whole province. During the early part of this month of February a remarkable wave of low temperature passed over the Punjab; on the 9th and 10th remarkably low temperatures were recorded.

THE results of a long series of experiments upon the nature of the chemical action between acids and the metal zinc have just been published by MM. Spring and van Aubel in the August number of the *Annales de Chimie et Physique*. Although one of the first chemical reactions which come under the notice of students, it has hitherto been among the least understood. The method of experiment was to plunge a quantity of zinc, which contained a small quantity of lead, and whose surface was known, into such a volume of the acid of known strength as would suffice for the elimination of a volume of hydrogen = Q. The hydrogen was collected in a vessel divided into aliquot parts, $q$, of Q; with the aid of a chronograph the times $t_1$, $t_2$, &c., necessary for the production of successive volumes $= q$ were noted, and from the data obtained the rapidity of the reaction $\frac{q}{t_1}$, $\frac{q}{t_2}$, &c., at corresponding epochs was estimated, thus following the reaction step by step from beginning to end. The results were eventually represented graphically, the abscissæ representing successive quantities $q$, and the ordinates the rapidity. The action of the acid upon the zinc is not most rapid at the origin, but increases to a maximum when the acid is about half its original strength, afterwards diminishing proportionally to the concentration to the end of the reaction, so that the curve after passing the maximum becomes a straight line. The early stage previous to attaining the maximum is called the *mise en train* of the reaction, or period of induction, and it is conclusively shown that during this period the acid, by a slow action, prepares at the surface of the metal an infinity of little electric couples by exposing the minute grains of lead contained in the zinc. Hence, De la Rive's idea that the solution of a metal by an acid is mainly due to electrolytic action is substantially correct. It will be remembered that De la Rive showed that pure zinc is only attacked by acids with extreme slowness, but that if traces of copper, iron, or lead be present the evolution of hydrogen is much more rapid. One most interesting result emerges from the experiments of Spring and van Aubel: the curves from all the experiments with hydrochloric acid intersect about −70° C., showing that at this temperature no action at all occurs between this acid, however concentrated, and zinc, and it is a known fact that zinc does not dissolve in liquefied hydrochloric acid gas, whose temperature of liquefaction is near −70°. Sulphuric acid acts upon zinc twenty-seven times more feebly than hydrochloric, and the electrolytic action appears to be the vastly preponderating one, the sulphate of zinc resulting from the action of $H_2SO_4$ upon the ZnO first formed by electrolysis; hence the true nature of this every-day reaction is probably as follows: $Zn + H_2SO_4 = ZnO + H_2 + SO_3$; $SO_3 + H_2O + Aq = H_2SO_4 + Aq$; $H_2SO_4 + ZnO = ZnSO_4 + H_2O$.

RUSSIAN geological literature has been enriched during the current year by a most valuable publication, which will be especially welcome to West European geologists. We mean the "Bibliothèque géologique de la Russie," published in Russian and in French by the Geological Committee, under the editorship of M. Nikitin. The first volume contains an index, as nearly complete as possible, of books, pamphlets, and articles published in Russia on geology, mineralogy, and palæontology during the year 1885. During the last few years geology has made rapid progress in Russia, and although all the chief works produced recently have been published either in the *Izvestia* or *Trudy* of the Geological Committee, or in the *Zapiski* of the Mineralogical Society, a great number of important papers are scattered in the Memoirs of the Academy and other scientific bodies, and thus often escape the attention even of Russian geologists. To West European geologists most of these papers have hitherto remained quite unknown. The "Bibliothèque géologique" mentions all of them, and gives short analyses, which sum up in Russian and French all the chief facts mentioned, and the conclusions arrived at, in the works and papers enumerated. The analyses are generally admirable, and most of them are due to the pen of Miss Mary Tswetaev.

M. MAINOFF's work on the "Juridical Customs of the Mordovians," published in the fourteenth volume of the Memoirs of the Russian Geographical Society for Ethnography, is a capital inquiry into the customs of this important branch of the Volga Finns, as they have shaped themselves under the double influence of the Russians and the Tatars. It is an important addition to the work by the same author which contained his anthropological measurements. M. Mainoff has devoted attention chiefly to marriage customs, but his work, which is the result of many years' acquaintance with the Mordovians, contains plenty of useful information on other subjects. It is worthy of notice that until quite recent times the Mordovians knew no such word as "relations" (French, *parents*), and that they used only the word *tev*, or *tevx*, which corresponds to the *gens* as explained by Mr. Lewis Morgan. Only those were considered as kinsfolk who had a common descent, and lived under a common roof. The compound family continues to exist among the Mordovians, but on a very limited scale. The remarks of M. Mainoff on the kidnapping of brides are very interesting. This form of marriage still survives among the Mordovians, but it takes place with the consent of the bride, and very often with the knowledge of her parents.

MR. HOWARD GRUBB, telescope-maker, was one of a group of gentlemen on whom the Lord Lieutenant of Ireland conferred the honour of knighthood on Monday last.

THE additions to the Zoological Society's Gardens during the past week include a Rhesus Monkey (*Macacus rhesus*) from India, presented by Mr. Thos. D. Wickenden; a Macaque Monkey (*Macacus cynomolgus*) from India, presented by Mr. Charles Crocker; four Black-eared Marmosets (*Hapale penicillata*) from South-East Brazil, presented respectively, two each, by Mr. George Best and Mr. J. Crick; a Purple-faced Monkey (*Semnopithecus leucoprymnus*) from Ceylon, presented by Mr. H. Hart; a Ruffed Lemur (*Lemur varius* ♀) from Madagascar, presented by Mrs. M. Kestell-Cornish; a Moustache Monkey (*Cercopithecus cephus*), two Lesser White-nosed Monkeys (*Cercopithecus petaurista*), two White-crowned Mangabeys (*Cercocebus æthiops*), an African Civet Cat (*Viverra civetta*), a Blotched Genet (*Genetta tigrina*), a Two-spotted Paradoxure (*Nandinia binotata*), a White-crested Tiger-Bittern (*Tigrisoma* *leucolophum*), a Madagascar Porphyrio (*Porphyrio madagascariensis*), five Tambourine Pigeons (*Tympanistria bicolor*), three Schlegel's Doves (*Chalcopelia puella*) from West Africa, presented by Mr. J. B. Elliott; a Common Cormorant (*Phalacrocorax carbo*), European, presented by Mr. T. M. Oldham; a Great Eagle Owl (*Bubo maximus*), European, a Virginian Eagle Owl (*Bubo virginianus*) from North America, presented by Mr. Charles Clifton; a Hygian Snake (*Elaps hygiæ*) from Port Elizabeth, presented by Mr. W. K. Sibley; a Tarantula Spider (*Mygale*), from Bahama, presented by Mrs. Blake; a Sand Lizard (*Lacerta agilis*) from Jersey, presented by Mr. F. T. Mason; a Prince Albert's Curassow (*Crax alberti*) from Columbia, a Slender-billed Cockatoo (*Licmetis tenuirostris*) from South Australia, deposited; three Oyster-catchers (*Hæmatopus ostralegus*), European, purchased; a Blood-breasted Pigeon (*Phlogœnas cruentata*), bred in the Gardens.

## *OUR ASTRONOMICAL COLUMN.*

MAGNITUDES OF "NAUTICAL ALMANAC" STARS.—In order to expedite the publication of short articles upon astronomical and meteorological subjects, prepared at the Harvard College Observatory, Prof. Pickering has decided to print each as completed as successive numbers of a series, which, when a sufficient amount of material has been collected, will constitute the eighteenth volume of the Annals of the Observatory. Each number is to be published and distributed soon after it is prepared.

The first of this series of papers is a collection the stars employed in the standard lists of the *Nautical Almanacs* published by the Governments of Great Britain, the United States, France, Germany, and Spain, together with their magnitudes, as derived from four standard authorities: the *Harvard Photometry*, the *Uranometria Argentina*, Wolff's photometric observations, and the *Uranometria Oxoniensis*, the second and third being reduced to the photometric scale employed in the other two catalogues, the Harvard and Oxford scales agreeing closely. At present the magnitudes assigned to these stars in the respective Almanacs do not agree, nor do they represent the most accurate results available. Prof. Pickering therefore offered to the Superintendents of the Almanac Offices to supply a discussion of the best values of the magnitudes at present attainable; and favourable replies having been obtained in the cases of the French, Spanish, and American Almanacs, it is expected that the improved values here given will be used in those works in future.

The list embraces 800 stars, and of these the magnitudes of all but 64 depend at least upon two, and generally upon three, authorities; 132 stars being common to all four of the adopted standard catalogues of brightness. The average values of the residuals from the adopted means for these 132 stars are respectively: Harvard, 0·062; Argentine, 0·093; Wolff, 0·094; Oxford, 0·106. The average probable error of the adopted magnitudes is 0·09, assuming the absence of systematic error. The total number of residuals is 2188, of which only 67 exceed two-tenths of a magnitude, and only 17 three-tenths. There are only two cases of a residual exceeding four-tenths, both in the Oxford *Uranometria*; the one being the low star $\theta$ Ophiuchi, the other the double star $\theta$ Serpentis.

COMET 1887 *e* (BARNARD, MAY 12).—The following ephemeris for Berlin midnight is given by Dr. H. Kreutz (*Astr. Nachr.*, No. 2799). The comet is very favourably placed for observation, but is extremely faint.

| 1887 | R.A. h. m. s. | Decl. ° ′ | log *r* | log Δ |
|---|---|---|---|---|
| Aug. 24 | 18 41 48 | 7 40·8 N. | 0·2320 | 9·9641 |
| ,, 28 | 18 49 6 | 7 16·5 | 0·2402 | 9·9868 |
| Sept. 1 | 18 56 22 | 6 51·3 | 0·2484 | 0·0093 |
| ,, 5 | 19 3 34 | 6 25·7 N. | 0·2567 | 0·0315 |

## *ASTRONOMICAL PHENOMENA FOR THE WEEK* 1887 *AUGUST* 28—*SEPTEMBER* 3.

(FOR the reckoning of time the civil day, commencing at Greenwich mean midnight, counting the hours on to 24, is here employed.)

*At Greenwich on August* 28

Sun rises, 5h. 7m.; souths, 12h. 1m. 8·9s.; sets, 18h. 55m.; decl. on meridian, 9° 45′ N.: Sidereal Time at Sunset, 17h. 22m.

Moon (Full on September 2, 11h.) rises, 16h. 6m.; souths, 20h. 25m.; sets, 0h. 45m.*; decl. on meridian, 19° 44′ S.

| Planet. | Rises. h. m. | Souths. h. m. | Sets. h. m. | Decl. on meridian. ° ′ |
|---|---|---|---|---|
| Mercury | 3 50 | 11 15 | 18 40 | 15 26 N. |
| Venus | 8 25 | 13 47 | 19 9 | 8 6 S. |
| Mars | 1 48 | 9 48 | 17 48 | 20 57 N. |
| Jupiter | 10 28 | 15 34 | 20 40 | 11 10 S. |
| Saturn | 1 54 | 9 48 | 17 42 | 20 6 N. |

* Indicates that the setting is that of the following morning.

*Occultations of Stars by the Moon* (visible at Greenwich).

| August. | Star. | Mag. | Disap. h. m. | Reap. h. m. | Corresponding angles from vertex to right for inverted image. ° ° |
|---|---|---|---|---|---|
| 28 | $\xi'$ Sagittarii | 6 | 18 54 | 19 56 | 36 304 |
| Sept. | | | | | |
| 1 | 45 Capricorni | 6 | 0 15 | 1 29 | 108 330 |
| 1 | 44 Capricorni | 6 | 0 33 | near approach | 216 — |
| 2 | $\chi$ Aquarii | 5½ | 23 7 | near approach | 190 — |

| August. | h. | |
|---|---|---|
| 28 | 17 | Mars in conjunction with and 0° 49′ north of Saturn. |
| 29 | 11 | Venus stationary. |

*Variable Stars.*

| Star. | R.A. h. m. | Decl. ° ′ | | h. m. | |
|---|---|---|---|---|---|
| U Monocerotis | 7 25·6 | 9 33 S. | Aug. 31, | | *m* |
| W Virginis | 13 20·2 | 2 48 S. | ,, 31, | 23 0 | *M* |
| δ Libræ | 14 54·9 | 8 4 S. | ,, 29, | 4 57 | *m* |
| | | | Sept. 2, | 20 40 | *m* |
| U Coronæ | 15 13·6 | 32 4 N. | Aug. 29, | 20 18 | *m* |
| S Libræ | 15 14·9 | 19 59 S. | Sept. 2, | | *M* |
| U Ophiuchi | 17 10·8 | 1 20 N. | Aug. 31, | 4 46 | *m* |
| | | | and at intervals of | 20 8 | |
| X Sagittarii | 17 40·5 | 27 47 S. | Aug. 31, | 22 0 | *m* |
| W Sagittarii | 17 57·8 | 29 35 S. | ,, 28, | 0 0 | *M* |
| R Scuti | 18 41·5 | 5 50 S. | ,, 28, | | *m* |
| R Lyræ | 18 51·9 | 43 48 N. | ,, 31, | | *M* |
| S Vulpeculæ | 19 43·8 | 27 0 S. | Sept. 2, | | *m* |
| $\chi$ Cygni | 19 46·2 | 32 38 N. | Aug. 29, | | *m* |
| S Sagittæ | 19 50·9 | 16 20 N. | ,, 31, | 0 0 | *m* |

*M* signifies maximum; *m* minimum.

## *THE FACTORS OF ORGANIC EVOLUTION.*

WHILE reviewing, a short time ago, Mr. Herbert Spencer's essay on the above subject (NATURE, vol. xxxv. p. 262), I promised to consider the present standing of the question as to whether, or how far, use and disuse admit of being regarded as true causes of change of organic type. Of course there is no question about the effects of use and disuse as regards the individual: the only question is as to whether, or how far, these effects admit of being inherited, so that modifications of structure which are produced by modifications of function in the individual become causes of corresponding, and therefore of adaptive, changes of structure in species. The importance of this question is second to none in the whole range of biology. For not only is it of the highest importance within the range of biology itself—governing, by whatever answer we give it, our estimate of the importance of natural selection, and thus requiring to be dealt with on the very threshold of biological philosophy—but its influence extends to almost every department of thought. For, as Mr. Spencer remarks in his preface, upon the answer which this question may finally receive will depend in chief part the sciences of psychology, ethics, and sociology. If functionally-produced modifications are inheritable, the phenomena of instinct, innate ideas, moral intuitions, and so forth, admit of a scientific explanation at the present moment; otherwise they do not, or, at least, not in so distinct nor in so complete a manner. Therefore, we can hardly feel that Mr. Spencer exaggerates the importance of this question when he says of it, "Considering the width and depth of the effects which our acceptance of one or other of these hypotheses [namely, that functionally-produced modifications are inherited, or that they are not] must have upon our views of Life, Mind, Morals, and Politics, the question—Which of them is true? demands, beyond all other questions whatever, the attention of scientific men."

That functionally-produced modifications are inherited was the great assumption upon which Lamarck founded his theory of evolution. Erasmus Darwin adopted the assumption, and it was also accepted by Charles Darwin as representing a highly important factor of organic evolution, although subsidiary to that of natural selection. Lastly, Mr. Spencer has always upheld the assumption, and, as we shall subsequently see, has done more than anybody else in the way of its justification. On the other hand, of late years a growing tendency has been displayed by those evolutionists who out-Darwin Darwin, not only to assign to natural selection a monarchical government over the whole realm of organic Nature, but also, and consequently, to deprive use and disuse of those lesser sovereignties which were so freely accorded to them by the "Origin of Species." This tendency has now reached a climax in the publication of an essay, by no less an authority than Prof. Weismann, wherein the Lamarckian principles of use and disuse are denied *in toto*.[1] We may therefore best begin our stock-taking of the whole subject by considering what Prof. Weismann has said; for assuredly the doctrine of use and disuse as themselves useless could nowhere meet with an abler champion.

In the first place, he is committed to this doctrine as a necessary consequence of his own theory of heredity, according to which *any* change *acquired* by the individual cannot be transmitted to progeny. This theory regards the individual organism as nothing more than what may be termed a temporary receptacle of "germ-plasma"—this germ-plasma being handed on from generation to generation, without ever being affected by any changes that may take place in the organisms which contain it. And the only reason why such *appears* to be the case—or why in the course of generations one specific type gradually changes through inherited modifications into another—is because the germ-plasma itself is liable to variation, and when the variations happen to be of a kind which lead to favourable modifications of the store-houses (organisms), these store-houses are preserved by natural selection, and with them the peculiar variations of the germ-plasma, which are thus carried on to the next generation. Hence natural selection is really at work upon variations of the germ-plasma, and hence also no change occurring in an organism during its own life-history can at all affect its progeny—any more, for instance, than the chipping or the twisting of a vessel can modify the chemical constitution of whatever substance the vessel may contain. In short, it is only so-called congenital variations—or variations of germ-plasma—that can be inherited; and, therefore, it is only upon such variations that survival of the fittest is able to act. All variations afterwards superinduced in the organism—whether by way of mutilation, disease, acquisition of faculty, or degeneration of structure—are destined to be immediately extinguished by the death of the organism. Now, from this general theory it necessarily follows that the effects of use and disuse in the individual cannot be transmitted to progeny; for, if they could, the fact would be fatal to the theory. Hence it is, as above observed, that Prof. Weismann is committed by his theory of heredity to a denial of the Lamarckian assumption, which, as we have seen, was accepted by Darwin.

But besides this merely *a priori* ground of deduction from his own theory, Prof. Weismann stands upon the affirmation that there is, as a matter of fact, no real evidence of the effects of use and disuse being inherited. For, he maintains, all the supposed evidence on this head admits of being fully interpreted by quite another principle. When an organ (or any structure) falls into disuse, in the course of generations it atrophies, becomes rudimentary, and finally disappears. This fact is generally taken as proof of the inherited effects of disuse—seeing that it is so strikingly analogous to these effects in the case of individual organisms. But there is an alternative possibility. The *raison d'être* of the organ before it fell into disuse, was its utility: it was originally built up under the nursing influence of natural selection solely on account of its serviceability. When therefore from changed conditions of life, or for any other reason, the organ ceased to be serviceable, the premium which had been previously set upon it by natural selection was withdrawn; individuals which happened to present the organ of a size below the average were no longer eliminated in the struggle for existence, but were allowed to propagate. Thus, by free intercrossing, the average size became less and less in every succeeding generation, until eventually, according to Weismann, it must altogether disappear. In short, as the organ was originally built up by natural selection, when natural selection was withdrawn, is any other explanation required of the fact that the organ progressively dwindled?

[1] "Ueber den Rückschritt in der Natur" (Freiburg, 1886).

Unknown to Prof. Weismann, this principle, under the name "Cessation of Selection," was enunciated by the present writer in a series of articles published in these pages so long ago as 1873-74. Attention is now drawn to this fact merely for the sake of informing biologists that the principle met with the full approval of the late Mr. Darwin, and also to state exactly the shape in which it was thus approved by him. For in one or two particulars the idea as published in NATURE differs from that which has been recently and independently arrived at by Prof. Weismann. As the issues of NATURE in question are out of print, and as the matter cannot be more briefly stated now than it was stated then, I may best begin by reprinting the portion of these articles which sets forth the principle of the cessation of selection, as this was accepted by Mr. Darwin.

"In a former communication (NATURE, vol. ix. p. 361) I promised to advance what seemed to me a probable cause—additional to those already known—of the reduction of useless structures. As before stated, it was suggested to me by the penetrating theory proposed by Mr. Darwin (NATURE, vol. viii. pp. 432 and 505), to which, indeed, it is but a supplement.[1] Epitomising Mr. Darwin's conception as the dwarfing influence of impoverished conditions progressively reducing the average size of a useless structure by means of free intercrossing, the present cause may be defined as the mere cessation of the selective influence from changed condition of life.

"Suppose a structure to have been raised by natural selection from 0 to average size 100, and then to have become wholly useless. The selective influence would now not only be withdrawn, but reversed; for, through Economy of Growth—understanding by this term both the direct and the indirect influence of natural selection—it would rigidly eliminate the variations 101, 102, 103, &c., and favour the variations 99, 98, 97, &c. For the sake of definition we shall neglect the influence of economy acting below 100, and so isolate the effects due to the mere withdrawal of selection. By the conditions of our assumption, all variations above 100 are eliminated, while below 100 indiscriminate variation is permitted. Thus, the selective premium upon variation 99 being no greater than that upon 98, 98 would have as good a chance of leaving offspring which would inherit and transmit this variation as would 99; similarly, 97 would have as good a chance as 98, and so on. Now there is a much greater chance of variations being perpetuated at or below 99, than at or above 100, for at 100 the hard line of selection (or economy) is fixed, while there is no corresponding line below 100. The consequence of free intercrossing would therefore be to reduce the average from 100 to 99. Simultaneously, however, with this reducing process, other variations would be surviving below 99, in greater numbers than above 99; consequently the average would next become reduced to 98. There would thus be 'two operations going on side by side—the one ever destroying the symmetry of distribution' round the average, 'and the other ever tending to restore it.' It is evident, however, that the more the average is reduced by this process of indiscriminate variation the less chance there remains for its further reduction. When, for instance, it falls to 90, there are numerically (though not actually, because of inheritance) 89 to 9 in favour of diminution; but when it falls to 80 there are only 79 to 19 in such favour. Thus, theoretically, the average would continue to diminish at a slower and slower rate, until it comes to 50, where, the chances in favour of increase and of diminution being equal, it would remain stationary.

"Having thus, for the sake of clearness, considered this principle apart, let us now observe the effect of superadding to it the influence of the economy of growth—a principle with which its action must always be associated. Briefly, as this

[1] As stated in the text, the leading idea in Mr. Darwin's suggestion was that impoverished conditions of life would accentuate the principle of Economy of Nutrition, and so assist in the reduction of useless structures by free intercrossing. Now, in this idea that of the cessation of selection was really implied; but neither in his own article nor in a subsequent letter by Mr. George Darwin on the same subject (NATURE, October 16, 1873), was it exhibited as an independent principle. It was inarticulately wrapped up with the much less significant principle of impoverished conditions. Afterwards, however, Mr. Darwin expressed himself as fully persuaded of the independent character of the more important principle, which he was really the first to perceive, although not clearly to express. Moreover, he then thought it was probably a principle of universal application, not only as regards rudimentary organs, but also as regards degenerated structures in general.

influence would be that of continually favouring the variations on the side of diminution, the effect of its presence would be that of continuously preventing the average from becoming fixed at 50, 40, 30, &c. In other words, the 'hard line of selection' which was originally placed at 100, would now become progressively lowered through 90, 80, 70, &c.; always allowing indiscriminate variation below the barrier, but never above it.[1]

"It will be understood that by 'cessation of selection from changed conditions of life,' I mean a change of any kind which renders the affected organ superfluous. Take, for example, the exact converse of Mr. George Darwin's illustration, by supposing a herd of cattle to migrate from a small tract of poor pasture to a large tract of rich. Segregation would ensue, the law of battle would become less severe, while variation would be promoted in a cumulative manner by the increase of food. The young males with shorter horns would thus have as good a chance of leaving progeny as 'their longer-horned brothers,' and the average length would gradually diminish as in the other case. Of course, as the predisposing cause of impoverished nutrition would now be absent, the reducing process would take place at a slower rate. Moreover, it is to be remarked that this principle differs in an important particular from that enunciated by Mr. Darwin, in that it could never reduce an organ much below the point at which the economy of growth, together with disuse, ceases to act. For, returning to our numerical illustration, suppose this point to be 6, the average would eventually become fixed at 3.

"That the principle thus explained has a real existence we may safely conclude, theoretical considerations apart, from the analogy afforded by our domestic races; for nothing is more certain to breeders than the fact that neglect causes degeneration, even though the strain be kept isolated."

Evidence of the wide-reaching operation of this principle under Nature must be sought for in cases where it is impossible that disuse can have had any part in the reducing process—seeing that we cannot all agree with Prof. Weismann in dismissing the agency of disuse on *a priori* grounds of deduction from his theory of germ-plasma. Now, although it is not at all an easy thing to find cases where the influence of the cessation of selection admits of being demonstrably dissociated from the possible influence of disuse, the following appear to meet the requirements of the proof:—

(1) The whole multitude of instances where recapitulative phases are absent from the developmental history of an embryo may stand for so many proofs of reduction without the agency of disuse. For, inasmuch as none of the structures represented in those phases elsewhere can ever have been of any *use* to the embryo from which they have disappeared, it is sufficiently evident that their obliteration can never have been due to *disuse*. And, forasmuch as such structures persist in the embryos of *allied* species, it appears equally evident that their reduction cannot be ascribed to natural selection acting through the economy of nutrition; for, were this the case, natural selection ought to have effected the reduction in the embryos of all the species.

(2) Even in adult organisms we meet with many structures which, although of obvious *use* in the sense of affording protection, yet cannot be said ever to be *used* in the sense of being actively employed, or of being employed in any way that could possibly lead to their structure being modified by their function. Of such, for example, are the hard coverings of animals and of parts of plants. It is impossible that the thickness of shells, for instance, can ever have been increased by their "use" as protective coverings, seeing that the use is here wholly passive—is not of the active kind which determines a greater flow of nutrition to the part. Hence, we can only attribute the formation of such structures to the unaided influence of selection. But, if so, we can only attribute to the cessation of selection their subsequent degeneration in all cases—such as that of male cirripedes, hinder parts of hermit crabs, &c.—where changed conditions of life have rendered these parts no longer needful in the struggle for existence. Here, indeed, economy of growth may have assisted in the reduction; but, whether or not, disuse can scarcely have done so, and this is the point with which we are at present concerned.

(3) In many species of social Hymenoptera the neuter insects have lost their wings. Now, as these neuter insects never have progeny, it is evident that the reduction of their wings cannot possibly have been due to the inherited effects of disuse. We must, therefore, set it down to the cessation of selection, joined, perhaps, with the economy of growth. This is a particularly cogent line of proof, seeing that in some species the head, jaws, and other parts of the neuters have been *enlarged*, in order the better to fit them for heavy work where strength or fighting is required. Had such an enlargement been met with in the case of an animal which leaves progeny, the fact might well have been attributed to the inherited effects of increased *use*. But, as the matter stands, these neuter insects are available as a demonstrative and a double proof of the possibility both of the development and the degeneration of important structures without the aid either of use or of disuse.

(4) In his essay on "Degeneration," Prof. Lankester names three distinct sets of conditions as those under which the process has taken place, and all these are conditions under which the cessation of selection must have taken place. First, "Any new set of conditions occurring to an animal which render its food and safety very easily attained, seem to lead as a rule to degeneration. . . . The habit of parasitism clearly acts upon animal organisation in this way. Let the parasitic life once be secured, and away go legs, jaws, eyes, and ears." In other words, so soon as these organs, which were originally built up by natural selection for the purpose of securing "food and safety," are rendered superfluous by food and safety being otherwise secured, all selective premium on their efficiency is withdrawn, and so they are allowed to degenerate by indiscriminate variation. Second, "Let us suppose a race of animals fitted and accustomed to catch their food, and having a variety of organs to help them in the chase—suppose such animals suddenly to acquire the power of feeding on the carbonic acid dissolved in the water around them just as green plants do. This would tend to degeneration; they would cease to hunt their food, and would bask in the sunlight, taking food in by the whole surface, as plants do by their leaves. . . . These vegetating animals . . . . show how a degeneration of animal forms may be caused by *vegetative nutrition*." Now, to "cease to hunt their food" is here equivalent to their ceasing to be under the influence of natural selection with respect to their food-hunting organs, just as in the previous case. Third, "Another possible cause of degeneration appears to be the indirect one of minute size. . . . The needs of a very minute creature are limited as compared with those of a large one, and thus we may find heart and blood-vessels, gills and kidneys, besides legs and muscles, lost by the diminutive degenerate descendants of a larger race." But, if "the *needs* of a very minute creature are limited as compared with those of a large one," this is the same as to say that in the "diminutive descendants of a larger race" natural selection will no longer operate for the maintenance of structures which have become needless. In fact, in this passage Prof. Lankester comes very near an express statement of the principle of the cessation of selection.

The sundry instances given in the above paragraphs may, I hope, be held sufficient firmly to establish this principle, and to show that it is one of universal application, wherever an organ or a structure has ceased to be of service to the species presenting it.[1] Now, quite apart from the reference in which we have

[1] It is desirable to remark that this numerical mode of representing the principle is adopted only for the purposes of exposition. The exact point at which equilibrium would be reached *in actual fact* we have no means of ascertaining, since such would depend in any given case upon the original force of inheritance, or the persistence with which heredity would assert itself when left entirely to itself—and of this we have no means of judging. Therefore, I adopt the numerical mode of representing the progressive decline of a structure under the cessation of selection merely to show that at *whatever* point we may suppose equilibrium to be reached—or a state of balance between heredity and indiscriminate variation to be attained—this point must become progressively lowered by the superadded influence of the economy of growth. It may, however, be remarked that the initial stages of reduction would probably take place more rapidly than subsequent stages, seeing that the maximum efficiency of a structure is maintained, not only by heredity, but also by the continued influence of selection. Therefore, when the influence of selection is withdrawn, indiscriminate variation would rapidly degrade the structure through the initial stages of its reduction.

[1] Or, if these instances are not held sufficient for this purpose, I may refer to Prof. Weismann's essay, where further instances are given, and also supplement them with the following passage from my old articles in NATURE:—

"If it be supposed that disuse is the chief cause of atrophy in wild species, then it has not produced so much effect in tame species as we should antecedently expect. . . . For, supposing the cessation of selection to be here the *only* cause at work, what degree of atrophy should we expect to find? Before I turned to the valuable measurements given in the 'Variation of Plants and Animals under Domestication,' I concluded (cf. NATURE, vol. ix. p. 441) that from 20 to 25 per cent. is the maximum of reduction we should expect this unassisted principle to accomplish, in the case of natural as distinguished from artificially-bred organs. Now on calculating the average afforded by each of Mr. Darwin's tables, and then reducing the averages to parts of 100, I find that the highest average decrease is 16 per cent., and the lowest 5; the average of the averages being rather less than 12. Only four hitherto been considering this principle—or with reference to use and disuse—we have here a consideration of great importance in regard to the subject of Prof. Lankester's essay above quoted. Apparently without having either heard or thought of the principle of cessation, Dr. Dohrn was led to attribute an important part in the drama of evolution to the effects of cessation, as these are witnessed in the phenomena of degeneration.[1] About the *facts* of degeneration there can be no doubt, and to this naturalist belongs the credit of having first perceived the wide range of their importance. But, on account of having missed the principle of cessation, both Dr. Dohrn and his English expositor, Prof. Lankester,[2] fell into an omission of *interpretation*. For they both attributed the facts of degeneration to a *reversal* of natural selection; they represented that degeneration could only take place under a change in the conditions of life such that organs or structures previously useful become, not merely useless, but deleterious. Degeneration was thus regarded as always the result of what may be termed active hostility on the part of natural selection; not as the result of a merely passive disregard. Hence the sphere within which the phenomena of degeneration might be expected —or admitted of being satisfactorily explained—was needlessly limited. For instance, Prof. Lankester writes: "It is clearly enough possible for a set of forces such as we sum up in the term 'natural selection' to so act on the structure of an organism as to . . . . . diminish the complexity of its structure." But in order "to diminish the complexity" of any useless structure, it is not necessary that natural selection should "act on the structure": the complexity, like the size, of the structure would necessarily diminish under the mere withdrawal of selection. And hence the phenomena of degeneration do not require, either that the organism presenting them should ever have found its useless organs actively deleterious, or that there should ever have been any "Functions-wechsels" in the case.[3]

The case of degenerated *complexity* proves that the cessation of selection may effect degradation without assistance from the economy of nutrition. I am therefore more disposed to think that the *size* of any useless structure may be reduced to a greater extent by the mere cessation of selection (apart from economy), than I thought when writing the articles above quoted. Here, however, we must remember that the hold which heredity has upon complexity is much less than that which it has upon size. This is evident, not only from obvious considerations of an *a priori* kind, but also from such cases as those of the blind crabs of Kentucky. Here the disused eyes have been lost, while the foot-stalks which originally supported them have been retained. Now, we can well understand why the eyes should have been the first to disappear under the cessation of selection, seeing that they were structures so highly organised that the continuous influence of selection must have been required to preserve them in a state of efficiency before the animals began to inhabit the dark caves; and, therefore, that when the animals did begin to inhabit these caves, such refined and complex structures would rapidly degenerate through the mere withdrawal of selection. But if we were to attribute any large share in this process of rapid degeneration to the economy of nutrition, we should be unable to explain the persistence of the foot-stalks. Therefore, the cessation of selection, when acting alone, is thus proved capable of reducing a complex structure more quickly than it can reduce a larger but less complex structure, in the same species and under the same conditions.

It is true that in a passage above quoted, and which was published two years before Dr. Dohrn's essay, I myself attributed the phenomena of degeneration to a "reversal of natural selection."[1] But I alluded to such reversal only in so far as it arose from the economy of nutrition (*i.e.* I did not suppose that degeneration can only occur when useless parts become actively deleterious, and therefore require the active agency of selection to remove them); and the effect of reading the subsequently published literature on the subject of degeneration has been to make me attribute more importance to the cessation of selection, and less importance to the economy of nutrition. Nevertheless, I still believe that these principles are inadequate to explain the final and total obliteration of organs which by their combined action they have rendered rudimentary.

And these remarks lead me to indicate the points wherein my hypothesis of the cessation of selection differs from that which has recently been published by Prof. Weismann. Briefly, he does not mention the assistance which this principle derives from that of the economy of nutrition, and he believes that it is in itself sufficient to explain the final and total obliteration of useless parts. Having already given my reasons for holding different views with regard to both these points, it will now suffice merely to re-state the principles which I suggested in the NATURE articles as having been most probably concerned in this final and total obliteration of useless parts. These principles are two in number, and are both quite independent of those which we have hitherto been considering. The first of them is inheritance at earlier periods of life, which progressively pushes back the development of a useless rudiment to a more and more embryonic stage of growth; and the second is the eventual failure of the principle of inheritance itself. For, "whether or not we believe in Pangenesis, we cannot but deem it in the highest degree improbable that the influence of heredity is of unlimited duration."[2] This view of the matter renders it abundantly intelligible why it is that, when once the cessation of selection—co-operating with the economy of nutrition—has with comparative rapidity reduced any useless organ to a rudiment, the latter should then persist for so enormous a length of time that in the result, as Mr. Darwin observes, "rudimentary organs are so extremely common that scarcely one species can be named which is wholly free from a blemish of this nature."

We have seen that in the cessation of selection we must recognise one of the principal causes of atrophy in species; in whatever measure we hold the presence of selection explanatory of evolution, in a corresponding measure must we hold the withdrawal of selection accountable for degeneration. But from this it does not necessarily follow that no other causes either of evolution or of degeneration are to be found. Those naturalists who adopt the light and easy method of out-Darwining Darwin, or close their eyes to every other "factor" save that of natural selection, may indeed rest satisfied with these two complementary principles as in themselves adequate to explain all

individual cases fall below 25 per cent., and of these two should be omitted (cf. 'Variation,' p. 272). Thus, out of eighty-three examples, only two fall below the lowest average expected (*i.e.* on the supposition that disuse has not had anything to do with the reduction). Moreover, we should scarcely expect disuse alone to affect in so similar a degree such widely different tissues as are brain and muscle. The deformity of the sternum in fowls also points to the cessation of selection rather than to disuse. Further, the fact that several of our domestic animals have not varied at all is inexplicable upon the one supposition, while it affords no difficulty to the other. We have seen that disuse can only act by causing variations; and so we can see no reason why, if it acts upon a duck, it should not also act upon a goose. But the cessation of selection depends upon variations being supplied to it; and so, if from any reason a specific type does not vary, this principle cannot act. Why one type should vary, and another not, is a distinct question, the difficulty of which is embodied by the one supposition, and excluded by the other. For, to say that disuse has not acted upon type A, because of its inflexible constitution, while it has acted on a closely allied type B, because of its flexible constitution, is merely to insinuate that disuse, having proved itself inadequate to cause reduction in the one case, may not have been the efficient cause of reduction in the other. But the counter-supposition altogether excludes the idea of a causal connection, and so rests upon the more ultimate fact of differential variability, as not requiring to be explained. Lastly, it is remarkable that those animals which have not suffered reduction in any part of their bodies are likewise the animals which have not varied in any other way, and conversely; for as there can be no causal connection between these two peculiarities, the fact of the intimate association between them tends to show that special reduction depends upon general variability, rather than that special variability depends upon special reducing causes."

[1] "Der Ursprung der Wirbelthiere und der Princip des Functionswechsels" (Leipzig, 1875).

[2] "Degeneration: a Chapter in Darwinism" (London, 1880).

[3] The same considerations apply to the size of an organism as a whole. If for any reason it ceases to be an advantage to be kept up to the ancestral standard of size, the cessation of selection as regards size would result in a gradual diminution of size, even though the ancestral standard of size were not actually deleterious. Yet, in the last of the passages above quoted from Prof. Lankester—and the passage in his essay where he most nearly approaches the principle of selection as withdrawn—the context shows that he only has in view the principle of selection as reversed. For he says:—"It cannot be doubted that natural selection has frequently acted on a race of animals so as to reduce the size of the individuals. The smallness of size has been favourable to their survival in the struggle for existence." Of course "it cannot be doubted" that this has been so in many cases; but as little can it be doubted that it has not been so in all. In any given case of diminution, it is not necessary to suppose that "the smallness of size has been *favourable* in the struggle for existence"; it is enough if the previous largeness of size has *ceased* to be so, or that smallness of size is no longer *deleterious*. Moreover, the same considerations apply to instincts. For example, it can scarcely ever have been a *fatal disadvantage* to the slave-making ants that they should be able to eat their own food; therefore the loss of their original instincts, which now renders them dependent on their slaves for being fed, can only have been brought about by the *cessation* of selection—not by its *reversal*.

[1] Prof. Weismann christens the principle which I have called Cessation of Selection, *Kehrseite der Naturzüchtung*; but, for reasons above given, I do not think that this is so good a name as that which he elsewhere uses incidentally, and which, indeed, is an unconscious translation of my own term —namely, *Nachlass der Naturzüchtung*.

[2] NATURE, *loc. cit.*, where see for a fuller discussion of the causes leading to eventual and total suppression.

the facts both of progress and of regress. But, unless we are satisfied to walk upon the high *priori* road to the exclusion of every other, we must not too readily assume that the presence and the absence of selection have been the only factors at work. In particular, we have now to consider whether use and disuse have co-operated with the presence and the absence of selection in bringing about the existing state of matters in organic Nature as a whole.

Now, the only way in which this inquiry can be conducted is by the method of difference. We must search through organic Nature in order to ascertain whether there are any cases either of evolution or of degeneration where it is manifestly impossible that either the presence or the absence of selection can have had anything to do with the process. If we can find any such cases, we shall not merely save Darwin from his friends by justifying his acceptance of the Lamarckian assumption: we shall prove that presumably in *all* cases where the presence or the absence of selection has been concerned in either building up or breaking down organic structures, these principles have been largely *assisted* in their operations by the inherited effects of use and disuse. For if it can be proved that these effects are inherited in cases where it is impossible that the principle of selection—or its cessation—can have obtained, it would be irrational to deny that they are also inherited in other cases where these principles do obtain.

Seeing that so accomplished a naturalist and so philosophic a thinker as Prof. Weismann has declared that there is no one case to be found such as those of which we are in search, we must be prepared to expect some difficulty in meeting with examples of the uncompounded influence of use and disuse—even supposing use and disuse to be the true causes of specific modification that they were taken to be by Darwin. In order to show the kind of difficulty that here besets inquiry, I will quote a passage from Mr. Spencer's recently-published essay upon the subject.

"When discussing the question more than twenty years ago ('Principles of Biology,' § 166), I instanced the decreased size of the jaws in the civilised races of mankind as a change not accounted for by the natural selection of favourable variations; since no one of the decrements by which, in thousands of years, this reduction has been effected would have given to an individual in which it occurred such advantage as would cause his survival, either through diminished cost of local nutrition or diminished weight to be carried. . . . Reconsideration of the facts does not show me the invalidity of the conclusion drawn, that this decrease in the size of the jaw can have had no other cause than continued inheritance of those diminutions consequent on diminutions of function, implied by the use of selected and well-prepared food. Here, however, my chief purpose is to add an instance showing, even more clearly, the connection between change of function and change of structure. This instance, allied in nature to the other, is presented by those varieties—or, rather, sub-varieties—of dogs, which, having been household pets, and habitually fed on soft food, have not been called upon to use their jaws in tearing and crunching, and have been but rarely allowed to use them in catching prey and in fighting."

There follows an account of a somewhat laborious examination of dogs' skulls in the Museum of Natural History, the result of which was to show that "we have two, if not three, kinds of dog, which, similarly leading protected and pampered lives, show that in the course of generations the parts concerned in clenching the jaws have dwindled;" after which the passage proceeds as follows:—

"To what cause must this decrease be ascribed? Certainly not to artificial selection; for most of the modifications named make no appreciable external signs: the width across the zygomata could alone be perceived. Neither can natural selection have had anything to do with it; for even were there any struggle for existence among such dogs, it cannot be contended that any advantage in the struggle could be gained by an individual in which a decrease took place. Economy of nutrition, too, is excluded. Abundantly fed as such dogs are, the constitutional tendency is to find places where excess of absorbed nutriment may be conveniently deposited, rather than to find places where the cutting down of the supplies is practicable. Nor, again, can there be alleged a possible correlation between these diminutions and that shortening of the jaws which has probably resulted from selection; for in the bull-dog, which has also relatively short jaws, the structures concerned in closing them are unusually large. Thus, there remains as the only conceivable cause, the diminution of size which results from diminished use."

Evidently Mr. Spencer has never heard or thought of the cessation of selection, either as explained thirteen years ago by myself, or as republished within the last few months by Prof. Weismann. For it is evident that, far from his having excluded all conceivable causes of the diminution save that of diminished use, it would be difficult to find a case more favourable to the influence of the cessation of selection. The dogs in question have been "habitually fed on soft food, have not been called on to use their jaws in tearing and crunching, and have been but rarely allowed to use them in catching prey and in fighting." In other words, for at least a hundred generations these dogs have been "leading protected and pampered lives," wholly shielded from the struggle for existence and survival of the fittest. Never having had to use their jaws either in "tearing, crunching, catching prey, or fighting," they, more than any other dogs—even of domesticated breeds—have not been "called on" to use their jaws for any life-serving purpose. Clearly, therefore, if the cessation of selection ever acts at all as a reducing cause in species, here is a case where it is positively bound to act. And, of course, the same remark applies to the analogous case of the diminished size of the jaws in civilised man.

Be it observed, I am not disputing that disuse may in both these cases have co-operated with the cessation of selection in bringing about the observed result. Indeed, I am rather disposed to allow that the large amount of reduction described in the case of the dogs as having taken place in so comparatively short a time, is strongly suggestive of disuse having co-operated with the cessation of selection. But at present I am merely pointing out that Mr. Spencer's investigations have here failed to exhibit the crucial proof of disuse as a reducing cause which he assigns to them: it is not true that in these cases disuse "remains as the only conceivable cause."

Far more successful, however, is his second line of argument. Indeed, to me it has always appeared, since I first encountered it fifteen years ago in the "Principles of Biology," as little short of demonstrative proof of the Lamarckian assumption. Therefore, if, as a result of reading the passage above quoted, one feels disposed to regret that before publishing it Mr. Spencer did not have his attention called to Prof. Weismann's essay on the cessation of selection, still more must one regret that before publishing that essay Prof. Weismann should have failed to remember the "Principles of Biology." For, had he done so, it seems impossible that he could ever have committed himself to the statement that there is no evidence of functionally-produced modifications being inherited, and thus he might have been led to pause before announcing—at least in its present shape—his theory of germ-plasma.

The argument whereby in my opinion Mr. Spencer succeeds in virtually proving the truth of the Lamarckian assumption is expanded in his recently-published essay, from which, therefore, I will quote.

"If, then, in cases where we can test it, we find no concomitant variation in co-operative parts that are near together—if we do not find it in parts which, though belonging to different tissues, are so closely united as teeth and jaws—if we do not find it even when the co-operative parts are not only closely united, but are formed out of the same tissue, like the crab's eye and its peduncle; what shall we say of co-operative parts which, besides being composed of different tissues, are remote from one another? Not only are we forbidden to assume that they vary together, but we are warranted in asserting that they can have no tendency to vary together. And what are the implications in cases where increase of a structure can be of no service unless there is concomitant increase in many distant structures, which have to join it in performing the action for which it is useful?

"As far back as 1864 ('Principles of Biology,' § 166) I named in illustration an animal carrying heavy horns—the extinct Irish elk; and indicated the many changes in bones, muscles, blood-vessels, nerves, composing the fore-part of the body, which would be required to make an increment of size in such horns advantageous. Here let me take another instance—that of the giraffe: an instance which I take partly because, in the sixth [last] edition of the 'Origin of Species,' issued in 1872, Mr. Darwin has referred to this animal when effectually disposing of certain arguments urged against his hypothesis. He there says:—

"'In order that an animal should acquire some structure specially and largely developed, it is almost indispensable that several other parts should be modified and co-adapted. Although every part of the body varies slightly, it does not follow that the necessary parts should always vary in the right direction and to the right degree' (p. 179).

"And in the summary of the chapter, he remarks concerning the adjustments in the same quadruped, that 'the prolonged use of all the parts, together with inheritance, will have aided in an important manner in this co-adaptation' (p. 199): a remark probably having reference to the increased massiveness of the lower part of the neck; the increased size and strength of the thorax required to bear the additional burden; and the increased strength of the fore-legs required to carry the greater weight of both. But now I think that further consideration suggests the belief that the entailed modifications are much more numerous and remote than at first appears; and that the greater part of these are such as cannot be ascribed in any degree to the selection of favourable variations, but must be ascribed exclusively to the inherited effects of changed functions."

The passage then proceeds to trace these modifications of structure in detail; showing that the changes in the fore-quarters entail corresponding changes in the hind-quarters, which when running "perform actions differing in one or another way and degree from all the actions performed by the homologous bones and muscles in a mammal of ordinary proportions, and from those of the ancestral mammal which gave birth to the giraffe." Thus it is shown that bones, muscles, blood-vessels, nerves, and indeed nearly all the constituent structures of the body, have everywhere been more or less modified as to relative size and function, in order to adapt the giraffe as a whole to the unusual development of its neck: this unusual development has entailed changes, and changes, and counter-changes, which have eventually spread throughout the whole organisation of the animal.

Now, it appears to me that we have in this a most cogent argument in favour of the inherited effects of use and disuse. For, seeing how immense must be the sum of the organic changes required to produce this mutual co-adaptation of many structures, the chances against their all happening to occur together by way of fortuitous variation must be, as Mr. Spencer observes, infinity to one. Yet unless they all did occur together in the same organism—and this repeatedly—the co-adaptations in question cannot have been due to natural selection.

With more or less success Mr. Spencer develops several other lines of argument; but as they cannot well be reproduced without occupying more space than can here be allowed, I will conclude by adding to his material yet another consideration which appears to me to be entitled to great weight. When we search through the animal kingdom, we meet with certain instincts which cannot reasonably be supposed to subserve any such life-preserving function as that which has led to the survival, through natural selection, of instincts in general. Now the existence of instincts which are thus not of vital importance to the species presenting them can only be explained by the hereditary effects of function. For instance, it is difficult to suppose that the instinct, which is still inherited by our domesticated dogs, of turning round and round to trample down a comfortable bed before lying down, can ever have been of so life-preserving a character as to have been developed by survival of the fittest. Or, if this instance be held doubtful, what shall we say to the courting instincts in general, and to the play-instincts of the bower-bird in particular, which are surely quite without meaning from any utilitarian point of view? And these instincts naturally lead on to the æsthetic faculties of mankind, few of which can be possibly ascribed to natural selection, as Mr. Spencer very conclusively shows.

And here it becomes needful again to say a few words on Prof. Weismann's essay, by way of criticism. For he, too, has there considered the case of instincts, but this in a manner which can scarcely be termed fortunate. For example, he particularly instances the case of hereditary fear of enemies as one which supports his argument against the inheritance of functionally-produced modifications. Now, this happens to be one of the instincts which I have elsewhere specially chosen as yielding particularly good proof of the hereditary transmission of individual experience, apart from natural selection. And the proof consists merely in showing, from abundant testimony, that "the original tameness of animals in islands unfrequented by man gradually passes into an hereditary instinct of wildness as the special experiences of man's proclivities accumulate; and that such instinctive adaptation to newly developing conditions may take place without much aid from selection is shown by the short time, or the small number of generations, which is sufficient to allow for the change."[1] But although I think that Prof. Weismann's selection of this instinct is a particularly unfortunate one for the purpose of showing that its *acquisition* can *only* be due to natural selection, I quite agree with him in holding that its degeneration in our domesticated animals is due to the withdrawal of natural selection—at least in considerable part.

[1] "Mental Evolution in Animals," p. 197, where see for evidence.

Again, he argues that if acquired mental proclivities are ever inherited we should expect the human infant, without any individual instruction, to converse. For, he argues, ever since man became human he has been a talking animal, and therefore, if there were any truth in the view that knowledge acquired by individuals tends to be transmitted to their progeny, here is a case where the fact ought to admit of abundant proof: yet every child requires to be taught its mother-tongue by its own individual experience.

Now, without waiting to show the manifest unfairness of this example—seeing how enormously complex a system of cerebral relations the speaking of even the simplest language implies—it is enough for our present purposes to observe that language has been itself the product of an immensely prolonged and highly elaborate evolution. Although it is true that man has always been a talking animal, it is very far from true that he *has always talked the same language*. As a matter of fact, he has talked in thousands of different languages, and if the genetic history of any one of them could now be traced back to its original birth, the probability is that it would be found to have passed through some hundreds of phases, no one of which would have been fully intelligible to the generations which spoke the others. Consequently, even if we were to adopt the impossible supposition that any length of time could be sufficient to enable heredity to elaborate so huge an amount of instinctive acquisition as would be required to render the knowledge of any language intuitive, there would still remain this answer to Prof. Weismann—namely, that if a child *could* talk by instinct, it would require to astonish its parents by addressing them in at least a hundred unknown tongues, before arriving at the one which alone they could understand.

So much, then, by way of answer to Prof. Weismann's supposed difficulty. But the matter does not end even here; for if he had searched the whole range of human faculties he could scarcely have found a worse example to quote in support of his argument, seeing that it admits of being turned against that argument with the most overwhelming effect. This argument is that the fact of speech not being instinctive is proof that acquired knowledge is not transmitted. Now, we have just seen it to be manifestly impossible that so elaborate, as well as so recent, a body of acquired knowledge should be transmitted—even though it were true that many instincts had been evolved in this way. Nevertheless, it might still be reasonably objected—as, indeed, Weismann says—that the simpler features which serve to characterise all spoken languages alike, and which, therefore, have always constituted the common elements of language as such—it might reasonably be urged that these simpler elements which are thus common to all languages might well be expected by this time to have become instinctive, if there is any truth at all in the Lamarckian doctrine of the inherited effects of continuous function. *But this is exactly what we find.* The only elements that are common to all languages are the simplest elements of *articulation*; and it is now established beyond doubt that the human infant is endowed with the instinct of making articulate sounds. Long before the powers of understanding are sufficiently advanced to admit of the child making any rational use of language, he begins to babble meaningless syllabic utterances. And although these utterances are extremely simple when contrasted with the enormous complexity which they are soon destined to attain in intelligible speech, yet, regarded in themselves, or as merely hereditary endowments, the evolution of mechanism which they represent is by no means contemptible. For they necessitate highly peculiar as well as highly co-ordinated movements of the larynx, tongue, lips, and respiratory muscles, not to speak of the special innervation which all this requires, or the yet more special cerebral conformation which it betokens. In short, the illustration of spoken language, far from making against the doctrine of Lamarck, is one of the best illustrations that can be adduced in its favour; for surely it is in itself a most significant fact that the young of the only talking animal should alone present the instinct of making articulate sounds—just as it also alone presents the instinct of alternately placing one leg before the other, in a manner suited to walking in an erect position.

Upon the whole, then, I conclude that the effects of use and disuse are certainly inherited; that the reducing influences of the latter are largely assisted by the cessation of selection; that the cessation of selection is itself assisted by the economy of growth, which constantly depresses the average size of any useless structure; and that in a comparatively few cases, where changed conditions of life have rendered a previously useful organ actively injurious, the influence of selection may not only be withdrawn, but reversed. And if in justification of these views I were required to adduce any single tests as crucial, I should point on the one hand to the neuter ants, and on the other hand to the bower-birds. For the neuter ants prove to demonstration the fact of developing such important structures as enlarged and strengthened jaws through the agency of selection, and of totally losing such important structures as wings through the cessation of selection—in both cases under circumstances which effectually preclude the possibility of any inherited effects of use and disuse. On the other hand, the bower-birds no less conclusively prove the fact of developing highly elaborate and most remarkable instincts, which are entirely without reference to any life-preserving function, and therefore can be ascribed only to the inherited effects of functionally-acquired peculiarities.

If this paper has been at all successful in its objects, it must have brought into prominence one point which I am particularly anxious to make clear—namely, that it is a precarious thing to differ, in any point of biological doctrine, from the matured judgment of Charles Darwin. The more deeply his work is studied, the more profoundly is the conviction impressed, that even though he did not always give it, he always had a reason for the faith that was in him. Therefore, before his followers venture to question a doctrine which was sanctioned by him, common prudence should dictate a careful pondering of the matter. Some of the readers of NATURE may have been led to suppose that as to this I am myself living in a glass house. For my recent suggestion of an additional "factor of organic evolution" has had the effect of bringing many stones about my head with regard to this very point. But these have mostly been thrown by men who have not taken the trouble to acquaint themselves with the exact nature of Mr. Darwin's final judgment upon the points in question. As a matter of fact, there is only one point upon which I have deviated at all from the latest editions of Mr. Darwin's works—namely, as to the degree in which free intercrossing is inimical to natural selection—and, curiously enough, this is just the point which my critics for the most part disregard. I am blamed for my arrogance in disputing the universally adaptive character of specific distinctions, in affirming the generality of some degree of sterility between species, and so forth; but all these criticisms only serve to exemplify the truth of what I am now saying—namely, that before anyone ventures to write about Darwinism he should take the trouble to ascertain exactly what it was that Darwin thought.

GEORGE J. ROMANES.

## *THE AUGUST METEORS OF* 1887.

THE circumstances attending the recurrence of this celebrated meteoric display were by no means favourable in the present year. On August 10 and 11 the moon rose before 11 p.m., so that during later hours the smaller and more numerous class of meteors, many of which would have been visible on a dark sky-ground, were obliterated. Apart from this, the night of the 11th was much overcast, and comparatively few observations could be secured. But, making every allowance for hindrances of this character, the recent shower has proved itself decidedly inferior to many of the conspicuous returns recorded in previous years.

But if this notable stream has been deficient in numerical strength, it has exhibited some features which, though previously observed, have never been capable of being so definitely and satisfactorily traced in their development as during the present year. I refer to the displacement of the apparent radiant point amongst the stars, and to the visible duration of the shower, both of which form important elements in determining the physical nature of the system and in theoretical investigations as to the perturbations which our earth may have exercised upon it during the frequent *rencontres* with its materials in past ages.

The very clear weather recently experienced enabled the progress of the display to be watched on fourteen nights between July 19 and August 14, and the radiant point on each one was determined separately, as by combining the results of several nights the changes in its position would have been rendered more difficult of detection. I first pointed out this change in the radiant in NATURE, vol. xvi. p. 362, and subsequently further details were published in the *Monthly Notices* for December 1884, pp. 97–98. In NATURE, vol. xxxiv. p. 373, will also be found the observations of this peculiarity made here in 1886, but they were not so complete as during the present year, when the radiant centres were successively derived as under.

*Great Perseid Radiant Point* 1887.

| Night. | Radiant. $\alpha$ | $\delta$ | Meteors. | Night. | Radiant. $\alpha$ | $\delta$ | Meteors. |
|---|---|---|---|---|---|---|---|
| | ° | ° | | | ° | ° | |
| July 19 ... | 19 | + 51 ... | 4 | August 1 ... | 35 | + 56 ... | 4 |
| 22 ... | 25 | + 52 ... | 5 | 6 ... | 42 | + 55 ... | 5 |
| 23 ... | 25 | + 52 ... | 4 | 7 ... | 43 | + 56 ... | 5 |
| 27 ... | 29 | + 54 ... | 5 | 8 ... | 43 | + 56 ... | 6 |
| 28 ... | 30 | + 55 ... | 10 | 10 ... | 42½ | + 57½ ... | 22 |
| 29 ... | 31 | + 51½ ... | 10 | 11 ... | 45 | + 57½ ... | 16 |
| 31 ... | 35 | + 54 ... | 11 | 14 ... | 53 | + 57 ... | 8 |

It will be noticed that these figures do not show a perfectly regular progression of the radiant in the direction of east-north-east. This is, however, entirely owing to observational errors which cannot be wholly eliminated from such determinations. Thus the radiant given above for August 6 is no doubt slightly east, and the one for August 10 slightly west, of the true positions. But these trivial discordances in individual positions do not affect the general result, which shows in the clearest manner possible that there is a rapid advance of the radiant from night to night. From all my observations since 1867, which include several thousands of Perseids, I believe this shower extends over a duration of at least forty days, from July 13 to August 22. The earliest visible meteors of the stream emanate from a point between Cassiopeia and Andromeda, while the latest ones diverge from the space separating Auriga and Camelopardus.

From its first oncoming to the epoch of culmination on the night of August 10 it does not gradually intensify but reaches a somewhat sudden maximum. I have sometimes found these meteors rather scarce on August 6, 7, and 8, and not much exceeding their observed frequency at the end of July. But on August 9 there is a marked increase, and on the following night it is apparent the shower attains its most brilliant effect. As to the displacement of the radiant this seems to be accelerated during the declining stages of the display. In July I find the degrees of right ascension of the shower nearly correspond with the days of the month, the diurnal advance being equivalent to about 1° of R.A., whereas on nights succeeding the maximum the change amounts to 2° of R.A. or even more. This difference in place is so striking that any observer may determine it for himself by watching the region of Perseus at the right epoch and charting, with the utmost accuracy, the directions of such meteors as presumably originate from the Perseid stream. These meteors generally leave streaks which furnish a ready means of fixing the paths with a degree of precision that could not be otherwise attained.

In NATURE for August 4, p. 318, I described my observations up to July 29 last. On July 31 I recorded 42 meteors in a watch of 3¼ hours, but the moonlight interfered considerably with the work, as it also did on following nights. The Perseids formed one-fourth of the visible meteors on July 31. I saw 25 meteors on August 1 in 3¼ hours, but the Perseid display was only just recognizable. At 12h. 18m. I observed a splendid fireball passing somewhat slowly from $338° + 43°$ to $164° + 70°$. It left a bright streak or thick train in the latter part of its course, and it was evidently a member of the July Aquariads. At first it was scarcely brighter than a third magnitude star, but when near Polaris it became very brilliant, and afterwards lit up the northern sky with a flash much stronger than the moonlight. I saw 7 other Aquariads on the same night.

On August 6 observations were continued, and 28 meteors were seen in 4¼ hours. Besides the usual shower of Perseids I was much interested in finding a companion radiant at $31° + 49°$, which was very sharply defined. I observed a shower on August 11–13, 1880, from $30° + 46°$ which may be the same; and there is a great probability that this system is connected with Comet I. 1870, which passed near the earth's orbit and would give a radiant near that of the meteor shower and at the same epoch.

On August 7, 23 meteors were seen in 2¾ hours. Only 5 Perseids were recorded. On August 8, 14 meteors were seen in 2¼ hours during moonlight, and of these one appearing at 10h. 34m. was as bright as Jupiter. Its course was from $6° + 67\frac{1}{2}°$ to $302° + 60\frac{1}{2}°$, and it left a bright streak. At 11h. 28m. a fireball was seen moving rather swiftly from $349° + 15°$ to $9° + 14\frac{1}{2}°$, so that its path was one of 20° just above $\gamma$ Pegasi. At its end point the meteor burst out with a great accession to its brilliancy, and there was a vivid flash, though the moon was near. The radiant of this fine meteor was probably near Delphinus at $304° + 11°$.

On August 10, before midnight, the Perseids were by no means numerous. Only 22 were seen during 1¼h., and after the moon rose the display was not critically watched, as observations made during moonlight are not comparable with those obtained under more favourable conditions. There were fine meteors now and then, but the phenomenon never developed into an imposing shower. On August 11 the sky was much overcast, and not many shooting-stars were discerned. In 1 hour before 11h. 30m., when the firmament was fairly clear, I counted 21 meteors, of which 16 were Perseids. On August 14 the weather became very fine, and I enumerated 45 meteors in a 4½ hours' watch. There were only 8 Perseids, and amongst the meteors I registered were about 5 Aquariads from the same radiant as at the end of July. I also noticed the Aquariad shower at the middle of August in 1877, and in 1879 on August 21, 14 meteors were traced from $339° - 10°$, so that it would appear this system is prolonged until the end of the third week in August, and without any apparent displacement of the radiant point. The members of the latter stream are widely dissimilar in their visible aspect to the Perseids, and move slowly, often covering considerable arcs before extinction. In its chief richness the shower belongs to the July meteoric epoch, though sometimes, as in the present year, remaining conspicuous until the middle of August or even later than that, as in 1879.

Bristol. W. F. DENNING.

## SOCIETIES AND ACADEMIES.

### PARIS.

**Academy of Sciences**, August 16.—M. Janssen in the chair.—Note on the work recently carried out at the Observatory of Meudon, by M. J. Janssen. Special reference is made to the many successful solar photographs already obtained, representing the history of the solar disk for the last ten years. The processes are now so perfected that on the same plate the details are taken both of the brighter and less luminous parts, such as the edge of the disk and the penumbræ of the spots. Photographs ten times enlarged were exhibited of the extremely interesting spots taken on June 22, 1885, and last June. The striæ of the penumbra and the faculæ surrounding the former consist of granulations, in form and size resembling those constituting the entire solar surface. The same phenomenon reappears on the large round spot photographed last July, so that it seems all but demonstrated that the whole solar disk has a uniform constitution, and that the so-called granulations are in fact the constituent elements of every part of the surface of the sun.—Fresh researches on the relations existing between the chemical and mechanical work of the muscular tissue (continued), by M. A. Chauveau, with the co-operation of M. Kaufmann. Here a determination is made of the coefficient of the quantity of mechanical work produced by the muscles performing useful work in the physiological conditions of the normal state. By translating into absolute measurements the indications furnished by the dynamograph already referred to, it is shown that the muscular work performed may be estimated at about 31 to 35 millionths of calorie.—Some further remarks on the radicular nature of the stolons in Nephrolepis, by M. A. Trécul. In reply to M. Lachmann's recent note, the author again shows that these stolons are not stems or stalks, but true roots. No matter what their length, they never produce leaves, have always the structure of roots, and as they alone represent the primary roots of Nephrolepis, the expression "radicular stolons," applied to them by the author, is fully justified.—New fluorescences with well-defined spectral rays (continued), by M. Lecoq de Boisbaudran. The author here treats fully the combination of alumina and the earth $Z\beta_2O_3$, which, without being pure, is very rich in $Z\beta$ and poor in $Z\alpha$. Alumina with 1/50 of this earth heated with sulphuric acid and moderately calcined shows a somewhat yellowish-green fluorescence, much more vivid than that of alumina containing the same quantity of $Z\alpha_2O_3$ impure. The fluorescences have also been examined of calcined alumina containing the oxides of Ce, La, Er, Tu, Dy, Yb, Gd, Yt, and U. During these researches several rays were noticed apparently belonging to none of the already determined elementary bodies. Some of these rays may perhaps correspond to the substances announced by Mr. Crookes; but each case will have to be determined for itself.—Determination of the longitude of the Observatory of Tacubaya, Mexico, by MM. Anguiano and Pritchett. Continuous observations spread over six months show a definite longitude of 6h. 36m. 46·56s. west of Greenwich, which will require a correction of close upon 5s. for the accepted longitude of the capital of Mexico.—Electric excitement of the liver, by MM. Gréhant and Mislawsky. The question is discussed, whether the excitement of the liver by electricity increases the quantity of urea contained in the blood. In opposition to the views of M. Stolnikow the experiments here described show that variations in quantity occur only in the arterial blood, and that the blood of the supra-hepatic veins presents no change in the weight of the urea after electric excitement of the liver.—Dissemination of the Bacillus of tuberculosis by flies, by MM. Spilmann and Haushalter. Observations recently made in consumptive-hospitals seem to show that the virus (Koch's Bacillus) may easily be disseminated by the house-fly.—Note on Hæmatocytes, by M. Fokker. The author recently showed that the protoplasm taken from a healthy animal and protected from microbes survives and may produce fermentations. Here he continues his researches, showing that this protoplasm is capable of generating a vegetable form different from that under which it existed in the animal organism. But the Hæmatocytes thus produced do not multiply themselves in a cultivating medium, and their development should perhaps be described as a case of heterogeny.

## BOOKS, PAMPHLETS, and SERIALS RECEIVED.

Dijmphna—Togtets Zoologisck—Botaniske Udbytte: Dr. Chr. Fr. Lütken (Kjobenhavn).—Seven, the Sacred Number: R. Samuell (K. Paul).—University College, Dundee, Calendar 1887-88 (Leng, Dundee).—Qualitative Chemical Analysis: Dr. C. R. Fresinius, 10th edition, translated by C. E. Groves (Churchill).—Notes to accompany a Geological Map of the Northern Portion of the Dominion of Canada: G. M. Dawson (Montreal).—Die Geoiddeformationen der Eiszeit: E. von Drygalski (Berlin).—Proceedings of the Linnean Society of New South Wales, 2nd series, vol. ii. Part 1 (Cunninghame, Sydney).—Verhandlungen der Naturhistorischen Vereines, Fünfte Folge, 4 Jahrgang, Erste Hälfte (Max Cohen, Bonn).

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