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Supplement to Nature,
May 30, 1895

Nature

A WEEKLY

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*"To the solid ground
Of Nature trusts the mind which builds for aye."*—WORDSWORTH

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INDEX

NATURE:

FIFTY-FIRST VOLUME.

L'ENVOI.

THE completion of a period of twenty-five years, and the publication of fifty volumes since NATURE was established, mark an epoch in its history. The Editor is anxious to take advantage of it to tender his best thanks to those who have, from the commencement of the Journal to the present time, not only contributed to its pages, but have so freely permitted him to appeal to them for advice and assistance.

He feels strongly that it is only owing to their aid and their careful pilotage that NATURE has so far escaped that shipwreck which has been the lot of somewhat similar ventures, not only in this country, but in Europe and America.

It would have been appropriate, if it had been possible, to have included in this, the first number of the fifty-first volume, a retrospect of the scientific progress achieved during the last quarter of a century. It was, however, plainly impracticable in such a limited space to give a just idea of the various triumphs which have been accomplished along the many lines of scientific thought and work.

But no elaborate retrospect is needed to prove that since 1869 our scientific progress has been at a rate which has never been surpassed in the world's history. Men and ideas have increased ten-fold; instruments and applications have increased a hundred-fold. The battle of scientific education has been fought and won, and the general interest in matters scientific is greatly increasing. Not only are these things so, but there is every indication that when L'ENVOI to the hundred and first volume comes to be written—by some other hand—a still more rapid progress will have to be indicated.

That the same distinguished man of science who wrote the first article in NATURE in 1869 has been good enough to start the fifty-first volume, will doubtless be as great a source of pleasure to the readers of NATURE as it is a source of pride to

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Schorlemmer, Prof. C., F.R.S.
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Scott, R. H., F.R.S.
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Seeley, Prof. H. G., F.R.S.
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Shore, Dr. E. A.
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Traquair, Prof. R. H., F.R.S.
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Wrightson, Prof. J.
Wynne, Dr. W. P.

Yearsley, P. M.
Yeo, Dr. J. B.
Young, Sir George.
Young, Dr. Sydney, F.R.S.
Young, Prof. C. A. (Princetown, New Jersey, U.S.A.).

NATURE

A WEEKLY ILLUSTRATED JOURNAL OF SCIENCE.

"To the solid ground
Of Nature trusts the mind which builds for aye."—WORDSWORTH.

THURSDAY, NOVEMBER 1, 1894.

PAST AND PRESENT.

JUST five-and-twenty years ago, the Editor of NATURE did me the honour to request that I would write the leading article for his first number. In complying with my friend's wish, I said that I could think of no more appropriate preface to a journal, the aim of which was "to mirror that fashioning by nature of a picture of herself in the mind of man," which is called science, than an English version of the wonderful rhapsody *Die Natur*, which is to be found among Goethe's works,[1] and which had been a source of instruction and delight to me from my youth up.

Whether my estimate of the fitness of these pregnant aphorisms for the place assigned to them was shared by more than half-a-dozen of the readers to whom they were submitted, is very doubtful; indeed, I feel bound to confess that a rumour reached my ears, to the effect that some authorities, apparently of the school of the most noble Festus, in their haste, failing to discriminate between the great poet and his translator, opined that much attempt to learn, if not much learning, had made me mad.

A verdict based on a mistake so flattering to any literary vanity I might possess could be borne with equanimity. Indeed, in view of the general state of opinion among those interested in physical science at the time, I had no right to imagine that a presentation of a theory of the universe based exclusively upon the scientific study of nature—a prose poem, which stands in somewhat the same relation to the philosophy of Spinoza as the "Essay on Man" to that of Shaftesbury and Bolingbroke—would be intelligible to more than a small minority; or acceptable to more than a fraction of even that fit though few company.

At that time, it was rare for even the most deservedly eminent of the workers in science to look much beyond the limits of the specialty to which they were devoted; rarer still to meet with any one who had calmly and clearly thought out the consequences of the application, in all the regions into which the intellect can penetrate, of that scientific organon, the power and fruitfulness of which, within their particular departments, were so obvious. Though few read, and fewer still tried to comprehend the writings of Francis Bacon, a respectable, almost venerable, tradition bid us glorify him as the guide, philosopher, and friend of science; and more especially held him up as our exemplar in his insistence upon the division of the world of thought into two—an old and a new—but, unlike the corresponding divisions of the terrestrial surface, separated by impassable barriers. In the new, the strict adherence to scientific method was inculcated, and a rich reward of benefits to man's estate promised to the faithful; in the old, on the contrary, scientific method was to be anathematised, while absolute dependence was to be placed on quite other mental processes. Men were called upon to be citizens of two states, in which mutually unintelligible languages were spoken and mutually incompatible laws were enforced; and th y were to be equally loyal to both.

People engaged in the ordinary business of life were not much troubled by difficulties which were not forced upon them by their avocations. Nor, among the men of science, did they press hardly on the mathematicians, the physicists, and the chemists.

[1] A better translation than mine and an interesting account of the very curious obscurity which hangs about the parentage of *Die Natur* are to be found in Mr. J. Bailey Saunders' recently published "Goethe's Aphorisms and Reflections."

At one time, the astronomers underwent sundry perturbations, yet these somehow got smoothed over and ignored. But there was serious trouble among the geologists and biologists. However sincerely they might try to shut their eyes, it was impossible to be wholly blind to the fact that for them the two worlds were not separable. On the contrary, it was becoming plainer and plainer that a vast tract, hitherto claimed for the old, was being steadily invaded and annexed by the citizens of the new world.

Fifty years ago the tension was already serious, but matters had not got so far as to seem desperate. It was possible for very eminent and, at the same time, perfectly sincere men, to keep their scientific and their other convictions in two separate logic-tight compartments. Indeed, it was said that some, perhaps too deeply bent on the search after final causes, found a reason for the duplicity of the cerebral hemispheres, in their adaptation to the purposes of this duplex intellection. Conducive to outward and inward peace as might be the convention, in virtue of which science was to be kept grinding at the mill of utility, and (by way of completing the resemblance to Samson) carefully blinded, or at any rate hoodwinked lest glimpses of a nobler field of action should end in an outbreak on the Philistines, the difficulty of observing it, as uniformitarian principles obtained the ascendant among the geologists, became insuperable. Outside the narrow circle of the peace-at-any-price "reconcilers," the *pax Baconiana* was plainly coming to an end in the middle of the century. It was finally abolished by the publication of the "Origin of Species."

The essence of this great work may be stated summarily thus: it affirms the mutability of species and the descent of living forms, separated by differences of more than varietal value, from one stock. That is to say, it propounds the doctrine of evolution as far as biology is concerned. So far, there is nothing new in Darwin's enterprise. So far, we have merely a re-statement of a doctrine which, in its most general form, is as old as scientific speculation. So far, we have the two theses which were declared to be scientifically absurd and theologically damnable by the Bishop of Oxford at the meeting of the British Association at Oxford in 1860.

It is also of these two fundamental doctrines that, at the meeting of the British Association in 1894, the Chancellor of the University of Oxford spoke as follows:—

> "Another lasting and unquestioned effect has resulted from Darwin's work. He has, as a matter of fact, disposed of the doctrine of the immutability of species."

And

> "Few now are found to doubt that animals separated by differences far exceeding those that distinguish what we know as species have yet descended from common ancestors."

Undoubtedly, every one conversant with the state of biological science is aware that general opinion has long had good reason for making the *volte face* thus indicated. It is also mere justice to Darwin to say that this "lasting and unquestioned" revolution is, in a very real sense, his work. And yet it is also true that, if all the conceptions promulgated in the "Origin of Species" which are peculiarly Darwinian were swept away, the theory of the evolution of animals and plants would not be in the slightest degree shaken.

Ever since I began to think over these matters it has been clear to me that the question whether the forms of life on the globe have come about by evolution, or in some other way, is an historical problem, and must be treated as such. Either there are records of the process, or there are not. If there are not, we are shut up to the devising of more or less probable hypotheses based on indirect evidence. If there are adequate records, our business is to decipher them, and abide by what they tell us. Now, in 1859, there was no doubt about the existence of records; nor any about the fact that they extended over a vast period of time; nor any about the order of succession of the facts they registered. But, there was also no doubt in the mind of any one who looked critically into these records, that, in spite of their seeming copiousness, they were the merest fragments, torn and tattered remnants of the continuous series of documents which once existed. But, very largely in consequence of the stimulus given by Darwin, palæontological research was taken up with new vigour, and with marvellous success. So that, in 1878, I felt justified in writing—

> "On the evidence of palæontology, the evolution of many existing forms of animal life from their predecessors is no longer an hypothesis but an historical fact."[2]

And in 1880—

> "If the doctrine of evolution had not existed, palæontologists must have invented it, so irresistibly is it forced upon the mind by the study of the remains of the Tertiary Mammalia which have been brought to light since 1859."[3]

I am not aware that these statements have ever been controverted; and, in view of the following deliverances of the author of the most authoritative recent treatise on Palæontology, I think they are not likely to be:

[1] British Association for the Advancement of Science, Oxford, 1894. Address of the Most Hon. the Marquis of Salisbury, President.
[2] "Collected Essays," vol. ii. p. 226. [3] *Ibid.*, p. 241.

"Recent investigations have utterly shattered the belief in cataclysms. The conviction has arisen that the process of the development and metamorphosis of organic beings was gradual and uninterrupted, and that sharp lines of demarcation are to be found only where considerable changes in the conditions of existence, and especially in the distribution of land and water, have brought about great modifications in the world of life or interruptions in the formation of sediment." (Zittel: "Handbuch der Palæontologie." Bd. I. p. 23.)

And, again, in the recently completed final volume of this standard work we read:

"The whole history of the evolution of the mammalia from the Trias to the present day, in spite of all deficiencies in the record, plainly shows that the genetic connection of the several Faunæ, whatever geological disturbances may have taken place, was never completely interrupted; and that each of these associations of animals has arisen by gradual transformation of the constituents of its predecessor, and has furnished the stock of its successor." (Bd. IV. p. 764.)

However often, therefore, thoughtlessness, or polemical dexterity, may confuse issues which are totally distinct, biological evolution rests, in perfect security, on the firm foundation afforded by the study of the remains of the animals and plants, which have successively peopled the world during the untold ages of its past history. The coming into being of the present forms of life has happened so, and in no other way.

And, as I pointed out, sixteen years ago,

"It is only the nature of the physiological factors to which that evolution is due which is still open to discussion."[1]

For me, the claim of the doctrine of evolution to be taken into account in all philosophical and other views of the nature of things turns upon whether it possesses a solid foundation in fact or is a mere speculation. No doubt, whenever astronomers universally accept what is called the Kant-Laplace theory of the heavens, a notable addition will be made to this indispensable objective foundation of the doctrine. Whenever chemists accept the evolution of the so-called elements from a *materia prima*, there will be a further grand addition. But, for the present, I venture to suppose that the palæontological base is surest. And, at any rate, so far as the claims of science to be heard in regard to the problems of human life are concerned, it is, far and away, the most important. If man has come into existence by the same process of evolution as other animals; if his history, hitherto, is that of a gradual progress to a higher thought and a larger power over things; if that history is essentially natural; the frontiers of the new world, within which scientific method is supreme, will receive such a remarkable extension as to leave little but cloudland for its rival.

[1] "Collected Essays," vol. ii. p. 226.

Experience teaches me it is by no means impossible that if I were to stop here, what I have said would be represented, and even believed, to be a repudiation of "Darwinism." Yet no conclusion could be more utterly devoid of foundation.

"The combined investigations of another twenty years may, perhaps, enable naturalists to say whether the modifying causes and the selective power, which Mr. Darwin has satisfactorily shown to exist in nature, are competent to produce all the effects he ascribed to them; or whether, on the other hand, he has been led to over-estimate the value of the principle of natural selection, as greatly as Lamarck over-estimated his *vera causa* of modification by exercise."

. . . . "'My sons dig in the vineyard,' were the last words of the old man in the fable; and though the sons found no treasure they made their fortune by the grapes.'"

These two paragraphs occur at the end of the critical notice of the "Origin of Species," which I wrote in 1859. The citations I have already given from Zittel sufficiently show what has come of "digging in the vineyard"; there is another (Bd. I. p. 42) much to the present purpose.

"For the naturalist, evolution (*die Descendenz theorie*) offers the only natural solution of the problem of the development and succession of organic beings. But as to the causes which bring about the modification of species, and especially the change in a given direction, opinions are yet greatly divided. That the principle of natural selection, discovered by Darwin, leaves many phenomena unexplained is no longer denied by even the warmest followers of Darwin."

It will be observed that at any rate one of these "warmest followers" has never thought of denying it. On the contrary, he has over and over again brought the difficulties prominently forward. Nevertheless, I doubt as little, now as heretofore, that the probabilities are greatly in favour of our finding a way to the causes of evolution by pertinacious study of variation and natural selection. There are large fields for inquiry open on all sides. How much has yet been done, for example, towards ascertaining the effect of external conditions on the struggle for existence within the organism and the production of varieties as a consequence of that struggle; or towards an adequate experimental study of variation? The supposition that problems such as these, and others that might easily be mentioned, could be finally solved, even in thirty-five years, is one that would not enter the mind of a competent biologist; and the parade of the mutual contradictions and the intrinsic weaknesses of the hypotheses which, hitherto, have been more or less tentatively propounded, as if they had anything to do with the truth or falsehood of the doctrine of evolution, should not be taken too seriously.

T. H. HUXLEY.

ECONOMIC PRODUCTS OF INDIA.

A Dictionary of the Economic Products of India. By George Watt, M.B., C.M., C.I.E., assisted by numerous contributors. In six volumes (vol. vi. in four parts), 1889-1893. Published under the authority of the Government of India, Department of Revenue and Agriculture. (Calcutta: Superintendent of Government Printing.)

THE completion of this important work adds enormously to the facilities previously existing for acquiring a knowledge of the vegetable, mineral, and animal products of our Indian empire. The object of the dictionary is to give a complete account of all Indian products that have been in any way utilised by man, however small or trivial the use of them may have been. As a large majority of the products are of vegetable origin, the appointment of a botanist as editor and principal author was essential, and the Government of India may be fairly congratulated on the result.

In the preface to the first volume the author states that he has had "to keep in view a two-fold purpose; viz. on the one hand to supply scientific information which may be useful to the administrative officer, and on the other to meet the requirements of the reader in search of definite information regarding Indian economics." Whether another purpose was to produce a large book is not stated, but no feature of the work is more conspicuous than that it consists of nine bulky and rather closely-printed volumes.

In general plan the dictionary consists of long and elaborate accounts of the more important articles of commerce produced in India, such as various grains, dyes, oils, tea, cotton, sugar, indigo, wool, silk, &c., and shorter notices of less valuable products. As a general rule each vegetable product is described under the scientific name of the plant from which it is obtained, cross references being supplied where necessary under English and Indian terms, and also under general headings, such as "Oils and Fats," "Timbers," &c. Animal and mineral products are described under various headings. A general index is promised, and is needed, for many important English and vernacular terms are not found in their places. For instance, silk-cotton, semul, jowári and juári, bájra, cholum, cumboo, charas, ganja, ngápi, civet (and Viverra) may be searched for in vain. Tasar is inserted, but there is no reference to tusser or tussah, the common spelling, in the place where these words would come in alphabetical sequence. The article on isinglass is slightly out of its proper place, and that on sharks' fins and fish maws, to which reference is made under both isinglass and fish, appears to have been omitted.

Each article, in the case of vegetable products, consists of the scientific name of the plant, with references, the English name, if one exists, and a list of vernacular names, followed by full references to the works, scientific or economic, in which the plant or its products have been described. Then follow paragraphs on the habitat, history and useful products, such as dyes, fibres, oils, gums, &c., each under a separate heading. Sometimes a paragraph explains the chemical composition. Under additional headings are related the uses to which the plant or its products are put in medicine, food, or the arts, and to the paragraph on medicine another is frequently added with "special opinions" by various medical officers. In the case of trees the structure of the wood receives special notice. The last paragraph of each article describes the "domestic and sacred uses." With mineral and animal products the plan is similar, but details are given, as a rule, under general headings, such as horns, skins, wool, iron, &c. In the first volume a botanical diagnosis of each plant is generally added, but not in the later volumes. A number for each separate product or use is inserted in the margin to facilitate reference, the numbers commencing afresh under each letter of the alphabet.

The longer articles contain full descriptions of cultivation, trade, manufactures, and other important subjects.

Whilst the bulk of the work is by Mr. Watt, many articles have been contributed, partly or wholly, by other writers. The list of contributors affixed to the first volume refers only to that volume, or to that and the second, for in the prefaces to the third and subsequent volumes several additional names are mentioned, one of them, Dr. J. Murray, being that of the author of several important articles. All of the principal contributors, except Mr. Watt (and he holds the degree of M.B.), belong to the Indian Medical Service, so that it is not surprising to find a very large space devoted to drugs and therapeutics. In fact, the whole work might fairly be termed a dictionary of economic products and materia medica. Many of the plants catalogued are apparently included solely because of some medicinal or supposed medicinal use, frequently by ignorant people. For example, on the last two pages of the work, *Zornia diphylla* is introduced on account of its use thus quoted under the head of "Medicine": "The root is given, along with that of *Bhadar jhapni*, to induce sleep in children. These plants shutting up their leaves at night have probably suggested the idea to the Ojhas." The quotation is from a report by Mr. Campbell on the "Economic Products of Chutia Nagpur." No reference can be found to *Bhadar jhapni* in its place in the dictionary.

The "special opinions," quoted from various medical writers on therapeutics, are of a miscellaneous nature, and no distinction is drawn between notices of the purposes for which drugs are used by competent physicians, and those of occasions on which they are prescribed by ignorant hakims or superstitious herbalists. All this part of the work might have been omitted with advantage; however useful such opinions might be in a special work on drugs and therapeutics, the details are out of place in a description of economic products. One instance may be quoted. Under the head of "Diamond," the following occurs: "*Medicine.*—Diamond dust is known to be a powerful mechanical poison. In Hindu practice it is, however, to some extent used as a drug" (just as gold, silver, pearls, and other precious substances are regarded by unscientific races as possessing great medicinal virtues). The whole extract is too long to quote, but the "special opinion" runs thus: "Employed as a poison, it is administered in the shape of dust, as in the late celebrated case when the Resident of Baroda, Sir Arthur Phayre, nearly lost his life." The Resident of

Baroda was Sir Robert Phayre, not Sir Arthur, the well-known Chief Commissioner of Burma, and the risk to life was due to an important fact which the distinguished surgeon, whose name is appended to the quotation, must have forgotten, the admixture of arsenic with the diamond dust. The latter is simply a mechanical irritant like quartz sand, or powdered glass, and to term any of these a powerful mechanical poison is to use a stronger term than is quite accurate.

The devotion of a large space to therapeutics is not the only instance in which the bulk of the work is increased by the discussion at length of matters that have but slight connection with the main subjects of the dictionary, as specified by the author. Perhaps the utilisation of several pages under *Triticum sativum* (wheat) in the discussion of the depreciation of silver, may be thought essential, but it is not clear why, under *Papaver somniferum*, long extracts should be quoted from various dispatches to illustrate the attitude of the Government of India on the Chinese opium question, nor what bearing on the economic products of India the seven pages can have, under *Vitis vinifera*, that are taken up with the history of the vine and of wine. Amongst the longer articles, 63 pages are given to tea (in addition to 19 on *Camellia theifera*), 77 to tobacco, 87 to indigo, 88 to opium, 123 to sheep, goats, wool and woollen manufactures, 152 to rice, 174 to cotton, 238 to silk and silk manufactures, and 380 to the sugar-cane and sugar. Almost every one of these articles would require a separate notice for adequate discussion. For many purposes a condensed account of the history, production, manufacture, and trade in each case would be more generally useful as well as more interesting; but, on the other hand, it is extremely difficult for the author of a work like the present to select only those data that are useful, and no editor can be expected to possess the special knowledge of every separate subject that will enable him to do justice to it, and to avoid mistakes.

In the preface to the first volume it is stated that economic products which belong to the animal and mineral kingdoms have been but very imperfectly touched upon. This plan, however, appears to have been modified subsequently, since silk and wool, as already noticed, form the subjects of two of the longest articles. The accounts of minerals have, for the most part, been written by officers of the Geological Survey, or copied from the Survey publications. It is questionable, however, if any geologist can have written the following passage under the heading of iron: "*Clay ironstone* exists in large deposits in many coal-measures, and in this situation is known as *black band*." It is, of course, only one variety that is known by this name.

No parts of the dictionary, however, stand more in need of scientific revision than those relating to vertebrate animals. A few instances will show this. A short article on "Pheasants, Jungle-fowl, Partridges, &c.," commences thus: "The pheasant families of birds, *Phasianidæ*, *Megapodidæ*, and *Gallinæ* (*sic*) comprise the pea-fowl, pheasant, jungle-fowl, and spur-fowl, while the partridge family, *Tetraonidæ*, includes the partridge, snow-cock, and certain forms of quail." A list follows in which the genera *Pavo*, *Argusianus*, and *Polyplectron* only are included in the family *Phasianidæ*, all other pheasants, amongst them the typical genus *Phasianus*, are placed with *Megapodius nicobariensis* in the family *Megapodidæ*, and the so-called family *Gallinæ* (which is really a sub-family of *Phasianidæ*) contains *Gallus* and *Galloperdix*. These mistakes are apparently copied from Murray's "Avifauna of British India." In the article "Oxen," the wild and tame yaks are rightly classed as one species, but the tame buffalo is separated, under the name of *Bubalus bos*, from the wild race, or *B. arni*. No reason is assigned for a distinction that is quite opposed to the views of all modern writers on mammalia, nor is any authority given for the name adopted, which is simply the old Linnæan name *Bos bubalus* reversed.

The omission of any notice of ngápi in its proper place has already been mentioned. This curious compound of partially decomposed fish with salt is a most important article of food in Burma, where it may be said to replace butter and cheese amongst a people, who, like the Chinese, hold milk and all substances obtained from it in abhorrence. The manufacture of ngápi is on a very large scale, and the trade in the article is extensive, yet apparently the only notice of the mode of preparation that occurs in the dictionary ("Fish," vol. iii. p. 367) is apparently erroneous, and certainly does not apply to one of the ordinary processes. Three or four different kinds of ngápi are mentioned in the *Burma Gazetteer*, and their manufacture described; all the processes are radically distinct from that briefly quoted in the dictionary, whilst no information is given in the latter as to the trade in the article or its value, except what may be inferred from the fact that the revenue from Burmese Fisheries in 1883 was twelve to thirteen lakhs of rupees.

In the article on sheep and goats, and in some others, the authorities for scientific names are quoted on the botanical, not on the zoological system, and it is rather strange to find the nilgai called *Boselaphus tragocamelus*, W. Sclater (instead of Pallas), and the Tibetan gazelle, *Gazella picticaudata*, Brooke (instead of Hodgson). The mutton of the dumba, or fat-tailed sheep, is said to be very coarse, whereas it is the best mutton in Asia. The common Indian story, repeated in the dictionary, under "Camel's milk," that the sweetmeat halwa, brought from the Persian Gulf, is composed of camel's milk and honey (vol. ii. p. 64), is a mistake. Errors like the two last (others might be quoted) are liable to occur in a work like the present, but the number of mistakes of various kinds in the articles on animals and animal products appears to be rather large. It is not easy to understand why, in an important Government work like the present, the aid of competent zoologists could not be obtained to revise the proofs.

A serious mistake may be pointed out in an article on the yeast plant, described under the somewhat pedantic heading of *Cerevisiæ fermentum*. Yeast, it is said, "lives and increases in the fermenting liquor, but appears to abstract nothing from it." This mistake may however have been noticed, for in a later article, on "Malt liquors," a correct account of the growth and nutrition of yeast is quoted.

One presumably Indian economic product, paper, can scarcely be said to be favourably represented by the material used in the present work, nor are the type and printing, especially in the later volumes, the best ever produced in India. Misprints are numerous. One is amusing: the Tibetan antelope is credited with no less than ten horns. It is to be hoped that commentators on the Apocalypse will not be led to believe that a ten-horned beast really inhabits Tibet.

On the whole, whilst in case a second edition is required, careful revision is desirable, which might in some cases take the form of abridgement and the omission of irrelevant matter, the principal feature of the work is the large amount of energy that has been expended in its preparation, and the great effort that has been made to bring together information from all quarters. To write a complete account of the products of India, and to give a full scientific and economic description, both of the products themselves and of the sources from which they are derived, are tasks far beyond the powers of any single individual, and that could only be thoroughly carried out by a committee of specialists. W. T. B.

CHINESE AND JAPANESE BUTTERFLIES.

Butterflies from China, Japan, and Corea. By John Henry Leech, B.A., F.L.S., F.Z.S., F.E.S., &c. 4to. With forty-three coloured plates. (London: R. H. Porter, 1892–1894.)

UNTIL within the last few years, almost nothing was known of the Palæarctic fauna, except that of Europe and the Mediterranean sub-region, and though butterflies are the most attractive and the easiest collected of all insects, those with which we were acquainted from Siberia, the greater part of China, and Japan, might almost have been counted on the fingers.

Since then, however, great progress has been made. In the first place, Russian exploration and consolidation have opened up vast regions of previously almost unknown parts of Asia to science, and the work begun on the Amur and in Turkestan by Schrenck and Fedchenko, has been worthily continued by the Grand Duke Nicholas Mikhailovitch and his coadjutors, among whom the brothers Groum-Grshimaïlo deserve the place of honour. When shall we see one of our own Royal Princes bringing out a work on the insects of one of our own colonies to compare with Romanoff's "Mémoires sur les Lépidoptères?" In Western Europe, such work is left to private enterprise.

The French Jesuit missionaries, especially the Abbé David, have penetrated to such out-of-the-way parts of China, as Mou-pin, and have brought back large collections of different kinds, including many very remarkable butterflies, which have been illustrated by Oberthür in his "Études d'Entomologie."

Since the time when China and Japan were thrown open to Europeans, English entomologists have not been behindhand in the work of collection and description. The fine collections formed in Japan by Lewis, Pryer, and Maries have been worked out so well by Dr. Butler and others, that the Lepidopteræ of Japan are now more thoroughly known than those of any other part of Asia except British India. The late Mr. W. B. Pryer published a work on the butterflies of Japan, in the country itself, in English and Japanese, with coloured plates of all the species known to him; but as this book is scarce, and the letterpress very meagre, we are glad that Mr. Leech has included Japan in the important work which forms the subject of the present article.

Mr. Leech commenced his entomological career by the publication of a useful little volume on British *Pyralidæ*, and by collecting excursions to the Canary Islands, Brazil, &c. Subsequently he became interested in the fauna of Eastern Asia, and devoted eight years to its study, and to the accumulation of materials for the present work, not only by employing experienced collectors like Pratt and Kricheldorf to explore the interior of China, but by personally visiting and forming large collections in the Himalayas, Corea, and Japan; in Japan, indeed, he succeeded in capturing almost every species of butterfly known to inhabit the country. By this means, he gradually accumulated the fine collections on which he has based his great work, in which he has been able fully to describe 650 species, a large proportion of which are figured in the forty-three excellent coloured plates which accompany it. We have also a map, and five plates of scenery (four of Western China and one of Japan), the second of which exhibits a side-view of the tremendous and almost perpendicular face of the mountain of Omei-Shan, in the neighbourhood of which Mr. Pratt obtained many of the most beautiful and interesting butterflies which he discovered.

The usefulness of the work is increased by an interesting introduction, dealing with the literature of the subject, the countries visited by the author and his collectors, and a table of geographical distribution, divided into the following columns: Japan, China, Corea, Amurland, Himalayas, Thibet, Europe, and "other countries and regions."

The author remarks in his preface: "It is a matter of regret that, owing to an almost complete absence of information respecting habits and life-histories of the majority of the species, the work is unfortunately less complete than the author could have wished." All honour to him for saying so. It is the duty of every entomologist to seek for and record everything of the kind which he can obtain; but entomologists are sometimes too much disposed to care only for the specimens they receive, and it would not occur to them to encourage their collectors, as they easily might do, to record anything more than dates and localities. Mr. Leech, however, seems both to have sought for and utilised such information, so far as it was accessible or obtainable.

On examining Mr. Leech's 650 species of butterflies, which are distributed among rather more than 150 genera, it becomes apparent that they are to a large extent mainly an amplification of the European fauna. About 300 species are found in Europe proper, divided into about 50 genera, of which only about 9 genera, each including but one, or at most two or three, species of very limited range, are not represented in Mr. Leech's work. These are *Triphysa*, *Nemeobius*, *Aurotis*, *Thestor*, *Zegris*, *Doritis*, *Spilothyrus*, *Thymelicus*, and *Cyclopides*; and there is no reason why some, even of these, should not extend to Western China. In China the European and Indian faunas meet and mingle; thus in the *Satyrinæ*, the Mountain Brown butterflies of the genus

Erebia are far more sparingly represented in China than in Europe; but *Lethe* is far better represented in China than in India, and *Ypthima* at least as well. The tropical subfamilies *Morphinæ* and *Acræinæ* are also represented in China, the first by four species, one of which, *Stichophthalma howqua*, is as large and handsome as a South American *Morpho*, and the other by one of the two Indian species, *Pareba vesta*, which extends its range to several parts of South-Western China. In Japan and the extreme east of China, we find one or two species belonging to peculiarly Nearctic forms, such as *Anthocharis scolymus*, for example.

In certain large genera, such as *Zephyrus*, *Thecla*, and *Papilio*, the number of Chinese species far exceeds those known to occur in Europe; but in the case of *Papilio*, at least, this is mainly due to the large number of properly Indian species which extend their range to China.

It is among the *Papilionidæ* and *Pieridæ* that we find some of the most interesting of the Chinese and Central Asian forms, especially those allied to *Parnassius*, *Aporia*, and *Colias*. There are only about thirteen well-marked genera of *Papilionidæ*, except the heterogeneous genus *Papilio* itself; and eight of these are represented in Mr. Leech's district, the other five being *Hypermnestra* (South-West Asia), *Eurycus* (Australia), *Euryades* (South America), *Thais* (South Europe), and *Bhutanitis* (Bootan). The headquarters of *Parnassius*, however, are perhaps in the mountainous districts rather beyond the range of the present work, as Mr. Leech enumerates only eight species, which seems to us to be rather a small number. Many curious genera allied to *Aporia* are also found in the south-western districts of China bordering on Thibet, such as *Mesapia*, *Davidina*, &c., most of which bear a general resemblance to our Green-veined White (*Pieris napi*). Of these, Oberthür's genus *Davidina* is the most curious, as the wing-cells are divided by longitudinal nervures, a characteristic which we do not meet with in any other butterfly. Only four species of *Colias* are enumerated, the headquarters of this genus also being apparently rather beyond Mr. Leech's limits. He has, however, sunk all the Japanese forms described as distinct by various authors, as varieties of *C. hyale*, Fabr.; but this is one of those questions which will never be disposed of to the satisfaction of entomologists without long and careful breeding of the supposed varieties or species. Some authors, however, have certainly gone too far in regarding mere varieties of butterflies as entitled to specific rank; while others have erred more seriously in the opposite direction, by placing together perfectly distinct species as varieties. It frequently happens that species which subsequently prove to possess very important distinctive characters, have a much greater superficial resemblance to others than obtains between seasonal or otherwise dimorphic forms of insects which belong indubitably to the same species. But if a good species is sunk as a synonym or a variety, the next entomologist who considers it to be distinct will very likely overlook the previous notices, and describe it as new. We are constantly discovering that names which stand as synonyms in our books really belong to insects which have since been described as new under other names.

In taking leave of this extremely interesting book, we must congratulate Mr. Leech on having successfully brought to a conclusion a work which will hold a worthy place among the many valuable local butterfly faunas which have been published in England, of recent years, by Godman and Salvin, Moore, Trimen, Distant, and others. W. F. K.

OUR BOOK SHELF.

Rainmaking and Sunshine. By John Collinson. (London: Swan Sonnenschein and Co., 1894.)

THE only object there can possibly be in giving a notice of this book is to warn intending purchasers of its contents, lest they be deceived by the title, and hope to find some account, more or less interesting, of the experiments that were made in America, a short time back, with the view of procuring a rainfall. This book has not even that recommendation. One has not much patience with weather prophets, who base their assertions on conjunctions of the planets, or some equally occult and absurd methods; but Mr. Collinson is in advance of all such vendors of nostrums. Not for him the uncertain, or partial, fulfilment of hazily expressed prophecies, not for him the long and careful study of signs and portents; he, himself, is the rainmaker, he is the dispenser of sunshine and cloud, he is gifted with the divine power that storms and floods and drought obey. Here is his own modest statement: "Thus when suitably placed as to residence, the results of his (the author's) action on magnets are certain to produce changes in the weather, and other effects, as interesting and useful, bearing on meteorological science generally. They are simply marvellous. Storms, floods, drought, &c., can be induced, on the one hand, and the prevalence of sunshine and warmth, in opposition to coldness and gloom, on the other. His action in this direction, judging from experience, could bring any district, and, indeed, the country generally, such favourable weather as would recall the glories of the Golden Age." (p. 18.) Another passage that makes one doubt whether the book is to be taken seriously, relates how a prophet (Query Dr. Falbe, says the author) foretold bad weather for March 28, 1893, sudden fall of the barometer, great conflicts of wind and water, and various other disasters. "About the same time Prof. Jenkins foretold that there would be a cyclone with snow on March 25. I took care that these storms did not happen." (p. 186.)

But there is one form of internal evidence which forbids us concluding that the author has perpetrated an elaborate joke. He claims to have given to a whole nation of holiday-makers ten days of enjoyable weather at Easter, but refused to exercise his godlike gift on behalf of suffering humanity at the following Whitsuntide, because "NO" (in very large capitals) "suitable sign of appreciation had then been received from any of those who largely benefited by the results of the fine Easter weather." (p. 214.) One would like to know what is the force of "then" in this sentence. Have the railway companies rewarded this gentleman since? And what would be a suitable sign of appreciation to a man so endowed? But enough of this nonsense; whoever else the book may amuse or edify, it will scarcely find readers among the subscribers to NATURE. W. E. P.

The Elements of Graphic Statics: a Text-Book for Students of Engineering. By L. M. Hoskins, Professor of Pure and Applied Mechanics in the Leland Stanford Junior University; formerly Professor of Mechanics in the University of Wisconsin. (London: Macmillan and Co., 1892.)

ALTHOUGH the fundamental ideas of Graphical Methods in Statics can be traced back to the writings of Stevinus, of Bruges (*c.* 1600), and although they must have been employed by scientific engineers, such as Brunel, the subject of Graphical Statics as known to the mathematician dates only from Maxwell's writings on the subject, and to Culmann's elaborate treatise in German; also to Colonel Sir George Clarke's exhaustive work.

The subject of Statics, which had come to a standstill, was revivified by the graphical methods now employed by every engineer and architect.

But as the subject is nothing unless employed practically by the draughtsman on the drawing board, it has not yet conquered the prejudices of the abstract mathematician, although many problems of allied descriptive geometry, required in the construction of inertia ellipses and curves (Part iii.), are well worthy of the attention of the pure geometer.

The present treatise is designed as an elementary textbook for the use of students of engineering; and the illustrations are drawn carefully to scale, representing each some real object.

The method of lettering, attributed to Bow, is now more appropriately assigned to Henrici; the author very rightly insists upon the fundamental importance of this lettering, in emphasising the reciprocity existing in the diagrams.

Incidentally the method of Graphical Statics emphasises the proper treatment of Statical problems, which is always to consider a system of balancing forces; and thus to banish the word Resultant from Statics unless employed to represent the force which if reversed will balance the remaining forces of the system. G.

A Naturalist on the Prowl. By Eha. Pp. 257. (London: W. Thacker and Co., 1894.)

From Spring to Fall. By "A Son of the Marshes." Edited by J. A. Owen. Pp. 239. (London: William Blackwood and Sons, 1894.)

THE author of "A Naturalist on the Prowl" knows how to write pleasantly on the natural history of the Indian jungle. There is not a dull page in his book. It is only rarely that we meet with a volume so full of interesting observations, and so free from stodginess. In "Eha's" company we travelled from the first to the last page, here admiring the keenness of his perception, there laughing at his humorous comments, and always made happy by his geniality. He does not "prowl" to kill, neither is he imbued with the spirit that induces many people to collect shells and postage-stamps as specimens; for though he recognises that "without a collection, a man's knowledge of natural history becomes nebulous, and his pursuit of it dilettante," he also knows that there is a possibility of a man degenerating into a mere collector, and ceasing to be a naturalist. Mr. R. A. Sterndale enriches the volume with eighty illustrations, mostly sketched from life.

The works of "A Son of the Marshes," on country life and scenery, are renowned for their simple beauty and sympathetic expression. Under the editorship of Mrs. Owen, the volume before us, like other books by the same author, is delightful reading.

Edible and Poisonous Mushrooms. By Dr. M. C. Cooke. (London: S.P.C.K., 1894.)

IT may be safely asserted that fewer kinds of fungi are used for food in Great Britain than in any other country in Europe. This is the more remarkable when we take into consideration the indebtedness of the present advanced state of Mycology to the researches of our countrymen, amongst whom may be mentioned Bolton, Sowerby, Badham, Berkeley, and Broome. The author of the work under consideration has also contributed very materially to a knowledge of edible kinds of fungi by various publications, and more especially in promoting annual fungus forays in various parts of the country. Poisonous fungi liable to be confounded with the numerous edible kinds are very few in number, and the majority of casualties, both at home and abroad, are caused by eating *Amanita phalloides*, a fungus very different in appearance from the common mushroom (*Agaricus campestris*), but which, probably from its neat and attractive appearance and size, appears to commend itself to unsuspecting persons, and being usually very abundant and widely distributed, is likely to be a continual source of danger until its characters and general appearance are more generally known.

Dr. Cooke very properly condemns the various rule-of-thumb methods for discriminating between edible and poisonous kinds of fungi, and shows that the essential characters of the various kinds must be thoroughly grasped, as being the only certain means of identification; and this method, with Dr. Cooke's book as a guide, should not prove a difficult task. The written descriptions of the various kinds, without being technical, are very clear and to the point, and the eighteen coloured plates are excellent. Finally, the best methods of cooking are given. The book is well printed, attractive externally, and very cheap.

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 intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

What are Acquired Characters?

FOR some while past, as we all know, a great contest has been raging as to whether acquired characters of an organism can or cannot be transmitted from one generation to another; and mighty authorities, on the one side, say that they can be; and great authorities, on the other, aver that they cannot be.

As a spectator of this contest, I have tried to understand it; and, in the first place, I have endeavoured to make out what is meant by the phrase "acquired characters"; or, in short, what is meant by the word "acquired," as used, in this connection, by Weismann, his friends, and his antagonists.

It is evident that the word is not used in its primary and natural signification; for, as on the theory of evolution (on which hypothesis the whole discussion proceeds), man has been evolved from an amœba or an ascidian, or some other early form, it follows that every character by which a man differs from this, his first progenitor has been acquired at some time between the two termini of the course of evolution, and, if the word were used in its ordinary sense, it would further follow that none of these characters could be transmitted by man to his offspring. This is manifestly untrue, for the issue of a woman is not simply an amœba. In fact, Weismann himself implies plainly that he does not use the word "acquired" in its ordinary signification, and asserts that its scientific value lies in its restricted use. ("Essays on Heredity," English translation, vol. i., first edition, p. 412.)

It becomes then very important to get at an accurate and workable definition of the word "acquired" for the purpose in hand; and such a definition must, I conceive, satisfy the following conditions:—(1) It must be such as to include all characters that are "acquired" within the restricted meaning of the word, and to exclude all characters that are not within the meaning; (2) it must be stated in physical, and not in metaphysical terms; (3) it must not be stated in terms derived from hereditability or the contrary, or in terms of any hypothesis or theory; (4) in order that it may be of use for scientific purposes, it must be stated in terms that admit of ascertainment and verification.

Of the importance of a clear definition of these words every one must, I think, be conscious; and if authority were required, we have that of Prof. Weismann himself. "I should wish to point out," he says, "that we ought above all to be clear as to what we really mean by the expression 'acquired character.'" ("Essays," vol. i. p. 169.)

Now, I do not profess to have read all that has come from the pen of Prof. Weismann, and still less the whole literature that has gathered round the controversy; but, after some search, I have hitherto failed to find any definition which has satisfied me; and, furthermore, I do not feel quite sure that all the advocates and opponents of Weismann use the phrase in one and the same meaning; and my object in writing this, is to ask for some assistance in finding the real and true definition of the phrase as used by both sides in this controversy.

Let me, to assist in the discussion, refer to some of the hints for a definition which I have been able to find.

At pp. 98–99 of vol. i. of the "Essays upon Heredity" (English translation, first edition), Prof. Weismann, having referred to "modifications which appear as the direct consequence of some alteration in the surroundings," and to the effect of a strange climate, says: "It is difficult to say whether the changed climate may not have first changed the germ, and if this were the case, the accumulation of effects through the action of heredity would present no difficulty. For instance, it is well known that increased nourishment not only causes a plant to grow more luxuriantly, but it alters the plant in some distinct way, and it would be wonderful indeed if the seeds were not also larger and better furnished with nutritive material. If the increased nourishment be repeated in the next generation, a still further increase in the size of the seed, in the luxuriance of the plant, and in all other changes which ensue, is at any rate conceivable, if it is not a necessity. But this would not be an instance of the transmission of acquired characters, but only the consequence of the direct influence upon the germ cells, and of better nourishment during growth."

This passage hints plainly at a definition of acquired characters to this effect. An acquired character is one produced by an external stimulus acting on the organism but not influencing the germ cells, whilst every character produced by an external stimulus acting on the organism and influencing the germ cells would not be acquired. But how are we to ascertain whether the germ cells—not, be it observed, the embryo, but the germ cells—have been influenced? Is there any chemical or microscopic means of answering this question? Is this influence a physical fact capable of ascertainment, and if so, how? It seems almost as if the presence or absence of an influence on the germ cells respectively, were inferred from the capacity or incapacity of transmission. But if so, I can hardly suppose that any one would suggest that any light can be got from such a definition, for this would be to proceed in a circle, and to reduce the statement that acquired characters cannot be transmitted to the following identical and useless proposition, viz. characters which by experiment are found not to be transmitted, and are therefore said not to affect the germ cells, are not capable of transmission, *i.e.* characters incapable of transmission are incapable of transmission.

At page 170 of the same volume occurs a passage which seems to suggest a slightly different definition of acquired characters. "I am also compelled to admit," says our author, "that it is conceivable that organisms may exert a modifying influence on their germ cells, and even that such a process is, to a certain extent, inevitable. The nutrition and growth of the individual must exercise some influence upon its germ cells." But a little afterwards, p. 171, there occurs another passage which, if I understand it aright, throws doubt on this conclusion; but this I will for the present neglect. In other passages, *e.g.* at p. 406, our author refers to direct influence of an external stimulus, such as climate, intending, I conceive, to draw the distinction between direct and indirect influence of an external stimulus on the germ plasma. These passages appear to me to suggest the following propositions with reference to acquired characters, viz.: (1) Every change produced by an influence of the organism on its own germ cells is not an acquired character; (2) an external stimulus may act on the organism, and the organism on the germ cells, and so produce a non-acquired character; (3) every other change produced in an organism by an external stimulus is an acquired character.

But, assuming these propositions to be true, do they admit of ascertainment by any appeal to physical facts? Do we know by any examination—physical, chemical, or experimental—what influence an organism produces on its own germ cells? If we do, then these propositions may be useful, and acquired characters will be a category of changes capable of scientific establishment by the appropriate means of inquiry; but if not, then they seem useless, except for the purpose of propounding an hypothesis or theory.

At p. 169 of the same volume of "Essays upon Heredity," I find Prof. Weismann saying: "An organism cannot acquire anything, unless it already possesses the predisposition to acquire it: acquired characters are therefore no more than local or sometimes general variations which arise under the stimulus provided by certain external influences"; and then he proceeds to illustrate this by saying that the so-called "exercierknochen" or bony growth caused by the pressure of a weapon in drilling depends on the capacity of the bone to react on the stimulus. "Every acquired character is simply the reaction of the organism upon a certain stimulus."

It would not, I think, be just to consider that in the sentence last quoted, Prof. Weismann proposes a definition of an acquired character as being the reaction of the organism upon an external stimulus, because many passages in our author's writings, and certain well-known facts, seem to show that there are many reactions upon stimuli which result in heritable characters. Thus, at p. 406, he says: "Every one will agree with him (Delmer) that the periodical change of leaf in temperate climates has been produced in relation to the recurring alternation of summer and winter. This is certainly the case, and it cannot be doubted that this character has been fixed by heredity." Heat and cold are external stimuli, and here, if I understand rightly, they are credited with heritable changes in the organism.

Some passages in Prof. Weismann's Romanes lecture tend in the same direction. At pp. 10, 16, and 50 he deals with the case of geotropism, positive and negative; and, if I rightly follow our author, he alleges (1) that geotropism, or the habit of the plant to respond in some particular way to the force of gravity, was not an original character of plants; (2) that it arose, or, in popular parlance, was acquired when plants became attached to the ground; and (3) that it is inherited. Now if I apply to this what is, as I understand, taught by the Professor, I conclude that the physical characters which produce the habit are due to the concurrence of two things, viz. (*a*) the original predisposition and (*b*) an external stimulus, viz. gravity. From this it would seem to follow that if all "acquired characters" are reactions on external stimuli, yet some such reactions are not "acquired characters."

The more I look at the matter, the more I feel it impossible to suppose that all the reactions on external stimuli are "acquired characters." For when I consider the vast part played by air, food, heat, moisture, gravity, and light, all of which are external stimuli on the development of plant life, and as I gather from passages already cited from our author on the production of some qualities, such as size, colour, &c., which are familiarly known to be inherited, I feel it difficult to suppose that it can be thought that all responses to external stimuli are to be considered as incapable of transmission. If I try to arrive at a definition by drawing a distinction between the principal and the minor causes of a change in the organism, or by calling some things conditions and other causes, I succeed no better; for here I should be introducing metaphysical distinctions. There is, so far as I know, no physical or logical distinction between principal and minor causes, or between cause and conditions in the case of two or more constituent parts of a cause, each of which is necessary, and none of which is by itself sufficient.

But this line of thought carries me further. Prof. Weismann ("Essays," vol. i. p. 411) deals with the case of "spontaneous characters, such as extra fingers or toes, patches of grey hair, moles, &c.," which he says may be transmitted. But do we know (*i.e.* do men of science know) that no external stimulus has had anything to do with the production of, say, a mole? It is one thing not to know affirmatively that this is the case, and another thing to know that it is not the case. Is the distinction between characters which seem to be due to an external stimulus and characters which seem to be spontaneous, one which is the subject of accurate scientific knowledge? Seeing that we only know organisms when subject to stimuli, do we know what they would be or would produce without stimuli? Have we any scientific knowledge of the organic world as developed entirely *ab intra* and independently of any external influence, *i.e.* of what plants and animals would be without light, heat, food, air? If we have not, then the distinction relied on may be perfectly true, but is of no value for scientific reasoning at the present day.

Then it has occurred to me to inquire whether I can make a safe distinction between the two kinds of change by reference to the development of the embryo. If a mole were to be found on the arm of a child at birth, we should be more inclined to regard it as spontaneous, as due to no external cause, than if it appeared in mature life. The embryo in the case of a child is no doubt protected from many external stimuli; but surely in the case even of the placental mammalia it would not be safe to aver that the embryo is protected from all external stimuli. In the case of an insect, the greater part of its development takes place under an exposure to external influences as complete as that of the adult insect; in the case of reproduction by gemmæ of multicellular animals, the protection must, I suppose, often be small or nothing at all; and in the case of the reproduction of the lower plants from gemmæ or from protonema, or of the higher plants by buds or from suckers, the embryonic condition, if it can be spoken of at all, is, I suppose, hardly distinguished as regards the influence of external stimuli from any other part of the career of the organism; so that I find myself unable to reach clear ground for the distinction between spontaneous and non-spontaneous variations.

But there are passages, to one of which I have already referred, which seem to suggest that the definition should be framed by reference to a distinction between a direct and an indirect influence, and that the definition should run thus: an "acquired character" is a reaction of the organism upon the direct influence of a stimulus, leaving reactions upon indirect influences to be treated as non-acquired characters. If so, what is the precise meaning of the word "direct" as applied to the influence? Does it refer to the repetition of the stimulus—so that a single change of climate would be a direct, and a repeated change of climate an indirect influence? Or does it refer to the supposed difference between influences operating on the somatic part of the organism and those which the organism itself exerts on its germ plasma? If so, I ask how is this ascertainable as a physiological fact?

Another limitation on the proposition that the reactions of the organism on external stimuli are acquired characters, I find in the second volume of Weismann's "Essays on Heredity" (English translation, p. 14), where the author, having referred to the characters, such as shape and size of finger-nails, likenesses of features, bearing, gait, handwriting, which are handed down from parent to child, goes on to add: "Characters only acquired by the operation of external circumstances acting during the life of the individual cannot be transmitted." Now, handwriting must, I suppose, be conceived of as a thing dependent on external circumstances: it is influenced by the material on which, the fluid by which, the pen or style by means of which the act is performed; but here the external circumstances have operated during many generations; so that the passage seems to suggest these propositions, viz. the reactions of the organism on external stimuli operating during the life of a single individual are not hereditary; the reactions of the organism operating during the lives of two or more individuals are hereditary. But such is not, I suspect, really the meaning of the author; it would be inconsistent with what he says at p. 40 of the same volume, where he says that the supposed increase of the musical sense "in the course of generations" by the exercise of the art can only have occurred on the supposition "that these modifications of an organ which are due to the exercise during the individual life can be transmitted to offspring"—a supposition which Prof. Weismann says "a close examination does not allow us to admit."

Another limitation on the class of reactions upon external stimuli constituting "acquired characters" is suggested by what I conceive to be Prof. Weismann's latest utterance on the subject, in his work "Das Keimplasma" (Jena, 1892). At p. 514 I find him saying—"By the term acquired characters I understand those which do not exist originally in the germ as tendencies, but first arise through peculiar influences which operate upon the body or particular parts of it. They are the reactions of these parts upon some external influences lying beyond the necessary conditions of development."[1] In the first of these sentences it seems to me that the Professor is not so much offering a definition as announcing a theory; for except by the inquiry whether the character is or is not heritable, I suppose that there is no means of ascertaining whether or not a change in the organism is due to a tendency in the germ.

But the second sentence seems to suggest a definition of "acquired characters" free from many of the difficulties we have hitherto encountered. They do not include all reactions of the organism on external stimuli, but only such as lie beyond the necessary conditions of development. Everything then turns on the meaning of "necessary conditions of development." What are necessary conditions of development? Let us take a tree which has put forth its leaves, its flowers, its seeds, in usual fashion, but which, having lost a limb by the saw of the gardener, and has thrown out around the wound that growth of new wood and bark with which we are familiar. The air, the sun, the soil are all external influences, and all necessary conditions of the development *which has actually occurred*, and so was the saw of the gardener. If we take the development which has actually occurred, every condition which led up to it was necessary, and each was as necessary as the other. Take the case narrated by Sir James Paget, of a fir tree which for a hundred and fifty years threw out successive annular growths over the part of its trunk from which a large piece of bark had been stripped off. ("Address to the Pathological Section of the British Medical Association," 1880, p. 15.) Were these rings part of its development, or were they not? If they were, the knife or saw was one of its necessary conditions. Most persons will reply that the saw was no necessary condition of the development of the fir tree, and that those only were necessary conditions without which the fir tree could not have lived.

If we consider what are the necessary conditions for the development of the seed of the fir tree, we can probably ascertain them; but the doctrine of evolution and the doctrine of the non-heritability of acquired characters will carry us back further, viz. to the primordial organism, and we must ask what were the necessary conditions for its development.

If this organism were supposed to have in itself a pre-existing law according to which it sought development, a contingent destiny inherent in its nature, then I can well understand how the conditions which satisfy that contingency, the circumstances which allow of that development might be said to be necessary to it. But if the organism have no such law and no such destiny, but instead thereof has only a capacity to vary in every possible direction, and if all the course of its actual variation be due to external circumstances operating by means of natural selection, then it seems to me that no one external thing can be said to be more essential to its development than another. The germ might have found itself in a different soil, in a different climate, exposed to different air, and then the development of the germ would have been different. There was no *à priori* necessity that it should be exposed to the particular conditions to which it was, in fact, exposed, or to any other particular conditions. If, therefore, we start from the actual development of an organism, and look back on the past, all the conditions which have led to its existing state are necessary; if we start from the germ prior to all development, and look to the future, then no given condition can be said to be necessary to its development, but all are contingent.

It is suggested that no influence lying beyond the necessary conditions of development can at any time have produced any heritable character, however long may have been the course of development. It seems, therefore, that in order to ascertain what conditions are necessary to development, we must go back to the amœba or ascidian or other primæval parent of our race, and we must conclude that those characters only will be transmissible by the human parent, which were reactions on the necessary conditions of the development of the primæval ancestor. Have we any scientific means of ascertaining what these conditions were, and so of ascertaining what characters are now heritable? Nay, if we adopt the evolutionary hypothesis, and believe that at least all existing animal organisms have sprung from a single parent, do not the diversities of the existing forms show that no one set of external circumstances were necessary conditions of development, but that the conditions consistent with development were infinite, or all but infinite, in number?

A further difficulty arises in my mind from a passage on the next page (p. 515), where I find our author mentioning wounds and mutilations as constituting one category of acquired characters. It is difficult to reconcile this with the statement of the Professor, in several passages, that acquired characters are reactions of the organism; for surely a wound is not a reaction of the organism, whilst the growth of the organism consequent on the wound—*e.g.* the growth of new wood

[1] It may be well to give the passage in the original: "Unter erworbenen Eigenschaften verstehe ich solche, welche nicht als Anlagen schon in Keim vorhanden sind, sondern erst durch besondere Einwirkungen, die den Körper oder einzelne Theile desselben treffen, enstehen. Sie sind die Reactionen dieser Theile auf irgend welche, ausserhalb der nothwendigen Entwickelungsbedingungen liegenden äusseren Einwirkungen."

and bark round the stump of a branch sawn off, is a reaction of the organism on the action of an external influence. It seems, therefore, fair to suppose that when our author speaks of a mutilation as an acquired character, he means the growth of the organism consequent upon the mutilation. But if so, a difficulty appears to arise; for the tendency to repair a wound is heritable, and therefore it seems difficult to suppose that the Professor would treat of it as not heritable. It may be said that the parent has the actuality of the repair, the child only the possibility of repairing; but this is, I suppose, all that can ever be expected of inheritance as applied to contingent reactions—*i.e.* reactions which only arise under peculiar circumstances. There can, I suppose, be no more characteristic heritable reaction than that of the pollen on the ovule, and of the ovule on the pollen; these reactions have taken place in the parent plant, but in the offspring they are originally potentialities, tendencies, contingencies, and they are converted into facts in the event, and in the event only, of the meeting of the pollen and the ovule.

Or take, again, the secretion of the gastric juice in response to the presence of food in the stomach. The parent has taken food, and the reaction has taken place; but the infant inherits, I suppose, the capacity to secrete and not the counterpart of the actual secretion of the parent.

It would seem that there is no essential difference between these three cases. The parent transmits the power to repair a wound, but not the actual reparation of a wound: it transmits the power of fertilisation, but not the fertilised ovules: it transmits the power to digest, but not the already secreted gastric juice.

The emphasis which Weismann has laid on the case of wounds and mutilations would suggest that his doctrine might be thus paraphrased: when an organism is endowed with a capacity, or, to use his word, a predisposition, to react in response to given stimuli, and has so reacted—then what the organism transmits to its progeny is the capacity or predisposition, and not the actual result of the reaction.

It is impossible to doubt that some characters of an organism are hereditary; that others are not, and that the ascertainment of the dividing line between the two classes is of the highest moment to the study of biology; and to Weismann we owe a debt of gratitude for having called pointed attention to this matter. I have at times been tempted to wish that men of science had applied themselves to ascertain the two categories of characters, and then by a careful induction had proceeded to learn the law of heredity without regard to any hypothesis or theory—without reference to germ or soma. But this is not the course which in fact has been taken; and therefore it seems highly necessary to inquire what is the precise meaning of the terms of the proposition affirmed by the one side, and denied by the other.

I conclude this, I fear, too lengthy paper with two questions: (1) Are the conditions which I have suggested as essential to a good definition correct ones; if not, in what are they erroneous? (2) What is the true definition of the words "acquired characters" in the present controversy which satisfies these conditions?

EDW. FRY.

Discontinuous Motion.

THE old theory of the motion of solid bodies through a frictionless liquid supposed that the liquid flowed according to the electrical law of flow. This theory was found to be unsatisfactory, because it makes the pressure negative when the velocity of the solid exceeds a certain critical value.

The theory of discontinuous motion removes the above objection, but is open to others of a different kind. Assuming for the sake of argument that the two theories give correct results when the velocity of the moving solid is respectively less and greater than the critical value, the theory of discontinuous motion ought to be capable of explaining the transition from one kind of motion to the other, and how and why it is possible for a vortex sheet to be called into existence when the critical value of the velocity is exceeded.

Although vortex sheets and other motions involving molecular rotation cannot be generated in a frictionless liquid by a conservative system of forces or by operations performed on the boundary, yet it is easy enough to produce such motions by ordinary mechanical agencies. If a mixture of ice and water be stirred up and the ice allowed to melt, the liquid will acquire molecular rotation owing to the presence of the particles of melted ice, even though it is absolutely devoid of viscosity. So also if liquid at rest were separated by an indefinitely thin horizontal plate, and the upper liquid were set in motion with horizontal velocity V and the plate were removed, the surface of separation would be a vortex sheet. But the production of motions of this kind requires methods of a somewhat artificial character, and it is difficult to see how they could be set up by a solid whose velocity is allowed to increase gradually from zero to some magnitude greater than the critical value. In fact, I entertain very little doubt that the final motion of the liquid would be quite different from what the theory of discontinuous motion would indicate.

There is, however, a further point, for there are strong grounds for believing that vortex sheets are unstable. No general proof of this proposition appears as yet to have been given, but in every case that has been examined the theorem has been found to be true, (i.) when the liquids on either side of the sheet are identical, (ii.) when the densities of the two liquids are different, but no bodily forces such as gravity and the like are in action. If, therefore, steady discontinuous motion existed at any particular instant, the probabilities are that the motion would be unstable, and the region of dead water in the rear of the moving solid would break up and be changed into a region of turbulent motion. The pressure in the rear of the solid due to this turbulent motion would be different from that of the dead water, and it is therefore not surprising that the theory of discontinuous motion should furnish results which do not agree very well with experiment.

We must also recollect that a frictionless liquid is an ideal substance which does not exist in nature. All fluids are more or less viscous; and it is just at the point where the pressure would otherwise tend to vanish and change sign that we should anticipate the effect of viscosity would appear, and prevent this state of things from taking place; and I believe that many of the difficulties which have arisen in connection with this subject are due to the fact that the effect of viscosity has been overlooked. A vortex sheet cannot exist in a viscous liquid; and if by any artificial means one were produced, it would immediately disappear, and molecular rotation would be propagated into the surrounding liquid. On the other hand, in a viscous liquid, molecular rotation requires no artificial means for its production; for a viscous liquid cannot move without molecular rotation, except in the single case in which the liquid moves like a rigid body having a motion of translation alone. In all other cases, if irrotational motion existed at any particular instant, the motion would immediately cease to be so, and molecular rotation would instantaneously be generated.

Unfortunately the equations of motion of a viscous liquid are so intractable that very little progress has been made in applying them to the solution of hydrodynamical problems. By means of Oberbeck's solution (*Borchardt's Journal*, vol. lxxx.) for the steady motion of translation of an ellipsoid in a viscous liquid, it can be shown that the above difficulties do not arise when viscosity is taken into account; but since the integration of the equations of motion proceeds upon the assumption that the squares and products of the velocities may be neglected, the solution is inapplicable except in the case of slow motions like those produced by the small oscillations of pendulums. The solution gives no information as to what will happen when a disc is moving through a liquid with a velocity of several feet per second.

A. B. BASSET.

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Capacity for Heat.

IN the course of some writing upon which I am now engaged, I have constantly to refer to the *capacity for heat of certain substances as compared with the capacity for heat of an equal volume of water*.

The phrase given in italics is a most clumsy one, but I know not how (accurately) to convey the same idea in a shorter way. "*Capacity for heat of unit volume*" has been suggested; but I think that a little reflection will show that it does not express accurately the exact meaning.

Specific Heat × *Specific Gravity* gives the numerical value required, but cannot be regarded as a definition. There can be no doubt but that a concise expression is wanted. In calorimetry it is often of greater importance to the experimenter to onsider the capacity for heat of volumes rather than the capacity of masses. In the course of a communication which I made to the Physical Society of London, at their last meeting, I appealed to the Fellows present to supply me with the missing phrase. In the discussion which followed the paper, this matter was only incidentally referred to; but, although I think that there was a general agreement as to the want, unfortunately the meeting closed without coming to any conclusion as to the best method of supplying the deficiency.

Will some of your readers help me in this matter?

12 Park Side, Cambridge. E. H. GRIFFITHS.

The Swallowing of One Snake by Another.

THE snake incident, described in NATURE, October 25 last, page 620, as having occurred in the reptile-house in the Zoological Society's menagerie, recalls to my mind two similar cases, recorded in the same periodical, vol. xxx. July 3, 1884 ("A Cannibal Snake," by E. H. Evans), and July 31, 1884 ("The Swallowing of One Snake by Another," by C. R. Osten-Sacken). The first case was observed in Java, the other was witnessed by me in Washington, D.C. In the latter case one of the snakes, although three-quarters of its length had already been engulphed in the other, succeeded in getting out, apparently unhurt, as it remained alive and well in the cage a long time afterwards.

In the *Figaro*, July 26, 1894, I found still another instance of the same kind, which happened in the Jardin d'Acclimatation in Paris. A large snake, while attempting to swallow a rabbit, was interfered with by another one, and passed with the rabbit into the body of its comrade of captivity ("L'un des deux passa à la suite du lapin dans le corps de son camarade de captivité"). C. R. OSTEN-SACKEN.

Heidelberg, Germany, October 28.

ON RECENT RESEARCHES IN THE INFRA-RED SPECTRUM.[1]

I PRESENTED to the Association in 1882, at Southampton, an account of some researches made by means of the bolometer, in the infra-red spectrum, formed by a glass prism; but though these labours have continued with occasional intermission during the past twelve years, it is for reasons, which will be explained later, only within the past three years that any notable advance has been made, and only within the past twelvemonth that such a measure of success has been attained as justifies the present communication.

This is not the time to give any historical account of discovery in the infra-red, but all those interested in the subject know that the first investigator here was Sir William Herschel, whose observations consisted essentially in finding that there was something which the eye could not see in a region which he proposed to call the "thermometric spectrum." His distinguished son, Sir John, made a curious anticipation of later discovery by indicating, though crudely, that this invisible heat was not uniformly distributed, and a similar conclusion was reached in an entirely different manner, through the thermopile, by the too early lost Melloni. So ignorant, in spite of these investigations, of those of the elder Draper and of the elder Becquerel, were we till lately, that when, quite within my own recollection and that of most of you, Lamansky in 1871 published, from his observations with the thermopile, a crude little illustration showing three inequalities in the energy curve, universal attention was excited by it among those interested in the subject.

Among other minds my own then received a stimulus which turned it in this direction, and having, as it seemed to me, exhausted the capacities of the thermopile, I invented an instrument for continuing the research, which was afterwards called the bolometer, and with which, in 1881, at an altitude of 13,000 feet upon Mount Whitney, I found spectral regions hitherto unreached, and whose existence had not been suspected.

I returned with a strong impression of the prospective importance of this discovery, and laboured at the Allegheny Observatory in improving all portions of the new method of research, especially of the bolometer and its adjuncts, with the twofold object of obtaining greater sensitiveness to heat, and greater precision in fixing the exact point in the spectrum where the change of heat originated. With the former object such a degree of sensitiveness was at that time reached, that the bolometer indicated a change of temperature of $\frac{1}{100000}$ of a degree Centigrade, and with the latter, such precision that it was possible to fix the relative position of a line, not merely with a possible error of a considerable fraction of a degree, such as Lamansky's determination is evidently subject to, but with a certainty that the error would be within a minute of arc. The range of the apparatus in wave-lengths was almost unlimited as compared with any other process, and both its sensitiveness and its possible precision seemed to be at that time notable as compared with previous methods, for a great advance was made on anything done before with the thermopile, when the presence of the well-known "D" line of sodium was rendered sensible (though barely sensible), even as a *single* line, by the change of temperature. The sensitiveness was also, as has been said, accompanied with the possibility of unusual precision. The results of this labour were laid before the British Association in the communication already alluded to, and which exhibited ten or twelve inflections of the curve in the portion till then almost unknown, which extends from a wave-length of of 1μ to a wave-length of nearly 3μ, at which point the glass prism then used became wholly opaque to radiation. The positions of these inflections were fixed with a precision quite impossible to the thermopile, but this exactness was only obtained in practice by a process so slow as to be almost prohibitory; and with this apparatus the writer personally made in those earlier years such a number of observations as he hardly likes to recall, so disproportionate did the labour inherent in this method seem to the final result.

The justification of this labour seemed to lie in the fact that it does not appear that photography has ever rendered anything much below a wave-length of 1μ—anything, at least, which has been reproduced for publication in a way which gives confidence that we are in touch with the original. The processes which involve the use of phosphorescent substances have given some indications of lines considerably below 1μ, but it is safe to state that the work which has just been referred to as communicated to this Association in 1882, presents almost the only indications which we have possessed, even up to the present time, about the lower infra-red solar spectrum.

Now the curve which was given, even in the later Allegheny observations made with the rock-salt prism, contained but a dozen inflections below the wave-length of $1{\cdot}5\mu$, and these inflections, with their correct prismatic and wave-length positions, represent, I think, most of our present knowledge in these regions even to-day.

To understand the method by which there were attained, but only at this great cost of labour, results till then unreached, it may be repeated that the bolometer had been rendered more sensitive than the thermopile, but that it was capable of being pointed and its position in the spectrum being measured, only by a tedious process which has been exclusively used till lately (but which that presently to be described advantageously replaces). Whichever process is used, when the bolometer thread touches a cold line in the spectrum (since what is black to the eye is cold to it), a larger current flows through the galvanometer, and the spot of light marking the needle's motion is deflected through a certain number of degrees.

From this point forward, the new process, whose results I am about to have the pleasure of bringing before

[1] A paper read to Section A of the British Association, at the Oxford meeting on August 11, by S. P. Langley.

you, differs widely from the old. In the old, two observers at least are engaged: one, who notes that reading of the micrometer or of the vernier, which fixes in angular measure the exact part of the spectral region whence (though nothing is visible) a thermo-electric disturbance has proceeded; and another, who simultaneously notes through how many divisions of the scale the spot of light from the galvanometer mirror is deflected by the same electric disturbance. The process may be compared to a groping in the dark, and it was only by these means that the considerable inflections of the energy curve much below the region about 1μ were then fixed by the bolometer, by being gone over again and again, with what seemed almost interminable repetition, and which did in fact call for over a thousand galvanometer readings to obtain the position and amount of each single inflection of the energy curve, with the degree of accuracy which was then obtained, and which was shown in the former memoir.

If it took two years to fix the position of twenty lines by this process, it would take two hundred years to fix two thousand, supposing they existed, and it became evident that if the bolometer continued to be the only means available, new and more effective methods of using it must be found.

New Methods.

About ten years ago a plan was first studied, which has ever since been maturing, by means of which this work could be carried on, not only with far greater rapidity, but with greater certainty, and by an automatic process. The idea in its original simplicity is very easily understood.

In the old process, just described, the deflection of a spot of light upon a scale was read by one observer, while another read simultaneously the position in the spectrum of the cold band, or line, which caused the thermo-electric disturbance.

Now, in imagination, let us take away both the observer at the circle and the one at the galvanometer, and, in the latter case, remove the scale also, and put in its stead a photographically sensitive plate. As the needle swings to the right or left, the spot of light will trace upon the plate a black horizontal line whose length will show how far the needle moves, and how great the heat is which originated the impulse. If this be all, when (under an impulse originated by the movement of the spectrum over the bolometer thread) the needle swings a second time, it will go over the same place; but if the plate have a uniform vertical movement, proportional to the horizontal movement of the spectrum, the combination of the two motions of the needle and the plate will write upon the latter a sinuous curve which will be, in theory at least, the same as the curve formerly deducible only with such pains from thousands of such galvanometer readings.

If we suppose that the movements of the invisible spectrum over the bolometer thread are controlled by clockwork so that this spectrum is caused to move uniformly, and that three movements are, by accurate mechanism, rendered absolutely synchronous with those of the moving plate, it is clear that we shall be able to readily deduce from the photographic curve traced on the latter, not merely the amount of the heat, but each particular position in the spectrum, of the thread of the bolometer, which alone can correspond with any given inflection of the curve.

Thus simple is the theory; but no one had better occasion to know how difficult the practice would be, than myself.

The researches by the old method, and the early attempts to improve them, were interrupted by my acceptance in 1887 of a position which implies the administrative charge of different branches of the public scientific service and of duties largely incompatible with original research. What time could be spared from these was, however, partly employed in elaborating the plan of investigation just referred to. An appropriation had been asked of Government for the establishment, on a modest scale, of an astro-physical observatory in Washington, whose first work should be the investigation of the whole infra-red solar spectrum, by some means which would open that great region to knowledge. It had been asked of Government because it seemed that such knowledge, if attained, might teach us facts about the sun and the absorption of its rays by the terrestrial atmosphere, which there was ground to hope would ultimately lead to results of such importance as to justify this national aid.

These observations were resumed in 1890 on the new system, with the aid of the Smithsonian Institution, which provided larger and more efficient apparatus, whose design embodied the results of nearly fifteen years' study of these subjects.

Pending the provision of a suitable observatory building, an inadequate and temporary one was erected in the Smithsonian Park in Washington, to shelter the apparatus, presently to be mentioned, with which it was designed to commence work while making provision for more permanent scientific quarters (which I may add are still lacking).

Apparatus.

The Foucault siderostat, perhaps the most powerful instrument of the kind existing, was originally made by Sir Howard Grubb, of Dublin, from my indications; but its dispositions have since been considerably modified. A beam from its 20-inch mirror is conveyed through the slit of a horizontal collimating telescope having a rock-salt objective of nearly seventeen centimetres aperture, and of ten metres focal length, to the prism or grating. The prism is of rock salt of corresponding dimensions, worked (by Brashear) with the precision of, and presenting all the external appearance of one of flint glass. It is mounted on a massive spectro-bolometer (as the instrument which supports the prism or grating used in producing the spectrum is called). This instrument includes a large azimuth circle, over the centre of which the prism is placed, and it also carries the bolometer, which registers the spectral heat. The focal length of the image-forming lens, or mirror, is in this instrument much greater than in the first one used, and all parts of the apparatus are correspondingly increased in size and stability. The most important and novel feature is, however, the mechanical connection of the large azimuthal circle carrying the prism, with a distant photographic plate, susceptible of vertical motion, and which latter takes the place of the scale formerly in front of the remote galvanometer, both circle and plate being moved by the same clockwork, which is of such steadiness and precision as to make the two movements as far as possible perfectly synchronous.

To fix our ideas, let us suppose that the slow-moving azimuthal circle carrying the prism revolves through one minute of arc in one minute of time; in which case the spectrum will move horizontally across the vertical bolometer thread at a proportional rate. Now, if the same mechanism which causes this circular motion of the prism, and of the spectrum, of one minute of arc in one minute of time, causes the photographic plate to move vertically before the galvanometer mirror at the rate of one centimetre of space in one minute of time; if there be no allowance to make for changes of temperature in the prism or for like corrections, if the mechanician has done his part in such perfection that everything works as it should, it obviously follows that, under such conditions, during every second of this minute a portion of the spectrum represented by the small quantity of one second of arc, will have glided before the bolometer thread, and that during this same second the plate will have been lifted automatically through one-sixtieth of a centimetre of space.

This is one relationship of time and space in actual use here, though others may be established by the use of the change-wheels with which the apparatus is provided. The essential thing is that the plate shall show with great precision, and even on simple inspection, not only the inflections of the energy-curve there written down, but the exact relative position in the distant spectrum which the bolometer thread occupied at the moment it caused the disturbance. In the case assumed, for instance, if we suppose that the record on the plate commences with the part of the spectrum whose angular value is 40°, then, since 1 millimetre corresponds to 6 seconds of arc, and so on, the existence of an inflection on the plate at 30 centimetres, 3 millimetres and seven-tenths of a millimetre, would show that the disturbances originated at the point in the spectrum corresponding to an angular measure of 40° 30′ 22″·2.

If the arm which carries the bolometer is n metres long, and if the thread of the bolometer is $\frac{1}{m}$ metres in diameter, the angular value of the bolometer thread is $\frac{1}{mn}$. At present the linear width of the bolometer thread is not very materially less than formerly, but it is used with a longer arm, and its virtual width is accordingly less. In present actual practice (to use round figures) the optical arm carrying the spectrum across the bolometer, is five metres in length; and if the bolometer thread be one-twentieth of a millimetre in width, its angular value is evidently $\frac{1}{100000}$ of the radius of the circle in which it moves, or a little over two seconds of arc. When the heat is distributed over so large an area, that part of it which falls on a thread of given diameter is, of course, proportionately less, so that the greater precision of measurement demands a more sensitive construction of the bolometer, as well as a more accurate mechanism for pointing it. Improvements have accordingly been introduced in the construction of the bolometer, and a need for greater sensitiveness in the galvanometer has necessarily gone with them. This increased sensitiveness has caused increased liability in the latter to both systematic and accidental perturbations, and the elimination of these has been found the most formidable difficulty of the whole process. It has been effected, largely, by placing the whole apparatus under constant temperature conditions.

I take pleasure also in acknowledging the advantage I have found in using both Prof. Boys's quartz threads and the extremely small mirrors which he, I think, first advocated in connection with the well-known form of galvanometer due to Lord Kelvin. These and other collective improvements made in the bolometer and in the galvanometer, have now made the former sensitive to changes of temperature in its strip which are demonstrably less than 1/1,000,000 of a degree Centigrade.

These are the principal pieces of apparatus, though I should mention that a method has been found by which the very large salt prisms used can be preserved in perfect polish while exposed to all the usual casualties of observation. The actual prism in most frequent use was made from a block of salt exhibited at the World's Fair by the Russian Government, and presented to the Smithsonian Institution by its Commissioners. It is about eighteen centimetres, or over seven English inches, in height.

Before entering upon a description of the results obtained, I desire permission to speak of the aid I have received from the gentlemen whose assistance I have been fortunate in securing: first, to Dr. Hallock, then to Prof. Hutchins, Mr. Hubbard, and Mr. C. T. Child, and lately to Mr. F. L. O. Wadsworth and Mr. R. C. Child; the imprint of the labours of the two latter gentlemen being upon almost all the details of the more recent work.

Results.

Let us recall that the infra-red spectrum from a rock-salt prism, such as that used here, is extremely contracted as compared with one from flint, and still more contracted as compared with the wave-length scale. The portion of the spectrum presented by photography reaches a little below the band whose wave-length is about 1μ, and this was asserted by one of the most eminent living authorities on the subject (Dr. John W. Draper), when the writer commenced this work fifteen years ago, to be the absolute end of the heat spectrum. The writer has, however, since carried his investigations by direct measurement to five or six times this wave-length, and by indirect measurement much farther still, though what is here now exhibited does not go beyond a wave-length of about 4μ. The invisible heat spectrum of a 60° rock-salt prism through this great wave-length, includes only somewhat less than two degrees of arc, and the first of these degrees contains the greater proportion of the energy.

On referring to the illustrations exhibited to the Association in 1882, or even to later publications of results obtained by rock-salt prisms, though with the old method, it will be seen that there are shown in the latter publication about a dozen measured inflections of the energy curve below $1\mu·5$, and it may be remembered that this curve was obtained only by two years' assiduous labour.

We have now before us three energy curves obtained by the new method, each exhibiting the whole infra-red spectrum under examination, with about a hundred inflections. These curves are nearly, but not exactly, similar.

The three were obtained in the same day, each from an entirely independent observation, so that each has given, in a fraction of a day, many times the results previously obtained by two years of labour, and, as it will be later shown, has given these results with a notable gain of accuracy.

But this is not all. These three curves have been taken with a rapid movement of the clockwork and a brief swing of the galvanometer, so as intentionally to suppress all minor inflections and to introduce only the leading features of the spectrum, as shown in eighty or a hundred of the leading inflections (lines), or groups.

This new bolometric method has, however, as will be shown later, a capacity of resolving these into nearly twenty times that number, the minor inflections having been thus designedly suppressed here at first, to better show the character and position of the principal ones. All these energy spectra, by the new as by the old method, are, of course, subject to the slight changes due to invisible clouds constantly passing before the sun, which, with the change of the sun's altitude, and of the consequent lengthening path of its rays, prevent any one of them from being exactly like the other; while, at the same time, everyone here may satisfy himself by direct inspection of the results before him, that there is scarcely any single one of their inflections which is not reproduced in the other two, in exactly the same place, though probably not exactly in the same degree; and when we take different spectral traces, made at different hours of the day, and even on different days of the month—traces which are absolutely independent of each other—and superpose them, experience shows that we may expect to see such an agreement as that in the three here chosen at random for illustration, or in the more detailed one, where the relative probable error is less than one second of arc. Three such traces only are here given (to prevent confusion), but if we follow these coincidences through not three, but ten or more plates, we may well judge (since there seems no possibility here of systematic error) that a result, which all confirm, is reliable, and that, on the other hand, a single inflection on one plate, which the other nine unite in repudiating, is due to some fortuitous cause.

But there is still a higher certainty to be obtained, by a method independent even of comparison or the exercise of judgment. It is founded on the well-known process of composite-photography, where, in photographing the successive members of an assemblage of persons, having similar general characteristics, as of race, character or education, the individual disappears, and the normal type alone remains. In order to apply this method to such results as ours, however, another step in the process must be introduced, and this is an interesting one, for the energy curve itself, however valuable, is a comparatively unfamiliar method of showing variations in the energy, which we are all alike used to seeing in the visible spectrum given by linear representations, and not by a system of inflections.

In describing this new step, which is to give us a *linear* spectrum in addition to the original curve, it will be desirable to also give evidence of the statement now made, that the present method is capable of recording far minuter inflections than those shown in the curves here exhibited, which, as has just been stated, have been taken only for the purpose of illustrating such more important features, as can be seen and verified by the audience, and especially for showing the agreement of independent observations. The evidence of the capacity of the apparatus to show this detail will best be illustrated by applying our purely thermometric method to some well-known lines in the visible spectrum, such as the familiar "D" lines of sodium. I have already stated that ten years ago the bolometer was barely able to distinguish this as a *single* line. At the present time our little thermometer, as you here see, now shows not only the two "D's" as separate lines, but the nickel line between them. First we have the complex energy curve (Fig. 1), where we see successively the inflections due to the motions of the galvanometer caused by the cold in D_1, then to the smaller chill from the nickel line (aided perhaps by that from some of the close atmospheric lines), then the chill from D_2.

Immediately below this curve is the more familiar linear representation of the same subject (Fig. 2). Now this linear representation, it is most important to observe, has been obtained, not by drawing, but by the subsequent application to the curve of an automatic process, by means of which its indications are reproduced by photogravure, as separate lines, while by the same automatic process the most complex spectral curves can be rendered into their linear equivalents.

I have no space to enter here on a description of this process, further than to say it is effected by means of a systematically distorted image of the curve, obtained by a special combination of spherical and cylindric lenses. You will see, on minute inspection, that the inflections of the galvanometer curve have been slightly "loaded," to produce a more effective contrast of light and dark. Except for this, which can in no way affect the position of a line, but only its intensity, the whole process is as absolutely automatic as any photograph of the visible spectrum.

This thermograph of the "D" lines has been chosen to indicate the grasp of this new thermometric method, by applying it to the test of an object in the visible spectrum, with which every physicist here is doubtless familiar. He may then be invited to recall that the distance between the "D's" in a rock-salt 60-degree prism is about eleven seconds of arc, and to observe that two lines about half this distance apart are here shown as sharply divided by this thermal method, as, for instance, are the components of the double star α Geminorum by a three-inch achromatic. Obviously, then, our method could indicate the existence of two lines, little, if any, more than one-quarter the distance between the "D's." Lines 3″ or less apart can then evidently be indicated by this method, even in its present stage of development.

And now, returning to what has been said about the evidence obtainable as to the perfect coincidence of these inflections in different energy curves obtained at different times, and to the consequent evidence that each inflection so given is real, and not the product of an accidental variation in the curve, we may conceive that from any number of such independent curves, any number of such linear representations of the spectra have been obtained; for example, that ten such linear representations of the whole spectrum as are here given of the D lines only, have been so found from ten complete energy curves taken on as many different days. From these ten linear representations, by the well-known processes of composite photography, *one* final photograph of the spectrum is formed, and on this it is evident we may expect to find only what is permanent and not what is accidental, granting that a rare accident may have introduced an occasional abnormal deflection.

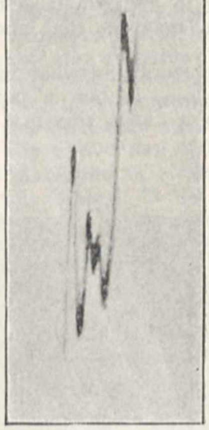

FIG. 1.

$_2D_1$

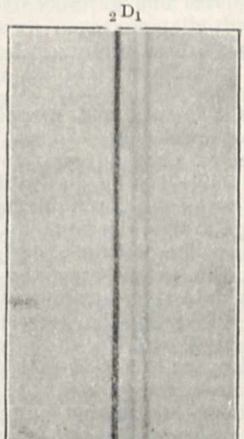

FIG 2

Now, considering that the part of the infra-red solar spectrum of rock-salt under review extends through nearly two degrees, or 7200 seconds, and that we have just seen by the illustration of the D lines that lines 3 inches apart can be thus divided, we may see for ourselves that, at any rate, over 2000 lines, if they exist, can be mapped. But these lines do exist, the whole of this new region being apparently as intimately filled by them as the visible spectrum by the Fraunhofer lines. In further evidence of this, here is a portion of the lower spectrum in the comparatively unknown part extending from $\lambda = 1{\cdot}4\mu$ to $\lambda = 2{\cdot}2\mu$ including the great band Ω shown as a single inflection in my first communication to this Association, but here resolved into thirty or more subordinate lines (Fig. 3). This illustration includes a part of the new region discovered on Mount Whitney in 1881; and in the small portion here exhibited, you may see that about 200 lines are discriminated.

I am now trying to bring what may be called the first stage of the long labour, a portion of which is here described, to a close, this first stage consisting chiefly in the discovery of, and mapping the relative positions of new spectral lines.

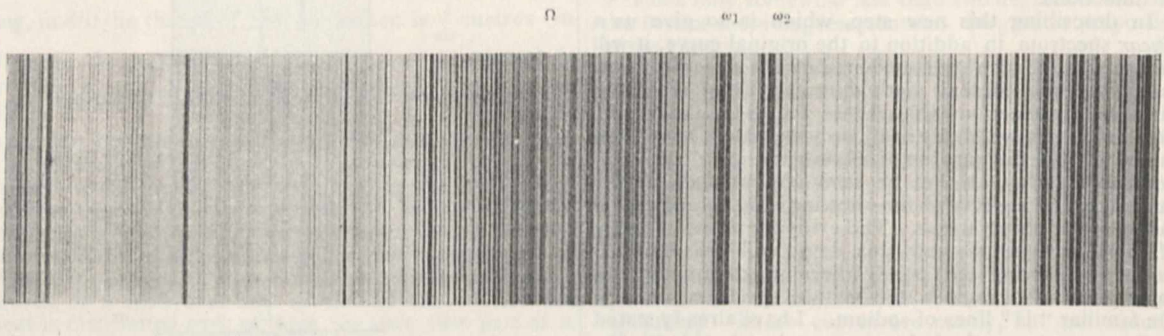

FIG. 3.—Bolograph of the portion of the infra-red solar spectrum lying between wave-lengths $1{\cdot}4\mu$ and $2{\cdot}2\mu$.

I will only refer to what it seems to me the second part of this work is likely to be, and to the different kind of interest which may not improbably belong to it, from that which belongs to this, the first.

We are thus far in the position of early students of the visible spectrum, who simply drew the lines they saw, without inquiring into their meaning. Nevertheless, to have discovered and mapped a great number of these lines is only a beginning, for their real value lies in their interpretation, and this is still chiefly to come. As to the possible importance of this interpretation, it is not enough to remind ourselves that three-quarters of the whole energy of the sun exists here, and not in the upper spectrum. We must remember also that while, as a rule, in the upper and visible spectrum a great proportion of the lines are caused by absorption in the solar atmosphere, and a perhaps smaller portion by telluric absorption, here, on the contrary, we are led, by everything we already know, to expect that the great telluric absorptions on which meteorological predictions and other immediately practical interests depend, may be expected to be found, and it is on the comparison of these energy curves taken at different periods of the year, and at different altitudes of the sun, that those who are engaged in the work see good cause to hope for important results in the future.

Before I conclude, let me present a collective view of the field in which work has been going on in these later years at the Smithsonian Observatory, on the same scale, with the visible spectrum. I say "on the same scale," meaning, not on a wave-length scale, which expands the invisible at the expense of the visible, and not on a prismatic scale alone, which expands the visible at the expense of the invisible, nor even on such a logarithmic one as that proposed by Lord Rayleigh, but on a conventional scale, which I will ask you to tolerate, as it is simply meant to show the actual extent and importance of the region covered here as compared with that known to Newton. In this illustration, with which I close my remarks, the mean dispersion throughout the invisible rock-salt spectrum, as far as 4μ, is taken as the standard, and both spectra are laid out on that common scale. On the left is the visible spectrum known to Newton; next this, is the region known through photography, now extending a little beyond the band, $\rho\,\sigma\,\tau$, which marks what at the time these researches were commenced, was considered by the then most distinguished investigator, in the infra-red, the end of the heat spectrum. Beyond, and on the right, is a part of the new regions of the spectrum developed by the bolometer, and of which charts may be shortly expected on the scale of which a specimen in detail has just been shown.

I cannot close this statement without expressing the gratification with which I have laid it before the same body that listened to that made on the same subject twelve years ago, or my sense of my good fortune, in doing so before an audience in which I recognise many of the same eminent men who so kindly received that first presentation of these researches.

THE TREATMENT OF DIPHTHERIA BY ANTI-TOXIC SERUM.

FOUR years ago Prof. Behring published his remarkable paper "On the mechanism of immunity against experimental diphtheria in animals." In this memoir the author stated that it was possible to immunise animals against the diphtheria bacillus by the injection of cultures attenuated by heat or the addition of 1 in 500 trichloride of iodine to the cultivating medium. The same result could be obtained by the inoculation of the pleural exudation of animals dead of experimental diphtheria. or by the injection of chemical compounds, such as trichloride of iodine, after inoculation of virulent diphtheria-bacilli.

Behring's most important discovery, however, was that the serum of animals immune against the bacillus of diphtheria and its poisons had the power of "destroying" in vitro and in the animal body the chemical poison secreted by this bacillus; and that animals, after a mortal dose of diphtheria poison had been injected, could be not only immunised, but actually cured, by the introduction into their system of the serum of animals immunised against the specific bacillus and its poisons. In a further series of researches he found that the serum of such animals possessed this power to a most remarkable and almost incredible degree, and that the therapeutic value of this serum increased up to a certain extent with the amount of diphtheria-toxine which had been introduced into the system; whilst, on the other hand, the same toxine injected in too large quantities might cause the serum to lose its "anti-toxic" property.

Behring afterwards immunised large animals, such as sheep, horses, &c., against diphtheria by the repeated injection of diphtheria-toxine, and he observed that their serum was of great value when injected into human beings suffering from this disease. Before proceeding, however, it may be convenient to discuss these statements at somewhat greater length, and illustrate them by an account of a few experiments.

Let us draw some blood from a horse which has been thoroughly immunised against diphtheria, and decant the serum after its separation. We now take a filtered diphtheria culture, a given quantity of which we know to prove fatal to a guinea-pig of a certain weight in a certain number of hours. Let us divide, for instance, 5 c.c. of this filtered culture into five doses, and inject 1 c.c. into a guinea-pig (A) weighing 500 grammes without the addition of any serum. Four other guinea-pigs of the same weight as the first receive the same amount of toxine and ·1 c.c., ·01 c.c., ·001 c.c., ·0001 c.c. of the horse's serum respectively. The first guinea-pig (A) will die in less than twenty-four hours, but of the others, the three first will recover without any symptom of illness; whilst the last, which has received ·0001 c.c. only, will probably die after some delay. We know, then, that the dose of serum which will protect a guinea-pig of a given weight against a known amount of toxine injected at the same time as the serum, lies somewhere between ·001 c.c. and ·0001 c.c., and, by further experiments, the exact dose may be accurately determined. In this way we may form an idea of the strength of the serum, but we must always remember that the data thus obtained are but approximate ones.

The curative serum may be introduced into the animal the day before the toxine is injected, but, in that case, a much larger quantity of serum will be necessary to protect the animal. If the serum be inoculated at the same time as the toxine, and in a different part of the body, the quantity of serum must also be increased if the curative effects are to be apparent. If some time is allowed to elapse between the injection of the toxine and that of the curative serum, the quantity of serum—in order to be effective—must be proportionately greater; but I have seen animals recover when the injection of the curative serum had been delayed *eleven hours* after the introduction of the toxine, which proved fatal to the control animals in 28–48 hours. In this latter case the dose necessary to cure was 5000 times that sufficient to immunise when the serum was injected with the toxine.

Can any opinion be formed as to the mechanism in which this serum exerts its action? The first explanation which suggests itself is that when the curative serum and the toxine are mixed together in the syringe or in the animal's body, the toxine is either destroyed or simply neutralised, just as the acid in a given solution may be neutralised by an alkali.

Yet there are several facts which negative this opinion, or rather which tend to prove that this destruction and neutralisation of the toxine, if destruction or neutralisation there be, is effected through the agency of the cells of the body. In the first place, if the whole were simply a chemical process, we should expect it to take place with mathematical precision. Thus, if ·001 c.c. of horse's serum did destroy or neutralise 1 c.c. of toxine, this neutralisation should be apparent whether the mixture be injected into a guinea-pig, a rabbit, or a sparrow. But it is not so; on the contrary, it has been found that in certain animals a very small amount of curative serum is sufficient to render harmless a certain amount of toxine when the same amount of serum utterly fails to do so in animals of another species. Even if the same species of animals be used the curative effect will differ in intensity, according to whether the mixture be injected directly into the blood, or into the subcutaneous tissue. Moreover, if a number of guinea-pigs be injected each with the same amount of toxine and varying doses of curative serum, it is of frequent occurrence that some animals will resist when they have received but a very small quantity of serum; while others, which have received equal or even larger quantities of serum, speedily perish. Had we to deal with a simple chemical neutralisation or destruction of poison, identical results should occur in all animals. The action of the curative serum may also be inhibited by weakening the cells of the animal body by the action of bacterial or other protoplasmic poisons. Hence I consider that the term "anti-toxic," which has been used up to the present to denote this property of the serum, is only approximately correct; and I would prefer to substitute the word "curative," until such time as the mode of action of the serum has been accurately determined.

Before speaking of the application of this method to the cure of diphtheria in the human subject, and to the results which have been obtained in the hands of such experimenters as Behring, Roux, Ehrlich, Kossel, Wassermann, Heubner and others, it must be pointed out how the disease in man differs from and resembles that produced experimentally in animals by the subcutaneous inoculation of the bacillus diphtheriæ, or of its poisons. In both man and animals the symptoms are, to some extent, produced (Roux, Behring, Sidney Martin) by the absorption of the poisons secreted by this specific bacillus. But in man the production of these poisons is facilitated by the fact that the bacillus lives on the surface of the membrane, where it is exposed continually to the action of the air entering and leaving the lungs. Now, it has been shown experimentally that the easiest way of obtaining a large amount of diphtheria toxine from the diphtheria-bacillus growing in an artificial medium, is to expose the surface of this medium to a current of air. In the respiratory passages of man these conditions are exactly fulfilled, and the bacillus is able to produce large quantities of toxine. In the second place, the upper respiratory passages are crowded with all sorts of non-pathogenic and some pathogenic micro-organisms, the chief among the latter being the staphylococcus albus and aureus, and the various kinds of streptococci. These multiply on the soil prepared by the bacillus diphtheriæ, and secrete their toxine, which being absorbed, add their poisoning action to the deleterious action of the specific bacillus. In the preceding paragraph, I have drawn attention to the fact that the toxines of another micro-organism will inhibit the action of the curative serum; and there can be little doubt that the toxines secreted by these micro-organisms in man will have the same effects. The formation of the membrane also, and the mechanical obstruction so produced, may in themselves be the cause of death. It must be added, also, that diphtheria is chiefly a disease of the poor, and that in many cases the diagnosis is delayed, and the treatment is not begun until the patient is almost moribund. It is plain, therefore, that in the diphtheria of man we have all the conditions which are favourable to the production of the poison, and the inhibition of the action of the therapeutic serum. Hence we cannot be astonished that the death-rate at the Hôpital Trousseau in Paris amounted during the last six months to 62 per cent. of all cases admitted for this disease, and not treated with curative serum.

An examination of the statistics lately published by Dr. Roux, the eminent director of the Institut Pasteur, will allow us to form some idea of the value of the curative serum when applied to man. The reason for choosing these statistics is that they contain all the elements necessary to enable us to form an accurate opinion. The mortality among 3971 children admitted during 1890-1894, and who were treated on ordinary lines, *i.e.* without curative serum, amounted to 51 per cent., the highest mortality being in 1890, when it reached 59 per cent., and the lowest in 1892, when it fell to 47 per cent. On February 1, 1894, Dr. Roux began the inoculations. As soon as a child suffering from diphtheria was admitted into the Hôpital des Enfants Malades, it received 20 c.c. of serum subcutaneosly, and if no improvement took place, 10 c.c. or more were again injected next day. 448 children were thus treated from February 1 to July 24, 1894, and 109 died, the mortality being 24·5 per cent. At the Hôpital Trousseau, on the other hand, 520 children were admitted during that period, and treated without the injection of curative serum. Of these 316 died, giving a mortality of 60 per cent. This difference is striking enough; but it is even more marked when we come to analyse the results. In the first place, we must eliminate from Dr. Roux's statistics 128 cases which were shown by bacteriological examination not to be diphtheria. This mistake is perfectly legitimate, as it has been shown that without a bacteriological examination, it is impossible to make an accurate diagnosis. The injection of the serum in a doubtful case is followed by no harm, and as the result of the bacteriological examination cannot be known for twenty-four hours, the injection of the serum will, at any rate, prevent the child catching the disease from his neighbour. The mortality among such cases we know to be very slight, and in the statistics of previous years, as well as in those of the Hôpital Trousseau, these cases are included among the cures, so that the proportion of cures appears to be much higher than it really is. These cases being excluded from Dr. Roux's statistics, there remain 320 children in which the bacteriological examination revealed the presence of the diphtheria-bacillus. Of these 20 died on admission, before any therapeutic measures whatever could be employed. Of the 300 other children 78 died, giving a mortality of 26 per cent. We may further divide these into uncomplicated cases, and those in which tracheotomy had to be performed for laryngeal obstruction. Of the uncomplicated cases, the mortality in Dr. Roux's wards amounted to 12 per cent.; whilst at the Hôpital Trousseau, where no serum was used, the mortality during the same period reached 32 per cent. In the preceding years it was 33 per cent. in the wards in which Dr. Roux carried out his experiments.

Turning now to the cases in which tracheotony had to be performed, we find that 49 per cent. of Dr. Roux's cases died. At the Hôpital Trousseau in the same period 86 per cent. of the tracheotomised children perished. Similar results have been obtained in Germany, the mortality in some wards falling as low as 14 per cent. Prof. Ehrlich has shown that the question of time is a most important element, and that the chances of obtaining a cure are infinitely greater when the remedy is applied at an early stage of the disease. The following table, reprinted from the *Deutsche Medicinische Wochenschrift*, will explain this:

Time elapsed from onset of disease to injection of serum.	Number of cases.	Number of cures.	Number of deaths.	Cures, per cent.
1	6	6	0	100
2	66	64	2	97
3	29	25	4	86
4	39	30	9	77
5	23	13	10	56·5

It will be noticed that the number of cures diminishes according to the length of time which has elapsed since the onset of the disease.

It appears to me to be difficult to explain away the results obtained in France and Germany by simply saying that the epidemic has been a mild one; for in other hospitals and institutions in which the curative serum has not been used, the mortality has remained the same, or even increased. In fact, it is plain that the serum treatment of diphtheria is now established on a firm basis, and that it is only right that we should at once give the children in this country the benefit of the results of experimental investigation which has been principally carried on abroad. The British Institute of Preventive Medicine is now taking steps to provide the serum at cost price.

Finally, it is right to draw attention to the fact that although the knowledge of the biology of the diphtheria bacillus, and of the effects of its poison, has been based on the investigations of different observers—French, German, and English—yet the discovery of the curative effect of the serum of immunised animals is the merit of one man only—Prof. Behring, of Berlin.

M. A. RUFFER.

NOTES.

THE Paris Academy of Moral and Political Sciences has bestowed the Audiffret prize of twelve thousand francs upon Di Roux for his treatment of diphtheria.

A STATUE of Claude Bernard, the eminent physiologist, was unveiled at Lyons on Sunday last, in the presence of a distinguished company.

THE *National Zeitung* states that news has reached Berlin from the Kilima-Njaro district that the German botanist, Dr. Lent, and the zoologist, Dr. Kretzschmar, have been killed, with several of their black followers.

DALZIEL'S correspondent at New York says that a dispatch received from Buenos Ayres gives particulars of a severe earthquake, attended by great loss of life, which occurred on Saturday, October 27, at the town of San Juan de la Fronteza, the capital of the Province of San Juan, in the Argentine Republic. Many of the principal buildings are said to have been destroyed. The shock extended to the towns of La Paz, Cordova, and Rosario. The New York *Herald* of Monday published a telegram from Buenos Ayres stating that two thousand persons have perished in the earthquake at La Rioja, and that twenty thousand are homeless. This alarming report has not, however, been confirmed.

NUMEROUS memoirs have already been based upon the rich collection of Dutch birds brought together by the late Mr. J. P. van Vickevoort Crommelin, of Harlem, and presented, after his death, by his daughter to the Leyden Museum. Ornithologists will therefore welcome the appearance of a complete catalogue of the collection, which has been compiled by Dr. Jentink, the director of the museum, and forms the fourteenth volume of the catalogues of the Muséum d'histoire naturelle des Pays-bas. Nearly three hundred species are in all enumerated, and the majority of these are represented each by a considerable number of specimens. A separate record is given of every individual, notifying the sex and age, the place and date of capture. An explanation which Dr. Jentink furnishes of the exact locality of all the places mentioned should be of material service to the student of the geographical distribution of these birds.

THE London Mathematical Society, having been on October 23 duly registered as a corporation under Section 23 of the Companies Act, 1867, will hold its first general meeting at its office, 22 Albemarle Street, on Thursday evening, November 8 next, at eight o'clock. The meeting is empowered by the Articles of Association to elect a council and officers, to frame bye-laws and to pass resolutions with regard to the affairs of the Society. The following gentlemen have been recommended by the present council (acting under Article 8) for election as the new council and officers for the ensuing session:—President, Major Macmahon, R.A., F.R.S.; vice-presidents, Messrs. M. J. M. Hill, F.R.S., A. B. Kempe, F.R.S., and A. E. H. Love, F.R.S.; treasurer, Dr. J. Larmor, F.R.S.; hon. secretaries, Messrs. M. Jenkins and R. Tucker; other members, Messrs. A. B. Basset, F.R.S., and G. H. Bryan, Lieut.-Colonel J. R. Campbell, Lieut.-Colonel A. J. C. Cunningham, R.E., Messrs. E. B. Elliott, F.R.S., J. W. L. Glaisher, F.R.S., A. G. Greenhill, F.R.S., E. W. Hobson, F.R.S., and W. H. H. Hudson. At the close of the preceding business, an address will be delivered by the retiring President (A. B. Kempe, F.R.S.), on "Mathematics," after which the meeting will proceed to the ordinary business.

DURING this month, the following lectures will be delivered at the Royal Victoria Hall, Waterloo Bridge Road, S.E.:—November 6, Prof. A. W. Rücker, F.R.S., on "The Electric Spark"; November 13, Mr. H. R. Mill, on "Unexplored England"; November 20, Miss Hope Rea, on "The Grand Canary and its People"; November 27, Miss F. Routledge, on "China."

IT is proposed to hold a "Light and Heat Exhibition" at Woodhouse Park next year. The park is situate at Shepherd's Bush, and is, therefore, in direct communication with all parts of London and the suburbs. The exhibits will be classified into eight divisions, viz.:—Gas lighting; electric lighting; various illuminants; heating and cooking apparatus; naval and military; scientific apparatus; photography; historical. The promoters' intention is to hold a series of technical exhibitions on the same site. Further information can be obtained of the Hon. Secretary, Woodhouse, Shepherd's Bush, W.

PROF. W. M. FLINDERS PETRIE will lecture on "Primitive Egypt," at the London Institution, on November 12. The following are among the other lecturers and subjects for the session ending March 4, 1895:—"Wonder-Working Plants," by Dr. David Morris, C.M.G.; "Climbing in the Himalayas," by Mr. W. Martin Conway; "Extinct Monsters," by Rev. W. N. Hutchinson; "The Newtonian Constant of Gravitation," by Prof. C. V. Boys, F.R.S.; "The Fauna of Rivers and Lakes," by Prof. Sydney Hickson; "Twenty Thousand Feet above the Sea," by Mr. Edward Whymper; "The Netherlands," by Mr. H. J. Mackinder; "Waves of Water and Waves of Light," by Mr. A. P. Laurie; "Nerves and Nerve Centres in Action," by Mr. Henry Power; "Comets," by Sir Robert Ball, F.R.S.; "The Germination of Barley," by Mr. A. Gordon Salamon; "Electric Currents in the Body," by Prof. Victor Horsley, F.R.S.; "The Beautiful as seen in Minute Nature," by the Rev. Dr. Dallinger, F.R.S.; "Theory and Practice of Preventative Inoculation," by Dr. E. E. Klein, F.R.S.

For the last week or so the weather in these islands has been of a very unsettled character generally. Several areas of low barometric pressure have reached us from the Atlantic, and were accompanied by strong gales on nearly all our coasts, and in the Channel very rough weather has been experienced, while thunder and lightning have also occurred in many places. The rainfall has been very heavy, especially in the west of Ireland and south of England, where it amounted to nearly four and a half inches during the week ending October 27, while in the Midland counties the fall reached about two inches. Since the beginning of the year the rainfall has about reached, or exceeded, the average in all districts except the Midland counties, where the deficiency is three inches, and the west coast of Scotland, where it is two and a half inches.

PROF. CLEVELAND ABBE includes the following among his notes in the *Monthly Weather Review* for July:—On June 3 a tornado passed north-eastward through the counties of Harney, Grant, and Union, in Eastern Oregon. The most novel feature attending the disturbance was the hail. It is stated that the formation was more in the nature of sheets of ice than simple hailstones. The sheets of ice averaged three to four inches square, and from three-fourths of an inch to one and a half inches in thickness. They had a smooth surface, and in falling gave the impression of a vast field or sheet of ice suspended in the atmosphere, and suddenly broken into fragments about the size of the palm of the hand. During the progress of the tornado at Long Creek a piano was taken up and carried about a hundred yards.

MR. A. TREVOR-BATTYE, about whose safety some anxiety has been felt, has reached Archangel from the island of Kolguef, where he had been studying bird-life. The proposed relief expedition, for which subscriptions had been invited, is consequently no longer necessary.

THE German collector, J. Menges, describes, in the last number of *Petermanns Mitteilungen*, his travels in the Habr Auel district of Somaliland, inland from Berbera. He visited the country in 1882, and gives a detailed account of his routes in pursuit of big game.

THE last Washington letter published by the American Geographical Society states that the boundary line between the United States and Mexico has been resurveyed, and a series of monuments erected along it at intervals for a distance of 700 miles. The field operations for the survey of the Alaskan frontier are also completed, and the height of Mount Logan has been confirmed as 19,500 feet, thus overtopping Mount St. Elias by 1500 feet. Another interesting result of the joint exploration is to show that both these mountains lie within British territory.

THE Indian Reservations of the United States in which the aboriginal inhabitants are secure against aggressive civilisation, are fast diminishing in number and extent. The last number of the *Bulletin* of the American Geographical Society mentions that four million acres of reserved land in the north-east of Utah, containing great mineral wealth, and hitherto the home of the Uncompahgre and Uintah Indians, is about to be opened to settlement, while nearly a million acres in South Dakota are to be withdrawn from the exclusive occupancy of the Indians of South Dakota, Oregon, and Idaho.

CONSUL C. S. SMITH gives an account of the Anglo-German frontier survey in East Africa, in the November number of the *Geographical Journal*. The survey, which was carried from the mouth of the Umba River on the east coast to Kilimanjaro, was effected in 1892, Mr. Smith being assisted by Mr. Imam Sharif, the Indian surveyor, who subsequently accompanied Mr. Bent to Arabia, and Lieut. G. E. Smith, R.E., and the German party with whom they co-operated was under the charge of Dr. Peters. The route by the Umba Valley to Kilimanjaro was found to be a practicable one, and the country traversed was, as a rule, characterised by fertile soil; but the land suffered from the want of settled inhabitants, on account of the raids of the Masai. Consul Smith suggests that immigrants from India be encouraged to colonise parts of the Umba Valley; and if trade is ultimately attracted there, he points out that the seaport of Wasin, although smaller than Mombasa, would be found quite practicable for steamers.

THE current number of *Himmel und Erde* contains a summary of a lecture by the Director of the Statistical Office of Berlin, on the increase of damage by lightning, and on the effect of lightning on the human body. The increase, which is unmistakable, is attributed to various causes, viz. the employment of electricity in various industries, the continual change of the form of the earth's surface by deforestation, drainage, &c., and the impurities introduced into the atmosphere by the growing consumption of coal. Prof. von Bezold has shown, some time since, that for Bavaria the average yearly number of fires caused by lightning amounted to 32 from 1833-43, to 52 from 1844-65, to 103 from 1866-79, and to 132 between 1880 and 1882. While in the year 1855, 134 persons were struck by lightning, and 73 were killed, the numbers thirty years later reached 189 and 161 respectively. It is noteworthy that persons struck by lightning generally perceive neither lightning nor thunder, but have the idea of being suddenly enveloped by fire.

To the facts of fishes out of water and of crabs on dry land we are accustomed, but the recent announcement of the discovery of a flying Copepod is a novelty of which the naturalist has probably never dreamed, though Giesbrecht's beautiful figures of the Naples forms might well have induced the thought of such a possibility. In the current number of the *Zoologischer Anzeiger* (No. 459), Dr. Ostroumoff, of the Sebastopol Biological Station, states that he and his son were coasting along the Khersonese peninsula on a calm clear morning last July, when they noticed numbers of the tiny green Crustacean, *Pontellina mediterranea* (Claus), flying just above the level of the water. "Many of these Copepods were resting on the surface-film, took springs into the air, where they described a long curve, and fell down again upon the water surface." Dr. Ostroumoff traces the origin of this very exceptional habit to a peculiarity in the manner of exuviation that *Pontellina* probably shares with certain other pelagic Entomostraca, *e.g.* *Evadne*, *Pleopis*, which cast their skins at the surface of the water "by the help of the air which becomes entangled in (*anhält*) their rejected coverings." We should like to hear something more upon this interesting phenomenon, which involves a complete change in the life conditions of the species. It is difficult, at any rate, to imagine that so light and hairy a form as *Pontellina* can return at will to the water again after once breaking through the surface film.

THE Bunsen flame spectra of the metals of the alkalies and the alkaline earths have been studied by Herren J. M. Eder and E. Valenta, by means of an apparatus which enabled them to make exposures of extraordinary length. The apparatus, described in a paper communicated to the Vienna Academy, consisted essentially of a circular strip of platinum gauze mounted in a slanting position on a nickel wheel. The lower edge dipped into a solution of the salt to be examined, and the upper edge passed through the Bunsen flame. The wheel was kept slowly rotating by clockwork, and the steady and uniform spectrum obtained was photographed. In the case of sodium and potassium no lines were discovered that were not already known from the spark and arc spectra, although the ultra-violet sodium lines of wave-lengths 3303 and 3853 were well rendered. But with an exposure of some thirty hours a large number of new bands appeared in the violet and ultra-violet spectra of the metals of the alkaline earths. They belonged chiefly to the oxides, and consisted partly of double bands arranged pretty regularly on a continuous background. The spectrum of boracic acid, obtained by burning a mixture of coal gas and $B(C_2H_5)_3$ in a Linnemann burner, showed six new violet and ultra-violet bands, which by their general character and their regular distribution correspond to those previously observed in the visible spectrum.

A REMARKABLE instance of the anomalous behaviour of bodies at very low temperatures is given by M. Raoul Pictet in the *Revue Scientifique*. For the preparation of pure chloroform by crystallisation at $-69°$ C. he used two copper refrigerators, of capacities of $2\frac{1}{2}$ and 32 litres respectively. The former being more convenient, it was used for the first series of experiments. About 2 kgr. of commercial chloroform were put into a glass test-tube, placed in the refrigerator, and surrounded by a temperature of about $-120°$, as indicated by a sulphuric ether thermometer. The chloroform appeared turbid at $-40°$ or $-50°$, and was filtered and further cooled. At $-68°{\cdot}5$ the cooling ceased, and very transparent crystals of chloroform appeared on the walls of the test-tube. The chemically pure chloroform thus obtained was used with great success in the Berlin hospitals, and larger quantities had to be obtained. M. Pictet noticed with great surprise that chloroform, refrigerated in the larger vessel, was cooled to $-81°$ without a trace of crystallisation. To test for experimental errors the small refrigerator was exposed to the same cooling process, and the chloroform crystals plunged into the larger one. But the crystals dissolved at once, though the same thermometer, successively plunged in the two refrigerators, fell from $-68°{\cdot}5$ to $-81°$, where no crystallisation was going on. Finally, the whole test-tube, with crystals at the walls, liquid chloroform in the centre, and thermometer standing at $-68°{\cdot}5$, was immersed bodily in the liquid chloroform at $-81°$. The thermometer gradually fell from $-68°{\cdot}5$ to $-81°$, while the crystals dissolved before the observer's eyes. M. Pictet works out an explanation of this striking phenomena on the basis of the theory of radiation and his own theory of the molecular constitution of solids and liquids, which leads him to the conclusion that heat oscillations corresponding to low temperatures traverse bad heat conductors with greater facility than they do compact and heavy substances such as metals.

THE fifth report upon the activity of the German Imperial Physico-Technical Institute appears in the *Zeitschrift für Instrumentenkunde*. Besides the electrical work, which chiefly dealt with resistances, the principal subjects of investigation were connected with thermometers, manometers, barometers, pyrometers, standard Hefner lamps and photometry, and the physical properties of various kinds of glass. The branch establishment at Ilmenau, in the Grand Duchy of Saxony, has been extended by the addition of a technical school for workers in glass instruments. Among the barometers tested were the aneroids employed by Dr. von Drygalski on his Greenland expedition. It appeared that low temperatures are capable of creating a temporary disturbance in the indications of these instruments. The chief optical work was connected with photometry. As regards the introduction of the Hefner lamp as a standard of illumination, the report points out that Germany is ahead of other countries in possessing a well-authenticated standard of light which answers all technical requirements. Recently the Institute has endeavoured to construct simple and portable photometers for technical purposes. Two such instruments have been constructed and found to work well. The photometry of the arc lamps illuminating "Unter den Linden" at Berlin, was also taken in hand by the Institute. The examination of different glasses related mainly to their solubility in water, which was found to give an indication of various other properties. The electrolytic precipitation of zinc and other metals from dilute solutions was investigated with a view to their preparation in a pure state. The examination of specimens of steel, of chronometer oils, and of coloured thermometer liquids, was among the many tasks allotted to this most useful and many-sided National Physical Laboratory.

IN the Pitt Press Mathematical Series there is no more useful volume than "Arithmetic for Schools," by Mr. Charles Smith.

The Cambridge University Press has published a second edition, in two parts, of Mr. Smith's work. Evidently the book has met with the reception it deserves.

THE importance of experimental work is fully recognised in the agricultural department of the Glasgow and West of Scotland Technical College. We have before us the reports on experiments on the manuring of hay, oats, and turnips, conducted in 1893 on the Home Farm of Cleghorn Estate, near Lanark, and on about fifty other farms scattered all over the south-western counties of Scotland. Prof. R. P. Wright, who directed the experiments, must derive satisfaction from the useful conclusions to which they have led.

THE first part of Mr. J. W. Taylor's "Monograph of the Land and Fresh-water Molluscs of the British Isles," published by Messrs. Taylor Bros., Sovereign Street, Leeds, has just appeared. It would be difficult to speak too highly of the fine coloured plate which forms the frontispiece to the part, or of the 138 well-drawn illustrations in the text. These figures will be recognised by all conchologists as faithful representations of the species they personify. The work is readable, concise, and accurate, so far as it has been published, and the scientific naturalist, as well as the systematic student, will find it useful and interesting.

MR. R. L. JACK, the Government Geologist of Queensland, has issued his report of the progress of the geological survey for last year. We learn from it that the most important work of the year was the production of a geological map of the Charters Towers gold field. The first edition of this map was issued early in the present year, and Mr. Jack does not claim too much when he says that no important centre of mining industry in Australia has been so thoroughly mapped. The underground work has now been completed, and it is expected that a second edition, embodying this work, will shortly be published. With Mr. Jack's report we received a report, by Mr. W. H. Rands, on the Towalla and Marceba gold fields.

WITHIN the past few years the number of students and workers in glacial geology has greatly increased. The third edition of Prof. James Geikie's "Great Ice Age," just published by Mr. Edward Stanford, appeals therefore to a much larger class than when the previous issue appeared seventeen years ago. The work has been enlarged, and most of it has been rewritten. The mass of glacial literature that has accumulated during the last fifteen years or so, has rendered it possible for the author to treat the glacial, and interglacial, deposits of the continent much more fully than in the second edition. The phenomena of existing glacial action in Alpine and Arctic regions, and the glaciation of Scotland, have been revised in the light of recent work, and several rearrangements of matter have been made. An important addition consists of two chapters on the glacial phenomena of North America, by Prof. T. C. Chamberlin. All glacialists will welcome this authoritative account of the glacial accumulations of Canada and the United States. It increases the value of what has always been a valuable treatise.

THE additions to the Zoological Society's Gardens during the past week include a Barbary Ape (*Macacus inuus*), a Turtle Dove (*Turtur communis*), four Barbary Turtle Doves (*Turtur risorius*), four Barbary Partridges (*Caccabis petrosa*), a Crested Lark (*Alauda cristata*) from Morocco, presented by Mr. Alfred J. Gosling; a Caracal (*Felis caracal*), from South Africa, presented by Mr. J. E. Matcham; a Brown Capuchin (*Cebus fatuellus*) from Guiana, presented by Mr. T. A. Jenkins; a Common Buzzard (*Buteo vulgaris*) from Aden, presented by Captain R. Workman; a Lanner Falcon (*Falco lanarius*), captured at sea, presented by Mr. Arthur J. Elliott; a Hawks-billed Turtle (*Chelone imbricata*) from the East Indies, presented by Captain E. F. Tyacke; two Long-nosed Crocodiles (*Crocodilus cataphractes*) from West Africa, presented by Mr. J. Banks Elliott, three Rusa Deer (*Cervus hippelaphus*, ♂ ♀ ♀) from Mauritius, presented by Rear-Admiral Kennedy; two Somili Ostriches (*Struthio molybdophanes*) from Somaliland, purchased; four Plumed Ground Doves (*Geophaps plumifera*) from Australia, received in exchange.

OUR ASTRONOMICAL COLUMN.

THE SPECTRUM OF δ CEPHEI.—M. A. Belopolsky has taken a number of photographs of the spectrum of δ Cephei—a variable of short period—and determined from them the velocity of the star in the line of sight. The results obtained showed a periodic variation, and M. Belopolsky used them to find the elements of the star's orbit, in the manner described by Dr. Lehmann-Filhés in the *Astronomische Nachrichten*, No. 3242 (see NATURE, August 2, p. 327). He finds that the eccentricity of the orbit is 0·46; and the apparent semi-major axis 180,000 geographical miles (207,000 English miles). The period of the star is 5d. 9h. The maximum velocity of approach is about 2·8 statute miles per second, and of recession 3·2 miles per second. The system, as a whole, is therefore moving away from our own system. It is found that the light-minimum occurs one day before the time of periastron passage given by the computed elements of the orbit.

THE ROTATION OF VENUS.—For eight years M. Flammarion has carried on observations of the polar caps of Venus, and, in the current *Comptes-rendus*, he discusses the bearing of his results upon the question of the planet's period of rotation. It will be remembered that Schiaparelli concluded in 1890 that the rotation and revolution periods of Venus were of the same length, viz. 225 days; but later observations by Trouvelot and others have led many astronomers to doubt this interpretation, and to believe that the rotation period of the planet is not very different from that of the earth. M. Flammarion remarks that if it is conceded that the polar caps are really due to snow or ice, their very existence is against Schiaparelli's view. As the two caps are often visible at the same time, it appears that the axis of Venus is but slightly inclined to the orbit. M. Flammarion's observations of markings on the planet are not sufficient to determine the period of rotation, but they appear to indicate that it is not far removed from twenty-four hours.

THE LOWE OBSERVATORY.—A few particulars with regard to the new astronomical observatory, which has lately been erected in Southern California by Prof. T. S. C. Lowe, are given in Saturday's *Times*. The observatory is seven miles by rail north of Pasadena, and sixteen miles north-east of Los Angeles. Its altitude is about 3600 feet above the sea, and 2000 feet above the hill at the base of the mountains, which are very steep at this point. While the crest of the range rises high above the observatory and shelters it on the north, leaving, however, the North Star visible, the entire southern horizon is unobstructed, extending to the rim of a large segment of the Pacific Ocean, about 100 miles distant, on the south and west. Astronomically, it is nearly at the intersection of the 34th parallel of north latitude with the 118th meridian of longitude west of Greenwich. The new observatory is well equipped with the great 16-inch Clark reflector and other instruments which have done notable work in the Warner Observatory at Rochester under the directorship of Dr. Lewis Swift, who will now superintend the Lowe Observatory.

THE MEAN PARALLAX OF STARS.—In the *Astronomische Nachrichten*, No. 3258, Prof. Hugo Gyldén gives the results of his attempts to discover a formula connecting the parallax of a star with its magnitude and its apparent motion. The fifty-six stars which have had their parallaxes determined with a satisfactory degree of accuracy were arranged in groups according to their magnitude and according to their apparent motion. After a lengthy series of tentative formulæ, the observed values were connected within about ten per cent. by the following formula,

$$P = 0''\cdot204\, e^{-0\cdot2138 m\, \Psi_m}$$

where $\Psi_m = 2 - \left(\frac{1}{e}\right)^{\frac{m}{10}}$ m is the magnitude, and P the parallax of stars exhibiting no proper motion. For stars with proper motion a term p^1 has to be added to P, the value of which tends to 0"·48 as the magnitude, and the proper motion increases. So long ago as 1872, Prof. Gyldén showed that it is justifiable to deduce the distance of a group of stars from their apparent mean brightness in all cases where the probability of a certain intensity of illuminating power is a function of this intensity alone, without depending upon position in space. And since the photometric law has been proved to be at least approximately valid in this case, it may be concluded that the brightnesses of stars reduced to the same distance are the same, on an average, for all distances which can enter into our consideration. But the most important result of the present investigation is the determination of the mean parallax of first magnitude stars reduced to the zero of apparent motion. The value for this, which is 0"·204, may be considered as identical with Peters's value of 0"·209, especially when it is borne in mind that the latter value is not reduced to zero apparent motion.

THE INSTITUTION OF MECHANICAL ENGINEERS.

ON Wednesday and Thursday evenings of last week, October 24 and 25, a general meeting of the Institution of Mechanical Engineers was held at 25 Great George Street; the President, Prof. Alexander B. W. Kennedy, occupying the chair. The two following papers were read and discussed:

"The Manufacture of Standard Screws for Machine-made Watches," by Mr. Charles J. Hewitt, of Prescot.

"Drilling Machines for Cylindrical Boiler Shells," by Mr. Samuel Dixon, of Manchester.

Mr. Hewitt's paper was of an interesting nature. He is the works manager and chief mechanic of the Lancashire Watch Factory, an establishment recently started at Prescot for the manufacture of watches on a large scale in one works. The factory system of watch production has been, as is well known, carried to a very successful issue in the United States, where the Elgin and Waltham Watch Companies annually make large numbers of excellent time-pieces wholly by machinery. As, in all cases, where highly skilled hand labour, performing intricate operations, is superseded by mechanical appliances, the machines used are of a highly organised and costly nature. In the case of the minute parts required in watch-making, this feature is very strikingly emphasised. Perhaps some of our readers may remember the exquisite little machine tools exhibited by the Waltham Watch Company, at the Inventions Exhibition, in the year 1885. These were a revelation to most English watchmakers, accustomed to the small factories and perfectly rude appliances of the British industry, in which the highest skill of the operators, due to special training from earliest youth, compensated for the lack of ingenuity displayed in the construction of the tools used. In the case of watches, as with so many other mechanical productions, the brain capital expended in the employment of construction of machines bears fruitful interest in the shape of less skilled labour required in their use. The same thing may be observed throughout the whole range of mechanical industry. The file, the hammer, and chisel are the primitive tools of the engineer, requiring simple inventive power in their inception, but great skill in their use. The planing machine, by which the same end is obtained mechanically, of producing a flat surface, as was got originally by chipping and filing, required knowledge and skill for its production, but a comparatively small amount of those qualities for its operation. The same thing is true, even to a greater extent, in the case of the still more modern machine tool, the milling machine, which is often attended by boys, possessing no mechanical knowledge whatever, during its production of finished forms such as would have required a highly skilled workman in former days.

The beautiful machines referred to by the author in his paper, examples of which were shown at the meetings, carry the same principle many steps farther. As was remarked, the machine shown for making watch-screws may be said to stand in the same relation to ordinary engineers' machine tools as costly gems to common building stones.

Mr. Hewitt commenced his description by dwelling upon the difficulties experienced by watchmakers in old times, when there was no general standard for dimensions and pitch of screws, or form of thread. Such was necessarily the case with hand-work, but a machine can be depended upon to turn out many thousands of parts exactly similar, so that a screw could be put into a watch made years previously. The advantage, naturally, is most apparent in the case of repairs and renewals. The standard of screws adopted by the Lancashire Watch Company at their Prescot Works, is that recommended by the committee of the British Association, and described in the report of 1882. It is a V-thread of $47\frac{1}{2}$ degrees, rounded top and bottom through $\frac{2}{11}$ of the height, and the pitch is directly related to the diameter of the formula $D = 6P^{\frac{6}{5}}$. In arranging the standard the first business was to make master taps, which were produced on a small screw-cutting lathe specially designed for the work, and having a corrected screw, accurate within very close limits. Taps being thus produced, screw-dies were made to the exact standard. When cut the thread requires hardening, and this causes some amount of distortion, which is corrected by grinding the threads with a soft steel lap charged with diamond dust, the operation being performed in the same lathe that cuts the thread. The die used is simply a tapped hole in the centre of a small thin disc of steel, it being an object to have as little metal as possible surrounding the hole, so as to reduce the distortion produced by hardening. Although the die is not split, the pressure exerted by the die-holder is sufficient to produce a slight modification in the diameter of the screw, and in this way the alteration caused by hardening is corrected. During the discussion this fact was questioned, but Mr. Hewitt says that the statement is absolutely correct. The machine itself is of an intricate design, as may be imagined when it is stated that perfect screws are turned out automatically from the plain rod or wire. There are four hollow spindles through which this wire is fed forward to the operating tools, which are four in number, and are carried on a revolving turret. There is also a further tool for making the slit in the screw-head for the turn-screw. It would be useless to attempt to describe the mechanism of this very ingenious lathe without the aid of elaborate drawings. Indeed, during the discussion several engineers, well skilled in mechanical appliances, confessed themselves unable to follow the train of mechanism, even with the aid of working drawings displayed on the walls of the theatre. It is enough to say that the machine will go on without any attention so long as the wire to form the screw lasts, when it stops of itself.

A short discussion followed the reading of the paper, but no fresh points were raised; the speakers, for the most part, contenting themselves with complimenting the author on the ingenuity of his design.

On the second evening of the meeting, Mr. Dixon's paper, on drilling machines for boiler shells, was read and discussed. The introduction of steel as a material for steam-boiler construction opened up a new era in that branch of industry. When iron plates only were used, a first-class boiler-shop possessed, as the chief part of its plant, simply a punching machine and a pair of rolls for bending the plates; the rest was done by handwork, and that of a highly skilled nature. Now that machinery has superseded the handicraftsman, rivetting is done by most costly and beautifully designed hydraulic apparatus, necessitating in its invention a knowledge of applied science of a high order. Flanging of the immense boiler-plates of the present day is also effected by heavy hydraulic presses. The rolls now used for bending plates have to be designed on true mechanical principles, whilst great advance has been made in drilling machinery. Thus both in the enormous boilers of our large steamships and in the diminutive mechanism of watches, we see the skilled handicraftsman being displaced by automatic machinery. It was soon found impossible to make steel boilers with the same plant that was used for the old type of iron boilers; the difference in the physical properties of the material alone demanded a change in treatment. The softer and less homogeneous iron enabled the rivet-holes to be punched, but it was found that this work done upon steel plates caused a deterioration of the metal; drilling, therefore, had to be substituted for punching. Iron plates were punched in the flat; but it was found that with steel when the holes were made in that way, they often would not go together accurately so as to take the rivets to the greatest advantage, the result being a weak joint. This did not matter so much when steam pressures were low, but with the greater demands made by the marine engineer in producing motive power economically, higher pressures had to be used, and there was no margin for loss in the line of rivetting. It therefore became customary to bend the plates and put them into shape to form the shell of the boiler before making the holes, which were then drilled in position, and were necessarily true. This procedure involved the use of special drilling machines, whilst economy demanded that several drills should work at once in one machine so as to save time and be under the care of only one attendant. The chief object of Mr. Dixon's paper was to describe the most recent of these machines. The drill spindles are carried on supports which bring them to the work, and are adjustable to the varying pitches and angles required. There is a cross-slide which can be raised or lowered for carrying the drills for the circular seams, and this is adjustable so as to suit the varying threads required. There are five drills for this purpose, whilst six more are arranged upon a vertical column upon the opposite side of the boiler shell for operating upon the butt seams. One of the chief difficulties in drilling holes in a built-up shell is the flexibility of the work, which causes it to give way and buckle when the pressure of the drills is brought upon it. So great has been this drawback that it has been found more advisable in many cases to use only one drill at a time, although there may have been four spindles on the machine. Mr. Dixon has overcome this objection in an ingenious manner by an internal support which gives great rigidity to the shell, and enables the larger number of drills to be brought into play at once without their accumulated pressure causing deflection. During the discussion an interesting point arose in connection with this feature. It was said that twist drills which, when properly ground, gave very clean holes and great accuracy of work, could not be used on boiler shells, as they so frequently broke in work. The author said this was perfectly true in ordinary cases, but it was due to the springing of the shell referred to. The statement is corroborated by the fact that twist drills can be advantageously employed on work firmly held on the drilling machine-table, whereas the older form of flat drill would have to be used where rigidity could not be obtained.

A NEW METHOD OF PREPARING PHOSPHORETTED HYDROGEN.

A NEW and extremely simple mode of preparing phosphoretted hydrogen is described by Prof. Retgers in the current *Zeitschrift für Anorganische Chemie.* After reviewing the usual mode of preparing the gas for demonstration purposes, by heating yellow phosphorus in an aqueous solution of potassium hydrate, and the other more rarely employed methods of preparation—such as by the interaction of calcium phosphide and hydrochloric acid, copper phosphide and potassium cyanide, and phosphonium iodide and water—the question of the direct combination of hydrogen and phosphorus is discussed. It appears that the currently accepted idea that ordinary molecular hydrogen does not combine with phosphorus is founded upon some old experiments of the French chemists Fourcroy and Vauquelin, who state that when phosphorus is melted in hydrogen gas, vapour of phosphorus becomes diffused in the hydrogen, and confers upon it the power of ignition in contact with oxygen without any combination between the phosphorus and hydrogen occurring. In view of the great readiness which, as Prof. Retgers has recently shown, warm hydrogen exhibits to unite with free arsenic, it was considered possible that the reason for the non-combination of hydrogen and melted phosphorus might be found in the low melting-point (44°) of the latter. Experiments were therefore made with red phosphorus, which, of course, is capable of being raised to a much higher temperature. When dry hydrogen is led through a glass tube containing red phosphorus, and afterwards through a wash-bottle containing water, practically pure hydrogen is found to escape. Immediately, however, a gas flame is brought under the part of the tube containing the phosphorus, combination occurs, and the gas issuing from the wash-bottle at once inflames in the air. The non-spontaneously inflammable gaseous hydride of phosphorus is also therefore accompanied by a smaller quantity of the spontaneously inflammable liquid hydride, and a sufficient quantity of the latter for demonstration may be isolated by leading the vapours through a U-tube immersed in a freezing mixture. Moreover, the solid hydride is likewise produced as a yellow deposit near the heated portion of the tube. Upon removing the flame from beneath the tube, the bubbles of escaping gas cease to take fire as they emerge into the air, and are found to consist of almost pure hydrogen. The production of phosphoretted hydrogen is consequently entirely dependent upon the elevation of the temperature considerably above the melting point of ordinary yellow phosphorus. The new mode of preparation is recommended by Prof. Retgers as being more convenient and elegant than the old-established method of boiling phosphorus in caustic potash, as forming an excellent example of the direct combination of two elements, and as furnishing ample demonstration of all three hydrides of phosphorus, the gaseous, liquid, and solid.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE.

OXFORD.—The Rolleston Memorial Prize has been awarded to M. S. Pembrey, of Christ Church, and E. S. Goodrich, of Merton College, the papers sent in by these two candidates having been judged to be equal. Mr. Pembrey was placed in the first class in the Honour School of Natural Science in 1889, and is demonstrator in the Physiological Department. Mr. Goodrich is still an undergraduate, and is assistant to the Linacre Professor. At a meeting of the Junior Scientific Club, held on Friday, October 26, Mr. W. J. Waterhouse, of Christ Church, exhibited some telephone cables, and Mr. W. P. Pycraft exhibited some Natterjack toads. Papers were read by Mr. W. Garstang, of Lincoln College, on some modifications of the Tunicate pharynx induced by the violent ejection of water, and by Mr. C. T. Blanshard, of Queen's College, on the genesis of the elements.

Sir Henry W. Acland, K.C.B., has announced his resignation of the Regius Professor of Medicine, the resignation to take effect at the end of the present year. Sir Henry is now in his eightieth year, and has long been a leading figure in scientific and medical matters in the University. His resignation will sever the link of many old associations. He has consistently and bravely supported the cause of science in Oxford, and that, too, at a time when scientific studies were regarded in anything but a favourable light by the rest of the University. It was largely due to his influence and energy that the University Museum was built, and he has never failed to support any movements for its further extension and for the improvement of the teaching which is carried on there. One of his latest efforts secured the building of the new Department of Human Anatomy, and he has had the satisfaction of seeing the medical school for which he worked so hard rise from almost nothingness into considerable dimensions, with every prospect of steady and healthy increase. He will carry with him on his retirement the affection and good wishes of all sections of the University.

SCIENTIFIC SERIALS

IN the *Nuovo Giornale Botanico Italiano* for October, Sigg. G. Del Guercio and E. Baroni describe the disease of Italian vines caused by a Schizomycete, and known as *gommosi bacillare* or *gelivure.*—Sig. C. Massalonga describes a large number of abnormal growths in different plants.—All the other papers concern the local Italian flora.

IN the *Journal of Botany* for August, September, and October, Mr. F. J. Hanbury adds seven more to the interminable list of new species of *Hieracium.*—Rev. E. S. Marshall describes and figures an apparently new species of *Cochlearia*, *C. micacea*, from Ben Lawers.—Messrs. J. G. and E. G. Baker discuss the botany of an interesting corner of Westmoreland, High-cup Nick.—The Ericaceæ and the Asclepiadeæ of South Africa are treated of, respectively, by Mr. H. Bolus and Mr. R. Schlechter.—Students of the local distribution of plants in Great Britain will find other papers to interest them.

Symons's Monthly Meteorological Magazine, October.—Protection from lightning, by A. McAdie. This is a summary of one of the Circulars of Information issued by the Weather Bureau, Washington. In addition to a number of rules for erecting lightning rods, the pamphlet contains statistical tables of injury to life and property by lightning in the United States. Full recognition is given by the author of the Report of the Lightning Rod Conference published in 1882, and of the experiments made by Prof. Oliver Lodge.—The recent drought in the Midlands, by the Rev. G. T. Ryves. During 26 days ending September 21, only 0·06 inch of rain fell at Tean Vicarage. Mr. Symons shows that at Barkby, Leicestershire, 1·10 inch of rain fell. The same record shows that the first nine months of

1894 have been dry, but not nearly so dry as in some previous years.—Enormous hailstones, by G. J. Symons. This contains some cuttings from various papers of a severe thunderstorm which occurred over a large part of the continent on August 26 and 27 last. At Beaucourt hailstones are said to have been picked up weighing nearly two pounds; at many places they weighed seven ounces and upwards, and many birds and some sheep were killed.—Climatological table for the British Empire for the year 1893, by G. J. Symons. The table contains data referring to temperature, rainfall, &c., at eighteen places. The highest temperature in the shade was 108° at Adelaide, on February 2, and the lowest − 48° at Winnipeg, on February 1. The highest temperature in the sun was 171° at Trinidad, which also had the greatest rainfall, viz. 92·5 inches; the least fall was in London, 19·8 inches.

SOCIETIES AND ACADEMIES.

PARIS.

Academy of Sciences, October 22.—M. Lœwy in the chair.—Experimental verifications of the theory of weirs, with liquid sheets submerged below or adherent, relative to the delivery and the contraction in the lower part of the liquid sheet, by M. J. Boussinesq.—M. A. Trillat claims priority in regard to processes of disinfection by formaldehyde.—On the rotation poles of Venus, by M. C. Flammarion (see p. 21). Variations of the level of water in a basin communicating with a tidal port, by M. A. de Saint-Germain. A mathematical paper.—Force acting at the surface of separation of two dielectrics, by M. H. Pellat. In the general case, the force is normal to the surface of separation and in the sense that the specific inductive capacity diminishes. Its value per unit of surface is given by the formula

$$f = \frac{K_1\phi_1^2 \cos 2\alpha_1}{8\pi} - \frac{K_2\phi_2^2 \cos 2\alpha_2}{8\pi},$$

α_1 and α_2 being the angles between the normal and the direction of the field, ϕ_1 and ϕ_2 the intensities of the field, on either side of the surface of separation.—Experimental researches on the freezing-point with different mixtures of alcohol and water, by M. Raoul Pictet. A table of the temperatures of crystallisation of definite mixtures is given, and the results are plotted in curves discussed in the paper.—A study of the combinations of hydrogen fluoride with water, by M. R. Metzner. The author has succeeded in obtaining only one hydrate possessing definite properties. It has the composition $HF.H_2O$ and contains 52·3 per cent of HF. The crystals of this composition melt at − 35° C.; they fume in the air, and have a specific gravity greater than 1·15. They are very soluble in the cold concentrated acid.—Researches on the mercuric sulphates, by M. Raoul Varet. The thermal data for the normal sulphate and for the basic salt $HgSO_4 . 2HgO$ are given in detail. Whereas sulphuric acid completely displaces HCN from its combination with potassium liberating + 25·4 Cal., hydrocyanic acid, even in dilute solution, replaces sulphuric acid in $HgSO_4$ with disengagement of + 23·5 Cal. Similarly hydrochloric acid displaces sulphuric acid in $HgSO_4$.—Antimony vermilion is not an oxysulphide, by M. H. Baubigny. Analysis of the colouring matter of antimony vermilion, precipitated by sodium thiosulphate, shows that it is simply a form of Sb_2S_3.—Bismuth nitrosalicylates, by M. H. Causse.—Salivary glands of the Apinæ (*Apis mellifica* ♂ and ♀), by M. Bordas. On an undescribed caterpillar ravaging the leaves and fruits of the fig-tree, in the arrondissement of Puget-Théniers, by M. Decaux.—On the mechanism of vegetable respiration, by M. L. Maquenne. The author shows that the ratio of CO_2 produced to O absorbed is sensibly altered by momentarily subjecting leaves to a vacuum, and the respiration is at the same time rendered more active. The conclusion is given: The respiration of plants appears to be the result of the slow combustion of a very oxidisable principle, which the living cell constantly secretes, shaded from the light, and which may accumulate when there is insufficient oxygen in the surrounding atmosphere.—The station of Schweizersbild, by M. Nüesch.—Three geological sections in French Congo, by M. Maurice Barrat.—Late geological researches in the Altai, by M. Vénukoff.—Rotation movements observed in an aerostatic ascension, by M. Vénukoff.

DIARY OF SOCIETIES.

LONDON.

THURSDAY, NOVEMBER 1.

LINNEAN SOCIETY, at 8.—Contributions to the Knowledge of Monocotyledonous Saprophytes: Percy Groom.—On an Error in the Descriptions of the Effect of a Centrifugal Force upon Growth: Rev. G. Henslow.—On Mediterranean and New Zealand Retepora, and a Fenestrate Bryozoan: A. W. Waters.

CHEMICAL SOCIETY, at 8.—The Electromotive Force of Alloys in a Voltaic Cell: A. P. Laurie.—The Action of Nitric Oxide on Sodium Ethylate: G. W. Macdonald and Orme Masson.—On Ethylic Butanetetracarboxylate: Dr. B. Lean.

MONDAY, NOVEMBER 5.

SOCIETY OF CHEMICAL INDUSTRY (Burlington House), at 8.—The Composition and Constitution of certain Alloys, by the late Dr. C. R. Alder Wright, F.R.S.: Mr. Watson Smith.—Note on Oxidised Linseed Oil: Mr. W. F. Reid.

ARISTOTELIAN SOCIETY (22 Albemarle Street), at 8.—An Essential Distinction in Theories of Experience: Mr. Bernard Bosanquet.

TUESDAY, NOVEMBER 6.

ZOOLOGICAL SOCIETY, at 8.30.—Descriptions of New Species of Œdionychis and Allied Genera of Coleoptera: Mr. Martin Jacoby.—On the Hyoid Arch of Ceratodus: Mr. W. G. Ridewood.—Third Report on Additions to the Batrachian Collection in the Natural History Museum: Mr. G. A. Boulenger, F.R.S.

ROYAL VICTORIA HALL, at 8.—The Electric Spark: Prof. A. W. Rücker, F.R.S.

WEDNESDAY, NOVEMBER 7.

GEOLOGICAL SOCIETY, at 8.—Notes on some Recent Sections in the Malvern Hills: Prof. A. H. Green, F.R.S.—The Denbighshire Series of South Denbighshire: Mr. Philip Lake.—On some Points in the Geology of the Harlech Area: Rev. J. F. Blake.

ENTOMOLOGICAL SOCIETY (11 Chandos Street, Cavendish Square), at 8.

THURSDAY, NOVEMBER 8.

MATHEMATICAL SOCIETY, at 8.—Mathematics, President's Address: A Generalised Form of the Hypergeometric Series, and the Differential Equation which is satisfied by the Series: F. H. Jackson.—Third (and concluding) Memoir on certain Infinite Products: Prof. L. J. Rogers.—On the Kinematics of Non-Euclidean Space: Prof. W. Burnside, F.R.S.

INSTITUTION OF ELECTRICAL ENGINEERS, at 8.—Notes on Electric Tramways in the United States and Canada (Supplementary Paper): H. D. Wilkinson.—Electric Traction, with Special Reference to the Installation of Elevated Conductors: R. W. Blackwell and Philip Dawson.

FRIDAY, NOVEMBER 9.

PHYSICAL SOCIETY, at 5.—The Photographic Action of Stationary Light Waves: J. Larmor, F.R.S.—On Vapour Pressure: Prof. S. Young, F.R.S.—On the Luminescence of Glass: John Burke.

ROYAL ASTRONOMICAL SOCIETY, at 8.

SATURDAY, NOVEMBER 10.

ROYAL BOTANIC SOCIETY, at 3.45.

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