THURSDAY, SEPTEMBER 22, 1887.

## *TWO RECENT WORKS ON MICROSCOPICAL TECHNOLOGY.*

*Elementary Microscopical Technology.* Part I. The Technical History of a Slide, from the Crude Materials to the Finished Mount. By Frank L. James, Ph.D., M.D. (St. Louis: *Medical and Surgical Journal* Company, 1887.)

*A Course of Elementary Practical Histology.* By W. Fearnley. (London: Macmillan and Co., 1887.)

THE above are the most recent of the numerous works now before the public on this somewhat restricted subject. The title of the second-named volume is somewhat of a misnomer, as the author deals with pure technology. Both works are for the most part expositions of those well-tried methods which now constitute the basis of the study; as such they differ, as might be expected, but little from their predecessors, except in matters of detail, in methods of treatment, and in literary style: but while this is the case, each volume has, nevertheless, a marked individuality.

Both works contain well-chosen woodcuts, illustrative of the leading apparatus described.

The first manual is a pleasing volume of 106 pages, being the first of a series which the author has found it necessary to prepare for the especial use of his own students. He has aimed at producing a work "in which nothing should be taken for granted, no previous acquaintance, on the part of the student, with the subject-matter presupposed," and we are pleased to admit that he has succeeded in his endeavour. The volume embraces a certain amount of botanical as well as zoological technique, and the most striking feature of it, apart from its general novelty, is the manner in which the several subjects are introduced. Thus, on p. 25, for example, we find the microtome first defined as "a receptacle for holding the material to be cut, a screw or other apparatus for feeding it to the knife, and a razor or knife with a very keen edge." The book is subdivided into fourteen chapters, each abounding in sound sense, and the product of great labour. Easy reading such as this is hard writing, and the author shows throughout a keen appreciation of the precise difficulties which beset a beginner. He guards against laborious idleness (that pitfall of the histologist) by giving the *rationale* of most of the complicated processes which he adopts; and his work leaves on the mind the impression that the various methods are "to be learned from experience and practice only, aided by the experience of others in similar cases," and that "experience, after all, is the great teacher, and the knowledge that is to guide one in doubtful cases is rarely to be obtained from text-books and manuals, no matter how elaborate, practical, and complete they be. They can only point the way, but individual experiment alone can make the successful worker." We shall look with interest for the continuation of this excellent work.

The last-named work is the most recent of the "Manuals for Students," which are so familiar in our laboratories and class-rooms; and it is, in many respects, a most remarkable book. There are in all 360 pages, and the author subdivides the whole into two parts, with an appendix. The first part is devoted to a consideration of apparatus and methods; it contains all that is customary and much that is useful, and it is by no means destitute of originality. The author has set down his experiences in a conscientious and painstaking manner; he states that his work "is intended as much for junior practitioners working in a private laboratory of their own as for medical students so called;" and from a declaration in his preface it is clear that he writes as a private medical man for medical men. Although there is a want of that system to which we are accustomed in text-books by recognized teachers, the book may be useful in extending beyond the usual boundaries the kind of work now universally imparted in our leading schools. It gives evidence of a large amount of honest labour, and there is incorporated within it much sound advice, notably that concerning the choice of a microscope; and it will be no fault of the author's if the student should go astray in the use of a high power. In discussing the immersion-lens an unnecessarily long disquisition is given upon the history and optics of the subject. The remarks offered might be advantageously condensed and re-placed in a footnote, giving references to the authorities cited; whereas, on the other hand, descriptions such as those given of the sub-stage condenser (p. 5) and of the camera lucida (pp. 24–25) are wholly insufficient, when it is considered that the author professes to write "for those who know little or nothing of the instrument." We see no reason why 1 per cent. solution of silver nitrate in distilled water need be placed in a bottle surrounded by black paper.

The second part is unique in construction, since it consists, in the main, of 145 pages of thick paper for the most part nearly blank. It is devoted to the enumeration of title-heads of those objects which the student is directed to examine, together with scant directions for so doing. By far the greater part, however, is given up to a muster of technical words, which under the head of "definitions of terms," the author would presumably have the student fill in, in school-boy fashion, for committal to memory. Novelty this, unexpected but unwarrantable, as it leads to a waste of valuable time and good paper. In giving directions for drawing under the microscope, the author advises (p. 28) that the typical parts be filled in—after getting from the text-book "all information about these." We have here something akin to an inversion of the order of procedure which experience and common-sense alike dictate. Whatever may be the success of this volume, it will remain memorable for its striking originality of style. To begin with, the inventor of a method or of a reagent is exalted to the dignity of a discoverer; and, to proceed, we read (pp. 10–11) that "if the student hesitates as to choice [of dry lenses recommended] he had better adopt the street urchin's mode of settling the matter by tossing up one of the coins of the realm"; while, by way of a novelty, we are informed (p. 61) that "this almost universal desideratum of the physiologist [anæsthetizing] is carefully concealed by professional anti-vivisectionists who obtain their livelihood by harrowing the feelings of the public." Perusal of almost every page of this very remarkable book furnishes similar eccentricities; but it must be remembered that it is intended

"for students of all denominations who can command the means and have the wish to construct for themselves a histological cabinet," and that it has been produced "between the numerous and unavoidable interruptions of a family medical practice." G. B. H.

## OUR BOOK SHELF.

*Precious Stones in Nature, Art, and Literature.* By S. M. Burnham. (Boston: Bradlee Whidden. London: Trübner and Co.)

MR. BURNHAM is the author of a work on limestones and marbles published a few years ago in which he indicated the resources of the United States and other countries in stone for decorative purposes. In the present volume he treats of precious stones in that exhaustive and thorough fashion which we are accustomed to regard as a special characteristic of German writers. He begins by describing, as far as is known, the origin, properties, classification, localities, imitations, and antiquity, of precious stones (antiquity here applies of course to their use as ornaments), and then proceeds to treat of their prices, the trade in them, the sumptuary laws relating to them, those of remarkable size, and notorious jewel robberies. This chapter is followed by a description of various notable collections, and of the Crown jewels of different countries, from which the author passes on to some very interesting chapters on the secular uses of precious stones, the different kinds of ornaments, and their sacred uses. A chapter on precious stones in literature, and their mystical properties, is succeeded by one on the curious art of engraving on precious stones, and then commences a series of chapters on the various stones. First, of course, comes an account of the diamond, its home, and of historical and remarkable diamonds, which is followed by descriptions of all the precious stones at present known, from the sapphire, emerald, and ruby, to coral, amber, jet, cat's-eye, and rock-crystal, to the number of about one hundred. The appendices give the sizes of large and remarkable diamonds, a classification of precious stones according to their principal constituents, the hardness and specific gravity of precious stones, their relative hardness, relative specific gravity, and finally a list of the localities in the United States in which gem-minerals have been found. It will be perceived from this very brief indication of the contents of the book that the work is perfectly encyclopædic in its treatment of its subject; nothing relating to precious stones is strange to or disregarded by Mr. Burnham. Of the value of the book to the gem collector, expert, or mineralogist, it is needless to speak, but we can answer for it that it is highly interesting to the general reader, or at least to all who like to hear about those rare and beautiful products of Nature to which man in all ages and in every country has attached a high value.

*Hydrophobia: An Account of M. Pasteur's System.* By Renaud Suzor. (London: Chatto and Windus, 1887.)

DR. RENAUD SUZOR is the delegate commissioned by the Government of the colony of Mauritius to come to Europe to study M. Pasteur's treatment of hydrophobia, and this volume is the result of his mission. It is greatly to the credit of Sir John Pope Hennessy, the Governor of that colony, and of the members of the Legislative Council, that they perceived the value to science and humanity of adequately studying M. Pasteur's recent discoveries on the subject of hydrophobia, and that they "unanimously voted" the appointment of a delegate to proceed to Paris to work under the distinguished discoverer. It is to be hoped that other and more prominent colonies may be led to follow this excellent example. This little volume amply justifies the selection of Dr. Suzor as delegate. It opens with an historical account of hydrophobia and its treatment from the earliest times—for this dreadful disease has been known and studied for more than 2000 years—down to the end of 1880. The second and principal part of the volume is occupied by translations of all M. Pasteur's communications on the subject to the Academy of Sciences, beginning with his first note in January 1881, and ending with a lengthy paper presented in November 1886. Finally, there is a description of the technique of M. Pasteur's method. The book is valuable as a clear and comparatively untechnical exposition of the Pasteur method; but it is still more valuable as an example of the manner in which Pasteur's wonderful discovery should be met and treated by Governments and others in authority, who are responsible for the prevention, as far as possible, of disease amongst the populations which they govern. The Governor of Mauritius has taken care that the neglect of this primary duty, in relation at least to hydrophobia, cannot be laid to his charge.

## LETTERS TO THE EDITOR.

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

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

### A Monstrous Foxglove.

A SOLITARY specimen of *Digitalis purpurea* was found last month in a damp wood near Old Colwyn, North Wales, which exhibited the following curious abnormalities in the structure of its flowers. In only one out of the six opened flowers of the raceme was the calyx normal (*i.e.* consisting of four broad and one narrow segment); in all the others it was divided almost to the base into five equal linear segments. The corolla in four out of the six flowers consisted of merely two narrow petals with long claws, placed at opposite points on the receptacle; in one flower these two distinct floral leaves were deeply divided into two and three lobes respectively, thus forming a perfect two-lipped flower, the lips, however, being quite separate from one another. In the only other flower the upper lip was altogether wanting, the three-lobed lower one alone being present, upon which, alternating with its lobes, were inserted one long and one short stamen. This was the only flower which possessed stamens.

The form and number of the styles also was variable and abnormal. In one flower only was the usual single shortly two-cleft style met with; two other flowers possessed each a single style forked below the middle; in two others there were two, and in the remaining flower three styles, all separate and similar.

The same abnormalities were seen in the corolla and styles of two unopened buds.

I should be happy to learn if such monstrous forms are at all usual in the foxglove. F. R. TENNANT.

Longport, Staff.

### The Law of Error.

DR. VENN, in a letter published in NATURE, September 1 (p. 411), adduces certain meteorological statistics which do not conform to the typical law of error or probability-curve. To discover the cause of this failure there would be required both a special knowledge of the subject-matter and the general conceptions which the calculus of probabilities supplies. The latter qualification is the only one to which the present writer can make any pretension.

The essential condition of the typical law being fulfilled is that each observation or statistical return should be made up of a great number of independent variable items. A good example is afforded by taking a great number, *e.g.* 100, digits at random from mathematical tables. The sums of that number

of digits will fluctuate about the mean 450 according to a probability-curve whose "probable error" is about 19.

(1) One explanation of the failure of the law is that the requisite plurality of items is wanting. Suppose we had taken sums of *two* (instead of a hundred) digits, the grouping of these sums would be best represented by a right line, or rather two right lines. If we took three digits at a time, the resulting form would be parabolic. A variant of this class of exception is when the larger items are few or unique, while items of an inferior order congregate in great numbers. Suppose each observation to consist *either* of the digits 3 or 6, *plus* ten items taken at random from the series '1, '2, . . . . '9. There would then be generated a curve like those in Dr. Venn's Fig. 2. If, instead of 3 and 6, we had two digits, 4 and 5, differing by very little from each other, the abnormal uniqueness of the larger items would be disguised. It is upon this principle, doubtless, that the population of a kingdom appears to conform (in respect of height or other attribute) to the law of error, while at the same time each province may present a distinct type. Suppose that the majority of our returns were, as the last-mentioned case, *either* 4 or 5 *plus* an aggregate of smaller items; but that a small proportion of the returns were governed by a widely disparate "large item," *e.g.* 8 or 9; in this case we might have the appearance presented by Dr. Venn's Fig. 1. The body of the curve would seem to be of the probability family; but there would be tacked on a tail appertaining to a different type. Dr. Charles Roberts has adduced some statistics of this species in a paper published in the *Medical Times*, February 7, 1885.

(2) We have hitherto supposed that the constituent items have no bias in one direction. Suppose, however, that instead of the digits 1, 2, . . . . 8, 9 being each equally eligible, 8 and 9 became inadmissible; and, whenever one of those digits was presented, we had to substitute 6 and 7 respectively. There would thus be two chances in favour of 6 and also of 7. An aggregate of 100 digits each selected according to this unsymmetrical scheme would be grouped about the mean value $10 \times (1 + 2 + 3 + 4 + 5 + 2 \times 6 + 2 \times 7)$, or 410, in a form which as to the body of the curve would be a probability-curve, but which would be unsymmetrical at the extremities. The most familiar example of this case is afforded by games of chance. If black and white balls, in an unequal proportion and immense numbers, are mixed up, then if you take at random batches of 100 (or 1000) balls the percentage of white or black balls will fluctuate in the manner described. It is quite possible that this principle should govern what Dr. Venn calls a "one-ended phenomenon," *i.e.* one in which unlimited variation is conceivable in one direction but not in the other. Dr. Venn's Fig. 1 seems fairly well to represent a biased probability-curve.

(3) We have hitherto supposed that the individual observation or return is the *sum* of the variable elements. But it may be a more complicated function. Thus it may be a product. The *logarithm* of the observations may fluctuate according to a probability-curve, while the observations themselves obey a law which has been investigated by Dr. Macalister in the Proceedings of the Philosophical Society (1879); related to the geometrical mean just as the probability curve is to the arithmetic mean. This grouping is to be expected wherever the analogies of *Fechner's law* prevail. This may be the rationale of the fact which I have elsewhere pointed out, that fluctuations of price rise much higher above, than they fall below, the mean. But, where the principle of estimation does not come in, it is not quite clear why the geometrical curve should be more appropriate to a "one-ended phenomenon" than the biased probability-curve which has been described under our heading (2). At any rate, in the case before us, Dr. Venn's Fig. 1, the numerical statistics which he has allowed me to inspect show much too close a correspondence between the body of the figure and the probability-curve to admit of the geometrical explanation. There is also this peculiar difficulty, that the longer limb of the given curve is the lower one.

F. Y. EDGEWORTH.

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## A Null Method in Electro-calorimetry.

BY reference to the last number of the *Electrical Review* (vol. xxi. p. 262), wherein is printed a short abstract of our paper on "A Null Method of Electro-calorimetry" read before the British Association on September 1, Mr. Huntly will find that the method of measuring specific heats suggested by him is in principle similar to that described by Mr. Gee and myself. The method has been employed for determining specific heats during the last two years, but we have delayed publication till the best working details of the method have been elaborated.

In certain practical details our method differs from Mr. Huntly's suggestion. The mass of liquid in each calorimeter is *not* the same. It is much preferable to have the masses inversely proportional to the specific heats, so that the thermal capacities of the liquids are equal. In this way it will be readily understood that the correction for radiation can be made to disappear altogether. For since the calorimeters are precisely equal, and their temperatures equal, the loss of heat by radiation must be the same from each; further, since the thermal capacities of the liquids are the same, as well as that of vessels and stirrers, it follows from the equality of the resistances that the same current will produce the same rise in temperature in each case, and conversely, since the heat radiated from each calorimeter is the same, and since the thermal capacities of the calorimeters and stirrers are equal, it follows that, if the same current traversing the equal resistances produces the same rise in temperature in each liquid, the thermal capacities of the two liquids are the same, whence the specific heat can at once be determined by determining the masses of the liquids. Virtually, then, the null method of obtaining the same rise of temperature in each calorimeter is attained by varying the mass of liquid in either or both calorimeters. In practice we approximate as nearly as possible to the condition by adding liquid to that calorimeter which rises in temperature most quickly, and then make a final adjustment by shunting a *very small fraction* of the current by means of the high resistance in the box. This is, we believe, the first time that a method for measuring specific heats has been published in which the correction for radiation and for the thermal capacity of calorimeters and stirrers has been entirely eliminated.

With the first apparatus we had made to embody these ideas, viz. that described in the *Electrical Review* (*loc. cit.*), an accuracy of at least one-tenth per cent. could be obtained from a single experiment, thoroughly confirming Mr. Huntly's anticipations as to the delicacy of the method. We have just introduced some considerable improvements in the apparatus which we hope will enable us to insure much greater accuracy than that hitherto obtained.

A few words are required in reply to some observations of Mr. Huntly. First, he suggests a bolometric method of determining the difference of temperature. We have so far preferred a thermo-electric method, which, without a specially constructed galvanometer, enables us to detect with certainty 1/2000 of a degree; the necessary corresponding variation in the resistance of a Pt wire would only be 1·6 parts in a million; besides some difficulties may arise in procuring two pieces of Pt wire which shall have the same temperature coefficient to 1 part in a million, even if they be cut from the same piece originally. Secondly, the time method described by Mr. Huntly at the end of his paper seems to me to have a fatal objection: it would be quite impossible to keep the current constant for a long time to the 1/2000 part which would be requisite to secure such accuracy as we can get with present arrangements.

WILLIAM STROUD.

## Mental Development in Children.

I SHOULD like to hear the opinion of psychologists on the following circumstance:—A female child, quick and intelligent, when about fifteen months old, learned to repeat the alphabet, shortly afterwards the numerals, days of the week, month, &c., and, subsequently, scraps of nursery rhymes, English and German; then to spell words of two and three letters. All this was learned readily, eagerly indeed, and for a time she remembered apparently every word acquired, indelibly. At about two years old further teaching was for a time remitted, as she was observed to be repeating audibly in her sleep what she had learned during the day. Subsequently, tuition was resumed, under a governess, but she had not only forgotten much of what she had previously known perfectly, but learns far less readily than formerly. She is now about three and a half years old, in perfectly good health and spirits, quick, and particularly observant, but the capacity for learning by rote is materially diminished; she is remarkably imitative, but shows no faculty whatever for writing, and as little for music.

I should like to hear of any parallel cases, and what the ultimate development has been; with any opinions upon the cause of their appearances.

M A.

September 18.

## *FIFTY YEARS' PROGRESS IN CLOCKS AND WATCHES.*[1]

II.

TO pass on to another phase of mechanical improvement, a wonderful advance in the mechanism of chronographic watch-work has been made during the period we refer to. In this department the first chronographs to be introduced were those having a kind of double hand, the lower portion of which carried a tiny vessel of ink. When an observation was requisite, the upper part of the hand passed through a small orifice in this ink-vessel, marking a dot upon the dial below. We have had of late years, however, much cleaner and more convenient arrangements. The most usual form is as follows. In addition to the ordinary minute and *centre*-seconds hands there are auxiliary hands, which always stand at zero when not moving. Pressure on the crown-piece sets them going, a second pressure stops them, and the third pressure sends them back to zero; and it is interesting to observe that they always return to zero—their normal position—the shortest way round the dial. The nature of the mechanism by which these operations are effected is briefly as follows. Pressure on the crown-piece causes a wheel carrying different sets of cams to advance step by step. These cams, which correspond to the starting, stopping, and returning of the hands, operate on springs and levers. The first motion frees the auxiliary hands, and also throws them into gear with the watch-train. The second motion throws them out of gear and clutches them so that they shall not shift. The third motion sends them back to zero, and this is effected in the case of both by what is called a heart-piece. This heart-piece, as regards the seconds-hand, is shown in outline at the centre of Fig. 8,[1] which has already appeared as Fig. 5 in the first article. It is to be mentioned

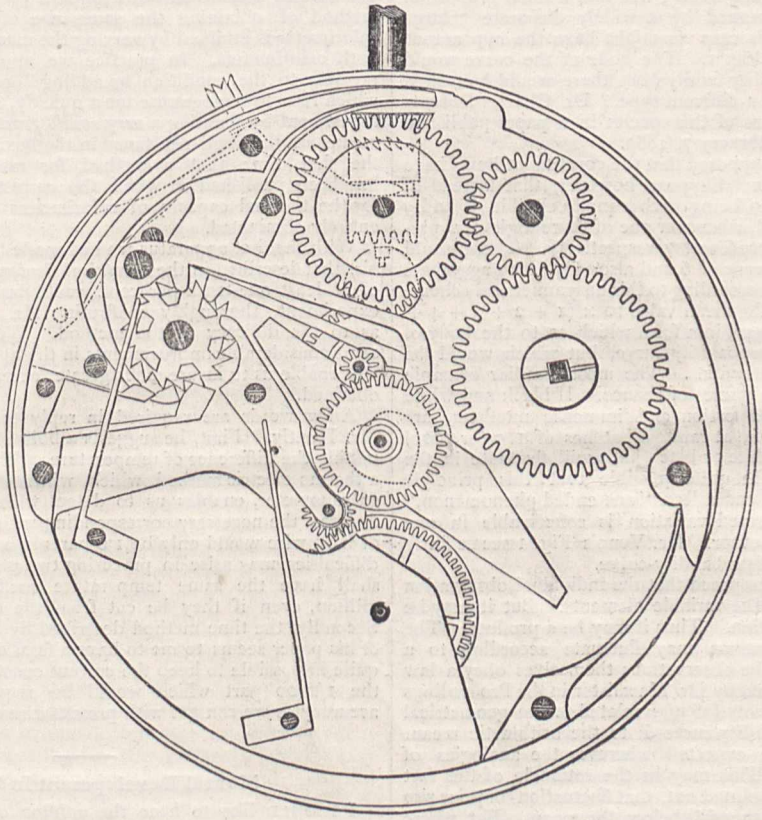

FIG. 8.—Chronograph with Swiss Keyless work.

that the heart-pieces go round with their respective hands. The third pressure releases the clutches and also causes the lever, shown above the heart, to descend upon it; the heart and hand being now free to move, the lever draws round the heart until it finds the lowest position of it, which, as is natural, is arranged to correspond to the normal position of the hand. The gearing-wheels and clutch-levers can be very well seen in Fig. 9.

In another form of chronograph a long seconds-hand is superimposed over another so as to appear as one with it. They both travel with the watch-train; until a first pressure stops one, and a second pressure the other; the interval between the two pressures can now be read off at leisure. A third pressure sends them flying to the place where they would have been if they had not been stopped at all, even should they have been kept standing for a week. They, also, always return the shortest way. The mechanical arrangements consist of a heart-piece for bringing the hands together, and they achieve the position where they would have been had they not been stopped by means of a kind of cylinder sliced through at an angle not perpendicular to its axis. Whilst the hands are travelling, the faces where the cylinder is cut are kept pressed together by springs, but they are parted when the hands are brought to rest. One half of the cylinder goes on with the watch-train, the other half (in connexion with the hands) remains suspended above it; at the third pressure of the crown-piece the top half is permitted to descend, when it naturally seeks its former position with

[1] Continued from p. 595.

[1] We are indebted to Mr. Britten for the use of Figs. 5, 9, 10, 11, 12, and 15, and to Mr. Glasgow and the Messrs. Cassell for Figs. 13 and 14. Readers who wish further insight into the details of our subject should consult both Mr. Britten's and Mr. Glasgow's books.

respect to the other, which has been permitted to go on with the watch-train.

The form of mechanism which is applied for the purpose of maintaining and showing a calendar has undergone considerable development. Calendars are now made to be perpetual, correcting themselves for everything, including leap-year. The following (Fig. 10) is the plan generally adopted in clocks, and is the invention of the late M. Brocot. M *m* is a lever which is worked by a pin, *e*, in a wheel in the clock-train going round every twenty-four hours. As *e* advances in the direction indicated by the arrow, M *m* is moved to the left, and the clicks G and H, which it carries, pass over the top of a single tooth each of the wheels A and B, the wheels being meanwhile held loosely in position by weak springs called "jumpers." As soon as *e* has passed the end of M *m*, the latter falls back by its own weight, dragging back A and propelling B each one tooth respectively. B has seven teeth, and works the days of the week; A has thirty-one, and serves for the days of the month. All months, however, have not thirty-one days, and provision is made for the difference by a supplementary thruster, N. In A there is a pin, *i*, which comes regularly below N every twenty-eighth day. The tail of N rests against a wheel, V F, which goes round once in four years. V F is graduated with notches of different depths. These notches correspond to the respective lengths of each month, and those representing the Februaries are conspicuously noticeable; that one which is the shallowest of the four identifying the leap-year. During the months of normal length, N maintains the position which is shown dotted in the diagram, and does nothing. But whenever there occurs a short month, the tail of N will enter one of the notches; in consequence, N will descend, and, engaging *i*, propel A forward a day or more, depending upon the depth of the notch. This happens whilst M *m*

FIG. 9.—Chronographic watch-work.

is travelling to the left. When M *m* falls back, the click G will act in addition, and as usual. Fig. 11 shows the dial; the hands on dials right and left are in connexion with A and B (Fig. 10). The hand upon the lowest dial shows the month of year; its progression is continuous. The hand at the top shows the equation of time, and alternates on each side of noon, + or −, as may be required. It is worked by a rack which reposes against a cam of suitable form revolving once in twelve months.

The phases of the moon are indicated (as may be seen in the diagram) by the passage of three shaded disks across a circular aperture.

Magnetism exercises the most destructive influence upon watches or chronometers, turning their balances into compass-needles, and causing the coils of their balance-springs to stick together.

In these days of large magnetic engines it has therefore been found necessary to revert to an idea of the elder Arnold, and to construct watches for the use of those having to do with such engines upon a plan which shall render them indifferent to magnetization. This result is obtained by making the balances of silver and platinum, or an alloy of iridium, or of some other non-magnetizable material, and the balance-springs of gold or palladium; and the use of steel is avoided in certain parts of the escapement. Watches carefully constructed on such plans give results little inferior, as regards time-keeping, to others.

Amongst the multifarious purposes to which clocks have recently been applied, we must not omit to mention those which are designed for registering the proper performance of a watchman's duty. The old-fashioned principle was that there should be a separate clock at

each station the watchman had to visit, and by pulling a string or handle he was enabled to leave record of his presence. Now, either he carries the clock (or large watch) with him, or else it is fixed at a central station, and is operated upon by electricity. In the first case there is a revolving paper dial inside the clock, and by placing the clock within specially arranged orifices at the

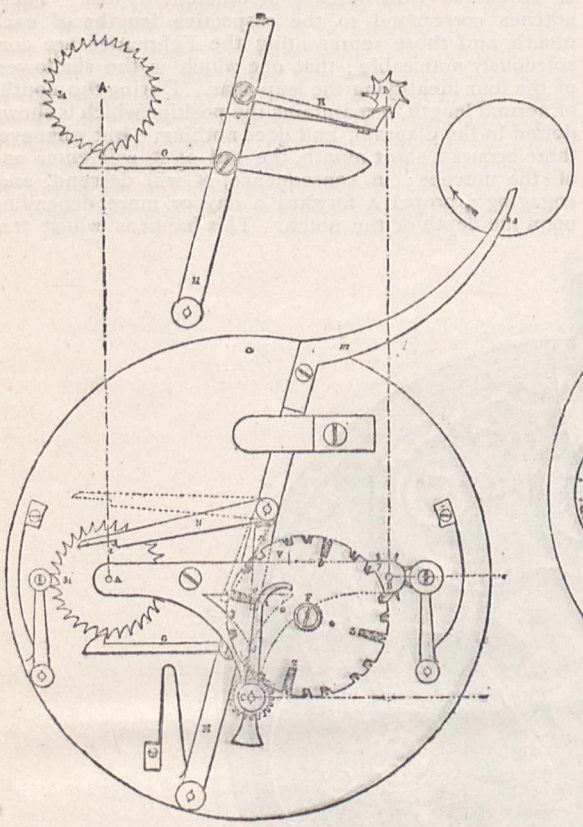

FIG. 10.—Brocot's Perpetual Calendar.

different stations he has to visit, he is enabled to get printed off upon the paper dial a mark or letter showing the time at which he was at the station. In the latter case the clock is provided with a large drum or cylinder, and wires lead to it from the different stations; and when a button at any station is touched, a mark follows upon the cylinder, indicating the where and when of the person who made it. At one large lunatic asylum the system is so perfect that the night superintendent, sitting in his own room, can follow the movements and whereabouts of all his men. Clocks have also been designed for registering the gross aggregate or integral of daily temperatures or barometric pressures. In the former case a watch is used, and has a balance compensated the wrong way, so that the effects of changes of temperature are magnified. In the latter case a barometer is used as the pendulum.

Until three years ago there was no public institution in Great Britain where a serviceable authentic trial of the performance of a watch under varying conditions as regards temperature and changes of position could be obtained. At that date, however, under the auspices of the Royal Society, a department of the Kew Observatory was established for the purpose. It satisfied a want which had long been felt, and provided with every requisite for

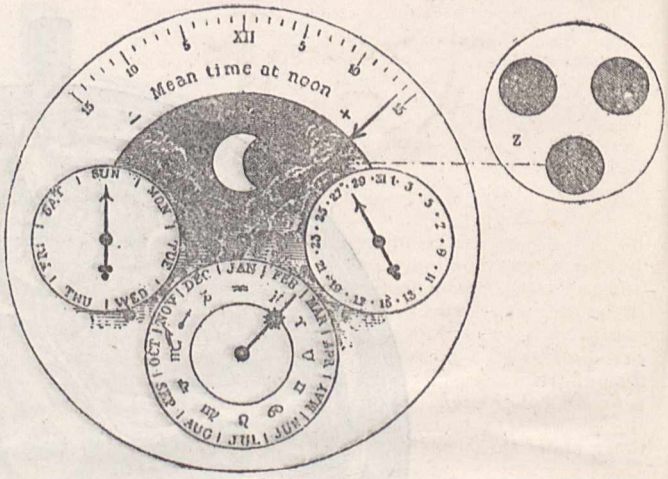


FIG. 11.—Dial of Brocot's Perpetual Calendar.

timing both in temperatures and positions, a considerable and increasing number of watches are regularly sent there for the purpose of obtaining its certificates. In Class A (the first class) merit-marks are awarded in addition to the certificates, in the following proportions: 40 for a complete absence of variation of daily rate, 40 for absolute freedom from change of rate with change of position, and 20 for perfect compensation for effects of temperature.

The subject of the application of the balance-spring, and the process of timing, which is subsequent, must be

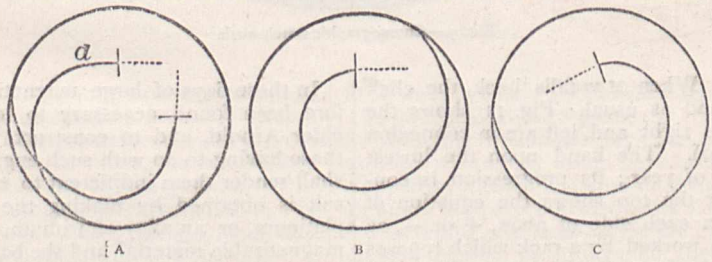


FIG. 12.—Diverse forms to which balance-springs are fashioned.

reckoned beyond the scope of the present article. But we may briefly allude to the fact that the causes which operate upon a watch to make it keep different times in different positions are generally three. For instance, the balance may be out of poise, the balance-spring may not be isochronous, and the action of the escapement generally is irregular in different positions. Putting the balance in poise is done roughly in a poising tool; the finer adjustment follows the results of trials when the watch is kept going in different positions. Isochronism is more important and more difficult of attainment. Without isochronism a watch might keep tolerably near time when placed successively 12, 3, 6, or 9 upwards, and still possess a very wide error between all these and the

dial up (flat) position. Want of isochronism would also cause it to vary its rate considerably as time went on. Isochronism is obtained by a careful adjustment of the weight of the balance to the motive power; and by suiting the length, number of coils, and forms of the curves at the terminations of the balance-spring to circumstances, as may be required. For example, A, Fig. 12, shows the contour of the curves which terminate the spring of a marine chronometer; B and C, the contours of a pocket chronometer spring. It must not be supposed that all marine or pocket chronometer springs are alike. The correct form is generally arrived at after prolonged trial and patient fashioning.

Technical education has not been neglected in recent years by English watch-makers. Indeed, the necessity

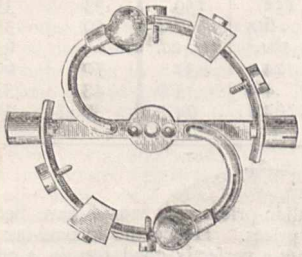

Fig. 13.—Loseby's Balance.

for it has been too keenly felt to allow them to forget it. But for a long time there was nobody to help or even to advise them. Under such conditions a small party took the matter into their own hands, and founded the Horological Institute. With very little encouragement they at first worked on, but have now the satisfaction of seeing their efforts successful to an extent which they could have scarcely anticipated. Workshops, science and drawing classes are to be found at the Institution; and examinations, under the auspices of the City and Guilds of London Institute, are periodically conducted, and certificates of proficiency granted.

Before concluding we give two diagrams which may be of interest. They refer to the subject of secondary compensation, one of them, Fig. 13 (Loseby's), representing

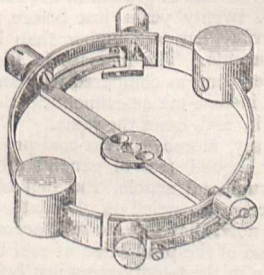

Fig. 14.—Kullberg's Balance.

one of the oldest, and the other, Fig. 14 (Kullberg's), one of the most recent forms of balance for the purpose. It will be seen that Loseby's object was effected by means of curved mercurial thermometers—the lower the temperature the more indirectly the mercury receded from the centre, checking the action of the compensation: with Kullberg's the supplementary compensation screws are checked directly.

There have been many improvements in the lever escapement. Fig. 15 shows one of the most remarkable. In this case the discharging is effected by means of two pins in the roller, and the impulse given by means of a pin in the lever working into the notch on the roller. The effect is that the unlocking takes place at about the line of centres, and the impulse is given more advantageously. Resilient escapements are those which will enable the watch-balance to make several turns in the same direction without injury to the escapement. They often save a breakage in the case of a blow or jerk; their invention is due to Mr. Cole. We ought not to close this article without mentioning the fact that the manufacture of the

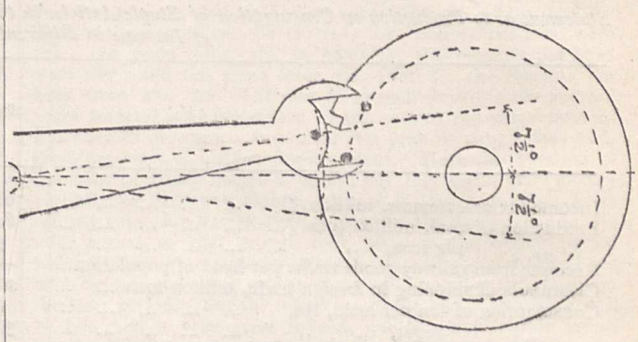

Fig. 15.—Savage's Two-pin Lever Escapement.

duplex escapement, which was at one time reckoned the very first, has been completely abandoned. Besides its liability to stop, it was found that the wear in the pivot-holes made its timing and adjustment exceedingly precarious.

HENRY DENT GARDNER.

## *THE BRITISH ASSOCIATION.*

### SECTION F.

### ECONOMIC SCIENCE AND STATISTICS.

OPENING ADDRESS BY ROBERT GIFFEN, LL.D., V.P.S.S., PRESIDENT OF THE SECTION.

### The Recent Rate of Material Progress in England.

IN coming before you on this occasion it has occurred to me that a suitable topic in the commercial capital of England, and at a time when there are many reasons for looking around us and taking stock of what is going on in the industrial world, will be whether there has been in recent years a change in the rate of material progress in the country as compared with the period just before. Some such question is constantly being put by individuals with regard to their own business. It is often put in political discussions as regards the country generally, with some vague idea among politicians that prosperity and adversity, good harvests and bad, in the most general sense, depend on politics. And it must always be of perennial interest. Of late years it has become specially interesting, and it still is so, because many contend that not only are we not progressing, but that we are absolutely going back in the world, while there are evident signs that it is not so easy to read in the usual statistics the evidence of undoubted growth as it was just before 1870–73. The general idea, in my mind, I have to add, is not quite new. I gave a hint of it in Staffordshire last winter, and privately I have done something to propagate it so as to lead people to think on what is really a most important subject. What I propose now to do is to discuss the topic formally and fully, and claim the widest attention for it that I possibly can.

There is much *primâ facie* evidence, then, to begin with, that the rate of the accumulation of wealth and the rate of increase of material prosperity may not have been so great of late years, say during the last ten years, as in the twenty or thirty years just before that. Our fair-trade friends have all along made a tactical mistake in their arguments. What they have attempted to prove is that England lately has not been prosperous at all, that we have been going backwards instead of advancing, and so on; statements which the simplest appeal to statistics was sufficient to disprove. But if they had been more moderate in their contentions, and limited themselves to showing that the rate of advance, though there was still advance, was different from and less than what it was, I for one should have been prepared to admit that there was a good deal of statistical evidence which seemed to point to that conclusion, as soon as a

sufficient interval had elapsed to show that the statistics themselves could not be misinterpreted. There has now been ample time to allow for minor variations and fluctuations, and the statistics can be fairly construed.

I have to begin by introducing a short table dealing with some of the principal statistical facts which are usually appealed to as signs of general progress and the reverse, and I propose to go over briefly the items in that table and to discuss along with them a few broad and notorious facts which cannot conveniently be put in the same form.

*Statement as to Production or Consumption of Staple Articles in the United Kingdom in the undermentioned Years, with the Rate of Increase in Different Periods compared.*

| | 1855. | 1865. | 1875. | 1885. | Ratio of increase per cent. | | |
|---|---|---|---|---|---|---|---|
| | | | | | 1855–65. | 1865–75. | 1875–85. |
| Income-tax assessments, million £ ... ... ... ... ... | 308 | 396 | 571 | 631 | 28 | 44 | 10 |
| Production of coal, million tons ... ... ... ... ... ... | 64 | 98 | 132 | 159 | 53 | 35 | 20 |
| ,, pig iron, ,, ... ... ... ... ... .. | 3·2 | 4·8 | 6·4 | 7·4 | 50 | 33 | 16 |
| Receipts from railway goods traffic per head of population... | — | 11*s.*[1] | 18*s.*[1] | 21*s.* 2*d.*[1] | — | 63 | 18 |
| Clearances of shipping in foreign trade, million tons ... ... | 10 | 15 | 24 | 32 | 50 | 60 | 33 |
| Consumption of tea per head, lbs. ... ... ... ... ... | 2·3 | 3·3 | 4·4 | 5·0 | 43 | 33 | 13½ |
| ,, sugar, ,, ,, ... ... ... ... ... | 30·6 | 39 8 | 62·7 | 74·3 | 30 | 58 | 19 |

[1] These figures are for 1860–64, 1870–74, and 1880–84.

The first figures are those of the income-tax assessments. What we find is that if we go back thirty years and compare the amount of income-tax assessments in the United Kingdom at ten years' intervals, there appears to be an immense progress from 1855 to 1875, the first twenty years, and since 1875 a much less progress. The total amount of the assessments themselves, stated in millions, was as follows:—

| | Millions. | | Millions. |
|---|---|---|---|
| 1855 ... ... | £308 | 1875 ... ... | £571 |
| 1865 ... ... | 396 | 1885 .. ... | 631 |

And the rate of growth in the ten-yearly periods which these figures show is—between 1855 and 1865, 28 per cent.; between 1865 and 1875, 44 per cent.; and between 1875 and 1885, 10 per cent. only.

Making all allowance for changes in the mode of assessment by which the lower limit of the tax has been raised, for the apparent increase before 1875, which may have been due to a gradual increase of the severity of the collection, and for the like disturbing influences, I believe there is no doubt that these income-tax assessments correspond fairly well to the change in the money value of income and property in the interval. How great the change in the rate of increase is, is shown by the simple consideration that if the rate of increase in the last ten years, instead of being 10 per cent. only, had been 44 per cent., as in the ten years just before, the total of the income-tax assessments in 1885, which is actually £631,000,000, would have been £882,000,000! Something then has clearly happened in the interval to change the rate of increase.

These figures being those of money values, an obvious explanation is suggested which would account in great part for the phenomenon of a diminished rate of increase in such values without supposing a reduction of the rate of increase of real wealth, of the things represented by the money values, to correspond. This is the fall of prices of which we have heard so much of late years, and about which in some form or another we shall no doubt hear something at our present meeting. It is quite clear that, if prices fall, then income-tax assessments must also be affected. The produce of a given area of land, for instance, sells for less than it would otherwise sell; there is less gross produce, and in proportion there is even less net produce, that is, less rent; consequently the net income appearing in the Income Tax Schedules is either less than it was or does not increase as it did before. The same with mines, with railways, and with all sorts of business under Schedule D. The things themselves may increase as they did before, but as the money values do not increase, but diminish, the income-tax assessments cannot swell at the former rate. It is the same with salaries and other incomes not dependent so directly in appearance on the fall in prices. Salaries and incomes are of course related to a given range of prices of commodities, and a fall in the prices of commodities implies that the range of salaries and incomes is itself lower than it would otherwise be, assuming the real relation between the commodities and incomes to be the same after the fall in prices as it would have been if there had been no fall in prices. Hence the income-tax assessments by themselves are not a perfectly good test in a question like the present. The change implied may be nominal only, so far as the aggregate wealth and prosperity of the community are concerned, though of course there can be no great and general fall of prices without a considerable redistribution of wealth, which must have many important consequences.

This criticism, however, does not apply to the remaining figures in the short table submitted, and to various other well-known facts, which we shall now proceed to discuss.

The production of coal, then, is found to have progressed in the last thirty years as the income-tax assessments have done. The figures in millions of tons at ten years' intervals are as follows:—

| | Million tons. | | Million tons. |
|---|---|---|---|
| 1855 ... ... | 64 | 1875 ... ... | 132 |
| 1865 ... ... | 98 | 1885 ... ... | 159 |

And the rate of growth in the ten-yearly periods which these figures show is between 1855 and 1865, 53 per cent.; between 1865 and 1875, 35 per cent.; and between 1875 and 1885, 20 per cent. only. The rate of growth in the last ten years is much less than in the twenty years just before. The percentages here, it will be observed, are higher than in the case of the income-tax assessments. The increase in the last ten years in particular is 20 per cent. as compared with an increase of 10 per cent. only in the income-tax assessments. But the direction of the movement is in both cases the same.

I need hardly say, moreover, that coal production has usually been considered a good test of general prosperity. Coal is specially an instrumental article, the fuel of the machines by which our production is carried on. Whatever the explanation may be, we have now, therefore, to take account of the fact that the rate of increase of the production of coal has been less in the last ten years than in the twenty years just before.

Then with regard to pig-iron, which is also an instrumental article, the raw material of that iron which goes to the making of the machines of industry, the table shows the following particulars of production:—

| | Million tons. | | Million tons. |
|---|---|---|---|
| 1855 ... .. | 3·2 | 1875 ... ... | 6·4 |
| 1865 ... ... | 4·8 | 1885 ... ... | 7·4 |

And the rate of growth which these figures show is between 1855 and 1865, 50 per cent.; between 1865 and 1875, 33 per cent.; and between 1875 and 1885, 16 per cent. only. Whatever the explanation may be, we have thus to take account of a diminution of the rate of increase in the production of pig-iron much resembling the diminution in the rate of increase of the production of coal.

At the same time the miscellaneous mineral production of the United Kingdom has mostly diminished absolutely. On this head, not to weary you with figures, I have not thought it necessary to insert anything in the above short table; but I may refer

you to the tables put in by the Board of Trade before the Royal Commission on Trade Depression. Let me only state very briefly that while the average annual amount of copper produced from British ores amounted in 1855 to over 20,000 tons, in 1865 the amount was about 12,000 tons only, in 1875 under 5000 tons, and in 1885 under 3000 tons. As regards lead, again, while the production about 1855 was 65,000 tons, and in 1865 about 67,000 tons, the amount in 1875 had been reduced to 58,000 tons, and in 1885 to less than 40,000 tons. In white tin there is an improvement up to 1865, but no improvement since, and the only set-off, a very partial one, is in zinc, which rises steadily from about 3500 tons in 1858, the earliest date for which particulars are given, to about 10,000 tons in 1885, considerably higher figures having been touched in 1881–83. There is nothing, then, in these figures as to miscellaneous mineral production to mitigate the impression of the diminution in the rate of increase in the great staples, iron and coal, in recent years.

Agricultural production, it is also notorious, has been at any rate no better, or not much better, than stationary for some years past, although down to a comparatively recent period a steady improvement seemed to be going on. Making all allowance for the change in the character of the cultivation, by which the gross produce is diminished, although the net profit is not affected to the same extent, and which might be held to argue no real decline in the rate of general growth if the population, diverted from agriculture, were more profitably employed, yet the facts, broadly looked at, taken in connexion with the other facts stated as to diminished rate of increase in other leading industries, seem to confirm the supposition that there may have been some diminution in the rate of increase generally.

It is, unfortunately, impossible to state in a simple manner the progress at different dates in the great textile industries of the country. Everything as regards these industries is thrown out by the disturbance consequent on the American War. It does not appear, however, that what has happened as regards the main textile industries, cotton and wool, would alter sensibly the conclusions above stated, drawn from the facts as to other main industries of the country. If we take the consumption of raw materials as the test, it would appear that the growth in the cotton manufacture is from a consumption of 28 lbs. per head in 1855 to about 38 lbs. per head in 1875, while in 1885 the consumption is nearly 42 lbs. per head, an increase of 4 lbs. per head in the last ten years, against 10 lbs. per head in the previous twenty. The percentage of increase in the last twenty years must therefore, on the whole, have been less than in the previous twenty, although in these twenty years the great interruption due to the American Civil War occurred. Of course the amount of raw material consumed is not here an absolute test. There may be more spinning and weaving now in proportion to the same quantity of raw material than was formerly the case. But the indications are at least not so certain and direct as when the consumption of raw material could be confidently appealed to. As regards wool the comparison is unfortunately very incomplete owing to the defect of data for the earlier years; but what we find is that the amount of wool consumed per head of the population of the United Kingdom has in the last ten years rather declined than otherwise from nearly 11 lbs. per head in the five years 1870–74 to 10 lbs. per head only in the five years 1880–84. Here, again, the explanation suggested as to cotton—viz. that there may be more spinning and weaving now in proportion to the same quantity of raw material than was formerly the case—applies. But the answer is also the same, that at any rate the indications of progress are no longer as simple as they were. The reality of the former rate of advance is not so clearly manifest.

Of course I need hardly add that in the case of another great textile, silk, there has been no progress, but the reverse, for some years; that this is also true of linen; and that the increase in the allied manufacture, jute, can only be a partial set-off.

In the textiles, then, as in other staple industries of the country, the rate of advance in the last ten years, measuring by things, and not merely by values, has been less than in the twenty years immediately before.

We pass on, then, to another set of figures included in the short table above submitted. We may look not only at leading industries of production directly, but at the broad figures of certain industries which are usually held to reflect, as in a mirror, the progress of the country generally. I refer to the railway traffics as regards the home industries of the country, and the entries and clearances of shipping in the foreign trade as regards our foreign business.

As regards railways what we find is, if we take the receipts from the goods traffic in the form in which they were summarized for the Royal Commission on Trade Depression, viz. reduced to so much per head of the population on the average of quinquennial periods, that in the five years 1860–64, which is as far back as the figures can be carried, the receipts per head were 11*s.*; ten years later, viz. in 1870–74, the receipts per head were 18*s.*; and ten years later, viz. 1880–84, the receipts per head were 21*s.* 2*d.* The rate of growth shown in the first ten years' interval is 63 per cent.; in the second ten years' interval it is only 18 per cent.; and in the last year or two, I may add, there has been no further improvement. Here the question of the value of money comes in again, but this would only modify partially the apparent change. There is also a question as to railway extension having been greater in the earlier than in the later period, so that growth took place in the earlier period because there were railways in many districts where they had not been before, and there was no room for a similar expansion in the later period. But the difference in the rate of growth it will be observed is very great indeed, and this explanation seems hardly adequate to account for all the difference. At any rate, to repeat a remark already made, the indications are no longer so simple as they were. There is something to be explained.

The figures as to the number of tons of goods carried are not in the above table; nor are such figures very good, so long as they are not reduced to show the number of tons conveyed one mile. But, *quantum valeant*, they may be quoted from the Board of Trade tables already referred to. The increase, then, in minerals conveyed between 1855 and 1865 is from about 40 million to nearly 80 million tons, or 100 per cent.; between 1865 and 1875 it is from 80 to about 140 million tons, or 75 per cent.; and in the last ten years it is from 140 to 190 million tons only, if quite so much, or about 36 per cent. only. As regards general merchandise, again, the progression in the three ten-yearly periods is in the first from about 24 to 27 million tons, or rather more than 50 per cent.; in the second from 37 to 63 million tons, or 70 per cent.; and in the third from 63 to 73 million tons, or 16 per cent. only. As far as they go there is certainly nothing in these figures to oppose the indications of a falling-off in the rate of increase in the general business already cited.

Coming to the movement of shipping in the foreign trade, the series of figures we obtain are the following, which relate to clearances only, those relating to entries being of course little more than duplicate, so that they need not be repeated: 1855, 10 million tons; 1865, 15 million tons; 1875, 24 million tons; 1885, 32 million tons. And the rate of growth thus shown is between 1855 and 1865 no less than 50 per cent.; between 1865 and 1875 no less than 60 per cent.; and between 1875 and 1885 about 33 per cent. only—again a less rate of increase in the last ten years than in the period just before. Here, too, it is to be noticed, what is unusual in shipping industry, that in the last few years the entries and clearances in the foreign trade have been practically stationary. The explanation no doubt is in part the great multiplication of lines of steamers up to a comparatively recent period, causing a remarkable growth of the movement while the multiplication of lines was itself in progress, and leaving room for less growth afterwards because a new framework had been provided within which traffic could grow. But here again it is to be remarked that the whole change can hardly, perhaps, be explained in this manner, while the remark already made again applies, that the fact of explanation being required is itself significant.

The figures of imports and exports might be treated in a similar manner, as they necessarily follow the course of the leading articles of production and the movements of shipping. But we should only by so doing get the figures we have been dealing with in another form, and repetition is of course to be avoided.

The short table contains only another set of figures, viz. those of the consumption of tea and sugar, which are again commonly appealed to as significant of general material progress. What we find as regards tea is that the consumption per head rises between 1855 and 1865 from 2·3 to 3·3 lbs., or 43 per cent.; between 1865 and 1875 from 3·3 to 4·4 lbs., or 33 per cent.; and between 1875 and 1885 from 4·4 to 5 lbs., or 13⅓ per cent. In sugar the progression is in the first period from

30·6 to 39·8 lbs. per head, or 30 per cent. ; in the second period from 39·8 to 62·7 lbs., or 58 per cent. ; and in the third period from 62·7 to 74·3 lbs., or 19 per cent. only. In the last ten years in both cases the rate of increase is less than in the twenty years before.

These facts, I need hardly say, would be strengthened by a reference to the consumption of spirits and beer, the decline in the former being especially notorious. In tobacco again in the last ten years there has been no increase of the consumption per head ; which contrasts with a rapid increase in the period just before—viz. from about 1·31 lb. per head in 1865 to 1·46 lb. per head in 1875.

No doubt the observation here applies that the utmost prosperity would obviously be consistent with a slower rate of increase per head from period to period in the consumption of these articles, and with, in the end, a cessation of the rate of increase altogether. The consumption of some articles may attain a comparatively stationary state, the increased resources of the community being devoted to new articles. But here, again, we have to observe the necessity for explanation. The indications are no longer so sure and obvious in all directions as they were.

It is difficult, indeed, to resist the impression made when we put all the facts together, leaving out of sight for a moment those of values only. We are able to affirm positively—(*a*) That the production of coal, iron, and other staple articles has been at a less rate in the last ten years than formerly ; (*b*) that this has taken place when agricultural production has been notoriously stationary, and when the production of other articles such as copper, lead, &c., has positively diminished ; (*c*) that there has been a similar falling-off in the rate of advance in the great textile industries ; (*d*) that the receipts from railway traffic and the figures of shipping in the foreign trade show a corresponding slackening in the rate of increase in the business movement ; and (*e*) that the figures as to consumption of leading articles, such as tea, sugar, spirits, and tobacco, in showing a similar decline in the rate of increase, and, in some cases, a diminution, are at least not in contradiction with the other facts stated, although it may be allowed that there was no antecedent reason to expect an indefinite continuance of a former rate of increase.

From these facts, however we may qualify them—and many qualifications have already been suggested, while others could be added—it seems tolerably safe to draw the conclusion that there has probably been a falling-off in the rate of material increase generally. The income-tax assessment figures, though they could not be taken by themselves in such a question, are, at least, not in contradiction, and there is nothing the other way when we deal with these main figures only. I should not put the conclusion, however, as more than highly probable. Some general explanation of the facts may be possible on the hypothesis that there is no real decline in the rate of growth generally at all ; that the usual signs for various reasons have become more difficult to read ; that owing to the advance already made the real growth of the country and, to some extent, of other countries, has taken a new direction ; and that the utmost caution must be used in forming final conclusions on the subject. But the conclusion of a check having occurred to the former rate of growth may be assumed meanwhile for the purposes of discussion. The attempted explanation of the causes of change, on the hypothesis that there is a real change, may help to throw light on the question of the reality of the change itself.

Various explanations are suggested, then, not only for a decline in the rate of our progress, but for actual retrogression. Let us look at the principal of these explanations in their order, and see whether they can account for the facts : either for actual retrogression, or for a decline in the general rate of material growth equal to what some of the particular facts above cited, if they were significant of a general change in the rate of growth, imply—a decline, say, from a rate of growth amounting to 40 per cent. in ten years to one of 20 per cent. only in the same period.

One of the most common explanations, then, as we all know, is foreign competition. The explanation has been discredited because of the exaggeration of the alleged evil to be explained ; but it may possibly be a good enough explanation of the actual facts when they are looked at in a proper way. In this light, then, the assertion as to foreign competition would be found to mean that foreigners are taking away from us some business we should otherwise have had, and that, consequently, although our business on the whole increases from year to year, it does not increase so fast as when foreign competition was less. Those who talk most about foreign competition have actually in their mind the unfair element in that competition, the stimulus which the Governments of some foreign countries give or attempt to give to particular industries by means, on the one hand, of high tariffs keeping out the goods we should otherwise send to such countries, and giving their home industry of the same kind a monopoly which sometimes enables them to produce a surplus they can sell ruinously cheap abroad ; and by means, on the other hand, of direct bounties which enable certain industries to compete in the home market of the United Kingdom itself, as well as in foreign markets. But there is a natural foreign competition as well as a stimulated foreign competition to be considered, and it may be the more formidable of the two.

Dealing first with the stimulated competition, the most obvious criticism on this alleged explanation of the recent decline in the rate of increase of our material progress is that the stimulus given by foreign Governments in recent years has not been increasing, or at any rate not materially increasing, so as to account for the change in question. People forget very quickly ; otherwise it would not be lost sight of that after 1860, as far as European nations are concerned, there was a great reduction of tariff duties—a change, therefore, in the contrary direction to that stimulus which is alleged to have lately caused a change in the rate of our own development. Since about five or six years ago the movement on the Continent seems again to have been in the direction of higher tariffs. France, Italy, Austria, Germany, and Russia have all shown protectionist leanings of a more or less pronounced kind. Some of our colonies, especially Canada, have moved in the same direction. But, on the whole, these causes as yet have been too newly in operation to affect our industry on a large scale. As a matter of fact, with one exception to be presently noticed, the period from 1860 to 1880 was one in which the effect of the operation of foreign Governments in regard to their tariffs could not be to stimulate additional competition of an injurious kind with us in the way above described, but to take away, if anything, from the stimulus previously given. The changes quite lately brought into operation, if big enough, and if really having the effects supposed, might stimulate foreign competition in the way described in the period now commencing ; but, as an explanation of the past facts, it is impossible to urge that foreign competition had recently been more stimulated by additions to tariffs than before, and that in consequence of this stimulus our own rate of advance had been checked.

The one exception to notice is the United States. Immediately after 1860 the civil war in that country broke out, and that war brought with it the adoption of a very high tariff. Curiously enough, however, that tariff operated most against us in the very years, that is, the years before 1875, in which our rate of advance was greater to all appearance than it has lately been. In 1883 there was a great revision of the tariff, having for its general result a slight lowering and not an enhancement of the tariff, and it is with this reduction, that is, with a diminution of the alleged adverse stimulus, that the diminution in our own rate of advance has occurred.

Of course the explanation may be that, although Governments have not themselves been active till quite lately in adding to their tariffs, yet circumstances have occurred to make the former tariffs more injurious in recent years than they were down to 1875. For instance, it may be said that, owing to the fall of prices in recent years, the burden of specific duties has become higher than it was. The duty is nominally unchanged, but by the fall of prices its proportion to the value of the article has become higher. This is no doubt the case to a large extent. On the other hand, *ad valorem* duties have been lowered in precisely the same way. The fall of prices has brought with it a reduction of duty ; and especially on articles of English manufacture, where the raw material is obtained from abroad, the reduction of duty, being applicable to the whole price, must certainly have had for effect to render more effective than before the competition of the English manufacturer. Whether on the whole the reduction of *ad valorem* duties consequent on the fall of prices has been sufficient throughout the range of our foreign trade to compensate the virtual increase of the weight of specific duties from the same cause seems to be a nice question. This being the case, it must be very difficult indeed to show that on the whole the weight of foreign tariffs, apart from the action of foreign Governments, has been increased in recent years so as to affect our own growth injuriously.

Foreign tariffs, it may be said, have become more effective for another reason. Manufacturing industry having itself developed abroad, the same amount of protection given to the foreign industry becomes more efficient than it was. But this, of course, raises the question of the effect of natural foreign competition, which will presently be discussed.

So much for the stimulus to foreign competition due to high tariffs. With regard to bounties, very little need be said. They have been the subject of much discussion and agitation for various reasons, and in what I have to say I propose not to touch on the practical question whether these bounties are injurious, and the nature of the political remedies that may or may not be possible. I limit myself strictly to the point, how far any effect which such bounties can have had would account for a diminution in the rate of material growth of the country generally in the last ten years as compared with the ten years just before. Dealing with the question in this strictly limited fashion, what I have to observe first is, that hitherto very few bounties have been complained of, except those on sugar production and refining; and next, that the whole industries of sugar production and refining, important as they are in themselves, hardly count in a question of the general history of the United Kingdom. Even if we refined all the sugar consumed in the United Kingdom and the maximum amount we have ever exported, the whole income from this source, the whole margin, would not exceed about £2,000,000 annually, not one six-hundredth part of the income of the people of the United Kingdom; and of this £2,000,000 at the worse we only lose a portion by foreign competition, while all that is really lost, it must be remembered, is not the whole income which would have been gained if a certain portion of our labour and capital had been employed in sugar refining, but only the difference between that income and the income obtained by the employment of the same labour and capital in other directions. The loss to the Empire may be greater, because our colonies are concerned in sugar production to the extent at present prices, of £5,000,000 to £6,000,000 annually, which would probably be somewhat larger but for foreign competition. But it does not seem at all certain that this figure would be increased if foreign bounties were taken away, while in any case the amounts involved are too small to raise any question of foreign bounties having checked the rate of growth of the general industry of the country.

*Per contra*, of course, the extra cheapness of sugar, alleged to be due to the bounties, must have been so great an advantage to the people of the United Kingdom, saving them perhaps £2,000,000 to £3,000,000 per annum, that the stimulus thereby given to other industries must apparently have far more than compensated any loss caused by the stimulus of foreign bounties to sugar production and refining abroad. But to enlarge on this point would involve the introduction of controversial matter, which I am anxious to avoid. I am content to show that nothing that can have resulted from sugar bounties could have affected seriously the general rate of material growth in the country.[1]

*Mutatis mutandis*, the same remarks apply to other foreign bounties, of which indeed the only ones that have been at all heard of are those on shipping. But as yet, at least, the increase of foreign shipping has not been such as to come into comparison with our own increase, while the portion of the increase that can be connected with the operation of bounties is very small. It would be useless to enter into figures on so small a point; but few figures are so well known or accessible as those relating to shipping.

In neither way, then, does there appear to be anything in the assertion that the protectionist action of foreign Governments in recent years can have caused the check alleged to the rate of growth in our industry generally, assuming such a check to have occurred. I may be dispensed, therefore, from entering on the theoretical argument, which I only notice *pour mémoire*, that in the nature of things no enhancement of foreign tariffs and no grants of foreign bounties could really check our own rate of growth, except by checking foreign growth still more, which is not the case we are considering, because the allegation is that foreign competition is increasing at our expense. That I do not insist on this argument is not to be considered as a sign that it is dropped or that I am not fully sensible of its logical completeness. It seems enough, at present, to fortify it by considerations from actual practical facts which no one can dispute.

The question of an increase of foreign competition from natural causes is more difficult. It is beyond all question, as I have pointed out elsewhere, that foreign competition in every direction from natural causes must continue to increase, and that it has increased greatly in recent years. But when the facts are examined, it does not appear that this competition has been the cause of a check to our own rate of growth. One of the facts most commonly dwelt upon in this connexion is the great increase of the imports of foreign manufactured articles into the United Kingdom. But the increase in the last ten years is not more than about £18,000,000, taking the facts as recorded in what is known as Mr. Ritchie's Return, viz. from about £37,000,000 in the quinquennial period 1870–74 to £55,000,000 in the quinquennial period 1880–84, or about 50 per cent. Out of £18,000,000 increased imports of such articles it is fair to allow that at least one-half, if not more, is the value of raw material which we should have had to import in any case; so that only £9,000,000 represents the value of English labour displaced by these increased imports. Even the whole of this £9,000,000 of course is not lost, only the difference between it and the sum which the capital and labour "displaced" earns in some other employment, which may possibly even be a *plus* and not a *minus* difference. If we add articles "partly manufactured" no difference would be made, for the increase here is only from £26,000,000 to £28,000,000 in the ten years. Such differences, it need not be said, hardly count in the general total of the industry of the country. Further, the rate of increase of these imports was just as great in the period when our own rate of growth was greater, as in the last ten years, the increase in manufactured articles between 1860–64 and 1870–74 being £19,000,000, viz. from £18,000,000 to £37,000,000, or over 100 per cent. as compared with 50 per cent. only in the last ten years, and in articles partly manufactured from £17,000,000 to £26,000,000, an increase of £9,000,000 as compared with an increase of £2,000,000 only in the last ten years. Making all allowance for the fall in prices in recent years, these figures will show a greater relative increase of imports of manufactured articles before 1875 than afterwards. It cannot, therefore, be the increased import of foreign manufactures which has caused the check to our own growth in the last ten years.

But foreigners, it is said, exclude us from their own markets and compete with us in foreign markets. Here again, however, we find that any check which may have occurred to our foreign export trade is itself so small that its effect on the general growth of the country would be almost *nil*. Take it that the check is as great as the diminution in the rate of increase in the movements of shipping, viz. from an increase of 55 per cent. to one of 33 per cent. only, that is, broadly speaking, a diminution of one-third in the rate of increase of our foreign trade, whatever that rate may have been. Assuming that rate to have been the same as the rate of increase in the movements of shipping itself, the change would be from a rate of increase equal to one-half in ten years to a rate of increase equal to about one-third only. Applying these proportions to the exports of British and Irish produce and manufactures, which represent the productive energy of the country devoted to working for foreign exchange, and assuming that ten years ago the value of British labour and industry in the produce and manufactures we exported, due deduction being made for the raw material previously imported, was about £140,000,000 (see my "Essays in Finance," first series), then it would appear that if the same range of values had continued, the check to the growth of this trade would have been such that at the end of ten years the British labour represented in it, instead of having increased 50 per cent., viz. from £140,000,000 to £210,000,000, would have increased one-third only, or from £140,000,000 to about £187,000,000. The annual difference to the energy of the country developing itself in the foreign trade would on this showing be about £23,000,000 only, an insignificant sum compared with the aggregate income of the people of the country; while the country, it must be remembered, does not lose the whole of this sum, but only the difference between it and the sum earned in those employments to which those concerned have resorted, which again may be a *plus* and not a *minus* difference. Even, therefore, if foreign competition is the cause of a check to our general growth, yet the figures we are dealing with in our foreign trade are such that any visible check to that trade which can have occurred must have been insufficient to cause that apparent diminution in the rate of our material growth generally which has to be explained.

It has to be remembered, moreover, that when the figures are

[1] See Appendix to "First Report of Royal Commission on Trade Depression," p. 130.

studied, and the fall of prices allowed for, it is not in our foreign trade that any check worth mentioning seems to have occurred at all. The diminution in the rate of increase in the movements of shipping is very largely to be accounted for in the way already explained, viz. by the fact that the increase just before 1875 was largely owing to the multiplication of lines of steamers, and that a framework had then been provided up to which the traffic has since grown. Even an increase of one-third in the movements in the last ten years may thus show as great an increase in real business as an increase of 50 or 60 per cent. in the movements in the twenty years before. Foreign competition, even from natural causes, is thus insufficient to account for the diminution in the rate of increase of our material growth in the last ten years.

These figures may be put directly another way. The increase of our foreign exports per head between 1860–64 and 1870–74 was from £4 14*s.* 11*d.* to £7 7*s.* 5*d.*, or about 55 per cent., and allowing for an average rise of prices between the two dates, may be put as having been at the extreme about 50 per cent. Between 1870–74 and 1880–84, instead of an increase, there is a decrease, viz. from £7 7*s.* 5*d.* to £6 12*s.* 9*d.*, but deducting about one-third from the former figure for the fall in prices, the real increase in the last ten years would appear to be as from £4 16*s.* 3*d.* to £6 12*s.* 9*d.*, or over 35 per cent. The difference in the rate of increase in the last ten years compared with the previous ten is thus the difference between 35 and 50 per cent. only, equal to about £21,000,000 annually on the amount of £140,000,000 assumed to represent the value of British industry in our foreign exports, deduction being made for the value of raw material included. A deduction of this sort from the annual income of the country is too small to account for such a check to the rate of our growth generally as that we are now discussing as probable, especially when we recollect that the labour is only diverted, and it is not the whole £21,000,000 that is lost, but only the difference between that sum and what is otherwise earned, which may even be a *plus* and not a *minus* difference.

To bring the matter to a point, an increase of 40 per cent. in the income of the country in ten years would, on an assumed income of 1000 millions only in 1875—and the figure must then have been more—have brought the income up to 1400 millions; an increase of 20 per cent. would have brought it up to 1200 millions only, a difference of 200 millions, which must have arisen from the alleged difference in the rate of our material growth in question if it had occurred. Clearly nothing can have happened in our foreign trade to account for anything more than the smallest fraction of such a difference. The figures are altogether too small. We may repeat again, then, that it is not the check to our foreign trade which foreign competition may have caused to which we can ascribe the recent check to our general rate of growth.

I need hardly add that in point of theory foreign competition was not likely to have the effect stated. I have set forth elsewhere in an elaborate essay ("Essays in Finance," second series, "Foreign Manufactures and English Trade") the reasons for holding this opinion; why it is, in fact, that as foreign nations grow richer we should be better off absolutely than if they were to remain poor, though relatively they might advance more than we do. But, whatever theory may say, in point of fact the check to the rate of our material growth cannot, for the reasons stated, have been due to anything which has happened to our foreign trade.

Another explanation which has been suggested, and to which I have myself been inclined to attach considerable weight as being plainly, as far as it goes, a *vera causa*, is the extent to which the hours of labour have been reduced in many employments in consequence of the improvement in the condition of the working classes in the last half-century, and the growth of a disposition to take things easier, which has been the result of the general prosperity of the country. Such causes, when they exist, and when they are brought into operation, must tend to diminish the rate of material growth in a country as compared with a period just before when they were not in operation. If we could suppose them brought into operation suddenly, all other things, such as the progress and development of invention, remaining the same, such a reduction of hours of labour and growth of a disposition to take things easy must produce a check to the former rate of growth.

After some consideration, however, although there is no doubt of the general tendency of the causes referred to, I begin to doubt whether they would explain adequately such a check to the rate of material growth generally throughout the country as is assumed to have occurred. As regards the shortening of the hours of labour, which is the more definite fact to be dealt with, it cannot but be observed that the shortening has by no means been universal. It has been conspicuous among certain trades organized into trade unions; but the unions, after all, only include about a tenth part of the labour of the country. There has been no such conspicuous shortening of the hours of labour among professional men, clerks, domestic servants, and many others whose labour is an essential part of the general sum total. Next—and this is perhaps even more important—the shortening of the hours of labour is not coincident with the beginning of the last ten years, though it has been in full operation for the whole of that period, but rather with the beginning or middle of the previous ten years, viz. 1865–75; so that it should have been fully in operation upon the production of 1875; and the check to our rate of growth if due to this cause should thus have been felt between 1865 and 1875, rather than between the latter date and the present time. The same with the general disposition to take things easy. This disposition did not spring up in a day in 1875, but was probably as effective as a cause of change in the earlier, as in the later, period. It must count for something as a cause of the annual production of the country being less at a given moment than it would otherwise be; but in comparing two periods what we have to consider is whether the growth of this disposition has been greater in one period than in another; and there are no data to support such a conclusion as regards the last ten years compared with the previous ten.

We must apparently, therefore, reject this explanation also. It is not adequate to account for the apparent change that has occurred in the rate of our growth from the year 1875 as compared with the period just before. Our progress in periods previous to 1875 took place in spite of the operation of causes of a similar kind which were then in operation, and there is no proof at all that the shortening of the hours of labour and the growth of a disposition to take things easy have been greater since 1875 as compared with the period just before than they were between 1865 and 1875 as compared with the period just before that. What is wanted is a new cause beginning to operate in or about 1875, and the shortening of the hours of labour and the growth of a disposition to take things easy do not answer that description sufficiently. Something of the apparent change may be due to an acceleration in recent years of the growth of a disposition to take things easy, but on the whole the explanation halts when we make a strict comparison.

Another cause which may properly be assigned as a *vera causa* of a check to the rate of material growth in the country is the unfavourable weather to agriculture, and the generally unprofitable conditions of that industry in recent years. *Pro tanto* such influences would make agricultural production less to-day than it would otherwise be. Employment in that industry would also be diminished comparatively, and perhaps absolutely, and a check to production generally would take place while labour was seeking new fields. But the check arising in this manner, as far as the general growth is concerned, has obviously not been very great. More land in proportion has been turned into permanent pasture, but very little land has gone out of cultivation altogether, and even the amount under the plough has not much diminished. Agricultural labour, in somewhat greater proportion than before, has been obliged to seek other employments; the flow of population from country to town has been increased somewhat; but nothing new has happened to diminish production generally to a serious extent, and it is a new cause, it must be remembered, for which we are seeking. As far as unfavourable weather is concerned, again, that is only a temporary evil. One year with another, the weather is not worse now than at any former time; the remarkably unfavourable weather which lasted from 1874 to 1880 has passed. The other conditions unfavourable to agriculture, especially foreign competition, are more enduring; but these seem much more unfavourable to rent than to production itself, which is the point now under consideration; and we do not know that they will be permanent at all when prices and wages are fully adjusted.

The disturbance to industry by the fall of prices generally is also a *vera causa* of a check to the rate of material growth. But the effect of such a cause seems to be confined within narrow limits, and it is not a new cause. It occurs in every time of depression due to discredit, being partly the effect and partly the cause of the depression itself. All that is new recently is the

extreme degree of the fall, and I must express the greatest doubt whether a mere difference of degree aggravates materially the periodical disturbance of industry, tending to check production, which a fall of prices from a high to a low level causes. So far as past experience has gone, at any rate, no such cause has been known to check production to any material extent. If any such cause tended to have a serious effect we should witness the results every time there is a shrinkage of values owing to the contraction and appreciation of an inconvertible paper currency, and I am not aware of any such contraction having had the effect described on production, though the effect in producing a feeling of depression is beyond all question. The facts as to the great contraction in this country between 1815 and 1820 are on record, while the experience of the United States after the civil war is also fresh in everyone's recollection. Contraction of currency and fall of prices, though they are painful things, do not stop production materially.

Another explanation suggested is that there is in fact no antecedent reason for supposing that the rate of material growth in a community should always be at the same rate—that a community may, as it were, get "to the top" as regards its development under given conditions, and then its advance should be either less rapid than it had been or it should even become stationary. The defect of this explanation is that it assumes the very thing which would have to be proved. Is there any other sign, except the alleged check to the rate of our material growth itself, that in or about the year 1875 this country got "to the top"? It has, moreover, to be considered that on *a priori* grounds it is most unlikely a community would get to the top *per saltum*, and then so great a change should occur as the apparent change we are considering. The persistence of internal conditions in a given mass of humanity is a thing we may safely assume, and if these conditions are consistent with a given rate of development in one period of ten years, it is most unlikely that, save for an alteration of external conditions, there would be another rate of development in the succeeding ten years. Human nature and capacities do not change like that. Scientific opinion, I believe, is also to the effect that the progress of invention, and of the practical working of inventions, which have been the main cause of our material growth in the past, have been going on in the last ten years, are still going on, and are likely to go on in the near future, at as great a rate as at any time in the last fifty years. Except, as already said, the apparent check to the rate of our material growth itself, there is no sign anywhere of our having got to the top, so that a stationary condition economically, or a condition nearly approaching it, has been reached.

Last of all, it is urged that the diminution in the material of our growth, which is in question, must be due to the fact that we are losing the natural advantages of coal and iron which we formerly had in comparison with the rest of the world. This is perhaps only another way of saying that we have got to the top by comparison, though the community of nations generally has not got to the top, and another way of saying also that foreign competition affects us more than it formerly did—an argument already dealt with. But the question whether coal and iron at home are really so indispensable to our material growth as is sometimes assumed appears itself so important that I may be excused for specially discussing this question, notwithstanding that it has virtually been disposed of, as far as any explanation of past facts is concerned, by what has been already said.

The argument proceeds on the supposition—which is no doubt well founded in the abstract and as far as the past experience of mankind is concerned—that in addition to natural capacities of its own a community requires for its prosperity certain natural advantages, fertility of soil, rich and easily-worked mines, a genial climate in which labour may conveniently be carried on, and so forth. A community possessing all these things, or the like things, will flourish, but as it ceases to lose any of them its prosperity must become precarious, and population must flow to the places where they can be secured. Of course climate is not a thing which changes, as far as any practical experience is concerned; but relatively the advantage of a fertile soil may be lost, as England has lately lost it in comparison with the United States and other new countries, its soil having become inadequate for the whole population; and still more the advantage of mines, especially mines of coal and iron, on which the miscellaneous industries of a manufacturing country depend, may be lost. Hence it is said the check to our rate of growth in recent years. We have long since lost our agricultural advantages by comparison. Now we are also beginning to lose the special advantages which coal and iron have given. Our mines are becoming less rich than those of foreign countries, and the balance is turning against us. Why should not population relatively flow from England to the United States and other countries as it has passed within the limits of the United Kingdom itself from Cornwall and Sussex to Staffordshire, Lancashire, Yorkshire, and the north? In this view the coal famine of 1873 was the sign of a check such as Mr. Jevons anticipated. What has happened since is only a sequence of the like causes.

I need not repeat in opposition to this view what has already been said as to the inadequacy of any actual decline in our foreign trade to account for such a check to our general growth as is supposed to have occurred. If the loss of our natural advantages of coal and iron in addition to agriculture are having the effects supposed, we ought to witness them in our foreign trade, and in fact we do not witness them to the extent required for the production of the phenomenon in question.

What I wish now specially to urge is that in consequence of the progress of invention and the practical application of inventions in modern times the theory itself has begun to be less true generally than it has been. It is no longer so necessary, as it once was, as in fact it always has been until very lately, that people should live where their food and raw materials are grown. The industry of the world having become more and more manufacturing and, if one may say so, artistic, and less agricultural and extractive, the natural advantages of a fertile soil and rich mines are less important to a manufacturing community than they were at any former period of the world's history, because of the new cheapness of conveyance. Under the new conditions, I believe it is impossible to doubt, climate, accumulated wealth, acquired manufacturing skill, concentration of population, become more important factors than mere juxtaposition to the natural advantages of fertile soil and rich mines. The facts seem at any rate worth investigating, judging by what has happened in England and other old countries in the last half-century, and by what is still happening there.

Take first the question of food. Wheat is now conveyed from the American Far West to Liverpool and London and any other ports in the Old World for something like five shillings per quarter—equal to about half a farthing on the pound of bread, or a halfpenny on the quartern loaf. The difference between the towns of a country with fertile soil, therefore, and the towns of a country with inadequate soil is represented by this small difference in the price of bread. At about fivepence the quartern loaf the staff of life may be about 10 per cent. cheaper in the fertile country than it is in a country which does not grow its own food at all, and which may be thousands of miles away. As the staff of life only enters into the expenditure of the artisan to the extent of 20 per cent. at the outside, and into the expenditure of richer classes to a smaller extent, the difference on the whole income of a community made by their living where the staff of life would be cheaper would be less than 2 per cent.—too small to tell against other advantages which may be credited to them. What is true of wheat is even more true of meat and other more valuable articles of food, where the cost of conveyance makes a less difference in the proportionate value of the food *in situ* and its value at a distant point. The same more and more with raw materials. Cotton and such articles cost so little to transport that the manufacturing may as well go on in Lancashire or any other part of the Old World as *in situ* or nearly *in situ*; and even as regards metals or minerals, except coal and perhaps iron, the same rule applies, the cost of conveyance being as nothing in proportion to the value of the raw material itself. As regards coal and iron, moreover, there are many places where they are not in absolute juxtaposition, and if they have to be conveyed at all they may as well be conveyed to a common centre. Iron ore and iron at any rate are beginning to be articles of import into the old countries of Europe to which the cost, in fact, offers very little difficulty. The additional cost to the miscellaneous manufacturing of a country through its having to bring iron and coal from a distance may thus be quite inconsiderable, and apparently is becoming more and more inconsiderable. As regards raw materials generally it has also to be considered that, owing to their immense variety, there is an undoubted convenience in a common manufacturing centre to which they can be brought. Hitherto they may have come to England and other old countries of Europe in part because coal and iron were abundant there in juxtaposition; but the habit once set up, there seems

no reason why they should not concentrate themselves on the old manufacturing centres. The ruder parts of the coal and iron industry may be attracted to other places, but the higher branches of manufacturing will be at no disadvantage if carried on at the old centres.

On the other hand, the old centres will retain the advantages, which are obviously very great, of climate, accumulated wealth, acquired skill, and concentration of population. That population under the new conditions is to go from them merely because they do not grow food which can be transported to them at the cost of a mere fraction of the aggregate income, and because they have not coal and iron in abundance and in juxtaposition, that abundance and juxtaposition, owing again to the diminished cost of conveyance, being no longer so indispensable as it was to the higher branches of manufacturing, appears certainly to be a "large order." What I have to suggest most strongly at any rate is that the advantages I have spoken of as possessed by old manufacturing centres are not unlikely to tell more and more under the new conditions, and that the indispensability of coal and iron is no longer to be spoken of as what it has been in the last century, during which apparently England owed so much of its precedence in manufacturing power to these causes.

To the same effect we may urge the specially great increase of the efficiency of coal in recent years. Cheap coal *in situ* cannot be relatively so important as it was in days when five or ten tons of coal were required to do the work which can now be done by one.

The truth is that the whole change that has been occurring is only a continuation of much larger historical changes. There has almost always in English history been some one industry that was supposed to be king. In the Middle Ages it was the growth and export of raw wool; last century it was the woollen manufacture itself; early in this century and down to a very late date cotton was king; more lately, since the beginning of the railway and steamship era, it has been coal and iron. How do we know, how can we know, that coal and iron are to reign indefinitely, any more than wool, or the wool'en manufacture, or cotton themselves have done? Changes are always going on, and for that reason I believe we should attach the more importance to the increasing signs that it is no longer necessary or indispensable for prosperous communities to live where their food and raw materials are grown; that there may be advantages of climate, of accumulated wealth, of acquired skill, of concentration of population which are now, under the new conditions, overwhelmingly more important. It would be absurd to dogmatize in such a matter. I hope, however, I have said enough to those who care to reflect to satisfy them that the indispensability even of coal and iron to the continuance of our material growth is no longer to be assumed, that there are wholly new conditions to be considered.

To come back to the practical point in all this discussion. Not only is there no sign in anything that has yet happened that the apparent check to our former rate of material growth is due to the loss of natural advantages which we once possessed, but the theory of natural advantages itself requires to be revised. Equally in this way as in the other ways that have been discussed, it is impossible to account for the apparent check to the former rate of our material growth which has been observed.

Having carried matters so far, however, and having found the insufficiency of the various causes which have been assigned for the check to our former rate of material growth, because they have not produced the sort of effect in detail which they ought to have produced so as to lead to the general effect alleged, or because they existed quite as much when the rate of growth was great as in recent years when a diminution has apparently been observed, it would seem expedient to inquire whether, in spite of the accumulation of signs to that effect, the apparent check to our rate of growth may, after all, not be a real one. To some extent I think we must conclude that this is the case. There are other facts which are inconsistent with a real and permanent check such as has been in question, and a general explanation of the special phenomena of arrest seems possible without supposing any such real check.

The first broad fact that does not seem quite reconcilable with the fact of a real diminution of the kind alleged in the rate of material growth generally is the real as distinguished from the apparent growth of the income-tax assessments when allowance is made for the fall of prices which affect, as we have seen, all aggregate values. Assuming the fall of prices to be about 20 per cent., then we must add one-fourth to the assessments in 1885 to get the proper figure for comparison with 1875. The total of 631 millions for 1885 would thus become 787 millions, which is a falling-off of 35 millions, or 4 per cent. only, from the figure of 822 millions, which should have been reached if the rate of growth had been the same between 1875 and 1885 as between 1865 and 1875. Allowing for the raising of the lower limit of the income-tax in the interval, this is really no decrease at all.

Of course this comparison may be thrown out if we are to assume the difference made by the fall of prices on the income-tax assessments to be 15 or 10 per cent. only, instead of 20 per cent. But a point like this would involve a most elaborate discussion, for which this address would hardly be the occasion. I hope to find a better opportunity shortly in a continuation of my essay of ten years ago on the accumulations of capital in the United Kingdom. There is no doubt, however, that an allowance must be made for the difference of prices, and when any such allowance is made the rate of material growth would not appear to be so very much less between 1875 and 1885 than in the period just before, as it does in the above figures.

Another broad fact not easily reconcilable with the fact of a great diminution in the real rate of material growth in the last ten years is the steadiness of the increase of population and the absence of any sign, such as an increase in the proportion of pauperism, indicating that the people are less fully employed than they were. The increasing numbers must either be employed or unemployed, and if there is an increase in the proportion of the unemployed the fact should be revealed in the returns of pauperism somehow. The existence of trade unions, no doubt, prevents many workmen coming on the rates who might formerly have done so, but there are large masses of workmen, the most likely to feel the brunt of want of employment, to whom this explanation would not apply.

What we find, however, is that population has increased as follows: between 1855 and 1865 from 27,800,000 to 29,900,000, or 7½ per cent.; between 1865 and 1875 from 29,900,000 to 32,800,000, or nearly 10 per cent.; and between 1875 and 1885 from 32,800,000 to 36,300,000, or over 10 per cent. If it is considered that the figures are not fairly comparable for the early period, owing to the specially large emigration from Ireland, which took away from the apparent numbers of the United Kingdom as a whole, but still allowed of as great an increase in the manufacturing parts of the country as there has been later, then we may take the figures for England only, and what we find is—between 1855 and 1865 an increase from 18,800,000 to 21,100,000, or 12½ per cent.; between 1865 and 1875 from 21,100,000 to 24,000,000, or nearly 14 per cent.; and between 1875 and 1885 from 24,000,000 to 27,500,000, or 14½ per cent. Whether, therefore, we take the figures for the United Kingdom or for England only, what we find is a greater increase of population in the last ten years than in either of the previous decades when the rate of material growth seemed so much greater. If there had been such real diminution in the rate of material growth, ought there not to have been some increase in the want of employment and in pauperism to correspond?

It is one of the most notorious facts of the case, however, that there has been no increase, but instead a very steady decrease of pauperism, excepting in Ireland, which is so small, however, as not to affect the general result. As regards England the figures are very striking indeed. The average number of paupers and proportion to population have been as follows in quinquennial periods in England since 1885:—

| | Number of Paupers. | Proportion to Population per cent. |
|---|---|---|
| 1855–59 ... ... | 895,000 | 4·7 |
| 1860–64 ... ... | 948,000 | 4·7 |
| 1865–69 ... ... | 962,000 | 4·5 |
| 1870–74 ... ... | 952,000 | 4·2 |
| 1875–79 ... ... | 753,000 | 3·1 |
| 1880–84 ... ... | 787,000 | 3·0 |

Thus there has been a steady diminution in the proportion to the population all through, accompanied by a diminution in the absolute numbers between 1865–69 and 1875–79, though there has since been a slight increase. In spite of all that can be urged as to a more stringent Poor-Law administration having made all the difference, it is difficult to believe that a real falling-off of a serious kind in the rate of our material growth in late years as compared with the period just before should not have led to some real increase of pauperism. Change of administra-

tion may do much, but it cannot alter the effect of any serious increase in the want of employment in a country.

The corresponding figures as to Scotland are much the same :—

| | Number of Paupers. | Proportion to Population per cent. |
|---|---|---|
| 1855–59 ... ... | 123,000 | 4·2 |
| 1860–64 ... ... | 125,000 | 4·2 |
| 1865–69 ... ... | 131,000 | 4·3 |
| 1870–74 ... ... | 123,000 | 3·7 |
| 1875–79 ... ... | 103,000 | 2·9 |
| 1880–84 ... ... | 100,000 | 2·7 |

Here there is the same steady diminution in the proportion of pauperism to population all through as we have seen in the case of England, accompanied in this case by a steady diminution of the absolute number of paupers since 1865–69. The Scotch administration has been totally independent of the English, but the same results are produced.

In Ireland, as already hinted, the history has been different. There has been an increase in the pauperism accompanied by a decline of population. But Ireland is too small to affect the general result.

We are thus confronted by the fact that if there had been a real check of a serious kind to the rate of our material growth in the last ten years as compared with the ten years just before, there ought to have been some increase in the want of employment and in pauperism, but instead of there being such an increase there is a decline. The population apparently, while increasing even more rapidly in the last ten years than before, has been more fully employed than before. To make these facts consistent with a check to the rate of our material growth we must contrive some such hypothesis as that employment has been more diffused as regards numbers, but the aggregate amount of it has fallen off—another form of the hypothesis as to the effect of shorter hours of labour already discussed ; but a little reflection will show that any such hypothesis is hardly admissible. It is difficult to imagine any change in the conditions of employment in so short a time which would make it possible for larger numbers to be employed along with a diminution in the aggregate amount of employment itself.

Another fact corresponding to this decrease of pauperism is the steady increase of savings-bank deposits and depositors. These deposits are not, of course, the deposits of working classes only, technically so called. They include the smaller class of tradesmen and the lower middle classes generally. But, *quantum valeant*, the facts as to a growth of deposits and depositors should reflect the condition of the country generally in much the same way as the returns of pauperism. What we find then is, as regards deposits, that the increase between 1855 and 1865 was from £34,300,000 to £45,300,000, or about one-third ; between 1865 and 1875 from £45,300,000 to £67,600,000, or about one-half ; and between 1875 and 1885 from £67,600,000 to £94,053,000, or just about 40 per cent.—a less increase than in the previous ten years, but not really less, perhaps, if allowance is made for the fall of prices in the interval, and in any case a very large increase. Then, as regards depositors, what we find is an increase between 1855 and 1865 from 1,304,000 to 2,079,000, or 59 per cent. ; between 1865 and 1875 from 2,079,000 to 3,256,000, or 56 per cent. ; and between 1875 and 1885 from 3,256,000 to over 5,000,000, or over 50 per cent. Whatever special explanations there may be, facts like these are at least not inconsistent with a fuller employment of the population in the last ten years than in the previous ten.

Yet another fact tending to the same conclusion may be referred to. The stationariness or slow growth of the income-tax assessments in general in the last ten years, as compared with the rapid increase in the ten years just before, has already been referred to as one of the signs indicating a check in the rate of advance in our material growth. But when the returns are examined in detail there is one class of assessments, more significant, perhaps, than any, of the general condition of the nation, viz. houses, which is found to exhibit as great an increase in the last ten years as in the previous decade. Between 1865 and 1875 the increase in the item of houses in the income-tax assessments in the United Kingdom was from £68,800,000 to £94,600,000, or just about 37 per cent. In the following ten years the increase was from £94,600,000 to £128,500,000, or just about 36 per cent. In "houses," then, as yet there is no sign of any check to the general rate of the material growth of the country. Allowing, in fact, for the great fall in prices in the last ten years, the real increase in houses would seem to have been more in the last ten years than in the ten years just before.

Other facts, such as the increase of Post Office business, may be referred to as tending to the same conclusion. But there is no need to multiply facts. If no hypothesis is to be accepted except one that reconciles all the facts, then these facts as to the increase of population, diminution of pauperism, increase of savings-bank deposits and depositors, increase of houses, must all be taken into account, as well as those signs as regards production and other factors, which have usually been most dwelt upon in discussing the question of the accumulation of wealth and the material growth of the people. If the signs of a check to production in some directions can be reconciled with the fact of an unchecked continuance of the former rate of growth generally, then the later facts cited as to increase of population, diminution of pauperism, and the like, may be allowed to have their natural interpretation and to be conclusive on the point.

Such a general explanation, then, of the facts as to production in leading industries and the like, referred to in the earlier part of this address, consistent with the fact that there is no serious falling-off in the rate of our material growth generally, is to be found in the supposition that industry by a natural law is becoming more and more miscellaneous, and that as populations develop the disproportionate growth of the numbers employed in such miscellaneous industries, and in what may be called incorporeal functions, that is, as teachers, artists, and the like, prevents the increase of staple products continuing at the former rate. This supposition, it will be found, has a good deal to support it in the actual facts as to industry and population in recent years.

The foreign trade shows some sign of the change that is going on. Looking through the list of export articles some remarkable developments are to be noticed. The following short table speaks for itself :—

*Exports of the undermentioned Articles in the Years stated, with the Rates of Increase in* 1855–65, 1865–75, *and* 1875–85 *compared.*

| | Quantities exported. | | | | Increase per cent. | | |
|---|---|---|---|---|---|---|---|
| | 1855. | 1865. | 1875. | 1885. | 1855–65. | 1865–75. | 1875–85. |
| Candles, million lbs. ... ... ... ... ... | 4 | 4 | 5·3 | 7·8 | Nil | 33 | 47 |
| Cordage and twine, thousand cwts. ... ... | 110 | 168 | 111 | 177 | 53 | −34 [2] | 59 |
| Plate glass, million sq. ft. ... ... ... ... | 0·3 | 0·6 | 1·6 | 3·9 | 100 | 166 | 143 |
| Jute yarn, million lbs. ... ... ... ... ... | not stated | 4·9 | 15·9 | 30·7 | — | 224 | 93 |
| Jute manufacture, million yds. .. ... ... | ,, | 15·4 | 102·1 | 215 | — | 563 | 110 |
| Iron hoops, sheets, &c. thousand tons... ... | ,, | 116 | 204 | 331 | — | 76 | 62 |
| Tinned plates, thousand tons ... ... ... | ,, | 63 | 138 | 298 | — | 119 | 116 |
| Other wrought iron, thousand tons ... ... | ,, | 214 | 239 | 348 | — | 12 | 45 |
| Oil and floor cloth, million sq. yds. ... ... | 0·5 | 2·4 | 6·3 | 11·3 | 380 | 162 | 79 |
| Paper other than hangings, thousand cwts.... | 106·1 [1] | 145 | 319 | 733 | 37 | 120 | 130 |
| Dressed skins and furs, millions ... ... ... | not stated | not stated | 0·37 | 3·45 | — | — | 832 |
| Soap, thousand cwts. ... ... ... ... ... | 205 | 140 | 251 | 402 | −32 [2] | 79 | 60 |
| Spirits, million gals. ... ... ... ... ... | 3·8 | 2·0 | 1·0 | 2·7 | −47 [2] | −50 [2] | 170 |
| Unenumerated, values, millions ... ... ... | — | — | £9·7 | £10·6 | — | — | 10 |

[1] 1858, not separately stated before.

[2] Decrease.

Thus there are not a few articles, of which jute is a conspicuous example, in which there has been an entirely new industry established within a comparatively short period; and, though the percentage of increase may not in all be so great in the last ten years as in the previous ten just because the industry is so wholly new, yet the amount of the increase is as great or greater. In other articles, such as soap and British spirits, there is a new start in the last ten years after a decline in the previous periods. Such cases as oil and floor cloth, paper other than hangings, and plate glass are also specially noticeable as practically new trades. The list I am satisfied could be considerably extended, but I am giving it mainly by way of illustration. Finally, there is the item of other articles not separately specified—an item which is always changing in the statistical abstract because every few years one or more articles grow into sufficient importance to require separate mention, so that any extended comparison of this item for a long series of years is impossible. Still it is ever growing, and what we find in the last ten years is that, in spite of the fall of prices, the growth is from £9,700,000 to £10,600,000, or nearly 10 per cent. Many of the articles referred to, it is plain, cannot run into much money, but the indications of a tendency are none the less clear. What is happening in the foreign trade is happening, we may be sure, in the home trade as well, of which in another way the increase in the imports of foreign manufactures, already referred to in another connexion, is really a sign, as it implies the growth of miscellaneous wants among the consumers.

The census figures as to occupations tend, I believe, to confirm this observation as to the special growth of miscellaneous industries, but the discussion of the figures would require more preparation than I have had time for, and perhaps more space than can well be spared.

As to the growth of incorporeal functions, which is another fact significant of the supposed change in the direction of the employments of the people, I propose to appeal to the testimony of the census figures. I need refer on this head only to the paper read some time ago to the Statistical Society by Mr. Booth. Among those classes of population whose numbers in England and Wales in the last ten years have shown a disproportionate growth are the following :—

*Numbers and Percentage of Self-supporting Population employed.*

| | Numbers. | | Percentage. | |
|---|---|---|---|---|
| | 1871. | 1881. | 1871. | 1881. |
| Transport ... ... ... | 524,000 | 654,000 | 4·9 | 5·6 |
| Commercial Class ... ... | 119,000 | 225,000 | 1·1 | 1·9 |
| Art and Amusement ... | 38,000 | 47,000 | 0·3 | 0·4 |
| Literature and Science ... | 7,000 | 9,000 | — | 0·1 |
| Education ... ... ... | 135,000 | 183,000 | 1·3 | 1·6 |
| Indefinite ... ... ... | 124,000 | 269,000 | 1·2 | 2·3 |
| Total ... ... | 947,000 | 1,387,000 | 8·8 | 11·9 |

Following the indication of these figures, whatever qualification they may be subject to, we are apparently justified in saying that an increasing part of the population has been lately applied to the creation of incorporeal products. Their employment is industrial all the same. The products are consumed as they are produced, but the production is none the less real. If a nation chooses to produce more largely in this form as it becomes more prosperous, so that there is less development than was formerly the case in what were known as staple industries, it need not be becoming poorer for that reason; all that is happening is that its wealth and income are taking a different shape.

It is quite conceivable, then, and is in truth not improbable, that a check to the former rate of material growth in certain directions may have taken place of late years without any corresponding check to the rate of material growth generally, which would seem to be inconsistent with such facts as the growth of population, diminution of pauperism, increase of houses, and the like. The truth would seem to be that with the growth of staple industries, such as cotton, wool, coal, and iron, up to a point, there being reasons for the remarkably quick development of each for many years up to 1875, there comes a growth of new wants, the satisfaction of which drafts a portion of the national energy in new directions. Just because certain staples developed themselves greatly between 1855 and 1875 the time was likely to arrive when they would grow not quite so fast. For the same reason the rapid increase for a certain period in the consumption per head of articles like sugar and tea was likely to be followed by a less rapid increase, the wants of consumers taking a new direction. Probably owing to the more and more miscellaneous character of modern industry, it will become more and more difficult to follow its development by dealing with staple articles only, while changes in aggregate values are untrustworthy as indications of real changes owing to changes in prices. Already there seems to be no doubt the staple articles are no longer a sufficient indication.

A supplementary explanation may be added which helps to explain another difficulty in the matter by which people are puzzled. I can imagine them saying that it is all very well to pooh-pooh the non-increase or slower increase of the production of staple articles and to assume that industry is becoming more and more miscellaneous; but other countries go on increasing their production of these same staple articles. The increase of the manufactures of cotton, wool, coal, and iron in Germany and the United States, they will say, has in recent years been greater in proportion than in England, which is undoubtedly true. The explanation I have to suggest, however, is that the competition with the leading manufacturing country, which England still is, is naturally in the staple articles where manufacturing has been reduced to a system, the newer and more difficult manufactures and the newer developments of industry generally falling as a rule to the older country. Even in foreign countries, however, there are signs of slower growth of recent years in the staple articles as compared with the period just before. In Germany, for instance, the production of coal increased between 1860 and 1866 (I take the years which I find available in Dr. Neumann Spallart's "Uebersichten") from 12,300,000 tons to 28,200,000, or nearly 129 per cent.; between 1866 and 1876 the increase was from the figure stated to about 50,000,000 tons, or about 77 per cent. only; between 1876 and 1885, another period of ten years, from the figure stated to 74,000,000 tons, or less than 50 per cent.—a rapidly diminishing rate of increase. In the United States of America the corresponding figures for coal are 15, 22, 50, and 103 million tons, showing a greater increase than in Germany, but still a rather less rate of increase since 1876 than in the ten years before. The experience as to the iron production would seem to be different, the increase in the United States and Germany having been enormously rapid in the last ten years; but I have not been able here to carry the figures far enough back for comparison. Still the facts as to coal in Germany are enough to show how rapidly the rate of increase of growth may fall off when a certain point is reached, and that the experience of the United Kingdom is by no means exceptional. As the staple articles develop abroad the rate of increase in such articles will diminish too, and foreign industry in turn will become more and more miscellaneous.

The conclusion would thus be that there is nothing unaccountable in the course of industry in the United Kingdom in the last ten years. In certain staple industries the rate of increase has been less than it was in the ten years just before, but there would seem to have been no increase or little increase in the want of employment generally, while there is reason to believe that certain miscellaneous industries have grown at a greater rate than the staple industries, or have grown into wholly new being, and that there has also been some diversion of industry in directions where the products are incorporeal. These facts also correspond with what is going on abroad, a tendency to decline in the rate of increase of staple articles of production being general, and industry everywhere following the law of becoming more miscellaneous. Abroad also, we may be sure, as nations increase in wealth the diversion of industry in directions where the products are incorporeal will also take place. What the whole facts seem to bring out, therefore, is a change in the direction of industry of a most interesting kind. If we are to believe that the progress of invention and of the application of invention to human wants continues and increases, no other explanation seems possible of the apparent check to the rate of material growth which seems to be so nearly demonstrated by some of the statistics most commonly appealed to in such questions.

At the same time I must apply the remark which I applied at the earlier stage to the opposite conclusion that there had been a real check to the rate of increase in our material growth. When

the main statistics bearing on a particular point all indicate the same conclusion, it is not difficult to reason from them and to convince all who study them; but when the indications are apparently in conflict it would be folly to dogmatize. I have indicated frankly my own opinion, but I, for one, should like the subject to be more fully threshed out. It is a very obvious suggestion, moreover, that one may prove too much by such figures—that it is an outrage on common-sense to talk of there being no check to the rate of growth in the country when times are notoriously bad and everybody is talking of want of profit. What I should suggest finally, by way of a hypothesis reconciling all the facts, would be that probably there is some check to the rate of material growth in the last ten years, though not of the serious character implied by the first set of figures discussed; that this check may even be too small to be measured by general statistics though it is sufficient to account for no small amount of *malaise;* and that the *malaise* itself is largely accounted for, as I have suggested on a former occasion, by the mere fall of prices, whatever the cause, as it involves a great redistribution of wealth and income, and makes very many people feel poorer, including many who are not really poorer, but only seem so, and many who are really richer if they only allowed properly for the increased purchasing power of their wealth. All these facts are quite consistent with the fact of a very slight real diminution in the rate of our material growth generally, and with that change in the direction of the national industry, significant of a general change beginning throughout the world which would seem to have occurred.

To some extent also it ought to be allowed that the tendency in the very latest years seems unsatisfactory, and that the developments of the next few years should be carefully watched. Up to now there is nothing really alarming in the statistics when they are analyzed and compared. It may be the case, though I do not think it is the case, that causes are in operation to produce that great check and retrogression which have not as yet occurred, though many have talked as if they had occurred. The exact limits of the discussion should be carefully kept in mind.

Fortunately, however, there is no doubt what some of the conclusions on practical points should be. If it be the case that the hold of an old country like England on certain staple industries of the world is less firm than it was, and, as I believe, must be less and less firm from period to period, owing to the natural development of foreign countries and the room there is among ourselves for development in new directions, then we should make assurance doubly sure that the country is really developing in new directions. If our dependence must be on the new advantages that have been described, such as acquired manufacturing skill, concentration of population, and the like, then we must make sure of the skill and of the best conditions of existence for the concentrated population. If, in point of fact, shorter hours of labour and taking things easy have contributed to check our rate of progress slightly, there is all the more reason for improving the human agent in industry so as to make work in the shorter hours more efficient. Looking at the stir there now is about technical education and such matters, and the hereditary character of our population, I see no cause to doubt that the future will be even more prosperous than the past. The national life seems as fresh and vigorous as ever. The unrest and complaints of the last few years are not bad signs. But the new conditions must be fully recognized. The utmost energy, mobility, and resource must be applied in every direction if we are only to hold our own.

## REPORTS.

*Fourth Report of the Committee, consisting of Prof. Balfour Stewart (secretary), Profs. Stokes, Schuster, G. Johnstone Stoney, Sir H. E. Roscoe, M.P., Captain Abney, and Mr. G. J. Symons, appointed for the purpose of considering the best methods of recording the Direct Intensity of Solar Radiation.*—In their last report the Committee gave a description of a copper inclosure which had been constructed by them. This consisted of a copper cube 3½ inches square outside, the faces of which were ⅛ of an inch thick. The cube was packed round with felt $\frac{3}{16}$ of an inch thick, and the whole was faced outside with thin polished brass plates. Thermometers were inserted into that side of the cube intended ultimately to face the sun, and into the opposite side, by means of which the temperature of these sides could be accurately determined. Finally, a thermometer was placed in the vacant space in the very centre of the inclosure. This last thermometer occupies the position that will ultimately be occupied by the internal thermometer, upon which the sun's rays are to fall through a hole; only at this stage the hole had not been constructed. It is obvious that when the instrument is finally in action, with a beam of solar rays (condensed by means of a lens so as to pass through the hole) falling upon the bulb, this thermometer will be subject to a heating effect from two separate causes. (*a*) It will, first of all, be subject to radiation and convection from the surrounding inclosure, which is gradually (let us suppose) getting hot through exposure to the sun. (*b*) It will, secondly, have a beam of solar rays of constant size and of constant intensity (except as to variations arising from atmospheric absorption, seasonal change in the sun's apparent diameter, or change in the sun's intrinsic radiation) continuously thrown upon it through the hole. In fine days when there is no abrupt variation of the sun's intensity the temperature of the internal thermometer will remain sensibly constant, or at least will only vary slowly with the sun's altitude; and this temperature will be such that the heat lost by radiation and convection from the internal hot thermometer will be equal to the heat which it gains from the sources (*a*) and (*b*), save as to a small correction, calculable from the slow variation of the temperature of the thermometer. Now, our object being to estimate accurately the intensity of source (*b*), we must be able, notwithstanding the gradual heating of the inclosure, to determine how much heat the internal thermometer gains from source (*a*). That is to say, we must be able to tell what would be the temperature of the internal thermometer if the instrument were still made to face the sun, but without any aperture. For the solid angle subtended by the hole at any point of the bulb is so small that we may regard it as a matter of indifference whether there be a hole or not, except as to the admission or exclusion of direct solar radiation. It was suggested by Prof. Stokes that a simple practical method of doing this would be to expose the instrument, without a hole, to an artificial source of heat, such as a fire or a stove, the intensity of which might likewise be made to vary. By this means the conditions of the instrument when facing the sun might be fairly represented. Experiments of this nature were made at Manchester by Mr. Shepherd, acting under the superintendence of Prof. Stewart, and these were reduced by Prof. Stokes. It was ascertained from these experiments that the internal thermometer represented with great exactness the temperature of the cube such as it was 3½ minutes before; in other words, there was a lagging time of the internal thermometer equal to 3½ minutes. We may thus find what would be the reading of the internal thermometer if the balance were perfect between the gain of heat by direct solar radiation and the loss of heat by communication to the environment; and as the latter is approximately proportional to the difference of temperature of the envelope and internal thermometer, and the deviation from exact proportionality admits of determination by laboratory experiments, we have the means of measuring the former. We must bear in mind that the lagging time of the final thermometer may be different from that of the thermometer with which the experiments were made. It was likewise ascertained that the difference between the temperature of the internal thermometer and that of the case need not exceed 20° Fahr., and that a comparatively small lens and hole would suffice for obtaining this result. In consequence of this preliminary information, we have made the following additions to the instrument described in our last report:—(1) We have had it swung like the ordinary actinometers with a motion in altitude and azimuth, and with two moderately delicate adjusting-screws, one for azimuth and another for altitude adjustments. (2) We have had a thermometer centrically placed in the interior. The graduation of the stem is very delicate, and extends from 20° to 120° Fahr., the reading being taken from one of the sides. The bulb is of green flint, and the stem of colourless glass. (3) We have also had a small plate of quartz cut and polished and mounted so as to cover the hole, and to be easily removed and replaced. The object of the plate is to prevent irregularities arising from irregular issue of heated air through the hole, entrance of cooler air blown in by wind, &c., and the choice of material was influenced by the wish to permit of frequent cleaning without risk of alteration by scratching. We ought to mention that as it would be difficult to procure the loan of a good heliostat, and expensive to make, we resolved that in the preliminary experiments the adjustments to keep the sun's image on the hole should be made by the observer. Hence the necessity for the adjusting-screws already described.

The Committee suggest that they should be reappointed, and that the sum of £10 be placed at their disposal to defray the expenses of further experiments connected with the instrument.

*Report of the Electrical Standards Committee, consisting of Prof. G. Carey Foster, Sir W. Thomson, Prof. Aryton, Prof. J. Perry, Prof. W. G. Adams, Lord Rayleigh, Prof. O. J. Lodge, Dr. John Hopkinson, Dr. A. Muirhead, Mr. W. H. Preece, Mr. Herbert Taylor, Prof. Everett, Prof. Schuster, Dr. J. A. Fleming, Prof. G. F. Fitzgerald, Mr. R. T. Glazebrook (secretary), Prof. J. J. Thomson, Mr. W. N. Shaw, and Mr. J. T. Bottomley.*—The Committee was appointed for the purpose of constructing and issuing practical standards for use in electrical measurements. The Committee report that the work of testing resistance coils has been continued at the Cavendish Laboratory, and a table of the values found for ten various coils is given. Of these two coils have been tested before, but, owing to the green coloration mentioned in the last report showing itself in the paraffin, the paraffin was removed and the coils refilled with ozokerit, which can be obtained more nearly free from traces of acid. This change in all cases produced an appreciable increase in resistance. Shortly after the Birmingham meeting of the Association the secretary received a letter from the Board of Trade, inclosing a copy of the general bases of a convention proposed by the French Government for the consideration of the Powers with the object of carrying out the resolution of the Paris Conference with regard to electrical standards. The convention stipulates that a legal character is to be given to (1) the legal ohm, (2) the ampere, (3) the volt, (4) the coulomb, (5) the micro-farad. These questions had been considered by the Committee at the Birmingham meeting, and the following series of resolutions, which the secretary was instructed to forward to the British Government, had been agreed to on the motion of Sir W. Thomson, seconded by Prof. W. G. Adams: (1) to adopt for a term of ten years the legal ohm of the Paris Congress as a legalized standard sufficiently near to the absolute ohm for commercial purposes; (2) that at the end of the ten years period the legal ohm should be defined to a closer approximation to the absolute ohm; (3) that the resolutions of the Paris Congress with respect to the ampere, the volt, the coulomb, and the farad be adopted; (4) that the resistance standards belonging to the Committee of the British Association on Electrical Standards now deposited at the Cavendish Laboratory at Cambridge be accepted as the English legal standards, conformable to the adopted definition of the Paris Congress. During the year the original standards of the Association have again been compared by the secretary. An account of this comparison, and of the very complete one made in the years 1879-81 by Dr. Fleming, the details of which have not been published previously, is given in the appendix. The general result of the comparison is to show that there is no evidence that any of the original coils have changed in value since the year 1876, when they were compared by Prof. Chrystal and Mr. Saunders. The Committee recommend the adoption of the watt as the unit of power. The watt is defined to be the work done per second by the ampere passing between two points between which the difference of electric potential is one volt. The Committee was also of opinion that it was highly desirable to proceed with the construction of an air condenser as a standard of capacity, and for this purpose they desire to be reappointed, with the addition of the name of Mr. Thos. Gray, and a grant of £100.

*Report of the Committee on Ben Nevis Meteorological Observations.*—The work of the Ben Nevis Observatory for the past year has been carried on by Mr. Omond and his assistants with the same intelligence, enthusiasm, and completeness as in previous years. With the two exceptions of October and November the temperature was every month below its normal. Atmospheric pressure at Fort William was very nearly the normal on the mean of the year, being only 0·012 inch under it. The maximum pressure for the year at the Observatory was 26·093 inch on November 24, and the minimum 23·45 inch on December 8. The maximum temperature for the year was 55°·8 in September, and the lowest 8°·4 in December, thus giving an absolute range of 47°·4. In addition to the regular work of the Observatory, Mr. Omond, superintendent, Mr. Rankin, first assistant, and Mr. Dickson, who has repeatedly relieved the regular observers at the Observatory, are engaged in carrying on original researches. The plotting of the observations of storms made at the sixty-four Scottish lighthouses is now far advanced. The results show a very large number of failures both of storms which have occurred of which no warning has been sent by the Meteorological Office, and of warnings issued with no accompanying or following storm. These failures are at present being investigated by the Ben Nevis observations in connexion with the observations at Fort William and other low-lying stations in Scotland. The directors of the Observatory have from the outset spoken with some earnestness on the absolute necessity of combining the double observation for all forecasting purposes—in other words, of combining the observations at the top of Ben Nevis with those made at the same instant at Fort William. The reason is obvious, it being by vertical gradients, and not by horizontal gradients, that the observations at high-level stations can be turned to their proper and fullest account in forecasting weather. Since none of the sea-level observations at Fort William are in the Meteorological Office, or indeed anywhere but in the office in Edinburgh, the opinion that the Ben Nevis observations are useless in forecasting falls to the ground. A recent discussion in Parliament, already referred to in NATURE, was then alluded to.

In the course of a discussion Prof. Cleveland Abbé said that the problems of meteorology demanded mathematical treatment more and more.

*Final Report of the Committee, consisting of Mr. R. H. Scott (secretary), Mr. J. Norman Lockyer, Prof. G. G. Stokes, Prof. Balfour Stewart, and Mr. J. G. Symons, appointed in August 1881, and reappointed in 1882-83 and 1884 to co-operate with the Meteorological Society of the Mauritius in the publication of Daily Synoptic Charts of the Indian Ocean for the year 1861.*—Your Committee have to report that the sum of £50 granted in 1881 has now been expended, and they inclose herewith a receipt for the amount, showing its disposition, from the Treasurer of the Mauritius Meteorological Society.

Dr. Meldrum, in a letter to the Secretary, dated June 4, 1887, says: "I am requested by the President and Council of our Meteorological Society to convey to yourself and the British Association their very best thanks, and to say that the Society will forward to the Association, through you, two copies of each of the publications that have been issued."

The following is a list of these publications:—

1. Daily Synoptic Weather Charts of the Indian Ocean for the months of January, February, and March, 1861. The charts for the remaining months of 1861, and remarks to accompany the months already published, are in preparation.

2. Tabular Statements of the number of gales experienced monthly between the parallels of 20° S. and 46° S., and the meridians of 0° and 120° E. during the last 39 years.

Dr. Meldrum further states that the following works are nearly ready for publication:—

I. Synoptic Weather Charts of the Indian Ocean for January 1860, in the course of which month a typical cyclone took place.

II. The Tracks of the Tropical Cyclones in the Indian Ocean, south of the Equator, from 1848 to 1886, as far as is known, together with the observations from which the tracks have been deduced.

III. The Mean Pressure and Temperature of the Indian Ocean for 5° square, in the months of January and July.

IV. Synoptic Charts of the Indian Ocean for each day, during the last 39 years, in which it is known that a cyclone existed.

V. The Average Limits in the Indian Ocean of the South-East Trade in each month, and of the North-West Monsoon from November to May.

*Fourth Report of the Committee, consisting of Prof. Balfour Stewart (secretary), Mr. J. Knox Laughton, Mr. G. J. Symons, Mr. R. H. Scott, and Mr. G. Johnstone Stoney, appointed for the purpose of co-operating with Mr. E. J. Lowe in his project of establishing on a permanent and scientific basis a Meteorological Observatory near Chepstow.*—This Committee met at 22 Albemarle Street on March 26, and passed the following resolution:—"As your Committee have heard no further results from the action referred to by Mr. Lowe in his letter quoted in their last report, and there thus appears to be an absence of local support, they see no prospect of the scheme ever being carried out. The fundamental idea presiding over the establishment of the observatory was that it should be one of permanence, and hence it is

obvious that adequate endowment is essential. To provide this, and properly equip the observatory, several thousand pounds are needed; but the Committee have no assurance that anything at all approaching the necessary amount has yet been subscribed or even promised. As they have now been in existence for between three and four years with this negative result, they are of opinion that the Committee should now be dissolved." In consequence of this resolution the Committee have not drawn the £20 voted at Birmingham, and they do not now request their reappointment.

*Report of the Committee on Tidal Observations in Canada.*—In the absence of Prof. Johnson, Mr. Robert E. Baynes presented this report. He said that no grant had yet been obtained from the Dominion Government, for though the Hudson Bay Expedition was ended, the Canadian Government had undertaken to pay half the expenses of the re-survey of the Gulf of St. Lawrence. This survey would probably take two years, but when it was concluded there was the greatest possible expectation that a special grant might be given to the Committee. In the meantime, Lieut. Gordon, commanding one of the Dominion cruisers, had been ordered to make certain preliminary observations.

*Report of the Committee on Magnetic Observations.*—The Committee had met at various intervals during the year. The subject which chiefly occupied them at present was the diurnal variation of terrestrial magnetism and the reduction of the observations. The great difficulty of the Committee was the want of proper observations in the southern hemisphere. The observations which had been made went to show that the two hemispheres were pretty well symmetrical, and at present the Committee had to take for granted that it was so. They hoped in another year to be able to give a more complete report, and some definite results.

*Report of the Committee on Standards of Light.*—The Committee have compared the standards hitherto proposed, but have not done much. Prof. Adams has, however, presented a report of some experiments, and the Committee think that if funds are provided they will be able to settle the question of standards.

*Report of the Committee on Differential Gravity Meters.*—Since last report the Committee have received from Mr. Boys an account of experiments in which he is engaged. They await the result of those experiments before proceeding with the construction of an instrument.

*Report of the Committee on the Translation of Foreign Scientific Memoirs.*—In reply to a communication from the Committee to the Royal Society, Prof. Reinold has informed them that the Royal Society is not at present able to undertake the publication of foreign memoirs in a systematic manner, but anything of special interest would be attended to.

## NOTES.

We learn that the Government of Jamaica offers a premium of £100 for the production of the best practical elementary text-book of tropical agriculture specially applicable to Jamaica, and embodying the first principles of agriculture. It is stated that the object of the manual is to create in the mind of the young an early and intelligent interest in the soil and its products, and particular attention is to be paid to simplicity, brevity, and freedom, as far as possible, from technical terms. It is stated that the propagation and cultivation of tropical economic plants should have due prominence. Manuscripts are to be forwarded to the Government of Jamaica on or before August 1, 1888.

The Iron and Steel Institute held their autumn meeting at Manchester last week. It was an entire success both as regards the papers and discussions and the excursions to industrial works and places of interest in the neighbourhood. We shall give a report of the proceedings in our next week's issue.

One point which seems to be determined by the news which has just reached Zanzibar concerning Emin Pasha is that Albert Nyanza and Muta Nzige are two distinct lakes, a point which has hitherto been doubtful. It is stated that in the recent campaign between Mwanga, King of Uganda, and his neighbours the whole country between these two lakes has been laid waste. Doubtless we shall soon have full details as to this, as well as to the results of the recent explorations, from Emin Pasha himself.

Mr. Richard Quain, F.R.S., Surgeon Extraordinary to the Queen, died on Thursday at his residence, 32 Cavendish Square, at the age of eighty-seven. He began his career in 1828, and speedily rose to high distinction. He wrote many books on medical subjects, such as "Anatomy of the Arteries of the Human Body," and was Honorary Fellow of the Medical and Surgical Society of Edinburgh, Emeritus Professor of Clinical Surgery in University College, Consulting Surgeon at University College Hospital, and President of the Royal College of Surgeons.

The Annual Congress of the Sanitary Institute of Great Britain was opened on Tuesday at Bolton. Lord Basing delivered the Presidential Address, in which he reviewed what has been done for the protection of public health since the importance of the question was brought home to the minds of legislators. An exhibition of sanitary appliances and apparatus was opened at the same time in the Drill Hall at Bolton.

The University College of Bristol has recently been enabled by the generosity of local firms to make a notable advance in the matter of engineering education. At a meeting held in the early part of the present year the desirability of instituting engineering scholarships was considered. The practical result of this meeting was that most of the firms of the neighbourhood agreed to institute bursaries, or scholarships, at their works. The holders of these are to be nominated by the College authorities. Some will be awarded on the results of the annual examinations, while others will be reserved for deserving students who may be unable to pay the usual premiums required on entrance into works. The educational scheme adopted at Bristol does not include any attempt to impart practical workshop instruction within the College walls, but the students will spend six months (April to October) in each year acquiring practical experience in the works and drawing offices of the engineers of the west of England. This system is found to answer so well that Messrs. Stothert and Pitt, of Bath, and the Bristol Wagon Works Company propose to make it obligatory on all their pupils to attend the College courses in the winter months for the first three years of their pupilage. Several firms have also signified their willingness to take College students for short periods, so that civil engineering and electrical engineering pupils may spend one or two terms of six months in works, while at the same time mechanical engineers may have experience in two or three different establishments during their College career. In return for these concessions the Council of the College has decided to permit deserving apprentices or artisans, nominated by the local engineers, to attend the College courses at reduced rates. It is expected that about nine first-class scholarships, and a larger number of second-class ones, will be available during the coming session.

Messrs. Crosby Lockwood and Co. will publish during the forthcoming season the following scientific and technical works:—"Flour Manufacture: a Treatise on Milling Science and Practice," by Frederick Kick, translated by H. H. P. Powles, illustrated; "A Dictionary of Terms used in the Practice of Mechanical Engineering"; "Practical Surveying," by George Wm. Usill; "The Mechanical Engineer's Office Book," by Nelson Foley (second edition); "British Mining: a Treatise on the History, Discovery, Practical Development, and Future Prospects of the Metalliferous Mines in the United Kingdom,"

by Robert Hunt, F.R.S. (second edition); "The Watchmaker's Hand-book," from the French of Claudius Saunier, translated and enlarged by Julien Tripplin; "Our Granite Industries," by Geo. F. Harris; "Marble and Marble-Workers," by Arthur Lee; "Tables, Memoranda, and Calculated Results for Mechanics, Engineers, Architects, Builders, Surveyors, &c.," by Francis Smith (fourth edition). Also the following new volumes in Lockwood's Series of Handy-books for Handicrafts: "The Mechanic's Workshop Handy-book," "The Model Engineer's Handy-book," "The Cabinet-Worker's Handy-book," "The Clock-Jobber's Handy-book," all by Paul N. Hasluck. Also the following new editions in Weale's Rudimentary Scientific Series: "A Treatise on Mathematical Instruments," by J. F. Heather; "The Mineral Surveyor's and Valuer's Complete Guide," by Wm. Lintern (second edition).

THE English edition of Naegeli and Schwendener's treatise on "The Microscope," by Mr. Frank Crisp and Mr. J. Mayall, will be published shortly by Messrs. Swan Sonnenschein and Co. The book was first sent to press in the autumn of 1878, was printed by April 1883, and was then entirely burnt in a fire at the printers'. It has since been revised and again printed, and will at length be in the hands of the public.

THE Syndics of the Cambridge University Press will publish early in October two works on "Elementary Chemistry." One, intended as a companion to lecture-work, is by Mr. Pattison Muir and Dr. Charles Slater; the other, intended to be used along with the book already mentioned, is a course of laboratory work by Mr. Pattison Muir and Mr. Carnegie. Both books deal with the subject of elementary chemistry in a manner somewhat different from that usually adopted in text-books.

THE International Shorthand Congress will meet in the Geological Museum, Jermyn Street, on September 26 and five following days. The inaugural address will be delivered by the Earl of Rosebery, and various papers on subjects connected with shorthand will be read. Men of science, like every other class of the community, are under a debt of gratitude to those who exercise the art of shorthand writing, and will wish the organizers of the Congress every success. We are glad to observe that one of the papers to be read is on the subject of shorthand in education, for the art is unquestionably an invaluable adjunct to any system of education, and is so useful, especially to those engaged in scientific pursuits, that it should be one of the subjects which every youth destined for a scientific career should acquire.

WE have received from the Essex Institute of Salem one of its occasional papers, describing a collection of Japanese pottery made by Prof. Morse, Director of the Peabody Academy of Science, Salem. The author is Mr. Sylvester Baxter, and his description is the first authorized account of the collection. An exhaustive work on the subject by Prof. Morse himself is preparing for publication.

*Science* states that in order to expedite the publication of short articles upon astronomical and meteorological subjects which may be prepared at Harvard College Observatory, it has been decided to print them as successive numbers of a series, which will constitute the eighteenth volume of the "Annals of the Observatory" when a sufficient amount of material has thus been collected. Each number will be published and distributed soon after it has been prepared.

DURING this month will appear, under the editorship of Dr. G. H. Rohé, a quarterly journal, the *Climatologist*, devoted to the consideration of questions in the domain of medical and sanitary climatology. As there is at present, says *Science*, no other journal in the world exclusively occupying this special field, the editor and publishers believe that there is room for such a publication. Each number will contain forty-eight quarto pages of reading-matter, the subscription price will be fifty cents per year, and the place of publication, S.E. Cor. Baltimore and South Streets, Baltimore, Md.

A SERIES of new salts, remarkable alike for their crystalline beauty and explosive proclivities, has recently been prepared by M. Klobb, of Nancy (*Ann. de Chimie et de Physique*, September 1887, p. 5). These salts, which contain at the same time groups of such opposite properties as ammonia and permanganic acid, are generally obtained by the addition of cold solutions of potassium permanganate to ammoniacal solutions of certain metallic salts; for example, with silver nitrate the compound $AgMnO_4 . 2NH_3$ is obtained as a crystalline dark-violet powder, decomposing on warming, with detonation. The salts of copper, cadmium, nickel, and zinc give analogous compounds, but it is around the salts of cobalt that the interest mainly concentrates. The ordinary simple salts of cobalt only yield compounds which are immediately oxidized, but the ammonio-cobalt salts, and especially the more stable ones known as luteo-cobalt salts, form the most interesting of the series. Luteo-cobalt permanganate, $(Co_2 . 12NH_3)6MnO_4$, is prepared by mixing concentrated solutions of luteo-cobalt chloride, $Co_2 . 12NH_3 . Cl_6$, and potassium permanganate in the proportion of one to twelve molecules, at a temperature not exceeding 60°; on cooling, the salt separates out in little black octahedra or pyramid-capped prisms belonging to the quadratic system, and exhibiting a fine lustre. If the carefully powdered crystals be warmed in a tube, they suddenly decompose with incandescence, and if the same warming operation be performed upon the crystals themselves, the instantaneous incandescence is accompanied by a loud detonation, the tube being shattered into fragments. If a crystal be struck with a hammer, a violent detonation is again the result, even powdering of the crystals in a mortar being accompanied by dangerous decrepitations. Compounds in which hydrochloric and hydrobromic acids partially replace the manganic acid have also been prepared, together with a most lovely salt of the composition $(Co_2 . 12NH_3)4MNO_4 . Cl_2—2KCl$, which forms dark-violet hexagonal plates, sometimes bearing low six-sided pyramids, and frequently grouped together in the form of six-rayed stars resembling the forms of snow-flakes. All these salts are of a more or less explosive character, but the luteo-cobalt permanganate itself is by far the most violent.

DURING last summer the Hydrographical Survey Office of Norway effected a series of soundings along the north-west coast of that country. The results have just been published. The Islands of Værö and Röst, at the extremity of the Lofodden group, were surveyed; and here the end of the great fishing-bank projecting from these islands appears to have been discovered. About 5 miles west of these islands a depth of 50 fathoms was found, the bottom being sand. Inside this the bottom gradually becomes more shallow, with occasional "skaller" or mounds. Outside the 50-fathom line the bottom gradually slopes, until about 50 miles west of Röst the depth is 100 fathoms. Here this depth runs in a line nearly north to south for a distance of about 60 miles, viz. from the southernmost islet by Röst to the latitude of Moskenæsö, one of the Lofodden Islands. South of the former islands the bank trends eastwards for about 6 miles, and, north of the latter, westwards for about 35 to 40 miles, when it again trends east and north-east. Inside the line indicated the bottom is everywhere fine sand mixed with pebbles and remains of shells. Some 75 miles west of Röst the depth was 150 fathoms, but on approaching this depth the bottom becomes clayey. Here the edge of the bank was struck, the depth oceanwards rapidly falling from 150 to 300 fathoms. About 85 miles west of Skomvær a depth of 438 fathoms was struck, the bottom being clay, and

a sounding from the series of the Norwegian North Atlantic Expedition taken 5 miles further out shows a depth of 593 fathoms with similar bottom. The lines for the 150, 200, 250, 300, and 350 fathom depths seem to run nearly parallel; but as they approach closer to the 100-fathom line of depth northwards, the bank apparently falls more abruptly into the ocean in this direction. This is borne out by former soundings along the coast of the Lofodden and Vesteraalen groups of islands. Thus outside the Islands of Langö, Andö, and Senjen, the edge of the bank will probably be found only 20 miles from the shore, whilst north of the latter island we know it sheers rapidly straight northwards from the shore. A provisional map, scale 1 : 200,000, of the districts sounded has been prepared. The discovery of the limits of this bank will, it is believed, be of great importance to the Norwegian fisheries, as it is the spawning-ground of the herring and cod which descend every year in immense shoals from the North Atlantic.

THE Report of the Trustees of the Australian Museum of Sydney for the past year shows progress in most directions. The number of visitors has increased, the collections are increasing rapidly, especially in the natural history departments, and the building is increasing in size, and is still too small. Catalogues of Australian zoology are in course of preparation, and amongst the new publications which will shortly be issued is a catalogue of shells, one of eggs, one of sponges and Medusæ, and one of Australian birds. The Trustees also append a Report from the Committee of Management of the Technological, Industrial, and Sanitary Museum, which, like so many other institutions of the same character, suffers sorely from want of adequate space. "The Curator reports that the crowded state of the Museum is inconvenient to visitors, and that, apart from locomotion having become difficult, it is now impossible for a teacher or a parent to gather young people around a show-case for purposes of instruction." We are accustomed in this crowded country to limited space and difficult locomotion, but what have they to do with such things in boundless Australia? The specimens are increasing with great rapidity owing to many valuable donations, which is all the more reason why the Museum should be properly housed.

THE last number (No. 28, vol. xii.) of the *Excursions et Reconnaissances* of Saigon contains the conclusion of Père Azemar's elaborate paper on the Stieng tribe, which was commenced in No. 27. It describes the forays, dress, ornaments, manners, religion, houses, intoxicating beverage, food, hunting, and industry of the Stiengs. The writer's knowledge of the tribe may be judged from the circumstance that he has resided amongst them as a missionary, as one of themselves, for five years. The greater part of the number is occupied with the second portion of his dictionary of the Stieng language. The letters H to V occupy nearly a hundred pages in double columns.

WE have received copies of two papers read by Mr. H. C. Russell before the Royal Society of New South Wales—one on floods in Lake George, the other on the history of floods in the Darling River—both being accompanied by excellent maps. Mr. Russell's object is to produce all the historical facts accessible to him relating to these floods, with the dates. He believes that there is a cycle of nineteen years in the occurrence of the floods.

THE Proceedings of the Liverpool Naturalists' Field Club for the year 1886-87 is largely occupied by a third "Appendix to the Flora of Liverpool," by Mr. Robert Brown. The second Appendix was published as far back as 1875, and during these twelve years much additional information has been gathered respecting the distribution of plants within the district of the Field Club. In Mr. Brown's present list special reference is made to about 168 species, while some species new to the neighbourhood and new localities are mentioned.

THE additions to the Zoological Society's Gardens during the past week include a White-crowned Mangabey (*Cercocebus æthiops*) from West Africa, presented by Mr. C. Washington Eves; two Vervet Monkeys (*Cercopithecus lalandii*) from South Africa, presented by Capt. Archibald Douglas, R.N.; a Bonnet Monkey (*Macacus sinicus*) from India, presented by Mrs. La Primandage; a Brown Capuchin (*Cebus fatuellus*) from Guiana, presented by Mr. W. R. Sheppard; a Sharp-nosed Crocodile (*Crocodilus acutus*) from Central America, presented by Mr. E. H. Blomefield; a Mississippi Alligator (*Alligator mississippiensis*) from Florida, presented by Mr. William J. Craig; four Common Chameleons (*Chamæleon vulgaris*) from North Africa, presented by Mr. H. Thornton; six Aurora Snakes (*Lamprophis aurora*) from South Africa, presented by Mr. Walter K. Sibley; a Raven (*Corvus corax*), British, deposited; a —— Ichneumon (*Urva cancrivora*) from Nepal, two Tesselated Snakes (*Tropidonotus tesselatus*), four Dark-green Snakes (*Zamenis atrovirens*), seven Common Snakes (*Tropidonotus natrix*, var.), South European, purchased.

## OUR ASTRONOMICAL COLUMN.

NEW VARIABLE.—Prof. Lewis Boss, in *Gould's Astronomical Journal*, No. 160, draws attention to the star DM. + 3° No. 766. Its magnitude in the DM. is given as 9·2m., and Argelander, who observed it twice with the Bonn meridian circle, gave it the same magnitude in the "Bonner Beobachtungen." Prof. Boss, however, was unable to find it with the Albany meridian circle in 1880 and 1881, but has since picked it up with the 13-inch equatorial of the Observatory as an 11·5m. star. It would therefore appear to be either a "temporary" star or a variable of long period.

THE DEARBORN OBSERVATORY.—The Report of the Director of the Dearborn Observatory recently issued is for the two years ending May 10, 1887. Prof. Hough's principal work is that with the great 18½-inch equatorial, and includes observations of difficult double stars and of Jupiter. During the period to which the Report refers 130 new double stars have been discovered and measured. Of these, 45 have a distance less than 0″·5, 11 have a distance between 0″·5 and 1″·0, and the remainder belong to the class of stars having very minute companions. The companion to Sirius has been measured in 1886 and also in 1887. The planet Jupiter has been systematically observed with reference to the physical phenomena on his surface, special attention having been paid, as in former years, to the great red spot. With regard to this remarkable object, Prof. Hough reports that in outline, shape, and size it has remained without material change since the year 1879. During 1885 the middle of the spot was very much paler in colour than the margins, causing it to appear as an elliptical ring. This ring-form has continued up to the present time, although during the last three years the spot has at times been so faint as to be scarcely visible. Four sketches of the planet made in 1886 are given in the Report. The appendices to the Report contain: a catalogue of 209 new double stars, and a description of a printing chronograph, by Prof. Hough; nebulæ found at the Dearborn Observatory 1866-68, by Prof. Safford; orbit of the Clark companion of Sirius, and motion of the lunar apsides, by Mr. Colbert. The last-named paper is of a "paradoxical" character, and we much regret that the Directors of the Chicago Astronomical Society should have recommended its publication.

THE SPECTRA OF HYDROGEN, OXYGEN, AND WATER VAPOUR.—Prof. Grünwald, of Prague, has recently published (*Astr. Nachr.* 2797), a brief account of a theory respecting the relationship of the spectra of gases and their compounds, which, if it should prove well founded, will be of the highest importance in the light it promises to throw on the structure of many of those substances we now call "elements." The fundamental idea is as follows:—Let [*a*] be the volume occupied by a primary chemical element, *a*, in the unit of volume of a gaseous substance, A. Let A be chemically combined with a second gaseous body, B, to form a third, C. The element *a* now takes the form *a′* and

the volume [$a'$]. Then the wave-lengths, $\lambda$, of the lines in the spectrum of A, which belong to $a$, are to the wave-lengths, $\lambda'$, of the lines in the spectrum of C, which belong to $a'$, as [$a$] is to [$a'$]. If there be no condensation the lines are the same as to their position, since the volume remains constant, though their relative intensities may vary greatly; the compounds of hydrogen with chlorine, bromine, and iodine may be cited as examples. Assuming this principle, the spectra of hydrogen and water vapour offer some very interesting relationships. Thus, the wave-lengths of the second spectrum of hydrogen, which seems to belong to a molecule, H′, of a more complicated structure, when divided by 2 give the wave-lengths of the lines of water vapour, the volume of the free molecule H′ being double that which hydrogen occupies in water vapour. The wave-lengths of the elementary spectrum of hydrogen can be arranged into two groups, $a$ and $b$, which give the lines of the water vapour spectrum when they are respectively multiplied by $\frac{19}{20}$ and by $\frac{4}{5}$. From this Prof. Grünwald concludes that hydrogen is composed of the combination of four volumes of the element $a$ with one of the element $b$. The first element, $a$, should be the lightest of all the gases, and much lighter than hydrogen; and since it should therefore probably enter largely into the constitution of the corona, Prof. Grünwald gives it the name of "coronium." The $D_3$ or "helium" line is found in the spectrum of the second element, $b$; and the Professor therefore gives $b$ the title "helium." The correspondences between the wave-lengths calculated by Prof. Grünwald for the elements $a$ and $b$ and those of lines actually observed in the spectrum of the sun are certainly striking. Following out the same method, the Professor finds the chemical formula of oxygen as follows—

$$O = H'O' = H'[b_4O''_5] = H'[b_4(b_4c_5)_5].$$

The line of the corona, 1474 K, should belong to the element "coronium," and would correspond—$5316 \times \frac{2}{3} = 3544$—to a line, as yet unknown, of the elementary spectrum of hydrogen, with wave-length 3544. Prof. Grünwald had hoped that the late eclipse would have afforded an opportunity of searching for this line. It is clear that the dissociation of hydrogen in the sun is a necessary consequence of this theory, since its two constituent elements will thus both be in the free state in the solar atmosphere.

## ASTRONOMICAL PHENOMENA FOR THE WEEK 1887 SEPTEMBER 25—OCTOBER 1.

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

*At Greenwich on September 25*

Sun rises, 5h. 52m.; souths, 11h. 51m. 42·7s.; sets, 17h. 51m.; decl. on meridian, 0° 50′ S.: Sidereal Time at Sunset, 18h. 8m.

Moon (one day after First Quarter) rises, 14h. 54m.; souths, 19h. 14m.; sets, 23h. 36m.; decl. on meridian, 19° 26′ S.

| Planet. | Rises. h. m. | Souths. h. m. | Sets. h. m. | Decl. on meridian. ° ′ |
|---|---|---|---|---|
| Mercury ... | 6 57 | 12 34 | 18 11 | 5 6 S. |
| Venus ... ... | 5 43 | 11 16 | 16 49 | 6 1 S. |
| Mars ... ... | 1 38 | 9 9 | 16 40 | 16 30 N. |
| Jupiter... ... | 9 7 | 14 3 | 18 59 | 12 56 S. |
| Saturn... ... | 0 20 | 8 10 | 16 0 | 19 30 N. |

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

| Sept. | Star. | Mag. | Disap. h. m. | Reap. h. m. | Corresponding angles from vertex to right for inverted image. ° ° |
|---|---|---|---|---|---|
| 25 ... | *f* Sagittarii ... | 5 ... | 23 27 ... | 0 30* ... | 125 330 |
| 26 ... | B.A.C. 7053 ... | 5½ ... | 17 35 ... | 18 55 ... | 73 270 |
| 26 ... | *o* Capricorni ... | 5½ ... | 17 36 ... | 18 55 ... | 73 269 |
| 27 ... | *v* Capricorni ... | 5½ ... | 0 1 ... | 0 48 ... | 93 7 |
| 28 ... | 42 Aquarii ... | 6 ... | 22 14 ... | 23 1 ... | 65 1 |

* Occurs on the following morning.

*Meteor-Showers.*

| | R.A. ° | Decl. ° | |
|---|---|---|---|
| Near δ Aurigæ ... ... | 78 ... | 57 N. ... | Swift. |
| From Lynx ... ... | 105 ... | 51 N. ... | Very swift. |

*Variable Stars.*

| Star. | R.A. h. m. | Decl. ° ′ | | h. m. | |
|---|---|---|---|---|---|
| U Cephei ... ... | 0 52·3 ... | 81 16 N. ... | Sept. 28, | 5 34 | *m* |
| R Ceti ... ... ... | 2 20·3 ... | 0 41 S. ... | ,, 28, | | *M* |
| Algol ... ... ... | 3 0·8 ... | 40 31 N. ... | Oct. 1, | 4 1 | *m* |
| λ Tauri... ... .. | 3 54·4 ... | 12 10 N. ... | Sept. 26, | 22 36 | *m* |
| | | | ,, 30, | 21 28 | *m* |
| R Boötis ... ... | 14 32·2 ... | 27 14 N. ... | ,, 28, | | *M* |
| δ Libræ ... ... | 14 54·9 ... | 8 4 S. ... | ,, 26, | 3 13 | *m* |
| U Coronæ ... ... | 15 13·6 ... | 32 4 N. ... | ,, 29, | 21 59 | *m* |
| R Scorpii ... ... | 16 10·9 ... | 22 40 S. ... | ,, 28, | | *M* |
| U Ophiuchi... ... | 17 10·8 ... | 1 20 N. ... | ,, 26, | 4 37 | *m* |
| | | | and at intervals of | 20 8 | |
| X Sagittarii... ... | 17 40·5 ... | 27 47 S. ... | Sept. 28, | 23 0 | *m* |
| | | | Oct. 1, | 20 0 | *M* |
| W Sagittarii ... | 17 57·8 ... | 29 35 S. ... | ,, 1, | 19 0 | *m* |
| β Lyræ... ... ... | 18 45·9 ... | 33 14 N. ... | Sept. 25, | 4 0 | $m_2$ |
| R Lyræ ... ... | 18 51·9 ... | 43 48 N. ... | Oct. 1, | | *m* |
| S Vulpeculæ ... | 19 43·8 ... | 27 0 N. ... | Sept. 30, | | *M* |
| η Aquilæ ... ... | 19 46·7 ... | 0 43 N. ... | ,, 26, | 3 0 | *m* |
| S Sagittæ ... ... | 19 50·9 ... | 16 20 N. ... | ,, 25, | 3 0 | *m* |
| | | | ,, 28, | 3 0 | *M* |
| R Vulpeculæ ... | 20 59·4 ... | 23 22 N. ... | ,, 30, | | *m* |
| δ Cephei ... ... | 22 25·0 ... | 57 50 N. ... | ,, 28, | 5 0 | *M* |
| | | | Oct. 1, | 23 0 | *m* |

*M* signifies maximum; *m* minimum; $m_2$ secondary minimum.

## THE UNWRITTEN CHAPTER ON GOLF.[1]

THERE are two ways of dealing with a difficulty—the metaphysical and the scientific way. The first is very simple and expeditious—it consists merely in giving the Unknown a name whereby it may be classified and categorized. Thenceforward the Unknown is regarded as having become part of knowledge. The scientific man goes further, and endeavours to find what lies concealed under the name. If it were possible for a metaphysician to be a golfer, he might perhaps occasionally notice that his ball, instead of moving forward in a vertical plane (like the generality of projectiles, such as brickbats and cricket-balls), skewed away gradually to the right. If he did notice it, his methods would naturally lead him to content himself with his caddie's remark—"Ye heeled that yin," or, "Ye jist slicet it" (we here suppose the metaphysician to be right-handed, as the sequel will show). But a scientific man is not to be put off with such flimsy verbiage as this. He *must* know more. What is "heeling," what is "slicing," and why would either operation (if it could be thoroughly carried out) send a ball as if to cover-point, thence to long slip, and finally behind back-stop? These, as Falstaff said, are "questions to be asked."

As the most excellent set of teeth, if but one incisor be wanting, gives pain rather than pleasure to the beholder; so is it with the works of the magnificent Clark, the sardonic Hutchinson, and the abstruse Simpson. These profess to treat of golf in theory as well as in practice. But in each a chapter is wanting, that which ought to deal with "slicing," "heeling," "toeing," "topping," &c., not as metaphysical abstractions enshrined in homely though unpleasant words, but as orderly (or disorderly) events due to physical causes and capable of receiving a physical explanation. Mayhap, with the aid of scissors and paste, some keen votary of the glorious game will employ this humble newspaper column to stop, however imperfectly and temporarily, the glaring gap which yawns in the work of every one of its exponents! If so, this scrap will not have been written in vain. It may even, in the dim future, lead some athletic pundit to elaborate *The Unwritten Chapter*.

Every one has heard of the uncertain flight of the projectile from Brown Bess, or from the old smooth-bore 32-pounders, and of the introduction of rifling to insure steadiness. Now, all that rifling secures is that the ball shall rotate about an axis nearly in its line of flight, instead of rotating (as the old smooth-bore projectiles did) about an axis whose direction is determined by one or more of a number of trivial circumstances whose effects cannot be calculated, barely even foreseen. Thus it appears that every deviation of a spherical projectile from its line of flight (excluding, of course, that due to gravity) is produced by rotation about an axis perpendicular to the line of flight.

[1] From *The Scotsman*, August 31, 1887.

This question was very skilfully treated by Magnus in 1852. He showed by experiment that, when a rotating sphere is exposed to a current of air whose direction is perpendicular to the axis of rotation, the side of the sphere which is advancing to meet the current is subject to greater pressure than is that which is moving in the direction of the current. This difference of pressures tends to make the sphere move in a direction perpendicular at once to the current and to the axis of rotation—the direction, in fact, in which the part of the sphere facing the current is being displaced. But it is a matter of no consequence whether the current of air comes against the sphere, or the sphere moves in the opposite direction (and with the same speed) through still air. Hence Magnus's experimental result amounts to this :—*If a spherical ball be rotating, and at the same time advancing in still air, it will deviate from a straight path in the same direction as that in which its front side is being carried by the rotation.*

The physical explanation of the difference of pressures in question requires analysis which would be altogether out of place in an article like this. But, even without it, we feel ourselves to be on perfectly safe ground when we recollect that Magnus's result was obtained by direct experiment, and therefore expresses a physical truth.

Bearing in mind the statement italicized above, let us now consider the anomalous behaviour of a golf ball. The key of the position is "slicing." He who understands this will, without much further trouble, master the rest of the difficulties above referred to. Slicing is effected by the player's drawing the club towards his body while it is in the act of striking the ball. The ball is thus treated almost precisely as is a whipping-top—*i.e.* it is not merely driven forwards, but is made to spin about a nearly vertical axis. The side of the ball to which the club was applied was drawn in towards the player. Hence, as the ball advances, its front is moving towards the player's right, and the deviation takes place to that side accordingly.

A "topped" ball "dooks" (*i.e.* plunges, as it were, headlong downwards). We can see at once that it should be so, in accordance with the general statement. For, in topping, the upper part of the ball is made to move forward faster than does the centre, consequently the front of the ball descends, in virtue of the rotation, and the ball itself skews in that direction.

When a ball is "under-cut" it gets the opposite spin to the last, and, in consequence, it tends to deviate upwards instead of downwards. The upward tendency often makes the path of a ball (for a part of its course) concave upwards in spite of the effects of gravity. This is usually regarded as a very strange phenomenon, even by men to whom "dooking" seems natural enough. As will be seen later, a "jerked" ball must, from the way in which the face of the club is moving at impact, have this spin, and consequently must skew upwards.

Since a "heeled" ball deviates to the right as a "sliced" ball does, it must be rotating in a similar manner. But a "toed" ball deviates to the left, and must, therefore, have the opposite spin. The way in which the spin is produced in these cases is not so easy to explain as it was in the case of topping. We may begin, however, by saying that the terms "heeling" and "toeing" are entirely misleading, if they be taken to imply necessarily the hitting of the ball with the heel or the toe of the club as the case may be. For, as will soon appear, a ball may be heeled off the toe of a club, or toed off the heel, at pleasure! And when a man holds his club properly, so that in the act of striking the ball *the club-head is moving in a direction exactly perpendicular to the face*, there will be neither heeling nor toeing whatever part of the face strikes the ball, provided it be struck by the face proper, and not by an edge. It will not be driven so far by the heel, or by the toe, as by the proper centre of percussion; but there will be no spin, and therefore no skewing.

The true explanation, therefore, of heeling and toeing is to be found in the fact that the club-head, when it strikes the ball, is not moving perpendicularly to the face; or, what comes practically to the same thing, the face of the club is not perpendicular to the direction in which the club is moving (*i.e.* it is to be presumed the direction which it is desired that the ball should take). In this case we may regard the motion of the head as resolved into two parts—one perpendicular to the face, the other parallel to it. The former gives translation only to the ball. The latter gives it not only translation, but rotation also. When the toe of the club is too much thrown back—*i.e.* when the heel is too much forward—the motion parallel to the face is from toe to heel, exactly as in "slicing." "Heeling" and "slicing" are thus practically the same thing, so far at least as the ball is concerned. But, so far as the player is concerned, they are quite different; and (what is of far more importance) the modes of cure are entirely dissimilar. To cure slicing, cease to pull in your arms; to cure heeling, place your club beside the ball as in addressing, and note the lie of the head. If that be incorrect, put it right; if it be correct, the fault lies in "gripping" (instead of holding loosely) with your right hand. Many a man's play has been spoiled for the day by his having applied (too often by his caddie's advice) the cure for "heeling" when the disease was "slicing," or *vice versâ*.

When the toe of the club is turned inwards, the face is pushed tangentially outwards behind the ball, so that the spin and its consequences are exactly the reverse of those just described.

From what has been said above, it is obvious that the flight of a ball, if it be nearly spherical and have its centre of gravity at its centre, depends solely upon the impulse originally given to it. [If the centre of gravity be not in the centre of the ball, it is only by mere chance (in teeing) that the ball escapes having a rapid rotation given to it, even by the most accurate of drivers. Should it fortunately escape initial rotation, still its flight cannot be regular. A simple and exceedingly expeditious test of this defect consists in placing the ball on mercury in a small vessel. If, in that position, it oscillates rapidly about the vertical, it should be at once rejected as absolutely worthless.] This is a point on which opinions of the wildest extravagance are often expressed. Some balls, it is said, "will not fly," &c. How if they were fired from a blunderbuss? Nobody seems to have made the trial in the only reasonable way—viz. by using a cross-bow or a catapult to give the initial speed. With such an instrument two homogeneous spherical balls of equal size and weight, whatever their other peculiarities, would be despatched under exactly the same conditions, and their behaviour could be *compared*—it would not require to be *contrasted*.

But he is correct (in meaning, though not in his English) who says that some balls "won't drive." It is easy to recognize a good ball by trial, but difficult to define one, at least without periphrasis. A good ball is one which acquires, under given conditions of good driving, as great an initial speed as possible, coupled with the minimum of rotation.

So far as we are aware, all direct scientific experiments on elastic resilience have been made at low speeds, and consequently with but slight distortion of the impinging bodies. But the circumstances of a "drive" in golf are of a totally different character; so that the results of the drive must be themselves regarded as the only data of the requisite kind which we possess. In this matter very valuable data (not for golf alone) might easily be obtained by measuring the height to which a ball rebounds when fired from a powerful catapult against a wooden or stone floor; recording on each occasion the extent to which the springs of the weapon were extended, and the appended weight which would produce the same extension. Some keen golfer may thus find thoroughly useful as well as congenial occupation, when his happy hunting-grounds are inches deep in snow. P. G. T.

## SCIENTIFIC SERIALS.

*Bulletin de l'Académie Royale de Belgique*, June.—On the problematic satellite of Venus, by Paul Stroobant. After a complete survey of the various appearances of this object between the years 1645 and 1768, the author discusses the different conjectures advanced by astronomers to explain the phenomenon. The theory of a true satellite is rejected on the ground that no orbit could be made to correspond with all the recorded observations, while the elements calculated by Lambert from some of them would make the planet ten times larger than its actual size. In the same way are disposed of the other suggestions that it might be the reflection of Venus on certain frozen particles in the atmosphere, or an inter-Mercurial planet, or a planet with a revolution slightly differing from that of Venus, or an asteroid, and the like. Several reasons are then advanced in support of the view that the pretended satellite is to be referred to certain small fixed stars near which Venus was passing when the various observations were taken. This explanation is specially obvious in one instance, where the movement attributed to the supposed satellite is precisely the proper motion, but in the opposite direction, of Venus at that moment in relation to the fixed stars.—On a specimen of crystalline iron-glance formed on some old iron

weapons, by W. Prinz. An examination of these crystals and of their physical properties, now for the first time detected on some ancient Frankish arms, shows that they are formed of specular iron, and their presence is compared with that of anhydrous ferric oxide in sedimentary deposits of all ages, produced, as on the arms in question, by the moist process at a low temperature. —On the origin of the curative effects of hypnotism, by J. Delbœuf. The author, who is one of the founders of the new branch of the medical art, based on the application of hypnotism to the cure of numerous maladies, here treats the subject as throwing light on the reciprocal action of mind on the body. He believes that there is a great future for hypnotism in the field of therapeutics, and describes in detail some of his own remarkable experiences and successful treatment of hypnotized patients.

## SOCIETIES AND ACADEMIES.

### PARIS.

**Academy of Sciences**, September 12.—M. Hervé Mangon in the chair.—Experimental researches on the morphology of the muscles, by M. Marey. By comparing the form of the gastrocnemian muscles in the white race with those of the Negro, the author has discovered a fresh example of the harmony that exists between the form and functions of the muscles. His conclusions were confirmed by experiments made on rabbits at the Physiological Station, and a fresh proof is thus afforded of the evolutionist doctrine that the organs tend to adapt themselves to the varying conditions under which their functions are performed. To complete these researches nothing now remains except to bring about variations in the muscular form by changing the outer conditions of locomotion without modifying the anatomical relations of the organs by the direct intervention of surgery, and then ascertain to what extent the modifications thus obtained become fixed by heredity.—Invasions, varying aspects, and intensity of the pestilence in the Caucasus, Persia, Russia, and Turkey, since 1835, by M. J. D. Tholozan. From a careful study of all the circumstances attending the various visitations of the plague in this region since the great epidemic of 1830–35, the author concludes that in the great majority of cases the outbreaks have been of a purely local character, appearing in one or two houses, spreading thence by secondary contagion to others in the village, occasionally also to one or two neighbouring villages, but scarcely ever advancing beyond the district and never sweeping over extensive regions, like the cholera and pest in former times. A remarkable instance was that of Resht in Northern Persia, where it carried off 2000 of the 24,000 inhabitants, lasting altogether over a twelvemonth, during which period the people emigrated freely to the neighbouring towns, which nevertheless remained unaffected despite the absence of prophylactic measures and quarantine regulations. He therefore considers that, without denying the possibility of future widespread diffusions like those of the past, the contagion has now entered a new phase of purely local or isolated development, without any tendency to spread further. The special conditions of its appearance in such places should therefore be studied, just as those, for instance, of typhoid fever are sought and found in the districts where this disorder happens to make its appearance. In Turkey the plague has from time to time acquired a certain intensity, but without ever assuming the deadly character of certain previous outbreaks, except in Mesopotamia in 1873. But in Persia it has often been attended by an excessive mortality, and a very great local development relatively to the actual number of the inhabitants. Its range has been mainly confined to an area stretching for 1700 kilometres from Merv to Bagdad, and for 1760 from Bassora to Astrakan, but within these limits mainly confined to isolated points and never radiating from them to any great distance.—Observations of Olbers' comet (1815 I.) on its return in 1887, made at the Observatory of Bordeaux with the 0·38 m. equatorial by MM. G. Rayet and Courty. The observations cover the time from September 8–10 inclusive, and comprise the mean position of three stars taken as points of comparison.—Observations of Brooks's new comet (August 24, 1887) made at the Observatory of Nice with the 0·38 m. Gautier equatorial, by M. Charlois. The apparent positions are given for the period from August 25 to September 2 inclusive. On the former date the comet had a nucleus of the tenth magnitude surrounded by an elongated nebulosity at the angle of position of 304°.—On the variations of the telluric currents, by M. J. J. Landerer. During the last nine years, the number of days when the current flowed north-east and south-west being indicated by 1, those on which it flowed in the opposite direction will be represented by 6·7. Several changes of direction very seldom occurred on the same day, and they were nearly always connected with violent atmospheric disturbances. From 8 a.m. to 9 p.m. the intensity of the current going north-eastwards attained a maximum towards 10 o'clock and two minima about 4 and 9 o'clock, the mean intensity of the maximum being 0·000124 ampere, that of the minima 0·000073 and 0·000074. For the opposite current this maximum and these minima become respectively one minimum and two maxima at about the same hours, with mean intensities 0·00064, 0·000122 and 0·000138 ampere.—Formation and elimination of ferruginous pigment in poisoning by toluylendiamine, by MM. Engel and Kiener. Having in a previous communication studied the ferruginous residuums of hæmoglobin, which accumulate in certain organs of animals poisoned with the sulphuret of carbon, the authors here submit the results of similar researches in the case of another substance, toluylendiamine.—Experimental researches in connexion with the physiological action of *Cytisus laburnum*, by MM. J. L. Prevost and Paul Binet. The experiments here described were made on frogs and on warm-blooded animals, such as cats, dogs, rats, guinea-pigs, rabbits, and pigeons, with the general results that Cytisus must be regarded as a good emetic with central action, acting rapidly and better by hypodermic injection than by ingestion.—Note on *Greeneria juliginea*, by MM. L. Scribner and Pierre Viala. This is a new species of microscopic fungus which has lately made its appearance in North Carolina, where in very hot and moist districts it attacks and destroys in a few days vines that had been spared by the black rot. Its true characters not being yet determined, the fungus must be provisionally included in the numerous class grouped by M. Saccardo under the general name of Sphæropsideæ.

## BOOKS, PAMPHLETS, and SERIALS RECEIVED.

A Text-Book of Algebra: W. S. Aldis (Clarendon Press).—Longman's Shilling Geography (Longmans).—Die Bildung des Natronsalpeters: Dr. C. Ochsenius (Stuttgart).—Die Crustaceen der Böhmischen Kreideformation: Dr. Ant. Fritsch und Jas. Kafka (Prag).—Fauna der Gaskohle und der Kalksteine der Permformation Böhmens, Band ii. Heft 1: Dr. Ant. Fritsch (Prag).—Astronomical Revelations (E. Dexter).—Manual of Mineralogy and Petrography, 4th edition: J. D. Dana (Trübner).—Botanische Jahrbücher für Systematik, Pflanzengeschichte und Pflanzengeographie, Neunter Band, 1 Heft: A. Engler (Engelmann, Leipzig).

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