

TURKISH TIME-PIECES.

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MANY years ago I ventured the opinion that the development of the mechanical clock was hindered for many centuries by the general use of the Roman system of hours. I am more than ever convinced of this. It is perfectly well known that prior to the Christian era trains of gearing and other mechanical expedients were in use whereby the hand of a clock could be made to travel with uniform motion on a dial. There was, to be sure, no true mechanical escapement, but Ctesibius had devised what I venture to call a water escapement, which, under certain restricted conditions, performed the true function of that element of the modern clock. But the ingenuity of the times was not adequate to the production of the varying movement necessary to keep time in a system in which the length of the hours was constantly changing; and so the clock waited many centuries until the system of hours was changed.

This subject has been brought quite forcibly to my mind by coming into the possession of a number of German and Swiss patents for clocks designed to keep Turkish time. It appears from the specifications that the Turkish system of hours is practically identical with that of ancient Rome, the day commencing and ending with sunrise, and the middle being at sunset, the two periods of day and night being divided into six hours each, which constantly vary in length with the change of season.

It is obviously impracticable to make up a railroad time-table on such a system, or to accommodate it to numerous other requirements of modern social life; and therefore the wonder is that anybody should think it worth while to construct a clock adapted to this system; but, as the patentees are in all cases residents of Constantinople, it may be inferred that, in devising these clocks, they are endeavoring to minister to a felt want of that capital.

The device employed is of the same character in all the patents, though in some automatic, in others requiring frequent attention. It consists in so adjusting the governing member (pendulum or balance-wheel) as to give it a faster or slower rate from month to month; that is to say, in the winter months, when the period from sunrise to sunset is short, to quicken the action of the movement so that the hand shall pass in proportionately less time over that portion of the dial which represents the hours of daylight than it does in summer, when the days are long. It is obvious at once that this does not accomplish the purpose sought for, and the inference is natural that in the German and Swiss Patent Offices the question of utility cannot have been raised on these applications. If the pendulum is adjusted to a slow beat in the month of June, when the hours from sunrise to sunset are long, it might measure time during the day, but that same slow beat will destroy its capability of measuring off the short hours of the night. A parallel statement is true for the month of December. For this reason these inventions are useless, though they may serve the purpose of the patentees by imposing on the credulous Moslem.

It does not seem impossible in the present state of the arts to construct a time-piece capable of marking off this kind of hours with reasonable precision. The exactness of an astronomical clock or even of an ordinary kitchen clock would be unnecessary. But the inventions above referred to do not approach a solution of the problem, the key to which is to be found in a clock presented to this Government by that of Japan at the close of the Centennial Exposition. In this the hand moves around the dial at a uniform rate throughout the year, the adjustment for different seasons being accomplished by shifting the figures on the dial. It is many years since I have seen this clock, but, as I recollect it, the top of the dial represents sunrise and the bottom sunset, the half-circumference on each side being divided into five hours by a set of figures which can be shifted in place as the seasons change so as to make the day hours long and the night hours short, and *vice versa*, the sunset hour being shifted also.

I see no great difficulty in producing this shifting of the sunset hour automatically to the right or left as the season may require, nor does it appear to me insurmountable to connect the intermediate hours with the sunset hour so that they shall be shifted proportionately with it. With such a contrivance an hour-hand

moving at an equal rate over the dial would point to the true hour by Turkish time at all seasons of the year, day and night. In fact, the problem seems to me so easy of solution that I can only explain the non-appearance of such clocks in the market by the supposition that no actual demand exists for them.

NOTES UPON THE ACTION OF DRUGS AND AGENCIES UPON THE RESPIRATORY MOVEMENTS.

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THE results of a research which I have recently completed in the laboratories of the University of Pennsylvania, although bearing very directly upon practical medicine, have, I think, sufficient scientific interest to be noted in the columns of *Science*.

Hitherto, the study of the action of agencies and drugs upon respiration has been made chiefly, if not solely, by noticing their effects upon the rate of respiratory movements. It is evident, however, that increased activity of rate does not necessarily imply increased activity of function, since the respirations, though more frequently repeated, may be so shallow as to have little effect. Aided by Dr. David Černa, now of the University of Texas, I have measured the amount of air taken in and out of the lungs of the dog under different conditions.

Emotional or nervous excitement was found to be a most potent agency; the dog seemingly expressing his feelings in his respiration as completely as a human being expresses his in his face; so that during excitement more than twice as much air is moved as during quiet. It has long been known that the dog, having practically no sweat-glands, cools himself through the respiration; and so it was found that heating the animal, by such arrangement of apparatus as not to cause pain, nor to bring hot air in contact with the lungs, nearly doubled the respiratory movement of air. Heat, therefore, is to the dog a powerful respiratory stimulant; when in excess, however, it depresses function, as it was found that if the heating were continued the air movement became almost null. The rapid respiration seen in human beings suffering from fever, indicates that they are affected by heat similarly to the dog.

Chloral was found to be a more positive, persistent, and certain respiratory depressant than the morphine salts; it always reduced the air movement, and the reduction, with repeated and increasing doses of chloral, was progressive, until finally respiration was completely arrested.

The actions of atropine, cocaine, and strychnine were studied both in the normal and in the chloralized dog. Each of these alkaloids was found to be a powerful respiratory stimulant, increasing most markedly the air movement. The rather unexpected result was reached that cocaine is probably the most powerful of the three, but that strychnine is the most persistent and certain in its action. Thus, whilst cocaine seemed to be almost powerless against overwhelming doses of chloral, the influence of strychnine never failed to be manifested.

The bearing of this research upon practical medicine is very evident. During the experimental preparation for my address before the Berlin Medical Congress in 1890, I discovered the great power of strychnine over the respiratory centres when almost completely paralyzed by chloroform or ether; a discovery which led to the universal practical use of strychnine in the treatment of the accidents of anæsthesia. Atropine has long been used in narcotic poisoning, but its value as a respiratory stimulant within the last year or two has been very seriously challenged. Our research, however, re-demonstrated its power as a respiratory stimulant. Cocaine has been used to some extent as a respiratory stimulant, but it seems to be much more efficacious than is generally thought. It was found in our research that in the deeply chloralized dog, after respiration had been brought up as far as possible by one respiratory stimulant, the second stimulant was able to still further increase the extent and power of the respiratory movements. I have apparently saved human life in respiratory failure, by adding cocaine to the strychnine which was being given in as large dose as was thought justifiable. Cocaine

and strychnine, however, have so much similarity of action upon the spinal cord that the use of one of them would probably somewhat increase any danger that may have been incurred by the administration of large doses of the other.

On the other hand, atropine has little influence upon the spinal cord, its general physiological action being quite distinct from that of cocaine or strychnine. It is therefore probable that by the consentaneous use of atropine and strychnine, or of atropine and cocaine, the physician may obtain the advantage of what, many years ago, I spoke of as the "crossed action" of drugs; the two drugs touching and reinforcing one another in their influence upon the respiratory functions, and spreading wide apart from each other in their unwished for and deleterious effects.

In conclusion, for the sake of any one who may be interested in the details of this research, it may be stated that it will shortly be published in full in the *English Journal of Physiology*.

LETTERS TO THE EDITOR.

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Man and the Glacial Period.

A MISLEADING paragraph in Dr. Brinton's otherwise excellent review of a recent publication under the above caption,¹ in connection with the Reverend Professor Wright's response,² seems to demand a further word. Dr. Brinton errs in saying "As a glacialist, the author of this volume stands among the first in the country, and his long study of that remarkable period in the geologic history of our planet invests everything he says about it with uncommon authority."

Within recent years there has grown up a new branch of geologic science, which has been called by its devotees "geomorphic geology," "geomorphology," and still more acceptably "geomorphy," and which is frequently spoken of as the "New Geology." It is the function of geomorphy to read geologic history from earth-forms, as the older geology read history from deposits and their fossils. Beginning a score of years ago with Powell's conception of the "base-level," at which erosion ceases, the primary idea has extended and expanded until now the geologist not only recognizes ancient base-levels in certain topographic forms, but is able to determine from steepness of slopes and other topographic relations the rate at which erosion has proceeded in the past, and thereby the attitude and altitude of the land during earlier ages. This branch of science has been successfully pursued by a number of geologists in this country and a few abroad, and is taught in three or four universities; and it has been found of especial use in the study of glacial deposits. It is, however, a sealed book to Professor Wright; not a syllable in his latest work, or in any other of his many publications, or in his public utterances before scientific societies, suggests that he is aware of the existence of the New Geology.

Within two decades the discriminating genius of Chamberlin and a score of fellow-workers in this country has thrown much light on the events and episodes of the glacial period. Largely through the application of geomorphy, it has been shown that the glacial deposits of north-eastern America represent two, three, or more distinct ice invasions occurring at different epochs in a long period, and that the earliest of these deposits is many times older than the latest — indeed the leading authorities agree that if the post-glacial period be represented by unity, then the entire glacial period must be represented by two figures. This succession of ice deposits and ice invasions is not, indeed, recognized by some of those glacialists whose observations have been confined to regions in which only a single deposit is represented; but with one or two exceptions (including our author's namesake, A. A. Wright, professor of geology at Oberlin) every geologist who has studied the

marginal drift holds to the bipartite or tripartite or multipartite character of glacial deposits and glacial history. This succession is not admitted by the Reverend Professor Wright. Accordingly, his ideas concerning early man have no definite time-basis and cannot be discussed intelligently by modern archaeologists — it would be as easy to discuss the opinions of an author who confounded not only all the successive dynasties recorded in the monuments and hieroglyphs of Egypt but also the works of the modern fellahin, or of a genealogist who argued that the families of a dozen successive generations dined together at the same board. As an exposition of the antiquity of man and the glacial theory, "Man and the Glacial Period" is a cry from the tombs of a dead past; it represents the primitive knowledge of a quarter-century ago, and might then have been considered authoritative; but its publication to-day is an offense to science.

Professor Wright objects to Dr. Brinton's "flippant treatment" of the Nampa figurine, and insists that a geologist who happened to detect the fraud on the ground should burden scientific literature with some detailed statement. It does not seem to occur to him that the gentleman in question avoided rushing into print simply because the fraud was too transparent to deceive geologists, who alone are competent to deal with questions concerning the geologic antiquity of man. Respectable and cultured gentlemen seem indeed to have been deceived by this alleged "find," — but they were not geologists; so, too, respectable and cultured people, including an illustrious naturalist, have been deluded by a Philadelphia adventurer with an alleged motor, — but no physicist was deceived; in like manner, intelligent and honest people have been deluded by a brazen pretender into the belief that the heavens may be frightened into tears by cannon-ading — but the meteorologists are not deluded; and the circle-squares and perpetual-motion inventors are abroad in the land, yet the mathematicians and the mechanicians are not deceived. And it would be folly for the physicist, the meteorologist, the mathematician, and the mechanician to rush into print and advertise each new fraud, for thereby the press would be flooded and libraries crowded, while fraud would only flourish the more for the advertising. So long as poor human nature remains as it is, the knave and the dupe we shall always have with us; and it is to be regretted that a presumably competent authority in his own specialty of theology should be willing to assume either rôle in another line of activity.

The author of the work has indeed visited many existing glaciers, and his observations would be of value to geologists if they could be accepted with confidence. A case in point is his measurement of the rate of flow in Muir glacier, in which he employed primitive methods and recorded a result so extraordinary as to challenge credulity. Subsequently, the measurement was repeated by Professor Reid by a superior method, with a widely different result which is in harmony with all other observations. Instead of acknowledging his evident blunder, or even passing over the matter in silence, Professor Wright has the assurance to "talk round" the issue (p. 47), and thereby impugns the excellent work of a later observer.

"Man and the Glacial Period" is published by a reputable house as one of an "International Scientific Series," and thereby acquires a respectability to which otherwise it could not aspire. Dr. Brinton has fairly, albeit charitably, shown its weakness from the standpoint of anthropology; other reviewers have shown that it sinks even lower when viewed from the standpoint of geology.³ In other ways, too, the title-page conveys erroneous impressions as to the profession and standing of the author. Thus, he takes unto himself the title "Assistant on the United States Geological Survey." The facts are, that he was temporarily employed by one of the collaborators of the bureau largely for the purpose of testing his competence as an observer; and that the test resulted unsatisfactorily to the bureau and was brought to an end several years ago.

In brief, the world would be wiser if the book were not written.

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¹ Science, vol. xx., 1892, p. 249.

² Op. cit., pp. 275-277.

³ E.g., Professor T. C. Chamberlin in *The Dial*, Vol. XIII., pp. 303-306, November 16, 1892.