SCIENCE

NEW YORK, MARCH 4, 1892.

THE NEED OF PSYCHOLOGICAL TRAINING.

A FEW — only a few — years ago we learned psychology from antiquated text-books that by tales of extraordinary occurrences, quotations of poetry, emphatic assertions, occasional proofs by the phrase "it is evident," and a few improperly observed facts, gave a complete exposition of the human mind in 500 or 600 pages — except in some cases where the author was kind enough to be satisfied with half that amount. To day a psychologist of that kind lectures to bare benches in the universities of Germany, and the new psychology has got such a hold in America that it is rapidly becoming a fashion, if not a fad.

Still, in this very fact there lies a great danger to the proper development of the science. There is a tendency to careless work, to rapid shuffling off of quasi-experimental researches, to a neglect of the drudgery of a scientific investigation of the fundamental problems, and to a pursuit of ghost stories, telepathy, and sensational hypnotic tales. Even where the psychologist is really a scientific man there is a tendency to rest contented with merely qualitative results where quantitative measurements could be made with the exercise of brains and patience.

In regard to the sensationalism and quackery that have assumed the garb of psychology we can do no more than every other science does in that respect, simply put the public on its guard. If, as is usually the case, the public prefers swindle to science; the matter is beyond our control. There is also little to be said against the so-called "theoretical" or "metaphysical" psychology that has blocked scientific development in the past and opposes it in the present. The "metaphysical" psychology is neither metaphysical nor psychological; the term is used merely to cover up the inability or the dislike for careful observation and experiment, it being much easier to sit at home in the study chair and spin out a work on psychology than to put on the apron, clean batteries and smoke chronograph drums in the laboratory.

What is to be called to attention here is the fact that we psychologists are not making the proper efforts toward exactitude in our experiments. In the first place it is becoming too common to consider that going through any careless series of manipulations is making an experiment. An experiment is the systematic variation of the conditions governing a phenomenon in order to observe the results of such a variation, The amount of systematic preparation required and of careful observation to be exercised depends on the stage of development in which the science finds itself. Any lack of preparation that could have been expected, or any deficiency in the necessary care, removes the pretended experiment from the realms of science to that of dilletantism. Dilletantism may be all very good as a source of amusement, but it must never be considered as science. As Wundt has remarked, "the most dangerous enemy of psychology to-day is not the metaphysical psychology of former days, but the self-sufficient amateurism that considers every aimless toying as a scientific experiment."

Aside from this amateurism there is another deficiency, perhaps of a still more important nature. In the various periodicals we meet accounts of qualitative experiments that might just as well have heen made quantitative. Of course qualitative experiments are necessary as preliminary investigations, but they are inexcusable where quantitative ones can be made. That is to say, although they are necessary as forerunners of measurements, and although at certain stages of investigation, they are of incalculable value, yet the scientist must never rest satisfied with them, but should regard them only as stepping-stones for further progress. I can find no better way of stating this than by repeating the words of Sir William Thomson: "I often say when you can measure what you are speaking about, and express it in numbers, you know something about it; but when you cannot measure it, when you cannot express it in numbers, your knowledge is of a meagre and unsatisfactory kind; it may be the beginning of knowledge, but you have scarcely, in your thoughts, advanced to the stage of science, whatever the matter may be" ("Popular Lectures and Addresses," I., 73, London, Macmillan, 1889).

The first step in an investigation is a determination of the relations of dependence between various phenomena; this, however, must give place as soon as possible to a measurement of the changes in the mutually related phenomena. This we can already do to a great extent in regard to mental processes. The degree of accuracy obtainable is in some cases scarce second to that of physical determinations, but in others it falls far below.

The future of psychology, however, lies in the possibility of increasing the accuracy of the measurements of mental phenomena. An advance in accuracy is a difficult thing; but it is of such importance that any sacrifice of time and trouble is justifiable for that purpose. To reduce the error of observations in a given problem by a tenth is a great task, and it becomes greater with each increase in accuracy. Psychology, however, is in the fortunate position of being in possession of methods more accurate than the majority of psychologists are able to apply. Wundt and his followers have gone ahead so rapidly that on the one hand their results can claim an accuracy only one degree less than that of physics, but on the other the psychologists who have never had a training in his laboratory are not quite able to keep up the pace. This, of course, does not apply to those domains of mind not yet subject to measurement. It is very true that there are still large groups of mental phenomena not yet investigated by experiment; we have not yet found a measure for hate, for enthusiasm, or for vertigo. There are still others in regard to which we stand at present just on the point of introducing experimental methods without having achieved anything of great importance; such are the subjects of pleasure, hallucination, the lower senses, etc. Yet again we find those that are fast yielding themselves up to qualitative and even quantitative analyses, e.g., volition in some of its results, the sense of equilibrium, pain (dermal pain quantitatively measured), smell (quantitative measurements by Zwaardemaker and Henry), etc. On the other hand the magnificent achievements in the domain of sight, the good ones in hearing, those in the senses of pressure and temperature, the accurate measurements of visual space, the measurements of the reaction-time, etc., have all tended to place experimental psychology on a high level and to furnish a foundation for a science of psychical measurements, or psychometry.

What is the reason, then, that we are doing second-rate work when we might do first? The trouble lies, it seems to me, in the lack of a proper training. We attempt to make experiments; but how many of us have received a practical training in the use of our apparatus? We make observations; but how many are familiar with the methods of observation and the computation of errors? We obtain tables of results; but how many know how to formulate the equation expressing those results? I know that, until I was brought face to face with the question of what to do with my figures when I had got them, it had not occurred to me to remedy my deficient training by a study of the methods of expressing results. We all of us daily use light, sound, heat, electricity, etc., in our experiments; but how many are familiar with the units and the methods of measuring these forms of energy? What a psychologist must have is a thorough course of training in psychometry, or the methods of psychical measurement.

Summing up, I would say that what we need in experimental psychology is: no quackery, little amateurism, a proper estimation of qualitative work as subordinate, a transformation of the qualitative into quantitative investigations, and, as the means of obtaining all this, a thorough laboratory training. E. W. SCRIPTURE.

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THE VESICLES OF SAVI.

IN the *Archives Italiennes de Biologie*, XVI., 1891, page 216, there is a reprint from the *Atti della R. Accad. dei Lincei*, VII., 1891, fasc. 6, of Dr. Alessandro Coggi's important notice of the development of Savi's "*appareil folliculaire nerveux*" in the torpedoes. Since Savi's announcement of his discovery of these peculiar follicles on the lower surface of the torpedo, 1841–44, an extensive series of publications has been made on the subject. The anatomy has received attention at the hands of Boll, Leydig, Kölliker, Max Schultze, Müller, and others; and the nature and functions have been variously determined. Leydig made the vesicles to be one of his three classes of organs for a sixth sense; Wagner supposed them to be electrical excitants; but the majority agreed in regarding them as tactile organs. In 1888, in my work on the "Lateral Canal System of the Selachia and Holocephala," published by the Museum of Comparative Zoology, it was proved that the vesicles belonged to the lateral system, as seen on the skates and sharks, and it was shown that they were not confined to the torpedo, but were found on such genera as Urolophus, Potamotrygon, and Disceus of the rays, where they were simple rudimentary remnants of the lateral canals. My conclusions are amply confirmed by Dr. Coggi from the embryology of torpedo, in the early stages of which he traces the ventral canals, as in embryos and adults of other Selachia. He finds various stages of canal disruption corresponding with those I had figured from the Batoids above mentioned.

Dr. Coggi's assertion that the hypothesis making the vesicles of Savi a special modification of the lateral line system was first brought forward by M'Donnell, 1864, is one to which I should take exception. It must be due to misunderstanding of M'Donnell's statements. That author enumerates five structures that "may be, or have been, confounded with different parts of the lateral line system," and he describes the last one of the five as "The bodies discovered by Savi in the torpedo (*appareil folliculaire nerveux*) — which last, however, may be related to the lateral line, as I shall afterwards attempt to show." This is sufficiently involved to make his meaning very doubtful. But to prove that M'Donnell did not advance the idea of identity of follicles and lateral lines we have only to turn to the penultimate paragraph of his article, where he classes the follicles with other tactile organs, and says that they, one and all, appear to be distinct from the system of the lateral line, which, he says, has more the appearance of a cutaneous excretive organ than of one of sensation (Trans. R. Irish Acad., XXIV., 1864, read 1862, page 161). Up to the present I have learned of no proof or assertion of identity of Savi's follicles and the lateral canal system previous to that in my work of 1888.

Respecting the utility of the follicles it may be added here that my conclusions are at variance with those of all who have heretofore discussed the matter, insomuch that I consider these organs to be practically without special function, and to represent only a transitory condition of the lateral system, intermediate between functional perfection, in the embryo, and ultimate more or less complete disappearance, during the life of the individual. As the organs are absent from particular species or from older individuals, and are rudimentary and irregular when present, this seems to me the only tenable conclusion. S. GARMAN.

Museum of Comparative Zoology,
Cambridge, Mass., Feb. 29.

BACTERIA IN DRINKING WATER.

DR. W. MIGULA (*Centralbl. f. Bakt. und Parasitenk.*, Bd. VIII., No. 12, p. 353) makes a contribution to our knowledge of this subject which is really a new departure as regards the examination of drinking water. He points out that, although considerable stress has been laid on the examination of water for pathogenic organisms, there is no reliable rule to guide the hygienist in his examinations for the ordinary saprophytic organisms and their relation to the purity of water to be used for drinking purposes. Dr. Migula washes out small flasks with bichloride of mercury; then, after rinsing them with the water to be examined, he leaves a specimen in the flask, which is plugged with sterilized cotton wadding and covered with an india-rubber cap. It is not necessary to pack the flasks in ice, as it is assumed that if any of the organisms multiply they will all do so, while if the putrefactive organisms (those that liquefy gelatine) grow more rapidly than the others, independent evidence is obtained of the impurity of the water. Cultivations are made in flat glass dishes in order to save the time required in manipulating plates and tubes during the cooling process. After examining 400 springs, wells, and streams, the author has come to the conclusion that where there are more than ten species in any sample of water, especially when these are not species ordinarily met with, the water should not be used for drinking purposes. He found that in only fifty-nine waters was this the case, but that 169 waters contained more than 1,000 organisms per cubic centimetre, sixty-six of these having over 10,000 (forty over 50,000). From these figures it will be seen that some of the sources of supply would be condemned by the old method but would be passed by the new, and some condemned by the new would be passed by the old. Migula found in all twenty-eight species, and in a series of tables he brings out the fact that the number of colonies does not by any means correspond with the number of species, though in some cases it undoubtedly does so. This is, in fact, an exceedingly variable quantity. It also comes out that putrefactive bacteria are almost invariably absent from spring water; that they are most frequently found where the number of species is great, and where the number of colonies is between 1,000 and 10,000 per cubic centimetre; that they also occur where the number of germs is below fifty per cubic centimetre, but very seldom where the number is over 10,000.

Dr. L. Schmelk, who recently (*Centralbl. f. Bakt. und Parasitenk.*, Bd. IV., No. 7, p. 195) pointed out that there is a great increase in the number of bacteria in the water supply of Christiania during the period that the upland snows are melting most actively, now (*Centralbl. f. Bakt. und Parasitenk.*, Bd., VII., No. 4, p. 102) gives further evidence collected during the last three years in proof of his theory. The numbers he finds for those years were ten or fifteen per cubic centimetre in March to 2,500 in April, 1888; 1,100 in 1889, and on March 28, 1890, 5,000; the breaking up of the winter snows having occurred this year much earlier than usual. This is the period during which the winter snows are melting, and after this is completed there is no marked increase in the number of bacteria in the lake water until the reappearance of the winter snows, some of the earlier falls of which during October, November, and December melt and disappear. In December the number of bacteria per cubic centimetre sometimes reaches 600, the highest point recorded during the year except in March. Dr. Schmelk thinks that the increase is due to the action of frost in breaking up the earth's surface, from which the contained organisms may be set free as soon as a thaw occurs and then washed away along with the surface soil, just as during great rain-storms. He also points out that the masses of ice projecting into a river may form "collecting" points for the particles suspended in the flowing water, as more bacteria are always found in the water obtained from such ice when melted than in the river water itself. He verified this by repeated experiments. He found, however, that when floating ice was melting in water, though it contained a few more organisms than water collected near the surface, it held far fewer than water taken from a considerable depth. In the Christiania water-supply he found some thirty species of bacteria, some of which occurred very seldom, some at certain periods of the year only, and a few all the year round. The amount of solids in the water varies from time to time, between 0.92 and 0.94 grammes per litre, and traces of ammonia can usually be found in water during the time that it contains most bacteria.

THE CHINOOK JARGON.

DURING my visits to the north Pacific coast I became familiar with the Chinook Jargon as spoken in various districts. The jargon is used nowadays most extensively on Puget Sound and in British Columbia, while its use on Columbia River and in the neighboring parts of Oregon and Washington is rather restricted. It has spread as far north as Chilcat and as far south as northern California. The Jargon, as spoken on Puget Sound and farther north, contains a much smaller number of words than the printed vocabularies, a great number of the Chinook words being dropped.

On Columbia River and Shoalwater Bay I found a few additional words belonging to the same dialect of the jargon which was recorded by Horatio Hale and George Gibbs. In recording these words I made use of the same phonetic spelling which has been used in the reports to the British Association for the Advancement of Science on the North-western Tribes of Canada: To accompany, *ă'ᴇc* bone of fish, *pēk'*; to call. *tᴇō'lak;* to carry on back, *tō'itc;* to dream, *mō'sum nă'nitc;* to give food, *ō'ma* (Chihalish); to give present, *k'oē'ᴇn;* grandchild, *kōi'm* (Chihalish); last, *ubō't* (= French au bout?); let us, *hau'ansē;* to make, *qē'lᴇmitl.*

Mamook has acquired an obscene meaning, and is no longer in use on the Columbia River. Muskrat, *tsini'stsinis;* fire is out, *tcqup;* to pursue, *mē'tl'ᴇn*, or *tᴇ'k·s'ᴇn:* to put aside, up, *t'ō'ᴇn;* to rest, *alē'm;* to roast, *p'ᴇ'nis;* robin, *pil k'oatē'n* (= red-belly); to sew, *kyē'pot;* soup, *bō'yō* (French); to stop, *k'a* (Chinook); tail, *tēl* (English); to vomit, *ō'ᴇ.*

One expression which is not found in the published vocabularies, and which is unknown on Columbia River, was obtained on the Siletz Reservation, Oregon: at that time, *kōpa k·oă'ᴇt.* In a few cases the meaning of the words differed somewhat from that given in the vocabularies: to sew, *mamook tipshin* (Hale, "The Oregon Trade Language," p. 60); it means, on Shoalwater Bay and in Clatsop, to mend. To lose the way, *tseepie wayhut* (Hale, p. 60), is not used on Shoalwater Bay, *tseepie* meaning only, to miss an aim. To vomit, *wagh* (Hale, p. 52), not in use in the same region. To tear, *kluh* (Hale, p. 45), means also, to fall.

A number of words which were considered as the sole and original property of the jargon prove to be of Chinook origin: *anah*, exclamation of pain or displeasure; *heehee*, to laugh; *humm*, stinking; *kwehkweh*, mallard duck; *lala*, long time; *liplip*, to boil; *na*, interrogative particle; *nah*, interjection: ho! look here!; *poh*, a puff of breath; *toto*, to shake.

I believe almost all onomatopoëtic words of the jargon are derived from the Chinook. The word *kwaddis*, whale, which is given as a jargon word, is of Tillamook origin. A few other words, the origin of which could not be traced, belong to the lower Chinook: *ekkeh*, brother-in-law; *kelapi*, to turn; *tukwilla*, nuts. Two words, which have been derived from English, are more probably of Chinook origin: *till*, tired (*tᴇl* in Chinook); *spose*, if, which is generally derived from "suppose," but is more frequently pronounced *pōs* on Columbia River. *Pōs* means in Chinook, if; so that *spose* may be explained as due to folk-etymology on the part of the traders, or *pōs* as folk-etymology on the part of the Chinook.

It is of interest to note that two Nootka words which are found in the jargon have very close analoga in Chinook: *chuck*, water (*tltcuk* in Chinook); *wawa*, to speak (*awă'wa* in Chinook). A number of Chinook terms which have been embodied in the jargon have become extinct in Chinook proper. This is due to the fact that they have been dropped after the death of persons whose names resembled these words: *tmē'maluct* (jargon, *mimaloose*) is now *tᴇmēuwa'lᴇma; it'amă'noac* (jargon, *tamahnowus*) is now *iō'tlema.*

FRANZ BOAS.

Worcester, Mass., February.

NOTES AND NEWS.

EXPERIMENTAL psychology can count four new laboratories among its acquisitions during the present academic year, those that have been or are about to be established at Heidelberg (Germany), Geneva (Switzerland), Cornell (New York), and the Catholic University (Washington).

— The Oriental Club of Philadelphia was organized in 1888 with Professor Herman V. Hilfrecht as president, Professor M. W. Easton, treasurer, and Stuart Culin, secretary. It has held regular monthly meetings since that time, at which formal papers were read and discussed. The membership of the club is limited to thirty, and now numbers twenty-five, including Professor Paul Haupt and Dr. Cyrus Adler of Johns Hopkins University, Professors Barton, Hopkins, and Collitz of Bryn Mawr College; Professors Jastrow, Easton, Hilfrecht, Brinton, and Peters of the University of Pennsylvania, the Rev. Dr. Morris Jastrow, and others, it being strictly confined to oriental scholars.

— At the February meeting of the Oriental Club of Philadelphia, Mrs. Cornelius Stevenson read a paper on "Two Ancient Forms of Religious Symbolism, the Stone Axe and the Flying Sun-Disc." "The stone axe," the speaker said, "is the weapon of the power above. It is the bolt flung from heaven in the lightning, and which was thought to contain a spark of the heavenly fire. In pre-historic archæology, the wielder of the bolt is generally represented under the shape of a bird, which, according to the development of the people, is either the embodiment or the messenger of the ruling spirit of heaven. In Egypt, with the development of sun-worship, the Halvk of Horus, the embodiment of the upper space, in the course of time was represented as entering the sun, which is spoken of as the 'body' in which the divine spirit dwells, and which, in the form of the Horus of Edfu, as the flying sun-disc, becomes the 'heavenly Striker.'"

— The Museum of Archæology and Palæontology of the University of Pennsylvania has been reorganized as a department of the university by the trustees, under the direction of a Board of Managers, of whom thirty are appointed by the University Archæological Association, and six by the trustees of the university. This action has been found necessary through the rapid extension of the collections and increased interest in the work. The museum is divided into four sections, American, Babylonian, Egyptian, and Oriental, each in charge of a special curator. The University Archæological Association, by whose efforts the collections were brought together, defrays all expenses. It now numbers about three hundred contributing members. Mr. Charlemagne Lowe is president of the department, and Dr. William Pepper, the provost, is president of the association.

— In his Shattuck lecture Dr. Cowles sums up the symptoms and the treatment of neurasthenia as follows: the central fundamental fact is nervous weakness, manifested primarily in two ways: (1) by an exactly parallel weakness of mental inhibitory control through voluntary attention, and (2) by the central motive element of a lowered emotional tone, from a sense of ill-being. The first of these indications may be concealed, even from the patient himself, by intensified interest and increase of effort; the second he feels and soon betrays. The complex auxiliary conditions of changes in the sensations, irritability and hyperæsthesia, languor and anæsthesia, and their causes, are manifested a little later than the primary mental effects. The point of attack in the treatment is the central emotional tone. There are two ways of approach to it: (1) through the body, restoring its strength and well-being, mental comfort and control follow; (2) through attracted attention and suggested ideas we reach the emotional tone, — healthful feeling and interest attend upon wholesome ideas.

— The Museum of Archæology of the University of Pennsylvania has just received from the Egypt Exploration Fund a colossal statue of Rameses II, which has been set up in the hall of the Library Building. The statue, which is eight feet in height, was found among the ruins of the Great Temple at Har-shefi (Hanls), the herakleopolis of the Greeks, during the excavations undertaken by the Egypt Exploration Fund, under the supervision of Mr. E. Naville in the winter of 1891. Hanls was the seat of government during the ninth and tenth dynasties of Mantheo (fourth millennium B.C.), as shown by the corroborative evidence of inscriptions found in contemporary tombs at Siût. Unfortunately, no remains of the older buildings were found, and the earliest dated fragments uncovered date from the twelfth dynasty, and even these were few. The temple was rebuilt by Rameses II., and this monument formed part of this later edifice (Ca. B.C. 1330). According to the curator, Mrs. Cornelius Stevenson, the hieroglyphs cut in the back and sides of the royal seat give the king's name and titles: the crowned "Horus," the "Mighty Bull," "Beloved of Amon," or "Maat," or "Ptah," or "Ra," or "Knum;" "Son of Rā," "Ramessu Meri Amon," "Chosen by Rā," "Lord of the two Sands," "Lord of Diadems," "Giving Life like Rā," etc.

— The committee appointed last September by the American Association for the Advancement of Science to raise the sum of five hundred dollars for the continuance, during the year 1892, of the American table at the Naples station, take pleasure in announcing to the American scientists that through the liberality of the American Association, the University of Indiana, the Association of American Naturalists, Professor C. O Whitman of Clark University, and Major Alexander Henry Davis of Syracuse, N.Y., the necessary sum of money has been subscribed, and the table is now at the disposal of the American biologists. Applications for the privilege of working at the station should be addressed to the committee, care of C. W. Stiles, Ph.D., Bureau of Animal Industry, United States Department of Agriculture, Washington, D.C.; or, should any American biologist in Europe not have time to communicate with the committee, application may be made to Geheimrath A. Dohrn, director of the zoological station, Naples, Italy. Scientific journals throughout the United States please copy.

— *The American Journal of Psychology* is about to make a slight change in its editorship; beginning with the next number E. W. Scripture, Ph.D. (Leipzig) is to be associated with President Hall.

— The experiment station of Cornell University has conducted three experiments carried through as many seasons, for the purpose of determining whether it is profitable to feed grain to cows when on good pasture. The first two experiments were made at the station, on lots of three cows each, the cows being in good condition and running on good pasture. As some objection was raised against this test on the ground that the pastures used were too rich and the cows too well fed to show the best results from grain feeding in the summer time, the experiment of 1891 was transferred to a herd of sixteen Jerseys and Jersey grades, belonging to Messrs. C. M. and W. L. Bean of McGrawville, N.Y. This herd had been accustomed to only a moderate grain ration in winter and never had any grain in summer. This herd was divided into two lots of eight cows each, the division being made by the station on the basis of weight, length of time in milk, length of time in calf, yield of milk per day and per cent of fat in milk, and was indorsed by the owners of the herd in the opinion that "the cows were as evenly divided as it was possible for them to be." The experiment continued from May 23 to Oct. 23, or twenty-two weeks. One lot of cows received each day four quarts of a mixture of two parts corn meal, one part bran, and one part cotton-seed meal by weight, fed in two feeds, night and morning, when the cows were brought in to be milked. The general results of the three years' experiments are summarized as follows: In 1889, in a season in which the pasturage was very luxuriant throughout the whole summer, with three cows in each lot, the grain-fed lot gave considerably less milk, which was so much richer in butter fat, that the total butter production was practically the same in the two lots. In this experiment the grain feeding was commenced about a month after the cows had gone to pasture. In 1890, in a season in which the pasturage was luxuriant, except for a short time in the middle of the summer, with three cows in each lot, the total amount of butter-fat produced was almost exactly the same in both lots. In this experiment the grain-fed lot continued to receive the same ration on pasture that they have been receiving during the winter on dry feed. In 1890, in an experiment on soiling with grass alone, with grass and grain, just about enough more butter was produced by the grain feed to pay for the increased cost of the grain ration. In 1891, in a season in which at no time the pasture was very luxuriant, with eight cows in each lot, the grain-fed lot produced just enough more milk and butter to pay for the increased cost of the grain ration. In this experiment the grain feeding was begun about two weeks after the cows went to pasture.

— The first lecture, on the religions of Egypt, in the University of Pennsylvania Lecture Association's course on "Ancient Religions," was delivered by Mrs. Cornelius Stevenson, at Association Hall, on the afternoon of Feb. 25. The title of this introductory lecture was "Primitive Egypt and its Relation to the Stone Age." It was prefaced with a general geographical description of the country, special notice being taken of the changes it has undergone since the opening of the historical period. The lecturer dwelt at length on the various theories concerning Egyptian origins, and on the originality of Egyptian culture, whose earliest seat was in Upper Egypt. Among the interesting survivals from prehistoric times are the stone implements, from which can be derived a notion of primeval ideas and customs. The first traces of religious awakening are betrayed in the cave-burial and the care of the departed. The problem of his present life and its mysterious cessation with death first made man think of spiritual things, and, from the sense of immortality which he felt in himself, led him to conclude upon a certain immortality of the soul, or survival of the spirit. Hence the various food-offerings to the dead, because the spirit was supposed to revisit the body as long as it was not decayed, and the tomb was looked upon as the habitation of the dead. Similar ideas are found among the oldest vestiges of man in western Europe, in the caves of the neolithic period.

— Dr. A. Woeikof of St. Petersburg, who is engaged on an investigation into the cause of the famine in Russia, says *Nature*, writes that it is chiefly due to drought from August to October, 1890, which injured the winter crops; to partial and insufficient snow, which melted early in the spring, and was followed by frost in April; and lastly to droughts and hot winds from May to July, 1891. In the southern portion of the Government of Samara the prospects up to June 10 were excellent, but the harvest was destroyed by two days of hot winds, on June 14 and 15. And in the southern central provinces also, where the winter crops had greatly suffered, a moderate harvest was hoped for after the middle of July, but four hot days, from July 13 to 16, quite destroyed the crops.

— The number of persons who approve of cremation seems to be steadily increasing, according to *Nature*. From the report of the Cremation Society of England for 1891, we learn that in 1885, the first year the crematorium at Woking was used, only 3 bodies were sent there; in 1886 the number was 10; in 1887, 13; in 1888, 28; in 1889, 46; in 1890, 54; and during the past year, 99. Crematoria are being built in various parts of the country. At Manchester a crematorium is in course of erection, and will, it is thought, be completed and opened for use during the coming spring. A company has also been formed, and is making rapid progress, with the same object at Liverpool; and the City of London Commission of Sewers is taking steps to obtain powers to erect a crematorium at their cemetery at Ilford. The Cremation Society at Darlington, and other associations, are moving in the same direction.

— The Journal of the Scottish Meteorological Society (third series, No. 8) contains a very interesting paper on silver thaw at Ben Nevis Observatory, by R. C. Mossman. The phenomenon is somewhat common at that observatory, and occurs during an inversion of the ordinary temperature conditions, the temperature being considerably lower at the surface than at higher altitudes, causing the rain to congeal as it falls. In the six years 1885-90, 198 cases of silver thaw were observed, with a mean duration of $4\frac{1}{2}$ hours in each case, and they nearly all occurred between November and March, during times of perfectly developed cyclones and anticyclones. An examination of the weather charts of the Meteorological Office showed that for the 198 days on which the phenomenon was observed the distribution of pressure was cyclonic on 137 days, and anticyclonic on 61 days. In anticyclonic conditions there was a cyclonic area central off the north-west coast of Norway, while the centre of the anticyclone was over the south of the British Isles. In cyclonic cases, an anticyclone lay to the south, over the Iberian Peninsula. The lowest temperature at which the phenomenon took place was 18°, and was rarely below 27°. Fully 90 per cent of the cases occurred when the thermometer was between 28° and 31.9°, so that the greater number of cases occurred just before a thaw. The most common type of cloud which preceded both cyclonic and anticyclonic cases of silver thaw was cirro-cumulus, frequently accompanied by cirrus and cirro stratus; and the changes showed that the higher strata of the atmosphere came first under the influence of the moist current, which took from three to eight hours to descend to the height at which cumulo-stratus forms. An examination of a series of storm charts prepared by Dr. Buchan disclosed the somewhat remarkable fact that 73 per cent of the cyclonic and 63 per cent of the anticyclonic cases of silver thaw on Ben Nevis were followed or preceded by gales on our northern and north-western coasts; and it would appear from the wind conditions that the barometric gradient at the height of Ben Nevis (4,407 feet) must be totally different from what obtained at sea-level during the occurrence of silver thaw on the hill-top, says *Nature*.

— There has been much talk in Germany about Dr. Peters's discovery of saltpetre in the Kilima Njaro district. This discovery accords with statements which were already well known. Dr. Fischer, after an examination of the Donjongai volcano, reported that in the neighborhood of the crater there were a series of curiously-shaped veins of a white substance which he took to be either saltpetre or soda. In 1879 Herr Jarler asserted that large quantities of sulphur would probably be found in the crater. The Berlin correspondent of the *Times*, by whom these facts are noted, adds that not far from the volcano there lie great swamps from which soda is obtained. It is expected that an expedition for the exploration of the district will soon be sent out by the German East Africa Company.

— It is well known that yellow-fever never develops in a cold or temperate climate, and several attempts have been made at various times to apply this fact to the treatment of the disease in tropical climates by artificially cooling the patient. Thus some thirty-five years ago trials were made with a cold chamber, the air of which was charged with oxygen, but without appreciable success. Quite recently Dr. Garcia has reintroduced a somewhat similar plan, an iced chamber being constructed so that the air within should be maintained at a temperature varying from 32° to 50° F., and nearly saturated with moisture. A fair trial was made with this at the works of the Juragua Iron Company in Cuba, where an epidemic of yellow-fever had broken out, seventeen well-marked cases, in all of which black vomit was present, being treated by means of the "polar chamber." Eleven of them recovered, the mortality consequently being at the rate of 35.3 per cent, or about the same as the usual rate of mortality at the mines under other methods of treatment. The course of duration of the disease did not appear to be in any way modified by the low temperature; the urine, though in some cases considerably increased, was not altered qualitatively. The phenomena depending on acholia occurred in the same manner and at the same period as in cases treated in the ordinary way. The same may be said of the gastric hæmorrhage. The cost of a patient's treatment by cold was found to amount to about $100, says *Lancet*.

— The sixteenth annual commencement of Meharry Medical Department of Central Tennessee College was held at Nashville, Tenn., Feb. 18. Twenty-five young men received the degree of M.D., one that of D.D.S , and three were awarded diplomas for having completed the course in pharmacy. G. W. Miller of South Carolina delivered the salutatory address, on "Practical Bacteriology." He gave an account of the different kinds of bacteria, how they could be cultivated, stained, and examined, and how one variety could be distinguished from another. The pharmaceutical class was represented by Robert Tyler of Mississippi, who gave an address on "The Relations between Physicians and Pharmacists." The valedictory address was given by J. W. Holmes of Texas, his subject being "The Advance of Modern Surgery." The speaker referred to the early history of surgery, especially that practised by the Egyptians and Grecians. He spoke of the reforms in surgery and the leaders in these reforms, the principal operations of importance from the sixteenth to the present century inclusive, and of some of the appliances which had accomplished much for surgery, such as an anæsthetics and antiseptics. He also gave elaborate descriptions of cranial and abdominal surgery, mentioning some of the most hazardous operations performed in these cavities, and also paid a high tribute to the modern surgeon for the achievements accomplished by him. The past year has been the most successful and encouraging ever known in the history of this school, the number of students and graduates being about fifty per cent greater than that of any previous session, one hundred and eighteen medical, and seven dental and nine pharmaceutical students being enrolled. The record of the alumni of Meharry Medical College has been most gratifying. Of those who have graduated within the past six years only two have failed to pass the required examination before the "Boards of Medical Examiners," standing equal with the white applicants from the different medical colleges of the South, with whom they were examined at the same time, and have been well received by the white physicians.

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Attention is called to the "Wants" column. All are invited to use it in soliciting information or seeking new positions. The name and address of applicants should be given in full, so that answers will go direct to them. The "Exchange" column is likewise open.

MOTION AND HEAT.

THE term "Mechanical Equivalent of Heat" does not present a perfectly accurate concept of the determinations of Dr. Joule and others. The great work actually done was the determination of the "Heat Equivalent of Molar Motion."

"The Mechanical Equivalent of Molar Motion" is the amount of mechanical work that it will do; and when the whole energy embodied in a given molar motion is converted into heat, the units of heat thus developed may again be converted into molar motion capable of doing the same work. Hence the term "Mechanical Equivalent of Heat" is accurate enough for purposes of calculation.

But the true equation is that molar motion is equivalent to so much mechanical work; molar motion may be converted into heat capable of the same amount of mechanical work that the molar motion could do before its conversion into heat, and therefore we have the "Mechanical Equivalent of Heat." This use of the consequence, that is, the mechanical work which molar motion can do, for the motion itself, tends to obscure the concept of the real relation between heat and molar motion.

The primal work of the energy, or force, which constitutes molar motion is to transfer a mass from one place, or part of space, to another, and so long as this work is continued and unresisted, no heat is developed. A body moving through space entirely unresisted, whatever may be its mass or velocity, develops no heat. It is only when the movement is resisted by impact or friction of some kind that the energy of motion assumes the form of heat; and it is only when thus resisted that this energy of motion can do mechanical work. To the extent that the energy embodied in resisted molar motion is expended in mechanical work it cannot be converted into heat.

Mechanical work consists in counteracting some other force, generally gravitation or cohesion. The force or energy embodied in a ball thrown upwards from the earth's surface develops no heat except such as may result from the friction of the air; and if at its precise point of highest elevation it lodges on the top of a house or some other support, none of the energy is thereby converted into heat. The ball has acquired what Mr. Balfour Stewart calls energy of position; and when this potential energy again becomes dynamic by the ball's falling to the earth, no heat is developed except by the friction of the atmosphere, until the ball strikes the surface of the earth. If the phenomena occurred in vacuum neither the energy of motion in the ball, while doing the work of lifting the ball to its highest elevation, thus counteracting gravity, nor the potential energy rendered dynamic by its fall, would develop any heat whatever until its impact against the earth's surface. Here, according to the law of conservation of energy, it would do work or develop heat equivalent to that expended in its upward projection.

But to the extent that the energy of the impact itself does mechanical work, that is, counteracts cohesion in the work of molar deformation, it develops no heat. If an egg and a metal ball of the same shape, size, and weight are dropped from the same height on a hard pavement, the heat developed by the two impacts cannot be the same if the egg is smashed. If the heat developed by the impact of the metal ball is X, that developed by the impact of the egg must be X minus the kinetic energy required to smash the egg.

One of the occupations of my boyhood was to attend a mill for grinding corn, and one of the first things learned in that business was that if the moving stone was properly balanced and a sufficiency of corn supplied, the meal came out very little heated; but if the stones came into contact from lack of having corn to grind or from want of proper adjustment or levelling of the moving stone, heat was developed rapidly.

It is for this reason that hard substances like flint and steel more readily develop by friction the heat necessary for combustion than softer substances; the energy of motion in the friction of softer substances is expended to a greater or less extent in molar deformation, and it is only the residue not thus expended that is available for conversion into heat.

This principle is constantly applied in practical mechanics to develop heat from friction when it is required, and to prevent its development when not wanted. Except for igniting combustibles, heat from friction is not generally wanted for practical use; but Dr. Mayer mentions an instance in which a manufactory used a surplus of water power to revolve two large iron disks against each other to develop heat by friction to warm the establishment. The very general object in mechanical work is to prevent the conversion of the energy of motion into heat by friction, and this is done both by diminishing the frictional resistance, and also by the use of solid lubricants whose molar deformation will furnish work for the energy unavoidably lost in friction, and thus prevent the development of heat and the local injury from the energy in that form.

Hence it was that Dr. Joule and others, in making the determinations of the so-called "Mechanical Equivalent of Heat," made use of substances in which there was no work or very little work of molar deformation for the energy, the heat equivalent of which was measured.

It seems, therefore, that two propositions may be stated:—

First, that molar energy, that is, the kinetic energy of a moving mass without friction, develops no heat while doing its primal work of transferring the mass from one place, or part of space, to another.

Second, that when the movement of the mass is resisted, the heat developed is the equivalent of only so much of its energy as is not expended in molar deformation or other mechanical work.

There is obviously another cause which may prevent the kinetic energy of molar motion from development into heat, and that is its conversion into the molecular motion of expansion. When expansion occurs, there is necessarily an enlargement of the intermolecular spaces or of the molecules themselves, or a movement of the molecules; and while we have as yet no such demonstration as is possible in molar phenomena, we can assert, without fear of scientific denial, that the phenomenon of expansion is a manifestation of molecular motion.

It is usual to regard expansion as the work of heat, and it is undoubtedly the work of the same energy which is embodied in molar motion, and which causes an elevation of temperature; which passes from one body to another by conduction, and from one body to another and from bodies into space by radiation.

But this energy, while doing the work of molecular motion in expansion, develops no more heat than while it is moving an unimpeded mass through space. The impact of moving masses free to expand from the instant of impact could develop no perceptible heat; that is, only so much of the energy as was not expended in the work of molecular motion of expansion would be available for the development of heat. If the bodies brought into impact were liquid ammonia, and this was set free in the atmosphere by the impact, not only the entire energy of the impact (unless the molar motion was almost beyond calculable velocity) would be expended in the work of expansion, but energy in the form of heat would be withdrawn from surrounding bodies to finish the work.

It is resistance to the molecular motion of expansion that develops heat when expansion occurs as the primal work, that is, which causes an elevation of temperature, and converts the energy or force into the other well-known phenomena of heat.

This resistance may be from cohesion in the matter to which the energy or force is imparted, from chemical affinity, from the walls of a containing vessel or other environment, or from a piston or other compression. In every case the development of heat, that is, the elevation of temperature, and the other phenomena indicating the conversion of the force or energy into the form of heat, is determined by the intensity of the molecular motion set up by the force or energy imparted to the body, and the resistance to it.

Hence, in the experiments to determine the so-called "Mechanical Equivalent of Heat" where expansion was used, means were provided for its perfect resistance. Here again the term does not express an accurate concept of the determination actually made; it was really the "Heat Equivalent of Molecular Motion." As in the other case, the expression is accurate enough for purposes of calculation, because the mechanical equivalent of molecular motion, that is, the mechanical work it will do, is the same as the mechanical equivalent of the heat developed by its perfect resistance.

It is not the motion in either case that is converted into heat, but it is the force or energy causing the motion which ceases to move the mass or molecules and causes an elevation of temperature and the other phenomena of heat.

It seems, therefore, that we can state two other propositions, namely:

Third, that so much of molar motion as is converted into molecular motion by impact or friction cannot be directly converted into heat; and,

Fourth, that the molecular motion set up by molar impact, friction, or otherwise, and manifested by expansion, can be converted into heat only by resistance to expansion.

This force, or energy, is dynamic when causing motion or when causing elevation of temperature and the other phenomena of heat; but it becomes potential, or "energy of position," when a ball is thrown up and lodged on the roof of a house, or when radiant and dynamic heat becomes the latent heat of liquefaction and evaporation, or when the dynamic radiation from the sun is stored up in the molecular structure of the hydro-carbons of vegetable and animal organisms by chemical affinity and the vital forces; and it becomes partly potential when heat is absorbed.

This force, or energy, is directly subject to observation only when dynamic; it apparently disappears when a ball thrown up lodges on the roof of a house, or when heat becomes latent in liquefaction and evaporation, and when heat and light are stored up in the molecular structure of vegetable organisms. But we know that by appropriate means it can be rendered again dynamic, with its full integrity and with the qualities it possessed before its imprisonment, including the equivalence of its different forms. It becomes dynamic in the form in which it was rendered potential; in the ball loosed from its perch the energy becomes dynamic as molar motion; in liquids and gases subjected to pressure the latent heat of liquefaction and evaporation becomes again dynamic as heat; and in the combustion of vegetable organisms the sun's energy becomes again dynamic substantially as it was locked up.

Light is undoubtedly a division of the heat form of this force, or energy. It is rendered potential in vegetable organisms, and becomes dynamic as heat, not as light, when the combustion of the organism occurs slowly and at a low temperature. It not only results from intense heat, but Professor Tyndall has demonstrated that heat rays, after they leave the body which sends them forth, may be concentrated into light rays. It will therefore be sufficiently accurate for our present purpose to consider both heat and light as together constituting a single form of this force, or energy.

If expansion is resisted by cohesion, chemical affinity, mechanical pressure, or otherwise, the temperature of the body rises in proportion to the increments of the force, or energy, received; radiation increases with rise of temperature, and if the resistance is sufficient, incandescence and the more intense radiation in the form of light, begin.

It may be impossible from lack of power in any machine which man can construct to compel by compression of expanded matter incandescent radiation. But when heat becomes radiant as it does from compression, it is only a question of intensity whether the matter radiating heat will become red hot and radiate light also.

In the combustion of hydrocarbons it is evidently the resistance to expansion which causes heat radiation, and as this resistance becomes more intense, light radiation also. In the vegetable or animal organisms which constitute the hydrocarbons a new molecular structure has been built up, in which force, or energy, coming dynamic from the sun has been stored up and rendered as completely potential as the energy of a ball lodged on the roof of a house, or as dynamic heat when it becomes the latent heat of liquefaction or evaporation. This force, or energy, thus stored up by chemical and vital action in the new molecular structure and rendered potential, is set free and again rendered dynamic by the chemical reaction of combustion, and the material elements return substantially to the condition in which they were before.

The force, or energy, thus set free by the chemical reaction at once begins the work of dynamic energy; and if the matter in which the reaction occurs is free to expand, the energy is expended in the molecular motion evidenced by expansion.

But if expansion is resisted by cohesion, chemical affinity, or mechanical compression, there is an elevation of temperature and the other phenomena of heat.

As resistance to expansion increases, heat becomes more intense; and when heat radiation is unable to carry off the energy as rapidly as it is set free, the matter becomes incandescent, and the more intense form of light radiation begins.

The graphic description of ordinary combustion in Dr. Josiah P. Cooke's "New Chemistry" leaves no doubt that this is what actually occurs, and that "the light comes from the incandescent solid particles," because they are more persistent in resistance to the molecular motion evidenced by expansion. The moment these particles are converted into carbonic dioxide, and aqueous vapor, and thus become free to expand, the matter ceases to be incandescent.

If we could provide some means in ordinary combustion for retaining the carbonic dioxide and aqueous vapor, with the molecules concentrated as they are in the carbon particles, the matter would doubtless continue incandescent after the reaction; and undoubtedly the energy expended in the expansion of the carbon dioxide and aqueous vapor, could be converted into radiant heat by sufficient compression of those gases.

The phenomena of explosions demonstrate even more clearly than ordinary combustion that the development of heat results from resistance to molecular motion. Loose gun-cotton exploded, will not develop heat sufficient to ignite gun-powder in contact with it; but if the gun-cotton is confined, its combustion develops heat sufficient to ignite gun-powder, and substances far more refractory. It is said that the reason for this peculiar result of the explosion of loose gun-cotton, is that there is not time to develop the heat. But the true reason undoubtedly is that the molecular motion set up is so intense, as compared to the resistance of the atmosphere, that the entire force or energy of the explosion is expended in that work, and there is little or no necessity for elevation of temperature or radiation.

In firing a gun, the energy developed by the explosion is divided into three parts: that which by reason of resistance to molecular motion causes elevation of temperature and radiation in the barrel; that which imparts molar motion to the projectile (which we know may also be converted into heat); and, third, the residue of molecular motion which is dissipated in the atmosphere at the muzzle of the gun, and neither develops heat in the barrel nor adds to the molar motion of the projectile.

If the foregoing inductions are sound, the heat developed by an explosion is determined by the resistance to the molecular motion exerted by the force or energy set free and rendered dynamic by the chemical reaction. This resistance consists of cohesion and chemical affinity in the matter in which the reaction occurs, and in the environment. If the whole force or energy set free and rendered dynamic is d, and the whole resistance is r, and x the units of heat developed, then $x = \frac{d}{r}$.

This explains why the attempts made to determine the energy of explosives by the units of heat developed in their explosion have resulted in unmitigated nonsense.[1]

This has doubtless been a source of error in determining the heat evolved or absorbed in chemical processes. The energy converted into heat by resistance to molecular motion, and afterwards lost by radiation or conduction, is estimated or otherwise taken into the account, but that which slips away in the form of unresisted molecular motion is not counted.

"Although these values," says Dr. Cooke in his "Chemical Philosophy," "are undoubtedly as fundamental constants of chemistry as the atomic weights, yet they have not been as yet so fully confirmed or so thoroughly collated as to enable us to present an entirely consistent system. Hence the table here given [of heat evolved or absorbed in different chemical actions] must be regarded as provisional, and as serving only to illustrate the principles of the subject.[2]

It is not necessary to the present inductions to determine whether the molecular motion evidenced by expansion, and which, when resisted, results in elevation of temperature and other phenomena of heat, is molecular vibration as supposed in the kinetic theory, or a rectilinear projection of the molecules, as I have tried to prove.[3] All we need to know is that this molecular motion, whatever may be its character or direction, is work done, and, as in the case of molar motion, the energy embodied in it cannot be converted into heat except by resistance.

Elevation of temperature, which is the first phenomenon of heat, seems to be a preparation for the flight of radiation, the flight becoming more rapid or intense as the temperature rises; but energy will not make the preparation nor begin the flight from the matter in which it is embodied, unless its work of molar or molecular motion is resisted or hindered.

Whether heat absorbed by matter is energy rendered partially potential by the partial counteraction of cohesion, or whether it continues fully dynamic in the work of increased molecular vibration as supposed in the kinetic theory, it is not necessary for our present purpose to determine. We know certainly that heat is absorbed by matter, and the phenomena of the atmosphere demonstrate that the capacity of matter to absorb heat diminishes by some as yet undetermined ratio with increase of tenuity. This diminution of capacity to absorb heat doubtless results from the smaller number of molecules to which motion can be imparted; and taken in connection with the induction that energy becomes radiant as heat and light when molecular motion is resisted, or hindered, it furnishes a very simple explanation of the intense heat and brilliant incandescence which small increments of energy develop in highly exhausted tubes.

The work of molecular motion being restricted by the paucity of the molecules, the small increments of energy, finding no sufficient work in moving them, elevation of temperature and incandescence follow, for substantially the same reason as in other cases where greater increments of energy are applied.

It seems to make no specific difference whether the increments of energy are imparted by the direct conduction or radiation of heat, or by resistance to a current of electricity.

Mr. Crookes, by concentrating increments of energy in a highly exhausted tube on iridio-platinum alloy, one of the most refractory metallic compounds, not only raised it to a white heat, but actually melted it: while the same measurable increments of energy applied to the same substance in the atmosphere, or in some other medium not more tenuous, would have caused hardly an appreciable elevation of temperature. The energy, in such case, would be expended in

[1] The true measure of the energy of explosions must be the amount of energy set free by the chemical reaction, and this is determined by the number of molecules put in motion (quantity of matter, etc.) and their velocity.

[2] "Chemical Philosophy," revised edition (1891), p. 174.

[3] "Molecular Motion in the Radiometer," etc. N. D. C. Hodges, New York, 1891.

molecular motion in the surrounding medium. And the brilliant incandescence in Geissler, Crookes, and Tyndall tubes from minute increments of energy are well known.

This increase of temperature and radiation from small increments of energy in highly tenuous matter seems to be what we ought to expect from the phenomena of this force or energy when it is in the form of molar motion. We then measure it by the mass and velocity of the moving body; that is, by its momentum, and this momentum is what is convertible into heat when the movement is resisted.

Increase in velocity compensates for decrease in mass, and hence a small projectile, at high velocity, will do the same work as a larger projectile at lower velocity; and the momentum, in each case, can be converted into the same units of heat. For obviously the same reason, the intense velocity imparted to the gaseous products of an explosion of dynamite enables this highly tenuous matter to do precisely the same work on a hard rock, as a hammer of a million times the mass, but moving with only one-millionth of the velocity.

But there is necessarily a limit to this substitution of velocity for mass; and this limit is in the capacity of matter to embody the energy; and when the force of energy is applied to matter in the form of heat we ought to expect to find the same limit. This application in the form of heat may be made by conduction, when the whole energy imparted is absorbed; or by radiation when only so much as is not reflected, is absorbed; but the resulting phenomena are the same, whatever may be the process by which the absorption is accomplished.

The fact developed in spectrum analysis, that incandescent matter absorbs the same rays of light which it emits, seems to be another illustration of the law that the capacity of matter to receive radiant energy is limited, and in this case by its capacity to radiate the energy received.

If the evolution of heat and elevation of temperature results from resisted molecular motion, it necessarily follows, that a single molecule, moving in unconfined space, whatever may be its velocity, would be at the absolute zero of temperature. But this is mere speculation of no scientific value, because we have no evidence that a molecule can become separated from other molecules, nor that it is possible to place it where it could move without resistance.

But there is another induction of practical importance in sustaining the assumption that we have just made. If the effect of heat imparted to matter by conduction or radiation is to set up the molecular motion evidenced by expansion, and this work of molecular motion must be resisted before radiation begins, it necessarily follows that the number of molecules in the body receiving heat, and to which motion can be imparted; in other words, the density of tenuity of the matter, must be an element, determining, in some measure, the capacity of the matter to absorb heat.

This explains why the atmosphere decreases in temperature with increase of tenuity, upwards from the earth's surface; and why we can assume absolute zero in space entirely unoccupied by ponderable matter, if there is any space thus entirely unoccupied, notwithstanding the presence of potential or dynamic energy, because it is only in conjunction with ponderable matter (resisted molar or molecular motion) that dynamic energy develops elevation of temperature, and the other phenomena of heat.

It is obvious that force or energy in the form of molar motion is being constantly converted by impact or friction into the form of heat. Taking the earth as a whole, during the period of human observation, this constant conversion of molar motion into heat has been compensated by a conversion of heat into molar motion, so that the equilibrium between the two forms of this force or energy has been preserved in terrestrial nature, and there has been no loss of motion nor increase of heat, since man began to observe nature and keep a record of his observations.

Resistance to movement, that is, to the work being done by the force or energy in molar motion, is necessary to convert the force or energy into the form of heat; and it may be that when this force of energy is applied to ponderable matter in the form of heat, and its proper work as heat is resisted, the surplus heat may be converted directly into molar motion.

It is certainly within the range of possibility, that, under certain conditions, a body of ponderable matter may receive increments of heat more rapidly than it can furnish work for it in the molecular motion of expansion, or discharge it by radiation or conduction; and, in such case, it seems inevitable that the body thus receiving more heat than it could furnish work for or discharge, if free to move, would be put in motion away from the source of heat, and that this motion would continue until a distance from the source of heat was reached, at which the heat received was not greater than could be employed in expansion or discharged in radiation and conduction.

Dr. Grove was inclined to the opinion that it was thus possible to convert heat directly into molar motion. He says, "There are, indeed, some delicate experiments which tend to prove that a repulsive action between separate masses is produced by heat. Fresnel found that mobile bodies heated in an exhausted receiver repelled each other to sensible distances; and Baden Powell found that the colored rings, usually called Newton's rings, change their breadth and position, when the glasses between which they appear are heated, in a manner which showed that the glasses repelled each other." [1]

But, however that may be, there is certainly a molar motion which always follows and evidences the molecular motion of expansion. The law that action and reaction must be equal and opposite, applies to molecular motion in a closed vessel. It is the operation of this law which secures uniform pressure in steam boilers, and other like devices for using gas expansion for mechanical purposes; and thus converts the molecular motion, evidenced by expansion, into molar motion. DANIEL S. TROY.

(To be continued.)

LETTERS TO THE EDITOR.

**** Correspondents are requested to be as brief as possible. The writer's name is in all cases required as proof of good faith.*

On request in advance, one hundred copies of the number containing his communication will be furnished free to any correspondent.

The editor will be glad to publish any queries consonant with the character of the journal.

A Question in Physics.

CAN there be a crowding of the particles of a gas to a much smaller compass without its being markedly heated? Can a gas expand without being cooled? At first thought the answer would seem to be an emphatic *no* in both cases; but it would appear that these conditions may exist sometimes. *Science*, Vol. XV., p. 387, published the results obtained by direct determination of the heating of air when compressed by a pump connected by a long tube with the cylinder. A compression to ten inches above atmospheric pressure gave a heating of about 4° F., ignoring the heat lost to the sides of the cylinder. The corresponding expansion into the open air gave a cooling of about 4°, neglecting the

[1] "Correlation and Conservation of Forces," p. 41. D. Appleton & Co. 1890.

heating effect of the cylinder. These results were strongly combated by Professor Ferrel in *Science*, Vol. XVI., pp. 192 and 193, and also by Professor Marvin. Professor Ferrel published the well-known thermo-dynamic formula, given in *Science* for Feb. 19, and applying it to the heating in the above case found it 43° F. instead of the 4° found by the experiment. It would seem, however, that these experiments had not been controverted, and it is probable that their justness may yet be established. This problem is far-reaching in its application, and it is for this reason that it is dwelt upon at some length.

The formula given by Professor Ferrel applies only in cases where a gas is compressed directly by an external force, and when all the heat developed in the work of compression is concentrated in the gas. One of Joule's experiments will serve to elucidate this point. He determined the mechanical equivalent of heat by immersing the cylinder into which the air was to be compressed and the compressing pump in the same water bath, and then determining the amount of compression and the total heat developed. This shows at once the truth of the following proposition. If a gas when compressed is to be raised to the temperature indicated by theory, it is very essential that all the heat developed in the work of compression enter it. This proposition seems self-evident; nevertheless, it would seem that nearly all the errors that have entered the various discussions and theories regarding this matter have arisen from a neglect of this obvious statement.

We may analyze Joule's experiment in order to gain a clearer understanding of the problem. Suppose the compressing pump had been in a bath by itself, and the cylinder in another bath; also that no heat was lost in the passage of the air from the pump to the cylinder. Under these circumstances a good deal of the heat due to the action of the pump would have passed into its bath, and only a small portion would have been carried by the hot air into the cylinder. Let us consider that a certain definite amount of heating would have taken place if all the heat had entered the air in Joule's original experiment, the formula gives the rise as 123° F. if the initial temperature of the air had been 60°, and the compression was to two atmospheres. In the present instance, however, most of the heat would have been absorbed by the bath around the pump, and would not have been available for heating the compressed air in the cylinder. It is impossible to consider that the same amount of work would have sufficed to heat the water around the pump, and then would have developed heat enough to raise the temperature of the air in the cylinder 123°.

Again, suppose that the compressed air, before entering the cylinder, had its temperature lowered to the outside temperature; is it not plain that all the heat developed in the work of compression would be disposed of, and none at all would be available for heating the compressed air? We see, then, that it is entirely feasible to bring about certain conditions under which a gas may be greatly compressed without being heated.

Let us take two equal cylinders connected by a tube and compress the air in one, A, to three atmospheres, the air in the other, B, being at atmospheric pressure. Let the air in A be at the temperature of the outside air. On opening communication between the cylinders the air in A will be slightly chilled, owing to the work of imparting a certain velocity to those particles rushing into B; while the air in B will be heated slightly from the impact of the particles rushing out of A. All the heat due to the work of compression, however, will have disappeared, and none will be available for heating the air in B (See Enc. Brit., Vol. XXII., p. 480, section 34).

Lastly, suppose that the air in A should be allowed to escape into the open air; the resistance to the rush of the air would be much less than in the last case, and hence a greater velocity would be imparted to the particles rushing from A, and the cooling would be slightly greater than before. The situation appears very plain, and there is no difficulty now in understanding why the earlier experimental heating and cooling was only 4°.

These views seem almost startling in their nature, and if true certainly have profound significance. Let us try to picture the real condition of the gas when under compression and flowing from one reservoir to another. The confined air has a certain potential energy and a capacity for work; it may flow into any reservoir where the air is at atmospheric pressure without losing its potential energy, and hence, if none of its energy is lost, it cannot be used up in heating the air. Is it not like the water in a pond having a certain head or capacity for work? We may enlarge the pond, and allow the water to flow over a larger area; but the capacity for work will be diminished very slightly. X.

Feb. 23.

The Balloon Problem.

THE problem of the amount of work done by the gas in a balloon expanding as the balloon rises, as proposed in *Science* for Feb. 19, may be much more significant than even the proposer has thought. Take a bag perfectly flexible and holding two cubic feet. Force out all the air and tie the neck. If we attempt to separate the sides, we shall find it impossible to do so; as the air presses upon it fifteen pounds to the square inch. Allow a cubic foot of dry air to enter and again close the bag. We shall find the same difficulty as before in further opening the bag. Consider that the air in the bag has been heated 490°, which will just fill the bag. To separate the molecules has required a work equivalent to lifting 2,160 pounds one foot, and for convenience we say that the gas in expanding has lifted the weight of the atmosphere. Is it proper, however, to think of the outside air as having been lifted? Has any more outside air been lifted than the 1.2 ounces that a cubic foot weighs? The work, then, has been internal and not external. This is a very important distinction. The external work has been only that required to lift the weight of air displaced.

This can be shown best, perhaps, by determining just how much change has taken place in the behavior of the bag to outside influences. If any external work has been done, we ought to be able to measure it. If the bag with its two cubic feet of air were left to itself, it would soar aloft, and it would require a weight of just 1.2 ounces to restrain it. We say the heated air displaces two cubic feet of air at the outside temperature; and since its density is just half that of the outside air, it can lift a weight equal to that of one cubic foot of air.

Instead of heating the air, let us connect the empty bag with a reservoir having a gas which has a density just half that of the air. Here the conditions are entirely changed. The reservoir, to all intents and purposes, is connected with the outside air, and when we connect the mouth of the bag with it, there is no more work required to expand the bag than if we had opened it into the outside air. In the case before, after closing the bag, we could not open it till some internal work had been done in expanding the air; but now that internal work is not needed, and the only work done by the gas in expanding the bag is that required to lift one cubic foot of air one foot. The lifting power of the bag is precisely the same as it was when it contained air at 490°. The amount of external work in expanding the bag, or capacity to do external work, is exactly the same.

Take the same bag, empty as at first, and connect it with a reservoir containing two cubic feet of air at the outside temperature but at a pressure of two atmospheres. The air will flow quickly into the bag and an equilibrium will be established with the pressure at one atmosphere in both the reservoir and bag. How much external work has been done? Has the air in expanding lifted an enormous weight? Certainly not; the external work has been equal to that required to lift two cubic feet of air, or 2.4 ounces, one foot. Here again we have entirely different conditions from those in the first case. On connecting the bag with the reservoir we virtually opened it to the outside air, and the outside air did all the work which in the first case was needed to be done in separating the particles of air, or in increasing their kinetic energy. We can see this at once by the following considerations. Open the bag into the free air; we can pull the sides apart to their fullest extent. Now connect the opened bag with the reservoir which has the air at the outside pressure, the conditions remain exactly as before, when the mouth of the bag was open to the outside air. Empty the bag and connect it with the reservoir. No change will take place, but the reservoir will virtually be connected with the outside air. Now gently force air into the reservoir; the connection of the bag with the outside air will remain as before, and when the bag is full the only work external to the reservoir will be that of lifting 2.4 ounces one foot.

When a balloon rises into the atmosphere, then, the gas does not expand, and in so doing perform an enormous amount of external work; but it simply displaces the air. The amount of work in this case would be very small indeed, and the consequent cooling of the gas slight. The conditions are precisely similar to what they were when we connected the bag with our reservoir having the air under pressure. In rising, the balloon continually arrives at a region in which the pressure is less and the expanding gas simply displaces the surrounding air. Every cubic foot expansion in the gas of the balloon at sea level displaces a cubic foot of air at a pressure of thirty inches. If the pressure of the outside air were suddenly diminished to ten inches, the work done would be that of lifting a gas weighing one-third of the air at normal pressure, or about .4 ounces to each cubic foot. This would cause almost an inappreciable cooling in the gas.

A very interesting point may be mentioned in this connection. What became of the energy stored in the reservoir in the air compressed to two atmospheres, after the air had expanded to normal pressure in the reservoir and bag? PARADOX.

February 26, 1892.

The Loup Rivers in Nebraska.

I AM gratified that my article of Jan. 29 possessed some interest for so able an authority as Professor W. M. Davis of Harvard, albeit, he is somewhat critical.

My main propositions, and I think they will stand, notwithstanding the objections of my critic, are these: —

1. The Loup rivers were probably once "separate tributaries of the Platte, all independent of each other, as roughly indicated by the dotted lines on the map" (Fig. 1, p. 59, *Science*, Jan. 29, 1892).

2. Pliocene lacustrine deposition along the Platte "crowded the mouths of these tributaries eastward and made them coalesce into a single large tributary."

3. Headwater erosion "swept the upper courses westward by a series of captures."

Instead of my first proposition, Professor Davis ascribes to me the postulate "that at the beginning of the current cycle of river history the several branches of the Loup River all pursued independent courses to the Platte." He makes definite my indefinite "once," but not in a way that I can accept. The plain inference from my second proposition is that the period of separate existence of these tributaries was in the Miocene.

Whether that is equivalent to the "postulate" of Professor Davis depends upon the definition of "cycle." The facts, as I have read them in the field, are these: In Miocene times tributaries of the Platte, now constituting the Loup system, were developed only to the stage of young rivers, not mature rivers, as Professor Davis supposes. Then came submergence and partial obstruction of their valleys; partial only, because the Pliocene marls will not average more than fifty feet in thickness, not one-fourth of the depth of the valleys. When Lake Cheyenne retired, the rivers resumed business in their former channels, except near the Platte, where the excessive deposition turned them eastward. The silt in the Platte valley has been penetrated to the depth of five hundred feet without reaching the bottom.

Here then is a cycle of river history interrupted in its infancy, and subsequently resumed. Its course was not half run when the rivers were drowned, and, even now, after their emergence and resurrection, they are still young rivers, with abundant vigor and abundant opportunities for headwater erosion and river piracy. If this series of events may be accounted a single cycle, notwithstanding the Lake Cheyenne episode, then I can adopt the "postulate" as equivalent to my first proposition.

If I understand him aright, Professor Davis does not raise any objections to my second proposition. He does indeed argue against a supposed contention of mine, which is not mine at all, namely, that the coalescence of the lower courses into one Loup River was due to headwater erosion.

The effects which I did assign to headwater erosion were limited to the "upper courses," as stated in the third proposition. In spite of all objections, that proposition seems to be reasonable and valid. No region on this continent is more favorable for the study of simple, unobstructed headwater erosion than these western plains. The rivers are young. Great blocks of table lands lie yet unbroken by drainage lines, and into these fresh ravines are constantly eating back. The tertiary beds are soft and practically homogeneous, so far as resistance to erosion is concerned, so that no question need be raised about dip, strike, folds, or alternations of hard and soft strata. Upon such a terrane the Miocene rivers established themselves with a south east course consequent upon the slope to the south-east. The Rocky Mountain upheaval, together with excessive deposition along the Platte, changed the slope to the north-east, transverse to the established direction of the rivers. Cross-cutting and captures of westerly headwaters was the natural result of this change of slope.

The eastward tilt which the whole country got at the time the Rocky Mountains were elevated also affected the development of the main Loup. Without that upheaval the northern tributaries would have been dammed back by the silt along the Platte, and formed a series of swamps, instead of coalescing in a free-flowing stream.

That objection of Professor Davis, which is based upon the "systematic location" of Prairie Creek "between two parallel and larger rivers in a district of horizontal beds," is not serious. In the first place, I never dreamed of ascribing it to headwater erosion. It is obviously the result of Pliocene deposition crowding the Loup so far from the Platte that subsidiary drainage was developed on the intervening space. In the second place, this latest product, appearing upon the surface of a great mass of Pliocene silt, cuts no figure in determining the primitive course of channels lying at the bottom of that mass of silt.

Further criticisms from Professor Davis will be most welcome

L. E. HICKS.

The Aboriginal North American Tea.

IN *Science* for Jan. 22, 1892, is an abstract of Bulletin No. 14, United States Department of Agriculture, on "The Aboriginal North American Tea," *Ilex cassine*, which recalls to me that during our civil war, when the Confederate soldiers were encamped in the vicinity of the Rappahannock River, especially during the winter of 1862–3, that not only they, but also the inhabitants of that region, used freely the leaves of the American holly tree, *Ilex opaca*, in the preparation of a decoction as a substitute for China tea. This species of holly is not only abundant in that region, but grows to a large size, trees of eighteen inches in diameter and over being not uncommon in the thickets bordering the low grounds of the Rappahannock.

I do not know how they came to begin the use of this decoction, whether from a local handing down of the Indian custom of using the cassena tea, as Wood styles the *Ilex cassine*, or whether it may not have been suggested by soldiers from Alabama, who were numerous in the Confederate army, and who would be more likely to know of the use the Creeks made of the leaves of the shrub holly.

In this connection the question arises as to whether any use was made during our civil war of the leaves of the New Jersey tea, *Ceanothus Americanus*, which were used during the Revolution as a substitute for Chinese tea. JED. HOTCHKISS.

Staunton, Va., Feb. 24.

AMONG THE PUBLISHERS.

THE laboratory course in psychology, by Dr. E. C. Sanford, which is being published in parts in the *American Journal of Psychology*, is to be issued at a later date in book-form. It is the only practical course ever published.

— Messrs. J. Wiley & Sons, publishers of scientific works, New York City, have just issued the fourth edition of Thurston's "Manual of Steam-boilers," and the fourth edition of his "Friction and Lost Work in Machinery and Millwork." These works, like all others on their list, are kept under constant revision, and thus given continually increasing value. Each new edition shows the perfecting touch of the author's hand. Messrs. Baudry & Cie., the French correspondents of this firm, have completed a translation into French of the "Manual of Steam-engine and Boiler Trials," by the same author, and will at once send it to press. They are also translating the "Manual of the Steam-engine," and expect to have that in type before the close of the year, under special arrangements with author and publishers.

— The number for March begins the seventh year of *The Forum*, and for its seventh year several new enterprises in periodical work are announced: first and foremost, the Silver Question. The March number contains two papers on it — one by Mr. Bland, who makes his best argument for silver, and the other by Mr. Leech, director of the mint. In the following months *The Forum* will publish the most thorough discussion of this subject that has been made, by the foremost writers of both continents. These articles will be a special feature of forthcoming numbers. The Educational Investigation into the work of the public schools in the several large cities of the Union has already been announced, and Dr. J. M. Rice, the special student of the most advanced school-work abroad, is now engaged in this task. His articles will begin in an early number. In the March number are two noteworthy educational articles — one by Mr. Clarence King, on "The Education of the Future," wherein he shows the narrow limitations of all our teaching, and points out the yet undeveloped fields and methods, and the other by Professor John Earle on "The Study of English." Another line of special work laid down by *The Forum* is an investigation and discussion of Municipal Government, which is confessedly the weak place in our whole governmental system. The present number contains an investigation made by Professor Peabody of Harvard into the municipal government of Dresden. The Progress of the most Important Arts and Sciences will make a continuous feature of the coming volumes of *The Forum*, such as Music, Sculpture, Painting, Architecture, the Practical Application of Electricity, the Advance of Preventive Medicine, the Progress of Astronomy. Another group of subjects — old subjects that scientific progress makes of perpetual interest — will be Good Country Roads, and How Well they Pay; Scientific Agriculture and its Possibilities; What the Coming Man will Eat and How He will Cook it; and the like. The especial development of *The Forum* will be in the direction of original investigation by experts and authorities, into all classes of subjects of the greatest concern to readers who wish to keep abreast in their thought and lives with the world's progress; and the aim is never to thresh over old straw.

— A revised edition of Herbert Spencer's "Social Statics," the book which has created such a stir among social reformers, will be issued shortly by D. Appleton & Co., simultaneously with its publication in England. Having been much annoyed by the persistent quotation from this work, in the face of repeated warnings,

CALENDAR OF SOCIETIES.

Philosophical Society, Washington.

Feb. 27. — M. W. Harrington, Notes on the Climate of Death Valley; L. A. Bauer, Wilde's Explication of the Secular Variation Phenomenon of Terrestrial Magnetism; B. Pickman Mann, An Attempted Solution of a Social Problem.

Society of Natural History, Boston.

Mar. 2.—W. G. Farlow, Notes on Collections of Cryptogams from the Higher Mountains of New England; G. Frederick Wright, Invasion of Eastern England by Norwegian Glaciers; Additional Evidence Concerning Human Remains under the Sonora Table Mountain, California.

The Numismatic and Antiquarian Society, Philadelphia.

Mar. 3.—Daniel G. Brinton, Mediæval and Aboriginal Dramas.

Wants.

Any person seeking a position for which he is qualified by his scientific attainments, or any person seeking some one to fill a position of this character, be it that of a teacher of science, chemist, draughtsman, or what not, may have the 'Want' inserted under this head FREE OF COST, *if he satisfies the publisher of the suitable character of his application. Any person seeking information on any scientific question, the address of any scientific man, or who can in any way use this column for a purpose consonant with the nature of the paper, is cordially invited to do so.*

A PROFESSORSHIP in Chemistry is wanted by one who has had five years' experience in that capacity. Would prefer to give instruction by lectures and experiments rather than by text-book methods. Would like a position in a college or university where there is a good student's laboratory. Special points of strength claimed are: (1) Thorough control of a class and good order during lectures and recitations. (2) Accuracy in experimenting with chemicals and skill in the manipulation of chemical apparatus. The permission of several distinguished educators has been given to refer to them if required. Would not care to accept a position paying less than $1,500. Address B. E., care of *Science*, 874 Broadway, New York.

ADDRESS WANTED.—Will some one please send the address of the Secretary of the American Philological Society. Also that of Herbert Spencer. "ADDISON," Room 84, 164 Madison St., Chicago, Ill.

ADDRESSES of Old Book Dealers wanted.—Wishing to obtain a number of old books out of print, I very much desire the addresses or catalogues of rare second-hand book dealers. If there is a directory or list of such dealers I should like to obtain possession of one. W. A. BLAKELY, Chicago, Ill.

WANTED.—(1) A white man versed in wood and iron working, able to work from specifications and plans, suited for an instructor of boys; his business to have charge of shops of school, outline and direct the work for foremen and students; salary to be $1,000 per annum (nine months). (2) A man (black preferred) to teach the colored, iron working and forging, subordinate to the preceding; salary, $720. (3) A man (white) competent to take classes in engineering (assistant's position), but with the ability to perform any of the work required in any of the ordinary engineering courses of our universities; salary from $1,000 to $1,500. A. H. BEALS, Milledgeville, Ga.

Exchanges.

[Free of charge to all, if of satisfactory character. Address N. D. C. Hodges, 874 Broadway, New York.]

For exchange.—A fine thirteen-keyed flute in leather covered case, for a photograph camera suitable for making lantern slides. Flute cost $27, and is nearly new. U. O. COX, Mankato, Minn.

To exchange; Experiment Station bulletins and reports for bulletins and reports not in my file. I will send list of what I have for exchange. P. H. ROLFS, Lake City, Florida.

Finished specimens of all colors of Vermont marble for fine fossils or crystals. Will be given only for valuable specimens because of the cost of polishing. GEO. W. PERRY, State Geologist, Rutland, Vt.

For exchange.—Three copies of "American State Papers Bearing on Sunday Legislation," 1891, $2.50, new and unused, for "The Sabbath," by Harmon Kingsbury, 1840; "The Sabbath," by A. A. Phelps, 1842; "History of the Institution of the Sabbath Day, Its Uses and Abuses," by W. L. Fisher, 1859; "Humorous Phases of the Law," by Irving Browne; or other works amounting to value of books exchanged, on the question of governmental legislation in reference to religion, personal liberty, etc. If preferred, I will sell "American State Papers," and buy other books on the subject. WILLIAM ADDISON BLAKELY, Chicago, Ill.

Wanted, in exchange for the following works, any standard works on Surgery and on Diseases of Children: Wilson's "American Ornithology," 3 vols.; Coues' "Birds of the Northwest" and "Birds of the Colorado Valley," 2 vols.; Minot's "Land and Game Birds of New England;" Samuels' "Our Northern and Eastern Birds;" all the Reports on the Birds of the Pacific R. R. Survey, bound in 2 vols., morocco; and a complete set of the Reports of the Arkansas Geological Survey. Please give editions and dates in corresponding. R. ELLSWORTH CALL, High School, Des Moines, Iowa.

For sale or exchange, LeConte, "Geology;" Quain, "Anatomy," 2 vols; Foster, "Physiology," Eng. edition; Shepard, Appleton, Elliott, and Stern, "Chemistry;" Jordan, "Manual of Vertebrates;" "International Scientists' Directory;" Vol. I. *Journal of Morphology;* Balfour, "Embryology," 2 vols.; Leidy, "Rhizopods;" *Science*, 18 vols., unbound. C. T. McCLINTOCK, Lexington, Ky.

To exchange Wright's "Ice Age in North America" and Le Conte's "Elements of Geology" (Copyright 1882) for "Darwinism," by A. R. Wallace, "Origin of Species," by Darwin, "Descent of Man," by Darwin, "Man's Place in Nature," Huxley, "Mental Evolution in Animals," by Romanes, "Pre-Adamites," by Winchell. No books wanted except latest editions, and books in good condition. C. S. Brown, Jr., Vanderbilt University, Nashville, Tenn.

For Sale or Exchange for books a complete private chemical laboratory outfit. Includes large Becker balance (200g. to 1-10mg.), platinum dishes and crucibles, agate motors, glass-blowing apparatus, etc. For sale in part or whole. Also complete file of *Silliman's Journal*, 1862-1885 (62-71 bound); Smithsonian Reports, 1854-1883; U. S. Coast Survey, 1854-1869. Full particulars to enquirers. F. GARDINER, JR., Pomfret, Conn.

of views which he had abandoned, and by the misquotation of others which he still holds. Mr. Spencer some ten years ago stopped the sale of the book in England and prohibited its translation. But the rapid spread of communistic theories gave new life to these misrepresentations; hence Mr. Spencer decided to delay no longer a statement of his mature opinions on the rights of individuals and the duty of the state. The volume includes also "The Man *versus* the State," a series of essays on political tendencies heretofore published separately. Mr. Spencer has secured an American copyright for his new volume.

— P. Blakiston, Son, & Co., announce "A Manual of Autopsies, designed for the Use of Hospitals for the Insane and Other Public Institutions," by I. W. Blackburn, M.D., pathologist to the Government Hospital for the Insane, Washington, D.C. This subject has for several years engaged the attention of the Association of Medical Superintendents of American Institutions for the Insane, that the abundant material afforded the alienist may be systematically studied in all its clinical as well as pathological aspects. A uniform method being desirable, this manual has been prepared by the direction of the association committee, with the co-operation of Drs. Welch and Hurd of the Johns Hopkins Hospital, that by following the described routine in examination, pathologists, and even non-expert physicians, may prepare the most concise, graphic, and useful record of actual cases. It is fully illustrated from original drawings, sectional and regional, and as an adopted standard, should be in the hands of every examiner who can contribute to the valuable records thus established.

— D. Appleton & Co. have just ready "Moral Teachings of Science," by Arabella B. Buckley, intended for readers who would not take up an elaborate philosophical work; and a "Manual of Chemical Technology," by Randolph von Wagner, translated and edited by William Crookes.