of anthropology, give evidence of wide-spread interest.

Universities are establishing special courses in anthropology, and teachers and investigators are being trained. Officers of anthropological museums are preparing men to be field workers and museum assistants. The public need no longer be deceived by accounts of giants and other wonderful discoveries. The wares of the mercenary collector are now at a discount since unauthentic material is worthless.

Anthropology is now a well-established science; and with all this wealth of materials and opportunities, there can be no doubt that in time the anthropologists will be able to solve that problem, which for the past half century has been discussed in this Association—the problem of the unity or diversity of prehistoric man in America.

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*THE FIELD OF EXPERIMENTAL RESEARCH.**

PHYSICAL research by experimental methods is both a broadening and a narrowing field. There are many gaps yet to be filled, data to be accumulated, measurements to be made with great precision, but the limits within which we must work are becoming, at the same time, more and more defined.

The upper ranges of velocities, temperatures and pressures, which manifest themselves in the study of the stellar universe, are forever beyond the range of experiment. But while the astronomer must wait for opportunities to observe, the experimenter can control his conditions and employ his methods and his apparatus at once to the question in hand. Still this work must be done within a certain range or must be limited to conditions more or less easy to recognize. In spite of this fact, however, the progress made during the past century is not likely to cease or abate in the next, and the ever-increasing number of workers bodes well for the future enrichment of our science.

Whatever may be our ideas of fundamental entities, as expressed in various theories; whether, as an example, we regard the ether as like an infinitely mobile fluid, or as an incompressible solid, or as a jelly; or whether we incline to think that being an electromagnetic medium, it may be without mechanical properties, which properties depend in some way upon the electromagnetic nature of the ether, we cannot reach sure ground without the experimental test.

The development in the field of research by experiment is like the opening of a mine, which, as it deepens and widens, continually yields new treasure, but with increased difficulty, except when a rich vein is struck and worked for a time. In general, however, as the work progresses there will be needed closer application and more refined methods. We may, indeed, find our limit of depth in the mine of experiment in inordinate cost, in temperatures too high, or in pressures beyond the limits of our skill to control.

It is but a few months since Professor Dewar, by the evaporation of liquid hydrogen in a vacuum, closely approached, if he has not reached, our lower limit of possible temperature. Investigations of the effects of low temperature upon the properties of bodies must, from the present outlook, be forever limited to about 20° C. above absolute zero, unless a lighter gas than hydrogen be discovered upon the earth, the actual existence of which it is, of course, impossible to conjecture. Before the actual experimental demonstration of this limit the limit itself was known to theory, at least approximately, but the spur of the experi-

*Address of the Vice-President and Chairman of Section B, Physics, before the American Association for the Advancement of Science at the Columbus meeting, August, 1899.

menter is the overcoming of difficulties and the possibility of new discoveries which come as surprises. In the case in question a liquid of extremely low density, only one-fourteenth that of liquid nitrogen, was produced, while still defined by clear and well-marked refracting surfaces.

When we turn to the consideration of the field for research work at high temperatures we are not confronted by the fact of a physical limit existing which may be approached but never reached. We can imagine no limit to possible increase of temperature, such as is the absolute zero a limit of decrease. While we may actually employ in electric furnaces temperatures which, according to Moissan, have a lower limit of 3,500° C., we can realize the possibility of temperatures existing in the stars measured by tens of thousands or hundreds of thousands of degrees of our temperature scale.

The moderate increase of working temperature given by the electric furnace enabled Moissan and others to reap a rich harvest of experimental results, and the natural inference is that much more might be expected from further extensions of the limits. These limits are, however, already set for us by the vaporization of all known substances. Our furnace itself keeps down the temperature by melting and volatilizing. We may indefinitely increase the energy in an electric arc and thus add to the heat evolved, but the addition only goes to vaporize more material. The limit of work then seems to be readily reached in the electric furnace, no materials for lining being available, not subject either to fusion or vaporization, thus using up the energy which would otherwise go to increase the temperature.

A suggestion as to a possible extension of temperature range may be made here. It may be requisite to work with closed receptacles under pressure, and to discharge through them electric currents of so great energy-value as to attain almost instantaneously the highest temperatures, to be maintained for only a very short time. We may imagine a huge condenser charged to a potential of, say, 10,000 volts as discharged through a limited body of gas contained in a small space within a strong steel tube which has a lining of refractory non-conductor. The energy may thus possibly be delivered so suddenly to a very limited body of material as to result in a momentary elevation of temperature passing all present known limits and capable of effecting profound changes in molecular constitution. We need all possible extension of the limits of research in this direction in order to discover some clue to the relations which the chemical elements bear to each other. The limit of possible strength of the containing receptacle, or some unforeseen factor, would probably set the new bounds. The point to be here enforced, however, is that far beyond any increase of working range in temperature, obtained in any way, there must still exist a further range unattainable by our best efforts and possibly forever outside of the field of experimental research. Our knowledge of this higher range can alone be derived from a study of the actions going on in the stars and nebulæ.

As with the temperature range so it is with the pressure range. We may easily work under conditions which involve no pressure, but when we attempt to conduct our inquiries with increase of pressure we soon find a limit to the tenacity of our strongest vessels or to our ability to produce and maintain extreme pressures. We may work, not easily it is true, with pressures up to a few tons to the square inch, but this is as nothing compared to the conditions which we know must exist within the larger celestial bodies, without reference to their condition, solid, liquid or gaseous. Can we ever hope to experimentally reproduce the condition of a mass of gas so compressed that in spite of a very high temperature its volume is less than that of the same mass cooled to solidification? Yet this extreme of condition must be the normal state within the bodies of many of the stars.

It has been aptly said that many, and perhaps most, of the important discoveries have been made with comparatively simple and crude apparatus. While this may be true, yet it is probably true also that future advance work is likely to require more and more refined means and greater nicety of construction and adjustment of apparatus. The expense or cost, if not the difficulty of the work, may become so great as to effectually bar further progress in some fields. When instruments require to be adjusted or constructed, to such refined limits as a fraction of a wave-length of light, but few can be found to undertake the work. The interferometer and echelon spectroscope of Michelson involve such minute adjustments that a wave-length of light is relatively thereto a large measure. It is well known that this comparative coarseness of light waves imposes a limit to the powers of optical instruments, as the microscope and telescope, such that no perfection of proportion, construction and correction of the lenses can remove.

In most fields of research, however, progress in the future will depend in an increasing degree upon the possession, by the investigator, of an appreciation of small details and magnitudes, together with a refined skill in manipulation or construction of apparatus. He must be ready to guide the trained mechanic and be able himself to administer those finishing touches which often mark the difference between success and failure. There must be in his mental equipment that clear comprehension of the proper adjustment of means to ends which is of such great value in work in new fields. He must also learn to render available to science the resources of the larger workshops and industrial establishments.

The application of physical principles upon a large scale in such works has frequently, in recent years, resulted in great gains to science itself. The resources of the physical laboratory are often relatively small and meagre compared with those of the factory. Experimental work in certain lines is now frequently carried on upon a scale so great and under such varied conditions as would be almost impossible outside of a large works.

In no field has this been more true than in that of electricity during the past few years. We need only instance the progress in alternating currents and in relation to the magnetic properties of iron. In large scale operations effects which would be missed or remain masked in work undertaken upon a more restricted scale receive emphasis sufficient to cause them to command attention. The obstacle of increasing costliness of equipment, which in some fields might act as a bar to further progress, can only be overcome by more liberal endowments of laboratories engaged in advance work. Even those in the community who can only understand the value of scientific work when it has been put to practical use may find in the history of past progress that many discoveries in pure science which had not, when made, any apparent commercial importance or value have in the end resulted in great practical revolutions.

Could Volta, when he discovered the pile one hundred years ago, have had any idea of its importance in practical work? Or, did Davy or his contemporaries at the time of his experiments with the arc of flame between the charcoal terminals of his large battery have any suspicion that in less than one hundred years the electric arc would grow to such importance that more than 100,000 arc lamps would become a single year's production in this country alone. Faraday, when he made his researches npon the induction of electric currents from magnetism, could not have had any idea of the enormous practical work in which the principles he dealt with as facts of pure science would find embodiment. When he wound upon the closed iron ring the two coils of wire which enabled him to discover the facts of mutual induction he had begun, without any suspicion of the fact, the experimental work which gave to science and to practice the modern transformer, now built of capacities ranging up to 2,500 H. P. each, and for potentials of 40,000 to 60,000 volts.

These examples, and many others which might be given, should convince even the most arrogantly practical man of the high value of scientific research, not alone as adding to the sum-total of knowledge and for the admirable training it gives, but because it cannot fail to have an ultimate practical effect. Discoveries which at first seem to have no useful nor practical outcome are often the very ones which underlie development of the greatest importance in the arts and industries.

The work of Hertz upon electric waves was to the physicist a grand experimental demonstration, tending to prove the truth of the electromagnetic theory of light, and subsequent progress was profoundly influenced by it, though no practical use followed at once. The physicist to-day may see in the wireless telegraph only an extension of Hertz's original work, for he need not consider the commercial or economic outcome. He may, however, recognize the fact that in the wireless telegraph, as developed by Marconi, practice calls for a broader theoretical view. Certain elements of construction and adjustment of apparatus, at first used and regarded as essential from a theoretical standpoint, have already been laid aside. The radiator, with its large polished brass spheres and special spark gap, has been found of no more effect than the simple pair of small balls ordinarily constituting the terminals for high potential discharges. It has been found that the transmitting and receiving apparatus do not require to be attuned, and that the receiving coherer is not the true recipient of the electric wave or disturbance in the ether.

These later developments are, in fact, departures, more or less wide, from the principles underlying the Hertz demonstration. A vertical wire is charged to a high potential and discharges to earth over a spark gap. During the discharge the wire becomes a radiator of electromagnetic pulses or waves, regardless of the spark radiation. The receiving vertical wire is likewise alone relied upon to absorb the energy. Being in the path of the electromagnetic wave conveyed in the ether from the transmitting wire, it becomes the seat of electromotive forces which break down the coherer. This, in substance, may be considered as a series of small or microscopic spark gaps which can be crossed by the comparatively low potentials developed in the receiving wire. We are thus taught to recognize the fact that the refinements in methods and apparatus needed for a delicate physical demonstration as of the Hertz waves in this instance may often be laid aside in practical application, where the end to be achieved is different. The sudden discharge of the Marconi transmitting wire may possibly give rise to a series of oscillations or high-frequency alternating waves in the wire, but since the first half of the first wave at each discharge will have the greatest amplitude it is doubtful if those which follow in the short train have any decided effect upon the receiver. According to this view the fact of the discharge being oscillatory may, indeed, have no essential relation to the work done, but may be an unavoidable incident of the very sudden discharge which itself would set up a single pulse in the ether sufficiently intense for the work even if unaccompanied by lower amplitude oscillations following the first discharge pulse.

Before leaving the consideration of this most fruitful field of experimental research opened by Hertz, it may be stated that the one gap in the work yet to be filled is the actual production of electric waves of a wave-length corresponding to those of the spectrum. If this could be done by some direct method, no matter how feeble the effect obtained, the experimental demonstrations of the electric nature of radiant heat and light would be fitly completed. Several years ago it occurred to me that it might be possible to devise a method for accomplishing the end in view, and so close the existing gap. Many years ago an observation on sound echoes showed clearly the production of high-pitch sounds from single pulses, or lower-pitch waves. A bridge over a mile in length was boarded at the sides, and vertical slats regularly and closely placed along its side formed, for a sound wave incident thereon, a series of reflecting edges or narrow vertical surfaces, a kind of coarse grating. It was found that a loud sound or pulse, such as that of a gun-shot, emanating from a point near one end of the bridge and two to three hundred feet in a line from the structure, was followed by an echo which was in reality a high-pitch musical tone. The pitch of this tone corresponded to the spacing of the slats in the bridge considered as a reflecting grating for sound.

Following this principle, it seems possible that a very sudden pulse in the ether or electromagnetic wave, incident at an angle upon a reflecting grating having from 20,000 to 40,000 ruled lines to the inch, if the plane of incidents were at right angles with the rulings, might be thrown into ripples of the wave-length of light and yield a feeble luminosity. If the color then varied with the angle of incidence chosen and with the angle through which the reflection passed to the eye the experiment would be conclusive.

Despite the diligent studies which had been made in the invisible rays of the spectrum, both the ultrared and ultraviolet, a work far from completion as yet, the peculiar invisible radiation of the Crookes tube remained unknown until the work of Lenard and Röntgen brought it to the knowledge of the world. The cathode discharge, studied so effectively by Hittorf and Crookes, and by the latter called 'radiant matter,' was but a part of the whole truth in relation to the radiation in high vacua. It is needless to recount the steps in the discovery of Röntgen rays. We now know that these rays come from the impingement of the 'radiant matter' or cathode rays. We know, also, that the higher the vacuum, and, therefore, the higher the electric potential needed to effect the discharge, the more penetrating or the less easily absorbed is the resulting radiation. Rays have been produced which in part pass through cast iron nearly an inch thick. The iron acting as a filter absorbs all rays of less penetrating power. A question may here be put, which it will be for future experiment to answer: Can we, by increasing the degree of vacuum in a Crookes tube by the employment of enormous potentials for forcing a discharge through the higher vacuum, produce rays of greater and greater penetrating power? What, in fact, may be the limit, or is there any limit, to the diminution of wave-length in the ether, assuming for the moment that this invisible radiation is somewhat of the same nature as light, but of higher pitch, though it may be unlike light in not representing regular wave trains.

Röntgen radiation, while spoken of as

invisible, is in reality easily visible if of great intensity. The parts of the retina which respond and so give the sensation of luminosity are apparently those around the eye and not directly opposite to the iris opening. Those parts of the retina sensitive to the rays are characterized by the preponderance of 'rods,' giving the simple sensation of illumination, apparently white in the case in question. The 'cones,' or those portions of the retinal membrane whose function is believed to be the recognition of color or differences of wavelength, appear not to be excited by the Röntgen radiation, or only very feebly. If this be true it would account for the less intensity of the luminous effect upon those portions of the retina near the optic axis of the eye. All this favors the view that the Röntgen radiation is without sustained pitch or wave trains, and resembles more a sharp noise or crash in sound.

For pressing experimental work in the highest vacua to its limit, as above suggested, we already have means at command for the production of the most complete exhaustions, requiring extremely high potentials to pass an electric discharge. We have, also, in well-known forms of high-frequency apparatus the means for producing electromotive forces limited only by our means for insulation. A recent apparatus devised by me and called a dynamostatic machine gives equal capability of producing high potentials of definite polarity, positive and negative. It should not be long, therefore, before work is undertaken in this suggested direction of pressing this matter of rays of high penetrating power much farther than has been done. The question arises whether any such rays can exist which are not appreciably absorbed in passing through dense substances. They would probably not affect a photographic plate nor a fluorescent screen. If they lost also the property of ionizing a gas and causing electric convection we might not even be able to discover them. That some influence or action in the ether does actually penetrate the dense masses in space is evidenced by gravitation, the mystery of mysteries. We are, however, not justified in going beyond the proved facts which can only be the result of experimental work and close observation. All else is speculation. The energy source of the Becquerel rays is another mystery apparently far from being cleared up, and if it be true, as recently announced, that a substance named radium has in reality nine hundred times the power of emitting these rays than is possessed by uranium and thorium, and that the radiation is able to cause visible fluorescence of barium platinocyanide, the mystery but deepens and makes us again think of the possible existence of obscure rays only absorbed and converted by a few special substances.

The diffusion which takes place when Röntgen rays pass through various media is another phenomenon which needs more attention from investigators. This effect seems to be produced by all substances in a greater or less degree. It, however, appears to be nearly absent in the case of those substances which give out light or fluoresce under the rays, as barium platinocyanide and calcium tungstate. It will be important to determine definitely whether the rays diffused by different substances are lowered in pitch or penetrating power as compared with the rays exciting the diffusion; whether, in other words, the rays from a tube with quite high vacuum excite similar rays by diffusion, or rays more absorbable; and if a lowering takes place whether it occurs in like manner and degree for all diffusing media.

The phenomenon may be akin to fluorescence, as when quinia sulphate converts the invisible ultraviolet rays of the spectrum into lower rays or visible light. This action may be at its extreme when barium platinocyanide, excited by Röntgen rays, so lowers the pitch as to produce rays within the visible spectrum, for this compound gives very little or no Röntgen-ray diffusion. Are there substances which under Röntgen rays fluoresce with invisible rays of the order of the ultraviolet of the spectrum? If, as is the case with solid paraffine, the irradiated substance gives rise to considerable diffusion it can, as I have noted, produce a secondary diffusion in other masses of the same substance, or of other substances, as indicated by feeble fluorescence of the sensitive barium salt, thoroughly screened from the direct source of rays and from the first or primary diffusion. It is probable that Tertiary diffusion could be found if we possessed a far more powerful or continuous source of the rays for exciting the initial diffusion. The ray emission, even in the most powerfully excited tube, is probably so intermittent that the active period is but a fraction of the total time. It may easily be that the limit of intensity of Röntgen-ray emission has not yet been reached, especially when artificially cooled anti-cathode plates are available.

There is much room for experimental work in this fascinating field. We need for it the means for the production either of a continuous electric discharge at from 60,000 to 100,000 volts or a high-frequency apparatus capable of giving an unbroken wave train; that is, a succession of high period waves of current without breaks or intermissions.

The ordinary high-frequency apparatus for obtaining discharges of high potential from alternating currents gives only a rapid succession of discharges each consisting of a few rapidly dampened oscillations. These discharges occupy but a small fraction of the total time. This is very different from a continuous sustained wave train, with the successive waves of equal amplitude following each other without break. Such sustained waves will, doubtless, be of use in research, especially in vacuum-tube work, and they would, of course, convey much more energy than the usual brokon or interrupted discharge known as a high-frequency discharge.

Some six or seven years ago I endeavored, while working upon the subject of high frequency, to fill the gap. The result was an apparatus which, with its modifications, deserves more study and experiment than I have been able to give to it. A brief description may not be out of place. A large inductance coil with a heavy iron wire bundle for a core, a coil of relatively few turns with no iron core, and a condenser of variable capacity, were connected in series across the mains of a 500-volt electric circuit. The smaller coreless coil and the condenser were arranged to be shunted by an adjustable spark gap with polished ball terminals. By simply closing for a moment the spark gap so as to form a low resistance shunt around the condenser and the small coil, and afterward slowly separating the balls, the local circuit of the condenser, small coreless coil and shunting gap become the seat of sustained oscillations, the frequency of which depends upon the relation of inductance and capacity in the local circuit. The energy supplied is that of a continuous current through the large inductance coil with the heavy core. The action of the apparatus is easily comprehended by a little study. The oscillating current in the local circuit may be made to induce much higher potentials in a secondary circuit inductively related thereto. In this case the turns of the secondary in relation to the primary are, as usual, such as to step-up the potential. In other words the potential developed in the secondary is determined by the transforming ratio.

We thus have a high-frequency apparatus

in which the waves are sustained in an unbroken series, and we employ as the source of energy a continuous current circuit. It shows that we may continuously supply energy to an oscillating system and so keep up the amplitude of electric oscillations, the frequency of which is that due to the capacity and inductance of the part of the circuit in which oscillations are set up.

While, in the forms of high-frequency apparatus alluded to, we may obtain almost any differences of electric potential up to millions of volts, assuming the apparatus large enough for the work, we do not get a sustained separation of positive and negative charges, as in the static machine, or in a less complete degree with the inductive coil. Professor Trowbridge, of Harvard, has, however, made use of large Planté rheostatic machines, the condenser plates of which are charged in parallel from 10,000 small storage cells connected in series. The discharge of the condenser plates is effected after they are connected in series by a suitable connection changing frame moved for the purpose. Very high potential discharges are thus obtained and the polarity is always definite. It is manifest that the size of the apparatus and the perfection of its insulation determine the possible performance. The objection to such an apparatus for experimental research or demonstration is the large number of cells required and the complicated arrrangements of circuits for charging them. I have, however, recently succeeded in removing all necessity for the presence of charging cells, and have produced what may be termed a dynamostatic machine which is worked by power or by current from a lighting circuit, either continuous or alternating, and may replace a static machine. It is, of course, not dependent upon the weather. I trust it may be of sufficient interest to merit the following brief description: A small electric motor has in addition to its commutator a pair of rings connected to its armature winding for obtaining alternating currents. The shaft of the motor drives synchronously a revolving frame bearing connections which, as in the Planté rheostatic machine, connect a series of condenser plates alternately in parallel for charging and in series for discharging at high potential. A small oil-immersed step-up transformer has its primary connected to the brushes bearing upon the two alternating current rings of the motor, and its secondary, giving say 20,000 volts, is periodically connected to the condenser plates while in parallel, by means of the revolving connection frame. The adjustment is such that only the tops of the alternating waves or their maxima are used to charge the condenser plates, while, also, those halves of the waves which are of the same polarity are alone used, the others being discarded or left on open circuit. The apparatus may be driven by power, in which case the electric motor becomes a dynamo, exciting its own field and supplying alternating current to the primary of the step-up transformer, or suitable alternating currents may drive it as a synchronous motor. Such a machine, run by continuous currents and having only eleven plates, gives sparks between its terminals over twelve inches long in rapid succession. It can be built cheaply, and is a highly instructive machine from the transformations it illustrates.

The machine is also arranged by the addition of a simple attachment, so that it may be used to charge insulated bodies, or to charge Leyden-jar condensers or the like, replacing the ordinary static machines. It might, in fact, be used to charge a second range of condenser plates in another rheostatic machine to a potential of 100,000 volts, for example. These, after coupling in series or cascade, might be made to yield potentials beyond any thus far obtained.

The interest in such experimental apparatus and the results obtained come largely from the apparent ability to secure a representation of the effects of lightning discharges upon a moderate scale, and the possibility of studying the action of air and other gases, as well as liquids and solids, at varying temperatures and pressures under high electric stresses. Broadly considered, however, the similarity of the effects to those produced in a thunder cloud is more apparent than real. The globules of of water constituting the electrified cloud do not possess charges of millions of volts potential, the effects of which are seen in the stroke of lightning. The individual globules may possess only a moderate charge. When, however, they are massed together in a large extent of cloud the virtual potential of the cloud as a whole, with respect to the earth, may be enormous, though no part of the cloud possesses it. The cloud mass not being a conductor, its charge cannot reside upon its outer surface or upon its lower surface nearest the earth, as with a large insulated conductor. The charge, in fact, exists throughout the mass, each globule of water suspended in the air having its small effect upon the total result.

When the cloud discharges, the main spark branches within and through the cloud mass in many directions. The discharge can at best be only a very partial one, from the nature of the case. These are conditions which are certainly not represented in our experimental production of high-potential phenomenon, except perhaps upon a very small scale in the electrified steam from Armstrong's hydroelectric machine, a type of apparatus now almost obsolete. Yet if we wish to reproduce, as nearly as possible upon a small scale, the conditions of the thunder cloud, we shall be compelled to again resort to it. In volcanic eruptions similar actions doubtless occur and give rise to the thunder clouds which often surround the gases sent out from the crater.

Considering, then, that the conditions in the thunder cloud are so different from those in our experiments with high potentials, we can easily understand that the study of lightning phenomena may present problems difficult to solve. Two forms at least of lightning discharge are quite unknown in the laboratory—namely, globular lightning and bead lightning, the latter the more rare of the two. Personally I cannot doubt the existence of both of these rare forms of electric discharge, having received detailed accounts from eye witnesses. On one occasion, while observing a thunder storm, I narrowly missed seeing the phenomenon of globular lightning, though a friend who was present, looking in the opposite direction, saw it. The explosion, however, was heard, and it consisted of a single detonation like the firing of a cannon. According to the testimony of an intelligent eye-witness, who described the rare phenomenon of bead lightning within an hour after it had been seen, it is a very beautiful luminous appearance like a string of beads hung in a cloud, the beads being somewhat elliptical and the ends of their axes in the line of their discharge being colored red and purple respectively. This peculiar appearance, not at any time dazzlingly bright, persisted for a few seconds while fading gradually.

Again, our knowledge of the aurora is not as yet much more definite or precise than it is in regard to the obscure forms of lightning alluded to above. Whether these phenomena will ever be brought within the field of research by experimental methods is an open question.

The endeavor in the foregoing rather disconnected statements has been to indicate directions in which the field of experiment may be extended and to emphasize the fact that research must be carried on by extension of limits, necessitating more liberal endowment of research laboratories. I have tried to make it clear that the physicist must avail himself of the powers and energies set in play in the larger industrial enterprises, and finally that the field of possible exploration in physics by experimental methods has its natural boundaries, outside of which our advances in knowledge must be derived from a study of celestial bodies.

The riddle of gravitation is yet to be solved. This all-permeating force must be connected with other forcec and other properties of matter. It will be a delicate task, indeed, for the total attraction between very large masses closely adjacent, aside from the earth's attraction, is very small.

Scientific facts are of little value in themselves. Their significance is their bearing upon other facts, enabling us to generalize and so to discover principles, just as the accurate measurement of the position of a star may be without value in itself, but in relation to other similar measurements of other stars may become the means of discovering their proper motions. We refine our instruments; we render more trustworthy our means of observation; we extend our range of experimental inquiry, and thus lay the foundation for the future work, with the full knowledge that, although our researches cannot extend beyond certain limits, the field itself is, even within those limits, inexhaustible.

ELIHU THOMSON.

PHOSPHORESCENT SUBSTANCES AT LIQUID-AIR TEMPERATURES.

A RECENT number of the *Philosophical Magazine** contains a paper 'On Phosphorescence,' by Herbert Jackson, which was delivered before the meeting of the British Association at Bristol, September 12, 1898, a portion of the paper being devoted to a review of the results obtained in researches relating to phosphorescent phenomena. It is evident from the paper that considerable investigation has been undertaken to ascertain the effects of high temperatures on phosphorescent substances, but that comparatively little has been done towards learning the behavior of the latter at very low temperatures, such as are obtained by the use of liquefied air. It is stated, however, in the paper referred to, that, "Professor Dewar has shown that great reduction of the temperature will cause phosphorescence to linger for a considerable time in many substances which had hitherto been considered as practically non-phosphorescent." This in particular refers to the phosphorescence produced in certain substances when exposed to light while at a temperature near that of liquefied air. Ivory, paper, and various other materials show phosphorescence under such conditions, but little or none at normal temperatures (20° C.).

Professor Dewar has found also that when a phosphorescent substance is excited by light at a normal temperature and then immersed in liquefied air the phosphorescent discharge is practically suspended, and continues so while the substance remains at the low temperature. August and Louis Lumiere have recently published a note in the *Comptes rendus*, CXXVIII., No. 9, 1899, p. 549, 'Influence des températures très basses sur la phosphorescence,' to which reference will be made presently.

The results obtained in some experiments made by the writer on the effect of liquid-air temperatures on phosphorescent substances are given below. These experiments were already completed when it was learned that the above-mentioned note in the *Comptes rendus* had been published. They were as follows:

Balmain's luminous paint, which is strongly phosphorescent at normal temperatures, was subjected to a very low temperature by the use of liquefied air, boiling

* *Phil. Mag.*, Lon. 46, 281, p. 402, Sept., 1898.