I should like to see in NATURE the views of some scientific men on this matter, both of Britain and America.

The question is certainly of great importance to scientific inquirers in nearly all branches of scientific endeavour, and it is to be hoped abler minds than mine may lay hold of the enterprise. W. K. MORRISON.

Devonshire, Bermuda Islands, October 15.

Siemens' Domestic Gas Fire.

DR. POLE'S letter on the Siemens' Domestic Fire drew my attention to the inquiry on the subject which Mr. Foster addressed to you in his letter published in NATURE of September 17.

I have had one of these fires in my office at the Society of Arts for some years. It was put in under Sir William Siemens' own superintendence, about the time when he described the grate in NATURE, so it must have been at the end of 1880 or the beginning of 1881. For a long time I used it with coke in the manner intended by the inventor; but practically I have found it more convenient to use ordinary coal, although it is doubtless less economical.

As Dr. Pole points out, the convenience of having gas ready to be turned on whenever the fire gets low or goes out, is very great; and in cases where a rather wasteful consumption of gas can be prevented, or is not considered of great importance, there can be no doubt but that the fitting of a few gas jets to an ordinary grate is a very great convenience. There is also a good deal of trouble saved in the lighting of the fire, as no paper or wood is required; the grate is simply filled with coal, and the gas turned on and lighted. The fire, I should say, burns up at least as rapidly as when lighted in the ordinary way.

If any of your readers are interested in the question, they are very welcome to see the grate at work whenever they like to call here. H. T. WOOD.

Society of Arts, John Street, Adelphi, London, W.C., November 2.

Diselectrification by Phosphorus.

IN the course of some experiments made a few weeks ago, upon the discharge of electricity by air which had been traversed by X-rays, it occurred to me to try whether similar action would be exerted by air in which phosphorus was being oxidised. I found that a gold-leaf (Dutch metal) electroscope was quickly discharged when a stick of phosphorus was held near it. A small metal crucible was afterwards connected with the electroscope, and a clean slice of phosphorus half an inch in diameter was supported within it at a distance of about half an inch from its sides and bottom. The electroscope was completely discharged in six seconds, the action being more rapid than that of a burning strip of nitrate of lead touch-paper one inch in width.

It might be found convenient to attach a lump of phosphorus instead of a fuse to the nozzle of a water-dropping collector in times of severe frost.

I do not remember to have met with a previous record of this observation. It is of interest in connection with the note on slow oxidation, in NATURE of October 29 (p. 631).

SHELFORD BIDWELL.

The Departure of Swallows.

"E. P." mentions in NATURE of October 22, a date, somewhere about October 20, I presume, which he considers is an unusual one for swallows. Now, though the bulk of the swallows have left by this time, it is by no means unusual to see them later on in the year. In 1894 I saw swallows in Kent, in the neighbourhood of Tonbridge, on October 20, 21, 25 and 27, and the last one on November 11; it was flitting about a village in a bewildered sort of way, with a crowd of village boys throwing mud and clods of earth at it.

The same year a flock of martins stayed near some buildings from October 28 to November 16; by this time many of them had died of cold.

The latest swallows I have seen this year I saw on October 23, near the same buildings. J. BROWN.

Tonbridge, Kent.

I BEG to send you the following extracts from my journal respecting the late appearance of the swallows.

1855, December.—It is worthy of record that several swallows were seen in this locality towards the end of November and during the first week of this month. I have ascertained that they were seen in other counties at the end of November; it must not be considered, therefore, as a merely local or solitary instance of the late appearance of these birds.

1863, October.—A few swallows were seen flying above the church on the 24th, and again on the 31st.

1867, November.—Some swallows were observed flying about during the last week.

These observations were made at Uckfield.

C. LEESON PRINCE.

The Observatory, Crowborough Hill, Sussex, October 26.

A Mechanical Problem.

THE mechanical problem proposed by your correspondent, "Cromerite," in the last issue of NATURE (October 29), is practically answered by the so-called "jumping beans" that are now being exhibited and sold in many parts of London. In this case a hard, rigid seed is seen to travel about in a series of small jerks, being slightly lifted from the ground at each movement. Upon dissection the seed is found to be hollow, the original contents having been devoured by a coleopterous larva—a soft fleshy maggot—which now partially occupies the cavity, and by its spasmodic movements causes the strange antics of the natural box in which it is enclosed. The walls of the seed appear to be quite rigid and inelastic. E. E. GREEN.

November 1.

HERTZ'S MISCELLANEOUS PAPERS.[1]

ANYTHING written by Hertz is of interest; and these papers are of interest, not only on this account, but also on account of their suggestiveness. It is always a question as to the desirability of republishing and translating papers published some years ago. Most valuable papers of ten years' standing have produced their effect. Their vitality has been transmitted to and reproduced in subsequent work, but what the scientific world requires is advance rather than revision. The work of pioneers is, however, largely an exception to this rule. They are generally in advance of their times, and much of their work is of value long after it was done. Such an one was Hertz. Most of his papers are suggestive of questions which still require answers, and they all breathe a spirit that, as he says himself of Helmholtz's work, evokes "the same elevation and wonder as in beholding a pure work of art." His papers are not mere enumerations of observations, nor mathematical gymnastics. Each has a definite purpose and an artistic unity. A life-giving idea pervades it. It is no mere dry bones, but an organic whole that lives for a purpose, and does some work for science.

Prof. Lenard has earned much gratitude for his Introduction. It gives a charming picture of Hertz, of his simplicity, his devotion to science, his loving regard for his parents. There is just enough added to the very well-selected letters to give the reader a continuous view of Hertz's work, and enable him to follow its development, and hence feel an interest in it and sympathy with the worker, thus fulfilling the best ideal of the biographer.

One of Hertz's first investigations was as to the kinetic energy of an electric current. The question is still of great interest. It is known that the magnetic induction that accompanies an electric current behaves exactly as if it were a mass moving with inertia. This is the inertia of magnetic induction. Hertz was, however, looking for a different inertia. He looked at the subject from the flow of electricity point of view. He thought that there might be some phenomenon corresponding to an inertia of the electric charges, which upon this theory are supposed to be flowing in opposite directions through a conductor. He supposed that these might have some inertia

[1] "Miscellaneous Papers." By Heinrich Hertz. With an Introduction by Prof. Philipp Lenard. Translated by D. E. Jones and G. A. Schott. Pp. 364. (London and New York: Macmillan and Co., Ltd. 1896.)

in addition to the magnetic inertia which accompanied their motion. To test this he tried two different forms of experiment, and obtained results which showed that if there were inertia of this kind, it must be small compared with that of the magnetic kind. The first method consisted essentially in a careful comparison of the extra current in a conductor with its calculated value; the second consisted in observing whether anything like the action by which the trade winds are deflected from a due northerly and southerly flow by the rotation of the earth, could be observed in a rotating conductor when traversed by an electric current. That there is some directed inertia in the conductor when traversed by an electric current is very probable, and in some cases we can be sure it exists. Hertz himself remarks that the inertia of the motion of the ions in electrolysis is considerably greater than what he was looking for in a metallic conductor. He could not make sufficiently delicate experiments with his apparatus to detect it, however, when using the small densities of current that were available in liquids; but the question is of great interest, and deserves further investigation. There can be no doubt that in gaseous discharges, kathode rays, as well as in electrolysis of liquids, there is a directed motion of matter accompanying the electric current which would be of the nature of the inertia Hertz was looking for, but failed to find. There seems much reason for thinking that in metallic conductors some similar actions are also taking place. Besides all this, there is the question as to how far the theory that all electricity is molecular and consists of electrons, involves the supposition of an inertia of this kind. Is the inertia of an electron completely specified by the magnetic force accompanying it? Does it occupy no space itself, and is its external field its all? We are hardly in a position to answer such questions. We might, however, be able to answer the former question as to the inertia of the directed matter movements accompanying the current, and as to another interesting question of a similar kind, namely, as to how far we can legitimately assume the current inside a conductor to be absolutely homogeneous. The self-induction of a single wire of a square m.m. in section is not exactly the same as that of, say, a hundred wires each of the thousandth of a square m.m. in section, and distributed over the square m.m. Subdividing the current would increase its self-induction. Outside the wire the distribution of magnetic force would be practically the same as before, but inside we would have it concentrated into a hundred small wires instead of being uniformly distributed, and the effect of this would be to slightly increase the self-induction, and the more so the smaller the section of each wire into which the square m.m. were subdivided. Hence we conclude that if the current in a real wire be from molecule to molecule, and so be concentrated on certain lines, its inertia should be somewhat greater than that calculated from the hypothesis that it is uniformly distributed over the section of the conductor. The difference between these two views is most clear when we consider the case of a Leyden discharging by its insulating medium becoming a conductor. If the Leyden be completely closed, and the medium become a conductor in such a way that the strain in each cubic cm. is there destroyed by conductivity, there will be no magnetic force anywhere accompanying this discharge of the Leyden, and consequently no magnetic inertia, if the conduction be perfectly homogeneous. Now it seems almost impossible that any directed change can take place without some accompanying inertia, and we may consequently conclude that either (*a*) an electric current has inertia such as Hertz was looking for, or (*b*) electric conduction currents are essentially heterogeneous, or (*c*) electric conduction is essentially accompanied by material inertia, or (*d*) two or all three of these are true. That (*c*) certainly exists in this case is incontestable in view of the known directed strains that Kerr and Duter have proved to exist in matter subject to electric stress. What is the complete answer, is the important question. It is still unsolved. It lies at the foundation of every theory of electric conduction. Hertz attacked it. It is still waiting solution.

The papers on the contact of elastic spheres and on hardness are most valuable contributions to the subject. They place the question of hardness on a scientific basis, and lay the foundation for a quantitative study of this most variable property of matter. There is no quality in which different materials differ more than in hardness. Electric conductivity is perhaps as various as hardness, compressibility, and viscosity, but hardly any other quality of matter is at all comparable with these in variety of range. Of these hardness is one of the most important and least known, and since Hertz's work on it can be scientifically studied. Innumerable subsidiary questions arise in connection with it. Why are some bodies so easily polished? Is the polishing of marble connected with the ease with which crystals of calcspar can be twinned by pushing over one corner? What is the essential difference between polishing and grinding? What is the effect of impurities on hardness? Is it comparable to their effect on electric conductivity? What is the cause of this effect?

In considering the cracking of a material like glass, Hertz seems to think that its cracking will depend only on the tension; that it will crack where the tension exceeds a certain limit. He does not seem to consider whether it might not crack by shearing with hardly any tension. It is doubtful whether a material in which there were sufficient general compression to prevent any tensions at all, would crack. Rocks seem capable of being bent about and distorted to almost any extent without cracking, and this might very well be expected if they were at a sufficient depth under other rocks to prevent their parts being under tension. It is an interesting question whether a piece of glass could be bent without breaking if it were strained at the bottom of a sufficiently deep ocean. On the other hand, there seems very little doubt that the parts of a body might slide past one another under the action of a shear, and would certainly crack unless there were a sufficiently great compressive stress to prevent the crack, and that consequently a body might crack, even though the tensions were not by themselves sufficiently great to cause separation, and might crack where the shear was greatest, and not where the tensions were greatest.

Then follow some papers on hygrometry and evaporation. A very interesting point is raised in this latter connection. Can a liquid evaporate at an unlimited rate if the vapour produced is removed as rapidly as it is evolved? From two points of view Hertz shows that there is a limit, and by his experiments went far to show that there was no other cause limiting the rate of evaporation. The first point of view was that a limit is imposed by the difficulty of supplying heat sufficiently rapidly to keep the surface temperature constant. He does not seem in his experiments to have attempted to supply this by radiation, but was content to allow the liquid to supply itself by conduction and convection from below. The second point of view was that the molecules could not leave the surface faster than they would be moving in the vapour that was formed. Hertz's investigation of this case only assumes an average velocity for the molecules; he does not consider the distribution of velocity among the molecules, nor whether they escape equally easily in all directions. The experimental investigation of the conditions of evaporation is extremely difficult; and until some more satisfactory method of studying these conditions be invented, the rough approximation seems to be sufficient to explain the observations. It might be interesting to see whether there was any difference between the superficial friction of a gas and a liquid which did not evaporate, and of a vapour in contact with its own liquid. In one case there would be no exchange of molecules between the two bodies that were sliding past one another, while in the second case there would be an exchange. A study of the conduction of heat between a gas and a liquid might also help to elucidate the nature of the exchange which takes place between a liquid and its vapour.

In the paper on the vapour pressure of mercury, there are some very rough approximations which are hardly sufficiently accurate for general application. One is as to the extent to which a saturated vapour obeys the laws of a perfect gas. Hertz assumes that this is more nearly true the lower the temperature. This is not generally so. For each kind of material there is a particular temperature at which its saturated vapour most nearly obeys these laws, and below as well as above this temperature it departs from these laws. Again, there is a process, described at the bottom of p. 203 and top of 204, which cannot possibly be carried out. He says: "Let a quantity of liquid at temperature T be brought to any other temperature. At this temperature it is converted into vapour without external work." This is absolutely impossible, and the equation he deduces from all this is not true, though it is sometimes a rough approximation to the truth.

There is a very interesting paper on the floating of bodies by thin sheets of rigid material like ice. Hertz shows that if the sheet be large enough it would be possible to cause a thin sheet of iron, which by itself would sink, to float by placing weights at its centre. The weights might so depress the centre and make the sheet so boat-shaped as to float both themselves and it.

In 1883 Hertz published a deduction from first principles of Maxwell's equations for the electromagnetic field in the symmetrical form, afterwards used by himself in his investigations on oscillatory discharge waves. He applies the very same arguments by which Helmholtz, Lord Kelvin, and others had argued from the work done by one electric current on another, that there must be a corresponding reaction of the second on the first current, and hence deduced electromagnetic induction. Hertz applies this argument to the case of a ring magnet changing in strength and producing magnetic force on another ring magnet in its neighbourhood, and doing work there, and shows thereby that there should be a magnetic force due to a changing electric field exactly corresponding to the electric force due to a changing magnetic field. This, of course, is what Maxwell describes as the magnetic effect of the changing electric displacement, and its effects are expressed by the very same equations as Maxwell deduces. The argument is no more and no less conclusive than in the corresponding application of the principle of the conservation of energy to deduce ordinary electromagnetic induction. Hertz is careful to point this out, for he was early imbued by Helmholtz with the fact that the principle of the conservation of energy is by itself utterly inadequate as a complete explanation of physical phenomena. He specially mentions himself Helmholtz's interest in this problem of the simplest basis for dynamics, and Hertz's last great work was to place general dynamics on a sound basis. The simplest of all cases is the easiest in which to see how the principle of the conservation of energy fails to give a complete solution. A body moving without any action from other bodies describes a right line at a constant velocity. The principle of the conservation of energy requires the constant velocity. But, why the right line? Conservation of energy cannot solve even the simplest of all examples. It would be well if some modern chemists would mark, learn, and inwardly digest this.

The part of Hertz's work which is of greatest interest just now is that in connection with kathode rays. He began with some very interesting observations on the aura accompanying spark discharges. It appeared to be projected from the positive electrode, and occasionally formed a vortex ring of incandescent gas, which lasted for an appreciable time between the electrodes of a jar discharging in air. Goldstein has noticed similar effects. and some recent experiments on the discharge of large Leyden batteries, in which some of the phenomena of globular lightning seem to have been reproduced, make it appear possible that this latter is a spherical vortex of incandescent air.

Hertz's study of kathode rays in 1883, set finally at rest two questions. In the first place he showed that the discharge in a gas may be as continuous as any other form of current. In no case are we absolutely certain that the current is absolutely continuous. On the large scale it certainly is; but all we know of electrolysis seems to show that on a sufficiently small scale the current is carried in detachments, and is consequently essentially discontinuous. This, however, was not the question at issue, and so far as a continuity of the same kind as that in any liquid electrolyte is concerned, Hertz showed that the discharge through a gas might be equally continuous. The second question he decided was as to the direction of flow of the average current in an exhausted space. He showed that the average flow at any point was nearly the same as if the whole space were a conductor: that there was no connection between the kathode rays and the flow of the current. From experiments on kathode rays projected down a tube, and quite away from both electrodes, he deduced that they produce no magnetic action outside the tube, although they are deflected by the magnet. His conclusion, that the kathode rays are not streams of electrified particles, was largely founded on this, and on another experiment on the action of electrostatic force on the particles. This experiment on the magnetic action of kathode rays is quite inconclusive, and it is very remarkable that Hertz should have attributed much importance to it. Whatever current was carried down his tube by the kathode ray must have come back the tube by the surrounding gas, and these two opposite currents should have produced no magnetic force outside the tube; and this is exactly what Hertz observed. In a similar way, what he observed in the case of a flat box was the average direction of the current, and he showed that this average direction was approximately the same as in a conducting sheet. This proved that if there were any concentration of the current along the direction of the kathode rays, this concentration was neutralised by a corresponding return current, so that the average current was as described. At the same time there does not seem much doubt but that the kathode rays only carry a very small part of the current. The third part of the paper is concerned with the electrostatic effects due to kathode rays. The experiments do not seem to fully justify the conclusion drawn, that kathode rays cannot be charged molecules. Sufficient account does not seem to have been taken of the shielding action of the conducting gas surrounding the kathode ray, nor of the way in which the potential is distributed between two electrodes in a gas. Hertz describes an experiment with two plates inside the tube kept at a considerable difference of potential. He says: "The phosphorescent image of the Ruhmkorff coil discharge appeared somewhat distorted through deflection in the neighbourhood of the negative strip; but the part of the shadow in the middle between the two strips was not visibly displaced." Now this is exactly what one might expect, because the fall of potential between two such strips is very small indeed, except close to the negative strip, and there the electric force *did* deflect the rays. Hence the conclusion is just the reverse of the one Hertz gives. From the experiment it appears that kathode rays do behave like electrified particles. It is very remarkable that in all these investigations Hertz does not once even mention, as a thing to be explained, the repulsive actions which Crookes observed, and which have been almost universally attributed to the impact of gas particles.

The other important paper, on the transmission of kathode rays through thin metallic films, is particularly interesting as the starting-point for Lenard's work, which has resulted in the discovery of the X-rays. A good deal of what Hertz observed would be accounted for by the production of X-rays where the kathode rays meet the diaphragms, and by the reproduction of kathode rays mixed with X-rays on the other side of the diaphragm, which would thus act as a sort of local electrode. That something exists in a vacuum on the far side of such a thin film, which does not ordinarily exist in X-rays in air, seems conclusively proved by there being something there which can be deflected by a magnet. There seems no doubt that kathode rays themselves are quite invisible, and that it is only where they are interfered with by gaseous molecules or by phosphorescent solids that they are sources of light. This is very much what one would expect. An electrified atom would not in general be a source of light unless its free movement were interfered with by impact.

The concluding article, on his master Helmholtz's seventieth birthday, is a noble and generous tribute to that great teacher's abilities and character. How truly he portrays the important characteristics of a University Professor! "It is true that Helmholtz never had the reputation of being a brilliant university teacher, as far as this depends upon communicating elementary facts to the beginners who usually fill the lecture-rooms. But it is quite another matter when we come to consider his influence on trained students, and his pre-eminent fitness for guiding them in original research." The most important duty of a University is to increase the knowledge of mankind, and to train up a new generation who may be able to continue the good work. It is thus mankind has advanced since the dawn of civilisation in Egypt. He who produced the most enthusiastic disciples has most advanced the well-being and the well-living of the race.

G. F. F. G.

THE BUREAU OF ETHNOLOGY AT WASHINGTON, U.S.A.[1]

THE Bureau of Ethnology at Washington has, during the last sixteen years, been carrying quietly on a work of the importance of which, we feel sure, that a number of students of anthropology have no knowledge whatever; we are equally sure that work itself, as well as those who labour in it, has not received due recognition. It is now nearly thirty years since the exploration of the Colorado River of the West was begun by the Act of Congress in America, and it is nearly twenty years since the various geographical and geological surveys which sprang up in connection therewith were dissolved, and since the foundation of the United States Geological Survey became an established fact. In the course of the work carried on by the Survey its various members made most exhaustive anthropologic researches among the North American Indians, and the myriads of facts which these self-sacrificing workers collected were fortunately rescued for the benefit of all students, and for all time, by the beneficent help of the Smithsonian Institution, which had secured provision for the publication of a series of monographs on almost every subject connected with the manners and customs, history, religion, and languages, &c., of the various Indian tribes with which they came in contact. Under the authority of the Act of Congress, the Secretary of the Smithsonian Institution entrusted the management of this great work to the former Director of the Rocky Mountain Region Survey, Mr. J. W. Powell, and thus the Bureau of Ethnology was practically established. It is a pleasant thing to be able to record that Congress supported the work both with patronage and with pecuniary assistance, and all will confess that the contributors to the success of the Bureau have worked with a will so as to employ in the best possible manner, and to the best possible end, the funds which have been placed at their disposal. We have before us thirteen handsome volumes of Reports, each containing several hundred pages of closely-printed matter, and profusely illustrated with well-executed coloured plates, and many hundreds of woodcuts. No reviewer of these volumes could attempt to give an adequate account of them unless he had some scores of pages at his disposal, and it goes without saying that all that any writer can do here is to call attention to the plan of Mr. J. W. Powell's volumes and to the general contents, hoping that the reader will devote some portion of his leisure to the perusal of a set of works which are at once of the greatest interest to those who study man and his ways, and of the first importance to the student of ethnography.

In setting out on his work, Mr. J. W. Powell says that throughout "prime attention has been given to language," for "with little exception all sound anthropologic investigation in the lower states of culture exhibited by tribes of men, as distinguished from nations, must have a firm foundation in language. Customs, laws, governments, institutions, mythologies, religions, and even arts cannot be properly understood without a fundamental knowledge of the languages which express the ideas and thoughts embodied therein." As a result of this opinion, the officials of the Bureau of Ethnology have devoted themselves to collecting materials for dictionaries of the North American languages, and for chrestomathies, and in time they hope to put grammars of the same before the world. With a view of enabling the philological student to determine what help he may or may not be able to obtain from these languages, the authors of the volumes before us give, every here and there, selected texts accompanied by interlinear transliterations, much in the same way as the early Egyptologists used to do in publishing hieroglyphic texts; and there is no doubt that this is a most useful plan. That it enables the careful reader, at times, to trip up his editor is true; but it is an honest method, and will be much appreciated by all painstaking students, for comparisons of words can thus easily be effected. Turning, though only for a moment, from language and from the characters which express language, that is to say writing, we see at a glance that the peoples of North America had many things in common with the most ancient civilised nations of antiquity. We do not for a moment believe that every custom and belief which may be found among them should be used to connect them with the ancient Chinese, or Indians, or Babylonians, or Egyptians; but it seems perfectly clear that every primitive nation, wherever it may live on the globe, or whatever may be the circumstances under which it lives, has certain fundamental ideas about the future life, and religion, and morality, which closely resemble those of other early nations. It seems tolerably clear, too, that many anthropologists have erred somewhat in tracing connections between peoples of totally different races, which they have deduced from observing that they had many beliefs in common. A careful examination of the characters employed by early nations to express their ideas makes this quite plain, for as pictures were used by them all for this purpose, we have only to trace the conventional sign back to its oldest form to find out what fundamental ideas existed in their minds. Primitive

[1] The Annual Reports of the Bureau of Ethnology to the Secretary of the Smithsonian Institution, by J. W. Powell, Director. 13 Annual Reports. (Washington: Government Printing Office, 1881-1896.)